Infrastructure and Fiscal Capacity *

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Abstract

This paper shows that infrastructure investments enhance fiscal capacity. Drawing on a novel dataset combining information on the location and timing of all road upgrades in Rwanda with a decade of administrative tax and census records, we estimate significant and sizable positive effects of road upgrades on surrounding municipalities' tax revenues. These effects are driven by firm entry as well as land value appreciation, captured through taxes on rental income at the local level. While the additional revenues are unable to finance the initial investments of the central government, they present a doubling of local municipalities' revenues after 6 years.

JEL classification: R41, R42, H25, H27, O18

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1 Introduction

Transportation infrastructure is critical for economic development.[1] Yet, many developing countries still invest too little, partly due to substantial project costs which appear to overburden public budgets (Brooks and Liscow, 2023; United Nations, 2025). Additionally, the presence of large informal sectors suggests that potential economic gains are uncertain to feed back into public funds. If, in contrast, governments are able to capture economic gains through the tax system, the effective costs of infrastructure projects would reduce, encouraging more investments. Empirical estimates on this self-financing aspect of infrastructure investments are scarce. This holds particularly true for low-income countries, where most action is needed.

This paper fills this gap by investigating whether upgrading road quality enhances tax revenues and to which extent this could refinance initial investments. The setting we study is Rwanda, which, despite being classified as a low-income country, is one of the fastest growing economies in Africa (International Monetary Fund, 2025). Part of the country's success could be attributed to the country's strong commitment to maintaining a high-quality transportation infrastructure (Esfahani and Ramırez, 2003; Allen and Arkolakis, 2022; Coşar et al., 2024). Such commitment necessitates large investments, however, straining the country's fiscal situation. As Rwanda collects only about 15% of its GDP as tax revenues, broadening its tax base is a pressing policy issue, which needs to be tackled in parallel to infrastructure investments.

For our analysis, we construct a novel dataset which maps the exact location and timing of all national road construction and –maintenance works in Rwanda to a decade of census information and the universe of administrative tax data at granular geographic levels to estimate the fiscal and economic returns to infrastructure investments. The empirical identification relies on the quasi-random staggered upgrading of existing highways, alleviating concerns of treatment endogeneity. Our results point to significant gains in local tax revenue upon the upgrading of nearby highways. These gains are concentrated among taxes collected by local governments such as taxes on rental income as well as fees from small businesses. In contrast, we measure precise null effects for centrally collected taxes, such as Corporate Income Tax (CIT) or Value Added Tax (VAT). In line with the fiscal effects, we estimate an immediate increase in firm entry, concentrated among small and medium-sized

[1] There has been an extensive literature linking infrastructure, such as road construction to economic progress across many dimensions (Donaldson, 2018; Allen and Arkolakis, 2022; Storeygard, 2016; Alder, 2025), with most evidence from middle und high income countries.

firms, while there are no significant changes for large and formal firms. A back of the envelope calculation shows that while doubling local municipalities' revenues on average after 6 years, the additional tax revenues are unlikely to refinance the infrastructure investments in the foreseeable future.

The causal interpretation of spatially targeted policy evaluation is usually impeded by the lack of a suitable control group. Treated and untreated regions might be inherently different and thus hardly comparable to one another. We overcome this issue by relying on the staggered paving of Rwanda's highways. In particular, we compare municipalities that are close to highways to those that are not, before and after the closest highway was paved, respectively. Leveraging this variation across time and space in an event-study design, we estimate the dynamic post-treatment as well as pre-treatment effects of road paving. Importantly, we complement our analysis by restricting the control group to municipalities which are close to a national road that has *not* been paved by the end of our study period.

We run our analysis on a novel dataset, which combines three sources of administrative data. First, in collaboration with the Rwanda Transport and Development Agency (RTDA), we mapped out all road construction works between 2011 and 2024. This high-resolution data contains information on the exact location of road segments as well as the timing of either its construction or its upgrade. We can further distinguish whether the road segment was upgraded from a gravel road to paved road or whether it was upgraded but remained an unpaved road. Importantly, we also know the costs of each upgrade or construction. Second, we map the road information into the administrative regions of Rwanda. In particular, we geographically link each road segment to Rwandan municipalities (third administrative unit) and ultimately add administrative data on the universe of tax declarations. We can differentiate between different taxes collected by the central government and those collected by local municipality governments. Third, we use four waves of the Rwandan establishment census to obtain information on economic activity at the same administrative level.

Our results convey a striking picture of how road upgrades affect local economic outcomes and fiscal capacity. We observe that upon highways being paved, locally collected tax revenues increase strongly and significantly. The point estimates increase gradually over time and suggest that local governments' revenues doubled after 6 years. Importantly, the pre-trend coefficients are close to zero and insignificant throughout – supporting the assumption of parallel trends and therefore, a causal interpretation of the estimates. We show that these increases can be attributed to a boost in rental taxes, reflecting increases in land values, and increases

in fees collected from local businesses, reflecting more economic activity. In a back-of-the-envelope calculation, we find that, in the initial 6 years of the highway being paved, the additional revenues could recover 3.1% of the initial investments directly. Assuming road quality to be constant, the constructions would take about 72 years to be refinanced.

For taxes collected by the central government, we measure precise zero effects both for the pre- as well as post-treatment periods. This discrepancy between local and central government also shows when we estimate the impact of road paving on firm entry. While we see a significant increase for smaller and informal firms, we measure no significant effect for larger formal firms. While it is difficult to disentangle the role of enhanced enforcement capacity and real economic gains, we argue for the latter channel being more relevant. It is important to mention that Rwanda's highways are financed by the central government. While our results therefore document the self-financing aspect of infrastructure projects, they also uncover a mismatch between who bears the costs and who reaps the benefits from a fiscal perspective.

In complementary analyses, we show that our results hold in a variety of different specifications. Finally, we argue that the type of road upgrades matter. When redefining our treatment to upgrades which leave roads unpaved, such as improving drainage or renewing the soil surface, we measure no significant impact on neither local nor central tax revenues.

This paper contributes to several strands of the literature. First, we contribute to the literature studying the economic effects of infrastructure in developing countries. Road paving in particular has been shown to have positive impacts on house values and local economic activity in a Mexican city (Gonzalez-Navarro and Quintana-Domeque, 2016). India's highway expansion has been found to have broadly positive economic effects (Datta, 2012; Ghani et al., 2016; Asturias et al., 2019; Das et al., 2024; Chatterjee et al., 2025). For rural roads, the evidence is somewhat more mixed for economic output in India (Shamdasani, 2021; Asher and Novosad, 2020) but more positive for Vietnam (Mu and Van de Walle, 2011). Gertler et al. (2024) report substantial welfare gains from road maintenance in Indonesia, but suggest no feedback effect into tax revenues. We provide clear evidence for such an effect in Rwanda. In China, Baum-Snow et al. (2017) show how the construction of transportation infrastructure majorly influenced the decentralization and structure of modern Chinese cities. Our study advances from these existing studies across two dimensions. First, to the best of our knowledge, we provide the first causal evidence

on how infrastructure projects directly impact local tax collections.[2] Second, we provide evidence on the positive economic impact of infrastructure in a low income setting, which is usually difficult due to data unavailability.

The second strand of the literature we contribute to studies the efficiency of public investments. Ryan (2021) shows how the setting up of a new electricity market in India lead to market integration and economic gains, justifying the initial investment. Faber (2014) and Allen et al. (2024) study how transportation infrastructure improves market efficiency in China and Colombia, respectively. In contrast, our study makes a more direct point in showing how the investment costs themselves are effectively lower when factoring in positive spillovers to public budgets.

Finally, we contribute to the ongoing debate on how low income countries can increase tax capacity (Besley and Persson, 2014; Okunogbe and Tourek, 2024; Bergeron et al., 2024). While infrastructure investments are usually viewed as an expenditure item only, we provide novel evidence that these investments can be effective in increasing local revenue collection – even in the medium to long term.

The rest of the paper runs as follows. Section 2 provides the institutional background on road development and fiscal decentralization in Rwanda. Sections 3 and 4 describe our data and empirical strategy. We present our results in Section 5 and evaluate efficiency in Section 6. The last section concludes.

2 Institutional Background

We begin by describing the institutional setting of Rwanda, detailing the evolution of its road development policy as well as providing more information on the structure of Rwanda's decentralized tax system.

Road development policy. Until recently, Rwanda's roads were widely acknowledged to be in bad shape. Periods of political instability had left the country's infrastructure largely unable to cater its citizens' needs. While first efforts to improve internal connectivity began in the early 2000s, even 10 years later only about a third of roads were classified as being in "good condition" (African Development Bank, 2011). To improve this situation and expedite the process of road upgrading, the Ministry of Finance established the Rwanda Transport Development Agency

[2]Other studies have shown that the provision of public goods (such as roads) and in particular, communicating them effectively can boost tax morale of citizens (Sandholtz and Vicente, 2024; Montenbruck, 2023; Fernández et al., 2025; Kresch et al., 2023), which is not the focus of this study.

(RTDA) in 2010. Among other duties, the agency was specifically assigned to implement and oversee the paving of all Rwandan national roads i.e. highways. These highways connect the major cities of Rwanda and additionally provide links to the border regions to Uganda, Kenya and Tanzania. Therefore, their functioning is crucial for intra- and international trade, worker mobility and economic activity overall. While the majority of highways already existed in 2010, most of them were unpaved or even simple gravel roads. Paving was therefore a natural first step in improving connectivity. As financial resources and manpower were constrained, the paving was done subsequently, with different stretches of different highways being paved at different points in time. Figure A.1 shows photographs of Rwandan highways both unpaved as well as paved. All roads in Rwanda are toll-free and thus do not constitute direct sources of government revenue.

Fiscal Decentralization in Rwanda. In Rwanda, taxes are collected and spent at two governmental layers. On the one hand, the central government manages the main and common tax types such as CIT, PIT, VAT as well as any other non-tax revenues e.g. proceeds from sales of land. On the other hand, local government of municipalities[3] impose taxes on immovable property, rental income and various fees, including trade licenses. A trade license is an annual contribution that each firm or individual has to pay to the local government in order to do business. Distinct from an actual tax on income or sales, it is a fee to operate, irrespective of whether any income is generated. Importantly, this also means that in the view of the central government, such businesses are still considered informal.

The decentralization of tax collection was introduced in 2005 with the goal of providing both financial means to– as well as empowering own resource-mobilization of municipalities. By delegating to local authorities, the hope was to strengthen fiscal autonomy, promote equitable and efficient resource allocation, and ultimately enhance service delivery at the local level. On average, local taxes contributed about 15% to total district budgets between 2011 and 2024, making it an important source of revenue for municipalities. In addition to their own revenues, local governments receive transfers from the central government. However, they are also encouraged to contribute their own revenues to centralized funds. In particular, to the Urban Transportation Funds (UTF), which finances road construction and maintenance. This highlights the importance of local taxes in financing infrastructure, also from a central planning perspective (Ministry of Infrastructure, 2021).

[3]In Rwanda, these are the third administrative unit, commonly called "sectors". Nevertheless, we stick with the term "municipality" to avoid confusion with the term for industry classification.

3 Data

We leverage a unique combination of administrative tax records, detailed road network data and comprehensive establishment censuses. These datasets offer granular insights into firm-level economic activity, tax collection at both central and local levels, and the spatial and temporal variations of the country's transportation infrastructure upgrades.

3.1 Road Network in Rwanda

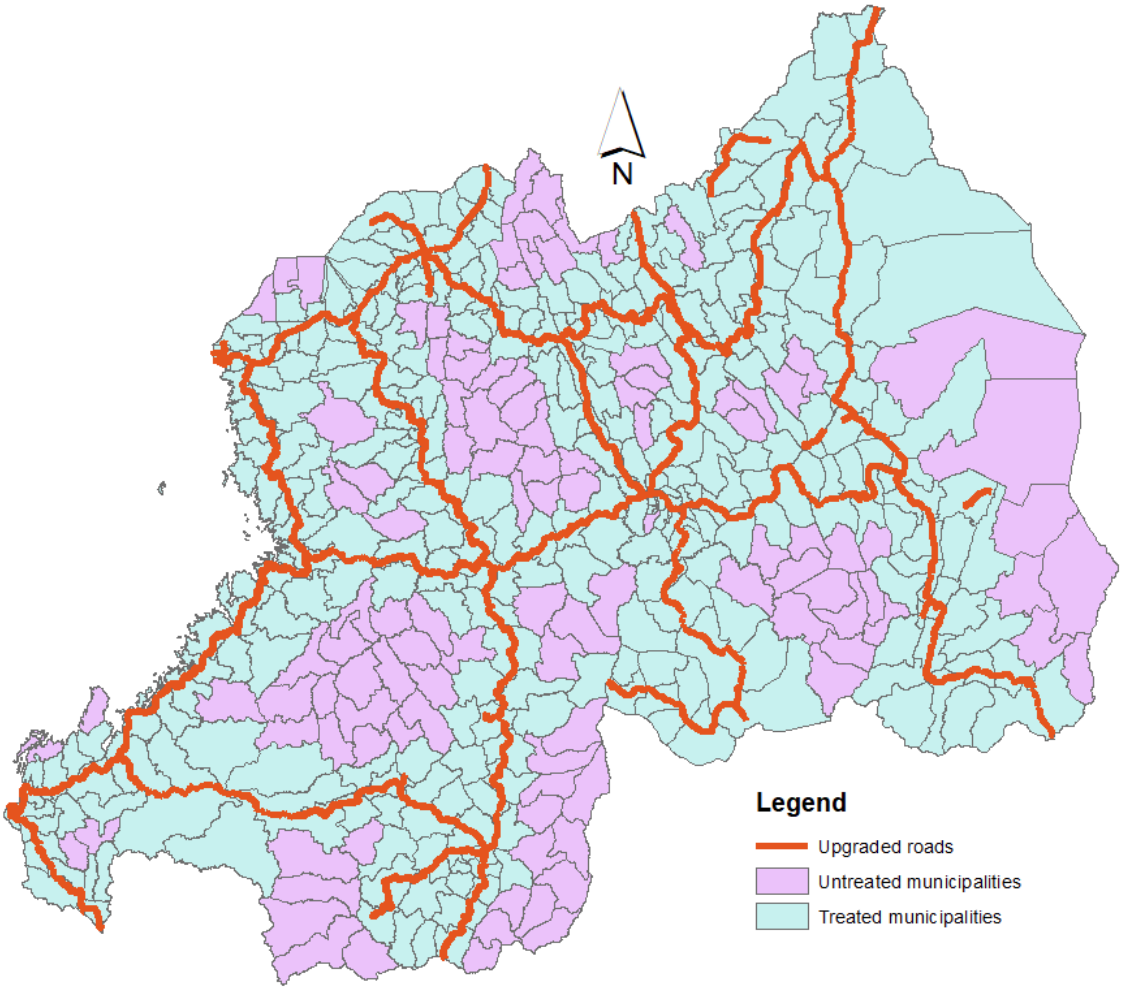
The data on Rwanda's road network and maintenance has been constructed through a collaborative effort involving the Ministry of Infrastructure (MININFRA), the Ministry of Finance and Economic Planning (MINECOFIN) and the RTDA. It provides critical information on the characteristics of various road segments, including their nature (e.g. upgraded, paved), their precise length in kilometers within each municipality, and the exact dates of any upgrade completion. This temporal and spatial granularity allows to identify effects of road improvements on local outcomes. In addition, the data provides information on the costs of each construction work, hence permitting us to relate any potential effect of the project to its cost. Overall, the granular road network allows to precisely identify municipalities affected by upgrades and track the evolution of the road infrastructure over the study period, offering a robust reference for our analysis. Figure 1 shows the distribution of the highway network across municipalities.

3.2 Administrative Tax Records

Our main analysis focuses on the impact of road upgrading on local tax collection. The tax data has been shared by the Rwanda Revenue Authority (RRA) and comprises micro-level information on the universe of tax declarations and fees, from both the central as well as local governments. Figure A.2 shows that the distribution of tax revenue is similar across municipalities close to national roads and those which are not. High tax collections, however, are more prevalent among the former ones.

Central tax data. The first dataset contains information on CIT, PIT, VAT, and other miscellaneous central taxes. The data is reported at the firm and individ-

Figure 1: National roads and treated municipalities in Rwanda



Notes: This figure shows the distribution of Rwanda's municipalities and paved highways. The light-blue color represents treated municipalities within 2km of paved highways while purple indicates untreated municipalities. The untreated municipalities account for around 27 % of the total local municipalities the country. Data Source: RTDA.

ual level, respectively, allowing for a detailed analysis of individual contributions to central government revenue. It covers the years 2012 through 2024, thereby providing a sufficiently long time series to capture the dynamic effects of road upgrades.

Local tax data. Taxes collected at the municipality level are referred to as local taxes. The data comprise information on all trade license fees and rental taxes which have been collected in a municipality in a given year. To gather information on tax liabilities, i.e. how much rent has been paid or whether a business is running, respectively, local governments often rely on informal sources such as village leaders.

Such practices have been shown to be an effective tool in other low-income settings as well (Balán et al., 2022). A shortcoming of the data is that we do not observe property tax collections, which are potentially important for local governments.[4] One reason for this is that property taxes in Rwanda are in practice only collected when properties are sold. Therefore, municipalities often lack consistent records. However, we argue that the evolution of rental taxes serves as a good proxy for property taxes as well. Both move in parallel to land values (ceteris paribus). Thus, any positive effect one would measure on rental taxes could be viewed as a lower bound on the joint effect on rental and property taxes. Figure A.3 shows that local tax collections are an important source for governments, making up to 20% of their total budget.

3.3 Establishment Censuses

To capture the effect of road upgrades on economic activity more broadly, we use the Rwandan Establishment Censuses collected by the National Institute of Statistics of Rwanda (NISR). We use four census waves, namely 2011, 2014, 2017, and 2019. A key feature of these censuses is their comprehensive coverage of all active businesses operating in the country, including formal and informal firms. This inclusiveness is particularly important in a low-income country setting where a significant portion of economic activity occurs within the informal sector. While the finest geographical unit in the publicly available census data are districts, by courtesy of the NISR, we are able to retrieve information at the municipality level – our geographical unit of interest.

To analyze patterns of firm creation in Rwanda, we construct a harmonized panel of newly established firms using the four census waves denoted as R_1, R_2, R_3, and R_4, respectively. For each census round R_t, we define the subset of newly created firms such that

$$\mathcal{N}(R_t) = \{F_i \mid y_i \in [t-3,\, t-1]\}, \tag{1}$$

where F_i stands for firm i and y_i is its respective founding year. By doing so, we essentially restrict our focus to firms created within three years preceding each census, thereby ensuring that each round provides a cohort of comparably young firms and mitigating potential biases arising from firm survival and maturity effects. The final panel of new firms is then obtained as a combination of these subsets across

[4] Among others, Brockmeyer et al. (2023), Montenbruck (2023) and Best et al. (2025) show that property taxes make a substantial part of city government revenues in developing countries.

all four census R_t:

$$\mathcal{P} = \bigcup_{t \in \{1,2,3,4\}} \mathcal{N}(R_t). \tag{2}$$

This approach yields a pseudo-longitudinal dataset of business creation dynamics within municipalities, capturing cohorts of recently established firms between 2008 and 2019.

4 Empirical Strategy

To identify the causal economic impact of road upgrading, we draw on our comprehensive dataset linking road infrastructure and administrative tax data. We implement an event-study-style analysis comparing the evolution of tax collections and other outcomes between municipalities which receive road upgrading to municipalities in the same region which did not.

In particular, we classify treatment municipalities as those whose boundaries intersect with the closest 2km band around any highway that has been paved during our study period. These are indicated by cyan color in Figure 1. Municipalities outside that band serve as control units. We leverage the variation in the timing of road upgrading across our sample and code a municipality as being treated only from the time the upgrading was completed. Thus, the treatment effects do not capture the mechanical effect of the construction itself. Our main analysis defines upgrading as construction works which change the road's status from unpaved to paved. In further analyses we also consider works which leave roads upgraded but still unpaved. In case a road has received multiple upgrades, the first year is coded as the treatment time.[5] Our main estimating equation reads

$$\ln(y_{it}) = \sum_{k=-6, k\neq -1}^{6} \beta_k \mathbf{1}[t = k] \times \mathbf{1}[i = treated] + \alpha_i + \gamma_t + \epsilon_{it}, \tag{3}$$

where y_{it} is the outcome of interest for municipality i in time t, $\mathbf{1}[t = k]$ denotes indicator variables for relative time k to the year a road was upgraded, $\mathbf{1}[i = treated]$ indicates whether municipality i is in the treatment group. Further, α_i and γ_t denote year and municipality fixed effects. ϵ denotes an idiosyncratic error term. This approach allows us to non-parametrically study the spatial dynamic effect of road upgrading at the municipality level, captured by the β_k's. The effects of road

[5]The vast majority of municipalities was treated only once. We show below that excluding multiple treated municipalities does not change our results.

upgrading may not be uniform across the various regions of Rwanda and across our study period. To account for this treatment heterogeneity, we rely on modern event-study estimations as developed by Sun and Abraham (2021).

A classical concern with the causal interpretation of dynamic spatial effects is that treatment does not occur randomly but rather in places which exhibit different economic trajectories than non-treated places. Put differently, the parallel pre-trends assumption may fail due to systematic selection into treatment. Clearly, as we study the upgrading of highways which already existed, the location of treated municipalities is not random. In contrast, our main specification compares municipalities that are close to highways to those that are not, before and after the highway was upgraded, respectively.

Whether they would be comparable in the absence of road upgrading, therefore remains an empirical question, which we address in four ways. First and foremost, we show that our results hold when restricting the control group to municipalities which are close to a national road that has *not* been paved by the end of our study period. Second, we explicitly estimate differences prior to the treatment, offering an empirical check on the plausibility of the parallel-trends assumption. Third, in our main analysis we exclude the most economically prosperous areas of Rwanda, which are most likely to exhibit differential growth paths compared to the rest of the country. Fourth, we run placebo tests in which we randomly assign the treatment timing across treatment units and re-estimate Eq. (3) 200 times in a bootstrap procedure. Obtaining significant results for random assignment of the treatment timing would suggest that structural differences between treatment and control group are driving the effects. Vice-versa, insignificant results would underpin the causal interpretation of our main analysis. In further analyses, we run several robustness checks such as redefining our control group to be municipalities close to highways which were the last to be upgraded within our study period i.e. the *last-treated units*, or including additional municipality-level controls such as population and topological indicators, interacted with time dummies, to Eq. (3).

5 Results

This section presents the estimation results following the methodologies described in Section 4 applied to the data described in Section 3.

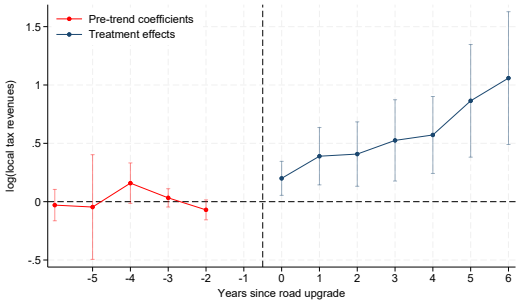
5.1 Road upgrading and local vs. central tax revenues

We begin by summarizing the estimated effects of road upgrading on local and central tax revenues. Figure 2 plots the event-study results as estimated by Eq. (3). The horizontal axes denote the year relative to treatment (k) and the vertical axis depicts the estimated coefficients $\hat{\beta}'_k s$ with 95% confidence intervals. Panel (a) considers the effect on local taxes (the sum of rental taxes and trade license fees) while panel (b) concerns central taxes (CIT, PIT, VAT; cf. Section 2). We exclude the districts of Nyarugenge and Kicukiro which are the most prosperous in the country.

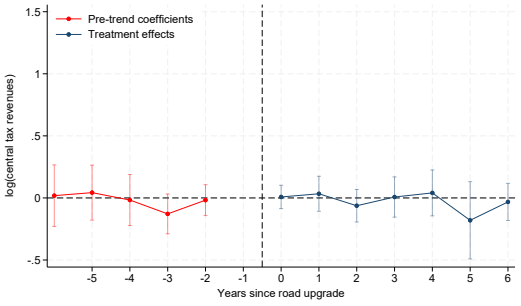
In all panels, the pre-treatment periods (in red) reveal that the parallel trends assumption is likely to hold. All pre-treatment periods are insignificant and close to zero, suggesting that neither local nor central tax collections in municipalities close to highways were on different growth trajectories than the ones not close to highways. We are therefore confident that the SUTVA condition is satisfied in the immediate lead-up to the road upgrading. As the treatment year is defined as the end of construction works, the flat pre-trends also ensure the absence of anticipation

Figure 2: Road upgrading and tax collection

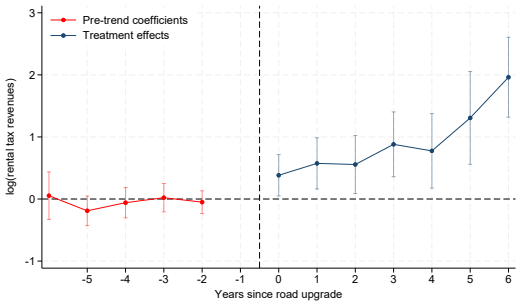
(a) Local taxes



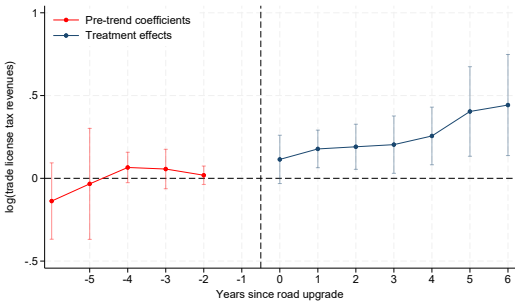
(b) Central taxes



(c) Local: Rental taxes



(d) Local: Trade license



Notes: This figure plots the β_k coefficients estimated from Eq. (3). Panel (a) has local taxes (rental and trade licenses) while panel (b) has central taxes (CIT, PIT, VAT) as the dependent variable. Panels (c) and (d) differentiate local taxes by rental taxes and trade licenses. Standard errors are clustered at the municipality level. Data Source: Administrative tax records from RRA.

effects, which could contaminate our interpretation.

Turning to the actual treatment effects (in blue) a striking picture emerges. In panel (a) we estimate positive and significant effects, increasing over time. Overall, they suggest that 6 years after a highway has been paved, local tax collections doubled. In contrast, panel (b) shows insignificant effects on central tax revenues for all treatment periods. Even after 6 years, we measure a precise null for the treatment effect. While this discrepancy is puzzling at first sight, one needs to consider the institutional background of the empirical setting. Most of Rwanda's firms are small, informal and not registered for CIT or VAT. It is therefore reasonable to assume that ad-hoc increases in economic activity may not be driven by large formal firms which increase operations in a meaningful way due to availability of a better road. We confirm this hypothesis in our analysis of firm entry below. Figure A.4 in the Online Appendix provides suggestive evidence that CIT collections increased in the short term but this does not suffice to see a significant impact on average.[6] Additionally, the PIT base is thin and largely concentrated in the better-off areas of Kigali, which we exclude from our sample (Kangave et al., 2020).

To see what drives the positive impact on local tax capacity, we differentiate between rental tax revenues and trade license fees in panels (c) and (d) of Figure 2, respectively. Reassuringly, both figures exhibit flat and insignificant pre-trends. Panel (a) shows a strong increase in rental tax revenues, ranging up to 200% 6 years after the road upgrade. This exceptionally large effect can be rationalized when taking into account the often very low baseline values of rental income in rural areas. Also, we conducted interviews with local tax collectors and municipality officials, who independently reported even larger albeit similar increases.[7] Finally, these increases align well with the literature showing that land values increase significantly upon road construction (Mohring, 1961; Sorin, 2025). In the same vein, while we cannot yet measure it, this result also suggests that property tax collections, a major source of revenue for local governments (Brockmeyer et al., 2023) could also be increasing.

Results for revenues from trade license fees are presented in Panel (d) of Figure 2. We measure significant and increasing treatment effects. The point estimates are lower than for rental taxes but still economically meaningful. After 6 years, revenues from trade license fees are about 50% larger than in the year before the upgrading.

[6] Even for small firms under the simplified 'flat-fee' regime, we do not observe a positive impact.

[7] During interviews we conducted with municipality officials in the Southern and Eastern Province of Rwanda in November 2025, it was stated that "[...] before the road pavements, rents for a small shop were about RWF 15,000. After the paving, it is now about RWF 50,000."

Finally, we argue that not all road upgrades are equally effective. In fact, when we redefine the treatment to road upgrades after which the road stayed unpaved, we observe no positive effect on either tax revenue (Figure A.5). Such upgrades could, for instance, consist of improving the drainage system on the side of the roads or changing the surface soil. This observation aligns well with the evidence by Storeygard (2016) who shows that trade costs in Africa are effectively reduced only through paved and not by unpaved roads. It also delivers clear policy insights on which type of road works might be more effective in boosting economic activity or enhance fiscal capacity.

5.2 Road upgrading and local economic activity

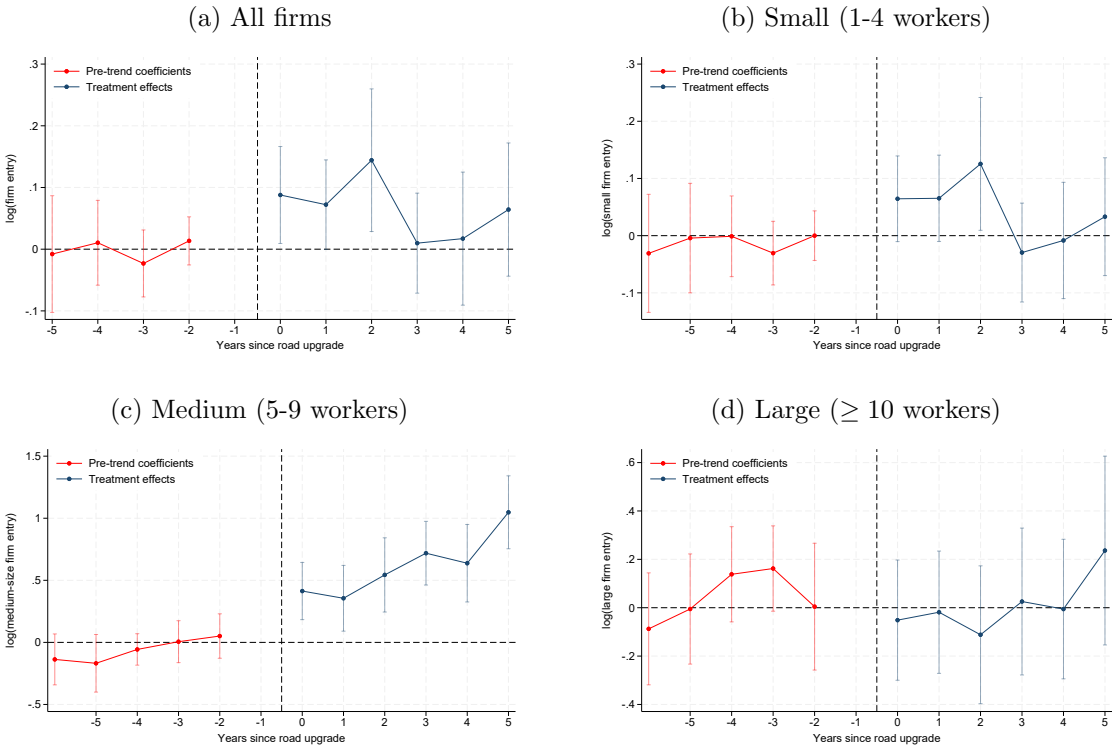
While the previous analyses focus on the pure fiscal effects, transport infrastructure may have positive spillovers along many other dimensions. In the following, we focus on firm entry as a key indicator of economic activity (Bartelsman et al., 2013).[8] Our results thus far set certain expectations on the impact of road paving on firm entry. In particular, we saw an increase in revenues from trade license fees collected from local business owners. Thus, we would expect an increase in small and medium sized firms, accordingly. In contrast, as we detect no effects on taxes collected by larger firms such as CIT and VAT, we should see no increase in activity by these firms.

To test these conjectures, we draw on the establishment census of Rwanda (cf. Section 3) which contains information on firm location and establishment year. Figure A.6 maps out the total number of firms in each municipality of Rwanda in 2011 (before the road paving) and 2019 (when a substantial share of roads had been paved), respectively. Descriptively, it highlights how much of the firm creation was concentrated along the paved roads, which we use in our analysis. For each municipality we count how many new firms show up in the census each year. We then rerun Eq. (3) with the number of new firms in a municipality as the dependant variable.

Figure 3 shows the results. Panel (a) illustrates our estimated coefficients for the total number of new firms. While pre-trend coefficients are close to zero and insignificant throughout, we find an immediate increase after the road upgrading. The effect slows down three years after the road has been paved. Panel (b) and (c) confirm that these increases are concentrated among smaller firms and most pronounced for medium sized firms. As expected, for firms with at least 10 employees

[8] Due to infrequent census rounds we are unable to measure yearly firm exit or changes in total number of firms (see Section 3).

Figure 3: Road upgrading and firm entry by size



Notes: This figure plots the β_k coefficients estimated from Eq. (3). The panels plot firm entry for firms of different sizes as the dependent variable. Standard errors are clustered at the municipality level. Data Source: Own calculations based on Rwanda's Establishment Census.

we measure zero treatment effects throughout. Figure A.7 further shows that, in line with the results by size, it is mostly informal firms (i.e. not registered under any central tax) which enter – another rationalization of the zero results for central tax revenues. Additionally, we do not find substantial differences between firms owned by men or women.

5.3 Robustness

We assess the validity of our empirical estimates in multiple robustness checks, focusing on the concern that treatment may have occured endogenously. We discuss their outcomes in the following but relegate the presentation to the Online Appendix.

The first check is to restrict our control group to municipalities which are close a highway that has *not* been paved by the end of our study period (see Figure A.8 for their geographical distribution). This alleviates any concern that treatment and control group are substantially different because the former is close to a highway and the latter are not. We do find very similar results, qualitatively as well as

quantitatively, for both central and local tax revenues. In a complementary regression, we include the more prosperous regions of Rwanda (Nyarugenge and Kicukiro) which we have excluded from our main sample. We further add the logarithm of population by year (interpolated between 2011 and 2021) as well as municipalities' elevation levels interacted with time dummies as controls to Eq. (3). Finally, we exclude municipalities which have been treated more than once. All results are shown in Figure A.9 and exhibit qualitatively and quantitatively similar results to Figure 2. The same conclusions hold for our results on firm entry.

To further investigate whether structural differences – rather than the treatment itself – between treated and control municipalities are driving the results, we run a placebo test 'in-time'. Specifically, we randomly assign the treatment timing across treated municipalities and re-estimate Eq. (3). We repeat this exercise 200 times. Figure A.10 plots the resulting estimates and shows that less than 5% the the post-treatment coefficients are statistically significant, speaking in favor of the actual treatment driving our results.

5.4 Discussion

Having established a robust relationship between highway paving and economic as well as fiscal gains, we now turn to discuss potential mechanisms and implications.

Real vs. reporting effects. The positive impact on tax revenues at the local level could in principle be driven by two factors. One is an increase in real economic activity and land value appreciation. The other one is a mere increase in enforcement capacity, as tax inspectors might have better access to local businesses and landlords. Likely, both channels are relevant in our context. However, we argue that the former one is likely to be the main driver of the effects for several reasons. First, we consider the effects on trade license fees, i.e. taxes on local businesses. If the continuous increase in tax collection would stem from better enforcement without affecting the actual prevalence of firms, we would expect to also see a continued rise in yearly firm entrance, reflecting that the tax authority has improved on catching non-complying new firms. Instead, the effects on firm entry slows down on average after 3 years. This suggests that the initial rise in revenue came from increased real firm entry. These firms continue to pay taxes (hence, the steadily enhanced revenue) but at some point no more businesses are entering. Second, regarding rental taxes, information on tax obligations is often transmitted through informal channels such as village leaders. Hence, while roads increase land value and therefore rents, they

do not affect the information set of village leaders. Furthermore, the empirical estimates align well with the qualitative evidence on how rental rates have increased after the highways have been paved (see footnote 7).

Insights. Our econometric analyses offer several important insights. First, they show how local tax collection is significantly positively affected by the road upgrades. Second, this effect is largely driven by rental taxes, likely reflecting higher land values. Third, also trade-license fees are increasing strongly, suggesting that local governments can benefit from the many traders and small firms. This result sheds new light on a common perception which deems taxes on small firms as rather unimportant (Keen, 2012). Fourth, we measure a precise null-effect for central tax revenues, stemming from CIT, PIT or VAT.

It is important to keep in mind that the actual costs of road upgrading are borne by the central government, however. Our results therefore imply a fiscal shift to decentralized entities akin to what has been found for infrastructure projects in other contexts (cf. Baum-Snow (2007)). Finally, by the virtue of our data on local revenues, we highlight that only considering central taxation is insufficient to really understand the fiscal impact of transportation infrastructure in low-income settings.

6 Is road upgrading fiscally efficient?

Above, we show how the paving of highways has a strong effect on local fiscal capacity. This suggests that - to some extent - the investment financed itself. Based on our estimation results, we now make this argument quantifiable.

First, we calculate the absolute fiscal gains from highway paving. Second, we compare this gain with the fiscal cost to understand how much of the cost is regenerated. As there were no observable positive effects on tax revenues of the central government, which would include e.g. CIT or VAT, we take a conservative view and only consider the local tax collections for which we see significant estimates throughout the post-treatment period. The year before the treatment itself (k=-1) serves as the baseline period. That is, we evaluate our estimated effects at the average level for all treated municipalities in the year when the road was upgraded. For each post-treatment year (k=0,1,...,6) we multiply this baseline level with the coefficients to get the average cumulative effect on tax revenues. Finally, we multiply this figure by the number of treated municipalities.

We start by calculating the gain in rental tax revenues (panel (c) of Figure 2).

We observe 85 municipalities with rental tax revenues a year prior to its closest highway being paved (i.e. at k=-1). Among these municipalities in that year, the average rental tax collection was RWF 17.5 million. The point estimate for the first treatment year is 0.38, which, transformed into percentage changes, equals 0.47 ($= e^{0.38} - 1$). Therefore, we calculate the absolute increase in rental revenues in k=0 as $0.47 \times 85 \times 17.5$ million = 693.5 million. We continue for the other treatment year, in which the implied effects are $0.77, 0.74, 1.41, 1.17, 2.69, 6.11$. Taken together, we find an absolute increase in rental tax revenues of 19.8 billion in the first 6 years of the closest highway being paved.

Turning to trade license fees collection (panel (d) of Figure 2), we have 100 municipalities with average collections of RWF 11.2 million at the baseline. The implied effects for the respective years are $0.12, 0.19, 0.21, 0.22, 0.29, 0.49, 0.56$. Following the aggregation as before, we calculate a total increase of 2.35 billion. Taking together rental tax and trade license revenues, we arrive at a total increase of 22.2 billion. In a next step, we relate this gain to the actual costs of the highway paving. We retrieve the administrative records from the RTDA breaking down the costs of each road stretch we study. In total, the costs amount to RWF 725.24 bio. We therefore arrive at a fiscal benefit-cost ratio of approximately 3.1 %. Assuming the last estimated impact in period 6 to be persistent in the following years, the highway pavements would refinance themselves after 72 years.

While this ratio appears sobering at first sight, there are a number of caveats to keep in mind. First, the local fiscal gains we calculate clearly represent a lower bound on the broader fiscal gains from highway upgrading. Due to lack of data, we omit any effects on property taxes, which are in principal important revenues for local governments. Second, we dismiss all insignificant coefficients estimated for central government collection (see Figure A.4 in the Online Appendix). Finally, we stop summing the revenues after 6 years. The strictly positive trend suggests, however, that the long-term benefit may be much larger.

7 Conclusion

While most developing countries are in dire need of infrastructure reforms they often lack the financial resources to take action. In this paper, we provide direct evidence that infrastructure investments can increase fiscal capacity and therefore enhance public budgets. We leverage detailed data on the timing and locations of highway pavings in Rwanda to estimate their impact on tax collections. Our

results point to a lasting increase in tax revenues for municipalities close to upgraded highways. Importantly, these positive effects are concentrated on tax types which are collected and used by local governments. We show that infrastructure investments may not yield direct fiscal returns to the central government, they play a key role in stimulating local economic activity and enhancing municipal revenue capacity.

Our analyses focus on the first order effects of infrastructure investments. In general equilibrium, economic and fiscal gains may by far exceed these already substantive local gains, but are beyond the scope of this paper. Rather, we view our results as providing the first empirical lower bound to the effective reduction in infrastructure costs, when factoring in fiscal gains.

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Online Appendix to

"Infrastructure and Fiscal Capacity"

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Background & Data

This appendix complements the main paper by providing more descriptive statistics of the road as well as the administrative tax data.

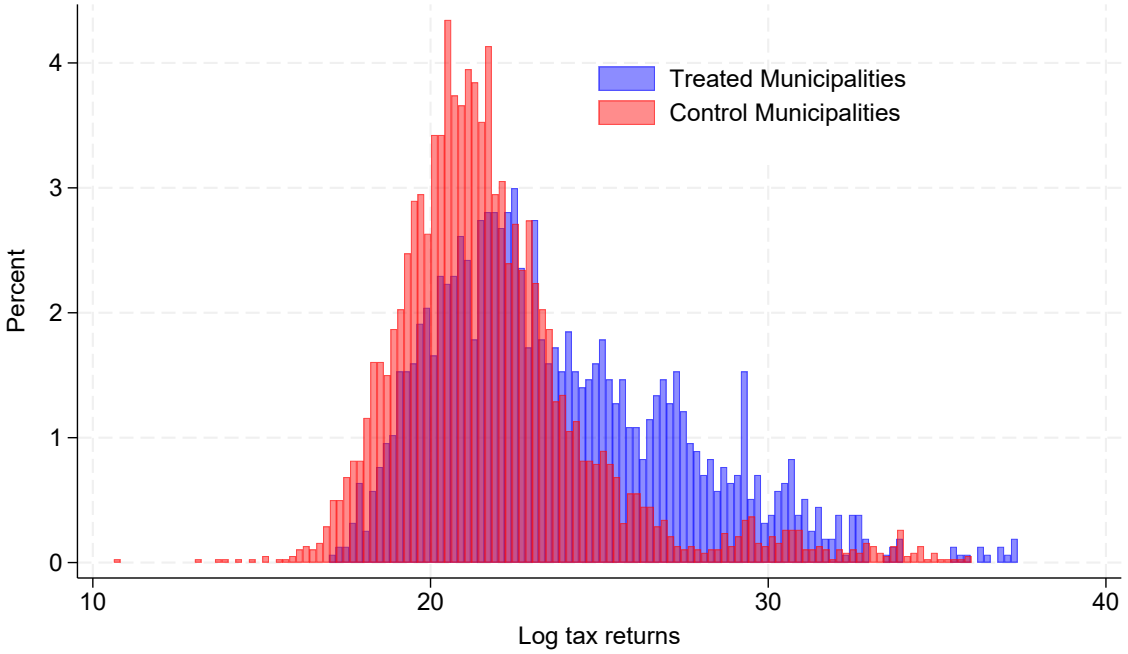
Figure A.1: Unpaved vs. paved road example

(a) Unpaved road

(b) Paved road

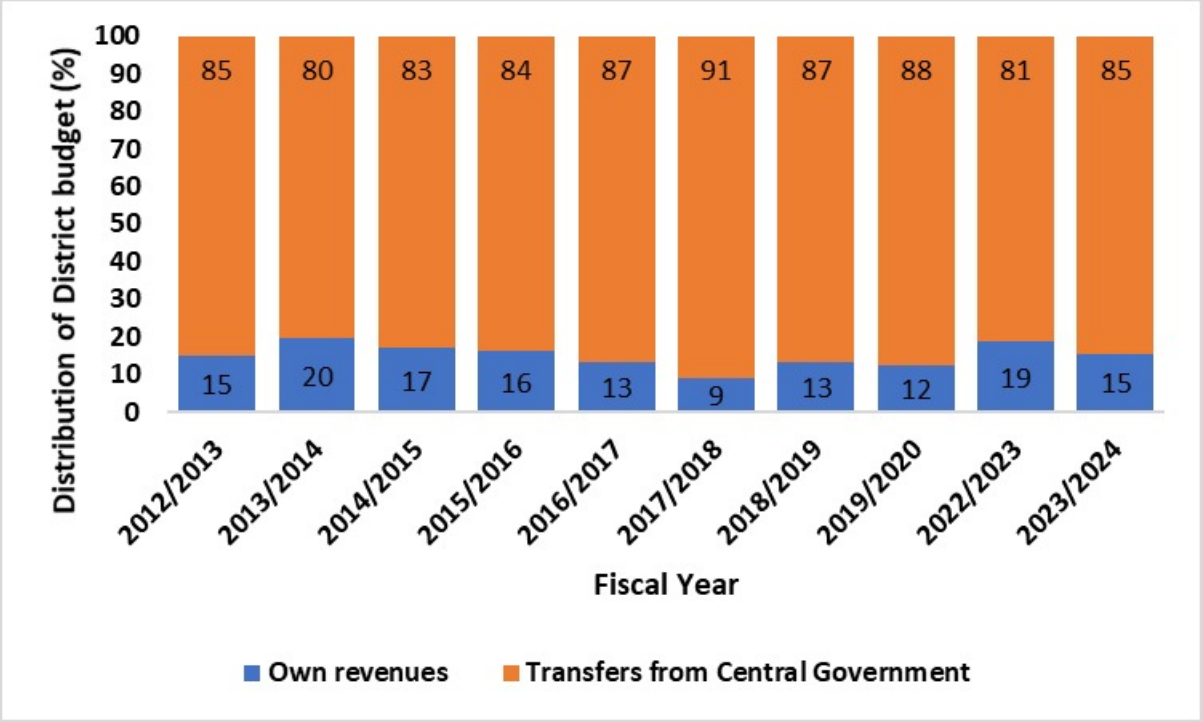
Notes: This figure shows examples of an unpaved road and a paved road in the Southern and Eastern Province of Rwanda. Source: authors' own photographs.

Figure A.2: Tax revenues from treated and control municipalities access



Notes: This figure share the distribution of the logarithms and density of the tax revenues across treated and untreated municipalities. Data Source: Administrative tax records from RRA.

Figure A.3: Trends in the sources of district budgets (2012-2024)

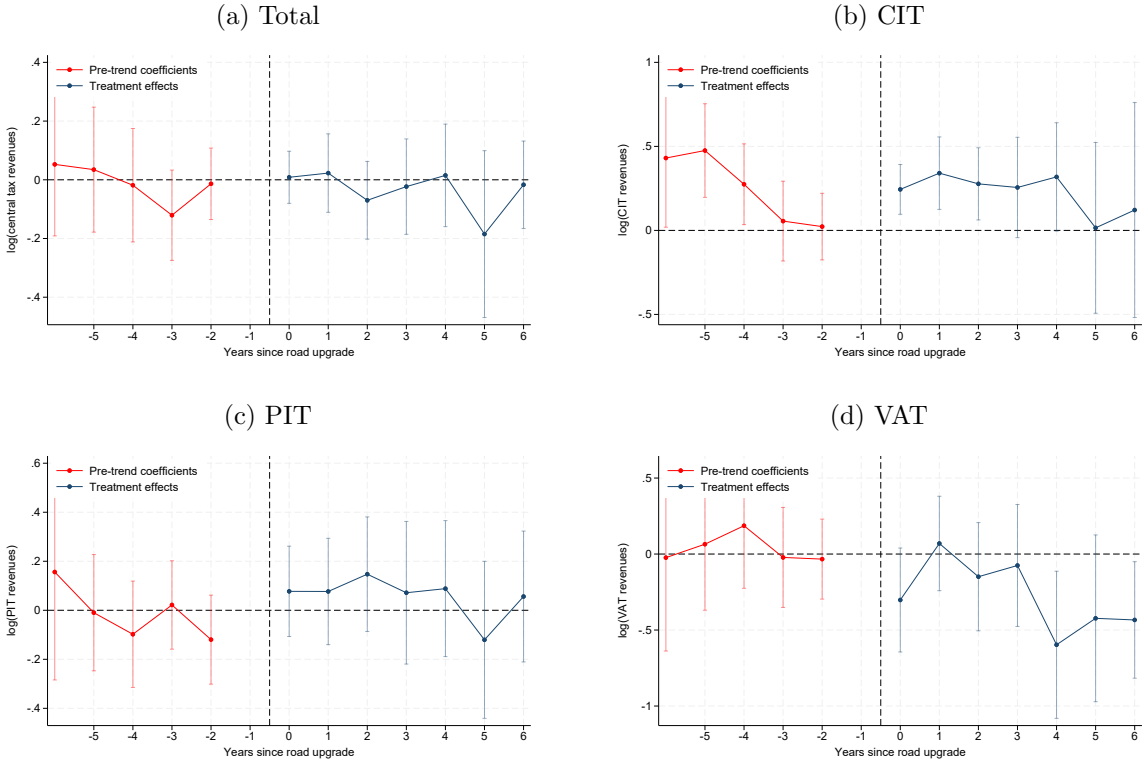


Note: This figure shows the structure of local districts public budgets. it shows how much is coming from the central revenues and how much comes from local taxes. Data Source: Administrative tax records from RRA.

Additional Results & Robustness

This section complements the main paper by providing complementary evidence and robustness checks.

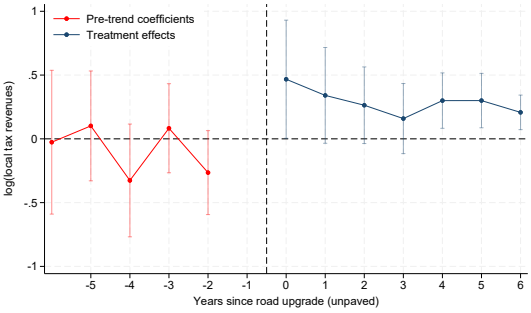
Figure A.4: Central tax collections



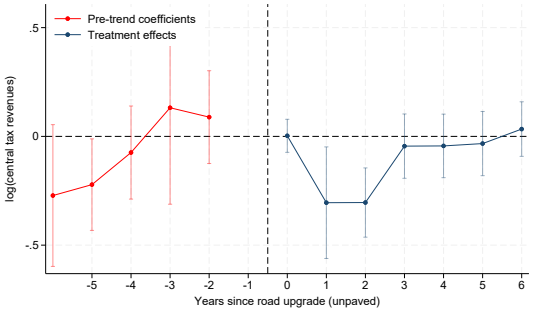
Notes: This figure plots the β_k coefficients estimated from Eq. (3). Standard errors are clustered at the municipality level. Data Source: Administrative tax records from RRA.

Figure A.5: Road upgrading without paving and tax collections

(a) Local

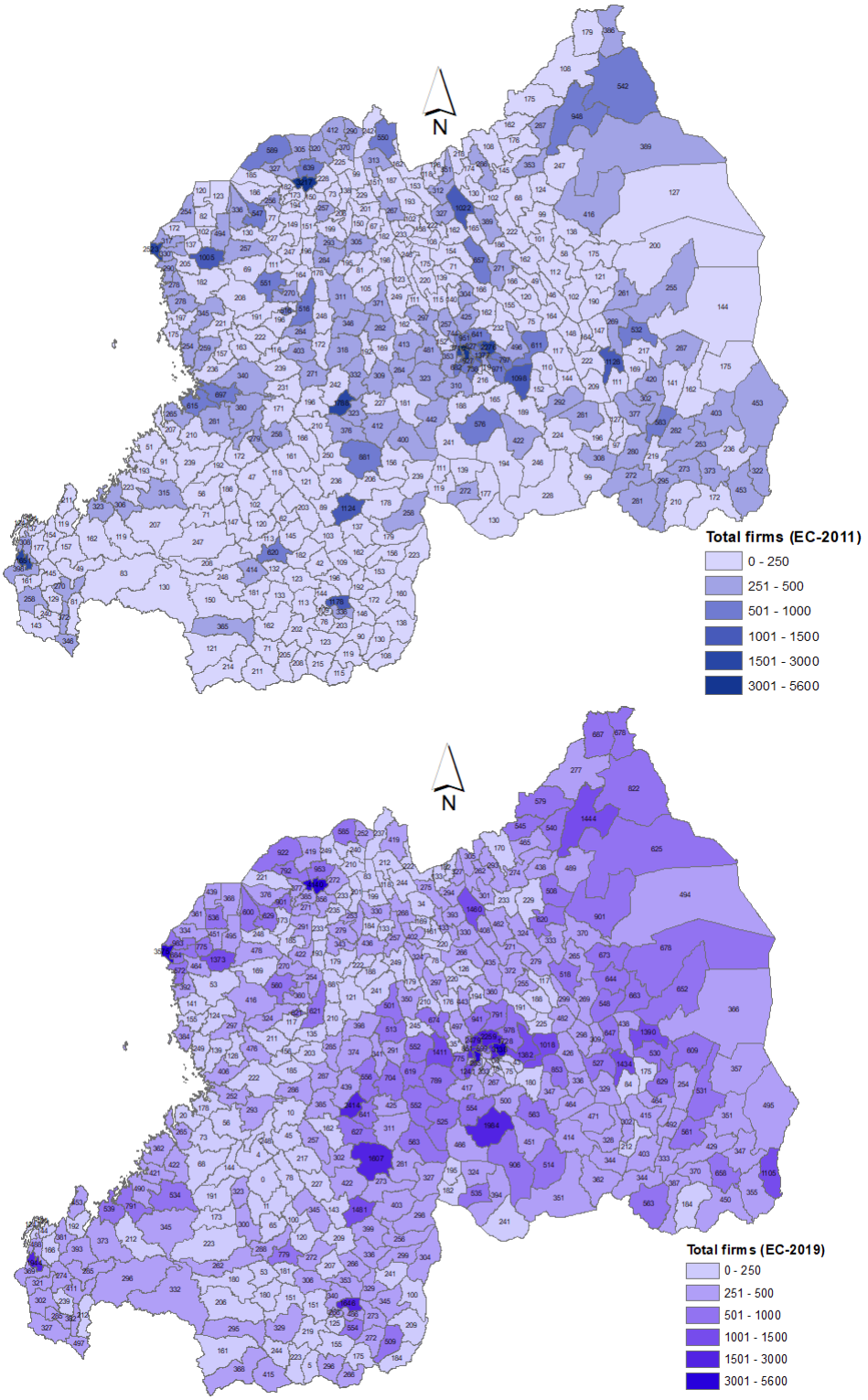


(b) Central



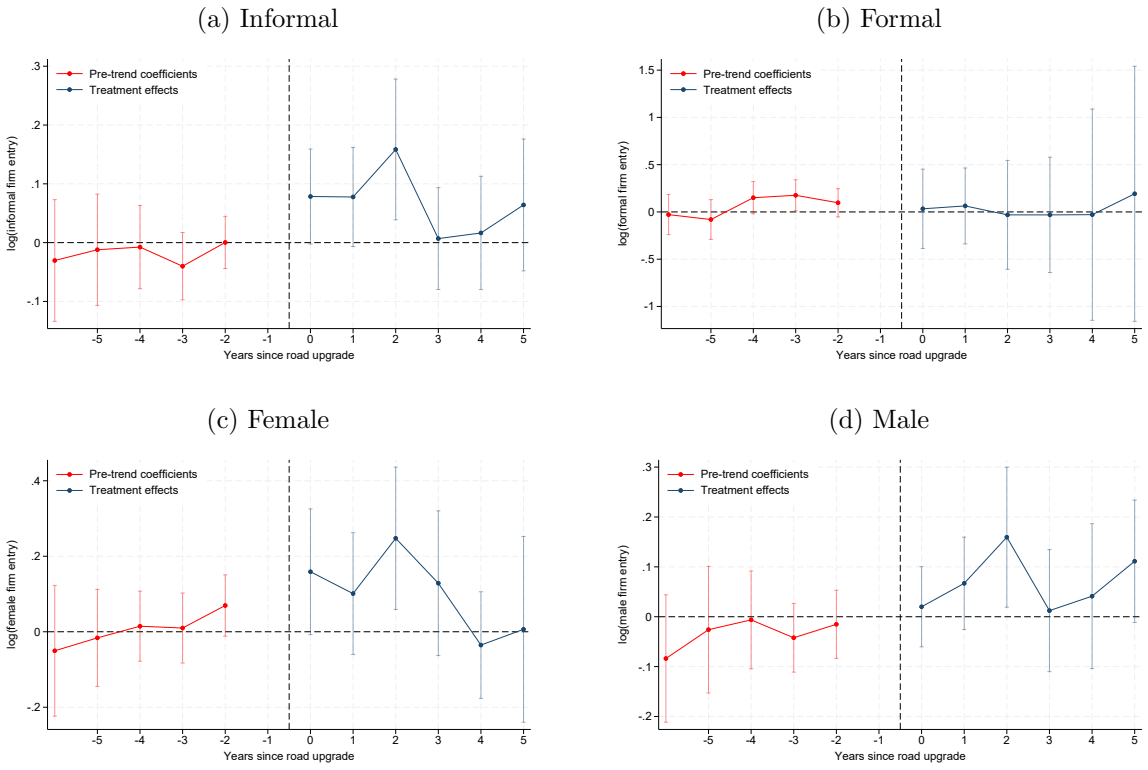
Notes: This figure plots the β_k coefficients estimated from Eq. (3) when upgrade (not paving) is the treatment. Standard errors are clustered at the municipality level. Data Source: Administrative tax records from RRA.

Figure A.6: Total number of firms across municipalities



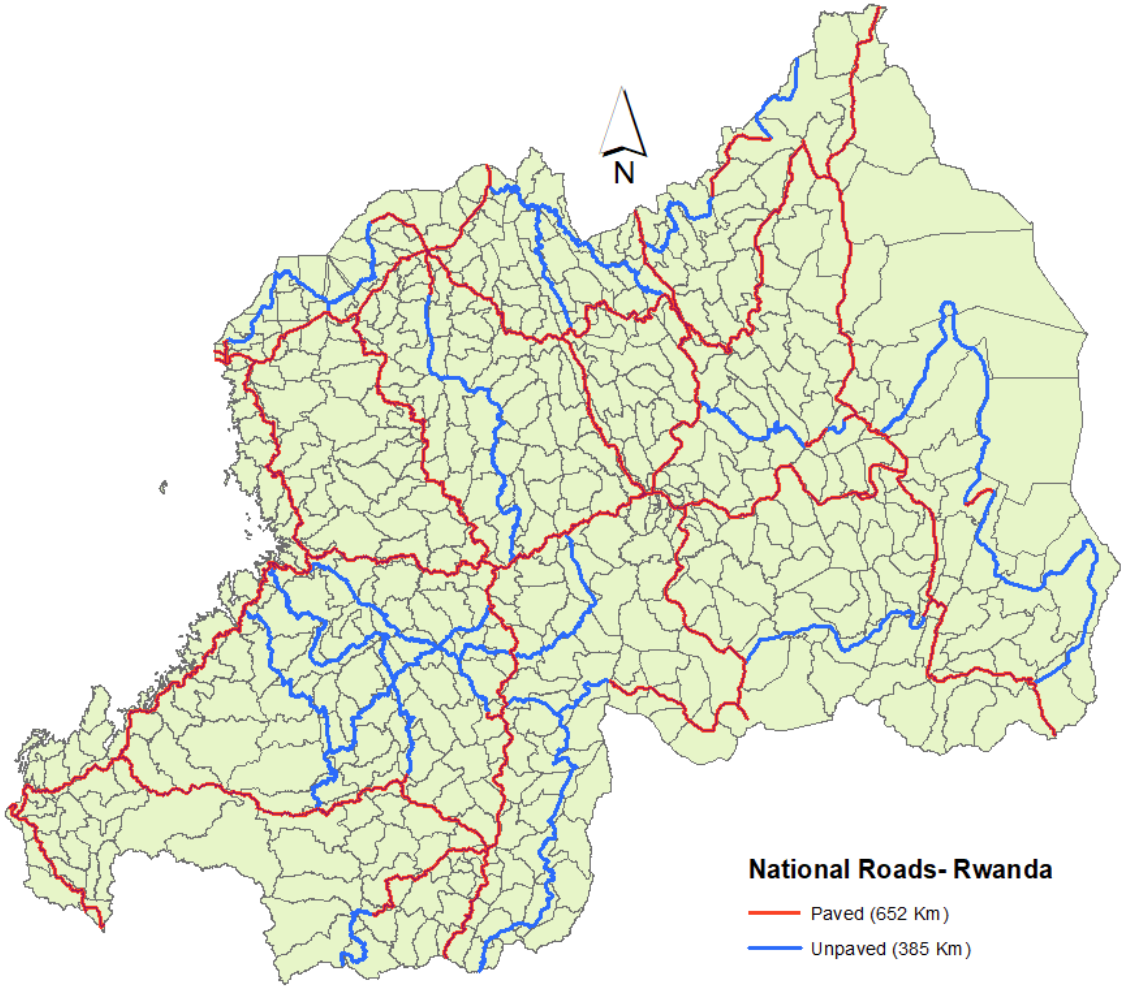
Notes: This figure examines plots the the geographic distribution of firms across Rwanda for 2011 (beginning of our study period) and 2019 (end of our study period). Data Source: Rwandan Establishment Census.

Figure A.7: Firm entry by status and gender



Notes: This figure plots the β_k coefficients estimated from Eq. (3). The panels plot firm entry for firms of different types as the dependent variable. Standard errors are clustered at the municipality level. Data Source: Own calculations based on Rwanda's Establishment Census.

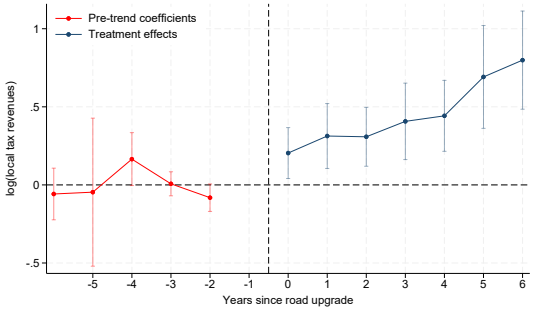
Figure A.8: National Upgraded and Non-upgraded roads



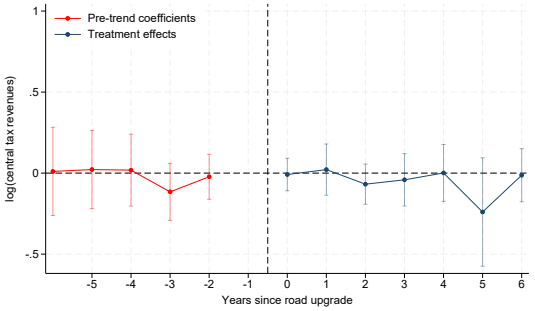
Notes: This figure indicates the distribution of upgraded and non-upgraded national roads in Rwanda. The light-blue color represents unpaved roads while the red color indicates paved roards. The figures shows that 2/3 of the national roads are paved. Data source: RTDA.

Figure A.9: Robustness of main results

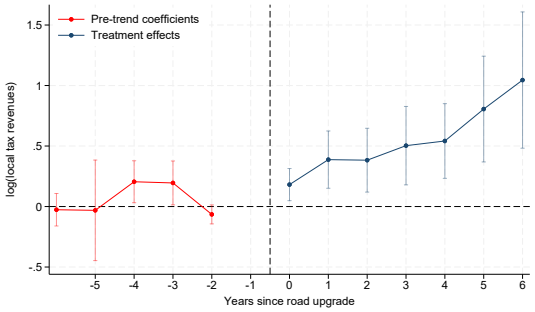
(a) Local (highway municipalities)



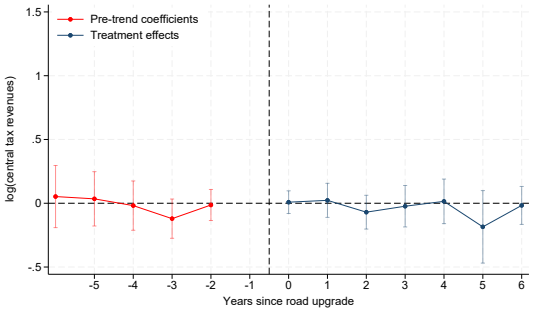
(b) Central (highway municipalities)



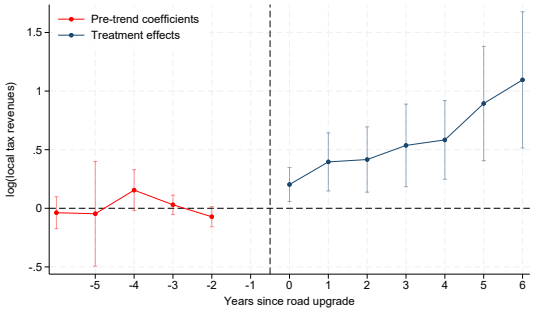
(c) Local (all municipalities)



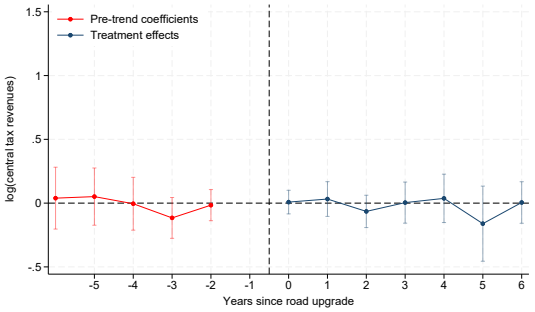
(d) Central (all municipalities)



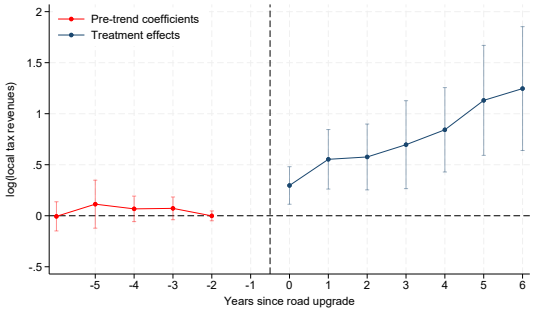
(e) Local (additional controls)



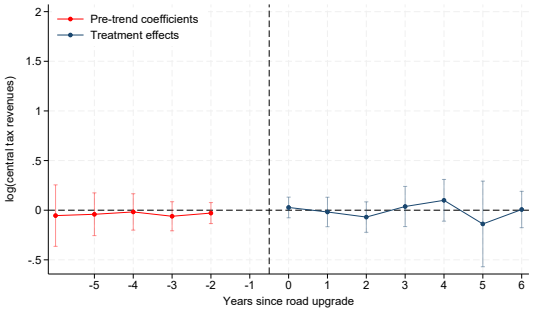
(f) Central (additional controls)



(g) Local (excluding multi-treated)

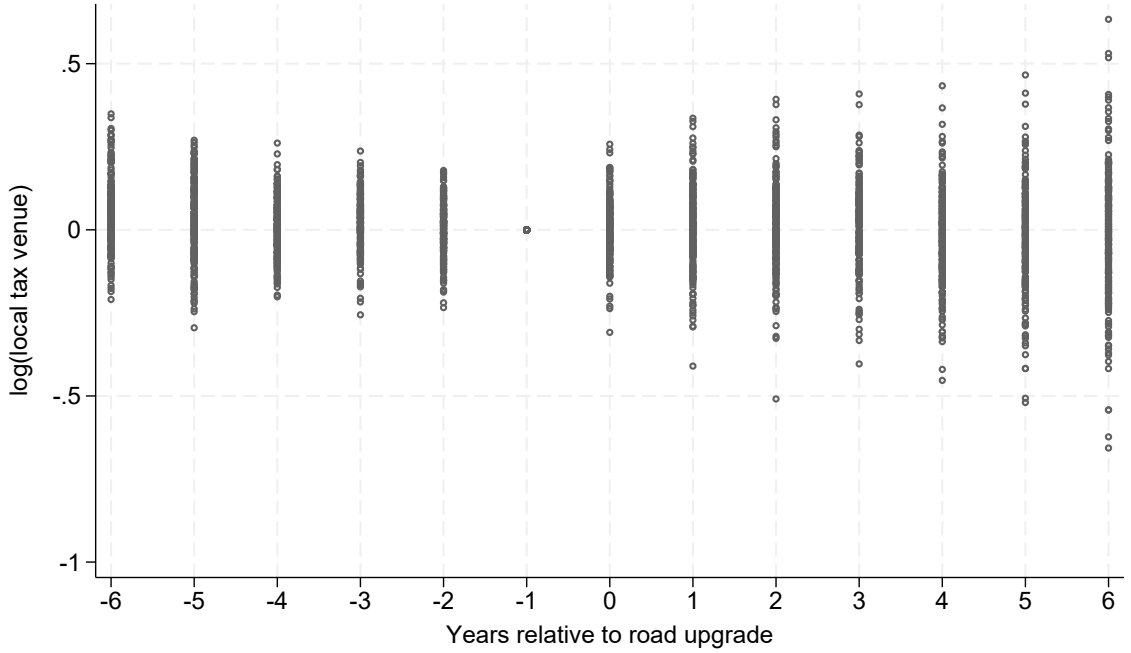


(h) Central (excluding multi-treated)



Notes: This figure plots the β_k coefficients estimated from Eq. (3). The first row takes only municipalities that have not been paved by the end of our study period as control observations. The second row includes all municipalities (including those in districts of Nyarugenge and Kicukiro). The third row adds elevation levels (interacted with time dummies) and the natural logarithm of municipal population as control variables. The fourth restricts the sample to municipalities which were only treated once. Standard errors are clustered at the municipality level. Data Source: Administrative tax records from RRA.

Figure A.10: Placebo: randomizing treatment timing



Notes: This figure plots the estimated coefficients from randomizing treatment timing across treated municipalities and repeatedly estimating Eq. (3) 200 times. The shares of positive and significant estimates per relative time (for $k = -6, -5, -4, -3, -2, 0, 1, 2, 3, 4, 5, 6$) are $0.095, 0.06, 0.01, 0.045, 0.04, 0.035, 0.055, 0.06, 0.045, 0.03, 0.025$ and 0.06, respectively. Data Source: Administrative tax records from RRA.