Cascades of Tax Policy through Production Networks: Evidence from Japan

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Abstract

To recover from the economic disruptions caused by COVID-19, many countries have recently implemented investment stimulative tax policies. The effectiveness of such tax policies has been evaluated conventionally by the effects on firms directly affected by the tax policies. However, the indirect effects through the supply chains of the affected firms can be of first-order importance. This paper estimates the indirect effects of tax incentives for investment on firm performance through production networks, exploiting a quasi-experimental event of an investment stimulus policy targeting small and medium enterprises and unique proprietary data of supply chains in Japan. After confirming the direct effects, I find that the indirect effects on the direct suppliers are even larger than the direct effects, while I find no effects on downstream firms. Given that the direct suppliers tend to be large in size, the policy resulted in a trickle-up effect and unintendedly benefited untargeted firms.

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

JEL Codes: H25

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1 Introduction

The effectiveness of tax incentives for investment has been evaluated by effects on those firms which are directly affected by the tax policies. However, *indirect* effects through the supply chains of the affected firms can be of first-order importance. An increase in output induced by an increased capital investment due to the tax policies must be followed by a higher demand for the actual ingredients of output to increase output (e.g., screws needed to assemble a car), leading to a potential upstream propagation. Meanwhile, an exogenous increase in the supply of the directly affected firms' output would drive down the market price of their products, possibly benefiting the downstream firms.

This paper estimates these 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. The policy I study, "bonus" depreciation, allows firms to deduct an additional percentage of capital expenditures in the first year of an asset's tax life. While this tax policy did not target particular industries, there is a variation in the degree of policy benefits emerging from discounting 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 and the firm-level input-output linkage data, I construct the supply chains of those firms with greater policy exposures 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 spillover 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. In particular, I found out that the indirect effects on the direct suppliers of the most affected firms are even larger than the direct effects. Furthermore, in contrast to the the literature that finds significant negative effects of a natural disaster on downstream firms, I found no statistically significant, positive spillover effects of a tax incentive on downstream firms.

These findings have the following implication. First, the fact that the direct suppliers of the treated firms benefited more than the treated firms might be driven by that one direct supplier supplies to multiple treated firms. Consistent with this hypothesis, there are much fewer firms in the direct supplier category than the treated firms. This tends to result in greater average treatment effects on the direct suppliers than the treated firms. This implies that the investment stimulus policies benefit the bottleneck suppliers more than the target firms.

Second, such large spillover effects are not observed among the customers of the treated firms. This is a stark contrast to the existing research on spillover effects in production networks. Carvalho et al. (2021) find that the negative shock of the 2011 Great East Japan Earthquake that hit a certain region of Japan resulted in negative spillover effects on upstream and downstream firms almost at equal magnitudes with a slightly stronger effects on downstream firms, and the effects fade away at the farther network distance. The pricing effects were stronger in their setting perhaps because of price rigidity. It tends to be easier for firms to increase the price of their products when there is a legitimate reason such as a natural disaster, rather than to decrease the price now to sell more and increase the price in the future when there is an idiosyncratic shock on the cost.

These findings are developed in two steps. The first stage is the estimation of direct effects. To estimate, I apply to Japan the established exposure measure from Zwick and Mahon (2017) (Hitherto, ZM) that studies the direct effects of the tax policy in the U.S. The main assumption is that within the same industry, the Japan and U.S. firms purchase similar equipment. Using this measure, I found statistically significant direct effects consistent with ZM.

After establishing the first-stage effects, I estimate the spillover effects using the supply chain data. For the supply chain data, I rely on a proprietary dataset collected by a major private credit reporting agency that has information on roughly half of all private and publicly-traded firms in Japan and covers almost all firms with more than five employees across all sectors of the economy. For each firm-year, I observe a set of firm-level characteristics as well as the identities of the firm's suppliers and customers, thus enabling me to construct the supply chain relationships for the firms in my sample. Using this unique dataset, I found statistically and economically meaningful spillover effects.

To establish these findings, I exploit a difference-in-differences approach for both steps. For the first step, I follow ZM in comparing firms' sale values in industries that, on average, invest in long-lived assets to firms' sales in industries that invest in short-lived assets. To categorize industries into ones with long-lived and short-lived assets, I apply to Japanese industries the ZM policy measure that uses the U.S. data. While there is a difference in class life definition between the U.S. and Japan, I manually check all the listed class lives of equipment between the two countries and find a high positive correlation roughly at 0.7. Partly to reduce the measurement error coming from a discrepancy, I follow Garrett et al. (2020) to construct a binary treatment variable. With this, as long as Japanese and U.S. firms have a tendency to purchase similar equipment, the policy measure I use can capture the direct effects of the policy.

Using this policy measure, first, I compare these treated firms to those untreated firms that are far distant away in the treated firms' supply chains using a difference-in-differences (DID) framework. This is a similar strategy to Carvalho et al. (2021), and I chose it for two reasons. First, very few firms do not lie in the affected firms' supply chains. Second, the direct transaction partners of the affected firms are expected to be contaminated by the spillover effects of the policy. The assumption behind this econometric design is the lack of a differential trend between the treated and control group firms prior to 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 demonstrate graphically that changes in sales are uncorrelated with the policy exposure prior to the initial implementation. Second, I show that my results are robust after controlling for (medium level) subsector-by-year-by-prefecture fixed effects, implying that (1) the threat coming from differential trends across subsectors is limited, (2) prefecture-level policies or shocks do not confound my estimates, and (3) local subsector trend is also not a confouding factor. With the empirical strategy, my baseline estimation shows that treated firms' sales grew by approximately 4 to 5% after the policy implemented in 1998 for 1993-2003 sample depending on specifications.

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 post-policy sales of firms at different distances—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, so these firms tend to be the direct transaction partners of the affected firms.

To alleviate this issue, I resort to conditional DID methods. These methods rely on a conditional parallel trend assumption by covariates that predict the supply chain distance. In particular, I follow the doubly-robust difference-in-differences (DRDID) estimators proposed by Sant'Anna and Zhao (2020), which is an extended version 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 to be correctly specified, the DRDID approach is valid if either the propensity score model or the outcome estimation model is correct. For the propensity score model, since it is well-known that physical distance plays a major role in forming a transaction link between firms, the main covariate used in this paper that predicts the selection into the closer distance position in the supply chain is the shortest physical distance between a firm and the treated firm to which the firm is connected in its supply chain.

I support the conditional parallel trend assumption in a number of ways. Since the input-output linkages are at the firm level unlike the policy exposure measure that is at the industry level, I can control for the smallest-level industry-by-year-by-prefecture fixed effects. Then, by including such fixed effects, I can greatly alleviate identification risks from (1) differential trends across industries, (2) prefecture-level policies or shocks, and (3) local industry trends. Then, 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 interpret the result with caution. 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 two strands of literature. One is a growing literature that studies the impacts of investment stimulative tax policies. The previous 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 a first contribution to the literature by demonstrating the significance of indirect effects through production networks.

The other is a growing literature of production networks. Among related papers, 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. Liu (2019) develops a rich model of industrial policies and production networks and extensively tests it with empirics. Estimating 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, my study complements these studies in the production network literature. My paper uses granular firm-level network data and provides distinct results that unlike a negative shock, a positive shock such as investment stimulus may not cascade over downstream firms due to price rigidity. Furthermore, my paper finds that the spillover effects are likely to be affected by the network structure such as bottlenecks.

2 Policy Background

The Japanese government implemented 30% accelerated depreciation in June 1998 for small and medium enterprises (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 was a response to Asian Currency Crisis in 1997. 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. 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 can be found at the Appendix, the main difference between SMEs and large corporations is whether a firm has more than 100 million yen common stock.[1] Note that these policies are endogenous of the macroeconomic shock which impedes the identification of direct and indirect effects through a simple pre-post time-series estimation approach. Thus, one needs an identification method that is plausibly exogenous to the shock.

2.1 Treatment Intensity Measure Constructed by ZM

To address this issue, 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^0s 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 $1v$ percent are depreciated following the MACRS rules. Then, the policy reduces the present value cost of investment by $v(1z^0)$. Since 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.

[1] Common stock is part of shareholders' equity in balance sheets.

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^0s 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. As noted in Garrett et al. (2020), there is a considerable variation in z_j^0's even within a specific sector. Later, I conduct a similar exercise after applying this measure to Japanese industries.

2.2 Application of ZM Treatment Measure to Japanese Industries

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.[2] I conducted a thorough manual comparison of of all the asset items for depreciation between the U.S. in 2002 and Japan in 1998 and finds a high positive correlation at 0.7. With this, as long as Japanese and U.S. firms have a tendency to purchase similar equipment if they are in the same industry, the policy measure I use can capture the direct effects of the policy in Japan.[3]

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.[4] 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

[2] Komori (2003) compares Japanese and US depreciation systems. His paper implies that while there are differences in the length of depreciation periods for many goods, the length tends to be similar (e.g., computer is 5 years in US and 6 years in Japan.)

[3] Not exactly the same, but Japan used similar depreciation rules to MARCS in 1998. See Komori (2003).

[4] The 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.

treatment intensity file.[5]

Partly to reduce the measurement error coming from a discrepancy, 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. According to them, 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 30th percentile as the cutoff for the balance across the sample sizes of different network distances including the treatment and control group. I conduct robustness checks with 20th and 40th percentiles of the z_j^0 distribution as cutoffs. I find that the results remain unchanged.

3 Data

I rely on a proprietary dataset compiled by the private credit reporting agency Teikoku Data Bank Ltd. (henceforth, TDB) to construct a firm-level production network of supplier-customer linkages. Generally speaking, firms give information to TDB in the course of obtaining credit reports on potential suppliers and customers. This information contains a set of firm-level characteristics, together with the identities of the firms' suppliers and customers. The TDB data uses its only 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

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

Table 1: 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.

investment only after 2000 with a restricted sample.

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. Consequently, 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 1993 fiscal year (FY) since the TDB industry code started using in 1993 a new industry code that match 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.[6] This leaves us with a balanced panel data of 521,792 firms across all the prefectures in Japan.

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 interests started in June 1998, and 1999 Economics Census is the census conducted closest to this time period. Figure 1 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 1 shows the comparison of geographic distribution at the prefecture level using the same two datasets. We can see no major difference between the two.

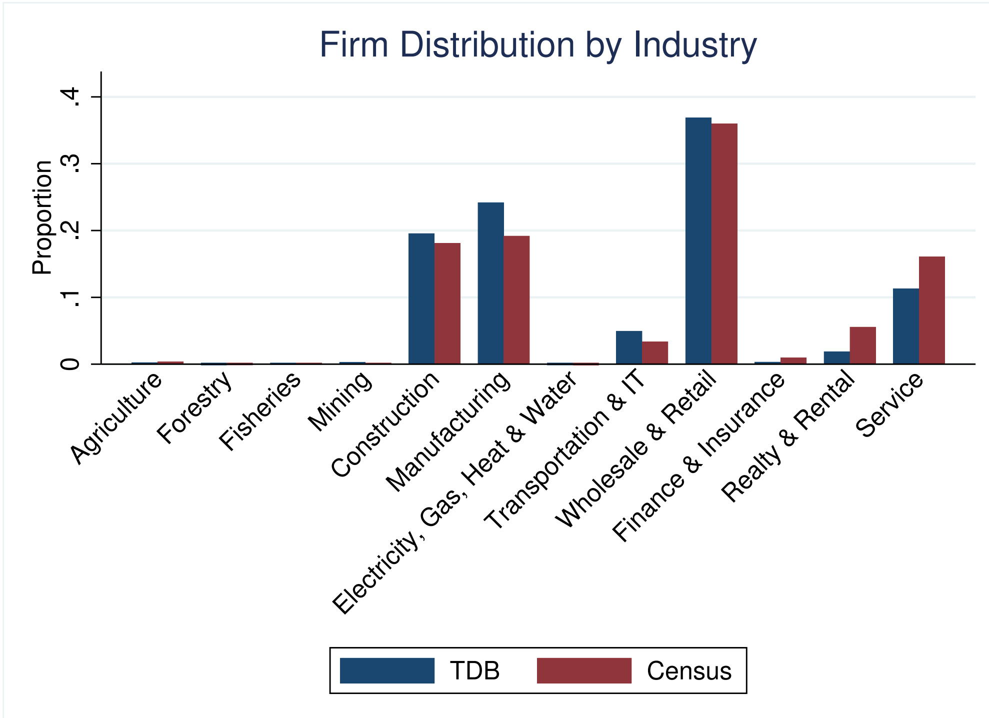
On the other hand, partly because I use a relatively long period (11 years) and require the dataset to be balanced, the final dataset contains very small firms relatively fewer than the 1999 census. Table 1

Each firm in the TDB dataset also provides a list of its transaction partners, allowing

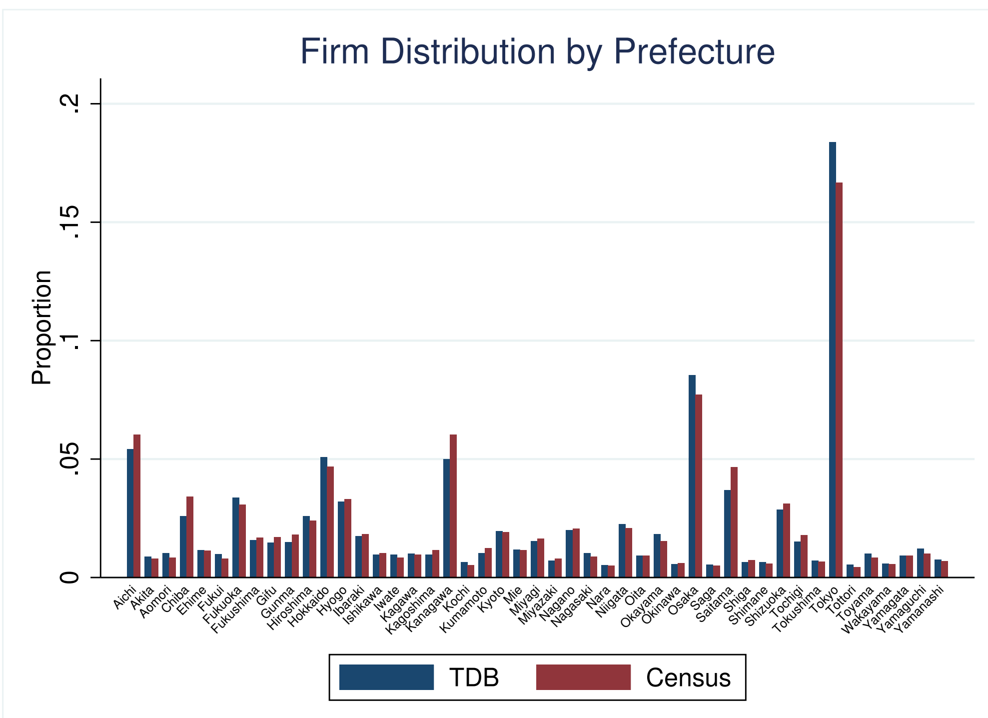
[6] I tried different year ranges, and the results remain unchanged.

Figure 1: Industrial Composition and Geographical Distribution

(a) Comparison of Industrial Composition between TDB and Census



(b) Comparison of Prefecture Composition between TDB and Census



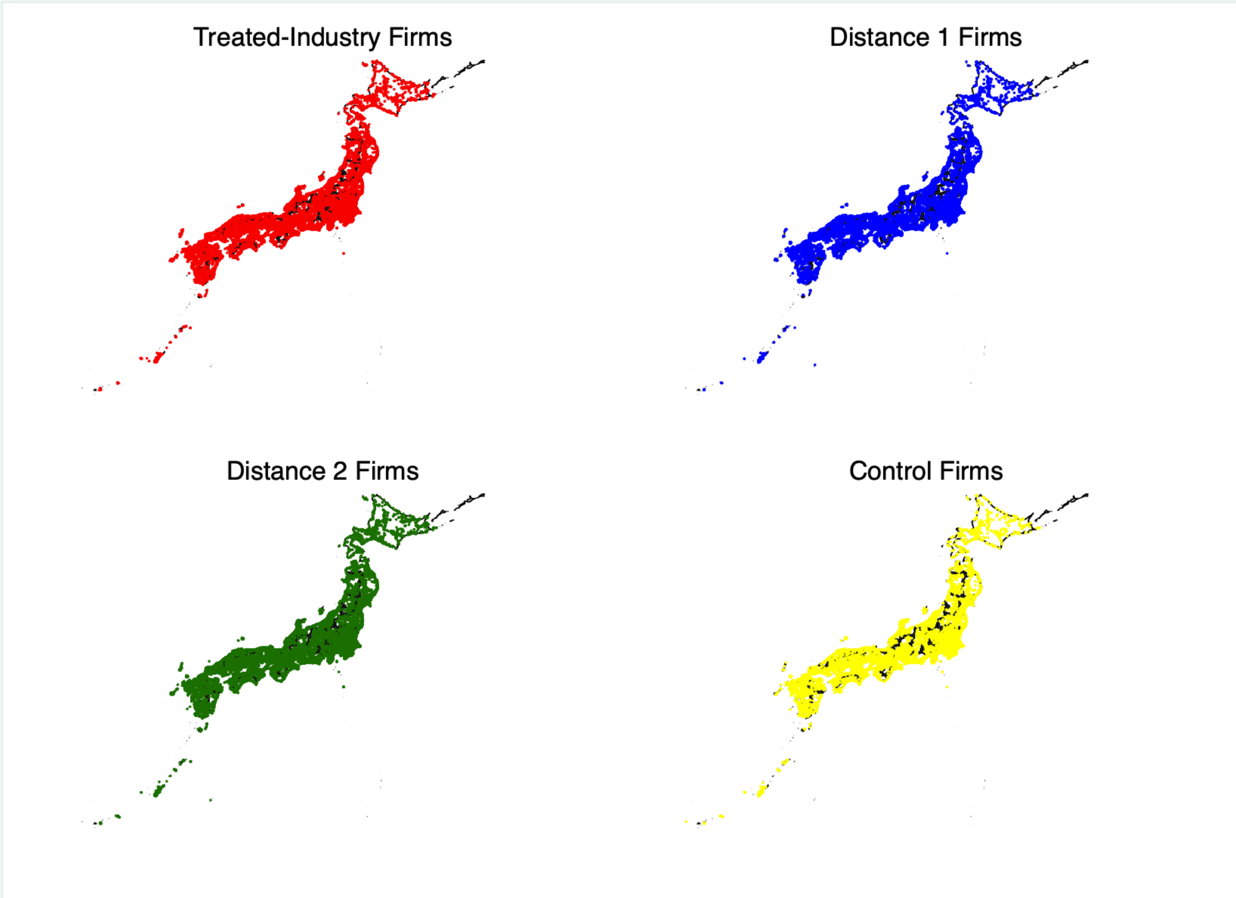
me to construct the production network of supplier-customer linkages for the firms in my sample. Given the occurrence of the tax policy in June 1998, I construct this network using the transaction data collected as of January 1998.

The TDB-based supplier-customer linkages have two limitations. First, the data only captures a binary measure of inter-firm supplier-customer relations. While the data contains information on whether one firm is another firm's supplier or customer, I do not observe a yen measure on their transaction volume.

Second, the forms used by TDB limit the number of suppliers and customers that firms can report to 9 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 9 transaction partners per category, including gigantic firms that transact with several thousand firms. With this procedure, 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 set of directed affected industry firms for all firms in my sample. 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 listed in 1998 as a customer of at least one downstream distance 1 firm and was not a distance 1 firm itself. With a similar recursive procedure, I identify the set of firms at various upstream and downstream distances from treated industry firms right before the intervention of policy intervention. 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. One may think that those

Figure 2: Location of Firms



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.

Figure 2 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.

Table 2 summarizes baseline characteristics of firms in fiscal year 1997. 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. These large firms may

Table 2: Summary Statistics by Distance

	Treated	Upstream 1	Downstream 1	Upstream 2	Downstream 2	Control
Log of Sales	6.00	6.84	6.88	6.23	6.09	5.32
	(1.28)	(1.77)	(1.77)	(1.40)	(1.34)	(1.12)
Log of Employment	2.79	3.15	3.34	2.93	2.70	2.24
	(1.07)	(1.46)	(1.49)	(1.20)	(1.14)	(0.94)
Log of Capital Stock	9.40	9.97	10.14	9.69	9.52	9.16
	(0.88)	(1.57)	(1.63)	(1.13)	(1.05)	(0.90)
Age	27.91	29.75	29.72	25.99	25.89	22.40
	(12.99)	(14.48)	(14.46)	(12.86)	(12.75)	(11.85)
N of Suppliers	2.75	12.22	11.30	3.79	2.89	1.61
	(6.51)	(75.41)	(63.59)	(8.81)	(2.79)	(1.09)
N of Customers	3.40	16.13	14.49	3.70	4.37	1.75
	(6.45)	(71.13)	(70.27)	(7.33)	(10.84)	(1.70)
Customers' log sales	16.16	16.77	15.78	16.39	17.18	14.50
	(2.52)	(2.42)	(2.25)	(2.41)	(2.11)	(1.66)
Suppliers' log sales	16.30	15.80	16.76	18.31	16.83	14.75
	(2.69)	(2.41)	(2.73)	(2.33)	(2.84)	(2.12)
Observations	75700	42598	56166	127022	172630	65852

have differential trends compared to SME control group firms, and therefore I overcome the selection issue by an alternative approach that allows for covariate-specific trends, which will be discussed in the next section. My main covariates for the selection model include the size-related variables and the physical distance from the treated firms with which these firms transact since physical distance is known to be one of the primary factors of firm linkages.

With this TDB dataset, I apply the ZM treatment intensity measures to TDB-defined industries (hitherto TDB industries) that closely follow 4-digit JSIC.[7] As done by Garrett et al. (2020), I compute the coefficient of variation within each sector. Figure B1 demonstrates a considerable variation in the z_j^0's even when the ZM measures are applied to Japanese firms by showing the coefficient of variation within each sector normalized by that of the manufacturing sector.

[7] When 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.

4 Econometric Framework

There are two steps. First, I estimate the direct effects of the tax policy on firms' performance.

$$Y_{ipNt} = \alpha_i + \delta_{pt} + \beta D_{N,t} + \varepsilon_{ipNt}, \tag{1}$$

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, and $D_{N,t}$ is the difference-in-differences term that is equal to 1 for the treated firms at period between 1998 and 2003 and 0 in the other periods and 0 for the firms that are 3 or more distance away in the treated firms' transaction networks throughout all the periods. All the regressions in this paper are clustered at the TDB industry level that corresponds to 4-digit NAICS levels, to address the concerns raised by Bertrand et al. (2004) about errors being correlated within policy units (industries). As noted above, we cannot use large firms in the treated industries as a control group since 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.

The identification threat to the estimation of β is the lack of a parallel trend on the outcome variable prior to the policy intervention in 1998. 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 more suitable to justify the underlying assumption.

As supports for the parallel trend assumption, I provide pre-trend analysis results. I estimate the following pre-trend analysis specification;

$$Y_{ipNt} = \kappa_i + \kappa_{pt} + \sum_{t \neq 1998} \beta_t D_N \lambda_t + \hat{\varepsilon}_{ipNt}, \tag{2}$$

where $D_N = 1$ 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. I cluster standard errors at N industry level since the treatment category is at N industry level.

Next, I turn to the second stage estimation on spillover effects. Following Carvalho et al. (2021), I estimate the following equation.

$$
\begin{aligned}
Y_{ipNt} = \gamma_i + \gamma_{pNt} + \sum_{k=1}^{2} \beta_k^{\text{down}} \times \text{Downstream}_i^{(k)} \times \text{Post}_t \\
+ \sum_{k=1}^{2} \beta_k^{\text{up}} \times \text{Upstream}_i^{(k)} \times \text{Post}_t + u_{ipNt},
\end{aligned}
\tag{3}
$$

where the comparison group is those firms in 3 or more distance away. Note that since transaction connections are at the firm level, I can control for local industry trends captured by γ_{pNt}. This fixed effect controls for transaction partners' own exposure to the tax policy as well. The identification threat is similar to the first stage, so I conduct similar pre-trend checks.

$$
Y_{ipNt} = \zeta_i + \zeta_{pNt} + \sum_{t \neq 1993} \beta_t^{(k)} Q_i^{(k)} \lambda_t + \xi_{ipNt}, \tag{4}
$$

where $Q_i^k = 1$ if $Q \in \{\text{upstream}, \text{downstream}\}$ firm i is connected to the treated firms at distance k and 0 otherwise, $\lambda_t = 1$ event if event time $= t$, and we include FY 1993 as reference year. This pre-trend specification for the spillover effects graphically results in the violation the parallel trend assumption.[8] This is likely to come from the fact that large firms transact with more firms in general than small firms. For example, Toyota has a final assembly line and purchases all the necessary parts for its automobiles from thousands of different suppliers at various levels of its supply chain. Given this, I employ an alternative specification to allow for covariate-specific trends.

[8] Results available upon request.

4.1 Doubly-robust Difference-in-differences

In particular, I follow the Doubly-robust difference-in-differences (DRDID) estimators from Sant'Anna and Zhao (2020), which is an extended version 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.

4.1.1 Set-up

To formally introduce the estimator and its assumptions, I will introduce some new notations. Readers who are familiar with the estimator and the paper can safely skip this section. I first focus on a canonical two-period DID setup and introduce the definition and assumptions of DRDID estimators following Sant'Anna and Zhao (2020). Next, I will the case with multiple periods following Callaway and Sant'Anna (2021) and introduce their suggested way to apply DRDID to the multiple-period case. For both cases, I focus on a case in which researchers have access to panel data sets in this paper.

Suppose we have two periods: $t = 0$ as a pre-treatment period, while $t = 1$ as a post-treatment period. Let Y_{it} be the outcome of interest for unit i at time t. We assume that one has access to outcome data at $t = 0$ and $t = 1$. Let $D_{i,t}$ be a binary variable equal to one if unit i is treated in period t and equal to zero otherwise. Since we focus on a canonical DID set-up first, $D_{i0} = 0$ for every i, which allows us to write $D_i = D_{i1}$. Using the standard potential outcome notation, let $Y_{it}(0)$ be the outcome of unit i at time t if i receives no treatment by time t and let $Y_{it}(1)$ be the outcome for the same unit if it receives treatment. Then, the realized outcome for unit i at time t is $Y_{it} = D_i Y_{it}(1) + (1 - D_i)\, Y_{it}(0)$. A vector of pre-treatment covariates is denoted by X_{it} or in a two-period case, just X_i. We assume that the first element of X_i is a constant. The following is the standard assumption in the literature.

Assumption 1. *The data* $\{Y_{it}, D_{it}, X_{it}\}_{i=1}^{n}$ *are independent and identically distributed (iid).*

Next, note that the parameter of interest is the average treatment effect on the treated (ATT):

$$\tau = \mathbb{E}\left[Y_{i1}(1) - Y_{i1}(0) \mid D_i = 1\right],$$

which can be written as

$$\tau = \mathbb{E}\left[Y_1(1) \mid D = 1\right] - \mathbb{E}\left[Y_1(0) \mid D = 1\right] = \mathbb{E}\left[Y_1 \mid D = 1\right] - \mathbb{E}\left[Y_1(0) \mid D = 1\right] \quad (5)$$

where I drop subscript i to simplify notation and follow this convention through this paper. Now, I am ready to introduce the remaining standard assumptions in conditional DID methods, the conditional parallel trend assumption (PTA) and overlap assumption:

Assumption 2. *(conditional PTA)* $\mathbb{E}\left[Y_1(0) - Y_0(0) \mid D = 1, X\right] = \mathbb{E}\left[Y_1(0) - Y_0(0) \mid D = 0, X\right]$ *almost surely (a.s.).*

Assumption 3. *(overlap) For some* $\varepsilon > 0, \mathbb{P}(D = 1) > \varepsilon$ *and* $\mathbb{P}(D = 1 \mid X) \leq 1 - \varepsilon$ *a.s.*

Assumption 2 states that in the absence of the treatment, the average conditional outcome of the treated and the control groups would have evolved in a parallel manner. The important difference from the standard PTA is that Assumption 2 permits covariate-specific time trends, although it rules out unit-specific trends. On the other hand, Assumption 3 states that at least a small portion of the population is treated while for every value of X, there is at least a small likelihood that a unit is not treated.

Under Assumptions 1-3, there are two main estimation procedures to estimate the ATT. One is the outcome regression (OR) approach such as done in Heckman et al. (1997) which relies on researchers' ability to model the outcome evolution. Given the assumptions, one can estimate the ATT with

$$\widehat{\tau}^{\text{reg}} = \bar{Y}_{1,1} - \left[\bar{Y}_{1,0} + n_{\text{treat}}^{-1} \sum_{i \mid D_i = 1} \left(\widehat{\mu}_{0,1}\left(X_i\right) - \widehat{\mu}_{0,0}\left(X_i\right)\right)\right], \quad (6)$$

where $\bar{Y}_{d,t} = \sum_{i|D_i=d,T_i=t} Y_{it}/n_{d,t}$ is the sample average outcome among units in treatment group d and time t, and $\widehat{\mu}_{d,t}(x)$ is an estimator of the true, unknown $m_{d,t}(x) \equiv \mathbb{E}\left[Y_t \mid D = d, X = x\right]$.

The other approach is the inverse probability weighted (IPW) approach suggested by Abadie (2005). This approach avoids directly modeling the outcome evolution and exploits the fact that under Assumptions 1-3, the ATT can be written as

$$\tau = \frac{1}{\mathbb{E}[D]}\mathbb{E}\left[\frac{D - p(X)}{1 - p(X)}\left(Y_1 - Y_0\right)\right]. \tag{7}$$

Abadie (2005) proposes the following IPW estimator:

$$\widehat{\tau}^{ipw,p} = \frac{1}{\mathbb{E}_n[D]}\mathbb{E}_n\left[\frac{D - \widehat{\pi}(X)}{1 - \widehat{\pi}(X)}\left(Y_1 - Y_0\right)\right], \tag{8}$$

where $\hat{\pi}(x)$ is an estimator of the true, unknown $p(X)$. and for a generic random variable Z such that $\mathbb{E}_n[Z] = n^{-1}\sum_{i=1}^{n} Z_i$.

The DRDID estimand combines these two approaches to form doubly robust moments/estimands for the ATT. Let $\pi(X)$ be an arbitrary model for the true, unknown propensity score. When panel data are available, let $\Delta Y = Y_1 - Y_0$ and define $\mu^p_{d,\Delta}(X) \equiv \mu^p_{d,1}(X) - \mu^p_{d,0}(X)$, $\mu^p_{d,t}(x)$ being a model for the true, unknown outcome regression $m^p_{d,t}(x) \equiv \mathbb{E}\left[Y_t \mid D = d, X = x\right], d, t = 0, 1$. Given these notations, the DRDID estimand is defined as

$$\tau^{dr,p} = \mathbb{E}\left[\left(w_1^p(D) - w_0^p(D, X; \pi)\right)\left(\Delta Y - \mu^p_{0,\Delta}(X)\right)\right], \tag{9}$$

where, for a generic g,

$$w_1^p(D) = \frac{D}{\mathbb{E}[D]}, \quad \text{and} \quad w_0^p(D, X; g) = \frac{g(X)(1-D)}{1-g(X)} \Big/ \mathbb{E}\left[\frac{g(X)(1-D)}{1-g(X)}\right]. \tag{10}$$

The generic DRDID estimators are in the following form:

$$\widehat{\tau}^{dr,p} = \mathbb{E}_n \left[\left(\widehat{w}_1^p(D) - \widehat{w}_0^p(D, X; \widehat{\gamma}) \right) \left(\Delta Y - \mu_{0,\Delta}^p \left(X; \widehat{\beta}_{0,0}^p, \widehat{\beta}_{0,1}^p \right) \right) \right], \tag{11}$$

where

$$\widehat{w}_1^p(D) = \frac{D}{\mathbb{E}_n[D]}, \quad \text{and} \quad \widehat{w}_0^p(D, X; \gamma) = \frac{\pi(X;\gamma)(1-D)}{1-\pi(X;\gamma)} / \mathbb{E}_n \left[\frac{\pi(X;\gamma)(1-D)}{1-\pi(X;\gamma))} \right], \tag{12}$$

such that $\widehat{\gamma}$ is an estimator for the pseudo-true γ^*, $\widehat{\beta}_{0,t}^p$ is an estimator for pseudo-true $\beta_{0,t}^{*,p}$, $t = 0, 1$, and for a generic β_0 and β_1, $\mu_{0,\Delta}^p(\cdot; \beta_0, \beta_1) = \mu_{0,1}^p(\cdot; \beta_1) - \mu_{0,0}^p(\cdot; \beta_0)$.

Sant'Anna and Zhao (2020) provide some guidance on the choice of first-step estimators to further improve the generic DRDID estimators. They propose the so-called "improved" DRDID estimator for the ATT proposed by Sant'Anna and Zhao (2020) which focuses on the case where a researcher is comfortable with linear regression working models for the outcome of interest, a logistic working model for the propensity score, and with covariates X included in all the nuisance models in a symmetric manner. Then, we consider the case in which

$$\pi(X, \gamma) = \Lambda\left(X'\gamma\right) \equiv \frac{\exp\left(X'\gamma\right)}{1+\exp\left(X'\gamma\right)}, \text{ and } \mu_{0,\Delta}^p\left(X; \beta_{0,1}^p, \beta_{0,1}^p\right) = \mu_{0,\Delta}^{lin,p}\left(X; \beta_{0,\Delta}^p\right) \equiv X'\beta_{0,\Delta}^p. \tag{13}$$

Their improved DRDID estimator is provided by the following three-step estimator

$$\widehat{\tau}_{imp}^{dr,p} = \mathbb{E}_n \left[\left(\widehat{w}_1^p(D) - \widehat{w}_0^p\left(D, X; \widehat{\gamma}^{ipt}\right) \right) \left(\Delta Y - \mu_{0,\Delta}^{\text{lin,p}}\left(X; \widehat{\beta}_{0,\Delta}^{wls,p}\right) \right) \right], \tag{14}$$

where the first two-steps consist of computing

$$\hat{\gamma}^{ipt} = \arg\max_{\gamma \in \Gamma} \mathbb{E}_n \left[DX'\gamma - (1-D)\exp\left(X'\gamma\right) \right]$$

and

$$\widehat{\beta}_{0,\Delta}^{wls,p} = \arg\min_{b\in\Theta} \mathbb{E}_n \left[\frac{\Lambda\left(X'\hat{\gamma}^{ipt}\right)}{1-\Lambda\left(X'\hat{\gamma}^{ipt}\right)} \left(\Delta Y - X'b\right)^2 \mid D=0 \right],$$

where in the third and last step, one plugs the fitted values of the working models (13) into the sample analogue of $\tau^{dr,p}$.

Given this estimator, now we are ready to extend it to multiple periods. Consider the case with $\mathcal{T}$ periods and denote a particular time period by t where $t = 1, ..., \mathcal{T}$. The first assumption is a standard one.

Assumption 4. *(Irreversibility of Treatment). $D_1 = 0$ almost surely (a.s.). For $t = 2, \ldots, \mathcal{T}$,*

$$D_{t-1} = 1 \text{ implies that } D_t = 1 \text{ a.s..}$$

Assumption 4 states that no i is treated at time $t = 1$, and that once i is treated, i will remain treated in the next period.

Let G be the time period when i first experiences the treatment. Under Assumption 4, for all units that eventually participate in the treatment, G defines to which group they belong. For the case whereby a unit is never treated in any time period, 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 $\mathcal{G} = \text{supp}(G)\backslash\{\bar{g}\} \subseteq \{2, 3, \ldots, \mathcal{T}\}$ be the support of G. Given these notations, the ATT of interest for a particular group g and time t is

$$ATT(g,t) = \mathbb{E}\left[Y_t(g) - Y_t(0) \mid G_g = 1\right]. \tag{15}$$

Now, with multiple time periods, we need the following standard no anticipation assumption.

Assumption 5. *(Limited Treatment Anticipation). There is a known $\delta \geq 0$ such that*

$$\mathbb{E}\left[Y_t(g) \mid X, G_g = 1\right] = \mathbb{E}\left[Y_t(0) \mid X, G_g = 1\right] \text{ a.s. for all } g \in \mathcal{G}, t \in \{1, \ldots, \mathcal{T}\} \text{ such that } t < g - \delta.$$

This assumption is satisfied in general when the treatment path is not a priori known and/or when units are not the ones who select treatment status. The next assumption the version of the conditional PTA with multiple time periods:

Assumption 6. *(Conditional Parallel Trends based on a "Never-Treated" Group). Let δ be as defined in Assumption 5. For fach $g \in \mathcal{G}$ and $t \in \{2, \ldots, \mathcal{T}\}$ such that $t \geq g - \delta$,*

$$\mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid X, G_g = 1\right] = \mathbb{E}\left[Y_t(0) - Y_{t-1}(0) \mid X, C = 1\right] \text{ a.s..}$$

Lastly, the following is the muti-period version of the overlap assumption:

Assumption 7. *(Multi-period overlap). For each $t \in \{2, \ldots, \mathcal{T}\}, g \in \mathcal{G}$, there exist some $\varepsilon > 0$ such that $P\left(G_g = 1\right) > E$ and $p_{g,t}(X) < 1 - \varepsilon$ a.s..*

The DRDID estimand with multiple time periods is define as

$$ATT_{dr}^{nev}(g, t; \delta) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}\left[G_g\right]} - \frac{\frac{p_g(X)C}{1-p_g(X)}}{\mathbb{E}\left[\frac{p_g(X)C}{1-p_g(X)}\right]}\right)\left(Y_t - Y_{g-\delta-1} - m_{g,t,\delta}^{nev}(X)\right)\right], \tag{16}$$

where $m_{g,t,\delta}^{nev}(X) = \mathbb{E}\left[Y_t - Y_{g-\delta-1} \mid X, C = 1\right]$. 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—as an appropriate reference time period under Assumption 5 and Assumption 6.

Furthermore, as in their Remakr 12, 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.

As stated in Remark 7 of Callaway and Sant'Anna (2021), one should include pre-treatment covariates that are potentially associated with the outcome evolution of $Y(0)$ during the post-treatment periods. I follow their suggestion to use a simple average to aggregate the ATT. For the propensity score model, then, I estimate the following polynomial logistic regression, using $t - 1$ for the pre-treatment periods and $g - \delta - 1$ for the post-treatment periods:

$$\text{logit}(p(X_i)) = a_0 + aX_i + bX_i^2 + cX_i^3 + e_i, \tag{17}$$

where X_i is a vector of covariates including (i) the natural logarithm of the shortest and average physical distance between firm i and treated firms and (ii) the natural logarithm of sales, shareholder's equity, and the number of employees in the pre-treatment period. (i) is included since it is well-known that a transaction linkage between two firms is predicted well by geographical proximity between the two. To compute this distance measure, I use the longitude and latitude of firm i and one of the i's direct or indirect transaction partners in the treatment group who are located closest to i. (ii) is included because large firms transact with more firms in general than small firms and because growing companies tend to increase the number of transaction partners.

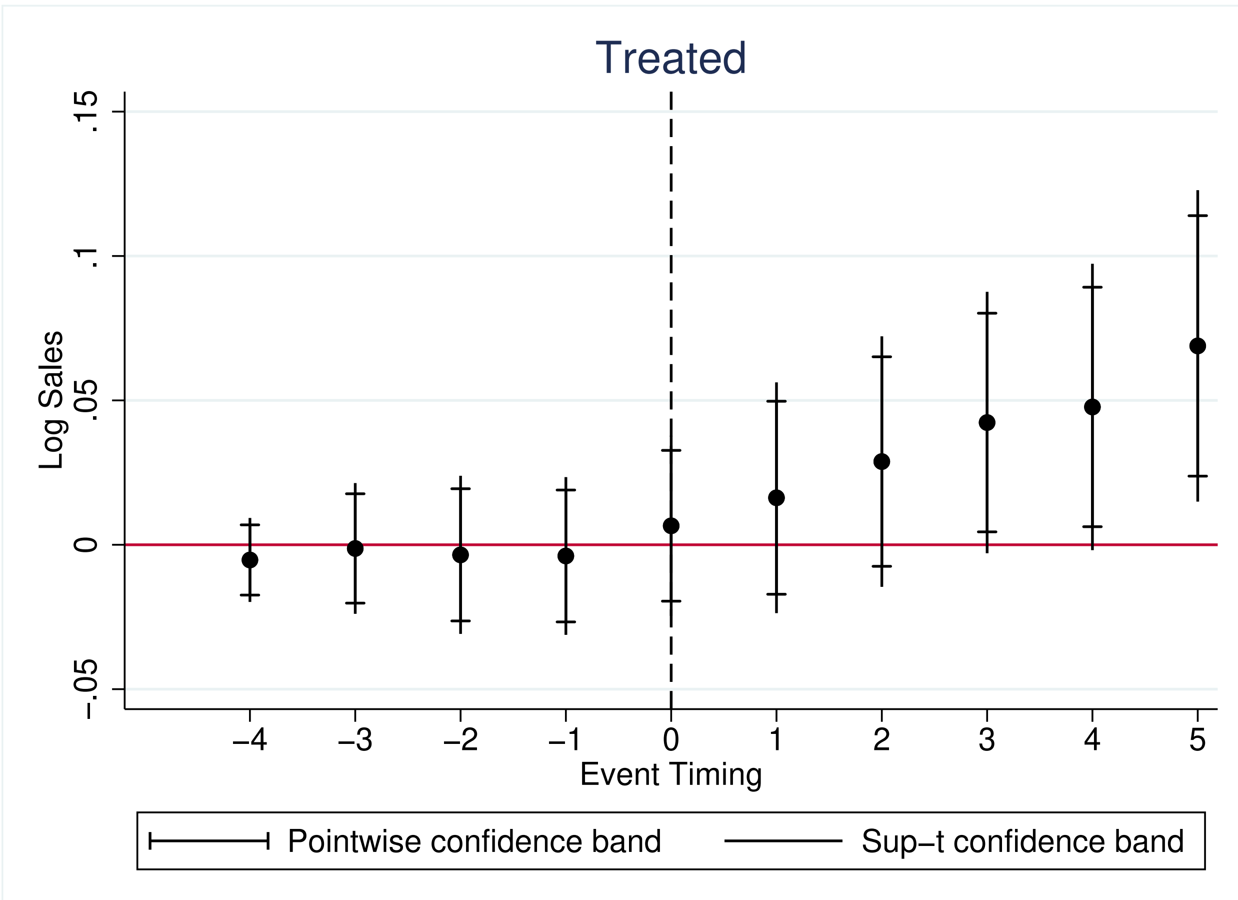
When I plot the dynamics of the effects, I follow the best practices of event study plots suggested by Freyaldenhoven et al. (2021) and use the (simultaneous) uniform confidence bands for the standard DID and similar uniform bands proposed by Callaway and Sant'Anna (2021) for DRDID estimators to graphically 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.

5 Results

In Figure 3, x-axis indicates time period where 0 is equal to FY1998 that corresponds to the year the policy took effect, -1 means FY 1997, and so forth. It displays the coefficients and 95% confidence interval from the regression of equation (2) and appears to support the parallel trend. I exclude the reference year 1993 from the display in the graph.

Table 3 demonstrates the direct effects estimated by equation (1). The first column corresponds to the baseline model of equation (1), the second column includes two-digit JSIC industry-by-prefecture-by-year fixed effects, the third column restricts the control group sample to SMEs whose capital stock level is smaller than 100 million yen roughly equal to one million dollars, 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 logarithmic scale. One can see the statistically significant and economically meaningful direct effects at 4% increase in sales after the policy under the baseline model. Furthermore, the results are robust to the inclusion of 2-digit industry-by-year-by-prefecture fixed effects, mitigating the concern of industry-trend effects. Since only SMEs were eligible for the bonus depreciation, I conduct a robustness check by restricting the control group sample to SMEs. 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, which will be discussed in more details later. The last column is the same as the second column except that the

Figure 3: 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 FY 1997, and so forth. Coefficients with 95% confidence intervals and uniform sup-t confidence intervals from equation (2). The reference year 1993 is excluded to display from the graph.

Table 3: First-stage Direct Effects of Investment Stimuli

	(1) Sales	(2) Sales	(3) Sales	(4) Sales	(5) Employment
DD	0.038	0.039	0.049	-0.073	-0.003
	(0.014)	(0.016)	(0.017)	(0.020)	(0.017)
_cons	5.589	5.587	5.567	5.335	2.491
	(0.004)	(0.005)	(0.005)	(0.003)	(0.005)
N	1557072	1553959	1542904	1020426	1553959
FE for 2-digit JSIC	No	Yes	Yes	Yes	Yes
SME Control	No	No	Yes	No	No
Unprofitable Firm	No	No	No	Yes	No

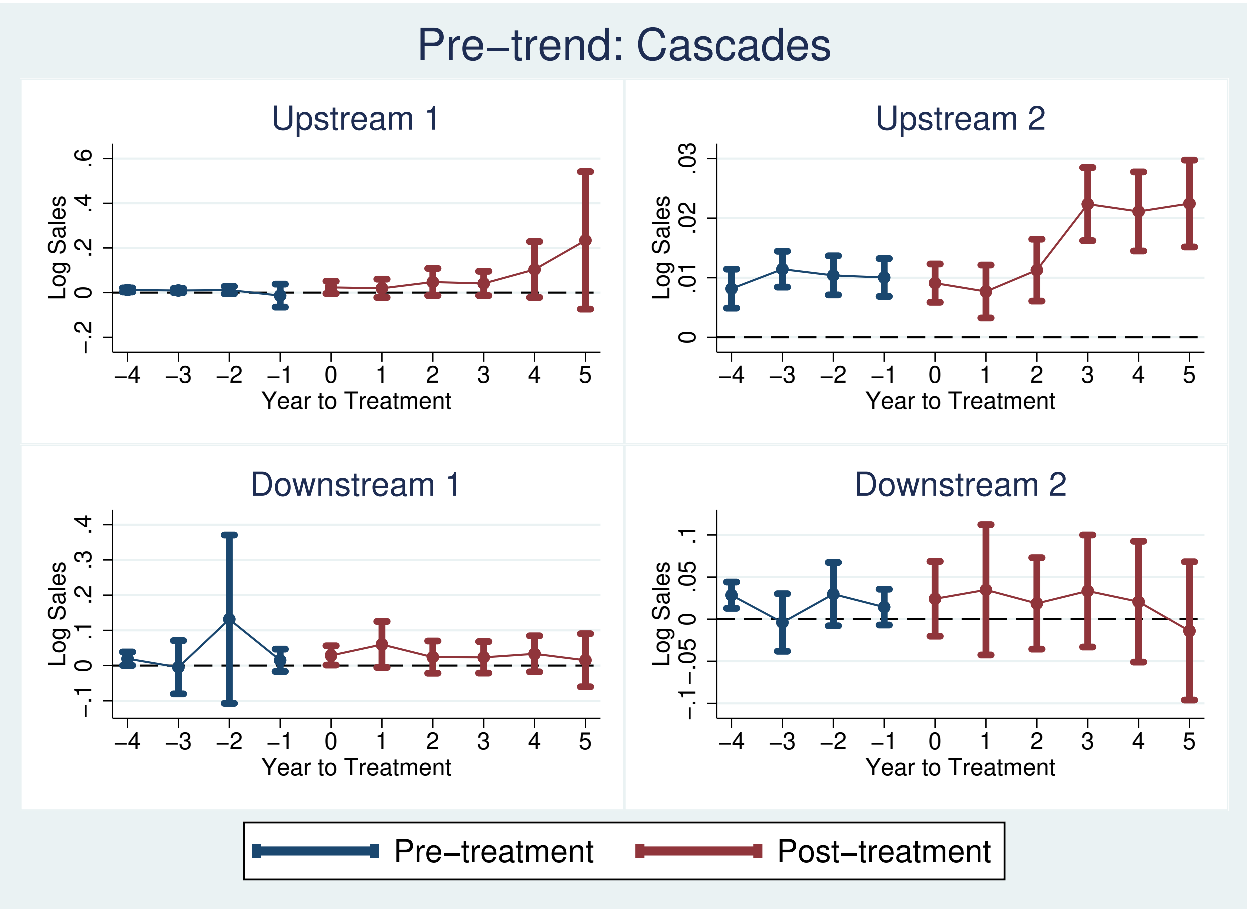
Notes: Standard errors in parentheses are clustered at the industry level. The first column corresponds to baseline estimates, the second column includes two-digit JSIC industry-by-prefecture-by-year fixed effects, the third column restricts the control group to small and medium firms whose capital stock level is smaller than 100 million yen roughly equal to one million dollars, 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 logarithmic scale.

outcome variable is the number of employees. The estimate is insignificant and shows no effect on the employment of the affected firms, an interesting contrast to the positive effects found in the previous literature. While I will discuss on this no employment effect in more details later, the lack of effects also implies that the revenue increase is driven by an increase in the other input in production, which is capital.

Figure 4 displays the coefficients and analytical 95% confidence interval from DRDID for each distance group (Upstream 1, Upstream 2, Downstream 1, Downstream 2). For each distance group, I restrict sample to that distance group and the control group, and then I estimate DRDID with this restricted sample. The graph appears to roughly support the parallel trend for each group except Upstream 2. Although the increasing trend accelerates toward the end of post-treatment periods like Upstream 1, the presence of pre-trend makes the coefficients invalid for Upstream 2. Therefore, I will not make a conclusive remark on the spillover effects over the indirect suppliers.

Table 4 shows the regression results of equation (3). The first column corresponds to the results of full sample with the standard DID, the second column corresponds to that of SME sample, and the third column shows the results of DRDID. First, from the dif-

Figure 4: 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 FY 1997, and so forth. Coefficients with 95% uniform confidence intervals from equation (4). The reference year 1993 is excluded to display from the graph.

Table 4: Second-stage Spillover Effects on Sales

	(1) Sales	(2) Sales
Upstream 1 $\times$ Post	0.078	0.026
	(0.046)	(0.009)
Upstream 2 $\times$ Post	0.016	0.017
	(0.002)	(0.002)
Downstream 1 $\times$ Post	0.031	0.001
	(0.021)	(0.027)
Downstream 2 $\times$ Post	0.020	0.008
	(0.027)	(0.030)
SME only	No	Yes

Notes: Standard errors in parentheses are clustered at the industry level. The first column corresponds to baseline DRDID estimates, and the second column is the same estimation except that the sample is restricted to small and medium firms whose capital stock level is smaller than 100 million yen roughly equal to one million dollars.

ference between the first and third columns, we can see a upward or downward bias in the estimates due to the difference in trend prior to the policy based upon the covariates mentioned above. With IPW adjusted DRDID, we still see mostly statistically significant and economically meaningful spillover effects in the supply chain. We can see from the third column that the spillover effects are the strongest in the direct suppliers of the affected firms, and the overall effects are stronger in upstream firms rather than downstream firms.

5.1 Additional Robustness Checks

5.1.1 Different Cutoffs of Treatment Category

As mentioned above, Garrett et al. (2020), I use the 30th 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 20th and 40th percentiles of the z_j^0 distribution and confirmed no change in results, as shown in Appedix C.

5.1.2 Unprofitable Firm Sample

Since bonus depreciation only affects firms with positive taxable income, I check robustness by estimating the direct effects using those firms which have negative aggregated net-tax profits in the post-treatment period. As the fourth column in Table 3 shows, these firms do not show positive effects of bonus depreciation.[9]

6 Discussions

6.1 Concurrent Tax Policies

6.1.1 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 3 demonstrate that there was no differential effect in 1997, mitigating this concern.

6.1.2 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, if 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 depreciate half of 20%, which is 10%,

[9] One may think that profit levels in the pre-treatment period can be used for another robustness check. I do not conduct this robustness check since those small firms which evade corporate taxes by reporting a small negative taxable income are likely to change their behavior in response to the tax policies in 1998.

for the 1993 fiscal year tax return. 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. I confirm this effect of the last-minute demand. Those treated firms with fiscal months between January and March experienced a small increase in 1998 sales amounts.[10]

6.1.3 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), Ohrn (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.

[10] Results are available upon request.

6.1.4 No Employment Effect and Labor Laws in Japan

While I found an increase in sales, I found no effect of investment stimuli on employment. This is a stark contrast to the findings of the previous scholarship in the U.S. The zero effect is surprising at a glance but is consistent with high fixed costs of hiring a new employee in Japan due to strict labor laws. 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 "official employees" or so-called *seishain*. This unique feature of Japanese contracts make it much harder for firms to lay off their employees even when firms are in 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.[11] This implies that Japanese firms pay large fixed costs to hire an additional employee. Therefore, even if labor and equipment in the firms' production functions are imperfect substitutes, the decreased price of equipment by the bonus depreciation must have been insufficient for firms to hire more employees.

6.2 Heterogeneous Effects by Network Distance

The third column from Table 4 implies that the price effects that benefit the downstream customers turn out to be weaker than the direct demand increase that benefits the upstream suppliers. Furthermore, the spillover effects for the direct supplier are even stronger than the direct effects on the treated firms.

First, the fact that the direct suppliers of the treated firms benefited more than the treated firms implies that one direct supplier supplies to multiple treated firms. Consistent with this, there are much fewer firms in the direct supplier category than the treated firms. This could result in greater average treatment effects that benefit direct suppliers than the treated firms. This implies that the investment stimulus policies benefit the

[11]See, e.g., https://shuchi.php.co.jp/the21/detail/8467.

bottleneck suppliers more than the target firms.

Second, such large spillover effects are not observed among the customers of the treated firms. This is a stark contrast to the existing research on spillover effects in production networks. Carvalho et al. (2021) find that the negative shock of the 2011 Great East Japan Earthquake that hit a certain region of Japan resulted in negative spillover effects on upstream and downstream firms almost at equal magnitudes with a slightly stronger effects on downstream firms, and the effects fade away at the farther network distance. The pricing effects were stronger in their setting perhaps because of price rigidity. It tends to be easier for firms to increase the price of their products when there is a legitimate reason such as a natural disaster, rather than to decrease the price now to sell more and increase the price in the future when there is an idiosyncratic shock on the cost. In fact, while the situation has been improving, even during the recent years, most of the SMEs in Japan can pass through only a portion of an increase in the input costs onto the prices of their products at the business-to-business level.[12] Then, these firms would sell more to (final) consumers rather than their customer firms, resulting in statistically insignificant effects on downstream firms. These findings together imply that there is considerable heterogeneity in the spillover effects depending on the position of firm networks. Also, the spillover effects are likely to be affected by the network structure such as bottlenecks.

7 Conclusion

This paper estimates these 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. These results are useful

[12] See, e.g., a recent survey conducted by the government, https://www.meti.go.jp/press/2023/06/20230620002/20230620002-1.pdf.

findings for policymakers who are evaluating the effectiveness of investment stimulative tax policies in the entire economy.

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Appendices

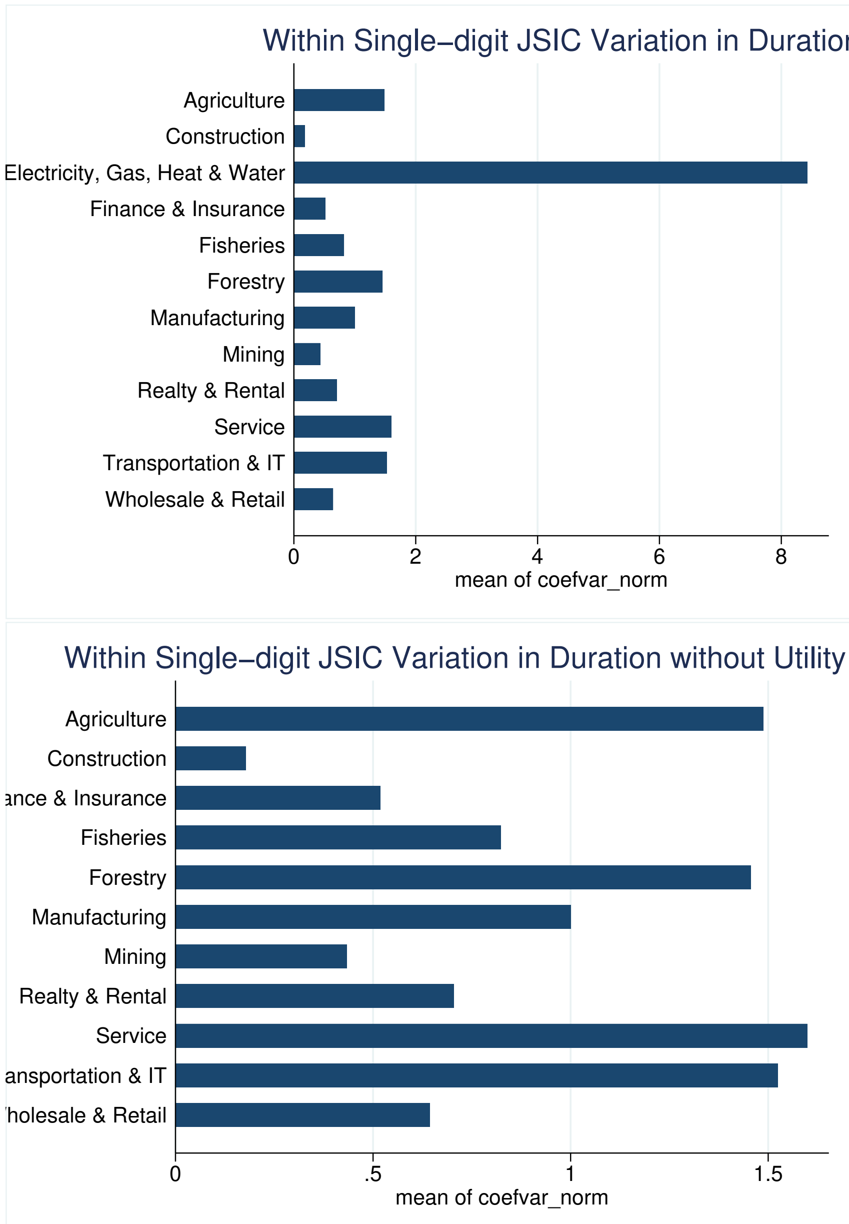
A Policy Background And Other Relevant Concurrent Tax Policies

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.[13] First, for fiscal consolidation, Japan increased the sales tax rate from 3% to 5% in April 1997. The Asian Currency Crisis followed this, and Japan started experiencing 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 as mentioned in the main text 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.

B Treatment Intensity Variation within Sector

[13] The 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.

Figure B1: Treatment Intensity Variation in Each Sector



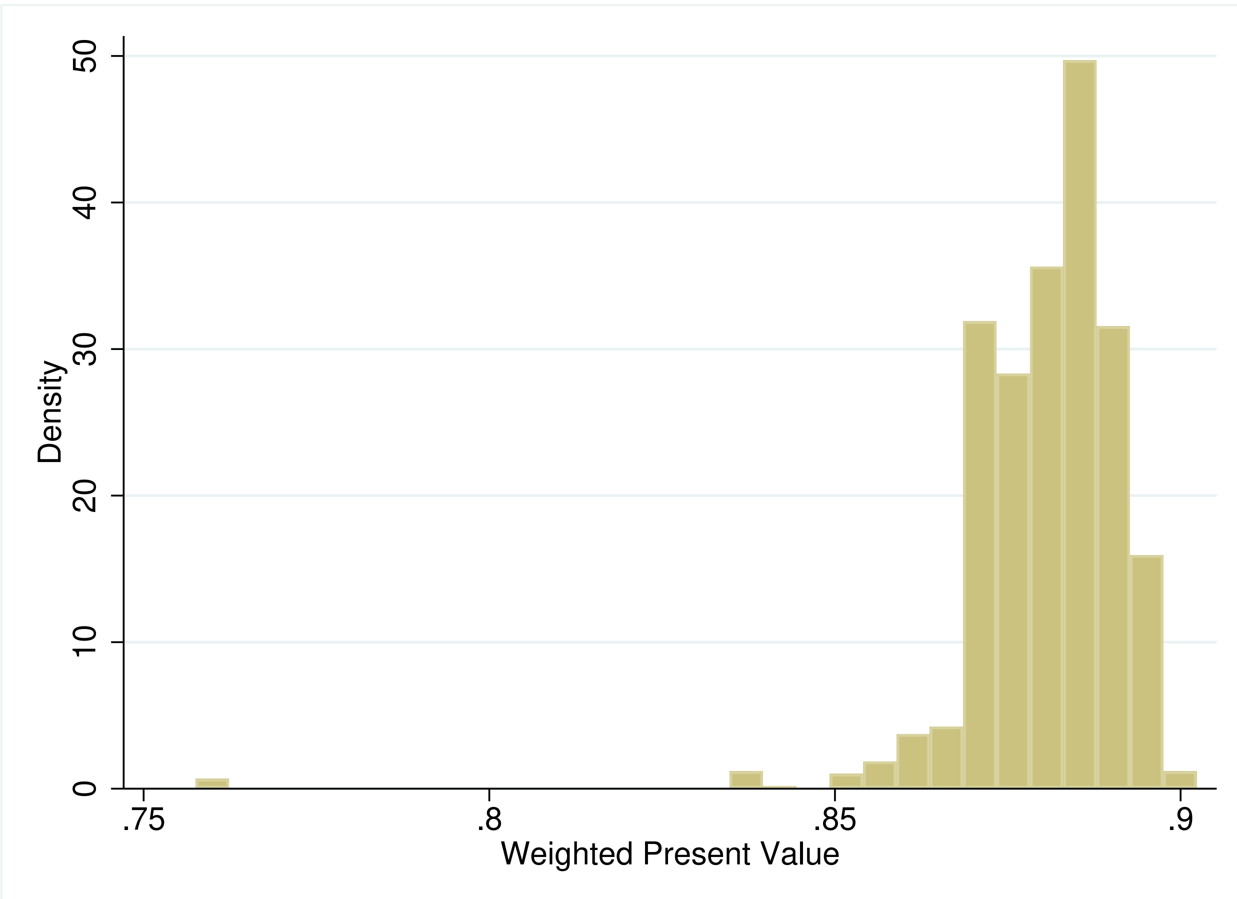
Notes: Author's calculations using data from Zwick and Mahon (2017). The top graph shows the within-1-digit-JSIC variation in duration of industries relative to manufacturing, while the bottom graph shows the same content excluding the utility sector. For each 1-digit JSIC, I calculate the within-1-digit-JSIC coefficient of variation of the treatment intensity measure from Zwick and Mahon (2017). I normalize each sector measure of weighted variation to the manufacturing sector.

C Additional Results and Robustness Checks

C.1 Different Cutoffs for Treatment

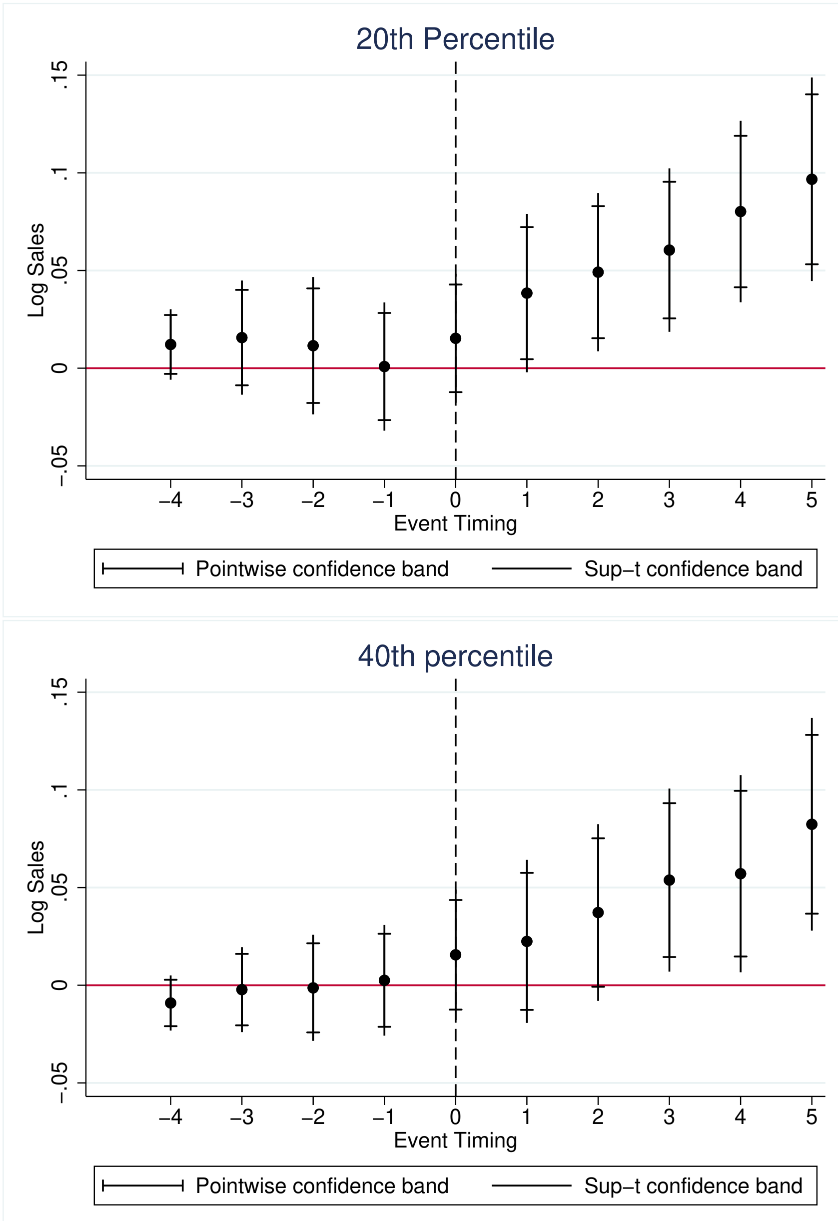
Figure C1 displays the distribution of the ZM weights applied to JSIC industries. In the main text, I use the 30th percentile of z_N measure adopted from ZM US industries to Japanese industries. I chose this cutoff for the balance of sample sizes across different network distances including the treated group. In this section, I show that the results in the main text are robust to the choice of this cutoff by demonstrating the the same event plots with the 20th and 40th percentiles as cutoffs for the treatment. Also, since there is a large break around the 20th percentile, for the direct effects, I include the 10th percentile version as well.

Figure C1: Distribution of ZM Weighted Present Value across JSIC



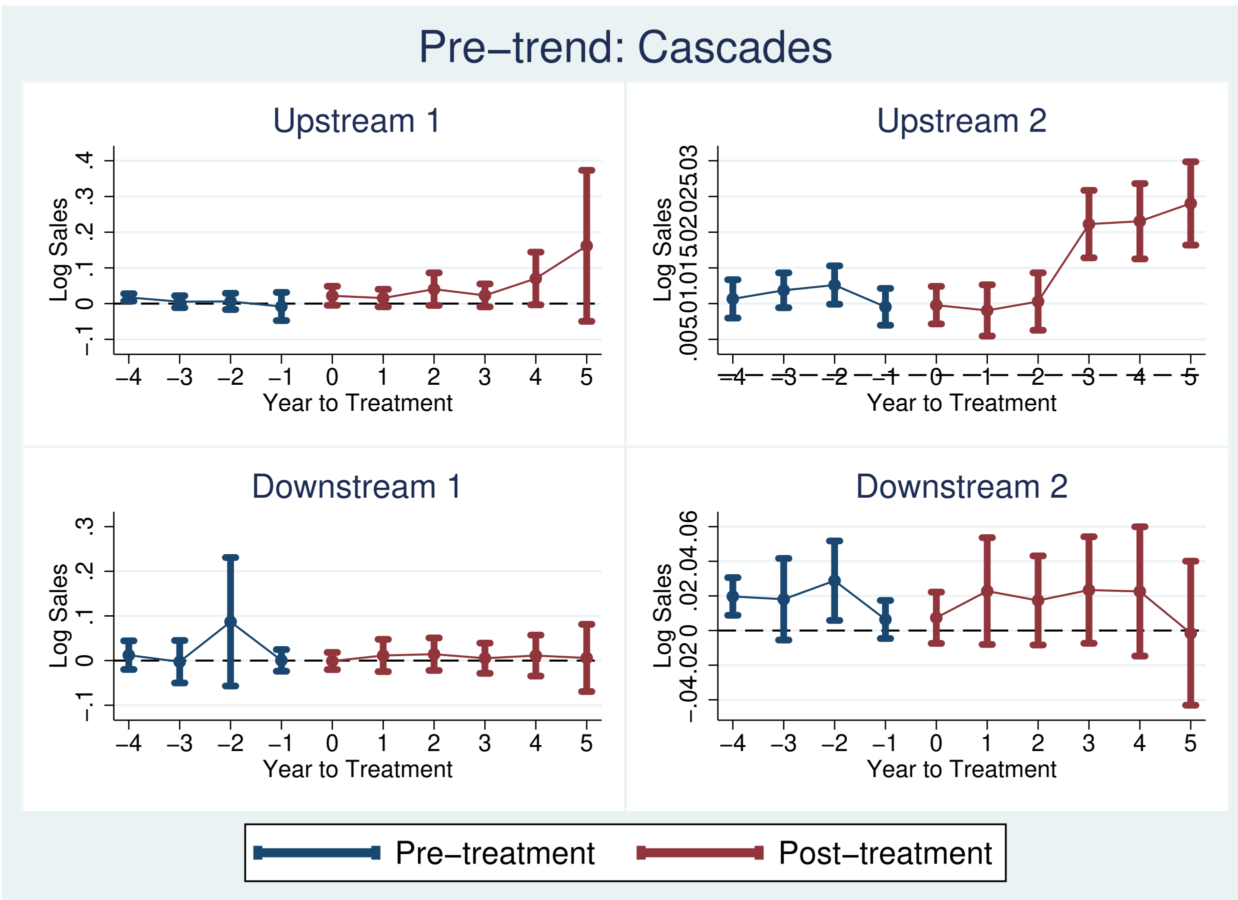
Notes: x-axis indicates the weighted present value z_N adopted from ZM to JSIC industries.

Figure C2: Pre-trend Test for Direct Effects with Different Cutoffs



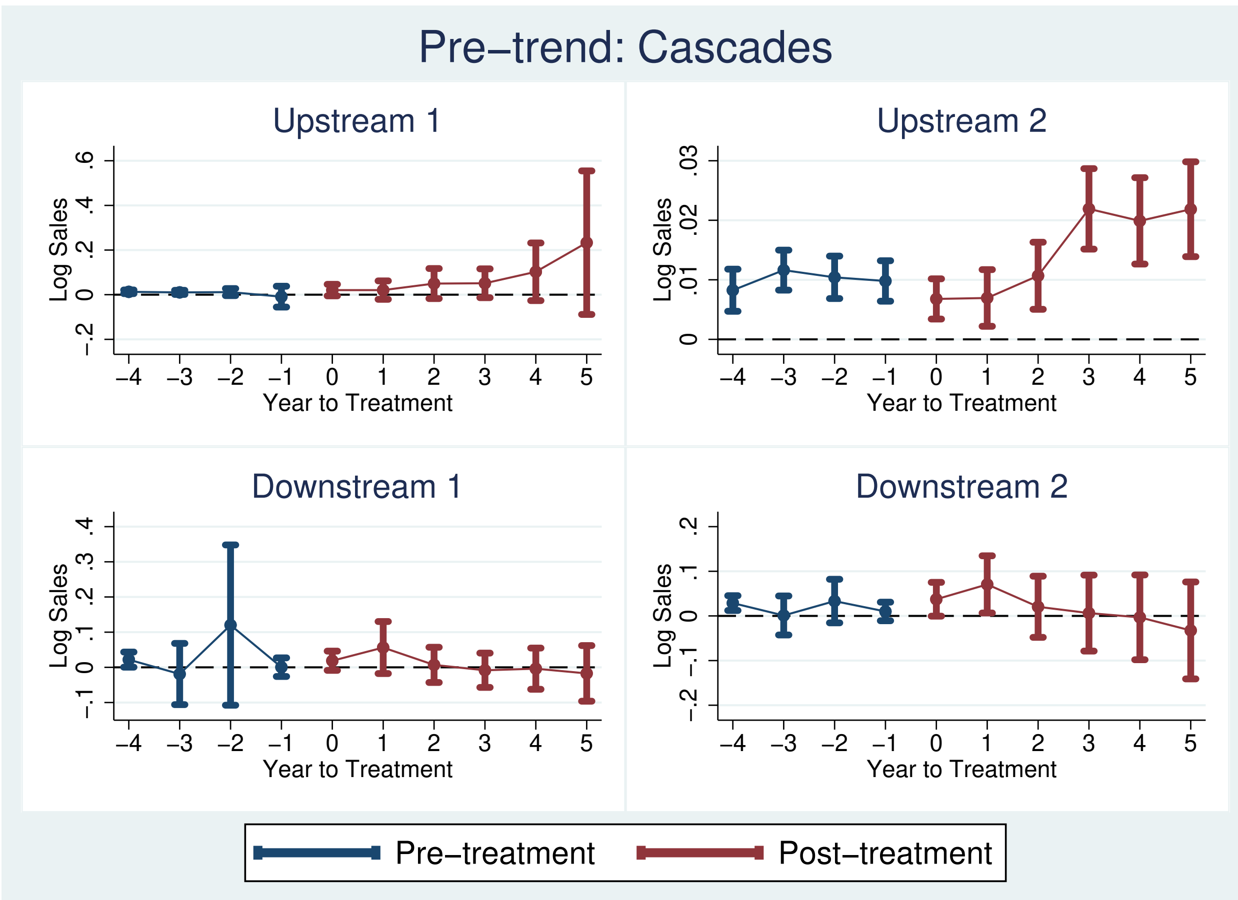
Notes: x-axis indicates time period where 0 is equal to FY1998 that corresponds to the year the policy took effect, -1 means FY 1997, and so forth. Coefficients with 95% point and sup-t uniform confidence intervals from equation (4). The reference year 1993 is excluded to display from the graph.

Figure C3: Pre-trend Test for Spillover Effects with 20th 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 FY 1997, and so forth. Coefficients with 95% uniform confidence intervals from equation (4). The reference year 1993 is excluded to display from the graph.

Figure C4: 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 FY 1997, and so forth. Coefficients with 95% uniform confidence intervals from equation (4). The reference year 1993 is excluded to display from the graph.