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

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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 negative fiscal externality such that the government's net revenue remains unchanged for each tax dollar they mechanically collect from the policy. Together, the results suggest that a robot tax could be a tax-neutral policy for job creation.

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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). During our analysis period, employers in Korea that purchased robots and automation software were 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, 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 \$10-400 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 the robot subsidy on investments in automation and on employment. These elasticities are central to the argument by Acemoglu et al. (2020a) that states that in a tax system that is biased against labor, a tax on automation can be welfare improving by increasing employment and reducing capital. Second, we estimate the effect of the tax credit on income inequality. A key motivation for a robot tax is that it can redistribute wages from workers who are complements of automation technology to workers who are substitutes (Guerreiro et al., 2022; Thuemmel, 2022; Costinot and Werning, 2022). Hence it is important to measure empirically changes in the distribution of income across workers for an accurate understanding of the welfare consequences of a robot tax. Third, our paper 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 nearly 100% and investments in credit-eligible items more than doubled. These changes in investments manifested gradually, suggesting that firms make lumpy investment decisions that require years to adjust. Similarly, the fall in the tax credit rate in 2018 decreased take-up and investments by a comparable magnitude. At the same time, we observe a 3.8% increase in employment, especially among firms that likely reduced their investments in response to the policy. 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 fell primarily in the upper half of the income distribution. The tax reform had no impact on the bottom of the wage distribution, but lowered earnings for workers in the fifth to seventh deciles. The heterogeneous wage effects that we find are consistent with task-based models of robot adoption (Acemoglu and Restrepo, 2018), where robots replace workers for manual non-cognitive jobs, but increase labor demand in remaining occupations. Since automation is a complement to high-skill labor, the 2018 tax reform led to a decrease in wages at the top of the income distribution despite raising aggregate employment.

Third, we estimate the fiscal externality of the 2018 tax reform and find suggestive evidence of a fairly large deadweight loss from taxing automation. 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 actually lose an additional $1.34 due to employers' behavioral responses. We show that this fiscal externality is driven by a reduction in corporate taxes collected from businesses that is partially offset by an increase in income taxes due to the rise in employment. For comparison, our estimate of the fiscal externality is similar to the cost of the 1981 tax cuts for top labor income in the United States (Hendren and Sprung-Keyser, 2020). Overall, the negative externality suggests that a robot tax would be ineffective for raising tax revenues. However, it would nevertheless be a tax-neutral policy tool for creating new jobs and reducing wage inequality.

Our paper contributes to three strands of literature. First, our study provides one of the first empirical estimates of the effects of a tax credit for automation. While previous papers studying the welfare implications of a robot tax have modeled its key economic trade-offs (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, we study a setting that more closely reassembles a robot tax in the sense that it applies universally to all automation investments without a selective application process that may reward particularly productive firms.

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). To study the behavior of firms that purchase robots, other studies leverage an event-study design around the time of robot adoption by specific businesses (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 a tax reform to identify the aggregate labor market impacts of robots and automation.

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 the 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 Parameters for Evaluating the Welfare Impacts of 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 these mechanisms and identify three key sets of parameters of interest required to evaluate the welfare effects of a robot tax. The remainder of the paper is then devoted to the estimation of these parameters.

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 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 only marginally productive tasks. A critical prediction for the applicability of this framework is that a robot tax should (a) deter the adoption of automation technologies and (b) increase firms' employment levels. Thus, the elasticity of investments in automation technology and the elasticity of employment to changes in the robot tax rate are of key interest.

Second, multiple papers have argued that a robot tax can increase social welfare by reducing income inequality (Guerreiro et al., 2022; Thuemmel, 2022; Costinot and Werning, 2022). In general, these studies solve a Mirrleesian model whereby the government sets taxes to maximize the average utility across individuals of different types, subject to a feasibility constraint and a balanced government budget. The added dimension of unobserved worker types 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 workers for whom the technology is a substitute, leading to pre-tax redistribution.

Third, this redistribution comes at a deadweight loss in efficiency from changes in equilibrium employment, production, wages, and prices. 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, but these changes in behavior impose a first-order cost on the government's budget, called the fiscal externality.

To summarize, our paper estimates three sets of key parameters for evaluating the welfare impacts of a robot tax: the investment effect and employment effect, the income effect, and the fiscal externality. 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. Given this limitation, 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 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 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 these companies, 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. Similar to the "robot tax" discussed in the theoretical framework, this reduction in the tax credit rates effectively increased the tax burden imposed on firms investing in automation technology. Table 1 summarizes the changes in the tax credit rates during our study period.

Our empirical strategy will compare firms affected by the 2015 and 2018 tax rate changes to firms that were unaffected, leveraging policy variation by firm size. Key to our empirical strategy is that Korea's National Tax Service defines firms' size based on their average revenue in the 3 preceding years. For tax purposes, small firms in the manufacturing sector must have average annual revenue below a strict cutoff of 80, 100, or 150 billion Korean Won (KRW) per year depending on the specific sub-industry, and assets below 500 billion KRW.[1] Firms with revenues above their industry-specific cutoff are classified as medium if they do not belong to a conglomerate and large otherwise.

The revenue cutoffs enable us to construct comparable treatment and control groups. 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. Similarly, we identify the impact of the 2018 policy by comparing medium and large firms to small firms. Since very small and very large firms may not follow the same trends over time, we restrict the sample in both analyses to firms with baseline revenues of 50-200 billion KRW per year, thereby eliminating small mom-and-pop stores and the largest conglomerates in the country. Intuitively, the analysis of the 2018 reform compares firms with an average annual revenue right above the 80/100/150 billion KRW cutoffs to those right below the industry-specific thresholds.[2]

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. Specifically, the National Tax Service changed the way it defined firm sizes (see Table 2). As a result, many companies that were previously considered medium firms were reclassified as small. While this offers us additional variation in the tax credit rate across firms, the reclassification also affected other aspects of employers' taxes. Namely, Moon (2022) shows that reclassified firms experienced a reduction in capital gains taxes, resulting in an increase in investments. Given these overlapping policies, we do not use the variation from

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

[2] 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.

the reclassifications. Instead, we use exposure to the changes in the tax credit rates based on firms' baseline revenues and the post-2015 definition of firm size. By defining firm size in this way, reclassified firms are contained in our control group and would thus bias against finding any impact on investments from an increase in the robot tax credits.

Second, besides the tax credit for automation technology, firms also have the option of claiming other types of tax subsidies 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).

Third, in addition to the tax reform, the year 2018 also coincided with the introduction of new labor market policies. 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, but to prevent any confounding effects, we drop firms that would have been affected by the new work-hours law given their baseline employment levels.

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 industrial robots. 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.[3]

In terms of external validity to other countries, it is worth noting that South Korea has the highest robot usage per capita 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 10-400 billion won in 2014 and are continuously in operation throughout the study period. 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 onward, 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

[3] 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, even if reality may be more nuanced.

rates are determined by their size, which depends on 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 in 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 in 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 the data contains firms with 10-400 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 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 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 estimates in the right three columns suggest that the 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:

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

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:

$$Y_{it} = \beta(Treat_i * Post_t) + \alpha_i + \alpha_t + \varepsilon_{it} \tag{2}$$

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 follows 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 50 and 200 billion KRW at baseline.

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, using the full dataset. 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, we make two adjustments to the sample. First, 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, investments, and wages for the remainder of our analysis. Second, we restrict the data to firms with average baseline revenues between 50 and 200 billion won per year so that we are comparing employers with similar characteristics.

Using the condensed sample, Figure 2 plots the effect of the 2015 and 2018 tax reforms on take-up of the tax credit over time, estimated from equation (1). 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 by nearly 100% 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 less likely to claim the tax credit relative to small firms. Panels (c) and (d) show that changes to the probability of take-up translates to around a three-fold change 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 across 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 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 implies 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 plot the estimates of equation (1) showing the impact of the two tax reforms on capital investments eligible for the TCIPIT. The magnitude of the estimates suggest that about two-thirds of the change in the amount of tax credits claimed is explained by an investment response. The gradual response to the tax reform suggests that firms pre-commit to investments and take time to adjust their future capital stock. The new tax credit rates are announced in the summer of the year before they go into effect, so firms only have half a year to respond before each reform.

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, the amount of tax credit claimed, and investments using different variants of equation (2). Column (1) reports the corresponding estimates to Figure 2. 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 average impact for the 2015 reform relative to the 2018 one. Our estimates suggest that the 2015 reform increased take-up by 88% and increased investments in credit-eligible capital by 132%. In comparison, take-up only decreased by 43% in the 2 years following the 2018 reform, and investments fell by 107% relative to the counterfactual. In column (2), we find similar results after introducing industry-year fixed effects.[4] 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.

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, all our regressions drop firms with over 300 workers at baseline to restrict the set of employers to only those not covered by the new maximum workweek law. In addition, column (3) 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. Moreover, column (4) drops firms with over 20% of their workforce affected by the minimum wage change. In all instances, the estimated effects on take-up, claimed tax credits, and investment remain stable.

In Appendix Table 2, we show that our results are also robust to the choice of revenue bandwidth. To construct comparable treatment and control groups, our preferred specification restricts the sample to firms with average revenues between 50 and 200 billion won per year at baseline. The first two columns expand the revenue bandwidth to the right (i.e. 50-300) and to the left (i.e. 30-200), respectively, whereas the third column shrinks the bandwidth to 60-190 billion won. The last column includes the full sample of firms with 10 to 400 billion KRW revenue per year. In all cases, we find that take-up responds sharply to changes in the tax credit rate. Investments tend to move in the same direction as take-up, except we cannot reject a null investment effect from the 2018 reform when we extend the sample to the right or use the full sample. The heterogeneity in response by firm size is suggestive that relatively smaller (i.e. lower revenue) firms in the treatment group responded quicker to the reform by reducing investments.

To summarize, the stability of our estimates across a variety of specifications indicates that firms increase take-up and claims in response to more generous tax credits for automation technology. Accompanying this change in claims is a gradual change in investments that appears to be quicker

[4] The data only started recording firms' sub-industries after 2015.

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 3 plots the estimates of equation (1) using log(employment) as the outcome variable. Leading up to the 2018 reform, we find no differences in pre-trends between small and medium/large firms. Following the reduction in tax credits though, we find a gradual increase in employment among medium and large firms, similar to the gradual reduction in investments observed in the previous analysis.

Table 5 tests the robustness of the employment response to a series of alternative specifications that control for potentially confounding policies. Similar to the analysis on take-up, column (2) controls for industry-year fixed effects, column (3) controls for share-of-minimum-wage workers interacted with year fixed effects, and column (4) drops firms with over 20% share of its workforce earning below the 2018 minimum wage. 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 investments in capital also took time to adjust. However, we find evidence of a 3-4% increase in employment by 2019, the year after the decrease in tax credits.

Panel A of Appendix Table 3 shows that the employment effect is also robust to different revenue bandwidths used to define the sample. Interestingly, we find that the employment effect is smallest when we use the full sample of firms. This is consistent with our earlier observation that the decrease in investments is also weakest when we use the entire sample. The correspondence between the two outcomes supports the view that the change in employment is driven primarily by the decrease in the tax credits for automation.

Consistent with common arguments for a robot tax, our results suggest that a reduction in tax credits for automation increases the number of jobs. However, our finding of a negative relationship between capital investment and employment stands 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 chosen based on an application that may potentially be selected for 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 examine the aggregate labor market impacts of robot adoption (Acemoglu and Restrepo, 2020).

6.2 Workers' Earnings

6.2.1 Average Effect on Worker's Income

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 (1) using individual worker's earnings as the outcome variable.

Figure 4 plots the dynamic treatment effects over time. We find that workers' earnings in small and medium/large firms were growing at the same rate in the three years before the tax reform. Starting in 2018, we observe a decrease in workers' wages of about 2%, but it is not statistically significant. Table 5 panel (B) confirms that while workers' earnings appear to fall across multiple specifications, we cannot rule out a null effect. In comparison, Appendix Table 3 shows that the earnings effect is statistically significant in alternative samples where we expand the revenue restrictions to include more firms. Overall, there is some weak evidence of a decline in average wages.

In theory, the impact of a robot tax on workers' earnings is ambiguous. Acemoglu and Restrepo (2018) highlights two competing forces that act upon workers' wages. On one hand, reductions in automation can have a negative productivity effect, leading to lower wages for workers that complement robots. On the other hand, increasing the cost of robots can increase demand for workers who were substitutes to robots, leading to higher wages. While these two mechanisms imply ambiguous aggregate wage impacts, they have clear predictions on the effect of a robot tax on the distribution of wages. If higher skilled workers are more likely to be complements of robots, then the decrease in the tax subsidy should raise wages at the bottom of the income distribution and decrease wages at the top. Thus, the null effect on average wages may be masking important heterogeneities. We show evidence of this heterogeneous treatment effect in the next section.

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 increase social welfare by reducing income inequality. To empirically measure the redistributive properties of the 2018 tax reform, we estimate equation (2) 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. 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.[5]

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 benefit 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 changed workers' rankings. The key parameter for normative assessments is the distribution of wages, irrespective of workers' types or 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 5 plots the quantile treatment effects along each decile of the income distribution. Consistent with the predicted impacts of a robot tax, our quantile estimates suggest that cutting the tax credits for investments in robots succeeded in reducing wage inequality. However, it 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 appear to 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. Given that we observe a positive employment effect, it is entirely possible that the null effect on the bottom of the income distribution is due to a change in worker composition, with firms hiring more low-skilled workers. Unfortunately, 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.

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

6.3 Heterogeneous Impacts

We next explore how the labor market impacts of the 2018 tax reform varied by workers' age. Intergenerational 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. While we are underpowered to statistically detect differences between the three groups, the point estimates suggest that both the employment and income effects were stronger for young and middle age individuals. This would be inconsistent with the assumption of models where robot and automation primarily impacts older workers. However, a key distinction between our setting and models of intergenerational robot taxation is that we focus solely on the impacts of a robot subsidy within-manufacturing rather than across all sectors. Part of the response by young workers could be to select into industries that are unlikely to be targeted by automation. Thus, it could still be the case that a robot tax primarily affects the old because they are more likely to be in industries exposed to automation like manufacturing, but within manufacturing, the effect is stronger among the young who were unable to find jobs in other sectors. 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. While we are underpowered to statistically detect differences between the three groups, the point estimates suggest that both the employment and income effects were stronger for young and middle age individuals. This would be inconsistent with the assumption of models where robot and automation primarily impacts older workers. However, a key distinction between our setting and models of intergenerational robot taxation is that we focus solely on the impacts of a robot subsidy within-manufacturing rather than across all sectors. Part of the response by young workers could be to select into industries that are unlikely to be targeted by automation. Thus, it could still be the case that a robot tax primarily affects the old because they are more likely to be in industries exposed to automation like manufacturing, but within manufacturing, the effect is stronger among the young who were unable to find jobs in other sectors.

7 The Fiscal Externality

Following the discussion in section 2, a key parameter in determining the optimal tax level is its fiscal externality on the government's budget. In this section, we calculate the fiscal externality of the 2018 tax reform.

To measure the fiscal externality, 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 the sum of firms' corporate taxes and income tax to connect with our empirical results. The tax collected from a firm, $B(Y, \tau)$, depends on the workers' and firm's actions Y (e.g. employment, wages, and profits) 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:

$$\underbrace{\frac{d[T(Y(\tau,t),t) - B(Y(\tau,t),\tau)]}{dt}}_{\text{Total}} = \underbrace{\frac{\partial T}{\partial t}}_{\text{Mechanical}} + \underbrace{\left(\frac{\partial T}{\partial Y}\frac{\partial Y}{\partial t} - \frac{\partial B}{\partial Y}\frac{\partial Y}{\partial t}\right)}_{\text{Behavioral}} \tag{3}$$

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 transfer 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:

$$\frac{\text{Behavioral Effect}}{\text{Mechanical Effect}} = \frac{\frac{\partial T}{\partial Y}\frac{\partial Y}{\partial t} - \frac{\partial B}{\partial Y}\frac{\partial Y}{\partial t}}{\frac{\partial T}{\partial t}} \tag{4}$$

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' taxes.

We estimate equation (4) in three steps. First, to compute the total effect of the 2018 tax reform, we estimate our difference-in-difference regression with the outcome variable being the amount of government tax credits each firm receives minus the sum of their corporate and income taxes (i.e. $T - B$). The estimates from this regression 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 simulate the mechanical effect with 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 policy 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:

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

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.[6] The first column implies that the reduction in the robot tax increased the amount of government transfers to firms by a statistically insignificant 4 million KRW (i.e. approximately $4,000 USD). We decompose this change into

[6] The number of observations differ across each estimate because the mechanical and behavioral effects require a stacked regression. The total effect uses the same sample as in section 4.1 to estimate a difference-in-difference. However, following equation (5), the mechanical effect requires separately estimating both the difference-in-difference and the annual sample means for the treatment group. Lastly, the behavioral component requires stacking the data to jointly estimate both the total and mechanical effects in order to compute their difference using the Delta method.

the effect on corporate taxes and income taxes, but find insignificant effects along both margins. In contrast, the government should mechanically expect to save 13 million KRW per firm due to the reduction in tax credits. Computing the difference between the total and mechanical effects suggests that behavioral adjustments by firms cost the government an additional 17 million KRW per firm. Taking the ratio of the behavioral and mechanical costs reveals a fiscal externality of -1.34.

Our estimate of the fiscal externality implies that for each dollar that the government mechanically collects by reducing the robot subsidy, they lose an additional $1.34 in other taxes. In other words, the government is making negative revenues by reducing the subsidy. The decomposition of the total change in taxes suggest that part of the cost is due to a reduction in corporate taxes collected, suggestive of a decrease in profits. On the other hand, revenues collected from income taxes went up, consistent with the rise in employment. While the confidence intervals are fairly wide, our measure of the fiscal externality fall within the range of previous estimates looking at other tax changes. For example, Hendren and Sprung-Keyser (2020) calculate that the Reagan tax cut of 1981 and the 1993 tax increase on top earners had fiscal externalities of -1.51 and -0.46, respectively. Taken together, we interpret the results to suggest that a robot tax has little impact on the government's budget.

8 Conclusion

Concerns over the rise in inequality and the loss of manufacturing jobs have motivated policy proposals to tax 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, average wages fell due to a reduction in earnings among middle and high income workers, leading to a reduction in wage inequality. Third, a reduction in robot subsidies has a net zero impact on the government's budget. 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). Our findings suggest that a robot tax can create new jobs and reduce inequality, at minimal cost to the government.

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 differentiate between labor-substituting and labor-complementary capital.

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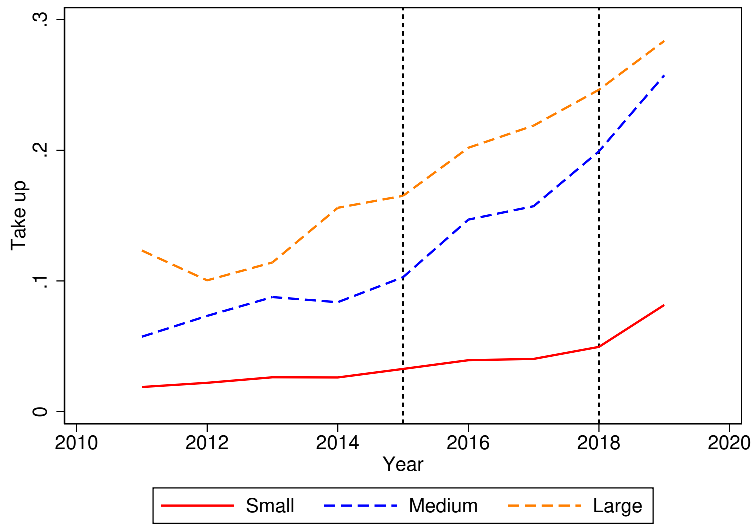
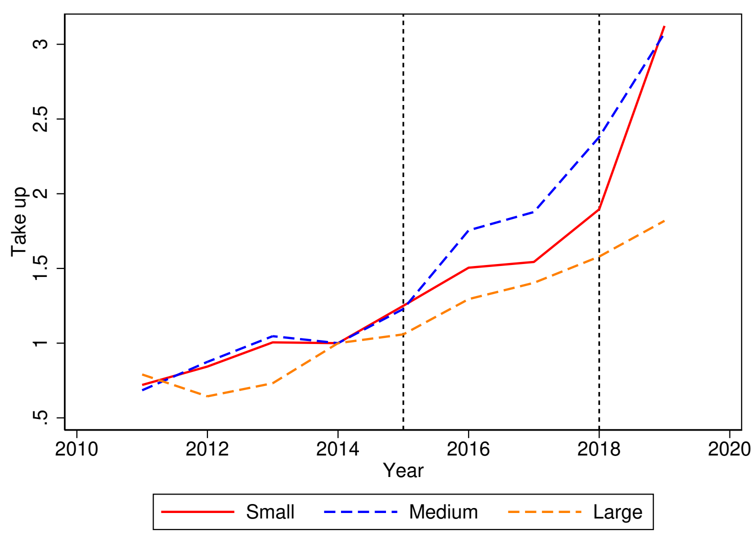
Figure 1: Take-up of Tax Credit Over Time



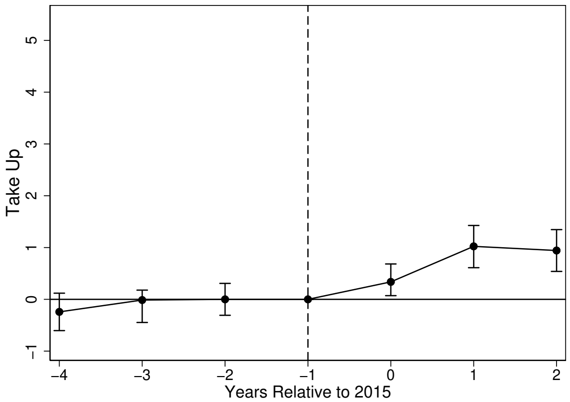
(a) Raw Trends



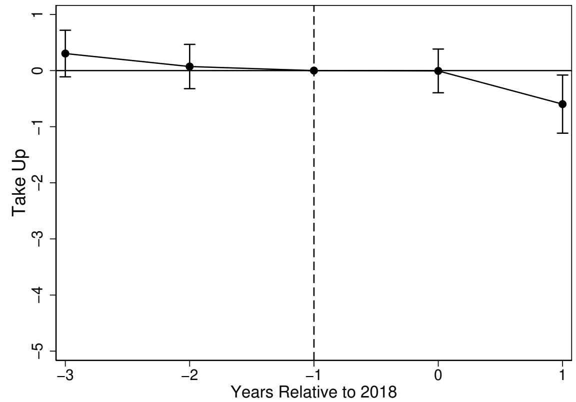
(b) Scaled by 2014 Averages

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.

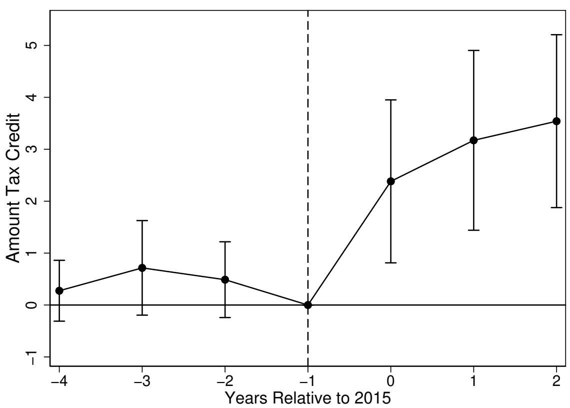
Figure 2: Effect on Investments and Tax Credits Claimed



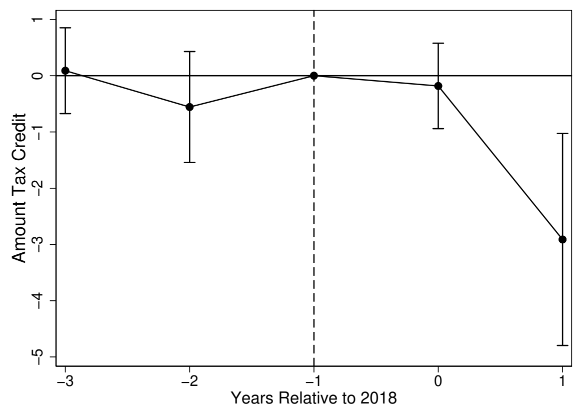
(a) Take-up (2015)



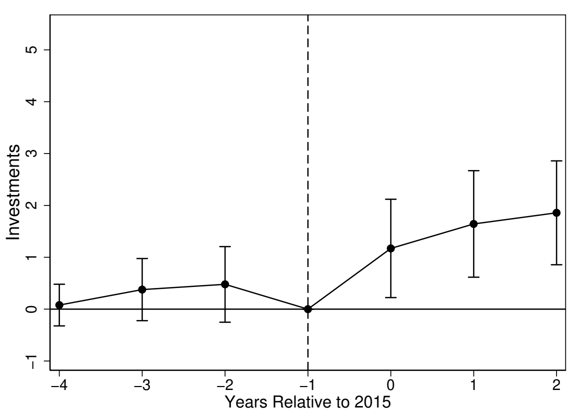
(b) Take-up (2018)



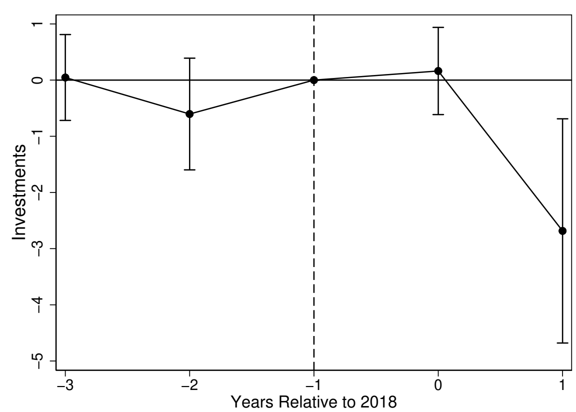
(c) Amount Claimed (2015)



(d) Amount Claimed (2018)



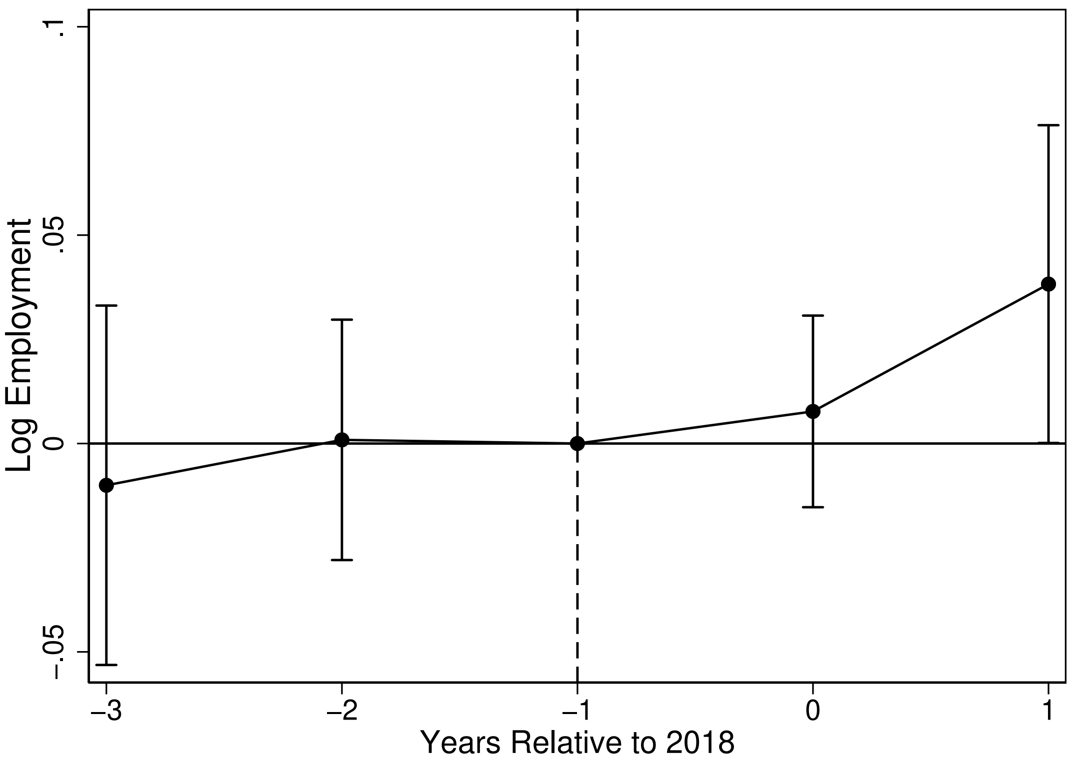
(e) Investments (2015)



(f) Investments (2018)

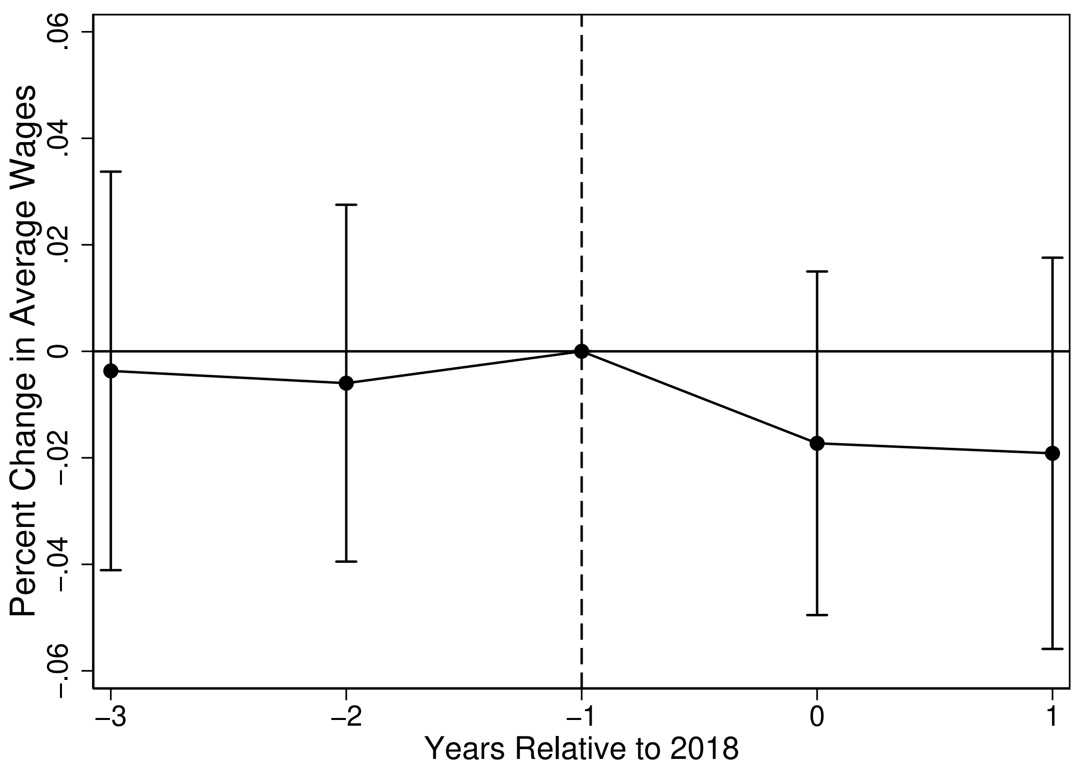
Notes: The figure plots the difference-in-difference estimates from equation (1) for take-up of the tax credit, amount of tax credit claimed, and investments in credit-eligible capital separately by the 2015 and 2018 tax reforms. The figure includes 95% confidence intervals using standard errors clustered by firm.

Figure 3: Effect of 2018 Reform on Employment



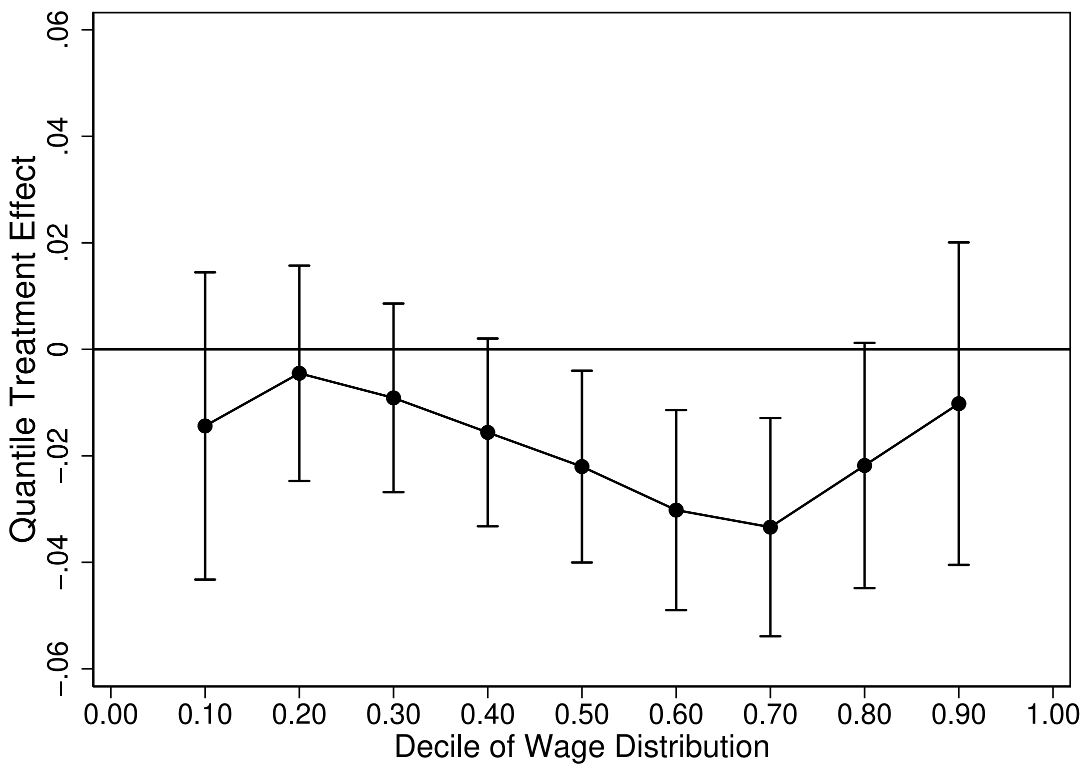
Notes: The figure plots difference-in-difference estimates from equation (1) for the effect of the 2018 tax reform on log employment. The figure includes 95% confidence intervals using standard errors clustered by firm.

Figure 4: Effect on Workers' Income



Notes: The figure plots difference-in-difference estimates from equation (1) for the effect of the 2018 tax reform on workers' earnings. 95% confidence intervals are computed using standard errors clustered by firm.

Figure 5: 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. 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

<table>
<tr><th></th><th>Before 2015</th><th>After 2015</th></tr>
<tr><td>Small</td><td>1) Number of workers <1,000

2) Capital ≤ 100 billion KRW

3) Revenue ≤ 100 billion KRW

4) Assets ≤ 500 billion KRW</td><td>1) Revenue ≤ 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 ≤ 500 billion KRW</td></tr>
<tr><td>Medium</td><td rowspan="2">There were only “small” and “general” firms.
If a firm is not “small”, then it is a “general” firm.</td><td>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</td></tr>
<tr><td>Large</td><td>Not small or medium firm</td></tr>
</table>

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	1.67	11.19	11.93	4.30	16.32	21.72
Taxes Payable	0.42	2.65	3.07	0.85	3.33	4.52
Profits	1.03	7.31	7.26	3.15	12.42	14.94
Take-up of Tax Credit						
Probability of Take-up	0.05	0.17	0.23	1	1	1
Credits as Share of Tax Payable	0.010	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. Monetary values are reported in billion won.

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

	(1)	(2)	(3)	(4)
PANEL A: t = 2011-2017				
Take-up	0.875^{***}			
	(0.152)			
Amount Claimed	2.661^{***}			
	(0.637)			
Investments	1.324^{***}			
	(0.394)			
N	11,767			
PANEL B: t = 2015-2019				
Take-up	-0.427^{**}	-0.436^{**}	-0.427^{**}	-0.437^{**}
	(0.190)	(0.215)	(0.190)	(0.202)
Amount Claimed	-1.389^{**}	-1.299^{**}	-1.389^{**}	-1.243^{**}
	(0.571)	(0.539)	(0.695)	(0.506)
Investments	-1.073^{*}	-1.048^{*}	-1.073^{*}	-0.949^{*}
	(0.601)	(0.589)	(0.601)	(0.544)
N	7,605	7,605	5,593	7,036
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Industry x Year FE		Y		
Share MW x Year FE			Y	Y
Drop MW > 20%				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 2. Column (2) controls for industry-year fixed effects. Column (3) controls for the share of workers in a firm at baseline that earned below the 2018 minimum wage, interacted with year fixed effects. Column (4) drops firms with over 20% of its workers affected by the 2018 minimum wage change. Standard errors are clustered by firm. *10%,** 5%,*** 1% significance level.

Table 5: Labor Market Effect of Robot Tax

	(1)	(2)	(3)	(4)
PANEL A: Log Employment				
$Treat * 2018$	0.008	0.001	0.008	0.007
	(0.012)	(0.012)	(0.012)	(0.013)
$Treat * 2019$	0.038^{**}	0.024	0.038^{**}	0.033^{*}
	(0.019)	(0.020)	(0.019)	(0.020)
$Treat * Post$	0.026	0.012	0.026	0.022
	(0.020)	(0.020)	(0.020)	(0.020)
N	7,605	7,605	7,603	7,036
PANEL B: %Δ Earnings				
$Treat * 2018$	−0.017	−0.025	−0.017	−0.019
	(0.016)	(0.021)	(0.016)	(0.017)
$Treat * 2019$	−0.019	−0.023	−0.019	−0.025
	(0.019)	(0.024)	(0.019)	(0.019)
$Treat * Post$	-0.015^{*}	−0.016	−0.015	-0.021^{**}
	(0.009)	(0.010)	(0.009)	(0.009)
N	927,123	927,123	926,748	856,495
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Industry x Year FE		Y		
Share MW x Year FE			Y	Y
Drop MW > 20%				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 1. Column (2) controls for industry-year fixed effects. Column (3) controls for the share of workers in a firm at baseline that earned below the 2018 minimum wage, interacted with year fixed effects. Column (4) further drops firms with over 20% of its workers affected by the 2018 minimum wage change. 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			
$Treat * Post$	0.011	0.032	0.002
	(0.032)	(0.020)	(0.020)
N	7,605	7,605	7,605
PANEL B: %Δ Earnings			
$Treat * Post$	-0.025	-0.018^{**}	-0.008
	(0.017)	(0.008)	(0.013)
N	180,536	566,775	179,700
Age	<30	30-50	> 50
Firm FE	Y	Y	Y
Year FE	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.

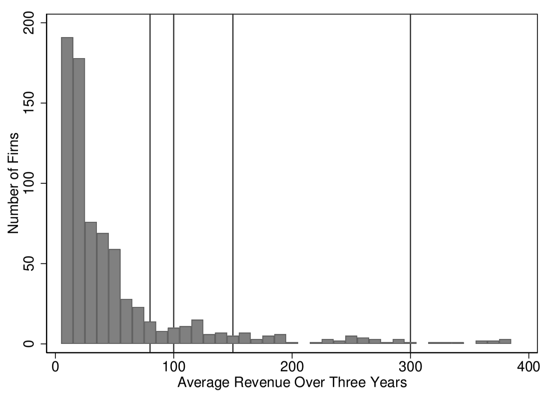
Table 7: Effect of Robot Tax on Fiscal Externality

	Total			Mechanical	Behavioral	Fiscal Externality
	Sum	Corp	Income			
	0.004	0.045	-0.040	-0.013^{***}	0.017	-1.340
	(0.074)	(0.067)	(0.024)	(0.001)	(0.042)	(3.245)
N	7,605	7,605	7,605	8,827	16,432	16,432

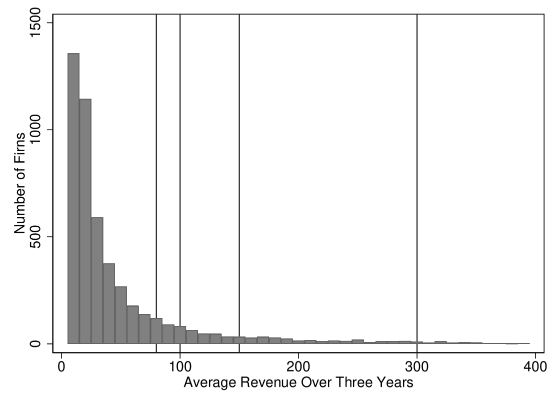
Note: This table reports the decomposition of the fiscal externality as described in section 7. Estimates are measured in billions of Korean won. Standard errors are clustered by firm. *10%,** 5%,*** 1% significance level.

Appendix A. Supplementary figures and tables

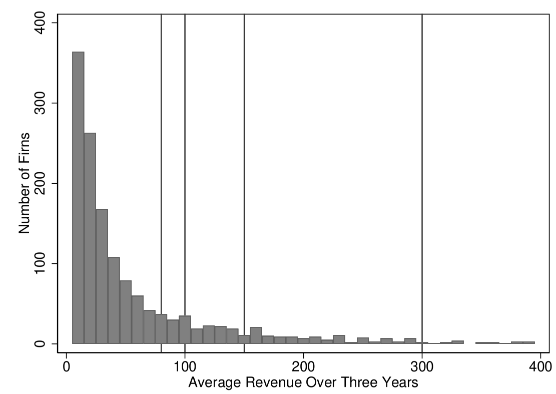
Appendix Figure 1: Revenue Distribution by Firm Size Threshold



(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 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, by Revenue Bandwidth

	(1)	(2)	(3)	(4)
PANEL A: t = 2011-2017				
Take-up	0.776^{***}	0.875^{***}	0.832^{***}	0.776^{***}
	(0.125)	(0.152)	(0.158)	(0.125)
Amount Claimed	2.330^{***}	2.661^{***}	2.874^{***}	2.330^{***}
	(0.480)	(0.637)	(0.776)	(0.48)
Investments	1.684^{***}	1.590^{***}	1.398^{***}	1.722^{***}
	(0.428)	(0.533)	(0.482)	(0.417)
N	13,230	20,951	8,918	48,657
PANEL B: t = 2015-2019				
Take-up	-0.392^{**}	-0.398^{***}	-0.742^{***}	-0.334^{***}
	(0.187)	(0.136)	(0.235)	(0.097)
Amount Claimed	-1.175^{**}	-1.114^{***}	-1.950^{**}	-0.670^{***}
	(0.573)	(0.322)	(0.791)	(0.176)
Investments	-0.760	-0.705^{**}	-1.678^{**}	-0.135
	(0.609)	(0.351)	(0.824)	(0.224)
N	8,160	14,390	7,360	32,450
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Bandwidth	50-300	30-200	60-190	10-400

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. Each column presents estimates from equation 2 for a different sample restriction on the range of baseline revenues. Standard errors are clustered by firm. *10%,** 5%,*** 1% significance level.

Appendix Table 3: Labor Market Effect of Robot Tax, by Revenue Bandwidth

	(1)	(2)	(3)	(4)
PANEL A: Log Employment				
$Treat * 2018$	0.013	0.011	0.005	0.007
	(0.011)	(0.011)	(0.014)	(0.009)
$Treat * 2019$	0.037^{**}	0.040^{**}	0.031	0.022
	(0.018)	(0.017)	(0.022)	(0.014)
$Treat * Post$	0.023	0.026	0.019	0.009
	(0.018)	(0.018)	(0.02)	(0.015)
N	8,160	14,390	5,593	32,450
PANEL B: %Δ Earnings				
$Treat * 2018$	-0.023^{*}	-0.026^{*}	-0.031^{*}	-0.024^{**}
	(0.013)	(0.015)	(0.017)	(0.011)
$Treat * 2019$	-0.020	-0.021	-0.027	-0.022^{*}
	(0.015)	(0.017)	(0.019)	(0.013)
$Treat * Post$	-0.023^{***}	-0.023^{***}	-0.031^{***}	-0.022^{***}
	(0.008)	(0.008)	(0.01)	(0.007)
N	1,140,724	1,732,443	805,163	2,985,895
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Bandwidth	50-300	30-200	60-190	10-400

Note: This table displays difference-in-difference estimates on the effect of the 2018 tax reforms on employment and wages. Each column presents estimates from equation 2 for a different sample restriction on the range of baseline revenues. 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 (4)) 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:

$$V_i(C(N, p, w, b(wN, \tau)), N) \tag{6}$$

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

$$W = \sum_i G_i(V_i) \tag{7}$$

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

$$\frac{d\tau}{dt} = -\frac{d[T - B]/dt}{d[T - B]/d\tau} \tag{8}$$

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

$$\begin{aligned}
\frac{dW}{dt} &= \sum_i G' \cdot V_C \left[\frac{\partial C}{\partial p}\Big(\frac{\partial p}{\partial t} + \frac{\partial p}{\partial \tau}\frac{d\tau}{dt}\Big) + \frac{\partial C}{\partial w}\Big(\frac{\partial w}{\partial t} + \frac{\partial w}{\partial \tau}\frac{d\tau}{dt}\Big) + \frac{\partial C}{\partial b}\Big(\frac{\partial b}{\partial w}\Big(\frac{\partial w}{\partial t} + \frac{\partial w}{\partial \tau}\frac{d\tau}{dt}\Big) + \frac{\partial b}{\partial \tau}\frac{d\tau}{dt}\Big)\right] \\
&= \sum_i G' \cdot V_C \Bigg[\underbrace{\left(\frac{\partial C}{\partial p}\frac{\partial p}{\partial t} + \Big(\frac{\partial C}{\partial w} + \frac{\partial C}{\partial b}\frac{\partial b}{\partial w}\Big)\frac{\partial w}{\partial t}\right)}_{\text{Denote by } MV_t\text{: direct effect of changing } t} + \underbrace{\left(\frac{\partial C}{\partial p}\frac{\partial p}{\partial \tau} + \Big(\frac{\partial C}{\partial w} + \frac{\partial C}{\partial b}\frac{\partial b}{\partial w}\Big)\frac{\partial w}{\partial \tau} + \frac{\partial C}{\partial b}\frac{\partial b}{\partial \tau}\right)}_{\text{Denote by } MV_\tau\text{: indirect effect through } \tau}\frac{d\tau}{dt}\Bigg] \\
&= \sum_i G' \cdot V_C \left[MV_t - MV_\tau \frac{d[B - T]/dt}{d[B - T]/d\tau}\right] \text{ by inputting eqn. 8} \\
&= \sum_i G' \cdot V_C \left[MV_t - \frac{MV_\tau}{d[B - T]/d\tau}\Big(\frac{\partial B}{\partial N}\frac{\partial N}{\partial t} - \frac{\partial T}{\partial N}\frac{\partial N}{\partial t} + \frac{\partial T}{\partial t}\Big)\right] \text{ by inputting eqn. 3}
\end{aligned} \tag{9}$$

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 (3) and (8) 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 (9), this optimality condition is equivalent to the comparison

$$\frac{\sum_i G' \cdot V_C \cdot MV_t}{\sum_i G' \cdot V_C \cdot \frac{MV_\tau}{d[B-T]/d\tau} \frac{\partial T}{\partial t}} - 1 \gtrless \frac{\frac{\partial B}{\partial N} \frac{\partial N}{\partial t} - \frac{\partial T}{\partial N} \frac{\partial N}{\partial t}}{\frac{\partial T}{\partial t}} \tag{10}$$

Intuitively, the social planner is comparing the social marginal value of increasing the robot tax (LHS) against the fiscal externality (RHS). Equation (10) 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.