Unintended Consequences of Eliminating Tax Havens*

Juan Carlos Suárez Serrato
Duke University & NBER

July 2018

Abstract

We show that eliminating firms' access to tax havens has unintended consequences for economic growth. We analyze a policy change that limited profit shifting for US multinationals, and show that the reform raised the effective cost of investing in the US. Exposed firms respond by reducing global investment and shifting investment abroad — which lowered their domestic investment by 38% — and by reducing domestic employment by 1.0 million jobs. We then show that the costs of eliminating tax havens are persistent and geographically concentrated, as more exposed local labor markets experience declines in employment and income growth for over 15 years. We discuss implications of these results for other efforts to limit profit shifting, including new taxes on intangible income in the Tax Cuts and Jobs Act of 2017.

*I am very grateful for comments and suggestions from Joe Altonji, Peter Arcidiacono, Alan Auerbach, Johannes Becker, Michael Devereux, Dhammika Dharmapala, Jonathan Dingel, Rafael Dix-Carneiro, Jim Hines, Niels Johannesen, Lisa Kahn, Karam Kang, Amit Khandelwal, Brian Kovak, Adam Looney, Jon Meer, Peter Merrill, Tom Neubig, Eric Ohrn, Toan Phan, Brendan Price, Nirupama Rao, Josh Rauh, Ethan Rouen, Stephen Ryan, Joel Slemrod, Chris Stanton, Max Todtenhaupt, Ludvig Wier, Daniel Xu, Danny Yagan, Owen Zidar, Gabriel Zucman, and Eric Zwick, as well as from seminar participants at CESifo Venice Summer Institute, Columbia, HBS, Trinity University, UC Davis, University of Copenhagen, University of Michigan, Washington University (Olin), and Yale. I am thankful to Nirupama Rao for sharing code, to Eric Ohrn for sharing data, and to John Oliver and Lin-Manuel Miranda for bringing §936 to my attention. Dan Garrett, Yamac Isik, Xian Jiang, Stephanie Karol, Ethan McClure, Linh Nguyen, Matt Panhans, and Harsh Parikh provided outstanding research assistance. This project benefited from funding from the Kauffman Foundation, the International Tax Policy Forum, and from visiting fellowships at Chicago Booth's Initiative on Global Markets and Yale University. All errors remain my own. Email: jc@jcsuarez.com; Web: www.jcsuarez.com.

1 Introduction

Profit shifting to tax havens by multinational corporations lowers tax revenues and is a key motivation for tax reform. For instance, the Tax Cuts and Jobs Act of 2017 introduced new taxes on intangible income with the aim of limiting profit shifting. While the debate over profit shifting focuses on tax revenues, this paper shows that limits to profit shifting raise the effective cost of investment, and incentivize multinationals to shift employment and investment abroad.

We show that eliminating tax havens has unintended consequences on the domestic economy by studying a policy that limited profit shifting by US multinationals. The policy we study, the repeal of Section 936 of the Internal Revenue Code, phased-out the ability of US multinationals to shift profits to affiliates in Puerto Rico.[1] Under §936, US corporations had an incentive to shift profits to Puerto Rico since §936 essentially excluded the income of Puerto Rican affiliates from corporate taxes. The repeal of §936 was equivalent to a loss of $232 billion in revenue for exposed firms, or to a 10 percentage point (pp.) increase in their effective corporate tax rate. Since firms exposed to §936 employed close to 11 million workers in the continental US and were responsible for 16.1% of all investment by publicly-listed firms, this large fiscal shock had the potential to cause lasting disruptions on the US economy.

Two sets of empirical findings demonstrate that repealing §936 was costly for the US economy. First, we use firm-level data to show that §936 firms responded to the repeal by lowering domestic investment and employment. We find that, relative to non-exposed firms, §936 firms reduced global investment by 23%, increased the share of investment abroad by 17.5%, and reduced US employment by 9.1%. These firm-level responses had significant effects on the US economy as exposed firms lowered domestic investment by 38%, and reduced their payroll by 1.0 million jobs.

Our second empirical finding is that firm-level effects are geographically concentrated and have persistent impacts on more exposed areas. We find that, 15 years after the repeal of §936, more exposed local labor markets saw slower growth in employment, income, wages, and home values, and that these areas became more reliant on government transfers. In particular, moving a location from the 25th to the 75th percentile of the distribution of exposure to §936 reduced employment growth by 7.2pp—about 35% of the growth in non-exposed locations.

The result that limits to profit shifting have significant unintended consequences on the real economy revises the view that profit shifting mainly distorts revenue.[2] In addition, these results reveal the importance of including regional effects in cost-benefit analyses of tax reforms, as fiscal shocks can have acute and long-lasting effects on local economies.

[1] Puerto Rico is a US possession but is treated as a foreign country for purposes of taxation. This paper studies measures of real economic outcomes in the continental US, excluding Puerto Rico.

[2] For instance, Gravelle (2016) argues that "profit shifting is largely a problem of lost revenue rather than inefficient location of investment," and the Tax Foundation (2016) notes "lawmakers are concerned about profit shifting because it has an impact on the amount of revenue that the corporate income tax ultimately collects."

We establish these findings in three steps. We first construct a model of multinational investment and profit shifting. Since profits generated in high tax countries may be shifted to low tax locations, firms with access to tax havens face a lower effective cost of investing in high tax countries. Repealing §936 eliminates access to a tax haven and raises the effective cost of investing in the US. The model predicts that multinationals would respond by lowering investment and employment, and by shifting economic activity to foreign affiliates.[3]

The second part of the paper tests these predictions using firm-level datasets. Our difference-in-differences methodology compares the investment and employment decisions of firms exposed to §936 to those of similar control firms. Using data from Compustat, we estimate that exposed firms decreased parent-level investment by 23%. While this is a substantial response, it implies a semi-elasticity of investment to taxes of -2.2, which is consistent with estimates from the literature.[4] In addition to lowering overall investment, firms reduced investment in the US by increasing their share of investment in foreign affiliates. These investment responses were accompanied by a decline in firm-level employment. Using the National Establishment Time Series (NETS) database, we find that exposed firms reduce US employment by 9.1% relative to control firms.[5] The data fit the predictions of the model and show that firms viewed the repeal of §936 as an increase to their effective costs of investing in the US.

This difference-in-differences approach yields unbiased estimates of the effects of repealing §936 if investment and employment in exposed firms would have been similar to that of control firms if §936 had not been repealed. This parallel trends assumption is supported by four facts. First, we present graphical evidence that control firms and firms exposed to §936 were on similar trends prior to the policy change. Second, we rule out that changes in other tax policies can confound the effects of §936. Third, the estimates are robust to analyzing firm-level changes or to including firm fixed effects. Fourth, we find similar effects when we analyze changes in industry-level investment using data from the Annual Survey of Manufactures. These facts significantly reduce the likelihood that our results are due to a spurious statistical anomaly.

In our third and final step, we show that firm-level responses had persistent effects on local labor markets. The establishment networks of firms exposed to §936 allow us to measure the degree to which each local economy was affected by this limit on profit shifting. We relate the fraction of establishments in a given county that could benefit from §936 to data on employment growth at the industry-county-year level from the Quarterly Census of Employment and Wages.

[3]The model is based on work by Hines and Rice (1994). Tax complementarities arise from profit shifting and are not due to complementarities in production or demand, as in Desai et al. (2009). Repealing §936 would lower US employment and investment if firms view the US as a high tax country and if capital complements labor.

[4]de Mooij and Ederveen (2008) conduct a meta-analysis that finds a median semi-elasticity of -2.9, Zwick and Mahon (2017) find a semi-elasticity of -2.89 when studying the effects of bonus depreciation, and Ohrn (Forthcoming) finds a semi-elasticity of -4.7 from studying the domestic production activities deduction.

[5]The employment effect is proportionate to the fall in investment. Specifically, a regression of US employment on firm investment predicts an 8.8% reduction in employment following a 23% decrease in investment.

We find that increasing a county's exposure to §936 from the 25th to the 75th percentile of the exposure distribution reduces employment growth by 7.2pp and income growth by 12.5pp. Using a long-difference specification across Census years, we also find a decrease in wage rates that is concentrated in the wages of workers without a college degree as well as decreases in housing prices and rental costs. Finally, we show that more exposed areas receive an additional $16 in unemployment benefits per-person and $30 in income replacement programs per-capita. Overall, the repeal of §936 had acute and long-lasting impacts on US local labor markets.

The key identifying assumption behind the local labor market effects is that exposure to §936 is not correlated with other shocks that could affect local labor markets. Several pieces of evidence justify this assumption. First, we show graphically that locations with different degrees of exposure have parallel trends prior to the repeal of §936. Second, the dynamic effects match the phase-out of §936 and, in particular, the effects stabilize after §936 is fully phased-out. Third, our preferred specification controls for industry-year fixed effects, which rule out concerns that the observed change in employment is due to industry-specific trends. Fourth, we show that exposure to §936 is not related to a battery of potential confounders and that the results are robust to including them as controls. Fifth, we find similar effects when we analyze different geographical units (counties, commuting zones, and conspumas); when we study changes in the employment-to-population ratio, which rules out the possibility that migration accommodates labor demand shocks; and when using data from several sources (QCEW, BEA, and IRS). Sixth, we conduct "placebo tests" that show that our results are not driven by exposure to firms with similar demographics as §936 corporations, which rules out that the results are driven by changes in the firm size distribution; or by exposure to the pharmaceutical industry, which was a heavy user of §936. Facing these facts, the most likely scenario is that the economic decline in more exposed local labor markets is driven by the firm-level responses we document in this paper.

We conclude our analysis of §936 by reconciling the firm-level effects on employment with the local labor market effects. If firm-level responses to the repeal of §936 interacted with agglomeration effects, had effects on suppliers of inputs, or affected local non-tradable industries, layoffs at §936 corporations could have spillover effects that amplified job losses within local labor markets. Our results suggest that every §936 layoff led to the loss of an additional 3.87 jobs in the local labor market, which is consistent with employment multipliers from Moretti (2010). In addition, an event-study across industries shows that tradable employment is the first to fall in response to the repeal of §936, and is followed by the non-tradable and construction sectors.

This paper contributes to the public finance literature by challenging the view (e.g., Gravelle (2016)) that profit shifting mostly affects tax revenues and is not a primary concern for real economic outcomes. By shining a light on the long-lasting effects on local economies, this paper shows that unequal geographic burdens from tax reform should be part of the policy debate.

This paper is also related to research quantifying profit shifting by multinational corporations.

Grubert and Mutti (1991) and Hines and Rice (1994) showed in pioneering work that reported profits are negatively correlated with corporate tax rates. Dharmapala and Riedel (2013) use earnings shocks at the firm level to document evidence of profit shifting within multinationals, which suggests smaller effects than earlier work.[6] Clausing (2009) estimates that profit shifting lowers US tax revenues by $100 billion annually. According to this methodology, §936 accounted for 10-15% of total profits shifted out of the US at the time of its repeal.

Our results contribute to the literature studying real responses of multinational corporations to international taxation. Grubert and Slemrod (1998) analyze the investment and profit shifting decisions of §936 corporations, and show that investments in Puerto Rico followed from a profit shifting strategy to avoid US taxation. Desai et al. (2009) show that foreign investment is complementary to domestic investment. Becker and Riedel (2012) find evidence of cross-border effects of taxes, such that taxation at the parent location may affect investment by affiliates. Kovak et al. (2017) show that increases in foreign employment are not associated with decreases in domestic employment. Finally, de Mooij and Liu (2018) study the effects of transfer pricing regulations, and find that anti-avoidance rules lead to a decrease in domestic investment.[7]

While most papers in this literature use variation in taxes or regulations across countries, this paper exploits a natural experiment that phased-out access to a tax haven worth about $232 billion in revenue to exposed firms. The size and timing of this reform provides a unique opportunity to reassess the effects of tax havens on domestic investment and employment, and this paper is the first to document the effects of profit shifting on local labor markets.

The results of the paper inform policies that limit profit shifting. In particular, the Tax Cuts and Jobs Act of 2017 enacted new minimum taxes on the intangible income of US multinationals. This paper suggests that these provisions may raise taxes and destroy jobs in the US. The results also contribute to a debate over the desirability of preferential tax regimes for mobile capital, and to whether it is optimal for tax havens to play this role (Dharmapala, 2008).

The paper proceeds as follows. Section 2 describes §936, Section 3 presents the model, and Section 4 describes how we measure exposure to §936. Section 5 tests the model predictions on firm-level data. Section 6 shows that local labor markets that were more exposed to this profit shifting strategy experienced declining growth in employment following the repeal of §936. Section 7 discusses the implications of these results for policies designed to combat profit shifting.

[6] See Dharmapala (2014) for an in-depth review of this literature and Heckemeyer and Overesch (2013) for a meta-analysis. Recent approaches that consider non-linear models (Dowd et al., 2017), as well as recent work by Clausing (2016) and Tørsløv et al. (2018), find larger estimates of profit shifting. Guvenen et al. (2017) show that profit shifting biases estimates of productivity growth and GDP.

[7] See Hines (1999) and Gordon and Hines (2002) for excellent reviews of this literature. Devereux and Griffith (1998) show that multinationals locate production facilities in countries with lower tax rates. Mintz and Smart (2004) show that income shifting across Canadian jurisdictions may increase investment. Overesch (2009) also finds significant effects of home country tax rates on inbound investment to Germany. Buettner et al. (2012) show that thin capitalization rules affect the choice of equity and debt at the firm level. Egger and Wamser (2015) and Albertus (2017) find that investment is hampered by controlled foreign corporation (CFC) laws.

2 The Possessions Tax Credit and the Repeal of §936

Starting with the Revenue Act of 1921 the US government exempted US multinationals from income taxes if the income originated in affiliated corporations located in Puerto Rico or other US possessions.[8] While the Tax Reform Act of 1976 replaced the exemption with a tax credit, possessions corporations were essentially exempted from US taxes under the new regime. The new credit was formally known as the US Possessions Corporations Tax Credit, but it was commonly referred to as §936, in reference to the relevant section of the Internal Revenue Code (26 U.S.C. §936, 1976).

§936 was susceptible to profit shifting maneuvers due to weak rules governing the transfer of intangible assets and relatively lax transfer pricing rules. Consider first the role of intangibles in profit shifting. A US multinational can shift tax obligations from the US to a tax haven by assigning a patent to a low tax affiliate. The company can then use royalty payments from the US parent to the low tax affiliate to lower its US taxable income, as well as its overall tax payments. While a transfer of intangible assets is usually considered a taxable transaction, transfers to possessions affiliates were allowed to be essentially untaxed (Eden, 1994, §351 of IRC). The Joint Committee on Taxation (2006) describes an example where a US pharmaceutical could develop a drug patent in the US, deduct labor and R&D costs for the purposes of the federal income tax, and then assign the patent to the affiliate in Puerto Rico, where royalty income would go untaxed. While the Tax Equity and Fiscal Responsibility Act of 1982 put an end to this particular maneuver, possessions corporations benefitted from relatively lax transfer pricing rules until 1986 when §936 was harmonized with transfer pricing regulations (§482 of IRC).[9] Concerns that §936 continued to be a conduit for profit shifting were heightened by a GAO (1993) report that found that the tax credits were greater than the average compensation of Puerto Rican employees in §936 firms.[10]

The Small Business and Job Protection Act of 1996 repealed §936.[11] The law allowed for a

[8] Other US possessions include Guam, American Samoa, Northern Mariana Islands, and the US Virgin Islands. However, the vast majority of exempted income originated in possessions corporations located in Puerto Rico. Affiliates qualified for tax exemption if 80% of their income originated in US possessions, and if at least 50% of their income derived from active business operations. In addition, the government of Puerto Rico provided tax holidays that exempted most of their income from local corporate taxes for up to 25 years (Bond, 1981).

[9] Hexner and Jenkins (1995) and Eden (1998) criticize the implementation of these rules and claim that intangible assets transferred to Puerto Rico represented profit shifting motives with little or no real investment.

[10] According to the GAO (1993) the tax credit per worker was about $25,000, which was greater than the average employee compensation. The cost per employee was even higher for drug companies, who received over $70,000 in tax credits per employee (Eden, 1998).

[11] §936 was a minor provision in the law, which focused on increasing capital expense limits for small businesses and raising the minimum wage. In contrast to bills where lobbying activity affects whether a piece of legislation is enacted (Kang, 2016), this bill was passed by the house almost unanimously (414-10), had bipartisan support in the Senate (74-24), and moved from committee to final passage very quickly (HR3448, 1996). This timing suggests the legislation was not subject to intense lobbying pressure. Moreover, we find no explicit mention of this bill or §936 in lobbying expenditure data (OpenSecrets.org, 2018). Note that the 1996 reform repealed the

10 year transition period during which companies that had previously claimed §936 credits could continue to do so. While the technical details of the phase-out are complicated, many firms saw the value of their credits decrease to 60% of their original value by 1996, and to 40% by 1998.[12]

Panel A in Figure 1 displays the fiscal cost of §936 to the US Treasury by industry using data from the IRS (2017a). This panel shows the gradual elimination of the tax credits, starting from about $4.9 billion in 2017 dollars, and ending in their full phase-out by 2006. In Section 4, we use firm-level data on the profitability and tax obligations of §936 firms to show that these tax credits were equivalent to a tax rate increase of 10pp, and to a revenue loss of $232 billion. This graph shows large drops in the value of these credits after 1996 and 1998, which matches statutory limits on the tax credits. Panel B shows the composition of these credits by industry. In 1995, about 57% of these credits were claimed by corporations in the chemical manufacturing industry. Firms manufacturing equipment, food, beverage, and tobacco claimed about 26% of the credits, while other industries claimed the remaining 17%.

While policymakers debated the effects of §936 on employment in Puerto Rico, they ignored the effects of repealing §936 on domestic employment. Table 1 shows US and Puerto Rico employment for US multinationals with more than 1,500 employees on the island in 1990.[13] Many of these firms are in the chemical, food, and electronic manufacturing industries. The pharmaceutical industry established a strong presence on the island since companies like Abbott Laboratories, Wyeth, Bristol-Myers Squibb, and Merck could engage in profit shifting by locating their intellectual property in Puerto Rican affiliates.[14]

Table 1 shows that just a few §936 firms accounted for about 650,000 jobs in the US, and that the 682 firms with Puerto Rican affiliates employed over 8.8 million US workers in 1990. By 1995, §936 firms employed close to 11 million workers in the US. Several of these large firms, including Baxter International, Bristol-Myers Squibb, and General Electric decreased their US employment by an average of 25% after the repeal of §936. These anecdotes show that repealing §936 could have important implications for employment in the US. This hypothesis is consistent with recent models that show that shocks to a small number of large firms can have significant consequences for aggregate fluctuations (Gabaix, 2011) and for trade (Bernard et al., 2016).

tax credits for tax years starting after December 31, 1995 (26 U.S.C. §936, 1996). For this reason, we use 1995 as the reference year in our empirical analysis.

[12]Firms could elect to use an economic activity or an income credit method to compute the value of the credits (Joint Committee on Taxation, 2006). For firms using the income credit method, the credit was limited to a fraction of the pre-repeal value of the credit. This limit was set at 60% in 1996, and was set to decrease to 40% by 1998. For firms using the economic activity method, the credits were determined by 60% of the sum of wage expenses plus a fraction of intangible property depreciation and local income taxes.

[13]We defer a detailed discussion on data sources to Section 4. Several of these firms are also listed in press accounts of firms that took advantage of §936 (Business Week, 1993; Los Angeles Times, 1993).

[14]In some cases the Puerto Rican operations of these large firms were not in their main industrial lines of business. For example, while Sara Lee is best known for its consolidated foods, the Puerto Rican operations focused on hosiery. Sara Lee cut employment in Puerto Rico in 2001 and then again in 2003, with no indication that these jobs would be shifted to US plants (New York Times, 2003).

While the repeal of §936 is credited with contributing to Puerto Rico's crisis, it is worth noting that the crisis began after the full phase-out of §936. We show in Appendix Figure A.1 that, outside of the 2001 recession, overall employment in Puerto Rico grew until the onset of the crisis in 2006.[15] Chari et al. (2017) analyze the effects of the debt crisis on growth in Puerto Rico. They argue that the cointegration of the economies of Puerto Rico and the United States broke down after 2012. This timing is convenient for our analysis as it rules out the concern that the domestic effects on employment and investment were caused by changes in Puerto Rico's economic prospects.

3 A Model of Profit Shifting with Tax Complementarities

This section builds on the frameworks of Hines and Rice (1994) and Grubert and Slemrod (1998) to show theoretically that profit shifting lowers the cost of capital in other countries. These tax complementarities generate empirical predictions that answer the question: how would the repeal of §936 affect investment and employment in the US?

Suppose a firm is located in Puerto Rico, the United States, and a third country C, and has the after-tax profits given by:

$$(1-t_C)f_C(K_C)+(1-t_{US})(f_{US}(K_{US})+f_{PR}(K_{PR}))+t_{US}f_{PR}(K_{PR})-\rho(K_C+K_{US}+K_{PR}),$$

where ρ is the nondeductible opportunity cost of equity capital, and where $f_j(\cdot)$ is a firm's production function in country j, which is increasing in capital, $f_j'(K)>0$, and exhibits decreasing returns to investment, $f_j''(K)<0$. The possessions tax credit is $t_{US}f_{PR}(K_{PR})$, which gives a zero tax rate on profits from operations in Puerto Rico.[16] Our analysis generalizes beyond the three country case, where global after-tax profits are given by $\sum_j[(1-t_j)f_j(K_j)-\rho K_j]$.

Firms make two sequential decisions. They first decide where to locate their capital. In the second period firms choose a profit shifting strategy. Firms engage in profit shifting by misreporting their profitability as r_j, which may deviate from their actual profitability $\bar{f}_j = f_j(K_j)/K_j$ in country j. Misreporting is costly to the firm since it may face penalties, litigation

[15] While this paper does not study the effects of §936 on the Puerto Rican economy, Feliciano and Green (2017) use industry-level data to show that the repeal of §936 led to a decline in manufacturing establishments. Mora et al. (2017) provide an in-depth study of the origins of the crisis. *La Crisis Boricua* had manifold causes including new sales taxes in Puerto Rico; demographic trends related to population control policies that started in the 1930s; high costs of living due to increased trade costs from the Jones Act; increased public indebtedness; and a reliance on imported oil for electricity generation, which deeply affected the economy as oil prices doubled between 2005–2012. In 2006, these conditions coalesced into the "perfect storm" behind Puerto Rico's debt crisis.

[16] We assume corporations in Puerto Rico would pay the US corporate rate absent §936, and that profits from country C would be reinvested in the country C in the case that $t_C < t_{US}$. While Puerto Rico had positive corporate taxes, their magnitude is negligible in practice and we omit them from the analysis. As in Hines and Rice (1994), we assume firms are able to avoid global US taxation of corporate income by reinvesting abroad. Altshuler and Grubert (2003) discuss several strategies that may allow firms to avoid taxes triggered by repatriation. Finally, we assume the cost of capital ρ is not affected by small changes in tax rates.

costs resulting from challenges from tax authorities, and real costs of transferring intangible assets.[17] We assume misreporting costs are proportional to local capital, are convex in the degree of misreporting, and take the form:

$$C(r_j, \bar{f}_j, K_j) = \frac{K_j(r_j - \bar{f}_j)^2}{2a}.$$

The parameter a modulates these costs with high values of a implying low misreporting costs.[18]

We start by analyzing profit shifting in the second period. Given a capital allocation $\{K_j\}$, firms solve the tax planning problem:

$$\max_{\{r_j\}} \sum_j K_j \left[(1-t_j)r_j - \rho - \frac{(r_j - \bar{f}_j)^2}{2a}\right], \qquad \text{subject to: } \sum_j \bar{f}_j K_j = \sum_j r_j K_j,$$

where the constraint implies that the firm must report total profits truthfully, but may shift the location of its profits.

Proposition 1 (Optimal Profit Shifting)**.** *Firms over-report (under-report) profits in countries where the corporate tax rate is below (above) the capital-weighted average:*

$$r_j = \bar{f}_j + a(\tilde{t} - t_j),$$

where the capital-weighted average tax rate $\tilde{t}$ is given by $\tilde{t} = \frac{\sum_j t_j K_j}{\sum_j K_j}$.

See Appendix A for detailed proofs of the results in this section. Proposition 1 states our version of the result in Hines and Rice (1994) that reported profitabilities are negatively correlated with the country's tax rate.[19] Panel A in Figure 2 describes this result graphically. Firms over-report profits in countries with low taxes ($\tilde{t} > t_j$) and under-report in countries with high taxes ($\tilde{t} < t_j$). The degree of misreporting depends on the cost of profit shifting: the relationship between r_j and the tax differential is relatively flat (solid line) when a is low (high cost of misreporting), and is steeper (dashed line) when a is large (low cost of misreporting).

In the first period, firms foresee their profit-shifting strategies and invest across countries to maximize a combination of economic and profit-shifting incentives. Substituting the optimal profit shifting strategies into the objective function and rearranging we obtain:

$$\max_{\{K_j\}} \underbrace{\sum_j \left[(1-t_j)f_j(K_j) - \rho K_j\right]}_{\text{Economic Incentives}} + a \sum_j K_j \underbrace{\left[(1-t_j)(\tilde{t} - t_j) - \frac{(\tilde{t} - t_j)^2}{2}\right]}_{\text{Net Benefit from Profit Shifting}}. \tag{1}$$

Firms' optimal investment plans trade-off the economic and tax incentives in Equation 1, and take into account the effect of their capital allocations on the capital-weighted average tax, $\tilde{t}$.

[17] This analysis takes the network of affiliates as given. For analyses of the extensive margin of operating in a tax haven see Grubert and Slemrod (1998), Desai et al. (2006), and Gumpert et al. (2016).

[18] While the results of the model depend on this function, we obtain similar qualitative patterns from alternative functional forms that capture convex misreporting costs that are proportional to the firm's activity in the country.

[19] As in Huizinga and Laeven (2008), misreporting for a given firm depends on its capital-weighted mean tax, $\tilde{t}$.

Proposition 2 (Profit Shifting Lowers the User Cost of Capital (UCC))**.** *Profit shifting (weakly) lowers the UCC in all countries. The decline is largest in countries with tax rates that are most different from* $\tilde{t}$*. UCCs can be observed from the first-order condition of Equation 1:*

$$(1-t_j)f_j'(K_j) = \rho - \frac{a(\tilde{t}-t_j)^2}{2} \equiv \tilde{\rho} \leq \rho.$$

The effective cost of capital $\tilde{\rho}$, which Grubert and Slemrod (1998) call the "income shifting adjusted user cost of capital," is reduced by the net marginal benefit from profit shifting.[20] Panel B in Figure 2 describes the intuition of this result. Without profit shifting, the firm sets capital so that the after-tax marginal product of capital equals the cost of equity ρ^{NPS} (dotted line). When firms can shift profits, the income-shifting adjusted user cost of capital $\tilde{\rho}_0$ (dashed line) is below ρ^{NPS}, which results in a higher level of investment $K_j^0 > K_j^{NPS}$.

While Proposition 2 shows profit shifting lowers the effective cost of capital in all countries, the mechanism for this effect is different across high and low tax countries. Affiliates in high tax countries know the returns from investment will be partly shifted to tax havens, which lowers the required return on investment. Multinationals have an incentive to invest in tax havens as real investment lowers the cost of profit shifting. When a country increases its tax rate, tax havens see additional investment as the higher tax rate increases the returns from profit shifting. In contrast, high tax countries see a decrease in investment as the returns from investment will now be taxed at a higher rate. The main result of the model formalizes this intuition by characterizing which countries are tax complements and tax substitutes with tax havens.

Proposition 3 (Profit Shifting Generates Tax Complements and Substitutes)**.**

- *Investment in high-tax countries* $(t_j > \tilde{t})$ *is decreasing in* t_i *for all* $j \neq i$*;*
- *Investment in low-tax countries* $(t_j < \tilde{t})$ *is increasing in* t_i *for all* $j \neq i$*; and, formally,*

$$sign\left(\frac{\partial K_j}{\partial t_i}\right) = sign\left(\tilde{t} - t_j\right).$$

To clarify the intuition behind Proposition 3, note that the repeal of §936 is equivalent to Puerto Rico introducing a corporate tax, t_{PR}. Differentiating the expression in Proposition 2, we see that repealing §936 would increase the effective cost of capital and reduce US investment:

$$\frac{\partial K_{US}}{\partial t_{PR}} = \frac{a(\tilde{t}-t_{US})}{\underbrace{-f_{US}''}_{>0}(1-t_{US})} \underbrace{\frac{\partial \tilde{t}}{\partial t_{PR}}}_{>0} = \frac{r_{US}-\bar{f}_{US}}{-f_{US}''(1-t_{US})}\frac{\partial \tilde{t}}{\partial t_{PR}} < 0 \quad \Leftrightarrow \quad r_{US} < \bar{f}_{US}.$$

[20] Given the quadratic misreporting costs in the model, the net marginal benefit is given by: $\frac{a(\tilde{t}-t_j)^2}{2} = \frac{(r_j-\bar{f}_j)^2}{2a}$. Gordon and Hines (2002) discuss a similar expression in their presentation of Hines and Rice (1994). See also Overesch (2009) and Mintz and Smart (2004). Note that this result relies on the assumption that the production function does not exhibit complementarities across countries. Since employment in Puerto Rican affiliates accounted for about 1.6% of employment in §936 firms, we view this as an innocuous simplifying assumption in this application. See Desai et al. (2009) and Becker and Riedel (2012) for models with such complementarities.

In Appendix A, we show that increasing t_{PR} raises the firm's average rate $\tilde{t}$ even after firms reallocate capital across countries. The second equality substitutes the optimal profit shifting rule from Proposition 1, which shows that repealing §936 would lower US investment as long as profits are under-reported in the US, $r_{US} < \bar{f}_{US}$. We show this graphically in Panel B of Figure 2, where the dot-dashed line shows that a tax increase in a tax haven raises the cost of investing in a high tax country, $\tilde{\rho}_1$, which lowers domestic investment: $K_j^0 > K_j^1 > K_j^{NPS}$.

Proposition 4 (Profit Shifting and Labor Demand)**.** *Suppose production depends on capital and labor and that capital complements labor in country j. Then:*

- *Employment in high-tax countries $(t_j > \tilde{t})$ is decreasing in t_i $\forall j \neq i$;*
- *Employment in low-tax countries $(t_j < \tilde{t})$ is increasing in t_i $\forall j \neq i$; and, formally,*

$$sign\left(\frac{\partial L_j}{\partial t_i}\right) = sign\left((\tilde{t} - t_j) \times \frac{\partial^2 f_j(K_j, L_j)}{\partial K_j \partial L_j}\right).$$

Proposition 4 shows that when labor and capital are complements, the tax complementarities in Proposition 3 also extend to the labor market. Panel C of Figure 2 shows the effects of profit shifting on the labor market. Since capital and labor are complements, labor demand depends on the cost of capital $(L^D(w, \rho))$ and it shifts out when firms can shift profits to tax havens: $L^D(w, \tilde{\rho}_1) > L^D(w, \rho^{NPS})$. Conversely, it shifts in when there is a tax increase in the low-tax country: $L^D(w, \tilde{\rho}_0) < L^D(w, \tilde{\rho}_1)$. This figure shows that there is more domestic employment when tax havens reduce the effective cost of capital: $E_j^{NPS} < E_j^1 < E_j^0$.

Overall, the model shows that profit shifting to tax havens complements economic activity in high tax countries and has the following empirical predictions for the repeal of §936:

Empirical Predictions. *Assuming the US is a high-tax country, $\tilde{t} \leq t_{US}$, and that capital complements labor in the US, repealing §936 would:*

- *Decrease profit shifting toward Puerto Rico and increase reported profitability in the US;*
- *Lower global investment and employment for exposed firms; and*
- *Shift investment and employment from the US toward low tax countries.*

While most of the literature focuses on the first empirical prediction, the contribution of this paper is to provide empirical evidence that profit shifting has real effects on the economy by using variation from §936 to test the last two predictions of the model.[21]

[21] OECD (2018) confirms the US was a high tax country in 1996. We do not test the first model prediction as we cannot measure affiliate-level profitability in our data. Appendix A discusses extensions of the model and shows that Propositions 1-4 are robust to allowing for heterogeneous costs of profit shifting across countries.

4 Measuring Exposure to §936

This section summarizes the data we use to test the predictions of the model and to quantify the effects of the repeal of §936 on local labor markets. Appendix B provides further details.

4.1 Firm-Level Exposure and Outcomes

We measure firm-level exposure to §936 using the National Establishment Time-Series (NETS, Walls & Associates (2012)), which links the universe of US establishments to firm headquarters. There are 682 US firms that had establishments in Puerto Rico in 1995 and that could have benefited from §936. These firms had broad networks of establishments and many employees; close to 11 million workers in the continental US had jobs in §936 firms in 1995. In Appendix B, we validate this measure of exposure by showing that it is close to estimates of take-up of §936 based on tax data and that it identifies firms with similar characteristics to §936 firms. As we discuss below, we conduct a number of tests to ensure that our results are robust to our definition of exposure.

We use data from Compustat (Capital IQ, 1980-2014) on firm-level (global) investment.[22] In 1995, §936 firms were responsible for 16.1% of all capital investment reported by Compustat firms. We also use the geographic segments variables to analyze whether repealing §936 led to offshoring of investment as predicted in our model in Section 3.[23]

To gauge the importance of the repeal of §936 for exposed firms, we compare Compustat data on federal taxes paid with the tax credits discussed in Section 2. In 1995, §936 tax credits accounted for 10.5% of their federal taxes. Given that these firms had an effective federal rate of 26% and an average profit ratio of 8.12%, the repeal of §936 would be equivalent to losing $232 billion in sales ($\approx \frac{\$4.9\text{bn}}{8.12\% \times 26\%}$ in 2017 dollars), which is comparable in size to the increase of Chinese imports studied by Autor et al. (2013).[24] This loss in revenue corresponds to 8% of the revenue of these firms.[25]

We complement the firm-level analysis by using data from the Annual Survey of Manufactures

[22] We merge the list of exposed firms by name, confirm and extend the merge manually, and further crawl the SEC's EDGAR database to confirm we include all firms related to Puerto Rico or §936. There are 219 exposed firms in Compustat, which are responsible for 69.5% of the total employment of §936 firms. Grubert and Slemrod (1998) note that, in their data, 96% of the tax credits were claimed by 214 firms.

[23] Geographic segments data divide global investment into US and foreign segments and it is not generally possible to separate investment by individual countries. In order to avoid outliers, we winsorize investment-to-capital ratios at the 5% level.

[24] We compare our setting to the results of Autor et al. (2013) in Appendix C. For comparison, the supply component of the China Shock studied by Autor et al. (2013) corresponded to an increase in imports of $175 billion between 1991 and 2007 (in 2017 dollars).

[25] These calculations are upper bounds as they attribute all the credits to Compustat firms. Using the employment share to assign 69.5% of the tax credits to these firms, this would imply the tax credits accounted for 7.3% of their federal taxes, and were equivalent to 5.6% of their revenue. Since §936 were highly concentrated within §936 firms (Joint Committee on Taxation, 2006), it is likely the true value is closer to the upper bound of 10.5%.

(ASM) (Annual Survey of Manufacturers, 1997) to measure the effects of repealing §936 on investment at the state-industry level. These data include investment by private firms and isolate investment in the US, as opposed to Compustat data which report investment by publicly-listed firms that may occur in foreign affiliates.

4.2 Geographic Exposure and Outcomes

To assess the effects of the repeal of §936 on local labor markets, we create a geographic measure of exposure. We begin by linking the exposed firms to their network of establishments in the US. Our measure of exposure is the fraction of establishments in each US county that belongs to firms that were exposed to §936. Figure 3 plots the distribution of this measure across the US. Panel A shows that the fraction of labor market exposure varies from 0% to 5%. Panel B shows that there is substantial variation across counties within a given state by plotting a standardized measure of exposure that subtracts state averages and divides by the national standard deviation. Our main measure of exposure is based on the fraction of establishments to avoid problems related to imputation of establishment-level employment. In robustness checks, we show similar results when we use a measure of exposure based on the fraction of workers in exposed firms,[26] and we also conduct robustness checks at different levels of geography including commuting zones and conspumas.[27]

Figure 3 shows that the regional exposure to §936 is not geographically clustered. Nonetheless, one may be concerned that exposure to §936 is correlated with demographic characteristics, industrial compositions, state policies, or other economic shocks that may confound the effect of §936. Figure 4 shows that exposure to §936 is generally not correlated with minimum wages and right to work laws (Valletta and Freeman, 1988; Meer and West, 2016); state taxes and tax revenues (Suárez Serrato and Zidar, 2017); R&D tax credits (Wilson, 2009); exposure to competition through trade (Autor et al. (2016) for China and Hakobyan and McLaren (2016) for NAFTA); the share of routine labor (Autor and Dorn, 2013); demographic and industry employment (US Census); or with capital stocks (BEA). The only statistically significant correlation is a positive one with the fraction of workers with a college education (Table A.2 reports these correlations). In Section 6, we confirm that controlling for these potential confounders does not affect our main results.

Our main dataset on labor market outcomes is the Quarterly Census of Employment and

[26] Barnatchez et al. (2017) evaluate the properties of the NETS data. They show the NETS measures firm-level employment and establishment location very well, but that establishment-level employment is imputed for some firms. Figure A.2 compares the establishment- and employment-based measures of exposure and shows that these metrics are highly correlated. Table A.1 displays the counties with the larger and smallest exposure for both measures.

[27] Figure A.3 plots the exposure to §936 for counties, states, commuting zones, and conspumas. The conspuma level is an aggregation of counties that respects state boundaries, and is the smallest consistently identifiable geographic unit that can be traced between the 1980 and 2010 US Censuses.

Wages (QCEW, 2017). These data are constructed mostly from administrative sources, with unemployment insurance systems providing highly reliable data on employment and earnings. These data are available at the 3-digit-NAICS industry-by-county level. This is important for our study since it allows us to control for industry-by-year fixed effects, which rule out the concern that §936 firms faced industry-specific shocks that may confound the effect of repealing §936.

We use three additional datasets on labor market outcomes at the county-level. First, we use BEA data on personal employment and income, which include other forms of employment and income, including proprietor's income (Bureau of Economic Analysis, 2018). Second, we use data from IRS tabulations at the county level on the number of filers and total taxable income (IRS, 2017b). Finally, we use data from the BEA on public transfers, such as unemployment insurance payments. While the BEA and IRS data have broader definitions of income, they are not available at the industry-county level.

Finally, we use data from the 1980–2000 US Censuses and the 2009 American Community Survey (Ruggles et al., 2010) to construct average wage rates at the conspuma-decade level that control for changes in workers' demographic characteristics. We construct these measures for all workers, and for workers with and without a four year college education. We also use these data to construct indexes of rental costs and home values.

Overall, these different datasets allow us to measure the exposure to §936 for firms and local labor markets, how exposed firms may have responded by adjusting employment and investment, and how these responses affected local labor markets.

5 Firm-Level Effects of Repealing §936

This section tests the predictions of the model and shows that exposed firms respond to the repeal of §936 by adjusting the scale and location of investment and employment.

5.1 Effects of Repealing §936 on Firm Investment

We begin by showing that the repeal of §936 led to a decline in the global investment of exposed firms. We use data from Compustat to estimate the following regression:

$$\frac{\mathrm{I}_{ct}}{\mathrm{K}_{c,1990-1995}} = \alpha_i + \gamma_t + \beta^t \text{Exposure §936}_c + \varepsilon_{ict}, \tag{2}$$

where $\frac{\mathrm{I}_{ct}}{\mathrm{K}_{c,1990-1995}}$ is the ratio of capital investment to the pre-reform capital of the firm, and where we allow the firm-level indicator of Exposure §936$_c$ to have year-specific effects, β^t. α_i are 3-digit industry fixed effects, γ_t are year fixed effects, and we cluster ε_{ict} at the firm level. The identifying assumption of this regression is that firms that took advantage of §936 were not on a differential trend from other firms, and that exposed firms were not subject to other shocks

or policy changes that coincided with the repeal §936.[28] We test the parallel trends assumption and show that our results are robust to restricting the sample to firms in similar industries as §936 firms. Figure 5 reports the results of this regression with 95% confidence intervals. The graph shows that exposed and non-exposed firms had very similar trends in investment before the repeal of §936, and that exposed firms saw a decline in investment after the repeal. This graph suggests that firms respond to the reform by lowering the investment rate and letting capital depreciate. Thus, while the phase-out of §936 lasts 10 years, firms respond by lowering investment immediately.

Table 2 reports the average effect of exposure after the repeal and shows that the decline in investment is robust across a number of specifications. Column (2) shows that this result is not affected by including firm fixed effects. Columns (3)-(5) show that we find stronger declines in investment when we compare exposed firms to control firms in industries that were heavy users of §936. In column (3) we restrict the sample to only include firms in 3-digit industries that contain at least one §936 firm, and in column (4) we restrict the sample to 2-digit sectors that were heavy users of §936. The last column imposes both restrictions and shows our preferred estimate of -0.23. These estimates confirm the model prediction that firms respond to limits on profit shifting as they do in response to tax increases: by lowering global investment.

The magnitude of the estimates in Table 2 is in line with previous estimates of the effects of corporate taxes on investment. To interpret this magnitude, note that the outcome variable had a mean near 1 in 2005, so that our preferred estimate in column (5) implies a reduction in investment of 23%. de Mooij and Ederveen (2008) conduct a meta-analysis of studies that analyze the effects of corporate taxes on investment and find a median semi-elasticity of investment of -2.9 with respect to the tax rate.[29] Since eliminating §936 tax credits was equivalent to a tax increase of 7.3-10.5pp for exposed firms, the median estimate from de Mooij and Ederveen (2008) would predict a decline in investment of 21.2%-30.5%, which is consistent with our results.[30]

[28] While the assumption that exposed firms were not affected by unobserved, time-varying shocks that coincide with the repeal of §936 is fundamentally untestable, there are no other tax reforms that could confound the effect of §936. For instance, the 2003 dividend tax cut studied by Yagan (2015) affects all of the firms in the sample equally since they are all C-corporations. Similarly, periods of bonus depreciation in 2001 and 2008 have differential effects across industries that rely on assets with different tax lives, as studied by Zwick and Mahon (2017). We rule this out by comparing exposed firms to control firms in similar industries. Finally, other changes such as the adoption of "check-the-box" regulations by the IRS in 1997 would not confound the effect of §936. "Check-the-box" allows multinationals to avoid having income classified under Subpart F when it is transferred between related closely-held-corporations. Since these transactions are between affiliates in third-party-countries and tax havens, they would not interact with §936, and they would not differentially affect §936 firms.

[29] In recent work, Zwick and Mahon (2017) find a semi-elasticity of -2.89, and Ohrn (Forthcoming) estimates a semi-elasticity of -4.7. We can also interpret our results as implying a semi-elasticity of -2.2.

[30] We use the pre-repeal estimate of the tax increase since multinational corporations can inter-temporally optimize their tax liabilities by not repatriating income, or by deferring certain credits. In addition, Compustat data show patterns of sticky reporting of taxes over time. However, we find that repealing §936 increased reported taxes by a similar amount. Table A.3 estimates the effect of repealing §936 on reported federal cash taxes paid, and shows that the tax rate of exposed firms increased by 6.9pp (column 5) after the repeal. Compared to Table

Nonetheless, the investment response to §936 had significant effects on aggregate investment. Since exposed firms conducted 16.1% of investment in all Compustat firms, the repeal of §936 resulted in an aggregate investment decline of 1.9-3.7% for publicly listed firms.[31]

We now test the model's prediction that, in addition to lowering investment, exposed firms would shift investment toward affiliates with lower taxes. We exploit geographic segments data from Compustat (Capital IQ, 1980-2014) to show that §936 firms shifted investment to foreign affiliates. We analyze the effects of the repeal on the share of foreign investment, $\text{ForInvShare}_{ct} = \frac{I^F_{ct}}{I^F_{ct}+I^{US}_{ct}}$, and estimate the following specification:

$$\frac{\text{ForInvShare}_{ct} - \text{ForInvShare}_{c1995}}{\text{ForInvShare}_{c1995}} = \alpha_i + \gamma_t + \beta^t \text{Exposure §936}_c + \varepsilon_{ict}, \tag{3}$$

where we again allow for industry and year fixed effects, and where we cluster the standard errors at the firm level. Figure 6 reports the results of this specification. This figure shows that exposed and non-exposed firms were on a similar trend before the repeal of §936, and that, consistent with the model, exposed firms increased the share of investment conducted abroad after the reform.

Table 3 quantifies the size of this effect using similar specifications as in Table 2.[32] Our preferred coefficient in column (5) suggests that the foreign investment share increased by 17.5% of its value in 1995. Since the average of $\text{ForInvShare}_{c1995}$ was about 45pp, this implies an increase of 8pp to about 53pp. Note also that the dynamics in Figure 6 complement those in Figure 5, and the combined effect of these two forces implies a decrease in domestic investment by exposed firms of 38%.[33]

We provide complementary evidence that these two forces led to a persistent decline in US investment by using data from the Annual Survey of Manufactures to analyze the effect of §936 on investment in US establishments. Since these data are available at the industry-year level, we generate a measure of exposure to §936 by collapsing the data in Figure 3 at this level, and estimate the following specification:

$$\frac{\text{CAPX}_{ist} - \text{CAPX}_{is1995}}{\text{CAPX}_{is1995}} = \alpha_i + \gamma_t + \beta^t \text{Exposure §936}_{is} + \varepsilon_{ist}. \tag{4}$$

We include industry and year fixed effects and allow for arbitrary correlation of standard errors at the state level. The continuous exposure measure is fixed in 1995 but we allow the effects of

2, this increase in tax rates implies a semi-elasticity of investment to the corporate tax rate of -3.3.

[31] The effects on investment are robust to using alternative measures of investment. Table A.4 and Figure A.4 show results of estimations on the percentage change in investment. Table A.5 and Figure A.5 show results as in Table 2, but where we winsorize the the investment-to-capital ratio at the 1% level instead of the 5% level. Columns (5) in Tables A.4 and A.5 imply semi-elasticities of investment of -2.0 and -3.3, respectively.

[32] Since the percentage change specification absorbs the firm fixed-effect, column (2) controls for industry fixed effects. Table A.4 shows that we obtain similar effects on investment when we use this specification.

[33] This follows from a $15\%\left(\approx \frac{8pp}{55pp}\right)$ decrease in the domestic share of investment and a 23% decrease in investment overall investment from Table 2.

exposure to §936 to vary over time by estimating year-specific coefficients β^t. We normalize the exposure measure by its inter-quartile range (IQR), so the interpretation of β^t is the effect of increasing exposure to §936 from the 25th-percentile to the 75th-percentile of the distribution. The identifying assumption of Equation 4 is that exposure to §936 is not correlated with other trends or omitted variables that may also affect capital investment. Figure 7 plots the results of this estimation and shows that Exposure §936$_{is}$ is not correlated with investment growth prior to the reform. Following the reform, however, we see a persistent decline in the flow of investment. This graph shows that limiting profit shifting, through the repeal of §936, raised the effective cost of capital, which had significant and persistent unintended consequences on domestic investment.

Table 4 reports the average effects of Exposure §936$_{is}$ by pooling the years after the repeal. This table shows that the decline in investment is robust to including state fixed effects, state-by-industry fixed effects, year-by-industry fixed effects, and year-by-state fixed effects. These specifications show that the effects of Exposure §936$_{is}$ are not driven by trends at the industry or state level that are somehow correlated with investment patterns. The last column in Table 4 suggests that going from the 25th to the 75th percentile of the distribution results in a decline in investment of 66% of the capital expenditures in 1995.[34]

The results of Figures 5-7 provide compelling evidence that capital investment responded to expected future tax rates. That is, while §936 tax credits were phased-out over time, investment decreased immediately after the reform as firms expected the tax credits to eventually disappear. Firms reduced their capital stocks by lowering investment, and allowing existing capital to depreciate over time. To understand how the reduction in the investment flow would affect the capital stock over time, we assume that prior to the reform capital in the US was in a steady state. For capital assets with an economic depreciation rate of 0.069, as in the case of industrial machinery, a 38% reduction in investment would result in a 19.3% reduction in the capital stock over 10 years.[35]

Overall, these results show that exposed firms decreased investment in the US and shifted investment to foreign affiliates, and that repealing §936 had persistent negative effects on US investment.

5.2 Effects of Repealing §936 on Firm Employment

This section tests the model prediction that exposed firms reduced employment in the US in response to an increase in the income-shifted adjusted cost of capital. We use data from NETS on the 682 firms with establishments in Puerto Rico, as well on a set of control firms that were

[34] Since the mean CAPX$_{is1995}$ was about \$349,000, this effect represents a decline of about \$230,000.

[35] Similarly, capital would decline by 8.4% over a decade if depreciation were 0.025, as in the case of office buildings, and by 27.7% if depreciation were 0.122, as in the case of computer electronics. Figure A.6 shows this dynamic graphically and Appendix D provides details of these calculations.

similar to the exposed firms, but that did not have establishments in Puerto Rico. We find this set of control firms by matching the exposed firms to other large firms that did not have operations in Puerto Rico, but that were in the same industry, were headquartered in the same Census region, and had a similar number of employees and establishments in 1995.[36]

We measure the effect of the repeal of §936 on the US employment of exposed firms by estimating the following regression:

$$\frac{\text{Emp}_{ct} - \text{Emp}_{c1995}}{\text{Emp}_{c1995}} = \gamma_t + \beta^t \text{Exposure } \S936_c + \varepsilon_{ct}, \tag{5}$$

where the exposure measure is a firm-level indicator, where we control for year fixed effects, and where the standard errors are clustered at the firm level.[37] The identifying assumption is that firms with exposure to §936 had similar trends to the matched control firms. Figure 8 presents estimates of this regression and shows a lack of differential pre-trends between exposed and control firms. The figure also shows that exposed firms gradually reduced employment during the phase-out of §936. As discussed in the previous section, while investment responded shortly after the repeal, capital stocks likely declined over time. The dynamics in Figure 8 are consistent with a decline in employment that matches the gradual decline in capital stocks.[38]

Figure 8 shows that, by 2010, exposed firms saw a relative decrease in employment of 9.1% (see Table A.6 for point estimates). One way to grasp the magnitude of this effect is to compare the change in employment to the change in investment using our merged Compustat-NETS data. Figure A.7 plots a regression of log US employment from NETS on log capital expenditures from Compustat and shows that a 1% increase in capital expenditures is associated with a 0.39% increase in US employment. Thus, the 23% reduction in capital expenditures from Table 2 would be consistent with an 8.8% ($\approx 39\% \times 23\%$) decrease in employment, which is very close to the estimate from Figure 8. In contrast to the effect of corporate taxes on investment, there is no consensus estimate of the effect of corporate taxes on labor demand. One of the incidental contributions of this empirical strategy is to provide an estimate of this important parameter. Compared to the tax increase of 7.3-10.5pp, the 9.1% reduction in employment implies a labor semi-elasticity between -1.26 and -0.87.

These results confirm the model prediction that US multinationals would reduce domestic employment in response to a reform that limited firms' access to tax havens. Given that exposed firms employed almost 11 million US workers in 1995, this unintended 9.1% decline in employment resulted in a loss of about 1.0 million jobs in §936 firms.

[36] See Appendix B.3 for details of the matching procedure used to identify the control firms.

[37] Since this regression estimates a firm-level change in employment, it already absorbs a firm-level fixed effect.

[38] In Figure A.8 we plot a similar figure to Figure 8 but where the dependent variable is the change in the number of US establishments. This figure shows that the change in US establishments lagged that of US employment. This is consistent with a dynamic where downsizing firms first lay off workers across locations and then incur the fixed costs of consolidating establishments.

6 Effects of Repealing §936 on Local Labor Markets

If workers were able to relocate across locations or shift to firms that were not affected by §936, one might view the results from Section 5 as necessary steps in transitioning to a new equilibrium.[39] However, if workers face adjustment costs across sectors or locations, or if §936 fostered agglomeration economies between local firms, the transition to a new equilibrium may come at the expense of substantial adjustment costs.[40] If this is the case, proposals to limit profit shifting should account for the persistent disruptions to local labor markets that may come as part of the unintended consequences of eliminating tax havens. This section studies the local labor market effects of repealing §936, and tests whether the firm-level responses in Section 5 are absorbed or amplified within local labor markets.

6.1 Effects of Repealing §936 on Employment Growth

We now use the geographic measure of exposure to §936 in Figure 3, Exposure §936$_c$, to estimate the relative effect of the repeal of §936 on employment growth at the industry-county level.[41] Our main outcome of interest is the employment growth between 1995 and a given year t:

$$\frac{\text{Emp}_{ict} - \text{Emp}_{ic1995}}{\text{Emp}_{ic1995}},$$

where the unit of observation is employment at the industry (i), county (c), year (t) level. Figure 9 provides a non-parametric comparison of places with different degrees of exposure by plotting the average employment growth for counties in the top and bottom terciles of Exposure §936$_c$. This figure shows that, while county-industries in the top and bottom terciles of Exposure §936$_c$ had similar trends before 1995, more exposed counties (top tercile) had a diminished rate of growth after the repeal of §936. The dynamics show a large change after 1999, which is consistent with the phase-out of the tax credits in Figure 1. By the end of the phase-out in 2006, places in the bottom tercile (low exposure) experienced an employment growth above 20% relative to 1995, while places in the top tercile (high exposure) had an average growth rate below 10%.

We build on Figure 9 by estimating a regression model that separates the effect of §936 from time trends. Consider the following regression:

$$\frac{\text{Emp}_{ict} - \text{Emp}_{ic1995}}{\text{Emp}_{ic1995}} = \alpha_{ic} + \gamma_{it} + \beta^t \text{Exposure §936}_c + \varepsilon_{ict}. \quad (6)$$

[39] One may even be inclined to see the effects from Section 5 in a positive light. That is, if §936 tax credits made some firms artificially big, eliminating §936 may result in a reduction in misallocation, especially if less productive firms selected into §936, or if §936 helped firms accumulate and exploit market power.

[40] For instance, Dix-Carneiro (2014) finds significant sectoral adjustment costs from trade liberalization, and Yagan (2017) shows that migration did not insure workers from local shocks during the great recession.

[41] As discussed in Section 4, Exposure §936$_c$ is the fraction of the county's establishments that could have taken advantage of §936 in 1995.

This regression includes industry-year fixed effects, γ_{it}, and industry-county fixed effects, α_{ic}. Since industry-county pairs vary in the number of jobs they represent, we weight these regressions by the share of national employment in each industry-county in 1995. The variable Exposure §936$_c$ is normalized in order to interpret β^t as the effect of increasing the Exposure §936$_c$ from the 25th to the 75th percentile of the distribution (inter-quartile range, or IQR). Finally, the standard errors are clustered to allow for two-way arbitrary correlations at both the state and industry levels.

Panel A of Figure 10 shows estimates of β^t for $t = 1990, \cdots, 2012$. The main result is that more exposed counties saw slower employment growth starting with the repeal of §936, and that this decline in employment growth persisted even after the end of the phase-out. For 2006, the graph shows that increasing the exposure to §936 from the 25th to the 75th percentile would decrease employment growth by 7.8%. Moreover, the decline in employment growth stabilizes after the full phase-out of §936 in 2006.[42] Panel B of Figure 10 plots the fitted values of this regression for different levels of exposure. This figure shows that going from almost zero exposure (5th percentile) to the mean level of exposure would decrease employment growth by 7% from about 23% to 16% in 2006, which is a change in employment growth of about 30% ($\approx \frac{7\%}{23\%}$). This graph provides compelling evidence that tax haven activities are complementary to domestic employment and that limits to profit shifting lead to persistent decreases in domestic employment growth.

6.2 Robustness Checks and Placebo Tests

The identifying assumption of Equation 6 is that Exposure §936$_c$ is independent of time-varying shocks that could explain the pattern in Figure 10. This section presents evidence that severely limits the likelihood that this assumption is violated in the data.

A useful check of the validity of this research design is to test for trends in employment growth prior to the repeal of §936. Panel A of Figure 10 shows that, prior to the repeal of §936, exposure to §936 is not predictive of employment growth, which is consistent with the difference-in-differences assumption. Table 5 presents alternative specifications of this regression. The first column includes industry-by-year fixed effects, which ensures that the results are not driven by differential exposure to industries that are growing or declining during this time period. The second column adds county fixed effects. Column (3) includes county-by-industry fixed effects, which ensure that the estimated β^t's are not confounded by differential growth rates in employment at the industry-county level. Columns (4)-(5) explore the robustness of the results

[42]Notably, the graph is relatively stable during Puerto Rico's debt crisis, which started in 2006. The fact that the effects stabilize after 2006 also rules out other policy changes that could be potential confounders. Specifically, the hypothesis that "check-the-box" regulations that were adopted in 1997 are responsible for these effects is inconsistent with this dynamic pattern since, according to Wright and Zucman (2018), profit shifting under check-the-box continued to grow between 2006-2012.

to using alternative weights. In Column (4), we winsorize the weights to limit the influence of large counties on the results. In Column (5), we drop county-industries with less than 1000 employees in 1995. The pattern of results is consistent across these specifications.

Our results are robust to using alternative geographic definitions of local labor markets and data sources. Panel A of Figure 11 plots the last two columns of Table 5, which show that we obtain similar results when we study employment at the conspuma or commuting zone level. These results rule out the concern that our main results are driven by very local geographic spillovers. Table 7 summarizes these results by estimating the effect for years 1990-1995 and 2004-2008. The first row of results shows that we obtain similar estimates at the conspuma or commuting zone level, or when we use data on employment from the IRS or the BEA.[43] The last columns of Table 7 report effects on income growth. Consistent with the effects on employment, we also find more exposed areas have slower income growth, and that this result is robust across different data sources and different definitions of local labor markets.[44]

As we discuss in Section 4, our main measure of exposure to §936 is based on the fraction of establishments in a county that could have taken advantage of §936. Panel B of Figure 11 shows the results of an estimation where we measure Exposure §936$_c$ as the fraction of the county's employment that could have benefited from §936 in 1995. Since this measure likely contains measurement error (see Section 4), we instrument exposure to §936 with our main (establishment-based) measure of exposure. This figure and column (1) of Table 6 show that we obtain similar results with this measure of exposure. Our results are also robust to using alternative measures of exposure to §936 that exclude certain firms, as shown in Panel B of Figure 11 and columns (2)-(4) of Table 6. The first alternative measure excludes large retailers that are likely located in most counties in the US and Puerto Rico. We again obtain similar results, which rules out the concern that our main results are driven by secular trends in the retail industry. A second alternative measure excludes firms with uncertain headquarter locations and finds similar effects. Finally, we find similar effects when we use a measure of exposure that focuses on the employment of publicly-listed firms in Compustat. This figure shows that our results are not sensitive to how we construct the measure of exposure.

We also explore the robustness of these results to using alternative measures of employment growth. If workers left counties that saw declines in employment after the repeal of §936, we might see no effects on the employment-to-population ratio. Panel A of Figure 11 (column (5) of Table 6) shows results when the dependent variable is the percentage change in the employment-to-working age population ratio, which shows similar if slightly smaller effects. This suggests that, while some workers do relocate in response to the shock, local workers are affected by

[43]Since IRS and BEA data are not available at the country-industry level, we only control for year fixed effects.

[44]Note that the definitions of income vary across data sources. While QCEW data report wage earnings, BEA data include proprietor's income, and IRS data correspond to total adjusted gross income in a county. These differences may explain the larger effects on income growth in IRS and BEA data.

the repeal of §936 since the employment-to-population ratio sees a significant decline. Finally, column (6) focuses on the ratio of manufacturing employment to the working age population, as in Autor et al. (2013), and finds slightly larger effects.[45]

We rule out the concern that our results are driven by exposure to firms with certain characteristics by conducting placebo tests that isolate potential confounding channels. One concern is that our results are driven by exposure to large firms that may have already been in decline. In Panel A of Figure 12, we use the set of control firms in Section 6.1 to generate a "fake shock." Since these firms have a similar size, are headquartered in a similar location, and have similar industrial characteristics, this test rules out that our exposure measure is picking up employment trends in large firms that had similar demographics as exposed firms. Since a large fraction of §936 tax credits were claimed by the pharmaceutical industry, a second concern is that the exposure measure is picking up industry-specific trends that are not related to §936. Panel B of Figure 12 defines exposure as the fraction of employment in the pharmaceutical industry, and shows that this is not the case. Both of these "placebo tests" show that these alternative mechanisms are not driving our main result.[46]

Given the evidence presented so far, a challenge to the exclusion restriction requires that the exposure to §936 be correlated with a shock that varies at the county-industry level, but that is independent of industry-wide dynamics. In our final robustness check, we explore this possibility by controlling for a battery of potential confounders. Our base specification in column (3) of Table 7 shows an effect in years 2004-2008 of -7.2%. Figure 13 plots this baseline result as well as robustness checks that control for local incentives for job creation and worker training (Bartik, 2017), right to work laws and minimum wage rates (Valletta and Freeman, 1988; Meer and West, 2016), state R&D and investment tax credits (Wilson, 2009), state tax revenue, and state taxes for personal income, corporate income, sales and property (Suárez Serrato and Zidar, 2017), measures of exposure to trade from NAFTA and China (Autor et al., 2016; Hakobyan and McLaren, 2016), and the share of routine workers (Autor and Dorn, 2013). The last specification includes all controls and further controls for changes in the local firm size distribution.[47]

Overall, the litany of robustness checks and placebo tests in this section show that the effects

[45] Appendix C provides an in-depth comparison with the results of Autor et al. (2013). Panel A of Figure A.9 plots the event study on manufacturing jobs per capita and Table A.7 provides additional specifications. In addition, Panel B of Figure A.9 and Table A.8 present the results when the outcome variable is the level change in the ratio of manufacturing jobs to the working age population, as in Autor et al. (2013).

[46] The estimates used to plot these graphs are presented in the Appendix. Table A.9 provides estimates for the employment measure, Table A.10 provides estimates for the measure that drops retailers, Table A.11 provides estimates for the measure that drops firms with uncertain headquarter location, Table A.12 provides estimates for the measure that relies solely on Compustat firms, Table A.13 provides estimates for the placebo test using the fraction of employment in the control firms, and Table A.14 provides estimates for the placebo test using the fraction of employment in the pharmaceutical industry.

[47] Specifically, we use data from the County Business Patterns (Census Bureau, 2018) to control for the percentage growth in the number of establishments of different sizes in each location. We present the full results of these 18 regressions in Appendix Tables A.15 and A.16.

of §936 on local labor markets are not confounded by other shocks, are not sensitive to how we measure exposure to §936, and do not depend on the data we use to measure labor market outcomes. These facts strongly support the conclusion that the local labor market results are caused by the large firm-level responses we document in Section 5.

6.3 Spillovers and Heterogeneous Effects of Repealing §936

The results from the previous section provide strong evidence that more exposed local labor markets experienced a significant and persistent economic decline following the repeal of §936. In this section, we consider the role of local spillovers as a mechanism that can amplify firm-level changes in employment. We might expect to find spillover effects if the reduction of economic activity in §936 corporations reduced the demand for locally sourced material inputs, if it affected the supply of inputs to competitors, or if the reduction in employment in §936 firms affected local non-tradable sectors.

To compute an estimate of job spillovers, we first calculate the expected number of layoffs in §936 firms in the county with the average level of exposure. Using the firm-level estimates from Figure 8, we calculate that the average county experienced a loss of 368 jobs in §936 firms. We now compare this number to the total number of jobs lost in the county with the average level of exposure. Using the average estimate between 2004-2008 from Table 7, going from no exposure to the average level of exposure would result in a loss of 1,792 jobs for the average county. This implies that, for every layoff in §936 firms, the county also lost an additional 3.87 ($\approx \frac{1{,}792-368}{368}$) jobs. Moretti (2010) studies local employment multipliers and finds similar estimates.[48] This suggests that the firm-level changes in employment and investment at §936 firms were not absorbed by other firms in the same sector, and that these job losses had significant multiplier effects for other workers in the local labor market.

An additional implication of the spillover hypothesis is that jobs in the tradable sector should be the first to be affected by the repeal of §936. Moreover, as argued by Moretti (2010), we would expect workers in tradable industries to be more mobile, so that the long-term effect on tradable jobs should be smaller than on service jobs. We test these hypotheses by estimating heterogeneous effects of the repeal of §936 across sectors, as in Mian and Sufi (2014).[49] Figure 14 tests these hypotheses and shows that tradable jobs were the first to decline and were followed by a decline in non-tradable jobs and construction. In addition, the decline in tradable jobs is

[48] For instance, Moretti (2010) finds that when a local labor market loses a skilled job in the tradable sector, this results in a loss of about 2.5 jobs in the non-tradable sector, in addition to about 0.26 jobs in the tradable sector. However, he finds considerable heterogeneity across industries including a loss of 4.9 jobs in the non-tradable sector for every layoff in the electronic manufacturing industry (which was represented in §936 firms).

[49] Mian and Sufi (2014) argue that local shocks to demand were an important driver of the employment drop in the great recession. In contrast, we use their sectoral definitions and find evidence of a supply-side response to the repeal of §936.

the first to stabilize, while other sectors decline for a longer period of time.[50] This suggests that local employment multipliers exacerbated the firm-level effects of the repeal of §936.

6.4 Effects of Repealing §936 on Wage Rates, Costs of Living, and Government Transfers

In addition to showing that local employment is affected by the repeal of §936, we now show that wage rates and costs of living are also impacted by policies that limit firms' access to tax havens. We explore the effects of the repeal of §936 on wage data and rental cost data from the Census, which allow us to examine effects on skilled (college-educated) and non-skilled workers and to adjust for changes in observable characteristics. We estimate regressions of the form:

$$\Delta y_{c,t} = \alpha_s + \gamma_t + \beta \text{Exposure} \S 936_c + \varepsilon_{ct},$$

where $\Delta y_{c,t}$ is the 10-year percentage change in wage rates, rental costs, or home values in a given conspuma. These regressions include state and year fixed effects and the standard errors are clustered at the conspuma level.

Table 8 reports the results of estimating the parameter β as a stacked difference for years 1990-2000 and 2000-2010. We find that more exposed conspumas saw a decline in wage growth after the repeal of §936. Going from the 25th to the 75th percentile of exposure reduced wages by 1.0%. Figure 15 plots these results and shows that exposure to §936 was not correlated with wage growth before the repeal of §936.

The results of the previous section show that non-tradable employment faced a steep reduction in labor demand. If low-skill workers are concentrated in non-tradable sectors, we might expect to see a larger decrease in their wages. Column (3) of Table 8 shows that this is the case, as the wages of low-skilled workers fall by 1.4%.[51] Consistent with the broad decline in employment, income, and wages, we also find reductions of 1.8% and 2.5% in rental costs and home values, respectively.

Finally, we consider the possibility that decreases in employment and income increased workers' reliance on government transfers. Table 9 estimates a similar specification on government transfers per capita at the county level using data from the Bureau of Economic Analysis (2018). Panel A shows the effects of an IQR increase in Exposure §936$_c$ on the percentage change in transfers per capita, relative to the 1995 level. We find that, in 2004-2008, compensation from unemployment insurance increased by 25.7% for every IQR increase in exposure. We also find an

[50] Figure A.10 shows these estimates with confidence intervals, and Table A.17 provides more details. Note that, while the decrease in construction seems abnormally large, the construction industry is accustomed to wild fluctuations and, while the tradable sector declined by 10% nationally from 1995-2006, the construction industry grew overall by 49%.

[51] The larger decline in the wages of low-skill workers is also consistent with relatively higher mobility costs. Table A.18 provides estimates on wages and costs of living that do not adjust for observable characteristics.

effect of 10.2% on transfers from income replacement programs.[52] However, we do not find significant changes for other forms of transfers including retirement, disability, medicare, and public medical benefits. Panel B shows the effect of Exposure §936$_c$ on the dollar change in transfers per capita relative to 1995. Since income replacement programs are larger than unemployment insurance programs, we see that an IQR increase in Exposure §936$_c$ would raise income maintenance transfers by $30 per person, and unemployment insurance transfers by $16. Panel B also shows a positive effect of $6 per person in education benefits, but this effect is not statistically significant in Panel A.[53] These results show that some of the tax revenues that are collected from firms as part of efforts to combat profit shifting will be spent on government transfers to affected workers.

Overall, this section shows that the repeal of §936 generated persistent disruptions in economic activity in areas that were more exposed to this policy, and that the costs of significant changes to the tax code can be more geographically concentrated than previously thought.

7 Conclusion

This paper exploits the repeal of §936 as a natural experiment that limited the ability of firms to shift profits to affiliates in Puerto Rico. Consistent with our model where limits to profit shifting raise the effective cost of capital, we find that reducing the tax attractiveness of Puerto Rico reduced employment and investment in the US. These economic losses may have been justified by the elimination of a tax expenditure for US taxpayers. However, the cost benefit analysis at the time ignored the effect of §936 on the domestic economy. Our analysis suggests that an accurate cost benefit analysis of §936, as well as of other policies that aim to limit profit shifting, should include the unintended consequences of these policies and account for the employment and investment losses in the US. In particular, the result that these losses affect some local economies more than others, and that they persist years after the repeal of §936 suggests policymakers should consider the regional consequences of changes to tax policy.

It is worth noting that the effect of the repeal of §936 on US economic activity is indicative that, at the time, firms did not have access to multiple substitutable tax havens. This is contrary to popular accounts of tax planning strategies by multinationals that assume firms can shift profits at no additional cost. However, this is consistent with results of Desai et al. (2006) that show the demand for tax havens is not as elastic as popular accounts suggest. In Appendix E, we study whether news related to the repeal of §936 affected the stock prices of exposed firms.

[52] According to the Bureau of Economic Analysis (2018), income replacement programs include Supplemental security income (SSI) benefits, the Earned Income Tax Credit (EITC), Supplemental Nutrition Assistance Program (SNAP), welfare payments (Aid to Families with Dependent Children (AFDC), previously Temporary Assistance for Needy Families (TANF)), and nutrition programs for Women, Infants, and Children (WIC).

[53] Table A.19 shows that Exposure §936$_c$ is uncorrelated with growth in government transfers prior to the repeal.

Investors viewed the repeal of §936 as a negative event for these firms, as evinced by a decrease in their stock prices following the announcement of plans to repeal §936.[54]

Puerto Rico might have been an especially desirable tax haven for US multinationals. The tight cultural and institutional connections between the US and Puerto Rico might have lowered the cost of setting-up and operating affiliates on the island. In addition, §936 was essentially condoned by the US treasury and firms did not face the uncertainty of being challenged by the IRS.[55] In ongoing work (Garrett and Suárez Serrato, 2018), we study whether §936 corporations responded to the repeal by increasing their exposure to other tax havens.

Our results have implications for current debates over policies that aim to limit profit shifting. First, if intangible-heavy firms are relatively more mobile, it may be desirable to have a preferential tax regime with lower tax rates for these firms (Keen, 2001). §936 essentially played the role of a preferential tax regime. As we show, its repeal had negative consequences for the US economy.

Second, our results are related to a debate over the desirability of tax havens (e.g. Dharmapala (2008)). Hong and Smart (2010) argue that since profit shifting increases multinational investment, tax havens can help governments screen mobile and non-mobile forms of capital in order to redistribute from domestic firm owners to workers.[56] In contrast, Slemrod and Wilson (2009) argue that enforcement costs negate these benefits in equilibrium, such that it is desirable to eliminate all tax havens.[57] The evidence in this paper cannot settle this debate since this ultimately depends on the equilibrium of a tax competition game between countries. However, this paper provides empirical evidence for the critical assumption in both Hong and Smart (2010) and Slemrod and Wilson (2009) that limits to profit shifting would reduce investment by multinationals.

Third, the Tax Cuts and Jobs Act of 2017 enacted new taxes on multinationals corporations.[58] In particular, the tax on global intangible low-tax income (GILTI) aims to limit profit shifting by US multinationals. Perhaps because the real effects of profit shifting are understudied, analysts have excluded these policies when scoring the macroeconomic impacts of the tax reform (e.g., Barro and Furman (2018)). Our results show that measures that limit profit shifting by US multinationals reduce economic growth. If GILTI corporations previously enjoyed an effective

[54]The decline in stock prices is concentrated in firms with more intangible assets, which reinforces the notion that it was easier for these firms to shift profits to Puerto Rico. While firms eventually responded by shifting investment abroad, the effect on stock prices suggests that firms did not have immediate access to tax havens that could serve as substitutes for Puerto Rico.

[55]For instance, income diverted to other tax havens faced the risk of falling under Subpart F, which would prevent it from fully escaping US taxation.

[56]In a sense, Hong and Smart (2010) argue that the benefits of a preferential tax regime can be obtained by allowing multinationals access to tax havens.

[57]Johannesen (2010) also shows that tax havens may be beneficial in a model with imperfect tax competition.

[58]New minimum taxes on multinationals include the base erosion and anti-avoidance tax (BEAT), as well as taxes on global intangible low-tax income (GILTI), and foreign derived intangible income (FDII).

rate lower than 13.125%, our results suggest that this component of the tax reform may actual lead to tax increases and job losses in the US.

Fourth, one interpretation of the new approach toward international taxation in the US is that taxes such as GILTI essentially play the role of preferential tax regimes for intangible corporate income (Keen, 2018). If this is the case, theory then suggests that preferential tax regimes allow governments to raise taxes on relatively immobile capital (Keen, 2001). Contrary to theory, however, this interpretation of the TCJA implies that the tax reform likely increased taxes on mobile corporations and decreased them on immobile firms.

Finally, our results show that unilateral efforts to combat profit shifting can backfire. This should caution policy reactions to news of outrageous profit shifting by multinationals, as poorly designed policies can be harmful to the economy. We view these results as motivation for researchers and practitioners to design policies to limit profit shifting that may not harm the economy. For instance, multilateral approaches such as the base-erosion and profit-shifting (BEPS) project of the OECD (2017), proposals such as the destination-based cashflow tax of Auerbach and Devereux (2015), or apportionment systems championed by Zucman (2015) may be able to combat profit shifting while limiting the unintended consequences we document in this paper.

References

26 U.S.C. §936, "Tax Reform Act of 1976," 1976. Tax Reform Act of 1976, Pub. L. 94-455, 90 stat. 1643, codified as amended at title 26 U.S.C. §936.

_ , "Small Business Job Protection Act of 1996," 1996. Pub. L. 104-188, 110 stat. 1832, codified as amended at title 26 U.S.C. §936.

Acemoglu, Daron and Pascual Restrepo, "Robots and Jobs: Evidence from US Labor Markets," Working Paper 23285, National Bureau of Economic Research March 2017.

Albertus, James F., "The Real Effects of U.S. Tax Arbitrage by Foreign Multinational Firms," SSRN (February 1, 2017). 2017.

Altshuler, Rosanne and Harry Grubert, "Repatriation taxes, repatriation strategies and multinational financial policy," *Journal of Public Economics*, 2003, *87* (1), 73 – 107. Proceedings of the Trans Atlantic Public Economics Seminar on "Taxation of Financial Incomet" 22-24 May, 2000.

Annual Survey of Manufacturers, "Report Series (Volume 1)," November 1997.

Auerbach, Alan and James Hines, "Anticipated Tax Changes and the Timing of Investment," in "Taxes and Capital Formation," National Bureau of Economic Research, Inc, 1987, pp. 85–88.

Auerbach, Alan J., "Tax Reform and Adjustment Costs: The Impact on Investment and Market Value," *International Economic Review*, 1989, *30* (4), 939–962.

_ **and Michael P. Devereux**, "Cash-Flow Taxes in an International Setting," 2015. University of California, Berkeley, December 1, 2015.

Autor, David H. and David Dorn, "The Growth of Low-Skill Service Jobs and the Polarization of the US Labor Market," *American Economic Review*, August 2013, *103* (5), 1553–97.

_ **,** _ **, and Gordon H. Hanson**, "The China Syndrome: Local Labor Market Effects of Import Competition in the United States," *American Economic Review*, October 2013, *103* (6), 2121–68.

_ **,** _ **, and** _ , "The China Shock: Learning from Labor Market Adjustment to Large Changes in Trade," WP 21906, NBER 2016.

Barnatchez, Keith, Leland Crane, and Ryan Decker, "An Assessment of the National Establishment Time Series (NETS) Database," Finance and Economics Discussion Series 2017-110, Board of Governors of the Federal Reserve System (U.S.) November 2017.

Barro, Robert J. and Jason Furman, "The macroeconomic effects of the 2017 tax reform.," *BPEA Conference Draft, Spring*, 2018.

Bartik, Timothy J., "A New Panel Database on Business Incentives for Economic Development Offered by State and Local Governments in the United States," Technical Report, W.E. Upjohn Institute 2017.

BEA, "BEA Depreciation Estimates," Technical Report https://bea.gov/national/pdf/BEA_depreciation_rates.pdf (Accessed: June 28, 2018), Bureau of Economic Analysis 2018.

Becker, Johannes and Nadine Riedel, "Cross-border tax effects on affiliate investment—Evidence from European multinationals," *European Economic Review*, 2012, *56* (3), 436 – 450.

Bernard, Andrew B., J. Bradford Jensen, Stephen J. Redding, and Peter K. Schott, "Global Firms," Working Paper 22727, National Bureau of Economic Research October 2016.

Bond, Eric, "Tax Holidays and Industry Behavior," *The Review of Economics and Statistics*, 1981, *63* (1), 88–95.

Buettner, Thiess, Michael Overesch, Ulrich Schreiber, and Georg Wamser, "The impact of thin-capitalization rules on the capital structure of multinational firms," *Journal of Public Economics*, 2012, *96* (11), 930–938. Fiscal Federalism.

Bureau of Economic Analysis, "State Personal Income and Employment: Methodology," Technical Report, Washington, D.C. 2010.

_ , "National Data: Private Fixed Assets by Industry," Web Site, Washington, D.C. 2017.

_ , "Local Area Personal Income Accounts, CA35," Web Site, Washington, D.C. 2018.

Business Week, "A Hurricane Heads for Puerto Rico," in "Eden (1994)," Vol. 54 1993, p. 52.

Capital IQ, "Compustat Segments (Annual Data)," Available: Standard & Poor's/Compustat 1980-2014. Retrieved from Wharton Research Data Services http://wrds-web.wharton.upenn.edu/wrds/connect/ (Accessed: March 14, 2016).

Census Bureau, "County Business Patterns," Web Site (Accessed: July 11, 2018), https://www.census.gov/programs-surveys/cbp.html 2018.

Chari, Anusha, Ryan Leary, and Toan Phan, "The Costs of (sub)Sovereign Default Risk: Evidence from Puerto Rico," Working Paper 24108, National Bureau of Economic Research December 2017.

Clausing, Kimberly A., "Multinational Firm Tax Avoidance and Tax Policy," *National Tax Journal*, 2009, *62* (4), 703–725.

_ , "The Effect of Profit Shifting on the Corporate Tax Base in the United States and Beyond," *National Tax Journal*, 2016, *69* (4), 905–934.

CRSP Stocks, "The Center for Research in Security Prices Stock / Security Files," Available: The Center for Research in Security Prices 1990-1996. Retrieved from Wharton Research Data Services http://wrds-web.wharton.upenn.edu/wrds/connect/ (Accessed: December 1, 2017).

de Mooij, Ruud A. and Sjef Ederveen, "Corporate tax elasticities: a reader's guide to empirical findings," *Oxford Review of Economic Policy*, winter 2008, *24* (4), 680–697.

de Mooij, Ruud and Li Liu, "At A Cost: the Real Effect of Transfer Pricing Regulations on Multinational Investment," 2018.

Desai, Mihir A. and Dhammika Dharmapala, "Corporate Tax Avoidance and Firm Value," *Review of Economics and Statistics*, August 2009, *91* (3), 537–546.

_ **and James R. Hines**, "Expectations and Expatriations: Tracing the Causes and Consequences of Corporate Inversions," *National Tax Journal*, September 2002, *LV* (3).

_ **, C. Fritz Foley, and James R. Hines**, "The demand for tax haven operations," *Journal of Public Economics*, 2006, *90* (3), 513–531. Special issue published in cooperation with the National Bureau of Economic Research: Proceedings of the Trans-Atlantic Public Economics Seminar on Fiscal Federalism 20–22 May 2004.

_ **,** _ **, and** _ , "Domestic Effects of the Foreign Activities of US Multinationals," *American Economic Journal: Economic Policy*, February 2009, *1* (1), 181–203.

Devereux, Michael P. and Rachel Griffith, "Taxes and the location of production: evidence from a panel of US multinationals," *Journal of Public Economics*, 1998, *68* (3), 335 – 367.

Dharmapala, Dhammika, "What problems and opportunities are created by tax havens?," *Oxford Review of Economic Policy*, 2008, *24* (4), 661–679.

_ , "What Do We Know about Base Erosion and Profit Shifting? A Review of the Empirical Literature," *Fiscal Studies*, 2014, *35* (4), 421–448.

_ **and Nadine Riedel**, "Earnings shocks and tax-motivated income-shifting: Evidence from European multinationals," *Journal of Public Economics*, 2013, *97* (Supplement C), 95 – 107.

Dix-Carneiro, Rafael, "Trade Liberalization and Labor Market Dynamics," *Econometrica*, 2014, *82* (3), 825–885.

Dowd, Tim, Paul Landefeld, and Anne Moore, "Profit shifting of U.S. multinationals," *Journal of Public Economics*, 2017, *148* (Supplement C), 1–13.

Dube, Arindrajit, Ethan Kaplan, and Suresh Naidu, "Coups, Corporations, and Classified Information," *Quarterly Journal of Economics*, 2011, *126* (3), 1375–1409.

Eden, Lorraine, "Puerto Rican Transfers and Section 936," *Tax Notes International*, 1994, *9*, 37–45.

_ , *Taxing Multinationals: Transfer Pricing and Corporate Income Taxation in North America*, University of Toronto Press, 1998.

Egger, Peter H. and Georg Wamser, "The impact of controlled foreign company legislation on real investments abroad. A multi-dimensional regression discontinuity design," *Journal of Public Economics*, 2015, *129* (Supplement C), 77 – 91.

Fajgelbaum, Pablo D., Eduardo Morales, Juan Carlos Suárez Serrato, and Owen M. Zidar, "State Taxes and Spatial Misallocation," Working Paper 21760, National Bureau of Economic Research November 2015.

Fama, Eugene and Kenneth French, "Common Risk Factors in the Returns on Stocks and Bonds," *Journal of Financial Economics*, 1993, *33* (1), 3–56.

Feliciano, Zadia M. and Andrew Green, "US Multinationals in Puerto Rico and the Repeal of Section 936 Tax Exemption for U.S. Corporations," Working Paper 23681, National Bureau of Economic Research August 2017.

Gabaix, Xavier, "The Granular Origins of Aggregate Fluctuations," *Econometrica*, 2011, *79* (3), 733–772.

GAO, "Puerto Rico and the Section 936 Tax Credit," Technical Report GAO/GGD-93-109, Government Accountability Office June 1993.

_ , "Puerto Rico Fiscal Relations with the Federal Government and Economic Trends during the Phaseout of the Possessions Tax Credit," Technical Report GAO-06-541, Government Accountability Office May 2006.

Garrett, Daniel and Juan Carlos Suárez Serrato, "How Elastic is the Demand for Tax Havens? Evidence from the US Possessions Corporations Tax Credit," July 2018. Mimeo.

Gordon, Roger H. and James R. Hines, "International Taxation," in "in," Vol. 4 of *Handbook of Public Economics*, Elsevier, 2002, pp. 1935–1995.

Gravelle, Jane, "Policy Options to Address Profit Shifting: Carrots or Sticks," *Tax Notes*, 2016, *121*, 121–134.

Grubert, Harry and Joel Slemrod, "The Effect of Taxes on Investment and Income Shifting to Puerto Rico," *Review of Economics and Statistics*, 1998, *80* (3), 365–373.

_ **and John Mutti**, "Taxes, Tariffs and Transfer Pricing in Multinational Corporate Decision Making," *The Review of Economics and Statistics*, 1991, *73* (2), 285–293.

Gumpert, Anna, James R. Hines, and Monika Schnitzer, "Multinational Firms and Tax Havens," *The Review of Economics and Statistics*, 2016, *98* (4), 713–727.

Guvenen, Fatih, Jr. Raymond J. Mataloni, Dylan G. Rassier, and Kim J. Ruhl, "Offshore Profit Shifting and Domestic Productivity Measurement," Working Paper 23324, National Bureau of Economic Research April 2017.

Hakobyan, Shushanik and John McLaren, "Looking for Local Labor Market Effects of NAFTA," *The Review of Economics and Statistics*, 2016, *98* (4), 728–741.

Heckemeyer, Jost H. and Michael Overesch, "Multinationals? Profit Response to Tax Differentials: Effect Size and Shifting Channels," 2013.

Hexner, J. Tomas and Glenn P. Jenkins, "Puerto Rico and Section 936: A Costly Dependence," *Tax Notes International*, 1995, *10*, 235–254.

Hines, James R., "Lessons from Behavioral Responses to International Taxation," *National Tax Journal*, 1999, *52* (2), 305–322.

_ **and Eric M. Rice**, "Fiscal Paradise: Foreign Tax Havens and American Business," *The Quarterly Journal of Economics*, 1994, *109* (1), 149–182.

Hong, Qing and Michael Smart, "In Praise of Tax Havens: International Tax Planning and Foreign Direct Investment," *European Economic Review*, 2010, *54* (1), 82–95.

HR3448, "H.R.3448 - Small Business Job Protection Act of 1996," Technical Report https://www.congress.gov/bill/104th-congress/house-bill/3448/ (Accessed: June, 28, 2018) 1996.

Huizinga, Harry and Luc Laeven, "International profit shifting within multinationals: A multi-country perspective," *Journal of Public Economics*, 2008, *92* (5), 1164 – 1182.

IRS, "SOI Tax Stats - Corporation Complete Report," Web Site https://www.irs.gov/statistics/soi-tax-stats-corporation-complete-report (Accessed: December 1, 2017), U.S. Internal Revenue Service 2017.

_ , "SOI Tax Stats - County Data," Web Site https://www.irs.gov/statistics/soi-tax-stats-county-data (Accessed: December 1, 2017), U.S. Internal Revenue Service 2017.

Johannesen, Niels, "Imperfect tax competition for profits, asymmetric equilibrium and beneficial tax havens," *Journal of International Economics*, 2010, *81*, 253–264.

_ **and Tim B.M. Stolper**, "The Deterrence Effect of Whistleblowing – An Event Study of Leaked Customer Information from Banks in Tax Havens," *Max Planck Institute for Tax Law and Public Finance*, 2017.

Joint Committee on Taxation, "An Overview of the Special Tax Rules Related to Puerto Rico and an Ananlysis of the Tax and Economic Policy Implications of Recent Legislative Options," June 23, 2006 2006.

Kang, Karam, "Policy Influence and Private Returns from Lobbying in the Energy Sector," *The Review of Economic Studies*, 2016, *83* (1), 269–305.

Keen, Michael, "Preferential Regimes Can Make Tax Competition Less Harmful," *National Tax Journal*, December 2001, *54* (4), 757–762.

_ , "Spillovers from tax reform in the united states," *IMF Working Paper presented at CESifo Venice Summer Institute*, 2018.

Kovak, Brian K., Lindsay Oldenski, and Nicholas Sly, "The Labor Market Effects of Offshoring by U.S. Multinational Firms: Evidence from Changes in Global Tax Policies," Working Paper 23947, National Bureau of Economic Research October 2017.

Los Angeles Times, "Hazy Forecast for Puerto Rico: Taxes: Clinton's Budget Will Phase Out Most Tax Breaks for U.S. firms. The Commonwealth May Try New Incentives," *Olmos, David R.*, 1993.

MacKinlay, A. Craig, "Event Studies in Economics and Finance," *Journal of Economic Literature*, March 1997, *35* (1), 13–39.

Meer, Jonathan and Jeremy West, "Effects of the Minimum Wage on Employment Dynamics," *Journal of Human Resources*, 2016, *51* (2), 500–522.

Mian, Atif and Amir Sufi, "What Explains the 2007–2009 Drop in Employment?," *Econometrica*, 2014, *82* (6), 2197–2223.

Mintz, Jack and Michael Smart, "Income shifting, investment, and tax competition: theory and evidence from provincial taxation in Canada," *Journal of Public Economics*, 2004, *88*, 1149 – 1168.

Mora, Marie T., Alberto Dávila, and Havidán Rodríguez, *Population, Migration, and Socioeconomic Outcomes among Island and Mainland Puerto Ricans: La Crisis Boricua*, Lexington Books, 2017.

Moretti, Enrico, "Local Multipliers," *American Economic Review*, May 2010, *100* (2), 373–77.

New York Times, "Sara Lee to Cut 1,200 Jobs in Puerto Rico," 2003.

OECD, "Base Erosion and Profit Shifting," Technical Report http://www.oecd.org/tax/beps/ 2017.

_ , "Tax Database," 2018.

Ohrn, Eric, "The Effect of Tax Incentives on U.S. Manufacturing: Evidence from State Accelerated Depreciation Policies," September 2017. Mimeo.

_ , "The Effect of Corporate Taxation on Investment and Financial Policy: Evidence from the DPAD," *American Economic Journal: Economic Policy*, Forthcoming.

OpenSecrets.org, "Bulk Lobbying Data," Web Site http://www.opensecrets.org/ (Accessed: June, 28, 2018) 2018.

Overesch, Michael, "The Effects of Multinationals' Profit Shifting Activities on Real Investments," *National Tax Journal*, 2009, *62* (1), 5–23.

QCEW, "Bureau of Labor Statistics: Quarterly Census of Employment and Wages," Web Site (Accessed: December 1, 2017), https://www.bls.gov/cew/ 2017.

Rao, Nirupama, "Corporate Inversions and Economic Performance," *National Tax Journal*, Forthcoming, *68* (4), 1073–1097.

Ruggles, Steven et al., "Integrated Public Use Microdata Series: Version 5.0," [Machine-readable database], Minneapolis: University of Minnesota 2010.

Slemrod, Joel and John D. Wilson, "Tax competition with parasitic tax havens," *Journal of Public Economics*, 2009, *93* (11), 1261–1270.

Suárez Serrato, Juan Carlos and Owen Zidar, "Who Benefits from State Corporate Tax Cuts? A Local Labor Markets Approach with Heterogeneous Firms," *American Economic Review*, 2016.

_ **and** _ , "The Structure of State Corporate Taxation and its Impact on State Tax Revenues and Economic Activity," Working Paper, Duke University May 2017.

_ **and Philippe Wingender**, "Estimating the Incidence of Government Spending," December 2014. Mimeo.

_ **and** _ , "Estimating Local Fiscal Multipliers," Working Paper 22425, National Bureau of Economic Research July 2016.

Tax Foundation, "What Does Research on Profit Shifting Tell Us?," Web Site https://taxfoundation.org/what-does-research-profit-shifting-tell-us/ (Accessed: July, 5, 2018) 2016.

Tørsløv, Thomas, Ludvig Wier, and Gabriel Zucman, "The Missing Profits of Nations," Working Paper 2018.

Valletta, R.G. and R.B. Freeman, "The NBER Public Sector Collective Bargaining Law Data Set," in Richard B. Freeman and Casey Ichniowski, eds., *When Public Employees Unionize*, Chicago: NBER and University of Chicago Press, 1988, chapter Appendix B.

Walls & Associates, "NETS: National Establishment Time-Series Database," 2012.

Wilson, Daniel J, "Beggar Thy Neighbor? The In-State, Out-of-State, and Aggregate Effects of R&D Tax Credits," *Review of Economics and Statistics*, 2016/02/22 2009, *91* (2), 431–436.

Wright, Thomas and Gabriel Zucman, "The Exorbitant Tax Privilege," *working paper*, 2018.

Yagan, Danny, "Capital Tax Reform and the Real Economy: The Effects of the 2003 Dividend Tax Cut," *American Economic Review*, December 2015, *105* (12), 3531–63.

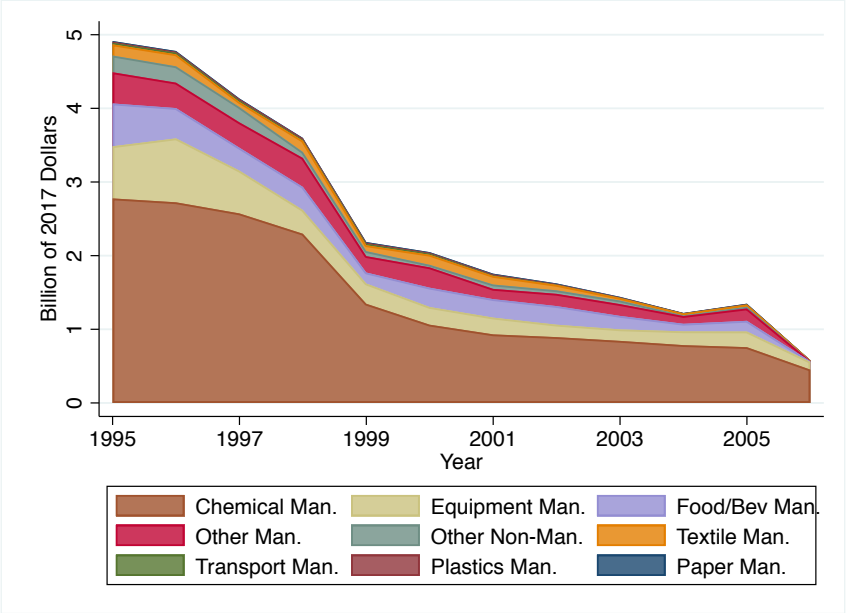
_ , "Employment Hysteresis from the Great Recession," *Journal of Political Economy, forthcoming.*, 2017.

Zucman, Gabriel, *The Hidden Wealth of Nations*, The University of Chicago Press, 2015.

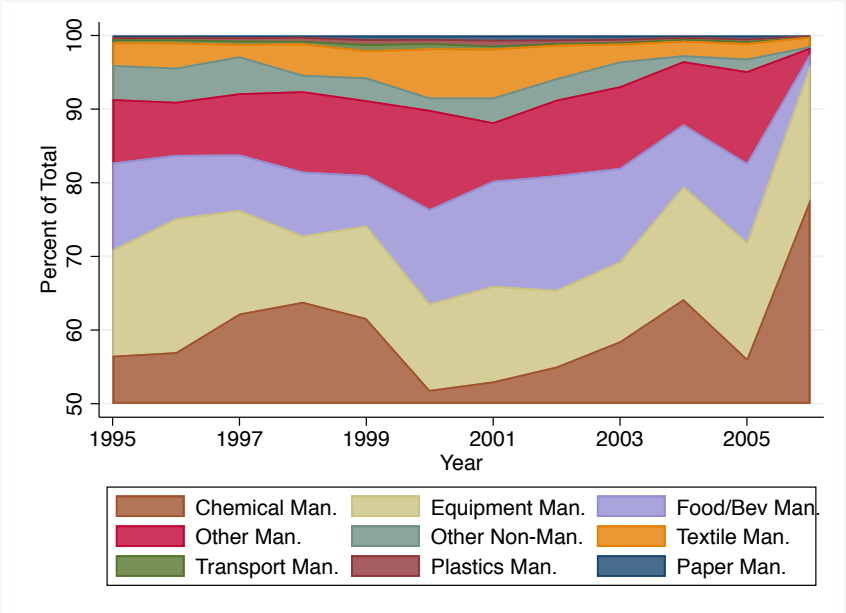
Zwick, Eric and James Mahon, "Tax Policy and Heterogeneous Investment Behavior," *American Economic Review*, January 2017, *107* (1), 217–48.

Figure 1: Fiscal Cost of §936: US Possessions Corporations Tax Credit

A. Total Tax Credits from §936 By Industry



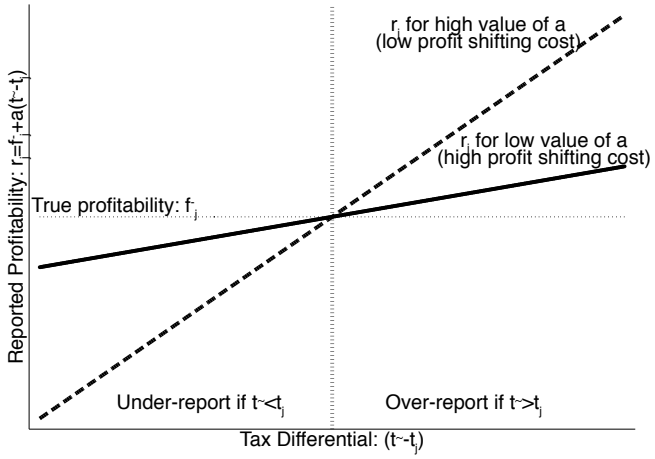
B. Share of Tax Credits from §936 By Industry



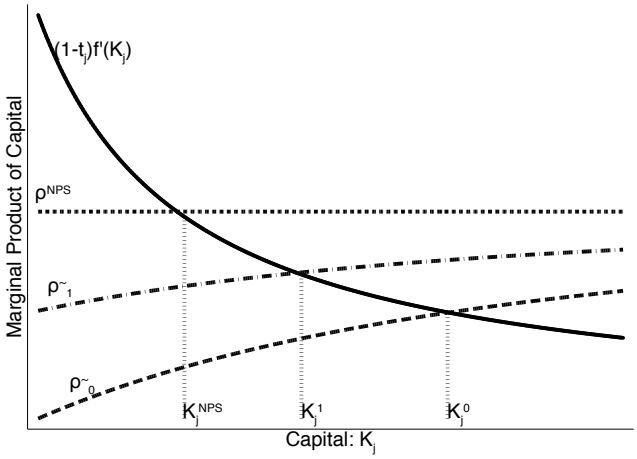
Notes: This figure displays the fiscal cost of §936 to the US Treasury. Panel A tabulates claims of US Corporations Possessions tax credits from 1995 to 2006 by industry using data from the IRS (2017a) "Statistics of Income" series. This graph shows the largest drops in the value of these credits happen after 1996 and 1998, which corresponds to statutory limits on the tax credits. Panel B shows the composition of these credits by industry. In 1995, about 55% of these credits are claimed by corporations in the chemical manufacturing industry. Firms manufacturing equipment, food, beverage, and tobacco claimed about 26% of the credits, while other industries claimed the remaining 17%. See Section 2 for more details.

Figure 2: Effects of Profit Shifting on Investment and Employment

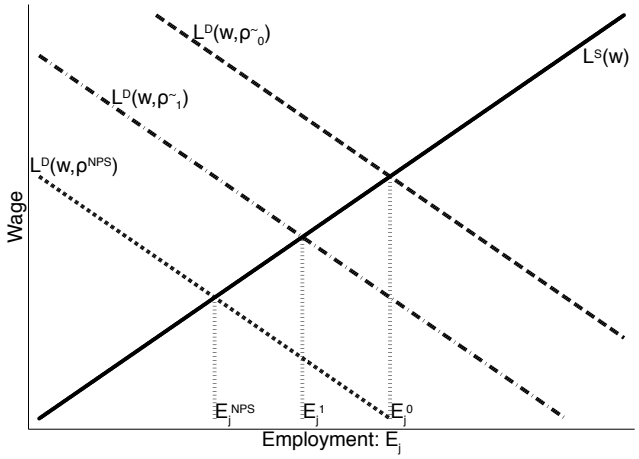
A. Optimal Profit Shifting Strategy



B. Profit Shifting and Domestic Investment

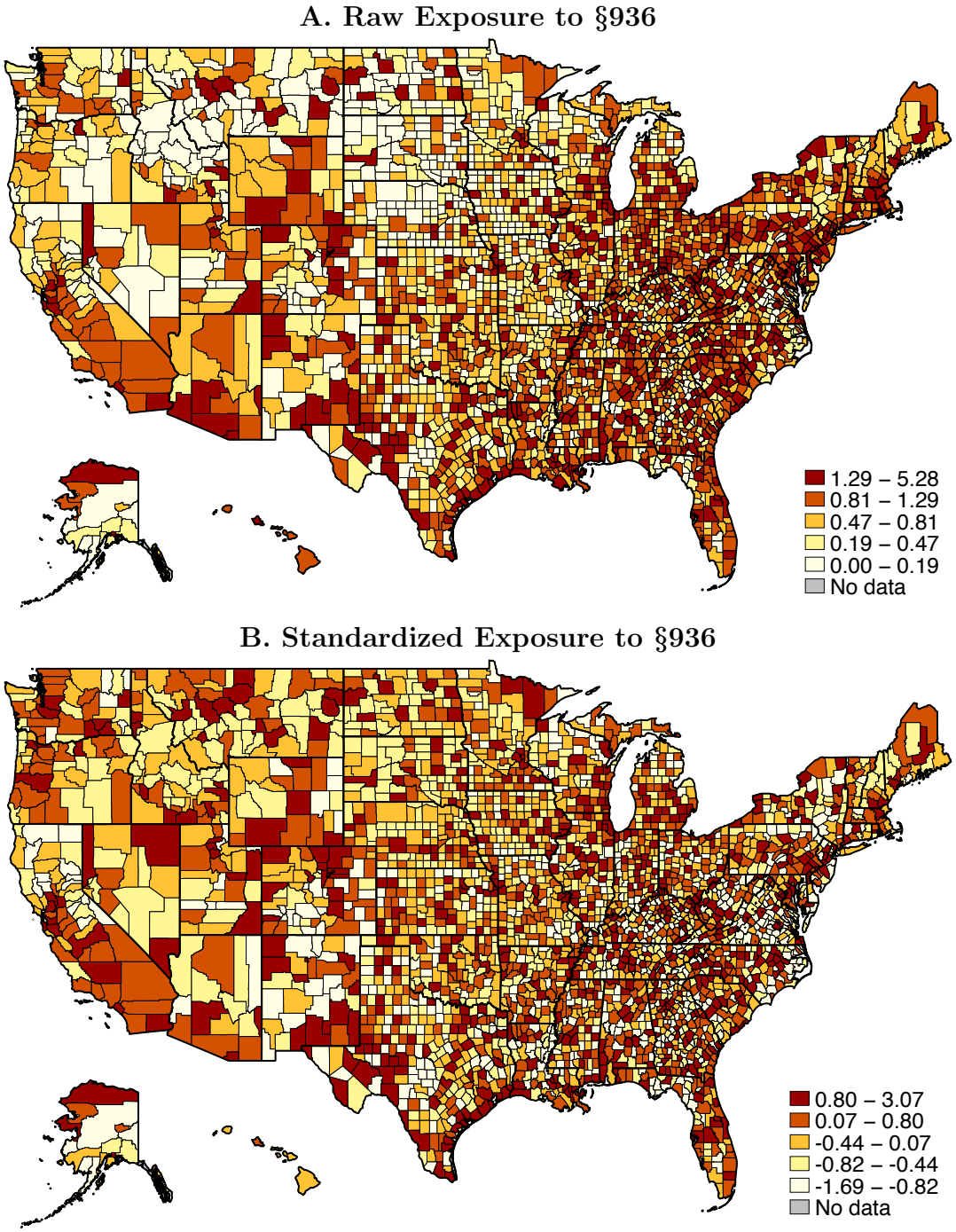


C. Profit Shifting and Domestic Employment



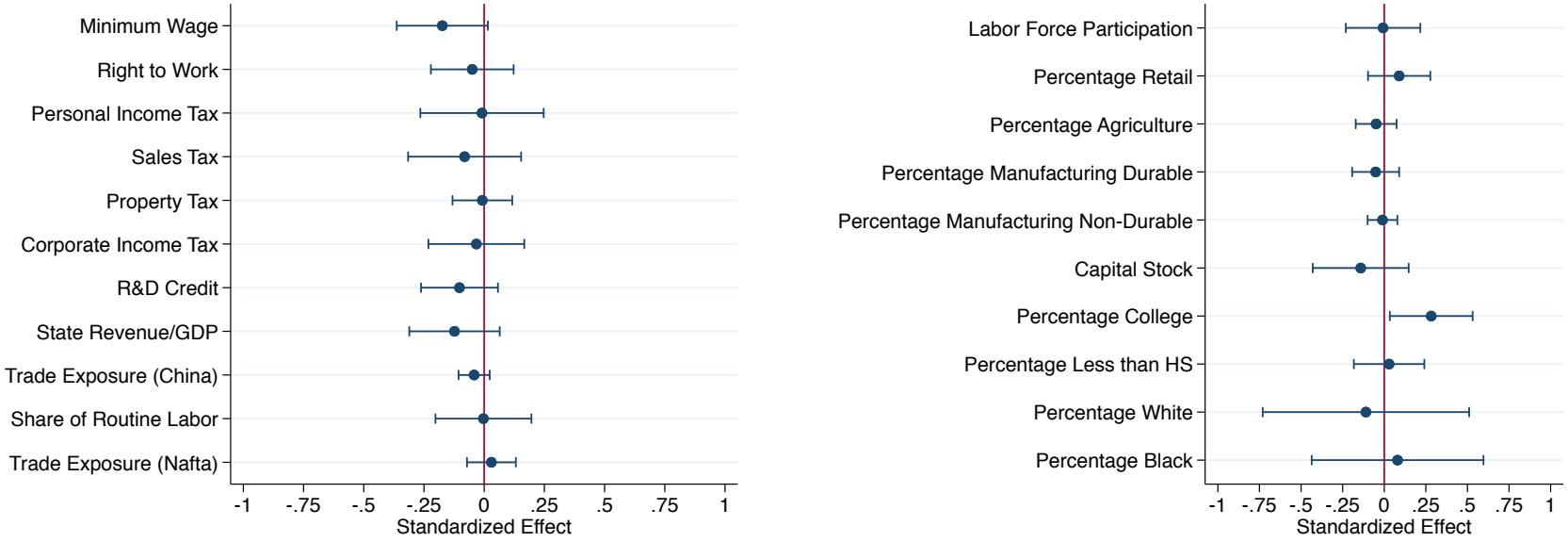
Notes: This figure displays the predictions of the model in Section 3. Panel A shows that reported profitabilities r_j depend on true profitability $\bar{f}_j$, the tax differential $(\tilde{t}-t_j)$, and the parameter governing the cost of profit shifting, a. Firms over-report profits in countries with low taxes $(\tilde{t} > t_j)$ and under-report in countries with high taxes $(\tilde{t} < t_j)$. The degree of over-reporting depends on the cost of profit shifting. If a is low (high cost of misreporting), the relationship between r_j and the tax differential is relatively flat (solid line). If a is large (low cost of misreporting), the slope is steeper (dashed line). Panel B shows the implications of profit shifting for domestic investment. Without profit shifting, the firm sets capital so that the after-tax marginal product of capital equals the cost of equity ρ^{NPS} (dotted line). When profit shifting is allowed, the income-shifting adjusted user cost of capital $\tilde{\rho}_0$ (dashed line) is below ρ^{NPS} and is increasing in K_j. This results in a higher level of investment $K_j^0 > K_j^{NPS}$. The dot-dashed line labeled $\tilde{\rho}_1$ shows the effect of a tax increase in a tax haven on the user cost in a high tax country. Reducing the tax gains from the tax haven results in lower domestic investment $K_j^0 > K_j^1 > K_j^{NPS}$. Panel C shows the effects of these tax changes on domestic employment assuming that capital and labor are complements in country j. This graph shows that the demand for labor depends on the cost of capital $L^D(w, \rho)$. Labor demand shifts out when profit shifting is allowed $L^D(w, \tilde{\rho}_1) > L^D(w, \rho^{NPS})$, and shifts in when there is a tax increase in the low-tax country, $L^D(w, \tilde{\rho}_1) > L^D(w, \tilde{\rho}_0)$. Therefore, there is more domestic employment when tax havens reduce the income shifted adjusted cost of capital: $E_j^{NPS} < E_j^1 < E_j^0$.

Figure 3: Distribution of Exposure to §936 at the County-Level in 1995



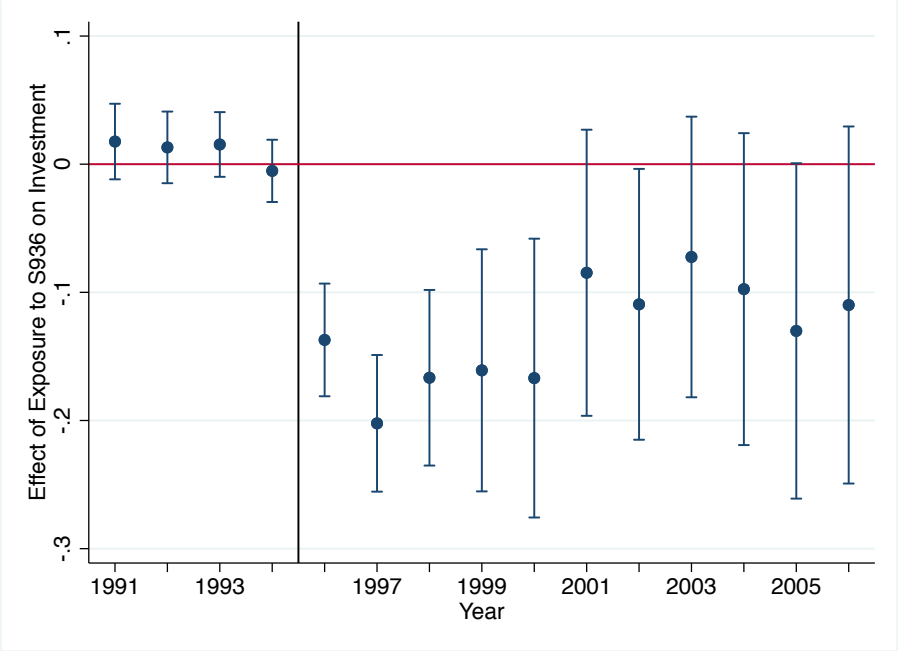
Notes: Author's calculations using data from NETS. This figure plots the distribution of the percent of establishments in each county that are part of firms with exposure to §936. Panel A shows that the fraction of labor market exposure varies from 0% to 5%. Panel B shows a measure of the exposure that is standardized within states and shows that there is substantial variation across counties within any given state. See Section 4 and Appendix B for more information about the data and measurement.

Figure 4: Exposure to §936, Demographic Characteristics, and Economic Polices



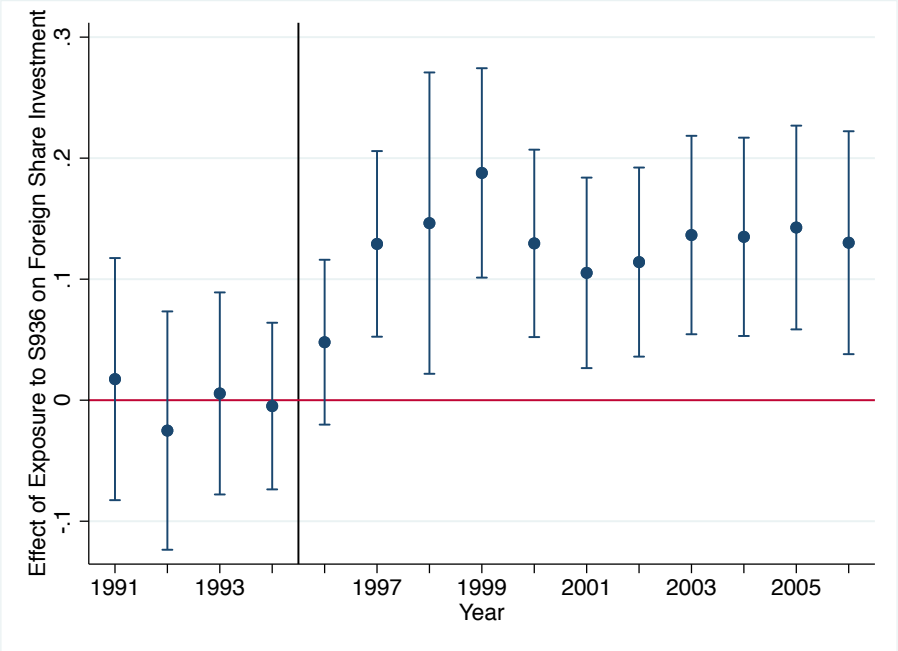
Notes: This graph shows the correlation of the Exposure to §936 at the county level and various policies, economic indicators, and other shocks. Blue lines span a 95% confidence interval. Author's calculations using data from NETS and various other sources. See Valletta and Freeman (1988); Meer and West (2016) for data on Minimum Wage and Right to Work laws, Suárez Serrato and Zidar (2016) for data on state taxes and tax revenues, Wilson (2009) for data on R&D tax credits, Autor et al. (2016) and Hakobyan and McLaren (2016) for data on exposure to trade, Autor and Dorn (2013) for data on share of routine labor, demographic and industry employment data are from the US Census, and data on capital stocks are from the BEA. See Appendix B for more detail on data sources.

Figure 5: Repealing §936 Reduced Investment-to-Capital Ratios of Exposed Firms



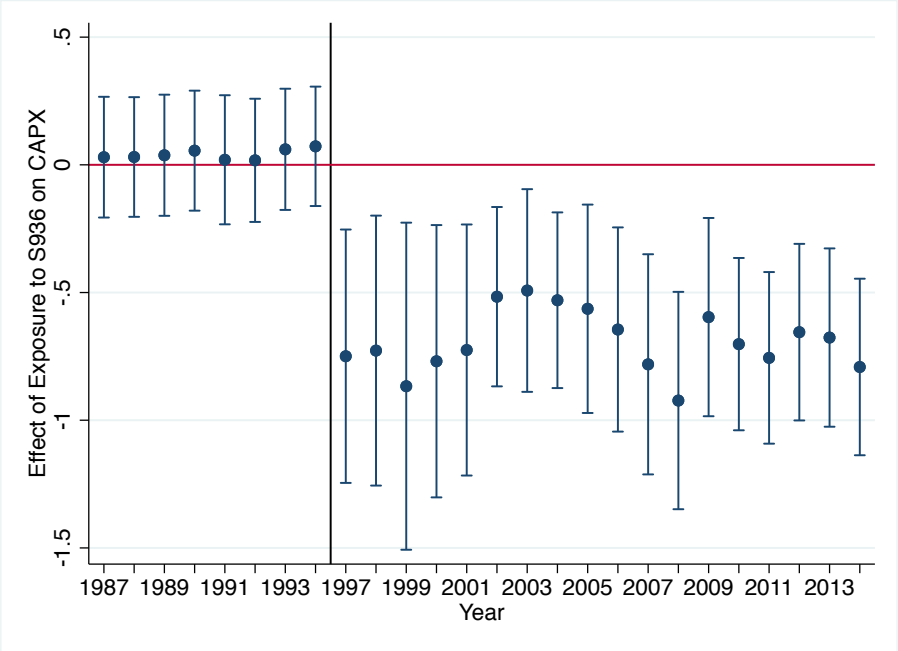
Notes: Author's calculations using data from COMPUSTAT. This figure shows estimates of Equation 2 where the dependent variable is investment divided by average capital in 1990 to 1995 and exposure to §936 is an indicator at the firm level. Investment is defined as capital expenditures and capital is defined as plants property and equipment. Blue lines span a 95% confidence interval. Consistent with the hypothesis that multinationals decreased investment, we see that exposed firms saw a decrease in investment following the repeal of §936 relative to non-exposed firms. Point estimates and additional specifications are shown in Table 2. Standard errors are clustered at the firm level.

Figure 6: Repealing §936 Increased the Foreign Share of Investment of Exposed Firms



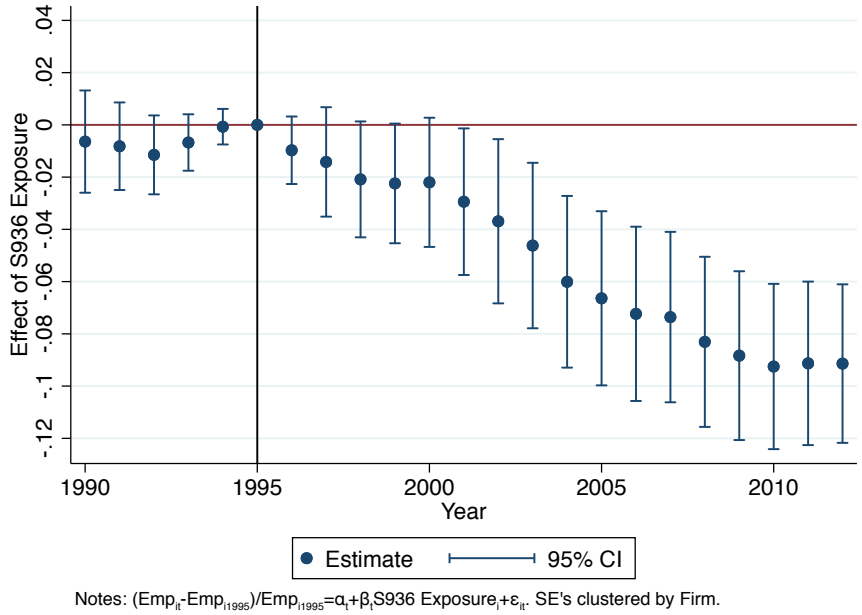
Notes: Author's calculations using data from COMPUSTAT Historical Segments. This figure shows estimates of Equation 3 where the dependent variable is the share of investment based in foreign affiliates and exposure to §936 is an indicator at the firm level. Blue lines span a 95% confidence interval. Consistent with the hypothesis that multinationals shifted investment abroad, we see that exposed firms saw an increase in the fraction of foreign investment following the repeal of §936. Coefficient estimates and additional specifications are shown in Table 3. See Section 5 for more discussion. Standard errors are clustered at the firm level.

Figure 7: Repealing §936 Reduced CAPX Investment in Exposed Industries



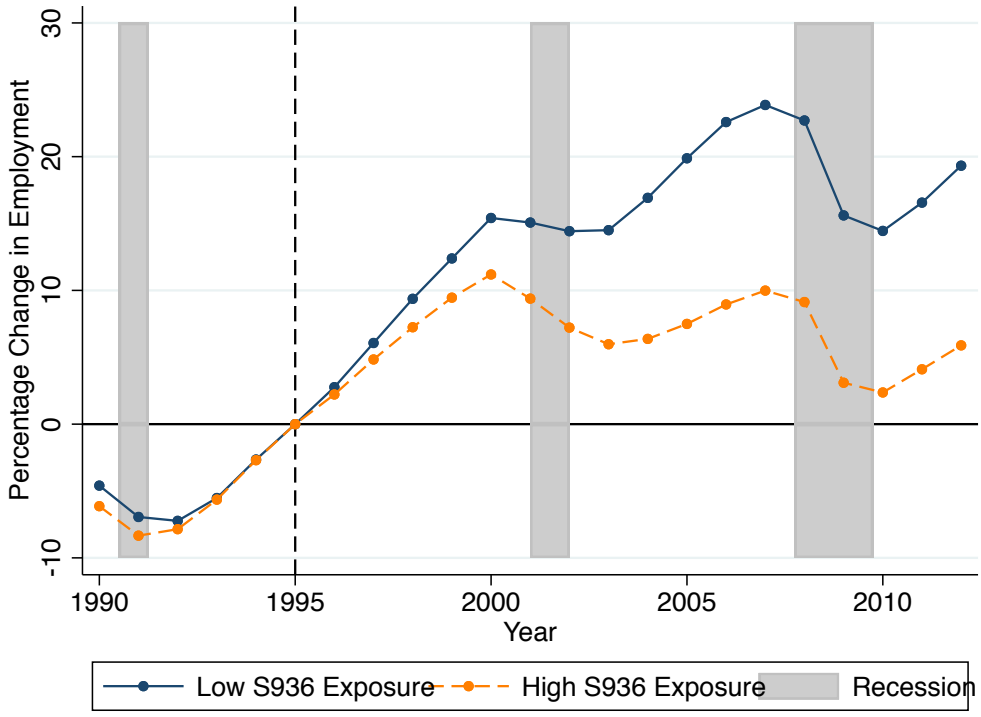
Notes: Author's calculations using data from NETS and the Annual Survey of Manufactures. This figure plots estimates of Equation 4 where exposure to §936 is the proportion of establishments in each state-industry divided by the interquartile range. Blue lines span a 95% confidence interval. Following the repeal of §936, we see a persistent decline in the flow of investment in state-industries with more exposure to §936. Coefficient estimates and additional specifications are shown in Table 4. See Section 5 for more information. Standard errors are clustered at the state level.

Figure 8: Firm-Level Employment Effects of Repealing of §936



Notes: Author's calculations using data from NETS. This figure shows the decline in employment at §936 exposed firms relative to similar control firms without exposure to §936, where exposure to §936 is an indicator at the firm level. See Appendix B.3 for a description of the procedure used to identify the comparison firms. Point estimates are displayed in Table A.6. See Section 5 for additional discussion. Standard errors are clustered at the firm level.

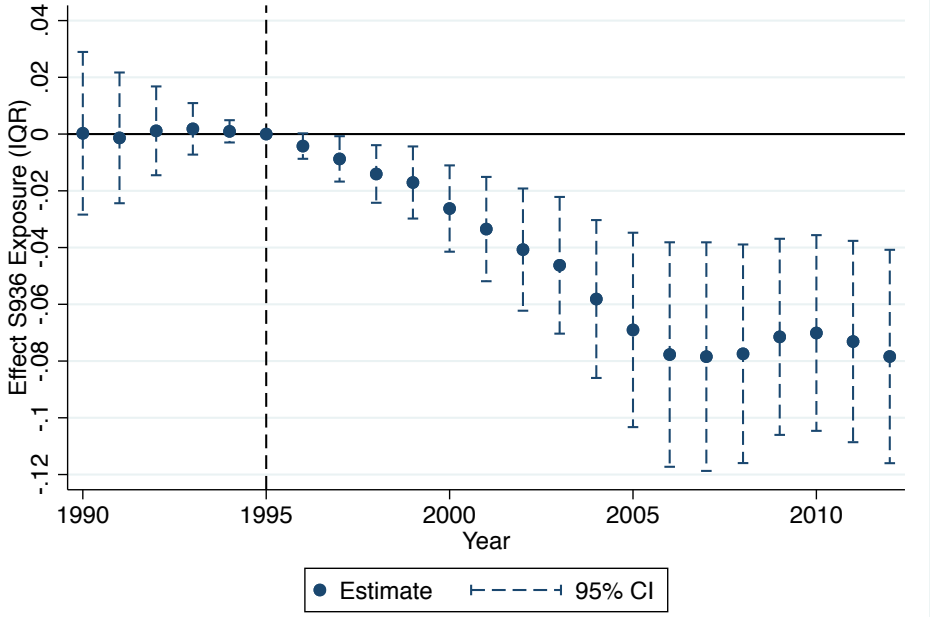
Figure 9: Employment Effects of Repealing of §936 by Tercile of Exposure



Notes: Author's calculations using data from NETS and QCEW. This figure plots average employment growth for counties in the top tercile of §936 exposure (High S936 Exposure) relative to the employment growth in counties in the bottom tercile (Low S936 Exposure). This graph shows that counties with a greater exposure to §936 experienced a relative decrease in employment growth during the 10 years that correspond to the phase-out of §936. For a parametric analysis of the effect of exposure to §936 on employment growth, see Figure 10. See Appendix B for a discussion of the data and Section 6 for more discussion. Average employment growth is weighted according to county-by-industry employment in 1995.

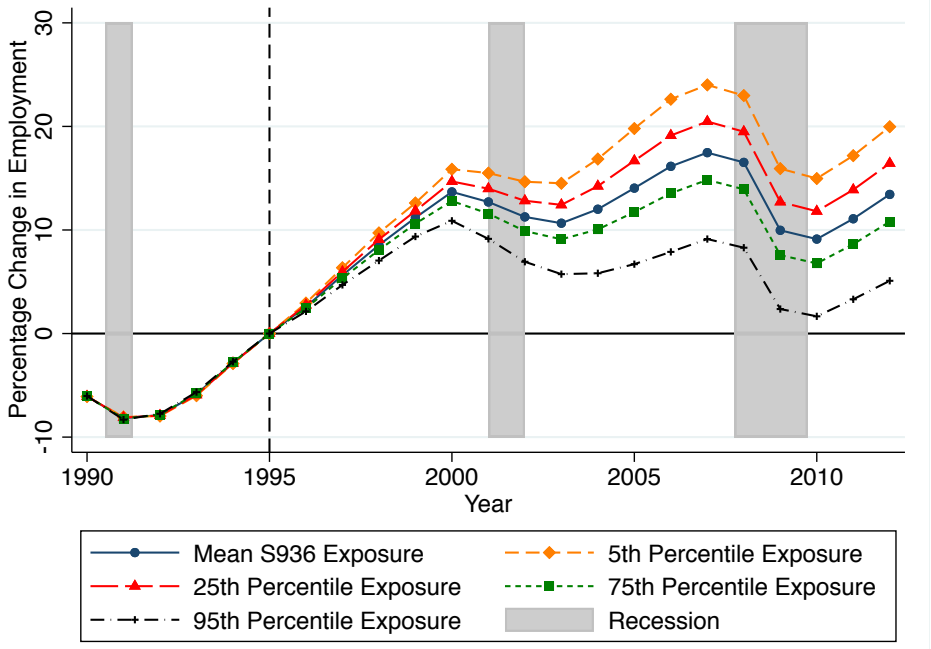
Figure 10: Effects of Exposure to §936 on Employment Growth

A. Estimated Coefficients for §936 Exposure (IQR Adjusted)



Notes: $(Emp_{ict}-Emp_{ic1995})/Emp_{ic1995}=\alpha_c+\gamma_{it}+\beta_t$S936 Exposure$_c+\varepsilon_{ict}$. SE's clustered by State and Industry.

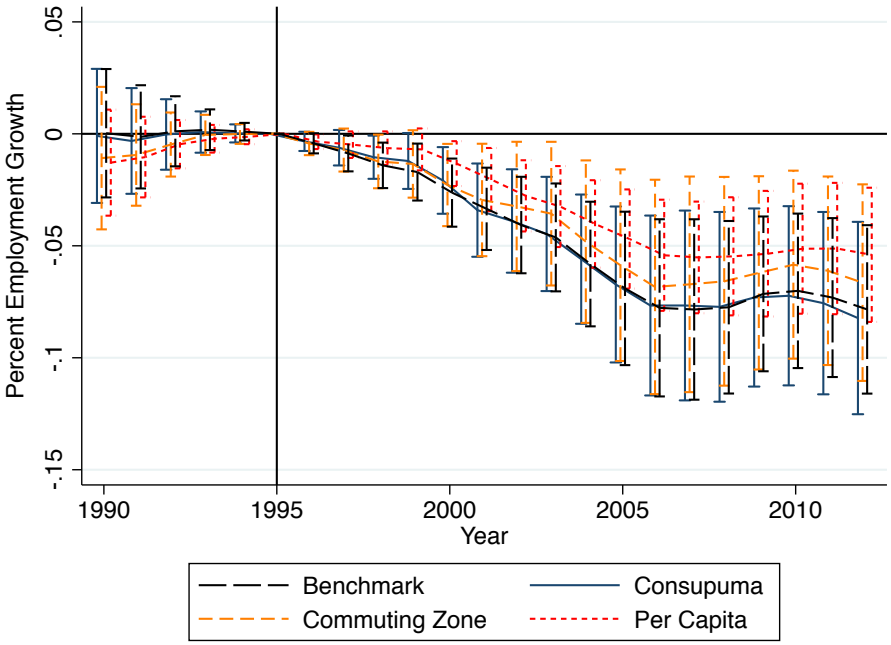
B. Implied Employment Growth at Selected Quantiles



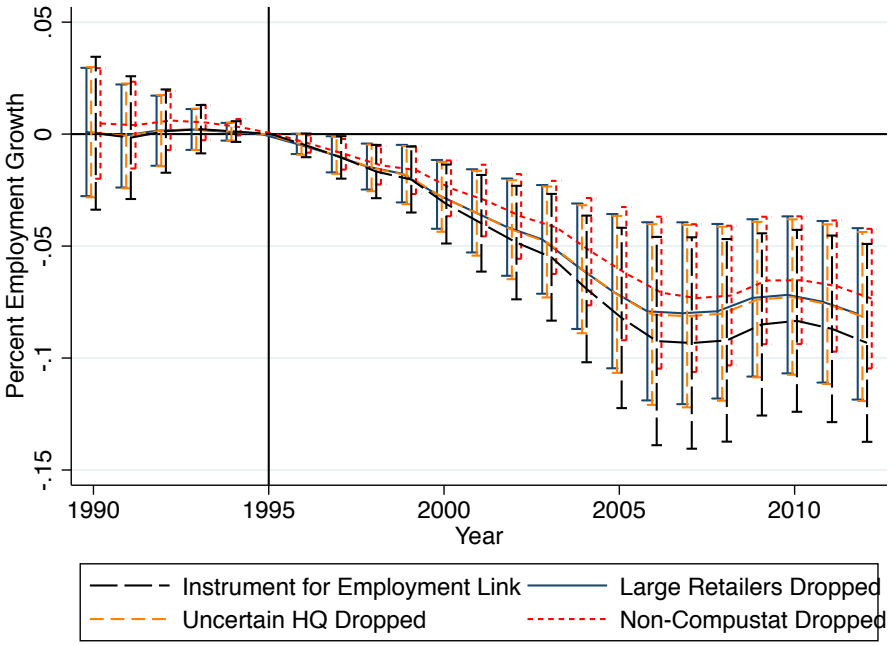
Notes: Author's calculations using data from NETS and QCEW. This figure shows estimates of β^t for $t = 1990, \cdots, 2012$ in Equation 6. Exposure is measured as the percent of establishments exposed to §936 divided by the interquartile range. This graph shows that, prior to the repeal of §936, exposure to §936 is not predictive of employment growth, which is consistent with the parallel trends assumption. The graph shows that counties with greater exposure to §936 experienced a relative decrease in employment growth during the 10 years that correspond to the phase-out of §936. For 2006, Panel A shows that increasing the exposure to §936 from the 25th percentile to the 75th-percentile decrease employment growth by 7.2%. Moreover, the decline in employment growth stabilizes after the full phase-out of §936 in 2006. See Appendix B for a discussion of the data and Section 6 for a discussion of the results. Coefficients are displayed in Table 5. Standard errors are clustered at the state and industry levels. Observations at the industry-county level are weighted by employment in 1995.

Figure 11: Effects of Exposure to §936 on Employment Growth: Alternative Specifications

A. Robustness to Geography and Employment-to-Population



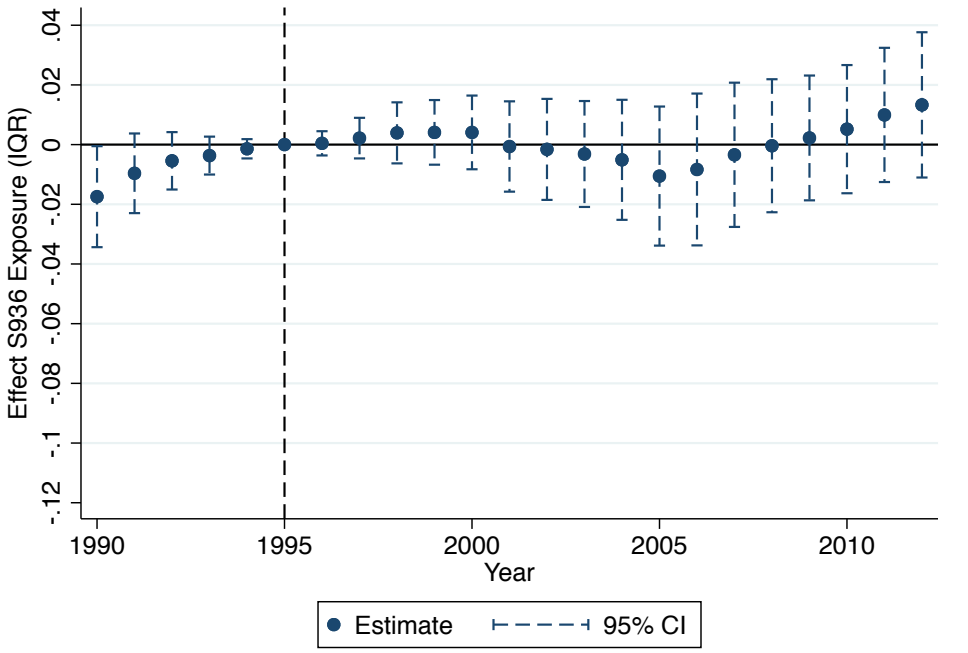
B. Robustness of Exposure Measure



Notes: Author's calculations using data from NETS and QCEW. This figure shows estimates of β^t for $t = 1990, \cdots, 2012$ from Equation 6. The top panel shows results from regressions using different measurement for the geography of interest and for population adjusted employment growth. Coefficient estimates and additional specifications are displayed in Tables 5 and 6. The lower panel shows alternative specifications to the sample of firms which are considered as potential links. Exposure is calculated from the NETS in terms of both employment and establishments and the establishments exposure is used to instrument for the employment exposure in the first specification. The second specification in the lower panel manually excludes large retailers from links, the third specification excludes those firms for which NETS does not report a headqaurters, and the fourth excludes firms that are not part of the Compustat Database from the links. Coefficient estimates for Panel B are displayed in Tables A.9, A.10, A.11, and A.12, respectively. Standard errors are clustered at the state and industry levels. Observations at the industry-county level are weighted by employment in 1995.

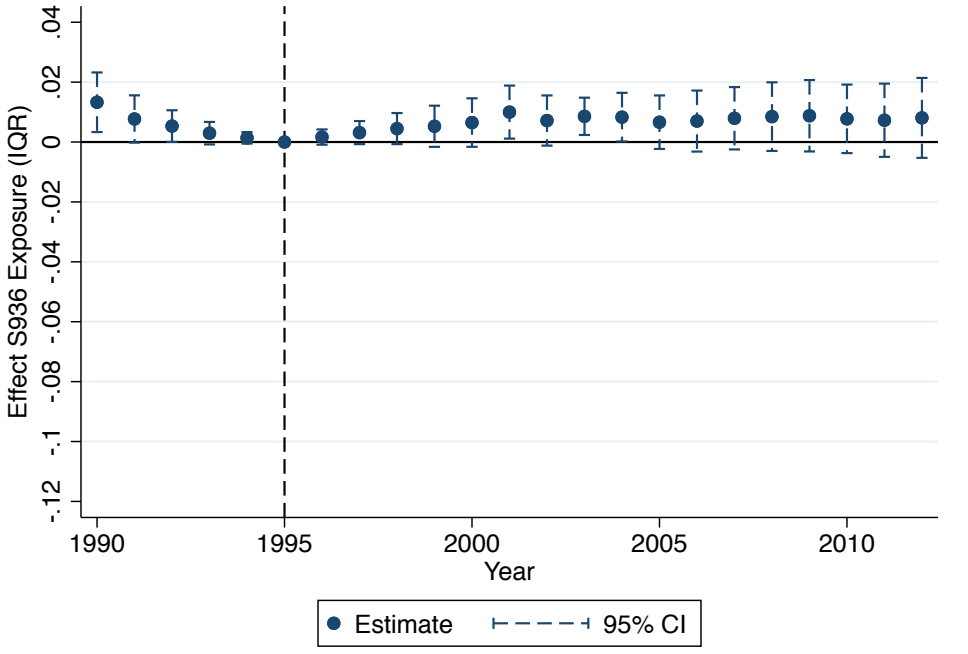
Figure 12: Effects of Exposure to §936 on Employment Growth: Placebo Tests

A. Placebo Test for Matched, Non-exposed Firms



Notes: $(Emp_{ict}-Emp_{ic1995})/Emp_{ic1995}=\alpha_c+\gamma_{it}+\beta_t S936\ Exposure_c+\varepsilon_{ict}$. SE's clustered by State and Indust

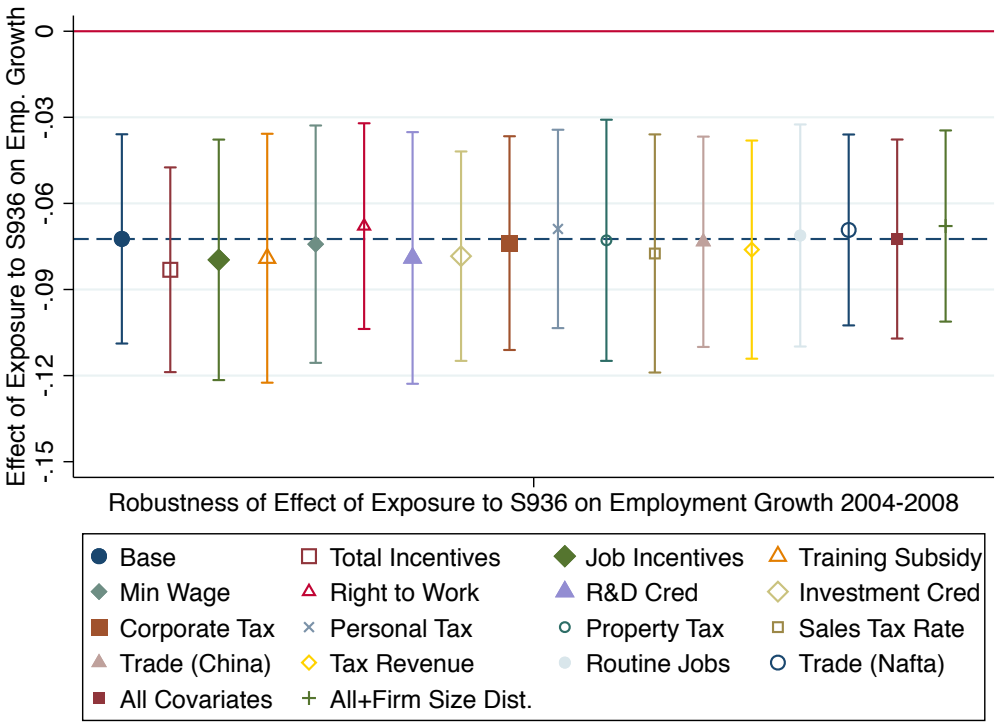
B. Placebo Test for Exposure to Pharmaceuticals



Notes: $(Emp_{ict}-Emp_{ic1995})/Emp_{ic1995}=\alpha_c+\gamma_{it}+\beta_t S936\ Exposure_c+\varepsilon_{ict}$. SE's clustered by State and Indust

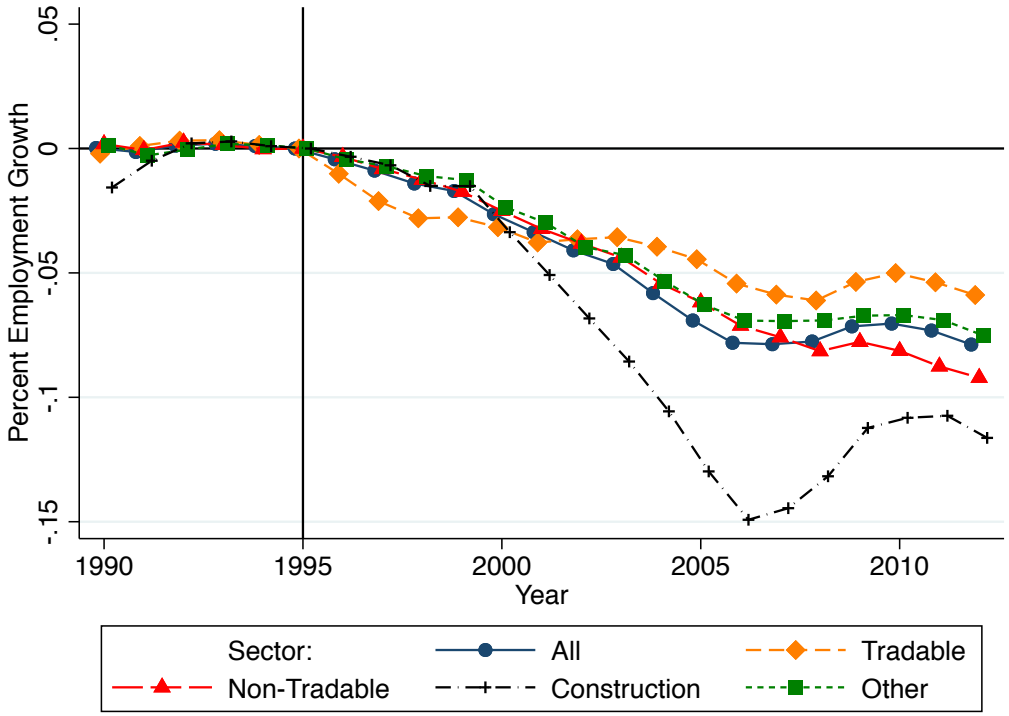
Notes: Author's calculations using data from NETS and QCEW. This figure shows estimates of β^t for $t = 1990, \cdots, 2012$ in Equation 6 using other measures of "exposure" that could be correlated with exposure to §936. The top panel shows results from regressions using fake §936 exposure from a sample of unlinked firms that are similar to linked firms. See Appendix B.3 for a description of the procedure used to identify the set of counterfactual firms for the Panel A shock. The lower panel replaces the exposure to §936 with exposure to the pharmaceuticals industry, NAICS 3254. See Appendix B for a discussion of the data and Section 6 for a discussion of the results. Both of these placebo exercises fail to find any evidence of a negative effect on local employment growth. Coefficient estimates and additional specifications are displayed in Tables A.13 and A.14, respectively. Standard errors are clustered at the state and industry levels. Observations at the industry-county level are weighted by employment in 1995.

Figure 13: Effects of Exposure to §936 on Employment Growth: Robustness



Notes: This figure shows the robustness of the coefficients shown in Table 7 to controlling for various county and state level characteristics. The coefficients represent the effect of increasing §936 exposure from the 25th to 75th percentile. In 2004-2008 the average change in employment growth due to an IQR increase in §936 exposure is -7.2%. Controlling for other state and county characteristics yields estimates between -6.8% and -8.3%. The coefficients and standard errors are displayed in Tables A.15 and A.16. See Appendix B for a discussion of the data and Section 6 for a discussion of the results. Standard errors are clustered at the state and industry levels. Observations at the industry-county level are weighted by employment in 1995.

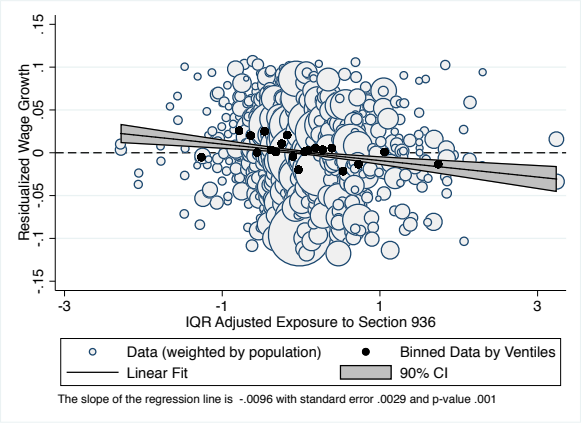
Figure 14: Heterogeneous Effects of Exposure to §936 on Employment Growth by Sector



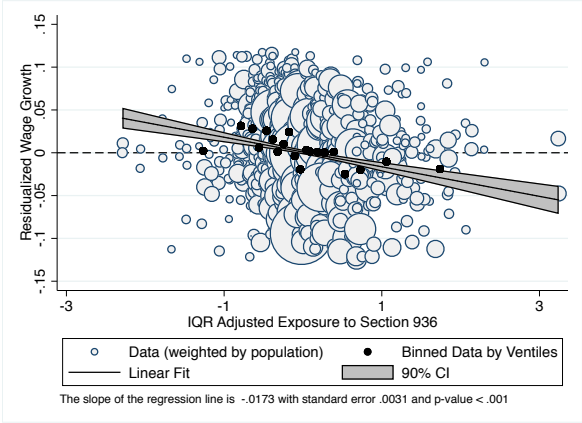
Notes: Author's calculations using data from NETS and QCEW and sector definitions from Mian and Sufi (2014). This figure shows the average effect and heterogeneous effects by sector. Estimates and standard errors are displayed in Table A.17 and Figure A.10. This figure highlights that the tradable sector sees a decline in employment growth before any of the other sectors. See Appendix B for a discussion of the data and Section 6 for a discussion of the results. Standard errors are clustered at the state and industry levels. Observations at the industry-county level are weighted by employment in 1995.

Figure 15: Effects of Exposure to §936 on Wages and Rental Costs

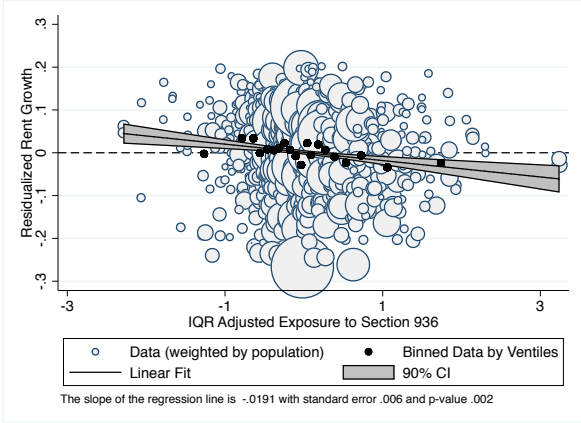
A. Wage Growth After 1990



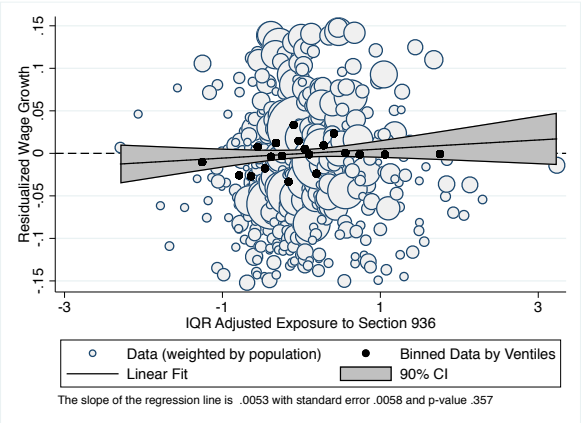
B. Low Skill Wage Growth After 1990



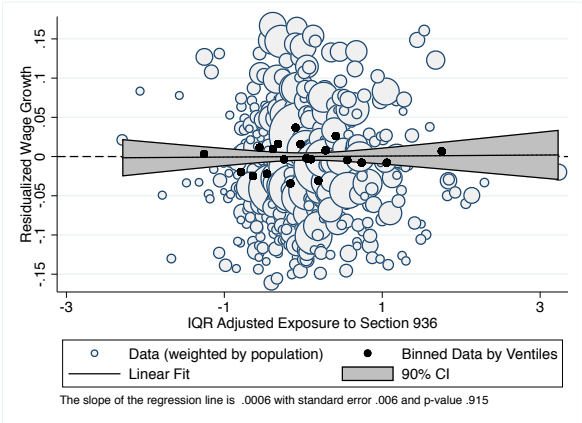
C. Rent Growth After 1990



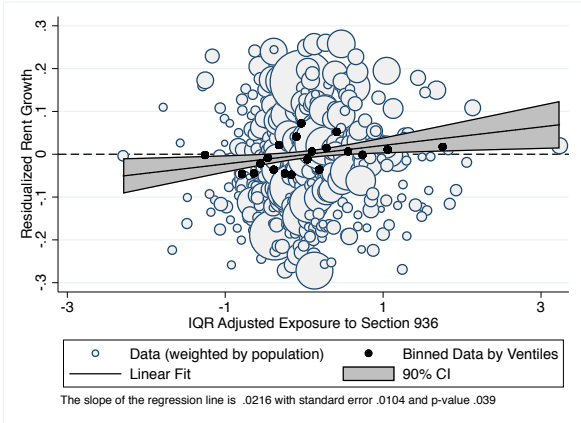
D. Wage Growth Before 1990



E. Low Skill Wage Growth Before 1990



F. Rent Growth Before 1990



Notes: Author's calculations using data from NETS and Census. Exposure to §936 is residualized. This figure shows that exposure to §936 was not correlated with wage and rent growth before the repeal of §936. See Appendix B for a discussion of the data and Section 6 for a discussion of the results.

Table 1: US and Puerto Rico Employment at Large NETS Companies in 1990

Company	US	PR	Industry
BAXTER INTERNATIONAL INC	44,680	5,918	Wholesale and Retail
PXC & M HOLDINGS INC	2,331	5,437	Wholesale and Retail
UNI GROUP INC	204	4,252	Food Mfg
GENERAL ELECTRIC COMPANY	263,673	4,017	Electrical Equipment
DIGITAL EQUIPMENT CORPORATION	52,516	3,686	Other Mfg
SARA LEE CORPORATION	38,011	3,287	Food Mfg
ABBOTT LABORATORIES	25,948	3,144	Chemicals
JOHNSON & JOHNSON	21,619	2,724	Other Mfg
WESTINGHOUSE (LATER CBS)	90,863	2,687	Other Non-Mfg
WYETH LLC	24,208	2,661	Chemicals
FLUOR CORPORATION	27,304	2,500	Services
BRISTOL-MYERS SQUIBB COMPANY	25,698	2,201	Chemicals
MERCK & CO INC	14,303	1,595	Chemicals
H J HEINZ COMPANY	15,500	1,500	Food Mfg
Sub Total	646,858	45,609	
Total	8,786,723	139,595	

Notes: This table lists US companies with employment in Puerto Rico greater than 1,500 according to the NETS (Walls & Associates, 2012). The first column shows the company name, the second column shows US employment in 1990, the third column shows Puerto Rico employment in 1990, and the last column includes a industry category derived from NAICS classification. The sub total row is the sum of the 14 companies displayed here. The total row includes all 682 firms with establishments in Puerto Rico.

Table 2: Effects of Repealing §936 on the Investment-to-Capital Ratio of Exposed Firms

Investment-to-Capital Ratio: $\frac{I}{K_1990-1995}$	(1)	(2)	(3)	(4)	(5)
Exposure to Section 936 X Post	-0.116	-0.125	-0.130	-0.218	-0.226
	(0.053)	(0.046)	(0.046)	(0.046)	(0.046)
	0.029	0.007	0.005	0.000	0.000
Observations	79393	79393	76868	52896	51503
Year Fixed Effects	Y	Y	Y	Y	Y
Firm Fixed Effects		Y	Y	Y	Y
S936 Exposed Industry			Y		Y
Major Sectors Only				Y	Y

Notes: Author's calculations using data from COMPUSTAT. This table shows estimates of Equation 2 where the dependent variable is the change in investment divided by average capital in 1990-1995 winsorized at the 5% level. The estimates in this table correspond to a pooled version of the regression displayed in Figure 5 where Exposure to Section 936 X Post is an indicator at the firm level for exposed firms interacted with an indicator for years after 1995. The coefficient in column (5) means that firms exposed to §936 decrease investment by 23% after 1995, which implies a semi-elasticity of 2.2. Additional results displayed are in Tables A.4 and A.5 to show robustness. See Section 5 for discussion and Appendix B for more information about the data and variables. Standard errors clustered at the firm level are shown in parentheses with p-values below.

Table 3: Effects of Repealing §936 on the Foreign Share of Investment of Exposed Firms

Change in Foreign Share of Investment					
	(1)	(2)	(3)	(4)	(5)
Exposure to Section 936 X Post	0.127	0.133	0.126	0.180	0.175
	(0.045)	(0.045)	(0.045)	(0.048)	(0.049)
	0.005	0.003	0.005	0.000	0.000
Observations	15697	15690	15281	9520	9278
Year Fixed Effects	Y	Y	Y	Y	Y
Industry Fixed Effects		Y	Y	Y	Y
S936 Exposed Industry			Y		Y
Major Sectors Only				Y	Y

Notes: Author's calculations using data from COMPUSTAT Historical Segments. This table shows estimates of Equation 3 where Exposure to Section 936 X Post is an indicator at the firm level for exposed firms interacted with an indicator for years after 1995. Consistent with the hypothesis that multinationals shifted investment abroad, we see that exposed firms saw an increase in the fraction of foreign investment following the repeal of §936. Yearly coefficients are shown graphically in Figure 6. See Section 5 for more discussion. Standard errors clustered at the firm level are shown in parentheses with p-values below.

Table 4: Effects of Exposure to §936 on Industry-State-Level Capital Expenditures

Growth in Capital Expenditures					
	(1)	(2)	(3)	(4)	(5)
Exposure to Section 936 X Post	-0.735	-0.736	-0.726	-0.684	-0.659
	(0.209)	(0.207)	(0.212)	(0.188)	(0.285)
	0.001	0.001	0.001	0.001	0.025
Observations	10694	10694	10694	10694	10694
Year Fixed Effects	Yes	Yes	Yes		
Industry Fixed Effects	Yes	Yes			
State Fixed Effects		Yes			
State X Industry Fixed Effects			Yes	Yes	Yes
Year X State Fixed Effects				Yes	
Year X Industry Fixed Effects					Yes

Notes: Author's calculations using data from NETS and the Annual Survey of Manufactures. This table displays the estimates of Equation 4 where exposure to §936 is the proportion of establishments in each state-industry divided by the interquartile range. Exposure to Section 936 X Post is an indicator at the firm level for exposed firms interacted with an indicator for years after 1995. Following the repeal of §936, we see a persistent decline in the flow of investment in state-industries with more exposure to §936. The yearly estimates are shown graphically in Figure 4. See Section 5 for more information. Standard errors clustered at the state level are shown in parentheses with p-values below.

Table 5: Event Study on Employment Growth

Exposure to Section 936	(1)	(2)	(3)	(4)	(5)	(6)	(7)
X 1990	0.000	0.000	0.000	0.007	-0.007	-0.001	-0.011
	(0.015)	(0.015)	(0.015)	(0.011)	(0.019)	(0.015)	(0.016)
X 1991	-0.001	-0.001	-0.001	0.004	-0.008	-0.003	-0.009
	(0.012)	(0.012)	(0.012)	(0.009)	(0.016)	(0.012)	(0.012)
X 1992	0.001	0.001	0.001	0.005	-0.003	-0.000	-0.005
	(0.008)	(0.008)	(0.008)	(0.006)	(0.011)	(0.008)	(0.007)
X 1993	0.002	0.002	0.002	0.003	-0.001	0.001	-0.000
	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)	(0.005)	(0.005)
X 1994	0.001	0.001	0.001	0.002	0.000	0.000	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
X 1996	-0.004	-0.004	-0.004	-0.004	-0.005	-0.003	-0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
X 1997	-0.009*	-0.009*	-0.009*	-0.008*	-0.009	-0.006	-0.007
	(0.004)	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)	(0.005)
X 1998	-0.014**	-0.014**	-0.014**	-0.013**	-0.015*	-0.010*	-0.012*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)	(0.005)	(0.006)
X 1999	-0.017*	-0.017*	-0.017*	-0.017**	-0.018*	-0.012	-0.013
	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.006)	(0.008)
X 2000	-0.026**	-0.026**	-0.026**	-0.025**	-0.029**	-0.021**	-0.023*
	(0.008)	(0.008)	(0.008)	(0.007)	(0.010)	(0.008)	(0.009)
X 2001	-0.033***	-0.033***	-0.033***	-0.032***	-0.039**	-0.034**	-0.030*
	(0.009)	(0.009)	(0.009)	(0.009)	(0.012)	(0.011)	(0.013)
X 2002	-0.041***	-0.041***	-0.041***	-0.039***	-0.045**	-0.039**	-0.032*
	(0.011)	(0.011)	(0.011)	(0.010)	(0.014)	(0.012)	(0.015)
X 2003	-0.046***	-0.046***	-0.046***	-0.044***	-0.051**	-0.045**	-0.036*
	(0.012)	(0.012)	(0.012)	(0.012)	(0.016)	(0.013)	(0.016)
X 2004	-0.058***	-0.058***	-0.058***	-0.055***	-0.065***	-0.056***	-0.048*
	(0.014)	(0.014)	(0.014)	(0.013)	(0.018)	(0.015)	(0.019)
X 2005	-0.069***	-0.069***	-0.069***	-0.065***	-0.078***	-0.067***	-0.059**
	(0.017)	(0.017)	(0.017)	(0.017)	(0.022)	(0.018)	(0.022)
X 2006	-0.078***	-0.078***	-0.078***	-0.073***	-0.087**	-0.077***	-0.068**
	(0.020)	(0.020)	(0.020)	(0.019)	(0.026)	(0.020)	(0.024)
X 2007	-0.078***	-0.078***	-0.078***	-0.073***	-0.087**	-0.077***	-0.067**
	(0.021)	(0.021)	(0.021)	(0.019)	(0.026)	(0.022)	(0.025)
X 2008	-0.077***	-0.077***	-0.077***	-0.072***	-0.085**	-0.077***	-0.066**
	(0.020)	(0.020)	(0.020)	(0.018)	(0.025)	(0.022)	(0.024)
X 2009	-0.071***	-0.071***	-0.071***	-0.066***	-0.078**	-0.073***	-0.062**
	(0.018)	(0.018)	(0.018)	(0.016)	(0.023)	(0.020)	(0.022)
X 2010	-0.070***	-0.070***	-0.070***	-0.065***	-0.076**	-0.072***	-0.058**
	(0.018)	(0.018)	(0.018)	(0.016)	(0.023)	(0.020)	(0.021)
X 2011	-0.073***	-0.073***	-0.073***	-0.069***	-0.080**	-0.076***	-0.061**
	(0.018)	(0.018)	(0.018)	(0.016)	(0.024)	(0.021)	(0.021)
X 2012	-0.078***	-0.078***	-0.078***	-0.073***	-0.087***	-0.082***	-0.066**
	(0.019)	(0.019)	(0.019)	(0.018)	(0.025)	(0.022)	(0.022)
Year by Industry Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Location Fixed Effects		Y					
Industry-by-County Fixed Effects			Y				
Winsorized Weights				Y			
Drops Small County-Industries (<1000)					Y		
Conspuma Geography						Y	
Commuting Zone Geography							Y

Notes: This table displays estimates of Equation 5, which are shown in Figure 11. Observations are county-industries in each year. Exposure to Section 936 comes from the NETS and employment growth comes from QCEW (2017). See Section 5 for discussion and Appendix B for more information about the data. Standard errors clustered at the state and industry levels are shown in parentheses. Observations are weighted by employment in 1995. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 6: Event Study on Employment Growth: Additional Specifications

Exposure to Section 936	(1)	(2)	(3)	(4)	(5)	(6)
X 1990	0.000	0.001	0.001	0.005	-0.013	-0.011
	(0.017)	(0.015)	(0.015)	(0.013)	(0.012)	(0.013)
X 1991	-0.002	-0.001	-0.001	0.004	-0.010	-0.000
	(0.014)	(0.012)	(0.012)	(0.010)	(0.009)	(0.009)
X 1992	0.001	0.001	0.001	0.006	-0.005	0.001
	(0.010)	(0.008)	(0.008)	(0.007)	(0.005)	(0.006)
X 1993	0.002	0.002	0.002	0.005	-0.002	-0.000
	(0.006)	(0.005)	(0.005)	(0.004)	(0.003)	(0.005)
X 1994	0.001	0.001	0.001	0.003	-0.001	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
X 1996	-0.005	-0.004	-0.005	-0.004*	-0.004*	-0.009*
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
X 1997	-0.011*	-0.009*	-0.010*	-0.009*	-0.005	-0.015**
	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)
X 1998	-0.017**	-0.015**	-0.015**	-0.014**	-0.006	-0.019**
	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)	(0.006)
X 1999	-0.020**	-0.018**	-0.018**	-0.016**	-0.007	-0.021**
	(0.008)	(0.007)	(0.007)	(0.005)	(0.005)	(0.007)
X 2000	-0.031**	-0.027**	-0.028***	-0.024***	-0.013*	-0.023*
	(0.009)	(0.008)	(0.008)	(0.006)	(0.005)	(0.009)
X 2001	-0.040***	-0.034***	-0.035***	-0.030***	-0.020**	-0.026*
	(0.011)	(0.009)	(0.010)	(0.008)	(0.007)	(0.010)
X 2002	-0.048***	-0.042***	-0.043***	-0.037***	-0.028**	-0.029**
	(0.013)	(0.011)	(0.011)	(0.010)	(0.008)	(0.009)
X 2003	-0.055***	-0.047***	-0.048***	-0.042***	-0.032***	-0.034***
	(0.014)	(0.012)	(0.013)	(0.011)	(0.009)	(0.009)
X 2004	-0.069***	-0.059***	-0.060***	-0.052***	-0.040***	-0.045***
	(0.017)	(0.014)	(0.015)	(0.012)	(0.010)	(0.009)
X 2005	-0.082***	-0.070***	-0.072***	-0.062***	-0.047***	-0.049***
	(0.021)	(0.018)	(0.018)	(0.015)	(0.011)	(0.011)
X 2006	-0.092***	-0.079***	-0.081***	-0.071***	-0.054***	-0.066***
	(0.024)	(0.020)	(0.021)	(0.017)	(0.013)	(0.013)
X 2007	-0.093***	-0.080***	-0.081***	-0.073***	-0.055***	-0.067***
	(0.024)	(0.021)	(0.021)	(0.017)	(0.013)	(0.014)
X 2008	-0.092***	-0.079***	-0.080***	-0.072***	-0.055***	-0.065***
	(0.023)	(0.020)	(0.020)	(0.016)	(0.013)	(0.014)
X 2009	-0.085***	-0.073***	-0.074***	-0.065***	-0.053***	-0.059***
	(0.021)	(0.018)	(0.018)	(0.015)	(0.014)	(0.012)
X 2010	-0.083***	-0.072***	-0.073***	-0.065***	-0.051**	-0.058***
	(0.021)	(0.018)	(0.018)	(0.015)	(0.015)	(0.012)
X 2011	-0.087***	-0.075***	-0.076***	-0.068***	-0.051**	-0.060***
	(0.021)	(0.018)	(0.018)	(0.015)	(0.015)	(0.013)
X 2012	-0.093***	-0.080***	-0.081***	-0.073***	-0.054***	-0.063***
	(0.023)	(0.020)	(0.019)	(0.016)	(0.015)	(0.014)
Year Fixed Effects	Y	Y	Y	Y	Y	Y
Year by Industry Fixed Effects	Y	Y	Y	Y		
Employment Instrument	Y					
Large Retailers Dropped from Links		Y				
No US Headquarter Dropped from Links			Y			
Only Compustat Sample Links				Y		
Employment Growth Per Capita					Y	
Manufacturing Employment Per Capita						Y

Notes: This table displays estimates of Equation 5, which are shown in Figure 11. Observations are county-industries in each year. Exposure to Section 936 comes from the NETS and employment growth comes from QCEW (2017). See Section 5 for discussion and Appendix B for more information about the data. Standard errors clustered at the state and industry levels are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Observations are weighted by employment in 1995.

Table 7: Effects of Repealing §936 on Employment and Income Growth

	Employment Growth					Income Growth				
Geography	County			Conspuma	C-Zone	County			Conspuma	C-Zone
Data Source	IRS	BEA	QCEW	QCEW	QCEW	IRS	BEA	QCEW	QCEW	QCEW
Years: 2004-2008	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposure to Section 936	-0.074	-0.117	-0.072	-0.071	-0.062	-0.164	-0.140	-0.125	-0.121	-0.124
	(0.016)	(0.019)	(0.018)	(0.019)	(0.022)	(0.032)	(0.033)	(0.033)	(0.033)	(0.039)
	0.000	0.000	0.000	0.001	0.007	0.000	0.000	0.000	0.001	0.002
Years: 1990-1995	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposure to Section 936	0.006	0.006	0.000	-0.001	-0.004	-0.001	-0.001	-0.004	-0.005	-0.009
	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)	(0.008)	(0.006)	(0.007)	(0.007)	(0.007)
	0.283	0.217	0.952	0.932	0.527	0.916	0.923	0.569	0.504	0.198
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows county, conspuma, and commuting zone level regression estimates of the effect of exposure to Section 936 on employment growth in (1) to (5) and income Growth in (6) to (10) before and after the rollback of Section 936 in the upper and lower panels, respectively. Exposure to Section 936 comes from the NETS while employment and income growth come from the IRS SOI, the BEA, and QCEW (2017) for columns (1) and (6), (2) and (7), and (3) to (5) and (8) to (10), respectively. See Section 5 for discussion and Appendix B for more information about the data. The QCEW regressions also include industry fixed effects in addition to the year fixed effects in all columns. Standard errors clustered at the state and industry levels are shown in parentheses with p-values displayed below. The independent variable of interest is the IQR adjusted exposure to §936 at the county level, so the estimates mean that an IQR increase in exposure to §936 is associated with a 7.2% decrease in employment and a 12.5% decrease in income according to columns (3) and (8). The lower panel fails to find evidence of any pre-trends using conventional levels of statistical significance.

Table 8: Effects of Repealing §936 on Wages, Rental Costs, and Home Values

	(1)	(2)	(3)	(4)	(5)
Stacked Differences After 1990					
		Wages			
	All	High Skill	Low Skill	Rent	Home Value
Exposure to Section 936	-0.010	-0.006	-0.014	-0.018	-0.025
	(0.002)	(0.002)	(0.002)	(0.004)	(0.008)
	0.000	0.003	0.000	0.000	0.002
Observations	984	984	984	984	984
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Differences Before 1990					
		Wages			
	All	High Skill	Low Skill	Rent	Home Value
Exposure to Section 936	0.006	0.005	0.004	0.032	0.034
	(0.008)	(0.005)	(0.008)	(0.012)	(0.024)
	0.475	0.376	0.660	0.012	0.160
Observations	492	492	492	492	492
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Estimates in this table come from regressions of economic outcomes at the conspuma level on the exposure to Section 936 scaled by the interquartile range. Exposure to Section 936 comes from the NETS and conspuma level outcomes are from the Census (Ruggles et al., 2010). These regressions use the adjusted values of the outcomes. See Section 5 for discussion and Appendix B for more information about the data. Standard errors clustered at the state level are shown in parentheses with p-values displayed below. Additional specifications are shown in Table A.18. Observations are weighted according to population.

Table 9: Effects of Repealing §936 on Government Transfer Payments Per Capita, 2004-2008

	Total Transfers (1)	Unemployment Benefits (2)	Income Replacement (3)	Education Benefits (4)	Retirement and Disability (5)	Medicare Benefits (6)	Public Medical Benefits (7)
Panel A. Percent change in transfers per capita relative to 1995							
Exposure to Section 936	0.025	0.254	0.101	0.075	0.005	-0.007	0.014
	(0.016)	(0.066)	(0.030)	(0.062)	(0.016)	(0.026)	(0.029)
	0.128	0.000	0.001	0.232	0.777	0.780	0.638
Panel B. Dollar change in transfers per capita relative to 1995							
Exposure to Section 936	6.426	15.883	30.214	6.439	1.527	-14.324	-26.040
	(41.776)	(3.289)	(5.449)	(2.995)	(19.083)	(13.078)	(40.346)
	0.878	0.000	0.000	0.037	0.937	0.279	0.522
Sample Proportion of Transfers	97.2%	1.9%	9.6%	1.7%	38.5%	25.1%	18.6%
Sample Transfers Per Capita	6,040.1	114.1	596.6	103.7	2,358.8	1,570.5	1,176.6
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates in this table come from regressions of government transfers in each category at the county level on the exposure to Section 936. The upper panel shows the results with the dependent variable measured in percent change per capita while the lower panel shows results with the dependent variable measured in change in dollars per capita. Exposure to Section 936 comes from the NETS and county level transfers come from the BEA local area personal income accounts (Bureau of Economic Analysis, 2018). Column (1) includes all transfers in columns (2) to (7), which does not include veteran's benefits, and covers 97.2% of all personal income transfers reported in Bureau of Economic Analysis (2018). Column (2) shows a positive effect on the dependent variable of unemployment insurance compensation, while Columns (3) and (4) show positive effects on income maintenance payments, including SSI and the Earned Income Tax Credit, and education and training assistance programs. Columns (5)-(7) show statistically insignificant effects on retirement and disability benefits programs, which includes most Social Security Administration payments except for Supplemental Security Income (SSI), medicare payments, or public assistance medical benefits. See Appendix B for more information about the data. Clustered standard errors are shown in parentheses with p-values displayed below. Analogous regressions for the 1990-1995 period in Table A.19 show statistically insignificant pre-trends.

Online Appendix: Not For Publication

This appendix includes supplemental information and additional analyses. Appendix A proves the propositions of the model. Appendix B describes the data sources. Appendix C compares our setting and results to those of the China Shock studied by Autor et al. (2013). Appendix D describes how changes in investment accumulate into changes in capital stocks over time. Appendix E shows that exposed firms experienced a decline in firm value following news of the repeal of §936. Appendix F characterizes heterogeneous effects of the repeal of §936 on local employment growth.

A Model Appendix

This appendix proves the results in Propositions 1-4. We then extend the results to allow for heterogeneous costs of profit shifting across countries.

A.1 Proposition 1

Form the Lagrangean of the profit shifting problem:

$$\sum_j K_j \left[(1-t_j) r_j - \rho - \frac{(r_j - \bar{f}_j)^2}{2a} \right] + \lambda \left\{ \sum_j K_j (\bar{f}_j - r_j) \right\}.$$

The first order condition with respect to r_j implies:

$$r_j - \bar{f}_j = a((1-t_j) - \lambda). \tag{A.1}$$

The first order condition with respect to λ implies:

$$0 = \sum_j K_j (\bar{f}_j - r_j) = \sum_j a K_j (\lambda - (1-t_j)) \Leftrightarrow \lambda = 1 - \tilde{t}, \tag{A.2}$$

where $\tilde{t} = \frac{\sum_j t_j K_j}{\sum_j K_j}$. Substituting Equation A.2 into A.1 yields the result in Proposition 1.

A.2 Proposition 2

Substituting the result of Proposition 1 into the objective function and rearranging we obtain

$$\max_{\{K_j\}} \underbrace{\sum_j \left[(1-t_j) f_j(K_j) - \rho K_j \right]}_{\text{Economic Incentives}} + a \sum_j K_j \underbrace{\left[(1-t_j)(\tilde{t} - t_j) - \frac{(\tilde{t} - t_j)^2}{2} \right]}_{\text{Net Benefit from Profit Shifting}}. \tag{A.3}$$

The first summation of Equation A.3 is the firm's objective in the absence of profit shifting opportunities. The second summation expresses the net-benefit from profit shifting across all locations. For country j, the benefit from profit shifting is captured by

$$K_j(1-t_j)(r_j - \bar{f}_j) = aK_j(1-t_j)(\tilde{t} - t_j),$$

where the profit shifting term is substituted by the term in Proposition 1. Similarly, the cost from profit shifting is captured by:

$$C(r_j, \bar{f}_j, K_j) = K_j \frac{(r_j - \bar{f}_j)^2}{2a} = aK_j \frac{(\tilde{t} - t_j)^2}{2}.$$

Firms' optimal investment plans thus trade-off the economic and tax incentives in Equation A.3. In addition, firms' capital allocations also impact their capital-weighted average tax: $\tilde{t}$. Note that the base-shifting effect $\frac{\partial \tilde{t}}{\partial K_i}$ is given by:

$$\frac{\partial \tilde{t}}{\partial K_i} = \frac{-(\tilde{t} - t_i)}{\sum_k K_k}.$$

A similar effect arises in analyses of formulary apportionment, as in Suárez Serrato and Zidar (2016) and Fajgelbaum et al. (2015).

The first order condition of Equation 1 with respect to K_i is:

$$\begin{aligned}
(1-t_i)f_i'(K_i) - \rho + a\left[(1-t_i)(\tilde{t} - t_i) - \frac{(\tilde{t} - t_i)^2}{2}\right] + a(1-\tilde{t})\frac{\partial \tilde{t}}{\partial K_i}\sum_k K_k &= 0 \\
(1-t_i)f_i'(K_i) - \rho + a\left[\underbrace{(1-t_i)(\tilde{t} - t_i)}_{(1-\tilde{t})(\tilde{t}-t_i)+(\tilde{t}-t_i)^2} - \frac{(\tilde{t} - t_i)^2}{2}\right] - a(1-\tilde{t})(\tilde{t} - t_i) &= 0 \\
(1-t_i)f_i'(K_i) - \rho + a\frac{(\tilde{t} - t_i)^2}{2} &= 0, \qquad \text{(A.4)}
\end{aligned}$$

where the second line substituted the base-shifting term and the third simplifies the middle term. Solving Equation A.4 yields the result in Proposition 2.

A.3 Proposition 3

Following Proposition 2, write the effect of changing tax t_j in country i as:

$$\frac{\partial K_i}{\partial t_j} = \frac{a(\tilde{t} - t_i)}{\mathbb{I}\{i=j\}f_i'(K_i) - (1-t_i)f_i''(K_i)}\left(\frac{\partial \tilde{t}}{\partial t_j} - \mathbb{I}\{i=j\}\right). \qquad \text{(A.5)}$$

Taking the derivative of $\tilde{t}$ we obtain:

$$\begin{aligned}\frac{\partial \tilde{t}}{\partial t_j} &= \frac{K_j}{\sum_k K_k} - \frac{1}{\sum_k K_k}\sum_k \left[(\tilde{t}-t_k)\frac{\partial K_k}{\partial t_j}\right] \\ &= \frac{K_j}{\sum_k K_k} - \frac{1}{\sum_k K_k}\sum_k \frac{a(\tilde{t}-t_k)^2\left(\frac{\partial \tilde{t}}{\partial t_j} - \mathbb{I}\{k=j\}\right)}{\mathbb{I}\{k=j\}f_k'(K_k) - (1-t_k)f_k''(K_k)},\end{aligned} \tag{A.6}$$

where we substitute Equation A.5 for all countries in the second line. Solving Equation A.6 for $\frac{\partial \tilde{t}}{\partial t_j}$, we obtain:

$$\frac{\partial \tilde{t}}{\partial t_j} = \frac{K_j + \frac{a(\tilde{t}-t_j)^2}{f_j'(K_j)-(1-t_j)f_j''(K_j)}}{\sum_k K_k + \sum_k \frac{a(\tilde{t}-t_k)^2}{\mathbb{I}\{k=j\}f_k'(K_k)-(1-t_k)f_k''(K_k)}}. \tag{A.7}$$

Since $f_k'(K_k) > 0$ and $-(1-t_k)f_k''(K_k) > 0$, we know $0 < \frac{\partial \tilde{t}}{\partial t_j} < 1$. For $i \neq j$, Equation A.5 then implies:

$$\text{sign}\left(\frac{\partial K_i}{\partial t_j}\right) = \text{sign}\left(\tilde{t} - t_i\right).$$

A.4 Profit Shifting and Labor Demand

We now explore the implications of Propositions 1-3 for labor demand. The tax planning problem in the second period is now as follows:

$$\sum_j K_j\left[(1-t_j)\left(r_j - w_j\frac{L_j}{K_j}\right) - \rho - \frac{(r_j-\bar{f}_j)^2}{2a}\right] + \lambda\left\{\sum_j K_j(\bar{f}_j - r_j)\right\}.$$

Since including the tax-deductible cost of labor in this expression does not affect Equation A.1, the result from Proposition 1 is unaffected.

In the first period, the choice of capital and labor inputs follow from maximizing:

$$\max_{K_j,L_j}\sum_j\left[(1-t_j)(f_j(K_j,L_j) - w_jL_j) - \rho K_j\right] + a\sum_j K_j\left[(1-t_j)(\tilde{t}-t_j) - \frac{(\tilde{t}-t_j)^2}{2}\right].$$

The first order condition for capital in this problem is identical to Equation A.4, which implies that the result from Proposition 2 is also unaffected. The first order condition for labor is:

$$\frac{\partial f_j}{\partial L_j}(K_j,L_j) = w_j,$$

which is not directly affected by t_j since labor costs are deductible.

To proceed, we introduce some convenient notation. First, we write $f_{12}^k = \frac{\partial^2 f_k}{\partial K_j \partial L_j}$ to denote second derivatives with respect to inputs 1, K_k, and 2, L_k. We also write $\frac{\partial w_k}{\partial t_j} = \kappa_{kj}\frac{\partial L_k}{\partial t_j}$ to

denote the equilibrium relation between labor demand and a country's wage rate. Since changes in labor demand will trace the country's labor supply curve, we assume $\kappa_{kj} > 0$. We now totally differentiate this equation as well as Equation A.4:

$$\begin{bmatrix} f_{11}^k - \mathbb{I}(j=k)\frac{f_1^k}{(1-t_k)} & f_{12}^k \\ f_{21}^k & f_{22}^k - \kappa_{kj} \end{bmatrix} \begin{bmatrix} \frac{\partial K_k}{\partial t_j} \\ \frac{\partial L_k}{\partial t_j} \end{bmatrix} = \begin{bmatrix} -\frac{a(\tilde{t}-t_k)}{(1-t_k)} \times \left(\frac{\partial \tilde{t}}{\partial t_j} + \mathbb{I}(j=k) \right) \\ 0 \end{bmatrix}. \tag{A.8}$$

The determinant of the first matrix is given by:

$$\Omega_{kj} = \left(f_{11}^k - \mathbb{I}(j=k)\frac{f_1^k}{(1-t_k)} \right) \times (f_{22}^k - \kappa_{kj}) - f_{21}^k f_{12}^k > 0.$$

Inverting Equation A.8 yields:

$$\begin{bmatrix} \frac{\partial K_k}{\partial t_j} \\ \frac{\partial L_k}{\partial t_j} \end{bmatrix} = \frac{1}{\Omega_{kj}} \times \begin{bmatrix} f_{22}^k - \kappa_{kj} & -f_{12}^k \\ -f_{21}^k & f_{11}^k - \mathbb{I}(j=k)\frac{f_1^k}{(1-t_k)} \end{bmatrix} \begin{bmatrix} -\frac{a(\tilde{t}-t_k)}{(1-t_k)} \times \left(\frac{\partial \tilde{t}}{\partial t_j} + \mathbb{I}(j=k) \right) \\ 0 \end{bmatrix}. \tag{A.9}$$

Consider now the effect of t_j on $\tilde{t}$. Generalizing Equation A.6, we now have:

$$\begin{aligned} \frac{\partial \tilde{t}}{\partial t_j} &= \frac{K_j}{\sum_k K_k} - \frac{1}{\sum_k K_k} \sum_k \left[(\tilde{t} - t_k) \frac{\partial K_k}{\partial t_j} \right] \\ &= \frac{K_j}{\sum_k K_k} - \frac{1}{\sum_k K_k} \sum_k \frac{a(\tilde{t}-t_k)^2(\kappa_{kj} - f_{22}^k)}{(1-t_k)\Omega_{kj}} \times \left(\frac{\partial \tilde{t}}{\partial t_j} - \mathbb{I}(j=k) \right). \end{aligned}$$

Solving for $\frac{\partial \tilde{t}}{\partial t_j}$ we now have:

$$\frac{\partial \tilde{t}}{\partial t_j} = \frac{K_j + \frac{a(\tilde{t}-t_j)^2(\kappa_{jj} - f_{22}^j)}{(1-t_j)\Omega_{jj}}}{\sum_k K_k + \sum_k \frac{a(\tilde{t}-t_k)^2(\kappa_{kj} - f_{22}^k)}{(1-t_k)\Omega_{kj}}}.$$

Since $\kappa_{kj} - f_{22}^k > 0$ and $\Omega_{kj} > 0$, we know $1 > \frac{\partial \tilde{t}}{\partial t_j} > 0$. Note that if $f_{12}^k = f_{21}^k = 0$, this equation reduces to Equation A.7.

From this equation and Equation A.9, we now have that, for $j \neq k$,

$$\frac{\partial K_k}{\partial t_j} = \frac{a(\tilde{t}-t_k)(\kappa_{kj} - f_{22}^k)}{(1-t_k)\Omega_{kj}} \frac{\partial \tilde{t}}{\partial t_j},$$

which confirms that the result of Proposition 3 holds in this setting. In addition, we now have the following implication of profit shifting for labor demand:

$$\frac{\partial L_k}{\partial t_j} = \frac{a(\tilde{t}-t_k) f_{21}^k}{(1-t_k)\Omega_{kj}} \frac{\partial \tilde{t}}{\partial t_j},$$

where $j \neq k$. Since $\Omega_{kj} > 0$, we have

$$\text{sign}\left(\frac{\partial L_k}{\partial t_j} \right) = \text{sign}\left((\tilde{t} - t_k) f_{21}^k \right).$$

This implies labor in a high tax country is a tax complement with the corporate rate in a tax haven if capital is a complement to labor in the high tax country, *i.e.*, $f_{21}^k > 0$.

Finally, notice the following relation between the effects on capital and labor:

$$\frac{\partial L_k}{\partial t_j} = \frac{f_{21}^k}{\kappa_{kj} - f_{22}^k} \frac{\partial K_k}{\partial t_j}.$$

That is, we would expect to see a larger passthrough of capital to labor whenever capital and labor are highly complementary (large f_{21}^k), whevener capital has weak decreasing returns (small f_{22}^k), and whenever changes in labor demand have small wage effects (small κ_{kj}).

A.5 Heterogeneous Profit Shifting Costs

The model so far assumes that the cost of evasion is symmetric in all countries. As discussed in Section 2, lax regulations related to profit shifting and §936 meant that Puerto Rico was an especially desirable tax haven for US multinationals. Our model can incorporate this feature by allowing for heterogeneous costs of profit shifting across locations. This section shows that the results of Propositions 1-3 are robust to allowing for the parameter a to vary by country.

Let a_j be the value of a for country j. The optimal profit shifting strategy is now:

$$r_j = \bar{f}_j + a_j(\hat{t} - t_j),$$

where $\hat{t} = \frac{\sum_k t_k K_k a_k}{\sum_k K_k a_k}$. This result is very similar to the one in Proposition 1 but notice that the "mean tax term" $\hat{t}$ also weights country's taxes by the parameters a_k. Intuitively, countries where profit shifting is more costly (lower values of a_j) will smaller differences between reported and actual profitability. In addition, these countries will have a smaller contribution to the mean tax.

We also obtain a similar result to Proposition 2. Equation A.4 now becomes:

$$(1 - t_j) f_j'(K_j) - \rho + a_j \frac{(\hat{t} - t_j)^2}{2} = 0.$$

This implies that countries where profit shifting is more costly (lower values of a_j) will see smaller effects on their user cost of capital.

With heterogeneous costs, the term $\frac{\partial \hat{t}}{\partial t_j}$ now includes the a_k's as well:

$$\frac{\partial \hat{t}}{\partial t_j} = \frac{a_j K_j + \frac{a_j^2 (\hat{t} - t_j)^2}{f_j'(K_j) - (1-t_j) f_j''(K_j)}}{\sum_k a_k K_k + \sum_k \frac{a_k^2 (\hat{t} - t_k)^2}{\mathbb{I}\{k=j\} f_k'(K_k) - (1-t_k) f_k''(K_k)}}.$$

It is it still the case that $0 < \frac{\partial \hat{t}}{\partial t_j} < 1$ and the result of Proposition 3 still holds. We can further state that:

$$\frac{\partial K_i}{\partial t_j} \propto a_i \left(\hat{t} - t_i\right),$$

so that countries where profit shifting is more costly (lower values of a_i) will be subject to smaller tax complementarities. Similar arguments generalize the result of Proposition 4.

These results show that, all else equal, when a country j has a low cost of profit shifting (higher a_j), tax complementarities on capital and employment will be stronger. This implies that the setting of §936 makes it likely that we can detect effects on domestic investment and employment.

A.6 Discussion of Empirical Implications

Consider now two examples that summarize the insights of Propositions 1-4 as applied to the elimination of §936.

Example 1 (US as a Low Tax Country: $\tilde{t} \geq t_{US}$)**.** *§936 would have the following effects:*

- *Profit shifting toward Puerto Rico* ($r_P > \bar{f}_P$) *and the US* ($r_{US} > \bar{f}_{US}$).
- *Profit shifting increases investment and employment globally.*
- *Profit shifting redirects investment and employment to the US.*

Repealing §936 would have the following effects:

- *Decrease profit shifting toward Puerto Rico, and increase shifting toward the US.*
- *Decrease investment and employment globally.*
- *Shift investment and employment away from Puerto Rico, and toward the US.*

Notice that if $\tilde{t} = t_{US}$,[59] and US is the country with the average tax, it would be the case that §936 or its repeal had no effect on US investment. Nonetheless, profit shifting toward Puerto Rico would mean that the US treasury would be reducing tax obligations of firms in other countries by paying for the US Corporations Possessions Tax Credit.

Example 2 (US as a High Tax Country: $\tilde{t} < t_{US}$)**.** [60]

[59] A convenient numerical example is the case when $K_{US} = K_C = K_{PR}$ and $t_C = 2t_{US}$

[60] A convenient numerical example is the case when $K_{US} = K_C = K_{PR}$ and $t_C = t_{US}/2$. In this case $\tilde{t} = t_{US}/2$.

- *Profit shifting toward Puerto Rico* ($r_P > \bar{f}_P$) *and away from the US* ($r_{US} < \bar{f}_{US}$).
- *Profit shifting increases investment and employment globally.*
- *Profit shifting redirects investment and employment to the US.*

Repealing §936 would have the following effects:

- *Decrease profit shifting toward Puerto Rico, and increase shifting away from the US.*
- *Decrease investment and employment globally.*
- *Shift investment and employment away from Puerto Rico and the US.*

Note that in this example profits are shifted away from other countries as well, such that the profits shifted to Puerto Rico may not represent a reduction in US tax obligations, but may represent obligations owed to other countries. However, as in the previous example, the US treasury would still be affected by the tax expenditure on the US Possessions Corporations tax credit.

These examples show that tax climates in other countries are an important consideration for whether tax changes, such as the repeal of §936, have positive or negative effects on the US economy. In particular, the view of the US as a high tax country is consistent with our empirical results showing negative domestic effects of repealing §936 on the US economy.

A.7 Additional Extensions

The model abstracted away from dynamic considerations of investment. The model can be extended in this direction by adopting a framework such as in Auerbach (1989) or Auerbach and Hines (1987). If firms faced a dynamic investment decision and they viewed the repeal of §936 as permanent, they would respond by lowering investment and letting existing capital depreciate during the phase-out of the tax credits. Since labor is seen as a static input, the corresponding labor demand would match the dynamics of the capital stock.

Similarly, the model does not take into account the location of firms within the US. The empirical analysis on the effects on local labor markets uses the exposure of a given labor market to §936 to estimate the effects of the repeal of §936 at the local level. We can specialize the model by adding a location component to firms and couching the analysis of the reform in a model of local labor markets, as in Suárez Serrato and Zidar (2016). We may further estimate aggregate

effects from regional variation using approaches such as those in Fajgelbaum et al. (2015) or Acemoglu and Restrepo (2017). Since these extensions would not change the main conclusions from the model in Section A or the empirical implications described in the previous section, we avoid needlessly complicating the presentation of the main model in the paper.

B Data Appendix

B.1 Data Sources

The size of the actual tax expenditure incurred by 26 U.S.C. §936 (1976) is provided at the three-digit NAICS code level by the IRS (IRS, 2017a). The SOI reports for corporations include the aggregate expenditure for the "US Possessions Tax Credit" up until the end of the phase-out in 2007. These data also include total revenues, net income, tax liabilities before and after deductions and credits, and several other enumerated federal corporate tax credits.

The primary data on the location of business establishments and employment comes from the National Establishment Time-Series (NETS) database from 1990-2010 (Walls & Associates, 2012). NETS contains the Duns and Bradstreet historical establishment data that is transformed into a time-series database. Of particular interest for this project, the data include the location of business establishments, employment at the establishment level, and links to firm headquarters. Other information includes relocations, estimated firm sales, counts of establishments, credit scores, active years, industries, and other indicators. The NETS data are supplemented with information about counties in the US.

This paper uses firm-specific data to measure the response of businesses and equity markets to the repeal. The firms with establishments in Puerto Rico that are identified in NETS are merged with information from the Compustat Historical Segments databases (Capital IQ, 1980-2014). Compustat data comes from public filings for publicly traded firms and includes values reported in annual 10-Ks regarding segment operation and financial statements. The financial information is reported on an annual basis. As in Rao (Forthcoming), we exploit the fact that the data allow investment to be separated into domestic and foreign segments. Stock returns surrounding the repeal of Section 936 come from CRSP Stocks (1990-1996) and other market indicators for use in an event study methodology come from Fama and French (1993). The CRSP data include daily returns for every publicly traded stock in the US for each active trading day. The Fama-French data includes weighted portfolios of returns to capture different types of market risk.

Data on manufacturing investment come from the Annual Survey of Manufacturers (ASM) through the Census Bureau. Data for 1997-2010 come from Ohrn (2017) while data for 1987-1995 come from Annual Survey of Manufacturers (1997). We combine data from the 1987-2006 surveys at the NAICS three digit level. We use the Census crosswalk between SIC codes and NAICS codes for years 1987-1995.

This paper uses data gathered by Suárez Serrato and Wingender (2016) from the Bureau of Economic Analysis, the Bureau of Labor Statistics, and the Internal Revenue Service. For each county, data on personal income and employment come from the Bureau of Economic Analysis' Regional Economic Information System (REIS) (Bureau of Economic Analysis, 2010). Employment and earnings at the industry and county level come from the Quarterly Census of Employment and Wages (QCEW) produced by the Bureau of Labor Statistics (BLS). The QCEW is made up of the universe of jobs covered by state unemployment insurance systems (QCEW, 2017). It accounts for more than 94% of total wages reported by the Bureau of Economic Analysis. Income by source comes from the IRS Statistics of Income (SOI) publications from 1989-2009 (IRS, 2017b). The SOI data include a breakdown of income at the county level into gross incomes, salaries, dividends, and interest, which allows changes in county income to be connected to the source of the income. This project also uses data from the Census Bureau. Skill specific outcomes including wages and rents come from calculations done by Suárez Serrato and Wingender (2014) using IPUMS (Ruggles et al., 2010). This includes skill specific values of log-wages, log-rents, and log-housing values. Other information from IPUMS includes aggregate values of population, employment, income, and earnings.

In order to calculate capital stock at the county level, we use the local QCEW earnings data at the industry level in conjunction with aggregate capital stock in the US by industry from the Bureau of Economic Analysis (Bureau of Economic Analysis, 2017). We then use two digit NAICS codes to distribute each industry's national capital stock across counties to match the distribution of employment across counties in the QCEW. The total capital stock for each county is then computed by summing across all of the industries in that county.

Data on the local import exposure from China comes from Autor et al. (2016). The main variable is import exposure per worker. The share of labor in "routine jobs" was gathered by Autor and Dorn (2013). An indicator of whether a state is a "right to work" state in 1984 is collected from Valletta and Freeman (1988). Data regarding income transfers from governments to individuals come from the Bureau of Economic Analysis Local Area Personal Income Accounts

(Bureau of Economic Analysis, 2018). These data include county level transfers disaggregated by program. We use the following categories: Retirement and disability insurance benefits, Medicare benefits, public assistance medical care benefits, Income maintenance benefits, Unemployment insurance compensation, and Education and training assistance, which amount to over 95% of total transfers in the data.

B.2 Validity of the Firm-Level Exposure Measure

Our measure of firm-level exposure is close to estimates of take-up of §936 based on tax data. There are 682 firms that could have taken advantage of §936 in our data.[61]

The GAO (2006) found that a total of 656 firms ever claimed §936 credits, while Grubert and Slemrod (1998) find that 419 manufacturing firms claimed §936 credits in 1987. Similarly, GAO (1993) shows that §936 firms had 105,000 workers in Puerto Rico in 1989, which is comparable to our estimate of 140,000 in 1990. To the extent that our measure of exposure to §936 is a slight upper bound, our estimates may be considered an intent-to-treat, and would represent a lower bound on the true effect. As we discuss in the paper, we conduct a number of tests to ensure that our results are robust to our definition of exposure.

We merge NETS with Compustat by firm name and then confirm and extend the merge manually. We further crawled the SEC's EDGAR database and searched for terms related to Puerto Rico, §936, and US Possessions Corporations Tax Credit in 10-K filings. We also note that a number of firms with operations in Puerto Rico are private. Overall, there are 219 exposed firms in Compustat, and we find that these firms are responsible for 69.5% of the total employment in §936 firms. It is also likely that the Compustat firms are responsible for the bulk of the §936 tax credits. The Joint Committee on Taxation (2006) notes that §936 credits were highly concentrated within §936 firms, and Grubert and Slemrod (1998) note that, in their data, 96% of the tax credits were claimed by 214 firms.

We use this matched sample from the NETS and Compustat to replicate the results of Grubert and Slemrod (1998), who study characteristics of §936 firms, and confirm that our measure of exposure captures the types of firms that actually received §936 credits. Specifically, we estimate a probit model of the likelihood that a firm is exposed to §936. Table A.20 shows that firms in the pharmaceutical industry, firms with more assets, more gross profits, and with R&D investment

[61] We conduct additional checks on our measure of exposure to §936 by manually excluding establishments in the public sector or non-profit sector.

were more likely to be exposed to §936. Note that, while pharmaceutical firms were more likely to claim §936 credits, only 4.2% of pharma firms in Compustat were exposed to §936.

Finally, in Appendix E we show that news of the repeal of §936 had negative effects on the stock prices of exposed firms. This result confirms that our firm-level measure of exposure correctly identifies firms that took advantage of §936.

B.3 Matching Exposed Firms to Controls in NETS

In order to measure the employment response of firms exposed to Section 936, we match exposed firms to a set of control firms that capture the counterfactual of how employment in these firms would have evolved if they were not exposed to the repeal of Section 936. Since the exposed firms are much larger than the average firm in the NETS, using any random set of companies as a control group is likely to lead to selection bias. For this reason, we create a matched sample of firms in NETS that are not exposed to Section 936 but are observably similar to the exposed firms.

We use the following procedure to find matches. We first identify the census region, industry defined by three digit NAICS code, employment quintile, and establishment quintile for each US-headquartered firm with at least one establishment in Puerto Rico in 1995. Since the NETS contains the universe of all firms, we are not able to run a matching algorithm on the entire dataset. We therefore restrict the set of potential matches to include every US-headquartered firm in the NETS from the same census region and industry classification as at least one of the exposed firms. Of these firms, we restrict the sample further to match the employment and establishment quintile of the relevant exposed firm. This procedure yields a list of 21,504 firms that are headquartered in the same census region, have the same 3-digit industry, and are in the same quintile of employment and number of establishments. This ensures that we match these characteristics perfectly. For each exposed firm, we select up to 10 control firms that match these characteristics to create the control data set of 2,000 firms. We use this control sample of matched firms to identify firm level employment effects in Figure 8 and to run the placebo test on local employment outcomes in Panel A of Figure 12.

C Comparison to the China Shock

In this section we take the work of Autor et al. (2013) (henceforth ADH) as inspiration for measuring the regional effects of aggregate policies or shocks and compare the effects of §936 to their results. We compare the repeal of §936 to the China Shock on three dimensions: (1) the size of the shock, (2) the regional effects on manufacturing employment, and (3) the ratio of revenue to employment losses.

To compare the repeal of §936 to the size of the China Shock, note that ADH report that imports from China increased by $364.44 billion between 1991 and 2007 (in 2017 dollars).[62] However, in their analysis of the labor market effects of import penetration, ADH isolate the supply-driven increase in imports from China. As they discuss in page 2140: "48 percent of the observed variation in rising Chinese import exposure can be attributed to the supply-driven component," which corresponds to an increase of $175 billion in 2017 dollars. As we discuss in Section 4, the repeal of §936 tax credits was equivalent to a loss in revenue of $232 billion in 2017 dollars.

Alternatively, we can compute the §936 credits that would have accrued to the imports from China. Taking the average profitability (8.12%) of §936 firms, the $175 billion in Chinese imports would result in $14.21 billion of taxable profits. Similarly, taking the effective federal rate (26%) of §936 firms, these profits would result in $3.7 billion of §936 tax credits. In comparison, the actual §936 tax credits of $4.9 billion in 1995 were 32% larger than these hypothetical tax credits.

As a second point of comparison, we compare the regional effects of §936 to those of the China Shock. Specifically, we compare the 10-year impact of the repeal of §936 with the estimated effect of the China Shock between 2000 and 2010. ADH report an inter-quartile range of the ten-year increase in import exposure of 1.51 (see their Appendix Table 1).[63] Taking their estimate of -0.72 (column 2 of their Table 2), this implies that increasing exposure to the China Shock from the 25th to the 75th percentile of the distribution lowered the share of manufacturing employees in the working-age population by 1.09 percentage points over a ten-year period. To facilitate a comparison of our results to those of ADH, in Panel B of Figure A.9 and Table A.8 we report regressions of the change in share of manufacturing employees in the working-age population that

[62] Table 1 (page 2131) of ADH shows an increase in 2007 dollars of $303.7 = 330.0 - 26.3$, which we multiply by an inflation adjustment of 1.2 from 2007 to 2017 (https://www.bls.gov/data/inflation_calculator.htm).

[63] Since the phase-out of §936 lasted a decade, we compare our effects to the ten-year effect of the China Shock starting in 2000. ADH report in footnote 27 a calculation for the seven year period between 2000 and 2007.

mirror their analysis. These results show that between 1995 and 2005, increasing the exposure to §936 from the 25th to the 75th percentile of the distribution lowered the share of manufacturing employees in the working-age population by 1.72 percentage points.

Given that §936 is a larger shock in revenue terms, it is perhaps not surprising to find a larger regional effect. As a third point of comparison, we compute the revenue-per-job-lost for both shocks. Since the average exposure to §936 is 0.88, we find an overall decrease in employment of 1.5 percentage points. In contrast, the average ten-year increase in imports per worker of 2.63 (ADH's Appendix Table 2) implies a supply-side-driven decrease in employment of 0.91 percentage points.[64] Relative to the population of 16-64 year-olds in 2000 of 178.7 million workers (ADH footnote 31, page 2140), the supply-driven component of the China Shock reduced manufacturing employment by 1.63 million jobs, while the repeal of §936 had an impact of 2.68 million jobs. Finally, comparing revenue losses to job losses, we find that the revenue-per-job-lost has a similar order of magnitude in both cases. Specifically, the China Shock impacts imply that a job was lost for every \$107,000 $\approx \left(\frac{175{,}000}{1.63}\right)$ dollars of imports, while §936 would result in a job lost per every \$87,000 $\approx \left(\frac{232{,}000}{2.68}\right)$ of lost revenue.

These comparisons illustrate two points. First, the shocks have comparable magnitudes, as we show by comparing equivalent tax credits or revenue losses. Second, the effects on manufacturing employment in both cases show a similar decrease in revenue for every job that is lost. This benchmarking of the effects of §936 with those of the China Shock shows that the employment effects of §936 have a reasonable magnitude, as they are commensurate with the effects from the China Shock.

D Changes in Investment Flows and Capital Stocks

The empirical results of Section 5 show that firms respond to the repeal of §936 by lowering the flow of capital investment. We present a stylized model of the capital stock to show how the domestic capital stock updates given the observed changes in investment. Capital stock is determined by a rule of motion where capital in time t is equal to the sum of non-depreciated capital from the previous period and investment from period t:

$$K_t = (1 - \delta)K_{t-1} + I_t.$$

[64] This results from $0.48 \times 2.63 \times 0.72$, where we again adjust for the supply-side share of the shock. See ADH's footnote 31 in page 2140 for a similar calculation on the average effect from 1990-2007.

δ is the exogenous depreciation rate of capital. Investment is taken to be a multiple of the steady state capital stock, denoted by the zero subscript.

$$I_t = \alpha K_0$$

Assuming that the firm is in a steady state before the repeal of §936, capital is the same every period, and the investment rate α is equal to the depreciation rate δ:

$$K_0 = (1-\delta)K_0 + \alpha K_0 \implies \alpha = \delta.$$

The empirical estimates presented in Tables 2 and 3 show that domestic investment decreases after the repeal of §963 through decreased total investment and decreased share of investment that is domestic. As discussed in Section 5, firms decrease domestic investment by 38%. Therefore, taking the new investment rate as $(1-0.38)\alpha$, the law of motion for capital becomes:

$$K_t = (1-\delta)K_{t-1} + (1-0.38)\alpha K_0.$$

Figure A.6 plots the trajectory for the capital stock of three different types of general industrial capital. Note that the figure assumes $K_0 = 1$ as a normalization, and uses BEA (2018) estimates of depreciation values for different types of capital. The figure showcases short, medium, and long duration assets for the general industry category. Buildings, which depreciate relatively slowly with a depreciation rate of 0.025, lose 8.4% of their value after 10 years. Machinery and Electronics (including computers) depreciate relatively faster — rates of 0.069 and 0.122, respectively — and lose 19.3% and 27.7% of their stock within ten years.

These results show that, while §936 has large and almost immediate effects on investment, the effects on the capital stock adjust slowly and lag the effects on investment depending on the depreciation rate.

E Effects of Repealing §936 on Firm Value

In this section we discuss an additional set of analyses that looks at the effects of news surrounding the repeal of §936 on market value. If investors believe the repeal of §936 will increase a firm's tax obligations, news of the repeal may reduce the stock price of exposed firms.[65] We look at

[65]Consistent with this hypothesis, Desai and Hines (2002) find that firms experience an increase in stock prices in response to the announcement that a firm will perform a corporate inversion. Note that Desai and Dharmapala (2009) argue that agency problems complicate the relation between tax planning and firm value. In addition, Johannesen and Stolper (2017) find that announcements that a firm has engaged in facilitating tax evasion have negative effects on stock prices.

the initial proposal to eliminate §936 as well as the date when the drafting of the legislation was announced. President Clinton first announced on February 16, 1993 that he wanted to limit the use of §936. Since this is the first mention of repeal, it is likely to have the largest effect on expectations. The US House of Representatives started writing the final bill to eliminate §936 on October 12, 1995.

We use an event study methodology (MacKinlay, 1997), where we estimate a regression explaining the return of firm f at date t, R_{ft}:

$$R_{ft} = X_t\beta_f + \gamma E_{ft}(k) + \epsilon_{ft},$$

where $X_t\beta_f$ includes market returns minus the risk free rate. γ is the parameter of interest and is equal to the Cumulative Abnormal Return (CAR) of firms exposed to §936.[66] Table A.21 reports results of this regression. Figure A.11 plots the results of this estimation where we pool both event dates. The graph shows that firms exposed to §936 saw CAR of –1.4% between days (0,12).

While we find a negative CAR on average, it is likely that more intangible intensive firms experience larger declines in values, as these are the kinds of firms that can benefit from profit shifting. Grubert and Slemrod (1998) suggest that firms with higher R&D intensity, advertising expenditures, and gross profits to sales are more likely to benefit from profit shifting. The intuition for the first two ratios is that firms that rely more on intangible capital will spend more on R&D and advertising. The intuition for the gross profits ratio is that a firm must first be profitable in order to shift its profits to low tax locations.

Table A.22 analyzes heterogeneous effects on firm value by regressing the CAR measures between 0-10 days on firm characteristics. The constant in this regression measures the average effect and the coefficient for each variable corresponds to an interaction term. Column (1) shows that R&D intensive firms saw a larger decline in firm value following these news events. This ratio is standardized so that a firm with a R&D intensity that is one standard deviation above the mean saw a CAR that was lower than the average by -0.47 percentage points, which is close to one third of the average effect. We do not find evidence that more profitable firms decreased in value following these news events; indeed we find the opposite. We include an indicator for having

[66] $E_{ft}(k)$ is an adjusted indicator for the event period that is k days long, so that $E_{ft}(k) = \frac{1}{k}$ for each of the k days following the event. We follow the event study implementation in Dube et al. (2011) to estimate all relevant parameters in one step. We use a period of 100 days before each event to establish the relationship between individual stock returns and the covariates in normal times.

had any advertising expenses and we find that this measure of intangible intensity has a negative effect on the CAR. We also control for the change in the effective tax rate at the firm level and for the fraction of employment in Puerto Rico, and we do not find significant interactions. Finally, the last column of this table shows that, as expected, the initial announcement that §936 would be repealed had a larger effect on firm values.[67]

Overall, these estimates suggest that investors viewed the repeal of §936 as an indication that firms' US tax obligations would increase, which would reduce stock prices. These results also speak to the degree to which investors view tax havens as substitutes. In a world where firms are able to shift profits across multiple tax havens and can substitute between them when one of them is eliminated, we would not expect to see any effects on firm value. Thus, our results suggest that firms are not able to switch between tax havens as seamlessly as one might have expected.

F Heterogeneous Effects of Repealing §936 on Local Employment Growth

In this section we explore the potential for the repeal of §936 to have heterogeneous effects on employment growth across local labor markets. We build on the specification of Table 7, and we allow for interactions of the exposure variable with other policies or shocks. We present detailed results in Tables A.24 and A.25 where we include interactions with each of these variables.

Panel A of Figure A.13 shows the interaction terms between each variable and the exposure variable. The two variables with the largest positive interaction terms are the corporate income tax rate and job creation incentives from Bartik (2017). The intuition for a positive interaction with job incentives is that these incentives may help unemployed workers find jobs sooner, which may limit the effects of a negative shock. The intuition for a positive interaction term with the corporate tax rate follows from understanding that in the equilibrium of a tax competition game, states with lower taxes attract more mobile firms which would also be more responsive to the repeal of §936. The two largest negative interaction terms are for trade exposure to NAFTA and for the share of routine workers. The intuition for these effects is that it is harder for local labor markets to recover when they are hit with multiple economic shocks.[68]

[67] We report additional results that show that these patterns are robust to controlling for the Fama and French (1993) 3 factors and momentum. Figure A.12 plots a similar graph to Figure A.11 and shows a smaller CAR of -.8%, and Table A.23 shows that heterogeneous results are robust to controlling for these four factors.

[68] Note that these interaction terms are not a form of bias for our main estimate. This specification controls for

Panel B of Figure A.13 shows how each of these interactions influence the marginal effect of exposure to §936 when these interaction terms take values above or below one standard deviation of the mean. The mean of the effects is about 8.0%, which is slightly larger than the effect without interactions. However, when we examine the distribution of marginal effects, we find a median effect that is very close to when we do not include these controls or interactions.

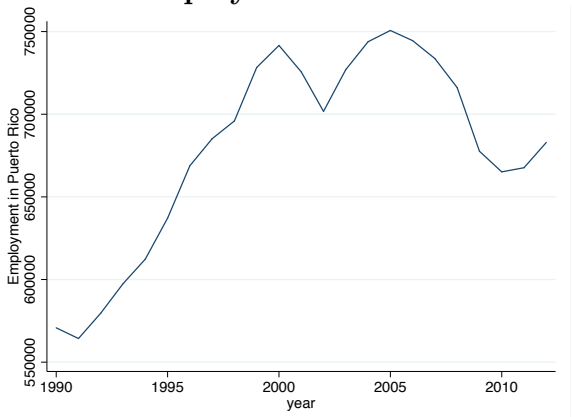
We summarize the heterogeneity across all of these interactions by computing the marginal effect for all counties in the specification that includes all of the interactions. Panel C of Figure A.13 plots the distribution of marginal effects across counties and shows that some states had much larger sensitivity to the repeal of §936. In particular, states with zero corporate income taxes, such as Texas and Nevada, are more affected by the repeal of §936. However, this figure also shows considerable heterogeneity.

Finally, Panel D of Figure A.13 takes a different approach to characterizing the heterogeneity in effects and plots estimates of quantile regressions. These estimates show that the repeal of §936 had larger effects on counties that were growing faster. This implies that, while the repeal of §936 saw declines in growth across all counties, it also a led to a compression in the distribution of growth rates.

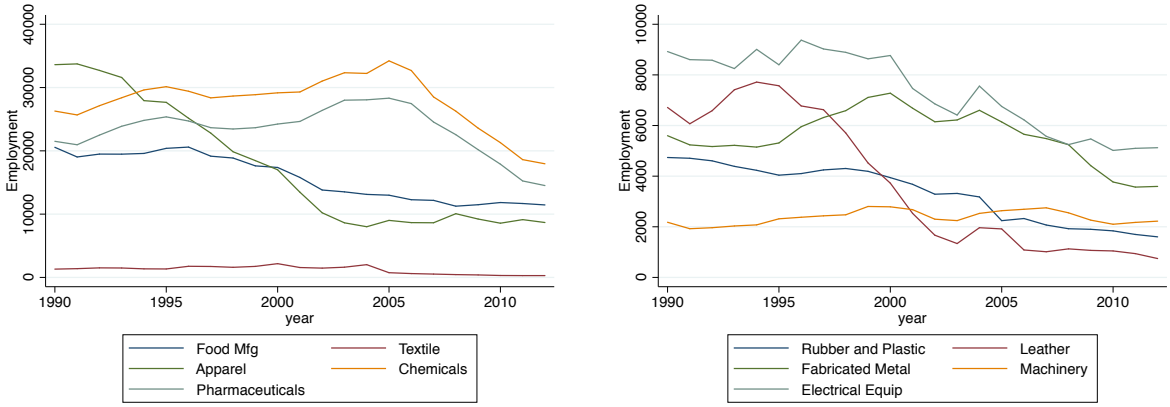
each variable and, as shown in Figure 13, controlling for both of these variables does not bias the main estimate.

Figure A.1: Employment in Puerto Rico by Industry

A. Total Employment in Puerto Rico



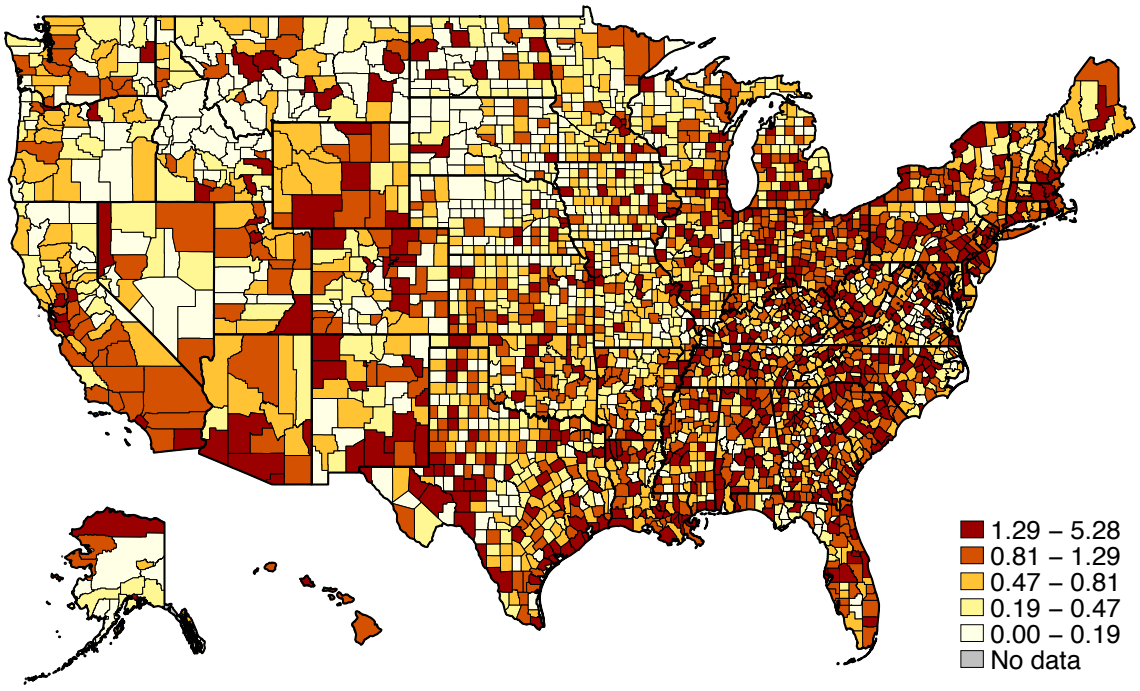
B. Employment in Puerto Rico by Industry



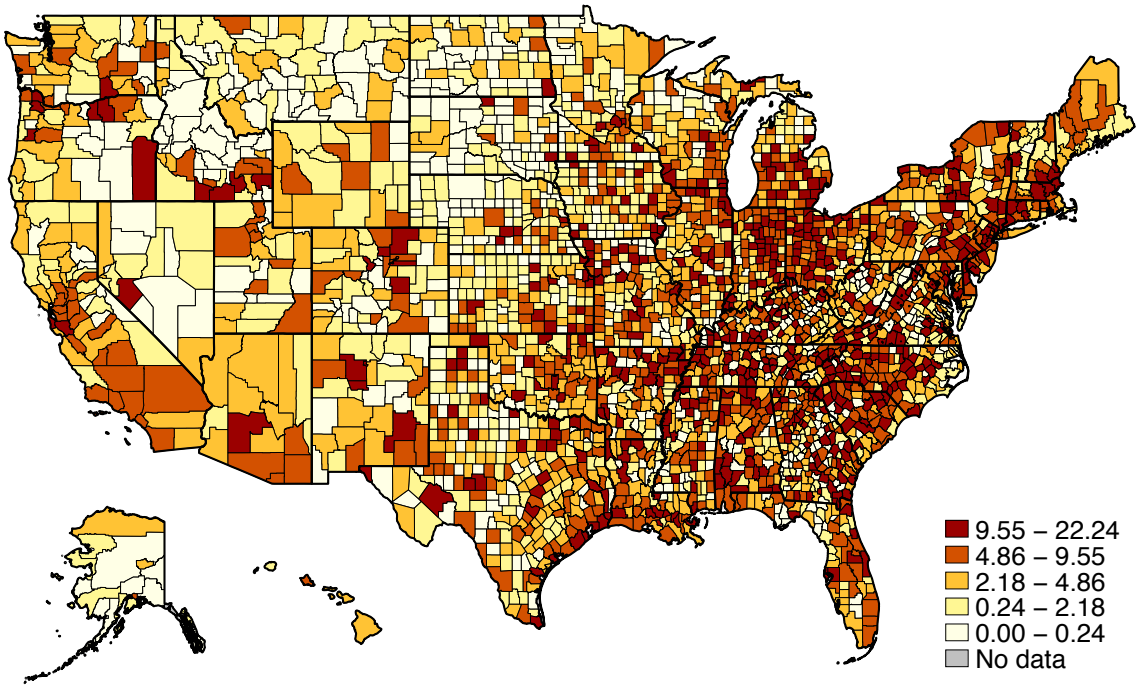
Notes: Author's calculations using data from NETS. This figure shows the total employment by all firms in Puerto Rico by industry from 1990 to 20012. Employment in Puerto Rico peaks in 2006 around 750,000 workers relative to 575,000 workers in 1990.

Figure A.2: Exposure to §936 in 1995 by Establishments and Employment

A. Exposure to §936 at the County Level, Establishments



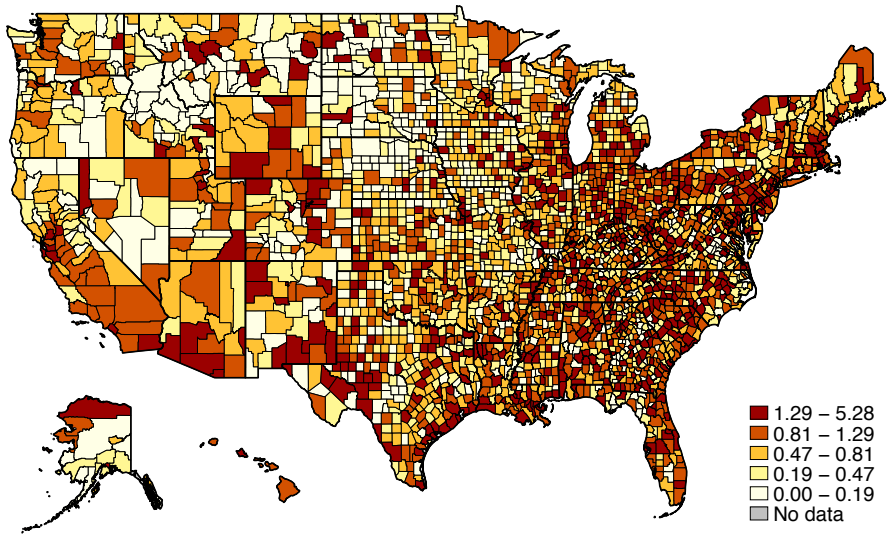
B. Exposure to §936 at the County Level, Employment



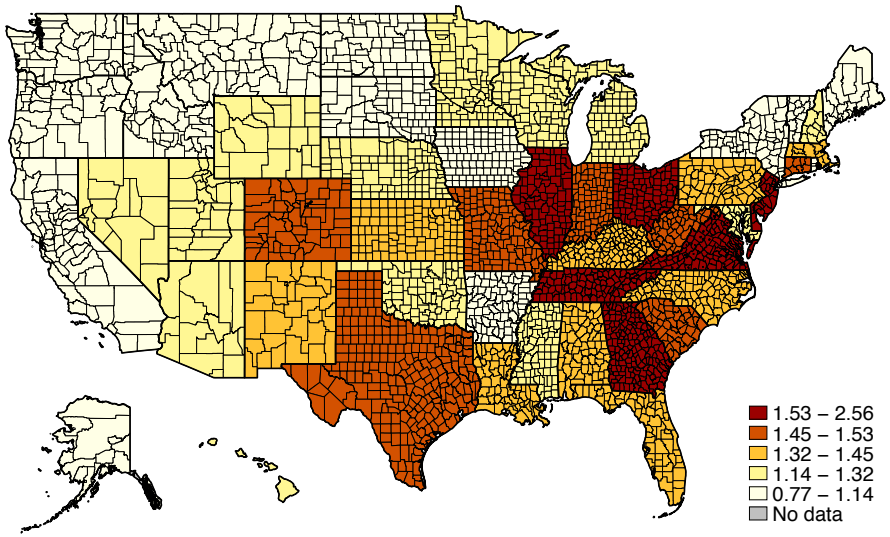
Notes: Author's calculations using data from NETS. This table shows the exposure variable calculated using employment instead of establishments. Panel A mirrors Figure 3 while panels B uses NETS employment data to generate links. For more information about the data and the exposure variable, see Section 4. This figure shows that the variation in exposure to Section 936 is robust to measurement, although we use the establishment definition in the main specifications because employment data in the NETS is likely subject to some mis-measurement (Barnatchez et al., 2017), the estimates of a regression using establishments links as an instrument for employment links are shown in Table A.9.

Figure A.3: Distribution of Exposure to §936 in 1995

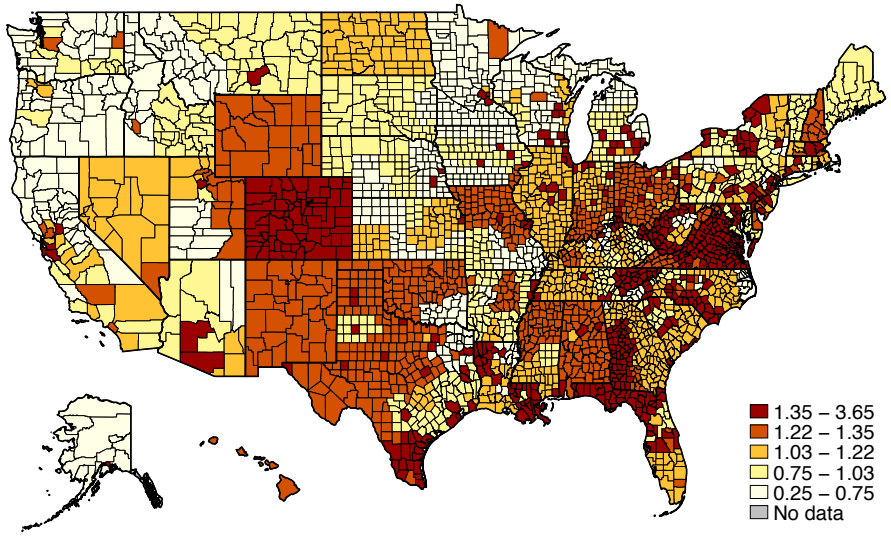
A. Exposure to §936 at the County Level



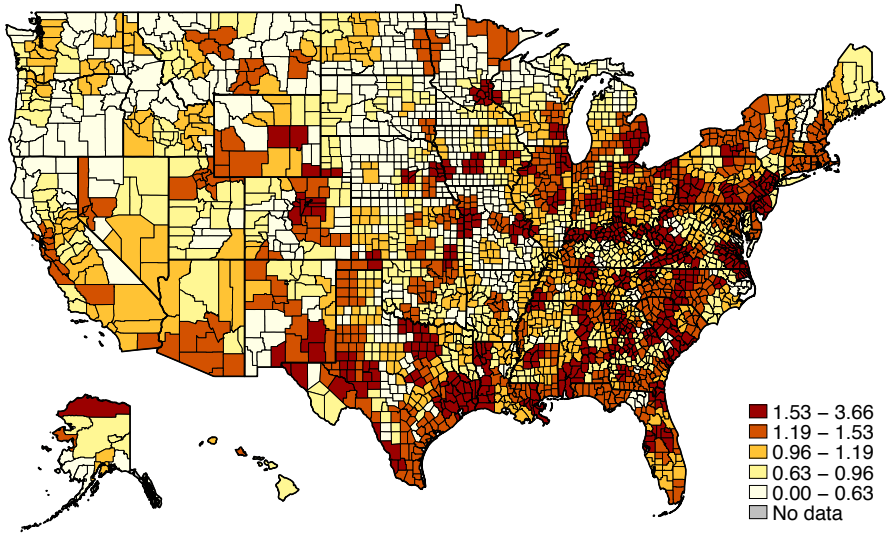
B. Exposure to §936 at the State Level



C. Exposure to §936 at the Conspuma Level

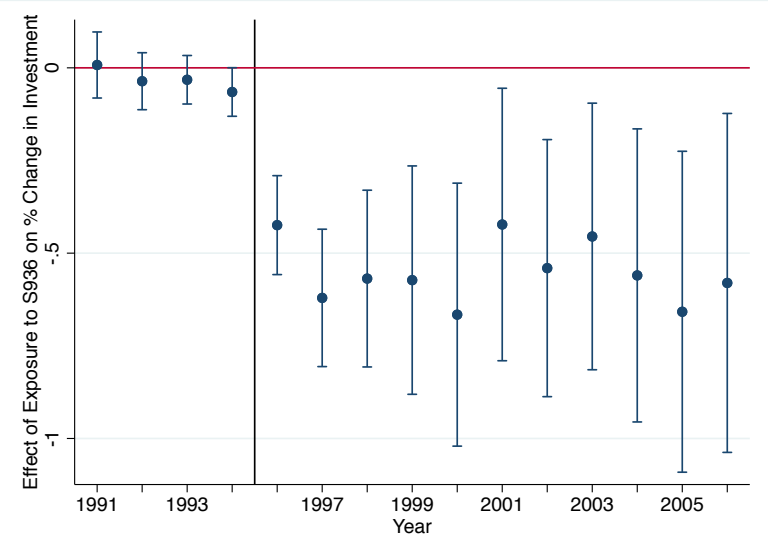


D. Exposure to §936 at the Commuting Zone Level



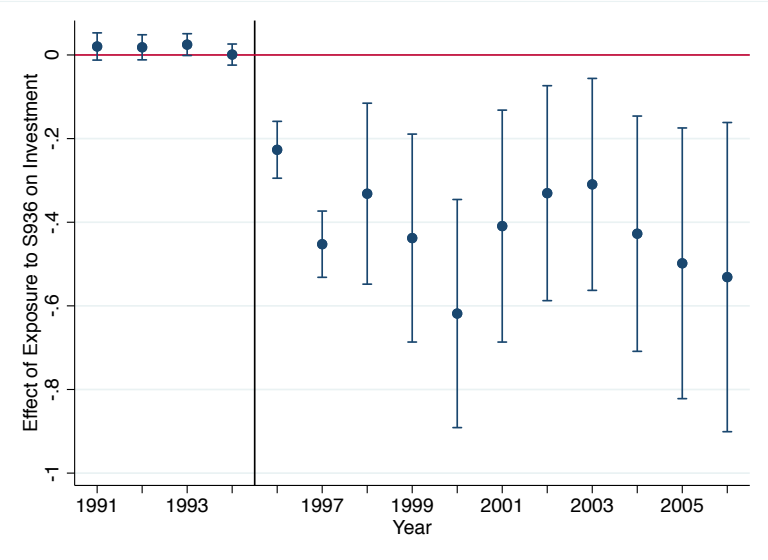
Notes: Author's calculations using data from NETS. This panel shows the establishment exposure to Section 936 aggregated to different geographic levels. Panel A mirrors Figure 3 while panels B, C, and D show the exposure at the State, Conspuma, and Commuting Zone levels, respectively. For more information about the data and the exposure variable, see Section 4. This figure shows that there is still substantial variation in exposure to §936 even with higher levels of aggregation beyond counties, which are used as the main level of analysis.

Figure A.4: Effect of Repealing §936 on the Firm-Level Investment Rate: Percentage Change



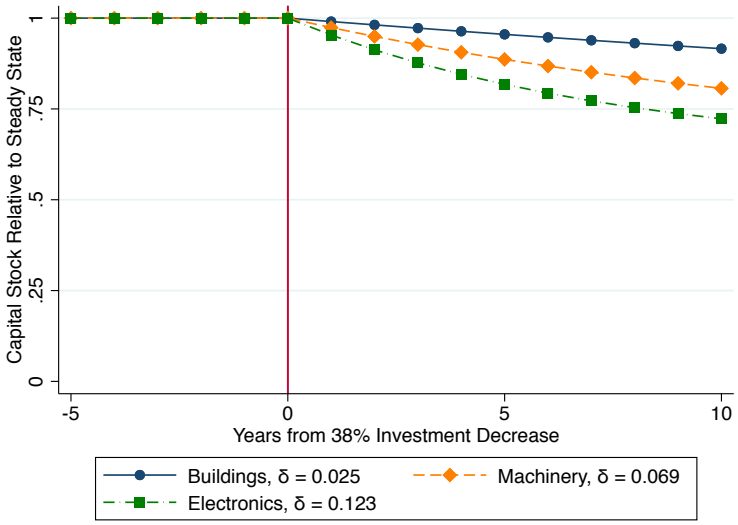
Notes: Author's calculations using data from COMPUSTAT. This figure shows estimates of Equation 2 where the dependent variable is the percent change in investment relative to the pre-period, and where exposure to §936 is an indicator at the firm level. Investment is defined as capital expenditures. Blue lines span a 95% confidence interval. Consistent with the hypothesis that multinationals decreased investment, we see that exposed firms saw a decrease in investment following the repeal of §936 relative to non-exposed firms. Point estimates and additional specifications are shown in Table A.4. Standard errors are clustered at the firm level.

Figure A.5: Effect of Repealing §936 on the Firm-Level Investment Rate: Winsorized at 1%



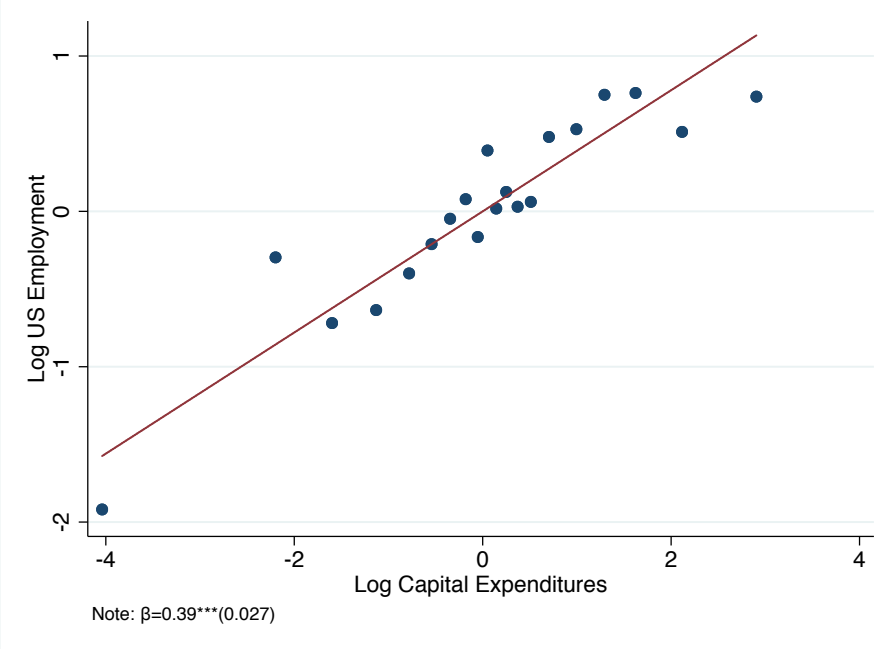
Notes: Author's calculations using data from COMPUSTAT. This figure shows estimates of Equation 2 where the dependent variable is investment divided by average capital in 1990 to 1995 and exposure to §936 is an indicator at the firm level. Investment is defined as capital expenditures and capital is defined as plants property and equipment. Blue lines span a 95% confidence interval. Consistent with the hypothesis that multinationals decreased investment, we see that exposed firms saw a decrease in investment following the repeal of §936 relative to non-exposed firms. Point estimates and additional specifications are shown in Table A.5. Standard errors are clustered at the firm level. The investment divided by capital is winsorized at the 1% level.

Figure A.6: Effect of 38% Decrease on Investment on Capital Stock by Type of Capital



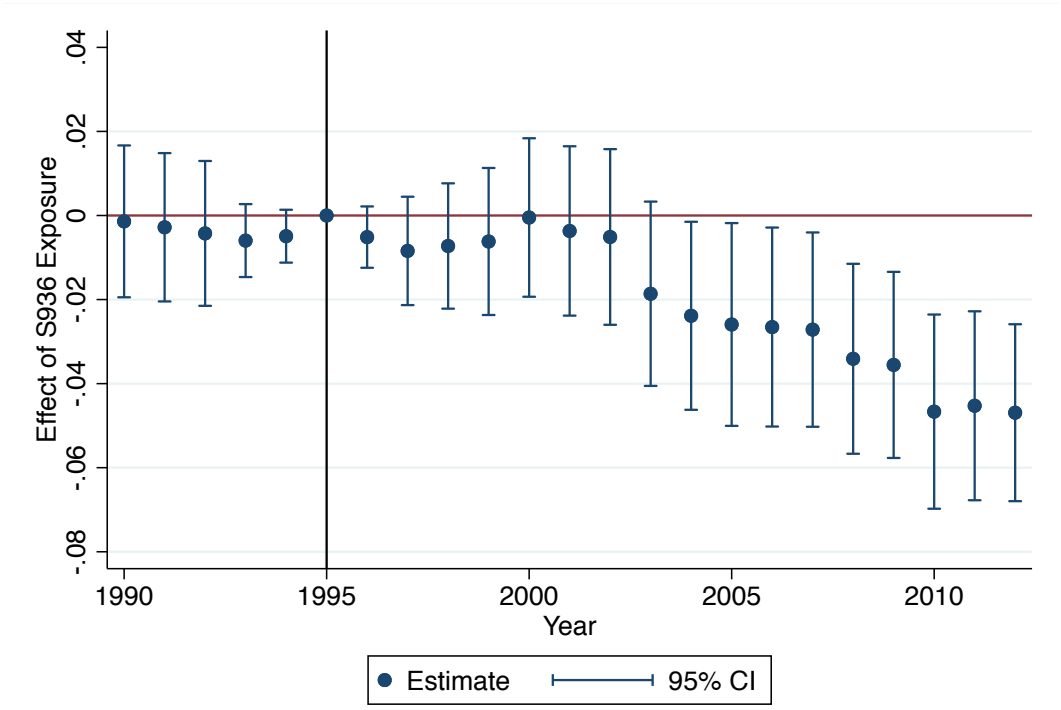
Notes: Author's calculations of the derivation shown in Appendix D calibrated using data from BEA (2018). This figure shows the trajectory of capital stock for different types of capital at a general industrial firm. We assume the capital stock is in a steady state before year 0 and that investment decreases to 38% of its steady state value after the reform. Buildings, which depreciate relatively slowly with a depreciation rate of 0.025, lose 8.4% of their value after 10 years. Machinery and Electronics (including computers) depreciate relatively faster — rates of 0.069 and 0.122, respectively — and lose 19.3% and 27.7% of their stock after 10 years.

Figure A.7: Relation Between Log Employment and Log Capital Expenditures



Notes: Author's calculations using data from COMPUSTAT and NETS. This figure plots the results of a regression of log US employment at the firm level on log capital expenditures. It shows that an increase in 1% of capital expenditures is associated with an increase in 0.39% in US employment. We use this estimated relation to benchmark the magnitudes of the employment and investment effects in Section 5.

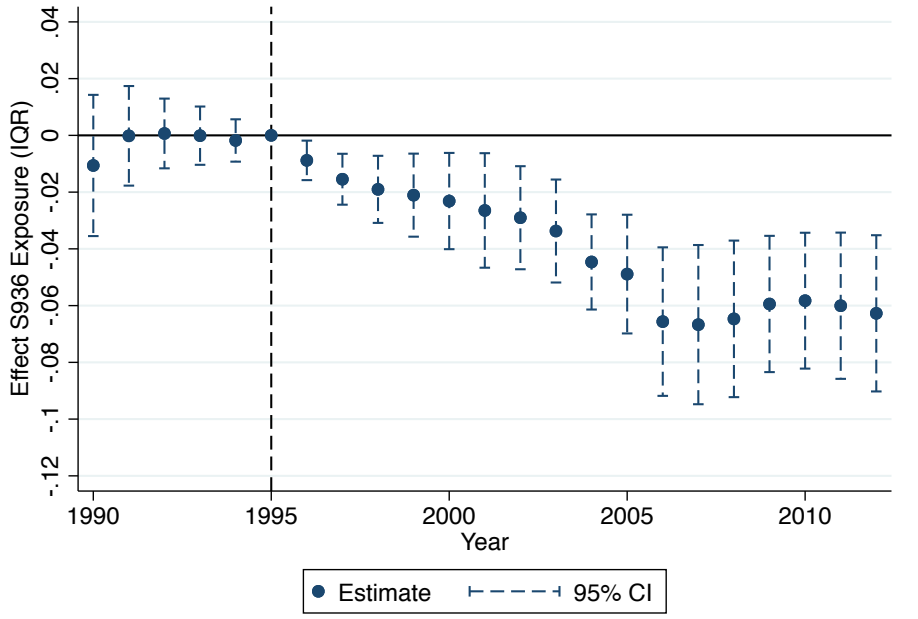
Figure A.8: Effects of Repealing of §936 on Number of Establishments in Affected Firms



Notes: Author's calculations using data from NETS. This figure shows the decline in establishments at §936 exposed firms relative to similar control firms without exposure to §936. See Appendix B.3 for a description of the procedure used to identify the comparison firms. Point estimates are displayed in Table A.6. By 2006, firms exposed to §936 had 2.7% less establishment growth than firms that were otherwise similar in 1995. See Section 5 for more discussion. Standard errors are clustered at the firm level.

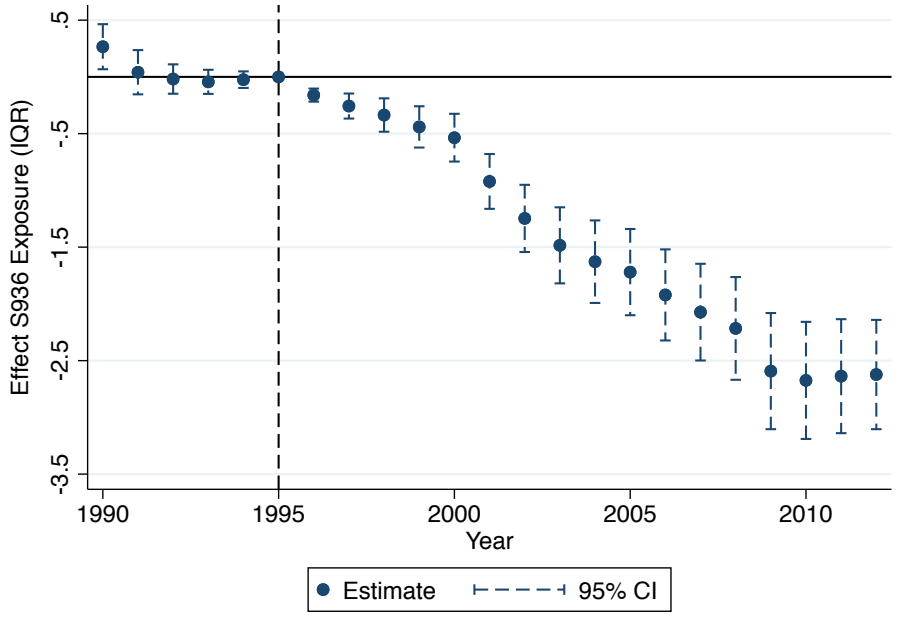
Figure A.9: Robustness Using Variable Definitions from Autor et al. (2013)

A. Effect of §936 Exposure on Employment-to-Pop Ratio in Manufacturing (% change)



Notes: $(Epop_{ict}-Epop_{ic1995})/Epop_{ic1995}=\alpha_c+\gamma_{it}+\beta_t S936\ Exposure_c+\varepsilon_{ict}$. SE's clustered by State and Indu

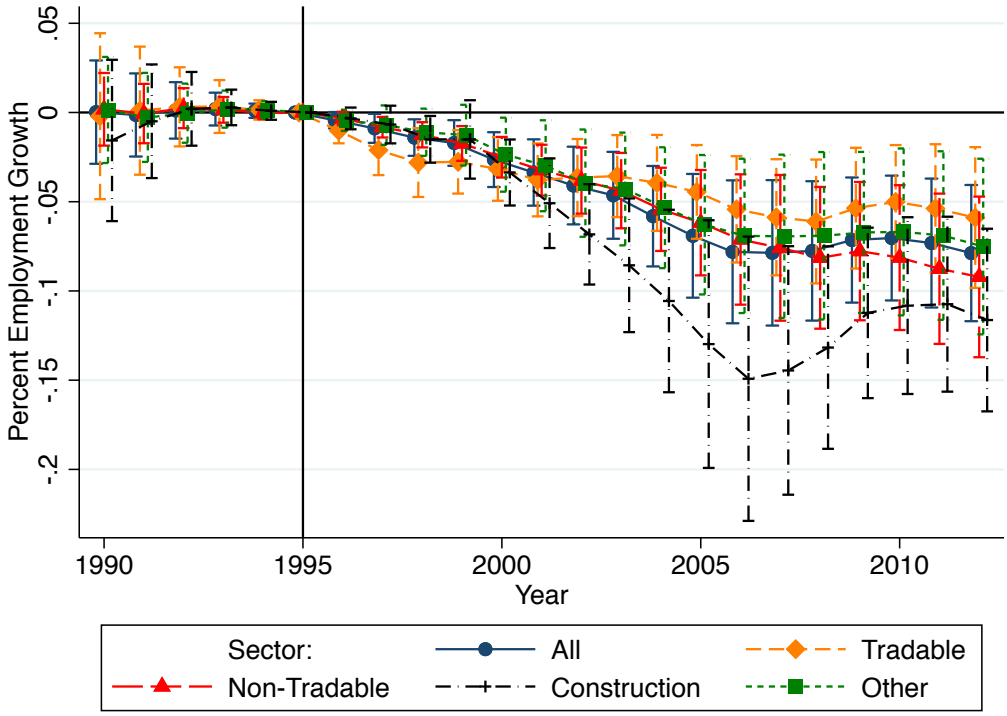
B. Effect of §936 Exposure on Employment-to-Pop Ratio in Manufacturing (level change)



Notes: $Epop_{ict}-Epop_{ic1995}=\alpha_c+\gamma_{it}+\beta_t S936\ Exposure_c+\varepsilon_{ict}$. SE's clustered by State and Industry.

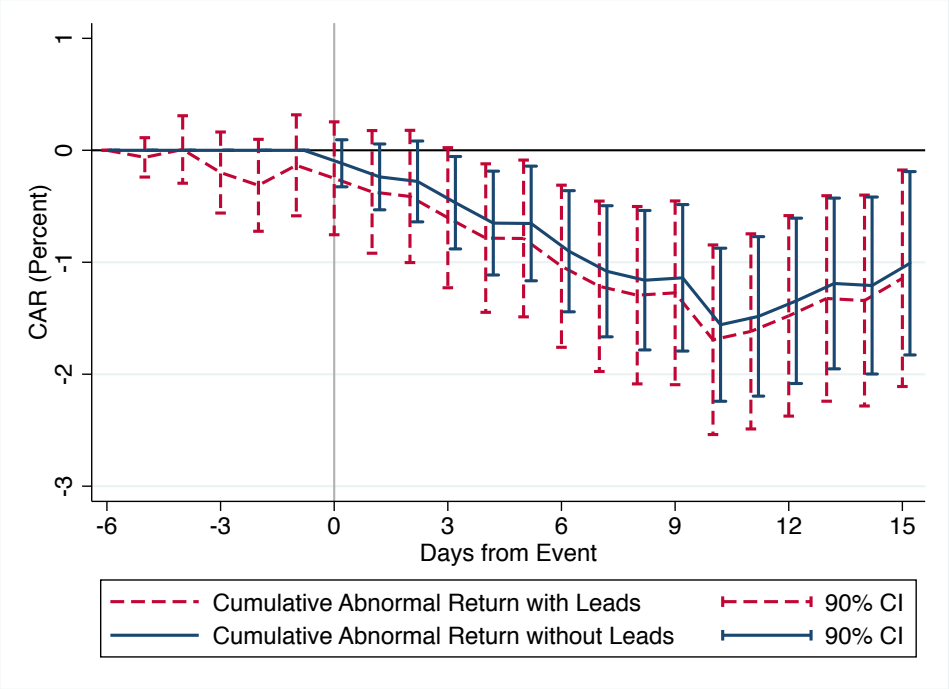
Notes: Author's calculations using data from NETS, QCEW, and the Census. See Tables A.7 and A.8 for regression coefficients and more specifications. These figures estimate the effect that exposure to §936 has on the proportion of working age adults who work in the manufacturing sector. Autor et al. (2013) introduces this variable as a potential avenue for assigning dollar values to shocks that influence local labor markets. The top panel show that increasing §936 exposure from the 25^{th} percentile to the 75^{th} percentile will decrease the percent of working age adults with manufacturing jobs by about 6% and the lower panel shows that proportion decreases by more than 2.5 percentage points. Standard errors are clustered at the state level.

Figure A.10: Heterogeneous Effects of Repealing of §936 on Employment Growth by Sector, with Confidence Intervals



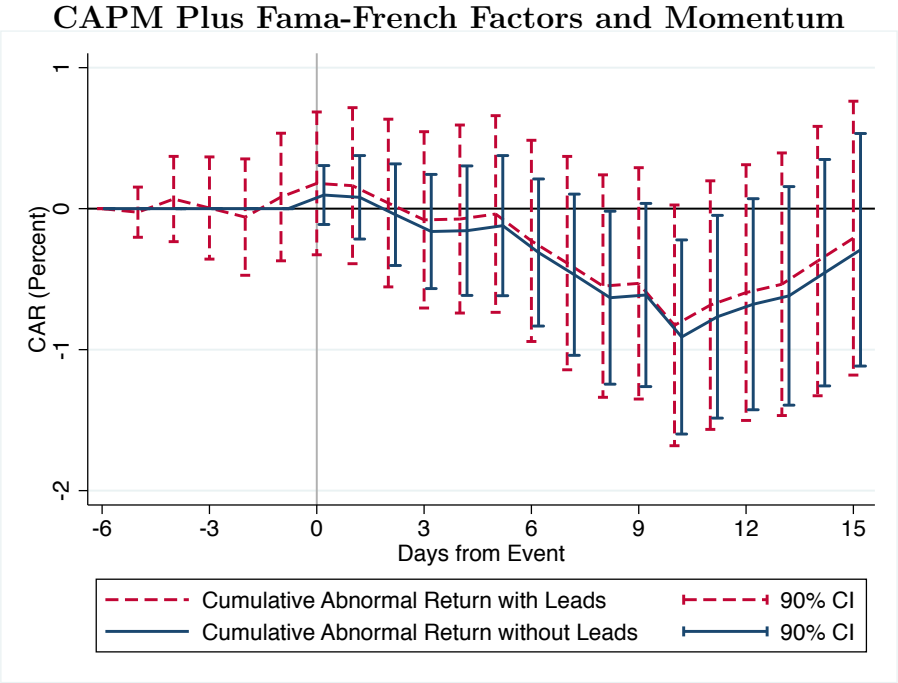
Notes: Author's calculations using data from NETS and QCEW and sector definitions from Mian and Sufi (2014). This figure shows the average effect and the heterogeneous effects by sector with confidence intervals corresponding to Figure 14. Estimates and standard errors are displayed in Table A.17. This figure highlights that the tradable sector sees a decline in employment growth before any of the other sectors. See Appendix B for a discussion of the data and Section 6 for a discussion of the results. Standard errors are clustered at the state and industry levels. Error bars show 95% confidence intervals. County observations are weighted according to employment in 1995.

Figure A.11: Repealing §936 Reduced Firm Value of Exposed Firms



Notes: Author's calculations using data from COMPUSTAT and CRSP. The event study takes place on two dates regarding the repeal of the Possessions Tax Credit: February 16, 1993 and October 12, 1995. Cumulative abnormal returns are calculated for firms with exposure to the Possessions Tax Credit. For more information about the events see Appendix E. This plots the results of Table A.21 for all intermediate dates in [-6,15]. It shows that between days (0,12) firms exposed to §936 experienced a CAR of -1.4%. The event study results are shown with additional specifications in Table A.22 that include interactions with firm characteristics. As a robustness check, Figure A.12 shows a specification that adjusts for the Fama and French (1993) 3 factors and momentum and finds a similar result. Standard errors are robust.

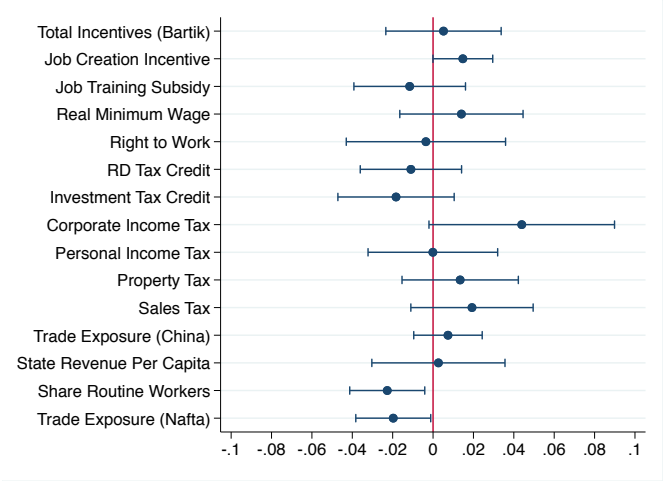
Figure A.12: Repealing §936 Reduced Firm Value of Exposed Firms: Robustness



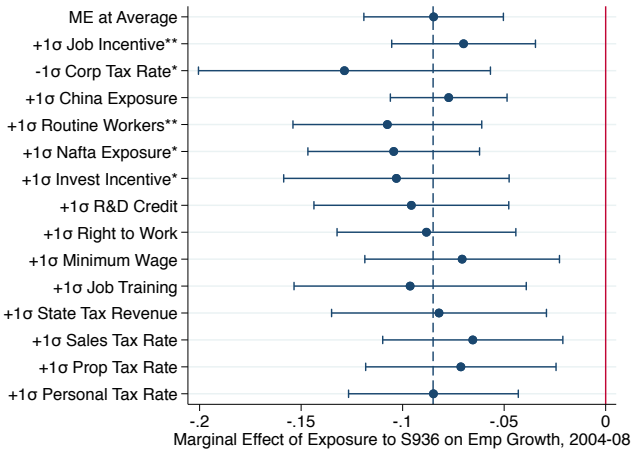
Notes: Author's calculations using data from COMPUSTAT and CRSP. This plots the results of Table A.21 for a wider range of dates. It shows that between days (0,12) firms exposed to §936 adjusting for the Fama and French (1993) 3 factors and momentum experienced a CAR of -.8%. The primary specification without Fama-French Factors or Momentum is shown in Figure A.11. Standard errors are robust.

Figure A.13: Heterogeneous Employment Effects of Repealing of §936

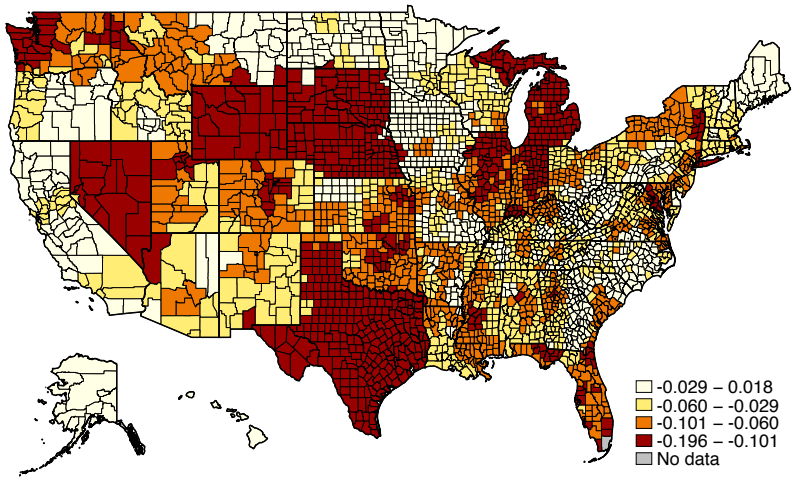
A. Interaction Terms



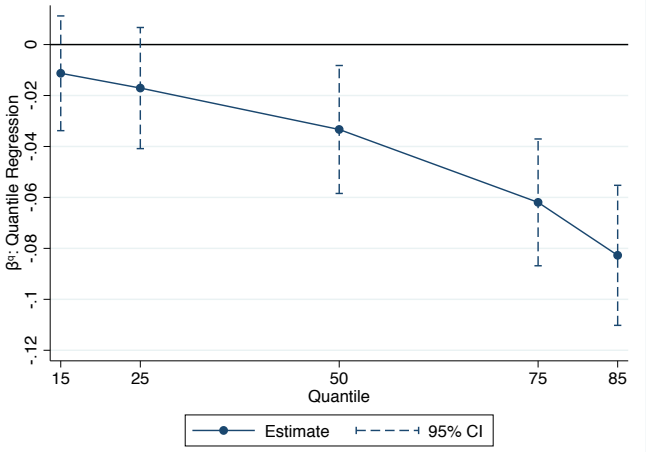
B. Effects of Interactions on Marginal Effect



C. Geographic Distribution of Marginal Effects



D. Quantile Regression Estimates



Notes: Author's calculations using data from NETS and QCEW. Panel A plots the individual interaction terms. Panel B plots the marginal effects evaluated at a one standard deviation change from the average. The marginal effect evaluated at the mean is about 8%. The median effect across counties is close to the effect we find when we do not include these controls or interactions. Panel C plots the distribution of marginal effects across counties and shows that some states had much larger sensitivity to the repeal of §936. Panel D takes a different approach to characterizing the heterogeneity in effects and plots estimates of quantile regressions. These estimates show that the repeal of §936 had larger effects on counties that were growing faster. This implies that, while the repeal of §936 saw declines in growth across all counties, it also a led to a compression in the distribution of growth rates. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.1: Counties with Notable Section 936 Exposure in 1995

Rank	County	S936 Est Exposure	County	S936 Emp Exposure
1	Livingston County, Michigan	0.003	El Dorado County, California	0.007
2	Sussex County, New Jersey	0.004	Sussex County, New Jersey	0.008
3	Hernando County, Florida	0.004	San Luis Obispo County, California	0.014
4	Kings County, New York	0.004	Bronx County, New York	0.015
5	El Dorado County, California	0.004	Hernando County, Florida	0.016
6	San Luis Obispo County, California	0.004	Livingston County, Michigan	0.018
7	Bronx County, New York	0.005	Yakima County, Washington	0.018
8	Tolland County, Connecticut	0.005	Whatcom County, Washington	0.018
9	Marin County, California	0.005	Jackson County, Oregon	0.019
10	Yavapai County, Arizona	0.005	Barnstable County, Massachusetts	0.020
448	Mecklenburg County, North Carolina	0.028	Mercer County, New Jersey	0.257
449	Vanderburgh County, Indiana	0.029	Durham County, North Carolina	0.258
450	DuPage County, Illinois	0.029	Trumbull County, Ohio	0.262
451	Hampton city, Virginia	0.030	Forsyth County, North Carolina	0.267
452	Shelby County, Tennessee	0.030	Lake County, Illinois	0.274
453	St. Louis city, Missouri	0.030	Brazoria County, Texas	0.280
454	Bibb County, Georgia	0.031	Hartford County, Connecticut	0.296
455	New Castle County, Delaware	0.032	Somerset County, New Jersey	0.312
456	Henrico County, Virginia	0.036	Aiken County, South Carolina	0.322
457	Bergen County, New Jersey	0.037	Genesee County, Michigan	0.342

Notes: This table lists the top and bottom 10 counties in terms of exposure to Section 936 through percent of linked establishments (S936 Est Exposure) and percent of linked employment (S936 Emp Exposure). The sample of counties for inclusion in this table is restricted to those with more than 100,000 population in 1995. Data come from NETS (Walls & Associates, 2012).

Table A.2: Puerto Rico Link, Demographic Characteristics, and Economic Polices

	(1) Min Wage	(2) Right to Work	(3) Pers. Income Tax	(4) Sales Tax	(5) Prop. Tax
Exposure to Section 936	-0.174	-0.050	-0.009	-0.081	-0.008
	(0.093)	(0.084)	(0.126)	(0.115)	(0.061)
	0.070	0.560	0.943	0.485	0.902
	(1) Corporate Tax	(2) R&D Credit	(3) State Revenue/GDP	(4) Trade Exposure (China)	(5) Share of Routine Labor
Exposure to Section 936	-0.033	-0.103	-0.123	-0.042	-0.003
	(0.098)	(0.078)	(0.092)	(0.032)	(0.098)
	0.741	0.200	0.190	0.199	0.975
	(1) LFPR	(2) % Retail	(3) % Agriuclture	(4) % Manuf. Durable	(5) % Manuf. Non-Durable
Exposure to Section 936	-0.007	0.090	-0.049	-0.052	-0.011
	(0.111)	(0.093)	(0.061)	(0.070)	(0.044)
	0.950	0.340	0.430	0.467	0.813
	(1) Capital Stock	(2) % College	(3) % Less HS	(4) % White	(5) % Black
Exposure to Section 936	-0.141	0.282	0.029	-0.110	0.080
	(0.143)	(0.124)	(0.105)	(0.308)	(0.257)
	0.329	0.027	0.783	0.723	0.756

Notes: This table shows correlations between the Exposure to Section 936 and other policy variables at the county level. Robust standard errors are shown in parentheses and p-values are displayed below standard errors. Exposure to Section 936 is calculated using data from NETS. Economic policies and other local characteristics come from the Bureau of Economic Analysis and the Bureau of Labor Statistics as gathered by Suárez Serrato and Wingender (2016), import exposure and routine labor shares come from Autor et al. (2016), and industry proportions are calculated from QCEW (2017). For more information on the calculation see Section 4 and for more information about the data see Appendix B.

Table A.3: Effect of Repealing §936 on the Federal Taxes Paid as a Percent of Pretax Income

Change in Federal Taxes Paid as a Percent of Pretax Income					
	(1)	(2)	(3)	(4)	(5)
Exposure to Section 936 X Post	0.039	0.033	0.031	0.072	0.069
	(0.021)	(0.024)	(0.024)	(0.034)	(0.034)
	0.063	0.173	0.194	0.032	0.040
Observations	34568	34568	33775	21643	21278
Year Fixed Effects	Y	Y	Y	Y	Y
Firm Fixed Effects		Y	Y	Y	Y
S936 Exposed Industry			Y		Y
Major Sectors Only				Y	Y

Notes: Author's calculations using data from COMPUSTAT. This table estimates a version of Equation 2 where the dependent variable is cash federal taxes paid divided by US pretax income. These estimates reflect the percent change in federal taxes paid as a percent of pretax income at firms exposed to Section 936 after 1995. The last column shows that federal taxes paid as a percent of US pretax income increased by 6.9pp at exposed firms after the repeal of §936. We use the share of revenue originating in the US as a proxy for the share of pretax income originating in the domestic market, which is 75% on average, and the sample is restricted to those firms with non-trivial pretax income. The estimated change in effective tax rate is similar to the tax increase estimates of 7.3-10.5pp discussed in Section 5. Compared to the effects of §936 on investment in Table 2, these estimates imply semi-elasticities of investment to the corporate tax rate of -2.9 for column (1) and of -3.3 for column (5). See Section 5 for additional discussion and Appendix B for more information about the data and variables. Clustered standard errors at the firm level are shown in parentheses with p-values below.

Table A.4: Effect of Repealing §936 on the Percent Change in Investment of Exposed Firms

Percent Change in Investment: $\frac{I}{I_1990-1995} - 1$					
	(1)	(2)	(3)	(4)	(5)
Exposure to Section 936 X Post	-0.469	-0.467	-0.475	-0.743	-0.761
	(0.173)	(0.151)	(0.151)	(0.155)	(0.155)
	0.007	0.002	0.002	0.000	0.000
Observations	78698	78698	76217	52727	51352
Sample Average I/K in 2005 Relative to 1995	3.814	3.814	3.818	3.553	3.551
Percent of 2005 Average	12.3%	12.3%	12.5%	20.9%	21.4%
Semi-elasticity of Investment	1.17	1.17	1.19	2.00	2.05
Year Fixed Effects	Y	Y	Y	Y	Y
Firm Fixed Effects		Y	Y	Y	Y
S936 Exposed Industry			Y		Y
Major Sectors Only				Y	Y

Notes: Author's calculations using data from COMPUSTAT. This table estimates Equation 2 where the dependent variable is the percent change in investment relative to investment in 1990-1995 winsorized at the 5% level. The estimates in this table correspond to a pooled version of the regression displayed in Figure A.4. These estimates reflect the percent change in investment at firms exposed to Section 936 after 1996 and show robustness to the variable measurement in the preferred specifications displayed in Table 2. See Section 5 for discussion and Appendix B for more information about the data and variables. Standard errors clustered at the firm level are shown in parentheses with p-values below.

Table A.5: Effect of Repealing §936 on the Investment Relative to Capital of Exposed Firms, Winsorized at 1%

Change in Investment: $\frac{I}{K_1990-1995}$	(1)	(2)	(3)	(4)	(5)
Exposure to Section 936 X Post	-0.400	-0.420	-0.431	-0.543	-0.561
	(0.132)	(0.098)	(0.099)	(0.110)	(0.111)
	0.002	0.000	0.000	0.000	0.000
Observations	79393	79393	76868	52896	51503
Sample Average I/K in 2005	1.773	1.773	1.796	1.608	1.626
Percent of 2005 Average	22.5%	23.7%	24.0%	33.8%	34.5%
Semi-elasticity of Investment	2.15	2.26	2.29	3.23	3.29
Year Fixed Effects	Y	Y	Y	Y	Y
Firm Fixed Effects		Y	Y	Y	Y
S936 Exposed Industry			Y		Y
Major Sectors Only				Y	Y

Notes: Author's calculations using data from COMPUSTAT. This table estimates Equation 2 where the dependent variable is investment divided by average capital in 1990-1995 winsorized at the 1% level. The estimates in this table correspond to a pooled version of the regression displayed in Figure A.5. These estimates reflect the percent change in investment at firms exposed to Section 936 after 1996. See Section 5 for more discussion and Appendix B for more information about the data and variables. The same regressions with the dependent variable winsorized at the 5% level are shown in Table 2. Standard errors clustered at the firm level are shown in parentheses with p-values below.

Table A.6: Event Study on Employment Growth and Establishment Growth between Firms Exposed to §936 and Non-Exposed Firms

Exposure to Section 936	(1) Employment Growth	(2) Establishment Growth
X 1990	-0.006	-0.001
	(0.010)	(0.009)
X 1991	-0.008	-0.003
	(0.009)	(0.009)
X 1992	-0.011	-0.004
	(0.008)	(0.009)
X 1993	-0.007	-0.006
	(0.006)	(0.004)
X 1994	-0.001	-0.005
	(0.003)	(0.003)
X 1996	-0.010	-0.005
	(0.007)	(0.004)
X 1997	-0.014	-0.008
	(0.011)	(0.007)
X 1998	-0.021	-0.007
	(0.011)	(0.008)
X 1999	-0.022	-0.006
	(0.012)	(0.009)
X 2000	-0.022	-0.000
	(0.013)	(0.010)
X 2001	-0.029*	-0.004
	(0.014)	(0.010)
X 2002	-0.037*	-0.005
	(0.016)	(0.011)
X 2003	-0.046**	-0.019
	(0.016)	(0.011)
X 2004	-0.060***	-0.024*
	(0.017)	(0.011)
X 2005	-0.066***	-0.026*
	(0.017)	(0.012)
X 2006	-0.072***	-0.027*
	(0.017)	(0.012)
X 2007	-0.074***	-0.027*
	(0.017)	(0.012)
X 2008	-0.083***	-0.034**
	(0.017)	(0.012)
X 2009	-0.088***	-0.036**
	(0.016)	(0.011)
X 2010	-0.093***	-0.047***
	(0.016)	(0.012)
X 2011	-0.091***	-0.045***
	(0.016)	(0.011)
X 2012	-0.091***	-0.047***
	(0.015)	(0.011)

Notes: This table displays estimates from regressions of employment growth and establishment growth on an indicator for firm level exposure to Section 936 interacted with year dummies corresponding to Panel B of Figure 8 for column (1) and Figure A.8 for column (2). Data on the exposure to Section 936, employment growth, and establishment growth come from the NETS. See Section 5 for discussion of results and Appendix B for more information about the data. Standard errors clustered at the firm level are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A.7: Event Study on the Ratio of Manufacturing Employment to Working Age Population at the County Level, Percentage Change

Exposure to Section 936	(1)	(2)	(3)	(4)	(5)
X 1990	-0.011	-0.011	0.015	-0.005	-0.011
	(0.013)	(0.013)	(0.010)	(0.012)	(0.013)
X 1991	-0.000	-0.000	0.018*	0.004	-0.000
	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)
X 1992	0.001	0.001	0.010	0.004	0.001
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
X 1993	-0.000	-0.000	0.003	0.002	-0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
X 1994	-0.002	-0.002	-0.002	-0.001	-0.002
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
X 1996	-0.009*	-0.009*	-0.001	-0.009**	-0.009*
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)
X 1997	-0.015**	-0.015**	-0.005	-0.016***	-0.015**
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)
X 1998	-0.019**	-0.019**	-0.009*	-0.019***	-0.019**
	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)
X 1999	-0.021**	-0.021**	-0.015**	-0.022**	-0.021**
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)
X 2000	-0.023*	-0.023*	-0.016*	-0.025***	-0.023*
	(0.009)	(0.009)	(0.007)	(0.007)	(0.009)
X 2001	-0.026*	-0.026*	-0.016	-0.029**	-0.026*
	(0.010)	(0.010)	(0.009)	(0.008)	(0.010)
X 2002	-0.029**	-0.029**	-0.022*	-0.030***	-0.029**
	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)
X 2003	-0.034***	-0.034***	-0.029**	-0.035***	-0.034***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)
X 2004	-0.045***	-0.045***	-0.037***	-0.047***	-0.045***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)
X 2005	-0.049***	-0.049***	-0.040***	-0.051***	-0.049***
	(0.011)	(0.011)	(0.010)	(0.009)	(0.011)
X 2006	-0.066***	-0.066***	-0.050***	-0.069***	-0.066***
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)
X 2007	-0.067***	-0.067***	-0.050***	-0.070***	-0.067***
	(0.014)	(0.014)	(0.012)	(0.013)	(0.014)
X 2008	-0.065***	-0.065***	-0.047***	-0.068***	-0.065***
	(0.014)	(0.014)	(0.012)	(0.013)	(0.014)
X 2009	-0.059***	-0.059***	-0.039***	-0.061***	-0.059***
	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)
X 2010	-0.058***	-0.058***	-0.042***	-0.060***	-0.058***
	(0.012)	(0.012)	(0.010)	(0.012)	(0.012)
X 2011	-0.060***	-0.060***	-0.046***	-0.063***	-0.060***
	(0.013)	(0.013)	(0.011)	(0.012)	(0.013)
X 2012	-0.063***	-0.063***	-0.050***	-0.066***	-0.063***
	(0.014)	(0.014)	(0.011)	(0.013)	(0.014)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects		Yes		Yes	Yes
Year-by-State Fixed Effects			Yes		
Winsorized Weights				Yes	
Drops Small County-Industries (<1000)					Yes

Notes: This table displays estimates from regressions of percentage change in percent of working age population working in manufacturing on exposure to Section 936 interacted with year dummies corresponding to Table 5. This regression is designed after the analysis of Autor et al. (2013). Exposure to Section 936 comes from the NETS and employment growth comes from QCEW (2017). See Section 5 for discussion and Appendix B for more information about the data. Standard errors clustered at the state and industry levels are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Observations are weighted by employment in 1995.

Table A.8: Event Study on the Ratio of Manufacturing Employment to Working Age Population at the County Level, Percentage Points

Exposure to Section 936	(1)	(2)	(3)	(4)	(5)
X 1990	0.266*	0.266*	0.456***	0.302**	0.266*
	(0.101)	(0.101)	(0.129)	(0.097)	(0.102)
X 1991	0.041	0.041	0.224	0.071	0.041
	(0.100)	(0.100)	(0.113)	(0.096)	(0.100)
X 1992	-0.019	-0.019	0.102	0.009	-0.019
	(0.066)	(0.066)	(0.078)	(0.064)	(0.066)
X 1993	-0.044	-0.044	0.008	-0.031	-0.044
	(0.054)	(0.054)	(0.050)	(0.054)	(0.054)
X 1994	-0.024	-0.024	-0.009	-0.021	-0.024
	(0.037)	(0.037)	(0.030)	(0.038)	(0.037)
X 1996	-0.161***	-0.161***	-0.061	-0.160***	-0.161***
	(0.029)	(0.029)	(0.032)	(0.029)	(0.029)
X 1997	-0.257***	-0.257***	-0.113*	-0.256***	-0.257***
	(0.056)	(0.056)	(0.047)	(0.056)	(0.056)
X 1998	-0.336***	-0.336***	-0.172**	-0.335***	-0.337***
	(0.075)	(0.075)	(0.051)	(0.075)	(0.075)
X 1999	-0.441***	-0.441***	-0.287***	-0.436***	-0.441***
	(0.093)	(0.093)	(0.050)	(0.095)	(0.093)
X 2000	-0.536***	-0.536***	-0.346***	-0.545***	-0.537***
	(0.107)	(0.107)	(0.067)	(0.108)	(0.108)
X 2001	-0.921***	-0.921***	-0.609***	-0.912***	-0.923***
	(0.123)	(0.123)	(0.076)	(0.124)	(0.123)
X 2002	-1.247***	-1.247***	-0.894***	-1.223***	-1.249***
	(0.151)	(0.151)	(0.096)	(0.152)	(0.151)
X 2003	-1.484***	-1.484***	-1.122***	-1.455***	-1.486***
	(0.171)	(0.171)	(0.116)	(0.171)	(0.171)
X 2004	-1.628***	-1.628***	-1.231***	-1.602***	-1.631***
	(0.185)	(0.185)	(0.130)	(0.186)	(0.186)
X 2005	-1.720***	-1.720***	-1.291***	-1.698***	-1.723***
	(0.194)	(0.194)	(0.134)	(0.196)	(0.194)
X 2006	-1.921***	-1.921***	-1.411***	-1.902***	-1.924***
	(0.205)	(0.205)	(0.141)	(0.209)	(0.205)
X 2007	-2.073***	-2.073***	-1.528***	-2.055***	-2.076***
	(0.217)	(0.217)	(0.148)	(0.223)	(0.218)
X 2008	-2.216***	-2.216***	-1.613***	-2.194***	-2.220***
	(0.231)	(0.231)	(0.158)	(0.236)	(0.231)
X 2009	-2.592***	-2.592***	-1.862***	-2.559***	-2.596***
	(0.261)	(0.261)	(0.179)	(0.265)	(0.262)
X 2010	-2.674***	-2.674***	-1.967***	-2.643***	-2.678***
	(0.263)	(0.263)	(0.181)	(0.267)	(0.264)
X 2011	-2.637***	-2.637***	-1.958***	-2.613***	-2.641***
	(0.256)	(0.256)	(0.176)	(0.260)	(0.257)
X 2012	-2.623***	-2.623***	-1.981***	-2.602***	-2.627***
	(0.246)	(0.246)	(0.168)	(0.249)	(0.246)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects		Yes		Yes	Yes
Year-by-State Fixed Effects			Yes		
Winsorized Weights				Yes	
Drops Small County-Industries (<1000)					Yes

Notes: This table displays estimates from regressions of change in percent of working age population working in manufacturing on exposure to Section 936 interacted with year dummies corresponding to Table 5. This regression is designed after the analysis of Autor et al. (2013). Exposure to Section 936 comes from the NETS and employment growth comes from QCEW (2017). See Section 5 for further discussion and Appendix B for more information about the data. Standard errors clustered at the state and industry levels are shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Observations are weighted by employment in 1995.

Table A.9: Event Study on Employment Growth at the County Level, Establishment Links Instrumenting for Employment Links

Exposure to Section 936	(1)	(2)	(3)	(4)	(5)
X 1990	0.000	0.000	0.000	0.010	-0.008
	(0.017)	(0.017)	(0.017)	(0.012)	(0.023)
X 1991	-0.002	-0.002	-0.002	0.007	-0.009
	(0.014)	(0.014)	(0.014)	(0.010)	(0.019)
X 1992	0.001	0.001	0.001	0.006	-0.003
	(0.010)	(0.009)	(0.009)	(0.007)	(0.013)
X 1993	0.002	0.002	0.002	0.004	-0.001
	(0.006)	(0.006)	(0.006)	(0.004)	(0.008)
X 1994	0.001	0.001	0.001	0.002	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
X 1996	-0.005	-0.005	-0.005	-0.005*	-0.006
	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)
X 1997	-0.011*	-0.010*	-0.010*	-0.010**	-0.011
	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)
X 1998	-0.017**	-0.017**	-0.017**	-0.015**	-0.018*
	(0.006)	(0.006)	(0.006)	(0.005)	(0.008)
X 1999	-0.020**	-0.020**	-0.020**	-0.019**	-0.022*
	(0.008)	(0.008)	(0.008)	(0.006)	(0.010)
X 2000	-0.031**	-0.031**	-0.031**	-0.029***	-0.036**
	(0.009)	(0.009)	(0.009)	(0.008)	(0.012)
X 2001	-0.040***	-0.040***	-0.040***	-0.037***	-0.047**
	(0.011)	(0.011)	(0.011)	(0.010)	(0.015)
X 2002	-0.048***	-0.048***	-0.048***	-0.044***	-0.054**
	(0.013)	(0.013)	(0.013)	(0.011)	(0.017)
X 2003	-0.055***	-0.055***	-0.055***	-0.050***	-0.061**
	(0.014)	(0.014)	(0.014)	(0.012)	(0.019)
X 2004	-0.069***	-0.069***	-0.069***	-0.063***	-0.078***
	(0.017)	(0.017)	(0.017)	(0.015)	(0.022)
X 2005	-0.082***	-0.082***	-0.082***	-0.074***	-0.095***
	(0.021)	(0.021)	(0.021)	(0.018)	(0.027)
X 2006	-0.092***	-0.092***	-0.092***	-0.083***	-0.106**
	(0.024)	(0.024)	(0.024)	(0.021)	(0.031)
X 2007	-0.093***	-0.093***	-0.093***	-0.083***	-0.105**
	(0.024)	(0.024)	(0.024)	(0.021)	(0.031)
X 2008	-0.092***	-0.092***	-0.092***	-0.081***	-0.103**
	(0.023)	(0.023)	(0.023)	(0.020)	(0.030)
X 2009	-0.085***	-0.085***	-0.085***	-0.074***	-0.095**
	(0.021)	(0.021)	(0.021)	(0.017)	(0.027)
X 2010	-0.083***	-0.083***	-0.083***	-0.073***	-0.092**
	(0.021)	(0.021)	(0.021)	(0.017)	(0.027)
X 2011	-0.087***	-0.087***	-0.087***	-0.077***	-0.097**
	(0.021)	(0.021)	(0.021)	(0.018)	(0.028)
X 2012	-0.093***	-0.093***	-0.093***	-0.082***	-0.106***
	(0.023)	(0.023)	(0.023)	(0.019)	(0.029)
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects		Yes		Yes	Yes
Industry-by-County Fixed Effects			Yes		
Winsorized Weights				Yes	
Drops Small County-Industries (<1000)					Yes

Notes: This table displays estimates from regressions of employment growth on exposure to Section 936 interacted with year dummies corresponding to Table 5. Observations are county-industries in each year. Exposure to Section 936 comes from the NETS and employment growth comes from QCEW (2017). See Section 5 for discussion and Appendix B for more information about the data. Estimates are shown graphically in panel A of Figure 11. Standard errors clustered at the state and industry levels are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Observations are weighted according to employment in 1995.

Table A.10: Event Study on Employment Growth at the County Level, Large Retailers Removed from Links

Exposure to Section 936	(1)	(2)	(3)	(4)	(5)
X 1990	0.001	0.001	0.001	0.008	-0.006
	(0.015)	(0.015)	(0.015)	(0.011)	(0.019)
X 1991	-0.001	-0.001	-0.001	0.005	-0.007
	(0.012)	(0.012)	(0.012)	(0.009)	(0.016)
X 1992	0.001	0.002	0.002	0.005	-0.002
	(0.008)	(0.008)	(0.008)	(0.006)	(0.011)
X 1993	0.002	0.002	0.002	0.004	-0.000
	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)
X 1994	0.001	0.001	0.001	0.002	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
X 1996	-0.004	-0.004	-0.004	-0.004	-0.005
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
X 1997	-0.009*	-0.009*	-0.009*	-0.009*	-0.009
	(0.004)	(0.004)	(0.004)	(0.003)	(0.005)
X 1998	-0.015**	-0.014**	-0.014**	-0.014**	-0.016*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)
X 1999	-0.018**	-0.018**	-0.018**	-0.018**	-0.019*
	(0.007)	(0.007)	(0.007)	(0.006)	(0.008)
X 2000	-0.027**	-0.027**	-0.027**	-0.026**	-0.030**
	(0.008)	(0.008)	(0.008)	(0.007)	(0.010)
X 2001	-0.034***	-0.034***	-0.034***	-0.033***	-0.040**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.012)
X 2002	-0.042***	-0.042***	-0.042***	-0.040***	-0.046**
	(0.011)	(0.011)	(0.011)	(0.011)	(0.014)
X 2003	-0.047***	-0.047***	-0.047***	-0.044***	-0.052**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.016)
X 2004	-0.059***	-0.059***	-0.059***	-0.056***	-0.066***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.019)
X 2005	-0.070***	-0.070***	-0.070***	-0.066***	-0.080***
	(0.018)	(0.018)	(0.018)	(0.017)	(0.022)
X 2006	-0.079***	-0.079***	-0.079***	-0.075***	-0.089**
	(0.020)	(0.020)	(0.020)	(0.019)	(0.026)
X 2007	-0.080***	-0.080***	-0.080***	-0.075***	-0.089**
	(0.021)	(0.021)	(0.021)	(0.019)	(0.027)
X 2008	-0.079***	-0.079***	-0.079***	-0.074***	-0.087**
	(0.020)	(0.020)	(0.020)	(0.018)	(0.026)
X 2009	-0.073***	-0.073***	-0.073***	-0.068***	-0.080**
	(0.018)	(0.018)	(0.018)	(0.016)	(0.023)
X 2010	-0.072***	-0.072***	-0.072***	-0.067***	-0.078**
	(0.018)	(0.018)	(0.018)	(0.016)	(0.023)
X 2011	-0.075***	-0.075***	-0.075***	-0.070***	-0.082**
	(0.018)	(0.018)	(0.018)	(0.017)	(0.024)
X 2012	-0.080***	-0.080***	-0.080***	-0.075***	-0.090***
	(0.020)	(0.020)	(0.020)	(0.018)	(0.025)
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects		Yes		Yes	Yes
Industry-by-County Fixed Effects			Yes		
Winsorized Weights				Yes	
Drops Small County-Industries (<1000)					Yes

Notes: This table displays estimates from regressions of employment growth on exposure to Section 936 interacted with year dummies corresponding to Table 5. Observations are counties in each year.Exposure to Section 936 comes from the NETS and employment growth comes from QCEW (2017). See Section 6 for discussion and Appendix B for more information about the data. Estimates are shown graphically in panel B of Figure 11. Standard errors clustered at the state and industry levels are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Observations are weighted according to employment in 1995.

Table A.11: Event Study on Employment Growth at the County Level, Firms without US Headquarters Removed from Links

Exposure to Section 936	(1)	(2)	(3)	(4)	(5)
X 1990	0.001	0.001	0.001	0.008	-0.006
	(0.015)	(0.015)	(0.015)	(0.012)	(0.020)
X 1991	-0.001	-0.001	-0.001	0.005	-0.007
	(0.012)	(0.012)	(0.012)	(0.009)	(0.016)
X 1992	0.001	0.002	0.002	0.005	-0.002
	(0.008)	(0.008)	(0.008)	(0.006)	(0.011)
X 1993	0.002	0.002	0.002	0.004	-0.000
	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)
X 1994	0.001	0.001	0.001	0.002	0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
X 1996	-0.005	-0.005	-0.005	-0.004	-0.005
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
X 1997	-0.010*	-0.010*	-0.010*	-0.009*	-0.010
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
X 1998	-0.015**	-0.015**	-0.015**	-0.014**	-0.016*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)
X 1999	-0.018**	-0.018**	-0.018**	-0.018**	-0.020*
	(0.007)	(0.007)	(0.007)	(0.006)	(0.008)
X 2000	-0.028***	-0.028***	-0.028***	-0.027***	-0.031**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)
X 2001	-0.035***	-0.035***	-0.035***	-0.034***	-0.041**
	(0.010)	(0.010)	(0.010)	(0.009)	(0.013)
X 2002	-0.043***	-0.043***	-0.043***	-0.041***	-0.047**
	(0.011)	(0.011)	(0.011)	(0.011)	(0.015)
X 2003	-0.048***	-0.048***	-0.048***	-0.045***	-0.053**
	(0.013)	(0.013)	(0.013)	(0.012)	(0.016)
X 2004	-0.060***	-0.060***	-0.060***	-0.058***	-0.067***
	(0.015)	(0.015)	(0.015)	(0.014)	(0.019)
X 2005	-0.072***	-0.072***	-0.072***	-0.068***	-0.081***
	(0.018)	(0.018)	(0.018)	(0.017)	(0.023)
X 2006	-0.081***	-0.081***	-0.081***	-0.076***	-0.091**
	(0.021)	(0.021)	(0.021)	(0.019)	(0.026)
X 2007	-0.081***	-0.081***	-0.081***	-0.076***	-0.090**
	(0.021)	(0.021)	(0.021)	(0.019)	(0.027)
X 2008	-0.080***	-0.080***	-0.080***	-0.075***	-0.088**
	(0.020)	(0.020)	(0.020)	(0.018)	(0.026)
X 2009	-0.074***	-0.074***	-0.074***	-0.069***	-0.081**
	(0.018)	(0.018)	(0.018)	(0.016)	(0.023)
X 2010	-0.073***	-0.073***	-0.073***	-0.068***	-0.079**
	(0.018)	(0.018)	(0.018)	(0.016)	(0.023)
X 2011	-0.076***	-0.076***	-0.076***	-0.071***	-0.083***
	(0.018)	(0.018)	(0.018)	(0.016)	(0.024)
X 2012	-0.081***	-0.081***	-0.081***	-0.076***	-0.091***
	(0.019)	(0.019)	(0.019)	(0.018)	(0.025)
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects		Yes		Yes	Yes
Industry-by-County Fixed Effects			Yes		
Winsorized Weights				Yes	
Drops Small County-Industries (<1000)					Yes

Notes: This table displays estimates from regressions of employment growth on exposure to Section 936 interacted with year dummies corresponding to Table 5. Exposure to Section 936 comes from the NETS and employment growth comes from QCEW (2017). See Section 5 for discussion and Appendix B for more information about the data. Estimates are shown graphically in panel B of Figure 11. Standard errors clustered at the state and industry levels are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Observations are weighted according to employment in 1995.

Table A.12: Event Study on Employment Growth at the County Level, Links Only Counting Compustat Firms

Exposure to Section 936	(1)	(2)	(3)	(4)	(5)
X 1990	0.005	0.005	0.005	0.011	0.001
	(0.013)	(0.013)	(0.013)	(0.009)	(0.018)
X 1991	0.004	0.004	0.004	0.010	0.001
	(0.010)	(0.010)	(0.010)	(0.007)	(0.014)
X 1992	0.006	0.006	0.006	0.010	0.005
	(0.007)	(0.007)	(0.007)	(0.005)	(0.010)
X 1993	0.005	0.005	0.005	0.007*	0.004
	(0.004)	(0.004)	(0.004)	(0.003)	(0.006)
X 1994	0.003	0.003	0.003	0.004*	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
X 1996	-0.004*	-0.004*	-0.004*	-0.004*	-0.005
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
X 1997	-0.009*	-0.009*	-0.009*	-0.009**	-0.009
	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
X 1998	-0.014**	-0.014**	-0.014**	-0.013***	-0.014*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)
X 1999	-0.016**	-0.016**	-0.016**	-0.017**	-0.016*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)
X 2000	-0.024***	-0.024***	-0.024***	-0.024***	-0.026**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.009)
X 2001	-0.030***	-0.030***	-0.030***	-0.029***	-0.034**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.011)
X 2002	-0.037***	-0.037***	-0.037***	-0.036***	-0.041**
	(0.010)	(0.010)	(0.010)	(0.009)	(0.013)
X 2003	-0.042***	-0.042***	-0.042***	-0.040***	-0.047**
	(0.011)	(0.011)	(0.011)	(0.010)	(0.015)
X 2004	-0.052***	-0.052***	-0.052***	-0.051***	-0.059**
	(0.012)	(0.012)	(0.012)	(0.011)	(0.017)
X 2005	-0.062***	-0.062***	-0.062***	-0.059***	-0.071**
	(0.015)	(0.015)	(0.015)	(0.014)	(0.021)
X 2006	-0.071***	-0.071***	-0.071***	-0.067***	-0.081**
	(0.017)	(0.017)	(0.017)	(0.016)	(0.024)
X 2007	-0.073***	-0.073***	-0.073***	-0.069***	-0.083***
	(0.017)	(0.017)	(0.017)	(0.016)	(0.023)
X 2008	-0.072***	-0.072***	-0.072***	-0.068***	-0.080***
	(0.016)	(0.016)	(0.016)	(0.015)	(0.022)
X 2009	-0.065***	-0.065***	-0.065***	-0.061***	-0.073***
	(0.015)	(0.015)	(0.015)	(0.013)	(0.021)
X 2010	-0.065***	-0.065***	-0.065***	-0.062***	-0.072**
	(0.015)	(0.015)	(0.015)	(0.013)	(0.021)
X 2011	-0.068***	-0.068***	-0.068***	-0.064***	-0.077***
	(0.015)	(0.015)	(0.015)	(0.013)	(0.021)
X 2012	-0.073***	-0.073***	-0.073***	-0.069***	-0.084***
	(0.016)	(0.016)	(0.016)	(0.014)	(0.022)
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects		Yes		Yes	Yes
Industry-by-County Fixed Effects			Yes		
Winsorized Weights				Yes	
Drops Small County-Industries (<1000)					Yes

Notes: This table displays estimates from regressions of employment growth on exposure to Section 936 interacted with year dummies corresponding to Table 5. Exposure to Section 936 comes from the NETS and employment growth comes from QCEW (2017). See Section 5 for discussion and Appendix B for more information about the data. Estimates are shown graphically in panel B of Figure 11. Standard errors clustered at the state and industry levels are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Observations are weighted according to employment in 1995.

Table A.13: Event Study on Employment Growth at the County Level, Fake Shock

Exposure to Section 936	(1)	(2)	(3)	(4)	(5)
X 1990	-0.017*	-0.017*	-0.018*	-0.017*	-0.031*
	(0.009)	(0.009)	(0.009)	(0.009)	(0.015)
X 1991	-0.010	-0.010	-0.010	-0.010	-0.018
	(0.007)	(0.007)	(0.007)	(0.007)	(0.012)
X 1992	-0.005	-0.005	-0.006	-0.005	-0.009
	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)
X 1993	-0.004	-0.004	-0.004	-0.004	-0.007
	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)
X 1994	-0.001	-0.001	-0.001	-0.001	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
X 1996	0.000	0.000	0.001	0.000	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
X 1997	0.002	0.002	0.002	0.002	0.005
	(0.003)	(0.003)	(0.004)	(0.003)	(0.006)
X 1998	0.004	0.004	0.004	0.004	0.009
	(0.005)	(0.005)	(0.005)	(0.005)	(0.009)
X 1999	0.004	0.004	0.004	0.004	0.012
	(0.006)	(0.006)	(0.006)	(0.006)	(0.010)
X 2000	0.004	0.004	0.004	0.004	0.015
	(0.006)	(0.006)	(0.006)	(0.006)	(0.011)
X 2001	-0.001	-0.001	-0.001	-0.001	0.009
	(0.008)	(0.008)	(0.008)	(0.008)	(0.014)
X 2002	-0.002	-0.002	-0.002	-0.002	0.009
	(0.009)	(0.009)	(0.009)	(0.009)	(0.015)
X 2003	-0.003	-0.003	-0.003	-0.003	0.005
	(0.009)	(0.009)	(0.009)	(0.009)	(0.016)
X 2004	-0.005	-0.005	-0.005	-0.005	0.005
	(0.010)	(0.010)	(0.010)	(0.010)	(0.017)
X 2005	-0.011	-0.011	-0.011	-0.011	0.001
	(0.012)	(0.012)	(0.012)	(0.012)	(0.019)
X 2006	-0.008	-0.008	-0.008	-0.008	0.007
	(0.013)	(0.013)	(0.013)	(0.013)	(0.022)
X 2007	-0.003	-0.003	-0.003	-0.003	0.014
	(0.012)	(0.012)	(0.012)	(0.012)	(0.022)
X 2008	-0.000	-0.000	-0.000	-0.000	0.018
	(0.011)	(0.011)	(0.011)	(0.011)	(0.022)
X 2009	0.002	0.002	0.002	0.002	0.022
	(0.011)	(0.011)	(0.011)	(0.011)	(0.022)
X 2010	0.005	0.005	0.005	0.005	0.025
	(0.011)	(0.011)	(0.011)	(0.011)	(0.022)
X 2011	0.010	0.010	0.010	0.010	0.034
	(0.011)	(0.011)	(0.011)	(0.011)	(0.024)
X 2012	0.013	0.013	0.013	0.013	0.038
	(0.012)	(0.012)	(0.012)	(0.012)	(0.026)
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects		Yes		Yes	Yes
Industry-by-County Fixed Effects			Yes		
Winsorized Weights				Yes	
Drops Small County-Industries (<1000)					Yes

Notes: This table displays estimates from regressions of employment growth on a placebo for exposure to Section 936 interacted with year dummies corresponding to Table 5. Observations are counties in each year. The placebo for exposure to Section 936 comes from a matched sample of firms in the NETS that do not have establishments in Puerto Rico and employment growth comes from QCEW (2017). See Section 6 for discussion and Appendix B for more information about the data. Estimates are shown graphically in panel A of Figure 12. Standard errors clustered at the state and industry levels are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Observations are weighted according to employment in 1995.

Table A.14: Event Study on Employment Growth at the County Level, Pharmaceuticals Exposure

Exposure to Pharmaceuticals	(1)	(2)	(3)	(4)	(5)
X 1990	0.013*	0.013*	0.014**	0.014**	0.012*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
X 1991	0.008	0.008	0.008	0.008*	0.006
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
X 1992	0.005	0.005	0.005*	0.006*	0.005
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)
X 1993	0.003	0.003	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
X 1994	0.001	0.001	0.001	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
X 1996	0.002	0.002	0.002	0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
X 1997	0.003	0.003	0.003	0.003	0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
X 1998	0.004	0.004	0.005	0.004	0.005
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)
X 1999	0.005	0.005	0.005	0.005	0.006
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)
X 2000	0.006	0.006	0.007	0.006	0.008
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
X 2001	0.010*	0.010*	0.010*	0.009*	0.011*
	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)
X 2002	0.007	0.007	0.007	0.006	0.008
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
X 2003	0.009**	0.009**	0.009**	0.007*	0.011**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
X 2004	0.008	0.008	0.008	0.007	0.011*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
X 2005	0.007	0.007	0.007	0.005	0.009
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
X 2006	0.007	0.007	0.007	0.005	0.010
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
X 2007	0.008	0.008	0.008	0.006	0.012*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
X 2008	0.008	0.008	0.008	0.006	0.012
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
X 2009	0.009	0.009	0.009	0.007	0.012
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
X 2010	0.008	0.008	0.008	0.006	0.011
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
X 2011	0.007	0.007	0.007	0.006	0.010
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
X 2012	0.008	0.008	0.008	0.007	0.011
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects		Yes		Yes	Yes
Industry-by-County Fixed Effects			Yes		
Winsorized Weights				Yes	
Drops Small County-Industries (<1000)					Yes

Notes: This table displays estimates from regressions of employment growth on exposure to Pharmaceuticals interacted with year dummies corresponding to Table 5. Observations are counties in each year. Data regarding the exposure to Pharmaceuticals and data on employment growth both come from QCEW (2017). See Section 6 for discussion and Appendix B for more information about the data. Estimates are shown graphically in panel B of Figure 12. Standard errors clustered at the state and industry levels are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Observations are weighted according to employment in 1995.

Table A.15: Effects of Repealing §936 on Employment Growth at the County Level: Robustness Part 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exposure to Section 936	-0.072	-0.083	-0.080	-0.079	-0.074	-0.068	-0.079	-0.078	-0.074
	(0.018)	(0.018)	(0.021)	(0.021)	(0.021)	(0.018)	(0.022)	(0.018)	(0.019)
	0.000	0.000	0.000	0.001	0.001	0.000	0.001	0.000	0.000
Total Incentives (Bartik)		-0.045							
		(0.011)							
		0.000							
Job Creation Incentive			-0.003						
			(0.003)						
			0.357						
Job Training Subsidy				0.005					
				(0.017)					
				0.775					
Real Minimum Wage					0.001				
					(0.011)				
					0.907				
Right to Work						0.045			
						(0.014)			
						0.002			
RD Tax Credit							0.003		
							(0.010)		
							0.752		
Investment Tax Credit								-0.032	
								(0.014)	
								0.027	
Corporate Income Tax									-0.022
									(0.016)
									0.174
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates in this table build on results shown in Table 5. The estimates in each column come from a regression of employment growth from QCEW on exposure to Section 936 and other local characteristics. State level policy data come from BEA and BLS data gathered by Suárez Serrato and Wingender (2016). All specifications include year, industry, and state fixed effects. See Section 6 for discussion and Appendix B for more information about the data. Standard errors clustered at the state and industry levels are shown in parentheses. Observations are weighted according to employment in 1995.

Table A.16: Effects of Repealing §936 on Employment Growth at the County Level: Robustness Part 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Exposure to Section 936	-0.072	-0.069	-0.073	-0.077	-0.073	-0.076	-0.071	-0.069	-0.072	-0.068
	(0.018)	(0.017)	(0.021)	(0.021)	(0.018)	(0.019)	(0.019)	(0.017)	(0.017)	(0.016)
	0.000	0.000	0.001	0.001	0.000	0.000	0.001	0.000	0.000	0.000
Personal Income Tax		-0.033							-0.008	-0.008
		(0.012)							(0.017)	(0.016)
		0.010							0.640	0.641
Property Tax			-0.026						-0.023	-0.023
			(0.012)						(0.014)	(0.015)
			0.038						0.115	0.127
Sales Tax				0.008					0.009	0.006
				(0.011)					(0.022)	(0.022)
				0.463					0.698	0.794
Trade Exposure (China)					-0.008				0.007	0.019
					(0.013)				(0.012)	(0.013)
					0.551				0.569	0.135
State Revenue Per Capita						-0.026			-0.002	0.004
						(0.014)			(0.017)	(0.018)
						0.061			0.898	0.825
Share Routine Workers							-0.013		0.014	0.033
							(0.014)		(0.011)	(0.013)
							0.344		0.217	0.016
Trade Exposure (Nafta)								0.008	-0.001	-0.012
								(0.019)	(0.015)	(0.014)
								0.674	0.970	0.381
Total Incentives (Bartik)									-0.043	-0.044
									(0.012)	(0.012)
									0.001	0.000
Job Creation Incentive									0.006	0.010
									(0.014)	(0.014)
									0.670	0.455
Job Training Subsidy									-0.010	-0.011
									(0.020)	(0.020)
									0.610	0.568
Real Minimum Wage									-0.006	-0.008
									(0.009)	(0.009)
									0.519	0.380
Right to Work									0.047	0.041
									(0.024)	(0.022)
									0.061	0.075
RD Tax Credit									0.011	0.008
									(0.010)	(0.011)
									0.292	0.461
Investment Tax Credit									0.003	0.002
									(0.018)	(0.018)
									0.864	0.903
Corporate Income Tax									-0.013	-0.012
									(0.017)	(0.016)
									0.459	0.458
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Size Dist. Cont.										Yes

Notes: See Section 6 for discussion of regressions and Appendix B for more information about the data. Standard errors clustered at the state and industry levels are shown in parentheses. Observations are weighted according to employment in 1995.

Table A.17: Event Study on Employment Growth at the County Level, Heterogeneous Effects by Sector

Exposure to Section 936	(1) Average Effect	(2) Tradable	(3) Non-Tradable	(4) Other	(5) Construction
X 1990	0.000	-0.002	0.002	0.001	-0.016
	(0.015)	(0.024)	(0.010)	(0.015)	(0.023)
X 1991	-0.001	0.001	-0.001	-0.003	-0.005
	(0.012)	(0.018)	(0.008)	(0.013)	(0.016)
X 1992	0.001	0.003	0.002	-0.000	0.002
	(0.008)	(0.011)	(0.006)	(0.008)	(0.011)
X 1993	0.002	0.003	0.001	0.002	0.003
	(0.005)	(0.008)	(0.004)	(0.005)	(0.005)
X 1994	0.001	0.001	-0.000	0.001	0.001
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
X 1996	-0.004	-0.010*	-0.003	-0.004	-0.003
	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)
X 1997	-0.009*	-0.021**	-0.008*	-0.007	-0.007
	(0.004)	(0.007)	(0.003)	(0.006)	(0.005)
X 1998	-0.014**	-0.028**	-0.013**	-0.011	-0.015*
	(0.005)	(0.010)	(0.004)	(0.007)	(0.007)
X 1999	-0.017*	-0.028**	-0.017**	-0.013	-0.015
	(0.007)	(0.009)	(0.005)	(0.009)	(0.011)
X 2000	-0.026**	-0.032**	-0.025**	-0.024*	-0.034**
	(0.008)	(0.009)	(0.006)	(0.011)	(0.009)
X 2001	-0.034***	-0.038**	-0.032**	-0.030*	-0.051**
	(0.009)	(0.010)	(0.007)	(0.013)	(0.013)
X 2002	-0.041***	-0.036**	-0.038**	-0.040*	-0.068***
	(0.011)	(0.011)	(0.009)	(0.015)	(0.014)
X 2003	-0.046***	-0.036**	-0.044**	-0.043*	-0.086***
	(0.012)	(0.012)	(0.011)	(0.016)	(0.019)
X 2004	-0.058***	-0.039**	-0.054**	-0.053**	-0.106**
	(0.014)	(0.014)	(0.012)	(0.017)	(0.026)
X 2005	-0.069***	-0.045**	-0.062**	-0.063**	-0.130**
	(0.018)	(0.013)	(0.015)	(0.020)	(0.035)
X 2006	-0.078***	-0.054**	-0.071**	-0.069**	-0.149**
	(0.020)	(0.015)	(0.019)	(0.022)	(0.041)
X 2007	-0.079***	-0.059**	-0.076**	-0.069**	-0.145**
	(0.021)	(0.017)	(0.021)	(0.023)	(0.036)
X 2008	-0.078***	-0.061**	-0.081**	-0.069**	-0.132***
	(0.020)	(0.018)	(0.020)	(0.024)	(0.029)
X 2009	-0.071***	-0.054**	-0.078**	-0.067**	-0.112***
	(0.018)	(0.017)	(0.020)	(0.023)	(0.024)
X 2010	-0.070***	-0.050**	-0.081**	-0.067**	-0.108**
	(0.018)	(0.016)	(0.021)	(0.024)	(0.025)
X 2011	-0.073***	-0.054**	-0.088**	-0.069**	-0.107**
	(0.018)	(0.018)	(0.021)	(0.024)	(0.025)
X 2012	-0.079***	-0.059**	-0.092**	-0.075**	-0.116***
	(0.019)	(0.020)	(0.023)	(0.025)	(0.026)
Industry-by-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: This table displays estimates from regressions of employment growth on exposure to Section 936 interacted with year dummies corresponding to Table 5. The second to fifth columns show the heterogeneous effects by industry using definitions from Mian and Sufi (2014). Exposure to Section 936 comes from the NETS and employment growth comes from QCEW (2017). See Section 6 for discussion and Appendix B for more information about the data. The estimates are shown graphically in Figure 14. Standard errors clustered at the state and industry levels are shown in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Observations are weighted according to employment in 1995.

Table A.18: Effects of Repealing §936 on Wages, Rental Costs, and Home Values: Robustness to Not Adjusting for Changes in Observable Characteristics

	(1)	(2)	(3)	(4)	(5)
Stacked Differences After 1990					
		Wages			
	All	High Skill	Low Skill	Rent	Home Value
Exposure to Section 936	-0.010	-0.002	-0.017	-0.019	-0.035
	(0.002)	(0.002)	(0.003)	(0.003)	(0.007)
	0.000	0.375	0.000	0.000	0.000
Observations	984	984	984	984	984
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Differences Before 1990					
		Wages			
	All	High Skill	Low Skill	Rent	Home Value
Exposure to Section 936	0.005	-0.004	0.000	0.021	0.029
	(0.008)	(0.004)	(0.007)	(0.010)	(0.023)
	0.505	0.387	0.958	0.045	0.212
Observations	492	492	492	492	492
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: This table shows additional estimates associated with Table 8. Estimates in this table come from regressions of economic outcomes at the conspuma level on the exposure to Section 936. Exposure to Section 936 comes from the NETS and conspuma level outcomes come from the Census (Ruggles et al., 2010). The outcome data in this table are not adjusted for differences in observable characteristics. See Section 6 for more discussion and Appendix B for more information about the data. Standard errors clustered at the state level are shown in parentheses. Observations are weighted according to population.

Table A.19: Effects of Repealing §936 on Government Transfer Payments Per Capita, 1990-1995

	Total Transfers (1)	Unemployment Benefits (2)	Income Replacement (3)	Education Benefits (4)	Retirement and Disability (5)	Medicare Benefits (6)	Public Medical Benefits (7)
Panel A. Percent change in transfers per capita relative to 1995							
Exposure to Section 936	-0.002	0.023	-0.001	0.001	-0.003	-0.003	-0.004
	(0.002)	(0.017)	(0.005)	(0.009)	(0.002)	(0.003)	(0.006)
	0.356	0.196	0.914	0.886	0.214	0.330	0.488
Panel B. Dollar change in transfers per capita relative to 1995							
Exposure to Section 936	14.982	0.893	5.815	-0.404	-1.808	2.957	8.633
	(18.979)	(1.883)	(5.050)	(0.714)	(4.126)	(4.940)	(14.574)
	0.434	0.637	0.255	0.574	0.663	0.552	0.556
Sample Proportion of Transfers	96.9%	3.1%	10.7%	1.6%	46.2%	19.4%	15.4%
Sample Transfers Per Capita	2,793.9	88.8	307.4	47.6	1,330.4	559.7	443.2
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates in this table come from regressions of government transfers in each category at the county level on the exposure to Section 936. The upper panel shows the results with the dependent variable measured in percent change of category transfers per capita while the lower panel shows results with the dependent variable measured in change in dollars per capita. Exposure to Section 936 comes from the NETS and county level transfers comes from the BEA local area personal income accounts (Bureau of Economic Analysis, 2018). Column (1) includes all transfers in columns (2) to (7), which does not include veteran's benefits. None of the specifications for this period find significant differences in transfer payments before the repeal of §936. See Appendix B for more information about the data. Clustered standard errors are shown in parentheses with p-values displayed below. Analogous regressions for the 2004-2008 period are shown in Table 9.

Table A.20: Probit Regressions of Probability of Being in Puerto Rico at Firm Level

	(1)	(2)	(3)	(4)	(5)
PR					
ln(Assets)	0.935	0.941	0.888	0.901	0.919
	(0.063)	(0.067)	(0.061)	(0.066)	(0.063)
	0.000	0.000	0.000	0.000	0.000
NAICS 3254 (Pharma)	0.098	0.123	0.225	0.223	0.225
	(0.033)	(0.042)	(0.042)	(0.031)	(0.033)
	0.003	0.003	0.000	0.000	0.000
ETR		-0.067	-0.055	-0.058	-0.063
		(0.041)	(0.041)	(0.046)	(0.045)
		0.102	0.176	0.204	0.164
Any Research & Development			0.148	0.135	0.137
			(0.060)	(0.066)	(0.067)
			0.013	0.042	0.040
Gross Profit / Operating Assets				0.140	0.142
				(0.040)	(0.038)
				0.000	0.000
Long-term Debt / Assets				-0.015	-0.014
				(0.067)	(0.067)
				0.824	0.835
Any Advertising					-0.084
					(0.144)
					0.559
Observations	8945	8945	8945	8945	8945
3 Digit NAICS Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: Author's calculations and data from COMPUSTAT. This table displays probit estimates of the probability of having any establishments in Puerto Rico in 1995 on firm characteristics. Continuous covariates ln(Assets), ETR, Gross Profit / Operating Assets, and Long-term Debt / Assets are normalized to z-scores so the estimated coefficients can be interpreted as the marginal effect of a standard deviation change.

Table A.21: Event Study Results on Firm Value

	(0,0)	(0,3)	(0,6)	(0,9)	(0,12)	(0,15)	(-5,15)
Pooled CAPM							
Event	-0.141	-0.447	-0.970	-1.191	-1.431	-1.078	-1.250
	(0.130)	(0.252)	(0.330)	(0.399)	(0.450)	(0.501)	(0.593)
	0.280	0.076	0.003	0.003	0.001	0.031	0.035
Pooled Fama-French 3 Factors Plus Momentum							
Event	0.061	-0.207	-0.478	-0.774	-0.825	-0.342	-0.255
	(0.128)	(0.250)	(0.320)	(0.399)	(0.458)	(0.505)	(0.597)
	0.634	0.408	0.135	0.052	0.072	0.499	0.669
February 16, 1993 Event CAPM							
Event	-0.440	-1.302	-1.851	-2.027	-2.292	-1.882	-2.433
	(0.207)	(0.413)	(0.535)	(0.647)	(0.712)	(0.780)	(0.919)
	0.034	0.002	0.001	0.002	0.001	0.016	0.008
February 16, 1993 Event Fama-French 3 Factors Plus Momentum							
Event	0.056	-0.757	-0.662	-1.090	-1.220	-0.616	-1.108
	(0.200)	(0.407)	(0.511)	(0.623)	(0.693)	(0.762)	(0.900)
	0.780	0.063	0.195	0.080	0.078	0.419	0.218
October 12, 1995 CAPM							
Event	0.074	0.306	-0.150	-0.406	-0.616	-0.315	-0.130
	(0.167)	(0.301)	(0.396)	(0.478)	(0.560)	(0.636)	(0.758)
	0.656	0.311	0.704	0.396	0.271	0.621	0.864
October 12, 1995 Fama-French 3 Factors Plus Momentum							
Event	0.064	0.252	-0.319	-0.464	-0.419	-0.072	0.589
	(0.167)	(0.308)	(0.402)	(0.501)	(0.595)	(0.665)	(0.784)
	0.700	0.414	0.427	0.354	0.482	0.914	0.453
Observations	21,105	21,732	22,359	22,986	23,613	24,240	24,240

Notes: Estimates in this table are average Cumulative Abnormal Return (CAR) for an equally weighted portfolio of publicly traded US firms with establishments in Puerto Rico between 1990 and 1996. Each column corresponds to an event study where the range of dates in the event period are equal to the title of the column where the event is defined to be day 0. The first panel uses CAPM to estimate expected returns while the second panel uses Fama-French 3 factors plus momentum (Fama and French, 1993). Robust standard errors are more conservative than standard errors clustered at the firm or date level. The included firms come from manually matching firms in NETS and COMPUSTAT. Stock return data come from CRSP Stocks (1990-1996). See Appendix E for discussion and Appendix B for more information about the data. Robust standard errors are shown in parentheses and p-values are displayed below standard errors.

Table A.22: Event Study Results on Firm Value with Heterogeneous Effects

	(1)	(2)	(3)	(4)	(5)	(6)
Exposure to Section 936	-1.523	-1.521	-1.515	-1.513	-1.527	
	(0.362)	(0.361)	(0.360)	(0.355)	(0.356)	
	0.000	0.000	0.000	0.000	0.000	
February 16, 1993						-2.577
						(0.520)
						0.000
October 12, 1995						-0.559
						(0.467)
						0.232
Research & Development	-0.467	-0.390	-0.387	-0.387	-0.379	-0.372
	(0.075)	(0.090)	(0.094)	(0.094)	(0.097)	(0.083)
	0.000	0.000	0.000	0.000	0.000	0.000
Gross Profit / Operating Assets		0.709	1.019	1.018	1.033	0.749
		(0.421)	(0.432)	(0.432)	(0.434)	(0.379)
		0.093	0.019	0.019	0.018	0.049
Any Advertising			-0.806	-0.801	-0.647	-0.603
			(0.470)	(0.462)	(0.440)	(0.400)
			0.087	0.084	0.142	0.133
Change in ETR				-0.053	0.026	-0.062
				(0.485)	(0.449)	(0.398)
				0.914	0.955	0.876
Relative PR Employment					-0.967	-0.762
					(0.755)	(0.628)
					0.201	0.225
Observations	406	406	406	406	406	406
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table estimates the heterogeneous effects of the event study using a two step process. Counterfactual returns for the event period are predicted using a regression on the 100 days of trading before the event. The dependent variable for regressions in this table is the [0,10] day cumulative abnormal return for stocks of firms with exposure to Section 936. Independent variables other than exposure to Section 936 and its interactions with dates are normalized to be mean zero and scaled to standard deviations for interpretation. Two digit industry NAICS controls are included in all specifications. Data for firm financial characteristics come from Compustat (Capital IQ, 1980-2014) and daily stock return data are obtained from CRSP (CRSP Stocks, 1990-1996). The corresponding regressions controlling for the Fama-French factors plus momentum are shown in Table A.23. See Appendix E for discussion and Appendix B for more information about the data. Robust standard errors are shown in parentheses and p-values are displayed below standard errors.

Table A.23: Event Study Results on Firm Value with Heterogeneous Effects and Fama-French Controls

	(1)	(2)	(3)	(4)	(5)	(6)
Exposure to Section 936	-0.867	-0.867	-0.858	-0.857	-0.866	
	(0.331)	(0.331)	(0.328)	(0.323)	(0.326)	
	0.009	0.009	0.009	0.008	0.008	
February 16, 1993						-1.344
						(0.470)
						0.004
October 12, 1995						-0.479
						(0.439)
						0.275
Research & Development	-0.313	-0.276	-0.273	-0.273	-0.268	-0.260
	(0.051)	(0.073)	(0.077)	(0.077)	(0.078)	(0.067)
	0.000	0.000	0.000	0.000	0.001	0.000
Gross Profit / Operating Assets		0.339	0.767	0.766	0.776	0.644
		(0.360)	(0.381)	(0.382)	(0.382)	(0.351)
		0.347	0.045	0.046	0.043	0.067
Any Advertising			-1.112	-1.109	-1.008	-0.898
			(0.417)	(0.410)	(0.393)	(0.374)
			0.008	0.007	0.011	0.017
Change in ETR				-0.043	0.008	-0.062
				(0.447)	(0.424)	(0.376)
				0.924	0.984	0.868
Relative PR Employment					-0.631	-0.508
					(0.729)	(0.604)
					0.387	0.401
Observations	406	406	406	406	406	406
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes : This table estimates the heterogeneous effects of the event study using a two step process. Counterfactual returns for the event period are predicted using a regression on the 100 days of trading before the event. The dependent variable for regressions in this table is the [0,10] day cumulative abnormal return for stocks of firms with exposure to Section 936. Independent variables other than exposure to Section 936 and its interactions with dates are normalized to be mean zero and scaled to standard deviations for interpretation. Two digit industry NAICS controls are included in all specifications. Data for firm financial characteristics come from Compustat (Capital IQ, 1980-2014) and daily stock return data are obtained from CRSP (CRSP Stocks, 1990-1996). The corresponding regressions without controlling for the Fama-French factors plus momentum are shown in Table A.22. See Section 5 for more discussion and Appendix B for more information about the data. Robust standard errors are shown in parentheses and p-values are displayed below standard errors.

Table A.24: Effects of Repealing §936 on Employment Growth at the County Level: Interactions Part 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exposure to Section 936	-0.072	-0.075	-0.072	-0.072	-0.068	-0.073	-0.073	-0.073	-0.074
	(0.017)	(0.016)	(0.017)	(0.017)	(0.017)	(0.016)	(0.017)	(0.017)	(0.015)
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Interaction of Exposure with:									
Total Incentives (Bartik)		-0.011							
		(0.010)							
		0.283							
Job Creation Incentive			0.002						
			(0.003)						
			0.396						
Job Training Subsidy				0.009					
				(0.007)					
				0.180					
Real Minimum Wage					0.020				
					(0.016)				
					0.206				
Right to Work						-0.012			
						(0.009)			
						0.188			
RD Tax Credit							-0.004		
							(0.010)		
							0.674		
Investment Tax Credit								-0.002	
								(0.004)	
								0.573	
Corporate Income Tax									0.040
									(0.017)
									0.021
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes all Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates in this table build on results shown in Tables 7 and A.15. The estimates in each column come from a regression of employment growth from QCEW on exposure to Section 936 and other local characteristics. All specifications include year, industry, and state fixed effects in addition to all listed controls. Displayed estimates are for the interaction of the listed control and exposure to Section 936. See Appendix F for discussion and Appendix B for more information about the data. Standard errors clustered at the state and industry levels are shown in parentheses. Observations are weighted according to employment in 1995.

Table A.25: Effects of Repealing §936 on Employment Growth at the County Level: Interactions Part 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exposure to Section 936	-0.072	-0.075	-0.072	-0.069	-0.075	-0.073	-0.075	-0.077	-0.085
	(0.017)	(0.018)	(0.017)	(0.017)	(0.018)	(0.017)	(0.019)	(0.018)	(0.018)
	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Interaction of Exposure with:									
Personal Income Tax		0.015							-0.000
		(0.013)							(0.019)
		0.277							0.996
Property Tax			0.001						0.013
			(0.014)						(0.017)
			0.971						0.437
Sales Tax				0.020					0.019
				(0.014)					(0.018)
				0.150					0.290
Trade Exposure (China)					0.013				0.007
					(0.007)				(0.010)
					0.076				0.468
State Revenue Per Capita						0.012			0.003
						(0.009)			(0.020)
						0.186			0.893
Share Routine Workers							-0.014		-0.023
							(0.009)		(0.011)
							0.151		0.046
Trade Exposure (Nafta)								-0.014	-0.020
								(0.008)	(0.011)
								0.106	0.081
Total Incentives (Bartik)									0.005
									(0.017)
									0.762
Job Creation Incentive									0.015
									(0.009)
									0.101
Job Training Subsidy									-0.012
									(0.016)
									0.485
Real Minimum Wage									0.014
									(0.018)
									0.443
Right to Work									-0.004
									(0.023)
									0.881
RD Tax Credit									-0.011
									(0.015)
									0.466
Investment Tax Credit									-0.018
									(0.017)
									0.290
Corporate Income Tax									0.044
									(0.027)
									0.115
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes all Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: See Apendix F for discussion of regressions and Appendix B for more information about the data. These estimates of interaction terms between §936 exposure and other county characteristics build on the regressions shown in Table A.24. Standard errors clustered at the state and industry levels are shown in parentheses with p-values below. Observations are weighted according to employment in 1995.

Table A.26: Quantile Regression Estimates of the Effects of Repealing §936 on Employment Growth at the County Level

	(1)				
	q15	q25	q50	q75	q85
Exposure to Section 936	-0.011	-0.017	-0.033	-0.062	-0.083
	(0.011)	(0.012)	(0.013)	(0.013)	(0.014)
	0.327	0.159	0.009	0.000	0.000
Year Fixed Effects	Y	Y	Y	Y	Y
State Fixed Effects	Y	Y	Y	Y	Y
Industry Fixed Effects	Y	Y	Y	Y	Y

Notes: Author's calculations and data from NETS and QCEW. This table shows quantile regression estimates of Equation 5. See Appendix F for discussion of regressions and Appendix B for more information about the data. Standard errors clustered at the state and industry levels are shown in parentheses with p-values below.