STOCKHOLM SCHOOL OF ECONOMICS
Department of Economics
BE551 Degree Project in Economics
Spring 2025

# Assessing the Local Economic Impact of Renewable Energy: Evidence from Swedish Wind Power Projects

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**Abstract**

Sweden's expansion of wind energy has faced growing municipal resistance, often rooted in concerns over limited local benefits, compared to potential environmental costs. This paper investigates whether wind power projects generate tangible socio-economic gains for host municipalities, focusing on income proxies and labour market indicators. We analyse data on wind farms constructed between 2014 and 2019, using modern difference-in-differences approaches that apply recent methodological innovations. Specifically, we implement estimators developed by Sun & Abraham (2021) and Arkhangelsky et al. (2021). Across all outcome variables, we find no statistically significant effects. These results suggest that current wind development practices may not deliver the local economic benefits often assumed in policy discourse, highlighting the need to better align national energy goals with community-level outcomes.

Key words: Energy transition, Wind power, Local economic effects, Difference-in-differences
JEL: C230, Q430, Q480

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Date Submitted: May 12$^{th}$, 2025
Date examined: May 26$^{th}$, 2025
Discussants: David Gadestam Ferraro & Emil Tallberg
Examiner: Johanna Wallenius

# Acknowledgements

We would like to warmly thank our supervisor, Elena Paltseva, for all her support, advice, and encouragement along the way. An additional thank you to Nikhilesh Pra kash, for taking his time to support us when we needed it the most. Finally, we thank Matheus Facure Alves for his clear explanations of causal inference methods, which helped us select appropriate models and deepen our understanding of the econometric techniques. And of course, thank you to all our friends and family for always believing in us.

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# 1. Introduction

Following the liberalisation of Sweden's electricity market in the 1990s, a country historically reliant on hydro-, and nuclear power pivoted decisively toward large-scale onshore wind power (Åstrand & Neij, 2004). Total installed capacity expanded from below 600 turbines in 2002 to 5,164 by the end of 2022, a nine-fold increase that positions Sweden among global leaders in wind energy (World Population Review, 2025). Public sentiment remains highly supportive of wind power expansion: the 2023 National SOM Survey reports that 82% of Swedes express positive attitudes toward wind energy (Svensk Vindenergi, 2023), comparable to Germany (81%) (FA Wind, 2024), but substantially higher than Norway (36%) (Kaltenborn, Keller, & Krange, 2023).

While national-level support is high, local wind power opposition increases with proximity to the wind turbines (Lindvall, Sörqvist, & Barthel, 2024). With the implementation of a municipal veto right in 2009, municipalities can block wind power developments within their jurisdiction without providing specific justification. In 2021, a total of 77% of proposed wind projects were denied, with 43% rejected explicitly due to lack of municipal support, even as national climate targets reaffirmed an urgent need for renewable expansion (Westander & Henryson, 2023). This fragmentation risks undermining Sweden's decarbonisation strategy.

While popular accounts often attribute resistance to 'Not-In-My-Backyard' (NIMBY) sentiments, recent evidence suggests a more complex reality. Concerns over distributional justice, such as limited local economic benefits, uncertain employment impacts, and the absence of formal compensation from investors to local communities, appear central to opposition (Lindvall, 2023). However, despite a growing branch of qualitative literature, causal evidence on whether wind power delivers tangible socio-economic gains to host municipalities remains scarce.

As this discussion becomes increasingly entrenched, we focus our study on the socio-economic benefits of wind power for local communities, to advance the discussion through an economic line of argumentation, hoping this approach can generate more societal support for wind power, specifically in the affected local communities. Through this, we define our main research question as follows:

*Do wind power projects have a positive socio-economic effect on local municipalities?*

To answer this question, we study wind power projects constructed between 2014 and 2019, and, using a modern difference-in-differences approach, focus on changes in municipal-level income proxies and labour market outcomes.

Our identification strategy combines two recent advancements in quasi-experimental estimation. First, we implement a staggered-adoption difference-in-differences design using the interaction-weighted estimator of Sun & Abraham (2021), which corrects for biases from heterogeneous treatment timing. Second, we apply the synthetic difference-in-differences (sDiD) estimator of Arkhangelsky et al. (2021) to relax the parallel trends assumption and optimise pre-treatment balance. Together, these methods ensure robust inference under plausible deviations from classical difference-in-differences assumptions.

Our findings suggest no statistically significant effects of constructed wind power on local financial assistance, job vacancies, or the unemployment rate. On a county level, we observe a moderate positive correlation between income inequality and the share of wind power rejections.

The following section offers contextual background on wind power development in Sweden, including public opinions, the application process, and the municipal veto. The subsequent sections of this paper are organised as follows: Section 3 reviews relevant literature; Section 4 presents the empirical methodology, including the identification approach and robustness checks; Section 5 describes the data sources and construction of the main outcome variables; Section 6 reports the main results; Section 7 discusses their implications; and Section 8 concludes. Additional material is provided in the Appendix.

# 2. Background

## 2.1 History and Development of Wind Power in Sweden

The development of wind power in Sweden traces back to the aftermath of the 1973-1974 oil crisis, which prompted the Swedish government to initiate a national wind power programme in 1974. The state-owned energy company Vattenfall was tasked with assessing potential sites for wind turbine installations. Early investigations concluded that extensive research and development would be necessary before wind power could be deemed a viable energy source. In 1977, Sweden's first experimental wind power facility was established at Kalkugnen near Älvkarleby, aimed at collecting data on performance and design requirements. A few years later, a second facility was built on Gotland, which featured one of the world's largest turbines at the time, with a capacity of 2 MW (Vattenfall, 2015).

Despite these early initiatives, interest in wind power waned during the 1980s. Following the referendum on nuclear power and the decision to eventually phase it out, Swedish energy policy pivoted toward established technologies such as thermal power using coal, peat, natural gas, and bioenergy. As a result, while countries like Denmark, Germany, and the United States made significant progress in wind energy during this period, Sweden lagged behind (Vattenfall, 2015).

Renewed interest in wind power emerged in the 1990s, driven by increasing public concern over climate change and fossil fuel dependence. In 1991, the Swedish government introduced investment subsidies for wind turbine installations, which spurred the construction of additional turbines along the coastline and revitalised earlier sites like on Gotland (Vattenfall, 2015). The sector gained further momentum in 2003 with the introduction of a green certificate system. Under this scheme, producers of renewable electricity received certificates for each MWh generated, which could be sold to electricity suppliers obligated to meet renewable energy quotas (Kooij et al., 2018). This provided a crucial revenue stream, making wind power more commercially viable. The system later expanded into a joint Swedish-Norwegian market, with a shared target of 46 TWh of new renewable energy production by 2030, a goal that was exceeded by 2021. As a result, Swedish authorities decided to phase out the certificate system, with no new projects eligible after 2021 and full termination planned by 2035. Future onshore expansions are expected to be driven by market forces rather than direct subsidies (Niskanen, Haikola, Magnusson, & Anshelm, 2024).

In 2021, Sweden adopted the *National Strategy for Sustainable Wind Power Expansion*, aiming to increase wind power production to 100 TWh by the 2040s, with approximately 80 TWh expected from onshore installations (Energimyndigheten, 2021). By 2022, wind power generated 33 TWh, accounting for about 19% of Sweden's electricity production, compared to 45% from hydropower and 29% from nuclear power (Energimyndigheten, 2023).

The Swedish state has designated specific "national areas of interest" for wind power development - geographic areas particularly suitable for future wind power expansion, although not yet actively utilised. While similar spatial planning mechanisms have effectively accelerated wind power deployment in countries such as Germany, their impact in Sweden has thus far been limited (Lauf, Ek, Gawel, Lehmann, & Söderholm, 2020). Recent technological advancements, particularly taller turbine towers, have broadened the number of regions suitable for wind development. As of 2024, 284 designated areas covering around 1.5% of Sweden's land area have been identified as priorities for future wind projects (Bjärstig, Mancheva, Zachrisson, Neumann, & Svensson, 2022).

Despite strong growth prospects, the permitting process remains a bottleneck. *Svensk Vindenergi* projects that Swedish wind power production will rise from 27.1 TWh in 2021 to 52.5 TWh in late 2025. However, to meet this target, around 274 new turbines per year are required, far exceeding the recent approval rate of 125 turbines annually. Delays are primarily due to high rejection and scaling-down rates during the permitting process, rather than a lack of developer interest (Westander & Henryson, 2023). These national challenges come at a time when wind power is also gaining momentum at the European level. Under the *Repower EU* and *Fit for 55* initiatives, the European Union aims to double wind-generated electricity between 2020 and 2030, positioning wind energy as a central pillar of its climate and energy objectives. This broader European push underscores the importance of overcoming domestic barriers to accelerate wind power deployment in Sweden in line with both national and EU-wide energy transition goals (European Comission, 2022).

Figure 1 below shows the development of wind power in Sweden between 2013 and 2023, with forecasted values for 2024-2026. The red line represents the number of wind turbines, the yellow line represents the installed capacity (MW), and the blue line represents actual production (GWh). (Svensk Vindenergi, 2024)

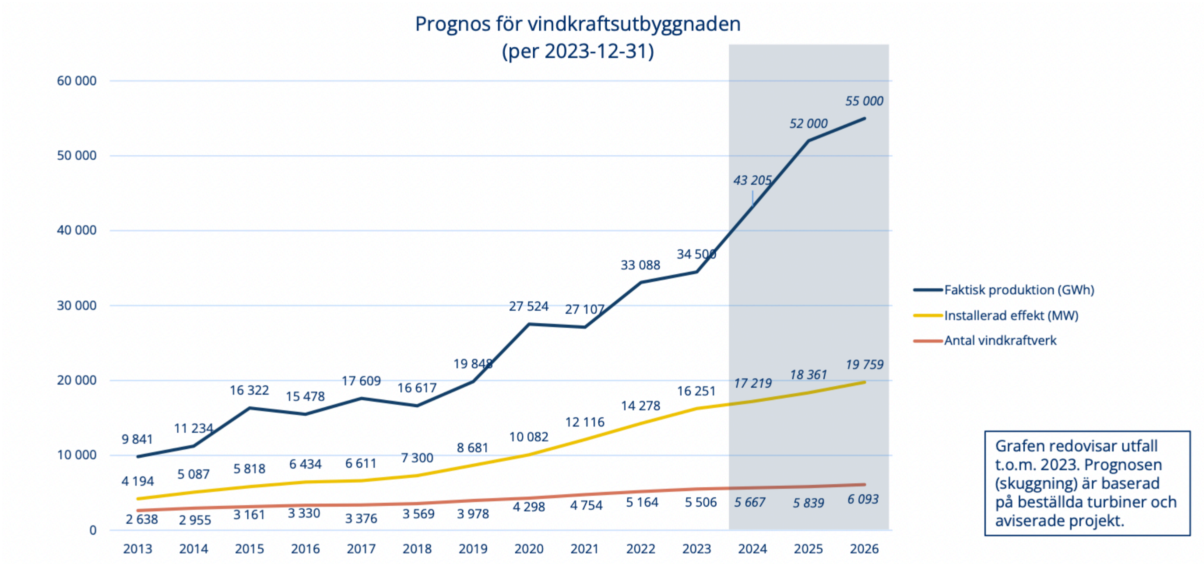


Figure 1 - Historic Development and Forecast of the Wind Power Development in Sweden 2013-2026.
*The blue line represents actual production (GWh), the yellow line represents installed capacity (MW), and the red line represents the number of wind turbines. Graph extracted from (Svensk Vindenergi, 2024).*

## 2.2 Opinions and Arguments For and Against Wind Power

Resistance to large-scale wind power development in Sweden is part of a broader historical pattern in which new energy technologies have faced environmental and social opposition (Anshelm & Simon, 2016). Understanding how past conflicts over hydropower and nuclear power unfolded helps to explain current debates about wind energy.

In the mid-20th century, plans to expand hydropower met growing opposition from conservationists, tourism advocates, and environmental groups. The battle over the Vindel River in the 1960s, driven by aesthetic, scientific and ecological concerns, marked Sweden's largest environmental conflict in history. Ultimately, political consensus led to legal protections for many rivers, limiting further hydropower expansion. Similarly, nuclear power faced resistance on different grounds. Fears over radioactive waste and reactor safety, intensified by global accidents like Harrisburg and Chernobyl, fueled protests and culminated in a referendum to phase out nuclear energy. Although this policy was later relaxed, both hydropower and nuclear expansion remain constrained. (Anshelm & Simon, 2016)

These examples illustrate how resistance to new energy technologies is not unique to wind power, but the underlying motivations and patterns have evolved. When wind power gained traction in the 1970s and 80s, two main objections emerged: concerns over low capacity and the degradation of visual landscapes. As technological improvements and policy incentives (like green certificates) led to large-scale expansions in the 2000s, local resistance intensified. Although experts identified few serious environmental harms, local groups raised concerns about bird and bat safety, human stress near turbines, and the devaluation of local knowledge versus expert claims (Anshelm & Simon, 2016).

Today, wind power opposition in Sweden revolves around both environmental and socio-economic arguments (Zerrahn, 2017). A central issue is distributive justice: while urban populations benefit from clean energy, rural communities often bear the environmental and social costs with limited compensation. This has fueled debates about fairness and democratic decision-making, as well as compensation agreements. By contrast, countries like Germany and Denmark have reduced opposition through cooperative ownership models, enhancing local participation and acceptance (Anshelm & Simon, 2016).

Despite these tensions, public opinion on wind power remains largely positive. As of 2023, 82% of Swedes express positive attitudes toward wind energy. However, opposition spikes when projects are sited near residential areas - a classic "not in my backyard" (NIMBY) pattern (Svensk Vindenergi, 2023). For example, 26% opposed turbines within five kilometres of their homes, though only 15% opposed wind power as a general strategy (Lindvall, Sörqvist, & Barthel, 2024). Importantly, opposition is more complex than simple NIMBYism (Wolsink, 2007). Socio-economic factors play a key role - individuals who benefit economically from wind power tend to be less concerned about its downsides. When profits flow mainly to national governments or international corporations, wind turbines can symbolise energy injustice and resource exploitation (Jenkins, McCauley, Heffron, Stephan, & Rehner, 2016). Further, informal compensation agreements are sometimes perceived as bribes, reinforcing local mistrust (Walker, Duncan, & Kurz, 2017). Regional differences also shape attitudes: in southern Sweden, opposition often focuses on aesthetic concerns, while in the north, fairness and justice dominate. Northern municipalities, with histories of economic deprivation and past hydropower exploitation, often view wind power as another example of harmful resource extraction from their territories (Lindvall, 2023).

## 2.3 Application Process and the Municipal Veto

The development of new onshore wind power projects in Sweden follows a structured, multi-stage permitting process designed to balance renewable energy expansion with environmental protection and societal interests (Svensk Vindenergi, 2024). Project developers begin by conducting feasibility studies and securing preliminary agreements with landowners. This is followed by a formal consultation process, regulated under Chapter 6 of the Environmental Code (Miljöbalk (1998:808)), where an Environmental Impact Assessment (EIA) is prepared. The County Administrative Board oversees this process and ensures that the EIA addresses all relevant environmental and societal factors.

Once the consultation is complete, the developer submits a formal permit application to the Environmental Assessment Delegation (Miljöprövningsdelegationen, MPD), accompanied by the EIA and consultation report. The MPD evaluates the project's environmental impact and compliance with legal and regulatory requirements. Its decision can be appealed to the Land and Environmental Court and, subsequently, to the Land and Environmental Court of Appeal (Svensk Vindenergi, 2024).

A pivotal element of the permitting process is the municipal approval, commonly referred to as the municipal veto. Introduced in 2009, this provision under Chapter 16, Section 4 of the Environmental Code (Klimat- och näringslivsdepartementet, 2009) gives municipalities the authority to approve or block wind power projects within their jurisdiction. The only exception is when the Swedish national government intervenes and grants approval. The municipality's decision is required before the MPD can issue a final permit. Importantly, municipalities are not legally obligated to provide reasoning for their decisions, and their stance is not subject to appeal (Svensk Vindenergi, 2024). They can also change their position at any time before the final permit decision (Mark- och miljödomstolen Mål: M 5427-22, 2023). While this ensures that local governments have a decisive role in determining land use, it creates considerable uncertainty for project developers, who may have already invested substantial time and resources before municipal approval is finalised. Criticism of the current framework centres on this unpredictability and the financial risks it imposes on developers if approval is withdrawn late in the process (Svensk Vindenergi, 2023).

Between 2014 and 2022, 53% of rejected wind power projects were denied due to the municipal veto, making it the most common reason for rejection. Figure 2 shows the share of rejected wind power projects that were denied because of the municipal veto per year in the period.

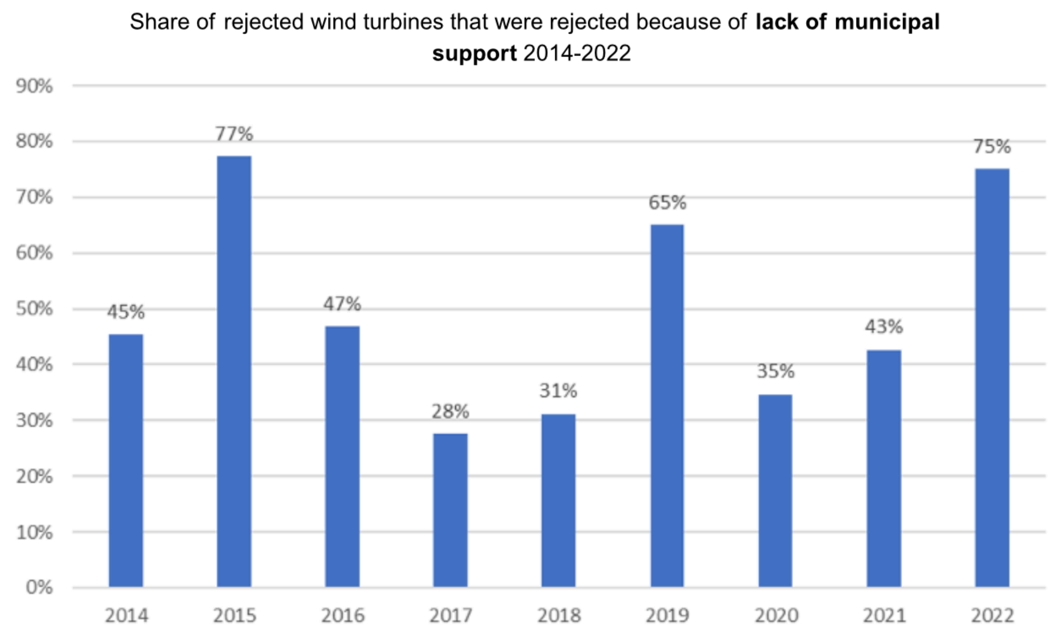


Figure 2 - Share of Rejected Projects that were Rejected Due to Lack of Municipal Support (the municipal veto) 2014-2022. *Graph extracted from (Westander & Henryson, 2023), translated to English.*

However, the municipal veto is not the only barrier. Other frequent grounds for rejection include: the protection of landscapes and outdoor recreation areas; anticipated disturbances for nearby residents; national defence or military interests; potential impacts on reindeer herding; and species or nature conservation concerns. Some projects are also rejected under technical provisions, such as the "box model", where applications outline a broad area with turbine details finalised later or dismissed without a specified reason. Across both full projects and individual turbine applications, the municipal veto remains the dominant cause of rejection (Westander & Henryson, 2023).

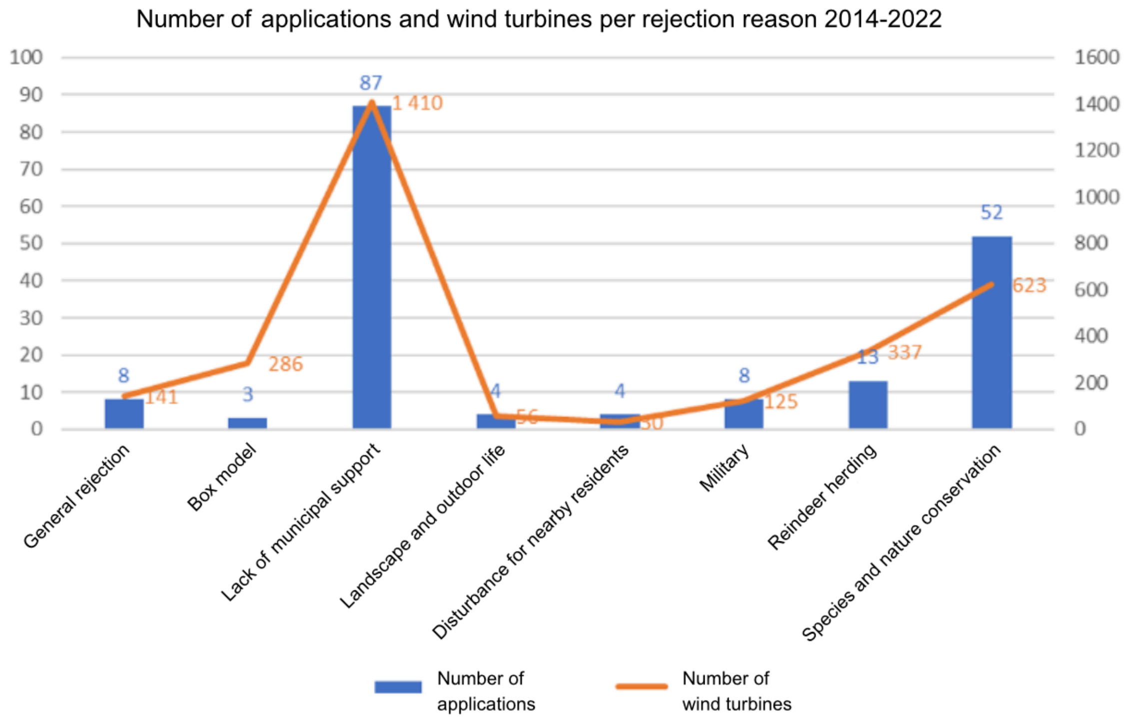


Figure 3 - Number of Applications and Wind Turbines per Rejection Reason 2014-2022.
*Graph extracted from (Westander & Henryson, 2023), translated to English.*

# 3. Literature Review

While a considerable amount of research has focussed on technical assessments of wind turbines and the macroeconomic effects of wind energy on sustainability, energy security, and energy pricing, a growing number of studies examine the local economic impacts of wind power development, often with mixed results. This section reviews relevant contributions in economics and research on public attitudes toward wind energy, highlighting how our study builds on and extends previous research.

## 3.1 Socio-Economic Impacts of Wind Power

Recent studies have examined the local impacts of wind power investments, applying a range of empirical strategies across geographic and temporal settings. This research examines both purely economic effects, as well as socio-economic effects relating to well-being and health. For instance, Karasmanaki (2022) highlights turbine noise and related health impacts, which have become more central to ongoing public debates and local discourse (Lindvall, 2023). Other studies examine more direct economic effects. Westlund & Wilhelmsson (2021) find negative economic effects of wind turbines through capitalisation on local property values. Specifically, properties located within an approximate eight-kilometre radius of a wind turbine, experienced a significant decline in value, with greater reductions observed for taller turbines and larger wind parks on lower-value properties. To explore the socio-economic dimensions of local wind power developments, Costa and Veiga (2020) investigate the labour market effects of wind energy investments in Portuguese municipalities between 1997 and 2017, using a two-way fixed effects (TWFE) model. Their findings indicate a decline in unemployment during the first and second years of construction, with effects particularly pronounced among men with lower levels of education. The authors also document a short-run decrease in overall municipal unemployment, suggesting positive spillovers to other industries during wind project construction. Lastly, they find short-, and long-run positive effects on municipal revenues through increases in property and tax income.

When examining wind power development as a driver for local development, one important contribution is made by Duarte et al. (2021). In their study, the authors explore the impact of wind power projects on rural development, analysing post-construction impacts in a Spanish municipality between 2006 and 2019. Employing the synthetic control method (SCM), Duarte et al. assess various indicators of rural development, including population trends, gross value added, and human capital proxies. Their results suggest that wind power projects did not halt depopulation, nor did they attract new businesses or significantly increase value-added per capita in the short run. However, in the long run, the treated municipality experienced higher growth in gross value added relative to its synthetic control, though employment effects remained statistically insignificant.

A second relevant contribution is by Lundin and von Platten (2024), who analysed the relationship between major wind power projects in Sweden between 2010 and 2015 and patterns of socio-economic inequalities. Using a logit estimator and proximity to turbines (within a 0-2 km radius) as a proxy for exposure, they track socio-economic variables, including education and earnings, over a 15-year period, comprising ten years prior to and five years post construction. Their findings indicate that individuals residing near wind power installations tend to have lower income and levels of education; however, these differences are largely driven by persistent urban-rural heterogeneity and remain stable over time. Additionally, the authors investigate patterns of project rejection, showing that wind power proposals are more likely to be rejected in municipalities with higher average income and education levels.

Our study builds on and extends these contributions by incorporating more modern methodological approaches and extending the scope of analysis. Methodologically, we advance the empirical strategy by employing both the Sun & Abraham (2021) estimator for heterogeneous treatment effects and a synthetic difference-in-differences (sDiD) approach, in addition to the conventional TWFE model. This allows us to address potential biases more robustly and study staggered treatment timing. Second, in contrast to (Duarte, García-Riazuelo, Sáez, & Sarasa, 2021), who focussed on a single rural municipality in Spain, we conduct a country-wide analysis of Sweden, encompassing both urban and rural municipalities. This broader geographical scope allows us to provide more nationally representative evidence on the socio-economic impacts of wind power expansion. Third, while we are constrained by limited accessibility of proprietary data, our study introduces greater temporal granularity, by using monthly municipal data instead of yearly indicators. Lastly, in a minor sub-study, we seek to contextualise the economic dimensions of local wind power resistance, by exploring the correlation of economic inequalities in Swedish counties with their rejection shares of proposed wind power projects.

## 3.2 Public Opinions

Beyond studies on the direct and indirect economic impacts of wind power, a substantial body of literature examines public opinion and local decision-making power, especially relevant in Sweden, where municipal vetoes play a decisive role. Several recent contributions in the Nordic context highlight how diverse stakeholder perspectives shape attitudes toward wind power development.

Niskanen et al. (2024) examine Swedish national and local news and press coverage between 2009 and 2022, highlighting the tension between national energy policy and local implementation. While national policy, driven by market-oriented mechanisms, increasingly supported wind power expansion, local opposition has intensified over time. The authors document how local opposition evolved from fragmented NIMBY (“not-in-my-backyard”) concerns to broader principled resistance, framed as “not-in-anyone’s-backyard.” This shift, increasingly adopted by right-wing political parties advocating nuclear-friendly policies, elevated wind resistance into a salient national political issue. However, the energy crisis following Russia's invasion of Ukraine prompted some municipalities to soften opposition, citing energy security concerns.

Lindvall (2023) explores how geographic concentration and perceived benefits shape municipal acceptance of wind projects in Sweden. Through interviews with municipal decision-makers, Lindvall finds that the lack of formal compensation mechanisms and limited perceived benefits provide little incentive for municipalities to approve projects, especially when potential social or environmental disruptions loom large. Regional variation emerges: in the north, where turbines are more concentrated and local economies are weaker, tax revenues are seen as more valuable; in the south, stronger economies and fewer turbines reduce the perceived benefits.

Prince et al. (2024) investigate how tourists perceive wind turbines in rural Swedish landscapes. While some tourists express concerns about disruptions to the traditional rural idyll, many view the landscape holistically and are less fixated on turbines than residents. In some cases, turbines are even seen positively, symbolising environmental progress and modernisation, echoing arguments identified by Rygg (2012). The imbalance between tourist and resident perceptions aligns with Rygg's findings from interviews in 13 Norwegian municipalities, where opposition to wind power was often rooted in emotional attachments to place, environmental concerns, and fears of negative impacts on tourism. Notably, Rygg highlights a divide between seasonal residents and full-time locals: while second-home owners were more protective of the cultural landscape, municipal officials and full-time residents often viewed wind power as an opportunity for modernisation, job creation, and infrastructure investment. Both Lindvall (2023) and Rygg (2012) underscore that stakeholder conduct during consultation processes critically shapes public perceptions - aggressive investor behaviour or overly enthusiastic municipal promotion can erode trust and amplify resistance.

# 4. Empirical Methodology

The key identifying assumption of the classic difference-in-differences (DiD) estimator is that of parallel trends, which requires that, in the absence of treatment, treated and control units follow similar trends in outcomes over time. Concretely, this refers to the traditional independence assumption stating that the treatment cannot be related to the levels of potential outcomes. This in turn means that the treatment assignment cannot be related to the growth in potential outcomes over time (Alves, 2022). If this assumption is violated, the treatment effect estimate may be biased. In the following section, we illustrate our empirical approach to reduce potential bias emerging from our empirical context and increase the robustness of our results.

## 4.1 Two-Way Fixed Effects Regression

For our analysis, we begin with a standard two-way fixed effects (TWFE) model to estimate the average treatment effect of wind power construction, accounting for both municipality-, and time-fixed effects. Unit-fixed effects control for time-invariant characteristics specific to each municipality, while time-fixed effects absorb shocks common to all municipalities in a given period. However, because treatment is implemented at different times across municipalities, a standard TWFE estimator can produce biased estimates when already-treated units are incorrectly used as controls for later-treated units.

To address this limitation and reduce bias, we employ a modern two-way fixed effects model as proposed by Sun & Abraham (2021), which accounts for heterogeneous treatment effects. Specifically, it constructs group-time average treatment effects ($ATT_{g,t}$) relative to the timing of treatment (e.g., two months before treatment is coded as $t = -2$). In the event-study design, treatment is modelled as *absorbing*, meaning that once a municipality becomes treated, it remains treated in all subsequent periods. For each unit $i$ $(i_0, \dots, i_n)$ and time $t$ $(t_0, \dots, t_n)$ we observe the outcome $Y_{it}$ and treatment status $D_{it}$ with $D_{it} = 1$ if the municipality $i$ is treated in the period, and $D_{it} = 0$ if the municipality is not treated in period $t$ (Sun & Abraham, 2021). To implement this, we follow the interaction-weighted estimator, which computes cohort-, and event-time-specific treatment effects ($CATT_{e,\ell}$) where $e$ indicates the treatment cohort and $\ell$ the time relative to treatment. These effects are aggregated using a set of interpretable weights that represent the relative share of municipalities contributing identifying variation in each event-time period. The estimator is defined as:

$$v_g = \frac{1}{|g|} \sum_{\ell \in g} \sum_e CATT_{e,\ell} \times \Pr(E_i = e \mid E_i \in [-\ell, T - \ell]) \qquad (1)$$

Here, $v_g$ represents a bias-robust average treatment effect at relative time $\ell$. By construction, this approach avoids the contamination of the comparison group by units (municipalities) treated at different times. To address potential confounding from seasonal variation in labour market patterns, we include municipality-month fixed effects in our specification for the outcome variables (*job vacancies* and *unemployment*). This accounts for seasonality that may differ in magnitude and pattern across municipalities, thereby improving the precision and validity of our estimated treatment effects. We implement this specification using the *sunab* function in the *fixest* package (R), including municipality and period fixed effects and clustering standard errors at the municipal level.

## 4.2 Synthetic Difference-in-Differences

To relax the assumptions of the standard difference-in-differences model, we employ a modern estimator introduced by Arkhangelsky et al. (Synthetic Difference-in-Differences, 2021). This approach combines elements from both the synthetic control method (SCM) and traditional difference-in-differences (DiD). It allows for synthetic controls to be used on a larger group of treated units, improving causal inference in panel data settings. Further, sDiD relaxes the key difference-in-differences assumption of parallel trends by using weights to construct a synthetic control group and matching pre-treatment trends. Thus, it is particularly well-suited for our context where treatment is staggered, and unit (municipality) characteristics vary.

Unlike the standard TWFE model, which gives equal weight to all observations, sDiD emphasises local comparisons. It restricts estimation to subsets of units and time periods that, in pre-treatment trends, are most similar to the treated municipalities. This localisation improves robustness, as it reduces reliance on poorly matched controls and can enhance precision if the weighted controls better approximate untreated potential outcomes. However, when systematic heterogeneity (by unit or time periods) is limited in the data, this focus can reduce precision relative to the standard DiD.

Formally, the sDiD estimator takes the following form:

$$\hat{\tau}_{sDiD} = \hat{\delta}^{treated} - \sum_{i=1}^{N_{control}} \widehat{w}_i^{sDiD} \hat{\delta}_i \qquad (2)$$

$\hat{\delta}^{treated}$ represents the average change in outcomes for treated units, and $\hat{\delta}_i$ is the adjusted outcome difference for the control unit $i$ (municipality $i$), weighted by the unit-specific $\widehat{w}_i^{sDiD}$. Time weights for period $t$ ($\hat{\lambda}_t^{sDiD}$) are included in $\hat{\delta}_i$, as they weigh the pre-treatment outcomes used to construct the counterfactual.

When implementing synthetic differences-in-differences, we estimate the models both with and without control variables for municipality characteristics. Specifically, we include an urban/rural municipal classification (groups A, B, and C) and population density categories, clustering standard errors at the municipal level.

## 4.3 Robustness

To assess the robustness of our findings, we estimate the effects using a range of alternative model specifications. Specifically, we compare our main results with estimates from a standard two-way fixed effects (TWFE) model and the synthetic difference-in-differences (sDiD) approach, applied to both level outcomes and first-differenced month-to-month trends to account for potential non-stationarity and serial correlation. In addition, we employ the Sun & Abraham (2021) estimator to explicitly account for treatment effect heterogeneity under staggered adoption, and to examine the dynamics of treatment effects over time, exploring long-term versus short-term outcomes. This estimator also allows for the examination of pre-treatment dynamics, providing indirect evidence on the plausibility of the parallel trends assumption.

To further evaluate the sensitivity of our results, we replicate all three modelling approaches; TWFE, Sun & Abraham, and sDiD, using alternative treatment capacity cutoffs of 10 MW and 20 MW respectively, alongside our main specification at the 15 MW project capacity cutoff. Through different project size definitions, we examine how treatment effects vary across different sample sizes and project scales, capturing potential differences in impacts between small-, and medium-sized, versus large-scale projects.

# 5. Data

For all data used in the main analysis and models, the time frame is restricted to the years 2014–2019 to mitigate potential confounding effects associated with the COVID-19 pandemic. All regressions are conducted using monthly aggregated data.

## 5.1 Data on Wind Power Projects – Vindbrukskollen

Our data on wind power projects in Sweden is exported from Vindbrukskollen, an interactive mapping tool maintained by the Swedish Energy Agency (*Energimyndigheten*) and the County Administrative Boards (*Länsstyrelserna*). Vindbrukskollen provides geospatial information on established wind power projects in Sweden, aggregated at a municipal level. The database contains detailed information on individual turbine coordinates, and documents all relevant stages of wind power projects, from proposal and construction to eventual dismantling. In addition, it includes a process history, encompassing assessments, notifications, and permitting activities (Vindbrukskollen, 2025).

For the purposes of this study, the analysis focuses exclusively on wind power projects that have reached the status *uppfört* (constructed). The raw dataset comprises 3,383 project-level observations and 22,465 individual turbines, covering the period from 1980 to the first quarter of 2025.

Our final panel is constructed at the municipality-month level, capturing whether new wind energy projects were constructed in a given municipality and time period. Although the original dataset contains information on individual turbines, the outcome variable is aggregated at the project level to mitigate within-project heterogeneity arising from staggered turbine completion dates. Furthermore, since each project is explicitly linked to a municipality, a project-level approach facilitates more precise spatial classification.

An important limitation of the Vindbrukskollen database lies in the voluntary nature of data reporting by project developers. Although municipal and county authorities can enter and edit administrative project information, the system relies heavily on developers to input and update project-specific data. This decentralised structure, characterised by multiple actors with varying responsibilities, gives rise to inconsistencies and gaps in the dataset, particularly in the documentation of intermediate project stages such as assessments and permitting. Moreover, the absence of standardised reporting practices across stakeholders increases the likelihood of outdated records and incomplete updates. Table 1 below summarises the differentiated data entry and editing rights assigned to each stakeholder within the Vindbrukskollen system, which helps contextualise the observed variation in data completeness and quality (Länsstyrelserna, 2022).

| Party | Permission to enter and edit |
|---|---|
| Developers (Projektörer och verksamhetsutövare) | • Project areas and wind turbines that their own organisation projects<br>• Wind turbines that were once planned but for various reasons are no longer relevant for planning and/or construction<br>• Constructed wind turbines owned by the organization |
| County councils and municipalities (Länsstyrelser och kommuner) | • Information on the supervisory authority<br>• Creating new projects |
| Office of the wind energy regulator (Vindbrukskollens kansli) | • Support function, information to users and manuals<br>• Supporting users to enter and update project information<br>• Working continuously with quality control of data<br>• Further development of the map service |

Table 1 – Vindbrukskollen: Data Entry and Editing Rights for Different Stakeholders
*Information from (Länsstyrelserna, 2022).*

During the data cleaning process, we exclude all projects that lack a valid year of completion, as these cannot be reliably assigned to a specific time period. Projects without information on turbine coordinates are retained in the analysis, provided that information on municipality and date is available.

Projects are treated as the unit of observation, and the Vindbrukskollen dataset is used to identify intervention timing for the treated group. To exclude small-scale developments that may differ qualitatively or economically from utility-scale wind farms, we apply a minimum capacity threshold. For the main analysis, this threshold is set at 15 MW, with alternative thresholds of 10 MW and 20 MW employed as robustness checks.

An important consideration when working with the Vindbrukskollen data is the evolving size and capacity of wind power projects over time. For example, the average wind turbine commissioned in 2022 had a capacity of 6.5 MW and a total height of 240 meters and is expected to generate approximately 23 GWh of electricity annually, whereas an average turbine constructed in 2015 had a capacity of 3 MW, a height of 150 meters, and an annual output of approximately 6.8 GWh (Westander & Henryson, 2023). This reflects a near threefold increase in electricity production per turbine over the period. For our analysis, this technological progression implies that applying a consistent minimum capacity threshold for wind projects over the entire 2014–2019 period may result in later projects comprising fewer turbines per project. This, in turn, could influence the estimated economic outcomes in our models.

## 5.2 Economic Outcome Variables

Ideally, income would serve as the primary economic outcome variable for our analysis, given its direct relevance as a measure of individual and regional economic welfare. However, due to data access constraints related to GDPR, specifically for monthly, municipality-level income data, we instead rely on three alternative proxies to approximate local economic conditions: financial assistance, job vacancies, and the unemployment rate.

Each of these variables captures a distinct dimension of economic activity. Financial assistance serves as an indicator of social welfare dependency, where higher levels typically signal weaker local economic conditions or higher levels of household financial distress. Job vacancies reflect local labour demand and business activity, offering insight into the health and growth of regional labour markets. The unemployment rate captures labour market effects and provides a standard measure of labour force underutilisation.

By employing these complementary outcome variables, we examine the relationship between wind power developments and economic conditions from multiple angles. This approach enhances both the robustness and interpretability of our results, mitigating potential bias or limitations associated with relying on any single proxy measure.

### 5.2.1 Financial Assistance

The Swedish National Board of Health and Welfare (Socialstyrelsen) publishes monthly data on financial assistance (Socialstyrelsen, 2025). This data is available aggregated by both county and municipality. The dataset includes information on the number of households receiving assistance, as well as the average amount of assistance per household and person. Additionally, demographic and background characteristics of recipients are available, including gender, number of children, primary reason for assistance (*hinder* - such as unemployment, sick leave, parental leave, etc.), and country of birth (Sweden or abroad).
We use the variable amount per household, which is the average amount of financial assistance paid out to a household (in SEK), aggregated by municipality and month. We adjust this to inflation, to match 2019 values.

Our main regressions are run using this inflation-adjusted variable, but we also, for robustness, run the regressions using a trend variable (first-differenced month-to-month), defined as follows:

$$x_{trend} = \frac{x_n - x_{n-1}}{x_{n-1}} \tag{4}$$

Where "n" denotes the period (month). Data source extracted 27 March 2025, with data from January 2014 to December 2024.

### 5.2.2 Job Vacancies

*Arbetsförmedlingen*, the Swedish Public Employment Service, provides monthly data on the number of reported job vacancies, disaggregated at the national, county, and municipal levels (Arbetsförmedlingen, 2025). The dataset covers the period from January 1996 to March 2025 and includes positions reported by employers either directly to *Arbetsförmedlingen* or through the self-service platform *Annonsera Direkt*. The data distinguishes between new job vacancies (*nya jobb*) and total job listings (*alla jobb*). In a supplementary dataset, information is also available on the number of new and total vacancies disaggregated by occupational category. For the purposes of our analysis, we focus exclusively on the new jobs count variable. To enhance comparability across municipalities and account for differences in population size, we construct a population-adjusted measure defined as the number of new job vacancies divided by the corresponding municipal population (using annual population figures (Statistiska Centralbyrån, 2025)).

Our primary regressions utilise this population-adjusted variable - new job vacancies per capita - as the outcome of interest. In addition, we conduct robustness checks using an alternative specification based on a trend variable (first-differenced month-to-month), defined as follows:

$$x_{trend} = \frac{x_n - x_{n-1}}{x_{n-1}} \tag{3}$$

Where “n” denotes the period (month).

### 5.2.3 Unemployment Rate

Our third economic outcome variable is the unemployment rate. Data on unemployment is sourced from *Arbetsförmedlingen*, the Swedish Public Employment Service, which provides monthly statistics disaggregated at the national, county, and municipal levels (Arbetsförmedlingen, 2025). The dataset contains detailed information on the number of registered unemployed individuals, along with counts of long-term unemployed segmented by duration, specifically those unemployed for more than 6 months, 12 months, and 24 months. In addition, the data includes demographic breakdowns by gender, age group, educational attainment, and country of birth (Sweden, other European countries, and non-European countries). For our analysis, we construct a population-adjusted unemployment rate, calculated as the total number of registered unemployed individuals in each municipality and month, divided by the corresponding municipal population (using annual population data (Statistiska Centralbyrån, 2025)). This standardisation allows for meaningful comparison across municipalities of varying sizes. Furthermore, to assess the robustness of our findings, we create an alternative specification based on a trend variable (first-differenced month-to-month), defined as follows:

$$x_{trend} = \frac{x_n - x_{n-1}}{x_{n-1}} \tag{5}$$

Where “n” denotes the period (month).

## 5.3 Data on Wind Power Project Rejections

In 2023, *Westander Klimat och Energi*, commissioned by *Svensk Vindenergi*, published a report examining 295 wind power project applications encompassing a total of 6,566 wind turbines submitted between 2014 and 2022 (Westander & Henryson, 2023). The report primarily presents aggregated data at the national level, with some disaggregation by energy zones and counties. However, it does not provide data at the municipal level. While it offers an overview at the county level, this information is not disaggregated by year but rather presented in aggregate form over the entire period. Furthermore, the report focuses predominantly on the number of turbines rather than the number of projects. Given that the primary focus of our study is on projects, rather than individual turbines, and considering the substantial variation in project size, direct comparison with the report's findings is limited. However, the report nonetheless provides a useful overview of the approval and rejection patterns of wind power in Sweden.

To provide an initial overview of the data, Figure 4 presents the annual number of project applications and the corresponding total number of wind turbines for the period 2014–2022. It is important to note that the year of application does not directly correlate to the year of construction. In the empirical analysis of our study, the focus is on the year in which the project was completed. The time span from application to completed construction varies considerably, ranging from a few years to over a decade.

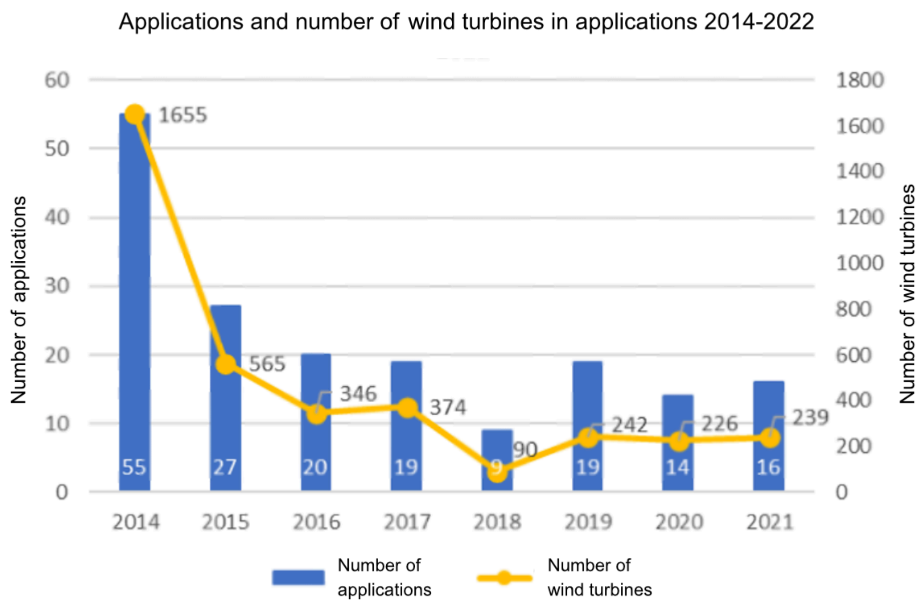


Figure 4 – Applications and Number of Wind Turbines in Applications 2014-2024.
*Graph extracted from (Westander & Henryson, 2023), translated to English.*

Figure 5 below illustrates the number of approved and rejected wind turbines, as well as the rejection rate, for *decisions made* by Environmental Assessment Delegations (*Miljöprövningsdelegationerna*, MPD) within the County Administrative Boards between 2014 and 2022.

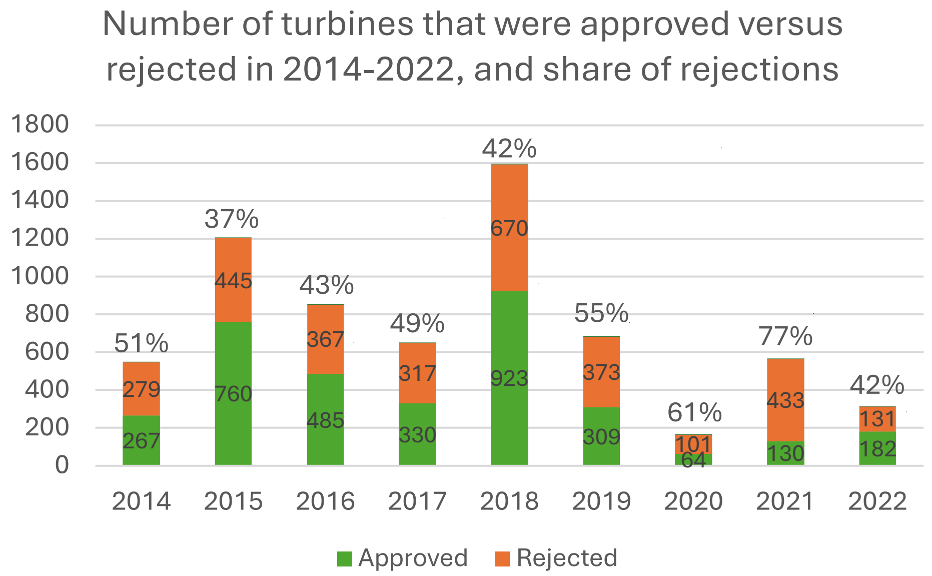


Figure 5 - Number of Turbines Approved versus Rejected in 2014-2022 and Share of Rejections. *Data from (Westander & Henryson, 2023).*

The report offers valuable insights into the various reasons why wind power projects and individual turbines are rejected. Over the entire period, the most common reason for project denial was the absence of municipal support (47%), reflecting municipalities' use of their right to veto projects. The second most frequent ground for rejection (21%) pertained to species protection and nature conservation, followed by concerns related to reindeer herding (11%) and military activities (4%).

## 5.4 Additional Data and Control Variables

In addition to the main data sources, we have used data on income inequality indicators to aid our analysis of the rejection shares of wind power projects. We also include two different control variables in our synthetic difference-in-differences models.

### 5.4.1 Income Inequality Indicators

For our supplementary analysis, we use data on income inequality indicators from *Statistikdatabasen* (Statistikdatabasen, 2025). The dataset covers the years 2011 to 2023 and is disaggregated by county and municipality. It distinguishes between two types of income: primary income, defined as the sum of income from employment and property, and disposable income, defined as the total of all taxable and tax-free income minus taxes and negative transfers. All income measures are equivalised, meaning they are adjusted for household size; the resulting unit of analysis is equivalised disposable income per person. We employ four different indicators of income inequality. First, the GINI coefficient, which quantifies the extent to which the income distribution deviates from perfect equality. Second, the at-risk-of-poverty rate, representing the share of individuals with an equivalised disposable income below 60% of the national median. Third, the high economic standard rate, defined as the proportion of individuals with an equivalised disposable income exceeding 200% of the national median. Finally, the relative median at-risk-of-poverty gap, which captures the difference between the median equivalised disposable income of those below the at-risk-of-poverty threshold and the threshold itself.

### 5.4.2 Control Variables

In our synthetic difference-in-differences models, we use dummy variables as control variables, based on two different data: population density and urban-rural municipal classification.

#### *5.4.2.1 Population Density*

We utilise data from *Statistikdatabasen* on population density, measured as inhabitants per square kilometre, covering the period 1991 to 2024, disaggregated annually at the municipal level (Statistikdatabasen, 2024). Based on this dataset, we classify municipalities into six population density categories: Very Low (<10 inhabitants/km²), Low (10–29), Medium (30–49), High (50–99), Very High (100–499), and Extreme (≥500).
We then construct corresponding dummy variables for each category, which are used as control variables in our synthetic difference-in-differences estimations.

#### *5.4.2.2 Urban-Rural Municipal Classification*

To distinguish between urban and rural municipalities, we use a municipal classification from 2023, developed by *Sveriges Kommuner och Regioner* (Sveriges Kommuner och Regioner, 2023). This classification system organises municipalities into three overarching categories, Groups A, B, and C, based on structural characteristics such as population size and commuting patterns. Within these three main groups, municipalities are further subdivided into nine specific categories.

Group A comprises large cities and municipalities near large cities. It is subdivided into:
- A1 ("Large cities"): municipalities with at least 200,000 inhabitants, of which at least 200,000 reside in the largest urban area;
- A2 ("Commuting municipalities near large cities"): municipalities where over 40% of the employed population commutes to work in a large city or municipality near a large city.

Group B includes medium-sized towns and their surrounding municipalities. It is subdivided into:

- B3 (“Medium-sized towns”): municipalities with at least 50,000 inhabitants, of which at least 40,000 reside in the largest urban area;
- B4 (“Commuting municipalities near medium-sized towns”): municipalities where over 40% of the employed population commutes to a medium-sized town;
- B5 (“Commuting municipalities with a low commuting rate near medium-sized towns”): municipalities where less than 40% of the working population commutes to a medium-sized town.

Group C captures smaller towns, urban areas, and rural municipalities. It is subdivided into:

- C6 (“Small towns”): municipalities where the largest urban area has a population between 15,000 and 40,000;
- C7 (“Commuting municipalities near small towns”): municipalities where more than 30% of the working population commutes to a small town or where more than 30% of the employed day population resides in another municipality;
- C8 (“Rural municipalities”): municipalities with fewer than 15,000 inhabitants in the largest urban area and a commuting rate below 30%;
- C9 (“Rural municipalities with a visitor industry”): rural municipalities that meet at least two criteria indicative of a significant visitor industry presence.

For analytical simplicity, we aggregate the subcategories and retain only the three main groupings (A, B, and C). Based on this classification, we construct three corresponding dummy variables that are included as control variables in our regression analyses.

## 5.5 Sample Selection and Descriptive Statistics

We restrict our sample to the period 2014–2019 to avoid confounding effects from the COVID-19 pandemic. Given that our dataset is monthly, this results in 72 time periods.

To define the treatment group, we initially identify all municipalities where at least one wind power project was constructed between 2014 and 2019, yielding 60 municipalities. We then narrow this group to 32 municipalities by including only those with projects of at least 10 MW capacity. Next, we limit the sample to municipalities that hosted exactly one project during the period, reducing the group to 22 municipalities. Finally, we exclude municipalities with less than six months of post-treatment data (i.e., projects initiated after June 2019), resulting in a final treatment group of 14 municipalities.

For our main regressions, we apply a stricter capacity cutoff of 15 MW, which reduces the treatment group to 11 municipalities. For robustness checks, we also estimate models using alternative treatment groups based on a 10 MW cutoff (14 municipalities) and a 20 MW cutoff (9 municipalities).

The control group consists of municipalities where one or more wind power projects were proposed between 2014 and 2019, but none were approved or constructed. This yields a sample of 128 municipalities.

For each of the three dependent variables (financial assistance per household, job vacancies per capita, and unemployment) we systematically assess both the treatment and control groups for the presence of outliers. We apply a time-series outlier detection method separately for each economic outcome, distinguishing between levels and trends. Specifically, for each variable, we calculate the first and third quartiles (Q1 and Q3) within each municipality and compute the interquartile range (IQR = Q3 – Q1). An observation is classified as an outlier if its value falls below (Q1 – 1.5×IQR) or exceeds (Q3 + 1.5×IQR). This procedure is implemented independently within each municipality to account for cross-sectional variation in value distributions. Following the identification of potential outliers, we visually inspect the temporal patterns of both trends and flagged observations, comparing municipalities to one another. Municipalities exhibiting the highest frequency and magnitude of outliers are subsequently excluded from the analysis. This process is conducted separately for each outcome variable, for both levels and trends, and for both treatment and control groups.

Additionally, the dataset on financial assistance contains some missing values, which further reduces the number of treated municipalities available for analysis, as we exclude all municipalities with any missing values. The size of the control groups is presented in Appendix 1-3, while Tables 2-8 below display the corresponding treatment groups. Figure 6 below shows two maps of Sweden's municipal division, one with the initial 14 treatment municipalities shaded in green, and one with the initial 128 control municipalities shaded in blue.

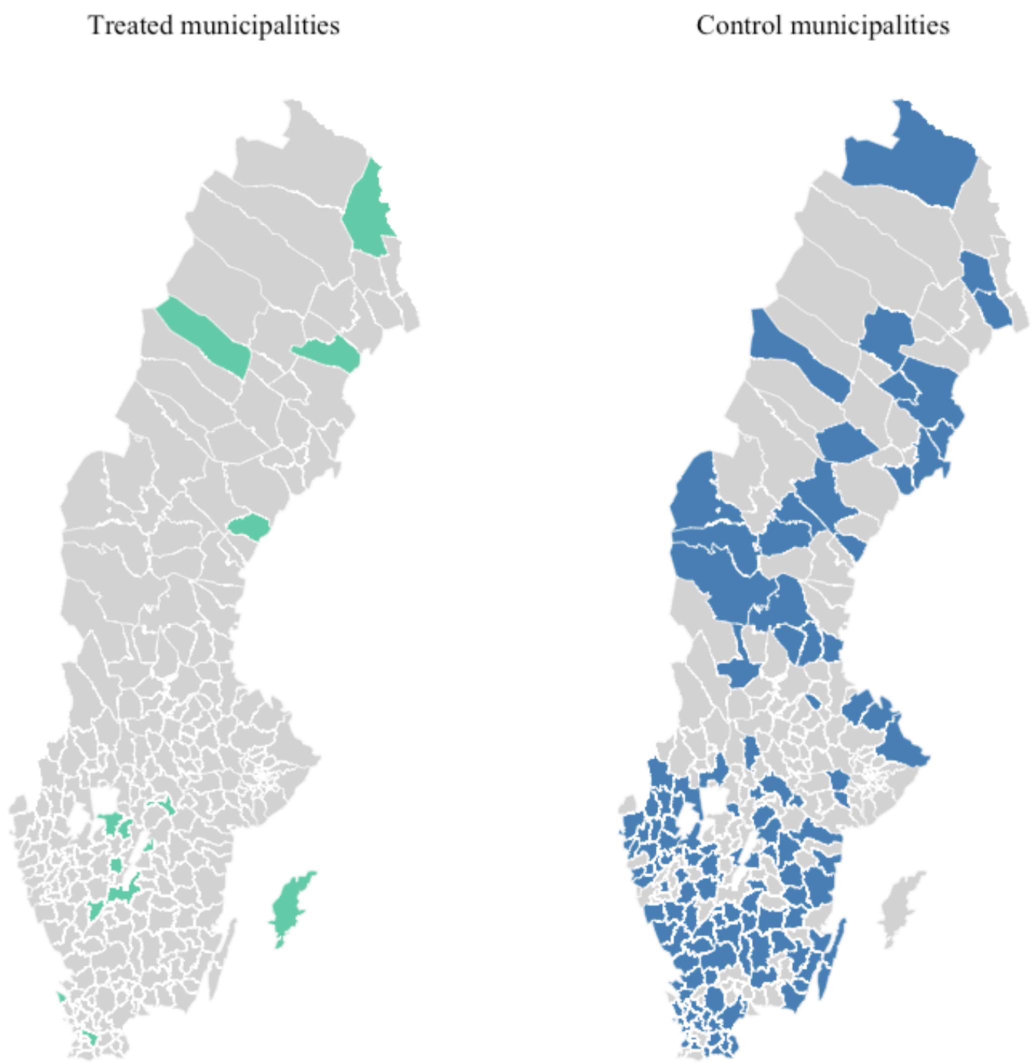


Figure 6 - Maps of Swedish Municipalities, showing Treatment and Control Group.
*The left-hand side shows the 14 treatment municipalities shaded in green. The right-hand side includes all 128 control municipalities shaded in blue.*

Treatment Groups at Different Capacity Cutoffs.

| | Cutoff: 10 MW | Cutoff: 15 MW | Cutoff: 20 MW |
|---|---|---|---|
| **Treatment Group after MW-limit and removing those with too few post-treatment periods** | Gotland<br>Hallsberg<br>Höganäs<br>Jönköping<br>Kramfors<br>Lund<br>Mariestad<br>Pajala<br>Piteå<br>Sorsele<br>Tidaholm<br>Tranemo<br>Töreboda<br>Vadstena | Hallsberg<br>Jönköping<br>Kramfors<br>Lund<br>Mariestad<br>Pajala<br>Piteå<br>Sorsele<br>Tidaholm<br>Tranemo<br>Töreboda | Jönköping<br>Kramfors<br>Mariestad<br>Pajala<br>Piteå<br>Sorsele<br>Tidaholm<br>Tranemo<br>Töreboda |
| **Number of treatment municipalities** | 14 | 11 | 9 |

Table 2 - Treatment Groups at Different Capacity Cutoffs.
*Municipalities with less than 6 months of post-treatment data have been excluded. Tables and selection made by the authors.*

### 5.5.1 Treatment Groups for Financial Assistance

Treatment Groups for Dependent Variable *Financial Assistance per Household - Levels.*

| | Cutoff: 10 MW | Cutoff: 15 MW | Cutoff: 20 MW |
|---|---|---|---|
| **Treatment Group after removing outliers** | Gotland<br>Höganäs<br>Jönköping<br>Piteå<br>Tidaholm<br>Tranemo<br>Vadstena | Jönköping<br>Piteå<br>Tidaholm<br>Tranemo | Jönköping<br>Piteå<br>Tidaholm<br>Tranemo |
| **Number of treatment municipalities** | 7 | 4 | 4 |

Table 3 - Treatment Groups for Dependent Variable Financial Assistance per Household – Levels.
*Separated by capacity cutoff. Tables and selection made by the authors.*

Treatment Groups for Dependent Variable *Financial Assistance per Household - Trends.*

| | **Cutoff: 10 MW** | **Cutoff: 15 MW** | **Cutoff: 20 MW** |
|---|---|---|---|
| **Treatment Group after removing outliers** | Gotland<br>Höganäs<br>Jönköping<br>Piteå<br>Tidaholm | Jönköping<br>Piteå<br>Tidaholm | Jönköping<br>Piteå<br>Tidaholm |
| **Number of treatment municipalities** | 5 | 3 | 3 |

Table 4 - Treatment Groups for Dependent Variable Financial Assistance per Household – Trends.
*Separated by capacity cutoff. Tables and selection made by the authors.*

### 5.5.2 Treatment Groups for Job Vacancies

Treatment Groups for Dependent Variable *Job Vacancies per capita - Levels.*

| | **Cutoff: 10 MW** | **Cutoff: 15 MW** | **Cutoff: 20 MW** |
|---|---|---|---|
| **Treatment Group after removing outliers** | Gotland<br>Hallsberg<br>Höganäs<br>Jönköping<br>Kramfors<br>Lund<br>Mariestad<br>Sorsele<br>Tranemo<br>Töreboda<br>Vadstena | Hallsberg<br>Jönköping<br>Kramfors<br>Lund<br>Mariestad<br>Sorsele<br>Tranemo<br>Töreboda | Jönköping<br>Kramfors<br>Mariestad<br>Sorsele<br>Tranemo<br>Töreboda |
| **Number of treatment municipalities** | 11 | 8 | 6 |

Table 5 - Treatment Groups for Dependent Variable Job Vacancies per capita – Levels.
*Separated by capacity cutoffs. Tables and selection made by the authors.*

Treatment Groups for Dependent Variable *Job Vacancies per capita - Trends.*

| | **Cutoff: 10 MW** | **Cutoff: 15 MW** | **Cutoff: 20 MW** |
|---|---|---|---|
| **Treatment Group after removing outliers** | Gotland<br>Hallsberg<br>Höganäs<br>Jönköping<br>Lund<br>Mariestad<br>Tranemo<br>Töreboda | Hallsberg<br>Jönköping<br>Lund<br>Mariestad<br>Tranemo<br>Töreboda | Jönköping<br>Mariestad<br>Tranemo<br>Töreboda |
| **Number of treatment municipalities** | 8 | 6 | 4 |

Table 6 - Treatment Groups for Dependent Variable Job Vacancies per capita – Trends.
*Separated by capacity cutoffs. Tables and selection made by the authors.*

### 5.5.3 Treatment Groups for Unemployment Rate

Treatment Groups for Dependent Variable *Unemployment Rate - Levels.*

| | **Cutoff: 10 MW** | **Cutoff: 15 MW** | **Cutoff: 20 MW** |
|---|---|---|---|
| **Treatment Group after removing outliers** | Gotland<br>Höganäs<br>Jönköping<br>Kramfors<br>Lund<br>Mariestad<br>Pajala<br>Piteå<br>Sorsele<br>Tidaholm<br>Tranemo<br>Töreboda<br>Vadstena | Jönköping<br>Kramfors<br>Lund<br>Mariestad<br>Pajala<br>Piteå<br>Sorsele<br>Tidaholm<br>Tranemo<br>Töreboda | Jönköping<br>Kramfors<br>Mariestad<br>Pajala<br>Piteå<br>Sorsele<br>Tidaholm<br>Tranemo<br>Töreboda |
| **Number of treatment municipalities** | 13 | 10 | 9 |

Table 7 - Treatment Groups for Dependent Variable Unemployment Rate – Levels.
*Separated by capacity cutoff. Tables and selection made by the authors.*

Treatment Groups for Dependent Variable *Unemployment Rate - Trends*.

| | **Cutoff: 10 MW** | **Cutoff: 15 MW** | **Cutoff: 20 MW** |
|---|---|---|---|
| **Treatment Group after removing outliers** | Hallsberg<br>Höganäs<br>Jönköping<br>Kramfors<br>Lund<br>Mariestad<br>Tidaholm<br>Tranemo<br>Töreboda<br>Vadstena | Hallsberg<br>Jönköping<br>Kramfors<br>Lund<br>Mariestad<br>Tidaholm<br>Tranemo<br>Töreboda | Jönköping<br>Kramfors<br>Mariestad<br>Tidaholm<br>Tranemo<br>Töreboda |
| **Number of treatment municipalities** | 10 | 8 | 6 |

Table 8 - Treatment Groups for Dependent Variable Unemployment Rate – Trends.
*Separated by capacity cutoff. Tables and selection made by the authors.*

### 5.5.4 Descriptive Statistics

Table 9 below presents summary statistics for the key datasets and variables used in our regressions. The table separates the treatment and control group. Data is separated into four sections: economic outcome variables, municipal data, wind power project data, and additional data.

Descriptive Statistics

| | Treatment | | | | | | | Control | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Mean | SD | Min | Median | Max | Count | N/A | Mean | SD | Min | Median | Max | Count | N/A |
| **Economic outcome variables** | | | | | | | | | | | | | | |
| *Financial Assistance* | | | | | | | | | | | | | | |
| Inflation-adjusted value per household (SEK) | 7217 | 871.7 | 3701 | 7205 | 10648 | 1356 | 12 | 7164 | 892.8 | 1702 | 7126 | 14893 | 8961 | 111 |
| *Job Vacancies* | | | | | | | | | | | | | | |
| New job vacancies | 268.8 | 447.9 | 0.000 | 81.00 | 2095 | 1008 | 0 | 228.5 | 457.2 | 1.000 | 83.00 | 5278 | 9144 | 0 |
| New job vacancies per capita | 0.006 | 0.004 | 0.000 | 0.005 | 0.053 | 1008 | 0 | 0.006 | 0.004 | 0.000 | 0.005 | 0.130 | 9144 | 0 |
| *Unemployment* | | | | | | | | | | | | | | |
| Unemployment rate | 0.032 | 0.009 | 0.019 | 0.029 | 0.059 | 864 | 0 | 0.035 | 0.012 | 0.011 | 0.035 | 0.076 | 9072 | 0 |
| **Municipal data** | | | | | | | | | | | | | | |
| *Population* | | | | | | | | | | | | | | |
| Population per municipality | 35086 | 41062 | 2489 | 17118 | 141081 | 84 | 0 | 31108 | 41094 | 2794 | 15979 | 344166 | 768 | 0 |
| *Density* | | | | | | | | | | | | | | |
| Population density per municipality (pop./km2) | 54.34 | 78.34 | 0.300 | 21.55 | 292.7 | 84 | 0 | 65.70 | 194.1 | 0.700 | 24.45 | 2193 | 768 | 0 |
| **Wind power projects** | | | | | | | | | | | | | | |
| *Vindbrukskollen* | | | | | | | | | | | | | | |
| Number of turbines per project | 12.71 | 18.90 | 2.000 | 5.000 | 68.00 | 14 | 0 | | | | | | | |
| Yearly production of project (GWh) | 130.8 | 212.0 | 12.80 | 38.40 | 727.4 | 14 | 0 | | | | | | | |
| Project capacity (MW) | 115.6 | 169.8 | 11.60 | 38.40 | 516.8 | 14 | 0 | | | | | | | |
| **Additional data** | | | | | | | | | | | | | | |
| *Inequality indicators* | | | | | | | | | | | | | | |
| Gini coefficient | 0.279 | 0.029 | 0.238 | 0.270 | 0.350 | 84 | 0 | 0.227 | 0.027 | 0.226 | 0.273 | 0.446 | 768 | 0 |

Table 9 - Descriptive Statistics for Economic Outcome Variables, Municipal Data, Wind Power Project Data, and Additional Data.

*The descriptive statistics table reports descriptive statistics across four main sections. The economic outcome variables are divided into three datasets: Financial Assistance, Job Vacancies, and Unemployment. The Financial Assistance section reports the amount of financial assistance per household, adjusted for inflation. The Job Vacancies section includes both the total number of new job vacancies and the number of new vacancies per capita. The Unemployment section presents the unemployment rate. The municipal data section summarises two datasets: population (total population per municipality) and population density (inhabitants per square kilometre). The wind power project data section draws from Vindbrukskollen, for the treatment group, the number of wind turbines per project, annual project production (GWh), and project capacity (MW). Finally, the additional data section reports the GINI coefficient as an indicator of income inequality. The treatment group comprises the 14 municipalities listed in Table 2, defined based on the 10 MW cutoff prior to outlier removal; This group includes all municipalities considered treated in any model specification. The control group includes all 128 municipalities that appear at least once in any control group (Appendix 1-3). Data (top to bottom) extracted from (Socialstyrelsen, 2025) , (Arbetsförmedlingen, 2025), (Statistikdatabasen, 2024), (Vindbrukskollen, 2025), and (Statistikdatabasen, 2025).*

## 5.6 Additional Evidence

As a supplementary analysis, we investigate the relationship between income inequality and the share of wind power turbine rejections across counties. To this end, we construct a scatter plot and compute a correlation matrix to explore the bivariate associations.

Two counties (Blekinge and Uppsala) are excluded from this analysis due to missing data. We conduct an outlier analysis using box plots for the following variables: GINI coefficient, wind turbine rejection share and number of rejected turbines. Based on this assessment, we exclude one municipality (Jämtland), identified as an outlier with respect to the absolute number of rejected wind turbines, and one additional municipality (Stockholm), identified as an outlier in terms of the GINI coefficient.

# 6. Empirical Results

Following the descriptive overview, we now turn to the main empirical analysis of wind power's local socio-economic impacts. Table 10 below presents the estimates from the two-way fixed effects (TWFE) using the capacity cutoff of 15 megawatts (MW). Robustness checks using alternative cutoffs of 10MW and 20MW are included in Appendix 4-5. For each of the three outcome variables - financial assistance, job vacancies, and the unemployment rate - we report specifications in both levels and first-differences (month-to-month trends). The unemployment rate shows a statistically significant decline in both specifications, with an estimated reduction of approximately 0.5 percentage points following treatment. While small in magnitude, this result appears relatively consistent across thresholds and grows slightly in size with increasing project size (between the 10 MW to 20 MW cutoff). This pattern could suggest a directional trend, but given methodological limitations, it must be interpreted with several caveats.

Estimates of Two-Way Fixed Effects (15 MW capacity cutoff)

| | **Estimate** | **Std. Err.** | **t** | **P > \| t \|** |
|---|---|---|---|---|
| **Financial Assistance per Household** | | | | |
| Levels | 245.0180 | 264.9020 | 0.92 | 0.356 |
| Trends | -0.004147** | 0.001789 | -2.32 | 0.021 |
| **Job Vacancies per capita** | | | | |
| Levels | 0.000047 | 0.000439 | 0.11 | 0.918 |
| Trends | -0.042508** | 0.014344 | -2.96 | 0.031 |
| **Unemployment Rate** | | | | |
| Levels | -0.005225*** | 0.001369 | -3.82 | 0.004 |
| Trends | -0.004526** | 0.001434 | -3.16 | 0.016 |

Note: Stars indicate significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table *10* - Estimates of Two-Way Fixed Effects
*Estimated average treatment effects using a standard two-way fixed effects (TWFE) model for financial assistance per household, job vacancies per capita, and the unemployment rate. Estimates are reported separately for levels and trends. The model includes municipality and period fixed effects (unit and time). Standard errors are clustered at the municipality level. Data extracted from (Socialstyrelsen, 2025) and (Arbetsförmedlingen, 2025).*

Overall, the estimates produced by the TWFE model exhibit sensitivity to model specification and threshold choice. Although the unemployment rate results appear relatively stable, the broader pattern across outcomes reveals inconsistencies in both statistical significance and effect magnitude. While our TWFE models include municipality-month fixed effects to account for heterogeneity in seasonal labour patterns, the standard TWFE model can produce biased estimates in staggered adoption designs (Sun & Abraham, 2021).

*Event-Study Evidence: Addressing Dynamic Effects and Pre-trends*. When comparing the standard TWFE estimates to results from the Sun & Abraham TWFE estimator, we see that the identified effects do not hold consistently across multiple post-treatment periods. Figure 7 and Figure 8 illustrate the dynamic treatment effects for levels of job vacancies per capita and unemployment rate, respectively. We do not report Sun & Abraham estimates for financial assistance, as the number of treated municipalities at the 15 MW threshold is too small for reliable inference. Results for the models using 10 MW and 20 MW cutoffs are presented in Appendix 6-10.

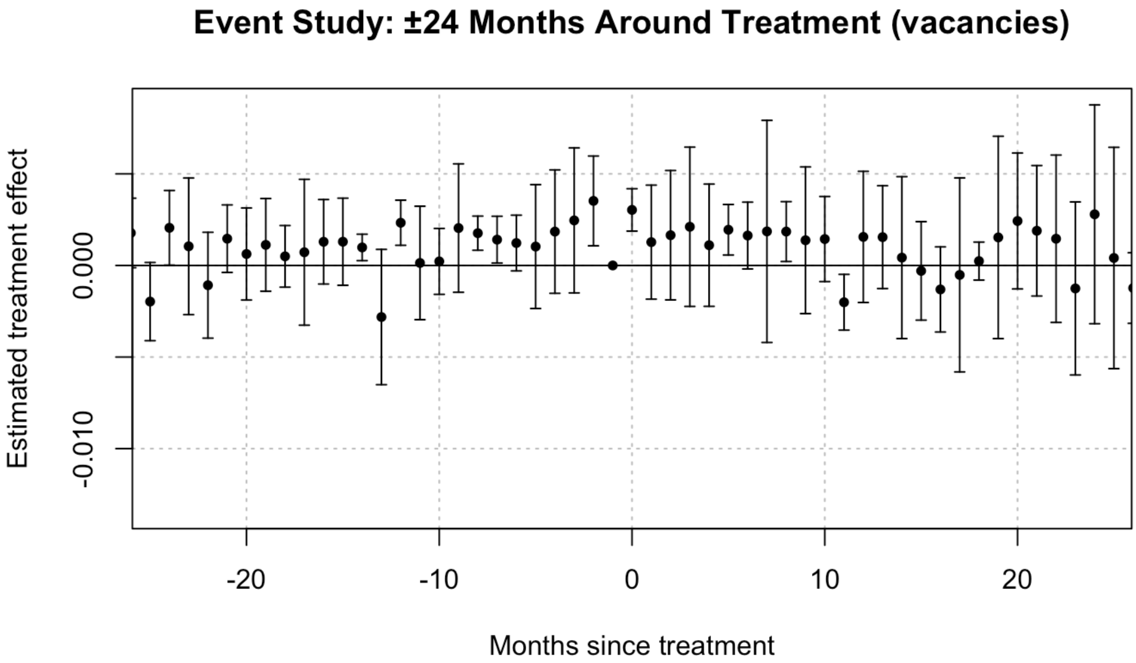


Figure 7 – Dynamic Treatment Effects on Job Vacancies – Levels (Sun & Abraham Estimator *(2021)*)
*Estimated event-study coefficients showing changes in job vacancies per capita from 24 months before to 24 months after wind project treatment (15 MW threshold). Omitted reference period is one month prior to treatment (event time -1). The model includes municipality-month seasonal fixed effects. Standards Errors are clustered at the municipality level. Data extracted from (Arbetsförmedlingen, 2025).*

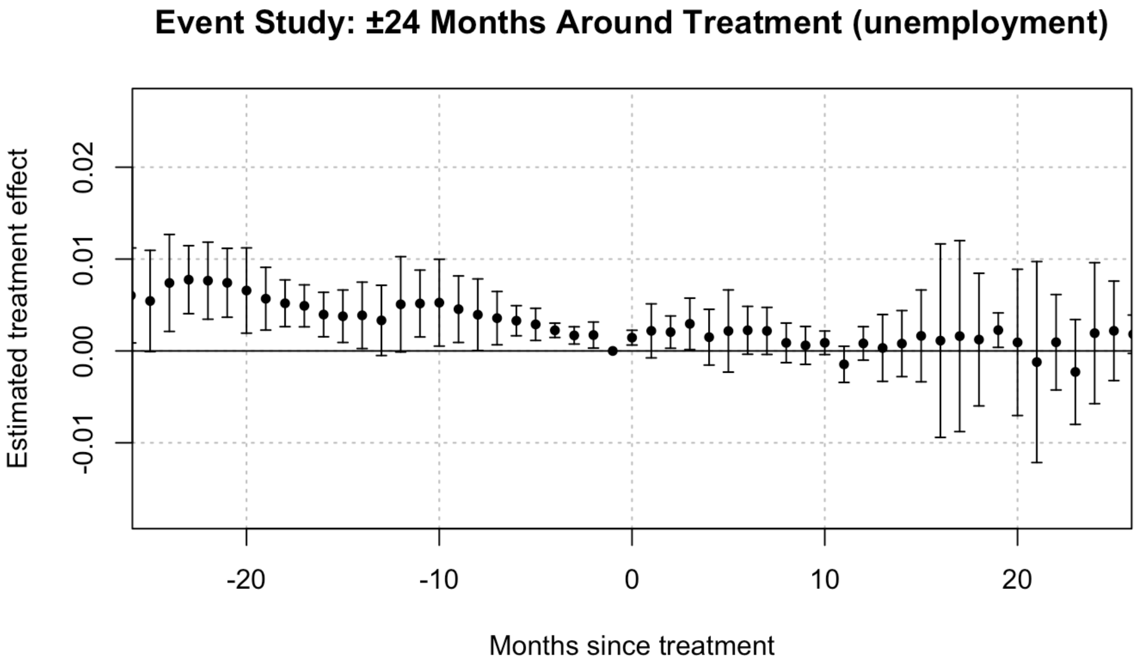


Figure 8 - Dynamic Treatment Effects on Unemployment Rate, Levels (Sun & Abraham Estimator *(2021)*)
*Estimated event-study coefficients showing changes in unemployment per capita from 24 months before to 24 months after wind project treatment (15 MW threshold). The omitted reference period is one month prior to treatment (event time –1). The model includes municipality-month seasonal fixed effects. Standard errors are clustered at the municipality level. Data extracted from (Arbetsförmedlingen, 2025).*

While the job vacancies event-study estimates hover around zero, several pre-treatment periods are statistically significant. Although some of this significance may stem from residual seasonality exhibited in unemployment dynamics, that may not be fully absorbed by the model's fixed effects structure, significant pre-treatment trends are reflected in both outcomes. Further, these patterns persist across alternative cutoffs of unemployment and job vacancies, as well as the 10 MW cutoff for financial assistance, reinforcing concerns about the validity of the TWFE results. To address these limitations and further assess robustness, the next section implements the synthetic difference-in-differences (sDiD) approach.

*Robustness: Synthetic Difference-in-Differences.* We re-estimate the results using synthetic difference-in-differences models for all three treatment thresholds (15 MW, 10 MW and 20 MW), evaluating level and trend effects separately. Each specification is estimated with and without control variables for urban/rural municipal classification (Groups A, B, and C) and population density categories. Table 11 presents the results for the baseline 15 MW threshold, with full results for the 10 MW and 20 MW thresholds appended in Appendix 11-12.

Estimates of Synthetic Difference-in-Differences (15 MW capacity cutoff)

| | Estimate | Std. Err. | t | P > \| t \| | [95% Conf. Interval] |
|---|---|---|---|---|---|
| **Financial Assistance per Household** | | | | | |
| *Levels* | | | | | |
| With Controls | -82.5358 | 278.482 | -0.30 | 0.767 | [-628.000, 463.278] |
| Without Controls | -80.6312 | 255.509 | -0.32 | 0.752 | [-581.000, 420.158] |
| *Trends* | | | | | |
| With Controls | -0.00059 | 0.00409 | -0.15 | 0.885 | [-0.00860, 0.00742] |
| Without Controls | -0.00042 | 0.00447 | -0.09 | 0.925 | [-0.00919, 0.00835] |
| **Job Vacancies per capita** | | | | | |
| *Levels* | | | | | |
| With Controls | -0.00056 | 0.00055 | -1.02 | 0.309 | [-0.00164, 0.00052] |
| Without Controls | -0.00056 | 0.00054 | -1.05 | 0.293 | [-0.00162, 0.00049] |
| *Trends* | | | | | |
| With Controls | -0.02142 | 0.02586 | -0.83 | 0.408 | [-0.07209, 0.02930] |
| Without Controls | -0.02185 | 0.02213 | -0.99 | 0.324 | [-0.06523, 0.02150] |
| **Unemployment Rate** | | | | | |
| *Levels* | | | | | |
| With Controls | 0.00011 | 0.00084 | 0.13 | 0.897 | [-0.00153, 0.00175] |
| Without Controls | 0.00012 | 0.00101 | 0.11 | 0.909 | [-0.00187, 0.00210] |
| *Trends* | | | | | |
| With Controls | -0.00036 | 0.00199 | -0.18 | 0.855 | [-0.00426, 0.00353] |
| Without Controls | -0.00040 | 0.00347 | -0.12 | 0.908 | [-0.00719, 0.00640] |

Note: Stars indicate significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11 – Estimates of Synthetic Difference-in-Differences
*Estimated treatment effects using the synthetic difference-in-differences (sDiD) estimator for financial assistance per household, job vacancies per capita, and the unemployment rate. Results are reported separately for level effects and month-to-month trends, both with and without control variables for urban/rural classification: A, B, C grouping, defined by Sweden's municipalities and regions (Sveriges Kommuner och Regioner, 2023); and population density categories. The sDiD estimator by Arkhangelsky et al. (2021) constructs counterfactuals using unit-, and time-specific weighting, rather than fixed effects. Standard errors are clustered at the municipal level. Data extracted from (Socialstyrelsen, 2025) and (Arbetsförmedlingen, 2025).*

A small number of specifications at the 20 MW threshold produce statistically significant estimates[1]. However, these effects are not observed at lower thresholds or consistently across model specifications.

Aside from these isolated cases, the sDiD results show no statistically significant treatment effects, neither in levels nor in trends, regardless of control variable inclusion. These findings suggest that the TWFE estimates are not robust to alternative identification strategies and imply that the observed effects may reflect model-driven bias related to treatment timing rather than true underlying impacts.

To illustrate how the synthetic difference-in-differences method works, Figures 9 and 10 below presents the model for a selected treatment municipality, with and without control variables

[1] Specifically, estimates of job vacancies at threshold 20 MW: vacancies *levels* both with and without controls are significant at the 5% level, and vacancies per capita trends with controls are significant at the 10% level. The estimate results relate to approximately -0.00117**, -0.00118**, and -0.03837*, respectively.

respectively. The vertical line represents the treatment date. Lambda weights in green (right-hand axis) indicate the relative influence of control periods in constructing the counterfactual trajectory.

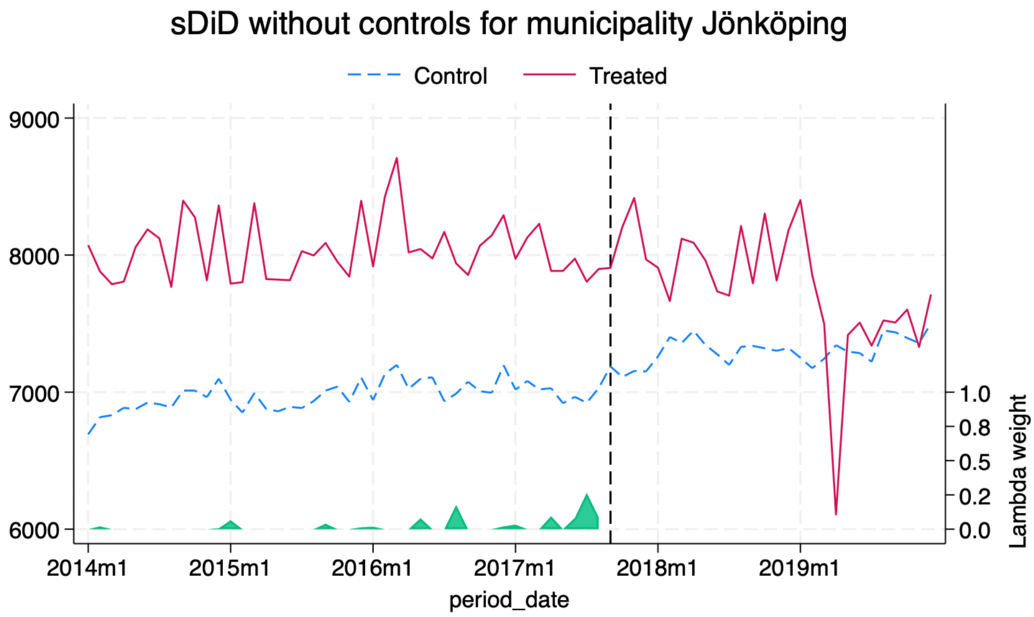


Figure 9 - Synthetic Difference-in-Differences Trajectory for Financial Assistance in Jönköping (15 MW, without controls)
*Estimated trends in financial assistance per household for the treated municipality Jönköping (solid red) and its weighted synthetic control group (dashed blue), using the synthetic difference-in-differences (sDiD) estimator with controls for urban/rural classification and population density. The vertical dashed line marks the onset of wind power operation in August 2017. The sharp decline observed in April 2019 aligns with similar patterns in other municipalities, and is not classified as an outlier. Lambda weights (* $\hat{\lambda}_t^{sDiD}$*: sDiD time-weights, shown in green; right-hand axis) indicate the relative influence of control periods in constructing the counterfactual. Data extracted from (Socialstyrelsen, 2025).*

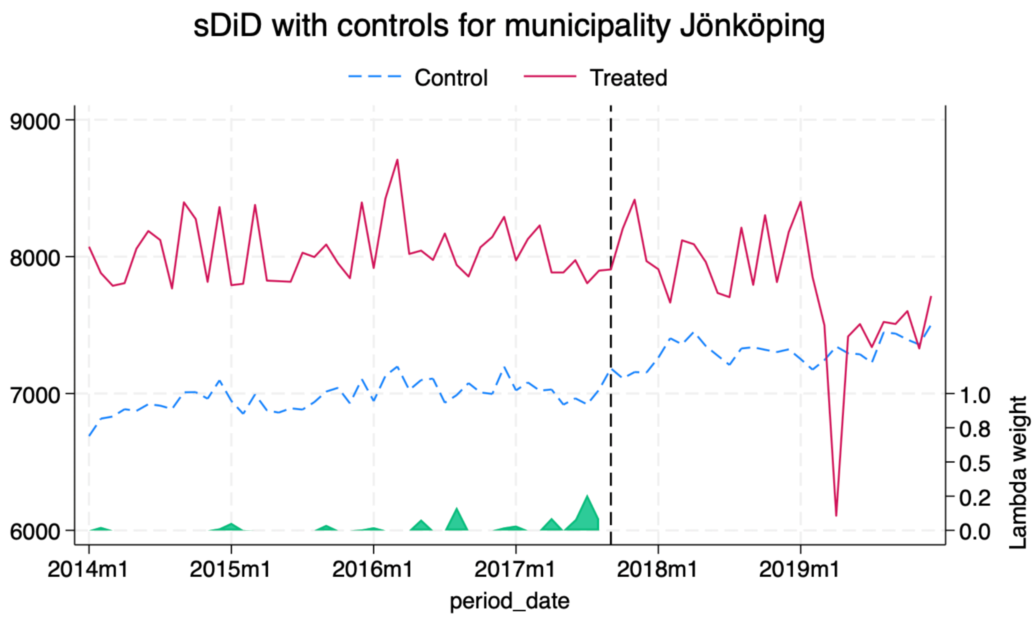


Figure 10 - Synthetic Difference-in-Differences Trajectory for Financial Assistance in Jönköping (15 MW, with Controls)
*Estimated trends in financial assistance per household for the treated municipality Jönköping (solid red) and its weighted synthetic control (dashed blue), using the synthetic difference-in-differences (sDiD) estimator with controls for urban/rural classification and population density. The vertical dashed line marks the onset of wind power operation in August 2017.*

*The sharp decline observed in April 2019 aligns with similar patterns in other municipalities and is not classified as an outlier. Lambda weights ( $\hat{\lambda}_t^{sDiD}$ : sDiD time-weights, shown in green; right-hand axis) indicate the relative influence of control periods in constructing the counterfactual trajectory. Data extracted from (Socialstyrelsen, 2025).*

*Additional Evidence.* From our correlational analysis, we identify a moderately strong, positive relationship (r = 0.65) between the GINI coefficient and the rejection share of wind turbines (for correlation matric including all variables, see Appendix 13). The scatter plot below visually shows this relationship.

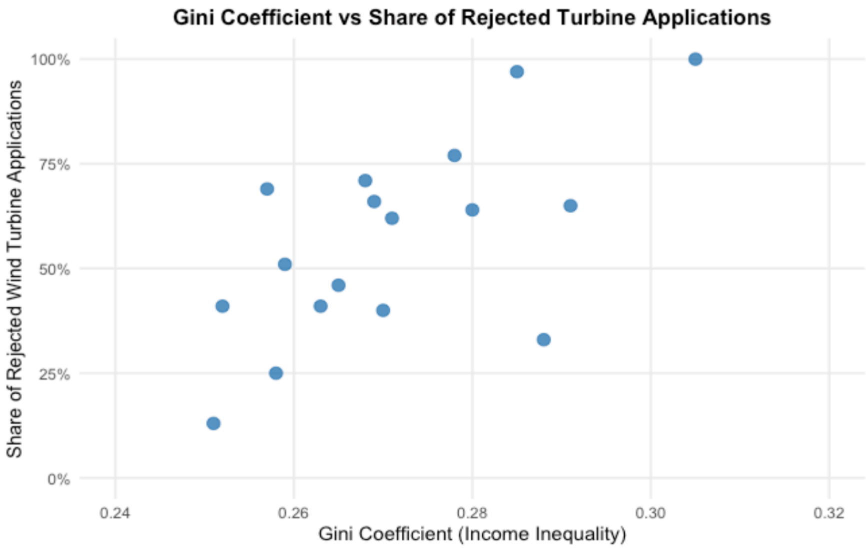


Figure 11 - Scatter Plot for Rejection Share and GINI Coefficient.
*Data extracted from (Westander & Henryson, 2023) and (Statistikdatabasen, 2025)*

# 7. Discussion

The results presented in this study reveal no consistent statistical significance, indicating that wind power developments did not lead to measurable local economic effects within the treatment municipalities during the studied period (2014-2019). Specifically, no robust impacts were observed on our welfare proxy (financial assistance) or labour market dynamics (job vacancies and unemployment rates). These findings align with the broader literature, which reports limited, and predominantly short-term labour market effects (Costa & Veiga, 2020) and no significant local individual income effects from wind energy developments (Lundin & von Platten, 2024).

We expected to find none or only minor effects on our socio-economic indicators. While the following sections provide a more detailed account of the methodological and data-related limitations of our study, we first focus on interpreting the implications of the statistically insignificant results. In particular, while financial assistance is an approximation to income effects, it can introduce a temporal lag. An increase in income associated with wind power project development may not immediately translate into reduced financial assistance, due to lagged adjustments in eligibility or reporting mechanisms. Moreover, financial assistance captures income effects on a particular population group, typically lower-income households, which may not correspond to population segments that gain from wind power project developments. Comparably, while unemployment is frequently used as an indicator of labour market dynamics, we saw several significant pre-treatment periods in the Sun & Abraham estimates. These significant periods remained for all three cutoff specifications, suggesting that the key DiD assumption of parallel trends may not hold across all periods. In context, existing literature shows short-term effects on unemployment during the construction period (e.g., during the first two years (Costa & Veiga, 2020)). As we define treatment as the post-construction period and observe significant pre-treatment trends, it is plausible that wind power developments affected unemployment prior to the completion, complicating causal attribution and potentially explaining the absence of significant post-treatment effects.

A similar reasoning may apply to the insignificant effects observed for job vacancies. Given that we measure vacancies at the local level, the absence of significant impacts may partly reflect limited local job creation after the project's construction, particularly if operation and maintenance tasks are carried out by existing personnel employed by the wind farm operators, rather than generating new positions locally.

*Methodological Robustness and Heterogeneity in Impact.* While the standard two-way fixed effects (TWFE) model produced significant estimates, these results did not persist under alternative model specifications or different capacity thresholds. Although this does not conclusively prove model bias, it reinforces concerns raised in the literature regarding the reliability of TWFE under staggered treatment adoption. Consequently, we employed more advanced estimators to account for timing and treatment heterogeneity. To assess dynamic treatment effects and evaluate the underlying identifying assumptions more appropriately, we turn to the estimator proposed by Sun & Abraham (2021).

The Sun & Abraham (2021) estimator identified several significant post-treatment periods. However, these effects were primarily short-lived, varied across time and thresholds, and were interspersed with non-significant intervals. While we employed municipality-month controls to reduce seasonality exhibited in vacancies and unemployment, some residual seasonality remained and is exhibited across the different capacity thresholds (Figures 7 and 8). It should be noted that the presence of seasonality impacts the stationarity assumption and can further bias the estimator of a standard TWFE. Moreover, the re-estimations with the synthetic difference-in-differences (sDiD) estimator were not statistically significant, indicating no robust treatment effects. This further suggests that the initial TWFE-based findings are likely driven by model bias rather than substantive effects.

Lastly, model specifications influenced how trends were incorporated in our analysis. While we ran TWFE and sDiD on both specifications (levels as well as trends), the Sun & Abraham estimator was applied using levels only, as it is not designed to accommodate first-differenced trends. This is a methodological characteristic of the estimator and should be kept in mind when interpreting differences across model results.

*Data Constraints and Limitations.* Our findings are influenced by the limitations inherent in the available data. The use of proxies, such as financial assistance as an indicator of income, introduces potential measurement imprecision which may limit the ability to capture more nuanced socio-economic effects. This, in combination with earlier mentioned limitations related to the represented sub-sample of the population, and potential lagged effects, are limitations that likely contributed to generally small and statistically insignificant results. Apart from potential bias resulting from the small sample size, additional complexities arise from seasonality in our data, particularly in municipalities reliant on agriculture or tourism, affecting estimates of unemployment and vacancies. Despite the use of controls, fluctuations persist to an extent, thus presenting a challenge to full statistical control for these outcome variables.

Secondly, despite using high-frequency, municipality-level data, our final sample is relatively small. To ensure comparability and adequate pre-, and post-treatment periods, our analysis focussed on 14 treated municipalities. In comparison, the sample of control municipalities remained significantly larger after outlier exclusion. This disparity may have contributed to larger standard errors and wider confidence intervals in the estimated models, reducing the precision and consistency of estimates. They may also help explain the observed significance in the sDiD job vacancies levels estimates at the 20 MW capacity threshold, a result that did not hold under alternative cutoffs (10 and 15 MW) and was likely influenced by a small subset of treated municipalities, limiting its robustness. Moreover, as a result of outliers and several gaps in the financial assistance dataset, corresponding samples of treated municipalities are particularly small (between n=3 and n=7). Consequently, the samples for 15 MW and 20 MW include the same municipalities, and the Sun & Abraham estimator could only be applied to the 10 MW project capacity threshold. These factors limit the robustness analysis of financial assistance.

It is important to consider the economic interpretation of the empirical design choices. In particular, municipalities that experienced frequent treatment (multiple wind power developments), were excluded from the analysis as the applied models do not accommodate staggered adoption both within- and across units. However, these municipalities are likely to have seen greater cumulative investment and potentially stronger local economic effects. Their exclusion may therefore bias the estimated average treatment effect downward, underestimating the full economic impact of wind power development. This design trade-off prioritises internal validity but may limit the external relevance of the findings to high-exposure contexts.

*Additional Evidence, Policy Implications and Further Research.* While this study found no consistent evidence of local economic effects from wind power developments in Sweden, this result aligns with much of the existing literature, which similarly reports limited income effects and weak or short-term labour market impacts. This begs the question of whether wind power development provides sufficient direct, and tangible economic benefits to municipalities to influence local economic development, and with that, make wind power more attractive to host.

Across Swedish municipalities, there is a recurring perception that the economic benefits of wind power developments are limited or unevenly distributed (Lindvall, Sörqvist, & Barthel (2024) and Lindvall (2023)). In a complementary analysis, we examined the relationship between county-level income inequality (through the use of GINI coefficients) and rejection shares on wind projects between 2014 and 2022. We find a moderate positive correlation ($r$ = 0.65), suggesting that inequality may play a role in local resistance to wind power, aligning with prior qualitative findings on distributive justice in energy transitions (Lindvall, 2023).

In contrast to Sweden's predominantly informal compensation arrangements between the wind power developer and host municipality, neighbouring countries like Norway and Finland impose wind power production taxes (Swedenergy, Finnish Energy, Green Power Denmark, Renewables Norway, 2024). Similarly, Germany's 2023 reform of the national renewable energy law (EEG) introduced direct payments from operators to municipalities relative to produced kW/h (Bundesministerium der Justiz, 2024). Assessments on their local economic impact and whether these mechanisms can reduce resistance and support local development would be valuable contributions in future empirical work.

Other relevant areas for future research concern the broader socio-economic and technological impacts of wind power development at the local level, particularly how it shapes trajectories of modernisation and clean energy transitions. Wind power is frequently framed not only as a means of energy production but also as a symbol of progress and sustainability. Municipality administrators often see renewable energy as a branding opportunity and a catalyst for attracting both related and new businesses and industries (Rygg, 2012). Beyond the core function of energy production, wind power projects often act as triggers for broader regional development. The preparation of sites for wind park construction can lead to infrastructure investments, such as for improved roads and transportation networks.

Lastly, while our study focussed primarily on traditional economic indicators, future research could explore the technological opportunities of wind infrastructure. The height and structural design of wind turbines make them ideal for hosting digital infrastructure, including environmental sensors, communication equipment, as well as edge computing data servers[2]. As digital connectivity and localised data processing become increasingly central to rural and regional development, understanding these complementary uses of wind infrastructure could broaden the scope of their socio-economic impact.

[2] Borchen-Etteln, Germany, hosts servers for their locally owned cloud and digital village twin, in a wind turbine of a near-by wind park (Deutschlandfunk - Kultur, 2025).

# 8. Conclusion

This thesis examined whether wind power developments between 2014 and 2019 generated tangible socio-economic benefits for host municipalities in Sweden. While wind energy enjoys strong national support and plays a growing role in Sweden's renewable energy strategy, local resistance remains high. Our analysis, using advanced difference-in-differences methods, found no statistically significant effects on financial assistance, unemployment, or job vacancies.

These findings suggest that current wind development practices may not translate into significant local economic gains, aligning with several recent studies indicating that the local economic impacts of wind power can be limited, particularly post-construction. It also underscores the importance of distinguishing between national energy benefits and local economic incentives. In such a context, local resistance may not stem from an opposition to renewable energy per se, but from the perception that costs (e.g. environmental, aesthetic, or procedural) are borne locally, while benefits accrue elsewhere.

These results provide a possible explanation for the persistent use of the municipal veto and contribute to the broader discussion on distributive fairness in the green transition. In particular, the absence of tangible economic benefits raises questions about how wind power projects might be structured to generate more visible or lasting gains for host communities, whether through direct compensation, revenue-sharing schemes, or stronger local employment guarantees.

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# Note on the Utilisation of AI Tools

In writing this thesis, we have utilised generative AI tools such as ChatGPT and Grammarly, mainly for debugging and writing code, as well as in some instances for grammatical correction and flow of text.

We understand inherent risks with AI usage, such as hallucinations. To mitigate these risks, we have checked and cross-referenced its output with published sources throughout the writing process. No AI was used to summarise academic papers or other sources in any capacity.

# Appendix

## Appendix 1: Control Groups for Dependent Variable Financial Assistance per Household

Control Groups for Dependent Variable *Financial Assistance per Household*

| | **Financial Assitance per Household - Levels** | **Financial Assistance per Household - Trends** |
|---|---|---|
| Control group after removing missing values and outliers | Alvesta, Ängelholm, Åre, Askersund, Bengtsfors, Berg, Bollnäs, Borås, Borgholm, Bräcke, Bromölla, Gislaved, Gnesta, Gnosjö, Grästorp, Gullspång, Härnösand, Hörby, Karlskrona, Karlstad, Kävlinge, Kinda, Klippan, Kristianstad, Kungälv, Kungsbacka, Lekeberg, Lidköping, Lilla Edet, Ljungby, Ljusdal, Lomma, Mark, Mellerud, Mjölby, Mora, Mörbylånga, Motala, Mullsjö, Munkedal, Norrköping, Ödeshög, Örebro, Östhammar, Ovanåker, Sävsjö, Simrishamn, Skara, Skellefteå, Söderhamn, Sollefteå, Sotenäs, Svedala, Svenljunga, Tanum, Tjörn, Torsås, Trollhättan, Uppvidinge, Valdemarsvik, Vara, Varberg, Vårgårda, Värnamo, Växjö, Vetlanda, Vimmerby, Ystad | Alvesta, Ängelholm, Åre, Askersund, Bengtsfors, Berg, Bollnäs, Borås, Borgholm, Bräcke, Bromölla, Gislaved, Gnesta, Gnosjö, Grästorp, Gullspång, Härnösand, Hörby, Karlskrona, Karlstad, Kävlinge, Kinda, Klippan, Kristianstad, Kungälv, Kungsbacka, Lidköping, Ljungby, Ljusdal, Mark, Mellerud, Mjölby, Mora, Mörbylånga, Motala, Mullsjö, Norrköping, Ödeshög, Örebro, Östhammar, Ovanåker, Skellefteå, Söderhamn, Sollefteå, Svedala, Svenljunga, Tanum, Tjörn, Torsås, Trollhättan, Uppvidinge, Valdemarsvik, Vara, Varberg, Vårgårda, Värnamo, Växjö, Vetlanda, Vimmerby, Ystad |
| Number of municipalities in control group | 68 | 60 |

Table A1 - Control Groups for Dependent Variable Financial Assistance per Household
*Tables and selection made by the authors.*

# Appendix 2: Control Groups for Dependent Variable Job Vacancies per capita

Control Groups for Dependent Variable *Job Vacancies per capita*

| | **Job Vacancies per capita - Levels** | **Job Vacancies per capita - Trends** |
|---|---|---|
| Control group after removing missing values and outliers | Åmål, Ängelholm, Åre, Årjäng, Askersund, Åstorp, Båstad, Bengtsfors, Berg, Bjuv, Bollnäs, Borås, Borgholm, Bräcke, Eslöv, Falkenberg, Falköping, Färgelanda, Gislaved, Gnesta, Gnosjö, Grästorp, Gullspång, Hällefors, Halmstad, Härnösand, Hässleholm, Helsingborg, Hjo, Hofors, Höör, Hörby, Hultsfred, Hylte, Kalix, Kalmar, Karlskrona, Karlstad, Kävlinge, Kiruna, Klippan, Kristianstad, Kungälv, Kungsbacka, Laholm, Landskrona, Lekeberg, Lerum, Lidköping, Lilla Edet, Linköping, Ljungby, Ljusdal, Lomma, Lysekil, Malmö, Mark, Markaryd, Mellerud, Mjölby, Mönsterås, Mora, Mörbylånga, Motala, Mullsjö, Munkedal, Nässjö, Nordmaling, Norrköping, Norrtälje, Norsjö, Nybro, Ödeshög, Örebro, Östhammar, Ovanåker, Överkalix, Ragunda, Robertsfors, Säffle, Sävsjö, Simrishamn, Sjöbo, Skellefteå, Skurup, Söderhamn, Sollefteå, Sotenäs, Staffanstorp, Stenungsund, Storuman, Strängnäs, Svalöv, Svedala, Svenljunga, Tanum, Tierp, Tingsryd, Tjörn, Tomelilla, Torsås, Trollhättan, Uddevalla, Ulricehamn, Umeå, Uppvidinge, Vaggeryd, Valdemarsvik, Vänersborg, Vara, Varberg, Vårgårda, Västervik, Växjö, Vetlanda, Vingåker, Ystad | Alvesta, Åmål, Aneby, Ängelholm, Åre, Årjäng, Arvidsjaur, Askersund, Åstorp, Båstad, Bengtsfors, Bjuv, Bollnäs, Borås, Borgholm, Bräcke, Bromölla, Dals-Ed, Eslöv, Falkenberg, Falköping, Gislaved, Gnesta, Gnosjö, Hällefors, Halmstad, Härjedalen, Härnösand, Hässleholm, Helsingborg, Hofors, Höör, Hörby, Hultsfred, Hylte, Kalix, Kalmar, Karlskrona, Karlstad, Kävlinge, Kiruna, Klippan, Kristianstad, Kungälv, Kungsbacka, Landskrona, Lerum, Lidköping, Linköping, Ljungby, Lomma, Lysekil, Malmö, Mark, Markaryd, Mellerud, Mjölby, Mönsterås, Mora, Mörbylånga, Motala, Mullsjö, Munkedal, Nässjö, Norrköping, Norrtälje, Norsjö, Nybro, Ödeshög, Örebro, Östhammar, Ovanåker, Ragunda, Robertsfors, Säffle, Sävsjö, Simrishamn, Sjöbo, Skellefteå, Skurup, Söderhamn, Sollefteå, Sotenäs, Staffanstorp, Stenungsund, Strängnäs, Svalöv, Svedala, Svenljunga, Tanum, Tierp, Tingsryd, Tjörn, Tomelilla, Torsås, Trollhättan, Uddevalla, Ulricehamn, Umeå, Uppvidinge, Vaggeryd, Valdemarsvik, Vänersborg, Vara, Varberg, Vårgårda, Västervik, Växjö, Vetlanda, Vimmerby, Vingåker, Ystad |
| Number of municipalities in control group | 117 | 112 |

Table A2 - Control Groups for Dependent Variable Job Vacancies per capita
*Tables and selection made by the authors.*

# Appendix 3: Control Groups for Dependent Variable Unemployment Rate

Control Groups for Dependent Variable *Unemployment Rate*

| | **Unemployment Rate - Levels** | **Unemployment Rate - Trends** |
|---|---|---|
| Control group after removing missing values and outliers | Alvesta, Åmål, Aneby, Ängelholm, Åre, Årjäng, Arvidsjaur, Askersund, Åstorp, Båstad, Bengtsfors, Berg, Bjuv, Bollnäs, Borås, Borgholm, Bräcke, Bromölla, Dals-Ed, Eslöv, Falkenberg, Falköping, Färgelanda, Gislaved, Gnesta, Gnosjö, Grästorp, Gullspång, Hällefors, Halmstad, Härjedalen, Härnösand, Hässleholm, Helsingborg, Hjo, Hofors, Höör, Hörby, Hultsfred, Hylte, Kalix, Kalmar, Karlskrona, Karlstad, Kävlinge, Kinda, Kiruna, Klippan, Kristianstad, Kungälv, Kungsbacka, Laholm, Landskrona, Lekeberg, Lerum, Lidköping, Lilla Edet, Linköping, Ljungby, Ljusdal, Lomma, Lysekil, Malmö, Mark, Markaryd, Mellerud, Mjölby, Mönsterås, Mora, Mörbylånga, Motala, Mullsjö, Munkedal, Nässjö, Nordmaling, Norrköping, Norrtälje, Norsjö, Nybro, Ödeshög, Örebro, Östhammar, Ovanåker, Ragunda, Robertsfors, Säffle, Sävsjö, Simrishamn, Sjöbo, Skara, Skellefteå, Skurup, Söderhamn, Sollefteå, Sotenäs, Staffanstorp, Stenungsund, Storuman, Strängnäs, Svalöv, Svedala, Svenljunga, Tanum, Tierp, Tingsryd, Tjörn, Tomelilla, Torsås, Trollhättan, Uddevalla, Ulricehamn, Umeå, Uppvidinge, Vaggeryd, Valdemarsvik, Vänersborg, Vara, Varberg, Vårgårda, Värnamo, Västervik, Växjö, Vetlanda, Vimmerby, Vingåker, Ystad | Alvesta, Åmål, Ängelholm, Åre, Årjäng, Åsele, Askersund, Åstorp, Båstad, Bengtsfors, Berg, Bjuv, Bollnäs, Borås, Bräcke, Bromölla, Eslöv, Falkenberg, Falköping, Färgelanda, Gislaved, Gnesta, Gnosjö, Grästorp, Gullspång, Hällefors, Halmstad, Härjedalen, Härnösand, Hässleholm, Helsingborg, Hjo, Hofors, Höör, Hörby, Hultsfred, Hylte, Kalmar, Karlskrona, Karlstad, Kävlinge, Kinda, Kiruna, Klippan, Kristianstad, Kungälv, Kungsbacka, Laholm, Landskrona, Lerum, Lidköping, Lilla Edet, Linköping, Ljungby, Ljusdal, Lomma, Lysekil, Malmö, Mark, Markaryd, Mellerud, Mjölby, Mönsterås, Mora, Mörbylånga, Motala, Mullsjö, Munkedal, Nässjö, Nordmaling, Norrköping, Norrtälje, Norsjö, Nybro, Ödeshög, Örebro, Östhammar, Ovanåker, Ragunda, Robertsfors, Säffle, Sävsjö, Simrishamn, Sjöbo, Skara, Skellefteå, Skurup, Söderhamn, Sollefteå, Staffanstorp, Stenungsund, Storuman, Strängnäs, Svalöv, Svedala, Svenljunga, Tierp, Tingsryd, Tjörn, Tomelilla, Torsås, Trollhättan, Uddevalla, Ulricehamn, Umeå, Uppvidinge, Vaggeryd, Valdemarsvik, Vänersborg, Vara, Varberg, Vårgårda, Värnamo, Västervik, Växjö, Vetlanda, Vimmerby, Vingåker, Ystad |
| Number of municipalities in control group | 126 | 119 |

Table A3 - Control Groups for Dependent Variable Unemployment Rate
*Tables and selection made by the authors.*

# Appendix 4: Results of TWFE for 10 MW Cutoff

Estimates of Two-Way Fixed Effects (10 MW capacity cutoff)

| | **Estimate** | **Std. Err.** | **t** | **P > \| t \|** |
|---|---|---|---|---|
| **Financial Assistance per Household** | | | | |
| Levels | 255.0770* | 139.3530 | 1.83 | 0.068 |
| Trends | -0.003509* | 0.001797 | -1.95 | 0.052 |
| **Job Vacancies per capita** | | | | |
| Levels | 0.000070 | 0.000367 | 0.19 | 0.852 |
| Trends | -0.055366** | 0.017562 | -3.15 | 0.014 |
| **Unemployment Rate** | | | | |
| Levels | -0.004744*** | 0.001340 | -3.54 | 0.004 |
| Trends | -0.003550** | 0.001337 | -2.65 | 0.026 |

Note: Stars indicate significance levels: ******* $p < 0.01$, ****** $p < 0.05$, ***** $p < 0.1$.

Table A4 - Results of TWFE for 10 MW Cutoff
*Data extracted from (Socialstyrelsen, 2025) and (Arbetsförmedlingen, 2025).*

# Appendix 5: Results of TWFE for 20 MW Cutoff

Estimates of Two-Way Fixed Effects (20 MW capacity cutoff)

| | Estimate | Std. Err. | t | P > \| t \| |
|---|---|---|---|---|
| **Financial Assistance per Household** | | | | |
| Levels | | | | |
| Trends | | | | |
| **Job Vacancies per capita** | | | | |
| Levels | -0.000225 | 0.000403 | -0.56 | 0.596 |
| Trends | -0.042232 | 0.020412 | -2.07 | 0.130 |
| **Unemployment Rate** | | | | |
| Levels | -0.006102*** | 0.001120 | -5.45 | 0.001 |
| Trends | 0.004197* | 0.001756 | 2.39 | 0.062 |

Note: Stars indicate significance levels: ******* $p < 0.01$, ****** $p < 0.05$, ***** $p < 0.1$.

Table A5 - Results of TWFE for 20 MW Cutoff

Note*: No estimates for Financial Assistance are presented since the treatment sample for financial assistance is identical at the 15 and 20 MW cutoffs. Data extracted from (Socialstyrelsen, 2025) and (Arbetsförmedlingen, 2025).*

## Appendix 6: Sun & Abraham Event Study at 10 MW Cutoff for Financial Assistance (Levels)

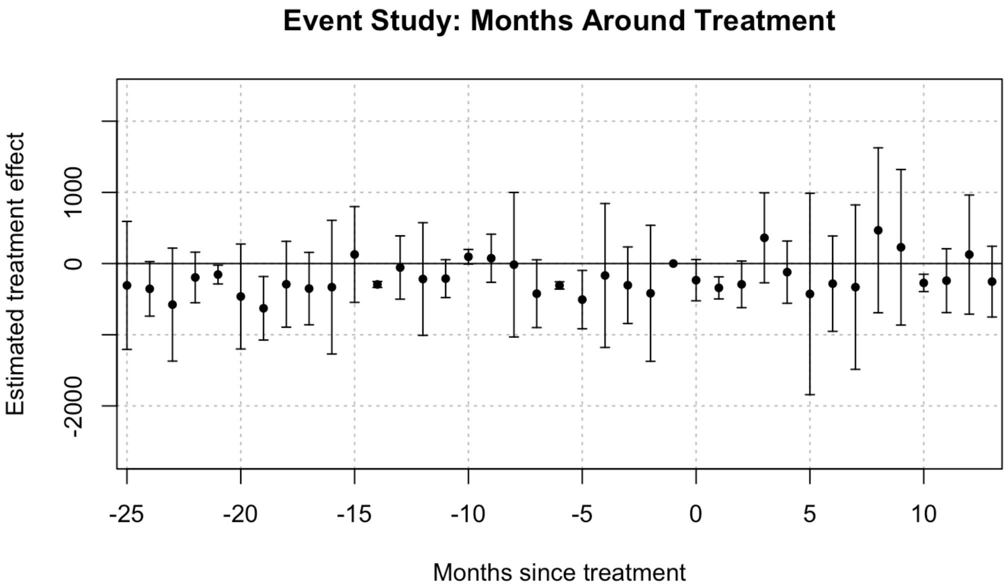


Table A6 - Sun & Abraham Event Study at 10 MW Cutoff for Financial Assistance (Levels)
*Data extracted from (Socialstyrelsen, 2025).*

## Appendix 7: Sun & Abraham Event Study at 10 MW Cutoff for Job Vacancies (Levels)

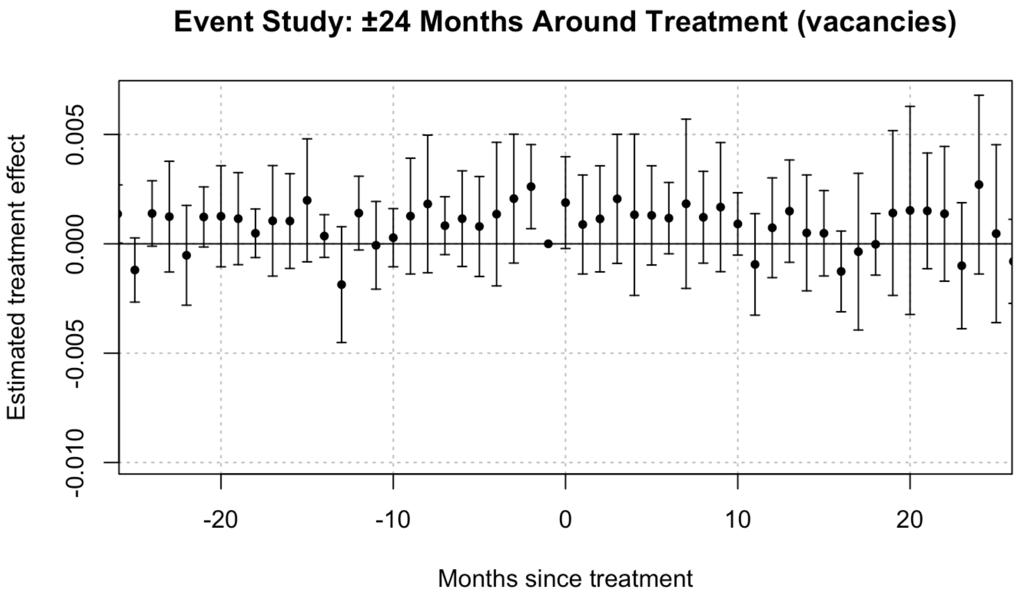


Table A7 - Sun & Abraham Event Study at 10 MW Cutoff for Job Vacancies (Levels)
*Data extracted from (Arbetsförmedlingen, 2025).*

## Appendix 8: Sun & Abraham Event Study at 10 MW Cutoff for Unemployment Rate (Levels)

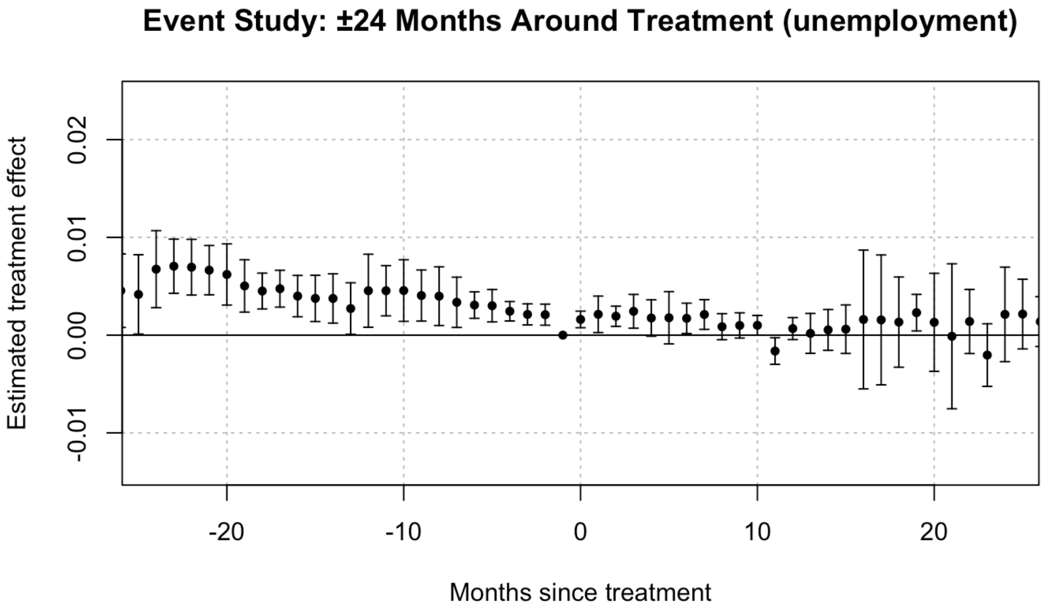


Table A8 - Sun & Abraham Event Study at 10 MW Cutoff for Unemployment Rate (Levels)
*Data extracted from (Arbetsförmedlingen, 2025).*

## Appendix 9: Sun & Abraham Event Study at 20 MW Cutoff for Job Vacancies (Levels)

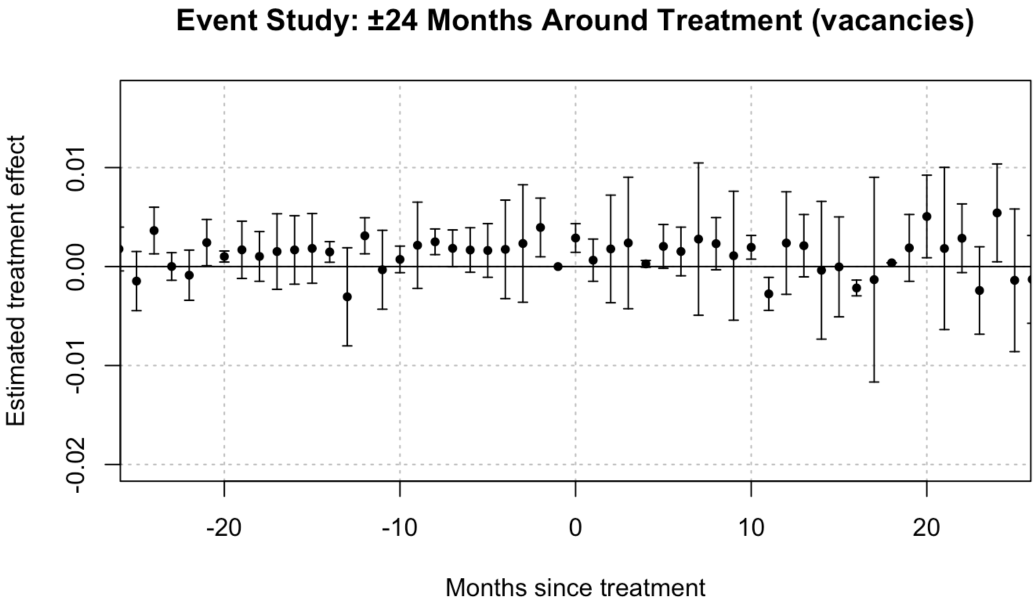


Table A9 - Sun & Abraham Event Study at 20 MW Cutoff for Job Vacancies (Levels)
*Data extracted from (Arbetsförmedlingen, 2025).*

## Appendix 10: Sun & Abraham Event Study at 20 MW Cutoff for Unemployment Rate (Levels)

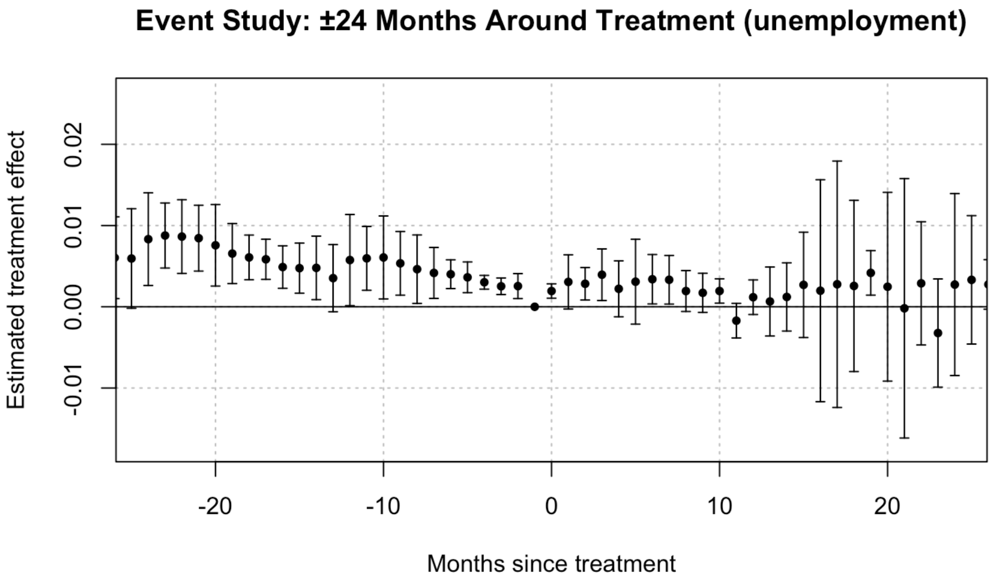


Table A10 - Sun & Abraham Event Study at 20 MW Cutoff for Unemployment Rate (Levels)
*Data extracted from (Arbetsförmedlingen, 2025).*

# Appendix 11: Results of sDiD for 10 MW Cutoff

Estimates of Synthetic Difference-in-Differences (10 MW capacity cutoff)

| | Estimate | Std. Err. | t | P > \| t \| | [95% Conf. Interval] |
|---|---|---|---|---|---|
| **Financial Assistance per Household** | | | | | |
| *Levels* | | | | | |
| With Controls | -86.1235 | 137.633 | -0.63 | 0.531 | [-356.000, 183.633] |
| Without Controls | -83.5283 | 112.539 | -0.74 | 0.458 | [-304.000, 137.045] |
| *Trends* | | | | | |
| With Controls | 0.00080 | 0.00418 | 0.19 | 0.848 | [-0.00739, 0.00900] |
| Without Controls | -0.00084 | 0.00384 | 0.22 | 0.827 | [-0.00669, 0.00837] |
| **Job Vacancies per capita** | | | | | |
| *Levels* | | | | | |
| With Controls | -0.00035 | 0.00045 | -0.77 | 0.443 | [-0.00124, 0.00054] |
| Without Controls | -0.00035 | 0.00043 | -0.82 | 0.414 | [-0.00120, 0.00049] |
| *Trends* | | | | | |
| With Controls | -0.01258 | 0.01669 | -0.75 | 0.451 | [-0.04528, 0.02010] |
| Without Controls | -0.01324 | 0.01951 | -0.68 | 0.497 | [-0.05148, 0.02500] |
| **Unemployment Rate** | | | | | |
| *Levels* | | | | | |
| With Controls | -0.00020 | 0.00093 | -0.21 | 0.831 | [-0.00203, 0.00163] |
| Without Controls | -0.00019 | 0.00098 | -0.20 | 0.842 | [-0.00211, 0.00172] |
| *Trends* | | | | | |
| With Controls | -0.00006 | 0.00161 | -0.04 | 0.970 | [-0.00322, 0.00310] |
| Without Controls | -0.00009 | 0.00171 | -0.05 | 0.958 | [-0.00345, 0.00327] |

Note: Stars indicate significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A11 - Results of sDiD for 10 MW Cutoff
*Data extracted from (Socialstyrelsen, 2025) and (Arbetsförmedlingen, 2025).*

# Appendix 12: Results of sDiD for 20 MW Cutoff

Estimates of Synthetic Difference-in-Differences (20 MW capacity cutoff)

| | **Estimate** | **Std. Err.** | **t** | **P > \| t \|** | **[95% Conf. Interval]** |
|---|---|---|---|---|---|
| **Financial Assistance per Household** | | | | | |
| *Levels* | | | | | |
| With Controls | | | | | |
| Without Controls | | | | | |
| *Trends* | | | | | |
| With Controls | | | | | |
| Without Controls | | | | | |
| **Job Vacancies per capita** | | | | | |
| *Levels* | | | | | |
| With Controls | -0.00117** | 0.00057 | -2.07 | 0.039 | [-0.00229, -0.00006] |
| Without Controls | -0.00118** | 0.00056 | -2.11 | 0.035 | [-0.00227, -0.00009] |
| *Trends* | | | | | |
| With Controls | -0.03837* | 0.02324 | -1.65 | 0.099 | [-0.08392, 0.00720] |
| Without Controls | -0.03908 | 0.02823 | -1.38 | 0.166 | [-0.09441, 0.01620] |
| **Unemployment Rate** | | | | | |
| *Levels* | | | | | |
| With Controls | -0.00071 | 0.00073 | -0.97 | 0.334 | [-0.00214, 0.00073] |
| Without Controls | -0.00069 | 0.00063 | -1.11 | 0.268 | [-0.00192, 0.00053] |
| *Trends* | | | | | |
| With Controls | -0.00191 | 0.00363 | -0.53 | 0.598 | [-0.00903, 0.00521] |
| Without Controls | -0.00194 | 0.00465 | -0.42 | 0.676 | [-0.01104, 0.00717] |

Note: Stars indicate significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A12 - Results of sDiD for 20 MW Cutoff

Note*: no estimates for Financial Assistance are presented since the treatment sample for financial assistance is identical at the 15 and 20 MW cutoffs. Data extracted from (Socialstyrelsen, 2025) and (Arbetsförmedlingen, 2025).*

# Appendix 13: Correlation Matrix for Income Inequality Indicators and Rejection Share

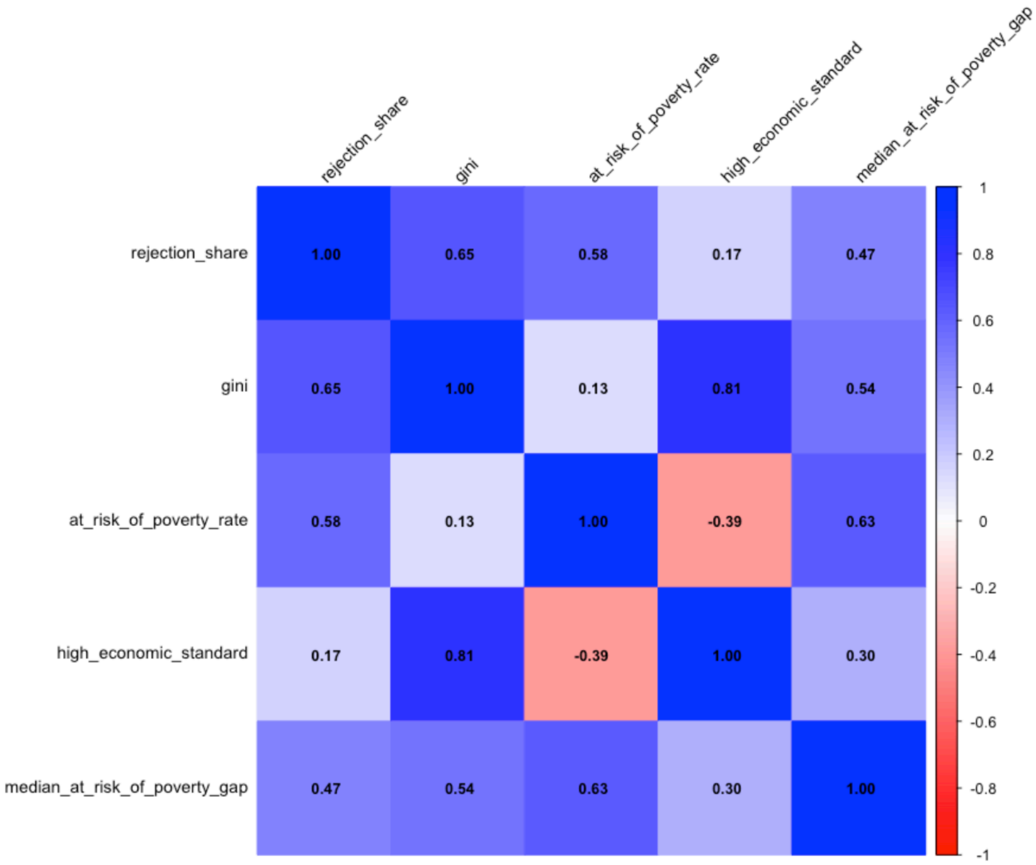


Table A13 - Correlation Matrix for Income Inequality Indicators and Rejection Share
*Data extracted from (Westander & Henryson, 2023) and (Statistikdatabasen, 2025)*