The Welfare Effects of a Robot Tax: Evidence from a Tax Credit for Automation Technologies in Korea*

DongIk Kang† Jung Hyuk Lee‡ Simon Quach§

October 4, 2024

Abstract

This paper studies the labor market impacts and fiscal costs of a tax credit for investments in robots and automation technology. Leveraging administrative tax data along with policy reforms in South Korea that vary the tax credit rate by firm size, we estimate three sets of parameters central to the welfare implications of a robot tax. First, we find that firms reduce investments in automation and increase employment following a reduction in the tax credit rate. Second, the tax reform reduced wage inequality due to slower wage growth in the upper half of the income distribution. Third, the tax credit has a positive fiscal externality, implying that behavioral responses to reductions in the tax credit increased the government's revenue beyond the direct mechanical impact of the policy. Together, the results suggest that a robot tax could be a cost-effective policy for job creation.

*We are grateful to the Korean National Tax Service for supplying us the data for this project. The authors are solely responsible for all errors and views expressed herein.

†Yonsei University. Email: dikang@yonsei.ac.kr

‡Korea Ministry of Economy and Finance. Email: leejunghyuk@korea.kr

§University of Southern California. Email: simonqua@usc.edu

1 Introduction

The number of robots operating in factories around the world has nearly tripled over the last decade (IFR, 2021). The rapid expansion of robots has generated concerns that it may give rise to large employment loss (Brynjolfsson and McAfee, 2014). Consistent with these concerns, many studies have found that robot adoption leads to decreases in employment and wages among low-skilled manufacturing workers (Acemoglu and Restrepo, 2020; Giuntella et al., 2022; Bessen et al., 2023; Bonfiglioli et al., 2024). In response to the labor displacing effects of robots, policymakers have debated introducing a tax on robots and automation (Prodhan, 2017). Economists have likewise began modeling the welfare implications of a robot tax (Thuemmel, 2022; Zhang, 2019; Acemoglu et al., 2020a; Guerreiro et al., 2022; Costinot and Werning, 2022). Despite growing interest in taxing investments in automation, there is little empirical evidence on the labor market impacts or fiscal costs of such a policy.

This paper studies the effects of a robot tax on employment, wages, and tax revenues by exploiting two reforms in South Korea that changed the amount of tax credits available for investments in new technologies. Korea is an interesting setting to study the effects of a robot tax as it is the country with the largest robots per capita in the world (IFR, 2021). In Korea, employers that purchase robots and automation software are eligible to claim a credit on their corporate taxes at the end of the year. The tax credit rate varies by firm size. Prior to 2014, small firms could deduct 7% of their investment costs from their taxes, and medium to large firms could deduct 3%. In 2015, the government increased the tax credit rate for medium firms to 5%, and then in 2018, the government decreased the rate for both medium and large firms to 3% and 1%, respectively. The 2018 tax reform has been called by some media outlets as the "world's first robot tax" (McGoogan, 2017).

To identify the causal impact of the two reforms, we implement a difference-in-difference design that compares manufacturing firms affected by a change in the tax credit rate to unaffected firms. Specifically, our empirical strategy compares employers right above and below the revenue cutoffs that the tax authorities use to define firm size. Our identification strategy relies on the assumption that absent the policy changes, the outcomes of small, medium, and large companies would have evolved at the same rate. We validate our assumption by showing that the outcomes between the two groups were parallel prior to the reform, the labor market effects of the tax changes were largest among firms whose investments were most impacted by the policy, and our estimates are robust to a series of specification checks to control for other possible confounding policies. Our analysis uses administrative tax records (2012-2019) of all manufacturing firms with at least $10 million annual revenue in 2014. The employer-employee panel data contains both firm-level financial information on taxes and revenues, as well as worker-level information on individuals' wages. Since we only observe wages and employment after 2015, we focus on the second reform for our analysis of the labor market impacts and use the first reform to complement our analysis on the effects on investments.

Our natural experiment enables us to estimate three key sets of parameters essential to understanding the welfare implications of robot taxation. First, we measure the impact of a robot tax on investment in automation technology and employment. These elasticities are central to the argument by Acemoglu et al. (2020a) that if the tax system is biased against labor, then a tax on automation can be welfare improving by increasing employment and reducing automation. Second, we estimate the effect of a robot tax on income inequality. A key motivation for a robot tax is that it can redistribute the wage premium of the workers that are complements to automation technology to the workers that are substitutes (Guerreiro et al., 2022; Thuemmel, 2022; Costinot and Werning, 2022). Third, our paper also estimates the fiscal externality of a robot tax on the government's budget. While the first two parameters measure the benefits of a robot tax, the last parameter is central to measuring its deadweight loss. To the best of our knowledge, we are the first paper to quasi-experimentally estimate these parameters in the context of a robot tax.

We report three sets of results. First, we find that firms respond to a reduction in the tax credit rate by cutting investments and increasing employment. After the 2015 reform increased the tax credit rate for medium-sized firms, we show that take-up of the tax credit increased by over 150% and investments in credit-eligible items quintupled. These effects took time to manifest, suggesting that firms make lumpy investment decisions that require years to adjust. As a result, the first two years after the 2018 reduction in tax credits only saw a 33% fall in take-up rate and no statistically significant investment effect. However, we find that smaller firms responded quicker to the reform and reduced investments in credit-eligible capital within two years. During the same period, we observe a 3.8% increase in employment concentrated solely among these firms that cut investments. Our estimates thus support the hypothesis that a robot tax leads to job creation (Acemoglu et al., 2020a).

Second, we show that although the 2018 tax reform reduced inequality, it did so by lowering workers' earnings. On average, we find evidence that workers' wages fell by about 2% following the tax reform. Decomposing the average income effect using a quantile difference-in-difference design, we show that earnings primarily fell in the upper half of the income distribution. Although we cannot reject zero income effects for those in the first four deciles of the income distribution, we find a statistically significant decrease in the labor income of workers in the fifth to seventh deciles. On the other hand, we do not find significant differences in the employment and income effects depending on the worker's age. In dynamic models of optimal robot taxation, it is often assumed that if younger generations anticipate the growth in automation, they would invest in new skills that enable them to be work in jobs that are not replaced by robots (Adão et al., 2020; Cavounidis et al., 2023). In that case, a robot tax would primarily benefit older workers. However, we are unable to find conclusive evidence that employment or income rises more for older workers than younger workers.

Third, we estimate the fiscal externality of the 2018 tax reform and find that firms' behavioral response to the policy increased the government's revenues above the mechanical impact of the tax credit cut. To calculate the fiscal externality, we first simulate the amount by which firms' tax credits would have fallen if their behavior continued in the same trend as the counterfactual control group. We then compare this mechanical effect to our difference-in-difference estimates of the actual change in firms' tax burden. The difference between the empirical and simulated estimates imply that for each dollar that the government mechanically collects from firms, they can expect to receive an additional $7.8 due to employers' behavioral response. We show that this excess tax revenue is driven by an increase in income taxes, since firms hired more workers in response to the robot tax.

The positive fiscal externality suggests that a robot tax is a cost-effective policy for increasing employment. In traditional settings, taxes usually have a negative fiscal externality — for example, the behavioral response to an income tax is to reduce labor supply, leading the government to collect less revenue than they would absent the response.[1] However, in our case, even though firms reduce investments in response to a reduction in the tax credit, we show that the increase in income taxes from greater employment actually exceeds the fiscal cost of lower capital investments. As a result, a robot tax can raise tax revenues while creating new jobs.

Our paper contributes to three strands of literature. First, our study provides one of the first empirical estimates of the effects of a robot tax. While previous papers studying the welfare implications of a robot tax have modelled the key economic trade-offs of a tax on automation (Thuemmel, 2022; Zhang, 2019; Acemoglu et al., 2020a; Guerreiro et al., 2022; Costinot and Werning, 2022), few studies have empirically estimated the magnitude of these forces. Perhaps the closest in spirit to our study is a paper by Hirvonen et al. (2022) that examines the effect of receiving a technology subsidy through a competitive grant program in Finland. Unlike our results, they find that firms that barely won a subsidy increased both investments and employment relative to firms whose applications were barely rejected. Compared to their context, the setting we study more closely reassembles a robot tax in the sense that it applies universally to all automation investments without a selective application process.

Second, we contribute to a broader literature on the effects of technology on employment and wage inequality (Katz and Murphy, 1992; Autor et al., 2003; Akerman et al., 2015; Feigenbaum and Gross, 2020). Papers that have focused specifically on the labor market impacts of robotization tend to use one of two empirical strategies. To examine the aggregate impacts of robotization, studies have used a shift-share approach exploiting variation in the adoption of robots across industries and variation in initial industry composition across local labor markets (Graetz and Michaels, 2018; Acemoglu and Restrepo, 2020; Dauth et al., 2021; Giuntella et al., 2022). Another set of studies identify behavior within firms that purchase robots using an event-study design around time the of robot adoption (Humlum, 2019; Dixon et al., 2021; Koch et al., 2021; Rodrigo et al., 2021; Bessen et al., 2023; Bonfiglioli et al., 2024). Relative to these studies, our estimates have a clearer policy interpretation as we leverage variation from an tax reform to identify the labor market impacts of robots and automation.

[1] See Hendren and Sprung-Keyser (2020) for estimates of the fiscal externality for other government programs and taxes.

Lastly, our paper is related to an extensive literature on firms' responses to capital taxation and investment subsidies. Many recent studies have used quasi-experimental methods to understand how changes in tax incentives for corporate investments affect firms' purchases of fixed assets (Yagan, 2015; Zwick and Mahon, 2017; Ohrn, 2018; Liu and Mao, 2019; Maffini et al., 2019; Garrett et al., 2020; Curtis et al., 2021; Moon, 2022). Generally, studies of capital taxation have focused on tax changes that either apply to all corporate profits or broad classes of fixed assets that include not only machinery but also vehicles, buildings, and furniture. However, the debate around robot taxes focuses on a specific type of technology that is arguably more likely to be a substitute, rather than a complement, to labor. Relative to prior work, the tax regime we study is the most similar to a robot tax envisioned by policymakers in that the tax credit targets new technologies, but omits tradition forms of capital. As a result, we find that taxes in our setting increase employment, whereas previous studies of capital taxation find that opposite result (Garrett et al., 2020; Curtis et al., 2021).

The remainder of this paper is organized as follows. Section 2 summarizes the main argument for a robot tax and describes the key parameters necessary to evaluate its welfare effects. In section 3, we explain the institutional details governing the robot tax credit in South Korea. Section 4 describes the administrative tax data and the characteristics of firms that take up the tax credit. In sections 5 to 7, we report our results for the impact of the tax credit on investments, the labor market, and the government's budget, respectively. Section 8 concludes by discussing the implications of our findings and areas for future research.

2 Theoretical Argument for a Robot Tax

The theoretical literature on robot taxes have identified two main channels for which a tax on automation would be socially optimal. In this section, we describe the intuition behind each of these channels, relate their predictions within a sufficient statistics framework, and identify three key parameters of interest required to understand the welfare effects of a robot tax. The remainder of the paper is then devoted to the estimation of these parameters.[2]

2.1 When is a Robot Tax Socially Optimal?

First, Acemoglu et al. (2020a) show that if the tax system is biased against labor in favor of capital, then an automation tax can improve welfare by raising employment. They explore a Ramsey problem where the social planner sets labor and capital taxes to maximize the utility of a representative agent subject to a balanced government budget and a resource constraint in

[2]We note that, similar to our empirical setting, theoretical models of optimal taxation define a "robot tax" more broadly than just a tax on robots. For example, Acemoglu et al. (2020a) models the tax as targeting any capital that replaces workers for the marginal task. Guerreiro et al. (2022) uses the word robots "to refer to all production inputs that are complements to non-routine workers and substitutes for routine workers". In either case, the models implicitly assume that the government can design a tax that applies to specifically capital that replaces workers, whether on the margin or on average.

market equilibrium. If labor taxes are too high and capital taxes are too low relative to the social optimum, then firms under-employ workers in favor of capital. In this case, Acemoglu et al. (2020a) demonstrate that a tax on automation will be welfare improving because, when taxes are biased against labor, the marginal automated task only provides second-order productivity gains whereas increases in employment has first-order welfare effects. The social planner should then aim to increase employment by reducing automation. Moreover, as a policy tool, an automation tax is especially beneficial because it does not reduce capital intensity uniformly but discourages the automation of marginal tasks only.

A critical prediction for the applicability of this framework is that such a robot tax should (a) deter the adoption of automation technologies and that (b) it should induce an increase in firms' employment levels. In other words, the elasticity of investments (in automation technology) and the elasticity of employment to changes in the robot tax rate is of key interest. Therefore, our first order of business is to estimate the impact of the robot tax on firms' investments in automation technologies and also its impact on the employment level.

Second, multiple papers have shown that a robot tax can increase social welfare by reducing income inequality (Guerreiro et al., 2022; Thuemmel, 2022; Costinot and Werning, 2022). Although Costinot and Werning (2022) adopt a sufficient statistics approach Guerreiro et al. (2022) and Thuemmel (2022) utilize calibrated models, in general, these studies solve a Mirrleesian model whereby the government sets taxes to maximize the weighted-average utility across individuals, subject to a feasibility constraint and a balanced government budget. The added dimension of multiple unobserved types of workers introduces a trade-off between efficiency and equity. By taxing automation, the social planner can reduce the wage premium for workers that complement the technology relative to those workers for whom the technology is a substitute, leading to pre-tax redistribution. However, this redistribution comes at a deadweight loss in efficiency from changes in equilibrium employment, production, wages, and prices.

As such, a key parameter in the second class of models is the distribution of income across workers and how it changes in response to a robot tax. Therefore, we estimate how changes in the robot tax rate affect the income distribution. Our empirical strategy will both estimate the effect of the robot tax on average wages, as well as its impact on different wage quantiles.

Due to limitations in data availability, we are only able to observe the wage distribution for employed workers, while in theory, the welfare consequences of changes in the wage distribution depends on *all* workers in the economy. Although this is unfortunate, we believe that two factors mitigate the impact of this limitation. First, the credibility of quantitative models used to assess the impact of robot taxation depends, in large part, on their ability to reproduce important features of the economy. Furthermore, these quantitative models can simulate the entire wage distribution, both of all workers and of employed workers only. Therefore, having the empirical changes in the distribution of wages of employed workers can serve as a critical benchmark for the calibration and evaluation of these models. Second, from an empirical standpoint, we estimate both the extensive margin impact on employment and the intensive margin impact on wages. Thus, a researcher or policymaker interested in utilizing the sufficient statistics approach to compute the optimal tax rate could supplement our income results with estimates of the extensive margin effect.

Another key parameter determining the viability of a robot tax is the deadweight loss associated with an increase in robot taxation. The relative size of the deadweight loss is critical to understanding the social cost of robot taxation. In sufficient statistic models, the deadweight loss is equivalent to the impact of the tax on the government's budget through changes in individuals' behaviors. Intuitively, since agents are already optimizing, behavioral responses to a tax have no first-order impact on welfare by the envelopment theorem. However, changes in behavior impose a direct deadweight burden on the government's budget, called the fiscal externality. We next describe our procedure for estimating the fiscal externality.

2.2 Nonparameteric Approach

To measure the deadweight loss, we apply the nonparametric approach developed by Lee et al. (2021). Similar to previous papers, we assume that the government has two tax instruments: a robot subsidy T and other taxes B. For concreteness, we will call B income tax to connect with our empirical results. The income tax collected from all workers in a firm, $B(Y, \tau)$ depends on the workers' and firm's actions Y (e.g. employment and wages) and a tax parameter τ. Analogously, the robot subsidy for the representative firm, $T(Y, t)$, also depends on the firm's decisions Y (e.g. investments) and subsidy rate t. The vector Y thus captures all possible actions by firms and workers. In general, actions can further depend on the two tax parameters through indirect general equilibrium effects on wages and prices.

Suppose the government would like to increase the robot subsidy rate t. Then the total effect on the government's budget can be decomposed into two components:

$$\underbrace{\frac{d[B(Y(\tau,t),\tau) - T(Y(\tau,t),t)]}{dt}}_{\text{Total}} = \underbrace{\left(\frac{\partial B}{\partial Y}\frac{\partial Y}{\partial t} - \frac{\partial T}{\partial Y}\frac{\partial Y}{\partial t}\right)}_{\text{Behavioral}} + \underbrace{\frac{\partial T}{\partial t}}_{\text{Mechanical}} \tag{1}$$

Intuitively, the mechanical component reflects the change in the government's budget if all agents' behavior remain constant. This would be equivalent to a lump-sum tax without deadweight loss. On the other hand, the behavioral component captures the impact of the tax change on the government's budget through its effect on firms' and workers' behavior. It is the behavioral component that leads to inefficiencies. To make the behavioral effect comparable to other programs in the public finance literature, we define the fiscal externality as the ratio of the behavioral and mechanical effect:

$$\frac{\text{Behavioral Effect}}{\text{Mechanical Effect}} = \frac{\frac{\partial B}{\partial Y}\frac{\partial Y}{\partial t} - \frac{\partial T}{\partial Y}\frac{\partial Y}{\partial t}}{\frac{\partial T}{\partial t}} \tag{2}$$

The fiscal externality represents the additional impact on the government budget due to behavioral responses, for each dollar of mechanical transfer.

To relate our fiscal externality parameter to that of previous work, Appendix B translates the sufficient statistics model developed by Costinot and Werning (2022) into the language of Lee et al. (2021). Our nonparametric approach has two advantages relative to previous work. First, when computing the fiscal externality, we do not need to take a stance on the particular actions that affect the government's budget. While Costinot and Werning (2022) model Y as labor supply decision by workers and the purchase of robots by firms, there are other ways in which changes in economic behavior can affect government's budget, such as tax avoidance or noncompliance. Our approach thus mirrors modern approaches in the optimal labor income tax literature where focus has shifted away from the labor supply elasticity to the elasticity of taxable income (Saez et al., 2012). Second, by generalizing the formula for the fiscal externality, it becomes easier to measure empirically. While it is often difficult to observe transactions for the purchase of robots, which is a key parameter in the sufficient statistics model of Costinot and Werning (2022), that is not necessary to compute the fiscal externality. Instead, our model shows that we can directly measure the fiscal externality by estimating the change in firms' tax burden (i.e. sum total of taxes minus subsidies).

Together, our estimates of the investment effect, the employment effect, the income effect, and the fiscal externality are core components for a normative analysis of a robot tax. For a complete welfare statement though, we would also need to know the effect of the automation tax on prices and the social preferences for redistribution. In addition, despite the nonparametric nature of our framework, we do require one key assumption that actions Y are chosen optimally by firms and workers so that any deviations only contributes to the fiscal externality. However, if there are labor market frictions that prevent the optimal allocation of labor, then changes in Y may actually be welfare enhancing, such as employment effects in the context of Acemoglu et al. (2020a). Given these limitations, our goal is not to make a normative claim in the paper, but instead to focus on a positive analysis of the effects of a robot tax that is motivated by the parameters needed for a normative assessment.

3 Institutional Setting

This section describes the institutional background surrounding the "Tax Credit for Investments in Productivity Improving Technologies" (TCIPIT) that our paper analyzes as a scarce real-life example of a robot subsidy/tax. The key features of this institution are that (1) the tax credit is applicable to a restricted set of capital investments that includes robots and automation, but excludes traditional forms of capital like machinery, buildings, and vehicles that have been studied in previous papers, and (2) policy changes in the tax credit rates vary by firm size, enabling our difference-in-difference design around the firm size cutoffs.

3.1 Tax Credit for Investments in Productivity Improving Technologies

The TCIPIT was first introduced in 1993, separately from tax credits for general capital investments. Firms claim the tax credit each year by reporting their annual spending on eligible investments. Examples of investments eligible for the tax credit include automated machinery like robotic arms as well as computers and software used to automate the firm's operations. A general list of eligible items are shown in Appendix Table 1. Although the TCIPIT covers some purchases that do not generally fit with the definition of industrial robots, the tax credit mostly applies to technologies aimed at automating firm processes, and importantly, cannot be applied to traditional forms of capital investments. As such, to the best of our knowledge, the TCIPIT is the closest representation of a robot tax in the real world. In fact, it has been referenced by both media (McGoogan, 2017) and economists (Guerreiro et al., 2022) alike as a policy directed at automation.

From the onset of the institution, the tax credit rates had varied by firm size with the policy intent of supporting small enterprises. Since 2002, the tax credit rates were set at 7% for small firms and 3% for "non-small" firms. An important change was made in 2015 after the Korean National Assembly enacted a special act that introduced the concept of "medium" sized firms into the tax code. To support the expansion of medium sized firms, the tax credit rate of TCIPIT was increased to 5% for medium sized firms in 2015, separate from the 3% for large firms. However, the tax credit rates were later reduced by 2 percentage points for both medium and large firms in 2018. As this was the first reduction in the tax credit rates since the introduction of the TCIPIT, it effectively increased the tax burden imposed on firms investing in automation technology. Table 1 summarizes the changes in the tax credit rates in the 2010's.

Our empirical strategy will compare firms affected by the 2015 and 2018 tax rate changes to firms that were unaffected, by leveraging the fact that the reforms changed the tax rate for only a subset of the firms. Table 2 summarizes the criteria that Korea's National Tax Service uses to define firm size for each sub-industry within manufacturing. Prior to 2015, firms are considered "small" for tax purposes if their employment, capital, revenue, and assets fall below pre-specified thresholds, and they are considered a "general" firm otherwise. Starting in 2015, the tax code eliminated the employment and capital criteria, and set the revenue cutoff as either 80, 100, or 150 billion Korean Won (KRW) depending on each firm's sub-industry.[3] The new tax code also distinguished between medium and large firms based on whether or not they are a part of a conglomerate. Since all the post-treatment periods of our analyses are after 2015, we define firm size using the post-2015 criteria.

We define different treatment and control groups for the 2015 and 2018 tax reforms, respectively. In our analysis of the 2015 reform, we compare medium firms that experienced an increase in the tax credit rate to small and large firms that did not. To identify the effects of the 2018 policy, which lowered the tax credit rate for both medium and large firms, we use small firms as a counterfactual control group. The analysis of the 2018 reform thus compares firms with an average annual revenue above the 80/100/150 billion KRW cutoffs to those below the industry-specific thresholds.[4] In both

[3] To quickly convert currencies, 1000 KRW is a little less than 1 USD during this time period.

[4] To test whether there is self-selection into the treatment or control groups, appendix figure 1 plots the distribution of revenues in 2017. We find no bunching at the thresholds, suggesting that firms are not manipulating their finances to qualify for a higher tax credit rate. In practice, it would be very difficult for firms to manipulate their firm size classification since it is determined by a 3 year running average of revenues.

analyses, we make the treatment and control groups more comparable by restricting the sample to firms with baseline revenues of 10-400 billion KRW per year, thereby eliminating both small mom-and-pop stores and the largest conglomerates in the country.

3.2 Threats to Identification

Throughout our analysis, we assume that the outcomes of firms right above and below the revenue cutoffs would have followed the same trend if not for the changes to the tax credit rates. Although we validate that the parallel trends assumption holds prior to the tax reforms, there are nevertheless two additional empirical challenges to our approach that we address in the paper.

First, the 2015 policy change was part of a broader tax reform that affected multiple tax incentives available to firms. For instance, Moon (2022) uses the 2015 reform to study how a reduction in capital gains taxes affects investments. In addition to increasing the tax credit rate for investing in new technologies, the 2015 policy also reduced capital gains taxes for firms with 100-150 billion KRW annual revenue by reclassifying them from medium to small status. Moon (2022) shows that these reclassified firms ended up increasing investments. Since we define firm size using the post-2015 definition, reclassified firms are contained in our control group and would thus bias against finding any impact on investments among the medium firms that saw an increase in robot tax credits. As a robustness check, we repeat our 2015 analysis after dropping all small firms that would have been affected by this reclassification and find similar results.

Second, besides the tax credit for automation technology, firms also have the option of claiming other sorts of tax credits and can only claim one per year. We find that the other tax credits are second-order in our context. Among manufacturing firms, the TCIPIT is larger than all the other investment-related tax credit programs *combined*. As of 2019, TCIPIT is the largest investment-related tax credit program in Korea in terms of the magnitude claimed (i.e. approximately 430 million USD). Moreover, even if the other tax credit impact firms' decision, the variation we exploit nevertheless changes the relative price of automation faced by firms.

Third, in addition to tax reforms, the year 2018 also coincided with a period of labor market changes. In particular, South Korea increased its minimum wage by 16% in 2018 and instituted a maximum workweek of 52 hours per week. Since the minimum wage applied to all firms, it would not confound our estimates unless its effects vary by firm size. To account for the potential confounder, we show that our results are robust to controlling for share-of-minimum wage workers interacted with time fixed effects and to simply dropping firms with a large share of minimum wage employees. As for the law on maximum work hours, it only applied to companies with at least 300 employees. The majority of firms in our sample are below that threshold and our results are robust to dropping these large firms.

3.3 Interpretation and External Validity

Given the unique features of the TCIPIT, it is reasonable to ask to what extent it resembles a "robot tax", and whether its implementation in Korea informs potential policies in other parts of the world. In regards to the first question, there are two characteristics of the TCIPIT worth considering. First, the TCIPIT is technically a subsidy rather than a tax. However, in standard theories of taxation (Mirrlees, 1971), a positive subsidy is simply a negative tax - both alter the price of investment for firms. In fact, Thuemmel (2022) argues within a Mirrlees optimal taxation model that robot taxes should first be negative (i.e. a subsidy), and then become more positive over time. Thus, the direction of employers' response to a change in subsidy has a theoretical foundation and should be informative about their response to a change in a tax. However, as with any natural experiment, extrapolating outside the support of the policy variation requires additional assumptions about the curvature of employers' response functions, which we will not make in this study.

Second, the tax credit can be applied to some items that are not often thought of as robots or automation technology as conceptualized by economists. However, such broad categorization is expected from any attempt to create a robot tax. From a legal perspective, one of the challenges of designing a robot tax is that it is fundamentally difficult to define a robot (Kovacev, 2020). For instance, without careful language, it would not be obvious that an ATM is legally different from a self-checkout kiosk, even though only the latter is generally viewed as a low productivity labor-displacing device. As such, while the TCIPIT may not fit a stylized version of a robot tax that targets solely labor-displacing technology, any robot tax in practice will likely also encompass a range of investments that may not be viewed as a robot. We therefore consider the TCIPIT the closest approximation to a robot tax in the world, as it focuses specifically on high-tech investments geared toward automation, and excludes traditional forms of capital.

In terms of external validity to other countries, it is worth noting that South Korea has the highest robot usage per capital in the world, with 50% higher robot density than the next leading country and nearly 4 times the density in the United States (IFR, 2021). The years that we study are also during a period of rapid growth in robot adoption. As such, our results are highly relevant for understanding how firms may react to a robot tax in an economy with large robot penetration, which we might expect in other developed countries in the future.

4 Data

Our analysis uses restricted-access administrative micro data from 2011 to 2019, provided by the National Tax Service (NTS) of South Korea. The NTS data is derived from two sources: corporate income tax records and individual income tax records. For security purposes, the NTS is only able to provide a selected sample of the data instead of the full universe of tax filings.

The NTS constructed the sample for us in three steps. First, the full universe of employers is restricted to only firms in the manufacturing industry. Previous analyses of the TCIPIT have shown that manufacturing firms make up 90% of all companies that ever take-up the tax credit (Kim et al., 2019). Given the low take-up of robots in other industries, we follow a common practice in the robots literature and focus solely on the manufacturing sector. Second, the sample is further restricted to firms with revenues of at least 10 billion won in 2014 (approximately $9 million USD in 2014) and are continuously in operation throughout the sample. The restriction in the second step removes small businesses that rarely take-up the tax credit, but still keeps firms that are small enough to be unaffected by the changes to the tax credit rates in 2015 and 2018. Third, for each employer in the firm sample, we merge on the annual income tax record of all employees from 2015 to 2019. Note that the worker level data is only available from 2015 onwards, so our analysis of the wage and employment effects of the robot tax will rely solely on the 2018 policy variation. In our final sample, we have about 7,000 firms and 850,000 workers per year.

The resulting employer-employee dataset contains detailed information on firms' finances and workers' earnings. For each firm, we observe a breakdown of their annual corporate income taxes including their tax base, various tax credits claimed, and tax payable. Moreover, since firms' tax rates are determined by their size, which depends on other financial characteristics of the firm, we also observe each firms' industry, annual revenue, and assets. All values are top-coded at 1 trillion KRW. For each worker, we are able to observe their annual earnings, age, and sex. One limitation of the worker level data is that there is no employee-identifier that can be used to follow the same worker over time due to concerns about privacy. As such, our estimates of the wage effect will measure the impact of the robot tax on workers' annual earnings, without accounting for changes in worker composition. From a normative standpoint though, changes in the composition of workers has no impact on welfare since the social planner cares only about the distribution of wages, and not how wages vary by workers' characteristics. Thus, the repeated cross-sectional data is sufficient for identifying the key parameters that factor into models of optimal taxation.

4.1 Descriptive Statistics

To gauge the economic significance of the robot subsidy, table 3 summarizes key variables from firms' income tax statements for 2015-2019, separately for small, medium, and large firms. The top panel shows that small firms have lower revenue and fewer assets than medium and large firms, which must be true by definition. This is naturally correlated with lower employment, profits, and taxes. However, given that we have restricted the sample to firms with at least 10 billion won revenue, the average small firm is still significantly larger than the majority of companies in Korea. A second point to note from the top panel is that medium and large firms are surprisingly similar along multiple dimensions. The reason for the similarity is that medium and large firms are distinguished predominantly by whether or not they are part of a major conglomerate, and is not directly dependent on their financial accounts. Despite differences in baseline characteristics, our empirical strategy is valid as long as small, medium, and large firms exhibit similar trends, which we can test in our difference-in-difference analysis.

In the bottom panel, we examine the take-up of the TCIPIT. In a previous arrangement with the Korean tax office, we analyzed a random sample of manufacturing firms and found that only about 1% of firms claim the credit in any given year. However, while few companies make use of the tax credit, it may nevertheless have a significant impact on the labor market as large employers are the most common claimants of the subsidy. For instance, Acemoglu et al. (2020b) finds that while only about 1% of manufacturing firms purchased robots between 2010-2015 in France, they account for 20% of all manufacturing workers in the country. In the bottom panel of table 3, we see that take-up increases with firm size. The share of firms that claim the tax credit at least once rises from 5% of small firms to nearly a quarter of all large firms as we move up the firm size distribution. In fact, the right three columns suggest that firms that claim the tax credit at least once between 2015 and 2019 employ about 11% of our sample.

In addition to potentially affecting many workers, the tax credit also has a sizeable impact on firms' budgets. Even unconditional on take-up, the tax credit is equivalent to 1% of small firms' tax payable and 1.5% for large firms in our sample. Among firms that take up the TCIPIT, that value rises to 15% and 5.2% respectively. Overall, the descriptive evidence suggests that changes to the robot tax heavily impacts the taxes owed by a small subset of firms that employ a large share of the manufacturing labor force.

5 Effect on Firms' Taxes and Investments

5.1 Empirical Strategy

In this section, we present our results on the effect of the 2015 and 2018 tax reforms on firms' take-up, assets, revenue, and profits. To identify the causal impact of these policies, we estimate the following regression:

$$Y_{it} = \sum_{\substack{t=T_0 \\ t\neq -1}}^{T_1} \beta_t D_{it} + \alpha_i + \alpha_t + \varepsilon_{it} \tag{3}$$

where Y_{it} is an outcome variable for firm i in event time t; D_{it} is a dummy that equals 1 in event year t for firms affected by the change in tax credit rates; and α_i and α_t are firm and year fixed effects. The β_t coefficients represents the difference between impacted and unaffected firms, relative to the year before the tax reform. Standard errors are clustered at the firm level.

While the dynamic coefficients allow us to test the parallel trends assumption, we also estimate the following simpler regression to aggregate our results:

$$Y_{it} = \beta(Treat_i * Post_t) + \alpha_i + \alpha_t + \varepsilon_{it} \tag{4}$$

where $Treat_i$ and $Post_i$ are dummies for the treatment group and post-tax reform years respectively.

Our empirical strategy assumes that absent the policy change, outcomes in small, medium, and large firms would have evolved at the same rate. We validate our empirical strategy by showing that treated and control firms had similar trends across the many outcome variables prior to each tax reform. Our analysis parallels the empirical strategy used by Banerjee and Duflo (2014) to examine the expansion and retraction of a program in India that targeted large firms but not small ones. Similar to their study, we estimate the effect of the two reforms separately. For the 2015 reform, we use the 2011-2017 data and define $Treat_i$ as a dummy indicator for medium sized firms. For the 2018 reform, we use the 2015-2019 data and define $Treat_i$ as a dummy indicator for both medium and large firms. To make small, medium, and large firms comparable, we restrict the sample to firms with annual revenue between 10-400 billion KRW. In both analyses, firm sizes are defined based on their characteristics in the year prior to the reform.

5.2 Take-up and Investments

To begin our examination of employers' response to the tax reforms, figure 1 plots the share of firms that take-up the tax credit each year, separately by firm size. Panel A shows that while take-up of the tax credit has been increasing over time across all three types of firms, this growth was quicker among medium and large firms than small firms. To account for differences in pre-trends, panel B multiplicatively scales the take-up rates so that average take-up rates equal one in 2014 for all three groups. The rescaling allows us to compare trends in terms of percent changes, rather than level changes. The figure shows that prior to 2014, take-up was trending at similar rates across firm sizes, but then after the tax credit rate increased for medium firms in 2015, their take-up rate grew quicker relative to the other firm sizes. Then in 2018 when tax credit rates fell for both medium and large firms, they did not experience the explosive rise in take-up that small firms had. Given the similar pre-trends after the simple rescaling, we similarly scale take-up rates, claimed tax credits, and investments for the remainder of this analysis.

Figure 2 plots the effect of the 2015 and 2018 tax reforms on take-up of the tax credit over time, estimated from equation 3. Panel (a) shows that in response to the increase in tax credits in 2015; medium sized firms gradually increased their take-up of the TCIPIT relative to large firms that did not experience a change to the tax credit rate. Similarly, panel (b) shows that after the reduction in tax credits for medium and large firms in 2018, they became 50% less likely to claim the tax credit relative to small firms. Panel (c) and (d) shows that changes to the probability in take-up translates to analogous changes in the total amount of tax credits claimed. In all cases, we observe no differences in pre-trends prior to the tax reforms, consistent with the identifying assumption that absent the policy changes, outcomes would have evolved at the same rate aross firm sizes. After the reforms though, we find that outcomes gradually move in the direction predicted by standard economic theory - higher tax credit rates induces greater take-up. The gradual response to the tax reform suggests that firms pre-commit to investments and take time to adjust their future capital stock.

The effect of the reforms on the amount of tax credits claimed is at least double its effect on the probability of take-up. This difference can be due to both a real change in investments and a mechanical effect from the change in the tax rate. The fact that take-up of the tax credit changed after the policies imply that there is at least an extensive margin investment response. However, that alone does not quantify the amount by which firms adjusted investments. To place a monetary value on the investment response, we infer investments in TCIPIT-eligible capital through the following accounting identity:

$$\text{Tax credit amount}_{it} = \text{Tax credit rate}_t * \text{Investment}_{it}$$

Since we do not directly observe investments, we instead define each firm's investment in a year as the ratio of the amount of tax credit they claimed and the tax credit rate.

Panels (e) and (f) of figure 2 plots the estimates of equation 3 showing the impact of the two tax reforms on capital investments eligible for the TCIPIT. The estimates suggest that at least part of the change in tax credit claims is driven by an investment response. However, the change in investments seem to lag the change in total tax credits claimed. Panel (e) shows that medium firms greatly increased investments 3 years after the increase in the tax credit rate in 2015. Similarly, panel (f) finds that medium and large firms reduced their investments a year after the reduction in the tax rate in 2018, albeit the estimate is fairly noisy. The lagged responses gives further evidence that firms require time to adjust investments. The new tax credit rates are announced only in the summer of the year before they go into effect, so firms only have half a year to respond before each reform. To summarize, in the short run, investments appear relatively rigid and the change in credits claimed is driven considerably by a pure mechanical change in the subsidy rate. However, in the long-run, firms make significant adjustments to investments in response to the tax reforms.

5.3 Robustness

Before proceeding to the labor market impacts of the robot tax credit, we first assess the robustness of our results on firms' tax credits and investments. Table 4 estimates the effect of the tax reforms on take-up, amount of tax credit claimed, and investments from variants of equation 4. Column 1 reports the estimates of equation 4 using our main sample. Consistent with the earlier figures, take-up increases with the tax credit rate. Since the increase in take-up is gradual and our data ends in 2019, we find a larger aggregated impact for the 2015 reform relative to the 2018 one. Our estimates suggest that the 2015 reform increased take-up by 168% and quintupled investments in credit-eligible capital. In comparison, take-up only increased by 33% in the 2 years following the 2018 reform, and there were no statistically significant investment effects.

Columns (2) to (6) assess the robustness of our results to various controls and sample restrictions. In column 2, we find similar results after introducing industry-year fixed effects. This stricter specification compares only treated and control firms within the same industry over time, thereby allowing us to control for industry-specific time trends. In column 3, we restrict the sample to firms with baseline annual revenues between 50 and 200 billion won. The tighter revenue bounds around the small vs. medium firm cutoff gives us more comparable firms, and we find that that the impact on investments after the 2018 reform is an order of magnitude larger for this subsample. Figure 3 plots the equivalent dynamic treatment effects for firms with baseline revenue of 50-200 billion KRW. We show that the parallel trends hold and there was a statistically significant fall in investments by 2019. The heterogeneity in response by firm size suggests that relatively smaller (i.e. lower revenue) firms responded quicker to the reform and reduced investments.

One potential confounder to our baseline estimates of the effects of the 2018 reform are interactions with contemporaneous changes to the minimum wage and maximum work hours in Korea in 2018. To account for these policy changes, columns 4 to 6 implements a series of stricter specifications. First, column 4 controls for the share of minimum wage workers within each firm in 2017, interacted with year fixed effects. The additional control allows for firms with different share of workers affected by the minimum wage policy to have different time trends. Second, column 5 further drops firms with over 20% of their workforce affected by the minimum wage. Third, column 6 drops all firms with over 300 employees at baseline to restrict the set of employers to only those not covered by the new maximum workweek law. In all instances, the estimated effects on take-up, claimed tax credits, and investment remain stable. The only restriction that affects the estimates are the revenue bounds by which we define our sample.[5]

Together, the stability of our estimates across a variety of specifications leads us to conclude that firms increase take-up and claims in response to more generous tax credits for automation technology. The change in take-up directly implies an extensive response in investments, albeit an aggregate investment response is only statistically detectable in the long-run. This investment response also appears to be quicker for firms with relatively lower revenues within the treatment group.

6 Effect on the Labor Market

This section examines the effect of changing the tax credit rate on the labor market. Given that we only have data on firms' employment and workers' earnings starting from 2015, we focus specifically on the 2018 tax reform.

6.1 Employment

Figure 4 plots the estimates of equation 3 using log(employment) as the outcome variable. Since we find that low revenue firms within the treatment group cut their investments more sharply by 2019, we plot our estimates separately for the full sample and for firms with baseline revenues between 50 and 200 billion KRW per year. In both cases, we show there are no differences in pre-trends between small and medium/large firms leading up to the 2018 tax reform. Following the policy change though, we find a gradual increase in employment that is statistically significant for precisely the subsample of firms for which we find significant investment cuts. The overlap between the group of firms that cut their investments and those that raised employment most significantly suggests that the labor market effects is driven by the tax policy and not other confounders.

Table 5 tests the robustness of the employment response to a series of alternative specification that control for potentially confounding policies. Similar to the analysis on take-up, column (2)

[5] In Appendix table 2, we also show that our estimates for the effect of the 2015 reform are robust to the exclusion of small firms to account for contemporaneous changes in the way firm sizes are defined for these companies.

controls for industry-year fixed effects, column (3) restricts firms to those with baseline revenue between 50-200 billion won, column (4) controls for share-of-minimum-wage workers interacted with year fixed effects, column (5) drops firms with over 20% share of its workforce earning below the 2018 minimum wage, and column (6) drops firms with over 300 employees. Across the range of regressions, Panel A consistently finds no impact on employment in 2018, the first year of the tax reform. This is to be expected since capital takes time to adjust, as indicated from our earlier result on investments. However, we start seeing evidence of an increase in employment in the year after the decrease in tax credits. The positive employment effect is particularly pronounced in columns (3) and (4) when we restrict the sample to only firms with baseline revenues close to the threshold that divides small and medium firms. Again, the increase in employment is largest for precisely the same subsample for which we find the strongest decline in investments. The point estimate suggests that among the most affected firms, employment increased by around 3.8% in response to the 2% decrease in tax credits.

Consistent with common arguments for a robot tax, we find that a reduction in tax credits for automation increases the number of jobs. Our finding of a negative relationship between capital investment and employment stand in contrast to studies of programs in the U.S. (Curtis et al., 2021) and Finland (Hirvonen et al., 2022), which find positive employment effects of government subsidies for capital investments. There are two non-mutually exclusive reasons for this difference. First, relative to studies on capital depreciation allowances in the U.S., the forms of capital investments covered by our tax credit are more narrowly defined and focuses primarily on the types of automation that are thought of to have second-order productivity effects, but first order impacts on employment. Second, while the Finnish case study examined the impacts of winning a competitive grant for investments in automation, we study a tax credit that is available to all firms. As a result, firms affected by our tax reform are not selected based on an application that may potentially be evaluated by its expected labor market impacts. Overall, our results on the positive employment effects of a robot tax are more in line with the findings of papers that specifically examine the labor market impacts of robot adoption (Acemoglu and Restrepo, 2020).

6.2 Workers' Earnings

6.2.1 Average Earnings

The positive employment effect suggests that labor demand increased in response to the decrease in tax credits. In that case, we might expect workers' earnings to also increase. However, if the decrease in capital made workers less productive, it is also possible that the tax reform decreased workers' earnings. To examine the impact of the tax reform on workers' incomes, we begin by estimating equation 3 using individual worker's log earnings as the outcome variable. Appendix figure 2 plots the effect on log income over time but finds that the parallel trends assumption does not hold. Wages in smaller firms are growing at a quicker rate than those at larger firms, even before the changes in tax credits.

To construct a plausible counterfactual for workers' earnings, we make two adjustments to the data. First, we drop all firms with over 300 employees at baseline. These large firms appear to have different wage trajectories than the rest of the sample. Second, we scale each workers' income by the average earnings within their firm size group in 2017. Formally, let $y_{ij(k),t}$ be the earnings of worker i in firm j of size k (i.e. small, medium, large) in year t. We define the variable

$$\tilde{y}_{ij(k),t} = \frac{y_{ij(k),t}}{\frac{1}{N_k}\sum_k y_{ij(k),t=2017}}$$

where N_k is the number of workers across all firms of size k. The key distinction between scaling earnings by their baseline levels and computing the log of earnings is that changes in the former represents a percent change relative to the average *worker* within the same firm size group, whereas the latter is the change in wages at the "average" *firm*. If there is substantial heterogeneity in wages across firms, these estimates may not necessarily be the same.

Figure 5 plots the dynamic treatment effects over time, taking $\tilde{y}$ as our outcome variable. We find that this transformation of the data appears to generate a reasonable counterfactual comparison group for our analysis. Workers' earnings in small and medium/large firms were growing at the same rate in the three years before the tax reform. In comparison, we observe a sharp decrease in workers' wages of about 2% immediately after the government lowered the tax subsidy for medium and large firms. The similar pre-trends and the precise timing of the reduction in wages suggests that our estimates reflect the impact of the tax reform.

Table 5 panel (B) tests the robustness our estimates of the impact on workers' earnings to alternative specifications. We drop firms with over 300 employees at baseline across all regressions to ensure there are no parallel pre-trends. Columns (1) summarizes the first regression displayed in figure 5. Similar to our previous robustness checks, column (2) compares firms within the same sub-industry, column (3) restricts the sample to firms with 50 to 200 billion KRW revenue in the baseline period, and columns (4) to (6) account for contemporaneous labor market policies. The negative income effect is more precisely estimated among the full sample of firms than the firms that adjusted their investments and employment. Nevertheless, all our estimates suggest that the tax reform had a negative income effect of about 2%. In the next section, we show that this average treatment effect masks significant heterogeneity across the income distribution, and we can even rule out a zero effect in the restricted sample once we focus on the most heavily impacted workers.

6.2.2 Inequality

From a normative perspective, the optimal robot tax depends not on its impact on average wages, but rather, on the entire distribution of wages. Namely, a robot tax can potentially increase social welfare by reducing income inequality. To empirically measure the redistributive properties of the 2018 tax reform, we estimate equation 4 as a series of quantile regressions. There are three key distinctions between our OLS and quantile difference-in-difference analyses:

Identification - The quantile difference-in-difference requires a stronger identifying assumption compared with the standard OLS version. Rather than assuming that average wages would have evolved at the same rate between small and medium/large firms, we now need to assume that the entire distribution of wages would have evolved similarly.[6] We test this assumption the same way as earlier by seeing whether our quantile estimates exhibit a pre-trend prior to 2018.

Interpretation - Our estimates of the quantile treatment effect represent the impact of the tax reform on percentiles of the unconditional earnings distribution. Note that with quantile regressions, changes in the distribution of wages reflect a combination of real wage effects as well as reshuffling in the ranking of workers. As such, it is unable to determine which workers benefits or lose due to the reform. From a social planner's perspective though, it does not matter if the reduction in tax credits for automation investments changed workers' rankings. The key parameter is the distribution of wages, irrespective of worker types and their location along that distribution. As Costinot and Werning (2022) notes in the discussion of their sufficient statistics model, "individual workers may move across the wage distribution, switching quantiles, as panel data would reveal, but a repeated cross-section of wages is sufficient. In so doing, we provide a normative rationale for quantile wage regressions."

Implementation - We estimate the unconditional quantile treatment effect using the *rqr* command in Stata. The command implements the recent model developed by Borgen et al. (2021) to estimate unconditional quantile treatment effects while handling high dimensional fixed effects in a computationally efficient way.

Figure 6 plots the quantile treatment effects along each decile of the income distribution. Our baseline specification of firms between 10-400 billion KRW, depicted by the solid black line, suggests that the reduction in the automation subsidy actually increased inequality. The estimates imply a 5% decrease in the bottom decile of the income distribution and no effect at the top. However, once we restrict the sample to only firms with annual revenues close to the firm-size cutoff, depicted by the dashed gray line, we see a very different pattern. In that case, the policy had no effect on the lower tail of the income distribution, and primarily decreased wages from the 5th to 9th deciles.[7]

To determine which of the two patterns in figure 6 represents the causal impact of the tax reform, we assess the parallel trends of the quantile estimates. Appendix figure 3 plots the dynamic treatment effects on the first decile of the income distribution separately for the baseline specification and the restricted sample of firms with similar annual revenues. We find that the baseline specification already exhibits a downward pre-trend prior to the 2018 tax change, leading us to believe that the estimates are confounded by differential time trends. In comparison, there are no significant pre-trends in the restricted sample. Thus, it appears that to construct a reasonable counterfactual control group, we need to constrain the sample to employers around the revenue threshold that divides small and medium/large firms.

Focusing on the restricted sample in figure 6, depicted by the dashed gray line, our quantile estimates suggest that cutting the tax credits for investments in robots succeeded in reducing wage

[6] See Havnes and Mogstad (2015) for another empirical paper that utilizes a similar quantile diff-in-diff strategy.

[7] We include only firm and year fixed effects in the main quantile regressions. In appendix figure 4, we show that our results are robust to adding industry fixed effects and accounting for minimum wage effects.

inequality, but did so by slowing wage growth in the upper half of the income distribution. We find that the policy had no discernible impact on the wages of jobs in the bottom four deciles of the income distribution, and lowered the wages of jobs in the fifth to seventh deciles by about 3%. Given that we do not observe any positive wage effects in the left tail of the distribution, our result appears be inconsistent with the predictions of optimal robot tax models that a cut in the subsidy rate would redistribute wages from non-routine workers to routine workers (Costinot and Werning, 2022; Thuemmel, 2022).

However, the negative income effect does not necessarily mean that the policy decreased social welfare. Instead, the welfare implications depend on whether the change in the income distribution is driven by a real wage effect or a composition effect. While optimal taxation models calculate welfare by summing utilities over *all* individuals in the economy, we are only able to observe the wages of employed individuals. As a result, it is entirely possible that the negative effect on the income distribution is consistent with a positive welfare effect. For example, suppose the tax reform caused employers to replace robots with low-skilled labor but had no other effect on the wages of existing workers. That response would be reflected as a decrease in average wages among the employed, but in reality, it increased the welfare of unemployed workers. On the other hand, if a reduction in robots made firms less productive, the decrease in average wages would reflect a negative welfare effect. Since our data lacks a worker identifier to distinguish between stayers, new hires, and separations, we are unable to disentangle the real wage and composition effects of the tax reform.

6.3 Heterogeneous Impacts

Worker Heterogeneity

We next explore how the labor market impacts of the 2018 tax reform varied by workers' age. Inter-generational models of technological adoption have assumed that young workers invest in skills that insulate them against expected changes in technology (Adão et al., 2020; Cavounidis et al., 2023). This assumption has been used within optimal taxation models to argue that although it may be efficient to tax robots at present, the optimal robot tax converges to zero in the long-run as new generations enter the labor market (Guerreiro et al., 2022; Thuemmel, 2022). Here, we test whether the decrease in tax credits had a more positive impact on older workers who may not have anticipated the growth of robots earlier in their careers.

Table 6 reports the estimates of the employment and income effects, separately for 3 age groups: workers younger than age 30, workers aged 30-50, and workers older than age 50. Given that the employment effect was primarily concentrated among companies with 50-200 billion KRW revenue per year, we restrict the sample to those firms. Although we only find statistically significant negative earnings effect among young workers, we cannot reject that the income effect is the same across all age groups. While our inability to find significant differences in the labor market outcomes by age can be interpreted as evidence against the theory that robots are primarily a substitute for middle-age production workers (Acemoglu and Restrepo, 2021), the point estimates on the income effect is positive only for older workers and negative otherwise. Thus, our results regarding heterogeneity across workers of different ages is inconclusive.

A key distinction between our setting and models of inter-generational robot taxation is that we focus solely on the impacts of a robot subsidy within-manufacturing rather than across all sectors. While the labor market effects of a robot tax may not seem to vary by age within-manufacturing, it could still be the case that the policy has greater benefits for older workers overall because they are more likely to be in the manufacturing industry. However, that distinction is only relevant for interpreting intensive margin responses like changes in workers' earnings. Since employment is an extensive margin response, if a robot tax creates more jobs for older workers in the economy as a whole, this would be reflected as a greater increase in manufacturing employment for older workers within our sample. Nevertheless, we find no statistically significant differences in the effect of the tax reform across age groups on either earnings or employment.

7 The Fiscal Externality

In this section, we calculate the fiscal externality of the 2018 tax reform in three steps building on equations 1 and 2, that decompose the total policy effect into the mechanical and behavior effects. First, to compute the total effect of the 2018 tax reform, we estimate equation 4 with the outcome variable being the amount of government tax credits each firm receives minus the sum of their corporate and income taxes. The difference-in-difference estimates thus measure the impact of reducing the tax credit rate on the government's net expenditures. Second, we simulate the mechanical effect as the amount that tax credits would have changed by if the treatment group continued to behave the same as the control group after the subsidy rate fell. Lastly, we calculate the behavioral effect as the difference between the total and mechanical effects, and the fiscal externality as the ratio of the behavioral and mechanical effects.

To relate the theoretical interpretation of the mechanical effect to the data, we introduce potential outcomes notation and define $Y^{treat}_{post,0}$ as the counterfactual amount that the treatment group would have claimed after the tax reform if the reform never occurred. The mechanical effect would then be defined as

$$ME = \begin{cases} -\frac{2}{3}Y^{treat}_{post,0} & \text{if firm size is large} \\ -\frac{2}{5}Y^{treat}_{post,0} & \text{if firm size is medium.} \end{cases}$$

Keeping employers' behavior unchanged, the cut in tax credits would decrease the amount of credits firms receive by 2 percentage points relative to the baseline subsidy rate. Thus, for large firms, the decrease from 3 to 1 percent would result in a reduction of two-thirds of the counterfactual credit amount, and for medium firms the decrease from 5 to 3 percent would result in a two-fifths decrease, as shown above.

We simulate $Y^{treat}_{post,0}$ by noting that the difference-in-difference estimates from section 5 measured the causal impact on the percent change in tax credits:

$$\beta_{DiD} = \frac{Y^{treat}_{post,1} - Y^{treat}_{post,0}}{Y^{treat}_{pre}}.$$

Together, the equations imply a simple formula for the mechanical effect:

$$ME = \begin{cases} -\frac{2}{3}(Y^{treat}_{post,1} - \beta_{DiD} \cdot Y^{treat}_{pre}) & \text{if firm size is large} \\ -\frac{2}{5}(Y^{treat}_{post,1} - \beta_{DiD} \cdot Y^{treat}_{pre}) & \text{if firm size is medium,} \end{cases}$$

where $Y^{treat}_{post,1}$ and Y^{treat}_{pre} are sample means, β_{DiD} is the effect on amount claimed from column (1) of table 4, and the standard error for their linear combination is computed via the Delta method.

Table 7 reports each component of the fiscal externality. The first column implies that the reduction in the robot tax cut net government expenditures by 0.15 billion KRW. We decompose this change via the effect on corporate taxes and income taxes, and find that the entire decrease in net transfers is driven by firms paying more income taxes. This is consistent with the fact that we saw a positive impact on employment as a result of the tax reform. Mechanically though, the fourth column shows that we would expect the tax change to only increase the government's budget by 0.02 billion KRW. Computing the difference between the total and mechanical effects suggests that behavioral adjustments by firms saved the government an additional 0.13 billion KRW per firm, on average. Taking the ratio of the behavioral and mechanical costs reveals a fiscal externality of 7.8.

Our estimate of the fiscal externality implies that for each dollar that the government mechanically collects by reducing the robot subsidy, they will collect an additional \$7.8 in other taxes. While the confidence interval is fairly wide, we can statistically reject a negative externality at the 95% confidence level. The direction of our estimate stands in sharp contrast to estimates from studies of changes to the income tax rate. For example, Hendren and Sprung-Keyser (2020) calculate that the Reagan tax cut of 1981 and the 1993 tax increase on top earners had *negative* fiscal externalities of -1.51 and -0.46, respectively. At the lower end of the income distribution, Hendren and Sprung-Keyser (2020) finds that increases in the EITC have a fiscal externality of -0.08. In general, increases in income taxes tend to lead to behavioral responses, such as a reduction in labor supply, that reduce the government's budget. On the other hand, a robot tax actually has the opposite effect because while it discourages investments, it also leads to more jobs from which the government can collect income taxes. As such, the direction of our fiscal externality is in line with other programs where reductions in government transfers encourages work.[8] Taken together, we interpret the results to suggest that a robot tax would be cost-effective way of increasing employment.

[8]For instance, in a review of the unemployment insurance literature, Schmieder and Von Wachter (2016) finds that estimates for each dollar of transfer from increasing the weekly unemployment benefits, the government bears an additional \$0.37 to \$4.58 behavioral cost.

8 Conclusion

Concerns over the rise in inequality and the loss of manufacturing jobs has motivated policy proposals to introduce taxes for robots and automation. Our paper leverages policy variation in a tax credit for automation technology in South Korea to empirically study the impact of a robot tax. We document three main results. First, reducing the tax subsidy causes firms to decrease investments in automation and raise employment. Second, although firms hired more workers, they did not raise workers wages nor did the tax reform reduce income inequality. Third, a reduction in robot subsidies has a large positive fiscal externality, implying that the behavioral response of firms actually increases the government's revenue. Together, these results provide empirical estimates of the key parameters that enter into optimal robot taxation models (Acemoglu et al., 2020a; Costinot and Werning, 2022; Thuemmel, 2022).

The results of our study support the view that robot adoption displaces workers. In general though, empirical evidence on the labor market impacts of robot adoption are mixed. Some studies find that robot adoption reduces employment (Acemoglu and Restrepo, 2020; Bessen et al., 2023; Bonfiglioli et al., 2024), whereas others find that reductions in manufacturing employment are compensated by reallocations to technical or service jobs (Graetz and Michaels, 2018; Humlum, 2019). To investigate the aggregate labor market effects of a robot tax, future research can use data on additional industries and a worker-level panel to observe spillover effects across sectors. In addition, while the types of capital covered in our setting appears to replace labor, previous papers have found that subsidizing other types of capital investments can have the opposite effect (Curtis et al., 2021; Hirvonen et al., 2022). It would be important from a policy perspective for future research to develop a way to identify which types of capital are labor replacing.

References

Acemoglu, Daron and Pascual Restrepo, “Robots and Jobs: Evidence from US Labor Markets,” *Journal of Political Economy*, 2020, *128* (6), 2188–2244.

_ and _ , “Demographics and Automation,” *The Review of Economic Studies*, 06 2021, *89* (1), 1–44.

_ , Andrea Manera, and Pascual Restrepo, “Does the US tax code favor automation?,” Technical Report, National Bureau of Economic Research 2020.

_ , Claire Lelarge, and Pascual Restrepo, “Competing with robots: Firm-level evidence from france,” in “AEA Papers and Proceedings,” Vol. 110 2020, pp. 383–88.

Adão, Rodrigo, Martin Beraja, and Nitya Pandalai-Nayar, “Technological transitions with skill heterogeneity across generations,” Technical Report, National Bureau of Economic Research 2020.

Akerman, Anders, Ingvil Gaarder, and Magne Mogstad, “The Skill Complementarity of Broadband Internet,” *The Quarterly Journal of Economics*, 2015, *130* (4), 1781–1824.

Autor, David H., Frank Levy, and Richard J. Murnane, “The Skill Content of Recent Technological Change: An Empirical Exploration,” *The Quarterly Journal of Economics*, 2003, *118* (4), 1279–1333.

Banerjee, Abhijit V. and Esther Duflo, “Do Firms Want to Borrow More? Testing Credit Constraints Using a Directed Lending Program,” *The Review of Economic Studies*, 02 2014, *81* (2), 572–607.

Bessen, James, Maarten Goos, Anna Salomons, and Wiljan Van den Berge, “What happens to workers at firms that automate?,” *Review of Economics and Statistics*, 2023, pp. 1–45.

Bonfiglioli, Alessandra, Rosario Crinò, Harald Fadinger, and Gino Gancia, “Robot Imports and Firm-Level Outcomes,” *The Economic Journal*, 06 2024, p. ueae055.

Borgen, Nicolai T, Andreas Haupt, and Øyvind N Wiborg, “A new framework for estimation of unconditional quantile treatment effects: The Residualized Quantile Regression (RQR) model,” 2021.

Brynjolfsson, Erik and Andrew McAfee, *The second machine age: Work, progress, and prosperity in a time of brilliant technologies*, WW Norton & Company, 2014.

Cavounidis, Costas, Qingyuan Chai, Kevin Lang, and Raghav Malhotra, “Obsolescence Rents: Teamsters, Truckers, and Impending Innovations,” Working Paper 31743, National Bureau of Economic Research September 2023.

Costinot, Arnaud and Iván Werning, "Robots, Trade, and Luddism: A Sufficient Statistic Approach to Optimal Technology Regulation," *The Review of Economic Studies*, 11 2022. rdac076.

Curtis, E. Mark, Daniel G Garrett, Eric C Ohrn, Kevin A Roberts, and Juan Carlos Suárez Serrato, "Capital Investment and Labor Demand," Working Paper 29485, National Bureau of Economic Research November 2021.

Dauth, Wolfgang, Sebastian Findeisen, Jens Suedekum, and Nicole Woessner, "The Adjustment of Labor Markets to Robots," *Journal of the European Economic Association*, 05 2021, *19* (6), 3104–3153.

Dixon, Jay, Bryan Hong, and Lynn Wu, "The robot revolution: Managerial and employment consequences for firms," *Management Science*, 2021, *67* (9), 5586–5605.

Feigenbaum, James and Daniel P Gross, "Answering the Call of Automation: How the Labor Market Adjusted to the Mechanization of Telephone Operation," Technical Report, National Bureau of Economic Research 2020.

Garrett, Daniel G., Eric Ohrn, and Juan Carlos Suárez Serrato, "Tax Policy and Local Labor Market Behavior," *American Economic Review: Insights*, March 2020, *2* (1), 83–100.

Giuntella, Osea, Yi Lu, and Tianyi Wang, "How do Workers and Households Adjust to Robots? Evidence from China," Working Paper 30707, National Bureau of Economic Research December 2022.

Graetz, Georg and Guy Michaels, "Robots at Work," *The Review of Economics and Statistics*, 12 2018, *100* (5), 753–768.

Guerreiro, Joao, Sergio Rebelo, and Pedro Teles, "Should robots be taxed?," *The Review of Economic Studies*, 2022, *89* (1), 279–311.

Havnes, Tarjei and Magne Mogstad, "Is universal child care leveling the playing field?," *Journal of public economics*, 2015, *127*, 100–114.

Hendren, Nathaniel and Ben Sprung-Keyser, "A Unified Welfare Analysis of Government Policies*," *The Quarterly Journal of Economics*, 03 2020, *135* (3), 1209–1318.

Hirvonen, Johannes, Aapo Stenhammar, and Joonas Tuhkuri, "New evidence on the effect of technology on employment and skill demand," *Available at SSRN 4081625*, 2022.

Humlum, Anders, "Robot adoption and labor market dynamics," *Princeton University*, 2019.

IFR, "World Robotics 2021 Industrial Robots," 2021.

Katz, Lawrence F. and Kevin M. Murphy, "Changes in Relative Wages, 1963-1987: Supply and Demand Factors," *The Quarterly Journal of Economics*, 1992, *107* (1), 35–78.

Kim, Bitmaro, Sukjin Woo, and Dongkyoo Kim, "Tax credit for investment in facilities for productivity improvement: 2019 Deep evaluation report for tax expenditures," 2019.

Koch, Michael, Ilya Manuylov, and Marcel Smolka, "Robots and firms," *The Economic Journal*, 2021, *131* (638), 2553–2584.

Kovacev, Robert, "A Taxing Dilemma: Robot Taxes and the Challenges of Effective Taxation of AI, Automation and Robotics in the Fourth Industrial Revolution," *Ohio St. Tech. LJ*, 2020, *16*, 182.

Lee, David S, Pauline Leung, Christopher J O'Leary, Zhuan Pei, and Simon Quach, "Are sufficient statistics necessary? nonparametric measurement of deadweight loss from unemployment insurance," *Journal of Labor Economics*, 2021, *39* (S2), S455–S506.

Liu, Yongzheng and Jie Mao, "How do tax incentives affect investment and productivity? Firm-level evidence from China," *American Economic Journal: Economic Policy*, 2019, *11* (3), 261–91.

Maffini, Giorgia, Jing Xing, and Michael P Devereux, "The impact of investment incentives: evidence from UK corporation tax returns," *American Economic Journal: Economic Policy*, 2019, *11* (3), 361–89.

McGoogan, Cara, "South Korea introduces world's first 'robot tax'," *The Telegraph*, Aug 2017.

Mirrlees, J. A., "An Exploration in the Theory of Optimum Income Taxation," *The Review of Economic Studies*, 1971, *38* (2), 175–208.

Moon, Terry S, "Capital gains taxes and real corporate investment: Evidence from Korea," *American Economic Review*, 2022, *112* (8), 2669–2700.

Ohrn, Eric, "The effect of corporate taxation on investment and financial policy: Evidence from the DPAD," *American Economic Journal: Economic Policy*, 2018, *10* (2), 272–301.

Prodhan, Georgina, "European parliament calls for robot law, rejects robot tax," *Reuters*, Feb 2017.

Rodrigo, Rodimiro et al., "Robot Adoption, Organizational Capital, and the Productivity Paradox," 2021.

Saez, Emmanuel, Joel Slemrod, and Seth H Giertz, "The elasticity of taxable income with respect to marginal tax rates: A critical review," *Journal of economic literature*, 2012, *50* (1), 3–50.

Schmieder, Johannes F and Till Von Wachter, "The effects of unemployment insurance benefits: New evidence and interpretation," *Annual Review of Economics*, 2016, *8* (1), 547–581.

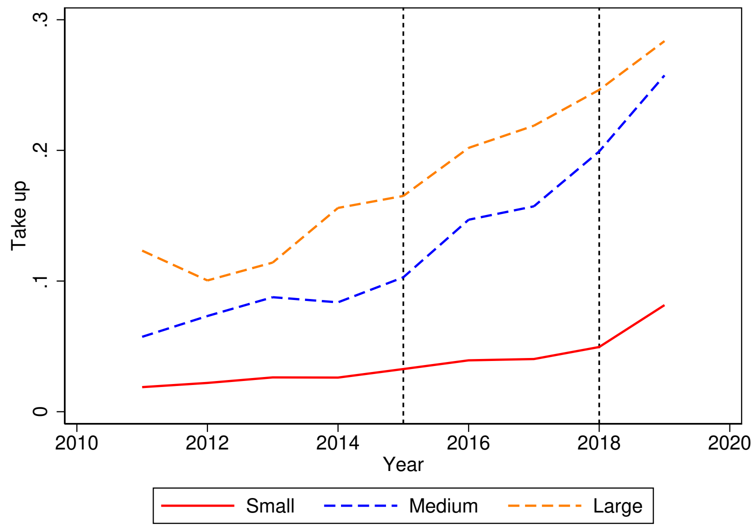
Thuemmel, Uwe, "Optimal Taxation of Robots," *Journal of the European Economic Association*, 11 2022. jvac062.

Yagan, Danny, "Capital Tax Reform and the Real Economy: The Effects of the 2003 Dividend Tax Cut," *American Economic Review*, December 2015, *105* (12), 3531–63.

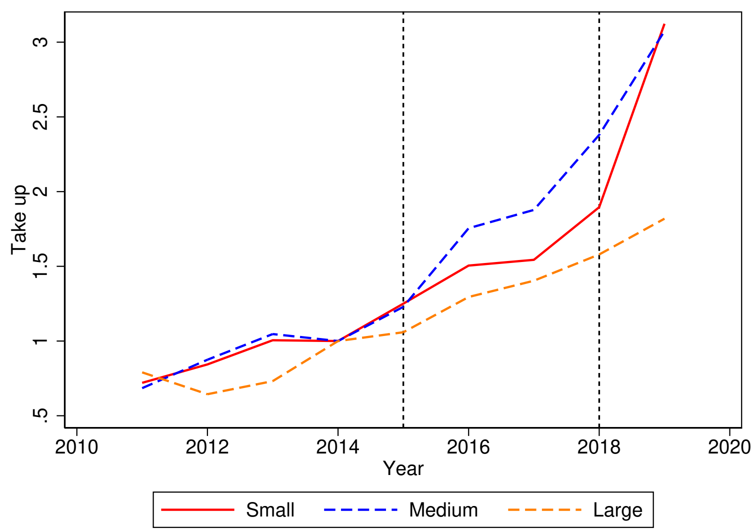
Zhang, Pengqing, "Automation, wage inequality and implications of a robot tax," *International Review of Economics Finance*, 2019, *59*, 500–509.

Zwick, Eric and James Mahon, "Tax policy and heterogeneous investment behavior," *American Economic Review*, 2017, *107* (1), 217–48.

Figure 1: Take-up of Tax Credit Over Time



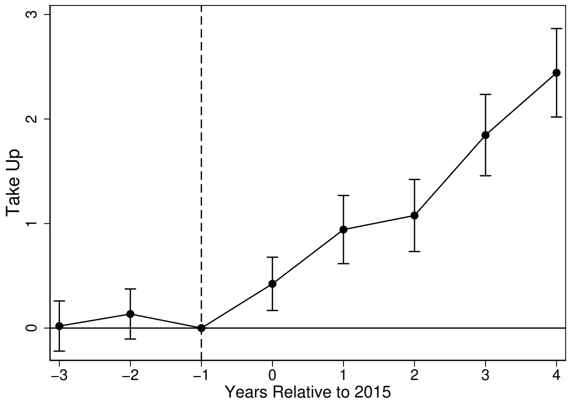
(a) Raw Trends



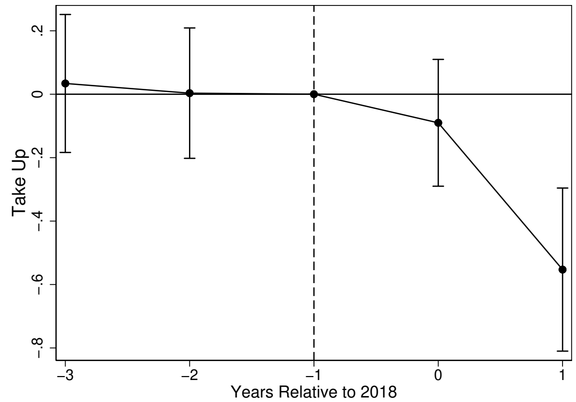
(b) Scaled by 2014 Averages

Notes: In panel (a), the figure plots the raw means in take-up over time for small, medium, and large firms. In panel (b), take-up is scaled by the average take-up of each group in 2014. The sample is restricted to firms with annual revenues between 10-400 billion KRW.

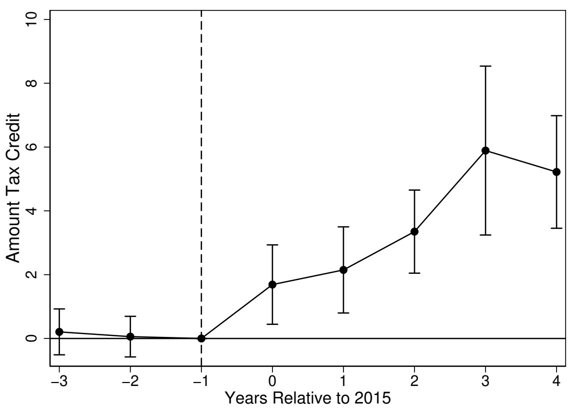
Figure 2: Effect on Investments and Tax Credits Claimed



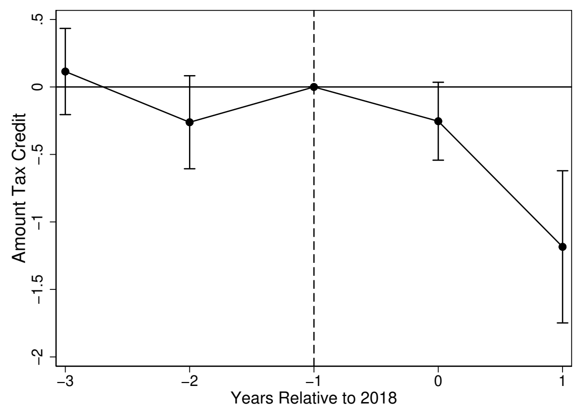
(a) Take-up (2015)



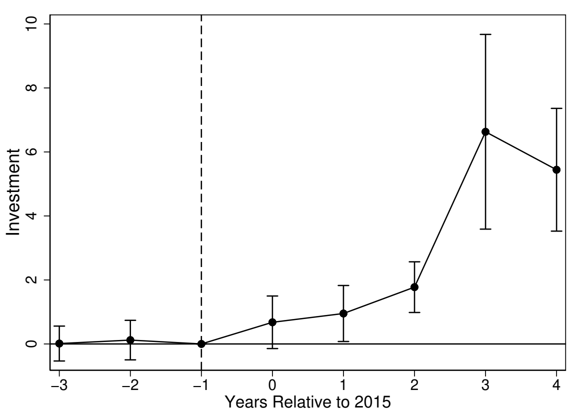
(b) Take-up (2018)



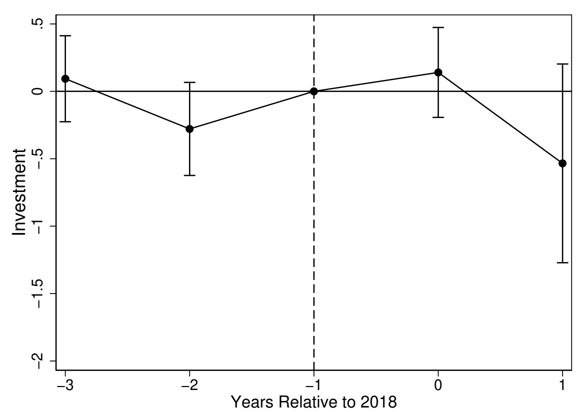
(c) Amount Claimed (2015)



(d) Amount Claimed (2018)



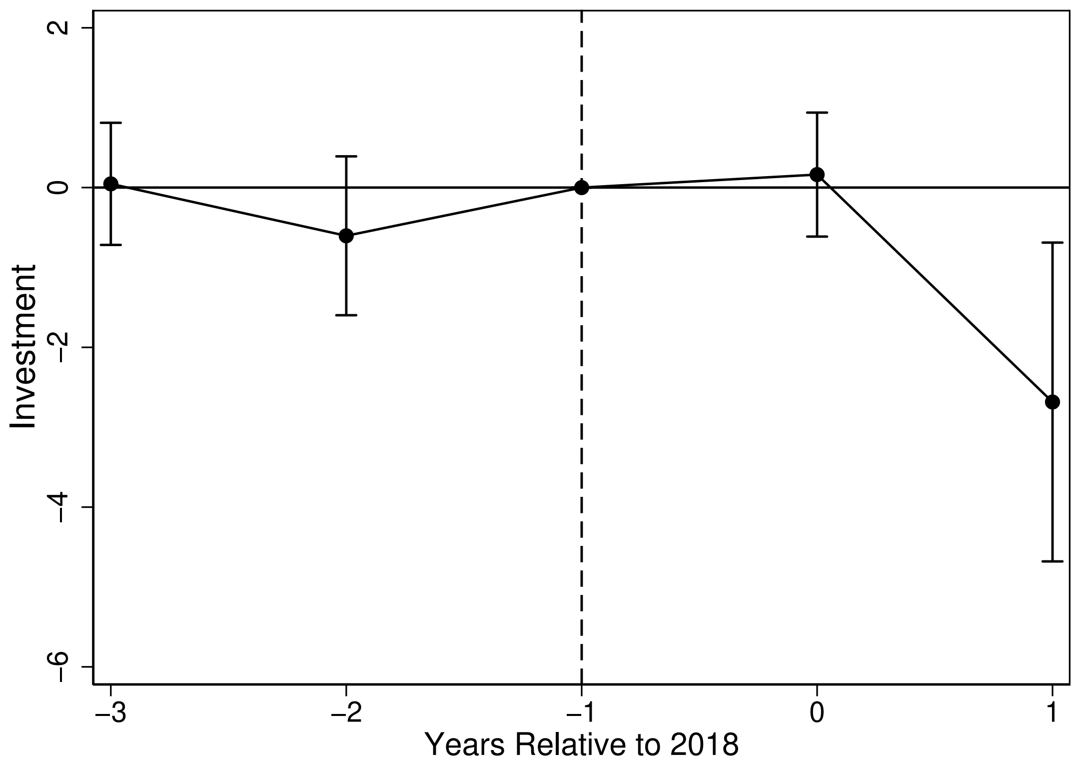
(e) Investments (2015)



(f) Investments (2018)

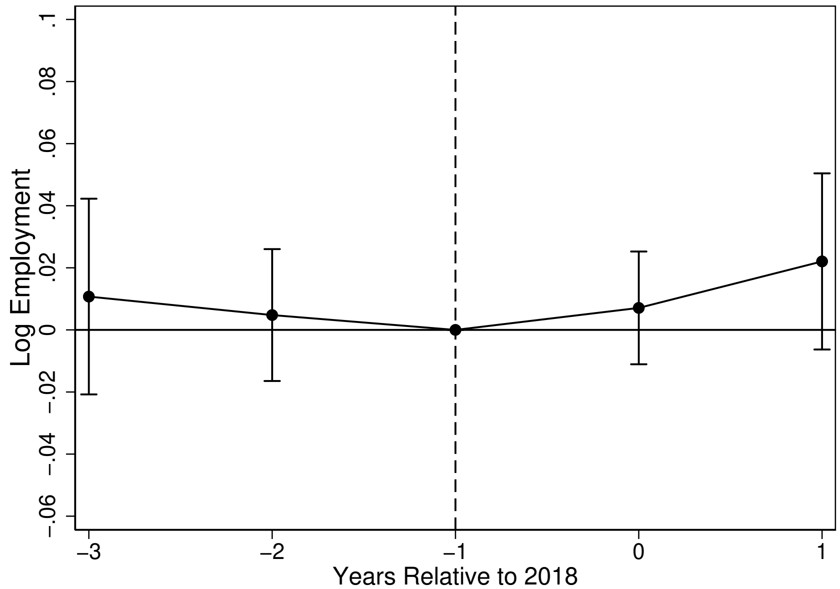
Notes: The figure plots the difference-in-difference estimates from equation 3 for take-up of the tax credit, amount of tax credit claimed, and investments in credit-eligible capital separately by the 2015 and 2018 tax reforms. The figure includes 95% confidence intervals using standard errors clustered by firm.

Figure 3: Effect of 2018 Reform on Investment (50<Rev<200)

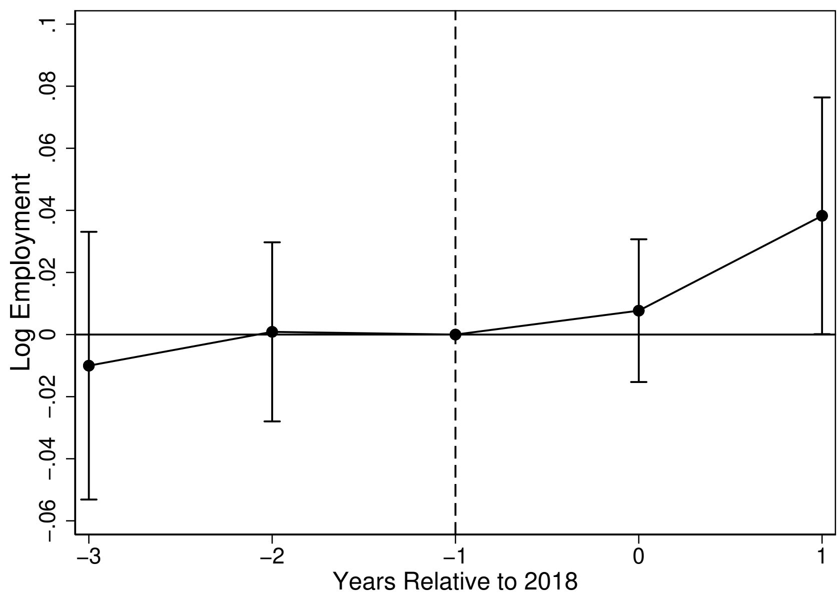


Notes: The figure plots difference-in-difference estimates from equation 3 for the effect of the 2018 tax reform on investments. The sample is restricted to firms with baseline revenue between 50-200 billion KRW. The figure includes 95% confidence intervals using standard errors clustered by firm.

Figure 4: Effect of 2018 Reform on Employment



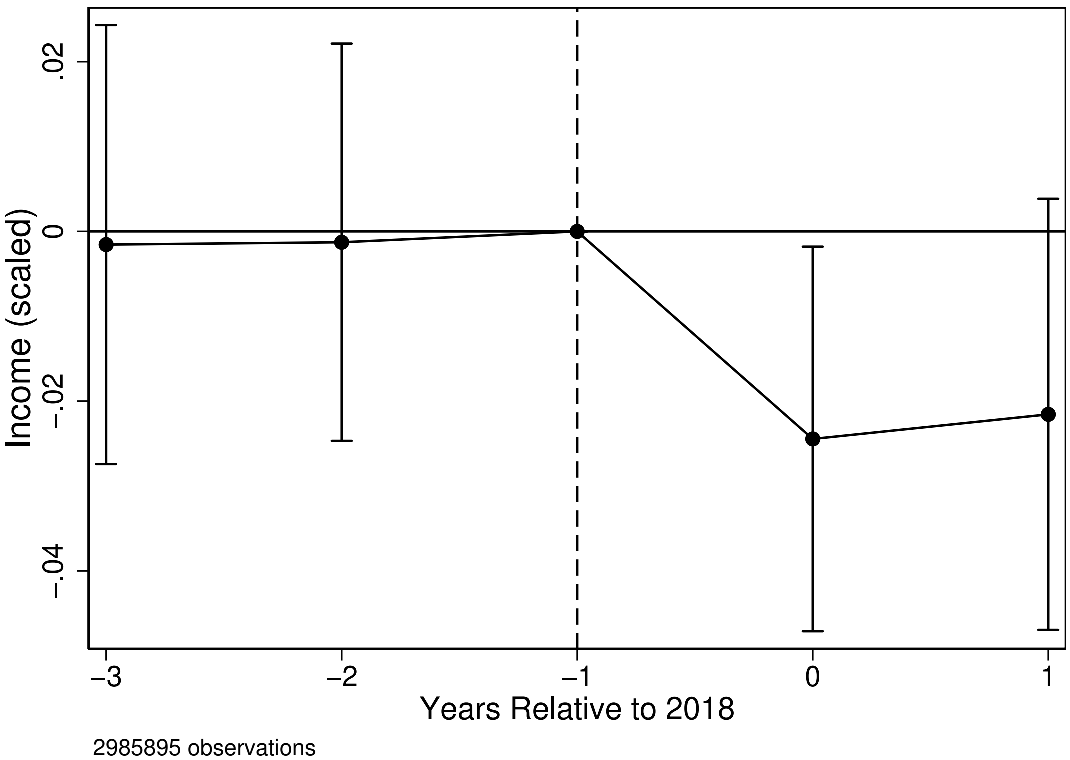
(a) Revenue 10-400 Billion KRW



(b) Revenue 50-200 Billion KRW

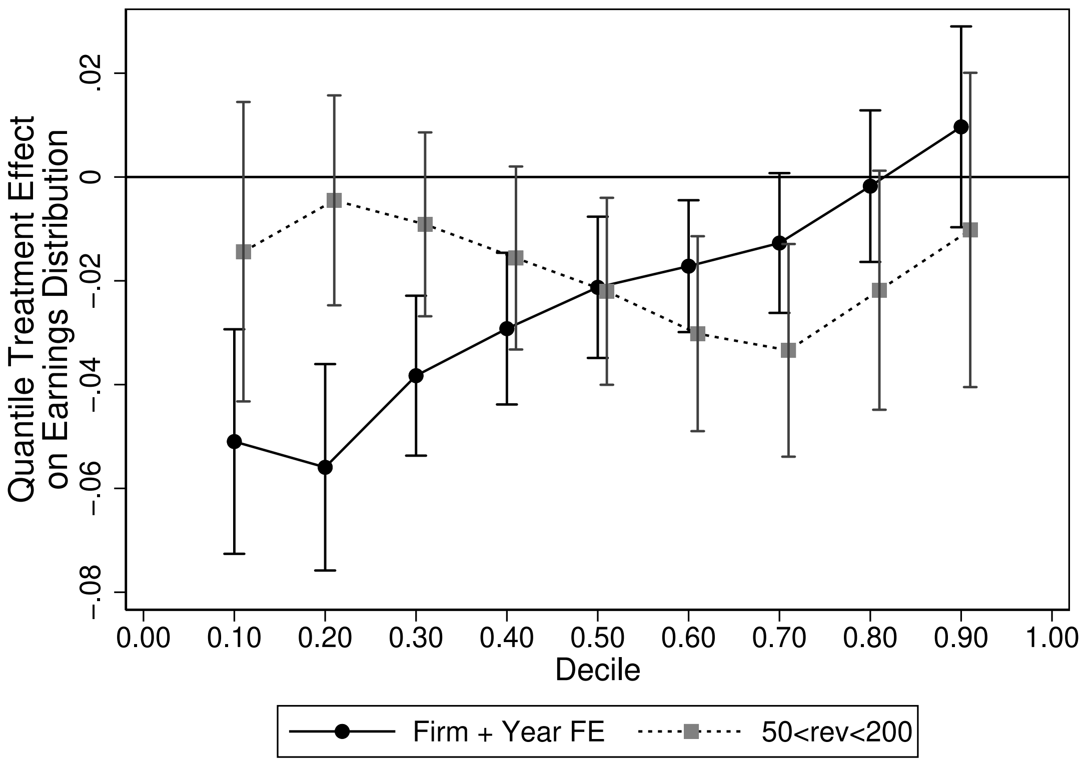
Notes: The figure plots difference-in-difference estimates from equation 3 for the effect of the 2018 tax reform on log employment. Panel (a) keeps all firms with baseline revenue between 10-400 billion KRW. Panel (b) restricts the sample to firms with 50-200 billion KRW. The figure includes 95% confidence intervals using standard errors clustered by firm.

Figure 5: Effect on Workers' Income



Notes: The figure plots difference-in-difference estimates from equation 3 for the effect of the 2018 tax reform on workers' earnings. 95% confidence intervals are computed using standard errors clustered by firm.

Figure 6: Effect on Distribution of Workers' Income



Notes: The figure plots quantile difference-in-difference estimates for the effect of the 2018 tax reform on deciles of workers' earnings. Our first specification controls for firm and year fixed effects as in equation 4. The second specification restricts firms to those with annual revenue of 50-200 billion KRW in 2017. 95% confidence intervals are computed using standard errors clustered by firm.

Table 1: Tax Credit Rates for Investments in Automation Technology

Period	**Small**	**Medium**	**Large**
2002-2014	7%	3%	
2015-2017	7%	5%	3%
2018-2019	7%	3%	1%

Notes: This table reports the tax credit rates that firms can claim for investing in automation technology in South Korea, conditional on their size classification.

Table 2: Definition of Firm Size

	Before 2015	**After 2015**
Small	1) Number of workers $<$1,000 2) Capital $\leq$ 100 billion KRW 3) Revenue $\leq$ 100 billion KRW 4) Assets $\leq$ 500 billion KRW	1) Revenue $\leq$ Threshold • 150 billion KRW: Clothing, Metal, Electric Appliances, Furniture • 100 billion KRW: Food, Tobacco, Textile, Wood, Chemicals, Electronic Devices, Automobile • 80 billion KRW: Beverages, Medical 2) Assets $\leq$ 500 billion KRW
Medium	There were only "small" and "general" firms. If a firm is not "small", then it is a "general" firm.	1) Not small firm 2) Does not belong to an "Enterprise group subject to limitations on mutual investment" 3) Does not take investment from a corporation with assets more than 10 trillion KRW
Large		Not small or medium firm

Notes: This table summarizes the definition of small, medium, and large firms before and after the 2015 tax reform. For the purposes of the tax credit, revenue is measured as the average annual revenue over the past 3 years.

Table 3: Descriptive Statistics (2015-2019), by Firm Size

	All firms			Conditional on Take-up		
	Small	Medium	Large	Small	Medium	Large
Firm Characteristics						
Employment	93	296	301	122	322	381
Assets	36	177	244	52	194	291
Revenue	36	172	227	46	182	258
Tax Base	16.71	111.86	119.27	43	163	217
Taxes Payable	4.22	26.5	30.73	43	163	217
Profits	1.03	7.31	7.256	3.152	12.419	14.936
Take-up of Tax Credit						
Probability of Take-up	0.05	0.17	0.23	1	1	1
Credits as Share of Tax Payable	0.01	0.015	0.015	0.153	0.074	0.052
Number of Firms	6098	763	203	1464	664	231

Notes: This table reports the average characteristic of firms in 2015-2019, separately by firm size in 2014. All monetary values are measured in 100 million won.

Table 4: Effect of Robot Tax on Take-up and Investments

	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: t = 2011-2017						
Take-up	1.682^{***}	1.682^{***}	1.931^{***}			
	(0.172)	(0.172)	(0.222)			
Amount Claimed	4.849^{***}	4.849^{***}	4.699^{***}			
	(0.703)	(0.703)	(0.76)			
Investments	5.219^{***}	5.219^{***}	5.31^{***}			
	(0.864)	(0.864)	(0.993)			
N	62559	62559	15129			
PANEL B: t = 2015-2019						
Take-up	-0.334^{***}	-0.343^{***}	-0.427^{**}	-0.427^{**}	-0.314^{***}	-0.334^{***}
	(0.097)	(0.101)	(0.19)	(0.19)	(0.101)	(0.097)
Amount Claimed	-0.67^{***}	-0.595^{***}	-1.389^{**}	-1.389^{**}	-0.641^{***}	-0.67^{***}
	(0.176)	(0.178)	(0.571)	(0.571)	(0.178)	(0.176)
Investments	-0.135	-0.078	-1.073^{*}	-1.073^{*}	-0.11	-0.135
	(0.224)	(0.228)	(0.571)	(0.601)	(0.228)	(0.224)
N	32450	32445	7605	7603	16311	32441
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Industry x Year FE		Y				
50<Revenue<200			Y	Y		
Share MW x Year FE				Y	Y	Y
Drop MW > 20%					Y	
Drop Employment>300						Y

Note: This table displays difference-in-difference estimates on the effect of the 2015 and 2018 tax reforms (Panels A and B, respectively) on take-up of the tax credit and firms' investments. Column (1) presents the estimates from equation 4. Column (2) controls for industry-year fixed effects. Column (3) restricts the sample to only firms with annual revenue at baseline between 50 and 200 billion KRW. Column (4) controls for the share of workers in a firm at baseline that earned below the 2018 minimum wage, interacted with year fixed effects. Column (5) drops firms with over 10% of its workers affected by the 2018 minimum wage change. Column (6) drops firms with over 300 workers employed in 2017. Standard errors are clustered by firm. *10%,** 5%,*** 1% significance level.

Table 5: Labor Market Effect of Robot Tax

	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: Log Employment						
$Treat * 2018$	0.007	0.004	0.008	0.008	0.004	0.007
	(0.009)	(0.009)	(0.012)	(0.012)	(0.009)	(0.009)
$Treat * 2019$	0.022	0.014	0.038^{**}	0.038^{*}	0.015	0.022
	(0.014)	(0.015)	(0.019)	(0.019)	(0.015)	(0.014)
N	32,450	32,445	7,605	7,603	27,916	32,441
PANEL B: %Δ Earnings						
$Treat * 2018$	-0.024^{**}	-0.027^{**}	-0.017	-0.017	-0.021^{*}	-0.025^{**}
	(0.012)	(0.012)	(0.016)	(0.016)	(0.012)	(0.012)
$Treat * 2019$	-0.022^{*}	-0.023^{*}	-0.019	-0.019	-0.018	-0.021
	(0.013)	(0.014)	(0.019)	(0.019)	(0.013)	(0.013)
N	2,985,895	2,985,895	927,123	926,748	2,590,019	2,983,486
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Industry x Year FE		Y				
50<Revenue<200			Y	Y		
Share MW x Year FE				Y	Y	Y
Drop MW > 20%					Y	
Drop Employment>300						Y

Note: This table displays difference-in-difference estimates of the labor market impacts of the 2018 reduction in tax credits for medium and large firms. Column (1) presents the estimates from equation 3. Column (2) controls for industry-year fixed effects. Column (3) restricts the sample to only firms with annual revenue at baseline between 50 and 200 billion KRW. Column (4) controls for the share of workers in a firm at baseline that earned below the 2018 minimum wage, interacted with year fixed effects. Column (5) further drops firms with over 10% of its workers affected by the 2018 minimum wage change. Column (6) and all of panel B drops firms with over 300 workers employed in 2017. Standard errors are clustered by firm. $^{*}10\%,^{**}5\%,^{***}1\%$ significance level.

Table 6: Labor Market Effect of Robot Tax, by Age

	(1)	(2)	(3)
PANEL A: Log Employment			
$Treat * 2018$	0.02	0.006	-0.002
	(0.022)	(0.012)	(0.014)
$Treat * 2019$	0.032	0.034^{*}	0.031
	(0.032)	(0.02)	(0.022)
N	7,605	7,605	7,605
PANEL B: %Δ Earnings			
$Treat * 2018$	-0.012	-0.025	-0.02
	(0.008)	(0.023)	(0.024)
$Treat * 2019$	-0.016^{**}	-0.022	0.012
	(0.008)	(0.024)	(0.03)
N	214,459	644,555	195,418
Age	<30	30-50	$>$ 50
Firm FE	Y	Y	Y
Year FE	Y	Y	Y
50<Rev<200	Y	Y	Y

Note: This table displays difference-in-difference estimates of the labor market impacts of the 2018 reduction in tax credits for investments in automation, separately by workers' age.

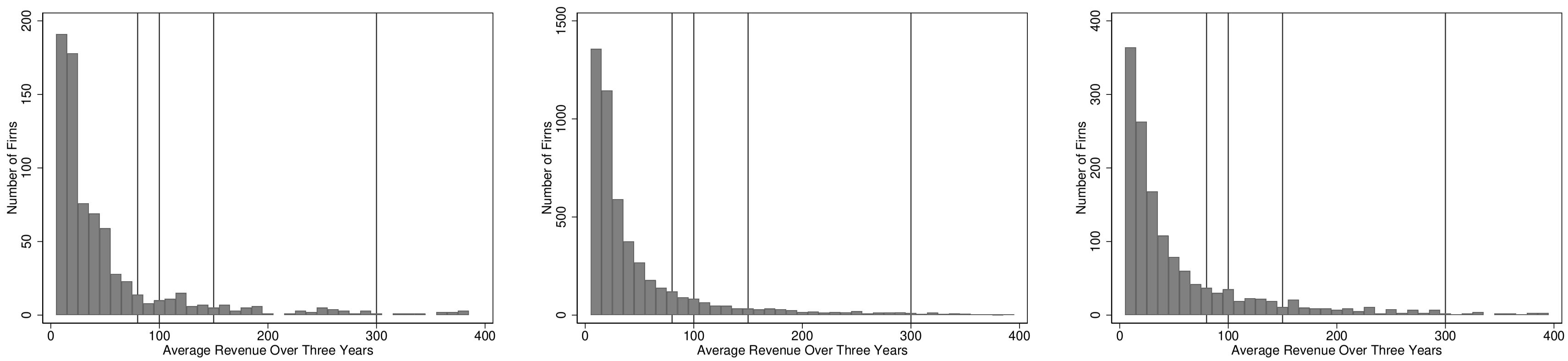
Table 7: Effect of Robot Tax on Fiscal Externality

	Total			Mechanical	Behavioral	Fiscal Externality
	Sum	Corp	Income			
	-0.147^{**}	-0.014	-0.132^{***}	-0.017^{***}	-0.127^{***}	7.828^{***}
	(0.063)	(0.058)	(0.016)	(0.002)	(0.036)	(2.262)
N	35100	35100	35100	37968	73068	73068

Note: This table reports the decomposition of the fiscal externality as described in section 7. Standard errors are clustered by firm. *10%,** 5%,*** 1% significance level.

Appendix A. Supplementary figures and tables

Appendix Figure 1: Revenue Distribution by Firm Size Threshold



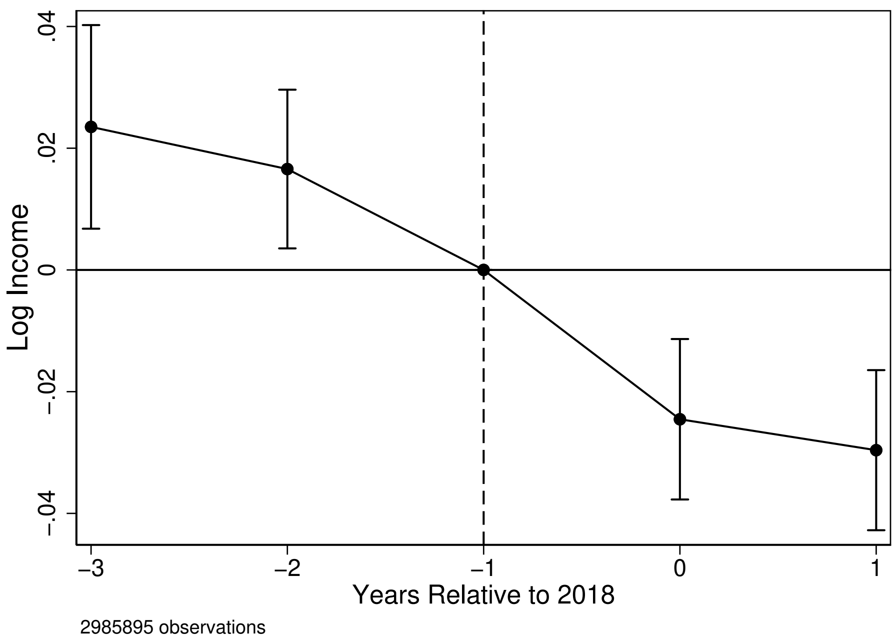
(a) Threshold: 80 Billion KRW

(b) Threshold: 100 Billion KRW

(c) Threshold: 150 Billion KRW

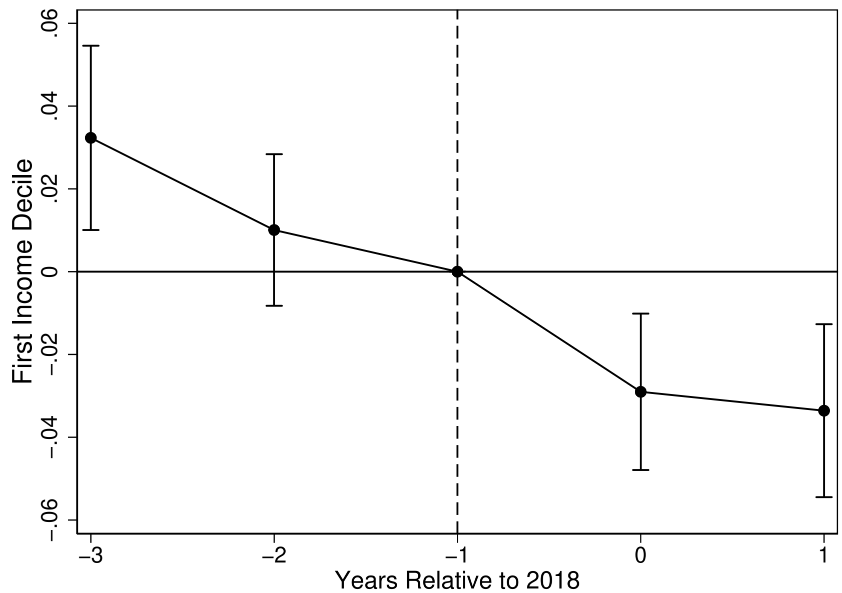
Notes: The figure plots the distribution of revenue across firms in 2017, by the revenue threshold used to separate small and medium/large firms. The threshold by industry is available in table 2.

Appendix Figure 2: Effect on Workers' Log Income

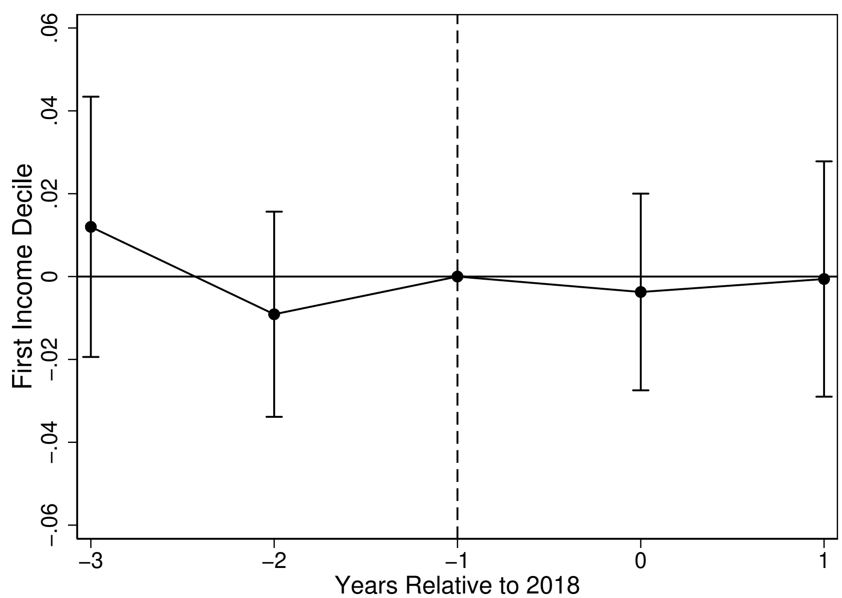


Notes: The figure plots difference-in-difference estimates from equation 3 for the 2018 tax reform on workers' earnings. 95% confidence intervals are computed using standard errors clustered by firm.

Appendix Figure 3: Effect of 2018 Reform on First Decile of Income Distribution



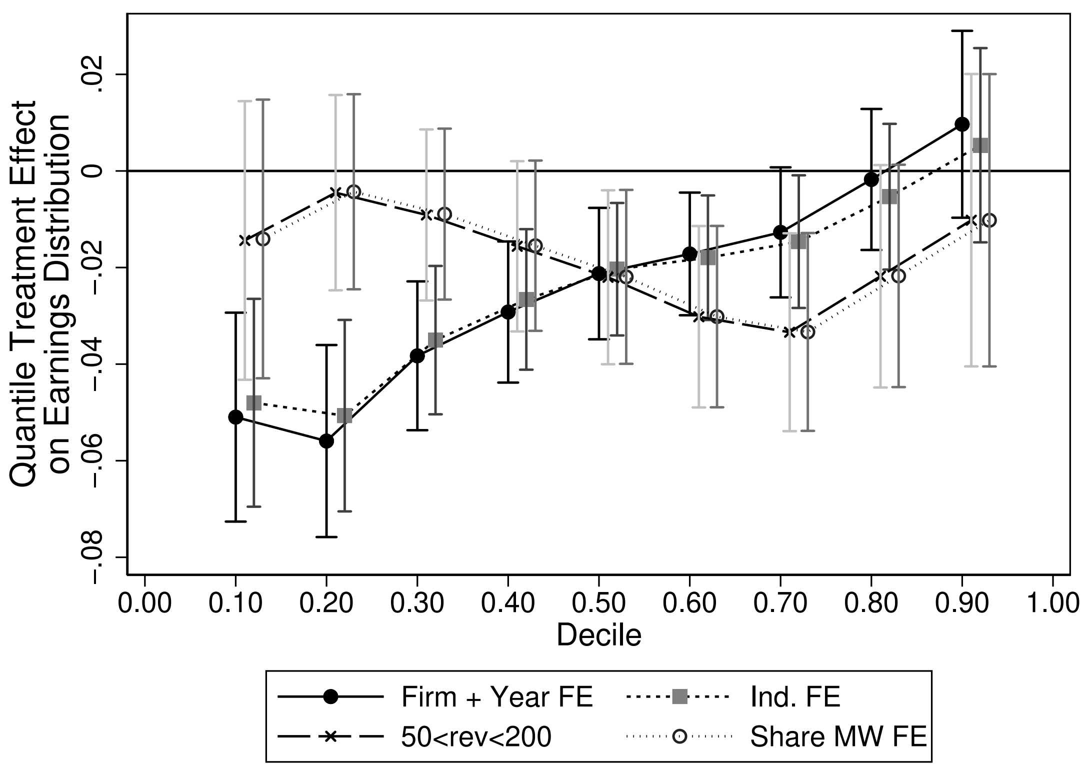
(a) Firm + Year FE



(b) 50<Revenue<200

Notes: The figure plots quantile regression estimates of equation 3 for the treatment effect on the tenth percentile of the earnings distribution. Panel (A) drops firms with over 300 employees in 2017, and Panel (B) further restricts the sample to firms with average revenues of 50-200 billion KRW per year. 95% confidence intervals are computed using standard errors clustered by firm.

Appendix Figure 4: Effect on Distribution of Workers' Income



Notes: The figure plots quantile difference-in-difference estimates for the effect of the 2018 tax reform on workers' earnings. Our first specification controls for firm and year fixed effects as in equation 4. The second specification added industry-year fixed effects. The third specification restricts firms to those with annual revenue of 50-200 billion KRW in 2017. The last specification keeps the firm revenue restriction and controls for share of workers below the new 2018 minimum wage interacted with year fixed effects. 95% confidence intervals are computed using standard errors clustered by firm.

Appendix Table 1: Eligible Investments to Claim Tax Credit

Category	Examples
Process automation facilities	• Automatic loader & unloader • Automatic control system for chemical synthesis • Automatic facilities for quality checks and measurements
Cutting-edge technology facilities	• Software for designing and producing products • 3D printers • Cloud computing service that is directly related to production • GPU/CPU for AI computation
System facilities for supply chain management	• Computers/software for electronically managing the supply chain including procurement, production planning, and inventory management
System facilities for customer relation management (Not eligible after 2019/01/01)	• Computers/software for electronically managing customer data for integration, analysis, marketing, and other customer relations
Information system facilities for logistics (Not eligible after 2019/01/01)	• Computers/software for management of logistics processes, such as purchases, order management, transportation, production, warehouse operations, and distribution networks
Knowledge management system (Not eligible after 2019/01/01)	• Electronic knowledge management systems to share the knowledge of the enterprise

Appendix Table 2: Effect of Robot Tax on Take-up and Investments, 2015 - Drop Small Firms

	(1)	(2)	(3)
Take-up	1.375***	1.375***	1.611***
	(0.143)	(0.143)	(0.184)
Amount Claims	4.32***	4.32***	4.363***
	(0.636)	(0.636)	(0.833)
Investments	3.541***	3.541***	3.508***
	(0.549)	(0.549)	(0.697)
N	7605	7605	4752
Firm FE	Y	Y	Y
Year FE	Y	Y	Y
Industry x Year FE		Y	
50<Revenue<200			Y

Note: This table displays difference-in-difference estimates on the effect of the 2015 tax reforms on take-up of the tax credit and firms' investments. Column (1) presents the estimates from equation 4. Column (2) controls for industry-year fixed effects. Column (3) restricts the sample to only firms with annual revenue at baseline between 50 and 200 billion KRW. All columns drop small firms from the sample. Standard errors are clustered by firm. $^{*}10\%,^{**}5\%,^{***}1\%$ significance level.

Appendix B. The Fiscal Externality Parameter in the Social Planner's Decision

In this section, we show how the fiscal externality that we estimate (see equation 2) corresponds to that from Costinot and Werning (2022). Let $V_i(C, N)$ be individual i's utility, which depends on their consumption C and labor supply N. Individuals maximize their utility by choosing C and N subject to a budget constraint $F(C, N, p, w, b(wN, \tau)) = 0$ that depends on prices p, wages w, and the income tax schedule $b(wN; \tau)$. For example, Costinot and Werning (2022) defines the budget constraint as $pC = wN - b(wN)$, where the slope of $b(\cdot)$ is τ. The budget constraint implies that individuals' utility can be written as a function of the tax parameter and equilibrium prices and wages:

$$V_i(C(N, p, w, b(wN, \tau)), N) \tag{5}$$

The social planner's objective is to maximize a social welfare function that increases with individuals' utility

$$W = \sum_i G_i(V_i) \tag{6}$$

subject to a balanced budget constraint $B - T = 0$. We apply the function $G_i(\cdot)$ to translate individuals' utility to a metric that can be aggregated across individuals. For example, W can be a weighted sum across individuals to allow for social preferences for redistribution. Due to the balanced budget, an increase in the robot tax rate t requires a decrease in income taxes, leading to an implicit function $\tau(t)$ where

$$\frac{d\tau}{dt} = -\frac{d[B-T]/dt}{d[B-T]/d\tau} \tag{7}$$

An increase in the robot tax rate t would then have the following welfare impact:

$$\begin{aligned}
\frac{dW}{dt} &= \sum_i G' \cdot V_C \left[\frac{\partial C}{\partial p}\Big(\frac{\partial p}{\partial t} + \frac{\partial p}{\partial \tau}\frac{d\tau}{dt}\Big) + \frac{\partial C}{\partial w}\Big(\frac{\partial w}{\partial t} + \frac{\partial w}{\partial \tau}\frac{d\tau}{dt}\Big) + \frac{\partial C}{\partial b}\Big(\frac{\partial b}{\partial w}\Big(\frac{\partial w}{\partial t} + \frac{\partial w}{\partial \tau}\frac{d\tau}{dt}\Big) + \frac{\partial b}{\partial \tau}\frac{d\tau}{dt}\Big)\right] \\
&= \sum_i G' \cdot V_C \Bigg[\underbrace{\left(\frac{\partial C}{\partial p}\frac{\partial p}{\partial t} + \Big(\frac{\partial C}{\partial w} + \frac{\partial C}{\partial b}\frac{\partial b}{\partial w}\Big)\frac{\partial w}{\partial t}\right)}_{\text{Denote by } MV_t\text{: direct effect of changing } t} + \underbrace{\left(\frac{\partial C}{\partial p}\frac{\partial p}{\partial \tau} + \Big(\frac{\partial C}{\partial w} + \frac{\partial C}{\partial b}\frac{\partial b}{\partial w}\Big)\frac{\partial w}{\partial \tau} + \frac{\partial C}{\partial b}\frac{\partial b}{\partial \tau}\right)}_{\text{Denote by } MV_\tau\text{: indirect effect through } \tau} \frac{d\tau}{dt}\Bigg] \\
&= \sum_i G' \cdot V_C \left[MV_t - MV_\tau \frac{d[B-T]/dt}{d[B-T]/d\tau}\right] \text{ by inputting eqn. 7} \\
&= \sum_i G' \cdot V_C \left[MV_t - \frac{MV_\tau}{d[B-T]/d\tau}\Big(\frac{\partial B}{\partial N}\frac{\partial N}{\partial t} - \frac{\partial T}{\partial N}\frac{\partial N}{\partial t} + \frac{\partial T}{\partial t}\Big)\right] \text{ by inputting eqn. 1}
\end{aligned} \tag{8}$$

The first line uses the envelop theorem so that any changes in N due to the tax parameters do not have a first order impact on welfare since individuals were already optimizing. The second row is simply reorganizing the change in welfare into two terms: the direct effect of a marginal change in the robot tax t and the indirect effect from a marginal change in the income tax rate τ. The last two lines use equations 1 and 7 to express the change in social welfare as a function of the behavioral and mechanical costs of raising the automation tax.

In deciding whether to raise or lower taxes, the social would need to know whether $\frac{dW}{dt} \gtrless 0$. From equation 8, this optimality condition is equivalent to the comparison

$$\frac{\sum_i G' \cdot V_C \cdot MV_t}{\sum_i G' \cdot V_C \cdot \frac{MV_\tau}{d[B-T]/d\tau} \frac{\partial T}{\partial t}} - 1 \gtrless \frac{\frac{\partial B}{\partial N} \frac{\partial N}{\partial t} - \frac{\partial T}{\partial N} \frac{\partial N}{\partial t}}{\frac{\partial T}{\partial t}} \tag{9}$$

Intuitively, the social planner is comparing the social marginal value of increasing the robot tax (LHS) against the fiscal externality (RHS). Equation 9 is a generalized version of the sufficient statistics formula derived by Costinot and Werning (2022): the benefits of a robot tax depend on its impact on prices, wages, and the income tax schedule (captured in MV_t and MV_τ), whereas the fiscal externality depends on the effect on the actions of households and firms that impact tax revenues. In this case, the fiscal externality only depends on labor supply decisions N, but it can depend more generally on other actions. If we add the same structure as Costinot and Werning (2022) to model how equilibrium wages and prices respond to the tax parameters, then we would recover the exact same sufficient statistics model as them.