Title
Essays on the Economics of Taxation

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Publication Date
2018

Peer reviewed|Thesis/dissertation

UNIVERSITY OF CALIFORNIA SAN DIEGO

Essays on the Economics of Taxation

A dissertation submitted in partial satisfaction of the
requirements for the degree of Doctor of Philosophy

in

Economics

by

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2018

The Dissertation of Xiaxin Wang is approved and is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California San Diego

2018

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ACKNOWLEDGEMENTS

I would like to acknowledge my advisor Roger Gordon for his consistent support and guidance throughout many years. My interest on taxation is mainly sparked by Roger, via his papers and his teaching. My understanding of taxation is mainly via digesting his comments on my attempts of various topics on taxation. His very insightful comments always push me to learn more about a topic. His style of guidance helps shape my way of doing research and asking questions. His personality also impresses me as a role model. I am grateful to be Roger's student in my life.

I am also very grateful to my other members of thesis committee for their invaluable advice and help: Professor Julie Cullen, Professor Eli Berman, Professor Joey Engelberg, Professor Ruixue Jia, Professor Thomas Baranga. I am particularly grateful to Julie and Eli for their help throughout my job market season, during which they have provided great help and considerable advice. During my study at UCSD, I have benefited from discussions and comments from many other faculty members, officemates, classmates, and friends at Department of Economics. I am grateful to all of them.

Last but not the least, I am grateful to my family, who provide unconditional support to me. Their patience and understanding are the engine of my persistence. I owe my deepest thanks to them.

Chapter 1 is currently being prepared for submission for publication of the material. The authors are Daixin He, Langchuan Peng, Xiaxin Wang. The dissertation author, Xiaxin Wang, was the primary investigator and author of this paper.

Chapter 2 is currently being prepared for submission for publication of the material. The dissertation author, Xiaxin Wang, was the sole author of this paper.

Chapter 3 is currently being prepared for submission for publication of the material. The dissertation author, Xiaxin Wang, was the sole author of this paper.

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ABSTRACT OF THE DISSERTATION

Essays on the Economics of Taxation

by

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Doctor of Philosophy in Economics

University of California San Diego, 2018

Professor Roger Gordon, Chair

This dissertation explores three topics in the economics of taxation. In Chapter 1, I (coauthored with Daixin He and Langchuan Peng) conduct the first formal comparison of two approaches to estimate the elasticity of taxable income (ETI), a central parameter in the public finance literature. We find that while the bunching ETI estimates are small and stable, the tax reform ETI estimates increase concavely over time and are much larger in the long run. These stylized facts imply that very different behavioral responses are captured by the two approaches. Our findings imply that although the bunching approach has advantages in identification and application, the tax reform ETI estimates are generally more relevant for policy making due to the behavioral responses they are able to capture. In Chapter 2, I examine the reported income-consumption relation between employees and the self-employed and explore its implications on tax evasion and avoidance for U.S. households. I find that, on average for the self-employed in the U.S., the amount of tax evasion is comparable to that of tax avoidance. But the corporate self-employed rely more on tax avoidance while the non-corporate self-employed rely more on tax evasion. In Chapter 3, I study the effects of the financial transaction taxes (FTT) in Latin America on government revenues and tax bases. I find that the FTT have no crowding-out effect on the revenue of other taxes as a whole. But there seems a significantly positive association between the FTT and the CIT revenue, which suggests that the FTT may be beneficial to corporate activities. In addition, I find a negative effect of the FTT on its own tax base.

Chapter 1

Understanding the Elasticity of Taxable Income: A Tale of Two Approaches

1.1 Introduction

The elasticity of taxable income (ETI) with respect to the marginal net-of-tax rate has been a central parameter in the public finance literature since Feldstein (1995, 1999). Feldstein (1999) shows that, even with tax evasion and avoidance, the ETI is a sufficient statistic for measuring the marginal efficiency cost of tax, and is therefore very useful for welfare analysis.[1] Empirically, there are two approaches to estimate the ETI, the traditional tax reform approach (e.g. Gruber and Saez (2002), Kleven and Schultz (2014)) and a recently developed bunching approach (e.g. Saez (2010), Chetty et al. (2011), Kleven and Waseem (2013)). While the traditional tax reform approach utilizes marginal tax rate changes induced by a tax reform to identify the behavioral responses summarized in taxable income changes, the bunching approach exploits the excess mass in the income distribution around a kink,

[1]Chetty (2009) shows this is true if only a resource cost is involved in sheltering. He further shows that if sheltering is also associated with a transfer cost, then the elasticity of earnings and the resource cost of sheltering income from taxation are necessary to measure the deadweight loss of tax. Measuring the elasticity of earnings requires information on true earnings before sheltering; this requirement is rather demanding because even administrative tax data may not include incomes issued by cash and thus will underreport true earnings. Likewise, measuring the resource cost of sheltering also requires information unavailable in our administrative tax data. Due to these data limitations, we only focus on the ETI.

where the marginal tax rate (MTR) changes, to identify behavioral responses local to a kink.

In addition, these two approaches have perceived strengths and weaknesses, respectively. The tax reform approach, as argued by Feldstein (1999), can generate an ETI estimate capturing behavioral responses at all margins (e.g. labor supply, tax avoidance, and tax evasion) to a tax change. Yet to apply this approach, it is necessary to have a tax reform. Moreover, how to address the endogeneity problem associated with this approach (caused by reversal causality and omitted variable bias) has been a central issue in literature. This approach generates estimates quite sensitive to instrumenting approach and regression specification (Saez et al. (2012)). By contrast, the bunching approach can be used in any setting with kinks or notches (where the average tax rate changes) in the tax code. The identification process can be transparently illustrated simply by showing the income distribution around a kink or a notch. Endogeneity is not a problem here, and the estimates are robust.[2] Due to these clear advantages, the bunching approach has recently been adopted in many settings (Kleven (2016)). But as an empirical strategy developed only recently, papers using the bunching approach have been more focused on "proof of concept" than policy evaluation. A deeper understanding of the behavioral responses captured by the bunching approach is clearly needed.

Although these two approaches are expected to measure the same underlying parameter, empirical evidence from previous papers suggests these two approaches could yield quite different estimates. Yet until now, there has been no formal comparison between these two approaches. Such an exploration is necessary, since otherwise we face a difficult choice between the two ETI estimates when trying to make policy implications out of them. In this paper, we compare the two approaches both empirically and conceptually, and show that they capture different behavioral responses to tax changes and thus have different policy

[2]Blomquist and Newey (2017) argue that, however, it may be necessary to know the functional form of the distribution of preference heterogeneity for the ETI to be identified using the bunching approach. Einav et al. (2017) show that a same bunching pattern can be consistent with different alternative models, which would have very different policy implications.

implications.

Our empirical analysis focuses on China's personal wage income tax and is based on novel administrative data. The data cover administrative information on personal wage income tax, including monthly wage income of all wage/salary earners in a city with a population of 4-5 million, from June 2009 to December 2013. Desirable for our research purpose, China's personal wage income faces a graduated rate structure and had a reform on September 1st, 2011, after which a very wide range of income intervals have experienced marginal tax rate changes. This setting provides an ideal environment to apply both tax reform approach and bunching approach to estimate the ETI. One unique feature is that China imposes a *monthly* tax rate schedule on wage/salary, unlike most other countries where personal income faces a *yearly* tax rate schedule. This feature provides a rare opportunity to study the evolution of income responses to a tax change over a long enough time series, which is infeasible in many other settings.

To start with, we apply the standard tax reform approach (following Gruber and Saez (2002), Kleven and Schultz (2014)) and the standard bunching approach (following Saez (2010), Chetty et al. (2011), assuming no optimization frictions) to estimate the ETI. The standard tax reform approach renders an ETI of 2.42, robust to different instrumenting approaches. The bunching ETI estimates vary from 0.09 to 0.41 in several middle-high taxable income kinks. For lower taxable income kinks, there is no evidence of bunching, suggesting a zero ETI, as also observed in Kleven and Waseem (2013).[3] Consistent with previous studies that obtain ETI estimates using two approaches for the same country (US: Saez et al. (2012) and Saez (2010), Denmark: Kleven and Schultz (2014) and Chetty et al. (2011), Sweden: Blomquist and Selin (2010) and Bastani and Selin (2014)), we find that in China, the tax reform ETI also seems systematically larger than the bunching ETI.

The ETI estimates from the two standard approaches, however, are not yet directly

[3]For top kinks, since observations are too few to generate precise bunching estimates, they are not included in our bunching analysis.

comparable. The two standard approaches differ in two important aspects: time and scope. As for time, the standard tax reform approach renders 1-year, 2-year or 3-year ETI depending on specification, while the bunching approach renders an ETI with unclear time property, since it only requires cross-sectional or pooled cross-sectional data. As for scope, the tax reform approach can generate a global ETI, while the bunching approach generates a local ETI for each kink. To make the two approaches more directly comparable, we develop a revised version for each approach to ensure that they obtain ETI estimates with the same property in time and scope.

Exploiting the advantage of our monthly income panel data and the tax reform in September 2011, we develop a revised tax reform approach that generates an ETI for each (three-month) period after the tax reform. The dynamic tax reform ETI estimates are consistent with the graphical evidence on the evolution of taxable income around the tax reform. Also relying on the monthly income panel data and the 2011 tax reform, we develop a revised bunching approach to explore the bunching responses to the introduction of post-reform kinks. Since there is no evidence of bunching at the post-reform kinks prior to the tax reform, all bunching after the reform can be attributed to the reform. Therefore, we can simply apply the standard bunching approach to estimate an ETI for each post-reform period for each bunching kink. Then for each post-reform period, we derive a global bunching ETI estimate using observed ETI estimates at various bunching kinks. Here we adopt a revised version of the approach by Gelber et al. (2015) to estimate a common global ETI underlying all kinks. With optimization frictions, a common global ETI could generate different bunching behaviors (corresponding to the observed ETI estimates using standard bunching approach with no optimization frictions) at different kinks. We estimate the common underlying ETI and optimization frictions using the observed ETI estimates. If the assumption of a common underlying ETI with optimization frictions is reliable, then it should not only explain the observed bunching at middle-high kinks, but should also explain the lack of bunching at lower kinks. We use the estimated structural ETI and optimization frictions to confirm this.

Finally, the revised bunching approach yields a sequence of dynamic global ETI estimates, which are compared to the dynamic tax reform ETI estimates.

We find two key differences between dynamic tax reform ETI estimates and bunching ETI estimates. First, the tax reform ETI estimates increase concavely over time, while the bunching ETI estimates are stable over time. Second, the tax reform ETI estimates (around 4 in the long-run) are much larger than the bunching ETI (around 0.5), and the difference is statistically significant. To account for the stylized facts, we develop a simple model where individuals in each period have some probability to permanently change hours of work without paying other costs, but can temporarily adjust hours by paying additional costs. The model implies that while the tax reform approach can capture the infrequent but permanent adjustment of hours of work to tax changes, the bunching approach generally reflects temporary adjustment. With stable wage rates, the two estimators should converge to the same underlying value. But with normal wage growth, the tax reform estimates converge to the true underlying parameter, whereas the bunching estimates can be far below the true figure.[4]

The major contribution of this paper is that it provides a first formal comparison of the two main approaches estimating the ETI.[5] Empirically, we document sharp contrasts between ETI estimates using two approaches around a tax reform. Conceptually and empirically, we show that the two approaches are measuring very different behavioral responses and thus are not interchangeable in general. The main findings of this paper imply that although the bunching approach has advantages in identification and application, the tax reform ETI estimates are generally more relevant for policy making due to the behavioral responses they are able to capture.

This paper is broadly related to papers reconciling different measures of the same

[4]Although we emphasize the behavioral responses to tax changes via adjusting hours of work, other margins of change are possible, e.g. responses in income underreporting, intertemporal income shifting, and changes in labor participation.

[5]Recently we noticed that Miguel Almunia and Michael Best are working on a similar topic using UK data independently. We would appropriately cite their work once their draft is available.

policy relevant parameter. For example, Chetty et al. (2011) and Chetty (2012) try to reconcile micro and macro labor supply elasticities using adjustment costs and optimization frictions; Peterman (2016) tries to reconcile micro and macro estimates of the Frisch labor supply elasticity. Different from these papers, both the tax reform approach and the bunching approach yield *micro* estimates of a supposedly same parameter, and thus their sharp difference seems more puzzling.

In addition, this paper contributes to the large empirical ETI literature by providing the first Chinese evidence. The ETI estimates using both tax reform approach and bunching approach are both very large compared to those obtained in other countries (see Saez et al. (2012) and Saez et al. (2009) for a comprehensive review). There are several potential reasons to account for the larger ETI estimates in China. First, China's personal income tax (PIT) system has a much simpler tax base and a more salient tax schedule (the tax schedule does not depend on marital status, number of dependents, and is not inflation indexed), as opposed to the much more complicated PIT systems in countries like the U.S. and Denmark. As noted in previous literature, a simple tax code or tax reform would generate larger responses than a complicated one. Second, different personal income components (wage/salary, self-employment income, and other incomes) are taxed differently in China, as opposed to a universal personal income tax imposed in many other countries. This implies more space for income shifting between reported wage/salary and reported other incomes to save taxes. There could be other aspects (e.g. social culture, tax administration) underlying China's much larger behavioral responses to tax change.[6] Although this paper is not able

[6]The differential definitions between taxable wage income and raw wage income would also partially account for the large elasticity in a mechanical way. Consider a person with a monthly wage income of 4,000 RMB. Suppose his wage income increases to 5,000 RMB in the next period, as a response to a decreasing tax rate. Then his raw income increases by 25%. Under the standard deduction 3,500 RMB, not considering other exemptions and deductions, his taxable income increases from 500 RMB to 1,500 RMB, which implies a 200% increase in the taxable income. Overall, the relatively large standard deduction to monthly income could account for a large ETI of the monthly wage income in China. Normally, the deductions and exemptions like those in the U.S. are not so large relative to income, especially when researchers focus on the high income earners, as many researchers do. Therefore, previous papers normally do not find very large difference between the ETI and the elasticity of raw income.

to provide a comprehensive cross-country comparison, this could be a fruitful direction for future research.[7] [8]

The remaining sections are organized as follows. Section 2 introduces China's personal income tax system, the 2011 tax reform, and the data. Section 3 applies the standard tax reform approach and the standard bunching approach to estimate the ETI. In section 4, after presenting a preliminary comparison between ETI estimates using two standard approaches in various countries, we develop a framework that compares the two approaches more directly. After documenting the differences between ETI estimates using two approaches, we explore the potential reason. Section 5 discusses the welfare implications from our ETI estimates and briefly evaluates the 2011 tax reform. Section 6 concludes.

1.2 China's personal income tax system and its 2011 reform

China's personal income tax system.

China imposes a uniform nation-level personal income tax (PIT) schedule, with no additional PIT at the provincial or local level.[9] The PIT is levied on the individual rather than on the household level and is independent of the marital status and the number of dependents. Unlike the U.S., in China, there is no program like the Earned Income Tax Credit (EITC) for low income earners and the marginal PIT rate is always non-negative. The tax schedule is not indexed for inflation, which makes the bracket cutoffs more salient over time (due to its stable nominal value) than if it is inflation indexed.[10]

[7] Note that our estimates are obtained from only one city of China and thus should be cautiously interpreted on its representativeness when compared with estimates obtained in other countries.

[8] A welfare analysis based on our ETI estimates (Online Appendix 1.6.8) implies that the deadweight loss of China's current personal wage income tax is high and thus a further MTR decrease is desirable, as it would increase tax revenue and decrease deadweight loss. An evaluation of the 2011 tax reform reveals an interesting efficiency-neutral property, despite that the main objective of the reform is undoubtedly out of a redistribution concern, as it reduces the MTRs for lower earners and increases the MTRs for higher earners.

[9] The personal income tax revenue (as well as the corporate income tax revenue) is shared between central (60%) and local (40%, in which normally 20% goes to province and 20% is retained locally) governments.

[10] In addition, there does not exist a comprehensive capital income tax in China, though many incomes that are counted as capital income in other countries are taxed under proportional tax rates (item 6, 7, 8, 9

China's PIT deals with different income items separately (similar to the Danish system, see Kleven and Schultz (2014)), unlike the US tax system which imposes a progressive rate on the comprehensive taxable personal income. All income components can be divided into three types: (1) wage/salary income, (2) self-employment income, and (3) other incomes. According to the statutory schedule, wage/salary income is subject to a multiple-tier progressive rate structure, self-employment income is subject to a different multiple-tier progressive rate structure, and other incomes are subject to a proportional rate (in general 20%). In practice, however, self-employment income in general is not taxed following such a progressive rate structure. Due to the absence of a reliable book-keeping, tax officials choose to levy a predetermined fixed amount of self-employment income tax (thus a presumptive tax) based on projected incomes for most self-employed businesses. The three types of incomes are also taxed on different time bases: wage/salary income is subject to a monthly schedule, self-employment income is subject to a yearly income schedule, and the other incomes are taxed each time the income is received. Another characteristic of China's PIT is that each income item is deducted separately instead of enjoying a deduction based on the comprehensive personal income.[11]

Since our main focus is the ETI w.r.t. the marginal tax rate and the self-employment income tax is not based on a rate structure, throughout this paper, we mainly focus on wage/salary income. Currently in China, for the majority of people, wage/salary income is their major income source. Bonuses are taxed differently from regular monthly wage by tax law, which could introduce complications both theoretically and empirically, as we discuss in detail in Online Appendix 1.6.2. However, our data show that too few incomes are taxed as bonuses to make bonuses an important concern and so we leave it out from our main analysis.[12]

in table 1.6).

[11]More details are discussed in Online Appendix 1.6.1. Table 1.6 shows details on the taxation of all 11 personal income components.

[12]Our data do not indicate which incomes are bonuses. Based on the actual tax rate and taxable income levels, we identify incomes following the "tax on bonuses" rule as bonuses. In 2013, only 0.46% (2,347 in all

Overall, China's wage/salary income tax is much simpler compared to personal income taxes in many other countries that are studied previously. Due to this, we expect to see much larger behavioral responses to tax changes in China. This is helpful for our empirical study of the behavioral responses to tax.

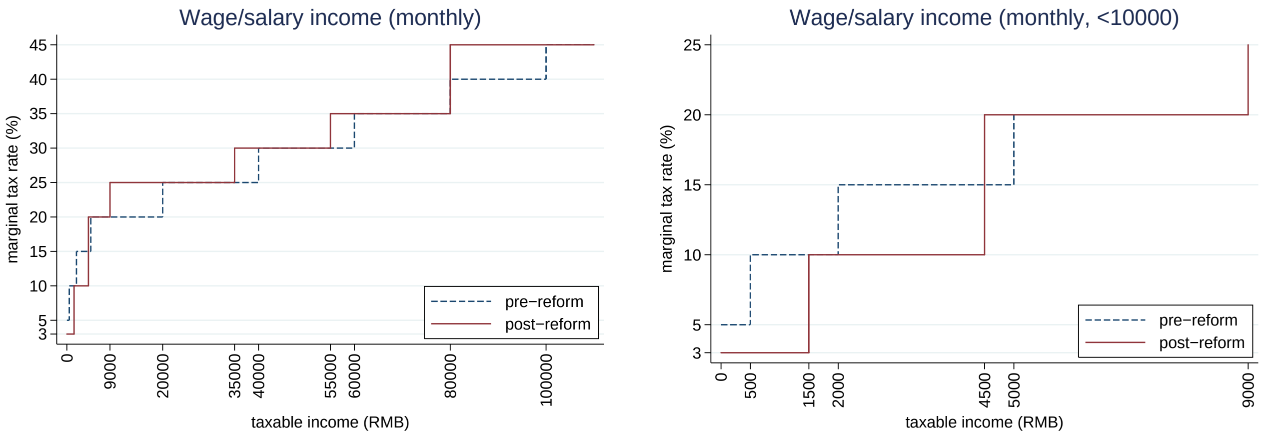
2011 PIT reform.

During our data period (June 2009-December 2013), the 2011 PIT reform is the only major change in the PIT, which changed the standard deduction and the tax rate schedule for the wage/salary income and the self-employment income. There is no major change in other relevant taxes during this period.[13] The 2011 PIT reform proposal was passed on June 30 and was put into effect on September 1, 2011. In particular, for the wage/salary income, the monthly standard deduction increased from 2,000 RMB to 3,500 RMB (exchange rate in 2011 is about \$1=6.3 RMB), the 9-tier rate became 7-tier, and bracket cutoffs also changed. Figure 1.1 shows the personal income tax schedule for wages/salaries. It is clear that the 2011 PIT reform changed the marginal tax rate for a large scope of incomes. In particular, for taxable wage/salary incomes less than 4,500 RMB, marginal tax rates decreased; for those higher than 4,500 RMB, marginal tax rates increased whenever the marginal tax rates changed. These changes created substantial variations in the marginal tax rate faced by individuals and thus provided a good chance to examine behavioral responses.[14]

505,159 individuals) of people have any bonuses in our data. But theoretically, people with annual income over 42,000 RMB should have part of their incomes issued as bonuses. In 2013, there are 192,893 individuals having annual earnings above 42,000 RMB. This fraction is very similar in other years. We are not entirely sure why there are so few people having bonuses. Perhaps many people receive bonuses in cash, as said in anecdotal evidence.

[13]Self-employed businesses do not need to pay corporate income tax (CIT), and the CIT rate is 25% for general firms and favorable rates apply for some specific firms. People need to pay social insurances (called *sijin* or *sanxianyijin* in China, including endowment insurance, medical insurance, unemployment insurance, employment injury insurance, maternity insurance, and housing fund, where maternity insurance is paid only by employers and the others are paid jointly by employees and employers). Even within a city, different firms may have different social insurance policies. There are occasional adjustments but no sharp change in social insurance policy in our sample city during our data period.

[14]For the self-employment income, the statutory tax schedule also changed (figure 1.10). But since most self-employment businesses pay a pre-determined fixed amount of income tax, it is not clear how the statutory changes in marginal tax rate map into changes in the pre-determined fixed amount income tax.



(a) (b)

Figure 1.1. Personal income tax schedule on wages/salaries
Notes: The 2011 PIT reform proposal was passed on June 30 and put into effect on September 1st, 2011.

Personal income tax administrative data.

Our personal income tax administrative data cover the whole population of a prefecture-level city in China from June 2009 to December 2013. The individual-level monthly panel dataset contains income and tax related information for all personal incomes subject to third-party reporting (mostly employer-reported). Variables include the unique individual ID, pre-tax monthly wage income, marginal tax rate, taxable income, tax liability, deductions and exempt incomes, sex, age, position, and occupation. No family-level information is available, as China's personal income tax does not depend on such information. Our sample city has a middle-sized population and a middle-high GDP level and so is not too unrepresentative of China.[15] The city has a population of 4-5 million and a 2014 GDP of 55-65 billion dollars

[15]The city is not unrepresentative also in that it does not heavily rely on certain industries as compared to the national level. The fraction of its GDP coming from the three economic sectors are 6.9% for primary sector, 52.1% for secondary sector, and 41% for tertiary sector, as compared to 10%, 43.9% and 46.1% for the national level in 2013. And the fraction of employees hired by state-owned units is 20.1%, as compared to 16.6% for overall China. These statistics are calculated from China Statistical Yearbook and the statistical yearbook of our sample city.

(using 2014 exchange rate). Disposable income per capita of this city in 2014 falls in the range of 4,000-5,000 dollars. All wages/salaries data are included while the self-employment income data are unavailable to us.[16] The number of wage earners in each month varies from around 550,000 to 700,000. Table 1.7 shows that tax revenue components from various personal incomes in our sample city are comparable to the national figures. It is clear that the wage/salary income is the major personal income and in this paper we mainly focus on it. We restrict our sample to individuals between 18 and 60 to focus on the working age people.

1.3 Two standard approaches to estimate the ETI

1.3.1 Standard tax reform approach

The traditional approach to estimate the ETI exploits the tax rate changes induced by a tax reform. China's 2011 PIT reform created exogenous changes in the marginal tax rates for people in all income levels, thus providing enough variation to apply this approach. We follow the literature and apply the following first-difference specification to estimate the ETI e:

$$log\frac{z_{i,t+k}}{z_{it}} = e \cdot log(\frac{1-\tau_{i,t+k}}{1-\tau_{it}}) + \eta \cdot log(\frac{y_{i,t+k}}{y_{it}}) + f_t(z_{it_m}) + \Omega \cdot X_{it} + \alpha_t + \xi_{it},$$

where $log\frac{z_{i,t+k}}{z_{it}}$ is the growth rate of real taxable income (nominal taxable income adjusted by CPI) for individual i from time t to time $t+k$, τ_{it} is the marginal tax rate, y_{it} is virtual income defined below, η is income elasticity, X_{it} denotes dummies for demographic characteristics (age, sex, occupation, position), α_t are month fixed-effects. $\xi_{it} = \varepsilon_{i,t+k} - \varepsilon_{it}$,

[16] Since the wage/salary income is subject to third-party reporting (the employers withhold and remit the wage/salary income tax for the employees), there should be minimal measurement error for this information. Importantly, employers report income for the employees even if their wages/salaries are below the standard deduction amount and do not need to pay any personal income tax. Self-employment income data are not reported to the department of local tax bureau that has all third-party reporting income and are thus not provided to us.

where ε_{it} is the error term of the function determining $log\, z_{it}$. We follow the common practice in literature to define taxable income z_{it} in a way that the tax base is constant throughout the period.[17] Without this adjustment, the dependent variable changes mechanically as the definition of the tax base changes; with this adjustment, what we estimate is entirely due to the MTR change. While most previous papers use yearly data, we use monthly data, since a monthly tax schedule is applied to wage/salary income in China, and we adjust the specification accordingly. Since our data only cover two years before and two years after the reform, our preferred regression uses 12-month (1-year) difference. 12 months is an appropriate choice since it is long enough to allow wage adjustment and not too long given our data covering period.[18]

Since tax rates are a function of taxable income, the log change in the net-of-tax rate is clearly endogenous. To address this problem, $log(\frac{1-\tau_{i,t+k}}{1-\tau_{it}})$ is instrumented using $log(1-\tau_{i,t+k}(\widetilde{z_{it}})) - log(1-\tau_{it}(\widetilde{z_{it}}))$. This instrument computes the predicted net-of-tax rate change at a taxable income level $\widetilde{z_{it}}$. The idea of such an instrument strategy is to just use exogenous changes in tax laws to provide identification. The traditional practice in literature (e.g. Gruber and Saez (2002), Kopczuk (2005)) is to use $\widetilde{z_{it}} = z_{it}$. However, as widely recognized, z_{it} is likely to be correlated with ξ_{it} because the mean reversion of income creates a negative correlation between ε_{it} and $\xi_{it} = \varepsilon_{i,t+k} - \varepsilon_{it}$. Some strategies are thus proposed to address this problem.

Our preferred instrument strategy follows that used in Ito (2014) and Blomquist and Selin (2010) to use the taxable income in the middle time between t and $t+k$ to generate the simulated MTR change. In our case, $k = 12$ and so the middle time is $t_m = t+6$ and the

[17] Taxable income is defined as raw income - standard deduction - other deductions - tax-exempt incomes. We apply the post-reform tax base by assuming the pre-reform observations are subject to post-reform standard deduction, as the only change in the tax base during the 2011 tax reform is change of the standard deduction.

[18] Our data have similar structure to Ito (2014), who uses monthly electricity consumption data and estimates the effects of marginal price and average price on the monthly electricity consumption. So we follow his specification in many aspects, i.e. using 12 month first-difference specification, using middle-time taxable income to construct instruments, using decile-by-month fixed effects to control for heterogeneous underlying changes in taxable income growth for different income levels.

instrument is based on $z_{i,t+6}$. As shown in Ito (2014) and Blomquist and Selin (2010), this instrument is not systematically affected by the mean reversion problem because $\varepsilon_{i,t+12}$ and ε_{it} do not directly affect $z_{i,t+6}$. If there is no serial correlation, $\varepsilon_{i,t+6}$ and $\xi_{it} = \varepsilon_{i,t+12} - \varepsilon_{it}$ are clearly uncorrelated. When there is serial correlation, Blomquist and Selin (2010) show that $cov(\varepsilon_{i,t+12} - \varepsilon_{it}, \varepsilon_{i,t+6}) = 0$ as long as the serial correlation depends only on the time difference between the error terms. The intuition is that since $\varepsilon_{i,t+6}$ is equally spaced from $\varepsilon_{i,t+12}$ and ε_{it}, it would be correlated with them in the same manner. Alternatively, Weber (2014) proposes an instrument approach to mitigate the mean reversion problem. She argues that using lagged terms of z_{it} instead of z_{it} itself to construct the predicted MTR would render instruments that are strictly more orthogonal to the error term than traditional instrument. We apply this approach to use one-year lagged z_{it} to construct the alternative instrument.[19] [20]

Most ETI literature simply ignores the income effect since previous literature generally finds it small (e.g. Gruber and Saez (2002) for the US and Kleven and Schultz (2014) for Denmark). But different economies could have different sizes of income effects. And as noted by Gruber and Saez (2002), it is theoretically unclear what sign to expect for the income effect estimates for constructs such as broad or taxable income. So we explicitly examine the size of income effects in China. Our empirical estimates start with a specification without income effects, and then control for the difference of the log of virtual income, where virtual

[19] We use the middle time taxable income based instrument rather than the Weber-type instrument as our preferred instrument strategy for three reasons. First, the Weber-style instrument does not guarantee a strictly exogenous instrument while the middle time taxable income based instrument used in Ito (2014) and Blomquist and Selin (2010) does under reasonable assumptions. Second, the Weber-style instrument strategy greatly shrinks our sample period that can be used for regression, while the middle time taxable income based instrument does not. Third, the Weber-style instrument faces a trade-off between two requirements that make an instrument valid. That is, a longer lag of taxable income based instrument will make the exclusion restriction more reliable (since serial correlation of error terms will be weaker) and the weak IV problem more acute (since the first stage result will be weaker). But no criterion is proposed on how to decide between them. Weber simply assumes a longer lag to be more orthogonal, given its first-stage result is not weak. By contrast, the middle time taxable income based instrument could satisfy the exclusion restriction assumption under reasonable assumption, without sacrificing the first-stage. In subsequent regressions, we only focus on middle time taxable income based instrument.

[20] We are unable to use the two-year lag of z_{it} to construct the instrument in our data since this would leave too few pre-reform months (only three).

income $y_{it} \equiv \tau_{it} \cdot z_{it} - T_t(z_{it})$, with $T_t(.)$ denoting tax liability, following Kleven and Schultz (2014), Blomquist and Selin (2010), Bastani and Selin (2014), and Jantti et al. (2015).[21] The instrument for virtual income is constructed in a similar spirit to that used for $log(\frac{1-\tau_{i,t+k}}{1-\tau_{it}})$. When income effect is important, the estimate e is an uncompensated elasticity due to budget set linearization implied by the virtual income formulation.[22] If income effect is small and unimportant, we can use the specification with no income effects and interpret e as the compensated elasticity.

There are many ways to specify $f_t(z_{it_m})$. For example, we can include flexible polynomial functions of z_{it_m}. But to avoid imposing a functional form assumption, we take a non-parametric approach. In particular, we include a set of decile dummies of taxable income for each t_m. By doing so, we have a set of decile-by-month fixed effects.[23] Such flexible controls of z_{it_m} account for heterogeneous income growth rates of different income levels. When we use the Weber-type instrument, we accordingly include a set of decile-by-month fixed effects based on $z_{i,t-12}$.

Regressions are weighted by middle-time taxable income ($z_{i,t+6}$) or lagged taxable income ($z_{i,t-12}$) depending on the instrument used.

Table 1.1 shows the regression results. Columns 1 and 2 are our preferred results, using middle-time taxable income based instruments. Column 1 shows the estimate without income effects.[24] The point estimate of ETI is 2.423 and is statistically significant at 1% level. Column 2 includes the income effect and shows that it is small and statistically insignificant

[21] As noted by Kleven and Schultz (2014), modeling the income effect in terms of virtual income deviates from some previous taxable income studies (e.g. Gruber and Saez (2002)), where the income effect is specified simply in terms of after-tax income $z_{it} - T_t(z_{it})$. But as noted by Blomquist and Selin (2010), Bastani and Selin (2014), Jantti et al. (2015), the virtual income specification more closely follows the labor literature of specifying income effects and therefore is widely adopted in these recent taxable income studies.

[22] The compensated elasticity is then $\zeta^c = e - \eta \frac{(1-\tau)z}{y}$, where y is virtual income and η is elasticity w.r.t. virtual income (Blomquist and Selin (2010)). In Gruber and Saez (2002), they use after-tax income as a proxy of virtual income, i.e. $y = (1-\tau)z$, and they have $\zeta^c = e - \eta$.

[23] Using percentile-by-month fixed effects renders very similar results.

[24] First-stage results are strong in all columns.

Table 1.1. Estimates of ETI for wage/salary income using tax reform approach

Dep. Var.: Δlog(taxable income)	[1]	[2]	[3]	[4]
Δln(1-τ)	2.423***	2.534***	2.739***	4.023**
	(0.188)	(0.645)	(0.500)	(1.646)
Δln(virtual income)		0.117		1.705
		(0.712)		(2.178)
Instrument based on	taxable income in t+6 (preferred specification)		taxable income in t-12	
Observations	1,210,376	1,210,376	572,509	572,509
First-stage F-stat for Δln(1-τ)	3647	1808	995	497.9
First-stage F-stat for Δln(virtual income)		161.8		43.44

Notes: The table shows elasticity estimates based on 2SLS regressions, where standard errors (shown in parentheses) are clustered by individual. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. All specifications include dummies of age, gender, occupation, position, middle-month taxable income (or one year lag taxable income) decile-by-month fixed effects, and base-month fixed effects. Regressions are weighted by middle-month taxable income (or one year lag taxable income).

in our case, consistent with most previous literature. Columns 3 and 4 show results using Weber-type instruments. Column 3 shows a compensated ETI estimate similar as our preferred specification. Column 4 shows a very large yet statistically insignificant income effect. Therefore, we will treat 2.423 as our compensated ETI estimate using the standard tax reform approach and ignore income effects throughout our paper. These estimates are in line with the ETI estimates obtained in our revised tax reform approach below.

1.3.2 Standard bunching approach

Due to the increasing availability of administrative tax returns data, there has been a surge of research using the bunching approach to estimate compensated elasticities. Notably, Saez (2010) and Chetty et al. (2011) use bunching at kinks and Kleven and Waseem (2013) use bunching at notches to uncover compensated elasticities and the underlying structural elasticities. In this section, we use the standard bunching approach developed by Chetty et al. (2011) using pooled cross-sectional data to estimate the ETI e without considering optimization frictions.

Conceptual framework and empirical strategy

The standard bunching approach to estimate the elasticity of taxable income can be briefly described as follows.[25] Consider individuals with preferences defined on after-tax income and before-tax income. The utility function is $u(z-T(z),\frac{z}{n})$, where z is earnings, $T(z)$ is tax over earnings, and n denotes ability. Suppose the initial marginal tax rate is τ_1 and an increase in the marginal tax rate starting at taxable income K is introduced, bringing marginal tax rate to $\tau_2=\tau_1+\triangle\tau$ for taxable income above K. Under this two-tier tax schedule, all individuals originally choosing K or less are not affected. The individual whose indifference curve was tangent to the original budget line at $K+\triangle z$ now has indifference curve tangent to the upper part of the two-tier budget line at K. This individual is called the marginal buncher because all the individuals initially locating between K and $K+\triangle z$ now would choose K. All the individuals initially choosing $(K, K+\triangle z)$ are called bunchers. Some individuals that originally chose more than $K+\triangle z$ may now choose taxable income between K and $K+\triangle z$.[26] Thus, in theory a convex kink at K would generate excess bunching at K. Assume e, the elasticity of taxable income with respect to net-of-tax rate, is constant for individuals around the kink K, then by definition we have

$$e=\frac{\triangle z/K}{\triangle\tau/(1-\tau_1)},$$

where only $\triangle z$ needs to be identified to estimate e. Denote the excess bunching amount by B, we have

$$B=\int_K^{K+\triangle z}h_0(z)dz=h_0(\bar{z})\triangle z\simeq h_0(K)\triangle z,$$

[25] The standard bunching approach tends to ignore the income effect and we follow this tradition in this paper. First, since we find small income effect in our case as in most previous studies, this seems reasonable. Second, Bastani and Selin (2014) use numerical simulation to show that, even when the kink is very large (and the income effect is thus plausibly large), income effects are unlikely to bias the ETI estimates from the standard bunching approach.

[26] See Kleven (2016) or Saez (2010) for a graphical illustration.

where $h_0(z)$ is the density function of taxable income when there is a constant marginal tax rate τ_1 throughout the distribution. The second equality is due to the mean value theorem for integrals, and $\bar{z} \in [K, K+\triangle z]$. When $\triangle z$ is small, $h_0(\bar{z})$ is approximated using $h_0(K)$. In theory, $h_0(K)$ is the density function at point K, while empirically we estimate the density on bins with width W. So we modify the above relation as

$$B \simeq \frac{h_0^W(K) \triangle z}{W},$$

where $h_0^W(K)$ is the density associated with bins of width W. Plugging it back to the definition of elasticity, we have

$$e \simeq \frac{B/h_0^W(K)}{\frac{K}{W} \cdot \frac{\triangle \tau}{1-\tau_1}}.$$

Then it suffices to estimate $b \equiv \frac{B}{h_0^W(K)}$, the fraction of excess bunchers normalized by the counterfactual density.

To estimate b, we apply the standard approach used in Chetty et al. (2011). Observations around kinks are first grouped into bins with width W. Denoting by c_j the number of observations and z_j the taxable income relative to kink K in bin j, we fit a flexible polynomial of order q to the bin counts in the empirical distribution[27], omitting the excluded region (z_L, z_U),[28] by estimating regression:

$$c_j = \sum_{i=0}^{q} \beta_i^0 \cdot (z_j)^i + \sum_{i \in (z_L, z_U)} \gamma_i^0 \cdot \mathbf{1}[z_j = i] + \varepsilon_j,$$

where γ_i^0 is a bin fixed effect for each bin in the excluded region. The initial estimate of the counterfactual distribution is the predicted values from the above regression by setting

[27] In practice, we take the seventh-degree polynomial, following Chetty et al. (2011).

[28] The excluded region is the region around the kink where excess bunching happens. In the case of kinks, the excluded region is typically determined visually, while in the case of notches, there is additional moment to help determine the bounds of the excluded region. See Kleven (2016) for a comprehensive review.

all the dummies in the excluded region to zero: $\hat{c_j}^0 = \sum_{i=0}^{q} \widehat{\beta_i^0} \cdot (z_j)^i$. The initial estimate of excess bunching, defined as the difference between the observed and counterfactual counts within the excluded region, is $\widehat{B}^0 = \sum_{j \in (z_L, z_U)} (c_j - \hat{c_j}^0)$. $\widehat{B}^0$ might overestimate $\widehat{B}$ because it does not account for the fact that the additional individuals at the kink come from points to the right of the kink. Hence the estimated counterfactual is likely to be based on an underestimate of individuals that would have been observed without the kink. Following Chetty et al. (2011), we address this concern by shifting the counterfactual distribution to the right of the kink upward until it satisfies the constraint that the number of observations in the counterfactual distribution is equal to the number of observations in the observed distribution. In particular, the final estimate of the counterfactual distribution is the predicted values $\hat{c_j} = \sum_{i=0}^{q} \widehat{\beta_i} \cdot (z_j)^i$ from the following regression:

$$c_j \cdot \left(1 + \mathbf{1}[j \geq z_U] \cdot \frac{\widehat{B}^0}{\sum_{j=z_U}^{\infty} c_j}\right) = \sum_{i=0}^{q} \beta_i \cdot (z_j)^i + \sum_{i \in (z_L, z_U)} \gamma_i \cdot \mathbf{1}[z_j = i] + \varepsilon_j. \tag{1.1}$$

Then we obtain $\widehat{B} = \sum_{j \in (z_L, z_U)} (c_j - \hat{c_j})$. The empirical estimate of b is given by

$$\hat{b} = \frac{\widehat{B}}{(\sum_{j \in (z_L, z_U)} \hat{c_j})/N}, \tag{1.2}$$

where N is the number of bins in the excluded region. Following Chetty et al. (2011), the standard error for $\hat{b}$ is bootstrapped. We randomly draw from the estimated vector of errors $\widehat{\varepsilon_j}$ in (1.1) with replacement and generate a new set of counts and apply the above technique to calculate a new set of estimates $\hat{b}^k$s. Define the standard error of $\hat{b}$ as the standard deviation of the distribution of $\hat{b}^k$s. Finally, e can be obtained as $\hat{e} \simeq \frac{\hat{b}}{\frac{K}{W} \cdot \frac{\triangle\tau}{1-\tau_1}}$, with standard error computed using the delta method $std(\hat{e}) \simeq \frac{std(\hat{b})}{|\frac{K}{W} \cdot \frac{\triangle\tau}{1-\tau_1}|}$.

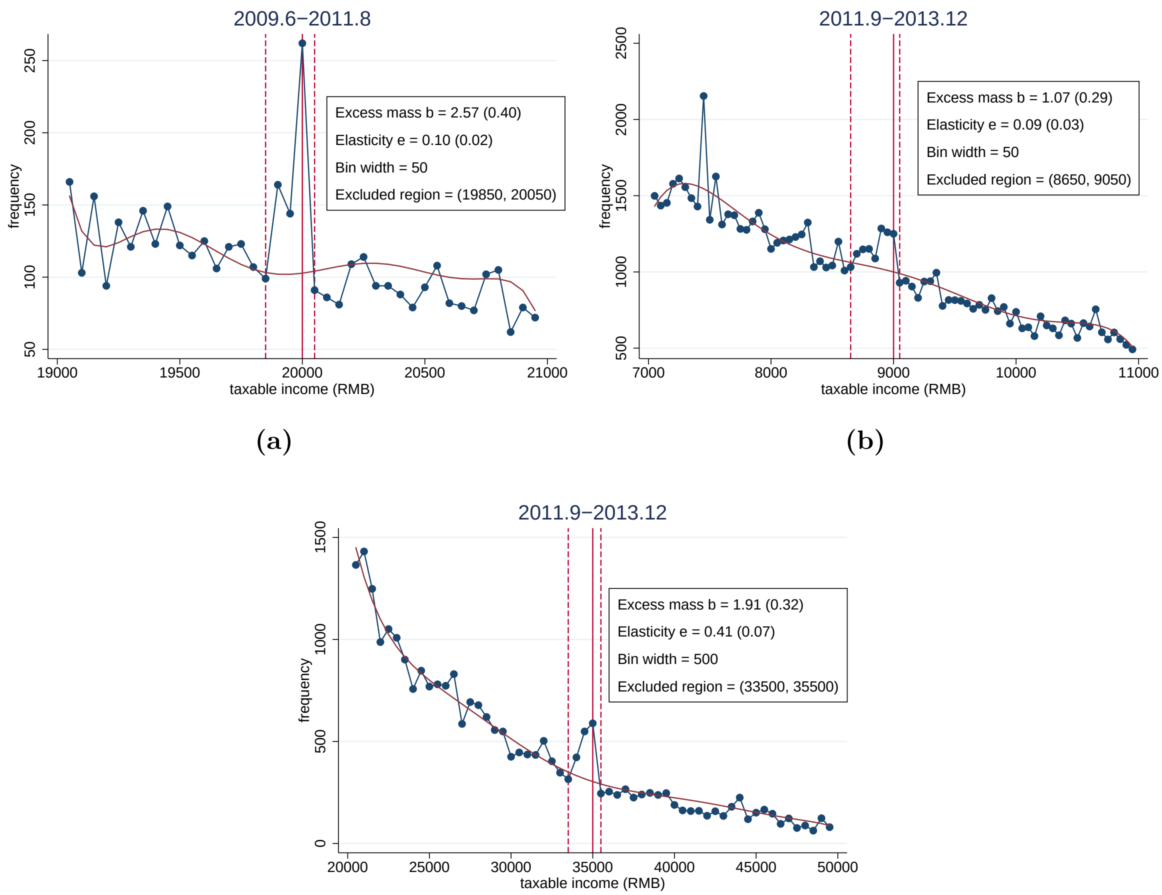
Standard bunching estimates

Unlike previous bunching papers that use a full sample, our main analysis applies the bunching method to a decimal sample. That is, we drop observations with taxable income exactly at a round number (e.g. 2, 5, 8) and keeping only those with decimal values (e.g. 2.12, 5.01, 8,58). This restriction is made in order to address the irregular bunching at non-kink places observed in our data. Previous literature accounts for regular bunching patterns at non-kink numbers by adding indicators of different "rounder" numbers.[29] Yet this approach cannot address the irregular bunching patterns in our case. Restricting sample to decimal TI values well addresses this issue and reveals reliable bunching patterns at kinks and exclude any bunching at non-kink places. In Online Appendix 1.6.3, we show the bunching patterns using full sample and decimal sample and discuss the sample restriction in detail.[30]

Given the large sample size of our dataset, restricting to a decimal sample would still render precisely estimated ETI for each kink. Figure 1.13 shows clear bunching at pre-reform kink 20,000 RMB before the tax reform and at post-reform kinks at 9,000 RMB and 35,000 RMB after the reform. Like Kleven and Waseem (2013), we find no evidence of bunching at bottom kinks, suggesting a zero ETI there, and observations are too few to generate precise bunching estimates at top kinks. Thus, we focus on the middle-high TI kinks to apply the standard bunching approach.

[29] Ignoring this rounder-number bunching behavior could have the standard bunching approach overestimate the ETI. Kleven and Waseem (2013) under the notch setting propose a way to address the rounder-number bunching problem by including an indicator for rounder numbers (i.e. multiples of 5K, 10K, 25K, and 50K) when estimating the counterfactual density function. Devereux et al. (2014) follow such approach to estimate the ETI of corporate income tax in the UK and Best and Kleven (2016) adopt a similar approach to deal with the rounder-number bunching for house prices. Some bunching analyses simply ignore the rounder-number bunching problem, probably because in their specific cases the rounder-number bunching problem is not salient (e.g. Chetty et al. (2011) and Saez (2010)).

[30] Admittedly, there may be concern that the decimal sample could underestimate the ETI since it might exclude taxable incomes adjusted to exactly at the kink more than those with other integer values. We address this concern by showing that using full sample would generate similar dynamic pattern of bunching estimates in Online Appendix 1.6.6.



(a) (b) (c)

Figure 1.2. Standard bunching estimates of ETI
Notes: The solid smooth curve depicts the estimated counterfactual distribution omitting the obervations in the excluded region, as specified by the area between the dashed lines.

Estimates.

Figure 1.2 shows the estimates of the excess mass and the elasticity of taxable wage income. The solid dotted line depicts the observed distribution and the solid smooth line shows the estimated counterfactual distribution omitting the observations in the excluded region. For kinks at 20,000 RMB and 9,000 RMB, width of bins is 50 RMB, while for kink at 35,000 RMB, where observations are much scarce, width of bins is 500 RMB. The observed elasticity for pre-reform kink 20,000 RMB is 0.10. The observed elasticities for post-reform kinks 9,000 RMB and 35,000 RMB are 0.09 and 0.41, respectively. The elasticities are all statistically significant at 1% level.[31] As a placebo test, in Online Appendix 1.6.4, we show that there was no bunching at all before a new kink was imposed, and that bunching disappeared within a short time after an old kink was abolished.

Who are the bunchers?

In this section, we provide the first formal test of the key assumption of the bunching approach, and then examine personal characteristics (i.e. sex, age, occupation, position) of bunchers versus non-bunchers.

Testing the key assumption of bunching approach.

The key assumption of the bunching approach is that the excess bunchers mainly come from those that could have earned slightly more than the income associated with the kink (assuming an increasing MTR at the kink). This assumption determines whether the bunching approach measures the income adjustment behavior as it claims but has never been formally tested. An alternative possibility is that a non-negligible portion of the excess bunchers around the kink point are those that could have earned less if the kink does not exist. It is possible that the introduction of the kink works as a salient reference value for people to adjust their earnings. Although this alternative hypothesis does not seem to be so likely as the null hypothesis, it is an empirical question to examine whether it is true. The idea to examine this key assumption is: if excess bunchers come from those that could have slightly higher income than the kink points, as the bunching theory predicts, then we should see the bunchers at kinks have a lower income growth rate than the nearby non-bunchers. Otherwise, we would see bunchers at kinks have a same or a higher income growth rate than nearby non-bunchers.

In accordance with the bunching estimates above, we focus on pre-reform kinks at 20,000 RMB taxable income and post-reform kinks at 9,000 RMB and 35,000 RMB. Figure

[31] Like all previous bunching papers, we find choosing different bin widths generates only slightly different estimates. To save space, we do not report these results.

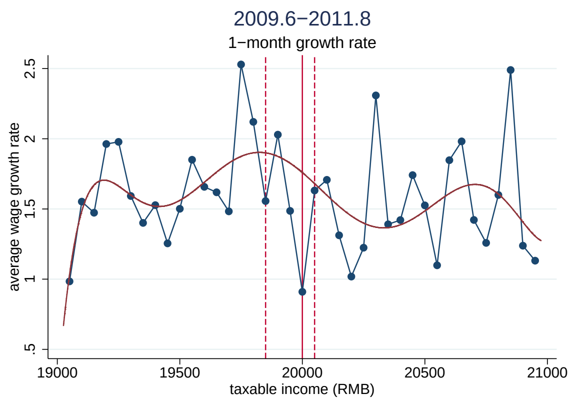
1.3 shows clear evidence that wage growth rates in the bunching region associated with post-reform kinks at 9,000 RMB and 35,000 RMB are lower than those in neighboring non-bunching area, though the evidence is less clear for the pre-reform 20,000 RMB kink possibly due to less observations. The solid line indicates the kink point and dashed lines embrace the excluded region, where we expect to see a lower income growth rate if the assumption of the bunching approach is correct. Note that since we have excluded round number taxable income observations and only use decimal taxable income observations, the lower wage growth rates in the excluded region are not due to income inertia for those locating at natural focal points. Using the following specification, we statistically test whether wages falling in the excluded region grow significantly lower than wages nearby:

$$Wage\,growth\,rate_{it} = \sum_{j=0}^{q} \beta_j \cdot (z_{it})^j + \gamma \cdot \mathbf{1}_{\boldsymbol{z_{it} \in (z_L, z_U)}} + \varepsilon_{it},$$

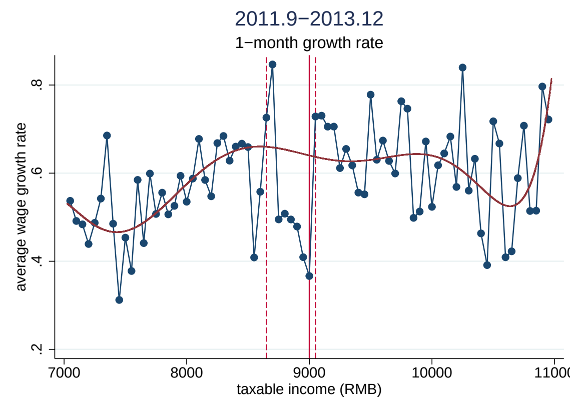
where z_{it} is the taxable income of individual i at month t relative to kink K, $Wage\,growth\,rate_{it}$ is the wage growth rate (from last month to current month) for individual i from month $t-1$ to month t, $\mathbf{1}_{\boldsymbol{z_{it} \in (z_L, z_U)}}$ indicates whether the taxable income z_{it} falls in the excluded region (z_L, z_U). We still take the seventh-degree polynomial, i.e. $q = 7$. The solid smooth lines in figure 1.3 depict the estimated counterfactual distribution omitting observations in the excluded region. Table 1.2 shows the test results, where only estimates of γ are reported. For each kink point, the estimate of γ is negative. The estimates are statistically significant at 1% level for post-reform kinks. This evidence thus supports the key assumption of the bunching approach.

Are bunchers different from nearby non-bunchers?

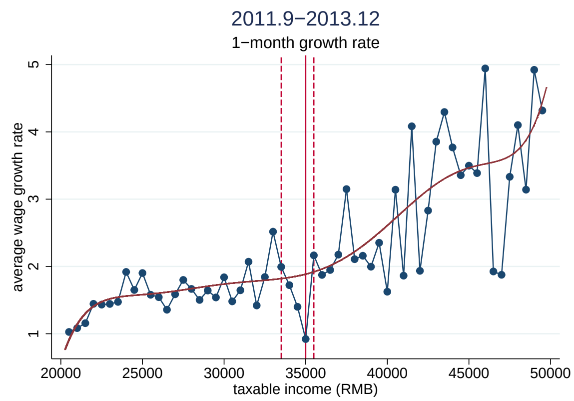
Table 1.10 compares personal characteristics (i.e. sex, age, occupation, position) for bunchers and nearby non-bunchers for each taxable income kink. Bunchers are defined as the



(a) Pre-reform TI kink 20000 RMB



(b) Post-reform TI kink 9000 RMB



(c) Post-reform TI kink 35000 RMB

Figure 1.3. Wage growth rates around taxable income kinks
Notes: The solid smooth curve depicts the estimated counterfactual distribution omitting the obervations in the excluded region, as specified by the area between the dashed lines.

individuals within the excluded region in the above figures, while the nearby non-bunchers are those outside the excluded region but also in the above figures. Since we pool monthly observations, it is possible that a person in some months falls into the bunching region while in other months falls into the non-bunching region. To address this problem, we define a person as a buncher if he/she ever falls into the excluded region and define a person as non-buncher if he/she never falls into the excluded region.

The main finding is that the bunchers at middle-high taxable income kinks tend to have a higher position (i.e. relative ranking in a workplace) than the non-bunchers, i.e.

Table 1.2. Do wages in excluded region grow slower than wages nearby?

Dep. Var.: wage growth rate	[1]	[2]	[3]
Kink point:	pre-reform taxable income 20000 RMB	post-reform taxable income 9000 RMB	post-reform taxable income 35000 RMB
excluded region	-0.462**	-0.141***	-0.574***
	(0.195)	(0.036)	(0.125)
Observations	3,747	57,696	20,983
R-squared	0.004	0.001	0.024

Notes: Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

bunchers are more likely to own some managing power in the workplace (with position of middle deputy and above). This makes sense as those with managing power should have more flexibility to manipulate wage income than the general staff and other people. The occupational comparison does not render sharp contrast and we do not draw clear conclusion from it. Indeed, within each occupation, the heterogeneity in position may be more important in deciding who bunches. In other personal characteristics, bunchers are slightly more likely to be male than female, and there is no systematic difference in age between bunchers and non-bunchers.

1.4 Tax reform ETI versus bunching ETI

How do the ETI estimates obtained from two standard approaches compare to each other? A preliminary comparison suggests that the tax reform ETI appears to be systematically larger than the bunching ETI, though we show that the two standard approaches do not generate directly comparable estimates. We then develop an approach to allow the two approaches to render more directly comparable ETI estimates. After that, we explore how the ETI estimates differ between two approaches and why.

1.4.1 A preliminary comparison of the ETI estimates obtained from two standard approaches

To start with, we summarize the ETI estimates using two standard approaches in the same country from existing literature. Table 1.3 shows such a comparison for four countries, China, Denmark, Sweden, and the U.S. For China, the tax reform ETI is 2.423, much larger than the bunching ETI estimates (0.09-0.41) at the kinks with clear bunching. Using the tax reform approach, Kleven and Schultz (2014) find the ETI estimates range from 0.04 to 0.06 for wage earners (and 0.10 for self-employed individuals) in Denmark; Blomquist and Selin (2010) find that the ETI among Swedish wage earners is 0.19 for male and 1.39 for female; Saez et al. (2012) find that for the U.S, "while there are no truly convincing estimates of the long-run elasticity, the best available estimates range from 0.12 to 0.40". Using the bunching approach, Chetty et al. (2011) reveal an observed elasticity below 0.02 for wage earnings in Denmark; Bastani and Selin (2014) obtain an elasticity of 0.001 for all Swedish wage earners at the first central government kink during 1999-2005; Saez (2010) find that for the U.S., the elasticity is around 0 for wage earners.[32] These empirical findings seem to suggest that for the same country, the tax reform approach generally renders a higher ETI than the bunching ETI does.[33] [34]

But are the estimates from two standard approaches directly comparable? No. The standard tax reform approach differs from the standard bunching approach in two major aspects: time and scope.

[32]The elasticity is around 1 for the self-employed at the first EITC kink in the U.S., and is around 0.2 for all individuals at the first federal income tax kink (i.e. taxable income $0).

[33]For countries that do not have ETI estimates from both approaches, the bunching ETI estimates generally seem quite small. For example in Pakistan (during 2006-2009), Kleven and Waseem (2013) use the bunching at notch approach and reveal structural elasticity around 0.03 for wage earners in middle income tax brackets (ranged from 0.03 to 0.28 for self-employed individuals).

[34]Relatedly, Fack and Landais (2016) find that bunching estimates of elasticities of reported charitable contributions with respect to the price of contributions (i.e. one minus the marginal subsidy rate) in France are much smaller than those obtained using the tax reform approach.

Table 1.3. ETI estimates using two standard approaches

Tax reform approach:	
China (this paper):	wage income: 2.423
Denmark (Kleven and Schultz, 2014):	wage earners: 0.04-0.06
Sweden (Blomquist and Selin, 2010)	wage earners: 0.19 male, 1.39 female
U.S.(Saez et al., 2012):	best available estimates 0.12-0.40
Bunching approach:	
China (this paper):	wage income at middle-high brackets: 0.09-0.41
Denmark (Chetty et al., 2011):	wage earners at top bracket: 0.02
Sweden (Bastani and Selin, 2014)	wage earners: around 0
U.S.(Saez, 2010):	wage earners: around 0

Time.

The first-differencing specification of the tax reform approach determines the time property of its ETI estimates. For a 1-year differencing specification, the ETI reflects a 1-year response; for a 3-year differencing, the ETI reflects a 3-year response. By contrast, the bunching approach only requires cross-sectional data and it is generally unclear what time property the bunching ETI is capturing. If the bunching ETI is obtained using observations at some arbitrary time point, the bunching ETI only reflects the cross-sectional distribution of taxable income at that time point, thus embedding no clear time property if no other information is known. By pooling observations in multiple time points together, we obtain an ETI with an even unclearer time property. Thus, the standard tax reform ETI and the bunching ETI normally capture behavioral responses at different time lengths.

Scope.

The tax reform approach uses the overall sample and usually obtains an average ETI for all people. In this sense, the tax reform ETI has a global property. Note that in some specific case, the tax reform approach could also obtain the ETI of some specific group. For example, if a tax reform only reduces the MTR for the top earners while keeping MTRs for other people unchanged, then the tax reform approach would obtain an ETI largely for the top earners. But in general, a tax reform would change the MTR in a broad range of income levels. This is the case for Chinese personal income tax, and in this general case the tax reform ETI has a global property. By contrast, the bunching ETI is estimated based on a close neighborhood of a kink. In this sense, bunching ETI has a local property. A local ETI is hardly comparable to a global ETI.

1.4.2 Towards a more direct comparison of two approaches

Given the above important differences, the ETI estimates obtained from two standard approaches are not necessarily directly comparable. To make the tax reform approach and the bunching approach render more directly comparable estimates, we need to ensure that they reflect the same time property and measure the ETI at the same scope. A two-step framework is devised to do this. First, we develop a revised version for each approach to ensure the ETI estimates from two approaches reflect income responses at the same time length. Second, we ensure the ETI estimates obtained from two approaches reflect the same scope. Since the tax reform ETI has a global property, we try to reveal a global ETI from the bunching ETI estimates in various kinks.[35] We follow the idea in literature (e.g. Chetty (2012), Gelber et al. (2015)) to assume a common underlying ETI for all people, which, with the existence of a fixed adjustment cost, would be consistent with the different observed ETI estimates at different kinks. Using the observed ETI estimates at various kinks, we can reveal the global bunching ETI underlying the observed bunching ETI estimates. This is then comparable to the global ETI obtained from the tax reform approach.

[35] An alternative way to make this comparison is to use the tax reform approach to estimate the ETI around each kink and compare it with the bunching ETI at that kink. However, using the tax reform approach to estimate ETI for a subsample of individuals whose predicted income is around a kink is dubious in methodology, because for a narrow income interval around a kink, most people would face the same MTR change, which makes identification difficult, if not impossible.

1.4.3 Revised tax reform approach

The idea of the revised tax reform approach is to estimate an ETI for each period after the tax reform. In particular, we exploit the advantage of the individual monthly panel data to estimate the ETI according to the deviation of the post-reform income growth trend from the pre-reform trend. Instead of using a first-difference specification, the regression specification is like an event-study form:[36] [37]

$$log(z_{it}) = \alpha + \sum_{j=1}^{\infty} \beta_j \cdot Post_j \cdot ln(1-\tau_{it}) + \lambda_t + v_i + base\,income\,decile_i \cdot t + \varepsilon_{it}. \tag{1.3}$$

z_{it} is the taxable income (both pre-reform and post-reform samples are adjusted by the post-reform tax schedule so as to face the same tax base), $Post_j$ is the jth period after the reform, $Post_j \cdot ln(1-\tau_{it})$ is instrumented using $Post_j \cdot ln(1-\tau_i^p)$, τ_i^p is the predicted MTR of individual i with base taxable income (3-month average taxable income prior to the announcement of the tax reform) under the post-reform tax schedule.[38] Monthly fixed effects λ_t control for time-specific shocks to z_{it} that are common across individuals. Individual fixed-effects v_i control for variation in outcomes across individuals that are constant over time. To account for the heterogeneous income growth trends for different income levels, we include a set of flexible controls $base\,income\,decile_i \cdot t$, i.e. the interaction between base

[36] In our specification, the change in the post-reform ln(1-MTR) to the pre-reform ln(1-MTR) is implicitly defined, noting that the individual specific pre-reform ln(1-MTR), e.g. the average ln(1-MTR) of an individual prior to the reform, has been absorbed by the individual fixed-effects.

[37] Admittedly, the average MTR increases over time, even before the tax reform, due to natural income growth and the progressive rate structure. But this tax rate change is much smaller than that induced by the tax reform. Applying the standard tax reform approach (1-year difference specification) using the pre-reform data, with the change in ln(1-MTR) instrumented by the tax rate change driven only by the natural income growth, an approach similar to the "bracket creep" approach by Saez (2003), we find small and insignificant ETI, possibly due to small variation in the MTR caused by the natural income growth and the progressive rate structure. Therefore, we ignore the MTR changes caused by the natural income growth and focus only on the MTR changes caused by the tax reform.

[38] We have tried to produce an estimate with just $Post \cdot ln(1-\tau_{it})$ and obtain an ETI estimate of 1.8. But this estimate has no clear time property, as it is an average ETI of all post periods. Thus, it is not directly comparable to our standard tax reform ETI estimate, which can be interpreted as a one-year ETI due to its first-difference specification using one-year lags.

income decile dummies and a linear time trend.[39] β_j is the ETI in the jth period after the reform. We use a 3-month period to mitigate monthly fluctuations and also for a direct comparison to the 3-month dynamic bunching ETI estimates below. Since the tax reform proposal was passed on June 30 and was put into effect on September 1st, 2011, people might respond as early as two months prior to the implementation of the reform. Thus, we exclude July and August of 2011 from the regression sample.

Our specification assumes that, prior to the tax reform, the average $log(z_{it})$ increases linearly over time, as is suggested by figure 1.4. The tax reform changed the MTR for some people permanently, which drives the dynamic adjustments of their taxable income. Using the pre-reform trend as the benchmark, we can estimate the ETI for each post-reform period by tracking the deviation of $log(z_{it})$ from its pre-reform trend. Our specification also largely avoids the mean reversion issue, which could be acute when using the first-difference specification, since we use a long pre-reform trend of income rather than income at a time point as the benchmark.

Before showing the ETI estimates, we provide graphic evidence on the evolution of the MTR and the log of taxable income around the tax reform. The graphical evidence is very helpful to judge whether the dynamic ETI estimates obtained below are reasonable. Figure 1.4 shows clearly that people facing an MTR increase experienced lower income growth after the reform, while those facing an MTR decrease had much faster income growth after the reform.[40] The figure implies that for the MTR increase group, the dynamic ETI increases over time, while for the MTR decrease group, the ETI experiences dramatic increase in initial periods after the reform, and stay largely constant in the end. This reflect asymmetric income responses to MTR increases versus decreases. Under our setting, MTR increases happen

[39] Using the interaction between base income percentile dummies and a linear time trend renders very similar results.

[40] We judge whether an individual faces an MTR increase or decrease based on the baseline taxable income, which is the average taxable income throughout April-June, 2011. From figure 1.11, it is clear which people face an MTR increase, which face an MTR decrease. For people who do not face an immediate MTR change at the reform month, most of them would face an MTR increase later due to the natural wage growth and the progressive tax rate schedule.

at high income levels while MTR decreases happen at low income levels. Therefore, these asymmetric income responses may be due to different capabilities to adjust income for high earners versus earners.[41]

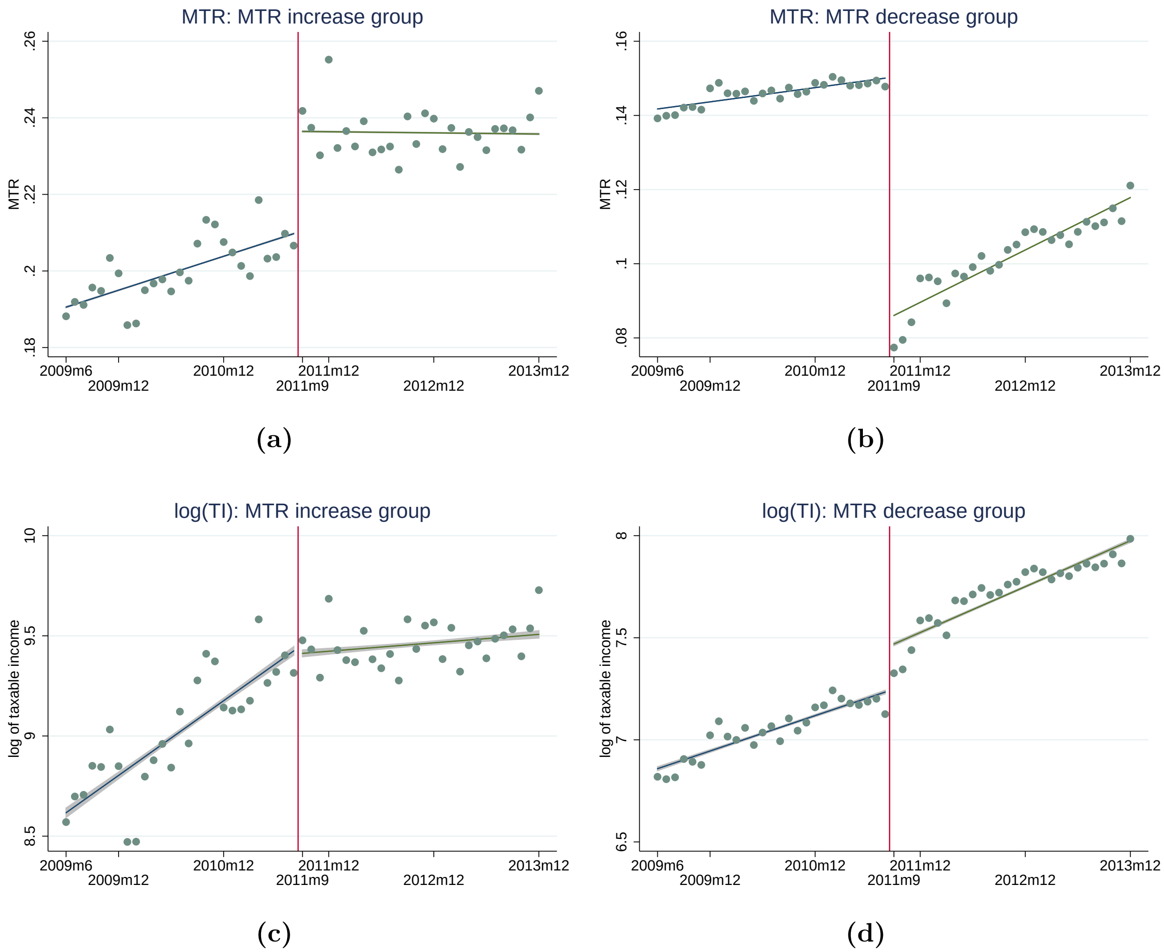


Figure 1.4. Evolution of the marginal tax rate and the taxable income
Notes: A linear polynomial is fitted with 95% CI shown in the figure. Each dot represents the average value in a given month.

With an anticipation of a future MTR change, besides the labor supply adjustment, people might also respond in other margins. One potential response is shifting income

[41]It would be interesting to examine whether similar people (i.e. with similar incomes) respond asymmetrically to tax increases versus decreases. Relatedly, Benzarti et al. (2017) find that prices respond more to increases than to decreases in value-added tax rates.

intertemporally. For example, anticipating an MTR decrease in a forthcoming tax reform, people would have incentive to shift income from pre-reform to post-reform periods. If such an intertemporal income shifting is salient, we should see people facing MTR decreases exhibit a dip in income between the annoucement of the tax reform and its implementation (July and August, 2011), and a surge in income in months after the reform. For those anticipating an MTR increase, we should see the opposite. This is observed in Denmark for wage income by Kreiner et al. (2016). However, in figure 1.4, we see no evidence for intertemporal income shifting. This cannot be explained by a smaller change in the MTR as compared to that in Denmark.[42] One possible explanation is the much shorter periods to make adjustments between the announcement and the implementation of the reform in China. China's 2011 PIT reform proposal was passed on June 30 and put into effect on September 1st, 2011, leaving only two months prior to the tax reform to make adjustment. By contrast, the Danish reform was passed in parliament at the end of May 2009 and changed the tax scheme from 2010 onward, leaving seven months to take adjustments. Kreiner et al. (2016) show that the income adjustment was obvious only starting from November, suggesting sufficient time is needed to make such adjustments.

Figure 1.5 shows clearly that the tax reform ETI increases steadily over time within the first year after the reform, and then converges to around 4, consistent with the income trends in figure 1.4.[43] Since a much larger portion of people (lower wage earners, over 70%) experience MTR decreases while a relatively small portion of people (higher wage earners, less than 10%) experience MTR increases, the ETI estimates reflect more of the income trend

[42] Kreiner et al. (2016) study the tax reform that reduced the highest marginal tax rate on earnings from 63 percent to 56 percent. Figure 1.4 shows that the average MTR decreases from 15 percent to 9 percent for those people facing MTR decreases, and the average MTR increases from 21 percent to 24 percent for those facing MTR increases, which is comparable to the tax rate change in Denmark.

[43] The magnitue of the ETI estimate is also in line with the naive ETI implied by figure 1.4. For example, the average log taxable income of the MTR decrease group increases by around 0.25 in the long run, and the average MTR decreases from about 0.15 to about 0.09 after the tax reform. This implies a long-run ETI of 3.66 (=0.25/(ln(1-0.09)-ln(1-0.15))), which is close to our estimate of 4.

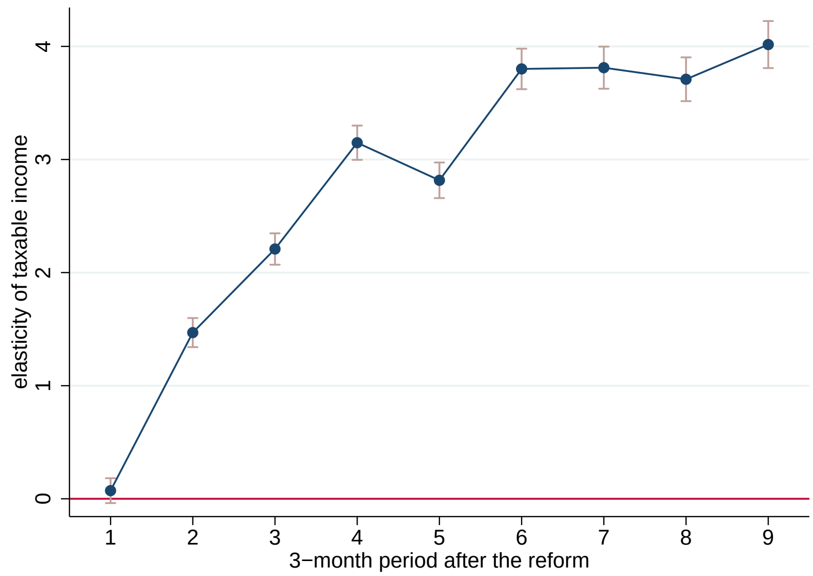


Figure 1.5. ETI estimates over time: revised tax reform approach
Notes: A 95% CI of the estimates is shown in the figure.

of the MTR decrease group. The concavely increasing trend of the ETI estimates shows that income adjusts gradually after a tax reform. The larger longer-run ETI also suggests a larger efficiency loss of tax in the long run. For our sample city of China, this figure suggests that the long-run ETI is about 4. In addition, the ETI estimate after one year of the tax reform is around 3, close to our ETI estimates using the standard tax reform approach with one-year difference specification.

Potential bias caused by kinks.

There is some concern that the existence of kinks might bias the tax reform ETI estimates. Due to the natural income growth, when one's income grows closely to the next kink, his income growth rate might slow down according to the bunching theory. For people experiencing MTR increases over a kink, the ETI may be under-estimated, since people won't reduce their income below the kink; for people experiencing MTR decreases, the ETI may be over-estimated, since people originally hindered by the kink will add to the aggregate responses of those not around the kink. Overall, the direction of the aggregate bias is unclear.

However, since income growth is unlikely to be hampered by the kink for a long time (shown below), the bias should be very small. In fact, excluding observations falling in the bunching regions in figure 1.2 does not change our ETI estimates, which is shown clearly in figure 1.18. Moreover, excluding observations around all kinks only slightly changes the estimates, as also found in Kleven and Schultz (2014).[44] Table 1.11 further shows how kinks could bias the tax reform ETI estimates. For people whose pre-reform TI is around the pre-reform kinks (i.e. falling in the bunching region of kinks), their ETI estimates are significantly larger; while for people whose predicted post-reform TI (based on pre-reform TI and income growth rates) is around the post-reform kinks, their ETI estimates are significantly smaller. These are consistent with the predictions by the bunching theory, as anticipating to be around kinks lowers the income growth rates, while getting rid of the constraint of kinks has income growth faster than normal. Finally and importantly, the ETI estimates for people not affected by kinks are very close to our original estimates, again showing kinks have little impact on the tax reform ETI estimates using the full sample.

1.4.4 Revised bunching approach

To come up with the global bunching ETI comparable with the dynamic tax reform ETI estimates, we use a two-step approach. First, we explore the dynamics of bunching behavior in response to the kink changes due to the tax reform. This will render a dynamic sequence of ETI estimates for each kink. Second, for each time period, using the approach described in detail below, we estimate a common structural ETI underlying the observed bunching ETI estimates at various kinks. This two-step approach finally gives us a sequence of global structural ETI for each period after the reform. In the following, we first introduce the approach to reveal the global underlying ETI using observed ETI estimates at various kinks. Then we apply this approach to reveal the global underlying ETI based on the observed ETI estimates using the standard bunching approach. In this process, we show that the global

[44] For non-bunching kinks, we exclude the ± 100 RMB inverval around each kink.

underlying ETI is not only consistent with the observed bunching at middle-high kinks, but can also explain the non-bunching evidence at lower kinks. Then we apply this approach to obtain a sequence of global bunching ETI estimates and compare them with those obtained from the revised tax reform approach.

Bunching approach with optimization frictions: conceptual framework

The standard bunching approach renders an observed ETI for each kink by assuming no optimization frictions. However, such an assumption seems unrealistic in general. Whenever possible, it is ideal to explicitly estimate the size of the optimization frictions to see if it can be reasonably ignored. We follow the literature to call the standard bunching ETI estimate $\widehat{e}$ the observed elasticity, and we want to know what underlying elasticity ε may have driven these observed elasticities when there exist optimization frictions. Here optimization frictions are defined broadly as any forces (e.g. real costs of adjusting earnings, inattention, or information costs) that prevent people from achieving optima. We adopt a modified approach based on Gelber et al. (2015) and use adjustment costs interchangeably with optimization frictions. We assume that there is a fixed ETI ε and a fixed utility cost ϕ that must be paid whenever adjusting income, which changes the utility function to $u(z-T(z), \frac{z}{n})-\phi$ if an adjustment is made.[45] To estimate ε and ϕ, at least two empirical moments are required. A key difference between our approaches is that Gelber et al. (2015) rely on a before-and-after comparison of

[45] Although the optimization frictions could be very large, as estimated in Kleven and Waseem (2013) in the notch case, the original approach by Saez (2010) does not consider the optimization frictions and thus cannot uncover the underlying structural elasticity. To obtain the underlying elasticity, two approaches are considered in literature (see Kleven (2016) for a detailed review). One approach is to utilize the variation in the size of kinks that is orthogonal to the underlying elasticity and optimization fractions. Along this approach, Gelber et al. (2015) assume a fixed optimization friction and underlying elasticity across kinks, which ensures them to be identified with two kinks. For more discussion on the assumption of a fixed adjustment cost, see Gelber et al. (2015). By contrast, Chetty et al. (2010) consider a more involved model where there are more parameters and thus need more bunching moments to make them fully identified. Another alternative approach to estimate the structural ETI from observed ETI estimates is the bound approach by Chetty (2012). The advantage of this bound approach is that it does not make specific assumptions on the utility function. But the cost of imposing weaker assumptions is that it requires large tax reforms to obtain tight bounds, which is not well satisfied in our case. In Online Appendix 1.6.5 we apply this approach and show very wide bounds. Thus, in our case, the bound estimates are not very informative and we stick to the parametric approach to uncover the structural elasticity.

bunching at the same kink where the jump in marginal tax rates ($\triangle\tau$) reduced, while we use bunching at different kink points to construct empirical moments. ε and ϕ are assumed fixed across kinks, which can be interpreted as the average elasticity and average adjustment cost for all people.

Consider a two-tier budget line with a kink at taxable income K. The MTR is τ_1 below K and increases to $\tau_2 = \tau_1 + \triangle\tau$ above K. When there is no adjustment cost, all individuals initially choosing $(K, K + \triangle z)$ would adjust income to K, and the person initially choosing $K + \triangle z$ is called the marginal buncher because he is indifferent between adjusting and not. When there is a positive adjustment cost ϕ, however, individuals initially choosing $(K, K + \underline{z})$ would find it too costly to adjust income to K, where $\underline{z}$ depends on ϕ. We assume $\underline{z} < \triangle z$ because otherwise the adjustment cost would be too large that there would be no bunching at the kink at all. Now all individuals initially choosing $(K + \underline{z}, K + \triangle z)$ would adjust income to K.[46]

Then the excess bunching with adjustment cost is

$$B = \int_{K+\underline{z}}^{K+\triangle z} h_0(z)dz \simeq h_0(K) \cdot (\triangle z - \underline{z}) = \frac{h_0^W(K) \cdot (\triangle z - \underline{z})}{W},$$

where the second equality holds approximately when $\triangle z$ is small or when the density is uniform in interval $(K, K + \triangle z)$. Then we have

$$b \equiv \frac{B}{h_0^W(K)} \simeq \frac{\triangle z - \underline{z}}{W}. \tag{1.4}$$

To estimate ε and ϕ, we take a parametric approach and assume a quasi-linear utility function $u(c, z; n) = c - \frac{n}{1+\frac{1}{\varepsilon}}(\frac{z}{n})^{1+\frac{1}{\varepsilon}}$, following Saez (2010), Kleven and Waseem (2013), and Gelber et al. (2015). Due to this assumption, ϕ has the same money metric unit as

[46] We follow Gelber et al. (2015) to make an intuitive assumption that the benefit of adjusting income to the kink K is increasing in distance from the kink for initial earnings in the range of $(K, K + \triangle z)$. This assumption is true, for example, when utility function is quasi-linear. See their paper for a more detailed argument.

consumption. With a linear tax schedule $T(z) = \tau z$, we have $c = z - \tau z$ and the utility maximization renders the optimal choice of earnings $z = n(1-\tau)^{\varepsilon}$. Then the person initially choosing $K + \underline{z}$ has ability $\underline{n} = \frac{K+z}{(1-\tau_1)^{\varepsilon}}$. Since this person is indifferent between adjusting income to K and making no adjustment, we have

$$u(K, \tau_1; \underline{n}) - \phi = u(K + \underline{z}, \tau_2; \underline{n}),$$

which renders

$$K(1-\tau_1) - \frac{\underline{n}}{1+\frac{1}{\varepsilon}}(\frac{K}{\underline{n}})^{1+\frac{1}{\varepsilon}} - \phi = K(1-\tau_1) + \underline{z}(1-\tau_2) - \frac{\underline{n}}{1+\frac{1}{\varepsilon}}(\frac{K+\underline{z}}{\underline{n}})^{1+\frac{1}{\varepsilon}}.$$

Plugging $\underline{n} = \frac{K+z}{(1-\tau_1)^{\varepsilon}}$ to the above equation and simplify it, we obtain

$$\frac{(K+\underline{z})(1-\tau_1)}{1+\frac{1}{\varepsilon}}[1-(\frac{K}{K+\underline{z}})^{1+\frac{1}{\varepsilon}}] - \phi = \underline{z}(1-\tau_2). \tag{1.5}$$

By definition, the underlying elasticity is $\varepsilon = \frac{\triangle z/K}{\triangle\tau/(1-\tau_1)}$, which combined with (1.4) renders

$$b \simeq \frac{\varepsilon \cdot \frac{\triangle\tau}{1-\tau_1} \cdot K - \underline{z}}{W}. \tag{1.6}$$

From (1.5) and (1.6), we can obtain an implicit relation $b = b(\varepsilon, \phi)$ at kink K. Denote by b_i the theoretical bunching at kink K_i and by $\widehat{b_i}$ the empirical moments estimated using (1.2).

We employ minimum distance estimation to estimate (ε, ϕ). The idea is to seek the values of the parameters that make theoretical bunchings b_is as close to the empirical bunching $\widehat{b_i}$s as possible:

$$(\widehat{\varepsilon}, \widehat{\phi}) = argmin_{(\varepsilon,\phi)} \sum_i (b_i - \widehat{b_i})^2.$$

Following Gelber et al. (2015), we obtain our estimates by minimizing the above equation numerically. Solving this problem requires evaluating b_i at each trial guess value of (ε, ϕ).

Here, we use observed bunching at various kinks to form empirical moments to estimate the two parameters $(\widehat{\varepsilon}, \widehat{\phi})$. Since $(\widehat{\varepsilon}, \widehat{\phi})$ is a function of the estimated amount of bunching $\widehat{b_i}$s, we can estimate their standard errors using the bootstrapped estimates of $\widehat{b_i}$s. In particular, we can solve for $(\widehat{\varepsilon}, \widehat{\phi})_j$ for the jth draw of $\widehat{b_i}$s, and the standard deviations of $(\widehat{\varepsilon}, \widehat{\phi})_j$ are the bootstrapped standard errors of $(\widehat{\varepsilon}, \widehat{\phi})$. With the estimates of $(\widehat{\varepsilon}, \widehat{\phi})$, we can also plug them back to (1.5) and (1.4) to obtain the estimates for $\underline{z}$ and $\triangle z$ for each kink K. The standard errors of $\underline{z}$ and $\triangle z$ are obtained similarly to those of $(\widehat{\varepsilon}, \widehat{\phi})$.

Revealing a common structural elasticity using standard bunching ETI estimates

To estimate both the underlying elasticity ε and the fixed adjustment cost ϕ, we need at least two empirical moments. In our standard bunching estimates (without optimization frictions), we focus on pre-reform taxable income kink at 20,000 RMB and post-reform kinks at 9,000 RMB and 35,000 RMB due to clear bunching evidence at these places. We thus use the bunching estimates at these kinks to form three empirical moments. Table 1.4 shows the estimates. If we assume a constant underlying elasticity of taxable income ε and a constant adjustment cost ϕ at all the three kinks, our estimates suggest that ε is 0.14 and ϕ is 1.22 RMB. Although the adjustment cost is small, it precludes those initially earning within around 30 RMB above the kink from adjusting earnings, while the marginal bunchers vary from earning 76 RMB above the kink to 317 RMB above the kink for the three kinks.

Table 1.4. Estimates for underlying ETI ε when there is fixed adjustment cost ϕ

	parameters	taxable income kink point K	point estimate	bootstrapped standard error
	underlying ETI ε	assumed constant across all kinks	0.14	0.02
	adjustment cost ϕ (RMB)		1.22	0.61
Bunching taxable income kinks	Δz (RMB)	pre-reform kink 20000 RMB	169.75	28.64
		post-reform kink 9000 RMB	76.39	12.89
		post-reform kink 35000 RMB	316.87	53.46
	$\underline{z}$ (RMB)	pre-reform kink 20000 RMB	26.83	13.90
		post-reform kink 9000 RMB	32.64	18.68
		post-reform kink 35000 RMB	25.58	13.01
Non-bunching taxable income kinks	Δz (RMB)	pre-reform kink 500 RMB	3.57	0.60
		pre-reform kink 2000 RMB	15.09	2.55
		pre-reform kink 5000 RMB	39.94	6.74
		post-reform kink 1500 RMB	14.70	2.48
		post-reform kink 4500 RMB	67.90	11.46
	adjustment cost $\underline{\phi}$ (RMB)	pre-reform kink 500 RMB	0.09	0.02
		pre-reform kink 2000 RMB	0.39	0.07
		pre-reform kink 5000 RMB	1.02	0.17
		post-reform kink 1500 RMB	0.53	0.09
		post-reform kink 4500 RMB	3.55	0.60

Notes: When there is fixed adjustment cost ϕ, for a kink point K, those initially choosing between $K+\underline{z}$ and $K+\triangle z$ will adjust income to K. Adjustment cost $\underline{\phi}$ makes the marginal buncher indifferent between adjusting income to K or not.

A key point here is that a small optimization friction could preclude a non-negligible amount of people from adjusting earnings to the optima. This could help explain why we observe a close to zero bunching at lower taxable income kinks. To see this, we explore the implication of our estimates of ε and ϕ based on bunching kinks for the non-bunching kinks. With the estimate of ε, we can use the definition of ε to obtain $\triangle z = \frac{\varepsilon \cdot K \cdot \triangle\tau}{1-\tau_1}$ for each non-bunching kink. Then for each non-bunching kink, we calculate the optimization friction $\underline{\phi}$ that would make the marginal buncher without optimization friction (i.e. the person originally choosing $K+\triangle z$) indifferent between adjusting income to K or not.[47] That is, $\underline{\phi} = \frac{(K+\triangle z)(1-\tau_1)}{1+\frac{1}{\varepsilon}}[1-(\frac{K}{K+\triangle z})^{1+\frac{1}{\varepsilon}}] - \triangle z(1-\tau_2)$. Table 1.4 shows that for each non-bunching kink except for the largest non-bunching kink (post-reform kink 4,500 RMB), $\underline{\phi}$ is smaller than ϕ, suggesting that the small estimated adjustment cost preclude any bunching at the lower

[47]We do not insert the estimates of ε and ϕ into equation 1.5 to solve for $\underline{z}$ for each kink because it does not necessarily have a solution.

kinks. As for the post-reform kink 4,500 RMB, figure 1.13 actually show small scale bunching, though not salient enough for our estimation purpose. Thus, a common structural elasticity with small adjustment cost could well explain both the observed bunching at middle-high TI kinks and the lack of bunching at lower kinks.

Estimating the global bunching ETI over time

The success of the structural ETI with optimization frictions to explain the bunching at middle-high taxable income kinks and the non-bunching at lower kinks provides support for assuming a common structural ETI underlying the observed bunching at various kinks. But the above bunching estimates are obtained by pooling all pre-reform (or post-reform) observations together and do not have a clear time property. To obtain the bunching ETI estimates over time, we explore dynamic bunching responses to changes of kinks because of the 2011 tax reform. In particular, we focus on the introduction of new kinks (9,000 RMB & 35,000 RMB) and explore the dynamic evolution of bunching scale after the reform. We do not use the abolition of old kinks (e.g. 20,000 RMB) due to asymmetric bunching responses to new kinks and to old kinks. It is observed in our data that bunching immediately disappear after the old kink (20,000 RMB) was abolished.[48] Thus, it seems desirable to just focus on bunching responses to the introduction of new kinks. Fortunately, we need at least two empirical moments to estimate ε and ϕ, and the introduction of kinks at 9,000 RMB & 35,000 RMB provide two such empirical moments. In addition, since 20,000 RMB is included in the range between 9,000 RMB and 35,000 RMB, using these two kinks would not narrow our coverage of income levels.

Since there is no bunching at the new kinks before the reform, we can simply apply the standard bunching approach to estimate the ETI at each kink for each period after the reform. Then for each period, we apply the above approach to uncover the structural ETI. One advantage of such approach is that it could allow optimization frictions to vary flexibly

[48] The asymmetric bunching responses to new kinks and to old kinks are also observed in Best and Kleven (2016), under the setting of housing transaction taxes.

over time. In principle, we can estimate a bunching ETI for each month. But observations at the middle-high kinks are too few for each month, so we combine three months as a period. To make a direct comparison, we show the revised tax reform ETI by 3-month period and overlap it with the dynamic sequence of global bunching ETI in figure 1.6.[49]

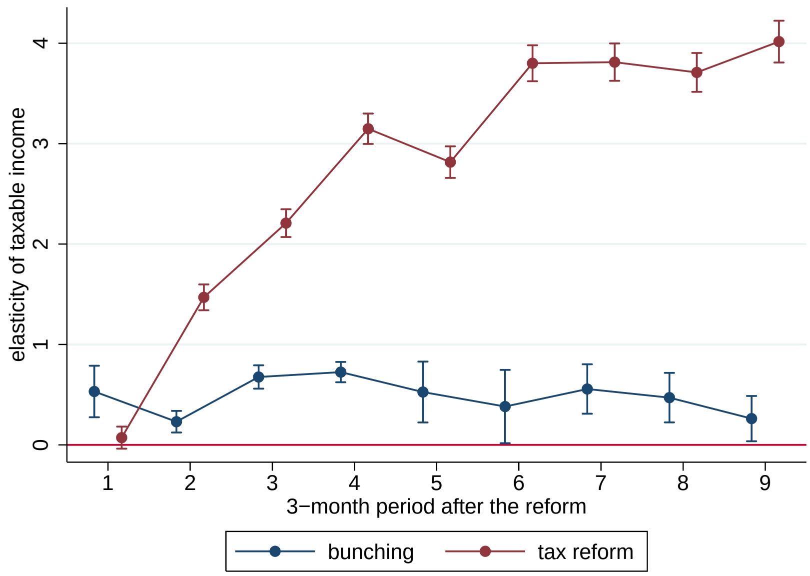


Figure 1.6. Global ETI estimates over time: tax reform approach vs. bunching approach
Notes: A 95% CI of the estimates is shown in the figure. The bunching ETI are estimated using the decimal sample.

1.4.5 Tax reform ETI vs. bunching ETI: key differences and potential explanations

Figure 1.6 shows a sharp contrast between the tax reform ETI and the bunching ETI over time. It clearly documents two key differences:

1. The tax reform ETI estimates increase concavely over time, while the bunching ETI estimates are stable over time.

[49] Although the global bunching ETI estimates shown in figure 1.6 are based on the decimal sample, in Online Appendix 1.6.6 we show that using the full sample would generate similar dynamic patterns of bunching ETI estimates.

2. The long-run tax reform ETI estimates (around 4) are much larger than the bunching ETI (around 0.5).

If the tax reform approach and the bunching approach both capture the same structural elasticity that governs people's behavior, we should not observe such differences. Thus, the two approaches are probably capturing different behavioral responses. What behavioral response does each approach fundamentally capture? Why do they capture such different behavioral responses? In the following, we explore the potential answer to these questions. We try to use a parsimonious model to account for the above two stylized facts altogether. The key ingredients of the model include: (1) infrequent but permanent adjustments of hours of work at no additional cost, and (2) temporary adjustment of hours of work with some adjustment costs. The model is able to not only reconcile these two key differences, but also generates other testable implications supported by our data.

A simple model

Since wage and salary are subject to third-party reporting, we follow the tradition in the literature (e.g. Chetty et al. (2011), Tazhitdinova (2018)) to interpret the behavioral responses as a labor supply (via changes in working hours) rather than income manipulation (avoidance or evasion).[50] Denote hours of work at time t by h_t and the wage rate by w_t. The latent income (corresponding to the optimal hours of work h_t under current tax rate) is $z_{lt} = h_t w_t$. We assume that w_t is exogenous to employees and grows at a rate g, i.e. $w_t = w_0(1+g)^t$, while h_t can be adjusted permanently with an exogenous probability $q < 1$ in each period, at no additional cost. Here $q < 1$ reflects that due to various restrictions (e.g. constraints of pre-existing contract, time to adjust production organization, and time

[50] Admittedly, we cannot exclude the possibility that manipulation partially accounts for the behavioral responses captured by the ETI estimates. In Online Appendix 1.6.7, we provide evidence that behavioral responses captured by tax reform ETI estimates include both income manipulations and real responses (i.e. hours of work), while it is less clear whether bunching ETI reflects any real responses beyond strategic income manipulations. While there has been no consensus about what behavioral responses are captured by the two approaches, we show that a simple labor supply model would be sufficient to reconcile the stylized facts we document.

needed to switch to a new job), people need time to adjust their hours of work to the new optima. Denote taxable income by z_t, which would be the same as z_{lt} without interruption. In addition, we assume that in each period after the potential permanent adjustment of h_t, people can still adjust h_t temporarily by paying an additional cost; this would make z_t deviate from z_{lt} only for the current period. Suppose the adjustment cost takes the form of $\phi + c \cdot f(|z_{lt} - z_t|)$, where $\phi > 0,\ c > 0,\ f' > 0,\ f'' > 0$. Here $z_{lt} = h_t w_t$ corresponds to the income under the old optimal hours of work h_t, while $z_t = h_t^{temp} w_t$ corresponds to the income associated with the temporarily adjusted hours of work h_t^{temp}. The assumed form of adjustment costs imbeds the idea that the costs of making a temporary adjustment of hours not only involve a fixed cost ϕ common to every one (corresponding to the technical cost of adjusting h_t temporarily, as well as search costs and informational costs), but also include a penalty, which convexly increases with the deviation from the hours of work specified by the current contract.[51]

Figure 1.7 illustrates how a tax change affects the optimal income choice when there is no adjustment cost. When the MTR increases from τ_1 to τ_2, the utility curve $u(z_t)$ (a value function of income z_t) shifts leftwards, and the optimal choice of income z_t (determined by hours of work h_t, for a given wage rate w_t) will decrease from z_{lt} to z_{lt}^{new}. The initial optimal choice lies at point A. When the tax rate increases, if a person does not adjust behavior, his utility would drop to point B; the total benefit of adjusting income increases as he moves from B throughout C. But the marginal benefit (MB) of adjusting decreases from B throughout C, and would be negative below C. The positive segment of the MB curve is shown in figure 1.8,

[51] A fixed cost ϕ for making a temporary adjustment of h_t, instead, is not sufficient to explain the stylized facts. Under our model, if there is bunching at a kink, it implies that people slightly above the kink find it worthwhile to making a temporary adjustment. If a fixed cost ϕ applies for all people making a temporary adjustment, then all people with income well above the kink would find it worthwhile making an adjustment. Therefore, they would immediately adjust to the new long-run optima, even when they do not face a chance to permanently adjust h_t. This, however, would be inconsistent with the concavely increasing tax reform ETI estimates.

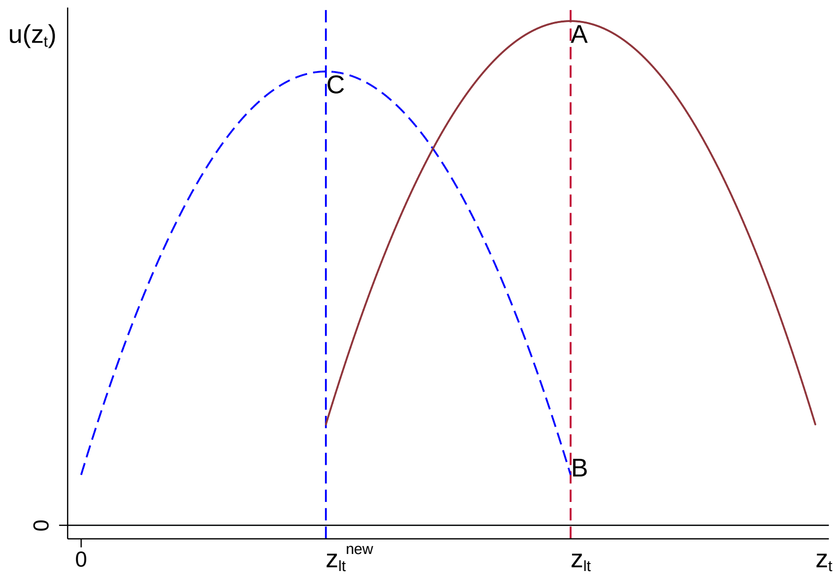


Figure 1.7. The impact of a tax change on the optimal income choice
Notes: Without considering adjustment costs, when the MTR increases from τ_1 to τ_2, the utility curve shifts leftwards, and the optimal choice of income z_t (determined by hours of work h_t, for a given wage rate w_t) decreases from z_{lt} to z_{lt}^{new}.

as this is the only possible part for a potential adjustment.

For the infrequent but permanent adjustment of hours of work, there is no additional adjustment cost. If the tax change is simply an increase of the MTR in all income levels, then all people would simply adjust hours of work to the new optimum when they face such a chance, which corresponds to an adjustment from z_{lt} to z_{lt}^{new}, as illustrated in figure 1.7. But we are considering an increase of the MTR only for incomes above a kink K. Then there are two cases. For people with a z_{lt} well above K, their new optimum z_{lt}^{new} should be higher than K, and therefore they would simply adjust hours to the new long-run optimum when they face such a chance. For people with a z_{lt} close enough to K, who face a smaller z_{lt}^{new} than K, the kink might induce them to adjust h_t less sufficiently to the new long-run optimum. The wage growth rate g and the discount rate δ together determine the extent of their adjustment of h_t. A larger g will make the gains from an insufficient adjustment of h_t smaller, since people will then stay close to the kink for a shorter time; a smaller δ (corresponding to a more patient person) will also make the gains from an insufficient adjustment of h_t smaller, since the gains from adjusting income to the kink will then have a lower weight in the total discounted utility of all periods. Therefore, people with a larger g or a smaller δ will adjust more fully to the new long-run optimal hours of work. Finally, there are some people who are not currently experiencing an increase in the MTR yet forecast such a change in the near future as their wage rate grows steadily. For these people, when they face a chance to permanently adjust hours of work, they would first adjust to a weighted average of their short-run optimum (under current tax rate) and their new long-run optimum, and adjust fully to their new long-run optimum when they face the next chance to make a permanent adjustment.

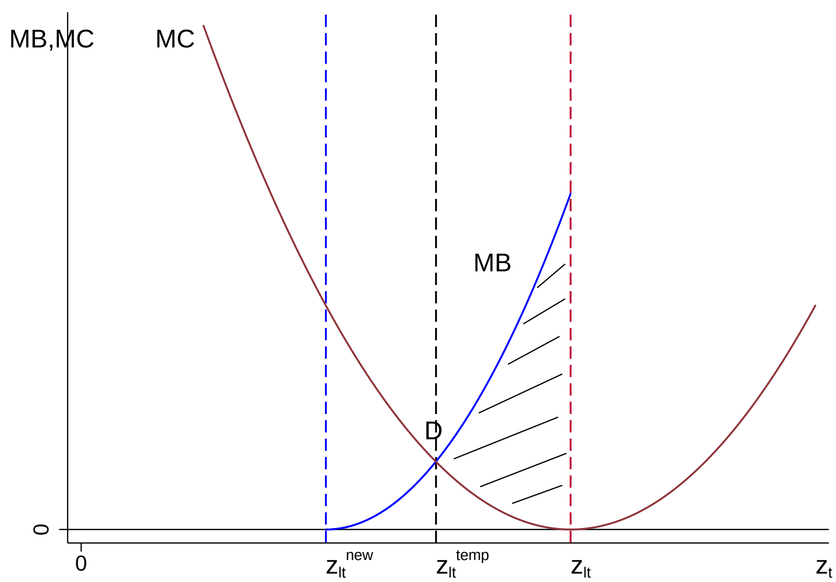


Figure 1.8. Marginal benefit and marginal cost of adjusting income
Notes: The upward sloping curve depicts the positive segment of the marginal benefit curve, and the U-shaped curve depicts marginal costs in relevant region.

In addition, in each period, people can temporarily adjust hours of work by paying a cost $\phi + c \cdot f(|z_{lt} - z_t|)$. The temporary adjustment only affects income for the current period, so people only need to compare gains and benefits from adjusting income for the current period. Since we have assumed $c > 0$, $f' > 0$, $f'' > 0$, the marginal cost (MC) of making a temporary adjustment increases convexly with the adjustment distance, which is illustrated in figure 1.8. Let D be the cross point of MC and MB curves. Starting from z_{lt}, MB decreases and MC increases as people adjust towards z_{lt}^{new}. The gain from the temporary adjustment is the area below MB and above MC. Obviously, the largest gain is obtained when people adjust to z_{lt}^{temp}, the income level corresponding to D. This gain, indicated by the shaded area, should then be compared with the fixed cost ϕ. If the shaded area is smaller than ϕ, then the optimal choice is to make no temporary adjustment. If the shaded area is larger than ϕ, then there are two cases. For people with a z_{lt} far above K, their cross point D is likely to be higher than K, their optimal choice is to adjust temporarily to z_{lt}^{temp}. For people with a z_{lt} close enough to K, their cross point D is likely to be lower than K; their choice is binded by the kink, and therefore they will temporarily adjust to K, contributing to the bunching there.

In the following, we show that the major implications of the above simple model can reconcile the key differences between the ETI estimates obtained using two approaches. Moreover, the model generates other testable implications supported by our data.

Major implications from the model

Concavely increasing tax reform ETI.

When people face a chance to permanently adjust h_t, most of them (those well above the kink) will simply adjust to the new long-run optimal hours, some of them (those close enough to the kink) will adjust partially to the new long-run optimum. As long as the wage growth rate g is not too small, or people are patient enough (δ small), people will mostly adjust h_t to the new long-run optima when they face a chance to make a permanent adjustment. Therefore, assuming a smooth distribution of initial income, the permanent adjustment of h_t would have little contribution to the bunching at the kink. Since in each period people independently face such a chance w.p. $q < 1$, we should observe they aggregately gradually adjust hours of work over time. This gradual adjustment to the new long-run optima would be captured by the tax reform ETI estimates.[52] The fraction of people that has shifted to the new optimum after j periods would largely equal to $1-(1-q)^j$, yielding a curve roughly corresponding to the concavely increasing ETI estimates obtained from the revised tax reform approach.[53]

Small and stable bunching ETI.

As the permanent adjustment of h_t contributes little to the bunching at the kink, bunching mostly reflects the temporary adjustment of h_t, and is therefore ruled by the adjustment cost. Without such cost, all people would immediately adjust to the new long-run optimal hours, and the bunching ETI and the tax reform ETI would both immediately reflect the long-run underlying ETI. When there are convex costs for a temporary adjustment of h_t, however, people will adjust hours only partially relative to the new long-run optima. When the costs for a temporary adjustment are large enough, the bunching ETI can be arbitrarily small. Since in each period, people with income falling into the same certain range will find it worth adjusting income (via adjusting h_t) to the kink (though they are different groups of people in different periods under a normal wage growth rate $g > 0$), assuming a largely stable income distribution, the bunching ETI estimates will be stable over time.

[52]The tax reform ETI etsimates also reflect the temporary adjustment of h_t. We have shown that with adjustment costs, people temporarily adjust h_t only partially. When the costs for a temporary adjustment are large enough, the temporary adjustment of h_t can be small enough, dwarfed by the full adjustment of those facing a chance to parmanently adjust h_t. Therefore, the tax reform ETI estimates mostly reflect the permanent adjustment of h_t.

[53]Our model implies concavely increasing tax reform ETI estimates for people at all income levels, since all people will gradually face the opportunity to permanently adjust hours. This implication is consistent with the empirical evidence in figure 1.4, which shows that people with lower income levels (those experiencing MTR decreases) experience gradual income adjustments, just like people with higher income levels (those experiencing MTR decreases) do. The lack of bunching at lower kinks and the existence of bunching at higher kinks, however, are explained by the fixed costs (now plus an additional convex cost) for a temporary adjustment of income (via changing hours) and the different gains from making such an adjustment across different kinks,

Convergence of two estimators in a stagnant economy.

If instead $g = 0$, i.e. an economy is stagnant, the bunching approach is then able to capture even the permanent adjustment of behavior. In this case, we should expect to see a gradual (and concave) increase of bunching at the newly introduced kink over time. People close enough to the kink but not making a temporary adjustment of h_t (because adjustment costs are higher than the accrued benefits from a temporary adjustment) would gradually face a chance in each period w.p. $q < 1$ to permanently adjust h_t without paying additional cost. Now they can adjust hours so that income is adjusted to the kink. The gradual permanent adjustment implies a concavely increasing pattern of bunching at the kink K. Therefore, our model implies that in a stagnant economy, the bunching ETI will not only capture the temporary adjustment of h_t in the short run, but also reflect the permanent adjustment of h_t in the long run. Thus, the bunching and the tax reform ETI estimators would finally converge to the same underlying parameter. Although this pattern is not ready to test using our data, it can be tested in an otherwise stagnant economy. This implication indicates the potential of the bunching approach in capturing the desired underlying behavioral response in some special cases, which could be explored beyond the personal income tax setting in the future.

Other testable implications

"Temporary bunchers" in a growing economy.

In general it is natural to assume a positive wage growth rate $g > 0$ for a given individual, which is normally true for most countries, and particularly true for China. This implies that the stable bunching pattern in each period reflects temporary adjustments of hours by different groups of people. Driven by normal wage growth, people would only make a temporary income adjustment when their income happen to be falling into the bunching region. As wage rate grows steadily, or due to the fluctuations of income over time, people will not stay in the bunching region for a long time. This "temporary bunchers" implication is supported by our data. To examine this, we show the distribution of individuals by bunching months. Table 1.5 shows that, for each bunching kink, most bunchers stay around the kink for no more than 3 months, suggesting bunching mainly as a symptom of a temporary income adjustment, consistent with the "temporary bunchers" implication.[54] For a given group of people, bunching does not persist due to natural income growth and the fixed kink position.[55] The bunching ETI well captures the temporary income adjustment distorted by the local MTR change in a stable way. Fundamentally, it is because different people make similar responses when their incomes are around kinks.

Table 1.5. Distribution of individuals by bunching months

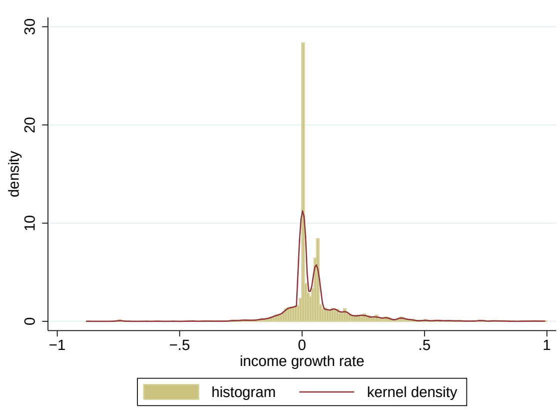
	Number of individuals		
Number of bunching months	Pre-reform kink 20000 RMB: 2009.6-2011.8 (27 months)	Post-reform kink 9000 RMB: 2011.9-2013.12 (28 months)	Post-reform kink 35000 RMB: 2011.9-2013.12 (28 months)
1	270	3,762	572
2	21	620	81
3	3	216	34
4	2	124	25
5	0	83	10
6	3	42	10
7	0	21	6
8	1	24	1
9	3	20	11
10	2	19	5
11	3	10	2
12	1	17	4
>12	8	28	11
Total	317	4,986	772

[54] Here we use the decimal sample. Using full sample yields similar results.

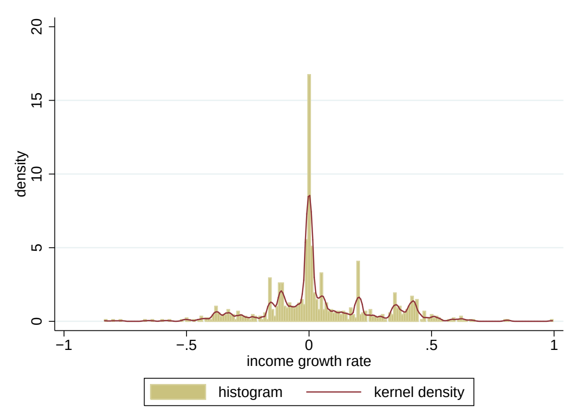
[55] Even if kinks are inflation indexed as in some countries, it is unlikely that a group of people will stay around a kink for a long time, given that income growth rates of different people are quite heterogeneous and these income growth rates are probably different from the general inflation rate or the average income growth rate of all people.

Bimodal distribution of income adjustment.

Our model assumes that people occasionally face the chance to permanently adjust hours of work, and when they do, they adjust hours permanently to their new long-run optimum. This implies that the distribution of income growth across periods should exhibit a bimodal pattern: for initial periods after the tax reform, a small portion of people facing a chance to permanently adjust hours will exhibit a larger income adjustment, while the rest of people not facing such a chance would follow their previous income path and exhibit a more stable income growth. To examine this, we focus on people experiencing MTR changes due to the tax reform. Figure 1.9 shows the distribution of individual average income growth from the 3-month period before the tax reform to the 3-month period after the reform. For people experiencing MTR decreases, who consist the majority of our sample, there is a clear bimodal distribution of income adjustment, thus providing support to our model. Yet for people experiencing MTR increases, the bimodal pattern is less salient, suggsting asymmetric responses to MTR increases versus decreases.



(a) MTR decrease group



(b) MTR increase group

Figure 1.9. Bimodal distribution of income adjustment
Notes: The figure shows the distribution of individual average income growth rate from the 3-month period before the tax reform to the 3-month period after the reform. Bin width=0.01.

Heterogeneity.

The heterogeneity among people may also have implications for bunching. In particular, people with a smaller c or ϕ would be more likely to be in the bunching region because they would face larger net gains from making the temporary income adjustment. Evidence from data provides support for this prediction. As table 1.10 shows, people in the bunching region tend to have a higher position than the nearby non-bunchers. These people are more likely to have managing power in the workplace (with position of middle deputy and above) and thus are facing a lower c or ϕ.

Alternative hypothesis

In the above section, we propose a simple model that can account for the stylized facts using parsimonious elements and generate testable predictions. In spite of this, there might be other alternative explanations that can account for the differential behavioral responses captured by the two empirical approaches. One hypothesis is that while the tax reform approach captures the behavior of all people, the bunching approach mostly captures behavioral responses of those that are more able to adjust taxable income to the kink place. As we show in table 1.10, the bunchers at middle-high taxable income kinks tend to have a higher position than the non-bunchers, i.e. bunchers are more likely to own some managing power in the workplace (with position of middle deputy and above). This evidence does show that people who are able to bunch are not the same with those who are not. However, this hypothesis alone cannot explain either of the stylized facts. It instead provides some complementary explanation to our model. Our simple model shows how the different ETI estimates obtained by two approaches can be generated from simple assumptions even among homogeneous individuals. Given our model, admitting that bunchers are somewhat different from general persons implies that while the tax reform approach can capture permanent behavioral responses of the general people, the bunching approach could more of capture the temporary behavioral responses of individuals with a higher position, who are more able to make such adjustment.

1.5 Conclusion

Bunching is a recently developed approach that has great potential to be applied in many fields such as taxation, social security, social insurance, welfare programs, and others.[56] While the techniques to estimate desired parameters from observed bunching are quite mature, the underlying behavioral responses captured by bunching are not all that clear. In this paper, we show that under personal income tax setting, the bunching approach captures very different behavioral response to marginal tax rate changes than that captured by the tax reform approach. We document that while the tax reform ETI estimates increase concavely over time, the bunching ETI estimates are stable and much smaller. While the tax reform approach can capture the infrequent but permanent adjustment of hours of work to tax changes, the bunching approach can only reflect temporary adjustment. We show that a simple model reconciles the sharp contrasts between ETI estimates obtained by the two approaches.

Of course, beyond the personal income tax context, bunching does not necessarily capture only temporary behavioral adjustments. In other cases, bunching may be able to reflect the longer-run behavioral responses. In fact, our model implies that in a stagnant economy, the bunching ETI can capture the permanent adjustment of behavior in the long run. But in most cases, bunching estimates alone are not sufficient to reveal the underlying structural parameter, as the adjustment costs that differentiate the bunching estimates from the tax reform estimates are not able to be pinned down solely by the bunching ETI. A lesson from this paper is that the behavioral responses underlying bunching should be carefully examined case by case and a policy implication from the bunching estimates should be cautiously made.

Our findings show the limitation of bunching ETI estimates for policy concerns and

[56] Kleven (2016) provides an up-to-date review on potential applications of the bunching approach.

therefore recommend the tax reform ETI as more relevent for policy making. In addition, the large ETI estimates obtained in this paper suggest that marginal tax rates are too high in China. We show that a uniform MTR decrease would bring about both revenue increase and efficiency improvement, at least for our sample city (Online Appendix 1.6.8). This clearly suggests reducing the MTRs in a future personal income tax reform. Any argument to increase the MTR for high income earners for redistributional reason does not receive support from our ETI estimates.

Chapter 1 is currently being prepared for submission for publication of the material. The authors are Daixin He, Langchuan Peng, Xiaxin Wang. The dissertation author, Xiaxin Wang, was the primary investigator and author of this paper.

1.6 Online Appendix

1.6.1 China's personal income tax schedule

There are 11 income components taxed under China's personal income tax. They are divided into three broader components: wages/salaries income, self-employment income, and other incomes. Self-employment incomes include (1) income from production or business operation by self-employed industrial and commercial households, and (2) income from the contract and operation of enterprises and institutions and the business income of the lease. Other income components include (1) remuneration for providing services,[57] (2) author's remuneration, (3) royalties income, (4) interest, dividend, and bonuses, (5) income from leasing property, (6) property transfer income, (7) accidental income, and (8) other incomes. Table 1.6, figure 1.1, and figure 1.10 show the tax rates, standard deductions, and changes in them due to the 2011 PIT reform. Overall, the 2011 tax reform increases the monthly standard deduction for monthly wage/salary from 2,000 RMB to 3,500 RMB, increases the MTR for high wage/salary earners and decrease the MTR for low wage/salary earners. For the self-employment incomes, the statutory MTR generally decreases for all income levels that experience an MTR change. But since most self-employed businesses pay a pre-determined fixed amount income tax, it is not clear how their tax liabilities changed in reality.

Self-employment income tax

Despite the statutory progressive tax rate, as shown in figure 1.10, in practice, based on the availability of a reliable accounting book, the rule to tax self-employment income is as follows. First, if a self-employed business has a complete accounting book on costs and

[57] Remuneration for providing services is paid to part-time work and enjoy a standard deduction of 800 RMB if this income does not exceed 4,000 RMB (see table 1.6), while wage/salary is paid to full-time work. Suppose a person has a wage earnings of 2,800 RMB before the tax reform, he can deduct 2,000 RMB and his taxable income is 800 RMB. If he instead has a wage earnings of 2,000 RMB plus a remunerationÂăforÂăprovidingÂăservices of 800 RMB, then he can deduct 2,000 RMB for wages and deduct 800 RMB for remunerationÂăforÂăprovidingÂăservices, which results in a zero taxable income. There is anecdotal evidence that some people may use the deduction by income item to avoid tax.

incomes and the tax bureau finds it reliable, then self-employment income implied by the accounting book will be taxed following the progressive tax rate structure. This approach, called *chazhang* levy, is usually applied to large scale self-employed businesses and is very rarely used in practice. Second, for most self-employed businesses, since the accounting book is largely absent or severely incomplete, it is hard to obtain a precise measure of self-employment income. Accordingly, the tax bureau adopt the so-called *heding* levy approach, i.e. tax levy based on assessment. This levy approach first requires an assessment of business volume based on business area, industry, measurable business costs (e.g. electricity and water), receipts (if any), and other useful information. Then based on the assessment, there are two tax levy approaches. The first approach is to generate taxable income based on the assessed income and a taxable income ratio varying by industry, and then obtain the tax liability by imposing the progressive tax rate on this taxable income. The second approach, which is applied to the majority of self-employed businesses, is a predetermined fixed amount tax approach, called *dinge* levy.[58] In particular, the tax bureau will make a list of all self-employed businesses that are subject to this approach and the fixed amount of personal income tax liability for each of them.[59] [60] In many cities, this list is required to be made publicly available online periodically. Theoretically, this predetermined tax rule might cause some distortion in firm's behavior, though possibly much less than that would have been caused by a progressive tax rate structure.[61] Given this background, and due to the lack of self-employment income data,

[58]We do not find figures on ratios of self-employed business subject to different tax levy approach. By by inquiring tax bureau staff, we confirm that for our sample city and all the other cities we have asked, most self-employment income is taxed under the *dinge* levy approach. The wide adoption of this approach may be due to its relatively low enforcement cost as compared to its alternatives.

[59]Each local tax bureau has its own way to determine the amount of this predetermined fixed amount tax. The formula is not made public. But it is said that the fixed amount may depend on a self-employment business's previous income, industry, and other useful information.

[60]A large fraction of self-employed businesses have a too small scale to pay any personal income tax. For example, currently in Anhui province, if the assessed monthly business revenue is less than 30,000 RMB, then no personal income tax needs to be paid. By inquiring relevant tax stuff, we know that in Jiang county of Shanxi province, only 10% of self-employed businesses need to pay personal income tax.

[61]In China, in addition to the personal income tax, self-employed businesses are also subject to many other taxes and fees including the value-added tax, business tax, city maintenance and construction tax, local education supplementary fee, stamp tax, housing property tax, operations tax, etc. In practice, all these taxes for the self-employed follow the *dinge* levy approach, i.e. having a predetermined fixed amount property.

throughout this paper, we mainly focus on wage/salary income. This is approapriate since our main focus is the ETI w.r.t. tax rate and the self-employment income tax is not based on a rate structure. In addition, for the majority of people, wage/salary income is the major income source.

1.6.2 Tax on bonuses

In China, bonuses are taxed separately and differently from regular monthly wage/salary. In the following, we first show how bonuses are taxed differently from regular wage, and then explore the implications of such difference.

Regular monthly wage follows the tax schedule in figure 1.1, which can also be summarized by table 1.8. In practice, tax liability is calculated using table 1.8 for its convenience. For example, for post-reform period, after obtaining the taxable income of a month, the tax liability is calculated as $TI \times MTR$ - quick deduction number.

Bonuses are not taxed in the same way as regular wage, since otherwise the tax law would impose a penalty on bonuses, given bonuses are normally much higher than regular wage and the tax rate structure is graduated. In the month when the bonuses are paid, regular wage and bonuses are taxed separately. Suppose bonuses in a month are B RMB, the regular wage in this month is W RMB, the standard deduction is D RMB. There are two cases. (1) if W>D, then W is taxed in regular way. For B, we first divide it by 12, and then find the corresponding MTR in table 1.8, the tax liability for B is then ($MTR \times B$ - quick deduction number) RMB. (2) if W<D, then W is not taxable. For B, we first divide B+W-D by 12, then find the corresponding MTR in table 1.8, the tax liability is then ($MTR \times (B+W-D)$ - quick deduction number) RMB.

By dividing bonuses by 12 before applying the MTR of the regular wage tax schedule, this "tax on bonuses" rule is designed to avoid the unusually high MTR pushed up by high bonuses. However, the formula of calculating tax liability for bonuses is inappropriately designed in the sense that it creates tax notches (i.e. there is a jump in total tax liability when income is slightly higher than the cutoff value). The problem lies in the inappropriate application of quick deduction number in regular wage tax formula to the bonuses tax formula. For example, in the post-reform period, if bonus is slightly lower than 18,000 RMB, then an MTR of 3% applies, and the tax liability is slightly lower than $18,000 \times 3$ RMB. But if bonus is slightly higher than 18,000 RMB, then an MTR of 10% applies, and the tax liability is slightly higher than $18,000 \times 10$ RMB. Interestingly, although there is evidence that the tax notches for bonuses are well-known by shrewd accounting staffs (we see clear bunching of bonuses at these notches), the tax formula for bonuses has never been corrected in tax law.

Our data do not indicate which income corresponds to bonuses. But we are able to observe which incomes apply the "tax on bonuses" rule. We thus define the incomes following the "tax on bonuses" rule as bonuses.

If bonuses are taxed in the same way as regular monthly wage, then the optimal wage and bonuses distribution (in the sense of paying least tax), for a given annual earnings, would pay largely equal wage and bonuses across months. Given the "tax on bonuses" rule, what is the combination of wage (W) and bonuses (B) that yields least tax, for a given annual earnings (E)? Let's take post-reform period (e.g. 2012 and 2013) for example, which corresponds to a standard deduction of 3,500 RMB. If E is less than $3,500 \times 12 = 42,000$ RMB, a combination of $W = E$ and $B = 0$ yield tax liability 0. If E lies between 42,000 RMB and 78,000 RMB ($= 42,000 + 1,500 \times 12 + 18,000$), then the optimal distribution is some combination between wage and bonuses so that the MTR for bonuses and regular wages are no more than 3%. The tax liability under the optimal distribution is $(E - 42,000) \times 3\%$ RMB, though there are infinite possible combinations of W and B to achieve this goal. For optimal bonuses policy and lowest tax liability for a given annual earnings, we solve the following tax miminization problem:

$$min_{W,B} T_1(W) + T_2(B) \, s.t. \, W + B = E,$$

where

$$T_1(W) = \begin{cases} 0 & if\, W \in [0, 42000] \\ (W - 42000) \times 0.03 & if\, W \in (42000, 60000] \\ (W - 42000) \times 0.1 - 12 \times 105 & if\, W \in (60000, 96000] \\ (W - 42000) \times 0.2 - 12 \times 555 & if\, W \in (96000, 150000] \\ (W - 42000) \times 0.25 - 12 \times 1005 & if\, W \in (150000, 462000] \\ (W - 42000) \times 0.3 - 12 \times 2755 & if\, W \in (462000, 702000] \\ (W - 42000) \times 0.35 - 12 \times 5505 & if\, W \in (702000, 1002000] \\ (W - 42000) \times 0.45 - 12 \times 13505 & if\, W \in (1002000, \infty), \end{cases}$$

and

$$T_2(B) = \begin{cases} B \times 0.03 & if\, B \in [0, 18000] \\ B \times 0.1 - 105 & if\, B \in (18000, 54000] \\ B \times 0.2 - 555 & if\, B \in (54000, 108000] \\ B \times 0.25 - 1005 & if\, B \in (108000, 420000] \\ B \times 0.3 - 2755 & if\, B \in (420000, 660000] \\ B \times 0.35 - 5505 & if\, B \in (660000, 960000] \\ B \times 0.45 - 13505 & if\, B \in (960000, \infty). \end{cases}$$

Solving this problem, we can obtain the lowest tax liability as

$$T(E) = \begin{cases} 0 & if\, E \in [0, 42000] \\ 0.03E - 1260 & if\, E \in [42000, 78000] \\ 0.1E - 6720 & if\, E \in (78000, 114000] \\ 0.2E - 18120 & if\, E \in (114000, 125550] \\ 0.1E - 5565 & if\, E \in (125550, 150000] \\ 0.2E - 20565 & if\, E \in (150000, 204000] \\ 0.25E - 30765 & if\, E \in (204000, 516000] \\ 0.3E - 56565 & if\, E \in (516000, 565500] \\ 0.2E - 15 & if\, E \in (565500, 570000] \\ 0.3E - 57015 & if\, E \in (570000, 669000] \\ 0.25E - 23565 & if\, E \in (669000, 862000] \\ 0.3E - 67665 & if\, E \in (862000, 1122000] \\ 0.35E - 123765 & if\, E \in (1122000, 1422000] \\ 0.45E - 265965 & if\, E \in (1422000, 1550333] \\ 0.3E - 33415 & if\, E \in (1550333, 1662000] \\ 0.45E - 282715 & if\, E \in (1662000, \infty), \end{cases}$$

and the optimal bonuses policy is

$$B(E)=\begin{cases}0 & if\, E\in[0,42000]\\ [0,E-42000] & if\, E\in(42000,60000]\\ [E-60000,18000] & if\, E\in(60000,78000]\\ 18000 & if\, E\in(78000,125550]\\ E-96000 & if\, E\in(125550,150000]\\ 54000 & if\, E\in(150000,565500]\\ E-462000 & if\, E\in(565500,570000]\\ 108000 & if\, E\in(570000,669000]\\ E-462000 & if\, E\in(669000,862000]\\ 420000 & if\, E\in(862000,1550333]\\ E-1002000 & if\, E\in(1550333,1662000]\\ 660000 & if\, E\in(1662000,\infty).\end{cases} \tag{1.7}$$

The lowest tax liability $T(E)$ is a continuous function of E. But with tax notches for bonuses, the optimal bonuses policy is not a continuous function of annual earnings E. From equation 1.7, we expect to see significant bunching of bonuses at values like 0 RMB, 18,000 RMB, 54,000 RMB etc. For annual earnings between 42,000 RMB and 78,000 RMB, there are infinite possible optimal bonuses falling in a certain range, though the optimal bonuses policy is unique for the other annual earnings. Would actual bonuses be consistent with equation 1.7? First, we find surprisingly few incomes applying the "tax on bonuses" rule. In 2013, only 0.45% (2,492 in 550,506 individuals) of people have any bonuses in our data.[62] This fraction is very similar in other years. Equation 1.7 shows that theoretically people with annual earnings above 42,000 RMB should have positive bonuses. In 2013, there are 192,893

[62] We restrict our sample to people with positive income in every month in 2013.

individuals having annual earnings above 42,000 RMB. Hence the few people having bonuses cannot be explained by the optimal bonuses policy. We are not entirely sure why there are so few people having bonuses. Perhaps many people receive bonuses in cash, as much anecdotal evidence says. Now let us just focus on those having positive bonuses, and see if they follow the optimal bonuses policy.

In figure 1.19, we compare the optimal bonuses policy and actual bonuses in 2013. We focus on annual earnings below the threshold of 565,500 RMB because almost all individuals with positive bonuses in 2013 have annual earnings below this threshold. Figure 1.19 shows that although actual bonuses do not coincide the optimal bonuses policy in a precise manner, they follow similar pattern. In particular, like the optimal bonuses policy, for a wide range of annual earnings starting from 0 RMB, the actual bonuses remain at a largely constantly low level, corresponding to the optimal bonus policy of 0 for initial levels of earnings. Then at the middle range of incomes, there is a linear relation between bonuses and annual earnings, similar to the optimal bonuses policy. Finally, when annual earnings surpasses certain level, the bonuses is around 54,000 RMB, which is exactly predicted by the tax minimization problem. Overall, the actual distribution of bonuses is a right-shift of the theoretical prediction. This is reasonable because the theoretical prediction is made by assuming there is only standard deduction, while in fact, the existence of other deductions and tax-exempt incomes would make the actual distribution shift right.

Of course, there are realistic concerns that actual bonuses may not follow the optimal distribution predicted in equation 1.7. For one, obviously, the tax minimization problem is quite complicated to solve. It might be hard to imagine that employers, even with the help of accounting staffs, make such complicated calculations when deciding how much wages and bonuses to pay. For another, in reality, people (even employers) may not be able to fully anticipate the annual earnings for each employee and make optimal arragement from the very beginning. It is possible that bonuses are a function of random shock that cannot be fully anticipated when the first several monthly wages are paid. Due to these concerns, it is possible to see the actual tax liability and amount of bonuses differ from the optimal one, for a given annual earnings.

Figure 1.19 does show some evidence for imperfect optimization, consistent with the above concerns. First, we do see non-zero bonuses paid for bottom levels of incomes, consistent with the hypothesis that annual earnings is not perfectly anticipated at the very beginning. Second, the actual distribution of bonuses does not follow the discrete distribution as predicted by equation 1.7. In particular, it seems to "smooth out" the highly nonlinear part in the optimal distribution (corresponding to earnings between 42,000 RMB and 125,550 RMB). There is no evidence for a flat part of bonuses at 18,000 RMB, as predicted by equation 1.7. But there is clear evidence for a flat part of bonuses at 54,000 RMB. How to reconcile these? The lack of evidence for the part of non-linear distribution of bonuses is probably due to the inability of making perfect solution of the tax minimization problem. In addition, the income range corresponding to the optimal bonuses at 18,000 RMB is much narrower than that corresponding to 54,000 RMB, suggesting the benefit of making correct optimization is much smaller in the former case than in the latter case. This explains the lack of a flat part of bonuses at 18,000 RMB and the existence of a flat part of bonuses at 54,000 RMB.

To sum up, in this section, we explore the theoretically optimal bonuses policy for a given annual earnings, and find that the actual distribution of bonuses follows the theoretical distribution in a smoother way. The places where the actual distribution of bonuses differ from the theoretically optimal distribution imply an imperfect optimization mode, consistent with the complicated and seperate tax schedules for bonuses and regular wage. However, our data seem to have too few incomes taxed as bonuses to make it an important concern for our main analysis. Thus, we do not account for the potential complexity brought about by bonuses in the main text.

1.6.3 Bunching evidence of full sample and decimal sample

Bunching evidence for full sample

Figure 1.12 displays evidence of bunching for full sample (including both round and decimal values) of taxable wage/salary income. There is clear evidence of excess bunching at kinks, in both pre-reform sample (2009.6-2011.8) and post-reform sample (2011.9-2013.12). There is also clear bunching in some non-kink points (e.g. 1,000, 4,000, 6,000 in pre-reform sample and 1,500, 3,500 in post-reform sample). This is because employers may have a tendency to report taxable incomes at "rounder" numbers ending in hundreds, thousands etc. Bunching at these places create a problem for us to reveal the "true" bunching scale caused by kinks. In previous literature, usually observed is a consistently smaller regular bunching pattern at non-kink rounder numbers (Kleven and Waseem (2013) Devereux et al. (2014) Best and Kleven (2016)). Then the relative bunching scale at kinks compared to that at non-kink rounder numbers would nicely reveal the bunching ETI estimate. But in our data, bunching patterns at non-kink rounder numbers are so irregular that we cannot use them as a reference to reveal the bunching scale caused by kinks. For example, for the pre-reform kink at 5,000 RMB, if we (reasonably) use bunching at 4,000 RMB and 6,000 RMB as reference to predict the counterfactual income distribution at 5,000 RMB, then it implies a close to zero ETI at kink 5,000 RMB. But one might argue why not use bunching at 4,500 and 5,500 as reference instead. These bunching patterns in our data imply that the standard bunching approach (even incorporating the "rounder"-number bunching as did by Kleven and Waseem (2013)) does not promise an ideal way to obtain the counterfactual taxable income distribution and thus cannot generate an ideal estimation of ETI using full sample.

In addition, the full sample bunching evidence also shows some unusual excess bunching points (e.g. taxable incomes of 3,637.5 RMB, 8,170 RMB, 9,069 RMB in the post-reform sample) that are not multiples of the salient round numbers (e.g. 50, 100, 500, 5000). Bunching at these taxable incomes do not receive any reasonable theoretical support. A careful examination of these unusual bunching points reveals that these unusual bunching points probably result from a one-shot wage release by certain employers.[63] In our bunching analysis, we need to exclude these outliers.

To address the above problems, we are motivated to seek for evidence of bunching at places that are systematically unaffected by such problem. Fortunately, when we focus on the decimal sample (i.e. those with decimal taxable incomes), these irregular bunching patterns are exlcuded.

Bunching evidence for decimal sample

Figure 1.13 shows bunching evidence for the decimal sample. It is clear that during pre-reform period, there is no bunching at all in kinks of 500 RMB, 2,000 RMB, and 5,000 RMB. There is clear bunching at 20,000 RMB. Bunching at higher kinks is also clear, although observations are too few to render precise estimates. For the post-reform sample, there is slight bunching evidence at 1,500 RMB and 4,500 RMB. Bunching is clear at 9,000 RMB and 35,000 RMB. Density at higher kinks is too noisy to render clear bunching evidence. Due to these graphical evidence, we focus on pre-reform kink at 20,000 RMB and post-reform kinks at 9,000 RMB and 35,000 RMB.

The lack of bunching at bottom TI kinks in the decimal sample is consistent with the comparable bunching scale at these kinks to neighboring non-kink places in the full sample (e.g. compare bunching at 5,000 RMB with that at 4,000 RMB and 6,000 RMB). And the bunching pattern at higher kinks in the decimal sample are similar to that in the full sample. This suggests restricting to a decimal sample would well excludes the irregular bunching at non-kink places while reveal desired bunching pattern sat kinks.

[63] For example, almost all taxable incomes of 3,637.5 (8,170) RMB in the post-reform sample is associated with pre-tax wage income of 7,137.5 (12,070) RMB, all of which are reported in May, 2012 (December, 2011).

1.6.4 Placebo bunching evidence: comparing bunchings before and after the tax reform

In this section, we use pre-reform and post-reform samples to examine bunching at the new kinks and old kinks. We show that there was no bunching at the newly imposed kinks before the tax reform. We also show that after an old kink was abolished, bunching disappeared within a short time.

Bunching of taxable income adjusted by standard deduction

In 2011 tax reform, not only the MTR brackets of taxable income changed, the standard deduction for wage incomes also changed. Thus, the post-reform bunching at kinks may correspond to different levels of pre-deduction wage incomes from the pre-reform ones. To account for this, we conduct a bunching analysis focusing on taxable incomes adjusted by the change of standard deduction. Note that standard deduction is 2,000 RMB before the reform and 3,500 RMB after the reform. Then for pre-reform observations, we deduct 1,500 RMB from their taxable income to render predicted post-reform taxable income. Similarly, for post-reform observations, we add 1,500 RMB to their taxable income to render predicted pre-reform taxable income. Such approach would hold other deductions and exempt incomes fixed, which were not affected by the reform, and would arguably render comparable predicted taxable income for pre-reform and post-reform samples.

We still consider pre-reform kink at 20,000 RMB and post-reform kinks at 9,000 RMB and 35,000 RMB and examine bunching at these kinks within one year before (2010.9-2011.8) and after (2011.9-2012.8) the tax reform. As figure 1.14 shows, there was no bunching evidence at pre-reform kink 20,000 RMB after the reform and no bunching evidence at the post-reform kinks 9,000 RMB and 35,000 RMB before the tax reform, thus providing a placebo test for the bunching at kink approach.

Bunching of unadjusted taxable income

As another placebo test, in figure 1.15, we examine whether bunching of *unadjusted* taxable income (1) was absent at post-reform kinks before the kinks were imposed and (2) whether it disappeared at the pre-reform kink after the kink was abolished. As expected, within one year prior to the reform, there was no evidence of bunching at 9,000 RMB (panel (c)) and 35,000 RMB (panel (d)). For the pre-reform kink 20,000 RMB (panel (a)), however, within one year after the kink was abolished, there was still clear evidence for excess bunching. This is quite unexpected because these excess bunchers should not be the same persons bunching at the same kink before the tax reform, due to the standard deduction change. Moreover, the individuals with post-reform taxable income around 20,000 RMB should have pre-reform taxable income around 21,500 RMB, assuming their raw income did not change and other exempt incomes and deductions fixed. Since 21,500 RMB is not a kink point before the tax reform, the excess bunching of taxable income at 20,000 RMB cannot be due to pre-reform bunching at kink. Is it possible that these excess bunchers were due to inattention of kink point change? If this is true, we expect to see the excess bunching persisted within a short time after the tax reform and later disappeared. However, by dividing the one year after the tax reform into two half-year periods (panel (b)), we see that within half year after the tax reform there was no bunching at the old kink, while the excess bunching emerged during the second half year after the reform. A further examination reveals that the excess bunching happened only in 2012.3. Thus we interpret it as a coincidence (possibly a firm issuing similar amount of wage income in that month) rather than systematic inattention of kink point change.

Adjustments of bunching behavior

How fast did people respond to the tax reform? To explore this, we cluster 3-month data because observations would be too scarce at these middle-high income levels if we examine each month separately.[64] Figure 1.16 panel (a) shows that bunching at pre-reform 20,000 RMB kink disappeared immediately after the tax reform. There is no bunching at all within 3 months after the tax reform, which suggests that the excess bunchers were well informed of the kink information both before and after the reform. While panel (b) shows fuzzy evidence on how bunching increased after 9,000 RMB taxable income became the new kink, panel (c) shows clearly that bunching increased over time at 35,000 RMB kink, suggesting it takes time to adjust wage income due to initial inattention and adjustment costs. The 2011.12-2012.2 period has more observations probably due to release of bonus at the end of a year (December).

1.6.5 Uncover structural elasticity using Chetty's bound approach

While we estimate the underlying elasticity and optimization friction using a structural model, Chetty (2012) takes an alternative approach. He bounds structural parameters without identifying how frictions affect behavior. The motivation for using a bound approach is that we cannot always ensure that the parametric specification we adopt is correct. The bound approach thus allows us to estimate the scope of the underlying elasticity with much weaker assumptions. The cost is that we in general need large tax reforms to obtain tight bounds, which is not well satisfied in our case.

We simply take his approach to estimate the bounds and compare with our main structural estimates. The closed form presentation for bounds on the structural elasticity of taxable income ε, as obtained in Chetty (2012), is $\varepsilon_L = \widehat{e} + \frac{4\delta}{(\triangle log(1-\tau))^2}(1-\rho)$ and $\varepsilon_U = \widehat{e} + \frac{4\delta}{(\triangle log(1-\tau))^2}(1+\rho)$, where $\widehat{e}$ is the observed elasticity, δ is the degree of friction, τ is marginal tax rate, $\rho = (1+\frac{1}{2}\frac{\widehat{e}}{\delta}(\triangle log(1-\tau))^2)^{1/2}$. Empirically, δ is measured by the utility cost of ignoring tax change as a fraction of net earnings. Chetty (2012) find that $\delta = 1\%$ may be a reasonable estimate for most cases and we also follow such practice. When we

[64] Best and Kleven (2016) in another setting use monthly figures and show sharper evidence on how bunching in housing market changes in response to stamp duty tax changes.

have multiple (say J) observed elasticities, we can also obtain the greatest lower bound $\varepsilon_L^{max} = max(\varepsilon_L^j)$ and the least upper bound $\varepsilon_U^{min} = max(\varepsilon_U^j)$.

Table 1.9 shows very wide bounds for the underlying elasticity. The unified bounds are $\varepsilon_L^{max} = 0.01$ and $\varepsilon_U^{min} = 17.62$. This is not surprising, since Chetty (2012) shows that with small marginal tax rate changes, in general very wide bounds for intensive margin elasticities would be obtained. Thus, in our case, the bound estimates are not very informative and we would stick to the underlying elasticity estimated using the parametric approach. Of course, the underlying elasticity estimated using the parametric approach lies in the above bounds.

1.6.6 Tax reform ETI vs. bunching ETI: full sample estimates

Our main bunching ETI estimates shown in figure 1.6 are based on the decimal sample. There may be concern that the decimal sample could underestimate the ETI since it might exclude taxable incomes adjusted to exactly at the kink more than those with other integer values. We address this concern here by showing that using full sample would generate similar dynamic pattern of bunching estimates.

First, using full sample, we apply the revised bunching approach to estimate the global bunching ETI using bunching at kinks 9,000 RMB and 35,000 RMB, as they are the kinks used for estimating the global bunching ETI in figure 1.6. Since we see no bunching at these two kinks before the tax reform, it is not necessary to use the pre-reform taxable income distribution at these two kinks to construct counterfactual density distribution.[65] Figure 1.17 shows that the global bunching ETI estimates using full sample are very similar to those using the decimal sample.

Second, we do observe bunching increases at kinks 1,500 RMB and 4,500 RMB in post-reform compared to pre-reform periods. Since there is pre-reform bunching at these two

[65] It is even undesirable to do so because the monthly (or three-month pooled) taxable income distribution around these kinks are volatile across periods due to not so many observations in these middle-high kinks. Using pre-reform taxable income desity as counterfactual distribution for post-reform sample generates unreasonable estimates for these middle-high taxable income kinks.

kinks, we need to use it to construct the counterfactual density for post-reform sample.[66] For each kink, we first obtain the fraction of observations bunching at the kink in a given neighborhood around the kink in pre-reform period. Then we use this fraction to adjust the observed bunching at each post-reform period. Then we apply the standard bunching approach to obtain the an ETI for each kink in each post-reform period. Finally, using the observed ETI at all post-reform kinks 1,500 RMB, 4,500 RMB, 9,000 RMB, and 35,000 RMB (we do not estimate ETI for top kinks due to too few observations), we estimate the global bunching ETI using the above approach. Figure 1.17 shows that the dynamic global bunching ETI estimates are much more precisely estimated using full sample. They are smaller than the global ETI estimated only using middle-high kins. Most importantly, we again find that the dynamic global bunching ETI estimates are stable over time.

1.6.7 Real responses versus income manipulations captured by tax reform and bunching ETI estimates

Traditionally, the behavioral responses to tax changes are usually divided into real responses (e.g. working hours, effort) and various income manipulations (e.g. income shifting, income underreporting). It is beyond the scope of this paper to exhaustively decompose all behavioral responses accounting for each ETI estimate. A comprehensive decomposition is also very difficult, if not impossible, as well realized in literature (e.g. Feldstein (1999)). In spite of this, revealing potential evidence on real responses versus income manipulations captured by each approach is useful because income manipulations and real responses in labor supply have different normative implications (Slemrod and Yitzhaki (2002), Chetty (2009)). It is argued that only real responses are relevant for optimal taxation, and that a good tax system should be devised to minimize the space for income manipulations. Thus if there is no evidence for real responses, then all the behavioral responses captured by the ETI

[66] Since there are large amounts of observations at these two kinks in each period, the density distribution is rather stable across period and using pre-reform distribution to construct the post-reform counterfactual density is reasonable.

estimates are simply a reporting behavior, implying small efficiency costs brought about by "real" distortions; at the same time, the large ETI estimates would also imply that the tax system has too many loops for income manipulations and should be amended correspondingly.

Tax reform ETI.

Although we do not provide direct evidence of income manipulations using our data, literature contains much evidence that income manipulation can be an important factor for the tax reform ETI estimates. An extreme case is Kreiner et al. (2016), who find that the intertemporal wage income shifting can account for almost all of the ETI estimate. Income manipulations in response to tax changes could take other forms, e.g. shifting income to bases with a lower tax rate.[67] There is also anecdotal evidence, via communication with Chinese employees, that firms sometimes pay cash as part of compensation, probably out of tax evasion purpose.

To explore potential real responses to tax, we resort to survey data and examine the response of working hours. In particular, we use CFPS (China Family Panel Studies) data in 2010 and 2012 as they are the most appropriate data for our research purpose.[68] We divide individuals into three groups due to their 2010 pre-tax monthly wage income. People with wages lower than 2,000 RMB face zero tax rate before and after the tax reform. If we only consider standard deduction, for simplicity, then people with wages in 2,000 RMB-8,000 RMB interval largely correspond to those facing MTR decreases, people with monthly wages larger than 8,000 RMB largely correspond to those facing MTR increases.[69]

Figure 1.20 shows that for the bottom earners who face no MTR change, working hours on average do not change. For people with wages in 2,000 RMB-8,000 RMB interval,

[67] See Gordon and Slemrod (2000) for evidence of such reclassification of income in the US.

[68] We do not use 2014 data to explore longer-run response because the working hours information in 2014 is not directly comparable to that in 2010 and 2012 data. See more details on CFPS data at http://www.isss.edu.cn/cfps/EN/.

[69] In particular, with only standard deduction, wages in 12,500-22,000 RMB interval and 38,500-42,000 RMB inverval face an MTR increase (see figure 1.11). Given potentially high volatility of top earnings, it is not unreasonable to assume most of these top earners face an expectation of increasing MTR.

there is evidence that many of them have their working hours increase after the tax reform. But there are also some people having working hours decrease or having no change. For those plausibly facing MTR increases, there is clear evidence that their working hours decrease. Admittedly, self-reported wages and monthly working hours are likely to suffer from measurement errors, figure 1.20 thus shows suggestive evidence on real responses in working hours. Overall, the working hours change for those facing MTR decreases are mixed and statistically indistinguishable from zero, while that for those facing MTR increases is unambiguous. It is reasonable that higher wage earners are more likely to adjust working hours in response to potential MTR changes, since they are more likely to have the ability or bargaining power to adjust working hours.

Bunching ETI.

By contrast to the tax reform ETI, it is more unclear to what extent the bunching ETI captures income manipulation versus real responses. First of all, there is some evidence showing that income manipulation is an important factor behind bunching. For example, Le Maire and Schjerning (2013) derive a dynamic extension to the bunching method, and show that over half of the bunching effect among Danish entrepreneurs is due to intertemporal income-shifting. However, finding clear evidence for bunching ETI reflecting real responses is much harder. Until now, there has been no paper documenting such evidence.[70] Although CFPS survey data provide information on working hours, and we have used it to provide evidence for real responses captured by the tax reform ETI, the self-reported wage is not accurate enough to examine potential real responses around the kinks. Furthermore, the number of observations in the CFPS data are too few to apply the bunching approach. Our data do not allow us to distinguish real responses and income manipulation captured by

[70] Chetty et al. (2011) pp. 781-782 try to explore the extent to which income shifting versus labor supply are responsible for the bunching around the top kink. They find no evidence that pension shifting is responsible for a large fraction of the bunching in taxable income at the top kink. But they note that there may be other income shifting responses that they are not able to examine due to the data limitation. Relatedly, Harju and Matikka (2016) estimate real responses and income-shifting responses for the owners of privately held corporations in Finland and find that income-shifting accounts for a majority of overall ETI.

the bunching ETI. But future research could make progress on this by connecting precisely measured income to working hours.

1.6.8 Welfare implications

Deadweight loss of China's wage income tax

Total deadweight loss.

We follow the formula in Feldstein (1999) to calculate the deadweight loss (DWL) of the personal wage income tax:[71]

$$DWL = 0.5\frac{\tau^2}{1-\tau}e \cdot TI.$$

Here e is the estimated ETI. In practice, DWL is calculated as

$$DWL = 0.5e\sum_i\sum_t\frac{\tau_{it}^2}{1-\tau_{it}} \cdot TI_{it}.$$

Then we can obtain the DWL as a percentage of the total tax revenue $\frac{DWL}{TR}$, which measures how much money metric welfare loss is caused by per unit tax revenue levied. Note that the DWL is proportional to e. Since our main results imply that the ETI estimates obtained from the tax reform approach are relevant for policy making, we use them for welfare calculations. Consider two cases, $e = 2.423$, the ETI estimated using the standard tax reform approach, and $e = 4$, the long-run ETI suggested by the revised tax reform approach. As table 1.12 shows, under current tax schedule (use 2013 data), when $e = 2.423$, $\frac{DWL}{TR}$ is 61%, which measures the 1-year average efficiency cost, when $e = 4$, $\frac{DWL}{TR}$ is 101%, which measures the long-run efficiency cost. Overall, this suggests a large efficiency loss of the current personal wage income tax. As a comparison, Feldstein (1999) calculates $\frac{DWL}{TR}$=32.2% for US personal income tax system in 1994, where his ETI estimate is 1.04.

[71] When there are transfer costs associated with sheltering (tax evasion or avoidance), then elasticity of earnings and resource cost of sheltering are needed to estimate the deadweight loss (Chetty (2009), Gorodnichenko et al. (2009)). Yet we do not have enough data to estimate these parameters. Therefore, we simply follow the standard formula by Feldstein (1999) to form a comparison with his estimates.

A 10% decrease in all marginal tax rates.

Given such high ETI estimates, is it possible to improve welfare by reducing the MTR so as to reduce the deadweight loss and also increase the tax revenue as the logic of the Laffer curve implies?[72] To explore this, we take an exercise by assuming an MTR decrease by 10% for all people, following the spirit of Feldstein (1999). As table 1.12 shows, if $e = 2.423$, a 10% decrease in all MTRs would induce the total tax revenue to slightly increase by 0.7% ((185.59-184.22)/184.22), and the deadweight loss drops by 14.3% ((96.27-112.38)/112.38). In the long run ($e = 4$), a 10% decrease in MTRs would result in a greater revenue gain of 7.7% ((198.48-184.22)/184.22) plus a smaller decrease in the DWL of 9.1% ((168.72-185.52)/185.52). Thus, an important implication from our ETI estimates is that a further MTR decrease would be unambiguously desirable from both revenue and efficiency concerns, given the large ETI estimates in China.

Efficiency effect of the 2011 tax reform

The large magnitude of ETI estimates obtained in China suggests large efficiency cost of current personal income tax system. But what is the overall efficiency effect caused by the 2011 PIT reform? While some people experience MTR decreases, some others experience MTR increases due to the tax reform. Therefore, the overall efficiency effect induced by the tax reform is not obvious. Fortunately, it suffices to examine the revenue change due to behavioral responses in order to measure the overall efficiency effect.[73] The total revenue change due to the tax reform can be decomposed into two parts, i.e. $\triangle TR = \triangle TR|_{mechanical} + \triangle TR|_{behavioral}$, where $\triangle TR|_{mechanical}$ denotes the mechanical tax revenue change due to changes in tax code (i.e. changes in standard deduction and tax rates) while holding individual behavior unchanged, and $\triangle TR|_{behavioral}$ denotes the tax revenue change due to all potential behavioral responses, such as responses in labor supply, tax evasion, or tax avoidance. Figure 1.21

[72] Note that since we do not have a linear tax schedule, the well-known result of revenue maximizing elasticity of taxable income w.r.t. the MTR equals 1 does not hold.

[73] See Auerbach (1985), Slemrod (1998), or equation (33) of Chetty (2009).

illustrates how we estimate $\triangle TR|_{behavioral}$. Panel (a) shows the evolution of monthly wage income tax revenue TR_t. The change in TR_t around the tax reform measures the total revenue effect $\triangle TR$. TR_t^{pred} denotes the predicted monthly total tax revenue assuming all observations were subject to the post-reform tax schedule. Before the tax reform, TR_t^{pred} were below TR_t due to a higher post-reform standard deduction and adjusted marginal tax rates. After the reform, the two figures simply coincide.[74] Then the difference between TR_t^{pred} and TR_t before the tax reform measures the mechanical revenue effect $\triangle TR|_{mechanical}$. Therefore, the change in TR_t^{pred} around the reform measures the revenue effect due to behavioral response $\triangle TR|_{behavioral}$.

To estimate the efficiency effect, we use the following specification:

$$TR_t^{pred} = \alpha + \mathbf{1}[t \geq c] \cdot (g_l(t-c) + \lambda) + \mathbf{1}[t < c] \cdot g_r(c-t) + \varepsilon_t, \tag{1.8}$$

where c indicates the tax reform time point (September, 2011) (e.g. for October, 2011, $t-c$=1), g_l and g_r are polynomial functions in left and right hand sides of the tax reform time, α measures the left limit level of y_t around the cutoff c, λ measures the effect of the reform on the outcome variable. In practice, we adopt a linear polynomial since it fits the data pattern well. But using a higher degree polynomial does not affect our conclusion. Panel (b) of figure 1.21 shows that the overall efficiency effect is a positive 4.06 million RMB, though statistically insignificant (with a s.d. of 5.53 million RMB and a p-value of 0.47). This implies that despite large marginal efficiency cost as implied by the ETI estimates, the overall efficiency effect of the 2011 tax reform is close to zero, suggesting the efficiency gains from those experiencing MTR decreases (the large amount of lower earners) largely offset the efficiency losses from those experiencing MTR increases (relatively few high earners). Even if λ only measures the immediate aggregate efficiency effect of the reform, the almost identical slopes before and after the reform additionally imply a close to zero long run aggregate

[74]The seasonal spikes in Decembers are possibly due to bonuses release at the end of a year.

efficiency effect. Although redistribution (reducing tax for the poor and increasing tax for the rich) is likely be the main concern for the 2011 tax reform design, the tax reform, somewhat unexpectly, reached an effect of no aggregate efficiency effect. Usually a tax reform is designed to be revenue neutral to keep fiscal balance. But China's 2011 personal income tax reform seems to have an efficiency-neutral property at the cost of an immediate revenue loss (clear from panel (a) of figure 1.21).

Table 1.6. Personal income tax on different income components

Item	Description	Tax rate		Standard deduction	
		before reform	after reform	before reform	after reform
1	wages/salaries	9-tier rates	7-tier rates	2000 RMB	3500 RMB
2	income from production or business operation by self-employed industrial and commercial households	5-tier rates	5-tier rates	2000 RMB monthly/ 24000 RMB annually	3500 RMB monthly/ 42000 RMB annually
3	income from the contract and operation of enterprises and institutions and the business income of the lease				
4	remuneration for providing services	20%	20%	4-6: each time when income is received, if income does not exceed 4000, deduct 800, otherwise deduct 20% of the income	
5	author's remuneration	20%	20%		
6	royalties income	20%	20%		
7	interest, dividend, and bonuses	20%	20%	No	No
8	income from leasing property	20%	20%	same as 4-6	
9	property transfer income	20%	20%	original value and reasonable fees	
10	accidental income	20%	20%	No	No
11	other incomes	20%	20%	No	No

Notes: 1. In general, item 1 is taxed monthly, item 2 is taxed yearly, other items are taxed each time the income is received.
2. We call item 2 and 3 self-employment income for simplicity.
3. For item 2, costs, fees, and losses are deductible; for item 3, necessary fees are deductible.
4. Item 4 applies punising rates for excessively high income.
5. Item 5 applies a further 30% exemption of the tax liability.

Table 1.7. Tax revenues from personal income components

Item		Our sample city (2009.6-2013.12)			China overall (2013)	
		obs	aggregate (RMB)	percent	aggregate (million RMB)	percent
1	wages/salaries	10978979	2222875452	59.05%	409500.31	62.81%
2	income from production or business operation by self-employed industrial and commercial households	0	0	0.00%	57710.38	8.85%
3	income from the contract and operation of enterprises and institutions and the business income of the lease	0	0	0.00%	12181.31	1.87%
4	remuneration for providing services	293572	151349019	4.02%	17412.64	2.67%
5	author's remuneration	162	167059	0.00%	435.84	0.07%
6	royalties income	21	4063418	0.11%	227.05	0.03%
7	interest, dividend, and bonuses	106681	896736376	23.82%	72564.32	11.13%
8	income from leasing property	7	5180	0.00%	2831.06	0.43%
9	property transfer income	2086	453636162	12.05%	67633.07	10.37%
10	accidental income	9617	22296639	0.59%	7746.65	1.19%
11	other incomes	1661	13006288	0.35%	3734.73	0.57%
	Total	11392786	3764135593	100.00%	651977.36	100.00%

Notes: The 2013 national figures come from China Tax Yearbook 2014.

Table 1.8. Tax schedule of regular monthly wage

pre-reform: standard deduction =2,000 RMB			post-reform: standard deduction =3,500 RMB		
TI	MTR	quick deduction number	TI	MTR	quick deduction number
0-500	5%	0	0-1500	3%	0
500-2000	10%	25	1500-4500	10%	105
2000-5000	15%	125	4500-9000	20%	555
5000-20000	20%	375	9000-35000	25%	1005
20000-40000	25%	1375	35000-55000	30%	2755
40000-60000	30%	3375	55000-80000	35%	5505
60000-80000	35%	6375	>80000	45%	13505
80000-100000	40%	10375			
>100000	45%	15375			

Notes: tax liability is calculated as MTR*TI-quick deduction number.

Table 1.9. Bounds on underlying elasticity of taxable income with degree of frictions $\delta = 1\%$

taxable income kink	$\Delta\log(1-\tau)$	$\hat{e}$	ε_L	ε_U
pre-reform kink 20000 RMB	-0.06	0.10	0.00	19.41
post-reform kink 9000 RMB	-0.06	0.09	0.00	19.39
post-reform kink 35000 RMB	-0.07	0.41	0.01	17.62
		unified bounds:	$\varepsilon_L^{max} = 0.01$	$\varepsilon_U^{min} = 17.62$

Table 1.10. Personal characteristics of bunchers v.s. neighboring non-bunchers

	pre-reform 20000		post-reform 9000 RMB		post-reform 35000	
	bunchers	non-bunchers	bunchers	non-bunchers	bunchers	non-bunchers
position	percent	percent	percent	percent	percent	percent
senior chief	5.95	3.89	2.8	1.83	6.03	4.1
senior deputy	1.44	2.88	2.45	2.17	17.68	4.87
middle chief	9.55	4.42	6.11	4.36	6.98	5.76
middle deputy	2.52	2.79	2.2	2.58	6.3	2.55
general staff	78.74	81.75	79.23	80.2	58.2	75.39
other people	1.8	4.27	7.2	8.87	4.81	7.32
occupation	percent	percent	percent	percent	percent	percent
principal of state organs	0.00	0.09	4.31	5.75	1.49	3.16
principal of institutions	0.18	0.35	0.05	0.09	0.14	0.10
principal of enterprises	5.23	3.37	2.85	2.69	7.66	5.55
scientific researcher	0.00	0.20	0.59	0.60	0.07	0.48
engineer or technician	0.18	1.51	5.93	7.06	9.55	4.90
health professional	0.00	0.00	0.19	0.42	0.00	0.14
business personnel	0.18	0.29	0.31	0.29	0.20	0.27
financial personnel	12.97	17.84	15.23	12.80	32.05	11.97
legal personnel	0.00	0.26	0.93	0.06	3.05	0.02
teaching staff	0.36	0.78	1.33	1.67	0.07	0.21
physical education staff	0.00	0.00	0.00	0.00	0.00	0.00
press/publishing and culture industry	0.00	0.00	0.33	0.06	0.07	0.15
clerk	67.21	55.55	49.97	47.92	31.44	55.70
purchasing agent/salesperson	1.44	0.99	0.80	0.72	0.47	0.83
restaurant/tourism/entertainment service person	0.00	0.00	0.06	0.15	0.00	0.09
agricultural industries operating personnel	0.00	0.00	0.01	0.06	0.00	0.01
production/transport equipment operators	4.86	6.33	4.38	4.82	6.84	6.27
other working person	7.39	12.43	12.75	14.84	6.91	10.15
percent of male	64.26	78.2	73.26	75.25	81.55	82.13
mean (s.d.) of age	36.99	41.08	41.28	40.84	42.73	42.98
	(8.88)	(8.85)	(8.92)	(8.96)	(7.94)	(8.15)
observations	555	3442	8080	52520	1496	20503

Notes: 1. Some examples for positions are as follows: senior chief-chairman, president, general manage, factory director; senior deputy-general manage, factory director; middle chief-department manager, workshop director; middle deputy-department vice manager, workshop vice director; general staff-people with employment relationship; other people-people without employment relationship or temporarily employed.
2. Bunchers are defined as those falling into bunching region associated with each taxable income kink or standard deduction point, while non-bunchers are those outside the bunching region but close to them as shown in the bunching figures. For an individual falling into bunching region in the figure period, we exclude his/her monthly observations that fall into non-bunching region.

Table 1.11. Standard tax reform ETI estimates under kink impacts

Dep. Var.: Δlog(taxable income)	[1]	[2]
Δln(1-τ)	2.423***	2.411***
	(0.188)	(0.192)
Δln(1-τ)*around pre-reform kinks		4.452***
		(0.867)
Δln(1-τ)*around post-reform kinks		-1.356***
		(0.398)
Instrument based on	taxable income in t+6	
Observations	1,210,376	1,210,376
First-stage F-stat for Δln(1-τ)	3647	1231
First-stage F-stat for Δln(1-τ)*around pre-reform kinks		210.9
First-stage F-stat for Δln(1-τ)*around pre-reform kinks		479.5

Notes: See notes for table 1.1.

Table 1.12. Deadweight loss in 2013

	current tax schedule		a 10% decrease in MTR for all people	
	e=2.423	e=4	e=2.423	e=4
DWL (million RMB)	112.38	185.52	96.27	168.72
TR (million RMB)	184.22	184.22	185.59	198.48
DWL/TR	0.61	1.01	0.52	0.85

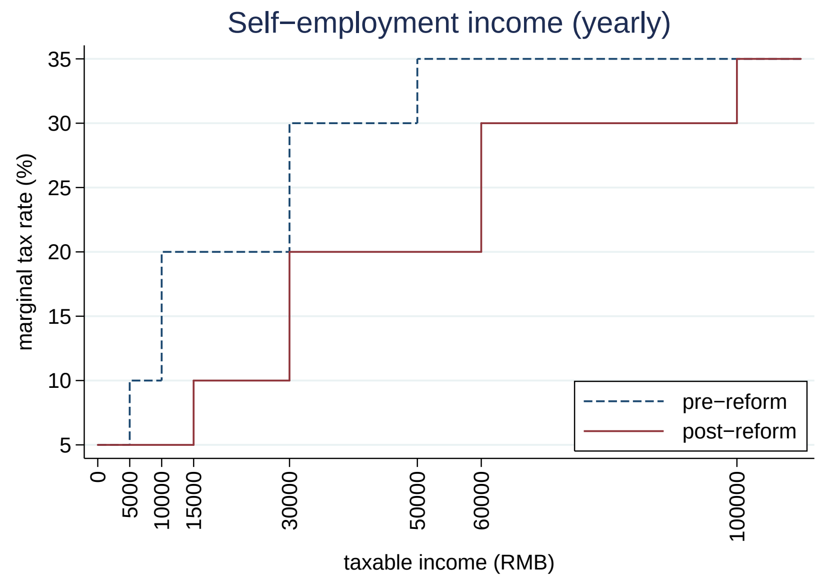


Figure 1.10. Statutory tax schedule on self-employment income
Notes: The statutory tax schedule applies to very few people with self-employment income in practice. Due to the absence of a reliable accounting book, most self-employed businesses pay a pre-determined fixed amount income tax. More details are discussed in Online Appendix 1.6.1.

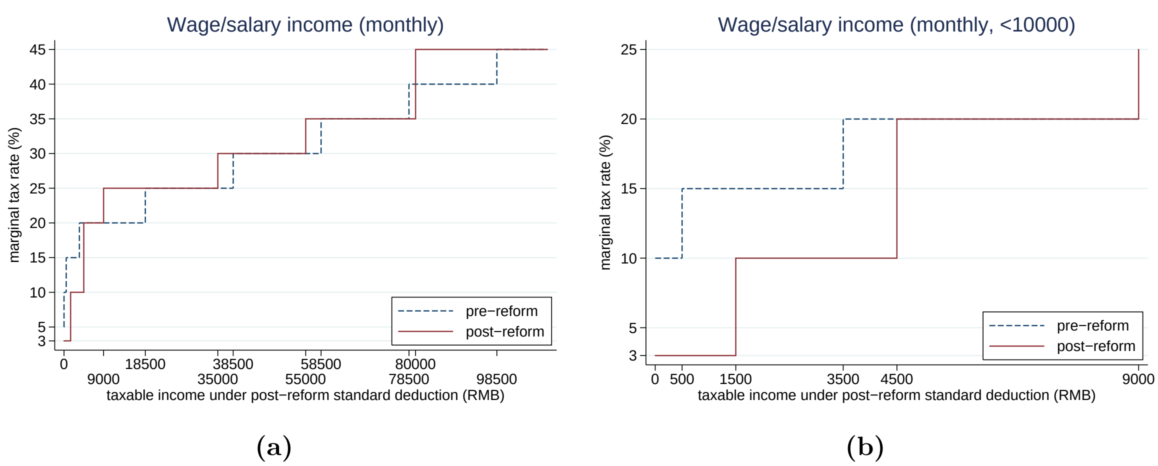


Figure 1.11. Personal income tax schedule under post-reform tax base
Notes: Under post-reform tax base, the post-reform standard deduction 3,500 RMB is adopted for both pre-reform and post-reform tax schedules. The new tax rates took effect on September 1st, 2011.

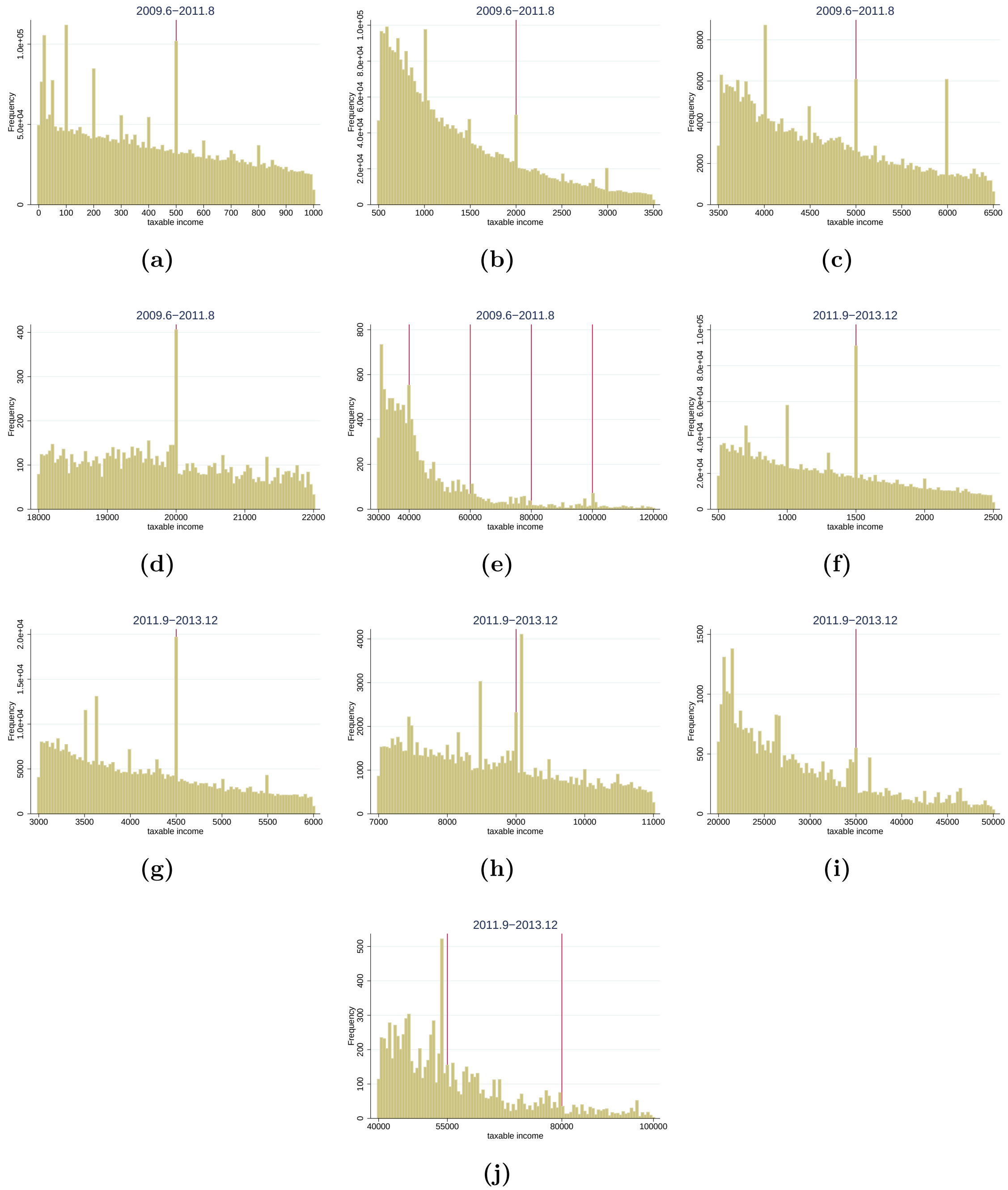


Figure 1.12. Bunching evidence for full sample

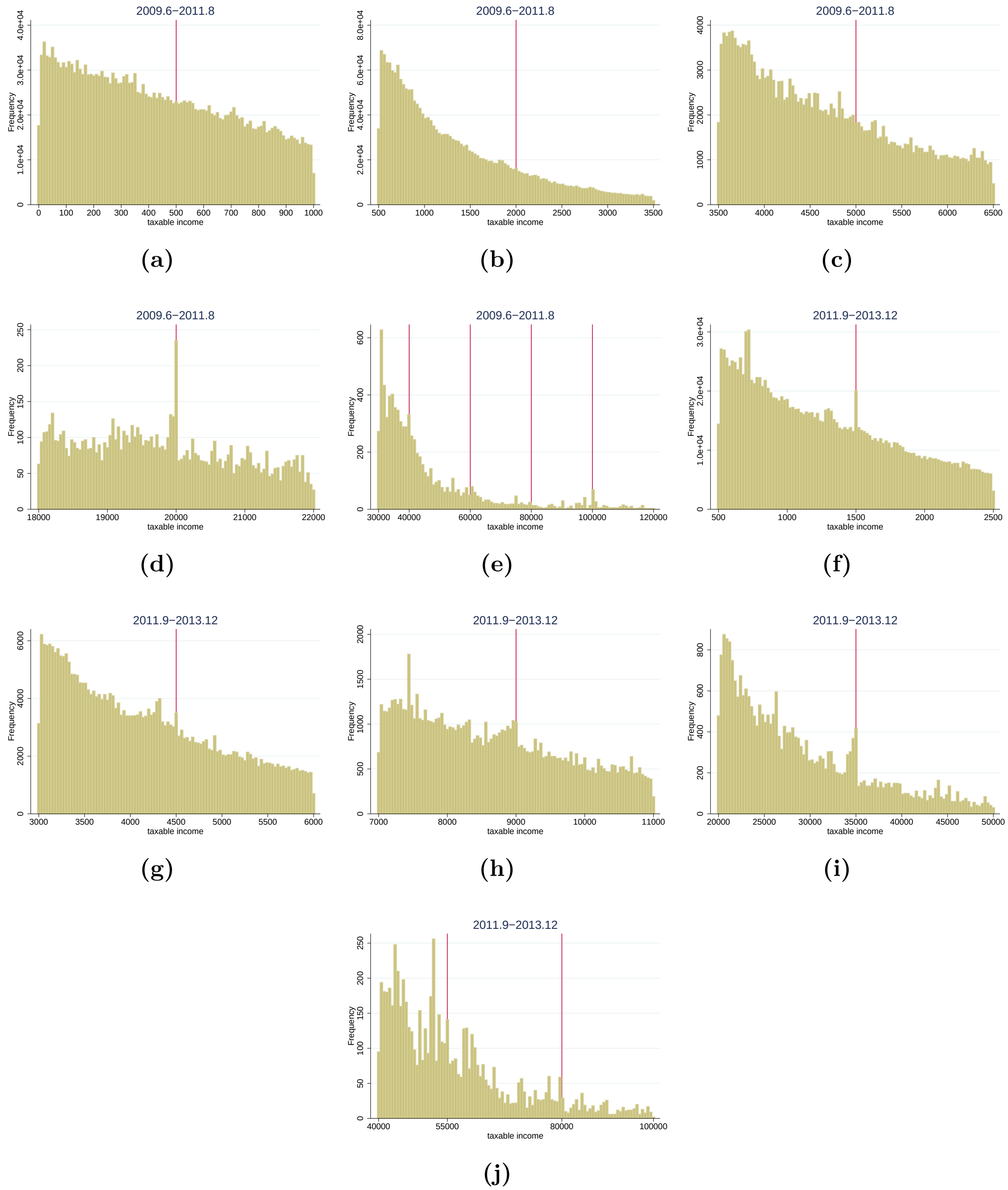
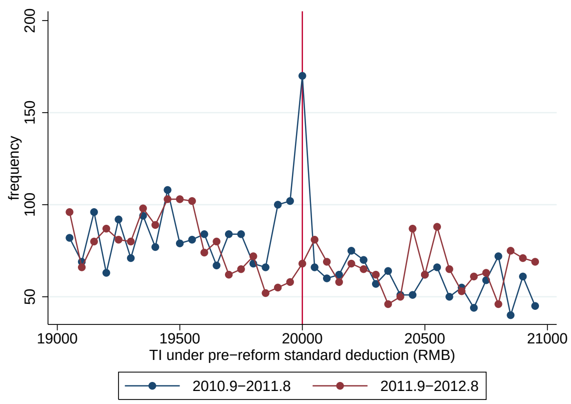
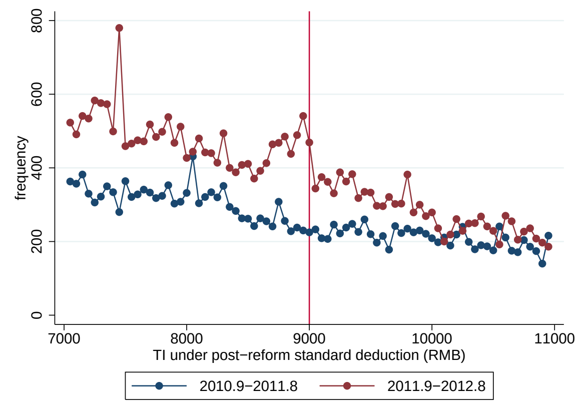


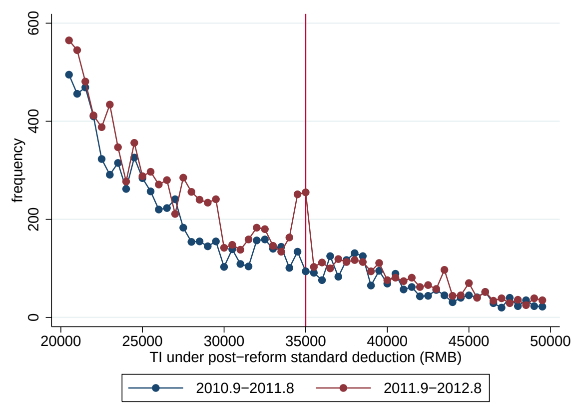
Figure 1.13. Bunching evidence for decimal sample



(a)



(b)



(c)

Figure 1.14. Bunching of taxable income adjusted by standard deduction

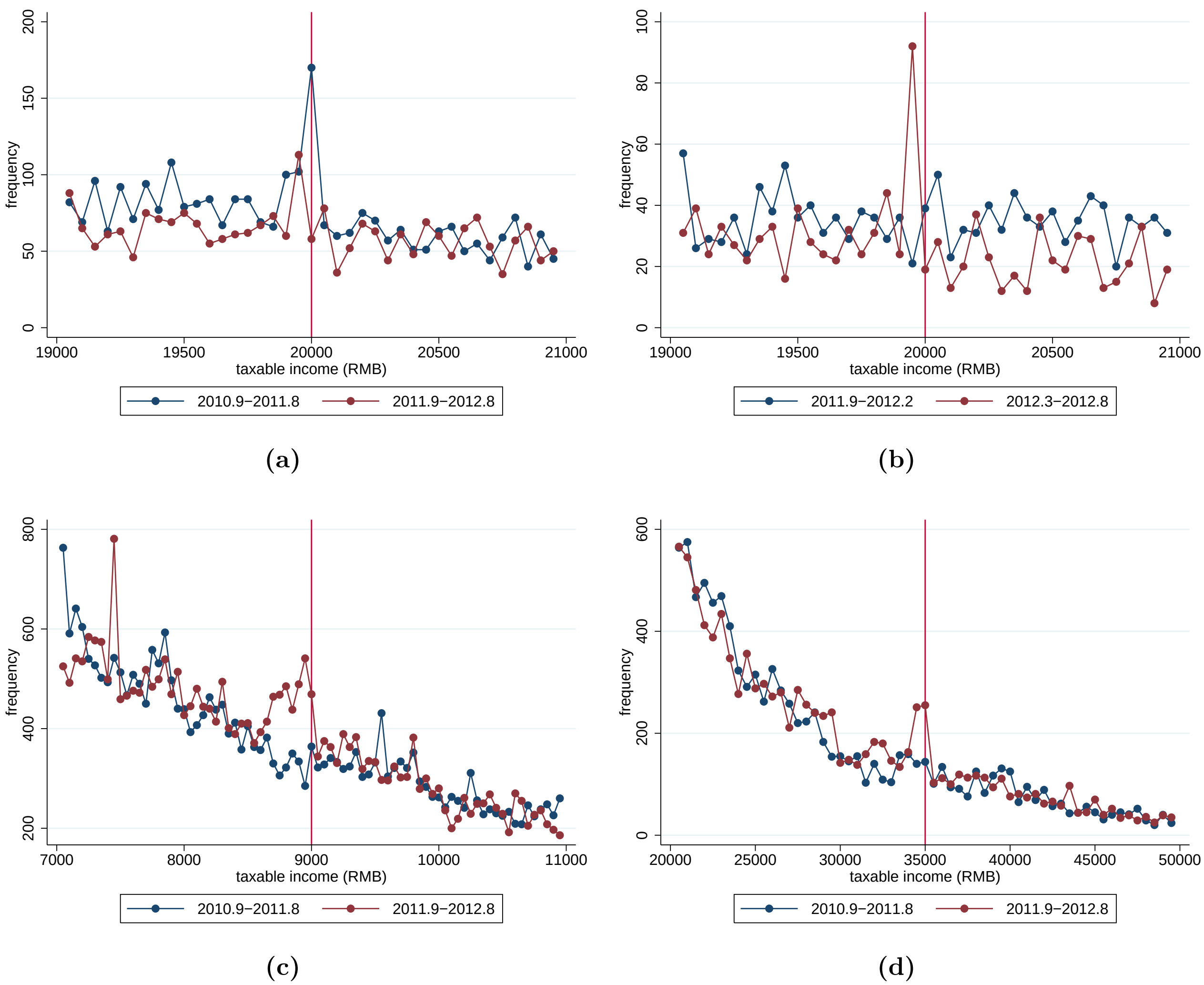
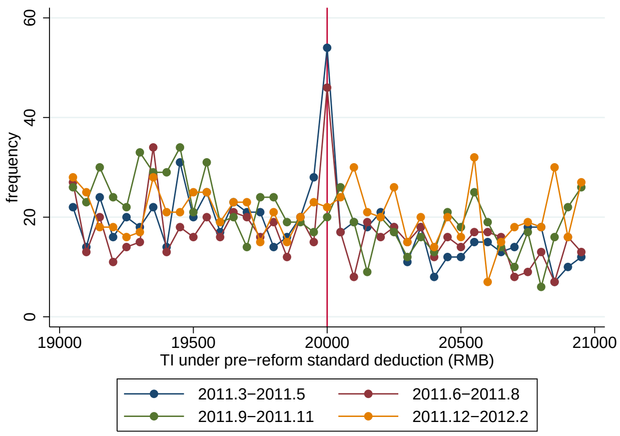
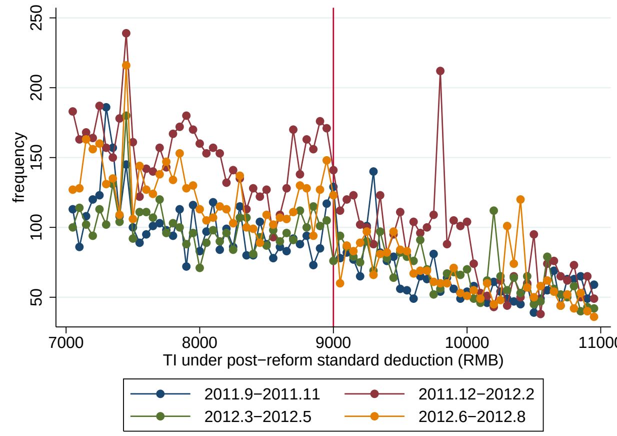


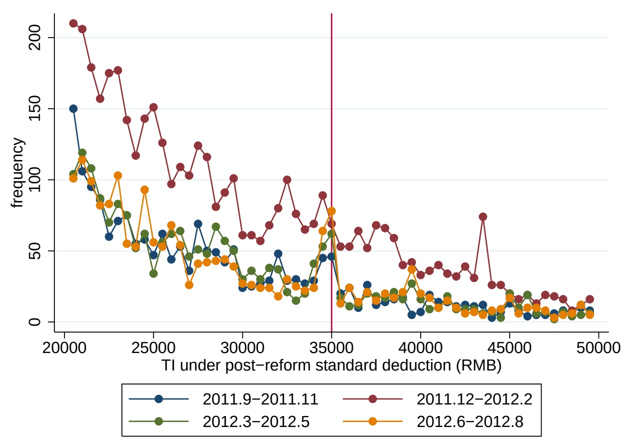
Figure 1.15. Bunching of unadjusted taxable income



(a)



(b)



(c)

Figure 1.16. Adjustments of bunching around tax reform

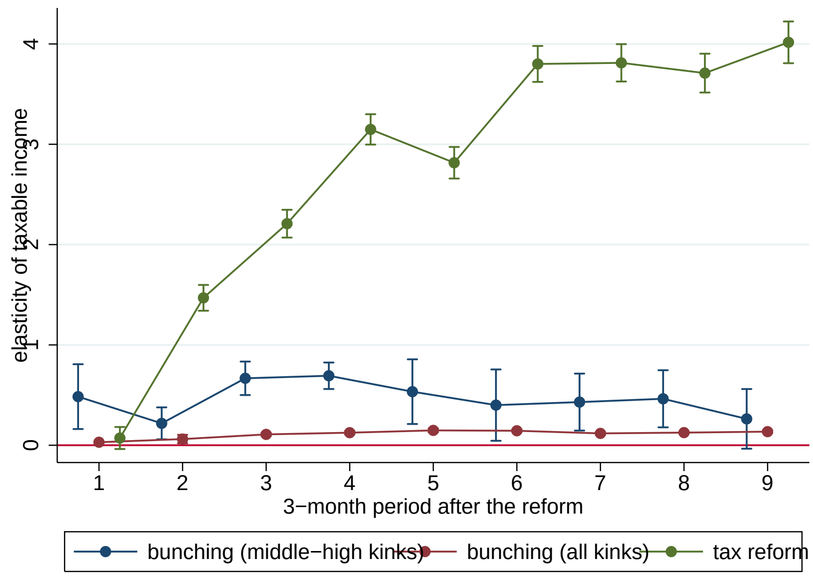


Figure 1.17. Global ETI estimates over time: tax reform approach vs. bunching approach (full sample)
Notes: A 95% CI of the estimates is shown in the figure. The bunching ETI are estimated using full sample.

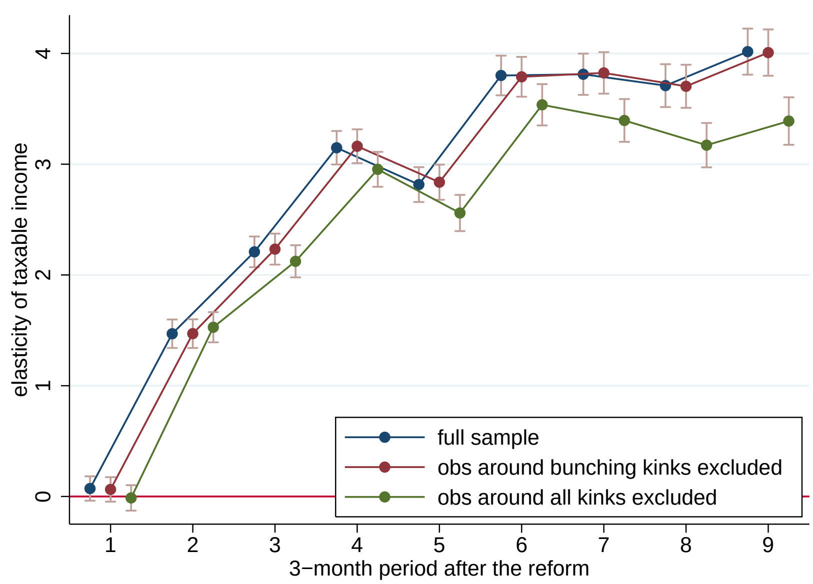


Figure 1.18. Dynamic tax reform ETI estimates: excluding observations around kinks

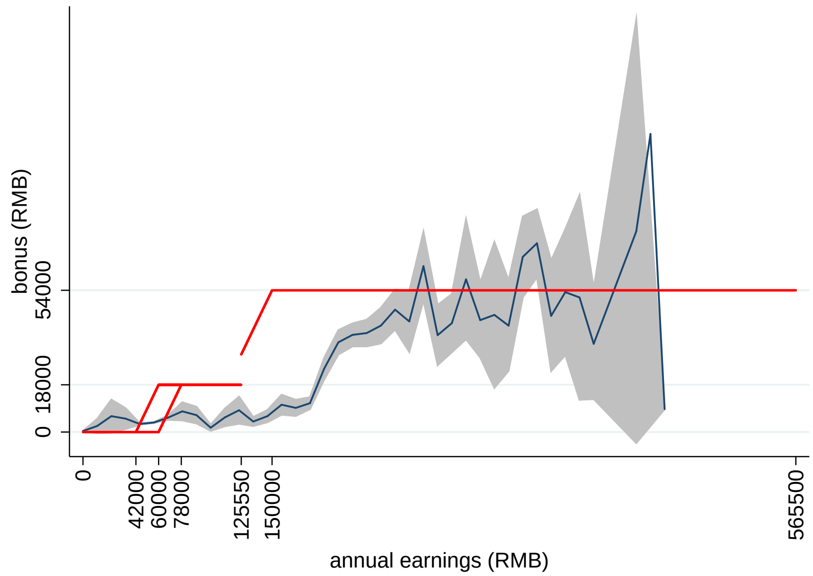
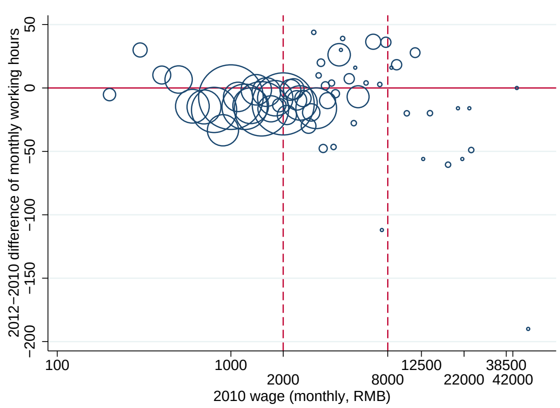
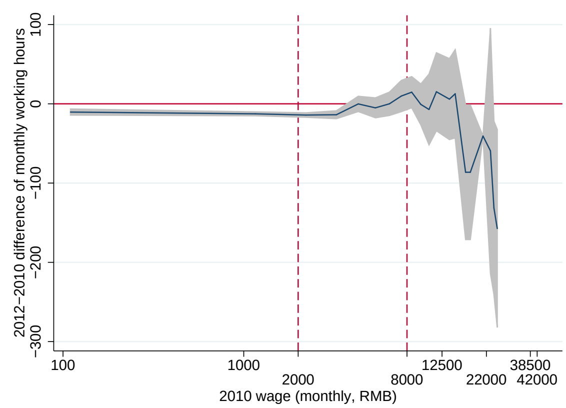


Figure 1.19. Relationship between bonuses and annual earnings in 2013
Notes: Red line indicates the optimal bonuses policy. Blue line and shadow area indicate fitted polynomial relation between bonuses and annual earnings with a 95% CI.

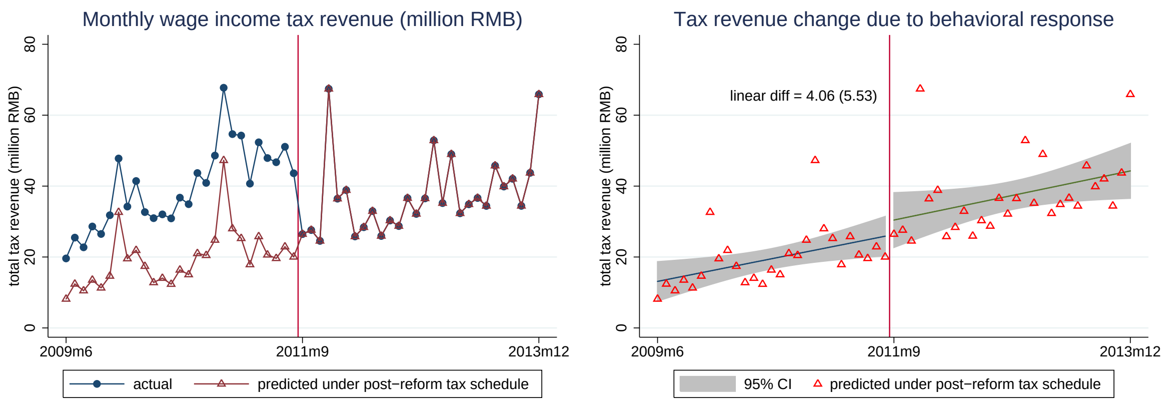


(a) Scatter graph



(b) fitted polynomial

Figure 1.20. Responses in monthly working hours to 2011 tax reform
Notes: This figure is drawn using 2010 and 2012 CFPS (China Family Panel Studies) data. The sample is restricted to wage earners aged 18-60. In panel a, a bubble indicates mean of change in monthly working hours from 2010 to 2012 in a 100 RMB bin, weighted by the number of individuals in a bin. In panel b, a polynomial is fitted with a 95% CI. The x-axis is shown in log scale. Wages lower than 2,000 RMB face zero tax rate before and after the tax reform. Wages in 2,000 RMB-8,000 RMB interval largely correspond to those facing MTR decreases. Wages larger than 8,000 RMB largely correspond to those facing MTR increases.



(a) **(b)**

Figure 1.21. Change in tax revenue around 2011 tax reform

Chapter 2

Tax Evasion and Avoidance among U.S. Households

2.1 Introduction

In many countries like in the U.S., incomes of wage earners are generally honestly reported due to third-party reporting, while a large amount of the self-employed under-report when they self-report incomes. For reasons easy to understand, the scale of evasion is hard to measure directly, let alone the complication added by considering avoidance. The IRS in the U.S. implements tax audit programs, e.g. the Taxpayer Compliance Measurement Program (TCMP) and the National Research Program (NRP), to measure evasion directly. But these data are not publicly available, and have the limitation that they only deal with evasion rather than avoidance.

Previous papers (Pissarides and Weber (1989), Hurst et al. (2014)) estimate the income under-reporting scale of the self-employed using cross-sectional or pooled cross-sectional data. While they have clearly related income under-reporting to tax saving, they have not considered a framework to distinguish two important forms of tax saving – tax evasion and tax avoidance.[1] By contrast, this paper uses the PSID panel data and exploits implications on tax evasion and

[1] Waseem (2015) does some work close to this. He explores a tax reform in Pakistan in 2009, which changed the personal income tax rate faced by sole proprietorships relative to partnerships, to estimate evasion and income shifting to tax-favored business forms separately. His work focuses on non-corporate businesses while I examine both non-corporate and corporate self-employed. Kleven et al. (2011) compare tax evasion and avoidance elasticities under Denmark setting.

avoidance from the evolution of reported income and consumption for families transitioning from employees to self-employed. This paper starts by conceptually distinguishing between evasion and three types of avoidance, and then empirically estimates the scale of evasion and avoidance. Furthermore, we distinguish the self-employed with corporate business from those without, in order to examine their behavioral differences in tax evasion and avoidance.

Before estimating the scales of tax evasion and avoidance, we distinguish carefully between evasion and avoidance conceptually. Tax evasion simply corresponds to income underreporting. Tax avoidance is ways to reduce tax liability for a given reported total income. We consider three types of tax avoidance. The type I avoidance is transferring income across different income forms (e.g. labor income versus capital gains income), due to the difference of marginal tax rates. The type II avoidance is income deferral. When a family owns business, it can choose to defer part of its income realization into the future when the tax rate could be lower, to save tax liability. The type III avoidance is manipulating items subject to exemptions and deductions under the personal income tax. Conditional on total income, the more expenditures subject to deduction a family have, the less would be the tax liability due.

Under the identifying assumption that true after-tax income equals across employees and the self-employed conditional on total consumption and a set of socioeconomic characteristics, we estimate the scale of income under-reporting by comparing the reported incomes of the two groups. But the existence of income deferral would have us overestimate the scale of evasion for early business years and underestimate it for later business period, because the deferred income would be finally realized, in particular when the business is sold. To deal with the complication brought about by the type II avoidance (income deferral), we conduct two tests and find no evidence that income deferral is empirically important in our data. We could thus ignore income deferral and interpret the income under-reporting as evasion. Compared with previous literature (Hurst et al. (2014)) using survey data (in a cross-sectional way) to estimate income under-reporting scale of the self-employed relative to employees, we obtain an estimate much closer to that implied by the IRS tax audit data.

Under the identifying assumption that without any tax avoidance, the tax liability should equal across employees and the self-employed conditional on reported total income, we estimate the scale of avoidance.[2] To estimate type III avoidance, we focus on the tax liability not accounting for dividends and capital gains as lower taxed forms. Then the reduction of such tax liability when a family becomes self-employed, conditional on total reported income, would reflect the scale of type III avoidance. To estimate the type I avoidance, we consider the tax liability considering dividends and capital gains as lower taxed forms, and compare it with the tax liability ignoring the favorable tax treatment of dividends and capital gains. Finally, we estimate the tax evasion scale based on such an assumption: conditional on total reported income, the decrease in tax liability reflects the tax avoidance scale; conditional on pre-tax true income (inferred based on estimated income under-reporting scale), the decrease in tax liability reflects both tax evasion and avoidance. Then comparing these two estimates renders the tax evasion scale.

Based on the above estimates, we then compare how much of the total tax saving comes from tax evasion versus avoidance. A main finding is that on average for the self-employed in the U.S., the amount of tax evasion is comparable to that of tax avoidance. Importantly, we further distinguish self-employed families with corporate business from those without, in order to investigate whether they behave differently in evasion and avoidance. We find that for tax saving purposes, tax evasion seems to be more important for the non-corporate self-employed, while tax avoidance may have played a more important role for the corporate self-employed.

Considering corporate tax and payroll tax could introduce some further complications. For example, while for S corporations, income shifting happens through shifting ordinary income to dividends and capital gains, for C corporations, they can also save tax by converting ordinary personal income into corporate income, which is likely to have a lower marginal tax

[2]This could be also interpreted as a definition of tax avoidance.

rate. In addition, S corporations could reduce payroll tax on income not paid out as wages, giving an additional channel for tax savings from avoiding wage payments. Since PSID does not provide information on whether a corporation is C or S type, we try to identify them assuming a corporation would take the form that minimizes its overall tax burden.

Considering corporate tax and payroll tax into our framework estimating tax saving reveals the following findings. First, incorporating corporate tax slightly change previous estimates on type I avoidance scale. Second, the main finding is still that while corporate self-employed rely more on avoidance, the non-corporate self-employed rely more on evasion to save tax. Third, payroll taxes do not create much tax burden for the average self-employed. But the corporate self-employed pay much less payroll tax than their being employees, while the non-corporate self-employed pay a lot more. If we regard payroll tax as a real burden for the self-employed, then incorporating does provide additional benefit for tax purposes.

Finally, we investigate the response of reported total income to PIT rate change conditional on consumption. The idea behind this test is that if the income under-reporting does represent tax evasion, then the scale of tax evasion should increase with the PIT rate, because the increase in PIT rate increases the benefit to evade tax. Similarly, we examine the response of asset income to the change in PIT-CGT (long-run capital gains tax) rates gap conditional on reported total income. The idea is that if the increase in asset income conditional on total income when a family becomes self-employed reflects tax avoidance, then we expect to see the a higher asset income when the PIT-CGT rates gap increases, because the relative decrease in CGT rate increases the benefit to avoid tax. Although these tests could be inaccurate due to data limitations, the empirical testing results render support to our tax evasion and avoidance hypotheses.

This paper contributes to the literature by making a first effort to use survey data to estimate scales of evasion and avoidance for self-employed businesses. Previous papers have exploited the reported income-consumption relation between employees and the self-employed to study income-underreporting (e.g. Pissarides and Weber (1989), Hurst et al. (2014)) or tax evasion (Gorodnichenko et al. (2009)). We push the set of assumptions used in these papers further to estimate tax evasion and avoidance in a unified framework.[3] In our framework, we not only consider personal income tax, but also exploit available information on wealth, dividends, and capital gains in the PSID data to account for corporate income tax and payroll tax. As tax evasion and avoidance are notoriously hard to observe and study, our approach provides an alternative way to study this topic.

The rest of the paper is organized as follows. Section 2 provides a conceptual framework to estimate evasion and avoidance. Section 3 briefly introduces the data. Section 4 shows our estimates of evasion and avoidance. Section 5 concludes.

2.2 Evasion versus avoidance: A conceptual framework

2.2.1 A conceptual illustration

To distinguish between evasion and avoidance, we need to illustrate their conceptual difference. The evaded income is the part of income that never shows up in any forms of income over the whole period of business life, which results in a tax evasion. By contrast, avoidance involves manipulating incomes in forms (ordinary income v.s. lower taxed income), reporting time (now v.s. future), or manipulating expenses to save tax payments. While evasion is illegal, avoidance is largely legal.

To facilitate a detailed investigation, we divide avoidance into three types. The type I avoidance is income shifting, i.e. transferring income from higher taxed to lower taxed account, which only changes the fraction of lower taxed incomes while keep total reported income unchanged. The type II avoidance is income deferral, i.e. the self-employed can choose to defer income realization into the future (e.g. in the form of capital gains), possibly

[3] Feldman and Slemrod (2007) adapt the spirit of this approach and apply it to a US setting, extending the methodology to data from unaudited income returns in order to estimate noncompliance rates for various income sources among US taxpayers. They infer noncompliance based on the relationship between reported income and charitable contributions rather than expenditures on food.

waiting until the CGT (long-run capital gains income tax) rates decreased, or when the PIT (personal income tax) rates decreased so that the capital income may be transferred back to ordinary income to save taxes. The type III avoidance is manipulating items that are subject to exemptions and deductions to reduce the tax liability. Since we mainly focus on the personal income tax liability, the deductions and exemptions available to families apply equally to employees and the self-employed. Later in this paper we also try to incorporate corporate income tax into the consideration of income-shifting.

In the following, we give a formal framework on how to estimate scales of income under-reporting, tax evasion and tax avoidance.

2.2.2 A framework to estimate income under-reporting, tax evasion and avoidance

Assumption 1. Conditional on consumption, employees and the self-employed have the same after-tax true income, which equals after-tax reported income + unreported income.

Assumption 2. Employees do not evade tax (under-report income).

Assumptions 1 and 2 are the main assumptions on which Hurst et al. (2014) build their work of estimating the scale of income underreporting for the self-employed.

Assumption 3. Conditional on pre-tax reported income, employees and the self-employed should have the same tax liability, if there were no avoidance.

Here we implicitly assume that without avoidance, the employees and the self-employed should have the same income structure, conditional on reported income. Any change in the income structure that would have the tax liability change is interpreted in this paper as tax avoiding behavior. For example, it seems to be natural that the self-employed has a more capital-intensive income structure than being an employee, due to ordinary business requirement. It might be argued that people do not intentionally change their income structure for the purpose of avoiding taxes. But a rational person would have taken this (tax liability change due to income structure change as becoming self-employed) into account when deciding whether to become self-employed. Thus it seems broadly reasonable to interpret the tax liability decrease due to any income structure change after being self-employed as tax avoidance.

Assumption 3A. Type II avoidance is negligible in our data.

This is actually a hypothesis tested in this paper. If we neglect the type II avoidance, we can infer the scale of income evasion by comparing the reported pre-tax income of the self-employed and the employees conditional on total consumption. Taking into account the type II avoidance, however, would add complication to the estimation of evasion. In early stages of business, the deferred income would make us overestimate the scale of evasion, while in late stages of business, the realization of deferred income would have us underestimate the scale of evasion. We apply several tests for the empirical importance of type II avoidance in section 4.2.3 below and find no evidence for this empirical importance. Thus, we impose such assumption in an ex post sense rather than ex ante. Admittedly, we concede that income deferral would be important for large corporations. One reason that we do not find evidence for the empirical importance of income deferral in this paper may be that the PSID sample generally focuses on small businesses.

Under Assumptions 1, 2 and 3A, comparing after-tax reported income of employees with that of the self-employed conditional on consumption would render unreported income scale. Previous literature (Pissarides and Weber (1989), Hurst et al. (2014)) estimates income under-reporting scale using the same approach by imposing only Assumptions 1 and 2, without considering the complexity caused by type II avoidance. We provide more evidence for the validity of this approach by testing the empirical importance of the type II avoidance.

Assumption 3B. Conditional on pre-tax reported income, employees and the self-employed should have the same tax liability that counterfactually assumes dividends and capital gains were ordinary income, if there were no type II-III avoidances.

Assumption 3C. Conditional on pre-tax reported income, employees and the self-employed should have the same tax liability, if there were no type I-III avoidances.

Under Assumption 3, comparing tax liability of a family being self-employed to its employee status, conditional on reported before-tax income, renders tax avoidance scale. In particular, under Assumptions 3A and 3B, we can estimate the tax saving scale due to type III avoidance. Under Assumptions 3A and 3C, we can estimate the tax saving scale due to type III avoidance and type I avoidance. Then we can infer the type I avoidance scale.

Assumption 4. Conditional on pre-tax TRUE income, employees and the self-employed should have the same tax liability assuming there were no evasion and avoidance.

Under Assumption 4, we can estimate the scale of tax evasion plus avoidance by comparing the tax liability of employees and the self-employed, conditional on before-tax true income. A brief illustration on how to obtain the before-tax true income is as follows: first estimate the income under-reporting scale (using Asspumtions 1, 2, and 3A); then predict the after-tax true income for all the self-employed observations. Adding tax liability to this after-tax true income renders before-tax true income for the self-employed. For the employees, the before-tax true income is simply their before-tax reported income, due to Assumption 2.

2.3 Data

In this paper I use the PSID (Panel Study of Income Dynamics) data from 1968 to 2011. I build up my PSID dataset based on data provided by Attanasio and Pistaferri (2014). They have assembled a 1968-2011 family level data set, including variables on demographic characteristics, income, some measure of consumption, and relative prices. I then merge other variables relevant to our study into this main dataset. All nominal values in this paper are deflated using 1979 CPI. In all analysis, we use PSID sample weights to obtain nationally-representative estimates.

The key variables for our paper are consumption, income, and tax liability. Consumption and income are reported in the PSID survey. We follow previous literature, e.g.Powell and Shan (2012) and Blundell et al. (2008), to use NBER's TAXSIM model to calculate tax liabilities for all years. Throughout the paper, we focus on federal personal income tax because the TAXSIM model only calculates state taxes after 1977. As a robustness check, we consider the overall federal and state tax liability for the post-1977 sample. We follow previous literature to define a family as self-employed if the head is self-employed. Table 2.15 shows summary statistics of some key variables. Useful variables like wealth and capital gains are not available for all years, but are used in parts wherever necessary. For more details of the construction of variables used in this paper, see Appendix 2.6.1.

2.4 Estimating evasion and avoidance

2.4.1 Estimating the scale of evasion

Conceptual framework of the consumption-income relation

To motivate our estimation of evasion scale, we consider the conventional model of life-cycle consumption under uncertainty. The consumer's problem is

$$Max_{\{C_t, A_{t+1}\}} E_0 \sum_{t=0}^{\infty} (1+\delta)^{-\tau} u(C_t)$$

$$s.t. \, A_{t+1} + C_t = w_t + A_t(1+r), \, A_0 \, is \, given,$$

where δ is rate of subjective time preference, r is real interest rate (assumed to be constant), $u(\cdot)$ is utility function (assumed to be strictly increasing and concave), C_t is consumption in year t, w_t is earnings in year t, A_t is assets or savings in year t. From this model it is easy to get the familiar result that $E_t u'(C_{t+1}) = \frac{1+\delta}{1+r} u'(C_t)$. If we simply assume that $\delta = r$, then the strict concavity of $u(\cdot)$ renders $E_t C_{t+1} = C_t$. The budget constraint renders

$\frac{A_t}{(1+r)^t} = A_0 + \sum_{\tau=1}^{t}(1+r)^{-\tau}(w_\tau - C_\tau)$. Using the boundary condition $lim_{t\to\infty}\frac{A_t}{(1+r)^t} = 0$, we obtain $E_0C_t = rA_0 + rE_0\sum_{\tau=1}^{\infty}(1+r)^{-\tau}w_\tau \equiv Y^P$ for all t, where Y^P is permanent income. It says that the expected consumption in every period should be equal to the expected permanent income, which depends on initial asset and annual earnings. As Hall (1978) pointed out, such a life-cycle permanent income hypothesis model matches quite well with data, except that sometimes it needs to be modified to allow some time for some people to adjust their consumption to the change in permanent income.

For a family i, the above model renders $C_{it} = Y_i^P$, where Y_i^P is the permanent (after-tax) income of family i. Assuming that the actual annual (after-tax) income evolves around the permanent income: $Y_{it} = Y_i^P f(X_{it}, \lambda_t) + e_{it}$, where X_{it} include a set of socioeconomic characteristics (dummies for age, education, marital status, race, state, head's hours worked, home ownership, disability, and current family scale), λ_t are year dummies, and e_{it} is transitory income disturbance, then $C_{it} = \frac{Y_{it}-e_{it}}{f(X_{it},\lambda_t)}$.

Conceptual framework of estimating income under-reporting

To take into account the complication of estimating income under-reporting scale brought about by the type II avoidance, we focus on one business life of self-employed families. In particular, we focus on the sample of families that start and end business within available data period. Since some families have multiple employee-self-employed cycles, we focus on their first employee-self-employed cycle, i.e. from the earliest year when a family was an employee, along the years when a family turns from employee to self-employed, until the year when a family ends their self-employed business. We exclude families that start self-employed business without ending it within our data period because we cannot examine the extent of type II avoidance for these families.

We assume that conditional on consumption, the self-employed and employees should have the same after-tax true income, consistent with the permanent income hypothesis model, same as the assumption made by Pissarides and Weber (1989) and Hurst et al. (2014).

The difference of after-tax reported income in the two groups thus represents the scale of income underreporting of the self-employed.[4] This assumption has the same spirit as that adopted by Gorodnichenko et al. (2009), who use the gap between reported consumption and reported income to represent the tax evasion scale in Russia. In addition, the assumption that consumption is an appropriate measure to compare the well-beings of employees and that of the self-employed also receives support from Sarada (2015). Pissarides and Weber (1989) and Hurst et al. (2014) assume that the log-linear Engel curve is a good representation of household behavior, which can be generated, at least to a first order approximation, under some preference assumption. This results in the following specification:

$$lnC_{it} = \alpha_0 + \alpha_1 lnY_{it} + \phi \cdot X_{it} + \sum_t \varphi_t \lambda_t + \xi_{it},$$

where Y_{it} is the after-tax true income. Such specification is consistent with the life-cycle permanent income hypothesis model noting that it can be obtained from $C_{it} = \frac{Y_{it} - e_{it}}{f(X_{it}, \lambda_t)}$, using first order approximation.

The above specification can be re-expressed as

$$lnY_{it} = \beta_0 + \beta_1 lnC_{it} + \gamma \cdot X_{it} + \sum_t \theta_t \lambda_t + \varepsilon_{it}.$$

For the employees, their reported income is true income due to third-party reporting. For the self-employed, however, the reported tax is generally lower than true income due to under-reporting, because the self-employed self-report income and are not subject to third-party reporting. Assume that for the self-employed $Y_{it}^T = kY_{it}^R$, where Y_{it}^R is the reported income of the self-employed and $k > 1$ measures the extent of under-reporting. The concealed

[4]This implicitly assumes that the past is irrelevant: Those with the same consumption now have the same income now, regardless of any differences in incomes they received in the past. It also implicitly assumes that the expectations of future income depend solely on current income. Otherwise, the self-employed should be expecting a fall in future income when their business eventually fails, whereas employees expect stable income, leading to different expectations for permanent income conditional on current income between the two groups.

fraction of true income is then $\frac{Y_{it}^T - Y_{it}^R}{Y_{it}^T} = 1 - \frac{1}{k}$. The relation between income and consumption for the self-employed can be written as

$$lnY_{it}^T = ln(kY_{it}^R) = \beta_0 + \beta_1 lnC_{it} + \gamma \cdot X_{it} + \sum_t \theta_t \lambda_t + \varepsilon_{it}.$$

Thus, if we run the following regression

$$lnY_{it} = \beta_0 + \beta_1 lnC_{it} + \beta_2 Self_{it} + \gamma \cdot X_{it} + \lambda_t + \varepsilon_{it},$$

where $Self_{it} = 1$ if family i is self-employed in year t, Y_{it} is after-tax reported income, then $\beta_2 = -lnk$ and the under-reported fraction of income is $1 - \frac{1}{exp(-\beta_2)} = 1 - exp(\beta_2)$. It is easy to show that when β_2 is close to zero, $1 - exp(\beta_2)$ approximately equals $-\beta_2$. For the convenience of interpreting results, we sometimes would simply use $-\beta_2$ as the income under-reporting fraction when β_2 is small. When estimating income under-reporting scale, I use overall sample. Since state tax liability is available only after 1977, I would use federal tax liability here, and use various other measures of tax liabilities as robustness checks later.

Due to the panel structure of PSID data, we can further control for family fixed effects μ_i. Not controlling for family fixed effects would induce correlated errors across time within the family, due to various omitted factors affecting consumption and income (e.g. age of kids) that might be correlated with the decision to become self-employed. Controlling for μ_i, we have the following specification

$$lnY_{it} = \beta_0 + \beta_1 lnC_{it} + \beta_2 Self_{it} + \gamma \cdot X_{it} + \lambda_t + \mu_i + \varepsilon_{it}.$$

While running regressions with family fixed effects, we cluster standard errors at family level. Like Hurst et al. (2014), all our estimates are weighted using the sampling weights.

Testing the empirical importance of type II avoidance

Test 1: focus on business end years.

In early stages of business, the deferred income would make us overestimate the scale of evasion, while in late stages of business, the realization of deferred income would have us underestimate the scale of evasion. To test the empirical importance of income deferral, we run the following regression:

$$lnY_{it} = \beta_0 + \beta_1 lnC_{it} + \beta_2 Self_{it} + \beta_3 Self_{it} \cdot End_{it} + \gamma \cdot X_{it} + \lambda_t + \mu_i + \varepsilon_{it},$$

where $End_{it} = 1$ if t is the final self-employed year for family i. (Note that we have excluded unemployed and retired observations, so after the final self-employed year the family returns to an employee.) β_2 measures the average income under-reporting scale in self-employed years in which businesses do not end, while $\beta_2 + \beta_3$ measures the income under-reporting scale in years when business ends. The idea to test the empirical importance of income deferral is: if the type II avoidance is important, then we expect to see that β_3 is positive, because deferred income in early years would be realized when business ends, which may lower income under-reporting. We focus on the sample of families that start and end business within our sample period. By doing this, we exclude families that were always employees or were always self-employed in our sample. For families that have multiple employee-self-employed cycles, we focus on their first cycle.

Table 2.1 column 2 shows that $\widehat{\beta_3}$ is insignificantly different from zero both economically and statistically. Column 4 shows that the income under-reporting in business end years is not significantly different from that in business non-end years in all self-employed year intervals. The results of this test thus do not provide evidence to reject the hypothesis that type II avoidance is empirically unimportant.

Table 2.1. Estimating income under-reporting

Dep var:	log(after-tax reported income)				
	[1]	[2]	[3]	[4]	[5]
log(total consumption)	0.434***	0.434***	0.432***	0.433***	0.425***
	(0.035)	(0.035)	(0.035)	(0.035)	(0.036)
self-year 0			-0.105***	-0.107***	-0.105***
			(0.019)	(0.025)	(0.025)
self-year 1-5			-0.093***	-0.100***	-0.097***
			(0.023)	(0.025)	(0.025)
self-year 6-10			-0.093**	-0.097**	-0.094**
			(0.040)	(0.042)	(0.042)
self-year >10			-0.061	-0.069	-0.066
			(0.047)	(0.050)	(0.051)
end*self-year 0				0.004	
				(0.034)	
end*self-year 1-5				0.042	
				(0.040)	
end*self-year 6-10				0.009	
				(0.093)	
end*self-year >10				0.045	
				(0.073)	
self-employed	-0.100***	-0.106***			
	(0.017)	(0.020)			
end*self-employed		0.018			
		(0.024)			
(end next year)*self-employed					
Other controls	Yes	Yes	Yes	Yes	Yes
Family fixed-effect	Yes	Yes	Yes	Yes	Yes
Observations	16,427	16,427	16,427	16,427	15,534
R-squared	0.683	0.683	0.683	0.683	0.683
ymean of employees	9.963	9.963	9.963	9.963	9.963

Notes: The sample for column 1-5 focuses on the first employee-self-employed cycle of families ever turning from employees to self-employed within sample period. Sample for column 5 further excludes business end years. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Test 2: exclude business end years.

A concern on the above test is that business may end due to failures, which would bring about a negative income shock that could offset the positive jump in income from realizing the past deferrals when business is sold. To address this concern and provide another test on the importance of income deferral, we exclude the business end years and run the following regression:

$$lnY_{it} = \beta_0 + \beta_1 lnC_{it} + \sum_k \rho_k \cdot SelfInt_k + \gamma \cdot X_{it} + \lambda_t + \mu_i + \varepsilon_{it},$$

where $SelfInt_k$ are interval dummies indicating if a family is in its kth self-employed year interval. The intervals include 0, 1-5, 6-10, >10 year intervals.

The idea of this test is: if income deferral is empirically important, then among the years that business does not fail, we would expect to see that income under-reporting is larger in earlier years and smaller in later years. Column 5 shows that the income under-reporting goes smaller with self-employed years. But test on the coefficient of "self-year 0" equals that of "self-year 1-5", "self-year 6-10", and "self-year >10" respectively renders p-values of 0.7595, 0.8125, and 0.4506, suggesting that the decrease in income under-reporting scale with year is not significant. Thus, this test also does not provide evidence to reject the hypothesis that type II avoidance is empirically unimportant.

Summary.

Based on the above evidence, we would assume that the type II avoidance is negligible in our study (Assumption 3A). A caveat is that we do not try to convey the information that income deferral is unimportant in general. It is possible that income deferral is important for large corporations. But since our study focuses mainly on small businesses, it is reasonable that income deferral is not as important to them as the other ways to save taxes.

Estimating the scale of income under-reporting

The empirical unimportance of type II avoidance ensures us to focus on the type I avoidance in later section. And we can interpret the estimate of β_2 in Table 2.1 column 1 as the income under-reporting scale of the self-employed comparative to their being employees, which is 10% of the income that should be reported. As column 2 shows, in all self-employed years, the income under-reporting scales are statistically significant (except self-year >10, probably due to few variation) and around the average scale. The income under-reporting scale becomes slightly lower in later years. In a later section, we will compare our estimated evasion scale with previous findings. Based on the mean value of dependent variable (9.963) of employees, we obtain the average unreported income level of the self-employed is [1-exp(-0.1)]*exp(9.963)=$2020 (1979 dollars).

Note that the consumption expenditures considered here should be mostly home-related rather than business-related. This is important because for the self-employed, the business expenses can be used as deductible so as to reduce taxable income. And this is true because, in many cases, the business expenses are clearly separable from the personal expenses, and the households generally won't report their business expenses in the above consumption components in the PSID survey. They know that they are mainly asked for the household consumption expenditures rather than business expenses.

Three points are worth noting, though. First, although the households may not intentionally report business expenses in their consumption expenditures, some parts of the consumption components (e.g. taxi fares and other transportation expenditures) may be difficult to distinguish between business and personal purposes. Given that such misclassified components may be small, our results won't be affected much. Second, for the self-employed, their consumption might include business purchases and their pre-tax business income would be reduced because they have spent the money on such consumption. This might result in an income under-reporting conditional on consumption, which is appropriately interpreted as evasion under our framework. Third, some self-employed might claim their home as business offices to deduct the rents from taxable income. Such claims could form evasion, which is appropriately accounted for in our framework.

Testing a relevant assumption

Under the identifying assumption that the self-employed should have the same after-tax TRUE income as the employees, conditional on consumption, we can interpret the income under-reporting of the self-employed as evasion. However, this assumption is fundamentally untestable. Here we examine a relevant yet untested hypothesis in literature: conditional on consumption, the families turning to self-employed should have the same reported income, before they become self-employed, as the always-employee families. If this is untrue, then the families becoming self-employed would have different income-consumption relation than the always-employee families when they were both employees, which would make our conceptual framework invalid.

To test this hypothesis, we focus on the sample of employee observations (using always-employee families and the employee period of the ever self-employed families) and run the following pooled cross-sectional regression

$$lnY_{it} = \beta_0 + \beta_1 lnC_{it} + \beta_2 Self_i + \gamma \cdot X_{it} + \lambda_t + \varepsilon_{it},$$

where $Self_i = 1$ if family i was self-employed at least in one year over our data period. From column 1 of table 2.16 we see that β_2 is zero. This is consistent with the tested hypothesis and renders some support for our identifying assumption.

Sample representativeness

Throughout this paper, we mainly focus on the families turning from employees to self-employed and end business finally (in their first employee-self-employed-employee cycle). To what extent can such sample of self-employed represents the whole sample of self-employed?

By showing the summary statistics of several key variables in table 2.15, we see that the self-employed observations in our sample have very similar means and standard deviations in log of total reported income (both before-tax and after-tax), log of imputed total consumption, age, and educational level of heads to those of the self-employed in the whole sample. For the employee observations in my sample, they also have similar means and standard deviations of the several key socio-economic characteristics to those of the employees in the whole sample. This suggests that by examining the families used in this paper, we are not focusing on a much different sample from the overall sample, which provides some evidence for the external validity of our results.

2.4.2 Comparing with previous estimates on income under-reporting

Andreoni et al. (1998) and Slemrod (2007) provide good summaries of previous findings on tax evasion. Here we only focus on previous estimates of income under-reporting scale in the U.S. and compare them with our finding. In particular, we compare our estimates with IRS estimates and those of Hurst et al. (2014), who also use U.S. household survey data (Consumer Expenditure Survey 1980-2003, PSID 1980-1997) for analysis.

An Internal Revenue Service report (IRS (1979), p.8, table 2) estimated that the unreported income in 1976 was about 6-8% of the total reportable income. The unreported fractions vary across different components, from 2-3% for wages and salaries to 36-40% for self-employment income.[5] Yet the overall unreported income figures should be considered more reliable than the components from which they were formed, as some of the estimates (e.g. wage/salaries) are more reliable than others (IRS (1979), p.6). The unreported tax was around 8.3-10.8% of the total tax that should be reported (calculated using figures in p.12 of IRS (1979)). Similarly, Slemrod (2007) calculated that the overall under-reported tax

[5]Self-employment income covers net earnings of farm and nonfarm proprietorships and partnerships (at times referred to as unincorporated business income) as well as net earnings of self-employed individuals working outside the context of regularly established businesses in the legal sector. (According to IRS (1979), p. 7, footnote 2.)

is 13% of the tax that should have been paid, with tax under-reported fractions of various components ranging from 1% for wage/salary to 52% for self-employment tax, using figures published by IRS based on 2001 National Research Program (NRP) data.

Table 2.2. Comparing income under-reporting estimates with previous research

			Income under-reporting fraction		
			employee	self-employed	self-employed - employee
IRS (1979): wage/salary reported: $881B in $902B; other incomes reported: $192B from $246B	Case A. (Upper bound)	employees earn all wage/salary, self-employed earn all other incomes	2.3%	22.0%	**19.7%**
	Case B. (PSID data)	13% of the wage/salaries are earned by self-employed, 37% of other incomes are earned by employees	5.5%	10.6%	**5.1%**
Hurst et al. (2014):					**around 25%**
This paper:					**10.0%**

These estimates, however, are not directly comparable to ours. And although Hurst et al. (2014) provides income under-reporting estimates, they do not directly relate their results to the income under-reporting scale obtained by authorized auditor agency like the IRS. To fill this gap and apply a preliminary comparison, we use the figures in table 2 of IRS (1979). The result is summarized in our table 2.2. First (Case A), suppose all wage/salary incomes are earned by the employees, while the other kinds of incomes are earned by the self-employed.[6] Then the employees report $881 billions from the $902 billions reportable wage/salaries, while the self-employed report $192 billions from the $246 billions other incomes (self-employment income, interest, dividends, rents and royalties, pensions, annuities, estates, and trusts, capital gains, and other incomes). Then the under-reported fraction is 2.3% for employees and 22% for the self-employed. And the income unreported rate is 19.7% for the self-employed relative to the employees.

[6] To simplify, we adopt the lower bound estimate of reportable income. Using the upper bound estimate would render an income under-reporting fraction of the employed relative to employees of 6.5% rather than the estimate of 5.1% using the upper bound estimate of reportable income.

To make further comparison, we relax the above assumption and use the income composition from PSID data to make a calculation (Case B). From our PSID sample, over half of the self-employed have wage/salaries, and around 13.6% of the wage/salaries are earned by the self-employed, around 37% of incomes other than wage/salaries belong to the self-employed.[7] Then the self-employed report $190.9 (13.6%*881+37%*192) billions from the $213.7 (13.6%*902+37%*246) billions reportable income, while employees report $882.1 (86.4%*881+63%*192) billions from the $934.3 (86.4%*902+63%*246) billions reportable income. Then the under-reported fraction is 5.6% for employees and 10.7% for the self-employed. And the relative unreported rate is 5.1% for the self-employed relative to the employees.

This income under-reporting estimate is comparable to ours, and actually not far from our 10% estimate. By contrast, Hurst et al. (2014) find that, on average, the self-employed under-report their income relative to the employees by about 25%, which is even larger than the upper bound 19.7% using figures in IRS (1979). Since we only use IRS (1979) data to make such a calculation, the concrete number might vary if an alternative year is used.[8] But the magnitude suggests that previous findings might have overestimated the income under-reporting scale of the self-employed relative to the employees.

One main conclusion of Hurst et al. (2014) is that the self-employed, who have been shown to have under-reported income to tax authorities, have also under-reported income to household surveys. But they do not actually compare the extent of income under-reporting of the self-employed in tax audits versus that in household survey. Here we fill this gap and find that, under our preferred specification, the self-employed under-report income to a similar extent in household survey as that in tax audits. This provides important evidence on how people respond to surveys on their incomes and is worth noting for relevant research using

[7] To understand why only 37% of incomes other than wage/salaries belong to the self-employed, note that although the average other incomes in a self-employed family is 3.56 times of that in an employee family, the number of employee family is 6.07 times of that of the self-employed family.

[8] But as far as I can search from online, the *Estimates of Income Unreported on Individual Tax Returns* can only be found for year 1979.

household surveys.[9] [10]

A detailed comparison with Hurst et al. (2014)

It is worth pointing out several important differences between the estimation of this paper and that of Hurst et al. (2014). The specification used in this paper is $lnY_{it} = \beta_0 + \beta_1 lnC_{it} + \beta_2 Self_{it} + \gamma \cdot X_{it} + \lambda_t + \mu_i + \varepsilon_{it}$ and the estimated income under-reporting fraction is $1 - exp(\widehat{\beta_2})$, while the specification used by Hurst et al. (2014) is $lnC_{it} = \alpha_0 + \alpha_1 lnY_{it} + \alpha_2 Self_{it} + \phi \cdot X_{it} + \lambda_t + \xi_{it}$ and the estimated income under-reporting fraction is $1 - exp(-\widehat{\frac{\alpha_2}{\alpha_1}})$. While we focus on families that turn from employees to self-employed (in their first employee-self-employed cycle) with heads aged 25-65 using PSID 1968-2011, Hurst et al. (2014) also include families not experiencing such switch with heads aged 25-55 using PSID 1980-1997; Hurst et al. (2014) focus on food consumption while we use imputed total consumption; while Hurst et al. (2014) use education dummies as instruments for permanent income, we directly control for education dummies in our OLS regressions; furthermore, we control for family fixed-effect to explore the within family change while Hurst et al. (2014) do not; the control variables are not entirely the same, our paper include richer controls than theirs. Due to such differences, it is not surprising that we get different estimates. We believe it is important to focus on the families turning from employees to self-employed. It also seems more natural to control for education dummies directly.

To apply a direct comparison, we focus on the PSID 1-year specification as shown

[9] See section 6 of Hurst et al. (2014) for a detailed discussion of the importance of income under-reporting by the self-employed.

[10] Why could our estimate of income under-reporting differ from the IRS estimate? One possibility is that using surveyed income might overestimate the income under-reporting scale if that people might be more careful on the accuracy of their reported income when they report income to the tax agency in case they could be punished due to under-reporting. But another possibility is that using surveyed income might underestimate the income under-reporting scale if people deliberately under-report more income to tax authorities than to household survey, given their intention to save taxes. Thus it seems hard to definitely predict whether using surveyed income would overestimate or underestimate the actual income under-reporting scale. Note that the income under-reporting scale implied by IRS (1979) is not used as the benchmark to judge whether the estimate using surveyed income underestimates or overestimates the actual income under-reporting scale, because IRS (1979) only looks at 1976 data while we focus on a much longer period. Instead, we use the IRS (1979) estimate to judge whether our estimate is reasonable in its magnitude. From this perspective, our estimate is close to the IRS estimate in magnitude and is therefore reasonable.

Table 2.3. Comparison with the estimation of Hurst et al. (2014)

	Specification	Household fixed effects	Other controls	Sample	Consumption	Income underreporting fraction
[1]	[1]-[5]: Hurst et al. (2014)	No	Hurst et al. (2014)	[1]-[3]: PSID 1980-1997, all families with heads aged 25-55	Food	32.1% (Original paper)
[2]		No	slightly different from Hurst et al. (2014)		Food	28% (Replication)
[3]		Yes	slightly different from Hurst et al. (2014)		Food	8.9% (Imprecise)
[4]		Yes	Controls in my paper	[4]-[8]: PSID 1968-2011, families turning from employees to self-employed with head aged 25-65	Food	-2.2% (Imprecise)
[5]		Yes	Controls in my paper		Total	76.0%
[6]	[6]-[10]: My paper	Yes	Controls in my paper		Total	10.0%
[7]		Yes	Controls in my paper		Food	4.9%
[8]		No	Controls in my paper		Total	12.8%
[9]		Yes	Controls in my paper	[9]-[10]: PSID 1968-2011, overall sample with heads aged 25-65	Total	5.3%
[10]		No	Controls in my paper		Total	6.3%

Notes: Other controls in Hurst et al. (2014) include five-year age dummies, a dummy denoting if the household head is black, a dummy denoting if the household head is married, and a series of family size dummies. In replicating their results, we use slightly different variables from their controls. We control for five-year age dummies, dummies denoting whether the household head is black and whether head is white; for marital status dummies, we control dummies denoting whether head is never married, widowed, divorced, or separated (using married status as omitted category). In addition to these, the main controls in our paper also include dummies for education and state, head's hours worked, homeownership, and disability.

in table 2 in Hurst et al. (2014). Table 2.3 shows the details of such comparison. Since their results using the OLS and IV results are similar for PSID 1-year specification, we focus on after-tax income and OLS result, which renders an estimate of income under-reported fraction of 32.1% (=1-exp(-0.12/0.31)), as row 1 shows.

In row 2, we try to replicate the estimation result of Hurst et al. (2014). We use the same specification as theirs, i.e. not controlling for household fixed-effects, using only PSID 1980-1997 waves, focusing on heads aged 25-55, using food expenditure as consumption measure. Other controls in Hurst et al. (2014) include age dummies, a dummy denoting if the household head is black, a dummy denoting if the household head is married, and a series of family size dummies. In replicating their results, we use slightly different variables from their controls. We control for dummies denoting whether the household head is black, and whether head is white; for marital status dummies, we control dummies denoting whether head is never married, widowed, divorced, or separated (using married status as omitted category). Thus, we control for the same characteristics in a richer way, which though should not affect the main estimates. As row 2 shows, the estimated income under-reported fraction is 28% ($\widehat{\alpha_1} = 0.301$, $\widehat{\alpha_2} = 0.099$, both significant at 1% level), not entirely the same but close to their original estimate.

In row 3, we further control for household fixed effects. The estimated income under-reported fraction is 8.9%, though imprecisely estimated ($\widehat{\alpha_1} = 0.128$, significant at 1% level, $\widehat{\alpha_2} = 0.012$, insignificant at 10% level). In row 4, we use the sample and controls used in my paper, but follow the specification in Hurst et al. (2014) except we control for household fixed effect. The estimated income under-reported fraction is -2.2%, though again imprecisely estimated ($\widehat{\alpha_1} = 0.135$, significant at 1% level, $\widehat{\alpha_2} = -0.003$, insignificant at 10% level). In row 5, we use total consumption rather than food consumption, with the others the same as in row 4. The estimated income under-reported fraction is 76% ($\widehat{\alpha_1} = 0.082$, $\widehat{\alpha_2} = 0.117$, both significant at 1% level), which is unlikely to be true.

Row 6 shows the main estimate of our paper. By simply replacing total consumption by food consumption, we get an estimated income under-reported fraction as 4.9% as shown in row 7, lower than the estimate using total consumption, but quite close to the estimate suggested by IRS (1979). Row 8 replicates our main estimate without controlling for household fixed effect, which renders 12.8%. These estimates are all statistically significant at 1% level. Finally, using the overall sample rather than the families we focus on (those turning from employees to self-employed and ending business finally) and total consumption, we obtain quite close estimates to that implied by IRS (1979), as show in row 9 and 10.

Now we sum up several main points obtained from the above comparison. From the ex ante view, to get consistent estimate of k, the specification used in Hurst et al. (2014) requires the coefficients of lnY_{it} and $Self_{it}$ to be consistently estimated, while our specification only requires the coefficient of $Self_{it}$ to be consistent, which is a weaker condition. From the empirical results, comparing row 3 and row 2 shows that controlling for household fixed effect would greatly change the magnitude and significance of estimate under Hurst et al.

(2014) specification, suggesting that not controlling for them might suffer from severe omitted variable problem. By contrast, our main results are quite robust to controlling household fixed effects (comparing row 6 and 8), suggesting our specification may be more robust. Finally and importantly, that the estimated income under-reporting fraction in this paper is stable and close to that implied by the IRS (1979) also suggests that the specification used in this paper may be more appropriate.

2.4.3 Estimating the scale of type III avoidance

Given the empirical unimportance of type II avoidance, the idea of estimating the scale of type III avoidance is that without considering dividends and capital gains, the tax liability decreases conditional on total reported income reflects tax savings other than type I avoidance, i.e. the tax liability decrease resulted from the manipulation of exemption and deduction items when a family becomes self-employed. Note that we focus on the personal income tax here, where employees and the self-employed face largely the same rules for exemption and deduction, and that the TAXSIM model does not distinguish between employees and the self-employed when calculating their tax liability. To confirm what we estimate here is type III avoidance, we also examine whether the items subject to exemption and deduction change when a family becomes self-employed, conditional on total reported income.

We use the following regression to estimate the extent of tax saving due to type III avoidance for families turning from employees to self-employed:

$$lnTax_{it} = \beta_0 + \beta_1 lnY_{it} + \beta_2 Self_{it} + \gamma \cdot X_{it} + \lambda_t + \mu_i + \varepsilon_{it}.$$

where Y_{it} is total reported income, Tax_{it} is the federal tax liability estimated using TAXSIM model. We use federal tax liability because the TAXSIM model only calculates state tax liability starting in 1977. But the result of using federal and state overall tax liability for the post-1977 sample render similar results. When calculating the tax liability to estimate the scale of type III avoidance, we counterfactually classify dividends into other property incomes that are subject to ordinary income tax rate and ignore capital gains by assuming all the capital gains are included in other property incomes. Capital gains information can be imputed for years since 1984, while dividends income has been separately reported since 1992. We present results for both whole sample years and for years since 1992.

Table 2.4. Scale of Type III avoidance

Dep var:	log(tax liability ignoring dividends and capital gains)			
	All years		Year>=1992	
	[1]	[2]	[3]	[4]
log(total income)	1.636***	1.637***	1.595***	1.596***
	(0.027)	(0.027)	(0.047)	(0.047)
corp self-employed		-0.086***		-0.085**
		(0.021)		(0.035)
non-corp self-employed		-0.057***		-0.055
		(0.017)		(0.035)
self-employed	-0.065***		-0.063**	
	(0.015)		(0.031)	
Other controls	Yes	Yes	Yes	Yes
Family fixed-effect	Yes	Yes	Yes	Yes
Observations	15,176	15,176	4,717	4,717
R-squared	0.907	0.907	0.924	0.924
ymean of employees	8.019	8.019	8.013	8.013

Notes: The sample focuses on the first employee-self-employed cycle of families ever turning from employees to self-employed within sample period. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.4 shows that on average a family faces 6.5% less tax liability after becoming self-employed due to type III avoidance, conditional on total reported income. For post-1992 sample, the type III avoidance scale is 6.3%. When we decompose the self-employed into those with corporate business an those without, we see that the corporate self-employed have more type III avoidance. To facilitate comparing tax savings between various avoidances and evasion, we also calculate the tax saving in dollars. The mean value of dependent variable is 8.013 for the employee observation, so we obtain that the self-employed on average save [1-exp(-0.063)]*exp(8.013)=$184.39 dollars from type III avoidance. Similarly, we obtain the tax savings from type III avoidance for the corporate self-employed and non-corporate self-employed is $246.09 and $161.61, respectively.

Table 2.5. Change of items subject to exemptions and deductions

Dep var:	# dependents	# age dependents	# dependent children	ln(property tax paid)
log(total income)	0.080***	0.002	-0.097***	0.104***
	(0.013)	(0.003)	(0.018)	(0.025)
self-employed	-0.01	0.009***	-0.027	0.031
	(0.012)	(0.003)	(0.019)	(0.025)
Other controls	Yes	Yes	Yes	Yes
Family fixed-effect	Yes	Yes	Yes	Yes
Observations	16,430	16,430	16,430	11,469
R-squared	0.947	0.256	0.878	0.752
log(total income)	0.080***	0.002	-0.097***	0.102***
	(0.013)	(0.003)	(0.017)	(0.025)
corp self-employed	0.016	0.009	-0.066**	0.113***
	(0.021)	(0.007)	(0.032)	(0.038)
non-corp self-employed	-0.018	0.009**	-0.015	0
	(0.012)	(0.004)	(0.018)	(0.026)
Other controls	Yes	Yes	Yes	Yes
Family fixed-effect	Yes	Yes	Yes	Yes
Observations	16,430	16,430	16,430	11,469
R-squared	0.947	0.256	0.878	0.753

Notes: The sample focuses on the first employee-self-employed cycle of families ever turning from employees to self-employed within sample period. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.5 shows how the exemption and deduction items used in the TAXSIM model change as a family becomes self-employed, conditional on reported total income. Among the exemption terms (number of dependents, number of age dependents, number of dependent children), only number of age dependents increase statistically significantly, but not economically significantly. This is intuitive as the exemption terms are generally hard to manipulate. Among the items subject to deduction, the property tax paid seems to increase, though not statistically significantly. Distinguishing the corporate self-employed from the non-corporate self-employed, it seems that the corporate self-employed increase the property tax paid significantly, although not statistically significantly. If the tax rate on corporate income is less than the personal income tax rate, as is likely to be true for owners of start-ups, then income will be shifted into the corporate income base and expenses will be shifted into the personal tax base. The higher property tax deduction for corporate owners could reflect this, and will be offset partially by higher corporate income tax.

2.4.4 Estimating the scale of type I avoidance

To estimate the scale of type I avoidance, we separately consider how a family reduce tax liability conditional on income by shifting income from ordinary income to the lower taxed dividends and capital gains.

Scale of type I avoidance due to shifting income to dividends

First, we examine whether dividends income increases conditional on total income after a family becomes self-employed. Focusing on post-1992 sample when dividends information is separable from other property incomes, table 2.6 shows that dividends increase by 7.2%. Though the estimate is imprecise for the whole self-employed, we see the corporate self-employed have significantly more dividends income.

Table 2.6. Change of dividends (since 1992)

Dep var:	log(dividends)	
	[1]	[2]
log(total income)	0.286***	0.279***
	(0.067)	(0.065)
corp self-employed		0.458**
		(0.195)
non-corp self-employed		-0.042
		(0.100)
self-employed	0.072	
	(0.100)	
Other controls	Yes	Yes
Family fixed-effect	Yes	Yes
Observations	5,465	5,465
R-squared	0.646	0.648

Notes: The sample focuses on the first employee-self-employed cycle of families ever turning from employees to self-employed within sample period. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** p<0.01, ** p<0.05, * p<0.1.

Then we estimate the scale of tax avoidance due to shifting income to dividends. In table 2.7, the first two columns simply replicates columns 3-4 in table 2.4, based on which we have obtained the tax saving levels from type III avoidance. The last two columns show the tax saving due to both type III avoidance and type I avoidance considering dividends. From column 3, we can calculate the average tax saving of the self-employed due to both type III avoidance and type I avoidance considering dividends as [1-exp(-0.066)]*exp(8.012)=$192.69. Comparing it with the tax saving due to type III avoidance, $184.39, then we obtain the average tax saving due to transferring ordinary income to dividends for the self-employed as $8.3. Similarly, we can obtain the tax saving due to dividends for the corporate self-employed and non-corporate self-employed as $13.57 and $5.55. Given the result obtained above that the income shifted to dividends is not too much, such a small avoidance scale is not surprising. Note that dividend is taxed as ordinary income before 2003 but faces a lower rate since 2003, so if we focus only on the several years since 2003, the estimate of avoidance scale due to shifting income to dividends might be larger. But a limitation is that the sample would be too small to render a reliable estimate. Given the relative small amount of dividends, we believe the small avoidance scale estimated here is close to truth.

Table 2.7. Avoidance scale due to shifting income to dividends and capital gains (since 1992)

Dep var:	log(tax liability ignoring dividends and capital gains)		log(tax liability considering dividends)		log(tax liability considering dividends and capital gains)	
	[1]	[2]	[3]	[4]	[5]	[6]
log(total income)	1.595***	1.596***	1.593***	1.594***	1.557***	1.559***
	(0.047)	(0.047)	(0.047)	(0.047)	(0.054)	(0.055)
corp self-employed		-0.085**		-0.090***		-0.208***
		(0.035)		(0.035)		(0.068)
non-corp self-employed		-0.055		-0.057*		-0.110**
		(0.035)		(0.035)		(0.045)
self-employed	-0.063**		-0.066**		-0.129***	
	(0.031)		(0.031)		(0.042)	
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Family fixed-effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,717	4,717	4,717	4,717	3,381	3,381
R-squared	0.926	0.926	0.926	0.926	0.902	0.902
ymean of employees	8.013	8.013	8.012	8.012	7.955	7.955

Notes: The sample focuses on the first employee-self-employed cycle of families ever turning from employees to self-employed within sample period. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Columns (5)-(6) focus on those with non-negative capital gains.

Scale of type I avoidance due to shifting income to capital gains

First, we examine whether capital gains increase when families become self-employed, conditional on total income and net wealth. Employees, as well as the self-employed, have capital gains. But transferring ordinary income to capital gains income may mostly be adopted by the self-employed families, who have their own businesses. Such income shifting may be mainly reflected in the capital gains from business/farms rather than from stocks or real estate. So we expect to see most increases in capital gains come from business/farms when a family turns from an employee to self-employed.

To test the hypothesis that conditional on reported total income, families would have higher capital gains when they become self-employed, we use the following regression:

$$CG_{it}/Y_{i0} = \beta_0 + \beta_1 Y_{it}/Y_{i0} + \beta_2 A_{it}/Y_{i0} + \beta_3 Self_{it} + \gamma \cdot X_{it} + \lambda_t + \mu_i + \varepsilon_{it},$$

where CG_{it} is annualized capital gains/losses, A_{it} is net wealth excluding home equity, Y_{it} is total family income. As a form of heteroskedasticity correction, these monetary terms are divided by Y_{i0}, the family income of household i in the last year prior to becoming self-employed. In addition to total reported income, we further control for net wealth because capital gains from investments may well be correlated with the net wealth of a family. For the dependent variable, in addition to total capital gains, we look at capital gains in three assets separately. To focus on the representative household, in this regression we exclude the top 1% and bottom 1% extreme values of total capital gains/losses.

Table 2.8 shows the regression results. Column 1 shows that, on average, a family would have higher capital gains after becoming self-employed, though not statistically significantly. Column 2 shows that, as expected, a family would have significantly more business/farm capital gains when it becomes self-employed. The capital gains from real estate and stocks are less than before becoming self-employed, though not statistically significantly. It seems that there is some evidence that the self-employed transfer ordinary income to capital gains

Table 2.8. Change of capital gains (since 1984)

Dep var:	capital gains/pre-self income	busi/farm capital gains/pre-self income	real estate capital gains/pre-self income	stocks capital gains/pre-self income
	[1]	[2]	[3]	[4]
total income/pre-self income	0.039	-0.056	0.087	0.008
	(0.032)	(0.084)	(0.080)	(0.008)
net wealth without home	0.048**	0.017	0.023	0.008*
equity/pre-self income	(0.019)	(0.012)	(0.014)	(0.004)
self-employed	0.06	0.108***	-0.021	-0.027
	(0.055)	(0.041)	(0.034)	(0.029)
Other controls	Yes	Yes	Yes	Yes
Family fixed-effect	Yes	Yes	Yes	Yes
Observations	3,785	3,785	3,785	3,785
R-squared	0.262	0.284	0.346	0.217
	[5]	[6]	[7]	[8]
total income/pre-self income	0.039	-0.056	0.087	0.008
	(0.032)	(0.084)	(0.080)	(0.008)
net wealth without home	0.047**	0.016	0.023	0.008*
equity/pre-self income	(0.019)	(0.012)	(0.014)	(0.004)
corp self-employed	0.104	0.195**	-0.082	-0.009
	(0.103)	(0.086)	(0.068)	(0.037)
non-corp self-employed	0.048	0.084**	-0.004	-0.032
	(0.056)	(0.038)	(0.034)	(0.031)
Other controls	Yes	Yes	Yes	Yes
Family fixed-effect	Yes	Yes	Yes	Yes
Observations	3,785	3,785	3,785	3,785
R-squared	0.262	0.285	0.346	0.217

Notes: The sample in columns 1-8 focuses on the first employee-self-employed cycle of families ever turning from employees to self-employed within sample period, and excludes the top 1% and bottom 1% extreme values of total capital gains. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, and current family scale. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

(business/farm capital gains in particular) income after becoming self-employed. Columns 6 shows that business/farm capital gains increase more for the corporate self-employed, as expected.

Then we estimate the scale of tax avoidance due to shifting income to capital gains. Here we make several assumptions. First, we assume that the imputed accrued capital gains is a good proxy for realized capital gains. Since we focus on the families turning from employees to self-employed and end their business in the end, all accrued capital gains should be realized within the sample period. Second, we choose to focus only on the capital gains from business/farm and assume they are long term capital gains, so subject to a lower tax rate than the ordinary income. Third, we focus on the cases when non-negative net capital gains. Any capital losses that help deduct income at ordinary income account has been considered when we calculate tax liability using TAXSIM. When we calculate corporate income, we average business life time capital gains and losses to calculate the net capital gains per business year. Such approach would help deal with the carryover of capital losses for corporations.

Columns 5 of table 2.7 show that the scale of type III avoidance plus type I avoidance considering both dividends and capital gains is on average [1-exp(-0.129)]*exp(7.955)=$344.9 for the self-employed, which implies a $152.21 tax saving due to shifting income to capital gains. Similarly, we obtain that the tax saving is $275.51 and $129.69 for corporate and non-corporate self-employed, respectively.

Testing a relevant assumption

Our identifying assumption is that the self-employed should have the same tax liability as the employees, conditional on reported total income, if there were no behavioral changes (e.g. shifting income and manipulating exemptions and deductions) after a family becomes self-employed. Then we can interpret the lower tax liability of the self-employed conditional on reported income as avoidance. This assumption is fundamentally untestable. But we can examine a relevant assumption: conditional on reported total income, the ever self-employed families should have the same tax liability, before they became self-employed, as the always-employee families. If this is untrue, then the families choosing to become self-employed would have different income structure from the always-employee families when they were both employees, which would make our conceptual framework to estimate tax avoidance scale invalid.

To test this hypothesis, we focus on the sample of employee observations (using always-employee families and ever self-employed families) and run the following regression

$$lnTax_{it} = \beta_0 + \beta_1 lnY_{it} + \beta_2 Self_i + \gamma \cdot X_{it} + \lambda_t + \varepsilon_{it},$$

where Tax_{it} can be tax liability ignoring dividends and capital gains, tax liability considering only dividends, or that considering both dividends and capital gains. From column 2-4 of table 2.16 we see that β_2 is close to zero in all three cases, which renders support for our identifying assumption.

2.4.5 Personal income tax saving from evasion v.s. avoidance

Previously we estimate the scale of income under-reporting and several types of tax avoidance. To compare the tax saving scale due to evasion versus avoidance, we need to come up an estimate of tax evasion scale. The idea to estimate it is as follows: conditional on pre-tax reported income, the reduction in tax liability reflects the scale of tax avoidance when a family becomes self-employed; conditional on pre-tax true income, the decrease in tax liability when a family becomes self-employed reflects the total tax saving from evasion and avoidance. Then we can infer the tax saving scale due to tax evasion from these two estimates.

The procedure to estimate the tax evasion is as follows. First, we run the same regression as we estimate the income under-reporting scale, except that now we distinguish corporate self-employed from the non-corporate self-employed:

$$lnY_{it} = \beta_0 + \beta_1 lnC_{it} + \beta_2 CorpSelf_{it} + \beta_2 NonCorpSelf_{it} + \gamma \cdot X_{it} + \lambda_t + \mu_i + \varepsilon_{it},$$

where Y_{it} is after-tax reported income. Throughout this part, tax liability is calculated ignoring dividends and capital gains and hence only type III avoidance is considered here. Second, based on the estimated coefficients of the corporate self-employed and the non-corporate self-employed indicators, we predict the after-tax true income for all the self-employed observations. Adding tax liability to this after-tax true income renders before-tax true income for the self-employed. For the employees, the before-tax true income is simply their before-tax reported income, given the assumption they do not under-report income. Finally, conditional on before-tax true income, the tax liability difference between employees and the self-employed is tax evasion plus type III avoidance scale. Then tax evasion scale can be inferred by comparing with the type III avoidance scale estimated earlier.

Table 2.9 shows the result of estimating tax evasion scale. We focus on post-1992 sample to make our results comparable to the tax avoidance estimates. For the self-employed, on average, the tax saving due to type III avoidance is $184.39, while that due to both evasion and type III avoidance is [1-exp(-0.201)]*exp(8.013)=$549.9. So the tax saving due to evasion is $365.51.

Table 2.10 summarizes the personal income tax saving scale from evasion versus several types of avoidance. Since we find no evidence for the empirical importance of type II avoidance (income deferral), we set it to be zero. The tax saving due to shifting ordinary income to dividends is also minimal. Tax saving due to shifting ordinary income to capital gains seems quite important, especially for the corporate self-employed, who are expected to have more flexibility to shift income. Type III avoidance (manipulating exemption and

Table 2.9. Estimating tax evasion + type III avoidance scale (since 1992)

Dep var: log(tax liability ignoring dividends and capital gains)	[1]	[2]
log(predicted before-tax true income)	1.600***	1.600***
	(0.047)	(0.048)
self-employed	-0.201***	
	(0.030)	
corp self-employed		-0.159***
		(0.034)
non-corp self-employed		-0.216***
		(0.034)
Other controls	Yes	Yes
Family fixed-effect	Yes	Yes
Observations	4,717	4,717
R-squared	0.923	0.923
ymean of employees	8.013	8.013

Notes: The sample for columns 1-4 focuses on the first employee-self-employed cycle of families ever turning from employees to self-employed within sample period. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** p<0.01, ** p<0.05, * p<0.1.

deduction) seems also important and is comparable to the tax saving due to capital gains. Overall, avoidance helps the self-employed as a whole save $344.90 tax liability while evasion helps save $365.51.

By decomposing the self-employed into those with corporate business and those without, we see large heterogeneity. For tax saving purpose, it seems that evasion has played a more important role for the non-corporate self-employed, while avoidance seems to be more important for the corporate self-employed. These results are largely consistent with our intuition. First, non-corporate self-employed are more likely to evade tax because they are usually much smaller and less likely to be inspected by the tax bureau than the corporate self-employed. Second, corporate self-employed families may have larger businesses and have

Table 2.10. Personal income tax saving from evasion v.s. avoidance (1979 dollars)

	Avoidance					Evasion
	Type I avoidance (income shifting)		Type II avoidance (income deferral)	Type III avoidance (exemption & deduction)	Overall avoidance	Overall evasion
	dividends	capital gains				
self-employed	8.3	152.21	0.00	184.39	344.90	365.51
corp self-employed	13.57	275.51	0.00	246.09	535.17	197.86
non-corp self-employed	5.55	129.69	0.00	161.61	296.85	425.06

Notes: The figures here are based on the estimates in previous tables.

more professional lawyers help them deal with tax filling problems, which would make them in a better place to use various ways to avoid taxes.

2.4.6 Beyond personal income tax – taking corporate tax and payroll tax into account

Besides personal income taxes, we are also interested in how other relevant taxes change when a family becomes self-employed. Here we consider payroll tax and corporate income tax.

In general, all the non-corporate firms are taxed similarly. Non-corporate businesses mainly take the form of sole proprietorships (single owner) and partnerships (multiple owners). Like S corporations, they are considered as "pass-through" (or "flow-through") entities, i.e. all their profits and losses pass through the business to the business owners or shareholders, who pay individual income tax on their share of the profits (or deduct their share of the losses) (Carroll and Prante (2011)). In general, they are taxed under personal income account and the major channel for them to avoid tax via income-shifting is shifting ordinary income to capital gains.

By contrast, different forms of corporations are taxed differently. The U.S. corporations choose C or S status for federal tax purposes. C corporations pay the corporate income tax on annual taxable income, and U.S. shareholders pay dividend taxes on issued dividends and capital gains taxes on qualified share buybacks. By contrast, S corporations have the same legal structure as C corporations but for tax purposes are pass-through entities that are levied ordinary income tax rather than corporate income tax. Another major difference is that, for S corporations, dividends are untaxed when distributed. When it comes to the payroll tax, while a C corporation self-employed pays payroll tax on its whole business income, an S corporation self-employed only pays payroll tax on wages and salaries (but needs to pay both employee and employer shares).[11]

To sum up, considering payroll tax and corporate tax brings about several further considerations to our framework of estimating tax saving from avoidances v.s. evasion. First, there is an additional way of income shifting (type I avoidance) for C corporations. For non-corporate firms and S corporations, income shifting happens through shifting ordinary income to dividends and capital gains. For C corporations, they can also save tax through converting ordinary personal income into corporate income, and in the process pays additional corporate tax on the accrued corporate income. Second, S corporations could reduce payroll tax on income not paid out as wages, giving an additional channel for tax savings from avoiding wage payments.

Given the above discussion, it is important to distinguish between C and S corporations for tax purposes. Due to the lack of information in PSID to identify whether a corporation is C or S status, we take an ex-post approach: assume one would choose the organizational form that minimizes its tax burden given the income structure. Of course, tax minimizing is not the solely important goal for an agent.[12] Our approach tends to exploit the extent how otherwise similar businesses could have benefited via minimizing their tax burdens.

[11] The tax treatment of these two types of corporations differ in some other smaller places. For a more detailed comparison between C corporation and S corporation, see Yagan (2015) section II.A.

[12] For example, Gordon and MacKie-Mason (1994) explore both tax and non-tax factors that determine the organizational form choice of firms.

Other complications.

Several additional complications might make the firms not taxed as the form it is listed. First, introducing organizational form change would complicate the analysis. For example, following an organizational form shift, any unrealized capital gains remain subject to the previous tax regime. However, organizational form change is relatively rare.[13] In this paper, we do not address the potential organizational form change and assume all the businesses are taxed as their current organizational forms.

The second complication is brought about by the "check-the-box" rule (adopted in 1997 and restricted only to certain types of firms), under which an eligible business entity would be able to select its federal income tax classification, i.e. whether to be taxed as a corporation or as a partnership. Then some non-corporate firms can "check-the-box" and choose to be subject to corporate tax rules even while remaining non-corporate for all non-tax purposes. Doubtlessly, such rule provides an additional avenue for tax avoidance, since the qualified firms can claim the form that renders a lower tax liability. But it is mainly relevant to firms with foreign subsidiaries, i.e. the multinational firms, and thus particularly useful for cross-border tax planning.[14] For the families in our PSID sample, however, we do not think cross-border tax planning is of major importance. Thus, we tend to ignore the tax avoidance brought about by the "check-the-box" rule.

A third complication is brought about by the existence of limited liability corporations (LLCs). The LLCs are pass-through business entities. In general, single-owner LLCs are taxed just like sole proprietorships, and multiple-owner LLCs are taxed just like partnerships. In the late 1980s and 1990s the LLCs combined pass-through tax treatment with the limited liability

[13]For example, Yagan (2015) finds that fewer than 4% of corporations in his sample ever switched between C and S status. One potential reason is that not all firms are qualified to such a switch. Major factors restricting a corporation to qualify for S status include that the corporation must have no more than 100 shareholders, all shareholders must be U.S. citizens or residents and not business entities, and the corporation should have only one class of stock. Moreover, consecutively switching is restricted by law and switching and is even rarer.

[14]For example, multinationals can use disregarded loans to strip earnings out of high-tax jurisdictions and relocate those profits to low- or even no-tax countries.

feature of the corporate form, and the classification of businesses as LLCs was simplified in 1997 by allowing them to check the box on Form 1065-B to elect to be treated as a corporation or partnership/sole proprietorship for tax purpose.[15] By assuming away the "check-the-box" rule, as we argued above, the LLCs can be generally regarded as non-corporate businesses for tax purposes.

Corporate taxes

For the corporate self-employed, they can shift income from ordinary income to dividends or (long-term) capital gains. By this way, they save the personal income tax due to lower tax rate on dividends and (particularly) capital gains, while still need to pay the corporate tax on the total business income when such income is accrued.[16] Due to the corporate tax due, the total tax saving would be less than that if we only consider the personal income tax due.

One obstacle to take corporate tax into account is that we do not directly observe corporate income from family-owned business in PSID. However, since we focus on the whole business life, and all corporate income finally would be distributed in terms of dividends or capital gains, we can infer the corporate income in the whole business life based on the lifetime net capital gains and dividends. Note the corporate tax schedule is progressive. In theory, companies can subdivide into multiple firms so that their income could be taxed at the rate of the lowest tax bracket. For simplicity, we assume that firms do exploit such advantage to enjoy the lowest corporate tax rate. Then we can infer the corporate income in each business year as $\frac{\sum_j (D_j + CG_j)/n}{1-t}$, which leads to a corporate tax of $\frac{t[\sum_j (D_j + CG_j)/n]}{1-t}$, where $\sum_j (D_j + CG_j)/n$ is the average dividends plus capital gains/losses during the whole

[15] In the tabulations from the Census Bureau's Statistics of U.S. Business Division and Nonemployer Statistics, all businesses are divided into organizational forms of S corporation, C corporation, partnership, and sole proprietorship, without listing the LLC separately (Carroll and Prante (2011)).

[16] The corporate tax is paid each year on accruing corporate income, and not when money is distributed as dividends or used to repurchase shares. What is left net of corporate tax can then be distributed as dividends, used to repurchase shares, or retained resulting in capital gains.

self-employed business life, t is the lowest federal corporate income tax rate.[17]

Payroll tax

Federal Insurance Contributions Act (FICA) tax is a United States federal payroll (or employment) tax imposed on both employees and employers to fund Social Security and Medicare. Social Security benefits include old-age, survivors, and disability insurance (OASDI); Medicare provides hospital insurance (HI) benefits for the elderly.

For regular employees, take 2010 for example, the employee's share of Social Security portion is 6.2% of gross compensation (wage and salaries) up to a limit of $117,000 of gross compensation (resulting a maximum Social Security tax of $7,254). The employee's share of the Medicare portion of the tax is 1.45% of wages, with no limit on the amount of wages subject to the Medicare portion of the tax. The employer is also liable for 6.2% Social Security and 1.45% Medicare taxes, making the total Social Security tax 12.4% of wages and the total Medicare tax 2.9%.

For the self-employed, a tax similar to the FICA tax, called SECA (Self-employment Contributions Act) tax, is imposed on their earnings, including labor income as well as any capital gains income accruing to the business. So they may not easily avoid payroll tax. Given the success of business, they might pay more payroll tax than as an employee. They are responsible for the entire percentage of 15.3% (12.4%+2.9%), which is applied to 92.35% (applied since 1990) of the business net earnings from self-employment, rather than 100% of the gross earnings; the difference, 7.65%, is half of the 15.3%, and makes the calculation fair

[17]Here we implicitly assume that the imputed capital gains are realized. In reality, the imputed capital gains include both realized and unrealized parts. For example, Looney and Moore (2015) also regard the imputed capital gains from current values of wealth as including unrealized capital gain. However, it is hard to identify what fraction of the imputed capital gains is realized versus unrealized. Alternatively, if we assume that all the imputed capital gains are unrealized, the inferred corporate income in each business year would be $\frac{(\sum_j D_j)/n}{(1-t)} + \frac{\sum_j (CG_j)}{n}$, and the associated corporate tax would be $t[\frac{(\sum_j D_j)/n}{(1-t)} + \frac{\sum_j (CG_j)}{n}]$. The actual case would lie between these two extreme cases. On the one hand, assuming the imputed capital gains are realized would overstate the corporate tax liability, on the other hand, by assuming the corporations are exploring the lowest corporate tax rate tend to underestimate the corporate tax liability. Overall, such a combination simplifies the calculation while also possibly renders a corporate tax liability close to reality.

compared to that of regular employees.

If a self-employed owns an S corporation, he pays payroll tax only on the wages (but needs to pay both employee and employer shares), i.e. paying FICA. So he can avoid payroll tax by transferring income from wage payments to capital gains and dividends which are not subject to the payroll tax. For the self-employed owning a C corporation, there is no such advantage; he pay SECA.

TAXSIM provides FICA tax liability estimate as sum of the employer and employee shares of payroll taxes, ignoring the favorable treatment of self-employment income. So we calculate the payroll tax liability for the employees using TAXSIM, following Bruce (2002) to assume that workers are also responsible for both employer and employee contributions. For the S-corporation self-employed, we calculate their payroll tax also using TAXSIM, since they only pay payroll on wage/salary. For the self-employed except for S-corporations, we calculate their payroll tax based on their net business earnings and self-employment payroll tax schedule.[18]

Incorporating corporate tax and payroll tax

Distinguishing between C and S corporations.

From the above discussion, we see that distinguishing between C and S corporations is important for tax purposes. Our approach to distinguish them is as follows: For all the corporate self-employed, we calculate the tax liabilities (personal income tax, payroll tax, and corporate tax for C corporations) associated with C and S status separately, and then compare them to infer the status a firm would choose to minimize tax burden.

The result implies that 30% of corporate self-employed is S corporations (1992-2010 as dividends information available since 1992). Although we can never ensure whether we identify their true organizational forms, we can compare the inferred fraction of S corporations

[18] Our calculation is based on the payroll tax rates and the contribution and benefit bases in the Social Security Administration website. See http://www.ssa.gov/oact/progdata/taxRates.html and http://www.ssa.gov/oact/COLA/cbb.html#Series.

in all corporations in our sample with the actual fraction. For example, Yagan (2015) shows that the fraction of S corporations in all corporations is 42% for 1996-2008 and Carroll and Prante (2011) show fraction of 32% for 2008. It seems that our identification of S and C corporations does not differ much from the reality. Note that when a business experiences a loss, the business owner would have incentive to register as an S corporation so that it can deduct the losses under personal income tax account. That the fraction of S corporations we obtained is slightly lower than the realistic fraction suggests that the corporation in our sample is more profitable than the average corporations.

Estimating type I avoidance scale considering corporate tax

Accounting for corporate tax would lower the benefit of income shifting for corporations (in particular for C corporations) because it increases the cost of income shifting by paying additional corporate tax on the accrued corporate income corresponding to shifted income. To estimate the type I avoidance considering corporate tax, we adopt a similar approach like before as follows.

Assumption 3B'. Conditional on pre-tax reported total income (personal income + corporate income), employees and the self-employed should have the same tax liability that counterfactually assumes dividends and capital gains were ordinary income, if there were no type II-III avoidances.

Assumption 3C'. Conditional on pre-tax reported total income (personal income + corporate income), employees and the self-employed should have the same tax liability (personal income tax + corporate income tax), if there were no type I-III avoidances.

Assumptions 3B' and 3C' are revised versions of Assumptions 3B an 3C, respectively. Here Type I avoidance is more broadly defined as shifting income from ordinary income to corporate income, capital gains and dividends to save tax.

Under Assumptions 3A and 3B', we can estimate the tax saving scale due to type III avoidance. Under Assumptions 3A and 3C', we can estimate the tax saving scale due to type III avoidance and type I avoidance considering corporate tax. Then we can infer the type I avoidance scale considering corporate tax.

Table 2.11. Avoidance scale considering dividends, capital gains and CIT (since 1992)

Dep var:	log(tax liability assuming dividends, capital gains, CIT are taxed at ordinary income account)		log(tax liability considering dividends, capital gains and CIT)	
	[1]	[2]	[3]	[4]
log(total personal income + corporate income)	1.591***	1.592***	1.549***	1.552***
	(0.047)	(0.046)	(0.054)	(0.055)
corp self-employed		-0.080**		-0.183***
		(0.035)		(0.065)
non-corp self-employed		-0.061*		-0.110**
		(0.035)		(0.045)
self-employed	-0.066**		-0.124***	
	(0.031)		(0.042)	
Other controls	Yes	Yes	Yes	Yes
Family fixed-effect	Yes	Yes	Yes	Yes
Observations	4,723	4,723	3,381	3,381
R-squared	0.924	0.924	0.905	0.905
ymean of employees	8.013	8.013	7.955	7.955

Note: The sample focuses on the first employee-self-employed cycle of families ever turning from employees to self-employed within sample period. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Columns (3)-(4) focus on those with non-negative capital gains.

Table 2.11 shows the estimation results. Columns 1 and 2 imply that the type III avoidance scale for the average self-employed is [1-exp(-0.066)]*exp(8.013)=$192.88. Similarly, for the average corporate and non-corporate self-employed, it is $232.19 and $178.71, respectively. As expected, the estimated type III avoidance scale only slightly differs from previous estimates, as they are based on slightly different assumptions. Columns 3 and 4 imply that the type III + type I avoidance scale for the average self-employed is [1-exp(-0.124)]*exp(7.955)=$332.34, which then suggests the type I avoidance scale is 139.46. Similarly, the type I avoidance scales considering corporate tax are $244.39 and $118.14 for corporate and non-corporate self-employed, respectively.

Payroll tax

From table 2.12, we see that a family has significantly less payroll tax liability when it turns from an employee to corporate self-employed, while significantly more payroll tax when it becomes a non-corporate self-employed. On average, payroll tax liability is [1-exp(-0.328)]*exp(7.201)=$374.93 less, conditional on reported total income, when a family becomes corporate self-employed, while $210.76 more when a family becomes non-corporate self-employed. For the average self-employed, the payroll tax liability is $43.6 more than their being employees, though not statistically significant.

Tax savings from evasion versus avoidances

Now we can incorporate the payroll tax and corporate income tax in our comparison. Table 2.13 summarizes the results. Payroll tax is separately listed because it does not work similarly to other taxes. Rather than a real lifetime burden, the payroll tax works like an insurance. First, incorporating corporate tax slightly change previous estimates on type I avoidance scale. Second, the main finding is still that while corporate self-employed rely more on avoidance, the non-corporate self-employed rely more on evasion to save tax. Third, payroll taxes do not create much tax burden for the average self-employed. But the corporate self-employed pay much less payroll tax than their being employees, while the non-corporate self-employed pay a lot more. If we regard payroll tax as a real burden for the self-employed, then incorporating does provide additional benefit for tax purposes.

Table 2.12. Change in payroll tax

Dep var: log(payroll tax)	[1]	[2]
log(total personal income +	0.767***	0.768***
corporate income)	(0.032)	(0.031)
self-employed	0.032	
	(0.060)	
corp self-employed		-0.328***
		(0.082)
non-corp self-employed		0.146**
		(0.068)
Other controls	Yes	Yes
Family fixed-effect	Yes	Yes
Observations	3,942	3,942
R-squared	0.858	0.863
ymean of employees	7.201	7.201

Notes: The sample for columns 1-2 focuses on the first employee-self-employed cycle of families ever turning from employees to self-employed within sample period. We use 1992-2010 data sample. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

2.4.7 Testing against alternative hypotheses

Is it possible that the above evidence is also consistent with hypotheses other than tax evasion and avoidance? Certainly yes. Can we provide additional evidence to support our hypothesis of tax evasion and avoidance? The answer is also yes.

First, to account for the drop in reported income conditional on consumption when a family turns from an employee to self-employed, an alternative hypothesis is that the self-employed consume more with the same true income than employees due to more social activities. To test the tax evasion hypothesis against this hypothesis, we investigate the response of the supposed income under-reporting to tax rate change. If the supposed income under-reporting did represent tax evasion, then we expect to observe more under-reporting

Table 2.13. Tax saving (PIT+CIT+payroll tax) from evasion v.s. avoidance (1979 dollars)

	Avoidance				Evasion	Payroll tax
	Type I avoidance (shifting ordinary income to corporate income, dividends and capital gains)	Type II avoidance (income deferral)	Type III avoidance (exemption & deduction)	Overall avoidance	Overall evasion	
self-employed	139.46	0.00	192.88	332.34	475.94	-43.6
corp self-employed	244.39	0.00	232.19	476.58	252.53	374.93
non-corp self-employed	118.14	0.00	178.71	296.85	557.37	-210.76

Notes: The figures here are based on the estimates in previous tables.

when tax rate is higher, since higher tax rate increases the marginal benefit of evading tax. Such a test has the same spirit as that did by Gorodnichenko et al. (2009), who show that the reported consumption-income gap (as a proxy for tax evasion) decreased when the flat tax reform (a uniform drop in marginal PIT rate for higher income earners) was adopted in Russia. Without a similar simple tax reform in the U.S., we explore the change in highest statutory PIT rates to test our hypothesis. The advantage of using statutory highest tax rate rather than the effective tax rate of each family is in its exogenous property.[19] Although the general families may not reach the highest bracket, the change in statutory highest tax rate may still well reflect the change in tax rate a family actually faces, since tax rates in all brackets often increase or decrease together.

To test this we apply the following specification:

$$lnY_{it} = \beta_0 + \beta_1 lnC_{it} + \beta_2 Self_{it} + \beta_3 Self_{it} \cdot PIT_t + \gamma \cdot X_{it} + \lambda_t + \mu_i + \varepsilon_{it},$$

[19] I have also tried to use the effective marginal tax rate (EMTR) instrumented by the predicted EMTR based on income in previous years to test the hypothesis, which renders the opposite sign of β_3. I guess the endogeneity problem due to the strong positive correlation between income and EMTR is hard to break out even when the traditional instrument is applied. A method to solve this problem might be contributing to an independent paper. Here I thus only show some suggestive evidence.

where PIT_t is the statutory highest personal/corporate income tax rate (in decimal). We expect to see a negative sign of β_3 since higher tax rate would increase the benefit of evading tax.

Second, to account for the drop in labor income fraction (i.e. the increase in asset income portion) conditional on reported total income when a family turns from an employee to self-employed, an alternative hypothesis is that the income structure naturally changes when a family becomes self-employed. To test the tax avoidance hypothesis against this, we investigate the response of asset income to the change in PIT-CGT (long-run capital gains tax) rates gap, conditional on total reported income. If the increase in asset income corresponds to income-shifting for tax saving purposes, then when the PIT rate increases relative to the CGT rate, the self-employed families would shift more ordinary income to capital gains. The reason we look at asset income is that it includes all capital gains income and even in years that we cannot obtain an estimated capital gains, PSID provides a consistent measure of asset income. A caveat is that while the (long-term) capital gains face a lower tax rate, the other parts in asset income face the same marginal tax rates as ordinary income.[20] So again we are proposing a suggestive test here. To do this we apply the following specification:

$$lnAssetInc_{it} = \beta_0 + \beta_1 lnY_{it} + \beta_2 Self_{it} + \beta_3 Self_{it} \cdot (PIT_t - CGT_t) + \gamma \cdot X_{it} + \lambda_t + \mu_i + \varepsilon_{it},$$

where $AssetInc_{it}$ is family asset income, $PIT_t - CGT_t$ is the rates gap between the PIT and the CGT. We expect to see a positive sign for β_2 and β_3.

Table 2.14 shows the results. In column 5 we exclude corporate self-employed because

[20] An exception is that since 2003 dividends income have faced a lower tax rate. But the post-2003 data only consists a small part in our 1967-2011 sample. By excluding the post-2003 period, our result does not change.

Table 2.14. Responses of evasion and avoidance to tax changes

Dep var:	log(total income)		log(asset income)		
	[1]	[2]	[3]	[4]	[5]
log(total consumption)	0.467***	0.467***			
	(0.037)	(0.037)			
log(total income)			1.063***	1.063***	1.103***
			(0.092)	(0.092)	(0.085)
self-employed	-0.112***	-0.074	1.651***	1.485***	1.961***
	(0.019)	(0.052)	(0.108)	(0.161)	(0.176)
PIT rate*self-employed		-0.077			
		(0.092)			
(PIT-CGT rate)*self-employed				0.747	0.769
				(0.577)	(0.602)
Other controls	Yes	Yes	Yes	Yes	Yes
Family fixed-effect	Yes	Yes	Yes	Yes	Yes
Observations	16,428	16,428	16,430	16,430	14,870
R-squared	0.691	0.691	0.593	0.593	0.626

Notes: The sample in column 1-8 focuses on the first employee-self-employed cycle of families ever turning from employees to self-employed within sample period. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** p<0.01, ** p<0.05, * p<0.1.

corporations (in particular C corporations) face corporate tax, which may affect their income-shifting activities in a non-linear way. Doing this does not change much of our results. We see expected signs in both tests, though the estimates are not precise because the changes in statutory tax rates are yearly and the highest rate may not well apply to the actual bracket people face. We thus regard this result as a preliminary evidence to support our tax evasion and avoidance hypotheses, rather than draw a definitive conclusion. Controlling for family fixed-effect or not does not change our results.

We make some preliminary interpretation on our estimates. During the 1986 tax reform, the highest PIT rate decreases from 0.5 in 1986 to 0.28 in 1988, with a transitional rate 0.385 in 1987. Column 2 implies that the under-reported income decreased by $0.077*(0.5-0.28) = 0.017 = 1.7\%$ due to this tax change. During the 1986 tax reform, the PIT-CGT rates gap decreased from 0.3 in 1986 to 0 in 1988, with a transitional rate gap 10.5 in 1987. Column 4 implies that this tax reform had the self-employed families reduce income shifting by $0.747 * 0.3 = 0.224$, i.e. 22.4% of their total income would have been transferred from asset to labor account during this tax reform.

2.4.8 Robustness checks and discussions

Sample selection

The main sample used in this paper is families turning from employees to self-employed. What if we include firms that were already in existence initially? In row 9 and row 10 in table 2.3 we use overall sample and obtain estimates of income under-reporting fraction close to our main estimate, which provides evidence that considering a broader sample won't affect the main results.

Use the same tax formula for a household

The tax liability for our main regressions are calculated using the current year tax formula. One concern is that the tax liability change could be due to either behavioral responses or simply due to mechanical tax formula change. To address such concern, we use the same tax formula for a household along its employee-self-employed cycle. In particular, we use the tax formula in the last year before a family turns from an employee to self-employed. Table 2.17 shows that using the same tax formula does not affect our main results. Although the concrete numbers change, the main result that the corporate self-employed rely more on avoidance while the non-corporate self-employed rely more on evasion remains. Despite the similar results, we use tax liability calculated using current year tax formula because it would allow us to compare the tax liability of the households turning from employees to self-employed with other households. For example, if we use the tax formula in the last year before a family turns from an employee to self-employed to calculate the tax liability, the tax liability for the always-employee and always-self-employed households will be unable to calculate.

Accounting for both federal and state tax liability

Since the TAXSIM model includes state tax code from 1977, to best utilize all years available PSID data, we focus on federal tax liability throughout the paper. Using overall tax liability including both federal and state tax for the post-1977 sample renders similar result.

Controlling for net wealth

Previous papers, e.g. Hurst et al. (2014), do not include wealth in their specification. One reason is that wealth information is not available in most years. Another reason is that wealth may be a function of other socioeconomic characteristics such as age and education. Thus by controlling X_{it} we have included a good portion of wealth. In this session, we try to directly control for net wealth for years when such information is available. We follow Hurst and Lusardi (2004) to use the fifth-order polynomial of net wealth (in 100000 dollar unit), as an approximation of some nonlinear wealth model. We also try to include a third-order polynomial because it may suffice to approximate a nonlinear wealth function.

Table 2.18 shows the results. In all regressions we focus on the observations with wealth information. Comparing columns 4-6 to columns 1-3, we see that controlling for family fixed-effect would have our estimates of evasion close to that obtained using the full sample but without controlling for net wealth. Based on the F.E. regressions in columns 5 and 6, we see that around 8-9% income is under-reported, whether we control for the third-degree or fifth-degree polynomial of net wealth. These estimates are close to that obtained in column 4, which does not control for net wealth at all. This suggests that by controlling the socioeconomic characteristics and family fixed-effect, we may have controlled for most information that net wealth would offer. Thus, we could be confident with results obtained without controlling for net wealth. Controlling the log form of net wealth or negative net wealth dummy do not change our results.

Different measures of consumption

The life-cycle consumption model implies a log-linear Engel curve relation between total income and total consumption, which motivates our using of total consumption as our main consumption measure. Previous research (Pissarides and Weber (1989), Hurst et al. (2014)) instead focuses on food consumption because food consumption is consistently available over most years. Replacing total consumption by food consumption, we get an income under-reported fraction of 4.9%, lower than the 10% estimate using total consumption.

Spousal wage/salary

When the head becomes self-employed, the wife might have behavioral responses. A possible response is that the wife may increase labor supply as precautionary behavior, in case that the head might not be successful in his self-employed business. In fact, many businesses experience losses in the beginning. Consider when the head becomes self-employed, his income decreases while his wife makes compensated income increase, which makes the total reported income largely unchanged and consumption increase due to potential expenditure increase for the self-employed, as we observe in our data. If that is true, then the evasion explanation may not hold. We thus focus on the households with employee wives, and examine whether spousal wage/salary increases when the head becomes self-employed. We find that on average, the spousal earnings do not change much. Even in the first year when the head becomes self-employed, the change in spousal income is not significantly positive (table 2.19).

Occupation change within an employee status

The main framework we use in this paper is to compare the consumption-income relation around a family's head turns from an employee to an self-employed. By doing this, we ignore the potential heterogeneity within employees. Consider if a person faces job change or occupation change as an employee leading to comparable changes in consumption as the person turns from an employee to self-employed, what would happened to the reported income and tax liability? If the income-consumption relation changes when a person changes occupation/job as an employee, then our framework should be revised to a comparison with the person who experience occupation/job change as an employee.

To explore such possibility, we examine the income-consumption relation when a head experiences occupation change as an employee. From 1968 to 2001 waves, PSID provides consistent 3-digit 1970 census code, for 2003-2011 the 3-digit 2000 census code is adopted. Since the match between the two codes are complicated and imperfect, we focus on the 3-digit occupation using 1968-2001 waves data for such analysis. We find that when the head experiences an occupation change as an employee, his income and consumption do not change. Furthermore, there is no change in his reported total income conditional on consumption, and no change in his tax liability conditional on total income. These results suggest that our framework is appropriately used and there is no need to compare with the the person who experience occupation/job change as an employee (table 2.20).

Accounting for transitory income shock

In our main specification, we implicitly assume that any observed change in income is permanent. But the more transitory the change in income, the larger change in income we would observe conditional on consumption. This raises the necessity to explicitly deal with the problem of transitory income shock. Both Pissarides and Weber (1989) and Hurst et al. (2014) have done this, but with some difference. Pissarides and Weber (1989) take a more parametric approach towards the income process that adjusts for the greater volatility of transitory income fluctuations for the self-employed. Ignoring the differences in variance between groups could overestimate the extent of under-reporting. Hurst et al. (2014) in their online appendix show that adjusting for the differences in variance using the Pissarides-Weber model decreases the estimated under-reporting only slightly. But Pissarides and Weber (1989) only adjust for differences in transient income volatility. Hurst et al. (2014) show that if self-employed permanent income is also more volatile than for employees, this attenuates the bias. After also correcting for differences in permanent income volatility, the estimates is close to original estimates. Using the same data (PSID) and fundamentally the same assumption as in Hurst et al. (2014), we tend to rely on their result to justify our approach in this paper.[21]

2.5 Conclusion

In this paper we have made some first attempts to estimate the scale of tax evasion and avoidance using panel survey data. We estimate the scale of income under-reporting, tax evasion and avoidance for families when they become self-employed. Compared with previous literature (Hurst et al. (2014)) using survey data to estimate income under-reporting scale of the self-employed relative to employees, we obtain an estimate much closer to that implied by the IRS tax audit data. Another finding is that for tax saving purpose, evasion may be more important for the non-corporate self-employed, while avoidance may be more important for the corporate self-employed. The rich information contained in PSID allows us to examine several aspects of family tax saving behavior. But there are also limitations. One limitation is that since the corporate income is not directly observed from PSID data, we infer it based on capital gains and dividends information. Another limitation is that since S or C status of a corporation is not reported in PSID, we take an approach assuming the corporation would take the organizational form that minimizes tax burden. To concur these limitations, future research may consider using administrative data linking individual/household to firms, which has become a popular trend recently. This paper does not examine heterogeneity in evasion and avoidance across occupations and industries, which may be an interesting direction for future research.

Chapter 2 is currently being prepared for submission for publication of the material. The dissertation author, Xiaxin Wang, was the sole author of this paper.

[21] This can be tested more formally later as Hurst et al. (2014) did in online appendix.

2.6 Appendix

2.6.1 Data and construction of key variables

The PSID (Panel Study of Income Dynamics) was conducted annually from 1968 to 1996 and biennially since 1997. In general, the survey in year t reports information in year t-1.[22] The initial PSID survey contained two groups of households. 61% is representative of the US population and 39% is a supplementary low-income subsample called SEO. The Latino families existed only during 1990-1995. In 1997 there is a significant drop in the size of core SEO families due to the budget constraint. To focus on a representative sample of American population, we follow the literature to exclude the SEO, Latino, and Immigrant subsamples in our analysis. Our sample focuses on families with household head aged between 25 and 65.

Self-employed definition

Our main idea is comparing the self-employed and the employees, in that the incomes of employees are subject to third-party reporting and thus are not underreported while the self-employed generally under-report income. We follow previous literature to define a family as self-employed if the head is self-employed. The PSID asks whether an individual (head and wife) on their main job is only employed by someone else, only self-employed, or both employed by someone else and self-employed. We define a family as self-employed if the head reports being self-employed only. It would be ideal to use the self-employed information of the wife. But the PSID has wife employment status only starting in 1976 wave and all waves

[22] An exception is that in PSID survey the households are asked to report current weekly or monthly food expenditures. PSID generally starts annual survey in March and finishes it within months, with the majority of families surveyed in the early half of a year. This creates some ambiguity in interpreting the timing of consumption information. Some papers, e.g. Hall and Mishkin (1982), Blundell et al. (2004), record consumption surveyed in year t as information in year t-1. By contrast, some other papers, e.g. Hurst et al. (2014), Powell and Shan (2012), record consumption surveyed in year t as information in year t. The latter approach forces them to focus on data prior to 1997 waves because after 1997, the PSID started collecting data biennially, making them unable to match income and expenditure occurred during the same year. In this paper, we follow the tradition by Hall and Mishkin (1982), Blundell et al. (2004), and thus have the benefit to use PSID data in all years.

since 1979. For the purpose of using a consistent definition of self-employed family and using as many years as possible, we follow Hurst et al. (2014) and Attanasio and Pistaferri (2014) to drop the households whose head is an employee while the wife (if any) is self-employed. This drops few observations and does not affect our main result.[23] By excluding observations whose head is unemployed or retired, we contrast the self-employed versus the employee observations.

The PSID has information on whether any member in a family owns a business or has a financial interest in any business enterprise. Based on this, PSID further asks whether the business is a corporation or unincorporated business. We use this to identify a family as corporate v.s. non-corporate. When a family owns more than one business, we define the family as corporate owner if any of its businesses is corporate. PSID asks who owned the business, conditional on a family having business, but only for 1985-1993 waves and for waves since 1994. For waves since 1985, head is the owner of most businesses noting that we have excluded the families whose head is an employee while wife is self-employed. In the other very few cases, other family members owns the business. So we assume that head is the owner of all businesses owned by a family.

Consumption and income

The key variables for our paper are consumption and income variables. PSID contains food consumption expenditure information (food at home, away home and food stamps) in all years except in 1973, 1988, and 1989 waves. Rent payments are also available in all survey waves except in 1988 and 1989. Before 1999, the survey occasionally collected information on other consumption components such as home insurance, utilities, and child care. Starting from 1999 wave, more comprehensive information on consumption has been collected, now covering about 70 percent of non-durable spending from the national accounts, including health expenditures, utilities, gasoline, car maintenance, transportation, education, and child

[23]Hurst et al. (2014) also claim that none of their results are sensitive to whether these observations are included or not.

care. Attanasio and Pistaferri (2014) point out that the PSID seems much better aligned with NIPA (national income and product accounts) than the CEX. They cite the view in Blundell et al. (2012) that the CEX matches NIPA well for items that are also in the PSID, and matches very poorly the items that are not in the PSID. This motivates us to impute a comprehensive measure of consumption for the PSID observations in the whole period using the post-1999 consumption information. The imputation procedure will be discussed later.

We follow the PSID definition of family labor income as the sum of all labor incomes, including wages and salaries, any separate reports of bonuses, overtime, tips, commissions, professional practice or trade, market gardening, additional job income, and miscellaneous labor income, and the labor part from business and farm incomes.[24] Family asset income is the sum of various asset incomes, including alimony, rent, dividends, interest, roomers and boarders, trust funds and royalties, and the asset part from farm and business income. We follow the PSID defined family total income as the sum of taxable income for head and wife, transfer income for head and wife, taxable income of other family members, transfer income of other family members, social security income of head, wife and other family members. Taxable income includes incomes from assets, earnings, and net profit from farm or business.

We follow Attanasio and Pistaferri (2014) to replace topcoded values for family income with Pareto estimates.

Imputation of overall consumption

Food consumption is surveyed in PSID consistently over the whole period. Since 1999 wave, a more comprehensive consumption lists have been included in PSID. We follow the method of Attanasio and Pistaferri (2014) to impute overall consumption for the whole period.

[24]Given its wide adoption in literature (Butrica and Burkhauser (1997), Bruce (2000), Bruce (2002), Hurst et al. (2014), Powell and Shan (2012), Sarada (2015)), we regard the PSID provided labor income definition the least controversial for the purpose of comparing labor income between the self-employed and employees. In addition, this labor income variable is traditionally used for the wage/salary entry in the TAXSIM model when calculating tax liability (e.g. Butrica and Burkhauser (1997), Powell and Shan (2012)). We thus follow the previous literature to use the PSID provided labor income variable.

First we use the following equation to impute the logarithm of net consumption

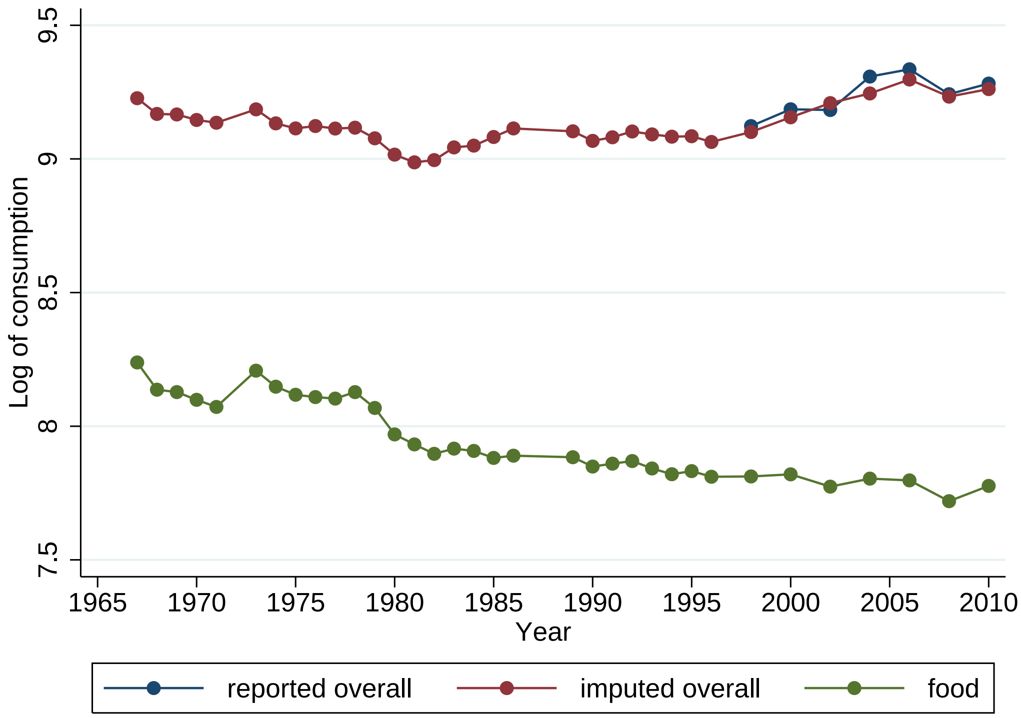
$$ln\, n_{it} = Z'_{it}\beta + p'_t\gamma + g(f_{it};\theta) + u_{it},$$

where $ln\, n_{it}$ is log of net-consumption (overall consumption excluding food, i.e. the sum of home insurance, electricity, heating, water, miscellaneous utilities, car insurance, car repairs, gasoline, parking, bus fares, taxi fares, other transportation, school tuition, other school expenses, child care, health insurance, out-of-pocket health, and rent[25]), Z_{it} are a list of socioeconomic variables, including age, education, marital status, race, state, number of kids (under 18), self-employed dummy, head's hours worked, home ownership, disability, family size. We control for relative prices p_t (overall CPI and the CPIs for food at home, food away from home, and rent) because the relative prices may affect how families allocate resources in food v.s. non-food consumption. $g(\cdot)$ is a (adopting third-degree following Attanasio and Pistaferri (2014)) polynomial function of food consumption f_{it} (sum of food at home, food away from home, and food stamps). u_{it} is error term. This equation can be approximately interpreted as a demand system that relates food consumption to net consumption.

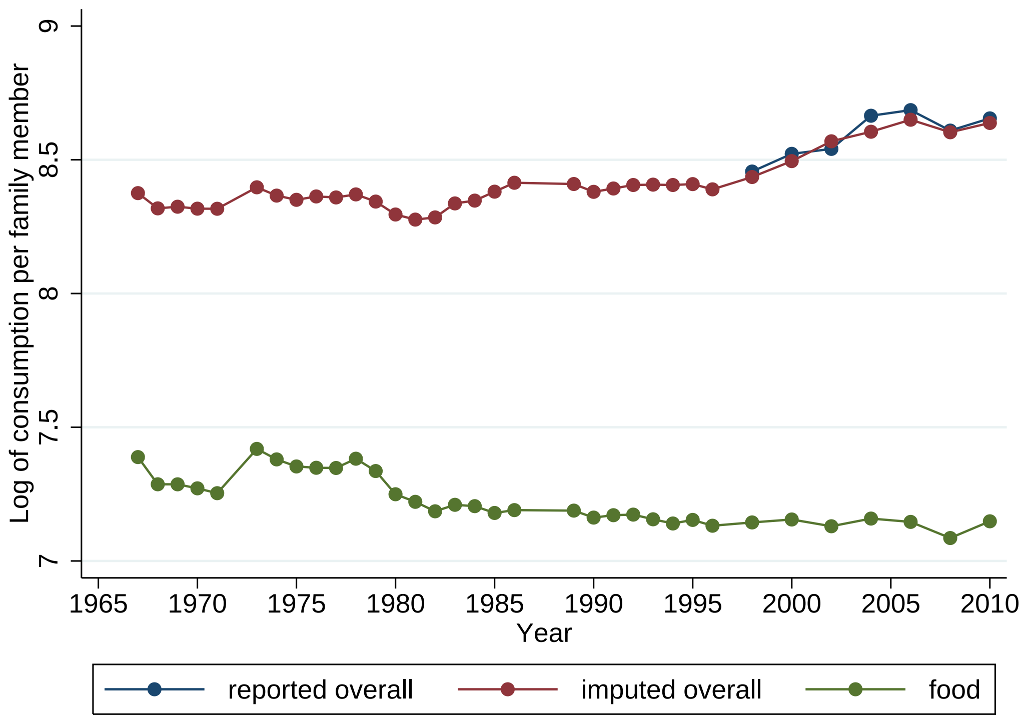
The above equation is estimated using data of 1999-2011 waves (i.e. 1998-2010 calendar years given that data are retrospective), which is the period we have a more comprehensive measure of consumption. Assuming that this relation applies to the whole period, at least after controlling for the relative prices, then we can impute the net consumption for the whole data period since we have consistent food consumption measure. We exclude 1972, 1987 and 1988 observations because in these years there is almost no information on food consumption. Then the imputed total consumption is $\widehat{C_{it}} = f_{it} + exp\{Z'_{it}\hat{\beta} + p'_t\hat{\gamma} + g(f_{it};\hat{\theta})\}$, where $\hat{\beta}$, $\hat{\gamma}$, and $\hat{\theta}$ are estimated using 1998-2010 observations.

Figure 2.1 drawn based on our imputation shows the evolution of consumption. The only difference between the two panels is that panel (b) looks at consumption adjusted by

[25] Rent is equal to actual payments for renters and an estimate of the rent equivalent for homeowners, which is equal to 6 percent of the self-reported house value (Flavin and Yamashita (2002)).



(a) Annual average of log(consumption)



(b) Annual average of log(consumption per family member)

Figure 2.1. Reported and imputed consumption

current family scale. Following Attanasio and Pistaferri (2014), we adopt the OECD adult equivalence scale to calculate the family scale as: $scale = 1 + 0.7 \times (\#adults - 1) + 0.5 \times \#kids$. While the family food consumption experiences a slightly decreasing trend over the whole period, the family per capita food consumption is relatively stable. The imputed total consumption fits quite well for the post-1998 observations.

Another method to impute the total consumption for pre-1998 observations is using the simultaneous CEX data. Since the CEX data have the most comprehensive cover of consumption, we could use it to impute total consumption for the PSID observations when CEX data are available. But compared to our current approach, using CEX data to impute total consumption has two limitations. First and foremost, as Attanasio and Pistaferri (2014) points out, the PSID seems much better aligned with NIPA than the CEX. They cite the view in Blundell et al. (2012) that the CEX matches NIPA well for items that are also in the PSID, and matches very poorly the items that are not in the PSID. This gives us a good reason to impute a comprehensive measure of consumption for the PSID observations in the whole period using the post-1998 consumption information. Second, the CEX data are only available from 1980 to 2010, thus not allowing us to impute total consumption for before-1980 observations in PSID data.

Tax liability

PSID provides family federal income tax data from 1968 to 1991 waves. These data are estimated by PSID editors using taxable income, number of exemptions (taking account of those over 65 or blind), using tables for single, married, and head of household which incorporate the average deductions from Statistics of Income tables published by the IRS. From 1992 forward it has been up to users to make these calculations. In this paper, we follow previous literature, e.g. Powell and Shan (2012) and Blundell et al. (2008), to use NBER's TAXSIM model to calculate tax liabilities for all years. Butrica and Burkhauser (1997) show that the TAXSIM tax liability estimates are close to the PSID provided estimates. In this paper, we use the tax liability obtained using TAXSIM model.[26] Throughout the paper, we focus on federal personal income tax because the TAXSIM model only calculates state taxes after 1977. As a robustness check, we consider the overall federal and state tax liability for the post-1977 sample.

Wealth

The PSID provides of a set of wealth measures in 1984, 1989, 1994 waves, and biennially since 1999 wave. See Hurst et al. (1998) for a full description of the PSID wealth data and each wealth component. Before 1999 wave, the PSID defines net wealth without home equity as the sum of farm/business value (profit if sold), checking and saving, real estate other than main home, stocks, vehicles, and other assets.[27] The net wealth including home equity then can be calculated simply as the sum of the above value and the home equity value. In this paper, we mainly use the net wealth without home equity. But using the alternative measure does not change our results. Since 1999, the money in private annuities or Individual Retirement Accounts (IRAs) has been included, and the PSID adds this component into the definition of net wealth. To keep a consistent definition, we exclude annuities/IRAs from the net wealth measure for the post-1999 observations. But keeping annuities/IRAs in the net wealth measure and use only post-1999 observations, the main results in our paper do not change.

Capital gains

Capital gains income is a profit that results from a disposition of a capital asset, such as stock, bond or real estate, where the amount realized on the disposition exceeds the

[26] Details on TAXSIM can be found in Feenberg and Coutts (1993). Butrica and Burkhauser (1997) describe how best to use the PSID data with TAXSIM. We follow the codes by Powell and Shan (2012) to prepare variables for applying TAXSIM model using PSID data. TAXSIM model includes state tax code starting in 1977. We focus on federal tax liability, which can be estimated in all years.

[27] Using 1984, 1989, and 1994 waves data, Hurst and Lusardi (2004) define household net wealth as the sum of savings and checking accounts, bonds, stocks, individual retirement accounts, housing equity, other real estate, and vehicles, minus all debt, which is quite similar to the net wealth definition provided by PSID.

purchase price. Conversely, a capital loss arises if the proceeds from the sale of a capital asset are less than the purchase price. The short-run (less than one year) capital gains income is subject to personal income tax rate, while the long-run (at least one year) capital gains income face a much lower rate. Due to this tax rate difference, shifting ordinary income to capital gains income may be one important way to avoid tax.[28]

Since the PSID survey does not ask capital gains income directly, and does not contain relevant information in most waves, previous papers using the PSID data simply set capital gains income (both long-run and short-run) as zero when calculating tax liability (e.g. Blundell et al. (2008), Powell and Shan (2012)). The PSID editors also adopt such assumption when estimating tax liability. This would normally underestimate tax liability.

But there is a small literature examining capital gains based on the wealth data provided by PSID (e.g. Juster et al. (2006), Bosworth and Anders (2008)). They impute capital gains based on the PSID wealth supplemental questionnaires. In 1989 and 1994 waves, respondents were asked about their active saving over the previous five years, defined as the net purchase of assets. These questions are specific to components of wealth where capital gains are most relevant, including (1) net investment in real estate other than home, (2) private businesses (including farms), and (3) corporate equities (stock in publicly held corporations, mutual funds, or investment trusts). We extend their approach to obtain active savings in later periods. The capital gains (passive saving) then can be computed as the change in net wealth minus active savings for each asset for waves 1984-89, 1989-94, 1994-99,

[28] Capital losses are subject to deduction, but with limits. Losses on investments are first used to offset capital gains of the same type. Short-term losses are first deducted against short-term gains, and long-term losses are deducted against long-term gains. Net losses of either type can then be deducted against the other kind of gain. If one has an overall net capital loss for the year, one can deduct up to $3000 of that loss against other kinds of income, including salary and interest income, for example. Any excess net capital loss can be carried over to subsequent years to be deducted against capital gains and against up to $3000 of other kinds of income. If a firm is not corporate, it can deduct losses like individuals, if it is corporate, the losses can be carried forward or carried back. If current capital loss is more than such limit, one can choose to carry back the loss on paid tax in previous years or carry the loss forward to later years. In general, the federal code allows 2 years for carry-back and 20 years for carryforward, while states vary widely over the carryover limits. See http://taxfoundation.org/blog/corporate-net-operating-loss-carryforward-and-carryback-provisions-state.

and subsequent two-year periods.[29] The overall capital gains is calculated by summing up the capital gains of three assets.

The five-year and two-year capital gains cannot be compared directly. To obtain comparable annual capital gains, we choose to annualize the capital gains by evenly allocating the capital gains in an N-year period into the N years. Then capital gains information is available annually starting from 1984.

An important caveat in interpreting our results is that this imputed accrued capital gains is different from capital gains income, because it includes not only realized capital gains (capital gains income) but also unrealized capital gains. Nonetheless, the imputed capital gains would still give us a sense of how families may have transferred income from higher-tax rate income types to lower-tax rate place. The PSID definition of taxable income does not consider capital gains, and so do most papers in the literature. To be consistent with previous studies, we don't include capital gains in total income or taxable income in the main body. But when we calculate tax liability that takes capital gains into account, we include capital gains in the total income measure.

Another caveat is that from our data, we cannot distinguish long-term from short-term capital gains, and what we choose to do is to assume them all to be long-term, as only long-term capital gains enjoy the favorable lower tax rate. This, of course, is not a perfect assumption, since in reality there is non-negligible short-term capital gains. But as table 2.3 of Auerbach et al. (1997) shows, the majority of capital gains in every year from 1985 to 1994 in the U.S. is long-term, which gives us some support for our assumption; this suggests that the tax saving imputed assuming all capital gains to be long-term would slightly overestimates the actual amount that could be saved.

[29]Households report their current value of net wealth rather than book value (value at buying). In particular, they are asked "If you sold all that and paid off any debts on it, how much would you realize on it?".

2.6.2 Appendix tables and figures

Table 2.15. Summary statistics of key variables

	all self-employed obs in whole sample			self-employed obs in my sample		
Variable	Obs	Mean	S.d.	Obs	Mean	S.d.
ln(before-tax reported income)	11562	10.26	0.87	5499	10.26	0.87
ln(after-tax reported income)	11560	10.12	0.78	5498	10.12	0.78
ln(imputed total consumption)	11562	9.39	0.45	5491	9.41	0.44
age of head	11690	43.81	10.60	5554	43.07	10.13
head education: <high school	11609	0.13	0.33	5522	0.09	0.29
head education: high school	11609	0.28	0.45	5522	0.28	0.45
head education: some college	11609	0.24	0.43	5522	0.25	0.43
head education: college degree or above	11609	0.35	0.48	5522	0.37	0.48
	all employee obs in whole			employee obs in my sample		
Variable	Obs	Mean	S.d.	Obs	Mean	S.d.
ln(before-tax reported income)	70899	10.00	0.66	11238	10.09	0.65
ln(after-tax reported income)	70895	9.89	0.60	11238	9.96	0.59
ln(imputed total consumption)	69977	9.09	0.45	11094	9.13	0.44
age of head	70952	40.39	10.75	11254	37.06	9.12
head education: <high school	70342	0.14	0.35	11186	0.11	0.31
head education: high school	70342	0.32	0.47	11186	0.26	0.44
head education: some college	70342	0.24	0.43	11186	0.25	0.43
head education: college degree or above	70342	0.30	0.46	11186	0.38	0.49

Notes: The tax liability here is calculated without considering dividends and capital gains because they are not available for the whole sample. My sample focuses on the families ever turning from employees to self-employed and ending business finally, in their first employee-self-employed-employee cycle.

Table 2.16. Placebo tests

Dep var:	log(total income)	log(tax liability ignoring dividends and capital gains)	log(tax liability considering dividends)	log(tax liability considering dividends and capital gains)
	[1]	[2]	[3]	[4]
log(total consumption)	0.652***			
	(0.017)			
log(total income)		1.667***	1.668***	1.645***
		(0.011)	(0.011)	(0.015)
ever self-employed	-0.002	0.007	0.007	0.02
	(0.011)	(0.009)	(0.009)	(0.013)
Other controls	Yes	Yes	Yes	Yes
Family fixed-effect	No	No	No	No
Observations	67,894	61,170	61,167	36,051
R-squared	0.553	0.801	0.801	0.768

Notes: The sample focuses on all employee observations. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.17. Personal income tax savings from evasion v.s. avoidance (use the same tax formula for a household)

	Avoidance					Evasion
	Type I avoidance (income shifting)		Type II avoidance (income deferral)	Type III avoidance (exemption & deduction)	Overall avoidance	Overall evasion
	dividends	capital gains				
self-employed	0.1%	5.2%	0.0%	7.0%	12.3%	11.8%
corp self-employed	0.2%	13.5%	0.0%	13.9%	27.6%	1.7%
non-corp self-employed	0.0%	3.5%	0.0%	4.5%	8.0%	15.4%

Notes: The figures here are based on the estimates in previous tables. We use the last year before a family turns self-employed as its tax formula year.

Table 2.18. Evasion results controlling for net wealth

Dep var: log(total income)	[1]	[2]	[3]	[4]	[5]	[6]
log(total consumption)	0.909***	0.857***	0.833***	0.633***	0.629***	0.616***
	(0.075)	(0.071)	(0.071)	(0.117)	(0.127)	(0.128)
self-employed	-0.122***	-0.158***	-0.179***	-0.079	-0.083*	-0.087*
	(0.033)	(0.034)	(0.033)	(0.053)	(0.050)	(0.050)
net wealth w/o home equity (w)		0.053***	0.119***		0.045*	0.075***
(in 100,000 dollar unit)		(0.012)	(0.014)		(0.026)	(0.024)
w^2		-0.001***	-0.005***		-0.001	-0.002
		(0.000)	(0.001)		(0.000)	(0.001)
w^3		0.000**	0.000***		0.000	0.000
		(0.000)	(0.000)		(0.000)	(0.000)
w^4			-0.000***			0.000
			(0.000)			(0.000)
w^5			0.000***			0.000
			(0.000)			(0.000)
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
Family fixed-effect				Yes	Yes	Yes
Observations	4,000	4,000	4,000	4,000	4,000	4,000
R-squared	0.495	0.516	0.523	0.736	0.742	0.743

Notes: The sample for column 1-6 focuses on the first employee-self-employed cycle of families ever turning from employees to self-employed within sample period. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.19. The change of employee spousal earnings when head becomes self-employed

Dep var: log(wage/salary)	[1]	[2]
self-employed	-0.019	
	(0.061)	
self-year 0		0.026
		(0.059)
self-year 1-5		0.068
		(0.070)
self-year 6-10		0.031
		(0.096)
self-year >10		-0.255**
		(0.119)
Other controls	Yes	Yes
Family fixed-effect	Yes	Yes
Observations	2,589	2,589
R-squared	0.178	0.187

Notes: The sample focuses on the employee wives whose husbands are in their first employee-self-employed cycle. Sample for column 5 focuses on all employee observations. Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.20. Occupational change as an employee

Dep var:	log(total income)	log(total consumption)	log(total income)	log(tax liability)
occupation switch	-0.016	0.002	-0.017	0.007
	(0.017)	(0.008)	(0.016)	(0.017)
log(total consumption)			0.312***	
			(0.037)	
log(total income)				1.571***
				(0.043)
Other controls	Yes	Yes	Yes	Yes
Family fixed-effect	Yes	Yes	Yes	Yes
Observations	10,344	10,358	10,343	9,749
R-squared	0.799	0.868	0.805	0.918

Notes: The sample focuses on all employee observations.Other controls include dummies for age, education, marital status, race, state, head's hours worked, homeownership, disability, current family scale, and year dummies. Standard errors clustered at family level are in parentheses. Estimates are weighted using the sampling weights of respective surveys. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Chapter 3

Financial Transaction Taxes (FTTs) in Latin America: Revenues and Tax Base Effects

3.1 Introduction

Faced by fiscal shortage due to the economic crisis, combined with a large informal economy and tax evasion, many Latin American countries adopted alternative methods to increase tax revenue. An important one is the financial transaction taxes (FTTs). FTTs are taxes levied on specific types of monetary transaction for a particular purpose. There are many types of FTTs: securities transaction tax (STT), currency transaction tax (CTT), bank transaction tax (BTT) and other alternatives. In Latin America, FTTs mostly take the form of a BTT. The BTT, in various literature also called BDT (bank debit tax) or BAD (bank account debit tax), is a tax levied on transactions of debits or credits in bank accounts. With the benefit of relying on a simple withholding operation by financial intermediaries, the FTT requires little preparatory work and no taxpayer cooperation. Thus, it is used to collect large revenues in a relatively short time. In this paper, we examine the FTT in Latin America.

Due to data availability, we focus on 1988-2005, when seven countries in Latin America ever adopted a BTT: Argentina, Brazil, Bolivia, Columbia, Ecuador, Peru, and Venezuela. The statutory tax rates varied across countries and over time. Due to possible disintermediation effects, the FTTs were adopted and repealed repeatedly in these countries' history, as shown in Table 3.1. In 1997, only one Latin American country adopted the FTT. By mid-1999, FTTs had been in place in Argentina, Brazil, Ecuador, and Venezuela. In mid-2005, FTTs existed in Argentina, Bolivia, Brazil, Colombia, Dominican Republic, Paraguay, Peru, and Venezuela. Since then, some countries gradually replaced the FTT with more conventional taxes, and only six countries (Argentina, Bolivia, Colombia, Dominican Republic, Mexico, Peru) retained an FTT on July 1, 2009 (Coelho (2009)). The variation of the existence of the FTT and the changing tax rates provide an opportunity for an empirical test of the effects of the FTTs on the economy.

In this paper, we investigate the effects of the FTTs on government revenues and tax bases. Since FTTs were typically introduced to handle the fiscal shortage problem in Latin American history, it is natural to look at the revenue effects of the tax. Many researchers have confirmed that the FTTs can provide a large amount of revenue in a short time. But we see no paper explore the effect on the composition of the overall tax revenue. How did FTTs affect the overall tax revenue? Were there any crowd-out effects of FTTs on revenues of other taxes? Did the FTTs affect revenues of various taxes differently? For example, did it negatively affect the excise tax revenue due to a negative income effect? Did it affect the corporate activities and thus affect the corporate tax revenues? In the revenue effects part, we explore in detail the FTT effects on revenues of other taxes. Finally, we explore the effect of the FTT on its own tax base.

Although the FTT works well in collecting revenues at a low cost, previous literature focuses on its negative effects. For example, Coelho (2009), Baca Campodonico et al. (2006), Kirilenko and Summers (2003)indicate that the BAD taxes have had important detrimental effects on the efficiency of banking systems in Latin America. Restrepo (2013) finds that the introduction of BAD taxes creates a strong incentive to shift away from holding bank deposits and into using cash and other quasi-currencies. He argues that, by lowering availability of deposits as a source of funding for banks, the introduction of such FTTs decreases provision of bank credit to the private sector, which ultimately affects economic growth at the industrial level. In particular, with the implementation of FTTs, industrial output growth is slower in those industries that are inherently more dependent on external financing, as well as those industries that have less tangible assets. This disintermediation effect is regarded as a main negative effect of the FTTs.

By contrast, the empirical evidence in this paper finds that the FTTs collect a bunch of revenue without lowering tax revenue from other sources. In particular, I find that FTTs are positively associated with corporate income tax revenues. This evidence seems to contradict the findings of Restrepo (2013), since my finding implies more corporate activities in the formal economy. Given the importance of financial intermediaries to firms, how can disintermediation and more corporate activities simultaneously happen? A tentative explanation is proposed in section 3.2.3, yet further evidence is needed to confirm the hypothesis.

3.2 Revenue effects

3.2.1 FTT revenue

Most papers on the FTT in Latin America explore its direct revenue effect and find the effect large and significant. Hence, we just state the results briefly here. As Table 3.1 shows, FTTs render a non-trivial contribution to the overall tax revenue in Latin American countries. For example, a one percentage FTT could contribute a revenue equal to 1.77% of GDP, or 6.59% of overall tax revenue in 2005. In general, the ratio of the FTT revenue in the total tax revenue varies from 1.71% (Peru, 2005) to 24.45% (Ecuador, 1999). The variation comes from the difference in tax rates, plus the difference in tax productivity, which is defined as the ratio of FTT tax revenue/GDP over the effective tax rate. Here, the effective tax rate is calculated by weighting the statutory tax rates according to the dates the specific tax rates applied. From the last two columns of Table 3.1, it is clear that the variation in the ratio of the FTT revenue over total tax revenue comes more from the difference in tax rates than from that in the FTT productivity. This is clear by looking at Ecuador 1999, 2000 and Venezuela 2002, 2003, when the ratios of FTT revenue over total tax revenue are over 10% while FTT productivities are only between 1 and 2.

3.2.2 Total tax revenue

Besides its direct effect on its own revenue, the FTT would have potential effects on revenues of other taxes and the overall tax revenue. If the FTT has a large replacement effect on other kinds of taxes, i.e. it may result in tax cuts of other taxes, then its overall effect on the total tax revenue may be small or even negative. We investigate this by estimating how a one dollar increase in the FTT revenue changes the sum of all other tax revenues. If this effect is zero, then we conclude that the FTT has a neutral effect on other tax revenues. Otherwise, the FTT may have behavioral effect on the overall economy and thus may have induced an efficiency loss (gain) if it decreases (increases) other tax revenues. To investigate this, we estimate the following equation:

$$Other\_tax\_rev\_ratio_{it} = \alpha + \beta FTT\_rev\_ratio_{it} + \gamma \cdot X_{it} + \lambda_i + e_{it}, \tag{3.1}$$

where $Other\_tax\_rev\_ratio_{it}$ is the ratio of other taxes revenue as a whole over GDP (percentage point) of country i in year t. The main explanatory variable $FTT\_rev\_ratio_{it}$ is the ratio of the FTT revenue over GDP for country i in year t, X_{it} are control variables, which include lagged term of the dependent variable, log of GDP, log of population, financial reform index. e_{it} is the error term. In all regressions, we control for country fixed effects to account for the time-invariant country-specific factors. To account for potential serial correlation of error terms within country, we use standard errors clustered at country level in all regressions. For a summary of the data and definition and sources of variables, see Table 3.13 and Table 3.12. Due to the data availability, our sample is an unbalanced panel from 1990 to 2005.

Table 3.1. FTT rates and revenues in Latin America: 1988-2005

Countries	Year	FTT tax rate (%)[1]	FTT revenue/GDP (%)[1]	Total tax revenue/GDP (%)[2]	FTT revenue/Total tax revenue (%)	FTT productivity
Argentina	1988	0.70	0.83			1.19
	1989	0.70	0.66			0.94
	1990	0.30	0.30	16.15	1.86	1.00
	1991	1.13	0.91	18.40	4.95	0.81
	1992	0.55	0.58	21.46	2.70	1.05
	2001	0.99	1.47	20.94	7.02	1.48
	2002	1.20	1.56	19.91	7.84	1.30
	2003	1.20	1.57	23.43	6.70	1.31
	2004	1.20	1.72	26.36	6.53	1.43
	2005	1.00	1.77	26.87	6.59	1.77
Bolivia	2004	0.15	0.45			3.00
	2005	0.28	0.82			2.93
Brazil	1994	0.25	1.28	29.25	4.38	5.12
	1997	0.20	0.86	26.61	3.23	4.30
	1998	0.20	0.89	27.44	3.24	4.45
	1999	0.38	1.40	28.72	4.87	3.68
	2000	0.33	1.35	30.09	4.49	4.09
	2001	0.37	1.45	31.00	4.68	3.92
	2002	0.38	1.54	31.67	4.86	4.05
	2003	0.38	1.48	31.23	4.74	3.89
	2004	0.38	1.49	32.08	4.64	3.92
	2005	0.38	1.36	33.12	4.11	3.58
Columbia	1999	0.20	0.71	14.31	4.96	3.55
	2000	0.20	0.60	14.02	4.28	3.00
	2001	0.30	0.75	15.63	4.80	2.50
	2002	0.30	0.71	15.58	4.56	2.37
	2003	0.30	0.71	16.11	4.41	2.37
	2004	0.40	0.89	16.86	5.28	2.23
	2005	0.40	0.71	17.43	4.07	1.78
Ecuador	1999	2.00	2.51	10.26	24.45	1.26
	2000	1.60	2.23	11.62	19.19	1.39
Peru	1990	1.42	0.89	11.85	7.51	0.63
	1991	0.81	0.58	13.18	4.40	0.72
	2004	0.20	0.32	14.65	2.18	1.60
	2005	0.16	0.27	15.76	1.71	1.69
Venezuela	1994	0.75	1.30	14.85	8.75	1.73
	1999	0.50	1.13	13.81	8.19	2.26
	2000	0.50	0.89	13.62	6.53	1.78
	2002	0.83	1.56	11.22	13.90	1.88
	2003	0.88	1.35	11.88	11.37	1.53
	2004	0.50	0.82	13.30	6.16	1.64

Data source: 1 From Baca Campodonico et al. (2006) except the Bolivia data and 2005 data, which are from Restrepo (2013). 2 From OECD Dataset: Revenue Statistics - Latin American Countries : Comparative tables.

Table 3.2. FTT and other taxes revenue as a whole

Dependent variable: Other_tax_rev_ratio						
	(1)	(2)	(3)	(4)	(5)	(6)
Lag_other_tax_rev_ratio			0.552***			0.558***
			(0.12)			(0.12)
FTT_rev_ratio	0.307	0.098	0.09			
	(0.93)	(0.50)	(0.43)			
ln_GDP_pc		15.918*	4.739		15.979*	4.971
		(7.67)	(5.64)		(8.02)	(5.71)
ln_population		8.026	6.990*		7.739	5.747*
		(8.32)	(3.04)		(7.38)	(2.58)
Fin_ref_ind		-1.062	-1.866***		-0.96	-1.523***
		(1.96)	(0.50)		(1.74)	(0.35)
Banking_crisis		-0.806	-0.796		-0.807	-0.801*
		(0.46)	(0.42)		(0.45)	(0.41)
FTT_dummy				0.465	0.164	0.35
				(1.15)	(0.55)	(0.48)
Constant	16.966***	-246.61	-148.345*	16.922***	-242.275*	-129.302*
	(0.36)	(141.39)	(63.50)	(0.40)	(122.25)	(63.62)
Observations	112	112	105	112	112	105
R-squared	0.006	0.486	0.653	0.008	0.486	0.656
Number of countries	7	7	7	7	7	7

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** p<0.01, ** p<0.05, * p<0.1.

Table 3.2 shows the FTT effect on the other tax revenues as a whole. In Column (1), when we only control for the FTT revenue ratio and the country fixed effects, the R-squared is only 0.006, suggesting severe omitted variable problem. Then we gradually add potentially important determinants of tax revenue ratio. In column (2), we add log of GDP per capita to account for the general economic development level, which is a crucial factor for the tax revenue ratio. To further account for other important social and economic factors, we add log of population and the financial reform index. The financial reform index, developed by IMF, is a normalized index accounting for seven aspects of financial system, including credit controls, interest rate controls, entry barriers, banking supervision, privatization, international capital flows, and security markets. A higher value denotes that financial sector policies are more liberalized and less restrictive to the functioning of the financial system. A detailed description of the index can be found in Abiad et al. (2010). In addition, the FTTs in Latin America were often introduced when there is a fiscal shortage due to poor economy. To account for this potential business cycle impact, we control for the banking crisis dummies. See Laeven and Valencia (2012) for a detailed description of the dummies. After controlling for these, the R-squared considerably increases to 0.486, and the coefficient for GDP per capita is significant both in statistical and economic explanations. However, the coefficient of FTT revenue ratio remains small and insignificant.

To further account for the potential omitted variable problem that we can never perfectly deal with, the lagged term of dependent variable is added as a control variable. The idea is that many factors affecting the level of other tax revenue cannot be fully controlled but can be reflected in the lagged term of tax revenue. By controlling for the lagged term, we also largely control for those confounding factors. Column (3) shows that the lagged effect is statistically significant at 0.01 significance level. Economically, lagged term explains over half of the current other tax revenue ratio level. The R-squared increases to 0.653 with the lagged term. But the coefficient for the FTT revenue ratio is still insignificant. Finally, as a robustness check, we use the dummy denoting whether the FTT applies in a specific year as

Table 3.3. FTT and total tax revenue

Dependent variable: Total_tax_rev_ratio						
	(1)	(2)	(3)	(4)	(5)	(6)
Lag_total_tax_rev_ratio			0.601***			0.631***
			(0.12)			(0.12)
FTT_rev_ratio	1.307	1.098*	0.857*			
	(0.93)	(0.50)	(0.42)			
ln_GDP_pc		15.918*	4.805		15.555	4.073
		(7.67)	(4.91)		(8.38)	(4.93)
ln_population		8.026	4.758		7.923	3.524
		(8.32)	(2.99)		(7.10)	(2.18)
Fin_ref_ind		-1.062	-1.281		-0.76	-0.871
		(1.96)	(0.75)		(1.76)	(0.62)
Banking_crisis		-0.806	-0.644		-0.791	-0.609
		(0.46)	(0.35)		(0.47)	(0.36)
FTT_dummy				1.556	1.226*	1.187**
				(1.12)	(0.52)	(0.45)
Constant	16.966***	-246.61	-111.992	16.928***	-242.154*	-85.858
	(0.36)	(141.39)	(60.24)	(0.39)	(119.01)	(57.20)
Observations	112	112	105	112	112	105
R-squared	0.092	0.531	0.725	0.083	0.518	0.73
Number of countries	7	7	7	7	7	7

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** p<0.01, ** p<0.05, * p<0.1.

the main explanatory variable. As columns (4)-(6) show, the main conclusion remains.

Now we see that the FTT has small and insignificant effects on the other taxes revenue as a whole. This implies no crowd-out effect on other tax revenues as a whole, which suggests that the FTT does a good job in collecting revenues. As shown in column (2) of Table 3.3, a one dollar increase in the FTT revenue is associated with a 1.098 dollar increase in total tax revenue. This relation is statistically significant at the 0.1 level.

3.2.3 Other tax revenues

But having no effect on other tax revenue as a whole does not imply that the FTT does not have behavioral effects at all. Would FTT have any impacts on the component of other tax revenues? To investigate this, we focus on revenues of several important kinds of taxes, also subject to data availability.

Excise tax revenue

We first look at the excise tax, which is directly relevant to credit and debit transactions. As mentioned above, due to the income effect of the tax, FTTs would cause people to consume less in general and thus would contribute to a decrease in the excise tax revenue. However, since the BTT typically occurs during withdrawing and depositing money to bank accounts, it will restrain people from transferring money between cash and bank account deposits. Since cash transaction is not convenient for customers, the tax might induce more transactions by credit card or debit card, especially for small amount transactions such as paying restaurant. This repelling effect on informal economy thus results in an increase in the excise tax revenue. So overall, the effect of the FTT on the excise tax revenue is ambiguous and waits for an empirical answer.

The estimating equation is the same as (1) except that the dependent variable is the excise tax revenue/GDP ratio. Due to data unavailability, we cannot control for the excise tax rate, and the sample does not include Bolivia since the dependent variable is not available for this country. Tables 3.4 shows there is almost no association between the FTT revenue and the excise tax revenue. This is robust whether we add more controls or not, and robust to using FTT dummy as an explanatory variable.

CIT revenue

To investigate the relation between the FTT and the corporate income tax (CIT) revenue, we estimate specification (1) using the CIT revenue/GDP ratio as the dependent variable. For CIT, we have collected statutory tax rates and CIT revenue/GDP ratios for all countries. But the dependent variable is not available for all years. For most countries, the dependent variable is available for 1990-2005. But for Ecuador, the CIT revenue/GDP ratio

Table 3.4. FTT and excise tax revenue

Dependent variable: Total_tax_rev_ratio						
	(1)	(2)	(3)	(4)	(5)	(6)
Lag_total_tax_rev_ratio			0.601***			0.631***
			(0.12)			(0.12)
FTT_rev_ratio	1.307	1.098*	0.857*			
	(0.93)	(0.50)	(0.42)			
ln_GDP_pc		15.918*	4.805		15.555	4.073
		(7.67)	(4.91)		(8.38)	(4.93)
ln_population		8.026	4.758		7.923	3.524
		(8.32)	(2.99)		(7.10)	(2.18)
Fin_ref_ind		-1.062	-1.281		-0.76	-0.871
		(1.96)	(0.75)		(1.76)	(0.62)
Banking_crisis		-0.806	-0.644		-0.791	-0.609
		(0.46)	(0.35)		(0.47)	(0.36)
FTT_dummy				1.556	1.226*	1.187**
				(1.12)	(0.52)	(0.45)
Constant	16.966***	-246.61	-111.992	16.928***	-242.154*	-85.858
	(0.36)	(141.39)	(60.24)	(0.39)	(119.01)	(57.20)
Observations	112	112	105	112	112	105
R-squared	0.092	0.531	0.725	0.083	0.518	0.73
Number of countries	7	7	7	7	7	7

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

is available only for 1993-2005. The CIT rates varied across countries and over years. But for Ecuador, the CIT rate was constant at 25% over 1993-2005, rendering no variation for estimating the coefficient of CIT rate. These two reasons motivate me to exclude Ecuador in my preferred regressions. After excluding Ecuador, I have a balanced panel over 1990-2005.

The estimating equation is again the same as (1) except that that we use the CIT revenue/GDP ratio as dependent variable. Table 3.5 shows the main results. Although my preferred regressions exclude Ecuador, columns 1-3 present the results including Ecuador. The regression results using this unbalanced panel show that the FTT revenue is positively associated with the CIT revenue, although such relation is not statistically significant. This imprecise estimate may be due to the small variation in FTT rates (except for 1999 and 2000, the FTT rates in the other years were zero in Ecuador) and no variation of CIT rates in Ecuador. Thus, we focus on regressions excluding Ecuador. The coefficients for FTT revenue/GDP ratio are significant both statistically and economically. The coefficients for other variables are as expected. In particular, per capita GDP is strongly positively associated with the CIT revenue/GDP ratio, implying a stronger entrepreneurship in richer economies.

To interpret the main results, we rerun the regressions using the FTT rate as the main explanatory variable. This has the advantage of rendering a better comparison with the coefficients of the CIT rate. In addition, one might interpret the above positive association between the FTT revenue/GDP ratio and the CIT revenue/GDP ratio as representing a common tax base of FTT and CIT, since at least parts of debit transactions in banks may overlap firm activities. Using FTT rates as the explanatory variable avoids such problem and is more likely to present a causal relation between FTT and CIT revenue. Table 3.6 shows the results. The estimated coefficients for all variables are comparable with Table 3.5, except that the estimates for the FTT measure is less precise. This is unsurprising since the FTT rate is less volatile than the actual FTT revenue/GDP ratio. The imprecise coefficients for CIT rate is also due to its relatively less variation.

I focus on the result in column 6, which is my preferred specification. A one percentage

Table 3.5. FTT revenue and corporate income tax revenue

Dependent variable: Corp_rev_ratio						
	All countries			Exclude Ecuador		
	(1)	(2)	(3)	(4)	(5)	(6)
Lag_corp_rev_ratio			0.553***			0.505***
			(0.09)			(0.09)
FTT_rev_ratio	-0.317	0.476	0.213	-0.301	0.959**	0.542***
	(0.61)	(0.37)	(0.21)	(0.86)	(0.24)	(0.11)
CIT_rate	0.105	0.1	0.041	0.105	0.111	0.052
	(0.08)	(0.07)	(0.04)	(0.08)	(0.07)	(0.04)
ln_GDP_pc		14.953**	4.572		16.234***	6.184**
		(4.03)	(2.58)		(3.25)	(2.36)
ln_population		-9.447	-1.915		-15.138*	-5.529
		(7.97)	(4.06)		(6.42)	(3.70)
Fin_ref_ind		-2.556	-0.805		-1.137	-0.054
		(1.85)	(0.62)		(1.61)	(0.34)
Banking_crisis		-0.574	-0.365*		-0.458	-0.288
		(0.51)	(0.15)		(0.64)	(0.21)
Constant	-0.539	43.946	-3.204	-0.501	131.558	45.664
	(1.87)	(106.97)	(48.94)	(1.82)	(88.75)	(45.24)
Observations	109	109	102	96	96	90
R-squared	0.144	0.489	0.687	0.142	0.55	0.702
Number of countries	7	7	7	6	6	6

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** p<0.01, ** p<0.05, * p<0.1.

Table 3.6. FTT rate and corporate income tax revenue

Dependent variable: Corp_rev_ratio						
	All countries			Exclude Ecuador		
	(1)	(2)	(3)	(4)	(5)	(6)
Lag_corp_rev_ratio			0.557***			0.504***
			(0.09)			(0.09)
FTT_rate	-0.753	0.427	0.297	-0.918	0.975	1.057**
	(0.78)	(0.53)	(0.39)	(1.26)	(0.80)	(0.34)
CIT_rate	0.106	0.095	0.039	0.106	0.1	0.051
	(0.08)	(0.07)	(0.04)	(0.08)	(0.07)	(0.04)
ln_GDP_pc		14.726**	4.579		16.037***	6.863*
		(4.14)	(2.76)		(3.59)	(2.75)
ln_population		-8.247	-1.716		-12.829	-6.291
		(7.67)	(4.15)		(7.14)	(3.83)
Fin_ref_ind		-2.641	-0.701		-1.284	0.663
		(1.69)	(0.73)		(1.28)	(0.34)
Banking_crisis		-0.601	-0.390*		-0.587	-0.365
		(0.55)	(0.16)		(0.69)	(0.21)
Constant	-0.544	25.459	-6.693	-0.469	93.848	52.973
	(1.99)	(100.98)	(48.94)	(1.99)	(98.09)	(44.32)
Observations	109	109	102	96	96	90
R-squared	0.161	0.477	0.686	0.161	0.523	0.703
Number of countries	7	7	7	6	6	6

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** p<0.01, ** p<0.05, * p<0.1.

point increase in the FTT rate on average increases the CIT revenue/GDP ratio by 1.057 percentage points, while a one percentage point increase in the CIT rate increases the CIT revenue/GDP ratio by 0.051. The mean values (standard deviations) of FTT and CIT rate for the six countries over 1990-2005 are 0.21 (0.34) and 24.8 (8.96) respectively. A one standard deviation increase in the FTT and CIT rate increases the CIT revenue/GDP ratio by 0.34*1.057=0.36 and 8.96*0.051=0.46 percentage points, respectively. This result implies that the FTT and CIT has quite comparable impacts on the CIT revenue, which is quite surprising.

To ensure that this result is not driven by one specific country, besides excluding Ecuador from my preferred regressions, I exclude each country in turn from the sample to see if the estimates change a lot. Table 3.7 shows the results. Column 1 restates the preferred regression result in Table 3.6. Columns 2-7 present the regression results excluding one additional country respectively. As can be seen, excluding one additional certain country from our sample in some cases (Bolivia and Peru) increases the positive relation between the FTT rate and the CIT revenue/GDP ratio, sometimes even increases the statistical significance (Peru). In some cases, excluding one country almost does not change the estimates (Columbia), implying that Columbia shares a same estimate with the average estimates for the other six countries. In other cases (Argentina, Brazil and Venezuela), excluding one certain country decreases the estimated coefficient of FTT rate. The largest change happens when we exclude Argentina from our sample, which lowered the coefficient of the FTT rate by almost half, and the coefficient becomes statistically insignificant. The estimates become imprecise because there is large variation in FTT rates in Argentina as shown in Table 3.1. And this implies that the FTT may have had the largest effect on increasing the CIT revenue in Argentina. Even excluding Argentina, the FTT rate is still positively associated with CIT revenue/GDP ratio, although with a smaller magnitude.

To account for scale of economies, I run weighted fixed-effects regressions for the six countries excluding Ecuador as a robustness check. Following the common practice, I use the

Table 3.7. FTT rate and corporate income tax revenue (Excluding Ecuador)

Dependent variable: Corp_rev_ratio									
Further Exclude country:	None	Argentina	Bolivia	Brazil	Columbia	Peru	Venezuela	None	None
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lag_corp_rev_ratio	0.504***	0.462**	0.475**	0.473**	0.489**	0.486***	0.421***	0.492***	0.517***
	(0.09)	(0.13)	(0.12)	(0.13)	(0.11)	(0.10)	(0.08)	(0.06)	(0.04)
FTT_rate	1.057**	0.561	1.194**	0.964**	1.040*	1.300***	0.802**	1.123*	0.979*
	(0.34)	(0.39)	(0.38)	(0.31)	(0.38)	(0.23)	(0.22)	(0.45)	(0.41)
CIT_rate	0.051	0.055	0.081	0.071	0.052	0.055	0.015**	0.047	0.035
	(0.04)	(0.05)	(0.07)	(0.07)	(0.05)	(0.05)	(0.01)	(0.04)	(0.03)
ln_GDP_pc	6.863*	7.926	7.668	6.842*	7.373*	7.671*	4.005***	8.033*	6.557**
	(2.75)	(3.93)	(3.65)	(2.49)	(2.96)	(2.78)	(0.84)	(3.14)	(2.35)
ln_population	-6.291	-6.754	-7.348	-7.127	-6.586	-7.473	-1.332	-5.51	-4.246
	(3.83)	(4.94)	(4.02)	(4.52)	(4.47)	(3.94)	(1.21)	(4.35)	(3.82)
Fin_ref_ind	0.663	0.582	0.891	0.31	0.498	0.826*	0.521	0.658	0.433
	(0.34)	(0.55)	(0.47)	(0.39)	(0.31)	(0.36)	(0.67)	(1.07)	(1.10)
Banking_crisis	-0.365	-0.59	-0.474	-0.25	-0.374	-0.384	-0.251*	-0.378	-0.324
	(0.21)	(0.37)	(0.27)	(0.13)	(0.30)	(0.23)	(0.11)	(0.24)	(0.17)
Constant	52.973	53.171	64.062	65.336	54.105	66.799	-8.23	33.582	22.973
	(44.32)	(53.76)	(42.49)	(56.41)	(53.62)	(48.84)	(24.09)	(59.82)	(55.49)
Observations	90	75	75	75	75	75	75	90	90
R-squared	0.703	0.704	0.711	0.717	0.709	0.704	0.748	0.672	0.664
Number of countries	6	5	5	5	5	5	5	6	6

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** p<0.01, ** p<0.05, * p<0.1. Columns 8 and 9 are based on weighted FE regression using 1990 GDP and 1990 population as weights, respectively.

GDP and population in 1990 (the initial year of my panel) as weights in columns 8 and 9, respectively. The results are similar to the main results shown in column 1.

The strongly positive relation between the FTT revenue and the CIT revenue is the most striking result from this research. How to interpret it? Can we propose an explanation that can reconcile the evidence here and the finding by Restrepo (2013)? Restrepo (2013) finds that with the implementation of FTTs, there is a shift from holding bank deposits into using cash and other quasi-currencies, and that, industrial output growth is slower in those industries that are inherently more dependent on external financing, as well as those industries that have less tangible assets.

Here I propose a tentative explanation. After the introduction of the FTTs, banks have less deposits. Suppose that the demand for credit from the firms do not change, then the banks have to ration the limited amount of deposits. When deposits are limited, banks may have a larger incentive to allocate them to high-type (less risky or have higher profiting ability) firms. The banks may give priority to the loans by high-type firms; then after ensuring the loaning demand of high-type firms, the banks allocate the remaining deposits to low-type (riskier or have lower profiting ability) firms. Thus, a decrease in bank deposits may increase the share of loans to high-type firms.

This screening effects will have a positive effect on overall firm profits. On the one hand, high-type firms obtain a larger portion of credit in the economy, thus a larger portion of deposits go to better projects; on the other hand, low-type firms have less capacity to invest in risky projects. Then overall the deposits are better allocated to profitable projects. This is the deposit rationing effects of the introduction of FTTs, which would benefit the economic growth. And this is also consistent with the evidence in Restrepo (2013) that industrial output growth is slower in the industries that are inherently more dependent on external financing, as well as in the industries that have less tangible assets, since these firms are more likely to be low-type firms.

There is a second effect of the introduction of FTTs, which is on the allocation of profits by firms. Since now there is a tax on the deposits in banks, firms may have less incentive to deposit their profits into banks. Rather, they now have an incentive to use a larger portion of profits to invest in new projects. High-type firms have a lot of profits for investment, which results in a higher growth, while low-type firms may barely have profits, thus less likely to expand their investment in risky projects. These together are beneficial for economic growth and can result in an increase in the CIT revenue.

Based on the above two reasons, the relative scale of high-type firms to low-type ones increases, which may be beneficial to CIT revenue collection since high-type firms are more likely to be larger firms, which are more likely to be monitored and audited by government. The decrease in tax evasion is the third explanation for the positive association between FTTs and the CIT revenue.

These hypotheses are testable using firm-level data. To test the first hypothesis, we need to examine whether bank lend a higher portion of loans to high-type firms after the FTT introduction. To test the second hypothesis, we can investigate whether high-type firms invest more after the FTT introduction while low-type firms invest less. The third hypothesis is much harder to test, but it is a common argument for the revenue boosting effect of a policy targeted at making firms more easily to be monitored.

VAT revenue

Then we consider the value-added tax (VAT). The income effect of the FTT turns to reduce transactions and thus has a negative effect on the VAT revenues. But by imposing a tax on cash transactions, FTTs were also supposed to discourage cash economy. Thus, it was hoped that more firms and individuals would shift into the formal economy, adding to the tax base. Thus, the overall effect is again ambiguous and remains an empirical question.

To investigate the relation between the FTT and the VAT revenue, we estimate specification (1) using the VAT revenue/GDP ratio as the dependent variable. The VAT revenue/GDP ratios are available for all seven countries during 1990-2005. But the statutory

Table 3.8. FTT revenue and VAT revenue

Dependent variable: VAT_rev_ratio	(1)	(2)	(3)	(4)	(5)	(6)
Lag_VAT_rev_ratio			0.676***			0.647**
			(0.06)			(0.16)
FTT_rev_ratio	0.07	-0.606**	-0.041	-0.694**	-0.629	-0.106
	(0.29)	(0.23)	(0.11)	(0.22)	(0.36)	(0.16)
ln_GDP_pc		-2.557	-1.792**		-0.844	-0.501
		(3.30)	(0.51)		(1.91)	(1.23)
ln_population		8.820*	2.938		4.859	1.126
		(4.32)	(1.56)		(3.99)	(1.15)
Fin_ref_ind		3.259*	0.195		2.615	0.331
		(1.57)	(0.85)		(1.89)	(1.36)
Banking_crisis		0.241	0.028		-0.206	-0.152
		(0.44)	(0.31)		(0.46)	(0.33)
VAT_rate				0.533**	0.224	0.013
				(0.14)	(0.16)	(0.11)
Constant	4.997***	-127.692*	-34.379	-3.232	-75.47	-13.596
	(0.11)	(53.31)	(25.20)	(2.09)	(59.86)	(21.27)
Observations	112	112	105	62	62	61
R-squared	0.001	0.6	0.79	0.493	0.692	0.818
Number of countries	7	7	7	6	6	6

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** p<0.01, ** p<0.05, * p<0.1.

VAT rates are not found for all countries during the same period. From several sources (volumes of *Corporate Tax Guides* published by Price Waterhouse Coopers and Ernst and Young, World Tax Database from University of Michigan), I obtain VAT rates for several countries in several years. But I do not find VAT rates for Bolivia. Thus, I have an unbalanced panel for six countries.

The main results are shown in Table 3.8. Columns 1-3 do not control for VAT rates. There is some evidence that the FTT is negatively associated with the VAT revenue. Columns 4-6 control for VAT rates for available observations, which reduce our sample by over 40%. But the negative association between FTT and VAT revenue remains. By comparing column 5 and column 4, we see that controlling for per capita GDP, population, financial reform index

Table 3.9. FTT rate and VAT revenue

Dependent variable: VAT_rev_ratio						
	(1)	(2)	(3)	(4)	(5)	(6)
Lag_VAT_rev_ratio			0.665***			0.637**
			(0.06)			(0.16)
FTT_rate	-0.331	-0.881*	-0.147	-1.062***	-1.146*	-0.211
	(0.58)	(0.41)	(0.19)	(0.26)	(0.55)	(0.20)
ln_GDP_pc		-2.842	-2.000**		-1.473	-0.629
		(3.65)	(0.65)		(1.82)	(1.20)
ln_population		8.628	3.304		6.091	1.411
		(4.63)	(1.79)		(4.49)	(1.12)
Fin_ref_ind		2.983	0.127		2.031	0.251
		(1.57)	(0.85)		(1.89)	(1.32)
Banking_crisis		0.337	0.052		-0.168	-0.146
		(0.52)	(0.33)		(0.44)	(0.33)
VAT_rate				0.494**	0.191	0.01
				(0.15)	(0.14)	(0.11)
Constant	5.093***	-122.018*	-38.878	-2.661	-90.309	-17.219
	(0.12)	(54.83)	(27.22)	(2.18)	(65.95)	(21.28)
Observations	112	112	105	62	62	61
R-squared	0.009	0.594	0.791	0.509	0.704	0.819
Number of count	7	7	7	6	6	6

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

and business cycle does not change the coefficient of the FTT revenue/GDP ratio. Column 6 shows that controlling for lagged dependent variable greatly decreases the magnitude of this relation, and the effect of VAT rate on VAT revenue is also greatly reduced. As Table 3.9 shows, the above results do not change much if we use the FTT rate as the main explanatory variable. Table 3.14 shows that if we use the FTT dummy as main explanatory variable, the estimated coefficient of FTT becomes even smaller and imprecise, possibly due to the smaller variation of the FTT dummy.

To facilitate a comparison between the FTT and the VAT, we focus on Table 3.9, which controls tax rates for both taxes. Consistent with previous part, I explain the results based on column 6. The mean values (standard deviations) of FTT and VAT rate for the six countries over 1990-2005 are 0.24 (0.41) and 15.24 (3.44) respectively. A one standard deviation increase in the FTT rate decreases the VAT revenue/GDP ratio by 0.41*0.211=0.08651 percentage points, while a one standard deviation increase in the VAT rate increases the VAT revenue/GDP ratio by 3.44*0.01=0.00344 percentage points. These associations are smaller than those in the CIT revenue relation.

PIT revenue

To investigate the relation between the FTT and personal income tax (PIT) revenue, we estimate specification (1) using the PIT revenue/GDP ratio as the dependent variable. From the OECD dataset, the PIT revenue/GDP ratio is only available for Argentina, Brazil, Columbia, and Peru during 1990-2005. And the PIT revenue/GDP ratio is relatively small compared to CIT and VAT, which is common for the developing countries.

Table 3.10 shows the main result. Along all specifications, the result is quite robust: the association between the FTT and the PIT revenue is small and insignificant. I use column 6 to illustrate. The mean values (standard deviations) of the FTT and the PIT rate for the six countries over 1990-2005 are 0.24 (0.37) and 31.23 (4.35) respectively. A one standard deviation increase in the FTT rate decreases the PIT revenue/GDP ratio by 0.37*0.093=0.03441 percentage points, while a one standard deviation increase in the PIT rate increases the PIT revenue/GDP ratio by 4.35*0.005=0.02175 percentage points. This insignificant association result is not surprising since the FTT does not seem to have any important direct or indirect relation with personal income earnings. Table 3.15 and Table 3.16 show similar results when using the FTT revenue/GDP ratio or FTT dummies as the main explanatory variables.

3.2.4 Summary

To sum up, by looking at the relation between the FTT and several other major taxes, we find that there is almost no association between the FTT and excise tax revenue, there is

Table 3.10. FTT rate and personal income tax revenue

Dependent variable: PIT_rev_ratio						
	(1)	(2)	(3)	(4)	(5)	(6)
Lag_PIT_rev_ratio			0.832***			0.840***
			(0.04)			(0.03)
FTT_rate	-0.116	0.019	-0.102	-0.058	0.051	-0.093
	(0.31)	(0.20)	(0.05)	(0.13)	(0.16)	(0.07)
ln_GDP_pc		2.326*	-0.19		2.329*	-0.145
		(0.82)	(0.39)		(0.93)	(0.38)
ln_population		0.288	0.565		0.408	0.35
		(1.37)	(0.26)		(1.11)	(0.33)
Fin_ref_ind		0.247	-0.116		0.169	-0.027
		(0.47)	(0.16)		(0.48)	(0.13)
Banking_crisis		0.126	-0.056		0.123	-0.056
		(0.10)	(0.04)		(0.10)	(0.03)
PIT_rate				-0.012	-0.009	0.005
				(0.03)	(0.01)	(0.00)
Constant	0.589***	-23.641	-8.209*	0.95	-25.446	-4.979
	(0.08)	(24.32)	(3.31)	(1.00)	(21.39)	(4.38)
ObserPITions	64	64	60	64	64	60
R-squared	0.017	0.537	0.909	0.034	0.546	0.912
Number of count	4	4	4	4	4	4

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** p<0.01, ** p<0.05, * p<0.1.

a small negative association between the FTT and the VAT and the PIT revenue. The most striking result is the robust finding that there is a significantly positive association between the FTT and the CIT revenue. And the effect of the FTT and the CIT rate on the CIT revenue are comparatively large.

Before explaining the results, a caveat is that due to data availability, I do not control for excise tax rates, VAT rates are missing for some observations, and PIT revenue/GDP ratios are available for only four, although big, economies. But the data for the CIT are quite good, and the result for the CIT is most striking and interesting. So the next step may be to dig deeper the relation between the FTT and the CIT revenue.

Given the above empirical results for various taxes, here I try to provide a preliminary explanation. For the excise tax, the FTT may have a too small income effect on people's purchasing power due to the trivial tax rate. Also due to the trivial tax rate, the FTT may have little effect on pushing the cash transactions of the specific commodities subject to excise taxes to credit and debit transactions. Thus, the overall effect on the excise revenue is also immaterial. As I cannot see any important direct or indirect relation between FTT and personal income earnings, the immaterial association between the FTT and the PIT revenue is not surprising.

For the corporate tax, the FTT, although trivial in its rates, may have a considerable effect on firm's behavior due the large amount involved in corporate transactions. This could be the reason why corporate tax responds significantly to the FTT. I propose three potential reasons explaining why there is a positive association in section 3.2.3. Potential tests and necessary data are also proposed.

For the VAT, a similar argument to that for the corporate tax might also applies. But the agents subject to the VAT are not only large corporations, but all agents in the commodity production and consumption chain. As I argued for the effect on the excise tax, the trivial tax rate may have negligible effect on general consumers and small-scale producers, thus diluting the potentially large effect on the large corporations along the whole production chain.

3.3 Tax base effects

In this section, I investigate the FTT effects on its own tax base to explore the potential efficiency effects. In the above section, by examining the revenue effects, we indirectly explore the efficiency effects. Here we inspect the effects directly.

Ideally, we can calculate the mechanical revenue effect of the FTT by multiplying the tax base before the introduction of the FTT by the tax rate change. Then the difference between the mechanial revenue change and the actual revenue change captures the revenue effects due to behavioral changes. The problem is that the data to measure the tax base, i.e. the transaction amounts of debits withdrawing or depositing, are very hard, if not impossible, to obtain, especially when the various exemption items vary considerably across countries.[1] In this paper, I choose to calculate the imputed tax base by dividing the FTT tax revenue by the effective FTT rate. This is meaningful because it would measure the actual base that was taxed, rather than the statutory tax base that should be taxed. Furthermore, this imputed tax base is comparable across countries rather than if we use some proxies for the tax base (such as credits, debits or deposits amount). Although some papers (Kirilenko and Summers (2003); Baca Campodonico et al. (2006)) examine the FTT disintermediation effect by looking at the revenue productivity, which is defined as the ratio of tax revenue over GDP to the tax rate, I think it is more appropriate to look at the imputed tax base when we are interested in the effects on the tax base.

The model to be estimated is

$$Tax\_base_{it} = \alpha + \beta \cdot tax_{it} + \gamma \cdot X_{it} + \lambda_i + e_{it},$$

where Tax_base_{it} is the (log of) imputed FTT base of country i in year t, tax_{it} is a

[1] See Coelho et al. (2001) for a detailed description of tax bases and exemption terms of the FTT in these Latin American countries.

measure of the FTT (tax rate or FTT dummy), X_{it} are control variables, including lagged term of the dependent variable, log of GDP, log of population, financial reform index, and banking crisis dummy. e_{it} is the error term. Note that in the above regression, the effective tax rate is included both in the denominator of the dependent variable and as an independent variable. If there is any measurement error in the tax rate, if will bias the estimate of β in two directions. First, it would cause a negative bias (away from zero). Second, it would induce a well-known attenuation bias, which will bias the estimate towards zero. The direction of the bias is then theoretically unclear. However, the measure of the effective FTT rate as it is constructed, i.e. the statutory FTT rate adjusted by its implementation date in a year, seems to be not subject to a severe measurement error. Therefore, we maintain that the endogeneity problem is not salient here.[2]

[2] Of course, if we alternatively use the FTT dummy (whether there is a FTT for one country in one year) as the main explanatory variable, then we could avoid the potential endogeneity problem entirely since there is no measurement error problem for the FTT dummy. However, in our data, since we could only obtain the imputed tax base in years having an FTT, we cannot run such a regression.

Table 3.11. FTT effect on its tax base

Dependent variable: ln_imputed_FTT_base					
	(1)	(2)	(3)	(4)	(5)
Lag_ln_imputed_FTT_base					0.412**
					(0.15)
FTT_rate	-0.423	-0.305	-0.29	-0.235***	-0.209**
	(0.66)	(0.16)	(0.16)	(0.04)	(0.08)
ln_GDP		1.893***	2.170***	2.122**	1.014*
		(0.39)	(0.29)	(0.58)	(0.51)
ln_population			-0.659	-0.985	-0.188
			(0.55)	(0.80)	(0.86)
Fin_ref_ind				0.401	0.656
				(0.71)	(0.53)
Banking_crisis				0.161**	0.01
				(0.06)	(0.07)
Constant	26.712***	-22.118*	-17.667	-11.049	-7.617
	(0.40)	(10.15)	(12.75)	(26.29)	(18.16)
Observations	41	41	41	41	29
R-squared	0.104	0.766	0.77	0.821	0.92
Number of countries	7	7	7	7	7

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** p<0.01, ** p<0.05, * p<0.1.

As Table 3.11 shows, when we control for the variables relevant to the tax base, we see that the FTT has a significantly negative effect on the tax base. As column (4) shows, a one percentage point increase in the FTT rate would reduce the tax base by approximately 23.5%, which is statistically significant at the 0.01 significance level. Even if we further control for the lagged FTT base, the effect is slightly weakened to 20.9%, and still statistically significant at the 0.05 significance level. Therefore, we see evidence that the FTT will narrow its tax base and thus will induce efficiency loss.

3.4 Concluding remarks

In this preliminary investigation of the effect of the FTT on revenue and tax base, I find that the FTT has no crowding-out effect on the revenue of other taxes as a whole. When looking at several major taxes, I find no association between the FTT and the excise tax revenue, and a small negative association between the FTT and revenues of the VAT and the PIT. I find a significantly positive association between the FTT and the CIT revenue. I attribute these results to a potential explanation that the FTT has trivial effects on small agents (e.g. most consumers) yet has considerable effects on large corporations. In addition, I find a negative effect of the FTT on its own tax base.

Better data on various tax rates and taxes revenue/GDP ratios can improve the empirical findings. The most striking finding – the positive relation between FTT and CIT revenue – is established with quite good data quality and robust to several specifications. Future work may explore this relation to a deeper extent.

This finding renders important insights. Previously the FTT is regarded as a way to collect revenue only for the short run, due to its potential disintermediation effects. It is also for this reason that the FTT is opposed by the IMF. However, here we see some evidence that the FTT may be beneficial to corporate activities or entrepreneurship. If it is true, then the FTT has important efficiency gain effects to the whole economy. Thus, future research can work on either (a) testing the potential hypotheses proposed in section 3.2.3 or (b) build formal model to explain such relation based on my potential hypotheses proposed in section 3.2.3.

Chapter 3 is currently being prepared for submission for publication of the material. The dissertation author, Xiaxin Wang, was the sole author of this paper.

3.5 Appendix Tables

Table 3.12. Variable definitions and data sources

Variable	Definition	Source
FTT_rate	Effective FTT tax rate weighted by dates (%)	Bacca-Campodonico et al. (2006)
FTT_rev_ratio	Fiscal revenues from the FTT over GDP (%)	and Restrepo (2013)
FTT_productivity	Ratio of FTT_rev_ratio over the effective tax rate	Self calculated
FTT_dummy	Dummy denoting whether FTT applies	Self calculated
ln_GDP_pc	Log of GDP per capita (constant 2005 US$)	World Bank dataset
ln_GDP	Log of GDP (constant 2005 US$)	World Bank dataset
ln_population	Log of population	World Bank dataset
Total_tax_rev_ratio	Total tax revenue as percentage of GDP	1. OECD dataset: Revenue
Excise_rev_ratio	Excise tax revenue as percentage of GDP	Statistics - Latin American
Corp_rev_ratio	Corporate tax revenue as percentage of GDP	Countries : Comparative tables; 2.
VAT_rev_ratio	VAT revenue as percentage of GDP	World Tax Database of Umich.
Other_tax_rev_ratio	Total_tax_rev_ratio-FTT_rev_ratio	Self calculated
CIT_rate	Statutory corporate income tax rate (%)	1. World Tax Database of UMich.
VAT_rate	Statutory value-added tax rate (%)	2. Corporate Tax Guides.
PIT_rate	Statutory top bracket personal income tax rate (%)	World Tax Indicators
Fin_ref_ind	Financial reform index	IMF Financial Reform Database
Banking_crisis	Dummies for years of banking crisis	Laeven and Valencia (2012)
ln_imputed_FTT_base	Dividing FTT tax revenue by the effective tax rate (log form)	Self calculated

Note: CIT revenue/GDP ratio for Bolivia and Ecuador, tax revenue/GDP ratio, VAT revenue/GDP ratio for Bolivia is from Latin America and the Caribbean Fiscal Burden Database (http://www.iadb.org/en/research-and-data/publication-details,3169.html?pub_id=IDB-DB-101).

Table 3.13. Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
FTT_rate	126	0.20	0.38	0.00	2.00
FTT_rev_ratio	126	0.35	0.59	0.00	2.51
FTT_productivity	41	2.32	1.22	0.63	5.12
FTT_dummy	126	0.33	0.47	0.00	1.00
ln_GDP_pc	126	7.97	0.55	6.69	8.68
ln_GDP	126	25.09	1.33	22.38	27.51
ln_population	126	17.12	0.91	15.68	19.04
Total_tax_rev_ratio	112	17.47	6.02	8.73	33.12
Excise_rev_ratio	96	1.24	0.93	0.00	4.55
Corp_rev_ratio	109	2.22	2.14	0.00	14.27
VAT_rev_ratio	112	5.02	2.08	0.00	10.40
PIT_rev_ratio	64	0.56	0.47	0.02	1.54
Other_tax_rev_ratio	112	17.08	5.82	7.75	31.76
CIT_rate	126	27.58	9.13	2.00	50.00
VAT_rate	68	14.44	3.38	6.50	21.00
PIT_rate	126	28.94	8.81	10.00	50.00
Fin_ref_ind	126	0.61	0.24	0.05	0.90
Banking_crisis	126	0.22	0.42	0.00	1.00
ln_imputed_FTT_base	41	26.46	1.52	24.04	28.86

Table 3.14. FTT implementation and VAT revenue

Dependent variable: VAT_rev_ratio						
	(1)	(2)	(3)	(4)	(5)	(6)
Lag_VAT_rev_ratio			0.697***			0.695***
			(0.04)			(0.15)
FTT_dummy	0.08	-0.828**	0.053	-0.521*	-0.384	0.219
	(0.54)	(0.26)	(0.14)	(0.25)	(0.44)	(0.34)
ln_GDP_pc		-2.615	-1.584*		-0.182	0.169
		(2.89)	(0.71)		(2.11)	(1.55)
ln_population		9.641*	2.331		3.398	-0.48
		(4.19)	(2.22)		(4.42)	(1.57)
Fin_ref_ind		2.885	0.217		3.439	0.694
		(1.61)	(0.93)		(2.09)	(1.57)
Banking_crisis		0.237	0.018		-0.174	-0.166
		(0.46)	(0.32)		(0.49)	(0.33)
VAT_rate				0.483**	0.176	-0.008
				(0.16)	(0.15)	(0.10)
Constant	4.996***	-140.999**	-25.785	-2.567	-56.04	7.941
	(0.19)	(53.08)	(34.55)	(2.38)	(63.33)	(19.88)
Observations	112	112	105	62	62	61
R-squared	0.001	0.604	0.79	0.451	0.672	0.82
Number of countrie	7	7	7	6	6	6

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** p<0.01, ** p<0.05, * p<0.1.

Table 3.15. FTT revenue and personal income tax revenue

Dependent variable: PIT_rev_ratio						
	(1)	(2)	(3)	(4)	(5)	(6)
Lag_PIT_rev_ratio			0.821***			0.830***
			(0.04)			(0.03)
FTT_rev_ratio	0.075	0.078	-0.01	0.114	0.084	-0.004
	(0.12)	(0.13)	(0.02)	(0.09)	(0.12)	(0.03)
ln_GDP_pc		2.588*	0.027		2.560*	0.078
		(0.92)	(0.43)		(1.01)	(0.43)
ln_population		-0.364	0.054		-0.133	-0.181
		(1.44)	(0.18)		(0.95)	(0.29)
Fin_ref_ind		0.345	0.098		0.214	0.186
		(0.68)	(0.19)		(0.60)	(0.16)
Banking_crisis		0.148	-0.052		0.145	-0.051
		(0.11)	(0.04)		(0.10)	(0.04)
PIT_rate				-0.02	-0.009	0.005
				(0.03)	(0.02)	(0.00)
Constant	0.526***	-14.335	-1.077	1.12	-17.831	2.438
	(0.06)	(24.81)	(1.82)	(1.08)	(18.20)	(2.82)
ObserPITions	64	64	60	64	64	60
R-squared	0.018	0.544	0.905	0.068	0.554	0.908
Number of countries	4	4	4	4	4	4

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** p<0.01, ** p<0.05, * p<0.1.

Table 3.16. FTT implementation and personal income tax revenue

Dependent variable: PIT_rev_ratio						
	(1)	(2)	(3)	(4)	(5)	(6)
Lag_PIT_rev_ratio			0.801***			0.814***
			(0.05)			(0.04)
FTT_dummy	-0.072	-0.237*	-0.038	-0.044	-0.235*	-0.031
	(0.08)	(0.09)	(0.03)	(0.05)	(0.10)	(0.04)
ln_GDP_pc		1.577**	-0.003		1.538**	0.034
		(0.29)	(0.34)		(0.28)	(0.32)
ln_population		2.452	0.304		2.689	0.072
		(2.64)	(0.29)		(2.28)	(0.34)
Fin_ref_ind		-0.239	0.047		-0.358	0.131
		(0.78)	(0.18)		(0.67)	(0.15)
Banking_crisis		0.105	-0.049		0.101	-0.049
		(0.08)	(0.04)		(0.08)	(0.04)
PIT_rate				-0.013	-0.008	0.005
				(0.04)	(0.01)	(0.00)
Constant	0.593***	-55.429	-5.211	0.983	-58.983	-1.624
	(0.04)	(45.10)	(3.72)	(1.17)	(40.45)	(4.49)
ObserPITions	64	64	60	64	64	60
R-squared	0.013	0.596	0.907	0.035	0.604	0.909
Number of countrie	4	4	4	4	4	4

Note: We apply fixed effect panel regressions for all results. Standard errors (in parentheses) are clustered at the country level, *** p<0.01, ** p<0.05, * p<0.1.

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