Stop That Train, I'm Starving

Impact of Closing Rural Rail Stations

Mohamed Coulibaly*

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Abstract

I exploit Côte d'Ivoire's 2011 post-presidential-election crisis as a natural experiment to document the effects of closing rural rail stations. Using nationally representative household microdata, I find sizable declines in per-capita spending and consumption in rural southern areas where stations were closed relative to adjacent areas that were never served by rail. These declines coincide with a reallocation of workers from higher-paying nonfarm sectors into lower-productivity family work. I use a Roy model with unobservable heterogeneous returns and sectoral mobility costs to rationalize these findings and recover marginal nonfarm returns. I show that the welfare gains from reopening rural rail stations exceed operating costs, though net gains diminish as coverage expands. Spatially targeted reopenings of a subset of the closed stations generate nearly the same welfare gains as full network restoration, implying a benefit–cost ratio almost twice as high.

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*Department of Applied Economics, HEC Montréal. Email: mohamed.coulibaly@hec.ca. I express my gratitude to Deico Coviello, Fabian Lange, and Ismael Mourifié for their guidance and continued support. I am also thankful to Jean-François Gauthier, Matteo Bobba, and Gilles Duranton for their insightful suggestions. I thank Souleymane Soumahoro, Sidi Mohamed Sawadogo, Fansa Koné, and N'dri Guillaume for their helpful comments. I further thank participants at the 64^{e} Congrès annuel de la Société canadienne de science économique (SCSE), the HEC Montréal Brown Bag Seminars, the Quebec Social Sciences Ph.D. Presentation Group, the McGill Applied Micro Breakfast, the CIREQ Weekly Ph.D. Student Research Workshop, and the 2024 Africa Meeting of the Econometric Society for their valuable feedback.

1 Introduction

Transport infrastructure is a cornerstone of economic development.[1] By reducing transport costs and connecting isolated areas to markets, transport networks shape how people and goods move and, ultimately, how economies prosper. In low- and middle-income countries, these networks can accelerate structural transformation by enabling workers to shift from low-productivity agriculture to more productive nonagricultural activities.[2] Yet despite this central role,[3] budget stress and declining public investment have led to the dismantlement of several existing transport networks in both developed and developing countries.[4] Quantifying the economic effects of station closures and potential reopenings offers guidance on how transport infrastructure shapes economic activity and how limited investments can be better targeted across space.[5]

In this paper, I study the effects of closing rural rail stations. I exploit a rare and sudden policy shift in southern Côte d'Ivoire following the 2011 post-presidential-election crisis, when a major rural rail service operating along a colonial-era line was discontinued. The closures abruptly cut off small farming communities from rail connections that had long linked them to urban markets for both passengers and agricultural goods. Although freight and intercity passenger services resumed later that year, the rural service remained closed. I use this unanticipated event as a natural experiment to document the causal impacts of losing rail access on household living standards and labor allocation.

[1] From a theoretical point of view, transportation infrastructure has been shown to promote local development by shaping market access and reducing trade costs (Donaldson, 2015; Redding, 2022; Krugman, 1992).

[2] Gollin and Rogerson (2014) present a theory on transport costs and structural transformation and Asher and Novosad (2020) present empirical evidence from Indian villages.

[3] Credible causal evidence on how variations in the availability of transport infrastructure affect development remains however difficult to establish. Major projects are costly, often economically targeted, and rarely randomized, which complicates evaluation. For example, see Brooks and Donovan (2020) on the difficulty of causal evaluation due to high costs, and Banerjee et al. (2020) on the endogenous placement of infrastructure. Empirical findings are mixed: some studies show sizable income gains (Donaldson, 2018; Brooks and Donovan, 2020), while others find limited effects (Asher and Novosad, 2020; Banerjee et al., 2020).

[4] Examples include the Beeching cuts in the United Kingdom (Gibbons et al., 2024), rail line closures in the United States (Frye, 2018), and privatization episodes in Latin America and Sub-Saharan Africa (Sharp, 2005; Bullock, 2005).

[5] Recent theory emphasizes that identical total investments can deliver very different welfare gains depending on their spatial allocation: see Allen and Arkolakis (2022) and Fajgelbaum and Schaal (2020) for equilibrium approaches to optimal transport network design, and Faber (2014) for empirical evidence that infrastructure placement shapes regional gains from trade. These studies highlight the importance of targeting infrastructure where marginal welfare benefits are highest.

I estimate the causal effects of rural rail station closures using a difference-in-differences design that exploits variation across space and time. The analysis draws on nationally representative microdata from Côte d'Ivoire covering periods before and after the 2011 post-presidential-election crisis, with detailed information on spending, demographics, and labor outcomes for more than 25,000 individuals in roughly 5,000 rural households. Each surveyed subprefecture is linked to its nearest rail station using geospatial coordinates for all stations that operated before the 2011 closures. The identification strategy compares rural subprefectures in southern regions where stations were closed (treatment) with nearby rural subprefectures that were never served by rail (control) before and after 2011. The closures provide a plausibly exogenous shock to rail access in treated areas, allowing credible comparisons with nearby rural areas that were not directly exposed.

I find that the closure of rail stations caused a 16 percent decline in household per capita spending and consumption in treated areas relative to nearby control areas. The validity of the difference-in-differences estimates relies on the assumption that spending and consumption would have evolved in parallel absent the closures. Using pre-closure data, I test and support the hypothesis that per capita spending and consumption evolved similarly in treatment and control areas prior to the closure. These estimated declines are not driven by migration: I find no evidence of differential population movement between treated and control areas, consistent with high migration costs in low-income rural settings.[6]

The decline in spending coincided with a shift in labor allocation. I find that workers moved out of the nonfarm sector and into contributing family work, a less productive segment of the agricultural economy. The nonfarm sector contracted by about one-fourth, while the farm sector remained unchanged. Other labor market outcomes, including overall labor supply and hours worked, were unaffected by the closures. Because nonfarm workers earn substantially more per hour than those in contributing family work in my data, this labor reallocation likely contributed to the observed decline in spending.

In summary, the reduced-form results show that rural rail station closures reduced per-capita

[6]See Bryan et al. (2014) and Morten and Oliveira (2024) on high migration costs in rural economies.

spending and reallocated workers from higher-paying nonfarm activities toward lower-paying contributing family work. These findings indicate that rural rail services play a central role in sustaining access to more productive nonagricultural employment opportunities.

I rationalize these findings using a Roy model of workers' sectoral choice, which highlights how sectoral mobility costs can constrain access to better-paying nonfarm opportunities. The model provides a framework for assessing the aggregate benefits and costs of reopening rural rail stations. It also serves as the basis for analyzing how limited infrastructure investments can be spatially targeted, by linking changes in local connectivity to aggregate welfare gains across space.

In the model, workers choose between farm and nonfarm employment based on relative returns and individual mobility costs.[7] I estimate the returns to nonfarm employment using an instrumental variable approach focusing on the treatment group in 2015. The instrument captures the increase in distance to the nearest rail station after the closures, a shock that was unanticipated and plausibly exogenous to workers' characteristics. The results indicate that workers in more isolated areas were less likely to work in nonfarm sectors, and that the implied returns to nonfarm employment in terms of per-capita spending are large. These findings suggest that high mobility costs can trap productive rural workers in lower-return activities, though the estimated effect reflects a local average return for workers most affected by the instrument and may not generalize to all rural workers.

I extend the analysis beyond the previous local estimates by applying the marginal treatment effect (MTE) framework of Heckman and Vytlacil (2005). This approach allows me to estimate how nonfarm employment affects per-capita spending across workers with varying mobility costs. Exploiting the continuous variation in my instrument, I recover the marginal returns to nonfarm work across rural workers in treated areas. The results show that the workers who would gain most from moving to nonfarm activities are also those least able to do so. This pattern implies that reopening rural rail stations could generate large aggregate benefits in terms of per capita spending, particularly if such policies reallocate these high-return workers.

[7] For empirical implementation, I group farm and contributing family work into one sector, since both activities are agricultural and often occur within the same households. See Section 5 for details.

I use these estimates to simulate counterfactual policies involving the reopening of rural rail stations. In the baseline simulation, I assume that the same stations that were closed are reopened, restoring rail access at their original locations. Under this policy, average per capita spending would rise by just over 5 percent, with gains concentrated among workers most likely to move to nonfarm sectors, who also have the lowest expected nonfarm returns. I then consider an alternative counterfactual exercise that achieves the same overall increase in nonfarm participation but reallocates opportunities toward high-return workers. In this scenario, per capita spending rises by over 9 percent, an increase of about 77 percent relative to the baseline simulation. These simulations illustrate that the gains from rail reopening depend critically on which workers gain rail access.

I build on the previous simulations to evaluate how counterfactual reopenings of a subset of rural rail stations can be most effectively allocated across space. Reopening every closed station may not be budgetarily feasible, shifting the policy question from how much infrastructure to rebuild to where to rebuild it. I compute predicted benefits for all possible combinations of reopening half of the closed stations. The analysis reveals substantial heterogeneity in potential gains: the most efficient allocation delivers more than twice the spending improvement of the least efficient one. I show that the most efficient allocation targets a larger share of the most constrained yet high-return workers, inducing the largest welfare-improving reallocation from farm to nonfarm activities. These findings highlight that spatial targeting matters, as similar aggregate investments can yield sharply different welfare outcomes depending solely on where stations are reopened.

I then assess whether welfare gains from rail reopenings are sufficient to justify operating costs. Aggregate gains are computed by multiplying the simulated increase in per-capita spending by the size of the affected rural population. Because the analysis focuses on reopening existing but inactive stations, I consider only operating rather than construction costs. I recover these costs using variation in rail subsidies before and after the closures.[8] Simulating policy scenarios with varying coverage yields large positive net benefits: initial reopenings generate benefit–cost

[8]Operating cost estimates are based on Burkina Faso data, as the rail line spans both countries and is operated by the same concessionaire. Both experienced synchronized closures, but detailed subsidy data are available only for Burkina Faso.

ratios well above one, although marginal gains decline as coverage expands. The results show that targeted reopenings of half the closed stations generate nearly the same welfare gains as full network restoration, yielding a benefit–cost ratio almost twice as high.

All counterfactual simulations described above assume that reopening affects sectoral mobility costs without directly influencing outcomes within sectors, consistent with the standard policy invariance assumption.[9] I extend the initial model to allow for spillovers between the nonfarm and farm sectors. The idea is straightforward: nonfarm workers often purchase agricultural goods locally and trade them across stations. When rail closures reduce nonfarm activity, local demand for farm products also falls, indirectly affecting farmers' earnings. I capture this mechanism in an extended version of the model where spending within sectors depends not only on workers' characteristics but also on changes in the overall size of the nonfarm sector. When such linkages are present, rail closures thus influence productivity across sectors through both direct and indirect channels.

In this extended setup, the total effect of rail station closures has two components: worker reallocation across sectors and aggregate spillovers. Using plausible restrictions on the share of the total effect attributable to reallocation, I identify and estimate informative bounds on the nonfarm-to-farm spillover effect. If reallocation explains 90 percent of the total effect, the implied reduction in farmers' per capita spending is at least 2.2 percent; if only 50 percent, the reduction exceeds 11 percent. These results suggest that even modest deviations from the no-spillover benchmark imply meaningful indirect losses for rural farmers following rail station closures. Since expanding nonfarm activity may also raise demand for local agricultural goods, the estimated gains from the previous counterfactual rail reopenings should be viewed as conservative lower bounds on the broader economy-wide benefits of improved rural connectivity.

Related Literature. This paper contributes to three strands of the literature.

First, it relates to research on transport infrastructure and welfare. Most studies examine the expansion of roads, railways, or bridges rather than their closure and typically find that improved

[9] See Heckman and Vytlacil (2005).

connectivity has mixed effects on incomes and living standards.[10] I instead study the closure of rural rail stations, a rare but policy-relevant shock, in a low-income context. My results show sizable short- to medium-run spending losses, similar in magnitude but opposite in sign to the income gains documented for rail or bridge expansions in other contexts (Donaldson, 2018; Brooks and Donovan, 2020).[11] This study is most closely related to Asher and Novosad (2020), who analyze rural road construction in India. Despite opposite policy shocks, both settings exhibit similar magnitudes of labor reallocation but sharply different welfare outcomes. While new rural roads increased nonfarm employment without raising living standards, rural rail closures in Côte d'Ivoire reduced nonfarm employment and caused sizable welfare losses. I show that these differences arise not from how many workers move across sectors but from who moves. When workers face heterogeneous returns, infrastructure that primarily shifts low-return individuals already near the margin yields limited welfare gains, whereas enabling high-return workers to move generates much larger aggregate effects. Thus, the welfare impact of infrastructure depends less on the scale of reallocation than on its composition.

This paper also connects to recent work on the targeted spatial allocation of infrastructure investments. Theoretical contributions such as Fajgelbaum and Schaal (2020) and Allen and Arkolakis (2022) show that identical total investments can yield very different welfare gains depending on their spatial distribution, while empirical evidence from Faber (2014) demonstrates that network placement shapes regional gains from trade. I complement this literature by exploiting quasi-experimental variation from rail disinvestment to estimate how infrastructure benefits vary across space and by quantifying the potential gains from targeted rail reopenings using an Empirical Welfare Maximization framework (Kitagawa and Tetenov, 2018; Sasaki and Ura, 2024).

Second, the paper contributes to the literature on structural transformation and productivity

[10] See, for instance, Donaldson (2018), Storeygard (2016), and Brooks and Donovan (2020) on the positive effects of transport expansion, and Asher and Novosad (2020), Banerjee et al. (2020), and Faber (2014) for more limited impacts. Recent evidence on rail closures in high-income countries also documents medium- to long-run losses (Gibbons et al., 2024; Frye, 2018).

[11] Donaldson (2018) find that colonial railways increased real incomes in India by about 16 percent, while Brooks and Donovan (2020) show that the construction of footbridges offset roughly an 18 percent decline in household income due to floods in Nicaraguan villages.

gaps. While most research emphasizes rural–urban migration as a path out of low-productivity agriculture (Young, 2013; Lagakos and Waugh, 2013; Gollin et al., 2013; Bryan et al., 2014), I study sectoral mobility within rural areas and document a case of reversed structural transformation: rail closures push workers out of less agriculture-intensive nonfarm jobs and into more agriculture-intensive contributing-family work.[12] My focus on heterogeneous mobility costs into the nonfarm sector connects to models emphasizing heterogeneity in mobility frictions and the need to understand their joint distribution with returns (Lagakos et al., 2020). I provide direct empirical evidence of an upward-sloping marginal treatment effect in nonfarm returns, consistent with constrained comparative advantage or reverse selection: the workers with the highest expected nonfarm gains are also the least likely to transition out of agriculture. This pattern suggests that reallocating these workers toward nonfarm sectors would yield sizable improvements in aggregate living standards.

Finally, this paper contributes to the literature on policy evaluation under treatment-effect heterogeneity (Heckman and Vytlacil, 2005; Carneiro et al., 2010; Mogstad and Torgovitsky, 2018). I extend the standard marginal treatment effect framework by allowing outcomes to depend on aggregate employment shifts across sectors, so that total effects of rail closures incorporate both reallocation and spillover components. I identify informative bounds on the nonfarm-to-farm spillover component by imposing economic sign restrictions and plausible assumptions about the share of total effects attributable to reallocation.

Roadmap. The remainder of the paper proceeds as follows. Section 2 describes the institutional context and data. Section 3 presents the empirical strategy. Section 4 reports the reduced-form findings. Section 5 develops the model and counterfactual simulations. Section 6 analyzes spatial targeting and spillover mechanisms. Section 7 concludes.

[12] See Gollin et al. (2013) for evidence on large productivity gaps between agricultural and nonagricultural sectors, and Young (2013) and Lagakos and Waugh (2013) for models emphasizing selection as the main source of these gaps. In contrast, Bryan et al. (2014) and Gai et al. (2025) highlight the role of mobility costs rather than selection in explaining rural productivity differences.

2 Background and Data

2.1 Context

Mixed rail services, trains carrying both passengers and freight, were historically used to serve low-traffic corridors where running separate lines was not economically viable (Burns, 2024). In Sub-Saharan Africa, these services became essential for connecting rural areas to urban centers. Over time, however, privatization reshaped rail operations across the continent. By the early 2000s, more than 70 percent of rail networks (excluding South Africa) were under private management, and profitability became a central concern (Proparco, 2011). In this shift, slow and less profitable mixed trains were among the first to be discontinued, especially in rural areas, while freight and intercity passenger lines were generally maintained for their higher revenue potential.

While road networks have expanded in urban areas, rural regions continue to face severe transport constraints. For instance, in Côte d'Ivoire, fewer than one in four rural residents live within two kilometers of an all-season road (Mikou et al., 2019). In these settings, mixed rail services often represent the only reliable link between rural areas and outside markets. Their closure can therefore impose significant economic and social costs where alternative transport options are scarce.

Côte d'Ivoire's railway was built during the colonial period to export raw materials, linking the Abidjan seaport to landlocked Burkina Faso. After independence, it remained publicly managed until the mid-1990s, when it became one of the first African railways to be privatized. Today, the line operates as a binational corridor connecting Abidjan to Ouagadougou, comprising a profitable freight system and a passenger service that historically included rural mixed trains.

Freight component as a corridor system. Since privatization, freight has been the financial backbone of Côte d'Ivoire's railway, contributing nearly 80 percent of operator revenue (Dagnogo et al., 2012). The system mainly supports international transit, carrying cement, fertilizer, and containers northward, and cotton, livestock, and agricultural goods southward, through a few major stations (Abidjan, Bouaké, Ferkessédougou, and Ouangolodougou). Unlike passenger services, freight operations remained stable even during political unrest, with annual volumes above 800,000 tons except during the 2002 temporary northern conflict and the 2011 post-presidential-electoral crisis (Figure A.1a). Freight traffic largely bypasses rural stations and plays little direct role in connecting small producers to markets (Konan, 2021).

Passenger services and the role of mixed rail services. Historically, Côte d'Ivoire operated two passenger services: the Express, serving major cities, and the Omnibus, a slower mixed rail service critical to rural connectivity. The Express functioned like an intercity train with few stops, whereas the Omnibus, government-subsidized and primarily serving southern rural areas, stopped at nearly every station and carried both passengers and small agricultural goods. It enabled small traders to ship products such as rice, bananas, okra, charcoal, and palm kernels from remote villages to urban markets (Dagnogo et al., 2012). This dual passenger–freight role made the Omnibus a lifeline for rural trade networks.

The service came to an abrupt end after the 2011 post-presidential-electoral crisis. The Omnibus was discontinued, leaving only the Express, with limited stops in major cities. Passenger numbers dropped from an average of 400,000 annually before 2010 to about 175,000 after 2011 (Figure A.1b).[13] Even during the 2002 northern conflict, the Omnibus had continued operating in southern areas (Dagnogo et al., 2012).

The closure left a major gap in rural transport access. In regions where roads remain poor, it effectively severed market links for small producers and traders, shifting the burden onto fragile rural road systems. What had once been a low-cost, inclusive transport mode was replaced by one increasingly centered on intercity mobility. Throughout the paper, I refer to these discontinued mixed services as "rural rail services" to distinguish them from the more profitable freight and intercity lines that continued to operate after 2011.

[13] The average for 2000-2009 excludes 2003 and 2004, which experienced a sharp drop in passenger numbers due to the short-term 2002 conflict in northern Côte d'Ivoire.

2.2 Data

Data sources. I use two main sources of data to analyze the effects of rural rail station closures. The first is a list of all 28 rail stations that were operational in Côte d'Ivoire before the 2011 post-presidential-election crisis. I match these stations with GPS coordinates extracted from OpenStreetMap (see Figure A.2a).

The second source is three waves of the Enquête Niveau de Vie (ENV), Côte d'Ivoire's national household budget survey, conducted in 2002, 2008, and 2015. These surveys are representative at the national and subnational (region) level. They follow similar sampling and questionnaire design across years. Each survey uses a two-stage random sampling method. First, primary sampling units (PSUs) are selected with probability proportional to size. Then, a fixed number of households are randomly drawn in each PSU. In rural areas, the ENV covers 5,819 households in 2002, 6,000 in 2008, and 7,115 in 2015, representing 32,464, 29,998, and 26,488 individuals, respectively. These households are drawn from 291, 300, and 593 PSUs.

To measure geographic distance to rail services, I geocode the survey locations. I start by constructing physical addresses for each PSU using locality and region names. I convert those into GPS coordinates using Google Maps. Then, I assign each household to a subprefecture, and compute the distance to all 28 rail stations using Vincenty (1975)' formula. I retain the minimum distance as the distance to the nearest station for each household, separately by year.

Because the surveys are repeated cross-sections, I also aggregate household and individual data at the subprefecture level.[14] This allows me to construct a (pseudo) panel of subprefectures, unbalanced across years, for my reduced-form empirical analysis.

Main outcomes. I focus on four sets of variables to measure household living standards, labor market outcomes, and basic demographics.

1. *Household per capita spending.* I construct this measure from the raw survey data to ensure consistency across the three survey waves. It aggregates household expenditures on educa-

[14] Subprefectures are the lowest administrative level in Côte d'Ivoire during my study period.

tion, health, clothing, personal care, transportation, and housing. I then divide the total by household size to obtain per capita spending.

2. *Household per capita consumption.* This measure is provided by the national statistical office and is already included in the surveys. It adds to the spending measure the value of goods consumed from the household's own production.

3. *Demographics.* I track the age and gender structure of individuals and household heads to verify the composition of my sample over time.

4. *Labor outcomes.* I analyze both extensive and intensive margins of labor supply for the working-age population (15–64 years old). At the extensive margin, I use an indicator for whether an individual worked during the past 12 months. At the intensive margin, I use total hours worked per year. I also classify employment into three mutually exclusive sectors: nonfarm, farm, and contributing family work.

More details on the construction of these variables are provided in Appendix 1.

3 Natural Experiment and Identification

3.1 The 2011 Post-Presidential-Election Crisis as a Natural Experiment

After two decades of strong post-independence growth, Côte d'Ivoire entered a period of economic decline and political instability marked by a 1999 military coup and a 2002 conflict that divided the country between a rebel-controlled north and a government-controlled south (Soumahoro, 2017). This de facto partition lasted until the disputed 2010 presidential election, which triggered a brief post-presidential-electoral crisis from December 2010 to April 2011. Violence was highly localized: three regions, the economic capital Abidjan (49.6 percent of all deaths), the Cavally region (30.3 percent), and the city of Duékoué (11.2 percent), accounted for more than 90 percent of total fatalities (Léon and Dosso, 2020).[15]

[15] Below, these three areas are excluded from the analysis sample.

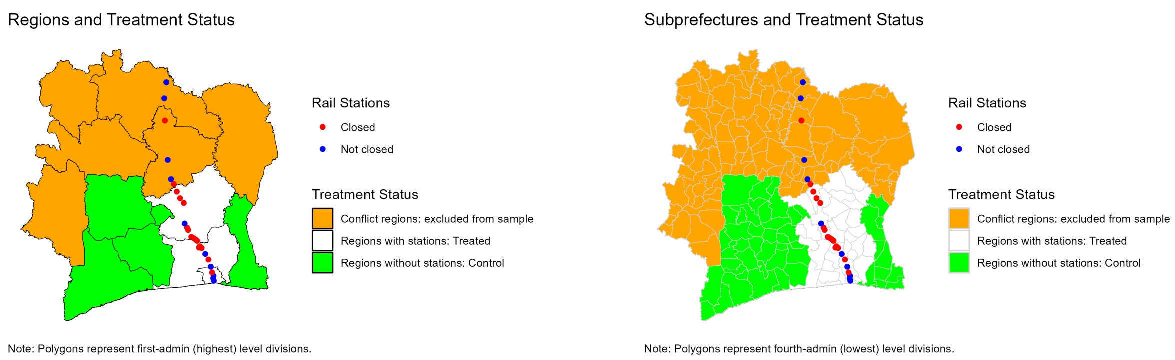
The crisis led to a nationwide closure of rail operations in early 2011. Freight and intercity passenger services resumed later that year, but rural rail services along the southern corridor remained closed. Of the 28 passenger stations operating before the crisis, only 10 reopened, all located in urban centers. The remaining 18 stations, including 16 in southern rural regions, remained closed (Figure A.2).

These closures were abrupt, exogenous to local economic conditions, and geographically concentrated. They were not driven by declining demand but became possible following the abrupt change in government. I exploit this sudden and localized closure of rural rail stations as a natural experiment to study how the loss of rail access affected household welfare and labor allocation in previously connected communities. Further details on the historical background of the 2011 post-presidential-election crisis are provided in Appendix 2.

3.2 Identification Strategy

Sample Selection I restrict the analysis to rural southern Côte d'Ivoire, where all rural rail stations under consideration operated before 2011 and where 16 of the 18 closures occurred after the 2011 post-presidential-election crisis. This geographic focus ensures that the analysis captures the main variation from the natural experiment while avoiding potential confounding from long-standing north–south differences. Unlike the northern areas, which were under rebel control between 2002 and 2010, the southern areas remained under government authority throughout the study period, providing a consistent setting before and after rural rail station closures.

I also exclude the three areas most directly affected by the 2011 post-presidential-election crisis: the economic capital city of Abidjan, the city of Duékoué, and the Cavally region. These zones accounted for over 90% of conflict-related casualties during the 2011 post-presidential-election crisis (Léon and Dosso, 2020). Thus, excluding them helps isolate the effects of rail station closures rather than those driven by short term violence. The final sample includes only rural subprefectures in southern Côte d'Ivoire, covering: 19,500 individuals (3,521 households) in 2002, 17,622 individuals (3,480 households) in 2008, and 13,198 individuals (3,947 households) in 2015.



(a) Baseline regions

(b) Baseline subprefectures

Figure 1: Spatial Coverage of Treatment and Control Areas in Côte d'Ivoire

Note: These figures present maps of Côte d'Ivoire distinguishing treatment and control areas. The former are shown in white, while the latter are shown in green, both located in the southern regions. Orange-colored areas represent the northern regions, which are excluded from the study sample.

Figure 1a and Figure 1b illustrate the spatial coverage of the study. Treatment subprefectures (in white) correspond to rural areas in regions previously traversed by the rail line, while control subprefectures (in green) are adjacent rural areas in the southern regions that were never served by rail. Excluded areas (in orange) primarily correspond to northern regions.[16]

Research Design. To identify the causal impact of rural rail closures, I use a difference-in-differences (DiD) approach that combines spatial and temporal variations. The temporal variation arises from the 2011 post-presidential-electoral crisis, which caused the permanent closures of rural rail stations in rural southern Côte d'Ivoire. The spatial variation comes from whether a subprefecture was located in a southern region historically traversed by the rail line before 2011. The treatment group includes all rural subprefectures in southern regions in Côte d'Ivoire that were traversed by the rail line and had stations closed after 2011. The control group includes rural subprefectures in adjacent southern regions that were never served by rail.[17]

This empirical design exploits sharp, plausibly exogenous variation in access to rural rail stations. The 2011 closures were abrupt, localized, and unrelated to pre-existing demand, as they

[16]The excluded areas also include the three most crisis-affected zones.

[17]The treated regions are: Abidjan (excluding the economic capital), Lagunes, and Lacs. The controls are: Bas-Sassandra, Comoé, Gôh-Djiboua, Sassandra-Marahoué, and Yamoussoukro.

resulted following the unexpected change in governemnt (see the previous subsection).

The assumption underlying identification is that, absent the closures, trends in living standards and labor outcomes would have evolved similarly in treatment and control subprefectures. This is supported by the unexpected nature of the closure shock and by pre-crisis balance between the two groups, as shown later. Additionally, the causal interpretation of my results rely on the fact that my considered control group, not directly exposed, is plausibly unaffected by rail station closures. I provide support for this assumption below by showing that the effects of rail station closures are locally concentrated around the treatment subprefectures being nearest to the rail line.

The formal regression specification is:

$$Y_{S,t} = \beta_0 + \beta_1(post \times treat) + \lambda_S + \lambda_t + \epsilon_{S,t} \tag{3.1}$$

where $Y_{S,t}$ denotes the subprefecture-level mean of the outcome variables (e.g., log spending, sectoral choices) in subprefecture S and year t. $post$ equals 1 for 2015 (after the closures) and 0 for 2002–2008; $treat$ equals 1 for treatment subprefectures. λ_S and λ_t are subprefecture and year fixed effects, respectively. Standard errors are clustered at the subprefecture level (Bertrand et al., 2004), and all regressions use survey weights.

Because all treated subprefectures were exposed in the same year, the design is a standard DiD rather than a staggered adoption one, and the two-way fixed-effects estimator yields the conventional treatment-on-the-treated effect (Wooldridge, 2021).

To test for parallel pre-trends, I estimate the same equation using only pre-treatment years (2002 and 2008), with $post = 1$ for 2008.

Descriptive Statistics. Table A.1 summarizes key characteristics of the sample. Treated subprefectures tend to have slightly older household heads and a higher share of female-headed households throughout my study period. Access to credit is limited in my sample with fewer than 25% of household heads report having any form of credit in 2008 or 2015. Fewer than one in five household heads identify as migrants in 2002 and 2008, confirming limited migration in rural southern

Côte d'Ivoire.

Both treatment and control groups show increasing (log) spending and consumption over time. However, growth was slower in the treatment group post-2011, where consumption and spending levels, initially higher before 2011, fell behind control areas by 2015. In terms of labor outcomes, employment rates remain broadly similar, but sectoral composition diverged. Before 2011, treated subprefectures had a higher share of nonfarm workers and fewer contributing family workers. After 2011, the opposite occurred: a visible shift back toward contributing family work in treated areas.

These descriptive patterns suggest that the closure of rural railway stations may have reduced household spending and consumption partly by constraining nonfarm employment opportunities. The next section tests these formally.

4 Reduced-Form Results: Spending and Labor Effects

4.1 Effects on Spending and Consumption

This section examines the main impacts of rural station closures on household per capita spending and consumption.

Table A.2 reports the baseline household-level pre-trend estimates (Panel A) and the DiD estimates (Panel B). Prior to the 2011 natural experiment of rural rail station closures, rural subprefectures served by rail followed similar trends as those never served. There is no evidence of systematic pre-closure differences. Pre-trend estimates for per capita spending and consumption are small and insignificant, supporting the parallel-trend assumption prior to the closures.

Following the closures, average household per capita spending declined by about 16 percent. Per capita consumption fell by a similar 15.7 percent in treated subprefectures relative to control areas. These are sizable and economically meaningful effects, indicating that rural rail station closures substantially reduced household living standards. The magnitude of these effects is substantial. They are comparable, though opposite in sign, to those found in studies of infrastructure expansion. For example, Donaldson (2018) reports that the colonial-era expansion of rail lines in India increased real incomes by about 16%, while Brooks and Donovan (2020) finds that the construction of new footbridges in Nicaraguan villages eliminated an 18% decline in labor income caused by floods.

Robustness. I next assess the robustness of these findings using several complementary strategies.

Spatial heterogeneity. Because rural rail stations are highly localized, I first test whether the estimated effects are spatially concentrated near the closed lines. I divide the treatment group into two subgroups: those located closer than the median distance to the nearest closed station and those farther away. Both subgroups are compared against the same control group. This corresponds to the same regression specification as in Equation (3.1), except that two treatment variables are included instead of one. The results, reported in Table A.3, show that the negative effects of rural rail station closures are indeed locally concentrated in areas closest to the former rail stations, with negligible and statistically insignificant effects in more distant rural subprefectures. These findings suggest that the control group provides a valid counterfactual. They are located even farther from the rail line than the more distant treated subprefectures.

Redefining treatment by distance. The baseline treatment definition, whether a subprefecture was located in a region served by rural rail stations, might be imperfect. Some control subprefectures may lie closer to a closed station than some treated ones, and individuals can move freely across subprefectures. To address this, I redefine treatment status using distance to the nearest rail station. Specifically, I estimate effects using three distance thresholds: 80 km, 100 km, and 120 km. Households within these thresholds of a closed station are considered treated. These alternative definitions mitigate potential misclassification in the baseline setup. They also provide a more geographically precise measure of exposure to the closures. While the first test captures within-treatment spatial variation, the second redefines treatment boundaries altogether.

Accounting for treatment intensity. Exposure to rail services varies among treated households: those living closer to a station likely relied more on it than those farther away. If so, treatment intensity varies continuously with distance. To account for this, I follow Callaway et al. (2024). They show that even with heterogeneous treatment intensities ('doses'), one can identify average effects under parallel trends by dose. I apply their framework, defining treatment intensity by proximity to a station and using households beyond 120 km as the control group.

Figures A.5a and A.5b present these robustness results. Across all specifications, the pattern remains consistent: while treated and control groups evolve similarly before 2011, household spending and consumption decline sharply in treated areas after the closures.

4.2 Other Outcomes: Population and Demographic Composition

I further examine whether the results might reflect selective migration or demographic shifts. If individuals moved out of treated subprefectures after the closures, or if household composition changed, the spending and consumption estimates could be biased.

To assess this possibility, I estimate DiD regressions using demographic and population-level outcomes: (i) the average age of household heads, (ii) the share of female-headed households, and (iii) total population size, using survey weights to approximate population counts. The surveys are probability-based, with household weights equal to the inverse of each household's selection probability. These weights allow the sample to represent the reference population, producing estimates that are representative at reasonably aggregated levels.

Table A.4 reports no significant post-2011 differences between treatment and control subprefectures along these dimensions. This suggests that the observed decline in spending is unlikely to be driven by selective migration or demographic changes. There is little evidence of population movement. This pattern aligns with prior research showing that high mobility costs in low-income rural settings constrain migration (Bryan et al., 2014; Morten and Oliveira, 2024; Gai et al., 2025).

4.3 Effects on Labor Market Outcomes

Finally, I examine the effects of rural rail station closures on labor supply and sectoral allocation among working-age adults. Household spending and consumption depend partly on the labor activities of working-age members. Examining these outcomes sheds light on the mechanisms behind the main results.

Panel A of Table A.5 reports the pre-trend estimates between treatment and control subprefectures. Labor supply and sectoral choices among working-age adults evolved in parallel between 2002 and 2008, indicating no systematic differences prior to the closures. These negligible pre-trends are consistent with those observed for household consumption and spending in the pre-treatment period.

Panel B of Table A.5 presents the post-treatment estimates, showing that while overall labor supply appears unaffected, the composition of employment shifts significantly across sectors. Specifically, the closures led to a reallocation of labor from nonfarm to contributing family work, with virtually no effect on farm activities. Nonfarm employment declines by about nine percentage points, a magnitude comparable in size but opposite in sign to the reallocation of labor out of agriculture documented by Asher and Novosad (2020) following rural road construction in India.

The null effect on farm work likely reflects that farming depends on access to land. This is unlikely to be influenced by the presence or absence of a rural rail station. In contrast, rural rail services provide vital connectivity to towns and cities, facilitating nonfarm self-employment opportunities. When transport links disappear, the nonfarm sector contracts. Many displaced workers shift into contributing-family jobs, which are typically unpaid. Consistent with this, only 2 percent of nonfarm workers and 12 percent of farm workers receive no compensation. Among contributing-family workers, the figure rises to 42 percent. Nonfarm workers earn, on average, more than nine times the hourly earnings of contributing-family workers. They are also far less agriculture-intensive, only 20 percent in agriculture compared with 72 percent among contributing-family workers (Figure A.6). This reallocation pattern is consistent with a reverse structural transformation (Gollin and Kaboski, 2023). This shift from paid nonfarm to unpaid family work thus provides a plausible mechanism underlying the observed decline in household spending and consumption.

4.4 Summary of Reduced-Form Findings

In summary, the reduced-form evidence indicates that rural rail station closures led to sizable declines in household living standards, with per capita spending and consumption falling by about 16 percent in affected areas. These effects are robust to pre-trend tests, alternative treatment definitions based on distance, and specifications accounting for treatment intensity. The analysis of demographic and population outcomes shows no evidence of selective migration or compositional change, reinforcing the causal interpretation of the results. Instead, the decline in spending coincided with adjustments in the labor market: the closures induced a reallocation of work from higher-paid, less agriculture-intensive nonfarm activities toward unpaid, more agriculture-intensive contributing-family work, consistent with a reverse structural transformation (Gollin and Kaboski, 2023). Taken together, these findings suggest that the spending decline was primarily driven by reduced access to better-paying nonfarm employment. Population shifts or aggregate labor contraction played little role.

5 Welfare Effects of Rural Rail Reopenings

The reduced-form evidence showed sizable declines in per capita spending after rural rail station closures, alongside a shift from higher-paid nonfarm to lower-paid contributing-family work. A natural mechanism is that sectoral mobility costs limit access to nonfarm jobs and closures raise these costs.

I formalize this mechanism using a Roy model of sectoral choice with heterogeneous returns and mobility costs, which I use to recover the distribution of marginal nonfarm returns and evaluate welfare under counterfactual rail policies.

5.1 Intuition: A Simple Roy Model of Costly Selection

To build intuition, consider a simple Roy model where workers choose between farm and nonfarm sectors by comparing relative nonfarm returns with the cost of accessing those jobs. Both returns and costs vary across individuals, so some who would earn more in nonfarm still do not enter because costs exceed gains.

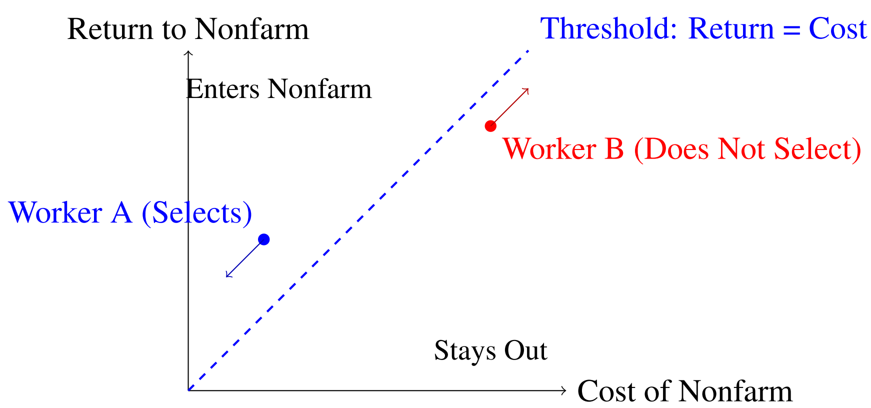


Figure 2: A Simple Roy Model of Costly Selection into Nonfarm

Figure 2 illustrates this costly selection. Worker A, with modest returns but low costs, selects into nonfarm, while Worker B, with high returns but high costs, does not. This reverse selection, where the workers with the highest expected nonfarm gains stay out of their comparative-advantage sector, provides a microfoundation for the reduced-form pattern: closures raised mobility costs, shifting workers out of higher-paying nonfarm jobs.

5.2 Generalized Roy Model: Selection Framework

I consider a two-sector Roy model for working-age employed adults who choose farm ($D = 0$) or nonfarm ($D = 1$).[18] Each sector yields a potential (log) spending outcome $Y_d(X, U_d)$, where $d \in \{0, 1\}$. Here, X is a vector of observed characteristics, and U_d represents unobserved individual heterogeneity in sector-specific productivity. Working in the nonfarm sector is contingent to an individual-specific cost $C(Z, U)$, where Z is an observed cost shifter and U an unobserved component of the cost.

Workers decide whether to work in the nonfarm sector by comparing their potential returns across sectors, net of costs. The selection rule is therefore:

[18] I aggregate farm and contributing-family work into a single farm sector. I provide descriptive evidence supporting this grouping in Section 5.3.

$$D = 1\{\, Y_1(X, U_1) - Y_0(X, U_0) \geq C(Z, U) \,\}. \tag{5.1}$$

Under this rule, both the return to nonfarm work and the cost of sectoral mobility are heterogeneous across workers. Some workers may have high potential productivity in nonfarm activities but still choose to remain in the farm sector if their mobility costs exceed their expected gain, precisely the situation illustrated in Figure 2. When $D = 1$, Y_1 is observed, and when $D = 0$, Y_0 is observed. The realized outcome is thus: $Y = D \cdot Y_1 + (1 - D) \cdot Y_0$.

I make the following standard assumptions (Heckman and Vytlacil, 2005):

Assumption 1

A.1 $Y_1(X, U_1, z) = Y_1(X, U_1)$ *for all* $z \in \mathrm{supp}(Z)$ *(exclusion restriction).*

A.2 $Z \mid X \perp (Y_1, Y_0, U)$ *(exogeneity of the cost shifter).*

A.3 *The distribution of* U *is absolutely continuous with respect to the Lebesgue measure.*

A.4 $\mathbb{E}[Y_d] < \infty, \quad d \in \{0, 1\}$ *(finite moments).*

Assumption **A.1** imposes that variation in the cost shifter affects outcomes only through the cost of working in the nonfarm sector, not directly through productivity. Assumption **A.2** ensures that the cost shifter is exogenous conditional on observables, justifying its use as an instrument. The remaining assumptions, **A.3** and **A.4**, are standard technical conditions ensuring well-defined parameters and continuous heterogeneity.

These assumptions, common in the heterogeneous treatment-effect literature (Heckman and Vytlacil, 2005, 2007; Mogstad and Torgovitsky, 2018), justify using Z as a valid instrument and allows linking rail policy shocks to workers' sectoral choices in a structurally consistent way.

5.3 Linear IV Estimation

I begin with a linear IV specification to estimate average returns to nonfarm work in the model using working-age employed adults in treated regions in 2015, consistent with the reduced-form sample. I adopt the following baseline specification:

$$Y = \alpha + \beta D + \varepsilon, \tag{5.2}$$

where Y denotes (log) per capita spending, and D is an indicator for working in the nonfarm sector. Here, $\alpha = \mathbb{E}[Y_0]$, $\beta = Y_1 - Y_0$ represents the individual return to nonfarm work, and $\varepsilon = Y_0 - \mathbb{E}[Y_0]$. Because sectoral choice is endogenous, workers with lower mobility costs are more likely to work in the nonfarm sector, OLS estimates of β are likely biased. An external source of variation in D is thus required to identify causal returns to nonfarm employment.

Instrument: Cost Shifter from Rural Rail Closures. I use as an instrument the policy-induced variation in the cost of accessing nonfarm employment:

$$Z = \Delta \log Dist_S \equiv \log(\text{Dist}_{S,\text{close}}) - \log(\text{Dist}_{S,\text{base}}),$$

where $Dist_S$ measures the distance from subprefecture S to its nearest rail station. This variable captures how much farther a subprefecture moved from rail access following the closures.

Identification of the IV estimand $\widehat{\beta}^{IV}$ relies on the standard conditions of *exogeneity*, *exclusion*, and *monotonicity* (Imbens and Angrist, 1994; Heckman et al., 2006), which follow from the structural model under Assumptions **A.1**-**A.4** (Vytlacil, 2002). Under these assumptions, the IV estimator recovers the mean return to nonfarm work for the subgroup of *compliers*, workers whose sectoral choice is affected by the instrument. The instrument plausibly satisfies these requirements. In my context, conditional on initial distance, variation in Z is driven by station closures rather than sorting or local shocks, and Section 4 shows no migration responses. Monotonicity is natural: higher distance should only lower nonfarm participation for everyone.

The IV estimation proceeds in two stages: the first relates nonfarm participation to the change in distance, and the second relates per capita spending to the same instrument. The IV estimator is given by the ratio of the reduced-form to the first-stage coefficients, capturing the local average effect of nonfarm work for households most affected by the closures.

Results. Table A.6 reports summary statistics for the estimation sample. Farmers and contributing-family workers are far more likely than nonfarm workers to live in the same household. Their per capita spending levels are likewise much more similar. Both occupations are also heavily agricultural, with over 70% of employment in that sector (Figure A.6). These patterns support treating farm and contributing-family work as a single "farm" category in the two-sector model.

I assess whether the instrument behaves as expected. Regressing Z on observable workers' characteristics (Table A.7) shows no systematic differences, with coefficients globally insignificant,[19] confirming that Z is unrelated to pre-existing worker attributes. Placebo first-stage and reduced-form regressions in 2015 control regions, where rail lines never existed, likewise show no relationship between Z and outcomes (Table A.8). These results confirm the instrument's validity and that control regions in Section 4 were plausibly unaffected by rail closures.

IV Estimates. Table A.9 reports OLS, first-stage, reduced-form, and IV estimates, controlling for initial distance to the nearest rail station, demographics, and their interactions. OLS estimates show a positive association between nonfarm employment and household spending, likely reflecting selection bias. The first stage confirms that greater exposure to rail closures reduces nonfarm employment, consistent with the DiD evidence of reallocation away from nonfarm work. The reduced-form results likewise show lower per-capita spending in more affected areas.

The IV estimate of β implies substantial gains to nonfarm employment for workers whose sectoral choice is affected by the instrument. Its magnitude exceeds the OLS estimate, suggesting that OLS understates the true return because lower-cost, lower-return workers self-select into nonfarm work. This pattern mirrors Gai et al. (2025), who find similarly large IV returns to migration, pointing to significant mobility costs among compliers. Finally, a 16% decline in spending alongside a 9.4 pp drop in nonfarm employment implies an elasticity near 1.8, which is broadly consistent with the IV estimate of 1.3.

[19] Except "never attended school," marginally significant at the 10% level.

5.4 Beyond Linear IV: The Marginal Treatment Effect (MTE)

Because returns to nonfarm employment are heterogeneous, the linear IV estimates in the previous subsection identify only an average effect for compliers, those whose sectoral choice responds to the instrument, rather than the full distribution of nonfarm returns. With a continuous instrument,[20] like in my setting, the linear IV estimand represents a weighted average of marginal treatment effects (MTEs) rather than a single, policy-invariant local average treatment effect (LATE) (Blandhol et al., 2022; Alvarez and Toneto, 2024; Heckman and Urzua, 2010). To recover this underlying heterogeneity and evaluate counterfactual rail policies, I adopt the MTE framework of Heckman and Vytlacil (2005), which models the distribution of marginal nonfarm returns and allows aggregation under alternative policy scenarios such as rural rail closures or reopenings.

Definition and Identification of the MTE. Following Vytlacil (2002), the selection rule in Equation 5.1 can be expressed as a single-crossing condition:[21]

$$D = 1\{\, U_D \leq p(X, Z)\, \},$$

where $p(X, Z)$ denotes the propensity score or probability of working in the nonfarm sector, and $U_D \sim \text{Unif}[0, 1]$ represents the unobserved cost (resistance) of working in the nonfarm sector.

Under this formulation, the Marginal Treatment Effect (MTE) is defined as:

$$\text{MTE}(u) = \mathbb{E}\left[Y_1 - Y_0 \mid U_D = u\right],$$

which measures the mean nonfarm return for a marginal worker whose unobserved cost of sectoral mobility is $u \in [0, 1]$. Low values of u correspond to individuals who are more likely to work in the nonfarm sector, while higher values indicate those less likely to work in nonfarm. The MTE thus traces out the entire distribution of expected nonfarm returns across workers with varying

[20] meaning one that varies continuously rather than discretely.

[21] For simplicity, I assume the cost function $C(Z, U)$ is separable in its observed and unobserved components: $C(Z, U) = C(Z) + U$. Rewriting the selection equation is possible under assumptions **A.1**-**A.4**.

unobserved costs.

The Marginal Treatment Effect (MTE) is identified via the local-IV estimand, that is, as the derivative of the observed outcome with respect to the propensity score (e.g., Heckman and Vytlacil, 2007). Identification of the MTE requires that the propensity score spans the unit interval and that the outcome varies smoothly with it. I estimate the MTE curve following Cornelissen et al. (2016) using a two-stage procedure.[22] In the first stage, I estimate the propensity score $p(X, Z)$ using a probit model. In the second stage, I estimate the outcome equation as a flexible function of the propensity score and compute the MTE as its derivative. For robustness, I report results under both a quadratic polynomial and a standard normal specification for the second stage.

Results. Figure A.7 presents the distribution of estimated propensity scores from the first-stage probit regression. The estimates display sufficient variation, covering almost the entire $[0, 1]$ interval with support from 0 to 0.91. Consequently, fewer than 10% of MTE values require extrapolation beyond the observed support.

Figures A.8a and A.8b present the estimated MTE curves with 90% confidence intervals. The MTE is upward sloping in the unobserved cost, mirroring the pattern in the illustrative example of Figure 2. These findings are robust across both polynomial and normal specifications, which yield similar patterns and magnitudes of heterogeneity in nonfarm returns.

The upward slope of the MTE indicates that workers who would gain most from moving into nonfarm employment are the least likely to doing so. This pattern, consistent with constrained comparative advantage or reverse selection on returns (Cornelissen et al., 2018), suggests that mobility frictions might distort sectoral allocation: some individuals with high potential nonfarm returns remain in low-return farm activities because of these costs. In equilibrium, this reverse selection could depress aggregate productivity, as workers with the largest potential gains are least likely to be employed in sectors where they are most productive (Hsieh et al., 2019).

[22] The approach of Cornelissen et al. (2016) assumes that potential outcomes are additively separable in observed and unobserved components conditional on the unobserved resistance to treatment, implying a partially linear outcome equation where nonlinearity enters only through a flexible function of the propensity score. Further details are provided in Appendix B of that paper.

The evidence of reverse selection further suggests that mobility costs can inhibit structural transformation by preventing high-return workers from shifting into more productive nonfarm sectors. This interpretation aligns with the reduced-form evidence of reverse structural transformation following rail station closures, pushing workers away from more productive nonfarm employment.

This empirical link motivates the next analysis, which evaluates the welfare effects of reducing such costs through rail reopenings.

5.5 Policy Counterfactual: Re-opening All Rural Rail Stations

The estimated MTE patterns reveal substantial heterogeneity in returns to nonfarm employment, with the highest-return workers being the least likely to work in nonfarm. This finding implies sizable potential gains from reallocating these workers, for instance, through the re-opening of rural rail stations. In this section, I quantify these gains by simulating a full re-opening of all previously closed stations and examining how lower mobility costs would reallocate workers across sectors and affect per capita spending.

From Rail Policy to Costs of Sectoral Mobility. I map rail policy to the cost shifter via

$$Z = \log Dist_{S,\text{rail}} - \log Dist_{S,\text{base}},$$

where $Dist_{S,\text{base}}$ denotes the baseline distance from subprefecture S to the nearest rail station (as if no closures had occurred), and $Dist_{S,\text{rail}}$ is the distance under a given policy scenario.

Under the counterfactual full re-opening, $Dist_{S,\text{rail}} = Dist_{S,\text{base}}$, so that $Z = 0$ for everyone. Because closures primarily altered sectoral sorting rather than migration (Section 4), I hold residence fixed across counterfactuals.

Since Z enters the model as a cost variable, confirmed empirically by the negative first-stage estimate (Section 4), a re-opening of stations effectively reduces sectoral mobility costs, encouraging additional entry into the nonfarm sector.

Policy Invariance and the PRTE Framework. To evaluate the average benefits of rail re-openings, I adopt the policy invariance framework of Heckman and Vytlacil (2005). The key identifying condition is that the joint distribution of (Y_1, Y_0, U_D) given X remains invariant across rail policies:[23]

Assumption 1 ***A.5*** $(Y_1, Y_0, U_D) \mid X$ *is invariant to rail policy changes.*

Assumption **A.5** implies that rail policy affects outcomes only through sectoral selection, not through changes in sector-specific productivity. Given this invariance, the estimated MTE curve can be used to simulate how average per capita spending would respond to alternative rail policies. Hence, the average policy benefit can be expressed as the *Policy-Relevant Treatment Effect* (PRTE; Heckman and Vytlacil 2001, 2005; Carneiro et al. 2010):[24]

$$\underbrace{\mathbb{E}[Y \mid \text{rail policy}] - \mathbb{E}[Y \mid \text{baseline}]}_{\text{Average Benefit}} = \underbrace{\int \text{MTE}(u)\, w(u)\, du,}_{\text{PRTE}} \tag{5.3}$$

where $w(u) = F_{P^*,X}(U_D \mid X) - F_{P,X}(U_D \mid X)$ measures the policy-induced change in the share of individuals entering nonfarm work at each level of unobserved cost u.[25] Intuitively, $w(u)$ indicates which part of the unobserved cost distribution the policy shifts, and hence who benefits from rail re-openings.

Economically, Equation 5.3 can be interpreted as a policy-relevant welfare aggregator. The MTE captures the individual-level nonfarm gain from reducing sectoral mobility costs for a marginal worker with unobserved cost u. The policy weight $w(u)$ measures how strongly rail policy shifts that segment of the population. The overall policy effect is thus the integral of these heterogeneous gains, weighted by the share of individuals who are induced to switch sectors. Average welfare gains therefore depend jointly on the shape of the MTE, reflecting how returns vary across workers, and on how the policy reweights the unobserved cost distribution through $w(u)$.

[23] The distribution of X is also assumed invariant to policy changes.

[24] I use the unnormalized definition of the PRTE following Heckman and Vytlacil (2001).

[25] $F_{P^*,X}(U_D \mid X)$ and $F_{P,X}(U_D \mid X)$ are the cumulative distributions of the estimated propensity score under the counterfactual and baseline rail systems, respectively.

Results. Figure A.9a shows the distribution of policy weights, $w(u)$, for the counterfactual re-opening of all rural rail stations. The policy weights are concentrated among low-cost individuals (low u) with lower expected nonfarm returns. Thus, workers with the highest potential nonfarm returns (high u) remain constrained even under full re-opening.

Table A.10 reports the PRTE estimates. Under the actual full reopening policy, average per capita spending rises by just over 5 percent. Because most benefits accrue to low-cost, low-return workers, the policy fails to reach the individuals with the largest potential gains.

To illustrate the scope for improved targeting, I simulate an alternative policy that achieves the same overall increase in nonfarm participation but reallocates the policy weights toward high-cost, high-return workers: those least likely to work in nonfarm (Figure A.9b). This targeted counterfactual yields substantially larger benefits, raising average per capita spending by more than 9 percent, or roughly 77 percent higher than under the previous counterfactual.

These results reveal a broader mechanism: identical aggregate increases in nonfarm participation can produce sharply different welfare gains depending on which margin of selection the policy activates. For instance, when returns to nonfarm work and mobility costs are heterogeneous, policies that ease constraints for high-cost, high-return workers reallocate individuals with the greatest potential productivity gains, generating far larger welfare effects per worker moved. This mechanism helps explain why large infrastructure investments often yield modest aggregate welfare effects despite sizable labor reallocation, as in Asher and Novosad (2020), and highlights the importance of targeting the binding segment of the unobserved cost distribution. My findings highlight that the welfare impacts of infrastructure depend critically on who is induced to move.

These labor reallocations are best viewed as a process of structural transformation. As mobility costs fall, labor shifts from low-productivity farm activities to higher-productivity nonfarm work, raising aggregate welfare. The efficiency of this transformation depends on which segment of the unobserved cost distribution policies relax. Broad reductions mainly move workers already close to the margin, while targeted interventions that enable constrained, high-return individuals to enter nonfarm employment accelerate more efficient and inclusive structural change.

Overall, rural rail reopenings generate meaningful welfare gains, but their distribution is uneven: high-return workers remain largely excluded from the nonfarm sector. Infrastructure policies that more directly target constrained workers more effectively, could therefore deliver disproportionate gains. The findings are robust across both polynomial and normal specifications of the MTE, which yield similar magnitudes of per capita spending gains (Table A.10).

5.6 Cost-Benefit Analysis: Are Rural Rail Stations Worth Operating?

I assess whether the welfare gains from reopening rural rail stations are sufficient to justify their operating costs. Because the PRTE captures the welfare-relevant benefits of reducing mobility costs, expressing these simulated effects as a Benefit–Cost Ratio (BCR) provides a direct measure of their economic efficiency. The BCR is defined as:

$$\text{BCR} = \frac{\text{Aggregate Benefit}}{\text{Total Operating Cost}}.$$

I obtain the aggregate benefit by translating PRTE estimates into monetary gains, multiplying the simulated increase in per capita spending by the affected rural population (see Appendix 1 for data sources and construction).

Because the counterfactual concerns reopening previously built but inactive stations, the relevant expenses are operating rather than construction costs. I approximate annual operating costs using Burkina Faso rail subsidy data from the shared Côte d'Ivoire–Burkina Faso concession during the synchronized closure period (Appendix 1; Figure A.10).

I evaluate several counterfactual scenarios that vary in the extent of rural station reopening, from three rail stations to all sixteen closed stations in southern Côte d'Ivoire (Figure A.11).

Results: Diminishing Marginal Gains. Figure A.12 shows very large net benefits for limited reopenings: reopening three rural stations yields a BCR above 4 (Table A.11). These results indicate that limited rail reopenings are highly cost-effective within the existing network, with each dollar spent on operating the three reopened stations generating roughly four dollars in per capita spending gains.

For comparison, Brooks and Donovan (2020) report a BCR of 2.4 for rural footbridge projects in Nicaragua, which focus on construction costs.[26]

The cost–benefit analysis indicates that while initial reopenings generate large net benefits, marginal gains decline as network coverage expands (Figure A.12 and table A.11). This pattern reflects that nonfarm mobility costs are most binding at early stages of reopening and become progressively less so as connectivity improves. This pattern aligns with broader evidence of limited or negligible net returns to transport infrastructure in more developed or already well-connected settings (Duranton and Turner, 2012).

Table A.11 confirms that the cost–benefit results are robust across both polynomial and normal specifications of the MTE curve, which produce quantitatively similar BCR estimates and patterns of diminishing marginal gains.

5.7 Summary of Welfare and Selection Findings

In summary, the analysis shows that rural rail access influences welfare through mobility costs that shape sectoral choices and, ultimately, the pace of structural transformation. The MTE estimates reveal reverse selection: workers with the highest potential nonfarm returns are least likely to enter nonfarm work, keeping many productive individuals in farm activities. Using these estimates within the PRTE framework, I find that reducing mobility costs through rail reopenings raises average per-capita spending by about 5 percent, and by nearly 9 percent when policies target high-cost, high-return workers. These reallocations reflect an efficiency-enhancing structural transformation, as labor shifts from low- to high-productivity sectors. The cost–benefit analysis indicates that limited reopenings are highly cost-effective, with diminishing returns as coverage expands. Together, the results show that welfare gains depend less on how many workers move than on which workers are able to, highlighting the value of targeting the most constrained.

[26] In contrast, my analysis excludes construction costs since it focuses on reopening already constructed rail stations.

6 Spatial Targeting and Spillover Mechanisms

This section extends the analysis to two complementary dimensions of policy design that shape the welfare impact of rural rail investments. First, I examine where stations should be reopened when full network restoration is infeasible. Using an empirical welfare–maximization framework, I evaluate how alternative spatial allocations of limited reopenings affect aggregate welfare, highlighting the importance of targeting areas where constrained high-return workers are concentrated. Second, I incorporate intersectoral spillovers, recognizing that changes in nonfarm employment can propagate to farm outcomes through local demand and market-integration effects. Together, these extensions provide a more comprehensive view of how the design and indirect consequences of rural rail policies influence structural transformation and aggregate welfare.

6.1 Spatial Targeting of Rural Rail Reopenings

The counterfactual simulations in Section 5 showed that fully restoring rural rail service would generate sizable welfare gains by reducing sectoral mobility costs and enabling labor reallocation toward more productive nonfarm work. In practice, however, reopening every closed station might not be feasible. Budget and administrative constraints might limit the number of reopenings, shifting the policy question from *how much* to rebuild to *where* to rebuild. When resources are scarce, the planner must decide which locations to prioritize to maximize welfare.

Previous results in Section 5 indicated substantial heterogeneity in nonfarm gains across workers, with the largest benefits accruing to those least likely to work in nonfarm sectors. This heterogeneity implies scope for spatial targeting: by reopening stations in areas where constrained high-return workers are concentrated, welfare gains can be amplified. I operationalize this idea by examining how aggregate welfare varies across all feasible spatial allocations of a fixed number of reopenings. Specifically, I consider the case where only half of the closed rural stations can be reopened and ask which should be prioritized to maximize welfare.

Formally, the problem can be expressed as choosing the reopening allocation A^* hat maximizes expected welfare:

$$A^* = \arg\max \max_{A \in \mathcal{A}_{8/16}} \mathbb{E}[Y \mid A],$$

where $\mathcal{A}_{8/16}$ denotes the 12,870 possible combinations of reopening eight of the sixteen closed stations. Under standard assumptions **A.1–A.5**, this Empirical Welfare Maximization (EWM) is equivalent to maximizing the policy-relevant treatment effect, PRTE(A), which summarizes the aggregate gains induced by each spatial configuration of reopenings. Intuitively, the optimal allocation relaxes mobility constraints for the most constrained yet high-return workers, generating the largest welfare-improving reallocation from farm to nonfarm activities. A full derivation of the welfare–maximization framework in terms of PRTE and estimation algorithm is provided in Appendix 5.1.

To implement this EWM, I compute PRTE(A) for every feasible allocation $A \in \mathcal{A}_{8/16}$. Each allocation corresponds to reopening a subset of stations, updating distances to the nearest active station, and recalculating the corresponding policy weights $w^A(u)$. The resulting distribution of PRTE(A) values captures the potential welfare variation across spatial configurations (see Algorithm 5.1).

Results. The simulated welfare effects reveal large variation across alternative reopening allocations. Figure A.13 shows the distribution of estimated PRTE(A) values for all 12,870 feasible reopening allocations. Welfare gains differ markedly depending on where stations are reopened. The empirical welfare–maximizing allocation yields an average spending increase of 5.07 percent, whereas the least efficient allocation produces only 1.98 percent. Reopening the same number of stations can therefore generate more than twice the welfare gain depending solely on their spatial allocation.

Figure 3 illustrates the spatial configurations of the optimal, worst, and a randomly chosen allocation. In the worst case, reopened stations cluster in the central corridor, leaving peripheral areas disconnected. By contrast, the optimal allocation distributes reopenings more evenly across space.

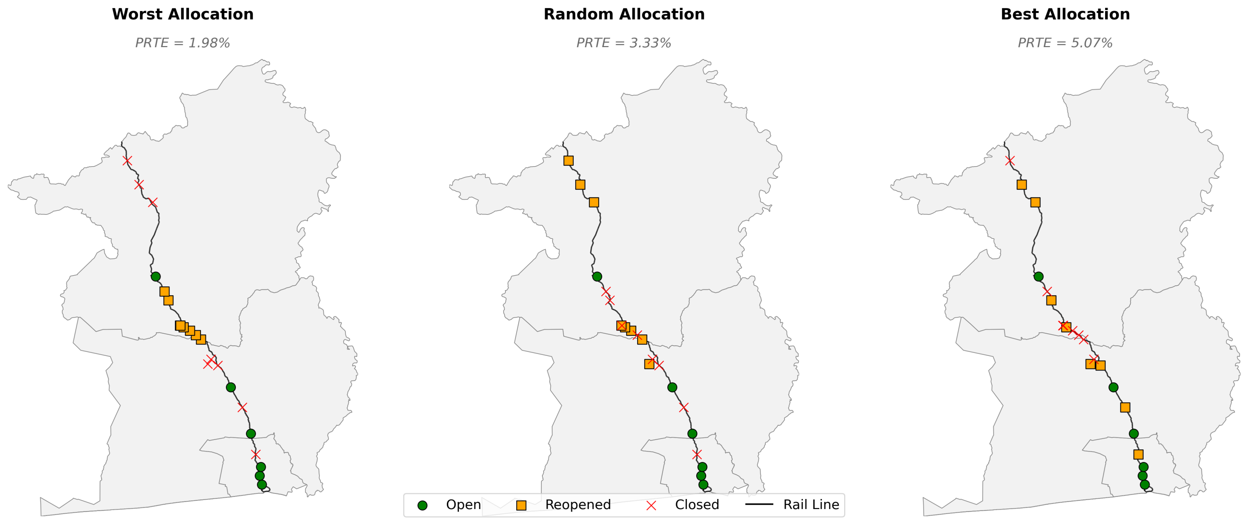


Figure 3: Worst, Random, and Optimal Station Reopening Configurations

Note: The map shows alternative reopening configurations of eight among the sixteen closed rural stations. The optimal (worst) allocation maximizes (minimizes) the estimated PRTE across all 12,870 possible combinations, consistent with the empirical welfare maximization framework in Appendix 5.1. Green circles indicate stations that were never closed; yellow squares mark stations reopened under the given allocation; and red crosses denote stations that remain closed. Peripheral areas that remain disconnected under the worst allocation gain access under the optimal allocation, which distributes reopenings more evenly across space.

To understand the mechanism behind these welfare differences, Figure A.14 compares the policy weights $w^A(u)$ under the best and worst allocations. The efficient allocation reallocates a much larger share of constrained, high-return workers from farm to nonfarm sectors, generating stronger welfare effects. The share of workers in the upper half of the expected nonfarm return distribution who switch sectors is more than three times larger under the optimal allocation. This composition effect captures more efficient structural transformation: targeted infrastructure investments enable higher-return workers to move into more productive activities, raising aggregate welfare through improved sectoral allocation.

Finally, Table A.13 compares the welfare gains and benefit–cost ratios (BCRs) across four reopening scenarios: the worst, random, and optimal allocations of half the stations, and the full network restoration. While full reopening maximizes welfare, the optimal targeted allocation achieves nearly the same gain at half the cost, implying a benefit–cost ratio almost twice as high.[27]

[27] The benefit–cost ratio for a random reopening of half the closed stations exceeds that of a full reopening, consistent

Together, these results show that spatial targeting can substantially enhance the returns to rural infrastructure by directing resources toward locations where constrained, high-return workers are concentrated. By improving the match between productive potential and access to transport, targeted reopenings magnify welfare gains even under tight budget constraints.

6.2 Spillovers from Reduced Nonfarm Sector

The spatial targeting analysis above evaluated the welfare effects of alternative reopening allocations under the maintained assumption of policy invariance, that rail policies affect welfare only through sectoral allocation. In practice, this assumption may hold only approximately. Changes in transport access can reshape local demand and input linkages between nonfarm and farm sectors, implying that the welfare effects of closures or reopenings may also operate indirectly through these spillover channels. To assess their magnitude, I use the variation in rail closures to test whether shifts in nonfarm participation generate measurable effects on farm outcomes.

In rural settings, nonfarm workers often purchase agricultural goods locally and trade them across connected markets. A contraction in nonfarm employment can therefore depress local demand for farm products, reducing farm earnings and amplifying welfare losses beyond direct reallocation.

Modeling Nonfarm-to-Farm Spillovers. To formalize this mechanism, I extend the generalized Roy model to allow sectoral outcomes to depend on aggregate changes in nonfarm employment. Specifically,

$$Y_{d,p} = Y_d + \underbrace{\beta_d \cdot (\mathbb{E}[D_p] - \mathbb{E}[D])}_{\text{Spillover Effect}},$$

where $d \in \{0, 1\}$ indexes the farm ($d = 0$) and nonfarm ($d = 1$) sectors, and p denotes a given rail policy regime. The term β_0 and β_1 capture, respectively, the indirect nonfarm-to-farm spillover and the within-nonfarm competition effect.

with the declining marginal net benefits documented in Section 5.

This extension preserves the exclusion restriction from Assumption **A.1**, since the cost shifter Z continues to affect outcomes only through sectoral participation within a given rail policy regime. However, it relaxes the policy-invariance assumption (**AS.5**), allowing sectoral outcomes to respond to aggregate shifts in nonfarm employment. The standard generalized Roy model arises as a special case with $\beta_1 = \beta_0 = 0$.

Lemma 1 *Under Assumptions **AS.1** to **AS.4** and the extended sector-specific outcome formulation, the total effect of rural rail station closures is:*

$$\underbrace{\mathbb{E}[Y \mid \textit{rail closure}] - \mathbb{E}[Y \mid \textit{no closure}]}_{\textit{Total Rail Closure Effect}} = \underbrace{\textit{PRTE}}_{\textit{Reallocation Effect}} + \underbrace{(\beta_1 \, \mathbb{E}[D_p] + \beta_0 \, \mathbb{E}[1 - D_p]) \cdot (\mathbb{E}[D_p] - \mathbb{E}[D])}_{\textit{Aggregate Spillovers}}.$$

Proof: See Appendix 5.2.

Lemma 1 shows that the total welfare effect decomposes into a direct reallocation component (the PRTE) and an aggregate spillover term capturing how changes in nonfarm employment influence sectoral outcomes.

Identification and Bounding Strategy. The components in the above equation are not jointly identified, but several are observable from the data: the total effect (the reduced-form DiD on per-capita spending), the change in nonfarm participation ($\mathbb{E}[D_p] - \mathbb{E}[D]$), and the post-closure employment shares $\mathbb{E}[D_p]$ and $\mathbb{E}[1 - D_p]$ (see Appendix 5.2). This leaves three unknowns, the PRTE, β_1, β_0, requiring an additional restriction.

I impose a standard sign restriction on the nonfarm-to-nonfarm effect, $\beta_1 \leq 0$, reflecting competition within the nonfarm sector and consistent with evidence of negative wage elasticity to labor supply (Borjas, 2003). This allows for an upper bound on the nonfarm-to-farm spillover effect.

Proposition 1 *Under Assumptions **AS.1–AS.4** and $\beta_1 \leq 0$, the nonfarm-to-farm spillover component in the extended model satisfies the following bound:*

$$\mathbb{E}[Y_{0,p}] - \mathbb{E}[Y_0] = \beta_0 \cdot (\alpha_p - \alpha) \leq \frac{(1 - c) \cdot \textit{DiD}}{1 - \alpha_p},$$

where $c = \frac{\textit{PRTE}}{\textit{Total Effect}}$, $\alpha = \mathbb{E}[D]$, $\alpha_p = \mathbb{E}[D_p]$, *and* DiD *is the reduced-form effect of rail station closures on per capita spending.*

Proof: See Appendix 5.2.

Proposition 1 provides an interpretable bound on the indirect losses for farm workers arising from the contraction of the nonfarm sector that as a function of c. Smaller c values imply greater importance of spillovers relative to direct reallocation.

Results. Table A.14 reports upper bounds for different plausible c values. Even small deviations from the no-spillover benchmark imply meaningful indirect effects on farm spending: if reallocation accounts for 90 percent of the total effect ($c = 0.9$), the implied decline in farmers' per-capita spending is at least 2.2 percent; if only half ($c = 0.5$) the reduction exceeds 11 percent.

These findings indicate that rural rail station closures likely depress welfare not only through reduced mobility but also via weakened nonfarm demand. Accounting for these intersectoral linkages reveals that transport shocks in rural economies can have aggregate consequences substantially larger than those captured by direct reallocation alone.

Implication of Potential Spillovers for Welfare Effects. The welfare analysis in Section 5 assumes sectoral spending is unaffected by reallocation, though evidence of nonfarm–to-farm spillovers suggests only approximate validity. As reopenings expand nonfarm activity and raise demand for farm goods, ignoring these linkages likely biases results downward. Reported welfare gains and benefit–cost ratios of rail reopenings are therefore conservative lower bounds.

7 Conclusion

Rural rail services remain critical infrastructure in many developing countries, particularly in areas where alternative transport options are scarce. Although often regarded as remnants of the past, these networks continue to connect isolated communities to markets and trade. Yet when budgets tighten, they are typically among the first services to be closed.

This paper shows that such closures can have substantial welfare costs. In southern Côte d'Ivoire, the abrupt closure of rural rail services after the 2011 post-presidential-election crisis led to significant declines in living standards. Using this episode as a natural experiment, I find that household per-capita spending and consumption fell by about 16 percent in affected rural areas relative to nearby controls. These declines reflect a shift of workers out of higher-paying nonfarm employment into lower-productivity family work rather than migration responses.

I rationalize these findings using a model of sectoral choice in which workers face heterogeneous returns and sectoral mobility costs. Rail access lowers these costs, enabling more workers to enter nonfarm employment. Based on estimated marginal nonfarm returns, I show that reopening all closed stations would raise average per capita spending by over five percent, with benefits exceeding operating costs for initial reopenings but declining as network coverage expands. Spatially targeted reopenings of half the closed stations generate nearly the same welfare gains as full restoration, implying a benefit–cost ratio almost twice as high.

I extend the model to account for nonfarm-to-farm spillovers that arise when nonfarm workers source part of their inputs from local farmers, creating a channel through which reduced nonfarm activity depresses farm earnings. The estimates suggest that even modest deviations from the no-spillover benchmark imply meaningful indirect declines in per-capita spending among rural farmers following rail station closures.

Although the analysis relies on data from Côte d'Ivoire, the mechanisms documented here extend to other contexts. The diminishing marginal returns to rail expansion help explain the limited welfare effects of transport investments in richer or already-connected areas (Duranton and Turner, 2012). Moreover, evidence from the model helps reconcile findings in the structural-transformation literature showing that movement out of agriculture does not necessarily translate into large welfare improvements (Asher and Novosad, 2020; Lagakos et al., 2020). My results highlight a key mechanism: welfare gains depend less on how many workers move and more on who is able to move. By emphasizing this composition margin, the paper provides a framework for understanding why similar infrastructure investments, or similar shifts out of agriculture, can produce widely different welfare effects across settings.

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1 Appendix: Data and Variable Construction

This appendix provides additional details on the construction of the variables and datasets used in the analysis. I describe the aggregation of household- and individual-level data to the subprefecture level, the construction of key outcome variables, and the steps taken to ensure comparability across survey waves. All procedures are implemented consistently across the three waves of the *Enquête Niveau de Vie* (ENV), for the years 2002, 2008, and 2015.

1.1 Aggregation and Unit of Analysis

Most reduced-form analyses of Section 4 are conducted at the subprefecture level. Subprefectures represent the lowest administrative units in Côte d'Ivoire during the study period and allow for consistent aggregation of socioeconomic outcomes across years. I aggregate household-level variables and individual-level variables separately. For household outcomes, I retain only subprefectures with at least 12 sampled households in a given survey year. For labor market outcomes, I require at least 12 sampled working-age adults (aged 15–64). This restriction ensures sufficient within-cell variation and reduces measurement noise in small subprefectures. The resulting dataset is an unbalanced panel of subprefectures across the three survey waves.

1.2 Construction of Key Variables

Household Per Capita Spending

To maintain consistency across survey years, I construct total household expenditure directly from the raw sections of the ENV. Each spending category is computed following the original questionnaire structure and recall periods, which vary across sections. I winsorize all expenditure components to limit the influence of extreme outliers before aggregation.

The construction follows these main steps:

1. **Education spending:** annual expenditures on school fees, supplies, uniforms, transportation, canteen meals, extracurricular activities, and tutoring.

2. **Health spending:** all medical expenses (consultations, medication, hospitalization, and other health-related costs) over the past three months, annualized by multiplying by four.

3. **Clothing spending:** reported annual expenditures on garments, tailoring, footwear, jewelry, and hairdressing.

4. **Personal care and leisure:** spending on hygiene products and domestic help, annualized where necessary.

5. **Transport and communication:** expenditures on routine and exceptional transport, fuel, vehicle repairs, and trips, adjusted to annual equivalents based on reported frequency.

6. **Housing:** rent, home repair, water, electricity, and cooking fuel costs. Water, electricity, and fuel expenditures are converted to annual terms using reported payment frequencies.

7. **Food:** separated into food consumed at home and food consumed outside the home, both annualized based on reported purchase frequency.

Each component is winsorized at the upper tail (typically the 99.9th percentile for all expenditure categories expect health expenditures at the 99.5th percentile) and then collapsed to the household level. Total household expenditure is the sum of all categories:

$$\begin{aligned}\text{Total household expenditure} = {} & \text{Education} + \text{Health} + \text{Clothing} + \text{Personal care} \\ & + \text{Transport} + \text{Housing} + \text{Food at home} + \text{Food outside home.}\end{aligned}$$

I then compute per capita spending by dividing total household expenditure by household size. This measure serves as a consistent indicator of household outcomes across survey waves.

Labor Market Outcomes

Labor market outcomes are derived from the individual questionnaire, specifically question *Eb.3* in the 2015 survey wave ("Quelle est votre catégorie socio-professionnelle à l'obtention de ce travail et actuellement?”). This question identifies detailed occupational categories ranging from civil servants and private employees to self-employed and agricultural workers.

I construct three mutually exclusive employment categories based on occupation codes and payment type:

1. **Nonfarm employment (*total_emp_NF*):** includes wage or self-employed work in non-agricultural activities.
   - Casual nonfarm workers: occupations 1–13 or 16, paid irregularly.
   - Regular nonfarm workers: same occupations, paid monthly.
   - Self-employed nonfarm workers: independent workers and employers (codes 14–15).
2. **Farm employment (*total_emp_F*):** includes individuals engaged in agricultural activities for pay or self-employment.
   - Casual farm workers: paid agricultural laborers (codes 18 or 20).
   - Self-employed farmers: independent agricultural producers (code 17).
3. **Contributing family work (*total_emp_FA*):** unpaid family workers (code 19), classified as casual or regular depending on payment frequency.

Each variable is coded at the individual level for those reporting any work activity during the reference year and averaged within subprefectures for the reduced-form analysis. The resulting measures capture the share of working-age adults employed in each sector.

1.3 Sample Restrictions and Weighting

All monetary variables are expressed in nominal CFA francs of the survey year. I apply the survey sampling weights provided in the ENV to maintain representativeness at the national level prior to aggregation. For subprefecture-level averages, I compute weighted means using household or individual weights as appropriate.

Individuals with missing occupational codes or employment status are omitted from the construction of labor variables. These individuals are not employed in the first place, so their exclusions do not affect representativeness.

1.4 Consistency Across Survey Waves

All variables are constructed using identical procedures across the 2002, 2008, and 2015 surveys. The ENV questionnaires maintain a consistent structure, though minor differences in item coding or recall periods are harmonized consistently based on the approach described above. Aggregation, winsorization, and labeling steps are fully reproducible and applied identically in each wave.

1.5 Cost Benefit Analysis

Aggregate Benefits

Aggregate benefits are computed as the product of the average simulated welfare gain in per capita spending and the size of the affected rural population. The monetary gain in per capita spending is obtained by multiplying the estimated Policy-Relevant Treatment Effect (PRTE) by the average baseline level of per capita spending in the treated areas in 2015. This measure captures the total increase in per capita spending that would result from reopening rural rail stations.

To estimate the population exposed to the policy, I draw on population data from the 2014 General Census of Population in Côte d'Ivoire for the three affected regions. I then scale these totals by the rural population share, as estimated from the nationally representative household survey data, to obtain the rural population in 2014. Finally, I project this figure to 2015 using official population growth rates, yielding an estimate of the affected rural population during the 2015.

Operating Costs

Because the policy counterfactual concerns reopening previously built but inactive stations, the relevant costs are operating rather than construction costs. I recover operating costs using variation in total rail subsidies before and after the closures, normalized by the number of stations. Direct data on rail subsidies for Côte d'Ivoire are not publicly available; however, I use data from Burkina Faso as a proxy for three reasons.

First, the rail line under study spans both countries and is operated by the same concessionaire under a joint bilateral agreement. Second, both countries experienced station closures during the same 2009–2011 period (Figure A.2). Third, detailed annual data on rail operating subsidies are available for Burkina Faso surrounding the closure years. Using these data, I estimate the average annual operating cost per rural station, which serves as the benchmark cost for Côte d'Ivoire (see Figure A.10).

2 Appendix: 2011 Post-Presidential-Election Crisis's Historical Background

Côte d'Ivoire, one of West Africa's fastest-growing economies after independence, experienced sustained growth averaging over 7 percent annually for two decades. Its heavy reliance on primary commodities, particularly cocoa and coffee, which accounted for more than half of exports by 2000, made it highly vulnerable to terms-of-trade shocks. A prolonged downturn in the 1980s, driven by falling export prices, eventually led to a currency devaluation in 1993.

The following decade was marked by growing political instability. A military coup in 1999 and a subsequent conflict in 2002 divided the country into a rebel-controlled north and a government-controlled south (Soumahoro, 2017). Under the Linas–Marcoussis peace agreement of 2003, this de facto partition persisted for nearly a decade, until the contentious presidential election of 2010.

The 2010 election was held in two rounds and produced unexpected results. In the first round (October 2010), the incumbent president led with 38.3 percent of the vote, followed by the main opposition candidate with 32.1 percent. In the second round (November 2010), the opposition candidate formed a broad coalition with the third-largest political party. When results were announced, the country's institutions split: the Independent Electoral Commission declared the opposition the winner (54.1 percent), while the Constitutional Council declared the incumbent the winner (51.45 percent).

With both candidates claiming victory, Côte d'Ivoire descended into a brief but intense post-presidential-election crisis between December 2010 and April 2011. Violence was highly localized: three areas, the economic capital Abidjan (49.6 percent of all deaths), the Cavally region (30.3 percent), and the city of Duékoué (11.2 percent), accounted for more than 90 percent of total fatalities (Léon and Dosso, 2020). These three areas are excluded from my analysis sample (see Section 4).

The conflict caused short-term disruption across major economic sectors. During the 2011 crisis, GDP contracted by 5.4 percent, but growth rebounded quickly, averaging over 5 percent annually from 2012 until the COVID period. Land borders were closed, and the national rail network was fully suspended between January and April 2011. Freight operations resumed in late April and intercity passenger services in June, but rural rail stations along the southern corridor remained closed.

Figure A.2 illustrates the geographic contraction of rail access: all rural stations between Abidjan and Dimbokro closed permanently after the crisis. Of the 28 passenger stations operating before 2011, only 10 reopened, all in major urban centers. The remaining 18 stations, 16 of them in southern rural regions, remained closed. Freight volumes recovered quickly, but passenger traffic fell by more than 50 percent, confirming the selective impact of the closures (Figure A.1).

The closures were abrupt, exogenous to local economic conditions, and geographically concentrated. They were not driven by declining demand but became possible following the sudden change in government. This makes the 2011 crisis a natural experiment for studying how the loss of rural rail access affected household welfare and labor allocation in previously connected communities.

3 Appendix Tables

Table A.1: Summary Statistics

	2002		2008		2015	
	Treatment	Control	Treatment	Control	Treatment	Control
Household head characteristics						
Age (years)	46.2	43.3	44.7	42.3	42.7	38.8
	15.990	*15.080*	*15.886*	*14.019*	*15.304*	*13.656*
Female (%)	20.6	11.4	20.0	9.0	24.0	13.9
	0.404	*0.318*	*0.400*	*0.286*	*0.427*	*0.346*
Credit access (%)	–		19.9	21.0	12.8	10.7
	–		*0.400*	*0.408*	*0.334*	*0.310*
Migrant (internal/external, %)	4.0	4.8	16.4	15.5	–	
	0.197	*0.213*	*0.371*	*0.362*	–	
Household outcomes (log)						
Consumption pc (XOF)	12.51	12.38	12.60	12.50	12.76	12.76
	0.684	*0.727*	*0.735*	*0.701*	*0.782*	*0.748*
Spending pc (XOF)	11.90	11.83	12.06	12.01	12.22	12.28
	0.759	*0.821*	*0.933*	*0.840*	*0.971*	*0.934*
Working-age adults outcomes						
Working (%)	71.9	71.9	73.9	71.2	65.9	64.1
	0.450	*0.450*	*0.439*	*0.453*	*0.474*	*0.480*
Non-farm workers (%)	19.6	16.0	27.6	22.6	27.0	30.6
	0.397	*0.366*	*0.447*	*0.418*	*0.444*	*0.461*
Farm workers (%)	45.8	43.0	51.4	45.1	44.0	41.0
	0.498	*0.495*	*0.500*	*0.498*	*0.497*	*0.492*
Contributing family workers (%)	34.3	40.6	18.4	29.7	18.8	18.9
	0.475	*0.491*	*0.387*	*0.457*	*0.391*	*0.392*
Sample size						
Households	980	2,561	1,000	2,480	1,534	2,406
Working-age adults (15–64)	2,862	7,289	2,488	6,976	2,761	4,562
Subprefecture	23	29	27	41	30	41
Minimum # of households	20	20	20	20	12	12
Average # of households	43	88	37	60	50	57

Notes: Means are reported with standard deviations in italics below each estimate. "Treatment" refers to rural households located in southern regions historically served by rail stations before 2011; "Control" refers to rural households in southern regions that were never served by rail. All values are survey-weighted. Monetary figures are expressed in XOF (CFA Franc). Credit access data were not collected in 2002, and migration data were not collected in 2015.

Table A.2: Effects of Rural Rail Station Closures on per capita Spending and Consumption

	(1) Spending pc (log)	(2) Consumption pc (log)
Panel A: Pre-treatment (2002 vs. 2008)		
c.treatment × year=2008	-0.017	0.022
	(0.1303)	(0.1113)
year=2008	0.157^{**}	0.068
	(0.0648)	(0.0646)
Constant	11.883^{***}	12.472^{***}
	(0.0306)	(0.0268)
Observations	88	88
R-squared	0.126	0.049
Number of subprefectures	44	44
Subprefecture FE	YES	YES
Treated average in 2008	12.05	12.62
Panel B: Full period (2002, 2008, 2015)		
c.treatment × year=2015	-0.161^{*}	-0.157^{*}
	(0.0894)	(0.0846)
year=2008	0.155^{***}	0.072
	(0.0564)	(0.0490)
year=2015	0.487^{***}	0.396^{***}
	(0.0619)	(0.0527)
Constant	11.867^{***}	12.455^{***}
	(0.0341)	(0.0275)
Observations	189	189
R-squared	0.371	0.335
Number of subprefectures	75	75
Subprefecture FE	YES	YES
Treated average in 2015	12.21	12.76

Notes: Each column reports estimates from the baseline Difference-in-Differences specification in Equation 3.1. The dependent variables are the natural logarithms of household per capita spending and per capita consumption, respectively. The variable *post* equals 1 for 2015 (after the rural rail station closures) and 0 for 2002–2008 for Panel B, and it equals 1 for 2008 and 0 for 2002 for the pre-trend panel A. *treat* equals 1 for subprefectures located in southern regions historically traversed by the rail line and affected by rural station closures (Abidjan–Lagunes–Lacs), and 0 for rural subprefectures in adjacent southern regions never served by rail (Bas-Sassandra, Comoé, Gôh-Djiboua, Sassandra-Marahoué, and Yamoussoukro). All regressions include subprefecture and survey-year fixed effects, and are weighted using household sampling weights. Standard errors, clustered at the subprefecture level, are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.3: Spatial Heterogeneity: Below vs. Above Median Distance to Nearest Rail Stations

	(1) Spending pc (log)	(2) Consumption pc (log)
Panel A: Pre-treatment (2002 vs. 2008)		
Below median × year=2008	-0.068	-0.062
	(0.2097)	(0.1701)
Above median × year=2008	0.039	0.115
	(0.1099)	(0.0942)
year=2008	0.157^{**}	0.068
	(0.0652)	(0.0649)
Constant	11.883^{***}	12.472^{***}
	(0.0306)	(0.0266)
Observations	88	88
R-squared	0.132	0.075
Number of subprefectures	44	44
Subprefecture FE	YES	YES
Below median treated average in 2008	12.11	12.69
Above median treated average in 2008	11.98	12.53
Panel B: Full period (2002, 2008, 2015)		
Below median × year=2015	-0.269^{***}	-0.275^{***}
	(0.0984)	(0.1035)
Above median × year=2015	-0.047	-0.033
	(0.1203)	(0.1028)
year=2008	0.156^{***}	0.073
	(0.0567)	(0.0492)
year=2015	0.487^{***}	0.397^{***}
	(0.0621)	(0.0529)
Constant	11.865^{***}	12.453^{***}
	(0.0336)	(0.0265)
Observations	189	189
R-squared	0.387	0.358
Number of subprefectures	75	75
Subprefecture FE	YES	YES
Below median treated average in 2015	12.19	12.72
Above median treated average in 2015	12.24	12.79

Notes: Each column reports estimates from an adapted version of the Difference-in-Differences specification in Equation 3.1, where two treatment indicators are included to distinguish treated subprefectures located below and above the median distance to the nearest rail station. The dependent variables are the natural logarithms of household per capita spending and per capita consumption, respectively. In Panel A (*Pre-treatment*), the variable *post* equals 1 for 2008 and 0 for 2002; in Panel B (*Full period*), *post* equals 1 for 2015 (after the rural rail station closures) and 0 for 2002–2008. "Below median" and "Above median" thus capture differential post-closure effects among treated subprefectures that were, respectively, closer to or farther from the nearest rail station prior to 2011. All regressions include subprefecture and survey-year fixed effects and are weighted using household sampling weights. Standard errors, clustered at the subprefecture level, are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.4: Population and Demographic Characteristics: Pre- and Post-Treatment

	(1) Population	(2) Population (log)	(3) Age	(4) Female
Panel A: Pre-treatment (2002 vs. 2008)				
c.treatment × year=2008	0.190	0.023	0.000	0.037**
	(0.2182)	(0.2462)	(0.0351)	(0.0154)
year = 2008	−0.225	0.129	−0.011	−0.026***
	(0.1686)	(0.1849)	(0.0213)	(0.0095)
Constant		11.166***		0.498***
		(0.0632)		(0.0038)
Observations	88	88	88	88
R-squared		0.028		0.158
Number of subprefectures	44	44	44	44
Subprefecture FE	YES	YES	YES	YES
Treated average in 2008	76,142.91	11.08	23.52	0.51
Panel B: Full period (2002, 2008, 2015)				
c.treatment × year=2015	0.099	0.081	−0.003	0.004
	(0.1716)	(0.1979)	(0.0272)	(0.0138)
year = 2008	−0.107	0.187	−0.007	−0.012
	(0.1323)	(0.1204)	(0.0167)	(0.0079)
year = 2015	−0.326*	−0.093	0.061***	−0.023**
	(0.1804)	(0.1758)	(0.0188)	(0.0095)
Constant		11.094***		0.497***
		(0.0812)		(0.0051)
Observations	185	189	185	189
R-squared		0.045		0.058
Number of subprefectures	71	75	71	75
Subprefecture FE	YES	YES	YES	YES
Treated average in 2015	65,950.01	10.87	25.31	0.49

Notes: Each column reports estimates from the baseline Difference-in-Differences specification in Equation 3.1, applied to demographic outcomes at the subprefecture level. The dependent variables are total population (proxied by the sum of survey weights), the natural logarithm of population, average individual age, and the share of females, respectively. In Panel A (*Pre-treatment*), the variable *post* equals 1 for 2008 and 0 for 2002; in Panel B (*Full period*), *post* equals 1 for 2015 (after the rural rail station closures) and 0 for 2002–2008. *treat* equals 1 for subprefectures located in southern regions historically traversed by the rail line and affected by rural station closures (Abidjan–Lagunes–Lacs), and 0 for rural subprefectures in adjacent southern regions never served by rail (Bas-Sassandra, Comoé, Gôh-Djiboua, Sassandra-Marahoué, and Yamoussoukro). All regressions include subprefecture and survey-year fixed effects and are weighted using household sampling weights. Standard errors, clustered at the subprefecture level, are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.5: Potential Mechanisms: Labor Market Outcomes

	(1) Working	(2) Hours	(3) Earnings	(4) Nonfarm	(5) Farm	(6) Family work
Panel A: Pre-treatment (2002 vs. 2008)						
c.treatment × year=2008	0.065	0.013	0.295	0.032	0.033	−0.062
	(0.0580)	(0.0660)	(0.2848)	(0.0688)	(0.0767)	(0.0657)
year = 2008	−0.028	−0.112***	0.612***	0.040	0.040	−0.108**
	(0.0387)	(0.0357)	(0.1813)	(0.0380)	(0.0461)	(0.0432)
Constant	0.722***			0.208***	0.442***	0.344***
	(0.0144)			(0.0164)	(0.0186)	(0.0163)
Observations	88	88	88	88	88	88
R-squared	0.029			0.065	0.051	0.299
Number of subprefectures	44	44	44	44	44	44
Subprefecture FE	YES	YES	YES	YES	YES	YES
Treated average in 2008	0.74	40.03	332.34	0.31	0.50	0.16
Panel B: Full period (2002, 2008, 2015)						
c.treatment × year=2015	−0.004	−0.036	−0.238	−0.094**	0.006	0.099***
	(0.0396)	(0.0396)	(0.2903)	(0.0409)	(0.0397)	(0.0347)
year = 2008	0.012	−0.126***	0.766***	0.055*	0.042	−0.116***
	(0.0282)	(0.0293)	(0.1395)	(0.0302)	(0.0338)	(0.0300)
year = 2015	−0.036	−0.200***	1.411***	0.156***	−0.033	−0.217***
	(0.0303)	(0.0268)	(0.2393)	(0.0335)	(0.0303)	(0.0297)
Constant	0.708***			0.199***	0.453***	0.341***
	(0.0197)			(0.0181)	(0.0192)	(0.0190)
Observations	189	185	185	189	189	189
R-squared	0.045			0.164	0.066	0.318
Number of subprefectures	75	71	71	75	75	75
Subprefecture FE	YES	YES	YES	YES	YES	YES
Treated average in 2015	0.68	35.09	430.26	0.29	0.43	0.18

Notes: Each column reports estimates from the baseline Difference-in-Differences specification in Equation 3.1, applied to labor market outcomes for the working-age population (15–64 years old). The dependent variables are: the share of individuals working (column 1), average weekly hours worked (column 2), average hourly earnings in local currency units (column 3), and the shares of workers employed in nonfarm, farm, and contributing-family work (columns 4–6, respectively). In Panel A (*Pre-treatment*), the variable *post* equals 1 for 2008 and 0 for 2002; in Panel B (*Full period*), *post* equals 1 for 2015 (after the rural rail station closures) and 0 for 2002–2008. *treat* equals 1 for subprefectures located in southern regions historically traversed by the rail line and affected by rural station closures (Abidjan–Lagunes–Lacs), and 0 for rural subprefectures in adjacent southern regions never served by rail (Bas-Sassandra, Comoé, Gôh-Djiboua, Sassandra-Marahoué, and Yamoussoukro). All regressions include subprefecture and survey-year fixed effects and are weighted using household sampling weights. Standard errors, clustered at the subprefecture level, are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.6: Descriptive Statistics (Working-Age Adults, Treatment Group, 2015)

	Nonfarm		Farm		Contributing family work	
	Mean	SD	Mean	SD	Mean	SD
Assortative matching (share within column)						
Nonfarm			0.108	0.311	0.101	0.302
Farm	0.190	0.393			0.640	0.481
Family work	0.064	0.246	0.236	0.425		
Working-age characteristics						
Age	36.0	11.148	40.1	11.939	30.6	11.157
Female	0.46	0.499	0.24	0.427	0.63	0.483
log spending pc	12.5	0.946	12.1	0.886	11.9	0.776
Sample size						
N	424		832		370	

Notes: Means and Standard Deviations (SD) reported for working-age adults in the treatment group in 2015. Estimates are survey-weighted. "Assortative matching" are shares within each employment-type column.

Table A.7: Balance Test for Treatment Group in 2015: Correlation with Instrument

	(1) Balance test
Age of person	0.000
	(0.0007)
Individual is female	-0.005
	(0.0195)
Never been to school	0.064*
	(0.0339)
log(Household size)	0.026
	(0.0229)
Number of rooms	0.009
	(0.0146)
Dwelling is apartment	0.022
	(0.0628)
Constant	0.161**
	(0.0686)
Observations	1,804
R-squared	0.012

Notes: Each coefficient reports the estimated correlation between the instrument $Z = \Delta \log(\mathrm{Dist}_S)$—the change in log distance from a subprefecture to its nearest rail station following the 2011 closures—and observable individual or household characteristics in the 2015 treated regions. Estimates are obtained from simple cross-sectional OLS regressions weighted by survey sampling weights. A lack of statistically significant coefficients indicates that the instrument is uncorrelated with pre-existing socioeconomic characteristics, supporting its exogeneity. Standard errors, clustered at the Primary Sampling Unit level, are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.8: Placebo Estimates for Control Group in 2015

	(1) First Stage	(2) Reduced Form
Instrument: log difference in distance	-0.117	1.657
	(0.7650)	(1.3725)
Observations	2,898	2,898
R-squared	0.040	0.230

Notes: Each column reports placebo estimates from the linear IV specification applied to individuals residing in control subprefectures in 2015; areas that were never served by rail stations. The dependent variable is log per capita spending, and the endogenous regressor is the choice of working in the nonfarm sector. The instrument is the survey-weighted mean log difference in distance to the nearest rail station before and after the 2011 closures, computed at the subprefecture level. All regressions include controls for initial (2008) log distance to the nearest rail station and its square, individual characteristics (age, gender, education), and household characteristics (log household size, number of rooms, apartment dwelling), as well as interactions of each control with initial log distance. Estimates are weighted using survey sampling weights. Standard errors, clustered at the Primary Sampling Unit level, are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.9: IV Estimates: Effect of Non-Farm Employment on Per Capita Spending in 2015

	(1) OLS	(2) First Stage	(3) Reduced Form	(4) IV
Non-farm employment share	0.319***			1.289**
	(0.0621)			(0.6197)
Instrument: log difference in distance		-0.145***	-0.187**	
		(0.0358)	(0.0853)	
Observations	1,804	1,804	1,804	1,804
R-squared	0.322	0.098	0.305	0.118

Notes: Each column reports estimates from the linear instrumental-variable specification described in Section 5, estimated for working-age adults residing in treated areas in 2015. The dependent variable is log per capita spending, and the endogenous regressor is the choice of working in the nonfarm sector. The instrument is the survey-weighted mean log difference in distance to the nearest rail station before and after the 2011 closures, computed at the subprefecture level. All regressions include controls for initial (2008) log distance to the nearest rail station and its square, individual characteristics (age, gender, education), and household characteristics (log household size, number of rooms, apartment dwelling), as well as interactions of each control with initial log distance. Estimates are weighted using survey sampling weights. Standard errors, clustered at the Primary Sampling Unit level, are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.10: Average Benefit of Reopening Rural Railway Stations

	Actual re-openings			**Counterfactual:reverse targeting**		
Model	**Estimate**	**90% LB**	**90% UB**	**Estimate**	**90% LB**	**90% UB**
Normal model	0.0527	0.0267	0.3033	0.1173	0.0119	0.4197
Polynomial model	0.0528	0.0283	0.3045	0.0945	0.0228	0.3978

Notes: This table presents PRTE estimates of re-opening all rural railway stations in treated areas in 2015. Confidence intervals are bootstrapped using a Bayesian bootstrap with 500 replications, clustered at the Primary Sampling Unit level. Estimates are based on the PRTE formula provided in Section 5.

Table A.11: Benefit–Cost Ratios (BCR) of Reopening Rural Rail Stations under Alternative Counterfactuals

Model	**Counterfactual (Stations Reopened)**	**BCR Estimate**	**90% LB**	**90% UB**
Normal	3 stations	4.20	2.63	43.39
	5 stations	3.15	1.67	26.62
	7 stations	2.65	1.57	19.21
	9 stations	2.25	1.28	15.11
	11 stations	2.06	1.07	12.56
	13 stations	1.75	0.91	10.63
	16 stations (all)	1.51	0.77	8.69
Polynomial	3 stations	4.18	2.85	43.71
	5 stations	3.20	1.91	26.71
	7 stations	2.67	1.68	19.38
	9 stations	2.25	1.34	15.17
	11 stations	2.07	1.16	12.56
	13 stations	1.75	0.98	10.63
	16 stations (all)	1.51	0.81	8.73

Notes: The table reports Benefit-Cost Ratios (BCRs) computed under both Normal and Polynomial PRTE models, across alternative counterfactual scenarios in which 3–16 rural stations are re-opened. The BCR compares the aggregate welfare gains from rural rail station reopenings to the estimated annual operating costs per station. Aggregate benefits are calculated as the product of the estimated PRTE and the average baseline level of per capita spending in treated areas in 2015, scaled by the affected rural population. Operating costs are proxied using government rail subsidy data from Burkina Faso, which shares the same concessionaire and closure timeline as Côte d'Ivoire (see Figure A.10). All confidence intervals are 90% credible intervals based on a Bayesian bootstrap with 500 replications, clustered at the Primary Sampling Unit level. BCRs above 1 indicate that the welfare gains exceed operating costs; reported BCRs remain statistically greater than 1 at the 10% level especiall for small reopenings.

Table A.12: Average Benefits under Optimal and Worst Reopening Spatial Allocations

Allocation Type	**Estimate**	**90% LB**	**90% UB**
Optimal allocation	0.0507	0.0273	0.3025
Worst allocation	0.0198	0.0026	0.0234

Notes: This table reports the estimated PRTE under the optimal and worst spatial allocations of rural rail station reopenings, based on the polynomial model specification. The optimal (worst) allocation corresponds to the configuration that maximizes (minimizes) the PRTE across all 12,870 possible reopening combinations, consistent with the empirical welfare maximization framework in Section 5. Confidence intervals are constructed using Bayesian cluster-robust bootstrapped standard errors with 500 replications, clustered at the Primary Sampling Unit level. Estimates represent the average percentage increase in per capita spending relative to the baseline of no closure regime.

Table A.13: Comparative Welfare Gains and Benefit–Cost Ratios Across Reopening Scenarios

	(1) Worst Allocation	(2) Random Allocation	(3) Optimal Allocation	(4) Full Reopening
Budget Share (relative to full)	0.5	0.5	0.5	1.0
Average Welfare Gain (%)	1.98	3.33	5.07	5.28
[90% CI]	[0.26; 2.34]	[0.13; 3.58]	[2.73; 30.25]	[2.83; 30.44]
Benefit–Cost Ratio (BCR)	1.14	1.91	2.90	1.51
[90% CI]	[0.15; 1.34]	[0.07; 2.05]	[1.56; 17.34]	[0.81; 8.73]
Relative Efficiency (vs. Full)	0.75	1.26	1.92	1.00

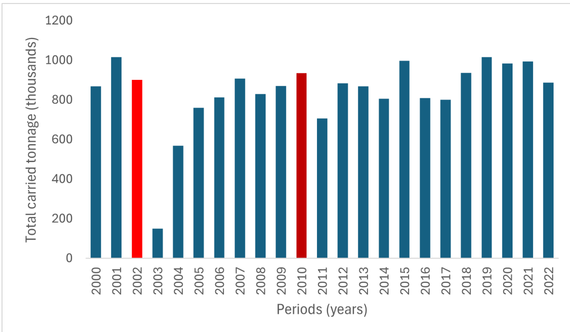
Note: This table compares welfare gains and benefit–cost ratios (BCRs) across four reopening scenarios: the worst, random, and optimal allocations of half the closed stations, and full network restoration. Each entry reports the average per-capita spending gain relative to the baseline, that is, the PRTE estimates using the polynomial specification. 90% confidence intervals, shown in brackets, are computed using Bayesian cluster-robust bootstrap standard errors with 500 repetitions. BCRs are defined as the ratio of welfare gain to total reopening operating cost, while Relative Efficiency measures each allocation's BCR relative to that of the full reopening scenario.

Table A.14: Upper Bounds on Nonfarm-to-Farm Spillover Effects under Alternative Values of c

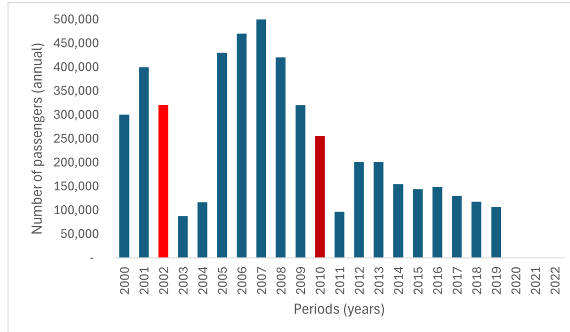
Scenario: $c =$	$\hat{\beta}_0 \cdot (\alpha_p - \alpha)$ (UB, %)	**SE**
1.00	0.00	0.000
0.90	-2.21^*	0.012
0.80	-4.41^*	0.024
0.70	-6.62^*	0.037
0.60	-8.82^*	0.049
0.50	-11.03^*	0.061
0.40	-13.23^*	0.073
0.30	-15.44^*	0.086
0.20	-17.64^*	0.098
0.10	-19.85^*	0.110
0.00	-22.05^*	0.122

Notes: The table reports upper-bound estimates of the nonfarm-to-farm spillover component, $\hat{\beta}_0 \cdot (\alpha_p - \alpha)$, expressed as a percentage change in per capita spending. The bounds are derived from Proposition 1, under the restriction $\beta_1 \leq 0$, which reflects a competitive mechanism in which an expansion in nonfarm employment reduces average per capita spending within the nonfarm sector. The parameter $c = \frac{\text{PRTE}}{\text{Total Effect}}$ captures the share of the total effect of rail station closures attributable to direct reallocation rather than spillovers. When $c = 1$, all observed effects are due to reallocation (no spillovers); as c decreases, the implied spillover losses for farm households increase. Standard errors are computed via the delta method. An asterisk (*) indicates that the bound is statistically different from zero at the 10% level.

4 Appendix Figures



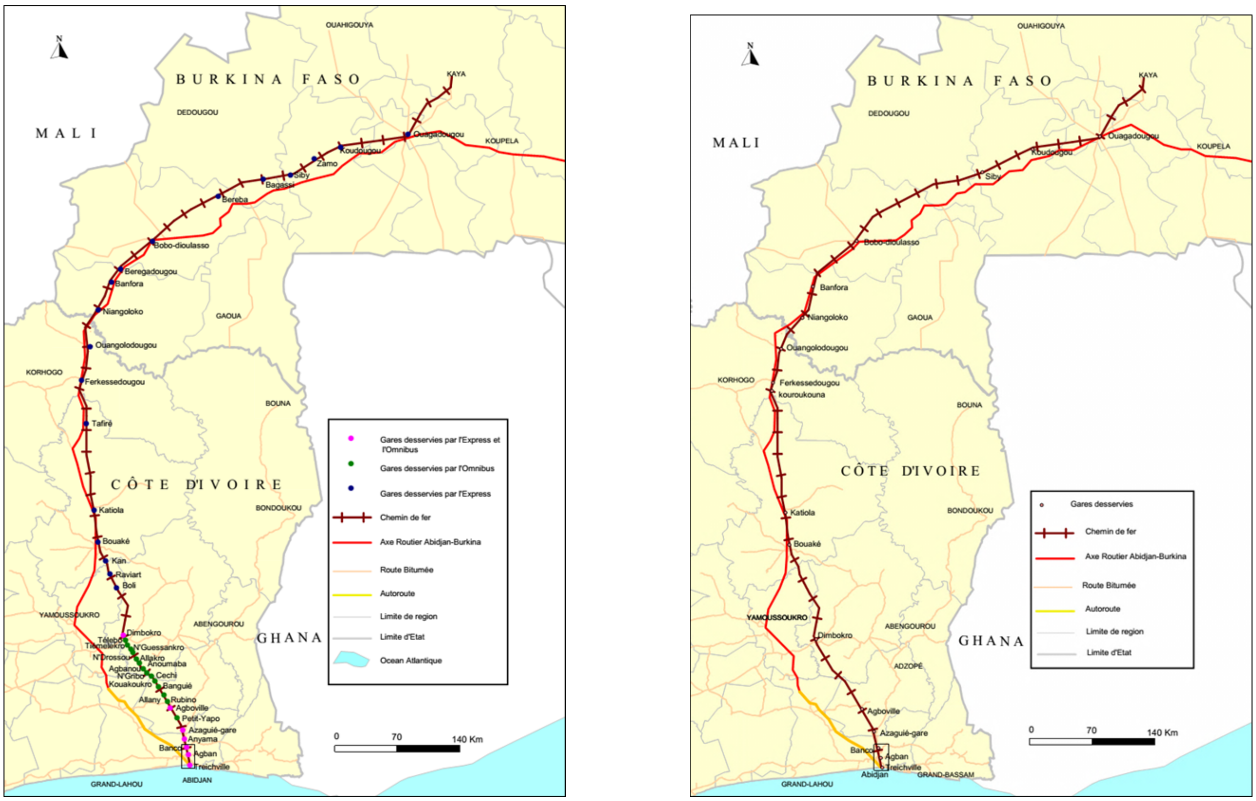
(a) Annual total carried tonnage



(b) Number of passengers

Figure A.1: Total Amount of Freight and Number of Passengers Carried from 2000 to 2015

Note: These figures are derived from varied sources, including INSD-BURKINA FASO 2024, ANStat Côte d'Ivoire, and Economie Ivoirienne. The figures present the annual total carried tonnage of freight on the railway line as well as the number of passengers each year from 2000 to 2022. The red bars represent period of insurection in Côte d'Ivoire, with (late) 2002 representing the rebel attack and (late) 2010 representing the post-electoral crisis. Note that the government in southern Côte d'Ivoire and the rebels in the rest of the country were at odds since the begining of the rebellion in 2002 until they reached an agreement in 2005 at peace talks in Pretoria to end the war. Also, while the presidential election was actually held in 2010, it was initially scheduled for 2008, and then for 2009, but was postponed twice. These delays were due to disputes over the voter list and some limited violent protests. The uncertainty surrounding the presidential election during the two years prior to the actual election date could explain the decreasing trend that began two years earlier.



(a) 2009

(b) 2011

Figure A.2: Rural Rail Stations Before and After 2010

Note: These figures show the number of rail stations that were open before and after the 2010 post-presidential-election crisis. The data reveals that 18 of all existing rail stations were closed following the crisis, with 16 of these closures occurring in southern Côte d'Ivoire.
Source: Dagnogo (2014)

(a) Women waiting for local products

(b) Women selling local agricultural products

Figure A.3: Small-Scale Trading Around the Rural Rail Services in Côte d'Ivoire

Source: Lombard and Ninot (2012)

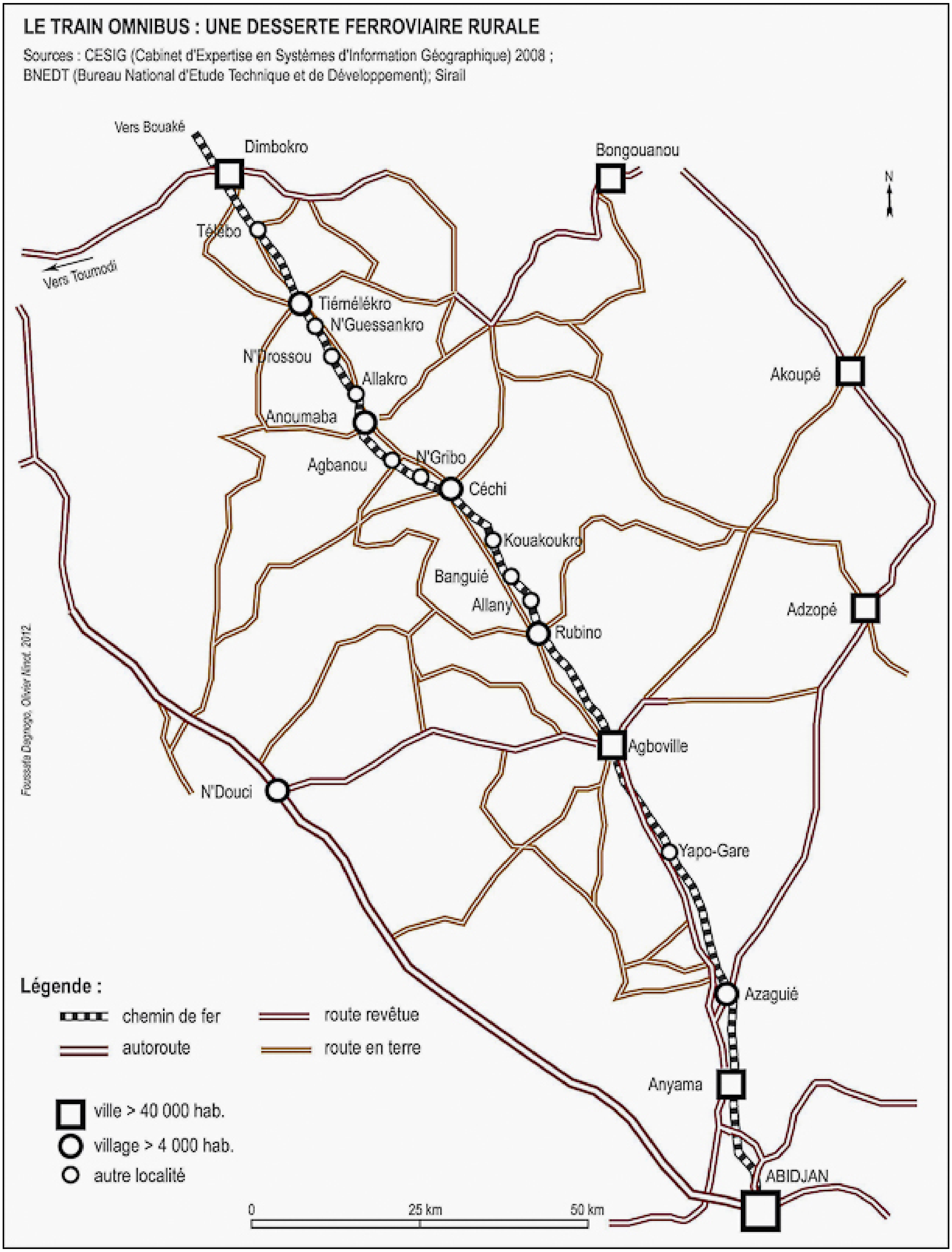
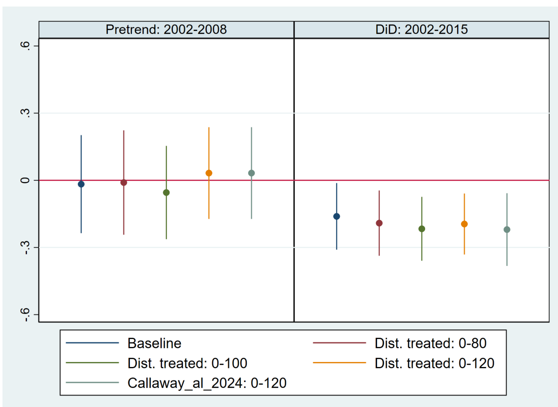
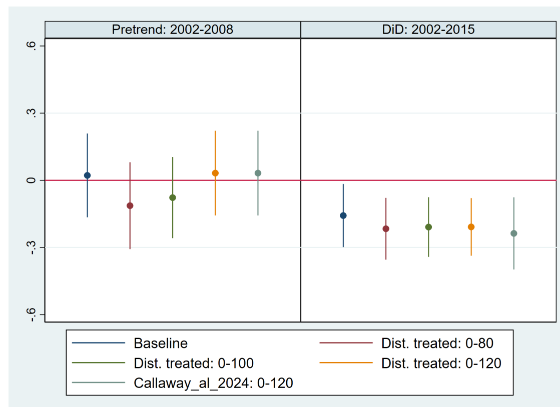


Figure A.4: The Local Rural Rail Service in Southern Côte d'Ivoire

Source: Dagnogo et al. (2012)



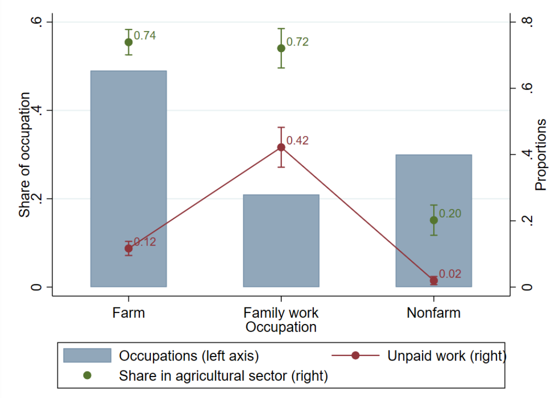
(a) Spending per capita



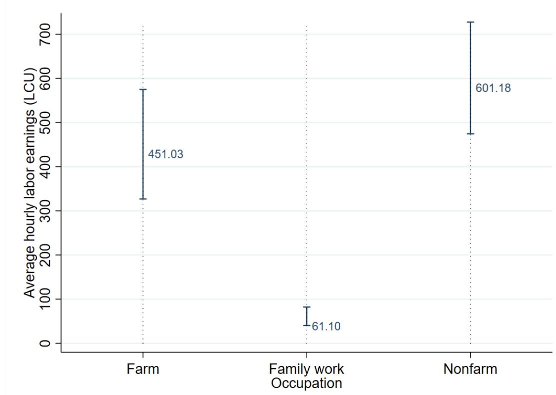
(b) Consumption per capita

Figure A.5: Robustness of DiD Estimates to Alternative Distance-Based Treatment Definitions and Continuous Treatment Intensity

Note: Each figure shows robustness checks for the baseline DiD estimates reported in Section 4. Estimates correspond to alternative definitions of exposure to rural rail station closures. The specifications labeled "Dist. treated: 0–80," "0–100," and "0–120" redefine treatment by distance to the nearest closed station, while "Callaway_al_2024: 0–120" implements the continuous-intensity estimator of Callaway et al. (2024), where exposure varies with proximity. Vertical bars represent 90% confidence intervals. Results are survey-weighted, and standard errors are clustered at the subprefecture level.



(a) Occupations and non-paying workers



(b) Average hourly wage per occupation

Figure A.6: Stylized Facts: Nonfarm, Farm, and Contributing Family Work

Note: The left panel shows the share of workers across nonfarm, farm, and contributing-family occupations, together with the share of individuals receiving no payment within each group. The right panel reports average hourly earnings by occupation. All estimates are survey-weighted and based on working-age adults in treated regions in 2015.

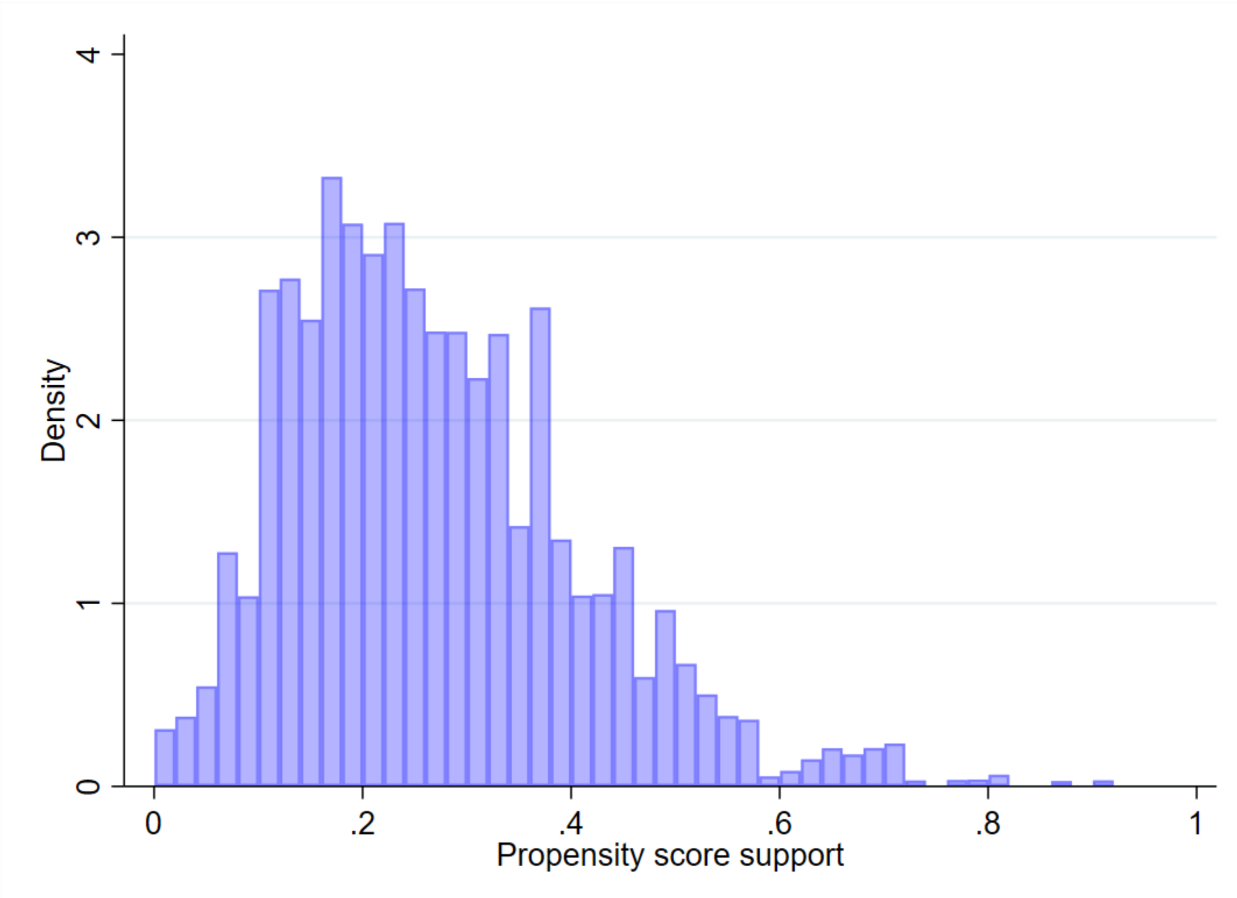
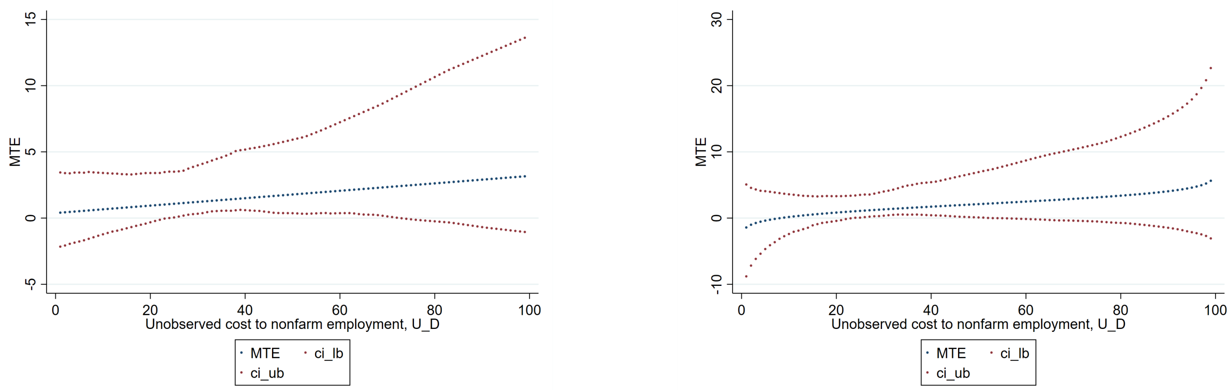


Figure A.7: Distribution of Estimated Propensity Scores

Note: The figure plots the distribution of the estimated propensity scores from the first-stage probit regression used in the MTE estimation. The sample includes all working-age adults with employment in treated regions in 2015. The estimated propensity scores exhibit substantial variation across individuals, with support ranging from 0 to 0.91, covering nearly the full $[0, 1]$ interval. Fewer than 10% of the MTE values therefore require extrapolation beyond the observed support.

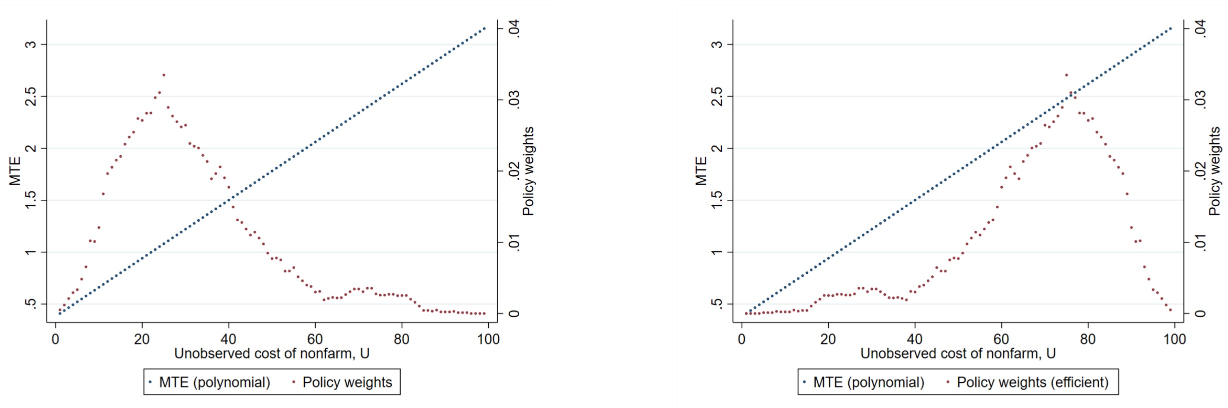


(a) MTE curve, polynomial model

(b) MTE curve, normal model

Figure A.8: MTE Curves for (Log) Per Capita Spending

Note: The plots depict the MTE curves for (log) per capita spending and evaluated at mean values of the covariates. The MTE is estimated based on the local IV approach (Heckman and Vytlacil, 2007), as described in Section 5. The 95 percent confidence interval is based on Bayesian cluster-robust bootstrapped standard errors with 500 repetitions. Standard errors are clustered at the Primary Sampling Unit of the survey data.



(a) Actual counterfactual of re-opening

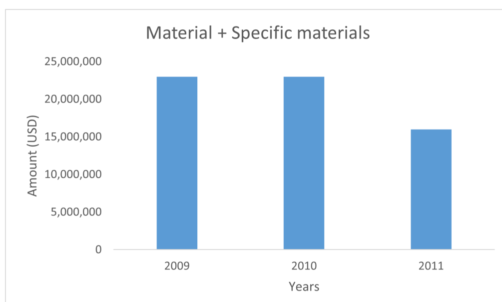
(b) Counterfactual with reverse targeting

Figure A.9: Policy Weights, $w(u)$, from Reopening Rural Rail Stations

Note: The plots depict the distribution of the policy weights, $w(u)$, associated with reopening all rural rail stations that were previously closed. The policy weights are estimated as the difference in the cumulative distribution function (CDF) of the propensity score, evaluated when $Z = 0$ and when Z equals its original value. The left-hand figure shows the actual policy weights, while the right-hand figure shows the reverse policy weights.

Figure A.10: Government Operating Subsidies to Rural Rail Stations

- Operating costs are estimated from Burkina Faso and used as a proxy for Côte d'Ivoire:
  - The considered rail line spans both countries and is operated by the **same concessionaire** under joint agreements.
  - Both countries experienced rural station closures during the same period (2009–2011).
  - Detailed rail operating subsidy data are available for Burkina Faso (BFA) and provide a credible proxy for Côte d'Ivoire (CIV).



- Between 2009–2011, **four rural stations** were closed in Burkina Faso.
- ⇒ **Estimated operating subsidy per rural station: 1.75 million USD/year.**

Notes: The figure reports annual government operating subsidies to the rail network in Burkina Faso, obtained from the World Bank BOOST database. These data are used to proxy the operating costs of rural railway stations in Côte d'Ivoire, as both countries share the same concessionaire and experienced parallel station closures during 2009–2011.

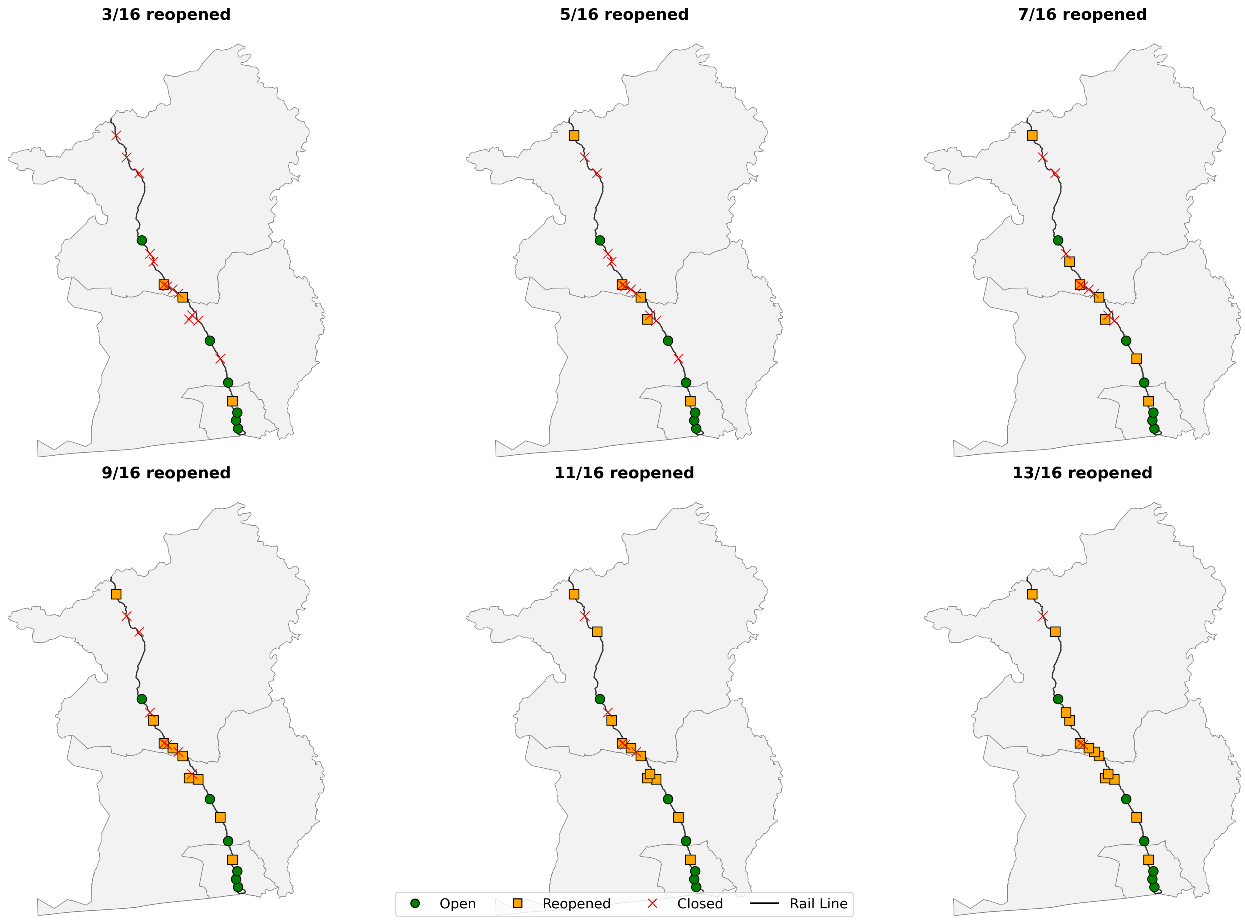


Figure A.11: Counterfactual Reopening of Rural Rail Stations

Note: The figure shows a 3×2 map comparing incremental reopening of rural rail stations in the treated areas, ranging from three to thirteen reopened stations, as described in Section 5. The full reopening of all stations, although included in the counterfactual analysis, is not displayed. Green circles indicate stations that were never closed; yellow squares mark stations reopened under the given allocation; and red crosses denote stations that remain closed.

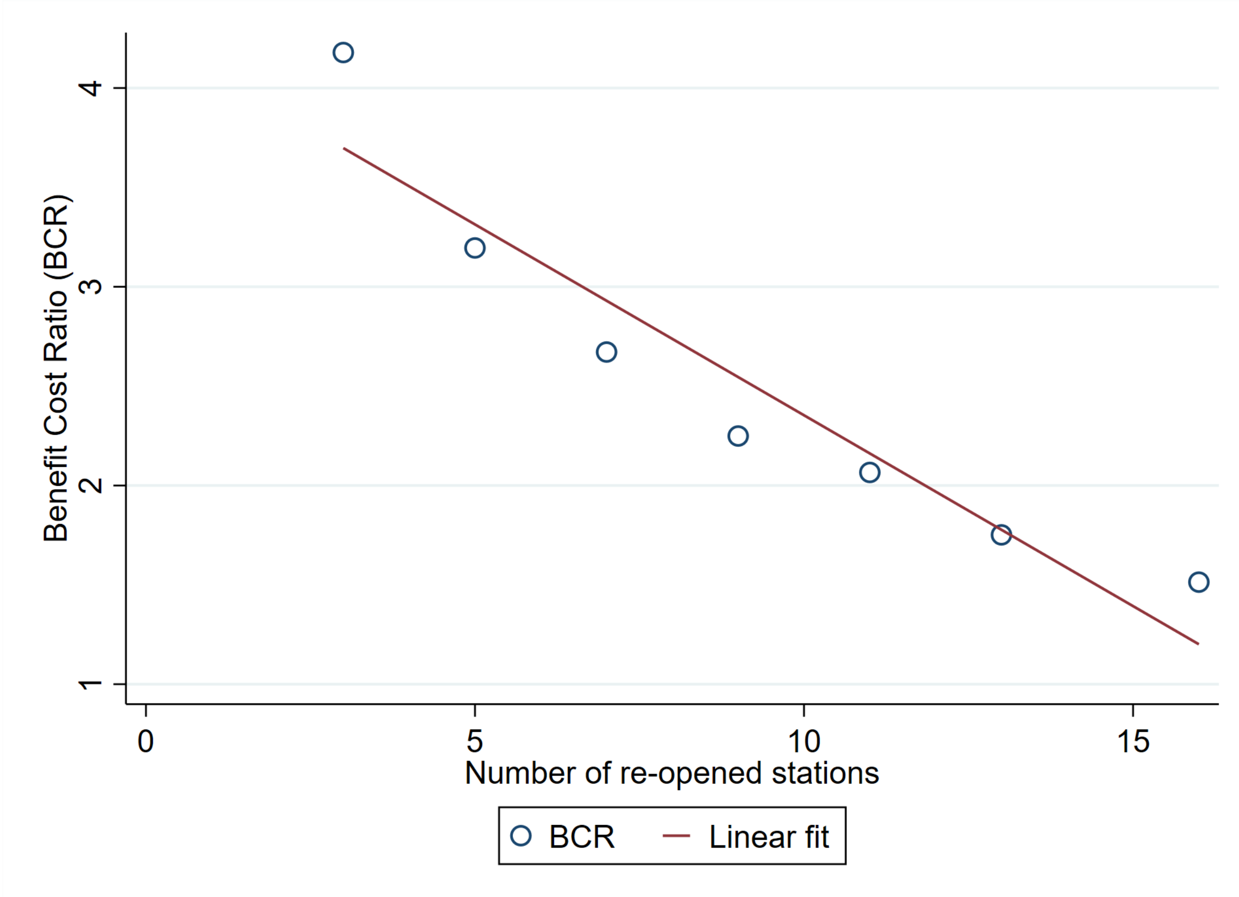


Figure A.12: Estimated BCRs

Note: The figure plots the estimated Benefit–Cost Ratios (BCRs) against the number of rural rail stations reopened under alternative counterfactual scenarios. Each point represents a simulated scenario, with the fitted line illustrating the declining marginal returns as more stations are reopened. A BCR above one indicates that aggregate welfare gains exceed annual operating costs of maintaining reopened stations.

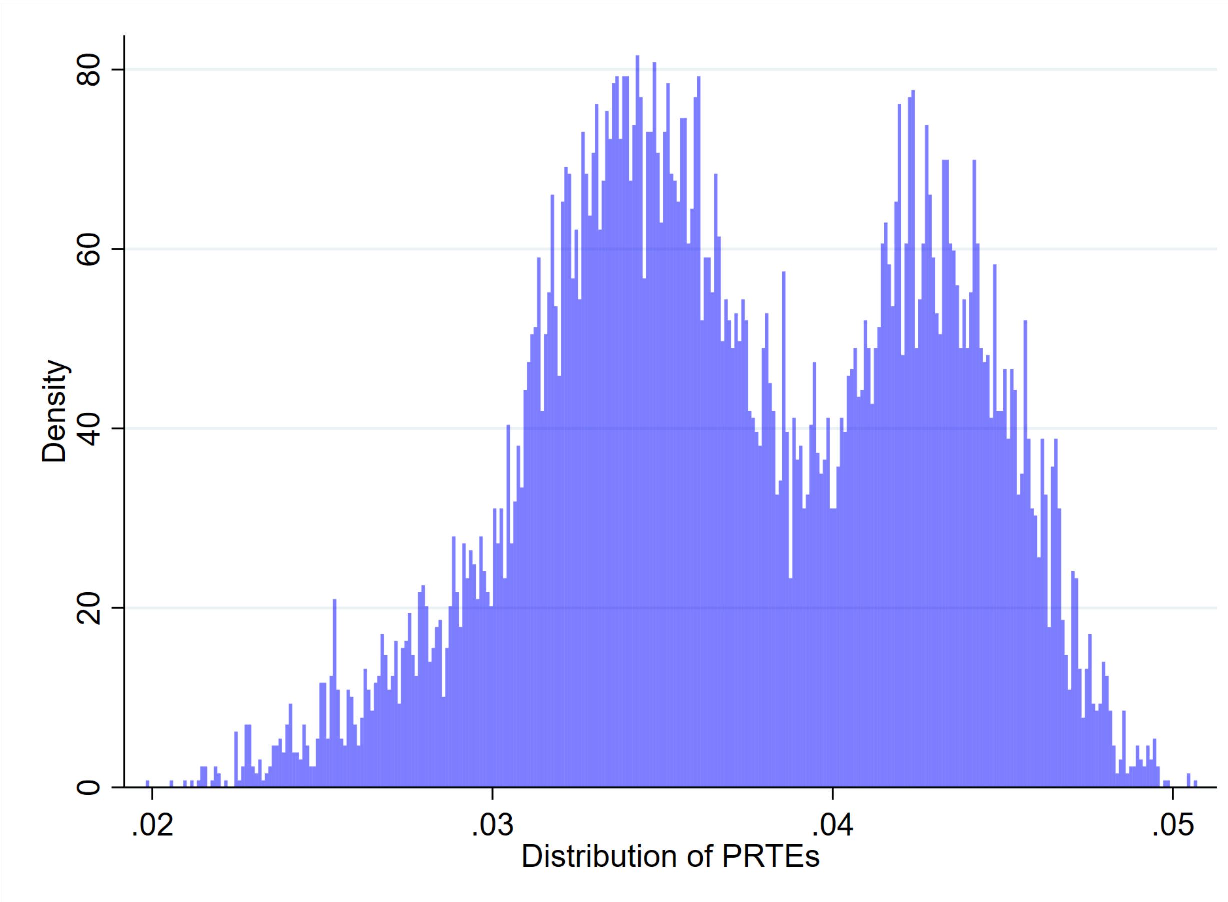


Figure A.13: Distribution of Estimated PRTE Across All 12,870 Reopening Allocations

Note: The figure plots the distribution of simulated Policy-Relevant Treatment Effects (PRTEs) for all 12,870 possible reopening allocations of 8 out of 16 closed rural rail stations. Each PRTE measures the expected per capita spending gain relative to the closure baseline. The dispersion across allocations reflects the variation in welfare gains arising from different spatial configurations of station reopenings.

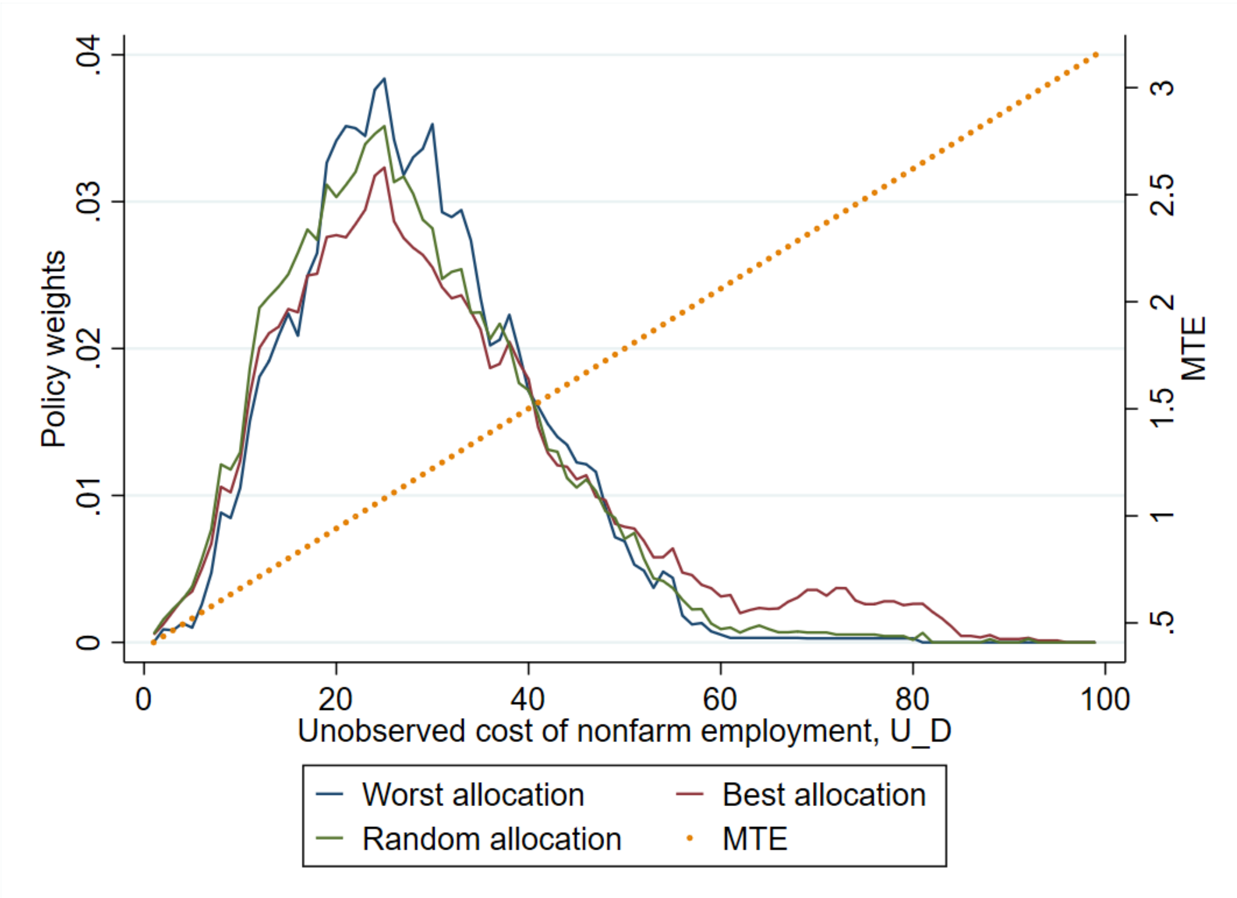


Figure A.14: Policy Weights, $w^A(u)$, for Worst and Optimal Reopening Allocations

Note: The figure compares the distribution of policy weights, $w^A(u)$, across the unobserved cost distribution for the optimal and worst reopening allocations shown in Figure 3. Each $w^A(u)$ measures the share of workers at a given unobserved mobility cost u who are induced to enter nonfarm employment under reopening allocation A. Higher values of u correspond to workers with higher mobility costs and higher potential nonfarm returns. The optimal allocation disproportionately reconnects these more constrained high-return workers, while the worst allocation primarily benefits workers with lower costs and lower expected returns. This pattern explains why spatial targeting yields large welfare heterogeneity across reopening configurations.

5 Appendix: Theoretical Frameworks and Extensions

This appendix provides the theoretical and computational details supporting the main analysis in Section 6. Appendix 5.1 derives the empirical welfare–maximization framework used to evaluate alternative rail reopening allocations. Appendix 5.2 extends the Roy model to allow for aggregate spillovers across sectors and presents the derivations of Lemma 1 and Proposition 1.

5.1 Spatial Targeting Via Empirical Welfare–Maximization Framework

This subsection formally derives and implements the empirical welfare–maximization problem summarized in Section 6.1. The objective is to characterize how alternative spatial reopening allocations affect aggregate welfare and to derive the corresponding optimal allocation rule.

Intuition. Reopening a rail station in a given location reduces mobility costs for a specific group of nearby workers. Because these workers differ in both their potential nonfarm returns and their costs of switching sectors, the welfare impact of reopening a given station depends on who gains access. Allocations that connect areas with a high concentration of constrained yet high-return workers generate the largest aggregate gains. The optimal allocation thus relaxes mobility constraints where they are most binding and productive, accelerating structural transformation in the rural economy.

Setup and Notation. Let each subprefecture S be characterized by a distance to the nearest active rail station under allocation A, denoted by $Dist_{S,A}$. Define the change in log distance relative to the baseline (pre-closure) network as:

$$Z_A = \log Dist_{S,A} - \log Dist_{S,base}.$$

This variable, compared to the initial rail closure-induced shock to rail access ($\log Dist_{S,close} - \log Dist_{S,base}$), improves rail access by reducing the closure-induced shock through reopenings under allocation A. Note that $Z_A \leq Z_{\text{close}}$ almost surely. That is, compared to the scenario with all closed rural rail stations, reopenings under allocation A would reduce mobility costs, thereby increasing nonfarm participation and improving welfare.

Under Assumptions **A.1-A.5**, expected welfare under allocation A satisfies:

$$\mathbb{E}[Y \mid A] = \mathbb{E}\left[D(A) \cdot Y_1 + (1 - D(A)) \cdot Y_0\right]$$

where $D(A)$ indicates nonfarm participation under allocation A. Hence, alternative reopening allocations affect welfare only through their impact on the induced sectoral allocation $D(A)$.

Empirical Welfare–Maximization Problem. The policymaker's problem is to select the reopening allocation A^* that maximizes expected welfare:

$$A^* = \arg \max_{A \in \mathcal{A}_{8/16}} \mathbb{E}[Y \mid A],$$

where $\mathcal{A}_{8/16}$ denotes the set of 12,870 possible reopening configurations that involve reopening eight of the sixteen closed rural stations. The welfare criterion is utilitarian, emphasizing average living standards rather than distributional concerns.

Because the baseline welfare level $\mathbb{E}[Y \mid \text{baseline}]$ is constant across allocations,[28] maximizing expected welfare is equivalent to maximizing the welfare gain relative to the baseline:

$$\max_{A \in \mathcal{A}_{8/16}} \{\mathbb{E}[Y \mid A] - \mathbb{E}[Y \mid \text{baseline}]\}.$$

Under Assumptions **A.1–A.5**, this welfare difference corresponds to the Policy-Relevant Treatment Effect (PRTE) defined in Section 5 (Equation (5.3)):

$$\text{PRTE}(A) = \int \text{MTE}(u)\, w^A(u)\, du$$

[28] It is finite per assumption **A.4**.

where $\text{MTE}(u)$ is the marginal treatment effect as a function of unobserved costs u, and $w^A(u)$ represent the policy weights capturing how each allocation changes the distribution of workers across sectors for different levels of unobserved costs (resistance).

Hence, the empirical welfare–maximization problem can be rewritten as

$$A^* = \arg\max_{A \in \mathcal{A}_{8/16}} \text{PRTE}(A).$$

This representation links the planner's decision directly to heterogeneity in returns and mobility costs, providing an empirical analogue to the optimal spatial allocation problems studied by Fajgelbaum and Schaal (2020) and Allen and Arkolakis (2022), but derived from microdata rather than from a calibrated general-equilibrium structure.

Implementation. To implement the empirical welfare maximization problem described in Section 6.1, I compute the policy relevant treatment effect, $\text{PRTE}(A)$, for every feasible reopening allocation $A \in \mathcal{A}_{8/16}$. Each allocation corresponds to reopening 8 of the 16 closed rural stations. The procedure for computing the PRTE under each allocation follows Algorithm 5.1.

Algorithm 5.1 ***Computing the PRTE for Each Reopening Allocation***

1. ***Initialize the set of allocations.*** *Construct the set $\mathcal{A}_{8/16}$ containing all 12,870 possible combinations of reopening 8 out of 16 closed stations.*

2. ***Compute updated distances.*** *For each allocation $A \in \mathcal{A}_{8/16}$, compute the distance from each subprefecture S to the nearest active rail station, denoted $Dist_{S,A}$.*

3. ***Update cost shifters.*** *For each subprefecture, update the policy-induced cost shifter as*

$$Z_{S,A} = \log(Dist_{S,A}) - \log(Dist_{S,base}).$$

4. ***Recover policy weights.*** *Using the updated $Z_{S,A}$, recover the policy weights $w^A(u)$ across*

the unobserved mobility cost distribution, following the expression in the previous subsection.

5. ***Compute welfare effects.*** *For each allocation A, compute the Policy-Relevant Treatment Effect:*

$$\textit{PRTE}(A) = \int \textit{MTE}(u) \cdot w^A(u)\, du.$$

6. ***Store and rank results.*** *Record the estimated PRTE(A) for each allocation and rank them by the resulting welfare gains, identifying the allocation A^* that maximizes PRTE(A).*

The resulting distribution of PRTE(A) values across all 12,870 allocations summarizes the potential welfare gains achievable under alternative spatial-targeting strategies.

Relation to Existing Frameworks. Unlike standard empirical welfare-maximization frameworks such as Kitagawa and Tetenov (2018), where assignment is exogenous, the current setup accounts for endogenous sectoral participation driven by spatially varying mobility costs. The resulting welfare problem is most closely related to the extensions in Sasaki and Ura (2024) and Liu (2022), which address policy evaluation in the presence of endogenous selection and heterogeneous returns.

Discussion. The empirical welfare–maximization approach used here should be interpreted as a partial-equilibrium exercise. It captures the first-order effects of reallocating labor in response to improved transport access while abstracting from general-equilibrium adjustments such as large-scale migration or or intersectoral linkages. Evidence from Section 4 supports the migration assumption is reasonable, supported by limited migration responses observed following the larger shock of rail station closures. This is also supported by existing literature on limited migration in low-income rural contexts (Bryan et al., 2014; Morten and Oliveira, 2024). Potential intersectoral spillover effects are explored in Appendix 5.2.

Summary. This appendix establishes the empirical foundation for the spatial-targeting exercise in the main text (Section 6.1). By mapping each feasible reopening configuration to its implied policy weights and corresponding PRTE, this framework quantifies how the spatial allocation of limited infrastructure investments shapes aggregate welfare and identifies the configuration that maximizes it.

5.2 Spillover Mechanisms

This subsection complements the main text in Section 6.2 by formally deriving the extended generalized Roy model with spillovers and proving the decomposition (Lemma 1) and bounding results (Proposition 1).

Extended Model and Notation. In the baseline Roy model presented in Section 5, sectoral potential outcomes are assumed to be policy-invariant (**A.5**). Here, I relax this assumption by allowing sectoral outcomes to depend on aggregate changes in nonfarm employment.

Let p index a given rail-policy regime (closure or reopening). For each sector $d \in \{0, 1\}$ (farm $= 0$, nonfarm $= 1$), I define

$$Y_{d,p} = Y_d + \underbrace{\beta_d \cdot (\mathbb{E}[D_p] - \mathbb{E}[D])}_{\text{Spillover Effect}},$$

where β_d measures how a change in the overall nonfarm share affects sectoral outcomes.

The parameter β_0 captures indirect nonfarm-to-farm spillovers through local demand or market linkages, while β_1 reflects within-sector competition among nonfarm workers.

Expected welfare under policy p is then

$$\mathbb{E}[Y \mid p] = \mathbb{E}\left[D_p \cdot Y_{1,p} + (1 - D_p) \cdot Y_{0,p}\right]$$

The extended model preserves the exclusion restriction of Assumption 1 because the cost shifter Z continues to affect outcomes only through sectoral participation within a given rail policy regime. However, it relaxes Assumption **AS.5** by allowing sectoral outcomes to respond to policy-induced changes in aggregate sectoral employment.

Proof of Lemma 1: Decomposition of Total Effects

Under **A.1-A.4**, the total change in expected welfare following rail closures within the extended model is:

$$\begin{aligned}\mathbb{E}[Y \mid \text{closure}] - \mathbb{E}[Y \mid \text{baseline}] &= \mathbb{E}\left[Y_{1,p} D_p + Y_{0,p}(1-D_p)\right] - \mathbb{E}\left[Y_1 D + Y_0(1-D)\right] \\ &= \mathbb{E}\left[Y_1 D_p + Y_0(1-D_p)\right] - \mathbb{E}\left[Y_1 D + Y_0(1-D)\right] \\ &\quad + \mathbb{E}\left[\beta_1 D_p + \beta_0(1-D_p)\right] \cdot \mathbb{E}[D_p - D].\end{aligned}$$

The first difference corresponds exactly to the Policy-Relevant Treatment Effect (PRTE) of Heckman and Vytlacil (2005):

$$\mathbb{E}\left[Y_1 D_p + Y_0(1-D_p)\right] - \mathbb{E}\left[Y_1 D + Y_0(1-D)\right] = \mathbb{E}\left[(Y_1 - Y_0)(D_p - D)\right] \equiv \text{PRTE}.$$

The second term captures the aggregate spillover from the change in the nonfarm share,

$$(\beta_1 \cdot \mathbb{E}[D_p] + \beta_0 \cdot \mathbb{E}[1-D_p]) \cdot \mathbb{E}[D_p - D].$$

Combining both gives:

$$\mathbb{E}[Y \mid \text{closure}] - \mathbb{E}[Y \mid \text{baseline}] = \underbrace{\text{PRTE}}_{\text{Reallocation Effect}} + \underbrace{(\beta_1 \mathbb{E}[D_p] + \beta_0 \mathbb{E}[1-D_p]) \cdot \mathbb{E}[D_p - D]}_{\text{Aggregate Spillovers}},$$

which is the equation in Lemma 1 in the main text. Hence, Lemma 1 follows. ■

Relation to Heckman and Vytlacil (2005). This decomposition is a special case of the non-invariance framework in Appendix B ("Relaxing (A-7)") of Heckman and Vytlacil (2005). In their general expression, policy changes affect outcomes through (i) pure selection, (ii) sectoral outcome shifts, and (iii) their interaction. In my setting, the policy enters symmetrically across sectors through the mean change in nonfarm employment ($\mathbb{E}[D_p] - \mathbb{E}[D]$), so the latter two components collapse into a single location-shift term, yielding the two-part decomposition above.

Proof of Proposition 1: Bounding the Spillover Component

Starting from Lemma 1, the aggregate spillover term satisfies:

$$(1-c) \cdot (\mathbb{E}[Y \mid \text{closure}] - \mathbb{E}[Y \mid \text{baseline}]) = (\alpha_p \beta_1 + (1-\alpha_p)\beta_0) \cdot (\alpha_p - \alpha),$$

where $\alpha = \mathbb{E}[D]$, $\alpha_p = \mathbb{E}[D_p]$, and $c = \text{PRTE}/\text{Total Effect}$ represents the share of the total effect due to reallocation.

While the individual components of the previous equation are not jointly identified, several can be obtained using existing estimates. For instance,

1. The left-hand side ($\mathbb{E}[Y \mid \text{closure}] - \mathbb{E}[Y \mid \text{baseline}]$), the total effect of rail station closures on per-capita spending, can be directly identified by the reduced-form DiD estimate on per-capita spending.

2. $\mathbb{E}[D_p]$ and $\mathbb{E}[1 - D_p]$ correspond to the observed post-closure shares of nonfarm and farm workers in treated regions, and can be directly recovered from the 2015 data.

3. $\mathbb{E}[D_p] - \mathbb{E}[D]$ can be identified by the DiD estimate of the effect of closures on nonfarm participation.

This yields one linear relation among three unknowns, c, β_1, and β_0.

To bound β_0, I impose the following economic sign restriction:

(A.6) $\quad \beta_1 \leq 0.$

This assumption captures competition effects within the nonfarm sector. It is consistent with evidence on the negative wage elasticity to employment (Borjas (2003)), and means that an increase in the share of workers in the nonfarm sector decreased per-capita spending (i.e., through earnings) in that sector.

Solving for β_0 yields:

$$\beta_0 = \frac{\frac{1-c}{\alpha_p - \alpha} \cdot \text{DiD} - \alpha_p \beta_1}{1 - \alpha_p}.$$

Under **(A.6)**, the highest possible value of β_1 (the least favorable case for the nonfarm competition effect) is $\beta_1 = 0$. Substituting this value yields the following lower bound:

$$\beta_0 \geq \frac{(1-c) \cdot \text{DiD}}{(\alpha_p - \alpha) \cdot (1 - \alpha_p)}.$$

Multiplying both sides by $(\alpha_p - \alpha)$, and remembering that the effect of rail station closures on nonfarm employment $(\alpha_p - \alpha)$ is negative (see Table A.5), gives:

$$\mathbb{E}[Y_{0,p}] - \mathbb{E}[Y_0] = \beta_0 \cdot (\alpha_p - \alpha) \leq \frac{(1-c) \cdot \text{DiD}}{1 - \alpha_p}.$$

This provides an informative upper bound on the magnitude of the nonfarm-to-farm spillover as a function of c. It implies that, for any plausible share of the total effect attributed to reallocation c, the minimum decline in farm spending consistent with the data and the assumption is given by the right-hand side of the inequality. The obtained bound is sharp, as it is attained for the highest possible value of β_1 ($\beta_1 = 0$).

■