Wind power capacity increases local income without affecting other sectors

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Abstract

Wind power is a central element for climate change mitigation. However, local opposition jeopardizes its diffusion. The main argument is that wind power displaces traditional sectors and population without providing any local benefits. I test these hypotheses for a pioneer (wind deployment since the early 2000') but worst-case region (due to lack of local participation in the wind development process and foreign ownership of wind capacity). Using a difference-in-difference framework with municipal data of Galicia (Northwest Spain) for the period 1996-2019, I find that wind power increases local disposable income (high confidence) without affecting other sectors (low confidence). Wind power has not reversed the negative population trend in rural municipalities.

Keywords

Wind energy, local disposable income, economic development, difference-in-difference.

Highlights

- Wind capacity increases municipal disposable income by ~5.5%.
- The effect of wind capacity on income increases over time.
- Rural municipalities benefit more from wind capacities.
- Wind capacity has no significant effect on the agricultural, livestock and hospitality sectors.
- Wind capacity has not been able to reverse depopulation in rural municipalities.

JEL codes

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1 Introduction

Wind power is one of the main technologies to decarbonize the electricity system (IEA, 2021). However, its further deployment is jeopardized by growing opposition of local communities (Lindvall, 2023; Stokes et al., 2023; Susskind et al., 2022). The main argument raised against further wind power diffusion is that it harms local development because it displaces population and other traditional sectors without providing any benefits to the communities where they are located. While wind power is one of the electricity generation technologies with lowest externalities (Smith et al., 2020), these consist of local disamenities (Lehmann et al., 2023), such as noise and landscape impacts, that are more tangible than global long-term phenomena such as the global warming caused by fossil fuels (Clarke et al., 2022).

Whereas there is a negative correlation between wind capacity and disposable income (Appendix Figure A.1), this relationship is confounded by the fact that wind turbines are usually located in more rural and thus poorer areas. To shed light on the local impacts of wind capacity, I estimate multiple difference-in-difference (DiD) models to identify the effect of wind installed capacity on disposable income, population and other sectors (agriculture, livestock and hospitality) in Galicia (Northwest of the Iberian peninsula) for the period 1996-2019. Galicia has been a pioneering region in wind deployment with the first difussion wave starting in the early 2000' and stagnating since 2010. The dispersed population and long time series with staggered adoption allows for causal identification via DiD design.

Causal identification relies on the parallel trends assumption (PTA), which is plausible for income, violated for population, and untestable[1] for other sectors. For this reason, the results presented here have different levels of certainty. All the evidence confirms that wind power has a positive effect on local disposable income and all of its components: profits, wages and net transfers. Due to the violation of the PTA, I cannot test the causal effect of wind power on population, although it is certain looking at descriptive data that wind power has at least not been able to reverse the negative population trend in rural areas where most wind capacity is installed. Finally, I find no significant effect of wind power capacity on the agricultural, livestock and hospitality sectors, although given the scarce data available, sectoral evidence is highly uncertain.

There is a rich literature on the impact of wind capacity on local employment and regional economic growth that I briefly review in the next section. However, there is little evidence on the impacts of wind capacity on local disposable income and its components and other sectors at municipal level, which is important to evaluate the trade-offs that local communities face in the context of an accelerating energy transition. Internal validity is ensured for the disposable income estimates by placebo tests, pre-trend tests, and pre-event coefficients with the Callaway and Sant'Anna heterogeneity-robust estimator (Call-

[1] The PTA is technically untestable, but its plausibility can be assessed in multiple ways that will be discussed in further sections.

away et al., 2021). The results are likewise robust to different specifications, estimators, and definitions of the treatment (binary, continuous and discrete). Finally, because the Galician case can be considered close to worst case for local impacts due to the limited participation of local communities in the development process (Simón et al., 2019) and the fact that most of the capacity is owned by foreign capital (Regueiro-Ferreira & Doldán-García, 2020), these estimates can be considered as a lower bound for other regions.

2 Background

There is rich but mixed literature on the local employment effects of wind power investments. Gonçalves et al. (2020) obtain positive results for both employment and wages in Brazil. Costa and Veiga (2021) find that the construction phase of wind turbines decreases unemployment in Portugal by 0.17-0.23 percentage points per 100 MW of installed capacity and have spatial spillover effects up to 30 km away. Fabra et al. (2023) find a decrease in unemployment during the maintenance phase, but not during the construction phase, and no effect on employment in Spain. For the case of Texas, Hartley et al. (2015) find a positive impact on both employment and wages for shale gas investments but not for wind ones. Slattery et al, (2011), however, find both positive economic and employment impacts of wind power deployment in West Texas. Finally, Mauritzen (2020) finds a positive effect of wind power on rural couties' wages in the US, noting "large geographic and socioeconomic variation", which may explain the mixed results mentioned above.

Other studies focus on economic impacts more generally, from specific case studies (Greene & Geisken, 2013) to global macroeconomic assessments (Montrone et al., 2022), including regional mixed-methods approaches (Munday et al., 2011; Shoeib et al., 2021). The most comprehensive studies (Brown et al., 2012; Brunner & Schwegman, 2022) evaluate the development impacts of wind power on US counties with instrumental variables and difference-in-differences approaches, respectively. Both of them agree that wind power increases counties' income. Brown et al. also find a positive impact on overall employment, although Brunner and Schwegman identify a change in the composition of employment, but not in total employment. May and Nilsen (2019), however, do not find any effect of wind power on German counties' GDP using wind speed as an instrumental variable. Whereas many papers assess employment effects at municipal level, all GDP or income effects of wind have been studied in the literature at higher spatial resolution (usually county). Municipal economic impacts are however relevant because most of wind power's externalities occur at the local level, and thus the cost-benefit analysis for local communities is done at high spatial resolution. This study is the first to provide comprehensive evidence of the local economic effects of wind power at the municipal level.

The Galician case is a paradigmatic example of wind deployment because capacity increased fast in the early 2000' with a top-down approach without community participation (Simón et al., 2019), but stagnated from 2010 due to increasing local opposition (Upham & García Pérez, 2015). According to Regueiro-Ferreira (2020), 88% of the wind power capacity in Galicia is owned by large energy groups and investment funds, entailing that most benefits from this activity do not stay in local communities. Copena and Simón (2018) identify land rents as the main benefit of wind capacity to local communities and quantifies the revenues in 0.84 million euros in 1999 growing to 10.1 million 2015, an average of 1,800 and 3,014 euros per MW of installed capacity, respectively. Likewise, Copena et al. (2019) quantify the revenues provided by wind installations to municipal governments in 17.8 M€ in 2017. The results presented here confirm the positive impact of wind capacity on local disposable income through its three components: profits, wages and net transfers.

3 Data

This analysis focuses on Galicia, in the Northwest of the Iberian peninsula. All the data have been provided by the Galician Energy Institute (INEGA) and the Galician Statistical Institute (IGE). On the one hand, the relatively small spatial coverage of this study (Galicia has an area of 29,574 km^2) limits the number of observations and statistical power. On the other hand, the homogeneity of this region and its demographic, economic and cultural differences with the rest of the Iberian peninsula ensure internal validity. Moreover, Galicia was one of the pioneers of wind capacity worldwide and the population is dispersed across its territory, which provides longitudinal flexibility for the analysis. I use the following data:

1. Wind installed capacity at farm level provided by INEGA. This dataset provides the current capacity and commisioning year of all installations and the municipalities where the farms are located. Some wind farms span seceral municipalities, in which case I assume an equal distribution of capacity among them. This assumption is only relevant for the continuous and discrete treatments that I test in addition to the main binary treatment.

2. Municipal income data provided by IGE:

   2.1. Gross disposable income per capita, available for the year 1996, that I use as baseline, and between 2000-2019.

   2.2. Municipal income accounts, available for the year 1996 and between 2010-2019, including total municipal income and its decomposition. Because the components are defined differently for the year 1996 and the series 2010-2020, I summarize the information in thee comparable categories:

profits (comprising gross operating surplus and mixed income), wages (compensation of employees), and transfers (including social benefits and contributions, taxes, net transfers and others).

3. Population. IGE provides two population data series: (3.1) historical population, sourced from the administrative registry, and (3.2) reference population, estimated by the statistical agency according to demographic information. The historical population series has longer coverage, but the IGE uses the reference population for their personal income data. To express the income accounts in per capita terms, I first fill missing data of the historical population series by linear interpolation. Then I extrapolate the temporal coverage of the reference population data estimating Equation 1, where RefPop and HistPop are reference and historical population, respectively, for municipality i in year t. The model includes a municipality fixed effect (α_i), a trend and their interaction. I predict the reference population for the missing years (1996-2001) with an in-sample mean absolute percentage error of 0.68%.

$$RefPop_{it} = \alpha_i + \beta_1 Trend_t + \beta_2 HistPop_{it} + \beta_3 Municipality_i \cdot Trend_t + \epsilon_{it} \quad (1)$$

4. Population density. IGE provides a population density indicator consistent with Eurostat definition and adapted to the high dispersity of Galician population. The first classification is Eurostat GU2011 definition of high, medium and low density areas (horizontal axis of Figure 1.b). IGE further disaggregates into three additional subcategories as shown in Figure 1.b.

5. Agriculture. IGE provides the area (hectares) dedicated to different types of crops (arable crops, fruits, vineyard and other) per municipality in 1999 and 2009.

6. Livestock. IGE provides an indicator in livestock units (according to the Eurostat LSU definition) for different types of livestock (Bovine, Ovine, Caprine, Porcine, Equine, Avian and Rabbits) per municipality in 1999 and 2009.

7. Hospitality. IGE provides data on the hospitality sector establishments by municipality, particularly accommodation and food providers.

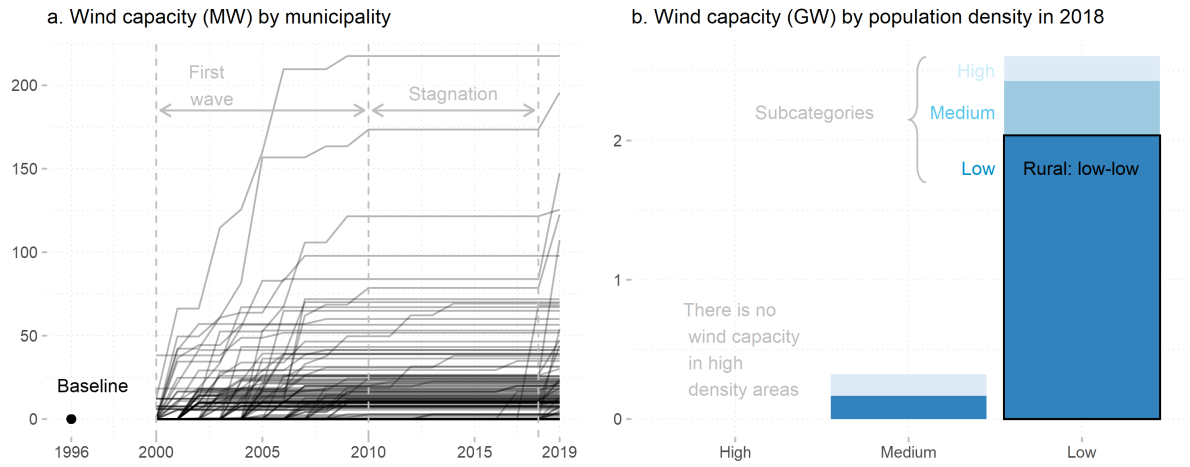


Figure 1. Wind installed capacity. (a) By municipality over time (source: INEGA). (b) By population density divided into three categories (horizontal axis) and three subcategories (colors), both labelled as high, medium and low. I define rural municipalities as those in the low-low category (source: IGE).

Figure 1 shows the main data: evolution of wind installed capacity per municipality (a), and installed capacity by population density of the municipality where it is located (b). Panel 1.a shows that installed capacity data are available between 2000 and 2019. The first decade of the XXI century was the first wave of wind deployment in Galicia. Between 2010 and 2018 wind capacity stagnated and it starts to increase again in 2019. This evolution will determine the empirical design further explained in the following sections.

Municipalities' population density (Panel 1.b) shows the trade-off between statistical power and internal validity. The more categories we include in the analysis, the larger dataset and the higher statistical power. However, because cities, towns and villages have different population and income trends, combining them could violate the parallel trends assumption required for identification. Panel 1.b shows that there is no wind capacity installed in high density areas, so these are excluded from the analysis (i.e. they are not used as controls). I perform the analysis for all remaining municipalities on the one hand (i.e. medium and low density categories), and for the most rural municipalities only, which I define as those in the low category and low subcategory according to IGE, representing 70% of total wind installed capacity in 2018. Table A.1 in the Appendix presents the summary statistics. Four municipalities are omitted due to inconsistencies across datasets (A Illa de Arousa, Cotobade, Oza and Cesuras). The final dataset comprises 310 municipalities, of which 121 have some wind capacity, between 1996 and 2019. For some parts of the analysis I will use a subset of these data comprising only municipalities that have constant capacity during the stagnation period (2010-2018, illustrated in Figure A.2 in the appendix).

4 Empirical strategy

4.1 Specifications

The causal estimand of interest is the effect of wind capacity on local disposable income, population, and output of the agricultural, livestock and hospitality sectors for municipalities that have installed capacity compared to the disposable income they would have had in the absence of wind capacity, i.e. the average treatment effect on the treated (ATT). The simplest strategy to estimate the effect of wind power capacity on municipal disposable income is to estimate a series of canonical 2x2 DiD models for each year (ATT(t)) using 1996 as the baseline period (Figure 3.a):

$$y_{it} = \alpha_i + \alpha_t + \gamma_1 D_{it} + \epsilon_{it} \tag{2}$$

Where y_{it} is the dependent variable, α_i and α_t are the individual and time fixed effects, and D_{it} is the indicator variable for treated units (i.e. 1 when municipalities have installed capacity and 0 otherwise). γ_1 is thus the causal DiD coefficient of interest.

Estimating one model for each year does not allow us to differentiate whether the coefficient is driven by a given level of capacity, by increasing capacity over time, or by potential dynamic treatment effects. For this reason, I create a "static subset" that excludes the staggered adoption period and includes only municipalities with constant capacity between 2010 and 2018 (see appendix Figure A.2). Estimating Equation 2 with this subset provides an estimate that is not confounded by increasing capacity and does not suffer potential negative weight bias characteristic of staggered adoption settings (Goodman-Bacon, 2021).

Because the treatment (wind installed capacity) is actually continuous, Section 5.1.2 explores three options: (i) a binary treatment (has wind capacity or not), a continuous treatment (log of installed capacity +1), and a discrete treatment differentiating municipalities with low or high capacity (cut at the median of the municipalities with some capacity, see Appendix Figure A.2). The continuous treatment could be interpreted as an Average Causal Response of the Treated ($ACRT$) for each increment of the dose (installed capacity) in the absence of selection bias (Callaway et al., 2021). However, there might be selection bias depending on the mechanism through which wind capacity affects income. If the mechanism is through capacity (e.g. taxes on installed capacity, land rents or wages), there is no selection bias because the outcome (income) is independent from the capacity factor, which likely determines dosage (i.e. investors install more capacity in locations with high capacity factors). However, if the mechanism is through the energy generated (e.g. profits derived from the electricity sold), then there may be self-selection bias because income depends on capacity factor. I explore these mechanisms in Section 5.1.3

by looking at the municipal income decomposition. To overcome this potential self-selection bias of the ACRT and give more detail regarding dosage (amount of installed capacity), I also provide the discrete results, that are unbiased estimates of the $ATT(d)$ (where dose d is the amount of installed capacity) because each level is compared to the control group (no capacity)).

The static subset also allows us to estimate dynamic treatment effects (Figure 3.b) because it excludes any change in capacity, so any time-varying effect is caused by the treatment itself:

$$y_{it} = \alpha_i + \gamma_t \Sigma_t D_{it} \cdot ET_t + \epsilon_{it} \tag{3}$$

Where γ_t are the dynamic effects for each period t, and ET_t is the event-time variable interacted with the treatment using 1996 as the baseline period and 2010-2018 as the treatment period. Note that because this equation is estimated using the static subset, event-time does not represent the time since treatment as in conventional event-studies, but only the calendar year.

Finally, I take advantage of the full dataset (Figure 1), including the staggered adoption and stagnation periods, by estimating the overall and event-time (year since installed) treatment effects with the heterogeneity-robust Callaway and Sant'Anna (CS) estimator (Callaway & Sant'Anna, 2021). The cohort-specific effects are too noisy due to the few municipalities installing capacities each year and the event-time effects confound the effect of increasing dosage (so they are not dynamic treatment effects), but this estimator gives a comprehensive overview of the results using the whole dataset and provides additional robustness (Figure 3.c).

For the agricultural and livestock sectors the model in Equation 2 is overespecified due to the individual fixed effects with few observations, so I estimate an even simpler model with a group dummy instead of individual fixed effects.

$$y_{it} = \beta_4 After_t + \beta_5 Treated_i + \gamma_2 After_t \cdot Treated_i + \epsilon_{it} \tag{4}$$

None of these strategies is perfect. Using the whole dataset, the yearly TWFE and CS estimators reveal the total effect but do not allow us to differentiate the effect of dosage. Using the static subset allows us to have more flexibility in the definition of the treatment, so we can estimate the effect of dosage and potential dynamic treatment effects, but we lose statistical power and cannot estimate event-time parameters. The event-time parameters of CS include both the dynamic treatment effect and the effect of increasing capacity. All these results, considered together (see Figure 3), provide a robust and comprehensive understanding of the effect of wind capacity on municipal disposable income.

4.2 Validity

The identification possibilities are constrained by data availability. Identification in the DiD framework relies on the parallel trends assumption (PTA): municipalities that install wind capacity would have evolved in the same way as those that did not install wind capacity in the absence of treatment (wind capacity). Figure 1.b shows that wind capacity tends to be installed in low density areas. The first precaution I take to prevent violation of the PTA is to exclude high density areas as controls. Then I create two subsets: one with all remaining municipalities (middle and low density according to the Eurostat definition), which will provide higher statistical power but has higher risk of PTA violation, and another one with only the most rural areas (low-low density according to the IGE definition), that provides lower statistical power but is safer against potential PTA violations. Then, I take a threefold approach to check the plausibility of the PTA: (i) pre-trends test, (ii) placebo test, and (iii) CS (Callaway & Sant'Anna, 2021) heterogeneity-robust staggered adoption estimator to test the pre-event coefficients on the full dataset. Because the PTA is sensitive to functional form (Roth & Sant'Anna, 2023), all tests are performed with the same log transformations as the main analysis. Using levels instead of logs for the dependent variables worsens validity due to the skewness of the municipal disposable income and population distributions.

4.2.1 Pre-trends

I check the plausibility of the parallel trends assumption by estimating a pre-trends test for each cohort of the form

$$y_{it} = \alpha_0 + \beta_6 Trend_t + \beta_7 Group_{it} + \beta_8 Trend_t \cdot Group_{it} + \epsilon_{it} \tag{5}$$

Where Trend is a numeric year variable, group is a treatment indicator variable, and the interaction between both reveals whether the trends are different for the treated and control groups. I estimate this pre-trends model for all cohorts between 2002 and 2009, using only the data before that year to account only for the trends before the treatment. For this reason, the latest cohorts have more statistical power than the earlier ones. Tables 1 and 2 show the pre-trend test results for disposable income and population, respectively. Disposable income has a positive trend for both groups whereas population has a negative albeit insignificant trend. The trend-group interaction is significant for neither income nor population, which supports the parallel trends assumption.

	2002	2003	2004	2005	2006	2007	2008	2009
Year	0.074***	0.057***	0.061***	0.062***	0.061***	0.059***	0.058***	0.057***
	(0.014)	(0.007)	(0.004)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
Group	−16.762	−10.338	−6.693	−7.503	−5.179	−1.563	0.120	2.108
	(55.466)	(31.303)	(20.500)	(15.764)	(14.077)	(11.809)	(11.781)	(11.474)
Interaction	0.008	0.005	0.003	0.004	0.003	0.001	0.000	−0.001
	(0.028)	(0.016)	(0.010)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
Num.Obs.	606	909	1212	1515	1818	2121	2424	2727
R2	0.073	0.092	0.177	0.270	0.338	0.394	0.450	0.490
R2 Adj.	0.068	0.089	0.175	0.269	0.337	0.393	0.449	0.490
Std.Errors	IID	IID	IID	IID	IID	IID	IID	IID

* p < 0.1, ** p < 0.05, *** p < 0.01

Table 1 Disposable income cohort-specific pre-trend tests.

	2002	2003	2004	2005	2006	2007	2008	2009
Year	−0.005	−0.007	−0.006	−0.006	−0.006	−0.005	−0.005	−0.005
	(0.083)	(0.040)	(0.025)	(0.017)	(0.013)	(0.010)	(0.008)	(0.007)
Group	7.453	12.396	13.088	13.551	10.964	15.026	19.599	17.584
	(332.912)	(186.981)	(123.310)	(96.010)	(86.598)	(72.398)	(71.657)	(69.174)
Interaction	−0.004	−0.006	−0.007	−0.007	−0.006	−0.008	−0.010	−0.009
	(0.166)	(0.093)	(0.062)	(0.048)	(0.043)	(0.036)	(0.036)	(0.035)
Num.Obs.	606	909	1212	1515	1818	2121	2424	2727
R2	0.000	0.001	0.000	0.000	0.001	0.005	0.006	0.004
R2 Adj.	−0.005	−0.002	−0.002	−0.002	−0.001	0.004	0.005	0.003
Std.Errors	IID	IID	IID	IID	IID	IID	IID	IID

* p < 0.1, ** p < 0.05, *** p < 0.01

Table 2 Population cohort-specific pre-trend tests.

4.2.2 Placebos

Using the same specification as for the main analysis (Equation 2), I create two sets of placebos: one in 2005 using the "first wave" period (2000-2009), and one in 2015 using the "stagnation period" including 2019 (2010-2019). There are thus 5 years of control period (2000-2004 and 2010-2014), and 5 years of placebo treatment (2005-2009 and 2015-2019), respectively. Within in each of these settings, I randomly assign placebo treatment to 50, 100 and 200 control units. Tables 3 and 4 show all the placebo tests for disposable income and population, respectively. None of the placebos is significant, which supports the plausibility of the parallel trends assumption.

Period	2000-2009			2010-2019		
Treated units	50	100	200	50	100	200
Placebo	0.008	−0.004	−0.004	0.013*	0.002	−0.006
	(0.010)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)
Num.Obs.	2343	2343	2343	1776	1776	1776
R2 Adj.	0.972	0.972	0.972	0.960	0.960	0.960

* p < 0.1, ** p < 0.05, *** p < 0.01

Table 3 Disposable income placebo test for 50, 100 and 200 simulated placebo-treated observations in the "first wave" (2000-2009) and "stagnation" (2010-2019) periods.

Period	2000-2009			2010-2019		
Treated units	50	100	200	50	100	200
Placebo	0.011	0.007	0.013	−0.001	−0.009	0.004
	(0.012)	(0.011)	(0.010)	(0.008)	(0.007)	(0.007)
Num.Obs.	2343	2343	2343	1776	1776	1776
R2 Adj.	0.998	0.998	0.998	0.999	0.999	0.999

* p < 0.1, ** p < 0.05, *** p < 0.01

Table 4 Population placebo test for 50, 100 and 200 simulated placebo-treated observations in the "first wave" (2000-2009) and "stagnation" (2010-2019) periods.

4.2.3 Pre-event coefficients

Finally, I use the CS estimator with the full dataset to estimate the event-time coefficients. Whereas both the pre-trends and placebo tests failed to identify a significant violation of the PTA for either population or disposable income, the CS pre-event coefficients (Figure 2) show significant results and downwards trend for population. This implies that control and treatment groups have different population trends already before treatment and thus DiD is not suitable to identify treatment effects. This analysis reveals the relevance of the CS estimator, not only to avoid bias in staggered adoption settings, but also to check the plausibility of the parallel trends assumption with pre-event-time coefficients. Because pre-trends and placebo tests may have low statistical power (Roth, 2022), the CS estimator is particularly useful to identify PTA violations.

Given this violation, I focus on the effect of wind capacity on local income in the body of the paper and show the main population specification in the Appendix (Section A.2). I also show DiD results for the impact of wind capacities on the agricultural, livestock and hospitality sectors, but due to the lack of pre-treatment data, it is not possible to check the PTA. These results should therefore be interpreted with caution.

Because the baseline year is 1996 and the capacity data starts from 2000 (and in many cases the identification starts from 2010 during the stagnation period), we can safely discard any potential anticipation.

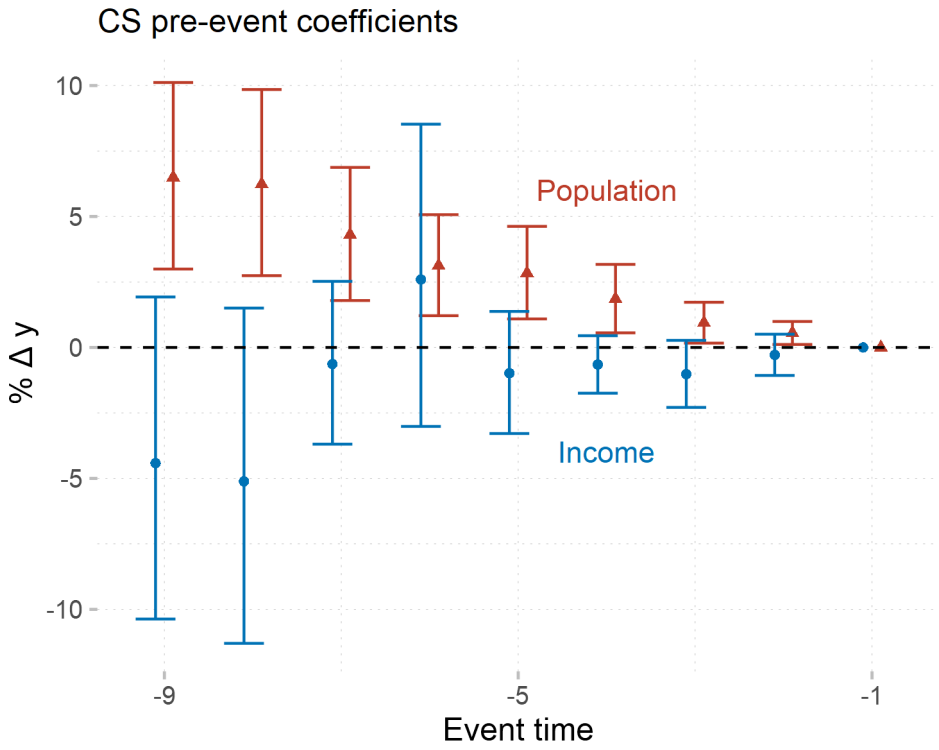


Figure 2. Callaway and Sant'Anna (2022) pre-event coefficients with universal baseline at event-time -1 for population and disposable income.

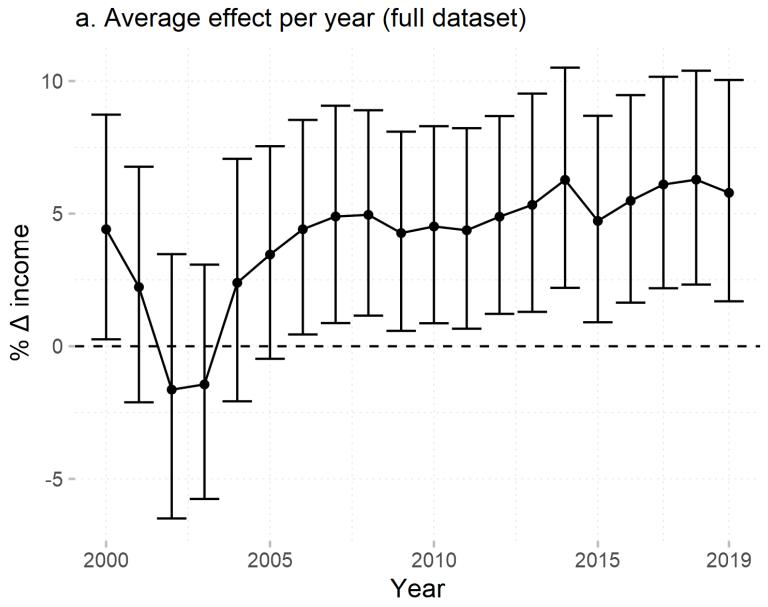
5 Results

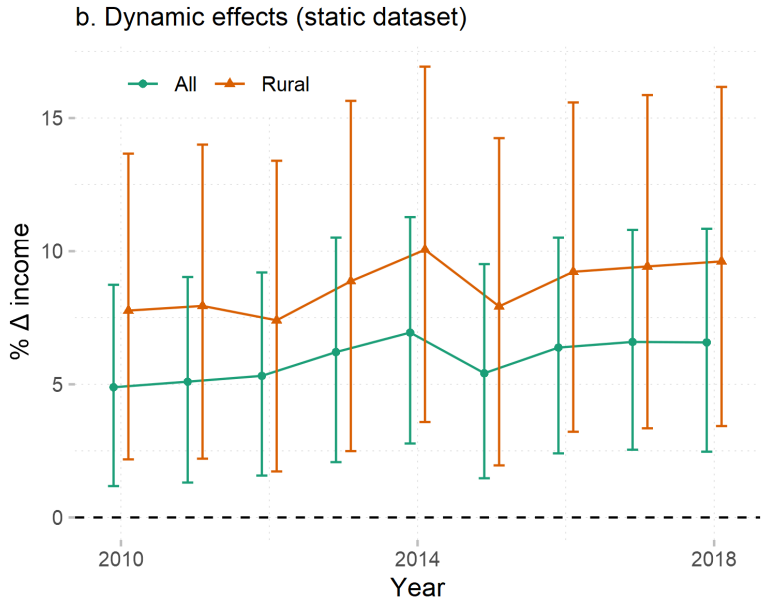
5.1 Income

5.1.1 Total effects

I first estimate the total effect of wind capacity on municipal disposable income using the full dataset (Figure 1.a) and estimating the canonical 2x2 TWFE (equation 2) using 1996 as the baseline for each year between 2000 and 2019. By comparing each year to the baseline separately I avoid the potential bias caused by negative weights in TWFE with staggered adoption (Goodman-Bacon, 2021). Figure 3.a shows that the results are non-significant during the first years, probably due to the few municipalities installing wind power and low installed capacity, but become significantly positive and increasing from 2006 onwards. Wind capacity increases municipal disposable income by around 5% and increasing over time. This effect, however, comprises (i) the actual effect of a given installed capacity, (ii) the effect

of increasing the "dosage" (capacity), and (iii) any potential dynamic effect over time. To isolate these effects, I build a "static dataset" (see Data section and Appendix Figure A.2) including only municipalities that have a constant capacity during the stagnation period (2010-2018).


a. Average effect per year (full dataset)
10
5
0
-5
% Δ income
2000
2005
2010
2015
2019
Year


b. Dynamic effects (static dataset)
All
Rural
15
10
5
0
% Δ income
2010
2014
2018
Year

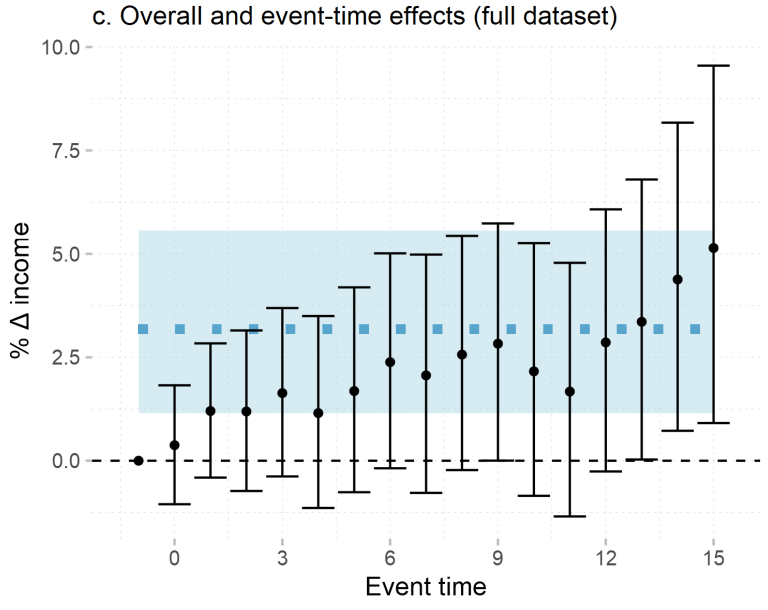

c. Overall and event-time effects (full dataset)
10.0
7.5
5.0
2.5
0.0
% Δ income
0
3
6
9
12
15
Event time

Figure 3. Effect of wind power capacity on municipal disposable income (percentage change) according to different identification strategies. (a) Total effect per year (full dataset, series of 2x2 TWFE estimates for each year according to Equation 2). (b) Dynamic treatment effects (static subset for rural and all municipalities according to Equation 3). (c) Event-time effects (years since the capacity is installed, full dataset, CS estimator). Error bars show 95% confidence intervals.

5.1.2 Static dataset

Using the static subset I estimate another TWFE (Equation 2) using again 1996 as baseline and the whole stagnation period (2010-2018) as the treatment period. I express the treatment in three different ways: (i) as a binary variable (whether the municipality has capacity or not), (ii) as a continuous variable (natural log of capacity+1 to keep control municipalities with 0 capacity) and (iii) discretisized (high or low capacity cut at the median of the municipalities with some capacity). I explore potential heterogeneity by estimating these coefficients for all municipalities (except high density) and for the most rural ones only (see Figure 1.b). The results presented in Table 1 suggest that wind capacity has a positive effect on disposable income and is generally stronger for rural municipalities. The coefficients in the tables are expressed in log-points and I use the conventional simplification of interpreting them directly as parts-per-unit changes.

The binary treatment (i) provides a summary of the overall effect, but misses the fact that different municipalities have different capacities. The results are positive and significant (5.5%) and stronger for rural municipalities (7.4%).

The continuous treatment (ii) should reveal the average causal response, i.e. the increase in disposable income per percentage increase in installed capacity. As pointed out by Callaway (Callaway et al., 2021), however, this average causal response may be prone to selection bias if there are heterogeneous treatment effects and dosage (the amount of capacity) is dependent on potential outcomes. In this case, it is likely that locations with higher wind have higher capacity, so self-selection may cause an overestimation of the average causal response. Whether this estimate is biased depends on the mechanism through which wind capacity affects income. According to Copena and Simon (Copena & Simón, 2018), who studied the income on wind power to land owners in Galicia, wind income is unrelated to the farm's efficiency, in which case we can interpret our coefficients as average causal responses, i.e. wind capacity by 1% increases municipal disposable income by 0.5% (0.7% in rural municipalities). If there are mechanims that depend on capacity factor, however, this coefficient could be overestimated.

The discrete treatment (iii) splitting municipalities that have capacity depending on whether they are below ar above the average avoids the potential selection bias of the continuous treatment because each level of capacity is compared with the control (no capacity) rather than being continuously compared

with other levels of dosage (capacity). The point estimates are all positive but too uncertain for the high category. These results are noisy due to the assumption that each farm's capacity is equally distributed across the municipalities where it is located (see Section 2 for details) due to the lack of higher spatial resolution data, and due to the high heterogeneity of the high capacity category (See Appendix Figure A.2).

	All municipalities			Rural only		
	Binary	Continuous	Discrete	Binary	Continuous	Discrete
Has capacity	0.055***			0.074**		
	(0.020)			(0.029)		
ln(capacity+1)		0.005**			0.007**	
		(0.002)			(0.003)	
High			0.029			0.046
			(0.026)			(0.038)
Low			0.082***			0.111***
			(0.023)			(0.032)
Num.Obs.	2870	2870	2870	1680	1680	1680
R2 Adj.	0.953	0.953	0.953	0.932	0.932	0.933

* p < 0.1, ** p < 0.05, *** p < 0.01
Municipality and Year fixed effects. Municipality-clustered standard errors.

Table 5. Effect of wind capacity on income with different treatment definitions according to Equation 2 with the subset of data shown in Appendix Figure A.2.

5.1.3 Income decomposition

Using the same static dataset I explore the mechanisms trough which wind capacity increases municipal disposable income. The available data allows us to differentiate between capital and labor income (labelled in Table 2 as profits and wages) and a third category that includes all government taxes and transfers and property rents. Whereas profits and wages are always a debit, the third category (labelled in Table 2 simply as transfers) is a balance of debits and credits. As this balance includes both positive and negative numbers, we indicate the absolute value in euros per capita, whereas profits and wages are logged, so the coefficient represents parts-per-unit changes.

Copena et al. (2019) quantify the direct income provided by wind farms to local governments in Galicia. They find that wind capacity provided 17.8 M€ to local governments in 2017 via taxes (land tax 37.7%, a specific wind tax 35.4% and a municipal economic activities tax 19.6%) and participation in capacity ownership (7%). This may explain why net transfers are 500€ per capita higher for municipalities with wind power. In addition to this direct income to local governments, the results presented in Table 1 suggest that wind capacity also increases local profits (5.8%) and local wages (8.1%). The results for

rural municipalities are more uncertain but the point estimates are all positive and higher for profits and wages, consistent with the results in Table 1.

	All municipalities			Rural only		
	ln(Profits)	ln(Wages)	Transfers	ln(Profits)	ln(Wages)	Transfers
Has capacity	0.058**	0.081***	503.871***	0.075*	0.102***	75.823
	(0.028)	(0.022)	(176.511)	(0.039)	(0.033)	(172.361)
Num.Obs.	2870	2870	2870	1680	1680	1680
R2 Adj.	0.878	0.983	0.879	0.869	0.968	0.862

* p < 0.1, ** p < 0.05, *** p < 0.01
Municipality and Year fixed effects. Municipality-clustered standard errors.

Table 6. Effect of wind capacity on the components of income according to Equation 2 with the subset of data shown in Appendix Figure A.2. Profits and wages are logged, transfers are in levels (€/capita) because it is a balance of positive and negative values.

5.1.4 Dynamic effects

The static subset allows us to study potential dynamic treatment effects (equation 3). Figure 3.b shows the percentage increase in municipal disposable income for each year during the study period. Because the static subset only includes municipalities that have a constant capacity during this period, the increase in the effect is driven by its own dynamics rather than by increasing capacity as in panel a. The results are consistent with Tables 1 and 2 in showing significantly positive results that are stronger for rural municipalities. The effect of wind power on municipal disposable income is positive and increases over time at the same rate for all municipalities.

5.1.5 Event-time effects

Finally, we can exploit the full variation of the dataset by using a hetergeneity-robust difference-in-difference estimator, such as Callaway and Sant'Anna (Callaway & Sant'Anna, 2021) (CS hereafter). This estimator provides cohort- and year-specific coefficients that can be aggregated in multiple ways avoiding bias inherent to the TWFE estimator with staggered adoption. Whereas this estimator allows us to analyze the full dataset taking into account the staggered nature of the capacity expansion, the fact that a few municipalities install capacity each year makes the cohort- and year-specific estimates very noisy. Figure 3.c shows the event-time effects (i.e. year since the capacity is installed) for 15 years since the installation using the never treated group (i.e. municipalities without capacity) as control group (see Figure 1 for pre-treatment coeffcients). These estimates cannot be interpreted as dynamic treatment effects because capacity may also be increaseing during this time. These results are uncertain but the

general positive coefficient and increasing trend are consistent with the the total effects in panel a and the dynamic treatment effects in panel b. The overall effect that takes advantage of the whole dataset shows a positive and significant result of 3.3%. This is consistent with all previous results: it is lower than the estimate obtained in Table 1 (5.5%) because the CS considers all data, including the first years when the effect was very low (Figure 3.a), whereas Table 1 is estimated with te static dataset during the stagnation period when the capacity was generally high and the sector mature.

5.2 Impacts on other sectors

This section presents weaker evidence than the the previous for the following reasons:

- I cannot test the parallel trends assumption due to the lack of pre-treatment data.
- Given the lack of longitudinal data for the agriculture and livestock sectors, the TWFE are the simplest 2x2 version (i.e. two groups, treated and untreated, and two periods, before and after treatment).
- The outcome variables are more limited and heterogeneous. While all municipalities have disposable income that evolves smoothly over time, the presence of specific types of crops, livestock or hospitality services is more specific for certain regions and may be more prone to non-parallel trends.

These limitations imply that, on the one hand, statistically significant results may not be causal due to the potential violation of the the identifying assumption. On the other hand, actual effects may not be identified due to the high uncertainty of the data. For these reasons, these results should be taken with caution and compared to further evidence.

5.2.1 Agriculture

The outcome variable for the agricultural sector is the logged area dedicated to each crop in each municipality. These data are only available for two years: 1999 and 2009. Because the first capacity data is for the year 2000, I exclude all municipalities that had capacity in this year and use it as baseline. Table 6 shows that although the point estimates are negative, the heteroskedasticiy-robust standard errors are high, so we cannot conclude that wind capacity has a significant impact on any of the specific crops or on the agricultural sector as a whole. Previous literature found positive impacts of wind capacity on corn yields (Kaffine, 2019).

	Arable	Fruits	Vineyard	Other	Agriculture
Wind	−0.056	−0.062	−0.003	−0.019	−0.063
	(0.295)	(0.301)	(0.390)	(0.121)	(0.256)
Num.Obs.	347	347	347	347	347
R2 Adj.	0.002	0.035	0.085	−0.008	−0.008

* p < 0.1, ** p < 0.05, *** p < 0.01
Group and Year fixed effects. Heteroskedasticity-robust standard errors.

Table 7. Effect of wind capacity on the agricultural sector according to Equation 4.

5.2.2 Livestock

The lifestock data has the same characteristics as the agricultural sector, but the outcome variable is measured in livestock units. Because the dependent is logged, the coefficients can also be interpreted in percentage terms regardless of the unit. The point estimates are mixed, but the uncertainty is generally high for individual types of livestock and for the sector as a whole, so I cannot identify any effect of wind power on the livestock sector.

The only exception is the equine livestock, that shows a positive and statistically significant coefficient. Given the limitations mentioned above it is hard to conclude that there is a positive causal effect despite the statistically significant coefficient. One hypothesis is that wind turbines are compatible with equine livestock and, because the land rents complement the farming income, they make the activity profitable. Another option, however, is that this is a spurious result due to the violation of the parallel trends assumption that I have not been able to test for this specific variable or just a spurious result. I have not found any literature on the impact of wind capacity on the livestock sector.

	Bovine	Ovine	Caprine	Porcine	Equine	Avian	Rabbits	Livestock
Wind	0.135	−0.117	0.267	−0.002	0.396**	−0.099	−0.106	0.049
	(0.316)	(0.201)	(0.240)	(0.315)	(0.196)	(0.362)	(0.211)	(0.223)
Num.Obs.	356	356	356	356	356	356	356	356
R2 Adj.	0.101	0.064	0.007	0.061	0.238	−0.004	0.126	0.071

* p < 0.1, ** p < 0.05, *** p < 0.01
Group and Year fixed effects. Heteroskedasticity-robust standard errors.

Table 8. Effect of wind capacity on the livestock sector according to Equation 4.

5.2.3 Hospitality

Data for the hospitality sector are available since 2008, so I take the same approach as in sections 5.1.2 and 5.1.3 of testing the effect on a clean dataset of municipalities that have a constant wind capacity

between 2010-2018 (the "stagnation subset" shown in Appendix Figure A.2). Uncertainty is lower than for the primary sector due to higher data availability and point estimates are positive but none of the effects is statistically significant. Previous literature has found a negative relationship between wind power and tourism demand (Broekel & Alfken, 2015).

	Accommodation	Food	Hospitality
Wind	0.073	0.047	0.029
	(0.099)	(0.045)	(0.042)
Num.Obs.	1671	1671	1671
R2 Adj.	0.866	0.944	0.951

* p < 0.1, ** p < 0.05, *** p < 0.01
Municipality and Year fixed effects. Municipality-clustered standard errors.

Table 5. Effect of wind capacity on the hospitality sector according to Equation 2 with the subset of data shown in Appendix Figure A.1.

5.3 Implications for Galicia's economy

I now use the estimates of the effect of wind capacity on municipal disposable income to evaluate the total effects of wind power in Galicia. Figure 4.a. shows the different treatment effects estimated with different assumptions: (i) an assumed constant treatment effect estimated with the static dataset (Table 5), (ii) the total effect for each year shown in Figure 3.a, (iii) an assumed constant overall effect according to the CS estimator and (iv) a linear approximation of the CS event-time effects. With each of these assumptions, I calculate the counterfactual disposable income municipalities with wind capacity would have had in the abstence of wind capacity. Then I calculate the difference between the actual disposable income and the counterfactual according to each of these assumptions to show the total income gains of wind power in Galicia.

Figure 4.b shows the total effect of wind power in Galicia in terms of euros per capita and million euros (assuming 2019's population). Wind power increased disposable income in Galicia by between 75 and 175€ per capita, or between 200 and 500 M€ in 2019. Because this increase in disposable income is concentrated in rural municipalities, that are usually poorer, wind power also decreased inequality (gini index across municipalities is 0.098 in 2019 and it would have been 0.1 without wind).

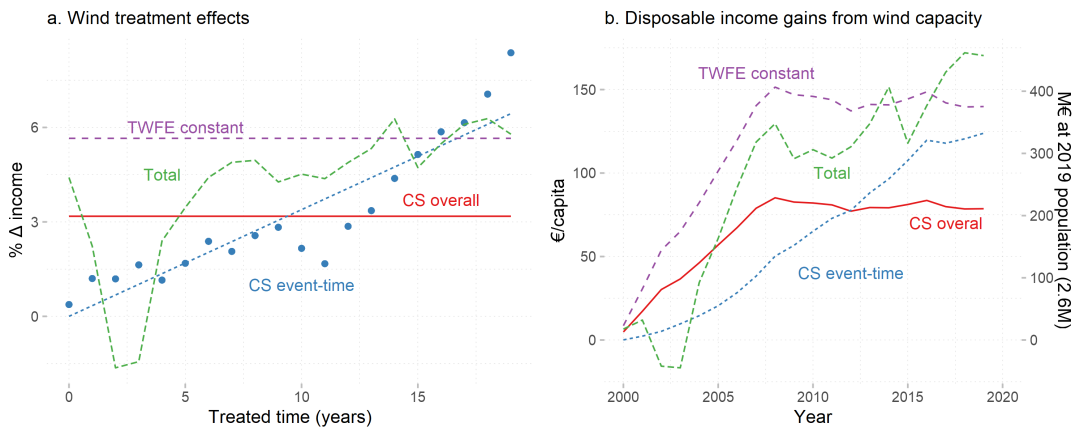


Figure 4. Effects of wind capacities on Galicias economy. (a) Different estimations of the wind treatment effects. (b) Disposable income gains in € per capita and million euros with the 2019 population (2.6 million).

5.4 Limitations and discussion

One of the main limitations of this research is both the quantity and quality of data. In terms of quantity, having data for a larger area would increase statistical power and allow wider heterogeity analysis. In terms of quality, capacity data are coarse both at spatial (wind farm level, that may comprise several municipalities) and temporal level (year of first installation). Higher spatial resolution would allow testing spatial effects and having higher precision. More temporal resolution would allow differentiating effects during construction and operation.

I have not been able to evaluate the effect of wind power on population due to the violation of the parallel trends assumption. Rambachan and Roth (Rambachan & Roth, 2023) argue that excluding hypothesis due to PTA violations may bias the literature and they provide methods of partial identification to tackle these kind of violations. However, data quality is not enough to perform this type of analysis.

I have only estimated the benefits of wind power capacity in terms of local disposable income. However, wind capacity may also have positive spill-over effects on neighbouring municipalities and create a related industrial base. Varela and Sánchez (Varela-Vázquez & Sánchez-Carreira, 2015) find that wind industry accounted for up to 1.5% of Galicia's GDP. Deployment of wind capacity has probably played a significant role in pulling wind-related manufacturing capacity.

Finally, local benefits should be weighted against local externalities. There is a vast body of literature estimating the environmental (Mattmann et al., 2016; Tabassum et al., 2014; Zerrahn, 2017) and economic (Brinkley & Leach, 2019) externalities of wind power. There is a growing body of literature on the role of citizens' participation on renewable energy deployment and acceptance (Elkjær & Horst, 2023;

Maitre et al., 2023; Schönauer & Glanz, 2023). Other measures, such as internalization of externalities for all technologies and public participation in renewable energy capital may contribute to increasing the local economic benefits of the energy transition (Braun, 2020; Standal et al., 2023; Steffen et al., 2022).

6 Conclusions

Wind power is fundamental to advance the energy transition but its diffusion is jeopardized by local opposition. The main argument is that wind capacity causes negative impacts to traditional economic sectors without contributing to the local economy. I test the causal effects of wind capacity on local disposable income and find that wind capacity increases local disposable income by 5.5%, and this effect is stronger for rural villages (7.4%). These effects are robust across different specifications and definitions of the outcome variable, and identification is confirmed by pre-trend tests, placebo tests, and pre-event coefficients according to Callaway & Sant'Anna (CS) heterogeneity-robust estimator for staggered adoption. The effects of wind capacity on income happen via the three components of disposable income: profits, wages and net transfers, and increase slightly over time.

I also test the effect of wind capacity on the agricultural, livestock and hospitality sector. I find no significant effects, but this evidence is weak because I cannot test the parallel trends assumption and the models have low statistical power due to the lack of data. There is a significant effect of wind capacity on equine livestock, which could suggest the complementarity between both activities, but could also be a spurious result given these data limitations.

I cannot estimate the causal effect of wind capacity on population due to the violation of the parallel trends assumption, but descriptive statistics suggest that wind capacity has not been able to reverse the negative population trend in rural municipalities.

Because Galicia represents a worst-case scenario for local wind benefits due to lack of local participation in the development process and foreign ownership of capital, these estimates may be interpreted as a lower bound for other regions.

Data and code availability

Data and code will be made publicly available upon pulication.

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Competing interests

None

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Appendix

A.1. Summary statistics

a. Income

Year	Min	Mean	SD	Max
1996	2124	5630.555	1111.973	11280
2000	4648	7618.910	1244.988	13348
2001	4890	8211.761	1311.234	13979
2002	5183	8542.652	1388.506	14826
2003	5582	9210.184	1454.136	15816
2004	5905	9832.735	1497.778	16085
2005	6219	10292.377	1589.479	17086
2006	6593	10833.313	1756.518	18104
2007	7179	11546.526	1962.311	19328
2008	6456	12059.416	2127.527	20374
2009	5925	11564.277	2071.919	19413
2010	5880	11609.306	2045.175	19459
2011	5814	11592.490	2093.931	23226
2012	5719	11130.923	1849.983	19715
2013	5495	11241.216	1900.300	20907
2014	5230	11185.887	1928.922	22590
2015	5545	11455.516	1964.570	21671
2016	5532	11807.219	2034.377	22082
2017	5672	12083.297	2041.780	22708
2018	5382	12574.342	2090.390	22756
2019	5784	13171.742	2401.369	26004

b. Population

Year	Min	Mean	SD	Max
1996	297.9277	8657.451	23405.87	280118.3
2000	270.0330	8649.014	23424.72	279179.6
2001	254.9085	8657.016	23442.37	280673.8
2002	257.5000	8656.545	23586.27	281751.0
2003	259.0000	8680.611	23668.05	283275.5
2004	259.5000	8705.611	23720.41	284164.0
2005	254.0000	8735.635	23809.47	285082.0
2006	253.5000	8768.573	23921.55	286181.5
2007	259.5000	8808.435	24044.16	287348.0
2008	259.0000	8851.421	24181.23	288706.0
2009	256.5000	8880.182	24275.96	289562.5
2010	265.5000	8889.644	24312.23	289589.5
2011	273.5000	8888.877	24365.23	289902.5
2012	274.0000	8870.687	24377.54	289664.5
2013	274.5000	8831.052	24309.80	288557.5
2014	274.0000	8787.250	24267.31	288159.5
2015	277.0000	8743.969	24255.78	288026.5
2016	281.5000	8704.882	24247.93	287849.0
2017	286.5000	8677.855	24238.80	286973.5
2018	292.5000	8662.653	24248.75	286192.5
2019	295.5000	8662.068	24345.75	287316.5

c. Cumulative capacity

Year	Min	Mean	SD	Max
1996	0	0.0000	0.000	0.0
2000	0	495.9032	2930.213	38200.0
2001	0	1736.4839	7311.359	66210.0
2002	0	2823.3226	8620.658	66210.0
2003	0	3722.9839	11320.681	114720.0
2004	0	4744.9839	13417.071	125595.0
2005	0	6179.2419	17218.020	160048.3
2006	0	6993.4355	19694.191	209648.3
2007	0	8179.9839	21368.181	209648.3
2008	0	8485.4677	21637.571	209648.3
2009	0	8867.5645	22194.110	217548.3
2010	0	8970.7903	22531.876	217548.3
2011	0	8979.0161	22536.325	217548.3
2012	0	9060.3065	22650.168	217548.3
2013	0	9087.8871	22649.192	217548.3
2014	0	9133.0484	22730.940	217548.3
2015	0	9133.0484	22730.940	217548.3
2016	0	9133.0484	22730.940	217548.3
2017	0	9226.9516	22755.647	217548.3
2018	0	9446.3065	22992.924	217548.3
2019	0	10785.7903	25876.040	217548.3

Table A.1. Summary statistics for three main variables: disposable income (thousand euros), porpulation (persons) and cumulative wind installed capacity (MW)

A.2. Population results

This section shows the results of the main specification (Equation 2) with binary treatment for population. Because the parallel trends assumption does not hold for population (i.e. treatment units have a consistently different trend than control units, Figure 2), these results do not show the effect of wind capacity of population, but only reflect the different trends between both groups. Although we cannot extract causal conclusions from these results, the coefficient being negative reveals that at least wind capacity has not been able to reverse the negative population trend (the population pre-trends in Table A.3 revel a non-significant negative trend). More research is needed to evaluate the effect of wind capacity on population dynamics.

Effect of wind capacity on population

	All	Rural
Wind	−0.102***	−0.047**
	(0.022)	(0.020)
Num.Obs.	2870	1680
R2 Adj.	0.995	0.994

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.4. Treatment effects of wind capacity on population according to Equation 2.

A.3. Additional figures

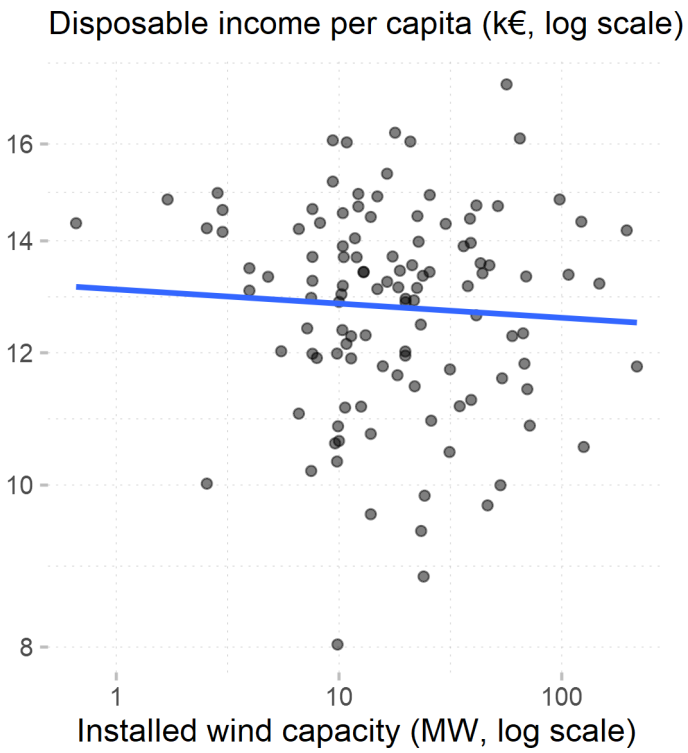


Figure A.1. Negative correlation between wind capacity (MW) and disposable income (thousand euros) by municipality in 2019.

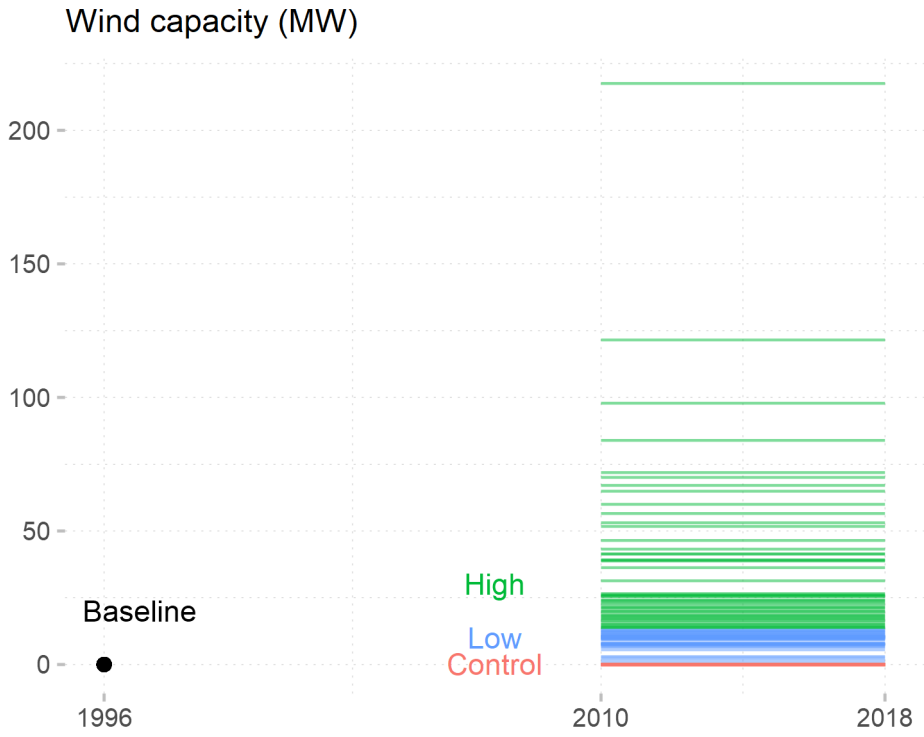


Figure A.2. Wind installed capacity in the stagnation period for municipalities with constant capacity during this period and classification for the binary treatment (high and low) cut at the median.