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Essays On Taxation and Firm Behavior in Developing Countries

A dissertation submitted in partial satisfaction of the
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ABSTRACT OF THE DISSERTATION

Essays On Taxation and Firm Behavior in Developing Countries

by

Anh Thu Pham

Doctor of Philosophy in Economics

University of California, San Diego, 2015

Professor Roger Gordon, Chair

This dissertation examines tax policy and firm behavior in developing countries. The first chapter examines firm manipulation and take-up rate of a 30 Percent temporary corporate income tax cut in Vietnam. The second chapter examines how the very same tax cut program affects firm capital investment and reported profits. The third chapter studies the differential association of job training on labor market outcomes in STEM and non-STEM fields.

Chapter 1

Firm Manipulation and Take-up Rate of a Temporary Corporate Income Tax Cut in Vietnam

Abstract. This paper documents firm take-up rates and manipulation around the eligibility thresholds of a temporary corporate income tax cut in Vietnam in 2009 and in 2011. The tax cut program was only available for firms with less than certain employment or asset cutoffs. I use regression discontinuity design and difference in differences approach to compare firms right below and above the cutoffs. The take-up rate among eligible firms was about 40 percent. I do not find evidence for take-up among ineligible firms. In addition, I do not find evidence that ineligible firms manipulated to qualify for the tax cut. Therefore, the tax cut program did not occur additional costs because of ineligible firms or manipulation.

Did a temporary corporate income tax cut in Vietnam reach its targeted group of firms? Did the tax cut create additional cost from un-wanted distortion because of eligibility rules? This paper studies these two questions by examining (1) the take-up rate of the tax cut program among eligible firms and ineligible firms, and (2) whether ineligible firms manipulate around the eligibility thresholds to qualify for the tax cut.

A 30 percent temporary tax cut program in Vietnam was implemented in 2009, 2011, and the last quarter of 2008. The Vietnamese government hoped that the tax cut program would help the Vietnamese economy during the recent financial crisis. Only small and medium sized businesses were qualify for the tax cut. Small and medium sized businesses were firms with asset or employment levels less than a specific cutoff defined by the government. I assume that firms right before and after the eligibility thresholds were similar in the absence of the tax cut. I use regression discontinuity design and difference-in-differences approach method to document the take-up rate of eligible and ineligible firms around the cutoffs. I also examine number of firms right above and below the eligibility thresholds.

I find that eligible firms were more likely to receive the tax cut than ineligible firms. I do not find evidence that ineligible firms received the tax cut. At least on paper, ineligible firms did not obviously violate the tax law by leaving evidence to be ineligible and still claimed the lower rate. Eligible firms were around 40- 60 percent more likely to receive the tax cut than ineligible firms in 2009 and 25 percent more likely to receive the tax cut than ineligible firms in 2011. There could be a lot of reasons why the take-up rate in 2011 was smaller than that in 2009. First, the rule was more complicated in 2011, so maybe fewer firms were sure whether they were eligibility. The tax cut program in 2011 was announced in August of 2011, while the 2009 program was announced in December of 2008. Therefore, maybe fewer firms in 2011 were aware of the existence of the program than in 2009. Additionally, firms around the cutoffs in 2011 were in a much smaller size from those around the cutoffs in 2009. Smaller firms might be less likely to be aware of the existence of the tax cut.

In addition, I do not find evidence that ineligible firms manipulated around the eligibility thresholds to qualify for the tax cut program. Specifically, I do not find that there were more firms right below the cutoffs than right above the cutoffs. The lack of manipulation could be due to the timing of the program announcement. Firms might not have enough time to manipulate asset or employment variables. Alternatively, the benefit of manipulation might not be high enough to outweigh the cost of manipulation.

This paper uses survey data collected by the Vietnamese General Statistics Office, which is a government agency. It is mandatory that all registered firms in Vietnam answer the survey. Since the survey is conducted by the government, firm reporting incentives to the survey would be the same as to the tax administrators and other government agencies.

This paper contributes to the discussion about incomplete take-up of many public programs. Lack of information, complexity, and social stigma are various explanations (Bhargava and Manoli 2015) for incomplete take-up. A weak institution and corruption add another layer of uncertainty to the take-up rate of a public program. Benefits of acquiring a public program might be smaller than costs. Local official divert transfers from the intended recipients (Reinikka and Svensson, 2004; Olken, 2006), demand bribes to issue permits to eligible recipients (Svensson, 2003), and take bribes to issue permits to ineligible recipients (Bertrand et al., 2007). For example, Niehaus et al. (2013) shows that on average, eligible households were around 21 percent more likely to receive a below poverty card (BLP) than ineligible households. 70 percent of ineligible households got a BLP card, while13 percent of eligible households did not get the card. Awareness of the eligibility rules of the BLP program was low. Similarly, lack of program awareness and corruption and other factors made the take-up rate of the temporary tax cut in Vietnam an empirical question. This paper documents the take-up rate of a temporary 30 percent corporate income tax cut program in Vietnam among eligible and ineligible firms. In contrast to what is reported in Niehaus et al. (2013), this paper does not find evidence that the ineligible received the tax cut. This, in part, might be because firms in Vietnam had to routinely report assets and long-term employment figures, which were used for tax cut eligibility rules, to government agencies. Therefore, on paper, it is easier in the case of Vietnam to verify firm eligibilities.

This paper also adds to the literature that studies how individuals and firms manipulate running variables around the cutoffs to qualify for program benefits. Some papers on the tax literature are Onji (2009) in the case of Japanese firms and Saez (2010) in the case of US individual tax payers. This type of manipulation can create extra costs to the government. For example, Camacho and Conover.(2011) estimates that around 8 percent of the Colombian population had their poverty scores lowered to qualify for the poverty program, which created a significant cost to the Colombian government. This paper provides evidence that at least in 2009 and 2011, the temporary tax cut program did not create additional costs to the Vietnamese government because of manipulation.

1.1 Policy backgrounds and program eligibilities

Vietnam implements a flat corporate income tax rate. The tax rate has been decreasing overtime. The corporate income tax rate was 32 percentage points before 2003. It was 28 percentage points from 2003-2007, and decreased to 25 percentage points in 2008.

During the recent 2008-2009 financial crisis, the Vietnamese government introduced a stimulus package in the hope to prevent the recession from happening in Vietnam. One of its policies was a 30 percent corporate income tax (CIT) reduction for small and medium sized businesses in 2009 and the last quarter of 2008. The same reduction for small and medium sized businesses was implemented again in 2011 and in 2012, but not in 2010. My data spans until 2011. The decision for the 2008-2009 tax reduction was passed on Dec 11, 2008. The prime minister announced the reduction on Dec 3, 2008. The tax cut program in 2011 was announced on August 6, 2011. The program in 2012 was announced on July 30, 2012. To my understanding, a maximum of one year tax extension was the only other policy change among small and medium sized businesses during this time period.

In 2009, a firm was eligible for the 2008- 2009 tax reduction if it satisfied one of the two conditions on assets or labor. The total assets were less than or equal to 10 billion VND (500 thousand USD) at the time of registration. Alternatively, the average number of long-term employees (more than three months of employment) as of last quarter of 2008 was less than or equal to 300 employees. For a firm established after October 2008, it was the number of employees in the first month that the firm received revenue.

An instruction on how to calculate the eligibility for the 2008-2009 tax cut based on the average number of employees was issued in Jan 13, 2009. The method was as the following. Firm A had 302 employees on Oct 1st 2008. In November, it hired 2 workers. In December, it laid off 10 workers. So the average number of long-term employees as of last quarter of 2008 was 302+(2*2-10*1)/3=300. Therefore, firm A was qualified for the reduction in 2009 and the last quarter of 2008. Firm B registered in Oct 2008. The first day it received revenue was Dec 2008. The number of long-term employees as of Dec 31st, 2008 was 295. Firm B was eligible for the tax reduction in 2009 and the last quarter of 2008.

In 2009, the government redefined the definition of small and medium sized businesses. In commerce and service sectors, a small and medium sized firm has less than or equal to 100 average long-term employees OR its assets are less than or equal to 50 billion VND(2,500,000 USD). In non-service sectors, it is less than or equal to 300 average long-term employees OR its assets are less than or equal to 100 billion VND (5,000,000,000 USD). When the policy was re-implemented in 2011 and in 2012, the government used the new definition of small and medium sized businesses in 2009. There were a few exceptions. Firms in banking, real estate, lottery, finance, and insurance were not eligible for the tax cut regardless of their sizes. Revenue under special excise taxes was not eligible. Subsidiaries whose parents were not small and medium sized businesses were not eligible for the tax cut in 2011 and 2012. In addition, firms in agriculture, aqua-culture, textile, electronic compartments, and public constructions were always eligible regardless of their sizes. Firms in manufacturing that had more than 300 average long-term employees were also eligible. The government used number of employees or assets in 2011 to determine eligibilities in 2011 and in 2012.

Overall, the majority of firms in Vietnam were qualified for the tax cut program. In 2009 and in 2011, according to my calculation, there were around 95 percent of firms were eligible for the tax cut.

1.2 Data

1.2.1 Data description

I use a panel survey data of all active firms in Vietnam from 2000 to 2011. This annual survey is conducted by the Vietnamese General Statistics Office. It is mandatory that all registered firms in Vietnam answer the survey. The dataset has information about firm's balance sheet, income statement, and some basic tax variables such as corporate income taxes and value added taxes. Firm reporting incentives to the survey would be the same as to the tax administrators and other government agencies since the survey is collected by the government.

Most firms in Vietnam choose their fiscal year to be the same as the calendar year. For tax purposes, the deadline of last year's corporate income tax form is March 31st of this year. The survey is rolled out on March 1st every year to ask about last year's information. All survey must be returned to the statistical office by July 15. Therefore, it is reasonable to assume that information in the survey is relatively close to the actual numbers that firms report to the tax authority on March 31st. Any revision in the tax form after July is more likely not to appear in the survey.

The survey includes all independent firms, firm's branches that pay corporate income taxes independently, and firm's subsidiaries. Tax id is a firm's unique identifier. A firm and all its branches have the same tax-id.

Subsidiaries and their parents have different tax-ids, which make it impossible to distinguish subsidiaries from their parents using tax-ids alone. In this paper, each observation in one year is treated as an independent firm in that given year.

Each year, surveyed firms are categorized into two groups: type-A or type-B firms. All type-A firms get a long survey. Type-B firms get a short survey. Type-B surveys do not have questions about long-term employment and total assets, which are criteria to determine the tax cut eligibilities. A firm's total employees (both short term and long term employees) in a previous year determines if that firm is a type-A or type-B firm in a given year. For example, in 2010, type-A firms were firms with more than 30 employees in 2009 in big provinces such as Hanoi and Ho Chi Minh city. All firms in small cities were also type-A firms in 2010. Among firms less than 30 employees in 2009 in big provinces, 15 percent was randomly selected to get type-A surveys in 2010. The rest got type-B surveys. The number of employees that determines the type-A survey threshold (30 employees in 2010, for example) and the percentage of small firms in big cities get randomly chosen for type-A surveys (15 percent in 2010, for example) change annually. From 2004 until 2010, there were a mixture of both type-A and type-B firms. From 2000 to 2003, and in 2011, all firms were type-A firms.

1.2.2 Construction of relevant variables

a/ Calculated tax rates

Unfortunately, the survey does not provide information about whether a firm received the tax cut in 2009 or in 2011. To examine the firm take up rate, I construct firm tax rates from the survey and group firms into high tax group and low tax groups. I calculate the tax rate by dividing an annual amount of corporate income tax liability by an annual reported profits before tax. This calculated tax rate might not be the exact tax rate that a firm was actually responsible for paying to the government. It is because the observed before-tax reported profits in the dataset might not be the same as the firm's taxable profits. They could be different because of different accounting methods or noise in the survey. There might also be other deductions and differential tax treatments that a firms might be qualified for but I do not observe in the data. For example, firms might have some revenues coming from activities that are not subjected to regular taxes.

To examine how well the calculated tax rate in the survey data describes the true distribution of the tax rate by law, I plot histograms of the calculated tax rates in 2003, 2004, 2009, and 2011. There were some major changes in the statutory tax rates in these years, so the histograms should reflect these changes. The tax rate in 2003 was 32 percentage points, and it was 28 percentage points in 2004. Figure 1.1 show that the calculated tax rate in 2003 concentrates at 0 and 32 percentage points, and these numbers in 2004 were at 0 and 28 percentage points. Firms paying 0 tax-rate were loss-occurring or break-even firms. The three major rates in 2009 and in 2011 were 0, 17.5, and 25 percent. Figure 1.2 shows the histogram of calculated tax rates in 2009 and in 2011. The histograms show 3 peaks at 0 percentage points, 17.5 percentage points and 25 percentage points, which were consistent with the tax law in 2009 and in 2011. Thus, although the calculated tax rates might have noises, they still more or less reflect the distribution of the true statutory tax rates by the law.

I also group firms into high tax group and low tax group. Recall that in 2009 and in 2011, if a firm received the tax cut, it would pay at a corporate tax at a rate of 17.5 percentage points. A firm that did not get the tax cut would pay 25 percentage points. I use mid-points to group firms into low tax and high tax group. I assign a firm to be in a low tax group if its calculated tax rate was between 0 and 21.25 percentage points. A firm in a high tax group if its calculated tax rate was more than 21.25 percentage points.

In section 1.3.2, I explain in details how I use the calculated tax rates and high tax and low tax group to back out the take-up rate of the tax cut program.

b/Average long-term employees

The survey has the number of employees and assets on Jan 1st and on

December 31st of each year. I use the number of employees on December 31st of each year as a proxy for the number average long-term employees of that year. This figure is not the same as the average number of employees, which was used to determine the employment eligibility thresholds for the tax cut in 2008, 2009, and 2011. However, it is the best available data to determine the program eligibility based on their employment levels.

c/ Initial asset in 2009

I have a panel dataset of firms from 2000 until 2011. I define the initial asset of a firm is the first time the firm appears in the dataset, or it is the firm assets in 2000. In the sample of firms that I examine in 2009, the median firm age was 11 years old. Therefore, more than half of these firms were established before 2000. With inflation, it is more likely that observed assets in 2000 were greater than the true initial asset. Therefore, there might be firms that I categorize as ineligible in my sample was indeed eligible. Thus, the coefficient of the take-up rate among ineligible firms would be up-ward biased. The coefficient of differential take-up of the tax cut program between eligible and ineligible firms would be downward biased. The fact that I do not find evidence for take-up of the tax cut among ineligible firms makes me less worry about the up-ward biased results of the ineligible coefficient.

d/ Asset in 2011

Assets in 2011 are the asset figures on December 31st in 2011.

1.3 Empirical Strategy

This section describes the empirical strategy to test for (1) take-up of the tax-cut program among firms around the eligibility thresholds and (2) whether firms manipulate around the eligibility cutoffs to qualify for the tax cut.

I use the regression discontinuity design method to examine manipulation. I use difference in differences method to examine the take up rates of eligible and ineligible firms. I describe each method in turn and explain why I choose them.

1.3.1 Testing for manipulation: Regression Discontinuity Design

Following Lee and Card (2008)[1], I use a parametric regression with a low polynomial function of the distance of the running variable to the cutoff.

$$y_{it} = \alpha_0 + f(R_{it} - a_{it}) + \alpha_1.1[R_{it} \leq a_{it}] + X_{it} + \epsilon_{it} \tag{1.1}$$

where R_{it} is the running variable.

a_{it} is the cutoff value.

$f(.)$ is a polynomial function of the distance of the running variable to the cutoff.

ϵ_{it} is an error term.

If a firm has $1[R_{it} \leq a_{it}]$ equals 1, that firm is eligible. If it equals 0, that firm is not eligible.

X_{it} : vector of co-variates such as firm ages, ownership dummies, province dummies, and industry dummies.

In regressions that examine manipulation around employment cutoffs, y_{it} represents the difference between the number of firms in the tax cut year and in the non tax cut year at a specific employment level. For example, if there were 5 firms that had 250 employees in the tax cut year and 3 firms that had 250 employees in the non tax cut year, $y_i t$ at the 250 employee level would be 2 firms. I use this variable to difference out any heaping at employment levels such as 250 or 300, etc.

In regressions that examine asset cutoffs, y_{it} is the number of firms per thousand VND in asset levels in the tax cut year. For example, if there were 5 firms at 10 million VND, y_{it} at 10 million VND in assets would be 5 firms. I do not take the difference per thousand VND asset level because it is not common to exist non-zero number of firms at a thousand VND asset level in

[1] I choose this test instead of McCrary 2008 because Lee and Card (2008) deals with discrete running variables in a parametric specification, which also works better when the sample size is small. McCrary(2008) needs a large sample and continuous running variable. The method in Lee and Card(2008) method is summarized in Lee and Liemieux (2010) and is used in Bharadwaj et al.(2012)

both the tax cut year and in the non-tax cut year. There were significantly less firms around the asset cutoffs than around the employment cutoffs. In addition, heaping at any thousand VND asset level does not seem to be a problem in the sample of firms I examine around the asset thresholds.

If ineligible firms manipulate assets or number of employees to qualify for the tax cut program, I expect more firms right below the cutoffs than right above the cutoffs. Thus, α_1 in this regression would be significantly greater than 0.

1.3.2 Testing for Differential Take-up among Eligible and Ineligible: Difference in Differences Approach

$$\begin{aligned} y_{it} = \beta_0 + \beta_1 year_1 + \beta_2.1[R_{it} \leq a_{it}] + f(R_{it} - a_{it}) + f(R_{it} - a_{it}) * year_1 \\ + \beta_3.1[R_{it} \leq a_{it}] * year_1 + X_{it} + \epsilon_{it} \end{aligned} \quad (1.2)$$

$year_1$ equals 1 in the tax cut year. It equals 0 in the non tax cut yeasr.

y_{it} could be the calculated tax rate, or the whether a firm is in a high tax group or low tax group (as defined in section 1.2.2).

a/ y_{it} is the calculated tax rate

If y_{it} is the calculated tax rate, constant term β_0 represents the average calculated tax rate in percentage points of ineligible firms in the non tax cut year. Therefore, the statutory reduced rate would be $0.3{*}\beta_0$ percentage points.

β_1 is the percentage point difference in tax rates of ineligible firms in the tax cut year and in the non tax cut year. Assume that the distribution of calculated tax rates in these two years are the same in the absence of the tax cut program, β_1 represents the percentage points reduction in the calculated tax rate of ineligible firms because of the tax cut program. As long as there are no other changes in the tax law during the two years, this assumption might be a reasonable Thus, the take-up rate among ineligible firms is $-100{*}\beta_1/(0.3{*}\beta_0)$ percent (since β_1 is presumably negative).

β_2 is the differential calculated tax rate between eligible and ineligible firms in the non tax cut year in percentage points. Eligible firm's calculated tax rate in the non tax cut year is $\beta_0 + \beta_2$ percentage points.

Coefficient β_3 is the percentage points reduction of eligible firms above and beyond what the reduction (β_1 percentage points) of ineligible firms. Therefore, the tax rate of eligible firms in the tax cut year is $\beta_0 + \beta_1 + \beta_2 + \beta_3$ percentage points. The take up rate of eligible firms is $-100 * (\beta_1 + \beta_3)/(0.3 * (\beta_0 + \beta_2))$ percent.

b/ y_{it} **is the probability that a firm is in a high tax group.**

y_{it} equals 1 if a firm is in a high tax group, and it equals 0 if a firm is in a low tax group.

β_0 represents the fraction of ineligible firms in a high tax group in non tax cut years.

Assume that the fraction of firms in the high tax group in non tax cut years is the same in tax cut years in the absence of the tax cut program. The only thing that can make the calculated tax rate distribution shift to the left, or make the fraction of firms in the high tax group decrease, was the tax cut program. $\beta_0 + \beta_1$ is the fraction of ineligible firms in the high tax group in the tax cut years. If ineligible firms claim lower tax rates, the fraction of ineligible firms in the high tax group decrease by β_1 in the tax cut years. Thus, the take-up rate among ineligible firms is $-100 * \beta_1/\beta_0$ percent (because β_1 is presumably negative).

Fraction of eligible firms in a high tax group in the non tax cut years is $\beta_0 + \beta_2$. Fraction of eligible firms in a high tax group in the non tax cut years is $\beta_0 + \beta_1 + \beta_2 + \beta_3$. In other words, fraction of eligible firms in the high tax group decreases by $\beta_1 + \beta_3$ in the tax cut year. The take-up rate of the tax cut program among eligible firms is $-100 * (\beta_1 + \beta_3)/(\beta_0 + \beta_2)$ percent.

1.4 Results

1.4.1 Treatment: How good does the eligibility predict the existence of the program

In this section, I present results of how well the asset and employment eligibility thresholds predict take-up rates of the tax cut in 2009 and in 2011. The tax cut program in 2009 used the 2008 level of employment or the initial assets at the time of registration to determine eligibilities. The program in 2011 used the 2011 levels of employment and asset. Since the eligibility requirement is either assets or employment, when I examine firm behavior around the employment thresholds, I restrict the sample to firms strictly greater than the asset cutoffs. Similarly, when I examine firm behavior around the asset thresholds, I restrict the sample to firms strictly greater than the employment cutoffs.

Generally, firms right below the thresholds on average were more likely to receive the tax cut than firms right above the thresholds. The results around the employment thresholds are stronger and more consistent across specifications than results around the asset thresholds. In general, there were significantly more firms around the employment cutoffs than around the asset cutoffs. In addition, I only observe a discrete jump in the take-up rates of eligible firms when I use the 2009 employment level as a running variable, even though the law uses 2008 employment level. Other running variables such as 2008 employment level, 2011 employment level, initial assets, and 2011 assets do not create discrete jumps in firm take up across the thresholds.This could be because variables in the survey are noisy proxies of true running variables. The employment level in December 31st of 2009 might be a better proxy of the average long term employment level in 2008 than this figure in December 31st, 2008. It could also be that firms mis-taken that the employment level in 2009, instead of 2008, was used for the eligibility determination.

I do not find evidence that ineligible firms received the tax cut. At least on paper, ineligible firms did not obviously violate the tax law by leaving evidence to be ineligible and still claimed the lower rate. Eligible firms were around 40- 60 percent more likely to receive the tax cut than ineligible firms in 2009 and 25 percent more likely to receive the tax cut than ineligible firms in 2011.

Employment threshold in 2009

Figure 1.3 plots the take-up rate of the tax cut program in 2009 among eligible firms and ineligible firms. Firms are grouped into 5 employment level bins. For example, firms between 296 and 300 employees are grouped into the 255 employment level bin. Firms between 301 to 305 employees are grouped into the 305 bin. The average take-up rate at each employment bin in 2009 equals (fraction of firms in that bin in the low tax group in 2009 minus fraction of firms in that bin in the low tax group in 2007) divides by fraction of firms in the high tax group in 2007. This calculation is analog to the calculation in section 1.3.2. I choose fraction firms in low tax group, instead of the calculated tax rate, to calculate the take-up rate because the fraction figure is less noisy than the calculated tax rate. Therefore, it is better for visualization purposes.

Figure 1.3(a) plots the number of long-term employees in 2009. Figure 1.3(b) plots the number of long-term employees in 2008, which was used by the law. The y axis presents the fractions of firms receiving the the tax cut in 2009. The x axis is the distance between the number of employees and the employment cutoff, which was 300, in 2009. On average, eligible firms were more likely to be in a lower tax group than ineligible firms. Thus, they were more likely to receive the tax cut. Figure 1.3(a) also shows that threshold crossing using the 2009 employment level creates a jump in the take-up rate.

Figure 1.3(b) shows that, on average, eligible firms determined by the 2008 employment level were also more likely to be in a low tax group. In other words, they were more likely to receive the tax cut than ineligible firms. However, there was no discrete jump after the employment threshold in 2008.

Table 1.2 and 1.3 provides regression analogs of figure 1.3 (a) and (b). The tables use equation 1.2. Column(1) of two tables have 0 polynomial function of the distance between the running variable value and the cutoff.

Specifically, $f[R_{it} - a_{it}] = 0$. Column 1 do not include any control variables. Similar to 1.3(a) and (b), column 1 of table 1.2 and 1.3 confirm that on average, eligible firms were less likely in the high tax group (or more likely in the low tax group) than ineligible firms. For example, in column 1 table 1.2, the interaction term between the indicator of being below the cutoff in 2009 and year 2009 has a coefficient of -0.169. This means that eligible firms, on average, were 16.9 percentage points less likely to be in a high tax group, or 16.9 percentage points more likely to be in a low tax group in the tax cut year, than ineligible firms. Column 1 table 1.3 implies that this difference between eligible firms and ineligible firms was 18.6 percentage points when I use the 2008 employment level.

Column 2 in table 1.2 and 1.3 has the distance function of the running variable in first order polynomial. In other words, $f[R_{it} - a_{it}] = R_{it} - a_{it}$. Column 3 is similar to column 2 with an addition of control variables such as age, ownership, province, and industry dummies. The coefficients of the interaction term between the indicator of being below the cutoff and the tax cut year are not significant in column 2 and 3 in table 1.3. Thus, I do not find evidence of a discrete jump of the take-up rate across the 2008 employment threshold.

Coefficients of the interaction term in column 2 and 3 of table 1.2 are significantly different from 0. This provides evidence that crossing the 2009 employment threshold created a discrete jump in the take-up rate of the tax cut in 2009. This implies that even though the law used the 2008 employment level for eligibility, the 2009 long term employment level was a better proxy of the average long term employment level in 2008. Alternatively, firms might have mis-interpreted that the government used the number of employees in 2009 to determine eligibility.

Coefficient of variable year2009 represents the reduction in the fraction of ineligible firms in a high tax group due to the tax cut program. It is the coefficient β_1 in equation 1.2. These coefficients were insignificant across different specifications in table 1.2 and 1.3. Therefore, I do not find evidence that ineligible firms claim a lower tax rate during the tax cut year.

The coefficients on being less than the cutoff, or being eligible, are also insignificant. Therefore, in the non-tax cut year, eligible firms, on average, was not more likely to be in a high tax group than ineligible firms.

I use column 1 of table 1.2 and 1.3 to calculate the average take-up rate of the tax cut program. According to section 1.3.2, the take-up rate among eligible firms in 2009 was 0.169/46.1= 37 percent if I use number of employees in 2009 as a running variable. The take up rate using number of employees in 2008 is 0.186/0.461= 40 percent.

I run the same regressions using the calculated tax rate as the dependent variable in table 1.7. I restrict the sample to only firms paying positive tax rate, which eliminate the bottom 5 percent of the calculated tax rates in the sample. I also drop the top 5 percent of the calculated tax rates in the sample, to make the trimmed sample symmetric. I do not plot the calculated tax rate because the data is too noisy to provide good visual. I only run 0 polynomial regression with control variables as described in equation 1.2. In other words, $f(R_{it} - a_{it}) = 0$. Column 1 and 2 in table 1.7 present the results using the 2009 employment level. The results of the take-up rate are consistent with the results using high tax group and low tax group. The calculated tax rate of eligible firms was 3 percent points lower than that of ineligible firms as a result of tax cut. I do not find evidence that ineligible firms in the tax cut year had lower calculated tax rates than in the non-tax cut year. Eligible firms, on average, did not have different calculated tax rates than ineligible firms in the non tax cut year. Therefore, according to section 1.3.2, the take-up rate in 2009 among eligible firms was 0.03/(0.3*0.169)= 59 percent.

Initial asset threshold in 2009

Figure 1.4, table 1.4, and column 3 and 4 of table 1.7 show the take-up rate of the program in 2009 by initial assets. Figure 1.4 shows that there was no discrete jump across the initial asset threshold. Table 1.4 does not provide consistent evidence for the differential take-up rate among eligible firms and ineligible firms. Column (3) and (4) of table 1.7 show the results when the calculated tax rate is the dependent variable. It is because the coefficients of the interaction term are insignificant. The noisy estimates might be because there were too few firms around the initial asset threshold.

Employment threshold in 2011

Figure 1.5 , table 1.5, and column (3) and (4) of table 1.8 present the take up rate of the tax cut program among eligible and ineligible firms around the employment eligibility threshold in 2011. All coefficients indicate that eligible firms were more likely to receive the tax cut than ineligible firms. However, crossing the employment threshold does not create a discrete jump in the program's take-up rate.

I do not find evidence that ineligible firms received a tax cut. The take-up rate of eligible firms was (0.177/(0.601+0.0819))=26 percent according to column 1 in table 1.5. According to column 1 table 1.8, the take-up rate of eligible firms was 0.016/(0.3*0.199)=26.7 percent.

Asset threshold in 2011

Figure 1.6 , table 1.6, and table 1.8 column 3 and 4 present the take up rate of the tax cut program among the eligible and ineligible around the asset eligibility threshold in 2011. Results are mixed.

Figure 1.6 and table 1.6 show results for high tax group and low tax group. Figure 1.6 does not show there was a jump in take-up across the asset threshold. Table 1.6 column 2 and 3 do not show the evidence for a jump across the threshold either. Table 1.6 show that, on average, eligible firms were 24.4 percentage points less likely to be in a high tax group than ineligible firms. Therefore, the take-up rate of eligible firms in 2011 were 24.4/63.2= 38.6 percent.

On the other hand, table 1.8 column 3 and 4 do not find evidence that eligible firms on average had lower calculated tax rates in 2011.

1.4.2 Manipulation around the Eligibility Threshold

From section 1.4.1, mostly I do not find evidence for a discrete jump in the program take-up across the thresholds. Only crossing the 2009 employment threshold creates a jump in the take-up rate. However, it is clear that firms right below the employment threshold were on average more likely to receive the tax cut than firms right above the employment threshold. Therefore, if ineligible firms manipulated the running variables to qualify for the tax cut, I should be able to also detect manipulation using the same variables. If ineligible firms manipulated the running variables to qualify for the tax cut, there would be more firms right below the threshold than right above the threshold.

I examine firm manipulation along the employment threshold in 2009 that had initial assets greater than the cutoff of initial asset in 2009. Since the initial assets were assets of firms at registration, the majority of firms cannot manipulate this figure, unless they were new entrants in 2009. There were only 2 entrants in the sample of firms I examine. Therefore, in 2009, I only examine manipulation around the employment eligibility threshold. In 2011, I examine manipulation around both the employment and asset thresholds. When I examine manipulation around the employment threshold in 2011, I restrict the sample to firms that were greater than the asset requirement in 2011. When I examine manipulation around the asset threshold in 2011, I restrict the sample to firms that were greater than number of employees requirement in 2011.

In terms of graphing, I construct empirical distributions of firms around the long-term employment eligibility threshold. I group firms into employment bins of 5 long-term employees. For example, firms between 296 to 300 long-term employees are in the bin of 300 employees. Firms between 301 and 305 long-term employees are in the bin of 305 employee bin. This grouping method ensures that eligible and ineligible firms are not in the same bin.

In terms of regression, to take care of heaping at number of employees that are divisible by 5, I take the difference between the number of employees in 2009 and the number employees in 2007 per employment level. This method assumes that the heaping patterns in 2009 and in 2007 are the same in the absence of the tax cut. I run the same analysis around the employment threshold in 2011. The regression equation is equation 1.1. I examine whether there are more firms before the cutoff than after the cutoff.

There were very few firms around the asset cutoff, and there were no heaping around the asset eligibility cutoff in 2011. Therefore, I do not compare the asset figures in 2011 with the asset figurse in 2007. I strictly compare number of firms right below and right above the asset cutoffs in 2011.

Figure 1.7 , table 1.11, and table 1.12 present the result of manipulation around the employment eligibility cutoff in 2009. Table 1.9 and 1.10 presents the result of manipulation around the employment cutoff in 2008. Neither could find evidence of manipulation. Figure 1.8, table 1.13, and table 1.14 present the results of manipulation around the employment cutoff in 2011. Figure 1.9 and table 1.15 present manipulation results around the asset cutoff in 2011. Neither could find evidence of manipulation around both of these cutoffs.

1.5 Conclusion

I find that the take up rate among eligible firms in 2009 was 40 to 60 percent. The take up rate among eligible firms in 2011 was around 25 percent. I do not find evidence that ineligible firms received the tax cut. In addition, I do not find evidence that ineligible firms manipulate around the eligibility cutoffs to qualify for the tax cut.

In conclusion, the recent 30 percent temporary corporate income tax cut in Vietnam did not 100 percent reach the eligible firms. The tax cut program did not create additional costs to the Vietnamese government due to ineligible firms claimed the tax cut or ineligible firms manipulated around the thresholds to qualify.

Table 1.1: Eligibility Cutoffs in 2009 and 2011

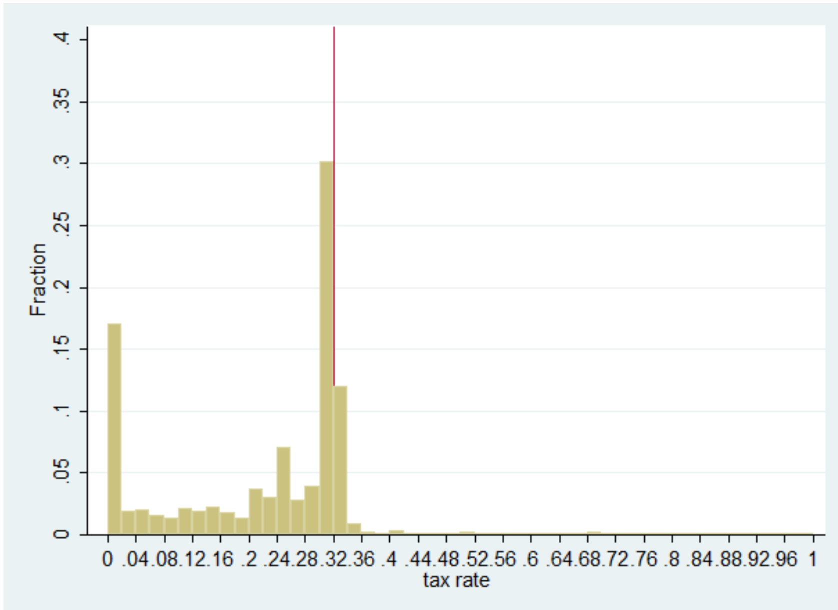
Year	Average long-term employees	OR	Asset
2009 all sectors	≤300 in 2008	OR	initial assets: ≤10
2011 service sector	≤100 in 2011	OR	≤50 in 2011
2011 non-service sector	≤300 in 2011	OR	≤100 in 2011

asset unit is in billion VND.

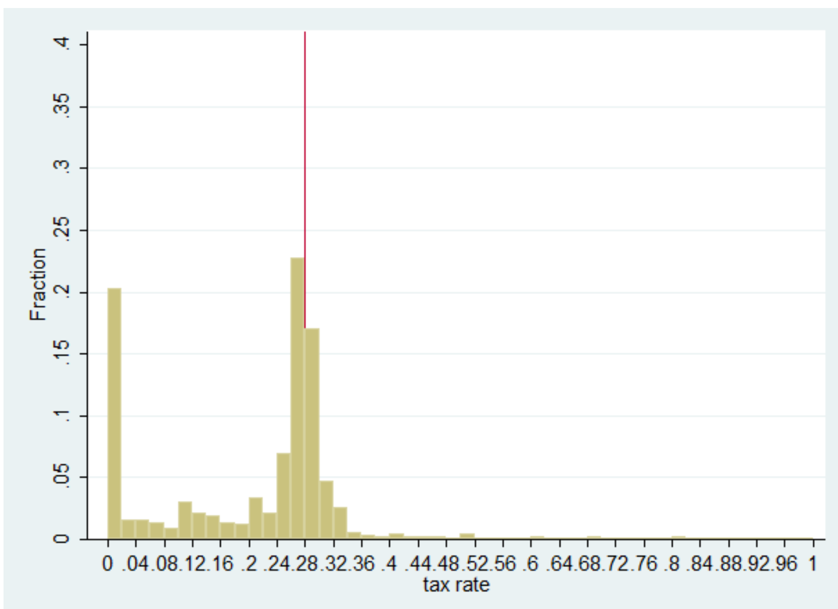
Table 1.2: Fraction of firms in a high tax group in 2009 around the employment cutoff in 2009

	0th	1st	1st & control variables
year2009	-0.0711	0.0111	-0.0231
	(0.0472)	(0.0564)	(0.0603)
≤ cutoff in 2009	0.0418	0.108	0.0352
	(0.0540)	(0.102)	(0.104)
≤ cutoff in 2009 & year2009	-0.169***	-0.339***	-0.236*
	(0.0622)	(0.113)	(0.123)
Constant	0.461***	0.430***	0.413***
	(0.0410)	(0.0535)	(0.0963)
N	887	887	885

Standard errors in parentheses. Standard errors are clustered at the employment level. A high tax group includes firms with calculated tax rate greater than 21.25 percent. Difference in differences approach uses equation 1.2. The base year is in 2007.Control variables are firm ages, ownership types, province, and 2 digit industry dummies. The sample includes firms between 50 employees from the left and the right of the cutoff in 2009, and the initial assets were greater than the initial asset cutoff. The cutoff values are listed in table 1.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$



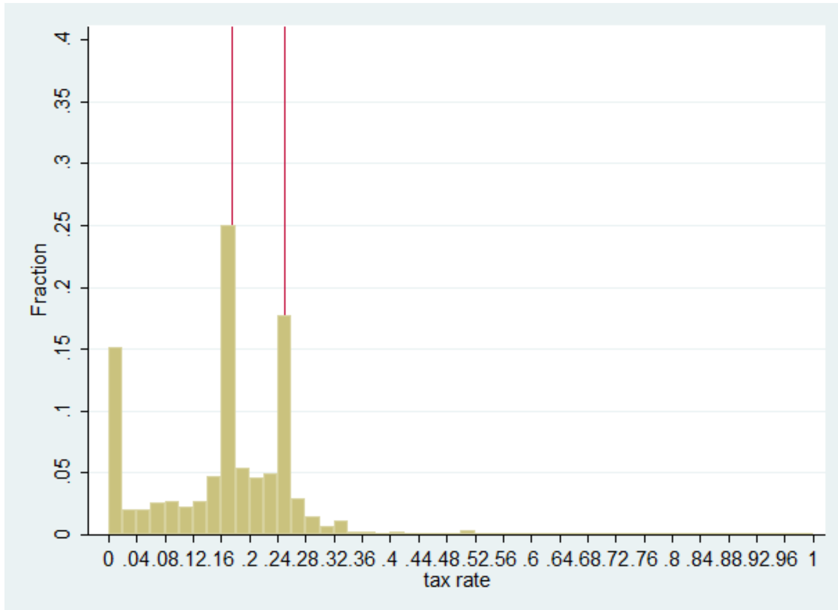
(a) 2003



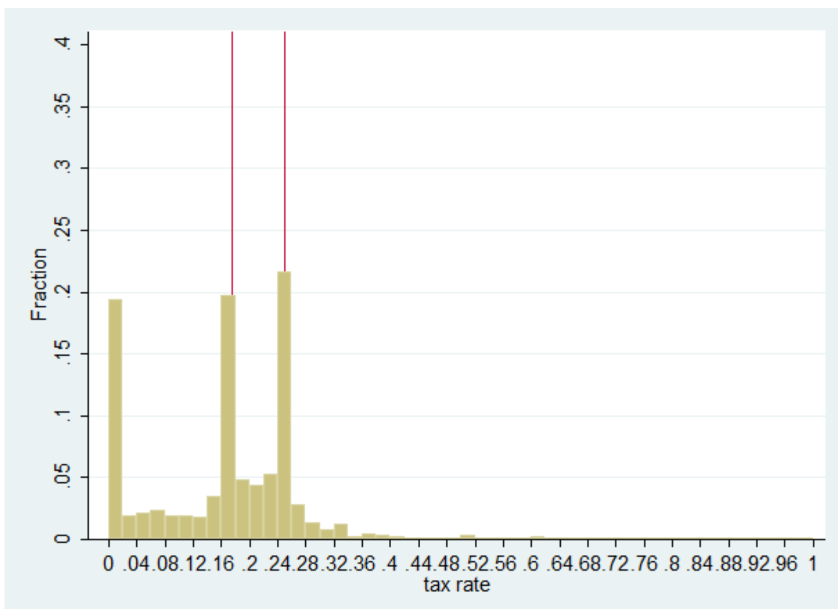
(b) 2004

Figure 1.1: Tax rate distributions in 2003 and in 2004

Note: The tax rate in 2003 was 32%. The tax rate in 2004 was 28%



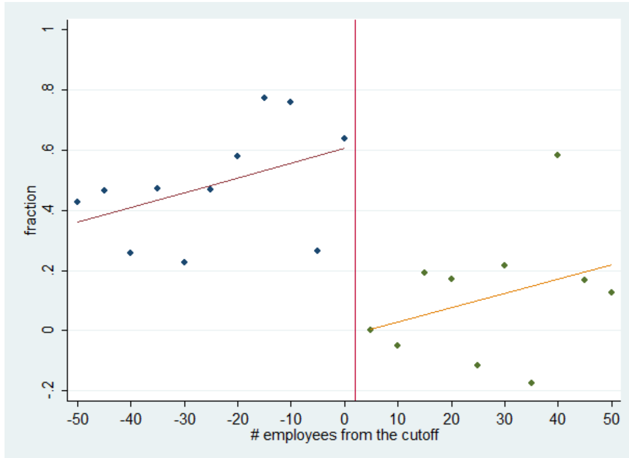
(a) 2009



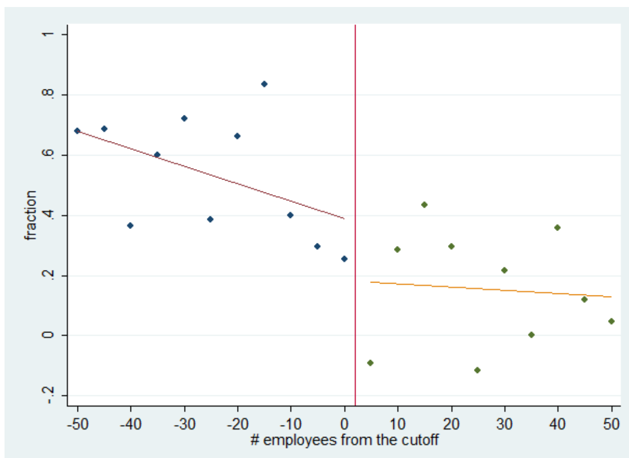
(b) 2011

Figure 1.2: Tax rate distributions in 2009 and in 2011

Note: The normal tax rates in 2009 and in 2011 were 25%. The reduced rates were 17.5%



(a) The number of employees in 2009



(b) The number of employees in 2008

Figure 1.3: Take-up in 2009 around the employment threshold

Note: The sample includes firms with initial assets greater than the initial asset cutoff in 2009, and within 50 long-term employees distance from the left and the right of the cutoff. The cutoff values are listed in table 1.

X axis is the difference between the number of employees and the employee cutoffs in 2009. Firms are grouped into 5 employment level bins. For example, if the difference is between -4 and 0 employees, firms are grouped into the 0 employment level bin. If the difference is between 1 to 5 employees, firms are grouped into the 5 employee bin. This grouping method ensures that not any firm is in 2 bins.

Y axis is the average take up rate at each 5 employment level bin in 2009. The average take-up rate at each bin in 2009 equals (fraction of firms in the low tax group (or the calculated tax rate less than 21.25 percent) in that bin in 2009 minus fraction of firms in the low tax group in that bin in 2007 (the calculated tax rate less than 21.25 percent)) divides by fraction of firms in the high tax group in that bin in 2007 (the calculated tax rate greater than 21.25 percent).

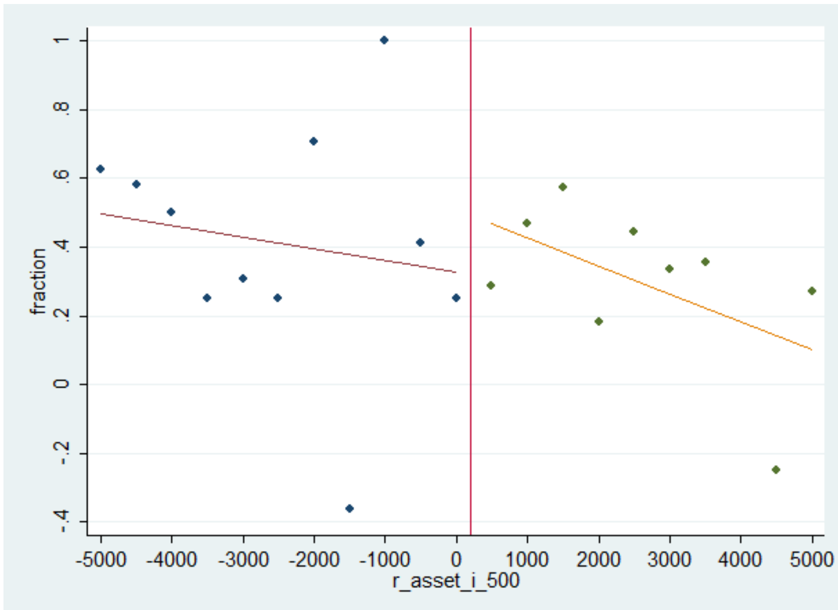


Figure 1.4: Take up in 2009 around the initial asset threshold

Note: The sample includes firms with more than the employment cutoff in 2008, and within 5 billion VND distance from the left and the right of the initial asset cutoff. The cutoff values are listed in table 1.

X axis is in million VND. X axis is the difference between the initial assets and the initial asset cutoffs in 2009. Firms are grouped into bins of 500 million VND initial assets. For example, if the initial asset difference is greater than 500 million VND and less than or equal to 0 million VND, firms are grouped into the 0 initial asset level bin. If the difference is greater than 0 million VND and less than or equal to 500 million VND, firms are grouped into the 500 million initial asset bin. This grouping method ensures that not any firm is in 2 bins.

Y axis is the average take up rate at each 500 million initial asset bin in 2009. The average take-up rate at each bin in 2009 equals (fraction of firms in the low tax group (the calculated tax rate $\leq$ 21.25 percent) in that bin in 2009 minus fraction of firms in the low tax group in that bin in 2007 (the calculated tax rate $\leq$ 21.25 percent)) divides by fraction of firms in the high tax group in that bin in 2007 (the calculated tax rate $>$ 21.25 percent).

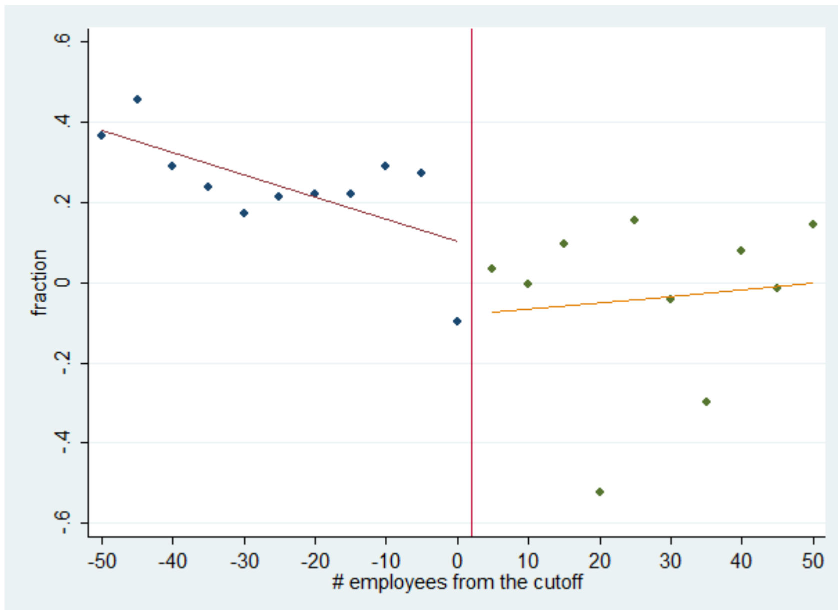


Figure 1.5: Take-up in 2011 around the employment threshold

Note: The sample includes firms with assets greater than the asset cutoff in 2011, and within 50 long-term employees distance from the left and the right of the cutoff. The cutoff values are listed in table 1.

X axis is the difference between the number of employees and the employee cutoffs in 2011. Firms are grouped into 5 employment level bins. For example, if the difference is between -4 and 0 employees, firms are grouped into the 0 employment level bin. If the difference is between 1 to 5 employees, firms are grouped into the 5 employee bin. This grouping method ensures that not any firm is in 2 bins.

Y axis is the average take up rate at each 5 employment level bin in 2011. The average take-up rate at each bin in 2011 equals (fraction of firms in the low tax group (the calculated tax rate $\leq$ 21.25 percent) in that bin in 2011 minus fraction of firms in the low tax group in that bin in 2007 (the calculated tax rate $\leq$ 21.25 percent)) divides by fraction of firms in the high tax group in that bin in 2010 (the calculated tax rate $>$ 21.25 percent).

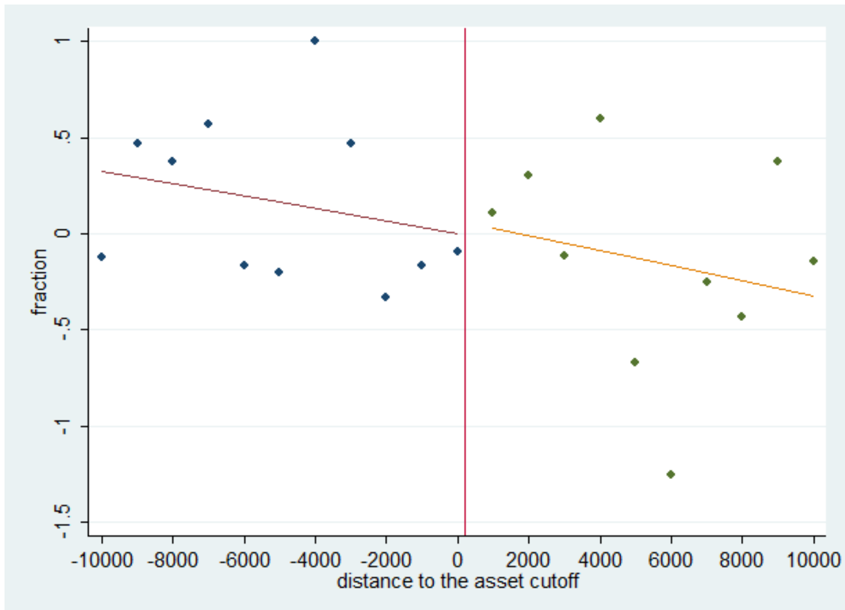
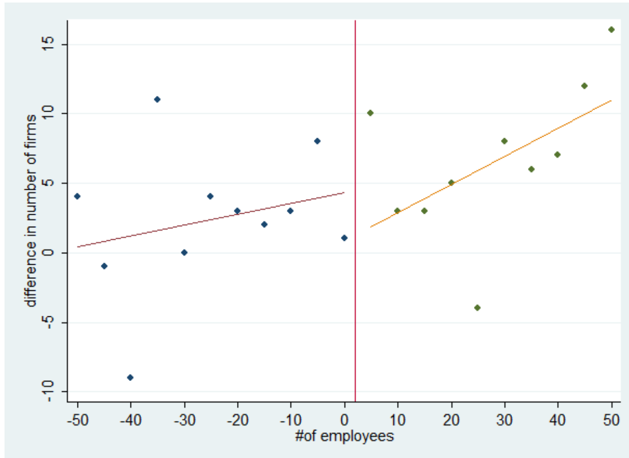


Figure 1.6: Take up in 2011 around the asset threshold

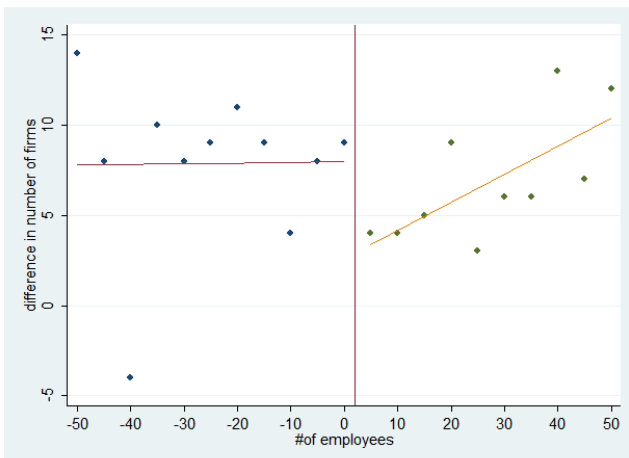
Note: The sample includes firms with more than the employment cutoffs, and within 10 billion VND distance from the left and the right of the asset cutoffs. The cutoff values are listed in table 1.

X axis is in million VND. X axis is the difference between the initial assets and the asset cutoffs in 2011. Firms are grouped into bins of 1 billion VND assets. For example, if the asset difference is greater than 1 billion VND and less than or equal to 0 million VND, firms are grouped into the 0 asset level bin. If the difference is greater than 0 million VND and less than or equal to 1 billion VND, firms are grouped into the 1 billion asset bin. This grouping method ensures that not any firm is in 2 bins.

Y axis is the average take up rate at each 1 billion asset bin in 2009. The average take-up rate at each bin in 2011 equals (fraction of firms in the low tax group (the calculated tax rate $\leq$ 21.25 percent) in that bin in 2011 minus fraction of firms in the low tax group in that bin in 2010 (the calculated tax rate $\leq$ 21.25 percent)) divides by fraction of firms in the high tax group in that bin in 2010 (the calculated tax rate $>$ 21.25 percent).



(a) all firms in the employment range



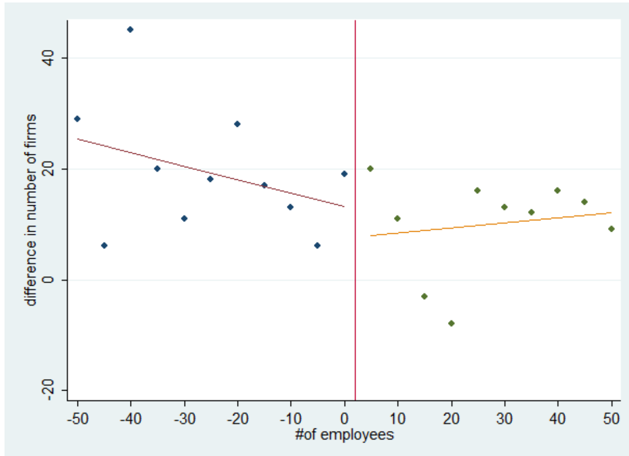
(b) firms that paid positive tax in 2009 in the employment range

Figure 1.7: Frequency of firms around the employment threshold in 2009

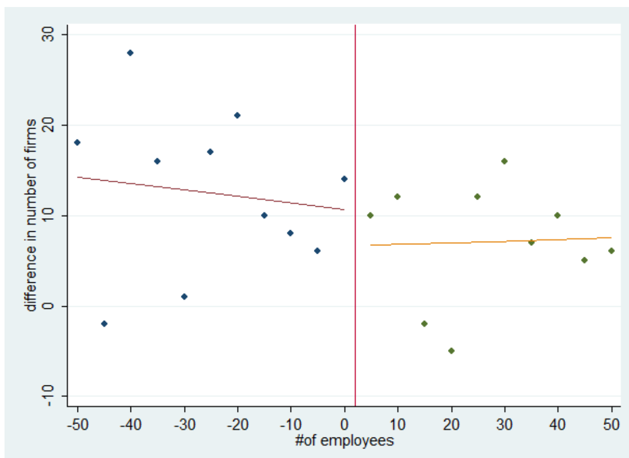
Note: The sample includes firms with initial assets greater than the initial asset cutoff in 2009, and within 50 long-term employees distance from the left and the right of the cutoff. The cutoff values are listed in table 1.

X axis is the difference between the number of employees and the employee cutoffs in 2009. Firms are grouped into 5 employment level bins. For example, if the difference is between -4 and 0 employees, firms are grouped into the 0 employment level bin. If the difference is between 1 to 5 employees, firms are grouped into the 5 employee bin. This grouping method ensures that not any firm is in 2 bins.

Y axis is difference between the number of firms in each bin in 2009 and the number of firms in each bin in 2007. The difference is to account for any possible heaping at employment levels divisible by 5.



(a) all firms in the employment range



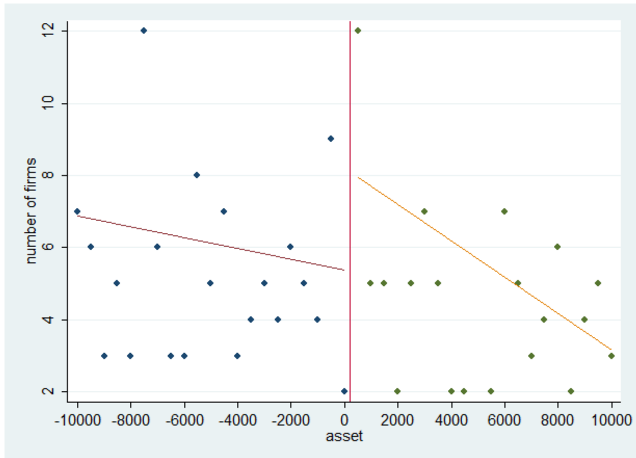
(b) firms that paid positive tax in 2011 in the employment range

Figure 1.8: Frequency of firms around the employment threshold in 2011

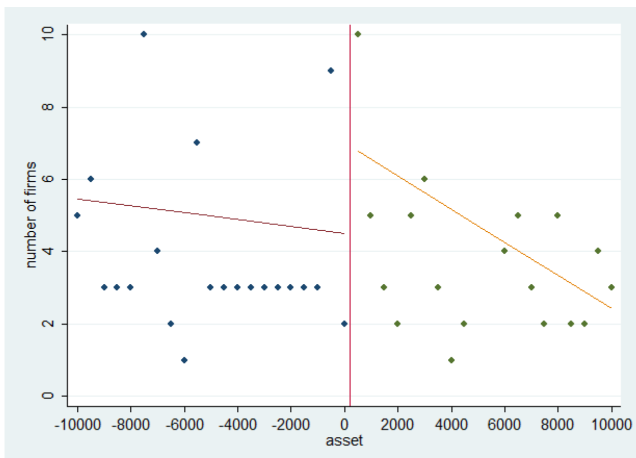
Note:The sample includes firms with assets greater than the asset cutoff in 2011, and within 50 long-term employees distance from the left and the right of the cutoff. The cutoff values are listed in table 1.

X axis is the difference between the number of employees and the employee cutoffs in 2011. Firms are grouped into 5 employment level bins. For example, if the difference is between -4 and 0 employees, firms are grouped into the 0 employment level bin. If the difference is between 1 to 5 employees, firms are grouped into the 5 employee bin. This grouping method ensures that not any firm is in 2 bins.

Y axis is difference between the number of firms in each bin in 2011 and the number of firms in each bin in 2010. The difference is to account for any possible heaping at employment levels divisible by 5.



(a) all firms in asset range



(b) firms that paid positive tax in 2011 in the asset range.

Figure 1.9: Frequency of firms around the asset threshold in 2011

Note: The sample includes firms with more than the employment cutoffs, and within 10 billion VND distance from the left and the right of the asset cutoffs. The cutoff values are listed in table 1. X axis is in million VND. X axis is the difference between the initial assets and the asset cutoffs in 2011. Firms are grouped into bins of 1 billion VND assets. For example, if the asset difference is greater than 1 billion VND and less than or equal to 0 million VND, firms are grouped into the 0 asset level bin. If the difference is greater than 0 million VND and less than or equal to 1 billion VND, firms are grouped into the 1 billion asset bin. This grouping method ensures that not any firm is in 2 bins. Y axis is the number of firms in each asset bin in 2011.

Table 1.3: Fraction of firms in a high tax group in 2009 around the employment threshold in 2008

	0th	1st	1st & control variables
≤ # cutoff in2008	0.0418	0.108	0.0393
	(0.0540)	(0.102)	(0.118)
year2009	-0.0862	-0.116	-0.114
	(0.0542)	(0.0818)	(0.0909)
≤ # cutoff in 2008 & year2009	-0.186**	-0.129	-0.112
	(0.0741)	(0.153)	(0.168)
Constant	0.461***	0.430***	0.148
	(0.0410)	(0.0535)	(0.286)
N	831	831	829

Standard errors in parentheses. Standard errors are clustered at the employment level. A high tax group includes firms with calculated tax rate greater than 21.25 percent. Difference in differences approach uses equation 1.2.T he base year is in 2007. Control variables are firm ages, ownership types, province, and 2 digit industry dummies. The sample includes firms between 50 employees from the left and the right of the cutoff in 2008, and the initial assets were greater than the initial asset cutoff. The cutoff values are listed in table 1 * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4: Fraction of firms in a high tax group in 2009 around the initial asset cutoff in 2009

	0th	1st	1st & control variables
$\leq$ intial asset cutoff	-0.0265	-0.397**	-0.296*
	(0.0806)	(0.163)	(0.172)
year2009	-0.202***	-0.413***	-0.295**
	(0.0736)	(0.119)	(0.138)
$\leq$ initial asset cutoff & year2009	-0.0330	0.346*	0.213
	(0.100)	(0.203)	(0.235)
Constant	0.615***	0.822***	0.610***
	(0.0607)	(0.0983)	(0.186)
N	369	369	368

Standard errors in parentheses. Standard errors are clustered at the initial asset level. A high tax group includes firms with calculated tax rate greater than 21.25 percent. Difference in differences approach uses equation 1.2. The base year is in 2007. Control variables are firm ages, ownership types, province, and 2 digit industry dummies. The sample includes firms between 5 billion VND from the left and the right of the initial asset cutoff, and the number of employees were greater than the employment cutoff. The cutoff values are listed in table 1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.5: Fraction of firms in high tax group in 2011 around the employment thresholds in 2011

	0th	1st	1st & control variables
≤ # employee cutoff	0.0819**	0.0173	0.0235
	(0.0379)	(0.0763)	(0.0746)
year2011	0.0200	-0.0356	-0.0481
	(0.0419)	(0.0601)	(0.0565)
≤ # employee cutoff & year2011	-0.177***	-0.0564	-0.0582
	(0.0498)	(0.104)	(0.104)
Constant	0.601***	0.631***	0.645***
	(0.0321)	(0.0443)	(0.228)
N	1862	1862	1860

Standard errors in parentheses. Standard errors are clustered at the employment level. A high tax group includes firms with calculated tax rate greater than 21.25 percent. Difference in differences approach uses equation 1.2. The base year is in 2010. Control variables are firm ages, ownership types, province, and 2 digit industry dummies. The sample includes firms between 50 employees from the left and the right of the cutoffs in 2011, and the assets were greater than the asset cutoffs. The cutoff values are listed in table 1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.6: Fraction of firms that are in a high tax group in 2011 based on asset threshold

	0th	1st	1st & control variables
$\leq$ asset cutoff	0.0534	-0.163	0.156
	(0.0816)	(0.152)	(0.159)
year2011	0.0393	-0.145	0.169
	(0.0853)	(0.121)	(0.148)
$\leq$ asset cutoff & year2011	-0.244**	0.138	-0.184
	(0.119)	(0.223)	(0.272)
Constant	0.632***	0.741***	-0.246
	(0.0595)	(0.0835)	(0.267)
N	282	282	268

Standard errors in parentheses. Standard errors are clustered at the asset level. A high tax group includes firms with calculated tax rate greater than 21.25 percent. Difference in differences approach uses equation 1.2. The base year is in 2010. Control variables are firm ages, ownership types, province, and 2 digit industry dummies. The sample includes firms between 10 billion VND the left and the right of the asset cutoffs in 2011, and the number of employees was greater than the employment cutoffs. The cutoff values are listed in table 1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.7: The changes in tax rates in 2009 among eligible and ineligible firms.

	(1) no controls	(2) controls	(3) no controls	(4) controls
year2009	-0.00348	-0.00181	-0.0400***	-0.0219
	(0.00929)	(0.00956)	(0.0125)	(0.0159)
employees≤ cutoff in 2009	0.0167	0.0107		
	(0.0109)	(0.0119)		
employees≤ cutoff in 2009& year2009	-0.0300**	-0.0234*		
	(0.0124)	(0.0140)		
initial assets≤ cutoff			-0.0152	-0.0189
			(0.0164)	(0.0190)
initial assets≤ cutoff& year2009			0.0209	0.0168
			(0.0176)	(0.0218)
Constant	0.169***	0.269***	0.203***	0.390***
	(0.00802)	(0.0320)	(0.0118)	(0.0439)
N	836	834	346	345

Standard errors in parentheses. Standard errors are clustered at the employment level or initial asset level. Difference in differences approach uses equation 1.2. The base year is in 2007. Control variables are firm ages, ownership types, province, and 4 digit industry dummies. The first two columns examine the employment threshold. The sample of the first two columns includes firms between 50 employees from the left and the right of the cutoff in 2009, and the initial assets were greater than the initial asset cutoff. The second two columns examine the initial asset threshold. The sample of the first two columns includes firms between 5 billion VND from the left and the right of the initial asset cutoff, and the employment level was greater than the employment cutoff. The cutoff values are listed in table 1.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.8: The changes in tax rates in 2011 among eligible and ineligible firms.

	(1) no controls	(2) controls	(3) no controls	(4) controls
year2011	0.00511	0.00643	0.0120	0.0228
	(0.00646)	(0.00636)	(0.0136)	(0.0149)
employment $\leq$ cutoff	0.00956	0.0118*		
	(0.00645)	(0.00625)		
employment $\leq$ cutoff & year2011	-0.0160**	-0.0202**		
	(0.00796)	(0.00806)		
assets $\leq$ cutoff			0.00336	-0.0164
			(0.0140)	(0.0166)
assets $\leq$ the cutoff & year2011			-0.0230	-0.0253
			(0.0193)	(0.0233)
Constant	0.199***	0.0304	0.198***	0.160***
	(0.00526)	(0.0230)	(0.00953)	(0.0533)
N	1757	1755	266	253

Standard errors in parentheses. Standard errors are clustered at the employment level or asset level. Difference in differences approach uses equation 1.2. The base year is in 2010. Control variables are firm ages, ownership types, province, and 4 digit industry dummies. The first two columns examine the employment threshold. The sample of the first two columns includes firms between 50 employees from the left and the right of the cutoffs in 2011, and the assets were greater than the asset cutoff. The second two columns examine the asset threshold. The sample of the first two columns includes firms between 10 billion VND from the left and the right of the asset cutoffs, and the employment level was greater than the employment cutoffs. The cutoff values are listed in table 1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.9: Are there more firms right below the employment threshold in 2008

	(1)	(2)	(3)	(4)
	0 polynomial	1 polynomial	2 polynomial	3 polynomial
≤ the cutoff	-0.686	0.736	0.753	-0.772
	(0.702)	(1.396)	(1.402)	(1.864)
Constant	0.490	-0.228	-0.438	0.336
	(0.502)	(0.789)	(0.891)	(1.086)
N	100	100	100	100

Standard errors in parentheses. Regression discontinuity design uses equation 1.1. Dependent variable is the difference between the number of firms at each employment level in 2008 and this number in 2007. The difference is to account for heaping. The sample includes firms between 50 employees from the left of the right of the employment cutoff in 2008, and initial assets were greater than the initial asset cutoffs. The cutoff values are listed in table 1 * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.10: Are there more firms that paid positive tax in 2009 below the 2008 employment threshold

	(1)	(2)	(3)	(4)
	0 polynomial	1 polynomial	2 polynomial	3 polynomial
≤ the cutoff	-0.0136	1.502	1.506	0.590
	(0.507)	(0.990)	(0.995)	(1.320)
Constant	0.326	-0.431	-0.524	-0.0693
	(0.363)	(0.557)	(0.628)	(0.761)
N	94	94	94	94

Standard errors in parentheses. Regression discontinuity design uses equation 1.1. Dependent variable is the difference between the number of firms at each employment level in 2008 and this number in 2007. The difference is to account for heaping. The sample includes firms between 50 employees from the left of the right of the employment cutoff in 2008, and initial assets were greater than the initial asset cutoffs. The cutoff values are listed in table 1. Firms paid positive tax in 2009. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.11: Are there more firms right below the employment threshold using 2009

	(1) 0 polynomial	(2) 1 polynomial	(3) 2 polynomial	(4) 3 polynomial
$\leq$ the cutoff	-0.928	1.089	1.103	-0.299
	(0.618)	(1.217)	(1.217)	(1.610)
Constant	1.388***	0.375	0.0359	0.749
	(0.439)	(0.684)	(0.768)	(0.935)
N	99	99	99	99

Standard errors in parentheses. Regression discontinuity design uses equation 1.1. Dependent variable is the difference between the number of firms at each employment level in 2009 and this number in 2007. The difference is to account for heaping. The sample includes firms between 50 employees from the left of the right of the employment cutoff in 2009, and initial assets were greater than the initial asset cutoffs. The cutoff values are listed in table 1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.12: Are there more firms that paid positive tax in 2009 below the 2009 employment threshold in 2009

	(1) 0 polynomial	(2) 1 polynomial	(3) 2 polynomial	(4) 3 polynomial
$\leq$ the cutoff	0.0837	1.269	1.294	1.027
	(0.491)	(0.978)	(0.978)	(1.311)
Constant	1.533***	0.938*	0.649	0.784
	(0.351)	(0.550)	(0.620)	(0.762)
N	92	92	92	92

Standard errors in parentheses. Regression discontinuity design uses equation 1.1. Dependent variable is the difference between the number of firms at each employment level in 2009 and this number in 2007. The difference is to account for heaping. The sample includes firms between 50 employees from the left of the right of the employment cutoff in 2009, and initial assets were greater than the initial asset cutoffs. The cutoff values are listed in table 1. Firms paid positive tax in 2009. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.13: Are there more firms below the 2011 employment threshold in 2011

	0 polynomial	1st	2nd	3rd
# of employees ≤ the cutoff	1.667	1.027	1.061	0.317
	(1.043)	(2.095)	(2.100)	(2.812)
Constant	2.000***	2.323*	1.866	2.247
	(0.741)	(1.180)	(1.331)	(1.640)
N	101	101	101	101

Standard errors in parentheses. Regression discontinuity design uses equation 1.1. Dependent variable is the difference between the number of firms at each employment level in 2011 and this number in 2010. The difference is to account for heaping. The sample includes firms between 50 employees from the left of the right of the employment cutoff in 2011, and assets were greater than the asset cutoffs. The cutoff values are listed in table 1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.14: Are there more firms below the cutoff using 2011 employment threshold among firms that paid positive tax in 2011

	0 polynomial	1st	2nd	3rd
# of employees ≤ the cutoff	0.982	0.999	1.002	0.829
	(0.857)	(1.716)	(1.725)	(2.309)
Constant	1.449**	1.441	1.407	1.495
	(0.612)	(0.965)	(1.094)	(1.347)
N	100	100	100	100

Standard errors in parentheses. Regression discontinuity design uses equation 1.1. Dependent variable is the difference between the number of firms at each employment level in 2011 and this number in 2010. The difference is to account for heaping. The sample includes firms between 50 employees from the left of the right of the employment cutoff in 2011, and assets were greater than the asset cutoffs. The cutoff values are listed in table 1. Firms paid positive tax in 2011. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.15: Are there more firms right below the 2011 asset thresholds.

	0 polynomial	1st	2nd	3rd
asset $\leq$ the cutoff	0.0149	0.0190	0.0212	0.0523
	(0.0308)	(0.0587)	(0.0585)	(0.0753)
Constant	1.036***	1.034***	1.058***	1.046***
	(0.0227)	(0.0326)	(0.0361)	(0.0410)
N	181	181	181	181

Standard errors in parentheses. Regression discontinuity design uses equation 1.1. Dependent variable is the difference between the number of firms at each employment level in 2011 and this number in 2010. The difference is to account for heaping. The sample includes firms between 10 billion VND from the left of the right of the asset cutoffs in 2011, and number of employees was greater than the employment cutoffs. The cutoff values are listed in table 1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 1.16: Are there more firms that paid positive tax in 2011 below the 2011 the asset cutoffs.

	0 polynomial	1st	2nd	3rd
asset $\leq$ the cutoff	-0.00633	0.00449	0.00851	0.0875
	(0.0351)	(0.0652)	(0.0649)	(0.0816)
Constant	1.047***	1.042***	1.069***	1.038***
	(0.0257)	(0.0359)	(0.0398)	(0.0442)
N	138	138	138	138

Standard errors in parentheses. Regression discontinuity design uses equation 1.1. Dependent variable is the difference between the number of firms at each employment level in 2011 and this number in 2010. The difference is to account for heaping. The sample includes firms between 10 billion VND from the left of the right of the asset cutoffs in 2011, and number of employees was greater than the employment cutoffs. The cutoff values are listed in table 1. Firms paid positive tax in 2011. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

1.6 Acknowledgements

Chapter 2, in part, is currently being prepared for submission for publication of the material

Chapter 2

A Temporary Corporate Income Tax Reduction and Its Effects on Firm Behavior: A Case Study of Vietnam

Abstract Should corporate income tax reductions be included in a stimulus package? How do they affect reported profits and investment? This paper evaluates these questions by examining a temporary 30 percent corporate income tax reduction in Vietnam in 2009 for small and medium sized businesses. Eligible businesses are firms with no more than 300 full-time employees or no more than 500 thousand dollars in initial assets. I use a difference-in-differences approach that compares eligible with ineligible firms around the 300 employee cutoff before and after the tax cut got implemented. To avoid employment manipulation, I use numbers of employees in the year before the policy was introduced to determine a firms eligibility. I estimate causal effects of the tax cut on capital investment, reported profits, and tax revenue. According to my estimates, the tax cut increased the capital stock by 11-13 percent. Relaxation of liquidity constraints, rather than a drop in the user cost of capital, plausibly explains the increase in investment. The estimates also imply large increases in before-tax reported profits in the tax cut year and even in the year after the tax cut ended. Due to increases in reported profits, tax revenue increased. I do not find evidence that increases in profits can be explained by profit-shifting from non-tax cut years to the tax cut year or by changes in capital or employment. Foreign-owned firms contributed to most of the increase in profits. Thus, it is plausible that multinational corporations shifted profits into Vietnam to take advantage of the low tax rate.

Overall, the temporary tax cut policy increased tax revenue. The tax cut increased the capital stock. Thus, a temporary reduction in the corporate income tax turned out to be a low cost policy for economic stimulus in Vietnam.

2.1 Introduction

Should corporate income tax reductions be included in a stimulus package? How do they affect reported profits? To answer these two questions, this paper evaluates the effects of a temporary 30% corporate income tax reduction in Vietnam in 2009 on corporate investment, reported profits, and tax revenue.

As in many developing countries, Vietnam applies a uniform corporate income tax rate. The tax cut program was first introduced and implemented in the last quarter of 2008. It was part of a stimulus package in response to the recent financial crisis. Only small and medium sized businesses were eligible. The policy was only intended to last through 2009 and was not implemented in 2010[1]. Small and medium sized businesses are firms with no more than 300 full-time employees as of the last quarter of 2008 or no more than 500 thousand USD in initial assets. I restrict my sample to firms that had initial assets of more than 500 thousand USD. The policy led to a jump in tax rates at the 300 employee threshold during the tax cut period. To avoid possible endogeneity, I use employment levels in 2007 to determine the eligibility threshold. This allows me to identify effects of the tax change by comparing changes in behavior of eligible firms compared to ineligible firms in a sample of firms close to the discontinuity at 300 employees. I use a firm level panel difference in differences approach to estimate the effect of the tax cut program on investment and reported profits.

There are two main findings. First, the tax cut program is estimated to have increased investment but not the employment level. Tangible fixed assets among eligible firms are estimated to have grown by approximately 11-13% in 2009. Even though statistically insignificant, the point estimate of the capital stock in 2010 is similar to that in 2009, which suggests that the capital stock of eligible firms remained higher than that of ineligible firms. The standard presumption would be that a temporary tax cut in 2009 would be expected to lead to a change in the timing of new investment between

[1] The program was re-implemented in 2011 and 2012. However, before 2011, firms did not know that the policy would be re-implemented.

2009 and 2010, but not the overall capital stock at the end of 2010. One possible rationale for this otherwise surprising finding is that smaller firms in Vietnam are credit-constrained, operating with a smaller capital stock than they would prefer due to lack of funding for new investments. The tax cut in 2009 provided extra funding, which was invested. As long as the capital stock remained smaller than desired, the new higher level of capital would be maintained in 2010, though might not increase further since the budget in 2010 re-tightened. Further investigations suggest that the added investment occurred in domestic firms, but not foreign-owned firms. This finding further supports the credit constraint mechanism because domestic firms were more likely to be credit constrained than foreign-owned firms.These results suggest that the temporary tax cut could increase the overall capital stock, as opposed to simply shifting the timing of investment.

Second, the policy led to an increase in business revenue and a large increase in reported profits in 2009 compared to before the tax cut. Specifically, reported revenue is estimated to have gone up by 12 percent. If we assume that the production function is Cobb-Douglas and the capital share is 1/3, a 12 percent increase in revenue is 3 times higher than what would have been predicted by a 11-13 percent increase in capital and no changes in labor. In addition, eligible firms were at least 11-13 percentage points more likely to report positive profits in 2009. Reported profits also increased by approximately $160,000 in 2009. This increase equals 26% of the mean profits in 2007. Foreign-owned firms, not domestic firms, contributed the most to the increase in reported profits. Due to a large increase in reported profits, a tax revenue calculation exercise suggests that tax revenue in 2009 did not decrease despite a smaller rate.

I show that the increase in reported profits was not due to changes in capital or labor. I decompose the increase in profits into two components: changes due to capital or labor and changes in reporting behavior. Controlling for capital and labor does not change the magnitude or significance of profit results. Thus, I attribute the increase in reported profits to changes in profit reporting behavior.

Additionally, reported profits did not decrease differentially in either 2008 or 2010 for firms that were eligible for the tax cut compared to firms that were not. This result suggests no profit-shifting across years. Thus, a possible explanation for the increase in reported profits in 2009 is changes in firm profit reporting behavior, such as a drop in tax evasion or a change in patterns of international income shifting among multinationals. Most of the increase in profits came from foreign-owned firms, suggesting that profit shifting by multinationals is the most likely mechanism. In fact, in 2012 many foreign-owned firms were convicted of undue shifting abroad of profits attributable to operations in Vietnam.

I find that profits of eligible firms were also higher than that of ineligible firms in 2010. The increase in reported profits in 2010 is surprising because the tax cut program was not in effect in 2010. Perhaps the changes in behavior leading to the increase in reported profits in the tax cut year are costly to undo in the next tax year when the policy was repealed. For example, during the low tax year, firms could start accepting checks or credit cards as forms of payment, even though such sales are harder to hide. This switch is difficult to reverse after the tax cut has expired because customers are now accustomed to a certain payment method. Or foreign-owned firms might report a higher output price and increase profits in the tax cut year. It is hard to report a very low price for the same item in the next year to save on tax money when the tax cut was repealed. Thus, firms would evade less in 2010 if they evade less in 2009.

This paper compliments the large existing literature on the effect of corporate income taxes on firm investment using developed country data or cross-country data. The existing literature focuses on permanent tax cuts and investment(e.g., Cummins et al.(1994), Cummins et al.(1996), and Hasset and Hubbard (1998) Djankov et al.(2010) and Ferede and Dahlby (2012)). In general, the literature concludes that a permanent increase in the user cost of capital via changes in taxes decreases investment. This study investigates the effects of a temporary, instead of a permanent, corporate income tax cut on investment in developing countries. Temporary tax cuts are often important components of stimulus packages. My results show that a temporary decrease in the corporate income tax increased investment in Vietnam.

This paper also contributes to a literature on the effect of a tax cut on reporting behavior in general and profit-shifting among multinationals in particular. In the US, Clotfleter (1985) shows a positive relationship between evasion rates and tax rates. Feinstein (1991) shows a negative relationship between marginal tax rates and evasion rates. Studies in the developing countries are Fishman and Wei (2004) in China, Kopczuk (2010) in Poland, and Gorodnichenko et al.(2009) in Russia. Gorodnichenko et al.(2009) and Kopczuk(2010) show that a permanent income tax reduction led to a large increase in individual reported incomes. They attribute the large increases in individual reported income to better tax compliance. Some studies on multinational income shifting from high tax countries to low tax countries are Swenson (2001), Clausing (2003), Huizinga and Laeven (2008), and Weichenrieder (2009). Unlike the existing studies, my paper analyzes a temporary tax cut rather than a permanent tax cut. The temporary tax cut allows me to examine the compliance behavior even after the tax cut was repealed.

The paper proceeds as follows. Section 2.2 introduces the institutional background of corporate income taxes in Vietnam. Section 2.3 presents the dataset. Section 4 presents how well the eligibility threshold predicted the program take-up rate in 2009. Section 2.5 presents my identification strategy. In section 2.6, I discuss what happens to investment under a temporary corporate income tax cut predicted by an economic theory without credit constraints and with credit constraints. I then present empirical results on capital and labor. I discuss results on sales and reported profits in section 2.7. Finally, I present a tax revenue calculation in section 8 and conclude the paper in section 2.9. Formal derivations of investment and tax evasion models are presented in the appendix.

2.2 Corporate Income Tax in Vietnam

As in many developing countries, Vietnam applies a uniform corporate income tax structure. The corporate income tax rate has been decreasing overtime. The tax rate was 32 percentage points until 2003, 28 percentage points from 2004 to 2008, 25 percentage points from 2009 to 2013, and 22 percentage points from 2014 until now. The corporate income tax is an important source of government revenues in developing countries. For instance, in Vietnam from 2006 to 2010, the corporate income tax revenue equaled approximately 16% of total tax revenue and 6% of total GDP [2]. These figures in the US were 10% [3] and 1.9% [4], respectively. The 2009 tax cut program was first introduced and implemented in the last quarter of 2008 and in the whole year of 2009. Only small and medium sized businesses were eligible. When the policy was first introduced in December 2008, the government declared that the program was only temporary and did not plan to continue it in the future. The policy was not implemented in 2010. The program was re-implemented in 2011 and in 2012. Due to the temporary tax cut, the tax rate in the last quarter of 2008 of small and medium sized businesses was 19.6 percentage points, instead of 28 percentage points. The tax rate in 2009 was 17.5 percentage points, a 7.5 percentage point or 30% decrease.

2.3 Data

2.3.1 Dataset

This paper uses an annual enterprise survey conducted by the Vietnamese General Statistical Office (GSO) from 2000 to 2011. It is mandatory that all registered firms in Vietnam answer the survey. The dataset has information about firm's balance sheet, income statement, and some basic tax variables such as corporate income taxes paid and value added tax liabilities.

[2] Chien luoc cai cach he thong thue 2011-2020

[3] "Policy Basics: Where Do Federal Tax Revenues Come From?", Center for Budget and Policy Priorities http://www.cbpp.org/cms/?fa=view&id=3822

[4] Tax policy center http://www.taxpolicycenter.org/taxfacts/displayafact.cfm?Docid=205

Firm reporting incentives to the GSO survey would be the same as to the tax administrators since both datasets are collected by the government.

Most firms in Vietnam choose their fiscal year to be the same as the calendar year. For tax purposes, the deadline for returning last year's corporate income tax returning form is March 31st.

The enterprise survey is rolled out on March 1st every year to ask about last year's information. All survey must be returned to the statistical office by July 15. Therefore, it is reasonable to assume that information in the enterprise dataset is relatively close to the actual numbers that firms report on their tax returnings to the government on March 31st.

2.3.2 Treatment and control groups

Even though the dataset is from 2000 to 2011, I only use data from 2004 to 2010. It is because the longer the panel is, the more observations are missing. 2011 is not used in the analyses because the eligibility rule in 2011 was different from that in 2009. I further restrict the sample to all firms that were always in the data from 2004 to 2010. In other words, it is a balanced panel dataset from 2004 to 2010. This further restriction is to avoid any possibility that the results might be caused by endogenous firms entry and exit due to the tax cut program. Only 1.25% of firms that were in my sample in 2004 exited the sample after 2004. Thus, I do not worry about exit as a result of the program.

The tax cut policy only applied to small and medium sized firms. Small and medium sized firms were firms that had no more than 300 long-term employees in the last quarter of 2008 OR had initial assets no more than 500 thousand USD when the firm first registered. Long-term employees are employees with more than 3-month contracts. Short-term employees are employees with less than 3 month contracts.

Since I do not observe a firm's assets if it registered before 2000 when the data was first available, the initial asset variable is set equal to the firm's assets in the first year the firm appeared in the enterprise dataset. Thus, the constructed initial assets are only accurate for firms that registered after 2000. It is assets in 2000 if the firm was established before 2000. The sample is a balanced panel from 2004 to 2010. These firms were all established on or before 2004. Thus, I do not worry about manipulation of initial assets when the policy was first introduced in December 2008.

As for the employment eligibility threshold, the policy was first introduced in December 2008. The government used the long-term employment level at the end of 2008 as the employment eligibility threshold. Firms might manipulate to go under 300 long-term employees in 2008 to be eligible for the tax cut program. In addition, the 300 long-term employee threshold might be an endogenous choice. For instance, if firms just below the threshold in 2008 were affected by the financial crisis the hardest, the government might choose this threshold to alleviate some consequences of the financial crisis on these firms. To avoid the potential endogeneity and manipulation around the 300 long-term employee threshold in 2008, I use the 2007 long-term employment level to predict the tax cut eligibility.

To define a more compatible treatment and control group, I borrow the idea from the regression discontinuity methodology that firms around the threshold are more likely to be similar to each other than firms further away from the threshold. However, I do not have enough observations to do a strict regression discontinuity design. Therefore, in this paper I use a difference in differences approach with firm fixed effects around the employment eligibility threshold. I restrict the sample to firms whose initial assets were more than 500 thousand USD, so that eligible firms were firms with no more than 300 long-term employees. I further restrict the sample to firms that had between 250 and 350 long-term employees in 2007. My treatment group consists of firms whose long-term employees were from 250-300 in 2007, and their initial assets were more than 500 thousand USD. My control group consists of firms whose long-term employees were from 301-350 employees in 2007, and their initial assets were more than 500 thousand USD[5]

[5] Another way to create treatment and control group is to restrict the sample to firms that had more than 300 long-term employees in 2007. I further restrict the sample to firms whose initial assets were between 400 thousand to 600 thousand USD because the initial

In summary, the treatment (eligible) group consists of firms that had long-term employees from 250-300 in 2007, and initial assets more than 500 thousand USD. Control (ineligible) group consists of firms that had long-term employees from 301-350 employees in 2007 and initial assets more than 500 thousand USD. I restrict the sample to the balance panel of firms that were always in the data from 2004 to 2010.

2.3.3 Summary Statistics

This section discusses the characteristics of eligible and ineligible firms in 2007. These firms existed from 2004 to 2010, their initial assets were more than 500 thousand USD, and their long-term employees in 2007 were between 250 and 350.

Table 2.1 shows that in 2007, firms were mostly in manufacturing (60%) , construction (12-15%) , and commerce (5-6%). Industry types were similar among eligible and ineligible firms. Table 2.2 shows that ownership types of eligible firms in 2007 were also similar to those of ineligible firms in 2007. Specifically, in both ineligible and eligible groups, around 27% of firms were foreign-owned and the rest were domestic.

Table 2.3 shows that the average total profit before tax in 2007 was 616 thousand USD with a large standard deviation around 1.6 million USD. The median profit was 167 thousand USD. The average total number of workers (including both short term and long term employees) was 388 employees, and the median was 318 employees. The mean and median age of a firm were 15 and 12 years old, respectively. The annual salary per labor was about 1800 USD, and the median was 1400 USD.

In table 2.4, among firms in the balanced panel dataset, 87.3% of firms reported positive profits in 2007, the rest reported 0 or negative profits. The

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asset threshold was 500 thousand USD. Eligible firms were firms that had more than 300 employees in 2007 and had initial assets between 400 to 500 thousand USD. Ineligible firms were firms that had more than 300 employees in 2007 and had initial assets between 500 to 600 thousand USD. This method gives me 84 observations each year, while the employment eligibility threshold method gives me 460 observations each year. Therefore, I use the treatment and control groups defined by the employment eligibility threshold method.

fraction of firms that always reported positive profits from 2004 to 2010 was 56%. The fraction always reporting negative profits was 1%.

2.4 Did the eligibility threshold predict the program take-up in 2009?

$$c_{it} = \alpha_0 + \alpha_1 Eligible + Z_{it} + \epsilon_{it}$$

$Eligible = 1$ if the firm is in the treatment group, and 0 if the firm is in the control group.

Z_{it} : other control variables such as ownership dummies, province dummies, and industry dummies

$c_{it} = 1$ if firm i in year t receives the cut, and 0 otherwise.

The coefficient of interest is α_1. If α_1 is significantly greater than 0, we can say that eligible firms were more likely to receive the tax cut in 2009 than ineligible firms, which validates the existence of the program and the eligibility threshold.

Unfortunately, the dataset does not provide information about whether or not a firm received the tax cut. In other words, the dataset does not have c_{it}. Therefore, I construct a firm tax cut status using its calculated tax rate. A calculated tax rate is computed by dividing the annual amount of corporate income tax liability by the reported profits before tax. This calculated tax rate is not the exact tax rate that a firm was actually responsible for paying to the government. This is because the observed regular accounting reported profit before tax in the dataset is not the same as the firm's taxable profit, which is based on the tax accounting standard. In addition, the corporate income tax liability variable has a lot of missing observations. Figure **??** is the histogram of calculated tax rate in 2009. The histogram has 3 peaks: 0%, 17.5% and 25%, which were the three dominant tax rates in 2009. Firms paying 0 % tax rate were loss-making firms. The histogram implies that the calculated tax rates may somewhat describe the true distribution of actual tax rates.

In 2009, if a firm received the corporate income tax cut, it would pay corporate income tax at the rate of 17.5%. A firm that did not get the tax cut would pay 25%. I use mid-points to group firms that received the cut and that did not. Specifically, I assign a firm to receive the tax cut in 2009 if its calculated tax rate was in the (0, 21.25%) range. A firm didn't receive the cut if the calculated tax rate was more than 21.25%.

Therefore, the constructed variable of firms tax-cut status in 2009 has measurement errors. If this measurement error is pure noise, the regression coefficient of the inferred tax cut status on the eligibility threshold indicator might suffer from a down-ward bias. Regardless, the first-stage regressions using the constructed tax-cut status could still provide some suggestive evidence that the eligibility threshold in 2007 predicts the tax-cut status in 2009.

Table 2.7 shows that the threshold of 300 employees in 2007 indeed predicted the probability of getting the tax cut in 2009. More specifically, firms that had between 250-300 long-term employees in 2007 were about 12-14% more likely to receive the tax cut in 2009 than firms that had between 301-350 long-term employees in 2007. Therefore, I can use the labor eligibility threshold in 2007 to assess the intent to treat results of the program.

This section demonstrates that the eligibility threshold can indeed predict the take-up probability in 2009. However, since the tax cut status suffers from measurement errors, I am not using the differential take-up between eligible and ineligible firms calculated in this section for later analyses.

2.5 Main Estimation Equation: Panel data Difference In Differences Approach with Firm Fixed Effects

I run a difference in differences estimation that accounts for firm fixed effects.

Basic estimation regression:

$$Y_{it} = \alpha_i + year_t + \beta_1 Eligible * year_{post} + \epsilon_{it}$$

Robustness checks:

$$\begin{aligned} Y_{it} = \alpha_i + year_t + \beta_1 Eligible * year_{post} \\ + \beta_2 ln(asset)_{2004} * year_{post} + \beta_3 ln(labor)_{2004} * year_{post} \\ + \beta_3 Y_{i,2004} * year_{post} + \epsilon_{it} \end{aligned}$$

Y_{it} is a dependent variable, which is before-tax profit, tangible fixed assets, number of employees, and total labor cost.

$year_t$ are year dummies from 2005 until 2010.

$year_{post}$ are 2007 year dummy, 2008 year dummy, 2009 year dummy, and 2010 year dummy. Base years are 2005 and 2006. Year 2007 is the placebo year because it was before the policy. Treatment years are 2008, 2009, and 2010.

The data for fixed tangible assets were only available starting from 2006. Regressions that have fixed tangible assets use year 2006 as a base year, as opposed to 2005 and 2006.

Eligible equals 1 if a firm is in the treatment group defined by the employment eligibility threshold and 0 if a firm is in the control group.

$ln(asset)_{2004} * year_{post}$ is the interaction between log asset in 2004 and year dummies from 2007 to 2010. $ln(labor)_{2004} * year_{post}$ is the interaction between log labor in 2004 and year dummies from 2007 to 2010. These variables control for possible differential time trends by different firm characteristics. They also control for the fact that different sized firms might be affected differently by the financial crisis.

$Y_{i,2004} * year_{post}$ is the interaction between the dependent variable in 2004 and different year dummies in the post period. These interactions allow for firms that had different initial values of dependent variables to grow differently in different years.

Coefficient of interest is β_1, which is the coefficient of the interaction between eligibility and years after the program was implemented. All standard errors in the difference-in-differences regressions are clustered at the firm level[6].

2.6 Capital and Labor

2.6.1 Predictions from the theory

This section presents how a constant return to scale firm in a perfectly competitive market would change its capital and labor in response to the temporary corporate income tax. The formal derivations are presented in the appendix 2.10.1.

First, I would like to examine the model under no credit constraints. In the absence of a corporate income tax, a firm invests until its lifetime present value of marginal revenue product of the last unit of investment equals its upfront cost. With a corporate income tax, in each period t, the firm pays its investment's upfront cost in that period. The firm then gets to deduct from the amount of taxes it pays to the government a portion of this cost each period until the whole upfront cost gets deducted. This deduction is called depreciation allowance. When the tax rate in period t decreases, for the last unit of new investment, the firm saves on corporate income tax an amount equal to the difference between the marginal revenue product of this last unit of investment and its first year depreciation allowance. Therefore, if the marginal revenue product of the new investment is greater than its first year depreciation allowance, the firm invests more. Otherwise, the firm invests less. In general, the marginal revenue product of the new investment is usually greater than the first year depreciation. Thus, if the tax rate in a period decreases, firms usually invest more in that period.

What happens to labor in the tax cut year depends on what happens to output. If output stays the same, the employment level decreases in the tax cut year because there is more capital. If output increases, the employment

[6]Bootstrap standard errors are similar to clustered standard errors

level can increase or stay unchanged in the tax cut year because there is now more demand for labor.

After the tax cut ended, factor prices revert to their steady-state levels. In the absence of credit constraints, the capital stock should also revert to its prior level. Thus, without credit constraints and without changes in factor prices, firms only respond to a temporary corporate income tax cut by re-timing their investment. As a result, the tax cut policy does not affect the overall capital stock.

If a firm is credit constrained, the firm's capital stock is below the steady-state level when the tax cut is introduced. The tax cut creates two effects on investment. First, it is likely to decrease the relative price of capital. Second, it relaxes the firm's credit constraints. The firm invest as much as it can afford. Thus, the capital stock in the tax cut year goes up. After the tax cut is removed, the firm has no incentive to bring the capital stock down to its previous level. The capital stock stays at its new higher level. As a result, a temporary corporate income tax reduction results in a continuing increase in the overall capital stock.

2.6.2 Empirical results on capital and labor

I use tangible fixed assets as a measure for investment. It is a direct measure of capital inputs. Though the investment data are available, they have a lot of missing observations. Tangible fixed assets include land, building, equipment, and machinery. If firm tangible fixed assets increased, investment also increased. The tangible fixed asset variable is a stock variable.

Table 2.8 column 1 and 2 show that tangible fixed assets of eligible firms increased in 2009 and in 2010 compared to ineligible firms. The point estimate in 2009 were approximately 12%. Even though insignificant, the point estimate in 2010 is of similar magnitude as the point estimate in 2010, suggesting that the capital stock in 2010 stays at the same level as the capital stock in 2009. I also examine the effects of the tax cut program on numbers of employees and total labor costs in column 3,4,5,and 6 of table 2.8. Neither labor nor labor costs showed any significant changes in 2009 and in 2010, implying number of employees, worker hours, and worker productivity did not change. Thus, I did not find evidences for changes in firm hiring and labor allocation decisions as a result of the tax cut program.

The continuing higher capital stock in 2010 compared to that in the control firms is consistent with the forecasts if firms were credit-constrained. If there were no credit constraints, I expect the capital stock would adjust back to the old equilibrium after the tax cut was over. Under a credit-constrained environment, the capital stock has not reached its steady state, so it will remain at its new higher level in years after the tax cut.

In table 2.9, I show that the new investment appears to come primarily from domestic firms. I do not find evidences that foreign-owned firms increased their investment. This finding further supports the credit constraint mechanism because domestic firms were more likely to be credit constrained than foreign-owned firms. Thus, domestic firms invested when the temporary tax cut program increased their budgets.

2.7 Reported revenue and profit

2.7.1 Theoretical Predictions

From section 2.6.2, we estimated that the capital stock increased by 12 percent, and labor did not increase due to the tax cut. Assume that the production function is Coubb-Douglas, i.e. $f(K, L) = K^{\alpha}L^{1-\alpha}$. Thus $log f(K, L) = \alpha log K + (1 - \alpha) log L$. In developed countries, the capital share is equal to 1/3, or $\alpha = 1/3$. Gollin (2002) shows that the capital share in less developed countries ranges from 1/5 to 1/3. Therefore, if the capital stock grows by 12 percent and if there are no changes in labor, I would expect output to grow at most by 4 percent. If factor prices do not change, revenue would also increase by at most 4 percent.

In terms of reported profits, I only observe regular accounting profits, not economic profits. Assume that regular accounting profits are very similar to tax-accounting profits. Derivations in appendix 2.10.2 describe how changes in capital and labor as a result of the temporary corporate income tax cut affect tax accounting profits. Intuitively, the higher capital stock in 2009 and in 2010 as a result of the tax cut should raise reported profits in these two years.

2.7.2 Empirical results on reported revenue and profit

Table 2.10 suggests that business revenue goes up 12 percent, while the theory only predict a maximum increase of 4 percent. Column 6 of table 2.10 controls for log tangible fixed assets, number of employees, and total labor cost, while column 4 and 5 do not. Results of column 6 are very similar to those of column 4 and 5. Thus, I find no evidences that the changes in sales could be explained by the changes in factor inputs.

In terms of profits, I use different transformations for the profit variable to reduce the effect of outliers on the estimated coefficients and to get more precision. The profit variable has a high variance and cannot be logged because it has many negative values. The first transformation is whether or not firms earn positive profits. The second transformation is when I drop the bottom and top percentile values to avoid large outliers. I call this new variable trimmed profit[7].

The program led to an increase in reported profits. Specifically, column 1 and 2 of table 2.11 show that eligible firms were 13 percentage points more likely to report positive profits in 2009 and 11 percentage points more likely to report positive profits in 2010. Column 3 and 4 of table 2.11 show that the tax cut program increased reported profits in 2009 by around $160,000. This number is equivalent to over 25% of the mean profit in 2007. The increase in profits in 2010 was similar in magnitude as the increase in 2009. Profits did not decrease in 2008. In table 2.13, I show that the increase in profits might entirely come from foreign-owned firms. Though foreign-owned firms

[7] I also tried truncating the profit variable. The truncated profit variable sets all values in the 1th percentile in the 2005-2010 sample to the 1th percentile value, and all values in the 99th percentile in the 2005-2010 sample to the 99th percentile value. The trimmed and truncated profit variables have similar results, so I only show the results of trimmed profit variable

were less responsive at the investment margin, they were more responsive at the reporting margin.

I find that the increase in reported profits did not come from changes in factor inputs such as labor or capital. I decompose the increase in profits into two components: changes in factor inputs and changes in reporting behavior. Factor inputs are firm tangible fixed assets, number of employees, and total labor cost. Assuming that workers get paid equal to their productivity, total labor cost conditional on the number of workers is a measure of labor productivity. Since tangible fixed assets are only available from 2006 on, I re-estimate column(2) and (4) of table 2.11 using 2006 as a base year, instead of using 2005 and 2006 as base years. Results are reported in column 1 and 3 in table 2.12. Column 1 and 3 of table 2.12 show results of profits without controlling for factor inputs. Column 2 and 4 of table 2.12 show results after controlling for factor inputs. The magnitude and significance level of the eligibility coefficients did not change when I include factor input variables. This finding implies that changes in profits did not come from any expansion in firms.

In a temporary tax cut situation, firms can save money by shifting profits from the non tax cut years to the tax cut years. Thus, firms could shift profits from 2008 and 2010 to 2009, because 2009 has the lowest tax rates in these three years. However, I find no evidences that the increase in reported profits could come from profit shifting across years because reported profits in 2008 and in 2010 did not decrease.

Another possible explanation for the large increase in reported profits in 2009 is income shifting from larger firms to smaller firms with joint ownership. This explanation is unlikely because reported profits mostly came from foreign-owned firms. Foreign-owned firms usually do not have both parent firms and subsidiaries physically located in Vietnam.

Third explanation for the large increase in reported profits in 2009 is that it is due to a drop in tax evasion. For foreign-owned firms, they could shift profits from countries with high tax rates to Vietnam in response to its lower tax rate.

Profits continued to remained high for eligible firms in 2010. The increase in reported profits in 2010 is surprising because the tax cut program was not in effect in 2010. Perhaps the changes in behavior leading to the increase in reported profits in the tax cut year are costly to undo in the next tax year when the policy was repealed. For example, during the low tax year, firms could start accepting checks or credit cards as forms of payment, even though such sales are harder to hide. This switch is difficult to reverse after the tax cut has expired because customers are now accustomed to a certain payment method. Or foreign-owned firms might report a higher output price and increase profits in the tax cut year. It is hard to report a very low price for the same item in the next year when the tax cut was repealed. Thus, firms would evade less in 2010 if they evade less in 2009. A model of this intuition is presented in the appendix 2.10.3.

Alternatively, foreign-owned firms might have thought the policy would still be in effect in 2010, so they continued reporting higher reported profits in 2010. However, this possibility is unlikely because the government announced clearly that the tax cut program would not continue after 2009.

The model in appendix 2.10.3 suggests that reported profits in 2010 should be lower than that in 2009. Since 2010 was not the tax cut year, it was beneficial for firms to evade more in 2010 than in 2009. The results of positive profits in column 1 and 2 of table 2.11 somewhat support the model's prediction. Point estimate in 2010 is approximately 0.11, while point estimate in 2009 is 0.13. The trimmed profit coefficients do not give the same results. The point estimate in 2010 is even greater than the point estimate in 2009. The point estimates in 2009 and in 2010 of the positive profit variable and the trimmed profit variable are not statistically different from each other, though.

2.8 Tax Revenue in 2009

This section shows that tax revenue did not decrease because of the tax cut program. Looking at reported profits alone does not give a definite story about tax revenue. First, tax revenue is not only affected by profits, but it is also affected by tax rates. It is unclear whether or not a firm would have to pay more in tax if it reported a higher profit in a low tax year. In addition, if a firm reported $100,000 higher in profits, and this firm had to pay a corporate tax rate of 17.5%, it is not necessarily true that this firm would have to pay $17,500 higher in tax. It is because negative or 0 profit firms do not need to pay the corporate income tax. Consider a firm that made a loss of $99,900 last year and makes a profit of $100 this year, their reported profit increases by $100,000. However, their tax liability goes from 0 to $17.5, not to $17,500. Or if a firm made a loss of $101,000 last year and makes a loss of only $1000 this year. This firm's reported profit also increases by $100,000, but its tax liability stays at 0.

The tax liability variable is available, but a lot of observations are missing. Specifically, 10% of these missing observations also reported negative profits before tax. Approximately 15% of positive profit firms did not report their tax liability data. In addition to the missing observations problem, tax liability also contains 0 and has a large variance. Given the problems with the data on tax payments, I instead create a taxable profit variable that replaces all negative value profit with 0. In other words, taxable profit=max(0, reported profit). Estimated tax payments would then equal 25% of this figure for ineligible firms and 17.5% of this figure for eligible firms.

Taxable profits increased in 2009 as shown in table 2.14 . I use column 1 of table 2.14 to calculate tax revenue. Column 1 only controls for firm fixed effects, year dummies, and interaction terms between the eligibility indicator and year dummies. Thus, the constant coefficient, $343,000, in column 1 of table 2.14 represents the average profits of firms in 2005 and in 2006. The coefficient of year dummy 2009 is 127,000. Therefore, the average profit of ineligible firms in 2009 was $343,000+$127,000=$470,000.

Let a be a firm's reported profit if the firm did not get the tax cut, and b is how much extra profit a firm would report if it received the tax cut.

Let α_i be the fraction of ineligible firms that received the tax cut and α_e be the fraction of eligible firms that received the tax cut.

Let π_i and π_e be profits of ineligible firms and eligible firms, respectively.

Thus,

$E(\pi_i) = a + b\alpha_i = 470,000$

$E(\pi_e) = a + b\alpha_e$

Thus $E(\pi_e) - E(\pi_i) = b(\alpha_e - \alpha_i) = b * \Delta\alpha = 115,000$. Hence, $b = \frac{115{,}000}{\Delta\alpha}$

This implies $a = 470,000 - b\alpha_i = 470,000 - \frac{115{,}000}{\Delta\alpha}\alpha_i$

The total tax payment would be paid if the firm did not receive the tax cut: $0.25a$

The total tax payment would be paid if the firm received the tax cut: $0.175a + 0.175b$

Tax revenue would increase if $-0.075a + 0.175b > 0$,

or $b/a > 0.075/0.175 \approx 0.429$

We have

$$\frac{b}{a} = \frac{\frac{115{,}000}{\Delta\alpha}}{470,000 - \frac{115{,}000}{\Delta\alpha}\alpha_i} = \frac{115,000}{470,000\Delta\alpha - 115,000\alpha_i}$$

The higher $\Delta\alpha$ is, the smaller b/a is. The smaller α_i is, the smaller b/a is. Assume that ineligible firms did not take-up the program, thus $\alpha_i = 0$. Thus, $\frac{b}{a} > \frac{115{,}000}{470{,}000\Delta\alpha}$.

If $\frac{115{,}000}{470{,}000\Delta\alpha} > 0.429$, we have $b/a > 0.429$. In order for $\frac{115{,}000}{470{,}000\Delta\alpha} > 0.429$, we need $\Delta\alpha < 0.57$. In other words, if the differential take-up rate between eligible and ineligible firms was smaller than 0.57, tax revenue would increase in 2009.

According to the Provincial Competitive Index Survey in Vietnam, approximately 60% of eligible firms knew about the policy in 2012. I assume that knowledge about the policy in 2009 was the same as in 2012. I assume that ineligible firms had a 0% take-up rate in 2009, and all eligible firms that knew about the program received the tax cut. Thus, the maximum differential take-up rate among eligible and ineligible firms would be 60% in 2009. These figures would lead to a conservative estimate of the impact of the tax cut program on tax revenue.[8] With the maximum differential take-up rate between

[8]In my take-up paper, I show that some ineligible firms took up. In addition, not all eligible firms who were aware of the program received the tax cut in 2012. However, the eligibility rules in 2012 was very different from that of 2009, so the take-up decision in 2012

eligible and ineligible firms of 60% and the 0% take-up rate among ineligible firms, tax revenue in 2009 did not fall despite smaller tax rates according to the above calculation.

2.9 Conclusions

This paper evaluates the impact of a temporary 30% corporate income tax cut in 2009 on firm asset accumulation and profit reporting in Vietnam. The temporary tax cut led to an increase in investment and a large increase in reported profits. The increase in reported profits was large enough that tax revenue did not fall in 2009 and rose in 2010. Credit-constrained firms are consistent with investment results. Further heterogeneity analyses between foreign-owned and domestic firms suggest that domestic firms responded to the program through investment, while foreign-owned firms responded through reporting. The increase in reported profits was not due to changes in factor inputs such as capital or labor nor was it because of profit shifting across years. Thus, I attribute the large increase in reported profit to a drop in tax evasion. Specifically, foreign-owned firms might have shifted profits from countries with high tax rates to Vietnam in response to the tax cut. Thus, the temporary reduction in the corporate income tax seems to have been a low cost policy for economic stimulus in Vietnam during the recent recession.

might be very different from that of 2009. For the purpose of calculating the magnitude of impact of the program on tax revenue, the higher the take-up rate is, the lower the actual impact is, given the same intent-to-treat estimator result. As a result, I prefer a conservative estimate and choose 60% as the differential take-up rate between the eligible and the ineligible in 2009.

Table 2.1: Percentage of firms in 2007 by industry types

Industry type	250-300 employees	301-350 employees
commerce	7.14	6.70
communication	0.38	0.00
construction	17.29	13.92
electricity, gas, and water supply	2.63	4.12
entertainment	1.13	1.55
finance	0.75	1.03
health and social work	0.75	0.00
hotels, restaurants	2.26	3.61
manufacturing	57.89	59.28
public administration and defense	1.88	3.61
real estates	1.13	0.52
sciences	2.26	0.52
transporation and storage	4.14	4.12
other services	0.38	1.03
Observations	266	194

Firms were between 250-350 long-term employees in 2007 and were no more than $500,000 in initial assets. All firms existed from 2004 to 2010.

Table 2.2: Percentage of firms in 2007 by ownership types

Ownership type	250-300 employees	301-350 employees
private domestic	20.30	15.90
central SOE	20.30	23.08
local SOE	16.17	18.46
equitized firms	16.17	14.87
foreign-owned firms	27.07	27.69
Observations	266	195

Firms were between 250-350 long-term employees in 2007 and were no more than $500,000 in initial assets. All firms existed from 2004 to 2010.

Table 2.3: Summary statistics in 2007

	mean	standard deviation	median	count
total profit before tax	606.128	1609.97	161.1187	458
total labor	388.1048	248.6725	318	458
total asset	13343.4	45501.31	5963.165	458
age	15.39301	11.95609	12	458
labor cost	650.7864	529.5661	513.144	458
annual salary per labor	1.837126	1.604988	1.404657	458
Observations	458			

Firms were between 250-350 long-term employees in 2007 and were no more than $500,000 in initial assets. Profit, labor cost, and asset are in thousand dollars. All firms existed from 2004 to 2010.

Table 2.4: Summary statistics in 2007 of fraction report positive profits

	(1) All firms		
	mean	sd	count
fraction of firms with positive profit	.8733333	.3329694	458
fraction always reporting positive profit from 2004-2010	.5622222	.4966654	458
fraction always reporting negative profit from 2004-2010	.0111111	.1049387	458
Observations	458		

Firms were between 250-350 long-term employees in 2007 and were more than $500,000 in initial assets. All firms existed from 2004 to 2010. All in fraction.

Table 2.5: Summary statistics in 2007 of fraction reporting positive profit

	(1) Domestic firms		
	mean	sd	count
fraction of firms with positive profit	.9179331	.2748846	329
fraction always reporting positive profit from 2004-2010	.6595745	.4745741	329
fraction always reporting negative profit from 2004-2010	.006079	.0778492	329
Observations	329		

Firms were between 250-350 long-term employees in 2007 and were more than $500,000 in initial assets. All firms existed from 2004 to 2010. All in fraction.

Table 2.6: Summary statistics in 2007 of fraction reporting positive profits

	(1) Foreign-owned firms		
	mean	sd	count
fraction of firms with positive profit	.7520661	.4336087	121
fraction always reporting positive profit from 2004-2010	.2975207	.4590684	121
fraction always reporting negative profit from 2004-2010	.0247934	.1561415	121
Observations	121		

Firms were between 250-350 long-term employees in 2007 and were more than $500,000 in initial assets. All firms existed from 2004 to 2010. All in fraction.

Table 2.7: Probability of getting the tax reduction in 2009 among firms that had initial assets more than $500,000 and were around 300 long-term employees in 2007

	(1)	(2)	(3)	(4)
≤300 employees in 2007	0.134**	0.127**	0.144**	0.178**
	(0.0524)	(0.0526)	(0.0573)	(0.0637)
Constant	0.514***	0.552***	0.413***	0.239
	(0.0404)	(0.0661)	(0.105)	(0.156)
ownership dummies	no	yes	yes	yes
province dummies	no	no	yes	yes
industry dummies	no	no	no	yes
N	359	359	359	359
F	6.546	2.376	0.936	0.953
r2	0.0180	0.0326	0.145	0.275

Standard errors in parentheses. Firms were between 250-350 long-term employees in 2007 and were more than $500,000 in initial assets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$

Table 2.8: The effects of the tax cut on log tangible assets, labor, and labor costs.

	ln tangible asset	ln tangible asset	ln # labor	ln # labor	ln labor cost	ln labor cost
Eligible & year2007	-0.0546	-0.0631	0.0245	-0.0216	0.00839	-0.0325
	(0.0557)	(0.0535)	(0.0313)	(0.0248)	(0.0456)	(0.0434)
Eligible & year2008	0.0259	0.00405	0.0155	-0.0423	0.0165	-0.0391
	(0.0595)	(0.0586)	(0.0460)	(0.0414)	(0.0553)	(0.0536)
Eligible & year2009	0.130*	0.120*	0.00683	-0.0492	0.0281	-0.0345
	(0.0715)	(0.0710)	(0.0595)	(0.0552)	(0.0644)	(0.0601)
Eligible & year2010	0.112	0.117	0.00444	-0.0551	0.0248	-0.0315
	(0.0818)	(0.0808)	(0.0588)	(0.0552)	(0.0726)	(0.0701)
Constant	7.156***	6.336***	5.830***	3.408***	6.110***	3.330***
	(0.0229)	(0.284)	(0.0175)	(0.480)	(0.0212)	(0.496)
Control variables A	no	yes	no	yes	no	yes
N	2289	2289	2755	2755	2755	2755
F	18.51	10.95	2.926	6.194	5.011	9.754
r2	0.118	0.127	0.0169	0.132	0.0147	0.0919

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Balanced panel data difference-in-differences approach with firm fixed effects. Balanced panel data from 2004 until 2010. Base years 2005 and 2006 in all regressions. In regressions that have tangible fixed assets, base year is 2006 because data were only available from 2006 on. Treatment years: 2007, 2008, 2009, and 2010. Year 2007 is a placebo year. Firms had between 250-350 long-term employees in 2007, and initial assets were greater than 500,000 USD. Eligible firms had between 250-300 long-term employees in 2007, and initial assets were greater than 500,000 USD. Ineligible firms had between 301-350 long-term employees in 2007, and initial assets were greater than 500,000 USD. Dependent variable ln tangible fixed assets, ln labor, and ln labor cost. Eligible & year2007 is the interaction between the eligibility indicator based on 2007 long-term employment and year 2007. Eligible & year2008, Eligible & year2009, and Eligible & year2010 have similar interpretations. Clustered standard errors are at the firm level. Control variables A: interaction between log labor and asset in 2004 with year dummies from 2007 to 2010.

Table 2.9: The effects of the tax cut program on tangible fixed assets among domestic and foreign-owned firms.

	(1) foreign	(2) foreign	(3) domestic	(4) domestic
Eligible & year2007	0.0536	0.0404	-0.0956	-0.103
	(0.0735)	(0.0748)	(0.0716)	(0.0687)
Eligible & year2008	0.0488	0.0108	0.0156	-0.00390
	(0.0955)	(0.0930)	(0.0740)	(0.0735)
Eligible & year2009	0.0496	0.0393	0.158*	0.143
	(0.117)	(0.118)	(0.0883)	(0.0881)
Eligible & year2010	0.0658	0.0738	0.128	0.121
	(0.137)	(0.135)	(0.100)	(0.0992)
Constant	10.43***	9.515***	9.830***	9.681***
	(0.0405)	(0.539)	(0.0278)	(0.580)
Control variables A	no	yes	no	yes
N	618	618	1671	1671
F	2.206	2.169	3.589	2.370
r2	0.0150	0.0325	0.0384	0.0461

Standard errors in parentheses. Balanced panel data difference-in-differences approach with firm fixed effects. Balanced panel data from 2004 until 2010. Base years 2005 and 2006 in all regressions. In regressions that have tangible fixed assets, base year is 2006 because data were only available from 2006 on. Placebo year: 2007. Treatment years: 2008, 2009, and 2010. Eligible firms had between 250-300 long-term employees in 2007, and initial assets were greater than 500,000 USD. Ineligible firms had between 301-350 long-term employees in 2007, and initial assets were greater than 500,000 USD. Eligible & year2007 is the interaction between the eligibility indicator and year 2007. Eligible & year2008, Eligible & year2009, and Eligible & year2010 have similar interpretations. Clustered standard errors are at the firm level. Control variables A: interaction between log labor and asset in 2004 with year dummies from 2007 to 2010. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.10: The effects of the tax cut program on inventories and sales.

	(1) inventory	(2) inventory	(3) inventories	(4) revenue	(5) revenue	(6) revenue
Eligible&year2007	-0.0215	-0.0444	-0.0222	0.0447	0.0199	0.0334
	(0.0780)	(0.0785)	(0.0801)	(0.0464)	(0.0484)	(0.0433)
Eligible&year2008	0.0694	0.0604	0.0779	0.0213	-0.0137	-0.00207
	(0.0893)	(0.0898)	(0.0876)	(0.0580)	(0.0583)	(0.0514)
Eligible&year2009	0.00485	0.00991	0.0233	0.120^{*}	0.0934	0.112^{*}
	(0.103)	(0.105)	(0.102)	(0.0706)	(0.0689)	(0.0578)
Eligible&year2010	-0.0141	-0.0147	-0.0131	0.0854	0.0566	0.0776
	(0.119)	(0.119)	(0.113)	(0.0792)	(0.0799)	(0.0622)
Constant	10.87^{***}	9.461^{***}	5.159^{***}	13.63^{***}	12.97^{***}	8.476^{***}
	(0.0455)	(1.007)	(1.476)	(0.0289)	(0.576)	(0.588)
Control A	no	yes	yes	no	yes	yes
Control B	no	no	yes	no	no	yes
N	2186	2186	2183	2291	2291	2286
r2	0.905	0.906	0.912	0.922	0.923	0.948

Standard errors in parentheses. Balanced panel data difference-in-differences approach with firm fixed effects. Balanced panel data from 2004 until 2010. Base year is 2006 regressions. Placebo year: 2007. Treatment years: 2008, 2009, and 2010. Dependent variables: log revenue and log inventories. Eligible firms had between 250-300 long-term employees in 2007 and its initial asset was greater than 500,000 USD. Ineligible firms had between 301-350 long-term employees in 2007 and its initial asset was greater than 500,000 USD. Eligible & year2007 is the interaction between the eligiblity indicator and year 2007. Eligible & year2008, Eligible & year2009, and Eligible & year2010 have similar interpretations. Clustered standard errors are at the firm level. Control variables A: interaction between log labor and asset in 2004 with year dummies from 2007 to 2010. Control variables B: control variables A and log tangible fixed asset, labor, and labor cost. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.11: The effects of the tax cut program on reported profits.

	(1) positive profit	(2) positive profit	(3) trimmed profit	(4) trimmed profit
Eligible & year2007	0.0489	0.0522	45.98	43.77
	(0.0324)	(0.0320)	(54.73)	(59.77)
Eligible & year2008	0.0567	0.0613	67.07	48.39
	(0.0382)	(0.0375)	(70.86)	(72.69)
Eligible & year2009	0.133***	0.134***	164.9**	152.3**
	(0.0382)	(0.0374)	(76.60)	(75.43)
Eligible & year2010	0.111***	0.118***	167.4**	177.3**
	(0.0363)	(0.0362)	(72.50)	(70.49)
Constant	0.807***	0.807***	254.9***	256.1***
	(0.0141)	(0.0136)	(24.61)	(25.04)
Control variables A	no	yes	no	yes
N	2748	2724	2693	2669
F	4.248	2.975	6.166	7.472
r2	0.0224	0.0547	0.0412	0.0814

Standard errors in parentheses. Balanced panel data difference-in-differences approach with firm fixed effects. Balanced panel data from 2004 until 2010. Base years 2005 and 2006 in all regressions. In regressions that have tangible fixed assets, base year is 2006 because data were only available from 2006 on. Placebo year: 2007. Treatment years: 2008, 2009, and 2010. Positive indicator dependent variable equals 1 if profits were greater than 0, and 0 otherwise. Trimmed profit dependent variable is raw profit that the 1 and 99 percentile values got dropped. Unit in thousand USD. Eligible firms had between 250-300 long-term employees in 2007, and initial assets were greater than 500,000 USD. Ineligible firms had between 301-350 long-term employees in 2007, and initial assets were greater than 500,000 USD. Eligible & year2007 is the interaction between the eligibility indicator and year 2007. Eligible & year2008, Eligible & year2009, and Eligible & year2010 have similar interpretations. Clustered standard errors are at the firm level. Control variables A: interaction between log labor, asset, dependent variable in 2004 with year dummies from 2007 to 2010. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.12: The effects on reported profits, controlling for factor inputs.

	(1) positive profit	(2) positive profit	(3) trimmed profit	(4) trimmed profit
Eligible & year2007	0.0490	0.0441	77.89	82.29
	(0.0371)	(0.0370)	(63.37)	(62.86)
Eligible & year2008	0.0590	0.0571	77.87	85.50
	(0.0411)	(0.0408)	(80.26)	(79.69)
Eligible & year2009	0.131***	0.134***	188.1**	194.1**
	(0.0417)	(0.0416)	(81.55)	(81.56)
Eligible & year2010	0.117***	0.119***	213.8***	222.1***
	(0.0409)	(0.0406)	(77.15)	(76.52)
Constant	0.829***	0.810***	311.7***	-1261.9***
	(0.0131)	(0.209)	(23.91)	(419.8)
Control variables A	yes	yes	yes	yes
Factor input variables	no	yes	no	yes
N	2269	2264	2222	2217
F	2.111	2.416	7.667	6.921
r2	0.0383	0.0450	0.0823	0.0993

Standard errors in parentheses. Balanced panel data difference-in-differences approach with firm fixed effects. Balanced panel data from 2004 until 2010. Base year is 2006 in all regressions. Placebo year: 2007. Treatment years: 2008, 2009, and 2010. Positive indicator dependent variable equals 1 if profits were greater than 0, and 0 otherwise. Trimmed profit dependent variable is raw profit that the 1 and 99 percentile values got dropped. Unit in thousand USD. Eligible firms had between 250-300 long-term employees in 2007, and initial assets were greater than 500,000 USD. Ineligible firms had between 301-350 long-term employees in 2007, and initial assets were greater than 500,000 USD. Eligible & year2007 is the interaction between the eligibility indicator and year 2007. Eligible & year2008, Eligible & year2009, and Eligible & year2010 have similar interpretations. Clustered standard errors are at the firms level. Control variables A: interaction between log labor, asset, dependent variable in 2004 with year dummies from 2007 to 2010. Factor input variables are tangible fixed asset, number of employees, and total labor cost. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.13: The effects of the tax cut program on taxable profits among domestic and foreign -owned firms.

	(1) foreign	(2) foreign	(3) domestic	(4) domestic
year2007	226.2*	174.9	113.6***	-538.9*
	(125.4)	(1002.3)	(27.34)	(297.3)
year2008	145.6	-785.2	110.7***	-499.7
	(139.3)	(1226.1)	(39.67)	(333.6)
year2009	1.622	-1104.2	162.8***	-276.9
	(105.4)	(1284.1)	(37.05)	(404.0)
year2010	-183.0	-119.9	149.9***	-975.4*
	(113.2)	(1147.2)	(41.27)	(504.9)
Eligible & year2007	23.15	-31.95	10.45	-1.656
	(182.6)	(184.5)	(37.49)	(38.69)
Eligible & year2008	35.52	69.00	-8.495	-24.88
	(216.2)	(214.8)	(48.58)	(49.20)
Eligible & year2009	359.5	423.6*	-0.671	-15.97
	(246.8)	(217.4)	(47.87)	(48.03)
Eligible & year2010	368.2*	514.9***	-12.34	-25.79
	(204.3)	(180.7)	(51.10)	(49.51)
Constant	589.6***	2526.9**	261.0***	835.6**
	(63.84)	(1071.3)	(12.41)	(357.0)
Control variables A	no	yes	no	yes
N	739	727	1971	1959
F	1.386	8.046	7.849	5.196
r2	0.0279	0.0913	0.0603	0.102

Standard errors in parentheses. Balanced panel data difference-in-differences approach with firm fixed effects. Balanced panel data from 2004 until 2010. Base years: 2005 and 2006 in all regressions. Placebo year: 2007. Treatment years: 2008, 2009, and 2010. The taxable profit variable is the raw profit variable that the 99 percentile values got dropped. All negative values of the taxable profit variable were set to equal 0. Unit is in thousand USD. Eligible firms had between 250-300 long-term employees in 2007, initial assets were greater than 500,000 USD. Ineligible firms had between 301-350 long-term employees in 2007, and initial assets were greater than 500,000 USD. Eligible & year2007 is the interaction between the eligibility indicator and year 2007. Eligible & year2008, Eligible & year2009, and Eligible & year2010 have similar interpretations. Clustered standard errors are at the firms level. Control variables A: interaction between log labor, asset, profit before tax in 2004 with year dummies from 2007 to 2010. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.14: The effect of the tax cut program on taxable profits.

	(1)	(2)
year2007	130.9***	-258.3
	(41.35)	(350.4)
year2008	120.2**	-137.4
	(51.37)	(377.7)
year2009	127.7***	-398.5
	(42.37)	(443.9)
year2010	65.94	-832.5
	(47.57)	(529.6)
Eligible & year2007	27.31	15.21
	(51.94)	(54.54)
Eligible & year2008	28.77	23.18
	(64.52)	(65.22)
Eligible & year2009	115.4*	105.8
	(69.00)	(66.61)
Eligible & year2010	123.4*	133.3**
	(66.51)	(64.89)
Constant	343.0***	343.0***
	(16.95)	(16.95)
Control variables A	yes	yes
Factor input variables	no	yes
N	2721	2721
F	5.506	4.569
r2	0.0313	0.0397

Standard errors in parentheses. Balanced panel data difference-in-differences approach with firm fixed effects. Balanced panel data from 2004 until 2010. Base years: 2005 and 2006 in all regressions. Placebo year: 2007. Treatment years: 2008, 2009, and 2010. Dependent variable: Taxable profits are reported profits with all negative values were set to equal 0, and values at the 99 percentile got dropped to avoid large outliers. Unit is in thousand USD. Eligible firms had between 250-300 long-term employees in 2007, and initial assets were greater than 500,000 USD. Ineligible firms had between 301-350 long-term employees in 2007, and initial assets were greater than 500,000 USD. Eligible & year2007 is the interaction between the eligibility indicator and year 2007. Eligible & year2008, Eligible & year2009, and Eligible & year2010 have similar interpretations. Clustered standard errors are at the firms level. Control variables A: interaction between log labor, asset, dependent variable in 2004 with year dummies from 2007 to 2010. Factor input variables are tangible fixed asset, number of employees, and total labor cost.e * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

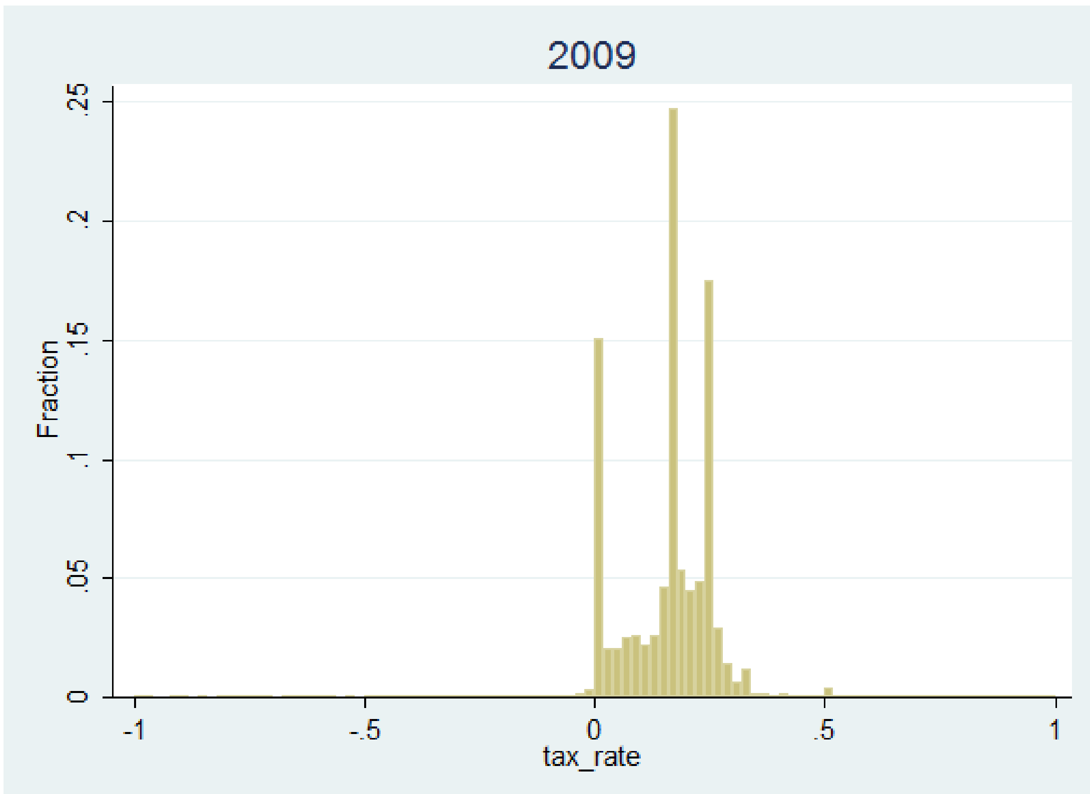


Figure 2.1: Calculated tax rate in 2009

2.10 Appendices

2.10.1 Theory of the Corporate Income Tax and Investments

Under no credit-constraints

Assume that firms have constant return to scale production functions. Assume that the market is perfectly competitive.

The relationship between investment I_s and capital K_s is as follows:

$I_s = K_s - (1-\delta)K_{s-1}$ with δ the capital replacement rate.

$K_0 = I_0$

$K_1 = I_1 + (1-\delta)I_0$

$K_2 = I_2 + (1-\delta)K_1 = I_2 + (1-\delta)I_1 + (1-\delta)^2 I_0$

$K_3 = I_3 + (1-\delta)K_2 = I_3 + (1-\delta)I_2 + (1-\delta)^2 I_1 + (1-\delta)^3 I_0$ ....

In period t, a firm pays tax on its tax accounting profits, which equals to its revenue minus labor cost minus all investment depreciation from period 0 until the period t. Therefore, its tax accounting before tax profit in period t is

$$\pi_t = p_t f(K_t, L_t) - w_t L_t - \int_0^t q_s D_{t-s} I_s e^{-r(t-s)} ds \tag{2.1}$$

$f(K_t, L_t)$: production function with capital K_t and L_t

δ: economic depreciation of capital

r: discount rate

D_{t-s}: capital depreciation rate from tax accounting standard at time $t-s$

q_s: price of a unit of investmentl I_s in period s

p_t : output price at period t.

Firms maximize their life-time profits. A firm chooses a sequence of investment $I_0, I_1, I_2, ...$ from period 0 to the end of time.

$$max_{I_0,I_1,I_2,...}E(\pi) = \int_0^\infty \underbrace{(p_t f(K_t,L_t)e^{-rt} - wL_t - q_tI_t)}_{\text{before tax economic profits}} - \underbrace{\tau_t(p_t f(K_t,L_t) - w_tL_t - \int_0^t q_s D_{t-s} I_s e^{-r(t-s)} ds)}_{\text{amount of tax paid in period t from equation 2.1}} dt$$

τ_t: corporate income tax rate at time t

FOC w.r.t I_s and differentiating $K_s, K_{s+1}, ...$ w.r.t I_s gives

$$\int_s^\infty p_t f_{K_t} e^{-(\delta+r)(t-s)}(1-\tau_t)dt + \int_s^\infty \tau_t q_s D_{t-s} e^{-r(t-s)}dt = q_s \tag{2.2}$$

Equation 2.2 implies that a firm chooses an investment I_s at time s until its marginal revenue product equals its upfront cost minus its depreciation until the end of time.

Assume that the firm's production function is Cobb-Douglas, i.e. $K^\alpha L^{1-\alpha}$. Thus f_K is a function of $k = K/L$. Therefore, we can write $f_K = \alpha(K/L)^{\alpha-1} = \alpha k^{\alpha-1}$, which is a decreasing function in k. Thus $f_{K_s k_s} < 0$

I am interested in how a temporary change in the corporate income tax rate affects capital labor ratio k. Assume that the corporate income tax rate τ_t temporarily changes in period t=s. Differentiate FOC 2.2 w.r.t to τ_s:

$$-p_s f_{K_s} + q_s D_0 + p_t(1-\tau_s) f_{K_s k_s} \partial k_s / \partial \tau_s = 0$$

Therefore,

$$\partial k_s / \partial \tau_s = \frac{p_s f_{K_s} - q_s D_0}{p_s(1-\tau_s) f_{K_s k_s}}$$

Since $f_{K_s k_s < 0}$, if the marginal revenue product is higher than the first year depreciation, the capital ratio increases as the tax rate in period s, τ_s, decreases. If the marginal revenue product is smaller than the first year depreciation, the capital labor ratio decreases as the tax rate decreases.

In period $s+1$ when there is no tax cut and firms choose investment I_{s+1} in period $s+1$. The first order condition when choose I_{s+1} is the same as when choosing I_{s-1}. Now the tax rate from period $s+1$ comes back to normal, which is the same as how it was before period s. Therefore, the capital labor ratio would come back to its normal level.

If capital labor ratio goes up, it is more likely that the capital stock goes up in a tax cut year. What happens to labor in the tax cut year depends on what happens to output. If output stays the same, the employment level decreases in the tax cut year because there is more capital. If output increases, the employment level can increase or stay unchanged in the tax cut year because there is now more demand for labor.

Credit constraints

Assume that firms are credit constrained and can only buy capital from their profits after tax. In other words,

$$\pi_{t,\tau_t} = (1-\tau_t)(p_t f(K_t, L_t) - w_t L_t) + \tau_t \int_0^t q_s D_{t-s} I_s e^{-r(t-s)} ds = q_t I_t \quad (2.3)$$

Differentiate 2.3 w.r.t τ_t, we get:

$$-(p_t f(K_t, L_t) - w_t L_t) - \tau_t p_t f_{K_t} \frac{\partial K_t}{\partial \tau_t} + \int_0^t q_s D_{t-s} I_s e^{-r(t-s)} d_s + \tau_t q_t D_0 \frac{\partial K_t}{\partial \tau_t} \\ -\tau_t \left(p_t f_{L_t} \frac{\partial L_t}{\partial \tau_t} - w_t \frac{\partial L_t}{\partial \tau_t} \right) = q_t \frac{\partial K_t}{\partial \tau_t}$$

It is followed by

$$\frac{\partial K_t}{\partial \tau_t} \underbrace{(\tau_t q_t D_0 - q_t - \tau_t p_t f_{K_t})}_{\text{A}} \\ = \underbrace{p_t f(K_t, L_t) - w_t L_t - \int_0^t q_s D_{t-s} I_s e^{-r(t-s)} d_s}_{\text{profit before tax}}$$

(it is because $p_t f_{L_t} = w_t$ by the FOC of labor, so the term on $\partial L_t / \partial \tau_t$ drops out.)

Since τ_t and $D_0 < 0$, then $A < 0$. Assume that profit before tax is positive, then $\partial K_t/\partial\tau_t < 0$. Thus, the capital stock increases if the tax rate decreases.

Now I want to examine what happen to the capital stock at period $t+1$ after the tax cut was repealed.

In period $t+1$, we get:

$$\begin{aligned}\pi_{t+1,\tau_{t+1}} &= (1-\tau_{t+1})(p_{t+1}f(K_{t+1},L_{t+1}) - w_{t+1}L_{t+1}) \\ &\quad + \tau_{t+1}\int_0^t q_s D_{t-s} I_s e^{-r(t-s)} ds = q_{t+1}I_{t+1}\end{aligned}$$

Differentiate 2.4 w.r.t τ_t, we get:

$$\begin{aligned}-\tau_{t+1}p_{t+1}f_{K_{t+1}}\frac{\partial K_{t+1}}{\partial\tau_t} + \tau_{t+1}q_{t+1}D_0\frac{\partial K_{t+1}}{\partial\tau_t} - (1-\delta)\tau_{t+1}q_t D_1\frac{\partial K_t}{\partial\tau_t} \\ = q_{t+1}\frac{\partial K_{t+1}}{\partial\tau_t} - (1-\delta)q_{t+1}\frac{\partial K_t}{\partial\tau_t}\end{aligned}$$

It is followed by

$$\frac{\partial K_{t+1}}{\partial\tau_t}\underbrace{(\tau_{t+1}q_{t+1}D_0 - q_{t+1} - \tau_{t+1}p_{t+1}f_{K_{t+1}})}_{<0} = \frac{\partial K_t}{\partial\tau_t}(1-\delta)\underbrace{(\tau_{t+1}q_t D_1 - q_{t+1})}_{<0}$$

(note that $\partial\pi_{t+1}/\partial L_{t+1} = 0$)

Thus, $\partial K_{t+1}/\partial\tau_t$ and $\partial K_t/\partial\tau_t$ have the same sign and hence are smaller than 0. Therefore, the capital stock in period $t+1$ increases as the tax rate τ_t in period t decreases.

2.10.2 Impacts on tax-accounting profits

Observed tax accounting profits before tax in year t:

$$\pi_t = p_t f(K_t, L_t) - w_t L_t - \int_0^t q_s D_{t-s} I_s ds$$

$$\pi_t = p_t f(K_t, L_t) - w_t L_t - \int_0^t q_s D_{t-s}(K_s - (1-\delta)K_{s-1})ds$$

Thus

$$\begin{aligned}\partial\pi_t/\partial\tau_t &= p_t f_{K_t}\partial K_t/\partial\tau_t + p_t f_{L_t}\partial L_t/\partial\tau_t - w_t\partial L_t/\partial\tau_t - q_t D_0 \partial K_t/\partial\tau_t \qquad (2.4)\\ &= \frac{\partial K_t}{\partial\tau_t}(p_t f_{K_t} - q_t D_0)(\text{because } p_t f_{L_t} - w_t = 0 \text{ due to FOC of labor })\end{aligned}$$

If $\partial K_t/\partial\tau_t < 0$ and $p_t f_{K_t} > q_t D_0$, then $\partial\pi_t/\partial\tau_t < 0$. In other words, tax-accounting profit decreases as tax In the credit constrained case, it is even more likely that $p_t f_{K_t} > q_t D_0$ because of the low level of the capital, or f_{K_t} is high. Thus, the tax-accounting profits in period t increase as the tax rate τ_t in period t decreases.

In period $t+1$, we have:

$$\pi_{t+1} = p_{t+1} f(K_{t+1}, L_{t+1}) - w_{t+1} L_{t+1} - \int_0^{t+1} q_s D_{t+1-s}(K_s - (1-\delta)K_{s-1})ds$$

Thus

$$\begin{aligned}\partial\pi_{t+1}/\partial\tau_t &= p_{t+1} f_{K_{t+1}}\frac{\partial K_{t+1}}{\partial\tau_t} - q_{t+1}D_0\frac{\partial K_{t+1}}{\partial\tau_t} + (1-\delta)q_t D_1\frac{\partial K_t}{\partial\tau_t}\\ &= \frac{\partial K_{t+1}}{\partial\tau_t}(p_{t+1} f_{K_{t+1}} - q_{t+1}D_0) + (1-\delta)q_t D_1\frac{\partial K_t}{\partial\tau_t}\end{aligned}$$

If $p_{t+1} f_{K_{t+1}} - q_{t+1}D_0 > 0$, $\partial K_{t+1}/\partial\tau_t < 0$, and $\partial K_t/\partial\tau_t < 0$, then $\partial\pi_{t+1}/\partial\tau_t < 0$. In other words, the tax accounting profits in period $t+1$ increase as the tax rate τ_t in period t decreases.

2.10.3 A two period tax evasion model

I extend the static framework of Cremer and Gahvari(1993) to construct a two period tax evasion model.

A firm in country A lives for 2 periods. The tax rates in these 2 periods are t_1 and t_2.

Assume the market is perfectly competitive and firms are price takers. Also assume that prices are determined by the world market and are not affected by policies in country A. For simplicity, assume that per unit output price and cost are the same in the two periods. I denote them as p and c, respectively. Assume output quantity in the two periods are x_1 and x_2.

Assume that firms evade by under-reporting sales. Let h_1 and h_2 be proportionals of sale that firms hide from the government, $0 \leq h_1, h_2 \leq 1$. A tax evading firm in the first period would report only $((1-h_1)p-c)x_1$ and in the second period $((1-h_2)p-c)x_2$ to the government.

In the first period, the firm faces an increasing and convex cost $g(h_1)$ to hide h_1 units of evasion. Thus, its total cost of hiding in the first period is $pg(h_1)x_1$.

Define $\Delta h = h_2 - h_1$. In the second period, to hide h_2 units of evasion, the firm faces an increasing and convex cost $g(h_2)$ and an increasing and convex adjustment cost $f(|\Delta h|)$. Also assume that $f'(0) = 0$. Thus, its total cost of hiding h_2 proportion of sale in the second period is $p(g(h_2) + f(\Delta h))x_2$. The adjustment cost function $f(\Delta h)$ represents the idea that it is expensive to change habits, or it is expensive to adjust the evasion rates. For instance, firms can hide evasion by only involving in cash transactions. However, if a firm allows credit card transactions in the tax cut year to attract more customers, it is hard for them to switch back to cash transactions at the end of the tax cut period.

Assume that the government decides to randomly audit firms in each period with the probability β. If a firm get caught evading in one period, it has to pay τ times the amount of tax it evades, where $\tau > 1$. It also has to pay an extra of $ph_1x\tau$ in the first period and $ph_2x\tau$ in the second period. Thus, a firm's expected tax payment in the two periods are $(((1-h_1)p-c)x_1+ph_1x_1\tau)t$ and $(((1-h_2)p-c)x_2+ph_2x_2\tau)t$.

Let δ be the time discount rate.

Thus, a firm's life time expected profit is as follows:

$$E(\pi) = \underbrace{(p - c - pg(h_1))x_1}_{\text{profit before tax period 1}} - \underbrace{[(1-h_1)p - c + h_1\beta p\tau]t_1x_1}_{\text{expected tax payment in period 1}}$$
$$+\,\delta\,\underbrace{(p - c - pg(h_2) - pf(\Delta h))x_2}_{\text{profit before tax period 2}} - \delta\,\underbrace{[(1-h_2)p - c + h_2\beta p\tau]t_2x_2}_{\text{expected tax payment in period 2}} \tag{2.5}$$

FOC w.r.t h_1

$$h_1 : [-\partial g/\partial h_1 - (-1 + \beta\tau)t_1]x_1 + \delta(\partial f/\partial \Delta h)x_2 = 0 \tag{2.6}$$

It follows that

$$\frac{\partial g}{\partial h_1}x_1 - \delta\frac{\partial f}{\partial \Delta h}x_2 = (1 - \beta\tau)p_1t_1x_1 \tag{2.7}$$

FOC w.r.t h_2

$$h_2 : \frac{\partial g}{\partial h_2} + \frac{\partial f}{\partial \Delta h} = (1 - \beta\tau)pt_2 \tag{2.8}$$

From 2.8, I have $1 - \beta\tau > 0$ since I assume that $g(.)$ and $f(.)$ are increasing functions. Intuitively, if $\beta\tau > 1$, there would be no evasion because the cost of evasion is always higher than its benefit.

Comparative statics: What happen to the evasion rates in the first period and the second period, h_1 and h_2, when the tax rate in the first period t_1 changes?

Differentiating LHS and RHS of equation 2.7 w.r.t t_1 gives us:

$$\frac{\partial^2 g}{\partial h_1^2}x_1\frac{\partial h_1}{\partial t_1} + \delta\frac{\partial^2 f}{\partial \Delta h^2}x_2\frac{\partial h_1}{\partial t_1} - \delta\frac{\partial^2 f}{\partial \Delta h^2}x_2\frac{\partial h_2}{\partial t_1} = (1 - \beta\tau)px_1$$

Thus

$$\frac{\partial h_1}{\partial t_1}\left(\frac{\partial^2 g}{\partial h_1^2}x_1 + \delta\frac{\partial^2 f}{\partial \Delta h^2}x_2\right) - \frac{\partial h_2}{\partial t_1}\delta x_2\frac{\partial^2 f}{\partial \Delta h^2} = (1 - \beta\tau)p_1x_1 \tag{2.9}$$

Differentiating LHS and RHS of equation 2.8 w.r.t t_1 gives us:

$$\frac{\partial^2 g}{\partial h_2^2}\frac{\partial h_2}{\partial t_1} - \frac{\partial^2 f}{\partial \Delta h^2}\frac{\partial h_1}{\partial t_1} + \frac{\partial^2 f}{\partial \Delta h^2}\frac{\partial h_2}{\partial t_1} = 0$$

Thus

$$\frac{\partial h_1}{\partial t_1}\underbrace{\left(\frac{\partial^2 f}{\partial \Delta h^2}\right)}_{\text{A}} = \frac{\partial h_2}{\partial t_1}\underbrace{\left(\frac{\partial^2 g}{\partial \Delta h^2} + \frac{\partial^2 g}{\partial h_2^2}\right)}_{\text{B}} \tag{2.10}$$

Substituting $\partial h_2/\partial t_1$ in equation 2.10 to equation 2.9, I get

$$\frac{\partial h_1}{\partial t_1}\left[\frac{\partial^2 g_1}{\partial h_1^2}x_1 + \delta x_2\frac{\partial^2 f}{\partial \Delta h^2} - \delta x_2\frac{(\frac{\partial^2 f}{\partial \Delta h^2})^2}{(\frac{\partial^2 f}{\partial \Delta h^2} + \frac{\partial^2 g}{\partial h_2^2})}\right] = (1-\beta\tau)px_1 \tag{2.11}$$

It implies

$$\frac{\partial h_1}{\partial t_1}\underbrace{\left[\frac{\partial^2 g}{\partial h_1^2}x_1(\frac{\partial^2 f}{\partial \Delta h^2} + \frac{\partial^2 g}{\partial h_2^2}) + \delta x_2\frac{\partial^2 f}{\partial \Delta h^2}\frac{\partial^2 g}{\partial h_2^2}\right]}_{C>0} = \underbrace{(1-\beta\tau)}_{D>0}px_1 \tag{2.12}$$

Model predictions:

- Since C and $D > 0$, $\frac{\partial h_1}{\partial t_1} > 0$. Thus,the evasion rate decreases in the first period when the tax rate in the first period decreases. This implies higher profits in the tax cut year compared to years prior the tax cut.

-The evasion rate responds less to a temporary tax cut responds less than to a permanent tax cut.

-The evasion rate in the second period h_2 decreases as tax rate t_1 decreases. This implies higher profits in the year immediately after the tax cut compared to years prior to the tax cut.

-Since $A < B$, the evasion rate in period 2 decreases less than the the decrease in the evasion rate in period 1 when the tax rate in the first period t_1 decreases. This implies higher profits in a tax cut year compared to the year immediately after the tax cut.

2.11 Acknowledgements

Chapter 2, in part, is currently being prepared for submission for publication of the material

Chapter 3

The Differential Correlation of Job Training on Labor Market Outcomes in STEM and non-STEM Fields

Abstract This paper is the first paper to examine the differential correlations of job training on labor market outcomes in STEM versus non-STEM fields and in fast-changing STEM versus other STEM fields. Cross sectional results indicate that training in STEM fields is associated with lower salary gains than in non-STEM fields. Training in fast-changing STEM fields is also associated with lower salary gains than in non-STEM fields. Longitudinal (panel) data with fixed effects regressions in the 1990s present mixed results on employment status and salary. Training in STEM fields is associated with a lower probability of being employed but a higher salary conditional on employment compared to non-STEM fields. In contrast, training in fast-changing STEM fields is associated with a higher probability of being employed but a lower salary conditional on employment compared to other STEM fields.

3.1 Introduction

Labor markets in the STEM (science, technology, engineering and math) fields have long been of interest to both policymakers and social scientists. There are concerns that we do not have enough STEM workers for the employers who seek to hire them, creating a threat to national competitiveness and growth. The question of whether or not there are enough STEM workers, or whether there is in fact a shortage, has been the focus of much scholarly and political debate (see, for example, Teitelbaum 2014; Atkinson and Mayo 2010; Rothwell 2014). On this point, also, the National Academies have been active, putting their weight squarely on the side of the notion that America lacks adequate STEM skills (Committee on Prospering in the Global Economy of the 21st Century 2005; Members of the 2005 Rising Above the Gathering Storm Committee 2010).

A theme in these recommendations conforms to the demands of many tech business leadersthat a shortage of STEM workers with the adequate skills is a reason to open more opportunities for immigrants with employer-desired skilled to come to the U.S. (Martin 2012). Critics counter that an increase in the labor supply would be a mistake because a large percentage of STEM workers do not work in STEM jobs, and there is little evidence of wage increases among STEM workersone of the hallmarks of a skill shortage (Teitelbaum 2014).

A possible way to reconcile the two sides of the debate presents itself: that there are plenty of STEM workers, but they lack the skills that employers demand due to skills obsolescence. Working in an environment of continual scientific and technological innovation and change could put a premium on work-related training and the continual updating of STEM skills. The foregoing provides the motivation of this paper: what outcomes are related to training in STEM in comparison with non-STEM fields? More specifically, what are the differential associations of job training on labor market outcomes (salary and employment status) in STEM and non-STEM fields? While there are concerns about the quality of the American workforce, no one is arguing that there is a shortage of non-STEM workers and for access to foreigners with degrees outside of STEM. If STEM labor markets are showing a skill scarcity, or a skill mismatch (Handel 2003), we might expect training related to skill upgrading to be more associated with positive outcomes in the labor market for STEM workers than with non-STEM workers.

This same logic can be extended to comparisons within STEM jobs. There are reasons to believe that some STEM fields will experience skills obsolescence faster than other fields. Building on Machlups (1962) idea of the half-life of knowledge, or the time it takes for half of all knowledge in a field to be superseded by new knowledge, some experts have argued that some STEM fields experience shorter knowledge half-lives. Specifically, they estimate that the half-life of knowledge mechanical engineering to be 7.5 years, for electrical engineering 5 years, and for software engineering, only 2.5 years (Smerdon 1996; also see Trimmer, Blanton, and Schambach 1998). Moreover, because knowledge of specific technologies is more valuable than experience in engineering (Brown 2009), we might expect training to be more associated with higher wages in these fast-changing engineering fields than other jobs. In addition, if rapid technological change makes skills become obsolete more quickly in STEM than in non-STEM, we would expect workers in STEM who received their degree several years in the past to benefit more from training more than their counterparts in non-STEM fields.

In this paper, we use publicly available national survey of college graduate (NSCG) 1993, 2003, and 2010 cycles. We use cross-sectional regressions to analyze the association between job training and workers' salary. The cross-sectional regressions also examine the association of training and when a person receives his or her highest degree. We also use restricted longitudinal dataset or panel dataset provided by the National Science Foundation in the 1990s to run fixed effect regressions. The fixed effect model examines the association between training and salary. It also examines the association between training and employment status (being employed versus unemployed or out-of-labor-force).

Cross-sectional analyses show that, in contrast to our expectations, training in STEM fields was associated with 6 or 7 percent decrease in salary compared with non-STEM fields. Training in mechanical, electrical, or software engineering fields was (fast-changing STEM fields) associated with 12 percent lower in salary than in other STEM fields. In addition, we do not find evidence that training created salary differences among workers who recently received their highest degree versus those who received it some time ago in the past.

Results of fixed effects regressions are mixed. Training in the STEM fields was associated with 6 percent lower in probability of being employed than in non-STEM fields. However, training in the fast-changing STEM fields was associated with 6 percent higher in probability of being employed than in other STEM fields. In contrast, conditional on being employed, training in STEM fields was associated with 9 percent increase in salary compared to that in non-STEM fields. Training in the fast-changing STEM fields was associated with 10 percent decrease in salary than that in other STEM fields.

This paper relates to the skill-obsolete literature. It has long been recognized that transitions to technological, innovation-based, post-industrial economies present great challenges for all workers (Bell 1973; Hodson, Hooks and Rieble 1992; Liu and Grusky 2013). Research on skill-biased technological change has focused on how technology changes such as computerization can limit the job opportunities for workers with less education (Goldin and Katz 2008), but workers at the higher end of the job market, in the so-called STEM careers (science, technology, engineering and math) can be vulnerable as well (Barley and Kunda 2004; Smith 2010), especially because hiring criteria for jobs at the higher end of the market tend to emphasize technical skill over other qualities (Osterman 1995).

If rapid technological change makes firms, and thus job markets, unstable, and creates rapid obsolescence in worker skills, STEM workers may be especially vulnerable. Whatever its benefits for overall economic growth, the skill obsolescence that may arise in the wake of rapid technological change creates a down side–negative externalities–even for educated workers(De Grip and Van Loo 2002; also see Brynjolfsson and McAfee 2014).

There is also a very long tradition of research on how technological change can render worker skills obsolete (Carver 1908; Tugwell 1931). This early work often focused on what is now called technological unemployment. More recent studies have identified two types of skill obsolescence: technical (which originates in the worker) and economic (which originates in the job or work environment) skills obsolescence (Neumann and Weiss 1995). In practice, these may not be easy to distinguish. For example, aging workers may find it more difficult to keep up with changes in the workplace; it that case, both factors are at play. A study of college graduates in the Netherlands found that skills learned in college were obsolete in about seven years, and self-reported obsolescence occurred in all fields, not just technical ones, and was not related to training, though STEM skills were not explored (Allen and Van der Velden 2002).

A possible solution to problems created by skills mismatches is job-related training, or lifelong learning, that can periodically upgrade worker skill sets as employer demands change (OECD 1996). Research on skill obsolescence shows that training is important in knowledge-based tasks in order to stave off obsolescence (Backes-Gellner and Janssen 2009; Kalleberg 2011). In this paper, we examine training in STEM and non-STEM fields as a way to measure the importance of training in alleviating worker's skill obsolesce.

This paper also contributes to the literature on the impacts of training on labor market outcomes. This research finds some positive outcomes associated with training. A study based on a 1982 survey of randomly selected employers and a 1987 survey of National Federation of Independent Businesses asked for information regarding employees, finding that reports that training occurred was related to perceived productivity increases (Bishop 1994). Using a survey about 1600 manufacturers and 1300 nonmanufacturers, Black and Lynch (1996) found that the number of employees involved in current training is associated with lower productivity, but higher numbers involved in past training are associated with greater productivity, as are the number of hours spent in formal, off-the-job training. For non-manufacturers, reports of computer training are associated with greater productivity regardless of industry.

Productivity may go up in some circumstances, but what about wages? Survey evidence from 1969 through 1980 shows wage increases of about 10 percent associated with training (Lillard and Tan 1992). Firm-level data merged with a survey of workers in Italy (self reports on whether participation in training occurred) in the 1990s showed strong positive effects on productivity, and a smaller and less robust positive effect on wages (Conti 2005). A similarly-designed study but focused workers in Britain found similar effects (Dearden, Reed and Van Reenen 2006). Bartel (1995) studied employment records at a single firm in the U.S. and found that training had positive effects on wage growth and perceptions of job performance. There is also evidence that training is associated with staying in the workforce (Blundell et al 1999).

Regarding who benefits the most from training, some studies also indicate that women enjoy higher returns from training than do men (Booth 1991; Greenhalgh and Stewart 1987). There are also indications that older workers show poorer training performance than younger workers (Kubeck et al 1996).

However, this literature typically has no focus on any particular sector such as STEM, and often include those with less than a college education. To our knowledge, this paper provides the first attempt to compare training in STEM and non-STEM fields.

3.2 Data

We examine two datasets. The first dataset is the National Survey of College Graduate (NSCG) in 1993, 2003, and 2010. The second dataset is the longitudinal dataset in the 1990s.

The NSCG has been conducted since the 1970s to provide data on the characteristics of college graduates in the US. The NSCG in the 1993, 2003, and 2010 cycles provided coverage of the nation's college-educated population as of the survey reference date.

In addition to the 1993, 2003, and 2010 survey cycles, the NSCG was also conducted every 2 years from 1993 to 1999 and every 3 years from 2003 to 2008. These surveys focused on the science and engineering (STEM) workforce component of the college-educated population. These datasets only includes individuals who either earned a STEM degree or were at one point working in STEM. From these surveys, the NSF constructed two longitudinal datasets in the periods of 19901999 and 20002009. The 1990s longitudinal dataset includes the year of 1993, 1995, 1997, and 1999. The 2000s longitudinal dataset includes the year of 2003, 2006, and 2010.

In this paper, we only run longitudinal analyses using the 1990s because the NSF provides longitudinal weights for the 1990s, but does not provide weights for the 2000s. We further restrict the sample to a balanced longitudinal dataset. In the 1990s balanced longitudinal dataset, people who either worked in STEM fields in 1993 or got a degree in STEM.

For both cross-sectional and longitudinal analyses, we further exclude individuals whose main reasons for training was because of licensing or certificate or because it was expected by their employers.

3.3 Types of training?

What do we know about training outcomes? Work-related training has long been a topic of social science research due to its importance in the creation of work-ready skills. However, we know little for certain about the training landscape, such as the frequency of training or overall trends, because there are no comprehensive data that are collected. The spotty evidence that is available, however, suggests that work-related training overall is in decline due to employer concerns about costs and losing trained workers (Cappelli 2012). Little is known about the prevalence of training in STEM jobs.

In our surveys, the question about training asks if an individual received job-related training in the past 12 months. According to figure 3.1, around 50 percent of individuals in the sample received training in the 1990s and the 2000s. This figure slightly decreased over time. According to figure 3.2 and 3.3, the main reason for workers to get training in the 1990s and the 2000s was to further improve skills in their current field.

Among all surveys conducted by the NSF, the survey in 1997 provided a little more details about the nature of the training. According to the 1997 survey, 80 percent who got training was getting trained in their occupational fields. Less than 20 percent of individuals who got training in their current fields paid for their training themselves. Also according to the 1997s survey, the median number of training days was 3-5 days, and the mean was 5-6 days. There was no difference between the length of training in STEM and non-STEM fields.

3.4 Empirical Strategy

3.4.1 Cross-sectional analysis

$$Y_{it} = \alpha_0 + year_t + \alpha_1 SE_{it} + \alpha_2 * train_{it} + \alpha_3 SE_{it} * train_{it} + \epsilon_{it} \quad (3.1)$$

Y_{it} is labor market outcome variable, which is salary in this regression specification

$SE_{it} = 1$ if an individual worked in a SE field, and 0 if the individual did not work in a SE field.

$train_{it} = 1$ if an individual received training, and 0 otherwise.

α_1 represents the differential salary between workers in STEM and non-STEM fields.

α_2 represents the differential salary between workers who got trained and those who do not.

α_3 is the coefficient of interest. It is the coefficient of the interaction between being in STEM fields and getting training. It represents the salary difference associated with training in STEM fields versus non-STEM fields.

The same regression specification is used when we compare fast-changing STEM and other STEM fields, and number of years when a person receives his or her highest degree.

3.4.2 Longitudinal analysis

$$Y_{it} = \beta_i + \beta_1 year_t + \beta_2 train_{1997} * year_t + \beta_3 SE_{1997} * year_t$$
$$+\beta_5 train_{1997} * SE_{1997} * year_t + \epsilon_{it} \qquad (3.2)$$

Y_{it}: labor market outcomes (employment status of individual i at year t). We examine two labor outcome variables: salary and whether a person is employed (versus. unemployed or out-of-labor force).

$train_{1997} = 1$ if an individual received training in 1997, and 0 otherwise

$SE_{1997=1}$ if an individual worked in a STEM field in 1997, and 0 otherwise

Individual fixed effects β_i.

Double differences: trained vs. non-trained individuals before and after receiving training. Coefficient β_2 of $train_{1997} * year_t$ represents salary or employment status differences associated with training in 1997 in different years.

Triple differences: STEM (trained and non-trained) vs. non-STEM (trained and non-trained) before and after, coefficient β_5 of $train_{1997} * SE_{1997} * year_t$.

The sample is restricted to individuals who did not receive training in 1993 and 1995. The regression equation estimates the effect of training in 1997 on labor market outcomes in 1997 and in 1999 among individuals who did not receive training in 1993 and 1995. The comparison year is 1993. The placebo year is 1995.

The identifying assumption of the triple differences approach is that individuals who received training in 1997 were similar to individuals who did not receive training in 1997. To test for this assumption, we test if the training status in 1997 affected labor market outcomes in 1995 in STEM and non-STEM fields separately. We also test if the training status in 1997 affected labor market outcomes in 1995 differentially in STEM and non-STEM fields. The identifying assumption holds if labor market outcomes in 1995 were not correlated with the training status in 1997.

β_2 is the coefficient of yearly return to training in 1997 in non-STEM fields. If training in 1997 did not affect labor market outcomes in 1995 in non-SE fields, β_2 in 1995 would not be statistically different from 0.

β_5 is the coefficient of differential yearly return to training in 1997 in STEM fields versus non-STEM fields. If training in 1997 did not affect labor market outcomes in 1995 differentially in STEM fields compared to non-STEM fields,β_5 in 1995 would not be statistically different from 0.

The coefficient of interest is β_5 in year 1997 and year 1999.

This specific identification strategy aims to get as close to a causal estimate as we can. This identification strategy tests for persistent differences or trends in salary or employment status between individuals who received training in 1997 in STEM versus non-STEM fields. Since the surveys in the 1990s was conducted every two years, the regression specification does not capture possible pre-training differences that might happen shorter than the two year time. For example, individuals who got trained in 1997 in STEM, albeit having a similar salary and probability of being employed in 1993 and 1995, might still be different from their counter-parts in non-STEM fields in 1996. Unfortunately, the data does not allow us to test for differences that happened in the shorter time frame than two years.

We also run the same regression specification to examine association between training and salary or employment status in fast-changing STEM versus other STEM fields.

3.5 Results

3.5.1 Cross-sectional analyses using NSCG 1993, 2003, 2010

In this section, we only examined salary differences associated with training in STEM and non-STEM. We do not examine employment status. It is because we cannot categorize individuals into STEM or non-STEM fields if they were un-employed or out-of-labor force.

Column 1 of table 3.1 and table 3.2 present results of unweighted regressions. Column 2 of table 3.1 and table 3.2 present results of regressions using survey sample weight. In table 3.1, coefficients of train_stem and train_stem_degree are coefficients of interest. Coefficient train_stem represents the association between training and salary differences in STEM and non-STEM fields. Coefficients train_stem_degree represents whether the association between training and salary differences in STEM versus non-STEM fields depends on how many years since the person receives their highest degree.

We find that training was associated with a 12 or 13 percent increase in salary in 1993, 2003, and 2010. Workers in STEM fields, on average, had higher salary than workers in non-STEM fields. This difference was about 20 percent. Interestingly, workers who received training in STEM were associated with lower gain in salary compared to their counterparts in non-STEM. This difference was about 6 or 7 percent. In addition, we do not find evidence that training was associated with differences in labor market outcomes depending how many of years since the person received his or her highest degree in STEM versus non-STEM (column 2 table 3.1).

The results on female workers were consistent with the literature. On average, females earned 30 to 40 percent less than their male counterparts. Training among females was associated with a 12 to 18 percent increase in salary compared to their male counterparts.

We find similar results when we compare training in fast-changing STEM fields versus other STEM fields in table 3.2. Coefficients of interest are the coefficient of train_stem_fast and the coefficient of train_stem_fast_year_degree. Individuals who got trained in STEM fields on average had a 13 percent higher in salary than those who did not (column 2 table 3.2). Workers in fast-changing STEM fields earned higher salary than those in other STEM fields. Training in fast-changing STEM fields, on average, was associated with lower salary than other STEM fields. We do not find evidence that training was associated with differences in labor market outcomes depending how many of years since the person received his or her highest degree in fast-changing STEM fields versus other STEM fields (column 2 table 3.2).

3.5.2 Longitudinal analyses in the 1990s

In table 3.3 ,3.4, 3.5, and 3.6, coefficient β_2 in 1995 of equation 3.2 is the coefficient of variable train_1997_1995. In table 3.3 and 3.3, coefficient β_5 in 1995 of equation 3.2 is the coefficient of variable train1997_SE1997_1995. In table 3.4 and 3.4, coefficient β_5 in 1995 of equation 3.2 is the coefficient of variable train1997_stem_fast1997_1995. Table 3.3 ,3.4, and 3.6, coefficient β_2 in 1995 of equation 3.2 show that these coefficients are not statistically different from 0. Therefore, the regressions that pass the placebo test are employment status in STEM versus non-STEM, in fast-changing STEM versus other STEM, and salary in fast-changing STEM versus other STEM fields. The regression of salary on training in STEM fields versus non STEM fields does not pass the placebo test.

Since individuals who worked in a STEM field in 1997 were all employed in 1997, I omit year 1997 in employment regressions (table 3.3 and 3.4).

In table 3.3 and 3.5, the coefficients of interest are coefficients of the triple differences variables train1997_SE1997_1997 and train1997_SE1997_1999 . These coefficients represent associations between training in 1997 and salary of employment status in 1997 and in 1999 in STEM versus non-STEM fields. Table 3.3 shows that training in 1997 in STEM fields was more likely to associate with lower probability of being employed in 1999 than in non-STEM fields. Table 3.4 shows that sign flipped when we compare fast-changing STEM versus other STEM fields. More specifically, training in 1997 in fast-changing STEM fields was more likely to associate with higher probability of being employed in 1999 than in other STEM fields.

Table 3.5 shows that conditional on being employed, training in STEM fields in 1997 was associated with higher salary in 1999 than in non-STEM fields. However, training in fast-changing STEM fields in 1997 was associated with lower salary in 1999 than in other STEM fields.

3.6 Conclusion

This paper is the first paper to compare the differential association of job training on labor market outcomes in STEM versus non-STEM fields, and in fast-changing STEM versus other STEM fields. We hope to contribute to the discussions about skills obsolescence and the shortage of STEM workers in the US.

Cross sectional results indicate negative associations of training on salary among STEM fields compared to non-STEM fields and among fast-changing STEM fields compared other STEM fields.

Longitudinal analyses with fixed effects regressions in the 1990s present mixed results on employment status and salary in STEM versus non-STEM. On the one hand, training in STEM is associated with a lower probability of being employed but a higher salary conditional on employment compared to non-STEM fields. On the other hand, regressions that compare fast-changing STEM and other STEM fields present opposite results. Training in fast-changing STEM is associated with a higher probability of being employed but a lower salary conditional on employment compared to other STEM fields.

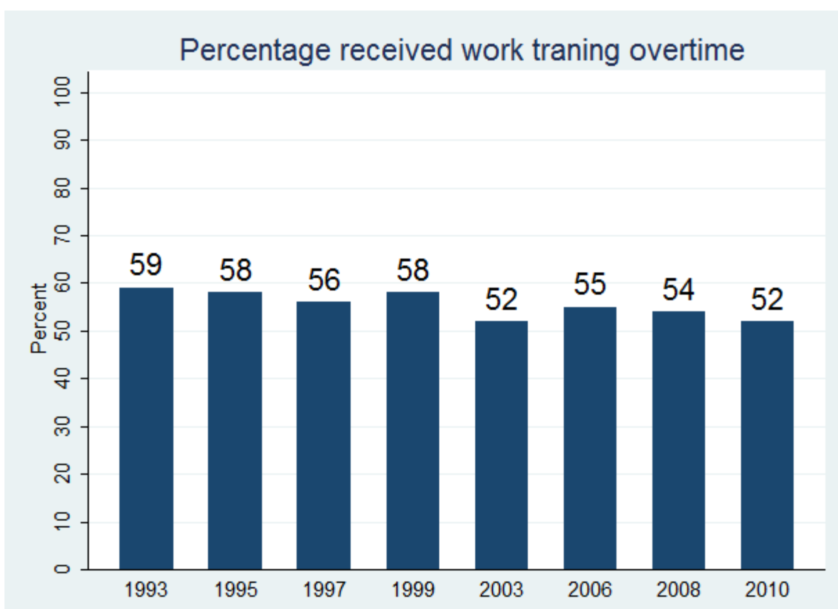
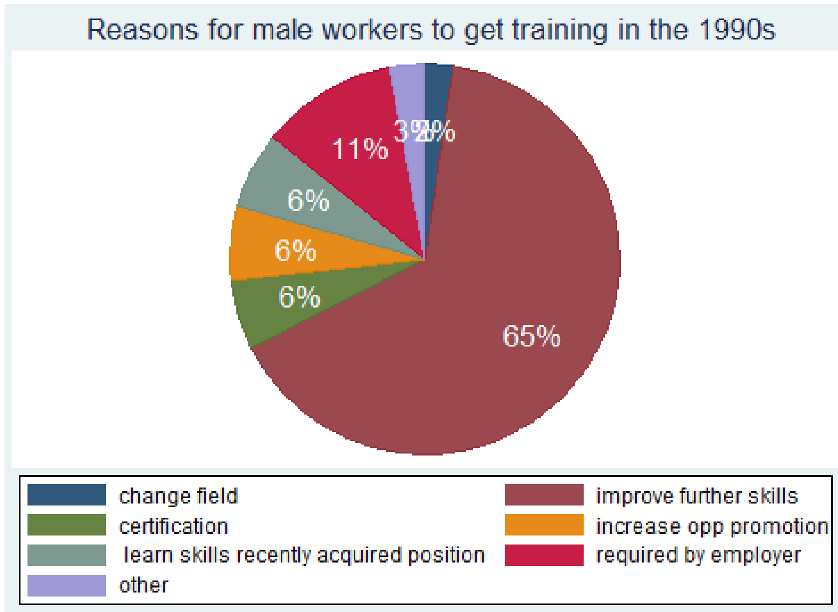


Figure 3.1: Percent who receive training over-time

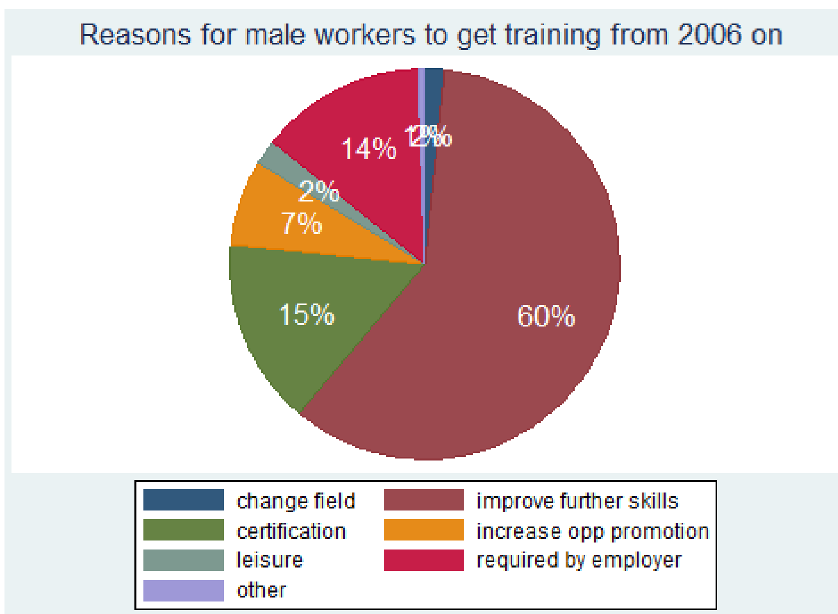
Table 3.1: Salary and training in STEM and nonSTEM fields

	(1) unweighted	(2) weighted
train	0.130***	0.120***
	(0.00729)	(0.0218)
stem	0.203***	0.212***
	(0.0106)	(0.0231)
train_stem	-0.0698***	-0.0732***
	(0.0119)	(0.0256)
female	-0.343***	-0.476***
	(0.00635)	(0.0186)
train_female	0.128***	0.182***
	(0.00719)	(0.0213)
year_from_degree	0.00860***	0.00696***
	(0.000374)	(0.00105)
train_from_degree	0.00156***	0.00210**
	(0.000337)	(0.000980)
age	0.0792***	0.0936***
	(0.00121)	(0.00366)
age squared	-0.000890***	-0.00106***
	(0.0000136)	(0.0000405)

Robust standard error. Exclude training for licenses and certificates. Coefficient of train_stem and train_stem_degree are coefficients of interest. Coefficient train_stem represents the association between training and salary differences in STEM and non-STEM fields. Coefficients train_stem_degree represents whether the association between training and salary differences in STEM versus non-STEM fields depends on how many years since the person receives their highest degree. Regressions control for year dummies, citizenship, race and ethnicity, highest degree, job fields, employer size, and employer corporate structure.

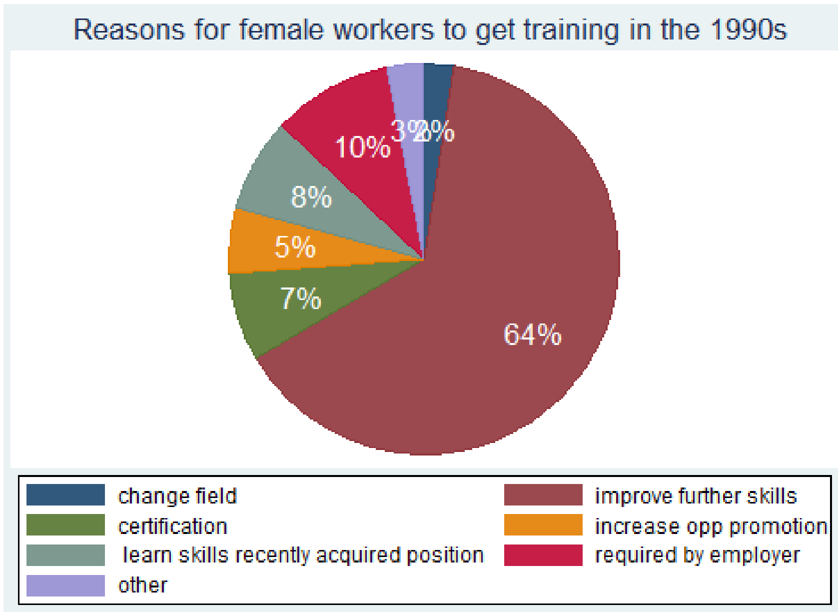


(a) the1990s

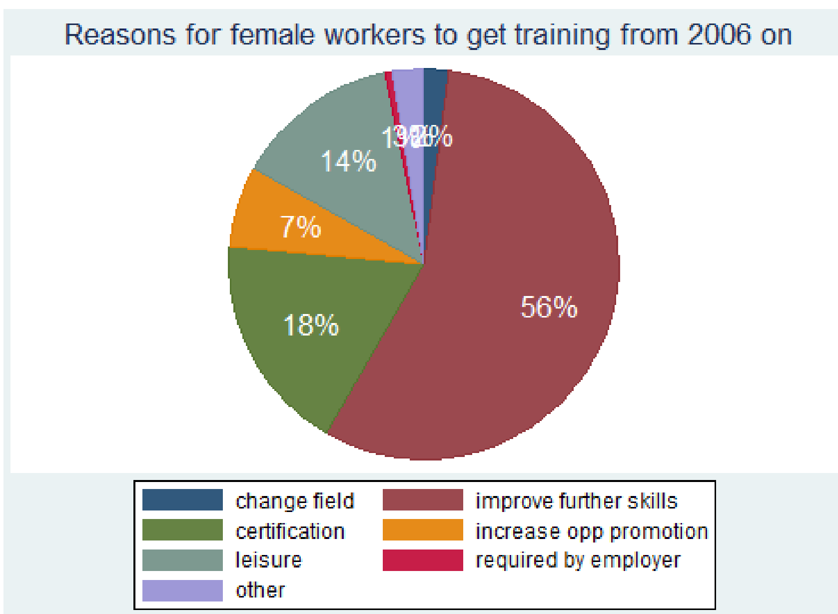


(b) the 2000s

Figure 3.2: Reasons for males to get training



(a) the1990s



(b) the 2000s

Figure 3.3: Reasons for females to get training

Table 3.2: Salary and training in STEM and nonSTEM fields

	(1) unweighted	(2) weighted
train	0.130***	0.120***
	(0.00729)	(0.0218)
stem	0.203***	0.212***
	(0.0106)	(0.0231)
train_stem	-0.0698***	-0.0732***
	(0.0119)	(0.0256)
female	-0.343***	-0.476***
	(0.00635)	(0.0186)
train_female	0.128***	0.182***
	(0.00719)	(0.0213)
year_from_degree	0.00860***	0.00696***
	(0.000374)	(0.00105)
train_from_degree	0.00156***	0.00210**
	(0.000337)	(0.000980)
age	0.0792***	0.0936***
	(0.00121)	(0.00366)
age squared	-0.000890***	-0.00106***
	(0.0000136)	(0.0000405)

Robust standard error. Exclude training for licenses and certificates. Coefficient of train_stem_fast and train_stem_fast_year_degree are coefficients of interest. Coefficient train_stem_fast represents the association between training and salary differences in fast-changing STEM and other STEM fields. Coefficients train_stem_fast_year_degree represents whether the association between training and salary differences in fast-changing STEM versus other STEM fields depends on how many years since the person receives their highest degree.

Table 3.3: Employment status and training in STEM and nonSTEM fields using panel data in the 1990s

	(1) unweighted	(2) weighted
[1em] train_1997_1995	0.00501	0.00976
	(0.0138)	(0.0166)
train_1997_1997	.	.
	.	.
train_1997_1999	0.0874***	0.0883***
	(0.0156)	(0.0200)
SE_1997_1995	-0.0153**	-0.0260**
	(0.00774)	(0.0118)
SE_1997_1997	.	.
	.	.
SE_1997_1999	-0.0578***	-0.0660***
	(0.00971)	(0.0135)
train1997_SE1997_1995	-0.00863	-0.00999
	(0.0156)	(0.0221)
train1997_SE1997_1997	.	.
	.	.
train1997_SE1997_1999	**-0.0561*****	**-0.0619****
	(0.0181)	(0.0249)
Constant	0.921***	0.879***
	(0.00198)	(0.00345)
N	24873	24651
r2	0.0121	0.0165

Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Robust standard error. Exclude training for licenses and certifcates. The coefficient of interest is train1997_SE1997_1999 . This coefficient represents the association between training in 1997 and employment status in 1999 in STEM versus non-STEM fields.

Table 3.4: Employment status and training in fast-changing STEM fields and other STEM fields using panel data in the 1990s

	(1) unweighted	(2) weighted
[1em] train_1997_1995	-0.00773	-0.00565
	(0.00776)	(0.0176)
train_1997_1997	.	.
	.	.
train_1997_1999	0.0231**	0.00942
	(0.0101)	(0.0179)
stem_fast_1997_1995	-0.0105	-0.00822
	(0.0117)	(0.0187)
stem_fast_1997_1997	.	.
	.	.
stem_fast_1997_1999	-0.000197	-0.0211
	(0.0155)	(0.0219)
train1997_stem_fast1997_1995	0.0234	0.0194
	(0.0219)	(0.0315)
train1997_stem_fast1997_1997	.	.
	.	.
train1997_stem_fast1997_1999	**0.0408***	**0.0606***
	(0.0243)	(0.0323)
Constant	0.959***	0.946***
	(0.00213)	(0.00387)
N	14700	14508
r2	0.00903	0.00398

Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Robust standard error. Exclude training for licenses and certifcates. The coefficient of interest is train1997_stem_fast1997_1999 . This coefficient represents the association between training in 1997 and employment status in 1999 in fast-changing STEM versus other STEM fields.

Table 3.5: Salary and training in STEM and nonSTEM fields using panel data in the 1990s

	(1) unweighted	(2) weighted
[1em] train_1997_1995	0.0449	0.0762*
	(0.0343)	(0.0422)
train_1997_1997	0.0715**	0.0553
	(0.0292)	(0.0344)
train_1997_1999	0.100***	0.0870**
	(0.0342)	(0.0440)
SE_1997_1995	0.129***	0.153***
	(0.0213)	(0.0305)
SE_1997_1997	0.0563***	0.0579**
	(0.0189)	(0.0252)
SE_1997_1999	0.112***	0.0960***
	(0.0208)	(0.0298)
train1997_SE1997_1995	-0.0358	-0.0526
	(0.0383)	(0.0514)
train1997_SE1997_1997	-0.0114	0.0322
	(0.0339)	(0.0420)
train1997_SE1997_1999	**0.0135**	**0.0933***
	(0.0381)	(0.0512)
Constant	10.68***	10.48***
	(0.00468)	(0.00851)
N	30313	30087
r2	0.0526	0.0451

Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Robust standard error. Exclude training for licenses and certifcates. The coefficient of interest is train1997_SE1997_1999 . This coefficient represents the association between training in 1997 and salary gains in 1999 in STEM versus other STEM fields.

Table 3.6: Salary and training in fast-changing STEM fields and other STEM fields using panel data in the 1990s

	(1) unweighted	(2) weighted
[1em] train_1997_1995	0.00263	0.0199
	(0.0194)	(0.0357)
train_1997_1997	0.0466**	0.0846***
	(0.0192)	(0.0280)
train_1997_1999	0.106***	0.207***
	(0.0187)	(0.0301)
stem_fast_1997_1995	-0.00222	0.00454
	(0.0278)	(0.0437)
stem_fast_1997_1997	0.0208	0.0449
	(0.0252)	(0.0377)
stem_fast_1997_1999	0.0449*	0.119***
	(0.0256)	(0.0450)
train1997_stem_fast1997_1995	0.0326	0.0133
	(0.0430)	(0.0637)
train1997_stem_fast1997_1997	0.0611	0.00583
	(0.0426)	(0.0554)
train1997_stem_fast1997_1999	**0.0241**	**-0.104***
	(0.0410)	(0.0591)
Constant	10.77***	10.70***
	(0.00474)	(0.00800)
N	18471	18273
r2	0.0816	0.0855

Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Robust standard error. Exclude training for licenses and certifcates. The coefficient of interest is train1997_stem_fast1997_1999 . This coefficient represents the association between training in 1997 and salary gains in 1999 in fast-changing STEM versus other STEM fields.

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