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Documenting Decentralization: Empirical Evidence on Administrative Unit Proliferation from Uganda

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Decentralization is an important and commonplace type of reform, yet our understanding of its effects remains limited. This paper documents the effects of the 2009–10 wave of district creation in Uganda, which increased the country's districts by 42 percent, using rich data on subdistrict units to assess the effects of district creation on a broad range of post-decentralization outcomes in a difference-in-differences framework. The effects of decentralization are concentrated in newly split off—rather than split from—districts, and are heterogeneous across outcome types. Newly split-off districts have more per capita frontline workers, but appear to have worse quality infrastructure and lower economic development. The study also presents suggestive evidence that administrative capacity decreases for newly formed districts post-split. These findings demonstrate the importance of considering a broad range of outcomes when thinking about decentralization.

JEL classification: D73, H77, O43, O12

Keywords: decentralization, administrative capacity, economic development

1. Introduction

The size and allocation of subnational units is an essential component of economic development. Intuitively, all forms of decentralization can be thought of as a trade-off between economies of scale and localization, though theories of elite capture and ethnic homogeneity suggest the importance of other dynamics as well. Yet economies of scale and other such factors may be more or less important across different types of development outcomes. Correspondingly, a given reform that decreases the size of subnational units may be beneficial or non-beneficial depending both on reform-specific underlying factors and on the types of outcomes under consideration.

The theoretical challenges to studying the effects of decentralization are two-fold. First, decentralization is often a bundle of policies, with many simultaneous changes. Second, even when the aspects of a given reform are limited, the results of these changes may be complex and multi-faceted; many prior papers study only one outcome in detail, which may explain some of the variation in results found when looking at descriptively similar reforms.

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The study focuses specifically on non-fiscal decentralization, specifically administrative unit proliferation in Uganda in 2009 and 2010. Unlike other types of decentralization reforms, administrative unit proliferation changes the level of aggregation of a locality without directly affecting resource control. In this set of reforms, every split creates one (or more) new district from an existing parent district; using a difference-in-differences framework, this study compares subdistrict units located in new and parent districts to subdistrict units in non-splitting districts over time. The analysis focuses on a large set of outcomes drawn from detailed subnational data and classified into types, and documents effect heterogeneity both by pre-reform status and outcome classification. The results suggest that while inputs into public services improve in new districts, broader measures of infrastructure quality and economic growth worsen. The results also present suggestive evidence of decreases in administrative capacity, a pathway that may help explain the effects of decentralization in this setting.

Administrative unit proliferation can be conceptualized as a non-fiscal subset of decentralization, the broader process of extending power and authority to subnational units. It is a common form of decentralization, particularly over the last three decades. From the early 1990s to the mid-2010s, almost half of Sub-Saharan African countries increased their number of administrative units by 20 percent or more, and there have been similar reforms in Brazil, Indonesia, Vietnam, and Hungary, among others (Grossman and Lewis 2014). Nonetheless, the broad literature on decentralization can offer insights into the effects of increasing the number of administrative units. A large literature has studied the effects of decentralization on public good provision and service delivery. In many contexts, including Argentina (Habibi et al. 2003; Galiani, Gertler, and Schargodsky 2008), Bolivia (Faguet 2012), Indonesia (Kis-Katos and Sjahir 2014), and Pakistan (Aslam and Yilmaz 2011), there is evidence that decentralization reforms increased investments in education and health, and even improved outcomes. In Uganda, research by Akin, Hutchinson, and Strumpf (2005) found decentralization led to decreased public-health expenditures, suggesting that results may vary with context.

An important literature focuses specifically on administrative unit creation, including discussion of both supply-side and demand-side explanations for how new decentralization reforms occur (Pierskalla 2019). Papers in this literature have looked at causes and consequences of decentralization in Senegal (Gottlieb et al. 2019), Kenya (Hassan 2016), Vietnam (Malesky 2009), and Indonesia (Kimura 2013; Pierskalla 2016), often assessing how administrative unit proliferation affects the political structure of a country.

Cross-country studies are more rare. Grossman, Pierskalla, and Dean (2017) examine the concept of decreasing marginal returns to administrative unit proliferation, using a cross-country panel model and examining the relationship between government fragmentation and the quality of health and education services. The authors find a positive and significant effect of the number of regional governments, but one which decreases as the number of regional governments increases. They attribute this leveling out of effects to inefficiencies arising from a lack of economies of scale, though they note that a lack of local administrative staff may also contribute.

A broader question which much of this literature addresses is why administrative-district proliferation might change service delivery.[1] One theory is homogeneity, whether ethnic or otherwise; if newly formed districts are more homogenous, it might be easier for governments to respond to the precise needs of citizens (Tiebout 1956; Alesina, Baqir, and Easterly 1999); collective action is also more easily achieved in homogenous units via the ability to sanction (Miguel and Gugerty 2005).[2] Yardstick competition (Maskin, Qian, and Xu 2000), in which local governmental leaders are able to show competence

1 Note that this discussion focused on larger-level administrative units, where these dynamics may play out differently than for smaller-level units. For discussions of smaller level units, see Hassan and Sheely (2017) and Ricart-Huguet and Sellers (2023).

2 At the same time, post-split variation in polarization can lead to unexpected consequences, including violence (Bazzi and Gudgeon 2021).

through their performance, may be heightened by the creation of more administrative units, leading to improved service delivery (Grossman, Pierskalla, and Dean 2017). In addition, if there are costs of administrative distance (Asher, Nagpal, and Novosad 2018), shrinking districts may improve service quality by reducing inattentiveness.[3] Recent evidence suggests that such effects may occur only under high levels of local accountability, such as elections (Singhania 2022).

There are also potential downsides to administrative unit creation when it comes to service delivery. Yardstick competition could lead to a race to the bottom on revenue or regulation (van der Kamp, Lorentzen, and Mattingly 2017). Administrative costs may increase, making service delivery more expensive (Zax 1989). Insufficient human capital and infrastructural resources may lead to decreases in service quality (Prud'homme 1995). In addition, unit proliferation may lead to other reductions in administrative capacity (Ahmad et al. 2005), whether via elite capture (Bardhan and Mookherjee 2006) or strengthening of patronage networks (Green 2010; Sadanadan 2012; Kenny 2013), particularly in the absence of monitoring institutions (Funk and Owen 2020).

Early research on the effects of decentralization showed promising results. In addition to their cross-national specification, Grossman, Pierskalla, and Dean (2017) explore the effects of Uganda's 2005–06 district creation. By using retrospective fertility information from the Demographic Household surveys (DHS), they find that child mortality decreases in newly created districts in the years after the split, with supplemental evidence suggesting that the findings may be driven by increased access to antenatal care. In Brazil, Dahis and Szerman (2021) find splits in the early 1990s resulted in increased public good provision and nighttime luminosity in subsequent decades. In the United States, Stansel (2005) and Hatfield (2015) show that local government fragmentation correlates cross-sectionally with economic growth in US metropolitan areas.

However, other studies have been more pessimistic. Cassidy and Velayudhan (2023) find that local government fragmentation reduces growth in Indonesia despite an increase in central government transfers, results they attribute to decreases in economies of scale and lack of governmental capacity. Lewis (2017) finds negative effects of administrative unit proliferation on infrastructure quality in Indonesia, attributed to the potential for corruption in the sector.

This paper focuses on 2009–10 administrative unit proliferation in Uganda, constructing service inputs, service results, infrastructure, economic development, and administrative capacity outcomes from more than 10 geocoded subnational datasets. By exploring the effects of decentralization across a defined set of outcomes types, the study documents how even within the same reform there can be heterogeneous effects arising from differences in the way the split occurs and the type of outcome considered. Further, the analysis provides suggestive evidence on the effects of reforms on administrative capacity, particularly for new districts.

Uganda remains an ideal setting for the study of administrative unit proliferation, and this set of reforms in particular has several relevant characteristics. First, Uganda has created a substantial number of new, larger-level administrative units in recent decades; these reforms alone increased the number of districts by 42 percent. Second, while all decentralization reforms are naturally bundled, these focus on level of control without changing resources. Third, parent districts inherited the name and administration of the pre-split district, whereas new districts are headquartered elsewhere and must build a new local government, providing variation within the reform.

In sum, this analysis finds limited evidence that the process of administrative unit proliferation meaningfully affects parent districts. New districts see varying effects by outcome type; there are improvements in inputs into service delivery in health and education, but a worsening of outcomes in infrastructure and economic growth, at least in the 5 to 10 years following the reform. The results further document sugges-

3 Alternatively, Nathan (2023) argues that the limited presence of the state is not necessarily the same as the limited effect of the state.

tive evidence of worsening administrative capacity, likely a mechanism of effect for these heterogeneous trade-offs. These results speak to the importance of looking broadly at multiple types of outcomes when considering the effects of decentralization, as well as taking implementation capacity seriously in thinking about the efficacy of reforms.

2. Administrative Unit Proliferation in Uganda

When the National Resistance Movement first took power in 1986, Uganda had 33 districts, which are the chief subnational unit of governmental administration; this was an increase from the 16 districts Uganda had at independence. The number of districts increased rapidly, to 55 by 2001 and 79 by 2007. In 2009, the government of Uganda created an additional 7 districts, followed by another 26 in 2010, for a total of 33 new districts over two years. In 2016, the district creation process began again; as of 2023, Uganda has a total of 135 districts. From 1986 to 2022, the number of districts roughly quadrupled; over the same time period, Uganda's population tripled, meaning the pace of district creation has outpaced population growth.

This analysis focuses on the 33 districts created in 2009 and 2010. Figure 1 divides Uganda into "non-split" districts which saw no changes, "parent" districts which lost part of their territories, and "new" districts which were created during the wave of splits. Limiting the research to this particular set of reforms allows the analysis to leverage a lengthy pre-period for most of the outcomes of interest and to follow many outcomes for nearly a decade post-split.

There are past studies that have examined the determinants of administrative unit proliferation in Uganda. Grossman and Lewis (2014) find that intra-district ethnic heterogeneity increases the likelihood of a future split from 1996 to 2011. They also find that new districts electorally reward the president of Uganda in the election following the split, suggesting that political considerations likely play a key role in determining district creation.

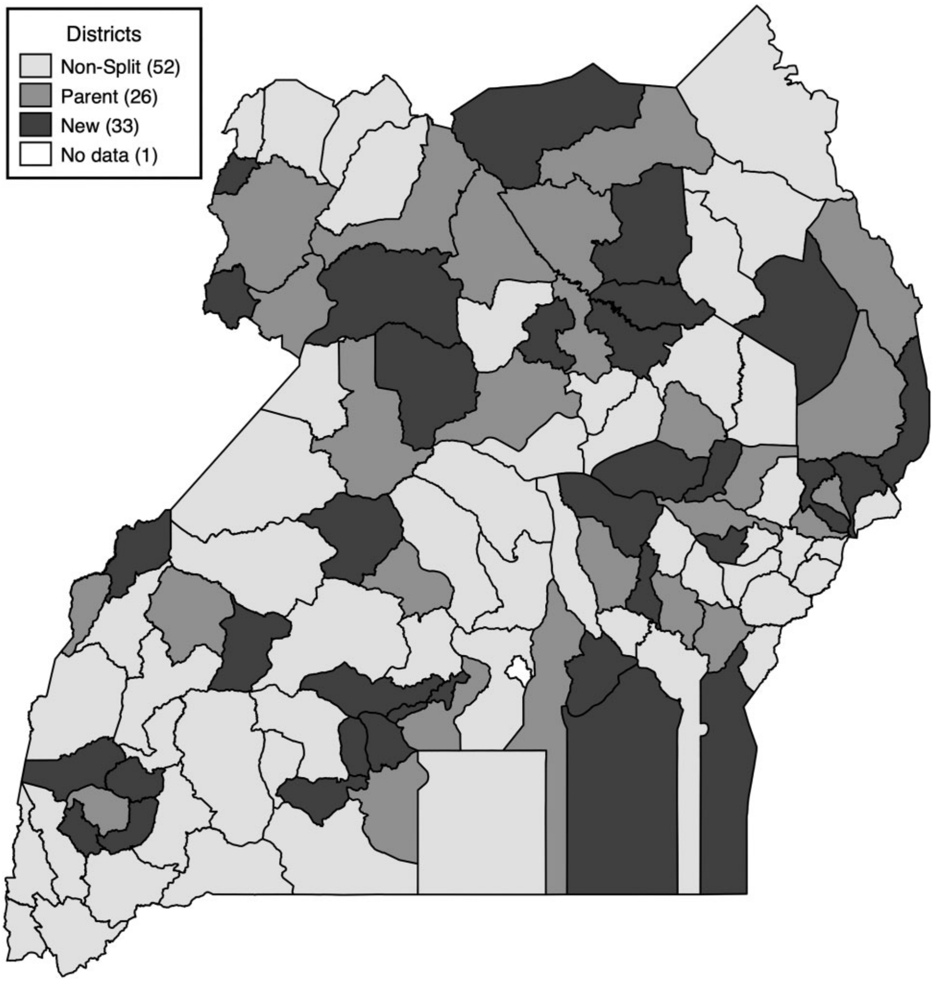
The chief stated rationale for creating new subnational units, including in Uganda, has been to improve the quality of services; Uganda's 1995 constitution allowed for the creation of new districts based on "the need to bring services closer to the people." In practice, the demand for new districts has been attributed to calls by local leaders (Rubongoya 2007; Manyak and Katono 2010), though Green (2010) notes that this explanation does not adequately capture timing. Instead, Green posits that new districts may be created as a form of patronage, as the creation of a new district results in new seats in parliament and new district-level administrative positions.[4]

Other work, including Green (2015), examines the challenges that Uganda has faced in decentralization, including a lack of local funding, and staffing challenges. Other authors, including Lewis (2014), also note the governmental capacity in new districts tends to be low, at least initially, and such concerns have been broadly echoed in the Ugandan press as well. In practice, a new district requires a minimum of 18 new administrative positions, plus recruitment of service provision staff, as well as a new physical headquarters (Green 2010).[5] In 2005, the Ugandan newspaper *New Vision* estimated a cost of $280,000 to $560,000 to enable a new district to function administratively (Ocwich 2005). These challenges did

4 Evidence on decentralization reforms across Africa suggests such dynamics are not unique to Uganda (Riedl and Dickovick 2013).

5 These administrative positions include a Chief Administrative Officer (CAO), Resident District Commissioner (RDC), deputy CAO, deputy RDC, District Auditor, Clerk, Community Based Services Manager, Education Officer, Engineer, Extension Coordinator, Finance Officer, Director of Health Services, Information Officer, Inspector of Schools, Land Officer, National Agricultural Advisory Services Officer, Personnel Officer, and Planner. While precise data on whether each position is filled in any given year has not been found by this author, contemporaneous reports express concern about low staffing levels in new districts (Masaba 2012).

Figure 1. District Status as of 2011



Source: Author's own calculations.

Note: This figure divides Ugandan districts into three categories based on whether they split during the 2009–10 wave of administrative unit proliferation in Uganda. Non-split districts saw no change to their administrative boundaries during this time. Parent districts were split off from, with new districts forming from within their boundaries. New districts split off from other districts, and were newly established following the reform. The single area with no data is Kampala, which is excluded from the analysis.

not go unnoticed during the period of district creation; some international donors opposed the creation of more new districts without a more systematic assessment of their viability (Maseruka 2008).

In short, Uganda in the late 2000s faced considerable demand for new districts; at the same time, there was a national and international sense of concern that the country may have over-stretched itself, capacity-wise (Ocwich 2005). These tensions lead to ex ante uncertainty as to the effects of new districts on various outcomes, and provide a natural setting to study the effects of the reform across outcomes and on administrative capacity directly.

3. Conceptualizing Development

In the literature, one can distinguish between hypotheses which outline ways in which administrative unit proliferation may improve, have an ambiguous effect on, or worsen economic development given local conditions. Below, the broad logic for each theory is summarized alongside context where it is relevant to generating predictions for Uganda:

- **Preferences:** Smaller districts may be more homogeneous, making it easier for policymakers to know local preferences and potentially reducing the set of competing policy demands. However, demand for any given service may increase or decrease; as such, without knowledge of local preferences, the effect of this channel on outcomes in Uganda is *ambiguous* post-split.
- **Administrative effort:** Yardstick competition, along with other similar mechanisms, means that a larger number of district units would result in improved outcomes. In contexts with local taxes, however, a race-to-the-bottom mechanism that lowers local taxes could theoretically worsen service outcomes. In Uganda, where local governments tax very little, this mechanism would overall be expected to *improve* outcomes post-split.
- **Administrative distance:** A smaller district, by definition, has lower average administrative distance and therefore potentially lower inattentiveness across the district. This mechanism would be expected to *improve* outcomes in Uganda post-split.
- **Economies of scale:** There may be economies of scale which exist in the provision of service delivery, which would lead to increases in cost when these services are provided at a lower level. This mechanism would be expected to *worsen* outcomes in Uganda post-split.
- **Administrative capacity:** There are a variety of potential mechanisms, including insufficient human capital resources, increased elite capture, or the strengthening of patronage networks, through which administrative unit proliferation may decrease the administrative capacity of local governments. This mechanism would be expected to *worsen* outcomes in Uganda post-split.

This paper addresses two gaps in applying these theories to the effects of decentralization.

First, it is essential to decompose "outcomes" in the context of economic development, as the relevance of a given theory may vary across outcomes. Economies of scale is an illustrative example; while cost reductions may be present whether hiring teachers en masse or electrifying a region, economies of scale are likely to be more relevant for infrastructure than hiring frontline workers. Rather than focusing solely on a single indicator, one can define four "bins" of outputs, each measured in multiple ways: service inputs, service results, infrastructure, and economic growth. Specific definitions for each of the four are discussed in depth in the subsequent section.

Roughly and imprecisely, this order corresponds to the relative complexity of each output set. In other words, hiring teachers is less complex than guaranteeing that they deliver high-quality educational services. Similarly, electrification is less complex than creating enabling environments in which businesses can be created. It is not as clear how service results and infrastructure compare; differentiating between the two is necessary chiefly because the strength of different mechanisms is likely to vary between them (e.g., economies of scale).

Second, this paper focuses more directly on administrative capacity. In political science, administrative capacity is one of the notions of state capacity, and specifically defined as the organizational capacity of the state (Centeno et al. 2017). Administrative capacity is defined as distinct from implementation capacity, which focuses on the ability of the state to achieve its goals. As decentralization may affect both the nature of those goals (e.g., via preferences) and the difficulty of achieving them (e.g., via economies of scale), administrative capacity is in itself a relevant domain of analysis. Past papers have positioned administrative capacity as a channel through which decentralization has other effects; while this is a

Table 1. Data Sources

Data source	Years	Level
Afrobarometer (Afrobr)*	2005 to 2018, intermittent	Village, household
Demographic and Health Surveys (DHS)*	2000 to 2018, intermittent	Household
GeoQuery Database (GeoQuery)		
DSMP-OLS nighttime lights	2000 to 2013, annual	Village
Population	2000 to 2015, imputed	Village
World Bank aid	2000 to 2014, annual	Village
Chinese aid	2000 to 2014, annual	Village
Other donor aid	2000 to 2014, annual	Village
Uganda Ministry of Education (MoES)	2006 to 2016, annual	School
Uganda Ministry of Finance (MoF)	2003 to 2015, annual	District
Uganda Ministry of Health (MoH)	2010 to 2018, intermittent	Post-split district
Uganda National Panel Survey (UNPS)*	2001 to 2018, intermittent	Household
Uganda Office of the Auditor General (OAG)	2006 to 2015, annual	District
Uganda Revenue Authority (URA)	2003 to 2018, annual	Post-split district

Source: Author's own compilation.
Note: This table describes the data used in the analysis for "Documenting Decentralization," including the name of the data source, the years for which the data were available, and the level of aggregation. Stars indicate that the data source is not representative at the district level.

useful framing, this analysis treats administrative capacity itself as an outcome of administrative unit proliferation, albeit a theoretically relevant one.

There is good reason to think that administrative capacity is salient to Uganda's 2009–10 reforms. First, the dramatic increase in districts is likely to lead to an insufficiency of human capital resources. Second, the nature of the reforms means that new districts start from a lower administrative capacity endowment than do parent districts; though other factors may also differ, contemporaneous evidence discussed earlier suggests that administrative capacity was lacking in these districts for some time post-creation. Importantly, these two points of differentiation are not unconnected; it is likely that the more complex an outcome, the more important administrative capacity may be to how that outcome is affected by administrative unit proliferation. However, administrative capacity is also hard to study, which likely contributes to its underdevelopment in the literature. As discussed later on, many measures of it are fundamentally district-level outcomes, meaning they exist at the level of the district and are difficult to measure in the pre-period. Both district-level and subdistrict-level outcomes are presented in subsequent tables with different means of imputation, and the results show consistent effects across them.

4. Data

This analysis utilizes a variety of data sources, including data from multiple Ugandan ministries, secondary data sets, and remote sensing data. The full set of data is summarized in table 1, including sources, abbreviations used in the tables, and years and frequency of observations. The data is also discussed in further detail in supplementary online appendix S2.

Most of these data exist naturally at the level of a subdistrict unit, either with universal coverage (including constituencies, schools, and villages from satellite data) or through a representative sampling strategy (including village and household data from the Uganda National Panel Survey, Demographic and Health Surveys, and Afrobarometer). In other cases, data from below the district level is used to recreate post-split districts, due either to inconsistencies in the level of data available (as in the Ministry of Health data) or due to a lack of precision in further subunit location (as in the Uganda Revenue Authority data).

For data sources where subdistrict-unit data exist, each subdistrict unit is mapped to the corresponding post-split district. For Afrobarometer data (BenYishay et al. 2017), Demographic and Health Surveys, all data from GeoQuery (Goodman et al. 2019) and the Uganda National Panel Survey, geocoordinates were already linked to post-split districts for all subdistrict units. In other words, chiefly due to having been geolinked post-2011, the data were available already associated with post-2011 boundaries for Uganda. For these data sources, the accuracy of the linkages was verified using subcounty locations or geocoordinates. For the Uganda Ministry of Education and Ministry of Health data, subdistrict units were associated with districts and subcounties in the relevant datasets. Almost all district splits aligned perfectly with pre-existing county and subcounty borders; correspondingly, with knowledge of the county and subcounty in which the subdistrict unit was located, each subdistrict unit could be mapped to the relevant post-split district in the historic data.

For budgetary data from the Uganda Ministry of Finance and audit data from the Uganda Office of the Auditor General, no units below the district level have data pre-split. As such, determining the appropriate pre-split counterfactual for parent and new districts requires imputation. This issue is discussed further in the paper and at length in supplementary online appendix S2. Due to these challenges, these findings may be viewed as speculative.

The three datasets indicated by * in the table do not have representative sampling at the district level. While this may limit the reliability of the results to some degree, the analysis follows the literature (including Grossman, Pierskalla, and Dean (2017) and Lewis (2017)) by including them in the analysis.

From these datasets, four broad classes of outcome measures are constructed:

- **Service Inputs**: This outcome class focuses on education and health inputs, based on data from the Uganda Ministry of Education and Sports (Uganda Ministry of Education and Sports 2016) and the Uganda Ministry of Health (Ministry of Health 2010, 2014, 2015, 2017, 2018) The school-level data include teacher-to-student ratios and a combined index of access to desks, chairs, and safe water. Aggregated facility-level data include the number of health workers per 1K population.
- **Service Results**: This outcome class uses utilization to (indirectly) measure service results, along with quality of care. School utilization includes primary 1 enrollment, primary 7 enrollment, and total enrollment from the Uganda Ministry of Education and Sports (Uganda Ministry of Education and Sports 2016). Healthcare utilization includes self-reports of access to healthcare, quality of healthcare, and access to healthcare for children from the Uganda Demographic and Health Surveys (Uganda Bureau of Statistics – UBOS and ORC Macro 2001; Uganda Bureau of Statistics – UBOS and Macro International 2007; Uganda Bureau of Statistics – UBOS and ICF Macro 2010; Uganda Bureau of Statistics – UBOS and ICF International 2012, 2015; Uganda Bureau of Statistics – UBOS and ICF 2018; Ministry of Health National Malaria Control Division – NMCD and Uganda Bureau of Statistics – UBOS and ICF 2020).
- **Infrastructure**: This outcome class measures village and household infrastructure, including nighttime lights (Elvidge et al. 2014), electrification from the Uganda National Panel Survey (Uganda Bureau of Statistics 2010, 2011, 2013, 2015, 2018), and amenities including electrification, sewage service, cell towers, and road quality from Afrobarometer (Afrobarometer Data 2005, 2008, 2012, 2015, 2018).
- **Economic Growth**: This outcome class contains the most diverse measures, including Uganda Revenue Authority (2018)-based measures of economic activity, and indices of well-being from Afrobarometer (Afrobarometer Data 2005, 2008, 2012, 2015, 2018), and indices of assets and welfare from the Uganda National Panel Survey (Uganda Bureau of Statistics 2009, 2010, 2011, 2013, 2015, 2018).

Additionally, there are two sets of intermediate measures:

- **Resources**: Measures of resources available to district administrations post-split are measured using data from the Ministry of Finance (Uganda Ministry of Finance, Planning and Economic Development 2020), and several AidData data releases (AidData 2017; Bluhm et al. 2018; AidData 2016). The analysis uses district-level budgetary data imputed to equalize spending by population pre-split (CIESIN 2016) to assess whether there are increases in per capita funding post-split. Additionally, it looks at whether there are changes in resources from donors using village-level data on per capita project-based foreign aid spending.

- **Administrative Capacity:** This set of measures includes a variety of measures of administrative, or implementation, capacity. The majority of these are district level, meaning pre-split values are imputed, and include the share of budgeted expenditures spent from the Uganda Ministry of Finance (Uganda Ministry of Finance, Planning and Economic Development 2020), per capita missing funds and audit status from the Uganda Office of the Auditor General (Office of the Auditor General 2007, 2008, 2009, 2010, 2011, 2012, 2013, 2014, 2015, 2016), and share of positions in the district health office that are empty from the Uganda Ministry of Health (Ministry of Health 2010, 2014, 2015, 2017, 2018). The last outcome is the share of unconfirmed, or unofficial, health workers; as this measure is available below the district level in the pre-period, no imputation is necessary.

5. Econometric Specification

The primary specification is

$$Y_{ijtr} = \beta_1 \text{ParentTreated}_{it} + \beta_2 \text{NewTreated}_{it} + \gamma_i + \nu_t + \zeta_{rt} + \varepsilon_{ijtr}$$

for a given subunit j (village, school, household, etc.) of district i in year t and subregion r.[6] For some datasets and specifications, the following specification is used:

$$Y_{itr} = \beta_1 \text{ParentTreated}_{it} + \beta_2 \text{NewTreated}_{it} + \gamma_i + \nu_t + \zeta_{rt} + \varepsilon_{itr},$$

where the only difference is that there is no subunit j smaller than the district i. In all regressions, standard errors are clustered at the level of post-split districts to account for serial correlation and heteroskedasticity (Bertrand, Duflo, and Mullainathan 2004).

The variable ν_t is a vector of year fixed effects, γ_i is a vector of district fixed effects, and ζ_{rt} is a vector of subregion and year interactions to account for regional time trends. The terms $\text{ParentTreated}_{it}$ and NewTreated_{it} are dummy variables which take the value 1 if an observation is from a parent or new district, respectively, in a time period after the split.

In both specifications, γ_i is defined the same in every year, with fixed effects corresponding to the post-split district locations even in pre-split periods. In other words, fixed effects control for unobserved location-specific characteristics for all districts that exist after the end of the splits in 2010. For subunit-level specifications, the fixed effect corresponding to a specific subunit (e.g., school, village) does not change over time, and always corresponds to post-split administrative location. For district-level specifications, imputation is used to create observations for parent and new districts in the pre-period, and each are identified with their own fixed effect.

Throughout, the coefficients of interest are β_1 and β_2, which, conditional on the assumption of parallel trends, represent the difference-in-difference estimates of the impact of district creation on parent and new districts, relative to unchanged districts in the same region. Specifically, for this regression to return a causal effect, the identifying assumption is that in the absence of administrative unit proliferation, splitting districts would have experienced changes in development outcomes at the same rates as non-splitting districts in the same region of the country.

The specification differentiates between districts which are split from (indicated by the dummy variable $\text{ParentTreated}_{it}$) and districts which are newly created by a split, or which split off (indicated by the

6 In the time frame in question, Uganda is divided into 10 subregions; each subregion contains as few as 7 or as many as 23 districts.

dummy variable NewTreated_{it}). In this setting, the difference is meaningful, as parent districts inherit the headquarters and administrative officials from prior to the split, whereas new districts must build a new administration. In the main specification, β_1 compares changes over time in units nested in parent districts to units in never-splitting districts, and β_2 compares changes over time in units nested in new districts to units in never-splitting districts.

However, a comparison between units nested in parent districts and units nested in new districts may also be of interest. A post-estimation Wald test after each specification tests the null hypothesis that $\beta_1 = \beta_2$, and reports the corresponding p-value in the line titled 'Parent vs. New p-value' in every table which reports analytical results. A rejection of the null hypothesis implies that the post-split outcomes in parent and new districts differ.

Trend visualizations, or simple averages for a given year for parent, new, and non-splitting districts (respectively) presented visually are available in the supplementary online appendix.

Throughout, Kampala is excluded from the analysis; as the largest city in the country and the largest subnational financial unit, Kampala is an extreme outlier on a variety of measures. Instead, districts which have undergone shifts are compared only to other district units.

It is worth noting that the districts that split are relatively selected, as can be seen in supplementary online appendix table S1.1. This table regresses pre-split outcomes on dummy variables for whether a district is a parent or a new district, controlling for subregion time trends. On average, the areas which become parent districts are relatively unselected; they look fairly similar to non-split districts. Areas which become new districts, however, are disadvantaged at the beginning of the sample. These differences are most prevalent in service input, though also exist in other types of outcomes. The key identifying assumption for the analysis is that these differences would have stayed fairly stable in the absence of the administrative unit proliferation reforms.[7]

In addition, each table reports both standard errors based on district-level clustering and corresponding p-values as well as sharpened q-values to account for multiple hypothesis testing by controlling for the false discovery rate (Anderson 2008, Benjamini, Krieger, and Yekutieli 2006).

The two-way fixed-effect specification implemented for differences-in-differences accounts for time-invariant differences between districts, as well as year-specific unobserved factors across all districts. However, trends in development correlated with the decision to split a district may potentially bias the results. If the government targets areas for new district creation based specifically on upward or downward trends in development outcomes—such as deliberately splitting worsening areas from improving ones—these dynamics would not be accounted for in the model. In order to assess the importance of this concern, the analysis also includes the event-study specification:

$$Y_{itr} = \alpha + \sum_{\tau=-t}^{T} \xi_\tau \text{TreatTime}_{it} + \gamma_i + \nu_t + \zeta_{rt} + \varepsilon_{ijtr},$$

where $\text{TreatTime}_{it} = 1(t_i = \tau) \times \text{Treated}_i$, with Treated_i a dummy variable indicating that i is at any point a parent district (in one specification) or a new district (in another specification). The variable t_i is the year of treatment for unit i, and τ the year in event-time. Results are discussed in the main text and reported in the supplementary online appendix.

Recent developments in difference-in-differences suggest that the two-way fixed effects (TWFE) estimator which is used here may return inaccurate results in the case of time-varying treatment effects (Goodman-Bacon 2021). The potential down-weighting of early-treated cohorts and bias introduced by

7 Note that another threat to identification may be time-varying confounders. As decentralization reforms can affect such a broad range of outcomes—even outcomes such as population—the analysis takes a conservative approach by not including any district- or subdistrict-level covariates. Population is included implicitly via per capita measures, and one set of outcomes focuses directly on budgetary allocations. Additionally, region-by-time fixed effects should account for time-varying regional favoritism, a highly common form of patronage (Hodler and Raschky 2014).

Table 2. Service Input Effects of District Creation

	Teachers per 100 students	School input index	Health workers per 1K
Parent	0.029	0.033	−0.039
	(0.028)	(0.037)	(0.058)
	[0.293]	[0.372]	[0.498]
	{0.991}	{0.991}	{0.991}
New	0.025	0.070	0.154
	(0.027)	(0.035)	(0.051)
	[0.369]	[0.049]**	[0.003]***
	{0.141}	{0.052}*	{0.011}**
Observations	122,160	132,260	555
Dataset	MoES	MoES	MoH
Level	School	School	District
Years	2006–16	2006–16	2009–18
Year FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Subregion × year FE	Yes	Yes	Yes
Non-split mean (2010)	1.05	0.01	0.95
Parent vs. New p-value	0.89	0.40	0.00

Source: Author's analysis based on data from the Uganda Ministry of Education and Sports (MoES) and the Uganda Ministry of Health (MoH).

Note: Outcomes include the ratio of teachers to 100 students; an index combining student–room ratios, the proportion of students with adequate space (defined as a desk and a chair), and whether a school has a safe water source; and the number of health workers per 1,000 individuals. All regressions include year fixed effects, district fixed effects, and an interaction between subregion and year fixed effects. The row Parent vs. New p-value tests the null hypothesis that the coefficients on Parent and New in the appropriate columns are equal. Standard errors clustered at the post-split district level are in parentheses. The p-values based on these values are reported in brackets. Sharpened q-values based on two-sided tests adjusted to control for the false discovery rate (or the proportion of Type I errors) following Benjamini, Krieger, and Yekutieli (2006) are reported in braces. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

time-varying treatment effects should ex ante be a minimal problem in this case, given that only one year separates early and late adopters. However, other concerns have also been newly raised about potential bias when there are several treatments, as is the case here (de Chaisemartin and D'Haultœuille 2020a). In order to assess the empirical extent to which there may be bias in the estimation, the study implements tests from de Chaisemartin and D'Haultœuille (2020b) and de Chaisemartin and D'Haultœuille (2020a). The results are discussed in the main text and reported in the supplementary online appendix.

6. Evidence on Development

Table 2 presents the effects of district creation on service inputs for parent and new districts, using annual, nationwide primary-school-level data from the Ministry of Education and Sports from 2006 to 2016, and intermittent, health-center-level data from the Ministry of Health from 2009 to 2018, aggregated to the post-split district level. New districts see an increase in school inputs and health workers as a result of district splits, suggesting that becoming an independent district resulted in increased provision of education and health service inputs (desks, water supply, health workers). The increase is meaningful in magnitude, particularly for health workers. The point estimate of 0.154 health workers per thousand people in the area corresponds to a 16 percent increase relative to the mean value in non-splitting districts. The results for parent districts are positive but not statistically significant in education, and negative for health, and the estimates are statistically significantly different from those for new districts on the results

Table 3. Service Results Effects of District Creation

	Enrollment			Health		Child health
	P1	P7	Total	Access	Quality	Services Index
Parent	−0.598	1.012	−8.639	0.077	0.040	0.040
	(2.461)	(0.864)	(20.737)	(0.042)	(0.040)	(0.052)
	[0.809]	[0.244]	[0.678]	[0.065]*	[0.326]	[0.440]
	{1.000}	{1.000}	{1.000}	{0.646}	{1.000}	{1.000}
New	2.429	1.270	9.931	−0.030	−0.013	0.031
	(2.086)	(0.918)	(15.706)	(0.040)	(0.041)	(0.061)
	[0.247]	[0.169]	[0.528]	[0.456]	[0.760]	[0.612]
	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}
Observations	122,330	122,330	122,330	19,129	27,077	18,609
Dataset	MoES	MoES	MoES	DHS	DHS	DHS
Level	School	School	School	Individual	Individual	Child
Years	2006–16	2006–16	2006–16	2000–18	2000–18	2000–18
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Subregion × year FE	Yes	Yes	Yes	Yes	Yes	Yes
Non-split mean (2010)	132.31	39.99	1,162.25	−0.24	0.08	0.08
Parent vs. New p-value	0.26	0.80	0.36	0.03	0.26	0.87

Source: Author's analysis based on data from the Uganda Ministry of Education and Sports (MoES) and the Demographic and Health Surveys (DHS).

Note: Outcomes include school-level enrollment in Primary 1, Primary 7, and all grades, along with DHS-based measures of access to healthcare, healthcare quality, and seeking out of healthcare services for child. All regressions include year fixed effects, district fixed effects, and an interaction between subregion and year fixed effects. The row Parent vs. New p-value tests the null hypothesis that the coefficients on Parent and New in the appropriate columns are equal. Standard errors clustered at the post-split district level are in parentheses. The p-values based on these values are reported in brackets. Sharpened q-values based on two-sided tests adjusted to control for the false discovery rate (or the proportion of Type I errors) following Benjamini, Krieger, and Yekutieli (2006) are reported in braces. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

for health workers, though not for education. Overall, the evidence is consistent with parent districts continuing to provide the same level of inputs as prior to the split while new districts differentially improve.

For all three outcomes, the trend visualization figures in supplementary online appendix fig. S1.1 suggest that on service inputs, new districts are catching up to parent and non-splitting districts over time. Pre-split, new districts tend to have persistently lower levels of staffing and inputs; post-split, they grow steadily closer to the levels in parent and non-splitting districts. These findings suggest that administrative unit proliferation led to an improvement in the quality of service inputs provided by new districts, but not parent districts. These results are consistent with a scenario in which outcome-improving mechanisms—administrative distance, for example—outweigh countervailing pressures from preferences, economies of scale, or administrative capacity, which may follow logically if such channels are not especially salient for this type of outcome.

Table 3 presents the effects of district creation on educational and health outcomes for parent and new districts. Annual school-level data on enrollment are used as an indicator of parental demand for schooling, and household survey data from DHS surveys measures both healthcare utilization and quality. The outcomes under education focus on demand for both Primary 1 (P1) and Primary 7 (P7), which are the first and last years of primary school under the Uganda education system. Primary 1 enrollment is likely to be most responsive, as parents are deciding in a given year whether to send their child to school for the first time. Primary 7 enrollment can be expected to take the longest to respond, as many children fail to re-enroll between years; however, the decision to continue attending school is likely more quality-influenced than the decision to start attending, meaning Primary 7 attendance might ultimately be more reflective of quality changes. The total column contains total enrollment for a given school. The results show consistently positive but quantitatively small estimates for new districts, and a range of small positive and negative estimates for parent districts, with none of the effects statistically significant. The evidence is not consistent with meaningful changes post-split for any measures of demand for schooling.

Under health, there are small negative coefficients for new districts under both health-service access and health-service quality. There are consistently positive coefficients for health access and health quality for services in parent districts, and one—the coefficient on health access—is weakly significant at the 10 percent level, though the results do not survive a correction for multiple inference. Child health services, which focuses on utilization, is positive for both parent and new districts, but the magnitudes are small and the estimates insignificant. On the whole, the evidence is inconsistent with meaningful changes in service utilization, suggesting that neither parent nor new districts see meaningful improvements in the quality of health and education services provided. Trend visualization figures in supplementary online appendix fig. S1.2 show that while the relative rankings of parent, new, and non-splitting districts across these measures vary, those relative rankings also seem to stay consistent over time.

In other words, these findings suggest that the improvements in service inputs identified for new districts may not necessarily translate to improved outcomes (at least as proxied by demand and utilization). It may be the case that the types of service improvements are not central to quality; parents may care more about student–teacher ratios than they do student–desk ratios, or newly hired healthcare workers may not provide the types of services households would seek out. If ensuring high-quality service provision is more complex than providing inputs into service provision, this would be consistent with increased importance of mechanisms like administrative capacity playing a larger role for this type of outcome. Alternatively, such effects may take longer than the medium-run time frame under consideration here.

Table 4 presents the effects of administrative unit creation on infrastructure outcomes. Infrastructure is measured in a variety of ways, using nighttime lights data from all Ugandan villages, direct measures of household electrification from the UNPS and DHS household surveys, a measure of sanitation infrastructure from the DHS household survey, and a measure of local village infrastructure from Afrobarometer enumeration area surveys.

The analysis shows negative but insignificant coefficients on electrification for new districts, with the nighttime lights and UNPS measures equivalent to a 6–7 percent decrease in electrification; however, the effects are not statistically significant. Effects are negative for parent districts as well, but considerably smaller in magnitude. There are negative effects for new districts on an index measuring household sanitation infrastructure, which are statistically significant, and relatively large negative and significant effects on an index measuring local access to schools, health centers, electrification, piped water, sewage, cell service, and paved roads from Afrobarometer data. The effects on parent districts are positive and insignificant, and smaller in magnitude, and differentiable from new districts, particularly for the infrastructure index.

As can be seen in the visualization of trends in supplementary online appendix fig. S1.3, these results are generally not driven by a worsening or degrading of infrastructure in new districts. Rather, infrastructure is increasing across the majority of measures, but at a relatively slower rate post-split in new districts than non-splitting and parent districts. In addition, across outcomes, the gap does not seem to be meaningfully closing over time, even for measurements taken in 2016 and 2018. If economies of scale are particularly salient for infrastructure, that may explain the discrepancy between these findings and other categories. However, economies of scale could be expected to be equally relevant to both parent and new districts; the differential results between the two are consistent with a need to leverage the broader range of mechanisms considered in this article.

Last but not least, table 5 presents a variety of data sources to gather evidence on economic growth. The first is businesses registered for tax purposes, which measures the number of relatively large businesses in post-split district areas who have registered with the central government, normalized by population. While the magnitudes are quantitatively small (the average non-splitting district has a registration rate of 0.46 percent), the relative magnitudes are substantial; district splitting resulted in a 71 percent increase in parent districts and a 50 percent decrease in new districts, both highly statistically significant. As tax levels

Table 4. Infrastructure Effects of District Creation

		Households have			
	Village night lights	Elec	Elec	Sanitation index	Infrastructure index
Parent	−0.004	−0.013	−0.001	−0.026	0.101
	(0.044)	(0.015)	(0.025)	(0.043)	(0.107)
	[0.920]	[0.391]	[0.975]	[0.544]	[0.348]
	{1.000}	{1.000}	{1.000}	{1.000}	{1.000}
New	−0.053	−0.025	−0.012	−0.094	−0.249
	(0.056)	(0.018)	(0.025)	(0.042)	(0.120)
	[0.348]	[0.159]	[0.628]	[0.027]**	[0.041]**
	{0.354}	{0.190}	{0.363}	{0.115}	{0.115}
Observations	73,402	20,987	56,461	56,504	10,811
Dataset	GeoQuery	UNPS	DHS	DHS	Afrobr
Level	Village	Household	Household	Household	Village
Years	2000–13	2001–18	2000–18	2000–18	2002–18
Year FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Subregion × year FE	Yes	Yes	Yes	Yes	Yes
Non-split mean (2010)	0.79	0.09	0.22	0.09	0.01
Parent vs. New p-value	0.23	0.34	0.53	0.13	0.01

Source: Author's analysis based on data from the Defense Meteorological Satellite Program Operational Linescan System (DMSP-OLS) accessed via GeoQuery (GeoQuery), the Uganda National Panel Survey (UNPS) data from the Uganda Bureau of Statistics, the Demographic and Health Surveys (DHS), and the Afrobarometer (Afrobr).

Note: Outcomes include nighttime lights measured via DSMP-OLS, household reports of electrification from UNPS and DHS, a sanitation index from DHS, and an index of village-level reports on schools, health centers, electrification, piped water, sewage, cell service and a paved road. All regressions include year fixed effects, district fixed effects, and an interaction between subregion and year fixed effects. The row Parent vs. New p-value tests the null hypothesis that the coefficients on Parent and New in the appropriate columns are equal. Standard errors clustered at the post-split district level are in parentheses. The p-values based on these values are reported in brackets. Sharpened q-values based on two-sided tests adjusted to control for the false discovery rate (or the proportion of Type I errors) following Benjamini, Krieger, and Yekutieli (2006) are reported in braces. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

are negligible at the district level in Uganda, these results are unlikely to be attributable to tax competition across localities.

The table also includes indices measuring various aspects of households' lives likely to result from growth, including indices of well-being, assets, and welfare. Results are null and inconsistent in direction for parent districts. However, the results are consistently negative for new districts, with reasonable magnitudes; the index of asset ownership is statistically significant, though the difference between parent and new districts is not. Although this pattern of results is not detected across all measures, it is consistent with an overall decline in economic activity and development for new districts.

Trends visualizations in supplementary online appendix fig. S1.4 show whether these declines are relative or absolute differs across outcomes. The results for tax registration share, for example, are driven by a slower rate of growth for new districts relative to parent districts and non-splitting districts (rather than an overall decline, which would be more consistent with business relocation). It is also worth noting that the gap does not seem to close, at least in the roughly eight years post-split for which this outcome is available. The household asset index does decline slightly in new districts, relative to roughly flat evolutions in parent and non-splitting districts, with no meaningful narrowing of the gap over time. Overall, the visualizations look more like a relative decline rather than an absolute one.

Similar to the results on infrastructure, positive to null results for parent districts and consistently negative results for new districts suggest that the findings are unlikely to be driven solely by economies of scale.

Table 5. Economic Growth Effects of District Creation

		Index of		
	Tax registration share	Well-being	Assets	Welfare
Parent	0.328	0.025	−0.073	−0.010
	(0.050)	(0.072)	(0.047)	(0.044)
	[0.000]***	[0.731]	[0.128]	[0.822]
	{0.001}***	{0.698}	{0.238}	{0.698}
New	−0.230	−0.129	−0.089	−0.048
	(0.045)	(0.103)	(0.039)	(0.033)
	[0.000]***	[0.213]	[0.025]**	[0.148]
	{0.001}***	{0.120}	{0.039}**	{0.110}
Observations	2,109	10,103	18,727	17,931
Dataset	URA	Afrobr	UNPS	UNPS
Level	District	Indiv	HH	HH
Years	2000–18	2005–18	2005–18	2001–15
Year FE	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Subregion × year FE	Yes	Yes	Yes	Yes
Non-split mean (2010)	0.46	1.00	0.00	−0.06
Parent vs. New p-value	0.00	0.15	0.77	0.42

Source: Author's analysis based on data from the Uganda Revenue Authority (URA), the Afrobarometer (Afrobr), and the Uganda National Panel Survey (UNPS) data from the Uganda Bureau of Statistics.

Note: Outcomes include the percentage of population self-registered for central government taxation, and indices of well-being (includes access to food, water, medicine, fuel, and cash), assets (includes possession of various household assets, including buildings, appliances, electronics, means of transportation), and welfare (includes ownership of clothing, blankets, and meals eaten per day). All regressions include year fixed effects, district fixed effects, and an interaction between subregion and year fixed effects. The row Parent vs. New p-value tests the null hypothesis that the coefficients on Parent and New in the appropriate columns are equal. Standard errors clustered at the post-split district level are in parentheses. The p-values based on these values are reported in brackets. Sharpened q-values based on two-sided tests adjusted to control for the false discovery rate (or the proportion of Type I errors) following Benjamini, Krieger, and Yekutieli (2006) are reported in braces. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

7. Evidence on Administrative Capacity

The second contribution of this paper is in looking more directly at what might be termed intermediate outcomes, or the effects of administrative unit proliferation directly on resource allocation and administrative capacity at the district level. While the extent to which these results may explain prior findings cannot be determined precisely, and a wide variety of potential explanations remain, the evidence is nonetheless consistent with no meaningful shifts in resource allocation, but substantial decreases in administrative capacity in new districts.

In theory, changes in resources for a district post-split can be conceived of as part of the administrative unit proliferation process; for example, increases in total funding or funding earmarked for administrative purposes may be an essential aspect of post-split support for a newly created administrative unit. On the other hand, if this is the case, improvements in service delivery outcomes may come about as the result of increased per capita funding, particularly if that money comes from the a higher level of government.

Table 6 directly examines whether district splits change per capita fund allocations. It is worth noting that separate budgetary data for splitting districts pre-split are not available; districts budget as a whole unit, meaning data below the level of the district do not exist. There are data on local population totals; correspondingly, this measure takes pre-split funds across the unit as a whole and makes a per capita allocation based on the areas which will become the parent and new districts post-split. This analytical strategy essentially forces parallel trends on parent and new districts. If funding were unequally allocated to parent over new areas pre-split, then the results would understate effects for new districts and overstate them for parent districts.

Table 6. Changes in Resources

	Per capita		Foreign aid		
	Total	Disc	World Bank	China	Other
Parent	1.125	0.291	−4.307	4.664	−24.617
	(0.790)	(0.138)	(13.912)	(28.838)	(51.955)
	[0.158]	[0.037]**	[0.757]	[0.872]	[0.637]
	{0.461}	{0.230}	{1.000}	{1.000}	{1.000}
New	1.005	0.527	−19.206	−31.665	−53.774
	(0.854)	(0.208)	(9.309)	(20.795)	(39.923)
	[0.242]	[0.013]**	[0.041]**	[0.131]	[0.181]
	{0.170}	{0.069}*	{0.091}*	{0.151}	{0.157}
Observations	1221	1221	78,585	78,585	78,585
Dataset	MoF	MoF	GeoQuery	GeoQuery	GeoQuery
Level	District	District	Village	Village	Village
Years	2003–15	2003–15	2000–14	2000–14	2000–14
Year FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Subregion × year FE	Yes	Yes	Yes	Yes	Yes
Non-split mean (2010)	14.76	1.73	58.25	24.60	140.54
Parent vs. New p-value	0.89	0.15	0.27	0.17	0.57

Source: Author's analysis based on data from the Uganda Ministry of Finance (MoF), and AidData's World Bank Geocoded Research Release, AidData's Global Chinese Development Finance Dataset, and AidData's Uganda AIMS Geocoded Research Release all accessed via GeoQuery (GeoQuery).

Note: Outcomes include per capita total and discretionary spending from budget data, assumed to be the same for parent and new districts pre-split, as well as per capita foreign aid in project-based spending from the World Bank, China and all other donors. All regressions include year fixed effects, district fixed effects, and an interaction between subregion and year fixed effects. The row Parent vs. New p-value tests the null hypothesis that the coefficients on Parent and New in the appropriate columns are equal. Standard errors clustered at the post-split district level are in parentheses. The p-values based on these values are reported in brackets. Sharpened q-values based on two-sided tests adjusted to control for the false discovery rate (or the proportion of Type I errors) following Benjamini, Krieger, and Yekutieli (2006) are reported in braces. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

However, these results are at least inconsistent with wildly divergent levels of resources post-split, such as might be attributable to significantly increased levels of national investment. Supplementary online appendix fig. S1.5 shows trends for budgetary data; differences in per capita allocations post-split are extremely small.

As a more localized measure, the table presents data on project-level aid at the district level. For this category of funding in particular, it is important to note that donors—including the World Bank, China, and various other international donors—often make internal determinations about where to allocate funds, and may, ex ante, prioritize some combination of need and implementation capacity.

Post-split, there is no evidence of a break in trend in per capita funding from the Ugandan government; discretionary funding may slightly increase, but total funding does not. That said, foreign-aid-project spending does decrease in new districts. These findings can be interpreted as suggestive, indirect evidence of diminished (or perceived diminished) governmental capacity, which may lead aid organizations to focus on non-splitting and parent districts in the roughly a decade following the split. Trend visualizations in supplementary online appendix fig. S1.5 confirm these patterns; they also reveal that World Bank project aid is particularly noisy. Though the results survive a multiple inference correction, different magnitudes of decreases between parent and new districts are not statistically distinguishable, suggesting the results should be interpreted with caution.

Last, table 7 presents available direct evidence on administrative capacity in post-split districts. The analysis includes several different empirical definitions of capacity, using data from the Ministry of Health, district budgets, and district audits. As discussed above with reference to budgets, some of these data exist

Table 7. Changes in Administrative Capacity

	Share of				
	Empty DHO	Unofficial HWs	Dist to budget	Per capita missing funds	Passed audit
Parent	−0.009	0.008	−0.003	640.715	−0.040
	(0.045)	(0.013)	(0.010)	(314.091)	(0.067)
	[0.833]	[0.527]	[0.756]	[0.044]**	[0.557]
	{1.000}	{1.000}	{1.000}	{0.281}	{1.000}
New	0.104	0.036	−0.019	823.189	0.013
	(0.045)	(0.013)	(0.009)	(377.255)	(0.061)
	[0.022]**	[0.007]***	[0.036]**	[0.031]**	[0.830]
	{0.037}**	{0.035}**	{0.037}**	{0.037}**	{0.199}
Observations	434	333	1221	985	1,085
Dataset	MoH	MoH	MoF	OAG	OAG
Level	District	District	District	District	District
Years	2009–18	2009–18	2003–15	2007–16	2007–16
Year FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Subregion × year FE	Yes	Yes	Yes	Yes	Yes
Non-split mean (2010)	0.40	0.05	0.43	282.79	0.45
Parent vs. New p-value	0.02	0.05	0.09	0.61	0.47

Source: Author's analysis based on data from the Uganda Ministry of Health (MoH), the Uganda Ministry of Finance (MoF), and the Uganda Office of the Auditor General (OAG).

Note: Outcomes include the share of positions in the office of the District Health Officer (DHO) left empty, assumed to be the same for parent and new districts pre-split, and the share of health workers in all facilities without official position types, where pre-split districts are recreated from facility-level data. Distance to budget outcomes are drawn from budget data, and assess the gap between budgeted and spent amount for total district funds. Pre-split, parent and new districts are assumed to have the same distance. Other outcomes are per capita unaccounted for funds and audit opinions, both assumed to be the same for parent and new districts pre-split. All regressions include year fixed effects, district fixed effects, and an interaction between subregion and year fixed effects. The row Parent vs. New p-value tests the null hypothesis that the coefficients on Parent and New in the appropriate columns are equal. Standard errors clustered at the post-split district level are in parentheses. The p-values based on these values are reported in brackets. Sharpened q-values based on two-sided tests adjusted to control for the false discovery rate (or the proportion of Type I errors) following Benjamini, Krieger, and Yekutieli (2006) are reported in braces. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

only at the pre-split district level in years prior to the split. For example, audits are conducted only at the district level, so subdistrict audits do not exist prior to the split. As discussed above, this construction assumes an equality of administrative outcomes for parent and new districts pre-split. The exception to this is the share of unofficial-health-workers data, which measure the number of health workers at various facilities district-wide who hold a position without having been administratively approved; these data are available pre-split (but not post-split) at the facility level, so post-split districts are recreated in the pre-split data and analyzed accordingly.

There are almost no effects of splits on parent district capacity, and consistent evidence of worsening administrative capacity in new districts. First, new districts have a considerably larger share of vacancies (an increase of 25 percent relative to the non-split mean) in the office of the District Health Officer (DHO). There are no effects on parent districts, and the coefficients across the two groups are statistically differentiable. This pattern is consistent with the fact that new districts must build offices from scratch; however, the post-period in this case runs through 2018, suggesting the problem persists. Health facilities in new districts also report a 3.6-percentage-point increase in health workers who lack administrative approval of their positions, a roughly 60 percent increase over the non-split mean. New districts also spend relatively less of their budgeted funds relative to non-splitting districts, though the magnitude of the decrease is only about 4 percent. While new and parent districts do not see any changes in trends around passing audits, new districts report considerable per capita increases in funds reported as unaccounted for at the time of audit, and the effects are statistically different between parent and new districts. While these findings are limited by the constraints on pre-split data, the results are nonetheless suggestive that managerial and financial capacity is diminished in new districts, even six to eight years post-split.

Trend visualizations in supplementary online appendix fig. S1.6 confirm that for empty district health office (DHO) positions and missing funds per capita, the increases are not just relative, but absolute. Consistent with reports on low staffing levels post-split (Masaba 2012), more than 50 percent of DHO positions are empty in new districts in all years post-split, with numbers 5 to 20 percentage points higher than in parent and non-splitting districts.

These findings are limited by the constraints on pre-split data, but they persist across a variety of measures and imputation strategies. Reassuringly, they are also present in the variable on share of unofficial health workers, which is constructed using subdistrict-level data and therefore not imputed. Overall, these results present direct, if suggestive, evidence of meaningful declines in administrative capacity for new districts post-split, declines which seem to persist at least for the near decade under study in this analysis.

8. Empirical Validation

Event-study models are estimated as a test for parallel pre-trends. It is worth noting that these data are somewhat unconventional from an event-study perspective; many data sources—with the exception of school input data and data on tax registration rates—are not available annually, and the gap between years is often inconsistent. Throughout, the most recent pre-split year is excluded as a point of comparison.

The first event-study model is estimated using the index of educational inputs represented in the second column of table 2 as an outcome. The results, in supplementary online appendix fig. S1.7, show no meaningful break in trends prior to the creation of new districts. Afterwards, inputs trend up in both parent and new districts, though inconsistently so in parent districts. By the end of the five-year period after the creation of a new district, new districts show clear improvements in educational inputs. To examine educational results, a second event-study model is estimated on total enrollment, represented in the third column of table 3. The results, in supplementary online appendix fig. S1.8, show no meaningful breaks in trend prior to district splits. There is some slight suggestion of an upward trend in new districts; however, as seen in the regression analysis, the results are not statistically significant. Parent districts show a clear lack of results.

For infrastructure effects, an event-study model is estimated on the index of local development indicators represented in the last column of table 4. The results, in supplementary online appendix fig. S1.9, show no clear evidence of a break in pre-trends for these outcomes, and are consistent with the suggestive results in the table, showing a clear upward trend for parent districts and a clear downward trend for new districts, though individually estimated coefficients are not statistically significant. On the whole, these results suggest that new districts saw worsening infrastructure effects in the years following a split. For economic growth effects, an event-study model is estimated on the percentage of the population registered for tax purposes; the differences-in-differences estimates are in the first column of table 5. The event-study model, visualized in supplementary online appendix fig. S1.10, shows parallel pre-trends. In parent districts, rates of registration sharply rise following districts splits; in new districts, rates of registration fall. This evidence is consistent with, as seen in the estimation results, negative effects on economic growth stemming from the creation of new administrative units, at least in a context in which government capacity is likely greatly diminished in the immediate post-period.

For resources, an event-study model is estimated on one of the annual variables located at the subdistrict level, and which suggests meaningful resource shifts in table 6: World Bank project aid measured at the village level. In the event-study model, visualized in supplementary online appendix fig. S1.11, there is no evidence of significant changes relative to non-splitting districts among parent districts. For new districts, some of the overall decline in resources seems (suggestively) to be driven by effects from the second year post-split. At a minimum, the results are not consistent with any targeting of splits based on aid resource allocation. For administrative capacity, no analogous test is conducted; for the majority of variables used, pre-periods are imputed (as the data do not exist other than at the district level), meaning any comparison returns clean pre-trends by design. For the remaining variable, which measures the share of unofficial health workers, the data have only a single pre-period, so nothing can be learned about pre-trends from the comparison.

These results show no meaningful reason to be concerned about pre-treatment breaks in trends, suggesting that the Ugandan government did not strategically time administrative unit creation in response to existing trends. On the contrary, they suggest relatively parallel pre-trends across examples from each outcome family, and also show the gaps that emerge between parent and new districts in the short run and medium run following administrative unit proliferation.

In addition, recent advances in the literature on two-way fixed-effects estimation for difference-in-differences suggest reasons to be concerned about both variation in treatment timing and the presence of multiple treatments. Specifically, under time-varying treatments, some timing groups may receive negative weights, biasing the results (Goodman-Bacon 2021). The tests proposed by de Chaisemartin and D'Haultœuille (2020b) and de Chaisemartin and D'Haultœuille (2020a) are used to assess the empirical importance of these concerns, specifically by calculating both the share of weights greater than zero and the weights attached to other treatments.[8] Tests are performed for the "parent" treatment and "new" treatment for each of the outcomes in the primary analysis and intermediate analysis tables; the results are available in supplementary online appendix table S1.2. For the vast majority of outcomes, close to 100 percent of weights are positive; no more than 10–15 percent of weights are negative, suggesting that treatment-timing heterogeneity does not meaningfully bias the results. In addition, there are no cases in which there are positive weights attached to the other treatment.

Overall, both the event-study specification and tests for biases from multiple and time-varying treatments do not suggest meaningful empirical cause for concern.

9. Conclusion

This paper explores the effects of administrative unit proliferation in Uganda, specifically looking at the effects of the creation of 33 new districts in 2009 and 2010, which increased the number of districts in Uganda by 42 percent, from 78 to 111.

More broadly, it is important to contextualize work on any individual instance of new administrative unit creation in several ways. All unit splits are not created equal; the current size of existing units and the overall level of local development may both matter considerably when thinking about a specific event. Even what is meant by the creation of a new administrative unit may vary; efforts to analyze the creation of new units are made challenging if other policies or increases in attention occur simultaneously. In the literature, the bundling of multiple changes and many possible theoretical results have continued to impede the study of this important phenomenon.

This paper uses a combination of rich and disaggregated administrative, secondary, and remote sensing data to assess the differential effects of district creation for new and parent districts relative to non-splitting districts in Uganda using a difference-in-differences framework. By utilizing many data sources and outcomes, the analysis can distinguish between different types of development outcomes. Additionally, this paper provides direct evidence on the effects of the reforms on district-level administrative capacity, which contemporary accounts suggest to have been of significant concern in the aftermath of the reforms.

The results shows that new districts (as opposed to parent) districts see improvements in frontline service inputs. Even over several years post-reform, these increases in service inputs do not translate to

8 The analysis uses the twowayfeweights STATA package for implementation.

increases in service outcomes, as measured by utilization and access. The results also show that new districts do markedly worse on a host of measures of infrastructure and economic development post-split. Even within the same reform, different types of effects co-exist when the analysis differentiates by outcome type and by the type of unit (parent vs. new). The analysis further documents novel empirical evidence on declines in administrative capacity.

On the whole, the evidence suggests that the experience of newly created administrative units may be mixed. These results also show direct evidence that district creation can worsen administrative capacity, which likely explains some share of the worsening of certain types of outcomes. These findings add an important consideration to the existing decentralization and administrative unit proliferation literatures. They suggest that context and implementation may be an even more important component of understanding such reforms than previously shown, and even may help to reconcile some of the potentially contradictory existing results on the benefits of decentralization.

Conflict of Interest Statement

The author has no conflicts of interest to declare.

Data Availability Statement

The raw Demographic and Health Survey (DHS) data are accessible free of charge on the DHS website (https://dhsprogram.com/Data/). Data cleaning do files for DHS data, other data used for analysis and analysis do files will be available with the paper.

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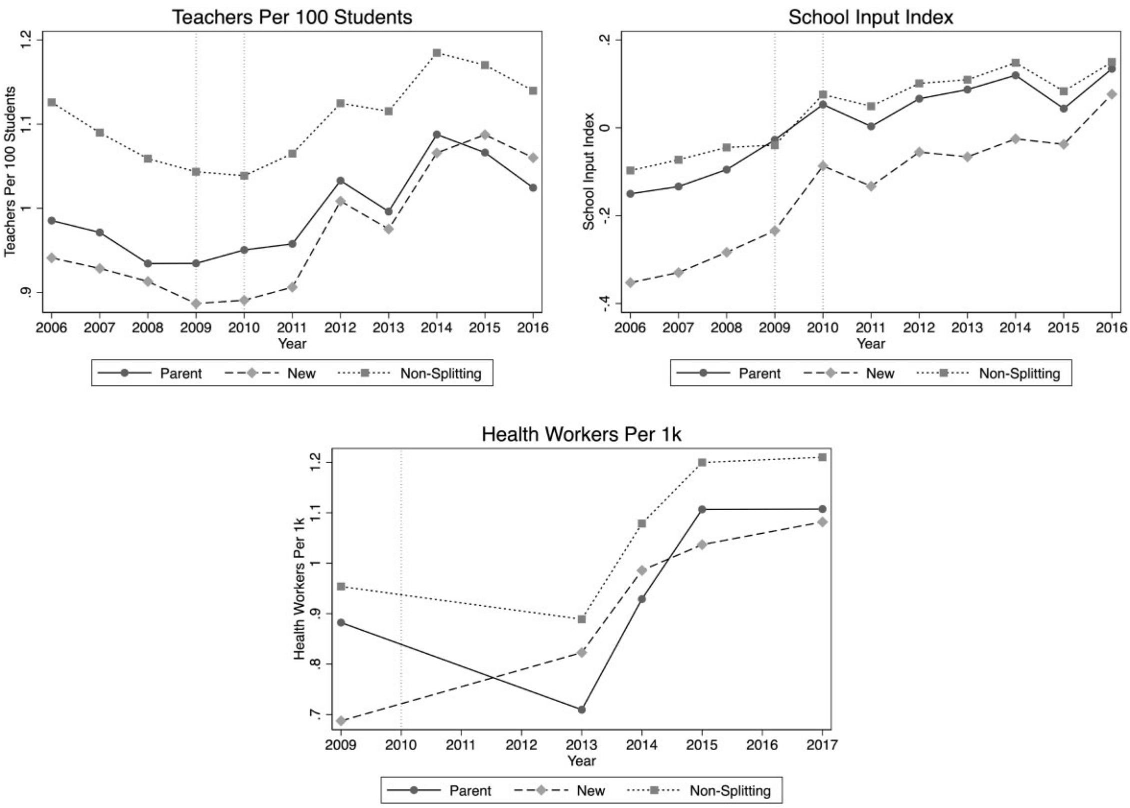
Supplementary Online Appendix

Documenting Decentralization: Empirical Evidence on Administrative Unit Proliferation from Uganda

Isabelle Cohen

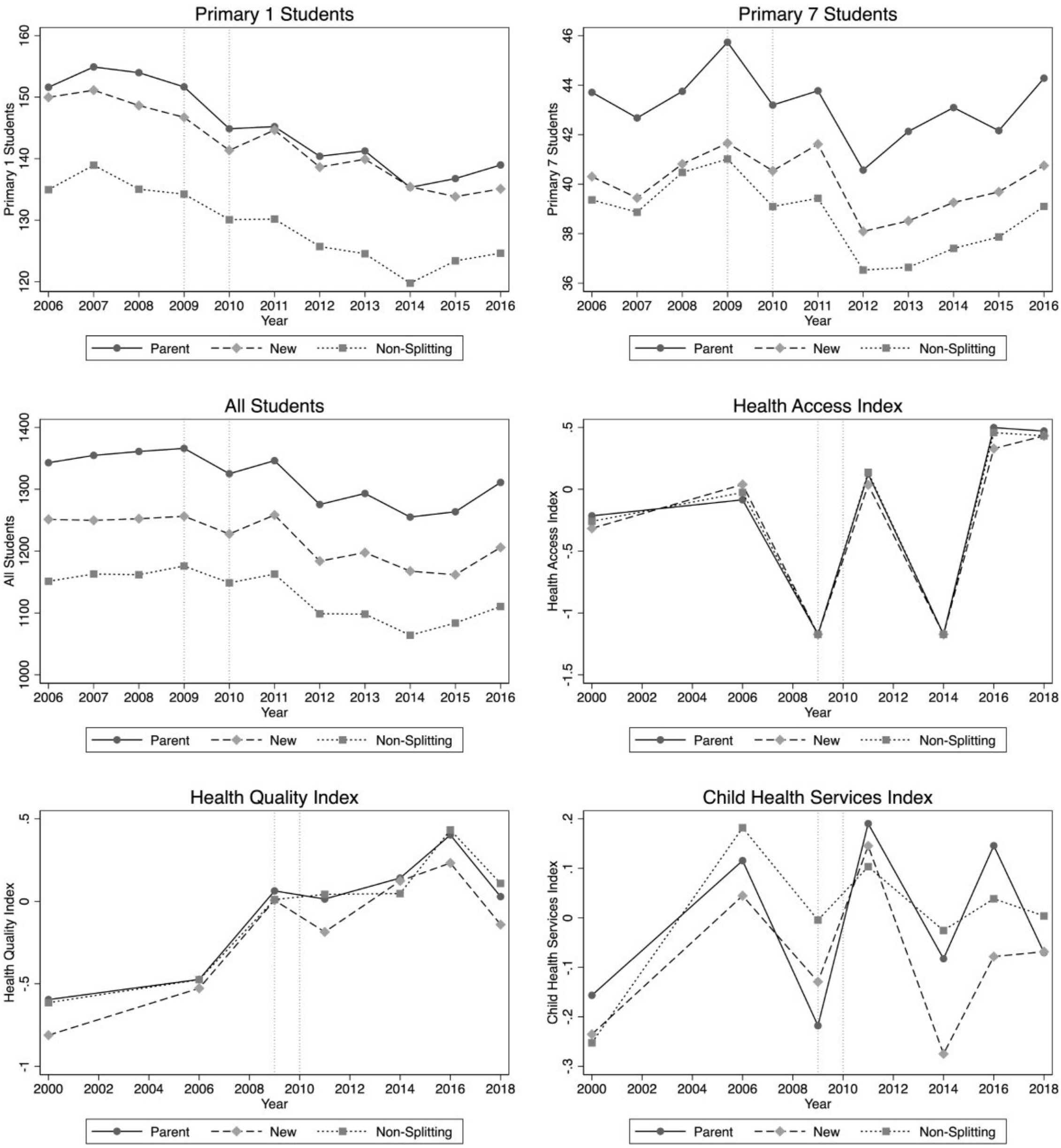
S1. Supplementary Tables and Figures

Figure S1.1. Service Input Trends Visualization



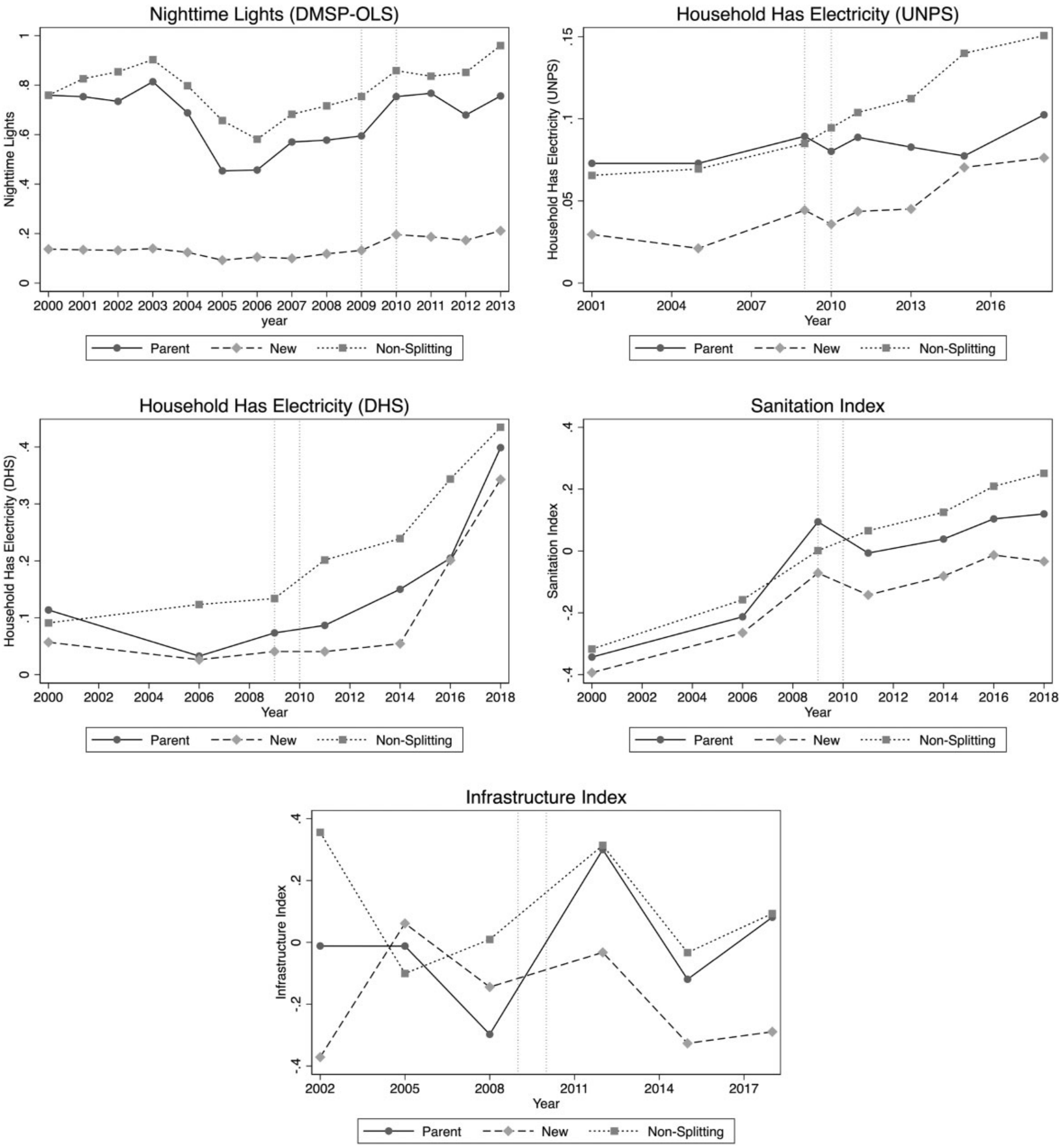
Source: Author's analysis based on data from the Uganda Ministry of Education and Sports (MoES) and the Uganda Ministry of Health (MoH).
Note: This figure visualizes the trends, taken as simple averages across districts in a given category in each year, for parent, new, and non-splitting district outcomes, including the ratio of teachers to 100 students; an index combining student–room ratios, the proportion of students with adequate space (defined as a desk and a chair), and whether a school has a safe water source; and the number of health workers per 1,000 individuals.

Figure S1.2. Service Results Trends Visualization



Source: Author's analysis based on data from the Uganda Ministry of Education and Sports (MoES) and the Demographic and Health Surveys (DHS).
Note: This figure visualizes the trends, taken as simple averages across districts in a given category in each year, for parent, new, and non-splitting districts for outcomes including school-level enrollment in Primary 1, Primary 7, and all grades, along with DHS-based measures of access to healthcare, healthcare quality, and seeking out of healthcare services for a child.

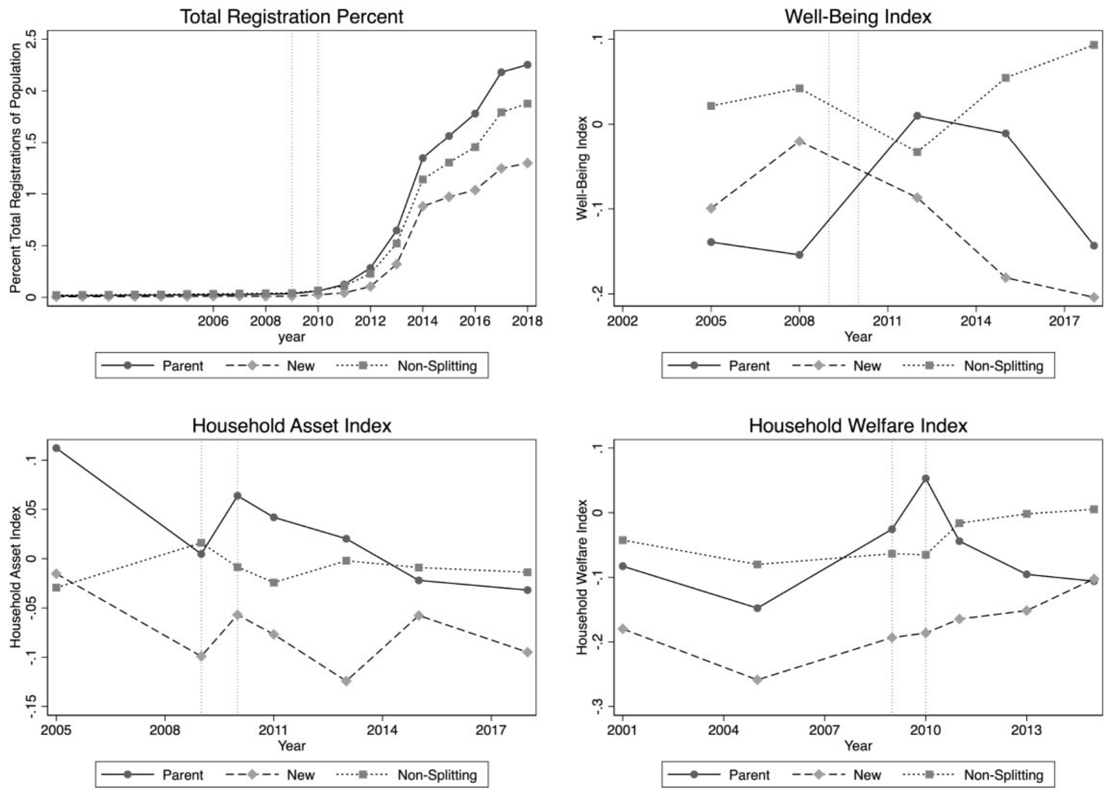
Figure S1.3. Infrastructure Trends Visualization



Source: Author's analysis based on data from the Defense Meteorological Satellite Program Operational Linescan System (DMSP-OLS) accessed via GeoQuery (GeoQuery), the Uganda National Panel Survey (UNPS) data from the Uganda Bureau of Statistics, the Demographic and Health Surveys (DHS), and the Afrobarometer (Afrobr).

Note: This figure visualizes the trends, taken as simple averages across districts in a given category in each year, for parent, new, and non-splitting districts for outcomes including nighttime lights measured via DSMP-OLS, household reports of electrification from UNPS and DHS, a sanitation index from DHS, and an index of village-level reports on schools, health centers, electrification, piped water, sewage, cell service, and a paved road.

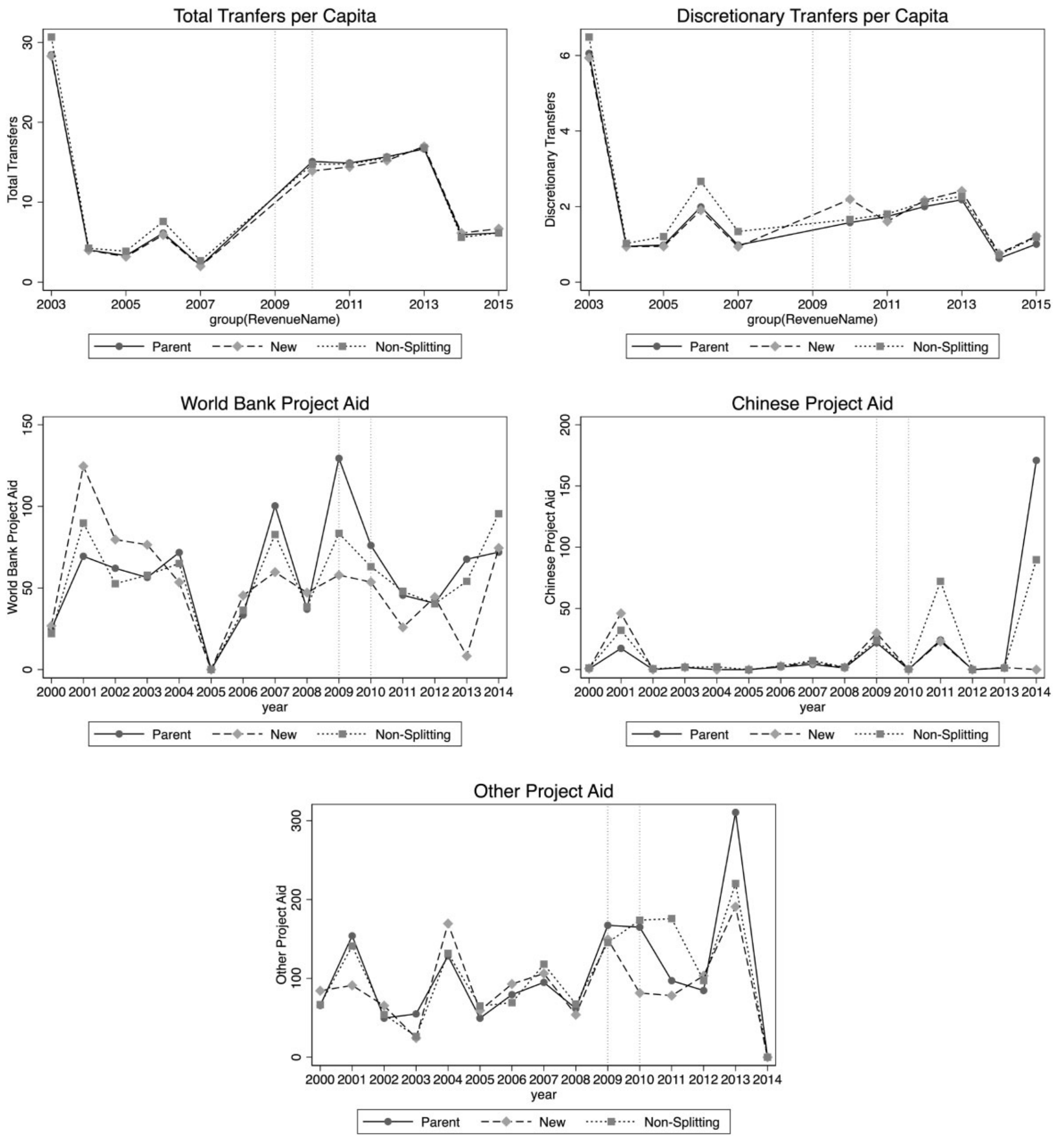
Figure S1.4. Economic Growth Trends Visualization



Source: Author's analysis based on data from the Uganda Revenue Authority (URA), the Afrobarometer (Afrobr), and the Uganda National Panel Survey (UNPS) data from the Uganda Bureau of Statistics.

Note: This figure visualizes the trends, taken as simple averages across districts in a given category in each year, for parent, new, and non-splitting districts for outcomes including the percentage of population self-registered for central government taxation, and indices of well-being (includes access to food, water, medicine, fuel, and cash), assets (includes possession of various household assets, including buildings, appliances, electronics, means of transportation), and welfare (includes ownership of clothing, blankets, and meals eaten per day).

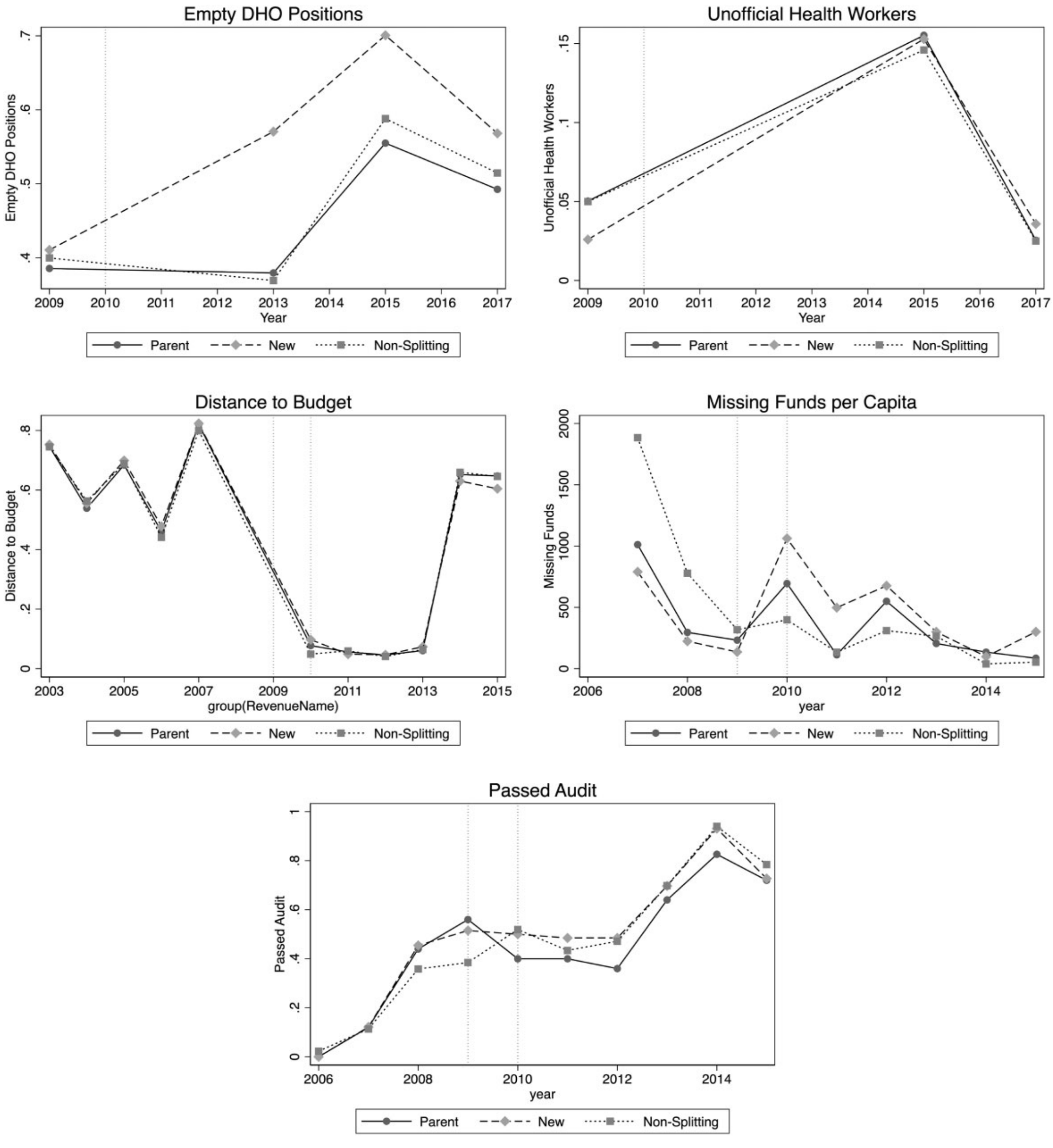
Figure S1.5. Resources Trends Visualization



Source: Author's analysis based on data from the Uganda Ministry of Finance (MoF), and AidData's World Bank Geocoded Research Release, AidData's Global Chinese Development Finance Dataset, and AidData's Uganda AIMS Geocoded Research Release all accessed via GeoQuery (GeoQuery).

Note: This figure visualizes the trends, taken as simple averages across districts in a given category in each year, for parent, new, and non-splitting districts for outcomes including per capita total and discretionary spending from budget data, assumed to be the same for parent and new districts pre-split, as well as per capita foreign aid in project-based spending from the World Bank, China and all other donors.

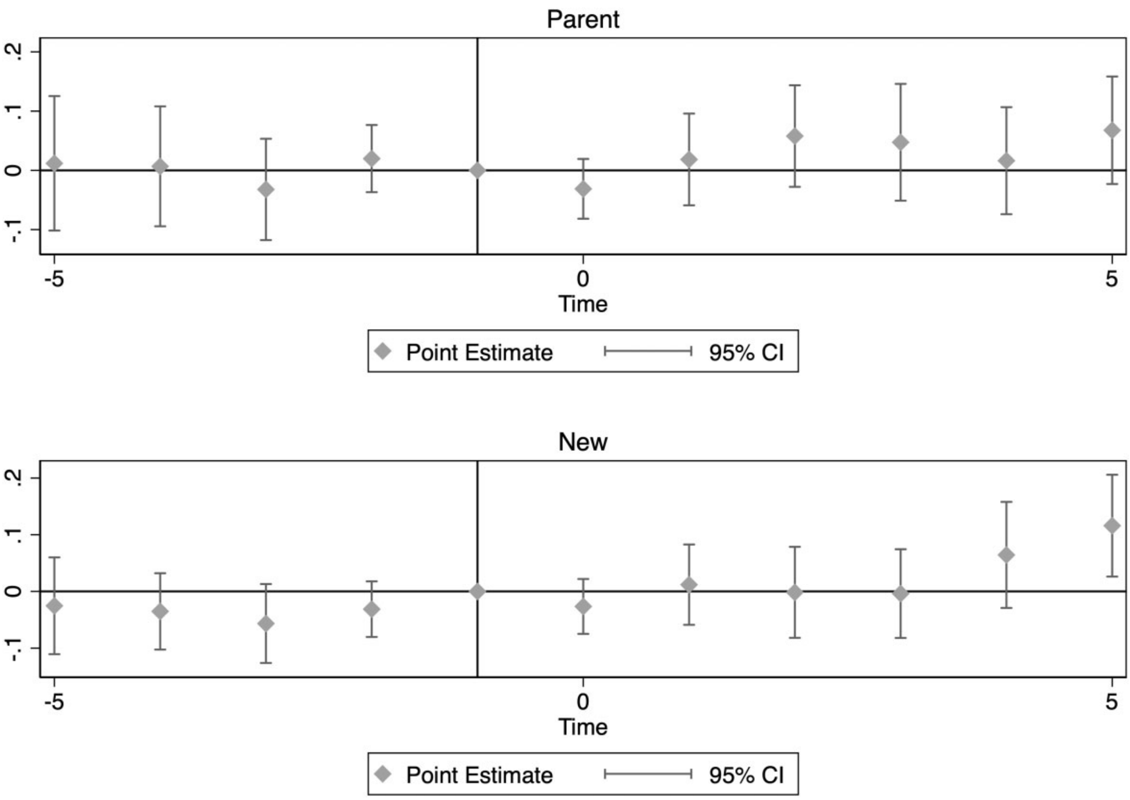
Figure S1.6. Administrative Capacity Trends Visualization



Source: Author's analysis based on data from the Uganda Ministry of Health (MoH), the Uganda Ministry of Finance (MoF), and the Uganda Office of the Auditor General (OAG).

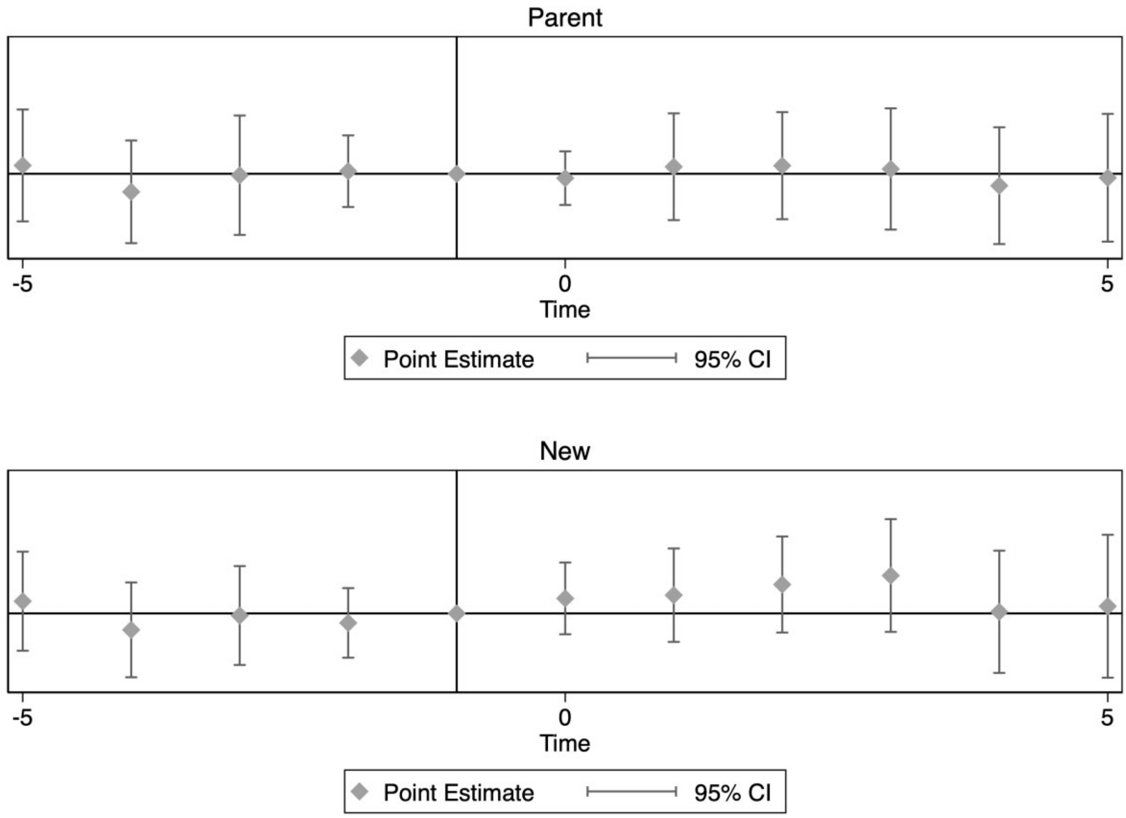
Note: This figure visualizes the trends, taken as simple averages across districts in a given category in each year, for parent, new, and non-splitting districts for outcomes including the share of positions in the office of the District Health Officer (DHO) office left empty, assumed to be the same for parent and new districts pre-split, and the share of health workers in all facilities without official position types, where pre-split districts are recreated from facility-level data. Distance to budget outcomes are drawn from budget data, and assess the gap between budgeted and spent amount for total district funds. Pre-split, parent, and new districts are assumed to have the same distance. Other outcomes are per capita unaccounted for funds and audit opinions, both assumed to be the same for parent and new districts pre-split.

Figure S1.7. Event-Study Estimation for School Inputs



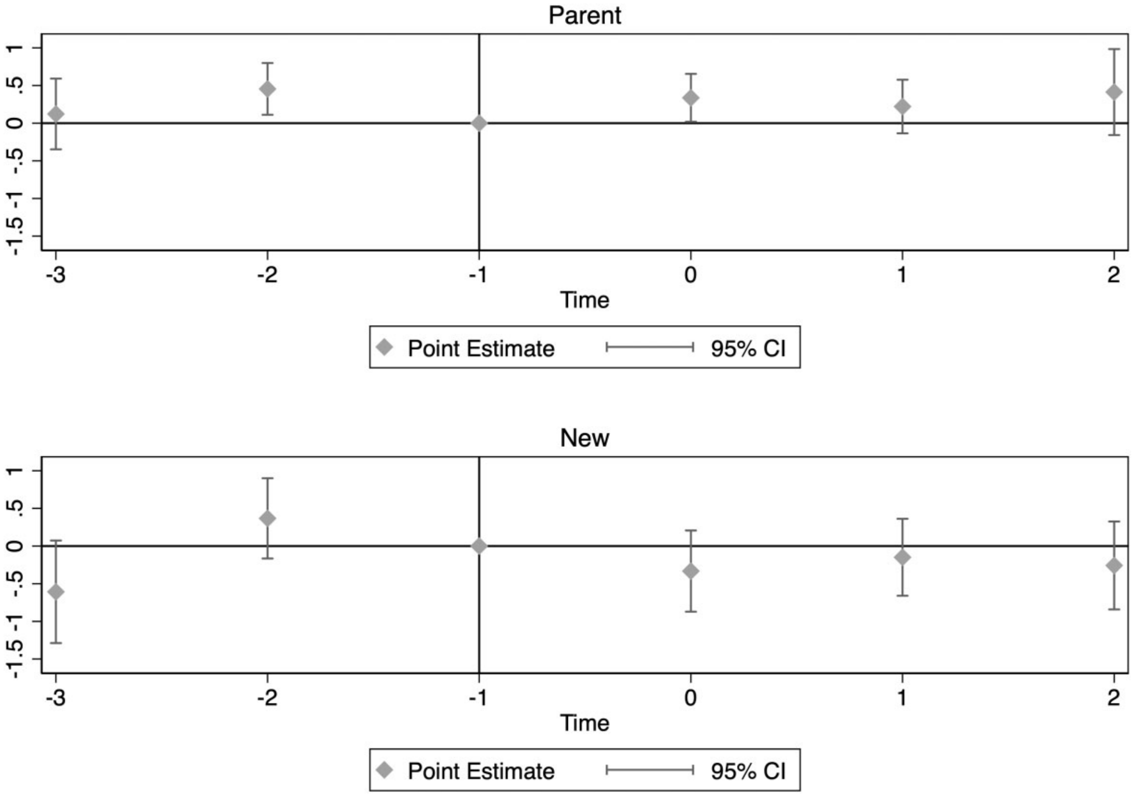
Source: Author's analysis based on data from the Uganda Ministry of Education and Sports.
Note: This figure visualizes the results of an event-study estimation on an index of school-level inputs. Estimation is done separately for parent and new districts, with each compared to non-splitting districts. Standard errors are clustered at the post-split district level; 95 percent CI stands for 95 percent confidence interval.

Figure S1.8. Event-Study Estimation for School Enrollment



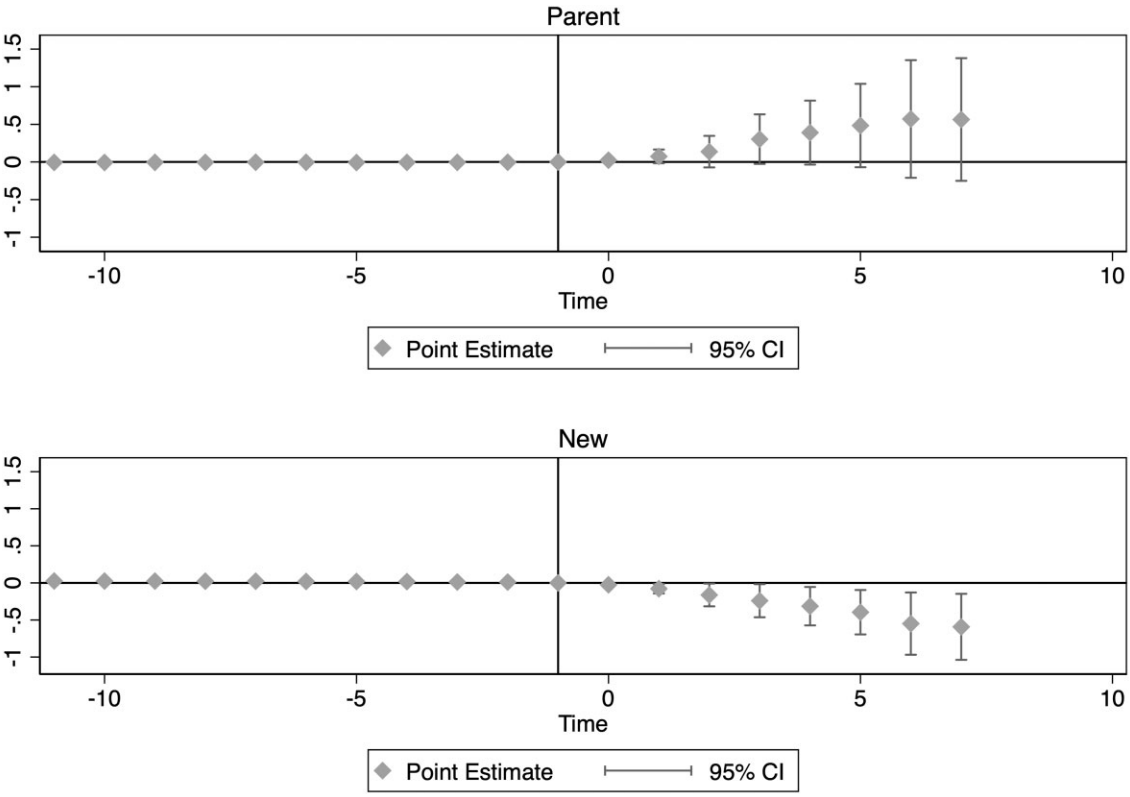
Source: Author's analysis based on data from the Uganda Ministry of Education and Sports.
Note: This figure visualizes the results of an event-study estimation on the number of total enrolled students in primary schools. Estimation is done separately for parent and new districts, with each compared to non-splitting districts. Standard errors are clustered at the post-split district level; 95 percent CI stands for 95 percent confidence interval.

Figure S1.9. Event-Study Estimation for Infrastructure Index



Source: Author's analysis based on data from the Afrobarometer.
Note: This figure visualizes the results of an event-study estimation on an index of village infrastructure. Estimation is done separately for parent and new districts, with each compared to non-splitting districts. Standard errors are clustered at the post-split district level; 95 percent CI stands for 95 percent confidence interval.

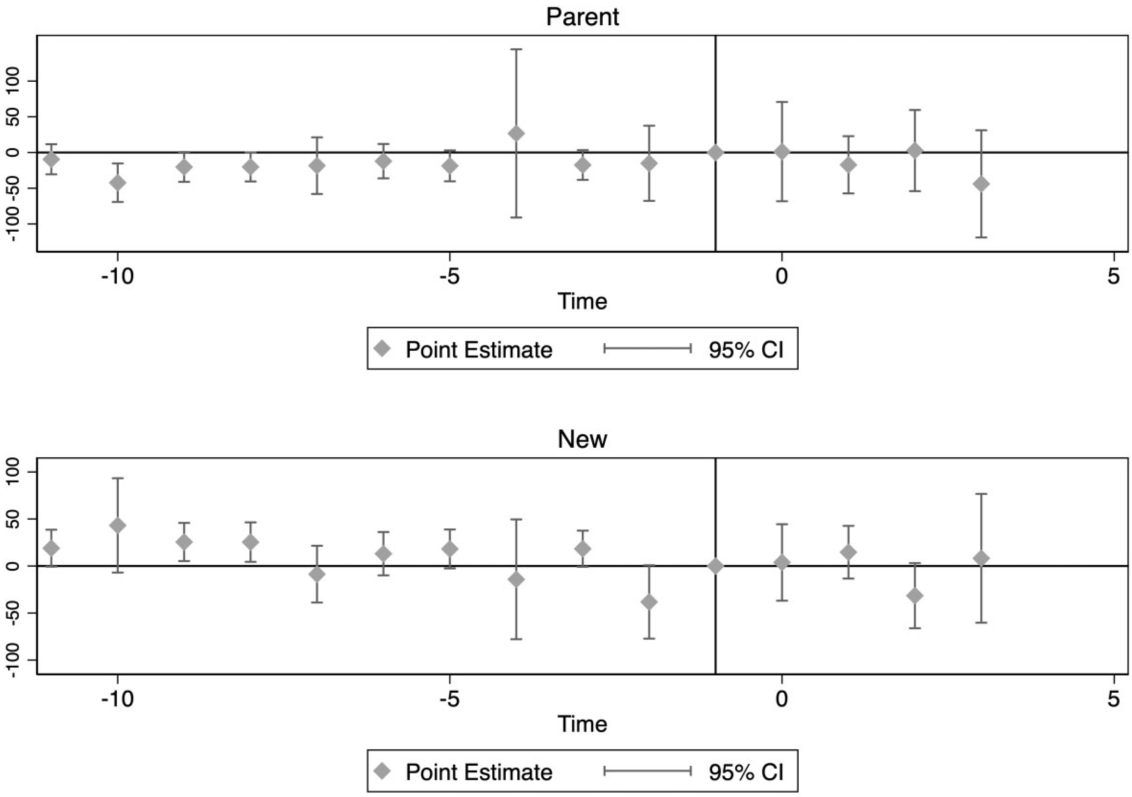
Figure S1.10. Event-Study Estimation for Tax Registration



Source: Author's analysis based on data from the Uganda Revenue Authority.

Note: This figure visualizes the results of an event-study estimation on the share of tax registrants in a district. Estimation is done separately for parent and new districts, with each compared to non-splitting districts. Standard errors are clustered at the post-split district level; 95 percent CI stands for 95 percent confidence interval.

Figure S1.11. Event-Study Estimation for World Bank Project Aid



Source: Author's analysis based on data from AidData's World Bank Geocoded Research Release.

Note: This figure visualizes the results of an event-study estimation on village-level project aid from the World Bank. Estimation is done separately for parent and new districts, with each compared to non-splitting districts. Standard errors are clustered at the post-split district level; 95 percent CI stands for 95 percent confidence interval.

Table S1.1. Pre-split Comparison

Variable	Dataset	Before	Non-split	Δ parent	Δ new
Service input					
Teachers per 100 students	MoES	2009	1.08	− 0.00577	− 0.103***
School input index	MoES	2009	− 0.0628	− 0.0347	− 0.231***
Health staff per 1K	MoH	2008	0.954	− 0.0855	− 0.282**
Service results					
Primary 1 students	MoES	2009	136	− 2.89	7.54
Primary 7 students	MoES	2009	39.9	2.07	− 0.336
All students	MoES	2009	1,163	11.4	18.2
Healthcare access	DHS	2006	− 0.191	− 0.00395	− 0.00762
Healthcare quality	DHS	2006	− 0.384	0.0118	− 0.0735
Child health services index	DHS	2009	− 0.0201	0.0174	− 0.066*
Infrastructure					
Nighttime lights	GeoQuery	2009	0.753	− 0.118	− 0.683**
HH has electricity	DHS	2009	0.153	0.0483	− 0.0407
HH has electricity	UNPS	2009	0.0736	0.0184	− 0.0384*
Sanitation index	DHS	2009	− 0.12	0.0631	− 0.0534
Infrastructure index	Afrobr	2008	0.0141	− 0.0862	− 0.11
Economic growth					
Tax registration share	URA	2009	0.0307	− 0.0032	− 0.0206**
HH Well-being index	Afrobr	2008	0.0321	0.0216	− 0.00523
HH Asset index	UNPS	2009	− 0.00578	0.111**	− 0.0167
HH Welfare index	UNPS	2009	− 0.0621	0.152***	− 0.0383
Resources					
Total budget per cap	MoF	2007	9.81	− 1.07*	− 1.28**
Discretionary budget per cap	MoF	2007	2.54	− 0.412**	− 0.475**
World Bank aid per cap	GeoQuery	2009	52.9	− 1.24	− 3.96
Chinese official aid per cap	GeoQuery	2009	7.47	− 3.52	0.55
Other foreign aid per cap	GeoQuery	2009	88.4	− 2.93	− 5.71
Admin capacity					
DHO share vacant	MoH	2008	0.4	0.0032	0.0206
Health staff positionless	MoH	2008	0.05	− 0.00336	− 0.026***
Distance to total budget	MoF	2007	0.647	0.0163**	0.0205***
Missing funds per capita	OAG	2009	0.274	− 0.16*	− 0.158**
Passed auditOAG	Audit	2009	0.228	0.0572	0.0286

Source: Author's calculations based on data from the Uganda Ministry of Education and Sports (MoES), the Uganda Ministry of Health (MoH), the Demographic and Health Surveys (DHS), the Defense Meteorological Satellite Program Operational Linescan System (DMSP-OLS) accessed via GeoQuery (GeoQuery), the Uganda National Panel Survey (UNPS) data from the Uganda Bureau of Statistics, the Afrobarometer (Afrobr), the Uganda Revenue Authority (URA), the Uganda Ministry of Finance (MoF), AidData's World Bank Geocoded Research Release, AidData's Global Chinese Development Finance Dataset, AidData's Uganda AIMS Geocoded Research Release accessed via GeoQuery (GeoQuery), and the Uganda Office of the Auditor General (OAG).

Note: The value under non-split is the mean value for a given outcome in the year indicated by that row for non-splitting districts. The parent (new) columns indicate the coefficient on an indicator for being a future parent (new) district in a regression of said outcome on indicators for future parent and new districts, controlling for region. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S1.2. Two Way Fixed Effects Effect Heterogeneity Tests

Variable	Dataset	Parent		New	
		%Weights >0	Other weights	%Weights >0	Other weights
Service input					
Teachers per 100 students	MoES	1.00	0.00	1.00	0.00
School input index	MoES	1.00	0.00	1.00	0.00
Health staff per 1K	MoH	1.00	0.00	1.00	0.00
Service results					
Primary 1 students	MoES	1.00	0.00	1.00	0.00
Primary 7 students	MoES	1.00	0.00	1.00	0.00
All students	MoES	1.00	0.00	1.00	0.00
Healthcare access	DHS	1.00	0.00	1.00	0.00
Healthcare quality	DHS	1.00	0.00	1.00	0.00
Child health services index	DHS	1.00	0.00	1.00	0.00
Infrastructure					
Nighttime lights	GeoQuery	1.00	0.00	1.00	0.00
HH has electricity	UNPS	0.98	0.00	0.97	0.00
HH has electricity	DHS	1.00	0.00	1.00	0.00
Sanitation index	DHS	1.00	0.00	1.00	0.00
Infrastructure index	Afrobr	0.95	0.00	0.85	0.00
Economic growth					
Tax registration share	URA	1.00	0.00	1.00	0.00
HH well-being index	Afrobr	0.95	0.00	0.83	0.00
HH asset index	UNPS	0.98	0.00	0.97	0.00
HH welfare index	UNPS	0.99	0.00	0.98	0.00
Resources					
Total budget per cap	MoF	1.00	0.00	1.00	0.00
Discretionary budget per cap	MoF	1.00	0.00	1.00	0.00
World Bank aid per cap	GeoQuery	1.00	0.00	1.00	0.00
Chinese official aid per cap	GeoQuery	1.00	0.00	1.00	0.00
Other foreign aid per cap	GeoQuery	1.00	0.00	1.00	0.00
Admin capacity					
DHO share vacant	MoH	1.00	0.00	1.00	0.00
Health staff positionless	MoH	1.00	0.00	1.00	0.00
Distance to total budget	MoF	1.00	0.00	1.00	0.00
Missing funds per capita	OAG	1.00	0.00	1.00	0.00
Passed audit	OAG	1.00	0.00	1.00	0.00

Source: Author's calculations based on data from the Uganda Ministry of Education and Sports (MoES), the Uganda Ministry of Health (MoH), the Demographic and Health Surveys (DHS), the Defense Meteorological Satellite Program Operational Linescan System (DMSP-OLS) accessed via GeoQuery (GeoQuery), the Uganda National Panel Survey (UNPS) data from the Uganda Bureau of Statistics, the Afrobarometer (Afrobr), the Uganda Revenue Authority (URA), the Uganda Ministry of Finance (MoF), AidData's World Bank Geocoded Research Release, AidData's Global Chinese Development Finance Dataset, AidData's Uganda AIMS Geocoded Research Release accessed via GeoQuery (GeoQuery), and the Uganda Office of the Auditor General (OAG).

Note: This table implements tests from de Chaisemartin and D'Haultœuille (2020b) and de Chaisemartin and D'Haultœuille (2020a). Columns 3 and 5 calculate the share of negative weights on the coefficient Parent and New in the main regression specification (respectively) for a given outcome. Columns 4 and 6 estimate the net weights placed on the other treatment (New for Parent, Parent for New, respectively) for a given outcome.

S2. Data Appendix

In this appendix, I discuss in detail each of the sources of data used in this paper, including how they were matched to post-split district boundaries, and describe all outcomes.

S2.1. Afrobarometer Data

S2.1.1. Description and Coverage

The Afrobarometer Survey is a pan-African series of national public attitude surveys on democracy, governance, and society. Although not a panel survey, it was conducted on a nationally representative sample in Uganda in 2005, 2008, 2012, 2015, and 2018 (Afrobarometer Data 2005, 2008, 2012, 2015, 2018). In theory, the data include roughly 10,000 households, relative to a goal of roughly 2,400 per survey from 2005 through 2015, and 1,200 in 2018; in practice, the number is slightly lower in most years, and only roughly 8,975 respondents answered the survey. These individuals are distributed across approximately 1,150 enumeration areas, most of which do not repeat across years.

The Afrobarometer is nationally representative; there is some variation across waves in terms of subnational representation. In every year, results are representative at least across subregions, but not at the district level.

S2.1.2. Geocoding

I use the geocoding conducted by BenYishay et al. (2017) to map households in each year to districts, and accordingly to treatment status; since the geocoding was done post-2011, all households and survey areas are naturally linked to their post-split districts.

S2.1.3. Variables

Using the Afrobarometer data, I construct measures of well-being and local development, which I construct into indices, described in detail below.

The variable HH Wellbeing Index$_{it}$ is an index of general well-being constructed from the Afrobarometer data. It contains measures of whether the household never, in the last year, lacked access to food, clean water, medicine, fuel, and cash income. Each question was asked in every survey round from 2005 to 2018. I normalize each relative to its own year, and sum the measures of well-being to construct a general well-being index.

The variable Infrastructure Index$_{it}$ is an index measuring the infrastructure of the enumeration area where the survey is conducted. This measure is based on enumerator observations of the area in question, and specifically records whether the enumeration area is observed to have an electricity grid that most houses could access, piped water that most houses could access, a sewage system that most houses could access, and cell phone service. The enumerator also records whether each of the following facilities is either in the area or within walking distance: post office, school, police station, health clinic, and market stalls. Last, the enumerator records whether the road at the entry to the enumeration area was paved, tarred, or concrete. Each of these questions is asked in every survey round from 2008 to 2018. I normalize each relative to its own year at the enumeration-area level, and sum the measures of local development to construct a development index.

S2.2. Office of the Auditor General Data

S2.2.1. Description and Coverage

Audit data come from annual reports by the Uganda Office of the Auditor General, conducted based on financial statements in a given year. One report, annually, focuses on local authorities, which includes district governments. Online, I was able to locate annual reports starting from the financial year of July 2006 to June 2007, through the financial year of July 2015 to June 2016. From 2006–07 to 2013–14, these reports are presented in an aggregated single volume (Office of the Auditor General 2007, 2008, 2009, 2010, 2011, 2012, 2013, 2014). In 2014–15 and 2015–16, the reports are presented in disaggregated form, with one form presented for each district, which I cite cohesively (Office of the Auditor General 2015, 2016).

S2.2.2. Geocoding

Each year contains data for districts which existed at the time. Among the data is an audit opinion; the opinion can be unqualified, qualified, adverse, or specify. In addition, for most districts in most years, the audit report further indicates the amount of "unaccounted for funds," or funds not accounted for by documentation possessed by the auditor. I use both the auditor opinion and the amount of unaccounted for funds (normalized to a per capita basis) as outcome variables. One obvious challenge with this dataset is that districts which do not yet exist, administratively, cannot be separately audited. Although there are district-level audit reports for parent districts starting in 2006–07, the first audit for any new district appears post-split.

To deal with this challenge, I take two approaches. For audit opinions, I assume that new districts "inherit" the audit opinion of their parent, meaning the data contain the parent's audit opinion in years prior to the creation of the district. For missing funds, I impute per capita amounts for the earlier years by combining the populations of the two districts, and create a per capita measure, essentially assuming missing funds were spread equally across the district.[9] In both cases, I use a district's own data once it has been created.

S2.2.3. Variables

Using the audit data, I construct two final measures of district financial performance.

The variable Missing Funds Per Capita$_{it}$ takes the total amount of missing funds reported in a given year for a given district and compares it against the population of the district in question. As noted above, pre-split areas are imputed on the basis of population. In other words, for a district pre-split, the population of the (future) parent and new districts are combined, and total missing funds are divided by that population value. This assumes an even distribution of missing funds across parent and new districts pre-split. Post split, missing funds in each parent and new district are compared to the actual population of the relevant parent or new district.

The variable Passed Audit$_{it}$ is a dummy variable which takes the value 1 if a district had a passing or unqualified audit opinion, meaning there were no material misstatements or errors in the financial statements. As noted, both parent and new districts "inherit" the audit opinion from the pre-split district, an inherent imputation which assumes even financial management quality in the relevant districts pre-split. Post-split, parent and new districts have their own audit opinions.

S2.3. Ministry of Finance Data

S2.3.1. Description and Coverage

I utilize annual budgetary data from the Ministry of Finance, Planning and Development, available from 2003 to 2015 (Uganda Ministry of Finance, Planning and Economic Development 2020). These data contain information on all sources of funding for district governments, broken down by funding type as well as by spending categories. I combine this with population data (CIESIN 2016) to get measures of annual per capita spending in total, by type, and by category for each district. One caveat is that data from this source are missing for 2008 and 2009, but available for all other years in the sample.

The average district receives roughly 90 percent of its funding in this period from the central government, with the other 10 percent coming from local revenues or external sources. Conditional funding accounts for 70 percent to 80 percent of governmental funding, and is allocated fairly specifically to in-

9 Note that population is actually measured at the village level for each year; therefore, it is possible to know the population of the area where a district will be prior to the formal creation of that district.

dividual line items, such as teacher salaries, library maintenance, secondary-school construction, teacher pensions, road rehabilitation, and infrastructure support. The remainder of central government funding comes in discretionary funding, which accounts for 10 percent to 20 percent of annual budgets, and which is much more loosely specified: for example, wage vs. non-wage spending, or district vs. urban spending.

S2.3.2. Geocoding

An obvious challenge with using budgetary data is that prior to the existence of a district, there is no district-level budgetary data. Take for example the district of Buyende, which was created from the district of Kamuli in 2009. Although there are district-level budgets for Kamuli starting in 2003, the first district budget for Buyende appears in 2010. To deal with this challenge, I impute per capita amounts for the earlier years by combining the populations of the two districts, and assuming the budget of the parent district is spread equally across the whole population.[10] Once a district is created, I then separate the population and budgetary allocations.

It is possible that this population-based imputation either over-allocates or under-allocates funding to new districts; this method would be an over-allocation if pre-split districts tend to fund the areas which will be kept in the district more heavily, and an under-allocation if pre-split districts tend to allocate more funds to the areas which will become new districts. However, there does not exist any centralized repository of budgets for levels lower than the district; as such, I believe this method provides a reasonable proxy for estimating per capita spending.

S2.3.3. Variables

Using the budgetary data, I construct three final measures using budgetary data.

The variable Total Budget Per Capita$_{it}$ is a measure of the total amount spent in a district in a given year, normalized by the population of the district. All values are deflated to USD 2011. For pre-split years, the amount is spread equally across the population of the pre-split district, constructed by combining the population of the (future) parent and new districts.

The variable Discretionary Budget Per Capita$_{it}$ is a measure of the total discretionary funds spent in a district in a given year, normalized by the population of the district. All values are deflated to USD 2011. Discretionary funds, as discussed above, consist of transfers not earmarked for specific purposes by the central government, but rather intended to be spent as needed or on priority projects by the district in question. For pre-split years, the amount is spread equally across the population of the pre-split district, constructed by combining the population of the (future) parent and new districts.

The variable Distance to Total Budget$_{it}$ measures the gap between budgeted spending and actual spending for a given district, normalized by the population of the district. All values are deflated to USD 2011. For pre-split years, the amount is spread equally across the population of the pre-split district, constructed by combining the population of the (future) parent and new districts.

S2.4. Demographic and Health Surveys

S2.4.1. Description and Coverage

I use both standard demographic and health surveys (DHS) and malaria indicator surveys (MIS) for Uganda, particularly the waves collected in 2000–01 (Uganda Bureau of Statistics – UBOS and ORC Macro 2001), 2006 (Uganda Bureau of Statistics – UBOS and Macro International 2007), 2009 (Uganda Bureau of Statistics – UBOS and ICF Macro 2010), 2011 (Uganda Bureau of Statistics – UBOS and ICF International 2012), 2014–15 (Uganda Bureau of Statistics – UBOS and ICF International 2015), 2016 (Uganda Bureau of Statistics – UBOS and ICF 2018), and 2018–19 (Ministry of Health National Malaria Control Division – NMCD and Uganda Bureau of Statistics – UBOS and ICF 2020).

10 Note that population is actually measured at the village level for each year; therefore, it is possible to know the population of the area where a district will be prior to the formal creation of that district.

The demographic and health surveys are not representative at the district level in Uganda, as elsewhere, though they are representative in 15 subregions of Uganda, defined largely on kingdom boundaries.

S2.4.2. Geocoding

Geocoordinates have been made available for the DHS datasets; in the downloaded data for all years, even dating back to 2000, these geocoordinates were already matched to districts based on geocoordinate-based definitions in 2010, 2016, 2017, 2018, and 2020. As such, I was able to directly use district names to link each enumeration area to post-split district locations.

S2.4.3. Variables

From these datasets, I construct the following indicators:

The variable Healthcare Access_{it} measures healthcare access, including whether a woman with a pregnancy in the last two years attended any antenatal care visits, attended 4+ antenatal care visits, or attended any antenatal care visits in the first four months of pregnancy, whether the mother received a postnatal care check within two days, and whether the newborn received a postnatal care check within two days.

The variable Healthcare Quality_{it} measures healthcare quality, including whether a woman reported skilled assistance during any antenatal care visits, whether the woman took iron tablets or syrup during her last pregnancy, whether the woman's blood pressure was taking during any antenatal visits, whether a urine sample was taken during any antenatal visits, whether a blood sample was taken during any antenatal visits, and whether the woman received more than two tetatnus injections during her last pregnancy.

The variable Child Health Services Index_{it} measures child healthcare utilization, including whether care was sought during a fever, whether care was sought for an acute respiratory infection, and whether the child had access to antibiotics for any recent fever.

The variable Sanitation Index_{it} measures access to sanitation, including whether the household uses improved drinking water, whether the household can access water in less than 30 minutes, whether the household has water access on the premises, whether the household has an improved sanitation system, and whether the household has a non-open sanitation system.

The variable HH Has Electricity_{it} is a self-reported variable on whether or not the household has electricity.

S2.5. Ministry of Education and Sports Data

S2.5.1. Description and Coverage

The education data used in the study consist of data at the school level for all schools in Uganda from 2006 to 2016 provided by the Ministry of Education and Sports (Uganda Ministry of Education and Sports 2016). This dataset includes, for each school, the number of teachers and students, as well as data on infrastructure, including classroom equipment and school water source.

The number of schools in Uganda increases over time, with roughly 11,817 schools in the data as of 2006, and 12,305 schools in the data as of 2016. The panel is unbalanced, with schools entering and exiting the data, due either to school openings, school closings, or data entry errors. In total, roughly 14,183 schools are represented in the data. Of those, 9,327 are represented in the full 11 years of data, and another 1,853 are represented in 10 years of data. The remaining 3,000 or so schools have 9 or fewer years of data; there are approximately 1,185 schools with 4 or fewer years of data.

S2.5.2. Geocoding

I use administrative unit data on the district, county, and subcounty location of the school to place it in the correct post-2011 district. District splits align almost always with county locations, though sometimes with subcounty locations as well. As I knew the subcounty and subcounty location of the school pre-split, I was able to locate each school in the correct post-split district even in data from prior to the split.

S2.5.3. Variables

From the Ministry of Education data, I construct the following indicators:

The variable Teachers per 100 students$_{it}$ calculates the ratio between the number of teachers and every 100 students. For example, if the outcome takes the value 1, that means the school has a 100-to-1 student-to-teacher ratio, or one teacher for every 100 students. If the outcome takes the value 2, that means the school has a 100-to-2 student-to-teacher ratio, or one teacher for every 50 students.

The variable School Input Index$_{it}$ measures the quality of school inputs, combining z-score normalized measurements of the ratio of rooms to students, the proportion of students with a desk and chair, and whether the school has access to piped water or a borehole (rather than relying on rainwater).

The variable Primary 1 Students$_{it}$ measures the number of students enrolled in Primary 1, the first grade in the school, in a given year.

The variable Primary 7 Students$_{it}$ measures the number of students enrolled in Primary 7, the last grade in the school, in a given year.

The variable All Students$_{it}$ measures the total number of students in a school in a given year.

S2.6. GeoQuery Data

S2.6.1. Description and Coverage

The GeoQuery dataset contains a combination of remote sensing data and data from research projects at AidData, which are mapped to each village in Uganda (Goodman et al. 2019). I utilize data on economic activity, including nighttime lights and vegetation indices, as well as data on aid volumes from three different sources; I discuss each in turn below.

For all aid measurements, the data contain information on project aid. This type of aid differs from budgetary or general support in that it is allocated to specific projects, and therefore specific locations. It is worth noting that the construction of the underlying dataset involves certain assumptions—for example, that aid which cannot be located at the subdistrict level is spread evenly across the district—which might bias the data towards or against higher volumes of aid for new districts in the pre-period. Projects can take various forms, such as supporting infrastructure, education, employment, or other developmentally related goals. I utilize population data to normalize each type of aid by population, creating per capita aid measures.

S2.6.2. Geocoding

The GeoQuery data extraction process involves matching geocoordinate-linked data to administrative boundaries. I use village-level administrative boundaries for Uganda in the post-split period, which were already linked to post-split districts (even for pre-split years). Correspondingly, every village is linked to its post-split district location throughout the analysis.

S2.6.3. Variables

From the different data sources in the GeoQuery data, I construct the following indicators:

The variable Nighttime Lights$_{it}$ is a measure of the average intensity of nighttime lights in a given village. I use the DSMP-OLS nighttime lights data, available annually from 2000 to 2013 (Elvidge et al. 2014).

The variable World Bank Aid Per Cap$_{it}$ measures the amount of World Bank project aid allocated to a given Ugandan village, measured annually from 2000 to 2014 and normalized relative to village-level population (AidData 2017).

The variable Chinese Official Aid Per Cap$_{it}$ measures the amount of Chinese official project aid allocated to a given Ugandan village, measured annually from 2000 to 2014 and normalized relative to village-level population (Bluhm et al. 2018).

The variable Other Foreign Aid Per Cap$_{it}$ measures the amount of project aid from other donors allocated to a given Ugandan village, measured annually from 2000 to 2014 and normalized relative to village-level population (AidData 2016). The largest donor in the "other" group is the African Development Fund; other prominent donors include the European Union, United States, Norway, and the United Kingdom.

In addition, across all per capita variables from this dataset and elsewhere, I use population data, available in 2000, 2005, and 2015, (CIESIN 2016). The data are available at the village level, which allows me to either use it at the village level or to easily aggregate it into the district units which exist post-2010. In order to smooth between years, I assume steady population growth between 2000 and 2005, 2005 and 2010, and 2010 and 2015, and impute accordingly.

S2.7. Ministry of Health Data

S2.7.1. Description and Coverage

Health data in the study come from the Uganda Ministry of Health's Human Resources for Health audit reports. The first report contains data current as of June 2010 (Ministry of Health 2010). Though there are some references to the report being annual, reports were available on the Uganda Ministry of Health's knowledge portal for 2010 (Ministry of Health 2010), 2014 (Ministry of Health 2014), 2015 (Ministry of Health 2015), 2017 (Ministry of Health 2017), and 2018 (Ministry of Health 2018). Note that while the 2010 report was released in 2010, the underlying data were collected in 2008 and 2009, meaning it can be considered pre-split data.

Each report contains district-level data on staff resources, including the number of allocated positions, the number of those positions filled, the number that remain vacant, and the number of individuals appointed without a formal administrative position (called "excess" in many tables). For most years, these data are disaggregated between health facilities and district health offices.

S2.7.2. Geocoding

Though most of the years contain only district-level data, facility-level data are available in 2010, as well as in 2017. Since facilities are in fixed locations, I use the 2010 facility-level data to recreate aggregates for post-split districts. In other words, a parent district may be listed in 2010 containing some number of health facilities of various levels. By using later data on health facility location, I can determine which of those facilities remain in the parent district, and which belong to the new district. I then re-aggregate to "post-split district level" to generate indicators, including the total number of health workers in the (as reported or reconstructed) district, which I normalize against population as measured in thousands, and the share of health workers without official positions.

Since pre-split districts only have a single district health office, a similar division is not possible for the district health office data. For this variable, I instead assume that staffing levels in the parent district apply for the new district pre-split.

S2.7.3. Variables

From this dataset, I construct the following indicators:

The variable Health Staff per $1K_{it}$ measures the number of health workers per thousand people in a given district. For example, if the outcome takes the value 1, that means the district has a 1,000-to-1 population-to-health worker ratio, or one health worker per 1,000 people. If the outcome takes the value 2, that means the district has a 1,000-to-2 population-to-health worker ratio, or one health worker per 500 people. Note that while the variable is amalgamated to the post-split district level, the underlying numbers are based on aggregating up from facilities. Correspondingly, no imputation was necessary.

The variable DHO Share Vacant_{it} measures the share of positions within the district health office that are vacant in the year in question. For pre-split districts, I impute such that parent and new districts have the same value.

The variable Health Staff Positionless_{it} is a district-level measurement of the share of health workers across all facilities who are "unofficial," meaning they have not gone through the requisite approvals process in order to receive their posting. Note that while the variable is amalgamated to the post-split district level, the underlying numbers are based on aggregating up from facilities. Correspondingly, no imputation was necessary.

S2.8. Uganda National Panel Survey Data

S2.8.1. Description and Coverage

The Uganda National Panel Survey is a multi-topic panel household survey conducted in Uganda. The survey dates back to 2005, and in that round included some retrospective questions focused on 2001 (Uganda Bureau of Statistics 2009). More recent rounds were conducted in 2009, 2010, 2011, 2013, 2015, and 2018, and include nationwide sampling of households, and tracking of households and communities across years (Uganda Bureau of Statistics 2010, 2011, 2013, 2015, 2018). The household survey involves questions on household members, education, health, labor-force status, housing conditions, assets, sources of household income, consumption, and more.

The panel is unbalanced, representing a total of 7,169 households which appeared in at least one round later than 2011. Of these, 1,336 households were surveyed in at least five of the seven rounds; another 1,162 were surveyed in three or more rounds. The remaining 4,670 were surveyed in either one or two rounds, due to household splits, households leaving or entering the sample, or a refusal to be surveyed. District information is recorded in the survey, and I use post-2011 locations to determine treatment status. The Uganda national panel surveys are not representative at the district level in Uganda, though they are representative of each of the four largest regions of Uganda.

S2.8.2. Geocoding

District locations were recorded in each year for every household in the survey, though more finely grained location information was not. As the surveys are panel surveys, I was able to use post-2011 information on district location to record the appropriate post-split district location for each household. Correspondingly, all households are linked to post-split locations even in the 2001 and 2005 data.

S2.8.3. Variables

From this dataset, I construct the following indicators:

The variable Has Electricity$_{it}$ measures whether a household indicates that they have access to electricity in a given survey round. For the 2001 retrospective and 2005 survey rounds, I construct this measure based on whether the household utilizes electricity for lighting, cooking, or other activities. For 2009 onwards, I construct this measure using a question which specifically asks whether the house has electricity.

The variable Asset Index$_{it}$ is an annualized index measuring asset levels for a household starting in 2005 and ending in 2018. Specifically, it includes measures of whether the household owns a house; other building; appliances such as a kettle or flat iron; electronics such as a television, radio, or cassette player; generator; solar panel; bicycle; motorcycle; other means of transportation; jewelry; and mobile phone. Each of these items is asked about in every survey round from 2005 to 2018. I normalize each relative to its own year, and sum the measure of normalized item ownership to construct an aggregate asset index. Note that prior to 2018, the survey asks whether any member of the household owns the asset at present; in 2018, the question differentiates between owning individually and owning jointly. For consistency, I consider all forms of ownership as equivalent to answering yes in an earlier year.

The variable Welfare Index$_{it}$ is an annualized index measuring general household welfare starting in 2001 and ending in 2015. Specifically, it includes measures of whether every member of the household has two sets of clothing; whether every member of the household has at least one set of shoes; whether every child in the household has a blanket; and the number of meals taken per day in the household. Each of these items is asked about in every survey round from 2005 to 2015, and retrospectively for 2001; however, these items do not appear to have been asked in the 2018 survey round. I normalize each relative to its own year, and sum the measure of normalized general welfare items to construct an aggregate welfare index.

S2.9. Uganda Revenue Authority Data

S2.9.1. Description and Coverage

I utilize data on registration for tax identification numbers (TIN) and payment of individual taxes from the Uganda Revenue Authority (URA) (Uganda Revenue Authority 2018). Individuals must register themselves with URA in order to create a TIN, which is used to pay taxes or for the payment of wage taxes by one's employer; a TIN is also required for various other transactions, such as land titling or transferring a vehicle registration. The data include both year of registration and location, dating back to the early 2000s. I also have access to tax payment rates for Uganda's individual taxes, including both the personal income tax and the presumptive tax, which is levied on small businesses; these data are available from 2013, as that was the year in which the tax collection data were first digitized.

S2.9.2. Geocoding

Upon registering, individuals are required to submit a physical address, which uniformly includes district locations and generally includes much richer detail. For all those who registered post-2011, post-split districts were extracted from the registration data. For those who registered pre-2011, information on town, county, and subcounty location was used to impute the correct post-2011 district, locating each individual in the correct post-split district for normalization.

S2.9.3. Variables

From this dataset, I construct the following indicator:

The variable Tax Registration Share$_{it}$ measures the number of individuals in a given district who are registered with the URA to pay individual taxes, normalized relative to the population of the district. This variable is constructed annually.