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Investment Effects of a Quasi-Robot Tax: Evidence from South Korea*

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Abstract

We study a 2018 reform in South Korea that reduced tax credits for automation investments. This reform increased the tax cost of investing in robots and thus resembles a robot tax. Exploiting this natural experiment with industry-level data on robot installations and firm-level data from *Orbis*, we document a sharp decline in automation investments after the reform in industries with a large share of affected firms. At the firm level, we find that affected firms increased employment, consistent with the notion that robots replaced workers. The effects are heterogeneous: financially constrained firms cut investment overall, while unconstrained firms substituted away from robots, hired more workers, and reallocated resources toward more productive uses. For the latter group, we find improvements in various measures of investment quality, suggesting that the tax credit induced inefficient overinvestment in automation. Our evidence informs ongoing debates on robot taxation and the efficiency of tax incentives.

Keywords: tax credits, automation, robot tax

JEL Classification: H25, H32, O33

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1 Introduction

Automation in industrial processes and services has advanced rapidly in recent years. The number of annual industrial robot installations in Asia increased from 199,481 units in 2016 to 380,078 units in 2021—a 90% increase in five years. Robots have become attractive partly because current tax practices favor their use. While regular employees must pay income tax on their employment income, robots face no similar tax on their "income". This tax treatment encourages firms to adjust their production processes towards automation.

In response to these trends, several countries (e.g., the USA, Canada, New Zealand, and Poland) are discussing the implementation of a tax on industrial robots, commonly referred to as a "robot tax". A robot tax would not only remove the preferential tax treatment of industrial robots but also compensate countries for lost income tax revenues. Governments could use the additional funds to retrain workers who lose their jobs to automation, as Bill Gates suggested.[1] However, opponents argue that taxing robots could stifle innovation and discourage firms from adopting new technologies, thereby reducing productivity growth and competitiveness. Additionally, there are concerns about the complexity of designing and implementing such a tax, particularly when it comes to defining what qualifies as a "robot" and measuring its economic contribution.

Despite the growing interest in the idea of a robot tax, there is almost no empirical evidence on the real effects of incentivizing or disincentivizing automation investment via taxes. We aim to provide some guidance on these effects by exploiting a reform in South Korea that reduced a long-standing tax credit on automation investments. For decades, South Korean firms benefited from a tax credit that applied to new investments in automated systems, including industrial robots. The 2018 reform reduced the tax credit available to larger firms and those that are part of a large business group. By effectively increasing the cost of automation for some firms, the reform can be interpreted as a quasi-robot tax and serves as a natural experiment that allows us to evaluate the potential impacts of a robot tax.

Understanding how tax regulations affect firms' real investment decisions is central to accounting research, as it connects tax policy to firms' resource allocation and reporting behavior. Recent studies highlight the importance of examining such real effects of tax regulation on corporate actions (see, e.g., Lester and Olbert, 2025). By focusing on the potential consequences of a robot tax, this paper contributes to this emerging line of research at the intersection of taxation, accounting, and real economic outcomes.

It is particularly worthwhile to study the investment effects of a quasi-robot tax (or a tax credit targeted at automation) because they are likely distinct from those of broader

[1] See https://qz.com/911968/bill-gates-the-robot-that-takes-your-job-should-pay-taxes.

tax incentives. Corporate tax rates influence the cost of capital for all types of investment, and general tax credits or accelerated depreciation provisions usually affect a wide range of assets. In contrast, the quasi-robot tax directly impacts a narrowly defined category of investment. This specificity may result in different overall investment responses, as firms weigh the unique costs and benefits of automation relative to other capital expenditures. Furthermore, such targeted measures could lead to substitution effects, prompting firms to shift resources between automation and other forms of investment in ways that broader policies do not. Finally, the consequences of discouraging automation—such as a shift toward more labor-intensive production and potential changes in investment efficiency—are of particular interest. Our analysis speaks directly to these margins by examining both the quantity and the composition of investment, as well as related consequences on employment and firm performance.

In the first part of our empirical analysis, we investigate whether the quasi-robot tax (i.e., the reduction in the tax credit) led to a decline in robot adoption within South Korea. We employ two identification strategies to examine this effect. First, we combine industry-level data on robot installations from the International Federation of Robotics (IFR) with firm-level financial data from *Orbis*. Using the *Orbis* data, we construct a measure of industry-level exposure to the reform based on the share of firms affected by the tax credit reduction. The policy change applied only to large and medium-sized firms, as well as to small firms affiliated with a large business group. We find that industries with a higher share of treated firms experienced significantly lower robot adoption following the reform. As a complementary analysis, we also test the effect on industry-level robot installations in a cross-country difference-in-differences (DiD) setting. We find that Korean industries decreased robot installations by 28% relative to their Japanese counterparts following the reform.

Second, we directly leverage the firm-level data, which enables us to implement a DiD design. As the *Orbis* data do not include direct measures of automation investments, we use investment in fixed tangible and intangible assets as proxy. The firm-level analysis corroborates the industry-level findings. On average, treated firms reduced their investment in tangible and intangible fixed assets (relative to total assets) by approximately 3%-points relative to unaffected firms.

In the second part of the analysis, we further explore the firm-level data to examine indirect effects of the reform and heterogeneity in firm responses. Specifically, we assess whether firms increased their reliance on labor to compensate for reduced automation. The existing literature draws no clear picture, as to whether robots and employees are substitutes or complements. Some studies find that robots substitute for human labor, particularly in routine and manual tasks, leading to job displacement and reduced wages

(Frey and Osborne, 2017; Acemoglu and Restrepo, 2020; Bonfiglioli et al., 2024; Giuntella et al., 2024; Bessen et al., 2025). Conversely, other research suggests that robots complement human workers by enhancing productivity and creating new employment opportunities without reducing overall employment levels (Autor, 2015; Graetz and Michaels, 2018; Dauth et al., 2021). Our findings contribute to this debate by providing firm-level evidence that supports the substitutability between robots and employees. We find evidence of a positive effect on firm-level employment in the sub-sample of less financially constrained firms, suggesting that robots and employees act as substitutes here; the average effect in the full sample is, however, statistically indistinguishable from zero.

While we observe that the reduction in the tax credit lowers the *quantity* of investment, it may also affect the *quality* of investment. A generous tax credit can induce overinvestment by encouraging firms to adopt automation technologies that are not necessarily productivity-enhancing. If so, scaling back the credit could improve allocative efficiency. Alternatively, the tax credit might have been necessary to overcome firms' reluctance to adopt new technologies, and its removal could exacerbate underinvestment. Moreover, the reduced credit implies that firms have less cash on hand, which could constrain investment, especially for financially constrained firms. To examine this, we use current and future turnover and profit as proxies for investment quality. We find positive effects among financially unconstrained firms, suggesting that the pre-reform tax incentive may have led to inefficient overinvestment in automation. No such effect is observed among financially constrained firms.

Taken together, our results suggest that the quasi-robot tax had asymmetric effects across firms, depending on their financial constraints. Unconstrained firms appear to have substituted away from robot investments toward other production technologies involving additional labor. This reallocation was accompanied by gains in investment quality. By contrast, financially constrained firms responded by cutting overall investment, with no comparable offsetting effects on employment or investment quality. These findings also imply that when tax incentives explicitly favor automation, countries may inadvertently encourage overinvestment in robotics, leading to inefficient capital allocation and potentially exacerbating disparities between financially constrained and unconstrained firms.

Our study is related to the large literature on how firm-level tax incentives influence investment decisions (see Jacob, 2022, for a survey). Lower corporate tax rates (Dobbins and Jacob, 2016; Giroud and Rauh, 2019; Coles et al., 2022; Link et al., 2024) and more generous depreciation allowances (Wielhouwer and Wiersma, 2017; Zwick and Mahon, 2017; Maffini et al., 2019; Ohrn, 2019; Garrett et al., 2020; Curtis et al., 2021) increase investment, but may decrease investment quality (Eichfelder et al., 2023). Effect sizes vary significantly, depending, e.g., on financial constraints (Zwick and Mahon, 2017; Dobbins and Jacob, 2016) or losses (Edgerton, 2010). Ohrn (2018) and Lester (2019) study the U.S. Domestic Production Activities Deduction Act, which allows firms to deduct part of income related to domestic production, and both find substantial investment responses. These papers studied broad-based tax incentives that lowered the cost for all or for most forms of investment, whereas we focus on a tax credit aimed only at a specific type of capital, automation investment.

We also contribute to this literature by providing novel evidence on how financial constraints shape firm-level responses to tax-based investment incentives. Zwick and Mahon (2017) show that financially constrained firms are 1.5 to 2.6 times more responsive to bonus depreciation than their less constrained counterparts. Orihara and Suzuki (2023) confirm the finding of a stronger investment response by more constrained firms to temporary investment incentives in a Japanese firm panel. Our findings extend this insight by demonstrating that financial frictions not only affect the magnitude of investment responses but also influence the underlying adjustment mechanisms and potential substitution patterns across input factors. In this sense, our findings also speak to the literature on the determinants of over- and underinvestment and investment efficiency (Biddle and Hilary, 2006; Biddle et al., 2009).

Furthermore, we contribute to the literature on the implications and the optimal design of robot taxation (see Bastani and Waldenström, 2024, for a survey of this largely theoretical literature). As the reduction in the tax credit is similar to the introduction of a robot tax, we can provide some empirical insights on the implications that the introduction of a robot tax would have.[2] Lastly, a concurrent working paper by Kang et al. (2024) also examines the South Korean reform. They document reduced robot adoption and some reallocation toward labor, but focus mainly on labor market and welfare effects from an economics perspective. For the subgroup of firms with revenues between 50 and 200 billion Korean Won, they find that employment increases by about 3.8%, while earnings per worker decrease. In contrast, our study takes an accounting perspective and centers on firms' real investment behavior and the implications of tax regulation for investment efficiency. We show how the reform reduced automation investment, triggered partial substitution toward labor, and affected the quality of investment. Unlike Kang et al. (2024), we provide evidence that scaling back tax incentives mitigated inefficient overinvestment

[2]Previous theoretical literature (see, e.g., Guerreiro et al., 2022; Thuemmel, 2023) discusses whether robots should be taxed and generally finds that taxing robots can reduce income inequality, increase welfare, and mitigate negative effects of automation on routine workers. These studies suggest that robot taxes could have positive effects on redistribution and welfare. Conversely, Mazur (2019) warns of potential negative effects, such as stifling innovation. Englisch (2018) argues that taxing shareholders or capital returns might be a more efficient approach than taxing robots directly.

in automation and improved investment quality among less financially constrained firms.[3] Together, the two studies offer complementary perspectives on the consequences of a quasi-robot tax: Kang et al. (2024) from a labor economics viewpoint, and this paper from an accounting perspective that links tax regulation to firms' real decisions.[4]

Our paper proceeds as follows. Section 2 describes the institutional setting in South Korea and derives our hypotheses. We present the data in Section 3 and discuss the empirical strategy in Section 4. Section 5 presents our empirical findings. Section 6 concludes.

2 Institutional Setting and Hypotheses Development

2.1 Institutional Setting

Industrial robots in South Korea. South Korea is one of the global leaders in automation. In the 1980s and 1990s, the South Korean government prioritized technological advancement within its economic development strategy, recognizing the importance of automation for transforming the manufacturing sector. Industrial robots have played a central role in this process. In this context, a "robot" is an "automatically controlled, reprogrammable, multipurpose manipulator programmable in three or more axes, which can be either fixed or mobile for use in industrial automation applications" (based on ISO 8373:2012, which is also the definition used by the IFR).

According to the IFR, South Korea was an early adopter of industrial robots, with 5,462 of them newly installed in 1993—comparable to the 10,492 installed in the substantially larger U.S. in the same year. While the country was always at the forefront of using robots in production, their use really took off around 2010, when new robot installations in South Korea caught up with those in Japan, also a substantially larger country (see Figure 1).

Today, robots are common in many industries. The use of robots is especially common in the electronics (e.g., household appliances, semiconductors, LEDs, solar cells) and automotive industries. In 2021, the average robot density in the manufacturing sector worldwide was around 141 newly installed robots per 10,000 employees. South Korea was the leading country with 1,000 robots per 10,000 employees, followed by Singapore (670 robots per 10,000 employees), Japan (399 robots per 10,000 employees), and Germany (397 robots per 10,000 employees) (IFR Statistical Department, 2022).

[3] There are also methodological differences: The identification in Kang et al. (2024) relies largely on the revenue thresholds, whereas in our sample often the membership in a corporate group decides whether a firm is treated.

[4] Hirvonen et al. (2022) study a subsidy for technology adoption in Finland. They find that the subsidy induced firms to use new technologies to produce new types of output rather than replace workers with technologies within the same type of production.

The automation tax credit. Investing in automation, particularly in industrial robots, involves significant costs for firms. Depending on the size and function of a robot, these investment costs can range from $20,000 to $400,000 per unit (EVS Tech Co. LTD, 2023).[5] To help local firms manage these substantial costs and strengthen their international competitiveness, the South Korean government introduced a tax credit for productivity-enhancing investments in 2002. The credit, codified in Article 24 of the Restriction of Special Taxation Act, applies to new investments in automated systems, including industrial robots.[6]

The credit is granted in the year the robot is purchased. Its generosity depends on firm size: small firms receive the highest rate, with 7% of the acquisition cost reimbursed by the government. Until 2017, medium-sized entities qualified for a 5% credit and large entities for a 3% credit.[7] From 2018 onward, these rates were reduced to 3% and 1%, respectively (see Panel A of Table 1). The credit directly lowers the corporate income tax liability. If the liability is insufficient in the year of purchase, unused amounts can be carried forward for up to five years.

In addition to the automation credit, South Korea offers several other investment-related tax credits. However, if a firm claims the automation tax credit, which is by far the most important, it cannot simultaneously claim another investment-type tax credit (such as an energy-saving or safety facility investment credit) for the same asset. Using tax return data, Kang et al. (2024) show that for manufacturing firms, the automation credit exceeded all other investment credits combined (amounting to roughly 430 million USD in 2019).[8] They also find that around 5% of small firms and almost 25% of large firms used the credit. Among users, the benefit corresponded to 5.2% to 15% of total tax payable, depending on firm size. Thus, the tax credit for automation investment is substantial, making it plausible that its reduction affects investment behavior.

Firm size definitions. South Korea applies a relatively broad definition of small- and medium-sized entities, based on two criteria: (1) firm size and (2) firm independence. Regarding firm size, a firm qualifies as a small firm if it has (1a) turnover below 80–140 billion Won (about USD 72–145 million in 2018, depending on the industry) and (1b) total assets below 500 billion Won (about USD 450 million).[9]

[5] The average price of robots produced in Korea remained stable at around 22,000 USD in the years around the reform (2017–2019) (IFR Statistical Department, 2022).

[6] For an excerpt from the list of assets eligible for this tax credit, see Table A1 in the appendix.

[7] The category of "medium" sized firms has existed only since 2015. Before that, firms were either small or "non-small" (i.e., large).

[8] In addition, the variation we exploit is specific to the tax credit for automation investments.

[9] The thresholds are 80 billion Won for beverages and medical industries, 100 billion Won for food, tobacco, textile, wood, chemicals, electronics, and automobile industries, and 150 billion Won for clothing, metal, electric appliances, and furniture.

Regarding firm independence, a firm qualifies as a small firm if it (2a) does not belong to a business group subject to mutual investment or debt guarantee restrictions, (2b) is not owned by a corporation with total assets of 500 billion KRW or more, and (2c) none of its affiliates exceed the small firm thresholds.

A medium-sized entity is one that does not qualify as a small firm but whose turnover in the previous three years is below 300 billion Won (about USD 270 million). Firms that qualify as neither small nor medium-sized entities are classified as large.

Small firm status will determine treatment in our empirical analysis.

TABLE 1: Automation investment tax credit and comparison of tax benefits

Panel A: Change in tax credit

	Small	Middle	Large
2017	7%	5%	3%
2018	7%	3%	1%

Panel B: Example calculation

	Until 2017	From 2018
Pre-tax income	400,000	400,000
Preliminary tax (24%)	96,000	96,000
Investment in automation	100,000	100,000
Tax credit	5% = 5,000	3% = 3,000
Final tax payment	91,000	93,000

Panel C: Comparison of tax benefits under different regimes

Year	Tax credit (3%)	Tax credit (5%)	Straight-line (10 yrs)	Bonus depreciation (40%)
1	5,400	7,400	2,400	11,040
2	2,400	2,400	2,400	1,440
3	2,400	2,400	2,400	1,440
4	2,400	2,400	2,400	1,440
5	2,400	2,400	2,400	1,440
6	2,400	2,400	2,400	1,440
7	2,400	2,400	2,400	1,440
8	2,400	2,400	2,400	1,440
9	2,400	2,400	2,400	1,440
10	2,400	2,400	2,400	1,440
Total tax benefit	**27,000**	**29,000**	**24,000**	**24,000**
PV of total tax benefit	**22,459**	**24,459**	**19,459**	**21,275**

Notes: Panel A shows the change in the automation investment tax credit before and after the 2018 reform. Panel B illustrates an example calculation for a firm investing 100,000 in automation. Panel C compares the total and present value (PV) of tax benefits under different regimes: A 3% tax credit, a 5% tax credit, straight-line depreciation over ten years and 40% bonus depreciation. We assume an investment of 100.000, a corporate tax rate of 24%, and a discount rate of 5%. Tax benefits are assumed to be realized at the beginning of the fiscal year.

The reform. In 2018, South Korea reduced the automation investment tax credit by two percentage points for medium-sized and large firms, with the change applying to all fiscal years after December 31, 2017.[10] This reduction has been described in the media as the introduction of a "robot tax."[11]

The reform stemmed from a broader political shift that was unrelated to firms' automation activities. Following the impeachment of the president in March 2017, the new government, in office from May 2017, introduced a series of policy changes that diverged sharply from those of the previous administration. A central goal of these new policies was to reduce inequality and enhance tax fairness. To achieve this, the government rolled back tax reductions for large corporations, aiming to improve the competitive position of small and medium-sized firms. This approach enabled these firms to compete more effectively with larger firms, and consequently, the reduction in tax credits for new automation investments was applied irrespective of firms' existing automation levels.[12]

This setting provides a suitable natural experiment. A central requirement for a DiD design is the exogeneity of the policy change. In this case, the reduction in tax credits was politically motivated rather than driven by firms' investment behavior, making it plausibly exogenous to firm-level automation decisions. This allows us to isolate the causal effect of the quasi-robot tax on investment outcomes.

Panel B of Table 1 illustrates the immediate impact of the reform. For a medium-sized firm with pre-tax income of $400,000 and an automation investment of $100,000, the tax credit fell from 5% ($5,000) to 3% ($3,000). As a result, the firm's final tax liability increased from $91,000 to $93,000. This simple example shows how the reform effectively raised the tax cost of robot adoption.

An important feature of the Korean tax credit is that it is granted in addition to regular tax depreciation. Unlike bonus depreciation schemes, which mainly accelerate the timing of deductions, the Korean credit provides an immediate and permanent tax benefit. Panel C of Table 1 compares four regimes: a 3% tax credit, a 5% tax credit, straight-line depreciation over ten years, and 40% bonus depreciation. We assume an asset with

[10] During our observation period, South Korea offered several other tax credits, including those for safety facilities, research and development, job creation, and energy and environmental initiatives. The credit for safety facilities was also reduced by two percentage points from 2018 onward. While we know the distribution of robots across industries, we do not observe the distribution of safety investments. Assuming equal distribution, the reduced credit for safety investments would matter relatively more in low-robot industries.

[11] See, e.g., The Korea Times, The Telegraph, Mashable, or The New York Times.

[12] Other measures included raising the top income tax rate, increasing capital gains taxation for major shareholders, reducing R&D credits for large firms, tightening inheritance and gift tax deductions, and increasing taxation of rental income.

acquisition costs of \$100,000 and a ten-year useful life,[13] a corporate tax rate of 24%, and a discount rate of 5%. The table reports yearly tax benefits, as well as their totals and present values. Both the 5% and the 3% credit yield larger benefits than 40% bonus depreciation. The reason is straightforward: tax credits reduce the tax burden on top of regular depreciation, whereas bonus depreciation only shifts deductions forward in time without changing their total amount. The difference is less pronounced once we account for discounting, but the credits still provide a larger present value benefit. Notably, even the reduced 3% credit after the reform yields a larger benefit than 40% bonus depreciation, which is typically considered a generous tax incentive. Also the effect of moving from a 5% to a 3% tax credit—as in the 2018 reform—is more pronounced than introducing a 40% bonus depreciation.

2.2 Hypotheses

We derive three hypotheses regarding the effects of reducing the automation tax credit in South Korea. These hypotheses address robot investments, the broader response of firms in terms of production and employment (and how these may vary depending on financial constraints), and the quality of investments following the reform.

The first question we address is how the reduction in the automation tax credit affects investments in robots. The reduced tax credit makes automation investments more expensive for the affected firms. Standard economic theory suggests that increasing the cost of capital goods, such as robots, reduces the quantity of investment. However, it is not necessarily obvious that the impact on robot investments will be large, given that firms may have already committed to a technological trajectory that involves automation. On the other hand, embedded in a broader policy shift, the reform likely influenced firms' expectations of future economic policy. Gallemore et al. (2025) provide evidence of particularly strong investment responses to reforms of this kind.

The impact of the tax credit reduction might differ depending on firm characteristics. Larger, financially unconstrained firms might continue to invest despite higher costs, while financially constrained firms may be unable to do so and reduce investment more sharply.

Therefore, we formulate the following hypothesis:

Hypothesis 1 *The reduction in the automation tax credit in South Korea led to a decrease in investments in robots. Financially constrained firms reduce investment by more than financially unconstrained firms.*

[13]Fixed assets in South Korea are depreciated over four to twelve years. For industrial robots, the usual period is five years. We assume ten years here and, hence, underestimate the absolute benefits of linear depreciation. See EY (2024).

The second question concerns how firms adjust more broadly to the increased cost of automation. If robot investments become less attractive due to the reduced tax credit, firms may respond by compensating through other means, such as hiring more employees. The direction of this response is not obvious, and previous empirical evidence has found mixed results.

On the one hand, robots may enhance overall productivity and be complementary to labor and other inputs (Autor, 2015; Graetz and Michaels, 2018). In this case, reducing automation may force firms to scale back production overall, and we would expect no increase in employment or other types of investment. On the other hand, robots may act as substitutes for workers (Acemoglu and Restrepo, 2020; Frey and Osborne, 2017). Then, reducing robot investment could lead firms to hire more employees to maintain production.

The ability to make such adjustments likely depends on financial constraints. Firms with sufficient internal or external funds may flexibly reallocate resources when robot investment becomes less attractive: for example, by hiring more workers or investing in alternative technologies. In contrast, with the lower tax credit, financially constrained firms face both higher costs of automation and reduced internal liquidity, which they cannot easily replace with external finance. Prior work shows that financially constrained firms respond more strongly to tax incentives (Zwick and Mahon, 2017) and are less likely to innovate and slower to adopt new technologies (Hall and Lerner, 2010; Brown et al., 2012). These findings suggest that financial capacity not only determines the magnitude of investment responses but also whether firms can substitute away from robots toward other inputs.

We, therefore, formulate the following hypothesis:

Hypothesis 2 *Following the reduction in the automation tax credit, financially unconstrained firms compensate the reduced use of robots by hiring more employees, whereas financially constrained firms do not.*

The final question concerns the *quality* of automation investments. In a setting without taxes or externalities, firms should invest efficiently, allocating capital to those investment projects with the highest marginal returns. However, investment tax credits lower the user cost of capital, potentially encouraging firms to overinvest in subsidized assets, including industrial robots, even when their marginal productivity is low. Such distortions may lead firms to adopt automation technologies beyond the economically efficient point. Removing the tax credit restores market-based investment signals, potentially prompting firms to allocate capital more efficiently.

We expect this reallocation effect to be strongest for financially unconstrained firms, who are able to adjust their input mix and reoptimize after the reform. In contrast, constrained firms may be unable to substitute or reallocate capital, and may even loose productivity if the lost credit makes them even more financially constrained. As a result, the net impact of the reform on investment quality is likely to vary with financial constraints.

We form the following hypothesis:

Hypothesis 3 *The reduction in the automation tax credit increased the quality of investments among financially unconstrained firms, but not among financially constrained firms.*

3 Data and Descriptives

3.1 Robot Data

The first database we use is provided by the International Federation of Robotics (IFR). It contains information on new installations and the operational stock[14] of industrial robots by country and industry[15] since 1993 for about 80 countries worldwide. The data availability differs across countries. All major industrial robot suppliers report information on new installations to the IFR (primary source). This primary source is supplemented with data submitted by national robotics associations, which also report to the IFR (secondary source). As a result, the data covers almost the whole population of robot installations.

Figure 1 shows IFR data on newly installed industrial robots in Korea, Singapore, Japan, and Germany—the four countries with the highest robot density over the period 1993 to 2021. In South Korea, we observe a pronounced decrease in newly installed robots starting in 2017 when the tax reform was announced. In the other countries, the number of newly installed robots stays roughly constant. Especially in Japan, the volatility of robot installations is high.

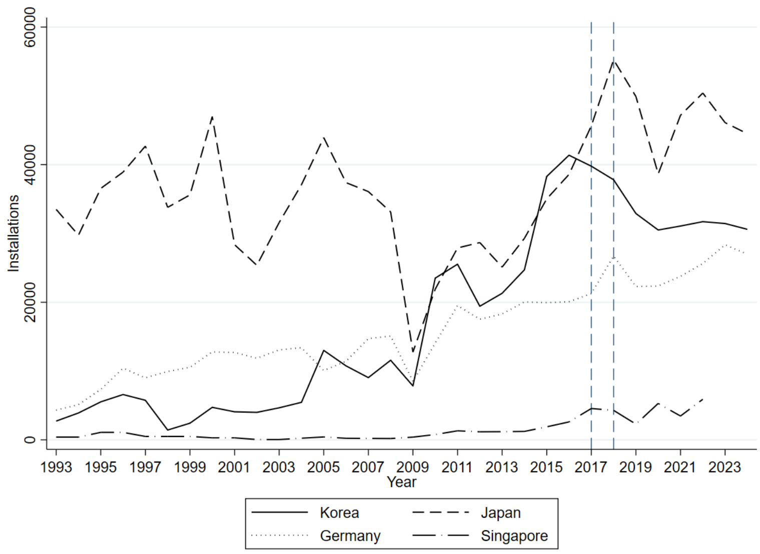
3.2 Orbis

The second database we use is the *Orbis* database provided by Moody's. This database contains firm-level balance sheet data as well as stakeholder information for more than 45 million private and listed entities worldwide. For our analysis, we create a balanced sample and use only unconsolidated financial statement information for entities in South Korea

[14]The operational stock, according to the IFR, measures the number of robots currently deployed.

[15]In addition to agriculture, forestry, fishing (A-B), mining and quarrying (C) and manufacturing (D) the industry classes included electricity, gas, water supply (E), construction (F) and education, research (P). The industry class 'manufacturing' is additionally subdivided into smaller sub-areas. In total, there are 31 different industry classifications.

Figure 1: Robot Installations in Leading Robot Countries



Notes: The graph shows newly installed industrial robots for Korea and three other leading countries over a time period from 1993-2021. Installations in absolute numbers. Vertical lines indicate the announcement of the tax reform (2017) and the implementation (2018). Data from the IFR.

from 2014 to 2021. We also remove entities that report negative total assets or negative employee numbers. We further drop observations with inconclusive information on ownership structure.[16] We drop entities from the following industries: "Mining and Quarrying", "Agriculture, Forestry, Fishing", "Electricity, Gas, Water Supply", "Construction", and "Education, Research and Development". Finally, we remove observations if EBIT exceeds 300 billion Korean Won (roughly 230 million USD), since firms with a tax base above this threshold experienced a change in the corporate income tax rate in 2018.

As described in Section 2, a firm is treated by the Korean tax reform if it is not classified as a small firm.[17] In line with the Korean tax code, we classify an entity as small if it does not exceed the industry-specific turnover thresholds, and it does not exceed the size threshold for total assets of 500 billion Korean Won (about 380 million USD). Additionally, an entity has to be independent to be classified as small. We classify an entity as independent if its global ultimate owner (GUO) is Korean and does not exceed the firm size thresholds.[18]

[16]More specifically, we drop firms where we have information on the global ultimate owner (GUO) of an entity, but the entity is classified as "Independent Co" or "Single location". We also drop observations that are classified as "Controlled subsidiaries" but for which we have no information on the GUO.

[17]We drop all entities that change their firm size status during our observation period.

[18]We do not have information on the size of non-Korean global GUOs. Thus, for simplicity, we assume a Korean entity not to be a small firm if its GUO is a non-Korean company. The underlying assumption is that foreign ownership is an indicator for a larger corporate group that exceeds the Korean small firm threshold.

Table 2 shows the summary statistics for all considered variables for our baseline firm-level regression analysis based on a matched sample presented in Section 4.

TABLE 2: Descriptive Statistics

	N	**Mean**	**1%**	**50%**	**99%**	**SD**
Gross Investment$_{it}$	20 288	0.06	-0.23	0.00	1.02	0.36
Net Investment$_{it}$	20 288	0.04	-0.26	-0.00	0.97	0.35
Employees$_{it}$	16 233	175	3	70	1 469	528
$StaffExpenses_{it}$ (million KRW)	20 236	3 341	80	1 517	23 162	5 012
$Turnover_{it}$ (million KRW)	20 273	76 606	1 032	22 761	736 894	161 703
$Turnover/Assets_{it}$	20 273	1.36	0.05	1.09	5.74	1.15
$GrossProfit_{it}$ (million KRW)	20 276	13 230	-1 350	4 447	131 507	26 688
Total Assets$_{it}$ (million KRW)	20 288	70 053	1 133	23 611	658 781	130 702
Age_{it}	20 288	20.24	4.00	19.00	37.00	9.01
HPI_{it}	20 288	-2.60	-4.29	-2.65	-0.40	0.86
$Cash_{it}$ (million KRW)	20 267	6 770	2	1 300	79 705	24 592
RD Expenses Intensity$_{jt}$	20 288	0.15	0.00	0.22	0.40	0.12
$Installations_{jt}$ (absolute)	20 288	913	0	75	16 783	2 904

Notes: This table presents summary statistics of our firm-level data for our matched sample. See Table A2 in the appendix for variable definitions. The average exchange rate in our sample period is about 1,300 Korean Won (KRW) per U.S. dollar. Yearly data from 2014 to 2021. Data from Orbis, the International Federation of Robotics, and the OECD.

4 Empirical Strategy

4.1 Industry- and Country-Level Analyses

The only available direct data on the use of industrial robots is provided by the industry-level statistics data from the IFR. Accordingly, we begin our empirical analysis of the reform's impact on automation investment at the industry level. Here, we exploit both within-country variation in exposure to the reform, adopting an intensity-to-treat design, and cross-country variation in a DiD design.

Within-country setting. For the within-country design, we use *Orbis* data to calculate the share of Korean firms in each industry that were directly affected by the tax credit reduction ($Share\ Treated_j$) prior to the reform.

Our within-country regressions are specified as follows:

$$\text{Installations}_{jt} = \alpha_1(Share\ Treated_j \times \text{Post}_t) + \eta_j + \zeta_t + \epsilon_{jt}, \tag{1}$$

where the dependent variable $Installations_{jt}$ is the number of robot installations in industry j in year t. We estimate two specifications of Equation (1). In the first, we use a Poisson pseudo-maximum likelihood (PPML), in the second, we estimate an OLS regression.[19]

In all specifications, our primary variable of interest is the interaction between *Share Treated*$_j$ and the post-reform indicator $Post_t$, which equals one for years 2018 and onward, and zero otherwise. This interaction term captures differential changes in robot installations across industries with varying treatment intensities following the implementation of the tax credit reduction. Under Hypothesis 1, we expect a negative estimate for α_1, indicating that industries with higher treatment intensity experienced a relative decline in robot installations after the reform.

We operationalize treatment intensity in two ways. First, we use the continuous variable $Share\ Treated_j^{cont}$, which measures the share of treated firms in each industry. Treated firms are those not classified as small and therefore affected by the reduction in the automation tax credit. Second, we use the binary indicator $Share\ Treated_j^{bin}$, which equals one if industry j is in the top two quintiles of the $Share\ Treated_j^{cont}$ distribution and zero if it is in the bottom two quintiles. Observations in the middle quintile are excluded.

All specifications include industry fixed effects (η_j) and year fixed effects (ζ_t), and standard errors are clustered at the industry level.

Cross-country setting. For the cross-country setting, we use Japanese industries as the control-group in our DiD framework. We estimate the following equation:

$$\text{Installations}_{jct} = \beta_1(Korea_c \times \text{Post}_t) + \gamma_{jt} + \epsilon_{jct}, \tag{2}$$

where Korea is a binary indicator that equals one if the industry-year observation relates to firms from South Korea. All other variables are defined as before. We include industry-year fixed effects and cluster at the country-industry level in this specification. We chose Japan as the control country as both countries share a similarly high robot density (see Figure 1) and have similar GDP per capita, employment and unemployment rates.[20]

4.2 Firm-level Analyses

To ensure comparability between treated and control firms, all firm-level regressions are estimated on a matched sample. We implement Coarsened Exact Matching (CEM) to

[19] In robustness tests, we also estimate a zero-inflated Poisson, or estimate OLS using the dependent variable *Installations Scaled*$_{jt}$, defined as the share of robot installations in industry j relative to the total number of installations in year t (see Section 5.3).

[20] In 2018, GDP per capita was 42,142 USD in Japan and 43,044 USD in South Korea; unemployment rate was 2.5% in Japan and 3.8& in South Korea. Data from the World Bank.

construct a balanced panel of comparable treated and untreated observations. Firms are matched based on their pre-reform average levels of total assets, staff expenses, and investment in fixed assets adjusted for depreciation. In addition, we match on firm age and require exact matches on industry affiliation and legal form.[21] By coarsening continuous variables into data-driven intervals and performing exact matches on categorical attributes, CEM produces strata in which treated and control firms are directly comparable across all observed pre-treatment characteristics.[22]

DiD Setting. We employ a DiD design on this matched sample to identify the impact of the reduced tax credit for automation investments.[23] We examine three sets of outcomes: investment (Hypothesis 1), employment (Hypothesis 2), and investment quality (Hypothesis 3). The design compares treated and control firms in Korea before and after the reform. Formally, we estimate:

$$Outcome_{it} = \delta_1(Treatment_i \times Post_t) + \gamma X_{jt} + \eta_i + \zeta_t + \epsilon_{it}, \tag{3}$$

where the coefficient δ_1 captures the treatment effect of the reform. The variable $Treatment_i$ is an indicator equal to one if firm i is affected by the tax credit reduction, i.e., if a firm does not qualify as a small firm prior to the reform.[24] The post-reform indicator $Post_t$ equals one for all years from 2018 onward, and zero otherwise. The interaction $Treatment_i \times Post_t$ is our main variable of interest. As a control variable X_{jt}, we include the industry-level intensity of R&D expenditures, obtained from OECD data. This accounts for contemporaneous changes affecting innovation and capital investment. All regressions include firm fixed effects η_i to absorb time-invariant firm characteristics and year fixed effects ζ_t to control for macroeconomic shocks. Standard errors are clustered at the firm level.

We use several outcome variables to test our three hypothesis.

To test Hypothesis 1, we would ideally use firm-level data on automation investments. As no such data are available, we proxy automation investment by the change in tangible

[21] While firm size is one criterion for small-firm classification, treatment status in practice often hinges on firm independence (i.e., whether a firm belongs to a business group). As a result, matching on total assets does not mechanically determine treatment and remains appropriate for balancing treated and control firms on pre-reform characteristics.

[22] We use Sturges' rule to determine binning intervals and construct strata with equal numbers of treated and control observations.

[23] A regression discontinuity design (RDD) is not feasible in our setting for two reasons. First, treatment status is determined by a combination of characteristics (firm size and group affiliation), rather than a single sharp cutoff. Second, there is insufficient density of observations around the relevant size thresholds to credibly estimate local effects.

[24] See Section 2 for the official definition of small firms under Korean law.

and intangible fixed assets. Intangible assets, such as software, can be bundled with robots, and form part of broader digitalization and automation strategies.[25] Accordingly, our dependent variable $Outcome_{it}$ is defined either as the change in tangible and intangible fixed assets plus depreciation, scaled by lagged total assets ($Gross\ Investment_{it}$); or as the change in tangible and intangible fixed assets excluding depreciation, also scaled by lagged total assets ($Net\ Investment_{it}$). Table A2 in the appendix provides detailed variable definitions.

To examine Hypothesis 2, we focus on two employment measures. The natural logarithm of the number of employees ($Ln(Employees)_{it}$) captures adjustments in headcount, while staff expenses ($Ln(Staff\ Expenses)_{it}$) reflect changes in both the number of employees as well as their typical wages. Using both variables allows us to get some insight into the wage structure of newly hired employees.

For Hypothesis 3, we consider six measures of investment quality: the logarithms of turnover, gross profit, and EBIT. These outcomes capture both the scale and efficiency of investment. Turnover provides information on firms' sales growth, while EBIT and gross profit proxy for operating performance. Scaling by assets accounts for firm size, allowing us to measure whether investments translate into higher productivity relative to resources employed.[26] These choices follow Eichfelder et al. (2023), who also study turnover and profit-based measures.[27]

IV analysis. While the DiD design identifies the direct effect of the reform, it does not establish whether subsequent changes in investment quality are indeed caused by changes in robot investment. To address this, we complement our baseline analysis with an instrumental variables (IV) strategy. This approach allows us to isolate the impact of reform-induced changes in investment on firm outcomes and to examine the time structure of these effects.[28]

Specifically, we use the reform as an exogenous instrument for investment. In the first

[25] See Brynjolfsson et al. (2021) for evidence on the complementarity between automation and intangible capital.

[26] An alternative outcome measure would be total factor productivity (TFP). We do not use it here because the tax reform directly affects both assets and employment, which are themselves inputs in the standard estimation of TFP. Any observed change in TFP would thus partly reflect the mechanical effect of altered factor inputs rather than a genuine change in underlying productivity, making it difficult to interpret TFP (the residual of an estimated production function) as a separate outcome.

[27] Eichfelder et al. (2023) also examine leads. This is not feasible in our DiD setting, but we address leads later in the IV analysis.

[28] We view labor adjustments as part of the investment transmission channel rather than a separate pathway. Firms respond to the reform by reallocating resources between automation and labor. Thus, changes in employment are a direct consequence of altered investment decisions, not an independent source of variation affecting turnover or profits.

stage, we estimate the effect of the reform on investment,

$$\textit{Gross Investment}_{it} = \theta_1 \textit{Treatment On}_{it} + \eta_i + \zeta_t + \epsilon_{it}, \tag{4}$$

where *Gross Investment*$_{it}$ is defined as the change in tangible and intangible fixed assets plus depreciation, scaled by lagged total assets. *Treatment On*$_{it}$ equals one for treated firms in the post-reform period. The specification includes firm fixed effects η_i, year fixed effects ζ_t, and clusters standard errors at the firm level.

In the second stage, we use the fitted values $\widehat{\textit{Gross Investment}}_{it}$ to estimate the impact of reform-induced changes in investment on firm outcomes,

$$\textit{Outcome}_{it} = \phi_1 \widehat{\textit{Gross Investment}}_{it} + \eta_i + \zeta_t + \nu_{it}. \tag{5}$$

We use the same outcome variables as above to measure investment quality. To capture dynamics, we estimate effects for the contemporaneous year as well as one- and two-year leads.

The IV analysis thus complements our baseline DiD estimates by linking changes in firm outcomes directly to investment responses. We now turn to our results.

5 Empirical Results

5.1 Investment Effects

We begin our empirical analysis by examining how the reduction in the automation investment tax credit affected robot installations by Korean firms (Hypothesis 1).

Industry-level results: Within-Korea setting. We begin by testing whether industries in Korea with greater exposure to the 2018 reduction in the automation tax credit experienced larger declines in robot adoption. Exposure is measured at the industry level using the pre-reform share of firms that do not qualify as small firms ("treated" firms). We interact this exposure with a post-reform indicator for years 2018–2021 and estimate two sets of specifications: (i) Poisson pseudo–maximum likelihood (PPML) regressions of the count of new robot installations with industry and year fixed effects; and (ii) OLS regressions in levels with the same fixed effects. To gauge sensitivity to functional form and to emphasize treatment contrast, we implement two measures of exposure: a continuous share (Columns (1) and (3) of Table 3) and a binary indicator that equals one for industries in the top two quintiles of the exposure distribution and zero for those in the bottom two quintiles (Columns (2) and (4)).

Across all four specifications in Table 3, the interaction term is negative and statistically significant, indicating that more exposed industries curtailed robot installations more strongly after the reform. The magnitudes are economically meaningful. In the OLS specification with the continuous exposure measure (Column (3)), a one–percentage-point higher pre-reform treated-firm share is associated with 33 fewer robot installations in the post period, which corresponds to roughly 4.2% of the pre-reform sample average. Using the binary specification, the PPML estimate in Column (2) implies that, conditional on fixed effects, post-reform installations in high-exposure industries are about $1 - \exp(-1.447) \approx 76\%$ lower than in low-exposure industries.

Thus, industries with a larger pre-reform share of firms affected by the credit reduction cut robot adoption more sharply after 2018. While the within-country design has the advantage of ruling out confounding factors from cross-country differences, its interpretation relies fully on differences in the intensity of treatment. To complement this evidence, we next turn to a DiD framework that compares Korean industries to their Japanese counterparts.

Industry-level results: Korea vs. Japan We next exploit a cross-country DiD design that compares robot installations in Korean industries to those in Japan, a country with similar levels of robot density and economic development (see eq. (1)). Treatment is defined at the country level, with Korean industries exposed to the reform while Japanese industries serve as the control group.

Results are reported in Columns (5) and (6) of Table 3. Across both PPML and OLS specifications, the interaction term $Korea_c \times Post_t$ is negative, indicating that robot adoption in Korea fell relative to Japan after the reform. The coefficient is statistically significant in the PPML specification (Column 5) and marginally insignificant in the OLS specification (Column 6). The PPML estimate implies that Korean industries installed about 30.2% fewer robots per year than their Japanese counterparts in the post-reform period. The OLS coefficient points in the same direction, corresponding to a decline of roughly 547 installations per industry-year, although the coefficient is imprecisely estimated.

These magnitudes are economically large and broadly consistent with prior evidence on investment responses to changes in bonus depreciation and other tax incentives. For example, Zwick and Mahon (2017) estimate that a 20 percentage point increase in bonus depreciation raised investment in eligible assets by between 10% and 17%. Similarly, Eichfelder et al. (2023) find that eliminating a 40% bonus depreciation led to a 17% to 20% decline in investment.

The event study plot in Figure 2 lends support to the validity of the design. It shows

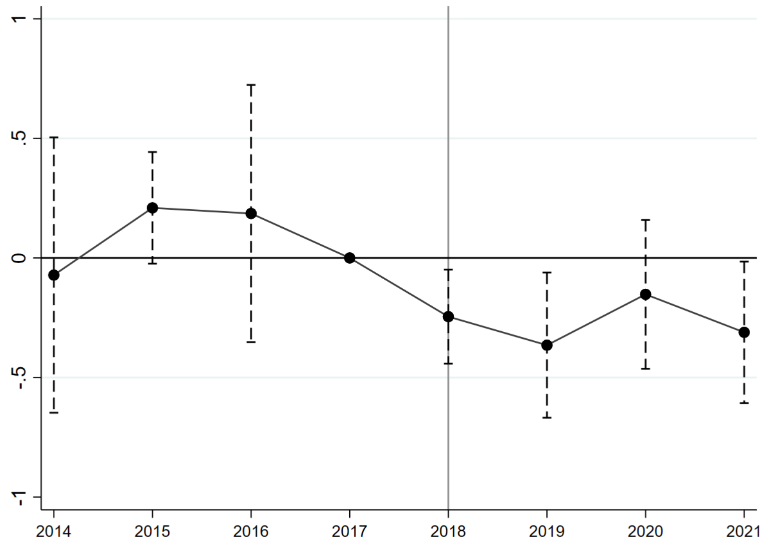
TABLE 3: Industry-Level Results: Robot Installations

	Within Korea				Korea vs. Japan	
	PPML		OLS		PPML	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Share Treated$_j^{cont}$*$Post_t$	-0.075**		-33.018*			
	(0.031)		(18.026)			
Share Treated$_j^{bin}$*$Post_t$		-1.447**		-1118.280**		
		(0.620)		(516.700)		
$Korea_c$*$Post_t$					-0.360**	-547.323
					(0.166)	(344.100)
Industry FE	Yes	Yes	Yes	Yes	No	No
Year FE	Yes	Yes	Yes	Yes	No	No
Year-Industry FE	No	No	No	No	Yes	Yes
Observations	224	176	248	200	338	384
Pseudo R^2	0.81	0.76			0.86	
Adjusted R^2			0.64	0.43		0.68

Notes: Estimated effects of the 2018 reform on industry-level investment. Columns (1)-(4) apply a within-Korea setting, Columns (5)-(6) apply a cross-country setting and compare Korea with Japan. The dependent variable is *Installations*$_{cjt}$, the absolute number of newly installed robots in country c (for Columns (5) and (6)) in industry j in year t. *Share Treated*$_j^{cont}$ measures the average pre-reform share of treated firms in each industry. *Share Treated*$_j^{bin}$ is a binary indicator that equals one if industry j is in the top two quintiles of the *Share Treated*$_j^{cont}$ distribution and zero if it is in the bottom two quintiles. $Korea_c$ is a binary indicator that equals one for Korean industry-year observation and zero for Japanese industry-year observations. $Post_t$ is an indicator variable equal to one after the implementation of the reform, thus after December 31, 2017. Columns (1)-(2) and (5)-(6) apply a PPML approach, Columns (3)-(4) apply an OLS approach. Specifications differ in their fixed effects as indicated. Yearly data from 2014 to 2021. Standard errors are given in parentheses and are clustered at the industry level. ***, ** and * label statistical significance at 1%, 5% and 10% level, respectively. Yearly data from 2014 to 2021 from the International Federation of Robotics and Orbis database of Moody's.

parallel pre-trends in robot installations between Korean and Japanese industries before 2018, and a persistent decline in Korea following the reform. This pattern reinforces the interpretation that the observed cross-country differences capture the causal effect of the quasi-robot tax.

FIGURE 2: Industry-Level Results: Event Study Korea vs. Japan



Notes: Graph shows coefficient estimates from an event study specification which compares robot installations between Korea (treated) and Japan (control). Coefficients are from regressions that replace the post indicator with year dummies, capturing annual differences between installations in Korea and Japan. Bars indicate 90% confidence intervals with standard errors clustered at the country-industry level. Yearly data from 2014 to 2021 from the International Federation of Robotics.

Industry-level results: Robustness We conduct three robustness checks to assess the stability of our industry-level results in the within-country setting. First, to address the large number of observations with zero installation, we re-estimate the PPML specifications using a zero-inflated Poisson (ZIP) model. Second, we implement OLS regressions where the dependent variable is the share of robot installations in industry j relative to total installations in year t ($Installations\ Scaled_{jt}$), thereby normalizing outcomes across industries and years. Third, we augment our baseline models with additional industry-level controls constructed from firm-level *Orbis* data to account for potential compositional changes in firm characteristics. The results, reported in Table A3 in the Appendix, consistently show negative and statistically significant effects of the reform on robot installations in exposed Korean industries.

Firm-level results. We complement the industry-level analysis with firm-level regressions based on data from *Orbis*. As the dataset does not contain robot-specific investment information, we use broader measures of investment as proxies. Following the DiD strategy outlined in Section 4, we regress investment on the interaction of $Treatment_i$ with a post-reform indicator. The dependent variable is either gross investment—defined as the change in tangible and intangible assets plus depreciation, scaled by lagged total assets—or net investment, which is constructed analogously but net of depreciation.

Table 4 reports the results. Columns (1)–(3) use gross investment as the dependent variable, while Columns (4)–(6) use net investment. Specifications gradually increase in stringency: Columns (1) and (4) include only year fixed effects; Columns (2) and (5) add firm fixed effects and an industry-level control for R&D intensity; Columns (3) and (6) include industry-by-year fixed effects. Across all six specifications, the estimated treatment effect is negative and highly significant. Coefficients are very similar across specifications (between -0.026 and -0.028), implying that treated firms reduced investment relative to total assets by about 3 percentage points relative to the control group. In economic terms, this corresponds to roughly eight percent of the standard deviation of the investment rate in the sample.

Table 4: Firm-Level Results: Investment

	Gross Investment			*Net Investment*		
	(1)	(2)	(3)	(4)	(5)	(6)
$Treatment_i * Post_t$	-0.028***	-0.028***	-0.028***	-0.026***	-0.026***	-0.026***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Control	No	Yes	No	No	Yes	No
Company FE	No	Yes	Yes	No	No	Yes
Year FE	Yes	Yes	No	Yes	Yes	No
Industry-Year FE	No	No	Yes	No	No	Yes
Observations	20,288	20,288	20,288	20,288	20,288	20,288
Adjusted R^2	0.004	0.087	0.092	0.003	0.054	0.058

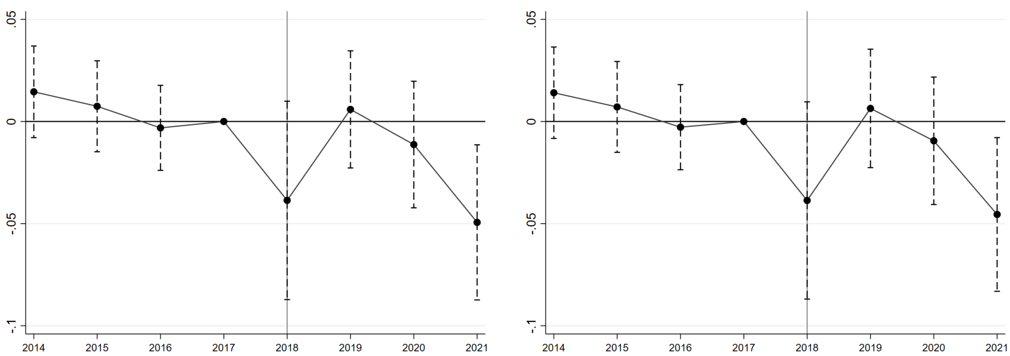
Notes: Estimated effects of the 2018 reform on firm-level investment in Korea. The dependent variable in Columns (1)–(3) is gross investment (change in tangible and intangible fixed assets plus depreciation, scaled by lagged total assets). Columns (4)–(6) use net investment (change in tangible and intangible fixed assets net of depreciation, scaled by lagged total assets). $Treatment_i$ indicates firms affected by the reduced tax credit. $Post_t$ is an indicator variable equal to one after the implementation of the reform, thus after December 31, 2017. Columns (2), (3), (5), and (6) include industry-level R&D intensity as a control. Specifications differ in their fixed effects as indicated. Standard errors are given in parentheses and are clustered at the firm level. ***, ** and * label statistical significance at 1%, 5% and 10% level, respectively. Yearly data from 2014 to 2021 from Orbis and the OECD.

Moreover, the estimates suggest that lower robot investment was not fully offset by substitution into other forms of capital. In untabulated robustness checks that separate tangible and intangible assets, we confirm that the negative investment effect is driven by tangible fixed assets, with no significant change in intangibles.

Figure 3 shows the corresponding event study estimates. Both for gross and net investment, treated and control firms follow largely parallel pre-trends up to 2017, supporting the validity of the design. In the reform year 2018, investment by treated firms drops sharply relative to the control group, consistent with the baseline regression results. In subsequent years, the coefficients fluctuate: the gap narrows in 2019 and 2020, but then again becomes larger and also statistically significant in 2021. This pattern points to an immediate investment response to the reform, followed by varying adjustments taking place across firms. This may suggest the presence of heterogeneous effects, an issue we investigate more systematically in the next section by splitting firms into two subgroups according to the financial constraints they face.

FIGURE 3: Firm-Level Results: Event Studies for Investment

(A) *Gross Investment* (B) *Net Investment*



Notes: Event study estimates of the reform effect on firm-level investment. Panel (A) shows gross investment (tangible and intangible fixed assets plus depreciation scaled by lagged total assets); Panel (B) shows net investment (tangible and intangible fixed assets net of depreciation scaled by lagged total assets). Coefficients are from regressions that replace the post indicator with year dummies, capturing annual differences between treated and control firms. Bars indicate 95% confidence intervals with standard errors clustered at the firm level. Yearly data from 2014 to 2021 from Orbis and the OECD.

5.2 Heterogeneity and Adjustment Mechanisms

We next investigate whether firms' responses to the quasi-robot tax vary with their degree of financial constraints, and whether such heterogeneity gives rise to adjustment along other margins such as employment and the quality of investment. The premise is that a firm's financial position shapes its ability to adjust investment behavior when faced with a less favorable tax treatment of automation.

To capture financial constraints, we stratify firms using the Hadlock–Pierce Index (Hadlock and Pierce, 2010). The index is a parsimonious measure based solely on firm size and age: larger and older firms are classified as less constrained, while smaller and younger firms are classified as more constrained. In practice, the index is constructed as a linear combination of the logarithm of firm assets and firm age (with a squared assets term), where higher values indicate greater financial constraints. Because it does not rely on financial statement variables that may themselves respond to investment choices, the index is less prone to endogeneity concerns than alternative proxies. We split the sample at the median value of the index to distinguish financially constrained from unconstrained firms.

We test two remaining hypotheses not already addressed in the previous section. First, financially unconstrained firms should be able to maintain overall investment levels by substituting away from automation toward other forms of capital, potentially including more labor input (Hypothesis 2). Second, such substitution may be associated with improvements in investment quality (Hypothesis 3). By contrast, financially constrained firms are expected to have limited scope for adjustment and to reduce investment overall.

We test these hypotheses in three steps: we first compare the investment effects across constrained and unconstrained firms, then analyze substitution into employment, and finally assess whether the reform altered the quality composition of investment.

Heterogeneity by financial constraints We begin the heterogeneity analysis by re-estimating the firm-level investment regressions separately for financially constrained and unconstrained firms. Results are shown in Table A4 in the appendix (DiD estimates) and Figure 4 (event study plots). The patterns align closely with our hypotheses. Among financially constrained firms (above-median Hadlock–Pierce Index), treatment effects are negative and statistically significant, ranging from -5.0% to -5.5% of total assets. These estimates are about twice as large as the average effect in the full sample, suggesting that the reduction in the tax credit tightened existing liquidity constraints and forced these firms to cut overall investment.

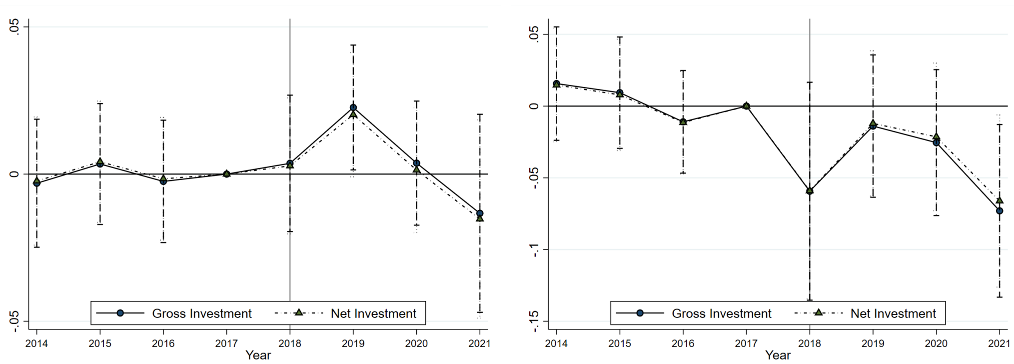
For financially unconstrained firms, by contrast, we estimate an insignificant (and even slightly positive) treatment effect. This indicates that these firms were able to reallocate resources: rather than reducing overall investment, they appear to have substituted away from automation toward other forms of capital spending.

The event study plots in Figure 4 corroborate this interpretation. Pre-reform trends are parallel in both subsamples. After the reform, however, investment paths diverge: financially constrained firms display a sustained contraction in investment, while unconstrained firms maintain or even expand their investment activity. This expansion may partly reflect a temporary reallocation of resources and production processes, requiring new or adapted machinery despite the overall reduction in automation incentives.

Employment effects. We next examine whether reduced automation incentives translated into changes in labor demand. The regressions follow the same firm-level DiD framework as in the investment analysis, with the dependent variable defined either as the natural logarithm of the number of employees or the logarithm of staff expenses. Table 5

Figure 4: Firm-Level Results: Financial Constraints & Investment

(A) *Low HPI* (B) *High HPI*



Notes: Event study estimates of the reform effect on firm-level investment for a sample split between financially unconstrained and financially constrained firms. Panel (A) compares net investment (change in tangible and intangible assets net of depreciation, scaled by lagged total assets) and gross investment between treated and control firms among financially unconstrained firms (low HPI). Panel (B) presents the same for financially constrained firms (high HPI). Financial constraints are measured using the Hadlock-Pierce Index (HPI); the sample is split at the median. Bars indicate 95% confidence intervals. Yearly data from 2014 to 2021 from Orbis and the OECD.

reports results for the full sample and separately for financially constrained and unconstrained firms.[29]

For the full sample, we find no significant change in employment. Among financially constrained firms, employment likewise remains unaffected. By contrast, unconstrained firms increase headcount significantly after the reform: Column (2) suggests an average rise of about 4% relative to control firms. The corresponding effect on staff expenses, however, is smaller and statistically insignificant. This divergence between headcount and staff expenses indicates that unconstrained firms primarily expanded employment at the lower end of the wage or skill distribution. The results are consistent with Hypothesis 2, which posits that financially unconstrained firms substitute away from automation and toward more labor-intensive production when incentives to invest in robots weaken.

Event study plots in Appendix Figure A1 support this interpretation. Employment trends in the pre-reform period are parallel across treatment and control firms in both subsamples. After 2018, however, trajectories diverge: employment remains flat among financially constrained firms but increases steadily among unconstrained firms, with statistically significant differences emerging from 2019 onward. These results underscore that labor substitution effects in response to the reform are concentrated among firms that did not face financial constraints.

[29]The number of observations is smaller for the employment outcomes because employment information is not available for all firms in the full sample used for the investment outcomes.

Table 5: Firm-Level Results: Employment

	$Ln(Employees)$			$Ln(Staff\ Expenses)$		
	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI
	(1)	(2)	(3)	(4)	(5)	(6)
$Treatment_i$*$Post_t$	0.020	0.043**	0.019	-0.003	0.013	-0.004
	(0.017)	(0.022)	(0.028)	(0.016)	(0.019)	(0.029)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Company FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,076	9,472	6,604	16,076	9,472	6,604
Adjusted R^2	0.95	0.95	0.93	0.94	0.94	0.91

Notes: Estimated effects of the 2018 reform on firm-level employment in Korea. The dependent variable in Columns (1)–(3) is the natural logarithm of the number of employees; Columns (4)–(6) use the logarithm of staff expenses. $Treatment_i$ equals one for firms affected by the reduced robot tax credit. $Post_t$ is an indicator variable equal to one after the implementation of the reform, thus after December 31, 2017. Columns (2) and (5) report results for financially unconstrained firms (low HPI) and Columns (3) and (6) for constrained firms (high HPI), where firms are classified based on the Hadlock–Pierce Index (HPI) of financial constraints and split at the sample median. All specifications include industry-level R&D intensity as a control as well as firm and year fixed effects. Standard errors are given in parentheses and are clustered at the firm level. ***, ** and * label statistical significance at 1%, 5% and 10% level, respectively. Yearly data from 2014 to 2021 from Orbis and the OECD.

Investment quality. Lastly, we examine whether the reform affected not only the quantity but also the quality of firm-level investment. One concern is that the automation tax credit may have induced overinvestment in robots, possibly beyond firms' privately optimal level. By reducing this distortion, the reform could have shifted resources toward more productive uses, thereby improving subsequent firm performance. We expect this mechanism to be particularly relevant for financially unconstrained firms, which are better able to reallocate investment across capital categories (Hypothesis 3).

Table 6 summarizes the results for different measures of investment quality. We measure investment quality with different measures of firm performance, i.e., turnover, EBIT, and gross profit. The estimated effects display strong heterogeneity. Among financially constrained firms, we find no statistically significant effect of reform-induced investment changes on any performance measure. By contrast, financially unconstrained firms show robust positive responses: turnover, EBIT, and gross profit all increase significantly in the years following the reform.

Table 6: Firm-Level Results: Investment Quality

	$Ln(Turnover)$			$Ln(Gross\ Profit)$			$Ln(EBIT)$		
	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Treatment_i * Post_t$	0.011	0.056**	0.013	-0.002	0.056*	-0.013	0.060*	0.171***	0.032
	(0.018)	(0.026)	(0.025)	(0.020)	(0.031)	(0.029)	(0.035)	(0.059)	(0.051)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Company FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,273	9,699	10,574	19,861	9,456	10,405	17,001	8,029	8,972
Adjusted R^2	0.94	0.93	0.90	0.89	0.86	0.83	0.78	0.72	0.63

Notes: Estimated effects of the 2018 reform on firm-level investment quality in Korea for the following measures of performance: $ln(Turnover)$, *ln(Gross Profit)*, $ln(EBIT)$. $Treatment_i$ equals one for firms affected by the reduced robot tax credit. $Post_t$ is an indicator variable equal to one after the implementation of the reform, thus after December 31, 2017. Columns labelled "Low HPI" refer to financially unconstrained firms, and "High HPI" to constrained firms, based on the Hadlock-Pierce Index split at the sample median. All specifications include industry-level R&D intensity as a control as well as firm and year fixed effects. Standard errors are given in parentheses and are clustered at the firm level. ***, ** and * label statistical significance at 1%, 5% and 10% level, respectively. Yearly data from 2014 to 2021 from Orbis and the OECD.

As a complementary exercise, we implement an instrumental variables strategy to isolate the causal effect of reform-induced investment changes on subsequent firm performance. The instrument exploits the treatment assignment directly, ensuring that we capture only variation in outcomes driven by changes in investment. This approach has two advantages: it allows us to trace effects dynamically over the two years following the reform, and it provides a cleaner interpretation of investment as the underlying transmission channel.

The IV results, reported in Appendix Table A5, reinforce the heterogeneity documented above. For financially constrained firms, reform-induced reductions in investment have no discernible impact on turnover or profitability in either the reform year or the subsequent two years. For unconstrained firms, by contrast, most of the estimates show large and statistically significant positive effects on all outcomes. Turnover and gross profit increase immediately in the reform year, and the gains persist into years $t+1$ and $t+2$, though becoming gradually smaller and statistically weaker. EBIT appears to increase only in years $t+1$ and $t+2$.

In sum, the reform triggered very different responses across firms. Financially constrained firms cut back on investment, and we find no evidence that these losses were offset by additional employees. Unconstrained firms, by contrast, restructured their spending: they scaled back automation but compensated with other forms of investment, hired additional—likely lower-wage—workers, and achieved improvements in investment quality. The results show that financial flexibility was decisive for how firms adjusted to the quasi-robot tax.

5.3 Robustness Tests

In this section, we assess the robustness of our main firm-level regression results. For each robustness check, we re-estimate the investment regressions for the full sample, as well as separately for financially constrained and unconstrained firms. In addition, we report the corresponding employment and investment quality regressions for the two subsamples to verify whether the observed heterogeneity in second-order effects remains consistent.

Our first robustness check uses a consistent sample for investment and employment outcomes across all specifications, rather than varying the sample depending on the availability of the respective dependent variables. This consistent sample comprises around 80% of the full investment-analysis sample. As shown in Table A6, most of our baseline findings remain intact. In particular, we continue to find a negative investment effect only among more financially constrained firms, while the positive employment effect continues to be concentrated among the less constrained firms. Moreover, the positive second-order effects on turnover growth among less constrained firms become even more pronounced in this restricted sample.

Our second robustness test addresses the potential confounding effects of the COVID-19 pandemic, which overlaps with the post-reform period. We are relatively confident that our results are not substantially biased by this coincidence, as our identification strategy is mostly based on within-country variation across Korean firms. Thus, issues would only arise if treated and non-treated firms responded differentially to the pandemic. In addition, the macroeconomic impact of COVID-19 in South Korea was relatively modest compared to other advanced economies. In contrast to many European countries or the U.S., South Korea did not impose a nationwide lockdown in 2020. Instead, the government relied on aggressive testing, contact tracing, and quarantine only for confirmed cases. Thus, manufacturing plants and offices generally remained open, though many firms adopted remote work or staggered shifts voluntarily. As a result, real GDP declined by just 0.7 percent in 2020.

Nevertheless, we formally test the sensitivity of our results by excluding the year 2020—the peak of the pandemic—from the sample. The results of this exercise, reported in Table A7, confirm the stability of our main conclusions.

Our third robustness check uses an alternative proxy to measure financial constraints. Instead of the Hadlock-Pierce Index, we use the logarithm of cash to proxy financial constraints. We present the results in Table A8, confirming the negative effects on investment as well as the positive effect of employment and investment quality for financially unconstrained firms.

Our final robustness check restricts the sample to firms that were, on average, profitable during the pre-reform period. This restriction mitigates concerns that loss-making firms may have responded differently to the reform due to limited tax liabilities, which could distort the effective value of the automation tax credit. The results, shown in Table A9, are fully consistent with our baseline findings. The investment response to the reform remains negative for financially constrained firms, while unconstrained firms continue to exhibit offsetting positive effects on employment and investment quality.

6 Conclusion

Our study analyses the impact of the reduction of a long-standing "productivity improvement" tax credit on the investment and employment behavior of South Korean firms. We argue that the reduction in this tax credit, which largely encouraged investment in industrial robots, can be interpreted as a quasi-robot tax. In 2018, Korea decreased the tax credit for large and medium-sized firms, but not for small firms.

Exploiting this natural experiment, we first show that reducing tax incentives for automation leads to a decline in both robot investment and broader production activities. Industries with a higher share of affected firms experienced a sharper drop in robot installations, and treated firms reduced their investment in fixed assets by about three percentage points relative to unaffected firms. Second, we find that financially unconstrained firms responded to the reduced incentive for automation by hiring additional employees, while financially constrained firms did not. This substitution toward labor suggests that robots and workers are, on average, substitutes in production. Third, our evidence indicates that scaling back the tax credit improved investment efficiency among financially unconstrained firms. These firms reallocated resources toward more productive uses, leading to gains in turnover, profitability, and gross profit, whereas financially constrained firms cut investment without such offsetting benefits.

Thus, a policy that was designed to encourage innovation may have led to unproductive capital allocation. Our analysis is, however, based on the South Korean context, which is characterized by exceptionally high robot density and long-standing automation policies. The extent of overinvestment we document may therefore be specific to this environment. In economies where automation is less pervasive, diminishing returns to robot investment may be weaker, and the effects of comparable tax reforms could differ.

Our study directly informs the ongoing global debate on the feasibility and implications of robot taxation. By providing empirical evidence on the economic effects of a robot tax-like policy, we bridge the gap between theoretical discussions and real-world outcomes. Our results highlight the inherent trade-offs: while robot taxes may generate fiscal revenue and, under some circumstances, reduce inequality, they also discourage investment in robots and distort firms' capital allocation decisions.

In the South Korean setting, we find that reducing automation incentives improved investment quality, suggesting that very generous, narrowly targeted tax credits can lead to inefficient overinvestment. From a policy perspective, this implies that broad-based, technology-neutral tax systems are less distortionary than selective incentives or taxes focused on specific technologies such as robots. If automation were to cause large-scale displacement of workers in the future, a robot tax might become a relevant instrument for redistribution. At present, however, we do not observe such widespread labor market disruptions and thus little need for a robot tax.

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Appendix

Table A1: Eligible Goods for the Tax Credit for Productivity Enhancement

Category	Scope of Application
1. Facilities for automated production and control of automated production	A. Computers for designing and manufacturing products and computers, peripheral devices (limited to workstations for computer-aided design (CAD) and computer-aided manufacturing (CAM), auxiliary storage, printers, plotters, workstations, modems, terminals, interfaces, and constant voltage power supply systems) and software for the management of business information to receive orders, deliveries, sales, etc. B. Work process control machines or systems for automatic control of manufacturing facilities and parts of such machines or systems: • Facilities operated with programmable logic controllers (PLC) and numerical control systems (NC or CNC) • Robot controllers and unit devices related to computer-integrated manufacturing systems (CIM) • Hydropneumatic valves, hydraulic valves, air compressors, and hydropneumatic actuators C. Machines or facilities equipped with microprocessors or numerical control systems automatically controlling the main work process or function of such machines or facilities. D. Manufacturing or processing systems assembled with two or more machines automatically controlled (including FMC, FMS, and Transfer Line). E. Automatic warehouse systems and loaders/unloaders for safekeeping, storing, and releasing raw materials, parts, and finished products. F. Distributed control systems, integrated information display systems, uninterrupted power supply systems, control valves, signal transmitters, and peripheral devices. G. Automatic control systems, systems for synthesis chemical reaction, and peripheral equipment.
2. Processing facilities and facilities for quality improvement	A. Facilities for the processing of semiconductors and facilities for the formation of high-purity silicon materials, including facilities for cutting and etching wafers, facilities for shaping circuits, and equipment for packing and assembling chips.
	Continued on next page

Table A1 – continued from previous page

Category	Scope of Application
	B. Wire elongating machines, wire stranding machines, wire winding machines, taping machines equipped with controllers; equipment for removing electrodes and magnetic poles; equipment for air-conditioning, including vacuum, clean, and explosion-proof equipment and equipment for hermetic sealing; equipment for coating, evaporation, and filming; equipment for exposing, developing, etching, and trimming; and equipment for surface-washing, grinding, chamfering, and laminating. C. High-Speed spinning equipment with a spin speed of at least 6,000 meters per minute and manufacturing equipment for high-strength, high-functional fibers. (...)

Table A2: Description of Variables

Variable	**Definition**	**Source**
Age_{it}	Years since incorporation.	Orbis
$Cash_{it}$	Cash.	Orbis
$Cashflow_{it}$	Cashflow.	Orbis
$Employees_{it}$	Number of employees.	Orbis
Gross Investment$_{it}$	Change in fixed assets compared to previous year adjusted for depreciations relative to total assets in the previous year. Gross Investment$_{it}$ = (Fixed Assets − l.Fixed Assets + *Depreciations*)/l.Total Assets.	Orbis
Gross Profit$_{it}$	Gross Profit.	Orbis
HPI$_{it}$	Hadlock & Pierce Index according to Hadlock and Pierce (2010): $HPI = -0.737 * Size + 0.043 * Size^2 - 0.040 * Age$. In line with Hadlock and Pierce (2010), $Size$ is the logarithm of total assets in million USD. *Age* is capped at 37.	Orbis
$Installations_{jt}$	Number of new robot installations in industry j in year t.	IFR
Installations Scaled$_{jt}$	Share of robot installations in industry j relative to the total number of installations in year t.	IFR
$Korea_c$	Binary treatment indicator. 1 for Korean observations, 0 for Japanese observations.	Orbis
Net Investment$_{it}$	Change in fixed assets compared to previous year relative to total assets in the previous year. Net Investment$_{it}$ = (Fixed Assets − l.Fixed Assets)/l.Total Assets.	Orbis
$Post_t$	Binary treatment indicator. 1 for post-reform observations (2018-2021), 0 for pre-reform observations (2014-2017).	Orbis
RD Expenses Intensity$_{jt}$	R&D expenses per industry divided by R&D expenses in all industries.	OECD
Share Treated$_j^{cont}$	Percentage share of treated firms in industry j prior to the reform.	Orbis
Share Treated$_j^{bin}$	Binary treatment indicator based on the industry's position in the distribution of average robot adoption intensity before the reform. 1 if the industry's pre-treatment mean share of treated firms is in the 4th or 5th quintile; 0 if it is in the 1st or 2nd quintile.	Orbis

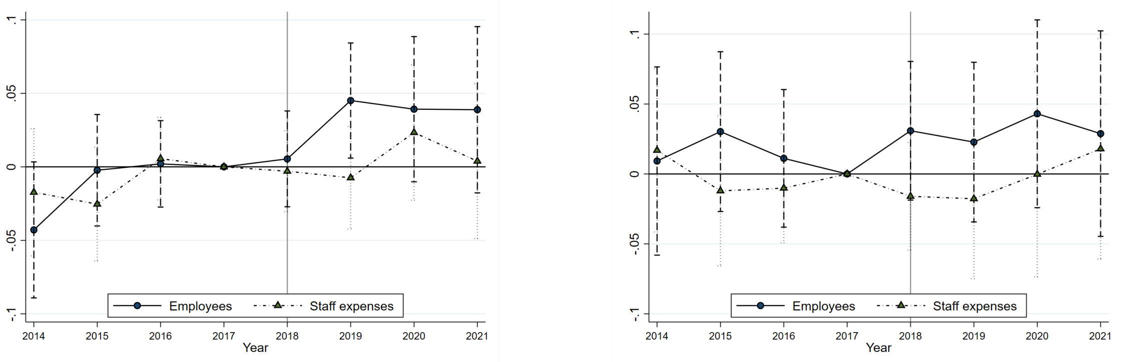
Variable	**Definition**	**Source**
Staff Expenses$_{it}$	Staff expenses / costs of employment.	Orbis
Total Assets$_{it}$	Total assets.	Orbis
$Treatment_i$	Indicator variable equal to one if firm i is affected by the tax credit reduction, i.e., if a firm does not qualify as a small firm prior to the reform. 0 otherwise.	Orbis
Treatment On	Binary indicator. 1 for treated observations in the post-reform period. 0 otherwise.	Orbis
Turnover$_{it}$	Turnover / Sales.	Orbis

Table A3: Industry-Level Results: Robustness

	ZIP		Share		PPML	
	(1)	(2)	(3)	(4)	(5)	(6)
Share Treated$_j^{cont}$*$Post_t$	-0.053*		-0.001*		-0.087**	
	(0.030)		(0.001)		(0.034)	
Share Treated$_j^{bin}$*$Post_t$		-0.777*		-0.032**		-1.515**
		(0.431)		(0.015)		(0.661)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	Yes	Yes
Observations	248	200	248	200	224	176
Pseudo R^2					0.82	0.77
Adjusted R^2			0.64	0.42		

Notes: Robustness tests for the estimated effects of the 2018 reform on industry-level investment. The dependent variable in Columns (1)-(2) and (5)-(6) is *Installations*$_{jt}$, the absolute number of newly installed robots in industry j in year t. The dependent variable in Columns (3)-(4) is *Installations Scaled*$_{jt}$ the number of newly installed robots in industry j in year t divided by the number of installed robots across all industries in year t. *Share Treated*$_j^{cont}$ measures the average pre-reform share of treated firms in each industry. *Share Treated*$_j^{bin}$ is a binary indicator that equals one if industry j is in the top two quintiles of the *Share Treated*$_j^{cont}$ distribution and zero if it is in the bottom two quintiles. $Post_t$ is an indicator variable equal to one after the implementation of the reform, thus after December 31, 2017. Columns (1)-(2) apply a zero-inflated Poisson approach, Columns (3)-(4) apply an OLS approach with the share of robot installations being the dependent variable, Columns (5)-(6) apply a PPML approach with additional control variable which vary at an industry-year level (we include total assets, turnover growth and the number of employees scaled by total assets at an industry-year level). All Columns include industry and year fixed effects. Standard errors are given in parentheses and are clustered at the industry level (for ZIP, we report robust standard errors as clustering is not possible.). ***, ** and * label statistical significance at 1%, 5% and 10% level, respectively. Yearly data from 2014 to 2021 from the International Federation of Robotics and Orbis database of Moody's.

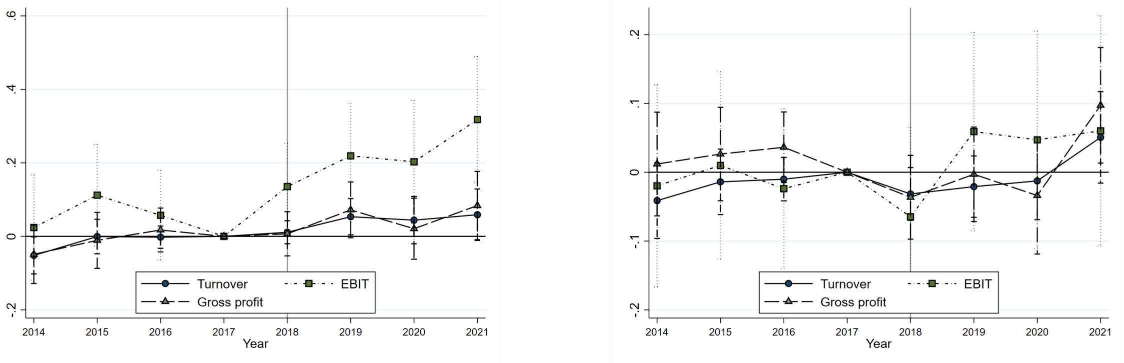
Figure A1: Firm-Level Results: Event Studies for Employment



(A) Low HPI (B) High HPI

Notes: Event study estimates of the reform effect on firm-level employment for a sample split between financially unconstrained and financially constrained firms. The dependent variables are the logarithm of the number of employees and the logarithm of staff expenses. All graphs compare employment outcomes between treated and control firms. Panel (A) focuses on financially unconstrained firms (low HPI), Panel (B) focuses on financially constrained firms (high HPI). Financial constraints are measured using the Hadlock-Pierce Index; the sample is split at the median. Bars indicate 95% confidence intervals. Yearly data from 2014 to 2021 from Orbis and the OECD.

Figure A2: Firm-Level Results: Event Studies for Investment Quality



(A) Low HPI (B) High HPI

Notes: Event study estimates of the reform effect on firm-level investment quality measures for a sample split between financially unconstrained and financially constrained firms. The dependent variables are the logarithms of turnover, EBIT, and gross profit. All graphs compare investment quality outcomes between treated and control firms. Panel (A) focuses on financially unconstrained firms (low HPI), Panel (B) focuses on financially constrained firms (high HPI). Financial constraints are measured using the Hadlock-Pierce Index; the sample is split at the median. Bars indicate 95% confidence intervals. Yearly data from 2014 to 2021 from Orbis and the OECD.

Table A4: Firm-Level Results: Investment by Financial Constraints

	Gross Investment		*Net Investment*	
	Low HPI	High HPI	Low HPI	High HPI
	(1)	(2)	(3)	(4)
$Treatment_i$*$Post_t$	0.003	-0.055***	0.001	-0.050***
	(0.008)	(0.016)	(0.008)	(0.015)
Controls	Yes	Yes	Yes	Yes
Company FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	9,704	10,584	9,704	10,584
Adjusted R^2	0.10	0.05	0.06	0.02

Notes: Estimated effects of the 2018 reform on firm-level investment in Korea for a sample split. The dependent variable in Columns (1)–(2) is gross investment (change in tangible and intangible fixed assets plus depreciation, scaled by lagged total assets). Columns (3)–(4) use net investment (change in tangible and intangible fixed assets net of depreciation, scaled by lagged total assets). $Treatment_i$ indicates firms affected by the reduced tax credit.$Post_t$ is an indicator variable equal to one after the implementation of the reform, thus after December 31, 2017. Columns (1) and (3) report results for financially unconstrained firms (low HPI) and Columns (2) and (4) for constrained firms (high HPI), where firms are classified based on the Hadlock–Pierce Index (HPI) of financial constraints and split at the sample median. All columns include industry-level R&D intensity as a control and include firm and year fixed effects. Standard errors are given in parentheses and are clustered at the firm level. ***, ** and * label statistical significance at 1%, 5% and 10% level, respectively. Yearly data from 2014 to 2021 from Orbis and the OECD.

Table A5: Firm-Level Results: Investment Quality—IV

Panel A: Financially Unconstrained (Low HPI)

	Invest	*Outcome*$_t$			*Outcome*$_{t+1}$			*Outcome*$_{t+2}$		
		Turnover	Gross Profit	*EBIT*	*Turnover*	Gross Profit	*EBIT*	*Turnover*	Gross Profit	*EBIT*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment_on	0.003									
	(0.007)									
$\widehat{Invest}$		23.656***	21.413**	-626.582***	7.659**	8.436**	27.320***	4.740*	3.002	20.768**
		(0.004)	(0.035)	(0.001)	(0.016)	(0.038)	(0.001)	(0.078)	(0.417)	(0.020)
Observations	11,912	11,907	11,610	9,823	10,419	10,150	8,517	8,930	8,685	7,236
F-Stat	0.19									

Panel B: Financially Constrained (High HPI)

	Invest	*Outcome*$_t$			*Outcome*$_{t+1}$			*Outcome*$_{t+2}$		
		Turnover	Gross Profit	*EBIT*	*Turnover*	Gross Profit	*EBIT*	*Turnover*	Gross Profit	*EBIT*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment_on	-0.049***									
	(0.014)									
$\widehat{Invest}$		-0.444	0.154	-0.530	-0.618	-0.343	-1.060	-0.837	-0.646	-0.878
		(0.516)	(0.561)	(0.844)	(0.568)	(0.664)	(0.939)	(0.552)	(0.691)	(0.968)
Observations	11,912	11,902	11,687	10,049	10,418	10,232	8,754	8,930	8,770	7,459
F-Stat	11.97									

Notes: Estimated effects of the 2018 reform on firm-level investment quality measures in Korea for an IV approach and a sample split between financially unconstrained (low HPI) and financially constrained (high HPI) firms. Column (1) presents the first stage, where the dependent variable is gross investment (change in tangible and intangible fixed assets plus depreciation, scaled by lagged total assets). *Treatment_on* is an indicator variable that equals one for treated observations in the post-reform period. Columns (2)-(10) present estimates of the second stage for different measures of investment quality dynamically over time. The dependent variable in Columns (2), (5), and (8) is the logarithm of turnover, the dependent variable in Columns (3), (6) and (9) is the logarithm of EBIT, and the dependent variable in Columns (4), (7), and (10) is the logarithm of gross profit. Columns (2)-(4) refer to outcome variables in the reform year, Columns (5)-(7) in $t+1$ and Columns (8-10) in $t+2$. Panel A reports results for financially unconstrained firms (low HPI) and Panel B for constrained firms (high HPI), where firms are classified based on the Hadlock–Pierce Index (HPI) of financial constraints and split at the sample median. All columns include firm and year fixed effects. R^2 is not meaningful in instrumental-variables regressions. For Panel A, Anderson–Rubin are given in parentheses as we have a weak instrument (F-Stat: 0.19). For Panel B, standard errors are given in parentheses and are clustered at the firm level. ***, ** and * label statistical significance at 1%, 5% and 10% level, respectively. Yearly data from 2014 to 2021 from Orbis.

Table A6: Firm-Level Results: Robustness Test Consistent Sample

Panel A: Investment & Employment

	Gross Investment			*Ln(Employees)*		
	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI
	(1)	(2)	(3)	(4)	(5)	(6)
$Treatment_i{*}Post_t$	-0.017	0.006	-0.043*	0 0.019	0.043*	0.017
	(0.011)	(0.008)	(0.022)	(0.017)	(0.022)	(0.028)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Company FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,916	9,372	6,544	15,916	9,372	6,544
Adjusted R^2	0.17	0.08	0.16	0.95	0.95	0.93

Panel B: Investment Quality

	Ln(Turnover)			*Ln(Gross Profit)*			*Ln(EBIT)*		
	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Treatment_i{*}Post_t$	0.022	0.058**	0.009	0.020	0.056*	0.001	0.081**	0.179***	-0.001
	(0.019)	(0.025)	(0.030)	(0.023)	(0.030)	(0.037)	(0.041)	(0.058)	(0.064)
Company FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,063	9,467	6,596	15,699	9,230	6,469	13,312	7,841	5,471
Adjusted R^2	0.94	0.93	0.90	0.88	0.86	0.81	0.76	0.72	0.63

Notes: Estimated effects of the 2018 reform on firm-level investment, employment, and investment quality for a consistent sample for investment and employment outcomes. Panel A displays results for investment and employment outcomes, Panel B displays results for investment quality outcomes. For each variable, the first column presents results for the full sample and the following two columns present results for financially unconstrained (low HPI) and financially constrained (high HPI) firms separately. For variable definitions, see Table A2 in the appendix. Firms are classified based on the Hadlock–Pierce Index (HPI) of financial constraints and split at the sample median. All columns include firm and year fixed effects. Standard errors are given in parentheses and are clustered at the firm level. ***, ** and * label statistical significance at 1%, 5% and 10% level, respectively. Yearly data from 2014 to 2021 from Orbis.

Table A7: Firm-Level Results: Robustness Test Without 2020

Panel A: Investment & Employment

	Gross Investment			*Ln(Employees)*		
	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI
	(1)	(2)	(3)	(4)	(5)	(6)
$Treatment_i * Post_t$	-0.032***	0.005	-0.052***	0.016	0.042**	0.010
	(0.011)	(0.008)	(0.018)	(0.016)	(0.020)	(0.026)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Company FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,752	8,491	9,261	14,163	8,305	5,858
Adjusted R^2	0.10	0.07	0.09	0.95	0.95	0.93

Panel B: Investment Quality

	Ln(Turnover)			*Ln(Gross Profit)*			*Ln(EBIT)*		
	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Treatment_i * Post_t$	0.020	0.056**	0.004	0.022	0.060**	0.001	0.071*	0.167***	-0.009
	(0.018)	(0.024)	(0.029)	(0.023)	(0.030)	(0.035)	(0.041)	(0.058)	(0.065)
Company FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,111	8,291	5,820	13,802	8,099	5,703	11,789	6,933	4,856
Adjusted R^2	0.95	0.94	0.90	0.88	0.86	0.81	0.76	0.72	0.63

Notes: Estimated effects of the 2018 reform on firm-level investment, employment, and investment quality for a sample without the year 2020. Panel A displays results for investment and employment outcomes, Panel B displays results for investment quality outcomes. For each variable, the first column presents results for the full sample and the following two columns present results for financially unconstrained (low HPI) and financially constrained (high HPI) firms separately. For variable definitions, see Table A2 in the appendix. Firms are classified based on the Hadlock–Pierce Index (HPI) of financial constraints and split at the sample median. All columns include firm and year fixed effects. Standard errors are given in parentheses and are clustered at the firm level. ***, ** and * label statistical significance at 1%, 5% and 10% level, respectively. Yearly data from 2014 to 2021 from Orbis.

Table A8: Firm-Level Results: Robustness Test with Alternative Fin. Constraints Measure

Panel A: Investment & Employment

	Gross Investment			*Ln(Employees)*		
	Full Sample	High Cash	Low Cash	Full Sample	High Cash	Low Cash
	(1)	(2)	(3)	(4)	(5)	(6)
$Treatment_i$*$Post_t$	-0.028***	0.006	-0.047***	0.020	0.038*	0.003
	(0.009)	(0.008)	(0.015)	(0.017)	(0.021)	(0.030)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Company FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,288	10,200	10,088	16,118	9,494	6,624
Adjusted R^2	0.09	0.11	0.08	0.95	0.95	0.92

Panel B: Investment Quality

	Ln(Turnover)			*Ln(Gross Profit)*			*Ln(EBIT)*		
	Full Sample	High Cash	Low Cash	Full Sample	High Cash	Low Cash	Full Sample	High Cash	Low Cash
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Treatment_i$*$Post_t$	0.011	0.048*	0.004	-0.002	0.051*	-0.024	0.060*	0.157***	0.040
	(0.018)	(0.027)	(0.026)	(0.020)	(0.031)	(0.031)	(0.035)	(0.056)	(0.058)
Company FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,273	10,190	10,083	19,861	9,975	9,886	17,001	8,624	8,377
Adjusted R^2	0.94	0.92	0.93	0.89	0.87	0.83	0.78	0.72	0.67

Notes: Estimated effects of the 2018 reform on firm-level investment, employment, and investment quality using the logarithm of cash as an alternative measure for financial constraints. Panel A displays results for investment and employment outcomes, Panel B displays results for investment quality outcomes. For each variable, the first column presents results for the full sample and the following two columns present results for financially constrained (low Cash) and financially unconstrained (high Cash) firms separately. For variable definitions, see Table A2 in the appendix. Firms are classified based on the logarithm of cash and split at the sample median. All columns include firm and year fixed effects. Standard errors are given in parentheses and are clustered at the firm level. ***, ** and * label statistical significance at 1%, 5% and 10% level, respectively. Yearly data from 2014 to 2021 from Orbis.

Table A9: Firm-Level Results: Robustness Test without Loss-making Firms

Panel A: Investment & Employment

	Gross Investment			*Ln(Employees)*		
	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI
	(1)	(2)	(3)	(4)	(5)	(6)
$Treatment_i * Post_t$	-0.035***	0.006	-0.060***	0.025	0.053**	0.024
	(0.010)	(0.007)	(0.015)	(0.017)	(0.021)	(0.028)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Company FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,856	8,512	9,344	14,097	8,333	5,764
Adjusted R^2	0.08	0.08	0.08	0.95	0.95	0.93

Panel B: Investment Quality

	Ln(Turnover)			*Ln(Gross Profit)*			*Ln(EBIT)*		
	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI	Full Sample	Low HPI	High HPI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Treatment_i * Post_t$	0.008	0.066**	0.004	-0.004	0.079**	-0.029	0.074**	0.227***	0.017
	(0.018)	(0.027)	(0.025)	(0.020)	(0.031)	(0.030)	(0.036)	(0.060)	(0.052)
Company FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,848	8,508	9,340	17,665	8,386	9,279	15,970	7,548	8,422
Adjusted R^2	0.95	0.93	0.91	0.91	0.87	0.85	0.79	0.72	0.65

Notes: Estimated effects of the 2018 reform on firm-level investment, employment, and investment quality for a sample without firms who on average made a loss prior to the reform. Panel A displays results for investment and employment outcomes, Panel B displays results for investment quality outcomes. For each variable, the first column presents results for the full sample and the following two columns present results for financially unconstrained (low HPI) and financially constrained (high HPI) firms separately. For variable definitions, see Table A2 in the appendix. Firms are classified based on the logarithm of HPI and split at the sample median. All columns include firm and year fixed effects. Standard errors are given in parentheses and are clustered at the firm level. ***, ** and * label statistical significance at 1%, 5% and 10% level, respectively. Yearly data from 2014 to 2021 from Orbis.

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