The Political Geography of Cities*

Capital cities, economic fundamentals, and urban growth

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Abstract

We study the link between subnational capital cities and urban development. We exploit new data on hundreds of first-order administrative and capital city reforms from 1987 until 2018 in an event study framework to estimate the capital city premium. We show that gaining subnational capital status has a substantial impact on city growth in the medium term and spills over to nearby cities. We provide new evidence that the capital premium varies with the size of the territory governed by the city and that political status complements favorable economic fundamentals. We consider two explanations for this advantage. First, we show that more educated individuals tend to migrate to capital cities. Second, we find evidence of increased public and private investment in capital cities. Moreover, private investment tends to favor capital cities with stronger economic fundamentals. Our findings provide insights into the role of capital cities in promoting regional development.

Keywords: Capital cities, administrative reforms, economic geography, urban primacy

JEL Classification: H10, R11, R12, O1

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1. Introduction

Subnational capitals are often the fastest-growing cities in their administrative regions and countries. For instance, in Brazil and India, the average capital city has grown 1.6 and 2.6 times faster than non-capital cities over the past three decades. In contrast, the United States shows no noticeable difference in growth rates between capital and non-capital cities. However, certain U.S. capitals, such as Austin in Texas, have experienced faster growth than most other cities, while others, such as Montpellier in Vermont, have stagnated and become economically irrelevant. This raises two related questions: Does a change in political status bring about faster urban growth? And if so, why do we observe such differences in the relative growth rates of politically important cities?

In this paper, we investigate how a change in a city's capital status affects its growth rate relative to other cities. We take advantage of recent decentralization reforms in developing and developed countries, which provide a quasi-experimental setting to estimate the capital city premium.[1] Our analysis focuses on urban growth in a global sample of cities. We collect data on hundreds of administrative reforms that lead to changes in a city's capital status. Using these data, we test *i)* whether new capitals increase density and attract more economic activity to a location and its surroundings, and *ii)* whether the capital premium varies with two margins of the reform decision: the size of the new region (a measure of political importance) and the fundamentals of the capital's location (a measure of economic potential). The answers have important policy implications. They tell us whether capitals influence regional growth patterns and to which degree the political status of a city complements or substitutes economic fundamentals. More generally, they provide insights into the circumstances under which capitals can shift economic activity toward their location and promote local or even regional development.

To examine these questions, we compile comprehensive data on cities and data on whether administrative reforms treated them. Using Geographic Information Systems (GIS) and a plethora of sources, we first catalog 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 capital status. We measure annual variation in economic activity at the city level using nighttime light intensity (similar to Storeygard, 2016; Campante and Yanagizawa-Drott, 2017; Henderson et al., 2018). To capture how attractive particular locations are, we compile an array of geographic characteristics for the greater area

[1] Grossman and Lewis (2014) document a trend towards administrative unit proliferation in sub-Saharan Africa. Our data show that this pattern holds globally at the highest level of subnational government.

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 short and medium-run 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 capital status of cities, which we compare to other (untreated) cities within a reformed region. While the choice to reform a particular region and promote or demote a city to a subnational capital is seldom random, we document that the timing of these reforms is usually unrelated to 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 typically unrelated to local conditions at the city level (see, e.g., Bai and Jia, 2021, or Düben and Krause, 2021, on location choices in Imperial China). To strengthen this approach and minimize the scope for dynamic selection into treatment, we focus on the effect of gaining the status of a subnational capital for the first time, typically when regions are split. Our design allows us to separate the potentially endogenous decision to split a region, which we control for with fixed effects, from the treatment of designating a new regional capital (similar to Bazzi and Gudgeon, 2021, who study district splitting and conflict). Testing for pre-trends suggests that the identifying assumptions hold in the sample of cities in reformed regions, the larger sample of all cities, and in matched samples where we only use a subset of large cities as controls. The pattern of leads and lags shows no anticipatory increases in activity but a substantial effect following the reform. It is inconsistent with unobserved shocks driving our results and robust to using estimation approaches of the event study model that allow for heterogeneous effects across cohorts of cities treated at different points in time. We also find no evidence suggesting that heterogeneous selection on observed fundamentals is driving our results. In other words, cities that become capitals but are capitals of larger administrative regions or in locations with better market access are not on a different growth path compared to other cities in the same initial region prior to the reform.

Our analysis establishes two main findings. First, cities elevated to subnational capitals enjoy significant advantages that persist in the medium run and spill over to nearby cities. Our data indicate that economic activity, as proxied by light intensity, in new capital cities increases by 15.7–25% five years after a reform, depending on the specification. The event-study estimates suggest that it takes about two years for these effects to materialize and then gradually increase until five years after the change in status. Furthermore, the benefits of being proximate to a capital city extend to both the larger agglomeration and surrounding cities. We find that cities within 100 km of the new capital benefit positively, while there is no evidence of negative spillovers to non-capital cities. Second, we find that the capital city premium varies based on the size of the territory governed by the new capital city and that political status complements favorable locational fundamentals. Our analysis shows that locating subnational capitals in areas with better economic fundamentals, particularly better internal market access, has a larger impact on city growth than locating them in areas suitable for agriculture. This indicates that politics plays a powerful role in shaping the location of economic activity but also that policymakers are constrained in using administrative reforms to shift economic activity toward the hinterland. Our additional results show that the capital premium dissipates slowly after cities lose their elevated status.

We use long-difference specifications to better understand the effects of capital status on economic activity. We find that increases in population density account for about two-thirds of the effect of increases in light intensity. The evidence for per capita effects is weaker and subject to measurement error. We also find a modest increase in urban built-up (i.e., housing and infrastructure). To validate these results and to gain more insight into the effects of political status on individual-level and household-level outcomes, we compare residents of capital cities with those of non-capital cities using data from the Demographic and Health Surveys (DHS). We find that residents of capital cities have higher asset accumulation, better access to electricity, higher educational attainment, and lower infant mortality than residents of other comparably dense cities in the same region. This pattern is robust to the exclusion of recent migrants to capital cities, except for educational attainment, where selective migration plays a larger role. Together with the evidence of spillovers, these results suggest that the investments associated with cities becoming capitals promote local and regional development more broadly.

We examine two main mechanisms to explain the advantages of capital cities. First, using microdata from recent migrants to cities that become new regional capitals, we provide direct within-city evidence of selective migration to new capital cities. Specifically, more educated individuals migrate to capital cities after they gain subnational capital status. Second, we find evidence of increased public and private investments in capital cities. Public investments in water and sanitation, infrastructure, and government are larger in capitals than in comparable cities. Private investments, on the other hand, are concentrated in finance and insurance, manufacturing, and other productive sectors in capital cities. We then test whether these investments are allocated differently based on economic fundamentals across capital and non-capital cities. Our results show that private investors favor capital cities with stronger economic fundamentals over similar cities without political status. Public investments also respond to the economic fundamentals of a location and its capital status but not to their combination. Thus, private investments contribute to the complementary relationship between political status and economic fundamentals. This effect is especially relevant for developing countries, where state capacity is scarce and typically concentrated in a few politically important cities.

Our finding that the political status of a city affects where economic activity occurs relates to a seminal literature in urban economics which has established that urban concentration changes over the process of development and is shaped by politics (Ades and Glaeser, 1995; Davis and Henderson, 2003). We show that capital city reforms can shift activity within an initial region and the entire country. To the best of our knowledge, our study is the first to document the effects of capital cities on the concentration of economic activity over a policy-relevant time frame in a global sample of cities. This differentiates our contribution from the nascent literature on subnational capitals. For example, isolated state capitals in the US are associated with more corruption, less accountability, and lower public good provision (Campante and Do, 2014), but we lack broader evidence on how location interacts with urban growth.[2] In related studies, Bai and Jia (2021) and Chambru et al. (2021) study the effects of subnational capitals on population growth in Chinese prefectures from 1000 to 2000 CE and French municipalities after the French Revolution. They observe long-run increases in population, public goods, and connectivity, while the latter also document a lack of short-term growth in the new departmental capitals of France. Our focus is different. Beyond establishing that capitals grow faster, we study why the capital premium is higher or absent in some cities and show that the characteristics of a location matter for whether administrative cities grow in the short and medium run. Moreover, both studies focus on highly centralized countries with a remarkable administrative capacity throughout their history, leaving open the question of whether this extends to developing countries today—many of whom are more decentralized but have limited state capacity. We highlight that the capital city premium is about twice as large in decentralized countries than in more centralized countries and occurs primarily in the developing world.

Our finding that economic fundamentals, such as internal market access, play an important role in capital city growth supports a central premise of economic geography. 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; Davis and Weinstein, 2002; Miguel and Roland, 2011; Bleakley and Lin, 2012; Michaels and Rauch, 2018). We add that granting capital status to cities in locations with good fundamentals can facilitate agglomeration in more productive locations. However, the capital premium dissipates slowly and does not persist once the status is lost. A related literature is concerned with whether fundamentals, sorting, or learning drive the large productivity gains observed for workers in bigger cities (see, e.g., Glaeser, 1999; Combes et al., 2008; de la Roca and Puga, 2017). We provide new evidence that migrants with

[2] Campante et al. (2019) add that isolation can also shelter autocrats from insurrection, which is precisely why they sometimes relocate *national* capitals to the hinterland.

higher initial educational attainment sort into capital cities.

Our results are also relevant to debates about what policies can be used to address spatial inequality (see, e.g., Lessmann, 2014; Kessler et al., 2011; Baum-Snow et al., 2020). Henderson et al. (2018) shows that city locations in developed countries tend to have been more influenced by agricultural characteristics and exhibit a more balanced distribution of economic activity than late agglomerators. We add a policy-relevant margin to this finding. Although countries that began to agglomerate late exhibit higher spatial concentration today, decentralizing their administrative structure can influence the location of economic activity. However, our results suggest that a key constraint for policymakers is the size of the administrative regions. Our finding of complementarity between politics and fundamentals also speaks to the literature on (optimal) place-based policies (Glaeser and Gottlieb, 2008; Kline, 2010; Neumark and Simpson, 2015; Rossi-Hansberg et al., 2019; Fajgelbaum and Gaubert, 2020). This literature has produced mixed empirical results, but theory suggests that optimal place-based policies can improve aggregate welfare. In the US, for example, such a policy would reduce congestion in the largest cities but concentrate workers with higher human capital in hubs that are well connected to other cities (Rossi-Hansberg et al., 2019). Thus, our findings that capital cities attract skilled migrants and that capital cities with high market access grow faster suggest that they can play a role in relocating workers to where they will be most productive.

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. An extensive list of single-country studies focuses on the diverse impacts of administrative reforms and capital cities but sparse international evidence of this phenomenon. Second, leveraging large amounts of remotely-sensed data allows us to focus directly on cities rather than administrative regions, which change due to territorial reforms. Third, taking a global perspective enables us to ask different questions over a shorter time frame. The heterogeneity in fundamentals and national contexts allows us to exploit variation usually unavailable 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 treatment heterogeneity with respect to two margins of the reform decision (the size of the new division and the location of the capital). Section 6 unpacks the light results, and Section 7 mechanisms. Section 8 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 are 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. Online Appendix A provides a complete overview of the sources, variables, their coverage, and summary statistics.

A. Capital city reforms

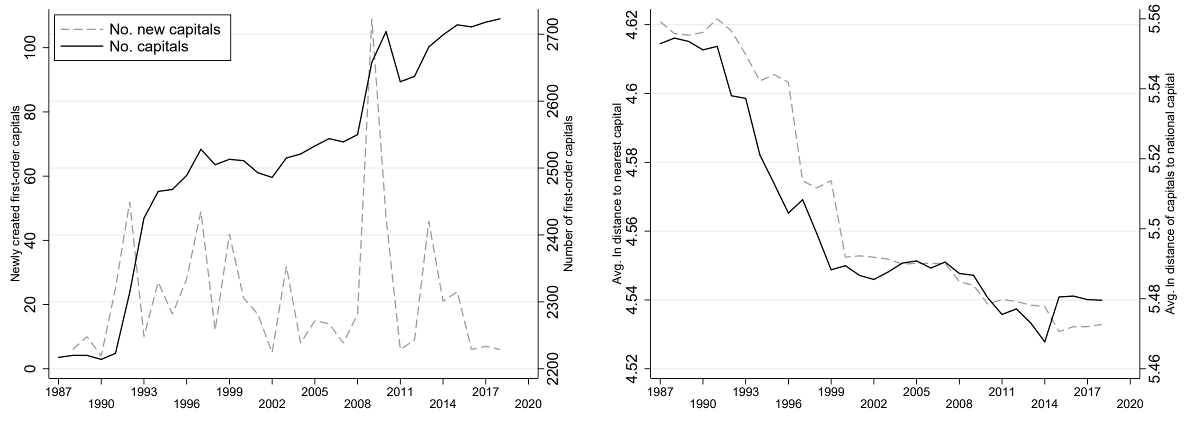
No off-the-shelf data systematically record administrative reforms, the boundaries of administrative units, and the location of capital cities across the world.[3] We compile new data containing the names and spatial extent of all first-order administrative units from 1987 until 2018, including the names and locations of capital cities over time. The data covers all types of territorial reforms, that is, splits and mergers of regions, area swaps, capital city re-locations, and the creation of new countries. Creating this data 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. 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 that re-allocates small differences in boundaries to match those reported in the most accurate data sources. When no suitable data are available, we use international or national atlases, georeference and digitize the corresponding map. Second, we geocode all capital cities. Online Appendix B contains a detailed explanation of the data construction and provides summary statistics.

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 on a few rare occasions, a capital city is simply moved within the same

[3] Two sources come somewhat close. First, 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. Second, 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, GADM).

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 of first-order administrative capitals and the density of the capital city network. 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 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.

region.[4] In fact, when we track each city from when it enters our sample, we observe 701 cities that have gained capital city status and 336 cities that 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.

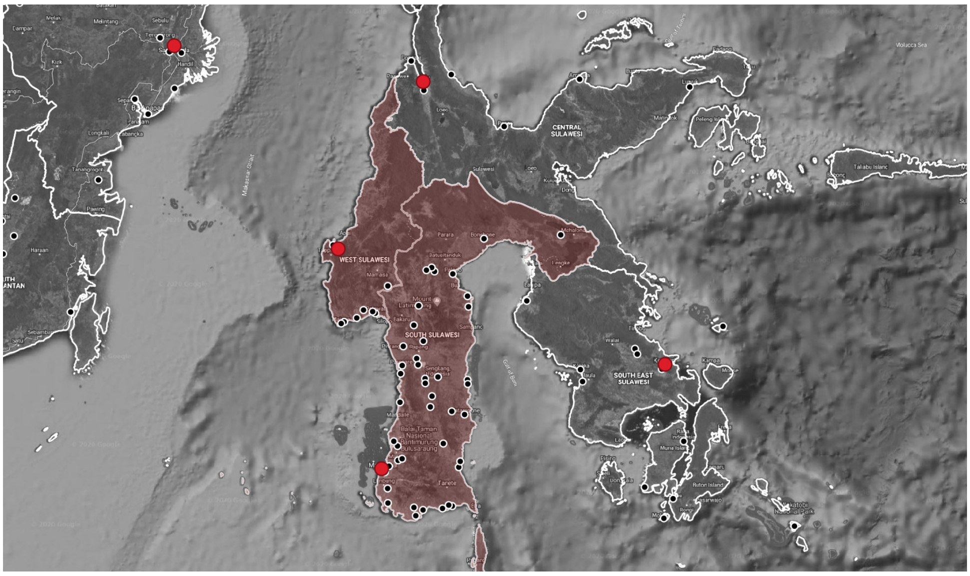
Figure II illustrates a typical provincial split, which is frequent in our data and will be the basis of our identification strategy. South Sulawesi (Sulawesi Selatan) was the fifth largest province of Indonesia with a population of about 8 million people in 2000. In 2004, West Sulawesi (Sulawesi Barat) 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 which 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 C provides descriptive statistics about which (static) variables correlate with the probability that a particular city becomes a capital during a territorial

[4] 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 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 circles indicate capital cities. Black circles indicate other cities detected using our approach.

reform.

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 that often run across several smaller cities. Using a globally consistent definition of cities is an important feature of our analysis.

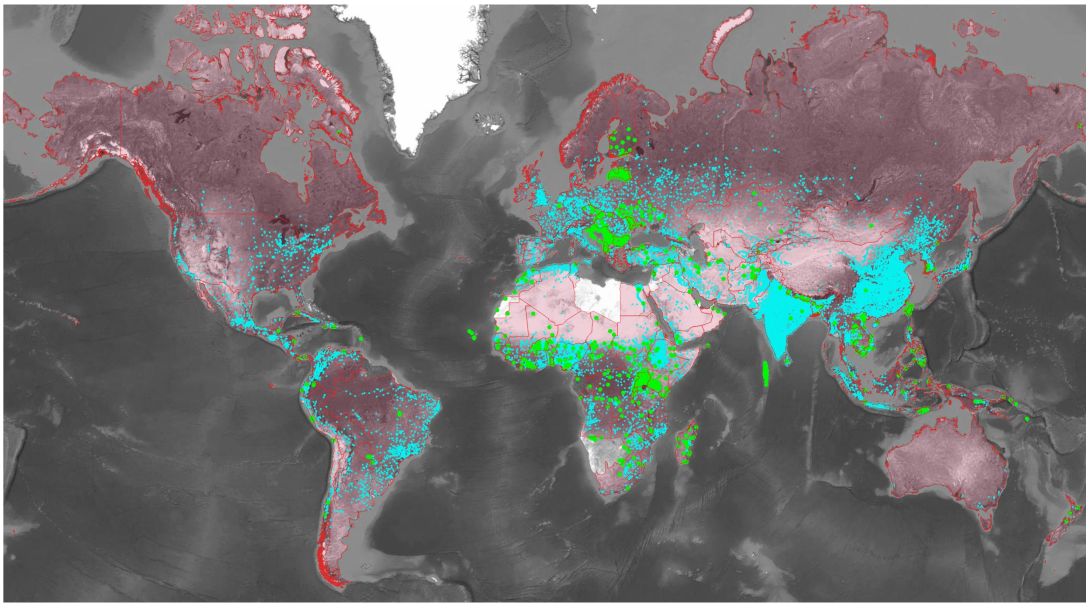
We rely on two products from the Global Human Settlement Layer (GHSL)[5] derived from global moderate resolution (30 m) Landsat images and auxiliary data. The first is a built-up grid at a resolution of 1 km. It indicates the density of buildings and other human structures detected in the underlying 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.[6] We use data from

[5]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.

[6]Both products are available for 1975, 1990, 2000, and 2015.

1990 and 2015 to define the initial and final footprint of a city.[7]

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. Countries included in our sample are highlighted in red (including India where cities cover the country outline).

Our definition of a city or an agglomeration applies a city clustering algorithm (Rozenfeld et al., 2011; Baragwanath et al., 2019; Dijkstra et al., 2021). 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 et al., 2021). 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 represent 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

[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 which defines a city, including the population threshold.

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 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. We discuss this issue further in Online Appendix D, where we show that gaining capital status over the period from 1990 to 2015 predicts inclusion in the 2015 sample (see Table D-1).

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. In this case, the envelope 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 coastline 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 (lights per square km) 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 (e.g., Storeygard, 2016; Baum-Snow et al., 2020) 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, 2022). For our main analysis, we use a version of this data which has been corrected for bottom coding[8] and top coding (see Bluhm and Krause, 2022, for details). Correcting for top-coding in the lights data ensures that light intensity is approximately linear in GDP and population density, even for smaller geographies in highly developed countries, such as the United States or Germany (Bluhm and Krause, 2022). We present results varying these adjustments later in the robustness section. For our baseline results, we normalize light by the area of the city in 1990 to study increases in density and refer to this measure as light density or light intensity.

Our preferred interpretation is that light intensity proxies for economic activity in the city. Much of this activity will be driven by changes in population[9] so that our results can be interpreted 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).

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 measured 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

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

[9]Henderson et al. (2018) show that conditional on country fixed effects, the R^2 from a regression of lights on population density is 0.775. In contrast, it falls to 0.128 for income per capita. Of course, this correlation is just suggestive, given that local purchasing power parities are not available in most countries.

direct and indirect effects of changes in trade costs in general equilibrium trade theory.[10] Moreover, we use ruggedness (Nunn and Puga, 2012) and the estimated malaria burden (Depetris-Chauvin and Weil, 2018) to proxy for how hospitable a location is for human settlement.

3. Empirical strategy

Capital city reforms rarely occur in response to exogenous shocks, such as natural disasters.[11] 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 regions that will be reformed similarly. In other words, all other cities in the region that will be reformed (i.e., split) are in the set of candidate cities to become capitals and were on similar growth trajectories before the reform took place.[12]

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 the status of a subnational capital. We specify a standard event-study specification with an effect window running from $\underline{j}$ to $\overline{j}$ for all $t = \underline{t}, \ldots, \overline{t}$

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

where $\ln \textsc{Lights}_{cit}$ is the log of light density in the urban cluster c in country i at time t, b^j_{cit} are treatment change indicators, which indicate whether a city became a capital

[10]Since we are not interested in changes in trade costs elsewhere, we do not construct costs using 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. 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.

[11]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.

[12]We discovered no evidence to indicate that capital cities or administrative boundaries are typically designed first. If these decisions are taken concurrently, then the set of candidate capital cities in the event of a split contains all non-capital cities in the initial region. Naturally, after the boundaries have been established, some cities will remain in the original "mother" region, while others will be located in the new "child" region. We examine matched samples using non-capital cities from either the initial region or the entire country further below and analyze the impact of "mother" capitals in Online Appendix F.

exactly j periods before or after period t (with bins 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-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-level 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 administrative structure in more than one region at the same time or changes in the overall degree of fiscal decentralization. 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 that gain capital city status, this structure implies a well-defined identification strategy. We compare cities that gain the status after an administrative region is partitioned to all other non-capitals in the initial region (i.e., we compare the new capital, Mamuju, to all other non-capital cities in the region highlighted in Figure II). This allows us to separate the treatment effect of the regional split on all cities from the treatment effect of becoming a capital. 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 meaningful patterns, such as a shift towards local density and/or market potential as transport costs fall (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 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 regions to mirror the dependency structure of the errors (Montiel Olea and Plagborg-Møller, 2019).

For most of the extensions and robustness checks, we collapse the event study to a difference-in-differences specification but keep all other aspects of the design the same.

[13] We summarize the dynamic treatment effects prior to period $\underline{j}+1$ and beyond period $\overline{j}-1$ in one estimate for each. More formally, following the notation from Schmidheiny and Siegloch (2019), we define

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

where d_{cit} is a treatment change indicator.

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 that affects treatment and control cities differently. We take two steps to make sure this assumption is credible. First, we restrict the estimation sample to the set of cities located within initial regions that are split only once to obtain a comparable control group. 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 regional 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 effect of gaining the status as a subnational capital. Hence, we limit the sample to cases where an initial region is split, such that one or several new capitals are created. We defer the issue of capital loss to Online Appendix F, in which we discuss the appropriate comparison groups for different treatments, the effects of capital loss, as well as related issues, such as "mother" capitals.[14]

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 the 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 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

[14] Our identification strategy is 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 of a subnational capital only once during the period of interest.

[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.

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	1,721	–
Gained status	701	329	277
Lost status	336	168	117
Panel B. Diff-in-diff period, 1992 – 2013			
Always capitals	2,211	1,798	–
Gained status	592	263	217
Lost status	275	123	86

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, regardless of whether the city clustering algorithm detects the city footprint. The urban clusters data in column 2 shows how many of these capital cities have been matched to cities that pass the detection thresholds of the city clustering algorithm. Finally, column 3 shows the subset of these which experienced a single reform.

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 (discussed in Online Appendix D). The last column highlights the switches which we effectively use for identification. The event-study design uses 277 cities that become capitals of which 217 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 regions that have been reformed within the period of observation. If we are concerned with potential spillovers and "forbidden comparisons", where treated units end up being used as controls due to the staggered design (see, e.g., Borusyak et al., 2021), then we would prefer a large control group since this reduces the weights of these comparisons in a staggered difference-in-differences setting. 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.

The size of the never treated control group can mitigate biases in staggered difference-in-differences designs (Borusyak et al., 2021) but is no panacea in event studies. Sun and Abraham (2021) show that the coefficients obtained from the two-way fixed effects estimator of the event study specification in eq. 1 can be contaminated by information from other periods. This occurs when there is heterogeneity in treatment effects across treatment cohorts, and this contamination affects leads, lags, and endpoint bins. Sun and Abraham (2021) and Callaway and Sant'Anna (2021) present alternative (and related) estimators that do not suffer from this problem. In robustness checks, we compare how fixed-effects estimation performs relative to alternatives in our setting.

Timing of reforms: Capital city reforms occur for various circumstances, and policymakers 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 devolution and territorial reform.[16] Up 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 a national referendum in August 2010, leaving little scope for anticipation effects.

Even when the splitting of regions is driven by local demands, such as in neighboring Uganda, the national parliament is usually involved in approving them, so 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 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).

[16] See Bluhm et al. (2021) for a study of the effects of this reform on ethnic voting.

Although the random timing of the reforms is appealing, it is not necessary for identification and is likely to be violated in several settings.[17] 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. To test whether selection is heterogeneous in terms of locational fundamentals and other observed factors, we interact the entire event-time path with these variables.

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 (circles). This is our baseline estimate for the larger sample 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. The latter are our preferred estimates since the control group now only consists of non-capital cities within the same initial region. Panel B repeats this setup for the sample of cities in reformed regions.

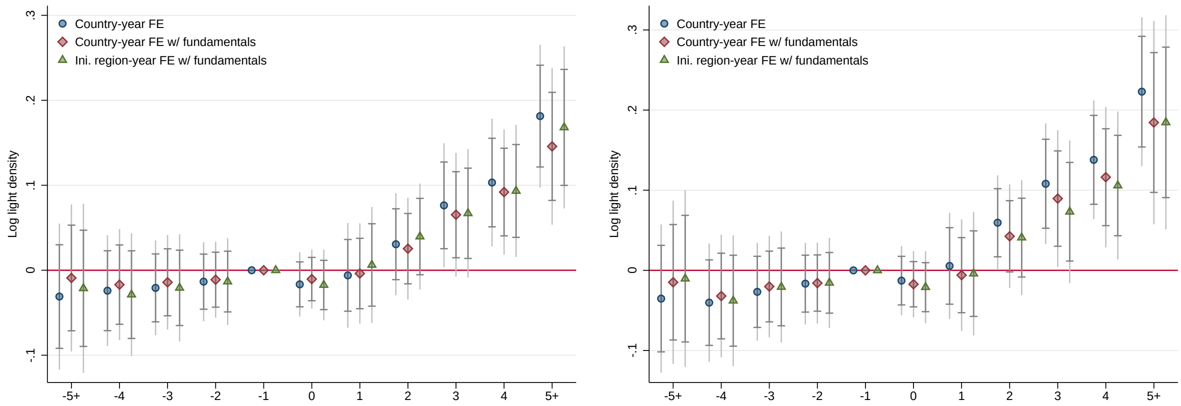
The estimates and their confidence bands strongly support the notion that gaining the status as a capital is exogenous to pre-reform changes in the economic activity of treated cities. The pre-trends are essentially flat. There are no systematic differences in city light intensities prior to a change in capital status. Both 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, and we find no evidence of pre-trends in any of the specifications we examine, we consider it unlikely that pre-testing bias is a major concern in our application (Roth, 2021).[18]

[17] 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 E-1 in Online Appendix E 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.

[18] Using the approach from Roth (2021) to calculate the power of detecting specific linear pre-trends in our setting, we find that the slope of a linear pre-trend which we would detect only half the time is about 0.0119 in the specification with country-year fixed effects in panel A of Figure V and about 0.0149 in the preferred specification with initial-region-by-year fixed effects in panel B. This leads to a mean

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. Circles represent point estimates from a regression with city and country-year fixed effects, diamonds represent specifications with additional controls for economic fundamentals, and triangles represent specifications with initial-region-by-year fixed effects. 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 errors clustered on initial regions are provided by the gray error bars. The orange error bars indicate 95% sup-t bootstrap confidence bands with block sampling over initial regions (Montiel Olea and Plagborg-Møller, 2019).

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. More importantly, the medium-run estimates across both panels and all specifications suggest that the light intensity of capital cities is 15.7 to 25.0% higher five years after the gain in status. The estimates are slightly larger in the sample of cities in reformed regions (panel B). Here light intensity begins to increase by approximately 4.2 to 6.1% two years after a city becomes a subnational capital and settles within a narrower band of 20.3 to 25% five years after the reform and beyond.

We investigate how the size of the event-study window influences the estimate of the medium-run effect (endpoints) and compare it to the difference-in-differences version in Online Appendix E. 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-

after pre-testing in period 5 and beyond of 0.0720 and 0.0895, respectively. Power rises quickly against more severe pre-trends. A linear pre-trend which we would expect to detect in 80% of cases has a slope of 0.0183 and 0.0230, implying a conditional expectation after pre-testing in period 5 and beyond of 0.111 and 0.1379, respectively. Hence, in our setting, it seems unlikely that strong linear pre-trends explaining about half our effect size in the final period would have gone undetected. Moreover, we have more than 97% power to detect linear pre-trends that have a conditional expectation after pre-testing which passes through the final period estimate in either specification.

in-differences estimate (see Figure E-2). The difference-in-differences is around 9.2 to 15.4%, depending on the specification, and utilizes only switchers during the period from 1992 to 2013 (see Table E-1). These estimates are lower than the medium-run estimates from the corresponding event studies, as they average over the first years and all subsequent post-treatment periods.

In Online Appendix E, we also explore whether these results are affected by the estimation method. Figure E-3 compares the estimates using TWFE to those obtained via the interaction-weighted (IW) estimator (Sun and Abraham, 2021), which correctly identifies cohort-average treatment effects in the presence of heterogeneous treatment effects.[19] No matter if we focus on a five-year event window or a 15-year event window to mitigate the influence of binning, we find that TWFE and IW estimation deliver quantitatively and qualitatively similar results. The estimates of leads, lags, and endpoints bins obtained via the two estimators are typically close and well within each other's confidence intervals. We continue to find no indication of pre-trends,[20] a rise from the second or third period onward, and effect sizes that begin to level off to their medium-run value sometime around the fifth post-treatment period.

In summary, our main results suggest that cities that become capitals grow substantially faster than their peers in the subsequent period. There is a build-up in economic activity during the first five years after which we observe a medium-term increase of 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 that is further away from the primate city than the median city loses about 12% of its economic activity when the oil price is high.[21] For the remainder of the paper, we report difference-in-differences estimates in the main text and relegate the corresponding event-study plots to Online Appendix E.

Unlike the economic advantages documented in Bleakley and Lin (2012), we find evidence that the capital premium does not persist. Online Appendix F provides a detailed analysis of cities that lose their status of a regional capital (usually during a

[19] The IW estimator is constructed in three steps. First, we estimate an event study specification where the relative time dummies are interacted with cohort (treatment year) dummies using TWFE. Second, we estimate the probability of first being treated in a particular year as the sample shares of each cohort in the relevant period(s). Finally, we form the IW estimator by taking averages of the estimates by cohort weighted by their probability of treatment. Following Sun and Abraham (2021), we balance the sample in calendar time, which is why the TWFE results differ marginally from Figure V.

[20] Only one pre-treatment coefficient (out of 15) in the long event window is significant using interval estimation. We do not compute bootstrap-based confidence bands due to computational speed but note that these would be wider than the point intervals.

[21] 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% to 10% in the ethnic homeland of a leader who is currently in office.

merger of first-order regions). Such an analysis necessitates a somewhat different design, as the relevant comparison group now consists of 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 losing their elevated political status.

Agglomerations and city peripheries: Most cities grew substantially at the extensive margin from 1990 to 2015. The average city expanded its area by almost 50%. The area of capital cities grew faster than that of other cities in the same initial region.[22] Unfortunately, we do not observe a city's urban extent yearly. Hence, we cannot calculate detailed measures of urban expansion. 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 light intensity, 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 larger agglomeration and include areas that were initially in the periphery but became part of a single agglomeration by 2015. This allows us to study changes in the light intensity of the overall agglomeration and changes outside of the 1990s core of each city.

Capital cities experience faster overall growth than non-capital cities, and their peripheries grow faster. 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 points but are otherwise similar. Panel B reports the same specifications but focuses on the growth of nighttime lights in the periphery, that is, only the area of each agglomeration that is initially less dense and subsequently passes the population threshold. 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 expansion are associated with gaining capital city status.

These results could be subject to endogenous selection because capital cities expand more quickly, and we study light growth in the union of their initial and final boundaries. We address this issue in two ways. First, Figure E-4 in Online Appendix E shows no evidence of pre-trends in lights for the larger agglomeration or the fringe. Second, we

[22] Table E-2 in Online Appendix E shows that capital cities expanded their average footprint by about 9.9% to 14.2% more than non-capital cities over the period from 1990 to 2015.

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	0.0818	0.0738	0.0821	0.1093	0.0922	0.0898
	(0.0285)	(0.0279)	(0.0282)	(0.0300)	(0.0292)	(0.0286)
Panel B. Growth in the periphery of the city						
Capital	0.0787	0.0624	0.0862	0.1035	0.0797	0.0935
	(0.0353)	(0.0341)	(0.0342)	(0.0365)	(0.0362)	(0.0348)
N	13373	13373	13373	4466	4466	4466
$N \times \bar{T}$	274408	274408	274408	86455	86455	86455
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.

replicate the agglomeration and periphery results with an analysis of city buffers based on a mechanical city growth model where all cities in a country grow at the same rate (as in Harari, 2020).[23] Table E-3 in Online Appendix E shows that these estimates follow a similar pattern, but the larger agglomeration and fringe grow considerably faster. This implies that any potential upward bias from selection is more than offset by adding smaller, less densely developed areas whose light grows more quickly.

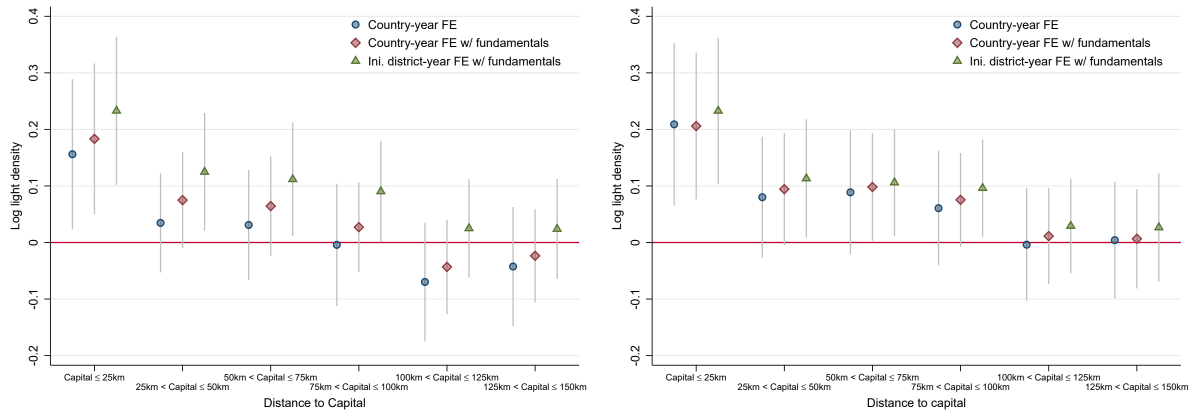
Spillovers to nearby cities and SUTVA violations: An important question in our context is whether new subnational cities draw economic activity from their immediate surroundings or whether creating capitals benefits more cities in a region. Moreover, negative or positive spillovers would violate the stable unit treatment value assumption (SUTVA). This assumption 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

[23] We follow the "completely" mechanical city growth model outlined in Harari (2020, Online Appendix E). The model assumes that all cities in a country grow at the same rate but start at different sizes. Applying it to our context involves four steps. First, we use our data on agglomerations to compute the change in area of the urban core in 1990 and 2015. Second, we obtain the country-specific growth rate of all cities by regressing the logarithm of the area of each city on a set of city and country-year fixed effects. Third, we convert the predicted increase in each city's area to a change in the radius of a circle with the same area. Finally, we buffer the 1990s core with these values.

are—by virtue of being located in the same initial region—relatively close by. 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
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). Circles represent point estimates from a regression with city and country-year fixed effects, diamonds represent specifications with additional controls for economic fundamentals, and triangles represent specifications with initial-region-by-year fixed effects. 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 regions are provided by the gray error bars.

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 0 km and going up to 150 km.[24] A considerable advantage of this specification over our baseline results is that it allows us to account for capital cities that 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 the status of nearby cities. The spillovers are identified by cities switching between rings as they move closer to (or further away from) capital cities.[25]

The country-year fixed effects specification (circles in panel A) in Figure VI provides the least evidence of spillovers. It uses all non-capital agglomerations in the same country

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

[25]Spillovers are identified through proximity to the closest administrative capital. These distances change both when nearby cities become capitals and when they lose their elevated status.

as a control, many located in regions far apart from treated agglomerations. The evidence favoring spillovers becomes stronger once we introduce initial-region-by-year fixed effects (triangles) or limit the sample to cities in reformed regions, as in panel B. Depending on the specification, we find evidence of positive spillovers affecting agglomerations up to 75 to 100 km away from a new capital. All specifications suggest a declining pattern of positive treatment effects, where satellite cities close to the new capital grow substantially faster. The 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 23.2% and 26.2%. This spatial pattern has an important policy implication. Instead of just drawing activity and population from cities in their immediate vicinity, creating new capital cities benefits more cities in the reformed region. As our sample only contains relatively dense cities with at least 20,000 inhabitants in 1990, capital cities likely draw activity and population from smaller cities and rural areas.

Alternative control groups: A potential concern is that our baseline results include a variety of cities in the control group, some of which are less likely to become subnational capitals than others. 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 the 90th percentile was ranked 10th.[26] Static selection is not a concern in difference-in-differences designs. However, despite finding no evidence in favor of pre-trends, our baseline results could still include cities in the control group that are on fundamentally different growth paths than cities that later become subnational capitals.

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.[27] 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 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 most restrictive specification with initial-region-by-year fixed effects. By definition, we are now only using variation from cities in reformed regions. Reassuringly, every single

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

[27]This approach is similar to Becker et al. (2021), 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.

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	0.1154	0.0790	0.0911	0.1033	0.0947	0.0913
	(0.0276)	(0.0296)	(0.0330)	(0.0284)	(0.0318)	(0.0351)
F-test pre-trends (p-val.)	0.186	0.447	0.470	0.364	0.190	0.113
N	625	413	354	618	433	370
$N \times \bar{T}$	13578	8963	7694	13437	9403	8038
Panel B. Control cities within ± 3 ranks of treated cities in initial region						
CAPITAL	0.1241	0.0906	0.0970	0.1055	0.1050	0.0931
	(0.0277)	(0.0286)	(0.0319)	(0.0287)	(0.0320)	(0.0362)
F-test pre-trends (p-val.)	0.270	0.457	0.250	0.468	0.476	0.384
N	784	497	411	767	516	431
$N \times \bar{T}$	17032	10781	8923	16670	11205	9366
Panel C. Control cities within ± 4 ranks of treated cities in initial region						
CAPITAL	0.1243	0.0888	0.0945	0.1048	0.1061	0.0966
	(0.0276)	(0.0277)	(0.0318)	(0.0276)	(0.0316)	(0.0357)
F-test pre-trends (p-val.)	0.194	0.458	0.408	0.406	0.460	0.340
N	913	564	457	887	581	469
$N \times \bar{T}$	19831	12229	9924	19270	12609	10190

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.

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 the initial light intensity or estimates of the 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.[28] 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. In Table E-4 in Online Appendix E, we repeat this matching exercise using all similarly ranked never-capital cities in the entire country as controls in a specification with country-year fixed effects. The results are qualitatively similar in these samples as well. They illustrate that our findings do not depend on using cities in the initial districts as the control group but also hold when we use other prominent non-capital cities in regions that never split.

Cross-country heterogeneity: The capital city premium estimated in our global sample of cities encompasses a diverse range of countries that differ in their level of development, political system, and degree of political and fiscal decentralization. While the external validity of this approach is high, and we take out a lot of cross-country heterogeneity with country-year or initial-region-by-year fixed effects, it is essential to understand this variation better since the effect in a particular setting will likely differ from the average. We focus on three sources of heterogeneity. First, designating new capital cities may have little to no impact on migration in well-established urban networks with limited population pressures but large effects in countries that are still urbanizing and undergoing structural transformation. Second, the seminar work by Ades and Glaeser (1995) suggests that the capital premium will likely be larger in autocratic countries. Third, first-order administrative regions, including their capitals, possess more political independence and power in federal countries, such as states in Germany or India, than in more centralized countries, such as departments in France or provinces in China.[29]

[28] 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.

[29] To test whether the capital effect differs across countries that agglomerated early or are mainly urbanizing today 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). 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

Table E-5 in Online Appendix E confirms that this heterogeneity is broadly in line with our expectations based on prior research on urban growth. We find that late developers drive the capital premium. The effect for late developing countries ranges from about 14.6% to 19.8%, depending on whether we account for time-varying fundamentals. The premium is also considerably larger in autocracies and federal countries, although the effect is still sizable in centralized countries. Finally, Table E-6 shows results for a subset of countries with data on fiscal decentralization. We find the expected pattern of larger effects in more fiscally decentralized countries.

Other robustness checks: We conduct various checks to verify our analysis. We briefly summarize their results here and report the corresponding tables in Online Appendix E. Our baseline estimates are robust to accounting for spatial autocorrelation (see Figure E-5) or using different versions of the lights data, provided that there is some adjustment for bottom-coding (see Table E-7). In fact, the 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 with a substantially larger initial population in 1990 and rise somewhat with initial city size (see Table E-8). Finally, none of these perturbations results in significant pre-treatment trends.

5. Scale and economic fundamentals

Administrative splits involve two crucial political decisions: *i)* what will be the size of the new region and *ii)* which city will become the capital? Both of these decisions are likely to affect the capital city premium because the scale is related to political importance and, hence, investments in state capacity, and each potential capital location within the new region has different economic fundamentals. As a result, these decisions represent two important subnational sources of (subnational) heterogeneity in the treatment effect, which we examine now. Of course, heterogeneity in the treatment effect implies that selection into the treatment could also be heterogeneous, which is why we test for pre-trends by interacting these static variables with the entire event-time path.

Scale and state capacity: In many developing countries, the state often has limited reach beyond the national capital and other politically important cities (Herbst, 2000;

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. To test for heterogeneous capital effects by political system, we use a democracy dummy: unity when a country has a polity score equal to or above six and zero otherwise. Heterogeneity between federal and non-federal countries is tested via the federalism indicator from Treisman (2008).

Michalopoulos and Papaioannou, 2013). Administrative reforms are one way for the state to build capacity at the local level and expand its influence to the periphery (see, e.g. Chambru et al., 2021). Our findings thus far suggest that creating more subnational capitals and, hence, more first-order units benefits these cities and regions irrespective of their size. However, it is improbable that subnational capital cities governing increasingly smaller territories would benefit similarly as those that are the capitals of more populated regions. Grossman and Lewis (2014), for example, argue that Uganda has become so heavily territorially decentralized that the intergovernmental bargaining power of single first-order units has been substantially weakened. Moreover, most federal systems distribute central government resources to regions using a fixed population-based formula.[30]

TABLE IV
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	0.1739	0.1228	0.2730	0.1801	0.2093	0.1927
	(0.0489)	(0.0475)	(0.0719)	(0.0738)	(0.0427)	(0.0453)
CAPITAL × SCALE	0.0741	0.0582	0.1432	0.0995	0.1025	0.1108
	(0.0339)	(0.0311)	(0.0431)	(0.0401)	(0.0301)	(0.0299)
Fundamentals	–	✓	–	✓	–	✓
City FE	✓	✓	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓	✓	✓
N	8438	8438	8438	8438	8438	8438
$N \times \bar{T}$	184687	184687	184687	184687	184687	184687

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 regions are provided in parentheses.

Table IV reports a series of regressions in which we interact the capital city status with a variable measuring the scale of the region a city belongs to (based on their 1990 values, in order to avoid selection in terms of changes in size). This keeps the population and number of cities constant but isolates the policy choice of creating a new region. The interaction of the capital city status with scale changes simultaneously when a new region and capital are created. 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

[30] The literature on the optimal size of local jurisdictions highlights several relevant trade-offs in this decision, 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).

of the non-agricultural economy, while 5 and 6 use counts of the number of cities in the resulting region. The measures of scale are standardized and time-varying (they change, based on their 1990 values, whenever a region is reformed). No matter how we specify this interaction with scale, we find evidence suggesting a trade-off: new capital cities only grow faster when they rule over larger regions. Since the reforms in our baseline sample are splits of larger regions, 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 wipes out the capital city effect in all specifications.

Figure E-6 in Online Appendix E 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, as well as 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.

In Online Appendix F, we examine whether "mother" capitals—i.e., cities that rule over a smaller area after a regional split but maintain their political status—experience a change in economic activity. We find no evidence suggesting their activity responds in either direction (but the relatively wide confidence intervals include a range of potential effects). We also briefly examine the role of preference heterogeneity in Online Appendix E. There, we use ethnic diversity in the initial region as a proxy for preference heterogeneity (Table E-9).[31] We find no evidence favoring the hypothesis that capital cities grow at different speeds when located in more ethnically homogeneous (or diverse) regions.[32]

Location and economic fundamentals: We now examine whether the place-based policy of designating cities as subnational capitals substitute or complement good economic fundamentals.[33] If they are substitutes, then the allocation of subnational capital and public investment can have a significant impact on the hinterland, i.e., in regions with few local advantages or primarily suited to agricultural production, while

[31]Similar to Eberle et al. (2020), we measure the ethnolinguistic fractionalization of initial regions using GHSL population data for 1975 and an algorithm developed by Desmet et al. (2020) which distributes data from the World Language Mapping System (WLMS)—the vector version of the Ethnologue project—on a 5×5 km grid.

[32]While this appears to contradict recent research suggesting that initial diversity inhibits agglomeration (Eberle et al., 2020), it only shows that such an effect is unlikely to run through changes in the density of capital cities relative to other cities. Eberle et al. (2020) suggest that this is a result of diverse groups spreading over smaller cities. Given that we typically include country-year or initial-region-by-year fixed effects, we would not pick up an effect if all cities are similarly affected by initial diversity.

[33]Long-run studies typically find that both fundamentals and path dependence are important determinants of city sizes (e.g., Davis and Weinstein, 2002; Bleakley and Lin, 2012; Michaels and Rauch, 2018).

inducing little change in areas where trade-related fundamentals are already strong.[34] If they are complements, then cities with better market access and otherwise favorable fundamentals would disproportionately benefit from the investments associated with capital city status.[35]

TABLE V

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	0.1924	0.1426	0.1433	0.1882	0.1393
	(0.0417)	(0.0316)	(0.0296)	(0.0390)	(0.0424)
CAPITAL × INT. TRADE	0.0957			0.0860	0.0481
	(0.0390)			(0.0381)	(0.0366)
CAPITAL × EXT. TRADE		0.0027		-0.0059	-0.0047
		(0.0170)		(0.0169)	(0.0156)
CAPITAL × AGRICULTURE			-0.0650	-0.0600	-0.0571
			(0.0172)	(0.0162)	(0.0163)
Panel B. Selected variables for each group of fundamentals					
CAPITAL	0.2688	0.1427	0.1381	0.2697	0.2026
	(0.0490)	(0.0316)	(0.0298)	(0.0454)	(0.0498)
CAPITAL × MARKET ACCESS	0.1293			0.1348	0.0957
	(0.0342)			(0.0316)	(0.0297)
CAPITAL × DIST. TO COAST		-0.0012		0.0234	0.0101
		(0.0264)		(0.0238)	(0.0227)
CAPITAL × WHEAT SUITABILITY			-0.0657	-0.0638	-0.0626
			(0.0212)	(0.0194)	(0.0195)
Fundamentals	–	–	–	–	✓
N	8338	8338	8338	8338	8338
$N \times \bar{T}$	182529	182529	182529	182529	182529

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 regions are provided in parentheses.

Table V reports 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

[34]Recent evidence suggests that the growth potential of secondary cities in agricultural hinterlands may be limited (see, e.g., Gollin et al., 2016).

[35]For example, Becker et al. (2021) 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.

three groups of fundamentals (internal trade, external trade, and agriculture). Each column takes a group of fundamentals and interacts them 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 accounting for 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. Columns 4 and 5 include all 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 that 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).

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 that 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 13.8%. Given that most of our reforms occur in developing countries, this finding echoes 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 the 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.

Figure E-7 in Online Appendix E 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. Moreover, the estimates of the pre-treatment coefficients and their sub-t confidence intervals strongly suggest that heterogeneous selection in terms of these fundamentals is unlikely.

6. Unpacking economic activity in capital cities

Our analysis shows that capital cities grow faster than other cities in the same country or initial region. We now examine whether this growth is primarily due to population changes resulting from the relocation of economic activity or whether it also reflects higher economic growth per capita. To disentangle these components of economic activity, we adopt two approaches. First, we analyze long differences in lights, population, lights per capita, and urban built-up. Second, we study individual and household-level outcomes.

Long differences: Table VI shows estimates from long difference regressions of the differences in log light intensity (density), log population density[36], log lights per capita, and log urban built-up between 1992 and 2013 on the fraction of years a city was a capital in the same period, initial-region fixed effects, and economic fundamentals. Due to the limited variation in the population data, we can no longer specify regressions that mirror our main identification strategy. Instead, we ask how much faster economic activity, population density, and urban built-up of capital cities grow relative to that of other cities in the same initial region (with similar fundamentals). The initial region fixed effects imply that we only exploit variation between capital and non-capital cities in regions that are ultimately reformed and estimate deviations from a common growth path.

Columns 1 to 4 show that cities that were capitals for a longer period experienced stronger growth in light intensity and population density between 1992 and 2013. Subnational capitals grow slightly more than a percentage point per year faster than other cities. Since the estimate for light intensity is about a third larger than the estimate of population density, we interpret this as evidence that increases in light intensity at the city level primarily reflect increases in population.[37] The result in column 3 points towards modest increases in light per capita, but this effect is estimated imprecisely and subject to significant measurement error. Finally, column 4 shows that there is also a modest increase in urban built-up within city cores. Estimates for the larger agglomeration are in line with these results (see Table E-10 in Online Appendix E).

The long differences suggest that population growth is the main driver of increased economic activity in capital cities. Population growth can arise from migration and changes in mortality within capital cities, but the magnitude suggests that it is unlikely

[36] We log linearly interpolate census-derived GHSL population data in 1990, 2000, and 2015 on the city level to obtain values for 1992 and 2013.

[37] Given the pattern of spillovers documented earlier, this increase is likely to come from rural areas, smaller cities (with less than 20,000 inhabitants in 1990), and cities far away from the capital (although we have no direct evidence establishing such a migratory response).

TABLE VI
Long differences for city level outcomes (1992–2013)

	Dependent Variables:			
	$\Delta \ln \text{LIGHTS}_{ci}$	$\Delta \ln \text{POP}_{ci}$	$\Delta \ln \text{LIGHTS/POP}_{ci}$	$\Delta \ln \text{URBAN INDEX}_{ci}$
	(1)	(2)	(3)	(4)
CAPITAL	0.3067	0.2267	0.0801	0.0340
	(0.0386)	(0.0375)	(0.0540)	(0.0082)
Fundamentals	✓	✓	✓	✓
Initial-Region FE	✓	✓	✓	✓
N	8020	8020	8020	6960

Notes: The table reports results from long-difference regressions of the change in city-level outcomes on the fraction of years in which a city is a capital (1992–2013). City-level outcomes are the change in log light density, the change in log population density, the change in log light per capita, and the change in the log of a remotely sensed urban index for built-up structures (re-scaled from −1 to 1 to 0 to 2). Standard errors clustered on initial regions are provided in parentheses.

to be caused solely by public employment.[38] More fundamentally, our results do not rule out per capita effects. The city-level population estimates are subject to a range of (potentially systematic) measurement errors arising from the interpolation of census data, assignment of the population to built-up pixels, and other factors, so the per capita results should be interpreted with caution. We turn to survey-based data in the following analysis to provide additional insights into these issues.

Individual-level evidence: We now compare individual and household-level outcomes among residents in capital cities to residents of non-capital cities in the same initial region. This serves two additional purposes. First, it allows us to validate the results obtained with night lights using indicators that directly measure development outcomes (and a different estimation strategy). Second, the survey data allow us to examine specific dimensions in which residents of capital cities may be better off. For example, we can test whether households in capital cities have better access to electricity, more schooling, or better health outcomes.[39]

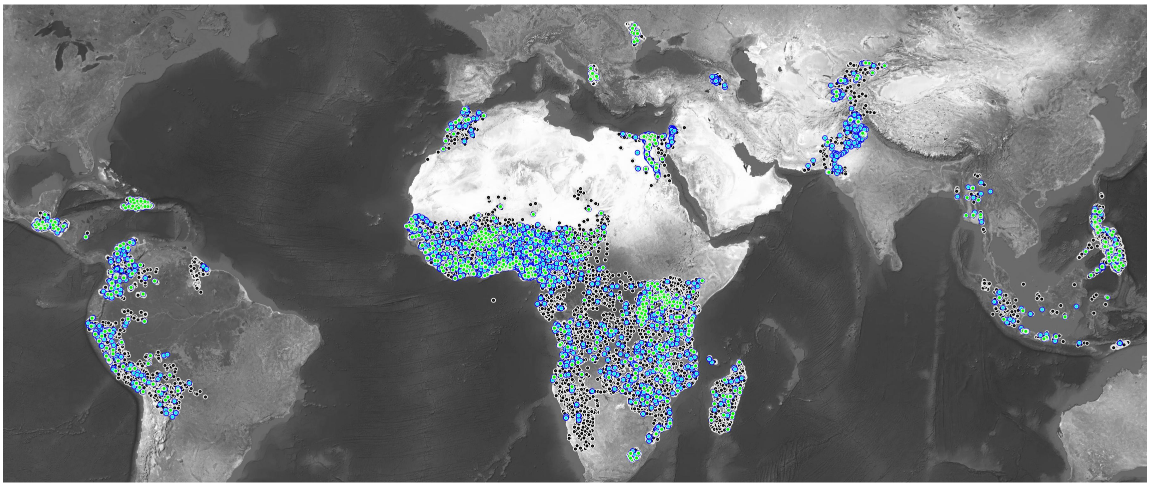
We compile a global sample of geocoded Demographic and Health Surveys (DHS) to test for differences in individual and household-level outcomes between residents of the capital and non-capital cities.[40] Figure VII plots our global coverage of DHS clusters in gray (about 80,000 clusters containing roughly 1.36 million households), the ones we can match to any of our cities in blue (about 25,000 clusters), and those that are located in

[38]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 with associated multipliers that would have to be much larger than what is suggested by the literature (Faggio and Overman, 2014; Becker et al., 2021; Jofre-Monseny et al., 2020).

[39]We adjust the approach of Henderson et al. (2020) to our setting, where we exclusively focus on differences between different types of cities, rather urban-rural differences.

[40]The data are described in detail in Online Appendix A.

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).

capital cities at the time of the survey in green. Capital cities are heavily over-represented among urban DHS clusters (56% of matched clusters are capitals).

Our strategy is to pool all this data and run regressions comparing household-level or individual-level outcomes in non-capital cities in the same initial administrative region to outcomes in capital cities. We control for the fundamentals of each city, including initial population density, and household-level characteristics, so that we estimate the capital city premium (or penalty) in otherwise comparable locations and households. We study two samples—all city dwellers and those born in the city—as a first test to see if migrants moving to capital cities differ from those who have always lived there.

Table VII presents the results of specifications regressing household-level or individual-level outcomes on a capital dummy at the time of the survey. Column 1 highlights that households in capitals accumulate, on average, more assets than households in non-capital cities. The effect is larger for all city dwellers (panel A) than for city natives (panel B) but makes up close to 7.5% of the sample average in both cases. Column 2 shows that residents of capital cities are about 9 percentage points more likely to have electricity in their households than residents of other cities within the same initial region. However, this falls to 3 percentage points when we restrict the sample to city natives. Turning to schooling, we find that residents of capital cities 16 years or older are 0.73 percentage points more likely to have completed eight years of schooling (column 3 in panel A). While this effect appears small, it is an increase of about 22% of the sample average because only around 3.3% of all respondents over the age of 16 have completed eight years of education. We find weaker results in terms of years of schooling (about 1% more) for all residents of capital cities. Remarkably, the schooling results disappear when we only

Table VII
Amenities in capitals: Within-region regressions

	Dependent Variable:				
	DHS wealth index	Electricity	Years of Edu ≥ 8	Ln years Schooling	Infant mortality
	(1)	(2)	(3)	(4)	(5)
Panel A. All city dwellers					
Capital	0.3413	0.0901	0.0073	0.0096	-7.2745
	(0.0391)	(0.0135)	(0.0021)	(0.0055)	(2.2455)
Outcome mean	4.4603	0.6848	0.0334	1.318	65.4767
N	144960	144960	236500	238579	651132
Panel B. City natives / born in city					
Capital	0.1951	0.0324	0.0054	-0.0001	-8.2015
	(0.0495)	(0.0142)	(0.0038)	(0.0082)	(3.7040)
Outcome mean	4.3129	0.7500	0.0302	1.2903	67.6722
N	39023	39023	65096	66036	183576
DHS controls	✓	✓	✓	✓	✓
Fundamentals	✓	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓	✓

Notes: The table reports results from regressions of various DHS measures on the current capital status of a city (i.e., the capital status in the corresponding survey year). Panel A reports results for all respondents/ households currently residing in a city. Panel B reports results for those that were born in the city of current residence. The dependent variables are the DHS wealth index, a dummy for the presence of electricity, a dummy for 8 years of schooling (or more) for respondents 16 years or older, log years of schooling for respondents 16 years or older, and infant mortality (per 1,000 live births). All columns include initial-region-by-year fixed effects, an indicator for the national capital, and DHS controls (log household size, female household head, log age of the household head, and three indicators for completed primary, secondary, and higher education of the household head). Columns 1 and 2 use data at the level of households. Columns 3 and 4 use individual-level data and add age, age squared, and a female indicator of the respondent to the household-level controls. All columns include locational fundamentals, as well as an indicator if the respondent lives in the national capital city. We allow all those additional controls to have a different effect in different survey years. In column 5 we use respondent-child-level data with the following controls: a gender dummy, an indicator variable for multiple children, and a set of period of birth dummies (each period corresponds to a decade, e.g., 1990s). Note that due limited degree of freedom, we allow the fundamentals in column 5 to only vary across 5-year periods. Standard errors clustered on the agglomeration provided in parentheses.

look at city natives in panel B, suggesting that the migrant population of capital cities is more educated than the native population. Finally, we also find a sizable reduction in infant mortality of around 7.3 to 8.2 children per 1,000 live births. This is up to 12.1% of mean infant mortality.

Taken together, these results imply that residents of capital cities in developing countries enjoy several benefits compared to those in non-capital cities and qualify that changes in population do not solely drive our main finding on economic activity. Proximity to government is associated with significantly greater household wealth, better access to electricity, and improved health outcomes. In addition, the lower infant mortality rate in capital cities suggests that capital cities experience a faster urban mortality transition than other cities. However, the larger effects for all city dwellers compared to city natives suggest that selective migration into capital cities may drive some of these results. Such migration flows could respond to a city becoming a subnational capital, so we revisit this issue when examining potential mechanisms.

7. Mechanisms

We focus on two underlying mechanisms that help to explain why capital cities grow faster. The first mechanism is selective migration, in the sense that educated migrants might be more likely to move to capitals than to other cities. The second mechanism is that capital cities in developing countries, particularly those with better market access, attract more public and private investments than other cities.

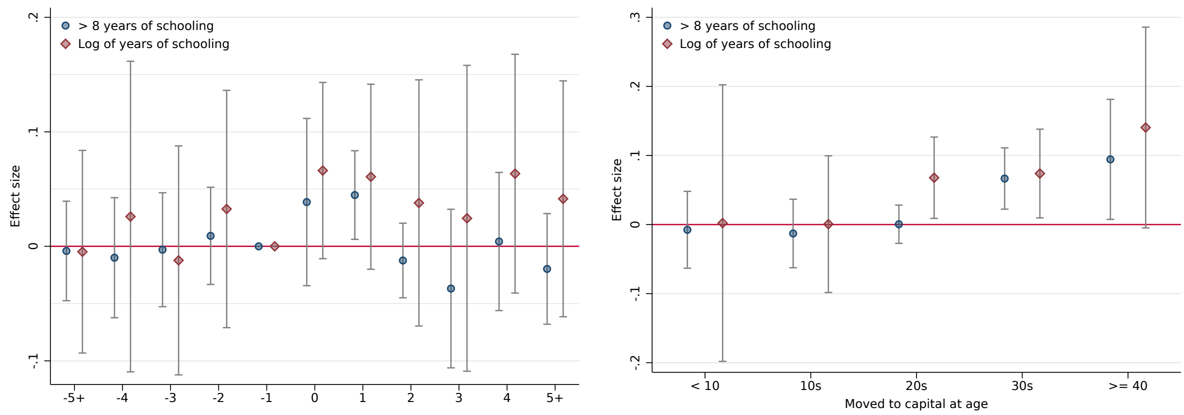
Selective migration: The DHS surveys allow us to go beyond studying cross-sectional differences among city dwellers. For a subset of 7,539 migrants living in cities that have become capitals in our sample period, we know how many years they have resided at their current location. We use this information to create a synthetic panel of recent migrants living in cities that eventually become capitals. For each of these individuals, we observe whether they moved to a (new) capital before or after it gained its political status, plus some initial characteristics, such as their age when they moved. We use this cohort-level panel in two ways. First, we set up an event study mimicking our main results, examining the schooling attainment of migrants who arrived shortly before or after a city became a capital. Second, we collapse these data to a difference-in-differences specification, allowing the treatment effect to vary by cohort. All specifications use only variation within the subset of cities that become capitals, individuals within the same city-year, and individuals within the same cohort during the move. Finally, we focus on the educational attainment of migrants only since human capital is embodied and not tied to the place of current residence.

Figure VIII illustrates the corresponding results. Panel A shows that we find little evidence that migration is selective in terms of educational attainment before a city becomes a capital city. Migrants arriving two years or more before a city becomes a capital seem to be no more or less likely to have completed eight years of schooling than those in the year before the reform. In the year after the status change, we observe an uptick in the probability of migrants having at least eight years of schooling (around 4.5 percentage points). However, the uncertainty around most of these event-study coefficients is considerable, especially regarding log years of schooling. Moreover, we do not know if these migrants acquired this additional human capital before or after they

Figure VIII

Selective migration: Within city evidence

(A) Event study (B) Difference-in-differences



Notes: The figure plots estimates of educational attainment from the DHS surveys on different treatment dummies. Panel A reports event-study results from fixed effects regressions of a dummy for more than 8 years of schooling for migrants of all ages (blue circles) and log years of schooling for migrants of all ages (red triangles) on a binned sequence of treatment change dummies defined as the year of the last move of a migrant minus the year a city changed its status. Panel B reports the results of a difference-in-differences specification with cohort-level heterogeneity. All specifications include city-year and cohort-at-move fixed effects (defined four bins from '0 to 10' to 'older than 40' at the time of the last move), a gender dummy, age, and age squared. 95% confidence intervals based on standard errors clustered on the city level are provided by the gray error bars.

moved. Panel B addresses both points by reducing the specification to before-and-after comparisons for different cohorts. We find that migrants arriving after a city became a capital who were at least in their 30s (and perhaps already in their 20s) at the time of the move appear to have higher human capital.[41] Given their age at the time of the move, this strongly suggests that these migrants have acquired their human capital before they moved to a new capital city. We take this as direct evidence that capital cities attract a (moderately) more skilled migrant population than other cities.

Public and private investments by sector: Observing a higher influx of skilled migrants toward capital cities can result from increased public or private investments in these cities. Similarly, our findings regarding the complementarity between capital city status and economic fundamentals may stem from differential investment patterns in either sector. In other words, do governments invest more in capital cities that are in favorable locations, or do private investors tend to favor capital cities with strong economic fundamentals over similar cities without political status? We conduct a series of empirical tests to distinguish between these two possibilities. As we lack census data on the employment structure and finances of cities, we use two proxies to gauge public

[41]Those that moved in their 30s or later have a 6.6–9.4 pp higher probability to have eight years of schooling and 7.5–15% more years of schooling than those in the same age group who moved before the city became a capital.

and private investments: geocoded data on World Bank projects from 1995 to 2014 and the fDi Markets database on private foreign direct investments, available from 2003 to 2018.[42]

World Bank projects are usually carried out in close cooperation with the government (national and regional) in the recipient country and even substitute for some of its basic functions in 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. The World Bank project locations were geocoded ex-post and include precision codes.[43] 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. The data are not primarily based on official statistics but collected from media, industry organizations, and investment agencies. The fDi Markets data reports the project's host city, the investment's value, and an estimate of the jobs created that can be connected to the investment.[44] Both sources contain sector codes, which allows us to distinguish investments in, for example, government from investments in water and sanitation.

Our regressions utilize (the log of) project value estimates as the dependent variable. On the right-hand side, we have the share of years in which a city was a capital and an interaction of this share with internal market access in 1990 (standardized as before). All regressions include initial-region fixed effects, the full set of fundamentals, including initial population, and a dummy variable for national capitals. Following a similar approach to the long differences presented earlier, this helps us understand whether capital cities attract more or less foreign-funded projects than comparable non-capital cities within initial regions and whether this effect varies with economic fundamentals. However, our results cannot indicate whether new capital cities attract more funding immediately after gaining status due to the coarse nature of the data and differences in sub-periods.[45]

Figure IX reports the regression results per sector. Panel A shows the results for (log) aid commitments at average levels of market access. We find that capital cities are considerably more likely to receive development projects of higher value than other cities in the same region (commitments in capitals are about 3.9 log points larger). The sectoral composition of projects suggests that capital cities primarily attract funds for

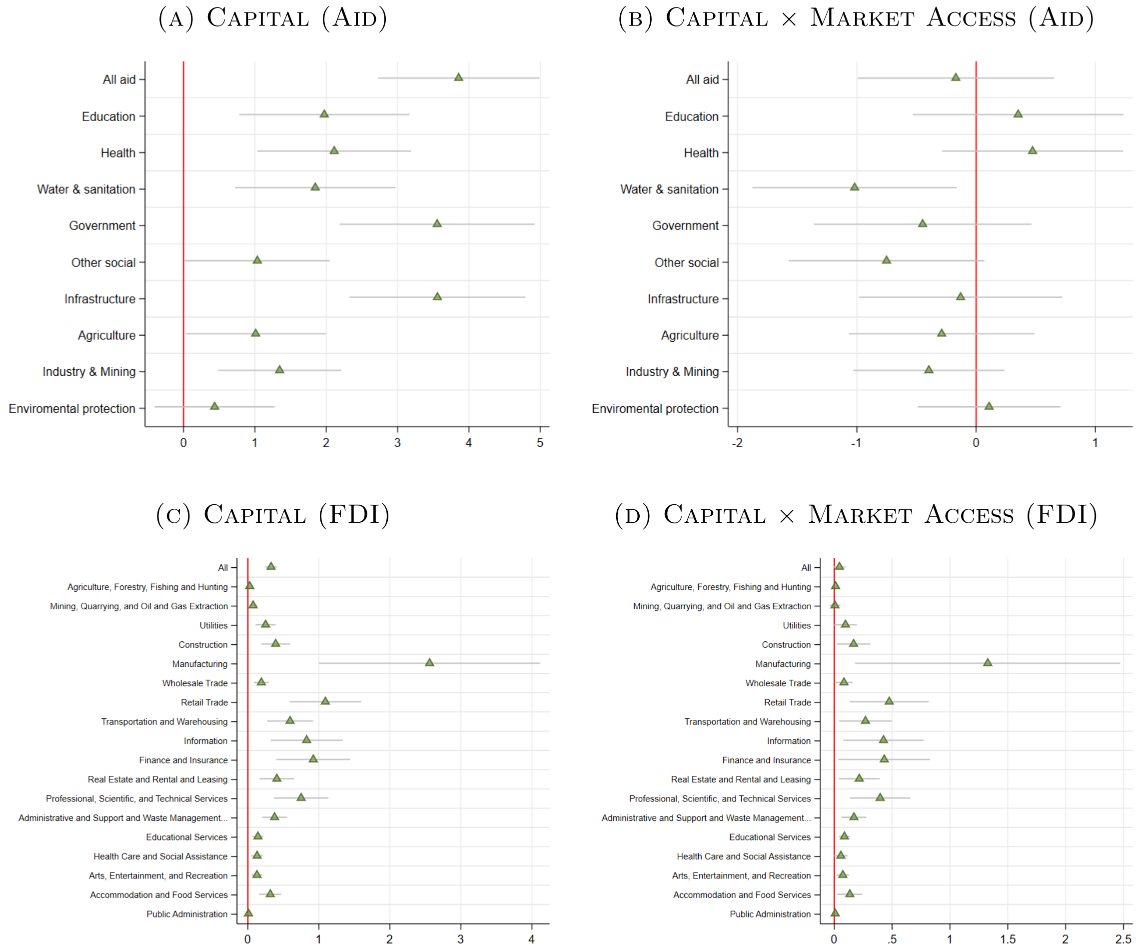
[42] fDi Market is proprietary and available via subscription at www.fdimarkets.com.

[43] We use a subset of these data 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. Restricting the sample to the two highest precision codes ensures we do not mechanically find more projects in subnational capitals, as projects for which less precise geographic information is available are often geocoded to regional capitals.

[44] 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.

[45] We obtain imprecise results using the benchmark event study and difference-in-differences specifications.

Figure IX
Capitals, aid and FDI by sector



Notes: The figure plots estimates from regressions of World Bank development projects (1995–2014) and foreign direct investments (2003–2018) in a particular sector on the fraction of years a city was a capital. Panels A and B show the results from regressions with the log of 1 + commitments in USD on the left-hand side. Panel A reports estimates of the capital city effect at average levels of internal market access. Panel B reports results for the interaction of capital status with market access. The effect of market access on non-capitals is reported in panel A of Figure E-11. The definition of sectors follows the OECD's Common Reporting Standard (see Online Appendix A for details). Panels C and D show the results from regressions with the log of 1 + value of FDI projects in USD on the left-hand side. Panel C reports estimates of the capital city effect at average levels of internal market access. Panel D reports results for the interaction of capital status with market access. The constituting baseline effects of the market access on non-capitals are reported in panel B of Figure E-11. The definition of sectors follows the NAICS 2-digit sector classification (see Online Appendix A for details). 95% confidence intervals based on standard errors clustered on initial regions are provided as error bars.

education, health, water and sanitation, government, and infrastructure.[46] Panel B plots

[46] We supplement this data with geocoded project-level data on China's global footprint of official financial flows 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. Figure E-9 in Online Appendix E shows that Chinese infrastructure investments are particularly concentrated in capital cities, while results for the interaction with market access are qualitatively similar.

estimates of the interaction between capital status and market access. We find little evidence that more aid flows toward capital cities with better market access, suggesting that a differential allocation of public investments is not driving the complementarity.[47] Panels C and D show the corresponding results for foreign direct investments. We find that subnational capitals attract considerably more FDI than non-capital cities. The value of FDI projects in capitals is about 5.7 log points larger. The sectoral composition of FDI also shows an interesting pattern. Capital cities attract considerably more high-value projects in manufacturing and, to a lesser extent, retail trade, information, finance, and insurance. More remarkably, we find evidence of significant and positive interaction of capital city status with market access in several sectors.[48] For example, capital cities that have a standard deviation higher level of market access receive an additional 1.5 log points more FDI in manufacturing (on top of the capital city effect) than other cities with a similar level of market access.[49]

This additional evidence supports the conjecture that capital cities attract substantial public and private investments and suggests that private investments disproportionately flow to capital cities with better economic fundamentals. This helps explain why capital cities with stronger fundamentals experience faster growth.

8. Conclusion

Our findings provide the first evidence that the recent expansion of administrative units and the corresponding shift in political status for some cities to first-order administrative capitals have an impact on the distribution 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 exhibit heterogeneity across two key aspects of the reform decision. First, the capital premium varies based on the size of the jurisdiction and, second, the location of the capital itself. Capitals with greater political importance or those situated in locations with better fundamentals experience substantially faster growth than capitals in less favorable locations or those that preside over smaller regions.

[47]We focus on internal market access but obtain close to identical results using the principal component for internal trade. The other fundamentals provide no clear picture, where the principal component and the specific proxy partly report opposing signs. Additional results are not reported but are available upon request.

[48]We observe a similar pattern using an estimated of jobs created in the project (see Figure E-10 in Online Appendix E. Moreover, Figure E-11 shows that *all* cities with better internal market access receive more development aid and more FDI. Above, we focus on whether this effect differs for capitals with better market access.

[49]We also find similar effects on the extensive margin (see Figure E-12 in Online Appendix E). At average levels of market access, cities that have been capitals during the entire period are about 25% more likely to receive a World Bank project and about 4% more likely to receive foreign direct investments.

We interpret these findings and our analysis of likely channels as evidence that subnational capitals are focal points for migrants and business within regions. Our results also suggest they can be part of a mix of optimal place-based policies in general spatial equilibrium. However, understanding the aggregate welfare effects of their number and location is an important avenue for further research. More broadly, these findings are relevant to policy-makers who decide to decentralize based on various political considerations. Administrative politics and public investments can be a tool for steering agglomeration in 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 in this paper opens the door to studying various questions about capital cities and their role in the administrative hierarchy. Our paper only exploits the varied nature of the underlying motivations and their unpredictability but does not contribute to untangling them. We leave such questions for future research and hope that our global data on administrative cities will be helpful in exploring them.

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Online appendix

A. Data Appendix

A-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 (2022).

Population: density within our cities is calculated 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 a 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 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.

List A-1
Countries in sample

Afghanistan (L,A,U); Albania (L,A,U); Algeria (L,A,U); Angola (L,A,U); Argentina (E,D,F); Australia (E,D,F); Austria (E,D,F); Bangladesh (L,D,U); Belarus (L,D,U); Belgium (E,D,F); Benin (L,D,U); Bolivia (L,D,U); Brazil (L,D,F); Bulgaria (L,D,U); Burkina Faso (L,A,U); Burundi (L,A,U); Cambodia (L,A,U); Cameroon (L,A,U); Canada (E,D,F); Central African Republic (L,A,U); Chad (L,A,U); Chile (E,D,U); China (L,A,U); Colombia (L,D,U); Congo (L,A,U); Costa Rica (L,D,U); Cuba (E,A,U); Czech Republic (E,D,U); Cote d'Ivoire (L,A,U); Democratic Republic of the Congo (L,A,U); Denmark (E,D,U); Dominican Republic (L,A,U); Ecuador (L,D,U); Egypt (L,A,U); Eritrea (L,A,U); Estonia (E,D,U); Ethiopia (L,A,F); Finland (E,D,U); France (E,D,U); Georgia (E,A,U); Germany (E,D,F); Ghana (L,A,U); Greece (E,D,U); Guatemala (L,A,U); Guinea (L,A,U); Haiti (L,A,U); Honduras (L,D,U); Hungary (E,D,U); India (L,D,F); Indonesia (L,A,U); Iran (L,A,U); Iraq (L,A,U); Ireland (E,D,U); Italy (E,D,U); Japan (E,D,U); Kazakhstan (E,A,U); Kenya (L,A,U); Korea, North (L,A,U); Korea, South (L,D,U); Kyrgyzstan (L,A,U); Lao People's Democratic Republic (L,A,U); Latvia (E,D,U); Lesotho (L,A,U); Liberia (L,A,U); Madagascar (L,D,U); Malawi (L,A,U); Malaysia (L,A,F); Mali (L,A,U); Mauritania (L,A,U); Mexico (E,A,F); Moldova (L,A,U); Mongolia (L,D,U); Morocco (L,A,U); Mozambique (L,A,U); Myanmar (L,A,U); Nepal (L,A,U); Netherlands (E,D,U); New Zealand (E,D,U); Nicaragua (L,D,U); Niger (L,A,U); Nigeria (L,A,F); Norway (E,D,U); Oman (L,A,U); Pakistan (L,D,F); Panama (L,D,U); Papa New Guinea (L,A,U); Paraguay (L,D,U); Peru (E,A,U); Philippines (L,D,U); Poland (E,D,U); Portugal (L,D,U); Romania (L,A,U); Russian Federation (E,A,F); Rwanda (L,A,U); Saudi Arabia (L,A,U); Senegal (L,A,U); Sierra Leone (L,A,U); Slovakia (L,D,U); Somalia (L,A,U); South Africa (E,D,U); South Sudan (L,A,F); Spain (E,D,F); Sri Lanka (L,A,U); Sudan (L,A,F); Sweden (E,D,U); Switzerland (E,D,F); Syrian Arab Republic (L,A,U); Taiwan (L,D,U); Tajikistan (L,A,U); Thailand (L,D,U); Togo (L,A,U); Tunisia (L,A,U); Turkey (L,D,U); Turkmenistan (E,A,U); U.K. of Great Britain and Northern Ireland (E,D,U); Uganda (L,A,U); Ukraine (L,D,U); United Arab Emirates (E,A,F); United Republic of Tanzania (L,A,U); United States of America (E,D,F); Uruguay (E,D,U); Uzbekistan (L,A,U); Venezuela (E,D,F); Viet Nam (L,A,U); Yemen (L,A,U); Zambia (L,D,U); Zimbabwe (L,A,U)

Notes: The list depicts the countries covered in our study. The letter in parenthesis indicate to which cross-country classification with respect to early-late urbanizer (E/L), political system (autocracy/democracy) and federal vs. unitary country they are assigned.

A-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 et al., 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 et al. (2020) who also use the DHS-WHO joint monitoring program definitions.

At least 8 years of schooling indicator: Is a dummy variable that is unity if the respondent has completed 8 or more years of schooling (based on V107) and zero otherwise. It is only defined for respondents who are at least 16 years old.

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 children of female respondents (variables b13-1 to b13-20). As common in the literature, we use the individual-child-level data to compute this measure and multiply the resulting dummy by 1000 to estimate a rate ("per thousand births").

Log household size: is the log of the number of household members (v136).

Female head of household indicator: defined according to the reported gender of the household head (v151).

Log head of household age: is the log of age (in years) of the household head (v152).

Household head completed primary education indicator: is calculated based an the educational achievement variable (v149) if the respondent is the household head (v150). The indicator is unity if the household head has completed primary education or started but not finished secondary education (v149). The indicator is zero otherwise.

Household head completed secondary education indicator: is calculated based an the educational achievement variable (v149) if the respondent is the household head (v150). The indicator is unity if the household head has completed secondary education (v149). The indicator is zero otherwise.

Household head completed higher education indicator: is calculated based an the educational achievement variable (v149) if the respondent is the household head (v150). The indicator is unity if the household head has engaged in higher education (v149). The indicator is zero otherwise.

Age in years of the respondent (v012 and mv012) in the DHS. Also included as a squared term.

Female indicator taking unity for all respondents in the IR dataset of the DHS and zero for all respondents in the MR dataset of the DHS.

Sex indicator for respondents children, takes unity if the respondent child is female (b4-01 to b4-20).

Multiple birth indicator is unity if a respondents child was born either as a twin or multiple (b0-01 to b0-20).

Period of birth indicator: Indicator for the period of birth of the reported children (by decade, i.e, 1990s).

List A-2
DHS survey sample

AGO (2006,2007,2011); ALB (2008); ARG (2008); BDI (2010,2011,2012,2013); BEN (1996,2001,2011,2012); BFA (1992,1993,1998,1999,2003,2010); BOL (2008); BRA (2008); CAF (1994,1995); CIV (1994,1998,1999,2011,2012); CMR (2004,2011); COD (2007,2013); COL (2010); DOM (2007,2013); EGY (1992,1995,2000,2003,2005,2008); GHA (1993,1994,1998,1999,2003,2008,2013); GIN (1999,2005,2012); HND (2011); HTI (2000,2006,2007,2012); IDN (2003); KEN (2003,2008,2009); KGZ (2012); LBR (2006,2007,2008,2009,2011,2013); LSO (2004,2005,2009,2010); MAR (2003); MDA (2005); MDG (1997,2008,2009,2011,2013); MLI (1995,1996,2001,2006,2012,2013); MOZ (2009,2011); MWI (2000,2004,2005,2010,2012); NER (1992,1998); NGA (2003,2008,2010,2013); PAK (2006); PER (2000,2004,2009); PHL (2003,2008); RWA (2005,2008,2010,2011); SEN (1992,1993,1997,2005,2008,2009,2010,2011,2012); SLE (2008,2013); TGO (1998,2013); TJK (2012); TZA (1999,2003,2004,2007,2008,2009,2010,2011,2012); UGA (2000,2001,2006,2008,2009,2011); ZMB (2007,2013); ZWE (1999,2005,2006,2010,2011)

Notes: The list depicts the countries and survey years for which we match DHS clusters to our cities.

A-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., 2020). 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.

A-4. 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.

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

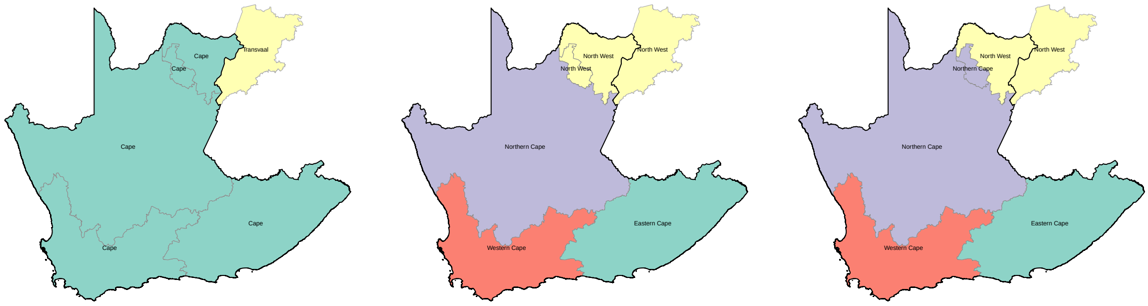
We begin by backing out a reform tree from the GAUL data using a simple spatial algorithm. For any pair of two years, we create the spatial intersection of the two vector data sets. This creates new areas or new affiliations whenever a border is moved, deleted or created. We then cycle forward by intersecting 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.

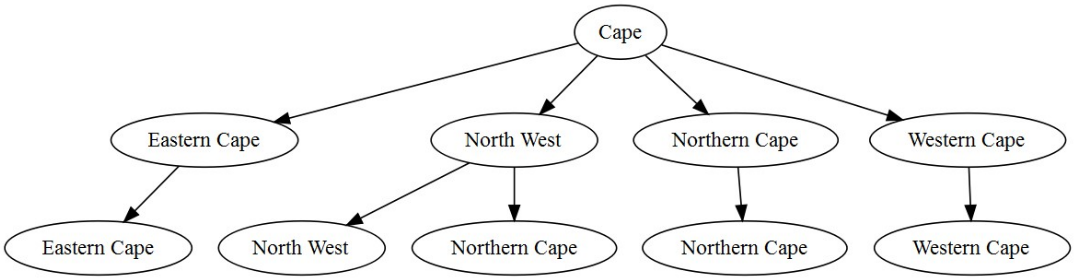
Figure B-1 provides an illustration of the two data sets created by this process. It shows the reform history of Cape Province in South Africa from 1992 onward (the green area in panel A). In 1994, the Cape Province was split into four new regions (panel B). Three of the successor provinces are congruent 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

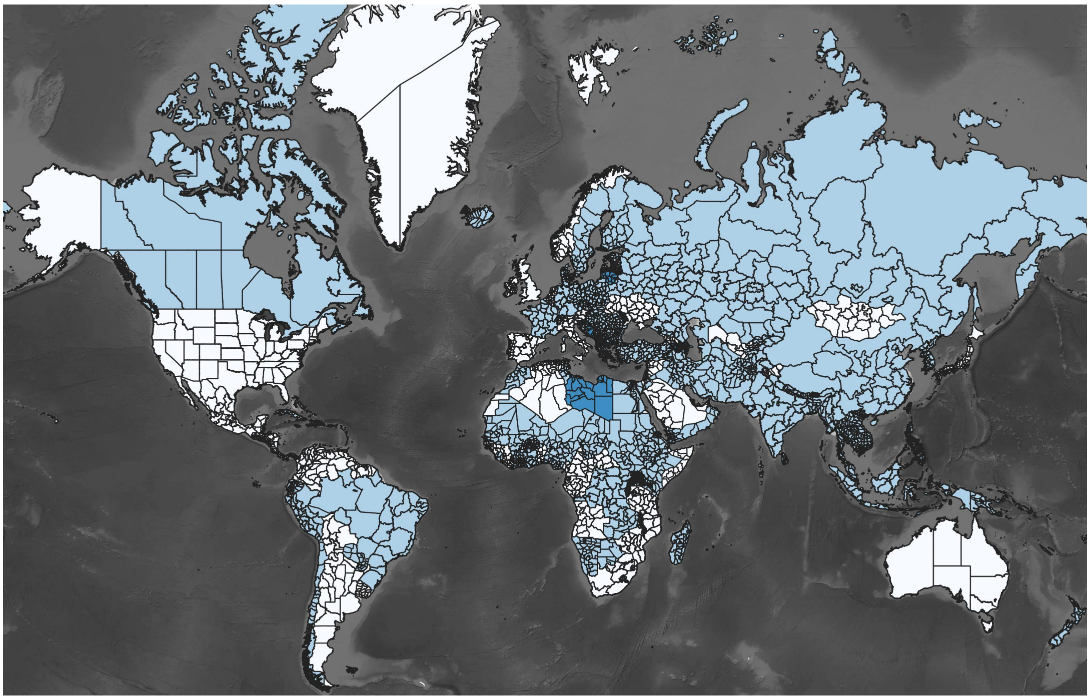


Notes: Panels A to C illustrate initial and successor regions of the Cape Province in South Africa. Panel D illustrates the evolutionary tree for the splinters which were formerly part of Cape Province, South Africa. The last level represents the situation after the 2005 reform.

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, 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 and the Google Maps' geocoding API. 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 (2021), who propose that central government planners in historical China face a trade-off when determining the location of regional capital cities. Being close to citizens implies that the administrative 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). Note that the argument holds for all locations of national capital, which might be isolated itself to put distance between autocratic rulers and their populations (Campante et al., 2019). 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.

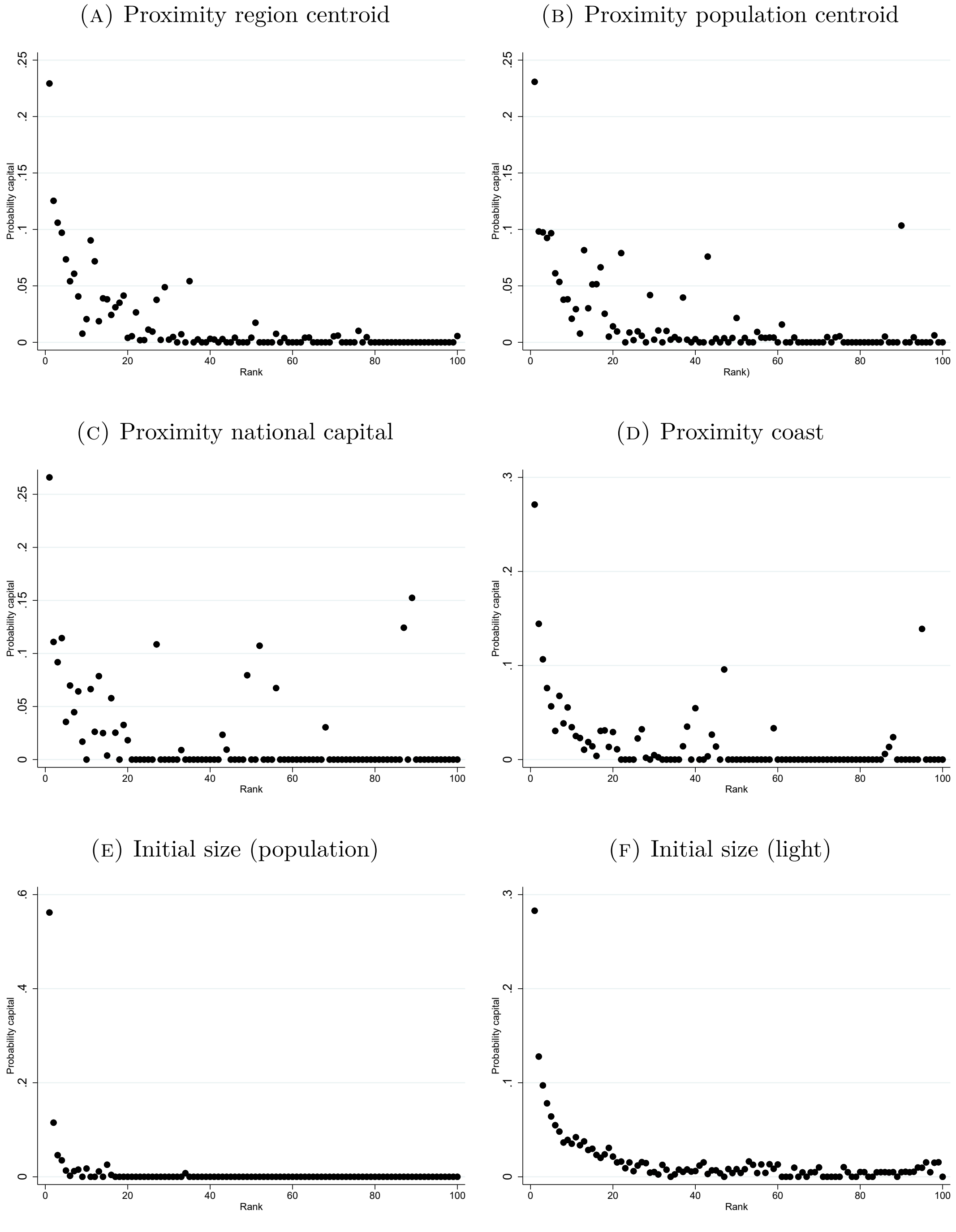
Panels A to C of Figure C-1 provide some evidence in favor of the idea that hierarchical distance also matters in our global sample of contemporary capital city reforms. We rank cities within regions with respect to their distance to the region centroid in panel A, their distance to the population-weighted 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. We find a few outliers where high ranks have a high probability of becoming a capital (due to a few regions in South Asia with relatively "remote" 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



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 detection 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

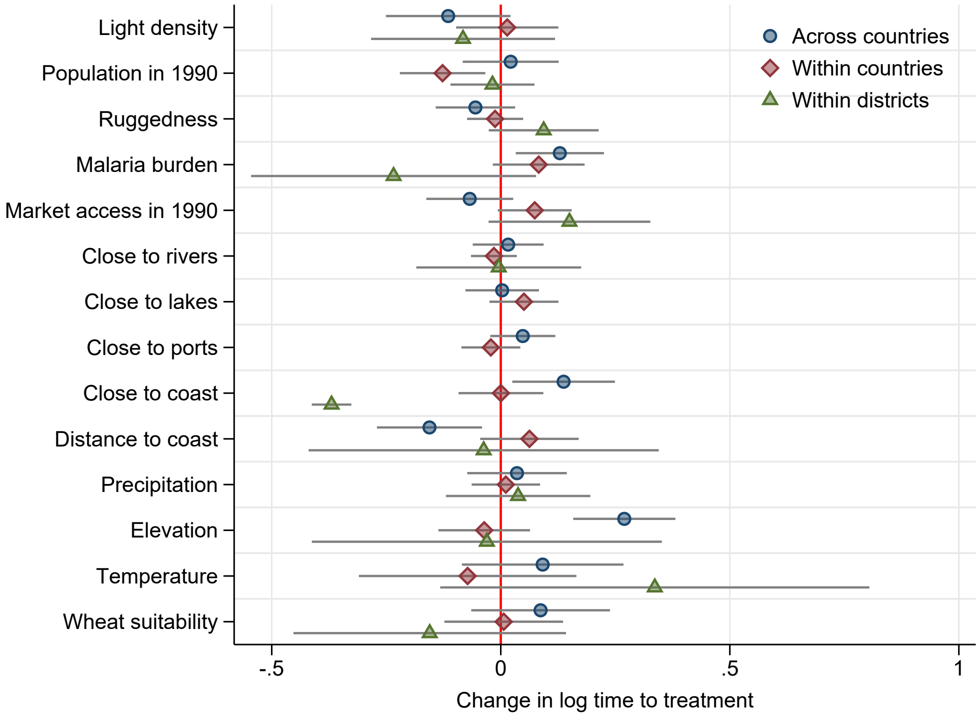
	Dependent Variable: Δ DETECTED$_{ci}$					
	All Cities			Reformed Regions		
	(1)	(2)	(3)	(4)	(5)	(6)
CAPITAL	0.1401	0.2184	0.2464	0.1478	0.2269	0.2672
	(0.0396)	(0.0404)	(0.0475)	(0.0583)	(0.0635)	(0.0736)
Fundamentals	✓	✓	✓	✓	✓	✓
Country FE	–	✓	✓	–	✓	✓
Initial-Region FE	–	–	✓	–	–	✓
City-unions	27906	27906	27906	10213	10213	10213

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. Additional results

E-1. Additional figures

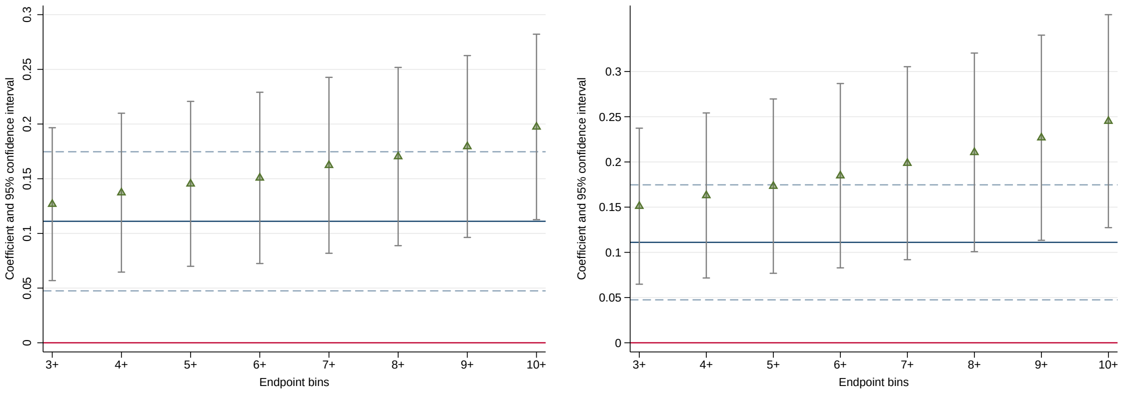
FIGURE E-1
Time to treatment



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

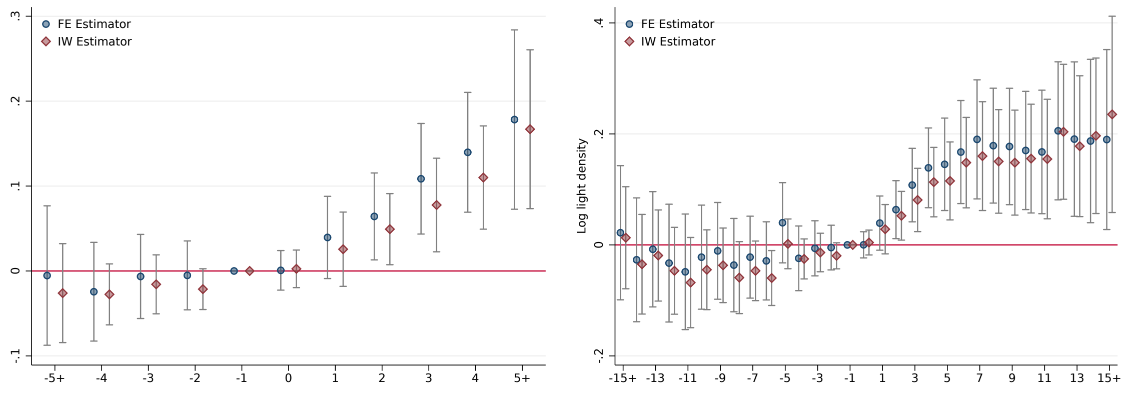
(A) All cities | (B) Cities in reformed regions



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 E-1. 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 E-3
TWFE versus IW estimator of dynamic treatment effects

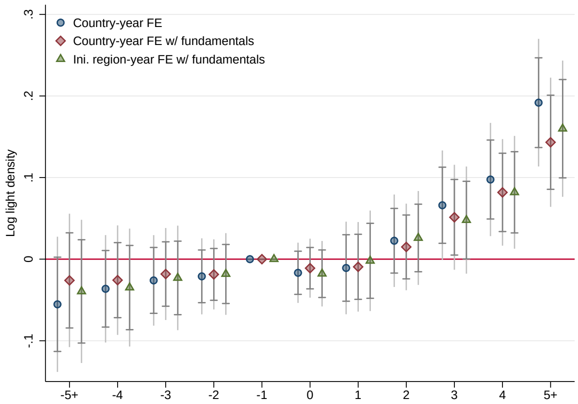
(A) 5-year event window | (B) 15-year event window



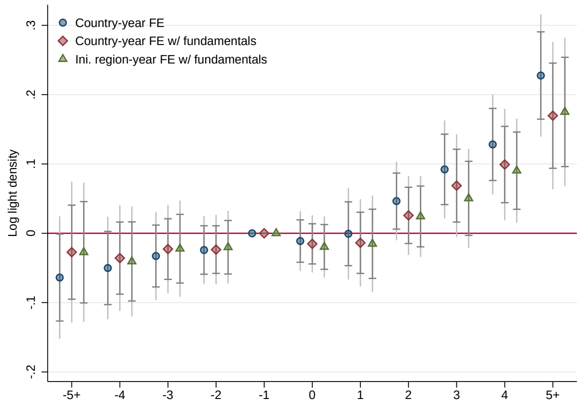
Notes: The figure illustrates event-study results from fixed effects regressions of the log of light intensity per square kilometer on a binned sequence of treatment change dummies, city fixed effects, initial-region-by-year fixed effects, time-varying locational fundamentals for a panel that is balanced in calendar time. Circles represent point estimates from two-way fixed effects estimation (TWFE). Diamonds represent point estimates from interaction-weighted (IW) estimation (see Sun and Abraham, 2021). Panel A shows estimates a five-year event window. Panel A shows estimates a 15-year event window. 95% confidence intervals based on standard clustered on initial regions are provided by the gray error bars.

FIGURE E-4
Agglomerations: Event-study estimates

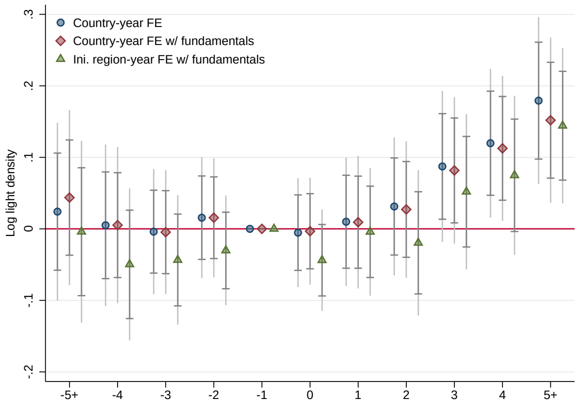
(A) All cities (core)



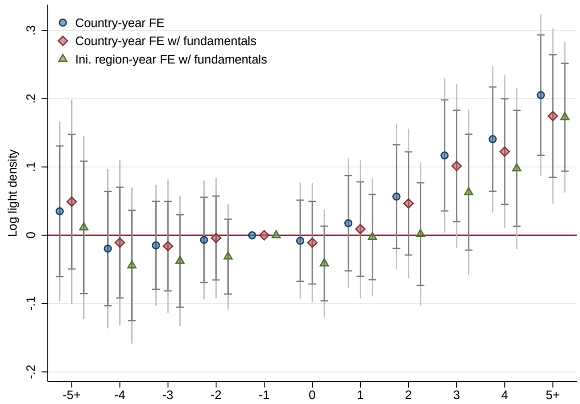
(B) Cities in reformed regions (core)



(C) All cities (fringe)



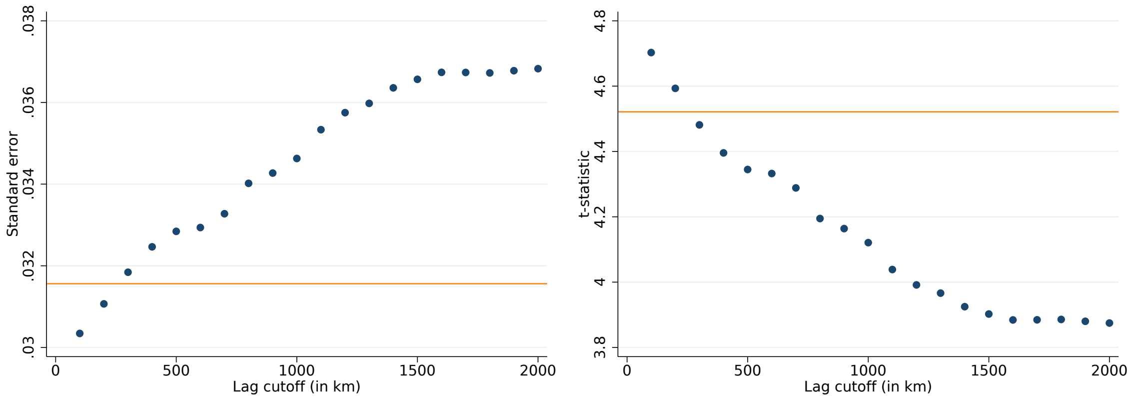
(D) Cities in reformed regions (fringe)



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. Circles represent point estimates from a regression with city and country-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 regions are provided by the gray error bars. The orange error bars indicate 95% sup-t bootstrap confidence bands with block sampling over initial regions (Montiel Olea and Plagborg-Møller, 2019).

Figure E-5
Accounting for spatial autocorrelation

(a) Standard errors (b) *t*-statistics

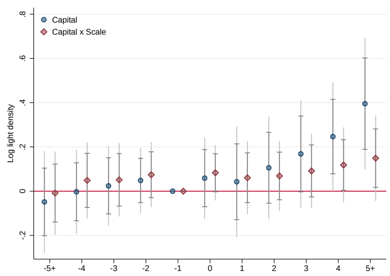


Notes: The figure illustrates results from varying the spatial lag cutoff when estimating standard errors which allow for cross-sectional dependence. All results are based on a variant of column 6 in Table E-1 where we restrict the sample to reformed areas and include city fixed effects, as well as initial-region fixed effects. Here we omit the time-varying effects of the fundamentals for computational reasons (to reduce the size of the regressor matrix small). The estimated effect in this specification is 0.1427 with a standard error of 0.0316. 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.

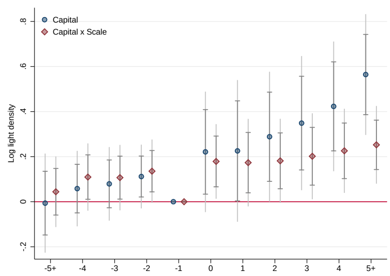
Figure E-6

Scale: Event-study estimates

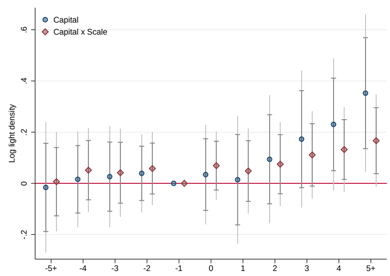
(A) Pop region



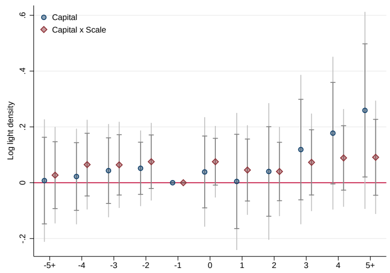
(B) Urb. pop region



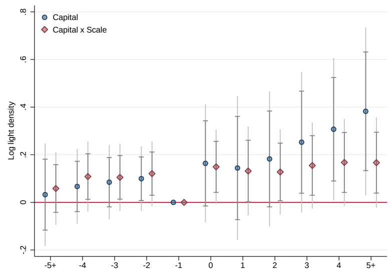
(C) # cities region



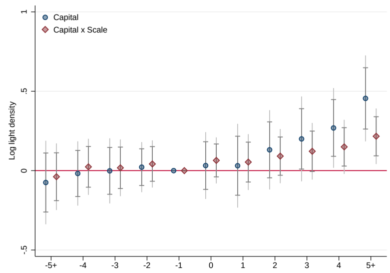
(D) Pop region (controls)



(E) Urb. pop region (controls)



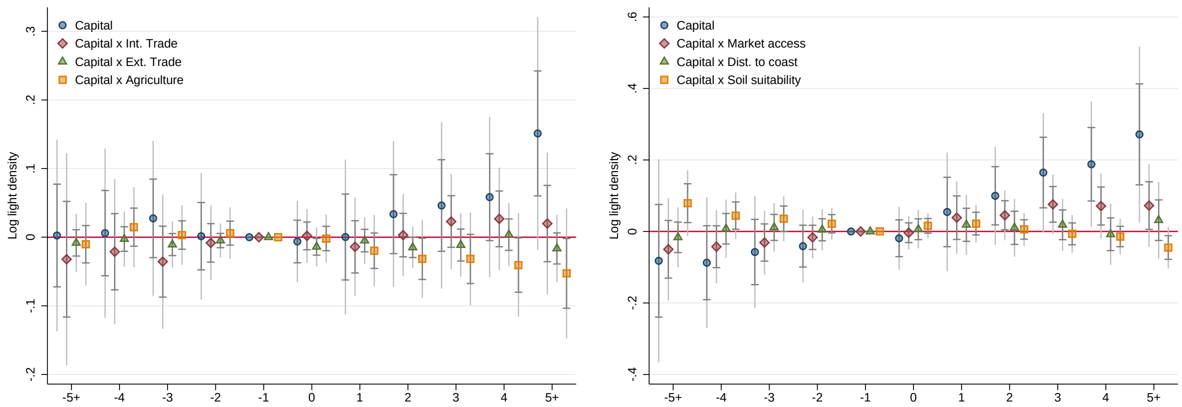
(F) # cities region (controls)



Notes: The figure reports event-study estimates corresponding to the difference-in-differences results presented in Table IV. 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 regions are provided by the gray error bars. The orange error bars indicate 95% sup-t bootstrap confidence bands with block sampling over initial regions (Montiel Olea and Plagborg-Møller, 2019).

Figure E-7
Fundamentals: Event-study estimates

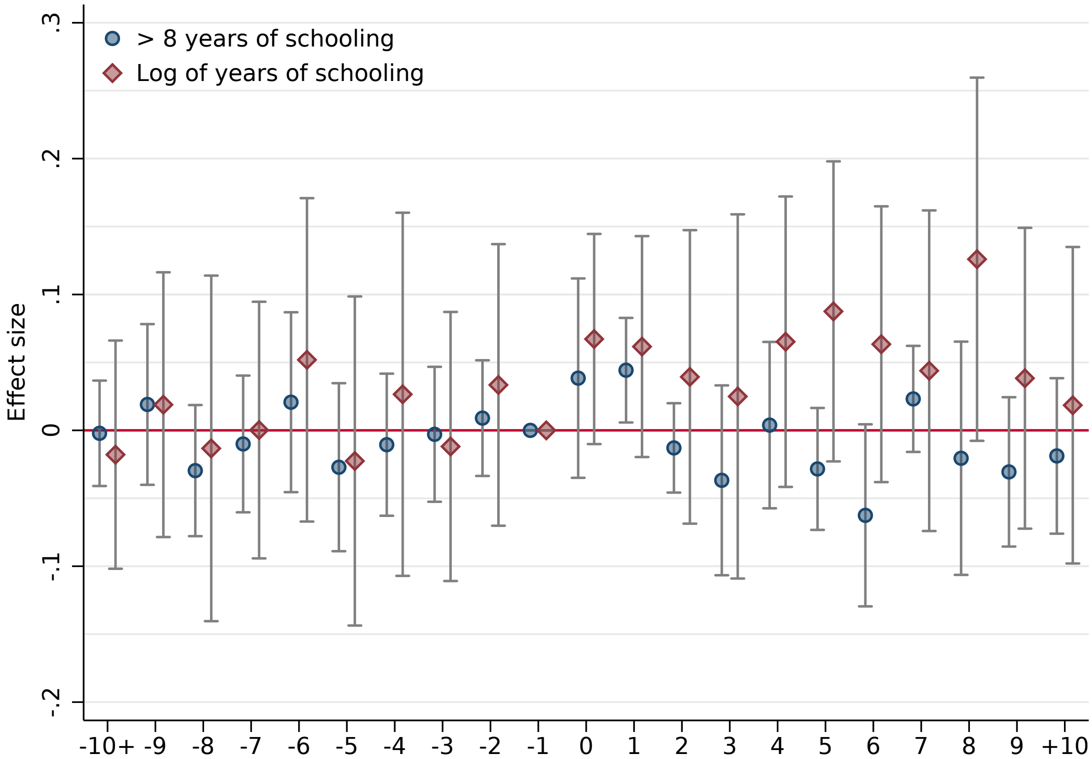
(A) Principal components (B) Single fundamentals



Notes: The figure reports event-study estimates corresponding to the difference-in-differences results presented in Table V. 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 regions are provided by the gray error bars. The orange error bars indicate 95% sup-t bootstrap confidence bands with block sampling over initial regions (Montiel Olea and Plagborg-Møller, 2019).

Figure E-8

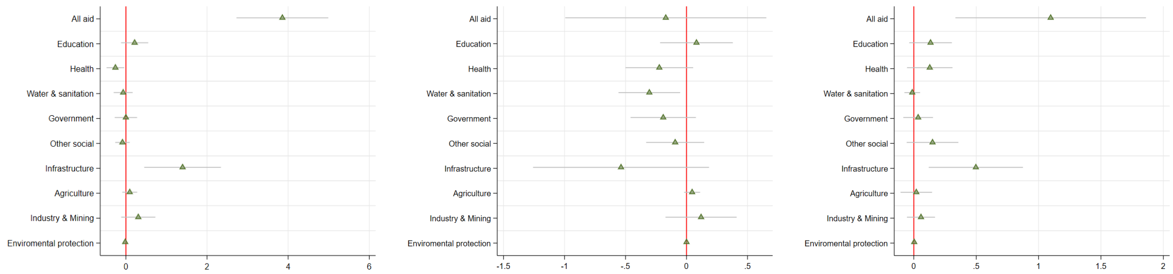
Selective migration: Within city evidence (long window)



Notes: The figure illustrates event-study results from fixed effects regressions of the more than 8 years of schooling dummy (blue circles) and log years of schooling (red triangles) on the binned sequence of treatment change dummies defined in the text. All specifications include the following individual-level controls: A gender dummy, a born-in-city dummy, age, and age squared. All specifications include city-year and cohort-at-move fixed effects as defined in the text. 95% confidence intervals based on standard clustered on the city level are provided by the gray error bars.

Figure E-9
Chinese aid by sector (2000-2014): Log commitments

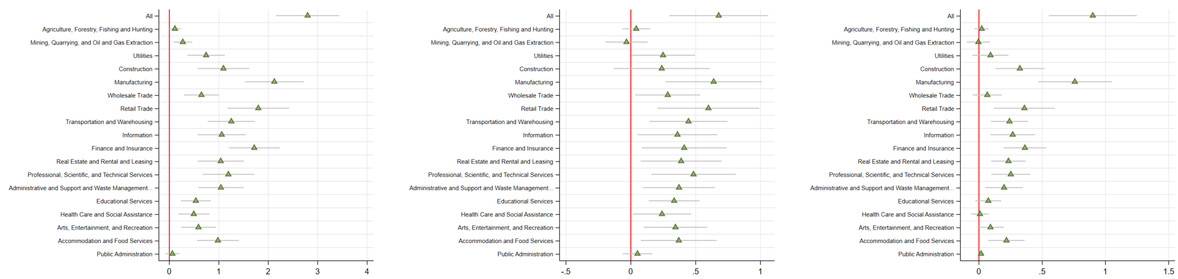
(a) Capital (b) Capital × MA (c) Market Access



Notes: The figure plots estimates from regressions of Chinese development projects (2000–2014) in a particular sector on the fraction of years a city was a capital. Panel A reports estimates of the capital city effect at average levels of internal market access. Panel B reports results for the interaction of capital status with market access. Panel C reports results for the market access baseline effect. The definition of sectors follows the OECD's Common Reporting Standard (see Online Appendix A for details).95% confidence intervals based on standard errors clustered on initial regions are provided as error bars.

Figure E-10
Capitals and FDI by industry (2003-2018): Ln Jobs

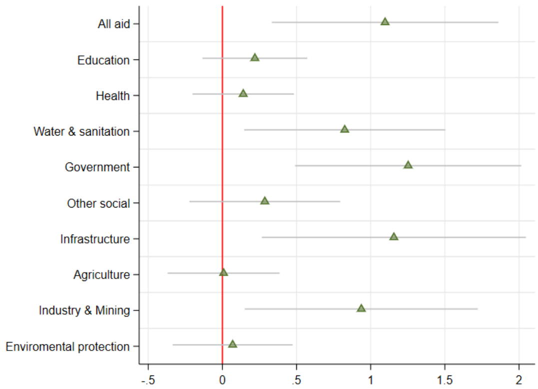
(a) Capital (b) Capital × MA (c) Market Access



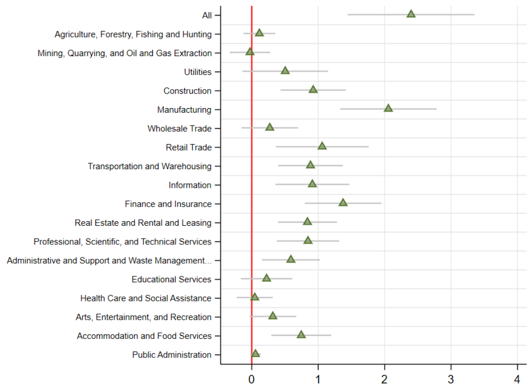
Notes: The figure plots estimates from regressions of log FDI created jobs + 1 in a particular sector on the fraction of years a city was a capital. Panel A reports estimates of the capital city effect at average levels of internal market access. Panel B reports results for the interaction of capital status with market access. Panel C reports results for the market access baseline effect. The definition of sectors follows the NAICS 2-digit sector classification (see Online Appendix A for details). 95% confidence intervals based on standard errors clustered on initial regions are provided as error bars.

FIGURE E-11
Cities, aid and FDI: Market access effect

(A) Aid (ln commitments WB)



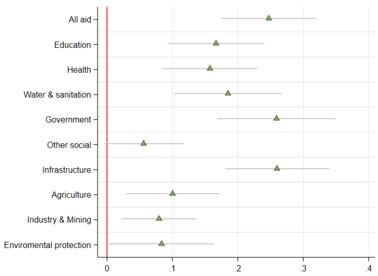
(B) FDI (Ln value)



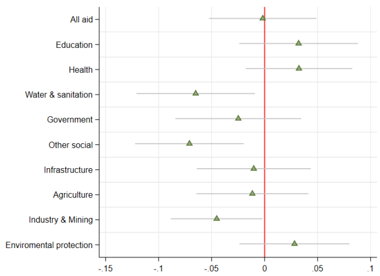
Notes: The figure plots estimates from regressions of the log of aid commitments +1 (WB) and log FDI projects values +1 on non-capital cities market access in 1990, the full set of fundamental controls and initial region fixed effects. FDI projects in a particular sector on the fraction of years a city was a capital. The FDI definition of sectors follows the NAICS 2-digit sector classification (see Online Appendix A for details). 95% confidence intervals based on standard errors clustered on initial regions are provided as error bars.

Figure E-12
Aid and FDI: Extensive margin

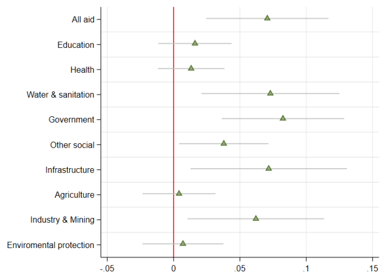
(a) Capital (WB)



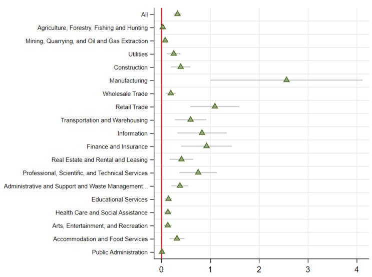
(b) Capital × MA (WB)



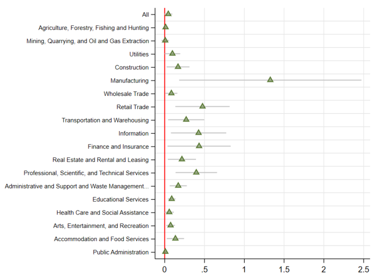
(c) Market Access (WB)



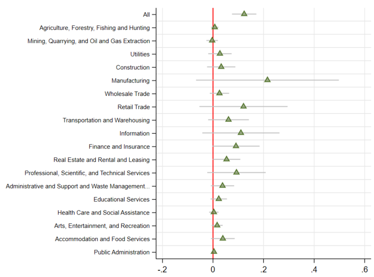
(d) Capital (FDI)



(e) Capital × MA (FDI)



(f) Market Access (FDI)



Notes: The figure plots estimates from regressions of an indicator for the presence of at least one aid project (between 1994 and 2014) or fdi investment (between 2003 and 2018) on the fraction of years a city was a capital (in the respective time period). Panel A (D) reports estimates of the capital city effect at average levels of internal market access. Panel B (E) reports results for the interaction of capital status with market access. Panel C (F) reports results for the market access baseline effect. The definition of sectors follows the OECD's Common Reporting Standard (see Online Appendix A for details) The definition of sectors follows the NAICS 2-digit sector classification (see Online Appendix A for details). 95% confidence intervals based on standard errors clustered on initial regions are provided as error bars.

E-2. Additional tables

Table E-1
Baseline differences-in-differences

	Dependent Variable: $\ln \text{Lights}_{cit}$					
	All Cities			Reformed Regions		
	(1)	(2)	(3)	(4)	(5)	(6)
Capital	0.1085	0.0886	0.1089	0.1433	0.1156	0.1132
	(0.0281)	(0.0276)	(0.0287)	(0.0303)	(0.0314)	(0.0331)
Fundamentals	–	✓	✓	–	✓	✓
City FE	✓	✓	✓	✓	✓	✓
Country-Year FE	✓	✓	–	✓	✓	–
Ini. Region-Year FE	–	–	✓	–	–	✓
N	23870	23870	23870	8438	8438	8438
$N \times \bar{T}$	524009	524009	524009	184687	184687	184687

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 E-2
City area changes: 1990–2015

	Dependent Variable: $\Delta \ln \text{area}_{cit}$			
	All Cities		Reformed Regions	
	(1)	(2)	(3)	(4)
Capital	0.1041	0.0941	0.1327	0.0991
	(0.0094)	(0.0137)	(0.0184)	(0.0265)
Fundamentals	–	✓	–	✓
Initial-Region FE	✓	✓	✓	✓
Cities	20740	20740	7501	7501

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 E-3

Agglomerations and fringes defined by buffers: 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	0.1366	0.1062	0.1331	0.1750	0.1342	0.1343
	(0.0316)	(0.0304)	(0.0315)	(0.0337)	(0.0347)	(0.0365)
Panel B. Growth in the periphery of the city						
CAPITAL	0.1460	0.1077	0.1365	0.1822	0.1329	0.1367
	(0.0316)	(0.0297)	(0.0307)	(0.0335)	(0.0337)	(0.0353)
N	23360	23360	23360	8302	8302	8302
$N \times \bar{T}$	512929	512929	512929	181810	181810	181810
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 (based on a common growth rate for all cities in a country as in Harari, 2020). Panel B reports the results for the fringe (the buffer areas net of the initial core). Standard errors clustered on initial regions are provided in parentheses.

TABLE E-4

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	0.0971	0.0977	0.0982	0.0859	0.0868	0.0837
	(0.0223)	(0.0231)	(0.0231)	(0.0243)	(0.0244)	(0.0249)
F-test pre-trends (p-val.)	0.325	0.155	0.146	0.596	0.632	0.763
N	797	1017	1215	765	984	1184
$N \times \bar{T}$	17328	22104	26410	16658	21432	25792

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 E-5

Cross country heterogeneity: Difference-in-differences

	Dependent Variable: ln LIGHTS$_{cit}$					
	(1)	(2)	(3)	(4)	(5)	(6)
CAPITAL × EARLY	0.0768	0.0468				
	(0.0651)	(0.0703)				
CAPITAL × LATE	0.1807	0.1366				
	(0.0370)	(0.0445)				
CAPITAL × AUTOCRACY			0.2184	0.1734		
			(0.0480)	(0.0539)		
CAPITAL × DEMOCRACY			0.1072	0.0688		
			(0.0327)	(0.0414)		
CAPITAL × UNITARY					0.1443	0.1108
					(0.0383)	(0.0438)
CAPITAL × FEDERAL					0.2828	0.1968
					(0.0473)	(0.0650)
F-test pre-trends (p-val.)	0.285	0.368	0.887	0.992	0.821	0.882
Fundamentals	–	✓	–	✓	–	✓
City FE	✓	✓	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓	✓	✓
N	8101	8101	8101	8101	8097	8097
$N \times \bar{T}$	177381	177381	177209	177209	177293	177293

Notes: Columns 1 and 2 of the table report 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) (urbanization in 1950 classification). Columns 3 and 4 interact the capital dummy with autocracy (non-democracy) and democracy (polity 2 score ≥ 6). Columns 5 and 6 interact the capital indicator with a unitary (non-federal) and federal indicator based onTreisman (2008). All regressions include city fixed effects and initial-region-by-year fixed effects. 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 provinces are provided in parentheses.

Table E-6

Fiscal decentralization: Difference-in-differences

	Dependent Variable: ln Lights$_{cit}$			
	Revenue share		Employment share	
	(1)	(2)	(3)	(4)
Capital	0.2555	0.1495	0.3195	0.2682
	(0.0701)	(0.0808)	(0.0597)	(0.0753)
Capital × Fiscal decentralization	0.1431	0.0887	0.2344	0.2213
	(0.0688)	(0.0671)	(0.0985)	(0.0955)
Fundamentals	–	✓	–	✓
City FE	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓
N	5479	5479	5599	5599
$N \times \bar{T}$	120337	120337	123090	123090

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 proxies for the degree of fiscal decentralization taken from Treisman (2008). Specifically; the subnational revenue share as a percentage of GDP (averaged 1994-2000), and the subnational government employment share (in 1997). The interactions of the capital city status with the proxies for the degree of fiscal decentralization ($\tilde{z}$) are standardized such that $\tilde{z} \equiv (z - \bar{z})/\sigma_z$. Standard errors clustered on initial regions are provided in parentheses.

Table E-7

Different light measures

	Dependent Variable: ln 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	0.0395	0.0899	0.0876	0.0899	0.1132
	(0.0311)	(0.0344)	(0.0306)	(0.0344)	(0.0331)
Fundamentals	✓	✓	✓	✓	✓
City FE	✓	✓	✓	✓	✓
Ini. Region-Year	✓	✓	✓	✓	✓
N	7299	8438	8438	8438	8438
$N \times \bar{T}$	135022	184687	184687	184687	184687

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 E-8
Initial city size

	Dependent Variable: $\ln$ Lights$_{cit}$				
	Initial city size				
	30k	40k	50k	75k	100k
Capital	0.1287	0.1456	0.1712	0.1603	0.1942
	(0.0333)	(0.0356)	(0.0372)	(0.0407)	(0.0546)
Fundamentals	✓	✓	✓	✓	✓
City FE	✓	✓	✓	✓	✓
Ini. Region-Year	✓	✓	✓	✓	✓
N	5608	4078	3133	1925	1363
$N \times \bar{T}$	122726	89187	68484	42064	29783

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 E-9
Ethnic diversity

	Dependent Variable: $\ln$ Lights$_{cit}$					
	All Cities			Reformed Regions		
	(1)	(2)	(3)	(4)	(5)	(6)
Capital	0.1085	0.0886	0.1081	0.1410	0.1145	0.1115
	(0.0281)	(0.0275)	(0.0286)	(0.0308)	(0.0314)	(0.0328)
Capital × ELF	-0.0025	-0.0029	0.0067	0.0132	0.0082	0.0124
	(0.0192)	(0.0184)	(0.0196)	(0.0205)	(0.0207)	(0.0214)
Fundamentals	–	✓	✓	–	✓	✓
City FE	✓	✓	✓	✓	✓	✓
Ini. Region-Year FE	✓	✓	✓	✓	✓	✓
N	23809	23809	23809	8378	8378	8378
$N \times \bar{T}$	522847	522847	522847	183547	183547	183547

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 E-10
Long differences (1992-2013): Larger agglomerations

	Dependent Variables: Change in			
	ln LIGHTS$_{ci}$	ln POP DENSITY$_{ci}$	ln LIGHTS P.C.$_{ci}$	ln URBAN INDEX$_{ci}$
	(1)	(2)	(3)	(4)
CAPITAL	0.3419	0.2513	0.0905	0.0321
	(0.0382)	(0.0410)	(0.0561)	(0.0073)
Fundamentals	✓	✓	✓	✓
Initial-Region FE	✓	✓	✓	✓
N	6933	6933	6933	5950

Notes: The table reports results from long difference regressions of the change in log light density of a city over different epochs on the fraction of years in which a city is a capital (1992-2013). ln LIGHTS$_{ci}$ is log light density, ln POP DENSITY$_{ci}$ is log population density (where we take the closest population values 1990 and 2015), ln LIGHTS P.C.$_{ci}$ is log light per capita and ln URBAN INDEX$_{ci}$ is the remotely sensed urban index for built-up structures (re-scaled from -1 to 1 to 0 to 2). Standard errors clustered on initial regions are provided in parentheses.

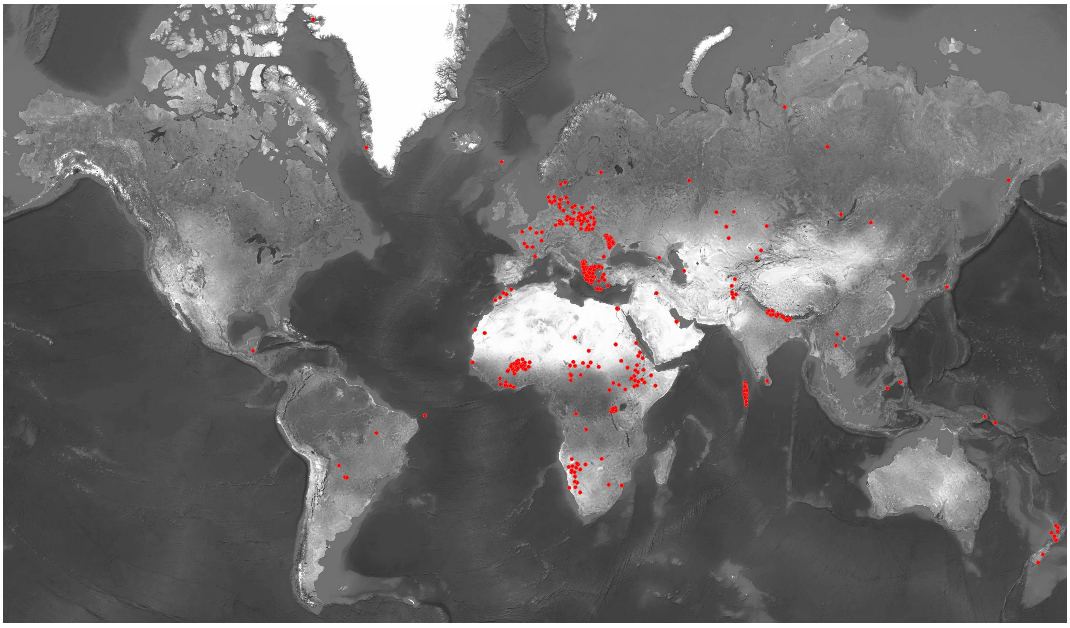
F. Former capitals and "mother" capitals

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 that lose the capital status relative to their peers (cities that remain capitals).

F-1. Former capitals

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

FIGURE F-1
Spatial distribution: Capital loss



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

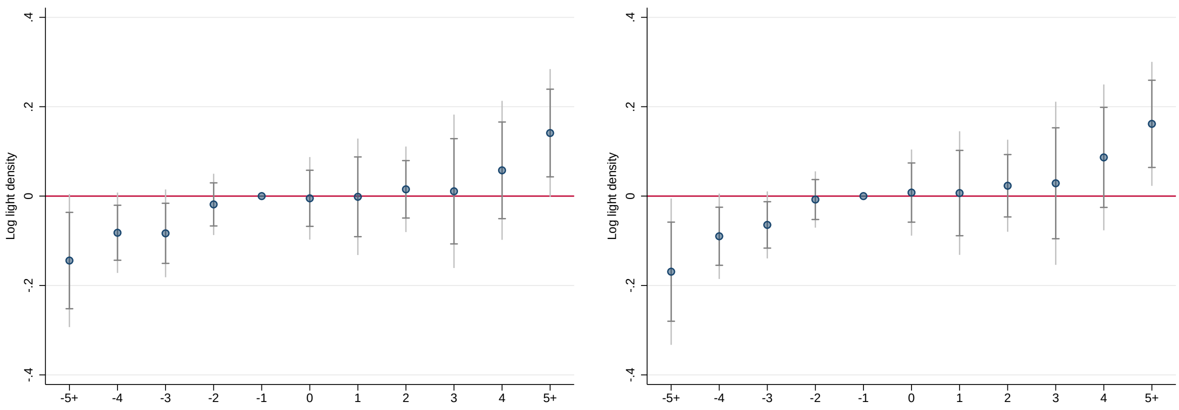
We first turn to our baseline specification which uses other non-capital cities as the control group. Figure F-2 reports event-study estimates using our preferred specification with initial-region-by-year fixed effects and controls for locational fundamentals. There are significant and negative pre-trends. 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 F-2

Former capitals vs. all cities

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



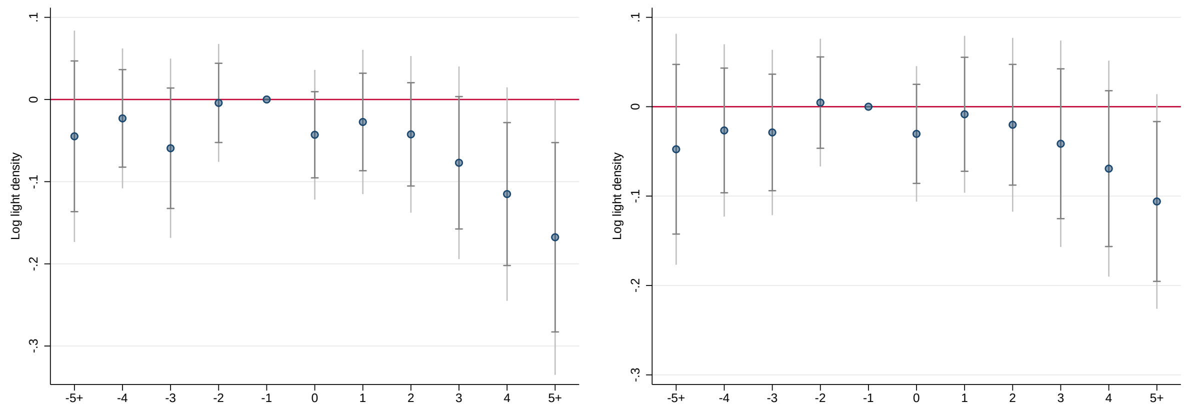
Notes: The figure illustrates results from fixed effects regressions of the log of light intensity per square kilometer on the binned sequence of treatment change dummies (capital loss) defined in the text. Panel A shows estimates for all ever capital cities based on a specification with country-year 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 regions 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 F-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 compared 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 its status and the other city becomes the capital of the whole region. Note that focusing on mergers has also implications for the type of 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 capital status are not declining relative to always capitals prior to treatment. After the capital status is removed, 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 F-3
Former capitals vs. always capitals

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



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 regions are provided by the gray error bars.

F-2. "Mother" capitals

A related issue to the loss of a political premium is the effect of decentralization 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.

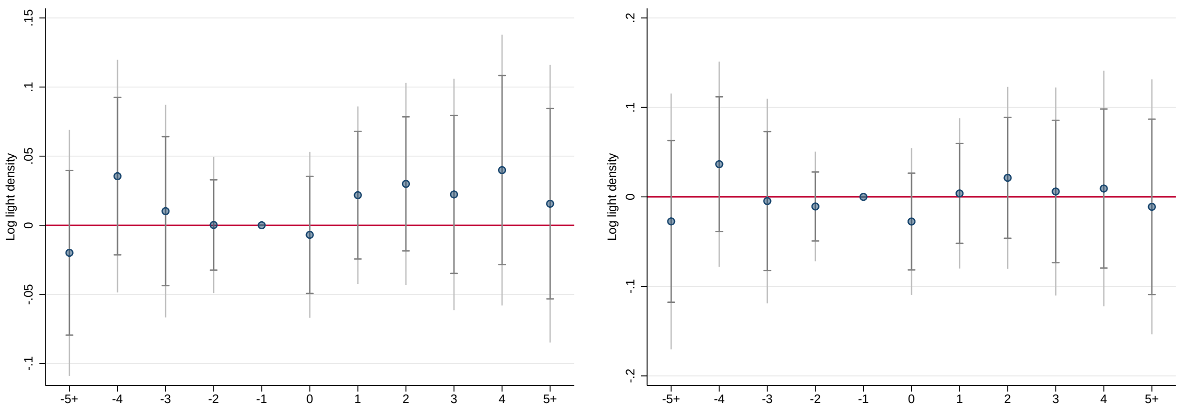
We specify the corresponding event for capitals that experience a reduction in their jurisdiction and estimate event studies comparing their performance to the set of always capitals. Figure F-4 presents the results. We find no evidence in favor of pre-treatment trends or any change in activity after 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 F-4

Mother capitals vs. always capitals

(a) All cities

(b) Cities in reformed regions



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 regions are provided by the gray error bars.

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