Hit the Road Juan: Welfare and Labor Market Effects of High-Quality Roads in Ecuador*

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Abstract

Road infrastructure boosts economic opportunities and thus contributes to poverty alleviation. This paper investigates the causal impact of paved primary roads on poverty and income mobility in Ecuador, with particular attention to the mechanisms through which these effects materialize. Exploiting variation in road expansion between 2012 and 2019, we track the construction of new major roads and link this information to socioeconomic outcomes reported in the national household survey. To achieve representativeness at a fine geographical scale, we employ the max-p region algorithm. Using staggered difference-in-differences estimators, we identify the causal effects of road infrastructure on poverty reduction and income dynamics. The findings indicate that access to paved roads significantly reduces poverty rates overall, though no discernible effects are found for extreme poverty. Middle-income households benefit from income growth following road access and these gains are attributable primarily to improvements in employment quality rather than increases in employment rates, with the largest effects concentrated in the primary sector.
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1 Introduction

United Nations Sustainable Development Goal (SDG) 9.1.1 identifies inadequate road accessibility as a fundamental constraint to socioeconomic development. Thus, substantial public and private investments are being directed towards the expansion and upgrading of transport infrastructure, premised on the notion that enhanced connectivity reduces transaction costs, facilitates market integration, and improves access to essential public services (Redding and Turner, 2015). By lowering transport costs, well-maintained roads foster price convergence across markets and enhance allocative efficiency. These mechanisms are associated with the accumulation of household assets, higher incomes, and ultimately, reductions in the incidence of poverty(Starkey and Hine, 2014).

In developing economies, investments in rural road infrastructure are generally associated with higher household incomes and lower prices for inhabitants of local communities (Asher and Novosad, 2020). However, empirical evidence on the effects of roads on poverty is mixed and highly contingent on the specific context and design of the road-building program (Kaiser and Barstow, 2022), highlighting the need for country- and program-specific evaluation. Road improvements can influence poverty through multiple channels, with enhanced access to employment representing a particularly important pathway. This effect operates both by increasing employment rates (Stringer et al., 2025; Khandker and Koolwal, 2011) and by facilitating transitions into higher-paying formal-sector jobs, thereby raising wages (Gertler et al., 2024; Perra et al., 2024).

This paper examines the impact of paved major roads on poverty alleviation and income mobility in Ecuadorian regions that gained access to such infrastructure between 2012 and 2019. Recognizing the critical role of road networks in facilitating access to services, economic opportunities, and social inclusion, we aim to quantify these effects across Ecuador's heterogeneous local contexts. By integrating geospatial data on road construction with socioeconomic information from the National Employment, Unemployment, and Underemployment Survey (Encuesta Nacional de Empleo, Desempleo y Subempleo [ENEMDU]), we trace the dynamic socioeconomic impacts of primary road networks over time. To ensure representative analysis at fine geographic scales, we employ the max-p region algorithm to aggregate data across neighboring parishes, effectively capturing regional variation. Using staggered difference-in-differences estimators, we identify the causal effects of road access on poverty and income mobility while disentangling the specific mechanisms driving these outcomes.

We find that paved roads reduce poverty rates in Ecuadorian regions, although the effects vary across income levels. For individuals living below the $6.85 per day poverty line, the poverty rate declines by 13.45 percentage points (pp) over the sample period; however, the effects for those earning less than $2.50 per day remain inconclusive. This pattern is consistent with the hypothesis that the poorest households may lack access to transportation services or face other mobility constraints despite road improvements and may reflect measurement challenges in relation to households with subsistence-level incomes (Abay et al., 2021; Jolliffe, 2001). In contrast, households above the poverty line and in the lower- to middle-income brackets experience clear gains. The primary mechanism for poverty reduction is labor income, which rises by 25.7 percent in treated areas. Increases are observed for both men and women, with the largest gains accruing to self-employed individuals and workers in small agricultural firms. Importantly, these income improvements are driven by enhanced employment quality: while overall employment rates remain largely unchanged, a greater proportion of workers report full-time positions with wages at or above the minimum wage.

These results are important for a number of reasons. First, while there are causal studies of the impacts of roadways on poverty in other areas, South America remains comparatively understudied. Among the existing studies (for example,Iimi et al. (2015); Briceño-Garmendia et al. (2015); Stringer et al. (2025)), only one older study considers poverty in Ecuador (Rudel and Richards, 1990). South America as a whole is comparatively more reliant on roads, with less developed rail and water transport than other regions (Briceño-Garmendia et al., 2015). Additionally, Ecuador has unique geographical characteristics and climate features: there are coastal plains in the west, a central belt of mountainous terrain (with some of the world's highest mountains), and dense rainforest in the eastern Amazon region. While the coast and mountain regions are relatively well-connected and developed, the east is not and is the site of the major road building that we study. This sparsely populated region is divided by steep ridges, dense forests, and torrential rivers, separating communities into ethnically distinct enclaves.

Second, our study contributes to the corpus of causal research on road building and poverty. Similar to previous research, we do not find strong poverty alleviation effects of road construction on the worst-off individuals. Instead, benefits accrue primarily through labor income gains to the nonimpoverished poor and middle class. However, while previous studies show these gains are driven by manufacturing employment (Spey et al., 2019; Gertler et al., 2024; Hine et al., 2019), we find that it is the primary sector that drives income gains in rural Ecuador. Moreover, our difference-in-difference (DID) identification strategy is methodologically similar to that of (Aggarwal, 2018; Nakamura et al., 2020; Xie et al., 2023; Charlery et al., 2016; Nguyen et al., 2017; Bucheli et al., 2018; Shamdasani, 2021). While this methodology has been criticized because of the potential for simultaneous confounds with road construction, it has the virtue of allowing for examination of the effect of major connecting roads. In contrast, explicitly randomized road construction of terminal node feeder roads has shown the effect of roads is smaller than what DID studies find (Asher and Novosad, 2020). Moreover, new construction of arterial connectors has both larger and more geographically diffuse effects through the simultaneous linking of more economic centers, as opposed to the connection of small villages to existing road networks. In Ecuador, these stronger regional linkages enhance the quality of employment through formal employment at higher wages.

Finally, we explore the dynamic effects of road infrastructure on poverty. Road construction often induces substantial economic reorganization, altering the spatial distribution of economic activity. For instance, Shiferaw et al. (2015) document shifts in manufacturing employment from historical hubs to newly connected peripheral areas. These adjustments occur over time, meaning that short-term evaluations may underestimate medium- and long-term impacts (Khandker and Koolwal, 2011). Indeed, Khandker and Koolwal (2011) and Mu and Van de Walle (2011) find that, with the exception of nonagricultural wages, the effects of roads tend to attenuate over time. However, such attenuation may partly reflect methodological artifacts, as staggered or incremental treatment implementation can induce negative weights and bias estimates toward zero (Imai and Kim, 2021; De Chaisemartin and d'Haultfoeuille, 2020). To address this, we employ the estimators proposed by Callaway and Sant'Anna (2021), which are robust to these issues. Using this approach, our results indicate that the effects of road access grow over time: per capita income remains elevated seven years after construction, while moderate poverty measures continue to decline.

Our study proceeds as follows: we discuss our methods in section 2, we present our results in section 3, and we conclude in section 4.

2 Methodology

2.1 Paved Major Roads

This study investigates the causal impact of high-quality road infrastructure—specifically, paved major roads—on poverty and income mobility in Ecuador.[1] Paved major roads are here defined as the arterial highways of the Red Vial Estatal, which connect provincial capitals, major cantonal centers, international border crossings, and other strategic economic nodes. These roads, managed by the central government, represent the highest-traffic segments of the national network (Coşar et al., 2022).

To accurately determine where and when new paved road access was gained, we compile segment-level pavement completion dates using both administrative records from Ecuadorian government agencies and satellite imagery. This process enables high-resolution temporal and spatial identification of road improvements throughout the country.

The integration of these data supports the precise geographic and temporal assignment of infrastructure exposure, which underpins our empirical strategy. The allocation of treatment and control status based on this network is detailed in section 2.3.

2.2 Geographical Units

A central methodological challenge in this context is the mismatch between the spatial resolution of road investment and the representativeness of survey data. While Ecuador's ENEMDU survey is only representative at national, provincial, and broad urban/rural levels, the impacts of road infrastructure may manifest at much finer geographic scales. Ecuador's administrative geographic divisions include national, province, canton, and parish levels.[2] Relying on administrative units such as provinces risks diluting treatment effects, because such large units may simultaneously contain treated and untreated areas.

To address this, we construct intermediate-level geographical units—termed max-p regions—by aggregating neighboring parishes using the max-p region algorithm of Duque

[1] Prior research has shown that road quality is just as important as roadway extent. Gertler et al. (2024) find better-maintained roads increase consumption using an instrumental variables approach in Indonesia. Worku (2010) shows asphalt is superior in terms of development outcomes using time-series econometrics on Ethiopian data.

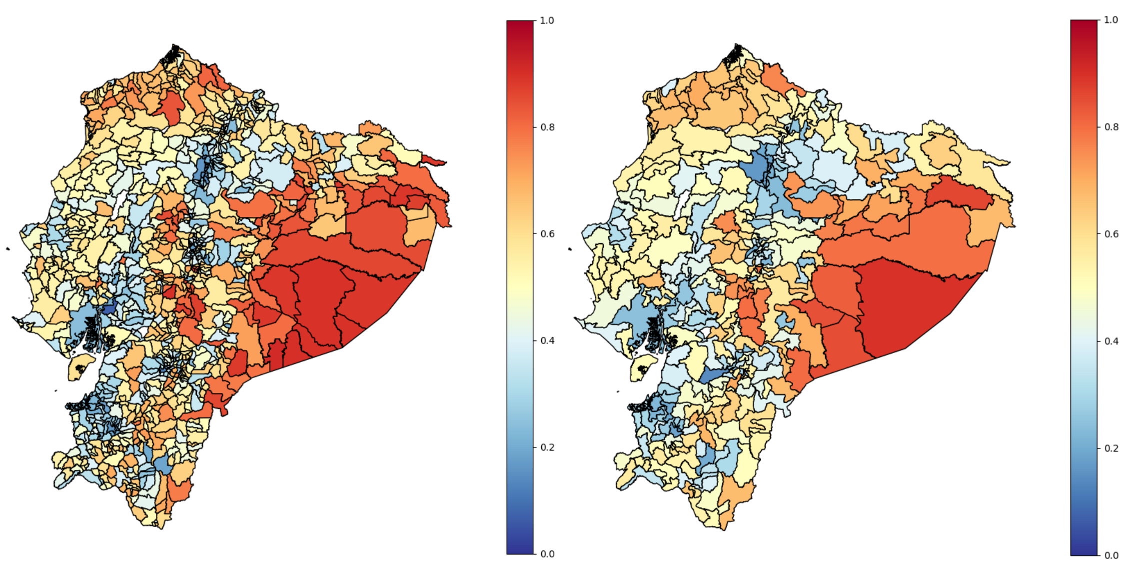
[2] In Ecuador, cantons are equivalent to municipalities in other countries and are divided into parishes. Parishes are the lowest-ranking territorial division.

et al. (2012). The rationale for implementing regional clustering arises from concerns regarding insufficient statistical representativeness at the municipal level, which could compromise the validity of the estimated effects when data are disaggregated to such a granular scale. Should the estimated effects prove robust to aggregation at the parish or canton level, the application of the max-p region clustering algorithm would be rendered unnecessary. This clustering approach maximizes the number of spatially contiguous, homogeneous regions, subject to minimum thresholds for pretreatment population and poverty levels.

The clustering procedure uses 2010 census data on population and World Bank Small Area Estimation (SAE) poverty rates, relying exclusively on pretreatment variables to avoid endogenous regional definitions[3]. Specifically, the max-p region algorithm is applied to Ecuador's 1,016 parishes, with spatial connectivity defined through a contiguity weights matrix. Each resulting max-p region contains at least three parishes (with the Galapagos excluded, as it is not part of the national road system), yielding a total of 296 regions for the study (figure 1). Robustness checks using regions of at least four parishes, or alternative pretreatment features for clustering, produce similar results (see section 3.5 and appendix B). Nevertheless, the three-parish configuration is preferred as it provides the best balance between precision and sample size.

Figure 1: Maps of 2010 World Bank SAE Poverty Headcount Ratios by Parishes and Max-p Regions

(a) Parish level (b) Max-p regions



[3] A more detailed explanation of the regional clusterization can be found in appendix A.

To assess the reliability of outcome measurement at the max-p level, we compute the coefficient of variation (CV) for each outcome variable W in each max-p region i and year t:

$$CV_{it}(\bar{W}) = \frac{\mathrm{se}(\bar{W}_{it})}{\bar{W}_{it}} \times 100\%, \tag{1}$$

where $\mathrm{se}(\bar{W}_{it})$ is the standard error of the regional mean. Table 1 summarizes the mean and median CVs for each outcome across years and spatial aggregations. By conventional standards, CVs below 10 percent indicate high precision, those between 10 and 20 percent are acceptable, and those above 20 percent are unreliable.

Table 1: Summary of Coefficient of Variation

Outcome	Max-p Region		Parish	
	Mean	Median	Mean	Median
Poverty Headcount Ratios – \$2.5 Poverty Line	38.5	34.4	41.6	38.9
Poverty Headcount Ratios – \$3.65 Poverty Line	25.2	20.2	30.4	25.5
Poverty Headcount Ratios – \$6.85 Poverty Line	12.2	9.14	16.3	12.8
Percentage of People Classified as Vulnerable	19.0	14.6	27.0	21.9
Percentage of People Classified as Middle Class	28.3	21.6	36.8	30.7
LOG(Overall Per Capita Income)	1.5	1.3	2.5	2.2
LOG(Labor Per Capita Income)	2.0	1.6	2.9	2.5
Employment Rate	1.3	0.9	1.5	0.9
Percentage of Employed People with Adequate Employment	22.2	17.1	28.2	22.9
Percentage of Employed People with Formal Employment	17.7	13.4	23.6	18.4
Ecuador's Official Moderate Poverty Headcount Ratio	18.2	14.13	23.5	18.8
Ecuador's Official Extreme Poverty Headcount Ratio	32.9	27.3	36.4	32.2

Note: Outcome variables presented in this table are explained in section 2.3 and appendix D.

Max-p region aggregation improves statistical precision relative to the parish level for most outcomes. Variables with mean or median CVs below or near 20 percent are considered reliable for regional analysis; however, results related to the $2.5 poverty headcount and Ecuador's official extreme poverty, which exhibit higher imprecision, should be interpreted with caution throughout the study.

This methodological structure ensures that the granularity of the treatment variable (road exposure) is matched with a suitable spatial scale for outcome measurement, providing a rigorous basis for causal inference.

2.3 Data Set and Treatment

We define our treatment units as those having access to a paved major road for the first time between 2012 and 2019. Accordingly, treatment status is assigned by overlaying the dated network of paved major roads (section 2.1) onto the boundaries of the max-p regions (section 2.2), which allows precise identification of the year each region first gained access to a new paved road segment.

To prevent contamination, we define excluded regions as max-p regions intersected by any major road paved prior to 2010. Never-treated regions are those not intersected by paved roads through 2019; they serve as control units. Treated regions comprise regions intersected by at least one road segment paved between 2012 and 2019, with the earliest segment determining the treatment year when multiple new roads were constructed.

Applying this procedure to the 296 max-p regions yields 101 analytical units (treated and never treated) for analysis. Appendix C provides maps illustrating the spatial assignment and timing of treatment.

According to this framework, the empirical analysis is based on a repeated cross-section (RCS) data set constructed by aggregating individual-level data from the ENEMDU at the max-p region and year level for the period 2010–2019. For each region-year, means and shares for all outcome variables are calculated using the appropriate ENEMDU expansion factors.

Table 2: Distribution of Observations in the Repeated Cross-Section Data Set

Year	Max-p Region Level			Survey Individual Records	
	Total	Treated	Control	Without Expansion	With Expansion
2010	77	0	77	4,749	673,163
2011	79	0	79	3,689	629,060
2012	79	5	74	4,323	714,122
2013	70	8	62	5,202	747,495
2014	91	9	82	12,244	669,160
2015	90	10	80	11,843	668,271
2016	89	15	74	11,972	694,137
2017	90	24	66	11,821	743,264
2018	83	27	56	4,006	879,854
2019	83	57	26	4,061	878,718
Total	831	155	676	73,910	7,297,244

The final data set includes 831 region-year observations based on 73,910 individual records (representing 7,297,244 weighted individuals). Table 2 details the annual distribution of region-level observations and underlying survey counts. Additionally, table D.1 in appendix D includes a cross-tabulation at the max-p region level to provide a better understanding of the staggered design.

Once a region is treated, treatment status remains fixed for all subsequent years.

For each region and year, we construct a comprehensive set of outcome variables, most of which are statistically reliable with mean or median coefficients of variation $\lesssim 20\%$ (table 1). These include poverty headcount ratios at the \$2.5, \$3.65, and \$6.85 per day thresholds (2017 PPP), the percentage of people classified as vulnerable (earning between \$6.85 and \$14 per day), and the percentage classified as middle class (earning between \$14 and \$81 per day). We also consider the logarithm of average per capita overall income and labor income, the employment rate, the percentage of employed individuals with adequate employment[4] (an Ecuadorian definition), and the percentage in formal employment. In addition, Ecuador's official measures of moderate and extreme poverty headcount ratios are included for robustness.

Further details and descriptive statistics for all outcome variables and other indicators from the RCS data set, disaggregated by treatment status, are provided in appendix D. Overall, this data set—combining finely dated, geospatially precise information on road infrastructure exposure with robustly aggregated socioeconomic measures—provides the empirical foundation for estimating the causal effects of paved major roads on poverty and income dynamics in Ecuador.

2.4 Empirical Strategy

The staggered adoption of paved major roads, as described in table 2, enables us to exploit recent advances in difference-in-differences (DiD) estimation for multiple time periods, specifically the methodology of Callaway and Sant'Anna (2021). This approach is well suited to our context because it accommodates heterogeneity in both treatment timing and treatment effects across regions.

Our identification strategy rests on the conditional parallel trends assumption, whereby the counterfactual trends of treated units are matched to those of never-treated max-p regions—those that did not gain access to paved major roads throughout the study period. To further strengthen the validity of this assumption, we implement a doubly robust estimator that conditions on key pretreatment characteristics: 2010 census data on population and World Bank SAE poverty rates. By incorporating these covariates, we account for underlying demographic and socioeconomic differences between treated and control

[4] In Ecuador, adequate employment is defined as having a job where the worker earns at least the minimum wage, works the legal number of hours (30–40 per week), and has access to social security.

regions, mitigating potential sources of bias unrelated to road access. Robustness checks are presented in subsection 3.5.

A major advantage of the estimator of Callaway and Sant'Anna (2021) is its ability to flexibly handle treatment effect heterogeneity, a critical feature given regional variation in both timing and exposure to paved road improvements. In contrast, traditional two-way fixed effects (TWFE) DiD models can produce biased or misleading estimates when treatment effects are heterogeneous or adoption is staggered, primarily due to inappropriate weighting and aggregation of effects.[5] The group-time average treatment effect (ATT) estimator directly addresses these limitations and allows for robust causal inference.

To examine how the impact of paved road access evolves with exposure, we estimate dynamic treatment effects. For each cohort of regions first treated in year g and each period t, we estimate the group-time ATT, $\mathrm{ATT}(g, t)$. These effects are then aggregated to estimate the ATT after e periods of exposure, denoted as $\theta_{es}(e)$, as follows:

$$\theta_{es}(e) = \sum_{g \in \mathcal{G}} \mathbf{1}\{g + e \leq \mathcal{T}\} P(G = g \mid G + e \leq \mathcal{T})\, \mathrm{ATT}(g, g + e), \tag{2}$$

where $\mathcal{G}$ is the set of all treatment cohorts, $\mathcal{T}$ is the total number of periods, and $P(G = g \mid G + e \leq \mathcal{T})$ denotes the proportion of regions first treated in period g that are observed at event time e. The indicator function $\mathbf{1}\{g + e \leq \mathcal{T}\}$ restricts the analysis to valid cohort-period pairs.

This aggregation provides a clear view of how treatment effects accumulate or dissipate with time since treatment, offering insight into the dynamics of poverty reduction and income mobility associated with paved road access. By focusing on event-time dynamics, our approach also overcomes potential aggregation biases that can arise in standard event study frameworks.

For a summary measure of overall program impact, we compute the average dynamic treatment effect across all exposure durations:

$$\theta_{es}^{0} = \frac{1}{\mathcal{T} - 1} \sum_{e=0}^{\mathcal{T}-2} \theta_{es}(e). \tag{3}$$

This statistic provides an interpretable estimate of the average effect of road access,

[5] See Goodman-Bacon (2021) for further discussion of these issues.

accounting for both varying treatment timing and exposure durations across the treated regions.

Overall, this empirical strategy enables a rigorous and flexible assessment of the causal effects of paved major roads on poverty and income mobility while directly addressing key challenges inherent to staggered policy adoption and treatment effect heterogeneity.

3 Results

3.1 Poverty Impacts

The overall treatment effect, θ^0_{es}, serves as a summary measure of the average impact of access to paved major roads across all treated regions and exposure periods. By averaging $\theta_{es}(e)$ over the full range of event times, we obtain a single, interpretable estimate of how road access has influenced poverty rates on average, abstracting from specific exposure durations. This parameter provides an aggregate view of the intervention's effectiveness in reducing poverty across the study sample.

Table 3: Overall Pre- and Post-ATT Effects on Poverty Headcount Ratios

Outcome Variable	ATT (θ^0_{es})	
	Pre	Post
\$2.5 Poverty Line	2.08	-1.63
	(2.99)	(1.90)
\$3.65 Poverty Line	-1.53	-9.85***
	(3.85)	(2.36)
\$6.85 Poverty Line	-2.88	-18.89***
	(4.41)	(3.06)

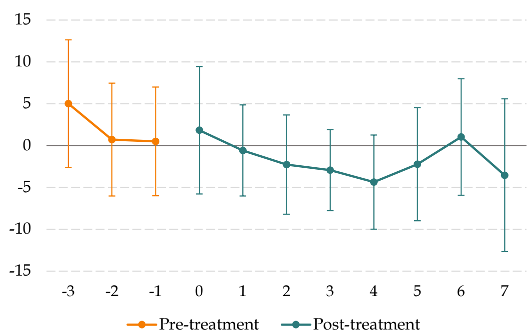
Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

The overall pre- and posttreatment ATTs on poverty headcount ratios, reported in table 3, provide insight into how access to paved major roads affects poverty across different income thresholds. For the \$2.5 poverty line, neither the pre- nor posttreatment effects are statistically significant, suggesting no clear evidence that road access influenced extreme poverty as measured by this threshold. However, these results should be interpreted with caution, given the high coefficient of variation associated with this outcome variable (see table 1). For the \$3.65 poverty line, the pretreatment ATT is negative but statistically insignificant, indicating that treated and never-treated regions were similar in their poverty trajectories before the intervention. Following road access, however, the ATT becomes significantly negative at -9.85 pp ($p < 0.01$), indicating a meaningful reduction in the share of individuals living below this threshold. For the \$6.85 poverty line, this pattern is even more pronounced: the pretreatment ATT is again negative and insignificant, while the posttreatment ATT reaches -18.89 pp ($p < 0.01$), reflecting a substantial reduction in moderate poverty following infrastructure improvements.

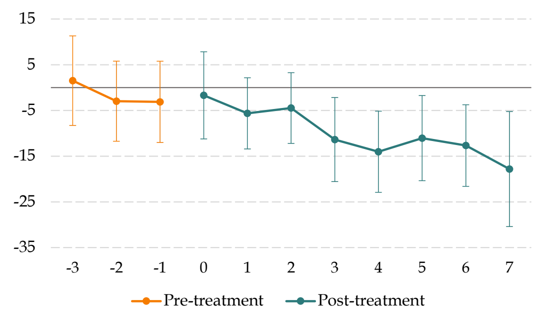
To more fully characterize the temporal evolution of treatment effects, we examine dynamic ATTs by event time, as depicted in figure 2. These dynamic results reveal that the impacts of paved road access on poverty are both gradual and persistent. For both the \$3.65 and \$6.85 poverty lines, ATT estimates in the pretreatment years ($T = -3$ to $T = -1$) are small and statistically insignificant, providing strong support for the parallel trends assumption and enhancing confidence in the causal interpretation of posttreatment effects.

Figure 2: Dynamic ATT Effects on Poverty Headcount Ratios

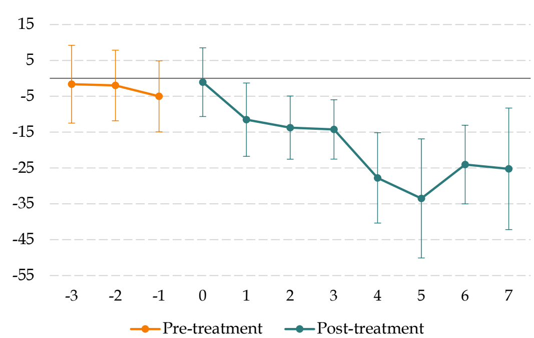
(a) \$2.5 Poverty line



(b) \$3.65 Poverty line



(c) \$6.85 Poverty line



Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. Confidence intervals: 95 percent.

After road access is established, treatment effects become more pronounced and statistically significant over time. For the \$3.65 poverty line, there is little detectable impact in the first two years after treatment. Starting in the third posttreatment year ($T = 3$), the ATT becomes significantly negative, reaching -11.37 pp ($p = 0.015$) and growing to as much as -17.83 pp by the seventh year ($p = 0.005$). This dynamic suggests a cumulative effect: as regions are progressively integrated into transport and economic networks, poverty reductions intensify and persist over the medium run.

The impacts for the \$6.85 poverty line are larger and materialize more rapidly. A significant negative ATT is observed as early as the first year after treatment ($T = 1$, -11.54 pp, $p = 0.027$), with the effect deepening in subsequent years. The largest reduction occurs in the fifth year posttreatment ($T = 5$, -33.49 pp, $p < 0.01$), before stabilizing at a substantial level through year seven. These patterns point to both immediate and enduring poverty alleviation effects, particularly among households near or above the lower-middle-income threshold.

3.2 Middle Class Impacts

The effects of paved major road access on income mobility are assessed by examining changes in the shares of the population classified as vulnerable and middle class. Table 4 presents the ATT before and after treatment. For both outcomes, the pretreatment coefficients are small and statistically insignificant, providing evidence that treated and control regions were comparable prior to the intervention and supporting the parallel trends assumption.

Table 4: Overall Pre- and Post-ATT Effects on the Percentage of People Classified as Vulnerable and Middle Class

Outcome Variable	ATT (θ^0_{es})	
	Pre	Post
Vulnerable	3.00	12.95***
	(3.47)	(2.80)
Middle Class	-0.12	5.94***
	(3.06)	(1.81)

Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

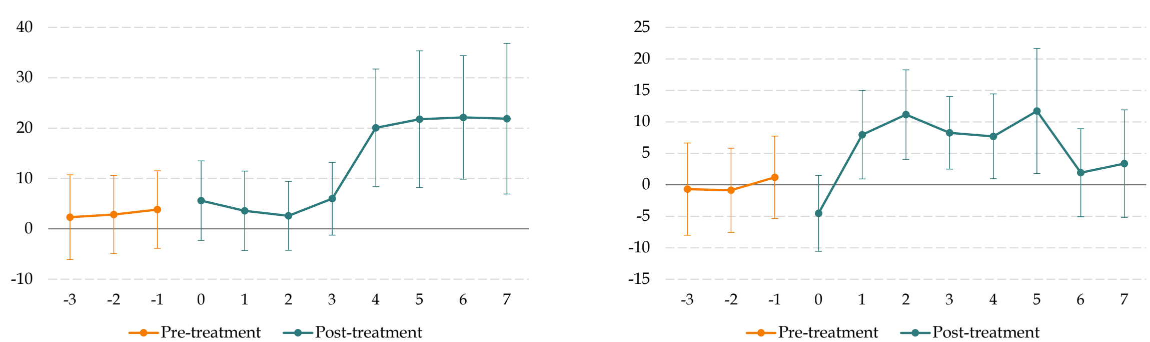
After road access is gained, there is a statistically significant increase of 12.95 pp in the share of vulnerable people ($p < 0.01$). This finding suggests that, in the wake of poverty reduction, a substantial proportion of the population transitions from poverty into the higher-income vulnerable category. Such a shift indicates improved income levels and economic security, although these individuals remain at risk of falling back into poverty.

For the middle class, the posttreatment ATT is also positive and statistically significant at 5.94 pp ($p < 0.01$), indicating a meaningful expansion of the middle class following road improvements. This result highlights the potential of infrastructure investments not only to reduce poverty, but also to foster upward income mobility and support the emergence of a more robust middle-income group.

The dynamic ATT results by event time offer further insight into the timing and persistence of these changes. For the vulnerable group, there is no statistically significant impact in the initial years after road access ($T = 1$ to $T = 3$). However, starting in the fourth year after treatment ($T = 4$), the ATT rises sharply and becomes statistically significant, with an effect of 20.06 pp ($p = 0.001$), peaking at 22.13 pp in the sixth year ($p = 0.000$). The effect remains substantial and significant through the seventh year. This temporal pattern suggests that income gains among formerly poor populations accumulate gradually, with the largest upward transitions into vulnerability occurring several years after improved connectivity.

Figure 3: Dynamic ATT Effects on the Percentage of People Classified as Vulnerable and Middle Class

(a) Vulnerable (b) Middle class



Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. Confidence intervals: 95 percent.

For the middle class, the positive impact emerges sooner. The ATT becomes statistically significant as early as the first posttreatment year ($T = 1$, 7.94 pp, $p = 0.027$) and remains significant through the fifth year. The largest increase is observed in the fifth year ($T = 5$, 11.72 pp, $p = 0.021$), after which the effect declines and loses statistical significance, suggesting that the most rapid expansion of the middle class occurs in the medium term after road completion.

Taken together, these results indicate that paved road investments not only reduce poverty, but also facilitate substantial upward mobility—first into the vulnerable group and then, for a significant share, into the middle class. The evidence points to dynamic and persistent welfare gains, with the largest improvements realized several years after treatment. This pattern is consistent with economic mechanisms whereby improved infrastructure enhances access to markets, employment, and services, promoting sustained income growth and supporting the consolidation of a more resilient and upwardly mobile middle-income sector.

3.3 Mechanisms

The observed reductions in poverty and improvements in income mobility are driven by significant gains in per capita income, with a notable emphasis on labor income, capturing percentage changes in these welfare indicators. Presumably, income improvement is driven by enhancement of adequate and formal employment.[6]

The observed reductions in poverty and improvements in income mobility are accompanied by statistically significant gains in both overall and labor per capita income, as shown in table 5. In the pretreatment period, ATT estimates for all income and employment outcomes are small and statistically insignificant, indicating no systematic differences between treated and control regions prior to the intervention and lending credibility to the parallel trends assumption.

Table 5: Overall Pre- and Post-ATT Effects on Income and Employment Outcomes

Outcome Variable	ATT (θ^0_{es})	
	Pre	Post
Overall Per Capita Income	3.7	24.2***
	(7.8)	(4.4)
Labor Per Capita Income	1.8	25.3***
	(9.3)	(6.0)
Employment Rate	0.0	-0.7
	(0.6)	(0.8)
Adequate Employment	2.8	7.9***
	(3.6)	(2.0)
Formal Employment	2.9	8.0*
	(4.3)	(4.2)

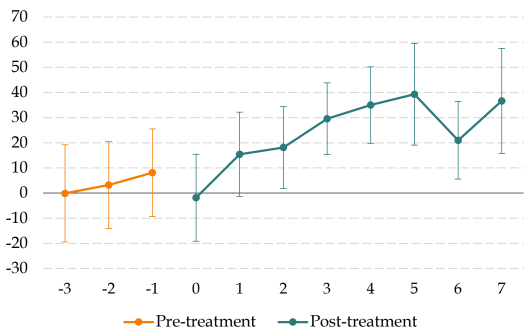
Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

[6]Refer to appendix D for variable description.

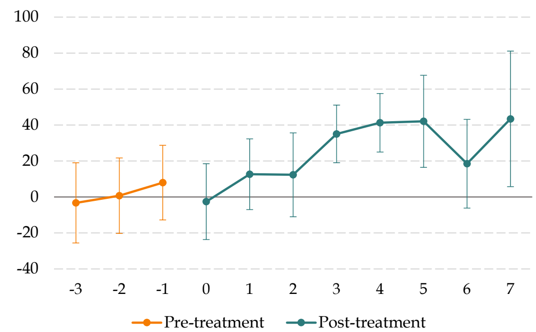
Following the expansion of paved road access, the ATT for log overall per capita income rises sharply to 24.2 pp ($p < 0.01$), while labor per capita income increases by 25.3 pp ($p < 0.01$). These results indicate substantial real income gains for households in treated regions, with the magnitude of the effect closely mirroring that observed for labor income.

Figure 4: Dynamic ATT Effects on Income and Employment Outcomes

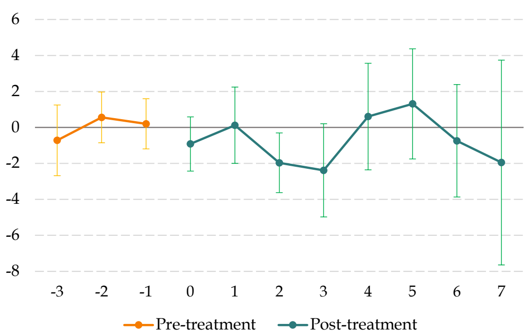
(a) Overall per capita income



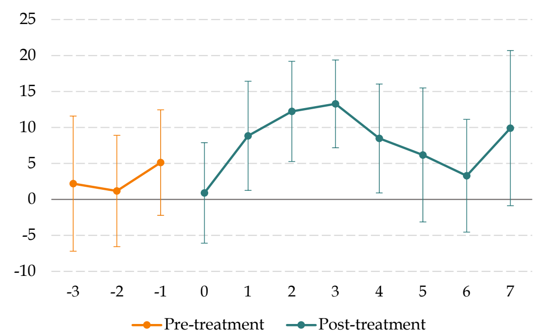
(b) Labor per capita income



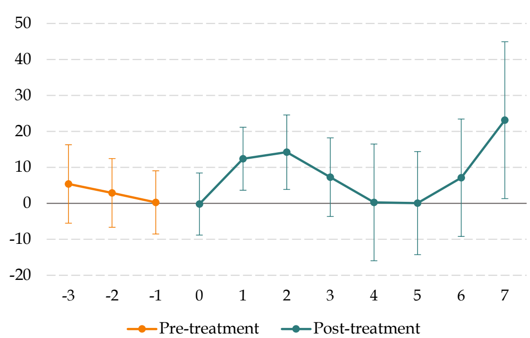
(c) Employment rate



(d) Adequate employment



(e) Formal employment



Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. Confidence intervals: 95 percent.

Dynamic ATT estimates further clarify the timing and persistence of these income gains (Figure 4). For both overall and labor per capita income, effects are negligible and statistically insignificant in the years prior to treatment and in the first posttreatment year. However, beginning in the second year after treatment, statistically significant increases emerge. For log overall per capita income, the ATT reaches 18.1 pp in year two ($p = 0.029$), rising to 35.0 pp by year four ($p < 0.01$) and remaining substantial and significant through year seven. Similarly, labor per capita income increases by 35.1 pp in year three ($p < 0.01$), peaking at 42.1 pp by year five, and maintaining large gains through later years. These temporal dynamics are consistent with the hypothesis that infrastructure investments drive sustained growth in household incomes, particularly as communities become progressively integrated into wider economic networks.

In contrast, the employment rate shows no statistically significant effect post treatment, suggesting that income gains are driven by improved job quality rather than an increase in the extensive margin of employment. This interpretation is corroborated by the patterns observed for job quality indicators. The share of employed people with adequate employment rises by 7.9 pp post treatment ($p < 0.01$), with dynamic effects becoming statistically significant from the first year and peaking in years three and four (ATTs of 13.3 and 8.5 pp, respectively). The share of employed people in formal employment also increases post treatment (ATT = 8.0, $p = 0.054$), with dynamic effects significant in years one and two after treatment (ATTs of 12.4 and 14.2 pp, respectively, both $p < 0.01$), and again at year seven (ATT = 23.1, $p = 0.038$).

Taken together, these results provide robust evidence that the welfare gains observed in poverty reduction and income mobility are driven not by a mere increase in employment, but by the transition to higher-quality, more stable, and formal jobs. Improved connectivity enhances access to formal labor markets, better matching between workers and firms, and increases in productivity and wages, all of which translate into sustained improvements in household income and welfare.

3.4 Heterogeneous Effects on Labor Income and Employment Quality

To deepen this analysis, we leverage the granular structure of the RCS data set—constructed by aggregating individual-level household survey data at the max-p region level—to examine treatment effects on labor income and employment quality across specific population subgroups defined by employment sector, firm size, or gender. For each subgroup, we first identify individuals with a given characteristic (for example, self-employed, employed in small firms, by sector, or by gender), after which we compute the per capita labor income and the shares of adequately and formally employed individuals at the region-year level. This disaggregated approach enables us to assess whether the observed welfare gains are broadly distributed or concentrated within particular groups, providing deeper insight into the mechanisms and inclusivity of income mobility following infrastructure investment.

Firm Size Heterogeneity. As reported in table 6, the average treatment effect on labor income is strongly positive for both the self-employed and those working in small firms, with statistically significant posttreatment increases of 23.2 pp ($p < 0.01$) and 21.9 pp ($p < 0.01$) respectively. By contrast, no significant income gains are observed for workers in medium firms.[7] The largest and most sustained effects are seen among the self-employed. These findings suggest that paved road access particularly benefits microentrepreneurs and workers in smaller enterprises, likely by improving market access and reducing transportation costs.

In terms of employment quality, both adequate and formal employment rates rise for self-employed and small-firm workers after road paving. The posttreatment ATT for adequate employment among the self-employed is 8.1 pp ($p < 0.01$) and 5.3 for small-firm employees ($p < 0.01$), while formal employment increases by 10.5 ($p < 0.1$) and 11.8 pp ($p < 0.05$), respectively. Adequate employment in Ecuador refers to earning at least the minimum wage while working 40 or more hours per week; formal employment implies legal registration, compliance with tax/social security laws, and full recognition of labor rights. The results thus indicate that infrastructure improvements help micro- and small-firm workers transition into more stable and regulated work arrangements. Among workers in medium-large firms, effects are smaller, less consistent, and sometimes negative, reflecting greater baseline formality and less scope for marginal improvements.

Sectoral Heterogeneity. Table 7 disaggregates the ATTs by economic sector—primary (agriculture), secondary (industry), and tertiary (services)—to investigate where the main gains in labor income and employment quality are concentrated. The most robust and policy-relevant effects are found in the primary sector, where the posttreatment ATT for labor income is 21.8 percent ($p < 0.01$), with similarly positive and significant effects for adequate employment (4.8 pp, $p < 0.1$). These results highlight that access to

[7] Estimates for large-size firms are not included due to the limited number of observations available for such analysis.

Table 6: ATT Effects on Labor Income and Employment by Firm Size Subsets

ATT	Labor Per Capita Income			Adequate Employment			Formal Employment		
	Self Employed	Small Firm	Medium Firm	Self Employed	Small Firm	Medium Firm	Self Employed	Small Firm	Medium Firm
Pre	-1.0	-0.8	28.8**	2.9	0.5	12.4*	4.1	4.5	8.3
	(10.8)	(10.7)	(14.3)	(4.8)	(2.2)	(6.3)	(5.1)	(4.2)	(6.3)
Post	23.2***	21.9***	-20.9	8.1***	5.3***	0.2	10.5*	11.8**	-7.2
	(6.1)	(6)	(15.4)	(2.6)	(1.8)	(5.8)	(6.2)	(4.6)	(5.8)
$t = -3$	-6.0	-9.6	34.9**	-0.3	-1.2	13.0	8.2	7.0	6.5
	(14.2)	(13.3)	(17.2)	(5.9)	(2.9)	(8.2)	(6.8)	(5.7)	(8.3)
$t = -2$	-1.4	-1.2	27.9*	4.5	-0.4	12.7*	4.4	3.7	11.7*
	(11.9)	(11.8)	(16.7)	(5)	(2.4)	(7.7)	(5.7)	(4.7)	(6.9)
$t = -1$	4.2	8.4	23.7	4.5	3.2	11.4	-0.3	2.8	6.8
	(11.6)	(11.5)	(16.5)	(4.8)	(2.5)	(7)	(5.3)	(4.4)	(6.9)
$t = 0$	-2.9	-3.1	22.8	-0.5	1.4	7.4	4.5	3.1	1.9
	(11)	(10.3)	(19.9)	(4.4)	(2.2)	(8.8)	(5.7)	(4.7)	(8.5)
$t = 1$	4.9	6.8	-49.9	-2.2	4.5*	16.5	5.1	10.7**	2.7
	(9)	(9.7)	(59.9)	(4.2)	(2.7)	(16.8)	(6.5)	(4.9)	(8)
$t = 2$	10.0	5.7	-2.7	2.4	6.4**	15.8*	9.0	15.2***	5.5
	(13.4)	(10.6)	(23.1)	(4.1)	(2.5)	(9.1)	(6.8)	(5.7)	(8.4)
$t = 3$	33.7***	20.8***	8.4	15.8***	6.2**	12.8	5.9	8.6	-1.1
	(10.9)	(7.8)	(22.1)	(4.2)	(2.6)	(10.1)	(8.4)	(6.3)	(8.8)
$t = 4$	36.7***	34.8***	-21.1	18.7***	5.6*	-12.2	7.9	3.4	-21.7*
	(7.8)	(8.5)	(30)	(4.5)	(3)	(8)	(11.4)	(8.5)	(11.8)
$t = 5$	53.2***	45.9***	-44.9	7.2	7.1*	-23.3**	-9.3	3.5	-17.8
	(14)	(13.9)	(35.1)	(5.2)	(4.2)	(10.3)	(12)	(8)	(11.7)
$t = 6$	6.6	18.2	-36.7	8.2*	6.6*	-19.7	25.2**	17.6*	-23.9*
	(15.8)	(14.5)	(33.2)	(4.9)	(3.4)	(14.8)	(11.9)	(9.3)	(12.6)
$t = 7$	43.8***	46.4***	-42.8	15.2**	4.4	4.4	35.7**	32.3***	-2.9
	(16.4)	(16.8)	(61.4)	(6.3)	(3.3)	(17.7)	(15.1)	(12.3)	(16)

Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. $^{*} p < 0.1$; $^{**} p < 0.05$; $^{***} p < 0.01$.

paved roads particularly benefits workers in agriculture and related activities, likely by reducing transport costs and enhancing market connectivity for rural producers.

While point estimates for labor income in the secondary sector are large and statistically significant post treatment (53.9 percent, $p < 0.05$), the parallel trends assumption is violated for this sector, as the pretreatment ATT is also positive and statistically significant (67.3 percent, $p < 0.05$). This indicates preexisting differences in trends between treated and control regions and raises concerns about causal interpretation. For this reason, we refrain from attributing posttreatment gains in the secondary sector to road access and do not emphasize these results in the discussion.

In the tertiary sector, effects are positive for labor income but not statistically significant, indicating that the main drivers of the observed welfare improvements are rooted in the primary sector. Patterns in adequate and formal employment echo these findings: significant posttreatment increases are seen in the primary sector, while the secondary sector exhibits inconsistent or negative effects (and is subject to identification concerns

Table 7: ATT Effects on Labor Income and Employment by Sector Subsets

ATT	Labor Per Capita Income			Adequate Employment			Formal Employment		
	Primary	Secondary	Tertiary	Primary	Secondary	Tertiary	Primary	Secondary	Tertiary
Pre	-6.8	67.3**	-0.2	-0.9	12.6	1.0	-0.2	-26.1**	2.3
	(9.0)	(29.7)	(13.2)	(2.4)	(13.3)	(6.3)	(4.6)	(11.8)	(7.2)
Post	21.8***	53.9**	14.6	4.8*	-20.5	0.2	6.8	-25.3	-2.9
	(7.5)	(25.8)	(14.7)	(2.5)	(23.7)	(5)	(5.3)	(21.7)	(6.7)
$t = -3$	-12.5	129.9**	-1.6	0.6	45.2**	-0.8	2.7	-30.1*	5.5
	(12.3)	(56.8)	(16)	(4)	(22.8)	(7.9)	(6.1)	(16.1)	(9.2)
$t = -2$	-4.4	14.6	-1.4	-1.9	6.2	-2.3	0.5	-13.2	-0.2
	(10.4)	(29.9)	(13.8)	(2.5)	(12.6)	(7)	(5.2)	(12.8)	(7.6)
$t = -1$	-3.5	57.4**	2.5	-1.5	-13.5	6.0	-3.7	-35.1**	1.6
	(10.0)	(27.6)	(14.2)	(3)	(17.5)	(6.6)	(4.9)	(17.4)	(7.5)
$t = 0$	-13.2	118.8	1.9	0.2	56.5	-1.5	0.0	-15.4	-1.5
	(11.0)	(118.5)	(14.1)	(2.4)	(58.5)	(6.7)	(5)	(11.8)	(7.5)
$t = 1$	19.6**	-5.6	-13.9	7.8**	-28.2	-16.9	8.3	-19.2	-2.8
	(9.4)	(30.2)	(41.9)	(3.5)	(26.7)	(12.3)	(5.3)	(29.3)	(13.5)
$t = 2$	12.2	26.5	31.8***	11.9***	-35.6	4.3	9.5	-20.6	8.3
	(10.9)	(33.8)	(11.4)	(4)	(27.6)	(6.7)	(6.2)	(26.4)	(6.6)
$t = 3$	11.8	23.2	-28.6	7.8*	-38.7	25.7	5.4	-44.1	-24.1
	(11.4)	(41.8)	(58.9)	(4.1)	(36.8)	(25.9)	(7.1)	(43.4)	(27.2)
$t = 4$	21.6*	45.6	31.5**	4.1	-6.3	1.8	1.3	0.5	-2.8
	(12.2)	(50.9)	(12.1)	(4.9)	(89.7)	(7.2)	(9)	(89.2)	(10.8)
$t = 5$	32.0**	82.4**	56.1***	-3.9	-8.2	-2.5	-0.8	-37.6**	-12.7
	(14.0)	(38.3)	(14.6)	(5.5)	(16.5)	(7.4)	(7.6)	(18.7)	(8.6)
$t = 6$	42.8***	18.5	13.4	4.0	-13.2	0.7	9.9	7.0	15.8
	(13.7)	(31.1)	(21.7)	(5.0)	(26.2)	(14)	(9.8)	(24.2)	(12.1)
$t = 7$	47.9	122.1**	24.3	6.1	-90.0	-9.7	21.0	-72.8	-3.2
	(31.7)	(62.1)	(30.5)	(5.0)	(91.9)	(16)	(14.9)	(93.3)	(13)

Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

as noted above). No statistically significant changes are detected in the tertiary sector.

Gender Heterogeneity. Disaggregating by gender (table 8), paved major roads generate significant and sizable gains in labor income for both men (29.6 percent, $p < 0.01$) and women (22.3 percent, $p < 0.01$) post treatment. However, quality of employment effects differ: men experience a significant rise in adequate employment (14.9 pp, $p < 0.01$), while the effect for women is negative and statistically insignificant. For formal employment, posttreatment effects are positive and marginally significant for men (4.9pp) and significant for women (9.7 pp, $p < 0.1$), suggesting that infrastructure-driven formalization is inclusive, although the pathway to adequate employment for women may remain constrained by persistent structural or social barriers.

Dynamic estimates show that men's income and adequate employment gains tend to accumulate steadily and peak in later years, while women's income gains are similarly strong but not accompanied by robust improvements in job quality measures. Notably, formal employment for women increases significantly in the years immediately following treatment, indicating some narrowing of gender gaps in access to regulated employment.

Table 8: ATT Effects on Labor Income and Employment by Gender Subsets

ATT	Labor Per Capita Income		Adequate Employment		Formal Employment	
	Men	Women	Men	Women	Men	Women
Pre	0.4	-2.9	1.5	3.8	6.2	1.5
	(8.5)	(9.6)	(3.8)	(4.3)	(5.3)	(4.6)
Post	29.6***	22.3***	14.9***	-2.9	4.9	9.7*
	(5.7)	(7.2)	(4)	(4.3)	(4.4)	(5.2)
$t = -3$	-4.6	-9.1	-1.0	4.5	7.4	6.8
	(10.9)	(11.9)	(5)	(5.7)	(6.7)	(5.9)
$t = -2$	-1.2	-3.7	-1.4	4.0	6.9	0.4
	(10)	(11)	(4.4)	(4.4)	(5.9)	(5.2)
$t = -1$	7.0	4.1	6.8	2.9	4.2	-2.8
	(10)	(11.1)	(4.2)	(4.5)	(5.3)	(5)
$t = 0$	-2.0	-3.8	2.0	0.2	1.0	0.5
	(10.7)	(11.2)	(4.6)	(4.1)	(5.3)	(5.2)
$t = 1$	17.6*	9.1	12.6**	0.8	9.2*	16.9***
	(10)	(10.6)	(5)	(4.5)	(5)	(5.7)
$t = 2$	17.0	10.1	15.5***	5.6	11.9**	14.1**
	(12)	(12.3)	(5.4)	(4.1)	(5.5)	(6.9)
$t = 3$	32.7***	33.2***	19.4***	0.6	8.2	0.2
	(7.8)	(9.6)	(4.8)	(4.2)	(6.1)	(7.3)
$t = 4$	41.1***	42.2***	17.4***	-0.4	-3.1	-0.9
	(8.9)	(8.4)	(6.5)	(7.4)	(7.6)	(11.7)
$t = 5$	46.6***	40.2***	14.1*	0.5	-1.7	2.5
	(14)	(13.5)	(8.2)	(7.2)	(6.9)	(11.6)
$t = 6$	29.8**	12.5	13.4*	-8.1	-1.7	19.0
	(13.2)	(14.7)	(8.1)	(8)	(7.7)	(11.8)
$t = 7$	54.2***	34.9	24.8**	-22.7*	15.3	25.0***
	(13.9)	(23.9)	(10.9)	(12.1)	(13.2)	(9.6)

Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Overall, these results underscore the heterogeneous returns to infrastructure investment, which are most pronounced among the self-employed, small-firm workers, and men, but are also substantial for women in terms of income and formal employment. The strong income and job quality effects for microentrepreneurs and primary sector workers suggest that road access disproportionately benefits populations previously constrained by poor connectivity. In Ecuador's context—where adequate employment requires a living wage and full-time hours, and formal employment connotes full legal protection—the evidence suggests that paved roads not only raise average incomes but also promote the transition to more-secure, legally recognized forms of work, particularly among traditionally vulnerable groups.

3.5 Robustness

This subsection assesses the robustness of our main findings to alternative specifications. The set of checks includes (1) replacing the World Bank's international poverty lines with

Ecuador's official moderate and extreme poverty thresholds; (2) reestimating treatment effects using not-yet-treated units as the control group; and (3) testing the sensitivity of results to alternative max-p region constructions based on different clustering criteria and minimum parish requirements. Across all exercises, we examine both ATTs and dynamic impacts over event time, confirming the absence of pretreatment divergences and the persistence of posttreatment gains. All supporting tables and figures are reported in appendix B, which documents the full set of robustness checks and provides the corresponding spatial maps, estimation results, and dynamic effect plots.

Does the Poverty Definition Matter? A central concern in poverty impact evaluations is whether estimated effects are sensitive to the choice of poverty metric. While our main analysis adopts the World Bank's international poverty lines of \$3.65 and \$6.85 per day (2017 PPP), policy relevance in the Ecuadorian context also requires testing robustness to nationally defined thresholds. To this end, we reestimate the model using Ecuador's official moderate and extreme poverty headcount ratios, providing a complementary lens aligned with national policy benchmarks.

Table B.1 reports the ATTs using these official definitions. For both moderate and extreme poverty, pretreatment ATTs are small in magnitude and statistically insignificant, indicating no systematic preintervention differences in trends between treated and control regions—an important validation of the parallel trends assumption underpinning the causal interpretation. Posttreatment estimates reveal a large and statistically significant decline in moderate poverty of -13.60 pp ($p < 0.01$) and a reduction in extreme poverty of -3.35 pp, marginally significant at the 10 percent level. The magnitude and sign of these effects closely mirror those obtained with the international poverty thresholds, suggesting that the poverty-reducing effect of road access is not an artifact of the poverty line chosen.

Dynamic treatment effects, depicted in figure B.1, further reinforce this conclusion. Both indicators remain statistically indistinguishable from zero in the pretreatment period, while the posttreatment trajectory shows persistent and significant declines—particularly for moderate poverty, which exhibits sustained reductions from the second year onward. This temporal pattern aligns closely with the dynamics observed under the \$3.65 and \$6.85 poverty lines, underscoring the internal consistency of the results.

Never Treated versus Not Yet Treated Our primary identification strategy employs "never-treated" max-p regions as the control group, consistent with best practices in the recent DiD literature and central to the credibility of the parallel trends assumption. Nonetheless, an important concern is whether never-treated regions may differ systematically from treated regions—either in observed or unobserved dimensions—potentially biasing the estimated effects. To address this concern and assess the robustness of our findings to control group composition, we reestimate the treatment effects using "not-yet-treated" regions—those that will eventually receive paved road access but remain untreated in the pretreatment period—as the counterfactual.

Table B.2 reports the ATTs for a comprehensive set of outcomes under this alternative specification. Across all variables, the pretreatment ATTs are statistically insignificant, suggesting that treated and not-yet-treated regions share similar preintervention trajectories, thereby reinforcing the plausibility of the parallel trends assumption in this alternative framework.

Posttreatment estimates remain consistent with our main results. At the international poverty lines, we find statistically significant reductions of -7.38 pp for the \$3.65 line and -16.59 pp for the \$6.85 line (both $p < 0.01$). Similarly, the share of the population classified as vulnerable rises by 11.76 pp, while the middle-class share increases by 4.83 pp (both $p < 0.01$). These results parallel those from the never-treated specification, underscoring the stability of the estimated welfare gains.

Economic welfare indicators also exhibit robust improvements: overall and labor per capita income rise by approximately 20 percent post treatment, and substantial gains are observed in adequate employment (8.09 pp, $p < 0.01$) and formal employment (8.03 pp, $p < 0.1$). Employment rates, however, remain statistically unchanged, mirroring our baseline findings.

Figures B.2 and B.3 visualize the dynamic treatment effects over event time, confirming that the trajectory and timing of impacts are virtually identical when using not-yet-treated regions as the control group. The absence of pretreatment deviations and the persistence of posttreatment benefits across all key outcomes provide compelling evidence that the estimated effects of paved road access on poverty reduction, income mobility, and labor market quality are not artifacts of control group selection. Instead, they reflect robust and generalizable causal relationships in the Ecuadorian context.

Alternative Regional Aggregations To verify that our results are not biased to the specific pseudo-regional aggregation chosen, we conduct robustness checks using two alternative max-p clustering specifications. In the first, parishes are grouped using 2010 census population data and World Bank SAE poverty rates, with the additional restriction that each max-p region must contain at least four parishes. In the second, we employ 2010 census population data and Unsatisfied Basic Needs (UBN) poverty rates as pretreatment clustering attributes,[8] ensuring a minimum of three parishes per max-p region. The resulting spatial configurations are displayed in figures B.4 and B.5, which contrast parish-level poverty distributions with the aggregated max-p regional units for each specification.

Table B.3 presents the overall pre- and posttreatment ATTs for the full set of outcome variables under both alternative aggregations. The pretreatment coefficients are consistently small and statistically insignificant across all outcomes, providing further evidence that the parallel trends assumption holds under these alternative geographic definitions. The posttreatment results closely mirror our baseline findings: significant poverty reductions at the $3.65 and $6.85 international lines, increases in the shares of the vulnerable and middle-class populations, and robust gains in both overall and labor per capita income. Labor market outcomes also remain consistent, with significant improvements in adequate and formal employment and no statistically meaningful change in the overall employment rate.

Moreover, the dynamic effects illustrated in figures B.6, B.7, B.8, and B.9 further corroborate the robustness of our findings. In both alternative aggregations, the trajectories of treatment effects over event time align closely with the baseline estimates, showing no pretreatment deviations and sustained posttreatment gains.

The persistence of these effects across alternative max-p configurations reinforces the conclusion that our main results are not sensitive to the specific regional partitioning of parishes. Instead, they reflect a robust empirical relationship between paved road access and improvements in poverty reduction, income mobility, and labor market quality, irrespective of the precise geographic aggregation applied.

[8]We use the pretreatment UBN poverty rate to cluster parishes on a proxy of structural poverty, rather than a purely monetary measure, thereby capturing persistent deprivation related to access to basic services and infrastructure.

4 Conclusions

Well-maintained roads facilitate the movement of goods and people, promoting economic development through the more efficient distribution and allocation of resources. Can roads also be a tool for the eradication of extreme poverty? In Ecuador, at least, our results paint a mixed picture. While poverty headcounts are reduced in some poverty categories as a result of road construction, we do not find evidence that access to roads moved people out of the most extreme category of poverty. The building program did, however, improve outcomes in nearly every other category examined: middle class income categories expanded, while labor incomes increased for most groups, including the self-employed, those working in the informal and formal sectors, and those in the primary sector.

These results corroborate previous findings while offering new insights. Previous research has also found that the worst-off individuals do not benefit from roads (Asher and Novosad, 2020; Spey et al., 2019; Hine et al., 2019; Gachassin et al., 2010). An important consideration is in making sure those in extreme poverty have adequate access to complementary transportation services. Yet even this may prove insufficient. Many of the poorest households in Ecuador rely on subsistence agriculture, so that we would not expect to see nonfarm income gains (or even measurable income). Nonetheless, access to both better and lower-priced inputs, when combined with agricultural extension activities, has been show to raise living standards for these types of households(Gebresilasse, 2023). Additionally, lowered output prices as a result of roads may provide welfare improvements (for example, Aggarwal (2018)). This would be a productive area for future research.

A final consideration, given that income gains occur largely within the primary sector, is how this income is produced and the way in which it affects the local environment. Roads and deforestation in the Amazon region are strongly linked (Barber et al., 2014; Mena et al., 2006; da Silva et al., 2023). It is conceivable that the socioeconomic gains we measure come as a result of more-extensive cultivation or extractive-industry activity, which may be unsustainable. This potentially brings difficult policy trade-offs and is worthy of continued monitoring by researchers.

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A Regional Clusterization and the Max-p Algorithm

Estimation at the parish level may yield biased or statistically inefficient results due to the lack of representativeness in the available data at such a fine spatial resolution. To address this limitation, we implement a spatial aggregation strategy by grouping contiguous parishes, thereby enhancing statistical reliability under the assumption of spatial dependence—specifically, that neighboring parishes are subject to analogous patterns of forest loss and exhibit similar macroeconomic conditions. The aggregation process, however, confers inferential benefits only if spatial homogeneity exists among adjacent units; in the absence of such similarity, aggregation does not improve representativeness, because the increase in population and sample size merely reflects an accumulation of near-independent observations without a corresponding reduction in estimation uncertainty.

Let us formalize this intuition with a stylized example. Suppose region A comprises four constituent parishes indexed by $i \in 1, 2, 3, 4$. Let $\boldsymbol{Y}_A$ denote the distribution of household incomes in region A, with $\boldsymbol{Y}_{A,i}$ representing the vector of observed incomes for n_i households in parish i. Each $\boldsymbol{Y}_{A,i}$ is of dimension n_i, such that the aggregate sample size in region A is $n_A = \sum_{i=1}^{4} n_i$. When income distributions are relatively homogeneous across parishes, smaller samples suffice to generate representative estimates. However, in the presence of interparish heterogeneity, a substantially larger n_A is required to characterize $\boldsymbol{Y}_A$ accurately. Consequently, inference efficiency improves with aggregation only if the underlying data-generating processes are sufficiently similar across the aggregated parishes.

To execute the aggregation, we utilize the max-p region algorithm proposed by Duque et al. (2012), which partitions a set of spatially contiguous units into p regions, subject to a constraint on a minimum threshold (for example, population size or number of poor) while minimizing intraregion attribute heterogeneity and maximizing interregion heterogeneity. To illustrate the mechanics of this method, Duque et al. (2012) provide an example where 9 spatial units are grouped based on average housing prices, constrained such that each resulting region contains no fewer than 120 housing units. The solution (shown in figure A.1) yields two clusters, one in the northeast comprising low-price units and one in the southwest with high-price units, thereby optimizing the trade-off between spatial contiguity and attribute similarity.

In our application, we implement regional clusterization in order to group parishes exhibiting similar poverty dynamics over the study period. This procedure ensures that

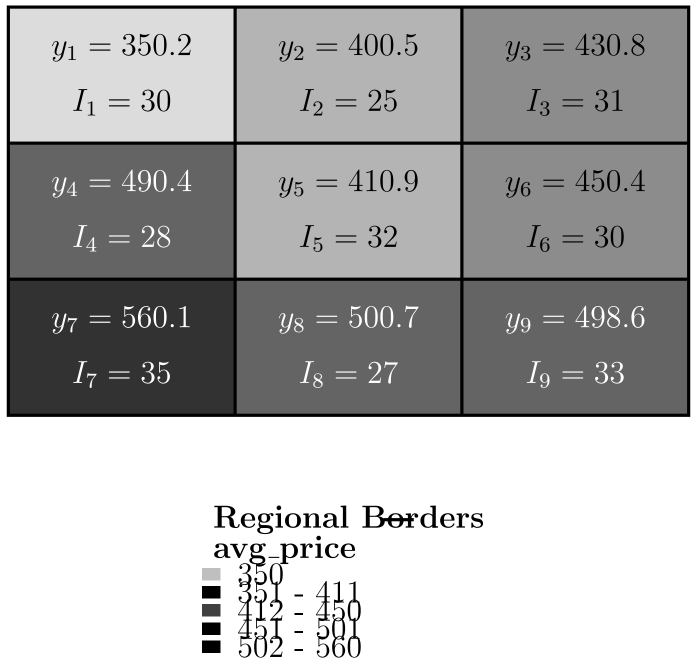


Figure A.1: Optimal Solution of the max-p region Algorithm for a Threshold of 120 Houses (*Source:* Duque et al. (2012), page number, figure 2.).

the resulting spatial units are both statistically representative and internally coherent. The max-p region algorithm is applied to a set of 1,016 parishes in Ecuador using spatially extensive attributes and a user-defined threshold to guide the optimization. Additional implementation details are provided in section 2.2.

B Results of Robustness Checks

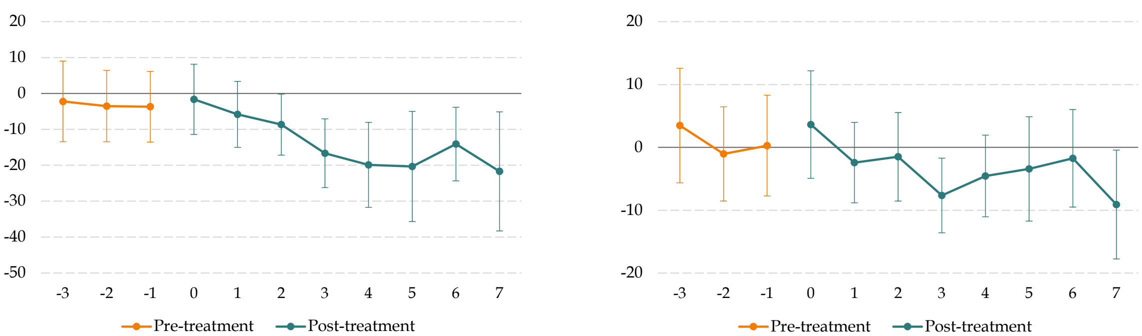
Table B.1: Overall Pre- and Post-ATT Effects on Ecuador's Official Poverty Headcount Ratios

Outcome variable	ATT (θ^0_{es})	
	Pre	Post
Moderate poverty line	-3.15	-13.60***
	(4.41)	(2.95)
Extreme poverty line	0.91	-3.35*
	(3.51)	(1.97)

Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Figure B.1: Dynamic ATT Effects on Ecuador's Official Poverty Headcount Ratios

(a) Official moderate poverty line (b) Official extreme poverty line



Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as control group. Confidence intervals: 95 percent.

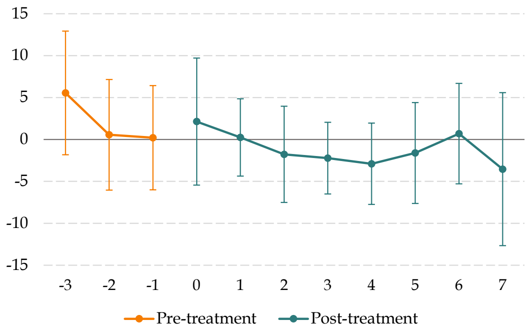
Table B.2: Overall Pre- and PostATT Effects With Not-Yet-Treated as Control Group

Outcome variable	ATT (θ^0_{es})	
	Pre	Post
$2.5 Poverty line	2.11	-1.12
	(2.90)	(1.58)
$3.65 Poverty line	-0.28	-7.38***
	(3.71)	(2.21)
$6.85 Poverty line	-1.39	-16.59***
	(4.23)	(3.12)
Vulnerable	1.69	11.76***
	(3.39)	(2.71)
Middle class	-0.30	4.83***
	(2.89)	(1.77)
Overall per capita income	3.36	20.00***
	(7.50)	(4.56)
Labor per capita income	1.68	20.80***
	(8.90)	(5.90)
Employment rate	0.26	-1.41
	(0.64)	(0.93)
Adequate employment	1.00	8.09***
	(3.50)	(2.06)
Formal employment	-0.84	8.03*
	(4.10)	(3.30)

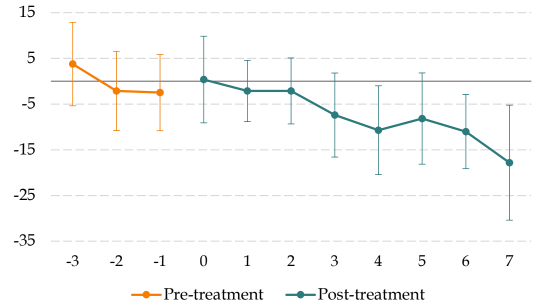
Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Not-yet-treated units are used as a control group.

Figure B.2: Dynamic ATTs With Not-Yet-Treated as Control Group

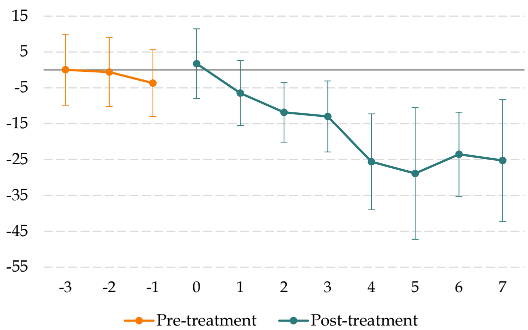
(a) $2.5 Poverty line



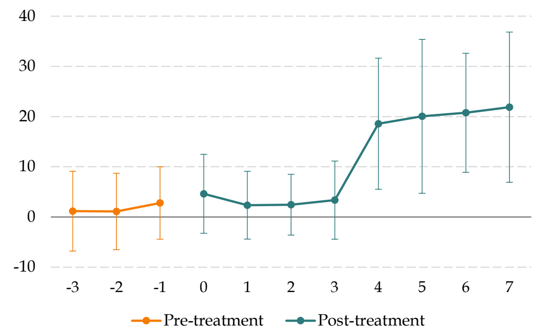
(b) $3.65 Poverty line



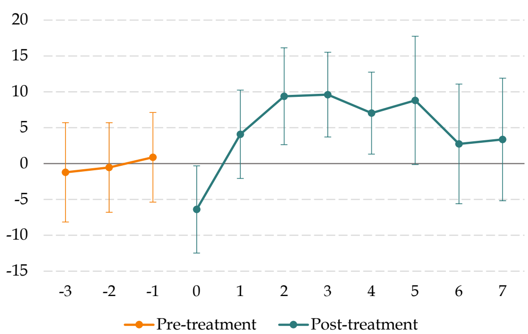
(c) $6.85 Poverty line



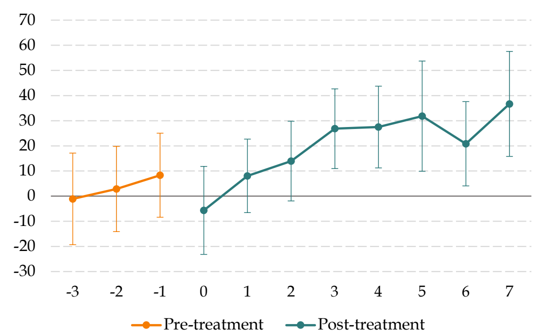
(d) Vulnerable



(e) Middle class

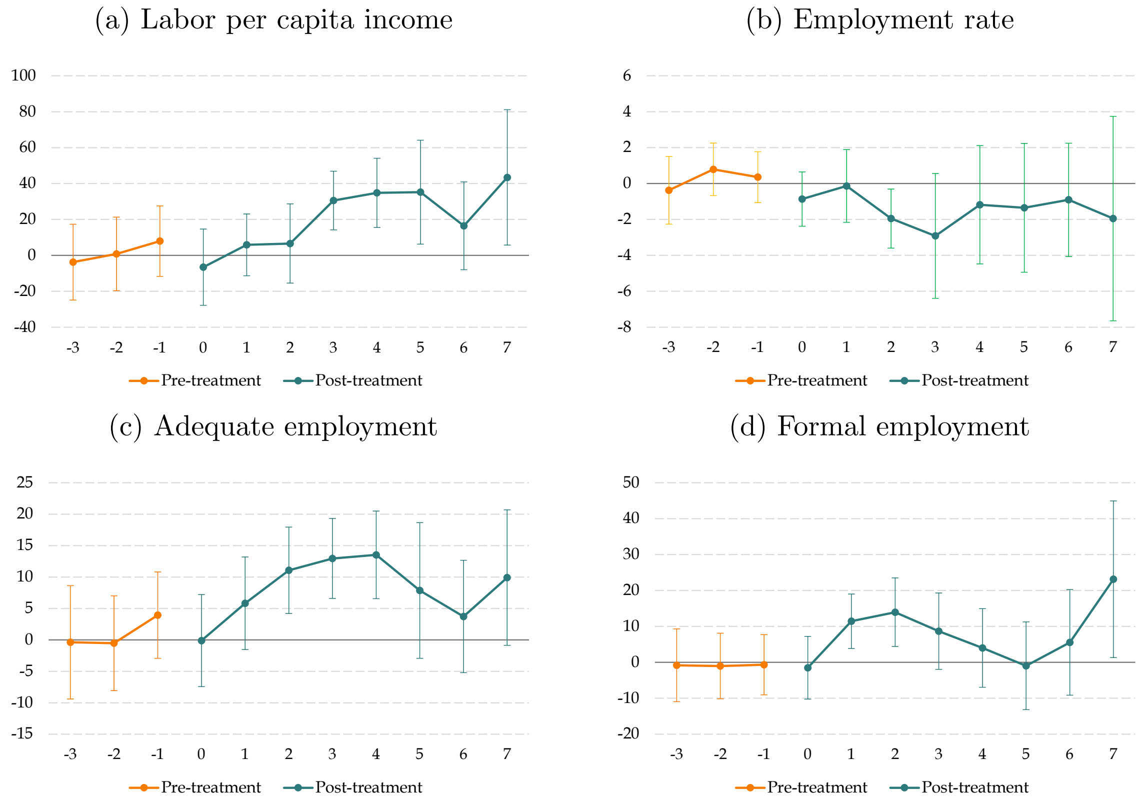


(f) Overall per capita income



Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Not-yet-treated units are used as a control group. Confidence intervals: 95 percent.

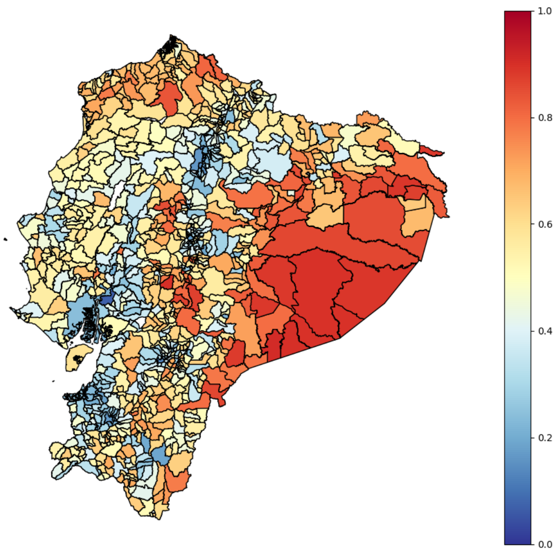
Figure B.3: Dynamic ATTs With Not-Yet-Treated as Control Group



Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Not-yet-treated units are used as a control group. Confidence intervals: 95 percent.

Figure B.4: Maps of 2010 World Bank SAE Poverty Headcount Ratios by Parishes and Max-p Regions with at least 4 Parishes

(a) Parish level



(b) ≥ 4 parishes by max-p region

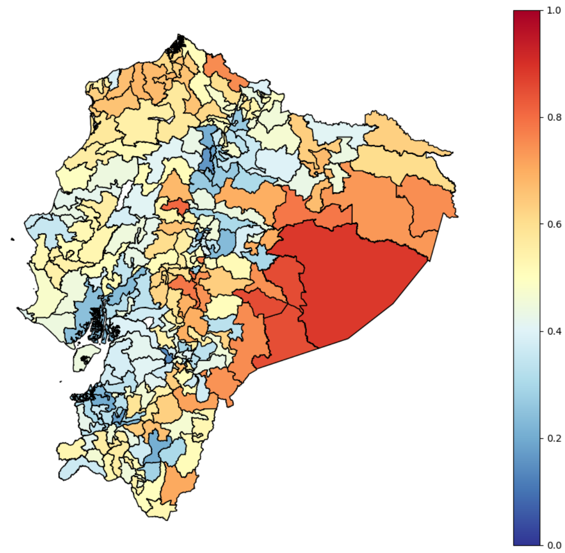
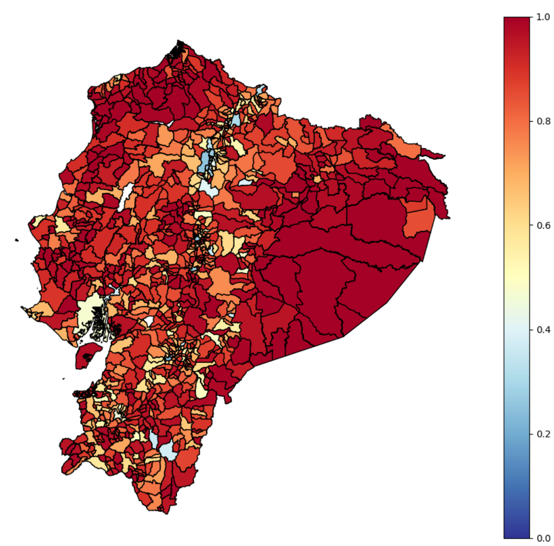


Figure B.5: Maps of 2010-Census UBN Poverty Headcount Ratios by Parishes and Max-p Regions Using Pretreatment UBN Poverty Rates for Clustering

(a) Parish level



(b) Max-p region using UBN poverty for clustering

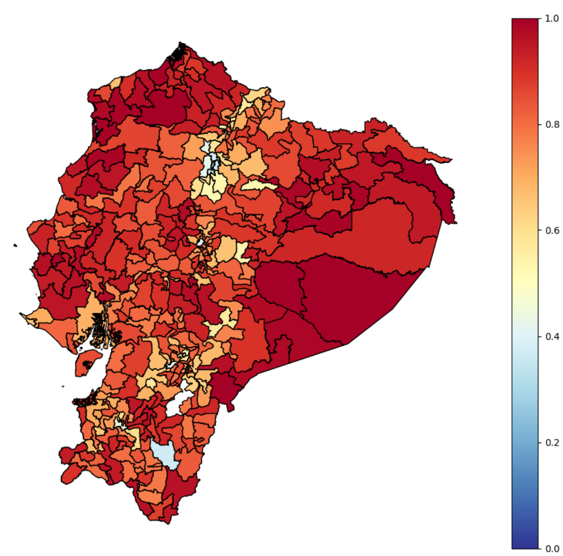


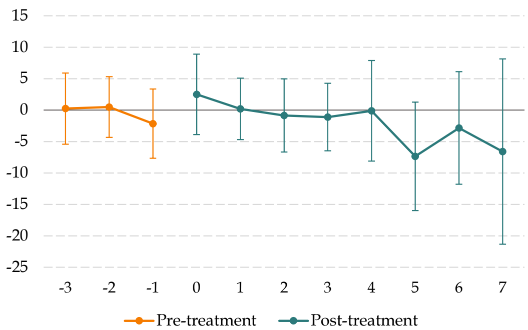
Table B.3: Overall Pre- and PostATT Effects by Alternative Max-p Aggregations

Outcome variable	$\geq$ 4 parishes (1)		Clustered on UBN poverty (2)	
	Pre	Post	Pre	Post
\$2.5 Poverty line	0.66	0.44	-0.48	-2.02
	(3.20)	(2.39)	(1.09)	(2.70)
\$3.65 Poverty line	0.81	-6.69**	-0.72	-10.53***
	(4.53)	(3.12)	(1.48)	(3.46)
\$6.85 Poverty line	4.07	-10.21**	0.07	-13.45***
	(3.95)	(4.81)	(1.63)	(3.66)
Vulnerable	2.49	7.17**	-0.06	9.13***
	(3.32)	(3.21)	(1.27)	(2.79)
Middle class	1.71	5.45**	0.00	4.32**
	(3.68)	(2.25)	(1.02)	(2.14)
Overall per capita income	6.08	16.46**	-0.38	23.50***
	(9.65)	(7.25)	(2.71)	(6.74)
Labor per capita income	10.02	19.41**	-0.18	25.67***
	(10.71)	(9.05)	(3.02)	(7.95)
Employment rate	-0.14	-0.58	0.04	-0.73
	(0.76)	(0.85)	(0.24)	(0.73)
Adequate employment	4.75	7.30***	-1.05	6.20***
	(3.61)	(2.40)	(1.04)	(2.36)
Formal employment	-0.66	7.53*	1.09	5.81*
	(3.82)	(4.48)	(1.39)	(5.38)

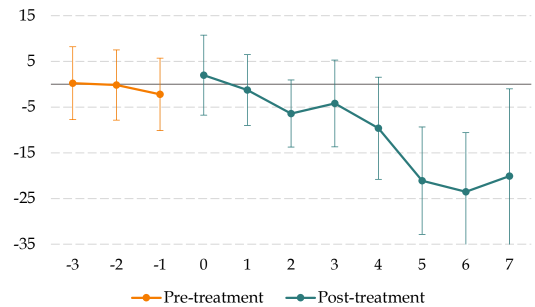
(1) The clustering employs 2010 census data on population and World Bank SAEpoverty rates, with each max-p region containing at least 4 parishes. (2) The clustering employs 2010 census data on population and UBN poverty rates, with each max-p region containing at least 3 parishes. Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Never-treated units are used as a control group.

Figure B.6: Dynamic ATTs Using Max-p Regions Clustered on Pretreatment UBN Poverty Rates

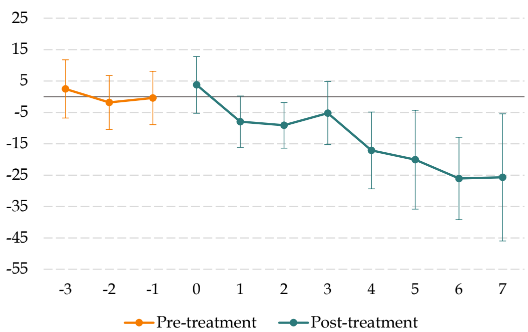
(a) $2.5 Poverty line



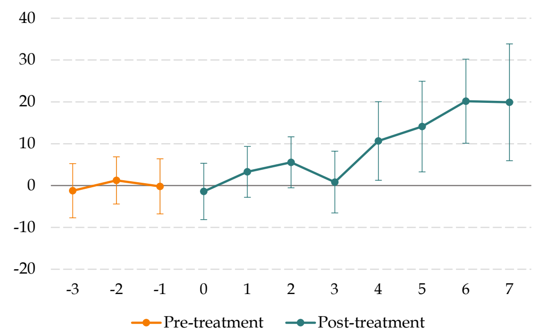
(b) $3.65 Poverty line



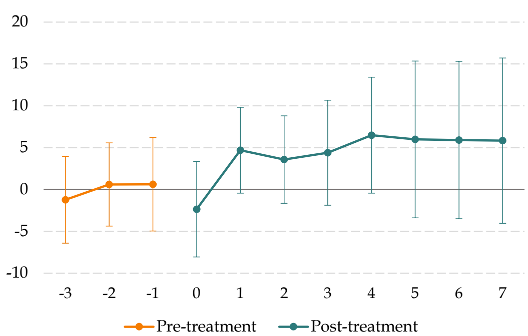
(c) $6.85 Poverty line



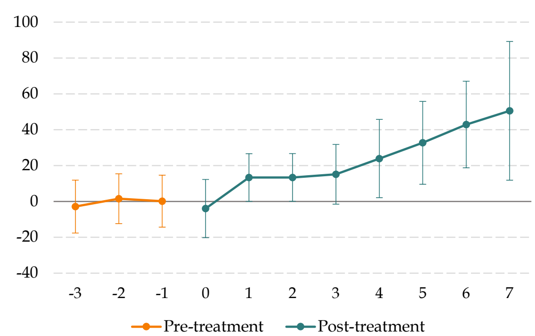
(d) Vulnerable



(e) Middle class



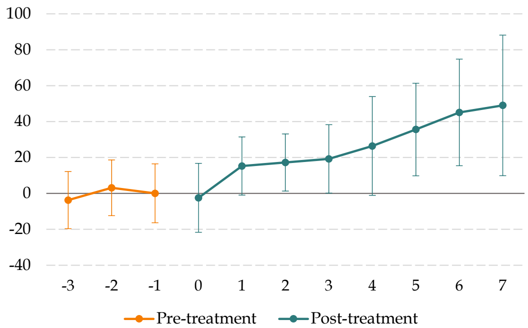
(f) Overall per capita income



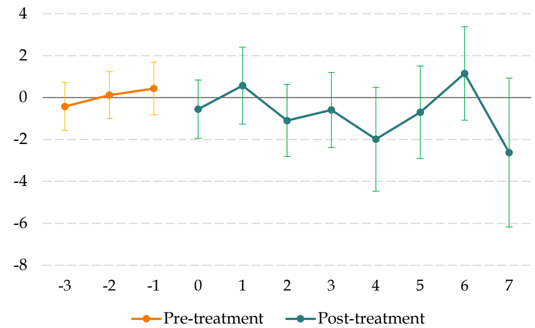
Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. Confidence intervals: 95 percent.

Figure B.7: Dynamic ATT Effects Using Max-p Regions Clustered on Pretreatment UBN Poverty Rates

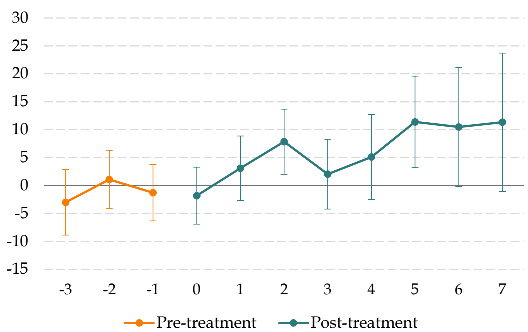
(a) Labor per capita income



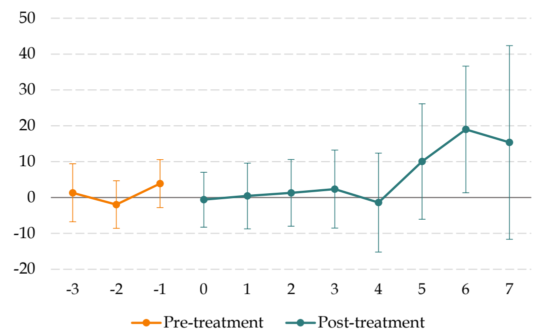
(b) Employment rate



(c) Adequate employment



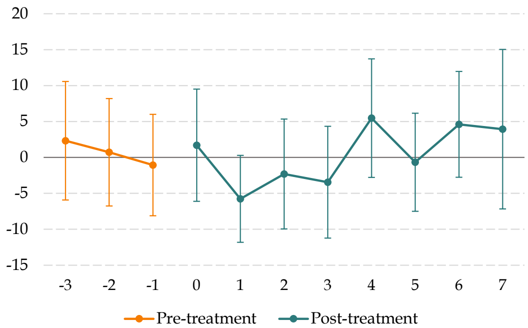
(d) Formal employment



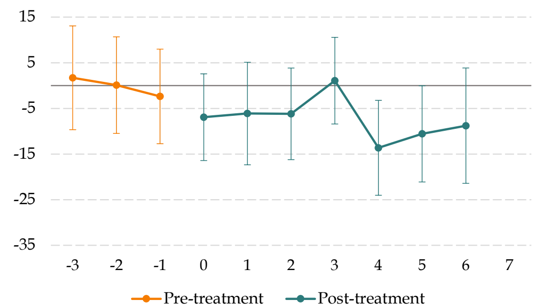
Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. Confidence intervals: 95 percent.

Figure B.8: Dynamic ATT Effects Using Max-p Regions with at least 4 Parishes

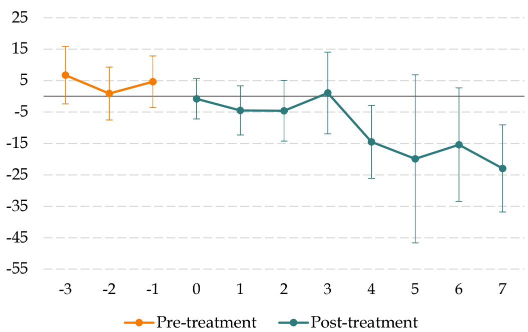
(a) \$2.5 Poverty line



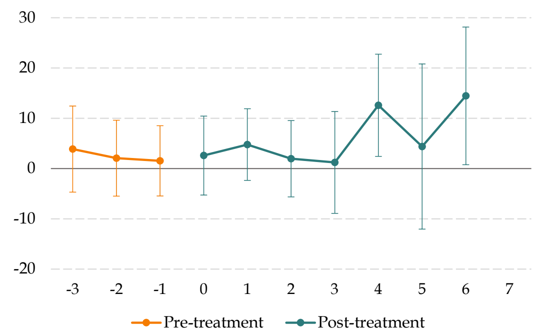
(b) \$3.65 Poverty line



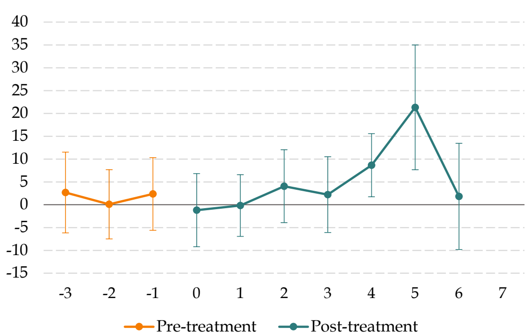
(c) \$6.85 Poverty line



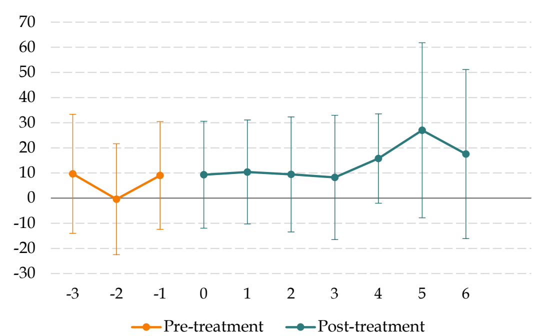
(d) Vulnerable



(e) Middle class



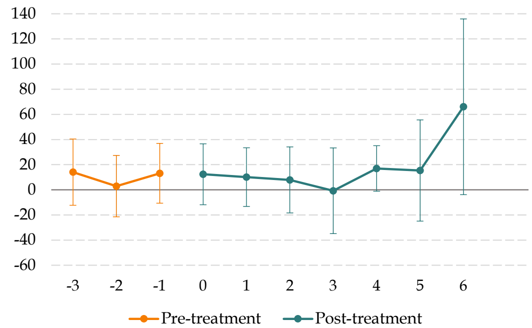
(f) Overall per capita income



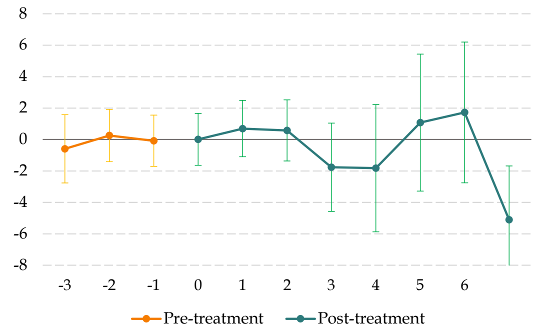
Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. Confidence intervals: 95 percent.

Figure B.9: Dynamic ATT Effects Using Max-p Regions with at least 4 Parishes

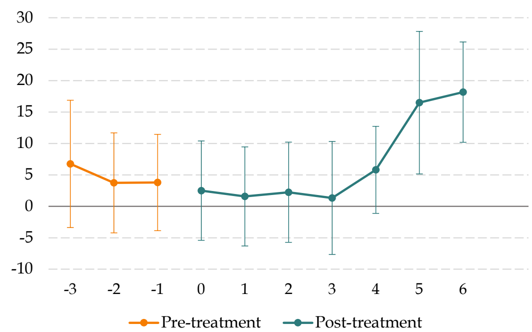
(a) Labor per capita income



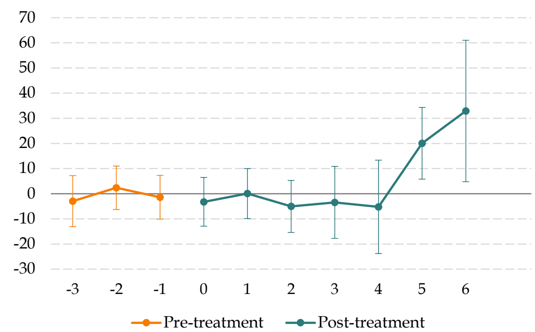
(b) Employment rate



(c) Adequate employment

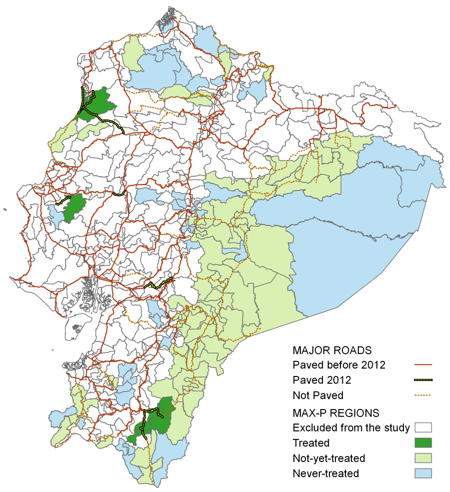


(d) Formal employment

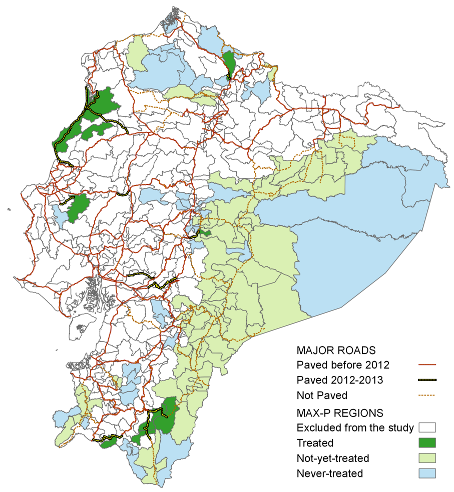


Note: Robust standard errors clustered at the pseudo-region (max-p) level in parentheses. Never-treated units are used as a control group. Confidence intervals: 95 percent.

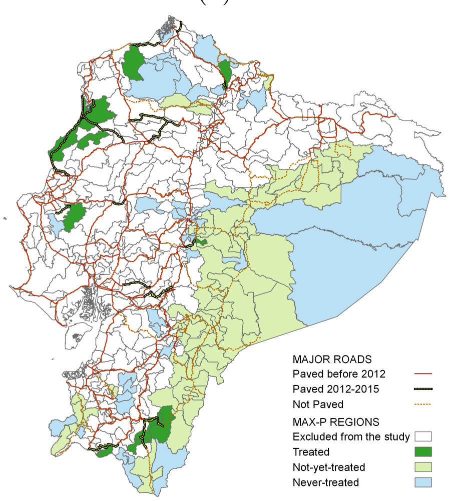
C Maps of Treatment Allocation Over Time



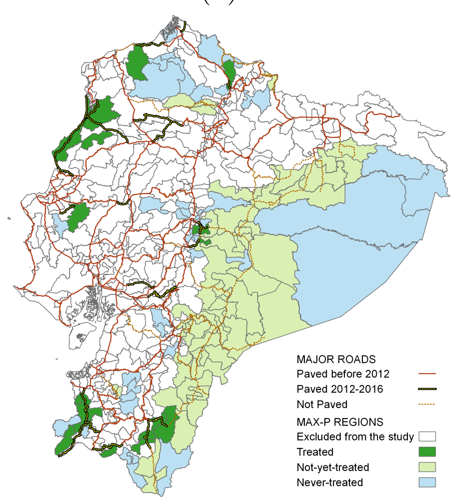
(a) 2012



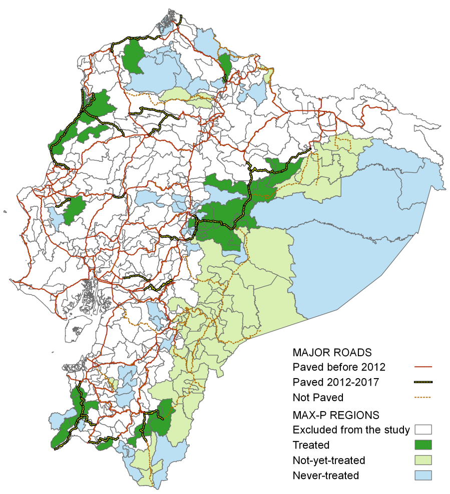
(b) 2013



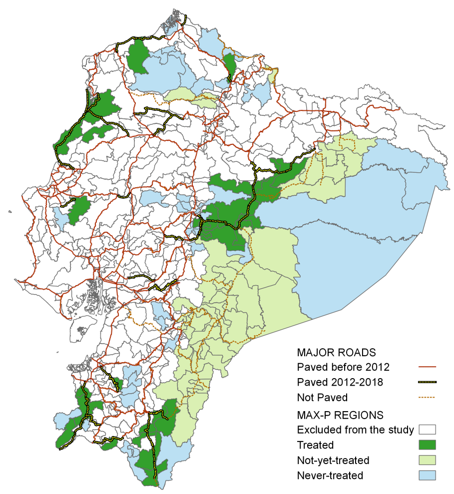
(c) 2015



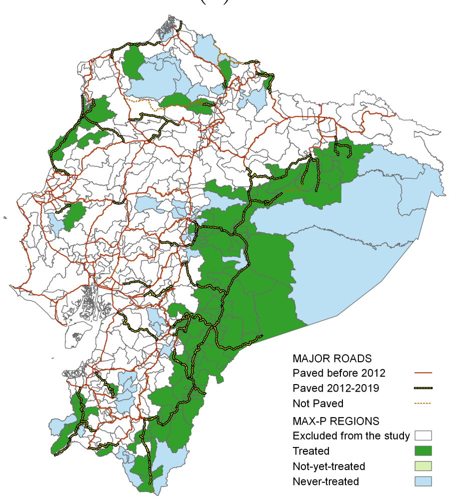
(d) 2016



(a) 2017



(b) 2018



(c) 2019

D RCS Data Set: Variable Description and Statistics

This appendix provides additional information on the construction and content of the RCS data set used in the empirical analysis. The RCS data set aggregates individual-level data from Ecuador's ENEMDU household surveys for the period 2010–2019, producing annual region-level indicators for 101 max-p regions. These geographical units either gained access to a paved major road between 2012 and 2019 or remained untreated throughout the study period, thereby enabling the staggered treatment design adopted in the main analysis.[9] The final data set contains 831 region-year observations, supporting evaluation of socioeconomic impacts over time and across space.

Once a region receives treatment, its treated status remains fixed in subsequent years. Table D.1 presents the distribution of max-p regions according to their year of treatment and illustrates the (cross-sectional and temporal) dynamic structure of the data set.

Table D.1: Max-p Regions by Year and Year of Treatment

Year	Year of treatment								Total
	Never-treated	2012	2013	2015	2016	2017	2018	2019	
2010	27	5	4	1	7	5	4	24	77
2011	28	5	4	1	7	7	4	23	79
2012	28	5	4	1	7	7	4	23	79
2013	26	5	3	1	7	6	2	20	70
2014	30	5	4	1	7	8	4	32	91
2015	33	5	4	1	5	9	2	31	90
2016	32	5	4	1	5	9	2	31	89
2017	32	5	4	1	5	9	2	32	90
2018	26	5	4	1	6	8	3	30	83
2019	26	5	4	1	6	8	3	30	83
Total	288	50	39	10	62	76	30	276	831

Table D.2 summarizes the mean values of key outcome variables and additional indicators, disaggregated by treatment status and year. For example, in 2019 the average poverty headcount ratio at the \$3.65 per day line was 23.9 percent in treated regions, compared to 30.5 percent in never-treated regions. These statistics provide descriptive context and underscore the differences and trends assessed in the main results.

Below, we describe the main variables included in the RCS data set:

- **Poverty (\$2.5, \$3.65, and \$6.85 per day):** Percentage of the population living

[9] The timing selection is due to the data availability and comparability across household surveys.

below the respective World Bank poverty lines (adjusted to 2017 PPP prices) in region i and year t.

- **Vulnerable (%):** Percentage of the population earning between \$6.85 and \$14 per day (2017 PPP).
- **Middle Class (%):** Percentage of the population earning between \$14 and \$81 per day (2017 PPP).
- **PC Overall Income (\$):** Mean per capita overall income for region i and year t.
- **PC Labor Income (\$):** Mean per capita labor income for region i and year t.
- **Employment Rate (%):** Share of the population ages 15 and over reporting employment during the reference week.
- **Adequate Employment (%):** Share of employed individuals earning at least the minimum wage and working at least 40 hours per week, not classified as underemployed or precarious.
- **Formal Employment (%):** Share of employed individuals working in registered establishments and covered by social security.
- **Self-Employed (%):** Share of employed individuals who are self-employed.
- **Small-Size Firm (%):** Share of employed individuals working in firms with 1–5 employees.
- **Mid-Size Firm (%):** Share of employed individuals working in firms with 6–50 employees.
- **Primary, Secondary, Tertiary Sector (%):** Share of employed individuals working in ISIC Rev.4 sectors: primary (agriculture, forestry, mining), secondary (manufacturing), and tertiary (services).
- **Men (%):** Share of the male population in region i and year t.
- **Years of Education:** Mean years of education in region i and year t.
- **Amazon, Sierra, Costa (%):** Share of the population living in each of Ecuador's three natural regions.

Table D.2: Mean Values of Key Variables by Group and Year

Variable	Group	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	Avg.
Poverty	Control	10.1	11.7	11.5	10.5	12.1	12.5	15.0	13.3	9.6	13.9	12.0
$2.5 Line	Treated			1.1	9.4	5.3	7.2	4.6	7.4	6.6	10.1	6.4
Poverty	Control	31.6	35.4	31.9	25.7	26.7	27.4	31.1	28.5	25.4	30.5	29.4
$3.65 Line	Treated			22.2	26.2	24.8	21.4	17.8	20.6	16.5	23.9	21.7
Poverty	Control	64.4	65.7	61.0	56.2	54.0	59.1	60.3	58.5	55.9	65.6	60.1
$6.85 Line	Treated			61.1	59.5	58.0	54.7	55.9	51.5	42.2	51.0	54.2
Vulnerable	Control	25.4	24.7	27.3	30.8	31.9	27.6	29.2	26.2	30.3	25.9	27.9
	Treated			29.1	31.5	32.1	26.9	34.1	31.5	38.0	35.4	32.3
Middle	Control	10.3	9.7	11.7	12.9	14.1	13.2	10.5	15.3	13.8	8.5	12.0
Class	Treated			9.8	9.0	9.9	18.4	10.0	17.0	19.8	13.6	13.4
PC Overall	Control	100.1	103.8	115.6	125.9	135.5	131.7	125.4	135.9	136.6	112.3	122.3
Income	Treated			121.5	112.1	121.9	149.3	135.7	155.0	160.1	143.7	137.4
PC Labor	Control	88.1	91.0	103.6	109.8	120.9	113.7	107.2	113.2	116.1	87.0	105.1
Income	Treated			115.0	94.2	105.6	131.5	120.4	137.3	133.2	119.4	119.6
Employment	Control	97.9	97.5	98.2	97.4	97.6	97.8	97.8	97.4	99.0	99.1	98.0
Rate	Treated			97.1	98.4	97.9	96.3	97.5	98.0	96.1	98.5	97.5
Adequate	Control	26.8	23.9	25.8	27.2	28.5	26.6	22.7	21.2	22.8	11.9	23.7
Employment	Treated			32.3	19.7	22.1	29.0	22.8	26.4	25.1	20.1	24.7
Formal	Control	30.2	37.4	34.6	35.5	35.8	35.4	33.9	32.6	29.4	30.4	33.5
Employment	Treated			46.5	50.9	46.9	57.4	51.0	38.2	42.3	34.9	46.0
Self	Control	41.7	45.2	41.9	40.7	40.0	39.4	40.2	41.0	40.3	40.1	41.0
Employed	Treated			41.8	40.7	40.1	38.9	41.5	39.2	37.8	38.8	39.8
Small-Size	Control	81.3	83.2	82.1	80.9	79.5	78.3	77.3	79.3	78.6	84.4	80.5
Firm	Treated			77.4	85.4	76.4	78.0	83.8	77.3	85.0	80.2	80.4
Mid-Size	Control	8.3	7.4	8.3	8.4	7.0	8.0	9.9	9.8	11.1	10.2	8.8
Firm	Treated			14.1	9.4	15.0	7.7	8.8	10.6	8.0	12.0	10.7
Primary	Control	65.6	66.2	67.7	63.9	61.5	62.6	62.7	64.7	68.3	76.9	66.0
Sector	Treated			62.4	58.2	58.4	61.8	62.6	58.3	64.2	67.9	61.7
Secondary	Control	5.2	4.9	3.5	5.6	5.5	5.5	4.8	4.2	6.5	7.4	5.3
Sector	Treated			2.7	3.5	7.2	2.8	5.4	3.9	3.5	3.1	4.0
Tertiary	Control	29.1	28.9	28.8	30.5	33.0	31.9	32.5	31.1	25.2	15.7	28.7
Sector	Treated			34.9	38.2	34.4	35.3	32.0	37.8	32.3	29.0	34.2
Men	Control	50.2	49.3	49.0	49.9	48.7	48.7	48.9	49.0	48.7	47.2	49.0
	Treated			48.1	52.0	50.0	48.5	48.5	49.4	51.1	49.6	49.7
Years	Control	7.3	7.4	7.5	8.2	8.6	8.7	8.8	8.5	8.5	7.6	8.1
Edu.	Treated			8.2	7.9	8.3	8.2	8.4	9.2	8.6	9.0	8.5
Amazon	Control	32.5	31.6	32.4	33.9	50.4	51.3	55.4	50.0	48.1	19.2	40.5
	Treated			20.0	12.5	11.1	10.0	6.7	36.8	33.3	54.1	23.1
Sierra	Control	51.2	51.4	52.0	48.8	39.3	39.5	36.5	40.9	48.3	73.1	48.1
	Treated			40.0	50.0	55.6	50.0	58.4	39.3	44.4	35.4	46.6
Costa	Control	16.4	16.9	15.6	17.4	10.3	9.2	8.1	9.1	3.6	7.7	11.4
	Treated			40.0	37.5	33.3	40.0	34.9	23.9	22.2	10.5	30.3

In summary, the RCS data set provides a region-year data framework from harmonized, population-representative microdata, enabling examination of the effects of major paved road access on a wide array of socioeconomic outcomes.