Effects of International Tax Provisions on Domestic Labor Markets*

Daniel G. Garrett
The Wharton School,
University of Pennsylvania

Eric Ohrn
Grinnell College &
The Brookings Institution

Juan Carlos Suárez Serrato
Stanford GSB & NBER

February 2024

Abstract

How does international taxation impact domestic workers? We study two fundamental elements of international tax systems by analyzing US provisions that isolate these elements. The first provision—the 1997 "Check-the-Box" regulations—lowered effective tax rates abroad by facilitating profit shifting from high tax foreign affiliates to tax havens. The second provision—the 2004 "repatriation holiday"—decreased tax costs of repatriating foreign earnings. Using a dynamic difference-in-differences framework, we estimate that local exposure to Check-the-Box significantly reduced domestic employment and earnings. This result implies that multinationals substitute domestic with foreign activity in response to lower effective tax rates abroad. We find the repatriation holiday had no effects on labor markets, indicating foreign cash holdings of US MNCs are not an important source of financing for domestic business activity. We conclude that policies that lower the foreign taxes of US MNCs are unlikely to benefit domestic workers.

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

JEL Codes: H2 H25 H32 J23 J38

---

*We are very grateful for comments and suggestions from David Agrawal, Lanier Benkard, Kat Bilicka, Jennifer Blouin, Anne Brockmeyer, Michael Faulkender, Fred Finan, Lucie Gadenne, Rachel Griffith, Jim Hines, Sasha Indarte, Chad Jones, Linda Krull, Becky Lester, Peter Merrill, Mitchell Petersen, Joe Shapiro, Cailin Slattery, Steve Tadelis, Francesco Trebbi, Shoshana Vasserman, Reed Walker, Mazhar Waseem, and Tom Winberry, as well as seminar and conference participants at AEA, the Institute for Fiscal Studies, the International Institute for Public Finance, the MannheimTaxation Conference, the Michigan Tax Invitational, NBER Business Tax, NBER Corporate Finance, the NTA, the Oxford Centre for Business Tax, the Swiss Workshop on Local Public Finance and Regional Economics, Stanford GSB, UC Berkeley Haas, UCSB, Wharton Applied Economics, and the Wharton Finance brownbag. We thank Jennifer Blouin, Linda Krull, Michael Faulkender, and Mitchell Petersen for generously sharing data. All authors are thankful for financial support from International Tax Policy Forum for financial support. Garrett thanks the Jacobs Levy Center for Equity Research for financial support. Suárez Serrato thanks Arnold Ventures for financial support. We also thank Alexander Almeida, Clare Doyle, Jason Harrison, and Tim Rickert for excellent research assistance. All errors remain our own.

1 Introduction

Historically, sovereign nations imposed substantial tax burdens on the foreign income of their domestic firms. In recent years, a number of countries including Italy, Japan, the UK, and the US have moved away from this practice under the belief that decreasing foreign taxes for domestic firms will create jobs and increase wages at home.[1] In this paper, we assess this belief empirically and ask: how do the designs of international tax systems affect domestic workers?

This paper answers this question by studying the domestic labor market effects of two US tax provisions that each isolate a fundamental design element of international tax systems. The first provision, "Check-the-Box," was implemented in 1997 and lowered the effective tax rate (ETR) on foreign income as it was earned. The second provision, the 2004 "repatriation holiday," substantially decreased the home country tax rate on transfers of prior earnings from foreign affiliates to the domestic parent. These provisions represent watershed moments in the evolution of the international tax landscape and measuring their effects is crucial for understanding how ongoing policy proposals—such as those for a global minimum tax—may impact workers.

Understanding the effects of international tax systems on domestic workers is challenging for a number of reasons. First, there are very few datasets on the activities of multinational firms with reliable measures of worker outcomes. Second, firm-to-firm comparisons are undermined either because most multinationals are impacted by tax changes (as in the case of Check-the-Box) or because multinationals respond to tax provisions in ways that introduce severe selection bias (as in the case of the repatriation holiday). Third, as economists and policymakers ultimately care about how tax policy impacts *all* domestic workers, firm-to-firm comparisons are insufficient because they do not capture important spillover effects on US labor markets.

This paper circumvents these problems by implementing a local labor markets approach that compares outcomes in more and less exposed domestic markets before and after the provisions are implemented. We determine local exposure to each provision via a novel mapping of the geographic footprints of US multinational corporations across domestic labor markets. Using a

[1]For example, in a press release, the US House Ways and Means Committee stated that the Tax Cuts and Jobs Act of 2017 "[m]odernizes our international tax system so America's global businesses will no longer be held back by an outdated 'worldwide' tax system that results in double taxation for many of our nation's job creators... [m]akes it easier and far less costly for American businesses to bring home foreign earnings to invest in growing jobs and paychecks in our local communities... [p]revents American jobs, headquarters, and research from moving overseas by eliminating incentives that now reward companies for shifting jobs, profits, and manufacturing plants abroad" (U.S. House Ways and Means Committee, 2017).

dynamic difference-in-differences design, we separately identify the causal effects of each provision on domestic workers, incorporating spillovers from the tax policies. In addition to describing the interplay between international tax systems and domestic labor markets, our estimates also identify economic fundamentals underlying the behavior of US multinational firms.

We find that Check-the-Box (hereafter CTB) had large, negative effects on local employment and earnings. On average, workers in the most exposed counties experienced three percentage points lower relative growth in employment and four percentage points lower relative growth in total earnings. Our estimates imply that, by 2003, one million fewer jobs were created in the counties that had the highest exposure to CTB relative to other counties. From an economic perspective, these results imply that, on average, multinationals substitute domestic with foreign activity in response to lower effective tax rates.[2]

When studying the 2004 repatriation holiday (hereafter RH), we find precise null effects of the provision on employment and earnings in US labor markets. The provision did not stimulate employment or earnings growth even for markets heavily exposed to the firms most likely to benefit from the policy: financially constrained firms and those with large stocks of earnings held abroad. These results show that foreign cash holdings of US MNCs are not an important source of financing for domestic business activity. Together, our results show that reforms that lower taxes on the foreign profits of US MNCs are unlikely to benefit domestic workers.

By identifying which multinationals responded to these provisions, we also offer a fundamental reassessment of the economic forces governing how international tax policies impact domestic labor markets. We first show that markets exposed to more R&D intensive MNCs did not experience job losses or earnings losses in response to CTB. This result is consistent with R&D intensive firms being unable or unwilling to substitute jobs abroad.[3] We then document that repatriating firms were more likely to be R&D intensive, to have significant patent activity, and even to be headquartered in Silicon Valley. That is, R&D intensive firms—which were less likely to offshore jobs in response to CTB—were also more likely to repatriate funds during the RH. We integrate these results by showing that CTB had a null effect on domestic labor markets that

[2] To the extent that lower taxes on foreign earnings decrease production costs, CTB induces both a global scale effect and a substitution effect between foreign and domestic activities. Our results suggest that the negative substitution effect on local employment dominated the positive global scale effect.

[3] As we discuss in Section 2.1, MNCs may be unable to move R&D production abroad due to agglomeration forces in the US knowledge economy. Similarly, R&D tax credits and other tax policies make it more desirable to base R&D activities in the US, making MNCs unwilling to shift such activities abroad.

were exposed to firms that later participated in the RH. These results suggest that the MNCs that eventually repatriated never left the US in first place. In contrast, less innovative MNCs responded to CTB by permanently shifting jobs abroad. Non-R&D-intensive firms were less likely to repatriate under the RH because their prior foreign earnings were likely used to finance real business activity abroad (see, e.g., Hanlon et al., 2015). Together, these results suggest that international tax policies designed to increase the competitiveness of US MNCs in the global economy did so at the expense of inducing real offshoring by non-R&D intensive firms.

We discuss the details of the provisions we study in Section 2. Each constitutes a watershed moment in the evolution of the international tax landscape. Check-the-Box allowed US MNCs to consolidate their foreign affiliates into a single taxable entity for US tax purposes. As a result, flows of passive income within US MNC affiliate groups were no longer subject to immediate taxation under Subpart F rules and 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 implementation. Tax analysts credit CTB for the proliferation of tax planning strategies that eventually necessitated a major overhaul of the US's international tax provisions as part of the Tax Cuts and Jobs Act of 2017 (Santos, 2018).

The RH—more specifically, 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 policy significantly increased the incentive for firms to return profits earned abroad back to the United States. US MNCs repatriated nearly $300 billion in foreign earnings under the holiday provision. RH provided a shock to the tax rate on repatriated earnings and set expectations that new foreign earnings could benefit from a future holiday. These expectations encouraged aggressive profit shifting by US MNCs, which motivated the OECD's base erosion and tax planning (BEPS) project (Kadet, 2020).

In Section 3, we describe how we map the distribution of domestic business activity of US MNCs using a novel match between financial statements data from Compustat and the location and employment of establishments from the National Establishment Time Series (NETS) database. This section also details how we construct measures of exposure to each provision for US local labor markets. We define exposure to CTB as the percentage of employees in each county working for US MNCs just prior to CTB implementation. Our exposure measure assumes that all MNCs are treated by CTB. Consistent with this assumption, Altshuler et al. (2023) use IRS tax data to document that, by 2000, MNCs with at least one checked-up foreign affiliate were responsible for 85% of the foreign profits of US MNCs. By the end of our sample in 2006, over 95% of the foreign profits of US MNCs were earned by MNCs with a disregarded affiliate under CTB. In the case of RH, we use data on the dollars repatriated by each MNC and define exposure as the dollars repatriated per worker in a county, with repatriated dollars allocated in proportion to employees across each repatriating firm's establishments.

We estimate the effects of CTB on local labor markets in Section 4. We use a dynamic difference-in-differences framework to compare employment and total earnings in counties most exposed to CTB to counties with less exposure to the policy. We find that local exposure to CTB decreases employment and total earnings beginning in 1997. Worker outcomes in the most exposed local labor markets continue to deteriorate through at least 2003. We show the time pattern of domestic labor responses coincides with trends in the establishment of "disregarded entities," the primary vehicle by which firms take advantage of the tax planning opportunities afforded by CTB. We estimate that employment dropped by more than three percentage points in the most exposed counties relative to less exposed counties 10 years on. We estimate a similar four percentage point decline in total labor earnings over the same time horizon. But, we find no effect of CTB on earnings per worker. A simple cross-sectional comparison based on these results indicates that, by 2006, the US lost approximately 1 million jobs and $42 billion in labor earnings per year as a result of CTB.

A causal interpretation of our estimates relies on the assumption of parallel trends—that outcomes in local labor markets with the most exposure to CTB would have moved in parallel with the outcomes of the less exposed markets in the absence of the policy. While this assumption is inherently untestable, we provide a series of checks to attribute the impacts we uncover to CTB. First, we flexibly control for a number of cross-sectional factors that have been documented to affect worker outcomes during the sample period including industry-by-year fixed effects to remove the impact of unrelated sectoral trends. We also use these factors to construct inverse probability of treatment weights which we employ to better achieve a *ceteris paribus* comparison. Second, our dynamic difference-in-differences approach shows parallel trends between the most- and less-exposed counties prior to 1997 on all outcomes. Third, we show the labor market effects are concentrated among counties exposed to the MNCs that stood to gain the most from CTB:

those with high foreign effective tax rates prior to the policy. Fourth, we show that the dynamic pattern of labor market effects closely matches the timing of policy take-up as measured in administrative tax data and is not biased by internal migration.

A particular identification concern is that technology is changing around the same time as CTB in a way that is potentially conducive to offshoring for non-tax reasons. The rapid expansion of the internet and personal computers across the globe during our sample could present an opportunity for US MNCs to offshore more cheaply than before. To this end, we are careful to control for, and interact with, local technological offshorability as measured as the share of jobs likely to be offshored (Autor and Dorn, 2013). The tax-induced declines in employment and earnings that we observe are of the same magnitude in low and high offshorablility markets, which shows that we are not simply measuring spurious technological shocks. Finally, we show that exposure to R&D intensive MNCs, those that are likely least able to substitute operations abroad, does not depress labor market outcomes after 1996. Based on our CTB analyses, we conclude that, on average, foreign and domestic business activities are substitutes in response to changes in foreign effective tax rates induced by CTB, implying that reforms that decrease foreign taxes are unlikely to create jobs or increase earnings at home.

We then explore the domestic labor market responses to the RH in Section 5. We find that the $290 billion in qualified repatriations under the holiday had no effect on domestic employment or labor earnings. This result is robust to a number of specifications designed to test the validity of the underlying parallel trends assumption. We also find no differential (or any) impacts due to repatriations by financially constrained MNCs, nor to repatriations from firms with large cash holdings abroad, an indicator of potential benefit from the policy. Motivated by these null results and following Blouin and Krull (2009), Dharmapala et al. (2011), and Faulkender and Petersen (2012), we compare payout behavior between repatriating firms and a matched sample of non-repatriating MNCs. We estimate that 66¢ of each dollar repatriated were paid out to shareholders via dividends and share repurchases before payout behavior normalizes in 2009. Based on these results, we conclude that foreign cash holdings are not an important source of financing for the domestic business activities of US MNCs. Reforms that decrease the tax rate on repatriations are unlikely to benefit domestic workers.

This project makes several important contributions. First, the paper contributes to our understanding of the effects of CTB. Despite the historical and economic importance of this policy, prior research has established few facts about its effects. Blouin and Krull (2019) and Dunbar and Duxbury (2015) show that CTB led to lower effective tax rates on foreign income. Blouin and Krull (2019) and Altshuler et al. (2023) link these lower effective tax rates to an increase in the organizational complexity of US MNCs and to the adoption of advanced tax planning strategies. Albertus (2020) and Altshuler et al. (2023) also show that the MNCs with most to benefit from the policy increased domestic R&D, which is consistent with our finding that exposure to the most R&D intensive firms did not depress local labor market outcomes after CTB implementation.[4] This is the first paper to explore the domestic labor market effects of CTB. Our results show that CTB led to significant declines in domestic employment and earnings likely due to the offshoring of US business activity abroad.[5]

This research also provides new evidence on the effects of the RH on domestic economic activity. Prior research has come to mixed conclusions, emphasizing the challenge of finding an appropriate control group for repatriating firms.[6] Highlighting this selection problem, Blouin and Krull (2009) find that repatriating firms had substantial cash available, but lower investment opportunities.[7] Our study contributes to prior work by documenting the prevalence of repatriation among R&D intensive, patenting, and Silicon Valley firms, which were less likely to offshore jobs in the preceding decade. Despite these differences in the characteristics of repatriators, our local labor market approach—which compares *places* rather than firms—identifies the employment and earnings effects of the RH, inclusive of any local spillovers. Based on the null effects we document, we conclude that the RH did not help the American Jobs Creation Act of 2004 achieve its job creation goals because cash held abroad is not an important source of financing for domestic business activity.

This study also contributes to our understanding of the substitutability or complementarity of the domestic and foreign business activity of MNCs. Williams (2018) documents that US

[4] Liu (2020) explores the investment effects of a similar tax change—the UK's 2009 switch to a territorial tax system—and finds that UK MNCs increased foreign investment in places that experienced the largest decreases in foreign effective tax rates.

[5] Clausing (2005) discusses the incentives for offshoring created by CTB from a theoretical perspective.

[6] Dharmapala et al. (2011) instrument for repatriations using pre-reform characteristics and find that the holiday led to a large increase in share repurchases, but did not stimulate real business activities. Faulkender and Petersen (2012) incorporate information on the prior levels of foreign earnings held abroad and find that financially constrained firms responded to the RH with increased investment. Brennan (2014) argues that large, repatriating MNCs cannot be reasonably compared to other firms and documents that the largest repatriating MNCs increase cash mergers, debt reductions, and R&D more than payouts to shareholders.

[7] Our analysis relies on data from both Blouin and Krull (2009) and Faulkender and Petersen (2012) to construct appropriate firm- and place-based comparison groups when analyzing the effects of the RH.

MNCs off-shore jobs to locations with lower corporate tax rates. Suárez Serrato (2018) shows that business activity in the mainland declined dramatically when affected US MNCs could no longer use Puerto Rico to avoid US taxes. Kovak et al. (2021) finds that increases in foreign employment of US MNCs due to bilateral tax treaties can lead to modest increases in domestic employment.[8] This study differs from these papers in that we analyze the effects of changes in US international tax policy that lower taxes on foreign profits. Our evidence is consistent with the idea that, on average, US MNCs substitute domestic and foreign economic activities in response to changes in foreign effective tax rates. We also show that R&D intensive firms are less prone to substitute foreign and domestic activities, suggesting that agglomeration economies and other incentives for R&D play key roles in shaping offshoring responses to tax policy.

Our results have the power to help design international tax regimes that balance domestic labor market concerns, the international competitiveness of US MNCs, and government revenue. We conclude that while policies that lower tax rates on foreign earnings and repatriations may help multinational firms, they do so at the cost of tax revenue and will likely have deleterious effects on domestic labor markets. Ultimately, our results suggest that while it is easy to lose domestic jobs when international tax changes encourage multinationals to produce abroad, it may be much harder to bring those jobs back home using tax-based counter-measures.

2 The Tax Provisions

2.1 Check-the-box (CTB)

Many nations, including the US, historically operated worldwide tax systems that imposed substantial tax burdens on the foreign income of their domestic firms.[9] Under the US worldwide system, 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

[8] A broader literature in international trade also explores the determinants and impacts of offshoring issues and finds that the association between domestic and offshore activity depends on the type of offshoring activity and motives for offshoring. Harrison and McMillan (2011) find that MNCs offshoring to low-wage countries decrease US employment, but that offshoring may complement domestic employment when the foreign and domestic business activities differ substantially. This heterogeneity manifests in other papers that find evidence of substitutability (Muendler and Becker, 2010; Monarch et al., 2014), evidence more consistent with complementarity (Desai et al., 2009; Wright, 2014), or that, on average, find no relationship (Slaughter, 2000, 2001; Ottaviano et al., 2013; Antras et al., 2017).

[9] Under a worldwide tax system, an MNC must pay taxes in its home country on both domestically-earned and foreign-earned profits. This system was considered the "international norm" by the US Treasury as recently as 2000 (U.S. Treasury Department, 2000).

was earned. Panel (A) of Figure 1 displays the organizational structure of a typical US MNC. The MNC consists of a US Parent firm who owns both affiliates in high tax and low tax (or tax haven) countries. Foreign affiliates generate two types of income: active and passive. Both types of income are taxed by the country in which it is earned. During the time period we study, taxation of active income was deferred until the income was repatriated from either affiliate to the US Parent. 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—either from the High Tax to Tax Haven affiliate or vice versa—triggered immediate US taxation at the difference between the foreign and US tax rates. As a result, strategies that shifted income from the High Tax Affiliates to the Tax Haven Affiliates via interest payments, royalties, or dividends triggered immediate US taxation.

On December 18, 1996, the US Treasury department released a new regulation, the "Simplification of Entity Classification Rules."[10] 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.[11,12] Panel (B) of Figure 1 shows how CTB affects the taxation, organizational structure, and production activities of our typical US MNC. 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 can "check-the-box" to be treated as a "disregarded entity" for tax purposes. If the High Tax Affiliate becomes a disregarded entity, the US tax authority now only recognizes a single Consolidated Foreign Corporation for tax purposes. Now, passive income

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

[11] 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.

[12] A similar regulation, the §954c6 "Look-Through Rule" was enacted in 2005. §954c6 effectively subsumed the CTB rules.

flows between the High Tax and Tax Haven affiliates are not immediately subject to US taxation under Subpart F rules. As a result, income can be shifted away from the High Tax Affiliate via interest, royalties, or dividends, which decreases taxable income in the high tax country and increase taxable income in the tax haven.

The income shifting strategies afforded by CTB lowered the foreign ETRs of high tax affiliates. While CTB was not intended to be a rule about international profit shifting, the Treasury quickly recognized what CTB had implications for Subpart F income and sought to amend the rule in 1998 (Oosterhuis, 2006). Congress intervened and maintained the CTB policy in part due to arguments from the business community that allowing US MNCs to pay lower taxes on foreign income made these firms more competitive in the global market as they compete with non-US MNCs that also have access to foreign tax avoidance strategies.[13]

Our goal is to understand how allowing US firms to shift profits from high-tax foreign affiliates to tax havens affects US labor markets. From the perspective of the US parent, the lower taxes on foreign earnings decrease production costs and induce both a global scale effect and a substitution effect between foreign and domestic production activities. Together these effects govern the reallocation of business activity within the MNC. The lower ETR produces a scale effect which increases employment and business activity worldwide because the MNC's average worldwide costs have decreased. The lower ETR also reduces the relative cost of producing in the High Tax Affiliate resulting in a substitution of employment and business activity toward the high tax country. If the substitution effect outweighs the scale effect then CTB will decrease employment and business activity in the US parent. On the other hand, if the scale effect dominates, we would expect US employment and business activity to increase.

Of course, certain types of MNCs may be less able to substitute their US production to high tax affiliates abroad. In particular, R&D intensive MNCs may be unable to shift their production of R&D activities outside of the US due to economies of scale in intellectual property production. In fact, for innovative firms, CTB itself may have even increased the incentive to produce IP in the US for two reasons. First, CTB increased the value of IP to the extent that it can be underpriced and sold to the Tax Haven Affiliate to facilitate income shifting. Second, given that

[13] As described by Oosterhuis (2006), the business community argued that "tax planning that reduces foreign taxes, not U.S. taxes, can be a good thing from a U.S. business competitiveness perspective where the business activity is inevitably abroad (without regard to taxes) and where the relevant competitors are not local but are other country multinationals that are also in a position to engage in earnings stripping transactions."

US tax rates were high globally during the period we study, all else equal, R&D production in the US results in more valuable tax deductions and credits. Thus, R&D intensive firms may less willing to shift their activites abroad and may actually be incentivized to increase R&D activities in the US in response to CTB. We therefore hypothesize CTB will have more positive (or at least less negative) effects on the US employment and business activities of R&D intensive firms.

The effects of CTB are readily apparent in filings of Form 8832. Figure 2 presents aggregate trends in entity elections made by foreign affiliates reproduced from Field (2008). Panel (A) shows that total entity elections made by US foreign affiliates increased from approximately 5,000 to more than 15,000 per year from 1997 to 2006. Panel (B) shows that disregarded entity elections for foreign affiliates were composed of both change of status elections by established foreign affiliates and initial elections by newly created international affiliates.

Unsurprisingly, we also see large reductions in Subpart F income after 1996. Figure 3 Panel (A) displays aggregate Subpart F income per total assets for the largest 7,500 foreign affiliates of US MNCs based on IRS SOI data (IRS, 2021). Prior to CTB implementation, the ratio of Subpart F income to total assets was approximately 0.85 percent.Immediately after the provision was implemented, this ratio decreased by more than 25%, falling to 0.63. In the following years, this ratio continued to decrease, reaching a low of 0.51 in 2006"

We employ Compustat data to show the effect of CTB on worldwide ETRs based on the approach of Dyreng and Lindsey (2009), which measures the tax expense response to an additional dollar of pretax profit. We extend this approach to a dynamic DD setting that compares MNCs to a matched sample of US domestic firms around CTB implementation. We present these results in Figure 4 and discuss the empirical approach in more depth in Appendix F. The coefficient estimates in Figure 4 represent the difference between MNC and domestic worldwide effective tax rates in each year relative to the difference in 1996. The figure shows that the worldwide effective tax rates for MNCs decreased by approximately 8 percentage points relative to matched domestic firms in the five years after CTB implementation. This result is consistent with the magnitudes estimated in Dunbar and Duxbury (2015) and Blouin and Krull (2019) based on alternative data sources and alternative comparisons.

To summarize, CTB made income shifting across foreign affiliates a profitable strategy for US MNCs starting in 1997. While CTB decreased effective tax rates on international profits when they were earned, it did not provide any tax relief if and when firms decided to ultimately repatriate these profits. In contrast, the second policy we examine decreases repatriation taxes without disturbing foreign ETRs.

2.2 The Repatriation Holiday (RH)

On October 22, 2004 the American Job Creation Act (AJCA) was signed into law. The AJCA was designed to benefit US MNCs and exporters after the World Trade Organization challenged the legality of a US export incentive.[14] The keystone of the act was a temporary tax holiday on repatriations of dividends from foreign subsidiaries.[15] 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.

Figure 3 Panel (B) shows total repatriations during the years 2003–2008 based on BEA data as assembled by Smolyansky et al. (2019). 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.[16] In sum, nearly $300 billion of repatriations made during the RH window qualified for the tax holiday (Redmiles, 2008).

The ACJA stipulated that repatriations were to 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

[14]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 alternative, permissible measures to replace the subsidy.

[15]The AJCA also contained several other incentives including 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. We are careful to control for any incentives created by the DPAD and expensing rules in our empirical analysis. 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.

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

expenses.[17] Despite these limited prescribed uses, Desai et al. (2009) estimate firms that repatriated were not more likely likely to make domestic investments, perform R&D, or pay off debt. Instead, Desai et al. (2009) find repatriators were more likely to make payouts to shareholders, estimating that repatriators paid out 77 cents per dollar repatriated. Consistent with this lack of response in real business activity, Blouin and Krull (2009) finds firms with relatively poor business prospects were more likely to repatriate funds from abroad. In contrast, Faulkender and Petersen (2012) estimate that the RH increased investment for cash-constrained MNCs and Brennan (2014) argues large MNCs used most repatriated cash for allowed activities. This debate partially motivates examining the effect of the RH on domestic worker outcomes using a local labor markets—as opposed to firm-level—approach.

3 Measuring Exposure to International Tax Provisions

Our goal is to examine the effect of international tax provisions on domestic labor markets using a local labor markets empirical approach. This approach is ideal in the setting of international taxes for two reasons. First, even when the appropriate data is available, firm-to-firm comparisons are undermined either because most multinationals are impacted by tax changes (as in the case of Check-the-Box) or because multinationals respond to tax provisions in ways that introduce severe selection bias (as in the case of the repatriation holiday). By comparing places that are more-exposed to each provision to places that are less-exposed, the local labor markets approach subverts the inability to construct an appropriate control group as in firm-to-firm analyses. Second, the local labor markets approach allows us to estimate both the direct effect of the policy on labor market outcomes for workers at MNCs as well as indirect effects on other workers. As economists and policymakers ultimately care about how tax policy impacts *all* domestic workers, capturing these spillover effects is crucial.

To implement this approach, we (1) identify firms that are affected by each provision, (2) map the geographic distribution of their business activity across the US to identify the places most affected by each policy, then (3) measure how employment and earnings outcomes evolve across

[17] To qualify for the holiday, repatriations had to meet several additional 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. 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.

local labor markets differentially exposed to each policy. To make the most- and less-affected labor markets comparable, we rely on both inverse probability weighting and a suite of flexible fixed effects.

We draw 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 the National Establishments Time Series (NETS) (Walls & Associates, 2012), and (4) county-industry employment and total earnings data from the Quarterly Census of Employment and Wages (QCEW, 2017). Focusing on county-industry level outcomes allows us to include industry-by-year fixed effects throughout, which flexibly control for secular industry trends. We also rely on a suite of cross-sectional control variables that the academic literature has shown to differentially affect workers across local labor markets.

3.1 Matching Compustat Firms to Places

We use Compustat to identify US MNCs. 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% of all US foreign affiliates and 95% of all US foreign assets.

To measure domestic exposure to each provision, we rely on the NETS database, which lists establishments and employment counts along with their geocoded location data for US firms. We create a firm-to-firm match by linking Compustat GVKEYs to NETS HQDUNS codes.[18] We check these matches manually and trim any obvious mistakes. Finally, for the largest unmatched Compustat firms, we manually check for matches in the NETS data. This process identifies the share of workers in each US county that work in each matched Compustat firm.

Appendix Figure B1 assesses this novel Compustat to NETS match and displays 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

[18] In practice, 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). See Appendix B for a complete description of our matching procedure.

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 income from foreign operations (PIFO) in each year. The very high coverage of large, international firms assures us our analysis of both policies is based on the vast majority of US MNC business activity.

3.2 Measuring Local Exposure to the Provisions

To measure exposure to CTB, 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, which yields 786 firms. Using this sample of MNCs, we calculate **CTB Exposure** as the share of employees in a county working for US MNCs in 1996, the year prior to CTB implementation;

$$\textbf{CTB Exposure}_c = \frac{\sum_i \text{Emp}_{ic1996}\mathbb{I}(\text{MNC}_i = 1)}{\text{Emp}_{c1996}}, \tag{1}$$

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 follow a similar process in constructing our measure of local exposure to the repatriation holiday. We start with data from Blouin and Krull (2009) based on FSP 109-2 disclosures. The Blouin and Krull (2009) data identify 357 firms that repatriated funds in response to the holiday as well as the amount that they repatriated. We identify 333 of these firms in our Compustat-NETS matched sample. For each repatriating firm, we apportion the total dollar value of repatriations to counties based on the fraction of the firm's NETS-reported employment in a given county. We then divide the apportioned repatriations by the total number of NETS workers in a county. This process yields our **REPAT Exposure** variable, which measures the total dollars repatriated per worker for each county;

$$\textbf{REPAT Exposure}_c = \frac{\sum_i \left\{ \text{Repatriations}_i * \frac{\text{Emp}_{ic2003}}{\text{Emp}_{i2003}} \right\}}{\text{Emp}_{c2003}} \tag{2}$$

where i denotes firm and c denotes county. This dollar-scaled measure of local exposure accounts for differences in the scale of repatriations across local labor markets.

3.3 Exploring Local Exposure Variables

The geographic variation in local exposure to CTB and the RH are shown in the first panels of Figures 5 and 6, respectively. Both maps show that there is considerable variation in exposure to both policies across and within states.[19] The CTB map scale indicates that the county most exposed to CTB had 60.86% of its labor force employed in MNCs in 1996, while 20% of counties had less than 0.15% of their employment in MNCs. REPAT exposure ranges from $0 to more than $50,000 repatriated per worker in a county.

In most empirical analyses, we rely on discretized treatment measures. This provides several benefits. First, discrete treatments facilitate the use of inverse probability weights, which allow us to compare more observably similar counties without strict functional form assumptions on control variables. Second, the discrete treatments weaken the identifying assumption behind our difference-in-differences estimates since we do not have to assume constant linear dose responses (Callaway et al., 2021). Third, discretization also eliminates the impact of outliers in treatment values. Although we primarily rely on discretized treatments, we show that our main empirical results are robust to the use of continuous treatments in Appendices G and J.

Our discretized measure of CTB Exposure is **CTB Treatment**. In our baseline models, CTB Treatment is equal to one for counties that are in the top quartile of the CTB Exposure distribution.[20] The cutoff for treatment is having 5.5% CTB Exposure, which is comparable to the employment-weighted national average of 5.6%. Those counties that are treated have average CTB Exposure of 10.4%, while the control counties have average CTB Exposure of 2.4%. As a result, the effects of CTB Treatment can be interpreted as the effect of increasing CTB Exposure, that is the MNC employment share in a county, by 8 percentage points.

We are cognizant that local labor markets that are most exposed to CTB may be different from those that are less exposed on a number of margins. Panel (B) of Figure 5 presents estimates from logistic regressions of CTB Treatment on variables representing local size, density, demographics, sectoral composition, and exposure to changes in trade, tax, and technological patterns and policies. To capture any unobserved determinants that might drive the business location decision of large corporations within the US, we construct and also include a measure

[19] In practice we include state-by-year fixed effects in all empirical specifications, so our identification only comes from within-state variation.

[20] In Appendix G we show that our results are robust to alternative cutoffs.

of exposure to a matched sample of large domestic firms.[21] The navy (upper) estimates show that counties most exposed to CTB have a higher percentage of college graduates, do more manufacturing, are less exposed to federal bonus depreciation policies (Garrett et al., 2020), are more exposed to NAFTA (Hakobyan and McLaren, 2016), are more exposed to IRC Section 936, a policy that impacted the ability of US MNCs to shift profits to Puerto Rico, (Suárez Serrato, 2018), and are less likely to be exposed to the matched sample of large domestic firms.

Based on these estimates, we construct inverse probability weights following Abadie (2005). We use these weights throughout the analysis to minimize concerns that differences in local characteristics unrelated to CTB (or RH) are driving or biasing our estimates. The construction of the weights is detailed in Appendix E. The orange (lower) marks show estimates from the same logistic regression of CTB Treatment on local characteristics, but now including the inverse probability weights. There are no longer statistically significant differences between treated and untreated counties for most local characteristics. The one difference (out of 19) that remains statistically significant is the correlation between CTB Treatment and IRC Section 936 exposure, which is now negative. The elimination of several statistically significant differences shows the power of the IPW method to produce a more apples-to-apples comparison, while the remaining difference in exposure to IRC Section 936 highlights the need to remain careful that other policies and local differences do not bias our estimates. To this end, in most models, we include controls that interact cross-sectional differences in local characteristics with time fixed effects.

As with CTB, in estimating the effects of the RH on local labor market outcomes, we compare local labor markets that are most exposed to repatriations to local labor markets that are less exposed based on our REPAT Exposure measure. We define **REPAT Treatment** equal to one for counties in the top half of the REPAT Exposure distribution. Treated counties are those that receive more than \$218 in repatriations per worker in the county. Based on this treatment definition, the median treated county receives \$569 in repatriations per worker more than non-treated counties, which is the same order of magnitude as other fiscal stimulus policies enacted during the same time period.[22,23]

[21] The variables describing the local characteristics and their measurement are detailed in Appendix A. Appendix C describes the process by which we match MNCs to large domestic firms to construct the Matched Domestics control.

[22] For example, a two-earner household received \$600 in federal rebate checks issued in 2001 (Shapiro and Slemrod, 2003).

[23] The average difference in repatriations between counties with REPAT Treatment equal to 0 vs 1 is \$1,383.

Figure 6, Panel (B) explores the relationship between REPAT Treatment and local characteristics following almost the same method as for CTB. The one major difference is that, now, we include exposure to a matched sample of MNCs that did not repatriate under the holiday rather than a matched sample of domestic firms.[24] The navy (upper) estimates shows counties treated by REPAT have larger populations, have more workers with college degrees, are more manufacturing intensive, are less exposed to federal bonus depreciation, and are more exposed to the Extraterritorial Income Exclusion, NAFTA, and IRC Section 936. The orange (lower) estimates show that, after applying the inverse probability weights, counties exposed to REPAT differ only in that they have a higher percentage of workers with college degrees. As with the CTB, when we estimate the effects of REPAT Treatment, we include this full suite of cross-sectional controls interacted with time-fixed effects to mitigate concerns that differences in local characteristics might bias our results.

3.4 Domestic Labor Market Outcomes

We use county-by-industry data from the Quarterly Census of Employment and Wages (QCEW, 2017) to measure our domestic labor market outcome variables. Our primary local labor market outcome is employment, which we measure in percent changes relative to the year prior to each policy implementation. For example, we measure our employment outcome for CTB as

$$\Delta \text{Emp}_{cjt} \equiv \frac{\text{Emp}_{cjt} - \text{Emp}_{cj1996}}{\text{Emp}_{cj1996}}$$

where c denotes county, j denotes NAICS 3-digit industries, and t denotes year. Measuring outcomes at the county-industry level allows us to flexibly control for unrelated secular trends by including industry-by-year fixed effects. In analyzing the RH, we normalize outcomes relative to 2003. We call this variable ΔEmp.

We also analyze the effect of both policies on (1) changes in total earnings from the QCEW (ΔEarn), (2) changes in wages, which we construct as total earnings divided by total employment using the QCEW (ΔWage), and (3) changes in employment-to-population ratio (ΔEPop) which

[24] To construct the matched sample of non-repatriating MNCs we propensity score match repatriating MNCs to non-repatriating US MNCs based on the characteristics that Blouin and Krull (2009) and Faulkender and Petersen (2012) find predict repatriation. In general, these variables measure growth at home and worldwide, effective tax rates at home and abroad, and the amount of cash held abroad ("permanently reinvested earnings"). We restrict each match to be within the same NAICS 1-digit sector. Appendix C fully describes the matching procedure.

we measure as county-level employment from the QCEW scaled by county-level working age population estimates (NCI, 2022).

4 Domestic Labor Market Effects of Check-the-Box

In this section, we measure the effects of CTB on local labor market outcomes. We begin by estimating dynamic difference-in-differences regressions of the form

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

Our unit of observation is a NAICS 3-digit county-industry. Δy_{cjt} is the percent change in a given labor market outcome 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 continuous, cross-sectional control variables interacted with year fixed effects that varies across specifications.[25] The sequence of β_y estimates describe the percentage point difference in the outcomes between treated and control counties relative to differences in 1996. In all analyses, we cluster standard errors at the county level as this is the level at which our treatment variable is defined (Cameron and Miller, 2015).

The identifying assumption underlying this dynamic difference-in-differences framework is that, in the absence of CTB, domestic labor market outcomes in treated counties would trend in parallel to outcomes in counties with less CTB Exposure. The key threat to this identifying assumption is that other shocks coincident to CTB differentially affect counties with high or low CTB Exposure. While the validity of this assumption is inherently untestable, we include the state-by-year fixed effects, industry-by-year fixed effects, and the suite of cross-sectional control variables interacted with year fixed effects to mitigate these concerns.[26]

In most regressions, we include four categories of cross-sectional controls: sectoral composition controls, population and demographic controls, trade, technology and tax controls, and a

[25] Note that we do not include county-industry fixed effects as our outcome variables are measured in percent changes within county-industry units.

[26] 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). When the outcome variable is ΔEpop, we do not include industry-by-year fixed effects as our observational unit is measured only at the county level.

control based on a matched sample of large domestic firms. All of these controls are interacted with year fixed effects to allow flexibly changing impacts. The sectoral composition controls are the share of workers in nondurable manufacturing, the share of workers in durable manufacturing, the share of workers in construction, the share of workers in agriculture, the share of workers in wholesale trade. The population and demographic controls are log population, log population density, the share of workers with less than a high school education, the share of workers with a college degree or more schooling, the white population share, and the Black population share. The trade, technology, and tax controls are exposure to the domestic domestic production activities deduction, exposure to the extraterritorial income exclusion, exposure to federal bonus depreciation, exposure to the China shock, the share of workers engaged in routine tasks, exposure to NAFTA, and exposure to IRC Section 936. The domestic firm control measures the share of workers in a county employed by other large publicly traded firms that have no direct exposure to CTB. Appendix A describes the measurement and sources of each of the first three categories of controls. Appendix C describes the matched domestic control variable.

To construct inverse probability weights à la Abadie (2005), we start by running a cross-sectional logistic regression for 1996 of CTB Treatment on cross-sectional control variables (recall, the estimates from this regression are the blue bars shown in Panel (B) of Figure 5). The regression estimates give each observational unit a treatment likelihood, $\hat{l}_c \in (0, 1)$. For units that are treated, we create a weight equal to $1/\hat{l}_c$. This increases the weight given to units that were unlikely to be treated. Analogously, we weight control counties by $1/(1-\hat{l}_c)$, which increases the weight given to control that were likely to be treated based on observables. These weights make the distribution of treated counties look more like the distribution of control counties based on the control variables we describe above. Panels (A) and (C) of Appendix Figure E1 present distributions of treatment likelihoods separately for treated and control counties prior to, and after, inverse probability weighting. After weighting, the distribution of treatment likelihoods for the treated counties closely matches the distribution of treatment likelihoods for control counties. We describe this weighting procedure more fully in Appendix E.

We present the estimates of β_y from Equation (3) in Figure 7, Panel (A). These estimates come from our fully saturated model that includes all controls in the inverse probability weighting scheme as well as directly in the regression interacted with year fixed effects. The figure shows that prior to CTB implementation, employment in treated counties trended similarly to employment in untreated counties. After CTB implementation in 1997, employment in highly exposed counties steadily declines through 2003. As shown by Altshuler et al. (2023), the fraction of total foreign profits earned by MNCs using CTB increased from zero to nearly 100% over these 7 years. The timing of the labor market response we document is similar to other rapid labor market responses to corporate tax shocks (Suárez Serrato, 2018; Garrett et al., 2020; Siegloch et al., 2022). The combination of no differential trends in employment during the pre-period and an immediate and consistent decline in employment after policy implementation provides strong evidence that CTB had a large negative impact on domestic local labor market outcomes.

Panel (B) of Figure 7 shows that this pattern is robust to the sets of control variables we use to construct inverse probability weights and that we directly include in the dynamic DD regression. The figure presents three dyanmic DD plots. The first uses just the sectoral composition controls. The second adds the the population and demographic controls. The third adds the trade, technology, and tax controls. This third regression almost matches the fully saturated model presented in Panel (A) with the exception of the control for exposure to the sample of matched domestic firms. All specifications show both flat pre-period trends and large decreases in employment in the counties most exposed to the policy beginning after CTB implementation.

To better understand the magnitudes of these domestic employment effects, we run the following regression that pools our β_y estimates,

$$\begin{aligned}\Delta y_{jct} = \alpha &+ \beta_1 \mathbf{CTB\ Treatment}_c \times \mathbf{1997\text{–}2002}_t \\ &+ \beta_2 \mathbf{CTB\ Treatment}_c \times \mathbf{Post2002}_t + \mu_{st} + \nu_{jt} + \mathbf{X'}_c \boldsymbol{\gamma}_t + \varepsilon_{cjt}, \end{aligned} \tag{4}$$

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 the years 1997–2002 and in 2003–2006, respectively. Here, β_1 is the percentage point change in employment in the most exposed counties relative to the less exposed counties in years 1997–2002 relative to the pre-period. β_2 represents the same effect in the years 2003–2006.[27]

Table 1 presents our pooled DD estimates as we progressively expand the sets of controls used in the weighting and in the regression. Specification (1) includes industry-by-year and state-by-year fixed effects, but does not use inverse probability weights or include other controls. Specifications (2), (3), and (4) correspond to the dynamic DD specifications presented in Figure

[27] In these pooled DD regressions, as with the variables of interest, we interact the controls with 1997–2002 and Post2002 indicators as opposed to individual year indicators.

7 Panel (B). Specification (5) corresponds to the fully saturated model presented in Panel (A) of the same figure. Across all specifications, CTB Treatment decreases employment by between 1.01% and 1.47% in the 1997–2002 period and by between 2.76% and 3.35% in the 2003–2006 period. The estimated effects are similar in both periods across all five specifications. Notably, we even find very similar results in Specification (1) when we do not use inverse probability weights or more granular fixed effects, which shows that our estimates do not depend on these methods.

4.1 Job Loss Estimates

We interpret the Specification (5) result using a simple relative comparison. We calculate the employment decline in treated counties relative to control counties assuming that treated counties experienced a 3.04% loss in employment due to the policy and that untreated counties experienced no disemployment effects. The QCEW data show that 33.9 million jobs were located in treated counties in 1996. Using this base, our simple relative job loss calculation suggests CTB resulted in 1.03 million lost jobs during the 10-year period following policy implementation. The last two rows of Table 1 show similar job loss calculations for each specifications as well as 95% confidence intervals.

These job loss estimates may be more or less conservative than the actual aggregate CTB effect if control counties lost or gained jobs due to the policy. There are two noteworthy mechanisms by which CTB could have impacted control counties. First, control units were also exposed to the policy because most control units had some MNC employment. Therefore, exposure to CTB may have resulted in job losses even for our control group. To the extent this is the case, our simple relative job loss calculation presented above understates the aggregate effect of the policy. Accounting for this mechanism increases our job loss estimates by over 40%.[28] On the other hand, workers that lose jobs in treated counties may migrate to control counties and find jobs there. In Appendix H, we show that net out-migration during the sample period is unchanged in treated counties relative to control counties. However, we do find positive point estimates in some

[28]To account for this mechanism in our calculation, we perform a linear extrapolation of our discrete treatment estimates. Our discrete CTB Treatment definition represents an 8 percentage point increase in MNC employment share, so a 1 percentage point increase in this share results in a 0.38 (=3.04/8) percent decrease in employment in the 10 years after CTB implementation. Multiplying this linearized estimate by average CTB Exposure (4.4%) and by the QCEW employment in 1996 (86.2 million) suggests CTB resulted in 1.44 million job losses by from 1996 to 2003–2006.

years. Focusing only on these increases in out-migration from treated counties, we calculate that the domestic employment effects we estimate could be up to 14% smaller. Accounting for both mechanisms at the same time suggests the simple relative job loss calculation we started with understates job losses by about 20%. Thus, ultimately, we find the simple estimate we began with is a reasonable, albeit slightly conservative, approximation of the aggregate job losses induced by the policy.

One of the strengths of our local labor markets approach is that our estimates combine both a direct effect of the policy on employment in MNCs as well as the indirect effects of these job losses on other local workers. Moretti (2010) finds that the loss of a single skilled job in the tradeable sector results in an additional 1.6 to 2.5 job losses in the same local labor market. Assuming a 2.5 job multiplier suggests our simple 1.03 million job loss estimate is composed of 294,000 jobs lost at MNCs directly affected by CTB and another 736,000 indirect job losses due to the policy. For context, during the period 1997-2006, foreign employment of US MNCs grew by approximately 3.2 million jobs, 1.3 million of which were in the European Union (BEA, 2023). Therefore, even if each of the 294,000 direct job losses were relocated abroad, this would only represent a small fraction of the increase in the total foreign employment of US MNCs. In Section 4.3, we further explore the connections between our employment results and offshoring.

4.2 Effects on Other Local Labor Market Outcomes

Figure 8 displays dynamic DD estimates describing the effect of CTB on other domestic labor market outcomes. Figure 8 Panel (A) shows the effect of CTB on total labor earnings. As with employment, the figure shows both no differential pre-trends prior to 1996 and large decreases in relative earnings after CTB implementation. The corresponding pooled DD estimate presented in Table 2 shows that total earnings decreased by 4.09% for treated units relative to control units between the pre-period and the 2003–2006 period. Assuming the policy had no effect on earnings in control counties, we estimate that CTB decreased total earnings by approximately $42.5 billion per year in 2003–2006.[29] This represents 0.3% of 2006 US GDP.

Figure 8 Panel (B) presents the effect of CTB on wages, which we define as total labor earnings divided by total employment. In contrast to our other results, we estimate that the CTB has a precise null effect on wages. A potential explanation for this null effect is that CTB impacted

[29] Treated counties reported total earnings of $1.04 trillion in 1996. $42.5 billion is 4.09% of this base.

the composition of the workforce. If CTB led to the elimination of primarily low-wage jobs this would simultaneously increase the observed average wages of those who were still employed while also depressing wages for the types of workers whose jobs were eliminated. These opposing effects could average out, resulting in the precise null we find.

In Panel (C), we estimate the effect of CTB on employment-to-population ratios, which we construct as QCEW county-level employment scaled by county-level, working-age population estimates from SEER. The figure shows that prior to 1996, ΔEPop was stable in treated counties relative to control counties. After CTB implementation, the EPop ratio begins to decline and ultimately decreases by more than 2% ten years after CTB implementation. Our pooled DD estimates show that CTB decreased EPop by 2.14% in 2003–2006 relative to the pre-period. This is smaller than our employment results, motivating the analysis in Appendix H, which directly estimates the effect of CTB on out-migration and quantifies any bias migration might have on our employment estimates. As we mentioned above, we find that the impact of CTB on out-migration from treated counties could explain at most 14% of our employment results.

To summarize, we find that CTB has large negative effects on both employment and total earnings. On average, workers in the most exposed counties experienced three percentage points lower relative growth in employment and four percentage points lower relative growth in total earnings. Our estimates imply that, by 2003, one million fewer jobs were created in the counties that had the highest exposure to CTB relative to other counties. In the following section, we verify that these results are attributable to CTB rather than some other coincident, correlated shock.

4.3 Attributing Estimates to CTB

The results we have presented thus far show counties with more MNC exposure in 1996 experienced significantly worse labor market outcomes than other counties in the ten years following CTB implementation. In this section, we present five pieces of evidence suggesting the effects we estimate are driven by CTB and not some other shock to MNCs that occurred in 1997.

Effective tax rates for US MNCs decline sharply. First, the change in worldwide ETRs displayed in Panel A of Figure 4 show that worldwide effective tax rates for MNCs decreased by approximately 8 percentage points relative to our matched sample of large domestic firms after CTB implementation. This result is consistent with the magnitudes estimated in Blouin and Krull (2019) using a different approach and suggests that the local labor market effects we document are due to a 1997 change that also affected worldwide effective tax rates.

Effect of CTB is driven by exposure to MNCs with the most to gain from CTB. MNCs with higher foreign ETRs prior to CTB implementation stood to gain the most from the income shifting opportunities afforded by CTB. Here, we show that such firms decreased the share of their sales going to the domestic market and that the impact on US workers is concentrated in the markets where these firms operate.

To perform these tests, we begin by splitting our matched NETS-Compustat sample of MNCs by whether they had foreign ETRs that were, on average, above or below 35%—the US statutory rate—in years 1992–1997. We consider firms with foreign ETRs above 35% to be high foreign ETR MNCs. We use Compustat Geographic Segments data to calculate Domestic Sales Share (domestic revenue as a share of total worldwide revenue) at the firm-level for our MNC sample. Then, using a dynamic DD model, we compare the Domestic Sales Share of high and low foreign ETR MNCs and display the estimates in Panel B of Figure 4. Upon CTB implementation in 1997, we see the Domestic Sales Share of high foreign ETR firms drop, indicating that these firms shifted business activity away from the US relative to the low foreign ETR firms.

We now turn to our local labor market design and test for treatment effect heterogeneity on this margin. We identify the top quartile of exposed counties to high and low foreign ETR MNCs, respectively, and separately estimate effects of exposure to each group following our baseline procedures (identical controls and IPW approach). To make the magnitudes of all of our estimates comparable, in this and all other heterogeneity tests, we scale the treatment to always represent an 8 percentage point increase in CTB Exposure, in line with our baseline CTB Treatment definition.

Panel (A), Figure 9 presents the employment effects of exposure to MNCs with high and low foreign ETRs.[30] We see much larger negative effects on employment for the counties exposed to the MNCs that stood to gain the most from the CTB provision. By 2006, county-industries exposed to high levels of high-tax MNCs lost nearly 4% employment. In contrast, exposure to MNCs who already had low foreign ETRs prior to CTB implementation and could gain less from the tax policy experienced smaller negative effects. The direction of the heterogeneity suggests our main estimates are due to policies that disproportionately affect MNCs with high foreign

[30] Pooled DD coefficients for all heterogeneity tests are presented in Table 3.

ETRs prior to 1997, those that stood to gain the most from CTB and that shifted activity out of the domestic market.

Effects of CTB are not primarily driven by statutory tax cuts. Based on the first two tests presented in this section, we surmise that the local labor market effects we observe are due to a shock to MNC tax policy beginning in 1997 that had larger effects on MNCs with higher foreign ETRs prior to 1997. While CTB is the most likely candidate, another possibility is that the results we observe could be due to the "race-to-the-bottom" in corporate tax rates among OECD countries with historically high tax rates, which was occurring during both our pre- and post-treatment periods.

There are three reasons to believe that CTB, and not background changes in corporate tax rates, is the primary driver of our results. First, the decline in global corporate tax rates began in the 1980s (Slemrod, 2004), but we find stable pretrends in all of our outcomes between 1992 and 1996, suggesting that such a global phenomenon was not impacting our estimates before 1997. Second, tax cuts in high-tax countries are largely inframarginal to firm incentives if profits are already being shifted out of high-tax countries. For example, if a US MNC is already shifting profits out of a high-tax country like Germany to a tax haven like Ireland using a disregarded entity, then Germany cutting their corporate income tax rate from 52% to 39% in 2001 has little impact on that firm's incentives. Such a rate change, while large on paper, has almost no impact on the taxes such a firm would owe to Germany. This notion is consistent with the finding in Dowd et al. (2017) that the elasticity of reported profits with respect to the net-of-tax rate is much smaller in high tax countries.

Third, we continue to find similar impacts when we restrict local exposure to firms that are not materially exposed to countries that did have large cuts to their corporate tax rates. Auerbach (2018) shows that Germany, Italy, and Japan had notable tax cuts between 1996 and 2001. To directly test whether these tax cuts are the driving force behind our results, we drop firms with exposure to those countries as well as other smaller countries that cut their corporate tax rates by at least 10 percentage points.[31] To create this subsample of MNCs that did not experience a large statutory tax cut, we use Exhibit 21a data from Dyreng and Lindsey (2009), which describe the location of foreign affiliates. Figure 9 Panel (B) compares our baseline dynamic DD estimates

[31] In addition to Germany, Italy, and Japan, we also identify Ireland, Poland, Slovakia, Czech Republic, Turkey, Portugal, and Iceland as countries experiencing large statutory tax cuts.

to estimates based on exposure to this subsample of MNCs that did not directly experience large corporate tax cuts and were less likely to benefit from the OECD race-to-the-bottom. We find very similar effects across both series, indicating that our baseline results are not primarily driven by the OECD race-to-the-bottom in corporate tax rates.

Effects of CTB are not driven by offshoring. As a fourth test, we explore whether our results could be due to determinants of offshoring unrelated to CTB. During the 1990s, technologies were introduced that decreased the costs of offshoring jobs for US MNCs. To test whether our results are driven by the introduction of these technologies rather than the CTB provision itself, we explore heterogeneity in our CTB Treatment effect across counties with different levels of "Offshorability" from Autor and Dorn (2013). Their measure is designed to capture the share of jobs that do not require "either direct interpersonal interaction or proximity to a specific work location" and therefore are more likely to be offshored (Autor and Dorn, 2013, p. 1584). Appendix Figure G2 shows the correlation between our measure of CTB Exposure and Offshorability. The two measures are positively correlated; a one standard deviation increase in Offshorability is correlated with a 1 percentage point increase in CTB Exposure. This positive correlation reinforces how important it is to attribute our results to CTB rather than aggregate trends in offshoring. We do this by creating an indicator equal to 1 for counties with above median Offshorability and including interactions with this indicator to measure CTB Treatment effects separately for each group of counties. The results presented in Figure 9, Panel (C) show that employment losses were nearly identical in counties with high- and low-offshorability. The similarity of the two dynamic DD plots suggests that the effects we estimate are not driven by contemporaneous patterns in offshoring, but are instead attributable to CTB. We note that while outsourcing does not drive our results, to the extent that the job losses we document are moved abroad, CTB may be a contributing factor to the aggregate offshoring patterns during our treatment period.

The timing of CTB elections and labor impacts coincide. As a fifth and final test, we directly attribute our labor market results to the CTB provision by analytically comparing the timing of the effects we estimate to the timing of disregarded entity elections, which were the principal mechanism by which firms used CTB to shift income from high- to low-tax foreign jurisdictions. To compare the employment losses we present in Figure 7 Panel (A) to the aggregate disregarded entity elections patterns presented in Figure 2 Panel (A), we scale CTB Treatment in year t by the cumulative number of disregarded entity elections (in 1,000s) made in years 1997 through year t. We then re-estimate Equation (3) using this scaled treatment. The β_y coefficients now represent the percentage point change in local employment per 1,000 aggregate disregarded entity elections.

Panel (D) of Figure 8 presents these new scaled β_y estimates. The graph shows that each 1,000 disregarded entity elections resulted in, on average, a -0.22 percentage point decrease in local employment in the most exposed counties relative to other counties. The stability of these estimates throughout the treatment period shows that the timing of disregarded entity elections closely matches the timing of the employment effects we estimate; as the number of disregarded entity elections accelerates during the 2000–2003 period, so do our employment effects. The coincident timing of the employment effects and disregarded entity elections provides strong evidence that the domestic labor market deterioration that we document is a direct result of the profit shifting opportunities afforded by the CTB provision.

Together, the five tests we present in this section suggest that the local labor market effects we document (1) coincide with large decreases in worldwide tax rates for MNCs; (2) are concentrated among counties exposed to MNCs that stood to gain the most from CTB; (3) are not primarily driven by the OECD tax race-to-the-bottom; (4) are not due to technological determinants of offshoring; and (5) closely match the time patterns of disregarded entity elections, the vehicles by which MNCs took advantage of the tax planning strategies afforded by CTB. Based on these five pieces of evidence, we conclude that the local labor market effects we document are directly attributable to the implementation of CTB in 1997.

4.4 Heterogeneity by Ability to Substitute Production Abroad

Thus far, we have shown that, on average, exposure to CTB leads to significant domestic employment and earnings declines. To the extent that lower taxes on foreign earnings due to CTB decrease production costs globally, these documented declines capture the net impact of both a global scale effect and a substitution effect between foreign and domestic activities. The net negative finding therefore implies that the negative substitution effect outweighs the positive scale effect and that US MNCs shifted production abroad in response to the provision.

In support of this implication, in Appendix F.2, we show that total revenues, total assets, and total pretax income from foreign operations (PIFO) all grew substantially for US MNCs during our entire sample period. These simple trends combined with the declines in domestic employment resulting from CTB imply US MNCs shifted production abroad after 1996. Panel (D) of Appendix Figure F1 more directly addresses this reallocation, showing that total PIFO per dollar of total revenue also increased steadily from 2% in 1994 to over 7% by 2011 during our sample period. These trends complement the better identified results presented in Panel B of Figure 4, which shows the the share of total revenue derived from domestic operations dropped significantly after CTB implementation for MNCs that stood to gain the most from the provision relative to other MNCs.

While our findings suggest that the firms substitute production abroad in response to CTB on average, as we discussed in Section 2.1, some types of MNCs may be less willing or able to substitute in this way. In particular, R&D intensive firms may be unable to move their R&D-related activities abroad due to agglomeration forces in the US knowledge economy.[32] These same firms may also be unwilling to move their production of IP abroad because US R&D tax credits are highly valuable due to the high tax rates against which they are credited. Thus, while we estimate that exposure to MNCs had large negative effects on domestic labor markets on average, this may not be case for markets exposed to R&D intensive firms.

To explore this hypothesis, we focus on heterogeneity by firm-level R&D Intensity.[33] We split our sample of MNCs by median R&D Intensity and then re-estimate the effects of CTB Treatment separately for each group. We present the results of this exercise in Panel (D) of Figure 9. The difference between the two plots is striking. Exposure to low R&D Intensity firms continues to have large, negative effects on local employment. In contrast, exposure to the sample of high R&D Intensity MNCs results in no local job losses. These results are consistent with the hypothesis that R&D intensive firms are less likely to substitute production abroad in response to CTB.[34] These results are also consistent with Albertus (2020) and Altshuler et al. (2023), who show that the MNCs with most to benefit from CTB increased domestic R&D

[32] Worldwide R&D activities are concentrated in the US (and even within the US) and this concentration makes R&D activities more productive (Moretti, 2021).

[33] We measure R&D Intensity as R&D expenditure scaled by total revenue. To account for the strong U-shaped correlation between firm size and R&D activity, we residualize our measure using ten asset size bins. Thus, our measure captures R&D intensity relative to other similarly sized firms.

[34] That the domestic labor market effects are concentrated among the firms that are more likely to be able or willing to substitute their production abroad further reinforces that the domestic labor market effects we find are due to CTB and not some other change in economic conditions.

activity in response to the provision.

4.5 Summarizing CTB Results

Overall, we find counties heavily exposed to CTB saw large employment and earnings declines after the provision was adopted. We estimate tax planning opportunities enabled by CTB led to losses of approximately 1.0 million jobs and $42.5 billion in total labor earnings per year. Losses were driven by exposure to firms that stood to gain the most from the policy and were smaller in markets exposed to MNCs who were less able to substitute production abroad. Together, these results suggest that international tax policies designed to increase the competitiveness of US MNCs in the global economy may have done so at the expense of inducing offshoring by non-R&D intensive firms.

5 Domestic Labor Market Effects of the RH

Prior to CTB, passive earnings of foreign subsidiaries were taxed immediately at the US statutory corporate tax rate net of credits for foreign tax payments. CTB allowed firms to avoid US taxation on passive earnings until they were repatriated. As a result, CTB increased the tax penalty associated with repatriating funds. This penalty led to large increases in cash holdings abroad for US MNCs (Faulkender et al., 2019).[35]

To the extent that these "trapped" funds are an important source of financing for domestic business activity, the deleterious labor market effects we observe in response to CTB may be due to the repatriation tax penalty created by CTB rather than the lower foreign effective tax rates the policy generated. If this is the case, then lowering the repatriation penalty should have large, positive effects on domestic labor markets.

In this section, we test for this mechanism by measuring how repatriations made during the 2004 repatriation holiday affected domestic labor markets. We estimate the effects of the RH on domestic employment and earnings in much the same way we analyzed the local labor market

[35] Foley et al. (2007) find MNCs facing higher repatriation taxes hold higher levels of cash abroad. Based on surveys of corporate executives, Graham et al. (2010) conclude US repatriation taxes resulted in a "lockout" effect whereby foreign earnings could not be used to finance domestic business activity. Hanlon et al. (2015) find increased foreign cash holdings due to lock-out is associated with higher likelihood of foreign—but not domestic—acquisitions.

effects of CTB. We run dynamic DD regressions of the form

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

Outcomes are percentage point changes in county-industry local labor market outcomes relative to 2003. Relative to the CTB analysis, we make two changes to our approach. First, where possible, we update the cross-sectional controls so they are based on 2003 variation. Second, we now control for—and weight by—exposure to the matched sample of non-repatriating MNCs rather than the CTB matched sample of domestic firms. Appendix C details the construction of this matched MNC group.

Figure 10 presents dynamic DD estimates from fully saturated models describing the effect of REPAT Treatment on employment, earnings, wages, and employment-to-population ratios. Corresponding pooled DD estimates are presented in Table 4. Panel (A) of Figure 10 shows the effect of the repatriations on local employment.[36] As with our CTB analysis, during the pre-period we see no differential trends between treated and control units. However, in contrast to the CTB findings, repatriations have no effect on domestic employment either during the holiday (2004–2006) or after (2007–2012). Our 95% confidence intervals rule out employment responses larger than 0.45 percentage points in 2004–2006 and larger than 0.71 percentage points in the years 2007–2012. Panel (A) of Figure 11 shows that these employment effects are robust to alternative sets of control variables and inverse probability weighting schemes.

Figure 10 Panel (B) shows a similar, although slightly less precise, null effect of repatriations on total labor earnings. Panel (C) focuses on wages (total labor earnings per worker). The dynamic DD plot shows that differences in wages between counties with high and low repatriations are stable in the pre-period and remain stable until 2008. These differences then increase gradually from 2008 through 2012. The pooled DD estimate shows that wages in high repatriation counties increased by 0.4 percentage points during the years after the holiday.[37]

Panel (D) of Figure 10 shows no effect of repatriations on employment to population ratios. Together, the results presented in Figure 10 show that repatriations under the holiday had a

[36] Appendix Table J1 presents further robustness results.

[37] One possible reason why the RH may have caused a delayed effect on earnings per worker is that repatriations under the holiday decreased financial constraints, which allowed repatriating firms to keep wages stable during the 2008–2009 recession (Chodorow-Reich, 2014). However, that these effects materialize for the first time two years after the end of the repatriation holiday suggests that the repatriations themselves have no direct effect on domestic wages.

negligible effect on domestic labor markets.

5.1 Mechanisms Behind the Null Effect

These results raise the question, "How could a nearly $300 billion cash infusion *not* result in increases in domestic employment and earnings?" Both Dharmapala et al. (2011) and Blouin and Krull (2009) suggest a potential explanation. Each paper showed that a large proportion of repatriations made in 2005 were immediately paid out to shareholders rather than invested in domestic productive capacities as the AJCA intended.

We explore this possibility by examining longer-run effects of total repatriations on total payouts using a firm-level dynamic DD methodology. We run regressions of the form

$$\text{Payouts}_{it} = \alpha + \sum_{y=1999,\ y\neq 2003}^{2010} \beta_y \Big[\textbf{Repatriations}_i \times \mathbb{I}(t=y)\Big] + \mathbf{X'}_{it}\boldsymbol{\gamma} + \mu_i + \nu_{jt} + \varepsilon_{it} \qquad (6)$$

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 and **Repatriations** is the total amount repatriated for each repatriating firms and zero for the matched control sample. $\mathbf{X'}_{it}$ is a vector of control variables that match the Dharmapala et al. (2011) analysis.[38] μ_i and ν_{jt} are firm and industry-by-year fixed effects. Given this specification, β_y captures the total payouts made in a given year per dollar of repatriations. Adding up the coefficients after the holiday gives the total amount of payouts per dollar of repatriations.

Coefficient estimates from Equation (6) are presented in Figure 12. The fully saturated specification in Panel (A) includes firm and industry-by-year fixed effects as well as the controls in Dharmapala et al. (2011). Panel (B) presents estimates from alternative specifications. Across all specifications we find a similar pattern. In the pre-period there are no differential trends in payouts for firms with varying levels of total repatriations under the holiday. Beginning in 2004, payouts per dollar of total repatriations increase. Estimates are statistically significant from 2004 through 2008. Adding up the coefficients from 2004–2008 suggests that 66 cents per dollar repatriated were paid out to shareholders in the form of dividends or share repurchases before 2009. To the extent that payouts are not an effective form of domestic job creation, this finding could largely explain the null effects of the RH on domestic labor markets.

[38] 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.

Of course, payouts to shareholders may be reinvested to stimulate domestic labor markets. Our results presented in Figure 10 address this possibility. A large literature has documented "home bias" in investment (e.g. Coval and Moskowitz, 1999; Huberman, 2001). Home bias is the phenomenon that investors are more likely to make local investments, where local can be defined as specifically as Census tract. Executives and high-level managers are likely to be large shareholders in the MNCs for which they work. They are also more likely to live near a plant that we use to define RH Exposure. As a result, if payouts were reinvested in ways that created jobs and stimulated local labor markets, then our Figure 10 should show positive effects of REPAT Treatment on labor market outcomes because payouts are likely to be reinvested locally. Given we do not see positive effects, we conclude it is unlikely that payouts stimulated by repatriations were reinvested in ways that stimulated domestic labor markets.

Faulkender and Petersen (2012) hypothesize that perhaps for most repatriating firms, capital markets facilitated efficient levels domestic investment prior to the holiday, potentially leading to the null labor market effects and the large payout response we document. They suggest this may not be the case for financially constrained firms. We explore this possibility by estimating the local labor market effects of repatriations made by financially constrained firms. We follow Faulkender and Petersen (2012) and define MNCs as financially constrained if their operating cash flow was negative in at least two of the four years just prior to the holiday. Panel (B), Figure 11 shows the effects of exposure to REPAT Treatment defined separately for financially constrained and for financially unconstrained firms. Results for unconstrained firms match our baseline estimates; all estimates are close to zero and stable. For constrained firms, local labor market outcomes seem to be in a slight decline in the pre-period (perhaps due to the presence of a constrained MNC). Coefficient estimates continue to decline slightly in the post period suggesting repatriations, even by constrained firms, do not have a material impact on domestic labor markets.

Another possibility is that we observe null effects of repatriations because the holiday only provided repatriation tax benefits for a small subset of firms who had higher tax costs of repatriations prior to the holiday. To test whether this heterogeneity is driving our null result, we construct separate exposure measures based on 2003 levels of permanently reinvested earnings (PRE), a measure of cash held abroad.[39] The idea is that firms with higher tax costs of repa-

[39]We use PRE data courtesy of Faulkender and Petersen (2012) and split firms by above/below median 2003

triating funds were more likely to build up cash reserves abroad prior to the holiday. For firms with highrepatriation costs, these funds are more likely to be an important source of domestic financing. Figure 11 Panel (C) displays the effects of exposure to REPAT Treatment for MNCs with high and low levels of PRE. Here, exposure to repatriations made by both types of firms has no effect on domestic employment. We find exposure to neither repatriations from MNCs with high or low PRE leads to increases in domestic employment. Thus, even for MNCs who had higher tax costs of repatriations prior to the holiday and whose cash held was potentially more valuable for domestic financing, we find no effect. We continue to conclude repatriations made under the holiday did not stimulate domestic labor markets.

5.2 CTB-Induced International Substitution and Repatriations

Our CTB analysis presented in Section 4 suggests that R&D intensive firms were less willing or able to substitute production abroad. If these firms never relocated jobs abroad in response to CTB, perhaps they were the firms who were most likely to bring cash back home to support domestic operations or pay shareholders.

We perform three separate analyses to explore this hypothesis. First, in Appendix I, we show that even controlling for foreign growth prospects and cash holdings, more innovative MNCs are more likely to repatriate under the holiday.[40] More specifically, we show that MNCs that are more R&D intensive, that do more patenting, that hold more patents per dollar of assets, and even firms who are headquartered in Silicon Valley are more likely to repatriate under the holiday. This evidence suggests innovative firms whose domestic and foreign production were more likely to be complementary were more likely to repatriate funds under the holiday.

Next, to more directly relate repatriations decisions and CTB responses, in Panel (A) of Figure 13 we separately estimate the effect of exposure in 1996 to MNCs that eventually repatriated under the holiday and to those who did not. The results show that exposure to firms that eventually repatriated did not lead to employment losses. Exposure to non-repatriating MNCs continues to have large negative effects. These differing results make sense given we have shown exposure to R&D intensive firms did not lead to job losses and repatriating firms were more

PRE.

[40] Blouin and Krull (2009) find that repatriation responses to the holiday were driven by low growth prospects abroad. Faulkender and Petersen (2012) argue that repatriations were driven by large cash holdings abroad (PRE), which as we describe above may proxy for the repatriation tax relief provided by the holiday.

likely to be R&D intensive firms who could not easily substitute production abroad.

In our third test, we test for heterogeneity in exposure to repatriations by R&D intensive and non-intensive firms. Figure 13 Panel (B) presents the results. Regardless of R&D intensity, exposure to repatriations continues to have no impact on domestic employment. Thus, even for firms that were likely less able to relocate production abroad, repatriations under the holiday had no effect on domestic labor markets. In sum, our results suggest that while innovative firms did not move production abroad in response to CTB, and repatriations were more likely among innovative firms, the repatriations of innovative firms had no discernible impact on domestic job creation.

5.3 Summarizing RH Results

In this section, we showed that exposure to repatriations did not lead to domestic employment or earnings growth. We also find no positive effects of exposure to repatriations made (1) by financially constrained firms, (2) by firms most likely to benefit from the holiday, and (3) by innovative firms who were potentially less able to reallocate production abroad. Thus, decreasing the effective tax rate on repatriations did not improve domestic labor market outcomes. We conclude that foreign cash holdings are not an important source of financing for domestic business activity.

6 Implications for International Tax Systems

While international tax systems can be notoriously complicated, most systems can be reduced to a simple framework consisting of two fundamental design elements: (1) the ETR levied by the home country on foreign income when it is earned and (2) the ETR on cash flows between foreign affiliates and domestic parents.[41] In this paper, we analyzed the domestic labor market impacts of two international tax provisions—each representing a watershed moment in recent tax history and adjusting one of these fundamental design elements.

Because CTB allowed firms to more easily shift income from high-tax to low-tax foreign jurisdictions, it decreased the ETR on foreign income when it was earned.[42] The RH decreased the

[41] Altshuler and Grubert (2003) make a similar simplification when theorizing how optimal repatriation behavior responds to changes in international tax systems.

[42] CTB is an example of a policy that decreased foreign effective tax rates on income when it is earned. Another example of such a policy used by governments around the world is rules on transactions between controlled

ETR on cash flows from foreign affiliates to domestic parents—the other lever—by approximately 85%.[43] By analyzing the domestic labor market effects of each of the design elements in isolation, our results comprehensively address how and why the designs of international tax systems affect domestic workers.

First, relying on the CTB variation, we show that domestic labor markets exposed to lower foreign ETRs experience relative losses of 1 million jobs and $42 billion in earnings per year. This headline result shows that the average response of US MNCs to lower tax costs abroad is substitution out of the US market. US and foreign business activity are more substitutable than previously believed. At the same time, exposure to firms that were less able to substitute their production out of the US did not result in the same deleterious effects. Together, these results imply that firms that are able will shift production abroad in response to decreases in foreign ETRs. Further, most US MNCs were able to shift production abroad during the period we study. As a result, CTB led to net declines in employment and earnings.

Second, relying on the RH variation, we find no effects of changes in ETRs on repatriations from foreign affiliates to domestic parents on domestic labor markets. This null result holds when exposure is isolated to represent repatriations from financially constrained firms or firms that stood to gain the most from the policy (those with large stockpiles of cash abroad). These results imply that foreign cash holdings are not an important source of financing of domestic business activity and that changes in international tax systems that lower ETRs on such cash flows are unlikely to affect domestic employment and earnings.

These conclusions have immediate implications for the design of international tax systems if policy makers are focused on domestic workers. First, the lever that matters for domestic workers appears to be the tax paid on current foreign profits, and not the tax on repatriation of prior earnings. Second, if most firms exhibit a high degree of domestic and international substitutability, as was the case among US MNCs during our period of analysis, increasing the rate on current foreign earnings can increase employment and earnings at home. These lessons call into question the wisdom of recent international tax reforms, such as those recently implemented in the UK, Italy, Japan, and the US, which simultaneously decreased ETRs abroad

foreign corporations (CFCs). The proposed Pillars 1 and 2 of the OECD's Base Erosion and Profit Shifting (BEPS) project would also change the taxation of foreign income when it is earned.

[43] In principle, any sovereign nation that taxes the foreign income of its MNCs can manipulate the effective tax rate on repatriations through a holiday or by limiting deferral.

and eliminated repatriation taxes.

References

Abadie, Alberto, "Semiparametric difference-in-differences estimators," *The review of economic studies*, 2005, *72* (1), 1–19.

Albertus, James F, "Does Foreign Tax Arbitrage Promote Innovation?," *SSRN 2691064*, 2020.

Altshuler, Rosanne and Harry Grubert, "Repatriation taxes, repatriation strategies and multinational financial policy," *Journal of Public Economics*, 2003, *87* (1), 73–107.

Altshuler, Roseanne, Lysle Boller, and Juan Carlos Suárez Serrato, "Does Foreign Tax Planning by U.S. Multinationals Impact their Domestic Economic Activity?," Working Paper 2023.

Antras, Pol, Teresa C Fort, and Felix Tintelnot, "The margins of global sourcing: Theory and evidence from us firms," *American Economic Review*, 2017, *107* (9), 2514–2564.

Auerbach, Alan J, "Measuring the effects of corporate tax cuts," *Journal of Economic Perspectives*, 2018, *32* (4), 97–120.

Autor, David, David Dorn, Gordon H Hanson, Gary Pisano, and Pian Shu, "Foreign competition and domestic innovation: Evidence from US patents," *American Economic Review: Insights*, 2020, *2* (3), 357–374.

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

_ , _ , and Gordon H. Hanson, "The China Shock: Learning from Labor Market Adjustment to Large Changes in Trade," *Annual Review of Economics*, 2016, *8* (1), 205–240.

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

BEA, "Data on Activities of Multinational Enterprises," Web Site, Washington, D.C. 2023. U.S. Direct Investment Abroad, All Nonbank Foreign Affiliates (data up to 2008), Employment.

Blouin, Jennifer and Leslie A Robinson, "Double counting accounting: How much profit of multinational enterprises is really in tax havens?," *Available at SSRN*, 2019.

_ and Linda Krull, "Bringing it home: A study of the incentives surrounding the repatriation of foreign earnings under the American Jobs Creation Act of 2004," *Journal of Accounting Research*, 2009, *47* (4), 1027–1059.

Blouin, Jennifer L. and Linda K. Krull, "Does Tax Planning Affect Organizational Complexity: Evidence from Check-the-Box," 2019.

Blouin, Jennifer L, Linda K Krull, and Leslie A Robinson, "Is US multinational dividend repatriation policy influenced by reporting incentives?," *The Accounting Review*, 2012, *87* (5), 1463–1491.

Brennan, Thomas J, "Where the money really went: a new understanding of the AJCA tax holiday," *Northwestern Law & Econ Research Paper*, 2014, (13-35).

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro HC Sant'Anna, "Difference-in-differences with a continuous treatment," *arXiv preprint arXiv:2107.02637*, 2021.

Cameron, Colin A. and Douglas L. Miller, "A Practitioner's Guide to Cluster-Robust Inference," *Journal of Human Resources*, 2015, *50* (2), 317–372.

Chirinko, Robert S. and Daniel J. Wilson, "State Investment Tax Incentives: A Zero-Sum Game?," *Journal of Public Economics*, 2008, *92* (12), 2362–2384.

Chodorow-Reich, Gabriel, "The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008-9 Financial Crisis," *The Quarterly Journal of Economics*, 2014, *129* (1), 1–59.

Clausing, Kimberly A, "The Role of US Tax Policy in Offshoring," in "Brookings Trade Forum," Vol. 2005 Brookings Institution Press 2005, pp. 457–482.

Coval, Joshua D and Tobias J Moskowitz, "Home bias at home: Local equity preference in domestic portfolios," *The Journal of Finance*, 1999, *54* (6), 2045–2073.

Dean, Steven A, "Attractive complexity: Tax deregulation, the check-the-box election, and the future of tax simplification," *Hofstra L. Rev.*, 2005, *34*, 405.

Desai, Mihir A., C. Fritz Foley, and James R. Hines, "Domestic Effects of the Foreign Activities of US Multinationals," *American Economic Journal: Economic Policy*, February 2009, *1* (1), 181–203.

Dharmapala, Dhammika, C Fritz Foley, and Kristin J Forbes, "Watch what I do, not what I say: The unintended consequences of the Homeland Investment Act," *The Journal of Finance*, 2011, *66* (3), 753–787.

Dowd, Tim, Paul Landefeld, and Anne Moore, "Profit shifting of US multinationals," *Journal of Public Economics*, 2017, *148*, 1–13.

Dunbar, Amy E and Andrew Duxbury, "The Effect of 'Check the Box' on US Multinational Tax Rates," *Available at SSRN 2630738*, 2015.

Dyreng, Scott D. and Bradley P. Lindsey, "Using Financial Accounting Data to Examine the Effect of Foreign Operations Located in Tax Havens and Other Countries on U.S. Multinational Firms' Tax Rates," *Journal of Accounting Research*, 2009, *47* (5), 1283–1316.

Faulkender, Michael and Mitchell Petersen, "Investment and capital constraints: repatriations under the American Jobs Creation Act," *The Review of Financial Studies*, 2012, *25* (11), 3351–3388.

Faulkender, Michael W, Kristine W Hankins, and Mitchell A Petersen, "Understanding the rise in corporate cash: Precautionary savings or foreign taxes," *The Review of Financial Studies*, 2019, *32* (9), 3299–3334.

Field, Heather M, "Checking in on Check-the-Box," *Loy. LAL Rev.*, 2008, *42*, 451.

Firpo, Sergio, Nicole M Fortin, and Thomas Lemieux, "Occupational tasks and changes in the wage structure," 2011.

Foley, C Fritz, Jay C Hartzell, Sheridan Titman, and Garry Twite, "Why do firms hold so much cash? A tax-based explanation," *Journal of Financial Economics*, 2007, *86* (3), 579–607.

Freeman, Richard B and Robert Valletta, "The effects of public sector labor laws on labor market institutions and outcomes," in "When public sector workers unionize," University of Chicago Press, 1988, pp. 81–106.

Garrett, Daniel G, Eric Ohrn, and Juan Carlos Suárez Serrato, "Tax Policy and Local Labor Market Behavior," *American Economic Review: Insights*, 2020, *2* (1), 83–100.

Graham, John R, Michelle Hanlon, and Terry Shevlin, "Barriers to mobility: The lockout effect of US taxation of worldwide corporate profits," *National Tax Journal*, 2010, *63* (4), 1111–1144.

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

Hanlon, Michelle, Rebecca Lester, and Rodrigo Verdi, "The effect of repatriation tax costs on US multinational investment," *Journal of Financial Economics*, 2015, *116* (1), 179–196.

Harrison, Ann and Margaret McMillan, "Offshoring jobs? Multinationals and US manufacturing employment," *Review of Economics and Statistics*, 2011, *93* (3), 857–875.

Huberman, Gur, "Familiarity breeds investment," *The Review of Financial Studies*, 2001, *14* (3), 659–680.

Internal Revenue Service, "Simplification of Entity Classification Rules," Notice of proposed rulemaking and notice of public hearing 26 CFR Part 301, Federal Register 1996.

IRS, "SOI Tax Stats - Controlled Foreign Corporations," Web Site https://www.irs.gov/statistics/soi-tax-stats-controlled-foreign-corporations (Accessed: July 15, 2021), U.S. Internal Revenue Service 2021.

IRS, "U.S. Population Migration Data," 2023. Accessed 5/17/2023 from https://www.irs.gov/statistics/soi-tax-stats-migration-data.

Jiang, Xian, "Information and communication technology and firm geographic expansion," 2023.

Kadet, Jeffery M, "BEPS Primer: Past, Present, And Future, Part 1," *Tax Notes International, July*, 2020, *6.*

Kovak, Brian K., Lindsay Oldenski, and Nicholas Sly, "The Labor Market Effects of Offshoring by U.S. Multinational Firms," *The Review of Economics and Statistics*, 05 2021, *103* (2), 381–396.

Liu, Li, "Where Does Multinational Investment Go with Territorial Taxation? Evidence from the United Kingdom," *American Economic Journal: Economic Policy*, 2020, *12* (1), 325–58.

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

Monarch, Ryan, Jooyoun Park, and Jagadeesh Sivadasan, "Gains from offshoring? Evidence from US Microdata," *Evidence from US Microdata (2014-11-11). FRB International Finance Discussion Paper*, 2014, (1124).

Moretti, Enrico, "Local multipliers," *American Economic Review*, 2010, *100* (2), 373–377.

—, "The effect of high-tech clusters on the productivity of top inventors," *American Economic Review*, 2021, *111* (10), 3328–3375.

Muendler, Marc-Andreas and Sascha O Becker, "Margins of multinational labor substitution," *American Economic Review*, 2010, *100* (5), 1999–2030.

NCI, "U.S. County Population Data - 1969-2020," Web Site (Accessed: April 28, 2023), Surveillance, Epidemiology, and End Results Program, https://seer.cancer.gov/popdata/ 2022.

Ohrn, Eric, "The Effect of Corporate Taxation on Investment and Financial Policy: Evidence from the DPAD," *American Economic Journal: Economic Policy*, May 2018, *10* (2), 272–301.

—, "The Effect of Tax Incentives on U.S. Manufacturing: Evidence from State Accelerated Depreciation Policies," *Journal of Public Economics*, 2019, *180*, 104084.

Oler, Mitchell, Terry Shevlin, and Ryan Wilson, "Examining investor expectations concerning tax savings on the repatriations of foreign earnings under the American Jobs Creation Act of 2004," *Journal of the American Taxation Association*, 2007, *29* (2), 25–55.

Oosterhuis, Paul W, "The Evolution of US International Tax Policy-What Would Larry Say?," *TAX NOTES INTERNATIONAL*, 2006, *42* (13), 1119.

Ottaviano, Gianmarco I P, Giovanni Peri, and Greg C Wright, "Immigration, offshoring, and American jobs," *American Economic Review*, 2013, *103* (5), 1925–1959.

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

Redmiles, Melissa, "The One-Time Received Dividend Deductions," Statistics of Income Bulletin, Internal Revenue Service 2008.

Santos, Ramon Tomazela, "Territorial tax systems: motivations and key considerations for effective change," *Tax Notes International*, 2018, *89* (10), 925–935.

Shapiro, Matthew D and Joel Slemrod, "Consumer response to tax rebates," *American Economic Review*, 2003, *93* (1), 381–396.

Siegloch, Sebastian, Nils Wehrhöfer, Tobias Etzel et al., *Spillover, Efficiency and Equity Effects of Regional Firm Subsidies*, ECONtribute, 2022.

Slaughter, Matthew J, "Production transfer within multinational enterprises and American wages," *Journal of international Economics*, 2000, *50* (2), 449–472.

__ , "International trade and labor–demand elasticities," *Journal of international Economics*, 2001, *54* (1), 27–56.

Slemrod, Joel, "Are corporate tax rates, or countries, converging?," *Journal of Public Economics*, 2004, *88* (6), 1169–1186.

Smolyansky, Michael, Gustavo A Suarez, Alexandra M Tabova et al., "US Corporations' Repatriation of Offshore Profits: Evidence from 2018," Technical Report, Board of Governors of the Federal Reserve System (US) 2019.

Standard & Poor's, "Compustat Fundamentals (Annual Data)," 1980-2014.

Suárez Serrato, Juan Carlos, "Unintended Consequences of Eliminating Tax Havens," Working Paper 24850, National Bureau of Economic Research July 2018.

__ **and Owen Zidar**, "The structure of state corporate taxation and its impact on state tax revenues and economic activity," *Journal of Public Economics*, 2018, *167*, 158 – 176.

U.S. Census Bureau, "United States Decennial Census," 1990. Downloaded from Social Explorer, May 2017.

__ , "USA Counties: 2011," October 2011. Accessed 4/6/2023 from https://www.census.gov/library/publications/2011/compendia/usa-counties-2011.html#LND.

U.S. House Ways and Means Committee, "Chairman Brady Introduces the Tax Cuts and Jobs Act," November 2017. Accessed 6/16/2023 from https://waysandmeans.house.gov/chairman-brady-introduces-tax-cuts-jobs-act/.

U.S. Treasury Department, "The Deferral of Income Earned Through U.S. Controlled Foreign Corporations," December 2000. Accessed 2/16/2024 from https://home.treasury.gov/system/files/131/Report-SubpartF-2000.pdf.

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

Wasi, Nada and Aaron Flaaen, "Record linkage using Stata: Preprocessing, linking, and reviewing utilities," *The Stata Journal*, 2015, *15* (3), 672–697.

Williams, Braden M, "Multinational tax incentives and offshored US jobs," *The Accounting Review*, 2018, *93* (5), 293–324.

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

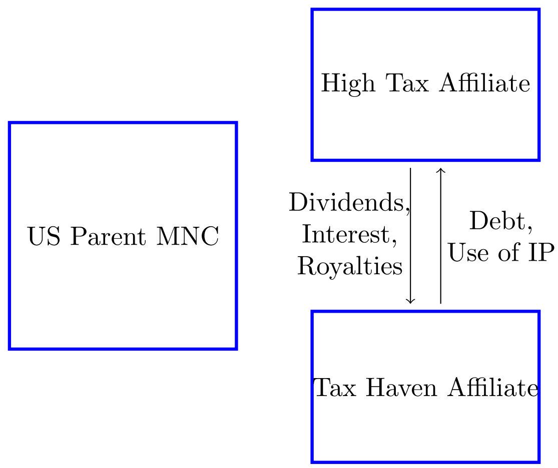
Wright, Greg C, "Revisiting the employment impact of offshoring," *European Economic Review*, 2014, *66*, 63–83.

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

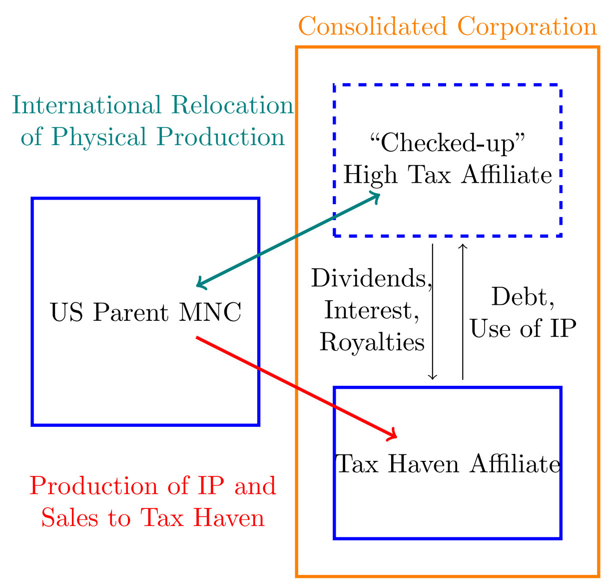
7 Figures and Tables

Figure 1: MNC Responses to Check-the-Box

(A) Standard MNC Organizational Structure prior to CTB



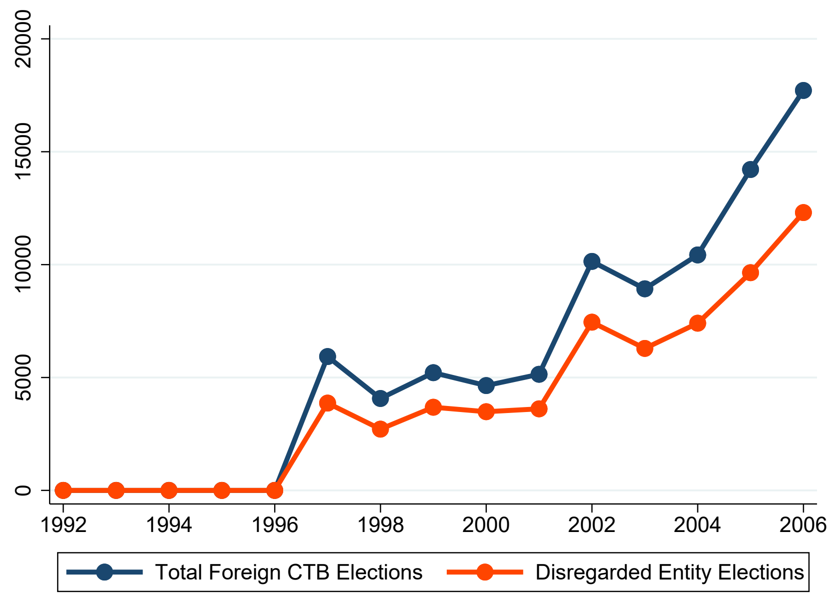
(B) Effects of CTB on MNC Organizational Structure and Production Decisions



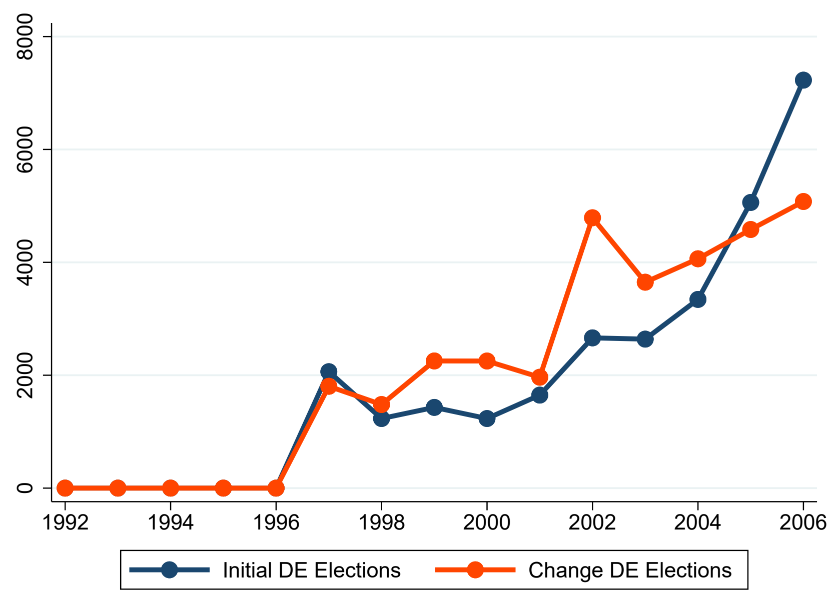
Notes: This figure shows a stylized version of MNC corporate structure before and after CTB. Panel (A) shows a standard MNC organizational structure prior to CTB. Panel (B) shows how CTB affects the MNC's organization structure and production decisions. CTB allows the High Tax Affiliate to "Check-the-box" to be treated as a disregarded entity. As a result, the US tax authority now only recognizes the Consolidated Foreign Corporation for tax purposes. As a result, passive income flows between the High Tax Affiliate are not immediately subject to Subpart F rules. Income shifting facilitated by CTB results in lower effective tax rates in the High Tax Affiliate. This could impact the MNC's international allocation of physical production activities as well as the production of intellectual property. The diagram is described in detail in Section 2.1.

Figure 2: Trends in Check-the-Box Elections

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



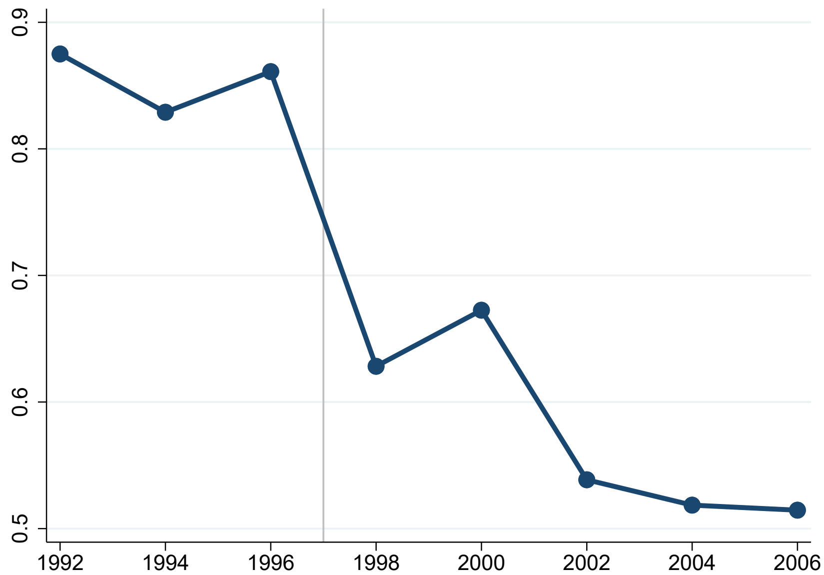
(B) Initial DE Elections and Changes to DE Status by Foreign Affiliates



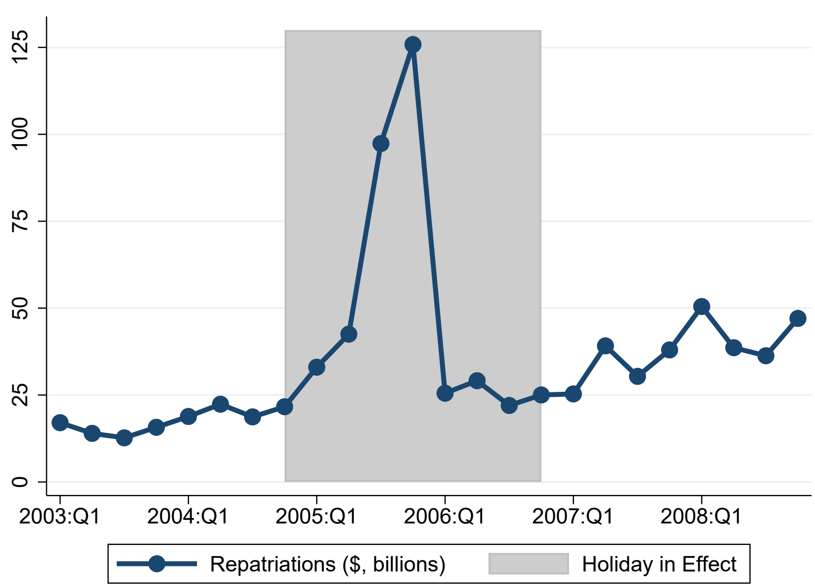
Notes: Figure 2 displays data from from Field (2008) that describes trends in check-the-box elections based on Form 8832s processed by IRS Statistics of Income division. Panel (A) shows (1) total CTB elections made by foreign affiliates and (2) total disregarded entity status elections made by foreign 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: Aggregate Trends in Subpart F Income and Repatriations

(A) Subpart F Income Per Total Assets



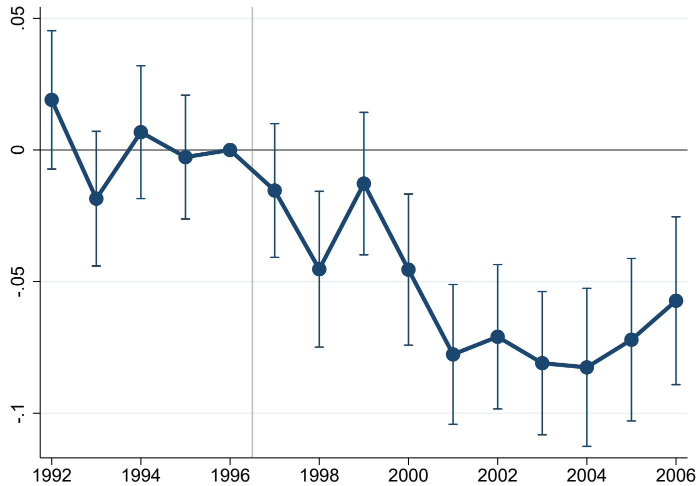
(B) Repatriations of Foreign Income by US MNCs



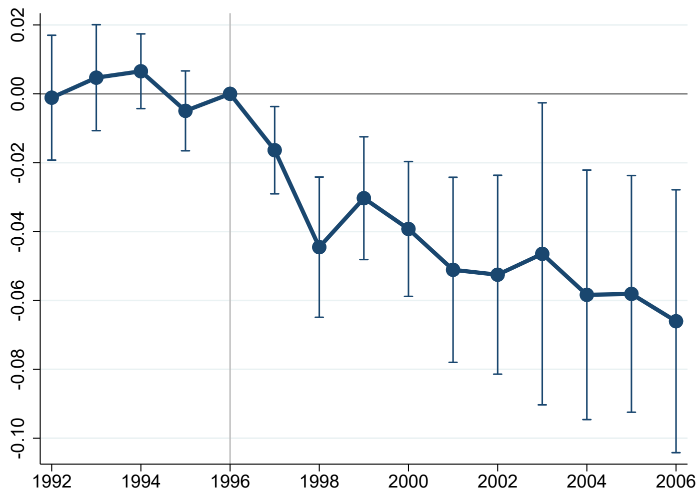
Notes: Figure 3A displays aggregate Subpart F income per total assets reported by US controlled foreign corporations (CFCs) in even years 1992–2006 based on IRS SOI data (IRS, 2021). During the years 1992–2002, the sample is the largest 7,500 CFCs. In years 2004–2006, the sample includes all CFCs. Figure 3B 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).

Figure 4: Effect of Check-the-Box on Firm Tax Rates and Foreign Activity

(A) Worldwide Effective Tax Rates



(B) Domestic Sales Share for High ETR MNCs



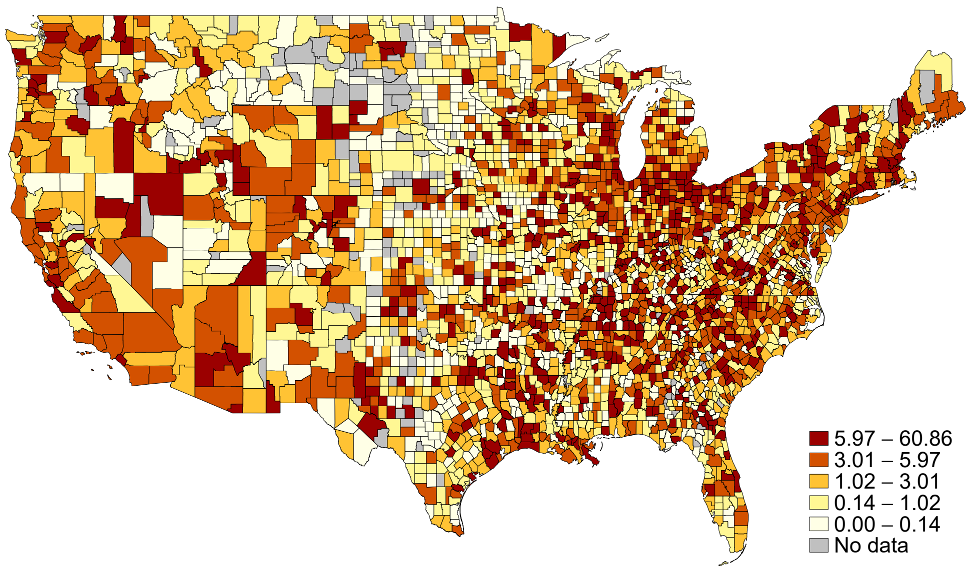
Notes: This figure shows how the effective tax rates and foreign activities of US MNCs are changing around CTB. Panel A of Figure 4 shows differences in effective tax rates between MNCs and matched domestic firms relative to the difference in 1996. Panel A displays ω coefficients and 95% confidence intervals from regressions of the from

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

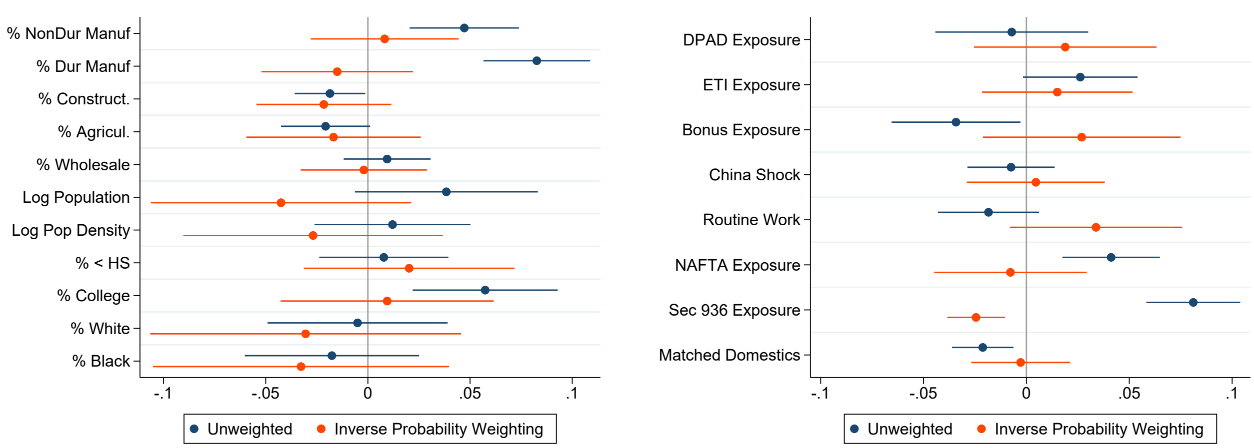
Panel (A) represents estimates from a fully saturated model that includes firm, industry $\times$ year fixed effects and controls (as defined in Appendix D). Additional specifications are shown in Appendix F. Panel B presents dynamic DD estimates describing differences in Domestic Sales Shares between MNCs with high and low foreign ETRs relative to the difference in 1996. The regression includes firm and industry-by-year fixed effects as well as controls constructed as tercile bins of pre-period firm size (total assets), debt ratios, and ROA interacted with year fixed effects. Domestic Sales Share is calculated as the ratio of domestic sales to the sum of domestic and foreign sales.Standard errors are clustered at the firm level in both panels.

Figure 5: Local Exposure to Check-the-Box

(A) CTB Exposure



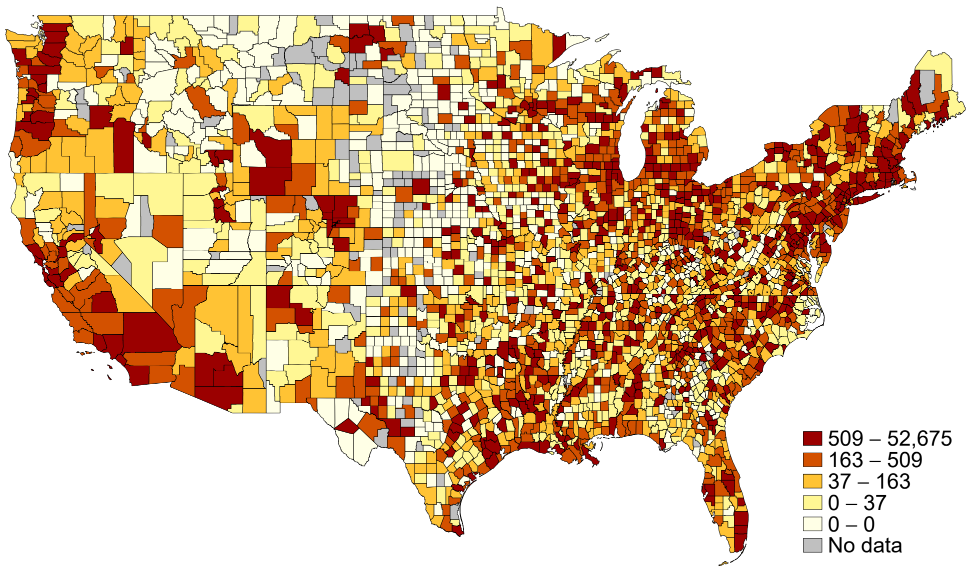
(B) Correlates of CTB Exposure



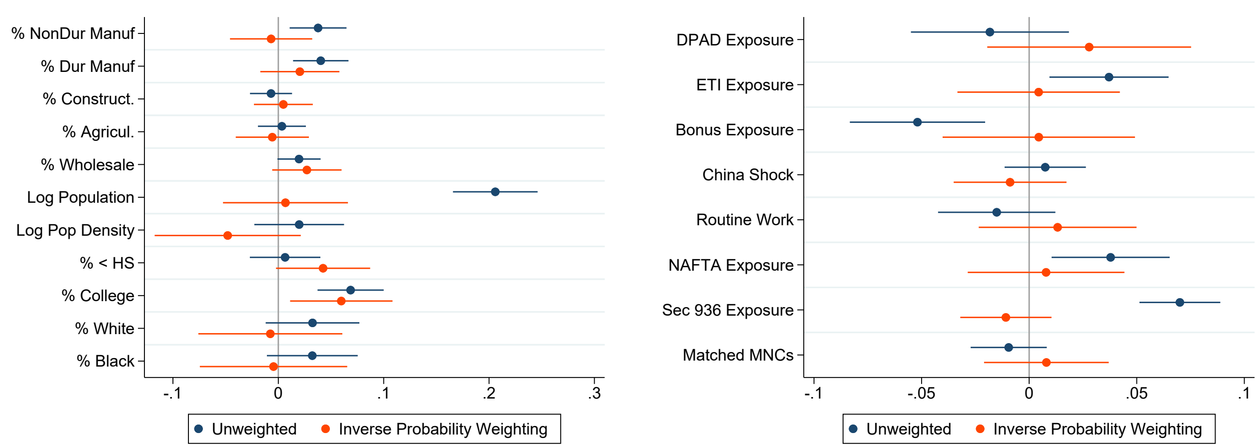
Notes: Panel (A) of Figure 5 displays county-level CTB Exposure. CTB Exposure is defined as the percentage of employees in a county working for MNCs in 1996. Both figures in Panel (B) displays coefficients from two logistic regressions of CTB Treatment on a number of county-level characteristics. The first regression is unweighted while the second regression uses inverse probability weights constructed using the results from the first regression. Construction of the inverse probability weights is fully described in Section 4 and Appendix E. All county-level characteristics are defined in Appendix A.

Figure 6: Local Exposure to the Repatriation Holiday

(A) REPAT Exposure



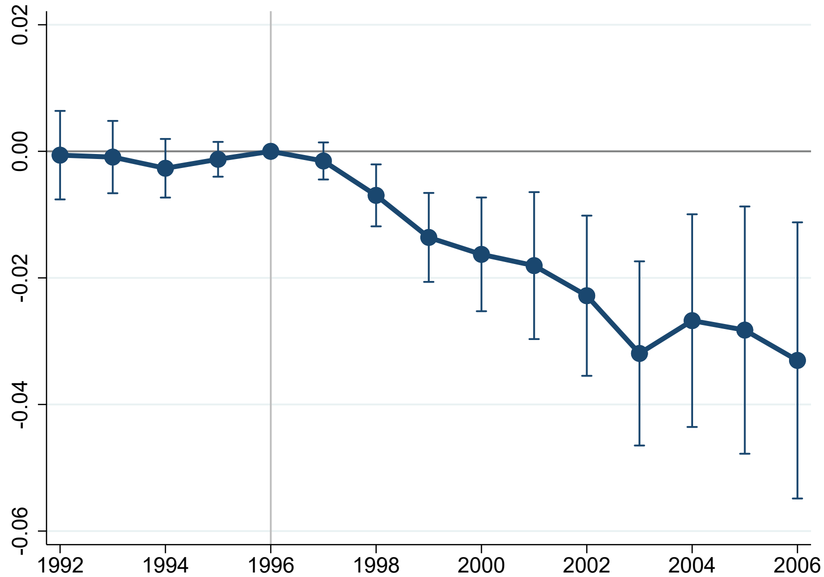
(B) Correlates of REPAT Exposure



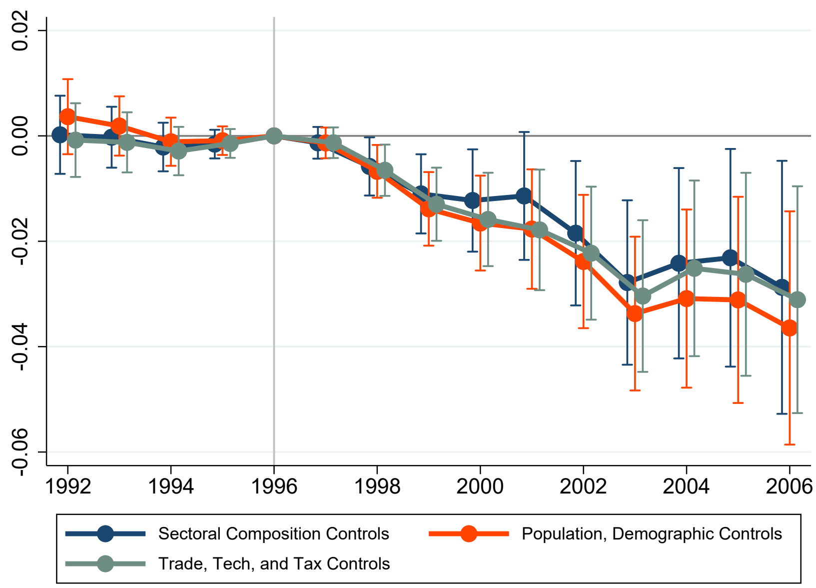
Notes: Panel (A) of Figure 6 displays county-level REPAT Exposure. REPAT Exposure is defined as the dollars of repatriations per worker in a county working in 2003. Both figures in Panel (B) displays coefficients from two logistic regressions of REPAT Treatment on a number of county-level characteristics. The first regression is unweighted while the second regression uses inverse probability weights constructed using the results from the first regression. Construction of the inverse probability weights is fully described in Section 4 and Appendix E. All county-level characteristics are defined in Appendix A.

Figure 7: Effect of Check-the-Box on Domestic Employment

(A) Fully Saturated Model

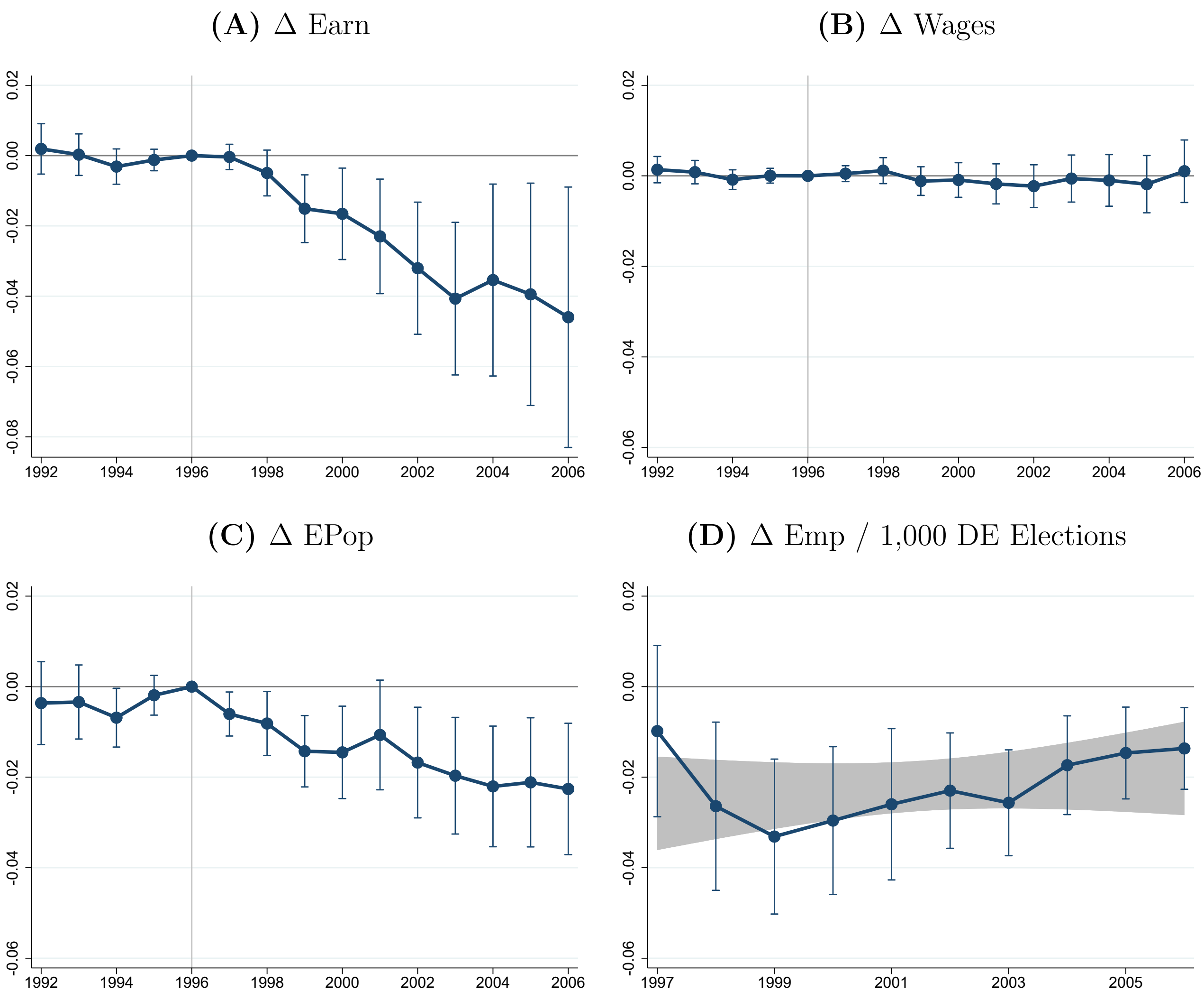


(B) Alternative IPW Weighting and Controls



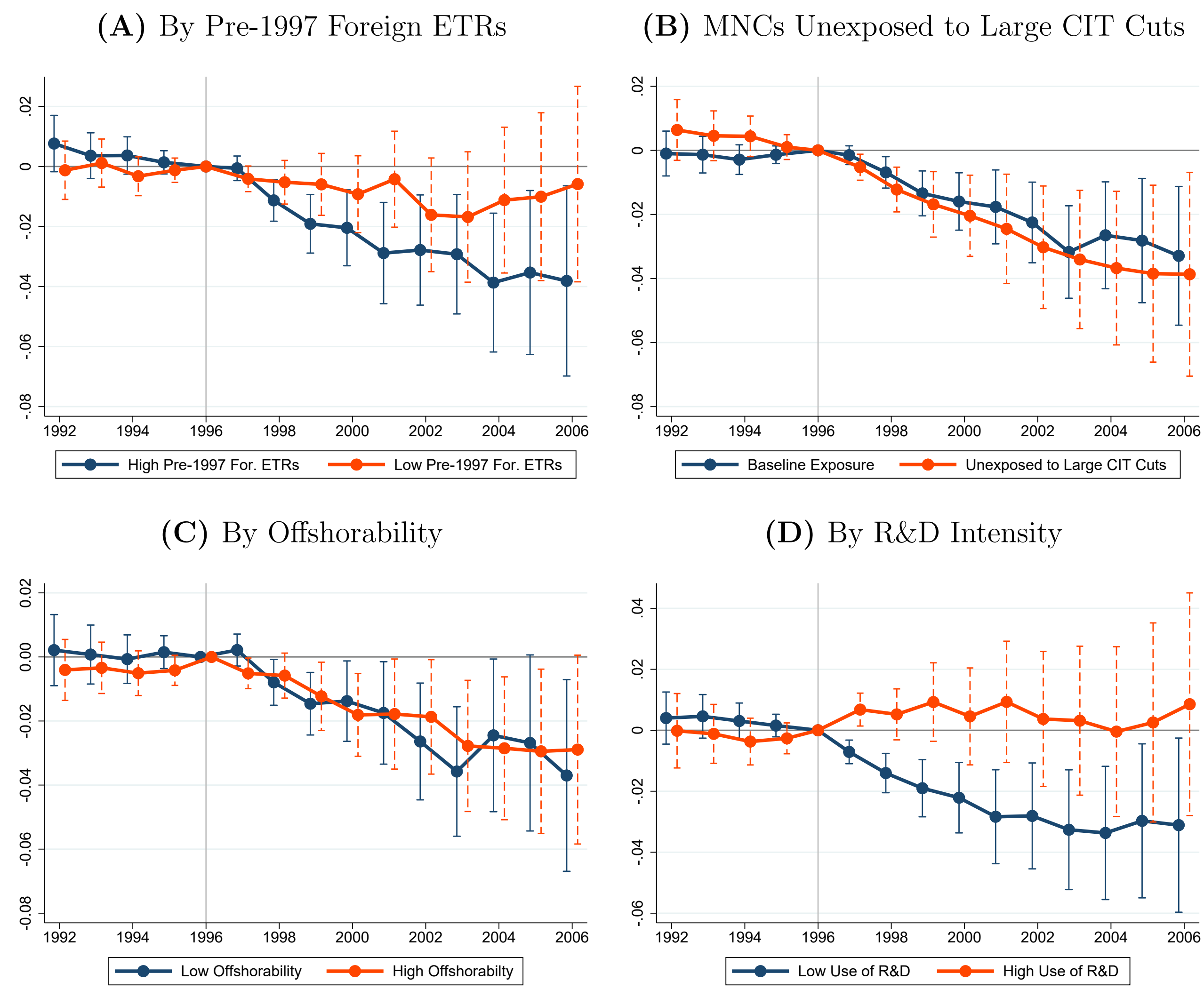
Notes: Figure 7 displays β coefficients and 95% confidence intervals from regressions in the form of Equation 3 which describe the effect of county-level CTB Exposure on the county-industry percent change in employment relative to 1996. Panel (A) displays estimates from the fully saturated specification which IPW weights by—and includes controls for—population and demographic controls, sectoral composition, trade, technology, and tax policies, as well as a control for exposure to the matched sample of large domestic firms. Panel (B) displays estimates from alternative specifications that progressively add demographic controls, sectoral composition, and trade, technology, and tax controls to both the IPW weighting construction and event study regression. All specificaitons across both panels include state-by-year and industry-by-year fixed effects. Standard errors are clustered at the county-level in all specifications.

Figure 8: Effects of Check-the-Box on Other Labor Market Outcomes



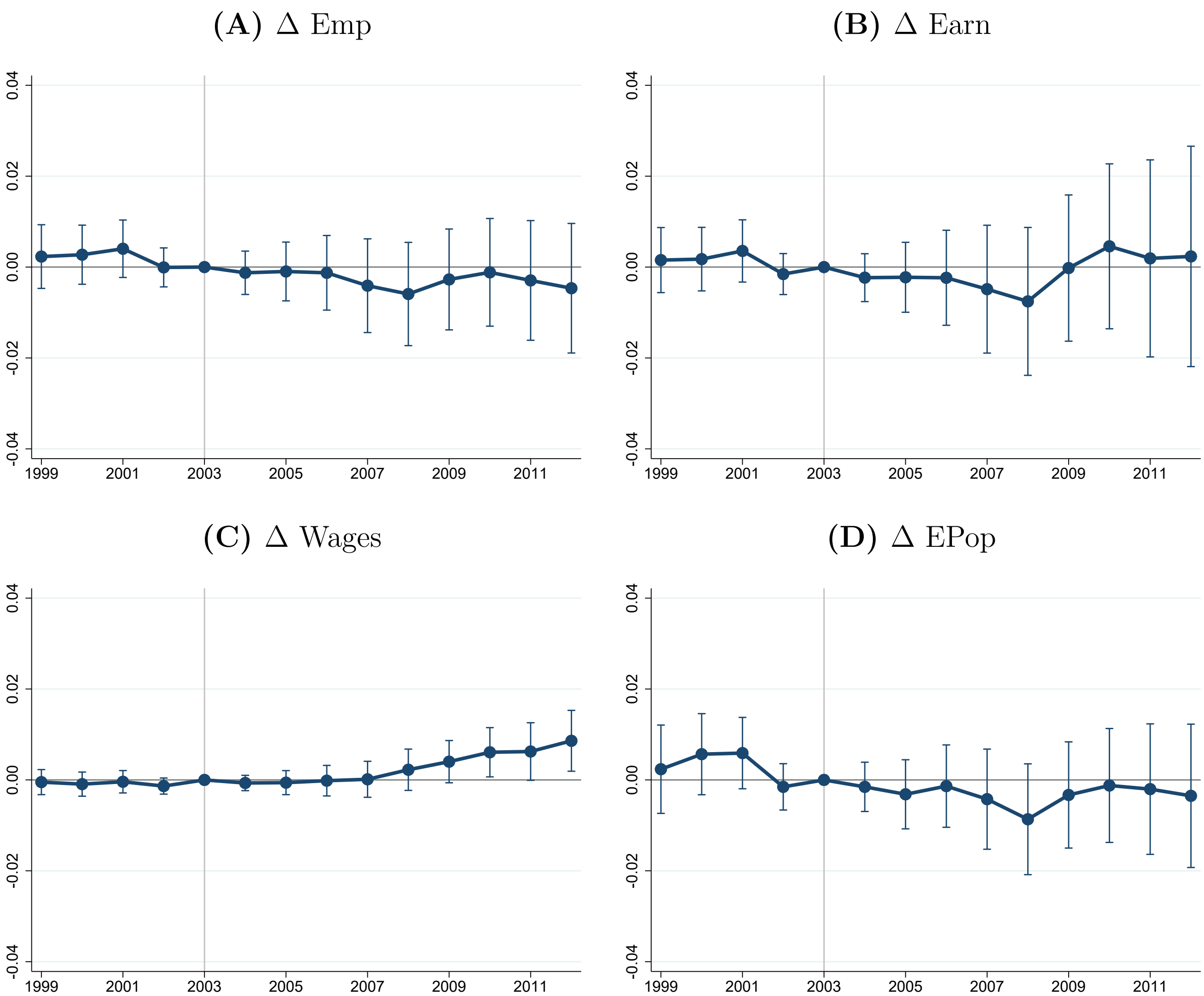
Notes: Panels (A)–(C) of Figure 7 displays β coefficients and 95% confidence intervals from regressions in the form of Equation 3 which describe the effect of county-level CTB Exposure on the county-industry percent change in wages, employment-to-population ratio, and total earnings relative to 1996. Panel (D) displays β coefficients and 95% confidence intervals from regressions in the form of Equation 3 describing the effect of cumulative disregarded entity elections on the percent change in employment relative to 1996. The recovered coefficients represent the percent change in employment per 1,000 additional disregarded entity elections in each year 1997–2006. All regressions include industry-by-year and state-by-year fixed effects as well as population and demographic controls, sectoral composition, trade, technology, and tax policies, as well as a control for exposure to the matched sample of large domestic firms. Standard errors in all specifications are clustered at the county level.

Figure 9: Heterogeneous Effects of Check-the-Box on 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) shows effects of exposure to CTB Treatment on employment separately for MNCs with high and low ETRs prior to CTB implementation. Panel (B) compares effects of exposure to MNCs that were unexposed to large CIT tax to our baseline employment effects of CTB Treatment. Panel (C) shows effects of exposure to CTB Treatment on employment separately for counties with high and low Offshorability measures from Autor and Dorn (2013). Panel (D) shows effects of exposure to CTB Treatment on employment separately for R&D Intensive MNCs and and Non-R&D Intensive MNCs. All specifications industry-by-year fixed effects and state-by-year fixed effects as well as the full suite of cross-sectional controls interacted with year fixed effects and use the full suite of cross-sectional controls to construct inverse probability weights. Standard errors in all specifications are clustered at the county level.

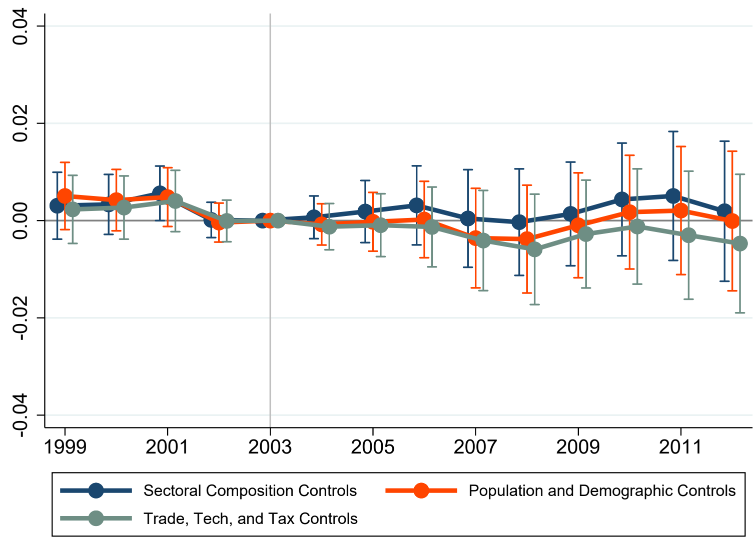
Figure 10: Effects of Repatriations on Domestic Labor Markets



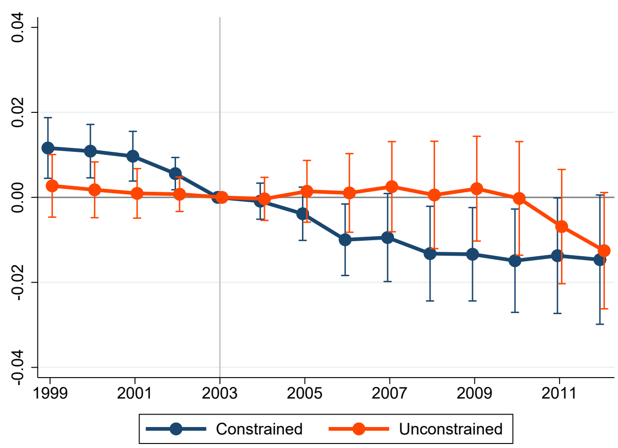
Notes: Figure 10 displays β coefficients and 95% confidence intervals from regressions in the form of Equation (5) which describe the effect of county-level RH Treatment on the percent change relative to 2003 of four different outcome outcome variables. Panel (A) displays the effect on county-industry changes in employment. Panel (B) displays the effect on county-industry changes in total earnings. Panel (C) displays the effect on county-industry changes in earnings per worker (wages). Panel (D) displays the effect on county-level employment-to-population ratios. Specifications in Panels (A)–(C) all include industry-by-year fixed effects and state-by-year fixed effects. Panel (D) does not include industry-by-year fixed effects. All specifications include the full suite of cross-sectional controls interacted with year fixed effects and use the full suite of cross-sectional controls to construct inverse probability weights. Standard errors in all specifications are clustered at the county level.

Figure 11: Repatriation Holiday Robustness and Heterogeneity

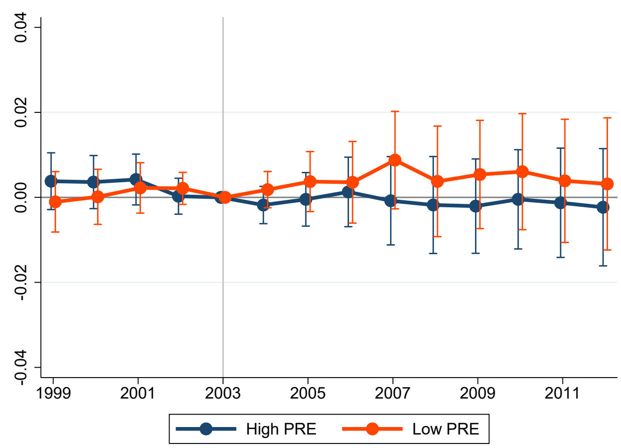
(A) Alternative Controls / Weighting



(B) By Financial Constraint



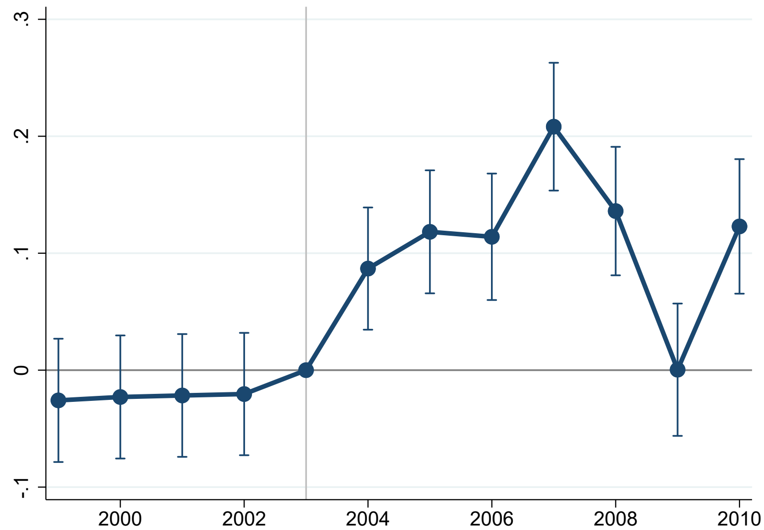
(C) By Permanently Reinvested Earnings



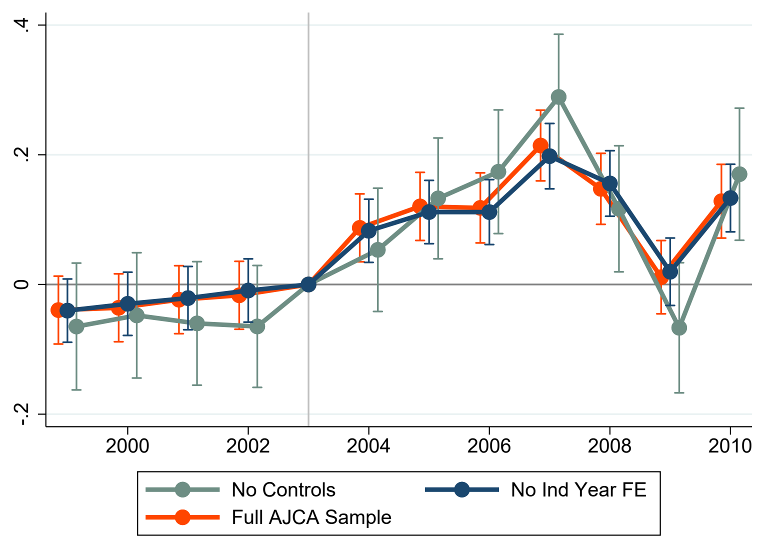
Notes: Figure 11 displays β coefficients and 95% confidence intervals from regressions in the form of Equation (5) which describe the effect of county-level RH Treatment on the county-industry percent change in employment relative to 2003. In Panel (A), three alternative specifications progressively add sectoral composition, demographic, and trade, technology, and tax controls to both the IPW weighting construction and event study regression. Panel (B) shows effects of exposure to RH Treatment on employment separately for financially constrained and financially unconstrained repatriators. Panel (C) shows effects of exposure to RH Treatment on employment separately for repatriators with high and low levels of permanently reinvested earnings prior to the holiday. All specifications include state-by-year and industry-by-year fixed effects. Specifications in Panels (B) and (C) include the full suite of county-level controls both in the regression and to construct inverse probability weights. Standard errors are clustered at the county-level in all specifications.

Figure 12: Effect of Total Repatriations on Payouts

(A) Effect of Total Repatriations on Total Payouts



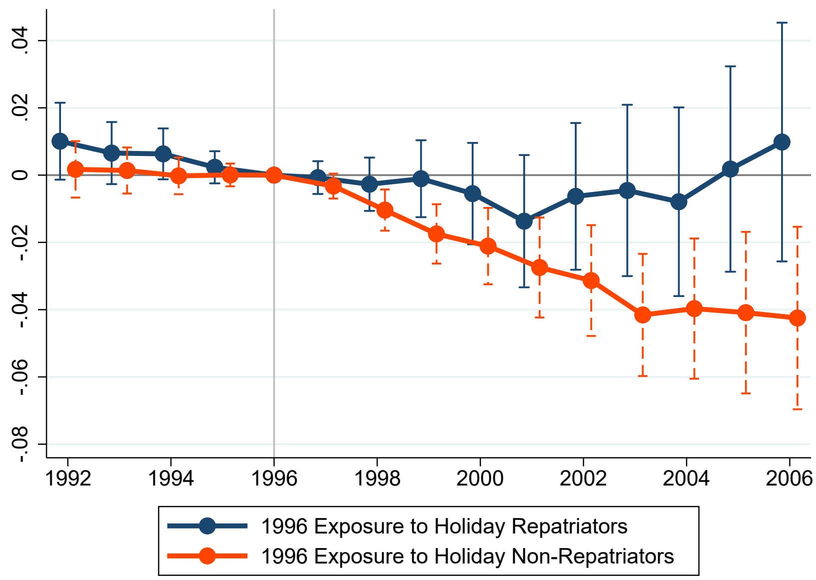
(B) Effect of Total Repatriations on Total Payouts; Robustness



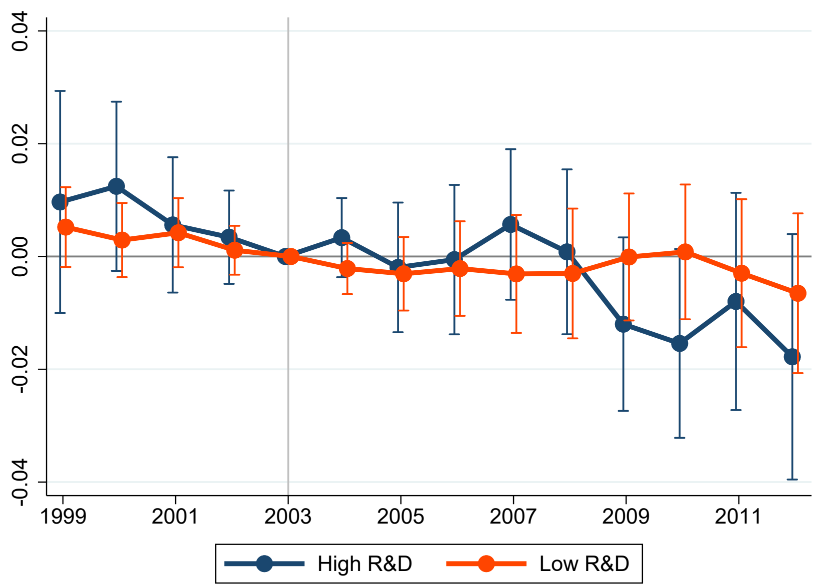
Notes: Figure 12 shows the effect of one dollar in repatriations per dollar in total assets on payouts per dollar of total assets for the repatriating MNCs and matched non-repatriating firms. Panel (A) displays coefficient estimates and 95% confidence internals from a regression of total payouts per 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 (1) do not include the control variables, (2) include year fixed effects instead of industry-by-year fixed effects, and (3) 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 13: Exposure to Check-the-Box and Repatriations

(A) Effect of CTB on Employment by 2004 Repatriation Decision



(B) Effects of Repatriations by R&D Intensity



Notes: Panel (A) of Figure 13 displays β coefficients and 95% confidence intervals from regressions in the form of Equation (3). Estimates are presented separately for 1996 MNCs who eventually repatriated under the RH and for 1996 MNCs who did not. Panel (B) displays β coefficients and 95% confidence intervals from regressions in the form of Equation (5). Estimates are presented separately for R&D intensive and R&D non intensive MNCs. All regressions include industry-by-year and state-by-year fixed effects, use the full suite of cross-sectional controls to construct inverse probability weights, and include the full suite of cross-sectional controls interacted with year fixed effect. Standard errors are clustered at the county level.

Table 1: Effect of Check-the-Box on Domestic Employment

	Δ Emp				
	(1)	(2)	(3)	(4)	(5)
CTB Treatment × 1997–2002	-0.0147***	-0.0101**	-0.0137***	-0.0136***	-0.0137***
	(0.00384)	(0.00398)	(0.00367)	(0.00358)	(0.00361)
CTB Treatment × Post 2002	-0.0332***	-0.0276***	-0.0335***	-0.0289***	-0.0303***
	(0.00962)	(0.0100)	(0.00905)	(0.00897)	(0.00901)
Industry × Year FE	✓	✓	✓	✓	✓
State × Year FE	✓	✓	✓	✓	✓
Sectoral Composition Controls		✓	✓	✓	✓
Population, Demographic Controls			✓	✓	✓
Trade, Tech, and Tax Controls				✓	✓
Domestic Firm Exposure Control					✓
Observations	1,197,240	1,197,240	1,197,240	1,197,240	1,197,240
Counties	2,968	2,968	2,968	2,968	2,968
1996 Treated Emp (Millions)	33.7	33.7	33.7	33.7	33.7
Relative Employment Loss (Millions)	-1.119	-0.932	-1.130	-0.976	-1.022
95% Confidence Interval	[-1.8, -0.5]	[-1.6, -0.3]	[-1.7, -0.5]	[-1.6, -0.4]	[-1.6, -0.4]

Notes: Table 1 displays β_1 and β_2 coefficients from regressions of the form

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

The outcome variable in all regressions is ΔEmp, the percentage point change in county-industry employment relative to 1996. Specification (1) includes industry-by-year fixed effects and state-by-year fixed effects. Specifications (2)–(5) progressively add sectoral composition controls, populations and demograpahic controls, and trade, technology, and tax controls in the construction of the inverse probability weights and to the regression interacted with time period fixed effects. All specifications also include state-by-year fixed effects. Specifications (1)–(3) include industry-by-year fixed effects. Standard errors are clustered at the county-level in all specifications. *, **, and *** denote statistical significance at the 10, 5, and 1% level. The Relative Employment Loss is calculated by multiplying the β_2 estimate by difference in MNC employment share between treated and non-treated counties and 1996 employment is treated counties from QCEW. More details on this calculation are provided in Section 4.

Table 2: Effects of Check-the-Box on Employment, Wages, and Earnings

	(1) Δ Emp	(2) Δ Earn	(3) Δ Wage	(4) Δ EPop
CTB Treatment × 1997–2002	-0.0137***	-0.0160***	-0.000529	-0.0117***
	(0.00361)	(0.00508)	(0.00152)	(0.00391)
CTB Treatment × Post 2002	-0.0303***	-0.0413***	-0.000696	-0.0214***
	(0.00901)	(0.0146)	(0.00294)	(0.00672)
Industry × Year FE	✓	✓	✓	
State × Year FE	✓	✓	✓	✓
Population, Demographic Controls	✓	✓	✓	✓
Sectoral Composition Controls	✓	✓	✓	✓
Trade, Tech, and Tax Controls	✓	✓	✓	✓
Domestic Firm Exposure Control	✓	✓	✓	✓
Observations	1,197,240	1,197,240	1,180,359	44,280

Notes: Table 2 displays β_1 and β_2 coefficients from regressions of the form

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

where the outcome varies from Δ Emp, Δ Earn, Δ Wage, to Δ EPop in Specifications (1)–(4), respectively. All specifications include the full suite of cross-sectional control variables interacted with year fixed effects and include the full suite of cross-sectional controls in constructing inverse probability weights. Standard errors are clustered at the county-level. *, **, and *** denote statistical significance at the 10, 5, and 1% level. More details are provided in Section 4.

Table 3: Heterogeneous Effects of CTB Exposure on Domestic Employment

	Pre-1997 Foreign ETR		Large CIT Cut		Local Offshorability		R&D Intensity	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High	Low	Exposed	Unexposed	High	Low	High	Low
CTB Treatment × 1997–2002	-0.0125***	-0.00443	-0.00671**	-0.0124***	-0.0117**	-0.0159***	0.00445	-0.0139***
	(0.00345)	(0.00337)	(0.00342)	(0.00343)	(0.00509)	(0.00498)	(0.00391)	(0.00319)
CTB Treatment × Post 2002	-0.0252***	-0.00581	-0.00220	-0.0250***	-0.0271**	-0.0336***	0.00293	-0.0222***
	(0.00853)	(0.00839)	(0.00841)	(0.00839)	(0.0119)	(0.0128)	(0.00918)	(0.00779)
Industry × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Sectorial Composition Cntrls	✓	✓	✓	✓	✓	✓	✓	✓
Population, Density Cntrls	✓	✓	✓	✓	✓	✓	✓	✓
Trade, Tech, and Tax Cntrls	✓	✓	✓	✓	✓	✓	✓	✓
Domestic Firm Exposure Cntrl	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1,197,240	1,197,240	1,197,240	1,197,240	1,197,240	1,197,240	1,197,240	1,197,240

Notes: Table 3 displays β_1 and β_2 coefficients from regressions of the form

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

The outcome variable in all regressions is the percentage point change in county-industry employment relative to 1996. In Specifications (1)–(4), and (7)–(8), the CTB Treatment is defined based on a subset of MNCs. The MNC split in Specifications (1) and (2) is above/below median pre-1997 foreign ETRs. The MNC split in Specifications (3) and (4) is by whether or not firms listed an OECD that experienced a more than 10 percentage point in the Exhibit 21a disclosures. The MNC split in Specifications (7) and (8) is by above/below median R&D Intensity (R&D expense scaled by total revenue controlling for firm-size deciles). To generate the estimates in Specifications (5) and (6), we interact both CTB Treatment varies in the above equation with a dummy equal to one for counties with above median share of highly "Offshorability" jobs (Autor and Dorn, 2013). Cross-sectional variation in offshorability interacted with fixed effects is also included in the regression. All specifications (2) include the full suite of cross-sectional control variables interacted with year fixed effects and include the full suite of cross-sectional controls in constructing inverse probability weights. Standard errors are clustered at the county-level. *, **, and *** denote statistical significance at the 10, 5, and 1% level. More details are provided in Section 4.3.

Table 4: Effects of Repatriations on Employment, Wages, and Earnings

	(1) Δ Emp	(2) Δ Wage	(3) Δ EPop	(4) Δ Earn
REPAT Treat $\times$ 2004–2006	-0.00147	-0.000607	0.00623*	-0.00279
	(0.00295)	(0.00113)	(0.00325)	(0.00356)
REPAT Treat $\times$ Post 2006	-0.00454	0.00415*	0.00744	-0.00234
	(0.00565)	(0.00238)	(0.00603)	(0.00858)
Industry $\times$ Year FE	✓	✓		✓
State $\times$ Year FE	✓	✓	✓	✓
Sectoral Composition Controls	✓	✓	✓	✓
Population, Demographic Controls	✓	✓	✓	✓
Trade, Tech, and Tax Controls	✓	✓	✓	✓
MNC Exposure Control	✓	✓	✓	✓
Observations	1,127,882	1,094,719	41,416	1,127,882

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

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

where the outcome varies from Δ Emp, Δ Earn, Δ Wage, to Δ EPop in Specifications (1)–(4), respectively. All specifications (2) include the full suite of cross-sectional control variables interacted with year fixed effects and include the full suite of cross-sectional controls in constructing inverse probability weights. Standard errors are clustered at the county-level. *, **, and *** denote statistical significance at the 10, 5, and 1% level. More details are provided in Section 5.

Appendix

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

A Variable Definitions

This appendix provides definitions and sources for all variables used in the empirical analysis.

A.1 Treatment and Outcome Variables

Variable Name	**Description**
CTB Exposure	County-level share of workers employed by US MNCs in 1996. *Source:* Authors' calculations based on Standard & Poor's (1980-2014) and Walls & Associates (2012) data.
CTB Treatment	County-level indicator equal to one for counties with CTB Exposure greater than 5.5% (top quartile of counties). *Source:* Authors' calculations based on Standard & Poor's (1980-2014) and Walls & Associates (2012) data.
REPAT Exposure	Dollars per worker repatriated by US MNCs under the repatriation holiday. *Source:* Authors' calculations based on Walls & Associates (2012), Standard & Poor's (1980-2014), and Blouin and Krull (2009) data.
REPAT Treatment	County-level indicator equal to one for counties with that received more than \$150 per worker in repatriations (top half of counties). *Source:* Authors' calculations based on Walls & Associates (2012), Standard & Poor's (1980-2014), and Blouin and Krull (2009) data.
ΔEmp	Percent change in county-by-3-digit NAICS industry employment relative to 1996. *Source:* Authors' calculations based on QCEW (2017) data.
ΔEarn	Percent change in county-by-3-digit NAICS industry total earnings relative to 1996.*Source:* Authors' calculations based on QCEW (2017) data.
ΔWage	Percent change in county-by-3-digit NAICS industry total earnings per employee relative to 1996.*Source:* Authors' calculations based on QCEW (2017) data.
ΔEPop	Percent change in county-level employee-to-population ratios relative to 1996.*Source:* Authors' calculations based on QCEW (2017) and NCI (2022) data.

Continued on next page

Table A1 – *Continued from previous page*

Variable	Description
Domestic Out-Migration (Δm)	Percent change in county-level workers leaving a county relative to 1996.*Source:* Authors' calculations based on IRS (2023) data.

A.2 Cross-sectional Controls / Weighting Variables

Sectoral Composition Controls

Variable Name	Description
% NonDurable Manufacturing	County-level percentage of workers workings in non-durable manufacturing industries. *Source:* Authors' calculations based on U.S. Census Bureau (1990) data.
% Durable Manufacturing	County-level percentage of workers workings in durable manufacturing industries. *Source:* Authors' calculations based on U.S. Census Bureau (1990) data.
% Construction	County-level percentage of workers workings in construction industries. *Source:* Authors' calculations based on U.S. Census Bureau (1990) data.
% Agriculture	County-level percentage of workers workings in agriculture. *Source:* Authors' calculations based on U.S. Census Bureau (1990) data.
% Wholesale	County-level percentage of workers workings in wholesale. *Source:* Authors' calculations based on U.S. Census Bureau (1990) data.

Population and Demographic Controls

Variable Name	Description
Log Population	Log of county-level population. *Source:* Authors' calculations based on NCI (2022) data.
Log Population Density	The log of population density where population density is calculated as county-level population divided by a county's number of square miles. *Source:* Authors' calculations based on NCI (2022) and U.S. Census Bureau (2011) data.
% < HS	Percentage of county-level population with less than a high-school degree *Source:* Authors' calculations based on U.S. Census Bureau (1990) data.
% White	Percentage of white residents in a county *Source:* Authors' calculations based on U.S. Census Bureau (1990) data.

Continued on next page

Table A3 – *Continued from previous page*

Variable	Description
% Black	Percentage of Black residents in a county. *Source:* Authors' calculations based on U.S. Census Bureau (1990) data.

Trade, Tax, and Technology Controls

Variable Name	Description
DPAD Exposure	County-level share of workers in top third of NAICS 4-digit industries benefiting from the Domestic Production Activities Deduction. *Source:* Authors' calculations based on Ohrn (2018) data
ETI Exposure	County-level share of workers in in top third of NAICS 4-digit industries that benefited most from the Extraterritoiral Income Exclusion *Source:* Authors' calculations based on Ohrn (2018) data.
Bonus Exposure	County-level share of workers in top third of NAICS 4-digit industries most benefiting from bonus depreciation. *Source:* Authors' calculations based on from Garrett et al. (2020) data.
China Shock	1999-2007 county-level change in Chinese import exposure. *Source:* Autor et al. (2016).
Routine Work	County-level share of workers in a commuting zone that work in occupations that involve routine tasks. *Source:* Autor and Dorn (2013).
NAFTA Exposure	Conspuma-level weighted average Mexican import tariff prior to NAFTA. *Source:* Hakobyan and McLaren (2016).
Sec 936 Exposure	County-level share of workers employed by MNCs with estblishments in Puerto Rico in 1995. *Source:* Suárez Serrato (2018).
Matched Domestics	County-level share of workers employed by matched domestic firms in 1996. *Source:* Authors' calculations based on Standard & Poor's (1980-2014) and Walls & Associates (2012) data.
Matched MNCs	County-level share of workers employed by matched non-repatriating MNCs. *Source:* Authors' calculations based on Standard & Poor's (1980-2014), Walls & Associates (2012), Blouin and Krull (2009) and Faulkender and Petersen (2012) data.

A.3 Other Variables

Variable Name	Description
Offshorability	An indicator equal to one for counties with above median scores in Autor and Dorn (2013) Offshore Z-Score, which is based on the share of jobs that do not require "either direct interpersonal interaction or proximity to a specific work location" and therefore are more likely to be outsourced *Source:* Authors' calculations based on Autor and Dorn (2013) data.
R&D Intensity	R&D expenditures scaled by total revenue, controlling for deciles of firm size (total assets). *Source:* Authors' calculations based on Standard & Poor's (1980-2014) data.
Worldwide Tax Expense (WWTE)	Total income taxes net of deferred income taxes scaled by total assets. *Source:* Authors' calculations based on Standard & Poor's (1980-2014) data.
Pretax Income	Worldwide pretax income scaled by total assets. *Source:* Authors' calculations based on Standard & Poor's (1980-2014) data.
MNC	An indicator equal to one if a firm reports non-zero pretax income from foreign operations in years 1994–1996.*Source:* Authors' calculations based on Standard & Poor's (1980-2014) data.
Total Revenue	Annual aggregate total revenue for all MNCs defined as treated by CTB. *Source:* Authors' calculations based on Standard & Poor's (1980-2014) data.
Total Assets	Annual aggregate total assets for all MNCs defined as treated by CTB. *Source:* Authors' calculations based on Standard & Poor's (1980-2014) data.
Total PIFO	Annual aggregate total pretax income from foreign operations for all MNCs defined as treated by CTB. *Source:* Authors' calculations based on Standard & Poor's (1980-2014) data.
PIFO Share of Revenue	Annual aggregate total pretax income from foreign operations divided by annual aggregate total revenue for all MNCs defined as treated by CTB. *Source:* Authors' calculations based on Standard & Poor's (1980-2014) data.
Foreign ETR	A continuous variable describing the average effective tax rate on foreign income. The mean of $txfo/pifo$ for years 1994 to 1996 within each firm. Firms with negative PIFO and positive foreign tax liabilities are considered to have a high Foreign ETR. *Source:* Authors' calculations based on Standard & Poor's (1980-2014) data.

Continued on next page

Table A5 – *Continued from previous page*

Variable	Description
Domestic Sales Share	A continuous variable describing the percent of sales arising from the domestic market. Domestic sales are from segments with *geotp*=2 and foreign sales are from segments with *geotp*=3 *Source:* Authors' calculations based on Standard & Poor's (1980-2014) segments data.
Any Patenting	Firm-level indicator equal to one for firms that were awarded a patent prior to 2004. *Source:* Authors' calculations based on Autor et al. (2020) data.
Patents / Total Assets	Firm-level total patents awarded prior to 2004 realtive to 2004 total assets. *Source:* Authors' calculations based on Standard & Poor's (1980-2014) and Autor et al. (2020) data.
Silicon Valley HQ	Firm-level indicator equal to one for firms that report their headquarters location in Silicon Valley. Silicon Valley includes Santa Clara, San Mateo, Alameda, and Santa Cruz counties. *Source:* Authors' calculations based on Walls & Associates (2012) data.

B 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 across space at a given point in time (Barnatchez et al., 2017). Establishments in a firm are tied together by a unique identifier called HQDUNS.

Matching

In order to match to NETS to Compustat, 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 an iterative process. 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 criteon, we move to a second stage. We require a precise match on latitude and longitude coordinates (one decimal), but allow for a fuzzy match on names. Again, we 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 establishments share a unique HQDUNS so no judgement calls were made. Finally, for unmatched Compustat firms with more $2 billion in total assets in 1996 or 2003, we manually attempted matches by searching through historical company filings looking for name changes previous names used by the HQ establishment. Typically, this required us to follow a 1996 Compustat firm through mergers, acquisitions, and restructuring through 2012 when NETS names were assigned.

In 1996, we matched 4,110 firms across Compustat and NETS. Figure B1 demonstrates how well our matching procedure worked. Panel (A) shows that our procedure succesfully matched approximately 50% of Compustat firms to NETS firms. The panel also shows we successfully matched approximately 60% of Compustat MNCs to NETS. Panel (B) shows these matches represented over 80% of aggregate total assets in Compustat and more over 90% of total pretax income from foreign operations.

C Constructing Control Firm Variables

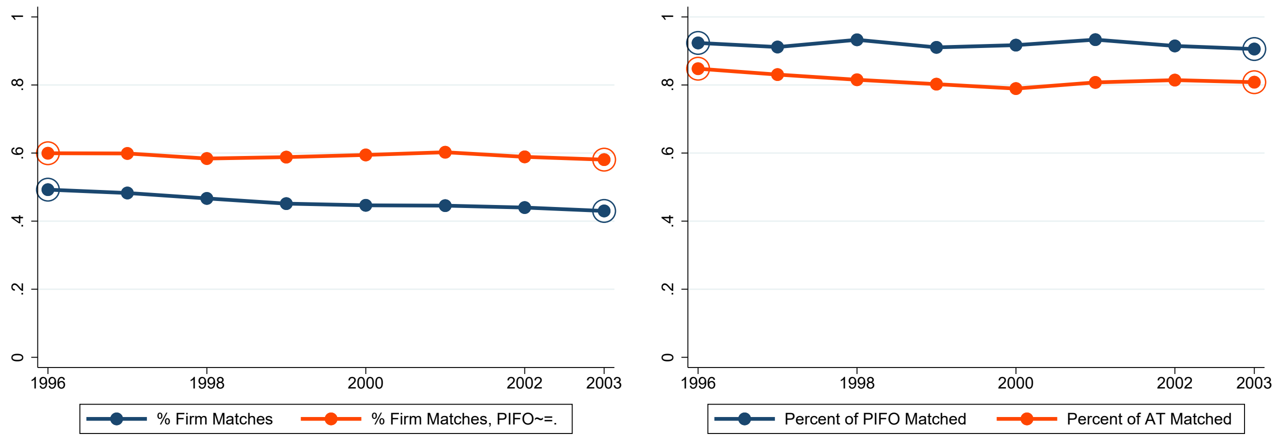
For both groups of treated MNCs—all MNCs in the case of CTB and repatriating MNCs in the case of the RH—we create a matched control group of firms. We use the matched control group to examine firm-level responses to the policies as well as to construct a county-level measure of exposure to the matched control group to account for observable determinants of MNC or repatriating MNC location choice.

For the analysis of CTB, we define the group of potential "control" firms as firms in our matched sample with no pretax foreign income during the sample years.[44] These matched domestic firms allow us to create a control variable at the county level that is intended to capture the unobservable characteristics of a local market that make it attractive to large businesses.

[44]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).

Figure B1: Assessing Compustat–NETS Match

(A) Percent of Firms Matched **(B)** Percent of Assets and PIFO Matched



Notes: Appendix Figure B1 describes the share of the Compustat data set that was successfully merged with NETS using the methods described in this appendix and in Section 3.1. 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 and Treatment variables.

We consider all 786 MNCs we identify in both Compustat and NETS as 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 sample consists of 786 US MNCs and 786 matched domestic firms. We calculate the share of employees working for the matched group of domestic firms in 1996. We include this variable interacted with year effects in our fully saturated model. We also use this group of control firms to estimate how CTB affected the ETRs of MNCs.

We construct the matched sample of non-repatriating MNCs for the RH analysis, we propensity score match repatriating MNCs to non-repatriating US MNCs based on characteristics that Blouin and Krull (2009) and Faulkender and Petersen (2012) 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, (7) the ratio of foreign assets estimated as described in Oler et al. (2007) to total worldwide assets, and (8) the level of permanently reinvested earnings.[45] We restrict each match to be within the same NAICS 1-digit sector.

According to Blouin and Krull (2009), during the repatriation holiday, 357 firms repatriated \$291.6 billion. This is a relatively small subset of US MNCs. The firms that chose to take advantage of the holiday differed from other MNCs on a variety of margins. We verify the differences documented in the literature: 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. They have higher levels of permanently reinvested cash outside of the US. Despite these differences, we were very successful in constructing a subset of firms that look nearly identical to the repatriating MNCs on every one of these margins because there are so many US MNCs and so few repatriated. We construct a county-level variable capturing the share of employees working in these matched MNCs in 2003. We include this variable in our weighting strategy and as a control in our fully saturated regression model while interacted with year fixed effects.

D Effect of CTB on Worldwide Effective Tax Rates

In this appendix, we measure the impact of CTB on world-wide effective tax rates by comparing the tax rates of MNCs in our Compustat-NETs sample to tax rates for our matched sample of domestic firms as described in Appendix C. We implement a dynamic version of the Dyreng and Lindsey (2009) method, which estimates ETRs by measuring the tax expense response to an additional dollar of pre-tax profit. We use Compustat data to estimate regressions of the form

$$\begin{aligned} WWTE_{it} = \alpha + \beta PI_{it} + \gamma[PI_{it} \times MNC_i] \\ + \sum_{y=1992,\ y\neq 1996}^{2006} \omega_h \Big(PI_{it} \times MNC_i \times \mathbb{I}(t=y) \Big) + \mu_i + \nu_{jt} + \varepsilon_{it} \end{aligned} \tag{D.1}$$

where $WWTE$ is firm i's current worldwide tax expense in year t (total income taxes net of

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

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. 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 these measures. The controls include net operating losses relative to assets, firm size (log assets), long term debt ratio (debt over assets), and R&D expense relative to assets. WWTE and PI are scaled by total assets and winsorized at the 1^{st} and 99^{th} percentiles.[46]

Estimates from Equation (D.1) 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.

Figure 4 presents ω estimates from our dynamic Dyreng and Lindsey (2009) regressions. We also present β, γ, and pooled ω estimates in Table F1.[47] We present ω estimates from the most fully saturated model in Panel (A) of Figure 4. These include firm and industry-by-year fixed effects as well as firm-level control variables.[48] Three results are apparent. First, differences in ETRs between MNCs and matched domestic firms are generally stable in the pre-period suggesting WWETRs were trending similarly for MNCs and the matched domestic sample prior to CTB implementation. Second, during the period 1997–2000 just after CTB implementation, the tax rates of MNCs drop by about 3 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

[46] 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.

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

[48] The set of control variables matches those in Dyreng and Lindsey (2009). They 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.

drop until 2004 before a slight recovery in 2005 and 2006.The timing of the WWETR response generally matches the time-patterns of CTB elections presented in Figure 2 and the time-patterns in domestic employment responses presented in Figure 7. The corresponding pooled estimate, presented in Column (3) of Table F1 suggests CTB decreased MNCs' worldwide ETRs by 5.24 percentage points after 1997. These estimates are consistent with the decline in effective tax rates documented by Blouin and Krull (2019).

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.[49] These plots correspond to columns (1), (2), and (4) of Table F1. 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 here is consistent with CTB lowering worldwide effective tax rates for MNCs. The timing of ETR effects roughly matches the time-patterns in CTB elections and employment losses due to the policy. We take this comovement as suggestive evidence that disregarded entity elections facilitated international tax planning activities resulting in lower ETRs and ultimately domestic job losses.

[49] We follow the same procedure as described in Appendix C but do not restrict the sample to firms observed in NETS.

Table D1: Effect of Check-the-Box on Worldwide Effective Tax Rates

	(1) WWTE	(2) WWTE	(3) WWTE	(4) WWTE
Pretax Income × MNC	0.0862***	0.0849***	0.0846***	0.0626***
	(0.00976)	(0.00971)	(0.00971)	(0.00739)
Pretax Income × Post × MNC	-0.0517***	-0.0513***	-0.0524***	-0.0473***
	(0.00914)	(0.00946)	(0.00957)	(0.00632)
Firm FE	✓	✓	✓	✓
Year FE	✓			
Industry-Year FE		✓	✓	✓
Controls			✓	✓
Sample	NETS	NETS	NETS	Compu.
Observations	17,556	17,386	17,386	23,911

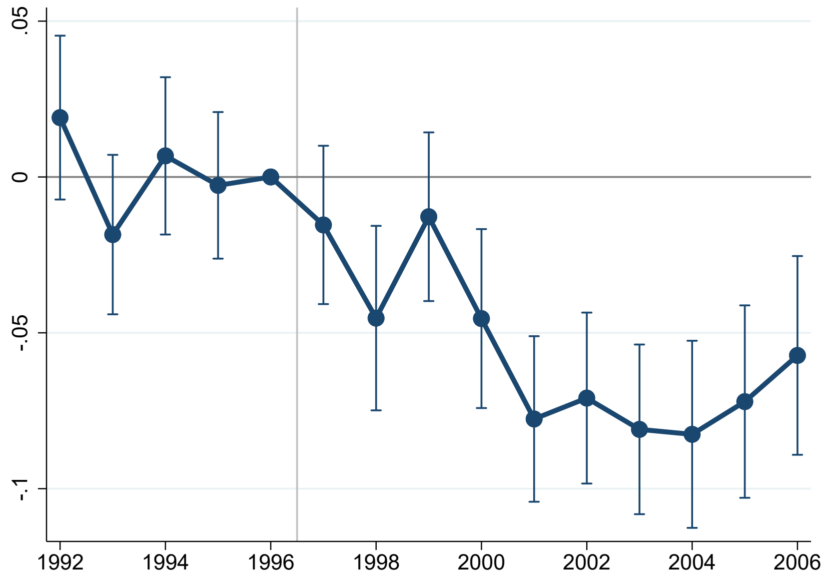
Notes: Table F1 displays γ, and ω coefficients from regression of the form

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

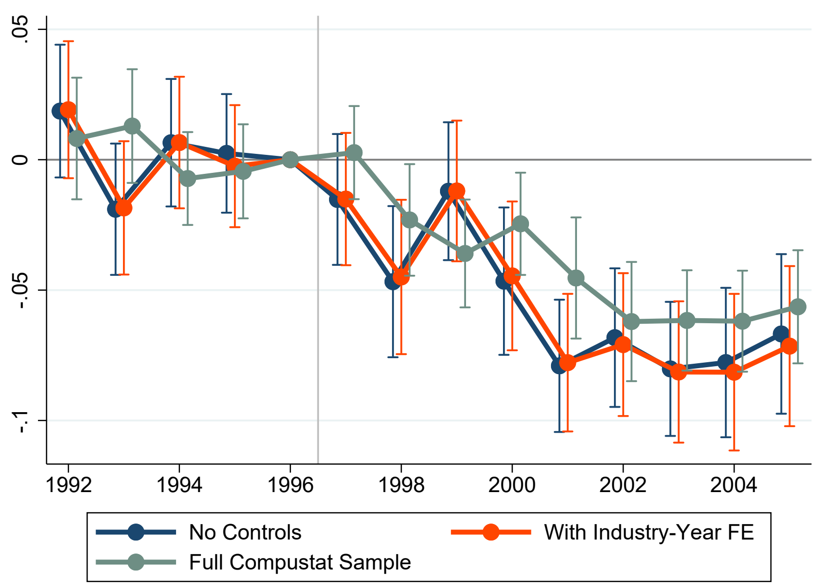
Coefficient ω is the coefficient of interest and measures the differential change in worldwide effective tax rates between MNCs and matched domestic firms after 1997. Specifications (1) – (3) rely on the Compustat-NETS sample of MNCs and matched domestic firms. Specification (4) includes all MNCs identified in Compustat and matched domestics. Specification (1) includes firm and year fixed effects. Specification (2) includes firm and industry-by-year fixed effects. Specifications (3) and (4) include firm and industry-by-year fixed effects as well as the time-varying firm-level controls described in Appendix F. Standard errors are clustered at the firm level. *, **, and *** denote statistical significance at the 10, 5, and 1% level.

Figure D1: Effect of Check-the-Box on Effective Tax Rates

(A) Fully Saturated Model



(B) Alternative Specifications



Notes: Figure D1 shows differences in effective tax rates between MNCs and matched domestic firms relative to the difference in 1996. The figure displays ω coefficients and 95% confidence intervals from regressions of the from

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

This figure displays alternative specifications to Panel A of Figure 4 without controls, with year as opposed to industry-year fixed effects, and using the full sample of Compustat firms. Standard errors are clustered at the firm level.

E Place-Level Inverse Probability Weighting

In this appendix, we describe the construction, utility, and impact of the inverse probability weights used in the primary analysis.

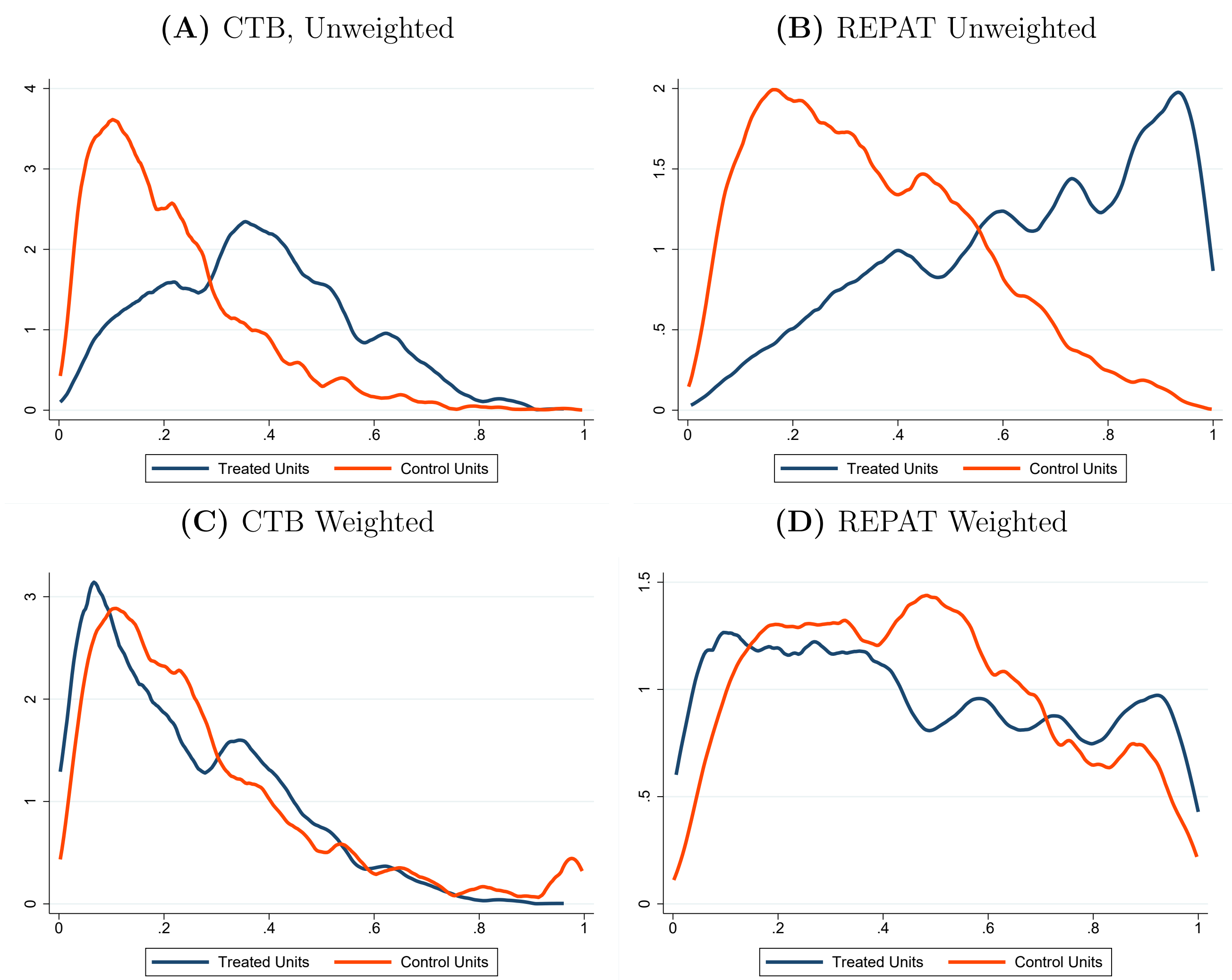
We employ inverse probability weights (Abadie, 2005) when comparing local labor markets with the most exposure to either policy to those with less exposure for two reasons. First, we focus on the variation in treatment particularly in places that are marginal in an attempt at identifying a more generalizable average treatment effect instead of an average treatment on the treated effect. Second, the locations with treatment are observably different than the places that we do not consider treated, as shown in the (B) panels of Figures 5 and 6. While we directly control for all of these observable characteristics interacted with year fixed effects to allow for flexibly changing impacts of the controls, they are all included as continuous variables with constant linear dose response assumed within each year by the linear functional form. By weighting the treatment and control samples to be more observably similar, we rely less heavily on the parametric functional form of these observable controls, because the controls are more similar for treatment and control.

To create these weights, we start by estimating a logistic regression for each CTB and the RH with coefficients shown in blue (top) of the (B) panels of Figures 5 and 6. These regression estimates generate likelihoods between 0 and 1 that describe how likely a county is to be in the treatment, which we display in Panels (A) and (B) of Figure E1. The orange (left) kernel density shows the likelihoods of treatment for the untreated group–those below the 75th percentile of CTB exposure in Panel (A) and those below the 50th percentile of REPAT exposure. The blue (right) densities are the estimated treatment likelihoods for the treated group. We can see that the treatment model based on observables has important explanatory power in both cases with treatment being successfully predicted based on observables because the blue lines are substantially to the right of the orange lines.

We then generate inverse probability weights based on these likelihoods, $\hat{l_c} \in (0, 1)$. For the treated group, the weight is defined as $1/\hat{l_c} \in (0, 1)$, which increases weight of county-industry observations that were unlikely to be treated—those that are most observably similar to non-treated county-industries. For the control group, the weight is defined as the analogue, $1/(1-\hat{l_c} \in (0, 1))$, which increases the weight for county-industries that are most observably similar to the treated units.

We show the likelihoods of treatment after including the inverse probability weighting scheme in Panels (C) and (D) of E1. In Panel (C), which shows the weighted treatment likelihoods for CTB in 1996, we find that that the likelihoods of treatment for treatment and control conditional on observables are largely overlapping with a distribution that looks much more like the original distribution of the control units. Similarly, in Panel (D), reweighting the treatment and control units around the repatriation holidays leads both samples to have similar densities that span from vary low to very high likelihoods in a nearly uniform pattern.

Figure E1: Assessing Matching Strategies using Kernal Density Plots



Notes: Figure E1 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 4. 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.

F Firm-level Outcomes around CTB

F.1 Potential to Benefit from CTB and Domestic Sales Share

In this appendix, we further discuss how we use Compustat Geographic Segments data to explore whether CTB induced US MNCs to shift operations abroad following CTB implementation in 1997. After manually checking and correcting underlying segments data using primary source financial statements, we calculate Domestic Sales Share as the ratio of domestic sales to total domestic and foreign sales.We then compare Domestic Sales Shares for MNCs with high foreign ETRs prior to 1997 to Domestic Sales Shares for MNCs with low foreign ETRs prior to 1997. As we discuss in Section 4.3, firms with high foreign ETRs were more likely to benefit from the implementation of the CTB policy. We use the same classification as in Section 4.3 and label an MNC as having a high foreign ETR if their foreign ETR was greater than 35% or if they had positive foreign taxes paid, but negative income from foreign operations.

Figure 4 presents dynamic DD estimates from a regression of the form

$$\begin{aligned} \text{Domestic Sales Share}_{it} = \alpha + &\sum_{y=1992,\ y\neq 1996}^{2006} \beta_y \Big[\text{High FETR}_i \times \mathbb{I}(t=y)\Big] \\ &+ \mu_f + \nu_{jt} + \mathbf{X'}_{ft}\gamma + \epsilon_{it}, \end{aligned}$$

where i, j, and t, index firms, industries, and years, respectively. High FETR is a firm-level indicator equal to one for firms with high foregin ETRs. μ_f and ν_{jt} are firm and industry-year fixed effects. $\mathbf{X'}_{ft}$ is a vector of pre-period bins for terciles of firm size, ROA, and debt ratios. β_y are the dynamic DD estimates which show differences in the outcome between high and low FETR MNCs relative to the difference in 1996.

Panel B of Figure 4 shows that differences in domestic sales shares between MNCs that stood to benefit most from CTB and those that stood to benefit less were stable in years 1992–1996. Then, upon policy implementation, domestic sales shares dropped for high FETR firms relative to low FETR firms. This shows that firms that stood to gain the most from CTB decreased their domestic presence relative to their foreign presence the most. This finding is consistent with the results presented in Figure 9, which show larger domestic labor market effects for places exposed to MNCs with high FETRs.

Table F1 presents the related pooled DD estimates where the year interactions are replaced with an indicator for 1997–2002 and an indicator for 2003–2006. Specification (1) includes just firm and year fixed effects. Specification (2) include firm and industry-by-year fixed effects. Specification (3) includes firm, industry-by-year fixed effects and the binned control variables interacted with year fixed effects described above. Focusing on Specification (3), we see the Domestic Sales Shares of MNCs that stood to gain most from CTB decreased by approximately 6.5% in years 1997–2002 and by 5.8% in years 2003–2006 relative to the pre-period.

The evidence presented in this appendix suggests that firms that stood to benefit the most from CTB decreased their domestic business activities the most in response to the implementation of the policy in 1997. These findings are consistent with the heterogeneous results presented in Panel (A) of Figure 9.

Table F1: Effect of Potential to Benefit from CTB on Domestic Sales Share

	Domestic Sales Share		
	(1)	(2)	(3)
High FETR × 1997–2002	-0.0370*	-0.0647***	-0.0645***
	(0.0205)	(0.0177)	(0.0170)
High FETR × Post 2002	-0.0356	-0.0573***	-0.0582***
	(0.0244)	(0.0185)	(0.0177)
Firm FE	✓	✓	✓
Year FE	✓		
Industry-Year FE		✓	✓
Controls			✓
Observations	9,732	9,530	9,501

Notes: Table F1 tests whether the domestic share of sales is changing for high foreign ETR MNCs who are more impacted by CTB. The unit of observation is a firm-year in Compustat Segments, and the sample includes all firm-years of US MNCs in our sample. The estimates come from a dynamic DD model that pools years 1997-2002 together as well as 2003-2006. Sales for US MNCs with high foreign effective tax rates before CTB move more of their sales abroad after CTB. Standard errors are clustered at the firm level. *, **, and *** denote statistical significance at the 10, 5, and 1% level.

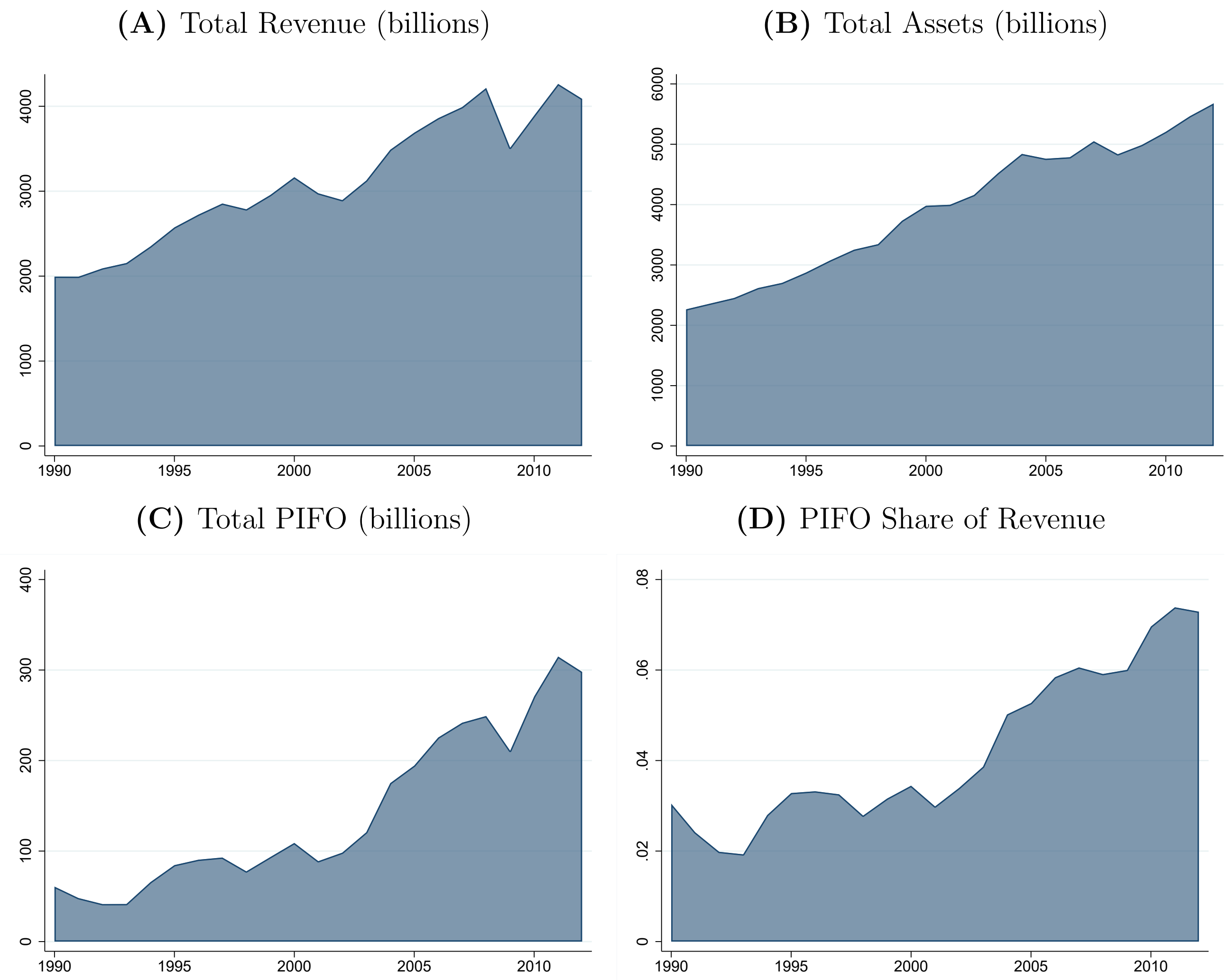
F.2 Other CTB Firm Outcomes from 1992-2012

In this appendix, we discuss how MNC firm outcomes evolve around the implementation of CTB in 1997. A potential threat to a causal interpretation of our CTB estimates is that some alternative change in 1997 negatively impacted US MNCs and US labor markets in which US MNCs operated that was not related to CTB. We show here that US MNCs experiencd strong growth after 1997, suggesting no change that negatively impacted MNCs occurred at the same time as CTB.

In Figure, we show how aggregate total revenue ($revt$), assets (at), and pre-tax income from foreign operations ($pifo$) and PIFO as a share of revenue evolve during the years 1990–2012 for our sample of 786 MNCs that we use to construct CTB Exposure.

Panel (A) of Figure F1 shows the trends in revenue for US MNCs around 1997. Starting in 1990, MNC revenue was \$2 trillion, which increased to \$2.9 trillion by 1997. By 2007, US MNC revenue had increased to \$4 trillion, before a slight decline during the great financial crisis in 2008-09. Similarly, Panel (B) shows that total assets increase from \$2.3 trillion in 1990 to \$3.3 trillion in 1997 and \$5.1 trillion by 2007. Figure (C) shows a larger increase in foreign-originated pretax income in percent terms, as $pifo$ increased from \$61 billion in 1990 to \$93 billion in 1997 and \$242 billion by 2007. This faster increase in foreign-originated pretax income is highlighted in Panel (D) where $pifo$ increase from 3.1% of revenue in 1990 to 3.2% in 1997 and 6.1% by 2007. We can see that US MNCs continued growing quickly—and becoming more international—after CTB was implemented in 1997. Overall, we do not find evidence that US MNCs began to shrink in 1997, suggesting no negative shock to MNCs drives our CTB results.

Figure F1: Growth of MNCs Exposed to CTB



Notes: Figure shows the growth of MNCs in our CTB sample during the period 1990 to 2012. Panel (A) displays total revenue for the sample in billions of USD. Panel (B) displays total assets for the sample in billions of USD. Panel (C) displays total pretax income from foreign operations (PIFO) in billios of USD. Panel (D) presents total PIFO as a share of total revenue. *Source:* Authors' calculations based on Compustat data.

G Robustness of the Check-the-Box Analysis

G.1 Additional Cutoffs for Discrete Treatment

In this appendix, we discuss the robustness of the baseline CTB results to the choice of discrete cutoffs for determining CTB treatment. The primary discrete cutoff we use is places with more than 5.5% of the local labor force working in MNCs in 1996. We choose 5.5% as the cutoff because it is both the population-weighted national average of CTB Exposure and because it treats 25% of counties as treated. This discretization affords us two benefits. First, we relax the usual assumptions about constant linear dose response associated with a continuous control, which also limits the impact of any outliers. Second, the discrete treatment facilitates the use of inverse probability of treatment weights that balance the sample on observable characteristics.

However, this discretization comes with a potential downside that the treatment cutoff is a degree of freedom within researcher control. We show, in this appendix, that our results are quantitatively extremely similar across different treatment cutoffs. In Table G1, we show estimates of the pooled difference-in-differences coefficients estimated separately for treatment defined at the 66th (4.8% CTB Exposure), 75th (5.5% CTB Exposure), and 80th (5.8% CTB Exposure) percentiles.

While discretizing affords us more flexibility in how the treatment could have non-linear dose responses, we compare the treatment coefficients by linearizing the impacts and assuming the missing intercept is negative (control units have the negative impact implied by the linearized estimate and their average exposure, as discussed in Section 4). For the baseline treatment threshold at the 75th percentile, the treatment group has 8 pp. more exposure to CTB than the control group on average (10.4% relative to 2.4%). The unweighted average CTB exposure in the estimation sample is 4.5%. If we take the CTB Treat $\times$ Post 2002 coefficient and divide by 8 pp., it implies that a 1 pp. increase in CTB exposure would lead to a 0.38 pp. decline in employment relative to 1996 after 2002. Multiplying 0.38% by the population (unweighted) average of 4.5% indicates an average impact of 1.7% employment loss relative to 1996. Multiplying this change by baseline QCEW employment in our sample in 1996, which is 85 million, suggests about 1.4 million jobs lost. Table G1 also shows the 95% confidence interval for this estimate of 600 thousand to 2.3 million.

The other cutoffs yield very similar quantitative impacts while going through the same pro-

Table G1: Effect of CTB on Employment, Alternate Treatment Cutoffs

	66 Pctle. Cutoff	75 Pctle. Cutoff	80 Pctle. Cutoff
	(1)	(2)	(3)
CTB Exposure × 1997–2002	-0.0140***	-0.0139***	-0.0155***
	(0.00350)	(0.00362)	(0.00381)
CTB Exposure × Post 2002	-0.0282***	-0.0304***	-0.0220**
	(0.00839)	(0.00908)	(0.0101)
Industry × Year FE	✓	✓	✓
State × Year FE	✓	✓	✓
Population, Density Controls	✓	✓	✓
Sectorial Composition Controls	✓	✓	✓
Trade, Tech, and Tax Controls	✓	✓	✓
Domestic Firm Exposure Control	✓	✓	✓
Observations	1,202,835	1,202,835	1,202,835
Counties	2,976	2,976	2,976
Average Effect (%)	-0.018	-0.017	-0.011
Employment Losses (Millions)	-1.5	-1.4	-1.0
95% Confidence Interval	[-2.4, -0.6]	[-2.3, -0.6]	[-1.8, -0.1]

Notes: Table G1 displays β_1 and β_2 coefficients from regressions of the form

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

where CTB Treatment is defined using the 66th percentile, 75th percentile (our baseline),and the 80th percentile of CTB Exposure. All specifications include the full suite of cross-sectional control variables interacted with year fixed effects and include the full suite of cross-sectional controls in constructing inverse probability weights. Standard errors are clustered at the county-level. *, **, and *** denote statistical significance at the 10, 5, and 1% level. More details are provided in Appendix Section G.1.

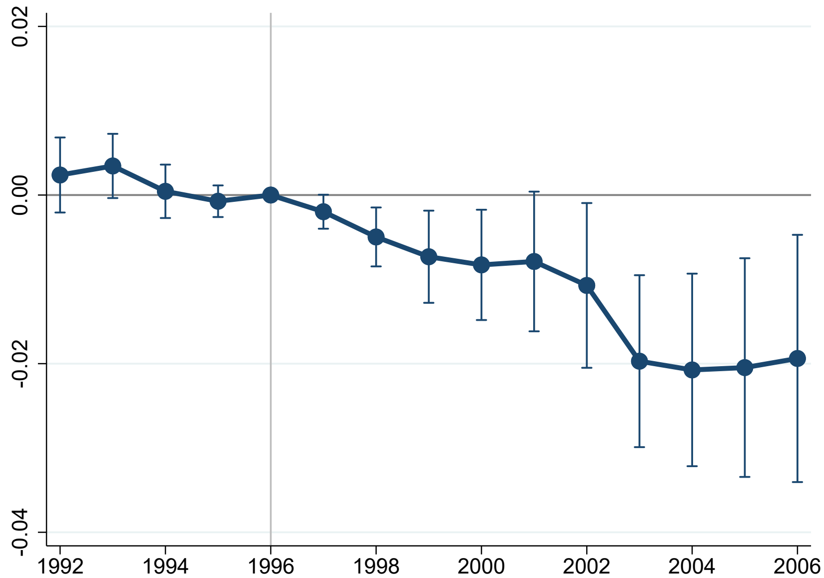
cess. The 66th percentile treatment is associated with a 7.1 pp. increase in CTB exposure, and the 80th percentile treatment is associated with a 8.8 pp. increase. The 66th percentile treatment estimates suggest an average employment decline of 1.8% or 1.5 million workers, slightly larger than the baseline, while the 80th percentile treatment estimates are consistent with a 1.1% decline in employment, or 1 million jobs lost, which is slightly smaller than the baseline estimate. The 95% confidence intervals for the 66th and 80th percentile treatment cutoffs are 600 thousand to 2.4 million and 100 thousand to 1.8 million, respectively.

G.2 Continuous Treatment

In this appendix, we present dynamic DD estimates of the effect of continuous CTB Treatment on local employment. Figure J1 presents the dynamic DD estimates where treatment is defined as the share of workers in a county working for MNCs in 1996. The regression includes the full suite of cross-sectional controls interacted with year fixed effects as well as industry-by-year and state-by-density-by-year fixed effects. Because the treatment is no longer binary, the regression is weighted by 1996 county-industry employment rather than with inverse probability weights.

We continue to see no effect of repatriations on domestic labor markets.

Figure G1: Effect of CTB on Employment; Continuous Treatment



Notes: Figure 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 4. 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.

G.3 Offshorability

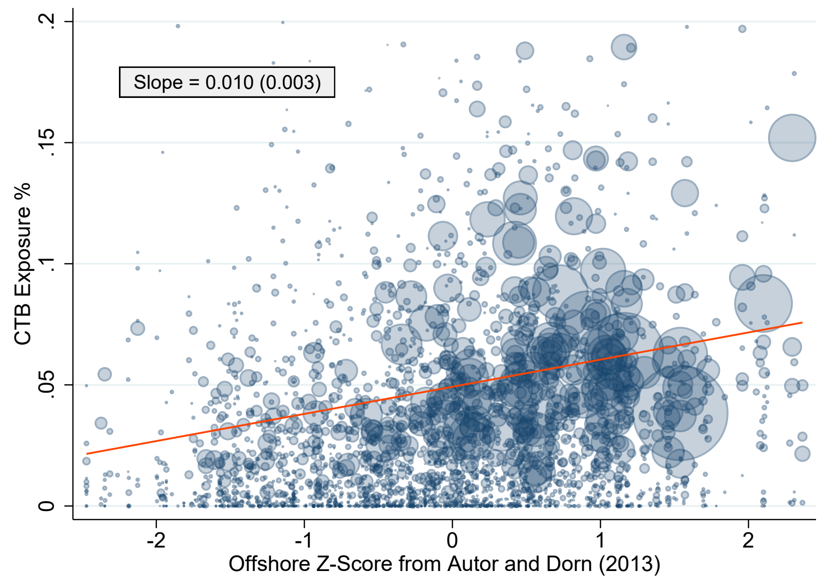
In this Appendix, we highlight the importance of one particular potential confounder: local labor "offshorability." Throughout the 1990s, there were massive technological changes, including the privatization and expansion of what became known as the internet in the US (Jiang, 2023), that may have made offshoring jobs easier in a technical sense that had nothing to do with US or other tax policies. Offshorability is a characteristic of labor that determines the technical substitutability of a worker. We use the definition of offshorability available from Autor and Dorn (2013), which is described as the following:

> We follow the standard approach in the literature of measuring the offshoring *potential* ("offshorability") of job tasks rather than the actual offshoring that occurs. To operationalize offshorability, we use a simple average of the two variables *Face-to-Face Contact* and *On-Site Job* that Firpo et al. (2011) derive from the US Department of Labor's Occupational Information Network database (O*NET). This measure captures the degree to which an occupation requires either direct interpersonal interaction or proximity to a specific work location.

Figure G2 shows the correlation between technical labor offshorability and exposure to CTB from exposure to US MNCs. This correlation is positive and a regression coefficient yields a slope of 0.01, statistically significant at the 1% level. A 1 standard deviation increase in offshorability is associated with a 1 pp. increase in CTB Exposure. Therefore, we note that including the offshorability control, and providing heterogeneity in treatment effects by offshorability is important to support the causal interpretation to our baseline CTB estimates. We do not find heterogeneous impacts of CTB by local offshorability of jobs, which supports the conclusion that CTB has a distinct impact from offshorability in labor market outcomes.

Figure G2: CTB Exposure and Offshoring Risk

(A) Correlation Between CTB Exposure and Offshore Z-Score



Notes: Figure 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 4. 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.

H Migration and employment-to-population ratios

One concern with a reduced-form analysis of a shock that has differential impact in the cross-section is that the observed impact on treated units could represent a reallocation between treated and control units. This is a specific violation of the stable unit treatment value assumption—generally referred to as SUTVA. For the sake of this appendix, we omit time subscripts and focus on the 2003-2006 impact of CTB from the pooled difference in differences model.

First, we introduce a two county example to highlight how this violation may manifest in the data using our variable definitions. Assume that one county is treated and the other county is a control county. Let Emp_{treat} be employment and $Emp_{treat,0}$ be the original employment in 1996 in a treated county. The main outcome variable measurement is simply

$$\Delta Emp_{treat} = \frac{Emp_{treat} - Emp_{treat,0}}{Emp_{treat,0}}.$$

Now, assume that some number of workers m migrate from the treatment county to the control county as MNCs in the treated county adjust domestic activity. Our method only observes Emp_{treat}, but it can be decomposed into the $\tilde{Emp}_{treat}$ jobs for workers who do not migrate, and the m lost jobs due to individual migration. For the treated county, our measurement of worker outcomes captures $\Delta Emp_{treat} = \frac{\tilde{Emp}_{treat} - m - Emp_{treat,0}}{Emp_{treat,0}}$, and it is the analogue in a control county: $\Delta Emp_{control} = \frac{\tilde{Emp}_{control} + m - Emp_{control,0}}{Emp_{control,0}}$. Taking the derivative of the difference between these quantities shows that an additional migrant from the treatment to the control has a predictable impact on the difference that we are interested in describing:

$$\frac{d}{dm}\left(\frac{\tilde{E}_{treat} - m - E_{treat,0}}{E_{treat,0}} - \frac{\tilde{E} + m - E_{control,0}}{E_{control,0}}\right) = \frac{-1}{E_{treat,0}} - \frac{1}{E_{control,0}}.$$

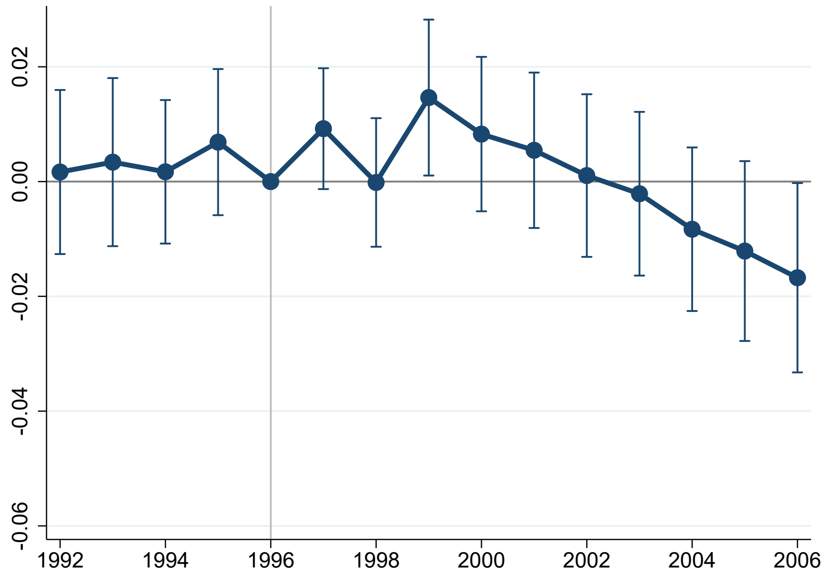
In this stylized example, we find that the impact of a migrant on our difference-in-differences estimator will be larger than that of an unemployed worker because the migrant moves into the worker sample of the control county. If the baseline employment is constant, the migration impact will be exactly twice as large as the impact of a newly unemployed worker.

We use data on worker *out*-migration through the IRS Statistics of Income to get unbiased estimates of m at an annual frequency.[50] These data allow us to measure the number of workers leaving a county without incidentally measuring the in-migration into control counties. We define a new out-migration variable as $\Delta m_{cjt} \equiv \frac{m_{cjt} - m_{cj1996}}{m_{cj1996}}$. Using the baseline county-level event study difference-in-differences specification, we estimate whether out-migration in treated counties is changing around the implementation of CTB. Estimates are shown in Figure H1 using the controls from the employment specifications. The observations are aggregated to the county level, so we are not able to include industry-by-year FE.

An emigrant from treated counties only shows up as a single migrant in this specification as immigrants to control counties are not included, so there is no potential SUTVA violation for this outcome. The point estimates are statistically insignificant at conventional levels in all years except 1999. The 1999 point estimate of 0.0146 implies that there was a 1.46% increase in out-migration from treated counties on average in 1999 relative to 1996. The average out-migration in treated counties in 1996 was 3,471 (about 6% of the total labor force), so treated counties experienced an additional out-migration of 51 workers in 1999 relative to what would have been

[50] IRS SOI report internal migration of tax returns since 1990 at the county level, which allows researchers to observe out-migration and in-migration separately.

Figure H1: Effect of Check-the-Box on Domestic Out-Migration



Notes: Figure H1 shows dynamic difference-in-differences estimates of the impact of CTB exposure on outmigration of tax filers as measured in the IRS SOI data. *Source:* Authors' calculations based on QCEW, NETS, and IRS SOI. Data defintions are available in Appendix A.

expected from the outcome in control counties.

Out migration is a flow and, ultimately, we care about the total impact of out-migration on employment (the stock). Summing all of the point estimates from 1997 to 2006 yields an estimate on net out-migration of -0.001, or -0.1%, which would imply that out-migration actually decreased in treatment counties relative to control counties although the impact is not statistically distinguishable from zero. If we instead take a more conservative approach to measuring the potential for migration to impact our main results by accentuating the impact on migration, we sum only the positive coefficients from 1997-2002 (omitting the negative coefficient in 1998), and find an aggregate impact on out-migration of 3.8% of 1996 migration. This impact amounts to 132 workers ($0.038 \times 3,471$) migrating out of treatment counties by omitting all of the negative annual coefficients.

Our baseline estimate is that treatment counties lost 1,864 jobs relative to control counties by 2003-06. The migration measurement only including positive coefficients suggests 132 of these lost jobs were actually migrants, and if we assume that all of these migrants went to control counties then our estimates could be lowered to 1,600 workers ($1,864 - (132 \times 2)$). This treatment effect is attenuated by 14% relative to our baseline estimate, suggesting that out migration is not a quantitatively large margin of adjustment even if we ignore the years where treated counties experienced negative differential out-migration.

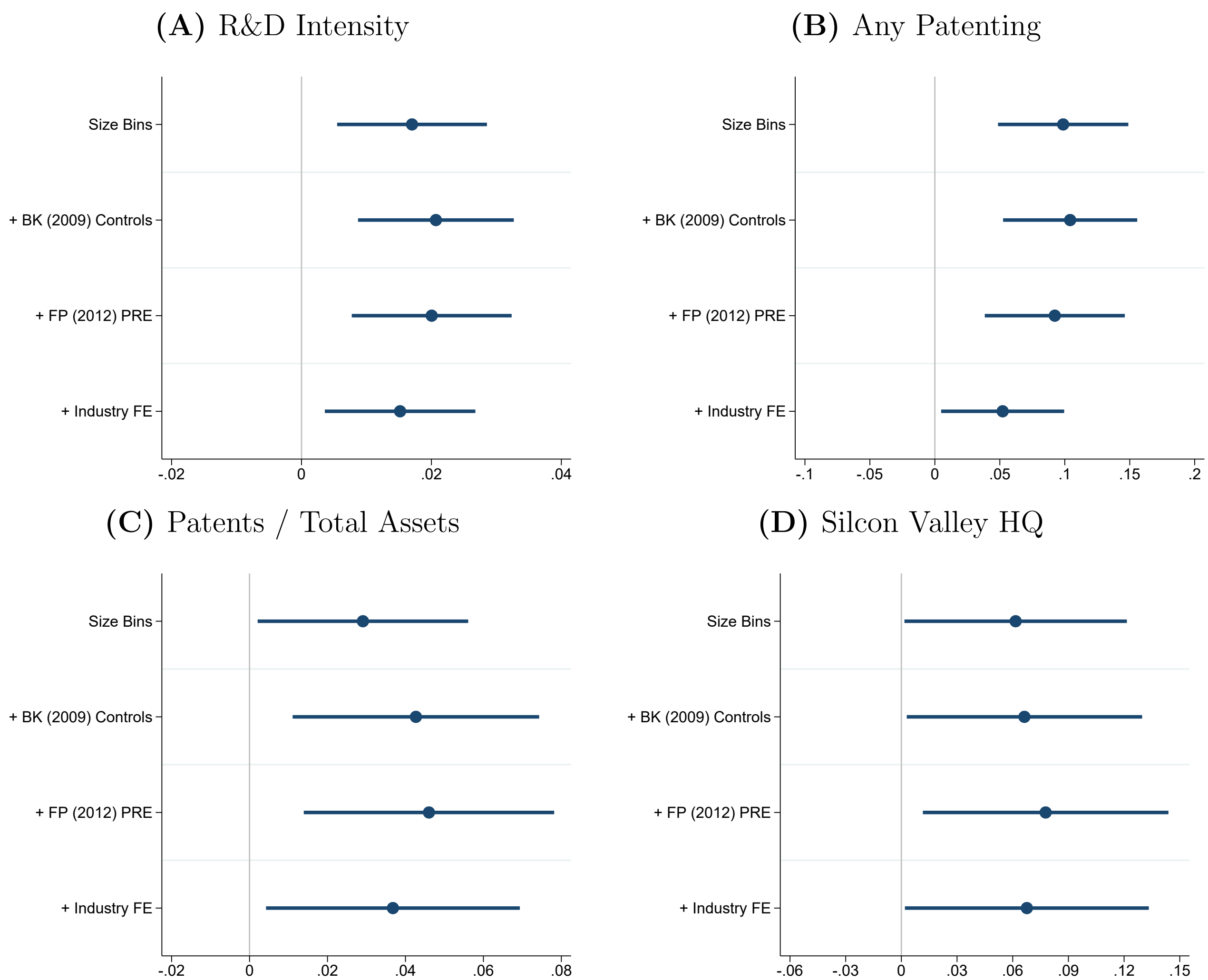
I Repatriations and Innovative MNCs

This appendix documents that innovative firms were more likely to repatriate under the RH, even conditional on other repatriation predictors already established in the literature. We run cross-sectional regressions of an indicator equal to one for firms that repatriated under the holiday on measures of innovative activity. We present the results in Figure I1. Each panel shows coefficients and 95% confidence intervals from four different regressions focusing on a single measure of innovative activity. The first regression in each panel includes the measure of innovative activity and ten firm size bin dummies. The second regression adds the Blouin and Krull (2009) determinants of repatriations. The third regression adds the Faulkender and Petersen (2012) measure of PRE. The fourth regression includes 2-digit NAICS fixed effects.

Panel (A) shows the effect of R&D intensity, defined as R&D expenditures scaled by sales, on the repatriator indicator. Across all four specifications, the coefficients on R&D intensity are positive and statistically significant indicating that firms with high R&D intensity are more likely to repatriate under the holiday controlling for other known determinants of repatriation behavior. Panel (B) shows qualitatively similar results for firms that have any patenting before 2004 where firm-level patenting data comes from Autor et al. (2020). In Panels (C) and (D), the measures of innovative activity are patents awarded per $10 million in total assets and whether the NETS data lists the firm HQ as being in Silicon Valley (defined as Santa Clara, San Jose, Alameda, and San Mateo counties).

Across all four of these measures of innovative activity, we find that more innovative firms were more likely to repatriate funds under the RH conditional on firm size, industry, and other previously established predictors of repatriations.

Figure I1: Repatriators and Innovative Activity



Notes: Figure I1 explores the relationship between the repatriation decision and measures of innovative activity. Each panel displays the coefficient on a repatriator dummy from four different cross-sectional regression. In the first the outcome is regressed on the repatriator dummy and ten firm size bins. The second regression adds predictors of the repatriation decision from Blouin and Krull (2009). The third includes adds log of permanently reinvested earnings from Faulkender and Petersen (2012). The fourth adds 2-digit NAICS fixed effects. The outcome variables in panels (A)–(D) are R&D Intensity, which we define as R&D expenditure dividend by revenue, an indicator for non-zero patents awarded by 2004, the number of patents awarded by 2004 per $10 million in total assets, and an indicator equal to one for MNCs with headquarters located in Silicon Valley, California.

J Repatriation Holiday, Extended Results

This appendix shows that the effects of the repatriation holiday on domestic employment are robust to a number of specification choices.

Table J1 shows additional specifications of the repatriation holiday DD results. In columns (1) through (4) of Table J1, we show that the staggered inclusion of population and demographic controls, local sectoral composition controls, trade, technology, and tax controls, as well as non-repatriating MNC exposure controls has no impact on our employment coefficients. The REPAT Treat $\times$ Post 2006 coefficients for these specifications range from -0.0008 to -0.004, all of which are statistically insignificant. The set of included controls does not have a material impact on the coefficients.

Second, in columns (5) and (6) of the same table, we show the robustness of the null result to the choice of treatment cutoff. In Column (5), we use a treatment cutoff at the 40th percentile, which is \$142 repatriated per worker in a county. This treatment definition now has a median gap between treatment and control of \$462 instead of \$569 in the baseline. The difference-in-differences coefficient with this treatment flips signs, but has a similarly tiny magnitude and is not statistically distinguishable from zero. Column (6) does the same test on a treatment defined for counties with over \$335 repatriated per worker, which implies a treatment at the median of \$697 additional repatriations per worker. Again, we fail to find any statistically significant impact of repatriations on local employment.

Figure J1 presents dynamic DD estimates showing the effect of repatriations on local employment using continuous variation in REPAT Treatment (thousands of dollars repatriated per worker in a county). The regression includes the full suite of cross-sectional controls interacted with year fixed effects as well as industry-by-year and state-by-density-by-year fixed effects. Because the treatment is no longer binary, the regression is weighted by 2003 county-industry employment rather than with inverse probability weights. We continue to see no effect of repatriations on domestic labor markets.

Table J1: Effects of Repatriations on Domestic Employment, Alternative Specifications

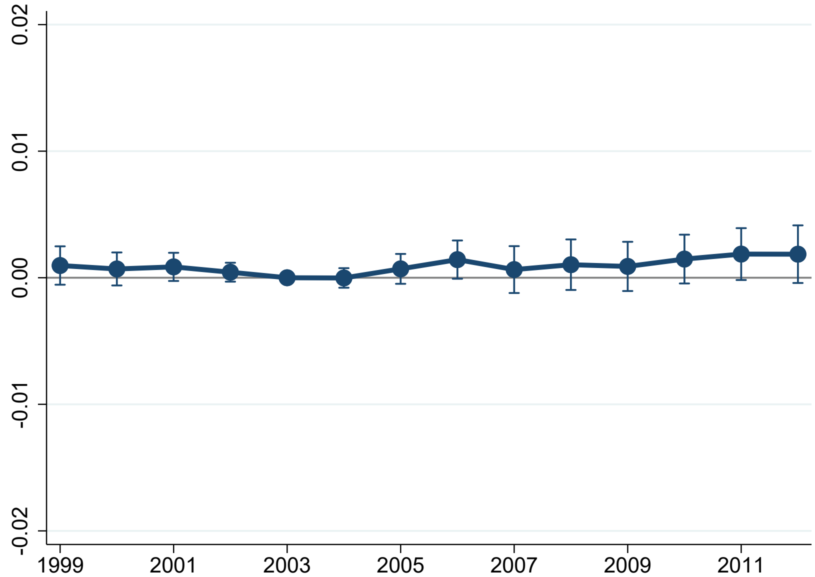
	50th Percentile Cutoff				40 Pctle. Cutoff	60 Pctle. Cutoff
	(1)	(2)	(3)	(4)	(5)	(6)
REPAT Exposure × 2004–2006	-0.00153	-0.000321	-0.00120	-0.00123	0.000772	0.000192
	(0.00275)	(0.00273)	(0.00295)	(0.00296)	(0.00299)	(0.00301)
REPAT Exposure × Post 2006	-0.000888	-0.000825	-0.00394	-0.00393	0.000869	0.00305
	(0.00529)	(0.00561)	(0.00564)	(0.00564)	(0.00577)	(0.00566)
Industry × Year FE	✓	✓	✓	✓	✓	✓
State × Year FE	✓	✓	✓	✓	✓	✓
Population, Demographic Controls	✓	✓	✓	✓	✓	✓
Sectoral Composition Controls		✓	✓	✓	✓	✓
Trade, Tech, and Tax Controls			✓	✓	✓	✓
MNC Exposure Control				✓	✓	✓
Observations	1,132,180	1,132,180	1,132,180	1,132,180	1,132,180	1,132,180

Notes: Table J1 displays β_1 and β_2 coefficients from regressions of the form

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

The outcome variable in all regressions is the percentage point change in county-industry employment relative to 1996. REPAT Treatment is defined as above median REPAT Exposure in Specifications (1)–(4) and as above the 40th percentile and 60th percentile of REPAT Exposure in Specifications (5) and (6). All specifications include industry-by-year fixed effects and state-by-year fixed effects. Specifications (1)–(4) progressively add sectoral composition controls, population and demographic controls, and trade, technology, and tax controls to both the IPW weighting construction and as cross-sectional controls interacted with time period fixed effects in the regressions. Specifications (5) and (6) use the full suite of cross-sectional controls in constructing the inverse probability weights and include the full suite of cross-sectional controls interacted with time period fixed effects in the regressions. Standard errors are clustered at the county-level in all specifications. *, **, and *** denote statistical significance at the 10, 5, and 1% level.

Figure J1: Effect of REPAT on Employment; Continuous Treatment



Notes: Figure J1 displays β coefficients and 95% confidence intervals from regressions in the form of Equation (5) which describe the effect of county-level continuous RH Treatment on the percent change in employment relative to 2003. The specification includes the full suite of cross-sectional controls interacted with year fixed effects in addition to industry-by-year and state-density-year fixed effects. The regression is weighted by 2003 employment. Standard errors are clustered at the county level.