THE EFFECTS OF FOREIGN MULTINATIONALS ON WORKERS AND FIRMS IN THE UNITED STATES*

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Governments go to great lengths to attract foreign multinationals because they are thought to raise the wages paid to their employees (direct effects) and to improve outcomes at local domestic firms (indirect effects). We construct the first U.S. employer-employee data set with foreign ownership information from tax records to measure these direct and indirect effects. We find the average direct effect of a foreign multinational firm on its U.S. workers is a 7% increase in wages. This premium is larger for higher-skilled workers and for the employees of firms from high GDP per capita countries. We find evidence that it is membership in a multinational production network—instead of foreignness—that generates the foreign-firm premium. We leverage the past spatial clustering of foreign-owned firms by country of ownership to identify the indirect effects. An expansion in the foreign-multinational share of commuting-zone employment substantially increases the employment, value added, and—for higher-earning workers—wages at local domestic-owned firms. Per job created by a foreign multinational, our estimates suggest annual gains of US$13,400 to the aggregate wages of local incumbents, two-thirds of which are from indirect effects. Our estimates suggest that—via mega-deals for subsidies from local governments—foreign multinationals are able to extract a sizable fraction of the local surplus they generate. *JEL Codes:* F23, J3, R1.

I. Introduction

Foreign multinationals account for a sizable fraction of value added, exports, and research and development in the United

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States (BEA 2017). These firms are affected by regulations on foreign investment, trade policies, and local subsidy competition.[1] A widely held belief is that attracting a foreign multinational to a location will have transformative effects on the outcomes of local workers and firms. The hard evidence on this belief has been limited by data unavailability and the challenge of identifying causal effects. The key questions for policy makers and local stakeholders center on the direct and indirect effects of a job created by a foreign multinational: How much more does a worker earn when she is hired by a foreign multinational? How are domestic firms and their workers in nearby locations affected by foreign firms?

This article makes four main contributions to understanding the effects of foreign multinationals. First, we use tax records to construct a panel data set for the United States that links the population of workers and firms with foreign-ownership information of the firms. Second, we develop a model that provides the theoretical underpinnings to study the direct effects that foreign multinationals have on their own workers and the indirect effects they have on domestic-owned firms and their workers in the local labor market. Third, we leverage the movers between firms to identify the foreign-firm premium, i.e., the wage gain for the same worker when moving from a domestic to a foreign firm. Fourth, we document and exploit the spatial clustering of foreign firms to construct an instrument for foreign investment in the local labor market, allowing us to identify the indirect effects of foreign multinationals on the value added, employment, and wages paid at domestic firms.

Our data are created by merging the population of annual U.S. corporate tax filings with the population of annual W-2 tax filings on the wage payments made by employers to workers during 1999–2017.[2] Then, we identify foreign multinationals in these data from a filing requirement for each U.S. corporation that is 25% or more foreign-owned. This information also includes the country of foreign ownership. To our knowledge, this is the

1. The OECD (2019) ranks the United States slightly above the OECD average in terms of foreign direct investment (FDI) restrictiveness. Prominent examples of subsidy deals offered to foreign multinationals include the BMW plant in Spartanburg, South Carolina (1992); the Toyota plant in Blue Springs, Mississippi (2007); and the Foxconn plant in Mount Pleasant, Wisconsin (announced in 2017).

2. Findings from the matched firm-worker tax records in the United States have been reported in studies by Yagan (2019), Kline et al. (2019), Lamadon, Mogstad, and Setzler (2020), and Smith et al. (2019).

first study to combine linked employer-employee panel data with foreign-ownership information in the United States.[3] These panel data provide a unique opportunity to investigate the direct and indirect effects of foreign multinationals in the U.S. labor market.

The primary challenge in studying the direct effects of foreign multinationals on their workers' wages is to disentangle the extent to which higher wages at foreign-owned firms are due to worker skill differentials as opposed to firm premiums. To estimate these premiums, we leverage the U.S. panel data to follow workers who move between foreign and domestic firms. We make four novel contributions to the study of the direct effects of foreign investment. First, this is the first article to estimate the foreign firm premium in the United States that controls for worker skill differentials. We find that the average foreign-firm premium is 7%. Second, because the United States is both the leading headquarter country of multinationals and the top recipient of foreign investment, it provides large samples of both foreign and domestic multinationals. We find that domestic-owned and foreign-owned multinationals have very similar premiums, suggesting that belonging to a multinational network, rather than foreignness, is the main driver of the foreign-firm premium. Third, because the United States is the top recipient of the world's foreign investment, it provides a rare opportunity to compare the effects of foreign firms by country of origin, with large samples from many diverse countries. We find that the foreign-firm premium is increasing in the GDP per capita of the origin country and that firms from higher GDP per capita countries tend to hire more skilled workers. Fourth, it has long been posited that high-skilled workers benefit more from foreign investment, primarily in developing contexts (e.g., Aitken, Harrison, and Lipsey 1996). We provide the first systematic evidence in favor of this hypothesis

3. Prior studies on foreign multinationals in the United States rely on firm-level data without worker-level information. Several studies combine the Bureau of Economic Analysis (BEA) survey of FDI in the United States and the Census of Manufactures data. Boehm, Flaaen, and Pandalai-Nayar (2019) merge ownership information from the LexisNexis Directory of Corporate Affiliations with the Longitudinal Business Database at the Census Bureau. Saha, Firkri, and Marchio (2014) document regional patterns of FDI based on NETS data. The data set closest to ours is the one described by the Bureau of Labor Statistics (2019), which has employer-employee links and country of ownership. However, it is for the 2012 cross section only, and the questions we address in this article require a panel to observe changes over time.

in the United States, finding that the wage premium is larger for higher-skilled workers and absent for the lowest decile of worker skill.

Regarding the indirect effects of job creation at foreign firms on local domestic firms and their workers, the key identification challenge is that foreign multinationals may increase employment in a location because of other factors that also cause contemporaneous growth at local domestic firms. To overcome this endogeneity, we document in our data that foreign firms cluster into locations by country of ownership, then exploit this clustering to construct an instrumental variable for local foreign employment.[4] Our identification strategy is analogous to the immigration literature that uses spatial clustering of immigrants to identify the effects of immigrants on native workers' wages (see Card 2001).[5] Equipped with this identification strategy, we find that an increase in employment at foreign-owned firms significantly raises the value added, employment, wage bill, and earnings of continuing workers at domestic-owned firms in the same commuting zone. The effects are larger in the tradable sector than the nontradable sector and larger among domestic firms with more than 100 employees. Exploring heterogeneity in the wage effects for continuing workers at domestic firms, we find a much larger effect for higher-earning workers and essentially no effect for lower-earning workers. Our estimates imply that, for every 1 job created by a foreign multinational, approximately 0.5 jobs and US$139,000 in value added are generated at domestic firms in the same local labor market.

With respect to policy implications, our estimates of the direct wage premium by foreign firms highlight sizable benefits of trade and investment policies that encourage foreign firms to invest in the United States. Furthermore, our estimates imply that local policy makers have incentives to compete for investments by foreign multinationals, for both the direct wage benefits and the sizable local indirect effects on domestic firms and

4. Earlier work by Head, Ries, and Swenson (1995) finds that Japanese affiliates are spatially clustered within the United States. We are the first to exploit this spatial clustering to identify the indirect effects of foreign multinationals.

5. While our identification strategy for indirect effects is distinct from the prior literature on spillovers from foreign multinationals, it is more closely related to prior work on agglomeration in urban economics (Bartik 1991; Moretti 2010; Combes et al. 2012; Allcott and Keniston 2018; Helm 2020).

their higher-earning workers. One additional job created by a foreign multinational generates, on average, annual aggregate wage gains for incumbent workers in the commuting zone of approximately \$13,400, two-thirds of which are from indirect effects. Outside data suggest that, in the aggregate, foreign multinationals in the United States receive \$4.6 billion in economic development subsidies per year on average.[6] Abstracting from indirect effects, we find that the value of these subsidies is far below the aggregate foreign wage premium of \$36 billion a year. However, when focusing on the mega-deals for large plants, we see that subsidies per job can be quite large. A comparison of our estimates to these subsidy deals reveals that foreign multinationals are able to extract a sizable fraction of the surplus from such investments in the bargaining with local governments for mega-deals. We note that while competing for foreign multinational investments with subsidies may entail local benefits, this does not imply that such subsidies are beneficial from a national welfare perspective; see the discussion by Glaeser and Gottlieb (2009).[7]

The results on direct effects relate to a large existing literature on wage differentials between foreign-owned and domestic-owned firms. Doms and Jensen (1998), Feliciano and Lipsey (1999), and several others find that the average wage at foreign-owned firms is higher than that at domestic-owned firms in the United States. We document in the U.S. tax data that wages are 19% higher on average at foreign firms relative to domestic non-multinationals, controlling for observables. Prior studies in other countries have found that the foreign wage premium only explains a small share of the wage differential between foreign-owned and domestic-owned firms (see Heyman, Sjöholm, and Tingvall 2007; Balsvik 2011; Hijzen et al. 2013). Our estimate of a 7%

6. According to data retrieved from the subsidy tracker database of the policy group Good Jobs First, the foreign-firm share in total annual economic development subsidies in the United States between 2012 and 2017 is about 20%. The so-called mega-deals (with subsidies larger than \$50 million) account for about half of all subsidies to foreign firms.

7. For the analysis of local labor market benefits of various place-based policies, see Gaubert (2018) and Ossa (2017), who model local policy makers using subsidies to compete for firms in spatial equilibrium with agglomeration. Other related studies include business relocation responses to state-level corporate tax changes (Suarez Serrato and Zidar 2016), agglomeration effects of infrastructure investment (Kline and Moretti 2013), and indirect effects of employment tax credits (Busso, Gregory, and Kline 2013).

foreign-firm premium implies that two-thirds of the foreign wage differential is the result of worker skill differentials across firms. Thus, the average wage differential shrinks substantially, but is still positive, when accounting for worker skill composition. One possible explanation for the significant wage premium for workers at foreign multinationals is that the United States is relatively remote from its major sources of foreign firms (e.g., Europe and Asia), and therefore the selected firms that establish affiliates in the United States are especially productive (Helpman, Melitz, and Yeaple 2004). These firms may also benefit from economies of scale associated with their operations in multiple countries. Another possibility is that firms anchor their wages to headquarter levels, as suggested by Hjort, Li, and Sarsons (2020).

The results on indirect effects relate to a number of studies on productivity spillovers outside the United States. This literature has found diverse effects. Aitken and Harrison (1999) and Lu, Tao, and Zhu (2017) find negative effects from foreign multinationals on the revenue productivity of domestic firms in the same industry in Venezuela and China, respectively.[8] A number of papers find positive effects on productivity at domestic-owned firms, sometimes associated with buyer-supplier linkages (see Javorcik 2004; Haskel, Pereira, and Slaughter 2007; Alfaro and Chen 2018; Jiang et al. 2018; Kee 2015; Alfaro-Urena, Manelici, and Vasquez 2019a; 2019b). Poole (2013) finds positive effects on wages at domestic firms from a greater share of coworkers with experience at foreign firms in Brazil, and Driffield and Girma (2003) find that foreign-firm entry causes domestic firms to bid up wages. In the U.S. context, Figlio and Blonigen (2000) use variation in foreign investment across counties in South Carolina to find positive effects on county average wages. Analyzing data on publicly traded firms in the United States, Keller and Yeaple (2009) find positive productivity spillovers from foreign investment on other firms in the same industry.[9] Greenstone, Hornbeck, and Moretti (2010) use a runner-up identification strategy for million-dollar

8. Consistent with competition effects, Atkin, Faber, and Gonzalez-Navarro (2018) document a decline in Mexican grocery store prices in response to entry by foreign retailers. See Gorg (2004) for a survey of the empirical literature on FDI spillovers.

9. Other related work on the indirect effects of foreign multinationals in the United States includes Aitken, Harrison, and Lipsey (1996), Branstetter (2001), and Blonigen and Slaughter (2001).

manufacturing plant openings, many of which are owned by multinationals, finding sizable productivity gains for local firms. We contribute to this literature by providing a novel identification strategy for the indirect effects of foreign firms and estimating these effects in comprehensive data on workers and firms.

II. Data and Descriptive Evidence

II.A. Data

We now discuss data sources and sample construction; see Online Appendix A for additional details. We construct a matched worker-firm panel data set from the population of annual U.S. Treasury tax filings from 1999 to 2017. For each worker-firm-year, W-2 tax forms provide information on earnings, the firm's employer identification number (EIN, which is masked to us), and the worker's residential ZIP code.[10] Earnings are defined as all remuneration for labor services deemed taxable by the IRS, including wages and salaries, bonuses, and exercised stock options. We obtain year of birth and sex information from SSA birth records. Following Lamadon, Mogstad, and Setzler (2020), the analysis sample focuses on workers between age 25 and 60 at the highest-paying employment relationship in each worker-year with earnings above the full-time equivalence (FTE) threshold, approximated by the annualized minimum wage.

For each firm-year, Forms 1120 (C-corporations), 1120S (S-corporations), and 1065 (partnerships) provide information on value added and the six-digit NAICS industry code, where value added equals sales minus cost of goods sold.[11] We refer to the three-digit NAICS code as the firm's industry and consider the full six-digit code for robustness.[12] Foreign ownership is indicated

10. In the event that the ZIP code is missing or invalid in year t but not in year s with $|t-s| \leqslant 2$, and the worker receives a W-2 from the same EIN in t and s, we impute it in t using the value from s.

11. In manufacturing and mining industries, the cost of goods sold contains production wages (labor compensation to workers directly involved in the production process). We construct a measure of production wages to add back into value added for these sectors (the difference between total wages associated with the firm through worker tax forms and nonproduction wages reported by the firm).

12. In the event that the NAICS code is missing or invalid in year t but not in year s with $|t-s| \leqslant 2$, we impute it in t using the value from s. If this also fails, we impute it from a separate filing, Form 5500.

by the filing of Form 5472, which is the information return for a U.S. corporation that is 25% or more foreign owned and includes the country of foreign ownership. We link worker data to firm data using the EIN. We keep only those firms that have at least one FTE worker. We use the terms "foreign" and "foreign-owned" interchangeably throughout.[13] We consider a firm to be a domestic multinational if it does not file Form 5472 but pays a foreign business tax. Because of difficulties in interpreting value added, we omit the finance, insurance, and real estate (FIRE) industries from all analysis.

To our knowledge, ours is the first panel data set for the United States that links the population of workers and firms with foreign ownership information of the firms. However, working with these data presents two challenges. First, since corporate tax filings provide the foreign ownership information, while the W-2 forms provide the information on employment and wages, we can only classify the foreign status of a worker's firm for those workers whose EIN on the W-2 is also associated with a corporate tax filing. As emphasized by Yagan (2019), many workers cannot be linked to a corporate tax filing, often because the employer is not required to file (especially if the employer is a government or nonprofit organization) or because the employer is a subsidiary and only the parent corporation files while the subsidiary uses its distinct EIN to issue W-2 forms. To overcome this challenge, we combine two sources of information on subsidiary linkages. The first source is Schedule K, line 3b, which provides the EIN of the parent corporation in the years in which the subsidiary is a filer, from which we learn the EIN of the parent corporation in future years in which the subsidiary is a nonfiler. The second source is the Affiliations Schedule from Form 851, which defines a subsidiary as 80% owned by another corporation. However, we only observe a running list of parent-subsidiary relationships taken from the Affiliations Schedules through 2016, so changes over time due to extensive-margin changes in subsidiary relationships may

13. Similarly, we refer to "domestic" and "domestic-owned" firms interchangeably. We note that even a domestic-owned firm could be in the hands of many small foreign owners, in particular when the company is publicly listed. Although we do not have hard data on this, we think these cases are likely to be rare and not necessarily associated with the same effects. In the event that the employer fails to file Form 5472 in year t but files as foreign owned with ownership country c in one of $(t-2, t-1)$ as well as one of $(t+1, t+2)$, we impute foreign ownership in year t as c.

be mismeasured when using the second source. For this reason, we only use the second source for subsidiary linkages that are not covered by the first source (i.e., subsidiaries that are missing Schedule K filings).

The second challenge is that our analysis requires a firm's activity to be associated with each commuting zone in which it is active. This differs from using the address of the firm's headquarter to define its location, as the headquarter may be chosen to obtain favorable state-level tax rates rather than represent the firm's actual location of activity, and the firm may be active in many locations. Because specific establishments of multiestablishment firms are not observable in U.S. tax data, we follow Yagan (2019) by inferring firms' commuting zone–level operations from workers' residential locations. We aggregate the number of workers and wages in the commuting zone of the worker's address on the W-2 to define the firms' local employment and wage bill. However, we do not observe value added at the firm commuting zone–level directly because it is reported only on firm-level tax forms. To overcome this challenge, we use the share of the wage bill paid in the commuting zone of each firm to allocate value added to commuting zones. For example, if 75% of a firm's wage bill is paid in the first commuting zone and 25% in the second commuting zone, we allocate 75% of value added to the first and 25% to the second.

We validate that the data are representative of the share of workers employed by foreign firms using statistics from the BEA and BLS. We find that between 5% and 6% of American workers are employed at foreign firms and the average worker at a foreign firm earns 25% more than the average worker at a domestic firm, which match the statistics from the BLS (2019). Online Appendix Figure A1 visualizes the share of American workers employed at foreign firms between 1977 and 2017. It compares three series available from the BEA to the series we construct from tax data. Each series follows different sample selection rules, yet during the years of overlap, the series are generally consistent. This figure also illustrates the striking rise in the importance of foreign-owned firms in the U.S. labor market. Only 2% of workers were employed by foreign-owned firms in the late 1970s, whereas around 6% are employed by foreign-owned firms today.

II.B. Descriptive Statistics

1. Differences between Foreign and Domestic Firms. Online Appendix Table A1 provides summary statistics on foreign and domestic firms for 2015. Four clear differences can be noted. First, the average foreign firm operates in about seven locations, whereas the average domestic firm operates in about two locations. Second, the average foreign firm is much larger than the average domestic firm, with about 28 workers per domestic firm and 172 workers per foreign firm. Third, value added per worker in the analysis sample is $220,100 at foreign firms and $153,100 at domestic firms, indicating that value added per worker is more than 40% higher at foreign firms. Fourth, the average worker in the analysis sample earns $75,700 at foreign firms and $60,700 at domestic firms, indicating 25% higher wages at foreign firms.[14]

2. Spatial Distribution of Foreign Employment. In Online Appendix Figure A3(a), we plot the share of workers employed at foreign firms in 2001 for each commuting zone. We find particularly high levels of employment at foreign firms along the East Coast and in Rust Belt cities in Indiana, Michigan, and Ohio, but especially low levels in the South. In Online Appendix Figure A3(b), we illustrate the changes in the share of employment at foreign firms by commuting zone from 2001 to 2015. Substantial changes have taken place across the United States, with Gulf Coast states such as Alabama, Louisiana, and Mississippi experiencing especially rapid growth, while parts of the East Coast and the Rust Belt have experienced sharp declines in the share of foreign employment.

3. Clustering by Nationality. In Figure I, we display the share of employment at Canadian, Western European, and East Asian firms as a share of total employment at foreign-owned firms by commuting zone, based on total FTE worker-year observations

14. Relatedly, Online Appendix Figure A2 provides value added and wage differentials (relative to the average domestic nonmultinational firm) by country of origin for the 34 countries with the most unique firms operating in the United States during 2010 to 2015. Specifically, we select the 40 countries with the most firms in 2010–2015 and drop 5 tax haven countries (e.g., the Cayman Islands) as well as the "other country" category. We see a clear pattern that the value added and wage differentials between foreign and domestic firms are greater for countries of origin with higher GDP per capita.

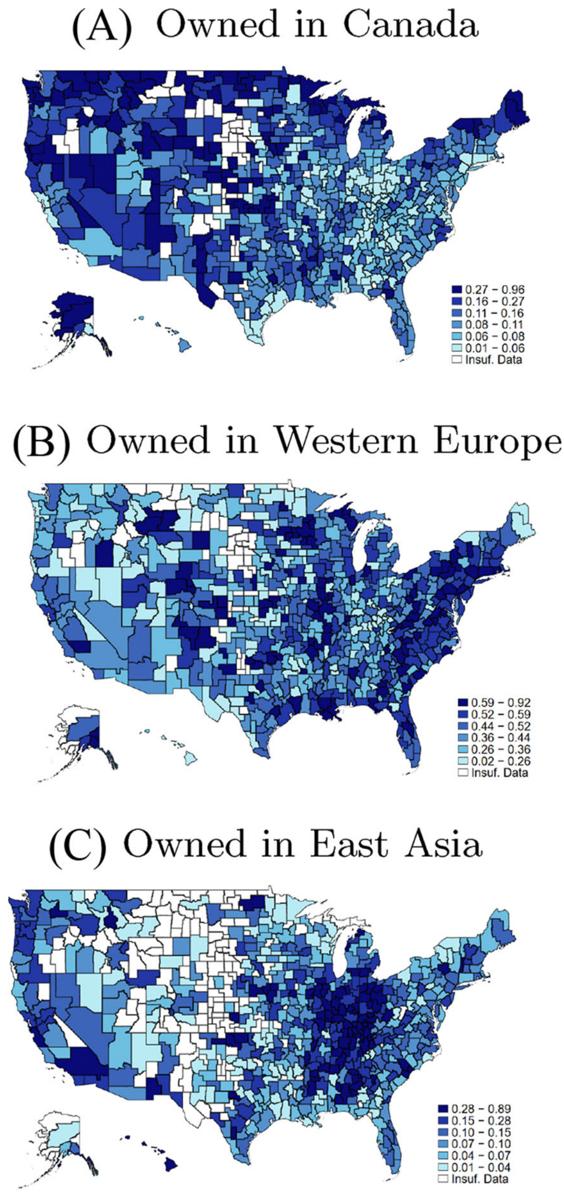

FIGURE I

Geographic Clustering of Foreign Firms by Country of Origin

The figures display spatial variation in the concentration of foreign employment that is at firms owned in particular groups of owner countries based on total FTE worker-year observations from 1999 to 2017.

from our sample. A clear visual pattern emerges: Canadian firms are more likely to be near the Canadian border, European firms are primarily engaged in the eastern part of the United States, and Asian firms account for a large share of foreign-owned firms near the West Coast as well as in the Midwest.

There are a number of plausible reasons firms cluster by nationality. First, the cost of shipping intermediate goods from the home country or the costs of communication may lead to clustering on distance (Keller and Yeaple 2013) or clustering on the availability of airline routes to the headquarter (Giroud 2013; Campante and Yanagizawa-Drott 2018). Second, foreign firms may be more likely to hire employees (in particular, managers) from their country of origin that already had business experience at the firm's headquarter, who may prefer to live near other immigrants from their country.[15] Third, foreign firms of a particular country of origin may share information, for example, by using similar plant site selection firms that already have business and political contacts in certain regions. Fourth, firms may cluster by industry, and some countries specialize in particular industries (Head, Ries, and Swenson 1995). This clustering by country of ownership will be important when discussing our identification strategy for indirect effects in Section V.

III. A Model of Foreign Multinationals

In this section, we develop a model in which foreign multinationals pay wages that are different from those of domestic firms to a worker of a given skill type (direct effects) and affect outcomes at local domestic firms (indirect effects). Rather than from foreignness per se, direct wage effects arise because more-productive firms need to pay higher wages to recruit their marginal employee. Furthermore, firms belonging to a multinational network may have access to more skill-augmenting technology, leading them to disproportionately employ higher-skilled labor and pay a greater premium to higher-skilled labor relative to lower-skilled labor. Indirect effects can arise from technology spillovers—which are beneficial to domestic firms—and competition effects—which are harmful to domestic firms. For brevity, the main text develops the case with two skill types (skilled and

15. Relatedly, Burchardi, Chaney, and Hassan (2019) document for the United States that foreign investment follows past ancestors' regional choices.

unskilled) and two firm nationalities (foreign and domestic). Online Appendix C provides derivations. Online Appendix D provides a more general case with an arbitrary number of skill types and firm types that differ by country of origin.

III.A. Model

1. Environment. We assume there is a large set of locations in the United States. All regions are trading frictionlessly within the United States, and workers are immobile across locations. We focus on the outcomes in one particular location and, to simplify notation, omit the location subscript. Let $N \in \{D, F\}$ denote the firm's country of origin, where D is domestic and F is foreign. Denote by M_N the number of firms of nationality N. Let $h \in \{s, u\}$ denote the skill type of a worker, where s denotes skilled and u denotes unskilled. Denote by L_{Nh} the number of employees at firms from nationality N with skill level h, and $L_N = \sum_h L_{Nh}$ is the total number of employees for nationality N. The share of workers that are skilled in a nationality N firm is $C_N \equiv \frac{L_{Ns}}{L_N}$. Each region is equipped with $\bar{L}_h$ potential employees of skill type h, and the employment rate is $E_h \equiv \frac{\sum_N L_{Nh}}{\bar{L}_h}$. In each location, the composition of skilled workers by nationality, C_N, as well as the local employment rate, E_h, are equilibrium objects.

2. Technology. Each firm produces a homogeneous good q that is freely traded, where the price is normalized to 1. A firm of nationality $N \in \{D, F\}$ produces using technology,

$$q_N(\ell_u, \ell_s) = \phi_N \left(\ell_u + \zeta_{Ns}\ell_s\right), \tag{1}$$

where ϕ_N is total factor productivity (TFP) and ζ_{Ns} is skilled labor augmenting productivity. We assume, and later provide evidence, that foreign firms are more productive than domestic firms in their usage of both unskilled labor (i.e., $\phi_F \geqslant \phi_D$) and skilled labor (i.e., $\phi_F \zeta_{Fs} \geqslant \phi_D \zeta_{Ds}$). Helpman, Melitz, and Yeaple (2004) provide a micro-foundation in which foreign firms are more productive because they must overcome a larger fixed cost of entry. Although we take the TFP of foreign firms $\phi_F > 1$ as determined prior to market entry, we allow for spillovers of TFP from foreign to domestic firms as

$$\phi_D = 1 + \tau \frac{L_F}{L_D + L_F}(\phi_F - 1), \tag{2}$$

where $0 \leqslant \tau \leqslant 1$ is the spillover rate. When $\frac{L_F}{L_D+L_F}$ is greater, domestic firms are more exposed to foreign multinationals, and τ determines the sensitivity to this exposure.

3. Labor Supply. Let w_{jh} denote the wage offered by firm j to a worker of skill type h. The utility of worker i when employed at a given firm j with wage offer w_{jh} is,

$$V_{ij} = \log w_{jh(i)} + \epsilon_{ij}, \tag{3}$$

where the wage of the outside option (nonemployment) is w_0. Unobserved preferences ϵ_{ij} can be determined by a wide range of characteristics, such as distance of the firm from the worker's home. Following recent work by Card et al. (2018), Lamadon, Mogstad, and Setzler (2020), and Berger, Herkenhoff, and Mongey (2019), we parameterize ϵ_{ij} as i.i.d. type 1 extreme value with dispersion $\frac{1}{\eta}$. When ϵ_{ij} is more dispersed (i.e., $\frac{1}{\eta}$ is greater), our preference specification allows workers to view firms as worse substitutes. Letting ℓ_{jh} denote the number of workers of skill type h in firm j, the implied labor supply to firm j is

$$\ell_{jh} = w_{jh}^{\eta} \frac{\bar{L}_h}{W_h}, \tag{4}$$

where $W_h = \sum_{k=0}^{M_D+M_F} w_{kh}^{\eta}$ is the aggregate wage index. Equation (4) shows that η can be interpreted as the firm-specific labor supply elasticity.

4. Labor Demand. Since ϵ_{ij} is unobserved to the firm, firms cannot price discriminate on idiosyncratic preferences and thus post a common wage for all workers of skill type h. We assume that there are many firms of its type in its region, so each firm acts monopsonistically competitively, meaning it does not take the effect of its own choice of w_{jh} or ℓ_{jh} on W_h into account. Given the production function in equation (1) and labor supply in equation (4), and normalizing $\zeta_{Nh} = 1$ for $h = u$, a firm with nationality N offers wage

$$w_{Nh} = \frac{\eta}{\eta+1}\phi_N \zeta_{Nh} \quad N \in \{D, F\}, \quad h \in \{s, u\}. \tag{5}$$

Because $\phi_N \zeta_{Nh}$ is the marginal product of labor for skill type h at a firm of nationality N, $\frac{\eta}{\eta+1}$ is the markdown on the marginal product of labor.

III.B. Direct Effects

From equation (5), the mean difference in log wages between foreign and domestic firms is

$$\underbrace{\mathbb{E}[\log w_{F\cdot}] - \mathbb{E}[\log w_{D\cdot}]}_{\text{Total foreign wage differential}} = \underbrace{\log \phi_F - \log \phi_D}_{\text{Unskilled foreign-firm premium}} + \underbrace{C_F \log \zeta_{Fs} - C_D \log \zeta_{Ds}}_{\text{Composition-weighted skilled foreign-firm premium}}. \tag{6}$$

In the absence of skill-augmenting technology ($\zeta_{Fs} = \zeta_{Ds} = 1$), skill composition is the same in foreign and domestic firms ($C_F = C_D$), so the total foreign wage differential simplifies to the productivity difference ($\log \phi_F - \log \phi_D$). For the more interesting case in which technology is skill augmenting, we summarize equation (6) with the following proposition:

PROPOSITION 1 (Direct effects). *If the TFP of foreign firms is greater than domestic firms (i.e., $\phi_F > \phi_D \geqslant 1$) and the production technology at foreign firms is more skill augmenting relative to domestic firms (i.e., $\zeta_{Fs} > \zeta_{Ds} \geqslant 1$), then*

(i) *The unskilled foreign-firm premium is positive;*
(ii) *The skilled foreign-firm premium is greater than the unskilled foreign-firm premium;*
(iii) *The skill composition is greater at foreign firms (i.e., $C_F > C_D$).*

III.C. Indirect Effects

We next investigate the indirect effects (i.e., the effects of entry and expansions by foreign firms on domestic firms). Because of the complex nature of the model, our focus is on providing the predicted effects of foreign-firm entry based on first-order approximations. Let $\Delta y \equiv y' - y$ denote a change to y. The effects of interest center on $\hat{X} \equiv \frac{\Delta L_F}{L_D + L_F}$, which is a small perturbation in employment at foreign firms relative to initial employment at all firms, and we take the initial equilibrium to feature a small share of employment at foreign firms when deriving the first-order approximation of equilibrium outcomes.

1. Wage. A first-order approximation of the wage at domestic firms yields the prediction

$$\underbrace{\Delta \log(w_{Dh})}_{\text{Domestic-firm wage change}} \approx \underbrace{\tau(\phi_F - 1)\hat{X}}_{\text{Technology spillover effect}}. \tag{7}$$

This equation states that the wage increase at domestic firms is proportional to the TFP increase at domestic firms.[16] The magnitude of the TFP increase depends on the spillover rate τ, the relative productivity of foreign firms $\phi_F - 1$, and the relative size of entering foreign firms $\hat{X}$.

2. Employment. Let $\bar{E}_N \equiv C_N E_s + (1 - C_N)E_u$ denote the nationality skill composition–weighted average labor market tightness. A first-order approximation for employment at a domestic firm is

$$\underbrace{\Delta \log(\ell_{Du} + \ell_{Ds})}_{\text{Domestic-firm employment change}} \approx \underbrace{\tau\eta(\phi_F - 1)(1 - \bar{E}_D)\hat{X}}_{\text{Technology spillover effect}} - \underbrace{\bar{E}_F \hat{X}}_{\text{Competition effect}}. \tag{8}$$

The equation shows that the employment response at domestic firms can range from negative to positive. Because of labor market competition effects, the model without productivity spillovers (i.e., $\tau = 0$) implies a decline in the output at domestic firms as the activity by foreign firms in a location increases. With large enough productivity spillovers, employment at domestic firms increases when the employment share at foreign firms grows. If the labor market is less tight (lower $\bar{E}$) or labor supply is more elastic (higher η), the technology spillover effect becomes stronger. Furthermore, competition effects are weaker when the labor market is less tight.

3. Value Added and Wage Bill. Denote by $R_N \equiv \frac{\zeta_{Ns}\ell_{Ns}}{\ell_{Nu}+\zeta_{Ns}\ell_{Ns}}$ the share of output at a firm with nationality N that is produced by skilled workers. The object R_N differs from C_N in that it depends

16. We show in Online Appendix C that this prediction does not rely on the first-order approximation and holds more generally (i.e., $\frac{dw_{Dh}}{dM_F} > 0$ if $\tau > 0$ and $\frac{dw_{Dh}}{dM_F} = 0$ if $\tau = 0$).

on the skill-augmenting productivity ζ_{Ns}. Using a first-order approximation,

$$\underbrace{\Delta \log q_D}_{\text{Domestic-firm value-added change}} \approx \underbrace{\tau(\phi_F - 1)\big(1 + \eta\big[1 - R_D E_s - (1 - R_D)E_u\big]\big)\hat{X}}_{\text{Technology spillover effect}} - \underbrace{\left(\frac{C_F}{C_D} R_D E_s + \frac{1 - C_F}{1 - C_D}(1 - R_D)E_u\right)\hat{X}}_{\text{Competition effect}}. \tag{9}$$

Since the value added and wage bill are proportional, equation (9) is also the first-order approximation to the log change in the wage bill. Similar to the employment response, the change in value added at domestic firms can range from negative to positive, depending on the same set of factors as the employment response but also depending on R_D, C_D, and C_F.

4. Value Added per Worker. Whether the log value added response (equation (9)) exceeds the log employment response (equation (8)) at domestic firms, and hence value added per worker increases, turns on various factors. In the simple case in which skilled and unskilled labor are symmetric (i.e., $C_F = C_D$, $E_s = E_u$, and $R_D = 0.5$), value added per worker must increase in the presence of technology spillovers in response to foreign-firm entry. However, if foreign firms are more skill intensive ($\frac{C_F}{C_D} > 1$), an expansion in employment at foreign firms leads domestic firms to substitute toward unskilled labor. All else equal, the substitution toward unskilled labor lowers value added per worker at domestic firms. Therefore, value added per worker at domestic firms could decrease even in the presence of positive technology spillovers. A similar argument holds for the wage bill per worker—unskilled workers receive lower wages, so substitution toward unskilled labor lowers the wage bill per worker, all else equal.[17]

17. For this reason, it is preferred to measure the indirect effects on wages (equation (7)) using continuing workers rather than the wage bill per worker in the empirical application below.

We summarize the above indirect-effect predictions in a proposition:

PROPOSITION 2 (Indirect effects). *If the TFP of foreign firms is greater than domestic firms ($\phi_F > \phi_D \geqslant 1$) and foreign firms have positive spillovers onto domestic firms (i.e., $\tau > 0$), then—up to a first-order approximation around an initial equilibrium featuring a small share of employment at foreign firms—an increase in the share of employment at foreign firms causes*

(i) *A positive effect on wages at domestic firms;*
(ii) *A positive effect on employment, the wage bill, and value added at domestic firms if $\tau(\phi_F - 1)$ is sufficiently large or E_s and E_u are sufficiently small;*
(iii) *Ambiguous effects on value added per worker and the wage bill per worker at domestic firms.*

III.D. Model Extensions and Limitations

Before proceeding to the empirics, we note several limitations of the model. Clearly, the model is highly stylized with only two types of workers and two types of firms. In Online Appendix D, we provide a more general case with an arbitrary number of skill types and firm types that differ by country of origin (where firms from different countries of origin can have access to different technologies). Regarding direct effects, our model predicts that firms from countries of origin with more skill-augmenting technology will disproportionately employ higher-skilled labor and pay a greater premium to higher-skilled labor relative to lower-skilled labor. We confirm this prediction in the next section when estimating a wage model with many skill types and many firm types.

By assuming that output is freely tradable, the model abstracts away from the product market competition effects associated with foreign-firm entry in a commuting zone. See Bloom, Schankerman, and Van Reenen (2013) for a method of separating product market competition effects from technology spillover effects. Furthermore, the simple model abstracts away from input-output linkages between firms. Access to cheaper local inputs or an increase in local demand would affect domestic firms' outcomes in a similar way as technological spillovers.

IV. Direct Effects of Foreign Multinationals

We next empirically examine the direct effects of foreign multinationals on workers in the United States. Our primary goal is to disentangle the extent to which higher wages at foreign-owned firms are due to worker skill differentials as opposed to firm premiums. We leverage the U.S. data to make four novel contributions about the direct effects of foreign investment. First, this is the first article to estimate the foreign-firm premium in the United States that controls for worker skill differentials. We find that the average foreign-firm premium is 7%. Second, because the United States is both the leading headquarter country of multinationals and the top recipient of foreign investment, it provides large samples of both foreign and domestic multinationals. We provide the novel finding that domestic-owned and foreign-owned multinationals have very similar premiums. Third, because the United States is the top recipient of the world's foreign investment, it provides a rare opportunity to compare the effects of foreign firms by country of origin, with large samples from many diverse countries. We reach the novel finding that the foreign-firm premium is increasing in the GDP per capita of the origin country. Fourth, it has long been posited that high-skilled workers benefit more from foreign investment, primarily in developing contexts (e.g., Aitken, Harrison, and Lipsey 1996). We provide the first systematic evidence in favor of this hypothesis in the United States.

IV.A. *Estimation Strategy for the Foreign-Firm Premium*

We now consider estimating the equilibrium wage equation (5) from Section III, but with the extension derived in Online Appendix D to allow for an arbitrary number of firm and worker types. For simplicity, we initially restrict the skill-augmenting technology parameter to be constant across firms. Under this restriction, the equilibrium wage setting with many skill and firm types is[18]

$$\log w_{i,t} = \psi_{j(i,t)} + x_i + \chi_{i,t}'\beta + \epsilon_{i,t}, \tag{10}$$

18. The derivation of equation (10) is provided in Online Appendix D without ϵ. We include the idiosyncratic unobservable ϵ in the empirical implementation to allow for measurement error. We provide estimates when allowing for heterogeneous skill-augmenting productivity parameters in Section IV.C.

where $j(i, t)$ denotes firm j that employs worker i in year t, ψ denotes the firm premium, x denotes worker skill, and χ denotes a vector of observable determinants of earnings.[19] Our main specification estimates equation (10) for 2010–2015 on the largest connected set of firms, with robustness checks presented below.[20] In χ, we control for location-year fixed effects, industry-year fixed effects, and a third-order polynomial in the age of the worker.

Our aim is to estimate equation (10) to characterize differences in ψ and x across countries of ownership. Equation (10) is identical to the two-way fixed effects regression proposed by Abowd, Kramarz, and Margolis (1999). The key identifying assumption for this regression is that workers do not select to move into firms based on the idiosyncratic error ϵ. However, selection based on the worker effects x, firm effects ψ, or observable controls χ does not violate identification. Card et al. (2018) propose an event study representation to visualize potential selection on ϵ. If the log wage residuals (controlling for χ) are on different trends for those who move into different firm types, this suggests workers select on ϵ, as x and ψ are time invariant. Because our goal is to identify the premium for foreign versus (nonmultinational) domestic firms, we consider an analogous event study for workers who move between foreign and domestic firms in Online Appendix E. As demonstrated in Online Appendix Figure A4, there is little evidence of pretrends prior to the moves, which is consistent with a measurement error interpretation of ϵ. Furthermore, when restricting to the sample of workers who lose their jobs in a mass layoff (and therefore are even less likely to select to move based on individual-specific idiosyncratic errors), pretrends are virtually the same as in the full sample.[21]

An important difficulty in estimating equation (10) remains. As shown by Andrews et al. (2008), limited mobility makes it challenging to precisely estimate firm premiums and worker effects. The earnings changes for workers who move across firms provide the identifying content on firm premiums, and the bias

19. Song et al. (2018) and Lamadon, Mogstad, and Setzler (2020) also estimate equation (10) on the U.S. tax data, but do not examine foreign ownership.

20. Equation (10) is typically estimated on short time intervals, as fixed effects are a worse approximation to the wage structure over a longer period of time (see the discussion by Lamadon, Mogstad, and Setzler 2020 and Lachowska et al. 2020).

21. We follow Yagan (2019) in using a 30% separation rate to define a mass layoff event.

in those firm premium estimates declines as the number of movers per firm grows. However, the modal firm in the United States has a single mover, providing the opportunity for massive limited-mobility bias in our context. To address this, we follow the approach of Bonhomme, Lamadon, and Manresa (2019) and estimate a set of grouped fixed effect models. Instead of obtaining a fixed effect for each firm, we allocate all firms in our data to $k = 10$ clusters ($k = 20$, 30, 40, 50 in robustness checks) with similar wage structures using k-means cluster analysis.[22] These clusters preserve the wage structure while reducing the number of fixed effects that must be estimated. Indeed, we find that 86% (92%) of all between-firm earnings variance is captured by only these 10 (50) clusters. Because there is much more mobility between these clusters than between the millions of unique firms, any bias should be mitigated. Last, by providing a parsimonious representation of firm heterogeneity, the k-means clustering procedure will also make it feasible to estimate the more-general model in which skill-augmenting productivity parameters are heterogeneous across firm types and, therefore, workers of different skill levels receive different premiums.

IV.B. Main Results on Foreign-Firm Premiums

We now provide the main estimates from equation (10). Throughout the analysis, we take domestic nonmultinationals as the reference group of firms. We treat domestic multinationals as a distinct group of firms so that we can investigate the similarity between domestic and foreign multinationals. Controlling for the observables listed above, the average worker at a foreign multinational earns 19.5% more than the average worker at a domestic nonmultinational, while workers at domestic multinationals earn 23.0% more on average. Using the estimates based on equation (10), we find that the average firm premium is 7.2% at foreign multinationals and about 8.4% at domestic multinationals. From the decomposition in equation (6) (and the analogous expression with many skill and firm types in Online Appendix D),

22. Lamadon, Mogstad, and Setzler (2020) are the first to provide bias-corrected estimates of firm premiums and sorting for the United States. Using the grouped fixed effects approach, they find that the variance of firm premiums is inflated by a factor of about three when ignoring limited-mobility bias, whereas the correlation between worker skill and firm premiums is deflated by a factor of about four.

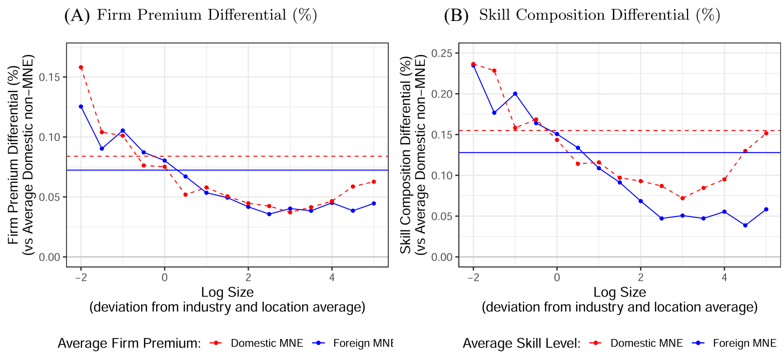

FIGURE II

Comparison of Foreign and Domestic Multinationals

This figure presents estimates of the model in equation (10) from the grouped fixed effect estimator during 2010–2015. The horizontal axis is an equally spaced grid of width 0.5 in the residual log firm size distribution, where each unit is associated with the nearest grid point. The vertical axis is the difference in the average firm premium (Panel A) or average worker skill level (Panel B) for foreign (blue, solid lines) or domestic (red, dashed lines) multinationals, relative to the average domestic nonmultinational in the same size bin. The horizontal lines indicate the overall averages (not conditional on a size bin).

this indicates that at both foreign and domestic multinationals, about two-thirds of the residual wage differential is due to a greater composition of high-skill workers at foreign multinationals relative to domestic nonmultinationals. Recall that we control for industry-year and commuting zone–year fixed effects in all direct-effects estimation, so reported differentials in log earnings, firm premiums, and worker composition do not reflect location or industry selection.

In Figure II, we show that the average firm premiums and worker compositions of foreign and domestic multinationals track one another closely across the firm size distribution. This evidence suggests that belonging to a multinational network, rather than foreignness, is the main driver of the foreign-firm premium. Multinational firms are more productive through selection—it is the most-productive firms that can overcome the entry costs to establish foreign affiliates (Helpman, Melitz, and Yeaple 2004). Furthermore, belonging to a multinational network confers productivity advantages through access to additional sources of inputs and technology. An implication is that domestic and foreign multinationals are expected to be more productive and thus have

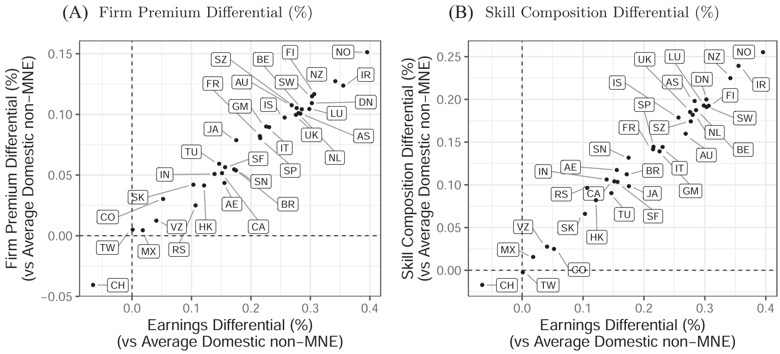

FIGURE III

Foreign-Firm Premiums and Worker Skill Composition

This figure presents estimates of equation (10) from the grouped fixed effect estimator during 2010–2015. The vertical axis is the difference in the average firm premium (Panel A) or average worker skill level (Panel B) for foreign multinationals with the countries of ownership indicated by the labels, relative to the average domestic nonmultinational.

substantial firm premiums relative to the reference group. Relatedly, Bloom and Van Reenen (2007, 2010) find that management practices are similar and of high quality for multinational relative to domestic firms across countries of ownership.

Both domestic and foreign multinationals hire more skilled workers compared with nonmultinationals. The difference in the skill composition holds across the entire firm size distribution but is particularly pronounced when comparing a smaller multinational firm to a nonmultinational firm in the same size bin (Figure II, Panel B). Similarly, the multinational wage premium appears to be highest when comparing between smaller firms (Figure II, Panel A). The similar shape of the multinational wage premium and skill composition differences across the size distribution may be related. In Section IV.C, we explore the extent to which the wage premium differs by worker skill type.

An important feature of the U.S. data is that there are sufficiently many unique firms from a large number of foreign countries to estimate country-specific foreign firm premiums. Figure III, Panel A plots the mean firm premium estimate for the 34 countries of ownership with the most firms against mean log earnings, where mean log earnings is normalized to be zero at domestic nonmultinational firms. We find substantial heterogeneity in the firm premium by country of origin. The Northern European countries of Norway, Finland, Sweden, and Denmark, as well as Ireland and New Zealand, have larger than average firm premiums. At the other extreme, small positive firm premiums are estimated for Colombia, Mexico, Russia, Taiwan, and Venezuela, while a negative 4 percent premium is estimated for China. The share of the wage differential explained by firm premiums is approximately the same across all countries at around 37%. This means that countries that offer higher premiums also attract more talented workers, as shown in Figure III, Panel B.

There are many possible reasons for this heterogeneity across countries of ownership. As the cost of entry increases, we expect the average premium of entering firms to increase.[23] Another possibility is that firms anchor their wages to headquarter levels, as suggested by Hjort, Li, and Sarsons (2020). Finally, it could be that countries with greater GDP per capita have access to more skill-augmenting technology (Caselli and Coleman 2006), which could explain higher firm premiums (we explore this case analytically in the model of Online Appendix D). To investigate this issue, Online Appendix Figure A6(a) plots the mean firm premium estimate for these countries of ownership against log GDP per capita, observing a clear pattern that countries of ownership with higher GDP per capita provide greater average premiums to their workers. Regressing the average firm premium on log GDP per capita and log distance from the United States yields a highly statistically significant coefficient of 0.031 for log GDP per capita and a statistically insignificant coefficient of 0.011 for log distance. This suggests that GDP per capita is more important than distance in explaining country heterogeneity in the firm premium. We find a similar pattern for average skill composition by GDP per capita in Online Appendix Figure A6(b). These findings are consistent with countries with higher GDP per capita having access to more skill-augmenting technology, leading to a higher composition of skilled workers and greater premiums as GDP per capita rises.

IV.C. Extension to Allow for Skill-Augmenting Productivity

The model of Section III allows for skill-augmenting productivity to differ between foreign and domestic firms.

23. Distance is a suggested mechanism by Helpman, Melitz, and Yeaple (2004). Egger, Jahn, and Kreickemeier (2018) find a pattern of foreign-firm wage differentials that increase in distance to the headquarter country in Germany.

In Online Appendix D, we generalize this model to allow for an arbitrary number of firm and worker types, which yields a more general regression,

$$\log w_{i,t} = \psi_{j(i,t)} + \theta_j x_i + \chi_{i,t}' \beta + \epsilon_{i,t}, \tag{11}$$

where, if θ_j is greater at foreign relative to domestic firms on average, then foreign multinationals have more skill-augmenting technology and in turn pay a greater relative premium to high-skilled workers.[24] Bonhomme, Lamadon, and Manresa (2019) provide a method for estimating equation (11); for brevity, we review the main results here while providing a detailed explanation of the estimator and findings in Online Appendix F.

We find that the foreign-firm premium is monotonically increasing in the skill of workers compared to the premium offered by domestic nonmultinationals to workers of the same skill level. Foreign multinationals pay a 19% greater premium to workers in the top skill decile, but a 1% negative premium to workers in the bottom skill decile. Furthermore, we find that domestic-owned multinationals pay a 21% greater premium to workers in the top skill decile than domestic nonmultinationals, but no premium to workers in the bottom skill decile. These results are consistent with multinationals having more skill-augmenting technology than nonmultinationals. Skill-augmenting technology would lead multinational firms (both foreign-owned and domestic-owned) to bid up the price of local labor for skilled workers such as managers, as found by Bloom et al. (2019), but not bid up the price of routine labor.

IV.D. Robustness of the Foreign-Firm Premium Estimates

Our main estimate of the average foreign-firm premium is robust to various alternative specifications. First, the grouped fixed effects estimator of equation (10) requires specifying the number of clusters to use in the k-means algorithm. Online Appendix Figure A7 demonstrates that the results are nearly identical when allowing for 10, 20, 30, 40, or 50 clusters, with an

24. Note that equation (10) is the special case of equation (11) in which $\theta_j = \bar{\theta}, \forall j$, that is, the skill-augmenting productivity is homogeneous. Equation (11) was estimated in the United States by Lamadon, Mogstad, and Setzler (2020), who also find evidence that θ_j varies across firms, but they do not examine foreign ownership.

average foreign-firm premium of about 7% relative to domestic nonmultinational firms in each case. Second, we find that the results are robust to controlling for third-order polynomials in log firm size (with polynomials in the firm's local employment and national employment across all of the firm's locations), with a mean foreign-firm premium estimate of 6.2%. Third, Online Appendix Figure A8 demonstrates that the results are nearly the same when performing the estimation for the 2001–2006 sample rather than the 2010–2015 sample considered above, with an average foreign-firm premium of 6.7% relative to domestic nonmultinational firms in 2001–2006. Fourth, when allowing for firm-worker interactions as discussed above, the average foreign-firm premium is 7.8% on average relative to domestic firms. Fifth, in Online Appendix E, we use a difference-in-differences design for workers that move across firms as a distinct but complementary approach to equation (10).[25] As reported in Online Appendix Table A2, we find that moving between domestic and foreign firms is associated with a 5%–8% wage change (relative to wage growth for workers who move between domestic firms), which is similar to the main estimate. The estimates are in the 5%–6% range when considering only moves that occurred in a mass layoff event at the worker's initial employer.

IV.E. Mechanisms behind the Foreign-Firm Premium

We briefly discuss five alternative explanations for the foreign-firm premium. We do not find any as convincing as the productivity selection mechanism of Helpman, Melitz, and Yeaple (2004).

1. Hours. One possibility is that the same worker earns more at a foreign firm because of working longer hours. Although the tax data do not include information about hourly wages, according to survey data by the Bureau of Labor Statistics (2019), foreign firms pay 20% more than domestic firms even for workers in production occupations for which the reported wages should be primarily at

25. An advantage of this approach is that it is straightforward to visualize the pretrends, as discussed in Section IV.A. A disadvantage is that it does not yield the joint distribution of (ψ, x) needed for the various dimensions of heterogeneity we explore.

the hourly wage instead of the annual salary level.[26] We therefore think it is unlikely that hours worked are the main driver of foreign premiums.

2. Layoff Risk. Foreign firms may be perceived as being more risky employers, as existing research has found (domestic) multinationals to be at greater risk of shutting down plants than non-multinational firms of similar size (Bernard and Jensen 2007). However, plant shutdowns account for only a small fraction of overall job separations. We find that the probability of staying at the same employer next year is actually higher for workers at foreign firms than for workers at domestic firms. We also find a lower likelihood of separations due to mass layoffs at foreign firms (see the sample sizes in Online Appendix Table A2). Therefore, the risk of job separation—both overall and due to layoffs—appears to be lower at foreign firms.

3. Amenities and Fringe Benefits. It could be that foreign firms have lower amenities than domestic firms, and thus must pay greater wages to achieve a similar level of compensation. We have not been able to find systematic data on this claim. Anecdotes, however, suggest that foreign firms tend to be attractive employers overall. Examining the 20 employers ranked as having the "Top 20 Employee Benefits and Perks for 2017" in the United States by Glassdoor, we see that 5 (25%) are foreign owned.[27] In survey data from Costa Rica, Alfaro-Urena, Manelici, and Vasquez (2019a) find that amenities and fringe benefits are better at foreign-owned firms.

4. Stigma. A stigma may be associated with working at a foreign-owned firm, for which higher wages compensate. Although such a stigma may exist, our evidence presented in Figure III shows that the wage premium is rising with GDP per capita of the

26. According to the Current Population Survey, 80% of workers in production occupations receive hourly wages as opposed to a fixed annual salary. The instructions in the Occupational Employment Report ask firms to report hourly wages for part-time workers as well as for salaried workers, who do not work a standard 2,080 hours per year (40 hours a week).

27. See https://www.glassdoor.com/blog/top-20-employee-benefits-perks-for-2017/.

owner country, whereas we might expect stigma to be negatively associated with GDP per capita of the owner country.

5. Information or Monitoring Costs. Foreign owners may have worse information about the skill of the workers they hire and overpay them. Alternatively, monitoring workers may be more difficult for foreign owners (Head and Ries 2008). In lieu of monitoring, firms may pay a premium to discourage workers from shirking, and the premium may be greater for workers with greater ability or those in positions of responsibility (Oi 1983; Katz 1986). We note that it would not affect our conclusion of a positive effect of foreign firms on their workers if the premium were due to information or monitoring costs.

V. Indirect Effects of Foreign Multinationals

As discussed in Section III, in addition to directly affecting the wages of their own workers, foreign multinationals may also affect domestic firms and their workers indirectly. The theory suggests that these effects can be positive or negative.

V.A. Empirical Strategy to Estimate Indirect Effects

In this section, we seek to measure the indirect effects of employment growth at foreign-owned firms on outcomes at domestic-owned firms. Using a functional form suggested by the first-order approximations derived in Section III.C, we consider the following regression equation:

$$\log y_{j,t} - \log y_{j,t-1} = \beta \widehat{X}_{cz(j),t} + \gamma' K_{j,t} + \epsilon_{j,t}, \tag{12}$$

where j is the firm; y is its outcome on a measure such as value added or wage bill; $cz(j)$ is its commuting zone; $\widehat{X}_{cz,t}$ denotes the growth in the employment share by foreign-owned firms in that commuting zone; and $K_{j,t}$ is a vector of controls discussed below. The parameter of interest is β, which is the indirect effect.

Identifying β is challenging for at least two reasons. First, there is a classic selection issue with the allocation of foreign multinational activity across locations. Foreign firms may choose to hire in regions where wages are already set to grow. For example, the foreign firm may be aware of new regional investments in production infrastructure or education and increase hiring in this region to benefit from the infrastructure or workforce improvements. Then, a naive regression of earnings growth on employment growth at foreign firms would overstate the effect of foreign firm activity. Conversely, foreign firms may choose to hire in regions in which the local economy is already set to decline. For example, the foreign firm may be aware that wages or intermediate goods prices are set to decline in this region, possibly because a large existing employer plans to lay off its workforce, so the foreign firm may increase activity to take advantage of falling prices. This case is further confounded by the importance of local tax incentives, which are estimated to be large in the United States and may be targeted especially toward attracting foreign firms to declining regions.[28] Then, a naive regression of earnings growth on employment growth at foreign-owned firms would understate the effect of foreign-firm activity.

Second, we may be mismeasuring growth in the employment share of foreign firms in the commuting zone, $\widehat{X}_{cz,t}$. As discussed in Section II, we expect there to be some measurement error in the linkages between the parent and its subsidiaries and how these change over time.

To overcome these identification challenges, we adapt the identification strategy common in the literature about the effects of immigration on nonimmigrants in the same region (Card 2001). This literature uses the fact that immigrants cluster into regions in the United States based on country of origin. To adapt this instrument to identify the effects of foreign-owned firm activity on workers, we first notice that employment at foreign-owned firms tends to be clustered by region and country of origin (see Figure I). For example, German-owned firms disproportionately employ workers in South Carolina in 2010 if they do so in 2005. This is analogous to the clustering of immigrants into regions.

We construct the instrument as the predicted change in employment at, for example, German-owned firms in South Carolina between 2009 and 2010 using only information about (i) the share of workers at German-owned firms in South Carolina in 2005 and (ii) the change in aggregate employment by German-owned firms in any other region in the United States between 2009 and 2010. Because this instrument is not formed using information

28. See the discussion by Greenstone, Hornbeck, and Moretti (2010). Relatedly, Criscuolo et al. (2019) find that regional investment subsidies are negatively selected in the United Kingdom such that naive regression estimates of their effects are severely downward biased.

about the change in employment by German-owned firms in South Carolina between 2009 and 2010, it does not depend directly on changes in South Carolina's business climate between 2009 and 2010. In other words, German firms' aggregate foreign employment growth (net of employment growth in South Carolina) in 2010 is plausibly exogenous of South Carolina's local unobservable shocks in 2010. In particular, it does not depend directly on infrastructure investments, improved educational opportunities, or changes in the generosity of tax incentives in South Carolina in 2010, so it does not depend directly on the confounding factors discussed above.

To formalize the approach, relative foreign-owned firm employment growth in the commuting zone, $\widehat{X}_{cz,t}$, is defined by

$$\widehat{X}_{cz,t} \equiv \frac{L^F_{cz,t} - L^F_{cz,t-1}}{L^F_{cz,t-1} + L^D_{cz,t-1}}, \tag{13}$$

where $L^F_{cz,t}$ and $L^D_{cz,t}$ are the number of employees at foreign- and domestic-owned firms in commuting zone cz and year t, respectively. The parameter of interest is the effect of a change in the regional share of employment at foreign-owned firms, $\widehat{X}_{cz,t}$, on the change in an outcome, such as the earnings growth of a worker at a domestic firm in the region.

To form the instrument, we use the tax data on the firm's country of foreign ownership to construct the share $S^o_{cz,t}$ of all employment in commuting zone cz at firms whose owners are located in origin country o, defined by

$$S^o_{cz,t} \equiv \frac{L^{F_o}_{cz,t}}{L^F_{cz,t} + L^D_{cz,t}}. \tag{14}$$

Analogous to Card (2001) and the subsequent immigration literature, we then construct the instrumental variable $\widehat{Z}_{cz,t}$ as

$$\widehat{Z}_{cz,t} = \sum_o \frac{\sum_{cz' \neq cz} \left(L^{F_o}_{cz',t} - L^{F_o}_{cz',t-1} \right)}{\sum_{cz'} L^{F_o}_{cz',t-5}} S^o_{cz,t-5}. \tag{15}$$

This variable is interpreted as the prediction of $\widehat{X}_{cz,t}$, formed only from the share of employment by firms from country o in cz dated at $t-5$ and the change in aggregate employment by o in the United States from $t-1$ to t. Note that we modify the approach from the immigration literature slightly by leaving out own-commuting-zone employment when constructing the aggregate change from $t - 1$ to t, which helps rule out confounding factors.[29] The denominator is the total number of FTE workers in the country of origin five years ago, which ensures that the aggregate change is measured relative to levels dated far before contemporaneous shocks. Because $\widehat{Z}_{cz,t}$ is not a function of cz-specific changes between $t - 1$ and t, it should satisfy that $\widehat{Z}_{cz,t}$ and the unexplained component of cz growth are orthogonal (conditional on observed determinants of growth $K_{j,t}$). However, we see four possible threats to identification as well as a threat to drawing inference on our estimates.

First, the instrument includes the past share of employment at foreign-owned firms from various origin countries, as well as the contemporaneous change in the employment at such firms in other regions. This raises the concern that there may be regional shocks correlated with our instrument. For example, regions near the Canadian border may also be affected by trade shocks originating in Canada that are correlated with the instrument. To deal with this concern, we include census division-year fixed effects in the regressions, which absorb all contemporaneous effects at the regional level.

Second, industry shocks may be correlated with the instrument. For example, German- or Japanese-owned firms may be more likely to be in the car industry and select commuting zones that are also abundant with other car industry firms. To deal with this concern, we also include fine industry-year fixed effects based on the three-digit NAICS code (six-digit NAICS in a robustness check discussed in Online Appendix H) to absorb any contemporaneous nationwide growth trends by industry.

Third, foreign investment growth may be disproportionately concentrated in urban regions (see Bakker 2020). To ensure that urban concentration does not confound the foreign shocks, we control for various measures of urban concentration, including log population size, log population density, an indicator for spatial overlap with a micropolitan statistical area, and an indicator for overlap with a metropolitan statistical area. We measure these in the preperiod to avoid controlling out the effects of interest.

29. We also consider leaving out nearby commuting zones in a robustness check (see Section V.C).

Fourth, Borusyak, Hull, and Jaravel (2020) recently showed that under their assumptions, instruments with a shift-share structure may be biased if they do not control for the sum of regional exposure shares by year. To address this, we always control for $\frac{L^D_{cz,t-5}}{L^F_{cz,t-5}+L^D_{cz,t-5}}$ in the indirect-effects regressions.[30]

Last, although it is plausible that the aggregate foreign employment growth of a country of origin (leaving out employment growth in a commuting zone) is orthogonal to local growth shocks in a particular commuting zone, this does not imply that the regression residuals are independent across nearby commuting zones. Spatially dependent residuals would not bias the regression coefficient estimate but would tend to downward bias standard errors in the regression, leading to overrejection of the null hypothesis (Adão, Kolesár, and Morales 2019; Borusyak, Hull, and Jaravel 2020).[31] To be conservative when drawing inference, we follow Borusyak, Hull, and Jaravel (2020) in transforming the regression into one that is clustered at the country-of-origin-year level. However, as discussed by Borusyak, Hull, and Jaravel (2020), their method does not incorporate that the instrument leaves out own-commuting-zone employment growth. They argue that the standard errors are still approximately valid for leave-one-out point

30. See their discussion of the "incomplete shares" problem. They also suggest interacting the domestic employment shares with time periods to allow for more flexible domestic-shock specifications, which amounts to including more than a dozen additional linear controls in our regressions. Of course, we already allow for extremely flexible domestic-shock specifications by including fine industry-year fixed effects and census-division-year fixed effects. If we fully interact the domestic shares with years to allow even more flexibility, we find stronger indirect effects than in our baseline estimates, but the standard errors become much less precise. In Online Appendix Table A4, we provide a robustness check in which we interact the domestic shares with indicators for groups of years (e.g., the financial crisis of 2007–2009), where grouping the years serves as a parsimonious way to allow for additional flexibility in the domestic shocks, finding that the estimates become somewhat larger but are not statistically significantly different.

31. Adão, Kolesár, and Morales (2019, 1951) summarize the overrejection issue as follows: "[W]henever two regions have similar [exposure] shares, they will have similar exposure to the [aggregate shocks], and tend to have similar values of the [regression] residuals. While traditional inference methods allow for some forms of dependence between the residuals, such as spatial dependence within a state, they do not directly address the possible dependence between residuals generated by unobserved shift-share components.... [T]raditional inference methods underestimate the variance of the OLS estimator of β, creating the overrejection problem."

estimates. As an alternative that accounts for the leave-one-out nature of our instrument, we also provide traditional standard errors clustered at the commuting zone–year level.

To summarize, in the baseline specification, we protect against potential confounders by including in the control vector $K_{j,t}$ industry-year indicators, census division–year indicators, measures of urban concentration, and the sum of commuting-zone exposure shares, then report standard errors clustered at either the country-of-origin-year or commuting zone–year level. In Online Appendix H, we demonstrate that the results are not sensitive to adding control variables.

V.B. Estimates of Indirect Effects on Local Labor Markets

We next discuss our baseline estimates of indirect effects. The instrument and endogenous variable are constructed from information on both foreign and domestic firms, while the sample in the regression includes only continuing domestic firms.[32] The outcomes of interest are value added, employment, the wage bill, and earnings of continuing workers at domestic firms, and the sample size may vary across outcomes. (For example, value added can be negative, in which case log value added is not defined.) All observations are weighted by the number of FTE workers in $t-1$. The control variables were discussed in the previous subsection.

The full sample results are presented in the first column of Table I. The first-stage coefficient is 0.56. The F-statistic is above 230 when clustering by commuting zone–year and above 40 when clustering conservatively by country-of-origin-year. Thus, lagged shares of foreign employment by country of origin in a commuting zone interacted with that country's aggregate employment growth provides an economically and statistically significant predictor of that country's employment growth in the commuting zone. Using the instrument, we estimate that a 1 percentage point increase in the share of employment at foreign firms in the commuting zone increases the value added, employment, and wage bill at domestic firms by 0.96%, 0.53%, and 0.63%, respectively.[33] These

32. The outcome sample includes both domestic multinationals and domestic nonmultinationals. We find that results are similar when restricting the sample to domestic nonmultinationals in a robustness check (see Section V.C).

33. Note that the indirect-effect estimates are semielasticities. In Section VI, we convert these estimates to dollars or jobs generated at domestic firms in response to one additional job created at a foreign firm.

TABLE I
INDIRECT-EFFECT ESTIMATES: MAIN RESULTS

	Full sample	By firm size			By sector	
		Size 1–9	Size 10–99	Size 100+	Tradables	Nontradables
Panel A:	Outcome: log change in value added					
Second-stage coefficient	0.96	0.12	0.54	2.66	3.38	0.50
(Std. err. clustered by commuting zone)	(0.30)	(0.10)	(0.18)	(1.14)	(1.56)	(0.23)
(Std. err. clustered by country of origin)	(0.51)	(0.08)	(0.20)	(1.64)	(3.10)	(0.23)
First-stage coefficient	0.56	0.59	0.53	0.49	0.53	0.48
(F-statistic clustered by commuting zone)	(232)	(361)	(241)	(112)	(128)	(143)
(F-statistic clustered by country of origin)	(42)	(40)	(52)	(65)	(46)	(51)
Number of firms by commuting zones (millions)	41.8	34.9	6.5	0.5	6.0	6.0
Number of workers (millions, measured at $t-1$)	416.8	96.2	158.5	162.2	98.3	63.3
Panel B:	Outcome: log change in employment					
Second-stage coefficient	0.53	0.02	0.40	1.55	1.22	0.72
(Std. err. clustered by commuting zone)	(0.14)	(0.08)	(0.16)	(0.52)	(0.43)	(0.25)
(Std. err. clustered by country of origin)	(0.18)	(0.07)	(0.17)	(0.54)	(0.43)	(0.26)
First-stage coefficient	0.56	0.59	0.53	0.50	0.53	0.48
(F-statistic clustered by commuting zone)	(235)	(364)	(246)	(119)	(130)	(143)
(F-statistic clustered by country of origin)	(44)	(39)	(52)	(66)	(49)	(53)
Number of firms by commuting zones (millions)	46.0	38.3	7.1	0.5	6.4	6.4
Number of workers (millions, measured at $t-1$)	477.3	105.1	175.8	196.5	107.3	71.1

TABLE I
CONTINUED

	Full sample	By firm size			By sector	
		Size 1–9	Size 10–99	Size 100+	Tradables	Nontradables
Panel C:	Outcome: log change in wage bill					
Second-stage coefficient	0.63	0.00	0.41	1.62	1.42	1.19
(Std. err. clustered by commuting zone)	(0.17)	(0.10)	(0.18)	(0.53)	(0.47)	(0.35)
(Std. err. clustered by country of origin)	(0.22)	(0.10)	(0.19)	(0.56)	(0.50)	(0.36)
First-stage coefficient	0.56	0.59	0.53	0.50	0.53	0.48
(F-statistic clustered by commuting zone)	(235)	(364)	(246)	(119)	(130)	(143)
(F-statistic clustered by country of origin)	(44)	(39)	(52)	(66)	(49)	(53)
Number of firms by commuting zones (millions)	46.0	38.3	7.1	0.5	6.4	6.4
Number of workers (millions, measured at $t-1$)	477.3	105.1	175.8	196.5	107.3	71.1
Panel D:	Outcome: log change in earnings of continuing workers					
Second-stage coefficient	0.15	0.01	0.06	0.40	0.39	0.19
(Std. err. clustered by commuting zone)	(0.07)	(0.05)	(0.07)	(0.14)	(0.16)	(0.09)
(Std. err. clustered by country of origin)	(0.08)	(0.07)	(0.08)	(0.15)	(0.17)	(0.09)
First-stage coefficient	0.56	0.59	0.54	0.50	0.53	0.49
(F-statistic clustered by commuting zone)	(239)	(367)	(249)	(123)	(134)	(149)
(F-statistic clustered by country of origin)	(44)	(39)	(52)	(66)	(48)	(53)
Number of firms by commuting zones (millions)	44.6	37.0	7.1	0.5	6.3	6.2
Number of workers (millions, measured at $t-1$)	369.6	83.4	130.9	155.3	87.2	54.4

Notes. The outcome sample only includes continuing domestic firms. Observations are weighted by lagged firm size. Controls are industry-year indicators, census division–year indicators, measures of urban concentration, and the sum of commuting-zone exposure shares.

estimates are statistically significant at the 0.01 significance level even when using conservative standard errors clustered at the country-of-origin-year level. Online Appendix Table A5 compares these estimates to what we would obtain using OLS, with and without our rich set of controls. We find that the OLS estimates are about half as large as the estimates using our instrumental variable. As we discussed in the previous subsection, one reason for OLS estimates to be smaller is measurement error in $\widehat{X}_{cz,t}$; another reason is the selection of foreign investment into declining regions induced, for example, by tax incentives or declining prices.

We also examine indirect effects on earnings at the worker level. To do so, we perform a regression for continuing workers in the same domestic firm and commuting zone. We use a within-worker differenced specification to remove both worker fixed effects and firm fixed effects. The regression controls are the same as above, except for individuals instead of firms as the observations, and a polynomial in age is included to control for heterogeneous age profiles in earning growth. The results are presented in Table I, Panel D for about 370 million worker-year observations. The full sample estimate indicates a positive and statistically significant effect on the average worker's earning growth of about 0.15. This is greater than the estimate of 0.10 that one would obtain using the difference between log wage bill and log employment effects in Panels B and C, highlighting the importance of controlling for worker composition to understand the earning growth effects of foreign investment.

Next we consider heterogeneity in the effects across firm types using the same empirical specification but applied to various subsamples. Table I, columns (2)–(4) explore heterogeneity in the indirect estimates for three size groups, using the number of FTE workers measured at $t-1$. Table I, columns (5)–(6) consider heterogeneity in the effect on tradable versus nontradables industries, using the classifications of Mian and Sufi (2014). We then repeat the regression in equation (12) for each of these groups of firms. We find that the effects are much larger among large firms and firms in the tradable sector. We estimate that a 1 percentage point increase in the share of employment at foreign firms in the commuting zone increases value added by 2.7% at firms with at least 100 workers and by 3.4% at firms in the tradable sector. By contrast, the point estimate is small and insignificant for firms with fewer than 10 workers and is smaller yet still statistically significant in the nontradables sector. The patterns are similar for the FTE employment, wage bill, and earnings of continuing workers.[34]

Last, to investigate inequality in the worker-level earnings effects, we split the sample into equally sized quintile bins by ranking lagged earnings within the commuting zone–year. In Table II, columns (2)–(6), we examine earnings growth effects for continuing workers at different lagged earning quintile bins. For the lowest-three quintile bins, we find positive but statistically insignificant estimates. For the top-two quintile bins, we find statistically significant estimates of about 0.3. This indicates that a 1 percentage point increase in the share of employment at foreign firms in the commuting zone results in 0.3% wage growth for high-paid continuing workers at domestic firms in the commuting zone, while low-paid workers experience little to no wage growth. This implies indirect effects primarily benefit high-skilled workers at domestic firms, as predicted by our model (see Section III).

V.C. Robustness of the Indirect-Effect Estimates

In Online Appendix H, we provide numerous robustness checks to address potential concerns with the research design, which we briefly summarize here. In a placebo test in which domestic firms' outcomes are measured in the preperiod, the estimated effects become small in magnitude and statistically insignificant for all of the outcomes, consistent with our identifying assumption. Next, a potential concern with shift-share instruments is that the second-stage coefficient may conflate the effects of contemporaneous and past shocks if the shocks have delayed impacts (Jaeger, Ruist, and Stuhler 2018). We check that our estimates are nearly identical when controlling for the lagged shocks, implying that our results are not confounded by delayed impacts. Furthermore, our findings are robust to leaving out any commuting zone within a 300-mile radius of the worker's residence when constructing the shocks, indicating that the estimates are not confounded by the possibility of workers responding to shocks in nearby regions. Excluding all 52 countries that Hines (2010) considers tax havens, we find similar estimates, indicat-

34. Iacovone et al. (2015) find qualitatively similar differences of the effects of FDI growth on domestic firms by firm size. They find negative effects from Walmart's entry into Mexico on small Mexican suppliers of retailers and positive effects on large suppliers.

TABLE II

INDIRECT-EFFECT ESTIMATES: RESULTS BY WORKER EARNINGS QUINTILE

		By earnings quintile group				
	Full sample	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Outcome: log change in earnings of continuing workers						
Second-stage coefficient	0.15	0.06	0.04	0.08	0.27	0.32
(Std. err. clustered by commuting zone)	(0.07)	(0.12)	(0.09)	(0.08)	(0.09)	(0.12)
(Std. err. clustered by country of origin)	(0.08)	(0.13)	(0.12)	(0.09)	(0.10)	(0.12)
First-stage coefficient	0.56	0.55	0.56	0.56	0.56	0.55
(F-statistic clustered by commuting zone)	(239)	(235)	(237)	(238)	(238)	(238)
(F-statistic clustered by country of origin)	(44)	(50)	(47)	(47)	(46)	(47)
Number of firms by commuting zones (millions)	44.6	20.1	19.6	18.8	17.0	16.1
Number of workers (millions, measured at $t-1$)	369.6	73.9	73.9	73.9	73.9	73.9

Notes. The sample includes only workers employed by the same domestic firm in the same commuting zone during t and $t-1$. The sample only includes continuing workers at domestic firms. We divide workers into five earnings groups within each commuting zone–year based on the ordering of their lagged earnings. Controls are industry-year indicators, census division–year indicators, measures of urban concentration, and the sum of commuting-zone exposure shares.

ing that misclassification of some domestic firms as foreign for tax avoidance purposes does not bias our findings. Another possible threat to identification is that aggregate employment growth from a country of origin may lower transportation costs for U.S. exports to that country. Since most U.S. exports are carried out by multinationals (Bernard, Jensen, and Schott 2005), we check if the estimates conflate foreign demand effects with foreign employment effects by dropping domestic multinationals from the outcome sample, finding that the estimates are unaffected. Finally, to incorporate entry and exit into the outcome measures, we consider the transformation of Davis, Haltiwanger, and Schuh (1998). The estimated effects become somewhat stronger, which ameliorates any concern that our main effects for continuing firms arise from survival bias.

V.D. Understanding the Mechanisms behind the Indirect Effects

We conclude this section by discussing a number of mechanisms that could explain the positive indirect-effects estimates. In our model in Section III, positive indirect effects arise from knowledge spillovers from foreign to domestic firms. We first note that knowledge spillovers could come in the form of technology or improved management practices. Bloom et al. (2019) find evidence for local spillovers in management practices associated with large plant openings using the “Million Dollar Plants” research design. In fact, most million dollar plants in their study belong to multinational corporations.

Outside the scope of our model, increased competitive pressure may lead to higher efficiency at domestic firms (see Bloom, Draca, and Van Reenen 2015). However, competitive pressure would also predict that these firms become smaller in the short run, contrary to our results. Yet another channel for positive indirect effects on local domestic firms is an increase in consumer demand for nontradables (see Moretti 2010). Although we do find sizable effects in this sector, the effects are even greater in the tradable sector—suggesting that consumer demand cannot be the only channel behind the indirect effects.

Another potential mechanism through which indirect effects may arise is the firms’ input-output network (see Aitken and Harrison 1999; Javorcik 2004). Increased foreign investment may result in cheaper intermediate inputs supplied to domestic firms or greater local demand for the output of domestic firms. Either would likely result in greater output and employment at domestic-owned firms, so input/output spillovers can be thought of as an alternative interpretation of the productivity spillovers in our model. Javorcik (2004) investigated spillovers at the national level in Lithuania and found primarily positive effects from foreign investment on upstream domestic firms. Similarly, Alfaro-Urena, Manelici, and Vasquez (2019b) find positive productivity effects for domestic firms selling to multinational firms in Costa Rica.[35]

VI. Local and Aggregate Implications

In this section, we use our estimates from Sections IV and V to take a look at the local and aggregate implications of foreign multinationals. We emphasize that the numbers calculated here are not meant to summarize the overall welfare effect of foreign multinationals. We abstract, for example, from any worker-firm-specific preference heterogeneity in the calculations below. The calculations below are based on aggregate outcomes in 2015.

VI.A. Aggregate Direct Effects

We start by conducting the following thought experiment: Suppose one replaces all foreign multinationals with domestic firms—each equipped with the average productivity of domestic firms. How much would this lower the aggregate wages in the United States? We abstract away from any indirect effects (e.g., local spillovers) or worker-firm interactions.[36] In Section IV, we estimate an average foreign wage premium of 7%—after removing the effect of worker skill differentials from the wage differential between foreign and domestic firms. The theory suggests that this wage premium arises because of the larger productivity

35. In an earlier draft of this article, we provided estimates of upstream and downstream effects when using industry-level input/output tables to measure exposure to upstream and downstream foreign-investment shocks. However, due to the absence of firm-to-firm transactions data in U.S. tax records, it is not possible to precisely measure upstream and downstream exposure at the firm level, and our instrument lacked the statistical power to distinguish between these channels. We hope that firm-to-firm transactions data will one day be available for the United States so that this analysis can be performed.

36. By comparing one commuting zone with another above, we estimate the local indirect effects of foreign firms but not the national indirect effects, which are differenced out.

of foreign firms. Given an aggregate wage bill at foreign multinationals in the United States of $515 billion, this suggests an aggregate national wage premium due to foreign multinationals in the ballpark of $36 billion annually.[37] These figures suggest large aggregate gains for workers in the United States because of foreign multinationals. Indeed, $36 billion exceeds the aggregate subsidies of $4.6 billion paid to foreign firms a year.

VI.B. Local Effects of a New Foreign Plant

Beyond aggregate wage effects, policy makers are often confronted with weighing the local economic benefits of a foreign firm against subsidy costs. To be concrete, consider the establishment or expansion of a foreign firm that would create 1,000 new jobs in a commuting zone. Unlike in the previous subsection, we do not compare this expansion to a domestic firm expansion of similar size. The reason is that here we are interested in the direct as well as the local indirect effects, and our identification strategy delivers the indirect effects of foreign firms but not of domestic firms. Hence, the thought experiment is having a new foreign plant with 1,000 jobs compared to not having a new plant. Below, we describe some of the expected direct and indirect local effects. We focus on a commuting zone with an initial employment share of 94% at domestic firms, which corresponds to the national average. The benefits estimated in this subsection are calculated from the perspective of a local policy maker, while the previous subsection on aggregate direct effects is from the perspective of a national policy maker. While a local policy maker considers it valuable to steal business from another location, a national policy maker would discount the benefits of cross-location business stealing. See the discussion by Glaeser and Gottlieb (2009).

1. Wage Gains for Domestic Incumbents. Since 87% of workers who are hired by foreign multinationals from domestic-owned firms were previously employed in the same commuting zone, our calculations assume that around 870 of the 1,000 new positions

37. We calculate the aggregate wage bill at foreign multinationals from the average wage of a full-time employee at foreign-owned firms (Online Appendix Table A1) and the number of workers at foreign multinationals from the BEA (6.8 million). We use per worker estimates from tax data, but we use BEA aggregate estimates because it is not possible to link all workers to firms in the tax data, as discussed in Section II.

will be filled by domestic incumbents. From our foreign-firm premium estimate, direct wage gains for domestic incumbents hired by the foreign firm sum to \$4.6 million.[38] Wage gains for domestic incumbents also include those that arise indirectly at domestic firms. Recall that we estimate a wage increase of 0.15% for workers at domestic firms, due to a 1 percentage point increase of the share of employment at foreign firms (see Table II). The average earning of a full-time employed worker at a domestic firm is \$62,600. Combining these figures suggests an indirect wage effect of \$8.8 million for domestic incumbents who are not hired by the foreign firm.[39] In total, we find a \$13.4 million wage gain for domestic incumbents due to 1,000 hires by a foreign firm, or \$13,400 per created job, of which two-thirds is from the indirect effects.[40]

2. Increase in Local Economic Activity. Beyond affecting the wages for incumbents, foreign multinationals also affect the overall size of economic activity in a location. While the theory suggests that the indirect effects on output at domestic-owned firms can be positive or negative, the empirical analysis in Section V suggests that the local indirect effects are positive on average. We calculate that 1,000 positions at a foreign-owned plant on average raise the value added in the commuting zone by \$359 million a year.[41] Furthermore, employment increases by around 1,500 positions (i.e., an indirect effect of 500 more jobs at domestic firms), and the total wage bill increases by \$112.8 million on average.[42] Our

38. Specifically, 870 workers $\times$ \$75,700 per worker $\times$ 7% = \$4.6 million.

39. Let ζ denote the commuting zone size. Ninety-four percent of ζ workers experience a $0.15 \times \frac{1{,}000}{\zeta} \times$ \$62,600 wage gain, resulting in an indirect gain of \$8.8 million for this group of workers.

40. Note that on a per job basis, the results are independent of the magnitude of the increase in employment at foreign-owned firms and independent of commuting zone size. The effects get slightly larger with a smaller fraction of initial employment at foreign-owned firms in the commuting zone.

41. The value added per worker at a foreign multinational is \$220,100 and \$154,300 at a domestic firm on average. In addition to a direct increase in value added in the commuting zone by \$220 million, the estimates in Table I suggest an indirect increase in value added by \$139.2 million (calculated as $\frac{1{,}000}{\zeta} \times 0.96 \times 0.94 \times \zeta \times$ \$154,300).

42. The estimates in Table I, Panel B suggest an indirect increase in employment of about 500 workers (calculated as $\frac{1{,}000}{\zeta} \times 0.53 \times 0.94 \times \zeta$). If the foreign employment share is zero, the predicted indirect increase rises to 530 workers. The foreign plant would lead, on average, to a direct increase in the wage bill at foreign-owned firms of \$75.7 million. Using the estimates in Table I, Panel C, we

estimate of a total local job multiplier of about 1.50 (0.50 indirect jobs for each 1 job created) is at the lower end of estimates in the urban economics literature, which typically range from 1.5 to 2.5 (see the review by Bartik and Sotherland 2019). While the literature lacks a directly comparable estimate of the job multiplier for foreign multinationals, Moretti (2010) finds that, for each job created in the tradable sector, 1.6 jobs are created in the nontradable sector, for a total job multiplier of 2.6.

3. Comparison to Local Subsidies. As discussed already, our estimates do not shed light on the national indirect effects of foreign firms, but they do shed light on the local indirect effects. These calculations are still policy relevant, as local governments actively engage in subsidy competition to attract firms (see Gaubert 2018; Ossa 2017). Extending data collected by the policy group Good Jobs First, Slattery (2020) analyzes 387 large subsidy deals given by state and local governments in the United States. In these data, firms promise to create 1,400 direct jobs and receive a subsidy worth \$150 million on average, so these mega-deals are a natural comparison for our hypothetical 1,000-job plant. About a quarter of these large subsidy deals go to foreign multinationals and the median subsidy per direct job given to a foreign parent is \$100,000.[43] Our estimate of \$13,400 annual wage benefits to domestic incumbents is a conservative estimate of total benefits, as it omits other nonwage benefits to the commuting zone (e.g., increased tax revenues, increased variety of employment options). At a discount rate of about 0.13, the average wage benefits per position at a foreign firm equal the typical subsidy payment. At a discount rate of 0.10, the net present value of the average wage gain exceeds the typical subsidy by \$34,000 per job. Since foreign multinationals are mobile in their location choices for large plant openings or expansions, it is intuitive that in the bargaining with local authorities over mega-deals, they typically extract a large fraction of the overall local benefits via subsidy payments.

compute an indirect increase in the wage bill at domestic-owned firms of \$37.1 million (calculated as $\frac{1{,}000}{\zeta} \times 0.63 \times 0.94 \times \zeta \times \$62{,}600$). The increase in the total wage bill is substantially larger than the increase in the wage premium for incumbents calculated in Section VI.B, as it includes wages paid to individuals that were previously working outside the commuting zone or were nonemployed.

43. The median subsidy given to a U.S. parent is \$60,000. We are grateful to Cailin Slattery for providing these statistics.

VII. Conclusions

In this article, we use employer-employee panel data from 1999 to 2017 to conduct a comprehensive analysis of the effects of foreign multinationals in the United States. We find that these firms pay a wage premium of about 7% on average, meaning that the same worker earns 7% more at a foreign-owned firm. The wage premium is larger for higher-skilled workers and absent for the lowest decile of worker skill. Our theory rationalizes these findings with a (skill-biased) productivity advantage of foreign firms. Empirically, we document that this foreign-firm premium is correlated with the GDP per capita of the origin country. Furthermore, on average, the firm premium is about the same for domestic multinational firms, suggesting that the multinational status itself is associated with higher wages for the same worker. Quantitatively, the wage premium paid by foreign multinationals is quite large in the aggregate—accounting for $36 billion annually in wages (which is about 0.6% of the entire private sector wage bill). Though we did not find that controlling for measures of local and national employment would substantially reduce the multinational wage premium, we do not observe a multinational firm's global employment size. In future work, it would be interesting to evaluate how much of the multinational wage premium arises from economies of scale associated with its global employment size.

In terms of policy implications, our estimates highlight sizable benefits of trade and investment policies that make it attractive for foreign firms to invest in the United States. Furthermore, our estimates imply incentives for local policy makers to compete for investments by foreign multinationals, since, in addition to direct wage benefits, we find positive and sizable local indirect effects on domestic firms and their workers—in particular, the higher-earning ones. We note that although it is rational for local policy makers to compete for foreign-multinational investments with subsidies, this does not imply that such subsidies are beneficial from a national welfare perspective. Our calculations suggest that the subsidies given to foreign multinationals for large plant investment or expansions account for a sizable fraction of the net present value of the wage benefits for incumbent workers. In other words, foreign multinationals are able to extract a sizable fraction of the surplus from such investments in the bargaining with local governments over mega-deals.

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Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

Data and code replicating the tables and figures in this article can be found in Setzler and Tintelnot (2021) in the Harvard Dataverse, https://doi.org/10.7910/DVN/LW9GTR.

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