Effects of International Tax Provisions on Domestic Labor Markets

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Abstract

We study the domestic labor market effects of two historical, but highly applicable, US international tax provisions. The first provision, "Check-the-Box," (CTB) decreased effective tax rates abroad by increasing the ability of multinational corporations to shift profits to low-tax jurisdictions. The second provision, the 2004 repatriation "holiday," (RH) decreased the tax costs of repatriating foreign earnings by 85%. To study the effects of each provision, we use a difference-in-differences framework that estimates the effect of local exposure to each provision on employment and earnings. According to our preferred specification, CTB resulted in losses of 1.7 million jobs and $64 billion in total labor earnings annually. We find the RH did not have any statistically significant effects on domestic labor markets. These results confirm the long-held, but empirically unsupported, hypothesis that international tax systems have large effects on domestic workers.

Keywords: international taxation, repatriations, tax avoidance, employment, earnings, local labor markets

JEL Codes: H2 H25 H32 J23 J38

1 Introduction

In 2018, the Tax Cuts and Jobs Act (TCJA) changed how the foreign earnings of U.S. multinational corporations (MNCs) were taxed. Prior to the reform, foreign profits of US MNCs were subject to foreign tax rates when earned. These profits were taxed again at a rate equal to the difference between the US and foreign tax rates in two instances: (1) when they were earned on passive income or were not permanently reinvested abroad and (2) when they were repatriated. In general, TCJA eliminated both the immediate taxation of passive profits and any additional taxes on repatriated earnings.[1] While most estimates suggest TCJA will and already has led to significant decreases in tax revenue, many proponents of the law argue that these losses are small compared to the increases in domestic business activity and employment that we will see as MNCs reinvest more profits back into the domestic segments of their businesses. Opponents of the law claim that in addition to the lost revenue, eliminating taxation on passive income earned abroad increases incentives to operate in foreign jurisdictions and shift income abroad through various means, resulting in decreased domestic business activity and employment as well as reduced domestic tax revenues.

To gain empirical leverage on this debate, we study the domestic labor market effects of two historical, but highly applicable, tax provisions. We find the 1997 "Check-the-Box" provision that led to lower foreign effective tax rates decreased domestic employment by 1.7 million jobs and decreased annual labor earnings by $66 billion per year. Conversely, the 2004 repatriation holiday, which decreased the tax rate on repatriated foreign earnings had no effect of domestic labor markets. Taken together these results imply provisions such as those included in TCJA which decrease effective tax rates on foreign earnings and decrease tax rates on repatriations are unlikely to improve domestic labor market outcomes.

The first provision we study, check-the-box (CTB hereafter), allowed firms to consolidate all foreign affiliates into a single taxable entity for the purposes of US tax filing. As a result, (1) flows of passive income within US MNC affiliate groups were no longer subject to immediate taxation under "Subpart F" rules and (2) sophisticated income shifting strategies became available to US MNCs. Recent empirical evidence suggests that these strategies worked; Blouin and Krull (2019) estimate that foreign effective tax rates dropped by more than 11% following CTB

[1] TCJA also included a number of much more minor provisions including a 12.5% global minimum tax that affected small subsets of MNCs.

implementation.

A priori, it is unclear how CTB and similar provisions that lower foreign effective tax rates should affect US domestic business activity. To the extent that foreign business operations are substitutes for US activity, we would expect firms to shift operations abroad, decreasing domestic business activity. On the other hand, if US and foreign activity are complements (as argued by Desai et al. (2009)), we would expect US domestic operations to increase following CTB implementation.

The second provision we study, Section 965 of the American Jobs Creation Act of 2004, allowed firms a one-time opportunity to repatriate earnings at 15% of their typical tax costs. This repatriation "holiday" (RH hereafter) significantly increased the incentive for firms to return profits earned abroad back to the United States. Under the holiday provision, US MNCs repatriated more than $290 billion in foreign earnings. Whether the repatriations generated by this holiday and by the TCJA's absolution of repatriation taxes will increase domestic business activity depends on the fungibility of foreign earnings with other sources of capital in financing domestic business operations. To the extent that firms can simply borrow against their unrepatriated earnings, we would expect to find no domestic business effects from firms returning money. On the other hand, if firms are cash constrained or would simply prefer to invest with retained earnings held abroad rather than domestic retained earnings or debt, we might see that the repatriation will have positive effects on domestic labor markets.

To study the effects of these provisions on local labor markets, we measure the exposure of each county in the US to each provision. We define exposure to CTB as the percentage of employees in each county working for US MNCs prior to implementation. Exposure to the RH is defined as the percentage of employees in each country working for a US MNC that repatriated foreign earnings as a result of the provision. We use a difference-in-differences event study framework to compare employment and total earnings in counties exposed to these provisions to the same outcomes in counties exposed to matched control groups of corporations, one for each policy.

Beginning with CTB, we validate our quasi-experimental approach by comparing worldwide effective tax rates of US MNCs to our matched sample of domestic firms. We find that worldwide effective tax rates trend similarly for the two groups in the preperiod. After the CTB provision is implemented in 1997, tax rates for MNCs begin declining relative to matched firms. Our

estimates suggest CTB decreased effective tax rates by 4.8 percentage points (hereafter, "pp."). These tax rate results highlight that this large shock to tax avoidance opportunities provides an excellent opportunity to examine the interplay between international tax avoidance and domestic labor markets.

Shifting our focus to domestic labor markets, we find local exposure to CTB decreases employment and total earnings beginning in 1997. Exposed local labor markets continue to deteriorate through at least 2003. We show the time pattern of domestic labor responses coincides almost exactly with trends in the establishment of "disregarded" entities, which are the primary vehicle by which firms take advantage of CTB. Estimates from our preferred specification indicate that one pp. increase in CTB Exposure decreases local employment by 0.44 pp. and local total earnings by 0.55 pp. during the sample period. This result is robust to a variety of threats to identification including industry or state level shocks and a variety of policies that recent papers have shown affected US local labor markets around the same time.

Based on our estimates we conclude that the US lost approximately 1.7 million jobs and $66 billion in labor earnings per year as a result of CTB. We conclude that (1) CTB and other laws such as the TCJA which decrease foreign tax rates or increase tax avoidance activities strip the US of business activity and that (2) foreign and domestic labor seem to be more substitutable than previous research has suggested.

In contrast, we find that local exposure to the $291 billion in qualified repatriations had no effect on local labor markets. We explore this surprising effect by comparing payout behavior by repatriating firms relative to our matched sample of non-repatriating MNCs. Prior to 2003, payouts between the two groups exhibited no differential trends. Then, from 2005-2008, payouts increased sharply for repatriating firms. Our estimates suggest that repatriating firms paid out 78 cents of every dollar repatriated to shareholders via dividends and share repurchases. To the extent that payouts don't stimulate local labor markets, the increase in payouts could explain our null result in this setting.

We also explore whether exposure to repatriations made by financially constrained firms and exposure to the headquarter locations of repatriating firms stimulate local labor markets. We find no effects on these margins suggesting that repatriations by financially constrained firms or payouts at the headquarter locations also do not increase domestic employment or earnings. Overall, our analysis of the 2004 Repatriation Holiday suggests that international tax policies

designed to stimulate domestic employment by lowering the cost of repatriation will not work.

This project makes several important contributions. First, the results are the first step in understanding how two specific tax provisions – CTB and the RH – affect domestic economic behavior. These findings are especially important with respect to CTB given the dearth of research on the effects of this provision. The little that we do know about the effects of CTB are highlighted by Blouin and Krull (2019) and Dunbar and Duxbury (2015). Both papers show CTB led to lower effective tax rates on foreign income. Analysis by Blouin and Krull (2019) suggests these lower rates were driven, at least in part, by increasing organizational complexity, which allowed for more advanced tax planning strategies. Dunbar and Duxbury (2015) further show that stock prices did not immediately respond to CTB announcements suggesting that the importance of the provision was lost on investors early on. The lack of stock price response is extra-interesting in the context of the large ETR decreases and domestic labor market effects that we document.

In studying the RH, we also add to our understanding of the causes and effects of repatriations driven by the 2004–2006 holiday. Blouin and Krull (2009) find that repatriating firms had lower investment opportunities, more free cash flows, and increases share repurchases in after the holiday relative to non-repatriating firms. Dharmapala et al. (2011) instrument for repatriations using pre-reform characteristics and also finds that the holiday led to a large increase in repurchases, yet yielded no additional employee compensation, capital investment, R&D, or even debt retirement. Our findings reinforce these in suggesting that repatriations driven by the holiday and repatriations, in general, do not affect the domestic business activity of US MNCs.[2]

Third, in addition to helping us better understand the effects of two specific provisions, the results presented herein contribute to the nascent literature documenting the domestic effects of international MNC activity and international tax policy more broadly. Desai et al. (2009) show that firms that invest abroad to take advantage of local economic conditions (as opposed to lower effective tax rates), increase domestic employment as well. Williams (2018) documents that US MNCs off-shore jobs to locations with lower corporate tax rates. Suárez Serrato (2018) shows that when US MNCs could no longer use Puerto Rico to avoid taxes, their business activity in the mainland declined dramatically. To these papers we add new evidence on the effect of foreign

[2]We note that these results are not in alignment with findings by Dyreng and Hills (2018), who follow a similar empirical strategy to ours in estimating whether the repatriations led to increases in employment. Our strategies differ in the construction of control groups and in how we define local exposure to repatriations.

effective tax rates and repatriation taxes on domestic employment and wages.

Fourth and finally, while our results help us learn more about the effects of CTB, the RH, and the domestic effects of international taxation in general, they also can also help us better design international tax regimes that balance domestic labor market concerns and government revenue goals. We find that lower effective tax rates on foreign income leads to reduction in local business activity while decreases in the tax rates on repatriations do not have comparable stimulative effects. The implications of these findings in this context are twofold. First, international tax systems that levy higher taxes on foreign income stimulate (or at least do not discourage) local employment and earnings. Second, tax systems that impose additional taxes on repatriated funds also do not stymie local labor market growth. Taken together, these conclusions suggest that TCJA and similar tax reforms, which both lower foreign tax rates and lower taxes upon repatriation do not optimally balance revenue and domestic labor market considerations.

The remainder of the paper is organized as follows. Section 2 explains details of the tax provisions that we study. Section 3 describes our data sources, the construction of our exposure measures, and the construction of our outcome variables. Section 4 presents the CTB domestic labor market analysis. In Section 5, we discuss the effect of the RH on domestic labor markets. Section 6 concludes.

2 The Tax Provisions

2.1 Check-the-box (CTB)

Historically, the US operated a worldwide tax system in which foreign income earned by US MNCs was taxed by the US at a rate equal to the difference between the US statutory rate and the foreign rate applied by the country in which the income was earned.[3] US taxation of "active" income was deferred until the income was repatriated. Taxation of "passive" income, on the other hand, was not allowed to be deferred. "Subpart-F" rules designated interest payments, royalties, and dividends as forms of passive income. Critically, such flows between related taxable entities triggered immediate US taxation. As a result, strategies designed to relocate income to low-tax jurisdictions via interest, dividends, or royalties payments between foreign affiliates resulted in immediate US taxation.

[3]In this section we describe the general rules of international business taxation for US MNCs, but there are sometimes exceptions.

On December 18, 1996, the US Treasury department released a new regulation, the "Simplification of Entity Classification Rules."[4,5] The regulation, which went into effect January 1, 1997, dramatically altered the way in which foreign affiliates were classified for US tax purposes. Rather than apply a standardized test, the new rules allowed MNCs to elect how their affiliates would be classified.[6] Under the new system, MNCs could simply "check-the-box" to have their foreign affiliates treated as "disregarded entities" from the perspective of the US tax authority. Income earned by disregarded entities was treated as earned by a single foreign affiliate group and payments made from one affiliate to another within the group did not violate Subpart F rules and, as a result, did not trigger immediate taxation.

Because foreign countries continued to consider the disregarded entities as corporations, while payments from one disregarded entity to another don't trigger US taxation, they were deducted from foreign taxes in one jurisdiction and taxed as income in another. This "hybrid" tax treatment afforded by the CTB regulation created numerous tax avoidance opportunities to US MNCs.

Consider loans between two foreign affiliates, one located in a low-tax jurisdiction and the other located in a high-tax jurisdiction. Prior to CTB implementation, interest payments on a loan from the low-tax to high-tax jurisdiction would have triggered immediate US taxation. After CTB, when the low-tax affiliate made a loan to the high-tax jurisdiction, interest expenses would be deducted in the high-tax jurisdiction and recorded as income by the affiliate in the low-tax country without triggering Subpart F rules.

Royalty payments between related parties for the use of intellectual property (IP) worked in a similar manner. To reduce their worldwide tax burden, MNCs often locate their IP in low-tax jurisdictions then license the use of the IP to affiliates located in high-tax locales. Royalty payments from the high- to the low-tax jurisdictions result in less taxable income in the

[4]Three main pressures led to the adoption of "Check-The-Box" rules. First, the IRS acknowledged that under pre-1997 regulations "taxpayers and the IRS must expend considerable resources on classification issues" (p. 21,990, Internal Revenue Service (1996)). Second, state entity classification tests were becoming more flexible and creating additional complications when adhering to both federal and local rules. Third, the IRS was concerned that smaller firms "may lack the resources and expertise to achieve the tax classification they want under the current classification regulations" (p. 21,990, Internal Revenue Service (1996)).

[5]For more on changes in the legal environment surrounding CTB implementation see Dean (2005) and Field (2008).

[6]Prior to 1997, MNCs were instructed to follow the "four-factor" test in determining whether foreign affiliates were taxable entities from the perspective of the US federal government. The four-factors were defined as (1) limited liability, (2) centralized management, (3) continuity of life and (4) free transferability of interest. When more than two factors applied to a foreign affiliate, the affiliate was considered a taxable entity and Subpart F rules triggered US tax liability on interest, dividends, or royalties earned by the affiliate were immediately taxable by the US.

high-tax jurisdiction and more taxable income in the low-tax jurisdiction, reducing overall tax payments. Prior to 1997, these royalty payments would trigger immediate US taxation. After CTB, payments between designated affiliates were disregarded for US tax purposes. Accordingly, we see large reductions in Subpart F income after 1996. Figure 1 displays aggregate Subpart F income per total assets for the largest 7,500 foreign affiliates of US firms firms from IRS SOI data (IRS, 2021). Prior to CTB implementation, the ratio of Subpart F income to total assets was approximately 0.85 percent. After CTB, this ratio decreased by more than a third, falling to approximately 0.57 percent.

[Figure 1 about here]

To take advantage of CTB, business entities must fill out IRS form 8832. Using this form, businesses can elect to be classified as a corporation, partnership, or an entity disregarded as separate from is owner (i.e. a disregarded entity). Figure 2 presents trends in entity elections made by foreign affiliates reproduced from Field (2008). Panel (A) shows that between 1997 and 2001 foreign affiliates made approximately 5,000 entity elections per year. A large majority of these were disregarded entity elections. Beginning in 2002, both disregarded entity elections and entity elections more broadly begin to rise steadily. By 2006, there were more than 17,000 entity elections. Approximately 12,000 of these were disregarded entities. Panel (B) shows the breakdown of disregarded entity elections made by foreign affiliates by initial elections or a change of status election. While both new elections and change of status elections increase over the 1997–2006 period, the increase in initial elections accelerates much more quickly during the 2002–2006 period than change of status elections.[7]

[Figure 2 about here]

Disregarded entities were the vehicle by which MNCs could implement complicated tax planning strategies. The clear rise in disregarded entity elections starting in 2002 suggests it may have taken some time for US MNCs to fully realize or the fully implement the organizational changes necessary to take advantage of the 1997 rule change. As a result, we might expect delayed effects of CTB on both effective tax rates and domestic labor markets.

[7] A similar regulation, the §954c6 "Look-Through Rule" was enacted in 2006. We stop our analysis in 2006 to isolate effects of CTB.

To summarize, the CTB regulations made income shifting across foreign affiliates a profitable strategy for US MNCs starting in 1997. Later, we show that CTB dramatically reduced worldwide effective tax rates for US MNCs relative to similar domestic firms in Section 4.1, consistent with complementary existing evidence from Dunbar and Duxbury (2015) and Blouin and Krull (2019). While CTB decreased effective tax rates on international profits when they are earned, it did not provide any tax relief if and when firms decide to ultimately repatriate these profits.

2.2 Repatriation Holiday (RH)

In 2004, the World Trade Organization ruled that the Extraterritorial Income Exclusion, an export incentive employed by the US, violated international treaties. Following the ruling, the European Union threatened to impose a series of substantial and escalating tariffs on US exports if use of the incentive was not discontinued. Firms and lobbyists, recognizing the impending suspension of the subsidy, began calling on Congress to enact a series of alternative, permissible measures to replace the subsidy. These efforts resulted in several interrelated provisions passed on October 22, 2004 as part of the American Job Creation Act (AJCA). These provisions were designed to benefit US MNCs and exporters affected by the repeal of the subsidy. The most visible of these provisions was a temporary tax holiday on repatriations of dividends from foreign subsidiaries.[8]

Under the tax holiday, 85% of qualified repatriations were deducted from US taxable income. Foreign taxes paid were still credited against taxes on the remaining 15% meaning that total US taxes on repatriations were very low, only 15% of the difference between US statutory and foreign income tax rates. The repatriation holiday was in effect from AJCA passage in Q4 2004 through the end of 2006.

To qualify for the holiday, repatriations had to meet several criteria. First, repatriations had to be paid in cash. Second, qualifying repatriations were capped by the maximum of (a) $500 million, (b) the earnings each firm classified as "permanently reinvested" on their latest financial statement, and (c) the amount the firm had historically repatriated from its foreign subsidiaries.

[8]The other interrelated provisions were the Domestic Productions Activities Deduction (DPAD), which allowed firms to deduct a percentage of income derived from domestic manufacturing activities from their tax bill and an expansion of expensing thresholds for small businesses. For more on the DPAD, expensing provisions, and their effects on business activity, see Ohrn (2018), Zwick and Mahon (2017), and Garrett et al. (2020). Other less consequential provisions included sales tax deductions for firms in states with no income taxes and a series of special interest tax breaks.

This maximum cap was reduced dollar-for-dollar by (i) the total debt the repatriating foreign subsidiary owed to related parties and (ii) the increase in related-party debt between the US MNC and the foreign subsidiary. Third, the repatriations were be used only for prescribed domestic business activities which consisted of hiring and training workers, investments in physical capital and intellectual property, R&D, financial stabilization and/or debt repayment that paved the way for job creation, acquisitions of some types of US business assets, and advertising and marketing expenses. To ensure that repatriations were directed to these uses, an investment plan approved by a firm's CEO and board of directors had to accompany the qualified repatriations. Despite the limited prescribed uses, Desai et al. (2009) estimate firms that repatriated were more likely to make payouts to shareholders, but were not more likely to make domestic investments, perform R&D, or pay off debt. Consistent with the lack of real business activity response, Blouin and Krull (2009) finds firms with relatively poor business prospects were more likely repatriate funds from abroad.

Figure 3, based on BEA data as assembled by Smolyansky et al. (2019), displays repatriations during the years 2003–2008. There is little evidence of any immediate increase in repatriations in the fourth quarter of 2004, however, repatriations increase slightly in Q1 and Q2 2005 before skyrocketing to approximately five times their pre-holiday levels in the second half of 2005.[9] In sum, $312 billion of repatriations during the holiday holiday window qualified for the tax holiday (Redmiles, 2008).

[Figure 3 about here]

3 Data

To measure the effect of both CTB and the RH on domestic labor markets, we rely primarily on four data sources: (1) financial statement data from Compustat (Standard & Poor's, 1980-2014), (2) FSP 109-2 disclosures of repatriations in response to the holiday collected by Blouin and Krull (2009), (3) business employment and establishment location data from NETS (Walls & Associates, 2012), and (4) county-industry employment and total earnings data from the Quarterly Census of Employment and Wages (QCEW, 2017).

[9] Desai et al. (2009) suggest regulatory details, which were not settled until after ACJA passage, led to this slightly delayed effect.

Our goal is to examine the effect of international tax provisions on domestic employment and earnings. Our general approach for each provision is to (1) identify firms that are affected by the provision, (2) construct an appropriate control set of firms, (3) to measure the geographic distribution of treatment and control group economic activity in the US, (4) define a policy exposure variable based on these geographic distributions, and (5) measure how employment and earnings evolve across local labor markets differentially exposed to each policy.

Before diving into the weeds, we pause to highlight two benefits of our local labor markets approach. First, by measuring outcomes at a geographic level based on administrative data, we capture both the direct effect of the provisions on affected firms and spillovers. In doing so, we capture the full domestic employment and earnings effects of the international tax changes. Second, a limited number of large and unique MNCs are affected by each of these policies. Establishing an appropriate control group of firms in this setting is hard. Local labor markets provide a more robust and less idiosyncratic laboratory in which to estimate unbiased and causal effects of tax and other policies affecting only a limited number of massive firms.

3.1 Matching Compustat Firms to Places

We start with Compustat. We focus on Compustat firms because publicly traded corporations represent almost all US multinational activity as measured by number of foreign affiliates and by assets held abroad. Blouin et al. (2012) report that from 1999–2004, 83% of US MNCs were publicly traded corporations that appeared in Compustat. During the period, these publicly traded MNCs owned just under 93% all US foreign affiliates and 95% of all US foreign assets.

To locate Compustat firms' activity, we need to know where their establishments are located. For this we rely on the NETS database, which lists establishments and employments count along with their geocoded location data. We match Compustat firms based on their 1996 company name and address to NETS establishments as defined by latitude, longitude and company name. We first geocode addresses in Compustat using ArcGIS then match names and latitude longitude coordinates to NETS using the reclink2 STATA package (Wasi and Flaaen, 2015).[10] Once we match a plant in the NETS data, we link the Compustat firm to the NETS headquarter using the HQDUNS identifier to achieve a firm-to-firm match. We check these matches manually and trim any obvious mistakes. Finally, we review unmatched Compustat firms and manually look

[10] See Appendix A for a more complete description of the match.

for their matches in the NETS data.

Figure 4 assesses the Compustat to NETS match and how the coverage of the match evolves between the passage of the two tax provisions in 1996 and 2003. As Panel (A) shows, we match just under 50% of Compustat firms to NETS in 1996. We match 60% of MNCs in the same year.

The firms we match are on average large and represent the lion's share of business activity in Compustat. Panel (B) shows our matched sample reports around 80% of total Compustat assets (AT) and 90% of total Compustat pretax foreign income (PIFO) in each year. The very high coverage of large, international firms assures us our analysis of both CTB and the RH is based on most US MNC business activity.

3.2 Measuring Exposure to CTB

To quantify exposure to CTB, we calculate the share of employees in each US county that work for MNCs in 1996 relative to the share working in a matched sample of control firms. We define US MNCs as firms in our matched Compustat-NETS sample reporting non-zero pretax foreign income in any year 1994–1996, the three years prior to CTB implementation. We define our group of potential control firms as firms in our matched sample with no pretax foreign income during the same years.[11,12]

We consider all MNCs treated by check-the-box as they have lower barriers to take advantage of the tax planning possibilities afforded by the policy. We propensity score match MNCs to potential control firms based on firm size (log of average total assets 1994–1996 winsorized at the 1st and 99th percentiles), and total asset growth rate from 1994 to 1996 (also winsorized at the 1st and 99th percentiles) and NAICS 2-digit fixed effects. We restrict all matches to be within same NAICS 1-digit sector.

Our matched sample consists of 816 US MNCs and 816 matched domestic firms. Figure 5 shows the distribution of our sample across NAICS 3-digit industries. The MNC and domestic distributions match each other nicely by industry. Table 1 presents a selection of descriptive statistics for the treatment and control sample.[13] Consistent with our understanding of multi-

[11] To the extent that these US domestic firms establish foreign affiliates in the post-CTB period, our results will underestimate the total effect of CTB on local labor markets.

[12] We exclude from our sample all firms in NAICS 2-digit sector 52, finance and insurance. We also drop all securities that are not publicly traded ($STKO$ equal to 0 or 3).

[13] Note, this descriptive table drops missing firms that are missing any of these variables in Compustat. One benefit of our approach is that we do not need to limit our sample of firms with complete financial statement data.

national firms, US MNCs were larger, did more R&D, and have less net operating losses. These differences highlight how challenging it is to study the behavior of larger international firms. The firms do not have any statistical differences in capital structure, investment as a share of assets, and valuation according to Market-to-Book ratios. To be clear, because we use a difference-in-differences estimation strategy, these level differences do not invalidate empirical design.

[Figure 5 about here]

[Table 1 about here]

Next, we use NETS data to construct a county-level level measure of relative exposure to MNCs, and therefore, a measure of domestic exposure to the CTB international tax provision. We define **CTB Exposure** as the percentage of workers in a county that work at the 816 multinational firms minus the share of workers that work in the 816 matched US domestic firms in 1996;

$$\textbf{CTB Exposure}_c = \frac{\sum_i Emp_{ic1996}\mathbb{I}(MNC_i = 1) - \sum_i Emp_{ic1996}\mathbb{I}(MNC_i = 0)}{Emp_{c1996}}, \tag{1}$$

where i denotes a firm in our sample, c denotes county, and $\mathbb{I}(MNC_i)$ is an indicator equal to 1 for MNCs in our matched sample.

We use this *differential* CTB exposure measure because we cannot directly observe the characteristics that make a particular location attractive to a large, publicly traded firms. MNCs are likely optimizing across domestic geographies according to some local characteristics. To the extent that the matched large, domestic firms make similar choices, this measure of exposure nets out effects of both observable and unobservable local characteristics that inform this choice.[14]

3.3 Measuring Local Exposure to the RH

We follow a slightly modified process in constructing our measure of local exposure to the RH. We begin with data from Blouin and Krull (2009) based on FSP 109-2 disclosures. The Blouin and Krull (2009) data identify 357 firms that disclosed repatriations in response to the holiday. We match these firms to our Compustat-NETS matched sample. We propensity score match

[14] In spirit, this identification strategy is similar to strategies used in Greenstone et al. (2010), Kline and Moretti (2014), and Berkes and Nencka (2020). These papers compare locations that were chosen with "runnner-up" locations, those that were almost chosen but were not. Here, we compare locations chosen by MNCs to locations chosen by their domestic peers.

these repatriating firms to non-repatriating US MNCs based on characteristics that Blouin and Krull (2009) find predict repatriation. These characteristics are (1) the change in foreign pretax income 2002–2004 scaled by worldwide pretax income, (2) the change in net income 2002–2004 scaled by worldwide assets, (3) the change in the firm's market to book value from 2002 to 2004, (4) the average operating cash flows divided by total worldwide assets over the period 2002 to 2004, (5) an indicator equal to one is the the average foreign tax rate from 2002 to 2004 is less than 0.35, the US statutory corporate income tax rate , (6) the average U.S. tax rate from 2002 to 2004, and (7) the ratio of foreign assets estimated as described in Oler et al. (2007) to total worldwide assets.[15] We restrict each match to be within the same NAICS 1-digit sector.

This procedure leaves us with a matched sample of 333 US MNCs that disclosed qualified holiday repatriations and 333 US MNCs that did not. Based on this matched sample we construct **REPAT Exposure** as the percentage of workers in each county working for repatriating firms minus the percentage of workers in each country working for the non-repatriating matched sample of firms.

Table 2 shows the balance in the characteristics that predict repatriation across the sample of repatriating MNCs and non-repatriating MNCs. The differences in the matching characteristics are not statistically significant for any variable. This balance highlights that our local labor market analysis provides an apples-to-apples comparison of local exposure to repatriating firms relative to local exposure to firms that had similar repatriation incentives but chose not to take advantage of the holiday.

[Table 2 about here]

3.4 Exploring Local Exposure Variables

We now describe the geographic variation in exposure to CTB and the RH. Figures 6 and 7 show the geographic variation in exposure to these tax policies through maps and through correlations with observable characteristics. The maps in the (A) panels of each figure show that there is considerable variation in exposure to both policies across and within states. In practice we include state-by-year fixed effects in all empirical specifications, so our identification only comes from within-state variation. The CTB map scale indicates that the county most exposed to CTB has 60.86 pp. more of its labor force employed in affected MNCs than in matched domestic firms.

[15] We winsorize each of these variables at the 1st and 99th percentiles.

The least exposed county has 52.82 pp. more of its labor force employed in matched domestic firms than in affected MNCs. The equivalent measures for REPAT exposure are 36.99 pp. and 54.12 pp.

We do find that exposure to the policies are correlated with some observable county characteristics and policy control variables, which motivates the care we take in crafting our empirical approach. For CTB, Panel (B) of Figure 6 shows that MNCs are more likely to be located in counties with more education, higher labor force participation, larger African American population share, and more manufacturing. There are also correlations with several policy control variables including training subsidies, exposure to Chinese imports, and share of non-routine work. We control for all of these correlates in most specifications. We address concerns regarding level differences in county characteristics by defining all outcomes as within-county changes. Further, since industry mix is correlated with the exposure variable, we will measure outcomes at the industry level and eliminate differential industry trends by employing industry-by-year fixed effects.

Similarly, REPAT exposure is also correlated with some county characteristics. Of note, repatriating firms are more likely to be located in places with a more educated population, a larger African American population share, and more manufacturing. They are more likely to be located in Right to Work states and are less likely to be in locations exposed to Chinese import competition. To reiterate, our empirical approach is designed to account for these differences.

[Figure 6 about here]

[Figure 7 about here]

3.5 Measures of Domestic Labor Market Activity

We use data from the Quarterly Census of Employment and Wages QCEW (2017) to measure our domestic labor market outcome variables. We use QCEW data to construct measures of local employment and total earnings at the county-by-3-digit-NAICS level. To analyze the effects of exposure to both policies, we transform each measure into percent point changes relative to the year prior to each policy implementation. For example, we measure our employment outcome for CTB as

$$\Delta Emp_{cjt} \equiv \frac{Emp_{cjt} - Emp_{cj1996}}{Emp_{cj1996}}$$

where c denotes county, j denotes NAICS 3-digit industries, and t denotes year. In analyzing the RH, we normalize outcomes relative to 2003.

4 Check-the-Box Analysis

In this section, we measure the effect of CTB Exposure on domestic labor markets. We begin by confirming that CTB differentially affects average effective tax rates (ETRs) for our sample of MNCs relative to our matched sample of US domestic firms.

4.1 Effect of CTB on Worldwide Effective Tax Rates

To measure the differential effect of CTB Exposure on tax rates of MNC and US domestic firms, we implement a modified dynamic version of the Dyreng and Lindsey (2009) methodology, which recovers worldwide ETRs. We use Compustat data to estimate regressions of the form

$$WWTE_{it} = \alpha + \beta PI_{it} + \gamma[PI_{it} \times MNC_i] + \sum_{y=1992,\ y\neq 1996}^{2006} \omega_h \Big(PI_{it} \times MNC_i \times \mathbb{I}(t=y) \Big) + \mu_i + \nu_{jt} + \varepsilon_{it} \tag{2}$$

where $WWTE$ is firm i's current worldwide tax expense in year t (total income comes net of deferred income taxes), PI is the firm's worldwide pretax income, MNC is an indicator equal to 1 if the firm is an MNE, $1[t=h]$ is an indicator equal to 1 in year t and μ_i and ν_{jt} are firm and industry-by-year fixed effects.[16,17,18]

Estimates from Equation (2) are interpreted as follows: β describes how much tax expense increases when an extra dollar of pretax income is earned by domestic firms. This parameter represents their average worldwide ETR. This is an average across firms and during the full sample period. γ describes how much *additional* tax expense is incurred when an MNC earns an additional dollar of pretax income. Adding β and γ together yields MNCs' worldwide ETR in the base year (1996). The ω_h parameter captures how much the tax penalty for being an MNC goes up or down relative to the penalty in 1996. If CTB had a dramatic impact on ETRs then we would expect a decrease in ω_h estimates after CTB implementation in 1997.

[16] Because our matched US domestic firm sample does not report any pretax foreign income, their worldwide tax expense and worldwide pretax income are simply the domestic analogs of their measures.

[17] WWTE and PI are scaled by total assets and winsorized at the 1^{st} and 99^{th} percentiles.

[18] Blouin and Robinson (2019) show that double-counting corporate profits across foreign affiliates may lead to biased estimates of the magnitudes and responses of foreign activity and profits in some data sets. However, this analysis does not suffer from this problem as Compustat data is based on consolidated financial statement data.

Figure 8 presents ω estimates from our dynamic Dyreng and Lindsey (2009) regressions. We also present β, γ, and pooled ω estimates in Table 3.[19] We present ω estimates from our preferred specification in Panel (A) of Figure 8. These include firm and industry-by-year fixed effects as well as firm-level control variables.[20] Three results are apparent. First, trends in ETRs do not differ between MNCs and matched domestic firms in the pre-period suggesting the matched domestic firms are an appropriate counterfactual. Second, upon CTB implementation in 1997, the tax rates of MNCs drop by about 2.5 pp. relative to domestics. Under the assumption that the ETRs of domestics pick up the effect of any statutory domestic changes, the drop is attributable to the foreign tax expense of the MNCs. Third, relative MNC tax rates continue to drop until 2003.[21] This delayed response suggests it may have taken some time for MNCs to take full advantage of the CTB regulation in their tax planning strategies. This timing roughly corresponds with the CTB election timing presented in Figure 2. The corresponding pooled estimate, presented in Column (3) of Table 3 suggests CTB decreased MNC worldwide ETRs by 4.84 pp. after 1997.

[Figure 8 about here]

[Table 3 about here]

In Panel (B), we show these patterns are robust to alternative specifications. In the first alternative, we include year instead of industry-by-year fixed effects and do not include firm level controls. In the second, we include firm and industry-by-year fixed effects, but no firm level controls. In the third, we run our preferred specification on all Compustat MNCs and a matched sample of domestics regardless of whether they exist in our Compustat-NETS matched sample. These plots correspond to columns (1), (2), and (4) of Table 3. Across all specifications, we find very similar patterns suggesting our dynamic Dyreng and Lindsey (2009) ETR estimates are robust to specification choice.

Overall, the ETR analysis presented in this subsection yields four three key results. First, CTB lowered worldwide effective tax rates for MNCs. Second, the absence of pretrends in

[19] In the pooled ω regression, we capture the magnitude of the relative decrease in ETR results by replacing the individual year interactions in Equation (2) with an indicator equal to 1 in years after CTB implementation.

[20] The set of control variables are the log of assets, net-operating-losses, long-term debt, advertising expenses, and R&D expenses. All control variables (other than log assets) are scaled by total assets, winsorized at the 1^{st} and 99^{th} percentiles, then interacted with (non-scaled) pretax income.

[21] Our β estimates are smaller than in Dyreng and Lindsey (2009). This is because we include firm and year (or industry-by-year) fixed effects in our regressions.

the analysis suggest our sample of matched domestic firms are a good control group for the MNC sample. Third, the timing of ETR effects roughly matches the slightly delayed trends in disregarded entity elections. We take comovement as suggestive evidence that disregarded entity elections facilitated international tax planning activities resulting in lower ETRs.

4.2 Domestic Labor Market Effects of Check-the-Box

To estimate the effects of CTB on domestic labor markets, we begin by estimating event-study regressions of the form

$$\Delta y_{cjt} = \alpha + \sum_{y=1992,\ y\neq 1996}^{2006} \beta_y \Big[\textbf{CTB Exposure}_c \times \mathbb{I}(t=y)\Big] + \mu_{st} + \nu_{jt} + \mathbf{X'}_c\boldsymbol{\gamma}_t + \epsilon_{cjt}. \quad (3)$$

Our unit of observation is a NAICS 3-digit county-industry. Δy_{cjt} is the percentage point change in the labor market outcome (employment or total earnings) relative to 1996 measured at the county-industry level. μ_{st} are state-by-year fixed effects. ν_{jt} are industry-by-year fixed effects. $\mathbf{X'}_{jc}\boldsymbol{\gamma}_t$ is a flexible vector of cross-sectional control variables interacted with year fixed effects that takes on different forms depending on the specification.[22] The sequence of β estimates describe the percentage point difference in the outcomes due to a one percentage point increase in CTB Exposure. In all analyses, we cluster standard errors at the county levels as this is the level at which our treatment variable is defined (Cameron and Miller, 2015).

The identifying assumption underlying this event-study difference-in-differences framework is that, in the absence of CTB, domestic labor market outcomes in locales with high levels of CTB Exposure would trend similarly to locales with low levels of CTB Exposure. The key threat to this identifying assumption is that other shocks coincident to CTB differentially affect high or low CTB Exposure units. While the validity of this assumption is inherently untestable, we design our approach to be resilient to many general concerns. In our preferred specification, we include industry-by-year fixed effects, state-by-year fixed effects interacted with population density bins, and a set of cross-sectional county-level control variables interacted with year fixed effects. These cross-sectional county-level controls include local exposure to import competition from China (Autor et al., 2016), exposure to NAFTA (Hakobyan and McLaren, 2016), differences in the share of routine labor (Autor and Dorn, 2013), property and sales tax rates (Suárez Serrato and

[22]Note that we do not include county-industry fixed effects as our outcome variables are percentage changes within county-industry units.

Zidar, 2018), state and county tax incentives and training subsidies (Bartik, 2017), local exposure to the DPAD and the ETI Ohrn (2018), and local exposure to bonus depreciation Garrett et al. (2020).[23] In the presence of these fixed effects and controls, our estimates are not based on concurrent shocks to any particular industries, states, communities with different densities, or a number of well documented economic phenomena that differentially affect units across space.[24] In addition, if other unobservable factors differentially counties according to CTB Exposure, then we might expect to see this in β_y estimates before 1996. We discuss additional tests below.

Figure 9 displays the event-study difference-in-differences estimates of CTB Exposure on domestic employment. Panel (A) displays β_y coefficients from our preferred specification. The figure shows that prior to CTB implementation, employment in high and low CTB Exposure counties trended similarly. After CTB implementation in 1997, a fairly immediate but relatively small drop in domestic employment. Small employment declines continue through 2001 then accelerate in 2002 and 2003 before stabilizing in years 2004–2006.

[Figure 9 about here]

In Panel (B), we show these patterns are consistent across a variety of alternative specifications. In the first alternative, we include only industry-by-year and state-by-year fixed effects. In the second, we add (to the first alternative) an interaction between the state-by-year fixed effects and five pre-period population density bins. In the third, we add our county-level policy control variables interacted with year fixed effects and pre-period employment growth bins interacted with year fixed effects. All specifications show no differential pretrends in the pre-period and employment declines that accelerate in years 2002–2003. Employment declines in all specifications are statistically significant in years 2003–2006.

To better understand the magnitudes these domestic employment effects, we run the following

[23] Note that any such variables that vary at the state-year level are already captured by the state-by-year fixed effects. Each of these cross-sectional control variables are more fully defined in Appendix C with additional robustness tests.

[24] Including state-by-year fixed effects eliminates concerns that state level policies that change during the period affect our estimates. Examples of state policies that have been shown to have domestic labor market effects include minimum wages (Freeman and Valletta, 1988), right-to-work laws (Meer and West, 2016), local taxes and revenues (Suárez Serrato and Zidar, 2018), R&D tax credits (Wilson, 2009), and state corporate investment incentives (Ohrn, 2019) and investment tax credits (Chirinko and Wilson, 2008).

regression that pools our β_y estimates,

$$\Delta y_{jct} = \alpha + \beta_1 \textbf{CTB Exposure}_c \times \textbf{1997–2002}_t + \beta_2 \textbf{CTB Exposure}_c \times \textbf{Post2002}_t + \mu_{st} + \nu_{jt} + \mathbf{X'}_c \boldsymbol{\gamma}_t + \varepsilon_{cjt}, \quad (4)$$

where most terms are defined as in Equation (3). We replace the individual year interactions with **1997–2002** and **Post2002**, which are indicators equal to one in period 1997–2002 and after 2002 respectively. Here, β_1 is the pp. employment change due to a one pp. CTB Exposure in years 1997–2002 relative to the pre-period. β_2 is the pp. employment change due to a one pp. CTB Exposure in years 2003–2006 relative to the pre-period.[25]

Table 4 presents our pooled difference-in-differences estimates of the effect of CTB Exposure on domestic employment. As we move from left to right, we include an increasingly complete set of controls and fixed effects to our model. Specification (3) is our preferred model and corresponds to estimates in Figure 9, Panel (A). Specifications (1), (2), and (4) corresponds to the first, second, and third specifications in Panel (B).[26]

[Table 4 about here]

Focusing on our preferred specification, we estimate a one pp. increase in CTB Exposure decreased domestic employment by 0.12 pp. in the 1997–2002 period and by 0.41 pp. after 2002. We provide further discussion of this magnitude and the resulting number of jobs lost in Section 4.3.1. Both the 1997–2002 and the post 2002 estimates are very similar across specifications. Short-run point estimates range from -0.06 pp. to -0.14 pp and are statistically indistinguishable across specifications. Longer-run effects range from -0.31 pp. to -0.53 pp., are all statistically significant at the conventional levels and, again, are statistically indistinguishable from one another. In short, we find CTB Exposure has small short-run and fairly large longer-run effects on domestic employment.[27]

We now repeat the same analysis from above switching the outcome to changes in total labor earnings. Event study difference-in-differences coefficients are presented in Figure 10. Table 5

[25] In these pooled difference-in-differences regressions, as with the variables of interest, we interacted controls with 1996–2002 and Post2002 indicators as opposed to individual year indicators.

[26] We rely primarily on Specification (3), as opposed to Specification (4), estimates in quantifying the effects of CTB on employment. Although Specification (4) directly compares county-industry that experienced similar employment growth rates in the pre-period, this may over-control and eliminate important variation. Adding pre-period growth bin-by-year fixed effects does not have any discernible effects on estimated effects.

[27] Appendix D shows additional robustness by including each individual county-level policy control separately. No single control seems to drive or substantially alter the long-run effects of CTB Exposure on employment.

contains our pooled difference-in-differences estimates. Analogous to the employment results, we see both (1) no differential pre-period trends between high and low CTB Exposure counties and (2) a large drop in total earnings in the period 2003–2006. In our preferred specification, we see a long-run decrease in earnings of 0.53 pp for a 1 pp. increase in CTB Exposure.

[Figure 10 about here]

[Table 5 about here]

We do see an interesting difference in the period 1997-2002 where total earnings do not appear to immediately drop. There are a number of plausible explanations for these divergent effects. One possibility is that, in the short-run, multinationals offshore low wage or part-time jobs while lower worldwide ETRs increase returns and wages for high wage or full-time workers. Unfortunately, the aggregate nature of the QCEW data does allow us to provide further evidence on these margins of adjustment.

4.3 Discussion of CTB Effects on Domestic Employment

In this section, we discuss several important aspects of our CTB estimates. First, we scale our estimates to find the number of jobs and total labor earnings lost due to the policy. Second, we analyze whether the effects we find are directly due to CTB. Third, we discuss how the precision of our CTB Exposure measure affects our estimates. Finally, we discuss how to interpret our estimated jobs and earnings losses in the presence of general equilibrium effects.

4.3.1 Magnitudes

In our preferred specifications above, we find that a 1 pp. increase in CTB Exposure led to a 0.41 pp. decrease in employment. Understanding how this magnitude relates to the difference in jobs lost or gained between heavily exposed and unexposed counties is challenging because CTB Exposure is the difference between exposure to firms benefiting from CTB and exposure to our matched control group. To interpret the jobs magnitude of our results more directly, we can estimate the effects of MNC Exposure and Matched Domestic Exposure separately. We present these results in Panel (A) of 12. Pooled difference-in-difference estimates are presented in Specifications (1) and (2) of Table 6. Based on the event study graphs, it is clear that all of

the employment decreases are driven by MNC Exposure. Effects of Matched Domestic Exposure are positive, but small and statistically insignificant.

Focusing just on MNC Exposure, we find a 1 pp. increase in exposure leads to a 4.7 pp. decrease in employment. Average MNC Exposure is 4.8% of workers, suggesting employment in the average county dropped by 2.3% relative to a completely unexposed county. By multiplying this change by the base employment in 1996, we can recover the total jobs lost due to CTB under the assumption that unexposed counties are totally unaffected by the policy. We fully admit this is a strong assumption and discuss its merits in depth in Section 4.3.4. Our QCEW sample gives us a domestic employment count of 87.6 million jobs in 1996.[28] Therefore, our 2.3% of jobs lost is equivalent to 2 million jobs.

To perform the same thought experiment with our preferred CTB Exposure estimates, which implicitly account for any unobserved place-based characteristics valued by both MNCs and large domestic firms, we estimate the difference in employment growth between counties that had no MNC exposure and had average MNC exposure. This means we, again, multiply our point estimates by the average share of MNC workers in a county. Based on our preferred CTB Exposure specification (Column (3) of Table 4), we find CTB Exposure led to a 2% decrease in employment relative to a county with no MNC exposure. Multiplying this average effect by the 87.6 million QCEW base suggests CTB decreased domestic employment by 1.7 million jobs relative to a world with no CTB Exposure.[29] To the extent that QCEW only counts a fraction of the workforce and our estimates apply to all employment, this number underestimates the domestic labor market effects of the policy. The US Bureau of Labor Statistics reports total nonfarm employment in the US in 1996 was approximately 121 million. Using this estimate, we find CTB led to the loss of 2.4 million jobs. This number is likely an upper bound because the base includes non-profit and government employment.

In a similar manner, we scale the Table 5 findings to calculate earnings losses. Using our preferred specification, we find total earnings decreased by 2.6 percent. Taking QCEW's reported $2.5 trillion in total earnings (wages and salaries) in 1996 as our base, we estimate the CTB regulation decreased total earnings by approximately $64 billion ten years after the regulation was adopted. This number represents 0.5 percent of 2006 GDP. While this number is striking,

[28] We limit our sample to private, non-farm employment in QCEW.

[29] Equivalent estimates are presented in Table 4 in the row labeled "Average Change in Employment '' for alternative specifications.

we recognize that we do not take full account of the effect of the policy on GDP. For example, firm owners may see gains due to lower worldwide effective tax rates that partially or even fully offsets these losses. Further, these domestic job losses may be paired with job gains overseas. Unfortunately, we cannot speak to these welfare margins.

4.3.2 Attributing Effects to CTB

The results above show counties with more MNC employment in 1996 saw worse labor outcomes in the following decade than counties with relatively less MNC employment. The ultimate concern is that these effects are due to other shocks to MNCs rather than the CTB provision. A particular concern is that global competition resulted in a race-to-the-bottom in corporate tax rates during our sample period. In this section, we present two pieces of evidence suggesting that CTB and not other shocks to MNCs like this are driving our results.

One way to directly attribute our labor market results to the CTB provision is to formally compare the timing of the effects we estimate DE elections, which were the principle avenue by which firms could shift income to low tax jurisdictions via the CTB provision. To make this formal comparison in aggregate DE elections (from Figure 2, Panel (B)) to employment losses due to CTB (from Figure 9 Panel (A)), we scale CTB Exposure by aggregate DE elections in each year. This changes the interpretation of the β_y coefficients to be the pp. change in employment resulting from every thousand DE elections. We present these rescaled coefficients in Figure 11. The figure shows that each 1,000 disregarded entity elections result in between -0.1 pp and -0.22 pp. On average, each 1,000 disregarded entity elections lead to a -0.17 pp. decrease in total domestic employment. The key is the stability of these estimates; every 1,000 disregarded entity elections has a strikingly consistent effect on domestic labor market outcomes. The smaller employment effects in 1997–2001 match the lower levels of DE elections while the large employment effects later coincident with many more DE elections. Given the co-incidence of the patterns, it is hard to reject the hypothesis that DE elections have a direct impact on US workers.

[Figure 11 about here]

As an additional comment on the delayed response, we refer back to the ETR analysis presented in Figure 8. The results there also show a delayed response in ETRs. This trend is

empirically similar to findings in Blouin and Krull (2019) that suggest US MNEs increase the length of their affiliate chains and organizational complexity in response to CTB especially after 2004. The delayed responses in ETRs, organizational complexity, DE elections, and labor market effects are all consistent with Slemrod (1992), who theorizes and finds empirical support for a hierarchy of responses to changes in tax law that progresses from timing changes to financial accounting changes and finally to real behavioral changes.

Next, we directly rule out global tax competition as a driver of our results. To do so, we construct new exposure variables using Exhibit 21a data from Dyreng and Lindsey (2009), which describe the location of foreign affiliates. We start by identifying the OECD countries that cut corporate tax rates by ten or more percent in 1997–2006. We then construct a CTB Exposure measure based on MNCs that had an affiliate in one of these "Big Tax Decrease" countries and their large domestic matched counterparts.[30] We construct a CTB "Small Tax Decrease" measure in a symmetric manner. We display event study estimates in Panel (B) of 12. Pooled difference-in-difference estimates are presented in Specifications (3) and (4) of Table 6. We find very similar effects for both exposure measures. Most importantly, we do not find larger labor market effects of for exposure to MNCs with affiliates in the countries with the largest tax cuts. We interpret this result to suggest that our findings are not driven by the global race-to-the-bottom in corporate tax rates during the sample period.

4.3.3 Precision of CTB Exposure Measure

Our CTB Exposure measure is based on the assumption that firms that had an international presence in 1996 were better positioned to take advantage of the tax planning advantages of CTB than those that did not. While this assumption is, on average, fairly innocuous, it is certainly not true that every MNC was able to take advantage of CTB.[31] If we were able to construct an exposure measure based solely on MNCs that were well positioned to take advantage of the regulation, then we would expect to estimate larger negative effects on domestic employment

[30] These countries are Ireland, Poland, Slovakia, Germany, Italy, Czech Republic, Turkey, Portugal, Iceland, and Japan.

[31] Recall, MNCs best positioned to benefit from the tax planning strategies afforded by CTB were those with both a large business presence in a high tax country and another affiliate in a low tax jurisdiction. After 1996, these types of firms could now shift income to the low tax jurisdiction via intra-company interest or royalty payments without triggering US taxation under Subpart F rules.

and labor earnings.[32,33]

A second way in which the precision of the CTB Exposure measure affects our results is that firms in the matched domestic sample may be able to initiate foreign operations and take advantage of CTB during our estimation window. In fact, many do. Appendix Figure E1 shows the percent of MNCs and (separately) the percent of matched domestic firms that report positive PIFO in a given year. As is evident from the figure, nearly 25% of domestic firms report positive PIFO by the end of the sample. If exposure to these domestic that because MNCs also leads to job and earnings losses, then this imprecision in our CTB Exposure measure would also lead us to underestimate the negative domestic labor market effects of CTB.

4.3.4 Implications of Potential GE Effects

Our aggregate estimates presented in Section 4.3.1 are made under the strict assumption that unexposed counties were unaffected by the CTB regulation. The two most salient violations of this assumption are due to (i) potential reallocations of capital between MNCs and domestics and (ii) potential reallocations of people between exposed and unexposed locales. With regard to (i), in our context, investors are likely to reallocate funds toward MNCs that can take advantage of lower worldwide tax rates. This would decrease available capital in unexposed places. To the extent that this also decreases employment in unexposed places, this channel pushes our estimates toward zero and understates the effect of CTB.

With regard to (ii), in Appendix F, we estimates the effect of CTB exposure on domestic population changes. We find no population (and likely migration) responses to CTB Exposure. In Appendix G, we scale our employment outcomes by these population numbers. We continue to find large effects of CTB on employment that follow the same qualitative patterns as our in our preferred specification. These results suggests migration patterns in response to the policy do not affect our estimates.

4.4 Summarizing CTB Results

Overall, we find counties heavily exposed to CTB saw large employment and earnings declines seven to ten years after the regulation was adopted. We estimate tax planning opportunities

[32]Because we do not have data on the international structure of the MNCs in our sample, we cannot directly observe these types of MNCs and elect to treat all as potential beneficiaries of the regulation.

[33]This process would also decrease the average county-level exposure we measure and, therefore, would likely have little effect on the aggregate implications of the provision.

enabled by CTB led to losses of 1.9 million jobs and $65 billion in total labor earnings per year. Timing comovements between our estimated effects and drops in MNC ETRs, increases in DE elections, and increases in MNC organizational complexity support the international tax planning mechanism. To the extent that our CTB Exposure variable is measured or findings pick up some GE effects of the policy, our estimates likely understate the effect of the CTB regulation on domestic job and earnings losses.

5 Repatriation Holiday Analysis

In this section, we measure how repatriations during the 2004 one-time repatriation holiday affected domestic labor markets.

5.1 Repatriating Firms and Match Quality

According to Blouin and Krull (2009), during the repatriation holiday, 357 firms repatriated $291.6 billion. This is a small subset of US MNCs. The firms that chose to take advantage of the holiday differed from other MNCs on a variety of margins. They experienced lower growth rates in their foreign incomes. Their ROAs were growing relatively slowly. They were declining in valuation. They had high levels of free cash flows. They were more likely to have a lower foreign tax rate than domestic tax rate and their domestic tax rates were relatively high. They had much higher shares of foreign assets. Despite these differences, because there are so many US MNCs and so few repatriated, we were very successful in constructing a subset of firms that look nearly identical to the repatriating MNCs on every one of these margins. (As a reminder) Table 2 displays descriptive statistics and a test for differences in means on the variables that Blouin and Krull (2009) find predict repatriation. No differences are statistically significant.

The assumption underlying our local labor markets approach is that the places in which the repatriating and non-repatriating matched sample of firms select to operate would experience the same trends in their labor market outcomes in the absence of the holiday and subsequent repatriations. Because we are able to find such a precisely matched control group, we are confident that any unobservable factors governing location choice for repatriating MNCs are likely to similarly govern the behavior of the control group, too.

5.2 Domestic Labor Market Effects of the Repatriation Holiday

We now estimate the effects of the RH on domestic employment and earnings in much the same way we analyzed the domestic effects of CTB. We run difference-in-differences event-study regressions of the form

$$\Delta y_{cjt} = \alpha + \sum_{y=1999,\ y\neq 2003}^{2012} \beta_y \Big[\textbf{REPAT Exposure}_c \times \mathbb{I}(t=y)\Big] + \mu_{st} + \nu_{jt} + \mathbf{X'}_c\boldsymbol{\gamma}_t + \varepsilon_{cjt}. \quad (5)$$

Outcomes are pp. changes in industry-county employment and total earnings relative to 2003. The β_y estimates—our coefficients of interest—show the effect of an additional one pp. increase in REPAT Exposure affects the outcome in each year.[34] Our preferred specification in analyzing the effects of the RH is the same as our preferred CTB specification. We include industry-by-year fixed effects, state-by-year fixed effects interacted with population density bins, and our suite of county-level cross-sectional policy controls interacted with year fixed effects.

Panel (A) of Figure 13 presents our preferred estimates of the effect of the RH on domestic employment. Critically, the figure displays no differential pretrends. That domestic labor market outcomes in counties where repatriating MNCs operate match the trends in counties where the control group is located strongly supports our assertion that the precisely matched sample of firms also generated a precisely matched sample of places.

[Figure 13 about here]

While our preferred event study specification displays no differential pretrends, it also displays no statistically significant effect of repatriations due to the holiday on domestic employment. To be clear, all post-period estimates are positive, but not statistically significant even at the 10% level. Alternative specifications presented in Panel (B) of Figure 13 show similar patterns. Across all three alternative specifications, there are no obvious pretrend issues, although omitting interactions between the state-by-year fixed effects and population density bins as well as the policy controls has two point estimates in the preperiod that are statistically different from zero. But, even in this specification, we find a relatively narrow zero during the holiday and no statistically significant effect during the years 2007–2012. Our most demanding specification,

[34] Recall, REPAT Exposure is measured as the share of the population working in repatriating MNCs relative to the share working in the non-repatriating control group of MNCs.

with all controls, our most robust set of fixed effects, and controls for pre-period employment growth rates shows no pretrend nor statistically significant effects in any year after the holiday.

As with the CTB analysis, we quantify these results by running pooled difference-in-differences regression of the form

$$\begin{aligned}\Delta y_{jct} = \alpha &+ \beta_1 \textbf{REPAT Exposure}_c \times \textbf{2004–2006}_t \\ &+ \beta_2 \textbf{REPAT Exposure}_c \times \textbf{Post2006}_t + \mu_{st} + \nu_{jt} + \mathbf{X'}_c \boldsymbol{\gamma}_t + \varepsilon_{cjt}. \end{aligned} \tag{6}$$

Here, β_1 and β_2 measure the employment growth relative to the preperiod due to a pp. increase in REPAT Exposure in the years 2003–2006, and in 2007 and beyond. We segment our findings into these two periods to match years during and after the holiday. We present the β_1 and β_2 estimates from our four specifications in Table 7.

[Table 7 about here]

Consistent with the event study analyses, the pooled estimates also show no statistically significant effects of the RH on domestic employment either contemporaneous to the holiday nor in the aftermath in any of the regressions. In our preferred specification (Column (3)), our post 2006 point estimate suggests a 1 pp. increase in REPAT Exposure is associated with a 0.2 pp. increase in employment. This estimate is not statistically different from zero at even the 10% level. Given that only 2% of workers in the average county are employed by repatriating MNCs, our point estimate and standard error suggest we can rule our with 95% certainty any employment effects of the RH above 0.9%. For context, the upper bound for our CTB employment effect is more than three times larger in magnitude (-3.1%).

Appendix Figure I1 and Appendix Table I1 present event study and pooled difference-in-differences results using the total earnings outcome. Event study trends coefficients match employment analysis almost exactly. As with employment, across all specifications, we find REPAT Exposure never has a statistically significant effect on domestic earnings.

5.3 Mechanisms Behind the Null Effect

Our RH results thus far beg the question, "How could a nearly $300 billion cash infusion *not* result in increases in domestic employment and wages?" Both Dharmapala et al. (2011) and Blouin and Krull (2009) suggest a potential explanation: a large proportion of repatriations

were paid out to shareholders and not invested in domestic productive capacities as the AJCA intended. Focusing just on 2005, Blouin and Krull (2009) find that firms paid out approximately 20% of repatriated funds. Instrumenting repatriations with indicators for a lower corporate tax rate abroad and possession of an affiliate in a low tax jurisdiction, Dharmapala et al. (2011) estimate firms paid out 60 to 92% of repatriated funds to shareholders in 2005.

To explore whether this potential mechanism is responsible for our null REPAT findings, we measure the effect of the repatriation decision on total payouts by running a regression of the form

$$\text{Payouts}_{it} = \alpha + \sum_{y=1999,\ y\neq 2003}^{2010} \beta_y \Big[\textbf{REPAT}_i \times \mathbb{I}(t=y)\Big] + \mathbf{X'}_{it}\boldsymbol{\gamma} + \mu_i + \nu_{jt} + \varepsilon_{it} \tag{7}$$

where Payouts_{it} are a firm's total payouts in year t (the value of shares repurchased plus total dividends) divided by average assets in the pre-period, **REPAT** is an indicator equal to one for repatriating firms and zero for the matched control sample. $\mathbf{X'}_{it}$ is a vector of time-varying control variables that match the Dharmapala et al. (2011) analysis.[35] $\mu_i + \nu_{jt}$ are firm and industry-by-year fixed effects. Given this specification, β_y captures the level differences in total payouts scaled by assets in year y.

Coefficient estimates from Equation (7) are presented in Figure 14. We present estimates from our preferred specification in Panel (A), which includes firm and industry-by-year fixed effects as well the same time varying firm-level controls as in Dharmapala et al. (2011). We present estimates from alternative specifications in Panel (B). Relative to our preferred specification these exclude time varying firm-level controls, use year instead of industry-by-year fixed effects and include the entire Compustat universe of repatriating firms and a matched control group (as opposed to restricting to our matched Compustat-NETS sample). Pooled difference-in-differences coefficients are presented in Table 8.

[Figure 14 about here]

[Table 8 about here]

Across all specifications we find similar patterns; payouts in the pre-period do not exhibit differential trends, further reinforcing our assertion that our matched MNC control group provides

[35] The set of control variables are (1) a proxy for Tobin's q, defined as the book value of firm debt plus the market value of firm equity less the book value of firm equity divided by the book value of firm assets, (2) cash scaled by total assets, and (3) ROA.

a suitable counterfactual for the repatriating firms. Beginning in 2005, payouts by repatriating firms increase and are statistically different from those of the matched control group in 2006 and 2007 before reverting to their original level in 2009–2010.

In Appendix H, we replicate all payouts analysis using total dollars repatriated scaled by average pre-period assets instead of the simple indicator for repatriating firms in our matched sample. This scaling allows us to estimate how many cents on the dollar of repatriations were paid out to shareholders. Across all specifications, we find very similar patterns and effects. Repatriating firms increase payouts by statistically significant amount in 2006 and 2007. Based on the results in Panel (A) of Appendix Figure H1, we estimate that for every dollar repatriated, firms increased payouts by 78 cents during the 2004–2008 period. Assuming that reinvestment as opposed to payouts creates jobs and increases earnings, this finding could largely explain the null effects of the RH on domestic labor markets.

We pause to note that these results provides the most complete evidence to date on the effect of the repatriation holiday on firm payouts. Prior work focused on contemporaneous payouts occurring only in 2005. We document, for the first time, that the largest payouts resulting from the holiday occurred in 2007 and that payouts reverted in 2009 and 2010. Finally, we also provide the first evidence that payouts trended similarly between repatriating and non-repatriating firms, suggesting anticipation effects and responses to the 2003 dividend tax cut do not drive the Dharmapala et al. (2011) and Blouin and Krull (2009) results. Although clearly a supporting result, we nonetheless feel these novel findings elucidate more fully the dynamics surrounding the repatriation holiday.

Although only 20% of repatriations were not distributed to shareholders, it is possible some types of firms were more likely to retain and reinvest repatriations. Based on the hypothesis that investments by financially constrained firms are more responsive to cash injections, we split our sample of repatriating and control firms by the median, in-sample value of the Hadlock and Pierce (2010) measure of financial constraint, which is based on firm size and age, and repeat our event study difference-in-differences payout analysis for both groups. These results are presented in Figure 15. Pooled difference-in-differences coefficients are presented in Columns (4) and (5) of Table 8. Both the constrained and unconstrained samples show patterns similar to the full sample. That is, payouts do not differ in the pre-period then increase in years 2005–2008 by both financially constrained and unconstrained firms.

[Figure 15 about here]

Given that payouts / retained repatriations are not concentrated by financial constraint, we wouldn't expect to find differences in labor market effects along this same margin. Nonetheless, we explore this possibility by creating separate measures of REPAT Exposure for financially constrained and unconstrained MNCs where we define financial constraint using the Hadlock and Pierce (2010) measure of financial constraint as above. We present event study coefficients for the employment outcome in Panel (A) of Figure 16 and pooled magnitudes in Columns (1) and (2) of Table 9. For locations with exposure to unconstrained firms, we see no pre-trends and no effects on employment after the holiday. For locations with exposure to constrained firms, the picture is slightly more messy. These locations seemed to be experiencing faster employment growth in years 2002 and 2003 that stagnated when funds were repatriated. While slightly more challenging to interpret, suggests that repatriation by more financially constrained firms did not stimulate domestic labor markets. Given that payouts responses did not differ by financial constraint, it is unsurprising that domestic labor market effects were also both zero.

[Figure 16 about here]

[Table 9 about here]

An empirical weakness of our local labor markets approach in this particular context is that payouts made to shareholders may be reinvested and stimulate domestic labor markets in places where repatriating MNCs are not located. To explore whether this weakness undermines our identification strategy, we rely on a large literature documenting "home bias" in investment (Coval and Moskowitz, 1999; Huberman, 2001). Home bias is the phenomenon that investors are more likely to make local investments where local may be defined at the federal or sub-federal level. We exploit this phenomena by recognizing that payouts made from repatriations are likely to be concentrated near headquarters (HQ) locations of repatriating MNCs. There are two reasons for this. First, local inhabitants are more likely to own stakes in MNCs which they live near and second, managers who work in HQ locations are likely to be large shareholders. If payouts are concentrated near HQs, then home bias would suggest these payouts are likely to be reinvested near HQs and thereby stimulate local labor markets.

We explore this line of reasoning by creating two REPAT exposure variables, one based only on HQ exposure, the other based on nonHQ exposure. We present event study results in Panel

(B) of 16 and corresponding pooled DD estimates in Columns (3) and (4) of Table 9. The nonHQ series nearly identically matches our results based on the full sample REPAT Exposure measure. This is by construction because most firm establishment and employment of large MNCs are not located in HQ locations. The HQ Exposure series has larger coefficients throughout the analysis period and the 2008 HQ Exposure coefficient is statistically significant at the 5% level. Focusing on the pooled coefficients, point estimates on HQ Exposure seem to increase employment by 0.5 pp. in the years 2007–2012, however this estimate is not statistically significant nor does it constitute an economically meaningful effect. Less than 0.1% of employees in the average county work at HQ locations of repatriating firms. Multiplying this base by the pooled Post 2006 effect suggests an overall effect of repatriations of 0.05% of 2003 employment or 49,000 jobs and implying every $6 million in repatriated funds created a single domestic job.

If home bias plays a significant role in determining investment behavior (which a large and growing body of evidence suggests), then our HQ domestic labor market analysis suggests the payouts to shareholders resulting from the repatriation holiday were not reinvested in ways that stimulated domestic labor markets.

5.4 Summarizing RH Results

Overall, we do not find that repatriations made during the holiday led to any discernible labor market effects. We find the holiday did lead to increased payouts by repatriating firms, but that these payouts did not provide significant stimulus even in places in which the funds were most likely to be reinvested.

An interesting way to interpret these results is that the repatriation holiday did not negatively impact domestic labor markets. While a one time holiday may bring back a significant amount of cash now, it may induce moral hazard if firms believe such a holiday could happen again. If so, firms may assume lower effective tax rates on operations abroad and move to shift production to these now relatively lower tax jurisdictions.[36]

[36]We recognize that to the extent that all MNCs' expectations of a future holiday increased, our REPAT Exposure analysis would not show a decrease in domestic labor market activity. Note, that our CTB Exposure estimates in 2003–2006 are stable, suggesting changes in the expectations of future holidays by the universe of US MNCs did decrease domestic employment or earnings.

6 Conclusion

In this paper, we analyzed the domestic labor markets impacts of two recently historical, but presently applicable, tax provisions. We find the Check-the-Box provision, which increased the ability of US MNCs to shift income out of high-tax foreign jurisdictions, dramatically decreased domestic employment and earnings. We estimate US employment and labor earnings dropped by 1.7 million and $66 billion per year respectively as a result of the provision. In contrast, we find no domestic labor market effects of the 2004 repatriation holiday which lowered the tax rate on repatriations and resulted in cash flows to the US mainland of $300 billion for repatriating MNCs.

Our study has three key limitations. First, because we do not have access to affiliate level employment and earnings data from US MNCs, it is challenging to uncover the precise firm behaviors that led to the US labor market losses we document. We look forward to research addressing these mechanisms based on BEA or IRS data. Second, we do not document the effect of the policies on foreign employment. This may be possible if a mapping (complete or partial) between US MNCs and foreign labor markets can be credibly established. Finally, our local labor markets approach is based on cross-sectional variation so general equilibrium effects such as those generated by payouts to shareholders that do not live near repatriating MNC establishments are not identified. While we have designed and implemented tests to combat this concern, we believe other identification strategies may be successfully employed to mitigate and/or capture these types of GE effects.

Despite these limitations, our results are directly applicable to the current policy environment. Over the past decade, we have seen a renewed focus on designing policies to create jobs and increase wages, especially with regard to international taxation and trade policies. Our findings suggest that evidence based international tax policies designed to promote domestic labor markets should include rules making profit-shifting from high-tax foreign affiliates to low-tax affiliates costly. While it is unclear if the implementation of such rules will create domestic jobs, we have shown that in the past, the elimination of such regulations dramatically harmed domestic US workers.

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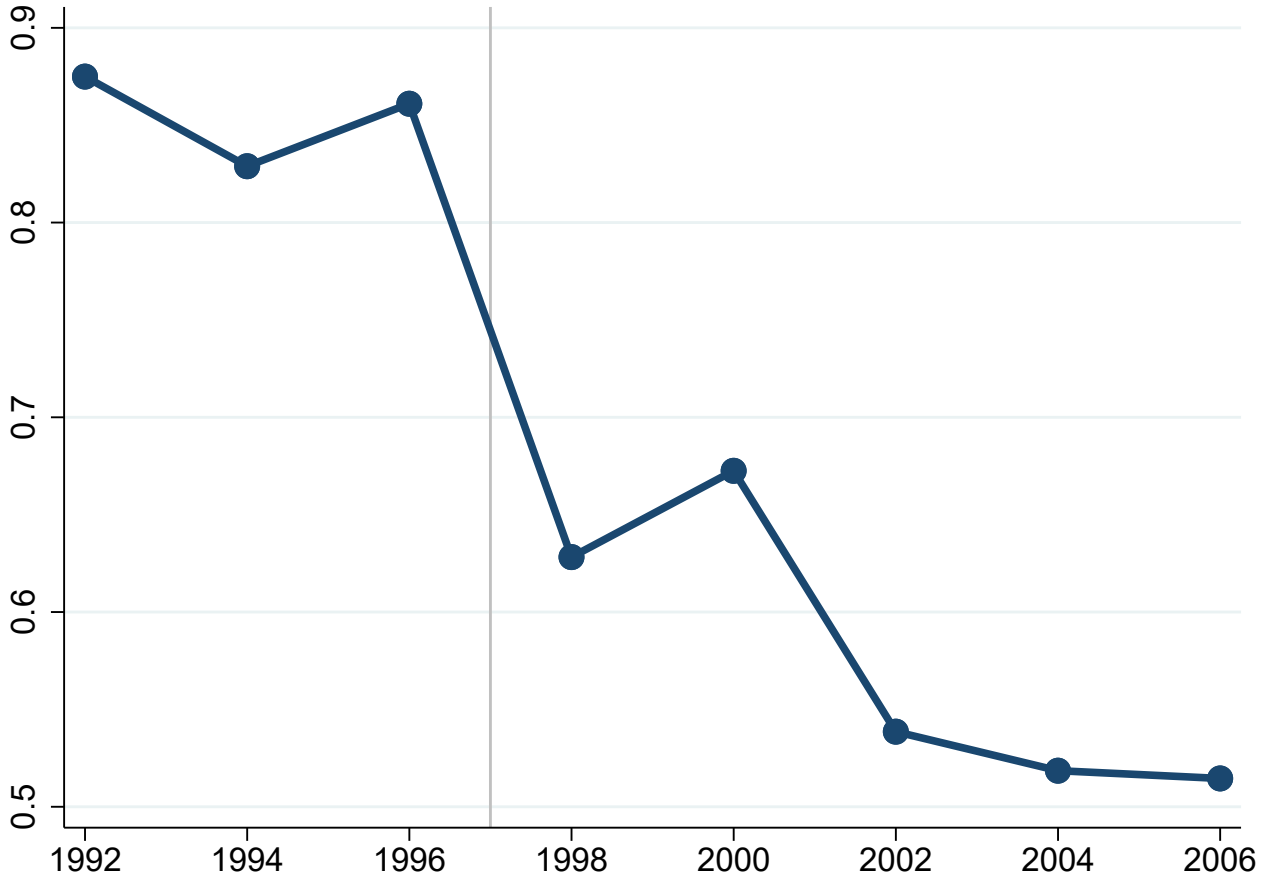
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7 Figures

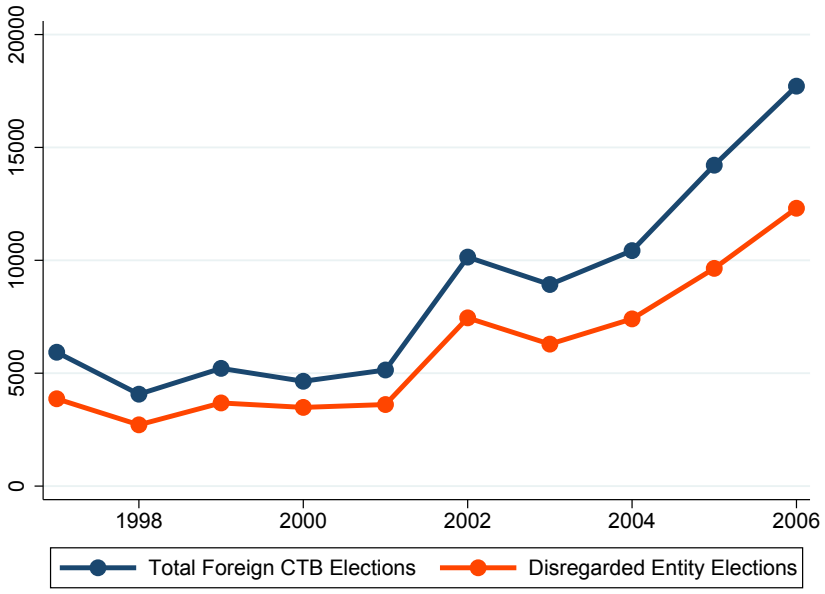
Figure 1: Aggregate Subpart F Income Per Total Assets



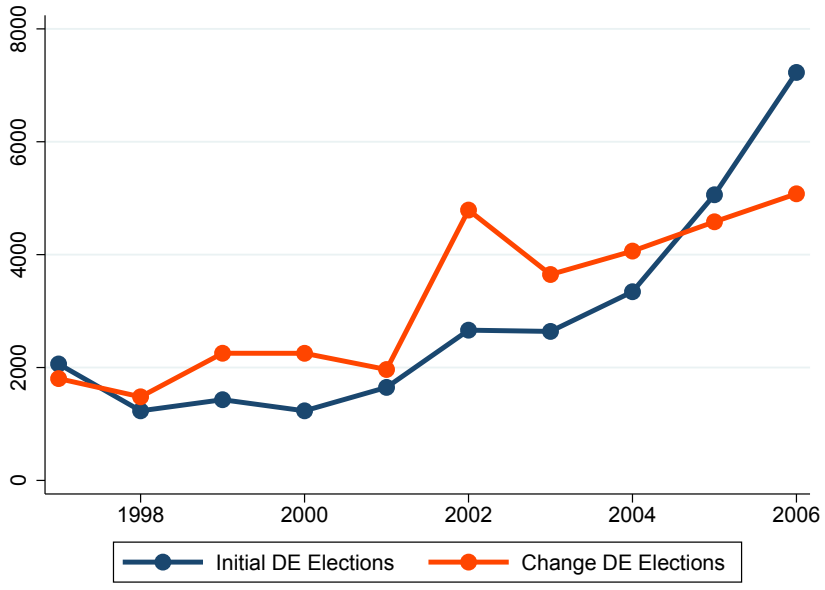
Notes: Figure 1 displays aggreagate Subpart F income per total assets reported by US controlled foreign corporations (CFCs) in even years 1992–2006 based on IRS SOI data (IRS, 2021). During the years 1992–2002, the sample is the largest 7,500 CFCs. In years 2004–2006, the sample includes all CFCs.

Figure 2: Trends in Check-the-Box Elections

(A) Total CTB Elections and Total Disregarded Entity Elections of Foreign Affiliates

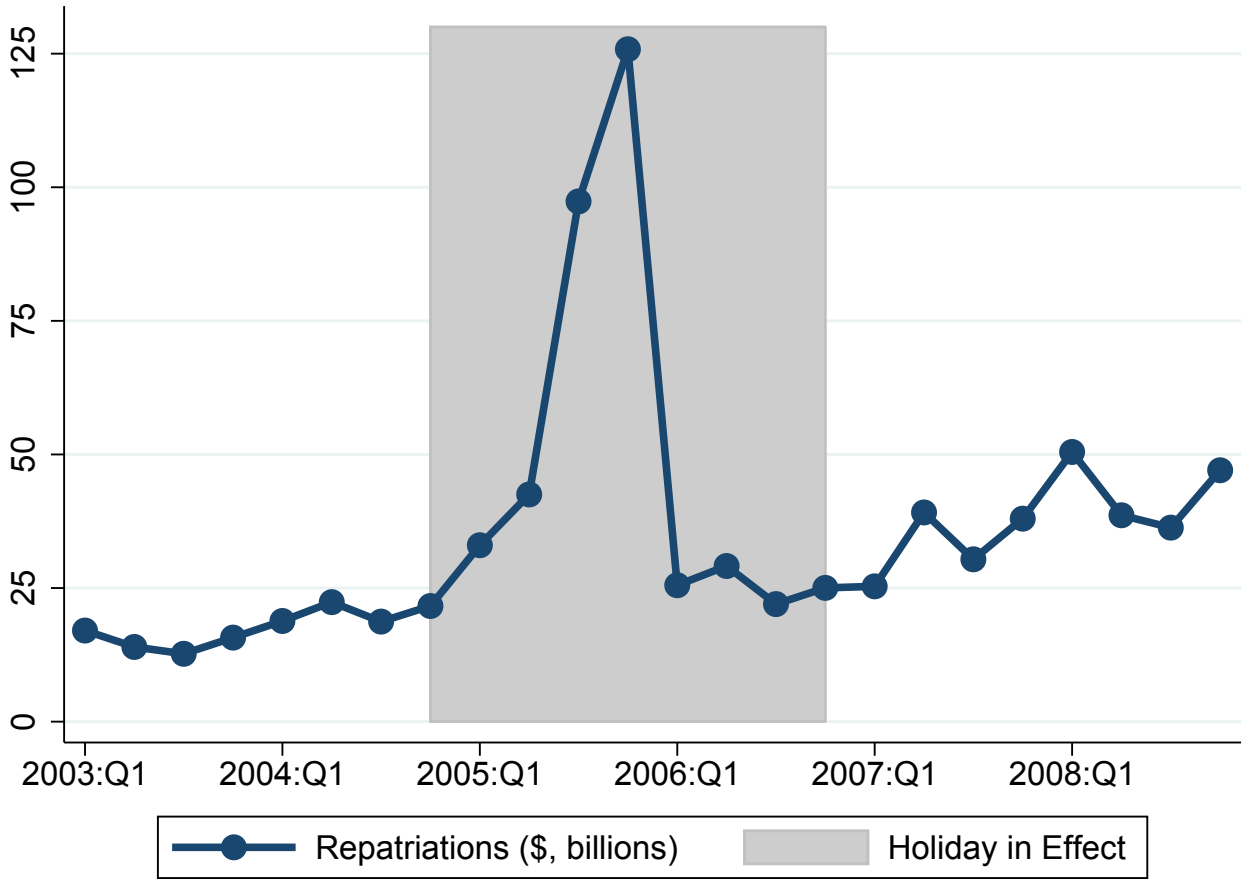


(B) Initial DE Elections and Changes to DE Status by Foreign Affiliates



Notes: Figure 2 displays trends in check-the-box elections from Field (2008). Panel (A) shows (1) total CTB elections made by foreign affiliates and (2) total disregarded entity status elections made by foreign affiliates during the years 1997–2006. Panel (B) breaks down the total disregarded entity status elections made by foreign affiliates into initial elections (by new affiliates) and changes in status to disregarded entity during the years 1997–2006.

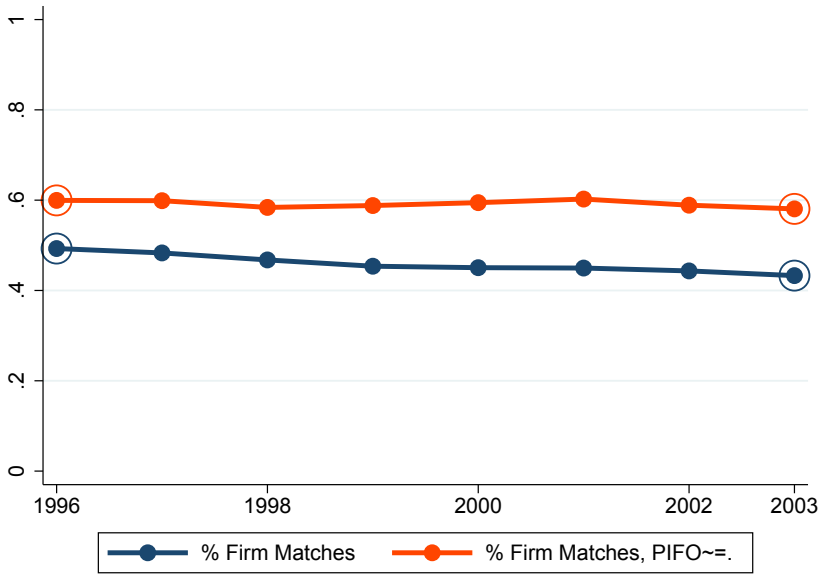
Figure 3: Repatriations of Foreign Income by US MNCs



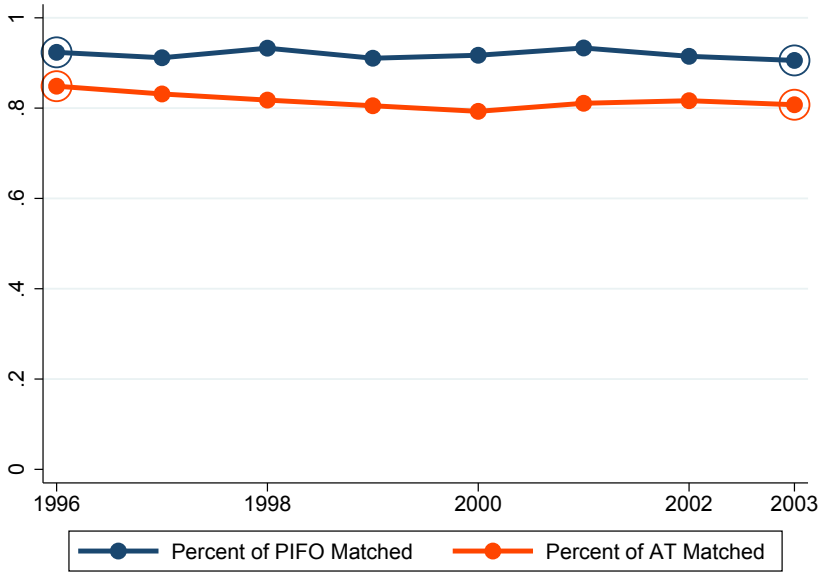
Notes: Figure 3 presents total repatriations of foreign income by US MNCs in billions of nominal dollars during the years 2003–2008 based on BEA data as assembled by Smolyansky et al. (2019). Repatriations began rising in Q4 of 2004 and peaked in Q4 of 2005 at $125 billion of repatriated earnings. The highest quarter of repatriations outside of the tax holiday in the 2000s happened in Q1 of 2008 with $50 billion of repatriated earnings.

Figure 4: Assessing Compustat–NETS Match

(A) Percent of Compustat Firms Matched to NETS by year, MNE Status

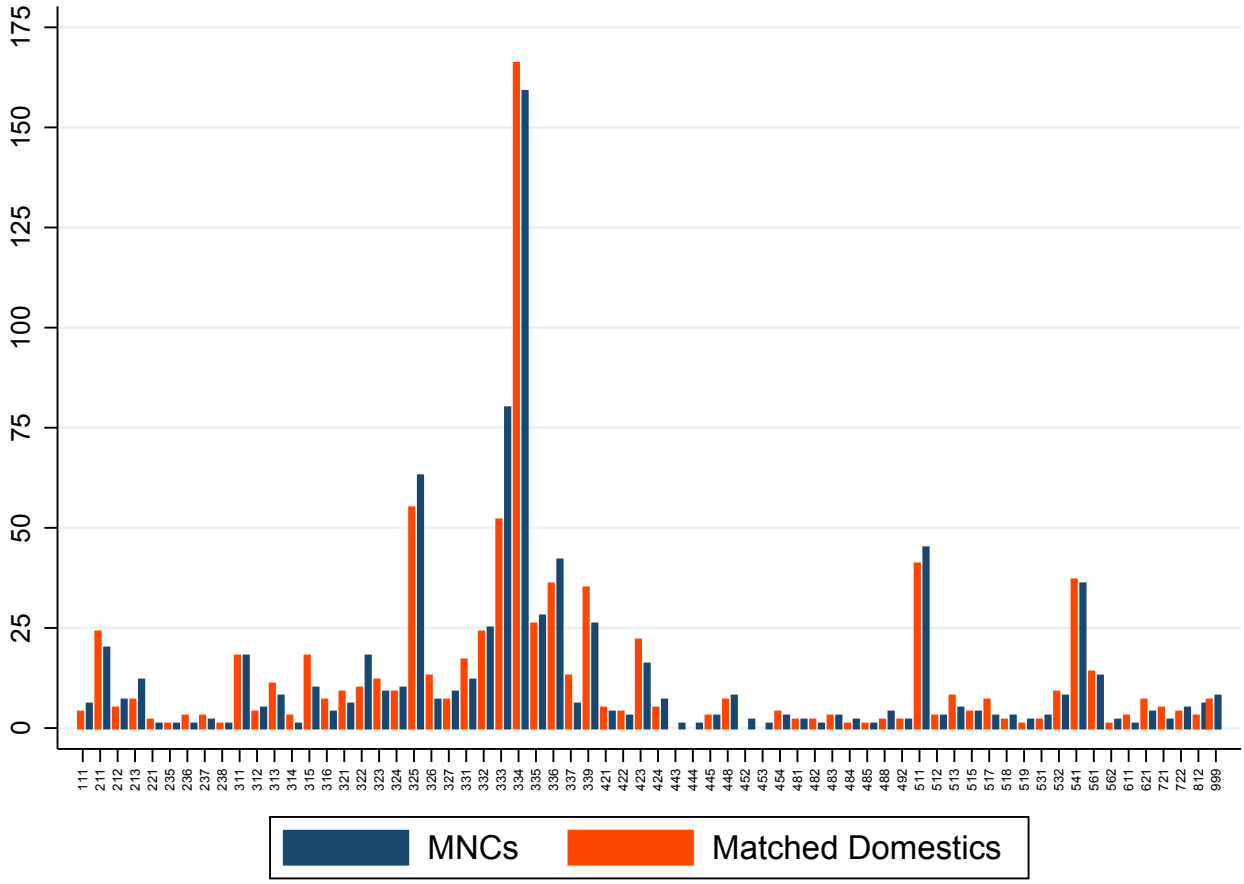


(B) Percent of Compstat PIFO and AT Represented by Compustat NETS Matches



Notes: Figure 4 describes the the share of the Compustat data set that was successfully merged with NETS using the method described in Section 3. Panel (A) shows the percentage of all Compustat firms matched to NETS and the percentage of US MNCs in Compustat matched to NETS during the years 1996–2003. Panel (B) shows the percent of total pretax foreign income (PIFO) and the percent of total assets (AT) represented by the matched firms. 1996 and 2003 statistics are highlighted as those are the years we use to construct of CTB and REPAT Exposure variables.

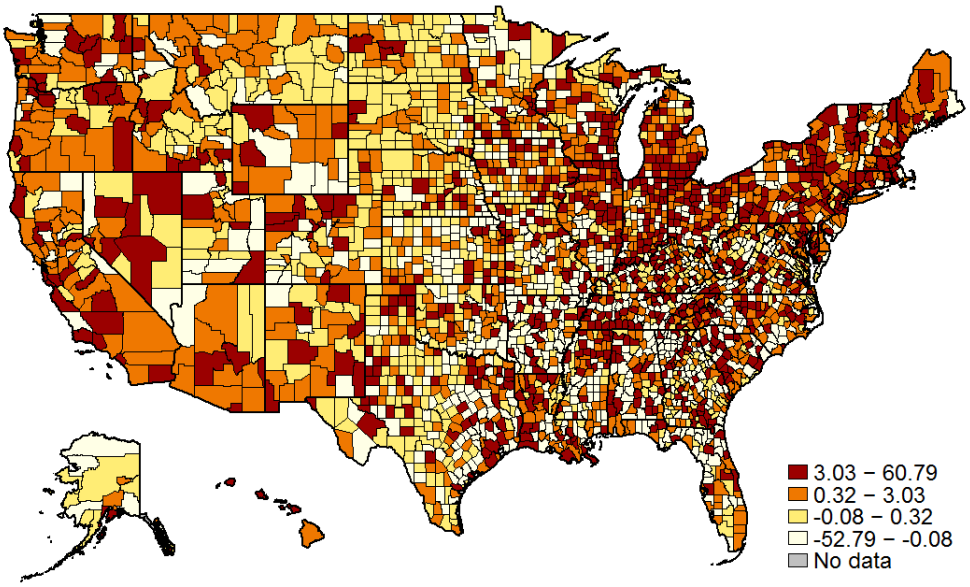
Figure 5: Distribution of MNCs and Matched Domestics Across Industries



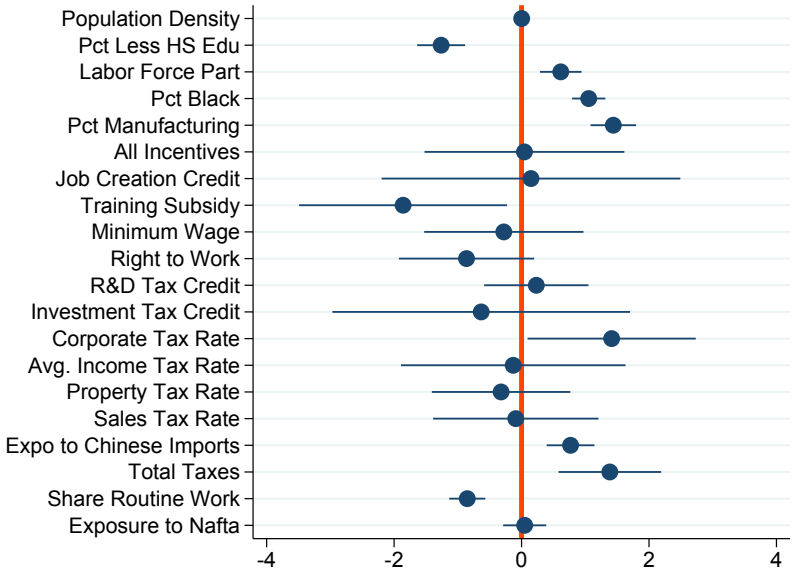
Notes: Figure 5 displays the count of US MNCs and Matched Domestic firms in the Compustat-NETS matched sample across 3-digit NAICS industries.

Figure 6: Local Exposure to Check-the-Box

(A) CTB Exposure



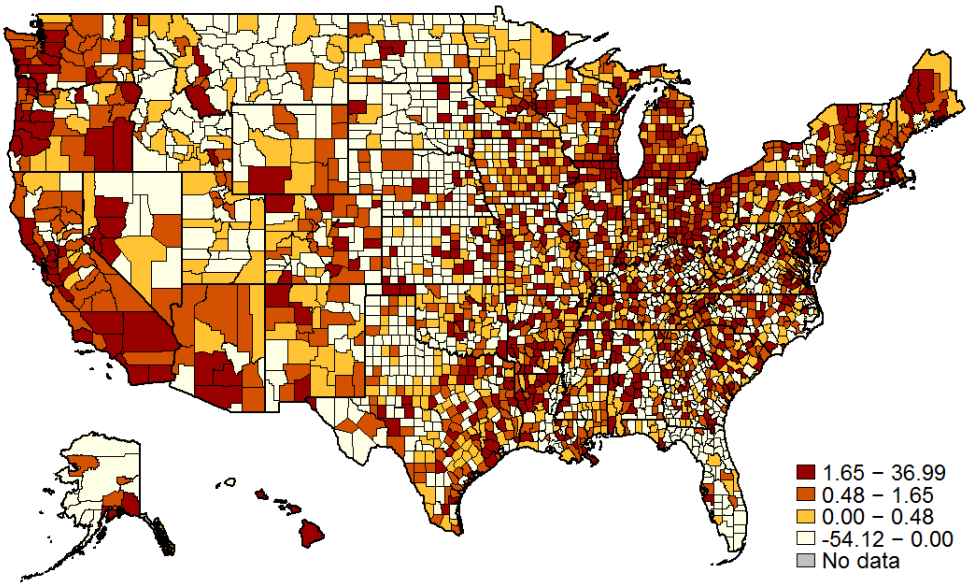
(B) Local Correlates of CTB Exposure



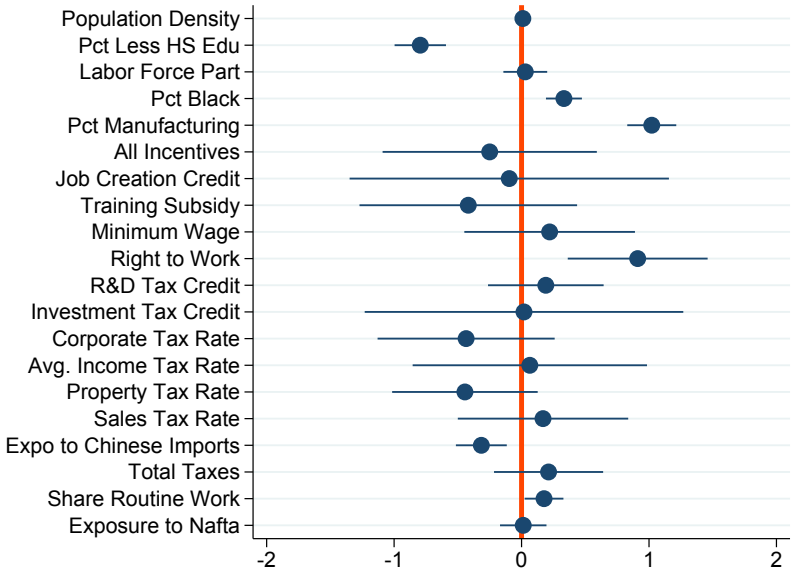
Notes: Panel (A) of Figure 6 displays county-level CTB Exposure. CTB Exposure is defined as the percentage of employees in a county working for MNCs in 1996 minus the percentage of employees in a county working for the sample of matched domestic firms in 1996. Panel (B) displays coefficients from a regression of CTB Exposure on a number of local characteristics and cross-sectional policy control variables. All local characteristics are measured in standard deviation units at the county level. Population Density is the number of residents per square mile. Pct Less HS Edu is the percentage of the population with less than a high school education. Labor Force Part is the labor force participation rate. Pct Black is the percentage of the population that are African American. Pct Manufacturing is the percentage of the population working in the manufacturing sector. All other variables are cross-sectional policy control variables. These are defined in Section 10.

Figure 7: Local Exposure to the Repatriation Holiday

(A) REPAT Exposure

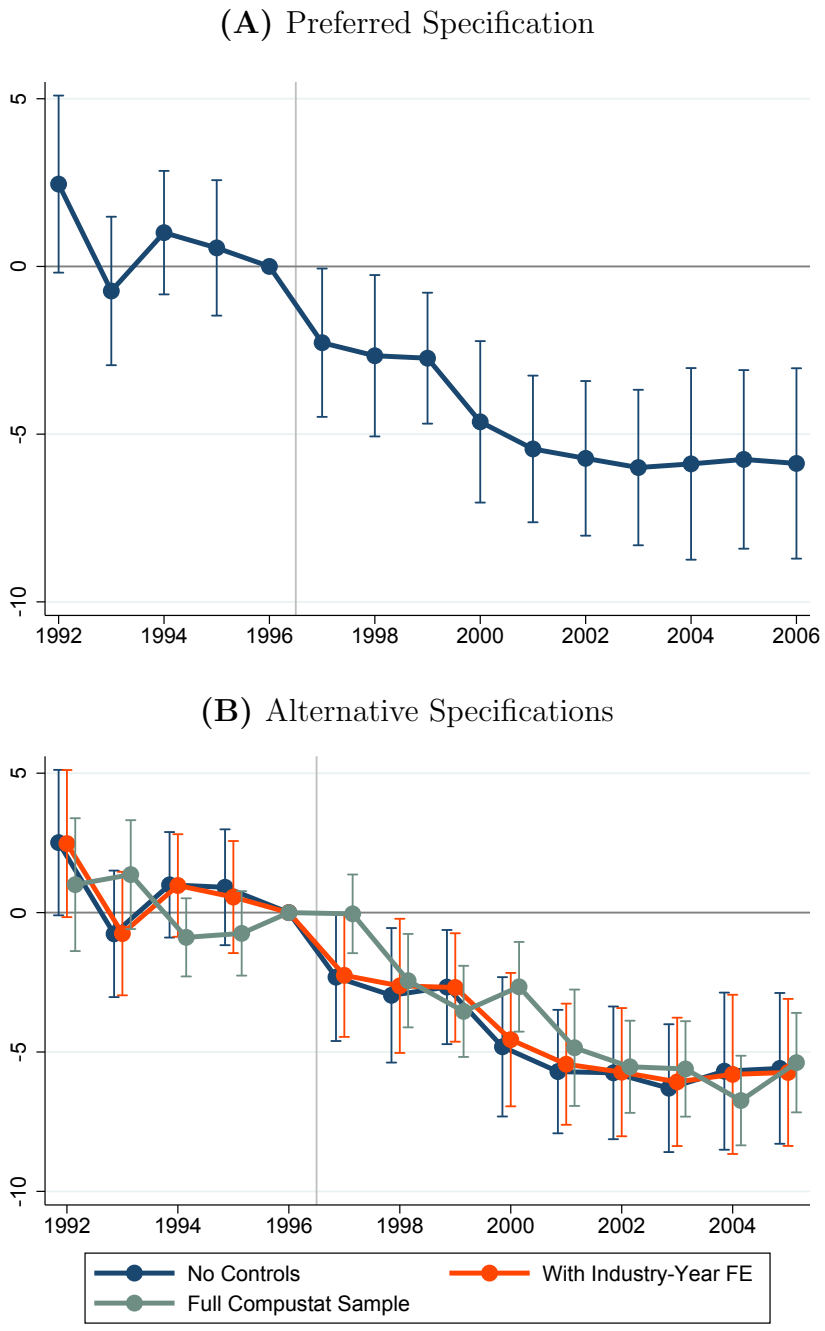


(B) Local Correlates of REPAT Exposure



Notes: Panel (A) of Figure 7 displays county-level REPAT Exposure. REPAT Exposure is defined as the percentage of employees in a county working for repatriating MNCs in 2003 minus the percentage of employees in a county working for the sample of matched non-repatriating MNCs in 2003. Panel (B) displays coefficients from a regression of REPAT Exposure on a number of local characteristics and cross-sectional policy control variables. All local characteristics are measured in standard deviation units at the county level. Population Density is the number of residents per square mile. Pct Less HS Edu is the percentage of the population with less than a high school education. Labor Force Part is the labor force participation rate. Pct Black is the percentage of the population that are African American. Pct Manufacturing is the percentage of the population working in the manufacturing sector. All other variables are cross-sectional policy control variables. These are defined in Section 10.

Figure 8: Effect of CTB on Effective Tax Rates

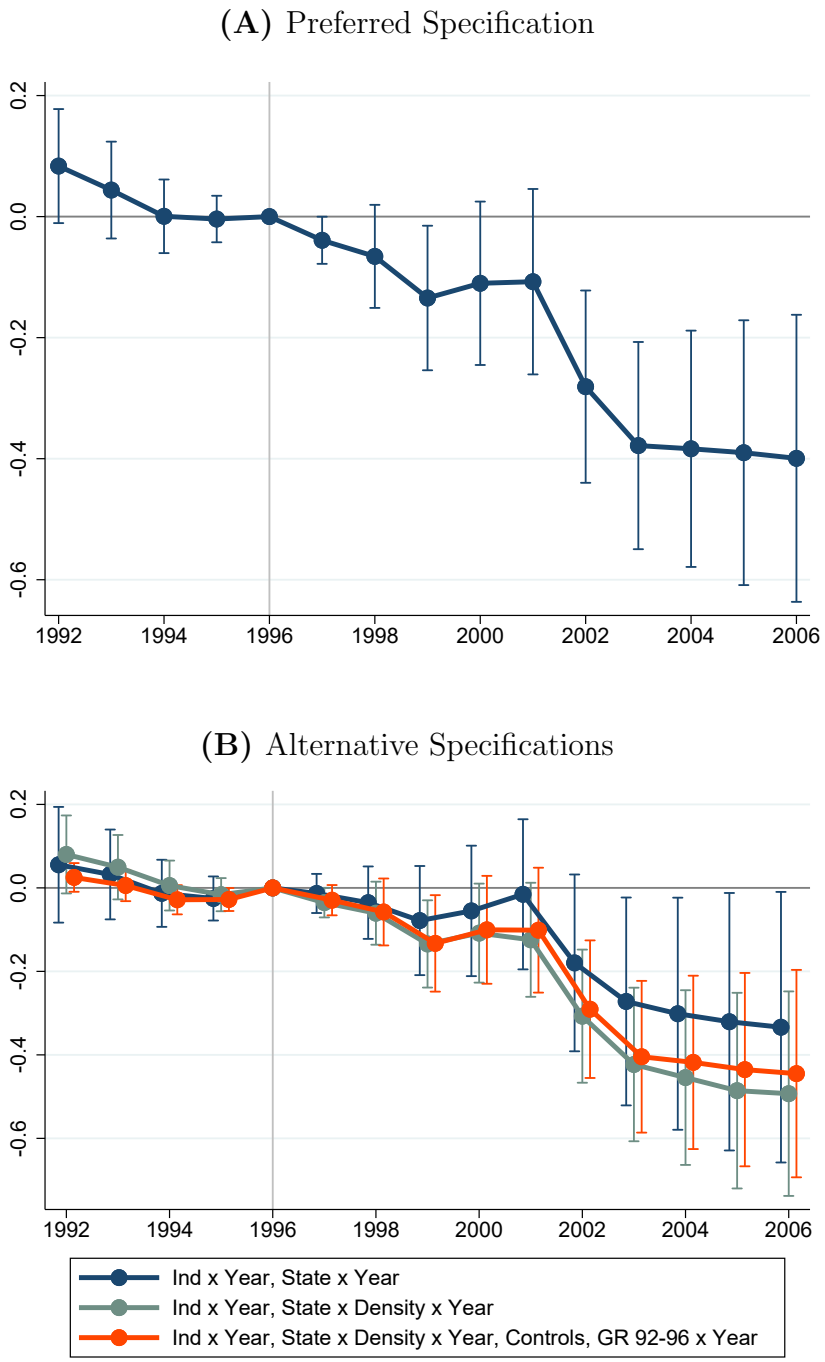


Notes: Figure 8 shows how effective tax rates of MNCs compare to effective tax rates of the matched domestic sample relative to the difference in 1996. To do so, both panels displays ω coefficients from regressions of the from

$$WWTE_{it} = \alpha + \beta PI_{it} + \gamma[PI_{it} \times MNE_i] + \sum_{h=1992}^{2006} \omega_h \left(PI_{it} \times MNE_i \times 1[t = h]\right) + \mu_i + \nu_t.$$

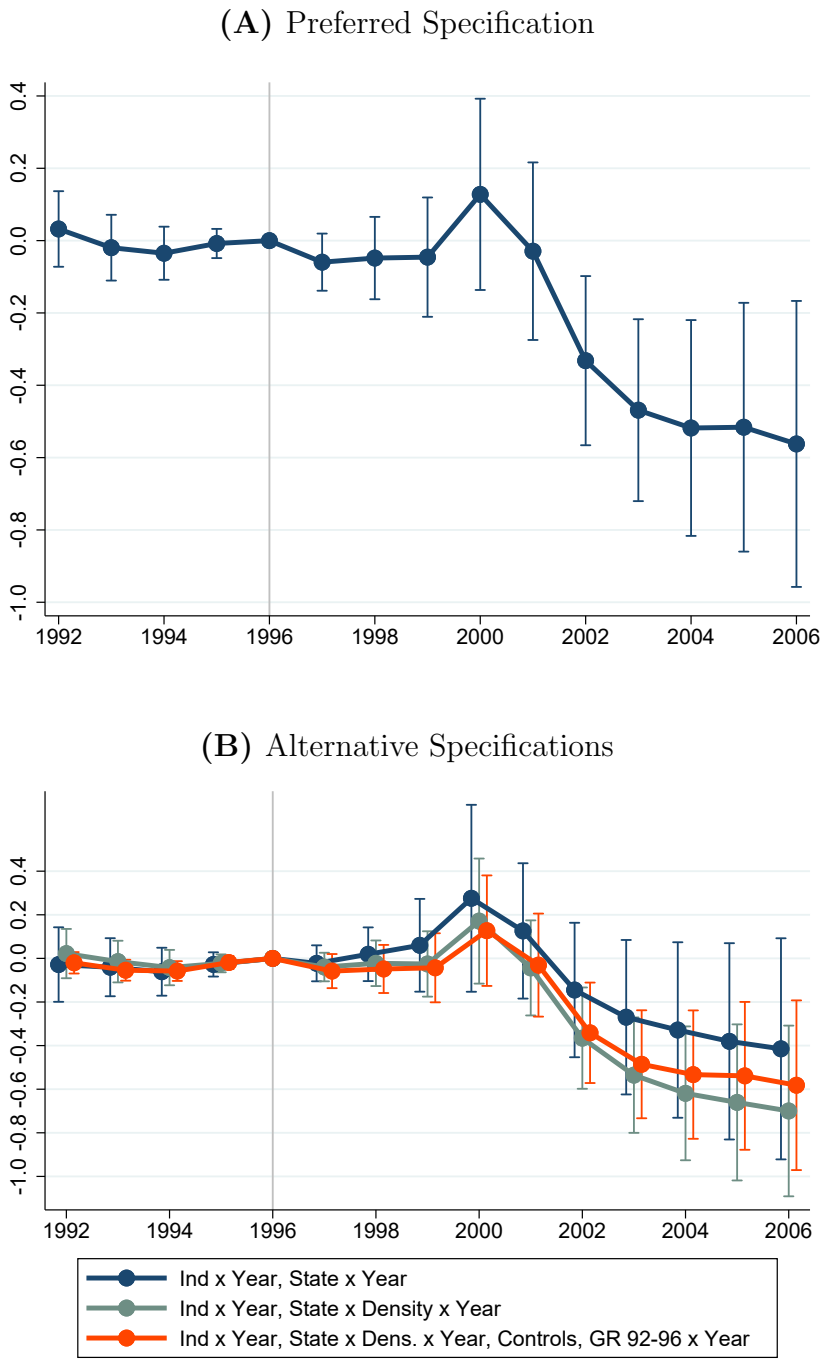
Panel (A) represents estimates from our preferred specification which includes firm, industry $\times$ year fixed effects and controls (as defined in Section 4.1). Panel (B) displays alternative specifications without controls, with year as opposed to industry-year fixed effects, and using the full sample of Compustat firms prior to limited to those matched to NETS. Standard errors are clustered at the firm level with 95% confidence intervals indicated for each annual estimate.

Figure 9: Effects of CTB Exposure on Domestic Employment



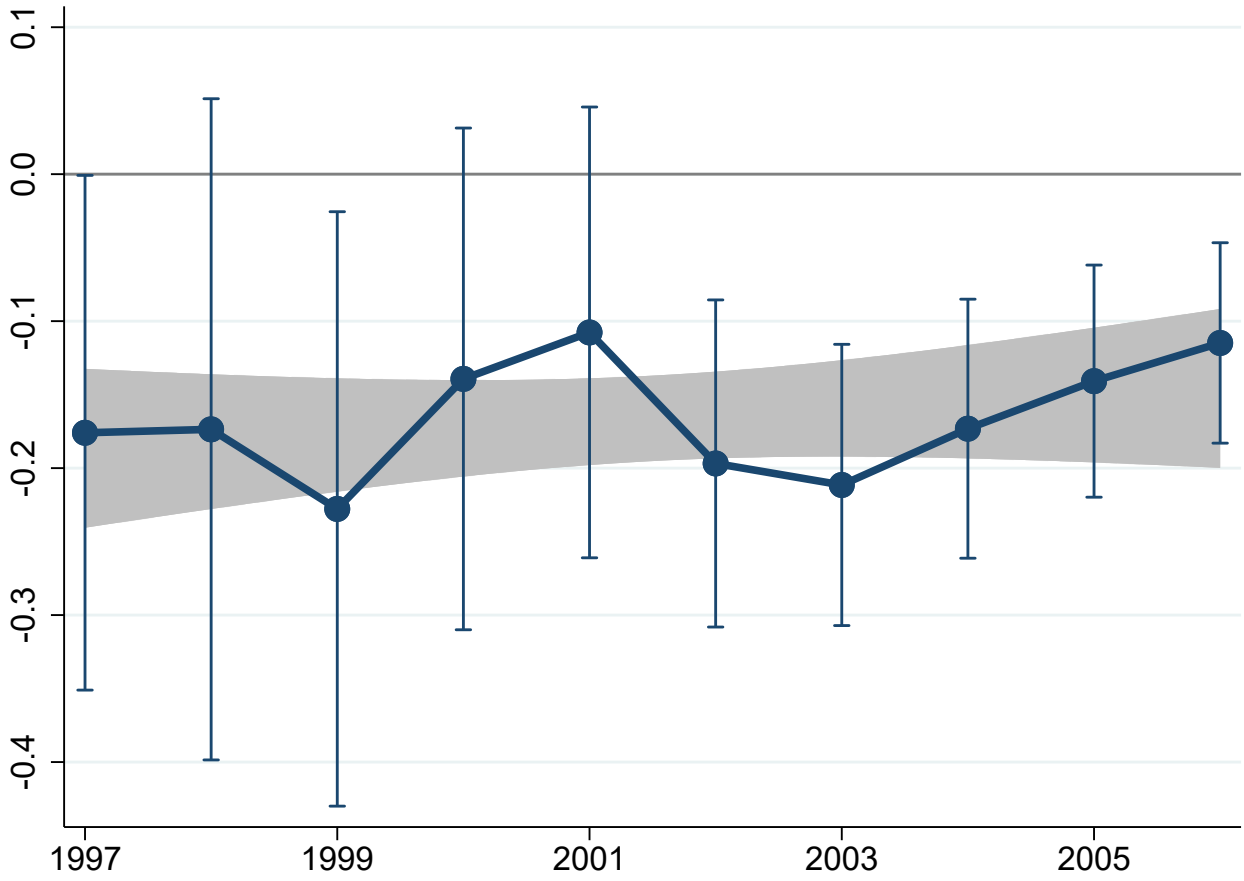
Notes: Figure 9 displays β coefficients and 95% confidence intervals from regressions in the form of Equation 3 which describe the effect of county-level CTB Exposure on the county-industry percent change in employment relative to 1996. Panel (A) displays estimates from our preferred specification which includes industry-by-year fixed effects, state-by-density-by-year fixed effects, and cross sectional policy controls as described in Section . Panel (B) displays estimates from alternative specifications. The first includes industry-by-year and state-by-year fixed effects. The second includes industry-by-year and state-by-density-by-year fixed effects. The third includes industry-by-year and state-by-density-by-year fixed effects cross-sectional policy controls and pre-period growth-by-year fixed effects. Standard errors in all specifications are clustered at the county level.

Figure 10: Effects of CTB Exposure on Domestic Earnings



Notes: Figure 10 displays β coefficients and 95% confidence intervals from regressions in the form of Equation 3 which describe the effect of county-level CTB Exposure on the county-industry percent change in total earnings relative to 1996. Panel (A) displays estimates from our preferred specification which includes industry-by-year fixed effects, state-by-density-by-year fixed effects, and cross sectional policy controls as described in Section . Panel (B) displays estimates from alternative specifications. The first includes industry-by-year and state-by-year fixed effects. The second includes industry-by-year and state-by-density-by-year fixed effects. The third includes industry-by-year and state-by-density-by-year fixed effects cross-sectional policy controls and pre-period growth-by-year fixed effects. Standard errors in all specifications are clustered at the county level.

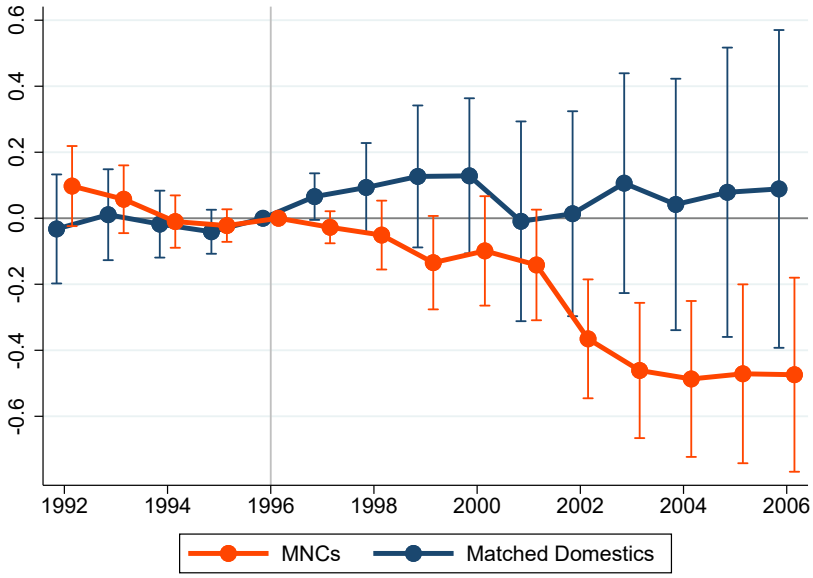
Figure 11: Domestic Employment Effect per 1,000 DE Elections



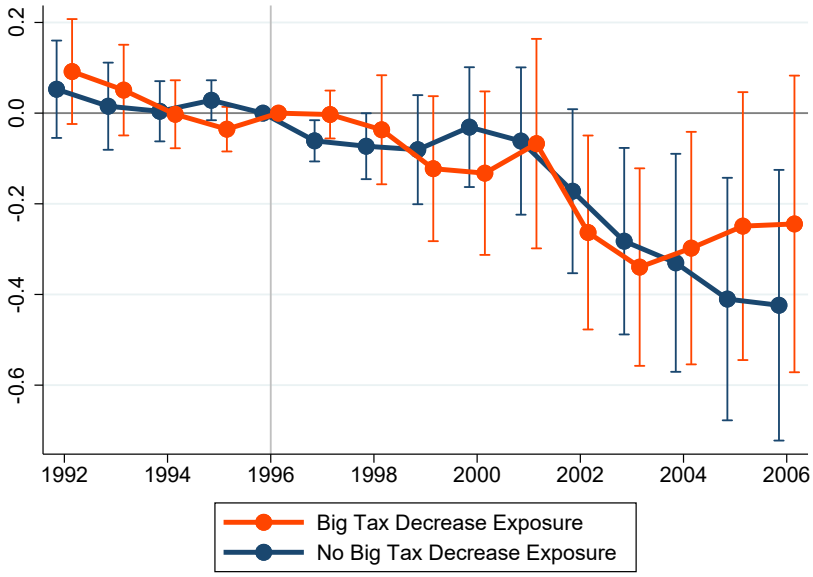
Notes: Figure 11 presents point estimates and 95% confidence internals of the effect of an 1,000 disregarded entity elections on employment growth rates during the years 1997–2006. To construct these estimates, we re-estimate Equation (3) during the years 1997–2006 after multiplying CTB Exposure by the number total disregarded entity elections made after 1996 presented in Figure 2 Panel (A). The gray area represent a linear estimate and 95% confidence interval through the estimated coefficients.

Figure 12: Heterogeneous Effects of CTB Exposure

(A) MNC Exposure vs. Matched Domestic Exposure

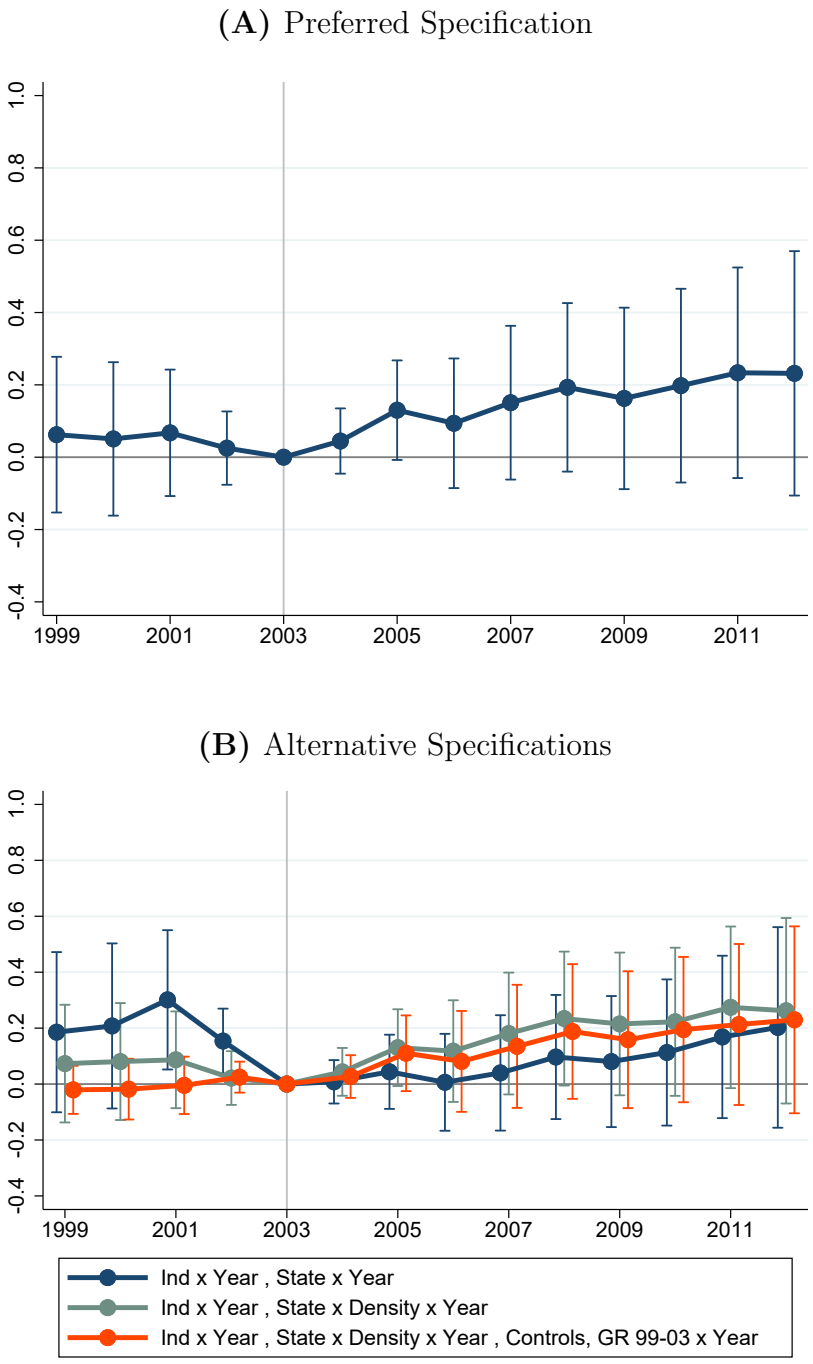


(B) By Affiliate Exposed to Large Tax Cut



Notes: Panel (A) of Figure 12 displays β coefficients and 95% confidence intervals from regressions in the form of Equation 3 where CTB Exposure is redefined separately based on county-level MNC Exposure and on county-level Matched Domestic Exposure, the two components that we use to construct our CTB Exposure variable. Panel (B) displays β coefficients and 95% confidence intervals from regressions in the form of Equation 3 where CTB Exposure is constructed based only on MNCs that did or did not report an affiliate in an OECD country that experienced a large corporate tax rate decrease during the 1996–2006 period. All specifications include industry-by-year and state-by-density-by-year fixed effects as well as county-level cross sectional control variables interacted with year fixed effects. Standard errors are clustered at the county-level. Corresponding pooled difference-in-differences estimates are presented in Table 5.

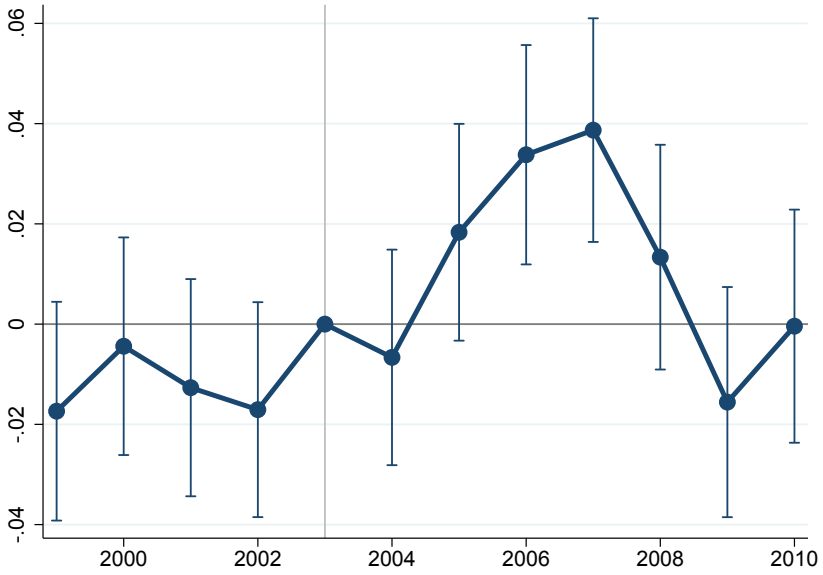
Figure 13: Effects of REPAT Exposure on Domestic Employment



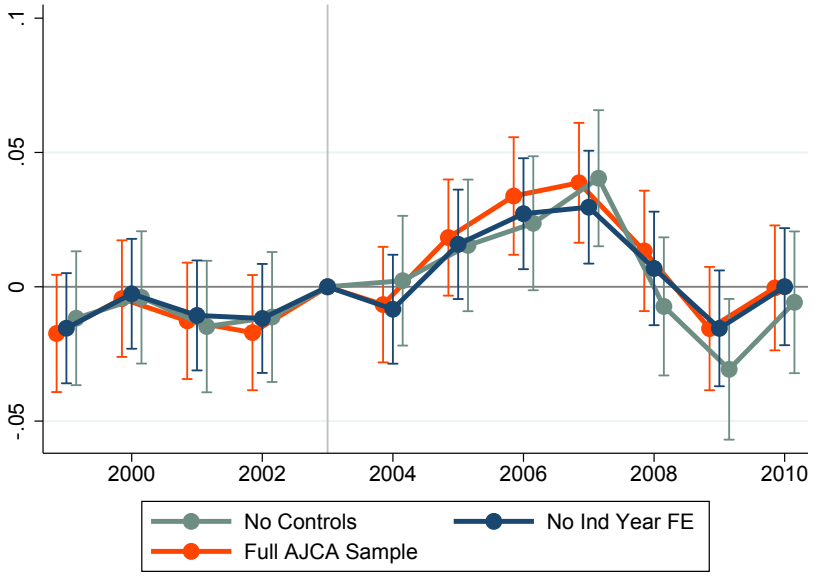
Notes: Figure 13 displays β coefficients and 95% confidence intervals from regressions in the form of Equation 5 which describe the effect of county-level REPAT Exposure on the county-industry percent change in employment relative to 2003. Panel (A) displays estimates from our preferred specification which includes industry-by-year fixed effects, state-by-density-by-year fixed effects, and cross sectional policy controls as described in Section 10. Panel (B) displays estimates from alternative specifications. The first includes industry-by-year and state-by-year fixed effects. The second includes industry-by-year and state-by-density-by-year fixed effects. The third includes industry-by-year and state-by-density-by-year fixed effects cross-sectional policy controls and pre-period growth-by-year fixed effects. Standard errors in all specifications are clustered at the county level.

Figure 14: Effect of Repatriating on Payouts

(A) Preferred Specification

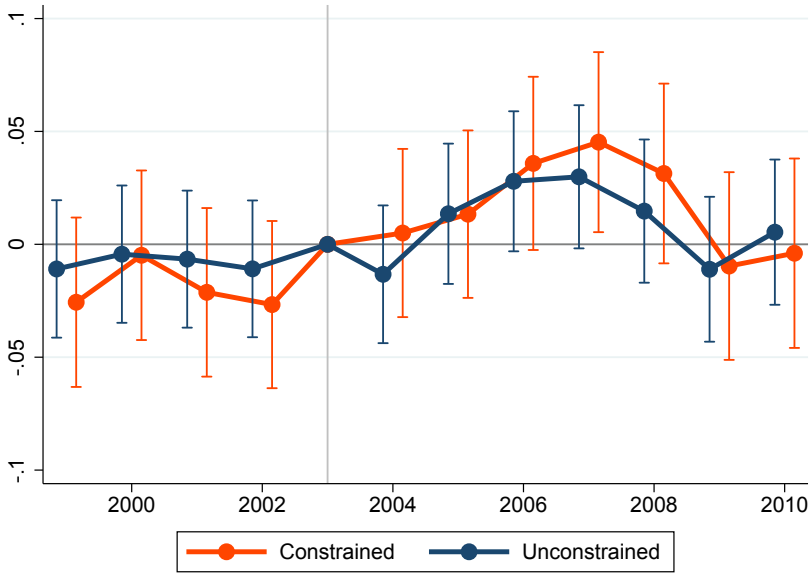


(B) Alternative Specifications



Notes: Figure 14 shows the difference in payout behavior between repatriating firms and the matched sample of non-repatriating MNCs. Panel (A) displays coefficient estimates and 95% confidence internals from a regression of total payouts per dollar of assets on an indicator for repatriating firm interacted with year dummies as well as firm fixed effects, industry-by-year fixed effects and controls for Tobin's q, cash scaled by assets, and ROA. Panel (B) presents alternative specifications that do not include the control variables, include year fixed effects instead of industry-by-year fixed effects, and use the full sample of repatriating firms matched to non-repatriating MNCs not limited to the NETS match. Standard errors are clustered at the firm level.

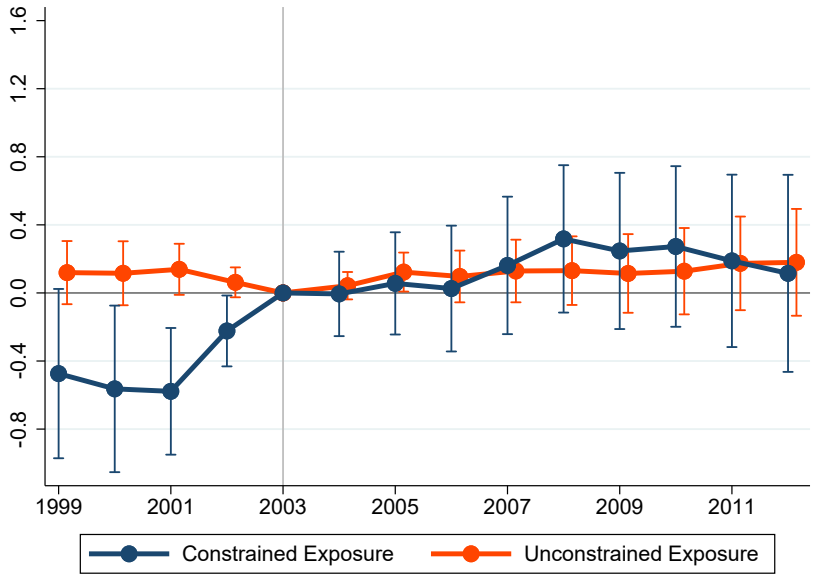
Figure 15: Effect of Repatriating on Payouts by Financial Constraint



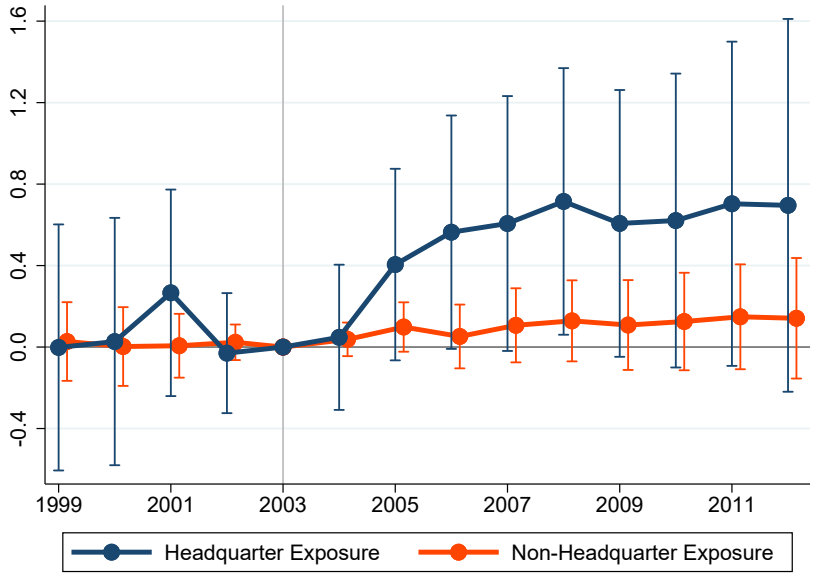
Notes: Figure 15 displays coefficient estimates and 95% confidence internals from regressions of total payouts per dollar of assets on indicators for repatriating due to AJCA, splitting the sample by firms in the top and bottom half of the Hadlock and Pierce (2010) measure of financial constraint (In practice, unconstrained firms and larger and older). The regressions include firm and industry-by-year fixed effects as well as controls for Tobin's q, cash scaled by assets, and ROA are included. Standard errors are clustered at the firm level.

Figure 16: REPAT Exposure Employment Effect Heterogeneity

(A) Heterogeneity by Financial Constraint



(B) Heterogeneity by Headquarters Location



Notes: Figure 16 explores heterogeneity in the effect of REPAT exposure on local employment. Panel (A) displays β coefficients and 95% confidence intervals from two regressions in the form of Equation 5. In the first, county-industry employment growth relative to 2003 is regressed on REPAT Exposure to financial constrained repatriating MNCs and financially constrained matched non-repatriating MNCs. In the second, REPAT Exposure is based only on financially unconstrained MNCs. Financial constraint is measured by Hadlock and Pierce (2010). Firms in the top half of the distribution are defined as constrained. Those in the bottom half are considered unconstrained. Panel (B) shows the effect of headquarters and non-headquarters REPAT Exposure on employment growth relative to 2003. All regressions include industry-by-year and state-by-density-by-year fixed effects as well as the cross-sectional controls described Section 10. Standard errors are clustered at the county-level.

8 Tables

Table 1: Descriptive Statistics and Balance for Check-the-Box Firms

	MNCs			Matched Domestics			
	n	mean	sd	n	mean	sd	Diff
Total Assets	815	3767.46	16687.90	816	1442.82	5531.98	2,324.642***
Long-Term Debt Ratio	815	17.12	16.33	816	18.53	18.77	-1.405
CapX Ratio	811	6.78	5.78	804	7.26	7.49	-0.483
R&D Expedtiture Ratio	815	4.83	7.43	816	3.79	7.59	1.041***
NOL Ratio	815	3.27	13.66	816	5.79	21.54	-2.526***
Market to Book	806	2.63	9.81	802	2.50	3.02	0.127

Notes: Table 1 shows descriptive statistics for MNCs and matched domestic firms that are used to construct the CTB Exposure variable. All variables are averaged over the period 1994–1996 and winsorized in each year at the 1% level. Long-term debt ratio is defined as long-term debt to assets. CapX ratio is the ratio of capital expenditures to assets. R&D Expenditure ratio is the ratio of research and development expense to assets. NOL Ratio is the ratio of tax loss carryforwards to assets. ROA is the return on assets, defined as net income divided by total assets. Stars indicate whether this difference in means for each variable between the MNC sample and matched domestic sample is statistically significant. $^{*}p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Descriptive Statistics and Balance for Repatriating Firms

	Repatriating MNCs			Matched MNCs			
	n	mean	sd	n	mean	sd	Diff
Change in NI / AT	333	4.54	10.57	333	2.89	28.06	1.649
Change in Market to Book	333	1.53	13.95	333	1.13	15.57	0.394
Operating Cash Flow / AT	333	11.26	6.32	333	11.63	9.40	-0.378
Lower Foreign Tax Dummy	333	67.57	46.88	333	64.86	47.81	2.703
Average US Tax Rate	333	16.42	59.54	333	15.62	40.69	0.803
Share Foreign Assets	333	35.63	20.30	333	35.89	29.16	-0.260
Change in Foreign Pretax Income	333	1.74	3.16	333	1.41	3.97	0.324

Notes: Table 2 shows descriptive statistics for repatriating MNCs and matched non-repatriating MNCs that are used to construct the RH Exposure variable. All variables are winsorized in each year at the 1% level and defined following Blouin and Krull (2009). Change in NI / AT is the change in net income scaled by assets from 2002-2004. Change in Market to Book is the change in the market value of common stock relative to the book value from 2002-2004. Operating Cash Flow / AT is the average of operating cash flows scaled by average assets from 2002-2004. Lower Foreign Tax Dummy is a variable equal to 1 if the average foreign tax rate is lower than the statutory US tax rate and zero otherwise, multiplied by 100. Average US Tax Rate is the average effective tax rate for 2002-2004. Share Foreign Assets is the average share of assets that are held in foreign jurisdictions. The Change in Foreign Pretax Income is the change in the share of worldwide pretax income that is derived from non-US sources. Stars indicate whether this difference in means for each variable between the repatriating MNC sample and matched non-repatriating MNC sample is statistically significant. None of the differences are statistically significant at conventional levels. $^{*}p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of Check-the-Box on Worldwide Effective Tax Rates

	(1) WWTE	(2) WWTE	(3) WWTE	(4) WWTE
Pretax Income	5.641***	5.408***	5.506***	5.008***
	(0.454)	(0.454)	(0.457)	(0.394)
Pretax Income × MNC	6.296***	6.175***	6.131***	4.531***
	(0.893)	(0.887)	(0.885)	(0.669)
Pretax Income × Post × MNC	-5.026***	-4.826***	-4.838***	-4.438***
	(0.679)	(0.690)	(0.694)	(0.489)
Firm FE	✓	✓	✓	✓
Year FE	✓			
Industry-Year FE		✓	✓	✓
Controls			✓	✓
Sample	NETS	NETS	NETS	Compu.
Observations	19,895	19,734	19,734	27,684

Notes: Table 3 displays β, γ, and ω coefficients from regression of the form

$$WWTE_{it} = \alpha + \beta PI_{it} + \gamma[PI_{it} \times MNC_i] + \omega\left(PI_{it} \times MNC_i \times 1[t \geq 1997]\right) + \mu_i + \nu_{jt} + \varepsilon_{it}.$$

Coefficient ω is the coefficient of interest and measure the differential change in worldwide effective tax rates between MNCs and matched domestic firms after 1997. Specifications (1) – (3) Compustat-NETS sample of MNCs and matched domestic firms upon which the CTB Exposure Measure is based. Specification (1) includes firm and year fixed effects. Specification (2) includes firm and industry-by-year fixed effects. Specifications (3) and (4) includes firm and industry-by-year fixed effects as well as the time-varying firm level controls discussed in Section 4.1. Standard errors are clustered at the firm level. *, **, and *** denote statistical significance at the 10, 5, and 1% level.

Table 4: Effect of CTB Exposure on Domestic Employment

	(1) Δ Emp	(2) Δ Emp	(3) Δ Emp	(4) Δ Emp
CTB Exposure × 1997–2002	-0.0628	-0.143***	-0.115**	-0.115**
	(0.0629)	(0.0498)	(0.0501)	(0.0493)
CTB Exposure × Post 2002	-0.307**	-0.529***	-0.409***	-0.416***
	(0.147)	(0.119)	(0.0999)	(0.100)
Industry × Year FE	✓	✓	✓	✓
State × Year FE	✓			
State × Density × Year FE		✓	✓	✓
Controls			✓	✓
GR 92–96 × Year FE				✓
Observations	1,035,333	1,035,333	1,035,333	1,014,686
Average MNC Employment	0.049	0.049	0.049	0.049
Aggregate Change in Employment (Millions)	-1.3	-2.2	-1.7	-1.8

Notes: Table 4 displays β_1 and β_2 coefficients from regressions of the form

$$\Delta\text{Emp}_{jct} = \alpha + \beta_1 \text{CTB Exposure}_c \times 1997\text{–}2002_t + \beta_2 \text{CTB Exposure}_c \times \text{Post2002}_t + \mathbf{X'}_c\boldsymbol{\gamma}_t + \mu_{jt} + \nu_{st} + \epsilon_{cjt}$$

The outcome variable in all regressions is the percentage point change in county-industry employment relative to 1996. Specification (1) includes includes industry-by-year and state-by-year fixed effects. Specification (2) includes industry-by-year and state-by-density-by-year fixed effects. Specification (3) is our preferred specification and includes industry-by-year and state-by-density-by-year fixed effects as well as county-level cross sectional control variables interacted with year fixed effects. Specification (4) adds pre-period growth quintiles interacted with year fixed effects to Specification (3). Standard errors are clustered at the county-level. *, **, and *** denote statistical significance at the 10, 5, and 1% level. The aggregate change in employment is calculated by multiplying the $beta_2$ estimate by the average percentage of MNC employment and the QCEW workforce in 1996. More details are provided in Section XXX.

Table 5: Effect of CTB Exposure on Domestic Earnings

	(1) Δ Earn	(2) Δ Earn	(3) Δ Earn	(4) Δ Earn
CTB Exposure × 1997–2002	0.0523	-0.0660	-0.0485	-0.0470
	(0.109)	(0.0766)	(0.0778)	(0.0747)
CTB Exposure × Post 2002	-0.348	-0.711***	-0.528***	-0.537***
	(0.217)	(0.181)	(0.159)	(0.158)
Industry × Year FE	✓	✓	✓	✓
State × Year FE	✓			
State × Density × Year FE		✓	✓	✓
Controls			✓	✓
GR 92–96 × Year FE				✓
Observations	1,035,333	1,035,333	1,035,333	1,014,686
Average MNC Employment	0.049	0.049	0.049	0.049
Aggregate Change in Earnings (Billions)	-42.2	-86.3	-64.0	-65.4

Notes: Table 5 displays β_1 and β_2 coefficients from regressions of the form

$$\Delta \text{Earn}_{jct} = \alpha + \beta_1 \text{CTB Exposure}_c \times 1997\text{–}2002_t + \beta_2 \text{CTB Exposure}_c \times \text{Post}2002_t + \mathbf{X'}_c \boldsymbol{\gamma}_t + \mu_{jt} + \nu_{st} + \epsilon_{cjt}$$

The outcome variable in all regressions is the percentage point change in county-industry total earnings relative to 1996. Specification (1) includes includes industry-by-year and state-by-year fixed effects. Specification (2) includes industry-by-year and state-by-density-by-year fixed effects. Specification (3) is our preferred specification and includes industry-by-year and state-by-density-by-year fixed effects as well as county-level cross sectional control variables interacted with year fixed effects. Specification (4) adds pre-period growth quintiles interacted with year fixed effects to Specification (3). Standard errors are clustered at the county-level. *, **, and *** denote statistical significance at the 10, 5, and 1% level. The aggregate change in total earnings is calculated by multiplying the $beta_2$ estimate by the average percentage of MNC employment and the QCEW total earnings in 1996.

Table 6: Heterogeneous Effects of CTB Exposure on Domestic Employment

	(1) Δ Emp	(2) Δ Emp	(3) Δ Emp	(4) Δ Emp
MNC Exposure × 1997-2002	-0.120**			
	(0.0593)			
MNC Exposure × Post 2002	-0.467***			
	(0.119)			
Matched Domestic Exposure × 1997-2002		0.0846		
		(0.0974)		
Matched Domestic Exposure × Post 2002		0.155		
		(0.215)		
CTB Exp, Big Tax Decrease × 1997-2002			-0.0966	
			(0.0710)	
CTB Exp, Big Tax Decrease × Post 2002			-0.310**	
			(0.126)	
CTB Exp, Small Tax Decrease × 1997-2002				-0.0748
				(0.0538)
CTB Exp, Small Tax Decrease × Post 2002				-0.368***
				(0.132)
Industry × Year FE	✓	✓	✓	✓
State × Density × Year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Observations	1,035,333	1,035,333	1,035,333	1,035,333
Average Share	0.048	0.024	0.025	0.024
Aggregate Change in Employment (Millions)	-2.0	0.3	-0.7	-0.8

Notes: Table 6 displays β_1 and β_2 coefficients from regressions of the form

$$\begin{aligned}\Delta \text{Earn}_{jct} = \alpha &+ \beta_1 \text{CTB Exposure}_c \times 1997\text{–}2002_t \\ &+ \beta_2 \text{CTB Exposure}_c \times \text{Post2002}_t + \mathbf{X'}_c\gamma_t + \mu_{jt} + \nu_{st} + \epsilon_{cjt}\end{aligned}$$

where the CTB Exposure variable is defined differently across all four specifications. In Specification (1), CTB Exposure is based only on MNCs that report an affiliate in an OECD country that experienced a large corporate tax rate decrease during the 1996–2006 period. In Specification (2), CTB Exposure is based only on MNCs that did not report an affiliate in an OECD country that experienced a large corporate tax rate decrease during the 1996–2006 period. In Specification (3), CTB Exposure is based only on MNCs that reported affiliate an affiliate in a tax haven. In Specification (4), CTB Exposure is based only on MNCs that did not report affiliate an affiliate in a tax haven. All specifications include industry-by-year and state-by-density-by-year fixed effects as well as county-level cross sectional control variables interacted with year fixed effects. Standard errors are clustered at the county-level. *, **, and *** denote statistical significance at the 10, 5, and 1% level. The aggregate change in employment is calculated by multiplying the β_2 estimate by the average percentage of MNC employment for each definition and the QCEW total employment in 1996.

Table 7: Effect of REPAT Exposure on Domestic Employment

	(1) Δ Emp	(2) Δ Emp	(3) Δ Emp	(4) Δ Emp
REPAT Exposure × 2004–2006	0.0175	0.0587	0.0620	0.0400
	(0.0619)	(0.0703)	(0.0640)	(0.0615)
REPAT Exposure × Post 2006	0.114	0.196	0.182	0.176
	(0.129)	(0.137)	(0.127)	(0.125)
Industry × Year FE	✓	✓	✓	✓
State × Year FE	✓			
State × Density × Year FE		✓	✓	✓
Controls			✓	✓
GR 99–03 × Year FE				✓
Observations	1,233,462	1,233,462	1,233,462	948,673
Average Employment in Repat Firms	0.020	0.020	0.020	0.021
Aggregate Change in Employment (Millions)	0.2	0.4	0.3	0.3

Notes: Table 7 displays β_1 and β_2 coefficients from regressions of the form

$$\Delta \text{Emp}_{jct} = \alpha + \beta_1 \text{REPAT Exposure}_c \times 2004\text{–}2006_t + \beta_2 \text{REPAT Exposure}_c \times \text{Post2006}_t + \mathbf{X'}_c \boldsymbol{\gamma}_t + \mu_{jt} + \nu_{st} + \epsilon_{cjt}$$

The outcome variable in all regressions is the percentage point change in county-industry employment relative to 2003. Specification (1) includes includes industry-by-year and state-by-year fixed effects. Specification (2) includes industry-by-year and state-by-density-by-year fixed effects. Specification (3) is our preferred specification and includes industry-by-year and state-by-density-by-year fixed effects as well as county-level cross sectional control variables interacted with year fixed effects. Specification (4) adds pre-period growth quintiles interacted with year fixed effects to Specification (3). Standard errors are clustered at the county-level. *, **, and *** denote statistical significance at the 10, 5, and 1% level. The aggregate change in employment is calculated by multiplying the $beta_2$ estimate by the average percentage of repatriating MNC employment and the QCEW workforce in 2003.

Table 8: Effect of Repatriation Indicator on Total Payouts and Capital Expenditures

	(1) Payouts/AT	(2) Payouts/AT	(3) Payouts/AT	(4) Payouts/AT	(5) Payouts/AT
Repatriator × 2004–2006	0.0135***	0.0136***	0.0176***	0.0182**	0.0143*
	(0.00475)	(0.00467)	(0.00513)	(0.00850)	(0.00796)
Repatriator × Post 2006	0.0136**	0.0116**	0.0160***	0.0203	0.0131
	(0.00573)	(0.00558)	(0.00611)	(0.0127)	(0.00858)
Firm FE	✓	✓	✓	✓	✓
Year FE	✓				
Industry-Year FE		✓	✓	✓	✓
Controls			✓	✓	✓
Constrained				YES	NO
Observations	5,404	5,319	5,082	2,433	2,481

Notes: Table 8 displays β_1 and β_2 coefficients from regressions of the form

$$\Delta y_{it} = \alpha + \beta_1 \text{Repatriator}_i \times 2004\text{–}2006_t + \beta_2 \text{Repatriator}_i \times \text{Post2006}_t + \mathbf{X'}_{it}\boldsymbol{\gamma}_t + \mu_{jt} + \epsilon_{it}$$

The outcome variable in all specifications is total payouts (dividends plus share repurchases) divided by average assets in years 1999–2003. Specification (1) includes firm and year fixed effects. Specification (2) includes firm and industry-by-year fixed effects. Specifications (3) – (5) include firm and industry-by-year fixed effects as well as time-varying firm controls for Tobin's q, cash scaled by assets, and ROA. Specifications (4) and (5) estimate the effect of repatriating on payout behavior separately for firms in the top and bottom of the financial constraint distribution as measured by the Hadlock and Pierce (2010) financial constraint measure. Standard errors are clustered at the firm level. *, **, and *** denote statistical significance at the 10, 5, and 1% level.

Table 9: Effect of Heterogeneous REPAT Exposure on Domestic Employment

	(1) Δ Emp	(2) Δ Emp	(3) Δ Emp	(4) Δ Emp
REPAT Exposure × 2004–2006	0.0994	0.0312	0.0530	0.0473
	(0.134)	(0.0560)	(0.202)	(0.0572)
REPAT Exposure × Post 2006	0.245	0.132	0.523	0.143
	(0.217)	(0.123)	(0.371)	(0.113)
Exposure	Constrained	Unconstrained	Headquarters	Non-Headquarters
Industry × Year FE	✓	✓	✓	✓
State × Density × Year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Observations	1,240,940	1,240,940	1,240,940	1,240,940
Average Employment in Repat Firms	0.001	0.019	0.001	0.019
Aggregate Change in Employment (Millions)	0.0	0.2	0.0	0.2

Notes: Table 9 displays β_1 and β_2 coefficients from regressions of the form

$$\Delta \text{Emp}_{jct} = \alpha + \beta_1 \text{REPAT Exposure}_c \times 2004\text{–}2006_t + \beta_2 \text{REPAT Exposure}_c \times \text{Post2006}_t + \mathbf{X'}_c \boldsymbol{\gamma}_t + \mu_{jt} + \nu_{st} + \epsilon_{cjt}$$

where the REPAT Exposure variable is based on subsamples of repatriating MNCs and matched non-repatriating MNCs firms. The outcome variable in all regressions is the percentage point change in county-industry employment relative to 2003. In Specification (1), REPAT Exposure is based on financially constrained firms, where constraint is defined as firms in the top half of the . In Specification (2), REPAT Exposure is based on financially unconstrained firms. Financial constraint is measured by Hadlock and Pierce (2010). Firms in the top half of the distribution are defined as constrained. Specifications (3) and (4) define REPAT Exposure based on employees in headquarters locations and non-headquarters locations. All estimates are based on our preferred specifications which includes industry-by-year and state-by-density-by-year fixed effects as well as county-level cross sectional control variables interacted with year fixed effects. Standard errors are clustered at the county-level. *, **, and *** denote statistical significance at the 10, 5, and 1% level. The aggregate change in employment is calculated by multiplying the $beta_2$ estimate by the average percentage of repatriating MNC employment and the QCEW workforce in 2003. More details are provided in Section XXX.

Appendix

This appendix includes additional information on the data and methods used in the paper as well as supplementary results.

A Matching NETS and Compustat

NETS

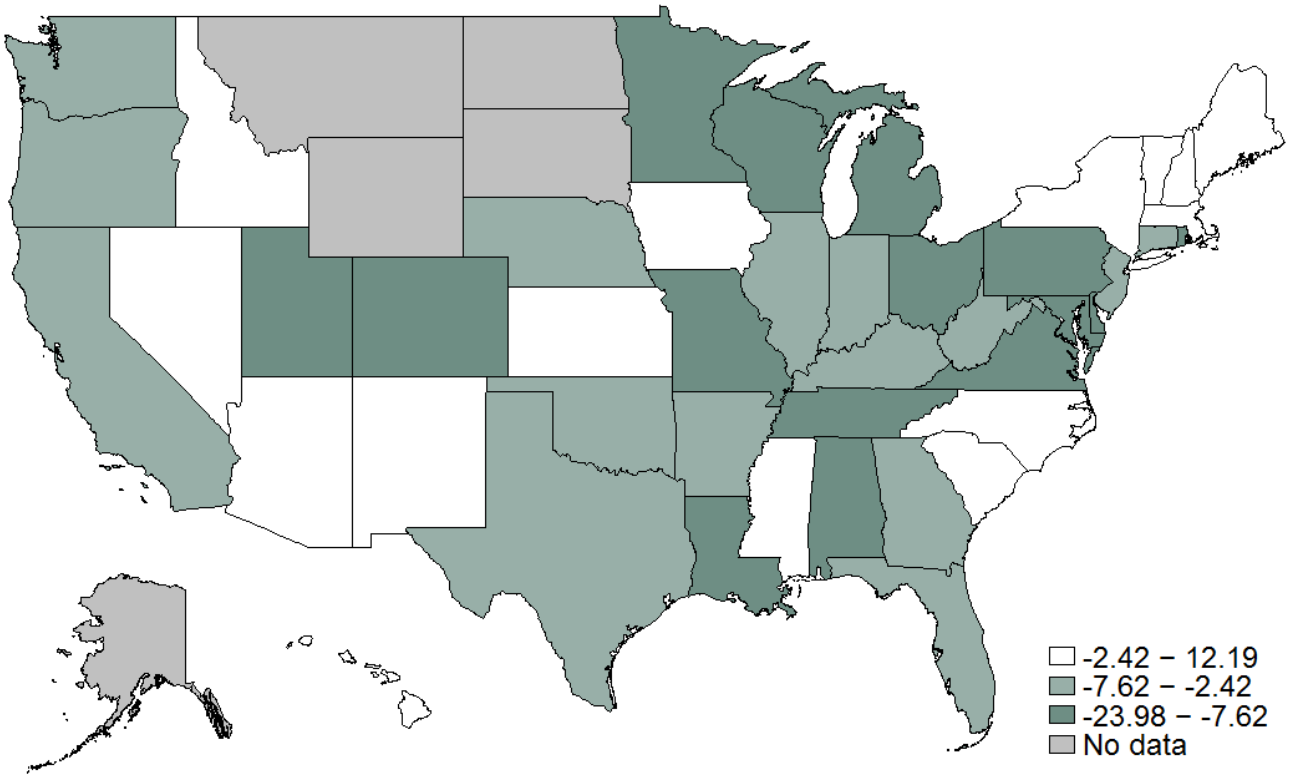
The version of NETS that we use is a list of all establishments in the US between 1990 and 2012. The database is assembled by Dun and Bradstreet from a number of sources. As such, it is not administrative data nor does it accurately capture nuanced panel dynamics of a given firm, but it does provide accurate coverage of the distribution of firms at a given point in time (Barnatchez et al., 2017). Within a year establishments in a firm are tied together by a unique identifier called HQDUNS.

Matching

In order to match to NETS, we attempt to find the HQDUNS of each firm in Compustat. We do this in two steps. First, we use ArcGIS to map Compustat addresses in 1996 to latitude and longitude coordinates. Second, we do a fuzzy match using the reclink2 STATA package (Wasi and Flaaen, 2015) in multiple stages. In the first stage, we find unique, exact matches based on names and coordinates with one decimal of latitude / longitude precision between Compustat data and any establishment in NETS. From the matched establishment, we recover the HQDUNS to match a single Compustat GVKEY to a single HQDUNS. After a unique match is determined the firm is removed from the Compustat pool. Once all possible matches are established using this criteion, we move to a second stage. Here, we require a precise match on coordinates (one decimal), but allow for a fuzzy match on names. Again, assign the HQDUNS of each matched establishment to a Compustat firm and iteratively remove the Compustat firms. Once this automated process is complete, we manually assign a match in the case of non-unique matches based on names and establishment characteristics. Most non-unique establishment here share a unique HQDUNS so no judgement calls were made. Finally, for unmatched Compustat firms with more $2 billion in total assets, we manually attempted matches. Typically, this required us to follow a 1996 Compustat firm through mergers, acquisitions, and restructuring through 2012 when NETS names were assigned.

B Population Density and Pre-period Employment Growth

Figure B1: Differences in Emp. Growth by Density and State, 1992–1996



Notes: Figure B shows how urban and rural area employment grows at different rates within states during the period prior to CTB implementation. We calculate the difference as employment growth rate in the most densely populated area in a state (top 20%) relative to the employment growth rate in the least densely populated area in a state (bottom 20%). There is no obvious pattern in growth rate differences by coastal / non-coastal or by geographic regions. *Data Sources:* QCEW (2017) and **?**.

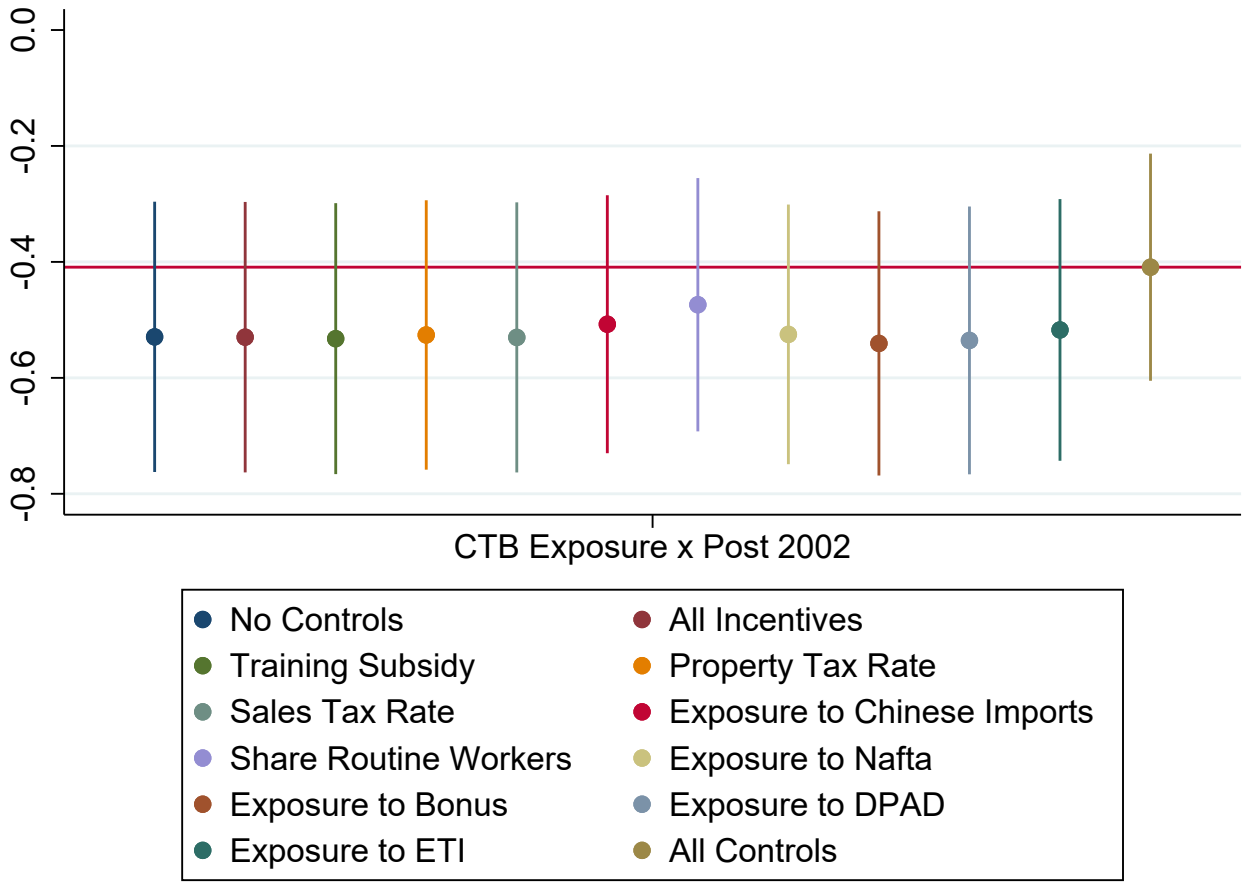
C Cross-sectional Policy Control Variables

In this section, we describe the "cross-sectional policy control variables" and their sources. We include interactions between these controls and year fixed effects in most specifications. Prior to interactions with year fixed effects, we standardize all variables by de-meaning and dividing by each variable's standard deviation. Cross-sectional variation is fixed in time where time varies according to the data source. Time-invariance avoids bad control problems in this context (Angrist and Pischke, 2008).

- **Property Tax Rate:** County-level average property tax rates from Suárez Serrato and Zidar (2018).
- **Exposure to Chinese Imports:** 1999-2007 county-level change in import exposure from Autor et al. (2016).
- **Share Routine Workers:** Share of workers in a commuting zone that work in occupations that involve routine tasks from Autor and Dorn (2013).
- **All Incentives:** County-level total state plus local average tax incentive from Bartik (2017).
- **Training Subsidy:** County-level average training subsidy from Bartik (2017).
- **Sales Tax Rate:** County-level average sales tax rate from Suárez Serrato and Zidar (2018).
- **Exposure to NAFTA:** Conspuma-level weighted average Mexican import tariff prior to NAFTA from Hakobyan and McLaren (2016).
- **DPAD:** County-level share of workers in top third of NAICS 4-digit industries benefiting from the Domestic Production Activities Deduction from Ohrn (2018).
- **Bonus Depreciation:** Share of workers in top third of NAICS 4-digit industries most benefiting from bonus depreciation from Garrett et al. (2020).
- **ETI:** Share of workers in NAICS 4-digit industries that benefited from the Extraterritoiral Income Exclusion from Ohrn (2018).

D Robustness of CTB Results to Individual Controls

Figure D1: Robustness of CTB Employment Effect to County-Level Controls



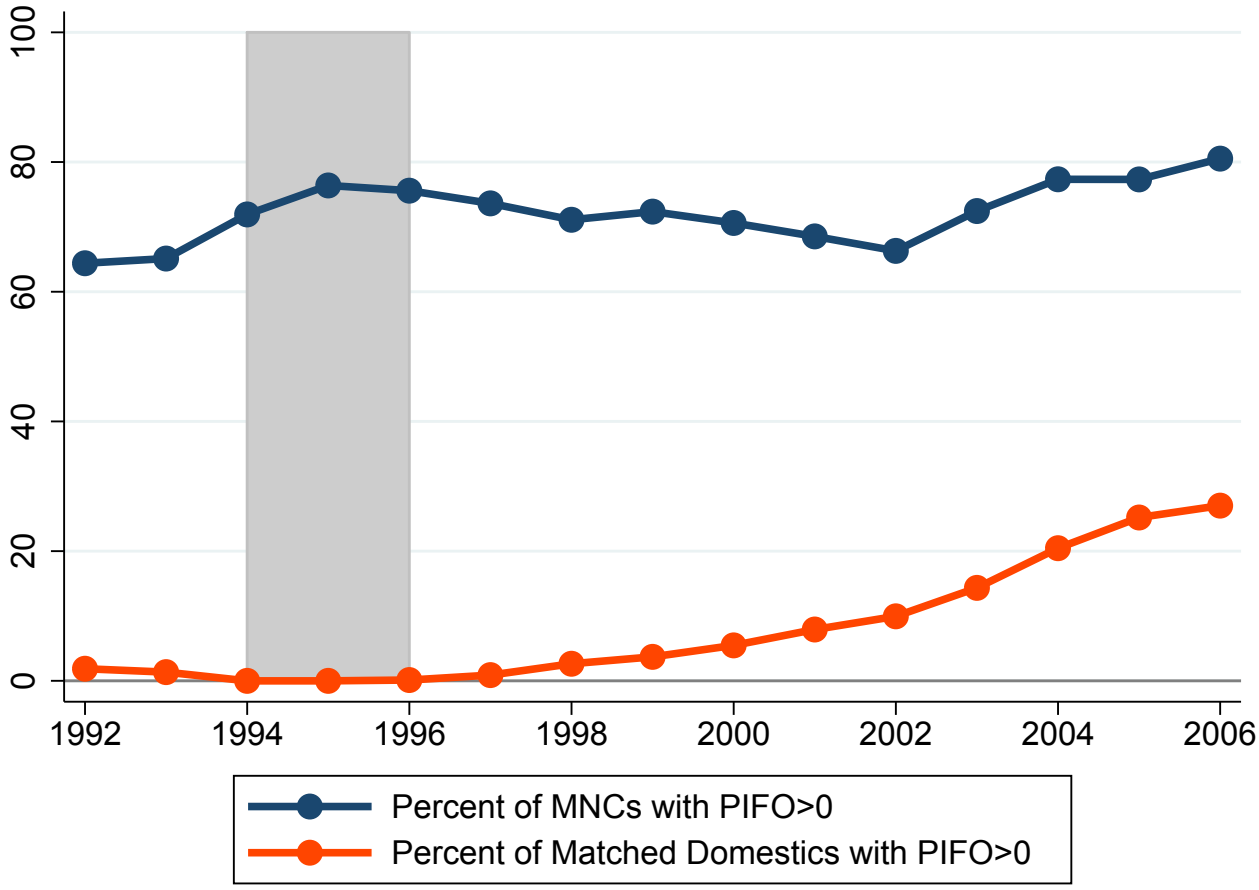
Notes: Table 4 displays β_2 coefficients and 95% confidence intervals from regressions of the form

$$\Delta \text{Emp}_{jct} = \alpha + \beta_1 \text{CTB Exposure}_c \times 1997\text{–}2002_t + \beta_2 \text{CTB Exposure}_c \times \text{Post2002}_t + \mathbf{X'}_c \gamma_t + \mu_{jt} + \nu_{st} + \epsilon_{cjt}$$

with alternative control variables included in the model. Each coefficient except for the rightmost presents estimates with a single control variable included. Control variables are defined in Appedix C. The rightmost coefficient is estimated in the presence of all controls and corresponds to our preferred long-run employment effect estimate presented in Column (3) of Table 4. All specification include industry-by-year fixed effects and state-by-density-by-year fixed effects. Standard errors in all specifications are clustered at the county level.

E MNCs and Matched Domestics Reporting PIFO

Figure E1: Percent of MNCs and Matched Domestics Reporting PIFO

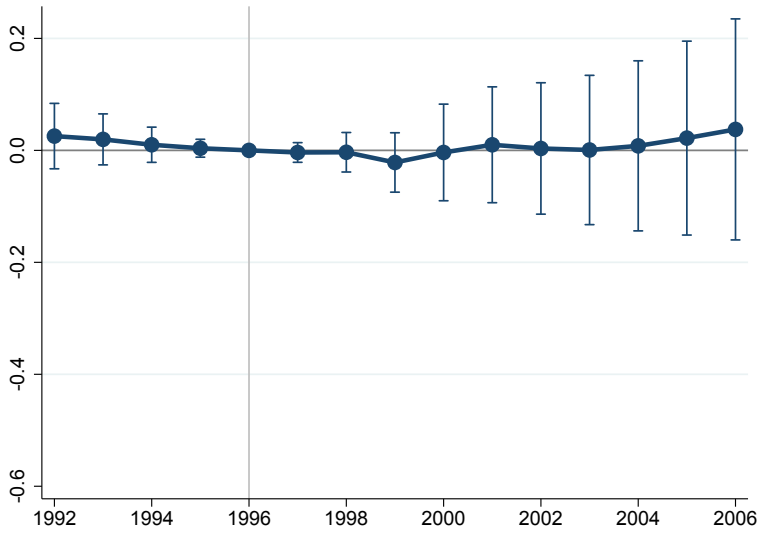


Notes: Figure E1 presents the percentage of MNCs and matched domestic firms used in the CTB analysis that report non-zero pretax foreign income (PIFO) in each year. The gray area represents the time period over which firms were defined as an MNC or domestic firm that could potentially be part of the matched control group. Firms that positive PIFO in any years 1994–1996 were defined as MNCs while firms that reported no PIFO in any of the years 1994–1996 were defined as domestics.

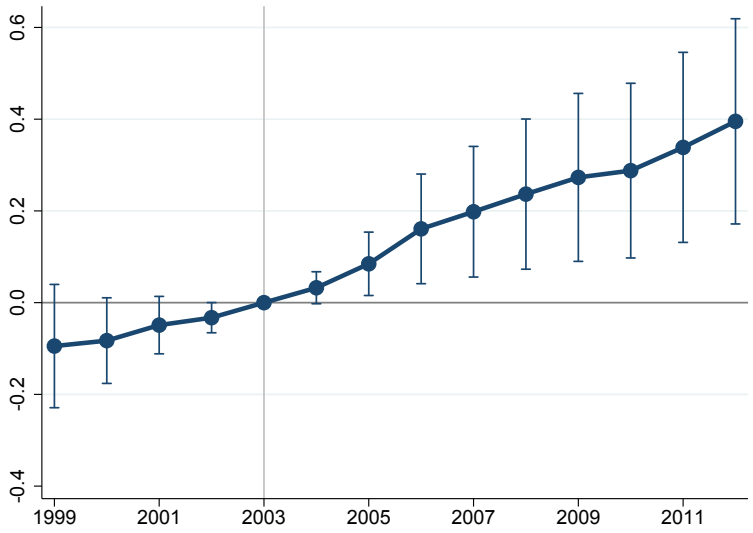
F Migration Responses to CTB

Figure F1: Population Responses to CTB and REPAT Exposure

(A) Effect of CTB Exposure on Population



(B) Effect of REPAT Exposure on Population

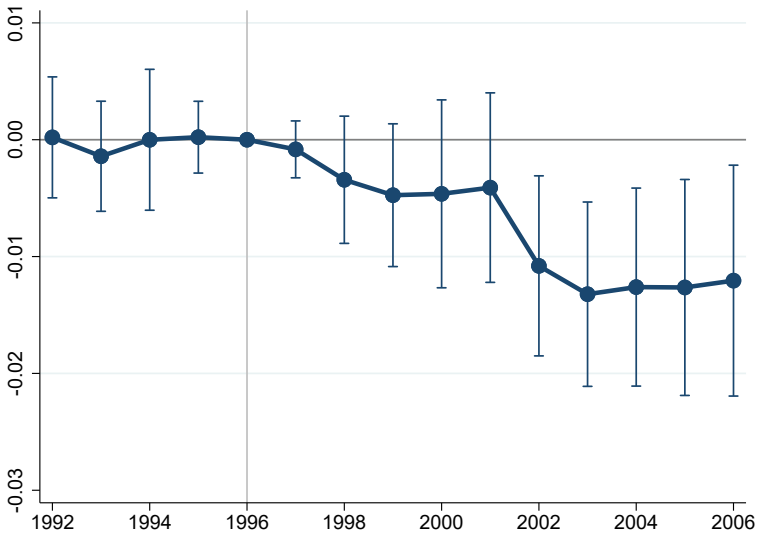


Notes: Figure G1 ... All regressions include industry-by-year and state-by-density-by-year fixed effects as well as the cross-sectional controls described Section 10. Standard errors are clustered at the county-level.

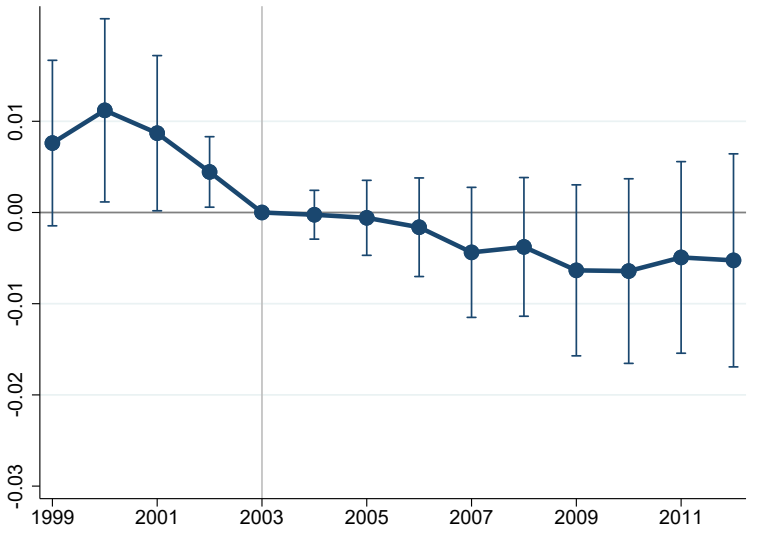
G Effects of Tax Provisions on Domestic E-Pop Ratios

Figure G1: EPOP Graphs

(A) Effect of CTB Exposure on Population



(B) Effect of REPAT Exposure on Population

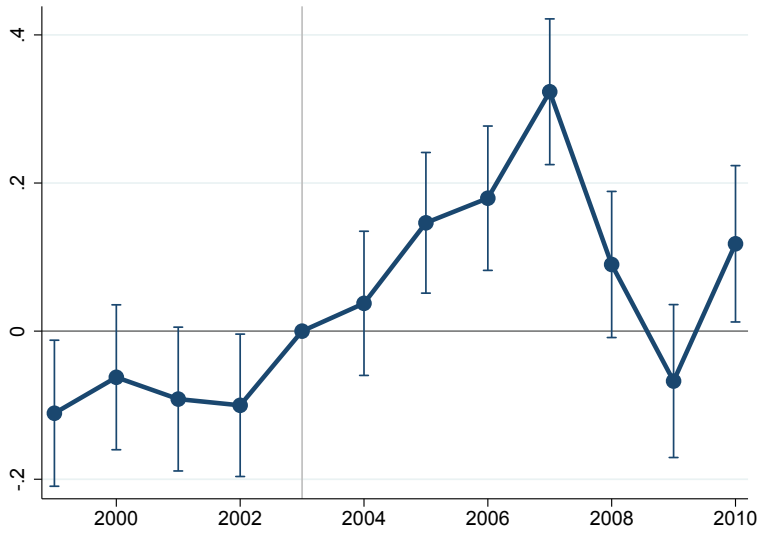


Notes: Figure G1 ... All regressions include industry-by-year and state-by-density-by-year fixed effects as well as the cross-sectional controls described Section 10. Standard errors are clustered at the county-level.

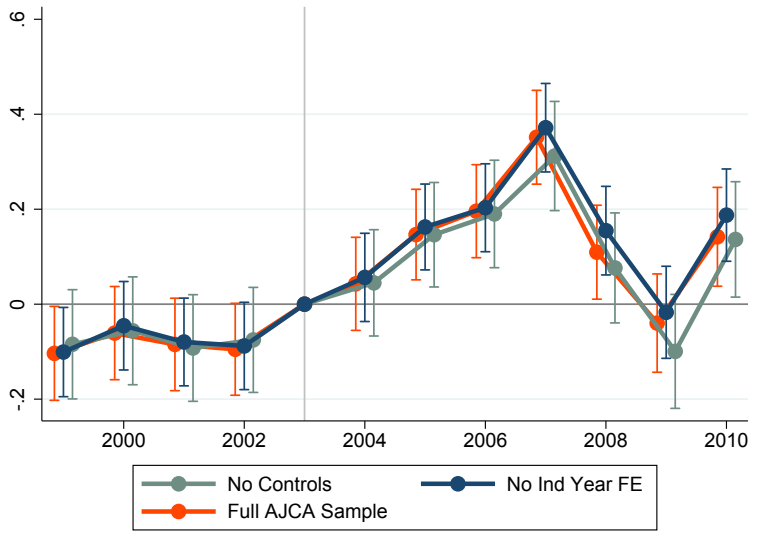
H Repatriations First Stage, Continuous Treatment

Figure H1: Effect of Total Repatriations on Payouts

(A) Effect of Total Repatriations on Total Payouts



(B) Effect of Total Repatriations on Total Payouts; Robustness



Notes: Figure H1 shows the effect of one dollar in repatriations per dollar in total assets on payouts per dollar of total assets for the repatriating MNCs and matched non-repatriating firms. Panel (A) displays coefficient estimates and 95% confidence internals from a regression of total payouts per dollar of assets on total AJCA repatriations per dollar of assets interacted with year dummies as well as firm fixed effects, industry-by-year fixed effects and controls for Tobin's q, cash scaled by assets, and ROA. Panel (B) presents alternative specifications that do not include the control variables, include year fixed effects instead of industry-by-year fixed effects, and use the full sample of repatriating firms matched to non-repatriating MNCs not limited to the NETS match. Standard errors are clustered at the firm level.

I Effect of REPAT Exposure on Domestic Earnings

Table I1: Effect of REPAT Exposure on Domestic Earnings

	(1) Δ Earn	(2) Δ Earn	(3) Δ Earn	(4) Δ Earn
REPAT Exposure × 2004–2006	-0.0369	0.0644	0.0560	0.0350
	(0.0782)	(0.0898)	(0.0809)	(0.0817)
REPAT Exposure × Post 2006	0.0502	0.255	0.229	0.207
	(0.210)	(0.204)	(0.190)	(0.192)
Industry × Year FE	✓	✓	✓	✓
State × Year FE	✓			
State × Density × Year FE		✓	✓	✓
Controls			✓	✓
GR 99–03 × Year FE				✓
Observations	1,233,462	1,233,462	1,233,462	948,673
Average Employment in Repat Firms	0.020	0.020	0.020	0.021
Aggregate Change in Earnings (Billions)	3.4	17.4	15.7	14.5

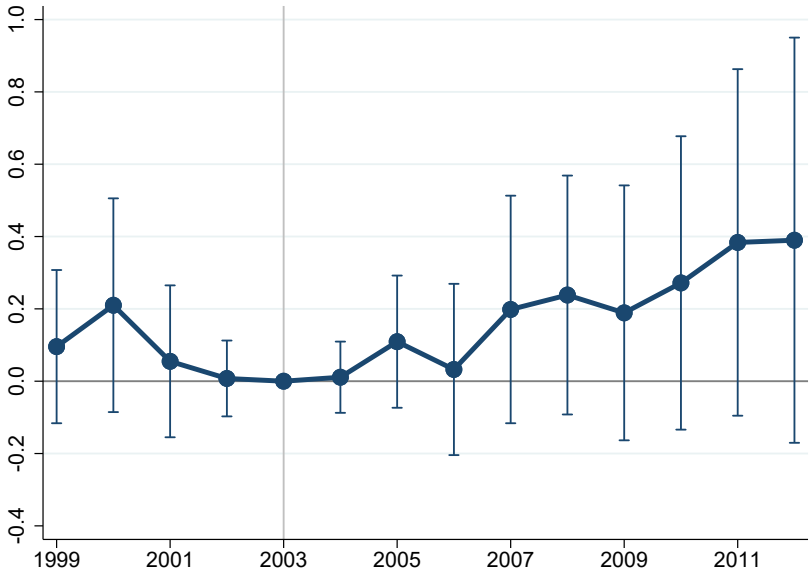
Notes: Table I1 displays β_1 and β_2 coefficients from regressions of the form

$$\Delta \text{Earn}_{jct} = \alpha + \beta_1 \text{REPAT Exposure}_c \times 2004\text{–}2006_t + \beta_2 \text{REPAT Exposure}_c \times \text{Post2006}_t + \mathbf{X'}_c \boldsymbol{\gamma}_t + \mu_{jt} + \nu_{st} + \epsilon_{cjt}$$

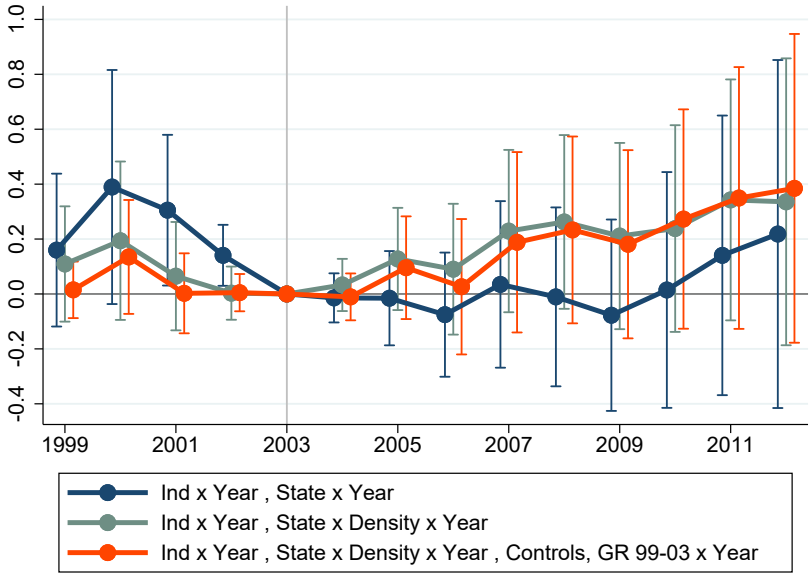
The outcome variable in all regressions is the percentage point change in county-industry total earnings relative to 2003. Specification (1) includes includes industry-by-year and state-by-year fixed effects. Specification (2) includes industry-by-year and state-by-density-by-year fixed effects. Specification (3) is our preferred specification and includes industry-by-year and state-by-density-by-year fixed effects as well as county-level cross sectional control variables interacted with year fixed effects. Specification (4) adds pre-period growth quintiles interacted with year fixed effects to Specification (3). Standard errors are clustered at the county-level. *, **, and *** denote statistical significance at the 10, 5, and 1% level. The aggregate change in employment is calculated by multiplying the $beta_2$ estimate by the average percentage of repatriating MNC employment and the QCEW total earnings in 2003. More details are provided in Section XXX.

Figure I1: Effects of REPAT Exposure on Domestic Earnings

(A) Preferred Specification



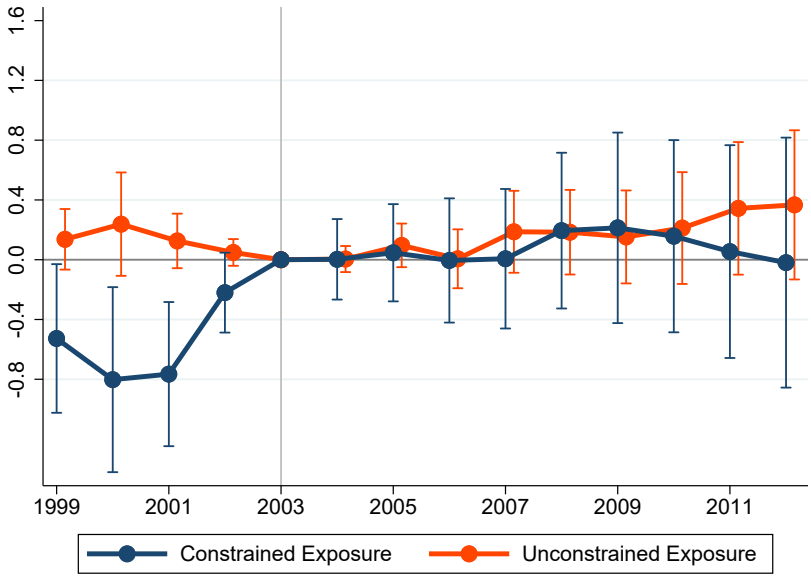
(B) Alternative Specifications



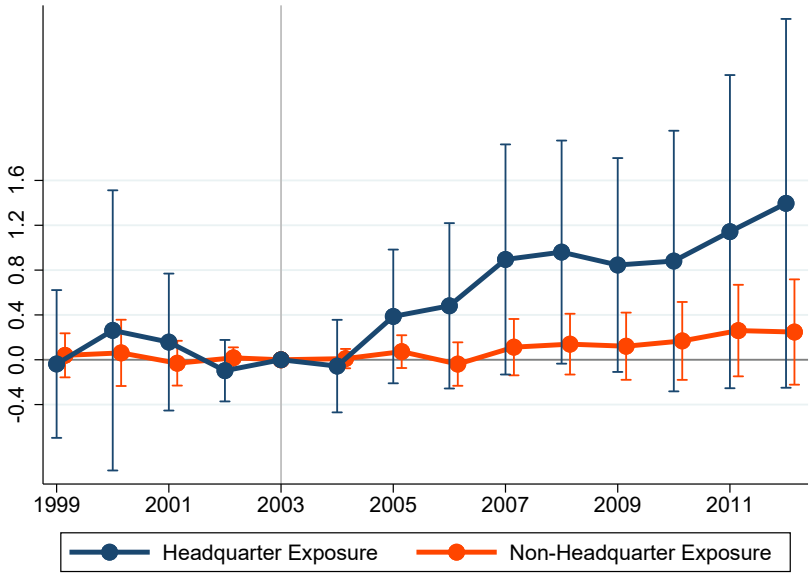
Notes: Figure I1 displays β coefficients and 95% confidence intervals from regressions in the form of Equation 5 which describe the effect of county-level REPAT Exposure on the county-industry percent change in total earnings relative to 2003. Panel (A) displays estimates from our preferred specification which includes industry-by-year fixed effects, state-by-density-by-year fixed effects, and cross sectional policy controls as described in Section 10. Panel (B) displays estimates from alternative specifications. The first includes industry-by-year and state-by-year fixed effects. The second includes industry-by-year and state-by-density-by-year fixed effects. The third includes industry-by-year and state-by-density-by-year fixed effects cross-sectional policy controls and pre-period growth-by-year fixed effects. Standard errors in all specifications are clustered at the county level.

Figure I2: REPAT Exposure Earnings Effect Heterogeneity

(A) Heterogeneity by Financial Constraint



(B) Heterogeneity by Headquarters Location



Notes: Figure I2 explores heterogeneity in the effect of REPAT exposure on local employment. Panel (A) displays β coefficients and 95% confidence intervals from two regressions in the form of Equation 5. In the first, county-industry employment growth relative to 2003 is regressed on REPAT Exposure to financial constrained repatriating MNCs and financially constrained matched non-repatriating MNCs. In the second, REPAT Exposure is based only on financially unconstrained MNCs. Financial constraint is measured by Hadlock and Pierce (2010). Firms in the top half of the distribution are defined as constrained. Those in the bottom half are considered unconstrained. Panel (B) shows the effect of headquarters and non-headquarters REPAT Exposure on employment growth relative to 2003. All regressions include industry-by-year and state-by-density-by-year fixed effects as well as the cross-sectional controls described Section 10. Standard errors are clustered at the county-level.