Municipal Tax Incentives and Solar PV Adoption: Causal Evidence from Catalonia

L. van Raalte*

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Abstract

This paper evaluates the causal effect of municipal property tax exemptions on solar photovoltaic (PV) adoption in Catalonia. Using a balanced monthly panel of 398 municipalities from 2015 to 2022, we apply a covariate-adjusted difference-in-differences estimator under staggered adoption. The policy increased installed PV capacity by 34.4% and led to an average monthly increase of 0.79 installations per treated municipality. Heterogeneity analysis reveals stronger effects in municipalities characterised by low-rise housing and in rural areas with higher income levels, suggesting that both structural and socioeconomic conditions influence policy effectiveness. A back-of-the-envelope calculation for residential systems yields an implied abatement cost of €119 per tonne of avoided CO_2, placing the policy within the range of other decentralised renewable energy support schemes. These findings underscore the potential of municipal fiscal instruments to accelerate residential decarbonisation and support climate policy goals.

Keywords: solar PV adoption; property tax exemption; causal inference; staggered difference-in-differences; municipal climate policy; Catalonia

JEL Codes: Q42; H23; Q48; C21

*MSc in Economics – Master Thesis, Universitat de Barcelona. Academic year: 2024–2025. Advisor: Associate Professor Jordi J. Teixidó Figueras. Department of Econometrics, Statistics and Applied Economics at the Public Policy section.

1 Introduction

Achieving complete decarbonization requires more than international agreements and national goals, it critically depends on the capacity of local governments to shape energy systems from the ground up. Although frameworks such as the Kyoto Protocol and the Paris Agreement have set essential global benchmarks, recent scholarship has emphasized that the practical implementation of the energy transition increasingly unfolds within local contexts (Capellán-Pérez et al., 2018; Leonhardt et al., 2022). Municipalities not only govern infrastructural and planning decisions, but also exert fiscal instruments that can directly influence household adoption of renewable technologies. As front-line actors with contextual knowledge and political autonomy, local governments are uniquely positioned to tailor climate policies to local socioeconomic and institutional conditions. Understanding how these decentralized instruments function in practice is therefore central to both evaluating policy effectiveness and designing equitable low-carbon transitions. This paper evaluates the causal impact of municipal property tax exemptions—known as bonificaciones del IBI—on the adoption of solar photovoltaic (PV) systems in Catalonia. These locally determined incentives reduce the annual property tax burden for households installing PV systems and differ in timing, duration and generosity. This municipal discretion generates substantial variation in policy exposure, which we exploit for causal identification.

The empirical analysis draws on a balanced monthly panel of 398 Catalan municipalities between 2015 and 2022. By merging official PV installation records from the Catalan Institute of Energy with annual municipal socioeconomic indicators, we estimate the average treatment effect on the treated (ATT) using a covariate-adjusted estimator within the staggered Difference-in-Differences framework (Callaway and Sant'Anna, 2021). This method accommodates staggered adoption, not yet and never-treated units, and observed covariates, enabling credible identification of dynamic and group-specific treatment effects.

The results indicate that the introduction of the property tax exemption significantly increased both the number of new installations and the total installed capacity of solar PV systems. On average, treated municipalities recorded 0.79 additional installations per month and achieved a 34.4% increase in installed capacity relative to their estimated counterfactual level. These effects are economically meaningful and underscore the efficacy of local tax incentives in shaping household energy investment decisions.

The heterogeneity analysis suggests that the policy was broadly effective in different structural and socioeconomic contexts, with no evidence of strong regressivity across municipalities. While treatment effects on the number of installations are positive and statistically significant in nearly all subgroups, the largest effects are observed in towns with predominantly low-rise housing, where spatial suitability likely facilitates widespread household participation. For total installed capacity, the most pronounced increases—exceeding 40%—occur in high-income rural municipalities and in rural areas characterised by low-rise housing, pointing to the combined role of financial capacity and structural feasibility in enabling larger-scale residential investments. Based on a back-of-the-envelope estimation, the implied cost of carbon abatement in the residential sector is approximately €119 per tonne of CO_2, indicating that the policy achieves meaningful emissions reductions at a relatively favourable cost for a decentralised fiscal instrument.

A substantial body of international research has established that the effectiveness of financial incentives for residential solar photovoltaic (PV) adoption depends not only on their monetary value but also on their salience—that is, their visibility and perceived relevance to households. In the United States, Hughes and Podolefsky (2015) exploit utility-level variation across California from 2007 to 2012 and estimate that a \$0.10/W increase in direct rebates, in conjunction with federal Investment Tax Credits (ITCs), raised PV adoption by approximately 10%. Similarly, Sun and Sankar (2022) employ a dynamic regression discontinuity design and find that a \$1,000 rebate increased installation rates by 0.15 systems per 1,000 households per month. Borenstein (2017) compares multiple incentive types across California and finds that upfront rebates were more cost-effective than tax credits, yielding emissions reductions at \$139–\$147 per ton of CO_2 and installation costs of roughly \$7,600 per additional system.

Crago and Chernyakhovskiy (2017) use county-level panel data from the U.S. Northeast (2005–2012) to estimate the impact of different solar policy incentives on annual residential PV capacity additions. In their fixed effects regressions, a \$1/W increase in upfront rebates is associated with a statistically significant 50% increase in capacity additions, underscoring the effectiveness of direct and salient subsidies. Property tax exemptions are included as explanatory variables in the same model, but their coefficients are not statistically significant. Therefore, failing to identify a robust behavioural effect of property tax incentives. Gallagher and Muehlegger (2011) report similar findings for the vehicle market, where immediate tax exemptions outperformed deferred tax credits in influencing consumer behaviour. These results confirm that immediate, visible incentives tend to elicit stronger behavioural responses.

European evidence supports these patterns, though with fewer studies and less dynamic evaluation. De Groote et al. (2016) analyse municipal subsidies for solar PV installations in Flanders, Belgium, and estimate a semi-elasticity of 0.221 for local subsidies, which declines modestly to 0.176 after controlling for housing and income. This implies that doubling a municipality's local subsidy rate—from €138 to €276—would increase PV adoption by 2.46%. Although modest, these subsidies were layered on top of highly generous regional green certificate schemes. Daniele et al. (2023) further show that simplifying photovoltaic permitting procedures in Italy led to a 13–25% increase in installations across affected municipalities, suggesting that clarity and ease of access—key components of salience—may be just as important as financial generosity. In this context, decentralised fiscal instruments such as property tax exemptions may prove more effective if they are simple, recurring, and clearly communicated to households.

Finally, Cansino et al. (2010) review tax policies across the EU-27 and identify only Italy and Spain as offering property tax exemptions specifically designed to promote renewable electricity adoption. This highlights both the rarity of such decentralised fiscal instruments and the absence of rigorous empirical evaluations of their impact.

In Spain, causal evaluations of this property tax exemption remain limited. Existing empirical studies have primarily examined solar thermal systems and often rely on descriptive or cross-sectional methods without identifying causal effects on adoption. González-Limón et al. (2013), using logistic regressions across 232 municipalities, identify key political and socioeconomic predictors of adopting local tax credits for solar thermal energy—such as left-wing governance and higher income levels—but do not evaluate the policy's impact on uptake. Sánchez-Braza and Pablo-Romero (2014) analyse panel data from 94 municipalities in Andalusia between 2003 and 2008 and report that municipalities with tax property exemptions installed, on average, 122.4 more square metres of solar thermal collectors—a 70.7% relative increase compared to those without such exemptions. However, their analysis is descriptive in nature and does not account for staggered policy adoption or unobserved municipal characteristics. In a separate line of inquiry, San-Martín and Elizalde (2024) use survey data from the Basque Country to highlight institutional trust and pro-environmental norms as important behavioural drivers of household renewable energy attitudes, though they do not assess realised adoption or the effect of fiscal incentives.

This paper makes four main contributions to the literature on local fiscal incentives and solar photovoltaic (PV) adoption. First, it provides causal estimates of the effect of municipal property tax exemptions on self-consumption solar PV uptake in Spain, using a covariate-adjusted estimator within a staggered difference-in-differences framework that accounts for variation in treatment timing. Second, it introduces a novel panel dataset, compiled through systematic collection and verification of official municipal ordinances. Third, the paper presents a detailed heterogeneity analysis showing how policy effects vary by income level, building typology, and settlement type, underscoring the role of local structural and socioeconomic factors in shaping responsiveness to fiscal incentives. Finally, it offers a novel estimation of the implied cost of carbon abatement based on observed policy impact estimates.

The remainder of the paper is organized as follows. Section 2 outlines the policy context and institutional framework. Section 3 describes the data sources and sample construction. Section 4 details the empirical strategy. Section 5 presents the main findings. Section 6 concludes.

2 Policy background

The transition to a low-carbon energy system has brought distributed renewables—particularly solar photovoltaic (PV) systems—to the forefront of European climate strategy. Under the European Green Deal, the European Union has committed to achieving net-zero greenhouse gas emissions by 2050, positioning renewable energy and decentralized electricity generation as central pillars of the decarbonization agenda (European Commission, 2020). In line with this objective, Spain's National Integrated Energy and Climate Plan (PNIEC) sets a target of expanding installed solar PV capacity from 11 GW in 2021 to over 39 GW by 2030, with an emphasis on self-consumption and citizen participation (MITECO, 2020). At the regional level, Catalonia's long-term energy roadmap projects that solar PV could provide up to 43% of electricity supply by mid-century (Institut Català d'Energia, 2023). However, current deployment levels remain significantly below these targets: by 2023, solar PV accounted for less than 10% of electricity generation in the region, underscoring a persistent implementation gap (Institut Català d'Energia, 2024).

To accelerate adoption, policymakers have introduced a range of instruments to support household investment in residential solar PV. While declining system costs and high solar irradiance make Spain well-positioned for solar generation, uptake has remained uneven across regions and socioeconomic groups. Barriers such as high upfront costs, regulatory complexity, and behavioural frictions continue to constrain adoption (IDAE, 2021; Colasante and de Luca, 2022;

Xu and Morales, 2024). Addressing these obstacles requires tools that are economically efficient, behaviourally salient, and administratively tractable. One of the most widely used local instruments in Spain is a reduction in the annual property tax—known as the bonificación del IBI (Impuesto sobre Bienes Inmuebles).

Authorised under Article 74.5 of Royal Legislative Decree 2/2004, municipalities in Spain can offer up to a 50% reduction of the property tax for residential properties that install qualifying solar PV or thermal systems (Ministerio de Hacienda, 2004). These incentives are implemented through municipal ordinances, which determine eligibility conditions, discount rates, and benefit durations. In practice, most municipalities grant reductions of 25% to 50%, typically for three to five years. Some municipalities also apply annual monetary caps to limit the maximum deduction per household, often ranging between 200 and 400 euros, which can moderate the realised value of the incentive for high-value properties. Eligibility generally requires that installations be legally registered, technically certified, and not mandated by urban planning obligations.

Despite the decentralized framework, local ordinance texts often share common policy goals. The stated objective is to incentivize household investment in clean energy while aligning local tax instruments with environmental goals. Importantly, the tax exemption must be explicitly requested by the taxpayer, reinforcing its character as a voluntary incentive. This structure reflects a broader pattern across Catalonia, where local governments use their fiscal autonomy to support energy transition goals through targeted, discretionary measures.

The property tax exemption differs from other renewable energy incentives in several key respects. Unlike national feed-in tariffs or investment subsidies, it does not involve direct transfers from the central government. Instead, it is financed through foregone local tax revenue and administered independently by municipalities. This structure introduces variation in both the generosity and timing of implementation across jurisdictions, making it particularly suitable for causal analysis of local climate policy and household responsiveness to municipal-level price signals.

Figure 1 illustrates the annual number of Catalan municipalities adopting the tax exemption between 2015 and 2024. Adoption accelerated notably after the repeal of the so-called "sun tax" under Royal Decree-Law 15/2018 and the regulatory clarification provided by Royal Decree 244/2019. These national reforms reduced legal uncertainty and established a more stable framework for self-consumption, enabling municipalities to introduce property tax discounts more confidently (Boletín Oficial del Estado, 2018, 2019). Most adoption occurred between 2019 and 2022, coinciding with the broader post-reform policy environment and the alignment with regional and European climate finance mechanisms.

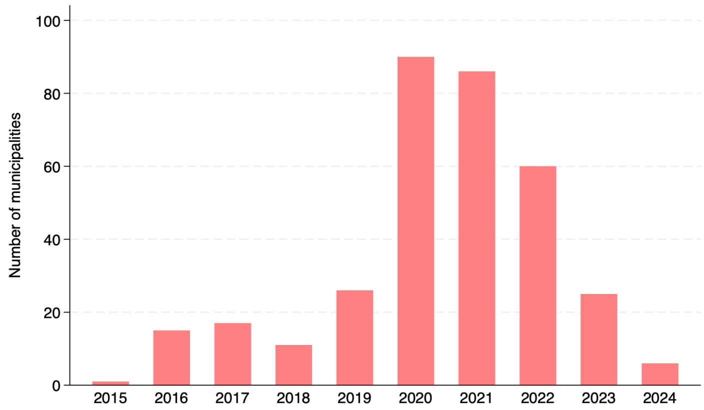

Figure 1: Number of municipalities adopting tax property exemption by year

During this period, Spain also launched its Recovery, Transformation and Resilience Plan (PRTR), supported by the European Union's NextGenerationEU programme (European Commission, 2021). Although these national funds did not finance the property tax exemptions directly, they contributed to a more favourable environment for distributed solar PVs through initiatives such as those regulated under Royal Decree 477/2021 (MITECO, 2021), implemented in Catalonia by the ICAEN. Since PRTR resources were applied uniformly across all municipalities, their macro-level effects are absorbed by time fixed effects in the empirical strategy.

In parallel, some municipalities have offered additional tax breaks through partial exemptions from the Impuesto sobre Construcciones, Instalaciones y Obras (ICIO), a one-time tax on construction projects. These exemptions typically range from 50% to 95% of the ICIO's 3–4% base rate, implying modest one-time savings (Boletín Oficial del Estado, 2004). For example, a 90% exemption on a €5,000 installation reduces the tax by just €180. Because the ICIO applies exclusively to new construction activities and exhibits limited temporal variation, these exemptions are excluded from the present analysis.

In sum, the property tax exemption constitutes a locally administered, fiscally decentralized policy tool aimed at promoting residential solar PV through recurring property tax reductions. Its legal foundations, environmental framing, and heterogeneous roll-out across municipalities make it both substantively relevant and methodologically tractable for evaluating local climate policy impacts. The next chapter details the construction of a novel panel dataset capturing the timing, characteristics, and effects property tax exemption policies across Catalonia between 2015 and 2022.

3 Data and sample construction

This study constructs a novel municipality-level panel dataset to estimate the causal effect of local property tax incentives on residential solar photovoltaic (PV) adoption in Catalonia. The initial universe comprises all 947 municipalities in the region. However, a series of sample restrictions is applied to ensure data completeness, internal consistency, and methodological validity. In particular, municipalities with fewer than 1,000 residents—averaged over the 2015–2022 period—are excluded from the analysis. These very small municipalities tend to introduce high levels of noise in administrative indicators and are often subject to irregularities in both fiscal policy implementation and renewable energy reporting. Moreover, the informational loss from their exclusion is minimal. According to official data from Statistical Institute of Catalonia (2021), the 482 smallest municipalities collectively account for only 2.5% of the total population of Catalonia, or approximately 192,465 residents out of 7.74 million in 2021. This restriction removes outliers while preserving external validity and ensures robust empirical identification in small-area panels. The panel spans from January 2015 to May 2024 and integrates three primary administrative sources. Data on solar PV installations are obtained from the Catalan Institute of Energy (ICAEN), which maintains a registry of all self-consumption systems in Catalonia. Each installation record includes a registration date and capacity in kilowatts. These records are aggregated to the municipality-month level, generating two outcome variables: (i) the number of new installations, and (ii) the log installed capacity in kilowatts[1]. Municipalities that adopted the property tax incentive prior to February 2015 are dropped to ensure the presence of a clean pre-treatment baseline. These restrictions yield a strongly balanced panel of 417 municipalities, each contributing a full sequence of 113 monthly observations, resulting in 46,330 municipality-month observations. This dataset forms the basis for subsequent merging with covariates and treatment timing information.

Socioeconomic covariates are sourced from the Statistical Institute of Catalonia (Idescat), which provides annual values for municipal population, average net income per capita, the Gini index of income inequality, and the share of residents aged 65 or older. Because these variables are updated only once per year, each value is held constant across the twelve months of the corresponding calendar year. While this reduces intra-annual variability, it maintains consistency with the monthly outcome structure and supports identification at the appropriate level of temporal granularity. Covariate data are available from 2015 to 2022; municipalities with missing values in any year of this period are excluded to preserve a fully balanced panel of covariates. Conditioning on these exclusions, and additionally removing municipalities that received treatment in all months (i.e. always-treated units), yields a final estimation sample of 398 municipalities and 38,208 monthly observations spanning 96 months.

Table 1 reports descriptive statistics for the main outcome and covariate variables in the full working dataset. Because socioeconomic covariates are available only through 2022, the number of observations for these variables is lower than for the outcomes. The estimation sample is further reduced by excluding municipalities treated before February 2015—where no clean pre-policy baseline exists—and those with time-varying treatment parameters such as mid-period amendments or repeals, which introduce ambiguity in treatment definition.

[1] The latter outcome is right-skewed and includes frequent zeroes; therefore, we apply the standard $\log(1 + x)$ transformation to the installed capacity variable. For robustness, we also estimate specifications using the inverse hyperbolic sine (arcsinh) transformation.

Table 1: Descriptive statistics of main variables

Variable	N	Mean	Std. Dev.	Min	Max
Number of new installations	45,991	1.872	4.909	0	125
Log total capacity (kW)	45,991	1.072	1.627	0	9.378
Average net income (€)	38,208	13,220	2,154	8,025	24,814
Gini index (0–100)	38,208	29.514	3.172	21.5	40.9
Share aged 65+ (%)	38,208	18.704	3.966	7.5	35.2
Population (residents)	38,208	15,978	83,654	917	1,606,253
IBI discount rate (%)	45,991	15.023	21.888	0	75.0
IBI discount duration (years)	45,991	1.439	2.227	0	10

Note: Municipalities with an average population below 1,000 over 2015–2022 were excluded. However, some municipalities (e.g. with 917 residents in a given year) remain included due to annual fluctuations.

Information on local fiscal incentives is manually collected from municipal fiscal ordinances and council resolutions, due to the absence of a national registry of property tax exemptions. For each municipality, the dataset records the adoption month, the discount rate applied to the property tax (IBI), and the duration of the exemption in years. Although some municipalities impose annual fiscal caps—typically between €200 and €400—to limit the monetary value of the incentive, these are not systematically documented and are therefore excluded from the construction of treatment variables. Moreover, due to inconsistencies and missing values in the administrative reporting of discount rates and durations, the analysis focuses on a binary treatment indicator that captures whether and when the policy was adopted.

To capture potentially heterogeneous treatment effects, two time-invariant variables are constructed to reflect structural and spatial characteristics of municipalities. The first is the degree of urbanisation, derived from the DEGURBA classification developed by Idescat in accordance with Eurostat standards (Eurostat, 2022). This typology is based on 1 km^2 population grid cells, which are classified as urban centres (more than 1,500 residents), urban clusters (300–1,500 residents), or rural areas (fewer than 300 residents). These classifications are aggregated to the municipal level based on population distribution, resulting in a three-category variable: (i) densely populated (urban centres), (ii) intermediate density (towns and suburbs), and (iii) thinly populated (rural). This typology captures settlement patterns, infrastructure connectivity, and built environment density, all of which are relevant for solar PV adoption due to their influence on spatial constraints, housing type and implementation feasibility.

The second variable captures the residential housing typology. The indicator reflects the proportion of dwellings located in low-density structures, defined as buildings containing one or two housing units. Municipalities in which this share exceeds 50% are classified as "low-rise," while the remainder are designated as "high-rise." The corresponding threshold was selected as it approximates the median value in the sample, resulting in two equal-sized groups that support balanced heterogeneity analysis. The labels are employed throughout the analysis for interpretive clarity, although they do not correspond directly to building height or the number of floors. Rather, they serve as proxies for structural density and residential configuration. This distinction is analytically relevant, as the feasibility of solar PV installation, and the incentives for household-level investment, vary considerably across different forms of the built environment. The typology is constructed using 2021 cadastral records and remains fixed throughout the observation window. [2]

The final sample includes 398 municipalities. Of the retained municipalities, 70 never adopted the policy during the study period and form part of the control group. Figures 2 and 3 show the geographic distribution of treatment status and excluded municipalities, respectively.

[2] The cadastral records (2021) only contain data for the municipalities with more than 2,000 residents in that year. Therefore, this specific sub-analysis is performed on a slightly smaller sample.

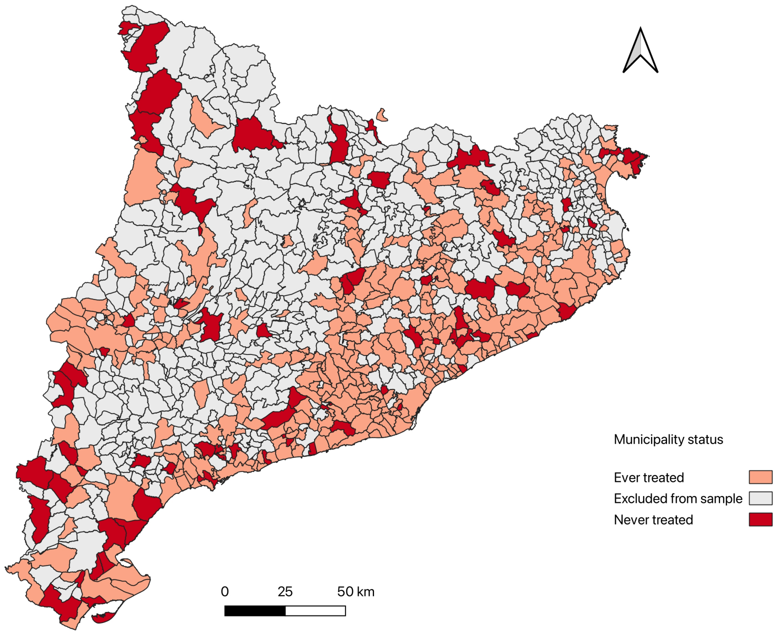

Figure 2: Geographic distribution of treatment and control municipalities

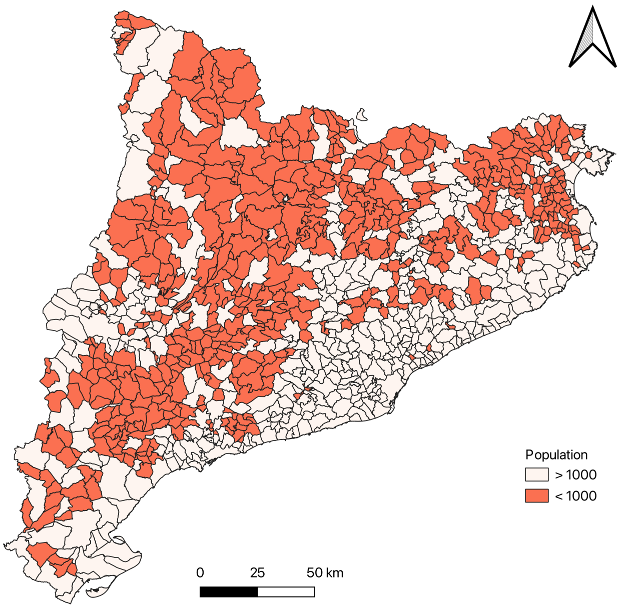

Figure 3: Excluded municipalities by population threshold (<1,000 residents)

4 Methodology

This study estimates the causal effect of municipal property tax reductions on solar photovoltaic (PV) adoption using a staggered Difference-in-Differences (DiD) framework that exploits the staggered rollout of the policy across Catalan municipalities between 2015 and 2022. The empirical strategy leverages variation in treatment timing to compare municipalities that introduced the property tax exemption at different points in time to a control group of municipalities that never adopted the policy or that have not yet adopted the policy. This variation enables credible identification of treatment effects, under assumptions discussed below.

As a benchmark, we begin by estimating a conventional two-way fixed effects (TWFE) model of the form:

$$Y_{it} = \alpha_i + \lambda_t + \beta \cdot \text{Treated}_{it} + \varepsilon_{it},$$

where Y_{it} denotes the outcome of interest for municipality i in month t, α_i and λ_t are municipality and month fixed effects respectively, and Treated_{it} is a binary indicator equal to 1 if the property tax incentive is active in municipality i at time t. This specification is estimated separately for two outcomes: the log installed capacity (kW), and the number of new installations. The coefficient β is interpreted as the average effect of treatment across all treated municipalities and time periods.

While widely used in panel data settings, the TWFE estimator is known to produce biased estimates when treatment is adopted at different times and effects are heterogeneous across groups or over time. In such settings, the TWFE approach implicitly compares already-treated municipalities to those treated later, violating the core assumption of valid control groups (Goodman-Bacon, 2021; De Chaisemartin and D'haultfœuille, 2023). Despite these limitations, the TWFE model is included as a point of comparison. It serves to illustrate how estimates from more robust approaches—designed to account for staggered adoption and treatment heterogeneity—differ from those obtained using standard methods, and to provide continuity with prior studies that have relied on fixed-effects specifications.

Our main results are based on the Difference-in-Differences estimator developed by Callaway and Sant'Anna (2021), which is designed for settings with treatment heterogeneity and staggered adoption. This estimator computes group-time average treatment effects ($\text{ATT}_{g,t}$), defined as

$$\text{ATT}_{g,t} = \mathbb{E}[Y_{it}(1) - Y_{it}(0) \mid G_i = g, t \geq g],$$

where $G_i = g$ denotes the group of municipalities first treated in period g. By comparing each treated group to municipalities not yet treated or never treated at time t, the estimator avoids the negative weighting and invalid comparisons that undermine TWFE. Group-time effects can be aggregated into overall post-treatment effects, providing policy-relevant measures of average programme impact across treated units.

Figure 4 illustrates the staggered rollout of the property tax incentives across municipalities. Each row corresponds to a municipality, with darker shading indicating the months in which the incentive is in effect. The variation in adoption timing, along with the presence of municipalities that never implemented the incentive, underpins the empirical identification strategy.

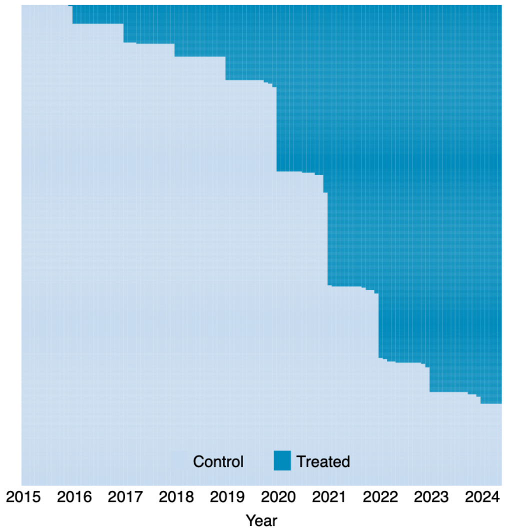

Figure 4: Rollout of IBI tax incentives across municipalities (2015–2024)

The identification strategy rests on a conditional parallel trends assumption, which states that, in the absence of treatment, the change in outcomes for treated municipalities would have matched those of the comparison group, conditional on observed pre-treatment characteristics:

$$\mathbb{E}[Y_{it}(0) - Y_{i,t-1}(0) \mid G_i = g] = \mathbb{E}[Y_{it}(0) - Y_{i,t-1}(0) \mid G_i > t].$$

This assumption is plausible in the present context for two reasons. First, the timing of the policy adoption appears to be shaped by idiosyncratic political or administrative decisions, rather than coordinated responses to local solar PV trends. Second, a stable group of never-treated municipalities remains throughout the observation period, offering a credible comparison group for identification. Although we do not observe clear pre-treatment trends, the overall trajectory of installations appears broadly similar between treated and untreated municipalities in the earlier part of the panel. As shown in Figure 5, both groups experience a gradual increase in installation activity until early 2022, after which their paths seem to diverge, with treated municipalities showing a steeper rise. This pattern is consistent with the interpretation that the divergence reflects the effect of the policy, and that in the absence of treatment, the groups would have continued to follow comparable trends. While not definitive, this visual evidence supports the plausibility of the conditional parallel trends assumption.

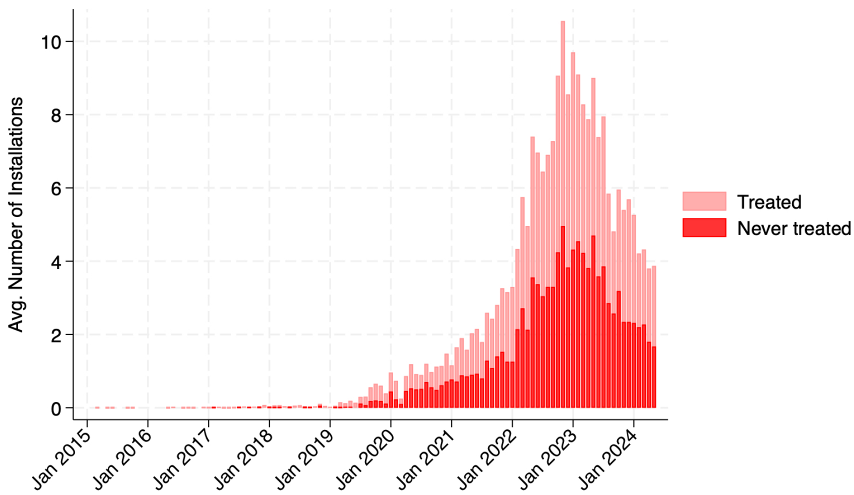

Figure 5: Average number of Installations (Ever treated vs. Never treated).

To empirically assess the parallel trends assumption, we estimate dynamic event study models and test for pre-treatment differences in outcome trajectories. In additional specifications, we incorporate a limited set of baseline covariates—specifically, population size, net income per capita, Gini index and the share of elderly residents—to improve covariate balance and assess robustness under conditional trends. Following best practices in staggered DiD settings (Baker et al., 2025), we evaluate covariate balance before and after reweighting using inverse probability weights (IPW). Appendix A Table 5 reports standardised mean differences (SMDs) for these key pretreatment variables, comparing ever-treated and never-treated municipalities. Prior to weighting, SMDs were substantial—35.2% for income and 27.3% for the elderly share—exceeding commonly accepted imbalance thresholds of 20–25% (Austin and Stuart, 2015; Stuart, 2010). After applying IPW, all SMDs fell below 6%, with percentage reductions ranging from 71.6% to 93.4% (see Appendix A Figure 12). These results indicate that the reweighting procedure substantially improved balance on observables, meeting accepted standards for covariate comparability and support the plausibility of the conditional parallel trends assumption in the weighted specifications.

To further evaluate the validity of the parallel trends assumption under staggered treatment adoption, we employ the Synthetic Difference-in-Differences (SDID) estimator developed by Arkhangelsky et al. (2021). This method constructs synthetic control units that match the pre-treatment outcome trajectory of each treated municipality, thereby relaxing the standard parallel trends assumption. Rather than assuming that pre-treatment trends are equal on average, SDID ensures that treated and synthetic control units are comparable by design, based on pre-treatment outcomes. While not used as our primary estimator, SDID serves as a robustness check that helps assess whether unobserved differences in pre-treatment trajectories could bias our main results. If the SDID estimates align with those from the Callaway and Sant'Anna estimator, it provides empirical support for the conditional parallel trends assumption.

Finally, we examine the distributional effects of the policy, heterogeneity analyses are conducted based on key pre-treatment characteristics, including household income, building morphology, and urban classification. These subgroup analyses are implemented within the same covariate-adjusted staggered DID framework used for the main specification, allowing for internally consistent estimation of treatment effect variation across structurally distinct municipalities. The analysis stratifies municipalities according to exogenous characteristics observed prior to any treatment exposure. Two of these—urban classification and housing morphology—are time-invariant over the study period and unaffected by policy adoption. Urban classification follows the harmonised DEGURBA typology, which assigns municipalities to rural, town, or urban categories using consistent population density thresholds and contiguity rules. Housing morphology is derived from cadastral records and indicates whether the majority of residential buildings in a municipality are low-rise. Both variables capture structural features relevant to solar adoption potential and access. In contrast, income is a dynamic variable that may plausibly evolve in response to local policies or broader economic trends. To avoid post-treatment bias, municipalities are classified into high- or low-income groups based on average household income in 2015, prior to any policy adoption. Classification is defined relative to the region median to ensure comparability across heterogeneous regional contexts. This approach aligns with best practices for subgroup analysis in causal inference settings, where the use of pre-treatment covariates as effect modifiers helps ensure the internal validity of estimated heterogeneity (Stuart, 2010; Athey and Imbens, 2016).

Formally, subgroup-specific treatment effects are computed as weighted averages of cohort- and time-specific effects:

$$\theta(z_k) = \sum_{g,t} \Pr(G = g \mid Z = z_k) \cdot \mathrm{CATT}_{g,t}(z_k),$$

where $Z = z_k$ defines a particular subgroup, and $\Pr(G = g \mid Z = z_k)$ is the proportion of municipalities in that group treated in month g. Estimation is conducted using the covariate-adjusted inverse probability weighted estimator, correcting for both timing and compositional differences. These subgroup estimates allow us to assess how the policy's impact varies across municipalities with different structural and socioeconomic characteristics.

Finally, to assess the economic and environmental efficiency of the intervention, we conduct a back-of-the-envelope cost-effectiveness analysis. This consists of combining the estimated treatment effects on installed PV capacity based on acceptable assumptions about annual electricity generation and emissions factors, as well as regional finance data on property tax revenues. By translating the average treatment effects into avoided CO_2 emissions and dividing the assumed fiscal cost per adopting municipality by the resulting abatement, we derive approximate cost-per-tonne CO_2 estimates for each sample. This calculation provides an interpretable benchmark for comparing the policy's efficiency across subsamples and relative to other local renewable energy incentives. The full derivation is presented in the Results section, with all computational steps documented in the Appendix B.

5 Results

5.1 Main results

Table 2 presents the estimated average treatment effects on the treated (ATT) of the property tax exemption policy on two key outcomes: the number of new residential photovoltaic (PV) installations per municipality per month, and the log installed capacity in kilowatts. The first outcome captures the frequency of adoption, while the second reflects the scale of investment in distributed solar generation. Estimates are based on a covariate-adjusted, balanced panel of 398 Catalan municipalities observed monthly from January 2015 to December 2022. The staggered DID estimator (Callaway and Sant'Anna, 2021) is used with inverse probability weighting to improve balance on key pre-treatment characteristics, including population, income, inequality, and share of elderly. All models include municipality and time fixed effects, with standard errors clustered at the municipality level.

Table 2: Estimated ATT Effects on Solar PV Adoption

Outcome	Method	Estimate (SE)	95% CI	N
Panel A: Number of Installations				
	TWFE	0.930*** (0.209)	[0.519, 1.340]	38,208
	CSDID	0.790*** (0.235)	[0.329, 1.250]	38,193
	SDID	1.480*** (0.191)	[1.104, 1.854]	38,208
Panel B: Log Installed Capacity (kW)				
	TWFE	0.312*** (0.053)	[0.208, 0.416]	38,208
	CSDID	0.296*** (0.072)	[0.155, 0.436]	38,193
	SDID	0.407*** (0.046)	[0.318, 0.497]	38,208

Notes: ATT estimates from TWFE, CSDID (with covariates), and SDID. Installed capacity is log-transformed; effects are expressed as percentage changes using the exponential transformation. All models include municipality and time fixed effects and adjust for key sociodemographic covariates. Standard errors clustered at the municipality level. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

According to the preferred specification, the introduction of the property tax exemption is associated with an increase of 0.790 installations per municipality per month (SE = 0.235, $p < 0.01$), and a 34.4 percent increase in installed solar capacity relative to pre-treatment levels.[3] The synthetic difference-in-differences (SDID) model yields larger estimates—1.480 installations and a 50.2 percent increase in capacity—reflecting its emphasis on achieving close pre-treatment fit between treated and control units through unit-specific weighting (Arkhangelsky et al., 2021). While this improves credibility under potential violations of parallel trends, it can also reduce representativeness by overweighting well-matched control units (Ben-Michael et al., 2019).The two-way fixed effects (TWFE) model also produces statistically significant estimates, though its estimates are difficult to interpret given the known sensitivity to treatment effect heterogeneity and the presence of potentially non-convex weights in staggered adoption settings (De Chaisemartin and d'Haultfoeuille, 2020; Roth et al., 2023). Despite methodological differences, all approaches point to sizable and policy-relevant effects, reinforcing the validity of the main results.

[3]Percentage increases for log-transformed outcomes are calculated as $(\exp(\hat{\beta}) - 1) \times 100$, where $\hat{\beta}$ is the ATT estimate.

To assess the role of covariate adjustment, we re-estimate the model without controlling for baseline differences across municipalities in Appendix A Table 6. The unadjusted staggered DID estimates increase to 2.059 installations and a 55.7 percent rise in capacity. This comparison highlights the presence of upward bias when observable differences are not accounted for and supports the use of covariates to satisfy the conditional parallel trends assumption.

To ensure that results are not driven by larger or commercial PV systems, we replicate the analysis using a restricted sample of installations below 10 kilowatts in table 3. This threshold, while not a legal definition of "residential," corresponds to the eligibility limit for enhanced national subsidies under Royal Decree 477/2021 (Annex I, Programme 1) and is widely used in Spanish policy frameworks to denote household-scale systems (Gobierno de España, 2021). The estimates for this subsample remain consistent with the full sample: 0.771 additional installations and a 30.9 percent increase in capacity. These results suggest that the observed effects are primarily driven by residential adoption.

Table 3: Estimated ATT Effects on Solar PV Adoption (<10 kW)

Outcome	Method	Estimate (SE)	95% CI	N
Panel A: Number of Installations				
	TWFE	0.673*** (0.201)	[0.277, 1.070]	38,208
	CSDID	0.771*** (0.232)	[0.316, 1.227]	38,193
	SDID	0.808*** (0.273)	[0.272, 1.344]	37,632
Panel B: Log Installed Capacity (kW)				
	TWFE	0.244*** (0.049)	[0.148, 0.341]	38,208
	CSDID	0.266*** (0.060)	[0.148, 0.383]	38,193
	SDID	0.305*** (0.055)	[0.196, 0.414]	37,632

Notes: Subsample restricted to installations under 10kW. All models include municipality and time fixed effects and adjusted for covariates included. Standard errors clustered at the municipality level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

As a robustness check on the functional form of the installed capacity outcome, we re-estimate the main specification using the inverse hyperbolic sine (IHS) transformation. The IHS transformation is often employed for continuous variables that exhibit strong right skewness and contain zero or negative values, as it approximates the natural logarithm for large values while remaining defined and approximately linear near zero (Burbidge et al., 1988). However, when the outcome is strictly non-negative—as is the case with monthly installed solar capacity (measured in kW)—recent methodological contributions suggest that the standard logarithmic transformation remains preferable, particularly when percentage-based interpretation is desired (Norton, 2022). Moreover, the general IHS transformation outcome does not yield a constant semi-elasticity, and its marginal effects vary with the outcome level, requiring assumptions that may not align with nonlinear estimators such as staggered DiD models (Pence, 2006; Bellemare and Wichman, 2020).

Despite these limitations, we implement the IHS transformation to test the sensitivity of our results to alternative functional forms and to address concerns about the potential arbitrary scaling of log-plus-one transformations in empirical research. The results, reported in Appendix A Table 7, show that the estimated treatment effect remains statistically significant and directionally consistent under the IHS specification. Given that the outcome variable typically exceeds 10kW in most municipality-month observations, the estimated coefficient of 0.35 may be approximately interpreted as a semi-elasticity, in line with the guidance provided by Bellemare and Wichman (2020). Its close similarity in magnitude and precision to the log-transformed estimate reinforces the robustness of the main result.

Taken together, the evidence indicates that the property tax exemption policy led to a substantial and statistically robust increase in both the frequency and the scale of solar PV adoption. These effects are consistent across estimators, subsamples, and outcome transformations, offering suggestive evidence that local fiscal policy can serve as an effective tool for accelerating the energy transition at the municipal level.

5.2 Event study analysis

To assess the plausibility of the identifying assumptions and examine the temporal dynamics of policy effects, we estimate event-time profiles using a covariate-adjusted inverse probability weighting (IPW) difference-in-differences estimator, following the framework of Callaway and Sant'Anna (2021). Figures 6 and 7 report average treatment effects on the treated (ATT) by event month, separately for the number of PV installations and for log installed capacity in kilowatts. The horizontal axis denotes months relative to policy adoption, with zero marking the first month of treatment; vertical axes show ATT estimates with 95% confidence intervals.

Pre-treatment coefficients are flat and statistically indistinguishable from zero across both outcomes, indicating no differential trends prior to policy implementation. This supports the credibility of the conditional parallel trends assumption and provides empirical reassurance that treated and untreated municipalities evolved similarly in the absence of the intervention.

Post-treatment estimates become positive and statistically significant, indicating a policy-induced increase in solar PV adoption. The number of installations rises steadily in the first year, with estimated effects stabilising between one and two additional installations per month. Installed capacity follows a similar dynamic, with ATT estimates ranging between 0.3 and 0.7 log points in the post-treatment period.

As is typical in staggered designs, confidence intervals widen at longer event horizons due to sample attrition and diminishing common support. Estimates beyond 60 months should be interpreted with caution, though the core dynamic pattern remains robust within the central post-treatment window.

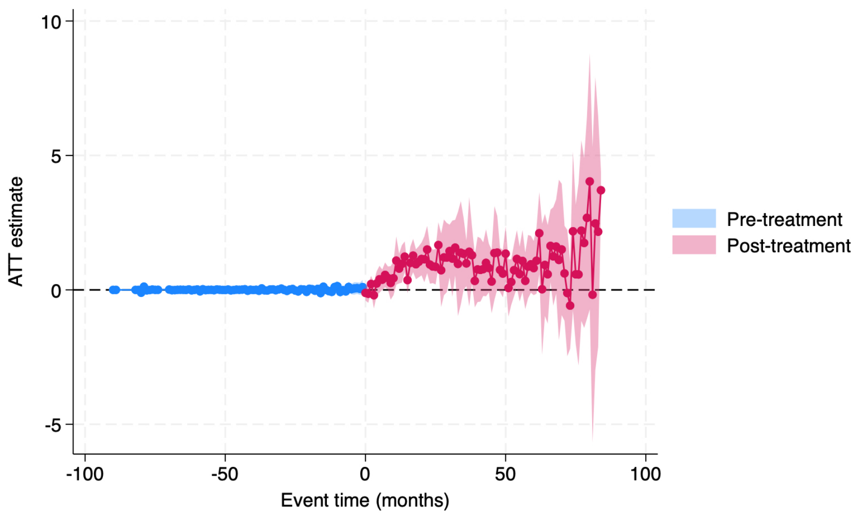

Figure 6: Event study estimates of ATT on number of installations (covariate-adjusted). No evidence of pre-treatment effects; sustained post-treatment increases with 95% CI bands.

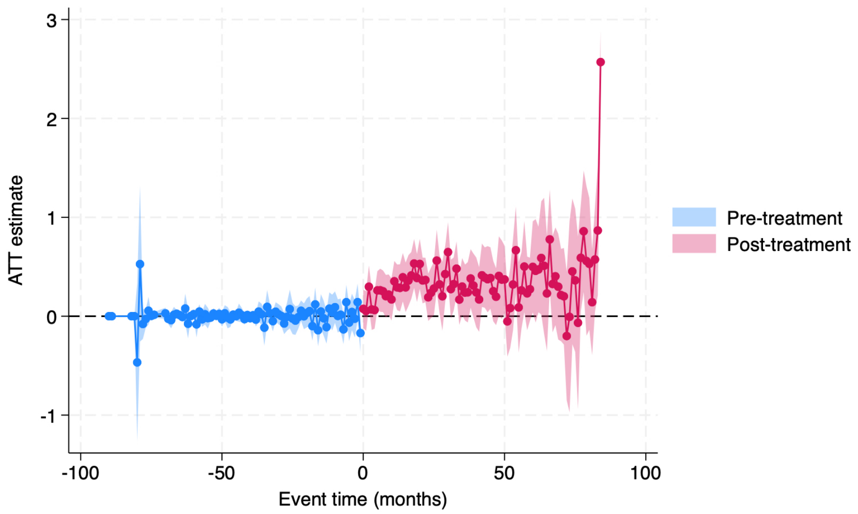

Figure 7: Event study estimates of ATT on log installed capacity (covariate-adjusted). No evidence of pre-treatment effects; sustained post-treatment increases with 95% CI bands.

To evaluate the robustness of the event-study results and support the choice of estimator, we replicate the analysis using a doubly robust inverse probability weighting estimator that combines propensity weighting with outcome regression. While this approach is consistent under correct specification of either model, it can be sensitive to misspecification—particularly in panel settings where outcomes are recorded monthly but covariates are updated annually (Sant'Anna and Zhao, 2020). This temporal mismatch may lead to instability in the outcome model and inflate sampling variability.

As shown in Appendix A Figures 13 and 14, the resulting dynamic treatment profiles are directionally consistent with those from the main specification but exhibit noticeably greater noise, especially in early post-treatment periods. Despite this, the average treatment effects remain highly similar across estimators: 0.966 for installation counts and 32.8 percentage for the installed capacity (Appendix A Table 8). This consistency reinforces the credibility of our findings and provides further empirical justification for the covariate-adjusted inverse probability weighting weighting estimator used in the primary analysis.

5.3 Heterogeneity analysis

To examine how the effects of the property tax exemption vary across municipal characteristics, we estimate subgroup-specific treatment effects using the covariate-adjusted difference-in-differences estimator. Figures 8 and 9 report the average treatment effects on the treated (ATT) for both outcome variables—number of installations and log installed capacity—across subgroups defined by pre-treatment income level, urban classification (urban, town, rural), housing typology (low-rise, high-rise), and their cross-classified combinations.

The results from the full sample reveal a broadly consistent pattern of policy effectiveness across socioeconomic and structural settings. Treatment effects on the number of installations are positive and significant for most groups, with particularly strong effects in high-income municipalities (ATT = 1.10, $p < 0.01$) and in towns with low-rise housing (ATT = 0.97, $p < 0.01$). Notably, the heterogeneity becomes more pronounced in the more finely stratified subgroups: within towns, building typology appears to differentiate uptake patterns, with stronger effects in low-rise areas; in contrast, income plays a more decisive role in rural municipalities, where effects are concentrated among higher-income areas. For installed capacity, the largest effects—around 43%—are found in low-rise rural areas and in high-income rural municipalities, while high-rise rural areas show negligible effects. These findings nudge to the role of structural suitability for solar PVs, with low-rise areas responding more strongly than high-rise contexts, and confirm that income-based differences in responsiveness do not reflect severe regressivity at the municipal level.

Estimates for urban and rural high-rise municipalities require cautious interpretation due to limited statistical power. Urban areas make up only 35 of the 417 municipalities in the sample (Appendix Figure 15). This reflects under-representation rather than systematically lower adoption in urban municipalities. Similarly, rural high-rise areas contribute just 2,219 treated observations—well below the 8,000–9,000 typical in other subgroups—resulting in imprecise estimates and wide confidence intervals.

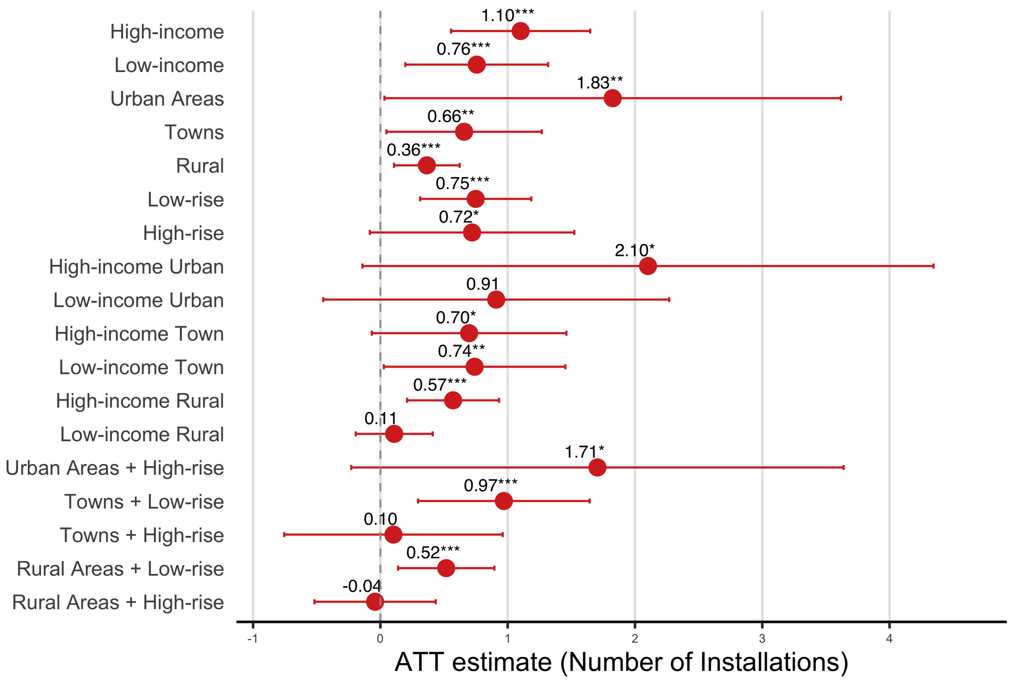

Figure 8: Heterogeneous treatment effects on number of installations (full sample). Covariate-adjusted ATT estimates with 95% CI bands.

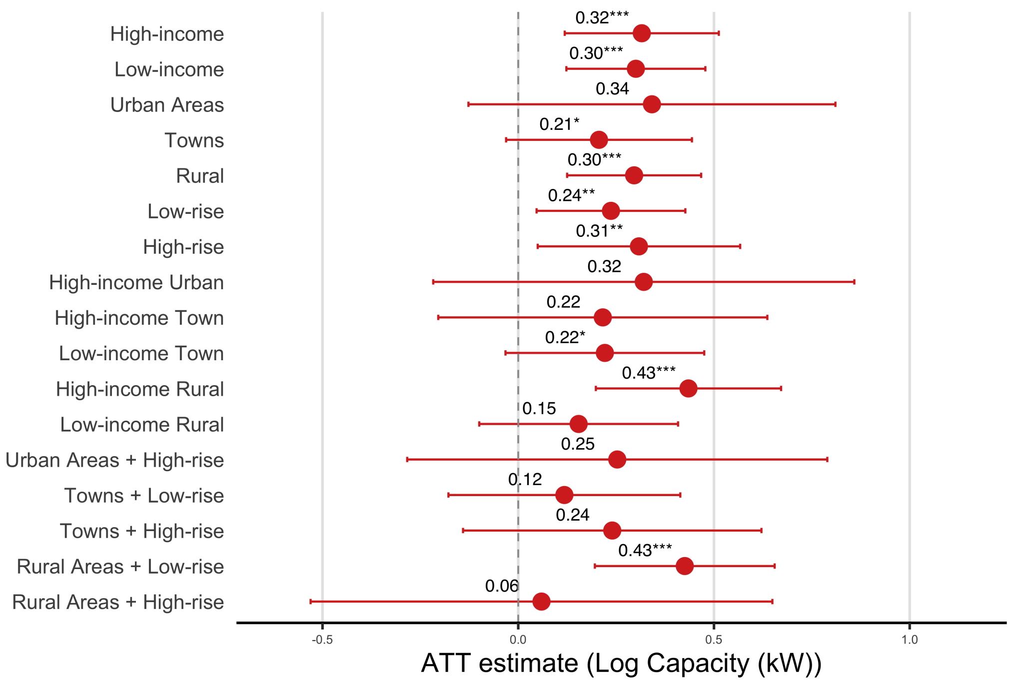

Figure 9: Heterogeneous treatment effects on log installed capacity (full sample). Covariate-adjusted ATT estimates with 95% CI bands.

Figures 10 and 11 display the corresponding ATT estimates from the residential subsample. The direction and significance of most subgroup effects remain broadly consistent with those found in the full sample, suggesting that non-household systems did not meaningfully distort the estimates in most settings. However, an important divergence appears for towns with low-rise housing: whereas the full sample showed a significant increase in installation counts but an imprecise and insignificant effect on installed capacity, the small-scale subsample yields statistically significant effects on both outcomes (ATT = 1.04 for installations, $p < 0.01$; ATT = 23.9% for capacity, $p < 0.05$). This shift suggests that a small number of large installations may have deflated the average capacity response in the full sample for this subgroup. Once those systems are excluded, the town-level estimates become more internally consistent and better aligned with the behavioural interpretation suggested by the increase in installation counts. The replication thus reinforces the robustness of the main findings while clarifying the capacity discrepancy observed in low-rise towns.

Overall, the majority of the subgroups exhibit positive and significant effects, supporting the interpretation that the property tax exemption induced behavioural change across a wide range of municipal contexts. The attenuation of some effects in the residential sample reflects the exclusion of systems but does not alter the core pattern of responsiveness observed in towns, low-rise areas, and high-income municipalities.

One exception is the subgroup of low-income urban municipalities, which displays a statistically significant decline in installed capacity (ATT = –31%). While this negative effect could reflect structural constraints or socioeconomic exclusion, interpretation should remain cautious given the limited number of adopting municipalities and the small treated sample size in this category.

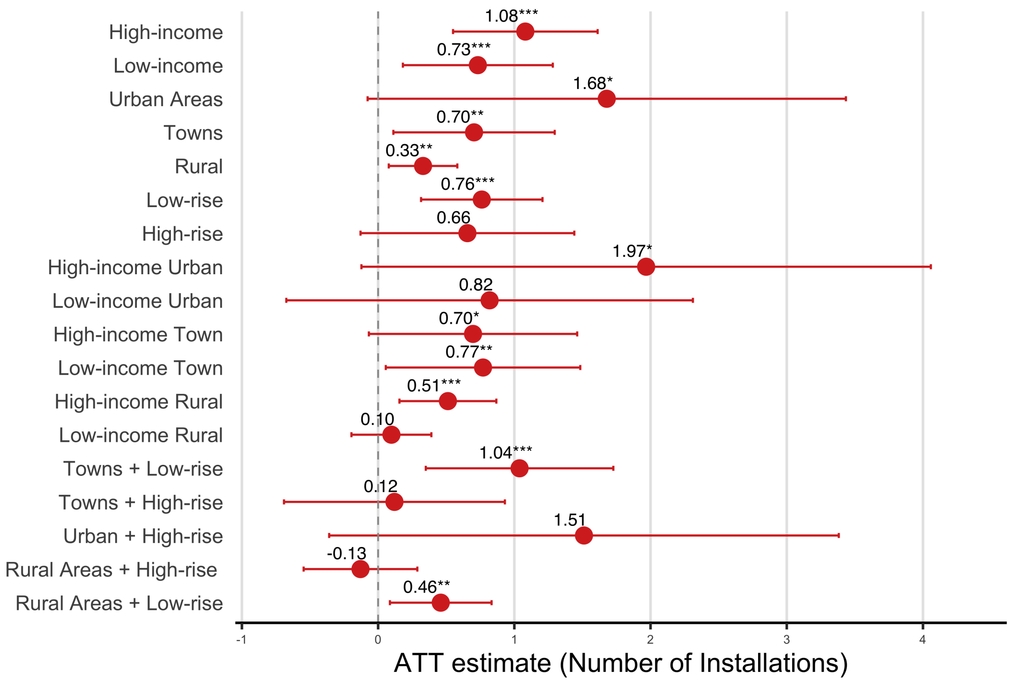

Figure 10: Heterogeneous treatment effects on number of installations (residential sample). Covariate-adjusted ATT estimates with 95% CI bands.

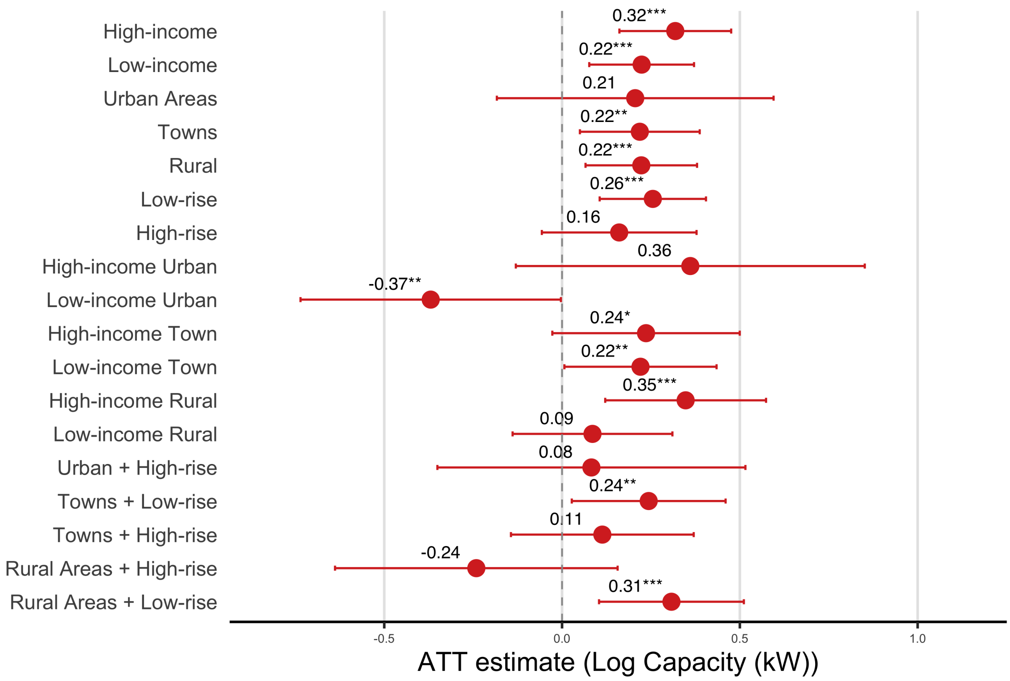

Figure 11: Heterogeneous treatment effects on log installed capacity (residential sample). Covariate-adjusted ATT estimates with 95% CI bands.

Taken together, these findings suggest that the policy was broadly effective across municipal income levels and structural settings. The strongest and most consistent effects are observed in towns and low-rise housing areas, where self-consumption systems are most feasible. The results do not point to strong regressivity between municipalities, though further research is needed to understand potential household-level disparities. The overall heterogeneity analysis reinforces a behavioural interpretation of the policy's effectiveness, rooted in household investment responses rather than isolated large-scale systems.

5.4 Intensive and extensive margins

Following Chen and Roth (2024) we estimate the policy effect separately along the extensive and intensive margins of adoption. Where the probability of any adoption presents the extensive margin and the amount adopted conditional on participation the intensive margin[4].
To address this, we define the extensive margin as a binary indicator equal to one if any PV capacity is installed in a municipality-month, and zero otherwise. The intensive margin is estimated separately on the restricted sample of observations with strictly positive adoption. Table 4 reports the corresponding ATT estimates. We find robust evidence of a statistically significant increase in adoption probability of approximately 7.5–7.8 percent, both for the full sample and for the subsample consistent of installations below 10 kilowatts. By contrast, the intensive margin effect is statistically significant only in the residential-scale subsample, where the ATT implies an average increase of 8.03 kW among adopting municipalities. However, this

[4]Their critique of transformation-based approaches—such as the logarithmic transformation of one plus the outcome variable and the inverse hyperbolic sine transformation— centres on the observation that these methods conflate two conceptually distinct responses: the intensive and the extensive margin.

result should be interpreted with caution, as it is based on a small number of observations and is driven by a few high-capacity outliers. The intensive effect in the full sample remains statistically insignificant and substantively uninformative, reflecting very low statistical power. These results reinforce the main conclusion that the property tax exemption policy primarily operated by encouraging new households to enter the residential solar market, rather than by inducing existing adopters to expand their installations. In line with Chen and Roth (2024), this decomposition offers a more transparent characterisation of policy responsiveness, avoids scale-dependent biases inherent to single-transformation models, and provides clearer insight into the behavioural mechanisms through which municipal tax exemptions influence solar PV uptake.

Table 4: Estimated ATT effects separated by margins

Group	Outcome	Estimate (SE)	95% CI	p	N
Panel A: Extensive Margin (Any Capacity Installed)					
All capacities	CSDID	0.075*** (0.028)	[0.019, 0.131]	0.009	38,193
Below 10 kW	CSDID	0.078** (0.031)	[0.018, 0.138]	0.011	38,193
Panel B: Intensive Margin (Conditional on Adoption)					
All capacities	CSDID	4.624 (20.161)	[–34.89, 44.14]	0.819	5,162
Below 10 kW	CSDID	8.033** (3.411)	[1.347, 14.720]	0.019	4,699

Notes: ATT estimates from covariate-adjusted CSDID models. The extensive margin reflects the probability of any PV capacity being installed in a municipality-month; estimates are expressed as percentage point changes. The intensive margin reflects the average installed capacity (in kilowatts) conditional on positive adoption. Standard errors are clustered at the municipality level. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5.5 Cost-effectiveness

To approximate the cost-effectiveness of the property tax exemption policy, we combine the estimated treatment effects with standard engineering assumptions and fiscal benchmarks. This back-of-the-envelope exercise yields an interpretable measure of carbon abatement cost per municipality, consistent with methodologies adopted in recent evaluations of residential renewable energy subsidies (De Groote et al., 2024; Kattenberg et al., 2023).

We begin by translating the estimated average treatment effects (ATT) from the main specification into changes in installed capacity. In the full sample, the ATT on the logarithm of installed PV capacity corresponds to an approximate increase of 34.4%. Evaluated at the pre-treatment mean of 46.07 kW among treated municipalities, this implies an average increase of 15.87 kW per municipality per month. In the small-scale subsample, where the ATT implies a 30.5% increase, and the pre-treatment mean is 18.78 kW, the corresponding average increase is 5.73 kW per month. These estimates reflect the monthly capacity additions attributable to the policy intervention.

To estimate avoided carbon emissions, we assume an average annual electricity yield of 1,400 kWh per kilowatt of installed PV capacity consistent with conservative benchmarks for Catalonia from European Commission Joint Research Centre (JRC) (2020). The implied annual electricity generation gains are 22,215 kWh in the full sample and 8,022 kWh in the subsample. Applying Catalonia's official 2023 electricity emissions factor of 260 gCO_2/kWh (Generalitat de Catalunya, 2024), these gains translate into annual emissions reductions of approximately 5.78 and 2.09 tonnes of CO_2, respectively.

We estimate the fiscal cost of the policy based on official municipal finance data. According to Institut d'Estadística de Catalunya (Idescat) (2024), the average annual IBI payment per property in Catalonia is €498.39. Given that most municipalities applied a 50% exemption for solar PV adopters, we assume a benchmark fiscal cost of €249.20 per municipality monthly extra installed capacity per year. While this benchmark is appropriate for the subsample sample, it likely understates the true cost in the full sample, which includes larger systems with higher cadastral values. As such, the abatement cost for the full sample should be viewed as a conservative lower bound.

Dividing the estimated fiscal cost by the corresponding annual emissions reductions yields an implied abatement cost of €43.12 per tonne of CO_2 in the full sample and €119.25 per tonne in the subsample. This figure compares reasonably with the official EU ETS benchmark of €80 per tonne $C0_2$ (European Commission, 2025). In contrast, the full-sample estimate of €43 likely understates the true abatement cost, as it includes larger installations typically located on properties with higher cadastral values. These cases inflate the estimated property tax exemption while not reflecting standard residential conditions. As a result, the average exemption across Catalonia is unlikely to represent typical household incentives, and the residential estimate of €119 per tonne offers a more accurate benchmark for evaluating policy cost-effectiveness.

The findings suggest that the property tax exemption was a cost-effective instrument for stimulating distributed solar generation and delivering measurable environmental benefits. Moreover, the policy compares favourably to other decentralised subsidy schemes evaluated in the academic literature, particularly when targeted at small-scale residential adopters. Full derivations are provided in Appendix B.

6 Conclusion

This paper provides causal evidence on the effectiveness of municipal-level fiscal incentives for promoting residential solar PV adoption. Exploiting the staggered implementation of property tax exemptions across Catalan municipalities, we estimate covariate-adjusted Difference-in-Differences models with municipality and month fixed effects. The results show that the policy led to a statistically significant increase in both the number of installations and the total installed capacity in kilowatts. On average, treated municipalities experienced 0.79 additional installations per month and a 34.4% increase in installed capacity, indicating that the policy successfully influenced both the adoption decision and the scale of investment.

These findings contribute to the growing literature on the role of financial incentives in the energy transition (Crago and Chernyakhovskiy, 2017; Hughes and Podolefsky, 2015). While most existing studies focus on national or regional programmes, our results demonstrate that decentralised instruments—such as municipal property tax exemptions—can also be effective. This suggests that local governments, despite fiscal and administrative constraints, can play a meaningful role in driving distributed renewable energy adoption.

The heterogeneity analysis confirms the robustness of the main effect across a wide range of municipal contexts. While somewhat larger gains are observed in high-income rural areas and

municipalities characterised by low-rise housing, treatment effects remain broadly consistent across subgroups, suggesting that the policy was inclusive in its reach at the municipal level. Importantly, the observed 7.5–7.8 percent increase in the probability of any installation suggests that the policy primarily operated through new adoption, rather than through system upgrades—highlighting its relevance for expanding participation in the energy transition.
To contextualise the environmental benefits, we conduct a back-of-the-envelope estimation of avoided emissions. In the residential subsample—where the fiscal valuation of exemptions is most reliable—the implied cost of carbon abatement is approximately €119 per tonne of CO_2. This estimate compares favourably with other decentralised subsidy schemes and suggests that municipal fiscal incentives can contribute to climate targets in a cost-effective manner.
Taken together, the results provide robust evidence that well-designed local fiscal incentives can play a meaningful role in accelerating the energy transition. The consistent effects observed across diverse municipal settings suggest that property tax exemptions—despite their relatively limited financial magnitude—may exert substantial behavioural influence. One possible explanation is their recurring and transparent structure, which could enhance their salience to homeowners. While this study does not directly examine policy visibility or perception, future research could explore how the design and communication of local incentives affect behavioural responsiveness. As subnational governments expand their role in climate policy, understanding not only the effectiveness but also the perceptibility of local instruments will be essential to ensure impact and equity.

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Appendix A

Table 5: Covariate balance before and after inverse probability weighting.

Label	T_M	C_M	W_T	W_C	SD Before	SD After	% Red.
Population	18082.69	5579.69	12027.94	8477.86	19.26	5.47	71.61
Income (€)	13343.90	12608.46	13215.69	13291.16	35.22	3.61	89.74
Over 65 (%)	18.51	19.68	18.69	18.77	27.29	1.80	93.39
Gini Index	29.39	30.10	29.51	29.61	23.33	3.22	86.20

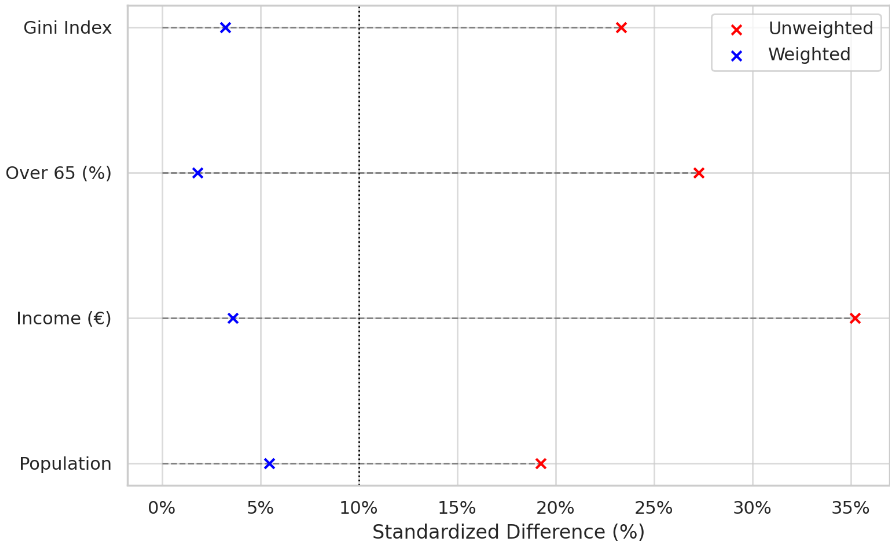

Figure 12: Standardised mean differences before and after inverse probability weighting (IPW), by covariate

Table 6: Estimated ATT Effects on Solar PV Adoption (without covariates)

Outcome	**Method**	**Estimate**	**95% CI**	**N**
Panel A: Log Installed Capacity (kW)				
	TWFE	0.346***	[0.230, 0.463]	46,330
	CSDID	0.443***	[0.290, 0.597]	46,330
	SDID	0.475***	[0.329, 0.622]	46,330
Panel B: Number of Installations				
	TWFE	1.178***	[0.677, 1.680]	46,330
	CSDID	2.059***	[1.432, 2.687]	46,330
	SDID	1.982***	[1.380, 2.585]	46,330

Notes: ATT estimates from TWFE, CSDID, and SDID models without covariates. The sample includes 417 municipalities over the full period January 2015 to May 2024. $^{***}p < 0.01$.

Table 7: Estimated ATT Effects on Installed Capacity (kW), IHS Transformation

Method	**Estimate (SE)**	**95% CI**	**N**
TWFE	0.365*** (0.061)	[0.244, 0.486]	38,208
CSDID	0.350*** (0.084)	[0.185, 0.514]	38,193
SDID	0.471*** (0.074)	[0.327, 0.616]	38,208

Notes: ATT estimates for installed solar capacity using the inverse hyperbolic sine (IHS) transformation. IHS behaves similarly to the log for large values while remaining defined at zero. All models include municipality and time fixed effects and adjust for income, population, inequality, and demographic structure. Standard errors clustered at the municipality level. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

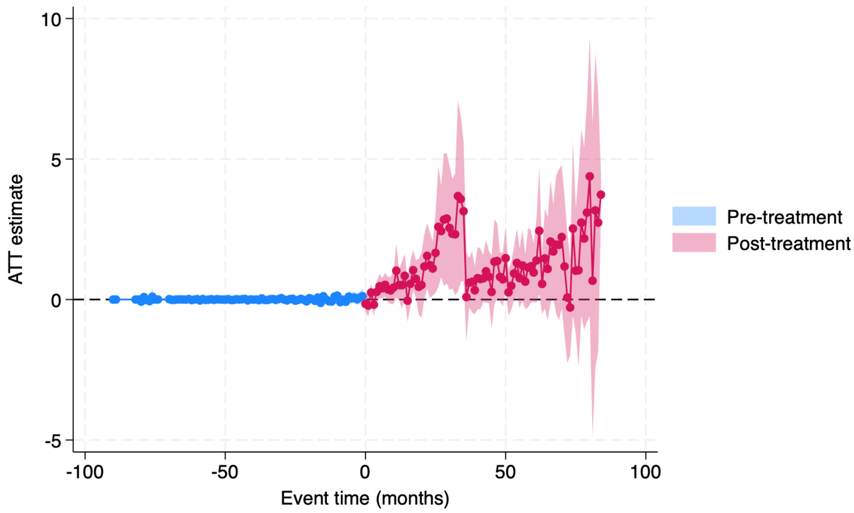

Figure 13: Event study estimates of ATT on log installed capacity (CSDID, covariate-adjusted, using doubly robust estimator). Pre-treatment estimates (blue) and post-treatment dynamics (red) with 95% CI bands.

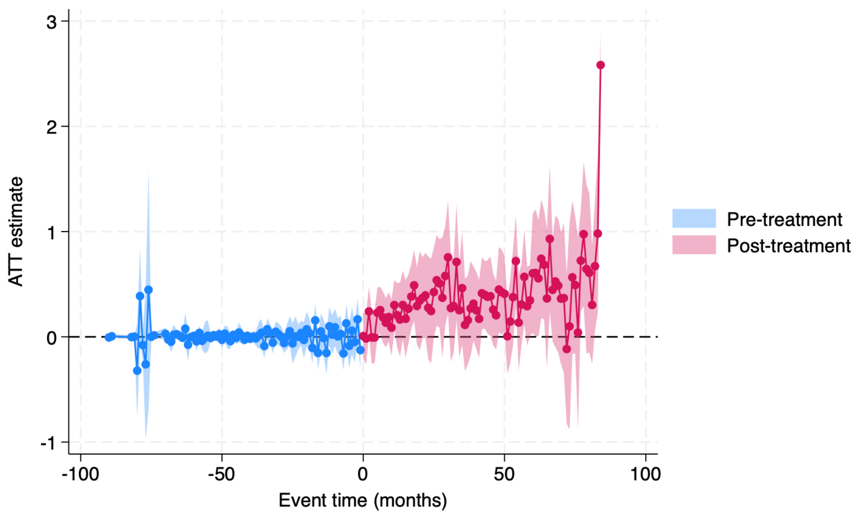

Figure 14: Event study estimates of ATT on log installed capacity (CSDID, covariate-adjusted, using doubly robust estimator). Pre-treatment estimates (blue) and post-treatment dynamics (red) with 95% CI bands.

Table 8: Estimated ATT Effects on Solar PV Adoption (DRIPW Estimator)

Outcome	**Method**	**Estimate (SE)**	**95% CI**	**N**
Number of Installations	DRIPW	0.966*** (0.305)	[0.369, 1.563]	38,193
Log Installed Capacity (kW)	DRIPW	0.284*** (0.099)	[0.088, 0.478]	38,193

Notes: Estimates are average treatment effects on the treated (ATT) using the doubly robust estimator (DRIPW) from Callaway and Sant'Anna (2021). All models include time-invariant municipality-level covariates updated annually. Results are nearly identical to those obtained using the standard IPW estimator (STDIPW), providing further support for the robustness of the preferred specification. Standard errors in parentheses. $^{***}p < 0.01$.

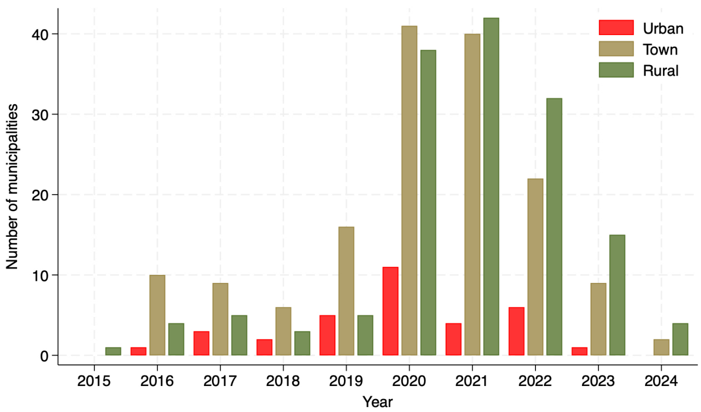

Figure 15: Shows the annual number of adopting municipalities by DEGURBA category. Urban areas adopted earlier, while rural uptake peaked later.

Appendix B: Carbon abatement calculations

To estimate the annual CO_2 abatement resulting from the property tax exemption, we proceed in three steps, applied separately to the full sample and the residential subsample.

Step 1: Installed capacity increase. The average treatment effect on the logarithm of one plus installed capacity is 0.344 in the full sample and 0.266 in the residential sample. These log-point estimates correspond to percentage increases of:

$$\text{Full sample: } (e^{0.296} - 1) \times 100 \approx 34.4\% \qquad \text{Residential sample: } (e^{0.266} - 1) \times 100 \approx 30.5\%$$

Multiplying these percentages by the pre-treatment mean installed capacities among treated municipalities gives:

$$\text{Full sample: } 46.07 \times 0.344 = 15.87 \text{ kW} \qquad \text{Residential sample: } 18.78 \times 0.305 = 5.73 \text{ kW}$$

Step 2: Annual electricity generation. We assume an average yield of 1,400 kWh per kW of installed PV capacity, consistent with long-term empirical benchmarks for Spain (IDAE – Instituto para la Diversificación y Ahorro de la Energía, 2021; European Commission Joint Research Centre (JRC), 2020).

$$\text{Full sample: } 15.87 \times 1,400 = 22,218 \text{ kWh/year} \qquad \text{Residential sample: } 5.73 \times 1,400 = 8,022 \text{ kWh/year}$$

Step 3: Avoided CO_2 emissions. Using Catalonia's 2023 electricity emissions factor of 260 gCO_2/kWh (Generalitat de Catalunya, 2024), equivalent to 0.26 $kgCO_2$/kWh:

$$\text{Full sample: } 22,218 \times 0.26 = 5,776 \text{ kgCO}_2 = 5.78 \text{ tonnes}$$

$$\text{Residential sample: } 8,022 \times 0.26 = 2,086 \text{ kgCO}_2 = 2.09 \text{ tonnes}$$

These figures yield the final estimated annual emissions reductions reported in the main cost-effectiveness analysis.