The Impact of Anti-Sweatshop Activism on Employment*

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December 12, 2018

Abstract

While literature on the anti-sweatshop campaigns has empirically rejected the negative impact on employment, this paper shows that anti-sweatshop activisms for multinational companies in Indonesia had a negative impact on employment. My result suggests that the result in literature comes from disregarding the differences in some dimensions of firm characteristics between treatment and control groups.

Keywords: Multinational Firms; Sweatshop; Employment
JEL Classification: F23, J21, J81, O15

*I thank Kala Krishna for her guidance and support. I also thank Michael Gechter, Julia Cajal Grossi, Martin Hackmann, Keisuke Hirano, John McLaren, Martin Rotemberg, Jason Scorse, James Tybout, and anonymous referees for helpful comments and suggestions. Finally, I am grateful to Ann Harrison for kindly giving me the data.

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1 Introduction

"Sweatshop" is a loosely-defined term denoting a factory where workers work long hours under poor conditions and are paid low wages. The anti-sweatshop movement is a campaign that seeks to improve these conditions for such workers by refusing to buy goods made by sweatshops. Since these campaigns are widespread, a natural question is: Are these campaigns actually good for workers in sweatshops?

While economic theory predicts that anti-sweatshop campaigns have a negative impact on employment (McLaren, 2013; Powell, 2014; Irwin, 2015), empirical literature suggest that they have an overall positive impact on workers in the hosting country. For example, Harrison & Scorse (2010) analyze the impact of these anti-sweatshop campaigns on wages and employment in Indonesia. Using a difference in differences approach, they argue that not only was the increase in wages larger for foreign-owned and exporting firms in the regions most affected by the anti-sweatshop campaigns, but also that there seemed to be no adverse employment effects for surviving firms. They show that the campaigns had small adverse effects on employment due to the exit of small firms, but that this was outweighed by employment expansions on the part of surviving firms. Their results suggest, somewhat counter-intuitively, that the anti-sweatshop campaigns were, on the whole, *good* for Indonesian workers.

In this paper, I show, using a case of multinational firms in Indonesia during 1990s, that there was actually the negative impact on employment. Specifically, in order to take into account a potential difference between firms in the treatment group and those in the control group, I use the synthetic control method proposed by Abadie & Gardeazabal (2003) and Abadie, Diamond, & Hainmueller (2010). The method enables me to construct an accurate control group by taking a convex combination of firms in the control group. Doing so makes clear graphically and econometrically that there is a negative impact of the anti-sweatshop campaigns on employment.

I argue that the discrepancy between my result and those in literature comes from disregarding differences in some dimensions of firm characteristics between treatment and control groups. For the point, out of several firm characteristics, I particularly focus on firm's age. In Indonesia, exporters and foreign-owned firms in the districts subject to the anti-sweatshop campaigns were significantly younger than other firms in the same districts or firms outside the districts. Since younger firms grow faster than older ones, omitting age as a control would bias upwards the key coefficient measuring the treatment effect in the regression. I show that once firms' ages are included in the controls of the difference in differences estimation, the key coefficient does indeed drop in size and becomes statistically insignificant. In addition, the change in the coefficient with the observed age variable suggests a potential difference of treatment and control groups in unobserved confounding factors. Using a recent econometric method of the coefficient stability approach by Oster (2016), I show that there could be a negative impact of the campaigns once taking into account the difference in unobserved confounding factors.

There is public and academic debate on the impact of the anti-sweatshop campaigns on employment in a hosting country. On the one hand, economists tend to argue that the anti-sweatshop campaigns are likely to reduce employment in a host country (Powell & Zwolinski, 2012; McLaren, 2013; Powell, 2014; Irwin, 2015). On the other hand, other scholars argue that there can be mechanisms with which there is no negative employment impact of these campaigns (Arnold, 2003, 2010;

Millar, 2003; Pollin et al., 2004; and others). Despite this intensive discussion, there are only a few studies on the topic which use a regression analysis with sample data. Harrison & Scorse (2010) is one of these papers which analyzes the impact of the anti-sweatshop campaigns on employment using firm-level data and find no adverse employment effects of the anti-sweatshop activism for surviving firms. Their employment analysis uses a difference in differences approach without using a matching technique and therefore does not exclude concern about a potential difference in firm characteristics in terms of both observed and unobserved factors between treatment and control groups. In contrast, in my analysis, I use the synthetic control method so that the treatment and control groups in the analysis become comparable in terms of observed characteristics. Moreover, the method is likely to make these firms comparable in terms of unobserved characteristics by matching employment in the pre-treatment periods. By doing so, my analysis shows that there was actually the negative impact of the anti-sweatshop campaigns on employment.

The paper proceeds as follows. Section 2 introduces the background. Section 3 presents a theoretical prediction of anti-sweatshop activism. Section 4 explains my empirical framework in a simple difference in differences with highlighting the key assumptions, and describes data. Section 5 explains the synthetic control method and shows my main results. Section 6 points out that the difference in results should be from the difference in firm ages and other unobserved characteristics between treatment and control groups. Finally, Section 7 offers some concluding thoughts.

2 Background

The sweatshop and the anti-sweatshop campaigns which I focus on are those in Indonesia around the early 1990s. Several organizations, institutions and human right activists, notably Jeff Ballinger, charged factories related to companies headquartered in developed countries (such as Nike, Adidas, and Reebok) due to paying low wages and having poor working conditions in Indonesia. They appealed to consumers in developed countries through the media to boycott these companies. With the surge of the appeals, the number of articles on sweatshops in major news and business outlets rose six fold in 1996 relative to 1989, which is reported in Figures A1 to A3 of Online Appendix A[1].

In reaction to these criticisms, Nike, for example, distributed a code of conduct to its contractors for the first time in 1992 and tried to monitor and improve working conditions in its supplier's factories. Therefore, the year of 1992 can be thought of as the start of the anti-sweatshop campaigns in Indonesia, because in addition to the Nike's distributing the code of conduct, Jeff Ballinger published in 1992 a negative article on Nike's sweatshop located in Indonesia.

3 Theoretical Prediction

In this section, I present a textbook model of the anti-sweatshop and show its predictions on employment[2]. See Figure 1 where the length of horizontal axis is the total freely-mobile labor working in textile, footwear, and apparel (TFA) sectors (i.e., the sum of labor hired in criticized companies

[1] I search this in Dow Jones Factiva database of international newspaper articles. I search the number of articles by using the keyword "sweatshop", by restricting the periods from 1989 to 1996, and by focusing on "Major News and Business Sources".

[2] This is a specific factors model from McLaren (2013).

L_{EM} and those worked in other companies L_D). Labor demand from the criticized companies decreases in the amount of labor and is measured from the left origin O_{EM}. The same is the case for the other companies, but their demand is measured from the right origin O_D. The equilibrium wage and labor allocation are determined by the intersection of these two demand curves, B (i.e., wage is w and labor allocation is L_{EM} and L_D).

Now, suppose that wages are increased among the criticized companies due to the anti-sweatshop activism as shown by Harrison & Scorse (2010). If it forces the criticized companies to raise their wages to say $w^{'}$, the wage and labor demand are determined by points A and C. As a result, labor demand by the criticized companies decreases from L_{EM} to $L^{'}_{EM}$, while that by the other companies increases from L_D to $L^{'}_D$. In sum, the simple competitive theory predicts that the anti-sweatshop activism which raises wages has a negative impact on employment among the criticized companies.

It is true that labor market imperfections can lead to a positive relationship between minimum wages and their employment. In particular, if there exist monopsony employers in the local labor market, the increase in wages can expand employment in these employers. However, as shown in Table A4 of Online Appendix, there are a lot of TFA companies (and non-TFA companies) within each district, giving a hardly convincing evidence of the monopsony labor market.

4 Empirical Framework and Data

4.1 Framework in simple DID

I introduce an empirical framework of a difference in differences which is behind my analysis with the synthetic control method in the next section and is directly used in Section 6 for elucidating the omitted variable bias in literature. The regression equation for a difference of log production worker employment (hereafter, simply employment) before and after the campaign is

$$\Delta \log l_{ir} = \beta_0 + \beta_1 \text{FOREXP}_i + \beta_2 \text{Treatment}_i + \beta_3 (\text{FOREXP} * \text{Treatment})_i + \boldsymbol{\gamma} \mathbf{Z_{ir}} + e_{ir}, \qquad (1)$$

where i denotes a firm and r denotes a region in which the firm locates. FOREXP_i is a dummy variable for an exporter or a foreign-owned firm, Treatment_i is a dummy variable for a firm located in districts where the targeted firms of the anti-sweatshop campaigns – Nike, Adidas, and Reebok– had their subsidiaries or transaction partners, and $\mathbf{Z_{ir}}$ denotes other control variables such as the change in minimum wages and province dummies. The key parameter is β_3, which captures the treatment effect (i.e., the difference of the average growth rate of employment between firms in the treatment group and those in the control group).

The key identifying assumption of the difference in differences approach is that, after controlling for covariates, the treatment and control groups have common trends[3]. The assumption is checked in the next sections.

[3]See Angrist & Pischke (2009) for the detail of the assumption.

4.2 Data

The data utilized in my analysis is Indonesian Annual Manufacturing Survey from 1988 to 1996, collected by the Indonesian government statistical agency, BPS (Badan Pusat Statistik)[4]. It includes about 12000 observations in 1988 and about 18000 in 1996, each of which is a manufacturing firm with 20 or more employees. Within the sample, I focus on a sample of firms in TFA sectors in order not to confound sectoral shocks with the impact of the anti-sweatshop campaigns. Therefore, the number of observations falls to about 2500 to 3000.

These firms in TFA sectors are located in districts shown in Figure 2. Districts with TFA firms are shadowed by gray or black. Within these areas, the districts colored with black are those with firms impacted by the anti-sweatshop campaigns. As can be seen, though TFA firms are located sparsely across districts, TFA firms which are impacted by the campaigns are located only in several districts.

4.3 Descriptive Statistics

Table 1 gives descriptive statistics for my sample used in the following analysis. There are several things to notice. First, values in many variables are different between the treatment and control groups in 1991. Specifically, firms in the treatment group is larger and younger, use more inputs, and produce more outputs than those in the control group. These differences suggest a potential difference in unobserved aspects too, whose effect is analyzed in Section 6. Second and more importantly, the trend of variables is also different across treatment and control groups in the pre-treatment period. Among these variables, the growth of material inputs and outputs are largely different. These differences motivate me to use the synthetic control group in my main analysis.

5 The Synthetic Control Method

5.1 Method and Result

In order to mitigate concern, raised the last section, about differences between treatment and control groups, I use the synthetic control method proposed by Abadie & Gardeazabal (2003) and Abadie, Diamond, & Hainmueller (2010). The method provides a data-driven procedure to choose weights for control groups and construct a "synthetic" control group which has a pre-treatment trend of the outcome variable comparable to the treatment group.

Formally, the impact of the anti-sweatshop campaigns, denoted by α_{it} is

$$l_{it} = l_{it}^{C} + \alpha_{it} D_{it}, \tag{2}$$

where l_{it}^{C} is the (counterfactual) outcome variable if firm i were not impacted by the campaigns and

$$D_{it} = \begin{cases} 1 & \text{if } i \text{ is in the treatment group and } t \geq T_0 \\ 0 & \text{otherwise.} \end{cases} \tag{3}$$

[4] I use the replication data from Harrison & Scorse (2010), which are available in the AER website at https://www.aeaweb.org/articles?id=10.1257/aer.100.1.247. Their data also include information on districts where the treated firms were located.

T_0 is a period starting the campaigns. The synthetic control method provides an approach to derive optimal synthetic weights $\mathbf{W}^* = (w_1^*, w_2^*, ..., w_J^*)'$, in which $w_j^* \in [0,1]$ is a synthetic weight for control firm j. $\mathbf{W}^*$ is chosen to minimize the difference of pre-treatment outcome variables and other covariates between the treatment and control groups. With these weights, the counterfactual outcome variable is obtained by $\hat{l}_{it}^C = \sum_{j \in \text{Control}} w_j^* l_{jt}$, which is called the synthetic control group.

In my analysis, the weights on firms in the control group are constructed so that the log of TFP, the log of output, the age of firms, the log output growth, the log price growth, and the log of employment in 1989 are as close as possible between the treatment and synthetic control groups. The inclusion of the lag employment variable can help controlling for unobserved factors, because only firms which are similar in terms of both observed and unobserved determinants of log employment should produce similar profiles over the pre-treatment periods. I check the robustness of these chosen variables in Online Appendix B2. The obtained results are qualitatively similar in other choices of variables, which give good fits over the pre-treatment periods. See Tables A2 and A3, and Figures A4 to A9 in Online Appendix B2.

Table 2 compares characteristics in the pre-treatment period between the average firm in the treatment group and its synthetic control group, as well as the average firm in the control group[5]. The results in column 3 correspond to those for the control group in the usual difference in differences analysis. As can be seen, the characteristics of the firm in the synthetic control group capture those in the treatment group well, while the same cannot be said for firms in the control group. The weights on each firm in the synthetic control group are reported in Table 3.

The result of the synthetic control method can be seen graphically by comparing Figure 3 and Figure 4. Figure 3 depicts the log employment profiles of the average firm in the treatment group (solid line) and the average of firms in the control group (dotted line)[6], while Figure 4 shows the same profiles for the average firm in the treatment group (solid line) and the firm in the synthetic control group (dotted line). It is noticed that Figure 3 shows a difference in the trends of log employment between treatment and unweighted control groups during the pre-treatment periods. The difference is statistically significant as checked in Table A1 of Online Appendix B1.

Using the synthetic control weights in Figure 4, although the average firm in the treatment group and the firm in the synthetic control group do not have a large difference in the log employment before 1992 (i.e., the year of the start of the anti-sweatshop campaigns), the two profiles start to become different after 1992. By 1996, the log employment profile for the average firm in the treatment group ends up lying below the profile for the firm in the synthetic control group. This result supports the theoretical prediction on the adverse employment effect of the anti-sweatshop campaigns.

The graphical result is supported by a regression-based result with the obtained synthetic weights. Before moving to the result, there are two points to mention. First, as in the usual difference in differences estimation, the first difference is the dummy variable *after* for years after the anti-sweatshop

[5]Because the synthetic control method can be applied only for the case of a single treatment unit, here I construct an average treatment firm by simply averaging out their variables. That is, for each year, I construct variables for the average treated firm by $\bar{x}_t = [\sum_{i \in \Omega_t^T} x_{it}]/N_t^T$, where Ω_t^T is the set of firms in the treatment group in year t, N_t^T is the number of treated firms in year t, and x_{it} is a variable for treated firm i in year t, a variable used in the analysis such as output, age, and TFP. The synthetic weights for the control group are selected so that these chosen variables match well with the average treated firm over pre-treatment periods. Xu (2016) extends the method into the case of multiple treatment units, where the extended method requires a large number of pre-treatment periods. Because my data have only a few years before the treatment, I do not use his method.

[6]Note that I need to line up the profiles at the initial point in Figure 3.

campaign (i.e., after 1992). Second, another difference is the treatment variable *FOREXPTR* which is equal to one for the average exporting or foreign-owned firm in the targeted districts. The variable of interest is the interaction of these two. The regression-based result is reported in Table 4.

As can be seen, the estimated coefficients in both OLS regression (column 2) and the firm fixed effect regression (column 3) show that the anti-sweatshop campaigns reduced the employment in the targeted firm by 25.8 percent. These coefficients are statistically and economically significant, suggesting the negative impact of the anti-sweatshop campaigns on employment.

5.2 Placebo Tests

To further assess the validity of these results, I provide a placebo test, proposed by Abadie, Diamond, & Hainmueller (2010). For the test, firstly, the same exercise as Figure 4 is conducted by swapping the actual treatment unit with a unit in the control group, as if the latter were the treatment unit and the former were the control unit. Then, I calculate the gap between the log employment for the chosen control unit and that for its synthetic control group. This process is repeated for all units in the control group. If most of the placebo exercises create larger gaps than the gap with the actual treatment unit, then my graphical result in Figure 4 would be less convincing as it would suggest that something else might be driving my results.

While conducting the placebo test, I further adjust three details in the exercise whose procedure is further illustrated in Table A4 of Online Appendix C. First, I aggregate individual firms within each province into an average domestic firm and an average exporter-or-foreign-owned firm respectively and use these average firms as placebo units. This is because the log employment of individual firms could fluctuate for many other reasons than the anti-sweatshop campaigns. Second, for the similar reason, if the number of firms within the aggregated cell is small, the aggregated unit is excluded from the analysis, because the noise of each firm within the cell may affect the result of the placebo test. For this exclusion, I use less than 10 firms in an aggregated cell as a criterion.

Third, after conducting the placebo exercise for each control unit, I exclude several placebo results from the figure, namely those with poorly-performed pre-treatment matches of log employment levels between the placebo treatment group and its synthetic control group (i.e., a placebo result which has the large gap of log employments in the pre-treatment periods). This follows a suggestion by Abadie, Diamond, & Hainmueller (2010). The results of placebo tests are reported in Figures 5 and 6.

The solid line is the gap between the log employments of the actual treatment and its synthetic control group, and the dotted lines are the gaps of log employments when I use a control-group unit as if it were the treatment unit. As is evident in Figures 5 and 6, the gap of the log employments between the actual treated group and its synthetic control group shows one of the lowest values in the figures. This supports my result on the negative impact of the anti-sweatshop campaign on employment.

5.3 Synthetic Control Method for Each Firm

The above analysis uses an average firm in the treatment group before implementing the synthetic control method, because the method allows a single treatment unit. Though the method detected the negative employment impact of the anti-sweatshop campaigns, the result might be based an

incorrect "synthetic" control group because I found it after averaging out characteristics of firms in the treatment group. If firms in the treatment group is highly heterogeneous in terms of covariates, their characteristics are averaged out.

Based on the motivation, following Acemoglu et al. (2016), I implement the synthetic control method for each treated firm repeatedly, rather than implementing the method after constructing the average treated firm. The synthetic control group for treated firm i at year t is

$$\hat{l}_{it} = \sum_{j \in \text{control group}} w_j^{i*} l_{jt},$$

where l_{jt} is the log production worker in control firm j in year t, and w_j^{i*} is a weight put on control firm j, obtained by implementing the synthetic control method for treated firm i. These weights are constructed by minimizing the difference of the log of TFP, the age of firms, the log output, the log output growth, the log price growth, and the log employment in 1989, 1990, and 1991. Using these synthetic control groups for multiple treated firms, the effect of anti-sweatshop activism at year t is defined by

$$\hat{\phi}(t) = \frac{\sum_{i \in \text{treatment group}} \frac{l_{it} - \hat{l}_{it}}{\hat{\sigma}_i}}{\sum_{i \in \text{treatment group}} 1/\hat{\sigma}_i} \quad \text{for } t = \{1989, 1990, ..., 1996\},$$

where

$$\hat{\sigma}_i = \sqrt{\frac{\sum_{t \in \text{pre-treatment periods}} (l_{it} - \hat{l}_{it})^2}{T}}.$$

T is the number of years in the pre-treatment periods. $\hat{\phi}(t)$ is the weighted average of the impact of treatment, with the weight being the measure of the quality of matching between treated firm i and its synthetic control firms in the pre-treatment periods, $\hat{\sigma}_i$. Since a better pre-treatment match gives smaller $\hat{\sigma}_i$, the measure of the impact of treatment, $\hat{\phi}(t)$, puts a larger weight on the treated firm with a good pre-treatment match.

Figure 7 shows $\hat{\phi}(t)$ with the actual treated firms from 1989 to 1996. There are two points to mention. First, the measure is close to zero during the pre-treatment periods, suggesting that the synthetic control method successfully constructs the synthetic control group. Second, the measure drops after 1993 dramatically, implying that there is a negative impact of anti-sweatshop activism.

In order to access the validity of the negative result, I implement a placebo test in the following procedure. First, from the control group, I randomly chose 20 firms, the same number as the number of firms in the actual treatment group, as if these were treated firms. Second, I calculate $\hat{\phi}(t)$ for this placebo treatment group and plot it in the same manner as Figure 7. Third, I repeat this procedure 100 times and plot these on the same figure. If there is actually a negative impact of the anti-sweatshop campaigns, then the effect of the treatment on log employment with the true treatment group should be more negative than most of the others with placebo treatment groups. The result is in Figure 8. The thick line shows the effect with the true treatment group and the thin dotted lines are the effects with placebo treatment groups. Figure 9 excludes from Figure 8 several placebo exercises which have $\hat{\phi}(t)$ deviating largely from zero at the point of 1992.

These results confirm the previous result, supporting the negative impact of the anti-sweatshop campaigns on employment.

6 Source of Discrepancy: Omitted Variable Bias

A natural question is what derives the difference between my result and those estimated using a difference in differences approach in literature. Here, my explanation is that their approach does not fully control for unobservable factors. In order to highlight that, I firstly regress the difference in differences regression introduced in equation (1) and obtain results shown in literature. The result is shown in column 1 to 4 of Table 5[7]. I also report other results in Table A7 of Online Appendix D, where FOREXP_i is divided into two separate dummy variables on exporting firms and foreign-owned firms.

In most specifications, the anti-sweatshop campaigns had positive and significant effects on employment changes. These results, combined with the finding that only small firms were likely to exit from the market, lead to the conclusion on no employment effect of the anti-sweatshop activism.

The regression equation introduced above does not include the age for each firm as a control variable, because time-invariant variables are eliminated by taking differences. However, literature on business dynamics shows that start-ups and young firms contribute more proportionally to aggregate employment growth than matured firms (Bravo-Biosca et al., 2013; Haltiwanger et al., 2013; Decker et al., 2014, 2016). In fact, it is firstly shown that the treatment group and the control group in our analysis have different age structures.

Figure 10 shows the average age for each group of firms over years. It shows that the average age is the lowest for exporters and foreign-owned firms in the targeted districts of the anti-sweatshop campaigns, the second lowest for exporters and foreign-owned firms outside the districts, the second highest for non-exporters and non foreign-owned firms in the districts, and the highest for non-exporters and non foreign-owned firms outside the districts. Second, Table 6 shows that younger firms have faster growth in employment than older firms within TFA sectors, as consistent with the business dynamics literature.

These two results imply that exporters and foreign-owned firms in the targeted districts of the anti-sweatshop campaigns experienced larger employment expansions (as reported in columns 1 to 4 of Table 5) partly because they were younger than other firms. Consequently, the inclusion of age variables into the estimation equation as a control should make the magnitude of the key parameter on the interaction term smaller, or even make the parameter statistically insignificant.

For this reason, I additionally incorporate dummy variables for age categories into the estimation equation. YOUNG is a dummy variable for firms with age 0 to 5, MIDDLE for age 6 to 10, and OLD for age 11 to 15, and a remaining category is for firms above age 16[8]. The results are reported in columns 5 to 8 of Table 5. As robustness checks, I get similar results by controlling for the age structure with different specifications which are shown in Tables A5 and A6 of Online Appendix D.

First, as consistent with the result in Table 5, the coefficient on the dummy variable for the younger firms is larger and tends to be more highly statistically significant. Second, the magnitude of the key coefficient, β_3, becomes smaller and in most of the specifications the coefficients become statistically insignificant. This suggests that the large increase in employment by targeted exporters

[7] These results are identical to those in columns 1 to 3 of Table 6B in Harrison & Scorse (2010). They do not run a regression for small firms.

[8] For checking whether the age variable should be included in the estimation equation, I use a LASSO estimator. In particular, as suggested by Belloni, Chernozhukov, & Hansen (2014), I implement the 1st stage of the double selection procedure (i.e., the model selection stage) and see whether age variables are selected in the procedure. In most specifications, an age variable is included as the result of the 1st stage.

and foreign-owned firms from 1990 to 1996, periods before and after the anti-sweatshop campaigns, is mostly explained by the age structure of firms in each group. Third, column 8 actually shows the increase in the magnitude of the key coefficient by including the age variable. This could be because when I focus on small-size firms, there are only two firms in the treatment group (whose ages are 12 and 13, respectively) and both of them are domestic exporting firms. Therefore, in addition to a small sample concern, the coefficient could capture an increase in employment in these domestic exporting firms, which were possibly not affected by the campaigns and hence hired people who could have been hired by foreign treated firms if there were no anti-sweatshop campaigns[9]. See also Table A7 in Online Appendix D where I show results by separating "FOREXP" variable into two separate dummy variables of domestic exporting and foreign-owned exporting firms.

More than the firm's age itself, it seems to suggest that firms in the treatment and control groups are potentially different in terms of unobservable characteristics. Intuitively, subsidiaries of Nike, Adidas, and Reebok or their transaction partners should be different from other exporting firms because these are firms selected by these discerning multinational firms. To see whether the potential difference in unobservables actually biases the estimate, I use the Oster's (2016) methodology, an extension of Altonji, Elder, & Taber (2005), which evaluates the robustness of regression outcomes based on the assumption that the relationship between the treatment and unobservables is recovered from the relationship between the treatment and observables[10].

The bias-adjusted coefficient on the interaction term in equation (1), β_3, is

$$\beta_3 = \tilde{\beta}_3 - \delta(\beta_3^\circ - \tilde{\beta}_3)\frac{(R_{max} - \tilde{R})}{\tilde{R} - R^\circ}, \tag{4}$$

where β_3, $\tilde{\beta}_3$, and β_3° are key parameters (i.e., a coefficient on FOREXP*Treatment in the regression) obtained from a regression with all explanatory variables including both observables and unobservables, with all observable control variables including firm's age, or with control variables without firm's age, respectively[11]. R_{max}, $\tilde{R}$, and R° are R-squareds corresponding to each of these regressions. δ is a parameter on the proportional selection relationship: $\delta\frac{\sigma_{To}}{\sigma_o^2} = \frac{\sigma_{Tu}}{\sigma_u^2}$, where σ_{To} and σ_{Tu} are covariances between treatment variable and observable control variables, and between treatment and unobservable control variables, respectively. σ_o^2 and σ_u^2 are variances of observed and unobserved control variables. Thus, if $\delta = 1$, it means that the unobserved controls are related to the treatment variable with the same extent as the relationship between the observables and the treatment variable.

I derive the bias-adjusted coefficient if unobservables have equal impacts, as observables, on the treatment variable (i.e., $\delta = 1$). For the level of R-squared which can be achieved by a regression with both observable and unobservable controls, R_{max}, I use a value suggested in Oster (2016), $R_{max} = 1.3\tilde{R}$. The result is shown in the second row from the bottom in Table 5. Its robustness

[9]Remember that Treatment$_i$ in our specification is a dummy variable for a firm located in districts where the targeted firms of the anti-sweatshop campaigns had their subsidiaries or transaction partners, due to the data limitation. Therefore, it is possible that some domestic exporting firms which were not related to the anti-sweatshop campaigns have Treatment$_i = 1$.

[10]Gonzaléz & Miguel (2015) use the Oster's (2016) methodology for checking the coefficient stability of the impact of civil war exposure on local collective actions.

[11]The equality in equation (4) holds with an approximation. See Assumptions 1 and 2 in Oster (2016). As for the results derived with Assumptions 1 and 2 (i.e., the restricted estimator), she mentions that "*In about 80% of cases one would draw the correct conclusions about the robustness from the restricted estimator. However, the restricted version generally understates the bias...*" in Section 5.1 of her paper.

is also checked in Table A8 of Online Appendix D1, where I use $R_{max} = 1.25\tilde{R}$ and $R_{max} = 1.1\tilde{R}$. Except for the regression with the sample of small firms, which actually increased their employment due to the anti-sweatshop campaigns, the bias-adjusted coefficients show the large negative impact. This implies that as long as unobserved factors have the same level of impact on the treatment status as observable covariates, there is a decline in employment by 70 to 90 percentage point.

Another related exercise is to derive the value of δ which is required for $\beta_3 < 0$, the negative impact of the anti-sweatshop activism. The obtained values are reported in the last row of Table 5. In the fifth column, it is shown that as long as the $\delta \geq 0.102$, the bias-adjusted coefficient, β_3 becomes negative, implying the negative impact of anti-sweatshop activism on employment.

7 Conclusion

There has been a conflict between a theoretical prediction and empirical findings on the impact of the anti-sweatshop activism on employment. This paper solves the conflict and shows that the anti-sweatshop activism had a negative impact on employment in Indonesia. Using the synthetic control method, it is confirmed that firms in the synthetic control group had much higher employment than firms in the treatment group after the anti-sweatshop campaigns. Then, using the usual difference in differences framework and a recent methodology on the coefficient stability approach, it is shown that the non-negative impact of the anti-sweatshop activism on employment in literature comes from paying less attention to variables such as the age of firms and moreover unobservable differences between treatment and control groups.

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A Table

Table 1: Summary statistics (mean) in 1991

	Control group				Treatment group
	(1)	(2)	(3)	(4)	(5)
	TR=0 FOREXP=0	TR=1 FOREXP=0	TR=0 FOREXP=1	all (1) (2) (3)	TR=1 FOREXP=1
Size	212.59	403.75	588.92	290.52	884.42
Production worker	186.71	350.18	522.26	254.35	794.23
Non production worker	25.88	53.57	66.66	36.17	90.18
log(capital)	18.41	19.50	20.61	18.86	21.26
log(output)	20.30	21.26	22.35	20.70	23.12
age	12.86	10.14	11.18	12.02	5.21
log(material)	19.78	20.63	21.60	20.14	22.51
log(wage for prod. worker)	13.75	13.98	14.17	13.84	14.07
log(wage for non-prod. worker)	14.42	14.81	15.04	14.59	15.16
Δ_{88-91} log(prod. worker)	0.16	0.25	0.40	0.20	0.36
Δ_{88-91} log(non prod. worker)	0.16	0.26	0.48	0.22	0.31
Δ_{88-91} log(capital)	5.76	5.59	6.13	5.74	6.43
Δ_{88-91} log(material)	0.24	0.23	0.38	0.25	0.56
Δ_{88-91} log(output)	0.24	0.27	0.43	0.26	0.65
Observations	666	266	73	1005	65

Notes: Here, I focus on TFA firms being in the dataset both in 1990 and 1996 because I take a difference between 1990 and 1996 in the later analysis. The first column is statistics for domestic firms (i.e., FOREXP $= 0$) in districts without affected firms (i.e., TR $= 0$), the second column is for domestic firms in districts with affected firms (i.e., TR $= 1$), the third column is for exporting or foreign-owned firms (i.e., FOREXP $= 1$) in districts without affected firms, the fourth column is for all firms in control group (i.e., (1)+(2)+(3)), and the fifth column is for exporting and foreign-owned firms in districts with affected firms. Therefore, the fifth group is in the treatment group while the remaining groups (summarized in the fourth column) are in the control group in the following analysis. Capital spending, output, materials, and wages are measured in rupiahs.

Table 2: Pre-treatment average of variables for each group

Variables	Treatment	Synthetic control	Average of all controls
Log(TFP)	3.55	3.33	4.23
Log(output)	23.75	22.93	20.64
Age	7.54	7.07	11.07
Log(output) growth	0.25	0.25	0.10
Log(price) growth	0.04	0.04	0.03
Lon(employment in 1989)	5.99	5.86	4.41

Notes: All variables are averages between 1988 and 1991 in each group. "Age" in 1988 and 1989 is not reported in the dataset. Hence, I made it from the "birth" variable. Log TFP is defined by log output minus a weighted sum of labor, capital, and material inputs with a weight being the cost share of the input.

Table 3: Synthetic control weights

Firm ID	Weights	Firm ID	Weights	Firm ID	Weights	Firm ID	Weights	Firm ID	Weights	Firm ID	Weights
2574	0	11873	0	13031	0.001	20112	0	21524	0	31843	0.001
2591	0	11877	0	13050	0	20121	0	21527	0	31846	0.001
2593	0	11880	0	13056	0	20128	0	21537	0	31848	0.001
2619	0	11881	0	13057	0	20132	0	21547	0	31850	0.002
2620	0	11884	0	13062	0	20148	0	21549	0	31857	0
2622	0	11887	0	13067	0	20191	0	21552	0	31860	0.001
2623	0	11891	0	13076	0	20201	0	21592	0	31866	0
2625	0	11892	0	13079	0	20213	0	21596	0	31882	0.001
2635	0	11895	0	13085	0.097	20223	0.001	21605	0	31885	0.002
2648	0	11902	0.001	13088	0.001	20225	0.001	21606	0.001	31886	0.002
2649	0.001	11907	0.001	13100	0	20229	0.001	21609	0	31887	0.001
2653	0.001	11909	0.001	13102	0.001	20236	0.001	21615	0	33838	0.024
2683	0	11916	0.394	13109	0	20247	0.001	21623	0	34302	0
2684	0	11917	0.001	13112	0.001	20251	0.001	21626	0	34305	0
2685	0	11922	0.029	13115	0	20254	0.001	21815	0	34306	0
3847	0	11925	0.001	13117	0.002	20256	0.001	21819	0	34351	0
3851	0	11926	0.005	13122	0	20257	0.001	23944	0.001	34355	0
3886	0	11984	0	13135	0.001	20258	0.001	23949	0	34362	0
3888	0	12076	0	13137	0.001	20260	0	23950	0	34371	0
4135	0.002	12083	0	13139	0.003	20261	0.001	23953	0		
4138	0.003	12086	0.001	13140	0.001	20270	0.001	23958	0.001		
4860	0	12106	0	13145	0.001	20274	0	23960	0		
6152	0	12113	0	13148	0.065	20275	0.002	23962	0		
6167	0	12130	0	13153	0.001	20279	0.001	23977	0		
6168	0	12135	0	13252	0	20284	0	23986	0		
6183	0	12168	0	13253	0.001	20291	0.001	23989	0		
6185	0	12180	0.001	13254	0.002	20292	0.009	23992	0		
6203	0	12203	0	13262	0	20294	0.003	24013	0		
6294	0	12205	0	13263	0.001	20297	0.003	24021	0		
6319	0	12207	0	13274	0.001	20409	0	24027	0		
6369	0	12230	0	13279	0	20433	0.001	24029	0		
6374	0.001	12239	0	13297	0.001	20514	0	24040	0.001		
6382	0	12241	0	13362	0	20522	0	24057	0.002		
6387	0	12244	0	13374	0	20556	0	24061	0		
6388	0.001	12250	0	13384	0	20586	0	24140	0.001		
6390	0	12258	0	13398	0	20640	0	27631	0		
6394	0.002	12261	0	13438	0	20659	0	27634	0		
6419	0	12271	0	13444	0	20660	0	27649	0.001		
6427	0	12272	0.001	13464	0	20681	0	27663	0.001		
6431	0	12273	0.001	13466	0	20697	0	27668	0		
6440	0	12294	0	13489	0.001	20767	0	27685	0		
6445	0	12318	0	13493	0	20815	0	27737	0		
6463	0	12323	0	13495	0	20833	0	27751	0		
6481	0.017	12329	0	13503	0.001	20844	0	27781	0.001		
6486	0.001	12342	0	13504	0.001	20857	0	27793	0.001		
6491	0.001	12350	0	13510	0.001	20859	0	27817	0		
6497	0	12354	0.001	13518	0.001	20865	0	27823	0		
6511	0.001	12372	0	13521	0	20866	0	27844	0		
6523	0	12379	0	13522	0	20908	0	27845	0		
6524	0	12383	0	13527	0	20923	0	27846	0.001		
6526	0	12385	0	13530	0	20925	0	27875	0		
6583	0	12397	0	13539	0	20927	0	27892	0.001		
6587	0	12398	0	13542	0	20928	0	27895	0		
6632	0	12413	0.001	13548	0.002	20930	0	27901	0.001		
6655	0	12422	0	13552	0	20932	0	27903	0.001		
6691	0	12425	0	13555	0.001	20934	0	27907	0.001		
6745	0	12427	0.001	13563	0	20936	0	28051	0		
6749	0.001	12442	0.001	13574	0.001	21005	0	28054	0		
6773	0.001	12452	0.001	13578	0	21085	0	28097	0		
6776	0	12457	0.001	13590	0.001	21089	0.001	28100	0		
6802	0.001	12458	0.003	13591	0	21113	0	28116	0		
6803	0.002	12472	0	13595	0	21114	0	28118	0		
6804	0	12485	0.001	13600	0	21116	0	28119	0		
6807	0.001	12490	0.001	13609	0	21123	0.001	28120	0		
6814	0.001	12501	0.002	13610	0	21141	0	28121	0		
6825	0	12503	0.001	13622	0.001	21153	0	28133	0		
6827	0	12505	0.001	13623	0.001	21154	0	28139	0		
6828	0.001	12514	0.003	13627	0.001	21170	0	28141	0.001		
6846	0.001	12527	0.001	13629	0	21172	0	28171	0		
6849	0.001	12528	0.001	13638	0.001	21175	0	28192	0		
6852	0	12533	0.001	13650	0	21178	0.001	28201	0		
6855	0	12535	0.002	13669	0.002	21179	0	28231	0		
6873	0.001	12536	0.001	13682	0	21181	0.001	28244	0		
6889	0.001	12542	0.001	13684	0.001	21184	0	28249	0		
6891	0.001	12543	0.002	13689	0.001	21271	0	28267	0.002		
6911	0.001	12547	0.001	13692	0	21291	0	28280	0		
6915	0	12552	0.002	13703	0.002	21292	0	28284	0.001		
6938	0	12553	0.001	13708	0.003	21304	0	28288	0		
6967	0	12566	0.002	13712	0	21331	0	28298	0.001		
6990	0.001	12567	0.008	13715	0	21335	0	28301	0.001		
6997	0.002	12569	0.001	13718	0	21344	0	28304	0.003		
7003	0.0041	12573	0.003	13721	0.001	21391	0	28308	0		
7034	0.001	12578	0.003	13722	0.002	21406	0	28457	0		
7050	0.001	12722	0	13730	0.002	21407	0	28530	0		
7069	0.005	12742	0	13732	0	21414	0	28533	0		
7080	0.002	12746	0.001	13744	0.049	21429	0.001	28539	0.001		
7081	0	12760	0.001	13746	0.001	21461	0	28541	0		
7083	0.017	12814	0	14164	0.001	21470	0.001	28544	0		
7084	0.001	12824	0.001	14172	0	21472	0	28561	0.006		
7097	0	12849	0	14176	0.001	21473	0	31708	0		
7099	0	12858	0	14192	0	21475	0	31794	0		
7910	0	12867	0	19866	0	21477	0	31808	0		
7934	0	12868	0.001	19867	0	21479	0	31811	0		
7941	0.001	12880	0	19869	0	21488	0.001	31813	0.001		
7958	0	12952	0	19870	0	21494	0	31817	0.001		
7965	0.001	12956	0.001	19873	0	21495	0	31820	0		
7967	0.001	12986	0.002	19874	0	21497	0	31830	0		
7968	0	12990	0.002	19971	0	21504	0	31833	0.001		
11869	0	12996	0	20017	0	21511	0	31838	0.001		
11872	0	13026	0.001	20069	0	21514	0	31841	0.001		

Notes: These are the firms only in TFA sectors and reported every year in the dataset from 1988 to 1996.

Table 4: Difference-in-differences with and without synth weights

	Without weights	With weights	
	(1) OLS	(2) OLS	(3) FE
after	0.166***	0.711***	0.711***
	(0.047)	(0.0572)	(0.0572)
FOREXPTR	1.619***	-0.258***	
	(0.184)	(0.0435)	
after*FOREXPTR	0.247***	-0.298***	-0.298***
	(0.047)	(0.0572)	(0.0572)
Observations	4680	1377	1377

Note: The sample is composed of all TFA firms surviving from 1988 to 1996. "after" is a dummy variable equal to one if the period is after 1992. FOREXPTR is a dummy variable if a firm is in the treatment group (i.e., FOREXP= 1 and Treatment= 1). Column 1 is a result obtained without the synthetic weights, and columns 2 and 3 are results with the weights. The number of observations are different across columns because some firms have zero in their synthetic weights.

Robust standard errors in parentheses are clustered at the province level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Change in log employment from 1990 to 1996 with and without age category variables

	Without age dummies				With age dummies			
	(1) all TFA	(2) no min	(3) large	(4) small	(5) all TFA	(6) no min	(7) large	(8) small
FOREXP	0.044	0.074	-0.012	-0.077	0.048	0.081	0.001	-0.087
	(0.026)	(0.031)**	(0.020)	(0.090)	(0.032)	(0.037)**	(0.020)	(0.096)
Treatment	0.006	0.011	-0.031	0.049	-0.008	-0.0003	-0.041	0.035
	(0.036)	(0.033)	(0.034)	(0.027)*	(0.026)	(0.024)	(0.020)*	(0.026)
FOREXP* Treatment	0.156	0.125	0.162	0.177	0.095	0.063	0.077	0.192
	(0.054)**	(0.049)**	(0.050)***	(0.091)*	(0.059)	(0.058)	(0.058)	(0.098)*
Δ Min Wage	-0.179		-0.116	-0.237	-0.191		-0.144	-0.231
	(0.045)***		(0.019)***	(0.091)***	(0.041)***		(0.021)***	(0.060)***
MIDDLE					0.155	0.145	0.192	0.109
					(0.032)***	(0.032)***	(0.050)***	(0.053)*
OLD					0.055	0.055	0.096	0.045
					(0.017)***	(0.020)**	(0.024)***	(0.038)
Observations	1123	1123	535	588	1123	1123	535	588
R^2	0.4695	0.4629	0.5409	0.3380	0.4789	0.4714	0.5542	0.3439
Bias-adjusted β_3 ($\delta = 1$)	–	–	–	–	-0.767	-0.985	-0.986	0.454
δ for $\beta_3 < 0$	–	–	–	–	0.102	0.061	0.073	-0.732

Notes: The sample is composed of all firms surviving from 1990 to 1996 in TFA sectors. TFA denotes textile, footwear, and apparel sectors. "no min" denotes a regression without a variable on the change in minimum wages. "large" denotes a regression only for firms with more than 99 employees, while "small" denotes that for less than 100 employees. Columns (1) to (4) are without the age category variable, while (5) to (8) are with it. Δ Min Wage is the change of the minimum wage in the region where the firm is located. MIDDLE is a dummy for firms with age 6 to 10, and OLD for age 11 to 15, at the point of 1996. YOUNG, a dummy variable for a firm with age 0 to 5, is not reported here, because by construction there should be no observations for the category. "Bias-adjusted β_3 ($\delta = 1$)" is the bias-adjusted treatment coefficient calculated by equation (4). "δ for $\beta_3 < 0$" is the magnitude of δ – the relationship between the covariance of treatment and unobservables and that of treatment and observables– required for making β_3 negative.

Robust standard errors in parentheses are clustered at the province level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Average change in log employment

age	log employment change	t-statistic
0 to 5	0.086	16.535
6 to 10	0.027	5.368
11 to 15	0.011	2.059
above 16	-0.003	-0.775

Notes: These are the averages of log employment growth for different age categories over periods between $t-1$ and t. t is from 1991 to 1996.

B Figure

Figure 1. The impact of anti-sweatshop campaigns

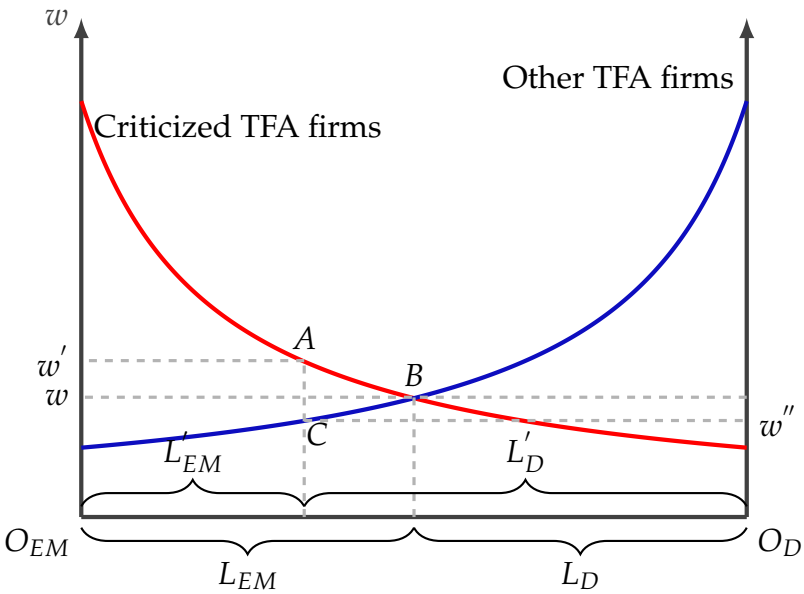


Figure 2. Indonesian map on districts with positive TFA firms (sample used in my analysis)

Notes: This is the map using the restricted sample. Districts with positive observations of TFA firms are filled with colors. Within these, districts with firms in the treatment group (i.e., Nike, Adidas, and Reebok) are colored with black.

Figure 3. Log employment profiles: treatment vs control groups

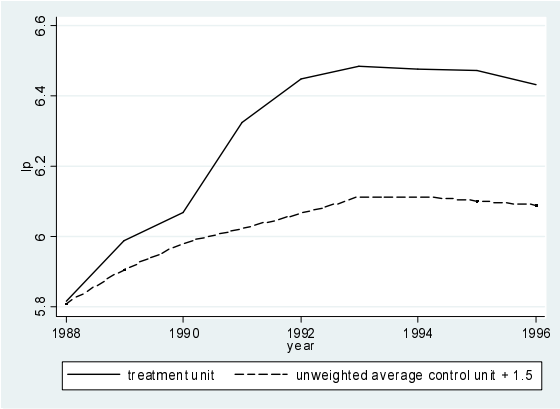


Figure 4. Log employment profiles: treatment vs synthetic control groups

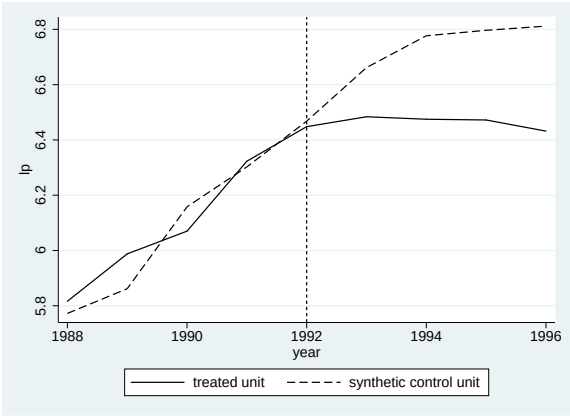


Figure 5. Gap of log employment: actual treatment and placebo groups (with adjustments 1 and 3)

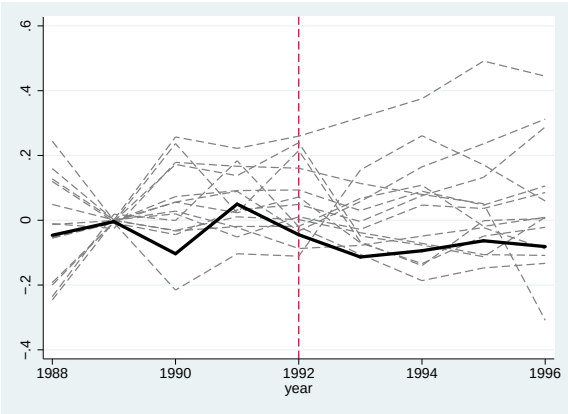


Figure 6. Gap of log employment: actual treatment and placebo groups (with adjustments 1, 2, and 3)

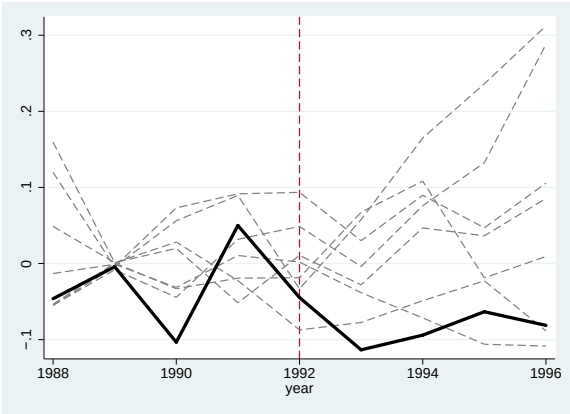


Figure 7. The impact of treatment

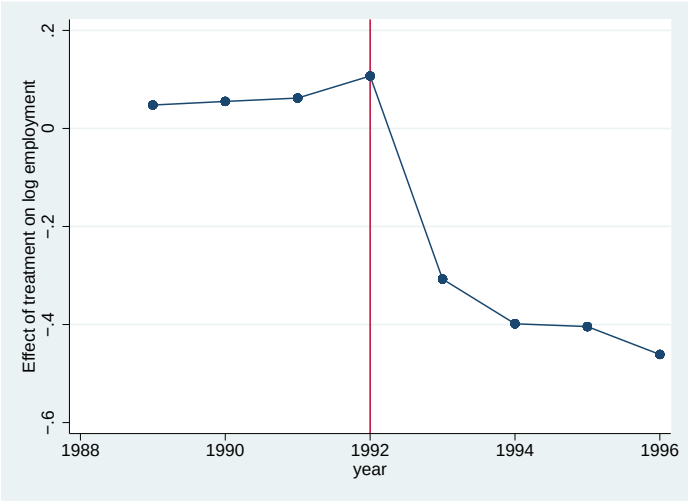


Figure 8. Firm level placebo test 1

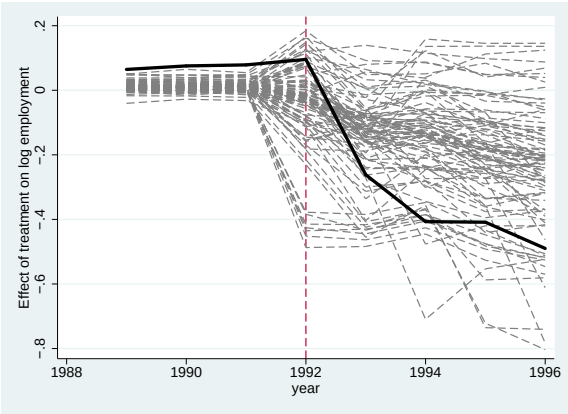


Figure 9. Firm level placebo test 2

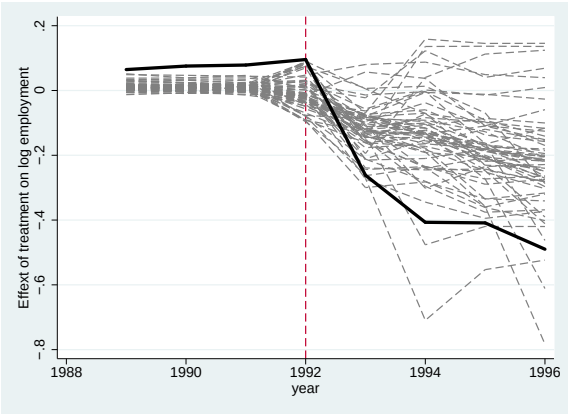
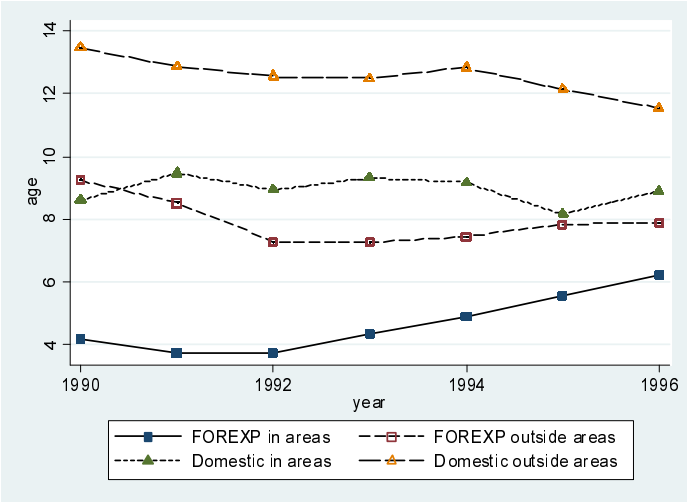


Figure 10. Average age for each group



Notes: "FOREXP in areas" is average age for exporters and foreign-owned firms in the targeted areas, "FOREXP outside areas" for exporters and foreign-owned firms outside the areas, "Domestic in areas" for non exporters and non foreign-owned firms inside the areas, and "Domestic outside areas" for non exporters and non foreign-owned firms outside the areas.

Online Appendix: The Impact of Anti-Sweatshop Activism on Employment

November 11, 2018

This is the online appendix for the paper, " The Impact of Anti-Sweatshop Activism on Employment."

A The Number of News Articles with the Word "sweatshop"

Figure A1 shows the number of news articles from 1988 to 1996. The data are from Dow Jones Factiva database, in which I search the number of news articles with the word "sweatshop" by focusing the source of these articles on Major News and Business Resources such as The New York Times and Reuters News. As you can see in Figure A1, the number of articles starts to rise in 1993, which supports my choice of 1992 as the beginning of anti-sweatshop activism in Indonesia. In Figures A2 and A3, I further show the number of news articles but using "sweatshop" and "Indonesia", and "Nike" and "Indonesia" as key words, respectively. Both figures have similar rises in the number of articles in 1992 or 1993.

Figure A1. The number of news articles with the word "sweatshop"

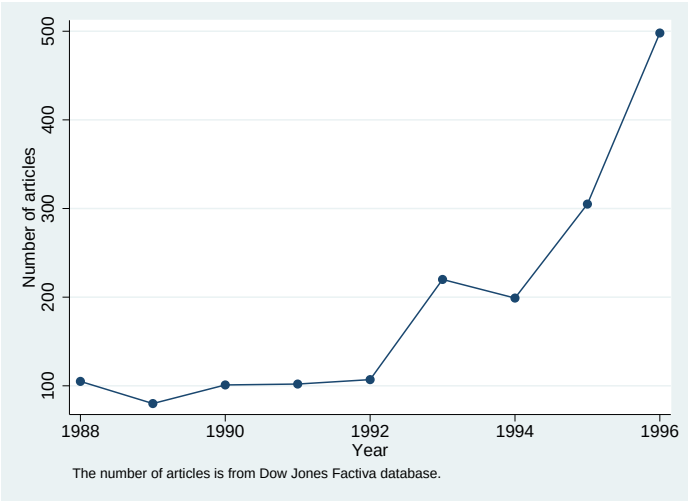


Figure A2. With "sweatshop" & "Indonesia"

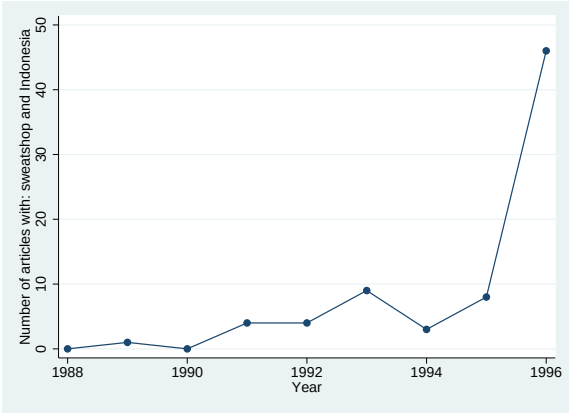
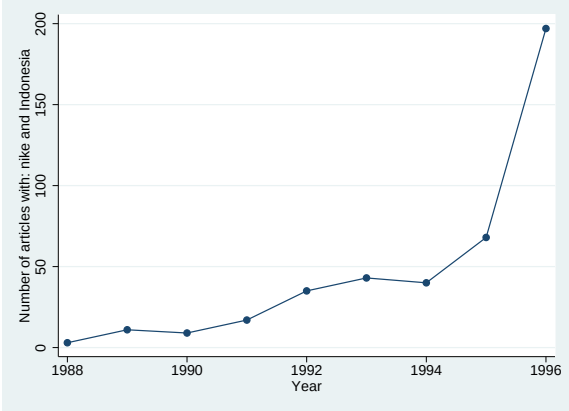


Figure A3. With "Nike" & "Indonesia"



B Additional Results on Robustness of the Synthetic Control Method

First, I show in Table A1 that the trends of log employment in the treatment and un-weighted control groups during pre-treatment periods (i.e., trends shown in Figure 3) are statistically different. Second, in Subsections B.2, I show seven other specifications of the synthetic control method for checking its robustness. I chose these seven specifications because these give good fits of variables in the pre-treatment periods between treatment and synthetic control groups. As you see in the subsection, the difference of log employment between treatment and synthetic control groups provides a similar pattern as that in Section 5. In addition, the difference in differences regression with obtained synthetic weights gives a negative coefficient on the interaction term, and many of them are statistically significant or close to being significant.

B.1 The difference of trends in pre-treatment periods

Table A1: The difference in growth of log employment

Variables	Treatment	unweighted control	t-statistic
Δlog(employment) 89-88	0.172	0.097	-6.606
Δlog(employment) 90-88	0.254	0.171	-4.958
Δlog(employment) 91-88	0.507	0.214	-14.586

Notes: These are the average growth of log employment between two periods during the pre-treatment periods.

B.2 Robustness on the Result from the Synthetic Control Method

Table A2: Pre-treatment average of variables for each group (specifications 1-6)

Variables used	Treatment	Synthetic control	Average of all controls
Specification 1			
Log(TFP)	3.55	3.69	4.23
Age	7.54	7.92	11.07
Log(output) growth	0.25	0.25	0.10
Log(price) growth	0.04	0.04	0.03
Lon(employment in 1989)	5.99	6.07	4.41
Specification 2			
Log(TFP)	3.55	3.80	4.23
Log(output) growth	0.25	0.25	0.10
Lon(employment in 1989)	5.99	6.10	4.41
Specification 3			
Log(TFP)	3.55	3.70	4.23
Age	7.54	7.89	11.07
Lon(employment in 1989)	5.99	6.03	4.41
Lon(employment in 1991)	6.32	6.36	4.52
Specification 4			
Log(TFP)	3.55	3.69	4.23
Lon(employment in 1989)	5.99	6.04	4.41
Lon(employment in 1991)	6.32	6.37	4.52
Specification 5			
Lon(employment in 1988)	5.82	5.85	4.31
Lon(employment in 1989)	5.99	6.02	4.41
Lon(employment in 1991)	6.32	6.35	4.53
Specification 6			
Log(TFP)	3.55	3.31	4.23
Log(output)	23.75	22.86	20.64
Log(minwage)	13.44	12.79	13.46
Age	7.54	6.98	11.07
Log(output) growth	0.25	0.25	0.10
Log(price) growth	0.04	0.04	0.03
Lon(employment in 1989)	5.99	5.84	4.41

Notes: All variables are averages between 1988 and 1991 in each group. "Age" in 1988 and 1989 is not reported in the dataset. Hence, I made it from the "birth" variable. Log TFP is defined by log output minus a weighted sum of labor, capital, and material inputs, with a weight being the cost share of the input.

Table A3: Difference-in-differences with synth weights (specificatioons 1-6)

Specification	(1)	(2)	(3)	(4)	(5)	(6)
after	0.509**	0.500**	0.667**	0.667**	0.617***	0.708***
	(0.180)	(0.182)	(0.187)	(0.186)	(0.138)	(0.067)
FOREXPTR	0.110	0.165	0.132	0.128	0.087	-0.269
	(0.798)	(0.838)	(0.532)	(0.524)	(0.718)	(0.158)
after*FOREXPTR	-0.096	-0.087	-0.254	-0.254	-0.204	-0.295***
	(0.180)	(0.182)	(0.187)	(0.186)	(0.138)	(0.067)
Observations	4203	4464	4284	4284	4185	1314

Note: The sample is composed of all TFA firms surviving from 1988 to 1996. "after" is a dummy variable equal to one if the period is after 1992. FOREXPTR is a dummy variable if a firm is in the treatment group (i.e., FOREXP= 1 and Treatment= 1). Observations are different across columns because some firms have 0 in their synthetic weights. Robust standard errors in parentheses are clustered at the province level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure A4. Treated vs synthetic control (specification 1)

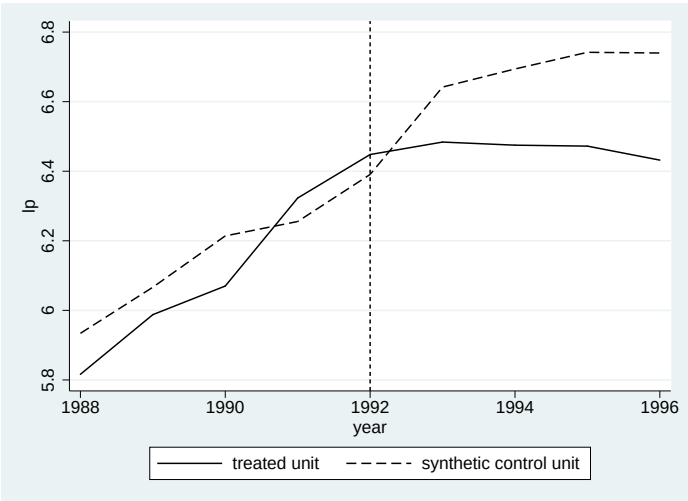


Figure A5. Treated vs synthetic control (specification 2)

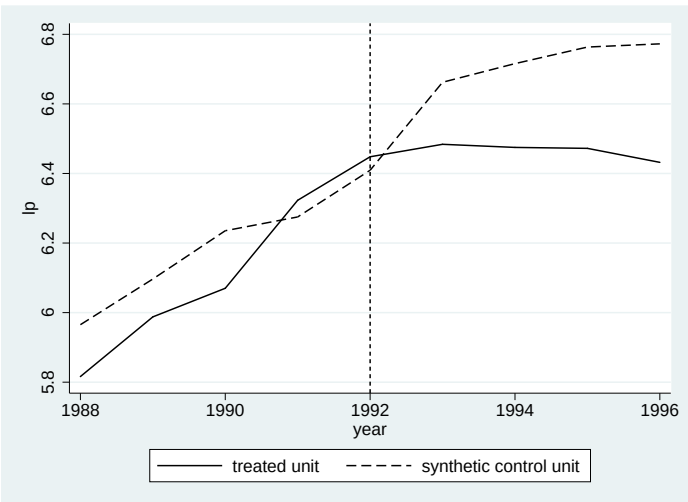


Figure A6. Treated vs synthetic control (specification 3)

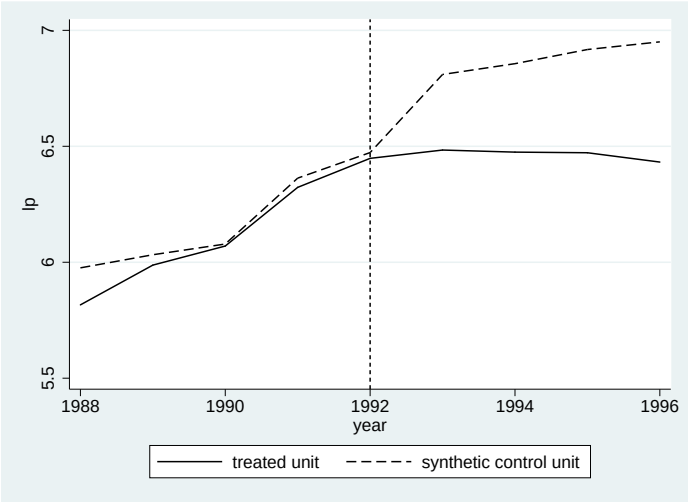


Figure A7. Treated vs synthetic control (specification 4)

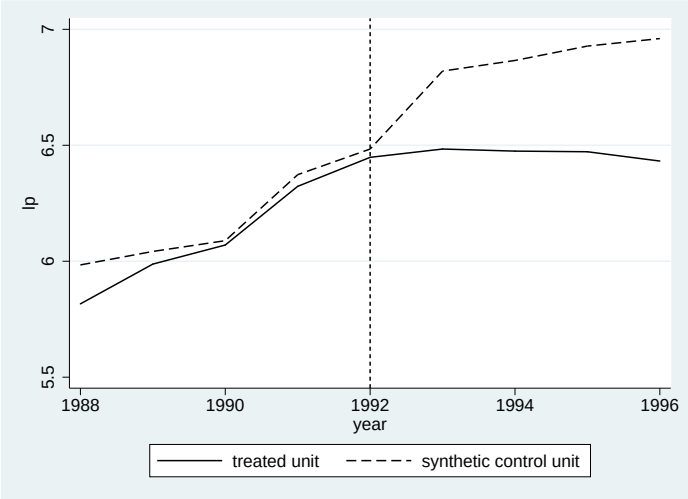


Figure A8. Treated vs synthetic control (specification 5)

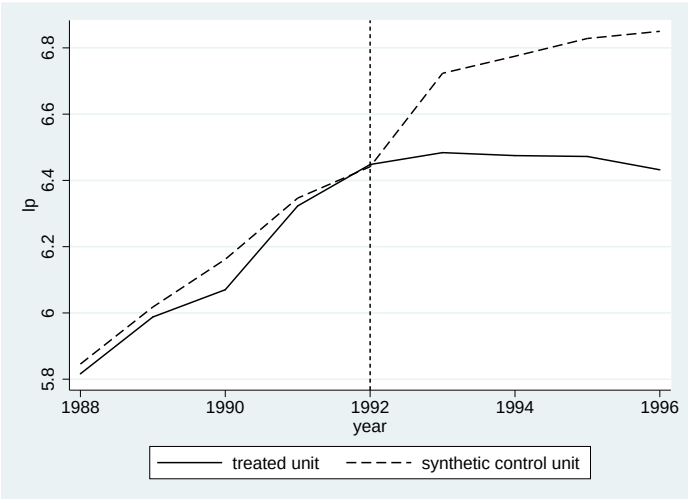
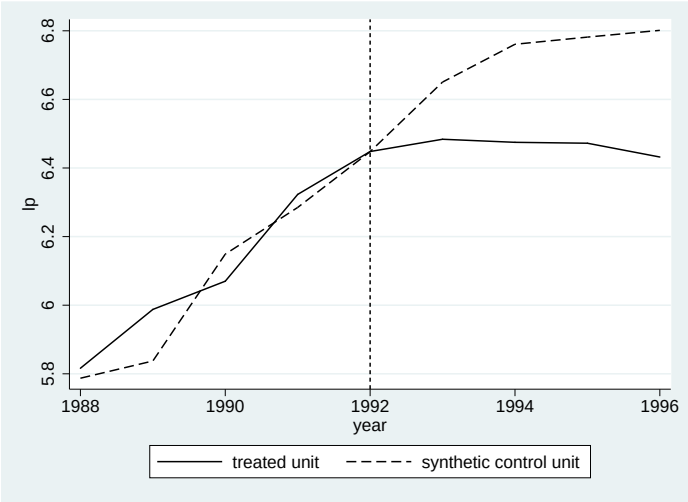


Figure A9. Treated vs synthetic control (specification 6)



C Additional Explanation on the Placebo Test

This section additionally explains a procedure of the placebo test implemented in Section 5.2. As I mentioned, I aggregate individual firms in each province into an average domestic firm and an average exporting or foreign-owned firm, respectively. You can see this aggregation in Table A4. For example, as written in columns 1 and 2, there are 68 domestic firms and 8 exporting or foreign-owned firms in province 31 before the aggregation. After the aggregation, these become one domestic firm and one exporting-foreign-owned firm, as shown in columns 3 and 4 of the same row. This is the first adjustment conducted in the placebo test.

Table A4: The number of firms before and after averaging out

	Before averaging		After averaging	
	(1) no-FOREXP	(2) FOREXP	(3) no-FOREXP	(4) FOREXP
12	15	0	1	0
13	4	0	1	0
14	0	12	0	1
16	1	0	1	0
31	68	8	1	1
32	180	15	1	1
33	123	3	1	1
34	18	2	1	1
35	46	8	1	1
51	11	11	1	1
71	1	0	1	0
73	6	1	1	1
Treared	0	20	0	1

Notes: The number in the 1st column is province ID. In the last row, "Treated" is the number of firms which are exported or foreign-owned firms in the targeted regions, each of these is not included in the numbers of the remaining column,

The second adjustment is that I omit from the analysis several averaged firms which are constructed from the small number of firms. In particular, I omit the average domestic firms in provinces 13, 16, and 71, and average exporting-foreign-owned firms in provinces 33, 34, 35, and 73.

The third adjustment is that I exclude a placebo result from the figure if the result shows a poor fit in the pre-treatment periods. In particular, I eliminate a placebo result if the gap of the log employment between treatment and synthetic control units deviates more than 0.18 (or -0.18 if it's negative) from zero in a year during the pre-treatment periods.

D Additional Results on Omitted Variable Bias

Tables A5 and A6 show the regression results of equation (1), but separating the sample into several sub-categories (i.e., all firms in columns 1 to 4, only large firms in columns 5 to 8 of Table A5, and only small firms in columns 1 to 4 of Table A6). Within these, each column is a result with or without age category variables, or a result focusing on a subsample of young or old firms respectively. As you can see in columns 4 and 8 of Table A5, firms with an age more than 10 have the negative impact of the sweatshop campaigns.

Table A5: Change in log employment from 1990 to 1996 with and without age category variables

	all firms				**large firms (# employee>99)**			
	(1) w/ age C.	(2) w/o age C.	(3) age 6-10	(4) age >10	(5) w/ age C.	(6) w/o age C.	(7) age 6-10	(8) age >10
FOREXP	0.048	0.044	-0.319	0.127	0.001	-0.012	-0.473	0.123
	(0.032)	(0.026)	(0.056)***	(0.038)**	(0.020)	(-0.60)	(0.111)**	(0.043)**
Treatment	-0.007	0.006	0.029	-0.012	-0.041	-0.031	-0.064	-0.028
	(0.026)	(0.036)	(0.012)**	(0.047)	(0.020)*	(0.034)	(0.033)*	(0.050)
FOREXP*	0.095	0.156	0.469	-0.063	0.077	0.162	0.589	-0.081
Treatment	(0.059)	(0.054)**	(0.062)***	(0.031)*	(0.058)	(0.050)**	(0.111)***	(0.036)**
Δ Min Wage	-0.191	-0.179	-0.263	-0.172	-0.144	-0.116	-0.456	-0.047
	(0.041)***	(0.045)***	(0.057)***	(0.061)**	(0.021)***	(0.019)***	(0.054)***	(0.020)**
MIDDLE	0.155				0.192			
	(0.032)***				(0.050)**			
OLD	0.055				0.096			
	(0.017)**				(0.024)***			
Observations	1123	1123	301	818	535	535	160	371

Notes: The sample is composed of all firms surviving from 1990 to 1996 in TFA sectors. TFA denotes textile, footwear, and apparel sectors. "w/ age C." is a regression with age category dummy variables, and "w/o age C." is one without it. "age 6-10" is a regression with young (i.e., age 6 to 10) firms, while "age >10" is one with middle and old aged (i.e., more than age 10) firms. Columns 1 to 4 are regressions with all firms, while (5) to (8) are those only with large firms. Δ Min Wage is the change of the minimum wage in the region where the firm is located. MIDDLE is a dummy for firms with age 6 to 10, and OLD for age 11 to 15, at the point of 1996. YOUNG, a dummy variable for a firm with age 0 to 5, is not reported here, because by construction there should be no observations for the category.
Robust standard errors in parentheses are clustered at the province level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A7 shows the regression results of regression equation (1), but separating "FOREXP" variable into two mutually exclusive separate dummy variables. In particular, I define dummy variable "DOMEXP", which is one if a firm is exporting but not owned by a foreign firm. Similarly, I re-define "FOREXP" as a dummy variable which is one if a firm both exports and is owned by a foreign firm. In columns 7 and 8 of Table A7, results on coefficient "FOREXP" and "FOREXP*Treatment" are not reported because there are no foreign-owned exporting firms with less than 100 employees in our sample.

Table A6: Change in log employment from 1990 to 1996 with and without age category variables

	small firms (# employee <100)			
	(1) w/ age C.	(2) w/o age C.	(3) age 6-10	(4) age >10
FOREXP	-0.087	-0.077	-0.308	-0.036
	(0.096)	(0.090)	(0.202)	(0.112)
Treatment	0.035	0.049	0.104	0.009
	(0.026)	(0.027)	(0.050)*	(0.045)
FOREXP* Treatment	0.192	0.177		0.234
	(0.098)*	(0.091)*		(0.120)*
Δ Min Wage	-0.231	-0.237	0.193	-0.293
	(0.060)**	(0.091)**	(0.087)**	(0.070)**
MIDDLE	0.109			
	(0.053)			
OLD	0.045			
	(0.038)			
Observations	588	588	141	447

Notes: The sample is composed of all firms surviving from 1990 to 1996 in TFA sectors. TFA denotes textile, footwear, and apparel sectors. "w/ age C." is a regression with age category dummy variables, and "w/o age C." is one without it. "age 6-10" is a regression with young (i.e., age 6 to 10) firms, while "age >10" is one with middle and old aged (i.e., more than age 10) firms. Columns 1 to 4 are regressions with small firms. The coefficient on "FOREXP*Treatment" in column 3 is missing, because the sample is too small to estimate. Δ Min Wage is the change of the minimum wage in the region where the firm is located. MIDDLE is a dummy for firms with age 6 to 10, and OLD for age 11 to 15, at the point of 1996. YOUNG, a dummy variable for a firm with age 0 to 5, is not reported here, because by construction there should be no observations for the category.
Robust standard errors in parentheses are clustered at the province level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

As can be seen in Table A7, domestic exporting firms in the treatment districts experience a positive and significant change in employment without the age variable, and a positive but insignificant change in employment with the age variable. On the other hand, foreign-owned exporting firms in the treatment districts experience a negative change in employment in the most specifications and coefficients reported are close to being statistically significant. This could be because the group of firms with "FOREXP*Treatment = 1" is a more accurate measure of treatment than "DOMEXP*Treatment = 1". In addition, within firms with "DOMEXP*Treatment" being one, some firms could be unrelated with Nike, Adidas, and Reebok, and therefore they might be able to additionally hire workers who could have been hired by the treated companies (from the perspectives of people who could have been hired by the treated companies, these "DOMEXP*Treatment" companies could be a good alternative, because they are located in the same districts and these exporting companies are likely to offer higher wages than other domestic companies in the districts).

D.1 Additional Results on the Bias-Adjusted Coefficients

Table A8 shows results on the bias-adjusted coefficients with different levels of R_{max}. Columns 1 to 4 are the results with $R_{max} = 1.25\tilde{R}$ and columns 5 to 8 are those with $R_{max} = 1.1\tilde{R}$. As can be seen

Table A7: Change in log employment from 1990 to 1996 with and without age category variables

	all firms		**no min**		**large**		**small**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DOMEXP	0.078	0.072	0.085	0.080	0.067	0.065	-0.077	-0.087
	(0.056)	(0.062)	(0.059)	(0.064)	(0.041)	(0.052)	(0.090)	(0.096)
FOREXP	0.042	0.093	0.119	0.172	-0.101	-0.031	–	–
	(0.185)	(0.192)	(0.182)	(0.191)	(0.147)	(0.150)		
Treatment	0.014	-0.001	0.019	0.005	-0.013	-0.029	0.049	0.035
	(0.034)	(0.025)	(0.032)	(0.023)	(0.032)	(0.022)	(0.027)*	(0.026)
DOMEXP*	0.156	0.104	0.135	0.082	0.137	0.065	0.177	0.192
Treatment	(0.076)*	(0.088)	(0.073)*	(0.086)	(0.064)*	(0.083)	(0.091)*	(0.098)*
FOREXP*	0.059	-0.068	-0.007	-0.128	0.093	-0.071	–	–
Treatment	(0.168)	(0.160)	(0.164)	(0.156)	(0.137)	(0.153)		
Δ Min Wage	-0.188	-0.199			-0.123	-0.152	-0.237	-0.231
	(0.040)***	(0.036)***			(0.022)***	(0.018)***	(0.064)***	(0.060)***
MIDDLE		0.157		0.147		0.200		0.109
		(0.034)***		(0.034)***		(0.052)***		(0.053)*
OLD		0.053		0.053		0.092		0.045
		(0.019)**		(0.022)**		(0.025)***		(0.038)
Observations	1123	1123	1123	1123	535	535	588	588

Notes: The sample is composed of all firms surviving from 1990 to 1996 in TFA sectors. TFA denotes textile, footwear, and apparel sectors. "no min" denotes a regression without a variable on the change in minimum wages. "large" denotes a regression only for firms with more than 99 employees, while "small" denotes that for less than 100 employees. Columns 1 and 2 are regressions with all firms, columns 3 and 4 with all firms without including "minimum wage growth" as an independent variable. Columns 5 and 6 are regressions only with large firms, and columns 7 and 8 are only with small firms. "DOMEXP" is the dummy variable for firms not owned by a foreign firm but exporting. "FOREXP" is the dummy variable for firms both exporting and owned by a foreign firm. Δ Min Wage is the change of the minimum wage in the region where the firm is located. MIDDLE is a dummy for firms with age 6 to 10, and OLD for age 11 to 15, at the point of 1996. YOUNG, a dummy variable for a firm with age 0 to 5, is not reported here, because by construction there should be no observations for the category. Robust standard errors in parentheses are clustered at the province level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

in the table, these results are qualitatively the same as those in Table 4.

Table A8: Bias-adjusted estimate: different levels of R_{max}

	$R_{max} = 1.25\tilde{R}$				$R_{max} = 1.1\tilde{R}$			
	(1) all TFA	(2) no min	(3) large	(4) small	(5) all TFA	(6) no min	(7) large	(8) small
Bias-adjusted β_3 ($\delta = 1$)	-0.682	-0.797	-0.808	0.411	-0.231	-0.281	-0.277	0.279
δ for $\beta_3 \leq 0$	0.122	0.073	0.087	-0.878	0.306	0.183	0.271	-2.196

Notes: Columns 1 to 4 are calculated with $R_{max} = 1.25\tilde{R}$ and columns 5 to 8 with $R_{max} = 1.1\tilde{R}$. "Bias-adjusted β_3 ($\delta = 1$)" is the bias-adjusted treatment coefficient calculated by equation (4). "δ for $\beta_3 \leq 0$" is the magnitude of δ – the relationship between the covariance of treatment and unobservables and that of treatment and observables– required for making β_3 negative. Robust standard errors in parentheses are clustered at the province level.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$