The (fiscal) Dividend of Infrastructure: Roads and Revenues in Rwanda *

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Abstract

This paper shows that infrastructure investments enhance local tax outcomes. 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 large and significant increases in tax revenues in municipalities near to upgraded roads. These effects are driven by firm entry as well as land value appreciation, captured through taxes on rental income at the local level. Finally, we show that while the additional revenues do not fully recover the central government's initial investment, local municipalities' revenues more than double within five years.

JEL classification: H20, H71, O18, R42

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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 what 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 linking the exact location and timing of all road construction works in Rwanda to a decade of census information and administrative tax data at granular geographic levels. The empirical identification relies on the staggered upgrading of existing highways, alleviating endogeneity concerns. 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). Consistent with these fiscal effects, we estimate an immediate increase in firm entry among small and medium-sized firms, with no significant changes for large formal firms or differences by entrepreneur gender.

[1] There has been an extensive literature linking infrastructure, such as roads, to economic progress across many dimensions (e.g., Fogel, 1964; Fernald, 1999; Storeygard, 2016; Donaldson, 2018; Allen and Arkolakis, 2022), with most evidence from middle and high income countries.

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 highway 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 their construction or their upgrade. 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 and ultimately add administrative data on the universe of tax declarations. We can differentiate between 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 and fiscal outcomes. We observe that upon the paving of highways, locally collected tax revenues increase strongly and significantly. The point estimates increase gradually over time and suggest that local governments' revenues more than doubled after 5 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. Importantly, tax rates remained largely constant throughout our study period, suggesting a broadening of local tax bases.

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 on larger formal firms. 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. Also, we argue that the type of road upgrade matters. When redefining our treatment to upgrades which leave roads unpaved, such as simply improving drainage or renewing the soil surface, we measure no significant impact on either local or central tax revenues.

Finally, we compare the estimated gains in revenue to the program's costs. A back-of-the-envelope calculation reveals that the increases in local taxes alone could recover at least 3.1% of the initial investments directly. An important caveat to this figure is that it does not account for the potentially larger long-run economic and fiscal gains which may evolve from better connectivity in general equilibrium. We therefore interpret our result as a lower bound to the effective direct reduction in infrastructure costs when factoring in fiscal gains.

This paper contributes to several strands of the literature. First, we contribute to and build on 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). Large infrastructure programs in India, such as highway expansions (Datta, 2012; Ghani et al., 2016; Asturias et al., 2019; Das et al., 2024; Chatterjee et al., 2025) as well as the colonial railroad (Donaldson, 2018), have been found to have broadly positive economic effects. 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 im-

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

pact 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 (Hendren and Sprung-Keyser, 2020; Ryan, 2021; Mountjoy, 2026). Examples in developing countries include Faber (2014) and Allen et al. (2024), showing how transportation infrastructure improves market efficiency in China and Colombia, respectively. In contrast, our study makes a more direct point with important implications for policy evaluation (Hendren and Sprung-Keyser, 2020). We provide a first empirical lower bound on an effective decrease in the net costs for governments, which unambiguously increases the Marginal Value of Public Funds of infrastructure investments.

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 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, focusing on its road development policy and the tax system.

Road development policy. Periods of political instability left Rwanda's road infrastructure in poor shape and despite early efforts, only about a third of roads were in "good condition" by 2011 (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 (RTDA), specifically tasked with implementing and overseeing 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 governments 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.

Tax collection was decentralized in 2005 to strengthen municipal fiscal autonomy and enhance local service delivery. 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 finance 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 Data

We leverage a unique combination of administrative tax records, detailed road network data, and comprehensive establishment censuses which we describe in the following.

[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.

3.1 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 length in kilometers within each municipality, and the year of any upgrade completion. This temporal and spatial granularity allows us to identify effects of road improvements on municipality outcomes at the yearly level. 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. Figure 1 shows the distribution of the highway network across municipalities.

3.2 Administrative Tax Records

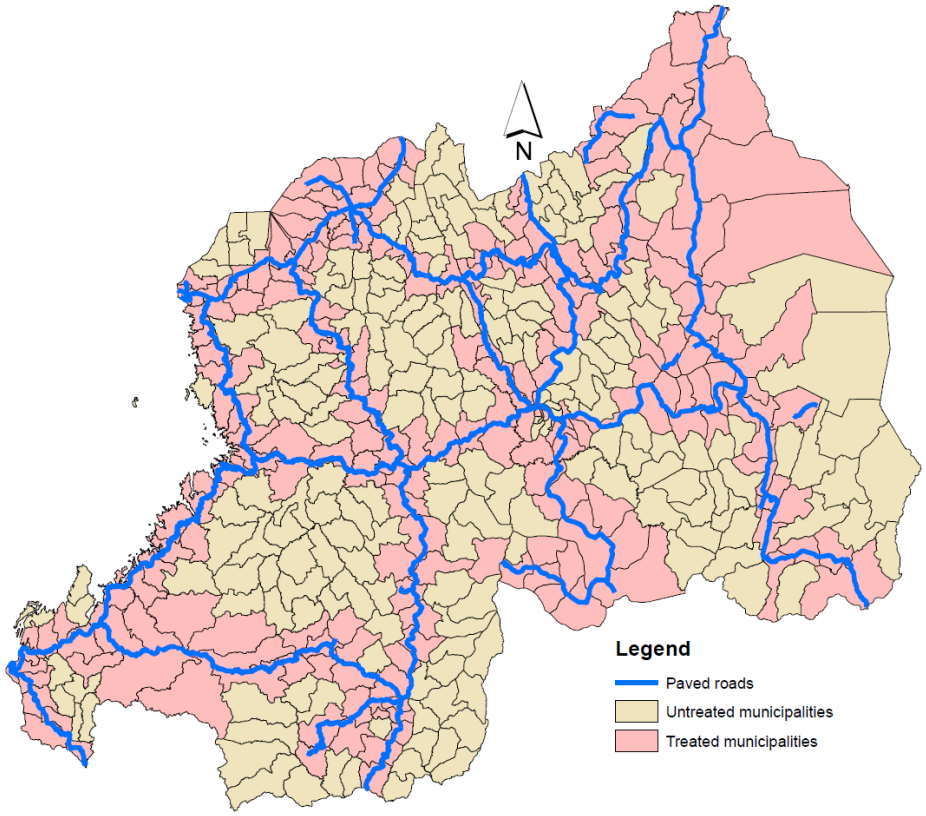
We use tax data which has been shared by the Rwanda Revenue Authority (RRA) and comprises micro-level information on the universe of tax declarations and fees from both the central as well as local municipal governments from the years 2012-2024. Figure A.2 shows that the distribution of total tax revenue 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 individual level, respectively, allowing for a detailed analysis of individual contributions to central government revenue.

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]

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

Figure 1: National roads and treated municipalities in Rwanda



Notes: This figure shows the distribution of Rwanda's municipalities and paved highways by 2023. The rose color represents treated municipalities within 2km of paved highways while tan indicates untreated municipalities. Data Source: RTDA.

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 formal as

well as informal businesses operating in the country, which is particularly important in our context. While the finest geographical units in the publicly available census data are districts, by courtesy of the NISR, we were able to retrieve information at the municipality level—our geographical unit of interest.

The infrequent census waves prevent a yearly analysis of municipal firm counts. However, using information on the founding years of firms, we can infer yearly firm creation dynamics. Naturally, this provides a lower bound to firm creation as we only capture firms which survive until the next respective census round, e.g., a firm which was founded in 2012 but was dissolved in 2013 will show up neither in the 2011 nor in the 2014 census and hence, will not be part of the constructed panel. The resulting firm entry panel runs from 2008 to 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 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 highlighted in rose 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 any mechanical effects 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

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

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, $\mathbf{1}[i = \text{treated}]$

[5] We show below that excluding multiple treated municipalities does not change our results.

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 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. (1) 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. (1).

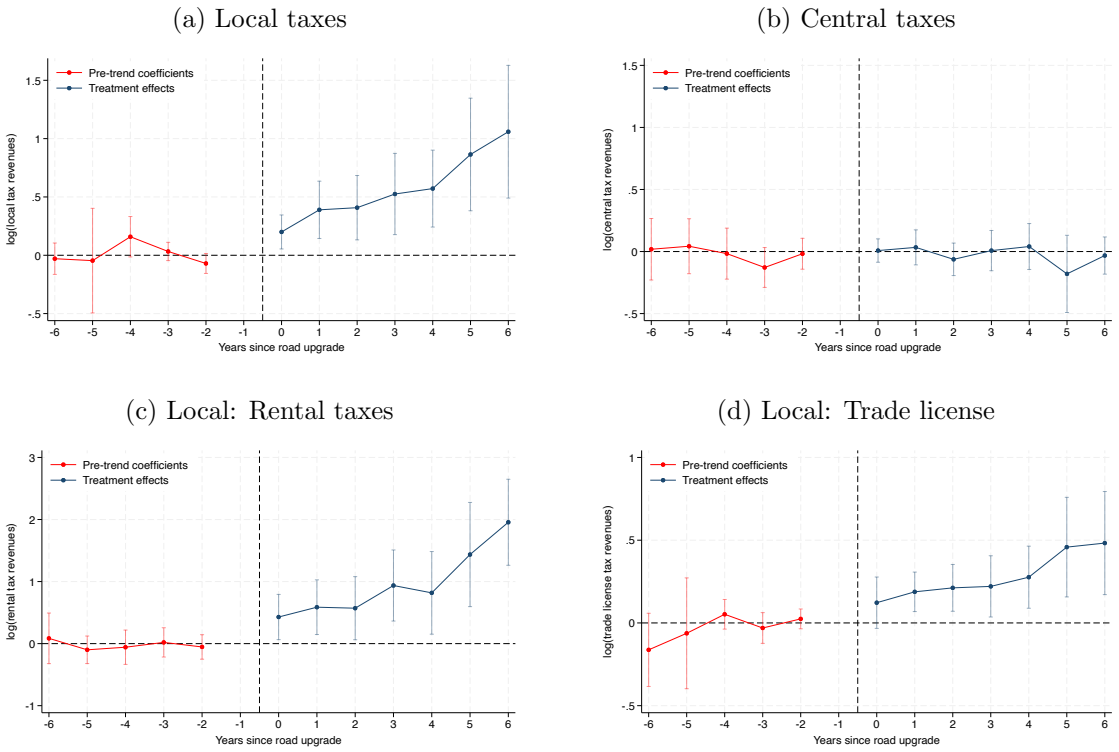
5 Results

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. (1). 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.

Figure 2: Road upgrading and tax collection



Notes: This figure plots the β_k coefficients estimated from Eq. (1). 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. Years: 2012-2024.

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 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 increase by 185%. 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 provides suggestive evidence that CIT collections increased slightly 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 outcomes, 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 (c) 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 measure it, this result also suggests that property tax collections, a major

[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."

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 55% larger than in the year before the upgrading.

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 enhancing tax collections.

It is important to note that throughout our study period, tax rates for both rental taxes as well as trade license fees remained constant. In fact, changes to these were made in 2011 and 2023, but not in between.[8] Thus, our results reflect a broadening of the tax base instead of adjustments in tax rates.

5.2 Road upgrading and local economic activity

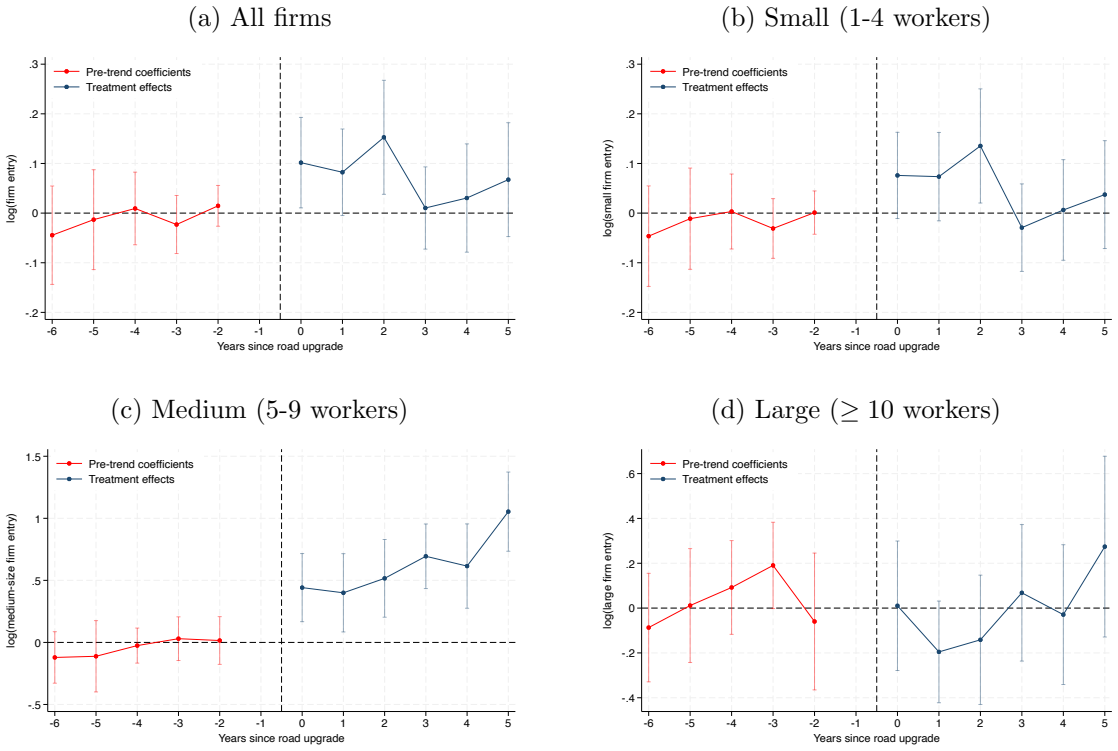
While the previous analyses focus on the pure fiscal effects, transportation infrastructure may have positive spillovers along many other dimensions. In the following, we focus on firm entry as a key indicator of economic activity (Bartelsman et al., 2013). Our results thus far suggest that we should expect an increase in small and medium-sized firms given the rise in trade license fees, but no increase in activity by larger and formal firms given the null effects on CIT and VAT.

To test these conjectures, we draw on the establishment census of Rwandan Establishment Census (cf. Section 3) which contains information on firm location and establishment year. Figure A.6 shows that throughout our study period the number of Rwandan firms grew substantially. For each municipality we count how many new firms are being established each year and rerun Eq. (1) with the number of new firms as the dependent variable.

Figure 3 shows the results. Panel (a) illustrates our estimated coefficients for

[8]For details, we refer to Laws 59/2011 and 048/2023 in the Rwanda Income Tax Law.

Figure 3: Road upgrading and firm entry by size



Notes: This figure plots the β_k coefficients estimated from Eq. (1). 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. Years: 2008-2019

the total number of new firms, showing flat pre-trends and an immediate increase after road upgrading that slows down three years later. Panels (b) and (c) confirm that these increases are concentrated among smaller firms and most pronounced for medium-sized firms. As expected, we measure zero treatment effects throughout for firms with at least 10 employees. Figure A.7 further shows 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 occurred endogenously. We discuss their outcomes in the following but relegate the presentation to the Appendix.

The first check is to restrict our control group to municipalities which are close to 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. (1). 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. (1). We repeat this exercise 200 times. Figure A.10 plots the resulting estimates and shows that less than 5% of 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, if increased tax collection stemmed solely from better enforcement, we would expect continued rises in firm entry as authorities catch more non-compliant firms. Instead, firm entry effects plateau after 3 years while revenues continue rising, suggesting real economic activity drives the results. 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 rents 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 revenue level for all treated municipalities just before 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 their 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 years, 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 fee 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 billion. 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 principle important revenues for local governments. Second, we dismiss all insignificant coefficients estimated for central government collection (see Figure A.4). 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 improve tax outcomes 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 while 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

"The (fiscal) Dividend of Infrastructure: Roads and Revenues in Rwanda"

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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.

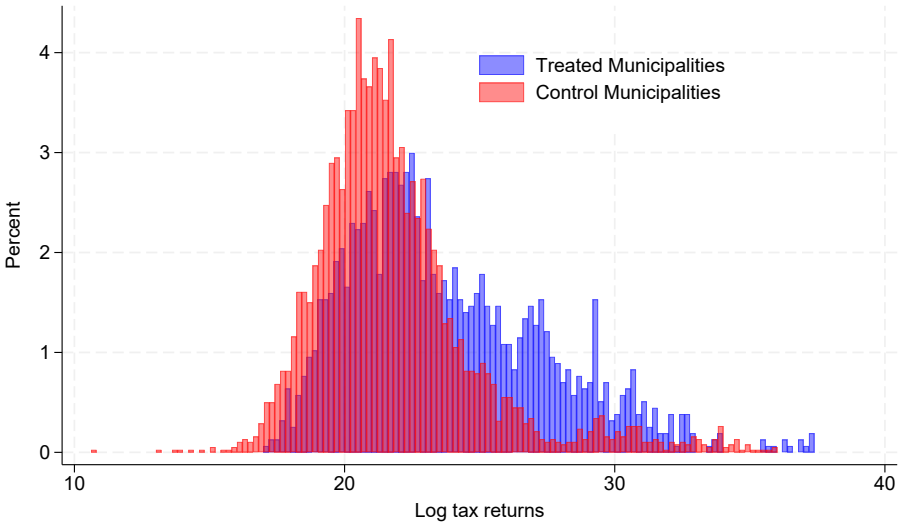
Figure A.1: Unpaved vs. paved road example

(a) Unpaved road

(b) Paved road

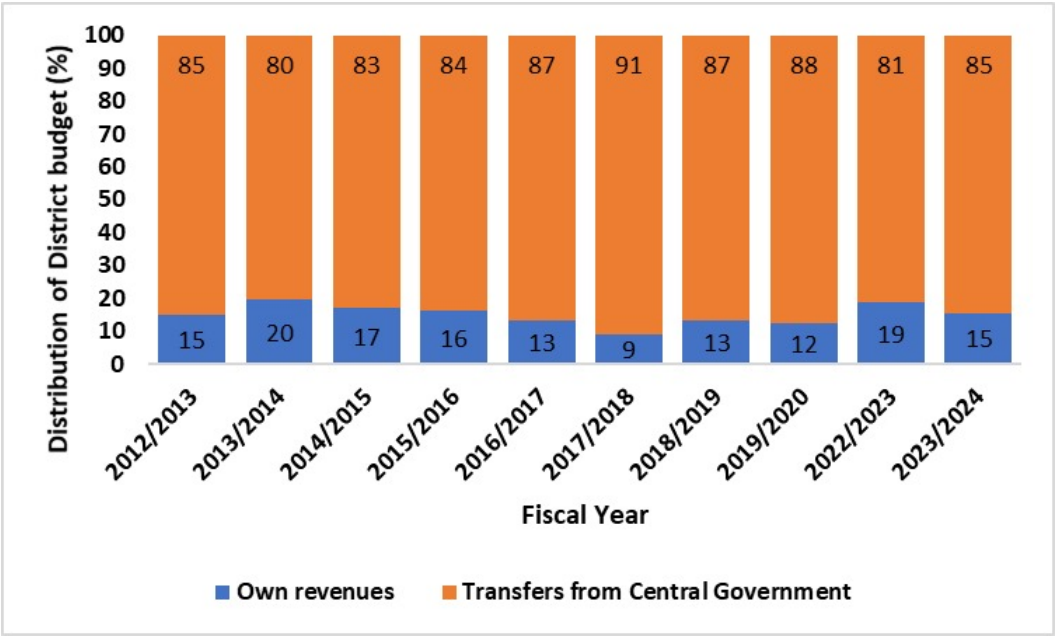
Notes: This figure shows examples of an unpaved highway and a paved highway in the Southern and Eastern Province of Rwanda. Source: authors' own photographs.

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)



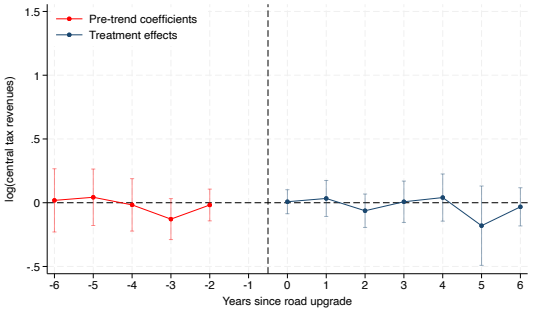
Note: This figure shows the structure of local districts public budgets. it shows how much is coming from the central revenues and how much comes from local taxes. Data Source: Administrative tax records from RRA.

Additional Results & Robustness

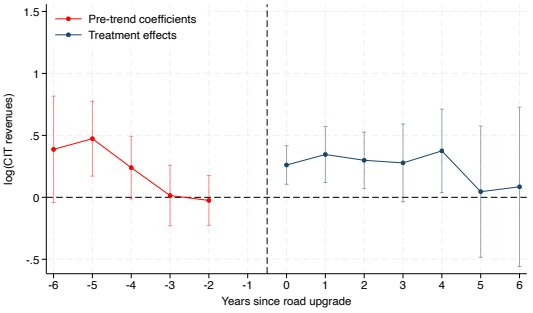
This section complements the main paper by providing complementary evidence and robustness checks.

Figure A.4: Central tax collections by tax type

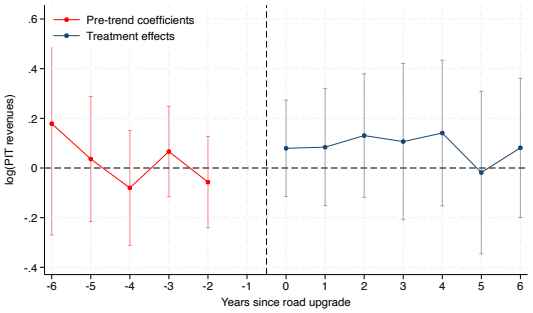
(a) Total



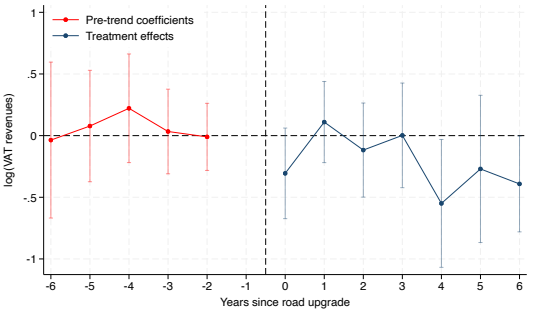
(b) CIT



(c) PIT



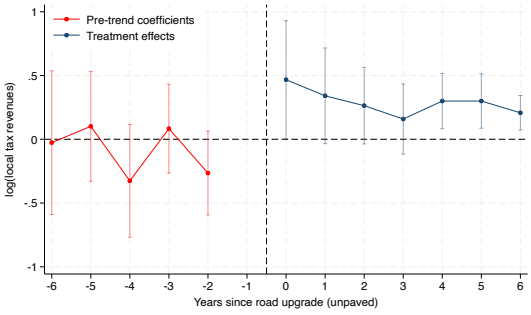
(d) VAT



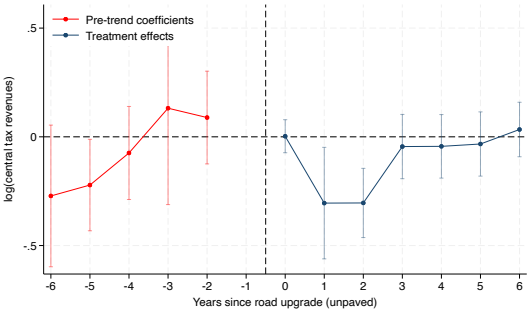
Notes: This figure plots the β_k coefficients estimated from Eq. (1). Standard errors are clustered at the municipality level. Data Source: Administrative tax records from RRA. Years: 2012-2024.

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

(a) Local

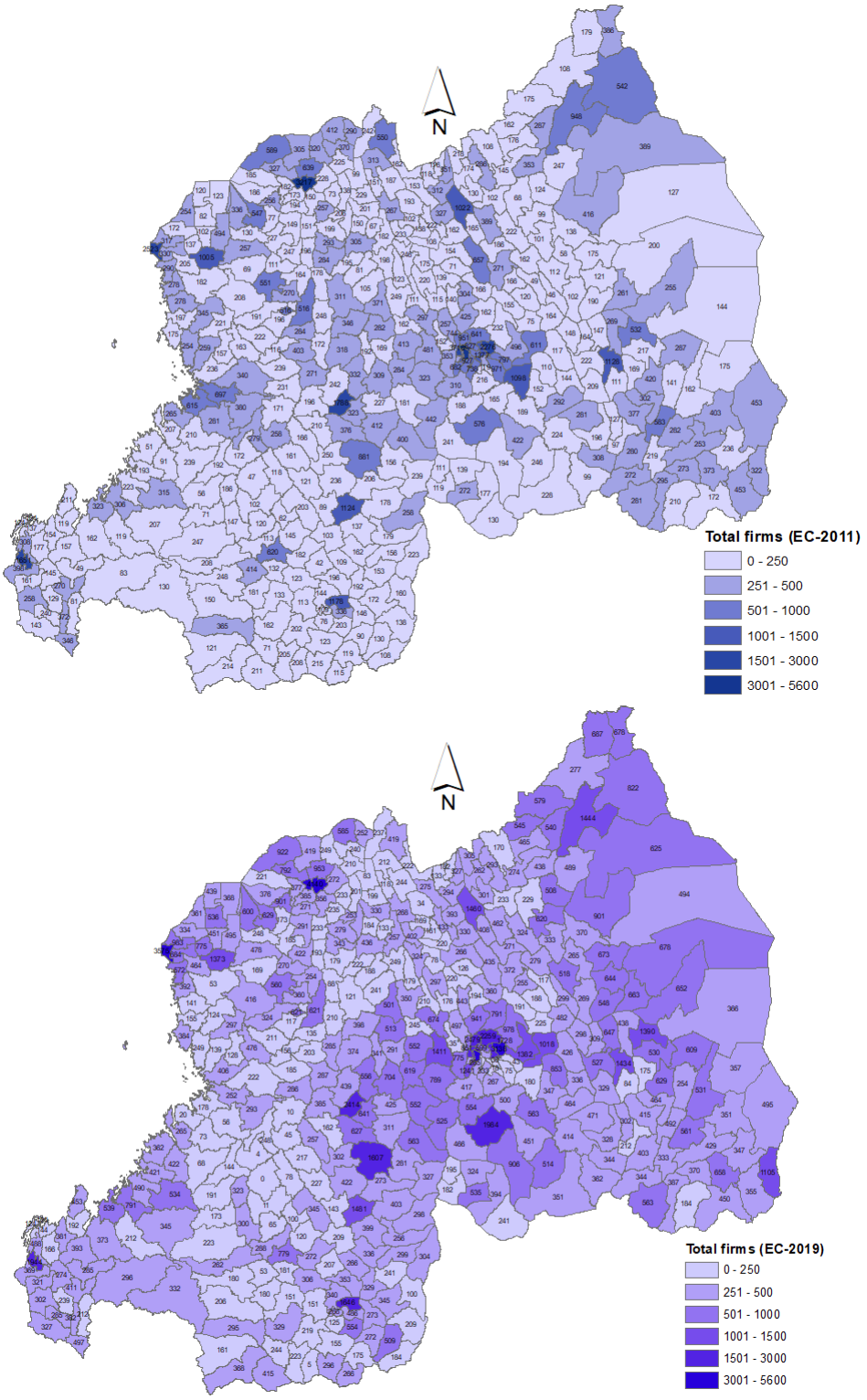


(b) Central



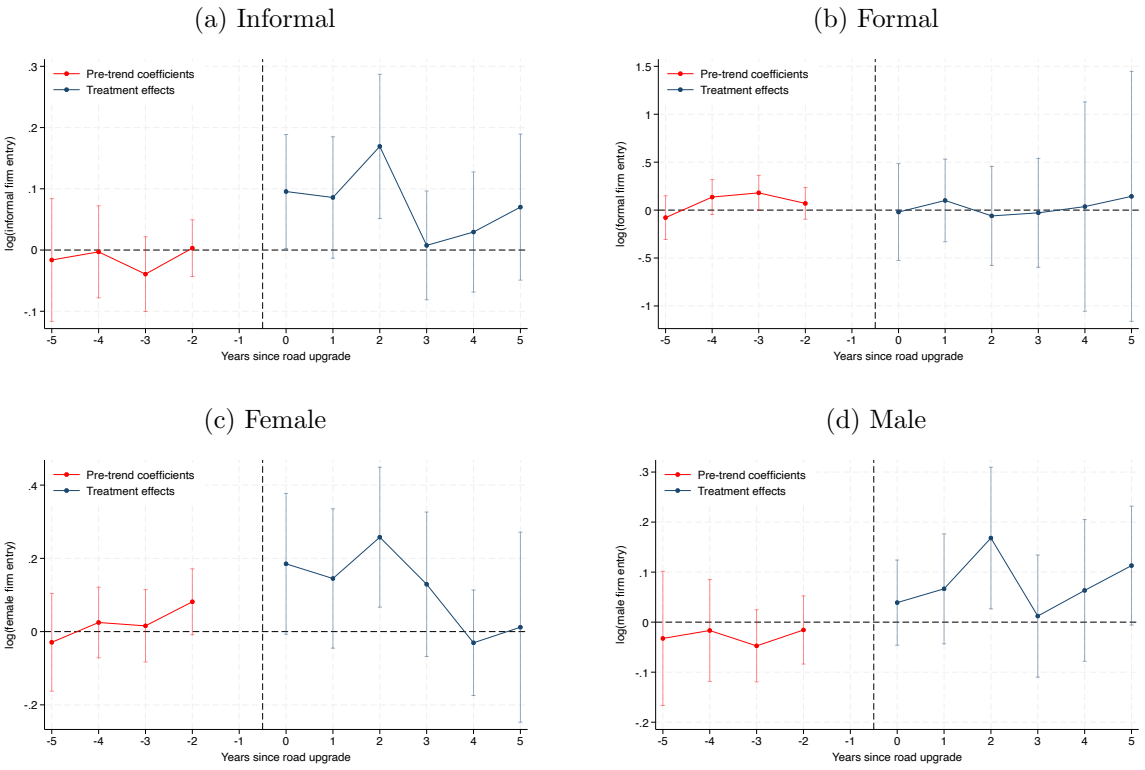
Notes: This figure plots the β_k coefficients estimated from Eq. (1) when upgrade (not paving) is the treatment. Standard errors are clustered at the municipality level. Data Source: Administrative tax records from RRA. Years: 2012-2024.

Figure A.6: Total number of firms across municipalities



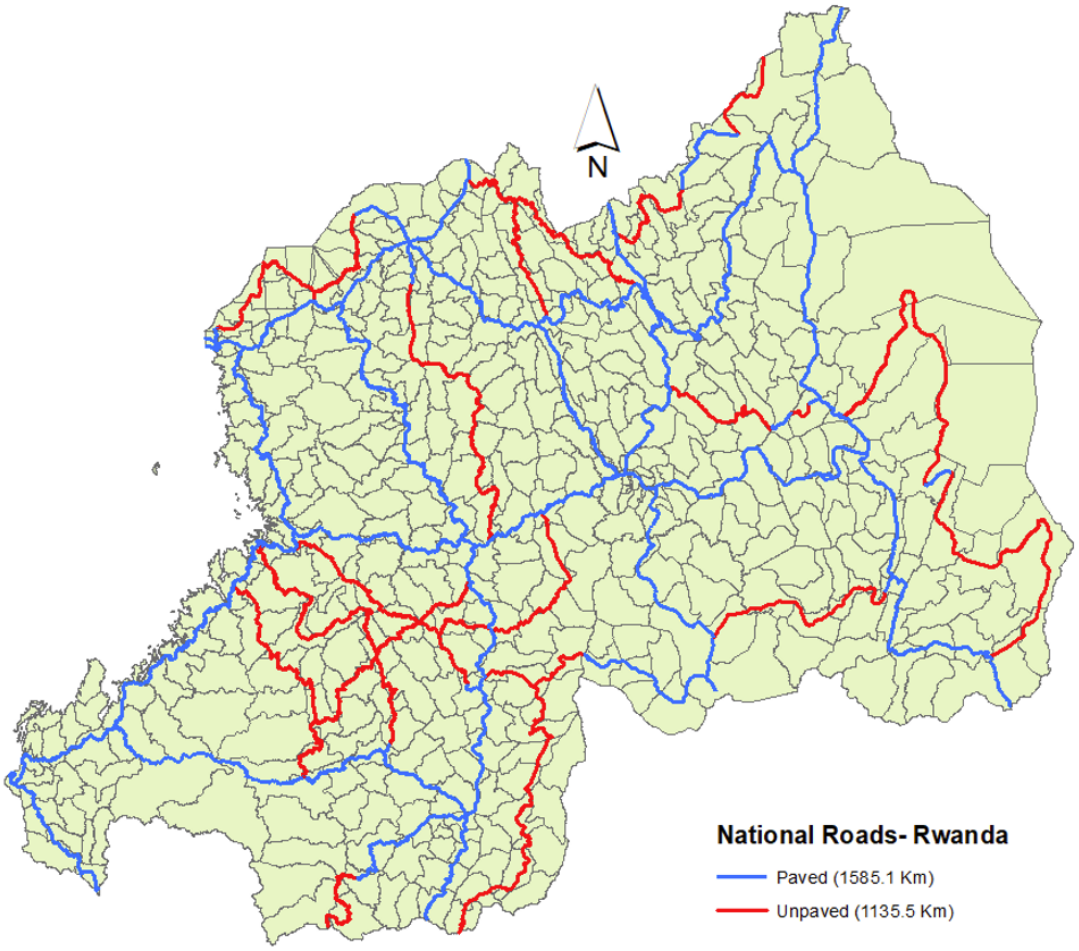
Notes: This figure plots the the geographic distribution of firms across Rwanda for 2011 and 2019. Data Source: Rwandan Establishment Census.

Figure A.7: Firm entry by status and gender



Notes: This figure plots the β_k coefficients estimated from Eq. (1). 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. Years: 2008-2019.

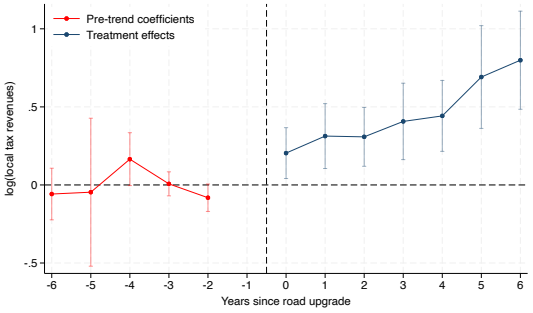
Figure A.8: National Upgraded and Non-upgraded roads



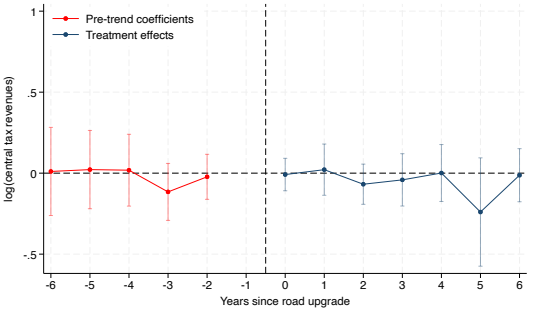
Notes: This figure presents the spatial distribution of paved and unpaved national roads in Rwanda. Light blue denotes paved roads, while red indicates unpaved roads. Source: Rwanda Transport Development Agency (RTDA).

Figure A.9: Robustness of main results

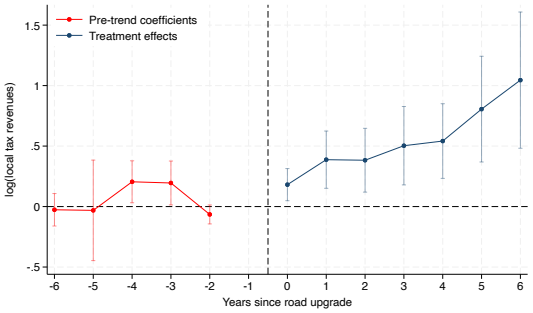
(a) Local (highway municipalities)



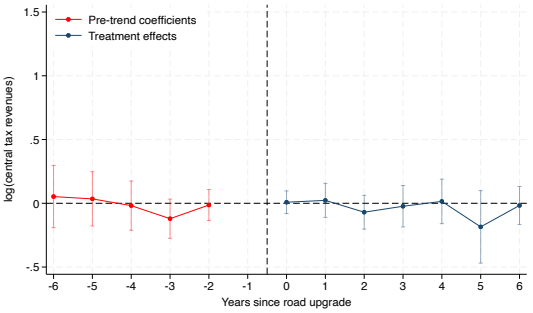
(b) Central (highway municipalities)



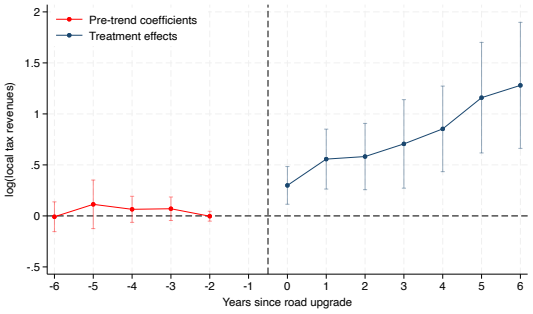
(c) Local (all municipalities)



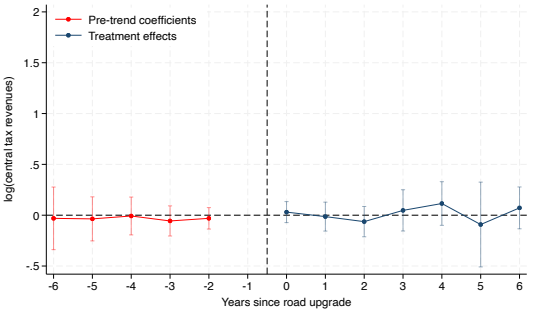
(d) Central (all municipalities)



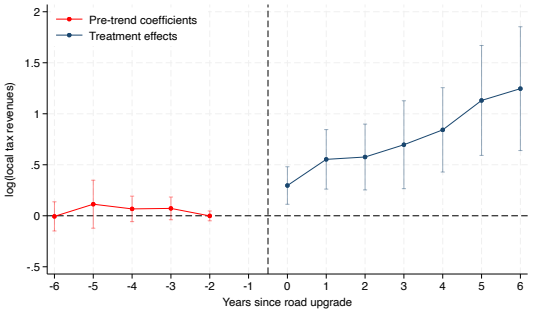
(e) Local (additional controls)



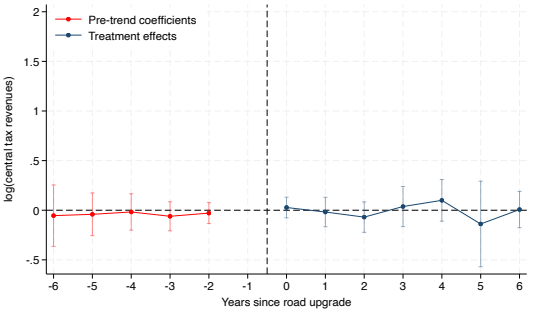
(f) Central (additional controls)



(g) Local (excluding multi-treated)

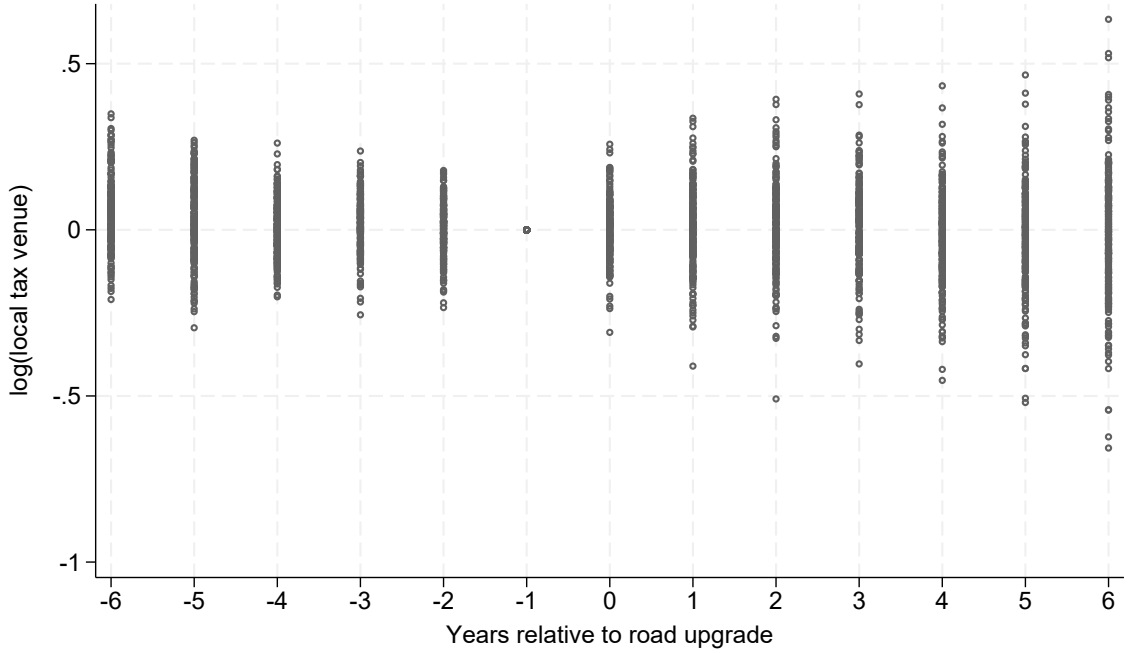


(h) Central (excluding multi-treated)



Notes: This figure plots the β_k coefficients estimated from Eq. (1). 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. Years: 2012-2024

Figure A.10: Placebo: randomizing treatment timing



Notes: This figure plots the estimated β_k coefficients from randomizing treatment timing across treated municipalities and repeatedly estimating Eq. (1) 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. Years: 2012-2024.