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HASHIMOTO, Yuki
RIETI

INOUE, Toshikatsu
Shiga University

SAKASHITA, Fumiyuki
RIETI

SUMIYA, Kazuhiko
RIETI

Research Institute of Economy, Trade & Industry, IAA
The Research Institute of Economy, Trade and Industry
https://www.rieti.go.jp/en/

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Estimating the Impact of Capital Subsidies under Repeated Applications*

Yuki HASHIMOTO
Research Institute of Economy, Trade and Industry (RIETI)
Toshikatsu INOUE
Shiga University
Fumiyuki SAKASHITA
Research Institute of Economy, Trade and Industry (RIETI)
Kazuhiko Sumiya
Research Institute of Economy, Trade and Industry (RIETI)

Abstract

This study estimates the effect of a repeatedly implemented capital subsidy program. When firms can reapply, estimates that ignore repeated applications may conflate the impact of the initial subsidy with subsequent ones. To isolate the effect of the initial subsidy, we adapt the framework of Cellini, Ferreira and Rothstein (2010) to a difference-in-differences setting. Using firm-level panel data merged with administrative records on Japan's largest SME subsidy program, we find that not accounting for future subsidies overestimates the effects of the initial subsidy on sales and employment.

Keywords: Capital subsidies, SMEs, Repeated applications, Treatment effects, Difference-in-differences

JEL classification: O25, O38, H25

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1 Introdution

Capital subsidies are a primary tool in industrial policy, with governments providing substantial public funds to promote innovation and enhance productivity (Juhász et al., 2024). Given the scale of these investments and the expected contribution of their returns to economic growth, estimating causal effects as unbiased as possible has become a crucial research question. To address this, a growing body of research examines the causal effects of subsidies using newly developed econometric methods (Dimos and Pugh, 2016).

However, the existing literature has insufficiently addressed methodological challenges of repeatedly implemented subsidies. Under recurring subsidy programs, current subsidy receipt may influence future subsidy receipt probabilities. In this policy evaluation context, Miller (2023) proposes two distinct concepts of the estimand: the "total effect," which does not control for future treatments, and the "partial effect," which isolates the impact of the current treatment by controlling for potential impacts of subsequent treatments. Most studies estimate only the total effect, with Cingano et al. (2025) as a notable exception. Some studies address subsequent subsidies by excluding later-treated samples (e.g., Bronzini and Iachini, 2014; Cerqua and Pellegrini, 2014; Zhao and Ziedonis, 2020), but this may introduce bias due to bad control. Given that subsidy programs with multiple application rounds are widely implemented across countries, further development is needed for the proper identification and estimation of the partial effect.

This study applies the approach of Cellini, Ferreira and Rothstein (2010, hereafter CFR) to identify the partial effects of the "Monodukuri Subsidy," a Japanese subsidy program targeting small and medium-sized enterprises (SMEs). CFR's seminal work demonstrates how partial effects can differ from total effects in school facility investment evaluations.[1] CFR's findings highlight the importance of considering long-term dynamics in policy evaluation, especially for treatments that can be implemented multiple times. Similar divergences between total and partial effects may occur in other policy contexts, but this analytical framework remains largely unapplied to subsidy evaluations. Although CFR's framework uses RDD, we apply their framework to the DID approach.[2] This estimation strategy allows us to identify the partial effect by estimating the total effect

[1] CFR refers to the total effect as the intent-to-treat (ITT) effect and the partial effect as the treatment-on-the-treated (TOT) effect.

[2] As discussed in Section 4, this is because RDD is not applicable to our data, although the screening process of this subsidy is potentially suitable for RDD.

and probability difference in future subsidy acceptance between the treated and control groups.

Our analysis shows that the estimated total effect was amplified by a higher likelihood of future acceptance of the same subsidy program. As a first step, we estimated the total effect of the subsidy on sales and employment using standard DID estimation, finding positive and statistically significant effects. We then calculated the future subsidy acceptance gap between accepted and rejected firms and found that initial recipients were more likely to be accepted for future subsidies. This higher probability of future acceptance for initially accepted firms accumulated the effects of both initial and subsequent subsidies, leading to a larger total effect than a partial effect. The magnitude of the discrepancy between the total and partial effects on sales has grown over the years, peaking at 14%. However, after controlling for future treatments, the partial effects remained positive and statistically significant, indicating the positive impact of a single subsidy on sales and employment. Conversely, the effect on sales per employee—interpreted as a proxy for labor productivity—was not significantly different from zero, suggesting that the subsidy did not improve productivity among adopted firms.

This study provides novel insights into the literature in two ways. Firstly, our estimation strategy builds upon previous studies that analyze the causal effects of a subsidy. Many previous studies fail to distinguish between the total and partial effects of subsidy receipt. Total effects are also policy-relevant and have been parameters of interest in some studies. However, they do not often control for future treatments, which can offset or reinforce the initial treatment effect, making the interpretation of policy evaluation more challenging. In contrast, partial effects compare the initial treatment between treated and control groups while controlling for future treatments, thereby capturing the "pure" effect of the initial treatment. If the difference in future treatment probabilities between treated and control groups is large, the divergence between total and partial effects increases, emphasizing the need to distinguish between them in analysis.

Secondly, our analysis provides robust evidence of the effects of the subsidy program on capital investment for SMEs. The effect of subsidies, typically a few percent or nearly zero, can be significantly influenced by overestimation or underestimation bias, altering the interpretation of policy implications. The finding that investment subsidies increase sales and employment but do not affect productivity measures aligns with studies by

Bernini and Pellegrini (2011), Criscuolo et al. (2019), and Branstetter et al. (2023). Furthermore, our analysis of heterogeneous effects confirms that the Small-Scale Type subsidy, which allows for flexible use of funds, enhances productivity, while subsidies restricted to capital investment for larger SMEs show no such effect.

The structure of this paper is as follows. Section 2 describes the analytical framework for addressing repeaters in subsidized programs. Section 3 outlines the Monodukuri subsidy program, and Section 4 explains the data used in our analysis. In Section 5, we conduct an event study analysis using conventional DID methods. Section 6 provides an estimation of the partial effect of the subsidy. Section 7 provides conclusions.

2 Total and Partial Effects with Repeated Applications

This section addresses methodological challenges in evaluating the causal effects of a subsidy program with repeated applications. When a subsidy design allows both previously accepted and rejected firms to reapply, isolating the impact of an individual subsidy becomes difficult. To illustrate this issue, consider three key variables: initial subsidy acceptance, future subsidy acceptance, and outcomes. The initial subsidy acceptance can affect the outcomes through two mechanisms. First, the initial subsidy can directly influence the outcomes. Second, initial subsidy acceptance may correlate with future subsidy acceptance, leading to differences in outcomes. We refer to the effect through the first mechanism as the partial effect, and the combined effect through both mechanisms as the total effect.

In evaluating the effectiveness of a subsidy, it is valuable to estimate the partial effect separately from the total effect. The partial effect is defined as the effect of a single subsidy, controlling for future acceptances. It provides a clearer picture of the subsidy's effectiveness, free from the influence of future acceptance probabilities. However, standard causal inference typically avoids controlling for future variables, and therefore captures only the total effect. To accurately estimate the partial effect, we apply the CFR framework to the DID estimation, isolating the direct impact of the initial subsidy from indirect effects.

Consider two scenarios to explain why the partial effect should be estimated. First, if receiving a subsidy increases the probability of future acceptance (e.g., due to improved application skills or reputation), the total effect exceeds the impact of a single subsidy by accounting for future subsidies. In this case, the total effect does not represent the impact of a single subsidy but rather the combined effect of multiple subsidies, complicating the assessment of total policy costs. Conversely, suppose the subsidy decreases the probability of future acceptance (e.g., due to policies favoring first-time recipients). In that case, the total effect underestimates the impact of a single subsidy, offsetting the initial subsidy's direct effect with indirect effects from future subsidies. Thus, when subsidy acceptance influences future acceptance, estimating the partial effect is preferable to the total effect.

We estimate a partial effect using the framework developed by CFR. We define the dynamic relationship between the partial effects and the outcome variable of the subsidy as follows:

$$Y_{i,t} = \begin{cases} \alpha_t D_i + \sum_{s=1}^{t} \alpha_{t-s} \tilde{D}_{i,s} + \eta_{i,t}, & (t > 0) \\ \alpha_0 D_i + \eta_{i,0}, & (t = 0) \\ \eta_{i,t}. & (t < 0) \end{cases}$$

Here, $Y_{i,t}$ denotes an outcome variable. D_i is a dummy variable representing the acceptance of the subsidy offered in $t = 0$. α_t represents the partial effect of the subsidy on sales t years later. $\tilde{D}_{i,s}$ is a dummy variable representing the acceptance of the subsidy offered in $s = 1, ..., t$. $\eta_{i,t}$ denotes the effects on outcomes caused by the other factors, which include fixed effects of firms, trend, and unobserved variables.

Next, we impose a parallel trend assumption on this model. The standard form requires that if both the treatment and control groups receive no treatment, their outcomes evolve in parallel from the baseline period onward. In our context, this standard assumption is extended as follows: if the two groups receive neither subsidies in $t = 0$ nor any future subsidies in $t > 0$, their outcomes follow parallel trends. Mathematically, our assumption can be expressed as:

$$E[\eta_{i,t} - \eta_{i,-1} | D_i = 1] = E[\eta_{i,t} - \eta_{i,-1} | D_i = 0]. \quad (t \geq 0)$$

The left- and right-hand sides represent the expected values of the unobserved components for accepted and rejected firms, respectively. This equation indicates that in the absence of subsidies, their unobserved components $\eta_{i,t}$, which equal outcomes $Y_{i,t}$, follow parallel trends from $t = 0$ onward. Although this assumption cannot be directly tested, parallel pre-trends provide supporting evidence.

Now we confirm that the DID estimator of year t, β_t, captures the total effect, as follows,

$$\begin{aligned}
\beta_t &= E[Y_{i,t} - Y_{i,-1}|D_i = 1] - E[Y_{i,t} - Y_{i,-1}|D_i = 0] \\
&= E\left[\alpha_t + \sum_{s=1}^{t} \alpha_{t-s}\tilde{D}_{i,s} + \eta_{i,t} - \eta_{i,-1}|D_i = 1\right] - E\left[\sum_{s=1}^{t} \alpha_{t-s}\tilde{D}_{i,s} + \eta_{i,t} - \eta_{i,-1}|D_i = 0\right] \\
&= \alpha_t + \sum_{s=1}^{t} \alpha_{t-s}\left(E[\tilde{D}_{i,s}|D_i = 1] - E[\tilde{D}_{i,s}|D_i = 0]\right) \\
&\quad + E[\eta_{i,t} - \eta_{i,-1}|D_i = 1] - E[\eta_{i,t} - \eta_{i,-1}|D_i = 0] \\
&= \alpha_t + \sum_{s=1}^{t} \alpha_{t-s}\Delta\pi_s, \quad (\Delta\pi_s = E[\tilde{D}_{i,s}|D_i = 1] - E[\tilde{D}_{i,s}|D_i = 0])
\end{aligned}$$

where $\Delta\pi_t$ represents the difference in future subsidy acceptance probabilities between accepted and rejected firms. By using $\Delta\pi_t$ and β_t, we can recursively compute the α_t as follows:

$$\begin{aligned}
\alpha_0 &= \beta_0 \\
\alpha_1 &= \beta_1 - \alpha_0\Delta\pi_1 \\
\alpha_2 &= \beta_2 - \alpha_1\Delta\pi_1 - \alpha_0\Delta\pi_2 \\
&\dots \\
\alpha_t &= \beta_t - \sum_{s=1}^{t} \alpha_{t-s}\Delta\pi_s.
\end{aligned}$$

The key assumption is that the partial effect is independent of the timing of the treatment or the firm's treatment history. This assumption allows for the estimation of α_t.[3]

We did not adopt an alternative specification that allows partial effects to vary by application year or past acceptance history. The simplification regarding heterogeneity

[3]Two alternative approaches exist for estimating α_t, but both have identification vulnerabilities. The first approach uses acceptance of future subsidies as control variables, while the second excludes treated and control firms that accept any future subsidies. However, both approaches are problematic if an unobserved variable simultaneously affects future subsidy acceptance and an outcome variable. This issue is known as "bad control" (Angrist and Pischke, 2009, Sec. 3.2.3).

by application year is justified by the stability of the study period. As discussed in detail in Section 4, our analysis focuses on subsidies implemented from 2016 to 2019, a period marked by no major institutional changes in the subsidy program and no significant events that influenced the Japanese economy or financial markets.

Including heterogeneity by past acceptance history raises econometric concerns. If the effect of receiving a subsidy in period t depends on whether the firm accepted subsidies in $t-1$, $t-2$, or earlier periods, the number of parameters to estimate grows exponentially with the length of the history. Additionally, estimating parameters by history requires splitting the data into smaller subsamples, which reduces sample sizes and increases standard errors. To avoid these challenges, we did not account for heterogeneity by acceptance history.

Our approach regarding the application year and past acceptance history aligns with the standard CFR practice, as seen in Gelber et al. (2016) and Giupponi and Landais (2023). We assume the partial effect is independent of the application year or past acceptance history.

3 Outline of the Monodukuri Subsidy Program

This section describes the Monodukuri subsidy program, which is analyzed in the following sections. This program has characteristics that make it suitable for a causal inference framework accounting for repeated applications. The Monodukuri subsidy was introduced in 2012 by the Small and Medium Enterprise Agency to support capital investment of SMEs involved in service, prototyping, or production process improvement, to boost capital investment and enhance productivity.[4] This is one of the largest subsidy programs for SMEs in Japan, with an annual budget of approximately 100 billion yen.

This study analyzes the 2016 subsidy program.[5] Two rounds were implemented that year, focusing on the first.[6] In the first round, 26,629 businesses applied, with 7,948

[4]From 2001 to 2009, Japan enforced the Industrial Cluster Policy. This policy involved government-designated industrial clusters, primarily consisting of SMEs and universities, and facilitated collaboration with regional financial institutions. The effectiveness of this policy was analyzed by Okubo et al. (2022).

[5]Since 2016, unique corporate numbers for individual enterprises have been available for analysis. As explained in Section 4, these identifiers are essential for linking multiple datasets.

[6]While firms rejected in the first round were technically eligible to apply for the second round, they faced substantial restrictions. Resubmission of the same proposals was not permitted; entirely new proposals were required. Consequently, only 11% of the firms rejected in the first round reapplied, and a small fraction was accepted. None of these firms could be matched with the TSR data used in the

adopted, resulting in an adoption rate of about 29.8%. Of the applications, 18,577 (69.8%) were from manufacturing sectors, 8,040 (30.2%) from non-manufacturing sectors, and 12 (less than 0.01%) from unknown sectors. SMEs adopted for subsidies make capital investments based on their business plans, with a portion of the investment cost (roughly 1/2 to 2/3 of the subsidy rate, with a maximum of 30 million yen) covered by the subsidy.

Firms submit applications containing their business plans to regional offices, where each application is evaluated from three aspects: "technical aspects," "commercialization aspects," and "policy aspects."[7] The screening of subsidy recipients occurs in two stages. First, regional offices select projects for adoption in descending order of their total evaluation scores within each prefecture. Scoring criteria and score distribution are standardized across prefectures, though minimum and maximum evaluation scores vary. Then, projects on the acceptance-rejection border undergo re-examination by the National Adoption Screening Committee, which makes the final decision. This national screening reviews projects based on three perspectives, with scores from each prefecture remaining unchanged. Consequently, projects with the highest evaluation points are not necessarily adopted first, leading to some rejected applicants having higher scores than the adopted ones. Applicants are categorized into three groups based on their evaluation scores: adopted, re-examined, and rejected.

Figure 1 illustrates the relationship between evaluation scores and adoption. The horizontal axis represents evaluation scores, while the vertical axis shows the adoption rate. In all prefectures, applicants above a certain threshold (the highest point of non-adoption) are adopted, and those below another threshold (the lowest point of adoption) are rejected. Between these points is a narrow mixed interval of adoption and non-adoption.

This subsidy has a significant number of repeat applicants. Out of 22,955 firms that applied in 2016, 10,610 (46%) submitted applications for at least one Monodukuri subsidy in 2017-2019, and 6,941 (30%) received some of these subsidies. Note that businesses previously selected for the Monodukuri subsidy will not be considered for applications with the same or similar business plans. However, they can apply with a different busi-

analysis (explained in detail in Section 4). Therefore, this study analyzes only firms that applied in the first round.

[7]For the 2016 program, firms with a total payroll increase of at least 1% in 2015 over the previous year or those that had announced to their employees a plan to increase their total payroll by at least 1% in 2017 over 2016 received additional points.

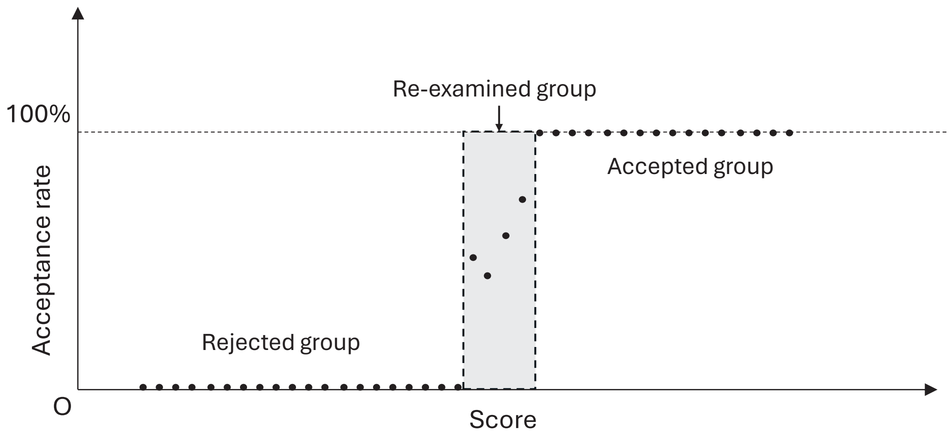

Figure 1: Acceptance Rate and Score

ness plan and may be selected again.[8] There are no restrictions on reapplying for the Monodukuri subsidy for firms that have not previously received it.

4 Data

We use two types of data to analyze the causal effects of the 2016 Monodukuri subsidy program. One is the data on applicant firms and their screening results for the 2016-2019 program (hereafter referred to as "application data") provided by the National Federation of Small Business Associations, which is contracted to manage and administer the subsidy program. This dataset enables us to investigate whether applicants for 2016 also applied for 2017 and subsequent programs, and if so, whether they were accepted. The other is data on outcome variables (2007–2019) for firms that applied to the program. We call this TSR data because the data was obtained from Tokyo Shoko Research (TSR).[9] Application and TSR data are merged using a common corporate number to construct panel data at the level of applicant firms.

The application data includes applications for and the adoption of subsidies. These data contain screening scores and adoption results for all applicants.[10] Firms that re-

[8]Since the 2020 subsidy program, this review system has been modified. Firms that have accepted a subsidy in the last three years are now at a disadvantage when applying for a new subsidy.

[9]Data on outcomes can also be obtained from the Economic Census for Business Activity and the Census of Manufacture. The Economic Census for Business Activity, conducted every five years, and the Census of Manufacture, conducted in the intervening years, can be linked as a panel dataset using a common firm identifier. However, Suga (2025) notes that the survey methods and the coverage of establishments in the Economic Census for Business Activity have differed across the three survey rounds conducted since 2009, and therefore, the use of these data in time-series analyses is not recommended.

[10]Although joint applications by multiple firms were possible for the 2016 program, the percentage of

ceive a subsidy must report results from the subsidized project annually for five years, including sales, operating income, and the number of employees. However, outcome data are available only for adopted firms, and outcomes for non-adopted firms after 2017 are not collected. Therefore, we use outcome indicators from the TSR data and merge them with application data. The TSR data include firms' sales, number of employees, and capital amount.[11] The TSR data covers more than one million registered firms in Japan, regardless of whether they are listed on the stock exchange.

The application and TSR data can be merged using the Japan Corporate Number, which the government designates. By joining the two data sets, we construct a panel data set (2007–2019) covering all subsidy applicants, including rejected firms. The percentage of applicants in 2016 that could be combined with the TSR data is around 50% on average, varying slightly by year. The majority of applicants that could not be merged are small businesses without registration or sole proprietorships. Since the number of firms in the TSR data changes yearly, the data used in the analysis is an unbalanced panel. See Appendix Table A.1 for details on connecting the application data and TSR data. In the matched sample, employees and capital were larger than the average for all applicant firms, and relatively small firms were not merged. Thus, the firms analyzed for subsidy effects in our study are relatively large among the applicants.

As mentioned in the previous section, the average adoption rate for the program in 2016 was about 30%. However, among all 47 prefectures, the adoption rate for the seven prefectures in the Kyushu region ranged from 42–67%. This higher adoption rate was due to prefectures in the Kyushu region being affected by the Kumamoto earthquake that occurred during the subsidy application period, which resulted in a higher adoption rate to support the recovery of SMEs in this region. Due to the differences in screening criteria between the seven prefectures in the Kyushu region and other prefectures, we exclude applicants from these seven prefectures from our analysis.

Table 1 shows the descriptive statistics of the application data. The subsample in

joint applicants was very low. Our application data show that only 0.07% of selected firms made joint applications. Given this small percentage, we have removed these joint applicant providers from our analysis.

[11] TSR also provides more detailed financial information. However, the financial information data primarily covers medium-sized and larger firms, with limited information available on small and medium-sized firms. Since most firms applying for the Monodukuri subsidies are SMEs, the merge ratio of the Application data and TSR financial information data is only around 20%. Furthermore, the merged firms are considerably larger than the population of firms (all applicant firms) in terms of capital amounts and number of employees. Therefore, we do not use the financial information data.

Table 1: Application Data of the Subsidy in the Matched Sample

	Application	Capital (million yen)	Employee (person)	N
Full sample	Accept	25.6 (30.8)	51.4 (80.7)	5070
	Reject	21.9 (25.4)	46.3 (79.2)	9541
Ratio		1.16	1.11	0.53
Subsample	Accept	23.5 (29.7)	42.4 (63.3)	470
	Reject	26.7 (44.9)	49.4 (52.4)	165
Ratio		0.88	0.86	2.85

Note: The table presents the mean values of capital, number of employees, and the number of applications (N) for each group. Standard deviations are shown in parentheses. As the capital has outliers that significantly affect the mean and standard deviation, the top 1% was trimmed to calculate the statistics. The fourth and seventh rows present the ratios of the mean values of the rejected group to those of the accepted group. The sample used to calculate these statistics is limited to firms that have at least one record in the TSR dataset from 2015 to 2019. This sampling limitation results in the exclusion of 30% of the application data.

this table represents the re-examined group shown in Figure 1. The data used in the table are derived from the information submitted during the application process and the scoring results of their applications. Table 1 indicates that, in the full sample data, although the average values differ, accepted and rejected applications exhibit wide and largely overlapping distributions in terms of capital and employment. The subsample data indicate differences in the distributions between the accepted and rejected groups. However, the subsample size (470 accepted and 165 rejected) is small compared to the full sample, which likely leads to substantial variation in the summary statistics and prevents firm conclusions about the degree of overlap.

As illustrated in Figure 1, the re-examined group falls within a narrow range of scores. Given that firm characteristics vary continuously and that the discontinuity in the adoption rate appears to serve as a cutoff point, it may seem that an RDD framework could be applied to identify the causal effect while controlling for firm characteristics. However, the subsample fails the balance of outcome variables in the pre-treatment period. In estimation strategies that utilize the systematic cutoff, such as RDD, researchers assess the balance of pre-treatment variables to verify the similarity of samples near the cutoff (Cattaneo and Titiunik, 2022). In our case, the difference in average sales between the accepted and rejected groups of the subsample is larger than that of the full sample. This imbalance suggests that the re-examination might be based on some factors unobservable to researchers, which invalidates the prerequisite for applying RDD.

Furthermore, examining the pre-trends of the outcome variable reveals a clear advantage in using the full sample for the subsequent estimation. Figure 2 shows the sales of firms matched with TSR data in the pre-treatment period. The full sample exhibits more parallel trends between the accepted and rejected groups than the subsample, suggesting that the parallel trend assumption is approximately satisfied in the full sample. Therefore, Figure 2 suggests that the full sample is more appropriate for DID estimation. After a detailed examination of pre-treatment growth trends and levels, we focus on the full sample analysis, using DID instead of RDD.[12]

5 Simple DID Estimation

We estimate the total effect of the Monodukuri subsidy on outcomes (sales, number of employees, and sales per employee) using the DID method. Specifically, we employ the two-way fixed effects model (TWFE) to estimate the difference in outcomes between accepted and rejected firms in the 2016 subsidy program. The model is as follows:

$$\ln \mathrm{Outcome}_{it} = \sum_{r=-T_0}^{-2} \beta_r \mathrm{Accept}_i I(t=r) + \sum_{s=0}^{T_1} \beta_s \mathrm{Accept}_i I(t=s) + \lambda_i + \tau_{tp(i)} + u_{it},$$

where Accept_i is a dummy variable indicating whether a firm received the subsidy. The parameter of interest is β_s, the coefficient of the interaction term between the acceptance dummy and the index function $I(\cdot)$. The time index t represents the year, with the subsidy acceptance determined between $t=-1$ and $t=0$. λ_i represents the firm i's fixed effects. $\tau_{tp(i)}$ denotes prefecture-specific time trends, allowing for heterogeneous trends by prefecture, $p(i)$. These trends allow for the heterogeneity caused by the application and screening process, which is separated by prefecture. u_{it} is the error term. We use a clustered standard error at the firm level for statistical inference.

We also estimate an augmented model incorporating firm-specific linear trends, such as:

$$\ln \mathrm{Outcome}_{it} = \sum_{r=-T_0}^{-2} \beta_r \mathrm{Accept}_i I(t=r) + \sum_{s=0}^{T_1} \beta_s \mathrm{Accept}_i I(t=s) + \lambda_i + \kappa_i t + \tau_{tp(i)} + u_{it},$$

[12] As an alternative identification strategy, one might consider matching treated and control firms based on their evaluation scores. However, a matched sample would consist of firms around the cutoff for acceptance/rejection, which is effectively the same identification strategy as RDD.

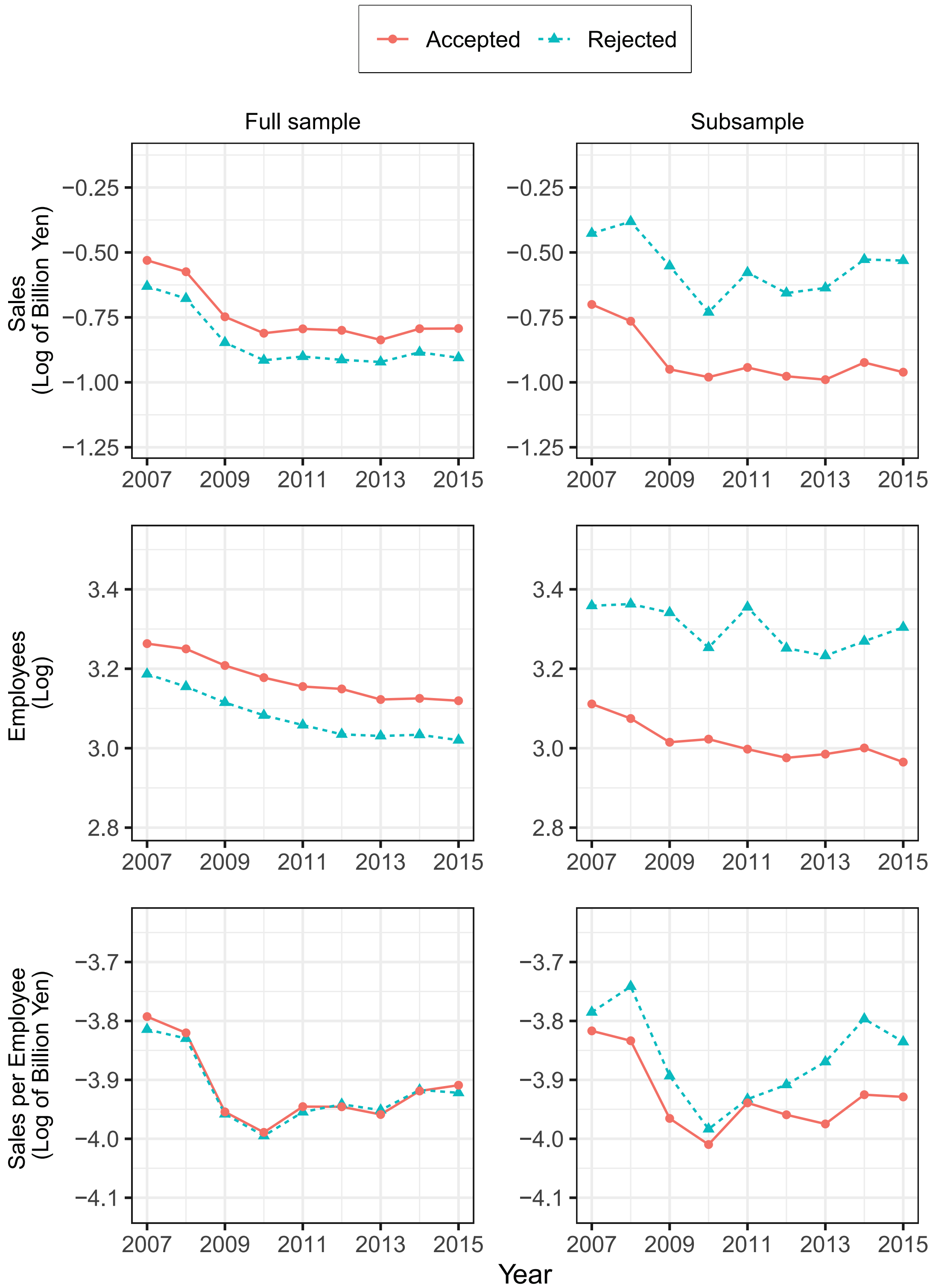

Figure 2: Average Outcomes before Application to the Subsidy

where κ_i is the coefficient of firm i's trend. This approach addresses potential bias from non-parallel trends between treated and control firms. The specification allows for firm-specific heterogeneous trends, enabling us to verify if differences in firm-specific growth rates affect the DID estimation results. Since the augmented model is estimated by taking first differences, the number of lag variables that can be estimated is reduced by one period.

Moreover, we restrict our sample to firms with consistent data availability from 2010 to 2015 to mitigate potential bias in TSR data collection, as TSR does not update its data annually for all firms. The updates occur irregularly based on TSR's decisions, which likely depend on various factors.[13] Consequently, analyzing an unrestricted sample may introduce selection bias into our estimation. Our sampling strategy aims to strike a balance between representativeness and minimal selection bias, thereby enhancing the validity of our empirical findings. This approach results in modest sample attrition, with the largest drop, 15% of the sample, occurring in 2007 and 2019. In contrast, among all applicant firms, we observe significantly higher attrition rates, around 50% over the entire period, which implies that using all applicant firms, rather than our selected firms, would lead to severe attrition bias.

Figure 3 and Table B.1 present the estimation result from both models with 2015 as the pre-treatment base year. Specifically, Figure 3 illustrates the estimates with 95% confidence intervals for both models. This figure shows consistent patterns in estimates for both models. Estimates of sales and employees are mainly negative and statistically insignificant in the pre-treatment period, but become positive and significant in the post-treatment period. This shift indicates a clear impact of the subsidy. The similarity of results between the baseline model and the firm-specific linear trend model supports the robustness of our estimations and suggests that the observed effects are not sensitive to model specification choices.

The estimates also reveal a meaningful effect size of the subsidy on sales and employees. We observe a significant positive effect immediately post-treatment, with sales

[13]TSR Data, a private credit research organization, updates its information through surveys conducted via on-site visits, telephone interviews, or mailed questionnaires. Priority is given to firms that customers have specifically requested. Listed companies are updated annually based on their securities reports. Although the timing of surveys varies across firms, it is not feasible to conduct timely surveys of all SMEs every year. As a result, there may be a delay of one to two years before some SMEs are incorporated into the database.

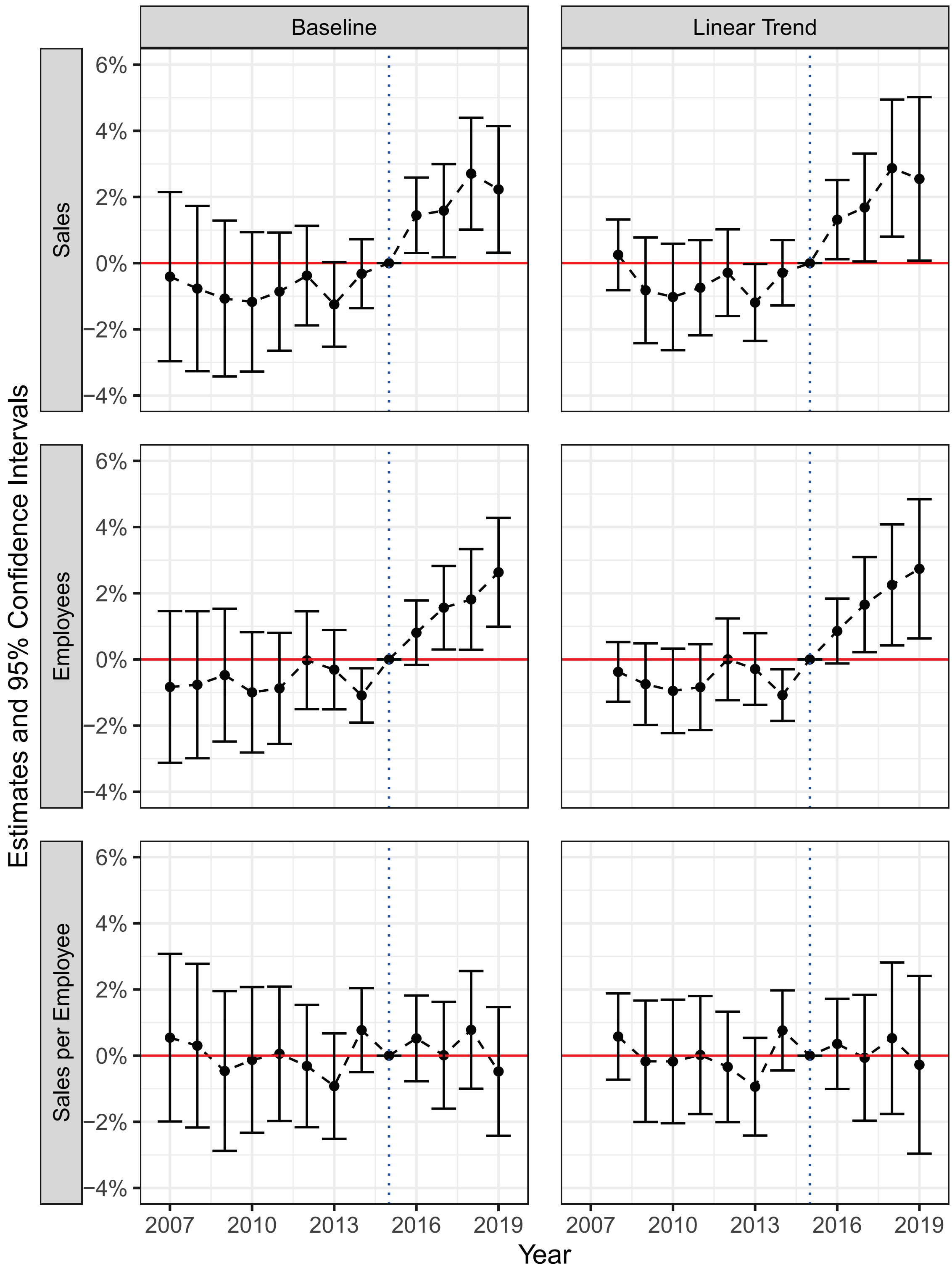

Figure 3: DID Estimates of the Total Effect of Subsidy in 2016

increasing by 1.45% in 2016 by the baseline model.[14] This positive trend continues and strengthens in subsequent years, peaking in 2018 with 2.71% growth in sales from 2015. The average subsidy received by the sample firms was 13 million yen (about $120,000 at 2016 exchange rates). Our estimation suggests that, from 2015 to 2018, each million yen of subsidy led to roughly a 0.2% increase in sales, or, in other words, each $100,000 of subsidy increased sales by approximately 2.2%. The number of employees increased at a similar rate from 2015 to 2019. Since the adopted firms increased both the sales and number of employees comparably, the effect on sales per employee is insignificant, with a coefficient close to zero. The results of the firm-specific linear trend model are consistent with those of the baseline model, showing similar patterns of significance and magnitude in the post-treatment period. The detailed results of the estimates, standard error, p-value, and number of observations are reported in Table B.1–B.3.

To check the robustness of the result, we conduct a sensitivity analysis proposed by Rambachan and Roth (2023). The details of the sensitivity analysis are presented in Appendix C. In this sensitivity analysis, we utilize a partial identification approach that allows the post-treatment trend to deviate from parallel trends. Specifically, we allow the trends of the treated and control groups to deviate by less than a constant parameter multiplied by the maximum difference in pre-trends between the treated and control groups. The estimation results indicate that the statistical significance of the baseline model remains unchanged even if the post-treatment trend is allowed to deviate by 0.1 times the maximum pre-treatment difference. However, when the multiplier exceeds 0.1, some estimates lose their statistical significance. Furthermore, when the multiplier surpasses 0.4, all estimates become statistically insignificant. This sensitivity analysis demonstrates that the baseline estimates remain valid under minor deviations from the parallel trends assumption, but lose significance with larger deviations, highlighting the importance of considering potential violations of this assumption when interpreting our results.

This section presented a conventional DID analysis of the subsidy's total effects on three outcomes. Our initial results suggested positive effects on sales and the number of employees; however, the effect on sales per employee was insignificant under the parallel

[14] The adopted firms were able to use the subsidy starting in June 2016, immediately after it was approved, and were required to complete their projects by the end of 2016. The significant positive effect observed in 2016 is likely to reflect the outcomes of firms that promptly undertook capital investment and experienced early benefits.

trends assumption. While these results provide a useful starting point, they do not account for the dynamic nature of the subsidy program, where firms can receive subsidies multiple times. The following section addresses this limitation by introducing an analytical framework that considers repeat applicants, aiming to provide a more accurate estimation of the subsidy's causal effects.

6 Estimation of Partial Effect

6.1 A comparison between Total Effect and Partial Effects

In this subsection, we estimate the partial effect of the Monodukuri subsidy in 2016 using the method described in Section 2. We then discuss this estimated partial effect and compare it with the total effects from the last section.

First, we estimate the difference in the acceptance probability of future subsidies between the two groups, $\Delta\pi_t$, using the following regression model:

$$\mathrm{Accept}_{i,t} = \sum_{s=1}^{T_1} \gamma_s \mathrm{Accept}_{i,0} I(t = s) + \tau_{tp(i)} + u_{it}, \quad (t = 1, 2, ..., T_1). \tag{1}$$

The parameter γ_t denotes the difference in the acceptance probability, $\Delta\pi_t$. Unlike the regression model in the previous section, this one does not include a firm-fixed effect. This is because the estimated $\Delta\pi_t$ is defined as the difference between different groups, not the average treatment effect ($\Delta\pi_t = E[\tilde{D}_{i,t}|D_i = 1] - E[\tilde{D}_{i,t}|D_i = 0]$).

After estimating $\Delta\pi_t$, we recursively calculate the partial effect, α_t, using the estimated total effect, β_t and the following equation:

$$\alpha_t = \beta_t - \sum_{s=1}^{t} \alpha_{t-s}\Delta\pi_s.$$

Figures 4 and 5, and Table 2 present these estimation results. We find that receiving a subsidy increases the likelihood of obtaining future subsidies, thereby amplifying the total effect. The results show significant differences in acceptance probability for 2018 and 2019. The accepted firms are 9.6 percentage points more likely to receive a subsequent subsidy in 2018 and 5.8 percentage points more likely in 2019 compared to rejected firms. This implies that the increase in future acceptance for accepted firms amplifies the total

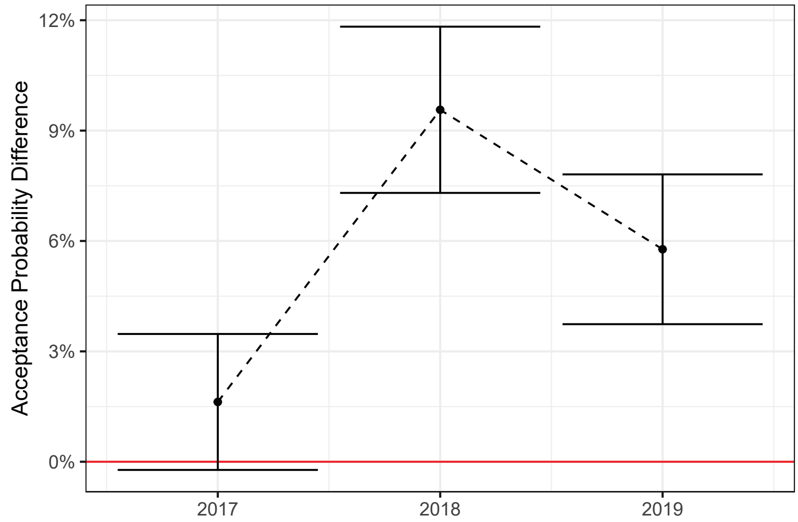

Figure 4: Estimation on Difference of Future Acceptance Probability

Note: The error bars show 95% confidence intervals calculated using the cluster-robust standard errors at the firm level. These estimation results are from the analysis that used sales as the outcome variable. Even when analyzing different outcome variables, the estimation results are nearly identical. Detailed results are shown in Table D.1.

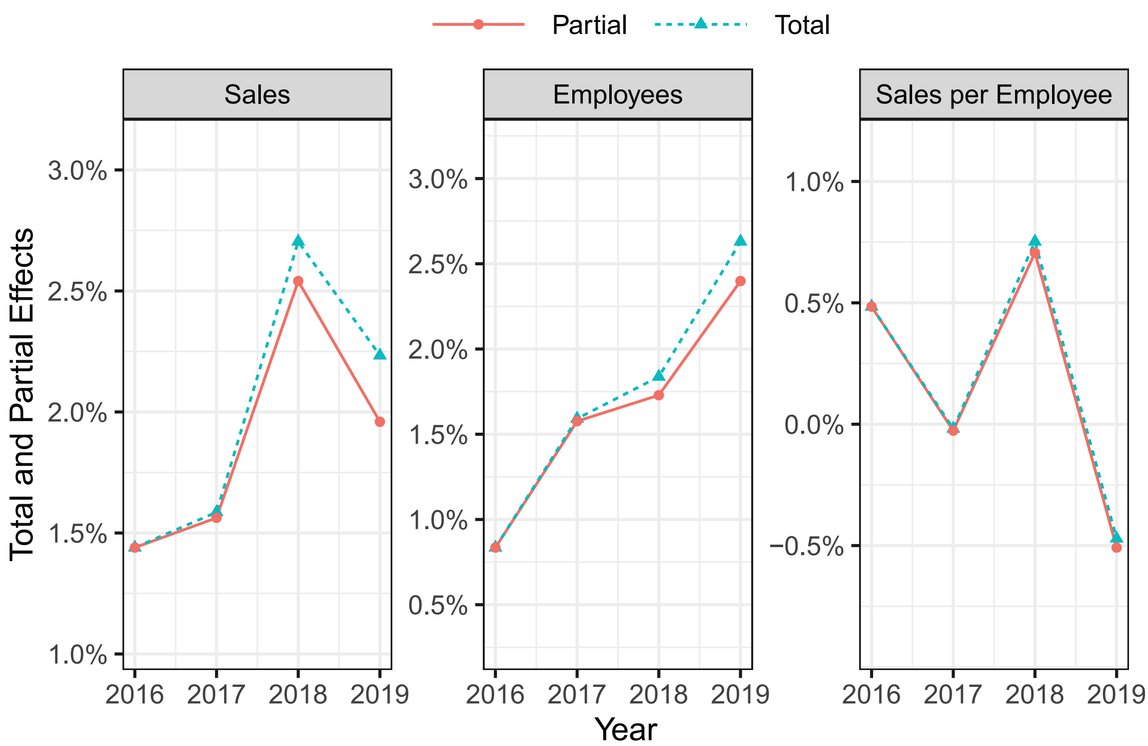

Figure 5: Estimation on Total and Partial Effects

Table 2: Result of Estimation on the Partial Effect of Subsidy

Outcome	Year	Total Estimate	Partial Estimate	Partial 95% C.I.
Sales	2016	0.0144	0.0144	[0.003, 0.026]
	2017	0.0159	0.0156	[0.002, 0.030]
	2018	0.0270	0.0254	[0.009, 0.042]
	2019	0.0223	0.0196	[0.001, 0.038]
Employees	2016	0.0084	0.0084	[-0.001, 0.018]
	2017	0.0159	0.0158	[0.003, 0.028]
	2018	0.0184	0.0173	[0.003, 0.031]
	2019	0.0263	0.0240	[0.009, 0.039]
Sales per Employee	2016	0.0048	0.0048	[-0.008, 0.018]
	2017	-0.0002	-0.0003	[-0.016, 0.016]
	2018	0.0075	0.0071	[-0.010, 0.024]
	2019	-0.0047	-0.0051	[-0.023, 0.013]

Note: This table reports the estimation results of the total and partial effects of the Monodukuri subsidy for 2016. The confidence interval (C.I.) of the partial effect is calculated by bootstrapping. We construct a bootstrap sample by randomly resampling firms with replacement from the original sample, ensuring all observations corresponding to a selected firm are included. We then calculate the partial effects from the bootstrap sample, repeating this process 5,000 times to obtain the bootstrap estimates.

effect. In particular, the gap between the total effect and the partial effect on sales grows over time. In 2017, the difference is small at 1.4% (0.02 percentage points). It increased to 6.4% (0.16 percentage points) in 2018 and further to 14.0% (0.27 percentage points) in 2019. The difference between the total effect and the partial effect on employee numbers gradually widened, mirroring the trend for sales. In contrast, for sales per employee, the total effect and the partial effect remained nearly identical.

Even though future acceptance inflate the total effect, the partial effect of the subsidy demonstrates a significant positive impact, providing evidence for the positive effect of a single subsidy. Table 2 reports bootstrap 95% confidence intervals for the partial effect, which remain consistently positive and exclude zero for sales and employees since 2017. However, the 95% confidence interval for the partial effect on sales per employee includes zero in all cases.

Based on the analysis, we confirmed that the total effect, which does not account for multiple subsidies, can differ from the partial effect, which considers the probability of repeated adoption. For the 2016 Monodukuri subsidy, the divergence between the two effects on sales and the number of employees widened over time.

Table 3: Descriptive Statistics for Applicants by Type of Subsidy

Subsidy Type	Capital (million yen)	Employee (person)	Sales (billion yen)	Firm Age (year)	Acception (%)	N
Standard	23.3 (26.8)	48.6 (81.3)	1.20 (2.94)	38.1 (18.5)	30	8161
Advanced	27.8 (33.9)	62.7 (89.2)	1.58 (2.91)	40.3 (17.6)	36	4106
Small-Scale	15.5 (19.2)	21.2 (41.4)	0.51 (1.29)	29.3 (19.6)	48	2351

Note: Means of variables are reported in each cell, with standard deviations in parentheses. For capital, the top 1% was trimmed to calculate statistics, as outliers significantly affected the mean and standard deviation. Capital and Employee data are sourced from application data, while Sales and Firm Age data are derived from TSR data. The sample includes only firms with at least one record in the TSR dataset from 2015 to 2019.

6.2 Heterogeneous Effects Across Subsidy Size

In the previous subsection, we estimated the average effect of the subsidy without considering variations across different subsidy types. This subsection extends our previous analysis to examine heterogeneous effects across subsidy types. Clarifying the heterogeneous effects of subsidy types provides insights for designing future programs, enabling policymakers to prioritize resource allocation to firm segments with high growth potential.

The 2016 Monodukuri subsidy program offered three subsidies: the Standard Type (hyojun-gata), Advanced Productivity Enhancement Type (kodo-seisan-kojo-gata, hereafter Advanced Type), and Small-Scale Type (syo-kibo-gata).[15] The descriptive statistics for each type are shown in Table 3. The type of subsidy is closely associated with the size of the firm applying for it.[16]

To examine the heterogeneous effects across subsidy types, we divide the data into three subsamples based on the subsidy type applied for in the 2016 program and use the main analysis estimation method for each subsample. Since firms could apply for only one type, there is no overlap of firms across these subsamples. To estimate the partial effects, we control for subsequent subsidy acceptance, regardless of the Monodukuri subsidy types received in 2017 and after.

Figure 6 presents the heterogeneous effects of subsidies on sales across different subsidy types. The probability of subsequent subsidy adoption varies by type, with Advanced

[15] The Standard Type (hyojun-gata) was a default category, offering a maximum amount of 10 million yen. The Advanced Productivity Enhancement Type (kodo-seisan-kojo-gata, hereafter Advanced Type) provided up to 30 million yen but required proposals with higher achievement criteria than those of the Standard Type. The Small-Scale Type (syo-kibo-gata) provided a ceiling of 5 million yen with fewer restrictions on its use.

[16] Although the Small-Scale Type did not have an additional restriction on firm size, the firms applying for this category were, on average, smaller than those in other categories. The firms receiving the Small-Scale Type have sales and employees at approximately 30% of those for the Standard Type.

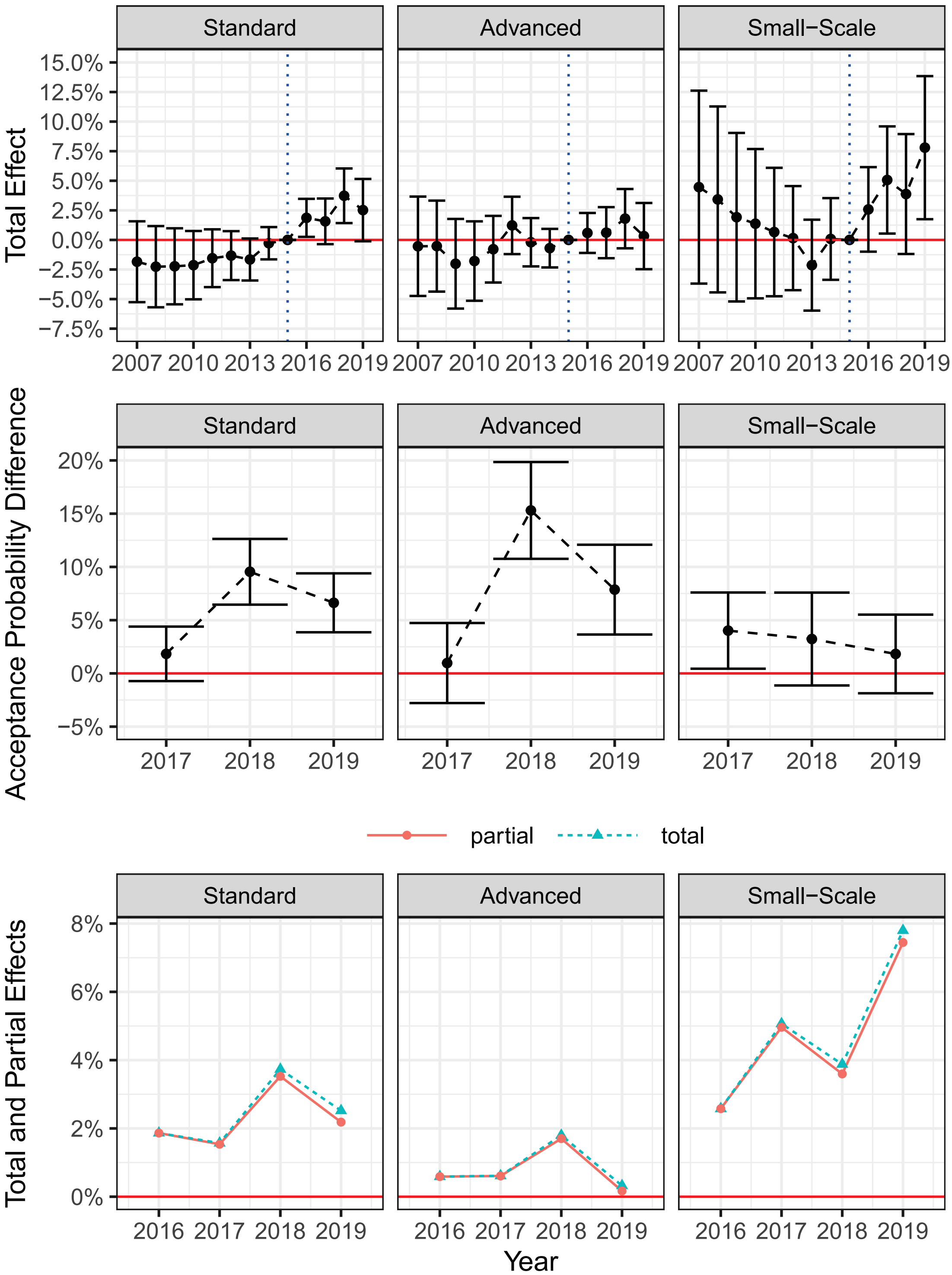

Figure 6: Heterogeneous Effects on Sales by Subsidy Types

Note: Standard, Advanced, and Small-Scale represent the types of subsidies, with maximum thresholds of ¥10 million ($91 thousand), ¥30 million ($273 thousand), and ¥5 million ($46 thousand), respectively. The top panels display the estimated total effects using the DID method, the middle panels show the differences in acceptance probabilities of future subsidies, and the bottom panels present the estimated total and partial effects.

Type recipients having a 15-percentage-point higher adoption rate two years later. For the Small-Scale Type, the minimal difference in adoption probability accounts for the small gap between the total and partial effects. The results also indicate that the Standard Type yielded a significant total effect of 3.7% in 2018, with a partial effect of 3.5%. In contrast, firms receiving the Advanced Type exhibited no statistically significant total and partial effects on sales. The Small-Scale Type generated effects materializing three years after subsidy receipt, resulting in a total effect of 7.8% and a partial effect of 7.4%. Nevertheless, the estimated effects for the small-scale firm group may be less reliable, as their sales exhibit pre-trends and their standard errors are significantly larger than those of the other two groups.

Additionally, Appendix E presents the estimated effects on the number of employees and sales per employee across subsidy types. The Standard Type shows a statistically significant positive total effect on the number of employees. However, the gap between the total and partial effects widens over time, as firms receiving this subsidy are 9.4% and 6.8% more likely to receive additional subsidies two and three years later, respectively. The effect on sales per employee differed markedly across types: it was negligible or negative for the Standard and Advanced Types, while the Small-Scale Type saw an increase from 2017.

The type-specific analysis confirmed the heterogeneous effects of subsidies, with the Small-Scale Type having the larger effects. The higher performance of the Small-Scale Type aligns with Criscuolo et al.'s (2019) finding. SMEs with higher capital constraints tend to benefit more from subsidies, which could result in a larger impact on outcomes (Hall and Lerner, 2010). In addition, the Small-Scale Type covers a broader range of eligible expenses, not limited to capital investment, which may account for its greater effectiveness relative to the other two types.[17] This suggests that subsidies allowing for more flexible use beyond capital investment may have a more positive impact on firm outcomes.

Note, however, that the standard error of the effect of the Small-Scale Type on outcomes is larger than those of the other two types, indicating greater variability. Furthermore, in the Small-Scale Type, a declining trend in sales and employees is observed during

[17] In the Standard and Advanced Types, capital investment was mandatory, with 99% of approved expenses allocated to machinery purchases exceeding 500 thousand yen. In contrast, for the Small-Scale Type, only 76% was designated for such machinery, while the rest covered outsourced manufacturing and raw materials.

the pre-treatment period (Figures 6 and E.1). These patterns suggest that the positive outcomes after treatment may partly reflect the influence of regression to the mean or Ashenfelter's dip, indicating potential overestimation. Therefore, caution should be exercised in attributing the positive effects of the Small-Scale Type entirely to the subsidy.

7 Conclusions

In this study, we estimated the partial effect of the Monodukuri subsidy in 2016. We applied CFR's framework for the DID approach to estimate partial effects. Our findings indicate that the simple DID estimator captures the amplified total effect of a single subsidy, as future adoption likelihood is higher. Even after adjusting for bias from the repeat-application process, results suggest that capital investment subsidies for SMEs promote scale expansion but not productivity gains.

Our findings are crucial for policy evaluation of the subsidy. It is essential to understand the distinction between the total effect and the partial effect in causal interpretation. The total effect represents the causal impact of the entire sequence of interventions, including the initial subsidy and subsequent subsidies. In contrast, the partial effect isolates the causal impact of a single subsidy, controlling for future subsidies. While the total effect may show larger gains, it cannot be attributed to a single policy intervention. The partial effect provides a more precise measure of the impact of a single subsidy, allowing for a more accurate evaluation of the project's performance in the given year. The degree of divergence between total and partial effects varies by outcome and the time elapsed since the base year, making it essential to examine their relationship for each outcome variable.

Future research should address the following challenges to refine our understanding of this issue. While future acceptance probabilities of accepted firms are estimated to be positive for the period 2017-2019, recent policy changes in the Monodukuri subsidy program may alter these dynamics. Since 2020, the program has implemented stricter criteria for firms that received subsidies within the previous three years. This policy shift may reduce probability differences or even make them negative. If accepted firms receive fewer future subsidies than rejected firms, the total effect reflects a diminished partial effect. These changes necessitate further evaluation of the subsidy program, and additional research is needed to understand the policy effects under repeated applications fully.

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A Data Construction

We merged TSR data with the subsidy application data for 2016-2019, as shown in Table A.1. The TSR data are organized by each firm's financial results, reported in different closing months. Merging these datasets requires establishing a correspondence between application years and closing months. This is especially relevant for the 2018 and 2019 programs, as subsidy use extended to January of the following year. A straightforward matching of application year to closing year would result in subsidy utilization being split across multiple periods.

To address this, we constructed the panel data by shifting the TSR year by one month. This adjustment better aligns the TSR data with application data, avoiding multi-year dispersion of subsidy usage. This allows for a more accurate assessment of the impact of subsidies on company performance within each fiscal year.

Table A.1: Connection between TSR and Application Data

Application data		TSR data
Application year	Period of using subsidy	Closing month and year
2016	Jun. 2016 to Dec. 2016	Feb. 2016 to Jan. 2017
2017	Mar. 2017 to Dec. 2017	Feb. 2017 to Jan. 2018
2018	Jun. 2018 to Jan. 2019	Feb. 2018 to Jan. 2019
2019	Mar. 2019 to Jan. 2020	Feb. 2019 to Jan. 2020

B The Results of Simple DID Estimation

Table B.1: Results of DID Estimation for Log of Sales

		Baseline			Linear Trend		
	Year	Estimate	S.E.	P value	Estimate	S.E.	P value
(Pre-treatment)	2007	-0.0041	0.0130	0.756			
	2008	-0.0077	0.0128	0.547	0.0025	0.0055	0.644
	2009	-0.0107	0.0120	0.373	-0.0082	0.0082	0.314
	2010	-0.0117	0.0107	0.277	-0.0102	0.0082	0.213
	2011	-0.0086	0.0091	0.346	-0.0074	0.0073	0.312
	2012	-0.0038	0.0077	0.625	-0.0029	0.0067	0.667
	2013	-0.0125	0.0065	0.056	-0.0119	0.0059	0.045
	2014	-0.0032	0.0053	0.547	-0.0029	0.0050	0.565
(Post-treatment)	2016	0.0145	0.0058	0.013	0.0132	0.0061	0.031
	2017	0.0159	0.0072	0.027	0.0168	0.0083	0.043
	2018	0.0271	0.0086	0.002	0.0287	0.0106	0.007
	2019	0.0223	0.0098	0.022	0.0255	0.0126	0.043
Observations			78154			70029	

Table B.2: Results of DID Estimation for Log of Employees

		Baseline			Linear Trend		
	Year	Estimate	S.E.	P value	Estimate	S.E.	P value
(Pre-treatment)	2007	-0.0083	0.0117	0.476			
	2008	-0.0077	0.0113	0.499	-0.0038	0.0046	0.412
	2009	-0.0047	0.0102	0.643	-0.0075	0.0063	0.234
	2010	-0.0100	0.0093	0.283	-0.0095	0.0065	0.145
	2011	-0.0088	0.0086	0.307	-0.0084	0.0066	0.205
	2012	-0.0003	0.0075	0.973	0.0000	0.0063	0.998
	2013	-0.0031	0.0061	0.614	-0.0029	0.0055	0.599
	2014	-0.0109	0.0042	0.009	-0.0108	0.0040	0.007
(Post-treatment)	2016	0.0081	0.0050	0.105	0.0086	0.0050	0.086
	2017	0.0156	0.0064	0.015	0.0166	0.0073	0.024
	2018	0.0181	0.0078	0.020	0.0225	0.0093	0.016
	2019	0.0263	0.0084	0.002	0.0274	0.0107	0.011
Observations			78740			70573	

Table B.3: Results of DID Estimation for Log of Sales per Employee

		Baseline			Linear Trend		
	Year	Estimate	S.E.	P value	Estimate	S.E.	P value
(Pre-treatment)	2007	0.0054	0.0129	0.674			
	2008	0.0030	0.0126	0.811	0.0058	0.0067	0.386
	2009	-0.0047	0.0123	0.706	-0.0017	0.0094	0.856
	2010	-0.0013	0.0112	0.909	-0.0018	0.0095	0.854
	2011	0.0006	0.0104	0.957	0.0002	0.0091	0.984
	2012	-0.0031	0.0094	0.740	-0.0034	0.0085	0.689
	2013	-0.0092	0.0081	0.257	-0.0094	0.0075	0.213
	2014	0.0077	0.0065	0.233	0.0076	0.0062	0.217
(Post-treatment)	2016	0.0052	0.0066	0.430	0.0036	0.0070	0.609
	2017	0.0001	0.0082	0.988	-0.0006	0.0097	0.948
	2018	0.0078	0.0091	0.390	0.0053	0.0117	0.652
	2019	-0.0048	0.0099	0.630	-0.0028	0.0137	0.840
Observations			77823			69740	

C Sensitivity Analysis

We employ the partial identification method of Rambachan and Roth (2023), which relaxes the assumption of parallel trends, to assess the robustness of the baseline estimation results.

Rambachan and Roth (2023) assume that the parameter of the interaction term between the treatment dummy and the year indicator function, β, are decomposed as follows:

$$\beta = \begin{pmatrix} 0 \\ \tau_{\text{post}} \end{pmatrix} + \begin{pmatrix} \delta_{\text{pre}} \\ \delta_{\text{post}} \end{pmatrix},$$

where τ_{post} represents the causal effect of the subsidy, δ_{pre} denotes the pre-trend, and δ_{post} captures the post-trend. Note that the parallel trends assumption implies that $\delta_{\text{post}} = 0$. A vector of trend, δ, are defined, as follows:

$$\begin{aligned}\delta &= [\underbrace{\delta_{-T_0}, \ldots, \delta_{-1}, \delta_0}_{=\delta_{\text{pre}}}, \underbrace{\delta_1, \ldots, \delta_{T_1}}_{=\delta_{\text{post}}}]' \\ &= [\delta'_{\text{pre}}, \delta'_{\text{post}}]'\end{aligned}$$

In our sensitivity analysis, the estimates are identified by the bound of relative magnitude (RM) such as,

$$\Delta^{RM}(M) = \{\delta : \forall t \geq 0, |\delta_{t+1} - \delta_t| \leq M \cdot \max_{s<0} |\delta_{s+1} - \delta_s|\}.$$

The RM bound allows for a random walk in the post-trend up to M times the maximum variation of the pre-trend. When M is 0, this bound is equivalent to the identification based on the parallel trends assumption. $M > 0$ permits the deviation from a parallel trend. By varying the value of M, we can assess the sensitivity of the estimated coefficients under the weaker assumption on the post-treatment trend.

The 95% confidence set is estimated using the conditional and least-favourable hybrid test developed by Rambachan and Roth (2023). The estimation of the confidence set is implemented for the baseline model estimation in Section 5. The estimation results are shown in Figure C.1 and Table C.1.

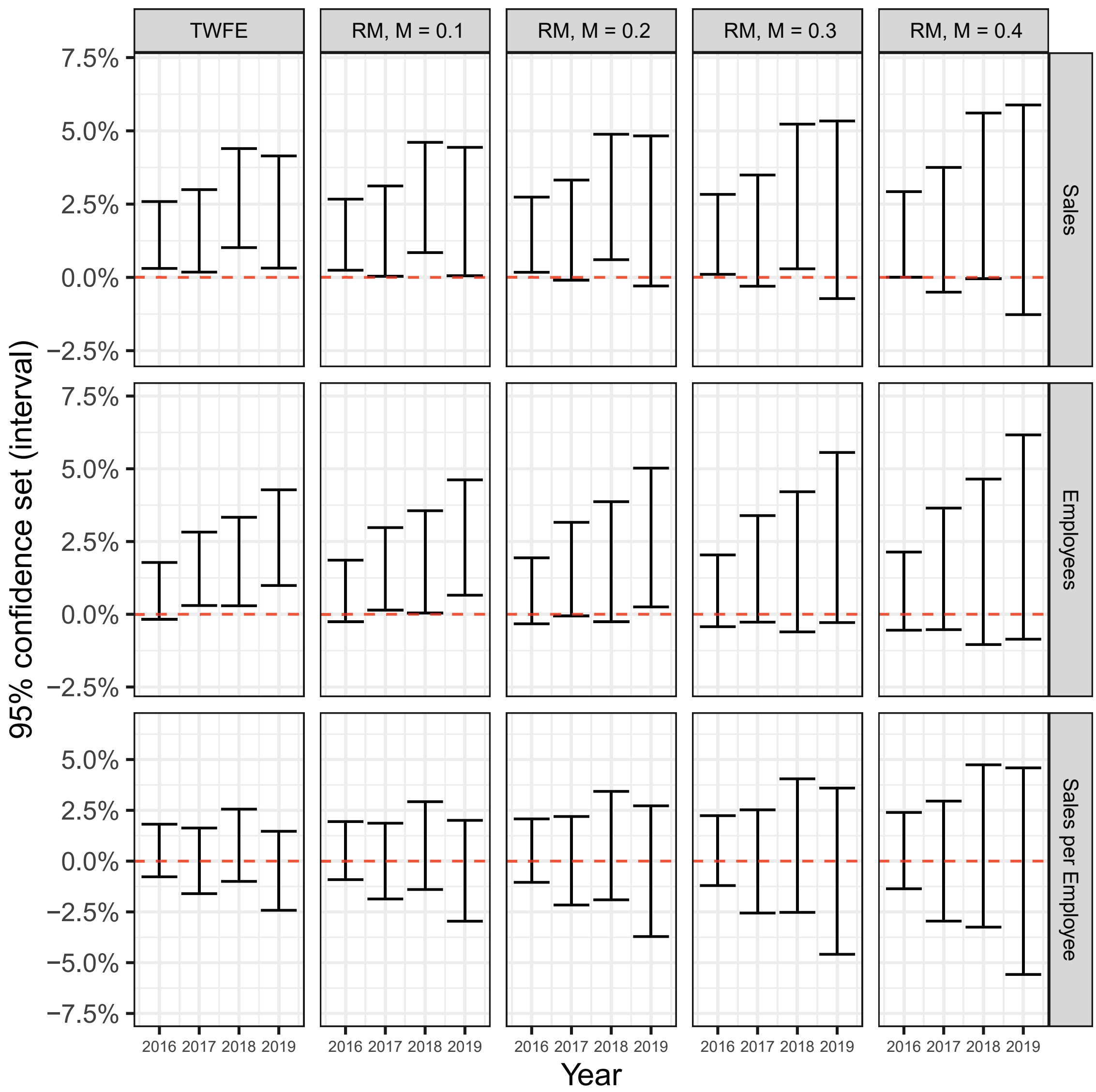

Figure C.1: Confidence Sets with Different M

The estimation results reveal the breakdown value of M that causes the estimated bands to include zero. In the baseline TWFE model, all coefficients are statistically significant at the 5% level, and this significance persists even at M=0.1. However, when M is increased to 0.2, the estimated bands for 2017 and 2019 include zero, indicating these coefficients are not statistically different from zero. At M = 0.5, all bands include zero, resulting in all coefficients becoming statistically insignificant. These results suggest that coefficient significance is robust only to slight deviations in parallel trends compared to the pre-trend.

Table C.1: Result of the Set Identification of the Total Effect

Dependent Variables	Method	M	95% Confidence Set (or Interval) 2016	2017	2018	2019
Sales	TWFE		[0.003, 0.026]	[0.002, 0.030]	[0.010, 0.044]	[0.003, 0.041]
	RM	0.1	[0.002, 0.027]	[0.000, 0.031]	[0.008, 0.046]	[0.001, 0.044]
		0.2	[0.002, 0.027]	[-0.001, 0.033]	[0.006, 0.049]	[-0.003, 0.048]
		0.3	[0.001, 0.028]	[-0.003, 0.035]	[0.003, 0.052]	[-0.007, 0.053]
		0.4	[0.000, 0.029]	[-0.005, 0.038]	[-0.001, 0.056]	[-0.013, 0.059]
Employees	TWFE		[-0.002, 0.018]	[0.003, 0.028]	[0.003, 0.033]	[0.010, 0.043]
	RM	0.1	[-0.002, 0.019]	[0.001, 0.030]	[0.000, 0.036]	[0.007, 0.046]
		0.2	[-0.003, 0.019]	[-0.001, 0.032]	[-0.003, 0.039]	[0.003, 0.050]
		0.3	[-0.004, 0.020]	[-0.003, 0.034]	[-0.006, 0.042]	[-0.003, 0.056]
		0.4	[-0.005, 0.021]	[-0.005, 0.036]	[-0.010, 0.046]	[-0.009, 0.062]
Sales per Employee	TWFE		[-0.008, 0.018]	[-0.016, 0.016]	[-0.010, 0.026]	[-0.024, 0.015]
	RM	0.1	[-0.009, 0.019]	[-0.019, 0.019]	[-0.014, 0.029]	[-0.030, 0.020]
		0.2	[-0.010, 0.021]	[-0.022, 0.022]	[-0.019, 0.034]	[-0.037, 0.027]
		0.3	[-0.012, 0.022]	[-0.026, 0.025]	[-0.025, 0.040]	[-0.046, 0.036]
		0.4	[-0.014, 0.024]	[-0.030, 0.030]	[-0.033, 0.047]	[-0.056, 0.046]

D Probability Differences in Future Acceptance

The estimation results for equation (1) are reported in Table D.1. The 95% confidence intervals (C.I.) are calculated using cluster-robust standard errors at the firm level. Our analysis focused on firms with consistently observed outcome variables, resulting in slight sample variations by outcome variable in the main model. However, these differences minimally impact the point estimates of probability differences.

Table D.1: Result of Estimation on the Probability Differences in Future Acceptance

		Probability Difference	
Sample's Outcome Variable	Year	Estimate	95% C.I.
Sales	2017	0.0163	[-0.002, 0.035]
(#Obs = 16949)	2018	0.0957	[0.073, 0.118]
	2019	0.0577	[0.037, 0.078]
Employees	2017	0.0181	[0.000, 0.037]
(#Obs = 17087)	2018	0.0958	[0.073, 0.118]
	2019	0.0587	[0.038, 0.079]
Sales per Employee	2017	0.0165	[-0.002, 0.035]
(#Obs = 16874)	2018	0.0959	[0.073, 0.119]
	2019	0.0579	[0.037, 0.078]

E Heterogeneous Effects Across Subsidy Size

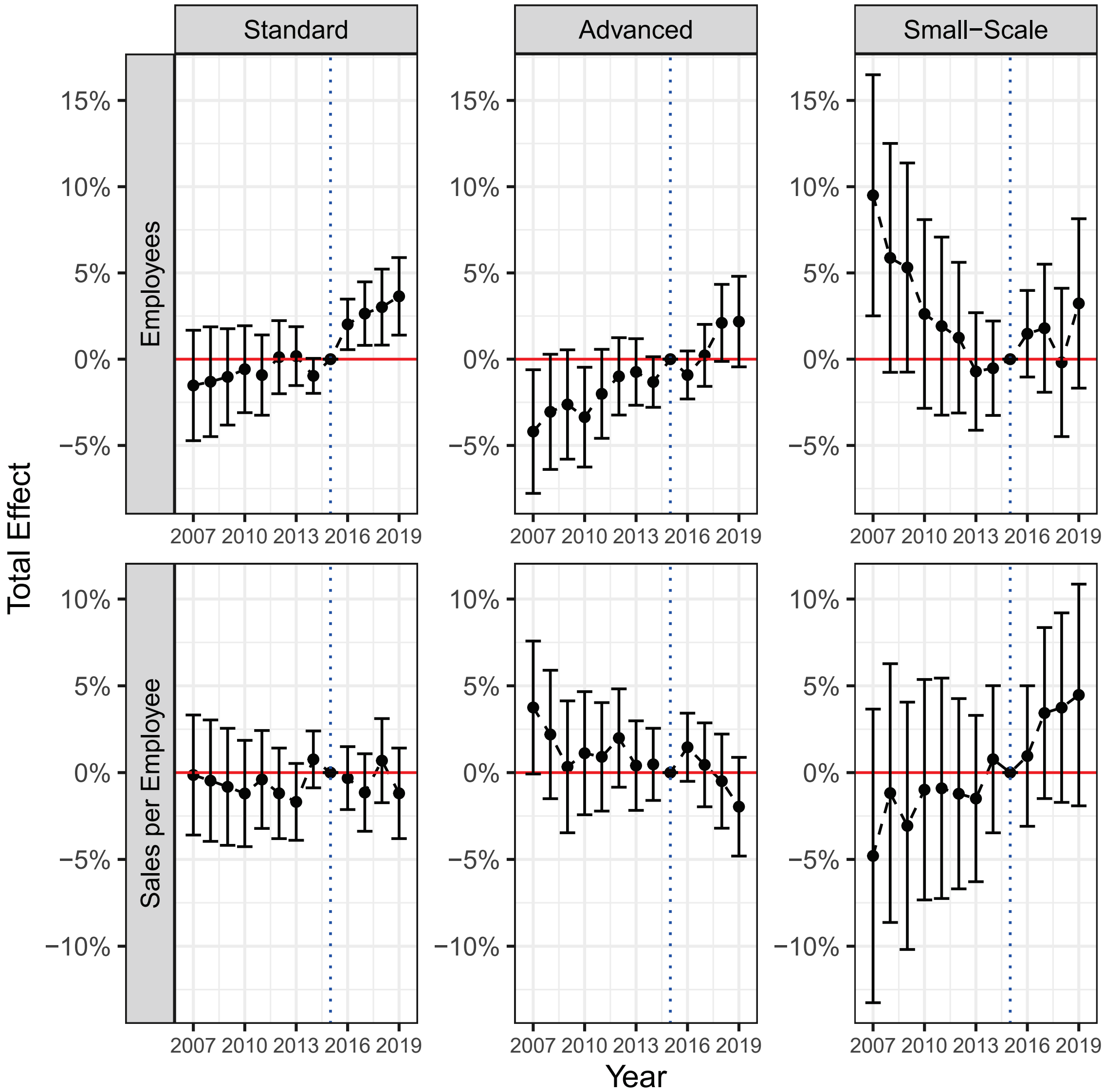

Figure E.1: Total Effect on Employees and Sales per Employee by Subsidy Type

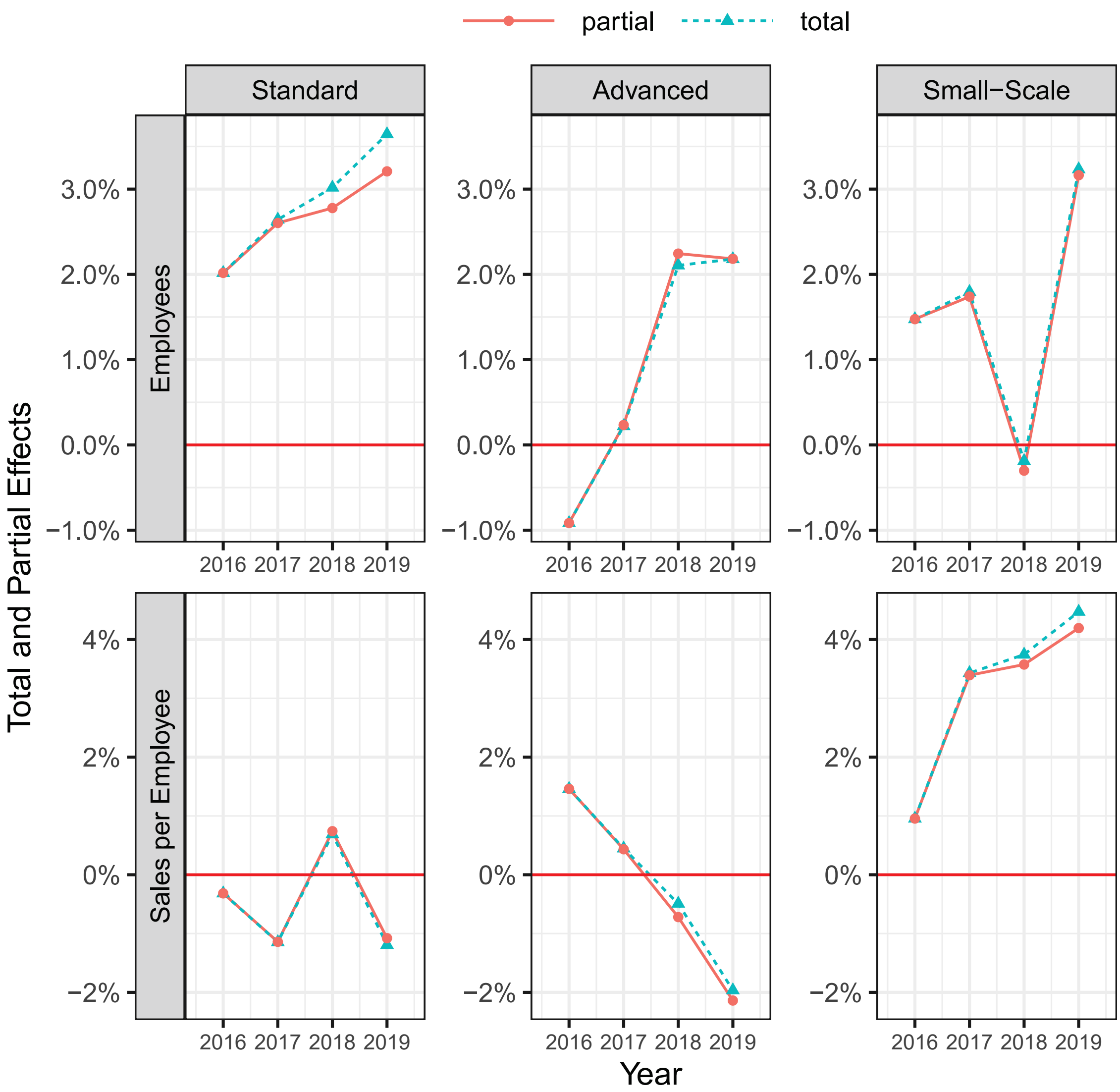

Figure E.2: Total and Partial Effects on Employees and Sales per Employee by Subsidy Type

Note: The difference in probability for being treated in the future rounds is nearly identical to the middle panels of Figure 6. Therefore, we omit an additional figure to avoid redundancy.