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Win-Wind Situation?

The Local Labor Market and Wind Power Investments in Sweden

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Abstract: This thesis examines the local economic impacts of wind power deployment in Sweden, focusing on the net labor market effects. Using a difference-in-differences model and a local projections model, the study quantifies the impact of wind power investments on unemployment at the municipal level. The results suggest that wind power decreases unemployment by approximately 0.7-2.7 persons per megawatt in operation. For an average wind power project, this corresponds to a persistent reduction in unemployment levels by 0.33%-1.2%. In contrast to previous literature, the effect is lower rather than higher during the construction period compared to the post-installation period. Furthermore, the heterogeneity analysis shows that no socioeconomic group is adversely affected and that men and those with lower levels of education benefit the most. Moreover, there are small beneficial spatial spillovers from wind turbines located within ten kilometers outside of a municipality's border but no effect further away. These findings highlight the potential of wind power investments as a tool for local economic development and contribute to the ongoing discussion on the transition to a more sustainable and resilient energy system.

Keywords: Renewable energy, labor market, difference-in-differences, local projections

JEL: C230, J210, Q430, Q480

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1 Introduction

Climate change is one of the biggest threats modern humans have ever faced. The accumulation of greenhouse gases in the atmosphere, primarily from the burning of fossil fuels, is causing the Earth's temperature to rise at an alarming rate, leading to widespread environmental and social damages (IPCC, 2022). Investments in renewable energy sources, such as wind energy, have emerged as a promising path forward by offering a clean, import-independent, and renewable source of electricity. As such, wind power deployment has become a critical tool in the transition toward a more sustainable and resilient energy system. For instance, the International Energy Agency state that global wind power generation should more than quadruple from 2021 to 2030 to align with the Paris Agreement (IEA, 2022).

While benefits from wind energy investments – in the form of energy security and climate change mitigation – accrue mostly on national and global levels, the local level experience both positive and negative impacts, many of which are economic. On the positive side, wind energy provides gross additions in local job opportunities, local tax revenues, and income to landowners and investors. On the negative side, workers in other sectors could be displaced, and negative externalities can arise – including visual disamenities and sound pollution – which could reduce nearby property values (Dröes and Koster, 2021). These local economic effects are imperative in influencing the local attitudes toward new wind power projects (Slattery, Johnson, et al., 2012; Mulvaney, Woodson, and Prokopy, 2013; Caporale and De Lucia, 2015) and, through this, local policies, and regulatory permissions. Thus, local economic benefits can be a political prerequisite and a moral condition[1] for reaching energy security and climate abatement targets. Consequently, the question arises to what extent the local communities are positively or negatively affected by new wind power investments.

To this end, this thesis analyzes the local economic impacts of wind power deployment, with the hope of better equipping policymakers and individuals to make informed decisions about the just and sustainable energy transition. Specifically, it aims to quantify the net labor market effects of wind power investments at the municipal level in Sweden. This is accomplished using a difference-in-differences (DiD) model with considerations taken to the growing literature on the validity of DiD models under heterogeneous treatment effects. Additionally, a local projections (LP-DiD) model is used.

Focusing on the labor market to assess the economic potential of wind energy is beneficial for three main reasons. First, local job opportunities are a highly politicized subject with a significant impact on affected individuals. Second, the labor market is naturally correlated with other indicators of economic development, such as private income, public expenditure, and GDP. Lastly, empirical information on the labor market is rich, enabling a detailed dynamic analysis of economic development and wind energy investments. Sweden is chosen because of its high penetration of wind capacity, with the largest capacity per capita in the EU and top four in absolute terms (Eurostat, 2023a; Eurostat, 2023e).

Little research has been conducted in this area. Besides guidance from a few related papers, decision-makers are left with estimates on the gross added jobs from exemplary

[1]Needless to say, the moral aspects of the green energy transition cover more groups than local communities, particularly related to the plethora of damages caused by climate change.

or specific projects. These gross estimates disregard the indirect effects of wind power investments, such as multiplier effects in the labor market and job losses in other sectors. With an ex-post empirical analysis, the total aggregate outcomes – the net effects – can be estimated. Although such methods have previously been used on similar research questions, this thesis fills a gap in the current literature. Notably, there is no previous research on a northern European country in this context. Moreover, previous literature has used various econometric models. Therefore, by applying two empirical methodologies for Sweden, I can assess the external validity of the previous literature and examine the extent to which the results are sensitive to the choice of empirical strategy.

The empirical analysis covers the period 2003-2022, the period when virtually all of the uptake of wind power has taken place. The data on wind projects is collected from the Swedish Energy Agency and County Administrative Boards of Sweden (2023). The labor market effects are proxied by the changes in monthly unemployment at the municipal level, both aggregate and stratified by a range of socioeconomic categories. The data on unemployment comes from the Swedish Public Employment Service (2023).

The results show that wind energy investments in Sweden have decreased local unemployment by an average of 0.74-2.66 persons per Megawatt (MW)[2] for multiple years after the installation date. For a median-sized wind power project in a municipality with a median-sized unemployment level, this corresponds to a reduction in unemployment levels by 3.7-13.3 persons, or 0.33%-1.2%. Contrary to previous literature, which finds that the impact peaks during construction, this paper finds a growing effect during construction that then plateaus after the installation date. The results also show that the effects are dynamic over time. All models, sample restrictions, and other robustness tests tell similar stories, although with slightly different magnitudes. Among the analyzed socioeconomic categories (sex, education level, age groups, and country of origin), men and those with lower levels of education gain comparatively more on average from local wind power investments. Moreover, there is no indication of negative effects on the nearby municipalities. On the contrary, a spatial analysis shows that unemployment is slightly reduced in municipalities that receive a wind turbine within ten kilometers outside its border. The spillover effects do not appear for larger distances.

The paper is organized as follows. Following this introduction, section 2 provides a background on the Swedish electricity market, and wind power development in Sweden. Section 3 briefly describes the Swedish labor market and the channels through which wind power development can affect the labor market. Then, section 4 outlines previous related literature. The empirical strategy is explained in section 5, and the data is described in section 6. Section 7 provides the empirical results. In section 8, the results are discussed, and section 9 concludes.

[2]For reference, a utility-scale onshore wind turbine typically holds a capacity of 2-4 MW.

2 Background

To enhance the understanding of the determinants of wind power deployment over space and time, this section describes the historical and current situation for wind power in Sweden with regard to policy development, the permission process, the structure of the power market, and public attitudes.

2.1 A brief overview of the Swedish power market

The Swedish wholesale power market was deregulated in 1996 and has since been increasingly integrated with the European energy market (Ei, 2023b). Electricity is predominantly traded on the day-ahead auctions with marginal pricing (pay-as-bid) (EPEX SPOT, 2023). Since renewable energy sources have minuscule marginal costs, wind power deployment decreases wholesale electricity prices (Cevik and Ninomiya, 2022; Antweiler and Muesgens, 2021), although large-scale integration could increase system costs and hence tariffs (Eicke, Eicke, and Hafner, 2022). In 2011, Sweden was split into four bidding zones in order to provide better price signals and make flows more efficient (Ei, 2023a). In short, a bidding zone is the geographical market area in which the market clearing price is determined.[1] Prices normally increase from north to south since there is typically an oversupply in the north and undersupply in the south (Ei, 2023b). The bidding zone reform created a relative increase in wind power investments in the under-supplied southern bidding zones (Lundin, 2022).

2.2 Wind power development in Sweden

Sweden has a long-standing tradition of utilizing renewable energy sources thanks to the numerous rivers in its northern parts. This hydropower, together with the expansion of nuclear reactors in the south, has enabled a low-carbon electricity mix with low exposure to global fossil fuel price volatility since the 1980s. Wind turbines started to be deployed in small numbers at the beginning of the 1990s. Since then, it has grown from 0.3% of electricity production in the year 2000 to 2.4% in 2010 and 17.1% in 2020 (Eurostat, 2023c). This development has been driven by, inter alia, a global reduction in manufacturing cost due to economies of scale (IRENA, 2022) and a competitive advantage in geographic conditions, including long coastal regions and land availability. Due to technological improvements, there has been a trend towards more turbines per wind park and bigger turbines overall (Swedish Energy Agency and County Administrative Boards of Sweden, 2023). Relative to other EU member states, as of 2021, Sweden has the largest onshore wind generation capacity per capita, the fourth largest in absolute terms (following Germany, Spain, and France), and the fourth largest relative to GDP (following Greece, Portugal, and Spain) (Eurostat, 2023a; Eurostat, 2023b; Eurostat, 2023e).

Despite a common energy policy framework, wind power development has not been uniform across Sweden, both within regions and across regions. Figure 2.1 illustrates

[1]The bidding zone delineations are shown in figure B.1

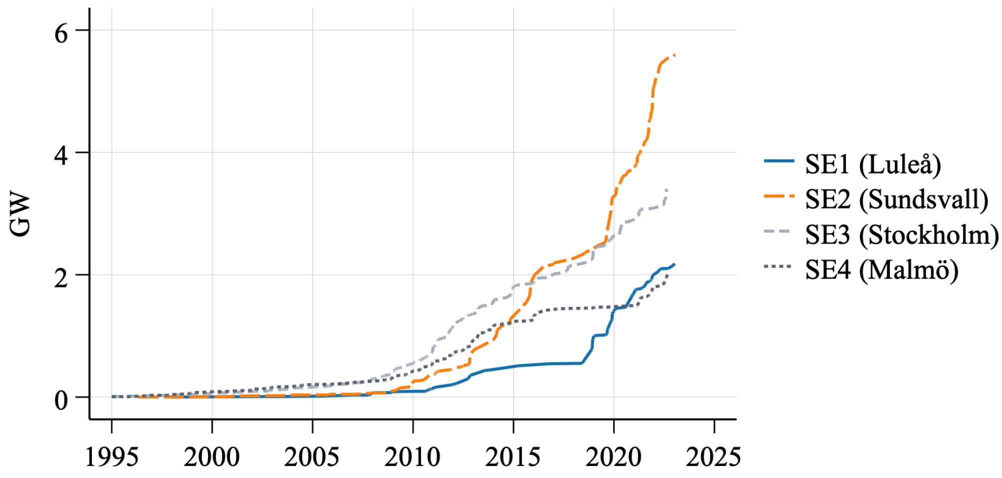


Figure 2.1: Wind power capacity per bidding zone

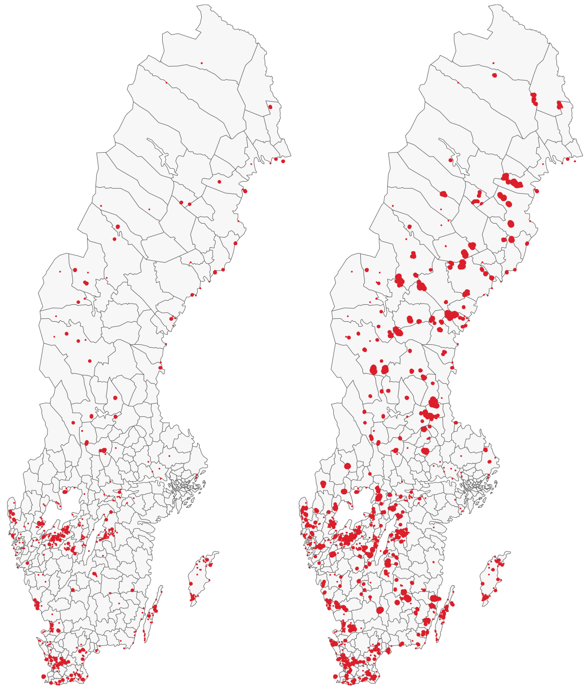
the number of turbines and cumulative capacity for the bidding zones.[2] Initially, mostly solitary wind turbines were deployed and were concentrated in southern Sweden (gray lines; SE3 and SE4). The two northern bidding zones (colored; SE1 and SE2) started to catch up in the 2010s and exceeded the capacity of the southern bidding zones in 2019 (Swedish Energy Agency and County Administrative Boards of Sweden, 2023). This is mostly due to a few large projects in northern Sweden exceeding hundreds of megawatts in total capacity. Notably, the Markbygden wind park in Piteå currently has an installed capacity of 1.2 GW (Swedish Energy Agency and County Administrative Boards of Sweden, 2023) and could reach up to 4 GW in 2024 – making it the largest onshore wind power park in Europe (Svevind, 2021). The inter-regional differences could be due to several factors, including wind resources, land availability, land ownership structure, and electricity prices (Lundin, 2022).

Intra-regional differences are also present, with some municipalities having no wind turbines in counties with otherwise high wind power development.[3] Figure 2.2 illustrates how wind turbines are scattered unevenly across municipalities. The municipal-level differences could be due to geography, demography, and economy, even relative to neighboring municipalities. Two research papers have analyzed the determinants of the location of wind power investment across municipalities in Sweden. Uncontested significant determinants include population density, wind speed, land area, previously installed capacity ("experience"), and if the municipality contains a national interest area or protected area (Ek et al., 2013; Lauf et al., 2020). Attitudes – proxied by whether the Swedish green party is part of the local government (Lauf et al., 2020) and an environmental index (Ek et al., 2013) – is not found to affect wind power investment location.[4] The binary population trend is also insignificant (Ek et al., 2013). Most importantly, unemployment is *not* found to determine wind power investments in Swedish municipalities (Ek et al., 2013; Lauf et al., 2020). Using a simple regression on added wind capacity with lagged

[2] 1 Gigawatt (GW) = 1000 Megawatt (MW) = 1 000 000 kilowatt (kW). Note that numbers expressed in watts always refer to the capacity of a generator, as opposed to energy (measured in, e.g., watt-hours, abbreviated Wh.)

[3] Sweden has 290 municipalities and 21 counties.

[4] More accurate proxies for attitudes, such as opinion polls, are not available on the month-municipality level.



(a) 2010 (b) 2022

Figure 2.2: Map over Swedish wind turbines
The symbols represent all wind turbines up until 2010 and 2022, respectively. The size of the circles represents the capacity of each turbine. Borders represent municipalities. Map created by the author with data from the Swedish Energy Agency and County Administrative Boards of Sweden (2023).

unemployment per capita, table 2.1 show that the same conclusions can be drawn from this paper's data set.

Wind turbines typically have lifespans of around 20-30 years (IRENA, 2012; Swedish Energy Agency, 2022c). Thus, because of the late development of wind power in Sweden – compared to, e.g., Denmark – few turbines have been decommissioned. Of the 13 200 MW installed onshore wind capacity ever installed, only 70 MW have been decommissioned (Swedish Energy Agency, 2022b). Moreover, this paper disregards offshore wind power since Sweden only has five active offshore wind parks with a total capacity of 220 MW – despite access to lakes, the Baltic Sea, Skagerrak, and Kattegat (Swedish Energy Agency and County Administrative Boards of Sweden, 2023). The last project, *Vänern*, was finished in 2013. In contrast, since 2013, the offshore wind industry has expanded vastly elsewhere in the EU; with a 650% increase in offshore wind capacity, compared to a 75% increase in onshore capacity (Eurostat, 2023a). Considering the different labor requirements of offshore wind, the dominant position of onshore technologies facilitates identification for Sweden with less heterogeneous treatment effects.

2.2.1 Permission process

Wind power developments are restricted in several ways besides geographical preconditions, which could further explain differences in wind power deployment across municipalities. The following physical, regulatory, and political requirements apply to utility-scale onshore wind turbine projects.

First, the developer must be able to purchase or lease necessary land, including ac-

Table 2.1: Added wind power capacity from past unemployment per capita

	Added capacity per capita$_{i,t}$		
$h =$	36	48	60
$unemp_{i,t-h}$ ($\times 10^3$)	-0.109	0.046	-0.037
	(0.577)	(0.838)	(0.902)
$popdensity_{i,t-h-36}$ ($\times 10^{-6}$)	-0.022	-0.028	-0.026
	(0.082)	(0.051)	(0.148)
Observations	58717	55261	51805
R^2	0.005	0.005	0.005
Municipalities	288	288	288

p-values in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The effect from past unemployment per capita (3, 4, and 5 years) on added capacity. Absolute unemployment and added capacity are normalized by population in December 2002. Time- and municipality-fixed effects included. Wind capacity data retrieved from Swedish Energy Agency and County Administrative Boards of Sweden (2023).

cess to (new) roads (Swedish Energy Agency, 2022a). Access to the electricity grid is also necessary and is subject to a permit process, administrative fees, and construction charges. The processing time and charges depend on various factors, notably the voltage requirements and distance to the nearest suitable grid access point (Svenska Kraftnät, 2021). For smaller projects, the distribution system operators (DSOs)[5] could have slightly different lead times and fees associated with grid connection, thus creating some regional differences.

Second, national regulation requires compliance with, inter alia, environmental protection, cultural heritage protection, biodiversity protection, military interests, aviation security, and human health (Swedish Energy Agency, 2020a). The requirements differ depending on the size of the project. Except for areas specifically protected by, for example, military interest, the regulation is largely the same across Sweden. In the early planning stage, wind developers can prospect the likelihood of obtaining necessary permits with guidance from municipal layout plans (Boverket, 2022) and the list of areas of national interest for wind energy (NIWAs) defined by the Swedish Energy Agency (2022a), both of which indicate areas with particular suitability for wind power development.

Lastly, local political support is beneficial for small and medium-sized projects, and de facto necessary for large projects. Local governments can facilitate or hinder new wind power projects in their municipality. For instance, although not legally binding, the layout plan can be crafted to send positive or negative signals to investors (Boverket, 2016). Moreover, large projects (and certain smaller projects) must also obtain an endorsement from the municipality (Swedish Energy Agency, 2020b). This is sometimes referred to as the "municipal veto". The municipal executive board is allowed to decide freely and does

[5]The approximately 170 Swedish DSOs operate the electricity distribution under monopoly within their geographical area, typically encompassing one or two municipalities. They have some leeway in, e.g., tariff decisions, but operate under the limits deemed reasonable by the national regulatory authority, Ei.

not have to explicitly motivate the decision (Swedish Energy Agency, 2022d). Moreover, an initial endorsement can be retracted in later stages.

If permits are successfully obtained, the construction phase then lasts for one to three years, depending on the size of the project (Swedish Energy Agency, 2022c). The lead time from early planning until the permission process is concluded takes several years and varies across projects.

To understand how political support affects wind power development in a given municipality, one should also analyze the public perception toward wind power. The overall support for wind power in Sweden is large, with slightly higher approval rates among women, urban residents, highly educated people, and people who identify as left-wing or vote for the Left Party, the Green Party, the Swedish Social Democratic Party, and the Center Party (Jönsson, 2022). Over time, rural inhabitants have grown less supportive of wind power expansion, while the opposite is true for urban inhabitants (Jönsson, 2022). The negative trend dominates on the aggregate. These general attitudes are likely to influence energy policies on the national level. For the municipal level, on the other hand, attitudes toward *nearby* wind power expansion are likely to be more important. Here, voters are less inclined to favor wind power expansion close to their homes, with a long-term negative trend (with the same socioeconomic, ideological, and urban/rural differences) (Jönsson, 2022). Thus, wind power sentiments are characterized by NIMBYism ("Not In My Backyard"). The local resistance has occasionally mobilized and put pressure on local politicians. On four occasions, municipalities have held guiding referendums on wind power projects, all of which ended up unfavorable of the projects (Skurup Municipality, 2022; Malung-Sälen Municipality, 2022; Söderhamn Municipality, 2022).

Altogether, regulatory restrictions and the municipal veto add to the geographical restrictions on suitable locations for wind power investments, with noticeable differences in wind power development across regions and municipalities. Failing to obtain the municipal endorsement is the main reason for the rejection of a wind power project (Swedish Energy Agency, 2022d) – reflecting the NIMBYism mentioned above. It is somewhat more difficult in the southern bidding zones (SE3 and SE4), and there is a slight overall trend towards fewer endorsements over time, similar to the trend in public attitudes (Swedish Energy Agency, 2022d).

3 Theoretical framework

Sweden has a relatively high unemployment rate compared to other EU member states. In December 2022 (December 2003), the national unemployment rate was 7.23% (6.5%), and the labor force was 69.3% (65.25%) of ages 15-75 (Statistics Sweden, 2023a). Numerous factors have influenced these unemployment figures at the aggregate level, including business cycles, inflation rates, matching, public policies, and capital investments such as wind power deployment.

As an economy receives such capital expansion, the labor market can be affected in many ways. To enhance the interpretation of the empirical results, this section briefly discusses how wind power investments could impact the local labor market.

3.1 Direct effects

The direct effects of wind power investments on the local labor market are determined by two factors: the total labor requirement and the share of locally employed workers. Both factors are dynamic within and between the short-run (construction and installation) and the long-run (operation and maintenance; O&M).[1]

Wind turbines are more labor-intensive than traditional energy facilities (Luciani, 2022). However, as with many renewable energy sources, while the manufacturing and installation of wind turbines are labor intensive, generating electricity from wind turbines is more capital-intensive (Luciani, 2022). For instance, the construction of wind turbines includes land scraping, road construction, transportation, and other highly labor-intensive activities. After the wind turbines are connected to the grid, they only need to be operated and receive occasional maintenance (and, at some point, be decommissioned).

For a typical Swedish wind power park, the Swedish Energy Agency (2022c) estimates that 331 person-years are required during construction and 372 person-years during operation. Since the O&M period is around ten times longer, the number of annual gross job additions is lower during O&M. The shifting labor requirements indicate that the most prominent direct labor market effects arise during the construction period. However, the Swedish Energy Agency (2022c) also estimates that only 45% of the construction jobs are sourced locally, compared to 92% for O&M. In other words, construction is more labor-intensive than O&M, but the share of local workers is higher during O&M.

3.2 Indirect effects

To adequately estimate a wind power project's local economic development potential, one should also account for the indirect effects. Wind energy investments can have negative effects on other activities, crowding out sectors such as agriculture and tourism (Broekel and Alfken, 2015). Likewise, when the clean energy transition is driven by environmental

[1] In addition, jobs are created during the technological development (engineers, investors) and permission process (law, marketing). This work is assumed to be marginal and conducted by firms in other municipalities or countries.

policies, the policies can have simultaneous adverse effects on fossil-fuel industries (Curtis, 2018; Walker, 2012; Greenstone, 2002; Hafstead and Williams, 2018; Dorrell and Lee, 2020). Additionally, wind energy is associated with negative externalities, such as visual disamenities and sound pollution, with some studies finding adverse effects on property values (Dröes and Koster, 2021; Westlund and Wilhelmsson, 2022). This can affect the desire of people to live, visit or work in the community, which in turn affects consumer spending and tax revenues (Brown et al., 2012).[2]

On the other hand, wind investments can have positive indirect effects on local economic development. In particular, it can cause a higher local aggregated demand from additional revenues to the local government, contractors, land owners, and the owners (when the owner is located in the same municipality as the power plant). Moreover, positive externalities, such as the perception of a municipality's degree of sustainability, could attract residents, commuters, and further investments to the municipality (Brown et al., 2012).

[2]To the extent that wind energy can be compared to traditional natural resources, the resource curse hypothesis suggests that the net effects could even be negative. For an overview, see Aragón and Chuhan-Pole (2015), Cust and Poelhekke (2014), and Van Der Ploeg and Poelhekke (2017)

4 Previous literature

Several assessments of the economic impacts of wind power deployment have been conducted over the last two decades. The geographical unit of analysis range from direct adjacency effects to local, regional, and national-level effects. Outcome variables of interest include GDP, public finances, private income, migration flows, other sectors' revenues, and house prices. The results are at times inconclusive – reflecting the identification challenges associated with these research questions – but tend to indicate minor positive effects from wind power on economic development. Only a few papers share the same research objective as this one, namely estimating the labor market effects from wind energy investments. Two different methodological categories dominate, namely Input-Output Models and ex-post econometrics analyses. The latter is used in the paper.

4.1 Input-Output Models

Input-Output (I-O) models are frequently used for forecasts and estimations of the economic effects of new investments and facilities. Although one can incorporate and disentangle the indirect and induced effects to a certain extent, the entire net effects cannot be captured (Brown et al., 2012). Moreover, the results often hinge upon assumptions on parameters that are associated with within-model heterogeneity and uncertainties (Lambert and Silva, 2012). Nevertheless, a brief overview is merited.[1]

Papers using Input-Output models find net job additions in the range of around 1-2 jobs per megawatt (MW) during construction and 0.06-0.6 jobs per MW during operation and maintenance (Slattery, Lantz, and Johnson, 2011; Williams et al., 2008; Reategui and Hendrickson, 2011; Zwaan, Cameron, and Kober, 2013; Ejdemo and Söderholm, 2015). Ejdemo and Söderholm (2015) provide the only labor market estimate for the same geographical region of analysis as this paper, as they conduct a county-level Input-Output analysis over the large wind power project of Markbygden in northern Sweden. They find employment effects in the lower range compared to I-O studies on other regions (predominantly the U.S.), with an increase of 0.8 jobs per MW during the construction phase and 0.0175 jobs per MW during the operation phase. They point towards the relatively high levels of inter-county commuting in northern Sweden as a potential explanation for the low effects.

4.2 Ex-post econometric estimates

The remaining part of this literature review focuses on papers sharing the overarching methodology and aim as this one, namely ex-post econometric analyses of *local* labor market effects. These papers cover the US (Brown et al., 2012; De Silva, McComb, and Schiller, 2016; Brunner and Schwegman, 2022; Shoeib, Renski, and Hamin Infield, 2022)

[1]For an extensive overview of the literature using Input-Output models for the assessment of economic development from wind power projects, see Brown et al. (2012), Ejdemo and Söderholm (2015), Slattery, Lantz, and Johnson (2011), and Aldieri et al. (2019).

and the Iberian peninsula (Spain (Fabra et al., 2023; Duarte et al., 2022) and Portugal (Costa and Veiga, 2021)) with mixed results for both regions. Over time, the literature has departed from a strict focus on the long-term impacts to focusing (at times exclusively) on the short-term impacts. Temporal heterogeneity, spatial spillovers, and the distribution of impacts over age, sex, skill level, sector, and urbanization are also considered.

The results are difficult to compare across papers since they use different metrics. Explanatory variables include added capacity and accumulated capacity, in absolute terms or per capita normalizations of various forms. Outcome variables include unemployment rates, net job additions, and added person-years. Throughout this section, the effects on the absolute number of jobs will be expressed in MW (per capita). In contrast, the percentage point change in (un)employment rates will be expressed in kW (per capita) to avoid unnecessary decimals. I present the results as they are presented by the authors, as the units are not easily transformed without access to input data.

In possibly the first econometric assessment, Brown et al. (2012) study counties in a wind-rich region in the US between 2000 and 2008. They find that each MW per capita in operation was associated with 0.5 additional jobs per capita or a 0.4% increase in employment for the median county with wind power over this period (Brown et al., 2012). For Texas over 2001-2011, De Silva, McComb, and Schiller (2016) find no overall employment effect. These two papers use a first-difference two-period model, comparing the first and last year of their sample period. Thus, they only capture the permanent impact of wind turbines on employment, associating the cumulative capacity to the labor market indicator while controlling for several variables. The main advantage of this methodology is that the absence of a temporal dimension allows the researchers to instrument the wind power deployment with arguably the only relevant and exogenous instrument; namely "wind energy potential", reflecting wind conditions and topology. Since this variable is time-invariant, it cannot be used in regular panel data models with fixed effects. However, the two-period first-difference model requires assumptions of constant treatment effects as it is a weighted average of newer installations, older installations, and unobserved turbines under construction. Moreover, it does not utilize the temporal variation in the data, yielding low statistical power with the number of observations equal to the number of counties or municipalities.

As described in section 3, the labor-intensiveness for wind turbines is typically much higher during construction than during operation and maintenance (O&M). For Portugal, this is confirmed by Costa and Veiga (2021), with annual data over 1997-2017. The authors use a staggered difference-in-difference model while controlling for demographic changes, GDP growth, and municipal spending. Three independent variables capture the post-opening effect (cumulative capacity) and the two last years of construction (two lags of the added capacity), respectively. During the construction phase (two years prior to installation), unemployment is reduced by 0.6-0.8 percentage points per KW per capita, or around 0.39-0.55 jobs per MW per capita (Costa and Veiga, 2021). Unemployed men and those with a low education level gain even more. In the long run, they find no overall effect but show that workers with a university degree experience a slight decrease in unemployment – reflecting that some part of the high-skill O&M requirements is met locally in Portugal (Costa and Veiga, 2021).

For the U.S., Brunner and Schwegman (2022) adopt an event-study design as well as a staggered difference-in-difference model with the cumulative capacity and lagged cumulative capacity as independent variables. They also distinguish between the construction and O&M phases but find no significant effects on total employment in either phase. Brunner and Schwegman (2022) also find that the long-term composition of the labor market is changed; the share of employment consisting of farming decreases (−0.2%) while it increases for the manufacturing sector (+1.3%) and the construction sector (+0.7%). This inter-industry effect is greater for rural counties than for urban countries (Brunner and Schwegman, 2022). Shoeib, Renski, and Hamin Infield (2022) also explore the impact heterogeneity between rural and urban U.S. counties using a similar method. While they find a small positive long-term employment impact from wind power investments overall, this effect disappears when only including rural counties.

In a study on Spain, Fabra et al. (2023) not only separate between construction and maintenance (O&M), but also between employment and unemployment effects. In their framework, employment captures the number of jobs created by local firms (which are not necessarily filled by local workers), while unemployment reflects the changes in local employment level and the size of the local workforce. Thus, any difference indicates changes in migration or commuting, i.e., spillovers to other municipalities. A rich dynamic analysis is enabled with the use of monthly data and a local projection model (all the above-mentioned papers use annual data). With a local projections model, the O&M effects are limited by the length of the future horizon, which Fabra et al. (2023) set to one year. They find no employment effects and only small unemployment effects; −0.19 percentage points (p.p.) during construction and −0.35 p.p. during O&M per KW per capita (Fabra et al., 2023). The largest gains during construction accrue to unemployed men, especially those aged 25-45. During the maintenance phase, the unemployment results are more evenly distributed across gender and age, although the effects are small for all groups (Fabra et al., 2023). The most affected sectors are services, industry, and construction (Fabra et al., 2023). Fabra et al. (2023) note that the overall unemployment impacts are largely insignificant when accounting for potential biases arising from staggered DiD designs. Duarte et al. (2022) find similar results using a synthetic control method over Aragon, Spain.

Furthermore, Costa and Veiga (2021) assess the labor market impacts from wind power investments in neighboring municipalities, defined by a weighted distance decay matrix calculated from the municipalities' center, with cut-offs at 30km, 50km, and 100km, respectively. Indeed, they find large and positive unemployment impacts (−0.17 p.p. per KW per capita) from construction in close-by neighboring counties (<30km). The insignificant effects from the larger cut-offs are explained by the existence of a commuting channel but the lack of a migration channel (Costa and Veiga, 2021). On the other hand, Brown et al. (2012) find no spillover effects from contiguous counties, contrasting their baseline local results. Likewise, Fabra et al. (2023) confirm their largely insignificant baseline results when accounting for the spatial spillovers of turbine deployment within a 30km distance from the municipality border.

4.3 Other economic impacts of wind power investment

This paper is also related to the broader literature on the local economic development impacts of wind energy. This literature shows mixed results. Positive findings on economic development include increases in economic activity (Brown et al., 2012; Brunner and Schwegman, 2022; Xia and Song, 2017), municipal tax revenues (Brunner, Hoen, and Hyman, 2022; De Silva, McComb, and Schiller, 2016), wages (Mauritzen, 2020), and private income (Brunner and Schwegman, 2022; Shoeib, Renski, and Hamin Infield, 2022; De Silva, McComb, and Schiller, 2016). On the other hand, some papers find negative effects on municipal revenues (Xia and Song, 2017), and population (Brunner and Schwegman, 2022). Concerns have also been raised on damage to infrastructure during the transportation of large construction components (Jacquet and Stedman, 2013; Greene and Geisken, 2013), although this is not well studied. Additionally, some do not find several insignificant results – reflecting either the absence of economic development impacts of wind power or the difficulty of assessing these impacts empirically.

Furthermore, wind turbines' impact on property and housing values has been studied quite extensively, with mixed results. Most of them use hedonic methods to assess the monetary value of living in the near proximity of wind turbines (within a 2-10km radius). Some find no effect (Hoen, Brown, et al., 2015; Hoen and Atkinson-Palombo, 2016; Lang, Opaluch, and Sfinarolakis, 2014). Others find quite large negative impacts with property value reductions in the range of -2% and -13% (Dröes and Koster, 2021; Westlund and Wilhelmsson, 2022; Jensen et al., 2018; Sunak and Madlener, 2016). Brunner and Schwegman (2022) contrast these findings on the municipal level as they identify an average increase in property values of 4% per MW per capita. The linkages between property values and the labor market are less obvious than, e.g., GDP. However, it could affect migration flows and the purchasing power of local residents.

5 Empirical strategy

The objective of the empirical analysis is to identify potential causal relationships between wind turbine deployment and the local labor market. Considering previous literature and the theory laid out in section 3, the central hypothesis is that wind power investments decrease local unemployment. Moreover, it is likely that the effects are dynamic over time, with the most significant impacts appearing during the construction phase (0-3 years before the installation date). Long-term effects are also likely, referred to as the effects during the operation & maintenance (O&M) phase or the post-installation period.

Identifying these causal effects is a challenging task for several reasons. In particular, the treatment (wind power investments) is heterogeneous in size, over time, and over space. Indeed, municipalities can be inflicted with multiple treatments of different sizes (capacity), possibly endogenously determined, with variation in treatment timing and potential dynamic treatment effects. To this end, a difference-in-difference (DiD) model with two-way fixed effects (TWFE) is developed, with several adjustments to address the research setting at hand. The adjustments are guided by the information provided in section 2 and from the growing literature on causal inference in staggered DiD. A local projections model is also specified as a complement to the DiD model.

5.1 Difference-in-differences

To analyze the short- and long-term impacts of wind power investment on the local labor market, a difference-in-difference (DiD) model is constructed. The DiD model is widely used in social sciences for causal inference in quasi-experimental settings and is thoroughly explained by Angrist and Pischke (2009), Cunningham (2021), and Roth, Sant'Anna, et al. (2023), among others. In short, the DiD estimator utilizes variation in treatment over time and space to construct counterfactual outcomes for treated units after the treatment has been inflicted. A crucial assumption is that of parallel trends, or that temporal changes would be identical across units in the absence of treatment.

$$y_{i,t} = \delta_1 w_{i,t+36} + \lambda p_{i,t-36} + \eta_i + \mu_t + \varepsilon_{i,t} \tag{5.1}$$

The simplest DiD model in this paper is represented by equation 5.1. $y_{i,t}$ is the labor market outcome variable for municipality i in month t. $w_{i,t+36}$ is the accumulated capacity per capita three years ahead. The lead is used to capture projects that are currently under construction. δ_1 is thus the treatment effect from the construction start and onwards from one MW of wind capacity. If $Y_{0,i,t}$ is the potential outcome for unit i at time t without treatment, and $Y_{1,i,t}$ is the potential outcome with treatment, then $\delta_1 w_{i,t+36} = E[Y_{1,i,t} - Y_{0,i,t}]$. $p_{i,t-36}$ is population density, which is lagged by three years due to potential contemporaneous effects from the labor market and wind power investments. η_i is municipality-fixed effects, capturing municipality-specific (un)observables. μ_t is month fixed effects, capturing time-varying variables on the national level, such as business cycles and the average electricity price. $\varepsilon_{i,t}$ is an error term clustered at the municipal level in all regressions in order to account for heteroskedasticity and serial correlation within municipalities.

The labor market variable $y_{i,t}$ and wind power capacity $w_{i,t}$ are normalized by the population in the month preceding the observation window for $y_{i,t}$ (December 2002). Obtaining estimates in per capita-per capita form improves comparability to previous literature. Using a constant population denominator prevents endogeneity issues that unavoidably arise when transforming independent and dependent variables with the same time-variant and unit-specific variable. The pre-sample lag is motivated by the potential impacts of wind power investments on migratory patterns (Brunner and Schwegman, 2022).

Note that by using a non-binary treatment variable w, the DiD coefficient δ_1 is the treatment effect from a given wind project in relation to its size. Expressed differently, equation 5.1 explicitly models the intensity of each treatment. Here, the intensity is defined as the nameplate capacity in megawatts to align with previous literature and due to data availability. Alternative metrics for the size include the number of turbines, expected annual generation, and a project's monetary value. These are not considered in this paper.

5.1.1 Identifying assumptions

Constant treatment effects over time

As opposed to the classic 2×2 DiD research design (e.g., Card and Krueger (1994)), this paper uses a two-way fixed effect (TWFE) estimator. Specifically, it uses a multi-period model with multiple non-binary treatments[1] inflicted with differential timing. Therefore, unbiased estimates of the TWFE DiD estimator require constant treatment effects. This is necessary even if the dynamic treatment effects are identical across units. Expressed differently, the treatment effect must not vary with the number of periods h in relation to the installation date, $\delta_1 w_{i,t+36+h} = E[Y_{1,i,t+h} - Y_{0,i,t+h}] = E[Y_{1,i,t+h+1} - Y_{0,i,t+h+1}]$ for some h. If there are dynamic treatment effects, Goodman-Bacon (2021) shows that the DiD estimate for the average treatment effect on the treated (ATT) is biased. In essence, the bias arises because early-treated municipalities act as a control group for future-treated municipalities, but where current potential outcomes are affected by past treatments.

Recent literature has proposed alternative estimators that are unbiased under dynamic treatment effects and differential treatment timing. These estimators work under binary and staggered treatments[2], binary and non-staggered treatments, and continuous and staggered treatments (Chaisemartin and D'Haultfoeuille, 2022).[3] Unfortunately, to the best of my knowledge, there is currently no available estimator that allows for continuous and non-staggered treatments that is feasible for this research setting.[4] Consequently, a standard TWFE DiD estimator is used in the main results. This calls for a thorough theoretical discussion on the validity of the assumption of constant treatment effects over time. In

[1]As an alternative specification, binary treatment could be used. This standard approach in classic DiD models and typically represents the enactment of a binary policy. However, the theoretical labor market impacts are not due to simply *having* wind turbines in the municipality. Instead, it is a consequence of the broader economic effects related to the size of the wind power plant.

[2]Chaisemartin and D'Haultfoeuille (2022) define "staggered treatments" as treatments that can only increase over time and that can change at most once.

[3]For an overview, see Chaisemartin and D'Haultfoeuille (2022) and Roth, Sant'Anna, et al. (2023).

[4]The closest estimator is provided in section 4.3 in De Chaisemartin and d'Haultfoeuille (2020). However, they show that interpreting the treatment effects is difficult. Moreover, this paper's large dimension (288×240) would require substantial computing power to conduct an analysis such as the `csdid` command in STATA.

addition, alternative specifications and empirical sensitivity analyses are also provided in this and the following section.

The most obvious potential temporal difference in treatment effect is between the construction and O&M periods. Equation 5.2 controls for this by splitting the treatment period into two; a construction phase and an O&M phase. $w_{i,t}$ is the accumulated installed capacity per capita (now without lead), and $\Delta w_{i,t} = w_{i,t} - w_{i,t-1}$ is the added capacity per capita at month t. $\sum_{h=1}^{36} \Delta w_{i,t+h}$ is the capacity that will be added in the next three years, i.e., the capacity under construction. Now, α captures the treatment effect during the construction period, and δ_2 captures the treatment effect during operation and maintenance (all periods post-installation). Still, constant treatment effects within each treatment period are necessary for unbiased estimates.

$$y_{i,t} = \alpha \sum_{h=1}^{36} \Delta w_{i,t+h} + \delta_2 w_{i,t} + \lambda p_{i,t-36} + \eta_i + \mu_t + \varepsilon_{i,t} \tag{5.2}$$

Under the O&M phase, the treatment effects are assumed to be constant over time since the labor requirements, revenue streams, tax streams, and other effects are not determined by the turbines' age h.[5] A potential violation of a constant δ_2 arises if there are multiplier effects in the local economy. For instance, an increase in direct employment could increase aggregate demand and thus incrementally increase local labor demand. However, multiplier effects are unlikely to be significant if the total employment effects are small in absolute value. If so, only large wind power plants could divert a municipality's long-term trend from the national business cycle. In Sweden, such large projects have been constructed only recently, and most are yet to be connected to the grid. The absence of "mega projects" during most of the sample period increases the theoretical validity of the assumption of constant treatment effects post-installation.

Restricting the number of allowed treatments per municipality could mitigate potential post-installation dynamic impacts. Since previously installed capacity increases the likelihood of future installed capacity (Ek et al., 2013; Lauf et al., 2020), and given labor market impacts from the previously installed capacity, the first project could spark a cycle of labor market impact through a series of wind power investments. This violates the assumption of constant treatment effects during the O&M phase. Therefore, in some regressions, municipalities with more than one treatment period are excluded when the construction of the second project begins. I refer to this as the single-treatment subsample.

The single-treatment subsample differs from the main sample in three main ways. First, it only includes municipalities' first projects. This affects regression estimates only if the treatment effect differs between the first and subsequent wind projects (or between early and late projects since early projects are often the first project). Second, the observations per municipality are reduced unevenly, which trims the long-term impacts of the post-installation treatment effect δ_2 for municipalities with several projects. If dynamic effects are indeed present, the point estimate will converge toward the short-term post-installation impacts. Third, the reduction in the number of observations yields lower statistical power, which increases the likelihood of committing a Type II error.

For the construction phase, constant treatment effects are less likely. For example, the local labor demand could shift as the tasks changes from land scraping and road

[5]It is conceivable that the maintenance requirements increase with a turbine's age. However, this change is assumed to be negligible for the total employment effects. Decommissioning would also require more labor at the end of a turbine's lifetime. As described in section 2, decommissioning effects are unlikely to be a problem since only 0.5% of all turbines have been decommissioned to date.

construction to turbine transportation and installation. Since the number of construction periods is limited, I can model the dynamic effects. Equation 5.3 splits the construction period into the first year of construction β_1, the second year of construction β_2, and the last year of construction β_3. Equation 5.4 expands it further by adding monthly leads for newly installed capacity.

$$y_{i,t} = \beta_1 \sum_{h=25}^{36} \Delta w_{i,t+h} + \beta_2 \sum_{h=13}^{24} \Delta w_{i,t+h} + \beta_3 \sum_{h=1}^{12} \Delta w_{i,t+h} + \delta_2 w_{i,t} + \lambda p_{i,t-36} + \eta_i + \mu_t + \varepsilon_{i,t} \quad (5.3)$$

$$y_{i,t} = \sum_{h=1}^{36} \gamma_{-h} \Delta w_{i,t+h} + \delta_2 w_{i,t} + \lambda p_{i,t-36} + \eta_i + \mu_t + \varepsilon_{i,t} \quad (5.4)$$

Another peculiar and unintuitive feature of TWFE DiD models is that the estimated treatment effect might not be a convex combination of treatment effects under heterogeneous treatment effects (Chaisemartin and D'Haultfoeuille, 2022; Goodman-Bacon, 2021). Weights can even be negative, which could yield results with reversed signs. Consequently, the estimated coefficient from a TWFE estimator could be biased (Roth, Sant'Anna, et al., 2023). De Chaisemartin and d'Haultfoeuille (2020) provide a technique to compute the weights of the ATT in Stata. An analysis of equation 5.1 show that ATT estimand is a weighted average of 34 434 ATTs where almost half of the weights are negative; 17 560 ATTs receive a positive weight, and 16 874 receive a negative weight. However, the negative weights only sum to −0.0413, whereas the sum of the positive weights is 1.0413. Consequently, the bias from negatively weighted ATTs in the simplest specification is small and should only affect the aggregated ATT marginally.

Parallel trends

In order for the DiD model to provide unbiased estimates, it is crucial that municipalities follow the same labor market trends over time in the absence of treatment. Algebraically, the expected value for the outcome variable under treatment $Y_{1,i,t}$ and in the absence of treatment $Y_{0,i,t}$ (with no construction in place) must satisfy: $E[Y_{1,i,t} - Y_{0,i,t} | X_{i,t}, t, i, \sum_{h=1}^{36} \Delta w_{i,t+h} = 0] = \delta w_{i,t}$. Therefore, the post-installation treatment effect $(Y_{1,i,t} - Y_{0,i,t})$ is linearly determined by the accumulated installed capacity per capita only. This is referred to as the parallel trends assumption (PTA). The PTA is violated if unobserved labor market shocks or secular trends systematically differ between treatment and control units.

Treatment selection bias is one risk factor for non-parallel trends. Indeed, wind power investment is not a random process.[6] For instance, it is possible that wind developers prefer to invest in municipalities with increasing unemployment, as it could simplify the employment of local workers. However, this is not supported empirically (see section 2.2

[6]Brunner and Schwegman (2022) and Brown et al. (2012) provide a concrete solution to the endogeneity of wind power investment by instrumenting wind power investment with wind resources, or average wind speeds. Wind speeds are a clear driver for wind turbine deployment and are arguably exogenous to other economic outcomes, including the labor market. Thus, it is a valid instrument. However, average wind speeds are, by definition, constant and can, therefore, not capture the timing of the treatment. Hence, it is only possible to use this instrument in a two-period model, such as a 2×2 DiD. To my knowledge, there is no valid instrument for wind installations over time. Nevertheless, as discussed in this section, using an IV strategy is arguably unnecessary.

and table 2.1). The determinants of wind power investments as found in the literature – previously installed capacity, population density, wind resources, land area, national interest areas, and protected areas – are controlled for in the regressions through the fixed effects or, for population density, as a covariate.

Another risk of a PTA violation is exogenous shocks with asymmetrical impacts between treatment and control groups. For example, it is conceivable that national-level shocks (e.g., the great recession) had different impacts on rural municipalities (high wind power investment probability) and urban municipalities (low wind power investment probability). If the recessions hit rural municipalities more, the treatment effect estimate would be biased upwards. Thus, I introduce a second sample criterion – the "no pure control" subsample – where I relax the PTA to hold for treated municipalities only. In this subsample, early-treated municipalities are only compared with later-treated municipalities and vice versa. The assumption of random assignment into treatment and pure control is thus bypassed, but the timing and size of the treatments could still be endogenous and violate the PTA. Additionally, a consequence of excluding the pure control groups is that the number of units and observations, hence the power, falls.

Conducting an event study is a common empirical strategy to assess the validity of the parallel trends assumption (PTA). Event studies test for pre-treatment differences in trends ("pre-trends") by comparing outcomes between control groups and treatment groups prior to the treatment. If parallel pre-trends are not rejected, this increases the credibility of the PTA. Note that it is an indicative test, not a proof (the counterfactual trends are inherently unobservable). Additionally, event studies could suffer from low power and reverse the roles of the null and alternative hypotheses (Roth, Sant'Anna, et al., 2023), which increases the likelihood of erroneously accepting the PTA. Moreover, event studies using TWFE are also subject to bias in the presence of dynamic treatment effects (Callaway and Sant'Anna, 2022). Therefore, results from the event studies should be interpreted with care.

The event study specification is shown in equation 5.5, which is equivalent (Schmidheiny and Siegloch, 2021) to a distributed-lag model; an adapted version of equation 5.1, where the variables for installed capacity and capacity under construction are swapped with leads and lags of added capacity. The second and third terms are endpoints, which are included due to the limited event window (Schmidheiny and Siegloch, 2021) of eight years. γ_{24} is the treatment effect for all projects older than two years. γ_{-60} is the treatment effect for all projects that will be installed six years from time t, as observed in the data (i.e., up until December 2022). Every lead ($h > 0 \iff -h \leq 0$) of added capacity $\Delta w_{i,t+h}$ trims the observation window for $y_{i,t}$ from above. The pre-construction length $\bar{h}$ is aimed at minimizing the deviation of the effect window in the event study to the main regressions while also attaining credible pre-trend estimations. Here, six years of leads ($\bar{h}$ = 60) trims the observation window for the labor market variable to 2003-2017.[7]

$$y_{i,t} = \sum_{h=-23}^{59} \gamma_{-h} \Delta w_{i,t+h} + \gamma_{24} w_{i,t-24} + \gamma_{-60} \left(w_{i,t=2022m12} - w_{i,t+59} \right) + \lambda p_{i,t-36} + \eta_i + \mu_t + \varepsilon_{i,t} \tag{5.5}$$

Figure 5.1 displays the γ_{-h} coefficients for two samples; the unrestricted sample (multiple treatments and pure control groups included) and the fully-restricted sample (single treatment and excluding pure control groups.)[8] Although the pre-trends are insignificant

[7] Figure A.1 uses a pre-treatment period of six years.

[8] Event studies with semi-restricted samples are presented in figure A.2.

for the unrestricted sample, parallel pre-trends cannot be confidently confirmed since γ_{-60} deviates from zero with a large confidence interval (note, however, that it is insignificant). For the fully-restricted sample in panel (b), the pre-trends are closer to zero and drop three years prior to the installation date.

Lastly, to what extent does the PTA hinge upon the functional form of the DiD specification? Roth and Sant'Anna (2022) argues that an indisputable PTA should hold under any monotonic transformation but show that the PTA is sensitive to the functional form in virtually all practical quasi-experimental settings. Alternative event studies are presented in the Appendix A.4 (no per capita normalization and with a dynamic per capita normalization). Again, there are no indications of a violation of the parallel pre-trends. This increases the credibility of the PTA.

The construction period lasts for three years (No anticipation effect).

That the construction period lasts for three years is an important but disputable assumption. The data on wind power turbines include the installation date, but the start of construction is unknown. Although the construction phase typically lasts a maximum of three years (Swedish Energy Agency, 2022c), it varies over time, project size, site-specific conditions, and more. Therefore, it is not obvious how to specify the length of the construction period. Previous papers considering the pre-installation effects use two years (Costa and Veiga, 2021; Fabra et al., 2023), but provide little to no justification for this choice.

I chose three years in order to minimize the probability of wrongly assigning a municipality to the control group. A premature assignment of control groups is equivalent to violating the canonical assumption of no anticipation effects, which causes bias (Roth, Sant'Anna, et al., 2023). On the other hand, a longer construction time span will assign municipalities as treated when, in fact, the treatment effect is small or nil. If many municipalities enter treatment prematurely, the estimated ATT for the first months of construction will be lower than if the construction period always lasted for three years. However, if the construction treatment group is defined as municipalities that will receive a wind turbine within the next three years ($\sum_{h=1}^{36} \Delta w_{i,t+h} > 0$), the ATT estimand should include all individual treatment effects, including small ones. This causes additional dynamic treatment effects during the construction period. Thus, the dynamic specification of equations 5.3 and 5.4 isolates the features from premature treatment assignments.[9]

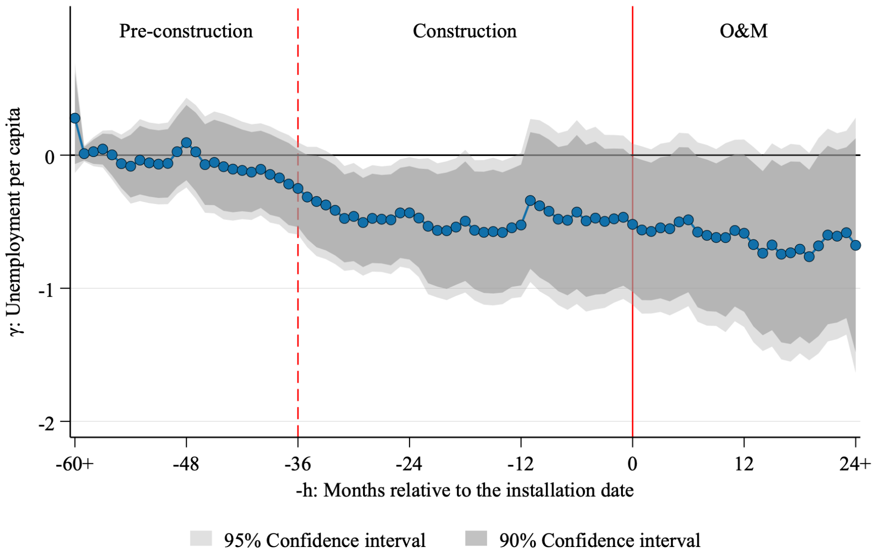
The event study in figure 5.1 shows that the pre-trend is broken approximately three years before construction starts. This provides empirical support for a three-year construction period.

No spatial spillovers (SUTVA)

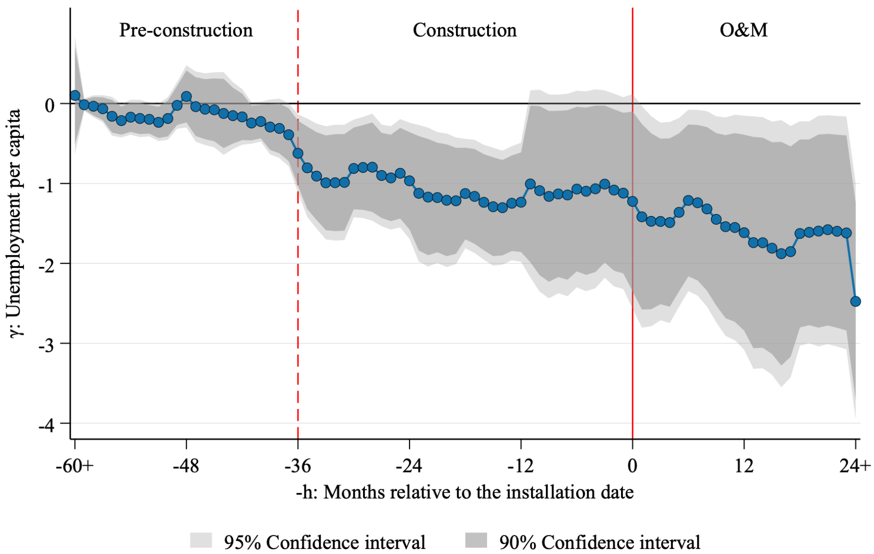
The last identifying assumption is that wind power plants only affect the municipalities in which they are located, also known as the Stable Unit Treatment Value Assumption (SUTVA). This is possibly violated by commuting workers and trade with nearby municipalities.

Nearby geographical spillovers can be accounted for by including them in the regression. Equation 5.6 show the simple DiD model with an additional variable, namely the cumulative capacity $w^{d}_{i,t+30}$ in area d, which is located within a sufficient distance from the border of municipality i. Four distance bins are used: 10 km, 30 km, 50 km, and 100 km.

[9]Appendix A.1 also provides estimates for one, two, and four construction years.



(a) Unrestricted sample



(b) Restricted sample

Figure 5.1: Event study

The figures map coefficients and confidence intervals of the lead and lags of added installed capacity per capita on unemployment per capita. Panel (a) uses the main sample described in section 6. Panel (b) excludes municipalities without wind power capacity in the final period and observations three years prior to the installation date of the second project. The leads restrict the observation window for unemployment to 2003-2017. The estimated coefficients γ_{-h} represent the change in unemployment per capita at $-h$ periods from the installation of one MW per capita. The first (leftmost) and last (rightmost) value of γ captures all future and past added capacity, respectively (binned values). The per capita terms are normalized for the population in the year prior to the main sample (December 2002). Note the different scales on the y-axes.

$$y_{i,t} = \delta_1 w_{i,t+36} + \delta_1^d w_{i,t+36}^d + \lambda p_{i,t} + \eta_i + \mu_t + \varepsilon_{i,t} \tag{5.6}$$

The spillover model increases the geographical coverage of the treatments. Hence, more municipalities are treated, more treatment periods are introduced, and the pure control group is reduced.

Another potential violation of the SUTVA arises if certain municipalities are affected more than others by wind power projects far away. Considering revenues to non-local owners and labor requirements for high-skill work (e.g., judicial tasks during the permit process, research and development, and certain maintenance tasks), municipalities in and around Sweden's three biggest cities (Stockholm, Göteborg, and Malmö) are plausibly more affected than others. These municipalities are excluded in a robustness check. To this end, I use the classification of municipalities from SKR (2022), and remove the 46 municipalities that are classified as either "big city" or "commuting municipality near big city"[10].

5.2 Local projections model (LP-DiD)

The TWFE DiD model is the main model in this paper. However, due to uncertainties regarding the identifying assumptions, notably that of constant treatment effects, a local projections difference-in-difference (LP-DiD) model is included as a complement.

The LP-DiD model is similar to an event-study design and can estimate short-run treatment effects. However, whereas the event-study design runs a single regression with a series of leads and lags of the independent variable, the LP-DiD model runs a series of regression over $h \in [\bar{h}, \underline{h}]$ horizons in relation to the start of the treatment, starting from $\bar{h}$ months prior to the installation month to $\underline{h}$ months after the installation month. Crucially, the LP-DiD estimator can be adapted to be unbiased under dynamic treatment effects and differential treatment timing (Dube et al., 2022).

This paper uses the LP-DiD model to obtain unbiased estimates in the presence of dynamic treatment effects. Moreover, it can also be used to assess the validity of the main result's assumption of constant treatment effects. The horizon is set to $h \in [60, -47]$, such that two pre-construction years are included to assess the pre-trends and four post-installation years to assess the assumption of constant treatment effects during O&M.

Equation 5.7 specifies the h number of regressions used for the LP-DiD model. γ_{-h} are the main variables of interest, capturing the treatment effect h periods in relation to the installation date. Equation 5.8 also include the lagged value of the independent variable at *t-61*, thereby explicitly controlling for pre-treatment values of unemployment (Dube et al., 2022). The specification with the lagged outcome dynamics is the model used in previous related literature (Fabra et al., 2023).

$$y_{i,t+h} = \gamma_{-h} \Delta w_{i,t} + \lambda p_{i,t+h-36} + \eta_i + \mu_t + \varepsilon_{i,t} \tag{5.7}$$

$$y_{i,t+h} = y_{i,t-61} + \gamma_{-h} \Delta w_{i,t} + \lambda p_{i,t+h-36} + \eta_i + \mu_t + \varepsilon_{i,t} \tag{5.8}$$

[10] These are: Ale, Alingsås, Bollebygd, Botkyrka, Burlöv, Danderyd, Ekerö, Göteborg, Haninge, Huddinge, Härryda, Håbo, Järfälla, Kungsbacka, Kungälv, Kävlinge, Lerum, Lidingö, Lilla Edet, Lomma, Malmö, Mölndal, Nacka, Nynäshamn, Partille, Salem, Sigtuna, Skurup, Sollentuna, Solna, Staffanstorp, Stenungsund, Stockholm, Sundbyberg, Svedala, Trelleborg, Tyresö, Täby, Upplands Väsby, Upplands-Bro, Vallentuna, Vaxholm, Vellinge, Värmdö, Öckerö, Österåker.

The local projections model is a variant of the TWFE DiD. Notably, the above-mentioned potential bias stemming from dynamic treatment effects is present. Dube et al. (2022) provides a solution to this bias applicable to the LP-DiD model, referred to as the "clean control condition." The clean control condition avoids comparing early-treated municipalities with late-treated municipalities. To this end, the control group at time t consists of municipalities that have never been treated at time t and will not receive treatment in t+h+*36* (the length of the horizon plus construction time). Moreover, the observations in the treatment group consist only of newly treated units that have never received treatment before and will not receive additional wind power installations between time t and time t+h+*36* (for $h \geq -36$). Equation 5.9 formally describes the clean control sample. Note that the clean control sample is similar to the single treatment sample used in some of the main regressions.

$$
\begin{aligned}
&\text{Treatment group} \begin{cases} \Delta w_{i,t} > 0;\ w_{i,t-1} = 0 & \text{if } h < -36 \\ \Delta w_{i,t} > 0;\ w_{i,t-1} = 0;\ w_{i,t} = w_{i,t+h+36} & \text{if } h \geq -36 \end{cases} \\
&\text{Clean control group} \begin{cases} w_{i,t} = 0 & \text{if } h < -36 \\ w_{i,t} = w_{i,t+h+36} = 0 & \text{if } h \geq -36 \end{cases}
\end{aligned}
\tag{5.9}
$$

Similar to the regular DiD estimator, the assumptions of parallel trends and no anticipation effects are necessary for the LP-DiD estimator to be unbiased (Dube et al., 2022). The above discussion on these identifying assumptions applies here, too, with the exception of constant treatment effects, which are modeled explicitly in the LP-DiD model.

6 Data

6.1 Wind power data

The data on wind power investment is collected from the wind power database *Vindbrukskollen* published by the Swedish Energy Agency and County Administrative Boards of Sweden (2023). This data set lists all individual wind turbines from 1980 to 2022 in Sweden. It provides information on the turbine's current status (e.g., operational, awaiting permit decision, withdrawn permit, rejected permit, and decommissioned), coordinates, application date, installation date, developer, project ID, height, capacity, expected annual production, and manufacturer.

Vindbrukskollen is updated directly by the wind power developers on a voluntary basis. The publishers do not verify that the inputs are correct. Hence, there could be measurement errors in the independent variable, including missing and erroneous data. 58 observations with obvious errors (installation date before 1970) or missing information (capacity) are removed from the data set. Nevertheless, the aggregate installed capacity closely match estimation from statistical agencies. For instance, Vindbrukskollen reports an aggregated national capacity of 11 947 MW in 2021 compared to 11 923 MW in 2021 reported by Eurostat (2023a). Consequently, the reliability of the wind turbine data is considered high. 74 non-utility scale wind turbines (less than 100 kilowatts, as defined by U.S. Department of Energy (2023)) are removed. This leaves 4915 wind turbines, with the first installed in February 1990 and the last in December 2022.

The data is aggregated to the accumulated added capacity per municipality and month. A wind power project can span several consecutive months of installed capacity and cover more than one municipality. Likewise, more than one wind power plant could be installed in a municipality in a given month. However, for simplicity, a "project" is henceforth referred to as an observation (municipality-month) of positive added capacity in order to align with the definition of the treatment. With this definition, a total of 921 projects are installed during the time frame and are scattered over 175 municipalities.

The capacity per capita of all projects is shown in figure 6.1. A few projects are much larger relative to the population than others. Notably, there are two outliers in the top-right corner, located in the municipalities of Åsele and Ockelbo. Since the two municipalities also have outliers in the middle of the sample, they will receive very large weights in the average estimated treatment effect. Therefore, Åsele and Ockelbo are removed from the data set.[1]

6.2 Labor market data

From the local policymaker's perspective, one of the most desirable labor market outcomes is (roughly) to increase the total number of jobs in the local economy. Unfortunately, there is no available monthly data on total employment at the municipal level in Sweden. A related variable is the number of new vacancies as reported to Swedish Public Employment

[1]Appendix A.2 provide results for all 290 municipalities

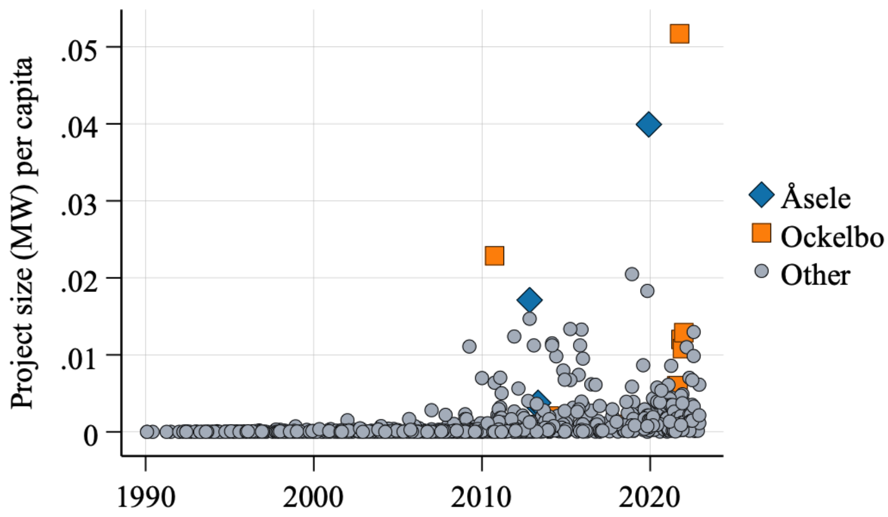


Figure 6.1: Outlier wind projects

Service (2023). However, this is only a fraction of all new vacancies and has very high volatility even within short time spans.

Instead, I use unemployment per capita as the labor market proxy, collected from the Swedish Public Employment Service (2023).[2] This dataset presents the absolute number of unemployed individuals within each municipality for each month in 1996-2022, broken down over age groups, gender, country of origin, and education level. Various unemployment definitions are available. I use the broadest metric – total unemployment – which includes all individuals registered at the Swedish Public Employment Service. Total unemployment makes no distinction between, inter alia, individuals who can immediately enter the labor market ("open unemployment"), individuals that participate in a labor market policy program (such as vocational training, work experience placement, and traineeships), part-time unemployed, temporarily employed, and employed with public support. The relative sizes of these groups bear political and personal importance. However, analyzing the flows between the groups would be a too complex task for this thesis.

Moreover, note that one additional job does not necessarily imply a one-unit reduction in absolute unemployment and vice versa. Thus, expectations concerning employment effects drawn from previous literature and the theoretical framework should be used carefully.

Monthly data on the workforce and unemployment ratio (unemployment as a share of the workforce) is available from 2008 (Swedish Public Employment Service, 2023). Theoretically, this would allow one to calculate the absolute employment levels (workforce - unemployed ≈ employed). However, upon closer inspection, the workforce figures are calculated by the Swedish Public Employment Service (2023) as the *monthly* unemployment levels plus the *yearly* employment levels. Hence, regressions based on the monthly workforce data would mainly reflect changes in monthly unemployment. Analyses on "monthly" employment levels or unemployment ratios are, therefore, not suitable.

[2]Statistics Sweden (2023b) reports more detailed labor market data (size of the labor force, hours worked, occupational status, et cetera) using random sampling. However, the smallest unit of analysis is counties.

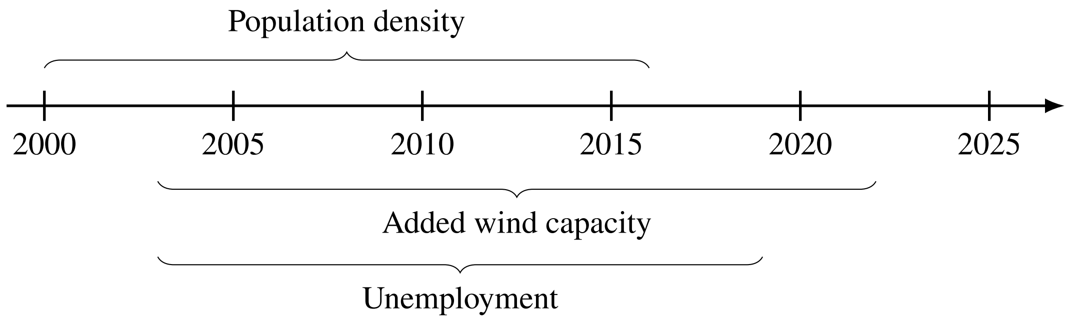


Figure 6.2: Observation windows
The temporal span for observations used in the main DiD regressions. The observation window for accumulated wind capacity is 1990-2022 and is excluded from the figure.

6.3 Observation windows and descriptive statistics

Information on population and land area is collected from Statistics Sweden (2023b). Monthly population data, and thereby population density data, is available from 2000 on the municipal level.

Although the unemployment data is available for 1996-2022, all is not included since most regressions use three years of lags for population density and three years of leads for installed wind turbines. Therefore, the observation window in the main sample is 2003-2019 for unemployment[3], 2000-2016 for population density, 1990-2022 for accumulated wind capacity, and 2003-2022 for added wind capacity. This is visualized in figure 6.2. Although the observation window for unemployment is determined by data availability and the regression specifications, it naturally limits any confounding effects from the Covid-19 pandemic on the labor market. Furthermore, note that 61 municipalities have (low levels of) installed wind capacity before January 2000 and thus enter the regression sample as treated.

Table 6.1 describes the average values and standard errors across variables, in total, and separated into the pure control group (no wind power projects) and treatment groups (at least one project). Additionally, the minimum and maximum values are presented in the last column. When excluding the two outlier municipalities, 113 municipalities have no wind generation capacity, and 175 municipalities have some wind generation capacity. On average, municipalities with wind energy capacity are larger geographically, have a smaller population, and have had lower population growth since 2003, compared to municipalities without wind power capacity. The mean GDP and GDP growth are similar across the groups. The unemployment per capita is slightly lower among municipalities with wind power capacity.

Moreover, the unemployment ratio (the metric typically used in the public debate) is slightly higher than unemployment per capita and has a similar relative size across the pure control and treatment groups. The average size of wind power capacity for municipalities with at least one wind power project is 71 MW or 27 turbines, spread over six projects. The municipality with the biggest capacity of 1202 MW is Piteå, and consists almost entirely of the Markbygden project. Lastly, the minimum and maximum values show that some municipalities have experienced negative population and GDP growth.

[3] In the event studies presented above, the six years of pre-installation leads limits the observation window for unemployment to 2003-2017. The same holds for the local projections models.

Table 6.1: Descriptive statistics

	Total	Wind power projects		Min / [Max]
		0	≥1	
Geographic				
Land area (km^2)	1396.0	793.1	1785.3	8.67
	(2440.1)	(959.5)	(2973.1)	[19 140]
Demographic				
Population	36 503	42 229	32 806	2372
	(75 999)	(100 045)	(55 221)	[984 748]
Population growth (2003-2022)	0.798	0.107	0.622	-0.256
	(0.155)	(0.175)	(0.138)	[0.599]
Population density	164	324.0	59.9	0.212
	(599)	(923)	(124)	[6236]
Economic				
GDP per capita (2020; MM SEK)	0.359	0.360	0.358	0.137
	(0.170)	(0.216)	(0.132)	[2.08]
GDP growth (2012-2022)	0.255	0.241	0.264	-0.358
	(0.155)	(0.169)	(0.146)	[0.994]
Unemployment per capita	0.0512	0.0547	0.0490	0.0220
	(0.0156)	(0.0181)	(0.0134)	[0.114]
Unemployment ratio	0.0610	0.0659	0.0579	0.0258
	(0.0209)	(0.0232)	(0.0186)	[0.132]
Wind power				
Capacity (MW)	43.10	0	70.93	0
	(111.6)	(0)	(136.2)	[1202]
Capacity per capita (MW)	0.00288	0	0.00473	0
	(0.00693)	(0)	(0.00839)	[0.0535]
Total turbines	16.20	0	26.67	0
	(34.26)	(0)	(40.68)	[358]
Wind power projects	3.764	0	6.194	0
	(6.395)	(0)	(7.232)	47
Municipalities	288	113	175	288

Mean coefficients; Standard-errors in parentheses. Last column: Min and max values.

Variables refer to the values in December 2022 unless otherwise specified. Per capita variables are normalized by the population in December 2002. Ockelbo and Åsele municipalities are excluded. Population, land area, and GDP data are collected from Statistics Sweden (2023b). Unemployment data from Swedish Public Employment Service (2023). Wind power data from the Swedish Energy Agency and County Administrative Boards of Sweden (2023).

7 Results

This section describes the empirical results of this thesis. It begins with the DiD specification using the unrestricted sample with increasingly dynamic specifications, followed by one of the dynamic specifications with increasingly stringent sample restrictions. Then, the LP-DiD estimates are presented. Lastly, heterogeneity analyses and robustness checks are conducted. All estimates reflect the causal effects of wind power investments on unemployment, provided all identifying assumptions hold. Section 8 discusses the main results more carefully by further elaborating on the validity of the results.[1]

7.1 Main results

Table 7.1 present the regression results for the unrestricted sample, using equations 5.1, 5.2, 5.3, and 5.4. All specifications with the unrestricted sample show that wind power investment significantly reduces unemployment in both the short term during construction and the long term during operation and maintenance. For the specification with a single treatment variable (Column (1)), the average marginal effect from one MW of installed wind capacity is a net reduction in unemployment by 0.785 units.

Columns (2)-(4) introduce separate treatment variables in order to analyze the dynamics between the pre- and post-installation periods. Column (3) also analyzes the treatment effects with three yearly treatment variables during construction. Column (4) includes 36 monthly leads for the construction phase. The 36 coefficients are not presented. Instead, a joint F-test shows that the coefficients are jointly different from zero.

In contrast to previous research, the treatment effect seems to increase in magnitude during the construction phase and reach its highest value during O&M. The average effect during the full construction phase is −0.538, increasing from −0.388 during the first year to −0.534 during the second year and −0.793 during the last year of construction. The post-installation treatment effect is estimated at approximately −0.995 in all dynamic models. The similarity in the O&M treatment effect between Columns (3) and (4) indicates that any bias stemming from insufficient modeling of the construction phase is low or non-existent. Therefore, equation 5.3 (three yearly treatment variables during construction) is used as the preferred specification in all remaining regressions. This enables a more concise presentation of the results and an explicit analysis of the dynamics during the construction phase.

As discussed in section 5.1.1, there are reasons to believe that regressions on the unrestricted sample would be biased. Therefore, table 7.2 presents the regression result for restricted samples using equation 5.3 (differentiated treatment effects with three separate construction years). The unrestricted sample is shown in column (1) for comparison. Column (2) includes only the first treatment in a municipality, thus excluding observations three years prior to the installation of a municipality's second project. Column (3) excludes never-treated municipalities. Column (4) impose both of these sample restrictions. All regressions find significant negative effects, with increasingly larger effects from the first year of construction to the O&M period. However, the sample restrictions alter

[1] All regressions are run in Stata/SE 17.0 for Mac. Data and Stata code are available upon request.

Table 7.1: Regression results, unrestricted sample

	Unemployment per capita$_{i,t}$			
	(1)	(2)	(3)	(4)
$w_{i,t+36}$	-0.785***			
	(0.000)			
$w_{i,t}$		-0.996**	-0.995**	-0.994**
		(0.002)	(0.002)	(0.002)
$\sum_{h=1}^{36} \Delta w_{i,t+h}$		-0.538***		
		(0.000)		
$\sum_{h=25}^{36} \Delta w_{i,t+h}$			-0.388**	
			(0.002)	
$\sum_{h=13}^{24} \Delta w_{i,t+h}$			-0.534***	
			(0.000)	
$\sum_{h=1}^{12} \Delta w_{i,t+h}$			-0.793***	
			(0.000)	
$popdensity_{i,t-36}$ $(\times 10^{-3})$	0.037***	0.037***	0.037***	0.037***
	(0.000)	(0.000)	(0.000)	(0.000)
Joint F-test $\gamma_h = 0\,\forall h$				8.247***
				(0.000)
R^2	0.342	0.346	0.346	0.346
Municipalities	288	288	288	288
Observations	58717	58717	58717	58717

p-values in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors clustered by municipality.

Regression results for the unrestricted sample with increasingly dynamic specifications. Lead variables for installed capacity in column (4) are omitted due to conciseness. All regressions include month- and municipality-fixed effects. The dependent variable (unemployment) and w (installed capacity) are normalized by the population as of December 2002.

Table 7.2: Regression results, restricted samples

	Unemployment per capita$_{i,t}$			
	(1)	(2)	(3)	(4)
$w_{i,t}$	-0.995**	-2.656***	-0.743*	-1.914**
	(0.002)	(0.000)	(0.012)	(0.003)
$\sum_{h=25}^{36} \Delta w_{i,t+h}$	-0.388**	-1.156***	-0.272*	-0.833*
	(0.002)	(0.001)	(0.025)	(0.023)
$\sum_{h=13}^{24} \Delta w_{i,t+h}$	-0.534***	-1.542***	-0.403**	-1.142**
	(0.000)	(0.000)	(0.003)	(0.006)
$\sum_{h=1}^{12} \Delta w_{i,t+h}$	-0.793***	-1.804***	-0.655**	-1.365**
	(0.000)	(0.000)	(0.001)	(0.005)
$popdensity_{i,t-36}$ ($\times 10^{-3}$)	0.037***	0.033***	0.169*	0.097*
	(0.000)	(0.000)	(0.045)	(0.047)
R^2	0.346	0.335	0.414	0.462
Pure control included	Yes	Yes	No	No
Multiple treatments allowed	Yes	No	Yes	No
Municipalities	288	235	175	122
Observations	58717	37917	35700	14900

p-values in parentheses; $^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Standard errors clustered by municipality.

Regression results using equation 5.3 for the unrestricted baseline (1), semi-restricted samples (2-3), and the fully-restricted sample (4). "Pure control included" indicates whether never-treated municipalities are included. "Multiple treatments allowed" indicates whether projects other than the first are included. All regressions include month- and municipality-fixed effects. The dependent variable (unemployment) and w (installed capacity) are normalized by the population as of December 2002.

the magnitude of the estimated effects. Since both subsamples restrictions give rise to different estimates than the unrestricted sample, the fully-restricted subsample (Column (4)) is preferred. The results from the fully-restricted subsample show that the first project in a municipality reduces unemployment by 0.833, 1.142, and 1.365 persons per MW for the first, second, and last construction year, respectively. The post-installation effects are higher, at -1.914.

7.2 Local projections model

The local projections model without a lagged outcome variable in figure 7.1 (a) tells a similar story to the main results; the labor market effect from wind power investments grows during the construction period. Afterward, during the post-installation period, it stabilizes at around a 2.2 unit decrease in unemployment per MW installed. The treatment effect seems to plateau partially during the O&M period – indicating that the post-installation treatment effect is constant, at least for the first four years. Still, the γ_{-h}'s show a slight reduction as h increases also for $-h > 0$, and the large confidence interval does not allow one to confidently rule out dynamic effects during O&M. Moreover, the pre-trends are worrying. As can be seen, the pre-treatment coefficients ($-h < -36$) are all significantly different from zero, invalidating this local projection model's results.

When explicitly controlling for past unemployment values, the pre-treatment values are not significantly different from zero. This is shown in panel (b) of figure 7.1. Once again, the estimated treatment effects are similar to the results in the preferred DiD model. Moreover, the post-installation treatment trend seems to plateau and remain constant from around 16 months after installation and onwards, although the standard deviation increases with -h.

The similarity in results from the unbiased LP-DiD estimator to the potentially biased DiD estimator increases the latter's validity. However, panel (a)'s negative pre-treatment trend and the standard deviations during O&M still call for cautious interpretations.

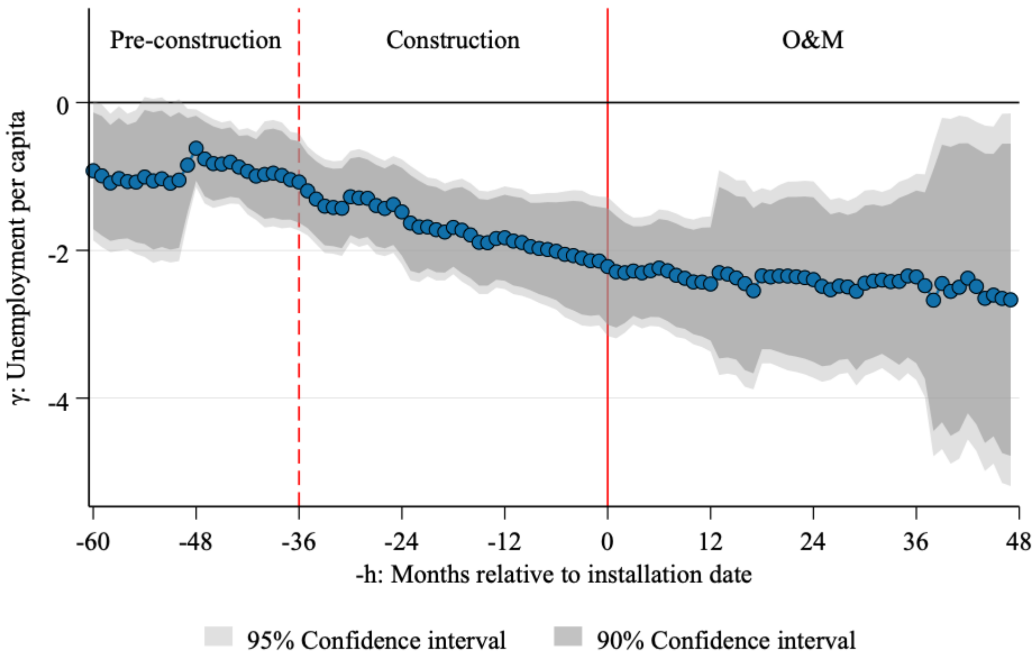
7.3 Heterogeneity analysis

The main results are a consequence of changes within different socioeconomic groups and regions. From a political economy perspective, assessing if some groups benefit more from wind power investments is important. The unemployment data is broken down into four group categories: sex (binary), age groups (16-17, 18-24, 25-54, and 55+), education (pre-gymnasium, gymnasium, post-gymnasium[2], and unknown), and country of origin (Sweden, European country, non-European country). The age group 16-17 and unknown education level are too small and irregular, respectively, to be analyzed.

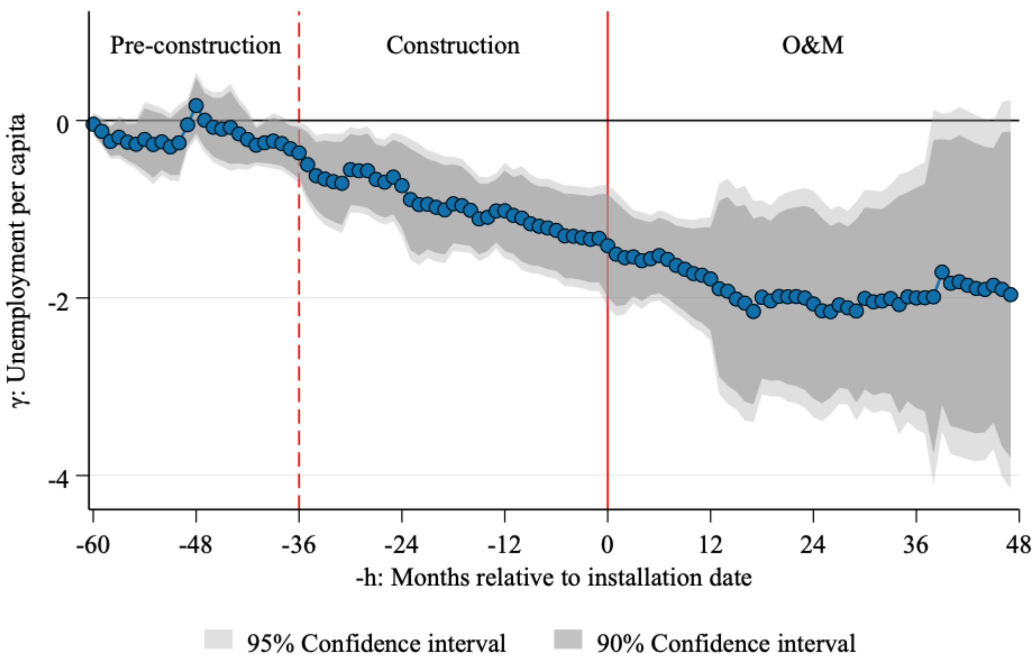
Except for sex, the relative size of the groups within each category differ. The treatment effect is expected to be larger for groups that constitute a larger share of the total unemployment, such as the age group 25-54 and Swedish-born. Hence, it is not only interesting to assess which groups are impacted and how, but also if they are over- or underrepresented in the aggregate effect.[3] Since the groups are exhaustive for each category, the sum of each variable's coefficients approximately equals the coefficient

[2]Gymnasium is the upper secondary education in Sweden. Post-gymnasium education could be, for instance, university studies.

[3]Since unemployment is a discrete variable, the statistical power is also greater for larger groups.



(a) Eq 5.7 (No lagged outcome variable)



(b) Eq 5.8 (Lagged outcome variable)

Figure 7.1: Local Projections (LP-DiD)

The figures map coefficients and confidence intervals for the γ_{-h}'s as specified in equations 5.7 (panel a) and 5.8 (panel b), using the clean control condition defined by equation 5.9. The estimated coefficients γ_{-h} represent the change in unemployment per capita at h periods from the installation date from one MW per capita. The per capita terms are normalized for the population as of December 2002. Note the different scales on the y-axes.

for the aggregate treatment effect (Column (1) in each table). Therefore, each group's contribution to the aggregate estimated effect can be calculated. The shares are presented in square brackets. For reference, the group's average share of total unemployment is shown in the bottom row of the tables. The significance of the over- and underrepresentations is not tested statistically.

Table 7.4 presents the results over sex and age groups. Men are shown to be more affected by wind installations than women, with significantly negative effects for each treatment period. Women are less affected, with significant impacts during the O&M period only. Young unemployed (18-24) are slightly underrepresented during the construction period, with significant effects only during the last year of construction. Other than that, there is no sign of over- or underrepresentation across age groups. The largest group – unemployed aged 25-54 – is the only group with statistically significant effects for each treatment period. One cannot reject the null hypothesis for the treatment effect on unemployed aged 55 or older on the 5% significance level. However, for all treatment periods other than the first year of construction, there are significant negative effects on the 10%-significance level for the oldest age group.

Table 7.3 shows regression results over three education levels and three country groups of origin. Unemployed without post-gymnasium education experience a significant decrease in unemployment from local wind power investments, both during construction and during O&M. Workers with a gymnasium education are seemingly overrepresented in the aggregate investment impacts on unemployment, and workers in the lowest educational group are possibly underrepresented, although to a small degree. Workers with higher education are not found to be impacted by local wind power deployment. Likewise, significant treatment effects are only found for unemployed individuals born in Sweden, possibly with some overrepresentation. No effect is found for persons born elsewhere.

As for the geographical impact heterogeneity, table 7.5 split the sample into Sweden's three NUTS1 regions.[4] The regression output show inconclusive results for all treatment periods and NUTS1 regions. Therefore, no conclusions can be drawn about the relative treatment effects between the regions. The insignificant results could be explained by the fewer observations and available control groups.

For the temporal impact heterogeneity, table 7.6 split the sample in pre- and post-subsamples in relation to 2007, 2012, and 2021, respectively. Most treatment variable coefficients are significant. For the coefficients without asterisks, most have negative coefficients with p-values of less than 10% or 15%. The exceptions are the first year of construction for the pre- and post-2012 subsamples. Moreover, the temporal splits suggest that the effects were considerably larger during the early period (especially prior to and including 2007). For the last column, post-2017, the treatment effects have reversed signs at high significance levels. Contrary to the main results, this indicates that unemployment in 2018-2019 increased by wind power installations. Note that the post-2017 subsample includes only 20 municipalities, of which eight[5] are treated during this period. A semi-restricted sample that excludes the pure control group but includes multiple treatments within a municipality is used in figure A.3. Here, the number of observations increases for the post-2017 subsample, and the estimated post-installation treatment effects then share the same significant sign as the main results (negative). Nevertheless, the treatment effects during the construction period are insignificant in the semi-restricted subsample for the post-2017 period.

[4]The NUTS1 regions are shown in figure B.1.

[5]Alvesta, Habo, Lomma, Norberg, Pajala, Sunne, Tranemo, Ånge.

Table 7.3: Regression results, education and country of origin

	Unemployment per capita$_{i,t}$						
		Education			*Country of origin*		
	All	Gym	Pre-gym	Post-gym	SE	EU	non-EU
$w_{i,t}$	-1.914**	-1.167**	-0.518*	-0.187	-1.491**	-0.047	-0.275
	(0.003)	(0.006)	(0.013)	(0.709)	(0.005)	(0.695)	(0.134)
		[61.0%]	[27.1%]	[9.8%]	[77.9%]	[2.5%]	[14.4%]
$\sum_{h=25}^{36} \Delta w_{i,t+h}$	-0.833*	-0.500*	-0.257*	-0.042	-0.682*	-0.095	0.014
	(0.023)	(0.048)	(0.027)	(0.929)	(0.020)	(0.173)	(0.903)
		[60.0%]	[30.9%]	[5.0%]	[81.9%]	[11.4%]	[-1.7%]
$\sum_{h=13}^{24} \Delta w_{i,t+h}$	-1.142**	-0.750**	-0.293*	0.291	-0.978**	-0.104	0.018
	(0.006)	(0.008)	(0.049)	(0.538)	(0.002)	(0.326)	(0.889)
		[65.7%]	[25.7%]	[-25.5%]	[86.4%]	[9.1%]	[-1.6%]
$\sum_{h=1}^{12} \Delta w_{i,t+h}$	-1.365**	-0.888**	-0.332*	0.001	-1.119**	-0.117	-0.053
	(0.005)	(0.009)	(0.021)	(0.998)	(0.005)	(0.264)	(0.664)
		[65.1%]	[24.3%]	[-0.1%]	[82.0%]	[8.6%]	[3.9%]
$popdensity_{i,t-36}$ $(\times 10^{-3})$	0.097*	0.100**	0.006	-0.167	0.134*	0.020	-0.058
	(0.047)	(0.008)	(0.769)	(0.285)	(0.045)	(0.251)	(0.213)
R^2	0.462	0.577	0.219	0.448	0.756	0.132	0.692
Municipalities	122	122	122	122	120	120	120
Share of total		49.8%	31.2%	18.2%	73.1%	8.9%	17.9%
Observations	14900	14900	14900	14900	13690	13690	13690

p-values in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; Standard errors clustered by municipality.

Value in square brackets indicates the group's share of the total effect for the corresponding treatment variable.

Regression results using equation 5.3 for the baseline, education level, and country of origin. All regressions use the fully-restricted sample (no pure control, maximum one treatment per municipality) and include month- and municipality-fixed effects. The dependent variable (unemployment) and w (installed capacity) are normalized by the population as of December 2002.

Table 7.4: Regression results, sex and age groups

	Unemployment per capita$_{i,t}$					
		Sex		*Age groups*		
	All	Male	Female	18-24	25-54	55-
$w_{i,t}$	-1.914**	-1.359***	-0.555*	-0.316*	-1.266**	-0.326
	(0.003)	(0.001)	(0.042)	(0.013)	(0.003)	(0.063)
		[71.0%]	[29.0%]	[16.5%]	[66.1%]	[17.0%]
$\sum_{h=25}^{36} \Delta w_{i,t+h}$	-0.833*	-0.651**	-0.182	-0.078	-0.611**	-0.143
	(0.023)	(0.006)	(0.206)	(0.356)	(0.003)	(0.231)
		[78.2%]	[21.8%]	[9.3%]	[73.3%]	[17.2%]
$\sum_{h=13}^{24} \Delta w_{i,t+h}$	-1.142**	-0.870**	-0.272	-0.141	-0.763**	-0.241
	(0.006)	(0.001)	(0.109)	(0.053)	(0.004)	(0.054)
		[76.2%]	[23.8%]	[12.3%]	[66.8%]	[21.1%]
$\sum_{h=1}^{12} \Delta w_{i,t+h}$	-1.365**	-1.028***	-0.337	-0.164*	-0.877**	-0.323
	(0.005)	(0.000)	(0.126)	(0.029)	(0.003)	(0.064)
		[75.3%]	[24.7%]	[12.0%]	[64.2%]	[23.6%]
$popdensity_{i,t-36}$ ($\times 10^{-3}$)	0.097*	0.029	0.068**	0.007	0.064	0.027**
	(0.047)	(0.260)	(0.006)	(0.609)	(0.088)	(0.005)
R^2	0.462	0.396	0.544	0.573	0.394	0.400
Municipalities	122	122	122	122	122	122
Observations	14900	14900	14900	14900	14900	14900
Share of total		50%	50%	16.7 %	63.7%	19.5%

p-values in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; Standard errors clustered by municipality.

Value in square brackets indicates the group's share of the total effect for the corresponding treatment variable.

Regression results using equation 5.3 for the baseline, sex, and ages. All regressions use the fully-restricted sample (no pure control, maximum one treatment per municipality) and include month- and municipality-fixed effects. The dependent variable (unemployment) and w (installed capacity) are normalized by the population as of December 2002.

Table 7.5: Regression results, NUTS1 regions

	Unemployment per capita$_{i,t}$		
	East (SE1)	South (SE2)	North (SE3)
$w_{i,t}$	1.176	0.062	-0.687
	(0.774)	(0.975)	(0.302)
$\sum_{h=25}^{36} \Delta w_{it+h}$	0.132	0.113	-0.391
	(0.920)	(0.854)	(0.334)
$\sum_{h=13}^{24} \Delta w_{it+h}$	3.792	0.391	-0.552
	(0.133)	(0.632)	(0.246)
$\sum_{h=1}^{12} \Delta w_{it+h}$	4.387	0.935	-0.660
	(0.340)	(0.435)	(0.239)
$popdensity_{i,t-36}$ ($\times 10^{-3}$)	0.652*	-0.012	-0.258
	(0.042)	(0.764)	(0.564)
R^2	0.602	0.456	0.652
Municipalities	17	56	49
Observations	2244	6466	6190

p-values in parentheses

Standard errors clustered by municipality.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Regression results using equation 5.3 for the three NUTS1 regions and the fully-restricted sample. All regressions use the fully-restricted sample (no pure control, maximum one treatment per municipality) and include month- and municipality-fixed effects. The dependent variable (unemployment) and w (installed capacity) are normalized by the population as of December 2002.

Table 7.6: Regression results, yearly subsamples

	Unemployment per capita$_{i,t}$						
		2007		2012		2017	
	All	$\leq$	$>$	$\leq$	$>$	$\leq$	$>$
$w_{i,t}$	-1.914**	-10.341**	-1.596**	-2.195***	-0.725*	-1.898**	0.950*
	(0.003)	(0.004)	(0.005)	(0.000)	(0.022)	(0.004)	(0.026)
$\sum_{h=25}^{36} \Delta w_{it+h}$	-0.833*	-1.424***	-0.525	-0.512	-0.037	-0.876*	0.807***
	(0.023)	(0.000)	(0.067)	(0.310)	(0.863)	(0.014)	(0.000)
$\sum_{h=13}^{24} \Delta w_{it+h}$	-1.142**	-1.554***	-0.827*	-1.059	-0.268	-1.182**	1.438***
	(0.006)	(0.000)	(0.013)	(0.059)	(0.074)	(0.004)	(0.000)
$\sum_{h=1}^{12} \Delta w_{it+h}$	-1.365**	-8.302	-1.093**	-1.598**	-0.463*	-1.078	1.316**
	(0.005)	(0.123)	(0.009)	(0.002)	(0.048)	(0.054)	(0.001)
$popdensity_{i,t-36}$ ($\times 10^{-3}$)	0.097*	0.202*	0.064	0.168*	0.027	0.105*	0.261
	(0.047)	(0.045)	(0.211)	(0.032)	(0.704)	(0.035)	(0.145)
R^2	0.462	0.554	0.538	0.506	0.533	0.433	0.415
Municipalities	122	122	87	122	57	122	47
Observations	14900	6532	8368	10721	4179	13882	1018

p-values in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; Standard errors clustered by municipality.

Regression results using equation 5.3 for different periods. All regressions use the fully-restricted sample (no pure control, maximum one treatment per municipality), and include month- and municipality-fixed effects. Under each year, the column to the left ($\leq$) is the sample prior to and including that year, and the column to the right ($>$) is the sample for all months after that year. The dependent variable (unemployment) and w (installed capacity) are normalized by the population as of December 2002.

7.4 Robustness checks

Table 7.7 presents regression results for the geographical spillover analysis. The local effects prevail when controlling for potential spillovers, although the point estimates are lower in magnitude when including the lower-distance bins. For the spillover effects, significant impact is only found for wind turbines located within 10 km from a municipality's border and only during the post-installation period. This O&M spillover within 10 km is also smaller than the local O&M effect, with an unemployment reduction of 0.3 persons per MW. Hence, a municipality that receives new wind projects seems to benefit more than the nearby municipalities. However, when turbines are located just outside a municipality's border, some beneficial spillover effects are identified after the installation date.

The prevalence of spillover effects invalidates the SUTVA. However, the lack of impact on large geographical areas ($\geq$ 30 km), in combination with similar estimates for the local treatment effects when controlling for spillovers, gives further confidence for the negative impact on local unemployment.

For spillovers for municipalities far away, Column (2) in table 7.8 shows the regression results for the specification with three construction years and excluding 46 municipalities in or nearby a big city. Since many of these municipalities do not have any wind power capacity, they are excluded as part of the pure control group. Hence, only 11 municipalities are removed compared to the baseline. The results are similar to the main results, with slightly lower coefficient estimates of around 0.15 units. Moreover, the significance level is lower.

Column 3 in table 7.8 excludes population density from the regression. This variable has been shown to affect both unemployment and wind power capacity (Lauf et al., 2020; Ek et al., 2013). When removing it, the estimates are similar to the baseline but larger in magnitude. The higher absolute values indicate that population density indeed affects both the dependent and the independent variable in the same direction.

The last two columns in table 7.8 display regression outputs with alternative per capita transformation of wind power capacity and unemployment levels. First, a dynamic specification – where the variables are divided by population at time t-36 – yields smaller treatment effects with lower significance levels for all treatment periods. When regressing the absolute wind power capacity on the absolute unemployment levels, the point estimates are similar to the baseline, but the estimates are insignificant.

Table 7.7: Regression results, spatial spillovers

	Unemployment per capita$_{i,t}$				
d	0km	10km	30km	50km	100km
$w_{i,t}$	-1.914**	-1.628**	-1.806**	-1.819**	-1.946**
	(0.003)	(0.009)	(0.003)	(0.003)	(0.002)
$\sum_{h=25}^{36} \Delta w_{i,t+h}$	-0.833*	-0.668*	-0.829*	-0.819*	-0.843*
	(0.023)	(0.042)	(0.014)	(0.028)	(0.021)
$\sum_{h=13}^{24} \Delta w_{i,t+h}$	-1.142**	-0.931*	-1.109**	-1.123**	-1.165**
	(0.006)	(0.013)	(0.004)	(0.006)	(0.004)
$\sum_{h=1}^{12} \Delta w_{i,t+h}$	-1.365**	-1.133*	-1.291**	-1.318**	-1.368**
	(0.005)	(0.015)	(0.006)	(0.008)	(0.004)
$w_{i,t}^{d}$		-0.300***	-0.110	-0.040	0.014
		(0.000)	(0.372)	(0.678)	(0.674)
$\sum_{h=25}^{36} \Delta w_{i,t+h}^{d}$		-0.083	0.125	0.060	0.011
		(0.464)	(0.246)	(0.298)	(0.767)
$\sum_{h=13}^{24} \Delta w_{i,t+h}^{d}$		-0.087	0.047	-0.041	-0.025
		(0.498)	(0.559)	(0.595)	(0.415)
$\sum_{h=1}^{12} \Delta w_{i,t+h}^{d}$		-0.144	-0.018	0.001	-0.024
		(0.290)	(0.874)	(0.988)	(0.676)
$popdensity_{i,t-36}$ ($\times 10^{-3}$)	0.097*	0.091	0.092	0.093	0.101
	(0.047)	(0.064)	(0.079)	(0.088)	(0.065)
R^2	0.462	0.471	0.466	0.463	0.463
Time fixed effects	No	No	No	No	No
Municipality fixed effects	No	No	No	No	No
Municipalities	122	122	122	122	122
Observations	14900	14900	14900	14900	14900

p-values in parentheses

Standard errors clustered by municipality.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Regression results using equation 5.6 for the fully-restricted baseline (column 1) and four distance bins. The dependent variable (unemployment) and w (installed capacity) are normalized by the population as of December 2002. Municipality- and month-fixed effects included.

Table 7.8: Additional robustness checks

	Unemployment per capita$_{i,t}$				
				Per capita transformations	
	All	Excl big cities	Excl covariates	Dynamic	None
$w_{i,t}$	-1.914**	-1.807**	-2.395**	-1.279*	-1.718
	(0.003)	(0.007)	(0.001)	(0.017)	(0.294)
$\sum_{h=25}^{36} \Delta w_{it+h}$	-0.833*	-0.804*	-1.196**	-0.582	-1.310
	(0.023)	(0.032)	(0.002)	(0.067)	(0.173)
$\sum_{h=13}^{24} \Delta w_{it+h}$	-1.142**	-1.099*	-1.514***	-0.833*	-1.088
	(0.006)	(0.010)	(0.001)	(0.023)	(0.190)
$\sum_{h=1}^{12} \Delta w_{it+h}$	-1.365**	-1.305**	-1.712**	-0.999*	-1.291
	(0.005)	(0.009)	(0.001)	(0.025)	(0.134)
$popdensity_{it-36}$ ($\times 10^{-3}$)	0.097*	0.766		-0.006	-3781
	(0.047)	(0.269)		(0.883)	(0.116)
R^2	0.462	0.466	0.481	0.460	0.137
Pure control included	No	No	No	No	No
Multiple treatments allowed	No	No	No	No	No
Municipalities	122	111	131	122	122
Observations	14900	13148	19449	14900	14900

p-values in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$; Standard errors clustered by municipality.

Regression results using equation 5.6 for the fully-restricted sample. Excl big cities *exclude municipalities around Stockholm, Göteborg, and Malmö.* Excl covariates *exclude population density from the regression. The last two columns use dynamic (t-36) and no per capita transformations of wind power capacity and unemployment rather than the static per capita transformation used in all other regressions. Municipality- and month-fixed effects included.*

8 Discussion

The empirical analysis suggests that wind power installations in Sweden have, on average, caused a significant and permanent reduction in unemployment within the municipalities where the turbines are located.

For the preferred specification with four treatment variables (three separate construction years and a post-installation period) and the fully-restricted subsample (no never-treated municipalities and only including municipalities' first wind power projects), the estimated treatment effects are the following. For the 122 municipalities with wind projects installed during the sample period, their first megawatt of installed wind capacity decreased unemployment by an average of 1.9 persons after the installation date. The median-sized first project (5 MW) thus corresponds to 9.57 individuals leaving unemployment, or a reduction of 0.85% in a municipality with wind power capacity and a median-sized unemployment level (1115 persons). For the construction phase, the median-sized first project reduced unemployment by roughly 4.2 persons in the first year of construction, 5.7 persons in the second year of construction, and 6.8 persons in the last year of construction (–0.37%, –0.51%, and –0.61%, respectively).

All model specifications and sample restrictions tell the same story, although the estimated magnitudes differ. As for the sample restrictions, the unrestricted sample provides estimates that are roughly half the magnitude of the fully-restricted sample. When only estimating the treatment effect from the first project within a municipality, the estimated treatment effect is around a third higher than the results from the fully-restricted sample. This could be due to a decreasing marginal effect from every additional wind project in a municipality. Alternatively, it could be due to heterogeneous effects over installation dates, where early projects had larger impacts per MW than recent projects due to, e.g., lower production costs. Moreover, by only excluding never-treated municipalities, the estimated impacts are more than a third bigger compared to the fully-restricted sample. This indicates a possible trend difference between treated and never-treated municipalities.

The regressions on the semi-restricted subsamples thus provide lower and upper bounds for the results: 0.74-2.66 marginal reduction in unemployment from one MW of wind energy capacity during O&M, 0.27-1.16 during the first year of construction, 0.40-1.54 during the first year of construction, and 0.66-1.80 during the last year of construction. For a median-sized project in a municipality with wind power capacity and a median-sized unemployment level, this corresponds to a reduction in unemployment of 3.7-13.3 persons (0.33%-1.2%) during O&M and 1.3-5.8 persons (0.12%-0.52%) during the first year of construction, 2.0-7.7 persons (0.18%-0.69%) during the second year of construction, and 3.3-9.0 persons (0.29%-0.81%) during the last year of construction. Assuming a project lifetime of 25 years, the median-sized project thus creates a net reduction of 99-355 person-years in unemployment in the municipality where the wind project is located.

The most notable difference to previous literature is that the impact of wind power investments increases in absolute value in relation to the installation date, and stabilizes after installation. In contrast, related papers that analyze dynamic effects have generally found a temporary impact during the construction phase which then decreases or vanishes during the post-installation period (e.g., Fabra et al. (2023) and Costa and Veiga (2021)). The different results could be due to country-specific effects through both the direct and indirect channels. For instance, the share of local workers among the directly employed is higher during O&M than during the construction period in Sweden (Swedish Energy Agency, 2022c). Although there is, to the best of my knowledge, no such data on the previously analyzed countries, there are reasons to believe that the share of local workers during O&M is greater in Sweden than, e.g., Spain, Portugal, and the USA. Since Sweden is more sparsely populated, commuting from other municipalities is relatively more expensive. Having workers travel from other municipalities for maintenance tasks might be prohibitively expensive in sparsely populated areas. However, travel costs might be less critical for construction since this is a labor-intensive phase where the developers can hire non-local workers by offering temporary accommodation close to the construction site. An alternative explanation is that the indirect effects (unemployment effects in other sectors) differ across the countries. There are several reasons why this could be the case, all rooted in the different labor market characteristics. However, given the available data, it is impossible to provide a satisfying answer to how different sectors are affected in Sweden on a monthly level. Therefore, future research could complement the monthly analysis with an annual analysis where sectoral data are available.

Previous literature has also found treatment effects with lower magnitudes (including some insignificant results). As the first study on a Northern European country, the different magnitudes from this paper and previous literature could be due to a larger impact in Sweden compared to other countries. However, most previous papers have used other metrics to measure the labor market effect, notably the employment effects. Hence, comparisons in levels are only valid if each reduction in unemployment causes an equal increase in employment, such that the workforce size is unaffected. This is not necessarily the case. One explanation for the seemingly different magnitudes is that the unemployment effects comprise changes in the workforce and employment levels. The latter arguably has a stronger theoretical basis. Nevertheless, the workforce size could also be affected if, for example, new wind energy projects cause emigration. Once again, answers could be revealed in future research by increasing the temporal unit of analysis (years), where data on other labor market metrics are available.

The heterogeneity analysis shows that no group is adversely affected by local wind power development. In other words, there is no local conflict of interest between socioeconomic groups in relation to the labor market when developing local wind power projects. However, some groups are not found to be affected at all, and some groups gain relatively more than others. The most notable difference across groups is between men and women. While men experience larger treatment effects in absolute value with significant results in all periods, women are only affected during the post-installation period. Similarly, unemployed with high education are not found to benefit from local wind energy investments. However, significant beneficial effects are found for those with lower education levels, especially among those with a gymnasium degree.

The relatively larger gains among men and those without higher education – which is also found in Portugal (Costa and Veiga, 2021) and Spain (Fabra et al., 2023) – bear high value for policy-makers who want to increase renewable energy. As men and lower educated persons are typically less favorable to new wind power projects in Sweden (Jönsson, 2022), wind energy proponents plausibly want to focus on swinging opinions among these groups. The results found in this paper could, therefore, facilitate renewable energy targets, given the strong influence of economic impacts in shaping sentiments towards new wind power projects (Slattery, Johnson, et al., 2012; Mulvaney, Woodson, and Prokopy, 2013; Caporale and De Lucia, 2015).

In addition, nearby municipalities are also not adversely affected. Hence, there is no regional conflict of interest either. On the contrary, municipalities that receive a wind project within ten kilometers of its border experience a reduction in unemployment of −0.3 units per MW after the installation. The lack of larger spillovers could be explained by a reluctance of regional migration and long-distance commuting. Moreover, the insignificant spillover effects for larger distances contrast the findings by Costa and Veiga (2021), who find effects on the 30 km distance in Portugal, but cohere with (Brown et al., 2012; Fabra et al., 2023), who do not find any spatial spillovers in the U.S. and Spain, respectively. Note that the smallest analyzed spillover distance bin in previous literature is 30 km. Therefore, it is possible that effects within smaller distances could be present in the previously studied regions.

The results' internal validity is most questionable regarding the potentially heterogeneous treatment effects. If the treatment effects either revert or grow as the wind project grows older, this is policy-relevant in itself as it affects the total lifetime labor market impact of a wind power project. However, it would invalidate the regression results from the DiD models. Several measures are made to limit and investigate this potential bias in the regressions, especially for the construction period. Moreover, the unbiased LP-DiD estimator finds significant treatment effects and suggests that the treatment effect plateaus after the installation date. However, the large confidence interval in the LP-DiD results, which increases with the age of a project, does not allow one to rule out dynamic effects during O&M. Therefore, future research should further analyze and investigate if there are more suitable econometric techniques for this type of research setting. Considering the increasing number of papers that addresses dynamic treatment effects in DiD models, it is plausible that new and improved statistical techniques will be developed in the months and years to come and that the validity of future related literature could be enhanced.

As for the external validity, the net labor market impacts analyzed in this paper concern Swedish municipalities for wind power installed during 2003-2022. As discussed above, there are reasons to believe that the effects differ in other countries. For regions with similar labor market characteristics, geographical conditions, and energy systems – such as the Nordic countries – it is plausible that the results are similar. However, as this is the first study of its kind on a northern European country, it is currently not possible to assess this claim. As the local economic impacts of renewable energy deployment are analyzed in additional regions, it would be possible to assess if there are systematic differences across countries and what these potential differences could stem from.

Furthermore, it is not possible to know if past projects' impacts can be extrapolated to future projects. Notably, wind energy has experienced a substantial cost reduction over the last decades, which is likely to continue into the future (IEA, 2023). Additionally, the labor market impacts from wind power might be associated with diminishing returns. In turn, this implies that the direct impact (employment, revenue streams) per MW could be reduced in the future. Moreover, turbines are set to become more complex, which might require workers with special education for both construction and O&M, who might live in other municipalities to a larger extent. Indeed, the robustness checks with temporal splits show that the treatment effect increases substantially in absolute value when only including the first years of the sample. The estimated treatment effects even shift signs for the post-2017 sample.[1] Therefore, the main results should not be extrapolated into

[1]Note, however, that the single treatment sample restriction limits the post-2017 sample to only 47 municipalities. With the unrestricted sample (table A.3), the estimated treatment effects are still decreasing in absolute value over time. However, the post-2017 sample now has significant and negative impacts in the

the future. On the other hand, projects and turbines are becoming bigger. And in a few years' time, the first large-scale decommissioning of wind turbines will also occur (Swedish Energy Agency, 2022d). Consequently, even though the marginal impact per MW is decreasing, wind energy as a source of local employment could persist for years.

Lastly, another venture for future research is to examine how the ownership structure affects a wind power project's economic impact in the municipality where the park and the owner(s) are located. Notably, the owners are often located in different municipalities or countries as the projects. This might especially be the case for large projects. In such cases, private revenues and tax revenues are collected in other regions or countries, thereby reducing the local economic effect. By separately analyzing locally-owned projects, a better understanding of the economic development potential can be developed.

O&M phase.

9 Conclusion

This thesis is the first to analyze the labor market impacts of wind energy investments on the municipal level in Sweden. The empirical analysis suggests that local communities are, on average, beneficially impacted by wind energy and that these effects persist beyond the construction phase. This finding is important for decision-makers, investors, and individuals who want to make informed choices regarding the green energy transition.

Looking ahead, the (Swedish Energy Agency, 2023) expects a doubling of the electricity demand in Sweden until 2035. To satisfy this demand, they identify onshore wind power as having the greatest economic and technological capability. However, securing public acceptance is a crucial prerequisite for this Swedish Energy Agency (2023). Notably, the economic impacts are a major factor in shaping personal sentiment towards new wind turbines, and public acceptance is a de facto prerequisite for new wind energy in many countries. Therefore, this type of finding could prove important for achieving renewable energy targets.

However, the local labor market impacts are not the sole driver of sentiments toward energy sources. Likewise, the strictly local impacts are not the only impacts of interest. In the transition toward a fossil-free energy system, it is important that a broad spectrum of issues is considered, including further economic variables, climate change, biodiversity, energy security, environmental justice, human rights, health, and much more. Future research can improve the understanding of how energy transitions affect different communities – in both positive and negative ways – through new research and by improving statistical techniques suitable for these analyses. After this, the trade-offs are ultimately a question for policymakers, businesses, and voters, with far-reaching consequences for decades to come.

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A Further results and sensitivity analyses

A.1 Four construction years

In the main results, three construction years are used. Table A.1 presents the regression output for annual construction indicators for one, two, three, and four construction years. In all regressions, the last three construction years are significant. However, as shown in the last column, the effects are only significant for three years prior to the installation date. Therefore, three construction years are deemed suitable for the main regression. Note that these regressions are similar to a placebo-in-time test, where "anticipation" is found for three years prior to the installation date, but not more.

Table A.1: Regression results, different number of construction years

	Unemployment per capita$_{i,t}$			
Construction years	1	2	3	4
$w_{i,t}$	-1.644**	-1.789**	-1.914**	-1.924**
	(0.003)	(0.003)	(0.003)	(0.006)
$\sum_{h=37}^{48} \Delta w_{i,t+h}$				-0.191
				(0.464)
$\sum_{h=25}^{36} \Delta w_{i,t+h}$			-0.833*	-0.869*
			(0.023)	(0.023)
$\sum_{h=13}^{24} \Delta w_{i,t+h}$		-1.046**	-1.142**	-1.214**
		(0.006)	(0.006)	(0.005)
$\sum_{h=1}^{12} \Delta w_{i,t+h}$	-1.159**	-1.268**	-1.365**	-1.396**
	(0.007)	(0.006)	(0.005)	(0.006)
$popdensity_{i,t-36}$ ($\times 10^{-3}$)	0.105*	0.100*	0.097*	0.096*
	(0.036)	(0.042)	(0.047)	(0.048)
R^2	0.451	0.457	0.462	0.444
Municipalities	122	122	122	122
Observations	14900	14900	14900	14409

p-values in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Regression results using equation 5.3 for the fully-restricted sample and disparate number of construction years. The dependent variable (absolute unemployment) and w (installed capacity) are normalized by the population as of December 2002. Municipality- and month-fixed effects included.

A.2 Including outlier municipalities

In the main results, Åsele and Ockelbo are omitted due to outlier treatment observations. Table A.2 shows the regression results when including all 290 municipalities for different sample restrictions. The regression output show that the treatment effects become slightly lower and have lower significance levels when including Åsele and Ockelbo. Still, the main result of beneficial labor market impacts is robust to the inclusion of the outlier municipalities. One explanation for the changes in results is that the outlier treatment observations are inflicted late in the sample. Since this outlier observation has very large weights in calculating the ATT, the overall weights of the corresponding months also increase. Indeed, as shown in tables 7.6 and 7.2, the treatment effect has been reduced over time (in the baseline sample, excluding outliers).

Table A.2: Regression results, including outliers

	Unemployment per capita$_{it}$			
	(1)	(2)	(3)	(4)
w_{it}	-0.716**	-2.315***	-0.538*	-1.750**
	(0.010)	(0.000)	(0.041)	(0.002)
$\sum_{h=25}^{36} \Delta w_{it+h}$	-0.121	-0.798*	-0.077	-0.574
	(0.177)	(0.036)	(0.312)	(0.120)
$\sum_{h=13}^{24} \Delta w_{it+h}$	-0.225	-1.025*	-0.146	-0.755
	(0.183)	(0.016)	(0.359)	(0.065)
$\sum_{h=1}^{12} \Delta w_{it+h}$	-0.373	-1.233*	-0.279	-0.952*
	(0.184)	(0.011)	(0.293)	(0.041)
$popdensity_{it-36}$ (10^{-3})	0.038***	0.033***	0.150***	0.130*
	(0.000)	(0.000)	(0.000)	(0.024)
R^2	0.333	0.341	0.429	0.516
Pure control included	Yes	Yse	No	No
Multiple treatments allowed	Yes	No	Yes	No
Municipalities	290	285	151	146
Observations	59125	46337	30804	18016

p-values in parentheses

Standard errors clustered by municipality.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Regression results using equation 5.3 for the fully-restricted sample. The dependent variable (absolute unemployment) and w (installed capacity) are normalized by the population as of December 2002. Municipality- and month-fixed effects included.

A.3 Yearly splits with the semi-restricted subsample

When including only the first wind power project in each municipality, the regressions on subsamples of recent years include few (eight) newly treated municipalities, since most municipalities have already been treated more than once at this point. Therefore, it is useful to relax the *no multiple treatment* sample restriction. This is done in table A.3. Similar to the results in table 7.2, the significance level and magnitude of the treatment effect are reduced in most periods, compared to the fully restricted sample. For the last column, post-2017, the treatment effects now have the same sign as the main results (negative) for the O&M treatment effects. No effect is found for the construction period in the post-2017 sample, and only for the last year(s) in the post-2007 and post-2012 samples.

Table A.3: Regression results, yearly splits with semi-restricted subsample

	Unemployment per capita$_{i,t}$						
		2007		2012		2017	
	All	$\leq$	$>$	$\leq$	$>$	$\leq$	$>$
$w_{i,t}$	-0.743*	-4.160	-0.677**	-0.766	-0.675***	-0.619*	-0.453**
	(0.012)	(0.138)	(0.004)	(0.058)	(0.000)	(0.043)	(0.009)
$\sum_{h=25}^{36} \Delta w_{i,t+h}$	-0.272*	-1.125***	-0.199	-0.500*	-0.006	-0.567***	-0.028
	(0.025)	(0.000)	(0.086)	(0.028)	(0.925)	(0.001)	(0.444)
$\sum_{h=13}^{24} \Delta w_{i,t+h}$	-0.403**	-1.223**	-0.320*	-0.236	-0.084	-0.615**	0.004
	(0.003)	(0.007)	(0.019)	(0.127)	(0.469)	(0.003)	(0.980)
$\sum_{h=1}^{12} \Delta w_{i,t+h}$	-0.655**	-2.645**	-0.528**	-0.013	-0.345*	-0.508*	-0.204
	(0.001)	(0.005)	(0.004)	(0.944)	(0.011)	(0.020)	(0.143)
$popdensity_{i,t-36}$ ($\times 10^{-3}$)	0.169*	0.228*	0.123	0.289*	0.072	0.186*	0.374
	(0.045)	(0.044)	(0.118)	(0.013)	(0.425)	(0.037)	(0.063)
R^2	0.414	0.558	0.495	0.488	0.534	0.368	0.507
Municipalities	175	175	175	175	175	175	175
Observations	35700	10500	25200	21000	14700	31500	4200

p-values in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Standard errors clustered by municipality.

Regression results using equation 5.3 for different periods. All regressions use the semi-restricted subsample (no pure control, but including multiple treatments per municipality), and include month- and municipality-fixed effects. Under each year, the column to the left (≤) is the sample prior to and including that year, and the column to the right (>) is the sample for all months after that year. The dependent variable (unemployment) and w (installed capacity) are normalized by the population as of December 2002.

A.4 Alternative event studies

Figures A.1 and A.2 show event studies for the semi-restricted samples and for a longer pre-construction period, respectively. The pre-trends are stable at around zero in all figures. This provides further credibility to the parallel trends assumption.

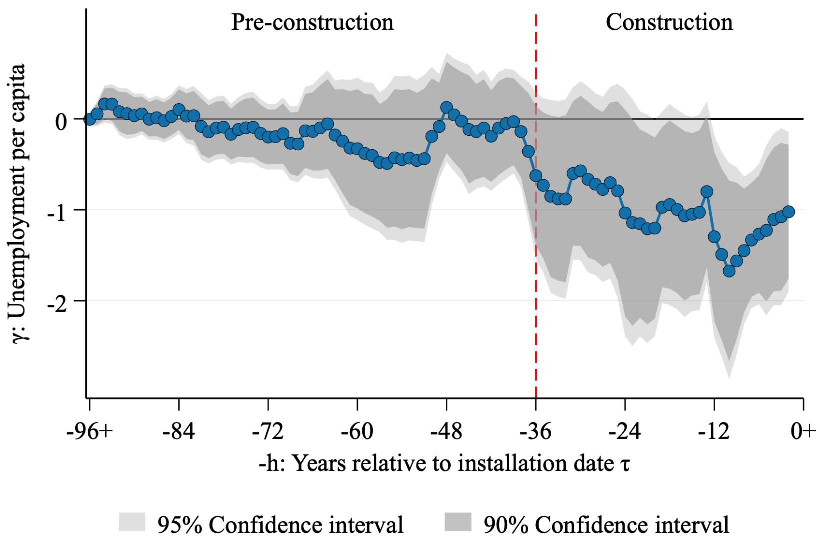
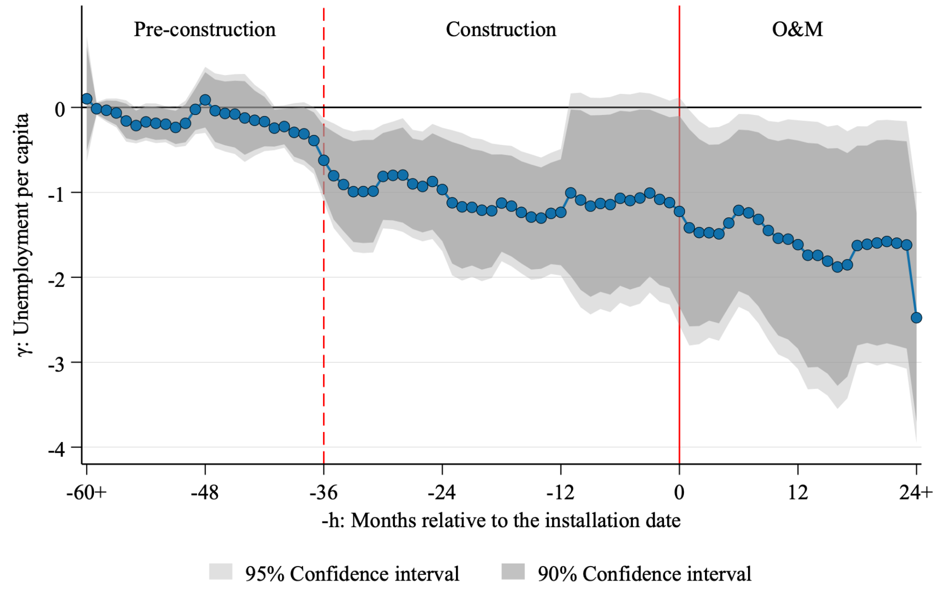
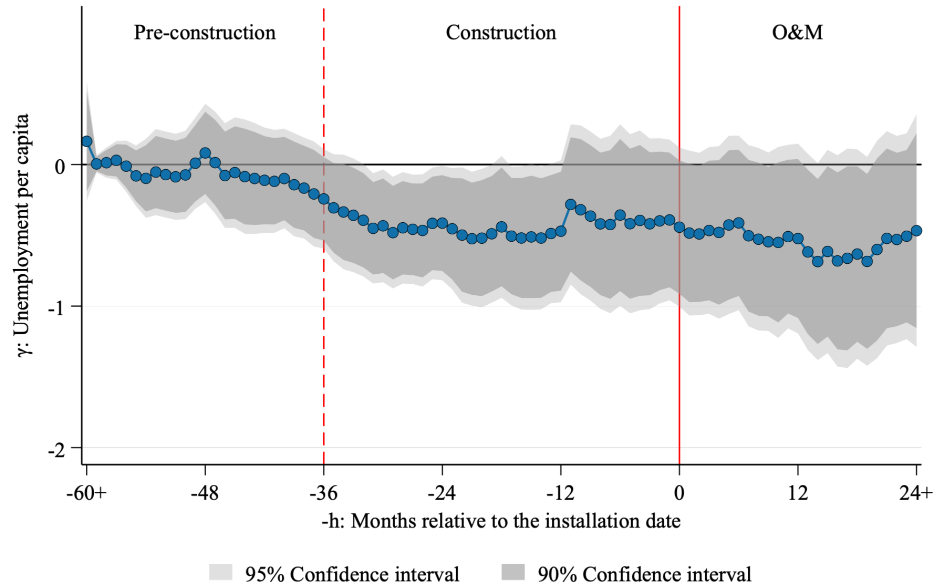


Figure A.1: Event study with a longer pre-treatment period (fully-restricted sample)
The figure map coefficients and confidence intervals of the lead and lags of added installed capacity per capita. Never-treated municipalities and observations three years prior to the installation date of the second project are excluded. The leads restrict the observation window for unemployment to 2003-2014. The estimated coefficients γ_{-h} represent the change in unemployment per capita at $-h$ periods from the installation of one MW per capita. The rightmost estimate (0+) represents the average effect from all already installed capacities. Likewise, the leftmost estimate (-96+) represents the average effect from all capacity installed between t+96 to December 2022. The per capita terms are normalized for the population in the year prior to the main sample (December 2002).



(a) No multiple treatments (0-1 treatments)

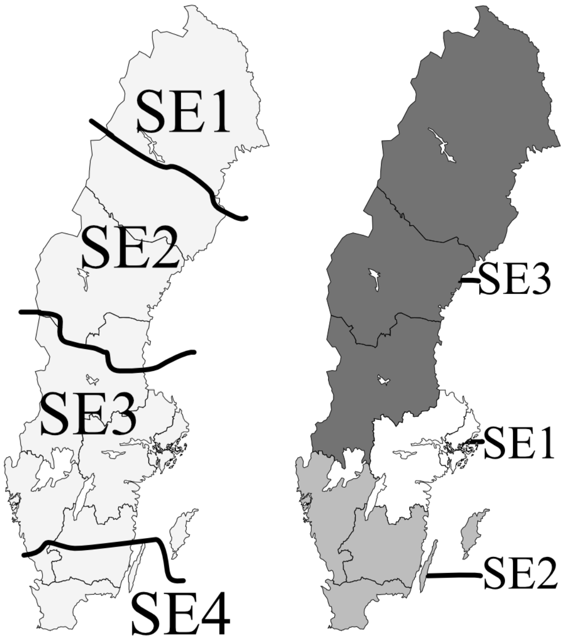


(b) No pure control (>0 treatments)

Figure A.2: Event study with semi-restricted samples

The figures map coefficients and confidence intervals of the lead and lags of added installed capacity per capita. Panel (a) excludes observations three years prior to the installation date of the second project. Panel (b) excludes municipalities without wind power capacity in the final period. The leads restrict the observation window for unemployment to 2003-2017. The estimated coefficients γ_{-h} represent the change in unemployment per capita at $-h$ periods from the installation of one MW per capita. The per capita terms are normalized for the population as of December 2002. Note the different scales on the y-axes.

B Additional maps



(a) Bidding zones (b) NUTS1

Figure B.1: Electricity market areas and administrative regions in Sweden
Panel (a) displays the four bidding zones in Sweden. The bidding zone delineations are approximations and drawn by the author using maps from Swedish Energy Agency and County Administrative Boards of Sweden (2023). Panel (b) displays the NUTS1 regions (colored and in text) and NUTS2 regions (borders) as defined by Eurostat (2023d). Maps created by the author.