Responsible Sourcing?
Theory and Evidence from Costa Rica[*]

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Comments welcome—*

Responsible Sourcing (RS) requirements by multinational enterprises (MNEs) impose minimum standards on worker compensation, benefits, working conditions and other production practices at their suppliers worldwide. We develop a quantitative general equilibrium model to study the incidence of RS on firms and workers in sourcing origin countries. We show that the welfare implications of RS are a priori ambiguous, and sensitive to alternative hypotheses on the motivation behind RS by MNEs and the market environment in which these policies are implemented. We derive testable comparative statics that help discriminate between the alternative hypotheses. We then build a new database covering the near-universe of RS rollouts by more than 400 MNE affiliates in Costa Rica (CR) since 2009, and combine it with firm-to-firm transaction records and matched employer-employee administrative microdata for all CR firms. We use these data to provide new evidence on the effects of RS rollouts, discriminate between model assumptions, and calibrate the model for counterfactual analysis. We find that RS is not just "hot air", documenting significant negative effects on the sales and employment of exposed suppliers, and positive effects on the earnings of their workers. Overall, we find that RS policies by MNEs in CR have increased domestic welfare, and that the gains are concentrated among initially low-wage workers, reducing inequality.

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1 Introduction

Demands by policy-makers and the general public for multinational enterprises (MNEs) to "clean up their supply chains" and implement Responsible Sourcing (RS) requirements for their suppliers in developing countries have become widespread over recent years (e.g., ILO, 2016). RS requirements mainly take the form of "Supplier Codes of Conduct" and typically include standards on working conditions (such as wage floors, guaranteed benefits, maximum working hours, paid leave and safety standards), as well as on other aspects of production (such as worker representation and environmental standards). Despite the growing adoption of RS practices by MNEs, there is limited theoretical work or empirical evidence on the economic incidence of these policies and their effectiveness at raising the welfare of stakeholders in the sourcing countries.[1] We set out to study the impact of RS in the context of a middle-income country, Costa Rica (CR). We focus our analysis on the part of RS practices that concern working conditions, leaving the analysis of, e.g., their environmental consequences for future research.

In this context, our paper makes several contributions. First, to guide the analysis, we develop a quantitative general equilibrium model to study the incidence of RS. We derive testable comparative statics that help discriminate between different hypotheses about both the motivation behind RS by the MNEs and the economic environment in which RS is implemented. Second, we build a new database that allows us to test these comparative statics. The microdata let us track the rollout of RS requirements by MNE affiliates in CR and trace their effect on CR suppliers and their workers through the domestic production network (in all sectors, including local services). Our identification strategy is based on firm-, firm-to-firm- and worker-by-firm-level panel datasets in an event-study design. We also use RS rollout decisions at the MNE headquarters (affecting their suppliers worldwide) as an instrument for RS rollouts affecting CR suppliers. Finally, we use the most general variant of our model supported by the evidence to derive expressions of the welfare incidence of RS in origin countries and decompose the competing forces. We then combine theory with evidence to calibrate the model for counterfactual analysis and quantify the welfare and distributional implications of RS in CR.

Our analysis proceeds in four main steps. We first develop the theory to guide the analysis. In the model, heterogeneous firms in the origin country produce goods for their domestic market and can also produce intermediate inputs for foreign-owned MNEs. We model RS policies as an increase in the marginal cost of intermediate good producers (wages in the model) that becomes mandatory for selling to the MNE and affects all of the suppliers' production (to all buyers, including domestic ones). Firms employ two types of workers in their production: low-wage workers for whom RS standards (on wage floors, benefits, and working conditions) may be binding and high-wage workers for whom RS standards are unlikely binding.

The model nests several alternative hypotheses on (i) the underlying motivation of RS policies by MNEs, and (ii) the market environment in which these policies are implemented. On (i), MNEs

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[1]See discussion of related work below.

face output demand on world markets that may be a direct function of their sourcing practices, capturing potential pressure put on MNEs by their consumers to develop more equitable practices. Alternatively, RS practices may also be motivated by reasons not directly related to short- or medium-run MNE profit maximization (i.e., without a discernible accompanying shift in MNE output demand). On (ii), markets in the sourcing country may be imperfectly competitive. In particular, domestic firms may exert monopsonistic market power on the labor market, leading to pre-existing markdowns on wages (providing a potential policy rationale for MNE-imposed wage floors). In turn, large MNE buyers may also have monopsonistic market power when sourcing inputs from the origin markets, leading to an imperfect pass-through of higher production costs from their suppliers to MNE input prices. Finally, RS rollouts may lead to direct productivity effects on suppliers due to transfers of technology or expertise by the MNE that may accompany RS announcements.

To discriminate between these hypotheses and guide the empirical analysis, we derive comparative statics comparing changes among "exposed firms" that were above an initial MNE buyer-specific productivity cutoff before the RS rollout–i.e., firms supplying to the MNE in the period before the new RS standards take effect–to otherwise similar firms who are suppliers to other MNEs over the same period. We show that all model variants–i.e., assumptions on either the MNE's motivation behind RS and/or the economic environment–can rationalize a negative effect on the total firm sales of exposed suppliers compared to non-exposed ones. However, different assumptions lead to opposing predictions on the intensive margin of sales to the MNE among RS-compliant continuing suppliers in the exposed group. If RS is accompanied by a positive demand shift for the MNE products, the model predicts positive effects of RS, on net, on continuing sales to the RS-MNE. Similarly, if RS is rolled out in an environment with labor market monopsony power, sales of suppliers adopting RS codes of conduct are also predicted to go up. The opposite (i.e., a fall in the intensive margin of sales) occurs under all other hypotheses.

In the second step, we take these comparative statics to the data using an event-study design. At the center of the analysis lies the construction of a unique collection of microdata. We make use of several administrative datasets that encompass matched employer-employee data, firm-to-firm transaction data, customs microdata, corporate tax returns, and foreign ownership registry data covering the period 2009-2017. We then combine those sources with a novel dataset covering the introduction of RS supplier requirements of more than 400 MNEs with affiliates sourcing on the ground in CR over this period. Using a comprehensive double-blind search of corporate filings, reports, press releases and media coverage, we identify 152 RS rollouts by 127 MNEs targeted at improving working conditions at CR suppliers over this period (including wage floors, guaranteed employee benefits, formality requirements, and safety standards). This combination of datasets allows us to trace the evolution of firm and worker outcomes among MNE suppliers before and after the rollout of new MNE-specific supplier codes of conduct over this period. It also allows us to assess the scale and relevance of RS codes of conduct in our empirical setting: we find that by the end of our sample in 2017, 45% of total production by CR domestic firms was

subject to an active RS policy (produced by firms also selling to MNE buyers with RS codes).

For the empirical analysis, as in the model, we define "exposure" to a new RS rollout by comparing changes in outcomes among suppliers that were selling to the MNE in the year before the new RS standards take effect to those among suppliers to other MNEs over the same period. To do so convincingly, we build on recent contributions on the identification and inference for treatment effects using difference-in-differences (DiD) with multiple time periods and variation in treatment timing (DiD with staggered treatments) (Borusyak et al., 2021, Callaway and Sant'Anna, 2020, De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2018, Sun and Abraham, 2020, Baker et al., 2021, Roth and Sant'Anna, 2021). We also address concerns that MNEs targeted their RS rollouts during periods in which their CR suppliers also experienced other shocks (that may not be apparent in the observed pre-trends). To do so, we implement the RS event study after instrumenting for the rollout treatment timeline with only rollouts that were decided at the global headquarters of the MNE (affecting supplier codes of conduct for the MNE worldwide).

We find that RS rollouts lead to a significant reduction in total firm sales and firm employment among exposed producers. For both outcomes, the effect is in the order of an 8 percent reduction 4 years after the RS rollout. Moving from supplier- to worker-level event studies in the employer-employee data, we find that those effects are accompanied by a roughly 1.5 percent average increase in worker monthly earnings among exposed firms. This effect is most pronounced among initially low-wage workers (defined as the bottom 20 percent in terms of monthly earnings), for whom we find a significant increase of 6 percent 4 years after the rollout. Using the firm-to-firm transaction data, we find that both total firm sales as well as sales of RS-compliant continuing suppliers to the RS-MNE are decreasing post-rollout. This initial evidence is consistent with a model in which MNEs decide to roll out RS without an accompanying positive shift to their output demand (at least in the short to medium run we are able to observe), and where RS was unlikely targeted at addressing significant pre-existing markdowns by domestic suppliers in the origin's labor markets. On the other hand, the initial evidence does not rule out imperfect cost pass-through from suppliers to the MNE or potential direct effects on supplier productivity as part of new supplier codes of conduct by the MNE.

In the third step, we proceed to estimate the model's parameters using estimation equations from the most general variant of the theory that is supported by the evidence. To estimate the extent of potential direct changes in labor productivity among suppliers due to RS, we measure whether the policy has an effect on the monthly earnings of initially high-wage workers at exposed suppliers. For them, RS rollouts on working conditions are indeed unlikely binding, but they benefit from productivity effects. We then back out the implied increase in the marginal cost of production due to RS, in addition to the degree of imperfect cost pass-through and the shape parameter of the domestic productivity distribution, from a combination of estimated RS impacts on total firm sales, sales to domestic buyers and sales to the RS-MNE among continuing suppliers. Finally, we estimate the elasticity of substitution across different worker types in production from the estimated effect on firm relative employment of initially low-wage vs. high-wage workers.

In the final step, we derive expressions for the welfare and distributional implications of RS in general equilibrium. We show that in a setting where 100% of the output produced by MNE suppliers is destined for exports, RS requirements are isomorphic to an export tax, and hence welfare improving (to the first order) for the origin economy through a classic terms-of-trade effect. However, these suppliers may also produce for the domestic market so that the RS policy "leaks" into the cost of production for domestic consumption, raising the local price index and impacting welfare negatively. This force is akin to a labor market distortion where RS leads to a misallocation of labor between RS-compliant vs. other domestic producers. These opposing forces are present in all model variants. In addition, we show that buyer market power by MNEs vis-a-vis their suppliers attenuates the gains from the export tax effect (due to imperfect cost pass-through to the MNE), whereas the extent to which there are additional direct effects on labor productivity due to RS leads to additional welfare gains in the origin country. Turning to the distributional effects of the RS policy, we show that RS leads to larger gains among initially low-wage workers, thus reducing domestic inequality.

Armed with the welfare expressions and the calibrated model, we proceed to the counter-factual analysis. We find that the RS requirements that have been implemented in CR over the period 2009-2017 have on net increased domestic welfare by about 0.3 percent. This is due to both improved terms of trade and a moderate increase in the labor productivity among exposed suppliers (roughly 1.4 percent). These aggregate gains are concentrated among initially low-wage workers, whose overall welfare we estimate to have increased by roughly 1.3 percent nationwide. High-wage workers also gain (because the increase in labor productivity outweighs the export tax effect that is negative for this group), but this effect (0.13 percent) is much smaller than the gains among low-wage workers.

To better understand the forces at work and assess the sensitivity of these findings, we report a number of additional results under alternative assumptions. In particular, we quantify the full range of welfare effects across alternative assumptions about the extent of RS "leakage" into the domestic price index, the buyer market power by MNEs, the share of workers affected by RS requirements, the share of the country's exports subject to RS and the demand elasticity faced by firms. We also compare the estimated size of the RS-induced cost shock to the optimal tariff chosen by a planner to verify that RS in CR falls on the increasing side of the inverted u-shape for optimal trade policy. We further document the quantification results for different assumptions of how much of the firms' cost increase is effectively captured by low-wage workers, and present robustness checks on whether CR suppliers respond to RS by splitting up into different production entities (serving MNEs vs. non-RS buyers). These results are informative to assess the sensitivity of our baseline point estimates in the setting of CR. They also point to ways in which the impact of RS may be different or similar in other empirical contexts.

It is also important to highlight some of the limitations of our study. First, CR is a relatively developed economy (middle-income) compared to many poorer low-income countries in which RS has been rolled out over recent years. It is important to note that the RS requirements we are

able to study here (on improved compensation, benefits, working conditions) are likely distinct from other aspects of RS in low-income countries, such as child labor bans. Whereas RS in CR appears to benefit initially low-wage workers, it would be a very different counterfactual in theory to instead ban a certain type of employment (see e.g. Faber et al. (2017)). This and other differences in the institutional and labor market environments naturally demand some caution when extrapolating findings from this study to other contexts.

Second, while our database is arguably unique, there are still limitations to what we can observe. In particular, informal work arrangements are unlikely fully captured in the employer-employee database. Since RS is in part aimed at enforcing domestic labor regulations (and requiring formality), this is a potentially important limitation. For example, it could imply that we fail to capture the true employment effect of initially low-wage workers among exposed suppliers or additional wage increases from workers switching into formal employment. If exposed suppliers relied on informal employment to start with, we may thus not be able to fully capture the true increase in the cost of production by only looking at wages and employment in the data. In our current approach, we address these concerns by estimating the unobserved cost shock to the firm not from observed wage increases among low-wage workers but instead by using the exposed firms' sales response combined with knowledge of other model parameters. As mentioned above, we then also assess the sensitivity of our welfare results to different assumptions of how much of that estimated cost increase is actually captured by low-wage workers.

This paper contributes to a small but growing empirical literature on the effects of MNE sourcing policies on supplier outcomes. Harrison and Scorse (2010) study the effect of anti-sweatshop campaigns targeting contractors for MNEs in the textile, footwear, and apparel sector in Indonesia. Using a DiD design across sectors and regions, the authors find that the campaigns led to wage increases, falling profits, and some firm exit. More recently, Boudreau (2021) studies an RCT on the introduction of safety committees across apparel producers in Bangladesh, and Amengual and Distelhorst (2020) study compliance with Gap Inc's code of conduct for labor standards. Both studies find that RS requirements increase compliance with the law and safety measures.[2] Relative to the existing literature in this space, this paper develops an open-economy general equilibrium model to study the welfare implications of RS and combines the theory with the near-universe of RS rollouts and firm-to-firm transaction data in CR.

The paper also relates to a larger literature on the direct effects of MNE production (through foreign direct investment (FDI)) on worker and firm outcomes in developing countries–including through the MNE's in-house policies on working conditions in their plants (see e.g., Harrison and Rodríguez-Clare (2010) for a review, Javorcik (2004), and Alfaro-Ureña et al. (2020) and Alfaro-Ureña et al. (2021) for two recent studies in CR). Related to in-house MNE labor policies, Hjort et al.

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[2]Bossavie et al. (2020) study the effects of improvements in Bangladeshi labor regulations after the tragic garment factory collapse of Rana Plaza in 2013. Using a synthetic control approach, they find that working conditions improved whereas female wages decreased on average. Herkenhoff and Krautheim (2020) introduce cost savings from unethical practices as a new determinant in a model of global sourcing decisions with incomplete contracts. Koenig et al. (2021) study the endogenous geography of international NGO campaigns against unethical practices in a model of international trade.

([2020](#)) find that MNEs frequently set wages similarly across countries and that wage increases at the headquarters lead to reduced employment among foreign plants of the MNE.[3] Instead of adding to the growing evidence on the impacts of FDI on host locations, this paper evaluates the implications of the relatively more recent increase in RS policies by MNEs for their suppliers in global value chains. Given these policies have the stated objective to benefit the welfare of workers in origin markets, our analysis sets out to fill this gap.

The paper also relates to an existing literature on the implications of "fair trade" certification (e.g., Dragusanu and Nunn, 2018, De Janvry et al., 2015, Podhorsky, 2013, 2015). Both the existing theory and evidence have emphasized the notion that fair trade redistributes the returns of agricultural production from imperfectly competitive intermediary wholesalers to farmers in developing countries (e.g., Dragusanu and Nunn, 2018, Podhorsky, 2015). In contrast, in our setting RS requirements are chosen and implemented by the MNEs on their own supply chain. More recently, Macchiavello and Miquel-Florensa (2019) study a "buyer-driven" quality and sustainability upgrading program among coffee farmers in Colombia by a large MNE in the global coffee trade. Using a spatial RD design, they find that eligible farmers increased the quality of their coffee and that the program led to sizeable income gains.

Finally, we also relate to a literature that studies the more general concept of Corporate Social Responsibility and firms' pro-social behavior. This literature discusses, in particular, the motivations (profit-maximization vs. ulterior motives) for these policies and their potential rationale, compared to having governments implement such policies (see Campbell, 2007, Hart and Zingales, 2017, Bénabou and Tirole, 2010, Eichholtz et al., 2010, Fioretti, 2020). Besley and Ghatak (2007) shows that it can be rational for firms to provide privately a public good when consumers value it, while Kotchen (2006) emphasizes the joint-production aspect of private and public goods.

The remainder of the paper proceeds as follows. Section 2 develops the theory. Section 3 describes the data and empirical context. Section 4 presents empirical evidence on the impact of RS rollouts in origin markets. Section 5 proceeds to model estimation. Section 6 presents the welfare analysis, counterfactual quantification and sensitivity analysis. Section 7 concludes.

2   Model and Comparative Statics

Consider a model with two countries, Home (CR in our empirical analysis) and Foreign (the rest of the world). Foreign MNEs have subsidiaries in Home that source inputs from Home producers. Perhaps motivated by the demand of Foreign consumers, MNEs may engage in Responsible Sourcing (RS) policies that increase labor costs at their suppliers at Home. We are concerned with analyzing the impact of these policies on production and welfare at Home.

We lay out the model and derive comparative statics with respect to these policies for observ-

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[3]Méndez-Chacón and Van Patten (2021) propose a historical case study of MNE investments by the United Fruit Company in non-wage amenities for its workers, also in the context of CR. They find that these investments can have positive long-run effects both locally and in the aggregate.

able outcomes in the Home market. These comparative statics depend, in particular, on how markets are structured at Home, and what motivates the MNE to implement an RS policy. In Section 4, we confront these comparative statics with the database we describe in Section 3, and select the most general variant of our theory that is supported by the empirical evidence. Section 5 then reports the equations used to estimate the model parameters. Finally, Section 6 derives the welfare implications of RS policies in general equilibrium for the Home country. Additional details and derivations of the model are provided in the Appendix.

2.1 Setup

Workers   There are two types of workers, low- and high-wage workers, indexed by $t = l, h$. Workers are endowed with their type and with one unit of labor that they supply inelastically. The aggregate supply of type t in country k is L_k^t. Workers derive utility from the consumption of a variety of local and imported goods ω, with CES utility:

$$U^k = \left(\int_{\Omega^k} d_\omega q_\omega^{\frac{\sigma-1}{\sigma}} d\omega \right)^{\frac{\sigma}{\sigma-1}}, \tag{1}$$

where Ω^k is the set of varieties available for final consumption in country $k = H, F$ (for Home and Foreign), $q(\omega)$ denotes consumption of the final variety ω, and the parameter d_ω is a demand shifter for variety ω.

Firms and MNEs   The model features two types of producers: "firms" and "MNEs". Foreign hosts a fixed mass N^M of symmetric MNEs, which are headquartered in Foreign and have a subsidiary at Home. Each MNE headquarter sells a final variety x, in the Foreign market only. The variety is produced by the MNE subsidiary at Home, then exported to the headquarter in Foreign. In addition, a fixed mass of non-MNE firms N^k operate in each country. They are hereafter simply referred to as "firms". Each firm produces a unique variety for final consumption, ω. Home firms may also produce distinct intermediate inputs for MNEs. Specifically, the subsidiary of MNE x at Home uses intermediate inputs $\omega(x) \in \Omega_x$ for its production, produced by Home firms.

We assume simple trade patterns to simplify exposition.[4] Home exports to Foreign only through the MNE subsidiaries and their use of Home intermediate inputs. That is, final varieties produced by Home firms are not demanded abroad. Conversely, MNEs do not re-export their final variety to Home. That is, Foreign exports to Home only through the export of final varieties by Foreign (non-MNE) firms (with iceberg trade costs ζ).

Firms are heterogeneous in productivity z, and use labor as the sole factor of production. To produce in each market (the final good market, or the intermediate input market for MNE x), they have to incur a fixed cost of production, then produce at constant marginal cost. The production functions for the final variety q_ω and the intermediate input $m_{\omega(x)}$ demanded by MNE

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[4]These restrictions on trade patterns are straightforward to relax at the cost of a more cumbersome exposition. We re-visit these stylized assumptions before proceeding to the quantification in Section 6.

x are, respectively:

$$q_\omega = z_\omega \left(\ell_\omega - f_k \right) \tag{2}$$

$$m_{\omega(x)} = z_{\omega(x)} \left(\ell_{\omega(x)} - f_M \right), \tag{3}$$

where f_k, for $k = H, F$ denotes a fixed operating cost to produce final varieties in country k, and f_M denotes the fixed operating cost incurred to produce an MNE-specific intermediate input. Producing a specific variety for an MNE requires dedicated fixed costs that are higher than those needed to produce for the domestic market, i.e., $f_H < f_M$. This assumption fits the empirical regularity that firms that serve MNEs tend to be positively selected (Alfaro-Ureña et al., 2020). All product markets are monopolistically competitive. The labor composite ℓ captures the fact the two labor types are imperfect substitutes in production, with constant elasticity of substitution ρ, as follows:

$$\ell = \left[\alpha^l \ell^{l \frac{\rho-1}{\rho}} + \alpha^h \ell^{h \frac{\rho-1}{\rho}} \right]^{\frac{\rho}{\rho-1}} . \tag{4}$$

In each country, firm productivities z are distributed Pareto with parameter $\theta \geq \sigma - 1$ and with minimum $\underline{z}_k$:

$$G_k(z) = 1 - \left(\frac{z}{\underline{z}_k} \right)^{-\theta} . \tag{5}$$

We assume a given firm has the same productivity across all its production lines.

The subsidiary of MNE x at Home combines intermediate inputs $\omega(x) \in \Omega_x$ to produce an export-oriented final variety x according to the CES production function:[5]

$$M_x = \left(\int_{\Omega_x} m_{\omega(x)}^{\frac{\sigma-1}{\sigma}} \, d\omega(x) \right)^{\frac{\sigma}{\sigma-1}} \tag{6}$$

The MNE subsidiary does not directly hire Home labor to produce, but relies entirely on intermediate inputs produced by Home suppliers. The headquarter in Foreign imports M, at cost, from its subsidiary, subject to an iceberg trade cost ζ and sells it as a final good q_x on the Foreign market, i.e.:

$$q_x = M_x/\zeta \tag{7}$$

Labor markets are perfectly competitive in this baseline model, but we explore below an alternative where firms have monopsony power on the Home labor market. While in this baseline model, the market structure in all product markets is one of monopolistic competition, we will also explore below an alternative where MNEs have buyer power in the intermediate inputs market at Home.

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[5]We assume the same elasticity of substitution between Home suppliers in the production of the domestic varieties and intermediate inputs, which simplifies the analysis. The Appendix provides the full derivations in the case when the two elasticities are allowed to differ. [We have the derivations, but have not typed this up in the Appendix.]

2.2 Responsible Sourcing

Definition of RS Policies   MNEs can decide to impose RS policies on their Home suppliers. In the model, we assume that MNEs ask their suppliers to incur labor costs that are higher than the prevailing market wage. This could capture both reduced hours at the same salary (through e.g., paid sick leave, maternity leave etc.), higher labor-related operating costs (through e.g., safety standards) and/or higher hourly wages. We assume that this increase in labor costs is binding for low-wage workers, but not for high-wage workers. In accordance with standard RS practices described in Section 3, a local firm that is part of an RS supply chain has to apply the same labor standards to all its workers, including those working on production lines dedicated to the domestic market.

Formally, we assume that at RS suppliers, the cost of labor for low-wage workers must be at least τw_H^l, where w_H^l is the prevailing market wage for low-wage workers at Home and $\tau \geq 1$ is the net increase in the labor cost of RS-suppliers. Therefore, the parameter τ indexes the size of the RS policy. For high-wage workers, the policy is not binding, and the cost of high-wage labor for an RS-supplier is the prevailing market wage. Summarizing, we have:

$$w_H^{l,RS} = \tau w_H^l, \tag{8}$$
$$w_H^{h,RS} = w_H^h. \tag{9}$$

2.2.1 Allowing for Different Hypotheses

How RS impacts the Home market and, ultimately, the welfare of Home workers, may depend on (i) whether the increase in the labor cost is a profit-maximizing choice for the MNE, or whether it serves to pursue broader motives beyond short- or medium-term profit-maximization (as these may affect MNE input demand due to RS); (ii) whether or not wages were too low in the first place, i.e., whether or not suppliers in the Home market exert monopsonistic market power on the labor market (leading to pre-existing wage markdowns); (iii) whether RS policies are accompanied by transfers of technology or expertise that increase labor productivity at their suppliers, or whether they are pure cost increases; and (iv) how much of the labor cost increase is borne by Home suppliers vs. how much is borne by the MNE through higher input prices. To shed light on these different forces, we set up the theory below to entertain several alternatives of the way and context in which RS is implemented. We describe them in turn below. In the following sections, we then use the data, empirical strategy and the comparative statics results derived from the model below to discriminate between these different hypotheses.

A. Motivation Behind the RS Policy   Following the literature on corporate social responsibility (e.g. Bénabou and Tirole, 2010), we consider two ways of thinking about RS requirements. First, RS could be a response to an exigence of foreign consumers, employees, or investors for fairer labor conditions in sourcing countries. In this case, RS can be derived as a profit-maximizing

strategy that accounts for the fact that increasing labor standards in sourcing countries has a direct positive impact on the MNE–through increased demand, more efficient foreign labor, or increased investment–, although it comes at the cost of higher production costs. In the context of our model, we capture this force by allowing the demand shifter d_x for the variety produced by MNE x to depend positively on labor conditions in the sourcing country, that is:

$$\frac{\partial d_x}{\partial \tau} \geq 0.$$

MNE profits are impacted negatively by higher production costs, on the one hand, but they are impacted positively by the corresponding demand shock on the other hand.[6]

Alternatively, RS could be a choice made by the MNE management pursuing altruistic motives, beyond profit maximization. In this case, managers of the MNE x choose wages to maximize their utility:

$$U\left(\Pi\left(x\right), \tau\right) \tag{10}$$

with U weakly increasing in each of its arguments. In contrast to the case above, τ does not affect firm profits directly beyond its effect on labor costs. To analyze this case, we take the RS policy τ as a parameter, chosen in the utility maximization problem (10), but outside of the profit-maximization problem of the firm. Then, conditional on the RS policy, the rest of the firm's decisions follow profit-maximization. We summarize these two options with the following alternative hypotheses:

Hypothesis A *RS is chosen outside of the profit maximization program of the firm.*

Hypothesis A' *RS is chosen to maximize profits, given that demand responds to labor conditions in the sourcing country.*

B. Labor Market Power   A natural question is whether RS policies are implemented in a context where wages are too low in the first place. In the baseline model presented above, wages are those that clear the market–they are not too low from an efficiency perspective, and raising them introduces, a priori, a distortion. This possibility is summarized in the following hypothesis:

Hypothesis B *The Home labor market is competitive.*

Alternatively, it could be that wages are set too low compared to an efficient benchmark. Capturing this possibility requires Home firms to exert labor market power on the Home labor market. We now extend the model to feature an upward-sloping labor supply curve that Home firms are facing in order to entertain this possibility (pre-existing wage markdowns), summarized as follows:

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[6]Results would be qualitatively similar with a model extended to allow for an effect on foreign investment or foreign employees.

Hypothesis B' *The Home labor market is monopsonistic.*

To generate this feature in the most tractable way, we assume that, in addition to their preferences over a CES consumption bundle, workers have heterogeneous preferences for jobs. Utility of worker h working on production line ω is:

$$U^h = C\varepsilon^h(\omega),$$

where $C = \left(\int_{\Omega^k} d_\omega q_\omega^{\frac{\sigma-1}{\sigma}} d\omega\right)^{\frac{\sigma}{\sigma-1}}$ as above, and idiosyncratic preferences $\varepsilon^h(\omega)$ are drawn i.i.d across workers and production lines, according to a Fréchet distribution with shape parameter κ. Workers are therefore ex-ante homogeneous but ex-post heterogeneous. Production of firms and MNEs are otherwise unchanged, with, for simplicity, only one ex-ante worker type, whose exogenous aggregate supply is L_k in country k. That is, workers are perfect substitutes in production and ℓ_ω is the number of workers hired on production line ω. With this setup, firms face an upward-sloping labor supply curve when hiring on their production line ω :

$$\frac{\ell_\omega}{L_H} = \left(\frac{w_\omega}{\Phi}\right)^\kappa; \text{with } \Phi = \left(\int_{\Omega_H \cup \Omega_x} w_\omega{}^\kappa d\omega\right)^{\frac{1}{\kappa}} \tag{11}$$

Notice that when $\kappa \to \infty$, the model collapses to a familiar setup in which all workers are identical and firms face a perfectly elastic labor supply, as in our baseline model with one type of worker (nested here). Importantly, we assume that firms set wages taking this upward-sloping labor supply curve into account. Wages are set according to monopsonistic competition.

As we show below, wages are now $\omega-$specific in this environment, and in particular increase with firm productivity z. RS policies impose a minimum labor cost $\underline{w}$. Formally, for production line $k = H, x$, firms have to abide to:

$$w_k^{RS} \geq \underline{w}. \tag{12}$$

RS policies are therefore only binding for firms who offer a wage below this threshold, i.e., as we show below, lower productivity firms.

C. Productivity Gains The RS policy described above is a net increase in labor costs for firms hiring low-wage workers. However, it is possible that RS policies incentivize firms to make their workers more productive, or that they are accompanied by transfers of technology or expertise by the MNE to its suppliers, making workers more productive at RS suppliers. We consider two alternatives:

Hypothesis C *RS is not accompanied by direct changes in labor productivity.*

Hypothesis C' *RS is accompanied by direct changes in labor productivity.*

Under hypothesis (C'), we define the labor productivity gain as $T \geq 1$. We assume that it impacts the labor productivity of all workers of the firm affected by an RS policy, low- and high-

wage workers alike, on all production lines. Denoting $\tilde{w}_H^{t,RS}$ the compensation paid to a type t worker by a supplier adopting RS policies, we therefore have:

$$\tilde{w}_H^{l,RS} = T\tau w_H^l,$$
$$\text{and } \tilde{w}_H^{h,RS} = T w_H^h.$$

From the point of view of suppliers, the net labor costs $w_H^{t,RS}$ incurred for high- and low-wage labor per efficiency unit are still given by (8) and (9). That is, τ measures the pure labor cost increase faced by the RS supplier, net of any labor productivity gains.

D. Cost Pass-Through to Input Prices   The impact of RS policies on Home welfare will depend on whether those policies are paid for by Home firms or whether their cost is passed through to the MNE. We consider two alternatives:

Hypothesis D *The cost increase of RS is fully passed through to the MNE.*

Hypothesis D' *The cost increase of RS is imperfectly passed through to the MNE.*

In the latter case, we capture the pass-through rate of the RS policy with a reduced-form parameter $\beta \in (0,1)$, taken to be constant across firms.[7]

2.3   Equilibrium

2.3.1   Baseline Model

We first derive here the main equilibrium equations for the baseline model: under Hypotheses A, B, C and D above. We then derive the comparative statics with regards to the RS policy under the various hypothesis laid out above.

Production on the Domestic Market   We omit the country subscripts whenever it is without loss of clarity. All firms with the same productivity make the same choices such that firm-level expressions are given as a function of their productivity z. Given the production function (4), a firm facing wage w^l, w^h chooses relative employment of low- and high-wage workers as follows:

$$\chi^t = \frac{w^t \ell^t}{W\ell} = \frac{\alpha^t \left(w^t\right)^{1-\rho}}{W^{1-\rho}}, \tag{13}$$

where ℓ is the labor aggregate in (4) and W is the corresponding labor cost index of the firm:

$$W = \left[\alpha^l \left(w^l\right)^{1-\rho} + \alpha^h \left(w^h\right)^{1-\rho}\right]^{\frac{1}{1-\rho}}.$$

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[7]To be microfounded in later versions of this draft.

On the domestic market, firms are monopolistically competitive and therefore price at constant markup over marginal cost, given demand (1):

$$p = \frac{\sigma}{\sigma - 1} \frac{W}{z}.$$

Conditional on choosing to produce, Home firm sales and employment of type $t-$labor are, respectively:

$$y_H = \left(\frac{\sigma}{\sigma - 1}\right)^{1-\sigma} z^{\sigma-1} W^{1-\sigma} P_H^{\sigma-1} X_H, \tag{14}$$

$$\ell_H^t = \alpha^t \left(\frac{\sigma}{\sigma - 1}\right)^{-\sigma} z^{\sigma-1} \left(w^t\right)^{-\rho} W^{\rho-\sigma} P_H^{\sigma-1} X_H + f_H \alpha^t \left(\frac{w^t}{W}\right)^{-\rho}, \quad t \in \{l, h\} \tag{15}$$

where X_k denotes total expenditure in $k = \{H, F\}$ and P_k is the ideal price index for consumption in $k = \{H, F\}$ corresponding to demand (1),

$$P_k = \left(\int_{\Omega_k} p_w^{1-\sigma} \, d\omega\right)^{\frac{1}{1-\sigma}}. \tag{16}$$

Profits on the Home market are $\pi_H = \frac{1}{\sigma} y_H - f_H W$, increasing in productivity, so that only firms above a productivity cutoff enter the market. The selection cutoff corresponding to zero profit is:

$$z_H^* = \frac{\sigma^{\frac{\sigma}{\sigma-1}}}{\sigma - 1} \frac{f_H^{\frac{1}{\sigma-1}} W^{\frac{\sigma}{\sigma-1}}}{P_H X_H^{\frac{1}{\sigma-1}}}.$$

MNEs and Input Production   Home firms may also, in addition, produce an MNE-specific input. We assume that the corresponding fixed costs are high enough that firms are more selected on the MNE market than on the domestic market. When selling to MNEs, firms set the price r of the intermediate input they produce according to monopolistic competition, such that:

$$r = \frac{\sigma}{\sigma - 1} \frac{W}{z}.$$

Using derivations similar to the ones above for the domestic market yields the selection cutoff for selling to an MNE x for production, as well as the sales and employment (by worker type).[8] All

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[8]Specifically,

$$z_x^* = \frac{\sigma^{\frac{\sigma}{\sigma-1}}}{\sigma - 1} \frac{W^{\frac{\sigma}{\sigma-1}}}{d\left(x\right)^{\frac{1}{\sigma-1}}} \frac{f_x^{\frac{1}{\sigma-1}}}{M_x^{\frac{1}{\sigma-1}} R_x^{\frac{\sigma}{\sigma-1}}},$$

$$y_x = \left(\frac{\sigma}{\sigma - 1}\right)^{1-\sigma} z^{\sigma-1} W^{1-\sigma} R_x^\sigma M_x,$$

$$\ell_x^t = \alpha^t \left(\frac{\sigma}{\sigma - 1}\right)^{-\sigma} \left(w^t\right)^{-\rho} z^{\sigma-1} W^{\rho-\sigma} R_x^\sigma M_x + \alpha^t f_x \left(w^t\right)^{-\rho} W^\rho.$$

these expressions are functions of the input cost index for the MNE subsidiary:

$$R_x = \left(\int_{\Omega_x} r_{\omega(x)}{}^{1-\sigma} \, d\omega\,(x) \right)^{\frac{1}{1-\sigma}}. \tag{17}$$

This input cost index corresponds to the production function (3) and is a function of M_x, the corresponding output of the MNE subsidiary.

In Foreign, the MNE imports the good produced by its subsidiary at Home. The final goods market in Foreign is monopolistically competitive, so that the MNE sells to Foreign consumers at price:

$$p = \frac{\sigma}{\sigma - 1} \zeta R_x.$$

Given the CES demand of Foreign consumers, given in (1), MNE sales in Foreign are:

$$pq = d_x \left(\frac{\sigma}{\sigma - 1} \zeta R_x \right)^{1-\sigma} X_F P_F^{\sigma-1},$$

so that the MNE subsidiary production M_x is:

$$M_x = d_x \left(\frac{\sigma}{\sigma - 1} \right)^{-\sigma} \zeta^{1-\sigma} R_x^{-\sigma} X_F P_F^{\sigma-1}. \tag{18}$$

Coming back to the Home supplier, equilibrium expressions for the selection cutoff for selling to an MNE x for production, as well as sales and employment (by worker type) are solved for as a function of general equilibrium outcomes X_F and P_F using the scale of MNE production (18), yielding:

$$z_x^* = \gamma_1 \frac{W^{\frac{\sigma}{\sigma-1}}}{d_x^{\frac{1}{\sigma-1}}} \frac{f_M^{\frac{1}{\sigma-1}}}{X_F^{\frac{1}{\sigma-1}} P_F}, \tag{19}$$

$$y_x = \gamma_2 \, d_x \, z^{\sigma-1} \, W^{1-\sigma} \, X_F P_F^{\sigma-1}, \tag{20}$$

$$\ell_x^t = \gamma_3 \, \alpha^t \, d_x \, z^{\sigma-1} \, \left(w^t \right)^{-\rho} W^{\rho-\sigma} \, X_F P_F^{\sigma-1} + \alpha^t f_M \left(w^t \right)^{-\rho} W^\rho, \quad t \in \{l, h\}, \tag{21}$$

where γ_1, γ_2 and γ_3 are constants. Finally, combining the definition of the cost index (17) with the selection cutoff in (19), the cost index for MNE subsidiary x can be expressed as:

$$R_x = \gamma_4 W^{\frac{\theta\sigma-\sigma+1}{(\sigma-1)^2}} \, d_x^{-\frac{\theta-\sigma+1}{(\sigma-1)^2}} \, X_F^{-\frac{\theta-\sigma+1}{(\sigma-1)^2}} \, P_F^{-\frac{\theta-\sigma+1}{\sigma-1}}, \tag{22}$$

where γ_4 is another constant.[9]

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[9]Specifically, $\gamma_1 = \frac{\sigma^{\frac{\sigma}{\sigma-1}}}{\sigma-1} \left(\frac{\sigma}{\sigma-1} \right)^{\frac{\sigma}{\sigma-1}} \zeta$, $\gamma_2 = \left(\frac{\sigma}{\sigma-1} \right)^{1-2\sigma} \zeta^{1-\sigma}$, $\gamma_3 = \frac{\sigma}{\sigma-1}\gamma_2$, $\gamma_4 = \frac{\sigma}{\sigma-1} \left(\frac{\theta}{\theta-\sigma+1} \right)^{\frac{1}{1-\sigma}} \gamma_1^{\frac{\theta-\sigma+1}{\sigma-1}} f_M^{\frac{\theta\sigma-\sigma+1}{(\sigma-1)^2}}$.

2.3.2 Monopsony in the Labor Market

To set the stage for the comparative statics under hypothesis (B'), we solve here for the model when labor supply is given by (11) and the labor market at Home is monopsonistically competitive. Each firm faces an upward-sloping labor supply curve, given by equation (11).[10] Taking this labor supply into account, as well as the CES product demand, firms set wages and prices to maximize profits. Because they face a firm-specific upward-sloping labor supply curve, firms restrict hiring to keep the wages of all their workers low. Formally, taking the first-order condition for profit maximization of the supplier leads to the following wage profile across heterogeneous firms:

$$w_H = \frac{\sigma - 1}{\sigma} \frac{\kappa}{\kappa + 1} zp,$$
$$w_x = \frac{\sigma - 1}{\sigma} \frac{\kappa}{\kappa + 1} zr.$$

Firms optimally set wages at a markdown $\frac{\kappa}{\kappa+1}$ over their marginal revenue product of labor. Using the product market clearing on the output markets pins down the scale of production of each firm on each production line, given this wage-price schedule. In equilibrium, a firm with productivity z optimally offers wages:

$$w_H^* = z^{\frac{\sigma-1}{\kappa+\sigma}} \Phi^{\frac{\kappa}{\kappa+\sigma}} \left(X_H P_H^{\sigma-1}\right)^{\frac{1}{\kappa+\sigma}} L_H^{-\frac{1}{\kappa+\sigma}} \left(\frac{\sigma-1}{\sigma} \frac{\kappa}{\kappa+1}\right)^{\frac{\sigma}{\kappa+\sigma}}, \tag{23}$$

and similarly on the intermediate input market.[11] When wages are optimally chosen, the sales of a firm with productivity z are given by:

$$y_H = z^{\frac{(\sigma-1)(\kappa+1)}{\kappa+\sigma}} \Phi^{\frac{\kappa(1-\sigma)}{\kappa+\sigma}} \left(X_H P_H^{\sigma-1}\right)^{\frac{1+\kappa}{\kappa+\sigma}} L_H^{\frac{\sigma-1}{\kappa+\sigma}} \left(\frac{\sigma-1}{\sigma} \frac{\kappa}{\kappa+1}\right)^{\frac{\kappa(\sigma-1)}{\kappa+\sigma}},$$

with a similar expression on the intermediate input market.[12]

Note that if a wage $\underline{w}$ is imposed to the firm, rather than being chosen optimally by the firm, firm sales depend on whether hiring is determined by the labor supply curve (which is the case when labor supply < labor demand), or whether it is determined by the labor demand curve (when labor supply ≥ labor demand). In the former case, we have:

$$y_H = \frac{\sigma}{\sigma-1} \underline{w}^\kappa \Phi^{1-\kappa} \Gamma\left(1 - \frac{1}{\kappa}\right) L_H, \tag{24}$$

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[10]Firms know the aggregate labor supply curve they face, but do not observe workers' individual preference shocks.

[11]Specifically, $w_x^* = z^{\frac{\sigma-1}{\kappa+\sigma}} \Phi^{\frac{\kappa}{\kappa+\sigma}} d_x^{\frac{1}{\kappa+\sigma}} \left(X_F P_F^{\sigma-1}\right)^{\frac{1}{\kappa+\sigma}} L_H^{-\frac{1}{\kappa+\sigma}} \left(\frac{\sigma-1}{\sigma} \frac{\kappa}{\kappa+1}\right)^{\frac{\sigma}{\kappa+\sigma}} \left(\frac{\sigma}{\sigma-1}\zeta\right)^{\frac{1-\sigma}{\kappa+\sigma}}.$

[12]Specifically, $y_x = z^{\frac{(\sigma-1)(\kappa+1)}{\kappa+\sigma}} \Phi^{\frac{\kappa(1-\sigma)}{\kappa+\sigma}} d_x^{\frac{1+\kappa}{\kappa+\sigma}} \left(X_F P_F^{\sigma-1}\right)^{\frac{1+\kappa}{\kappa+\sigma}} L_H^{\frac{\sigma-1}{\kappa+\sigma}} \left(\frac{\sigma-1}{\sigma} \frac{\kappa}{\kappa+1}\right)^{\frac{\kappa(\sigma-1)}{\kappa+\sigma}} \left(\frac{\sigma}{\sigma-1}\zeta\right)^{\frac{1+\kappa}{\kappa+\sigma}(1-\sigma)}.$

where Γ is the gamma function. In the latter case, we have:

$$y_H = \left(\frac{\sigma}{\sigma - 1}\right)^{1-\sigma} z^{\sigma-1} \underline{w}^{1-\sigma} P_H^{\sigma-1} X_H, \tag{25}$$

and similarly on the intermediate input market.[13]

2.4   Impact of RS Policies: Comparative Statics for Model Selection

Consider the suppliers of an MNE x^{RS} that puts in place an RS policy. There are two types of responses to the policy: on the extensive margin and on the intensive margin. First, on the extensive margin, pre-existing suppliers to the MNE may refuse to engage in the policy and drop out of the intermediate input market. Still, these firms would keep on supplying the domestic market, without adopting the RS policy.[14] Second, on the intensive margin, those who accept the conditions of the policy see their sales impacted by the increased labor costs, both on the intermediate input market and the domestic market.

To reflect these two margins, we consider the impact of RS on two groups of firms: *compliers*, and *exposed* firms. Complying firms are part of the exposed firms' group but continue to sell to MNE x^{RS} after the RS policy is put in place. The comparative statics that pertain to compliers capture, therefore, the intensive margin effect of the policy. Exposed firms include compliers, as well as pre-existing suppliers of MNE x^{RS} that drop out of the sales relationship to x^{RS} once the RS policy is put in place. The comparative statics that pertain to exposed firms capture, therefore, both the intensive and the extensive margins effects of the policy. Formally, we define the sales of exposed suppliers on the market for inputs for x and the domestic market, respectively, are:

$$Y_x = \int_{z_x^*}^{\infty} y_x(z)\, dG(z) \text{ and } Y_H = \int_{z_H^*}^{\infty} y_H(z)\, dG(z),$$

where the set of firms is held fixed when taking comparative statics.

In all the comparative statics presented here, we compare the sales response of firms impacted by RS (exposed or compliers) to the sales of similar firms–in terms of productivity–that sell to another MNE x^N that does not implement RS over this period. Those are indexed by N (for Non-RS). We use hat notations $\hat{x} = d\log x$ to denote log changes in variable x following the implementation of RS. We derive comparative statics by computing the effect of a small RS policy $\left(\hat{\tau}, \hat{T}\right)$.

We first examine comparative statics in the baseline case where RS policies are not profit-maximizing for the firm, the Home labor market is competitive, RS is not accompanied by a productivity improvement, and the increase in labor cost is fully passed through to the MNE.

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[13]When labor supply < labor demand, $y_x \propto \underline{w}^\kappa$, and when labor supply $\geq$ labor demand, $y_x \propto \underline{w}^{1-\sigma}$.

[14]This is the case because serving an MNE and serving the domestic market are ordered hierarchically: fixed costs of producing on the domestic market are low enough that there exists a measure of firms supplying the domestic market but not the MNE market.

That is, hypotheses A, B, C, and D constitute the baseline.

2.4.1 Baseline

We first consider the impact of the policy on compliers, on the MNE market as well as on the domestic market. Given (14) and (20), comparing a given (complying) supplier of MNE x^{RS} to a non-RS-supplier of MNE x^N with the same productivity yields:

$$\hat{y}_{x^{RS}} - \hat{y}_{x^N} = (1-\sigma)\,\chi^l\hat{\tau} < 0 \tag{26}$$

$$\text{and} \quad \hat{y}_{H^{RS}} - \hat{y}_{H^N} = (1-\sigma)\,\chi^l\hat{\tau} < 0, \tag{27}$$

where χ^l is the initial wage bill share of low-wage workers for those firms, as defined in equation (13), and where we have indexed the domestic market with a superscript RS or N depending on whether the supplier is hit by an RS shock - which carries through to the domestic market - or not. Equations (26) and (27) show that the cost shock $\chi^l\hat{\tau}$ is passed-through to sales of compliers with an elasticity of $1-\sigma$, for both MNE sales and domestic sales.

Next, we consider the impact of the policy on exposed firms. The extensive margin response to the RS policy differs between the MNE market and the domestic market because firms that drop out of the MNE market still sell to the domestic market.[15] Combining the extensive and intensive margin responses, the total effect of RS policies on the sales of the pre-existing suppliers of the MNE x^{RS} is:

$$\hat{Y}_{x^{RS}} - \hat{Y}_{x^N} = \frac{\sigma - 1 - \theta\sigma}{\sigma - 1}\chi^l\hat{\tau} < 0 \tag{28}$$

$$\text{and} \quad \hat{Y}_{H^{RS}} - \hat{Y}_{H^N} = (1-\sigma)\,\chi^l\hat{\tau} < 0, \tag{29}$$

Summing those up, the effect of the policy on the total firm sales of exposed firms, $Y_{tot} = \int_x Y_x dx + Y_H$ is also negative:

$$\hat{Y}_{tot^{RS}} - \hat{Y}_{tot^N} = \left(1 - \sigma - \xi\sigma\frac{\theta - \sigma + 1}{\sigma - 1}\right)\chi^l\hat{\tau} < 0,$$

where we have defined

$$\xi \equiv \frac{Y_x}{Y_x + Y_H}, \tag{30}$$

the sales share (also equal to the wage bill share) among exposed firms dedicated to the production line for MNE x^{RS} compared to all their production lines.

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[15]Specifically, the relative change in the productivity cutoff for serving the MNE market is: $d\log z^*_{x^{RS}} - d\log z^*_{x^N} = \frac{\sigma}{\sigma-1}\chi^l d\log\tau$, while for the domestic market it is: $d\log z^*_{H^{RS}} - d\log z^*_{H^N} = 0$.

2.4.2 Assessing Hypothesis A' (Profit-Maximizing RS)

We now turn to assessing hypothesis A' against the benchmark model. Under this hypothesis, the MNE chooses the RS policy τ optimally, trading off increased input costs against increased demand for its product, since $d'_x(\tau) > 0$. Given the Foreign MNE x profit function:

$$\pi = \frac{1}{\sigma} d_x \left(\frac{\sigma}{\sigma - 1} \zeta R_x \right)^{1-\sigma} X_F P_F^{\sigma-1}, \tag{31}$$

and given the expression for the input cost R_x in (22), taking the first order condition of the profit maximization problem with respect to τ yields an optimal choice of τ that verifies:

$$\frac{d \log d_x}{d \log \tau} = \frac{\theta\sigma - \sigma + 1}{\theta} \chi^l. \tag{32}$$

Mimicking the effect of the policy on the MNE, complying suppliers see two effects of the policy: an increase in labor costs that tends to decrease their sales on all markets, and an increase in the demand coming from the MNE, because of the demand shock $d'_x(\tau) > 0$. The positive demand shock associated with the RS policy positively impacts the suppliers' sales to the MNE but does not impact their domestic sales. On net, combining the cost effect and the demand effect, the effect of the policy on compliers is now as follows:

$$\hat{y}_{x^{RS}} - \hat{y}_{x^N} = \frac{\theta - \sigma + 1}{\theta} \chi^l \hat{\tau} > 0 \tag{33}$$

$$\text{and } \hat{y}_{H^{RS}} - \hat{y}_{H^N} = (1 - \sigma) \chi^l \hat{\tau} < 0. \tag{34}$$

In contrast to the baseline case, the relative sales of suppliers that take on the RS policy increase on the MNE input market.

Next, we consider the impact of the policy on exposed firms. Just like the RS policy increases MNE profits for the MNE x^{RS} compared to the MNE x^N, it also increases the relative sales and input demand of MNE x^{RS}. In turn, the selection cutoff for suppliers to produce inputs for MNE x^{RS} goes down, i.e., serving the MNE becomes easier.[16] Since there is entry to serving the MNE x^{RS} (rather than exit of pre-existing suppliers), this change in the extensive margin does not impact the total effect of RS on the sales of pre-existing suppliers beyond its impact on the intensive margin of sales of pre-existing firms, captured above. Therefore, the total sales of exposed firms compared to non-exposed firms is simply:

$$\hat{Y}_{x^{RS}} - \hat{Y}_{x^N} = \frac{\theta - \sigma + 1}{\theta} \chi^l \hat{\tau} > 0,$$

$$\hat{Y}_{H^{RS}} - \hat{Y}_{H^N} = (1 - \sigma) \chi^l \hat{\tau} < 0.$$

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[16]Specifically, the relative change in the productivity cutoff for serving the MNE market is:$d \log z^*_{x^{RS}} - d \log z^*_{x^N} = -\frac{1}{\theta}\hat{\tau} < 0$ while on the domestic market $d \log z^*_{H^{RS}} - d \log z^*_{H^N} = 0$.

Note that the effect of the policy on the total (domestic+MNE) sales of these firms, $\hat{Y}_{tot^{RS}} - \hat{Y}_{tot^N}$, has an ambiguous sign, since one change is positive and the other negative.

2.4.3 Assessing Hypothesis B' (Labor Market Monopsony)

Next, we examine how RS impacts firm sales under the hypothesis that labor markets are monopsonistic (Hypothesis B'). Our strategy is still to compare firms with similar productivity, some being exposed to RS, and others not. Given that RS policies, defined in (12), are binding only for firms at which wages are low, several cases arise, depending on where the firm wage, pre-RS, lies compared to the wage floor $\underline{w}$ imposed by the RS policy. To that end, we denote $w_k^*(z)$ the monopsony wage level of a firm with productivity z on production line $k = H, x$. Three main cases arise.

First, if $w_k^*(z) \geq \underline{w}$, that is, given (23), when firm productivity is high enough, RS is not binding. There is no relative effect of RS on suppliers that adopt it, versus those with equivalent productivity that do not.

Second, when $w_k^*(z) < \underline{w} \leq \frac{\kappa}{\kappa-1} w_k^*(z)$ RS is now binding and corresponds to a wage increase from $w_k^*(z)$ to $\underline{w}$ for impacted firms. In this case, the sales of compliers go up, both on the final market and on the intermediate goods market. This sales increase comes from the following mechanism: higher wages make the firm hire more employees compared to the monopsonistic case where the firm voluntarily restricted its hiring. This leads to higher production and higher sales, given that the wage (hence price) increase is moderate - but of course, to lower profits.[17]

Third, if $\underline{w}^{RS} > \frac{\kappa}{\kappa-1} w_k^*(z)$, which could be the case for the lowest productivity firms, these firms see their sales decrease. The wage increase is too high to sustain higher sales.

Overall, we have that for both $k = H$ and $k = x$:

$$
\begin{cases}
\hat{y}_{k^{RS}} - \hat{y}_{k^N} = 0 & \text{if } w_k^*(z) \geq \underline{w}^{RS} \\
\hat{y}_{k^{RS}} - \hat{y}_{k^N} \geq 0 & \text{if } w_k^*(z) \leq \underline{w}^{RS} \leq \frac{\kappa}{\kappa-1} w_k^*(z) \\
\hat{y}_{k^{RS}} - \hat{y}_k \leq 0 & \text{if } w_k^*(z) \leq \frac{\kappa-1}{\kappa} \underline{w}^{RS}
\end{cases}
$$

In practice, note that this third case is likely to be of limited empirical relevance. First, because these lower productivity firms are likely to exit the market. Second, because the RS wage is unlikely to be high enough to trigger a wage increase of more than $\frac{\kappa}{\kappa-1}$, which corresponds to a 20% increase in wages for typical values of the parameter κ.[18]

Turning to the effect on exposed firms, we need to take into account the extensive margin effect of the RS policy. Because it reduces profits for all firms for which RS is binding, the policy is

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[17]These qualitative patterns mask two different subcases: one where firm hiring is set by the labor supply curve, hence sales are given by (24). This happens so long as $\underline{w} \leq w^{eq}$, where w_k^{eq} is a firm-specific equilibrium wage for which labor supply equals labor demand on production line k. In the other subcase, the wage increase is high enough that the labor supply is higher than labor demand, hence sales are pinned down by (25), but the wage increase is not too high, so that labor hired is still above the monopsonistic level.

[18]For instance, Berger et al. (2021) find values of the labor supply elasticity ranging from $\kappa \in (3, 7)$, which leads to $\frac{1}{\kappa-1} \in (16\%, 50\%)$.

accompanied by exits of preexisting suppliers that were close to the selection cutoff. Therefore, the effect of the RS policy on exposed firms is overall ambiguous:

$$\hat{Y}_{tot^{RS}} - \hat{Y}_{tot^{N}} \text{ has ambiguous sign.}$$

2.4.4 Assessing Hypothesis C' (Productivity Gains)

We turn to assessing hypothesis C' against the baseline case. Recall that we aim to detect potential labor productivity gains associated with the RS policy, denoted by $\hat{T}$. To test this particular hypothesis, we turn the focus to comparative statics on the wages paid to workers rather than on the sales of the supplier. Indeed, the labor productivity gain T is paid to the workers so that it is neutral in terms of sales for the firms.

To identify whether $\hat{T} \neq 0$, we compare the wages at exposed firms vs. non-exposed firms. Specifically, we focus on the impact of the policy on workers' compensation, noted $\tilde{w}^t$ in the model for $t = l, h$. We leverage the fact that high-wage workers are only impacted by the productivity gain, not by the net increase in labor costs of the RS policy.

Formally, define the average wage of workers of type $t = l, h$ across all exposed firms:

$$\bar{w}^{t,RS} = \frac{\left(\int_{z^*}^{\infty} \tilde{w}^t \, \ell^t\left(z\right) dG^{RS}\left(z\right) \right)}{\left(\int_{z^*}^{\infty} \ell^t\left(z\right) dG^{RS}\left(z\right) \right)}, \tag{35}$$

where $G^{RS}\left(z\right)$ is the distribution of productivities of treated firms, and define similarly the average wage of workers across non-exposed firms $\bar{w}^{t,N}$. We then simply have, for high-wage workers:

$$\hat{\bar{w}}^{h,RS} - \hat{\bar{w}}^{h,N} = \hat{T},$$

while $\hat{\bar{w}}^{h,RS} - \hat{\bar{w}}^{h,N} = 0$ in the baseline (hypothesis C).

2.4.5 Assessing Hypothesis D' (Imperfect Pass-Through)

Finally, we assess hypothesis (D') against the baseline case. That is, we now consider the case where only part of the labor cost increase is passed through to the MNE, and CR firms bear $(1 - \beta)$ of the increase in costs due to the policy. In this case, the sales of compliers are impacted as follows:

$$\hat{y}_{x^{RS}} - \hat{y}_{x^{N}} = \beta \left(1 - \sigma\right) \chi^l \hat{\tau} < 0, \tag{36}$$

$$\hat{y}_{H^{RS}} - \hat{y}_{H^{N}} = \left(1 - \sigma\right) \chi^l \hat{\tau} < 0. \tag{37}$$

Compared to the baseline case, sales to domestic firms fall more than sales to the MNE. In addition, the effect of the policy on the extensive margin of firms serving MNE x^{RS} is still negative and is stronger than in the baseline case since the profits of all firms on the x^{RS} market go down, making it harder to operate and raising the productivity threshold for operation.

Putting the extensive and intensive margins together, we get that the effect of the policy on

the average sales of exposed firms is:

$$\hat{Y}_{x^{RS}} - \hat{Y}_{x^N} = \underbrace{\beta\left(1-\sigma\right)}_{\text{intensive}<0} + \underbrace{\left(\sigma+1-\beta\right)\frac{\sigma-1-\theta}{\sigma-1}}_{\text{extensive}<0} \chi^l \hat{\tau} \ < 0, \tag{38}$$

$$\hat{Y}_{H^{RS}} - \hat{Y}_{H^N} = \left(1-\sigma\right)\chi^l\hat{\tau} \ < 0. \tag{39}$$

Since both the domestic and MNE sales of the exposed suppliers decrease, their total sales decrease too, specifically:

$$\hat{Y}_{tot^{RS}} - \hat{Y}_{tot^N} = \left[1-\sigma-\xi\sigma\frac{\theta-\sigma+1}{\sigma-1}+(1-\beta)\xi\left(\sigma-\frac{\theta}{\sigma-1}\right)\right]\chi^l\hat{\tau} < 0, \tag{40}$$

where ξ was defined in (30).

In the empirical analysis below, we proceed in two steps to take the model to the data. We first use the reduced-form analysis in Section 4 to qualitatively test for a number of comparative static results. This allows us to discriminate between Hypotheses A vs. A' and B vs. B'. In particular, while all variants of our theory can rationalize a negative effect of RS on the total and domestic sales of exposed firms, Hypotheses A' and B' make sharp predictions that the intensive-margin sales to the MNE should be unambiguously *increasing* at the same time. In contrast, Hypotheses A and B predict that all sales should be declining significantly. In Section 5 we then proceed to the parameter estimation using the variant of our theory supported by the initial evidence. These parameters include potential changes in labor productivity among all workers (Hypothesis C vs. C') and the extent of differential cost pass-through to the MNE (Hypothesis D vs. D').

3 Data and Context

In this section, we briefly describe the data used in this paper and the analysis sample. First, we bring together several administrative datasets that encompass firm-to-firm transaction data, matched employer-employee data, corporate tax returns, customs data, and foreign ownership data. The construction of these datasets is described in detail in Alfaro-Ureña et al. (2020) and Alfaro-Ureña et al. (2021). We combine these administrative data with a novel dataset on RS policy rollouts. After describing the datasets, we provide descriptive statistics for the analysis sample of RS policy rollouts, MNEs whose policy rollouts we study, and their suppliers.

3.1 Administrative Data

Firm-to-Firm Transaction Data This dataset tracks the near-universe of formal firm-to-firm relationships in CR between 2008 and 2017. This information is collected by the Ministry of Finance through the D-151 tax form. Firms must report the tax identifier of all their suppliers and buyers with whom they generate at least 2.5 million Costa Rican colones (around 4,200 U.S.

dollars) in transactions that year, in addition to the total amount transacted. We use this data to identify the domestic firms affected by a new RS policy of an MNE affiliate in CR. The data also provide information on the 4-digit industry that each firm belongs to.

Corporate Income Tax Returns   We then add the yearly corporate income tax returns from the Ministry of Finance of CR for the same 2008 to 2017 time period. These returns cover the universe of formal firms in the country and contain typical balance sheet variables (e.g., total sales, net assets, input costs, etc.) and other firm characteristics (such as its four-digit industry). We observe 375 4-digit industries in the CR data.

Matched Employer-Employee Data   The CR Social Security Administration collects this panel, which contains the employment records of all formal workers in CR from 2006 to 2017. We observe (at least once) 1.9 million unique person identifiers (PIDs). For each PID, this data records demographic characteristics (such as the date of birth, nationality, sex) and the monthly labor earnings and occupation at each employer.

Foreign Ownership Data   We use a comprehensive dataset on the foreign ownership of firms in CR. We combine information from: (i) three annual surveys conducted by BCCR, (ii) the reporting of firms that are active under the Free Trade Zone regime, (iii) the records of the investment promotion agency of Costa Rica (CINDE), and (iv) Orbis. Jointly, these records allow us to confirm which foreign firms in the country are part of an MNE group. For each firm, we track the MNE group it belongs to, so as to be able to link it to the RS policies put in place at the group level, as described next.

3.2   Responsible Sourcing (RS) Policies

We construct a new database that tracks the RS policies rolled out by MNEs with an affiliate in CR. We use "RS policy" to describe the introduction of new MNE requirements concerning their suppliers. While there is no unique definition of RS, the International Chamber of Commerce defines RS as a "commitment by companies to take into account social and environmental considerations when managing their relationships with suppliers". The introduction of a suppliers' code of conduct is an example of an RS policy (and, as we document below, by far the most frequent type of RS policy in our context).

The first step in building the database is to identify the MNEs with on-the-ground supplier relationships in CR. To do so, we identify 484 MNEs with an affiliate in CR whose total average yearly input purchases exceed 1 million U.S. dollars (where the average is computed across all years of operation of that affiliate in CR). As Table 1 shows, these 484 affiliates account for 77.2% of the local input purchases, 83.4% of the employment, 82.2% of the total sales, 87.3% of the imports, and 94.7% of the exports of all firms in CR that belong to corporate groups where at least one firm is partially foreign-owned (which is our definition of MNEs).

The second step is to conduct a comprehensive search for RS policy rollouts by these 484 MNEs (both locally or beyond CR). To do so, we implemented a double-blind comprehensive search process executed by two independent teams (whose output we then cross-check and combine into one final database): we searched all company reports, press releases, corporate filings, and publications available online (including company websites) containing information about corporate social responsibility and supplier requirements. In addition, for each MNE, we conducted online searches in both local (CR) and international media outlets to gather additional information on rollouts. In line with our theoretical framework, we focus on RS requirements targeted at improving working conditions (a theme sometimes referred to as "fair and humane working conditions" in the policy literature on RS). Common examples in the database are minimum wage floors, benefit guarantees, formality requirements for all employees, safety conditions at the workplace, restrictions on hours worked by employees, and the creation of worker organizations for representation vis-a-vis the management. For each RS policy that we identified in this database, we recorded the year of implementation, whether the policy was MNE-wide (introduced by the MNE headquarters for suppliers to affiliates worldwide) or specific to CR (or Central America), and a number of additional details we describe below.

3.3 Empirical Context

For empirical context, we now provide descriptive statistics on the RS rollouts in our analysis sample, the MNEs that rolled out these policies, and their suppliers.

RS Policy Rollouts   We focus on MNEs that had at least one rollout between 2009 and 2017 of an RS policy concerning the working conditions at their suppliers. We hone in on the 2009 to 2017 period given the coverage of the administrative data above (that start in 2008).[19]

This analysis sample consists of 152 RS policy rollouts by 127 distinct MNEs. Of these 127 MNEs, 107 MNEs have only one RS policy rollout in the 2009 to 2017 period of interest, 16 MNEs have two rollouts, three MNEs have three rollouts, and one MNE has four rollouts. The RS policy rollouts are fairly equally distributed across time, ranging from 13 to 23 rollouts per year. Of the 151 RS policies in the analysis sample, 94% (142) involve a new supplier code of conduct or a substantive change to an existing supplier code.[20]

The primary source of information for 92% (139) of these policies is the website of the parent of the MNE (or a report found elsewhere but characterizing the entire MNE group). The remaining 8% (12) RS policies were found on the website of the CR affiliate of the MNE. This breakdown of the data source is in line with the fact that 85% (129) of these policies apply to the entire MNE group (not only to the CR affiliate). MNE-wide policies have the advantage of a more plausible exogeneity of the introduction of the RS policy to the sourcing conditions of the MNE in CR.

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[19] In addition, we restrict attention to MNEs that did not have an RS policy rollout between 2005 and 2008.

[20] Among the remaining 6%, six RS policies involve a recurrent workshop (training course or consultancy) offered to supplier firms, two involve a recurrent networking event or awards ceremony for supplier firms, and one involves a one-off workshop (training course or consultancy) offered to supplier firms.

92% (139) of the RS policies apply to all the direct suppliers of their affiliates (as opposed to policies relevant only to specific suppliers, such as those operating in the mining sector). While 41% (62) of the policies apply not only to the direct suppliers but also to the entire value chain (e.g., suppliers of the suppliers), for now, we only study the effects on the direct suppliers.

MNEs With RS Policies   The analysis sample involves 127 MNEs with at least one RS policy event meeting the conditions described above. On average, across years of activity in CR, the MNEs in the analysis sample employ 685 workers, sell 97 million U.S dollars, and export 41 million U.S dollars. MNEs that did not report RS rollouts over this period on average employ 452 workers, sell 50 million U.S dollars, and export 15 million U.S dollars. 38% of the 127 MNEs in the analysis sample are from the United States, 27% are from Europe, and the other 35% are either from Latin America or Asia. 40% operate in a manufacturing sector, 44% in services, 14% in retail (including repair and maintenance), and 2% in agriculture. In comparison, the sample of MNEs that did not implement an RS policy is less skewed towards the United States or Europe (only 49% together) and manufacturing (32%). For details, see Table 2.

Suppliers Affected by RS Policies   In line with our empirical strategy described below, we characterize the domestic firms supplying to each MNE in the analysis sample in the year *before* the RS policy rollout of that MNE. On average, these suppliers employ 16 workers, sell 1.2 million U.S dollars, and pay 7,000 U.S dollars. 11% operate in a manufacturing sector, 54% in services, 26% in retail (including repair and maintenance), and 9% in agriculture. For details, see Table 3.

4   Evidence

4.1   Empirical Strategy

In this section, we use the database described above to estimate the effects of RS rollouts on outcomes at the level of suppliers (firms), individual workers, and firm-to-firm transactions. We will then confront this empirical evidence to the corresponding model's comparative statics from Section 2.4, to assess which model hypotheses fit the data best.

Supplier-Level Specifications

Using the database, we run event-study specifications of the following form:

$$y_{ist} = \alpha_i + \gamma_{st} + \sum_{\eta=k_l}^{\eta=k_u} \beta_\eta I(\text{Years since RS}_{it} = \eta) + \epsilon_{ist} \,, \tag{41}$$

where y_{ist} is an outcome (i.e., log firm sales or log total employment) of firm i from the sample of firms who are suppliers to MNE affiliates in CR at some point over the sample period 2008-2017. s indexes one of the 375 4-digit sectors in CR and t indexes years. α_i are firm fixed effects and γ_{st} are sector-by-year fixed effects. The term $\sum_{\eta=k_l}^{\eta=k_u} \beta_\eta I(\text{Years since RS}_{it} = \eta)$ captures the event-study

design: $I(\cdot)$ is an indicator function and η indexes the number of years before or after the rollout of the RS policy by the MNE that is linked to the firm.[21] Following the theory, we define exposure to a given RS rollout (RS_{it}) for domestic firms that were selling to the MNE in question in the year before the rollout (at $\eta = -1$). To adjust for autocorrelation across years for the same producer, we cluster the standard errors (ϵ_{isrt}) at the level of firms.

The main identification concern for estimating the β_η coefficients is that RS rollouts could have been targeted by the MNE during periods when CR suppliers experienced other contemporaneous shocks (e.g., to their productivity). We investigate and address such endogeneity concerns in several ways. First, to limit concerns of different time trends across different types of firms, we restrict the estimation sample to only CR firms that have been suppliers to MNEs at some point during the estimation sample 2008-2017.

Second, we assess the presence of confounding shocks that may have preceded the MNE's RS rollout decision using the event-study design (documenting the β_η both before and after the rollout). To do so convincingly, we build on recent advances in the applied econometrics literature on DiD estimation with multi-period ("staggered") treatment events. Several recent papers have shown that estimating specification (41) with two-way fixed effects regressions can fail to recover average treatment effects even if the treatment events were as good as randomly assigned (Borusyak et al., 2021, De Chaisemartin and d'Haultfoeuille, 2020, Goodman-Bacon, 2018, Sun and Abraham, 2020, Baker et al., 2021, Roth and Sant'Anna, 2021). This can be the case when treatment effects are dynamic (evolving over time) because already-treated units enter the control group. Moreover, two-way fixed effects estimation produces variance-weighted averages of potentially heterogeneous treatment effects, complicating their interpretation and link to economic theory.

To address these concerns, we build on recent work by Sun and Abraham (2020) who explicitly focus on event-study designs with leads and lags of treatment indicators (instead of the more standard DiD case with a single binary treatment indicator that has been the focus of the bulk of the recent literature above). As, e.g., Goodman-Bacon (2018) and Sun and Abraham (2020) show, event-study designs already address several of the estimation concerns that are present in the pooled DiD with staggered treatments and dynamic effects. The main concern that remains in specifications of the form in 41 above is that different cohorts of treated firms over time may be subject to *different* dynamic paths of treatment effects. So, in our context, the concern would be that those firms exposed to RS in earlier periods of our sample (e.g., around 2010) may experience systematically different time paths of treatment effects compared to those firms exposed in later years (e.g., 2015-2017). The estimation method developed by Sun and Abraham (2020) addresses such concerns by estimating the dynamic effect for each treatment cohort separately (i.e., for units treated in the same calendar year) and then calculating the weighted average of these cohort-specific effects, with weights equal to each cohorts sample share. We thus report estimation

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[21]We include all periods η observed in the sample (i.e., $k_l = -9$ and $k_u = 8$) except the omitted period at $\eta = -1$, and we report estimates for $\eta \geq -4$ and $\eta \leq 4$ in the figures and tables.

results for (41) both using standard two-way fixed effects and the estimation procedure developed by Sun and Abraham (2020).

Third, we present the event study both before and after using only RS rollouts in other MNE affiliate countries (due to global RS rollouts) as instruments for rollouts among CR suppliers. Using RS rollout decisions that were made at the MNE headquarters, covering all supplier relationships around the globe, as an IV aims to address the concern that RS rollouts could have been targeted at the precise point in time during which CR suppliers started to experience contemporaneous shocks. To the extent that MNEs may implement other organizational changes at the same time as RS rollouts, also note that our theory in Section 2 and quantitative analysis in Sections 5 and 6 allow us to incorporate and disentangle any such contemporaneous effects on MNE output (and thus their input demand from suppliers) or transfers of technology affecting supplier productivity. In particular, our estimation allows us to separately quantify any contemporaneous positive shocks to MNE output and any contemporaneous direct productivity effects due to RS among their CR suppliers. While our model uses particular labels of such contemporaneous effects (i.e., positive MNE demand shocks due to RS for the former), their consequences for CR welfare would be isomorphic regardless of the label.[22]

Fourth, our definition of rollout exposure in RS_{it} can also give rise to a somewhat mechanical bias in the event-study coefficients β_η: given that supplier sales to MNEs can be subject to annual fluctuations for many other reasons, defining exposure to RS in terms of a positive MNE sales event in year $\eta = -1$ may pick up particularly successful periods among the exposed group of suppliers (i.e., picking lucky or successful firm-by-year combinations in which a supplier happened to sell to an MNE). This lumpy nature of sales events could give rise to positive pre-trends and negative post-trends even in the absence of any actual impacts of RS. To address this concern, we estimate specification (41) both before and after including an additional set of event-study indicators, $\sum_{\eta=-4}^{\eta=4} \delta_\eta I(\text{Years since MNE}_{it} = \eta)$, where the η years are identical to the RS event-study years and MNE_{it} switches on to unity for all CR suppliers that had active sales relationships to any MNE in CR at the event year $\eta = -1$ (one year before the RS policy was rolled out). When including these additional event-study terms in (41), we thus estimate the event study of RS rollouts among exposed CR suppliers after controlling for the full timeline of potential effects that may stem from having had a positive sales relationship to any MNE (regardless of RS rollouts) at the event time $\eta = -1$.

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[22]Imagine, for instance, that MNEs roll out other supply chain modernizations at the same time as rolling out RS. Instead of a positive demand shock that is driven by global consumers valuing RS (as in our model), the MNE would experience an increase in output (and its input demand from RS suppliers) due to an increase in its competitiveness. First, we will be able to assess such possibilities in the data when looking at the intensive margin effect on sales to MNEs below. Second, to the extent that such positive MNE effects are the case, their consequences for CR welfare (due to increased input demand) would be isomorphic to the case of "RS as profit maximization" that we outline in the theory.

Worker-Level Specifications

In addition to the supplier-level effects above, we also estimate the effects of RS rollouts at the level of individual workers using the matched employer-employee database. We use the same identification strategy as outlined above but include a richer set of fixed effects reflecting the difference in the dimensionality of the worker-level panel data. In particular, we estimate event-study specifications of the following form:

$$y_{ijst} = \alpha_{ij} + \gamma_{st} + \sum_{\eta=k_l}^{\eta=k_u} \beta_\eta I(\text{Years since RS}_{jt} = \eta) + \epsilon_{ijst}\,, \tag{42}$$

where α_{ij} are now fixed effects for worker (i)-by-firm (j) pairs, and γ_{st} and $\sum_{\eta=k_l}^{\eta=k_u} \beta_\eta I(\text{Years}$ since $\text{RS}_{jt} = \eta$) are the same as above (i.e., the firm's sector-by-year fixed effects and the firm (j)-level RS event-study terms). As above, we estimate this specification before and after including the full timeline of effects for having sold to any MNE (to address any mechanical biases discussed above), before and after using the IV specification and before and after using the recent estimation method by Sun and Abraham (2020). And again, we cluster the standard errors at the level of the treatments (at the level of firms j here).

Specification 42 thus estimates the effect of RS rollouts by MNEs on the outcomes y_{ijst} of workers who are employed by exposed suppliers (compared to outcome changes at suppliers to MNEs who did not roll out RS requirements over the same period), conditional on worker-by-firm fixed effects (α_{ij}). We focus on the log annual earnings of workers, divided by the number of months of employment of the worker during the year. Through the lens of the model, we interpret the effects in terms of wage changes.[23]

Transaction-Level Specifications

To estimate the effect on the intensive margin of sales to the RS-MNE by RS-compliant suppliers in the exposed group, we estimate event-study specifications at the level of firm-to-firm transactions. In particular, we create an estimation sample that only includes firm-to-firm sales where any MNE affiliate in CR is the buyer (i.e., only sales transactions with MNE buyers in all years).

With these data, we estimate the same specification as in 42 above, where y_{ijst} are log transaction amounts (sales) between CR supplying firms i selling to MNE buyers indexed by j. α_{ij} are thus buyer-by-seller fixed effects. Here, the event-study terms are defined at the level of the MNE buyers j instead of their exposed suppliers, so that η indexes the number of years before or after the MNE j rolls out the RS policy. The identification strategy is the same as discussed above, except for the fact that at the MNE level, we no longer require additional controls for potential

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[23]While we do not separately observe the number of hours worked per month, we take within-worker changes in the earnings per month of work as a meaningful measure of changes in compensation. We also exclude workers for whom we know the employment was not full-time during a given month.

mechanical effects among exposed suppliers (for having sold to any MNE at year $\eta = -1$). Given the bilateral nature of the transaction data, we include both suppliers' (i) sector-by-year and MNE (j) sector-by-year fixed effects.

The transaction-level version of specification 42 thus estimates the timeline of the effects of RS rollouts by MNEs j on the average transaction sales amount among their continuing suppliers, after conditioning on supplier-by-MNE fixed effects (α_{ij}). As above, we report estimation results across the standard two-way fixed effects estimator, using the IV (global rollouts) and using the recent estimation method by Sun and Abraham (2020).

4.2 Estimation Results

Tables 4, 5 and 6 and Figures 1, 2 and 3 present the event-study estimation results at the supplier, worker and transaction level respectively.

Supplier Level Panels A and B of Table 4 report results on log supplier total sales and log total employment. For each panel, column 1 presents the two-way fixed effect specification with firm and sector-by-year fixed effects. Column 2 adds the additional controls for having sold to any MNE at event period $\eta = -1$ as discussed above. Column 3 presents the same specification as in column 2, but estimated using the procedure by Sun and Abraham (2020). Column 4 presents the same specification as in column 2, but after instrumenting for the treatment event dummies using only RS rollouts that were global in nature. Panels A and B of Figure 1 then graph the point estimates of the IV specification in column 4 for both the effect on firm sales and employment.

Several findings emerge. Log total sales of exposed suppliers decrease by, on average, about 8 percent 4 years after the RS rollout. This is accompanied by a close to 1:1 decline in total firm employment (also about -8 percent). In both cases, the concern of mechanical positive pre-trends and negative post-trends is apparent in column 1, in which we do not control for having sold to any MNE at event year $\eta = -1$. After we condition for the full treatment event timeline of having sold to any MNE at $\eta = -1$ (including leads and lags of that parallel event in the regression), pre-trends disappear as we see in the figures and table columns 2-4.

The estimation method by Sun and Abraham (2020) in column 3 produces very similar point estimates of the event study compared to the two-way fixed effects estimation in column 2, suggesting that heterogeneous treatment effects across different cohorts do not appear to lead to significant bias in our setting. After we instrument for RS treatment events using only global rollouts–that account for about 85% of all RS events in CR over this period in our sample–we find again very similar point estimates relative to the baseline two-way fixed effects specification in column 2. This provides further reassurance that in our setting, RS rollouts by MNEs were unlikely targeted at contemporaneous shocks to their suppliers in CR.

Worker Level Panels A and B of Table 5 and Panels A and B of Figure 2 present the worker-level results from specification 42 above. Panel A presents the results including all workers who have

at some point worked at a supplier to an MNE during the sample. Panel B restricts attention to those workers in the bottom 20 percent of the monthly earnings distribution (where each worker's monthly earnings are assessed in the first year when we observe her in the data (starting in 2006), and are compared to the (inflation-adjusted) monthly earnings of other workers in their first year in the data).

As above, Figure 2 plots the event-study coefficients from the IV specification in column 4 in Panels A and B of Table 5. As depicted in the figure, we find that on average the earnings of all workers increase post-RS rollout, by about 1.3 percent 4 years after the rollout. This average effect is more pronounced for initially low-wage workers in Panel B, who see their monthly earnings increase by on average 6 percent 4 years after the rollout. As discussed for specification 42 above, all regressions include worker-by-firm as well as sector-by-year fixed effects, and as above we control for the potentially confounding effect of having sold to any MNE at event year $\eta = -1$. Also as above, the estimation based on Sun and Abraham (2020) in column 3 confirms both the OLS and IV point estimates.

Transaction Level Table 6 and Figure 3 present the transaction-level results from specification 42 above. Figure 3 plots the event-study coefficients from the IV specification in Table 6. As depicted in the figure, we find that intensive margin sales of RS-compliant exposed firms to the RS-MNE decline by about 6.5% four years post-rollout. As discussed for specification 42 above, all regressions include buyer-by-seller as well as MNE sector-by-year fixed effects. Also as above, the estimation based on Sun and Abraham (2020) in column 2 confirms both the OLS and IV point estimates.

5   Model Estimation

The evidence in the previous section suggests i) that our empirical design using RS rollouts in CR over the period 2009-2017 provides a valid natural experiment to study the effects of RS in a sourcing origin country, and ii) that RS rollouts in CR have led to a significant decline in the total sales and employment of exposed firms, accompanied by an increase in the monthly earnings of workers at those firms. Using the transaction-level data, we also find that the intensive margin of sales to the RS-MNE have been negatively affected among RS-compliant exposed firms.

5.1   Model Selection

Interpreted through the lens of the model and the comparative statics in Section 2.4, these results suggest that RS in CR has been implemented in a setting where MNEs do not roll out RS as a function of short or medium-run positive effects on their output demand (ruling out A'), and where RS is unlikely targeted to address significant pre-existing labor market monopsony power (wage markdowns) by CR suppliers (ruling out B').

Guided by this initial evidence, we now proceed to estimate the most general version of the model in Section 2 that is supported by this evidence (that is, allowing for potential productivity gains in Hypotheses C vs. C' and imperfect pass-through in Hypotheses D vs. D'). These estimations then allow us to proceed to the counterfactual analysis of the welfare incidence of RS in CR in Section 6 below.

5.2   Parameter Estimation

We require estimates of the size of the RS-induced cost increase ($\hat{\tau}$), the size of any contemporaneous increases in labor productivity ($\hat{T}$), the elasticity of substitution that firms face in their output demand (σ), the shape parameter of the CR firm productivity distribution (θ), the extent to which intermediate costs are passed through to the MNE (β), and the elasticity of substitution in production between worker types (ρ). Tables 7 and 8 present the moments used for parameter estimation (discussed in detail below), and the parameter estimates respectively.[24]

Using Matched Employer-Employee Data to Estimate $\hat{T}$

Following expression 35 in Section 2, we can use the change after the RS rollout in the earnings of initially high-wage workers at exposed firms compared to those at otherwise identical MNE suppliers to estimate the accompanying direct productivity effect of new supplier codes of conduct of MNEs.

$$\hat{\hat{w}}^{h,RS} - \hat{\hat{w}}^{h,N} = \hat{T}$$
$$\text{and } \hat{\hat{w}}^{l,RS} - \hat{\hat{w}}^{l,N} = \hat{\tau} + \hat{T}$$

Using this insight, we estimate specifications similar to 42 above, but only for workers who were in the top one-third of all monthly worker earnings (again defined for each worker when observed for the first time in our database). Since these employees are at the upper end of the earnings distribution and predominantly work in white-collar office and management positions, it would seem natural to think of this group as unlikely to be affected by RS requirements.

For parameter estimation, we are interested in the average effect of RS rollouts among suppliers, after allowing for the adjustment period that they experience just after the rollout (as revealed in the event-study graphs of the previous section). To this end, we estimate the same event-study specification in 42 using the IV specification that we plot in the figures above, but now with a single event time dummy for $\eta \geq 4$ instead of including each event time dummy separately (for e.g., $\eta = 5$, $\eta = 6$, etc.). All else (including fixed effects, controls, and event time dummies for $\eta < 4$) remains the same. Table 8 then reports the point estimate for $\eta \geq 4$ from this specification.

To compare the effect on high-wage workers to that on low-wage workers (for whom RS is

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[24]Note that our parameter estimates and the welfare quantification in the next section do not currently have standard errors. In future versions of this draft, we plan to bootstrap the entire estimation procedure (drawing random samples with replacement hundreds of times starting with the regressions underlying Table 7).

likely binding in this setting), Panel B of event-study Figure 2 above reports a gain in earnings of about 6 percent. The first column of Table 7 presents the pooled post-rollout effect on worker earnings for workers in the top one-third. We find that high-wage workers employed at exposed suppliers to MNEs experienced a 1.4 percent increase in their monthly earnings compared to their peers at non-exposed suppliers to MNEs. In our baseline model calibration, we will use the point estimate of $\hat{T} = 0.014$ in Table 8. Moreover, we discuss the estimation of $\hat{\tau}$ and its relationship to the observed earnings increase among low-wage workers just below.

Using Sales Effects to Estimate $\hat{\tau}$, θ and β

Armed with an estimate of $\hat{T}$, we proceed to estimate the size of the RS cost shock $\hat{\tau}$ in addition to θ and β. In principle, there are two natural approaches to this. The first one follows the second comparative static on earnings just above, we could use the observed effect on low-wage worker earnings (i.e., roughly 6% in Panel B of Figure 2) to estimate $\hat{\tau}$ (i.e., $0.06 - \hat{T} = 0.046$), then use the estimated $\hat{T}$ and $\hat{\tau}$ to pin down σ, θ and β using the comparative statics on total supplier sales (equation (40)), domestic sales (equation (39)) and intensive-margin sales to the RS-MNE (equation (36)). Alternatively, the second approach is to use an existing estimate of the elasticity of substitution faced by firms in CR (e.g., from Alfaro-Ureña et al., 2020, who estimate σ in a way consistent with our model and using the same firm-level microdata), and then use knowledge of σ and $\hat{T}$ from above in combination with the three comparative statics on sales above to pin down the unknown cost shock $\hat{\tau}$ as well as θ and β.

In our preferred approach, we use the latter option above because –while there are already many studies estimating σ using firm-level panel data in similar contexts (including in CR)– we are unaware of any empirical estimate of the magnitude of the unobserved shock brought about by RS rollouts on suppliers. As we discuss in Section 3, supplier codes of conduct impose a number of different requirements that often include mandatory wage floors (that we can observe in the earnings data) but also other worker benefits, such as paid leave, health benefits, etc. (which we would not be able to observe from worker compensation). In addition, it could be that our estimation using the employer-employee microdata fails to capture other parts of the costs imposed on suppliers, as all workers are formal in this data. To the extent that we are missing costly switches from informal to formal contracts for suppliers (which would not be reflected in our wage regressions with worker-by-firm fixed effects), we would be under-estimating the true cost increase from employing initially low-wage workers (who could have formerly had informal work arrangements).

In this context, we judge the assumption that the full cost shock must be pinned down from the earnings effect (estimated among low-wage workers in the official data) to be somewhat stronger than the assumption that we have a credible estimate of the elasticity of demand that firms in CR are facing on average. Furthermore, the $\sigma = 5.03$ that Alfaro-Ureña et al. (2020) estimate from the firm-level microdata falls squarely at the center of a large number of existing estimates from similar empirical settings to CR (see, e.g., Hottman et al., 2016, for a discussion of

existing estimates). And of course, we can always cross-check the magnitude of the implied $\hat{\tau}$ with that obtained from only using worker-level data on nominal compensation (i.e., $0.06 - \hat{T} = 0.046$).

To proceed, we use the three comparative static equations discussed above (36, 39 and 40) in combination with knowledge of $\hat{T} = 0.014$ and $\sigma = 5.03$ in order to estimate $\hat{\tau}$, θ and β.[25] The empirical estimates that solve this system of three equations in three unknowns are presented in columns 2-4 of Table 7. Similar to the estimation of the earnings effect from column 1, we continue to estimate the same specification used in the event-study graphs from the previous section, but pool the post-rollout effects at $\eta \geq 4$. To compare the effect on sales to RS-MNE buyers to the effect on sales to domestic (non-MNE) buyers (comparison which pins down the degree of buyer market power by the MNE), we use the estimated coefficients of the effect of the RS rollouts on sales to domestic buyers (column 3) and sales to RS-MNE buyers (column 4).

The estimated fall of 11.4% in total supplier sales from column 2 (obtained from pooling post-rollout periods for $\eta \geq 4$) is slightly stronger than the estimated fall of 8% displayed in Figure 1 (obtained for $\eta = 4$). For the effect on sales to the RS-MNE and domestic buyers, we find point estimates of close to identical size (-8.6% and -9% respectively), suggesting that cost pass-through does not appear to significantly differ between MNE buyers vs. domestic buyers. As can be seen above, the ratio of those two effects ($-8.6/-9 = 0.96$) pins down the differential pass-through parameter β in the model. In turn, the effects on total firm sales and the relative sales to domestic buyers vs. RS-MNE buyers jointly pin down the shape parameter of the productivity distribution $\theta = 7.5$ and the implied cost shock for low-wage workers $\hat{\tau} = 0.149$. Comparing the $\hat{\tau} = 0.149$ estimate to the estimate of the increase in monthly earnings of low-wage workers (6%, see Panel B of Figure 2) suggests that, indeed, not all of the RS-induced increases in labor costs at suppliers may be captured directly by workers (or at least not in their nominal earnings). All these parameter estimates are reported in Table 8.

Using the Effect on Relative Employment to Estimate ρ

The final parameter in Table 8 is the elasticity of substitution between low- and high-wage workers, that we estimate from the RS effect on the relative employment of high- vs. low-wage workers. Formally, the differential impact of the policy on high- vs. low-wage employment at exposed firms compared to non-exposed firms is:

$$d\log \frac{\ell_{xRS}^l + \ell_{HRS}^l}{\ell_{xRS}^h + \ell_{HRS}^h} - d\log \frac{\ell_{xN}^l + \ell_{HN}^l}{\ell_{xN}^h + \ell_{HN}^h} = -\rho\hat{\tau},$$

To estimate ρ, we again group workers into earnings groups that are likely to be affected by RS rollouts on working conditions (i.e., workers below the 20th percentile of all worker monthly

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[25]We also require to calibrate the cost-share of low-wage workers (who we define as the bottom 20% of workers in terms of their monthly earnings, at the first time we observe each worker relative to to the (inflation-adjusted) first monthly earnings of other workers in the data), namely $\chi^l = 0.15$. To estimate 40, we also require to calibrate the average sales share to the RS-MNE by exposed firms (ξ). At event period $\eta = -1$, this average is about 0.25 across the whole sample.

earnings, based on the earnings of the worker when first observed in our data) and those that are unlikely to be affected by such RS rollouts (i.e., workers in the top third of monthly earnings, again based on their first-time appearance in the data). We then estimate the supplier-level specification in 41 with the log ratio of low- over high-wage worker employment on the left-hand side, again using the same IV specification as shown in the event-study graphs from the previous section, but pooling the post-rollout effects at $\eta \geq 4$ as above. Column 5 in Table 7 shows that this ratio fell by -10.05%. Column 6 in Table 8 reports the corresponding value of $\rho = 0.71$ (obtained after combining the -0.105 point estimate of the change in the ratio of low- to high-wage worker employment with our estimate of $\hat{\tau} = 0.149$).

6   Welfare Implications and Counterfactual Analysis

In this final section, we present the counterfactual analysis of the welfare incidence of RS in CR. We first derive welfare expressions using the most general version of the model in Section 2 for which we find empirical support in Section 4. We then use the parameter estimates from the previous section in combination with a number of additional moments in the CR data to proceed to the quantification. In the final part, we present additional counterfactual results to conduct sensitivity analysis across alternative assumptions and parameter values and to assess how the incidence of RS may change in different empirical contexts.

6.1   Welfare Implications of RS

In this section, we derive the GE implications of the policy on the welfare of Home workers, and the corresponding distributional effects, between high- and low-wage workers, under the maintained Hypotheses A, B, C' and D'. For ease of exposition, the expressions we derive here compute welfare changes following infinitesimal RS policies adopted by all MNEs with a subsidiary in Home. Throughout, we use hat notations $\hat{x} = d \log x$ to denote log changes in variable x following an RS policy $(\hat{\tau}, \hat{T})$. For exposition, we also report the welfare effects of RS in CR in the case where the Melitz-type selection channel is shut off, but firm heterogeneity is kept. As shown by Burstein and Vogel (2017), this can be done by taking the limit $\theta \to \sigma - 1$. The general case, where $\theta > \sigma - 1$, is detailed in the Appendix. All qualitative results and the economic intuition described below carry through to this more complex case.

For type $t = l, h$, we have

$$U_H^t = \frac{1}{L_H^t} \frac{X_H^t}{P_H}$$

where X_H^t is the total expenditure of type t workers and P_H is the ideal price index, common to both types, derived from utility (1). We first analyze the average utilitarian welfare impact of the

RS policy on all Home workers, $U_H = \sum_{t=l,h} \frac{L_H^t}{L_H} U_H^t$, for which:

$$\hat{U}_H = \sum_{t=l,h} \frac{X_H^t}{X_H} \hat{U}_H^t = \hat{X}_H - \hat{P}_H,$$

where $X_H = \sum_{t=l,h} X_H^t$ is the total expenditure of country H. We then move on to examining the distributional impact on the policy $\left(\hat{U}_H^l, \hat{U}_H^h\right)$ on low- vs. high-wage workers.

Average Welfare

To report the results, we first introduce some notations. The Appendix provides more detailed steps in the derivation of the expressions below. Let $\lambda_{kk'}$ denote trade shares as is standard in the literature in international trade (with λ_{kk} denoting the share of trade with country k itself).[26] Second, let Λ denote the share of total expenditure on the Home-produced goods that is spent on goods produced by RS-compliant firms.[27] Λ thus measures the degree of "leakage" of RS policies into the domestic price index. And as before, the cost share of low-wage workers is χ^l. Then, the welfare impact of RS policies by Foreign MNEs at Home can be expressed as:

$$\hat{U}_H = (\beta - \Lambda) W^{tax} \chi^l \hat{\tau} + (\lambda_{FH} + \Lambda \lambda_{HH}) W^{prod} \hat{T}, \tag{43}$$

where

$$W^{tax} = \frac{\sigma \lambda_{HH} \lambda_{FH}}{1 + (\sigma - 1)(\lambda_{FF} + \lambda_{HH})} \geq 0$$

$$\text{and } W^{prod} = \frac{(\sigma - 1)\lambda_{FF} + \sigma \lambda_{HH}}{1 + (\sigma - 1)(\lambda_{FF} + \lambda_{HH})} \geq 0.$$

The first term in (43) captures the welfare impact of the change in the terms-of-trade of Home vs. Foreign due to the RS requirements by MNEs on Home production. To see this, note that the term $W^{tax} \hat{\tau}$ above is equal to the welfare effect at Home of an *export tax* of $\hat{\tau}$ imposed on all Home exports (since all exports in our model are done by MNEs). In a world where the RS policy is fully passed through to the Foreign MNE ($\beta = 1$) and does not impact domestic production

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[26]Specifically, we define:

$$\lambda_{HH} = \frac{\int_{\Omega_{HH}} \left(\frac{\sigma}{\sigma-1} \frac{W_H}{z}\right)^{1-\sigma} dG_H(z)}{\int_{\Omega_{HH}} \left(\frac{\sigma}{\sigma-1} \frac{W_H}{z}\right)^{1-\sigma} dG_H(z) + \int_{\Omega_{FH}} \left(\frac{\sigma}{\sigma-1} \frac{\varsigma W_F}{z}\right)^{1-\sigma} dG_F(z)} \ ; \ \lambda_{FH} = 1 - \lambda_{HH},$$

and

$$\lambda_{FF} = \frac{\int_{\Omega_{FF}} \left(\frac{\sigma}{\sigma-1} \frac{W_F}{z}\right)^{1-\sigma} dG_F(z)}{\int_{\Omega_{FF}} \left(\frac{\sigma}{\sigma-1} \frac{W_F}{z}\right)^{1-\sigma} dG_F(z) + N^M \left(\frac{\sigma}{\sigma-1} R_x\right)^{1-\sigma}}.$$

where $\Omega_{kk'}$ is the set of varieties produced in k and marketed in k'.

[27]Formally, $\Lambda = \frac{\int_{\Omega_{HRS}} \left(\frac{\sigma}{\sigma-1} \frac{W_H}{z}\right)^{1-\sigma} dG_H(z)}{\int_{\Omega_{HRS}} \left(\frac{\sigma}{\sigma-1} \frac{W_H}{z}\right)^{1-\sigma} dG_H(z) + \int_{\Omega_{HN}} \left(\frac{\sigma}{\sigma-1} \frac{W_H}{z}\right)^{1-\sigma} dG_H(z)}$, where Ω_{HRS} denotes the final varieties produced by firms impacted by RS and Ω_{HN} those produced by firms not impacted by RS.

at all ($\Lambda = 0$), the welfare impact of the net increase in the labor cost of Home suppliers to the MNE is simply $W^{tax}\hat{\tau}$, scaled by χ^l (as the policy does not impact all workers but only a fraction χ^l of labor costs). That is, the RS policy is akin to an export tax, and leads to unambiguous welfare gains through classic terms-of-trade effects. In this case, from the point of view of Home, the RS policy increases the price of exported goods compared to imported consumption, leading to higher welfare.[28]

This positive effect can then be mitigated or even reversed, however, by two forces. First, the lower the pass-through of the policy β into input prices paid by the MNE, the lower the welfare gains, as a low β effectively directly dampens the size of the export tax paid on exports done by the MNE subsidiary. Second, the higher the leakage of the policy to the domestic market, as captured by Λ, the lower the benefits of the policy: the export tax effect is now coupled with what is akin to a distortive production tax on domestic production. The price of consumption for Home workers increases as a result, dampening the possibly positive welfare effect of the export tax. Overall, at the limit where $\beta = 0$, the welfare effect of the increase in net labor costs that the RS policy entails leads to an unambiguously negative welfare effect for Home workers, for any $\Lambda > 0$: the policy is then only distortive. Similarly, when the policy "leaks" to all domestic production ($\Lambda = 1$), the welfare effect of the policy is unambiguously negative, for any $\beta > 0$, as the policy becomes a distortive production tax on domestic production. The formula reveals a knife-edge case, when $\beta = \Lambda$. At this point, an RS policy is exactly welfare neutral for Home workers.

The second term in (43) captures the effect of an increase in labor productivity due to RS. In particular, the term $W^{prod}\hat{T}$ captures what would be the welfare effect of a productivity increase of $\hat{T}$ for all workers at Home. As the RS policy only applies to a fraction of workers (those at RS-compliant producers), the welfare effect of a productivity increase is scaled by the fraction $\lambda_{FH} + \Lambda\lambda_{HH}$.

Distributional Implications

The model also allows us to zoom in on the heterogeneous effects of the policy on high- versus low-wage workers. After some algebra, the change in welfare of high- and low-wage workers can be written as follows:

$$\hat{U}_H^l = (1-\Lambda)\,\lambda_{FH}\left(\frac{\sigma\lambda_{HH}}{1+(\sigma-1)\left[\lambda_{FF}+\lambda_{HH}\right]}\frac{\beta-\Lambda}{1-\Lambda} + \frac{1-\chi^l}{\chi^l}\right)\chi^l\hat{\tau} + \left(\lambda_{FH}+\lambda_{HH}\Lambda\right)W^{prod}\hat{T}$$

$$\hat{U}_H^h = (1-\Lambda)\,\lambda_{FH}\left(\frac{\sigma\lambda_{HH}}{1+(\sigma-1)\left(\lambda_{FF}+\lambda_{HH}\right)}\frac{\beta-\Lambda}{1-\Lambda} - 1\right)\chi^l\hat{\tau} + \left(\lambda_{FH}+\lambda_{HH}\Lambda\right)W^{prod}\hat{T},$$

where the first term on the left in both expressions is due to the export tax channel and the second term due to changes in labor productivity for all workers at RS-compliant producers (which is

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[28]This result follows because the policy impacts the terms-of-trade–i.e., Home is modeled as a large economy. If instead, Home was modeled as a small open economy in a Melitz framework–i.e., Foreign demand was modeled as $Ap^{-\sigma}$ with A exogenous and unchanged by the export tax–then an export tax would still be welfare increasing, as shown by Demidova and Rodríguez-Clare (2009) and recently discussed in Caliendo and Parro (2021). We further discuss and compare our results to existing insights about optimal tariffs as part of the counterfactual analysis below.

identical between the two types). To further simplify, these expressions can be written as follows:

$$\hat{U}_H^l = \hat{U}_H + (1 - \Lambda)\left(1 - \chi^l\right)\lambda_{FH}\hat{\tau} \tag{44}$$

$$\hat{U}_H^h = \hat{U}_H - (1 - \Lambda)\chi^l\lambda_{FH}\hat{\tau}, \tag{45}$$

so that

$$\hat{U}_H^l - \hat{U}_H^h = (1 - \Lambda)\lambda_{FH}\hat{\tau} > 0.$$

The policy is unambiguously progressive: it increases the relative welfare of low-wage workers, directly targeted by RS requirements, compared to high-wage workers for whom the policy is not binding. A leakage of the policy to the domestic market Λ away from a pure export tax mutes this distributional impact. Furthermore, it is easy to see that, absent a productivity effect ($\hat{T} = 0$), $\hat{U}_H^h < 0$ for any value of the pass-through β or the leakage Λ. That is, the labor-cost-increase (export tax) channel of the policy is unambiguously welfare-reducing for high-wage workers. They bear the cost of the policy in terms of the allocative distortion, but do not reap its benefit as they are not targeted by the wage subsidy. Low-wage workers, who are directly targeted by the policy, see a welfare increase for a high β and low Λ, but a welfare decrease for a low β and high Λ, for reasons similar to the ones laid out when discussing the average effects of the policy. The welfare impact on high-wage workers can still be positive, on net, to the extent that the increase in labor productivity outweighs the negative impact of the pure export tax channel.

6.2 Counterfactual Analysis

Perimeter of the Policy Analysis

We aim to quantify the welfare incidence of moving from an initial equilibrium without RS in CR to one in which the average amount of RS policies we observe in CR over the sample period 2009-2017 had been implemented. The extent of RS policies we observe in the data are then subject to the cost and productivity changes, $\hat{\tau}$ and $\hat{T}$, that we estimate in Section 5 above.

Before proceeding, some comments are in order. First, note that the welfare expressions derived above are exact only for small cost shocks and correspond to a first-order approximation for the actually observed shocks we estimate. In particular, as the size of the policy $\hat{\tau}$ increases, the initially positive effect of the "export tax" plateaus and then decreases, as shown in the existing literature on optimal tariffs (see, e.g., discussions in Costinot and Rodríguez-Clare (2014) and Caliendo and Parro (2021). To this end, we can verify that the policy we consider is small enough to be in the increasing portion of the welfare curve. In fact, we can solve for the optimal export tax policy $\hat{\tau}$ in the context of our model, and find: $\hat{\tau}^* = (1/\lambda_{FF}) * (1/(\sigma - 1))$. Quantitatively, this is about 0.25, and note that this optimal tariff would still have to be divided by the cost share of low-wage workers ($\chi^l = 0.15$) in our setting. So we can be reassured that the export tax effect we quantify falls well within the bounds of a welfare-improving trade policy.

Second, our model (and thus welfare expressions) are stylized for expositional purposes on

several fronts that may matter for the quantification: (i) the trade patterns in the model are simplistic: all of CR's exports are done by MNEs and all of MNE production is assumed to be exported. These simplifications matter both for how we measure the amount of leakage, Λ, and the Home country's trade share, λ_{HH}; (ii) the model assumes that MNEs only use intermediate inputs without employing domestic labor. Hence, any RS rollouts that may also affect low-wage workers at MNE production facilities are ignored; and (iii) our model only has one identical MNE sector that either fully implements RS or not. However, in our data, only a fraction of MNE production is subject to active RS policies. (i), (ii) and (iii) above have implications for how we measure the extent of RS and its incidence when taking the welfare expressions in (38) and the distributional consequences to the data. We discuss these below.

Additional Data Moments

In addition to parameters we estimate in the previous section, the welfare expressions in (43) above require three moments observed in the data: the share of output sold on the domestic market that is affected by RS in CR (Λ), the country's share of trade with itself ($\lambda_{HH} = 1 - \lambda_{FH}$) and the share of Foreign demand that is not spent on the relevant RS-MNE products from CR (λ_{FF} in the model).

For the first moment (Λ), our model would suggest summing all non-MNE sales by all non-MNE firms in CR in the denominator, and summing all non-MNE sales by non-MNE firms that are affected by RS in the numerator. We plot this ratio for each year of our data in the second column of Table 9. To measure which CR firm is subject to RS requirements, we only count firms that we observe selling to an MNE with an active RS policy in place in any given year (not counting firm-by-year observations without active sales relationships). For context, the first column of Table 9 also shows the ratio of total production (not only domestic sales) by non-MNE firms in CR that are actively selling to RS-MNEs relative to total non-MNE domestic production.

However, the model abstracts from both MNEs selling on the domestic market and from domestic firms directly exporting part of their sales. To address this, we make additional use of the customs microdata that we are able to match to our firm identifiers and compute the degree of leakage Λ as follows: in the numerator, we sum the non-MNE and non-export sales of all firms (CR or MNEs) that are subject to RS requirements in a given year. In the denominator, we sum the non-MNE and non-export sales of all firms to the domestic market.

Column 3 in Table 9 reports this last ratio for each year of data between 2009-2017. Consistent with growth in the adoption of RS by MNEs over this period, we find that an increasing fraction of output for the domestic market has been subject to "leakage" from MNE RS policies. This fraction increases from 21% in 2009 to 40% by the end of our sample in 2017. For the model calibration, we will take the average degree of leakage over this period, $\Lambda = 0.32$.

For λ_{HH} it is important to keep in mind the model's stylized nature: because of (i), (ii) and (iii) above (in addition to the trade balance) the share of CR's total trade in goods and services with itself is also equal to the share of RS-affected exports over domestic production. In reality,

this does not hold true, of course, because not all exports are done by MNEs and not all MNEs implement RS at the same time. But for the welfare quantification in (43), it is the latter (the share of RS-affected exports over domestic production) that we need to measure. The most accurate way of doing so is to link the customs microdata in CR to the firm identifiers in our database, which we are able to do. These data allow us to estimate the share of all CR exports that are coming from RS-active CR firms or RS-active MNE affiliates in CR.

Column 4 of Table 9 shows the share of active RS MNEs over total exports from all MNE affiliates in CR. And column 5 shows the share of all CR exports of all RS-active firms (both firms actively selling to an RS-MNE and RS-MNEs exporting). The average share in column 5 over the sample period is 0.4 of all CR exports. We can use this knowledge to adjust the CR share of trade with itself. The CR Central Bank estimates the overall λ_{HH} (counting all trade flows) to be on average about 0.7 over this period. The challenge for us is that those 30% of foreign trade are not all subject to RS (so that the "true" λ_{HH} in our welfare analysis should be larger). To adjust this, we use the average of column 5 above, suggesting that 40% of CR exports are driven by firms subject to active RS policies (i.e., 12% of the overall 30%). With that, we can adjust the model-consistent expression for $\lambda_{HH} = 0.88$, which we use in our baseline calibration.

For both Λ and λ_{HH} above, we also report additional results in the final section across the full range of these fractions (both to test sensitivity and to provide insights on how results may differ across other empirical contexts). Finally, for $\lambda_{HF} = 1 - \lambda_{FF}$ (the share of Foreign expenditure on RS-affected CR exports), we use a value very close to zero (at 0.00001).

The Welfare Incidence of RS in CR

We proceed to quantify the welfare implications in a counterfactual that compares a no-RS equilibrium to one in which the average observed amount of RS was implemented in CR over the period 2009-2017 (as reflected in the data moments discussed in the previous subsection). Those RS requirements have the same features ($\hat{\tau}$, $\hat{T}$, etc.) as those estimated in the data in Section 5 and shown in Table 8.

Figure 4 presents the estimation of expression (43) above and its break-up by different types of workers. Panel A presents the percentage change in welfare in the aggregate ("average welfare" in the model expressions above). We also plot two additional curves that provide the break up of the total welfare effect into its two components discussed above: the effect due to the export tax channel and the effect due to RS-induced changes in labor productivity. On the x-axis, we plot different scenarios for the degree of RS leakage into the domestic economy, Λ, with the red dots indicating the case of CR over the sample period. Panels B and C then present the corresponding plots for low- and high-wage workers, respectively.

Several insights emerge. First, we estimate the welfare incidence of RS in CR to be net positive for both types of workers and in the aggregate. Second, the gains are largest among the affected low-wage category: we estimate that the bottom 20% of workers in the initial earnings distribution have gained on average 1.3% in real incomes due to RS policies on the ground in CR over this

period. The majority of this effect stems from the export tax channel (the fact that their factor's compensation in production has increased among RS-compliant firms), at roughly 1.1%. The remainder stems from the increase in overall labor productivity, $\hat{T}$ at exposed firms. While the export tax channel is a decreasing function of the leakage term on the x-axis, the productivity effect is increasing. This makes sense from expression (43) above, as the leakage term reduces the benefit of the export tax (increasing domestic prices, thus lowering the terms of trade effect) whereas the productivity effect is increasing in the leakage (as more domestic output benefits from direct labor productivity improvements).

For high-wage workers, we find much less pronounced effects: their welfare increased by an estimated 0.13%. Notice that for non-targeted high-wage workers, the export tax effect is negative, until it hits zero at the limit when all of domestic production is affected by RS (eliminating terms of trade effects). As is intuitive, for high-wage workers the dominant effect is the increase in overall labor productivity that we find to accompany RS rollouts in CR.

The aggregate economy-wide welfare implications $\hat{U}_H$, as given in (43), are a weighted average of the implications above with the weights proportional to the cost shares of the two groups of workers. Since the bottom 20% of workers account for an estimated 15% of the labor cost share, the aggregate effect is more muted than the effects on the affected low-wage workers. We find that on average $\hat{U}_H = 0.3\%$. Similar to the case of high-wage workers, welfare gains in the aggregate increase with the extent of domestic leakage, Λ, whereas the opposite was the case for the low-wage workers. The reason is that while Λ dampens the positive effect of the export tax, it also amplifies the benefits from productivity gains due to RS. And we find that in the aggregate, the latter effect dominates the former in the CR empirical context.

6.3   Additional Results

Here, we assess the sensitivity of our baseline counterfactuals above across a number of alternative considerations. These results also serve to assess how the impacts of RS may differ across alternative empirical contexts.

Role of RS Exports   Figure 5 fixes the observed CR value for $\Lambda = 0.32$ and plots the welfare incidence and its two components as a function of the (inverse of the) share of exports subject to RS over total production. The red dots indicate our estimate for CR ($\lambda_{HH} = 0.88$).

Sensitivity Across Alternative σs   Figure 6 plots the welfare incidence and its two components as a function of the elasticity of substitution in demand faced by CR firms on their market, $2.5 \leq \sigma \leq 7.5$, with our estimate $\sigma = 5.03$ marked by the red dots. The figure includes three panels again for low- and high-wage workers and the aggregate economy. For this sensitivity analysis, we re-estimate the full set of parameters from Section 5 above before proceeding to the welfare quantification in Section 6.1 for each of the different parameter assumptions about σ, in steps of 0.1 between $\sigma = 2.5$ and $\sigma = 7.5$. As we can see, the effects of the export tax channel are decreasing in values of sigma. The reason for this is that we are using knowledge of the demand

elasticity in order to back out the unobserved cost shock to firms due to RS. Assuming lower values of σ, given the observed effects on supplier sales, thus implies a larger underlying cost shock $\hat{\tau}$ (and terms of trade effect) ceteris paribus.

Implications of MNE Buyer Power   Figure 7 again fixes the observed CR value for Λ and plots the welfare incidence and its two components as a function of MNE buyer market power that we summarize in $0 \leq \beta \leq 1$, with our estimate $\beta = 0.96$ marked by the red dots. We do this again across all three panels (low-wage, high-wage and aggregate economy). In all cases, a higher cost pass-through to the MNE yields better welfare results for the origin country, for both types of workers. β only affects the export tax channel, implying that a higher pass-through improves the terms of trade effects due to RS.

Aggregate Implications as a Function of Share of Workers Affected by RS   Figure 8 plots the welfare incidence and its two components as a function of the share of the workforce (in terms of total labor costs), with our estimate for CR ($\chi^l = 0.15$) marked by the red dots.

What if Low-Wage Workers Only Capture Fraction of Estimated $\hat{\tau}$?   Figure 9 plots the welfare incidence and its two components as a function of the fraction of $\hat{\tau}$ that is captured by low-wage workers' real wages. The model's baseline assumption is that all RS-induced cost increases are captured in the real compensation of initially low-wage workers (including nominal earnings, but also potentially including benefits, sick leave, etc.).

Sensitivity to Longer-Term Effects on MNE Demand/Profitability   [Work in progress. We re-estimate the parameters and welfare counterfactuals with model expressions under Hypothesis A' instead of A. We then plot the welfare incidence across alternative assumptions about the longer-term effect on MNE output demand due to the RS policy (that we may miss in our empirical context).]

Sensitivity to Allowing MNEs to Face a Different σ   [Work in progress. We derive an alternative system of comparative static expressions for the parameter estimation, in which we allow the demand elasticity faced by MNEs to differ from that faced by domestic firms. We then quantify the new counterfactual welfare expressions for the welfare incidence in this case across a range of alternative parameter combinations (with either larger or smaller elasticities faced by MNEs)].

Checking if Domestic Firms Split Up into Separate Entities Post-RS   [Work in progress. We leverage the employer-employee microdata to check if RS leads to i) disappearing firm entities among exposed firms, and/or ii) entry of new firm entities that employ a disproportionate share of workers who used to be listed under the initial identifier of the exposed firms. This is to investigate the possibility that suppliers may create new separate firm entities (aimed at splitting production for the MNE vs. other buyers) in order to partially evade compliance.]

7   Conclusion

Despite widespread growth in the adoption of RS policies by MNEs vis-a-vis their global suppliers, there has been relatively little theoretical work or empirical evidence on the economic consequences in sourcing origin markets. We first develop a theory to study the incidence of RS requirements that nests several alternative assumptions about the motivation behind RS by the MNE and the economic environment in which RS is being implemented. We then build a unique database in the context of CR and confront the model predictions with the data to document some initial evidence that helps us discriminate between alternative hypotheses. In the final step, we use the theory to derive counterfactual expressions of the welfare incidence in general equilibrium, and use the data to calibrate the model for counterfactual analysis.

We document several insights. In the theory, we show that the welfare effect of RS in origin countries is a priori ambiguous. We show that RS can lead to adverse consequences in environments where the cost pass-through to MNE buyers is incomplete and where affected suppliers also produce a significant share of output destined for domestic consumers. Underlying this, we document the interplay of an export tax effect due to RS (that increases domestic welfare due to a classical terms-of-trade effect) and a labor market distortion that arises as the RS requirements "leak" into domestic production (and the domestic price index). Additional gains arise to the extent that RS is on average accompanied by direct effects on the labor productivity of suppliers, due to e.g., parallel re-organizations of the supply chain, including transfers of technology or expertise.

Empirically, we find that RS is not just "hot air": sales and employment of exposed suppliers decline in the years post-RS rollout, and the monthly earnings of workers increase, especially so among the initially low-wage workers. On its own, the reduced-form evidence would, however, be insufficient to evaluate the welfare consequences of RS in origin countries. To this end, we interpret the evidence through the lens of the model, estimate several key parameters and use the richness of the data to compute additional important moments for the model's calibration. We find that RS has on average led to an increase in labor productivity among all workers of about 1.4%, that the effective cost increase for low-wage workers is about 15% and that the cost pass-through to MNEs does not appear to be any different than the pass-through to domestic buyers. We also document that the share of RS-active firms in the production of goods and services for the domestic market ("leakage") is on average about one third over this period in CR (and 40% at the end of our sample in 2017), and that the share of total CR exports made by RS-active firms (both CR firms and MNE affiliates) is about 40% on average. Using these estimates, we then quantify the welfare incidence of RS in CR over the period 2009-2017. We find that on net RS has led to welfare gains in CR that are in the order of about 0.3% of real income. These gains are concentrated among initially low-wage workers who gain about 1.3% in welfare nationwide.

Finally, it is important to note that we are able to study the impacts of RS in the context of a middle-income country, where RS is mainly aimed at improving the wages and conditions of initially low-wage workers. The counterfactual would be a very different one in theory if we were

instead to study the effects of, e.g., child labor bans in a low-income country context. Banning a type of employment is a distinct proposition from requiring a wage floor, and caution is in order when extrapolating our findings to very different economic contexts or RS policies. There are also many important questions related to RS that this paper does not answer, as for example potential environmental provisions of RS, which we leave for an exciting agenda for future research on these topics.

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8 Figures and Tables

8.1 Figures

Figure 1: Supplier-Level Event Study
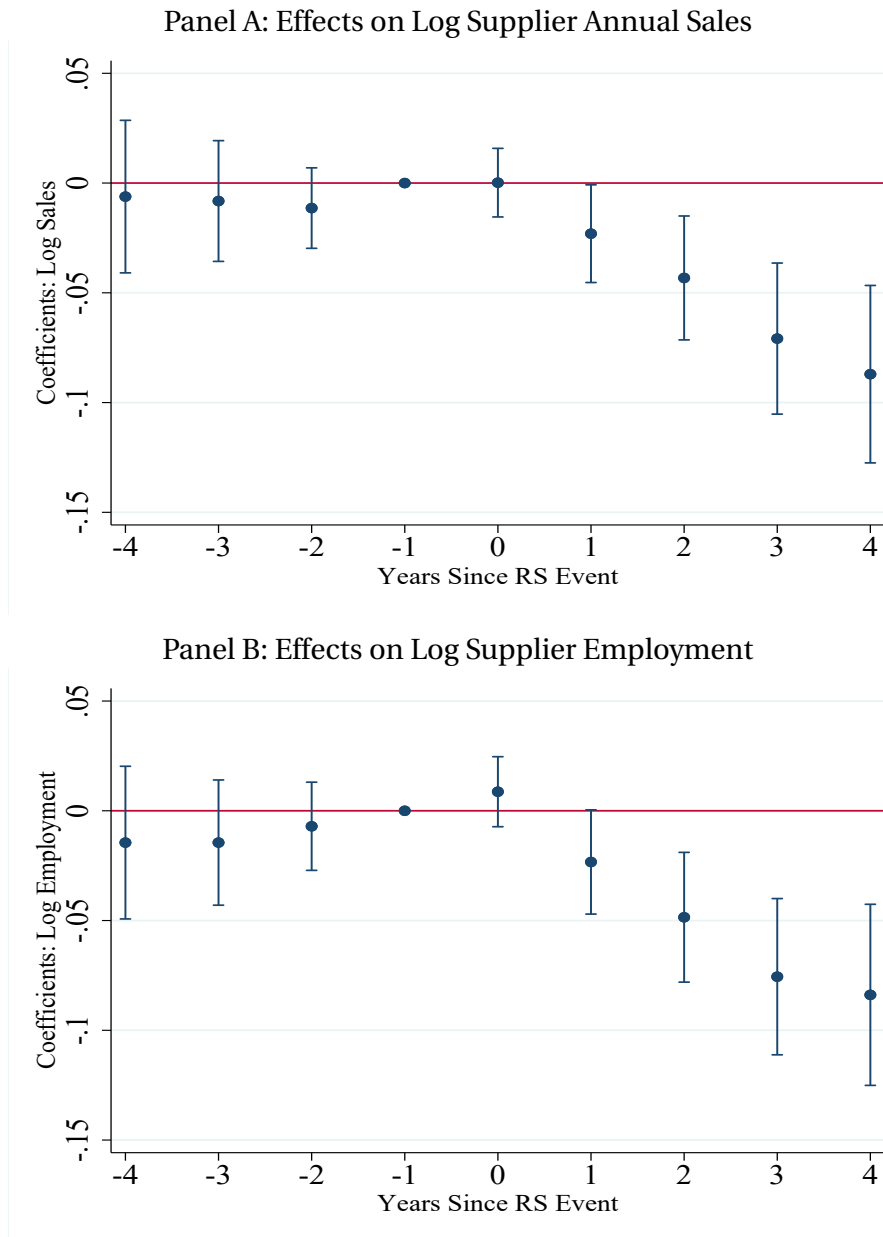
Panel A: Effects on Log Supplier Annual Sales

Panel B: Effects on Log Supplier Employment

Notes: The figure plots estimates from the event study specification in column 4 of Panels A and B in Table 4. The outcome in Panel A is the log of total annual firm sales. The outcome in Panel B is the log of total annual worker-months (number of months worked summed across all workers) at the firm. 95 percent confidence intervals are based on standard errors clustered at the level of firms. See Section 4 for discussion.

Figure 2: Worker-Level Event Study
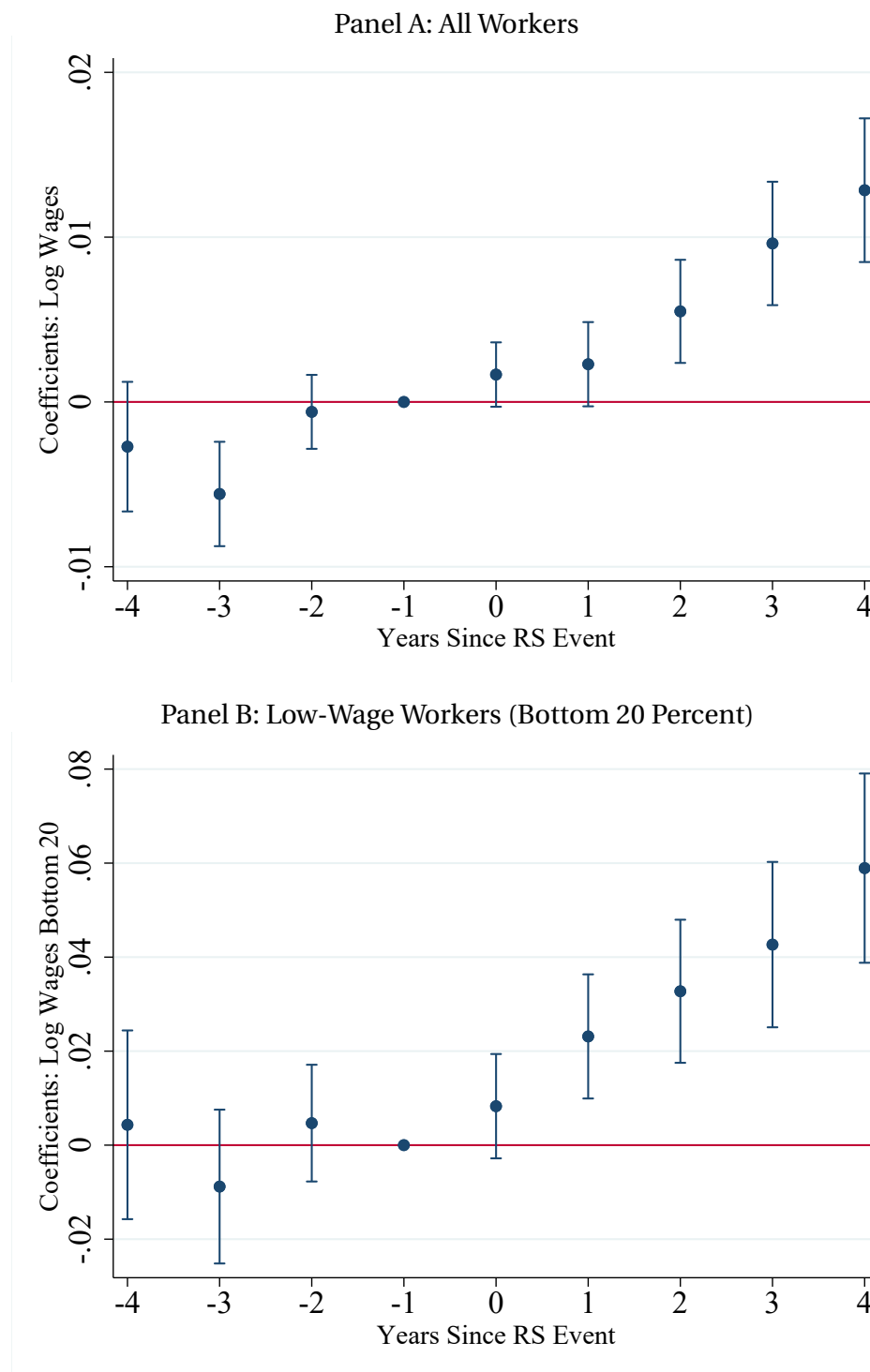
Panel A: All Workers

Panel B: Low-Wage Workers (Bottom 20 Percent)

Notes: The figure plots estimates from the event study specification in column 4 of Panels A and B in Table 5. The outcome is the log of worker annual earnings divided by the number of months of employment. In Panel B, low-wage workers are defined as the bottom 20% of all workers in the data, measured in terms of monthly earnings in the first year we observe each worker since 2006, and relative to the (inflation(CPI)-adjusted) first-time monthly earnings of other workers in the data. 95 percent confidence intervals are based on standard errors clustered at the level of firms. See Section 4 for discussion.

Figure 3: Transaction-Level Event Study
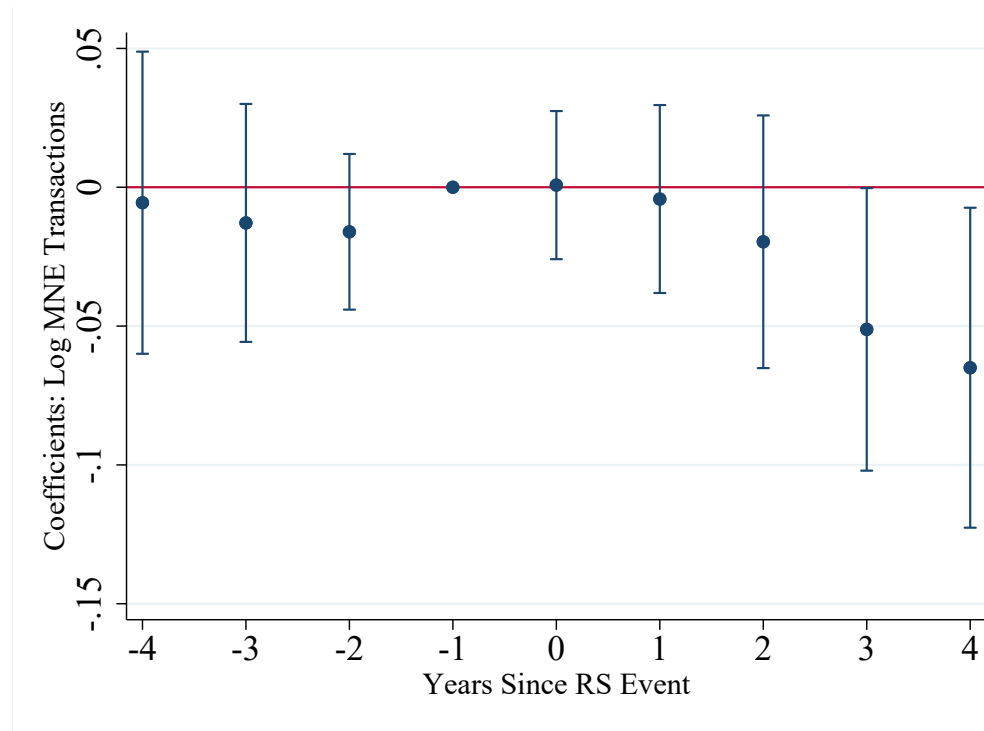
Notes: The figure plots estimates from the event study specification in column 3 of Table 6. The outcome is the log annual transaction sales to the MNE. 95 percent confidence intervals are based on standard errors clustered at the level of firms. See Section 4 for discussion.

Figure 4: Welfare Incidence of RS in CR

Panel A: Aggregate Economy

Panel B: Low-Wage Workers

Panel C: High-Wage Workers

Total   Export Tax Only   Productivity Only
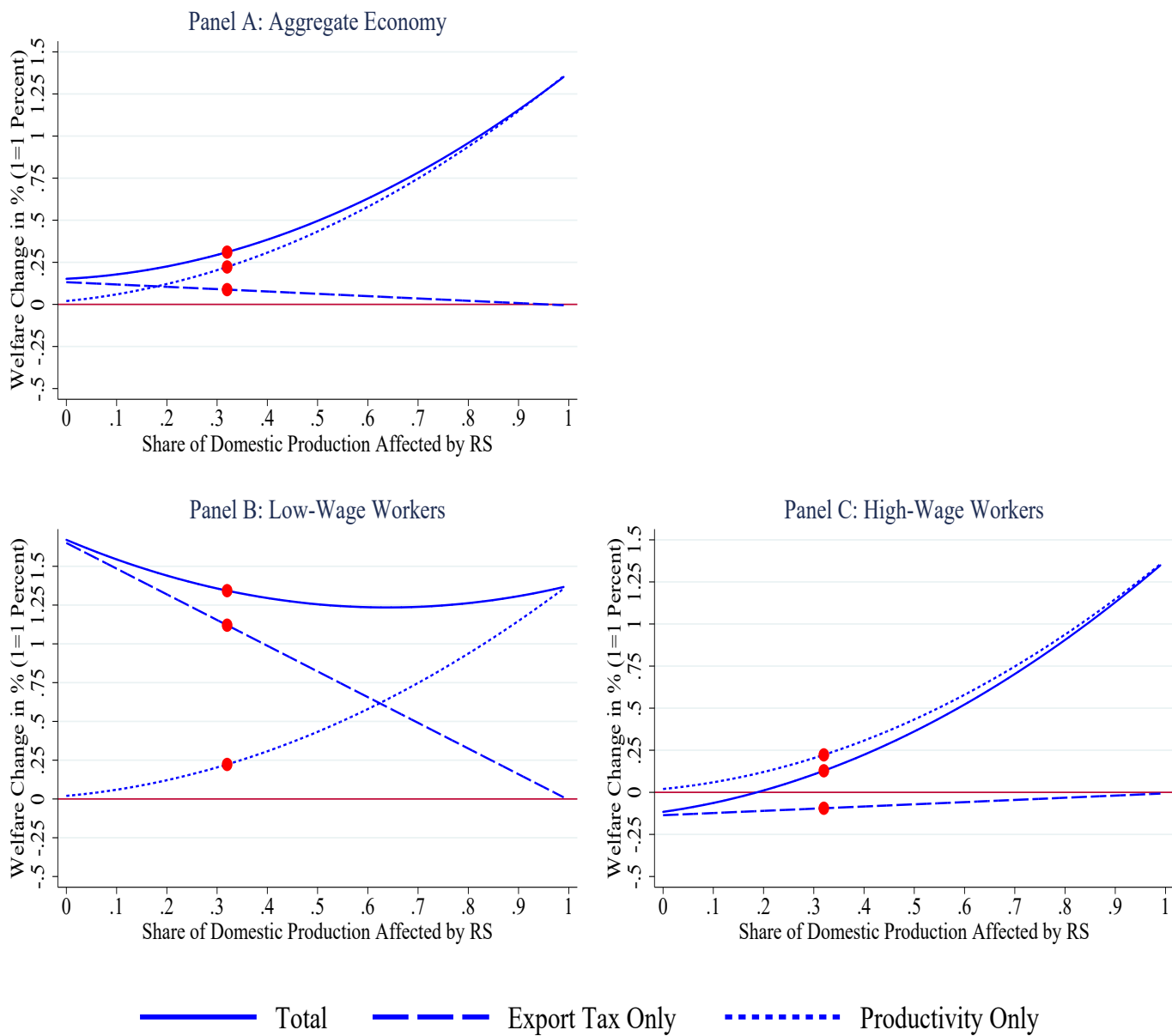
Notes: See Section 6 for discussion.

Figure 5: The Role of RS Exports

Panel A: Aggregate Economy

Panel B: Low-Wage Workers

Panel C: High-Wage Workers

——— Total   – – – Export Tax Only   ········ Productivity Only
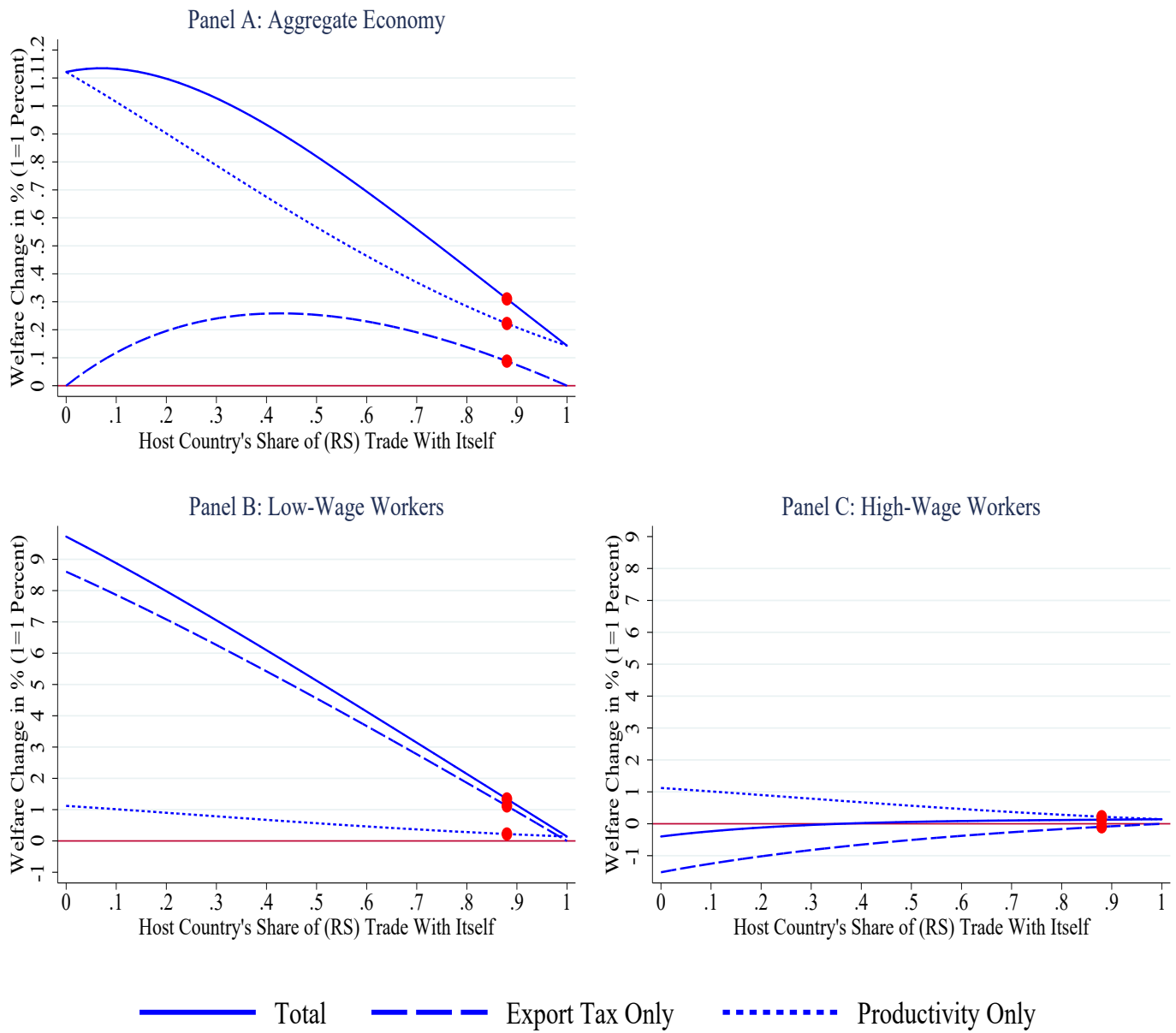
Notes: See Section 6 for discussion.

Figure 6: Sensitivity to Alternative σs

Panel A: Aggregate Economy

Panel B: Low-Wage Workers

Panel C: High-Wage Workers

————— Total   — — — Export Tax Only   ·········· Productivity Only
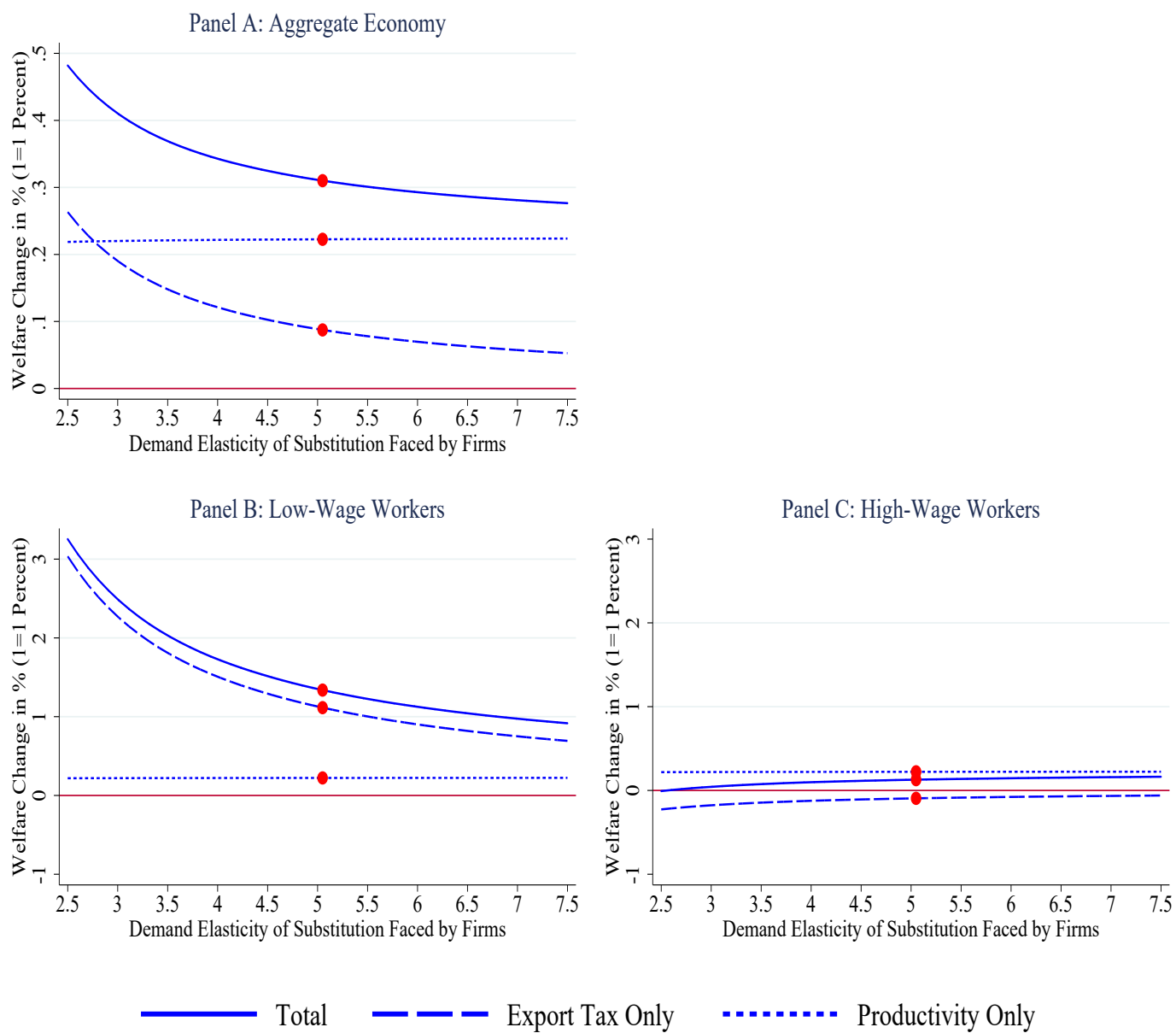
Notes: See Section 6 for discussion.

Figure 7: Sensitivity to MNE Buyer Market Power

Panel A: Aggregate Economy

Panel B: Low-Wage Workers

Panel C: High-Wage Workers

Total ——— Export Tax Only - - - - Productivity Only · · · · · · ·
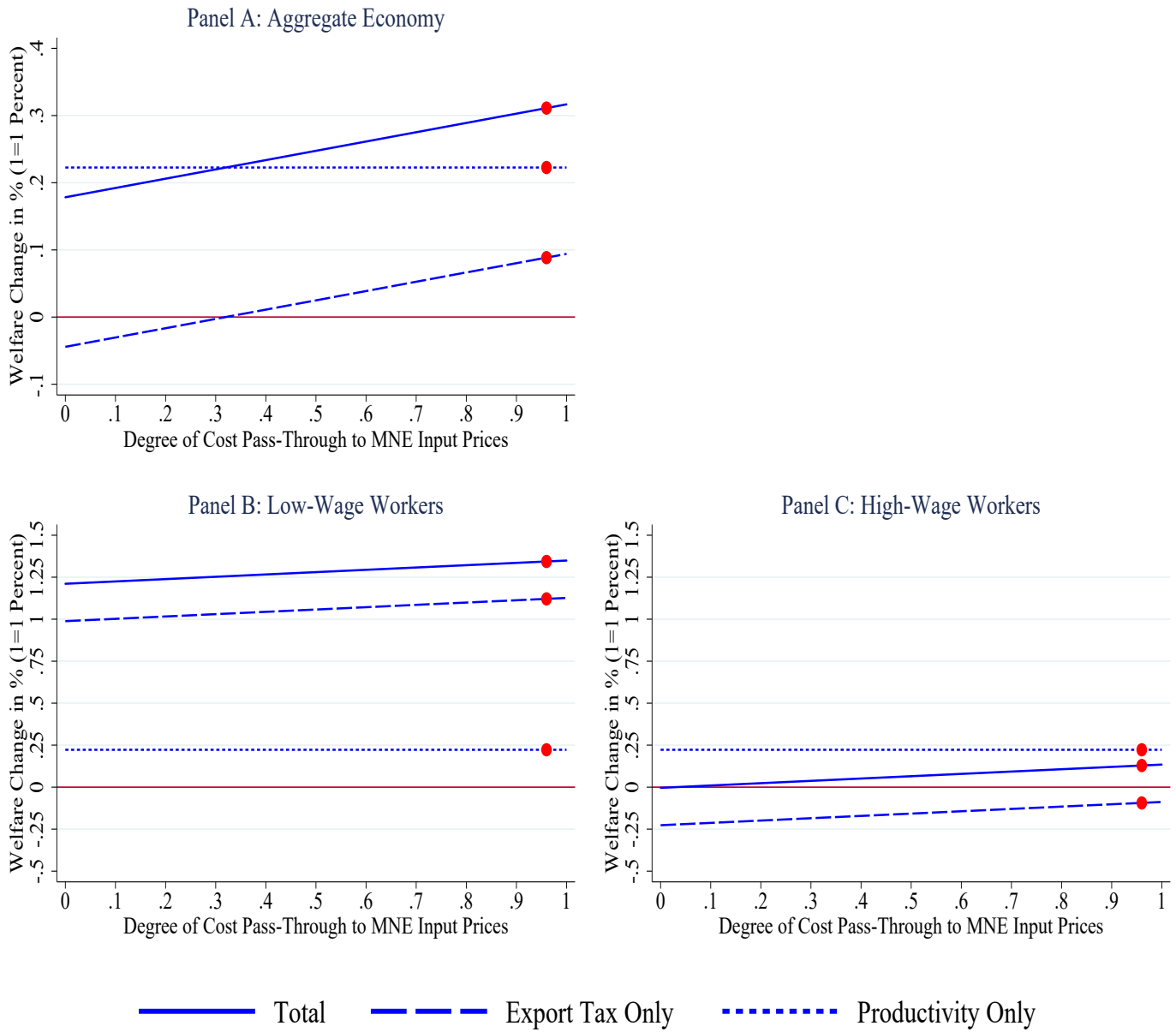
Notes: See Section 6 for discussion.

Figure 8: Role of Affected Workforce
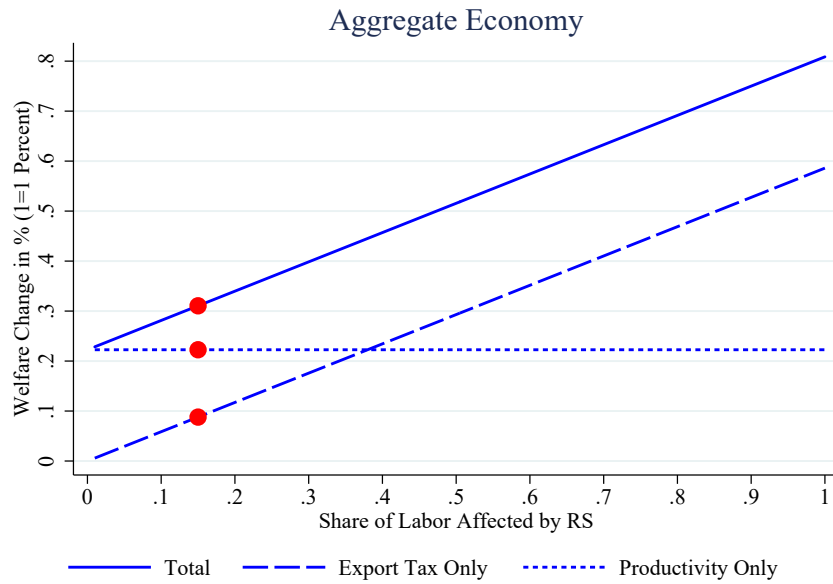
Notes: See Section 6 for discussion.

Figure 9: Incidence as a Function of How Much of $\hat{\tau}$ Low-Wage Workers Capture

Notes: See Section 6 for discussion.

8.2 Tables

Table 1: MNE Sample Coverage

Total Sales	82.2%
Number of Workers	83.4%
Wage Bill	84.9%
Exports	94.7%
Imports	85.8%
Value Added	87.3%
Domestic Purchases	77.2%
Total Net Assets	85.8%

Notes: Table 1 presents the total coverage for the period 2008 to 2017 (summing all years) of the values for the 484 MNEs out the values for the full sample of 2,156 firms part of a corporate group with partial foreign ownership (across eight variables).

Table 2: Descriptive Statistics for the Sample of MNEs

	# Firms	Mean	S.D.	Median
A. MNEs not implementing an RS policy				
Total Sales	357	50311.4	140347.2	18113.7
Employment	357	451.6	946.6	191.3
Wage Bill	357	5497.7	11157.9	2540.5
Exports	272	15273.9	36943.1	1608.7
Imports	348	11966.6	25611.8	2068.5
Value Added	357	11678.5	26314.7	4754.8
Domestic Purchases	357	68.1	124.3	36.4
Total Net Assets	355	56137.2	116333.4	18124.0
Firms in Manuf. Sectors	357	32.2	46.8	0.0
Firms in Agric. Sectors	357	6.2	24.1	0.0
Firms in Ret. & Wholes. Sectors	357	15.1	35.9	0.0
Firms in Serv. Sectors	357	46.5	49.9	0.0
Firms with HQ in USA	357	30.8	46.2	0.0
Firms with HQ in Europe	357	17.9	38.4	0.0
B. MNEs implementing an RS policy				
Total Sales	127	97398.9	181906.8	43035.4
Employment	127	684.5	1530.4	290.0
Wage Bill	127	11273.2	17225.2	5288.8
Exports	115	41292.4	163885.1	3721.5
Imports	127	35968.2	142562.6	8464.9
Value Added	127	39168.7	113688.9	11862.6
Domestic Purchases	127	96.0	119.0	58.3
Total Net Assets	127	168345.7	524325.7	37573.7
Firms in Manuf. Sectors	127	40.2	49.2	0.0
Firms in Agric. Sectors	127	1.6	12.5	0.0
Firms in Ret. & Wholes. Sectors	127	14.2	35.0	0.0
Firms in Serv. Sectors	127	44.1	49.8	0.0
Firms with HQ in USA	127	37.8	48.7	0.0
Firms with HQ in Europe	127	26.8	44.5	0.0

Notes: Table 2 presents descriptive statistics for: (A) the sample of MNEs that do not implement an RS policy and (B) the sample of MNEs that implemented an RS policy. With the exception of the number of workers, the mean, standard deviation, and median are in thousands of CPI-deflated 2013 U.S. dollars. These statistics are averages across 2008 to 2017.

Table 3: Summary Statistics for Domestic Firms (Treated Suppliers and Never-Treated Suppliers)

	Treated suppliers	Never-treated suppliers	Difference
	(1)	(2)	(3)
Time Invariant Characteristics			
Agriculture, Forestry and Fishing	9.498	4.773	4.725***
	(29.32)	(21.32)	(0.44)
Manufacturing	11.35	14.18	-2.834***
	(31.72)	(34.89)	(0.53)
Sewerage and Waste Management	0.814	0.635	0.179
	(8.98)	(7.94)	(0.14)
Construction	7.562	3.580	3.982***
	(26.44)	(18.58)	(0.40)
Wholesale and Retail Trade	26.49	29.10	-2.605***
	(44.13)	(45.43)	(0.72)
Transportation and Storage	8.235	10.08	-1.850***
	(27.49)	(30.12)	(0.46)
Accommodation and Food Services	5.759	3.233	2.526***
	(23.30)	(17.69)	(0.36)
Information and Communication	2.511	4.734	-2.223***
	(15.65)	(21.24)	(0.28)
Real Estate	2.476	2.791	-0.314
	(15.54)	(16.47)	(0.26)
Professional, Scientific and Technical	11.35	14.38	-3.027***
	(31.72)	(35.09)	(0.53)
Administrative and Support Service	7.295	7.044	0.251
	(26.01)	(25.59)	(0.42)
Education	0.624	0.654	-0.0301
	(7.88)	(8.06)	(0.13)
Human Health and Social Work	1.641	1.097	0.544**
	(12.71)	(10.42)	(0.20)
Art, Entertainment and Recreation	1.052	0.943	0.109
	(10.20)	(9.67)	(0.16)
Other Services	2.701	2.232	0.468
	(16.21)	(14.78)	(0.26)
Mining and Quarrying	0.463	0.346	0.117
	(6.79)	(5.88)	(0.11)
Time Variant Characteristics			
Total Sales (thous. U.S. dollars)	1,263.4	3,428.1	-2,164.7***
	(4,569.67)	(14,455.64)	(77.89)
Number of Workers	16.21	35.48	-19.27***
	(46.32)	(113.09)	(0.73)
Total Sales (thous. U.S. dollars) / Worker	115.1	141.2	-26.14***
	(345.04)	(438.12)	(4.98)
Wage Bill per Worker	6.958	8.312	-1.354***
	(6.82)	(6.05)	(0.10)
Share of Importers	24.92	42.72	-17.80***
	(43.25)	(49.47)	(0.62)
Share of Exporters	7.074	15.63	-8.552***
	(25.64)	(36.31)	(0.37)
Number of Firms	5,196	14,256	

Notes: Table 3 presents descriptive statistics for the sample of suppliers experiencing an RS-policy event from an MNE buyer (column (1)) and other suppliers to MNEs that did not experience an RS-policy event (column (2)). For each sample of firms, we characterize their broad sector, total sales, employment, total sales per worker, average annual earnings, share of importers and exporters. All time-varying variables correspond to averages across time for each supplier. In the case of column (1), we only use the year before their event to compute the averages. Standard deviations in parentheses.

Table 4: Supplier-Level Event Study

Panel A: Effects on Log Supplier Annual Sales

	(1)	(2)	(3)	(4)
	TWFE	TWFE	SA	IV
$\eta = -4$	-0.140***	-0.016	0.010	-0.009
	(0.017)	(0.016)	(0.017)	(0.018)
$\eta = -3$	-0.096***	-0.020	0.003	-0.011
	(0.013)	(0.013)	(0.013)	(0.014)
$\eta = -2$	-0.051***	-0.011	-0.002	-0.012
	(0.009)	(0.009)	(0.008)	(0.009)
$\eta = -1$	0	0	0	0
	(0)	(0)	(0)	(0)
$\eta = 0$	-0.002	-0.004	-0.008	-0.000
	(0.007)	(0.007)	(0.007)	(0.008)
$\eta = 1$	-0.037***	-0.026**	-0.025**	-0.023**
	(0.011)	(0.010)	(0.010)	(0.011)
$\eta = 2$	-0.076***	-0.046***	-0.049***	-0.044***
	(0.013)	(0.013)	(0.013)	(0.014)
$\eta = 3$	-0.114***	-0.073***	-0.089***	-0.071***
	(0.016)	(0.015)	(0.016)	(0.017)
$\eta = 4$	-0.149***	-0.091***	-0.120***	-0.087***
	(0.019)	(0.018)	(0.019)	(0.020)
Firm FE	Yes	Yes	Yes	Yes
Year-4DSect FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Adjusted R^2	0.84	0.85	0.85	-
# Observations	145930	145930	145930	145930
# Firms	19437	19437	19437	19437
# Sector-Year Bins	2503	2503	2503	2503

Panel B: Effects on Log Supplier Employment

	(1)	(2)	(3)	(4)
	TWFE	TWFE	SA	IV
$\eta = -4$	-0.132***	-0.026	-0.003	-0.017
	(0.017)	(0.016)	(0.017)	(0.018)
$\eta = -3$	-0.081***	-0.021	-0.001	-0.015
	(0.014)	(0.013)	(0.013)	(0.014)
$\eta = -2$	-0.030***	-0.005	0.004	-0.007
	(0.010)	(0.009)	(0.009)	(0.010)
$\eta = -1$	0	0	0	0
	(0)	(0)	(0)	(0)
$\eta = 0$	0.013*	0.007	0.004	0.008
	(0.008)	(0.007)	(0.007)	(0.008)
$\eta = 1$	-0.014	-0.019*	-0.017	-0.023*
	(0.011)	(0.011)	(0.011)	(0.012)
$\eta = 2$	-0.047***	-0.040***	-0.041***	-0.048***
	(0.013)	(0.013)	(0.013)	(0.015)
$\eta = 3$	-0.083***	-0.070***	-0.087***	-0.074***
	(0.016)	(0.016)	(0.017)	(0.018)
$\eta = 4$	-0.114***	-0.091***	-0.120***	-0.082***
	(0.019)	(0.019)	(0.020)	(0.021)
Firm FE	Yes	Yes	Yes	Yes
Year-4DSect FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Adjusted R^2	0.78	0.79	0.79	-
# Observations	145592	145592	145592	145592
# Firms	19398	19398	19398	19398
# Sector-Year Bins	2503	2503	2503	2503

Notes: See Section 4 for discussion. Panels A and B present estimates for specification 41. The first-stage F-statistic for the final IV column exceeds 50. Standard errors clustered at the level of firms. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: Worker-Level Event Study

	Panel A: All Workers			
	(1)	(2)	(3)	(4)
	TWFE	TWFE	SA	IV
$\eta = -4$	-0.005***	-0.001	0.006***	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)
$\eta = -3$	-0.005***	-0.003*	0.002	-0.007***
	(0.001)	(0.001)	(0.002)	(0.002)
$\eta = -2$	-0.000	0.001	0.003***	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$\eta = -1$	0	0	0	0
	(0)	(0)	(0)	(0)
$\eta = 0$	0.002*	-0.000	-0.002*	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
$\eta = 1$	0.003***	0.000	-0.003***	0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
$\eta = 2$	0.006***	0.002*	-0.002	0.007***
	(0.001)	(0.001)	(0.001)	(0.002)
$\eta = 3$	0.011***	0.006***	-0.000	0.011***
	(0.001)	(0.002)	(0.002)	(0.002)
$\eta = 4$	0.013***	0.009***	0.002	0.016***
	(0.002)	(0.002)	(0.002)	(0.002)
Year-4DSect FE	Yes	Yes	Yes	Yes
Worker-Firm FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Adjusted R^2	0.84	0.84	0.84	-
# Observations	4347181	4347181	4347181	4347181
# Firms	59776	59776	59776	59776
# Workers	750828	750828	750828	750828

	(1)	(2)	(3)	(4)
Panel B: Low-Wage Workers (Bottom 20 Percent)				
	TWFE	TWFE	SA	IV
$\eta = -4$	-0.004	0.011	0.016	0.004
	(0.009)	(0.009)	(0.010)	(0.010)
$\eta = -3$	-0.015**	-0.007	-0.005	-0.009
	(0.007)	(0.008)	(0.008)	(0.008)
$\eta = -2$	-0.001	0.002	0.004	0.005
	(0.006)	(0.006)	(0.007)	(0.006)
$\eta = -1$	0	0	0	0
	(0)	(0)	(0)	(0)
$\eta = 0$	0.011**	0.009*	0.008	0.008
	(0.005)	(0.005)	(0.005)	(0.006)
$\eta = 1$	0.025***	0.019***	0.017***	0.023***
	(0.006)	(0.006)	(0.007)	(0.007)
$\eta = 2$	0.034***	0.027***	0.024***	0.033***
	(0.007)	(0.007)	(0.008)	(0.008)
$\eta = 3$	0.047***	0.038***	0.035***	0.043***
	(0.008)	(0.008)	(0.010)	(0.009)
$\eta = 4$	0.055***	0.046***	0.042***	0.059***
	(0.009)	(0.009)	(0.011)	(0.010)
Year-4DSect FE	Yes	Yes	Yes	Yes
Worker-Firm FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Adjusted R^2	0.72	0.72	0.72	-
# Observations	551383	551383	551383	551383
# Firms	30407	30407	30407	30407
# Workers	121084	121084	121084	121084

Notes: See Section 4 for discussion. Panels A and B present estimates for specification 42. In Panel B, low-wage workers are defined as the bottom 20% of all workers in the data, measured in terms of monthly earnings in the first year we observe each worker since 2006, and relative to the (inflation(CPI)-adjusted) first-time monthly earnings of other workers in the data. The first-stage F-statistic for the final IV column exceeds 50. Standard errors clustered at the level of firms. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6: Transaction-Level Event Study

	(1) TWFE	(2) SA	(3) IV
$\eta = -4$	-0.005	0.001	-0.006
	(0.026)	(0.027)	(0.028)
$\eta = -3$	-0.017	-0.018	-0.013
	(0.020)	(0.020)	(0.022)
$\eta = -2$	-0.027**	-0.028**	-0.016
	(0.013)	(0.013)	(0.014)
$\eta = -1$	0	0	0
	(0)	(0)	(0)
$\eta = 0$	-0.005	-0.005	0.001
	(0.013)	(0.013)	(0.014)
$\eta = 1$	-0.011	-0.012	-0.004
	(0.016)	(0.016)	(0.017)
$\eta = 2$	-0.018	-0.021	-0.020
	(0.021)	(0.020)	(0.023)
$\eta = 3$	-0.039	-0.042*	-0.051**
	(0.024)	(0.025)	(0.026)
$\eta = 4$	-0.052*	-0.050*	-0.065**
	(0.027)	(0.028)	(0.029)
Year-4DSect FE	Yes	Yes	Yes
Year-MNEBroadSect FE	Yes	Yes	Yes
MNC-Supplier FE	Yes	Yes	Yes
Adjusted R^2	0.68	0.68	-
# Observations	263694	263694	263694
# MNEs	421	421	421
# Suppliers	13466	13466	13466
# Sup Sector-Year Bins	2531	2531	2531

Notes: See Section 4 for discussion. The table presents estimates for specification 42. The first-stage F-statistic for the final IV column exceeds 50. Standard errors clustered at the level of firms. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 7: RS Effects Used for Parameter Estimation

Dependent Variable:	(1) Log Monthly Earnings of Workers in Top Third	(2) Log Annual Supplier Sales	(3) Log Domestic Sales of Suppliers	(4) Log Intensive-Margin Sales to the MNE	(5) Log Employment Ratio of Low- vs High-Wage Workers
Point Estimate of RS Rollout after Adjustment (η≥4)	0.0141 (0.005)	-0.114 (0.023)	-0.090 (0.031)	-0.086 (0.033)	-0.105 (0.016)
Number of Obs	1257349	145930	145930	263695	128825
Sector-by-Year FE	Yes	Yes	Yes	Yes	Yes
Worker-by-Firm FE	Yes	.	.	.	.
Supplier FE	.	Yes	Yes	.	Yes
Supplier-by-Buyer FE	.	.	.	Yes	.
Controls	Yes	Yes	Yes	.	Yes

Notes: See Section 5 for discussion. The table presents IV estimates for specifications 41 (columns 2, 3, 5) and 42 (columns 1 and 4). All first stages have F-statistics well above the critical values. Standard errors clustered at the level of firms.

Table 8: Parameter Estimates

Model parameter:	T_hat	τ_hat	σ	β	θ	ρ
Our estimate	0.014	0.149	5.03	0.96	7.47	0.71
Moments used in estimation	Column 1 of Table 7	Columns 2-4 of Table 7	Alfaro-Urena et al. (2021)	Columns 3 & 4 of Table 7	Columns 2-4 of Table 7	Column 5 of Table 7

Notes: See Section 5 for discussion.

Table 9: Estimates of RS Domestic "Leakage" (Λ) in CR

	Leakage	Leakage	Leakage	Trade Share	Trade Share
	(1)	(2)	(3)	(4)	(5)
2009	0.30	0.29	0.21	0.43	0.42
2010	0.33	0.32	0.25	0.37	0.37
2011	0.35	0.34	0.27	0.37	0.37
2012	0.36	0.35	0.30	0.40	0.40
2013	0.38	0.36	0.33	0.47	0.46
2014	0.39	0.38	0.34	0.47	0.46
2015	0.43	0.42	0.39	0.33	0.34
2016	0.45	0.42	0.40	0.32	0.34
2017	0.44	0.42	0.40	0.38	0.39

Notes: Column 1 presents the fraction of total production by non-MNE firms in CR that is done by firms subject to an active RS requirement in any given year. Column 2 shows the same fraction, but excluding sales to MNEs in both numerator and denominator. Column 3 presents the fraction of non-MNE and non-export sales by any RS-active firm (MNE or domestic) relative to total output that is not exported or sold to MNEs. Column 4 presents the share of RS-active MNE exports relative to total MNE exports in CR. Column 5 presents the fraction of total exports by any RS-active firm (MNE or domestic) relative to total exports in CR. See Section 6 for discussion.

9 Appendix

Theory Appendix

[To be added here.]

Additional Figures and Tables

[To be added here.]