

Cascades of Tax Policy through Production Networks: Evidence from Japan

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Abstract

This paper estimates the indirect effects on firm performance of tax incentives for investment through production networks. It exploits the quasi-experimental event of an investment stimulus policy targeting small and medium enterprises, along with unique proprietary data of supply chains in Japan. I find that the indirect effects on large direct suppliers might surpass the direct effects, while no discernible effects are found on downstream firms. The findings question the political basis for size-dependent policies on firms.

Keywords: Spillover Effects of Tax Policies, Bonus Depreciation, Production Networks

JEL Codes: F14, H25, R15

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1 Introduction

The effectiveness of tax policies targeting firms has been evaluated conventionally based on the effects on the firms that are directly affected by the tax policies. However, the indirect effects through the supply chains of the affected firms can be of first-order importance. In particular, this paper focuses on an investment stimulative tax policy. An increase in output, induced by an increase in capital investment following the tax policy, must be followed by a greater demand for the inputs of production (e.g., screws required for assembling a car), leading to potential upstream propagation. Simultaneously, an increase in the supply of the directly affected firms' output could drive down the market price of their products, potentially benefiting downstream firms as well. This paper estimates these indirect effects, exploiting a quasi-experimental event of an investment stimulus and unique proprietary data of supply chains in Japan.

The policy I study, "bonus" depreciation for small and medium enterprises (SMEs), allows SMEs to deduct an additional percentage of capital expenditures in the first year of an asset's tax life. Although this tax policy was not directed at particular industries, there is a variation in the degree of policy benefits emerging from the discounting factor and the fact that longer-lived assets experience a larger reduction in the present value cost of investment. This is because bonus depreciation accelerates deductions from further in the future. Therefore, those firms in the industries that tend to use such longer-lived assets are affected by the policy more than other firms. Using this quasi-experimental variation in the treatment intensity, I define industries with greater policy exposures as treated industries and label those firms which belong to these industries as treated firms. Exploiting the firm-level input-output linkage data, I construct the supply chains of the treated firms and investigate the spillover effects of the tax policy.

Exploiting the natural experimental setting and unique data set, I found statistically significant and economically meaningful direct *and* indirect effects of tax incentives for investment. The extensive micro-data on inter-firm transactions allow me to trace and

quantify the extent of cascades along supply chains. Specifically, I observed that SMEs among the direct suppliers experience a sales increase that is half the magnitude of the treatment effects. Additionally, there is weak evidence suggesting that the estimate for the average direct suppliers that are larger in size than the average treated firms is even greater than the estimate for the treated firms.¹

On the other hand, in contrast to the literature indicating significant negative effects of a natural disaster on downstream firms, I do not find statistically significant effects on the downstream firms. As a potential channel behind this result, treated firms increased the number of direct customers following the policy change. This increase might have occurred through the displacement of market share previously held by large firms in the same industries and markets, which were ineligible for the policy, leading to the absence of downstream effects.

These findings are developed in two steps. The first stage is the estimation of direct effects. [Zwick and Mahon \(2017\)](#) (hitherto, ZM) estimated the direct effects of bonus depreciation in the U.S. by comparing the performance of firms in industries that, on average, invest in long-lived assets with those in industries that invest in short-lived assets. Their exposure measure is at the industry level. For this study, I apply to Japan their established exposure measure. Despite a difference in class life definition between the U.S. and Japan, a manual check of all listed class lives of equipment between the two countries reveals a high positive correlation, roughly at 0.7. To mitigate potential measurement errors arising from the discrepancies, and to construct the supply chain of the treated firms, I follow [Garrett et al. \(2020\)](#) in constructing a binary treatment variable. This approach ensures that as long as Japanese and U.S. firms tend to purchase similar equipment within the same industry, the policy measure I employ captures the direct effects of the policy.

However, since I hypothesize the presence of spillovers through production networks,

¹Note that I cannot formally test that the difference between the average direct suppliers and small direct suppliers and the difference between the spillover effects and direct effects are statistically significantly different from zero, due to the lack of a theory for heterogeneous analyses within conditional difference-in-differences methods.

simply using all the firms in industries that tend to invest in short-lived assets as the control group would contaminate the estimate. Then, to overcome this issue, I follow [Carvalho et al. \(2021\)](#). Based on a theoretical prediction that shock propagation decays as subjects are farther away from the shock source, they estimated the spillovers of a negative shock on firms' sales caused by a natural disaster through production networks by comparing the directly unaffected firms that are close to the directly affected firms to those directly unaffected firms that are far distant away in the directly affected firms' supply chains using a difference-in-differences (DID) framework. Similarly, I compare the sales values of the treated firms to the sales values of those untreated firms that are far distant away in the treated firms' supply chains using a DID framework.²

The assumption behind this econometric design is the parallel trend of outcomes between the treated and control group firms in the absence of the policy, meaning that my measure of policy exposure is not correlated with other shocks which coincide with the implementation of bonus depreciation and affect sales. I address this identification threat in the following ways. First, I graphically demonstrate that changes in sales are uncorrelated with the policy exposure prior to the implementation. Second, I show that my results are robust after controlling for (2-digit NAICS) sector-by-year-by-prefecture fixed effects, implying that (1) the threat coming from differential trends across sectors is limited, (2) prefecture-level policies or shocks do not confound my estimates, and (3) local sector trend is also not a confounding factor. With the empirical strategy, my baseline estimation shows that treated firms' sales grew by approximately 4% after the policy implemented in 1998 for 1993-2003 sample.

Given the first-stage result, I examine whether the presence of direct and indirect input-output linkages to the treated firms had an impact on firms' performance in the years after the tax policy. In particular, I compare the sales of firms at different distances—

²Since the vast majority of firms are indirect transaction partners to each other, few firms are not connected to a certain set of firms in general. Then, I resort to their identification strategy that uses firms that are distant from the treated firms in the firms' production network.

in the supply chain network sense—from the affected firms to a control group of firms that are relatively more distant. This is also similar to the approach used in [Carvalho et al. \(2021\)](#). The identification threat to this specification is that the direct or indirect connection to the affected firms correlates with other shocks which coincide with the implementation of bonus depreciation through which sales are affected. This threat is particularly concerning since large firms tend to trade with many more firms than small firms do. Thus, these large firms tend to be the direct transaction partners of the affected firms. Then, the standard DID estimate may pick up the differential effects of macroeconomic shocks on large firms and SMEs. Furthermore, [Bernard et al. \(2019\)](#) empirically demonstrate that physical distance plays a major role in forming a transaction link between firms, and therefore, the standard DID estimates may be contaminated by the agglomeration effects of macroeconomic shocks.

To alleviate these issues, I resort to conditional DID methods. In particular, I follow the doubly-robust difference-in-differences (DRDID) estimators proposed by [Sant’Anna and Zhao \(2020\)](#) and use the multi-period version of DRDID suggested by [Callaway and Sant’Anna \(2021\)](#). The DRDID approach is valid if either the propensity score model or the outcome estimation model is correct. For the propensity score model, I use covariates relevant to firm sizes together with physical distance to the treated firms.

I support the conditional parallel trend assumption required for conditional DID methods by the standard graphical demonstration of the pre-trend. Using DRDID estimates, I graphically show no differential trend in sales among firms at different distances in supply chains prior to the policy shock, except the indirect suppliers. As for the indirect suppliers, the effects seem to be within the extrapolated linear pre-trend in the spirits of [Dobkin et al. \(2018\)](#) up to some post periods, and therefore I will refrain from making a causal inference over the estimate for the indirect suppliers. Furthermore, following [Garrett et al. \(2020\)](#), I try different definitions of the treatment status. I confirm that the results remain unchanged. Although the assumption underlying the research design is

fundamentally unverifiable, my empirical strategies and robustness checks significantly alleviate the identification threat.

These results contribute to three strands of literature. One is a growing literature that studies the impacts of investment stimulative tax policies. The literature extensively examines the direct effects of such tax policies (e.g., among many papers, closely related ones are [Hall and Jorgenson \(1967\)](#); [Cummins et al. \(1994\)](#); [House and Shapiro \(2008\)](#); [Edgerton \(2010\)](#); [Zwick and Mahon \(2017\)](#); [Ohrn \(2018\)](#); [Ohrn \(2019\)](#); [Fan and Liu \(2020\)](#); [Guceri and Liu \(2019\)](#); [Garrett et al. \(2020\)](#); [Curtis et al. \(2021\)](#); [Tuzel and Zhang \(2021\)](#)) in various settings. This study is the first contribution to the literature by demonstrating the significance of indirect effects through production networks.

Another is a growing literature of production networks. Among related papers,³ [Carvalho et al. \(2021\)](#) study the cascading effects of the Great Earthquake in Japan through production networks using a similar data set. I use a similar identification strategy that they propose but study investment stimulative tax policies that have different policy implications from theirs. Furthermore, the heterogeneity of cascading patterns is significantly different from theirs. [Liu \(2019\)](#) develops a rich model of industrial policies and production networks and extensively tests it with empirics. Estimating the parameters of his model, he computes the predicted aggregate impacts of industrial policies in China and South Korea, considering the propagating effects of the historical sectoral policies through industry-level input-output linkages. This paper differs from his study in that (a) he studies sectoral policies targeting a particular sector unlike tax incentives for investment and (b) my study focuses on reduced-form estimates of the spillover effects through granular firm-level supply chains rather than a rich model prediction in [Liu \(2019\)](#). Thus,

³See, e.g., [Acemoglu et al. \(2016\)](#) for industry-level analyses; [Barrot and Sauvagnat \(2016\)](#) for input-specificity analyses; [Boehm et al. \(2019\)](#) for firm-level cross-country transmission; [Demir et al. \(2024\)](#) for the propagation of financial shocks by liquidity-constrained importers; [Carvalho et al. \(2018\)](#) for the propagation of demand shocks on innovation activities; [Ozdagli and Weber \(2017\)](#) for monetary policy shocks; [Auer et al. \(2019\)](#) for inflation comovements across countries; [Kawakubo and Suzuki \(2022\)](#) study how the economy can quickly manage the supply chain disruptions with a cut in the chain by finding new trade partners using the Great East Japan Earthquake of 2011.

my study complements these studies in the production network literature.

The last one is the literature that studies the distortions caused by size-dependent policies on firms. For instance, examining such a policy in France, [Garicano et al. \(2016\)](#) reveal that the policy that increases labor costs once a firm grows large disincentivizes efficient firms to grow and leads to significant losses incurred mainly by workers and to a lesser extent by large firms.⁴ While they find some negative effects of the policy on large firms due to the additional taxes of labor, my paper finds suggestive evidence that some large firms benefit from the chauvinism toward small firms through production networks. And yet, this finding does not undermine their claim that the distortion culminates in large efficiency losses and costs incurred by workers. Moreover, while their findings imply how size-dependent policies may not be *economically* justifiable due to the large efficiency loss, those of my paper imply how such policies may not be even *politically* justifiable, given that large firms could benefit more from such policies.

2 Policy Background

The Japanese government increased bonus depreciation rates from 11% to 30% in June 1998 for small and medium enterprises (SMEs). While the detailed definition of SMEs under these corporate-targeted tax policies can be found in Appendix [A.1](#), the main difference between SMEs and large corporations is whether a firm has more than 100 million yen common stock (or initial capital for most private firms). Those SMEs with common stock of less than 30 million yen can choose between 30% bonus depreciation and a 7% tax credit for eligible investment expenditures. The earliest possible statistics in 2012 show that among all the approved applications for this policy, approximately 95% (198.2 billion yen) was bonus depreciation and 5% (11.5 billion yen) was tax credits.⁵

⁴Exploiting the same French regulation, [Aghion et al. \(2023\)](#) find the negative effects of the policy on innovative activities of firms.

⁵Retrieved on January 25, 2024 at https://warp.da.ndl.go.jp/info:ndljp/pid/10311345/www.mof.go.jp/tax_policy/reference/stm_report/houkoku01.pdf.

The bonus depreciation (without a choice of a tax credit) policy for SMEs has been implemented every year since 1972. The bonus rate and tax credit rate remain unchanged after 1998. The details of changes over these rates prior to 1998 and details of other relevant tax policies can be found in Appendix A. Notice that while the rate is lower, the bonus depreciation policy for SMEs had been effective for 26 years by June 1998. Then, it is natural to assume that the market was at the steady state in 1998 with a higher capital intensity than the period prior to 1972.

2.1 Treatment Intensity Measure Constructed by ZM

For exogenous variations in the treatment intensity of the 1998 policy, I follow ZM who estimate the direct effects of bonus depreciation in the U.S. In the absence of bonus depreciation, the Modified Accelerated Cost Recovery System (MACRS) in the U.S. lays out tax rules for the depreciation of newly purchased assets. The present value of depreciation deductions associated with \$1 of investment can be expressed as

$$z^0 = \sum_{t=0}^T \frac{1}{(1+r)^t} D_t,$$

where T is the class-life of the asset, D_t is the fraction of the dollar that is depreciated in year t , and r corresponds to the rate used to discount future cash flows. MACRS rules determine T and D_t in each period for each type of investment. Longer-lived assets are depreciated more slowly over longer lives and have smaller z^0 s than shorter-lived assets. Then, tax deductions provided by longer-lived assets are generally less than shorter-lived assets in present value terms.

Bonus depreciation allows firms to immediately write off v percent of eligible investments. The remaining $1 - v$ percent are depreciated following the MACRS rules. Then, the policy reduces the present value cost of investment by $v(1 - z^0)$. This effect is larger for the asset with smaller z^0 —i.e., assets with longer class-lives—and thus z^0 captures a

measure of bonus depreciation treatment intensity.

ZM compute an industry-level measure of z^0 in the following procedure. They first obtain z^0 for each asset class defined by MACRS using 7 percent as the discount rate. Next, they use administrative tax return data on sample firms to calculate the share of each eligible asset class purchased at each 4-digit NAICS industry level. Finally, ZM weight the asset-class-level z^0 s by the industry shares to create z_j^0 for each industry j , which measures the present value of depreciation deductions for the average asset industry j purchases.

Using the US administrative data, ZM computes the industry-level (continuous) exposure measure of U.S. bonus depreciation in 2002. Assuming that US and Japanese firms in the same industry tend to buy similar equipment and machinery, I apply their measure to Japanese industries. To construct a supply chain dataset, I follow [Garrett et al. \(2020\)](#) to construct a binary treatment variable. They categorize industries into “treated” industries if they are in the bottom third of the z_j^0 distribution. I conduct robustness checks with the 25th and 40th percentiles of the z_j^0 distribution as cutoffs with the results being unchanged.

3 Data

I rely on a proprietary dataset compiled by Teikoku Data Bank Ltd. (henceforth, TDB), a private credit reporting agency in Japan, to construct a firm-level production network of supplier-customer linkages. This dataset contains a set of firm-level characteristics of sales, the number of full-time employees, age, common stock amounts, headquarter locations, and industry, together with the identities of the firms’ suppliers and customers.

The TDB sample is neither a census nor a representative survey since the entry of any particular firm occurs at the request of TDB’s clients. As shown in Appendix [B](#), the TDB dataset well represents 1999 Economic Census in the industry and geographic composition of firms but excessively samples large firms compared to the census. Therefore, the

results in this paper are biased if there is a heterogeneity in the effects of the tax policy. Indeed, as [Zwick and Mahon \(2017\)](#) found, small firms are affected more by the bonus depreciation policy than large firms. Therefore, my estimates are likely to be underestimated.

Given the nature of the data collection process, TDB does not annually update the data on every firm. Thus, I restrict my sample to the subset of firms which report sales figures and firm-level covariates for all 11 years between 1993 and 2003. I start with fiscal year (FY) 1993 since the TDB industry code started using in 1993 a new industry code that matches JSIS more closely than before at disaggregate levels. Given the starting year, and given that expanding a period greatly reduces sample size (especially, small firms), I set the ending year at FY 2003, so that I have 5 years before and after the onset of the policy excluding the policy year. This leaves me with balanced panel data of 424,367 firms across all the prefectures in Japan. A detailed explanation of the data sets can be found in [Appendix B](#).

In addition to the basic statistics, each firm in the TDB dataset also provides a list of its transaction partners, allowing me to construct the production network of supplier-customer linkages for the firms in my sample. Given the occurrence of the tax policy in June 1998, and given that I focus on the intent-to-treat (ITT) effects, I construct this network using the transaction data collected as of January 1998.

The TDB-based supplier-customer linkages only capture a binary measure of inter-firm supplier-customer relations. Then, I do not observe a yen measure on their transaction volume. Note that I restrict my sample to the subset of firms that have at least one transaction partner within the TDB database.

With this data structure, I construct a measure of network distance to the treated firms. Exploiting the 1998 production network data, I first label the immediate customers and suppliers of treated firms as, respectively, “downstream distance 1” and “upstream distance 1” firms. Similarly, I then designate a firm as “downstream distance 2” if it was

Table 1: Summary Statistics by Distance

	Treated	Upstream 1	Downstream 1	Upstream 2	Downstream 2	Control
Log of Sales	6.00 (1.28)	6.84 (1.77)	6.88 (1.77)	6.23 (1.40)	6.09 (1.34)	5.32 (1.12)
Log of Employment	2.79 (1.07)	3.15 (1.46)	3.34 (1.49)	2.93 (1.20)	2.70 (1.14)	2.24 (0.94)
Log of Capital Stock	9.40 (0.88)	9.97 (1.57)	10.14 (1.63)	9.69 (1.13)	9.52 (1.05)	9.16 (0.90)
Age	27.91 (12.99)	29.75 (14.48)	29.72 (14.46)	25.99 (12.86)	25.89 (12.75)	22.40 (11.85)
N of Suppliers	2.75 (6.51)	12.22 (75.41)	11.30 (63.58)	3.79 (8.81)	2.89 (2.79)	1.61 (1.09)
N of Customers	3.40 (6.45)	16.13 (71.13)	14.49 (70.27)	3.70 (7.33)	4.37 (10.84)	1.75 (1.70)
Customers' log sales	16.16 (2.52)	16.77 (2.42)	15.78 (2.25)	16.39 (2.41)	17.18 (2.11)	14.50 (1.66)
Suppliers' log sales	16.30 (2.69)	15.80 (2.41)	16.76 (2.73)	18.31 (2.33)	16.83 (2.84)	14.75 (2.12)
Observations	75684	42590	56158	126995	172618	65860

listed in 1998 as a customer of at least one downstream distance 1 firm and was not a downstream distance 1 firm itself. With a similar recursive procedure, I identify the set of firms at various upstream and downstream distances from the treated firms right before the policy intervention.⁶ To retain a sufficient number of observations for the control group, I use those firms which are at distance 3 or more away from the treated industry firms. While large firms are known to change transaction partners frequently, SMEs change less frequently. The treated firms are SMEs, and 82% of their direct customers remain as the direct customers of the treated firms in the post-treatment period, while 80% remain as the direct suppliers. Note that throughout this paper, I abbreviate downstream distance 1 as “Downstream 1,” upstream distance 1 as “Upstream 1,” and so forth.

Table 1 summarizes the baseline characteristics of firms in FY1997. One can tell that those direct suppliers and customers of the treated firms are relatively larger than the rest of the groups and have noticeably more transaction partners. This is because those large firms tend to transact with many more firms than SMEs.

⁶This way of construction follows [Carvalho et al. \(2021\)](#) and implies that each firm is labeled as at most one downstream and one upstream network distance group.

4 Econometric Framework

There are two steps: estimation of the direct effects and that of the indirect effects.

4.1 Direct Effects

First, I estimate the direct effects of the tax policy on firms' performance using the following baseline specification:

$$Y_{ipNt} = \kappa_i + \kappa_{pt} + \sum_{t \neq 1993} \beta_t D_N \lambda_t + \varepsilon_{ipNt}, \quad (1)$$

where Y_{ipNt} is firm i 's outcome variable such as sales value in natural logarithmic scale at year t which lies in industry N and is located at prefecture p , $D_N = 1$ if industry N is the treatment industry, and 0 otherwise, $\lambda_t = 1$ event if event time = t , and I include the first year 1993 as reference year and thus omitted from the graph. Following [Bertrand et al. \(2004\)](#), I cluster standard errors at N industry level since the treatment category is at N industry level.

The identification assumption underlying equation (1) is that ε_{ipNt} is not correlated with D_N . Considering the sensitivity issue raised by [Roth and Sant'Anna \(2022\)](#), I chose the natural logarithmic scale for the outcome variables since there is a large and skewed heterogeneity in firms' scales. Thus, scale-free percentage change measures across time periods are suitable to justify the underlying assumption.

4.2 Spillover Effects through Production Networks

Next, I turn to the estimation of the spillover effects. Recall that Upstream 1 and Downstream 1 firms tend to be much larger than the control group firms. This is because large firms tend to directly transact with many firms (e.g., Toyota). Since the remarkable size difference could imply a differential trend in the counterfactual, rather than relying upon

the standard parallel trend assumption, I resort to the DRDID method from [Sant'Anna and Zhao \(2020\)](#). DRDID estimators are consistent if either (but not necessarily both) a propensity score or outcome regression working models are correctly specified.

DRDID estimators require two standard assumptions in the conditional DID estimators: the so-called conditional parallel trend assumption and overlap assumption. The former assumption essentially states that the parallel trend assumption holds conditional on covariates that drive a differential trend. In other words, the assumption allows for covariate-specific time trends. The latter assumption states that some fraction of the population is treated and that for every value of the covariates, there is a positive probability that the unit is not treated.

Since my sample is a panel data with multiple time periods, I use [Callaway and Sant'Anna \(2021\)](#) version of DRDID that extends the two-period DRDID estimator from [Sant'Anna and Zhao \(2020\)](#) to the one with multiple time periods. The idea is simple; for the conventional event-study plot, they suggest conducting two-period DRDID for two subsequent periods. In my case, for an event study plot, they suggest to implement DRDID on 1993 vs. 1994, 1994 vs. 1995, and so forth, for the pre-treatment period, and then 1997 vs. 1998, 1997 vs. 1999, and so forth, for the post-treatment period. For the estimate, I follow their aggregation scheme, which is essentially averaging out the pre- and post-treatment estimates. I conduct the DRDID for every network distance. For instance, when I conduct DRDID on Upstream 1 firms, I restrict the sample to Upstream 1 firms and the control group first, and then I implement DRDID. The details of the specification from [Sant'Anna and Zhao \(2020\)](#) and [Callaway and Sant'Anna \(2021\)](#) are explained in Appendix D.

To take out the direct effects on the connected firms that may vary across different network distances, I control for the (finest-level) industry-by-year-by-prefecture fixed effects. Note that since some industries perfectly predict network distance in my sample, the inclusion of these fixed effects as covariates in the selection equation will violate the

overlap assumption. Then, I use an outcome residualized by these fixed effects.⁷

To compute the propensity score, I use the following covariates: the outcome of the previous period, the natural logarithm of common stock, that of employment, that of the shortest physical distance to any of the treated firms being connected, and the average physical distance to the treated firms. The first three covariates are measures of firm sizes, and the other two are used since geographic distance is known to affect a firm linkage. Furthermore, I include the polynomials of these covariates up to the third order since overfitting costs little but since the fourth order turns out to be not only computationally challenging but also fails to converge.

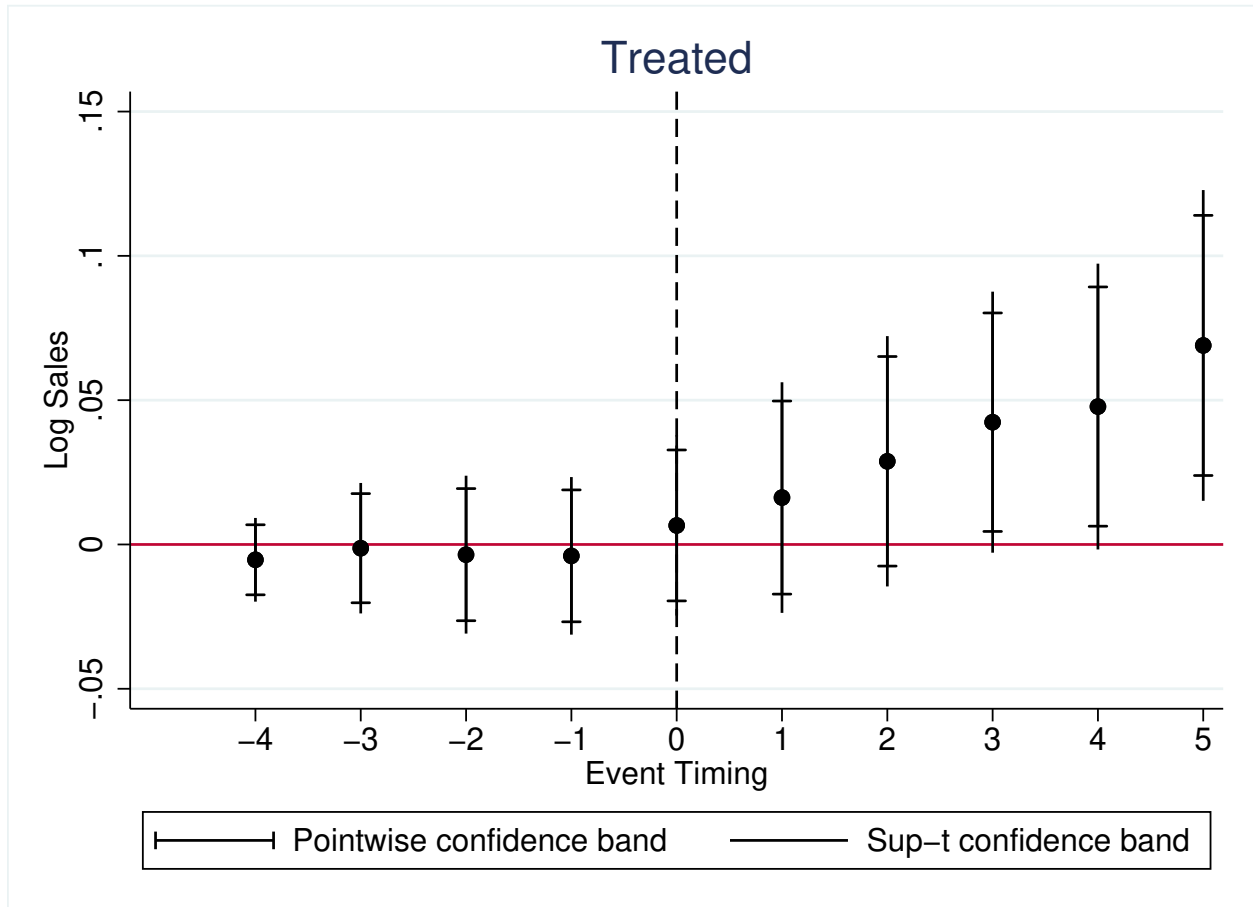
When I plot the dynamics of the effects, I follow the best practices of event study plots suggested by [Freyaldenhoven et al. \(2021\)](#) and use the (simultaneous) uniform confidence bands for the standard DID and similar uniform bands proposed by [Callaway and Sant'Anna \(2021\)](#) for DRDID estimators to graphically show the parallel trend in a conservative way. The motivation for the uniform confidence intervals (CIs) is to circumvent the multiple-hypothesis testing implicitly conducted with multiple time periods. For DRDID estimators, I follow [Callaway and Sant'Anna \(2021\)](#) and only display the uniform confidence bands.

5 Results

Figure 1 is a standard event-study plot for the direct effects. It supports the parallel trend assumption and shows that the policy effects kick in after the onset of the policy.

⁷See Appendix D for discussions on potential issues with the residualized outcome and how these concerns are mitigated in my study.

Figure 1: Pre-trend Test of Direct Effects



Notes: x-axis indicates time period where 0 is equal to FY1998 that corresponds to the year the policy took effect, -1 means FY1997, and so forth. Coefficients with 95% confidence intervals and uniform sup-t confidence intervals from equation (1). The reference year FY1993 is excluded to display from the graph.

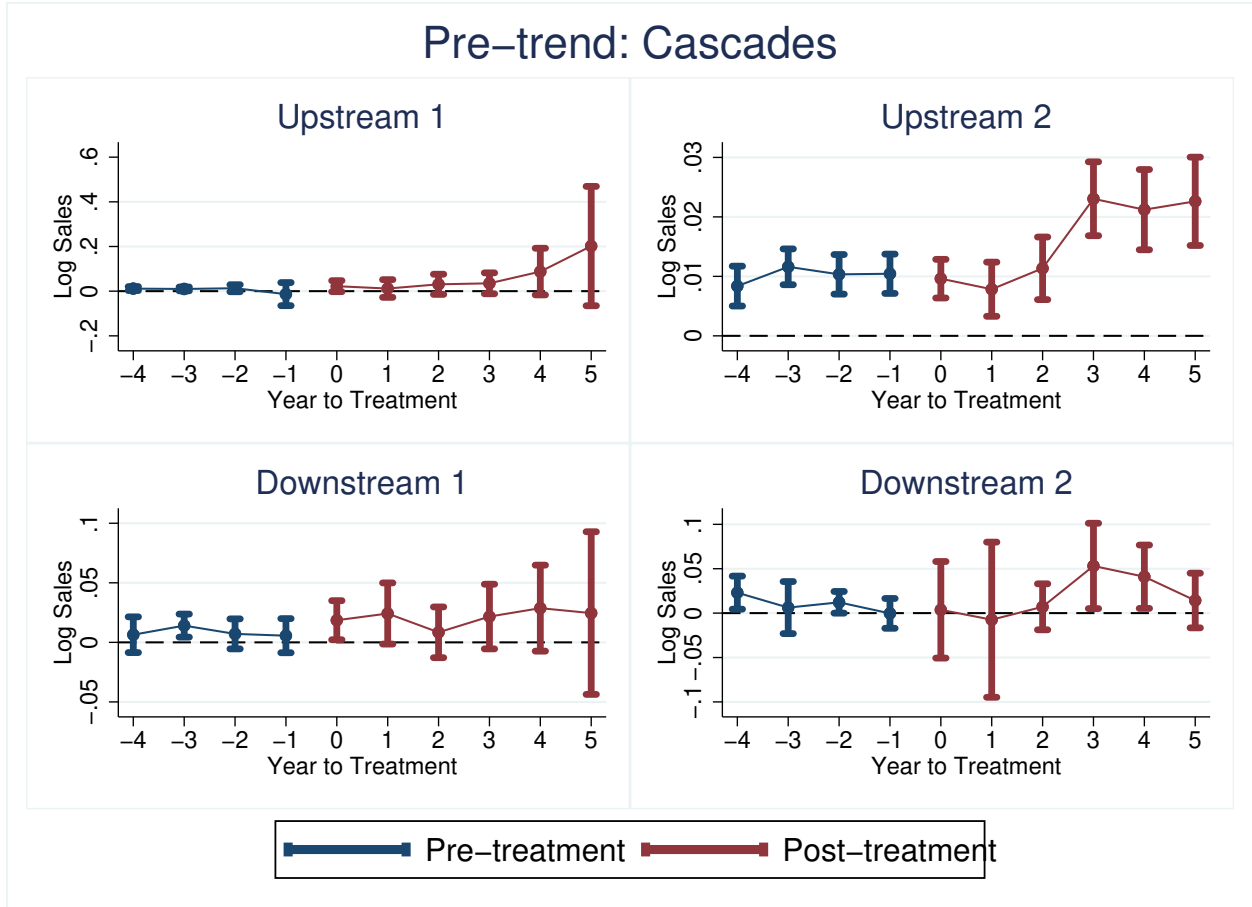
Table 2: First-stage Direct Effects of Investment Stimuli

	(1)	(2)	(3)	(4)	(5)
	Sales	Sales	Sales	Sales	Employment
DD	0.038 (0.014)	0.045 (0.014)	0.054 (0.014)	-0.081 (0.019)	-0.015 (0.014)
N	1556984	1553716	1542595	1020291	1553716
2-digit JSIC FE x Year FE	No	Yes	Yes	Yes	Yes
SME Control	No	No	Yes	No	No
Unprofitable Firm	No	No	No	Yes	No

Notes: Standard errors in parentheses are clustered at the industry level. The first column corresponds to baseline estimates, the second column includes two-digit JSIC industry-by-prefecture-by-year fixed effects, the third column restricts the control group to SMEs, the fourth column restricts the treatment group to firms whose aggregated net-tax profits in the post-period are negative, and the fifth column uses the number of employees as the outcome variable. All the outcome variables are in a natural logarithmic scale.

Table 2 demonstrates the direct effects estimates that are statistically significant and economically meaningful at approximately 4% increase in sales after the policy under the baseline model of the first column. Furthermore, the results are robust to the inclusion of 2-digit sector-by-year-by-prefecture fixed effects, mitigating the concern of industry-trend effects. Since the treated firms are all SMEs, I conduct a robustness check by restricting the control group sample to SMEs in the third column. The results are similar, while I use the full control group sample as my baseline to compare the estimates with the spillover effects. The fourth column is another robustness check that uses unprofitable firms for the treatment group. Since the policy is a tax deduction, unprofitable firms are not expected to benefit from the treatment, and the results confirm this. The last column is the same as the second column except that the outcome variable is the number of employees. Table 2 also shows no effect on the employment of the affected firms, which is consistent with the results of the second bonus depreciation shock in 2008 U.S. [Garrett et al. \(2020\)](#).

Figure 2: Pre-trend Test for Second-stage Spillover Effects



Notes: x-axis indicates time period where 0 is equal to FY1998 that corresponds to the year the policy took effect, -1 means FY1997, and so forth. Coefficients with 90% uniform confidence intervals from the DRDID estimation. The reference year 1993 is excluded to display from the graph.

Figure 2 consists of four event-study plots from DRDID for each distance group (Upstream 1, Upstream 2, Downstream 1, Downstream 2). The graph appears to roughly support the parallel trend for each group except Upstream 2. Therefore, I will not make an inference on the spillover effects over the indirect suppliers.

Table 3 shows the regression results for DRDID estimation. The first column corresponds to the results of DRDID, and the second column shows the DRDID results with SMEs. The first row shows that there are spillover effects on the direct suppliers. Note that while the spillover effects on the direct suppliers are statistically significant at the five percent significance level when restricted to the SME sample, the effect is statistically significant only at the 10 percent level (p -value = 0.079 for the two-sided test and 0.039

Table 3: Second-stage Spillover Effects on Sales

	(1)	(2)
	Sales	Sales
Upstream 1 \times Post	0.065 (0.037)	0.028 (0.009)
Upstream 2 \times Post	0.016 (0.002)	0.017 (0.003)
Downstream 1 \times Post	0.021 (0.014)	0.008 (0.017)
Downstream 2 \times Post	0.019 (0.015)	0.009 (0.016)
SME only	No	Yes

Notes: Standard errors in parentheses are clustered at the (finest-digit) industry level. The first column corresponds to the baseline DRDID estimates, and the second column is the same estimation except that the sample is restricted to SMEs.

for the one-sided test) with the full sample. Thus, it is weak evidence. On the other hand, we do not see statistically significant effects on the downstream firms.

As mentioned above, [Garrett et al. \(2020\)](#), I use the 33rd percentile of the z_j^0 distribution as the cutoff for the discretized treatment measure for my main results. I conduct robustness checks with the 25th and 40th percentiles of the z_j^0 distribution and confirmed no change in results, as shown in [Appendix C](#).

6 Discussions and Limitations

As demonstrated above, there is considerable heterogeneity in the spillover effects by the network distance of the connected firms. The results on the direct suppliers provide suggestive evidence that some large firms enjoyed larger spillover effects than SMEs and even the treated firms, implying the potential influence of markups.⁸ The markup differences are implied not only by the sales size differences but also by the average number of suppliers for the treated firms. The smaller number of suppliers for the treated firms compared to Upstream 1 provides supporting evidence that the treated firms have fewer

⁸See, e.g., [Baqae and Farhi \(2020\)](#) for models that show that the presence of distortions such as markups can alter the productivity shocks' propagation patterns.

substitutes for suppliers and thus less markups than Upstream 1.

The estimates of the indirect effects on the direct suppliers could also be attributed to the possibility that the direct suppliers might have included equipment providers. These providers potentially benefit more than those firms who purchased equipment for production if the equipment is expensive. However, it is somewhat hard to imagine that firms report these providers as their suppliers given the infrequency of purchasing capital goods. Nevertheless, since the TDB dataset does not allow me to identify what kind of good a firm sold to another firm, I humbly admit this possibility.

In contrast to the upstream spillovers, I do not see statistically significant effects on downstream firms, whether they are large or small firms. This is a stark contrast to the existing research on spillover effects in production networks. While the dataset does not allow me to test the price channel as theoretically analyzed by [Grassi et al. \(2017\)](#), the treated firms could have searched for new customer firms to avoid lowering the prices. Firms tend to avoid lowering prices since they do not need to provide customers a reason when they lower prices but they do need to explain hard when they increase prices. To sell more without decreasing the price of their products, they can search for new customers that can be the existing or potential customers of untreated large firms in the treated industries. Such behavior is indeed found and demonstrated in [Appendix C](#).

While I found an increase in sales, I found no effect of investment stimuli on employment. This is a similar result to the second bonus period found by [Garrett et al. \(2020\)](#). Although they found employment effects after the first bonus depreciation tax policy in 2002 U.S., they found a lack of effects on employment after the second bonus depreciation tax policy that took place in 2008 U.S. As one potential reason, they pointed out the frequent renewal of the policy hinted firms that the policy would be long-lasting, which drives up the capital intensity of firms permanently. This leads to the absence of employment effects after the second policy shock. Given the 26 years of bonus depreciation prior to 1998 in Japan, it is natural to expect a similar result with theirs. The result is also consis-

tent with the high fixed costs of hiring a new employee in Japan due to strict labor laws. The details can be found in Appendix C.3. Finally, note that I encountered some technical difficulties conducting additional robustness checks that the literature has done. These checks include block permutation tests and placebo production networks. I explain the details in Appendix C.4.

7 Conclusion

This paper estimates the indirect effects of tax incentives for investment through production networks, exploiting a quasi-experimental event of an investment stimulus and unique proprietary data of supply chains in Japan. I confirm the direct effects of such policies that are consistent with the existing literature. I further provide novel results that corporate tax policies cascade through production networks with remarkable heterogeneity in the networks. These results are useful findings for policymakers who are evaluating the effectiveness and distortions of investment stimulative tax policies in the entire economy. Additionally, the results of this paper imply that the political justification for a policy restricting targets to SMEs may not be valid given the possibility that large firms may absorb some effects through the production networks.

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Appendices

A Policy Background And Other Relevant Concurrent Tax Policies

In this section, I note the details of the tax policy of interest and other major tax policies and changes that were implemented around 1998.⁹ The Asian Currency Crisis hit Japan, leading to a significant economic downturn in July 1997. Many banks went bankrupt toward the end of 1997, and the government allowed some of these banks to bail out. To stimulate the economy, the government announced on January 9, 1998 that the government would introduce special income tax allowances, decrease the corporate tax rate while increasing the corporate tax base to maintain some level of fiscal health and make several changes in the system of depreciation on investment goods.

In particular, the Japanese government increased bonus depreciation rates from 11% to 30% in June 1998 for SMEs (*“Chushokigyo Toshi Sokushin Zeisei”*) as part of *“Sogo Keizai Seisaku”*, to stimulate the economy by encouraging SMEs to buy more machinery and equipment. This policy was implemented together with increased taxable base that effectively decreased the corporate tax rates from 37.5 to 34.5% for non-SMEs and from 28 to 25% for SMEs in 1998, and to 30% and 22% in 1999, respectively. The effective corporate tax rates further decreased to 30% for non-SMEs and 22% for SMEs in 1999 without changing the taxable base. While the detailed definition of SMEs under these corporate-targeted tax policies are written out in the next section, the main difference between SMEs and large corporations is whether a firm has more than 100 million yen common stock (or initial capital for most private firms). Japan used similar depreciation rules to MARCS in

⁹The references come from the government reports: https://www.mof.go.jp/pri/publication/policy_history/series/h1-12/4-1-11.pdf, https://dl.ndl.go.jp/view/download/digidepo_3515892_po-553f.pdf?contentNo=6&alternativeNo=, and <https://www.cao.go.jp/zei-cho/history/1996-2009/etc/1997/zeicho1.html>.

1998. See [Komori \(2003\)](#) for more details..

The bonus depreciation (without choice of tax credit) policy for SMEs has been implemented every year since 1972. The bonus depreciation rate was initially 20% in 1972, which was reduced to 16.7% in 1976, 14% in 1980, 13% in 1994, and 11% in 1995. The bonus rate and tax credit rate remain unchanged after 1998, and corporate tax rates stay the same after 1999. Firms cannot use this tax policy if they report to use the equipment for the following businesses: utilities except gas, railway, air transportation, banking, and entertainment except movie. However, SMEs can circumvent this restriction by claiming they have businesses that cover multiple industries. In other words, SMEs can use the tax benefits as long as they at least partially use equipment for non-excluded business categories. This means that they can use the policy even if they belong to industries that conduct businesses of these excluded business categories. This implies that no industries are essentially excluded from the bonus depreciation policy, and thus, I include all the industries in my sample.

Additionally, the government announced in April 1993 that it would introduce a temporal stimulative policy for SMEs that is effective of July 1, 1993 until June 30, 1994. It further announced in April 1994 that it would extend the policy until December 31, 1994. The short term stimulative policy included (i) a choice between 30% bonus depreciation tax and 7% tax credit and (ii) a choice between 36% bonus depreciation and 8.4% tax credit if a firm purchases equipment to reduce employment. Notice that (ii) stimulates the investment by a labor-intensive firms to increase its capital intensity, and thus it stimulates labor-intensive firms more than capital-intensive firms. Since those firms which tend to buy equipment with a longer depreciation schedule are expected to be more capital-intensive than other firms, this short-term stimulative policy is not anticipated to affect the treated firms in the same way the 1998 policy change did. In particular, the control firms are expected to have benefited from (ii) more than the treated firms, canceling off the bonus depreciation effects of (i) in my framework.

A.1 Definition of SMEs

SMEs are the entities listed in 1 and 2 below. However, among small and medium-sized business operators, there is an exempted business operator, which is a corporation whose annual average income for each fiscal year ended within three years prior to the beginning of the fiscal year exceeds 1.5 billion yen. Please see the more precise legal definitions through the National Tax Agency website at <https://www.nta.go.jp/taxes/shiraberu/taxanswer/hojin/5433.htm>.

1. Corporations other than those listed in (a) through (c) below (excluding fiduciary corporations) among corporations with initial capital or common stock (equity) of 100 million yen or less.
 - (a) Corporations in which one half or more of the total number or total amount of its issued shares (excluding its own shares) are owned by a single large corporation.
 - (b) In addition to (a) above, a corporation in which two-thirds or more of the total number or total amount of its issued shares are owned by more than one large corporation.
2. Corporations with 1,000 or fewer full-time employees but without issued share.

A.2 Other Concurrent Tax Policies

A.2.1 Sales Tax Increase in 1997

There were some other tax policies implemented around 1998. First, for fiscal consolidation, Japan increased the sales tax rate from 3% to 5% in April 1997. This must have affected the Japanese economy, raising some identification concern if this differentially affected the treated firms. The dynamic effects in Figure 1 demonstrate that there was no differential effect in 1997, mitigating this concern.

A.2.2 First-year Simplified Method

The government announced on January 9, 1998 that the government would abolish in April 1998 the First-year Simplified Method (*Shonendo Kanbenhou*) that allowed the firms to depreciate half of the depreciation amount of the first year for investment goods (except structures such as buildings) purchased in whichever month of the fiscal year. For example, suppose a firm whose fiscal month is April buys a computer in March 1993, and suppose that the firm is allowed to depreciate 20% of the purchased amount for the first year. Then, the simplified method allowed the firm to immediately depreciate half of 20%, which is 10%, for the 1993 fiscal year tax return, and depreciate the other half throughout the rest of the year. Since April 1998, all firms must distribute the depreciation amount on a monthly prorated basis and divide the depreciation amount by the proportion of months left for depreciation. For instance, if a firm whose fiscal month is April buys a computer in March 1993, the firm is allowed to depreciate only $1 / 12$ of 20% of the purchased amount. The abolishment announcement (and the anticipation effects from policy discussions before the announcement) might have created a “last-minute” demand and incentivized those treated firms whose fiscal month ends in or before April to purchase eligible equipment relatively more than the control group firms whose fiscal month ends in or before April. Notice that this change is a one-time shock and does not confound the effects in the later years which I found.

A.2.3 Corporate Tax Decrease

As mentioned above, the effective corporate tax rates were reduced in both 1998 and 1999, in response to the Asian Currency Crisis. One may wonder if this change affected the results in this paper. Using the Domestic Production Activities Deduction (DPAD), [Ohn \(2018\)](#) finds that a decrease in the effective corporate tax rate increases investment. The ZM bonus depreciation intensity measure is constructed based upon depreciation duration schedule differences not based upon corporate tax rates and other indicators, which

mitigates the identification threat to some extent. Meanwhile, I cannot eliminate the possibility that those industries that invest in relatively long (or short) duration equipment benefits more from the corporate tax cut. However, the effects of a bonus depreciation from [Zwick and Mahon \(2017\)](#) introduced in the period concurrent with the effective corporate tax cut through DPAD is consistent with the effects introduced in the period without DPAD. Assuming that Japanese and U.S. industry structures are similar, their findings further mitigate the concern.

A.3 Benefits of Discretization of Treatment

According to [Garrett et al. \(2020\)](#), they discretize the treatment variable for two reasons. First, it removes the effects of outliers in the z_j^0 distribution—e.g., the power generation industry has a z_j^0 that is much lower than other industries. Second, z_j^0 values rely on an assumption about the discount rate. Their discretized treatment measure eliminates this assumption. This paper also discretizes its treatment measure not just for the two reasons but also for mitigating measurement errors caused by differences between U.S. and Japanese industries with respect to purchasing patterns of eligible equipment. I use the 33rd percentile as the cutoff for the balance across the sample sizes of different network distances including the treatment and control group.

B Detailed Data Description

The TDB data uses its own industry codes that closely match the Japan Standard Industrial Classification (JSIS). TDB collects firm data on employment, sales, capital stock, and the location of the firm’s headquarters. Firms in their data set report the date on which its fiscal year ends as well. On the other hand, TDB started collecting data on investment only after 2000 with a restricted sample. One may think that those large firms in the treated industries can be a control group since they were not qualified for the policy.

However, these large firms tend to transact with many more firms than SMEs and tend to be distance 1 or 2 away firms in the dataset. Thus, given that I expect spillover effects through production networks, I cannot use them as a control group.

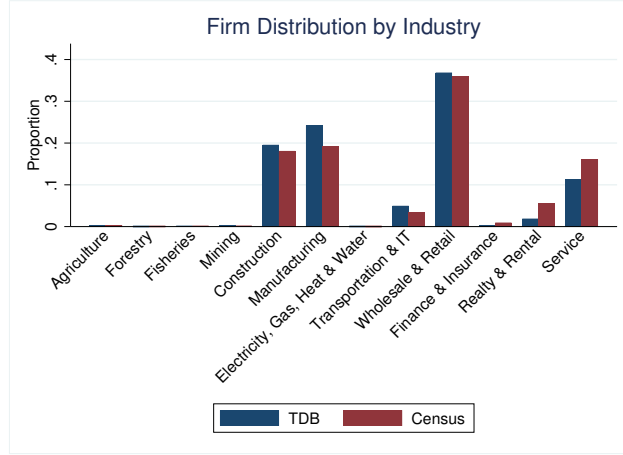
B.1 Comparison against 1999 Economic Census

To examine biases in the sample, I compare the 1997 TDB dataset with 1999 Economic Census. I chose FY 1997 for TDB dataset since the main policy of interest started in June 1998, and 1999 Economics Census is the census conducted closest to this time period. Figure 3 displays the comparison of industrial composition at the JSIS major classification level. As it shows, there is no major difference between the two. Furthermore, Figure 3 shows the comparison of geographic distribution at the prefecture level using the same two datasets. We can see no major difference between the two.

Figure 3: Industrial Composition and Geographical Distribution

(a) Comparison of Industrial Composition between

TDB and Census



(b) Comparison of Prefecture Composition between

TDB and Census

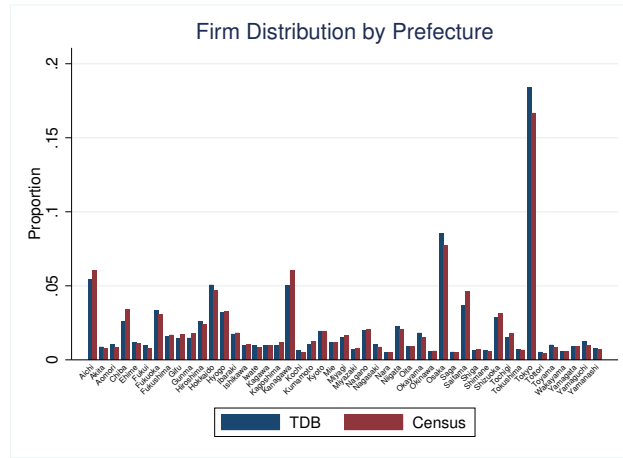


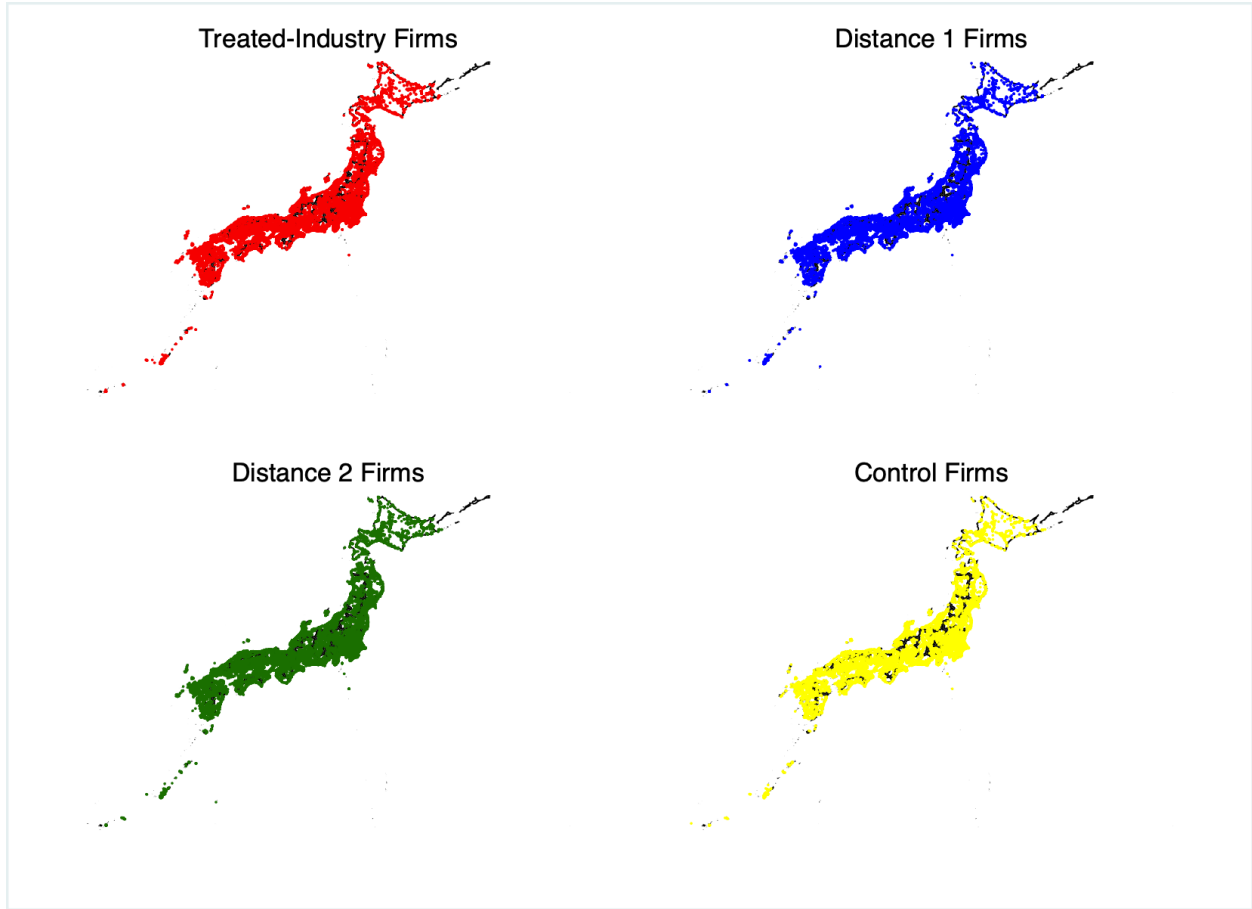
Table 4: Firm Size Distribution

	Number of Employees									
	0 – 4	5 – 9	10 – 19	20 – 29	30 – 49	50 – 99	100 – 299	300 – 999	1000 – 1999	2000+
TDB	0.19	0.23	0.23	0.11	0.10	0.08	0.05	0.01	0.003	0.002
Census	0.50	0.21	0.14	0.05	0.04	0.03	0.02	0.01	0.001	0.001

Notes: This table reports the fraction of firms with the number of employees in each of the respective bins.

“TDB” refers to the 1997 TDB dataset. “Census” refers to the 1999 Economic Census for Business Frame.

Figure 4: Location of Firms



On the other hand, Table 4 shows the proportion of firms in each size bin based on the number of employees. We can see that the proportion of firms with less than 5 employees is disproportionately small in the TDB sample compared to the census. Thus, the results of this paper should be interpreted with caution. Given that ZM find that smaller firms face greater liquidity constraints and thus benefit more from the bonus depreciation policy, at least the direct effects may be underestimated in this paper.

Figure 4 displays the location of the headquarters of the firms in my sample. It shows that the sample firms are not concentrated in a particular region, alleviating the concern on a geographic bias in the supply chain.

B.2 Supplier-customer Information

The forms used by TDB for firms to report transaction partners limit the number of suppliers and customers to nine each. Nonetheless, given that each firm in the dataset may also be reported by other firms as a transaction partner, I overcome this limitation by augmenting the customer and supplier relations with those reported by other firms. That is, I construct a firm's transaction network by supplementing the list of suppliers (customers) reported by the firm itself with the reports of other firms that state the firm as their customer (supplier). This procedure leads to the list of suppliers and customers of firms that have more than nine transaction partners per category, including large firms that transact with several thousand firms.

B.3 Concordance between the U.S. and Japanese industries

I use concordance tables provided by the United Nations to match Japanese industries categorized by 4-digit JSIC with U.S. industries categorized by 4-digit NAICS.¹⁰ Using this concordance, I assign the treatment intensity measures to each of JSIC industries that have corresponding industries to 4-digit NAICS industries listed in the ZM industry-level treatment intensity file.¹¹ With the TDB dataset, I apply the ZM treatment intensity measures to TDB-defined industries (hitherto TDB industries) that closely follow 4-digit JSIC.¹²

¹⁰The crosswalks are available at <https://unstats.un.org/unsd/classifications/Econ>. There is no direct crosswalk between JSIC and NAICS, and therefore, I first use a crosswalk between ISIC and NAICS, and then I use a crosswalk between ISIC and JSIC.

¹¹When there are multiple JSIC industries corresponding to a single 4-digit NAICS industry, I take a simple average of the intensity measures.

¹²When there are multiple 4-digit JSIC industries corresponding to a single TDB industry, I take a simple average of the ZM intensity measures. If there are multiple TDB industries to a single JSIC industry, I assign the same ZM intensity measure for this JSIC industry to all the corresponding TDB industries.

C Additional Results and Robustness Checks

C.1 Different Cutoffs for Treatment

In the main text, I use the 33rd percentile of the policy exposure measure adopted from ZM. In this section, I show that the results in the main text are robust to the choice of a cutoff by demonstrating the same event study plots with the 25th and 40th percentiles as cutoffs for the treatment.

C.2 Change in Firm Transaction Pattern: the Number of Customers

Figure C4 demonstrates the estimates of equation (1) in which the outcome variable is the number of customers. Given that the downstream control group firms may be the final producers and mechanically have zero customers, I restrict the control group to be firms at Upstream 3 or higher. Given that not many but some treated firms have zero customers, taking the log of the outcome may result in biases introduced by zeros, and thus, I do not transform the outcome in any way.

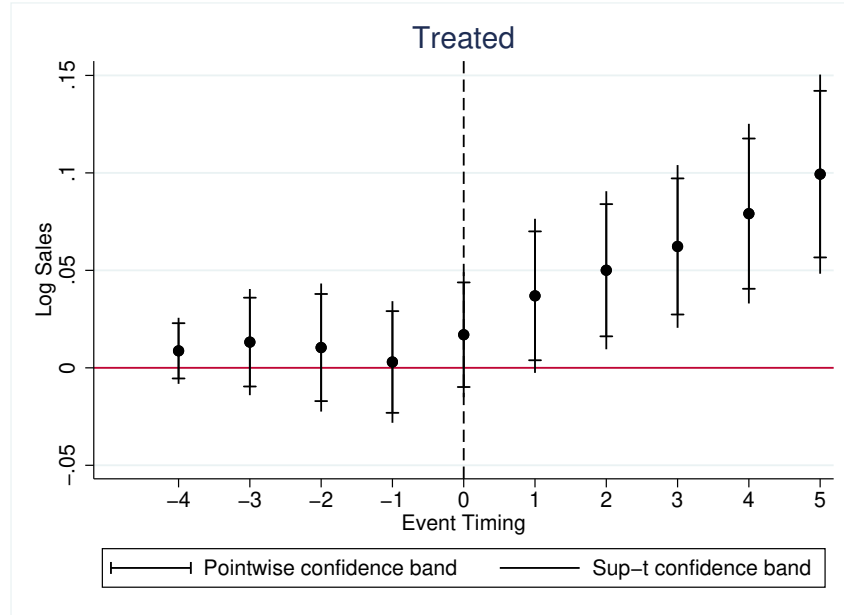
C.3 Labor Laws in Japan and No Employment Effects Results

Japanese employment contracts are remarkably different from those in other countries in that Japanese firms hire new employees not for specific jobs or tasks but for general purposes as “permanent employees” or so-called *seishain*. This unique feature of Japanese contracts makes it much harder for firms to legally lay off their employees even when firms are in a downturn and need to downsize specific departments or projects since the Japanese contracts make it possible for these employees to be transferred from one department to another within the firms.¹³ This implies that Japanese firms pay large fixed costs to hire an additional employee. For this reason, Japan has seen a steady increase in (part-

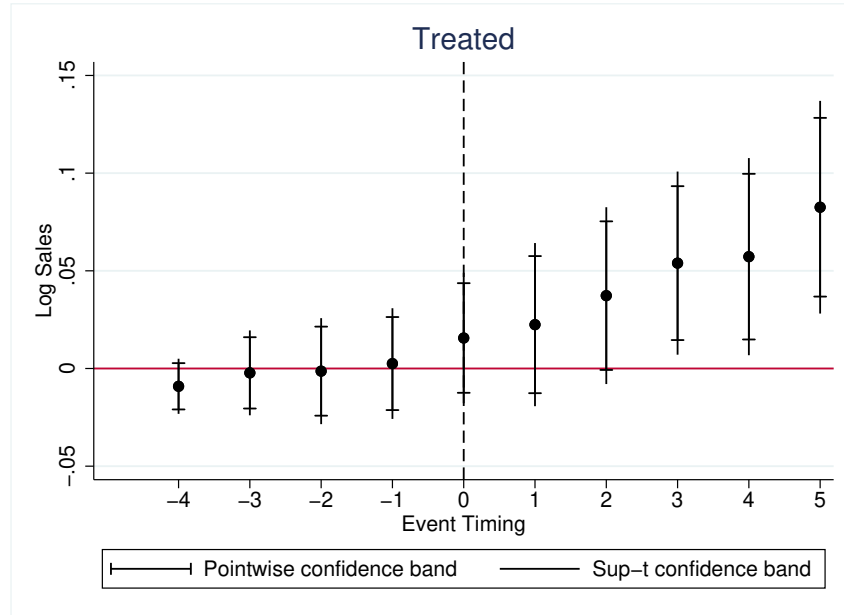
¹³See, e.g., <https://shuchi.php.co.jp/the21/detail/8467>.

Figure C1: Pre-trend Test for Direct Effects with Different Cutoffs

(a) 25th percentile



(b) 40th percentile



Notes: x-axis indicates time period where 0 is equal to FY1998 that corresponds to the year the policy took effect, -1 means FY1997, and so forth. Coefficients with 95% point and sup-t uniform confidence intervals from equation (1). The reference year 1993 is excluded to display from the graph.

Figure C2: Pre-trend Test for Spillover Effects with 25th Percentile Cutoff

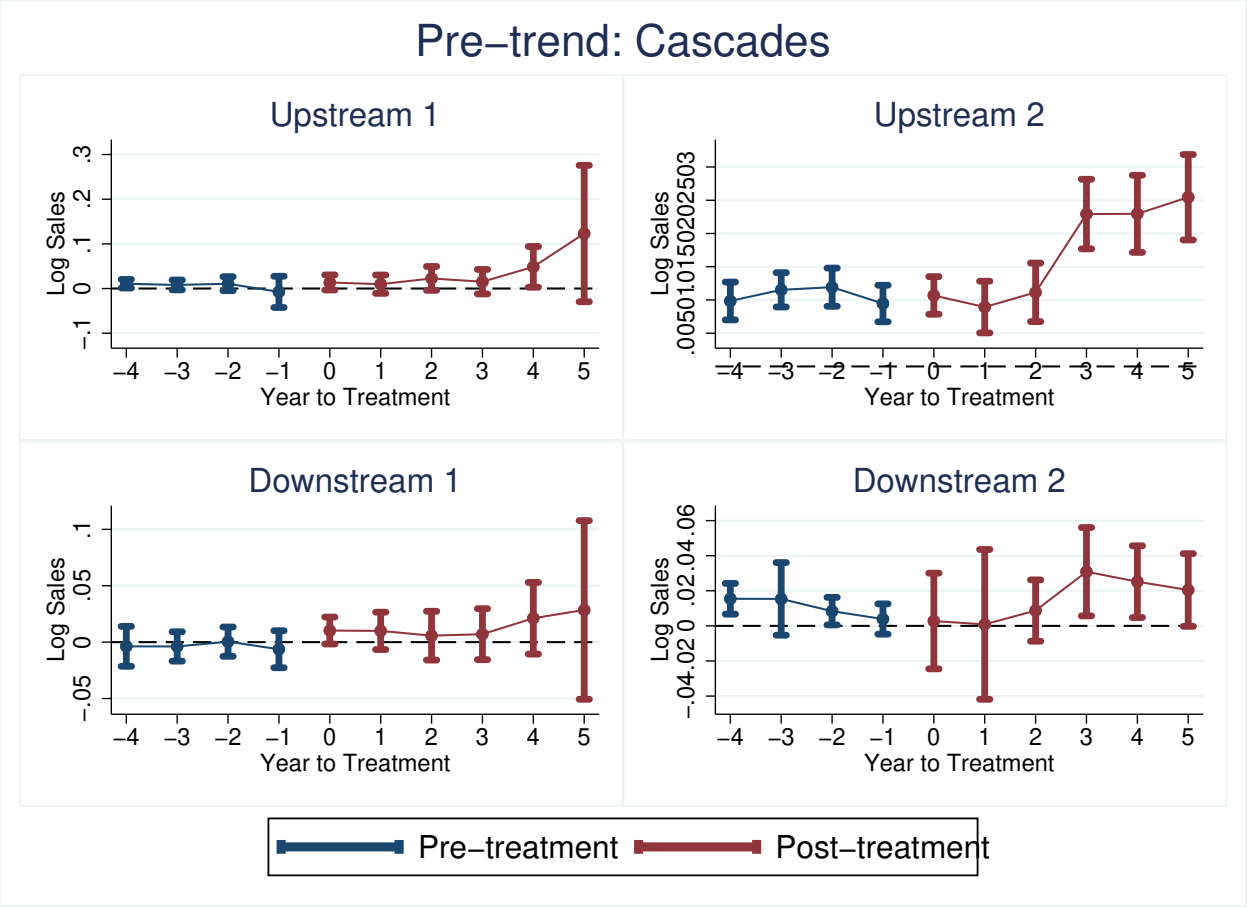
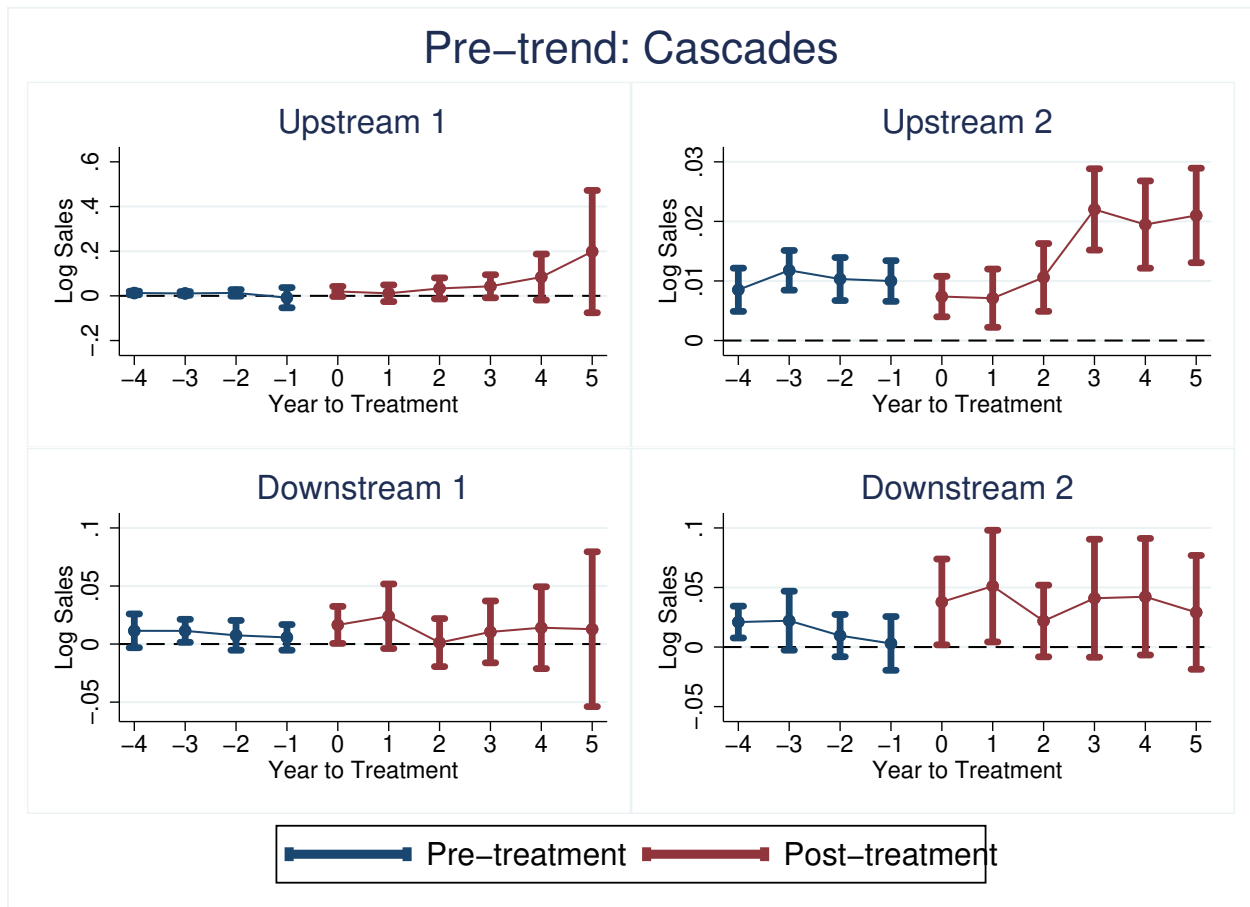
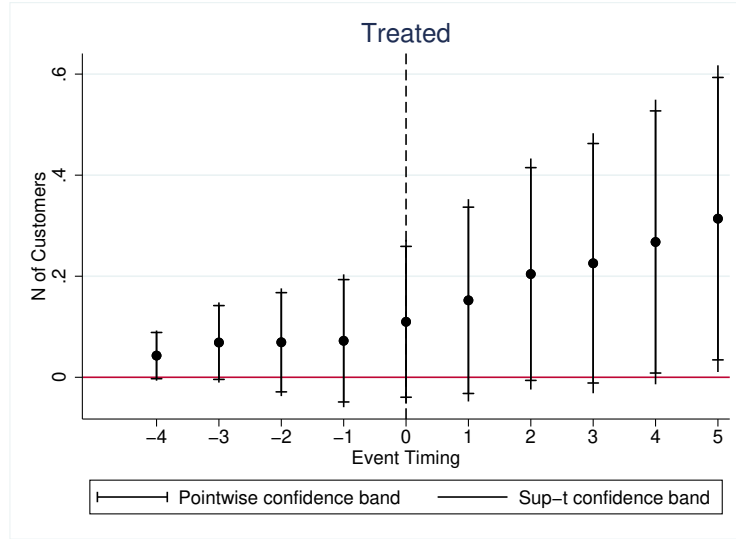


Figure C3: Pre-trend Test for Spillover Effects with 40th Percentile Cutoff



Notes: x-axis indicates time period where 0 is equal to FY1998 that corresponds to the year the policy took effect, -1 means FY1997, and so forth. Coefficients with 90% uniform confidence intervals from the DRDID estimation. The reference year 1993 is excluded to display from the graph.

Figure C4: Graphical Results on the Direct Effects on the Number of Direct Customers



Notes: x-axis indicates time period where 0 is equal to FY1998 that corresponds to the year the policy took effect, -1 means FY1997, and so forth. Coefficients with 95% confidence intervals and uniform sup-t confidence intervals from equation (1). The reference year 1993 is excluded to display from the graph.

time and full-time) temporary employees since 1984.¹⁴ The TDB dataset unfortunately does not record the number of temporary employees. Therefore, even if labor and equipment in the firms' production functions are imperfect substitutes, the decreased price of equipment by the bonus depreciation might have been insufficient for firms to hire more permanent employees and could have induced firms to hire more temporary employees given the downturn.

C.4 Technical Difficulty of Some Robustness Tests

C.4.1 Block Permutation Tests

As conducted (in slightly different forms) by Chetty et al. (2009), Ohn (2018), and Zidar (2019), block permutation tests that randomly assign placebo implementation years and randomly pick placebo-treated industries appear to be desirable for this study as well. Ohn (2018) conducted a series of 2,000 block permutation tests, which will take roughly

¹⁴See, a report written by the Ministry of Health, Labor, and Welfare at <https://www.mhlw.go.jp/file/06-Seisakujouhou-11650000-Shokugyouseiteikyokukenyukiroudoutaisakubu/0000120286.pdf>, retrieved on March 29, 2024. Back in 1999, roughly one fourth of employees are temporary employees.

5,000 hours on the server of TDB. In addition to the time challenge, given that the TDB on-site data center to which contracted researchers have access conducts server maintenance on a regular basis, conducting block permutations of an even smaller number would be unfortunately technically challenging.

C.4.2 Placebo Production Network

Another placebo test that is desirable but technically challenging is to conduct the same analysis with a randomly generated production network, as done by [Carvalho et al. \(2021\)](#). [Carvalho et al. \(2021\)](#) start with the actual production network constructed using their data, draw a random production network uniformly at random while preserving the identity and the number of customers of all firms, and use the resulting network to redefine all firms' upstream and downstream network distances to the treated firms. This falsification exercise is unfortunately infeasible in my setting. The vast majority of firms in my sample lie within two network distances, and therefore many firms still lie in the plausibly affected network distances.

D Doubly-robust Difference-in-differences from [Sant'Anna and Zhao \(2020\)](#) and [Callaway and Sant'Anna \(2021\)](#)

In this section, I shall explain the DRDID estimators from [Sant'Anna and Zhao \(2020\)](#) and [Callaway and Sant'Anna \(2021\)](#), which are extended versions of the semiparametric DID with inverse probability weighting (IPW) approach suggested by [Abadie \(2005\)](#). In contrast to the semiparametric DID approach that requires the model for propensity score is correctly specified, the DRDID approach is valid if either the propensity score model is correct or the outcome estimation model is correct.

Let G be the time period when firm i first experiences the treatment. G defines to which group units belong. For the case whereby a unit is never treated in any time pe-

riod, we arbitrarily set $G = \infty$. Denote by G_g a binary variable equal to one if a unit is first treated in period g (i.e., $G_{i,g} = \mathbf{1}\{G_i = g\}$) and let C be a binary variable equal to one for units that do not receive the treatment in any time period (i.e., $C_i = \mathbf{1}\{G_i = \infty\} = 1 - D_{i,T}$). Let $\mathcal{G} = \text{supp}(G) \setminus \{\bar{g}\} \subseteq \{2, 3, \dots, \mathcal{T}\}$ be the support of G .

The DRDID estimand with multiple time periods is define as

$$ATT_{dr}^{nev}(g, t; \delta) = \mathbb{E} \left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{\mathbb{E}\left[\frac{p_g(X)C}{1-p_g(X)}\right]} \right) (Y_t - Y_{g-\delta-1} - m_{g,t,\delta}^{nev}(X)) \right], \quad (2)$$

where $m_{g,t,\delta}^{nev}(X) = \mathbb{E}[Y_t - Y_{g-\delta-1} \mid X, C = 1]$, $\delta \geq 0$ is known and used to indicate the time period for the standard limited treatment anticipation by units, and $p_g(X)$ indicates the propensity score based on X covariates. [Callaway and Sant'Anna \(2021\)](#) demonstrate that we can use the time period $t = g - \delta - 1$ —i.e., the most recent time period when untreated potential outcomes are observed for units in group g .

Furthermore, as in Remark 12 of [Callaway and Sant'Anna \(2021\)](#), while the limited anticipation condition implies that $ATT(g; t) = 0$ for all $t < g - \delta$, it is common practice to also estimate these pre-treatment effects and use them to check the credibility of the underlying identifying assumptions. We can do this easily by replacing the “long differences” ($Y_t - Y_{g-\delta-1}$) with the “short differences” ($Y_t - Y_{t-1}$) for all $t < g - \delta$. I follow their suggestions and compare 1994 against 1993, 1995 against 1994, and so forth, and then use 1997 as the reference for all the post periods. I follow their suggestion to use a simple average to aggregate the ATT.

For the propensity score model, I estimate the following polynomial logistic regression, using $t - 1$ for the pre-treatment periods and $g - \delta - 1$ that is FY1997 in my case for the post-treatment periods:

$$\text{logit}(p(X_i)) = a_0 + aX_i + bX_i^2 + cX_i^3 + e_i, \quad (3)$$

where X_i is a vector of covariates including the outcome of the previous period, the natural logarithm of capital stock, that of employment, that of the shortest physical distance to any of the treated firms being connected, and the average physical distance to the treated firms. The first three covariates are measures of firm sizes, and the other two are used since geographic distance is known to affect a firm linkage. Furthermore, I include the polynomials of these covariates up to the third order since overfitting costs little but since the fourth order turns out to be not only computationally difficult but also fails to converge. For the actual implementation, I follow [Rios-Avila et al. \(2023\)](#) and use *csdid* Stata package written by researchers including Callaway and Sant’Anna.

To take out the direct effects on the connected firms that may vary across different network distances, I control for the (finest-level) industry-by-year-by-prefecture fixed effects. Note that since some industries perfectly predict network distance in my sample, the inclusion of these fixed effects as covariates in the selection equation will violate the overlap assumption. Then, I use an outcome residualized by these fixed effects. While partialling out fixed effects only from the left-hand side of a reduced-form equation is a common practice in empirical literature, as pointed out by [Gormley and Matsa \(2014\)](#), not partialling out the fixed effects from the right-hand side could result in an omitted variable bias in the estimates caused by a potential correlation between the treatment and fixed effects that are not partialled out. In my case, a potential omitted variable comes from a correlation between the network distance and industry-specific shocks that are uncorrelated with firm-level size and physical distance. As in the direct effects, I mitigate this concern by graphically showing the conditional parallel-trend in the pre-treatment periods.