

Bilicka, Katarzyna

Working Paper

Labor market consequences of antitax avoidance policies

Upjohn Institute Working Paper, No. 21-354

Provided in Cooperation with:

W. E. Upjohn Institute for Employment Research, Kalamazoo, Mich.

Suggested Citation: Bilicka, Katarzyna (2021) : Labor market consequences of antitax avoidance policies, Upjohn Institute Working Paper, No. 21-354, W.E. Upjohn Institute for Employment Research, Kalamazoo, MI,
<https://doi.org/10.17848/wp21-354>

This Version is available at:

<https://hdl.handle.net/10419/262382>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

10-21-2021

Labor Market Consequences of Antitax Avoidance Policies

Katarzyna Bilicka

Utah State University, NBER, and CEPR, kat.bilicka@usu.edu

Upjohn Institute working paper ; 21-354

Follow this and additional works at: https://research.upjohn.org/up_workingpapers



Part of the [Labor Economics Commons](#)

Citation

Bilicka, Katarzyna. 2021. "Labor Market Consequences of Antitax Avoidance Policies." Upjohn Institute Working Paper 21-354. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research. <https://doi.org/10.17848/wp21-354>

This title is brought to you by the Upjohn Institute. For more information, please contact repository@upjohn.org.

Labor Market Consequences of Antitax Avoidance Policies

Upjohn Institute Working Paper 21-354

Katarzyna Bilicka
Utah State University, NBER, and CEPR
Email: kat.bilicka@usu.edu

October 2021

ABSTRACT

In this paper, I analyze the local labor market consequences of multinational firms reallocating employees across their affiliates in response to antitax avoidance policies. I leverage the introduction of a worldwide debt cap in 2010 in the United Kingdom as a quasi-natural experiment that limited one of the forms of profit shifting—debt shifting—for a group of multinational corporations (MNCs). Multinationals affected by the reform reallocated their employees from the United Kingdom to foreign locations. This affected London-based service sector firms the most. I show that this led to a reduction in the number of jobs available in regions exposed to the reform in the United Kingdom. In foreign countries, the initial reallocation of labor across firms resulted in a much larger expansion of the affected local labor markets. These results suggest that a reallocation of labor across firms generates asymmetries in how negative and positive firm-level shocks are amplified through regional markets.

JEL Classification Codes: H25, H26

Key Words: Debt shifting, multinational companies, local labor markets

Acknowledgments: I acknowledge generous financial support from the Upjohn Institute Early Career Research Award. I am grateful for all the comments I received from participants at the MannheimTax, RIDGE Public Economics workshops. All omissions and errors are my own.

1 Introduction

Multinational tax avoidance has been a subject of political discussion in recent years, as there is growing academic evidence on how little tax multinational corporations (MNCs) pay (Bilicka, 2019; Torslov et al., 2018). The political pressure has been exacerbated by the revelations from Panama and Paradise papers in 2015 that exposed details of some of the tax avoidance schemes to the public. This spurred some countries to introduce stricter rules to limit the extent of tax avoidance by MNCs. However, such restrictions in practice affect not only profit-shifting activities, but also result in reallocation of real activities, such as investment and employment, away from countries introducing those rules (Bilicka et al., 2020; Serrato, 2018). Hence, these reallocations may affect long-run regional employment and growth and exacerbate regional inequalities. As such, it is critical to understand whether and how such restrictions affect local domestic markets.

In this paper, I analyze the effects of an antitax avoidance regulation on employment in regions where MNCs affected by the regulation are located. To provide causal evidence, I leverage the introduction of the worldwide debt cap rule (WDC) in the United Kingdom in 2010 as a natural experiment. The WDC was aimed at tackling debt shifting by MNCs by setting up a maximum ratio of debt allowed to be held in the United Kingdom relative to the overall debt for each MNC. Interest expenses above the so-called gateway ratio were disallowed for deductibility purposes, substantially increasing the cost of capital in the United Kingdom for MNCs that failed the gateway test. These worldwide antitax avoidance measures are becoming more prominent policy tools, with the United States implementing similar restrictions in the Tax Cuts and Jobs Act in December 2017. Bilicka et al. (2020) show that although the WDC reduced excessive borrowing in the United Kingdom, it led to debt shifting toward foreign subsidiaries. This reallocation of debt was followed by the reallocation of real business activities. Affected MNCs shrank the size of total assets, fixed assets, and employment they held in the United Kingdom, while expanding elsewhere.

I take advantage of the same reform but focus on the local labor market implications of the employment reallocation from the United Kingdom to foreign countries that resulted from this antitax avoidance restriction. Consistent with Slemrod (1992) hierarchy of behavioral responses, when firms can easily shift profits between jurisdictions, they respond to regulations moving paper profits. However, if a regulation prohibits them from doing so, their accounting responses may result in reallocations of real activities. This is what Bilicka et al. (2020) find for the WDC in the United Kingdom, suggesting the effectiveness of that

regulation in limiting profit shifting. From a policy perspective, shifts in the allocation of debt and consequently profits between subsidiaries of MNCs matter for tax revenue collection across countries and regions. This effect could be exacerbated if the reallocation of debt generates distortions in the firm’s real business activities, especially employment. If MNCs employ a large part of the population in a particular region, this may disproportionately affect local employment levels and have spillover effects for regional growth.

The analysis proceeds in two steps. First, I show how the WDC affects firm-level employment across subsidiaries belonging to the affected MNCs. This effectively replicates Bilicka et al. (2020), focusing on the longer panel of employment data. I show that affected MNCs reduced employment in the United Kingdom by 8.5 percent and increased employment in their foreign subsidiaries by 8.6 percent between 2010 and 2018. These estimates are larger than in Bilicka et al. (2020) suggesting the long-run effects of this reform on the employment of affected MNCs. I also show an increase in domestic employment in the United Kingdom, but this is not concentrated in regions more exposed to the WDC. This suggests no substitution toward domestic labor, at least locally. To complement these findings, I explore the regional and industrial variation that the data offer. I show a large decline in employment in London and a substantial heterogeneity in local labor market responses across U.K. counties. The large labor decline in London is concentrated among service firms. This is consistent with a potentially high mobility of employees in service sectors and as a consequence a lower cost of reallocation or rehiring new employees.

In the second part of the paper, I focus on the regional implications of this employment reallocation. To do so, I use the location of MNCs subsidiaries in the United Kingdom and in foreign countries, assign firms to county-city regions, and match these with regional jobs data from Eurostat. This means that the foreign implications presented here are only relevant for European Union countries for which I have data.¹ Treated regions are those that have at least one MNC that failed the gateway test and reported employment in their financial statement.² For the remainder of the paper, I refer to those treated regions, as exposed regions. First, I use regional employment statistics and show that the number of jobs available in exposed regions in the United Kingdom declined following the WDC. I find similar increase in unemployment rate and reduction in regional GDP growth. Second, within

¹The list of countries is as follows: Austria, Belgium, Bulgaria, Germany, Estonia, Spain, Finland, France, Greece, Hungary, Ireland, Luxembourg, Latvia, Malta, Netherlands, Norway, Poland, Portugal, Sweden, Slovenia, Slovakia. They are also graphed in Figure A3.

²As an alternative treatment group, I use regions where at least one MNC failed the gateway test *and* reduced their employment in the United Kingdom as a result.

each region, I aggregate the firm-level employment data and show that in exposed regions the total employment by MNCs declined relative to regions not exposed to WDC. This complements the results using regional data. In placebo tests, I show no effects for regions that were simply exposed to MNCs in general. In turn, in foreign European countries, in regions exposed to WDC, I find an increase in the number of jobs available. These results suggest that the reallocation of labor by MNCs that employ a large proportion of the population locally affects local labor markets by reducing the number of jobs available and creating unemployment.

The size of the local labor market effect abroad I estimate is larger than the one in the United Kingdom. This occurs for three reasons. First, as MNCs expand in the foreign locations where they reallocate employment, local labor markets can experience productivity gains through supply-chain linkages (Alfaro and Chen, 2018; Javorcik, 2004). This may affect local domestic firms as well, increasing local labor market size and reducing unemployment. Note that I do not have sufficient prereform data to investigate the domestic labor effects in foreign countries. Second, the initial decline in firm-level employment by MNCs in the United Kingdom persist until 2015, after which I observe an increase. This suggests that the negative shock is not permanent and that affected firms eventually offset the effects of WDC. Third, in the United Kingdom, I also find that domestic firms increased the number of people they employed, but not necessarily in the regions exposed to WDC. This suggests that domestic firms did not benefit from this negative shock by substituting labor locally, unlike in Desai et al. (2009). However, it is entirely plausible that domestic labor substitution occurred in regions not exposed to WDC; that is, not locally.

This paper provides a new contribution to the literature on the effects of antitax avoidance restrictions on local labor markets. Two papers closest to this one are Bilicka et al. (2020) and Serrato (2018), the former of which analyzes the effects of the WDC reform for employment reallocation at the firm level. They stop short of analyzing the consequences of these reallocations for the local labor markets. Serrato (2018) shows that the repeal of a tax code that allowed U.S. MNCs to exclude income from Puerto Rico from U.S. corporate taxes led them to shift investment and employment away from the United States. He uses regional unemployment data for the United States to show that firm-level responses affected local labor markets that were more exposed to the reform. My results differ in three dimensions. First, the policy I analyze is a more general regulation adopted by other countries to counteract profit shifting. For example, in 2017 the U.S. Tax Cuts and Jobs Act included provisions to limit interest deductibility of MNCs, while in 2019, the European Commission

recommended the implementation of the Anti-Tax Avoidance Directive, which sets similar interest deductibility limitations.³ Thus, my findings are likely to have broader policy implications for European and U.S. policy, as more countries are adopting such regulations. Second, the U.S. reform applied to a *domestic* tax haven, thus it is very specific in its nature. The WDC applied to profit shifting globally. Third, unlike Serrato (2018), whose analysis is restricted to considering the effect of antitax avoidance regulation in a country that implemented this regulation, I consider the implications to foreign countries as well. Thus, I can show that a reduction in employment in a U.K. subsidiary of a multinational that has local labor market implications in the United Kingdom generates a much larger increase in employment in a local labor market where another subsidiary of that firm is located. This suggests a reallocation of labor across markets and asymmetries in how negative and positive shocks are amplified through regional markets. This evidence is consistent with Giroud and Mueller (2019), who show similar shock propagation to local labor markets, but within the United States and using consumer demand shocks.

More broadly, this paper adds to several strands of literature. First, there is evidence that profit shifting elicits real responses by MNCs, especially in terms of investment (Becker and Riedel, 2012; Egger and Wamser, 2015; Grubert and Slemrod, 1998; Mintz and Smart, 2004). While Desai et al. (2009) show that domestic and foreign investment are complements, Kovak et al. (2017) show that this does not necessarily translate to employment. These results focus on firm-level implications, while the present paper addresses local labor market consequences of such real responses. Second, I add to a growing literature that examines how MNCs respond to economic and policy shocks and how these shocks propagate across their subsidiaries, affecting the local and global economy (Almedia et al., 2015; Biermann, 2019; Boutin et al., 2013; Desai et al., 2007; Garrett et al., 2020; Giroud and Mueller, 2015, 2016, 2019; Huber, 2018; Kalemli-Ozcan et al., 2016; Santioni et al., 2017). The literature on shock propagation focuses on how shocks to one establishment, through the firm network, affect the rest of the firm. The shocks that this literature analyzes are often linked to consumer demand (Giroud and Mueller, 2019) or investment opportunities (Giroud and Mueller, 2015). This literature does not consider the implications of antitax avoidance restrictions and how they propagate across firm subsidiaries. Third, this paper also relates to a large body of trade literature on spillover effects of multinationals on domestic markets and producers. The evidence from the literature is mixed, with Harrison and Aitken (1999) and Lu et al.

³For more details see: EU commission https://ec.europa.eu/taxation_customs/business/company-tax/anti-tax-avoidance-package/anti-tax-avoidance-directive_en.

(2017) finding negative effects, while Javorcik (2004), Haskel et al. (2007), Alfaro and Chen (2018), Keller and Yeaple (2009), Figlio and Blonigen (2000), Kee (2015), and many others finding positive effects. More recently, Setzler and Tintelnot (2019) show positive spillover effects of foreign MNCs on wages of domestic firms. My paper is related to those findings as I show that negative shocks to MNCs affect local labor markets positively, while I find no support that expansion of real operations by affected MNCs abroad has any significant spillover effects on domestic firms' employment.

2 Policy Context

Profit shifting has been at the forefront of political debate, as countries try to curb the ability of MNCs to move profits away from high-tax to low-tax jurisdictions. The three most popular profit-shifting methods include debt shifting (Desai et al., 2004; Huizinga et al., 2008), transfer pricing, (Cristea and Nguyen, 2016; Davies et al., 2018) and location of patents in low-tax jurisdictions (Dischinger and Riedel, 2011). Debt shifting relies on subsidiaries of MNCs located in low-tax countries lending money internally to subsidiaries located in high tax-countries and using interest deductibility to reduce taxable profits in high-tax countries. Transfer pricing relies on mispricing of goods when traded internally so that their costs can be written off against profits in high-tax countries. Location of patents in low-tax countries relies on firms' ability to lease those patents and pay royalties to low-tax countries at the same time reducing their taxable profits in high-tax countries.

Many countries already have restrictions in place to limit the extent to which MNCs can use those various profit-shifting strategies. For example, thin-capitalization rules set up a fixed ratio, such as the debt-to-equity ratio or the interest coverage ratio, and interest expense associated with debt exceeding the ratio is often disallowed for a tax deduction. These rules consider each subsidiary of an MNC separately, and despite being shown to be effective at reducing debt shifting (Blouin et al., 2014; Buettner et al., 2012), they have been widely criticized, as firms are able to circumvent those rules easily. Transfer pricing restrictions exist too, with countries agreeing to uphold "arms-pricing" restrictions, where firms have to show that the goods they trade internally have a price at which they would trade on the external market. Finally, patent location and the use of tax havens is being restricted by Controlled Foreign Company rules (Clifford, 2019).

In January 2010, the U.K. tax authority (the HMRC) introduced the WDC to restrict the generous tax deductions for financing expenses enjoyed by MNCs in the United King-

dom. These new rules were meant to complement the use of thin-capitalization rules. This worldwide approach evaluates the MNCs’ allocation of debt across affiliates by comparing the amount of debt located in each host country to a worldwide consolidated benchmark. In case of the United Kingdom, this benchmark was set to be worldwide debt in 2010. This was replaced in April 2017 by Earnings before interest tax and depreciation (EBITDA). These rules are likely to be more effective, given that the entire structure of MNCs debt needs to be adjusted to circumvent those (Desai and Dharmapala, 2015; Dharmapala, 2014).

The WDC applied to “relevant” MNCs with a corporate tax residence in the United Kingdom. The relevance was determined by ownership status, and only affiliates that were owned by more than 75 percent were affected by the reform. Further, the MNCs subject to this reform had to have more than 250 employees, above €50m turnover, and/or above €43m balance sheet total assets. As such, the affected firms were large and employed a substantial number of people in the United Kingdom. The WDC required MNCs to calculate its net U.K. debt across all of their “relevant” subsidiaries and divide that by worldwide gross debt. If the ratio exceeded 75 percent, the interest deduction was disallowed for the exceeding level of interest expenses. Bilicka et al. (2020) discuss in detail the particular types of liabilities and assets that form the net U.K. debt.

The timing of the WDC coincides with the introduction of territorial tax reform in the United Kingdom in 2009. This reform exempts dividend repatriation by MNCs from being taxed in the United Kingdom and has been shown to increase dividend repatriation (Egger et al., 2015), payouts to shareholders (Arena and Kutner, 2015), and shifting profits to low tax countries (Langenmayr and Liu, 2020). Further, U.K. government introduced a package of statutory corporate tax rate cuts that reduced its tax rate to 20 percent in 2015 and to 19 percent by 2018. Bilicka et al. (2020) already carefully show that these two reforms did not affect firms that failed the gateway test differently; hence, I do not focus on the confounding effects of those reforms in this paper.

3 Data and Methodology

3.1 Data

To examine the effects of the WDC on labor markets, I use the dataset from Bilicka et al. (2020) and complement it with information on the location of multinational affiliates and hand-collected regional jobs and unemployment data. The firm-level data with the ownership

and financial information come from Bureau van Dijk (BvD) Osiris matched with Orbis for non-U.K. financials and FAME for U.K. financials. I use FAME data for the U.K. portion of this study, as FAME offers much more detailed financial information that allows me to construct the gateway test ratios for firms with affiliates in the United Kingdom following the HMRC guidance exactly. I use these gateway test ratios to construct treatment and control groups and then match into the parent and foreign subsidiary information for firms from the ORBIS data. Combining these data sources together, the benchmark sample covers financial data for MNCs, both at the group level and the subsidiary level, during the period 2007–2018.

The regional U.K. data is at the county-city level (NUTS3 regions) and come from Eurostat. I have information on population, employment, GDP, number of jobs, and unemployment rate at the county-city level, such as Manchester, Aberdeen, or Bristol and their surrounding regions. Figure A1 in the Appendix shows distribution and borders of county-cities in the United Kingdom. Since the unemployment data come from Eurostat, my sample is limited to analyzing the local labor market implications for the United Kingdom and for foreign labor markets located in Europe.⁴ I test the validity of the conclusions offered by the regional data by aggregating firm-level data at the county-city levels for the sample where I have Eurostat data and for the whole sample of firms from Orbis. In this paper, I focus on three variables in particular: number of jobs, unemployment rate, and regional GDP. Note that for some cities, data were not collected in all years. In those cases, I fill in the data by calculating an average of the surrounding time periods. For example, if employment in Aberdeen was 85,000 in 2008 and 90,000 in 2011, I calculate it to be 87,500 in both 2009 and 2010.

3.2 Empirical strategy

Firm level The empirical analysis proceeds in two steps. First, I analyze the firm-level effect of the reform on employment of affected MNCs in a difference-in-differences framework. MNCs that failed the gateway test in 2010 are in the treated group, while those that passed the test are in the control group. This replicates Bilicka et al. (2020), using a longer postreform time series and using affiliate-level information instead of aggregating at

⁴The following countries have county-city employment data in Eurostat: Austria, Belgium, Bulgaria, Germany, Estonia, Spain, Finland, France, Greece, Hungary, Ireland, Luxembourg, Latvia, Malta, Netherlands, Norway, Poland, Portugal, Sweden, Slovenia, Slovakia, United Kingdom.

the MNC level.⁵ I estimate the following model:

$$Y_{i,j,s,t} = \alpha + \beta \times Failed_i \times Post_t + \psi_t + \kappa_i + \epsilon_{i,j,t}, \quad (1)$$

where $Y_{i,j,s,t}$ is employment in subsidiary j that belongs to multinational i , located in host country s , in year t . $Failed_i$ is a dummy variable that equals 1 if MNC i failed the gateway test in 2010 and 0 otherwise, $Post_t$ is a dummy variable that equals 1 from 2010 onward, ψ_t is the time fixed effect, κ_i is the subsidiary fixed effect, and $\epsilon_{i,j,t}$ is the error term. The parameter of interest is β , which captures the effect of the WDC on MNCs' employment. In the empirical analysis I distinguish between U.K. subsidiaries and foreign subsidiaries, as we would expect the effects to be opposite for those two types of affiliates.

Regional level Second, I use regional statistics to understand the effect of WDC on regional employment. I distinguish between two types of regions that theoretically should have been more affected by the reform: 1) regions where a subsidiary of an MNC's firm that failed the gateway test is located and it reports employment numbers, 2) regions where such MNCs also reduced their employment in the United Kingdom following the WDC. In the main part of the paper, I focus on the first identification strategy but show corresponding results from the second strategy in the Appendix. Further, I use placebo regions with more employment by MNCs to see whether the effects I observe could simply be the effect of exposure to MNCs. As such, I estimate the following models:

$$Y_{k,t} = \alpha + \beta Post_t \times Region_k + \psi_t + \mu_k + \epsilon_{i,j,s,t} \quad (2)$$

where $Y_{k,t}$ is employment in region k , in year t . $Post_t$ is a dummy variable that equals one from 2010 onward; $Region_k$ is a dummy equal to 1 for treated regions; i.e. those with more exposure to firms that failed the gateway test. ψ_t is the time fixed effect, μ_k is region fixed effect, and $\epsilon_{k,t}$ is the error term. The parameter of interest is β , which captures the effect of the WDC employment in regions more exposed to WDC. As a validation, I also aggregate firm-level data at the regional level by calculating the total number of employees for MNCs in each city and run the same set of regional regression as outlined in Equation (2).

⁵I need to use affiliate level information here, because some MNCs have multiple subsidiaries in the United Kingdom and I match their location to the regional unemployment data.

3.3 Sample description

For the regional analysis to be valid, we require the exposed regions to have similar observable characteristics to the regions that were not exposed to WDC prior to the reform. Table 1 shows regional descriptive statistics using the main identification strategy; in Panel A, I compare regions that were exposed to WDC and those that were not in the United Kingdom; in Panel B, I do the same for foreign regions. We can see that there is no statistically significant difference between regions that were exposed to WDC and those that were not in terms of population, number of employed people, activity rate, unemployment rate and regional GDP in the United Kingdom. In Figure A1, I show the distribution of intensity of exposure to WDC by region, and in Figure A2, I include a map of all regions in the United Kingdom for which data for regional employment is collected, how many people were employed in each region and the unemployment rate. The list of regions with available data is included in Table A3 in the Appendix.

For foreign regions, there is no difference between regions that were exposed to WDC and those that were not in terms of population, number of employed people and regional GDP prior to WDC. Regions that were exposed to the WDC had higher activity rate and lower unemployment rate though. In Figure A3, I show the countries for which I have the regional unemployment data available.

4 Firm-Level Results

In Table 2 I report results using firm-level data. In column 1 I include U.K. subsidiaries of MNCs, in column 2 I include domestic firms, and in column 3 I include only foreign subsidiaries belonging to MNCs that had at least one U.K. affiliate in 2010. In columns 1 and 3 treated is a dummy equal to 1 when the MNC was exposed to WDC, in column 2, treated is a dummy equal to 1 when the region was exposed to WDC.

Consistent with Bilicka et al. (2020), I show that after the WDC there was a significant reduction in employment in subsidiaries belonging to the affected MNCs in the United Kingdom. After 2010, there was also an increase in the number of people employed by domestic firms in the United Kingdom, but not necessarily in regions exposed to WDC. Further, there was an increase in the number of people employed by affected MNCs abroad. The magnitudes of the U.K. reduction and foreign increase are very similar; MNCs reduced employment in the United Kingdom by 8.5 percent and increased that abroad by 8.6 percent. There are two reasons why these estimates are different than Bilicka et al. (2020). First, in this paper,

I consider a longer sample period to show the lasting effect of the WDC of employment. Second, to match MNC affiliates with regional data I have to use subsidiary-level data instead of aggregating at the MNC level in the United Kingdom.

In Figure 1, I plot the dynamic evolution of the employment changes at the firm level. We can see that before the WDC, employment of affected and unaffected MNCs evolved similarly, both in the United Kingdom (Panel A) and abroad (Panel B). After the WDC, employment in the control group has remained stable, while in the United Kingdom the number of people employed by affected MNCs has gradually declined. The opposite happened in foreign countries. Note that from 2016 onward the difference between treated and control groups employment in the United Kingdom is not statistically significant. Part of this can be attributed to a gradual increase in the number of people employed by domestic firms in the United Kingdom (Panel C) that increased in 2016. Those firms are likely picking up some of the unemployment that was created by the MNCs that reduced the number of people they employed. Note that the increase in employment amongst domestic firms in the United Kingdom that I estimate in column 2 in Table 2 is concentrated outside of the regions exposed to the WDC. This could mean that there is a substitution between domestic and multinational firms' employment, but that does not necessarily need to occur regionally.

Mechanism What could be driving the observed effects? In Figure 2, I explore the regional variation in labor reallocation. As such, I estimate the effect of WDC separately for firms in London and outside of London and plot those coefficient estimates in Panel A. Most of the employment decline in the United Kingdom is driven by firms located in London. They see a 22 percent decline in the number of people employed by treated firms. Panel B in that figure demonstrates that this is driven by a gradual fall in employment by treated firms and we do not see any effect for the control group firms in London. This suggests no substitution effect between firms that were affected by WDC and those that were not in London. Also, in Panel C, I do not find a significant reduction in the *average* employment outside of London.

What is different about firms in London? Forty-one percent of London firms in 2010 belong to service industry, 23 percent to finance, 9.5 percent are construction, and 8.3 percent manufacturing. Outside of London only 26 percent of firms are services, while 29 percent are manufacturing and 16.5 percent finance. In Figure 3, I divide the sample of subsidiaries in London into sectors in which these firms operate and estimate the effect of the reform for each sector separately. I show that the negative effect for London is driven primarily by service and construction firms. Those are the only two significant coefficient estimates in

Panel A. This is perhaps not surprising, as service firms are likely to be most mobile when it comes to capital and employment and may be able to relocate most efficiently and at the lowest cost.

In Panel B, I divide the sample of subsidiaries outside of London into sectors and estimate the effect of WDC within each sector separately. Even though the average estimate for employment reallocation for outside of London in Panel A of Figure 2 is not statistically significant from zero, there is a substantial sectoral heterogeneity here. I show that service firms actually reduce their employment outside of London too and the magnitude of this effect is comparable to those London firms. This again points toward capital mobility being important when firms are relocating their operations. This is consistent with the fact that I find no effect for manufacturing or wholesale trade sectors, which have much lower capital mobility. In Panel C in Figure 3, I show that the only sector where we see a significant increase in employment abroad is a service sector too.⁶

In unreported exercises, I check whether the decline in employment in the service sector is linked with increase in other sectors, especially among firms unaffected by WDC. I find no such effects, which suggests that the observed effect is about service firms reallocating away from the United Kingdom to more preferable foreign locations and not about substitution between sectors and firm types.

5 Regional Estimates

To understand the implications of firm-level reallocation of employment on local labor markets, I proceed in two steps. First, I use regional statistics for the number of jobs available. Second, I aggregate firm-level data at the county-city level and show results using aggregate number of people employed by MNCs in each region.

In Table 3 I show results using the number of jobs available. In all columns I control for logarithm of population to account for changes in population across regions. In Panel A, I show results for U.K. regions and in Panel B for foreign regions. I find that the number of jobs available in the United Kingdom declined in the exposed regions, while increasing in the foreign regions. These results are consistent with the firm-level results in which firms that failed the gateway test relocated their employees from U.K. to foreign countries. This result is consistent for regions that were exposed to firms failing the gateway test (column 1)

⁶Note that I do not estimate the sectoral regressions for mining and construction in foreign countries due to small sample size.

and those regions that were exposed to firms that reduced their employment in the United Kingdom (column 2). In Table A1 in the Appendix, I show that exposed regions had a higher unemployment rate and lower regional GDP in the United Kingdom after the WDC as well. At the same time, exposed regions had lower unemployment and higher regional GDP in foreign countries.

One may be concerned that after the WDC and the financial crisis regions that were more exposed to MNCs did economically worse and that can be reflected in higher unemployment, lower regional GDP and fewer jobs available. Results from columns 3 in Table 3 show that this is not the case. I find no significant effect of WDC on regions that were simply more exposed to MNCs, both in the United Kingdom and abroad.

Firm-level aggregated data In Table 3 in columns 4–5 I show results using aggregate firm-level data. I find that in regions exposed to WDC in the United Kingdom, there is a substantial reduction in aggregated number of people employed by MNCs. The magnitude of the effect suggests a 17 percent decline in the number of people employed by MNCs in those regions relative to regions without MNCs that failed the gateway test in the United Kingdom. In foreign countries, I find an increase in the number of people employed by MNCs in regions that were more exposed to WDC. These results are qualitatively similar to the regional ones. Note that the estimated magnitudes are quantitatively different than using regional data, likely because FAME and ORBIS data do not have a complete coverage of all firms in each region.

5.1 Mechanisms and magnitudes

The firm-level reallocation of labor appears to have large local labor market effects both in the United Kingdom and abroad. In spite of the fact that we observe similar magnitude of the firm-level reduction of employment in the United Kingdom and increase abroad, the regional data suggest some asymmetries. The reduction in the county-city jobs available in the United Kingdom is 3.2–3.9 percent in magnitude, while the foreign increase is larger, 5.0–7.5 percent. There are three potential reasons for this disparity in magnitudes. First, as MNCs expand in the foreign locations where they reallocate employment, local labor markets can experience productivity gains through supply-chain linkages (Alfaro and Chen, 2018; Javorcik, 2004). These can create larger local gains than the initial firm-level expansion, which may affect local domestic firms as well, increasing local labor market size and reducing unemployment. Thus the initial expansion is followed by further growth due to economies

of scale. Note that I do not have sufficient prereform data to investigate the domestic labor effects in foreign countries.

Second, the initial decline in firm-level employment by MNCs in the United Kingdom persist until 2015 after which I observe an increase. This suggests that the negative shock is not permanent and that affected firms eventually offset the effects of WDC. Third, in the United Kingdom, I also find that domestic firms increased the number of people they employed, but not necessarily in the regions exposed to WDC. This suggests that domestic firms did not benefit from this negative shock by substituting labor locally, unlike in Desai et al. (2009). However, it is entirely plausible that domestic labor substitution occurred in regions not exposed to WDC; that is, not locally. This substitution would explain a much smaller effect of the WDC on the U.K. local labor markets.

References

- Laura Alfaro and Maggie X. Chen. Selection and Market Reallocation: Productivity Gains from Multinational Production. *American Economic Journal: Economic Policy*, 10(2): 1–38, May 2018.
- Heitor Almedia, Chang-Soo Kim, and Hwanki Brian Kim. Internal capital markets in business groups: Evidence from the asian financial crisis. *The Journal of Finance*, 70(6): 2539–2586, 2015.
- Matteo P. Arena and George W. Kutner. Territorial Tax System Reform and Corporate Financial Policies. *Review of Financial Studies*, 28(8):2250–2280, 2015.
- Johannes Becker and Nadine Riedel. Cross-border tax effects on affiliate investment—Evidence from European multinationals. *European Economic Review*, 56(3):436–450, 2012.
- Huber K. Biermann, M. Causal Evidence on the International Transmission of Crises Through Multinational Firms. Technical report, 2019.
- Katarzyna Bilicka, Yaxuan Qi, and Jing Xing. Real responses to anti-tax avoidance policies: Evidence from the uk worldwide debt cap. *Working Paper*, 2020.
- Katarzyna Anna Bilicka. Comparing uk tax returns of foreign multinationals to matched domestic firms. *American Economic Review*, 109(8):2921–53, August 2019.
- Jennifer Blouin, Harry Huizinga, Luc Laeven, and Gaëtan J.A. Nicodème. Thin Capitalization Rules and Multinational Firm Capital Structure. Technical report, 2014.
- Xavier Boutin, Giacinta Cestone, Chiara Fumagalli, Giovanni Pica, and Nicolas Serrano-Velarde. The deep-pocket effect of internal capital markets. *Journal of Financial Economics*, 109(1):122 – 145, 2013.
- Thiess Buettner, Michael Overesch, Ulrich Schreiber, and Georg Wamser. The impact of thin-capitalization rules on the capital structure of multinational firms. *Journal of Public Economics*, 96(11):930 – 938, 2012. Fiscal Federalism.
- Sarah Clifford. Taxing multinationals beyond borders: Financial and locational responses to cfc rules. *Journal of Public Economics*, 173:44 – 71, 2019. ISSN 0047-2727.

- Anca D. Cristea and Daniel X. Nguyen. Transfer Pricing by Multinational Firms: New Evidence from Foreign Firm Ownerships. *American Economic Journal: Economic Policy*, 8(3):170–202, August 2016.
- Ronald B. Davies, Julien Martin, Mathieu Parenti, and Farid Toubal. Knocking on tax haven’s door: Multinational firms and transfer pricing. *The Review of Economics and Statistics*, 100(1):120–134, 2018.
- Mihir A. Desai and Dhammika Dharmapala. Interest Deductions in a Multijurisdictional World. Technical report, 2015.
- Mihir A. Desai, C. Fritz Foley, and James R. Hines. A Multinational Perspective on Capital Structure Choice and Internal Capital Markets. *Journal of Finance*, 59(6):2451–2487, December 2004.
- Mihir A. Desai, C. Fritz Foley, and Kristin J. Forbes. Financial Constraints and Growth: Multinational and Local Firm Responses to Currency Depreciations. *The Review of Financial Studies*, 21(6):2857–2888, 03 2007.
- Mihir A. Desai, C. Fritz Foley, and James R. Hines. Domestic Effects of the Foreign Activities of US Multinationals. *American Economic Journal: Economic Policy*, 1(1):181–203, February 2009.
- Dhammika Dharmapala. What do we know about base erosion and profit shifting? a review of the empirical literature. *Fiscal Studies*, 35(4):421–448, 2014.
- Matthias Dischinger and Nadine Riedel. Corporate taxes and the location of intangible assets within multinational firms. *Journal of Public Economics*, 95(7):691 – 707, 2011.
- Peter Egger, Valeria Merlo, Martin Ruf, and Georg Wamser. Consequences of the new uk tax exemption system: Evidence from micro-level data. *The Economic Journal*, 125(589): 1764–1789, 2015.
- Peter H. Egger and Georg Wamser. The impact of controlled foreign company legislation on real investments abroad. A multi-dimensional regression discontinuity design. *Journal of Public Economics*, 129(C):77–91, 2015.
- David N. Figlio and Bruce A. Blonigen. The Effects of Foreign Direct Investment on Local Communities. *Journal of Urban Economics*, 48(2):338–363, September 2000.

- Daniel G. Garrett, Eric Ohrn, and Juan Carlos Suárez Serrato. Tax policy and local labor market behavior. *American Economic Review: Insights*, 2(1):83–100, March 2020. doi: 10.1257/aeri.20190041.
- Xavier Giroud and Holger M. Mueller. Capital and Labor Reallocation within Firms. *Journal of Finance*, 70(4):1767–1804, August 2015.
- Xavier Giroud and Holger M. Mueller. Firm Leverage, Consumer Demand, and Employment Losses During the Great Recession. *The Quarterly Journal of Economics*, 132(1):271–316, 10 2016.
- Xavier Giroud and Holger M. Mueller. Firms’ internal networks and local economic shocks. *American Economic Review*, 109(10):3617–49, October 2019. doi: 10.1257/aer.20170346. URL <https://www.aeaweb.org/articles?id=10.1257/aer.20170346>.
- Harry Grubert and Joel Slemrod. The effect of taxes on investment and income shifting to puerto rico. *The Review of Economics and Statistics*, 80(3):365–373, 1998.
- Ann E. Harrison and Brian J. Aitken. Do Domestic Firms Benefit from Direct Foreign Investment? Evidence from Venezuela. *American Economic Review*, 89(3):605–618, June 1999.
- Jonathan E. Haskel, Sonia C. Pereira, and Matthew J. Slaughter. Does Inward Foreign Direct Investment Boost the Productivity of Domestic Firms? *The Review of Economics and Statistics*, 89(3):482–496, August 2007.
- Kilian Huber. Disentangling the effects of a banking crisis: Evidence from german firms and counties. *American Economic Review*, 108(3):868–98, March 2018.
- Harry Huizinga, Luc Laeven, and Gaetan Nicodeme. Capital structure and international debt shifting. *Journal of Financial Economics*, 88(1):80–118, April 2008.
- Beata Smarzynska Javorcik. Does Foreign Direct Investment Increase the Productivity of Domestic Firms? In Search of Spillovers Through Backward Linkages. *American Economic Review*, 94(3):605–627, June 2004.
- Sebnem Kalemli-Ozcan, Herman Kamil, and Carolina Villegas-Sanchez. What Hinders Investment in the Aftermath of Financial Crises: Insolvent Firms or Illiquid Banks? *The Review of Economics and Statistics*, 98(4):756–769, October 2016.

- Hiau Looi Kee. Local intermediate inputs and the shared supplier spillovers of foreign direct investment. *Journal of Development Economics*, 112(C):56–71, 2015. doi: 10.1016/j.jdeveco.2014.09.
- Wolfgang Keller and Stephen R. Yeaple. Multinational Enterprises, International Trade, and Productivity Growth: Firm-Level Evidence from the United States. *The Review of Economics and Statistics*, 91(4):821–831, November 2009.
- Brian K. Kovak, Lindsay Oldenski, and Nicholas Sly. The Labor Market Effects of Offshoring by U.S. Multinational Firms: Evidence from Changes in Global Tax Policies. Research Working Paper RWP 17-12, Federal Reserve Bank of Kansas City, December 2017.
- Dominika Langenmayr and Li Liu. Where Does Multinational Profit Go with Territorial Taxation? Evidence from the UK. Technical report, 2020.
- Yi Lu, Zhigang Tao, and Lianming Zhu. Identifying FDI spillovers. *Journal of International Economics*, 107(C):75–90, 2017. doi: 10.1016/j.jinteco.2017.01.
- Jack Mintz and Michael Smart. Income shifting, investment, and tax competition: theory and evidence from provincial taxation in Canada. *Journal of Public Economics*, 88(6): 1149–1168, June 2004.
- Raffaele Santioni, Fabio Schiantarelli, and Philip E Strahan. Internal capital markets in times of crisis: The benefit of group affiliation in Italy. Working Paper 23541, National Bureau of Economic Research, June 2017.
- Juan Carlos Suárez Serrato. Unintended Consequences of Eliminating Tax Havens. NBER Working Papers 24850, National Bureau of Economic Research, Inc, July 2018.
- Bradley Setzler and Felix Tintelnot. The Effects of Foreign Multinationals on Workers and Firms in the United States. NBER Working Papers 26149, National Bureau of Economic Research, Inc, August 2019.
- Joel Slemrod. Do Taxes Matter? Lessons from the 1980’s. *American Economic Review*, 82(2):250–256, May 1992.
- Thomas R. Torslov, Ludvig S. Wier, and Gabriel Zucman. The missing profits of nations. Working Paper 24701, National Bureau of Economic Research, June 2018.

Table 1: Regional Comparison: Means

	(1) Not exposed	(2) Exposed	(3) Diff	(4) t-test
Panel A: U.K. exposure				
Population	320,815.633	647,096.396	−326,280.762	−1.221
Number of employed	137.700	343.572	−205.872	−1.378
Activity rate %	61.376	61.598	−0.222	−0.222
Unemployment rate	8.200	8.596	−0.396	−0.747
GDP	8,261.479	25,583.157	−17,321.678	−1.267
Panel B: Foreign exposure				
Population	951,309.078	2,079,438.818	−1,128,129.740*	−1.914
Number of employed	475.552	1,154.213	−678.661*	−2.035
Activity rate %	57.354	58.435	−1.081	−0.812
Unemployment rate	11.684	9.172	2.512**	2.374
GDP	28,082.979	73,839.150	−45,756.171**	−2.237

NOTE: This table presents mean characteristics of regions in the treated and control groups in 2010. Exposed to failed MNC is 1 when a region has at least one MNC that failed the gateway test and we observe employment for that MNCs subsidiary. In Panel A, I show statistics for U.K. regions. There are 68 unexposed and 56 exposed regions in Panel A. In Panel B, I show statistics for foreign regions. There are 185 unexposed and 10 exposed regions in Panel A.

Table 2: Firm-Level Baseline Results

Dep.var <i>log_empl</i>	(1) MNEs	(2) domestic	(3) MNEs
post=1 \times treated=1	-0.085** (0.033)	-0.163** (0.068)	0.086** (0.035)
post=1		0.140*** (0.047)	
Year FE	✓	✓	✓
Firm FEs	✓	✓	✓
Sample	U.K.		non-U.K.
Observations	69,594	20,336	336,141
# firms	9,470	13,858	51,841
Mean	4.037	3.685	3.939

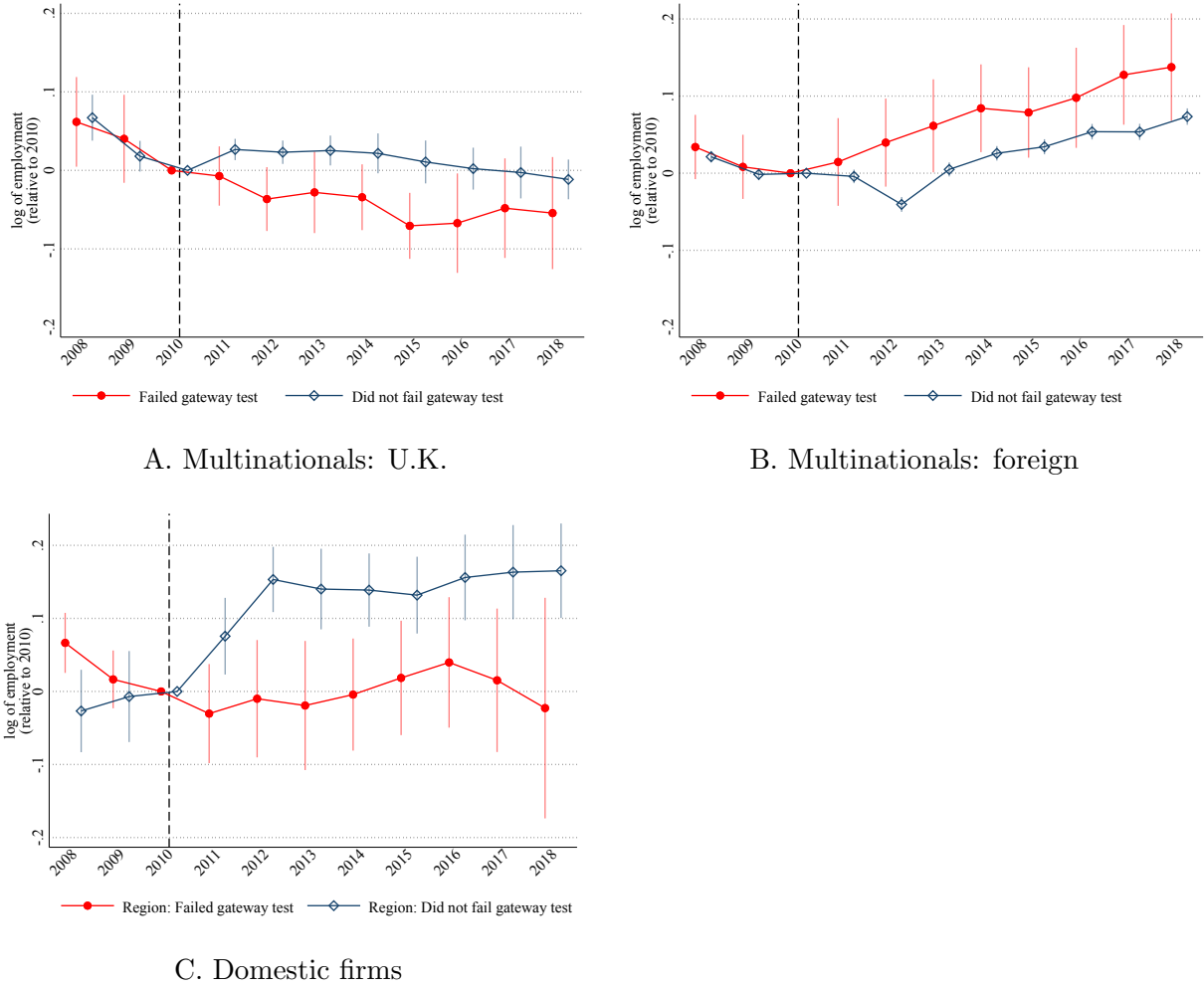
NOTE: In columns 1 and 3, treated is equal to 1 when MNC failed the gateway test in 2010. In column 2, treated is equal to 1 when the region was exposed to WDC by having at least one MNC that failed the gateway test. Post is equal to 1 from 2011 onward. Standard errors clustered at the firm level. The sample spans years 2007–2018. I am not able to include the effect of the WDC on domestic operations in foreign countries, since I only have data after 2011 for them.

Table 3: Regional Level Results: Number of Jobs

Dep.var. <i>log_empl</i>	(1)	(2)	(3)	(4)	(5)
	Exposed to failed firms	Large empl change	Exposed MNEs	Exposed to failed firms	Large empl change
	Regional data			Firm-level data	
Panel A: U.K. regions					
treated=1 × post=1	−0.039*** (0.010)	−0.032*** (0.010)	−0.017 (0.010)	−0.168* (0.095)	−0.219** (0.101)
Observations	1,586	1,586	1,586	543	447
# Regions	122	122	122	255	268
Mean	5.039	5.039	5.039	5.077	5.076
Panel B: Foreign regions					
treated=1 × post=1	0.050** (0.020)	0.075** (0.030)	−0.017 (0.012)	0.031*** (0.010)	0.026** (0.156)
Observations	3,955	3,955	3,955	2,394	2,398
# Regions	644	644	644	922	921
Mean	5.764	5.764	5.764	5.825	5.826
Year FE	✓	✓	✓	✓	✓
Region FEs	✓	✓	✓	✓	✓

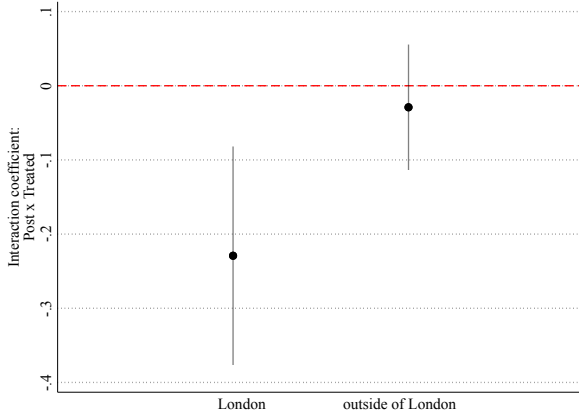
NOTE: Treated is equal to 1 in columns 1 and 4 when a region is exposed to at least one MNC that failed the gateway test. In columns 2 and 5, treated is equal to 1 when a region is exposed to at least one MNC that reduced employment following the WDC, and in column 3, treated is equal to 1 when a region is exposed to MNCs presence at all; post is equal to 1 from 2011 onward. Here, I use only subsample until 2014. In all columns I control for log of population in each region. Standard errors clustered at the county-city level.

Figure 1: The Effect of the Reform on Firm-Level Employment

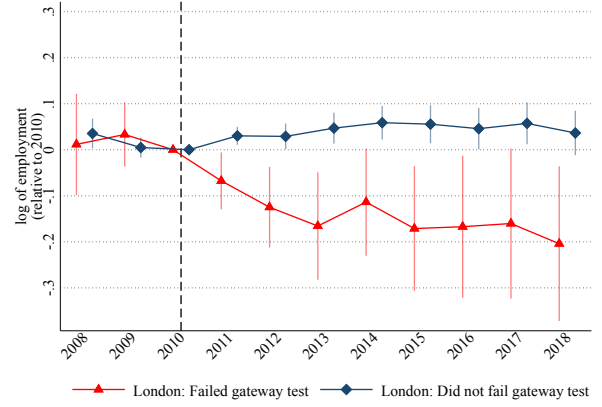


NOTE: In Panels A and B, treated is equal to 1 when MNC failed the gateway test in 2010 (red line with circles); control is equal to 1 when MNC did not fail the gateway test in 2010 (blue line with hollow diamonds). In Panel C, region is treated when there is at least one MNC that failed the gateway test in that region. Region is not treated if there were no MNCs exposed to WDC in that region. Standard errors clustered at the firm level. The sample spans years 2007–2018. Panel A includes employment in MNCs in the United Kingdom, Panel B includes employment in MNCs in foreign countries, and Panel C includes employment in domestic firms in the United Kingdom.

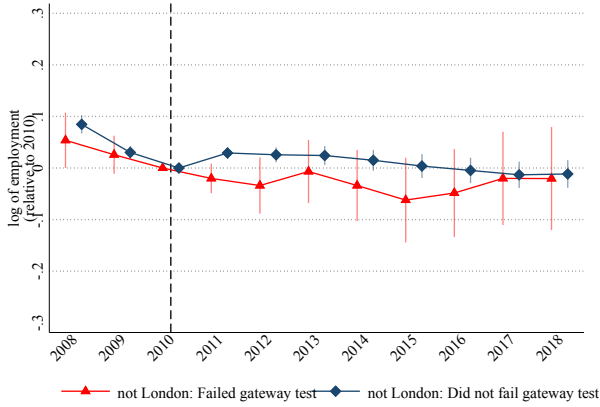
Figure 2: Heterogeneous Effect: Regions



A. Regional differences



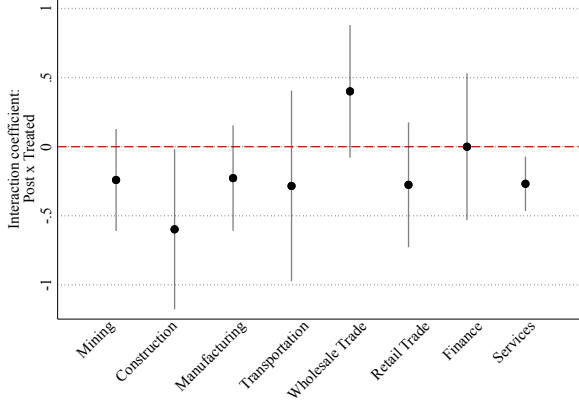
B. London



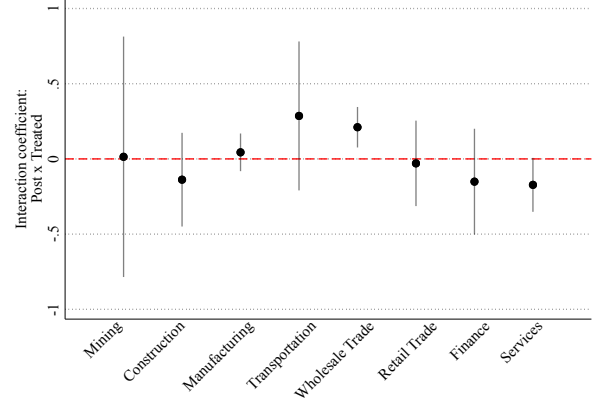
C. Outside of London

NOTE: In Panel A, I compare the main difference-in-difference coefficient for London and outside of London sample; 30% of MNCs are located in London. In Panel B, I compare sectors within London, and in Panel C, the same sectors outside of London. I exclude agriculture because there is not enough variation to estimate that for London.

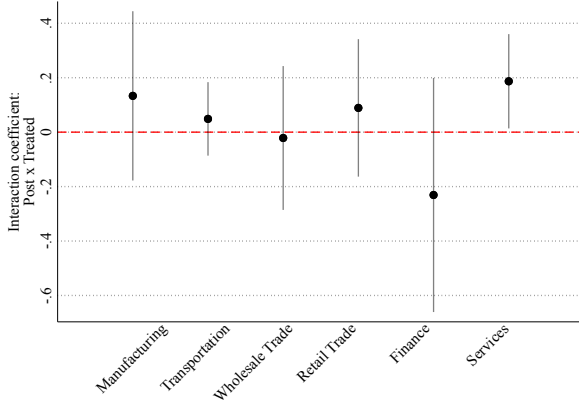
Figure 3: Heterogeneous Effect: Sectors



A. Sectoral changes - London



B. Sectoral changes - outside of London



C. Sectoral changes - foreign

NOTE: In Panel A, I compare the main difference in difference coefficient for London and outside of London sample; 30% of MNCs are located in London. In Panel B, I compare sectors within London, and in Panel C, the same sectors outside of London. I exclude agriculture because there is not enough variation to estimate that for London. In Panel C, I replicate this analysis for foreign firms. There is not enough variation in mining and construction.

Appendices

Table A1: Regional-Level Results: Unemployment and GDP

	(1) Exposed to failed firms	(2) Large empl change	(3) Exposed MNEs	(4) Exposed to failed firms	(5) Large empl change	Exposed to MNEs
dep. var.	<i>log unemployment</i>			<i>log GDP</i>		
Panel A: U.K. regions						
treated=1 × post=1	0.039** (0.019)	0.048** (0.019)	0.036 (0.041)	−0.023*** (0.007)	−0.025*** (0.007)	−0.025* (0.015)
Observations	1,560	1,560	1,560	754	754	754
# firms	123	123	123	123	123	123
Mean	4.996	4.996	4.996	5.045	5.045	5.045
Panel B: Foreign regions						
treated=1 × post=1	−0.136* (0.073)	−0.182** (0.128)	−.242*** (0.049)	0.034* (0.018)	0.049** (0.021)	0.025 (0.020)
Observations	3,792	3,792	3,792	911	911	911
# Regions	640	640	640	566	566	566
Mean	5.777	5.777	5.777	5.739	5.739	5.739
Year FEs	✓	✓	✓	✓	✓	✓
Region FEs	✓	✓	✓	✓	✓	✓

NOTE: The dependent variable is logarithm of unemployment rate in columns 1–3 and logarithm of GDP in columns 4–6. Treated is 1 in columns 1 and 4 when a region is exposed to at least one MNC that failed the gateway test. In columns 2 and 5, treated is 1 when a region is exposed to at least one MNC that reduced employment following the WDC, and in columns 3 and 6, treated is 1 when a region is exposed to MNCs presence at all; post is equal to 1 from 2011 onward. Here, I use only subsample until 2014. In all columns I control for log of population in each region. Standard errors clustered at the county-city level.

Table A2: Regional Comparison: Means, Alternative Exposure Definition

	(1) Not exposed	(2) Exposed	(3) Diff	(4) t-test
Panel A: U.K. exposure				
Population	324,981.441	693,635.819	−368,654.378	−1.194
Number of employed	143.235	368.984	−225.748	−1.305
Activity rate %	61.355	61.685	−0.331	−0.293
Unemployment rate	8.290	8.528	−0.238	−0.424
GDP	8,775.283	27,655.851	−18,880.568	−1.192
Panel B: Foreign exposure				
Population	1,002,886.421	2,845,212.800	−1,842,326.379	−1.808
Number of employed	520.082	1,548.790	−1,028.708	−1.529
Activity rate %	57.227	62.519	−5.292***	−3.956
Unemployment rate	11.575	7.525	4.050**	2.407
GDP	31,195.593	98,896.807	−67,701.214	−1.670

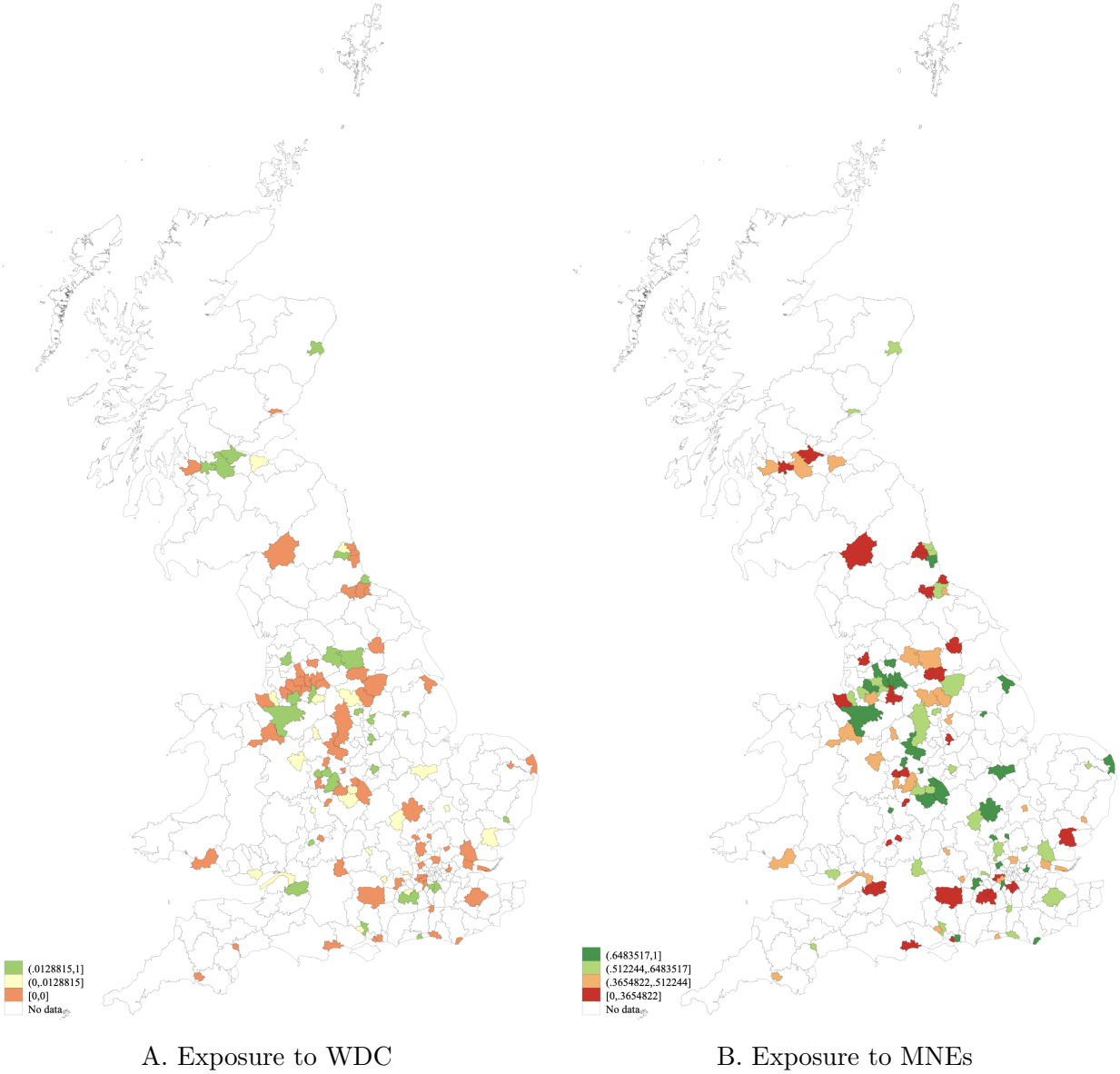
NOTE: This table presents mean characteristics of regions in the treated and control groups in 2010. Exposed to failed MNC is 1 when a region has at least one MNC that failed the gateway test, and we observe employment decline for that MNCs subsidiary. In Panel A I show statistics for U.K. regions. There are 68 unexposed and 56 exposed regions in panel A. In Panel B I show statistics for foreign regions. There are 176 unexposed and 8 exposed regions in Panel A.

Table A3: List of Cities Included in the Regional Analysis

Aberdeen City	City of London	Liverpool	South Tyneside
Barking and Dagenham	Colchester	Luton	Southampton
Barnet	Coventry	Maidstone	Southend-on-Sea
Barnsley	Crawley	Manchester	St Albans
Basildon	Croydon	Mansfield	St. Helens
Basingstoke and Deane	Darlington	Middlesbrough	Stevenage
Bath, NE Somerset	Derby	Milton Keynes	Stockport
Bedford	Derry & Strabane	Newcastle upon Tyne	Stockton-on-Tees
Bexley	Doncaster	North East Lincolnshire	Stoke-on-trent
Birmingham	Dudley	North Lanarkshire	Sunderland
Blackburn with Darwen	Dundee City	North Tyneside	Sutton
Blackpool	East Staffordshire	Northampton	Swansea
Bolton	Eastbourne	Norwich	Swindon
Bournemouth	Enfield	Nottingham	Tamworth
Bracknell Forest	Exeter	Nuneaton, Bedworth	Telford, Wrekin
Bradford	Falkirk	Oldham	Tunbridge Wells
Brent	Gateshead	Oxford	Wakefield
Brighton and Hove	Glasgow City	Peterborough	Walsall
Bristol	Gloucester	Plymouth	Warrington
Bromley	Great Yarmouth	Poole	Warwick
Burnley	Guildford	Portsmouth	Wigan
Bury	Harlow	Preston	Wirral
Cambridge	Harrow	Reading	Woking
Cannock Chase	Hartlepool	Redditch	Wolverhampton
Cardiff	Hastings	Richmond u. Thames	Worcester
Carlisle	Hounslow	Rochdale	Worthing
Chelmsford	Ipswich	Rotherham	Wrexham
Cheltenham	Kingston u.Thames	Salford	Wycombe
Cheshire West, Chester	Leeds	Sheffield	York
Chesterfield	Leicester	Slough	
City of Edinburgh	Lincoln	Solihull	

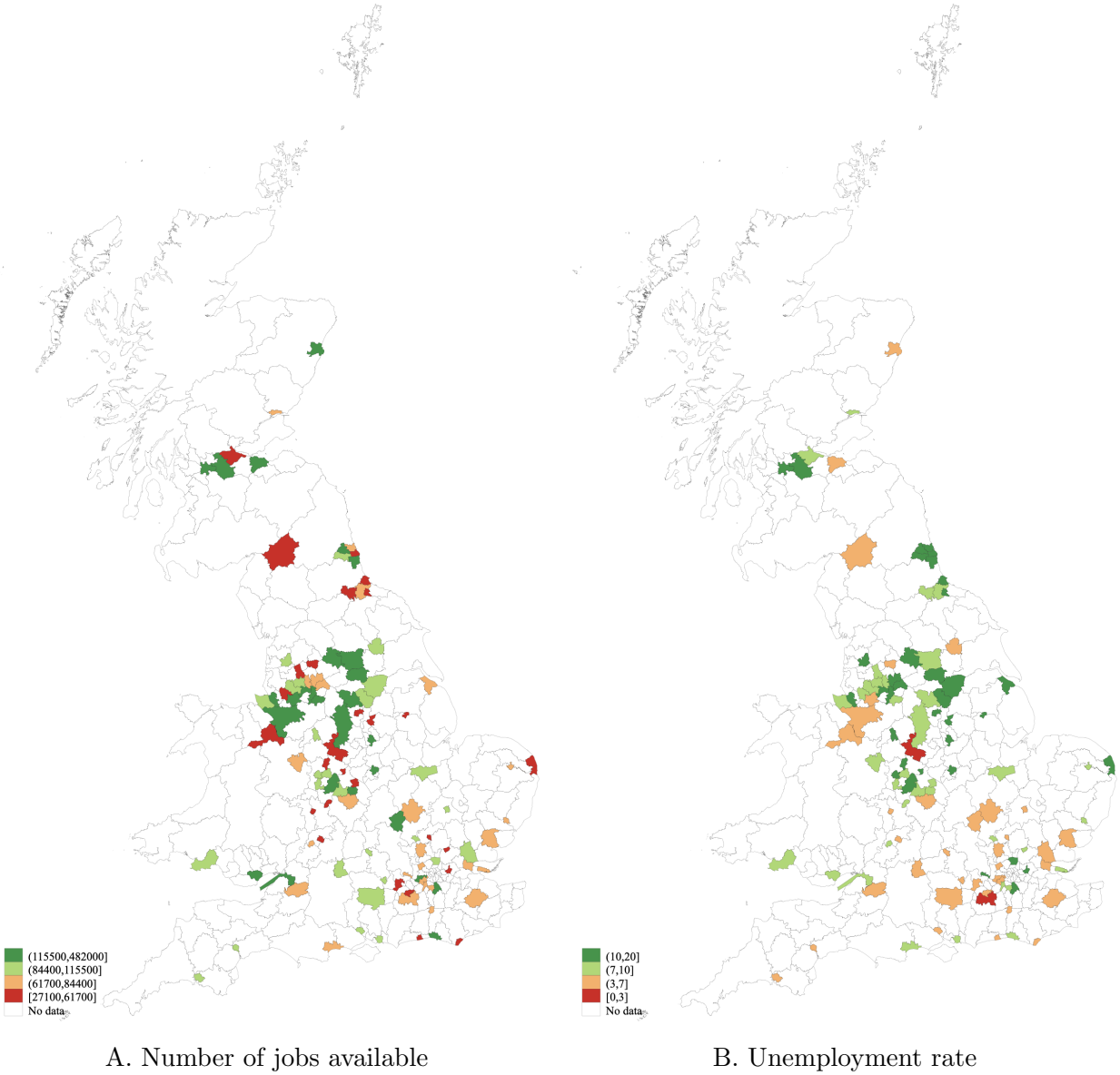
NOTE: This is the list of 122 cities with data for jobs and unemployment. MNCs in the United Kingdom are located across 663 cities, but most of those do not report employment data to Eurostat.

Figure A1: Map of U.K. Counties: Exposure



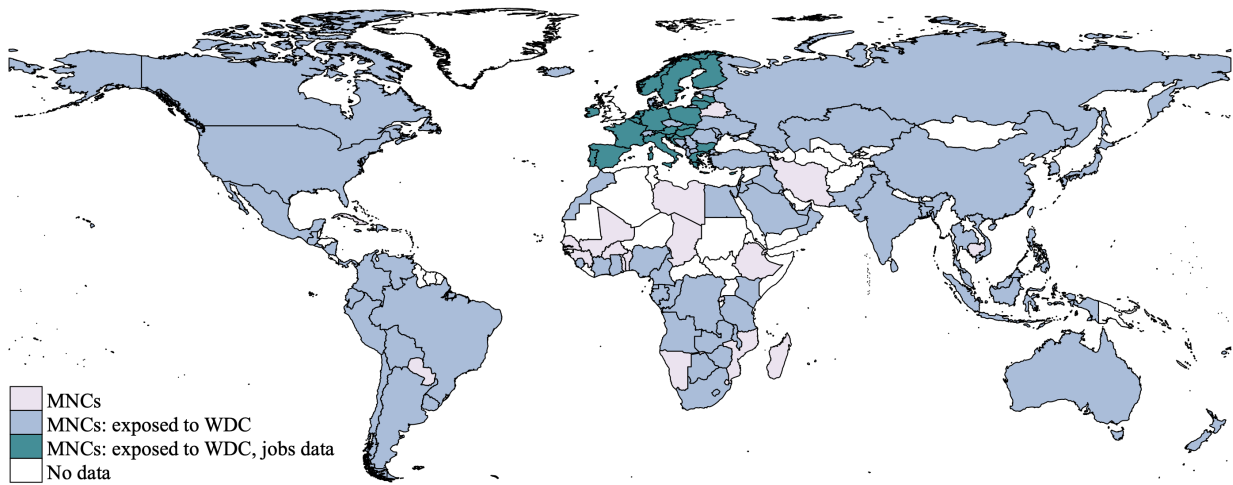
NOTE: U.K. counties, Nomenclature of Territorial Units for Statistics, level 3. The districts of England (also known as local authority districts or local government districts to distinguish from unofficial city districts) are a level of subnational division of England used for the purposes of local government. In Panel A: exposure to WDC: in orange are counties that were not exposed to WDC, in yellow counties with small exposure and in green counties with large exposure. In Panel B: in red small exposure to MNCs, in green large. In the remaining countries, we do not have any multinational firms in our dataset.

Figure A2: Map of U.K. Counties: Jobs and Unemployment

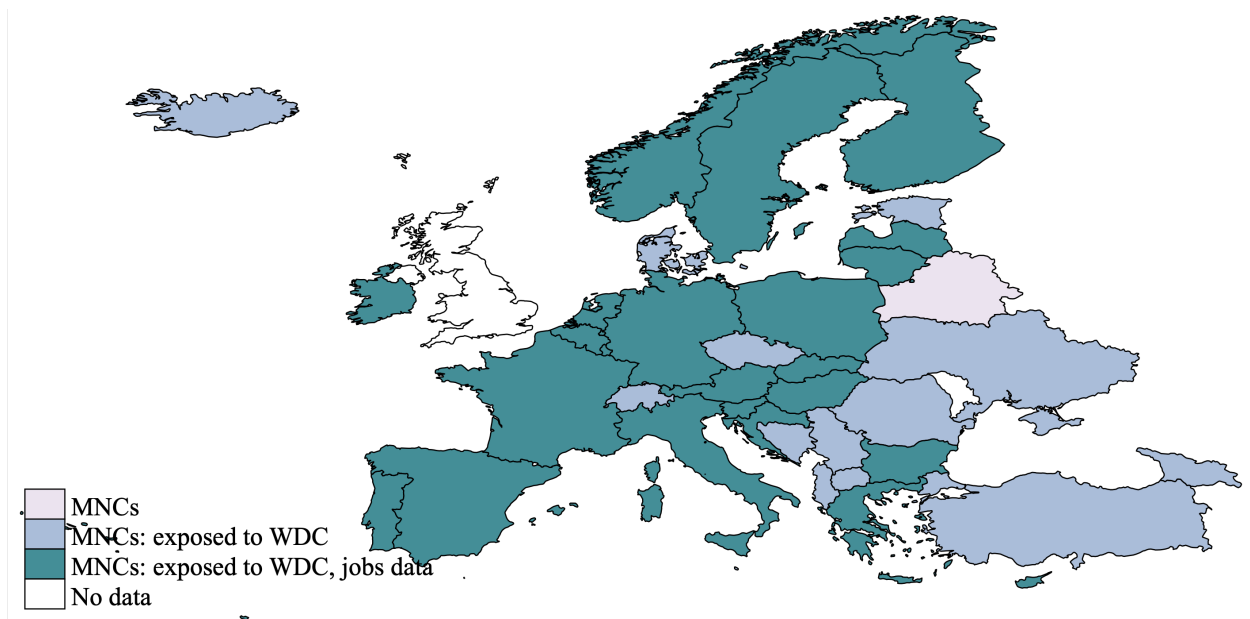


NOTE: U.K. counties, Nomenclature of Territorial Units for Statistics, level 3. The districts of England (also known as local authority districts or local government districts to distinguish from unofficial city districts) are a level of subnational division of England used for the purposes of local government. In Panel A: number of jobs available from Eurostat. Panel B: unemployment rate from Eurostat. All data in 2009, one year before WDC. In the remaining countries, we do not have any multinational firms in our dataset.

Figure A3: Map of the World: WDC and MNE Exposure



Panel A: World map, WDC exposure



Panel B: Europe availability of jobs data

NOTE: Data from Orbis and FAME, BvD matched with Eurostat employment information.