Universität St.Gallen

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October 2021 Discussion Paper no. 2021-11

School of Economics and Political Science, Department of Economics

University of St.Gallen

Editor:	Mirela Keuschnigg University of St.Gallen School of Economics and Political Science Department of Economics Müller-Friedberg-Strasse 6/8 CH-9000 St.Gallen Email seps@unisg.ch
Publisher:	School of Economics and Political Science Department of Economics University of St.Gallen Müller-Friedberg-Strasse 6/8 CH-9000 St.Gallen
Electronic Publication:	http://www.seps.unisg.ch

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[1] We thank Markus Rosenbaum and Jonas Klärchen provided excellent research assistance. Richard Bluhm acknowledges financial support from the Humboldt foundation. We are grateful for helpful suggestions by Samuel Bazzi, Sascha Becker, Roland Hodler, Ruixue Jia, Paul Raschky, Tobias Rommel, Mark Schelker, Kurt Schmidheiny, Claudia Steinwender, Michael Peters, Beatrix Eugster, Michelle Torres, Nick Tsivanidis, and comments from seminar participants at UC San Diego, University of St. Gallen, University of Bergen, University of Hannover, Monash University, The College of William \& Mary, and participants at APSA, DENS, UEA, and the Cities and Development workshop.

Abstract

We study the link between subnational capital cities and urban development using a global data set of hundreds of first-order administrative and capital city reforms from 1987 until 2018. We show that gaining subnational capital status has a sizable effect on city growth in the medium run. We provide new evidence that the effect of these reforms depends on locational fundamentals, such as market access, and that the effect is greater in countries where urbanization and industrialization occurred later. Consistent with both an influx of public investments and a private response of individuals and firms, we document that urban built-up, population, foreign aid, infrastructure, and foreign direct investment in several sectors increase once cities become subnational capitals.

Keywords

Capital cities, administrative reforms, economic geography, primacy

JEL Classification

H10, R11, R12, O1

1. Introduction

The last four decades have ushered in a wave of decentralization reforms in developing countries and, to a lesser extent, in developed countries. These reforms have a variety of objectives. International organizations (e.g., the World Bank) and national donor agencies (e.g., USAID) emphasize the potential of decentralization reforms to improve public service delivery and local governance but their effects go well beyond these often cited goals. Decentralization reforms frequently coincide with a redesign of the territorial structure and a proliferation of administrative units. While this trend has been documented, we know little about how the creation of new administrative units, and hence new subnational capitals, shapes the concentration of economic activity in rapidly urbanizing developing countries.[1]

The key objective of this study is to examine how the change in status of a city to a subnational capital influences the location of economic activity in the short and medium run. Contrary to the literature on decentralization reforms, which is typically concerned with subnational states or municipalities in a single country or region, our analysis focuses on city growth in a global sample of cities. We compile data on hundreds of first-order administrative reforms that result in changes of the capital status of cities. Using these reforms and the varied contexts in which they occur, we ask *i)* whether capital cities increase density and attract more economic activity to a location and its surroundings, *ii)* whether these effects are heterogeneous, and *iii)* through which mechanisms, such as public or private investments, these effects occur. Answering these questions has important policy implications. We shed light on the circumstances under which politics can shift activity to preferred locations and whether it can complement or substitute for (a lack of) locational fundamentals.

To do so, we require comprehensive data on cities and whether they were treated by administrative reforms. Using Geographic Information Systems (GIS) and a plethora of more traditional sources, we assemble data on all first-order sub-national units and the location of their capitals over the period from 1987 until 2018. We then detect the boundaries of all cities with a population above 20,000 people in 1990 (and 2015) using data derived from high resolution daytime images (in an approach similar to Rozenfeld et al., 2011; Baragwanath et al., 2019; Eberle et al., 2020) and assign these cities their time-varying administrative status. We measure annual variations in economic activity at the city level using nighttime light intensity and consider variations in light intensity at the local level to be mostly driven by differences in population density (see e.g. Henderson et al., 2018). To capture how attractive particular locations are, we compile an array

[1] Grossman and Lewis (2014) coin the term 'administrative unit proliferation' and document a rise in first-order administrative units in sub-Saharan Africa. Our data shows that this pattern holds globally at the highest level of subnational government. As Blom-Hansen et al. (2016) point out, developed economies simultaneously reduced the size of the lowest level of government through municipal mergers.

of geographic characteristics for the greater area inhabited by each city, ranging from agriculture over internal market access to the ease of external trade. This gives us a globally comparable sample of cities and their characteristics.

We analyze the effects of capital city reforms on city growth using event studies and difference-in-differences specifications. Our primary source of identifying variation is more than three decades of panel variation in the political status of cities. While the choice to reform a particular province and promote or demote a city to a subnational capital is seldom random, we document that the timing of these reforms is usually unrelated with pre-reform characteristics of these cities and that unobserved confounders are likely to affect all cities in a reformed region similarly. This is aided by our focus on first-order capital cities. Their importance in the political hierarchy of a country implies that reforming them often requires constitutional changes and includes political considerations which are unrelated to local conditions at the city level. To strengthen this approach and minimize the scope for dynamic selection into treatment, we focus on cities in regions which are reformed. Testing for pre-trends suggests that the identifying assumptions hold both in this subset, the larger sample, and matched samples with comparable control cities. The pattern of leads and lags shows no anticipatory increases in activity but a substantial effect following the reform, which is inconsistent with unobserved shocks driving our results.

Our analysis establishes several new findings. First, we show that there are sizable advantages to receiving the status of a subnational capital, which persist in the medium run. Economic activity (density) in new capital cities rises by 8–15% following a territorial reform. The event-study estimates shows that these effects take about two years to materialize and gradually increase during the first five years after the change in status.[2] Second, we find evidence of positive spillovers. Both the larger agglomeration and surrounding cities benefit from being proximate to a city with elevated political status. Our analysis suggests that there are positive but decreasing benefits for cities located up to 100 km away from the new capital. Third, we document that these average effects summarize substantial heterogeneity across local contexts. We focus on three aspects: *i)* we document that locating subnational capitals in areas with better fundamentals, in particular better internal market access, has a larger effect on economic activity than locating them in areas with, say, better agricultural fundamentals, *ii)* we show these advantages are not uniform across the level of development but are greater in countries which have started to agglomerate later, and *iii)* we find evidence suggesting that these effects depend on economies of scale, that is, the size of the territory governed by the new capital city. This suggests that politics exerts a more powerful force on the spatial equilibrium when urbanization is occurring and the urban network is not fully settled,

[2]This matches recent micro-level evidence. Dahis and Szerman (2020), for example, document similar effect sizes in Brazilian municipalities.

but also that policy-makers are constrained in how much they can shift economic activity into the hinterland in the medium run.

When it comes to mechanisms, we observe significant increases in housing supply (urban built-up), find indirect evidence of in-migration to new capitals, and evidence of increased private investments in finance and insurance, manufacturing, and other productive sectors in capital cities. Public investments are also larger in capitals and appear to be concentrated in water and sanitation, infrastructure, and government. This suggests that our findings are unlikely to be driven by increases in public employment in capital cities alone. Consistent with the pattern of private and public investment, we find micro-evidence that residents of capital cities have a better access to utilities, education, and have lower rates of child mortality compared to residents of non-capital cities.

Our results contribute to several larger bodies of work. The first is the literature on how politics influences the concentration of people in particular locations. Seminal papers in this literature have established that political instability is associated with primacy (Ades and Glaeser, 1995; Davis and Henderson, 2003) and that excessive concentration in primate cities can be costly in terms of productivity (Henderson, 2003). More recently, Henderson et al. (2018) show that city locations among countries which have developed earlier tend to have been more influenced by agricultural characteristics and exhibit a more balanced distribution of economic activity. Our results add a policy-relevant margin to this finding. Although countries which began to agglomerate late exhibit a higher level of spatial inequality today, decentralizing their territorial structure can influence their spatial equilibrium.

A central insight of economic geography is that locally increasing returns to scale and path dependence can explain why we observe cities in places that do not seem to have favorable fundamentals today (Krugman, 1991; Allen and Donaldson, 2020). Economic and political shocks can have a large and persistent effect on the location of economic activity. Empirically, the literature has focused on large historical shocks, such as wars, and their long run effects on agglomeration (Davis and Weinstein, 2002; Miguel and Roland, 2011; Michaels and Rauch, 2018) or on historically important advantages which are now obsolete (e.g. Bleakley and Lin, 2012). Bai and Jia (2020), in a closely related contribution, study the effects of provincial capitals on population in Chinese prefectures in the very long run and show that prefectures that lose a provincial capital eventually fall back into insignificance. We show that economic fundamentals, such as internal market access, play an important role for capital city growth in the medium run. Granting capital status to cities in locations with good fundamentals spurs more agglomeration in more productive locations, which is likely to be welfare improving (Allen and Donaldson, 2020) when the relative importance of particular fundamentals shifts only slowly. Our finding of complementarity between politics and fundamentals also speaks to the literature on place-based policies (Glaeser and Gottlieb, 2008; Kline, 2010; Neumark and Simpson, 2015), which has yielded mixed results empirically, but highlights the potential (and challenges) of exploiting agglomeration economies through public policy.

Beyond that, our paper also relates to several areas in political economy. A nascent literature examines the costs of isolated state capitals. State capitals in the US that are located away from the respective economic centers are associated with more corruption, less accountability and lower public good provision (Campante and Do, 2014). This raises the question whether some locations are generally more suited to become capitals which we address here. A related literature in political science focuses on administrative unit proliferation in developing countries. Grossman and Lewis (2014) document this trend in sub-Saharan Africa. Last but not least, our paper speaks to an extensive theoretical literature on federalism and the optimal size of jurisdictions (see e.g. Oates, 1972; Alesina et al., 2004; Coate and Knight, 2007). Elevating cities to subnational capitals is a direct consequence of unit proliferation and the cities themselves are the most immediate realization of bringing politics closer to the people.

Several aspects of this paper aim to move the current literature forward. First, we offer new global data on first-order administrative and capital city reforms. There is an extensive list of single country studies focusing on the diverse impacts of decentralization reforms but sparse global evidence of this phenomenon. Second, leveraging large amounts of remotely-sensed data allows us to focus directly on cities, rather than administrative units which change as a result of territorial reforms. Third, taking a global perspective enables us to ask a different set of questions over a shorter time frame. The average causal effect of gaining capital city status is unlikely to be meaningful locally. The heterogeneity in fundamentals and national contexts, however, allows us to exploit interesting variation that would not be available within a single country.

The paper is organized as follows. Section 2 presents the data on capital city reforms and describes the global sample of cities. Section 3 discusses the empirical strategy. Section 4 presents the results and discusses them. Section 5 investigates heterogeneity and Section 6 mechanisms. Section 7 concludes.

2. Data

We start by describing our data, focusing on the construction of our main variables of interest. Other data sources are introduced later when they are used for the first time. A key constraint is that all data need to be available on a global scale, which is why we rely heavily on remotely-sensed data. This is not necessarily a disadvantage. Little to no data is available on the city level in developing countries and, even if more were available, it would be difficult to harmonize measurement across countries. Satellite-based measures are consistently defined for the entire globe and allow us to apply uniform definitions throughout. Moreover, when examining the mechanisms, we supplement this data with other measures that have been manually compiled and geocoded. Online Appendix F provides an overview of the sources, variables, their coverage, and descriptive statistics.

A. Capital city reforms

No off-the-shelf data systematically record administrative reforms, the boundaries of administrative units, and the location of capital cities across the world, although two sources come somewhat close. The Global Administrative Unit Layers (GAUL) project of the Food and Agriculture Organization (FAO) tracks the spatial evolution of administrative units between 1990 and 2014 across the world. Drawing on input from a variety of sources, the Statoids project collects (non-spatial) information on capital cities and administrative units (Law, 2010). Unfortunately, both data sets are riddled with errors and omissions, cover different time spans, and do not contain coordinates of capital cities. Other sources only document the most recent boundaries and contain no information about the relevant time-frame of these administrative borders or their capital cities (such as the Database of Global Administrative Areas).

To fill this gap, we compile new data containing the names and spatial extent of all first-order administrative units from 1987 until 2018, including the names and locations of capital cities over time. The data covers all types of territorial reforms, that is, splits and mergers of provinces, area swaps, capital city re-locations and the creation of new countries. While we draw on the GAUL project and Statoids, we complement these two with a variety of sources, ranging from national atlases over Wikipedia to various editions of the Atlas Britannica.

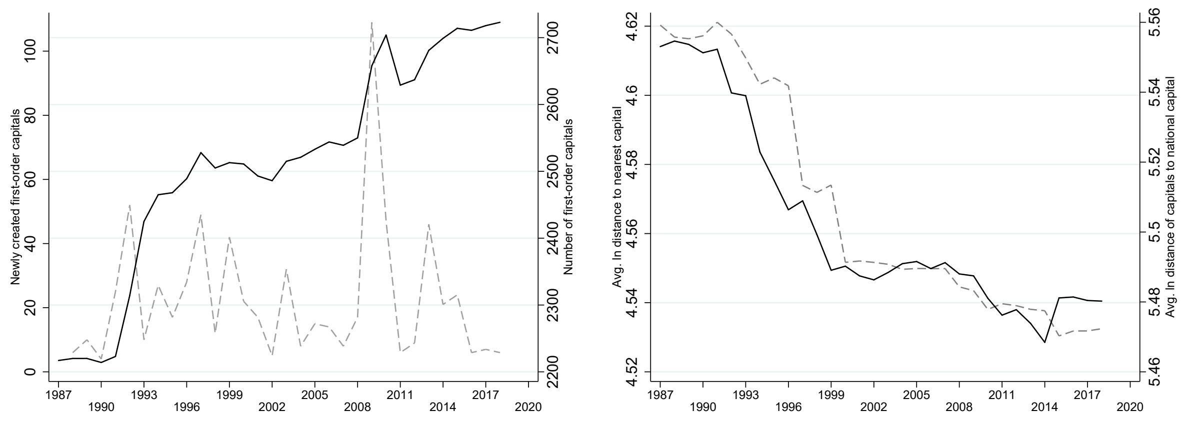
Panel A of Figure I illustrates the variation in the number of capital cities over time. We observe a net increase of 506 capitals and new first-order units over the entire period from 1987 to 2018. Note that this understates the variation in our data, as some cities lose their capital status at the same time, some countries become independent over this period, and in a few rare occasions a capital city is simply moved within the same region.[3] In fact, when we track each city from when it enters our sample, we observe 701 cities which have gained capital city status and 336 cities who have lost this status over the same period. Panel A of Figure I also shows that a substantial number of new capitals has been created in every decade since 1987 (net of the creation of new countries). Panel B of Figure I highlights that new capitals are both intensifying and expanding the capital network over time, i.e., reducing the average distance between capitals and the national capital, and the distance of non-capitals to any subnational capital.

After identifying all administrative units and their capital cities, we create harmonized

[3] Our sample includes all countries which have a population of at least 1.5 million people, a land area of at least 22,500 km^2, and have gained independence before 2000. Smaller states typically only have one administrative layer and are not well captured by our approach. To document that this is the case, we compiled time-varying administrative data on these countries as well.

Figure I
Subnational capitals: Global trends

(A) Number of subnational capitals (B) Density of capital network



Notes: The figure illustrates how global trends in territorial reforms change the number and network of first-order administrative capitals. Panel A illustrates the net number of capital cities over time and the number of cities which became capital cities in each year. Newly independent countries are included in the former but not in the latter. We omit the Sudan and South Sudan after their separation in 2011. Panel B plots the average log distance of cities to the nearest capital in gray and the average log distance to the national capital within countries in black.

geospatial data. This is a two step process. First, we identify suitable vector data which accurately represents the boundaries of each unit within a country at a particular point in time.[4] When no suitable data are available, we use international or national atlases, georeference and digitize the corresponding map. Second, we geocode all capital cities.[5]

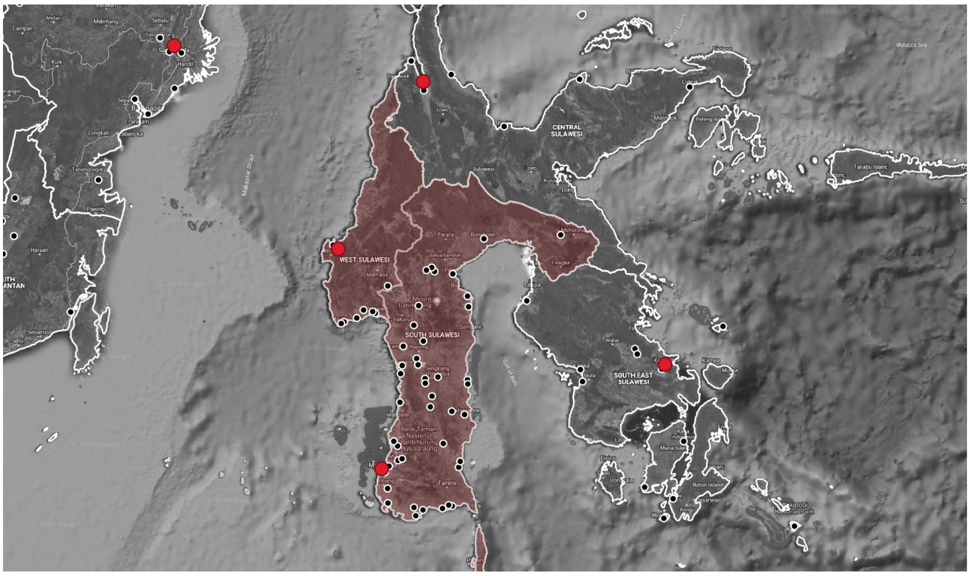
Figure II illustrates a typical provincial split, which is frequent in our data and will be the basis of our identification strategy. South Sulawesi (Sulawesi Selatan) was the fifth largest province of Indonesia with a population of about 8 million people in 2000. In 2004, West Sulawesi (Sulawesi Barat) was created out of the northwestern segment of the southern province. The new province had a population of little more than one million people and completed the partition of the island into north, south, east and west that was started in 1964. Makassar remained the capital of the south, while the city of Mamuju received the new status of a provincial capital.

Online Appendix B contains a detailed explanation of the data construction and provides summary statistics. Online Appendix C provides descriptive statistics about which (static) variables correlate with with the probability that a particular city becomes a capital during a territorial reform.

[4]This involves a variety of sources (e.g., GAUL, GADM, Digital Chart of the World, United Nations Environment Program, and AidData's GeoBoundaries project) and an algorithm which re-allocates small differences in boundaries to match those reported most accurate data sources.

[5]Research assistants verified the data for each country-year and flagged potential errors for quality control and arbitration.

FIGURE II
A provincial split: West and South Sulawesi in Indonesia



Notes: The figure illustrates the split of the former province of South Sulawesi into South and West Sulawesi in 2004. Post-2004 boundaries are indicated in white. The pre-reform area of the province is shaded in red. Red dots indicate capital cities. Black dots indicate other cities detected using our approach.

B. Urban boundaries and economic activity within cities

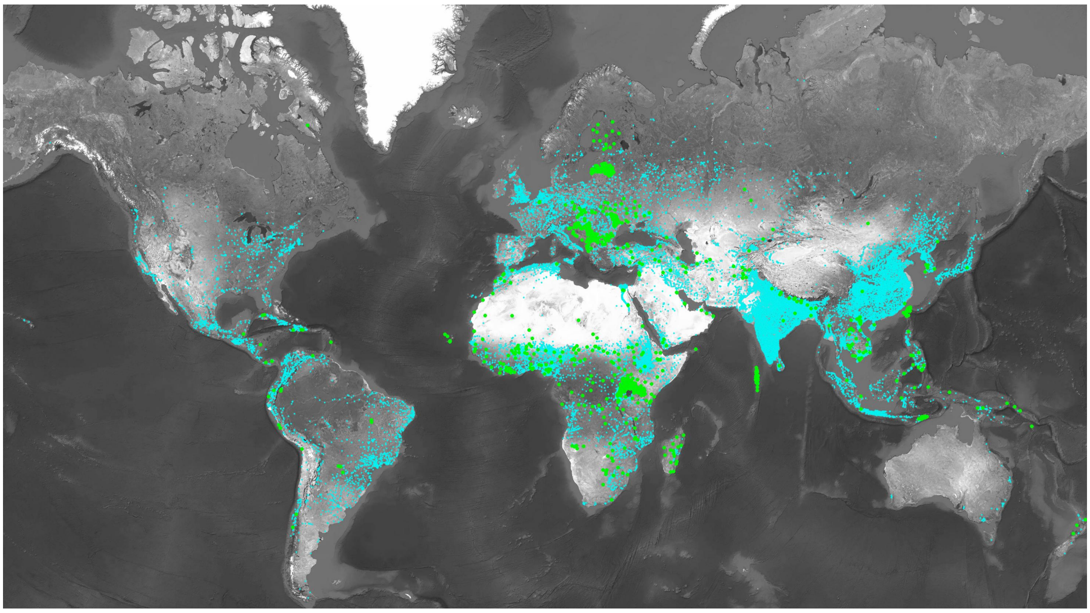
Our city-level approach requires us to identify the urban footprint of a host of potential control cities in addition to the administrative capitals. We follow a recent literature in urban economics which uses daytime images to accurately delineate city boundaries and nighttime light intensities as a proxy for economic activity within those boundaries (e.g. Baragwanath et al., 2019). Remotely-sensed city footprints diverge from administrative definitions in the sense that they tend to capture larger agglomerations which often run across several smaller cities. Using a globally consistent definition of cities is an important feature of our analysis.

We rely on two products from the Global Human Settlement Layer (GHSL)[6] derived from global moderate resolution (30 m) Landsat images and auxiliary data. The first is a built-up grid at a resolution of 1 km. It indicates the density of buildings and other human structure detected in the underlying high resolution data. The second is a population grid at the same resolution. It takes census estimates of the population at the smallest spatial scale available and distributes them using built-up intensities. Both products are available for 1975, 1990, 2000 and 2015. We use the 1990 and 2015 data to define the initial and final footprint of a city.[7]

[6]The data is constructed by the Joint Research Centre and the Directorate General for Regional and Urban Policy of the European Commission. It can be accessed at https://ghsl.jrc.ec.europa.eu.

[7]The GHSL project also provides a pre-classified layer of cities, the GHS settlement model, which is available for the same years. We do not use this layer in order to be able to control every parameter

Figure III
Locations of capital and non-capital cities in 1990



Notes: The figure shows the coordinates of 24,315 cities with a population above 20,000 people detected using the clustering algorithm described in the text. All cities are shown in blue. Cities elevated to capitals during the 1987-2019 period are highlighted in green.

Our definition of a city or an agglomeration follows a recent literature on urban boundaries by applying a city clustering algorithm (Rozenfeld et al., 2011; Dijkstra and Poelman, 2014; Baragwanath et al., 2019). We consider a city to consist of a connected cluster of 1 km pixels with at least 50% built-up content per pixel or a minimum population density of 1,500 people per pixel (as in Dijkstra and Poelman, 2014). Any cluster with an estimated population of at least 20,000 people is a city. While this is lower than the typically employed threshold of 50,000 people, it allows us to capture more secondary cities and towns in initially less urbanized developing countries. In fact, our data represents the global urban population quite well. Our data suggest an urban population of 2.59 billion in 1990 compared to the 2.27 billion reported by the World Bank. The difference becomes smaller still when we use the 2015 boundaries, with which we find 3.95 billion urban dwellers based on our data compared to 3.96 billion reported by the World Bank. We later document the robustness of our results to this parameter.

Our primary level of analysis is the universe of cities in 1990. Figure III shows the coordinates of about 24,000 cities detected in this manner. We also define larger *agglomerations* as the union of the initial and final boundaries, which will allow us to study overall growth later on. Naturally, we obtain fewer agglomerations than cities when joining the boundaries, as cities expand and merge into one over time. When studying agglomerations, we focus on new parts of a city forming around a 1990 city or cities which

which defines a city, including the population threshold.

become amalgamated and ignore new cities detected only in 2015. The main reason is that the detection probability of a city increases (relative to non-capital cities) when it becomes a capital.[8]

Figure IV illustrates this approach using the city of Mamuju, Indonesia. We observe a significant increase in the urban perimeter as the city grew from less than 50,000 people in 1990 to slightly more than 175,000 by 2015. The envelope here corresponds to the 2015 boundaries, as they fully contain the urban area in 1990. The early boundaries, on the other hand, give an accurate indication of the older core of the city.

FIGURE IV
Urban footprint of Mamuju (Mamudju) in 1990 and 2015

Notes: The figure shows the urban area of Mamuju (or Mamudju) in Indonesia, as detected using the algorithms and data described in the text. The white boundaries delineate the 1990 footprint, while the yellow boundaries indicate the 2015 footprint (which coincides with the larger agglomeration). Slight differences in the coast line imply that one urban pixel is missing in both. The background shows a contemporary Google Maps image. Note that some of the urban areas with partial forest cover have a per pixel population density that easily crosses our threshold of 1,500 people. Google images are used as part of their "fair use" policy. All rights to the underlying maps belong to Google.

Our primary outcome is the log of nighttime light intensity from the Defense Meteorological Satellite Program Operational Linescan System (DMSP-OLS). These data have been used in a variety of small scale and city level applications (starting with Storeygard, 2016) but suffer from sensor saturation in cities which severely understates economic activity in urban centers relative to rural areas (Henderson et al., 2018; Bluhm and Krause, 2018). For our main analysis, we use a version of this data which has been corrected for bottom coding[9] and top coding (see Bluhm and Krause, 2018, for

[8] We discuss and provide evidence for this selection effect in Online Appendix D and show that focusing on the initially detected set of locations leads to more conservative estimates.

[9] We use a simple adjustment to remove artificial variation at the bottom. The stable lights detection

details). We present results varying these adjustments later in the robustness section. We normalize light by the area of the city in 1990 to study increases in density (and henceforth refer to this measure as light density or intensity).

Our preferred interpretation is that light intensity proxies for population density in the city.[10] We thus view our results in light of a large literature in urban economics which emphasizes the importance of city size and population density for productivity (see e.g. Rosenthal and Strange, 2004; Combes et al., 2010). While density is not always synonymous with more economic activity, an emerging literature documents that city dwellers in developing countries are substantially better off than those living in the countryside. Gollin et al. (2016) and Henderson et al. (2020), for example, document that amenities in cities are higher than in the country-side (in addition to high urban wages). They find a positive density gradient in access to public goods and many other outcomes. Henderson and Turner (2020) argue that this relatively high productivity of developing country cities could imply developing country urbanization might be too slow.

C. Additional data

To capture how economic fundamentals vary with city locations, we compute a large set of geographic characteristics for a 25 km radius around the centroid of each agglomeration and assign these to the cities constituting the larger agglomeration. While the overwhelming majority occupy an area far smaller than this, the main advantage of focusing on such large areas is that we capture how well suited the area surrounding the city is for different economic activities.

We use three types of fundamentals describing how attractive a particular location is for agriculture, internal trade, or external trade. All of these are time-invariant. The set of agricultural characteristics consists of wheat suitability, temperature, precipitation, and elevation. External trade integration is measure by a set of distances: a dummy if the city is within 25 km of a natural harbor or the coast, and the continuous distance to the coast. Our measures of internal trade are dummies whether a city is within 25 km of a river or lake and a measure of market access in 1990. Market access of each city is defined by the sum of the cost of trading with every other city, the population of that other city and the market access of every other city to others in the same country. Donaldson and Hornbeck (2016), for example, show that such a measure summarizes the direct and

process carried out by the U.S. National Oceanic and Atmospheric Administration (NOAA) filters our background noise by effectively setting all clusters of pixels with a value of 3 or less equal to zero (Storeygard, 2016). Since we know that all light in our sample originates from a city, we undo this filtering by imposing a lower bound of 3 DN for each city pixel.

[10] Henderson et al. (2018) make a similar claim and use data on subnational regions to show that, conditional on country fixed effects, the R^2 from a regression of lights on population density is 0.775, whereas it falls to 0.128 for income per capita. This correlation is just suggestive, given that local purchasing power parities are not available in most countries.

indirect effects of changes in trade costs in general equilibrium trade theory. Since we are not interested in changes in trade costs elsewhere, we do not construct costs using along the actual road or rail network but use geographic distances to create a measure of the initial market access of each city at the start of the sample.[11] Moreover, we use ruggedness (Nunn and Puga, 2012) and the estimated malaria burden (Depetris-Chauvin and Weil, 2018) to proxy for how hospitable a place is for human settlement.

3. Empirical strategy

Capital city reforms rarely occur in response to exogenous shocks, such as natural disasters.[12] In the absence of a randomized experiment on the location of subnational capitals, we will use observational data and leverage two aspects of the reform process: *i)* the timing of reforms is often idiosyncratic and, more importantly, *ii)*, unobserved confounders are likely to affect all cities in reformed regions similarly. In other words, other cities in the region that will be reformed (i.e., split) were likely candidates to become capitals and were on similar growth trajectories before the reform took place.

A. Event-study design

Our base specification tests the role of capital cities in an event-study framework, where we exploit the switching of some cities into status of a capital. We specify a standard event-study specification with an effect window running from $\underline{j}$ to $\overline{j}$ for all $t = \underline{t}, \dots, \overline{t}$

$$\ln \textsc{Lights}_{cit} = \sum_{j=\underline{j}}^{\overline{j}} \beta_j b_{cit}^j + \mu_c + \lambda_{(i,d)t} + \mathbf{z}_c' \boldsymbol{\gamma}_t + e_{cit} \tag{1}$$

where $\ln \textsc{Lights}_{cit}$ is the log of light density in the urban cluster, b_{cit}^j are treatment change indicators binned at the endpoints[13], μ_c are city fixed effects and $\lambda_{(i,d)t}$ are country-year or initial-region times year fixed effects, $\mathbf{z}_c$ are time-invariant fundamentals and γ_t are time-

[11] Specifically, we define market access for each city c as $MA_c = \sum_{c \neq d} pop_{1990} \times dist_{cd}{}^{-\theta}$ where we set the distance elasticity θ to 1.4 following Baragwanath et al. (2019) and $dist_{cd}$ is the geographic distance from city c to city d. We exclude each city c from the summation to focus only on its relationship to other cities. Baragwanath et al. (2019) find that a non-trivial proportion of market access in India is explained by cities that are close by.

[12] We do observe an instance where the capital city was moved from Rabaul to Kokopo in Papua New Guinea's East New Britain province following the destruction of the former by a volcanic eruption.

[13] Borrowing the notation from Schmidheiny and Siegloch (2019), we define

$$b_{cit}^j = \begin{cases} \sum_{s=\underline{t}-\underline{j}}^{\overline{t}-\underline{j}-1} d_{cis} & \text{for} \quad j = \underline{j} \\ d_{ci,t-j} & \text{for} \quad \underline{j} < j < \overline{j} \\ \sum_{s=\underline{t}-\overline{j}+1}^{t-\overline{j}} d_{cis} & \text{for} \quad j = \overline{j} \end{cases}$$

where d_{cit} is a treatment change indicator.

varying coefficients on the fundamentals. We omit b_{cit}^{-1} so that all effects are estimated relative to the last pre-treatment period.

Our combination of city and country-year fixed effects implies that we essentially stack many individual country event studies. In this setting, λ_{it} nets out all country-wide variation in a specific year. This does not just include business cycle variation but also the national level decision to reform the territorial structure in more than one region at the same time. For most of our specifications, we go one step further and define $\lambda_{(i,d)t} \equiv \lambda_{dt}$ as initial-region times year fixed effects. Together with our focus on cities which gain the status as a capital, this structure implies a well-defined identification strategy: we compare the cities who gain the status after an administrative region is partitioned to all other non-capitals in the unpartitioned region. Shocks that affect all cities in the initial region within a particular year, such as the decision to reform the territorial structure or common trends, are absorbed. The influence of the fundamentals in the baseline period is absorbed by the city fixed effects. However, allowing time-varying coefficients on the fundamentals accounts for a variety of economically meaningful patterns. For example, trade-related variables could become more important for city growth over time while the influence of agricultural variables could fade (as in Henderson et al., 2018), or—later in the development process—market potential could become less important relative to local density (as in Brülhart et al., 2020).

The event-study design allows us to test for pre-trends and study the dynamics of the estimated treatment effect. When testing for pre-trends, we set $\underline{j} = -5$ to $\overline{j} = 5$ for a symmetric window around the treatment date. We rely primarily on visual evidence of the underlying specifications, where we report confidence intervals (clustered on initial provinces/regions) together with simultaneous confidence bands (which have the correct coverage probabilities for the entire parameter vector at 95%). We construct sup-t bootstrap confidence bands with block sampling over initial provinces to mirror the dependency structure of the errors (Montiel Olea and Plagborg-Møller, 2019).

B. Identifying variation and identification assumptions

The key identification assumption in these types of event-study designs is that light intensity in cities and the change in capital status are not both driven by some time-varying unobserved factor which affects treatment and control cities differently. First, we restrict the estimation sample to the set of treatments and obtain comparable control groups. Second, we discuss and analyze the timing of events and test for pre-trends in the outcome variable.

Treatment and control groups: Our data on subnational capitals and first-order administrative regions contains a wide variety of reforms (splits, mergers, re-locations and wholesale changes in the administrative territorial structure). A potential concern could be that these treatments are very different, in that they imply different pre-treatment trends and subsequent treatment effects, conditional on the chosen control group. For example, losing the status as a provincial capital in a merger could be associated with a secular decline in the importance of the city, resulting in pre-treatment trends. Moreover, our data and time frame are not well suited to deal with negative shocks to durable housing. We exploit the strengths of our setting and focus on the most policy-relevant effect of gaining the status as a subnational capital. Hence, we limit the sample to cases where an initial region or province is split, such that one or several new capitals are created. We defer the issue of capital loss to Online Appendix E, in which we discuss, appropriate comparison groups for different treatments, the effects of capital loss, as well as related issues such as lump capitals.[14]

TABLE I
Identifying variation

	All admin cities	Matched to urban clusters in 1990	Clusters in 1990 with single changes
Panel A. Event-study period, 1987 – 2018			
Always capitals	2,118	1,729	–
Gained status	701	335	281
Lost status	336	169	116
Panel B. Diff-in-diff period, 1992 – 2013			
Always capitals	2,211	1,807	–
Gained status	592	269	221
Lost status	275	124	85

Notes: The table shows summary statistics of the capital cities and urban clusters data. The capital cities data in column 1 covers all administrative centers, no matter if the city footprint is detected by the city clustering algorithm or not. The urban clusters data in column 2 shows how many of these capital cities have been matched to cities which pass the detection thresholds of the city clustering algorithm. Column 3 shows the subset of these which experienced a single reform.

Table I illustrates the capital city reforms we observe in our data and the subset we use for identification. For the event study, we obtain data on the treatment status from $\underline{t} = 1987$ to $\bar{t} = 2018$. We later collapse the estimates into a difference-in-differences design, for which we only use information on cities that switch their status during the period from $\underline{t} = 1992$ until $\bar{t} = 2013$. The first column shows the total number of cities and their changes in status, no matter if we actually observe them in the satellite-derived data on city footprints or not. The second column indicates how many urban

[14]Our identification strategy is also not well suited to deal with multiple treatments. There are several instances in our data (about 9% of the ever treated cities) where a capital was moved or a new administrative region was created sometime in the 1990s, followed by another reform in the 2000s. We discard all multiple treatments and focus only on instances where a city received the status as a subnational capital only once during the period of interest.

clusters derived from the 1990 satellite data were always capitals or experienced a change in status.[15] While we observe a large share of administrative cities in 1990, not all of them pass the population threshold of 20,000 in 1990 and many close-by capitals are matched to the same cluster. Several administrative cities in developing countries which are heavily decentralized by the end of the period, such as Uganda, are initially too small. We prefer to focus on the 1990 universe of cities, as this avoids selection problems by which cities pass the detection threshold in later years precisely because they became a subnational capital.[16] The last column highlights the switches which we effectively use for identification. The event-study design uses 281 cities which become capitals of which 221 switchers are observed during the 1992-2013 period for which we observe our outcomes.

We typically compute our results for two samples: *i)* all cities and *ii)* cities in provinces which have been or will be reformed within the period of observation. If we are concerned with potential spillovers and "forbidden comparisons",[17] then we would prefer a large control group. If we are concerned about obtaining a control group that closely resembles the treatment group, then we would prefer to restrict ourselves to places that are in close proximity. Given that our control group is more than an order magnitude larger than the treatment group in either sample (mitigating the first set of concerns), we have a preference for the latter approach but report both for completeness.

Timing of reforms: Capital city reforms occur for a variety of circumstances and policy-makers may pursue a range of political and economic objectives (e.g. granting regional autonomy, avoiding conflict, improving service delivery, and more). Our focus on first-order units implies that these reforms are seldom carried out without the influence of national politics. This helps identification in our context, as it makes the timing of reforms less predictable and, therefore, pre-trends at the city level less likely.

Anecdotal evidence supports this conjecture. The 2010 restructuring of Kenya's provinces illustrates this well. A constitutional reform process was started following the post election violence in 2008. A key objective of this process was to reduce ethnic tensions in the country which was, at least in part, to be achieved by a devolution of power and territorial reform (see Bluhm et al., 2021, for a study of the effects of this reform

[15]We match administrative cities to an urban cluster if the centroid of the administrative city is within 3 km of the urban cluster or the names are identical. Note that some clusters contain several administrative cities so that the fraction of matched cities is somewhat higher than implied by the table.

[16]We discuss selection issues in Online Appendix D. We show that gaining capital status over the period from 1990 to 2015 predicts inclusion in the 2015 sample (see Table D-1).

[17]A growing literature in applied econometrics highlights the weaknesses of using the regression framework to estimate panel difference-in-differences designs. The main concern is that the fixed effect estimator uses all possible 2-by-2 comparisons to construct the variance-weighted estimate, including comparisons where treated units are used as controls (Borusyak and Jaravel, 2017). Having a large control group, as in our case, essentially solves this issue by placing next to no weight on these comparisons. Abraham and Sun (2018) study the corresponding event-study design and show that it does not suffer from this problem when the pattern of treatment effects is the same for all cohorts.

on ethnic voting). Up on to this point, Kenya was organized into eight large provinces. The first attempt at constitutional reform had failed in 2005 and even in January 2010 "it appeared that the political disputes which had undermined previous attempts at constitutional reform were likely to resurface" (Kramon and Posner, 2011, p. 93). There were lengthy debates about how many tiers and counties the new administrative structure should have, which were finally settled when the parliamentary committee "agreed to the least controversial position: a two-tier system with 47 county governments whose boundaries would be congruent to the country's pre-1992 regions" in April 2010 (Kramon and Posner, 2011, p. 94). The new constitution was adopted by national referendum in August 2010, leaving little scope for anticipation effects. Even when splitting of regions is driven by local demands, such as in neighboring Uganda, the national parliament is usually involved in approving them, so that the timing of splits becomes difficult to predict. Uganda decentralized its administrative structure from 34 regions in 1990 to 127 by 2018. The reforms were carried out in several waves. While most splits were eventually approved, some were denied by the parliament (Grossman and Lewis, 2014). National involvement in these types of reforms is not limited to Africa. Indonesia created eight new provinces and more than 150 new second-tier regions after the fall of Suharto in 1998. Splitting required parliamentary and presidential approval. India's national parliament created three new states in 2000. There were local movements in favor of these states for cultural and economic considerations, but previous attempts to carve out new territories had failed repeatedly before their final adoption (Agarwal, 2017).

Although random timing of the reforms is appealing, it is not necessary for identification and likely to be violated in several settings.[18] The parallel trends assumption needed for our strategy to work is substantially weaker. On top of static selection, it allows for time-varying omitted variables to affect the treatment and control group, provided that these two are affected equally. We consider this assumption particularly plausible in the sample of cities in reformed regions with initial-region-by-year fixed effects, as all cities in those regions are indirectly affected by the same territorial reform.

4. Results

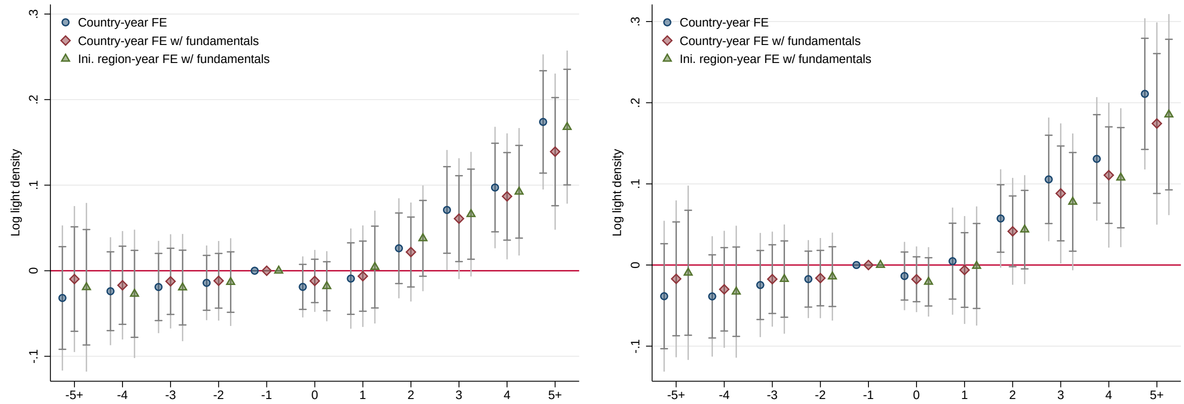
Baseline results: Figure V reports the results from our main event-study specification based on two different samples. Panel A plots estimates based on a specification using all cities and country-year FEs (dots). This is our baseline estimate for the larger sample

[18]Identification is straightforward if the timing of the intervention is exogenous to city level characteristics (conditional on the fixed effects and observed covariates). If the pre-reform time indexes can be swapped, there cannot be any pre-trends. Figure A-1 in Online Appendix A shows that the timing of capital city reforms is difficult to predict, at least with time-invariant initial city characteristics and especially once we focus only on within country variation.

where the control group consists of all other (non-capital) cities in the same country. The diamonds report results for a specification that purges the time-varying effects of the fundamentals, and the triangles show estimates obtained by adding initial-region-by-year fixed effects. Panel B repeats this set-up for the sample of cities in reformed regions.

Figure V
Capital cities and light intensity: Event study

(A) All cities (B) Cities in reformed regions



Notes: The figure illustrates event-study results from fixed effects regressions of the log of light intensity per square kilometer on the binned sequence of treatment change dummies defined in the text. Dots represent point estimates from a regression with city and county-year fixed effects, diamonds represent specifications with additional controls for locational fundamental, and triangles represent specifications with initial-region-by-year fixed effects in addition. Panel A shows estimates for all cities. Panel B shows estimates for cities in reformed regions. All regressions include city fixed effects. 95% confidence intervals based on standard clustered on initial provinces are provided by the gray error bars. The orange error bars indicate 95% sup-t bootstrap confidence bands with block sampling over initial provinces (Montiel Olea and Plagborg-Møller, 2019).

The estimates and their confidence bands strongly support the notion that gaining the status as a capital is exogenous to pre-reform changes in the economic activity of treated cities. The pre-trends are essentially flat. There are no systematic differences in city light intensities prior to a change in capital status and the pointwise confidence intervals and the sup-t bands rule out a wide range of positive anticipation effects. We view this as strong evidence for the validity of our identification strategy. Any unobserved confounding factor would have to very closely mimic the timing implied by this observed pattern. Moreover, since both the full sample and the sample of cities in reformed regions reveal a very similar pattern, we find it unlikely that pre-testing bias is a serious concern in our application (Roth, 2018). In all dynamic specifications, we do not detect a spike in activity in the first year. This is intuitive, in the sense that constructing new buildings, an influx of public and private investment, moving an administration, or a migratory response all take time. Treatment may also occur towards the end of the calendar year, leaving little scope for an instantaneous effect. In the sample of cities in reformed regions (panel B), light intensity begins to increase by approximately 4.5 to 5.9% two years after a city becomes a subnational capital and increases to 18.3 to 20.4% five years after the reform and beyond. The triangles correspond to our preferred specification since the control group now only consists out of non-capital cities in reformed regions and all annual shocks specific to those regions are absorbed.

We investigate how the size of our primary event-study window influences the estimate of medium-run effect (endpoint bins) and compares to the difference-in-differences version in Online Appendix A. Consistent with the gradual rise of the estimated effects, we find that the estimate of the medium-run effect is closer to 20% for longer event windows (estimated on fewer treatments) but its confidence interval always contains the difference-in-differences estimate (see Figure A-2), which utilizes only switchers during the 1992 to 2013 period. The difference-in-differences is around 8.9–14.9%, depending on the specification (see Table A-2). It is lower than the medium-run estimate from the event study, as it averages over the first years and all subsequent post-treatment periods.

In summary, our main results suggest that cities which become capitals grow substantially faster than their peers in the subsequent period. There is a build up in economic activity during the first 5 years after which we observe a medium-term increase around 20%, with some variation across specifications. This is up to half of the within city standard deviation in light intensity. To put this in perspective, consider the results in Storeygard (2016) where an African city which is further away from the primate city than the median city loses about 12% of its economic activity when the oil price is high.[19] For the reminder of the paper, we report the difference-in-differences estimates in the main text and relegate the corresponding event-study plots to Online Appendix A.

We also find evidence that this political premium does not persist, unlike the economic advantages documented in Bleakley and Lin (2012). Online Appendix E provides a detailed analysis of cities that lose their status as a regional capital (usually during a merger of first-order regions). Such an analysis necessitates a different control group and a somewhat different design, as the relevant comparison group are now cities that remain capitals over the entire period. Our results suggest that former capitals lose economic activity, roughly mirroring the initial increase in light intensity, starting around four years after a loss in status.

Agglomerations and city peripheries: Urban sprawl is an important component of city growth and a function of geography, policies, and the economics structure of the wider area surrounding a city (Burchfield et al., 2006). Most cities grew substantially at

[19]The effect is also larger than the effect of funneling public funds to specific regions documented in the literature on political favoritism (although the level of analysis is not the same). Hodler and Raschky (2014) estimate that being the birth-region of a national leader increases nighttime light intensity by about 3.9%, while De Luca et al. (2018) estimate an increase of 7%-10% in the ethnic homeland of a leader who is currently in office.

the extensive margin over the period from 1990 to 2015. The average city expanded its area by almost 50% and the area of capital cities grew faster than that of other cities in the same initial region.[20] Unfortunately, we do not observe a city's urban extent in every year so that we cannot calculate detailed measures of sprawl. Instead, our baseline results focus on the universe of cities detected in 1990 and treat their urban extent as fixed (to represent 'the core'). This avoids potential endogeneity issues in the selection of cities and allows us to focus on increases in density but comes at the cost of neglecting initially less densely developed areas of cities. In Table II we loosen this assumption by accounting for cities that ultimately merge into a single larger agglomeration and include areas which were initially in the periphery. This allows us to study changes in the light intensity of the overall agglomeration and changes outside of the 1990s core of each city.

TABLE II

Larger agglomerations and city fringe: Difference-in-differences

	Dependent Variable: ln LIGHTS$_{cit}$					
	All Cities			Reformed Regions		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Growth of the larger agglomeration						
CAPITAL	0.0769	0.0697	0.0803	0.1048	0.0901	0.0930
	(0.0280)	(0.0274)	(0.0277)	(0.0287)	(0.0281)	(0.0277)
Panel B. Growth in the periphery of the city						
CAPITAL	0.0744	0.0593	0.0833	0.1056	0.0848	0.0993
	(0.0345)	(0.0333)	(0.0337)	(0.0353)	(0.0353)	(0.0345)
N	13410	13410	13410	4517	4517	4517
$N \times \bar{T}$	275205	275205	275205	87591	87591	87591
Fundamentals	–	✓	✓	–	✓	✓
Agglomeration FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	–	✓	✓	–
Ini. Region-Year FE	–	–	✓	–	–	✓

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status. Panel A reports results based on the larger agglomeration (the envelope over 1990 and 2015 of the urban clusters detected in 1990). Panel B reports the results for the fringe (areas the urban clusters detected in 1990 that meet the detection threshold by 2015). Standard errors clustered on initial regions are provided in parentheses.

Capital cities experience faster overall growth than non-capital cities and their peripheries grow faster as well. Panel A of Table II reports our six specifications at the level of agglomerations, that is, cities detected in 1990 including the parts that only pass our density threshold of 1,500 people or 50% built-up per sq. km by 2015. The estimates tend to be smaller than our difference-in-differences estimates by about 2–4 percentage

[20] Table A-5 in Online Appendix A shows that capital cities expanded their average footprint by about 9.8% to 13.8% more than non-capital cities over the period from 1990 to 2015.

points but are otherwise similar. Panel B reports the same set of specifications but focuses on light growth in the periphery, that is, only the area of each agglomeration that is initially less dense and subsequently passes the population threshold. We find that new developments around capital cities are growing at a pace comparable to the larger agglomeration but somewhat slower than the core. The results are statistically significant at conventional levels in all columns, apart from column 2 where the effect is less precisely estimated but within a standard error of other estimates. Taken together, this strongly suggests that both increasing density in the center and urban sprawl are associated with gaining the status as a capital city.

Spillovers to nearby cities and SUTVA violations: An important question in our context is whether new subnational cities draw economic activity from their immediate surroundings or whether creating capitals benefits more cities in a region. Moreover, the presence of any such negative or positive spillovers would violate the stable unit treatment value assumption (SUTVA) inherent in our approach, which requires that the treatment status of any one unit (capital cities) does not affect the treatment status of other units (non-capital cities). Our preferred specification is vulnerable to this problem as it compares the status of cities that are—by virtue of being located in the the same initial region—relatively close-by. We can view this as omitted variables problem. If there are positive spillovers to nearby cities, then our baseline results are attenuated and *vice versa.* Provided that spatial spillovers have a monotonic pattern in distance and some cities are unaffected, it suffices to include dummies capturing the proximity to treated cities and their change in treatment status (see e.g., Asher et al., 2019, for a similar approach).

Figure VI explores this possibility by adding indicators for the (time-varying) distance to the nearest capital city in the country, where each indicator captures agglomerations in a 25 km ring around the treated agglomeration, starting from bigger than 0 km and going up to 150 km.[21] A considerable advantage of this specification over our baseline results is that it allows us to account for capital cities which we did not match to an urban cluster in 1990. Even if the urban extent of some capitals is not observed, the distance of all other urban clusters to these "unobserved" capitals with known coordinates is straightforward to compute, so that we indirectly capture the entire universe of capital cities, including *all* changes in status of nearby cities.

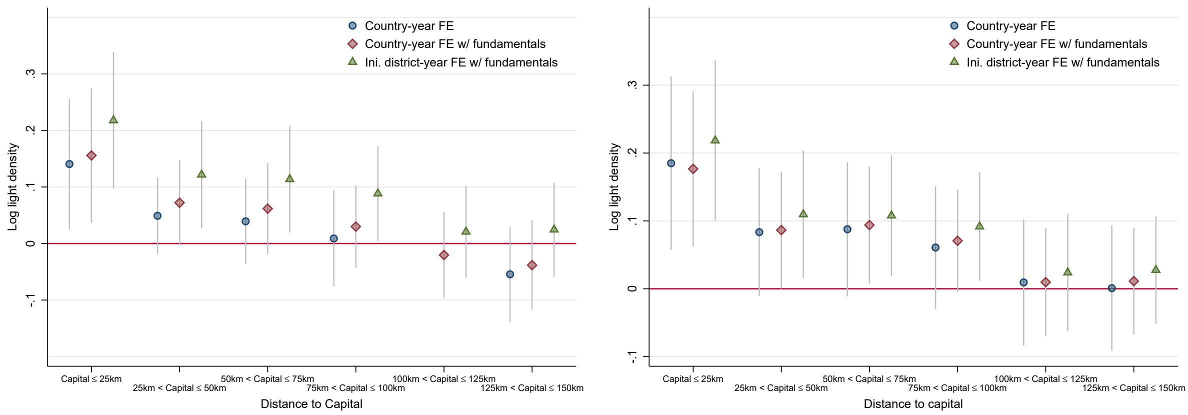
The county-year fixed effects specification (dots in panel A) in Figure VI provides the least evidence of spillovers. It uses all non-capital agglomerations in the same country as a control, many of which are located in regions far apart from treated agglomerations. The evidence in favor of spillovers becomes stronger once we introduce initial-region-by-year fixed effects (diamonds) or limited the sample to cities in reformed regions, as in panel

[21] We use 150 km as an upper bound for spillovers, but the results are not sensitive to this choice.

FIGURE VI

Spillovers to nearby agglomerations: Difference-in-differences

(A) All cities | (B) Cities in reformed regions



Notes: The figure reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status as well as several spillover dummies for different distance intervals (panel A for all agglomerations, panel B for agglomerations within reformed areas). Dots represent point estimates from a regression with city and county-year fixed effects, diamonds represent specifications with additional controls for locational fundamental, and triangles represent specifications with initial-region-by-year fixed effects in addition. Panel A shows estimates for all cities. Panel B shows estimates for cities in reformed regions. All regressions include city fixed effects. 95% confidence intervals based on standard clustered on initial provinces are provided by the gray error bars.

B. Depending on the specification, we find evidence of positive externalities affecting agglomerations which are up to 75 to 100 km away from a new capital. All specifications suggest a declining pattern of positive treatment effects, where satellite towns close to the new capital grow substantially faster but this effect disappears after a distance of 75 to 100 km. Accounting for these indirect effects increases the estimate for the capital itself, particularly in the sample of cities in reformed regions. We now estimate a treatment effect between 22.8% and 25.9%. This spatial pattern has an important policy implication. Rather just drawing activity and population from its immediate surroundings, creating new capital cities appears to benefit more cities in the reformed region.

Alternative control groups: A potential concern is that our baseline results include a variety of cities in the control group, many of which are unlikely to ever become subnational capitals. In fact, future capitals are usually among the biggest and brightest cities in the pre-reform region. Out of the 221 cities which became capitals during the period from 1992 to 2013, the median city was ranked second in terms of its 1990 population in the initial region, while the city at 90th percentile was ranked 10th.[22] Static selection is not a concern in difference-in-differences designs. However, in spite of finding no evidence in favor of pre-trends, our baseline results could still include cities in the

[22]See the discussion of correlates of capital locations in Online Appendix C.

control group that are on fundamentally different growth paths than cities which later become subnational capitals.

Table III
Different control groups: Matched difference-in-differences

	Dependent Variable: $\ln \text{LIGHTS}_{cit}$					
	Light intensity in 1992			Population in 1990		
	Distance from treated city					
	Any	> 50 km	> 75 km	Any	> 50 km	> 75 km
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Control cities within ± 2 ranks of treated cities in initial region						
Capital	0.1106	0.0779	0.0887	0.1005	0.0921	0.0885
	(0.0272)	(0.0291)	(0.0326)	(0.0279)	(0.0311)	(0.0347)
F-test pre-trends (p-val.)	0.219	0.350	0.470	0.415	0.204	0.112
N	639	422	360	632	443	377
$N \times \bar{T}$	13886	9161	7826	13745	9623	8192
Panel B. Control cities within ± 3 ranks of treated cities in initial region						
Capital	0.1185	0.0882	0.0939	0.1021	0.1014	0.0889
	(0.0273)	(0.0282)	(0.0319)	(0.0281)	(0.0311)	(0.0353)
F-test pre-trends (p-val.)	0.300	0.436	0.250	0.513	0.537	0.383
N	801	506	417	783	528	439
$N \times \bar{T}$	17406	10979	9055	17022	11469	9542
Panel C. Control cities within ± 4 ranks of treated cities in initial region						
Capital	0.1197	0.0862	0.0924	0.1012	0.1028	0.0929
	(0.0273)	(0.0274)	(0.0315)	(0.0270)	(0.0308)	(0.0349)
F-test pre-trends (p-val.)	0.209	0.484	0.399	0.433	0.497	0.340
N	932	574	464	904	593	477
$N \times \bar{T}$	20249	12449	10078	19644	12873	10366

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status. Panels A to C match treated cities to a varying number of control cities on the basis of their rank in terms of light intensity or population within the initial region. All regressions include city fixed effects, initial-region by-year fixed effects, and time-varying coefficients on the fundamentals. We report an F-test for pre-trends tests for the null hypothesis that all leading terms in the equivalent event-study specification are jointly zero. Standard errors clustered on initial regions are provided in parentheses.

We use a simple form of nearest-neighbor matching to assess whether the definition of the control group influences our results. We rank all cities in the initial region according to their initial light intensity or population in 1990 and designate all cities that are within k-ranks of the treated city as potential controls, where k ranges from 2 to 4 positions.[23] This creates a trade-off. While selecting among a subset of comparable cities in the initial region makes it more likely that these are good controls, positive spillovers imply that

[23]This approach is similar to Becker et al. (2020), who construct controls for Bonn—the temporary capital of the Federal Republic of Germany from 1949 until 1990—using cities ranked 20 places below and above Bonn in terms of their 1939 population.

nearby cities are affected by the change in status of the capital city and therefore, as we just showed, represent a treatment group of their own.

Table III reports a range of results addressing these issues, all of which are based on the the most restrictive specification with initial-region-by-year fixed effects. By definition, we are now only using cities in reformed regions. Reassuringly, every single estimate indicates a positive and significant effect of capital city status on city growth. We find effects in columns 1 and 4 that are close to our difference-in-differences results no matter if we use initial light intensity or estimates of city population in 1990 to define the control group, or if we consider only two, three or four similarly ranked cities. We also conduct simple omnibus tests for pre-trends using the equivalent event-study specification for each of these samples. In every case, we fail to reject the null hypothesis that the coefficients on all leading terms are jointly zero by a wide margin. The remaining columns remove observations whose minimum distance to a capital city is smaller than 50 or 75 km to mitigate the concerns about potential SUTVA violations.[24] There is some indication that the effects could be smaller once cities affected by spillovers are excluded. However, all estimates are well within two standard errors of one another and based on a different specification with substantially less variation in distance to treated cities than the more comprehensive spillover analysis presented above.[25]

Other robustness checks: We conducted a range of other checks verifying our analysis. We only briefly summarize their results here and report the corresponding tables in Online Appendix A. Our baseline estimates are robust to accounting for spatial autocorrelation (see Figure A-3) or using different versions of the light data, provided that there is some adjustment for bottom-coding (see Table A-3). In fact, our bottom correction and the non-filtered series from NOAA produce almost identical results. Correcting for top-coding then has a similar effect in terms of increasing the estimated magnitudes by another 2–3 percentage points. The estimated effects are robust to considering only cities that have a substantially larger initial population in 1990 and rise somewhat with initial city size (see Table A-4). Finally, none of these perturbations results in significant pre-treatment trends.

[24]We disregard the time variation in distances to capital cities in this table to construct a conservative test which excludes all cities which were *ever* located within 50 or 75 km of a capital city. Results using time-varying distances are similar.

[25]In Table A-6 in Online Appendix A, we repeat this matching exercise using all similarly ranked cities in the country as controls in a specification with country-year fixed effects (to not limit the comparison to the same initial region). The results are qualitatively similar in these samples as well.

5. Heterogeneity

We now turn to three sources of heterogeneity—economic fundamentals, the level of development, and economies of scale—to better understand which combination of factors is conducive to growth in cities which become capitals.

Locational fundamentals: It is an open question in the literature whether political factors, such as designating subnational capitals, can substitute for the lack of good economic and geographic fundamentals or whether they, at best, complement these fundamentals. Long-run studies typically find that both fundamentals and path dependence are essential determinants of city locations. (see e.g. Davis and Weinstein, 2002; Bleakley and Lin, 2012; Michaels and Rauch, 2018). The allocation of subnational capitals and public investments may have large effects in the hinterland, i.e., contexts with fewer local advantages or locations primarily suited for agricultural production, and induce little change in areas where (trade-related) fundamentals are strong to begin with, or vice versa. Recent evidence suggests that the growth potential of secondary cities in the agricultural hinterland might be limited. Gollin et al. (2016), for example, highlight that recent urbanization in resource-depended economies has been concentrated in low productivity "consumption cities." Urbanization in low productivity cities may be exacerbated by elevating cities to capitals in less favorable locations.[26]

Table IV tackles the question of fundamentals in our setting. We present two sets of results. Panel A shows results from regressions where we group our large set of potentially relevant fundamentals into aggregate indexes and reduce the underlying dimensionality by extracting the first principal component from three groups of fundamentals (internal trade, external trade, agriculture). Each column takes a group of fundamentals and interacts it with the treatment status. Panel B repeats this analysis using a representative fundamental from each group, as composite indexes are difficult to interpret. All variables are standardized to have mean zero and unit variance to facilitate comparisons across different specifications. We initially omit the time-varying coefficients on the control fundamentals in this specification, but this omission hardly affects the qualitative results. All heterogeneity analyses are based on the full difference-in-differences specification for cities in reformed regions without spillovers.

Columns 1 to 3 in panel A show individual regressions where the capital city status is interacted with an index of how easy it is to trade internally, trade externally, or produce agricultural goods around the location of the city. Column 4 and 5 include all

[26] Other results in the literature can be viewed through this lens. For example, Becker et al. (2020) document that Bonn's temporary status as the national capital of (West) Germany created little development apart from direct public employment. The city narrowly won its status over Frankfurt, which had considerably stronger fundamentals in the 1940s, and was always considered a temporary component of the division of Germany.

TABLE IV

Heterogeneity in fundamentals: Difference-in-differences

	Dependent Variable: $\ln \text{LIGHTS}_{cit}$				
	(1)	(2)	(3)	(4)	(5)
Panel A. Principal components for each group of fundamentals					
CAPITAL	0.1919	0.1394	0.1405	0.1885	0.1393
	(0.0412)	(0.0308)	(0.0289)	(0.0390)	(0.0426)
CAPITAL × INT. TRADE	0.0979			0.0902	0.0516
	(0.0391)			(0.0384)	(0.0369)
CAPITAL × EXT. TRADE		0.0036		-0.0010	0.0003
		(0.0152)		(0.0154)	(0.0147)
CAPITAL × AGRICULTURE			-0.0582	-0.0531	-0.0512
			(0.0168)	(0.0158)	(0.0157)
Panel B. Selected variables for each group of fundamentals					
CAPITAL	0.2655	0.1398	0.1371	0.2713	0.2042
	(0.0472)	(0.0304)	(0.0293)	(0.0443)	(0.0495)
CAPITAL × MARKET ACCESS	0.1309			0.1390	0.0991
	(0.0336)			(0.0313)	(0.0298)
CAPITAL × DIST. TO COAST		-0.0011		0.0247	0.0110
		(0.0257)		(0.0236)	(0.0224)
CAPITAL × WHEAT SUITABILITY			-0.0548	-0.0564	-0.0556
			(0.0206)	(0.0187)	(0.0186)
Fundamentals	–	–	–	–	✓
N	8418	8418	8418	8418	8418
$N \times \bar{T}$	184304	184304	184304	184304	184304

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status and interactions of the status with a particular fundamental. The interactions of the capital city status with some other variable $\tilde{z}$ are standardized such that $\tilde{z} \equiv (z - \bar{z})/\sigma_z$. All first principal components are scaled to represent better suitability. All regressions include city fixed effects and initial-region-by-year fixed effects. Standard errors clustered on initial provinces are provided in parentheses.

variables at the same time and add the time-varying coefficients on the fundamentals. In nearly all specifications, we find strong evidence of complementarity between gaining the political advantage of a capital city and economic fundamentals. A two standard deviation decrease in the index of internal trade offsets the positive capital city effect in column 4, although this effect is no longer significant in column 5. External trade integration appears to matter little for the relative growth rates of subnational capitals, whereas cities which become capitals in agricultural locations attract considerably less activity than those located in other locations. While only suggestive, these results are in line with a reduced importance of agriculture for city locations or productivity and a greater importance of connectivity-related fundamentals today (Henderson et al., 2018), as well as with a larger role of internal market access, rather than external market access, for city growth in Sub-Saharan Africa (Jedwab and Storeygard, 2020).

Panel B unpacks these three groups. Column 1 interacts the treatment status with a city's internal market access (to other cities in the country) in 1990. Here we observe a strong interaction effect. A city which becomes a capital in a location with a level of market access that is a standard deviation above the mean experiences an additional increase in light intensity of 13.9%. Given that most of our reforms occur in developing countries, this finding echos Brülhart et al. (2020), who show that market access remains a strong determinant of regional productivity in developing countries, even as its importance declines in developed economies. Column 2 uses distance to coast as a measure of external market access. The coefficient points in the expected direction, but the estimated effect is small and insignificant. Column 3 uses wheat suitability as a proxy for locations in the agricultural hinterland. Mirroring the results from above, it shows that greater suitability for agriculture is negatively correlated with the growth of capital cities.[27]

Early and late developers: Next, we turn to the difference between developed and emerging economies to explore whether redesigning the territorial structure has different effects on the spatial equilibrium when a country agglomerated early or when it is urbanizing until today. Creating new capital cities could have little to no effects on migration in well-established urban networks with limited population pressures, whereas similar interventions in developing countries with growing populations and ongoing structural transformation could lead to a lasting shift in the location of activity. To test this conjecture and avoid constructing potentially endogenous sample splits, we rely on the country-level classification into early and late developing countries provided by Henderson et al. (2018).[28]

The results in Table V show a clear pattern. No matter if we interact the capital city status with an indicator for late development according to education, urbanization or GDP in 1950, we always find that the effects are driven by late developers. The results for early developers are small and insignificant at conventional levels in nearly all samples apart from column 1, whereas the effect for late developers is remarkably stable across the different sample splits. The effect for late developing countries ranges from about 12.5% to 18.6%, depending on whether we account for time varying fundamentals or not, but varies little across the different splitting variables.[29]

[27] Figure A-5 in Online Appendix A shows that the event-study estimates of internal trade and market access rise in line with the baseline capital city effect, while the negative effect of agricultural fundamentals only starts to appear in the medium run and is barely significant. Here too, the sub-t confidence intervals highlight that pre-trends are an unlikely explanation.

[28] Henderson et al. (2018) use a simple algorithm to let the data decide at which point the unexplained variance over the 'late' and 'early' samples is minimized. Their dependent variable is a contemporary cross-section of light intensity in a grid cell, while they define 'late' or 'early' according to urbanization, schooling and GDP per capita in 1950.

[29] Figure A-6 in Online Appendix A shows the corresponding event studies for each column of the table. No matter how the sample is split, we observe flat pre-trend and a steep rise after the reform

Table V
Late versus early developing countries: Difference-in-differences

	Dependent Variable: $\ln$ Lights$_{cit}$					
	Late developer according to					
	Education 1950		Urbanization 1950		GDP per capita 1950	
	(1)	(2)	(3)	(4)	(5)	(6)
Capital × Early	0.0525	0.0343	0.0786	0.0648	0.0256	0.0136
	(0.0210)	(0.0244)	(0.0559)	(0.0572)	(0.0277)	(0.0338)
Capital × Late	0.1702	0.1425	0.1489	0.1179	0.1541	0.1227
	(0.0466)	(0.0466)	(0.0338)	(0.0351)	(0.0345)	(0.0362)
Fundamentals	–	✓	–	✓	–	✓
City FE	✓	✓	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓	✓	✓
N	7259	7259	8519	8519	8251	8251
$N \times \bar{T}$	158847	158847	186511	186511	180806	180806

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status in early and late developing countries defined according to Henderson et al. (2018). All regressions include city fixed effects and initial-region-by-year fixed effects. Standard errors clustered on initial provinces are provided in parentheses.

Online Appendix A shows that these late developer results can not be fully explained by differences in political systems. Expanding on Ades and Glaeser (1995), we find that capitals in autocracies grow faster compared to their democratic counterparts. Yet, the size of the effect is much smaller than the early vs. late distinction and only present in the full sample of cities (see Table A-8).

Economies of scale and the size of subnational units: Our results thus far suggest that creating more subnational capitals and, hence, more first-order units benefits these cities and regions irrespective of their size. It is unlikely that subnational capital cities which rule over ever smaller territories would stand to benefit in the same way, as those who are the capitals of more populated regions.[30] The literature on the optimal size of local jurisdictions highlights a number of relevant trade-offs, ranging from scale economies, over externalities in the provision of public goods, to preference homogeneity (Oates, 1972; Alesina et al., 2004; Coate and Knight, 2007). While our empirical framework is not suited to directly address most of these questions, we can examine if the positive effects of gaining capital city status documented here depend on the scale of the jurisdiction the city administers.

Table VI reports a series of regressions in which we interact the capital city status

in the sample of late developers. The results for early developers follow no systematic, or statistically significant, pattern.

[30] In a different but related context, Grossman and Lewis (2014) argue that Uganda has become so heavily decentralized that the intergovernmental bargaining power of a single first-order unit has been substantially weakened as a result.

Table VI
Economies of scale: Difference-in-differences

	Dependent Variable: $\ln$ Lights$_{cit}$					
	Pop (region)		Urban pop (region)		No. cities (region)	
	(1)	(2)	(3)	(4)	(5)	(6)
Capital	0.1786	0.1350	0.2703	0.1830	0.2106	0.1983
	(0.0461)	(0.0441)	(0.0685)	(0.0699)	(0.0416)	(0.0441)
Capital $\times$ Scale	0.0784	0.0674	0.1426	0.1024	0.1039	0.1151
	(0.0327)	(0.0301)	(0.0416)	(0.0379)	(0.0299)	(0.0300)
Fundamentals	–	✓	–	✓	–	✓
City FE	✓	✓	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓	✓	✓
N	8519	8519	8519	8519	8519	8519
$N \times \bar{T}$	186511	186511	186511	186511	186511	186511

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status and interactions of the status with a the log of regional population, log of urban population in regions, and the log number of cities within the region. All regressions include the base term of scale variable. The interactions of the capital city status with some other variable measuring the scale of the new region ($\tilde{z}$) are standardized such that $\tilde{z} \equiv (z - \bar{z})/\sigma_z$. Standard errors clustered on initial provinces are provided in parentheses.

with a variable measuring the scale of the region or province. Columns 1 and 2 use the regional population in 1990. Columns 3 and 4 use the urban population of the region in 1990, which we take as a proxy for the size of the non-agricultural economy, while 5 and 6 take a count of the number of cities in the resulting region. The measures of scale are standardized and time-varying (they change whenever a region is reformed). No matter in which way we specify this interaction with scale, we find evidence suggesting a trade-off: new capital cities benefit only when they rule over larger regions. Since the reforms in our sample are splits of larger provinces, this implies that creating new capitals of increasingly smaller regions weakens the effect of this reform on economic activity. A two standard deviation decrease in scale all but wipes out the capital city effect in all specifications. This could occur through a variety of channels, such as limited public investments in smaller regions or a small migratory response when the population of the new region is smaller.[31]

We briefly examine the role of preference heterogeneity in Online Appendix A, where we use ethnic diversity in the initial region as a proxy for preference heterogeneity (Table A-7).[32] We find no evidence in favor of the hypothesis that capital cities grow

[31] Figure A-7 in Online Appendix A shows the corresponding event studies for each column of the table. We continue to observe an upward trend in the estimates for the capital city effect, at average levels of the scaling variable, and also observe an increase after treatment in the estimated interaction effects. Pre-trends appear to be flat, with the exception of the estimate two periods before treatment when the scaling variable is the urban population in the initial region.

[32] Similar to Eberle et al. (2020), we measure the ethno-linguistic fractionalization of initial regions using GHSL population data for 1975 and an algorithm developed by Desmet et al. (2020) which

at different speeds when they are located in more ethnically homogeneous (or diverse) regions. While this finding does not corroborate recent research suggesting that initial diversity inhibits agglomeration as a result of diverse groups spreading over smaller cities (Eberle et al., 2020), it only shows that such an effect is unlikely to run through medium-run changes in the density of capital cities.

Taken together, our estimates imply that there is strong heterogeneity in the medium-run effect of gaining capital city status. We interpret this as evidence that "territorial politics" can have a substantial effect on the location of economic activity and urbanization in developing countries, but that this effect varies by locational fundamentals and the territorial design of the administrative structure.

6. Mechanisms

There are a range of mechanisms which could explain why capital cities attract more economic activity. We first examine changes in housing supply and population at the city level to investigate whether new capital cities are, in fact, becoming denser than non-capital cities. Second, we use individual level data from the Demographic and Health Surveys (DHS) to test if residents of capitals are better off than residents in comparably dense cities. Third, we utilize city-level data on international financial flows to examine whether capital cities mainly receive public funds in the form of development projects or also an influx of private investment from abroad.

Housing supply and population growth: Ideally, we would like to have city-level measures for the housing stock, housing prices, and annual data on city-level population. As these are not available for our global sample of cities, we construct proxies based on remotely-sensed and census-derived data.

Table VII examines changes in urban land cover within a city.[33] We derive three frequently used spectral indices of land cover by creating annual composites from daily images taken by the Landsat 5 and Landsat 7 satellites over the entire period from 1987 to 2018. The Normalized Difference Built-up Index (NDBI), the Urban Index (UI) and the Normalized Difference Vegetation Index (NDVI) are well-established spectral indices of urban and non-urban land cover, which are typically used as inputs for more advanced land cover classifications (e.g. the USGS data used by Burchfield et al., 2006). Since we already know the urban extent of each city in 1990, taking the average value of each index within each city and year allows us to track annual changes in urban land cover within the city core, just as in our main specification. Columns 1 to 6 show that each

distributes data from the World Language Mapping System (WLMS)—the vector version of the Ethnologue project—on a 5×5 km grid.

[33] Figure A-8 in Online Appendix A reports the corresponding event studies for the preferred specification with time-varying effects of the static fundamentals.

Table VII
Built-up area within city: Difference-in-differences

	Dependent Variable: Built-up area within city					
	NDBI		UI		NDVI	
	(1)	(2)	(3)	(4)	(5)	(6)
Capital	0.8458	0.6760	1.2785	0.9415	-0.9127	-0.5415
	(0.2201)	(0.2295)	(0.2947)	(0.3148)	(0.2443)	(0.2653)
Fundamentals	–	✓	–	✓	–	✓
City FE	✓	✓	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓	✓	✓
N	8519	8519	8519	8519	8519	8519
$N \times \bar{T}$	180572	180572	180572	180572	180572	180572

Notes: The table reports results from fixed effects regressions of the Normalized Difference Built-Up Index (NDBI), Urban Index (UI), and the Normalized Difference Vegetation Index (NDVI) on capital city status. All coefficients are scaled by 100 for exposition. Standard errors clustered on initial regions are provided in parentheses.

of these measures either indicates a significant increase in urban land cover (due to new housing, businesses or infrastructure) or a corresponding decrease in vegetation in case of the NDVI when a city gains capital status. Depending on the sample, the estimated effect sizes are about 10–15% of the typical within-city standard deviation for these indices.

Online Appendix A shows that we also observe increases in built-up in the larger agglomerations, including areas outside of the 1990s core (Table A-9), while our results using nighttime lights remain similar when we control for annual built-up (Table A-10). These findings add two relevant insights. First, part of the increase in economic activity is directly reflected in housing supply and other physical infrastructure. Second, our main results appear to pick up changes in density and economic activity that go beyond additional buildings and infrastructure.

Our next set of results focuses on the population response. Here we compute city-level estimates based on the GHSL population grids at three points in time (1990, 2000 and 2015). Due to the limited time period and measurement errors inherent in the spatial and temporal interpolation of census data, we can no longer specify regressions that mirror our main identification strategy. Instead, we ask how much faster the population density of capital cities grows relative to that of other cities in the same initial region (and with similar fundamentals).

Table VIII regresses medium-run differences in city population (within the initial boundaries) on the fraction of years a city was a capital in three different periods. The results show that cities which were capitals for a longer period during the respective window experience stronger population growth. The average capital city in the sample of reformed regions has an initial population of about 740,000 people in 1990, while cities that become capitals start out with a population around 200,000. There is some

TABLE VIII
Population response: Long differences

	Dependent Variable: Δ ln POP_{ci}					
	1990–2000		2000–2015		1990–2015	
	(1)	(2)	(3)	(4)	(5)	(6)
CAPITAL	0.1631	0.1347	0.1887	0.1513	0.3655	0.2978
	(0.0139)	(0.0150)	(0.0186)	(0.0230)	(0.0312)	(0.0371)
Fundamentals	–	✓	–	✓	–	✓
Initial-Region FE	✓	✓	✓	✓	✓	✓
Cities	8535	8535	8535	8535	8535	8535

Notes: The table reports results from long difference regressions of the change in log population of a city over different epochs on the fraction of years in which a city is a capital. Standard errors clustered on initial regions are provided in parentheses.

variation across time-periods and specifications but, on average, subnational capitals appear to grow slightly more than a percentage point per year faster than other cities.[34] Column 5 and 6 shows that the population density of capital cities which are capitals for the full period is around 34.7–41.1% higher than that of non-capital cities in the same initial region by the end of the 1990–2015 period, depending on whether we adjust for economic fundamentals or not.[35] We interpret this as indirect evidence that short and medium term increases in light intensity at the city level actually translate into an influx of population from other locations.[36]

Urban (dis)amenities: The logic of a spatial equilibrium implies that if households could be systematically better off in one location than in another, then they would move to the better location until (urban) disamenities take over and utility levels are equalized. Channeling government services and investments to particular cities can influence which locations are considered attractive. Recent empirical evidence even suggests that developing country cities offer higher wages and better amenities than rural locations (Henderson and Turner, 2020; Gollin et al., 2021).[37] This evidence is does not exploit the type of between city variation we are interested in here but gives us a framework to analyze whether residents of capital cities are better off than residents of comparably dense cities without political status. Following Henderson et al. (2020), we

[34]Measurement error permeates these population estimates but the mismeasured variable is on left hand side. Hence, the interpolation error is unlikely to be related to our treatment.

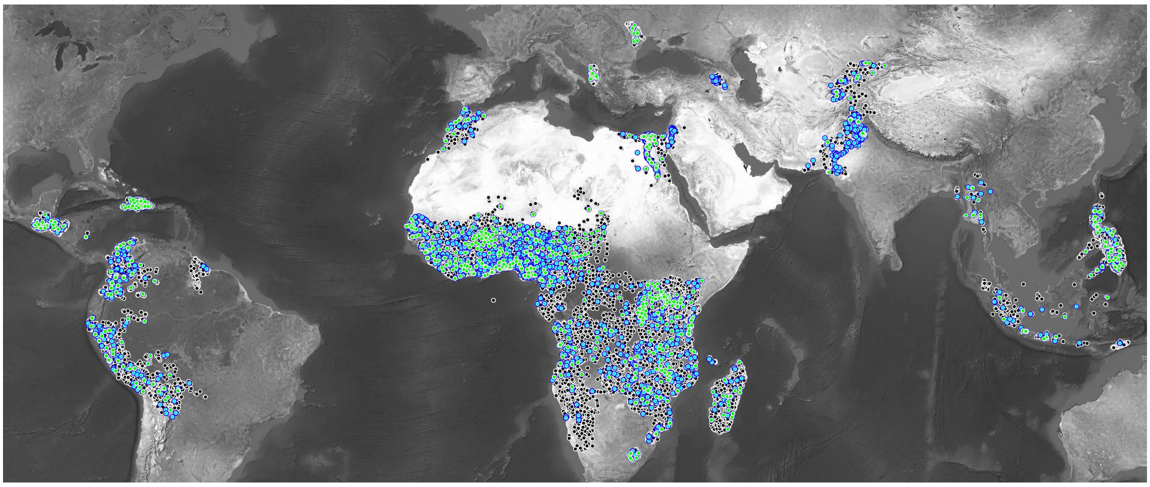
[35]We also estimate these specifications only using cities which gain capital status (have a fraction of years as a capital of less than one) and find comparable effects (not reported).

[36]Given the pattern of spillovers documented earlier, this increase is likely to come from rural areas and cities far away from the capital (although we have no direct evidence documenting such a migratory response). However, we do find some evidence that the larger agglomeration appears to grow faster than the core. Table A-11 in app:sumstat shows analogous regressions.

[37]Suggesting that high moving costs or attachment to rural locations must play a role (see e.g., Henderson and Turner, 2020).

utilize a global sample of geocoded DHS data, to test if there are differences in wealth, access to utilities, as well as educational, and health outcomes between capital and non-capital cities.[38] Figure VII plots our global coverage of DHS clusters in gray (about 80,000 clusters containing roughly 2.7 million respondents), the ones we are able to match to any of our cities in blue (about 25,000 clusters and 750,000 respondents), and those that are located in capital cities at the time of the survey in green (56% of the matched clusters). Note that capital cities are heavily over-represented among urban DHS clusters (56% of matched clusters are capitals).

Figure VII
DHS coverage



Notes: The figure illustrates the spatial distribution of DHS clusters (dark blue with gray borders), DHS clusters matched to our cities (light blue with dark blue borders) and DHS cluster matched to capitals (green with light gray borders).

Our strategy is to pool all of this data and run fixed-effects regressions comparing individual-level outcomes in non-capital cities in the same initial administrative region to outcomes in capital cities. In a second step, we then control for the fundamentals of each city, including initial population density, to estimate a capital city premium (or penalty) using otherwise comparable locations. Table IX presents the results of the individual-level specifications regressing individual-level outcomes on a capital dummy at the time of the survey. Column 1 highlights that DHS respondents in capitals are, on average, wealthier than non-capital respondents by around a quarter to half a standard deviation of the DHS wealth index. Column 2 shows that residents of capital cities are roughly six percentage points more likely to have electricity than residents of other cities within the same initial region. We find no differences in the access to safe water and sanitation (columns 3 and 4) and a negligible effect on the probability of obtaining eight or more years of schooling

[38]Our proxies for the various outcomes are also strictly following the methodology of Henderson et al. (2020) they are explained in detail in Online Appendix F.

Table IX
Amenities in capitals: Fixed effects regressions

	Dependent Variable:					
	DHS WEALTH INDEX	ELEC-TRICITY	SAVE WATER	IMPROVED SANI-TATION	YEARS OF EDU > 8	INFANT MOR-TALITY
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. No controls						
Capital	0.4755	0.0669	0.0013	0.0032	0.0036	-10.4288
	(0.0306)	(0.0082)	(0.0029)	(0.0027)	(0.0012)	(1.4404)
Fundamentals	–	–	–	–	–	–
Individual controls	–	–	–	–	–	–
Ini. Admin-Year FE	✓	✓	✓	✓	✓	✓
Respondents	303521	303521	303521	303521	263212	660511
Panel B. Individual controls & fundamentals						
Capital	0.2557	0.0623	-0.0002	-0.0007	0.0040	-5.0330
	(0.0465)	(0.0118)	(0.0035)	(0.0033)	(0.0017)	(1.9739)
Fundamentals	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
Ini. Admin-Year FE	✓	✓	✓	✓	✓	✓
Respondents	301157	301157	301157	301157	261003	660511

Notes: The table reports results from regressions of various DHS measures on the capital status of a city during the year was taken. The DHS measures on the left hand side are the DHS wealth index, indicator variables for the presence of electricity, save water, improved sanitation, more then 8 years of schooling, as well as infant mortality. The indicator variables are coded following Henderson et al. (2020). All columns include initial-region-by-year fixed effects. Panel B adds locational fundamentals, as well as the following respondent-level controls which we allow to have different effect in different survey years: a gender dummy, age, age squared, and an indicator if the respondent lives in a cluster classified as urban (columns 1 to 5). In column 6 we use respondent-child-level controls: a gender dummy, an indicator variable for multiple children, and their interactions with a linear time trend. We also include a set of year of birth dummies in column 6. Standard errors clustered on the agglomeration provided in parentheses.

(column 5). However, we find a significant reduction in infant mortality between a $6th$ and $12th$ of mean infant mortality (depending on the presence of controls).[39] Panel B shows that the effect sizes decrease in several instances once we control for fundamentals but the general pattern remains.

Taken together, these results imply that residents of capital cities in developing countries enjoy a number of benefits compared to those in non-capital cities. The increase in household wealth suggests that our main finding on density also runs through increased productivity and higher wages. The proximity to government appears to manifest itself in better access to electricity and better health outcomes, but only minor differences

[39]Results are virtually the same if we use the share of years a city is capital as the main explanatory variable variable, see Table A-12 in Online Appendix A.

in primary education. Of course, some benefits could be offset by increases in crime, congestion and other urban disamenities not captured here.

Public and private investments by sector: Another important question is whether capital cities purely attract public investments (and employment) or whether we observe a private response as well. Bairoch (1991), for example, describes places where bureaucrats and property owners rule in the absence of industry as "parasite cities." Lacking census data on the employment structure of cities, we use two proxies for public and private investments: *i)* geocoded data on World Bank projects from 1995 to 2014 and geocoded data on Chinese-financed development projects from 2001 to 2014, and *ii)* the fDi Markets database on private foreign direct investments, which is available over the period from 2003 to 2018.[40]

World Bank projects are usually carried out in close cooperation with the national government in the recipient country and even substitute for some of its basic functions in particularly poor countries. The data includes all projects approved in the World Bank IBRD/IDA lending lines over this period (AidData, 2017), including many infrastructure investments. It contains more than US$630 billion in commitments (in 2011 dollars) which were spent on 5,684 projects in 61,243 locations. We supplement this data with geocoded project level data on China's global footprint of official financial flows over the period from 2000 to 2014 (Bluhm et al., 2020). The data include 3,485 projects (worth US$273.6 billion in 2014 dollars) in 6,184 locations across the globe. China invests heavily in economic infrastructure and services, ranging from roads over seaports to power grids.

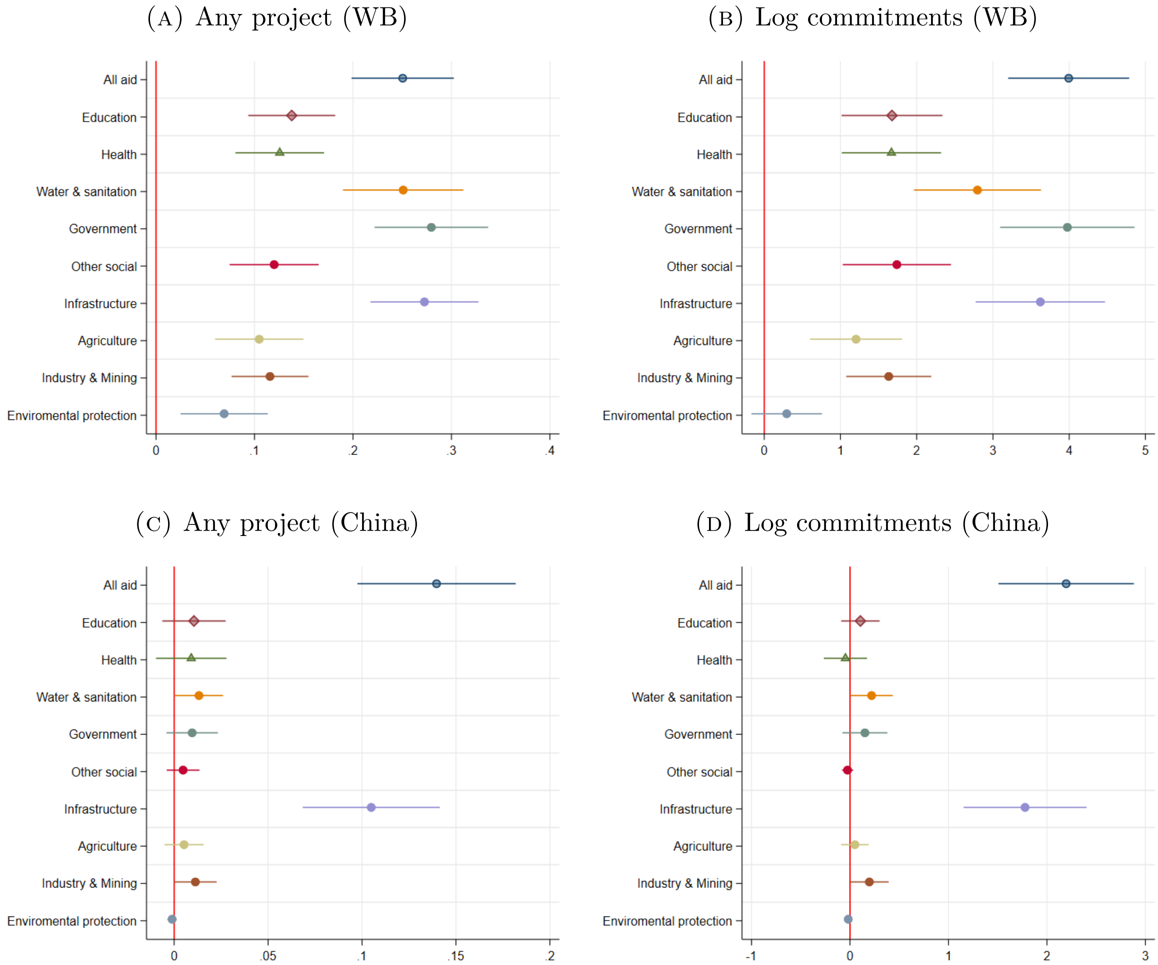
Both the World Bank and China project locations were geocoded ex post and include precision codes indicating if an exact building, city or administrative region were identified.[41] We use a subset of these data which were coded to be either exact or near to the exact location. We then match these projects to our universe of capital and non-capital cities if they fall within 10 km of the city centroid. Aiddata codes the sector of each project following a variant of the OECD Creditor Reporting System (CRS). This allows us to distinguish investment in, say, government from investments in water and sanitation. We examine two outcomes for overall and aid per sector in a series of cross-sectional regressions: whether a city had any project committed over the entire available period (e.g. 1995 to 2014 for the World Bank) and the total amount committed. On the right hand side we have the share of years in which a city was a capital. All regressions include initial-region fixed effects, control for the full set of fundamentals (including initial population), and include a dummy for national capitals. Similar to the long difference approach taken with population, this help us to understand whether capital cities attract

[40] fDi Market is proprietary and available via subscription at www.fdimarkets.com.

[41] Restricting the sample to the two highest precision codes makes sure that we do not mechanically find more projects in subnational capitals, as projects for which less precise geographic information is available are often geocoded to provincial capitals.

more or less projects funded from abroad than comparable cities, but this evidence does not show that new capital cities attract funding immediately after gaining status.

FIGURE VIII
Capitals and aid by sector



(A) Any project (WB)
(B) Log commitments (WB)
(C) Any project (China)
(D) Log commitments (China)

Notes: The figure plots estimates from regressions of development projects in a particular sector on the fraction of years a city was a capital. Panels A and C show the results from regressions with binary variables on the left hand side indicating whether a city has received any project in a particular sector by the World Bank and China, respectively. Panels B and D show the results from regressions with the log of 1 + commitments in USD on the left hand side indicating the amount of commitments a city has received in a particular sector by the World Bank and China, respectively. The definition of sectors follows the OECD's Common Reporting Standard (see Online Appendix F for details). 95% confidence intervals based on standard errors clustered on initial regions are provided as error bars.

Figure VIII reports the regression results per sector and shows several interesting patterns. First, capital cities are considerably more likely to receive any development project than other cities in the same region. For example, a city that has been a capital throughout the entire period has a 25 percentage point higher probability of receiving any World Bank project and was about 14 percentage points more likely to receive a Chinese-funded project than a non-capital city. Second, the sectoral composition of projects suggest that capital cities primarily attract funds for water and sanitation, government and civil society, and infrastructure when it comes to funding by the World Bank. China, on the other hand, appears to invest relatively more in the physical infrastructure of subnational capitals compared to other cities or sectors. In fact, most of the other sectors, apart from water and sanitation or industry and mining show no differences across capital and non-capital cities. Subnational capitals thus attract more public funds from international donors or creditors and these funds primarily go to sectors that improve living standards, government operations, and connectivity. While we cannot rule out that this allocation is the product of regional favoritism or corruption (Hodler and Raschky, 2014; De Luca et al., 2018), or test whether these projects are effective, this lines up well with the DHS evidence presented above (and the related evidence in Gollin et al., 2016; Henderson and Turner, 2020; Henderson et al., 2020).

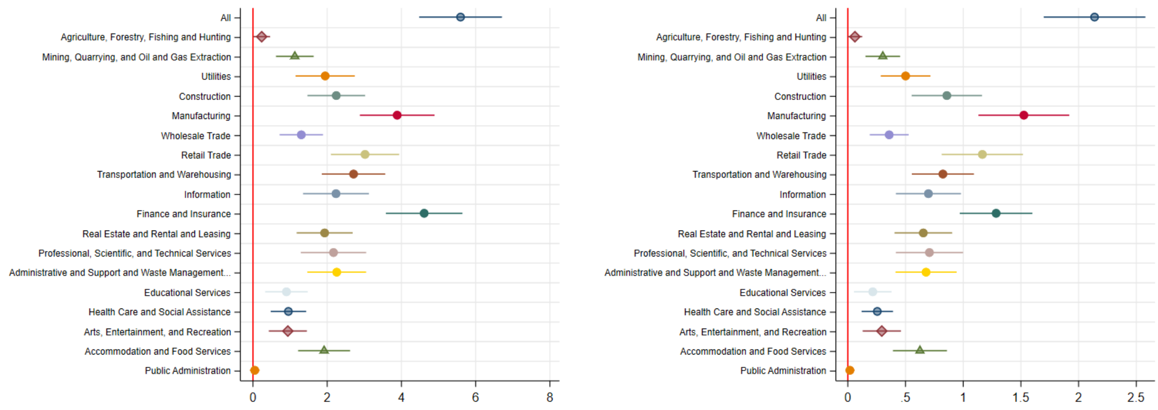
Having documented that capital cities attract more public funds begs the question whether the increase in activity and density is primarily the product of more public employment (and the related investments) or whether private employment increases as well. This is far from a theoretical conjecture. A significant part of the increase in the urban extent of Mamuju after 1990 documented in Figure IV occurs in the area of the city where the new provincial headquarters were built. Recent findings on public employment multipliers appear to depend on the context and usually come from developed countries.[42] Public investment multipliers could be substantially larger in developing countries where the initial capital stock is low.

The fDi Markets tracks global FDI investments and joint ventures by sector, provided that they lead to a new physical operation in the host country. Similar to the China data, the data are not primarily based on official statistics but collected from media, industry organisations and investment agencies. The fDi Markets data reports the host city of the project, the value of the investment and an estimate of the jobs created that can be connected to the investment. We use a subset of the global data for cities in reformed regions. We geocode each host city and match it to an urban cluster or administrative center when it is within 10 kilometers of an FDI project. We run the same set of regressions used for aid projects, only this time the cumulative value of FDI projects in a city or the cumulative number of projected jobs a company plans to create are on the left hand side, and the fraction of years a city was a capital from 2003 to 2018 plus fundamentals and initial-region fixed effects are on the right hand side. The FDI data uses the North American Industry Classification System (NAICS). We aggregate

[42] Faggio and Overman (2014), for example, find no evidence of increases in private employment in response to substantial increases in public employment in more rural areas of the UK. Becker et al. (2020) study the relocation of the German government to Bonn in 1949 and Faggio et al. (2019) study the move back to Berlin in 1999. Public jobs crowded out private employment in Bonn almost one-to-one, while Berlin's service sector benefited (with 0.55 private jobs for every public job). Jofre-Monseny et al. (2020) use the capital city status of Spanish cities to identify the effects of public employment and find evidence of significant crowd-in of private sector employment (1 to 1.3 jobs).

Figure IX
Capitals and FDI by industry

(A) Ln value (B) Ln jobs



Notes: The figure plots estimates from regressions of FDI projects in a particular sector on the fraction of years a city was a capital. Panel A shows the results from regressions with the log of 1 + value of FDI projects in USD on the left hand side indicating the amount of FDI a city has received in a particular sector. Panel B shows the results from regressions with the log of 1 + estimated number of jobs of FDI projects in the city on the left hand side indicating the increase in private employment a city has experienced in a particular sector. The definition of sectors follows the NAICS 2-digit sector classification (see Online Appendix F for details). 95% confidence intervals based on standard errors clustered on initial regions are provided as error bars.

their highly detailed classification to the 2-digit NAICS industry level, which delivers a level of aggregation similar to the CRS codes used for aid projects.

Figure IX suggests that subnational capitals attract considerably more FDI than non-capital cities. The value of FDI projects in capitals is about 5.8 log points larger and these project come with an 8-fold increase in the number of jobs compared to projects in non-capital cities in the same initial region. The sectoral composition of FDI also shows an interesting pattern. Capital cities attract more high value projects with more jobs in manufacturing, finance and insurance, and retail than other cities. Differences to other cities in terms of private investments in public administration and agriculture are negligible. This suggests that core industries with international linkages locate preferably in subnational capitals.

Although the evidence documented on these mechanisms are only a series of partial correlations, they generally support the conjecture that capital cities attract substantial public and private investments. Our findings on medium run population changes also make it unlikely that the fast growth of subnational capitals can only be attributed to increases in public employment.[43]

[43]Public employment shares in developing countries are often not particularly high (e.g. provincial level public employment ranges from about 8 to 47 per thousand in Indonesia, see OECD, 2016), so that the population increases documented above would imply an implausibly large expansion of the public sector. To see this, consider an initial public employment share of 5%. If the population of a capital city grows by 30% from 1990 to 2015 as a consequence of the change in political status, then public

7. Conclusion

Our results provide the first evidence that the recent proliferation of administrative units and the corresponding change in status of some cities to first-order administrative capitals affect the location of economic activity in developing countries. Leveraging a new and global panel of administrative reforms from 1987 until 2018, we find that new capital cities attract significantly more economic activity in the short and medium run. These benefits spill over to nearby cities but are not constant across the level of development or city locations. Capitals in inferior locations, as defined by a lack of internal market access or high agricultural suitability, experience considerably weaker growth than those in superior locations.

We interpret these findings and our analysis of likely channels as evidence that subnational capitals are focal points for migrants and business within regions. If these capitals coincide with productive locations, then accelerating agglomeration in these cities is likely to impact aggregate welfare positively (see Allen and Donaldson, 2020). More broadly, these findings are relevant to policy-makers who decide to decentralize based on various political considerations. Territorial politics and public investments can be a tool for steering agglomeration in rapidly urbanizing developing countries. However, we also illustrate how ineffective such policies are if they target unfavorable locations in the hinterland or when their implementation no longer delivers sufficient economies of scale.

The global data we provide in this paper opens the door to studying various questions about cities and their role in the administrative hierarchy. So far, we know little about the politics behind the observed locations of subnational capital cities apart from a few historical cases. Our paper only exploits the varied nature of the underlying motivations and their unpredictability but makes no contribution to untangling them. We leave such questions for future research.

employment would have to rise 6-fold to explain all of this increase. If the increases are more modest, for example a doubling in public employment, then the associated multipliers would have to be much larger than what is suggested by the literature (Faggio and Overman, 2014; Becker et al., 2020; Jofre-Monseny et al., 2020). This is in line with Bai and Jia (2020), who document similar results for the very long-run development of China's prefectures when they host provincial capitals.

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Online appendix

A. Summery statistics and additional results

A-1. Summary statistics

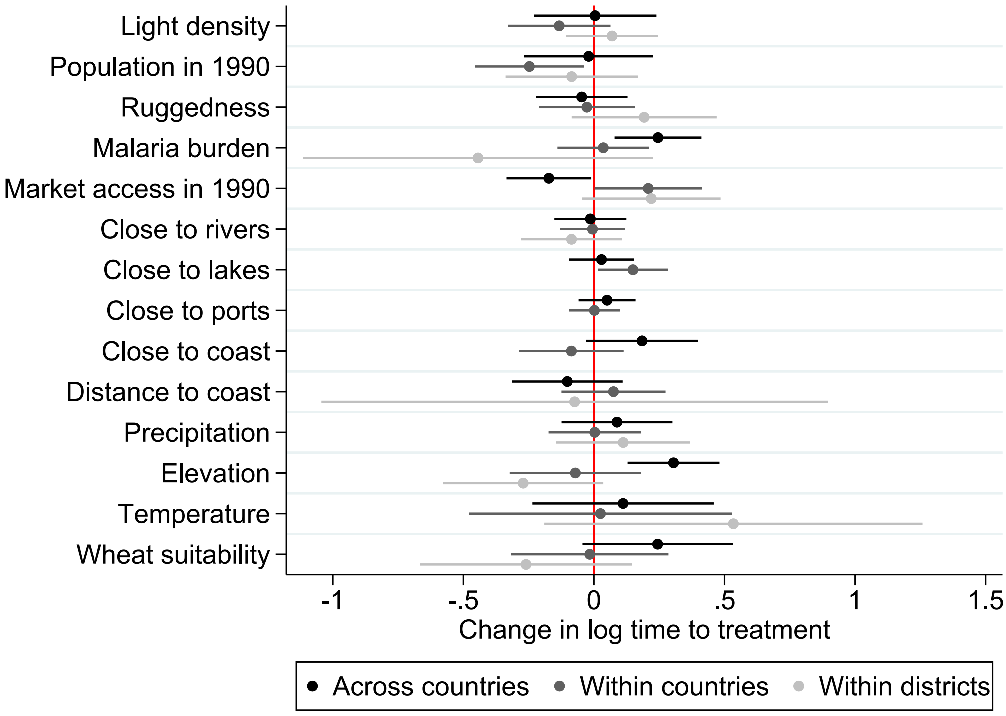
TABLE A-1
Summary statistics: Fundamentals

	Mean	SD	Min	Max	N
Panel A. Cities (all)					
Log light density	2.95	1.29	1.26	7.65	515,934
Log population 1990	10.84	0.88	9.25	17.06	515,934
Ruggedness	14.49	15.43	0.46	120.22	515,934
Malaria suitability	0.01	0.02	0.00	0.17	515,934
Market access (pop 1990 based)	10.32	1.30	3.46	13.55	515,934
River within 25km	0.35	0.48	0.00	1.00	515,934
Lake within 25km	0.02	0.14	0.00	1.00	515,934
Port within 25km	0.05	0.22	0.00	1.00	515,934
Coast within 25km	0.16	0.37	0.00	1.00	515,934
Distance to coast	371.14	362.96	2.57	2,504.02	515,934
Average precipitation	9.29	5.38	0.05	81.39	515,934
Average elevation	458.67	577.53	-26.41	5,023.05	515,934
Average temperature	19.94	6.89	-7.59	32.09	515,934
Wheat suitability	2,296.89	2,074.38	0.00	7,252.34	515,934
Panel B. Cities (within reformed areas)					
Log light density	2.34	1.12	1.26	7.51	182,048
Log population 1990	10.80	0.83	9.90	16.80	182,048
Ruggedness	13.50	15.61	0.53	110.43	182,048
Malaria suitability	0.01	0.03	0.00	0.16	182,048
Market access (pop 1990 based)	10.61	1.31	3.48	13.55	182,048
River within 25km	0.38	0.49	0.00	1.00	182,048
Lake within 25km	0.02	0.13	0.00	1.00	182,048
Port within 25km	0.03	0.16	0.00	1.00	182,048
Coast within 25km	0.11	0.31	0.00	1.00	182,048
Distance to coast	480.58	378.24	2.57	2,188.57	182,048
Average precipitation	9.77	4.53	0.05	75.78	182,048
Average elevation	486.82	600.83	-25.44	5,023.05	182,048
Average temperature	22.24	5.67	-5.49	30.60	182,048
Wheat suitability	1,982.01	1,760.43	0.00	6,886.30	182,048

Notes: Panel A of the table reports the summary statistics for our sample of all cities. Panel B reports summary statistics for the sample of cities located within reformed regions.

A-2. Additional figures

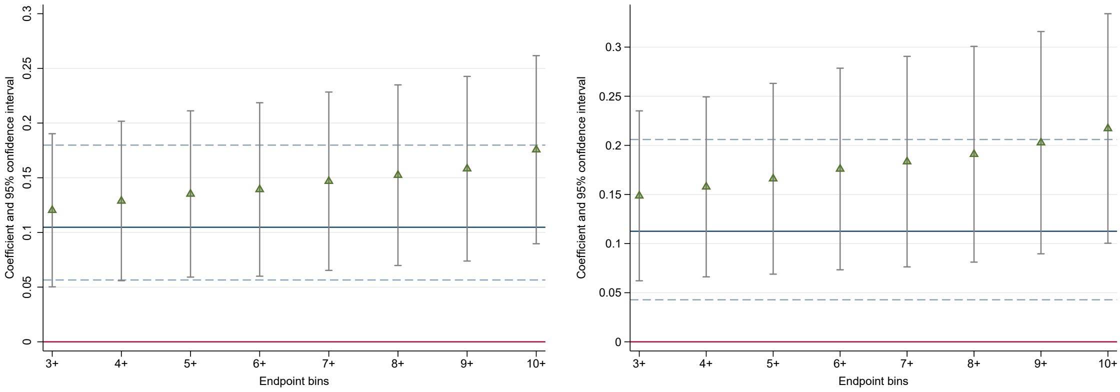
FIGURE A-1
Time to treatment



Notes: The figure illustrates results from cross-sectional regressions of the time to treatment (in logs plus one) on initial city characteristics. The regressions was run three times, once without fixed effects, with country fixed effects, and with initial region fixed effects. The coefficients are standardized beta coefficients. Some coefficients are omitted in the specification with initial region fixed effect for a lack of within region variation. 95% confidence intervals clustered on initial regions are indicated by the error bars.

FIGURE A-2
Endpoint binning and medium-run effect size: Event-study estimates

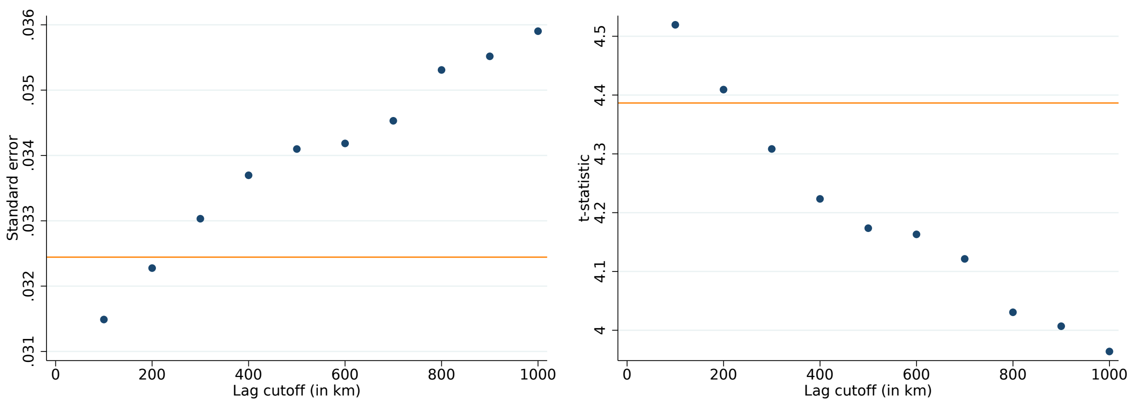
(A) All cities (B) Reformed Areas



Notes: The figure shows point coefficients and 95% confidence intervals of the endpoint bins estimated in several event studies with varying window sizes. The underlying event studies use five pre-treatment periods and extend the event window from 3 (or more) to 10 (or more) periods. The effect in the last pre-period is normalized to zero. Panel A is based on column 3 and panel B is based on column 6 of Table A-2. The blue line indicates the difference-in-differences estimate corresponding to each panel and the dashed blue lines provide the 95% confidence intervals of these estimates.

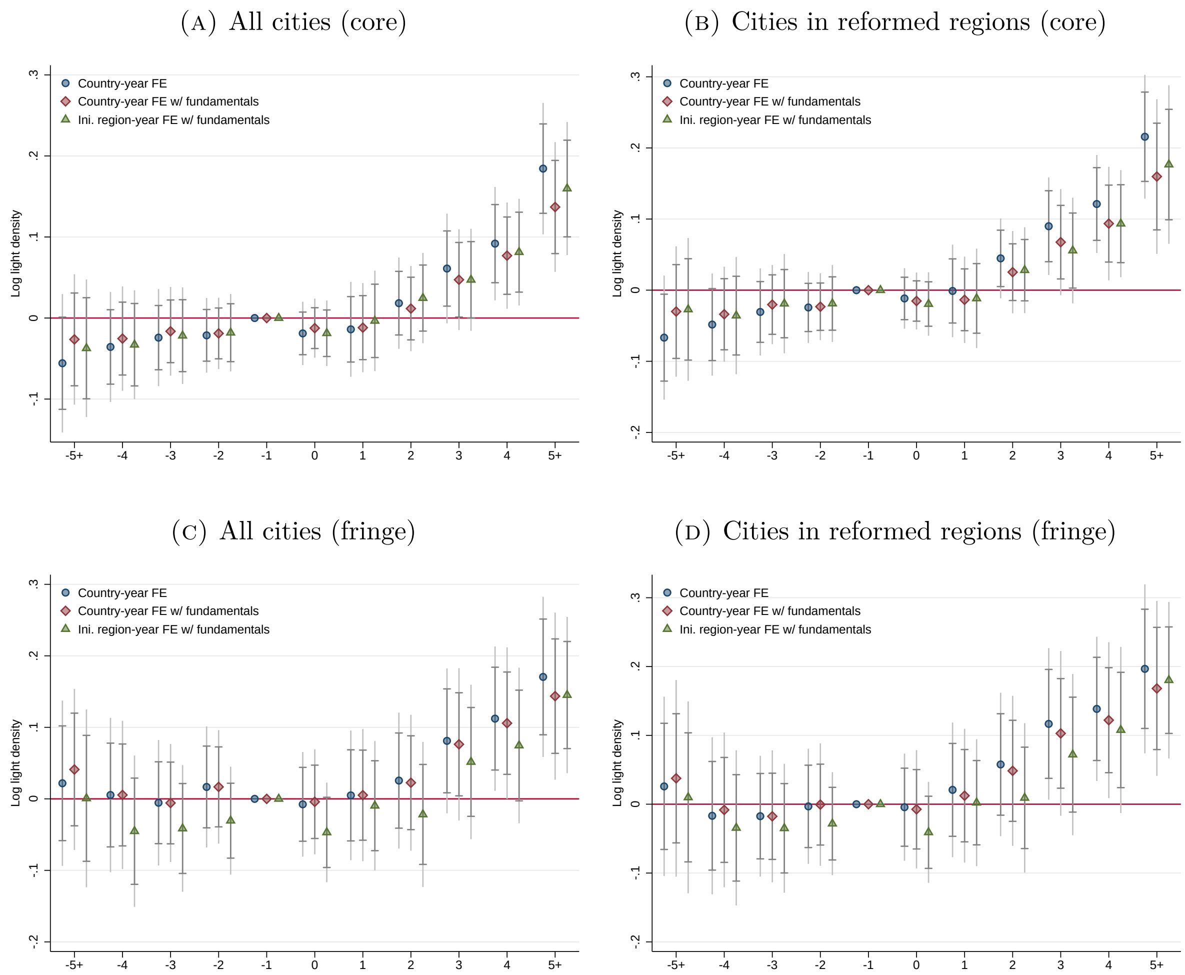
FIGURE A-3
Accounting for spatial autocorrelation

(A) Standard errors (B) t-statistics



Notes: The figure illustrates results from varying the spatial lag cutoff when estimating standard errors which allow for cross-sectional dependence. All results are based on a variant of column 6 in Table A-2 where we restrict the sample to reformed areas and include city fixed effects, as well as initial-region fixed effects. Here we omit the time-varying effects of the fundamentals for computational reasons (to reduce the size of the regressor matrix small). All Conley errors are estimated with a uniform kernel and a time-series HAC with a cutoff of 1,000 years to allow for arbitrary dependence over time. Panel A shows estimates of the resulting standard errors, with the original error clustered on initial regions highlighted in orange. Panel B shows estimates of the resulting t-statistics, with the original t-statistic clustered on initial regions highlighted in orange.

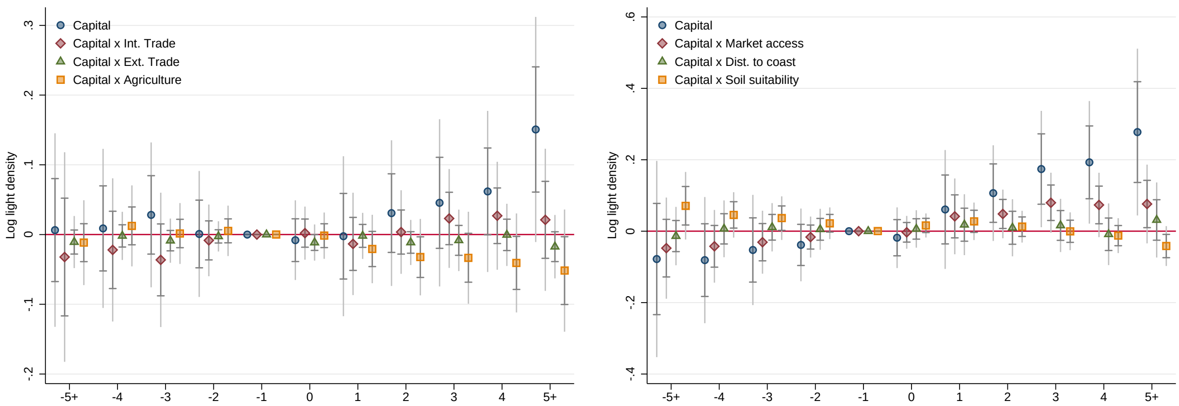
Figure A-4
Agglomerations: Event-study estimates



Notes: The figure reports event-study estimates corresponding to the difference-in-differences results presented in Table II. The upper panels report results for the core of the agglomeration. The lower panels report results for the fringe (new parts that were added after 1990). Panels A and C show estimates for all cities. Panels B and D show estimates for cities in reformed regions. Dots represent point estimates from a regression with city and county-year fixed effects, diamonds represent specifications with additional controls for locational fundamental, and triangles represent specifications with initial-region-by-year fixed effects in addition. All regressions include city fixed effects. 95% confidence intervals based on standard clustered on initial provinces are provided by the gray error bars. The orange error bars indicate 95% sup-t bootstrap confidence bands with block sampling over initial provinces (Montiel Olea and Plagborg-Møller, 2019).

Figure A-5
Fundamentals: Event-study estimates

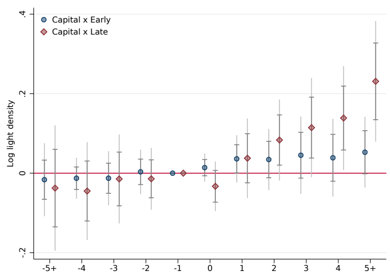
(A) Principal components (B) Single fundamentals



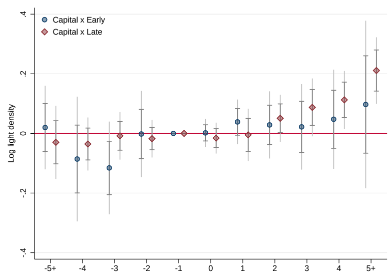
Notes: The figure reports event-study estimates corresponding to the difference-in-differences results presented in Table IV. Panel A reports estimates corresponding to column 5 of panel A, whereas panel B reports the estimates corresponding to column 5 of panel B of the table. All regressions include city fixed effects and initial-region-by-year fixed effects. 95% confidence intervals based on standard clustered on initial provinces are provided by the gray error bars. The orange error bars indicate 95% sup-t bootstrap confidence bands with block sampling over initial provinces (Montiel Olea and Plagborg-Møller, 2019).

Figure A-6
Early vs. late: Event-study estimates

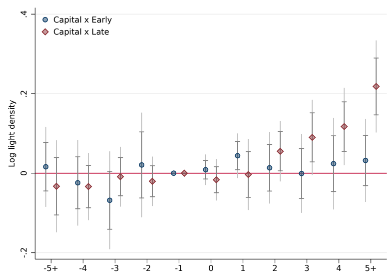
(A) Schooling



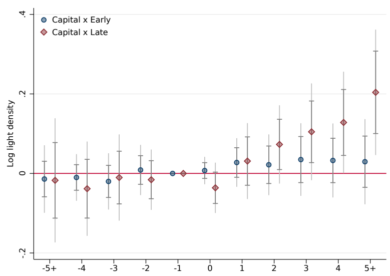
(B) Urbanization



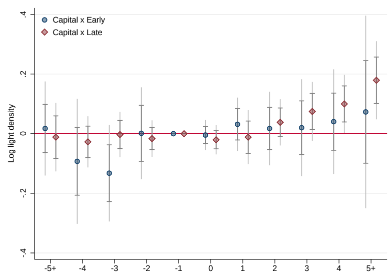
(C) GDP in 1950



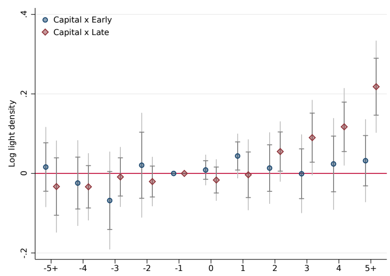
(D) Schooling (controls)



(E) Urbanization (controls)



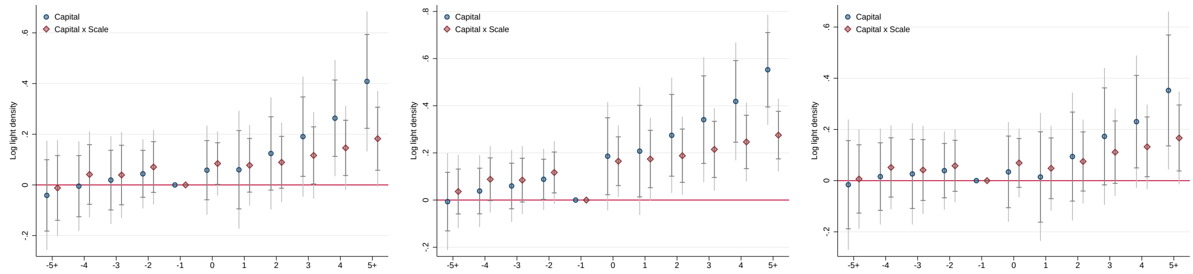
(F) GDP in 1950 (controls)



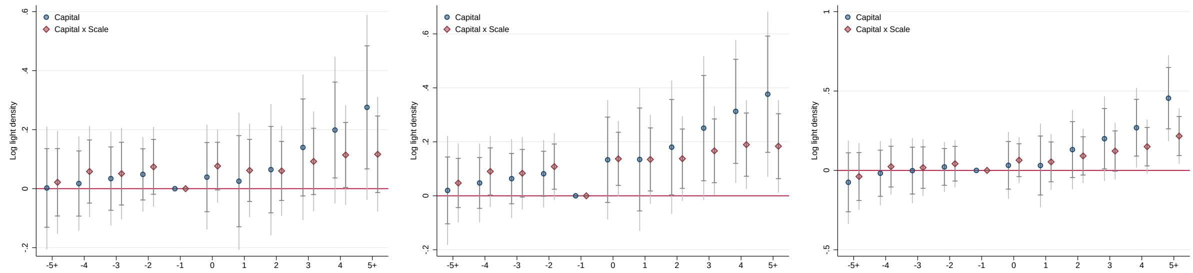
Notes: The figure reports event-study estimates corresponding to the difference-in-differences results presented in Table V. Panels A to C report the event studies without controls (corresponding to columns 1, 3 and 5 of the table). Panels D to F report the event studies including controls (corresponding to columns 2, 4 and 6). All regressions include city fixed effects and initial-region-by-year fixed effects. 95% confidence intervals based on standard clustered on initial provinces are provided by the gray error bars. The orange error bars indicate 95% sup-t bootstrap confidence bands with block sampling over initial provinces (Montiel Olea and Plagborg-Møller, 2019).

Figure A-7
Scale: Event-study estimates

(A) Pop region (B) Urb. pop region (C) # cities region



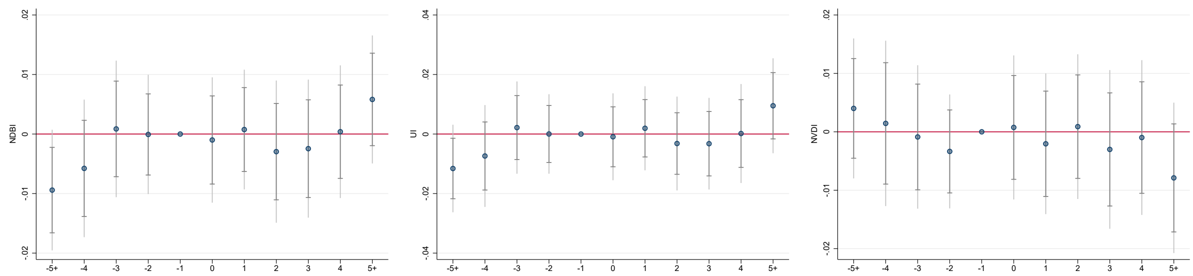
(D) Pop region (controls) (E) Urb. pop region (controls) (F) # cities region (controls)



Notes: The figure reports event-study estimates corresponding to the difference-in-differences results presented in Table VI. Panels A to C report the event studies without controls (corresponding to columns 1, 3 and 5 in the table). Panel D to F report the event studies including controls (corresponding to columns 2, 4 and 6). All regressions include city fixed effects and initial-region-by-year fixed effects. 95% confidence intervals based on standard clustered on initial provinces are provided by the gray error bars. The orange error bars indicate 95% sup-t bootstrap confidence bands with block sampling over initial provinces (Montiel Olea and Plagborg-Møller, 2019).

Figure A-8
Built-up: Event-study estimates

(A) NDBI (B) UI (C) NDVI



Notes: The figure reports event-study estimates corresponding to the difference-in-differences results presented in Table VII. Panel A to C cover all six columns of the table. All regressions include city fixed effects and initial-region-by-year fixed effects. 95% confidence intervals based on standard clustered on initial provinces are provided by the gray error bars. The orange error bars indicate 95% sup-t bootstrap confidence bands with block sampling over initial provinces (Montiel Olea and Plagborg-Møller, 2019).

A-3. Additional tables

TABLE A-2
Baseline differences-in-differences

	Dependent Variable: $\ln \text{LIGHTS}_{cit}$					
	All Cities			Reformed Regions		
	(1)	(2)	(3)	(4)	(5)	(6)
CAPITAL	0.1043	0.0852	0.1049	0.1389	0.1121	0.1111
	(0.0278)	(0.0272)	(0.0283)	(0.0294)	(0.0305)	(0.0324)
Fundamentals	–	✓	✓	–	✓	✓
City FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	–	✓	✓	–
Ini. Region-Year FE	–	–	✓	–	–	✓
N	23910	23910	23910	8519	8519	8519
$N \times \bar{T}$	524889	524889	524889	186511	186511	186511

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status. Standard errors clustered on initial regions are provided in parentheses.

TABLE A-3
Different light measures

	Dependent Variable: $\ln \text{LIGHTS}_{cit}$				
	Stable lights raw	Stable lights bottom fix	Average lights raw	Bluhm & Krause '18 raw	Bluhm & Krause '18 bottom fix
	(1)	(2)	(3)	(4)	(5)
CAPITAL	0.0364	0.0838	0.0826	0.0838	0.1111
	(0.0301)	(0.0336)	(0.0298)	(0.0336)	(0.0324)
Fundamentals	✓	✓	✓	✓	✓
City FE	✓	✓	✓	✓	✓
Ini. Region-Year	✓	✓	✓	✓	✓
N	7380	8519	8519	8519	8519
$N \times \bar{T}$	136837	186511	186511	186511	186511

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer using different light measures on capital city status. We add one before taking logs of lights per area in km in columns 1 and 4 to keep city-years with no observed light. The raw average lights data record a non-zero light intensity in every city-year. Standard errors clustered on initial regions are provided in parentheses.

Table A-4
Initial city size

	Dependent Variable: $\ln \text{LIGHTS}_{cit}$				
	Initial city size				
	30k	40k	50k	75k	100k
Capital	0.1301	0.1492	0.1744	0.1658	0.2017
	(0.0325)	(0.0350)	(0.0363)	(0.0403)	(0.0534)
Fundamentals	✓	✓	✓	✓	✓
City FE	✓	✓	✓	✓	✓
Ini. Region-Year	✓	✓	✓	✓	✓
N	5667	4123	3174	1953	1385
$N \times \bar{T}$	124066	90219	69428	42722	30309

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status. Columns 1 to 5 restrict the estimation samples to cities with an initial population above 30 up to 100k inhabitants. Standard errors clustered on initial regions are provided in parentheses.

Table A-5
City area changes: 1990–2015

	Dependent Variable: $\Delta \ln \text{AREA}_{cit}$			
	All Cities		Reformed Provinces	
	(1)	(2)	(3)	(4)
Capital	0.1032	0.0933	0.1296	0.0949
	(0.0093)	(0.0136)	(0.0180)	(0.0259)
Fundamentals	–	✓	–	✓
Initial-Region FE	✓	✓	✓	✓
Cities	20779	20779	7577	7577

Notes: The table reports results from long difference regressions of the change in the log area of a city on the fraction of years in which a city is a capital. The regressions are estimated using the sample of agglomerations, that is, cities which exist in 1990 and have expanded by 2015 or merged into a larger city. Standard errors clustered on initial regions are provided in parentheses.

Table A-6

Different control groups: Countrywide matches

	Dependent Variable: ln Lights_{cit}					
	Light intensity in 1992			Population in 1990		
	Control city ranks within ... of treated city					
	± 2	± 3	± 4	± 2	± 3	± 4
	(1)	(2)	(3)	(4)	(5)	(6)
Capital	0.0955	0.0952	0.0957	0.0826	0.0834	0.0799
	(0.0220)	(0.0228)	(0.0229)	(0.0240)	(0.0241)	(0.0246)
F-test pre-trends (p-val.)	0.378	0.198	0.183	0.669	0.593	0.753
N	815	1041	1243	785	1012	1220
$N \times \bar{T}$	17724	22632	27026	17098	22048	26584

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status. Panels A to C match treated cities to a varying number of control cities on the basis of their rank in terms of light intensity or population within the entire country. All regressions include city fixed effects, initial-region by-year fixed effects, and time-varying coefficients on the fundamentals. We report an F-test for pre-trends tests for the null hypothesis that all leading terms in the equivalent event-study specification are jointly zero. Standard errors clustered on initial regions are provided in parentheses.

Table A-7

Ethnic diversity

	Dependent Variable: ln Lights_{cit}					
	All Cities			Reformed Districts		
	(1)	(2)	(3)	(4)	(5)	(6)
Capital	0.1019	0.0840	0.1020	0.1370	0.1121	0.1090
	(0.0290)	(0.0284)	(0.0296)	(0.0315)	(0.0319)	(0.0332)
Capital × ELF	-0.0045	-0.0061	0.0048	0.0056	0.0009	0.0071
	(0.0203)	(0.0195)	(0.0208)	(0.0214)	(0.0214)	(0.0220)
Fundamentals	–	✓	✓	–	✓	✓
City FE	✓	✓	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓	✓	✓
N	23248	23248	8323	8323	8323	
$N \times \bar{T}$	510505	510505	510505	182379	182379	182379

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status. The interactions of the capital city status with ethnic diversity ($\tilde{z}$) are standardized such that $\tilde{z} \equiv (z - \bar{z})/\sigma_z$. Standard errors clustered on initial regions are provided in parentheses.

TABLE A-8
Democracy

	Dependent Variable: $\ln \text{LIGHTS}_{cit}$					
	All Cities			Reformed Districts		
	(1)	(2)	(3)	(4)	(5)	(6)
CAPITAL	0.1206	0.1107	0.1266	0.1471	0.1214	0.1228
	(0.0281)	(0.0276)	(0.0294)	(0.0349)	(0.0356)	(0.0376)
CAPITAL × DEMOCRACY	-0.0166	-0.0384	-0.0402	-0.0173	-0.0193	-0.0239
	(0.0094)	(0.0112)	(0.0119)	(0.0241)	(0.0266)	(0.0272)
Fundamentals	–	✓	✓	–	✓	✓
City FE	✓	✓	✓	✓	✓	✓
Ini. Region-Year FE	–	–	✓	–	–	✓
N	23910	23910	23910	8519	8519	8519
$N \times \bar{T}$	523349	523349	523349	186267	186267	186267

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status interacted with democracy. The interactions of the capital city status with democracy ($\tilde{z}$) are standardized such that $\tilde{z} \equiv (z - \bar{z})/\sigma_z$. Standard errors clustered on initial regions are provided in parentheses.

TABLE A-9
Larger agglomeration and city fringe: Built-up

	Dependent Variable: Built-up area within city					
	NDBI		UI		NDVI	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Growth of the larger agglomeration						
CAPITAL	0.7554	0.5997	1.1443	0.8460	-0.8784	-0.5841
	(0.2189)	(0.2241)	(0.2889)	(0.3029)	(0.2391)	(0.2551)
Panel B. Growth in the periphery of the city						
CAPITAL	1.1046	0.7849	1.1337	0.9505	-0.5110	-0.5062
	(0.3219)	(0.3056)	(0.4129)	(0.4000)	(0.3330)	(0.3280)
N	7592	7592	7592	7592	7592	7592
$N \times \bar{T}$	160595	160595	160595	160595	160595	160595
Fundamentals	–	✓	–	✓	–	✓
Agglomeration FE	✓	✓	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓	✓	✓

Notes: The table reports results from fixed effects regressions of the Normalized Difference Built-Up Index (NDBI), Urban Index (UI), and the Normalized Difference Vegetation Index (NDVI) on capital city status. Panel A reports results based on the larger agglomeration (the envelope over 1990 and 2015 of the urban clusters detected in 1990). Panel B reports the results for the fringe (areas the urban clusters detected in 1990 that meet the detection threshold by 2015). All coefficients are scaled by 100 for exposition. Standard errors clustered on initial regions are provided in parentheses.

Table A-10
Robustness: Controlling for built-up

	Dependent Variable: $\ln \text{Lights}_{cit}$					
	NDBI		UI		NDVI	
	(1)	(2)	(3)	(4)	(5)	(6)
Capital	0.1364	0.1064	0.1353	0.1058	0.1359	0.1065
	(0.0323)	(0.0347)	(0.0322)	(0.0348)	(0.0323)	(0.0345)
Built-up	0.0024	0.0022	0.0024	0.0022	-0.0027	-0.0026
	(0.0015)	(0.0013)	(0.0013)	(0.0012)	(0.0008)	(0.0007)
Fundamentals	–	✓	✓	–	✓	✓
City FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	–	✓	✓	–
Ini. Region-Year FE	–	–	✓	–	–	✓
N	8519	8519	8519	8519	8519	8519
$N \times \bar{T}$	180572	180572	180572	180572	180572	180572

Notes: The table reports results from fixed effects regressions of the log of light intensity per square kilometer on capital city status. Built-up is measured by the Normalized Difference Built-up Index (NDBI), the Urban Index (UI) or the Normalized Difference Vegetation Index (NDVI). Standard errors clustered on initial regions are provided in parentheses.

Table A-11
Population changes: Agglomeration & city fringe

	Dependent Variable: $\Delta \ln \text{Pop}_{ci}$					
	1990–2000		2000–2015		1990–2015	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Growth of the larger agglomeration						
Capital	0.1724	0.1411	0.2176	0.1746	0.4059	0.3295
	(0.0152)	(0.0159)	(0.0218)	(0.0250)	(0.0365)	(0.0407)
Panel B. Growth in the periphery of the city						
Capital	0.0725	0.0715	0.1461	0.1576	0.2306	0.2385
	(0.0717)	(0.0727)	(0.0332)	(0.0332)	(0.0815)	(0.0820)
Fundamentals	–	✓	–	✓	–	✓
Agglomeration FE	✓	✓	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓	✓	✓
N	7418	7418	7418	7418	7418	7418

Notes: The table reports results from long difference regressions of the change in log population of a city on the fraction of years in which a city is a capital. Panel A reports results based on the larger agglomeration (the envelope over 1990 and 2015 of the urban clusters detected in 1990). Panel B reports the results for the fringe (areas the urban clusters detected in 1990 that meet the detection threshold by 2015). Standard errors clustered on initial regions are provided in parentheses.

Table A-12

Subnational capitals: Individual level evidence (capital share)

	Dependent Variable:					
	DHS wealth index	Elec-tricity	Save water	Improved sani-tation	Years of Edu > 8	Infant mor-tality
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. No controls						
Capital share	0.4790	0.0665	0.0011	0.0031	0.0033	-10.6488
	(0.0310)	(0.0083)	(0.0029)	(0.0027)	(0.0011)	(1.4511)
Fundamentals	–	–	–	–	–	–
Individual controls	–	–	–	–	–	–
Ini. Region-Year FE	✓	✓	✓	✓	✓	✓
Respondents	331992	331992	331992	331992	288442	660511
Panel B. Individual controls & fundamentals						
Capital share	0.2563	0.0627	-0.0002	-0.0008	0.0035	-5.3415
	(0.0476)	(0.0120)	(0.0035)	(0.0033)	(0.0017)	(2.0236)
Fundamentals	✓	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓	✓	✓
Respondents	329011	329011	329011	329011	285652	660511

Notes: The table reports results from a regressions of various DHS measures on the capital share (the fraction of years the city was a capital). The DHS measures on the left hand side are the DHS wealth index, indicator variables for the presence of electricity, save water, improved sanitation, more then 8 years of schooling, as well as infant mortality. The indicator variables are coded following Henderson et al. (2020). All columns include initial-region-by-year fixed effects. Panel B adds locational fundamentals, as well as the following respondent-level controls which we allow to have different effect in different survey years: a gender dummy, age, age squared, and an indicator if the respondent lives in a cluster classified as urban (columns 1 to 5). In column 6 we use respondent-child-level controls: a gender dummy, an indicator variable for multiple children, and their interactions with a linear time trend. We also include a set of year of birth dummies in column 6. Standard errors clustered on the agglomeration provided in parentheses.

B. Tracking capital cities and subnational units

We separately track changes in the geography of subnational units and capitals over time, and cross-reference both results at the end to minimize the scope for error. We start cataloging subnational capitals using the two most comprehensive databases available today (i.e., the Statoids database, Law, 2010 and the City Population database, Brinkhoff, 2020). We use the Global Administrative Unit Layers (GAUL) vector data as a baseline to track subnational units over time, which only records the spatial extent of administrative units but contains no information on their capitals. The three databases have varying temporal coverage. Statoids often tracks capitals and subnational units back to the founding of a country and is usually accurate (up until 2013/2014), but lacks any spatial information. City Population and GAUL cover short time periods, from 1998 until 2020 and 1990 until 2014, respectively.

B-1. Administrative units over time

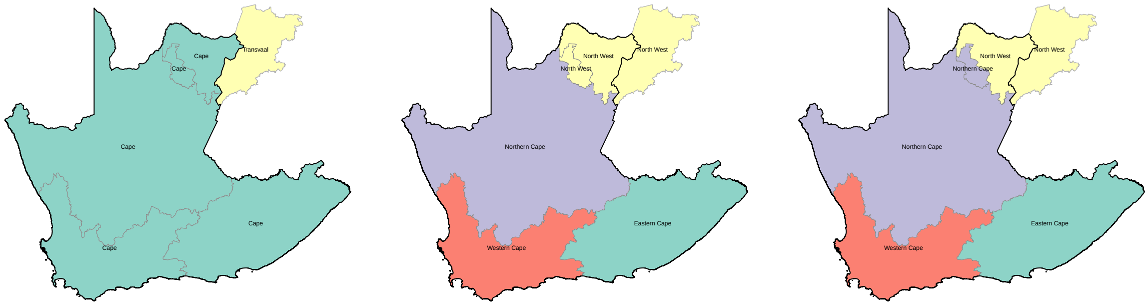
We begin by backing out a reform tree from the GAUL data using a simple spatial algorithm. For any pair of two years, we create the spatial intersection of the two vector data sets. This creates new areas or new affiliations whenever a border is moved, deleted or created. We then cycle forward by intersecting the result of the previous intersection with the next year of official data and so on. During each iteration, we also record the current region identifier and add it to an identification string which in the last year contains 24 (i.e. $2014 - 1990$) identifiers.

We obtain two data sets in this manner. The first is a spatial data set of micro-regions, which in the final year contains the smallest spatial unit whose borders were *not reformed* in any of the preceding years. We call this unit a splinter. The second is a kind of evolutionary tree for each contemporary splinter, summarizing its entire history of regional affiliations and its respective administrative center back until 1990. Note that splinters only result from border reforms that cut across borders from the previous year. If borders are simply abolished, no new splinter will be created but the identity of the region changes. Hence, the combination of the spatial splinter data set and the reform tree identifies all administrative reforms in a general and spatially consistent manner. Moreover, the reform tree allows us to easily compare the results to other non-spatial data sources, such as City Population or Statoids.

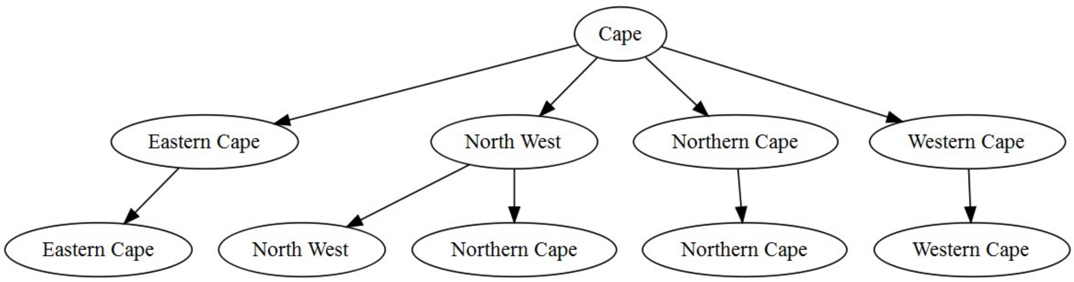
Figure B-1 provides an illustration of the two data sets created by this process. It shows the reform history of Cape Province in South Africa from 1992 onward (the green area in panel A). In 1994, the Cape Province was split into four new regions (panel B). Three of the successor provinces are congruent with the former province, while the fourth region (North-West) includes some areas of the former Transvaal (the neighboring province to the north east, marked in yellow in panel A). Furthermore, a part of the

Figure B-1
Reform History of Cape Province, South Africa

(a) Cape province in 1992 (b) Cape province in 1995 (c) Cape province in 2005



(d) The reform tree

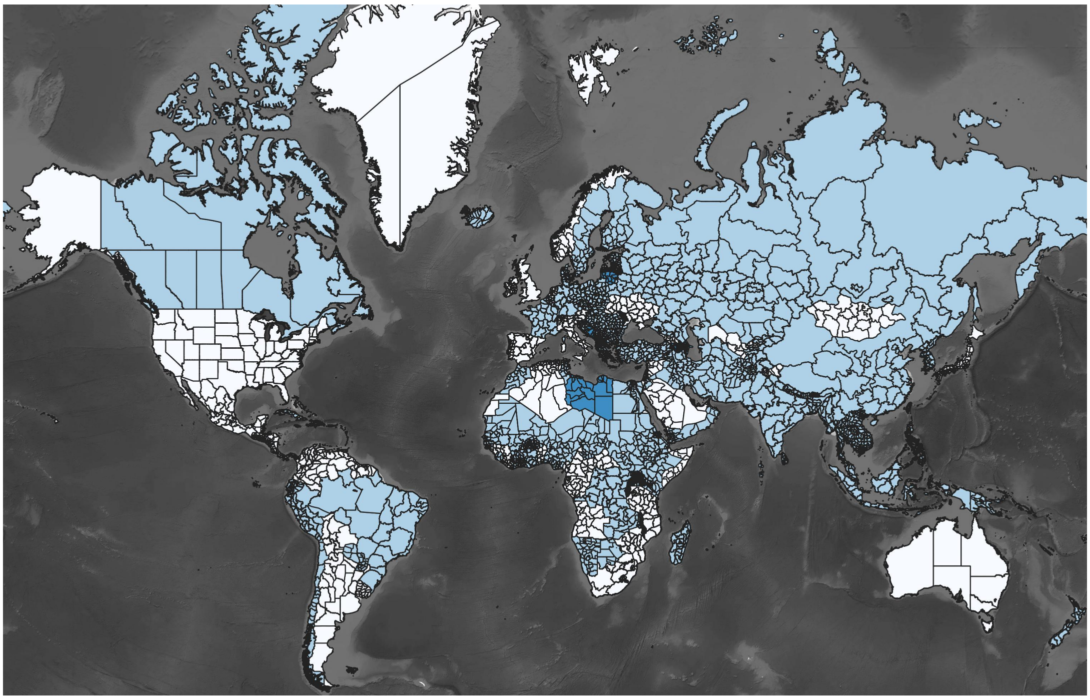


Notes: Panel a to c illustrate initial and successor regions of the Cape Province in South Africa. Panel d) illustrates the evolutionary tree for the splinters which were formerly part of Cape Province, South Africa. The last level represents the situation after the 2005 reform.

North-West was assigned to the Northern Cape in 2005 (see the yellow area in panel B, which turns purple in Panel C). As a result, all splinters of Cape Province are affiliated with at least two different administrative regions over this period (panel D).

Next, we compare the resulting reform tree with Statoids and City Population to document discrepancies (of which there are many). First, the different sources do not always agree on what unit constitutes the first-order administrative level. GAUL sometimes contain macro regions, which have no political function and are easily identified using other data sources. Whenever we detect a case in which GAUL seems to disagree with other sources, or misses a reform entirely, we collect additional spatial data for these regions. From 2000 onward, AidData's GeoBoundaries database and GADM provide a lot of high quality data, although neither of them is without error. Data for the early 1990s is harder to obtain and sometimes requires us to digitize offline maps. In rare cases, we were able to recover the correct shapes merging regions. Uganda, for example, consecutively split its larger regions into smaller units, so that the most recent vector data was sufficient to reconstruct an administrative map for each year. In summary, we found that around 40% of all countries in GAUL had missing or incomplete data during the period from 1990 to 2014 (see Figure B-2 for an illustration).

Figure B-2
Corrections made in GAUL data from 1990-2014



Notes: The figure plots the corrected GAUL countries. Countries in white are correct in GAUL, bright blue are those we had to fix, dark blue are those we are unable to fix, because we lack correct maps for one or more years during the sample period.

Finally, we extended the corrected sample to full period from 1987 to 2018. Extending the sample from 2014 onward is straightforward, since many statistical offices upload official vector files and we could use newer version of AidData's GeoBoundaries database and GADM to fill in the gaps. Extending backward from 1989 to 1987 was more cumbersome. We relied mostly the 1980s and early 1990s editions of the Atlas Britannica.

B-2. Capital cities over time

This workflow starts out with two lists of capital city-years obtained from Statoids and City Population. The lists where provided to two trained coders who independently cross-referenced and checked each entry for inconsistencies. The coders resolved any differences using additional data sources such as the CIA Factbook (Central Intelligence Agency, 2020), Wikipedia, or secondary literature. A third coder compared these two sets of results and resolved differences, if there were any, in a final arbitration process.

Next the two expert coders geocoded the locations of all administrative cities, i.e., the longitude and latitude of the city centroids using the OpenStreetMap's (OSM) Nominatim API (OpenStreetMap contributors, 2020) and the Google Maps' geocoding API (see Google Maps API, 2020). OSM and Google accurately identified the coordinates of most cities without any problems. Unfortunately, not all cities were coded automatically and some cities were not coded correctly. In those cases, we manually identified the coordinates the city. In Uganda, for example, we had to manually geocode around 60 out of 136 administrative centers. The manual coding included another arbitration layer in case of disagreements.

Finally, we merge the remotely-sensed universe of urban clusters in 1990 and 2015 with the coordinates of administrative cities. We consider exact matches all cases where the centroid of a capital city falls within 3 km of an urban cluster. In the few instances where no urban cluster is within this distance of an administrative center, we proceed by matching on names. Any cluster within 50 km of a capital city with almost the same name, defined as a Levenshtein edit distance of less than 3, is considered a match.

C. Capital locations

We now take a closer look at the political geography determinants of capital locations within regions and provide some descriptives on which cities are likely to become capitals within a new administrative region.

We take our inspiration from Bai and Jia (2020), who propose that central government planners in historical China face a trade-off when determining the location of capital cities. Being close to citizens implies that the location can efficiently exercise control (levy taxes and provide services at a low cost). Proximity to the national capital, in turn, makes the local administration more accountable to the national government and minimizes the cost of delivering local taxes to the central government (for similar arguments see Bardhan and Mookherjee, 2000; Campante and Do, 2014). The optimal solution to this problem minimizes a location's 'hierarchical distance': the distance to all citizens within a province and the national capital (with some weight on either objective). Of course, other factors are likely to play a role in these location decisions today, which is why we consider a range of additional variables from proximity to the coast to the size distribution of cities in the initial region.

Panels A to C of Figure C-1 provide some evidence in favor of the idea that hierarchical distance also matters in our global sample of contemporary capital city reforms. We rank cities within regions with respect to their distance to the region centroid in panel A, their distance to the population-weighted centroid in panel B, or their distance to the national capital in panel C. In all three cases, cities that occupy lower ranks (are closer) are considerably more likely to become a capital when a region is split. Panel D adds the proximity to the coast as a proxy for the external trade orientation and documents a similar pattern. Note that we find a few outliers in all cases where high ranks have a high probability of becoming a capital. This is due to a handful of vast regions in South Asia in which relatively "remote" cities (by global standards) are capitals.

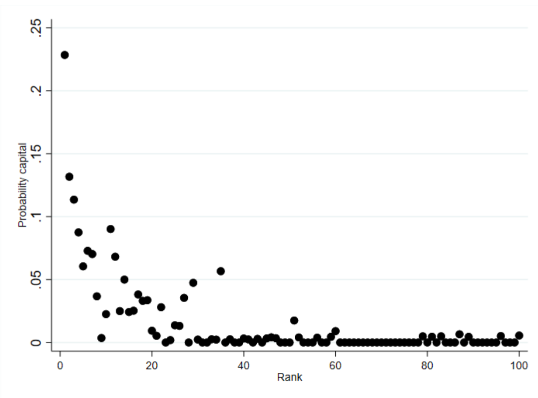
Finally, we examine initial size, either based on population or light density, as a predictor of gaining the status as a regional capital.[1] Panel E shows a strong relationship between the initial size of a city and the probability of becoming the region's capital. The largest city in a region is also the region's capital in almost 60% of the cases, the second-largest city in around 17% of cases, while the chances of being a capital for the third and fourth-largest cities are in the single digits. Cities that rank five or higher have an average probability below 1%. The relationship wakens if we rank by initial light, where the decline in the probability is smooth, and the largest city becomes the capital in only 30% of cases (panel F).

[1]Note that the largest city does not minimize the distance to all citizens by definition, although there is a high correlation 0.64.

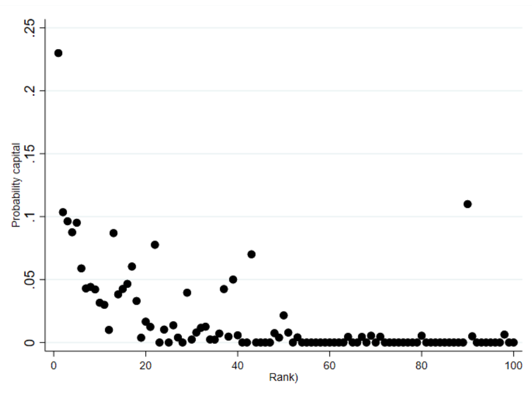
Figure C-1

Determinants of capital locations within regions: City ranks

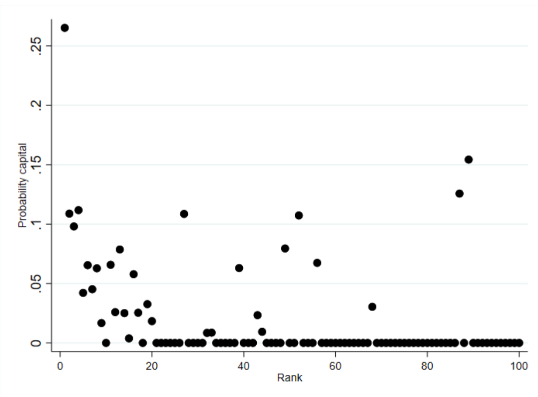
(a) Proximity region centroid



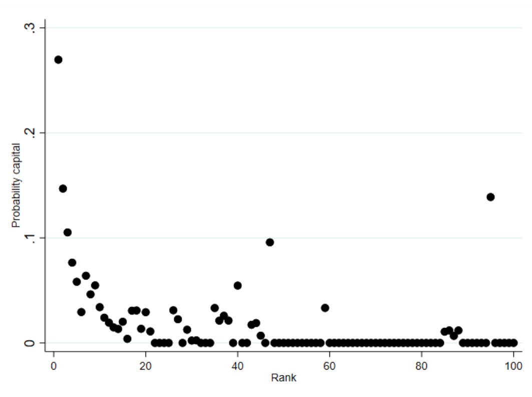
(b) Proximity population centroid



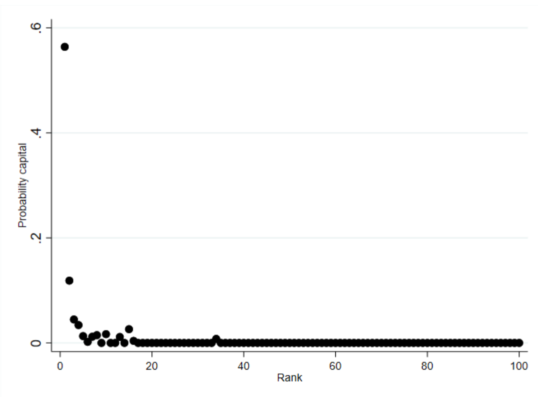
(c) Proximity national capital



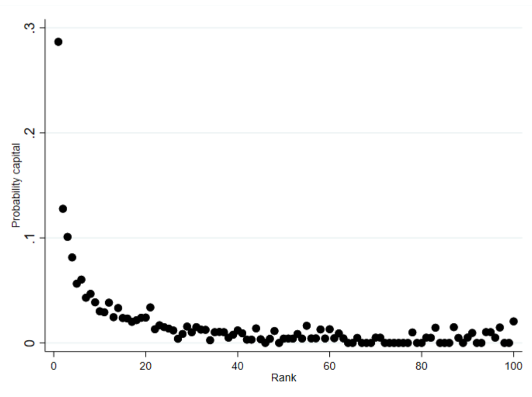
(d) Proximity coast



(e) Initial size (population)



(f) Initial size (light)



Notes: This figure shows scatter plots of the average probability that a city becomes a capital across over the distribution of city characteristics along various dimensions. Panel A ranks cities in terms of proximity to the regional centroid. Panel B ranks cities with respect to proximity to the population-weighted centroid of a region. Panel C uses the proximity to the national capital and panel D the proximity to coast. Panels E and F rank cities based on their initial size, either by population or light density.

D. Selection issues: City detection

Throughout the main text, we focus on the cities that we were able to detect in 1990. We then analyze changes in the core and in the larger agglomeration, including new developments in these cities from 1990 until 2015. Defining the sample of cities avoids a sample selection issue that we illustrate in more detail in this appendix.

The selection effect arises since the status of a city as a subnational capital also influences the detection likelihood in 2015. Our main result is that cities grow faster once they gain capital city status. Recall that we only observe urban boundaries at two points in time (1990 and 2015). If a small city becomes a subnational capital in the interim and grows faster as a result, it is more likely to cross our detection thresholds and classified as an urban cluster (or city) in 2015. Suppose we track light density (or other outcomes) in these cities over the entire period, even though they are only detected later. In that case, we include this dynamic selection bias and, with that, the possibility of pre-trends.

We design a simple test to illustrate this selection effect. We regress the change in status from 1990 to 2015 on the share of years a city is a subnational capital during the same period. The change in status is the first difference of a binary variable indicating whether a city was detected in a particular year in the union of urban clusters found in either in 1990 or 2015. Table D-1 reports the results from several specifications, where we incrementally add country and initial-region fixed effects for our two samples. Columns 1 to 3 show that a city that becomes a capital halfway through the period from 1990 to 2015 has a 7.3 to 11.8 percentage points higher probability of being detected in 2015. The estimated effect sizes are smaller for the sample of cities in reformed regions, but the overall pattern remains the same. Obtaining the status as a first-order capital during the sample significantly increases the likelihood of detection in 2015.

TABLE D-1
City detection probability

	Dependent Variable: Δ DETECTED$_{ci}$					
	All Cities			Reformed Districts		
	(1)	(2)	(3)	(4)	(5)	(6)
CAPITAL	0.1453	0.2200	0.2369	0.1373	0.1610	0.1734
	(0.0382)	(0.0381)	(0.0413)	(0.0513)	(0.0480)	(0.0477)
Fundamentals	✓	✓	✓	✓	✓	✓
Country FE	–	✓	✓	–	✓	✓
Initial-Region FE	–	–	✓	–	–	✓
City-unions	28006	28006	28006	10889	10889	10889

Notes: The table reports results from a regressions of the change in detection status of a city between 1990 and 2015 on the fraction of years in which a city is a capital. Standard errors clustered on initial regions are provided in parentheses.

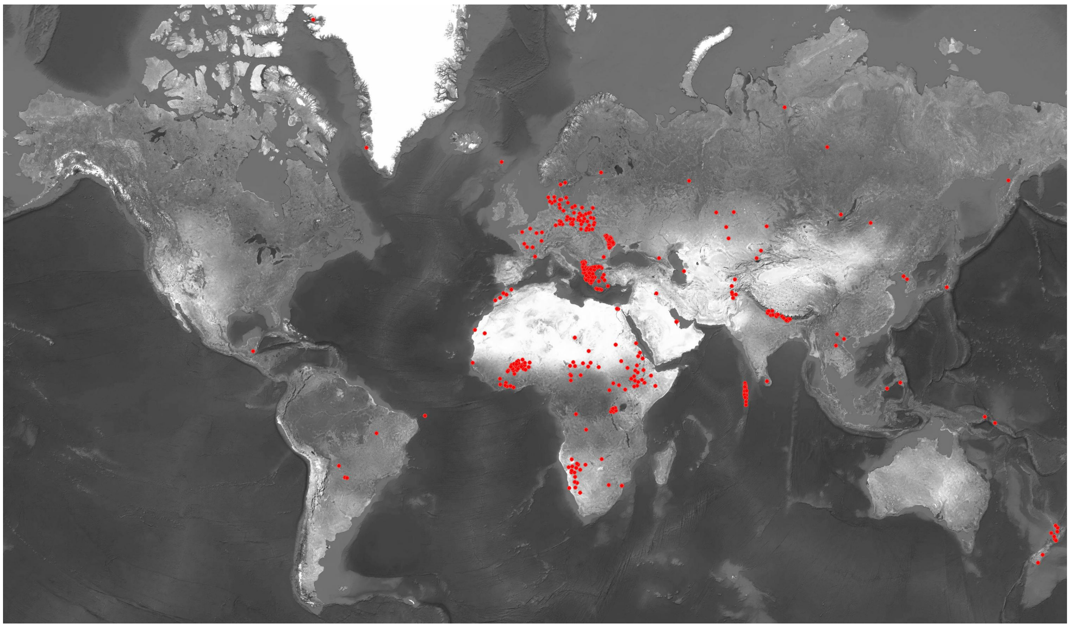
E. Capital loss

This appendix provides descriptive statistics on cities that lose their status as a capital, discusses pre-treatment trends, and the appropriate comparison groups for these cities. We also report evidence on the effects of cities which lose this political status relative to their peers (cities that remain capitals).

E-1. Former capitals

Many cities across the globe have lost the status of as a capital during the last three decades (see Figure E-1). About 94% of the observed 169 status losses in our sample occur during a territorial centralization (mergers of two or more regions). In the other 6% of cases, a different city becomes a capital within the same region.

FIGURE E-1
Spatial distribution: Capital loss

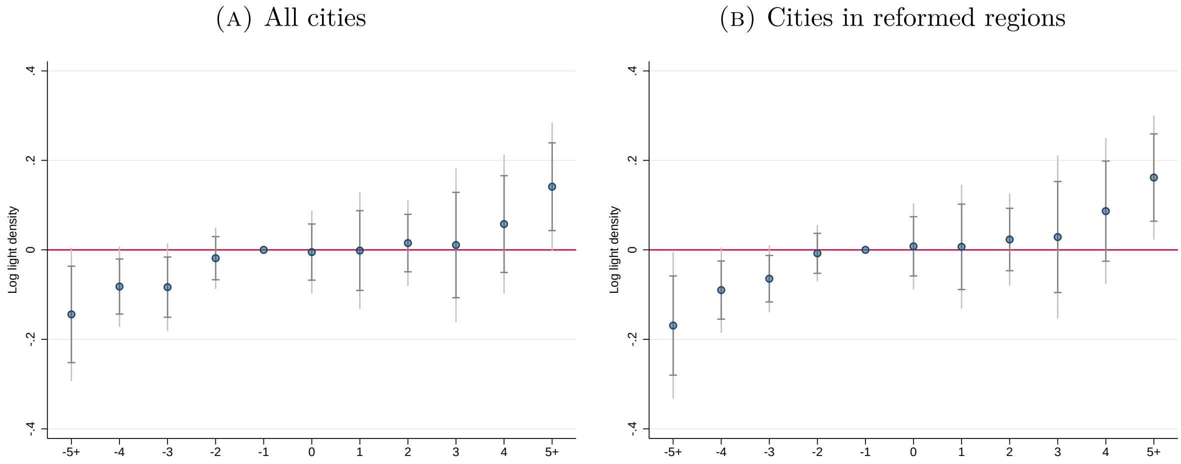


Notes: The figure plots all the cities that lose their capital status during the 1987 to 2018 period.

We first turn to our baseline specification which uses other non-capital cities as the control group. Figure E-2 reports event-study estimates using our preferred specification with initial-region-by-year fixed effects and controls for locational fundamentals. There are significant and negative pre-trends. Capital cities that lose their status perform worse relative to non-capitals prior to treatment. Regardless of why this occurs, identification is not feasible in our primary setting.

Of course, it makes more sense economically and statistically to compare capitals that lose their status to cities that remain capitals. Unfortunately, this also implies that we now work with a drastically reduced sample size (of 392 capital cities) and a design that

Figure E-2
Former capitals vs. all cities



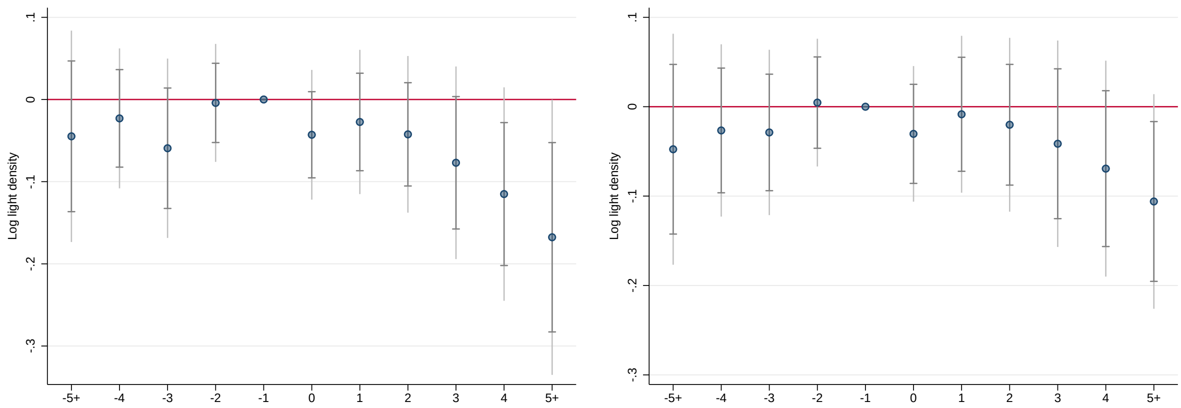
Notes: The figure illustrates results from fixed effects regressions of the log of light intensity per square kilometer on the binned sequence of treatment change dummies (capital loss) defined in the text. Panel A shows estimates for all ever capital cities based on a specification with country-year effects. Panel B shows estimates for ever capital cities in reformed regions based on a specification with final-region-by-year fixed effects. All regressions include city fixed effects. 95% confidence intervals based on standard clustered on final provinces are provided by the gray error bars.

more closely resembles a staggered event study with a small control group. Moreover, we do not have enough degrees of freedom to allow for time-varying coefficients on the locational fundamentals. In Figure E-3 we run event studies on the set of ever capitals using again binned treatment change indicators for city loss. Note that we exclude cities that become capitals during our sample period. Hence, the comparison groups differ a lot in comparison to our standard approach. The identifying variation in panel A is based on the difference between cities that are always capitals within the country compared to capitals that lose that status sometime during our sample period. The identifying variation in panel B is restricted to mergers of administrative regions in which one city loses the status and the other city becomes the capital of the whole region. Note that focusing on mergers has also implications for the type fixed effects we can include. Instead of initial-region-by-year fixed effects, we now use final-region-by-year fixed effects. This allows us to compare cities within the at some point merging region and control for unobserved trends in the constituent parts prior to their merger.

The results show a clear pattern. We find no evidence suggesting the presence of pre-trends. Hence, capitals that will subsequently lose their political status are not declining relative to always capitals prior to treatment. After the political status is remove, we observe a steady loss of economic activity that takes longer to materialize than our main result but suggests a decline of similar magnitude in the medium-run.

FIGURE E-3
Former capitals vs. always capitals

(A) All cities (B) Cities in reformed regions FEs



Notes: The figure illustrates results from fixed effects regressions of the log of light intensity per square kilometer on the binned sequence of treatment change dummies (capital loss) defined in the text. Panel A shows estimates for all ever capital cities based on a specification with country-year fixed effects. Panel B shows estimates for ever capital cities in reformed regions based on a specification with final-region-by-year fixed effects. All regressions include city fixed effects. 95% confidence intervals based on standard clustered on final provinces are provided by the gray error bars.

E-2. "Mother" capitals and regions

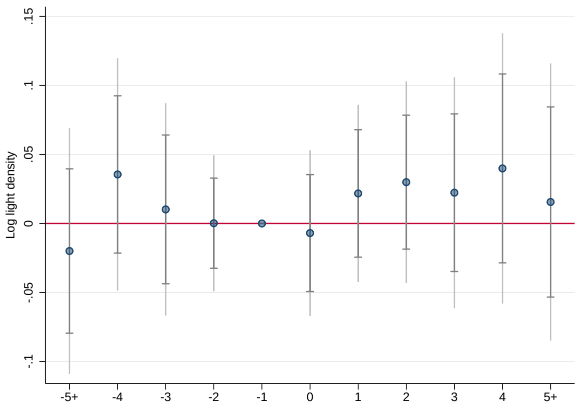
A related issue to the loss of a political premium is territorial decentralization's effect on existing capitals that lose part of their territory. We refer to these cities as "mother capitals", i.e., capitals that rule over a smaller jurisdiction after a decentralization reform that creates new additional capitals in the initial region.

We specify the corresponding event for capitals that experience a reduction in their jurisdiction and estimate event studies comparing their performance to the set of always capitals. Figure E-4 presents the results. We find no evidence in favor of pre-treatment trends or any change in activity after a city becomes a "mother capital". The economic gains of new capital cities appear not to come at the cost of the old ones, at least not in the short to medium run.

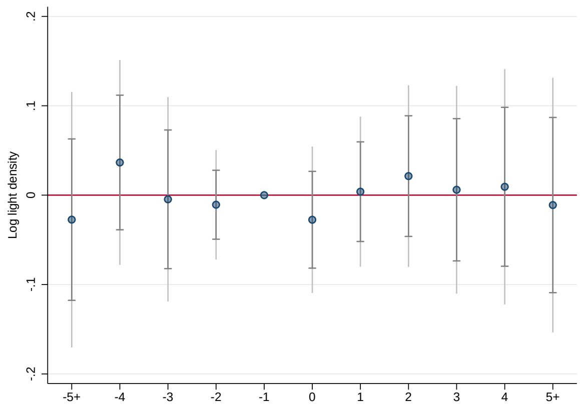
FIGURE E-4

Mother capitals vs. always capitals

(A) All cities



(B) Cities in reformed regions FEs



Notes: The figure illustrates results from fixed effects regressions of the log of light intensity per square kilometer on the binned sequence of treatment change dummies (mother capitals). Panel A shows estimates for all ever capital cities based on a specification with country-year fixed effects. Panel B shows estimates for ever capital cities in reformed regions based on a specification with initial-region-by-year fixed effects. All regressions include city fixed effects. 95% confidence intervals based on standard clustered on initial provinces are provided by the gray error bars.

F. Data Appendix

F-1. Remotely-sensed data

Light density: We calculate our light density measures by taking the average value per pixel within the year (to mitigate between multiple satellites) and then summing the average pixel values across our city shapes before dividing by the city area. The luminosity data is based on the raw NOAA data of the OLS-DMSP (stable light) product https://sos.noaa.gov/catalog/datasets/nighttime-lights/. The bottom correction is implemented following Storeygard (2016) and the top coding correction following Bluhm and Krause (2018).

Population: density within our cities is calculating by first taking the sum of population based on the Global Human Settlement Layer Raster (GHSL R2018A/2019A) and then dividing by the area of our cities (https://ghsl.jrc.ec.europa.eu).

Ruggedness: We calculate average ruggedness within 25km of our cities by taking the average pixel value of the ruggedness raster provided by Diego Puga (available at https://diegopuga.org/data/rugged/ Nunn and Puga (2012)).

Malaria suitability: Malaria Ecology Index from Kiszewski et al. (2004) in raster format for GIS (https://sites.google.com/site/gordoncmccord/datasets?authuser=0).

Market access: Own calculation based on GHSL raster. See main text for details.

River within 25km: We generate a dummy for all cities located within 25km of river, based on our city coordinates and river shapes from Natural Earth 1:10m grid version 4.1.0 (https://www.naturalearthdata.com/downloads/10m-raster-data/).

Lake within 25km: We generate a dummy for all cities located within 25km of a lake, based on our city coordinates and "lake centerlines" from Natural Earth 1:10m grid version 4.1.0 (https://www.naturalearthdata.com/downloads/10m-raster-data/).

Port within 25km: We generate a dummy for all cities located within 25km of a port, based on our city coordinates and port locations obtained from the World Port Index 2010 (https://msi.nga.mil/Publications/WPI).

Coast within 25km: We generate a dummy for all cities located within 25km of the coast, based on our city coordinates and the coastlines from from Natural Earth 1:10m grid version 4.1.0 (https://www.naturalearthdata.com/downloads/10m-raster-data/).

Precipitation: Average precipitation is calculated within 25km buffers of our city coordinates, we average yearly values from Jan 1990 to Dec 2014 from the monthly totals. The precipitation data is obtained from NOA (Version 4.01 https://psl.noaa.gov/data/gridded/data.UDel_AirT_Precip.html).

Elevation: Average elevation within a 25km buffer of the city is calculated based on the SRTM Version 4.1 raster (Jarvis et al., 2008).

Temperature: Average temperature is calculated for 25km buffers around our cities. We use the average temperature from Jan 1990 to Dec 2014 from the monthly totals as inputs, which is obtained from NOA (Version 4.01 https://psl.noaa.gov/data/gridded/data.UDel_AirT_Precip.html).

Wheat suitability: Average wheat suitability is calculated for 25km buffers around our city coordinates. The wheat suitability values are obtained from the FAO GAEZ Agro-climatically attainable yield for intermediate input level rain-fed wheat for the baseline period 1961-1990 at 5 arc minutes.

Built-up: We calculate built-up and vegetation measures using the entire archive of Landsat images from 1987 until 2018, available at a resolution of 30m from Landsat 5 and 7 in *Google Earth Engine*. The measures are based on spectral bands, denoted by ρ_x, and calculated as follows: $NDBI = (\rho_{SWIR1} - \rho_{NIR}) / (\rho_{SWIR1} + \rho_{NIR})$, $UI = (\rho_{SWIR2} - \rho_{NIR}) / ((\rho_{SWIR2} + \rho_{NIR})$, and $NDVI = (\rho_{NIR} - \rho_{red}) / (NIR + red)$. Prior to calculation we extract the average values of cloud free images of the Landsat input as is standard in such calculations.

F-2. DHS data

DHS wealth index: is taken directly from the DHS surveys (v190). In general the DHS describes their wealth index as being: "...a composite measure of a household's cumulative living standard. The wealth index is calculated using easy-to-collect data on a household's ownership of selected assets, such as televisions and bicycles; materials used for housing construction; and types of water access and sanitation facilities."(https://www.dhsprogram.com/topics/wealth-index/wealth-index-construction.cfm). Note that the specific assets considered are country dependent.

Electricity indicator: is an indicator variable for the availability of electricity in the household (V119).

Save water indicator: is a indicator variables set to unity if the respondent household has access to either: protected wells or springs, boreholes, packaged water, and rainwater (v113) (see Henderson and Turner, 2020, for a similar classification).

Improved sanitation indicator: is a indicator variable equaling unity if the respondent household has access to either shared or non-shared faculties that flush/pour to piped sewer systems, septic tank, pit latrine; ventilated improved pit latrine, pit latrine with slab and compositing toilets, as well as flushing to unknown locations (v116). Again we follow Henderson and Turner (2020) who follow the DHS-WHO joint monitoring program.

More than 8 years of schooling indicator: Is a dummy variables being unity if the respondent has completed more than 8 years of schooling. The variables is based on the year of schooling variable from the DHS surveys (V107).

Infant mortality: is defined as the probability of dying before the first birthday. The corresponding rate is normalized as a ratio per 1000 live births. The variable is constructed based on the "age at death" responses about the individuals children female respondents who have children provide (variables b13-1 to b13-20). As common in the literature we use the individual level data and multiply the child mortality dummy by 1000.

Table F-1
DHS survey sample

ISO	Interview year	Respondents	urban	female
AGO	2006	163	91	100
AGO	2007	78	100	100
AGO	2011	3548	93	100
ALB	2008	506	96	71
ARG	2008	171	100	73
BDI	2010	649	72	68
BDI	2011	1654	99	61
BDI	2012	1464	96	100
BDI	2013	32	100	100
BEN	1996	641	100	78
BEN	2001	1997	90	69
BEN	2011	1898	97	77
BEN	2012	2649	96	77
BFA	1992	2021	100	77
BFA	1993	1693	68	78
BFA	1998	222	100	66
BFA	1999	7	100	29
BFA	2003	2440	98	76
BFA	2010	3452	97	68
BFA	2014	1730	100	100
BOL	2008	7358	99	75
BRA	2008	23	100	74
CAF	1994	313	27	78
CAF	1995	238	46	82
CIV	1994	4771	88	76
CIV	1998	1169	100	79
CIV	1999	940	100	76
CIV	2011	1822	100	67
CIV	2012	2412	100	68
CMR	1991	2772	76	83
CMR	2004	5224	98	67
CMR	2011	7456	99	68
COD	2007	10028	98	69
COD	2013	5768	99	70
COD	2014	516	93	72
COL	2010	18276	99	100
DOM	2007	15403	96	52
DOM	2013	6292	96	51
EGY	1992	4713	83	100
EGY	1995	6595	74	100
EGY	2000	7228	78	100
EGY	2003	3452	81	100
EGY	2005	7661	75	100
EGY	2008	6975	71	100
EGY	2014	8532	77	100
GHA	1993	1354	88	80

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Table F-1 – *Continued from previous page*

ISO	Interview year	Respondents	urban	female
GHA	1994	98	100	89
GHA	1998	930	96	75
GHA	1999	620	88	76
GHA	2003	2515	94	55
GHA	2008	2645	95	53
GHA	2013	35	100	66
GHA	2014	3710	99	69
GIN	1999	2149	97	75
GIN	2005	2004	95	65
GIN	2012	3275	98	69
HND	2011	2483	99	80
HTI	2000	3378	89	81
HTI	2006	2879	91	73
HTI	2007	109	100	56
HTI	2012	7020	95	63
IDN	2003	2385	98	77
KEN	2003	4002	95	82
KEN	2008	1087	93	72
KEN	2009	557	100	69
KEN	2014	8308	99	70
KGZ	2012	2210	81	80
LBR	2006	149	71	52
LBR	2007	2822	100	57
LBR	2008	792	100	100
LBR	2009	167	100	100
LBR	2011	778	100	100
LBR	2013	1673	100	71
LSO	2004	443	87	74
LSO	2005	89	100	73
LSO	2009	354	100	73
LSO	2010	206	100	73
LSO	2014	753	93	71
MAR	2003	3854	99	100
MDA	2005	2969	100	76
MDG	1997	1216	96	100
MDG	2008	1906	95	70
MDG	2009	137	100	70
MDG	2011	95	34	100
MDG	2013	172	58	100
MLI	1995	1062	100	80
MLI	1996	1121	100	79
MLI	2001	2869	100	78
MLI	2006	3177	100	76
MLI	2012	2758	100	72
MLI	2013	505	87	67
MOZ	2009	3256	99	100
MOZ	2011	4946	99	77
MWI	2000	835	97	80

Continued on next page

Table F-1 – *Continued from previous page*

ISO	Interview year	Respondents	urban	female
MWI	2004	270	100	75
MWI	2005	345	100	74
MWI	2010	1211	100	74
MWI	2012	828	100	100
MWI	2014	650	100	100
NER	1992	2201	47	81
NER	1998	1195	92	69
NGA	1990	5194	57	100
NGA	2003	3168	87	75
NGA	2008	10456	89	67
NGA	2010	1481	85	100
NGA	2013	13150	90	68
PAK	2006	2462	93	100
PER	2000	10256	99	100
PER	2004	3794	100	100
PER	2009	8710	99	100
PHL	2003	5862	96	76
PHL	2008	3241	97	100
RWA	2005	1128	94	69
RWA	2008	1174	96	47
RWA	2010	197	31	69
RWA	2011	1532	92	68
RWA	2014	113	40	73
SEN	1992	817	82	80
SEN	1993	1584	80	81
SEN	1997	3752	94	67
SEN	2005	11160	88	77
SEN	2008	8782	85	100
SEN	2009	1424	89	100
SEN	2010	2831	87	75
SEN	2011	2940	92	75
SEN	2012	998	93	100
SEN	2013	1080	88	100
SLE	2008	2799	99	70
SLE	2013	4732	100	69
TCD	2014	2155	98	71
TGO	1998	3441	93	70
TGO	2013	2328	98	70
TGO	2014	1530	96	69
TJK	2012	2096	90	100
TZA	1999	1395	100	54
TZA	2003	331	100	100
TZA	2004	2053	97	94
TZA	2007	952	95	100
TZA	2008	844	96	100
TZA	2009	408	100	82
TZA	2010	1227	98	81
TZA	2011	463	96	100

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Table F-1 – *Continued from previous page*

ISO	Interview year	Respondents	urban	female
TZA	2012	1941	95	100
UGA	2000	473	94	77
UGA	2001	414	100	81
UGA	2006	1231	100	79
UGA	2008	31	100	77
UGA	2009	860	94	100
UGA	2011	6756	94	89
UGA	2014	501	96	94
ZMB	2007	2416	100	52
ZMB	2013	4613	96	53
ZMB	2014	2186	100	55
ZWE	1999	1431	100	68
ZWE	2005	2729	98	56
ZWE	2006	228	89	53
ZWE	2010	1905	97	56
ZWE	2011	1018	100	58

Notes: The table depicts the DHS survey included in our sample. The survey years the number of respondents in each suvery that we can match to our data as well as the percentage of urban dwellers and female respondents within each DHS survey.

F-3. Investment data

Development aid (World Bank): Development aid provided by the World Bank is obtained AidData (2017). This geocoded dataset includes all projects approved from 1995-2014 in the World Bank IBRD/IDA lending lines. It tracks more than $630 billion in commitments for 5,684 projects across 61,243 locations. We construct several aid variables following the sectoral classification. The sectoral classification are in order; Education, health, water supply & sanitation, government and civil society, other social infrastructure & services, economic infrastructure and services, agriculture forestry and fishing, industry and mining and construction, and environmental protection. They correspond to the broadest classification of the project types provided by the World Bank. Note that any project can have multiple (up to 5) project classifications. In such cases the same project appears under multiple headings.

Development aid (China): Development aid like financial flows for China are obtained from AidData's Geocoded Global Chinese Official Finance Dataset, Version 1.1.1. (Bluhm et al., 2018). This dataset geolocates Chinese Government-financed projects that were implemented between 2000-2014. It captures 3,485 projects worth $273.6 billion in total official financing. The dataset includes both Chinese aid and non-concessional official financing. We construct several aid variables following the sectoral classification. The sectoral classification are in order; Education, health, water supply & sanitation, government and civil society, other social infrastructure & services, economic infrastructure and services, agriculture forestry and fishing, industry and mining and construction, and environmental protection. They correspond to the broadest classification of the project types provided by the World Bank. Note that any project can have multiple (up to 5) project classifications. In such cases the same project appears under multiple headings.

FDI: The raw data for our FDI outcomes (dummy, log investment value, and log estimated jobs) comes from the fDi Markets database (https://www.fdimarkets.com) a service provided by the Financial Times group. The database contains in detail information on FDI projects across the world for the period 2003 until 2018, including information about the investing company the origin country the company is based and much more. Important for us the database has the estimated jobs created the value spend, the host city name and if the project is a greenfield investment. We geocoded the projects using the same OSM algorithm we employed for the location of the capital cities using the host city information. In a next step we match the FDI to our cities if the projects host city (which do not need to meet any population threshold) fall within a 10km buffer of our detected cities. Finally, we summarize the invested dollar value and the estimated jobs by the host city location and take the logs of them. Note that we only gathered data for our reformed areas, since the terms of use only allow us to use 10% of their sample. The data is then aggregated to the NAICS 2 digit level. The 2 digits NAICS classification we use are in order: Agriculture, Forestry, Fishing and Hunting; Mining, Quarrying, and Oil and Gas Extraction; Utilities ; Construction; Manufacturing; Wholesale Trade; Retail Trade; Transportation and Warehousing; Information; Finance and Insurance; Real Estate and Rental and Leasing; Professional, Scientific, and Technical Services; Administrative and Support and Waste Management and Remediation Services; Educational Services; Health Care and Social Assistance; Arts, Entertainment, and Recreation; Accommodation and Food Services; Public Administration.

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