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Fast Cars and Debutantes: Manufacturing-Induced Firm and Labor Outcomes in the South

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a thesis submitted to the Faculty of Emory College of Arts and Sciences
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Abstract

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The American South has emerged as a major hub for manufacturing investment, driven by its comparative advantages in labor costs, regulation, and infrastructure. This study investigates the impact of large-scale auto manufacturing plant openings on firm and labor market outcomes across the region. Focusing on fourteen plants opened between 1981 and 2011, I use a Difference-in-Differences (DiD) framework and commuting zones as the geographic unit of analysis to estimate aggregate effects on firm density, employment, and real wages. The results show significant and persistent increases in the number of large firms and employment rates, consistent with agglomeration and clustering effects. Impacts on small firms are more heterogeneous, with evidence suggesting initial declines followed by recovery. Wage gains are modest but statistically significant, reflecting gradual labor market adjustments. These findings contribute new evidence on the regional spillovers of industrial investment and provide a methodological framework for analyzing manufacturing-led growth in the American South.

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1 Introduction

In recent decades, the American South has emerged as an unexpected center of industrial revitalization. Once overshadowed by the manufacturing dominance of the Midwest and Northeast, the South now leads the nation in manufacturing employment, attracting a wave of large-scale industrial investment. This transformation is driven by distinct comparative advantages, including lower labor and energy costs, right-to-work laws, minimal union presence, and access to key transportation networks (Holmes 1998; Moore 1998). These features have made the South an increasingly attractive destination for domestic and foreign manufacturers alike, particularly in the automobile industry, where new factories signal more than just corporate expansion —they reshape and invigorate regional economies as firm and labor market anchors. Today, the South[1] has surpassed the Midwest in total manufacturing jobs and has not experienced the drastic decline in manufacturing employment that was previously anticipated (Buera and Kaboski 2012).

As manufacturing further expands to the South, what is the aggregate economic impact of large-scale manufacturing factory openings on firms and labor markets in the American South? While the role of manufacturing in regional development has been widely studied, most work focuses on case studies or narrow geographic units. In contrast, I take a broader empirical approach, quantifying the regional spillover effects of fourteen large-scale automobile manufacturing factories opened between 1981 and 2011. Using a panel of county-level data from 1976 to 2016 and employing modern Difference-in-Differences (DiD) techniques, I estimate the effects of these openings on the number of firms, firm size composition, employment rates, labor force potential, and real wages.

Manufacturing continues to move to the South, both as a result of intrinsic firm decisions and policy incentives; by understanding the effects of these relocations both policymakers and scholars can better address and study growth and development in the region. Despite ongoing structural shifts toward a service-oriented economy, manufacturing remains a vital mechanism for regional growth - especially in rural or economically lagging areas. Auto-manufacturing factories, as capital-intensive and labor-demanding enterprises, often act as anchor institutions, drawing in suppliers, complementary businesses, and workers (Greenstone, Hornbeck, and Moretti 2010; Moretti 2010; Hornbeck and Rotemberg 2019). Yet, empirical evidence on the aggregate impact of such investments - particularly beyond the immediate host county - remains limited.

Given the ongoing geographic shift of American manufacturing to the South, this paper highlights how modern large-scale manufacturing manifests in positive firm and labor out-

[1] The American South, referred to as the South in this paper, is classified as 17 states and three constructed subregions per the U.S. Census Bureau. These states include Alabama, Arkansas, Delaware, District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, West Virginia.

comes. As firms continue to relocate from traditional industrial centers to regions with more favorable economic conditions, understanding the broader consequences of these moves becomes essential. Policymakers increasingly rely on industrial incentives and infrastructure investment to attract manufacturers, yet the long-term, region-wide economic returns of such strategies remain uncertain.

To quantify these effects, I treat the commuting zone (CZ) as the unit of treatment assignment, capturing economic spillovers that cross county borders but remain within local labor markets (Duranton and Venables 2018; Autor, Dorn, and Hanson 2013). I apply standard and staggered Difference-in-Differences (DiD) estimators, including the Extended Two-Way Fixed Effects and Callaway and Sant'Anna approaches, which allow for variation in treatment timing and effect heterogeneity. This design choice strengthens causal inference and allows for dynamic estimation of treatment effects over time.

My results show that factory openings significantly increase the number of large firms and employment levels, with effects that persist and grow over time. Wages also rise, though more gradually. In contrast, effects on small firms are more ambiguous —possibly due to consolidation, competition, or firm growth out of the small size threshold. The robustness of these results is addressed in several alternative sample and treatment timing tests, finding results to be robust but having treatment effect heterogeneity when looking at individual firms. This paper presents a regional, data-driven assessment of industrial investment impacts using a comprehensive empirical framework to the literature. While I do not distinguish between foreign and domestic manufacturers in this analysis, future research could investigate whether their impacts differ - an important consideration for local and state-level industrial policy. Ultimately, these findings underscore the continued relevance of manufacturing as a driver of economic development, even in an economy increasingly dominated by services (Lawrence 2017; Fort et al. 2018; Economist 2023).

2 Literature Review

The economic impact of large-scale manufacturing factories extends beyond the firms themselves, influencing regional business ecosystems and labor markets through spillovers. These spillovers materialize in the form of firm clustering, labor market effects, and productivity gains, creating long-term economic multipliers. The economic geography literature has long emphasized the role of agglomeration economies in shaping firm and worker outcomes (Marshall 1890; Krugman 1991). Large-scale manufacturing factories generate localized spillovers through input-output linkages, labor pooling, and knowledge diffusion (Moretti 2010; Greenstone, Hornbeck, and Moretti 2010). These externalities induced by spillovers enhance regional productivity and influence subsequent firm entries, expansions, and employment dynamics (Duranton and Puga 2004). Theories of urban economics and industrial organization suggest that firms benefit from clustering, particularly in specialized industries where knowledge spillovers are prominent (Rosenthal and Strange 2004). Furthermore, recent empirical studies confirm that large manufacturing firms serve as anchor institutions, creating economic networks that attract suppliers and complementary businesses (Hornbeck and Rotemberg 2019). Research has further linked industrial clustering to wage effects, firm entry rates, and innovation spillovers (Ellison, Glaeser, and Kerr 2010).

The literature also explores how different industries experience agglomeration economies, showing that certain industry type can expand or contract spillover effects. Henderson (1997) distinguishes between urbanization and localization externalities, showing that manufacturing spillovers tend to be strongest in industries with extensive supplier linkages. Ellison, Glaeser, and Kerr (2010) further emphasize that firms in industries with complex production processes, such as automobile manufacturing, benefit disproportionately from supply-chain linkages and shared labor pools. This distinction is crucial for understanding why large-scale manufacturing factories generate spillovers beyond their immediate location. Additionally, these spatial spillovers extend beyond the immediate location of firm entry, influencing not only the host county but also neighboring regions through labor market integration and infrastructure development (Duranton and Venables 2018). These effects can be stronger in rural areas where large firms act as regional employment hubs, diffusing economic activity across a broader geographic space (Kline and Moretti 2014). Effectively, the location and relocation of firms not only affects the simplest unit of the geography or classification (e.g. the county where the factory is located or its industry) but also spill over into other geographic units or industry. For my paper it is imperative to understand the context of these geographic spillovers as these local labor markets heavily influence one another, especially in more non-urbanized areas of the South where these firms are located.

The reason why these firms chose to locate in the South is due to the concept of the Southern comparative advantage (lower labor costs, land affordability, and favorable regulations), resulting regional economic development (Holmes 1998; Autor, Dorn, and Hanson 2013). The South's comparative advantage in manufacturing stems from right-to-work laws, transportation networks, and energy affordability, which have incentivized firm relocation (Holmes 1998; Moore 1998). Economic development incentives have been widely studied in relation to factory openings and local employment effects (Greenstone, Hornbeck, and Moretti 2010). The literature suggests that government subsidies and infrastructure investments can significantly boost firm attraction and industrial expansion (Chirinko and Wilson 2017). However, while firm location decisions have been extensively analyzed, the spillover effects of these firms on the broader regional economy remain underexplored and unquantified.

In addition to the location characteristics of the Southern comparative advantage, state and local economic development policies play a crucial role in determining manufacturing factory locations; yet, their long-term benefits remain debated. Bartik (1991) finds that incentives such as tax breaks and infrastructure investments can successfully attract firms but may not always generate sustained economic benefits if firms relocate once incentives expire. This raises concerns about whether state and county-led efforts to attract manufacturing factories create genuine spillovers or simply shift economic activity across regions without net gains. The critical point here is that counties do incentivize firms to relocate; however, these incentives may be only driven by the host county and the outcome of that incentive may spill over into surrounding labor markets. My paper attempts to capture these spillovers and expand the literature by looking at local labor markets as opposed to singular host counties while looking at the outcomes over time.

Beyond employment and firm expansion, large manufacturing factories contribute to skill upgrading and human capital development. Blanchard and Olney (2017) find that industrial investments lead to increased educational attainment and workforce training, as local firms adapt to meet the labor demands of new manufacturing entrants. Additionally, the literature suggests that the presence of high-tech manufacturing clusters enhances workforce skills through technology diffusion and knowledge-sharing networks (Baldwin and Okubo 2014). These mechanisms provide a crucial understanding of how firm location choices contribute to long-term labor market improvements beyond immediate job creation but have not been studied in the context of local spillovers.

Greenstone, Hornbeck, and Moretti (2010) find that counties that win manufacturing factory competitions experience higher productivity growth, wage increases, and local economic expansion. These effects stem from the indirect employment multiplier, where the presence of an anchor firm catalyzes job creation in related sectors (Moretti 2010). This supplements previous work in the quasi-experimental field where Greenstone and Moretti (2003) compare counties that win and lose industrial factory bids, finding that winning counties experience not only immediate employment growth but also long-term productivity gains. These findings support the argument that manufacturing expansions can alter economic trajectories over extended periods, making them a key focus for research on industrial policy; yet the scope of the analysis is limited to singular counties and does not address the regional spillovers that these manufacturing firms have - especially in rural areas which are very easily impacted.

Empirical research using Difference-in-Differences (DiD) and synthetic control methods provides robust evidence that manufacturing firm openings increase employment in supplier industries and service sectors (Bartik 2020; Charles, Hurst, and Notowidigdo 2018). Additionally, Moretti (2021) shows that firms relocating to industrial clusters experience higher productivity growth, benefiting from human capital externalities. From a labor economics perspective, large manufacturing factories impact wages and workforce composition. Autor, Dorn, and Hanson (2013) demonstrate that manufacturing expansion reduces local unemployment, but wage effects vary by skill level. Manufacturing clusters further create training and skill development opportunities, enhancing worker mobility and long-term earnings growth (Acemoglu and Restrepo 2020). However, spillover effects are not uniform across firms and workers. Faggio, Silva, and Strange (2017) emphasize that firms with complementary capabilities and high-skilled workers benefit more from agglomeration than smaller or lower-skilled firms. This suggests that the effects of manufacturing clusters may be concentrated among certain types of businesses and employees, making it essential to analyze spillover heterogeneity when evaluating economic impact.

Lastly, understanding cross-country analyses and multinationals also provide valuable context and insights into the study of the effects of auto-manufacturing. Studies on European and Asian manufacturing hubs indicate that foreign direct investment (FDI) in manufacturing generates long-term economic gains through technology diffusion and skill upgrading (Javorcik 2004; Bloom, Draca, and Van Reenen 2016). The entry of multinational manufacturers has been shown to stimulate local entrepreneurship and increase competition, leading to efficiency gains (Aghion, Braun, and Fedderke 2008). This is crucial for the analysis of auto-manufacturing's impact as a portion of the firms that relocated to the South are foreign multinationals, and these spillover effects may manifest similarly in the U.S.

The literature underscores the wide-ranging economic spillovers that large-scale manufacturing factory openings can generate, especially in firm and labor outcomes. While past research has established strong links between firm clustering, labor market dynamics, and regional economic growth, gaps remain in quantifying these aggregate effects in the context of geographic spillovers and heterogeneity. Additionally, the impact on firm and worker outcomes at a granular local labor market level has been historically understudied and a region-specific designation has not been applied. This paper seeks to fill three key gaps in the literature: (1) understanding of the impacts of large-scale manufacturing in the American South; (2) understanding how these firm relocations spill over into local labor markets as opposed to simply the host county; and (3) quantifying the aggregate effect that the relocations of these firms have, as opposed to a case-by-case estimate. By applying rigorous empirical methods, this paper seeks to further elucidate the role of manufacturing in shaping local economic growth and development, attempting to quantify an aggregate overall effect.

3 Methodology

3.1 Data

This paper relies on data from two primary sources: the U.S. Census Bureau's County Business Patterns (CBP) survey and the Bureau of Labor Statistics (BLS) Quarterly Census of Employment and Wages (QCEW). The CBP data provides detailed information on firm-level outcomes, including the number of establishments, employment, and annual payroll, while the BLS data offers worker-level measures, specifically employment counts and annual average weekly wages (this measure better captures the wages of employees compared to annual payroll from the BLS data which may be more reflective of salaried roles as opposed to hourly). Both datasets were restricted to cover the period from 1976 to 2016 and have been filtered for all Southern counties, allowing for a comprehensive longitudinal analysis of the region on a county-by-county basis. Furthermore, this allows for a lead and lag time of five years from the first and last treatment to measure pre-trends and treatment maturation in the event study methods employed and recommended by the literature. To assign treatment, the paper uses commuter zones (CZs). This allows me to control for labor market spillover that might occur from the opening of a manufacturing facility - building comprehensive and relevant local labor markets - as firms and workers often interact across county lines within the same economic region. Several controls are added and edited in order to capture county variation and isolate the true effect of these facilities opening.

Key variables from the CBP data include the number of firms, total employment, and payroll, broken down by industry and firm size. This dataset is particularly suited for understanding changes in firm concentrations, as it captures aggregate changes in firm births, deaths, and relocations over time. To focus on the broader impact on the firm environment, I focus on the aggregate firm count variable for each county and use the employment count variables to generate two new variables - small and large firms. Small firms are those that have employee counts of less than 50 and large firms are those with or greater than 50 employees. The large firms variable is reflective of all firms that are not considered small by the BLS standards, which is below 50 total employees (U.S. Bureau of Labor Statistics 2023); this variable captures and groups both medium and large size firms together. These two variables add granularity to the type of firms that are affected by the treatment, allowing for a better understanding of how auto-manufacturing openings affect firms of different sizes.

Aggregate labor-related outcomes are sourced from the BLS-QCEW. This dataset provides annualized county-level employment counts and average weekly wages across industries; similar to the CBP data, I have restricted this data to Southern states and for the years 1976 to 2016. These labor-level measures allow for the analysis of how the treatment may have an impact on employees directly. To ensure consistency with the CBP data, only counties with non-missing observations for employment and wages are included. I generate an employment rate variable which is the employment level of a county divided by its total population, giving a proxy for the employment capacity of a county relative to the county's population. I then generate the potential labor force variable, which is all of the working-age people (due to data granularity, those ages 18-64) of the county divided by the total population. This allows me to measure the effect that the presence of a factory has on the migration of the labor force. Lastly, all wages have been inflated to 2016 dollars to measure real impact across the staggered adoption time and the log was taken to better understand the change in wages as a percentage. Summary statistics can be in Table 2, separated by treated and untreated populations. While there are differences in the mean and standard deviations between treated and untreated counties, I show in later analysis that parallel trends hold and these differences are addressed properly through the DiD methodologies.

To address cross-county differences, this dataset was supplemented with population and demographic data from estimates constructed by the Survallience, Epidemiology, and End Results Program using U.S. Census Bureau data (SEER Program 2020). The fraction of the nonwhite population was the control variable crafted by dividing the nonwhite population by the total population. The area of a county in square miles was then appended to compute the population density, where population was divided by total area. This control seeks to provide a continuous, granular view of rurality as opposed to a binary or categorical approach to better capture variation between counties. This is with the understanding that more rural, sparse counties would have a lower density compared to higher, more urban counties. The final control was the percentage of the population that has only a high school degree from the U.S. Department of Agriculture Economic Research Services's county-level education dataset (USDA-ERS 2025b). This data was not available on an annual basis and was linearly interpolated between data points.

There are two key concerns regarding covariates in the methods I later employ. I show that these are justified covariates by performing a balance test (see Table 3) and do not find a statistically significant relationship between changes in covariates and treatment status; this justifies the usage of these covariates in this analysis as they are not considered bad controls (Pei, Pischke, and Schwandt 2019). In Appendix **??** I show an alternative specification where I fix the covariates to their most recent value before treatment (i.e. $(t = g - 1)$) where g is the adoption year of the treatment to mitigate any potential mapping of the treatment, though these covariates are balanced per the balancing test.

The final element of the data set is all information regarding treatment assignment. The treatment variable is defined as the opening year of a manufacturing facility; this yearly assignment is due to both the granularity of the BLS and QCEW data along with definite dates of when the first car rolled off the line at the respective factory. To capture labor market spillovers, I then assign the overall treatment at the Commuter Zone (CZ) level. A CZ is considered treated if any county in the CZ has a factory opened, meaning all counties in the CZ have the same treatment and adoption variables.

CZs are constructed by the U.S. Department of Agriculture based on commuting patterns from the 2000 Census (USDA-ERS 2025a). CZs provide more meaningful economic geography than counties alone, as they better capture the functional boundaries of local labor markets, given that firms often operate across county boundaries within the same CZ. While there are CZ's for the 1980s and 1990s, a majority of opening facilities occurred in the 2000s and there is no universal CZ. I decided to solely assign 2000s CZs as they are not vastly different from the 1980s and 1990s designations. In the instance where a second firm opens in CZ, I elected to drop this adoption, as I am concerned with measuring the first instance of a factory opening. This only occurs within one CZ in Tennessee; all other factory openings are mutually exclusive to their respective CZs. Treatment assignment at the CZ level for manufacturing in this manner is similar and consistent to the literature, e.g Autor, Dorn, and Hanson (2013) and Batistich and Bond (2023). Figure 1 shows how the CZs are reflected in the South.

The assignment of treatment at the CZ level helps mitigate concerns about spillovers between treated and untreated units, satisfying a key component of the Stable Unit Treatment Value Assumption (SUTVA). Since CZs represent cohesive labor markets, firms and workers interact primarily within their assigned CZ, limiting interference across treatment units. Additionally, only the first factory opening within a CZ is considered for treatment, avoiding complications from multiple treatments of varying intensity. These design choices ensure that the observed effects are attributable to treatment within the defined labor market and not due to cross-zone spillovers or variation in treatment dosage. One potential concern is that labor market effects - such as worker migration or firm supply chain adjustments - might extend beyond a single CZ. While this is possible, the structure of CZs already accounts for natural commuting patterns, minimizing systematic bias in the control group. Furthermore, because my analysis focuses on the aggregate effects of firm development and employment at the local level, any broader regional spillovers would likely dilute rather than inflate the estimated effects. As firm location decisions tend to be driven by long-term infrastructure and policy considerations rather than short-run regional fluctuations, it is unlikely that firms in untreated CZs would significantly alter their behavior in response to factory openings in adjacent CZs. This reinforces the assumption that the treatment effects remain independent across units.

As I do not have information on tax and related incentives that might have been used to encourage firms to relocate to a county, I assume that all potential incentives originate from the county and not the state. Under this assumption, SUTVA would hold as the incentives for a factory do not subtract from other incentives in the surrounding labor market nor the untreated populations. This is due to the incentives originating from the county itself and not the state. In the case where incentives originated from the state, it is possible the incentive would then subtract from other potential incentives for untreated populations, violating SUTVA. Thus, due to data availability and the nature of host-counties and local labor markets, I assume that any incentives that a factory receives originate from the host-county itself and do not affect other treat counties and untreated counties.

3.2 Model Specification

This section outlines the empirical strategy employed to estimate the causal effects of auto-manufacturing factory openings on firm and worker outcomes. Given the staggered adoption of treatment across regions, the estimation strategy leverages a staggered Difference-in-Differences (DiD) design with multiple approaches to account for treatment heterogeneity and dynamic effects. Specifically, I employ three complementary models: the standard Two-Way Fixed Effects (TWFE) model, the Extended Two-Way Fixed Effects (eTWFE) model following Wooldridge (2021), and the Callaway and Sant'Anna (2021) estimator (CSDID). Together, these models provide a robust framework for estimating effects while addressing potential biases arising from variations in treatment timing and cohort-specific dynamics.

3.2.1 Two-Way Fixed Effects (TWFE)

The baseline specification follows the conventional Two-Way Fixed Effects (TWFE) approach, which estimates the average treatment effect on the treated (ATT) by comparing treated and untreated groups before and after the opening of auto-manufacturing factories. Formally, the TWFE model is specified as follows:

$$Y_{ct} = \alpha + \beta \text{Treated}_{ct} + X'_{ct}\gamma + \delta_c + \eta_t + \epsilon_{ct} \tag{1}$$

Where Y_{ct} represents the outcome of interest (e.g., firm establishments, employment, wages) for county c and year t. Treated_{ct} is an indicator equal to 1 if county c experiences an auto-manufacturing factory opening in year t or later and 0 otherwise. $X'_{ct}\gamma$ is a vector of time-varying controls, including economic conditions and demographic characteristics. δ_c represents county fixed effects, controlling for time-invariant heterogeneity across counties. η_t represents year-fixed effects, capturing common shocks affecting all counties in a given year and ϵ_{ct} is the idiosyncratic error term. The coefficient of interest, β, captures the ATT of auto-manufacturing factory openings on the outcomes of interest. Standard errors are clustered at the CZ level, as that is the treatment assignment level.

3.2.2 Extended Two-Way Fixed Effects (eTWFE)

While the TWFE model provides a stable baseline for this analysis, the staggered nature of treatment time could possibly result in biased estimates across cohorts (Goodman-Bacon 2021; De Chaisemartin and d'Haultfoeuille 2020; Borusyak, Jaravel, and Spiess 2024). Moreover, the TWFE approach assumes a constant treatment effect, ignoring the possibility that impacts may differ depending on pre-existing conditions, industry composition, or regional characteristics. To account for potential biases in the TWFE model, I implement the Extended Two-Way Fixed Effects (eTWFE) approach following Wooldridge (2021). This model extends the TWFE framework by incorporating Mundlak (1978) terms, which control for time-invariant unobserved heterogeneity correlated with treatment timing. The eTWFE specification is given by:

$$Y_{ct} = \alpha + \sum_{g \in G} \sum_{t=g}^{T} \theta_{gt} \text{Treated}_{cgt} + X_{ct}' \gamma + \delta_c + \eta_t + \epsilon_{ct} \tag{2}$$

Where θ_{gt} coefficients represent the ATT that a group (cohort) g experiences at time t; this flexible specification of θ_{gt} avoids the shortcoming of TWFE, namely bad controls and negative weights (Wooldridge 2021) (Goodman-Bacon 2021). By including these pre-treatment averages, the eTWFE model accounts for baseline differences that might otherwise bias the estimated treatment effects.

The eTWFE approach improves upon the TWFE model by providing cohort-specific treatment effects and mitigating the negative weighting problem inherent in standard DiD designs with staggered treatment adoption. This refinement ensures that the estimated treatment effects more accurately reflect the impact of automanufacturing factory openings. By allowing for heterogeneity across cohorts and dynamic treatment effects, the eTWFE model provides more credible estimates, particularly when treatment effects evolve over time or vary across regions.

3.2.3 Callaway and Sant'Anna Difference-in-Differences (CSDID)

To further address treatment heterogeneity, I implement the Callaway and Sant'Anna (2021) estimator (CSDID). This approach estimates group-time-specific treatment effects by comparing treated units to appropriate comparison groups based on their treatment timing.

$$y_{ct} = \alpha + \sum_{g} \sum_{k} \phi_{gk} \cdot \mathbf{1}[t - g = k] + X'_{ct}\gamma + \delta_c + \eta_t + \epsilon_{ct} \tag{3}$$

where the summation $\sum_g \sum_k \phi_{gk} \cdot \mathbf{1}[t-g=k]$ captures event study effects, where g indexes the treatment cohort (the first period when a county is treated) and k denotes the event time relative to treatment (with $k = 0$ indicating the treatment year). The coefficient ϕ_{gk} represents the estimated treatment effect for cohort g at event time k, while the indicator function $\mathbf{1}[t - g = k]$ equals 1 if county c belongs to cohort g and is k periods away from treatment, and 0 otherwise.

Formally, the group-time average treatment effect (ATT) is given by:

$$\Delta_{g,t} = \mathbb{E}[Y_{ct}(1) - Y_{ct}(0) \mid G_c = g, T = t] \tag{4}$$

Where $\Delta_{g,t}$ represents the ATT for cohort g in period t. G_c indicates the cohort to which county c belongs based on its treatment year. And T represents the post-treatment period.

The CSDID estimator avoids the negative weighting issue of TWFE by comparing treated units only to untreated units in the same period, ensuring that comparisons are made between comparable groups. This model explicitly accounts for heterogeneous treatment effects by estimating separate treatment effects for each cohort and post-treatment period, thus addressing one of the primary limitations of the TWFE model. CSDID additionally allows for flexible dynamic treatment effects and provides insights into how treatment effects evolve over time.

In addition to estimating average treatment effects, I calculate both aggregate ATTs and dynamic treatment effects through event studies. The aggregate ATTs provide an overall measure of the impact of factory openings, while the event study approach allows for an examination of treatment effects over time, including potential pre-trends and post-treatment dynamics. These estimates are presented in both tabular and graphical form, facilitating a comprehensive understanding of the treatment effects.

4 Results and Analysis

4.1 Firm Effects

I begin by presenting descriptive evidence on the establishment patterns in treated and untreated counties over time. The top row of Figure 2 illustrates aggregate counts of establishments, small firms, and large firms, separately for treated and untreated counties. Prior to treatment, the trends in both groups track each other closely, potentially reinforcing the parallel trends assumption. Given the staggered nature of the treatment over 30 years, assessing parallel trends in raw data is complex; however, the pre-treatment behavior in both the counterfactual description and the event study estimates further supports this assumption.

The middle row of Figure 2 presents counterfactual analyses comparing firm counts relative to years before the factory opening (e.g., at t = (-5, -1)) for those treated counties. The dashed black line represents the expected trajectory of firm counts absent treatment, while the red line tracks true outcomes. The clear post-treatment divergence between these two lines provides compelling evidence that factory openings have a causal effect on local firm growth. These results provide basic evidence that there is some causal impact of the opening of a factory, especially in large firms where the direction of the raw data is opposite of the counterfactual prediction.

The bottom row of Figure 2 displays event study estimates for establishments, small firms, and large firms using multiple difference-in-differences estimators. Across all specifications, the estimated pre-treatment effects remain statistically indistinguishable from zero as coefficients oscillate or hover at zero with confidence intervals spanning across zero, reinforcing confidence in the identification strategy. While minor divergences in pre-trends appear for establishments and small firms at earlier event times (e.g., at t = (-5, -4)), these are not of great concern as the estimates at t = (-3, -2), where comparisons are more credible, are virtually zero.

Post-treatment effects for large firms exhibit a sustained and statistically significant upward trajectory, particularly in later post-treatment periods. This pattern suggests a more uniform and persistent treatment effect for this outcome. The effect is somewhat less pronounced for establishments and small firms, likely reflecting heterogeneity in treatment effects across firm sizes. There are two possible explanations for the initial decrease and then later increase in small firms. The first is firm growth dynamics; some small firms may expand beyond their initial classification or merge with other firms, reducing the count of small firms and establishments. Effectively, small firms themselves grow out of the classification rather than firm death as a result of the factory relocation. A second explanation for the initial decrease in small firms is firm cannibalization. As a result of the factory relocation, firms lose employees as they choose to now work for the factory or other large firms resulting in firm deaths and employee absorption into the bigger firms. Over time, as the cannibalistic behavior of the larger firm stabilizes, their small firms begin to grow in count again, resulting in an increase in later periods (e.g. t = 7, 8, 9, 10). Direct verification of these two explanations is challenging given data limitations; the observed patterns align with expected firm growth behavior following a major industrial expansion though, giving basis for the conjecture.

Table 4 quantifies these effects, reporting post-treatment aggregate ATTs across different estimation methods. These results provide strong evidence that large firm growth in response to treatment remains consistently positive, in line with event studies. More conservative estimators, such as CSDID, report slightly less significant estimates which may be influenced by unobservable factors influencing firm location decisions, such as local tax incentives. However, the estimated effect for eTWFE remains strongly significant at 5 firms ($p < 0.01$) (column 4). Given the pre-treatment mean of 103.36 large firms in treated counties, this represents an economically meaningful increase of approximately 5% over the post-treatment period. This robust finding strongly suggests that large firms are particularly responsive to the establishment of new factories in their commuting zones, consistent with theories of agglomeration economies, firm clustering, and these factories acting as anchor firms.

The economic implications of these findings align with existing research on local agglomeration and firm behavior in response to industrial expansion. The increase in large firms post-treatment suggests that new manufacturing facilities act as anchor institutions, attracting complementary businesses and suppliers that benefit from geographic proximity. This is consistent with prior literature indicating that large firms often cluster to leverage economies of scale, shared labor pools, and input-output linkages.

Furthermore, the differential response across firm sizes highlights important heterogeneities in firm dynamics. While the ATT on small firms remains less conclusive due to two competing explanations, the strong and precise positive impact on large firms suggests that manufacturing expansions disproportionately benefit firms with greater capital and infrastructure needs. This could be driven by increased demand for specialized suppliers, improved access to skilled labor, or indirect spillover effects that enhance the local business ecosystem. While the overall establishment trends support a causal interpretation, the presence of potential heterogeneous treatment effects suggests that additional factors - such as industry composition, regional economic conditions, and policy environment - may influence firm responses. Future research could explore these mechanisms in greater depth using firm-level data or industry-specific case studies.

4.2 Labor Market Effects

Similar to the analysis for firms, I begin by presenting descriptive evidence on employment patterns, labor force potential, and wages in treated and untreated counties over time. The top row of Figure 3 illustrates employment rates, labor force potential, and average weekly wages, separately for treated and untreated counties. As with firm outcomes, visualizing parallel trends is challenging due to the staggered nature of the treatment. Thus, I turn to the middle and bottom rows of Figure 3 to support the parallel trends assumption.

The middle row of Figure 3 presents counterfactual analyses for treated counties' employment rates, labor force potential, and wages before the factory opened. The dashed black line represents the expected trajectory absent treatment, while the blue line tracks actual outcomes. A clear divergence between these two lines in post-treatment periods provides compelling evidence that factory openings serve as labor market anchors, leading to increased employment and higher wages. This observation is not noted in potential labor force effects but may be muted due to large negative outcomes in singular counties.

The bottom row of Figure 3 displays event study estimates across multiple difference-in-differences models. For the potential labor force, the estimated pre-treatment effects remain statistically indistinguishable from zero, reinforcing confidence in the identification strategy. It appears that there may be potential pre-trends for both employment rate and log weekly wages. As these estimates become closer and confidence intervals span across zero in closer time periods, I do not believe there is an immediate pre-trends violation. However, I show pre-trends testing for all outcomes for both the TWFE and CSDID models using banded confidence intervals in Figure A.7 (Rambachan and Roth 2023). The employment rate event study shows a strong and sustained post-treatment increase, indicating significant job growth. Labor force potential also rises over time, suggesting that factory openings attract new residents or retain local populations who otherwise might have migrated. Wage effects materialize more gradually but become significant in later post-treatment years, consistent with a tightening labor market where firms must offer higher wages to retain workers.

Table 5 quantifies these effects. The eTWFE model (column 4) estimates an increase in employment rates of 0.020 ($p < 0.01$), a meaningful rise given the pre-treatment mean of 0.52. The effect on labor force potential is smaller but significant, with an ATT of 0.003-0.004 across models, implying that factory openings encourage population retention and migration into the region. Wage effects are also positive and significant, with the eTWFE model estimating an increase of 0.035 log points ($p < 0.01$), equivalent to approximately a 3.5% rise in average weekly wages.

These results suggest that factory openings function not only as firm anchors but also as labor market anchors —expanding employment opportunities, attracting and retaining a local workforce, and increasing worker earnings. The increase in labor force potential indicates that these openings may contribute to demographic shifts, either by encouraging population retention or attracting new residents seeking employment opportunities. The strong wage effects suggest heightened labor competition, as firms must offer higher pay to prevent job-hopping within the local market. The gradual nature of wage growth implies that firms adjust compensation structures over time as labor demand increases. Together, these findings underscore the transformative impact of manufacturing expansions on local labor markets, reinforcing theories of agglomeration and regional economic development while highlighting the interconnected nature of firm and labor dynamics in response to industrial investment.

4.3 Robustness

To ensure the validity of my empirical findings, I conduct a series of robustness tests addressing potential threats to identification. These tests assess the sensitivity of my estimates to alternative specifications, treatment effect heterogeneity, and potential violations of the parallel trends assumption. Below, I outline the key robustness exercises and their implications.

4.3.1 Alternative Samples

To assess whether my findings are driven by a particular region, time period, or firm, I conduct three exclusion-based robustness tests: (1) dropping one state at a time, (2) dropping a single treatment cohort at a time, and (3) re-estimating the effects while retaining only one factory at a time. The results from these exercises are visualized in Figures 4 and 5.

For firm outcomes, the results remain largely robust across most exclusions, see Figure 4. The exclusion of specific states does not significantly alter the findings. The removal of Texas and Florida, considered to be two huge economic driving states of the South, flips the estimates positive for total establishments and small firms but remains statistically insignificant. The key outcome variable I am testing for this exclusion is large firms and this remains statistically significant for each exclusion of a state test. Similarly, dropping specific cohorts shows general consistency, with the exception of 2003 - the Texas Toyota factory. Lastly, when estimating the impact of individual factories, heterogeneity in firm responses is evident, with certain factories (such as Toyota Texas, Mercedes-Benz Alabama, and BMW South Carolina) showing outsized influence on the aggregate effects. These findings highlight the importance of contextualizing firm responses within their unique regional economic landscapes. Future research could refine this analysis by implementing a case-by-case approach, comparing similar factories with tailored control groups that account for local economic policies, workforce characteristics, and supply chain dynamics. This approach could provide deeper insights into why certain factories generate larger spillover effects and whether firm-specific factors, such as industry specialization or production scale, drive variations in outcomes.

For labor outcomes, the robustness checks similarly indicate that the findings are not driven by any single state or cohort, see Figure 5. The employment rate and wage effects remain stable across most exclusions, with some minor deviation from excluding Tennessee, Kentucky, and Toyota Kentucky (1986). Similar to firms, when I look at a firm-specific effect, there is variation in treatment effects and magnitude. In particular, it appears that factories that had an opening date in the 1980s and 1990s have more positive outcomes compared to those open in the 2000s and 2010s. This divergence could be driven by firm characteristics, including differences in industry composition, the scale of production, or technological advancements that influence employment and wage structures. Earlier factories may have benefitted from more favorable economic conditions, stronger regional incentives, or less labor market competition, leading to stronger spillover effects. Additionally, changing labor dynamics, such as differences in workforce mobility, automation, or the rise of service-sector employment, could contribute to weaker effects in more recent periods. Future research could explore these mechanisms by incorporating firm-level data, analyzing industry-specific trends, and constructing tailored control groups that account for policy shifts and labor force composition over time. A deeper examination of firm heterogeneity - such as ownership structures, workforce characteristics, and supply chain integration - could help disentangle these complex relationships and provide a more nuanced understanding of how manufacturing expansions shape local economies across different time periods. Nevertheless, these robustness tests remain consistent across states and cohorts, supporting the interpretation that manufacturing openings encourage employment, attract new residents or retain existing populations, and result in wage increases.

Together, these robustness exercises provide strong evidence that the estimated effects on firms and labor markets are not driven by a single state or cohort but rather represent broader regional trends associated with manufacturing expansion while highlighting potential treatment effect heterogeneity.

4.3.2 Shifting Adoption Times

To assess the validity of the parallel trends assumption and examine the implications of shifting adoption timing, I analyze pre-treatment effects and variations in treatment onset. Specifically, I accelerate the adoption timing in increments of 1, 3, and 5 years to test for potential anticipation effects. Table 6 reports the results. I do not execute placebo treatment times in the post-period (e.g. $g_{new} = g + 1, 3, 5$) as these placebos would capture endogenous periods.

The pre-treatment time shifts (-5, -3, -1) reveal no statistically significant effects for establishments and small firms, reinforcing confidence in the parallel trends assumption. This finding suggests that alternative treatment times do not yield significant outcomes. However, large firms and employment rates exhibit some prior to treatment effects, particularly in the -3 and -1 shifts. This suggests that businesses may expand and hire in anticipation of a factory's arrival, positioning themselves within new supply chains or securing a competitive hiring advantage. This early response likely reflects firms adjusting their operational strategies in response to public announcements, infrastructure developments, or early-stage investment commitments tied to the factory's opening. The presence of significant pre-treatment effects for large firms but not for small firms further supports this narrative, as larger firms are often better positioned to react proactively to long-term industrial developments.

Additionally, wages and labor force potential see adjustments three years prior to the official opening, indicating a maturation of the announcement effect. This pattern potentially suggests that as the factory opening nears, labor market pressures begin to materialize, prompting firms to raise wages and increasing the share of working-age individuals in the local population. This response could reflect the combined influence of higher labor demand, workforce migration into the area, or retention of local workers who may have otherwise sought employment elsewhere.

These findings highlight the importance of considering announcement effects when estimating treatment impacts. Firms may proactively adjust operations, hire workers, or raise wages ahead of the official opening, meaning that standard event study specifications could understate the immediate post-treatment effects. Moreover, these results suggest that the impact of a possible policy intervention may begin prior to the official opening, warranting further investigation into the role of preemptive firm behavior and labor market adjustments. Future research could refine these estimates by incorporating alternative treatment definitions that account for the announcement period or construction phase.

4.3.3 Considering Construction Effects

As observed with the shifting of treatment time, there appears to be a potential trend and effect prior to the formal opening of the factory. This makes sense when we think about the announcement and construction of the factory as opposed to the true opening of the factory. Firms in a treated county may choose to change operational behavior (employee more, pay more, consolidate, etc.) as a result of a credible commitment of the factory arrival, i.e. there is an effect when factory construction begins. This means that prior to the true treatment, there may be changes in the measured outcomes. I consider two ways of quantifying these effects: (1) a [3 x 3] TWFE specification capturing the construction and opening effects separately and (2) the original specifications but using the ground-breaking year as the treatment start and the window being until the opening of the factory.

In order to test the [3 x 3] TWFE specification, consider a model where I have information not only on the opening year of the factory but the year that construction began. We then construct a [3 x 3] specification where the first treatment (Construction, (β_1)) is captured separately from the secondary treatment (Opening, (β_2)) where:

$$Y_{ct} = \alpha + \beta_1 \text{Construction}_{ct} + \beta_2 \text{Opening}_{ct} + X'_{ct}\gamma + \delta_c + \eta_t + \epsilon_{ct} \quad (5)$$

and all coefficients are the same as the original TWFE specification. Table 6 displays these results. Across all controlled estimates, I see an intensification in the ATT for each outcome (comparing β_1 versus β_2). This potentially implies that observed effects are more driven by the opening of the factory than its construction. This is especially true for labor outcomes and large firms, similar to the original specifications. Multiple treatments on the same units are a challenge in the staggered setting; as a result, this method is an attempt to show a proof of concept for a [3 x 3] specification and supplement the original results.

An alternative to the specification above is to simple measure the construction effect and juxtapose it with the opening effect in the original specification. To capture this construction effect, I restrict the panels such that the window of analysis is $(-5, t \leq Opening)$. This creates a new panel where any post-treatment observations are before the official opening of the factory, and the captured effect is that of construction. The average time between construction and opening is 2.5 years; this gap can be as low as zero years and as high as six years, depending on the beginning state of construction (e.g. starting as a greenfield or simply retooling existing infrastructure). Including any true treatment periods, meaning those that have the factory open, would be endogenous as they may be capturing the opening effect. Thus, the appropriate window for this analysis is $(-5, t \leq Opening)$

Table 8 shows the results for this temporal selection specification for all outcomes on all models. When simply looking at the construction effect, there appears to be potential significant changes in the labor outcomes aside from wages in CSDID and eTWFE; nevertheless, these estimates are smaller than that of the opening effect, potentially alluding to a dynamic effect that is initiated by construction but has greater impact after the factory opens. Additionally, eTWFE predicts similar significance and results, showing that these results are robust to alternative methods.

Across multiple robustness tests (including more computationally intensive ones, such as randomized inference in Figure A.8), my findings remain consistent, reinforcing the validity of the estimated treatment effects while highlighting important nuances. The alternative sample analysis highlights consistent treatment effects across exclusions, with notable variation when removing key economic hubs such as Texas and Florida or earlier factory openings from the 1980s and 1990s. This suggests that economic conditions, industry structure, and labor dynamics shape treatment heterogeneity. Similarly, the shifting adoption times analysis potentially reveals anticipation effects among large firms and employment rates, indicating that businesses may expand and hire in response to announcement signals and early-stage investment commitments. Wage and potential labor force adjustments materialize three years before the factory opening, possibly highlighting a maturation of labor market pressures as firms compete for workers and local populations respond to employment opportunities. Further, while there is potential for the construction of these factories to encourage growth, I show these results to be limited and not as strong in impact as the true opening of the factory. Overall, these robustness checks emphasize the importance of considering preemptive firm behavior, labor market expectations, and announcement effects when estimating treatment impacts.

5 Conclusion

This paper provides new evidence on the economic spillovers of large-scale auto-manufacturing factory openings in the American South. Using a panel of county-level firm and labor data from 1976 to 2016 and a robust staggered DiD framework, I show that these openings serve as critical anchor points for local economic growth and development. The results indicate a sustained and statistically significant increase in the number of large firms following factory openings, consistent with theories of agglomeration and firm clustering. These effects are economically meaningful and robust across multiple specifications, suggesting that large-scale manufacturing generates measurable firm development within regional labor markets.

Labor market outcomes reveal complementary dynamics. Employment rates increase significantly and persistently after factory openings, while wages rise gradually but meaningfully, especially in the later post-treatment years. The increase in labor force potential, though more modest, supports the view that manufacturing investment can attract new residents or retain existing populations. These findings position auto-manufacturing as both a firm and labor market anchor, shaping the broader economic trajectory of treated regions.

Importantly, effects are not uniform. Small firm dynamics display initial declines followed by later recovery, potentially driven by cannibalization, firm growth, or shifting market structures. The anticipation effects observed in pre-treatment years for employment and wages further highlight the forward-looking behavior of firms and workers in potential response to announced factory openings. These patterns reinforce the need to consider the timing of treatment effects and heterogeneous responses when evaluating the impact of industrial investment.

This study also contributes to the literature by quantifying aggregate effects across multiple manufacturing openings rather than focusing on case studies. By using commuting zones (CZs) as the unit of treatment, I capture spillovers across county lines and within labor markets that more accurately reflect economic geography. Robustness checks, including leave-one-state-out analysis and alternative adoption timings, confirm that the results are not driven by outliers or regional idiosyncrasies. Nevertheless, heterogeneity across factories and time periods suggests that local context matters: earlier factories (1980s-1990s) appear to generate stronger effects than more recent openings, possibly reflecting changing labor market conditions, policy environments, or technological shifts.

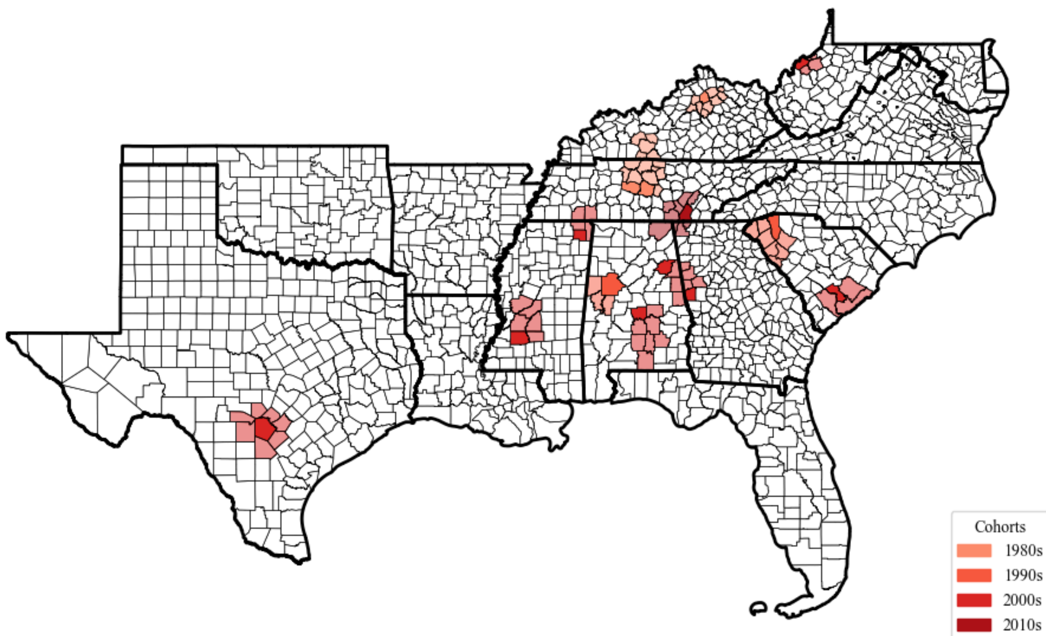
While the identification strategy is supported by multiple tests, including covariate balance and robustness checks, the assumption of quasi-random treatment remains a limitation, as some unobservable factors may still influence factory siting. Moreover, the data limitations prevent direct observation of some mechanisms, such as firm size transitions or inter-firm supply chain linkages. Future research could benefit from firm-level longitudinal data and industry-specific studies that explore these channels more precisely.

Despite these limitations, this paper underscores the enduring relevance of manufacturing in regional development, even within an increasingly service-based economy. Auto-manufacturing continues to shape firm behavior, employment outcomes, and local labor market structures across the South. As states and regions pursue economic growth and development strategies, understanding the full scope of these spillovers is vital. Future work should further investigate heterogeneity in effects, the persistence of long-term gains, and the broader implications for industrial policy and workforce development.

In sum, large-scale manufacturing factory openings remain powerful drivers of economic change. Their ability to catalyze firm growth, enhance employment opportunities, and raise wages positions them as central actors in local economic ecosystems. This study offers new evidence that industrial investment, when properly contextualized and measured at the appropriate geographic scale, continues to play a critical role in shaping regional economic trajectories in the modern era.

6 Figures and Tables

Figure 1: Factory Openings and Commuting Zone Exposure in the American South by Cohort Decade



Note: This figure maps factory openings in the American South by decade, with darker shades indicating the counties where a factory was established and the color denoting the decade of opening. Lighter shades represent counties within the same commuting zone as a county that received a factory, indicating indirect exposure to the treatment. Cohorts are categorized into four periods: the 1980s, 1990s, 2000s, and 2010s. Treatment effects are defined at the commuting zone level, where counties without direct factory openings may still be considered treated if they are within a commuting zone containing a new factory. County and state boundaries are outlined for reference.

Table 1: Automobile Manufacturing Plants in the South Opening between 1976 and 2016

Factory Name	**Company**	**County**	**State**	**Year Opened**
Bowling Green Assembly	GMC	Warren	Kentucky	1981
Nissan North America, Inc. Smyrna	Nissan	Rutherford	Tennessee	1983
Toyota Motor Manufacturing Kentucky	Toyota	Scott	Kentucky	1986
Spring Hill Manufacturing	GMC	Williamson	Tennessee	1990
BMW US Manufacturing Company, LLC	BMW	Greenville	South Carolina	1994
Mercedes-Benz U.S. International, Inc.	Mercedes-Benz	Tuscaloosa	Alabama	1997
Honda Manufacturing of Alabama	Honda	Calhoun	Alabama	2001
Toyota Manufacturing Texas	Toyota	Bexar	Texas	2003
Nissan North America, Inc. Canton	Nissan	Madison	Mississippi	2003
Hyundai Motor Manufacturing Alabama	Hyundai	Montgomery	Alabama	2005
Kia Motors Manufacturing Georgia	Kia	Troup	Georgia	2006
Mercedes-Benz Vans, LLC.	Mercedes-Benz	Dorchester	South Carolina	2006
Toyota Manufacturing Mississippi	Toyota	Prentiss	Mississippi	2007
Hino Motors Manufacturing U.S.A. Inc.	Hino Motors	Wood	West Virginia	2007
Volkswagen Chattanooga Assembly Plant	Volkswagen	Hamilton	Tennessee	2011

Notes: Spring Hill Manufacturing (GMC in 1990) was opened in the same commuter zone as Nissan North America (Nissan in 1983). Due to this, treatment was only applied in 1983 in order for treatment dosage to be uniform and SUTVA to hold across analysis. Because of this, only fourteen of the fifteen plants identified were used for analysis.

Table 2: Summary Statistics by Treatment Group

	Obs	Mean	Std. Dev.	Min	Max
Panel A: Outcomes					
Establishments					
Untreated	50,699	1472.74	4373.55	1	99,121
Treated (g-1)	78	1831.08	4320.46	42	30,690
Small Firms					
Untreated	50,699	1397.92	4107.57	1	91,621
Treated (g-1)	78	1721.63	4026.75	42	28,710
Large Firms					
Untreated	50,699	73.67	268.27	0	7,406
Treated (g-1)	78	107.91	293.40	0	1,963
Employment Rate					
Untreated	50,699	0.396	0.167	0	1.695
Treated (g-1)	78	0.426	0.184	0	0.963
Potential Labor Force					
Untreated	50,699	0.579	0.038	0.376	0.750
Treated (g-1)	78	0.594	0.028	0.499	0.656
log(Weekly Wage)					
Untreated	50,637	6.447	0.219	5.616	7.783
Treated (g-1)	77	6.470	0.186	5.951	6.921
Panel B: Covariates					
Fraction Nonwhite Population					
Untreated	50,699	0.184	0.176	0	0.869
Treated (g-1)	78	0.207	0.196	0.003	0.806
Fraction Pop. HS Education					
Untreated	50,699	32.65	5.824	7.8	66.6
Treated (g-1)	78	33.29	5.342	18	48.7
Population Density					
Untreated	50,699	124.38	370.38	0.082	9,019.13
Treated (g-1)	78	132.53	218.04	1.86	1,243.66

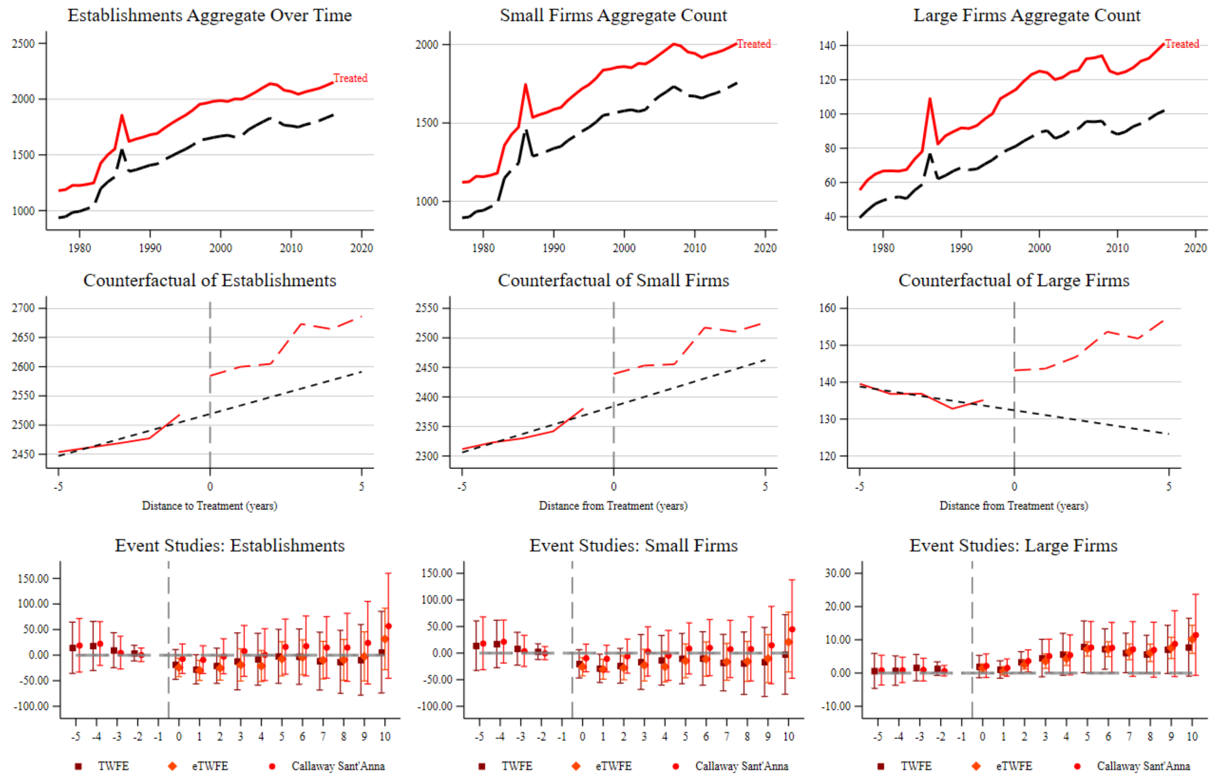
Notes: This table reports summary statistics for key outcomes and covariates by treatment status. *Treated (g-1)* includes counties treated in period g and evaluated one year prior to treatment. All variables are measured at the county-year level. This descriptive comparison provides baseline context for treatment and control groups prior to the event study estimation. Mean values, standard deviations, and min/max ranges are presented for each subgroup.

Table 3: Descriptive Statistics and Balance Test (Pei, Pischke, and Schwandt 2019)

	(1) Mean % Difference	(2) p-value
A. Adapted Balancing Test		
Fraction of Nonwhite Population	-3.47	0.06*
Interpolation of Education on Year	3.25	0.08*
Population Density	4.04	0.20
B. F-Test for Joint Significance of Controls		
p-value		0.566

Notes: * p<0.10, ** p<0.05, *** p<0.01, **** p<0.001. This analysis follows (Pei, Pischke, and Schwandt 2019), where a percentage difference of 10 would indicate that the opening of a factory is associated with a change in outcome of 10% in treated versus control counties. Coefficients corrected for bias in the following way: $100 \times (\exp(\beta) - 1)$. Standard errors are estimated using the delta method and are clustered at the commuter zone level.

Figure 2: Comparing Pre-Trends, Counterfactuals, and Event Studies for Firm Outcomes



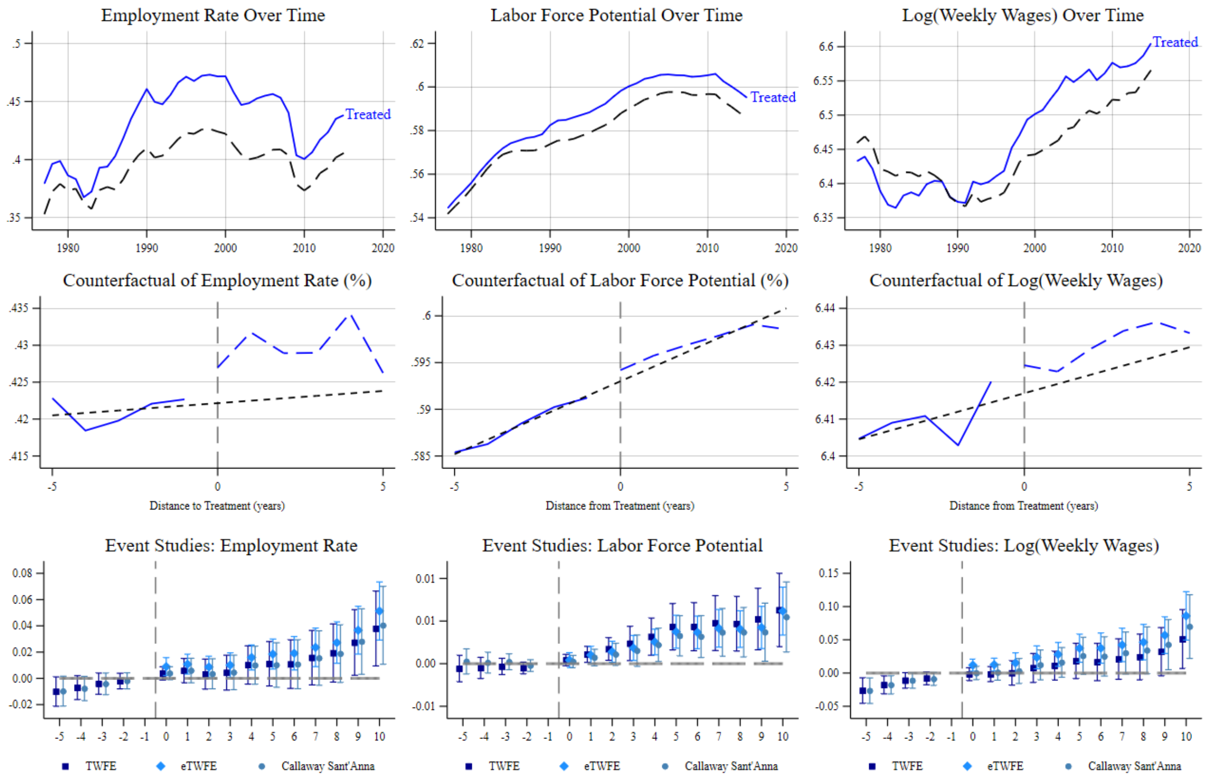
Note: Going from left to right, the order is total establishments, small firms (those of employee sizes less than 50), and large firms (those of employee sizes equal to or greater than 50). The rows from top to bottom compare the aggregate number of firms for untreated and treated (red), building a counterfactual of not-yet-treated versus treated not including covariates, and then displays event studies for the outcome variable with covariates. ETWFE is an adapted two-way fixed effect model to combat treatment heterogeneity(Wooldridge 2021). CSDID attempts to separately address treatment heterogeneity and staggered adoption (Callaway and Sant'Anna 2021). Models contain covariates for fraction of nonwhite population, fraction of population holding only a high school degree, and population density. All results are clustered at the commuter zone level.

Table 4: Firm-Level Effects at the County-Level Resulting From a Factory Opening in a Commuter Zone

	TWFE		ETWFE		CSDID	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Establishments						
ATT on Establishments	-17.56	-15.51	-18.27	-12.24	-9.18	12.54
	(53.50)	(33.89)	(49.07)	(15.27)	(42.84)	(25.88)
Mean Untreated	1472.73	1472.73	1472.73	1472.73	1472.73	1472.73
Mean Treated (g-1)	1783.78	1783.78	1783.78	1783.78	1783.78	1783.78
Observations	52,500	52,500	52,500	52,500	52,499	52,499
B. Small Firms						
ATT on Small Firms	-22.01	-20.03	-22.93	-17.38	-14.52	6.21
	(48.99)	(31.39)	(44.52)	(14.47)	(38.70)	(23.44)
Mean Untreated	1397.92	1397.92	1397.92	1397.92	1397.92	1397.92
Mean Treated (g-1)	1678.82	1678.82	1678.82	1678.82	1678.82	1678.82
Observations	52,500	52,500	52,500	52,500	52,499	52,499
C. Large Firms						
ATT on Large Firms	4.34	4.40	4.55	5.00***	5.19	6.20**
	(4.78)	(3.17)	(4.73)	(0.96)	(4.31)	(3.14)
Mean Untreated	73.67	73.67	73.67	73.67	73.67	73.67
Mean Treated (g-1)	103.36	103.36	103.36	103.36	103.36	103.36
Observations	52,500	52,500	52,500	52,500	52,499	52,499
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes

Note: Asterisks indicate statistical significance: * p<0.10, ** p<0.05, *** p<0.01, **** p<0.001. Coefficients are representative of the Aggregate ATT post-treatment (10 years time). To see the effect of treatment on a year-by-year basis, refer to visual event studies in Figures 2. The leftmost column is for the uncontrolled models, and the rightmost incorporates covariates. Covariates include the fraction of the non-white population, the fraction of the population with at least a high school diploma, and population density for the county. ETWFE is an adapted two-way fixed effect model to combat treatment heterogeneity(Wooldridge 2021). CSDID attempts to deal with treatment heterogeneity and staggered adoption in a different calculation to eTWFE (Callaway and Sant'Anna 2021). All results are clustered at the county level.

Figure 3: Comparing Pre-Trends, Counterfactuals, and Event Studies for Labor Outcomes



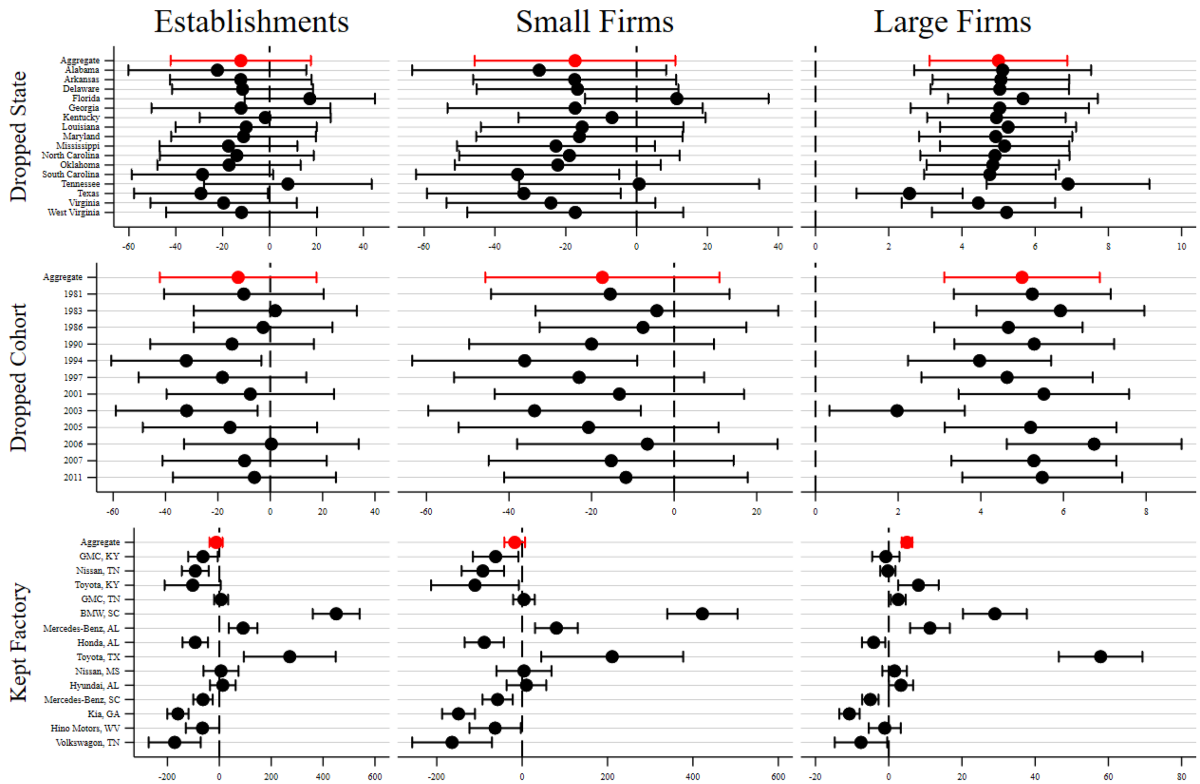
Note: Going from left to right, the order is employment rate, labor force potential, and log(annual average weekly wage) (in real 2016 U.S. Dollars) for each county. The rows from top to bottom compare the aggregate level and mean rates for untreated and treated (blue), building a counterfactual of not-yet-treated versus treated not including covariates, and then displays event studies for the outcome variable with covariates. ETWFE is an adapted two-way fixed effect model to combat treatment heterogeneity(Wooldridge 2021). CSDID (Callaway and Sant'Anna 2021) attempts to separately adress treatment heterogeneity and staggered adoption in separate ways. Models contain covariates for fraction of nonwhite population, fraction of population holding only a high school degree, and population density. All results are clustered at the commuter zone level.

Table 5: Labor-Level Effects at the County-Level Resulting From a Factory Opening in a Commuter Zone

	TWFE		ETWFE		CSDID	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Employment Rate						
ATT on Employment Rate	0.016	0.017	0.017**	0.020***	0.013	0.014*
	(0.013)	(0.012)	(0.007)	(0.006)	(0.008)	(0.008)
Mean Untreated	0.52	0.52	0.52	0.52	0.52	0.52
Mean Treated (g-1)	0.56	0.56	0.56	0.56	0.56	0.56
Observations	51,500	51,500	51,500	51,500	51,499	51,499
B. Potential Labor Force						
ATT on Potential Labor Force	0.004**	0.004***	0.004**	0.003***	0.004***	0.003*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mean Untreated	0.58	0.58	0.58	0.58	0.58	0.58
Mean Treated (g-1)	0.59	0.59	0.59	0.59	0.59	0.59
Observations	51,500	51,500	51,500	51,500	51,499	51,499
C. Log(Weekly Wages)						
ATT on Log(Weekly Wages)	0.028*	0.027*	0.029**	0.035***	0.017	0.024*
	(0.015)	(0.015)	(0.012)	(0.010)	(0.012)	(0.012)
Mean Untreated	6.45	6.45	6.45	6.45	6.45	6.45
Mean Treated (g-1)	6.46	6.46	6.46	6.46	6.46	6.46
Observations	51,500	51,500	51,500	51,500	51,499	51,499
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes

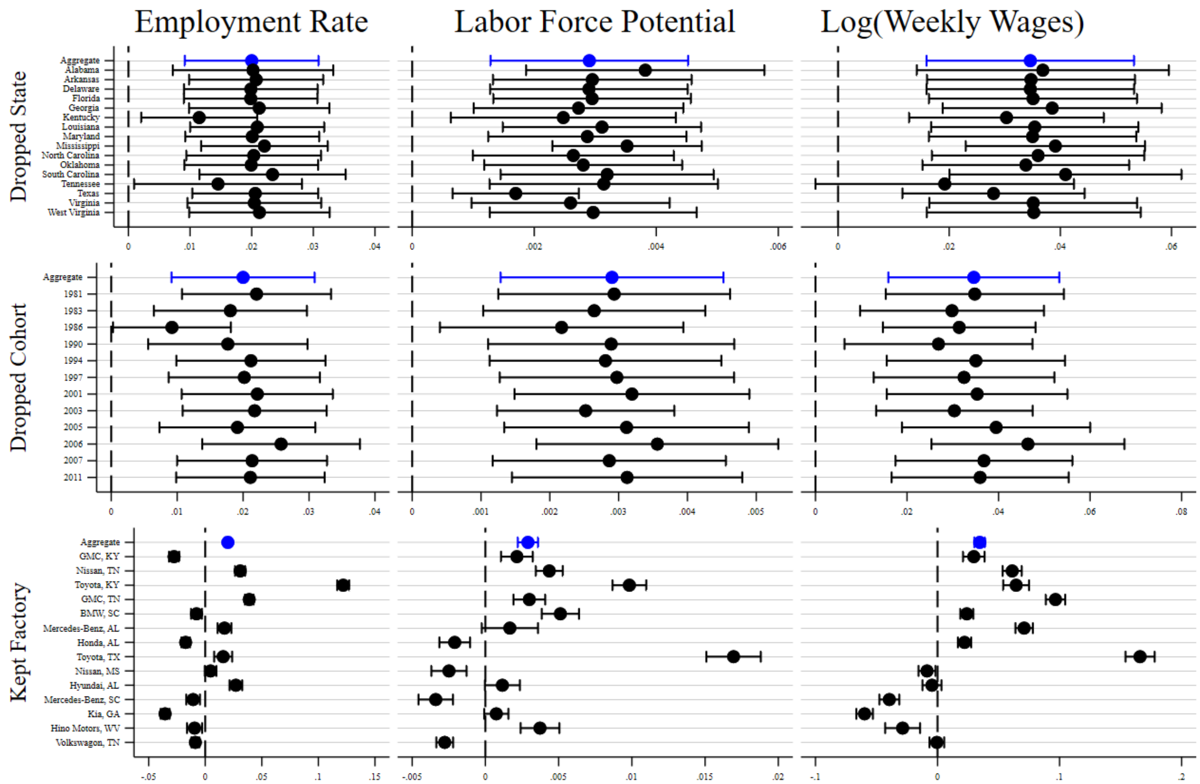
Note: Asterisks indicate statistical significance: * p<0.10, ** p<0.05, *** p<0.01, **** p<0.001. Coefficients are representative of the Aggregate ATT post-treatment (10 years time). To see the effect of treatment on a year-by-year basis, refer to visual event studies in Figures 3. The leftmost column is for the uncontrolled models, and the rightmost incorporates covariates. Covariates include the fraction of the non-white population, the fraction of the population with at least a high school diploma, and population density for the county. ETWFE is an adapted two-way fixed effect model to combat treatment heterogeneity (Wooldridge 2021). CSDID attempts to separately address treatment heterogeneity and staggered adoption (Callaway and Sant'Anna 2021). All results are clustered at the county level.

Figure 4: Robustness Checks on Firm Variables Using eTWFE (Wooldridge 2021)



Note: Going from top to bottom, the rows present different robustness tests applied to the main results. The top row sequentially drops a single state from the sample and re-estimates the regression to assess whether results are driven by any particular state. The middle row drops an entire treatment cohort year to ensure that findings are not dependent on the inclusion of specific adoption years. The bottom row isolates the impact of individual factories by selecting one treated factory at a time and comparing its effect against the control group. Estimates are generated using eTWFE, an adapted two-way fixed effects model designed to address treatment heterogeneity (Wooldridge 2021). All models include covariates for the fraction of the nonwhite population, the fraction of the population with only a high school diploma, and population density. All results are clustered at the commuter zone level.

Figure 5: Robustness Checks on Labor Variables Using eTWFE (Wooldridge 2021)



Note: Going from top to bottom, the rows present different robustness tests applied to the main results. The top row sequentially drops a single state from the sample and re-estimates the regression to assess whether results are driven by any particular state. The middle row drops an entire treatment cohort year to ensure that findings are not dependent on the inclusion of specific adoption years. The bottom row isolates the impact of individual factories by selecting one treated factory at a time and comparing its effect against the control group. Estimates are generated using eTWFE, an adapted two-way fixed effects model designed to address treatment heterogeneity (Wooldridge 2021). All models include covariates for the fraction of the nonwhite population, the fraction of the population with only a high school diploma, and population density. All results are clustered at the commuter zone level.

Table 6: Shifting Adoption Times and Measuring Impact on Outcome Variables

	Time Shift		
	-5	-3	-1
A. Establishments			
ATT on Establishments	-20.19	-20.28	-12.55
	(18.48)	(15.30)	(14.85)
Mean Untreated	1450.32	1450.34	1450.34
Mean Treated (g-1)	1725.90	1730.21	1731.11
Observations	52140	52140	52143
B. Small Firms			
ATT on Small Firms	-30.15	-32.93*	-15.88
	(20.78)	(21.17)	(19.43)
Mean Untreated	1350.10	1350.10	1350.10
Mean Treated (g-1)	1590.90	1605.30	1606.11
Observations	52140	52140	52143
C. Large Firms			
ATT on Large Firms	25.31**	28.63***	39.70***
	(15.53)	(14.89)	(12.84)
Mean Untreated	80.27	80.27	80.27
Mean Treated (g-1)	110.19	113.52	113.55
Observations	52140	52140	52143
D. Employment Rate			
ATT on Employment Rate	3.22**	2.72***	6.15***
	(5.23)	(4.55)	(3.82)
Mean Untreated	0.51	0.51	0.51
Mean Treated (g-1)	0.58	0.57	0.57
Observations	50816	50816	50816
E. Labor Force Potential			
ATT on Labor Force Potential	1.80	2.05***	2.99***
	(0.89)	(0.81)	(0.75)
Mean Untreated	0.62	0.62	0.62
Mean Treated (g-1)	0.61	0.61	0.63
Observations	50816	50816	50816
F. log(Weekly Wages)			
ATT on log(Weekly Wages)	0.038	0.070***	0.099***
	(0.029)	(0.028)	(0.027)
Mean Untreated	6.38	6.38	6.38
Mean Treated (g-1)	6.21	6.22	6.29
Observations	50816	50816	50816

Note: Asterisks indicate statistical significance: * p<0.10, ** p<0.05, *** p<0.01, **** p<0.001. Coefficients are representative of the Aggregate ATT post-treatment. Adoption times are shifted in each column to test the robustness of treatment. The leftmost column is advancing the treatment by five years, and the rightmost delays treatment by five years. Covariates include the fraction of the non-white population, the fraction of the population with at least a high school diploma, and population density for the county. ETWFE is the model used and an adapted two-way fixed effect model to combat treatment heterogeneity(Wooldridge 2021). All results are clustered at the commuter zone level.

Table 7: TWFE Estimates of Construction and Opening Effects

	Uncontrolled		Controlled	
	Construction (β_1)	Opening (β_2)	Construction (β_1)	Opening (β_2)
A. Establishments				
ATT on Establishments	-21.423	-19.600	-15.669	-18.068
	(25.078)	(68.363)	(19.539)	(43.578)
B. Small Firms				
ATT on Small Firms	-20.495	-24.495	-15.220	-22.966
	(23.181)	(62.673)	(18.114)	(40.240)
C. Large Firms				
ATT on Large Firms	-0.947	4.778	-0.476	4.784
	(2.533)	(6.073)	(2.212)	(4.141)
D. Employment Rate				
ATT on Employment Rate	0.002	0.022	0.002	0.023
	(0.008)	(0.015)	(0.008)	(0.014)
E. Labor Force Potential				
ATT on Labor Force Potential	0.001	0.005***	0.001	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
F. Log(Weekly Wage)				
ATT on Log(Weekly Wage)	0.007	0.032	0.007	0.032
	(0.009)	(0.021)	(0.009)	(0.020)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Weights	No	No	No	No

Notes: Asterisks indicate statistical significance: * p<0.10, ** p<0.05, *** p<0.01, **** p<0.001. Coefficients (β_1 and β_2) are representative of the respective effect of the periods they represent. The leftmost column pair is for the uncontrolled models, and the rightmost incorporates covariates. Covariates include the fraction of the non-white population, the fraction of the population with at least a high school diploma, and population density for the county. The base model is a TWFE specification extended to a [3x3] application. All results are clustered at the county level.

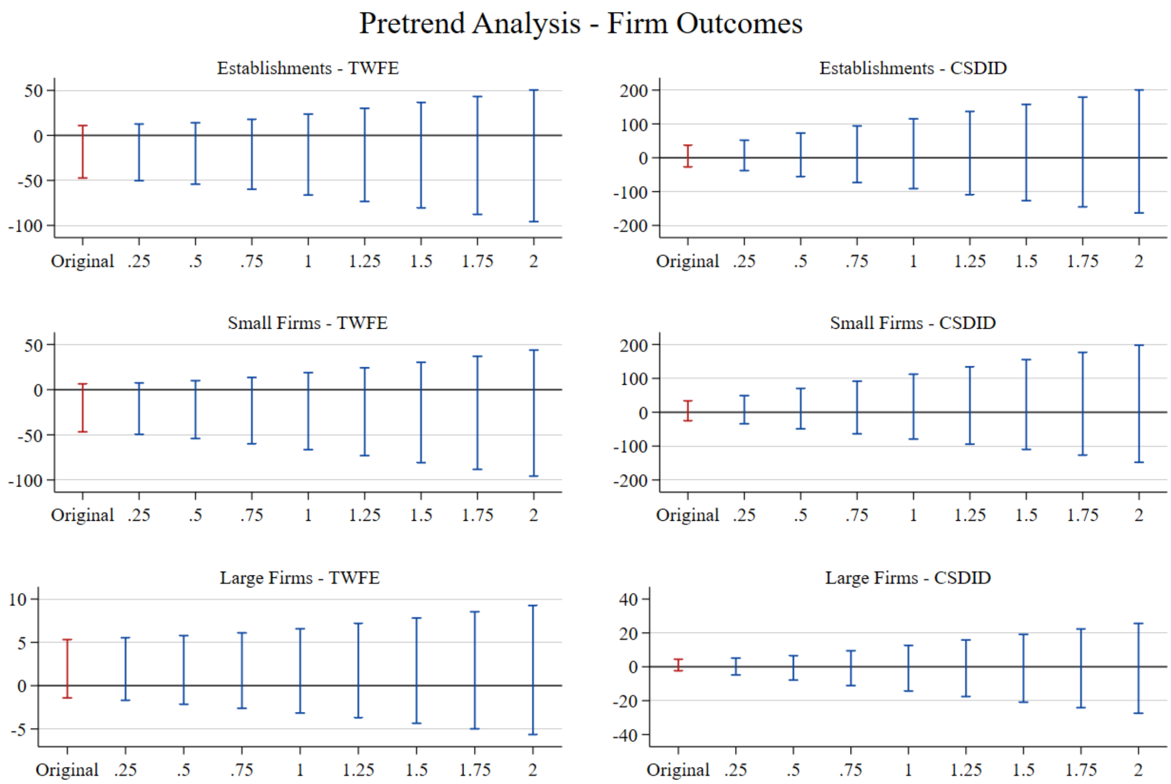
Table 8: Measuring Impact of Plant Construction Period (Horizontal Format)

	Establishments	**Small Firms**	**Large Firms**	**Employment Rate**	**Labor Force Potential**	**log(Weekly Wages)**
TWFE	-8.01	-8.98	0.98	0.007	0.002	0.012
	(24.15)	(22.31)	(2.47)	(0.008)	(0.001)	(0.012)
ETWFE	0.72	-1.54	2.23***	0.010***	0.001***	0.014***
	(11.72)	(11.15)	(0.64)	(0.003)	(0.000)	(0.005)
CSDID	23.27	21.03	2.29	0.010**	0.001*	0.016
	(25.65)	(23.87)	(2.15)	(0.005)	(0.001)	(0.010)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	No	No	No

Note: Asterisks indicate statistical significance: * p<0.10, ** p<0.05, *** p<0.01. Coefficients represent the Aggregate ATT of once construction has started but not before the first car rolled off the line of the plant, i.e. the window of analysis is $(-5, t \leq Opening)$). The TWFE, ETWFE, and CSDID specifications incorporate covariates. Covariates include fraction of non-white population, high school completion rate, and population density. ETWFE accounts for heterogeneous treatment timing (Wooldridge 2021). CSDID accounts for both treatment heterogeneity and staggered adoption (Callaway and Sant'Anna 2021). Results are clustered at the commuter zone level.

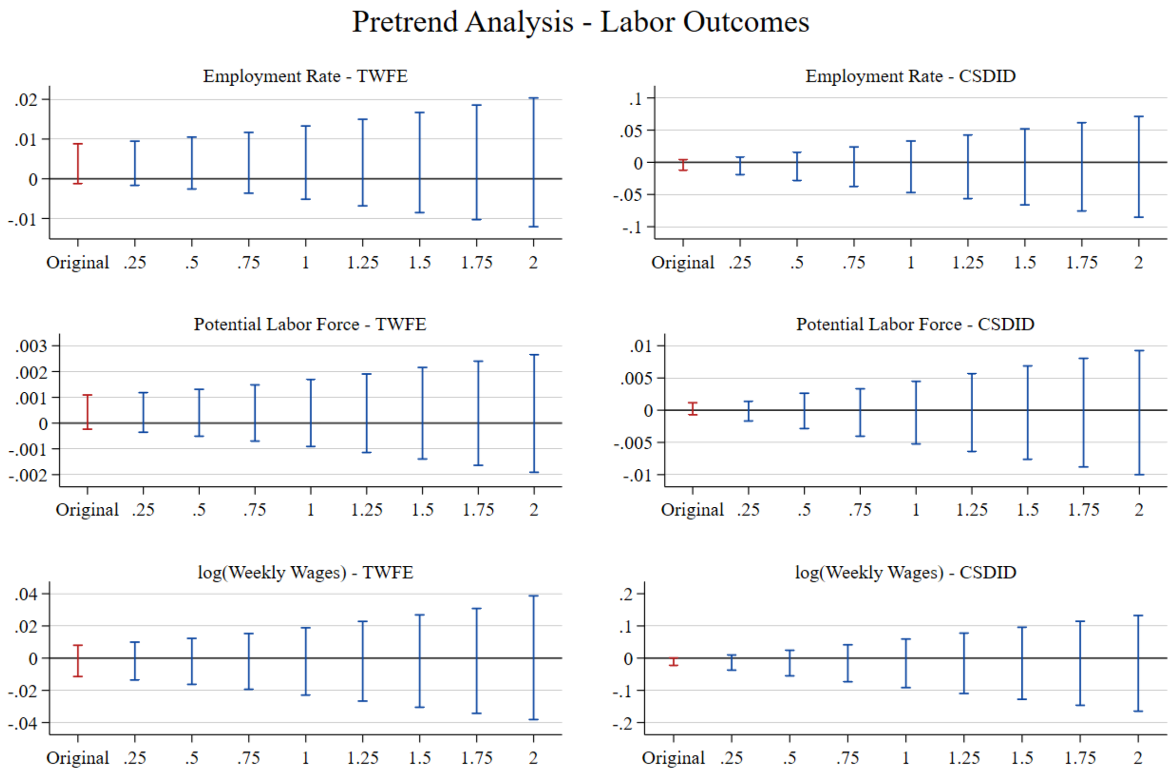
A Appendix

Figure A.6: Pretrend Sensitivity - Firm Outcomes



Note: This figure presents a pretrend analysis of firm outcomes using sensitivity testing to provide more credible parallel trends given potential violation (Rambachan and Roth 2023). The panels display estimated pretrend coefficients under two different estimation strategies: Two-Way Fixed Effects (TWFE) and the Callaway and Sant'Anna Difference-in-Differences (CSDID) estimator (Callaway and Sant'Anna 2021). Each panel examines different firm outcomes - establishments, small firms, and large firms - while varying sensitivity parameters to assess robustness. The "Original" specification corresponds to baseline estimates, while subsequent points explore alternative model assumptions wiht varying levels of confidence that parallel trends holds.

Figure A.7: Pretrend Sensitivity - Firm Outcomes



Note: This figure presents a pretrend analysis of firm outcomes using sensitivity testing to provide more credible parallel trends given potential violation (Rambachan and Roth 2023). The panels display estimated pretrend coefficients under two different estimation strategies: Two-Way Fixed Effects (TWFE) and the Callaway and Sant'Anna Difference-in-Differences (CSDID) estimator (Callaway and Sant'Anna 2021). Each panel examines different firm outcomes - establishments, small firms, and large firms - while varying sensitivity parameters to assess robustness. The "Original" specification corresponds to baseline estimates, while subsequent points explore alternative model assumptions with varying levels of confidence that parallel trends holds.

Table A.9: Fixed Covariates and Weights, Firm-Level Effects

	TWFE	ETWFE	CSDID
	(1)	(3)	(5)
A. Establishments			
ATT on Establishments	-26.084	-22.136	9.308
	(41.494)	(18.197)	(32.360)
Mean Untreated	1472.74	1472.74	1472.74
Mean Treated (g-1)	1783.78	1783.78	1783.78
Observations	-	-	-
B. Small Firms			
ATT on Small Firms	-31.248	-28.045	2.118
	(38.472)	(17.280)	(28.859)
Mean Untreated	1397.92	1397.92	1397.92
Mean Treated (g-1)	1678.82	1678.82	1678.82
Observations	-	-	-
C. Large Firms			
ATT on Large Firms	5.031	5.750***	7.038*
	(3.984)	(1.085)	(4.157)
Mean Untreated	73.67	73.67	73.67
Mean Treated (g-1)	103.36	103.36	103.36
Observations	-	-	-
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Weights	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Note: Asterisks indicate statistical significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$, **** $p<0.001$. Coefficients are representative of the Aggregate ATT post-treatment (10 years time). The leftmost column is for the uncontrolled models, and the rightmost incorporates covariates. Covariates include the fraction of the non-white population, the fraction of the population with at least a high school diploma, and population density for the county. ETWFE is an adapted two-way fixed effect model to combat treatment heterogeneity(Wooldridge 2021). CSDID (Callaway and Sant'Anna 2021) attempts to deal with treatment heterogeneity and staggered adoption. All results are clustered at the county level.

Table A.10: log(Population) Weights, Labor-Level Effects

	TWFE	ETWFE	CSDID
	(1)	(2)	(3)
A. Employment Rate			
ATT on Employment Rate	0.017	0.020***	0.014*
	(0.012)	(0.006)	(0.008)
Mean Untreated	0.40	0.40	0.40
Mean Treated (g-1)	0.42	0.42	0.42
Observations	-	-	-
B. Potential Labor Force			
ATT on Potential Labor Force	0.004***	0.003***	0.003**
	(0.001)	(0.001)	(0.001)
Mean Untreated	0.58	0.58	0.58
Mean Treated (g-1)	0.59	0.59	0.59
Observations	-	-	-
C. Log(Weekly Wages)			
ATT on Log(Weekly Wages)	0.025*	0.032***	0.021*
	(0.014)	(0.009)	(0.012)
Mean Untreated	6.45	6.45	6.45
Mean Treated (g-1)	6.46	6.46	6.46
Observations	-	-	-
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Weights	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Note: Asterisks indicate statistical significance: * p<0.10, ** p<0.05, *** p<0.01, **** p<0.001. Coefficients are representative of the Aggregate ATT post-treatment (10 years time). The leftmost column is for the uncontrolled models, and the rightmost incorporates covariates. Covariates include the fraction of the non-white population, the fraction of the population with at least a high school diploma, and population density for the county. ETWFE is an adapted two-way fixed effect model to combat treatment heterogeneity(Wooldridge 2021). CSDID (Callaway and Sant'Anna 2021) attempts to deal with treatment heterogeneity and staggered adoption. All models are weighted by log(population). All results are clustered at the county level.

Table A.11: Fixed Covariates and Weights, Firm-Level Effects

	TWFE	ETWFE	CSDID
	(1)	(3)	(5)
A. Establishments			
ATT on Establishments	45.488	-22.136	9.308
	(69.474)	(18.197)	(32.360)
Mean Untreated	1472.74	1472.74	1472.74
Mean Treated (g-1)	1783.78	1783.78	1783.78
Observations	-	-	-
B. Small Firms			
ATT on Small Firms	34.157	-28.045	2.118
	(63.554)	(17.280)	(28.859)
Mean Untreated	1397.92	1397.92	1397.92
Mean Treated (g-1)	1678.82	1678.82	1678.82
Observations	-	-	-
C. Large Firms			
ATT on Large Firms	11.086*	5.750***	7.038*
	(6.296)	(1.085)	(4.157)
Mean Untreated	73.67	73.67	73.67
Mean Treated (g-1)	103.36	103.36	103.36
Observations	-	-	-
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Weights	Yes	Yes	Yes
Controls	Yes	Yes	Yes

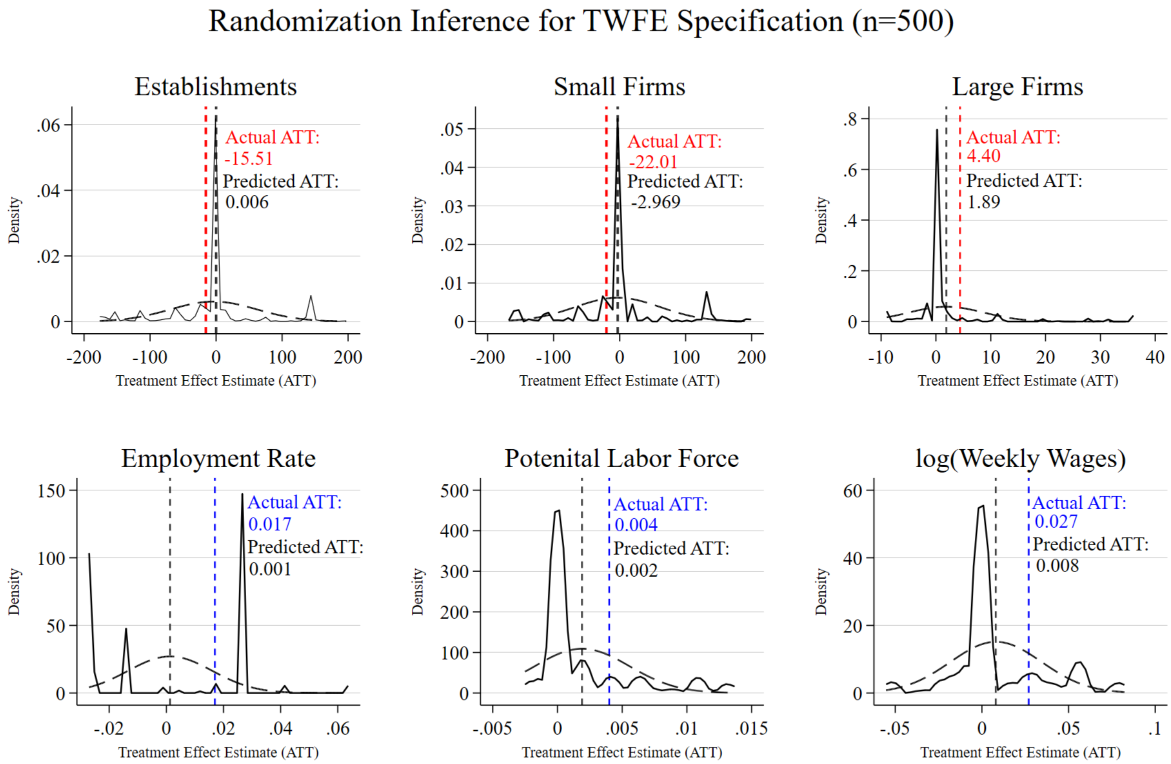
Note: Asterisks indicate statistical significance: * p<0.10, ** p<0.05, *** p<0.01, **** p<0.001. Coefficients are representative of the Aggregate ATT post-treatment (10 years time). The leftmost column is for the uncontrolled models, and the rightmost incorporates covariates. Covariates include the fraction of the non-white population, the fraction of the population with at least a high school diploma, and population density for the county. Covariates are fixed at their closest pretreatment value to treatment ($t = g - 1$) to account for any endogeneity in the post-treatment period. ETWFE is an adapted two-way fixed effect model to combat treatment heterogeneity(Wooldridge 2021). CSDID (Callaway and Sant'Anna 2021) attempts to deal with treatment heterogeneity and staggered adoption. The model is weighted by log(population). All results are clustered at the county level.

Table A.12: Fixed Covariates and Weights, Labor-Level Effects

	TWFE	ETWFE	CSDID
	(1)	(3)	(5)
A. Employment Rate			
ATT on Employment Rate	0.013	0.020***	0.014*
	(0.012)	(0.006)	(0.008)
Mean Untreated	0.40	0.40	0.40
Mean Treated (g-1)	0.42	0.42	0.42
Observations	-	-	-
B. Potential Labor Force			
ATT on Potential Labor Force	0.006***	0.003***	0.003**
	(0.001)	(0.001)	(0.001)
Mean Untreated	0.58	0.58	0.58
Mean Treated (g-1)	0.59	0.59	0.59
Observations	-	-	-
C. Log(Weekly Wages)			
ATT on Log(Weekly Wages)	0.026*	0.032***	0.021*
	(0.014)	(0.009)	(0.012)
Mean Untreated	6.45	6.45	6.45
Mean Treated (g-1)	6.46	6.46	6.46
Observations	-	-	-
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Weights	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Note: Asterisks indicate statistical significance: * p<0.10, ** p<0.05, *** p<0.01, **** p<0.001. Coefficients are representative of the Aggregate ATT post-treatment (10 years time). The leftmost column is for the uncontrolled models, and the rightmost incorporates covariates. Covariates include the fraction of the non-white population, the fraction of the population with at least a high school diploma, and population density for the county. Covariates are fixed at their closest pretreatment value to treatment ($t = g - 1$) to account for any endogeneity in the post-treatment period. ETWFE is an adapted two-way fixed effect model to combat treatment heterogeneity(Wooldridge 2021). CSDID (Callaway and Sant'Anna 2021) attempts to deal with treatment heterogeneity and staggered adoption. The model is weighted by log(population). All results are clustered at the county level.

Figure A.8: Robustness Checks on Outcome Variables Using Randomized Inference



Note: This figure presents robustness checks on outcome variables using randomized inference. The black density plots display the distribution of estimated treatment effects from randomly assigned placebo treatments, providing a benchmark for evaluating the observed treatment effect. The black dashed line represents the mean coefficient from these random placebo assignments, while the colored dashed line denotes the actual estimated treatment effect from the model. The top row shows firm outcomes, while the bottom row displays the labor outcomes. Due to computational constraints, the analysis employs a standard two-way fixed effects (TWFE) model rather than eTWFE and was ran for 500 iterations. This robustness test assesses whether the estimated treatment effects are distinguishable from those obtained under random assignment, supporting the validity of the main results.

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