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PhD Thesis

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Tax Enforcement and Compliance: Theory and Empirics

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Contents

Acknowledgements

I started my university studies at the Department of Mathematical Sciences in 2009 with a pretty strong conviction of where my studies would eventually lead me. Today I am very happy that I was completely wrong. A number of people have been invaluable in guiding me here and this dissertation has gained enormously from the support and guidance of numerous people. I would like to start off by thanking some of them.

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Introduction - English

"In this world nothing can be said to be certain, except death and taxes" - these are the words of Benjamin Franklin, written in 1789 in a letter to the French scientist Jean-Baptiste Leroy. Two hundred years later in 1989 an article called *"The anatomy of tax evasion"* was published. The article starts: *"Three things are certain in life: death, taxes, and mankind's unrelenting effort to evade both"* (Klepper and Nagin, 1989).[1]

Any society with a need for government revenue must be concerned with enforcement of their tax system. In a reality characterized by increased globalization, ever-fading borders and a myriad of complex interactions between agents, enforcement of national tax systems inevitably becomes a complex issue. As the second quote above suggests, agents have an incentive to manipulate tax payments. Such opportunities present themselves in many shapes and sizes. A multinational firm can have a complex international structure and thereby exist in the intersection of multiple sets of national tax rules. This combination provides leeway for the firm to choose the location of the tax potential it generates. A private business owner can label income as either wages or profits and thereby decide which tax base it belongs to, and ultimately how it is taxed. Wage earners can over-report charitable contributions or over-state commuter allowances to lower tax liabilities. While these examples involve widely different agents and situations, they all constitute typical examples of tax avoidance or, in the case of legal violations, tax evasion. This dissertation consists of three self-contained chapters each dealing with some dimension of tax enforcement and compliance. Based on both detailed and novel data-sources, and various empirical and theoretical methods, the three chapters consider both the tax compliance decision of firms as well as enforcement and design features of rules

[1] Klepper, S., Nagin, D., 1989. *The Anatomy of Tax Evasion.* Journal of Law, Economics, and Organization, Vol. 5, No. 1, pp. 1-24.

governing the personal income tax.

Chapter 1 studies multinational firms and their responses to international tax rules on both the financial and location margin. The chapter considers one of the most advocated anti-tax-avoidance measures today: *Controlled Foreign Corporation rules*. By including income of foreign low-tax subsidiaries in the domestic tax base, these rules create incentives to move income away from low-tax environments. The study combines a large panel dataset on the worldwide operations of multinational firms with a novel and individually collected tax dataset on the details of CFC legislation in 25 different countries over an 11 year period. The particular design of CFC rules together with data on ownership ties across borders enable the use of unique time- and cross-sectional variation to identify effects of this border-crossing tax measure. Exploiting specifically the tax threshold used to identify low-tax subsidiaries, the study finds that multinationals redirect profits into subsidiaries just above the threshold and change incorporation patterns to place fewer subsidiaries below and more above the threshold.

In *chapter 2*, written in collaboration with Niels Johannesen, we incorporate the idea of multiple heterogeneous evasion technologies in a simple theoretical framework and consider the implications for the government's enforcement efforts. In standard models of tax enforcement, with a single evasion technology, stricter enforcement unambiguously leads to more compliance and more revenue. We show that in the presence of multiple evasion technologies, stricter tax enforcement may lead to more tax evasion and it may be associated with a loss of government revenue and lower welfare. Intuitively, increasing the probability of detecting one of the evasion technologies induces some firms to switch to the other technology. If the latter technology is more expensive but allows for more evasion, then this behavioural effect may dominate other effects and evasion may increase while revenue drops.

Finally, *chapter 3*, written in collaboration with Panos Mavrokonstantis, deals with a slightly different dimension of tax policy and enforcement. The study is based on administrative data from the Tax Department of the Republic of Cyprus and explores interesting new variation in tax rules and enforcement policies. We study both behavioural responses to tax enforcement as well as the importance of enforcement policy design for behavioural elasticities, using the context of charitable contributions in Cyprus. The chapter provides three sets of results.

First, exploiting salary-dependent thresholds governing the documentation requirements when claiming deductions for contributions, we estimate that reported donations increase by 0.7 pounds when taxpayers can claim 1 pound more without providing documentation. Second, using a reform that retroactively shifted the reporting threshold we estimate that at least 36 percent of these observed responses are purely due to changes in reporting. Third, we estimate a tax price elasticity of reported donations of -0.5 and show that this parameter is highly sensitive to policy design features such as thresholds determining reporting standards.

All three chapters are included separately below. Appendices with supplementary material as well as a separate bibliography can be found immediately after each chapter.

Introduction - Danish

Ethvert samfund med offentlige udgifter og et behov for skatteindtægter må bekymre sig om skatteunddragelse og andre udfordringer med håndhævelsen af eksisterende skatteregler. I en nutid med stigende globalisering er håndhævelse af nationale skattesystemer uundgåeligt et komplekst emne. Privatpersoner såvel som firmaer har incitamenter til at udnytte muligheder for at sænke deres skattebetalinger. Sådanne muligheder opstår ofte og i mange forskellige situationer. Et multinationalt selskab kan have en yderst kompleks international struktur og kan dermed eksistere i krydsfeltet mellem flere forskellige sæt nationale skatteregler. Denne kombination giver selskabet mulighed for at vælge placeringen af det skattepotentiale, som selskabet repræsenterer. En privat ejer af et selskab kan klassificere indkomst som enten profit eller løn og kan dermed vælge, hvilken skattebase indkomsten tilhører, og følgeligt hvordan den skal beskattes. En lønmodtager kan over-rapportere sit kørselsfradrag eller alternativt sine donationer til velgørende formål for at sænke sine skattebetalinger. Disse eksempler involverer meget forskellige agenter og situationer, men de repræsenterer alle typiske eksempler på skatteunddragelse eller skatteundvigelse. Denne afhandling består af tre kapitler, som fra forskellige vinkler omhandler håndhævelsen af skatteregler. Baseret på detaljerede datakilder, og forskellige empiriske og teoretiske metoder, behandler de tre kapitler både skattehåndhævelse i forhold til multinationale selskaber, som opererer på tværs af grænser, såvel som privatpersoner, som vælger de oplysninger, der opgives til skattemyndighederne.

Kapitel 1 omhandler multinationale selskaber og internationale skatteregler. Studiet ser på en af de mest anvendte anti-unddragelses regler: CFC (controlled foreign corporation) regler, og hvordan selskaber reagerer på den finansielle margin og i forhold til den internationale placering af datterselskaber, når de rammes af sådanne regler. Analysen finder, at multinationale selskaber re-allokerer profit til datterselskaber, som ikke rammes af reglerne og ændrer deres inkorporationsmønstre således, at de placerer færre datterselskaber i lande, som rammes, og flere i lande, som ikke rammes af reglerne.

Kapitel 2, skrevet i samarbejde med Niels Johannesen, omhandler ideen om, at selskaber har flere forskellige metoder til rådighed, hvorpå de kan unddrage eller undvige skat. I et simpelt teoretisk setup introducerer vi denne mulighed og studerer implikationerne for effekten af regeringens håndhævelsesindsats. I traditionelle modeller fører strengere håndhævelse utvetydigt til mindre unddragelse og mere skatte-provenu. Vi viser, at hvis man tillader flere forskellige unddragelses-metoder, så kan strengere håndhævelse føre til mere unddragelse og til et tab af skatteprovenu og velfærd.

Kapitel 3, skrevet i samarbejde med Panos Mavrokonstantis, omhandler en anden dimension af skattepolitik og håndhævelse. Studiet er baseret på administrativ data fra Cyperns skatteministerie og udnytter interessant ny variation i skatteregler og håndhævelses-metoder. Vi undersøger reaktioner på håndhævelses-initiativer, såvel som vigtigheden af den valgte håndhævelsesmetode for beregnede elasticiteter, ved at se på donationer til velgørende formål i Republikken Cypern. Studiet viser, at privatpersoner ændrer deres rapporterede donationer, når dokumentationskrav ændrer sig, men at en stor del af denne reaktion blot repræsenterer ændringer i rapporteringsadfærd og ikke i reel donationsadfærd.

1 | Taxing multinationals beyond borders: Financial and locational responses to CFC rules

Taxing multinationals beyond borders: financial and locational responses to CFC rules

Sarah Clifford†

May, 2018

Using a large panel dataset on worldwide operations of multinational firms, this paper studies one of the most advocated anti-tax-avoidance measures: *Controlled Foreign Corporation rules*. By including income of foreign low-tax subsidiaries in the domestic tax base, these rules create incentives to move income away from low-tax environments. Exploiting variation around the tax threshold used to identify low-tax subsidiaries, we find that multinationals redirect profits into subsidiaries just above the threshold and change incorporation patterns to place fewer subsidiaries below and more above the threshold. Roughly half of the resulting increase in global tax revenue accrues to the rule-enforcing country.

Keywords: *CFC legislation · Multinational firms · Tax avoidance · Corporate taxation*
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†Department of Economics, University of Copenhagen, Øster Farimagsgade 5, 1353 Copenhagen K, Denmark (e-mail: sarah.clifford@econ.ku.dk). I acknowledge financial support from the Danish Council for Independent Research. I am especially grateful to Niels Johannesen for valuable feedback and comments. I am also grateful to Simon Boserup, Anzhela Cedelle, Michael Devereux, Henrik Kleven, Claus Thustrup Kreiner, Panos Mavrokonstantis, Benjamin Ly Serena, Adam Sheridan, Martin Simmler, Eddy H.F. Tam and Thomas R. Tørsløv for helpful comments and suggestions. Furthermore I gratefully acknowledge the useful comments and suggestions from seminar and conference participants at the University of Copenhagen, London School of Economics, Oxford University CBT, ZEW Mannheim and Max Planck Institute for Tax Law and Public Finance. All remaining errors are my own.

I. Introduction

The combination of a globally integrated market and sizeable tax rate differentials has created a foundation for tax planning opportunities. In 2014 thousands of documents surfaced in Luxembourg giving the public rare insight into the concrete tax avoidance schemes of large multinational enterprises. Evidence from this so-called *Luxleaks* scandal resonates with an empirical literature documenting substantial tax-related profit shifting (Huizinga & Laeven 2008, Heckemeyer & Overesch 2013, Dharmapala & Riedel 2013). The loss on global corporate tax revenue associated with this behaviour is considerable. Zucman (2014) estimates that the use of low-tax jurisdictions by multinational firms is associated with a 20 percent reduction in the taxes paid by US owned corporations.

In response to this comprehensive migration of profits to low-tax environments, many countries have extended their international tax policy rules with measures designed to curb profit shifting. One of the most important examples is legislation known as *Controlled Foreign Corporation (CFC) rules*. The importance of this measure is reflected in its global prevalence as well as in the political debate where the measure is highly advocated by the OECD in their 2015 BEPS report as well as by the EU commission in several proposed directives.[1] Around two thirds of the OECD countries have CFC legislation and several of the remaining nations are debating the introduction of such a regime.

The CFC measure is essentially a border-crossing corporate tax on multinational firms. The rules concretely impose an immediate and direct tax on selected parts of the income of low-tax foreign subsidiaries. The German rules constitute a typical example of the CFC regimes in place across the world. Germany classifies any foreign subsidiary taxed at a rate lower than 25 percent as a low-tax subsidiary, and targets income such as interest and royalty income from such subsidiaries. The German CFC rules work by including the interest and royalty income of foreign subsidiaries taxed at rates below 25 percent in the German corporate tax base and taxing this income at the much higher German corporate tax rate. Though the design of CFC legislation varies across countries, the German setup is highly representative. Important for the empirical design in this paper most countries set a low-tax threshold as the

[1] For instance *The Anti Tax Avoidance Directive* proposed in January 2016.

German 25 percent cut-off. From a pure tax perspective, these rules create an incentive for multinationals to move income types such as interest income away from low-tax subsidiaries and into higher tax host countries above the specified threshold. In line with the incentive to relocate income, they create an incentive to reorganize geographical firm structure and open new subsidiaries in higher tax environments. One obvious objective of this direct tax is to protect the domestic tax base by eliminating incentives for geographical relocation of profits to foreign low-tax subsidiaries. When profits can escape the reach of domestic tax rules it threatens the enforceability of these rules and ultimately the ability of governments to collect tax revenue. Base protection and enforceability in general are integral parts of a country's tax system and are crucial for the feasibility of optimal tax policy design.

In this paper, we explore the potential of CFC rules in obtaining this objective by studying the behavioural responses of multinational firms. We investigate how the financial and locational structures within multinationals change in response to this tax measure and what this means for domestic and global tax revenue. The analysis draws on a large global dataset containing subsidiary-level panel data from more than 50 countries spanning 25 different CFC regimes.[2] With its global reach and panel structure the data enables within-subsidiary analysis using exogenous reform variation. We exploit changes in CFC treatment created by the interplay of the low-tax threshold and general tax reforms over time. The low-tax threshold embedded in most rules further facilitates direct non-parametric discontinuity estimates of location responses.

Throughout we uncover sizeable behavioural responses to the CFC tax measure. The distribution of financial profit displays a sharply discontinuous pattern around the low-tax threshold consistent with relocation of profits into higher-tax environments. Such non-parametric evidence is supported by a within-subsidiary analysis exploiting reform-induced movements across the threshold. When a reform moves a subsidiary below the CFC tax threshold, we estimate a 13 percent subsequent drop in financial profit within the subsidiary. This suggests that CFC rules, by imposing a potential tax penalty, induce multinationals to move substan-

[2]The data spans 25 different CFC regimes, but we are only able to include 20 of these regimes in the main analysis due to specific requirements related to the empirical design. We explain these requirements in the later sections.

tial amounts of income out of low-tax environments.

To understand the mechanisms behind this effect we broaden the focus from the single subsidiary to the multinational group and consider spillover effects. The fraction of subsidiaries placed below the tax threshold is a measure of group-exposure to the CFC policy. Using variation in this measure, we present evidence that increases in exposure leads to increases in financial profit within higher tax environments. When more subsidiaries are targeted by the tax the un-targeted subsidiaries generate more profit. The combination of these results points to an underlying shifting mechanism within multinationals going from low-tax targeted subsidiaries to higher tax subsidiaries outside the scope of the rules. By separating spillover effects on domestic and foreign corporations we find that both groups are on the receiving end of relocated profits. Consequently, the CFC instrument leads to increases in corporate tax revenue, that benefit both the rule-enforcing country and other higher-tax countries. In essence the measure protects both the global as well as the domestic tax base without any coordination between countries. This domestic component in the revenue gain is potentially important in a context of international tax policy where the need for coordination has been highlighted as crucial for a successful fight against tax avoidance (OECD/G20, 2015)[3].

Aside from responses on the allocation of income, we also investigate extensive margin responses on geographical firm structure. Exploiting the unique design of CFC rules we are able to present non-parametric evidence and elicit a direct discontinuity estimate of the effect of CFC tax policy on the discreet location choice. In contrast to the traditional choice model framework our alternative approach avoids the heavy reliance on parametric assumptions and allows us to directly observe the identifying variation in the data. We find that groups affected by a CFC regime display a higher propensity to choose hosts just above the tax threshold and a lower propensity to choose hosts below the threshold compared to groups unaffected by the legislation.

Despite the magnitude of the monetary sums involved and the potential relevance for tax policy design, behavioural responses to CFC tax rules have received very limited attention.

[3]The OECD/G20 write in their Base Erosion and Profit Shifting (BEPS) Project, 2015 report: "*The G20 and the OECD have recognised that BEPS by its very nature requires coordinated responses, which is why countries have invested the resources to participate in the development of shared solutions*".

Altshuler & Hubbard (2002) focus on a reform from 1986 of the US CFC rules and use country-level aggregate data on asset holdings to show that certain US owned firms become less sensitive to host country tax rates after a tightening of the rules.[4] Ruf & Weichenrieder (2012) study the asset allocation within German multinational groups and conclude that CFC rules affect the geographical pattern of asset holdings. Egger & Wamser (2015) introduce a multi-dimensional regression discontinuity design to analyse the effect of CFC legislation on the allocation of foreign real investments of German multinationals. They also find substantial differences across subsidiaries treated differentially by the rules.[5]

This paper contributes to several strands of literature. Firstly we contribute to the literature on CFC rules and tax avoidance in several dimensions. We present the first non-parametric graphical evidence showing discontinuities created by CFC tax legislation. Such graphical evidence is provided both for the intensive margin of income allocation and for the extensive margin of subsidiary location. On the intensive margin, we directly study generated profits within multinational corporations. While previous literature has focused solely on the allocation of assets, we concentrate on the actual object of taxation and thereby avoid assumptions on the relationship between asset value and income flows. In essence, we use an outcome that directly reflects the corporate tax base of countries. Further, we present the first evidence on the consequences of CFC legislation for the domestic corporate tax base. Identifying a domestic component within the global tax base response is potentially crucial for policy makers with national objectives of creating tax revenue by fighting corporate tax base erosion.
The remaining contributions to this strand of literature are related to identification issues and elimination of potential biases. We present the first micro-study of CFC rules isolating within-subsidiary responses to exogenous variation in targeting. The reach of the dataset enables specifications relying solely on time variation from tax reforms affecting foreign subsidiaries differentially. Further, we depart from the one country case and instead introduce a com-

[4]Mutti & Grubert (2007) show however, that after the 1997 implementation of check-the-box rules, US multinationals can avoid triggering the US CFC rules by creating hybrid entities.

[5]A small theoretical literature on CFC rules exists. Weichenrieder (1996) shows that an exogenously imposed CFC rule increases the cost of capital for treated foreign subsidiaries, while Haufler et al. (2017) endogenize the choice of a CFC rule in an optimal tax framework. Within this framework, they derive conditions for the optimality of a CFC rule in combination with other tax instruments.

prehensive multi-country framework covering many different CFC policy regimes. Studying multinationals from a single regime means that any reform of the tax component determining treatment will affect all subsidiaries within the same foreign host country identically. Introducing a second home country secures variation across subsidiaries within the same host country. This variation is essential because it provides control groups to account for the confounding influence of changes in market conditions affecting corporations operating on a specific market. The multiple country framework hereby allows us to present a study isolating within-subsidiary variation while controlling for market time-trends to identify causal effects of CFC legislation.

Our analysis also contributes to a second strand of literature dealing with the tax determinants of the location choices within multinationals. Barrios et. al. (2012) find that the subsidiary location choice within multinational corporations is sensitive to taxes in several dimensions - both host country corporate taxes and parent-country tax rules affecting foreign income. Buettner & Ruf (2007) find a high sensitivity especially to statutory tax rates in host countries using a sample of German multinational corporations. Devereux and Griffith (1998) find an effect of average effective tax rates on the foreign location choices of US multinationals. Voget (2011) looks at the decision to relocate headquarters and finds an increased probability of relocation for multinationals headquartered in home countries with higher tax levels on foreign repatriated income. He introduces a dummy for the presence of a CFC regime in the headquarter-country, and concludes that such legislation increases the probability of relocation.[6]

We contribute to this literature by investigating the consequences of the CFC anti-tax-avoidance measure for the geographical pattern of subsidiaries within multinational firms. The pure extensive margin of response is a yet unexplored margin in the context of this tax measure.[7] We further contribute by exploiting the unique design of CFC rules to visualize the variation in the data and avoid the heavy parametric assumptions necessary in previous literature on location choices. The unique setup allows us to produce a non-parametric dis-

[6]For a survey of empirical evidence on the effect of taxation on firm-choices see Devereux & Maffini (2007)

[7]Ruf & Weichenrieder (2012) present an analysis of the asset allocation choice within multinationals, which is slightly related in the sense that they model the geographic location choice of large amounts of passive assets using a conditional logit framework.

continuity estimate of the effect of tax policy on location choices.

The rest of the paper is organized as follows: section II describes the international tax environment facing corporate groups and explains in detail the components of CFC legislation. Section III introduces the data. Section IV puts forward brief theoretical considerations on the incentives created by CFC rules and describes the main variation we exploit for identification. Section V presents the results on both the intensive and extensive margin of response and section VI concludes.

II. The international tax environment for corporate groups

A salient feature of the corporate landscape today is a remarkably high level of global integration. Such integration complicates taxation of corporate income and renders corporate tax policy a border crossing issue. Consider a multinational group comprising a parent and several subsidiaries. When profit is generated in both the home country of the parent and in one or several of the subsidiary host countries, a fundamental question arises of how to tax this income. Most jurisdictions accept the premise that all corporations in the structure are separate taxpayers and hence each subsidiary is subject to corporate income tax in the host country of incorporation. However, due to the ownership structure of the group all profits are essentially owned by the parent company and this entity can determine a partial or full repatriation of the income at any point in time. To avoid double taxation most home countries operate an exemption system when within-group profits are remitted. This effectively means that such income is not subject to further taxation by the home country when received by the parent[8]. While this method solves the problem of double taxation it evidently also leaves scope for tax planning using host countries with low tax rates. With the objective of protecting their domestic corporate tax base, high tax home countries have an interest in minimizing potential abuse of this scope.

[8]A few countries opt for a credit system instead of an exemption system. Under a credit system the income is taxed when repatriated to the home country, but a credit is given for all foreign taxes already paid.

II.1. Controlled Foreign Corporation (CFC) rules

In an attempt to control such tax planning, many countries have introduced so-called *Controlled Foreign Corporation (CFC)* rules. This policy introduces a potential direct tax on foreign subsidiaries of domestic corporations - a corporate tax beyond borders. Consider a multinational firm consisting of three entities: a parent corporation in Germany, a subsidiary in Singapore and a subsidiary in Canada. CFC legislation in Germany will place a tax on the interest income of the Singaporean subsidiary since Singapore has a low tax rate, while the interest income of the Canadian subsidiary will remain untaxed by Germany because Canada already taxes the income at a high rate.[9] Broadly, CFC legislation enables the home country to tax the income of foreign subsidiaries, if this foreign income lives up to certain criteria. Included criteria vary slightly across regimes, but they tend to include three basic components: a control-, a tax level- and an income source criterion. In consequence, the part of the income of foreign subsidiaries which is most likely to be involved in tax avoidance strategies, is included in the tax base of the parent corporation and taxed directly by the home country. We briefly explain the common features of each criterion to clarify the incentives created by this type of tax legislation.

The control criterion broadly ensures that the shareholder is in possession of control over financial decisions of the subsidiary and is hence able to utilise this ownership privilege for tax avoidance purposes. Most CFC regimes that are currently in place require ownership of the majority of voting rights or some financial privilege such as the right to profit upon distribution. In the example above, the German parent must have control over the financial decisions of the Singaporean subsidiary for CFC rules to activate.

The tax-level criterion is included to embed a focus on low taxed income as this seems a logical trait of income used as a vehicle for avoidance motives. Typically this requirement takes the form of a fixed minimum tax level calculated by looking at the actual taxes paid. This level can be stated as a fixed absolute rate, or as a percentage of the calculated tax liability had the subsidiary been liable for taxation in the parent country. It is only the subsidiary in Singapore

[9] It is possible that the interest income of the Singaporean subsidiary can be exempt from the rules, if the subsidiary has enough active business income. Such specific details of each set of rules are explained in Appendix C.

and not the Canadian which is targeted by the German CFC regime, because Singapore has a low tax rate on corporate profits while Canada does not.

The final criterion concentrates on the source of the income. Most often legislation targets only passive (or tainted) income. The term passive income generally covers sources such as financial income from portfolio holdings, certain types of rental income and royalties from intellectual property. These types of income are perceived as very mobile making them ideal for income shifting with avoidance motives. Income of the Singaporean subsidiary earned through sales of actual products is not affected by the German CFC rules while interest income is included and potentially taxed by Germany.

If a subsidiary fulfils all specified criteria, the passive income of the subsidiary is included in the tax base of the parent corporation and taxed at the rate of the home country. The taxation takes place on accrual basis, and is not postponed until a potential repatriation. Overall this potentially creates an incentive for multinationals to generate passive income in higher tax environments, than would otherwise have been optimal absent the legislation.

The data source underlying our empirical analysis allows us to look at multinational firms from 25 different parent countries with active CFC legislation in some part of the sample period.[10] A simplified overview of these 25 regimes is given in Appendix C. The table in this appendix describes the specifics of each of the three aforementioned criteria for each separate country, as well as further details that are potentially relevant. The data source further includes multinational firms from a large number of parent countries with no CFC legislation in the sample period. An initial simple comparison between the 25 CFC countries and the remaining sample countries reveals an interesting starting point. On average corporate multinational groups under a CFC regime appear to be generating passive income in higher tax environments compared to groups where the parent is located in a country with no such regime. This pattern is illustrated in Figure 1 showing the empirical cumulative distribution function across the range of corporate tax rates for financial profit. Each point represents the

[10] This number is based on an inspection of the legislation present during the sample period in the 74 non-haven home-countries that account for at least 50 majority-owned observations where the main outcome variable is non-missing. The legislation of countries that account for less than 50 observations is not explicitly scrutinised.

fraction of total positive financial profit generated within corporations facing a corporate tax rate below the specified value. The figure shows the function separately for groups with a parent based in a CFC country, and for groups where the parent is unaffected by such rules. Importantly we observe a CDF for targeted groups that is consistently located below the CDF for non-targeted groups. This feature implies that groups facing a CFC regime tend to choose higher tax environments when allocating passive income. CFC targeted groups generate only 8 percent of their total positive financial profit in host countries with tax rates below 25 percent, while non-targeted groups generate 22 percent of positive financial profit in these low tax environments. Clearly there are considerable differences in the distributional patterns of passive income when we compare potentially targeted and non-targeted groups. The fundamental question is then whether this difference can be attributed to effective CFC tax legislation and subsequently what this means for domestic and global corporate tax revenues.

III. Data

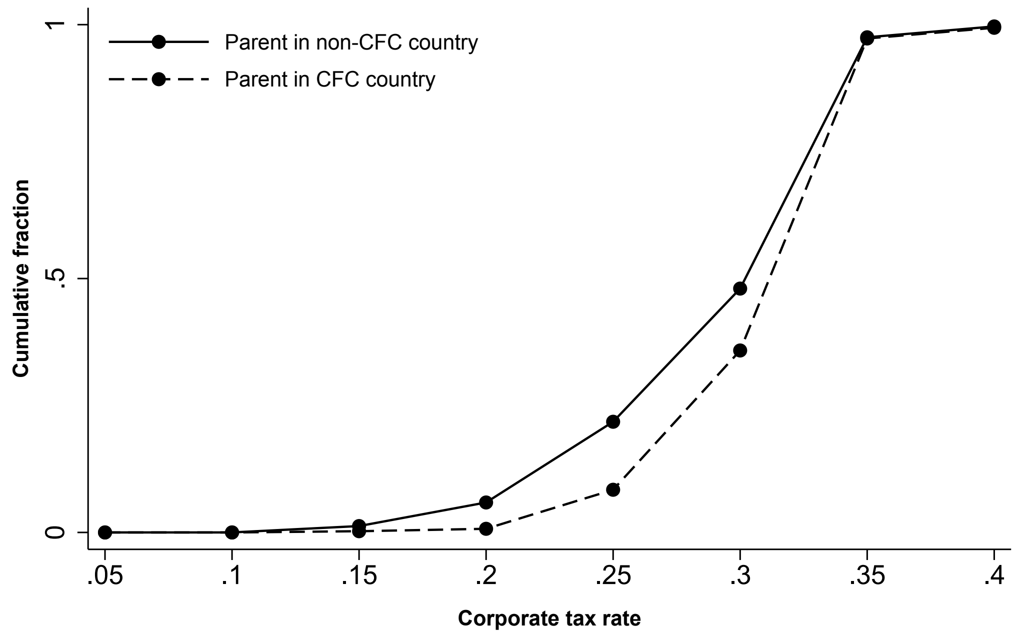
Throughout the analysis we use data from the Orbis database (2013 version) provided by Bureau van Dijk. The database contains firm-level panel data across the period 2003-2013 on a large number of financial and organisational variables. Importantly ownership links and percentages are provided for each separate firm - a feature that enables the linkage of each subsidiary to its parent corporation.[11] Since CFC legislation imposes a tax on foreign subsidiaries of domestic corporations, the relevant rules for a specific subsidiary are those in place in the country of the parent. Consequently the recorded ownership links make it possible to disentangle the relevant CFC rules for each particular subsidiary.[12] We are not able to distinguish

[11]The ultimate owner or parent firm is here defined as the highest placed shareholder in the corporate structure who has the majority ownership of the subsidiary either directly or indirectly.

[12]In the case of ownership chains, situations can emerge where there are some doubt as to which country's CFC legislation is relevant. To exemplify consider a German parent owning a Canadian subsidiary which again owns a Singaporean subsidiary. If the subsidiary in Singapore fulfils the requirements of both the Canadian and the German CFC rules, it can be unclear which CFC regime should activate. This kind of double CFC taxation is not explicitly dealt with in most regimes, but seems to be less of an issue in practice. Consequently we will abstract from this mostly theoretical issue and consider the legislation active in the country of the ultimately owner (here the German corporation) to be the relevant legislation.

Figure 1:

Empirical CDF - financial profit across corporate tax rates



Notes: The figure is constructed using only observations with positive financial profit. Each point represents the fraction of total financial profit belonging to corporations located in countries with a corporate tax rate below or equal to the value indicated on the x-axis. All multinational groups are divided into two: those where the parent is located in a home country with a CFC regime and those where the parent is located in a home country with no CFC regime. We exclude all groups where the parent is situated in a tax haven, since investment patterns in these jurisdictions might be very distinct and not representative of investment patterns more generally. Including these observations results in a very similar figure. All entities of the multinational groups are considered - i.e. both the parent and (majority owned) subsidiaries. Excluding the parent corporations or using only fully owned subsidiaries result in a very similar figure.

between direct and indirect ownership, but since CFC rules generally target both directly and indirectly owned subsidiaries this distinction is irrelevant in our context. Ownership links are only available in the database for the most recent date of information. To the extent that these ownership structures have changed in the period from 2003 to 2013, we are potentially misspecifying the link between parent and subsidiary in parts of the sample period. In line with other studies using this type of data, this is not considered a great concern, since such misspecification will create noise and should therefore only bias our results towards zero.

The final dataset contains multinational groups from more than 50 different home countries with subsidiaries in more than 50 different host countries.[13] We define a multinational group as a corporate construction consisting of a parent company and at least one foreign subsidiary.[14] Multinational firms in the data vary significantly in size - from a few members to global groups counting hundreds of entities. We exclude banks since these are practically never included under CFC rules, but otherwise we allow all firm types. We do not consider multinational groups from tax haven countries since these are never affected by CFC rules and their investment and incorporation patterns are plausibly very different from those of other countries.[15]

As mentioned 25 home countries have active CFC legislation in some part of the sample period. Information on the legislation in each of these countries during the sample period was individually collected using various sources including the *International Fiscal Association* (2013) and numerous legal documents. This legislation is briefly summarised in Appendix C. The dataset is further extended with statutory tax rates from *KPMG*, *Ernst & Young* and *Deloitte*. Note that throughout most of the analysis we isolate variation at a level where country controls are obsolete. However, where country control variables are needed we add country characteristics from the *World Bank Development Indicators* and *Worldwide Governance Indicators*. Descriptive statistics of subsidiary level financials, as well as group characteristics are given in the Appendix Table 4.[16]

Note that the dataset is not balanced across the 25 different CFC regimes - some countries account for a larger share of the total number of included multinational groups. In the Appendix Figure 8 we show the number of observations attributable to each of the 25 home countries. First note that a number of policy regimes each contribute with a large observation

[13]Excluding observations where the main outcome variable is missing, the dataset contains 63 non-haven home-countries with more than 100 majority owned observations. These are listed in the Appendix Table 5. The total number of host countries is 91 and of these 52 supply more than 100 observations. These are listed in the Appendix Table 6.

[14]When considering profit shifting we require at least three entities within the group (i.e. at least two subsidiaries) to ensure the option of shifting income to another location is feasible in the short run. Changing the definition to allow groups with only one subsidiary introduces more noise, but does not qualitatively change any results.

[15]We use the list provided in Hines (2010) to define a tax haven.

[16]Note that we exclude all observations where financial information is based on consolidated accounts, since we cannot disentangle the individual shares of each subsidiary.

count alleviating the concern that results could be driven by few unrepresentative regimes. It is also worth highlighting that a number of regimes contribute with very few observations - for instance three regimes account for less than 1000 observations each.

IV. Variation and Empirical methodology

A common feature across many CFC regimes is the inclusion of a low-tax threshold. Of the 25 sample countries with CFC rules 21 of them have a low-tax criterion formulated directly as a threshold value. In our empirical methodology we exploit this threshold for identification and hence our analysis is based on these countries. The CFC regime in the United States has features aside from the threshold making our approach infeasible and hence we look specifically at the case of the US in Appendix B. In the Appendix Figure 9 we show the placement of the low-tax threshold over the sample period for the remaining 20 CFC countries. The levels vary significantly across regimes, and within most countries we observe movements of the threshold over time. Since thresholds can be specified as a percentage of own tax level, these movements are often results of general tax reforms not directly related to CFC rules. Such indirect level-changes arguably render endogenous placement less likely. Below we explain in more detail how tax conditions vary across this threshold and what incentives this creates for the multinational firm.

Denote the statutory corporate income tax rate in home country j, $\boldsymbol{\tau}_j$ and similarly denote the tax rate in host country i, $\boldsymbol{\tau}_i$. Assume that home country j has a CFC regime in place with a low-tax threshold at α.[17] A subsidiary in host country i with a parent in home country j then faces the tax rate $\tau_{i,j}$ on passive profits, where

$$\tau_{i,j} = \begin{cases} \boldsymbol{\tau}_j & \boldsymbol{\tau}_i < \alpha \\ \boldsymbol{\tau}_i & \boldsymbol{\tau}_i \geq \alpha \end{cases}$$

Consider the full range of possible subsidiary tax rates i.e. the range of host country rates. In the decision of where to locate passive income, the multinational firm faces a tax notch just

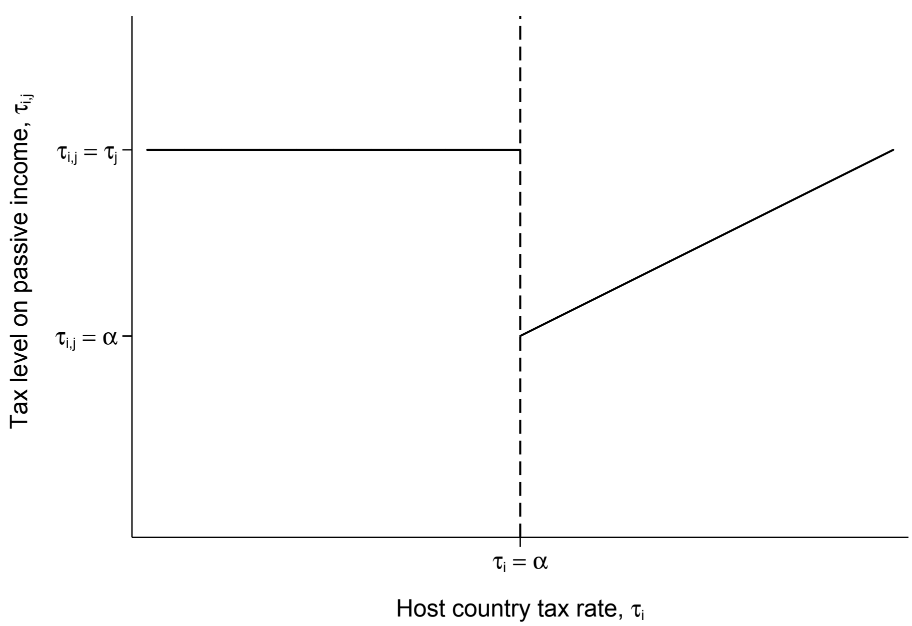
[17] Note that the stylised presentation here assumes a low-tax cut-off specified in terms of a statutory flat corporate tax rate in the host country. In reality this is typically not the case: thresholds are more often specified in terms of an effective tax measure taking the actual tax paid by the subsidiary into account. However, the statutory tax rate acts as a proxy for this effective tax rate in these cases.

at the low-tax threshold α. Since thresholds are always placed below the home country rate, this notch always features as a downward drop with a magnitude of $\alpha - \boldsymbol{\tau}_j < 0$. Considering a single home country we depict the tax schedule faced by the multinational firm across possible host rates in Figure 2. In the empirical analysis we consider multiple home countries and CFC regimes with different threshold values. While both the absolute size of α as well as the relative size of α compared to τ_j varies across regimes, they all introduce a tax schedule qualitatively illustrated by Figure 2. In the following analysis we utilise the incentives created by this tax schedule to identify the effects of the CFC tax measure.

Our variable of interest is an indicator variable $\mathbb{1}_{[CFC]sht}$ which is equal to 1 if host country

Figure 2:

Taxes on passive income under CFC regime with low-tax threshold

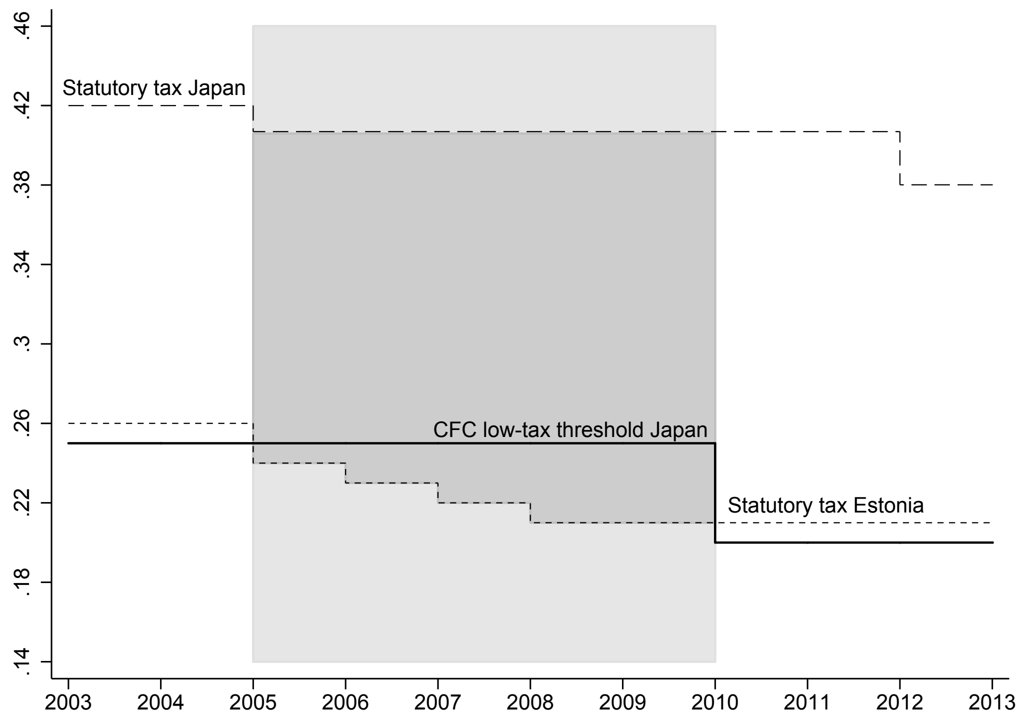


Notes: A stylized illustration of the effective tax rate applicable to the passive profit of multinationals as a function of the host country tax rate faced by the subsidiary generating the profit. We denote the low-tax threshold α, the home country tax rate τ_j and the host country tax rate τ_i.

s has a corporate income tax rate at time t that falls under the low-tax threshold in the CFC legislation of home country h. Related to Figure 2 this variable is equal to 1 if a subsidiary is placed to the left of the vertical dashed line and 0 if a subsidiary is placed to the right of the dashed line. Since we are using a panel dataset we can exploit within-subsidiary variation from reform-induced movements across the low-tax threshold. Consider Figure 3 showing a stylized illustration of the Japanese CFC regime during the sample period. For

Figure 3:

Illustration - time variation in CFC status



Notes: The figure illustrates the CFC regime in Japan from 2003 to 2013 and the primary time variation used to identify responses to these tax rules. The low-tax threshold in Japan was set at 25pp before 2010, but was changed in 2010 to a value of 20pp. In 2005 a tax reform took place in Estonia moving the top statutory corporate tax rate from 26 to 24 percent and thereby crossing below the CFC low-tax threshold in Japan. The reform therefore moved Estonian subsidiaries with parent firms in Japan into potential CFC taxation. In 2010 the Japanese low-tax threshold was lowered and thereby Estonian subsidiaries was moved back above the threshold and out of potential CFC targeting. Note that this is a stylized illustration that does not factor in the differences between statutory and effective tax rates and how this matters for the Japanese CFC regime.

illustrative purposes we use Japan and Estonia since this specific example encompasses two of the main dimensions of time variation in the data. In 2005 a tax reform in Estonia changed the statutory corporate income tax rate from 26 to 24 percent and thereby moved the rate below the CFC low-tax threshold in Japan. The reform hereby moved Estonian subsidiaries with parent corporations in Japan into potential CFC targeting. In 2010 Japan lowered its CFC low-tax threshold and hence moved Estonian subsidiaries back above the threshold. The grey shaded area indicates horizontally the period in which Estonian subsidiaries were targeted by the Japanese CFC legislation, while the darker grey shaded area illustrates vertically the potential tax penalty introduced by these CFC rules for Estonian subsidiaries. This is one example illustrating the two main types of time-variation: host country tax reforms and home country movements of the low-tax threshold. Given our sample restrictions the data contains shifts from 60 different country-pairs. Our main empirical strategy, when analysing financial responses, exploits only variation coming from shifter corporations i.e. subsidiaries that are moved across the relevant threshold by reforms. In our analysis of the discreet location choice we use a regression discontinuity approach to produce a direct discontinuity estimate around this embedded low-tax threshold.

V. Results

If multinational firms react to CFC rules we should be able to observe this reaction around the tax threshold triggering potential CFC taxation. In Figure 4 each subsidiary is sorted according to the statutory tax rate in their respective host country relative to this threshold. This means that a Canadian subsidiary of a German parent will appear on the graph at a value of 1 in year 2012, since the Canadian rate of 26 percent is 1 percentage point higher than the German low-tax threshold of 25 percent. Consequently, subsidiaries placed to the right of zero are not targeted by CFC taxation while subsidiaries placed to the left are potentially targeted as a result of meeting the low-tax criterion. Figure 4 plots in black the overall ratio of financial to operating profit allocated within each separate bin around the low-tax threshold. A striking feature of the overall pattern is a massive jump in the relative amount of financial profit just at the threshold value. The jump constitutes more than a doubling from around 20 percent to almost 50 percent. To illustrate the variation and incentives created by CFC rules we include in grey the average statutory tax rate on passive income across subsidiaries in each bin. The figure clearly shows the tax notch created by the legislation at the threshold. CFC rules mechanically create this tax notch by upholding the home country rate as the tax rate on the left hand side of the threshold.

From a pure tax perspective this notch in the tax schedule creates an incentive to generate passive income in subsidiaries located just above the threshold. The discontinuous pattern we observe in the relative concentration of financial profit is exactly consistent with these incentives around the cut-off. Further, the decreasing pattern on the right side of the threshold indicates tax sensitivity in line with the increasing average statutory tax rate. We do not observe a pronounced pattern to the left of the threshold consistent with the flat tax schedule under CFC rules. In Appendix Figure 11 we show an alternative version of the figure using only subsidiaries with a known ownership percentage above 50 percent. This is almost exclusively fully owned subsidiaries. The figure shows a very similar pattern and also feature a doubling of the ratio exactly at the threshold.

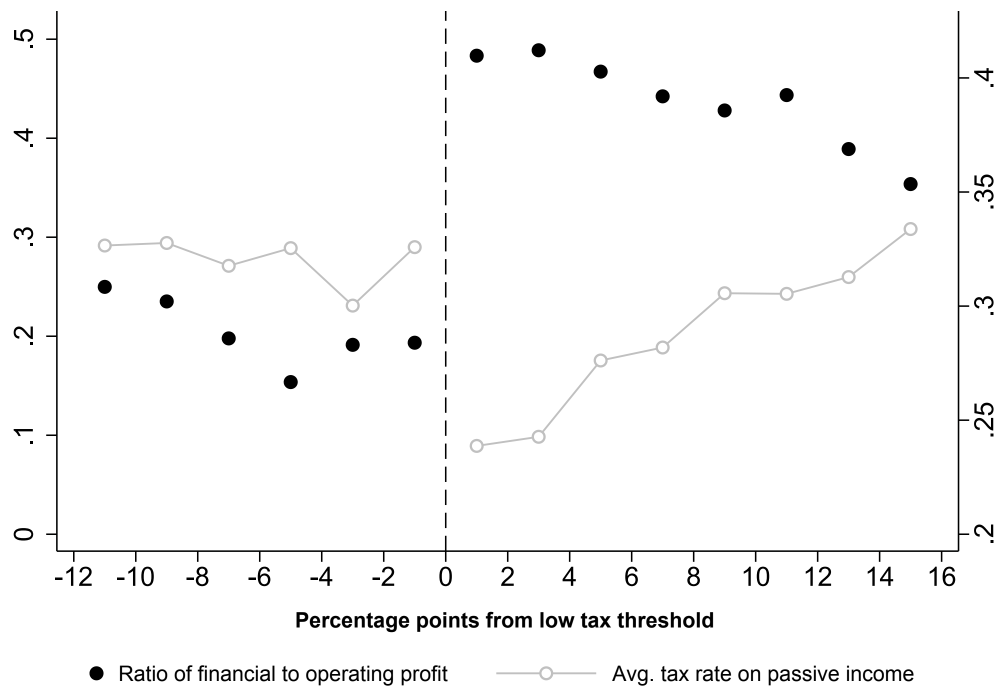
Absent these rules we have no reason to expect any discontinuity in passive profit around this ownership-specific tax level. Consequently, we can interpret the discontinuity as representing the overall response of Multinationals to CFC rules. However, the pattern in itself does not reveal the behavioural mechanisms behind this reaction. On the intensive margin multinationals might react by redirecting passive profits from targeted subsidiaries below the threshold into non-affected subsidiaries above the threshold. This would affect the relative levels of passive profits within otherwise similar subsidiaries on either side of the threshold. On the extensive margin multinationals might incorporate new subsidiaries or relocate subsidiaries to environments outside the scope of CFC rules to avoid additional taxation. This would affect the composition of subsidiaries above and below the threshold, and could have an effect on the geographical distribution of real economic activity. From a policy perspective the separation of these two responses is important to evaluate consequences for the distribution of economic activity and the size and distribution of corporate tax revenue. In the following analysis we investigate each of these behavioural margins separately using variation created by the tax threshold.

V.1. The intensive margin

First we look at the levels of passive profits within foreign subsidiaries and how CFC rules provoke shifting strategies within multinational firms. Such behaviour is what we previously referred to as the intensive margin of response. The starting point of our empirical methodology is a simple semi-log linearised model described by the following equation for corporation

Figure 4:

Financial profit around low-tax threshold



Notes: Filled circles (left axis) represent the total ratio of financial to operating profit within each 2 percentage point bin. Hollow circles (right axis) represent the average tax rate on passive income faced by the subsidiaries within each bin. Above zero (i.e. above the tax threshold) this is the average host CTR faced by subsidiaries in their respective host countries. Below zero (i.e. below the tax threshold) this is the average home CTR i.e. the corporate tax rate in the country where the parent of the subsidiary is located. We condition on positive operating- and financial profit and use only home-countries with an implemented CFC regime (with a specified tax threshold). All foreign subsidiaries are included. We leave out domestic subsidiaries to avoid random fluctuations due to home-bias. We further exclude financial corporations and banks to ensure that operating profit is a meaningful scaling variable. We are not able to extend the figure further to the left due to data coverage. The data availability at such low tax rates is very limited.

f based in host country s, with parent p situated in home country h, at time t,

$$ln(\text{profit})_{fspht} = \alpha + \beta_1 \mathbb{1}_{[CFC]sht} + \beta_2 X_{ft} + \beta_3 \Gamma_f + \beta_4 \Gamma_{t\times s} + \beta_5 \Gamma_{t\times h} + \varepsilon_{fspht}$$

The CFC targeting indicator variable $\mathbb{1}_{[CFC]sht}$ is equal to 1 if host country s has a corporate income tax rate that falls under the low-tax threshold in the CFC legislation of home country h at time t. The parameter β_1 measures (roughly) the percentage change in profit when moving into CFC targeting. We include firm specific covariates, X_{ft}, to control for firm size. Disregarding tax incentives there could be other rational reasons for generating a large amount of passive income. A large subsidiary carrying out substantial active economic business, such as commodity production and sale, will presumably have a need for more liquid assets compared to a smaller and less active subsidiary. We control for this need using firm size as a proxy. The main specifications also include fixed effects on the subsidiary level to exclude all variation *not* coming from shifters, as well as fixed effects on the *year-by-host* and *year-by-home* country level to flexibly control for time trends. Our identifying variation comes from tax reforms moving subsidiaries across the threshold. This variation mainly consists of reforms changing host country tax rates or home country thresholds, but we also allow shifts to come from the implementation of a CFC regime during the sample period.[18] Note that the specification above eliminates the need for country-level controls, due to the rich set of fixed effects. Tax policy changes affect different subgroups of firms on a single market differentially and hence we can identify effects from changes within the single subsidiary relative to other subsidiaries operating on the same market.

Using the specification presented above we look at the relationship between CFC rules and financial profit. Results are given in Table 1. In the first column we use the total stock of assets as our measure of firm size.[19] The coefficient estimate on the CFC indicator variable is negative, large and significant at the 1 percent level.[20] The size of the estimate suggests that

[18] Turkey, Iceland and China introduce CFC legislation with a low-tax threshold during our sample period. Consequently any potential subsidiary situated in a country below this threshold experience a shift in the year of introduction. We do not allow shifts to come from a reform introducing or removing a low-tax threshold from an already existing CFC regime. Such reforms typically replace a blacklist or a similar provision, and these are often designed to overlap as much as possible such that the same subsidiaries are targeted before and after. In practice we therefore drop observations where the home country is Denmark after 2006, Italy before 2010, Sweden before 2004 and South Africa before 2008.

[19] In the asset measure we subtract the amount of intangible assets, since income from intangible assets is also affected by CFC legislation.

[20] We report two-way clustered standard errors in the dimensions host-country and home-country using the formula $\hat{V}(\hat{\beta}) = \hat{V}^{s}(\hat{\beta}) + \hat{V}^{h}(\hat{\beta}) - \hat{V}^{s\cap h}(\hat{\beta})$, where each component on the right-hand side are the one-way clustered variance matrices on the host, home and interaction level (Cameron et al. 2011). The result is robust to using one-way clustering in these dimensions instead. Clustering on the host-country level produces a standard error of 0.0586, while clustering in the home-country dimension yields standard error 0.0271. Both are significant on at least the 5 percent level. The result is also robust to excluding multinational groups with parent company in NZ, BR, AU, CA and US from the sample. As discussed these countries have CFC

Table 1:

CFC rules and the allocation of financial profit within multinationals

	Outcome: ln(financial profit)				
	(1)	(2)	(3)	(4)	(5)
CFC Indicator	-0.1410^{***} (0.0342)	-	-0.1570^{***} (0.0438)	-0.1346^{***} (0.0443)	-0.1389^{***} (0.0423)
CFC × Ind^{Large}	-	-0.1847^{***} (0.0706)	-	-	-
CFC × (1 - Ind^{Large})	-	-0.1186^{***} (0.0273)	-	-	-
ln(Employees)				0.2085^{***} (0.0228)	
ln(Turnover)			0.1668^{***} (0.0100)		
ln(Other assets)	0.6975^{***} (0.0359)	0.6975^{***} (0.0359)			
Subsidiary FE	✓	✓	✓	✓	✓
Year × Host FE	✓	✓	✓	✓	✓
Year × Home FE	✓	✓	✓	✓	✓
Total obs	234 236	234 236	199 309	155 712	260 135
Subsidiaries	63 742	63 742	55 835	47 155	69 488
Total obs w. CFC=1	9 252	9 252	9 028	7 179	10 019
R^2	0.40	0.40	0.06	0.03	0.01

Notes: The unit of observation is majority-owned subsidiaries (excluding banks) within multinational groups with at least 3 entities, where the parent corporation is not located in a tax-haven country. The dependent variable is the natural logarithm of financial profit. Single-year shifts in the CFC indicator (i.e. subsidiaries moved below (above) the threshold by a reform one year and back above (below) by another reform the next year) are not acknowledged as shifts since reforms can happen at any time during a year and hence we cannot know how many months were between such reforms. In practice we keep the CFC indicator constant across such shifts (i.e. if the shift is above and back below the indicator remains at 1 throughout). Two-way clustered standard errors at the home-country and the host-country level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

a subsidiary moving into potential CFC taxation experiences a 13 percent drop in financial profit.[21] In the second column of Table 1 we show response heterogeneity by the size of the tax notch introduced by CFC legislation. For a subsidiary that is moved below the threshold

legislation, but we are not able to include them as CFC countries in the analysis due to our identification strategy exploiting the low-tax threshold. If we exclude them from the sample we get a CFC parameter estimate of $-0.1455(0.0380)$.

[21]When interpreting the size of the estimate we have to correct for the fact that we are dealing with a dummy variable in a logarithmic specification. Using the formula $exp(\hat{c} - \frac{1}{2} \cdot \hat{V}_{\hat{c}}) - 1$ proposed by Kennedy (1981) we get -0.132. If we instead use differences in predicted values we also get -0.132.

we calculate the size of the tax notch as the difference between the host country rate before the shift and the home country rate after the shift. This difference represents the realised change in the tax rate on passive profits before and after the shift across the threshold. For a subsidiary that is moved above the threshold we analogously take the difference between the home country rate before the shift and the host country rate after the shift. The variable Ind^{Large} is an indicator variable equal to 1 if the notch size at the time of the shift is larger than the median (11.5 percentage points) in the distribution of different notch-sizes. By interacting this indicator variable with the CFC indicator we test whether patterns in observed effect sizes are consistent with the size of the tax incentives created by CFC rules.[22] We observe a large and statistically significant response in both groups, however consistent with a larger tax incentive in the group facing larger notches we see a substantially larger coefficient estimate here.[23]

In columns (3)-(5) we check robustness to different measures of firm size. The stock of assets as included in the first specification is arguably a good measure of actual firm size and the scope of active firm operations. However, since we are unable to exclude financial assets, this measure might also capture some of the response we are trying to measure. To ensure that this does not bias our results we use two alternative measures - the number of employees and total turnover in columns (3) and (4). Both measures have considerably less coverage in the dataset, but both produce numerically large and statistically significant estimates of the CFC parameter. In column (5) we completely exclude the size measure and rely on the subsidiary fixed effects to capture any time persistent need for liquid capital within a company. The estimated effect size is very stable across all three specifications ranging from 13 to 15 percent. In conclusion our flexible setup, exploiting only within-subsidiary time variation, produces robust estimates that are very insensitive to controlling for firm size differences and even appears to make such controls obsolete. In all five columns we include all majority owned

[22] If a company has multiple shifts across the low-tax threshold we let the shift that happens first determine the value of the indicator Ind^{Large}.

[23] Note however that we do not have enough statistical power in the estimation to statistically differentiate the two coefficients.

subsidiaries, however results are very similar if we condition on full ownership.[24]

Since we are exploiting variation created by a threshold, we can perform a simple placebo test by randomly changing the placement of this threshold. For each group of subsidiaries with the same host and home country we randomly add or subtract a number drawn from a uniform distribution on the interval $[1; 10]$. We hence maintain the same time-profile of the threshold, but randomly move it up or down with the same amount for the entire group of companies initially facing the same conditions. Using these placebo thresholds we then use our main specification from Table 1 column (1) to obtain a placebo estimate of the effect. The Appendix Figure 10 shows the distribution of these estimates resulting from 4000 replications of the procedure. As the figure clearly shows the distribution is centred closely around zero, and our true estimates from Table 1 are all placed far in the left tail of this placebo distribution. This strengthens the notion that our estimates reflect true effects of CFC rules, and are not created by random fluctuation or uncaptured time trends.

Using the mean notch-size (8.81) and the mean tax rate on financial profit before a reform (26.84)[25] our main estimate from column (1) implies an elasticity with respect to the net-of-tax rate close to 1. Compared to previous studies estimating the elasticity of corporate taxable income, this elasticity estimate is rather large. Dwenger & Steiner (2012) use a repeated cross-sectional dataset on tax returns for German corporations and find an overall elasticity estimate of 0.58. Importantly they also perform a heterogeneity analysis suggesting that elasticities might be considerably larger for companies with greater profit shifting opportunities. Devereux et al. (2014) use UK tax records and find estimates ranging from 0.13 to 0.56. However they note specifically that they estimate responses for a group of companies

[24]The setup also allows us to create separate CFC indicator variables for each CFC home country in the sample, and estimate effects separately. As explained in the data-section the amount of data and hence variation coming from each regime varies substantially. The five countries providing most variation are Germany, United Kingdom, Spain, France and Japan. If we estimate separate effects for these five countries we get parameter estimates ranging from -0.1032 to -0.1600 using the main specification from Table 1 column (1). However, this split introduces statistical power issues when only using shifters across the threshold for identification, and hence these estimates generally have large standard errors and should be interpreted with caution.

[25]This is the mean in a distribution using the home country rate for reforms moving subsidiaries above the threshold and the host country rate for reforms moving subsidiaries below the threshold.

with limited international ties.[26] In this light our relatively large estimate is not surprising as we are looking exclusively at firms with international activities. An international reach gives opportunities for manipulation of the tax base that is not readily available to smaller domestic firms, and hence we would expect these multinational firms to display more sensitivity to tax rates. This is indeed what we find.[27]

V.1.1. Comments on potential sources of bias and the Cadbury-Schweppes case

Often when relying on reforms for identification we worry about endogeneity of timing. Perhaps some driving force provokes tax reform and also affects financial profits within firms directly. Circumstances such as political distress or an economic crisis affecting government budgets could provoke tax reform, but also directly affect investments and firm profits. However, in our setup treatment is determined by an interaction between a tax rate and a low-tax threshold. This means that a tax reform in some host country will cause the same change in the statutory tax rate for all subsidiaries on the market, but the reform will only affect the CFC status of some subset due to differences in the low-tax threshold. Similarly a reform moving the threshold in some parent country will only change the CFC status of some foreign subsidiaries due to differences in foreign host rates. The existence of subsidiaries affected by the same reforms but for whom CFC treatment stays unchanged is exactly the factor that al-

[26] Devereux et al. (2014) write in their conclusion: *"We speculate, though present no evidence in support and leave for future research, that very large companies may also have relatively high elasticity, as they may have more opportunities to avoid tax, or to shift activities between countries."*

[27] Note that the logarithmic specification in this analysis excludes non-positive recordings of the outcome variable financial profit. Since our sample period encompasses a financial crisis this restriction affects a substantial number of observations. Note however that the nature of our specification is such that we are exactly interested in the response of corporations with positive passive profits. We measure the effect of CFC tax legislation as the change in profit when moved across the tax threshold by a reform. Corporations with no or negative passive profit have a zero tax base and hence little or no incentive to react to a tax change. Loss-carryforward might create some incentives for subsidiaries expecting future positive profits, but such carryforward is not always allowed by CFC rules. Consequently, while this restriction might create some sample selection on profitable subsidiaries, we are exactly measuring responses on the relevant treated population.

lows us to include host specific time trends and home specific time trends and thereby control for any common factors that potentially drive tax reforms. Consequently, we do not rely on exogenous timing of tax reforms, but rather on a much weaker condition of random assignment of subsidiaries that move across a threshold by these reforms. A host country with a desire to strategically set their tax rate to avoid activating foreign CFC legislation would have to respond to several sets of rules characterized by very different definitions of what it means to be a low tax environment. Moreover threshold changes most often happen as a result of general tax level reforms since thresholds are often fixed as a proportion of own tax level. These indirect movements of low-tax thresholds via general tax reforms and the fact that tax thresholds are placed at very different levels makes problems with non-random assignment very unlikely in this setup.[28]

On a related note we should also address any potential bias created by strategic behaviour on the side of the multinational firm. As noted above, we might also expect multinationals to react on the extensive margin by incorporating new subsidiaries or relocating subsidiaries around the threshold. This could affect the composition of subsidiaries around the cut-off and make a comparison between financials of subsidiaries above and below the threshold less meaningful. Note that by relying only on within-subsidiary time variation our intensive margin estimate is not influenced by such a comparison. However, if the potential sorting around the threshold determines which companies are most likely to move across the threshold our result could be biased by a non-random sample of shifters. First note that such a bias would only arise if the selected group has a non-representative responsiveness to taxes. This means that selection on other characteristics or levels of certain variables, which are not related to tax-responsiveness, would not cause a bias. Further, reforms move subsidiaries both below and above the threshold and hence we are identifying both from companies that were initially placed below and initially placed above the threshold. Finally, some tax reforms introduce quite substantial changes in tax rates and in some instances we observe a host tax reform at the same time as a change in a low-tax threshold. Such events cause subsidiaries relatively

[28] A slight worry in this regard could be related to the implementation of a new CFC regime. If home countries design new regimes such that the threshold is just below the tax level of their main trading partner for instance, this could cause non-random selection into treatment. However, results are robust to dropping these observations and only relying on host country tax reforms and home country threshold movements.

far from the threshold initially to move across. The distance to the threshold prior to a shift is distributed within the interval $[-7; 8.5]$. If we drop shifters who are less than 5 percentage points from the threshold before they move across and focus only on those who move from a position further away, we get a coefficient estimate of -0.1350 using the main specification from column 1. With a standard error of 0.1832 this estimate is not statistically significant, due to a small number of firms subject to these specific events. However, the size of this estimate suggests no tendency towards a different responsiveness among companies further from the threshold.

Before moving on to an investigation of the mechanisms behind this response we should mention a ruling by the European Court of Justice with a special relevance for CFC rules within the European Union. In 2006 the European Court of Justice dealt with a case, known as the Cadbury-Schweppes case, concerning the compatibility between the UK CFC rules and EU law. The ECJ ruled that the UK rules were in violation of the EU freedom of establishment because a foreign subsidiary in another EU country could incur a tax that would never fall on a domestic subsidiary. Such taxation within the EU was deemed unjustifiable unless in the case of a fully artificial arrangement created for tax savings purposes. While this decision did not render CFC rules incompatible with EU law, it has been argued that it limited the scope of these rules within the EU by narrowing the focus to fully artificial arrangements (Ruf & Weichenrieder 2013). Using our main specification, we test whether we observe a muted response after the ruling within the EU. We test this by interacting the CFC indicator with a post 2006 indicator and an EU indicator. The coefficient estimate on this double interaction term is 0.019 suggesting a slightly smaller response within the EU after the ruling.[29] In other words a slightly smaller change in financial profit is measured if the shift across the threshold comes from an EU subsidiary with an EU parent after the ruling, compared to shifts before the ruling or between non-EU relationships. Note however that this estimate has a very large

[29] To produce this estimate we interact the CFC indicator with both an indicator for the year of the shift being after 2006 and an indicator for parent and subsidiary both being EU members at the time of the shift. Since the decision in principle apply for all EEA members we include Norway, Liechtenstein and Iceland. We drop subsidiaries with multiple shifts, since these are hard to classify as either affected or unaffected by the ruling, but results are similar if we include them. The parameter estimate on this double interaction is equal to 0.019 (0.123), while the coefficient estimate on the CFC indicator in the same regression is -0.148 (0.065).

standard error (0.123) and hence does not provide any conclusive evidence on the consequences of this ruling.

V.1.2. Spillover effects and revenue consequences

Entering potential CFC taxation leads to a sizeable drop in financial profit within a foreign subsidiary of a multinational firm. This conclusion raises the question of whether the observed drop arises from a genuine downsizing or from a redirection of profits. In other words, does the missing profit reappear within the multinational group and if so where? Mapping the underlying behavioural patterns within the multinational firm is essential for the ability to evaluate consequences for the magnitude and distribution of global corporate tax revenues – a question of high importance to policy makers.

To investigate group dynamics and spillover effects we introduce the concept of group-exposure to CFC taxation into a framework very similar to the one presented earlier. Group-exposure measures the degree to which a multinational group is affected by CFC legislation. One way to construct such a measure is to consider the relative number of subsidiaries within a group placed in environments below the relevant tax threshold at a given point in time. Based on this idea we use the fraction of subsidiaries placed below the threshold as a proxy for group-exposure. By using the relative portion of the group placed within the targeted tax area we account for the large differences in group-size. This measure differentiates the importance of a single shift across the threshold and allows such a shift to create a larger change in exposure for a smaller group than for a larger group. This is potentially important as we would expect a single shift to have larger spillover effects on another group member if the group is small, compared to a situation where there are many other group members.

Using variation in the exposure measure we can evaluate spillover effects within a multinational group when a subsidiary is targeted by CFC taxation. For identification we again use the time-variation coming from reform-induced movements across the threshold. However, this variation is not sufficient in this context because such single shifts create very little variation in a group-level variable when groups are relatively large. To facilitate the identification of spillover effects we therefore add one very specific dimension of cross-sectional variation by allowing the comparison between two subsidiaries within the same host country, owned through the same home-country. Such comparisons allow us to exploit the variation coming from the different placements of subsidiaries within two otherwise similar groups, while at the same time only comparing subsidiaries subject to the same host country shocks, market conditions and home-country influences. Hence, instead of including subsidiary fixed effects, we include *home-by-host* country fixed effects to allow for this additional source of variation. In line with the analysis above we flexibly control for time trends by including both host specific time trends and home specific time trends. Note that this type of subsidiary comparison introduces additional variation to identify the effect of the group-exposure variable because two similar subsidiaries can be members of groups with different geographical placements of other within-group subsidiaries. However, it does not add additional variation to identify the CFC indicator, as variation in CFC treatment is exactly at the home-by-host level.

Table 2 presents estimation results. In the first column we show the analogue of the previously estimated effect of CFC targeting. Since the identifying variation in this specification is the same reform-induced time variation as used previously we should expect a similar effect unless firm-heterogeneity is not sufficiently controlled for without the full set of subsidiary fixed effects. The estimate is slightly larger, but as expected the effect is very comparable both in magnitude and significance level to the estimates obtained earlier.

In the second column we introduce the group-exposure measure. For each subsidiary observation, the exposure variable measures the fraction of group members within the same multinational group that falls under the CFC low-tax threshold at that point in time. From column (2) we observe a marginally significant positive effect of this variable. The result implies that other entities within the same multinational group generate more financial profit when a group member falls below the threshold and the share of CFC targeted subsidiaries increases. A control for the total number of subsidiaries in each group is included to make sure the effect is not a scale effect related to group size, but indeed a within-group compositional effect. The magnitude of the estimate suggests that an increase of 10 percentage points in the fraction of targeted subsidiaries within a group raises the financial profit of other group members by about 3.1 percent.

In column (3) we interact the exposure variable with our CFC targeting indicator. The purpose of this interaction is to differentiate the spillover effect based on relative placement to

Table 2:

CFC rules and the allocation of financial profit within multinationals

	(1)	(2)	(3)	(4)	(5)
CFC Indicator	-0.1768^{***} (0.0555)	-0.2180^{***} (0.0583)	-0.1722^{***} (0.0563)	-0.1721^{***} (0.0561)	-0.1759^{***} (0.0568)
Group Exposure		0.3069^{*} (0.1623)			
Exp. × CFC			-0.0023 (0.2053)	-0.0020 (0.2048)	-0.0020 (0.2047)
Exp. × (1 - CFC)			0.4712^{**} (0.1997)		
Exp. × (1 - CFC) × Dom				0.4374^{***} (0.1605)	0.4372^{***} (0.1606)
Exp. × (1 - CFC) × (1 - Dom)				0.5233^{*} (0.3119)	
Exp. × (1 - CFC) × (1 - Dom) × CTR^{low}					0.5896^{**} (0.2958)
Exp. × (1 - CFC) × (1 - Dom) × (1 - CTR^{low})					0.3988 (0.3537)
Group size	0.0001 (0.0001)	0.0000 (0.0001)	0.0000 (0.0001)	0.0000 (0.0001)	0.0000 (0.0001)
ln(Other assets)	0.9429^{***} (0.0261)	0.9424^{***} (0.0262)	0.9421^{***} (0.0263)	0.9421^{***} (0.0262)	0.9421^{***} (0.0262)
Host × Home FE	✓	✓	✓	✓	✓
Year × Home FE	✓	✓	✓	✓	✓
Year × Host FE	✓	✓	✓	✓	✓
Obs (total)	234 236	234 236	234 236	234 236	234 236
Obs (w. CFC=1)	9 252	9 252	9 252	9 252	9 252
R^2	0.50	0.50	0.50	0.50	0.50

Notes: The unit of observation is majority owned subsidiaries (excluding banks) within multinational groups with at least 3 entities, where the parent corporation is not located in a tax-haven country. The dependent variable is the natural logarithm of financial profit. The group exposure measure is the fraction of subsidiaries within a multinational group situated below the relevant tax threshold. Single-year shifts in the CFC indicator (i.e. subsidiaries moved below (above) the threshold by a reform one year and back above (below) by another reform the next year) are not acknowledged as shifts since reforms can happen at any time during a year and hence we cannot know how many months were between such reforms. In practice we keep the CFC indicator constant across such shifts (i.e. if the shift is above and back below the indicator remains at 1 throughout). In columns (3)-(5) the CFC exposure variable is centered around the sample mean (0.12) calculated from groups with at least one CFC targeted subsidiary. Two-way clustered standard errors at the home-country and the host-country level in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

the low tax cut-off and hence CFC treatment status. The interaction investigates whether the spillover and hence the gain in financial profit accrues to all other group members or only those *not* themselves targeted by CFC legislation. Results presented in the third column indeed indicate the latter to be the case. Considering only members above the threshold, measured effects are significant at the 5 percent level, positive and larger than the overall effect presented in column (2). This is however not the case for members who are CFC targeted themselves, where effects are very close to zero and insignificant at all traditional significance levels.

In column (4) we additionally include an interaction with an indicator for being a domestic subsidiary. The objective of this specification is to explore whether the positive effect for non-targeted subsidiaries also applies to domestic subsidiaries placed in the home country of the group parent. Note that these companies by construction of the rules always appear above the low-tax threshold. The result of this interaction suggests that domestic subsidiaries, as well as other subsidiaries above the threshold, experience an increase in financial profit when group-exposure increases. For domestic subsidiaries the effect is significant at the 1 percent level and indicates a 4.4 percent increase when group exposure increases by 10 percentage points. For other foreign subsidiaries the effect is only marginally significant but indicates a 5.2 percent increase in financial profit if group exposure increases by 10 percentage points. If we consider the case of a multinational group with 10 members, then results suggest that a reform moving one of the subsidiaries below the threshold would cause a 4.4 percent increase in financial profit within each domestic group member and a 5.2 percent increase within each foreign member above the threshold.

In the final column we introduce yet another interaction to differentiate the effect on foreign subsidiaries above the threshold. The variable CTR^{low} is an indicator for the host tax rate being in the range between the low-tax threshold and the home country rate. By interacting with this variable we look separately at foreign subsidiaries just above the threshold and foreign subsidiaries in countries with a higher tax environment than the home country. In line with expectation and previous evidence we observe a large positive spillover-effect on foreign subsidiaries just above the threshold. The effect on these subsidiaries is significant at the five percent level and materially larger than the effect on other foreign subsidiaries. The effect on subsidiaries above the home-country rate is still positive, but smaller and insignificant. In other words we cannot reject that there might be some positive effect on these high-tax corporations, but the main effect seems to be driven by the lowest-tax subsidiaries outside targeting.[30]

[30] As noted all standard errors reported in Table 2 are two-way clustered standard errors in the dimensions home-country and host-country. Clustering only in one of these dimensions produces similar standard errors. Looking at column (5) the three main estimates all remain significant on at least the 5 percent level if we

The overall patterns described in this section are consistent with shifting behaviour within multinationals in the direction from targeted to non-targeted subsidiaries. The results seem to document a behavioural pattern where multinationals extract financial profit from subsidiaries falling under the threshold, and place this additional profit within members placed above. The consequence of such behaviour is a movement of profit up along the corporate income tax range into higher tax environments, whereby more global corporate tax revenue emerges. The patterns also suggest that part of this additional tax revenue accrues to the home country administering the CFC rules, since domestic subsidiaries attract more financial profit when other foreign group-members are targeted.

From a policy perspective the exact division between countries of the global tax revenue gain appears crucial. To quantify the part accruing to the home country, we do a simple back-of-the-envelope calculation using median group characteristics. In median terms groups with positive exposure have 9 percent of their subsidiaries placed under the low-tax threshold. The remaining 91 percent outside CFC targeting consist of 42 percent domestic subsidiaries and 58 percent foreign subsidiaries. The median amount of positive financial profit generated yearly within a domestic subsidiary in a CFC country is roughly 60 percent larger than the median amount generated within a foreign subsidiary above the cut-off. If we multiply the median amount of positive financial profit within domestic subsidiaries with the estimated spillover effect and the median number of domestic subsidiaries, we get a number reflecting total profit accruing to the home country from spillover effects within a representative group. This number is relative to some specified increase in exposure, however by comparing it to the analogous number for other foreign subsidiaries above the cut-off we get an idea of the relative divide of the tax revenue gain. The comparison tells us the fraction of shifted profit, following a subsidiary moving below the threshold, going to the home country. The simple calculation reveals a fifty/fifty allocation.[31] In other words about half of the tax-base increase

cluster in either the home-country dimension or in the host-country dimension.

[31] The calculation goes as follows. The median yearly amount of positive financial profit within a domestic subsidiary in a CFC country is USD 154.000, and hence $154 \times 0.04374 \times 0.42 = 2.83$. The median yearly amount of positive financial profit within a foreign subsidiary above the cut-off is USD 95.000, and therefore $95 \times 0.05233 \times 0.58 = 2.88$. Consequently the relative part going to the home country must be $2.83/(2.83 + 2.88) = 0.50$.

and resulting tax revenue gain from these rules seem to accrue to the rule-enforcing country, while the other half benefits other higher-tax countries.

V.2. The composition of income

To supplement these results we now turn to an analysis of the relationship between passive and active profits within each subsidiary before turning to the pure extensive margin analysis. Since CFC legislation only targets passive income, it is still possible for subsidiaries to benefit from a low host tax rate on the active part of profits.[32] This distinction creates a discrepancy between the impact of CFC legislation for a subsidiary with a high ratio of passive to active profit versus a lower ratio company. Under CFC rules a high-ratio company will potentially be liable to additional home country taxation on a large fraction of income, and correspondingly the benefit of enjoying a low tax rate on active income is small. In other words, the effect of CFC legislation on the average tax rate at the subsidiary level is large, because a larger portion of the profit base is affected. The change in the average tax rate will all else equal be less severe for a lower ratio company. This means that, measured by the change in average tax rate, the incentives created by CFC legislation intensify with the ratio of passive to active income. As previously discussed we can imagine several reactions to this increase in average taxes including the redirection of passive income to unaffected subsidiaries or relocation of the subsidiary to an unaffected location or a change in the role of the subsidiary to make it more reliant on active income streams. Devereux & Griffith (1998) argue that average taxes are specifically important in the location decision of multinationals. Each of these reactions would cause the number of high-ratio subsidiaries to drop within CFC affected low-tax environments. Moreover, since the change in average tax rate caused by CFC rules is increasing in the passive-active ratio we would expect the relative drop in the number of very high-ratio subsidiaries to be larger than the drop in the number of subsidiaries with more moderate ratios. This would for instance arise if the probability of relocating a subsidiary increases with the average tax rate.

For each subsidiary and year we calculate the ratio of financial to operating profit, conditioning

[32] This is the case for almost all CFC regimes, however examples do exist of legislation that targets both dimensions - for instance the CFC rules in Brazil.

only on operating profit being non-negative. Operating profit reflects actual sales and other entries traditionally viewed as active economic business. Consequently, this ratio proxies the overall ratio of passive to active profit within the subsidiary. We set up a simple linear probability model where the left hand side is a binary indicator for this ratio being above a threshold value. The right hand side variables include the CFC treatment indicator as well as $home \times year$ and $host \times year$ fixed effects. This specification encompasses reactions both on the intensive- and extensive margin by comparing the propensity to have a high passive-active ratio among targeted and non-targeted subsidiaries controlling for all time-invariant and time-variant differences between host and home countries.[33] Essentially we compare the probability among targeted and non-targeted subsidiaries within the same host country of having a high ratio of passive to active income. To make sure we are capturing differences related to CFC legislation we exclusively compare within host countries situated close to the low-tax threshold. Specifically we focus within 5 percentage points from each relevant cut-off.[34]

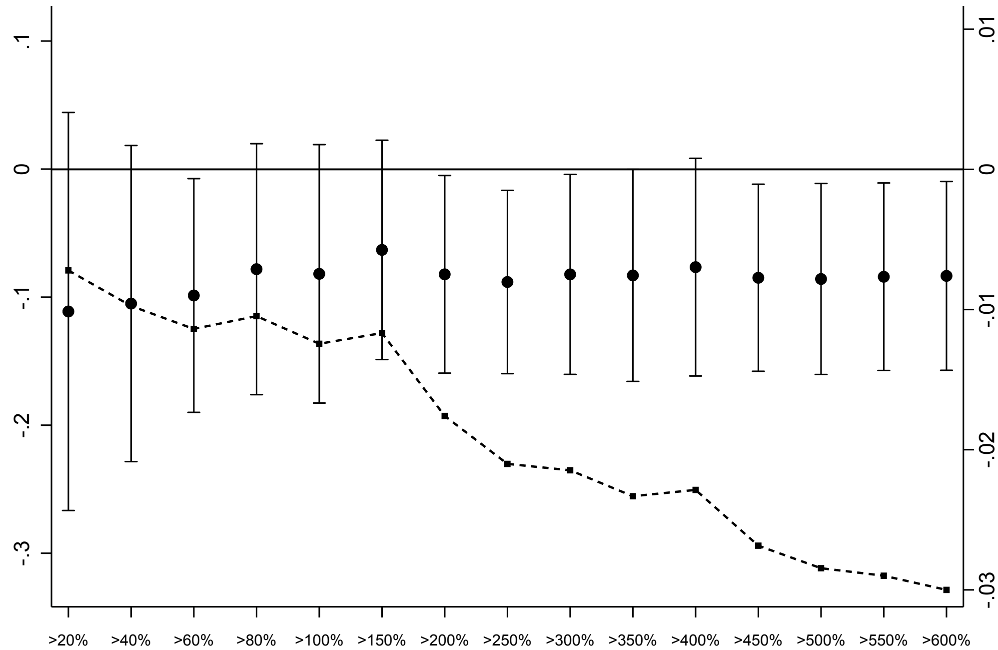
Figure 5 plots in circles the coefficient estimate of the CFC targeting variable in the described model. Each point corresponds to a different threshold value for the left-hand-side indicator. All threshold choices result in a negative estimate and most estimates, especially within the larger fractions, are statistically significant at the 5 percent level. This consistent result indicates that CFC targeting reduces the probability of having a high ratio of passive to

[33] Note that we do not include subsidiary fixed effects in this specification. Firstly, focusing on within-subsidiary variation to a large extent excludes responses on the extensive margin. If the multinational wishes to relocate a subsidiary and does this by opening a new subsidiary in a different country and transferring operations, then we cannot observe this link in the data. Second, in a specification relying solely on time variation we are likely to incur problems with mean reversion which could spuriously create a pattern of larger reactions among larger ratio companies. Companies with very high ratios at the beginning of the sample period are more likely to experience a drop in this ratio simply due to mean reversion. Such mean reversion is likely to be more pronounced when we focus solely on very high ratio companies, and hence mean reversion could create the pattern we expect to emerge as a result of CFC rules. By using cross-sectional variation within host countries we avoid this issue.

[34] We include all observations from non-CFC countries as controls. Varying this bandwidth affects the number of observations in the estimation and hence affects significance levels. However, the qualitative results are similar with bandwidths closer or further from the threshold.

Figure 5:

Linear probability model for financial profit ratio



Notes: The LHS is an indicator variable equal to 1 if the ratio of financial to operating profit is above the specified threshold indicated on the x-axis. Circles (right axis) represent estimates of the coefficient on the CFC targeting indicator and vertical capped lines represent 95 % confidence intervals using standard errors clustered in the two dimensions home and host country. All estimations include *home* × *year* fixed effects and *host* × *year* fixed effects, as well as a control for group size. We condition on non-negative operating profit for the ratios to be meaningful, but allow negative or zero financial profit. We only include observations within +/- 5 pp of the relevant cut-off. We include all observations from Non-CFC countries as controls. The dashed connected line (left axis) shows each estimate scaled by the fraction of positives of the LHS variable i.e. the CFC indicator estimate as a fraction of the baseline probability. We exclude observations before/after the introduction of a low-tax threshold, but keep observations before/after the introduction of a CFC regime. We exclude observations from tax haven multinationals or from multinationals from CFC countries with no low-tax threshold. Apart from these restrictions we include all majority owned subsidiaries within groups consisting of at least 3 members.

active profit. Moreover to create a transparent picture of the magnitude of these effects, we also express each estimate as a fraction of the baseline probability. We scale each estimate with the sample probability of having such a large ratio of passive to active income. These scaled estimates are given by the squares and connected dashed line. Concretely, targeting reduces the probability of having more financial than operating profit by 14 percent and the probability of having three times as much financial as operating profit by 24 percent. In line with the increasing change in average tax rate, we observe a monotonically decreasing pattern of the relative estimates. This suggests that the absolute effect of CFC legislation is increasing for increasing ratios as we would expect.[35]

V.3. The extensive margin - location of new subsidiaries

Until now we have mainly focused on the allocation of income within a multinational firm conditioning on the observed corporate structure. However, the underlying firm structure might also be responsive to tax rules in the sense that multinationals might adjust the location of new subsidiaries or move existing subsidiaries to new locations as a result of CFC legislation. We proceed by investigating this extensive margin of response. Using the recorded date of incorporation we can identify subsidiaries created during the sample period from 2003 to 2013. Only considering foreign subsidiaries with a parent based in a non-haven country, the dataset contains a total of 69.302 new incorporations in the sample period.[36] The set of new subsidiaries has an international reach and spreads across 99 different host countries. Multinationals from CFC countries with a low-tax threshold account for roughly half of these new incorporations.

Analysing the discrete location choice is challenging due to the existence of unobservable market conditions rendering some locations attractive for non-tax related reasons. To overcome this issue we use multinational groups that are not affected by a CFC regime to account for these unrelated factors. Specifically we look at the incorporation patterns of CFC affected groups relative to other unaffected multinational groups.[37] Hereby we implicitly assume that multinationals from countries with no CFC legislation respond to the same underlying market conditions in each potential host country as affected groups. In other words, a German and

[35]Note that the analysis above does not rely on a logarithmic transformation of the outcome variable, and hence includes both zero- and negative recordings of financial profit.

[36]This number refers to all majority owned subsidiaries. We exclude domestic subsidiaries as we are interested in the characteristics determining foreign location choice conditional on the choice to open a new foreign subsidiary. To identify tax haven countries we use the list of countries provided in (Hines, 2010) Table 1.

[37]In the group of unaffected multinationals we do not include groups where the parent is located in a tax haven country. We include all other groups owned by parents in countries with no CFC legislation. For robustness we show that results are robust to including groups from the five countries with CFC legislation that we are not able to include in the analyses (US, BR, CA, AU, NZ).

an Austrian multinational firm respond to the same non-tax related market conditions on the Swiss market when considering an investment in Switzerland. This could for instance be a well-functioning financial market, an effective banking sector or simply an increase in demand for some product or service.

First we compute for each home country the probability of opening a new foreign subsidiary in each host-year combination. This probability is calculated as the number of incorporations within that host-year pair divided by the total number of incorporations during the year. The underlying question answered by these probabilities is roughly: given that a German multinational wants to set up a new subsidiary in 2010, what is the probability that this new subsidiary will be Swiss. This probability is calculated as the fraction of new German incorporations in 2010 that were placed in Switzerland.

We calculate these yearly incorporation probabilities separately for each home country with CFC rules containing a low-tax threshold. The Appendix Figure 12 shows the distribution of new incorporations across home countries. Panel (a) shows only home countries with a CFC low-tax threshold. It is evident that for some home countries we observe a large number of new incorporations while for others we observe few. This lack of observations severely complicates the calculation of informative overall distributions for some home countries. For this reason we require a total of at least 200 observations at the overall home country level to be able to calculate distributions. This is a very non-restrictive requirement in the sense that it only excludes four home countries which have such a limited observation-count that such calculations would be uninformative. Consequently we calculate yearly distributions for the remaining 16 home countries. We perform the same yearly calculation for the collected group of multinationals from Non-CFC countries. By subtracting the probabilities calculated for the latter group from the corresponding probabilities for each CFC country, we generate a set of *incorporation probability differences*. These differences reflect how the incorporation pattern of multinationals from each CFC country differs from the pattern of new incorporations made by all international groups that are unaffected by CFC legislation. If a CFC home country has an underlying incorporation pattern identical to the pattern of unaffected countries then all these differences will be random noise. Conversely, a significant positive value indicates that the respective CFC country has an over-probability of incorporation while a negative value signifies an under-probability.

In Figure 6 panel (a) we sort these differences in incorporation probabilities relative to the low-tax threshold and compute the average within 2 percentage point bins. The figure shows a striking pattern of negative probability mass below the cut-off and positive mass above. This means that we observe a tendency for CFC groups to place less new subsidiaries in targeted environments below the threshold and more new subsidiaries in environments above the threshold relative to unaffected groups.

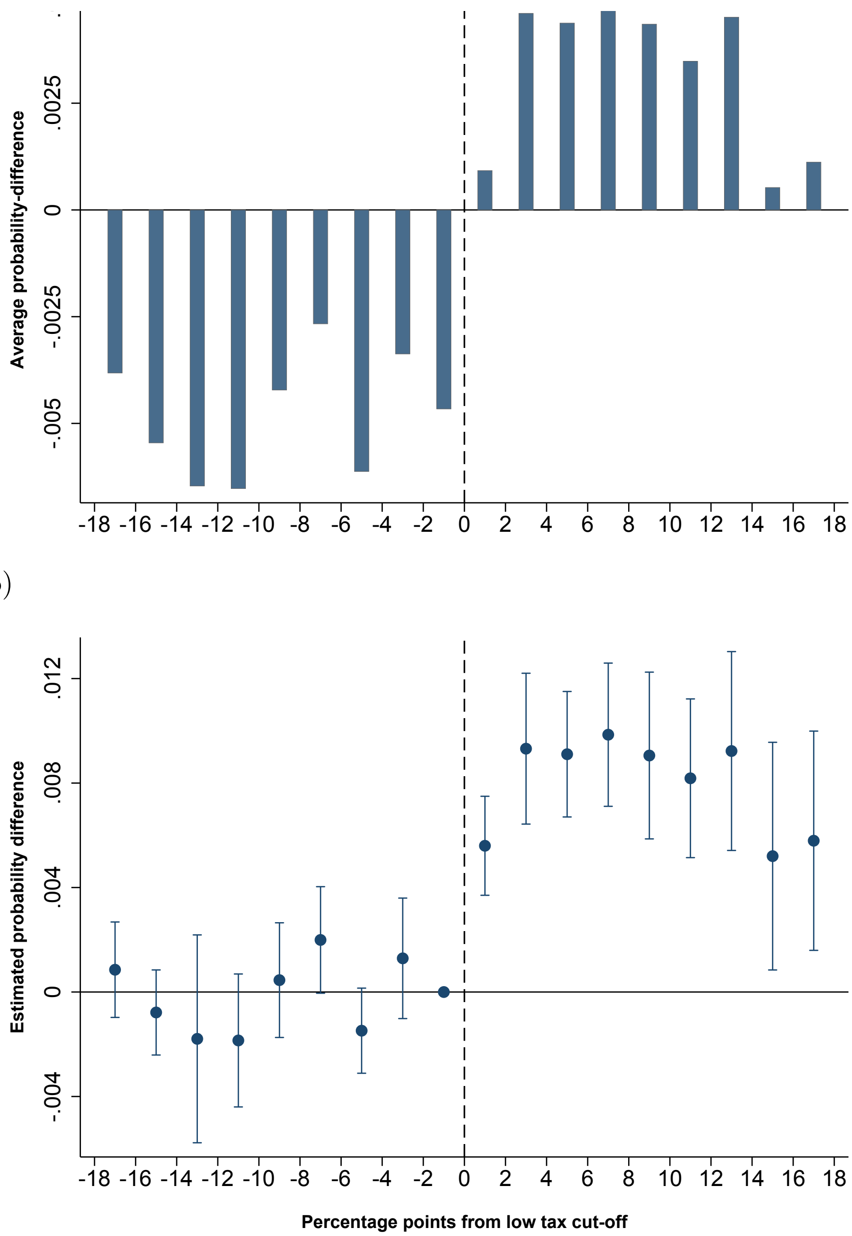
Remember that the threshold-value varies substantially across CFC countries and across years creating a diverse combination of host countries within most bins. This also means that the same host country will appear in several different bins representing different placements relative to the thresholds. The result from this figure is consequently more subtle then simply showing that CFC groups invest in higher tax environments. The specific point in the range of corporate tax rates at which CFC groups go from incorporating less to incorporating more than non-CFC groups aligns with the exact tax threshold specified in the CFC legislation relevant for them. This interesting descriptive pattern would seem to indicate some effect of CFC rules on incorporation choices. A minor point relates to the first bin immediately above the threshold. While the difference in incorporation probability is on average positive in this bin it is not as large as the difference in the subsequent bins. Note however that in the sample period we observe a large number of tax reforms and for the majority of these reforms the host corporate tax rate is lowered. Assuming it is costly to set up a new foreign subsidiary and firms are aware of the risk of tax reform, firms might not want to incorporate too close to the threshold. At these very close locations the risk of moving below the threshold due to a later tax reform is large and this might compensate for the slight tax difference between the first and subsequent bins. Importantly we would not expect the same risk-consideration regarding actions that are less costly to change after a reform such as the location of profits. In the construction of this figure we as noted use all CFC home countries with more than 200 observed incorporations to ensure meaningful distributions. As earlier we again exclude years before the introduction of a low-tax threshold and years after the removal of one, but keep years before the introduction of a full CFC regime. The pattern remains if we use the sum of differences within each bin instead of the average difference (see the Appendix Figure 13).

Figure 6:

Incorporation pattern around low-tax threshold

(a)

(b)



Notes: Only foreign majority owned new incorporations are included. Incorporations from earlier years made by groups in home countries that later implement a low tax threshold are dropped (analogously for home countries that remove a low-tax threshold). We only consider CFC home countries with at least 200 observed incorporations and only groups whose parent is not based in a tax haven. In panel (a) bars represent the average probability-difference within 2pp bins relative to the cut-off. In panel (b) bars represent parameter estimates from a regression of the probability differences on bin dummies describing the placement relative to the tax cut-off. We use the [-2,0) bin as the base bin. Capped lines illustrate 95 percent confidence intervals calculated using a non-parametric bootstrap approach with 500 bootstrap replications. We re-sample within the original dataset of new incorporations clustering at the home country by main industry level. The main industry classification is taken from the NACE rev. 2 overall classification.

To obtain a direct estimate of the effect of CFC targeting on the incorporation decision we exploit the discontinuity created by the low-tax threshold. Consistent with the nature of the low-tax threshold as a cut-off point determining CFC targeting, we observe a sharp upward jump just at the threshold value. Assuming that host countries just above and just below are otherwise similar, and specifically that there are no other systematic differences between these countries affecting CFC groups and non-CFC groups differentially, this jump represents a direct estimate of the effect of CFC targeting on the incorporation probability. In Figure 6 panel (b) we plot the mean within each bin relative to a base bin below the threshold. These points are hence the coefficients from a simple regression of the probability differences on dummies for placement relative to the cut-off. Confidence bans are calculated using a non-parametric bootstrap approach with re-sampling within the original set of new incorporations to account for the uncertainty underlying the initial calculations of probability-differences.[38]
From this figure we again clearly observe a jump just at the threshold value. The size of the jump is equal to the estimated mean within the first bin above the threshold. As shown in the Appendix Table 7 column (1) this direct discontinuity estimate is equal to 0.0056.[39] This suggests that CFC targeting causes the probability of incorporation in a host country to drop by 0.56 percentage points.

Comparing mean values of the bins closest to the low-tax threshold is essentially a very simple and intuitive non-parametric implementation of a regression discontinuity design. We continue with a more elaborate regression discontinuity approach investigating robustness to different specifications of the setup and validity of the underlying assumptions.[40]

[38]We cluster re-sampling at the main industry level within home countries to account for any dependence between choices over time and across similar multinationals. We use the NACE Rev. 2 to classify the main industry of the parent corporation. This is a very aggregate-level industry classification - i.e. examples of a main industry is for example *Manufacturing* or *Information and Communication*. For parent companies where NACE is unknown we cluster on the group level to account for correlation over time. However, if we instead pool all these multinationals together in one cluster within home countries and treat all missing observations as one industry we get similar results.

[39]In the Appendix Table 7 column (2) we show analogous results allowing CFC countries unaffected by a low tax threshold to enter the control group. This produces a very similar pattern and a similar discontinuity estimate of 0.0045.

[40]For a comprehensive and intuitive description of RD designs, with a focus on their application within

As noted the main assumption underlying our estimate is similarity between hosts above and below the threshold. Within the RD framework this corresponds to the requirement that host countries are not able to perfectly manipulate their placement relative to the low-tax threshold. Such full manipulation could create a situation where host countries with most to gain from avoiding CFC targeting place themselves just above the threshold and hence we would get an upward biased estimate of the effect of CFC targeting. There are several reasons why this is not likely to be the case despite host countries having full control over their corporate tax rate. Firstly host countries typically set only one tax rate. Low-tax thresholds vary substantially by regime and hence a host can never obtain the same placement for all foreign CFC regimes. Second, most low-tax thresholds vary substantially over time, and hence to obtain the same placement within a single regime over time a host country will have to implement tax reforms following the reforms of the specific home country. Considering the amount of other factors that determine tax rates this is unlikely to be practically feasible.

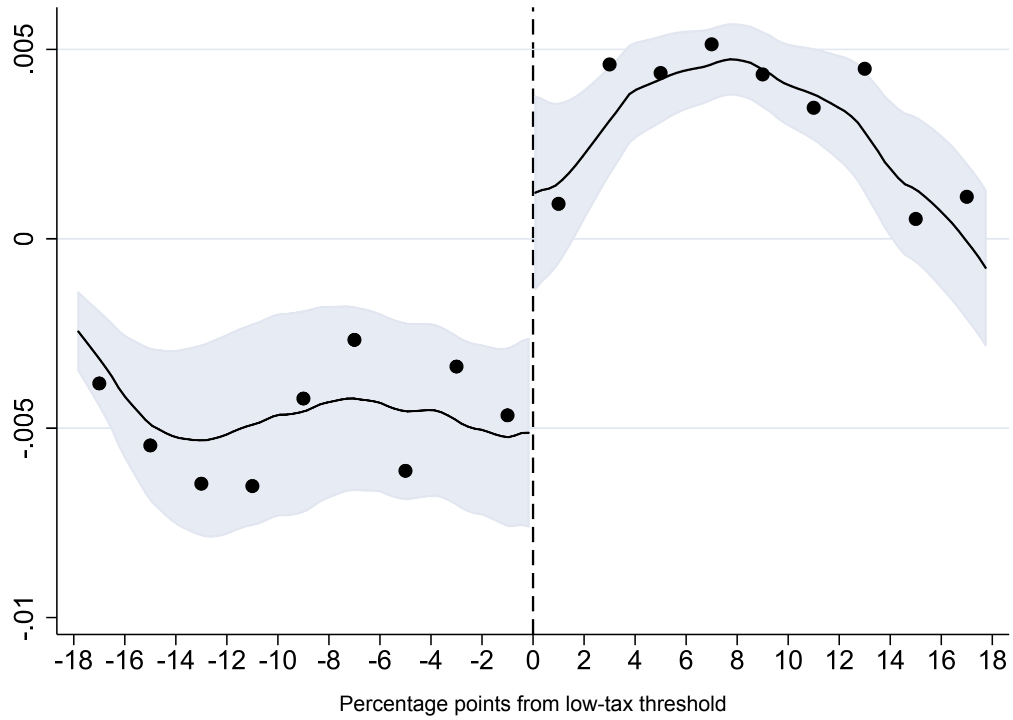
We can more directly test this assumption by looking at the distribution of host countries across the running variable. In the Appendix Figure 14 we consider all the CFC home countries used in the main analysis and calculate for each observed host country in the sample the relative placement to the low-tax threshold in each year. Panel (a) shows the combined histogram for all home countries. The peaks in the figure are created by popular round numbers which tend to be popular choices of corporate tax rate in many countries. Panel (b) shows the same histogram, but removing host countries with a corporate tax rate in the set $\tau \in \{20, 25, 30, 35\}$. Removing these round-number tax rates eliminates the major peaks in the distribution and importantly we do not observe any discontinuity in the distribution at the low-tax threshold. This validates the notion that host countries are not able to perfectly manipulate the running variable and hence our discontinuity estimate is not likely to be biased by sorting of host countries.

In Figure 7 we non-parametrically estimate the CFC targeting effect in a RD setup using local linear regressions on each side of the threshold. We use a triangular kernel function and a bandwidth of 6 percentage points in the estimation. Essentially this means that we use 6 ($\times 2$) percentage points around the threshold to estimate the main effect putting most

economics, see Lee & Lemieux (2010).

Figure 7:

Regression Discontinuity Design - Extensive margin response



Notes: Dots represent average probability differences (CFC vs. non-CFC) within 2 percentage point bins relative to the low-tax threshold. The solid line represents fitted values from local linear regressions using a bandwith of 6 percentage points and a triangular kernel. The grey shaded area represents 95 percent confidence intervals using a nonparametric bootstrapping approach with random samples collected from the main dataset of new incorporations. The re-sampling is clustered at the home-country by main-industry level using the NACE rev. 2 industry classification at the most aggregated level.

weight on observations closest to the cut-off. To estimate standard errors we again use a non-parametric bootstrapping approach to account for the additional uncertainty stemming from the calculated probability differences.[41] The discontinuity estimate corresponding to the specifications in Figure 7 is reported in Table 3 column (1). This estimate is very similar to the simple difference in means reported earlier and indicates that CFC targeting causes the probability of incorporation in a host country to drop by 0.63 percentage points. In columns (2)-(3) we report estimates based on different choices of bandwidth around the threshold.

[41]In practice we use 250 values around the threshold to construct the fitted lines in figure 7 and bootstrap each point drawing random samples from the original dataset of new incorporations. When drawing random samples we cluster the re-sampling at the home-country by main industry level to account for the correlation in choices over time and across multinational groups from the same overall industry within the same country.

The estimate is very robust to either decreasing (column 2) or increasing (column 3) this bandwidth. In the Appendix Figure 15 we show estimates for all bandwidths in the range 1-15. Apart from the very small bandwidths where estimates are naturally noisy we obtain statistically significant estimates at the 1 percent level which are all close in magnitude to the main estimate. In columns (4)-(6) of Table 3 we instead take a parametric approach and fit a polynomial to either side of the threshold. We fix the degree of the polynomial to be the same, but allow all parameters to vary on each side of the cut-off. From Figure 7 it is clear that a linear specification would produce an upward biased estimate since we clearly have some curvature on the right side of the threshold. Therefore we start in column (4) with a 2^{nd} degree polynomial and increase the degree to 3 and 4 in the subsequent columns. Using the 2^{nd} degree polynomial produces a relatively large estimate perhaps indicating that we should allow more flexible curvature. Both estimates using higher degree polynomials are very close to the main estimate from column (1).

Exploiting discontinuities and non-parametric estimation has not been the traditional ap-

Table 3:

Regression Discontinuity Estimates

	(1) LL	(2) LL	(3) LL	(4) Pol	(5) Pol	(6) Pol
RD Estimate	0.00633*** (0.00110)	0.00622*** (0.00130)	0.00637*** (0.00099)	0.00918*** (0.00092)	0.00620*** (0.00108)	0.00671*** (0.00126)
Bandwidth	6	4	8	–	–	–
Order of polynomial	–	–	–	2	3	4
Observations	3 138	1 926	3 992	9 848	9 848	9 848

Notes: The table shows regression discontinuity estimates using different specifications. The first three columns shows estimates using local linear regressions with a triangular kernel on each side of the threshold. The unit of the indicated bandwidth is percentage points. Columns (4)-(6) show estimates fitting a polynomial on each side of the threshold. We use the same order of the polynomial on both sides of the threshold but allow all parameters of the polynomial to vary. We use all observations on either side and do not restrict to a certain bandwidth around the threshold.

proach in the literature on location choices. This is likely due in part to the rareness of such discontinuities and partly to the lack of within-country variation. To ensure robustness of our results we also implement the standard choice model approach.[42] The relevant choice model in this context is the McFadden alternative specific conditional logit model, used to analyse the impact of alternative specific variables on choice probabilities. Again we only consider the choice of host country in the initial year of incorporation, since a location decision tends to be relatively persistent causing dependence between initial and subsequent location. We model the effect of CFC targeting on the choice between different foreign host countries conditional on choosing a foreign location. For each year where a multinational opens one or more new subsidiaries the dependent variable takes a value of 1 for the host countries of actual placement and zero for non-selected host countries. Results are reported in the Appendix Table 8. In the first two columns we simultaneously model the location choices of a single multinational across all years, such that each alternative is a specific host in a specific year. This enables the inclusion of year fixed effects. In the last two columns we model the choice within years, such that each alternative is a specific host and a new choice is taken in each year. All specifications include host fixed effects and we cluster standard errors on the alternative level. In both approaches the coefficient on the CFC indicator is negative and sizeable regardless of the inclusion of standard control variables. While the size of these estimates is difficult to compare with our earlier non-parametric results, this result nevertheless confirms the conclusion that CFC tax rules affect international location choices.

The main findings above suggest that CFC targeting dampens incorporation probabilities by about 0.5-0.7 percentage points. Using our main estimate, a host country moving below the CFC threshold of a foreign home country would experience a 0.63 percentage point drop in the probability of hosting a new subsidiary. This is a rather large effect given that the mean incorporation probability for a specific host in a given year is 4.9 percent if we for each home country and year only consider host countries that actually get a new incorporation. Using this mean as our baseline, CFC targeting causes a 12 percent reduction in the probability of incorporation.

[42]See Barrios et al 2012 for a very related implementation of this approach.

VI. Conclusion

The prevalence of CFC legislation among both developed and developing economies today suggests a belief among policy makers in the effectiveness of this measure in curbing international tax avoidance. This belief is expressed by the OECD in their 2015 final report on base erosion and profit shifting, BEPS (OECD/G20, 2015), where a full chapter is solely focused on CFC rules. Also in the EU such views are apparent with CFC rules featuring as one of the five anti-abuse measures included in the anti-tax avoidance directive adopted in 2016. In the context of the EU Directive for a Common Consolidated Corporate Tax Base (CCCTB) relaunched in 2016, CFC legislation is highlighted as a key tool to avoid erosion of the common EU tax base via shifting to third countries.[43]

The conclusions presented in this paper largely support the view that CFC rules are effective in redirecting profits away from low-tax environments. Studying the allocation of financial profit around tax thresholds embedded in current CFC legislation reveals large discontinuities consistent with a movement of profit into higher tax environments. This mechanism is confirmed through an analysis exploiting reform time variation, where we find a 13 percent reduction in financial profit upon entering CFC targeting. This effect could suggest that the largest international tax savings opportunities are effectively eliminated through this border-crossing tax measure.

Our results further provide highly policy relevant insights into the final destination of reallocated profits. We find that the profit reduction within low-tax subsidiaries is accompanied by a subsequent increase within higher tax environments including both the domestic market and foreign higher tax countries. Based on estimated spillover effects we conclude that roughly half of this tax base increase accrues to the domestic rule-enforcing country. This highlights the potential of these rules to obtain both international and national objectives of protecting the ability to tax corporate income at higher effective rates. In an international tax environment where coordination has proved challenging the access to an unilateral policy

[43] This directive suggests common rules across EU countries for computing the corporate tax base combined with an apportionment formula for dividing the taxable profits among member states. Within such a consolidated tax system, member states remain vulnerable to geographical income shifting using low-tax third countries and hence the issues addressed by CFC rules remain highly relevant.

tool with a direct effect on the domestic tax base is potentially essential.

Aside from the effect on the allocation of profits we further look into the consequences of these border-crossing taxes for location decisions. By taxing across domestic borders targeting specific locations, CFC rules affect the discreet investment choice through after tax returns. The design of CFC rules provides us with a unique opportunity to study the responsiveness of location choices to tax policy without imposing the strict parametric assumptions which are usually necessary in this context. Comparing location patterns, around discontinuities introduced by CFC legislation, we show non-parametric evidence that the location choice responds significantly to these rules. A direct discontinuity estimate suggests that the magnitude of such responses are large and can lead to significant changes in the corporate structures of multinationals. This would indicate that CFC rules discourage multinationals from opening tax haven subsidiaries and subsidiaries in other low-tax environments for tax saving purposes. While our results do not speak to the type or function of subsidiaries for which location choices are affected, it seems likely that at least some real economic activity is influenced by these location effects. One could imagine that the location of a subsidiary with heavy R&D activity would be affected due to large amounts of targeted royalty income.

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Appendix A - Figures and Tables

Table 4:

Descriptive statistics

	Observations	Mean	Std.Dev.	Median
All majority owned subsidiaries[1]				
Size of group[2]	3 991 878	160	328	31
Host corporate tax rate	3 940 536	28.81	6.93	29.55
Home corporate tax rate	3 980 530	30.73	5.68	30
CFC low-tax cut-off (specified in home country)	2 174 993	20.42	4.03	21
Nr Employees	753 491	183	1 967	18
Total Assets[3]	1 259 164	157 398	$3.68 \cdot 10^6$	2 995
Turnover	822 219	98 240	$1.2 \cdot 10^6$	4 837
Operating Profit	841 491	1 862	498 573	57
Financial Profit	860 345	4 699	$1.27 \cdot 10^6$	0

[1] Excluding multinationals from Tax haven countries and banks.
[2] Measured by the number of firms in the database with the same parent, i.e. members of the group which are not in the database will not be counted.
[3] All subsidiary-specific financial variables are measured in thousands of USD.

Table 5:

Home countries in the dataset

Argentina	Colombia	Iran	Moldova	Sierra Leone	The Philippines
Australia	Croatia	Israel	Montenegro	Slovakia	The United Kingdom
Austria	Denmark	Italy	New Zealand	Slovenia	The United States
Belarus	Estonia	Japan	North Korea	South Africa	Turkey
Belgium	Finland	Kazakhstan	Norway	South Korea	Ukraine
Bosnia and Herz.	France	Kuwait	Poland	Spain	United Arab Emirates
Brazil	Greece	Latvia	Portugal	Sweden	Uruguay
Bulgaria	Germany	Lithuania	Romania	Taiwan	Uzbekistan
Canada	Hungary	Macedonia	Russia	Thailand	
Chile	Iceland	Malaysia	Saudi Arabia	The Czech Republic	
China	India	Mexico	Serbia	The Netherlands	

Notes: The table lists non-haven parent countries with more than 100 observations for which ownership is above 50 percent and the main outcome variable (financial profit) is not missing.

Table 6:

Host countries in the dataset

Argentina	Croatia	Iceland	Malaysia	Russia	The Czech Republic
Australia	Cyprus	India	Malta	Serbia	The Netherlands
Austria	Denmark	Ireland	Mexico	Slovakia	The Philippines
Belgium	Estonia	Italy	New Zealand	Slovenia	The United Kingdom
Bosnia and Herz.	Finland	Japan	Norway	South Korea	The United States
Brazil	France	Kazakhstan	Peru	Spain	Turkey
Bulgaria	Germany	Latvia	Poland	Sweden	Ukraine
China	Greece	Lithuania	Portugal	Switzerland	
Colombia	Hungary	Luxembourg	Romania	Taiwan	

Notes: The table lists host countries with more than 100 subsidiary observations for which ownership is above 50 percent and the main outcome variable (financial profit) is not missing.

Table 7:

Subsidiary location choice

Placement relative to threshold	(1)	(2)
$\mathbf{1}_{[-18;-16)}$	0.00085 (0.00093)	−0.00053 (0.00083)
$\mathbf{1}_{[-16;-14)}$	−0.00078 (0.00083)	−0.00132* (0.00074)
$\mathbf{1}_{[-14;-12)}$	−0.00179 (0.00203)	−0.00218 (0.00182)
$\mathbf{1}_{[-12;-10)}$	−0.00185 (0.00130)	−0.00088 (0.00093)
$\mathbf{1}_{[-10;-8)}$	0.00045 (0.00112)	0.00089 (0.00097)
$\mathbf{1}_{[-8;-6)}$	0.00199* (0.00104)	0.00173* (0.00102)
$\mathbf{1}_{[-6;-4)}$	−0.00148* (0.00083)	−0.00095 (0.00071)
$\mathbf{1}_{[-4;-2)}$	0.00129 (0.00118)	0.00120 (0.00098)
$\mathbf{1}_{[-2;0)}$	0	0
$\mathbf{1}_{[0;2)}$	0.00560*** (0.00097)	0.00447*** (0.00096)
$\mathbf{1}_{[2;4)}$	0.00932*** (0.00147)	0.00620*** (0.00128)
$\mathbf{1}_{[4;6)}$	0.00910*** (0.00123)	0.00511*** (0.00124)
$\mathbf{1}_{[6;8)}$	0.00985*** (0.00140)	0.00645*** (0.00119)
$\mathbf{1}_{[8;10)}$	0.00905*** (0.00163)	0.00554*** (0.00138)
$\mathbf{1}_{[10;12)}$	0.00818*** (0.00155)	0.00324** (0.00136)
$\mathbf{1}_{[12;14)}$	0.00923*** (0.00194)	0.00489*** (0.00140)
$\mathbf{1}_{[14;16)}$	0.00520** (0.00222)	0.00156 (0.00149)
$\mathbf{1}_{[16;18)}$	0.00579*** (0.00214)	0.00382** (0.00169)
Total obs	9 857	10 508
R^2	0.006	0.006

Notes: The unit of observation is incorporation probability differences (calculated as explained in the main text). We sort each of these probability differences relative to the relevant low-tax threshold and include on the right-hand-side a dummy for each 2 percentage point bin. We report estimates for the 18 bins closest to the threshold. Dummies for the remaining bins are included in the regression but estimates are not reported here. We use the bin just below the threshold as the base bin. Standard errors are calculated using a bootstrap approach where we re-sample within the pool of new incorporations in the sample years. In column (1) we exclude home-countries with less than 200 observations in the original sample and in the control group we only include countries with no CFC legislation. In column (2) we exclude home-countries with less than 200 observations in the original sample and in the control group we include all countries with no CFC legislation and countries with CFC rules but no low-tax threshold.

Table 8:

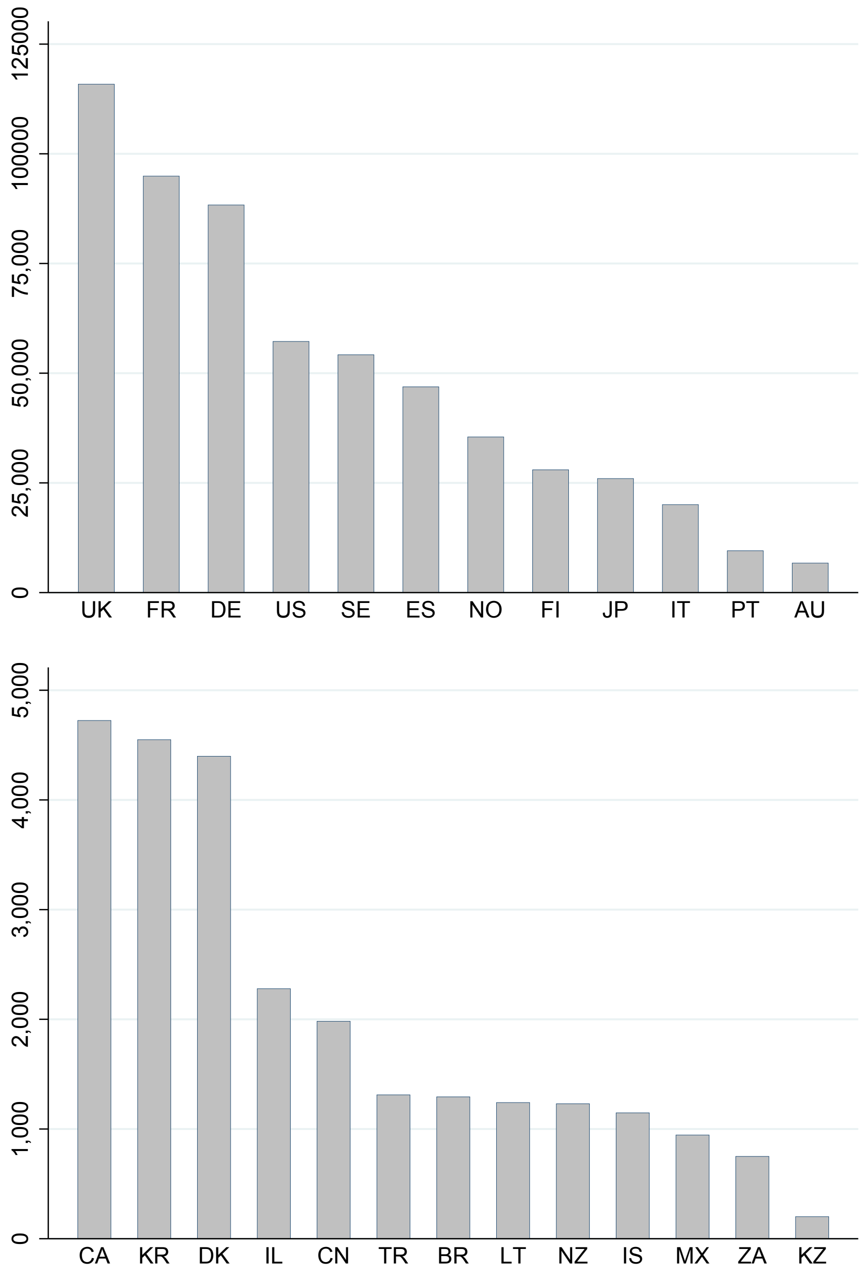
McFadden's conditional logit choice model

	(1)	(2)	(3)	(4)
CFC Indicator	-0.2234^{***}	-0.3252^{***}	-0.4515^{**}	-0.3983^{*}
	(0.0734)	(0.0838)	(0.1838)	(0.2113)
CTR (Host)	-0.0478	-0.0544	-0.0525^{***}	-0.0559^{***}
	(0.0314)	(0.0338)	(0.0135)	(0.0131)
GDP relative (Bilateral)		-0.1873^{**}		-0.1930^{**}
		(0.0889)		(0.0791)
GDP per capita (Host)		-0.0000		-0.0000
		(0.0001)		(0.0000)
Corruption (Host)		-0.2328		-0.2284
		(0.4557)		(0.3296)
Population (Host)		-0.0000		-0.0000
		(0.000)		(0.000)
Distance (Bilateral)		-0.0003^{***}		-0.0003^{***}
		(0.0000)		(0.0000)
Potential Host FE	✓	✓	✓	✓
Potential Year FE	✓	✓		
Total obs	42 319 441	29 867 199	5 156 403	3 676 885
Choices	38 190	28 738	39 662	37 229

Notes: The table shows results from McFadden's alternative specific conditional logit model. CTR is the top statutory corporate tax rate in the host country. Columns 1-2: For each group that incorporates a new subsidiary in the sample period we create a binary variable for each potential host country in each year, equal to one if the group incorporates a new subsidiary in the specific host in the given year. Columns 3-4: For each year we observe a multinational group incorporate a new subsidiary we create a binary variable for each potential host country in the sample, equal to one if the group chooses that location for a new subsidiary. In the cases where the same multinational firm opens several subsidiaries in the same year and host country, we collapse and count this as only one new incorporation since these are unlikely to be independent observations. Each estimation contains fixed effects at the potential host country level and in columns 1-2 we are also able to include fixed effects on the potential year level. Standard errors are clustered at the alternative level i.e. in columns 1-2 at the potential host times potential year level and in columns 3-4 at the potential host level.

Figure 8:

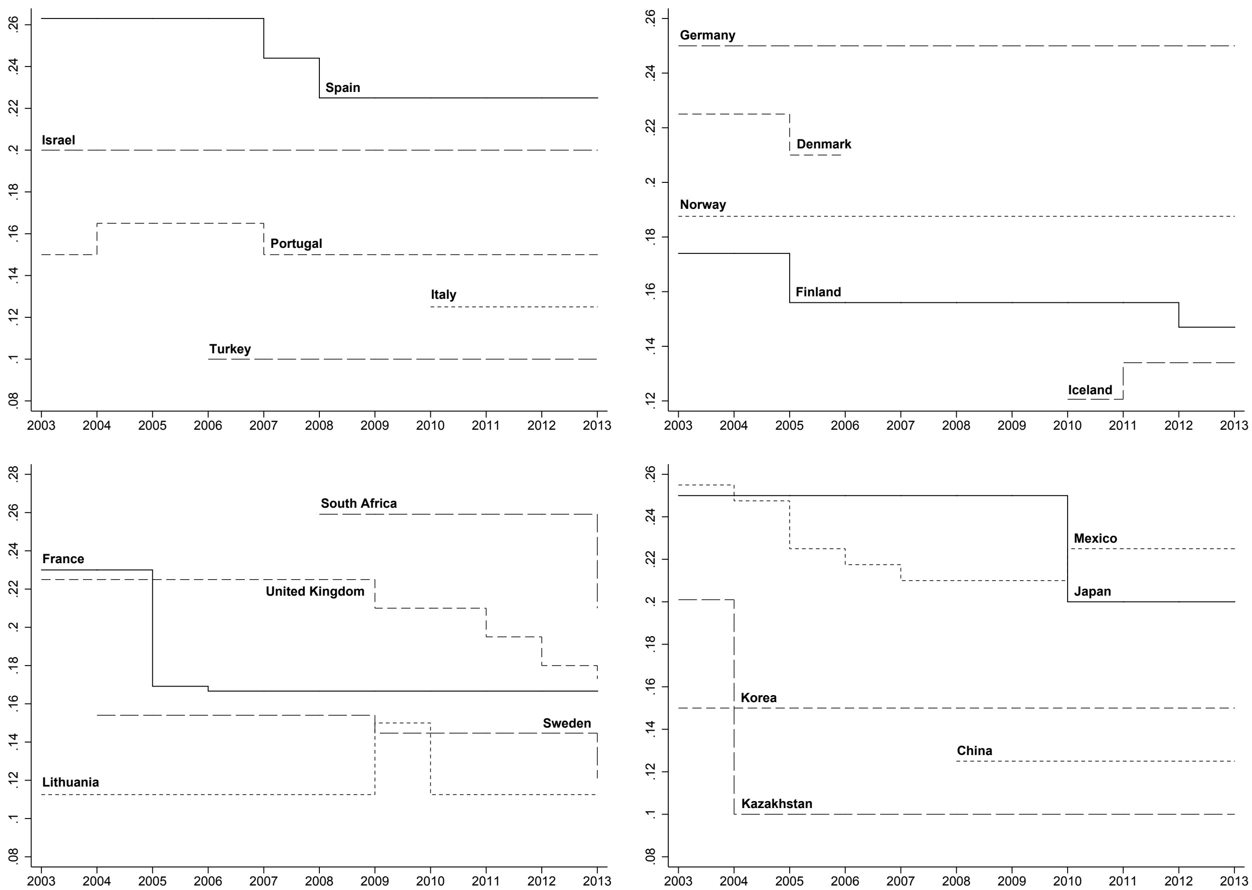
Number of observations by parent-country (only countries with CFC legislation)



Notes: The figure shows the number of observations on the key outcome variable, financial profit, within multinational groups by home-country of the group parent. We only consider observations where the parent is located in a home country with CFC legislation and only subsidiaries with a known ownership percentage above 50 percent are included. The top panel shows countries with more than 5000 observations, and the lower panel shows countries with less than 5000 observations.

Figure 9:

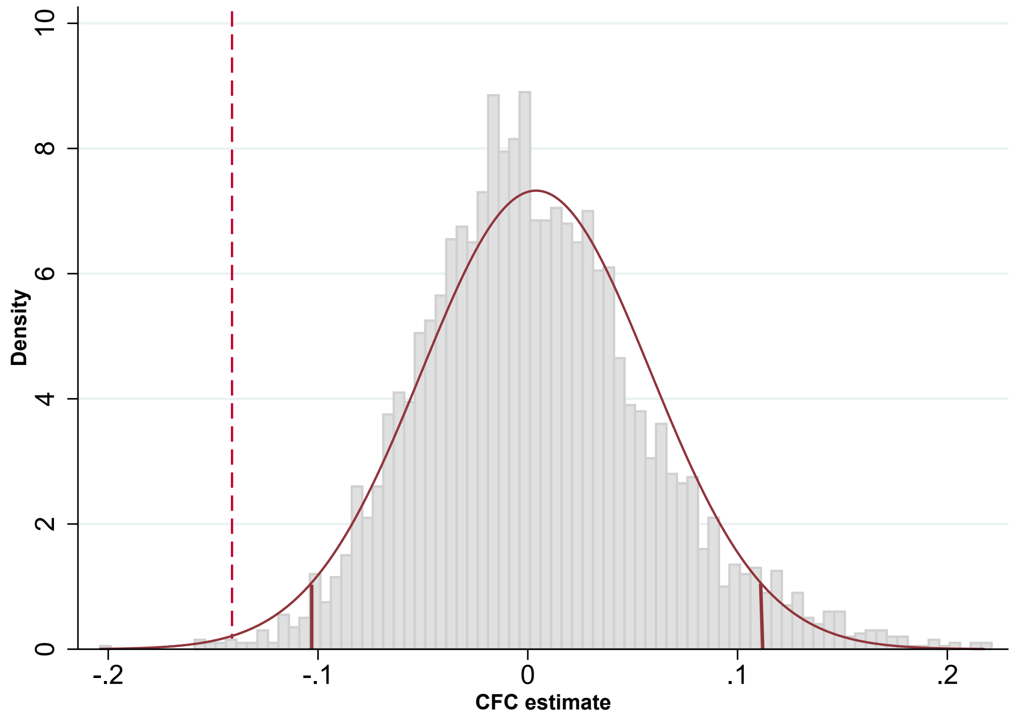
The CFC low-tax thresholds over the sample period 2003-2013



Notes: The graph illustrates low-tax thresholds over the sample period in the countries where a threshold is specified in the CFC legislation. Note that CFC legislation in many cases refers to an effective tax rate instead of a statutory tax rate. In these cases the illustration shows the approximation given by the statutory rate.

Figure 10:

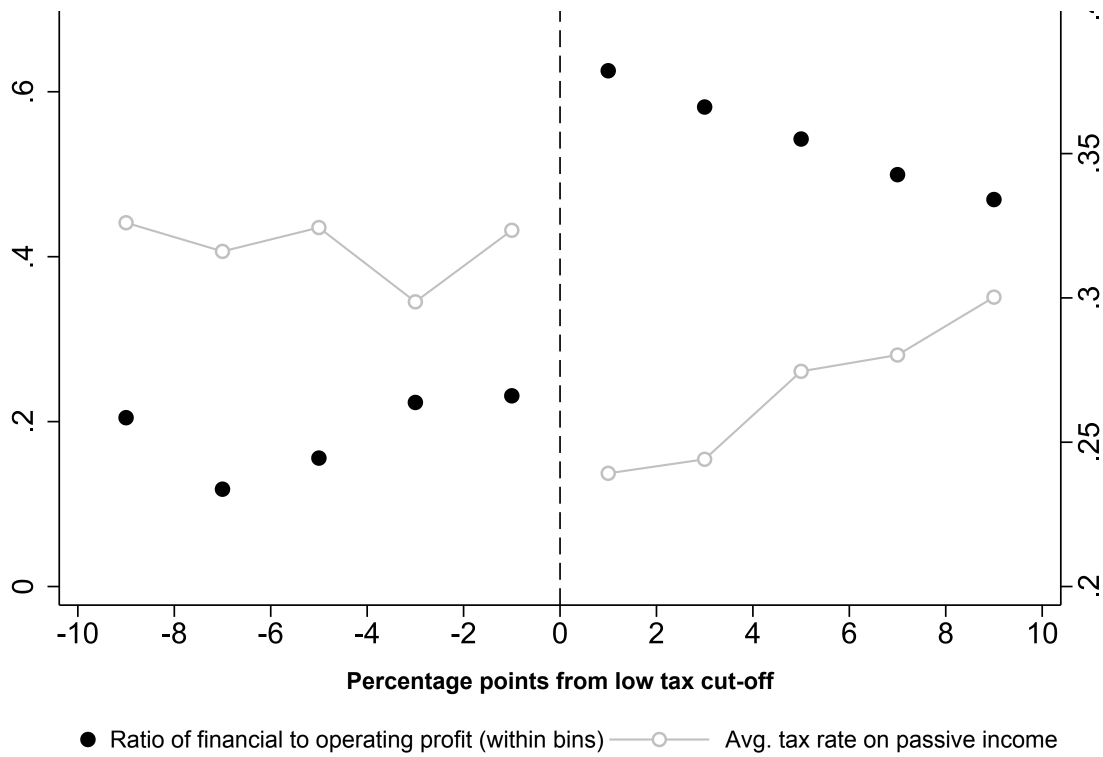
Placebo test - Moving threshold



Notes: The graph shows a histogram of placebo CFC estimates. To produce each estimate we randomly subtract or add a number drawn from a uniform distribution on the interval $[1; 10]$ to the true low-tax threshold. We draw a number for each home-host combination and add/subtract the same number in all years. We use the main specification: dependent variable ln(financial profit), controlling for ln(other assets), year × host time trends, year × home time trends and subsidiary fixed effects. This estimation is replicated 4000 times and the histogram shows the resulting distribution of these estimates. The solid line shows the density function of the normal distribution with mean (0.0040) and standard deviation (0.0544) equal to the mean and standard deviation in the pool of placebo estimates. The vertical solid lines indicate the critical values (0.025, 0.975) of this distribution. The dashed vertical line shows the main estimate (-0.1410) from Table 1 using the true threshold for estimation.

Figure 11:

Financial profit around low-tax threshold

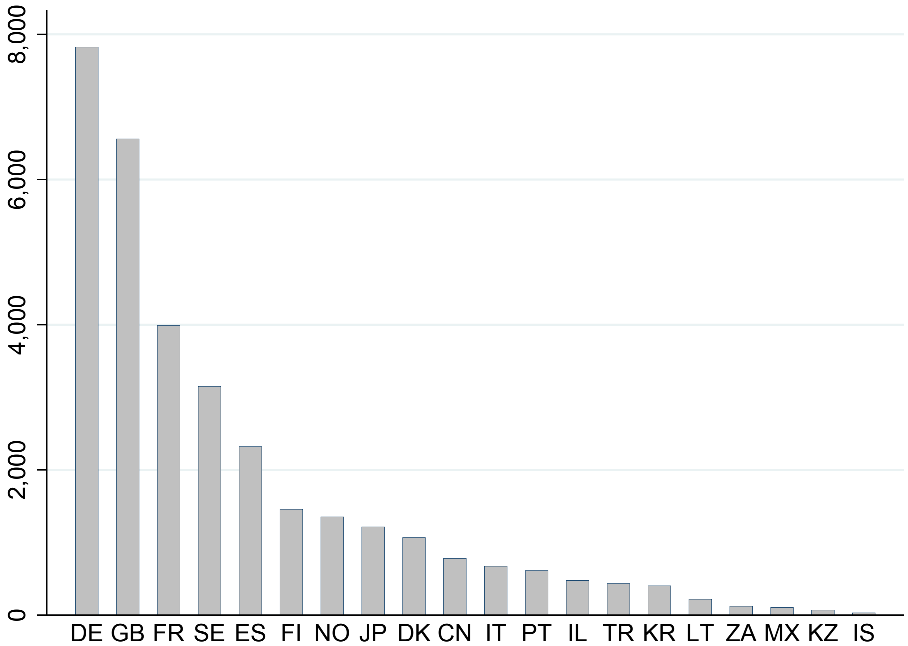


Notes: Filled circles (left axis) represent the total ratio of financial to operating profit within each 2 percentage point bin. Hollow circles (right axis) represent the average tax rate on passive income faced by the subsidiaries within each bin. Above zero (i.e. above the tax threshold) this is the average host CTR faced by subsidiaries in their respective host countries. Below zero (i.e. below the tax threshold) this is the average home CTR i.e. the corporate tax rate in the country where the parent of the subsidiary is located. We condition on positive operating- and financial profit and use only home-countries with an implemented CFC regime (with a specified tax threshold). Foreign subsidiaries with a known ownership percentage above 50 percent are included. We leave out domestic subsidiaries to avoid random fluctuations due to home-bias. We further exclude financial corporations and banks to ensure that operating profit is a meaningful scaling variable. We are not able to extend the figure further to the left due to data coverage. The data availability at such low tax rates is very limited.

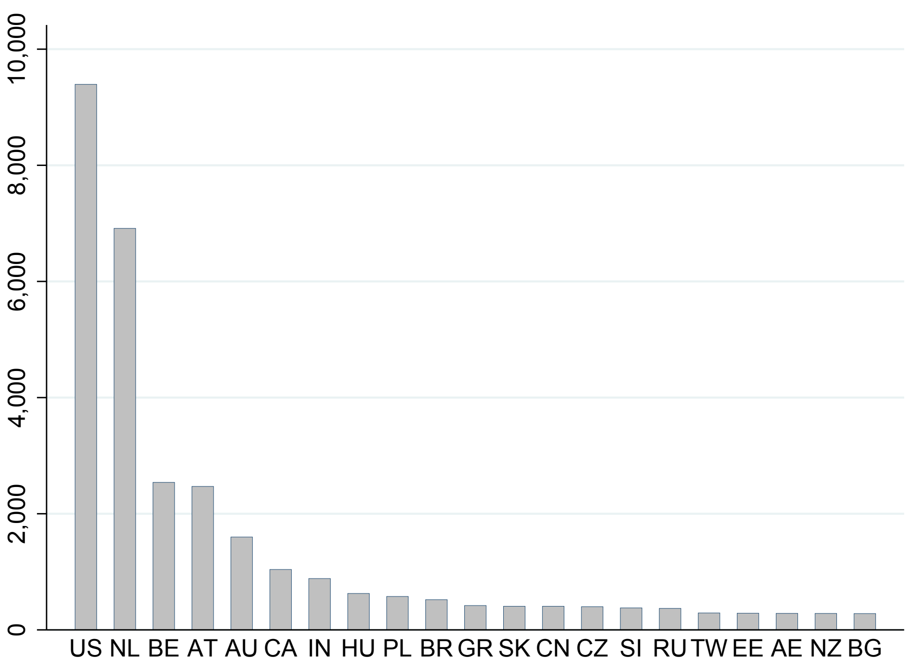
Figure 12:

Number of new incorporations (2003-2013) by parent-country

(a) Countries with CFC low-tax threshold



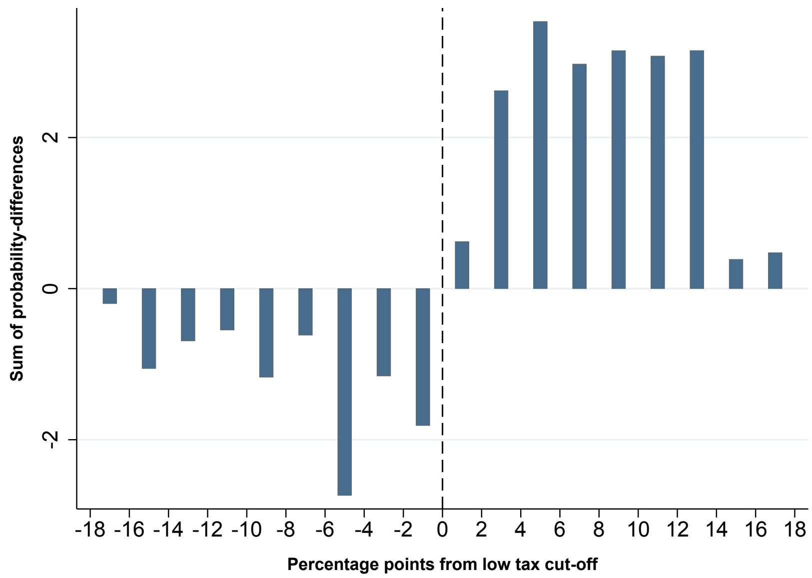
(b) Other countries excluding tax havens (>250 obs)



Notes: The figure shows the distribution of new majority owned foreign incorporations across home countries (i.e. place of group-parent). Panel (a) shows all home countries with a CFC regime including a low-tax threshold, while panel (b) shows the remaining home-countries excluding tax haven countries and countries with less than 250 new incorporations (We include home countries with less than 250 incorporations in the analysis, but for space issues we cannot show them here).

Figure 13:

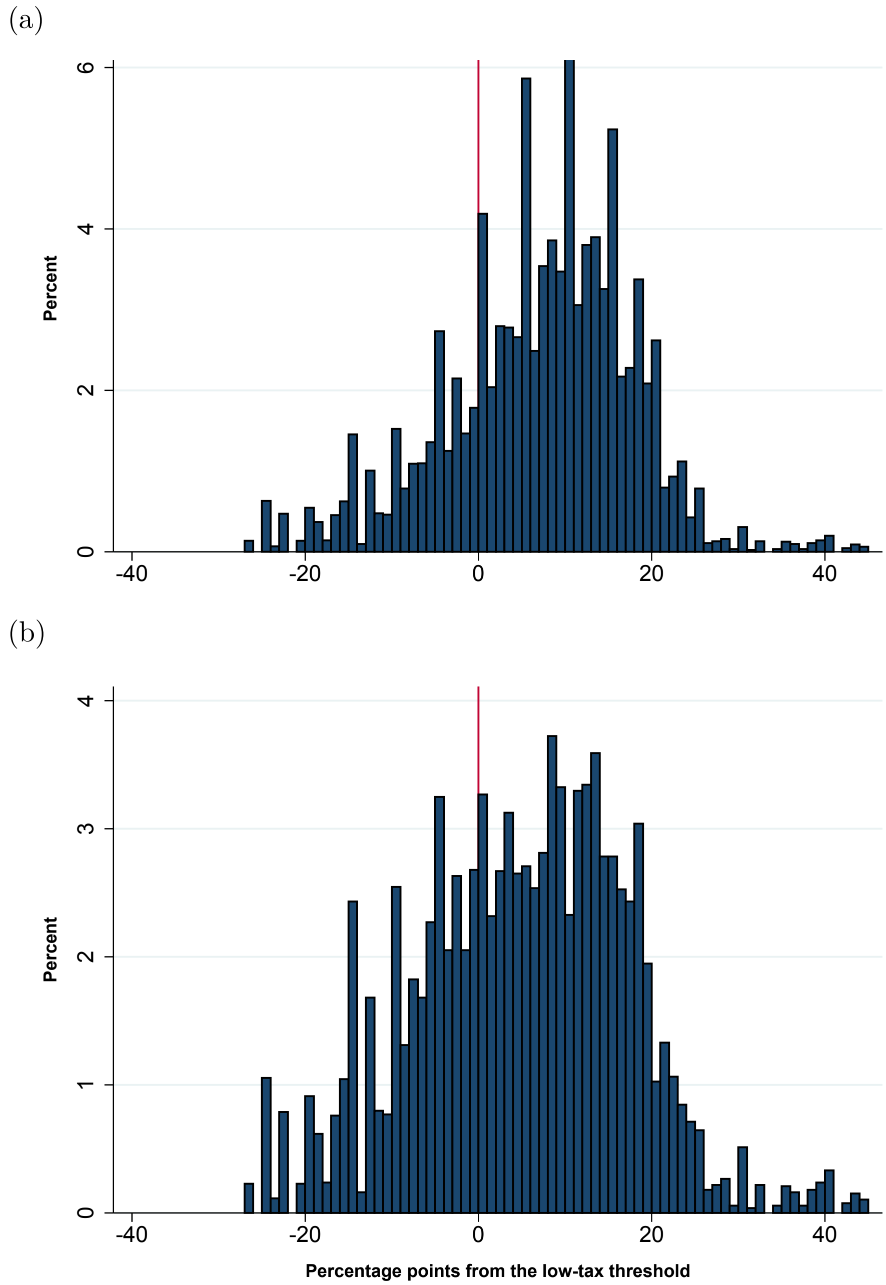
Differences in incorporation probabilities (CFC groups vs. other groups)



Notes: Bars represent the sum of probability-differences within 2pp bins relative to the cut-off. Only majority owned foreign subsidiaries are included. Incorporations from earlier years made by groups in home countries that later implement a low tax threshold are dropped (analogously for home countries that remove a low-tax threshold). We only consider CFC countries with more than 200 observed incorporations and only groups whose parent is not based in a tax haven.

Figure 14:

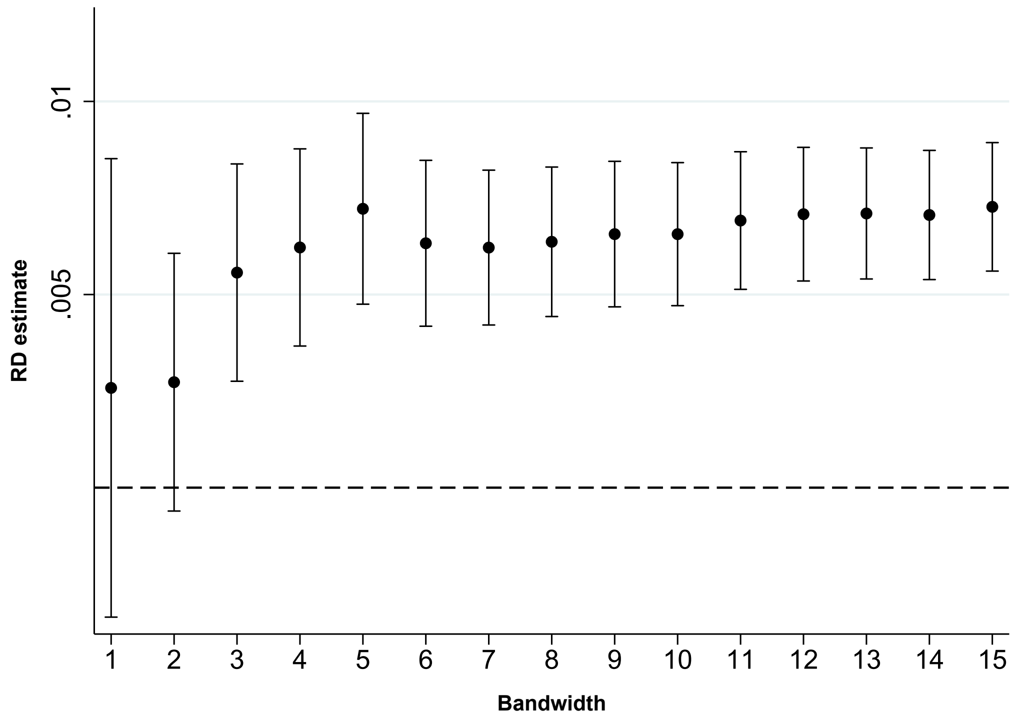
Distribution of host countries relative to the threshold



Notes: The figure shows histograms of the number of host countries relative to the low-tax threshold. I.e. For every year each of the 133 host countries in the dataset are sorted by their corporate tax rate relative to the low-tax threshold of each of the 16 CFC home countries used in the main analysis (all CFC home countries with more than 200 observed incorporations in the sample period). The figure hence shows how many (observed) host countries have a tax rate above/below the low-tax threshold specified in these 16 CFC countries. In panel a) we show all host countries in all years. In panel b) we drop host countries that have a corporate tax rate in the set $[20, 25, 30, 35]$. The red line indicates $x = 0$.

Figure 15:

Robustness of the RD estimate to bandwidth choice



Notes: The figure shows RD estimates using local linear regressions and a triangular kernel. Each point represents an estimate with a different choice of bandwidth as indicated in percentage points on the x-axis. Capped lines represent 95 % confidence intervals calculated using non-parametric bootstrap with clustered re-sampling at the home by main industry level (Using the NACE rev. 2 overall industry classification).

Appendix B - A special case: the US Subpart F legislation

As shown in Appendix C the United States introduces CFC legislation, known as *Subpart F*, already in 1962 and the tax code still contains *Subpart F* legislation throughout our sample period 2003-2013. However, in 1997 the implementation of other tax provisions in the tax law almost effectively deactivates the US CFC regime. In many ways one can argue that the US has CFC legislation, but lacks an active CFC regime. Therefore we regard the United States as a special case and we do not include the US rules in the main analysis. We explain in more detail below why the United States is not included as a CFC country and we look at the robustness of the main result to the inclusion of the United States.

In 1997 *check-the-box* tax regulations are enacted in the US.[44] With this legislation multinationals are given the right to choose the entity classification of each of their subsidiaries. By allowing multinationals to choose which subsidiaries are regarded as separate corporations and which are disregarded for federal tax purposes, this legislation effectively dismantles the US CFC legislation. The simplicity of a setup that avoids the US *Subpart F* rules is best illustrated through an example. Consider a US multinational corporation that owns a subsidiary in Belgium named Sub BE. Assume that Sub BE owns a subsidiary in Ireland, Sub IE. The US multinational can choose the classification of these two entities for US tax purposes due to *check-the-box* regulation and can hence elect Sub IE to be a disregarded entity. This means that for US tax purposes Sub IE becomes an integrated part of Sub BE and hence the two subsidiaries constitute one single entity. Say that Sub IE provides a loan to Sub BE, and hence interest payments are paid from Belgium to Ireland. Because Sub IE is a disregarded entity, these interest payments effectively disappear in the eyes of the US tax authority because they happen within a single entity. Consequently such standard passive income accruing to a subsidiary in a low-tax country is not affected by the US CFC rules. In Belgium both subsidiaries classify as corporations and hence the Belgian subsidiary can deduct the interest payments as a deductible expense. In this very simple way a US multinational can generate passive

[44]The description of the legislation in this section does not give a full overview of the details, exceptions etc. of *check-the-box* regulations. We only attempt to give an overview of how *check-the-box* matters for the practical feasibility of a CFC regime.

income in every low-tax environment without activating US CFC legislation. Importantly this outcome is obtained through a specific entity classification which multinationals are free to choose themselves. To some extent one could argue that *check-the-box* rules make CFC targeting voluntary for US multinationals.

In 2006 another piece of legislation, with similar consequences for CFC taxation, comes into effect. This law is known as the CFC *look-through-rule* and is at first a temporary measure which is later extended for multiple years. This law excludes several types of passive income from *Subpart F* taxation provided that the income stems from another related controlled foreign corporation. By disregarding payments from one subsidiary to another this rule basically obtains the same outcome as *check-the-box* regulations in terms of the effect on the CFC regime.

In two parliamentary hearings in 2012 and 2013 a subcommittee of the U.S. Senate committee on homeland security and governmental affairs investigates issues related to profit shifting by US multinationals. With a special focus on Microsoft, Hewlett-Packard and Apple Inc. the subcommittee looks into profit shifting strategies and the relation to specific aspects of the US tax code.[45] In the hearing memorandum from the first hearing report the subcommittee concludes on part of the investigation: *"In FY2011, Microsoft Corporation excluded an additional $2 billion in U.S. taxes on passive income at its offshore subsidiaries, relying on the "check-the-box" regulations and the controlled foreign corporation (CFC) "look-through" rule, which have undermined the intent of the tax code's Subpart F to prevent the shifting of passive CFC profits to tax havens to avoid U.S. tax"*[46]. In the second hearing report they write *"..., regulations, temporary statutory changes, and certain statutory exceptions have nearly completely undercut the intended application of Subpart F."*[47]

These statements vividly describe the consequences of *check-the-box* regulation for the US CFC regime. Such rules effectively remove the relevance of *Subpart F* legislation. For this reason we do not regard the United States as one of the CFC countries in our main sample, but for completeness we look separately at the US case below. We also check the robustness

[45]For a detailed description of the corporate structure of Apple Inc., and how they have circumvented the US *Subpart F* rules using *check-the-box*, see Ting (2014).

[46]From the United States Senate, hearing report (2012), memorandum page 2.

[47]From the United States Senate, hearing report (2013), memorandum page 12.

of our main result to re-classifying the US as a country with an active CFC regime.

First we use the main specification from Table 1 column (1) and include the United States as a CFC country. Remember that the US legislation contains a low-tax threshold set at 90 % of own-tax level. In Table 9 column (1) we simply replicate the main specification from Table

Table 9:

The US CFC rules

	Outcome: ln(financial profit)		
	(1)	(2)	(3)
CFC Indicator	-0.1410^{***} (0.0342)	-0.0805^{**} (0.0357)	-0.1407^{***} (0.0347)
CFC^{US} Indicator			-0.0181 (0.0419)
ln(Other assets)	0.6975^{***} (0.0359)	0.6974^{***} (0.0359)	0.6975^{***} (0.0359)
Subsidiary FE	✓	✓	✓
Year × Host FE	✓	✓	✓
Year × Home FE	✓	✓	✓
Total obs	234 236	234 236	234 236
Subsidiaries	63 742	63 742	63 742
Total obs w. CFC=1	9 252	24 025	9 252
R^2	0.40	0.40	0.40

Notes: The unit of observation is majority-owned subsidiaries (excluding banks) within multinational groups with at least 3 entities, where the parent corporation is not located in a tax-haven country. The dependent variable is the natural logarithm of financial profit. Single-year shifts in the CFC indicator (i.e. subsidiaries moved below (above) the threshold by a reform one year and back above (below) by another reform the next year) are not acknowledged as shifts since reforms can happen at any time during a year and hence we cannot know how many months were between such reforms. In practice we keep the CFC indicator constant across such shifts (i.e. if the shift is above and back below the indicator remains at 1 throughout). Two-way clustered standard errors at the home-country and the host-country level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

1, and hence the CFC estimate is *not* based on information for the US. In column (2) we add the US as a CFC country along with the original sample. The coefficient estimate on the CFC indicator remains negative, sizeable and significant at the 5 percent level. In other words the choice of whether to include the US does not change the main result of a large negative impact of CFC targeting on passive profits. Note however that the inclusion of the US causes a drop in the magnitude of the estimate. For the reasons explained above we do not expect US owned subsidiaries to experience a change in CFC status when crossing the threshold. Since US owned subsidiaries constitute a relatively large portion of the observations in the sample a lack of response among these subsidiaries has the potential to severely bias the coefficient estimate towards zero. In column (3) we again only include the original sample of CFC countries in the definition of the CFC Indicator, but create a separate variable for the US system. In other words CFC^{US} is an indicator variable equal to 1 if firm f in year t is owned by a US parent corporation and is below the US threshold. While the coefficient estimate on the US targeting variable is negative, it is close to zero and statistically insignificant on all traditional significance levels.

The fact that we obtain a statistically insignificant estimate, when looking solely at a single CFC regime, is not surprising as the specification using a full set of subsidiary fixed effects requires substantial reform variation. However, if we compare the coefficient estimate for the US to coefficient estimates for countries that contribute with comparable levels of data and variation we generally obtain numerically much larger coefficients for other countries. Looking at the five CFC countries providing most variation in the data we get coefficient estimates ranging from -0.1032 to -0.1600.[48] Note that each of these estimates are associated with large standard errors and hence substantial uncertainty. Consequently this comparison between coefficients is suggestive but in no way statistically conclusive.

While it is beyond the scope of this paper to provide a full picture of the US tax rules for foreign income, our results suggest that the US is in fact a special case. Why the tax code includes these counter-working provisions is an interesting question in itself but a question outside the main focus of this analysis.

[48]These countries are The United Kingdom, Japan, Germany, Spain and France.

Appendix C

Overview of CFC legislation (2003-2013)

Country	Year of intro.	Concerning years	Tax cut-off	Country list	Control requirement	Exemption related to type of income/ assets
Australia	1990	2003-2013	-	Grey list	Three tests of control: (i) Strict control: > 50 % interest owned by 5 or less res. shareh. (must be 1 % entities and control is associate inclusive - AI). (ii) Objective de facto: Single res. entity with (AI) control interest of > 40 % (iii) Subjective de facto: Other forms of "control" in economic sense by 5 or fewer res. entities. Further: Separate entity only attributable if it has (AI) control interest > 10 % (special rule of > 1 % if control based on (iii))	Active income test: several conditions on transparency and compliance with accounting principle plus tainted income ratio must be less than 5 %. De minimis: Entity is in a listed country AND EDCI[1] < min(AUD 50.000, 5% of gross turnover)
Brazil	2001	2003-2013	-	-	Company must be either controlled or affiliated. Controlled: shareh. has rights assuring influence over business decisions and power to appoint majority of administrators (directors or officers). Affiliated: requires "relevant influence" - this is assumed when > 20 % of voting stock is owned.	. [2]
Canada	1972	2003-2013	-	-	Taxpayer's equity percentage > 1 % and total equity percentage of taxpayer and related persons > 10 %. Further taxpayer (along with the three classes of persons)[3] must have voting control.	If tainted (FAPI) income < $ 5.000.
China	2008	2008-2013	50 %	White list[4]	> 10 % of shares with voting rights and > 50 % of shares w. other res. shareholders. Or effective control.	If "mainly" active (in practice: > 50% active income)
Denmark	1995	2003-2006	75 %	-	Controlling interest	< 33.3% of total taxable income is financial
Denmark	1995	2007-2013	-	-	Decision-making influence: > 50% of voting rights	< 50% of taxable income is tainted < 10 % of total assets are financial
Finland	1995	2003-2013	60 %	Blacklist	> 50 % capital or voting rights, or entitled to > 50 % of yield of net wealth - aggr. across res. shareh. AND	If income mainly from shipping, industrial or oth. comparable production

Continued on next page

[1] EDCI: "Eligible designated concession income" - refers to income in (listed) countries that is not deemed "comparably taxed", i.e. income benefiting from preferential regime or a gap in tax base.

[2] The Brazilian CFC regime targets both passive and active income.

[3] The three classes of persons: 1. persons not dealing at arm's length with the taxpayer. 2.Any four Canadian resident persons - there does not have to be any relationship between the taxpayer and the four other Canadian shareholders. 3. Persons not dealing at arm's length with the four shareholders mentioned earlier.

[4] Only effective since 2009

Table 1 – *Continued from previous page*

Country	Year of intro.	Concerning years	Tax cut-off	Country list	Control requirement	Exemption related to type of income/ assets
					each shareh. > 25% interest.	activity. If entity located in tax treaty country, not on "blacklist", and has "economic substance".
France	1980	2003-2004	67%	-	> 10% of shares, interest shares, financial rights or voting rights.	
France	1980	2005-2012	50%	-	> 50% of shares, interest shares, financial rights or voting rights. OR > 5% of shares and > 50% shares, interest shares, financial rights or voting rights held by resident shareholders.	If profits come from an "effective industrial or commercial activity" carried out in own territory AND < 20% of profits derive from "tainted" income.
Germany	1972	2003-2007	25	White- and blacklist[5]	> 50 % of shares or voting rights held by resident shareholders.	< 10 % of overall gross income is passive and the disregarded amount < €62.000 (both at CFC level and at shareh. level across subs.). Selected industries are exempt - such as insurance and banking.
Germany	1972	2008-2013	25	White- and blacklist[6]	> 50 % of shares or voting rights held by resident shareholders.	< 10 % of overall gross income is passive and the disregarded amount < €80.000 (both at CFC level and at shareh. level across subs.) Selected industries are exempt - such as insurance and banking.
Iceland	2009	2010-2013	67 %	-	> 50 % of capital or voting rights. If in other way "control" can be established.	If located in a tax treaty state AND < 50 % tainted (passive) income.
Israel	2003	2003-2013	20	-	> 50 % of any "means of control"[7] owned by res. shareh. in aggr.; OR > 40 %, and > 50% along with non-res. relative. OR a res. has right to prevent taking of substantive managerial decisions. The shareholder in question must hold separately (or with relative or similar) > 10 % of any "means of control".	< 50 % of income and of profits derive from passive (tainted) income.
Italy	2002	2003-2005	-	Blacklist	> 50 % of voting rights OR sufficient voting rights to	Carrying out true industrial/commercial

Continued on next page

[5] Only function as unofficial and unbinding lists of guidance

[6] Only function as unofficial and unbinding lists of guidance

[7] The term "means of control" is defined as one of the following: a) right to profits, b) right to appoint director or chief executive officer, c) right to vote in the general meeting, d)right to a share in the remainder of assets after a dissolution, e) right to instruct someone who has any of the rights listed above.

Table 1 – *Continued from previous page*

Country	Year of intro.	Concerning years	Tax cut-off	Country list	Control requirement	Exemption related to type of income/ assets
					exert "dominant influence" OR contractual ties permitting "dominant influence"	activity (as main activity) on local market OR If proven that CFC is not set up for tax avoidance purposes (> 75% of income taxed in non-blacklist states).
Italy	2002	2006-2009	-	Blacklist	> 20 % of financial rights	(Same as above)
Italy	2002	2010-2013	50%	Blacklist	> 50% of voting rights OR sufficient voting rights to exert "dominant influence" OR contractual ties permitting "dominant influence". If in blacklist: > 20 % of profits.	< 50% "tainted income" and entity in a non-blacklist territory OR in a blacklist territory carrying out true industrial/ commercial activity on local market OR in a blacklist territory and > 75 % of income taxed in non-blacklist states.
Japan	1978	2003-2009	25	-	> 50 % of shares, capital, voting rights or rights to receive dividends (separately or aggr. across res. shareholders (each > 5%))	If CFC engages in "substantive business" (list of criteria for this classification is given)[8]
Japan	1978	2010-2013	20	-	> 50 % of shares, capital, voting rights or rights to receive dividends (separately or aggr. across res. shareholders (each > 10%))	If CFC engages in "substantive business" (list of criteria for this classification is given)
Kazakhstan	1995	2003	67%	-	>10 % authorized capital or voting shares (directly or indirectly)	.[9]
Kazakhstan	1995	2004-2013	10	Blacklist[10]	>10 % authorized capital or voting shares (directly or indirectly)	
Korea	1996	2003-2013	15[11]	Blacklist[12]	A "special relationship"[13] between the resident and the	If CFC actively engages in business

Continued on next page

[8] Parts of the CFCs income can still fall under CFC taxation if this income is passive and generated for certain types of assets (interest from bonds, royalties from IP etc.)

[9] Kazakhstan does not differentiate between active and passive income of the CFC, and also has no exemptions related to types of income.

[10] Introduced in 2005.

[11] This rate refers to the average effective income tax rate for the most recent three consecutive years.

[12] Only in the years before 2010.

[13] A "Special relationship" is defined as either of the following: a) ownership of > 50 % of voting shares b) third party owns > 50 % of voting shares of the two parties considered c) common interests through investment, transaction of goods, loan etc. and power to make business decisions on behalf of the company d) common interests through investment, transaction of goods, loan etc. and third party has power to make business decisions on behalf of both parties considered.

Table 1 – *Continued from previous page*

Country	Year of intro.	Concerning years	Tax cut-off	Country list	Control requirement	Exemption related to type of income/ assets
					foreign company must be present. Furthermore the resident must separately[14] own > 10 % of shares or capital.	through fixed place such as office, shop, factory etc.[15]. Or "De minimis" : If earned profits < KRW 200 million.
Lithuania	2002	2003-2013	75%	Grey- and blacklist	> 50% of shares, or other rights to profit (or rights to acquisition thereof) aggr. across related residents. Shareholder must separately hold > 10 % of shares or other rights to profits (or rights to acquisition thereof)	If income of the CFC comprises < 5 % of the income of the shareholding (controlling) corporation.
Mexico	1997	2003-2013	75%	Blacklist[16]	Ability of shareh. (or connected persons) to determine timing of profit distribution (control is assumed until proven otherwise). Based on avg. daily participation	<20% of the CFC's total income is passive.[17]
New Zealand	1988	2003-2013[18]	-	Grey list[19]	Group of five or less res. hold > 50 % "control interest" or control exercise of decision making rights. Or single res. holds >40% "control interest".[20] (Res. generally only taxed if owning >10 % income interest)	The CFC is exempt if < 5% of gross income is passive. Australian CFCs (taxed in AU) are (generally) exempt. Insurance industry is exempt.
Norway	1992	2003-2013	67 %	White- and blacklist	> 50 % of shares or capital is owned or controlled (separately or aggr. across res. shareholders)	If a DTA is in place and income of the CFC is "not mainly of passive character"
Portugal	1995	2003-2013	60%	Blacklist	Resident shareholder have holding of > 25 % OR > 10 % if > 50 % of share capital held by Portuguese tax res. entities	Entity excluded if: 1. > 75 % of profits from agricultural/industrial activity in own juris., or commercial activity not with Portuguese entities. 2. Cannot engage in specified transactions[21].

Continued on next page

[14] When ownership percentage is determined the ownership of the following relatives are counted: spouse, lineal relatives and siblings, spouse of lineal relatives in same household, lineal relatives and siblings of a spouse in the same household.

[15] There are a few exceptions to to this rule, namely: a) the exemption does not come into effect if the CFC is doing business in wholesaling, financing, insurance, property rental or business services AND its aggregate revenue and aggregate purchase cost generated from these activities exceeds 50% of total revenue or total purchase cost b) exemption is not valid if CFC's primary business (i.e. revenue from activity represents more than 50 % of total revenue) is owning stocks, equities or bonds or providing IP rights, leasing ships, aircraft or equipment or making investments in trusts or funds.

[16] This list is currently just for reporting purposes and no longer determines CFC treatment. Resident taxpayers must each year report any income earned from a listed country.

[17] If the CFC is liable for CFC taxation both passive and active income will be taxed.

[18] The rules were significantly reformed in 2008. Before both passive and active income of the CFC was taxed, while from 2009 only passive income was targeted.

[19] This list exempted companies resident in one of the mentioned countries if they had not benefited from certain specified tax preferences. The original list contained: Australia, Canada, Japan, Germany, UK, US and Norway. Spain was added in 2006. The grey list was abolished in 2008.

[20] A "control interest" can be either shareholdings, rights to receive income or rights to receive distributions of net assets. It can be either direct or indirect and also counts holdings of associated persons.

[21] The business of the corporation musty not involve the following: a. Transactions traditionally related to banking activities. b. Transactions related to insurance business (if income relates to the insurance of assets located mostly outside own jurisdiction). c. Transactions related to equities, other securities, IP, industrial property, knowhow or the rendering of technical assistance. d. Leasing of assets (except landet property in win jurisdiction.

Table 1 – *Continued from previous page*

Country	Year of intro.	Concerning years	Tax cut-off	Country list	Control requirement	Exemption related to type of income/ assets
South Africa[22]	2001	2008-2013	75%	-	> 50 % of participation rights[23], or > 50% of voting rights aggr. across res. persons. (voting rights in listed companies not counted, and if single res. holds < 5 % in a listed company also not counted). Shareholder must separately (or with connected person) hold > 10 % of participation rights or voting rights to be subject to CFC taxation.	If CFC has a "place of business" (FBE), defined by characteristics such as "on site employees" etc., AND the tainted income < 5% of total amounts accrued to the CFC attributable to the FBE.
Spain	1994	2003-2013	75%[24]	Grey list	> 50 % capital, equity, profits or voting rights (separately or "group control")	< 15 % of total income is tainted income OR < 4% of total gross income is tainted income. [25]
Sweden	2004[26] /1991	2004 - 2013	55 %	White-, grey- and blacklist	Holding/having control over > 25 % of capital or voting rights (separate or w. entities "in association")	Income assignable to int. shipping business and shareholder also engaged in shipping business
Turkey	2006	2006-2013	10	-	> 50% of capital, share of profit or voting power. Separately or w. other res. shareh.	< 25% of gross income is passive OR annual gross income < 100.000 TL.
UK[27]	1984	2003-2013[28]	75 %	Grey list	> 50 % shares, capital or voting power or entitled to > 50 % of distributed profits (or options to acquire rights in the future) aggr. across UK res. Further: each must separately have rights to > 25 % of profits.	1. CFC distributes > 90 % of chargeable profits to UK res. 2. CFC on excluded countries list AND non-local source income < max(£ 50.000, 10 % of com. quantified income)[29]. 3. > 35 % voting power allotted the public AND principal members[30] own < 85 % of voting power.

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[22] South Africa has over the years implemented many changes to their CFC regime, and hence going through all variants before the introduction of the low-tax threshold in 2008 is beyond the scope of this table. We therefore only show the overall characteristics of the regime between 2008-2013, and not for the full sample period.

[23] "Participation rights": The benefits attached to a share in a company, or interest of a similar nature, or to voting rights.

[24] A CFC resident in a tax haven will per assumption live up to the low tax requirement.

[25] A special exemption applies to foreign holding companies, as passive income from such a source might just be "formally passive" . Requirements regarding participation as well as the source of the income applies.

[26] There is some disagreement as to whether the system in place before 2004 could also be classified as a CFC regime. Significant changes were however completed in 2004.

[27] Note that the United Kingdom has undergone major changes to other parts of the corporate tax system in this period - amongst others a shift from a credit system to a dividend exemption system.

[28] Small changes were made to the legislation in 2011, including two new exemptions regarding overseas trading and IP companies. The UK CFC regime was more fundamentally reformed in 2013 with the introduction of different "gateways" and a focus on taxing profits deriving from UK activity, rather than worldwide activity. Initial drafts for this reform were published already from 2009, and hence might have been expected by some corporations. Importantly the "low tax threshold" was not altered in this reform, but was re-formulated, now in terms of an exemption instead of a requirement for CFC status.

[29] Commercially quantified income is profits before tax, while non-local source income is gross income.

[30] A principal member is an entity, who together with associated parties possesses > 5 % of the voting power.

Table 1 – *Continued from previous page*

Country	Year of intro.	Concerning years	Tax cut-off	Country list	Control requirement	Exemption related to type of income/ assets
						4. CFC runs true business in juris. of residents and management. 5. CFC's transactions do not achieve sign. reduc. in UK tax (or have this motive)
USA	1962	2003-2013	90%	-	$>$ 50 % of total combined voting power or total value of the stock owned in aggr. by res. shareholders. Separately shareh. must own $>$ 10 % of total combined voting power to be subject to CFC taxation	Income may be excluded if subject to "high rate of foreign tax". De minimis: If gross tainted (FBCI) income $<$ min(5 % of gross income, $ 1 million)

Hungary also has a CFC regime, but it is only targeted at Hungarian individual shareholders, and not corporations. Therefore this regime is not relevant for the analysis in this study, where the focus is on Multinational corporations. Argentina has a set of rules known as the "international fiscal transparency rules" (IFT), which bear a lot of resemblance to a CFC regime. These rules contain a "blacklist" of low-tax countries targeted by the rules and they target passive income of foreign corporations. However they only apply for share-corporations and they do not require control over the foreign corporation.

2 | Tax enforcement with multiple evasion technologies

Tax enforcement with multiple evasion technologies

Sarah Clifford and Niels Johannesen†

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Abstract

In standard models of tax enforcement with a single evasion technology, stricter enforcement unambiguously leads to less evasion and more revenue. We show that in the presence of multiple evasion technologies, stricter tax enforcement may lead to more tax evasion and it may be associated with a loss of government revenue and lower welfare. Intuitively, increasing the probability of detecting one of the evasion technologies, induces some firms to switch to the other evasion technology. If the latter technology is more expensive but allows for more evasion, then this behavioural effect may dominate other effects and evasion may increase while revenue drops.

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1 Introduction

Economic theories of tax enforcement typically assume that taxpayers have access to a single technology that allows them to evade or avoid taxes. When stricter enforcement increases the likelihood that non-compliance is detected, taxpayers respond by becoming more compliant. This prediction goes back to the canonical work by Allingham and Sandmo (1972) and is found in most later theoretical treatments of tax evasion and avoidance (Slemrod and Yitzhaki, 2002).

The standard framework for studying tax compliance, however, is a simplified representation of reality, because taxpayers in the real world typically have many *different* ways to reduce their tax liabilities. For instance, wealthy individuals may evade wealth taxes by simply misreporting the value of their assets (Seim, 2017) or by holding wealth through secret Swiss bank accounts (Johannesen, 2014a). Owners of closely held businesses may evade income taxes by simply underreporting sales (Slemrod et al., 2017) or by having their offshore shell corporations issue invoices that raise deductible costs (U.S. Senate, 2008). Multinational firms may evade corporate income taxes by mispricing intra-firm sales (Cristea and Nguyen, 2016) or by financing foreign investments with hybrid financial instruments (Johannesen, 2014b).

Clearly, the alternative technologies for evading taxes may be subject to different risks of detection, involve different levels of resource costs and have different abilities to shield income. In many contexts, it is meaningful to distinguish between *simple* techniques that are inexpensive, for instance because they can be implemented by the tax payers themselves, but are easily detected, and more *sophisticated* techniques that are costly, for instance because they require the assistance of professional advisors, but are difficult to detect even for experienced tax inspectors. The question we ask in this paper is whether the presence of multiple heterogeneous techniques for evasion changes the prediction that stricter tax enforcement unambiguously leads to more tax compliance and more revenue collection.

To address this question, we develop a simple model of tax enforcement where firms have access to two different tax evasion technologies: a *simple* and a *sophisticated* technology. The simple technology requires few resources, but involves a high risk of detection and shields only a small fraction of income. The sophisticated technology requires more resources, but is associated with a lower risk of detection and shields more income than simple evasion. Assuming that the cost of implementing a given evasion technology is fixed and that firms are heterogenous with respect to the size of their revenue, we describe a baseline equilibrium where the smallest firms optimally choose to be fully compliant, intermediate firms opt for the simple evasion technology whereas large firms choose the sophisticated evasion technology.

In this framework, we study the government's choice of enforcement policy. Unlike the standard model with a single evasion technology where the government chooses a single audit probability, our model includes two audit parameters: the probability of detection for a firm that uses the simple and the sophisticated evasion strategy respectively.[1] Our key insight is the following. When the government increases the probability that the simple evasion technique is detected, some firms abandon this technique: relatively large firms adopt the sophisticated evasion technology whereas relatively small firms become compliant. If substitution toward the sophisticated technology is sufficiently strong, the overall effect of the enforcement effort is to *increase* evasion. Intuitively, the firms that adopt the sophisticated technique in response to a small policy change are indifferent between the two evasion techniques - the higher cost of the sophisticated technique is exactly offset by lower expected tax payments - but the substitution is associated with an increase in evasion. Moreover, the loss of revenue following from increased evasion may exceed the mechanical revenue gain from conducting more audits so that the total effect is to *decrease* government revenue.

An important premise of our analysis is that different techniques for reducing tax liabilities are substitutes so that enforcement efforts against one technique may cause increased use of other techniques. This premise finds broad support in the empirical literature. For instance, evidence from both the U.S. and Equador shows that third-party reporting of business sales induces firms to report more revenue, but also to report more costs suggesting that there is

[1]The two probabilities may differ either because audits are targeted a particular type of evasion or because general audits detect the two evasion techniques with different success rates. In either case, the government may set the two detection probabilities independently. Under targeted audits, it may increase the frequency of audits targeted one evasion technique and leave the frequency of the other audits unchanged. Under general audits, it may increase the time allocated to detect one evasion technique and leave the time to detect the other technique unchanged.

considerable substitution between underreporting of sales and overreporting of costs (Carrillo et al., 2017; Slemrod et al., 2017). Further, there is evidence that international cooperation to enforce taxation of foreign accounts induces tax evaders to shift assets from cooperating to non-cooperating tax havens (Johannesen and Zucman, 2014) and to transfer ownership to anonymous offshore holding companies (Omartian, 2017). Finally, a related study from the trade literature documents that a crackdown on one method to evade custom duties in the Philippines leads to a significant increase in the use of another method (Yang, 2008). The main exception is the finding that tax evaders who voluntarily disclosed foreign assets under a tax amnesty in Norway significantly increased their tax payments without relying more on domestic tax avoidance techniques (Alstadsæter et al., 2018).

Our main contribution is to qualify one of the most important predictions of the existing theoretical literature on tax evasion: more tax enforcement is associated with less tax evasion (Allingham and Sandmo, 1972). While this result is unambiguous when tax payers have access to a single evasion technique, we show that it does not necessarily hold in the presence of multiple heterogenous evasion techniques: increasing enforcement against simple tax evasion techniques may trigger substitution toward more sophisticated techniques, capable of shielding more income, so that the total effect is to increase evasion and decrease revenue. We note that this result cannot emerge when the marginal enforcement efforts are targeted sophisticated tax evasion: in this case substitution is toward simple tax evasion, which helps reducing overall evasion and raising government revenue. This result implies that substitution between evasion strategies makes it more attractive to fight sophisticated rather than simple evasion techniques and thus sheds some light on the optimal sequence in governments enforcement policies, as explored in a small existing literature (e.g. Elsayyad and Konrad, 2012).

The rest of the paper is structured as follows: Section 2 presents a simple framework illustrating the essential mechanisms leading to our overall conclusions. Section 3 discusses the circumstances under which it is likely that increased enforcement will lead to increased evasion. Section 4 relaxes some of the simplifying assumptions made in the first sections before section 5 concludes.

2 Model

In this section, we consider a simple framework that illustrates the main mechanisms of interest. In later sections, we extend this basic setup and incorporate more realistic assumptions on the business landscape.

The basic framework

Consider an economy with a large number of firms with taxable profits π distributed according to the density function $f(\pi)$. Denote the corporate tax rate t such that firms that do not engage in any tax evasion have after-tax profits given by,

$$\pi - t\pi$$

There are two available tax evasion technologies: *simple* (technology 1) and *sophisticated* (technology 2). Technology i is associated with a fixed cost of c_i, allows for evasion of taxation on a share s_i of a given firm's profits and is detected with probability p_i for $i = 1, 2$. Expected after-tax profits if the firm adopts technology i are:

$$\pi - (1 - s_i)t\pi - c_i - p_i s_i(t + \phi)\pi$$

where $\phi \geq 0$ is a fine imposed in case evasion is detected. Define the parameter $\theta_i = s_i t - p_i s_i(t + \phi)$ which expresses the expected tax savings as a share of profits under evasion technology i. We can thus restate after-tax profits using evasion technology i as,

$$\pi - t\pi - c_i + \theta_i\pi$$

Now assume that $\theta_2 > \theta_1 > 0$ and that $c_2 > c_1$. The assumption that both θ_1 and θ_2 are positive means that both evasion technologies generate a tax saving in expectation (but not necessarily large enough to compensate for the fixed cost of implementing the strategy). If θ_i were negative, technology i would never be used even if it were costless. The assumptions $\theta_2 > \theta_1$ and $c_2 > c_1$ imply that technology 1 is a low cost, but also low tax savings, technology whereas technology 2 is a high tax savings but high cost technology. The difference in expected tax savings ($\theta_2 > \theta_1$) can be due to differences in either the probability of detection ($p_1 > p_2$)

or in the share of the tax base that can be shielded with the technology ($s_1 < s_2$). The assumption that one technology has higher expected tax savings and the other has lower costs is necessary for both technologies to be used. If one technology had the highest costs and the lowest expected tax savings, it would be strictly dominated by the other technology.

Firms' problem

Assume that firms can evade taxation on part of their profits using either the simple or the sophisticated technology. If firms are maximizing expected profits, then strategy i is (weakly) preferred to no evasion if: $\theta_i \pi \geq c_i$. This simply states that expected tax savings from evasion need to exceed the costs in order for evasion to be profitable. This expression defines a threshold level of profits $\pi_{10} \equiv c_1/\theta_1$ above which evasion technology 1 is preferred to no evasion and another threshold level of profits $\pi_{20} \equiv c_2/\theta_2$ above which technology 2 is preferred to no evasion.

Technology 2 is (weakly) preferred to technology 1 if $(\theta_2 - \theta_1)\pi \geq c_2 - c_1$. This simply states that the additional expected tax savings from technology 2 relative to technology 1 needs to exceed the additional costs in order for technology 2 to be preferable to technology 1. This expression defines a threshold level of profits $\pi_{12} \equiv (c_2 - c_1)/(\theta_2 - \theta_1)$ above which evasion technology 2 is preferred to evasion technology 1.

Finally assume that $\pi_{10} < \pi_{20}$. Under this assumption, the low-profit firms optimally choose no evasion, the medium-profit firms optimally choose simple evasion and the high-profit firms optimally choose sophisticated evasion:

$$\text{Choice of the firm} \begin{cases} \text{No evasion} & \text{if } \pi \in (\pi_L; \pi_{10}) \\ \text{Evasion technology 1} & \text{if } \pi \in (\pi_{10}; \pi_{12}) \\ \text{Evasion technology 2} & \text{if } \pi \in (\pi_{12}; \pi_H) \end{cases}$$

where π_L and π_H represent the profits of the company with the lowest and highest profit in the distribution respectively. If $\pi_{20} < \pi_{10}$, evasion technology 1 would never be used because, in the entire interval of profits where it dominates no evasion, it would also be dominated by technology 2.

Governments' problem

Government revenue can be stated as,

$$R = \int_{\pi_L}^{\pi_H} t\pi f(\pi)d\pi - \int_{\pi_{10}}^{\pi_{12}} \theta_1 \pi f(\pi)d\pi - \int_{\pi_{12}}^{\pi_H} \theta_2 \pi f(\pi)d\pi$$

The first term represents total tax revenue without any evasion. The second and third term represent expected tax losses from the exploitation of technology 1 and 2 respectively.

The simple evasion technology

Suppose that the government can (costlessly) increase the probability of detecting evasion using technology 1. The effect on government revenue is then given by,

$$\frac{dR}{dp_1} = \int_{\pi_{10}}^{\pi_{12}} s_1(t+\phi)\pi f(\pi)d\pi + \theta_1\pi_{10}f(\pi_{10})\frac{d\pi_{10}}{dp_1} + (\theta_2-\theta_1)\pi_{12}f(\pi_{12})\frac{d\pi_{12}}{dp_1}$$

where

$$\frac{d\pi_{10}}{dp_1} = \frac{\pi_{10}}{\theta_1}s_1(t+\phi) > 0 \qquad \text{and} \qquad \frac{d\pi_{12}}{dp_1} = -\frac{\pi_{12}}{(\theta_2-\theta_1)}s_1(t+\phi) < 0$$

The first term of dR/dp_1 is positive and is the mechanical revenue gain associated with increased detection of firms using evasion technology 1. The second term is the behavioural revenue gain associated with firms moving from evasion technology 1 to no evasion: $\theta_1\pi_{10}$ represents the increase in the tax bill of each of these firms whereas $f(\pi_{10})(d\pi_{10}/dp_1)$ measures their mass. The third term is negative and is the revenue loss associated with firms shifting from evasion technology 1 to evasion technology 2: $(\theta_2-\theta_1)\pi_{12}$ represents the decrease in the tax bill of each of these firms whereas $f(\pi_{12})(d\pi_{12}/dp_1)$ measures their mass.

Under standard assumptions on the welfare function of the government, this leads to the following result,[2]

[2]If for instance the government for some fairness reason places explicit value on limiting the total number of firms evading instead of the total profits escaping taxation then result *b* of the proposition below might not

PROPOSITION 1:

a. Revenue may drop when p_1 is increased if the substitution between technologies is sufficiently strong.
b. If this is the case then welfare must fall because firms are all (weakly) worse off.

Define total evasion as (expected) missing taxes from undetected use of the two technologies,

$$S = \int_{\pi_{10}}^{\pi_{12}} s_1^e t\pi f(\pi)d\pi + \int_{\pi_{12}}^{\pi_H} s_2^e t\pi f(\pi)d\pi$$

where $s_i^e = s_i(1-p_i)$ for $i = 1, 2$. I.e. these are the due tax payments, that ends up not being paid after detection efforts by the government corresponding to p_1 and p_2.

The effect of increasing p_1 on total evasion is then,

$$\frac{dS}{dp_1} = (s_1^e - s_2^e)t\pi_{12}f(\pi_{12})\frac{d\pi_{12}}{dp_1} - s_1^e t\pi_{10}f(\pi_{10})\frac{d\pi_{10}}{dp_1} - \int_{\pi_{10}}^{\pi_{12}} s_1 t\pi f(\pi)d\pi$$

The first term represents the increase in evasion coming from firms moving from technology 1 to technology 2. The second term represents the drop in evasion because some firms move from technology 1 to no evasion. The third term represents the mechanical drop in evasion coming from the increase in the detection rate among those employing technology 1.

Consequently we have the following result,

PROPOSITION 2: *Total evasion may increase when p_1 is increased if the substitution between technologies is sufficiently strong.*

hold.

The sophisticated evasion technology

Note that the effect on revenue from a costless increase in p_2 is given by,

$$\frac{dR}{dp_2} = (\theta_2 - \theta_1)\pi_{12} f(\pi_{12}) \frac{d\pi_{12}}{dp_2} + \int_{\pi_{12}}^{\pi_H} s_2(t+\phi)\pi f(\pi) d\pi \qquad > 0$$

where,

$$\frac{d\pi_{12}}{dp_2} = \frac{\pi_{12}}{\theta_2 - \theta_1} s_2(t+\phi) > 0$$

The first term represents the increase in revenue from firms shifting from technology 2 to technology 1. The second term represents the mechanical increase in revenue from detecting more firms employing technology 2. Since the effect on revenue associated with firms shifting between technologies is here positive, the overall effect on revenue from increased detection efforts on sophisticated evasion is unambiguously positive.

The direction of the effect on total evasion is similarly also unambiguous in this case,

$$\frac{dS}{dp_2} = (s_1^e - s_2^e) t \pi_{12} f(\pi_{12}) \frac{d\pi_{12}}{dp_2} - \int_{\pi_{12}}^{\pi_H} s_2 t \pi f(\pi) d\pi \qquad < 0$$

The first term represents the drop in evasion from firms moving from evasion technology 2 to technology 1, while the second term represents the mechanical drop in evasion because more firms using technology 2 are detected by the authorities.

3 When does increased enforcement reduce revenue?

In proposition 1 and 2 above we find that a (costless) increase in enforcement, directed at the simple evasion technology, can result in less government revenue and more tax evasion if the substitution between technologies is sufficiently strong. Before investigating the robustness of this result to various model assumptions, we first investigate which conditions on the parameters of the model make this scenario likely.

Fixed costs and expected tax savings

First, we look at the fixed costs of the two technologies: c_1 and c_2, as well as the expected tax savings: θ_1 and θ_2. Note that we can rewrite the previous expression for the marginal effect on revenue as

$$\frac{dR}{dp_1} = \underbrace{\int_{\pi_{10}}^{\pi_{12}} s_1(t+\phi)\pi f(\pi) d\pi}_{M} + \underbrace{\pi_{10}^2 f(\pi_{10}) s_1(t+\phi)}_{B_1} - \underbrace{\pi_{12}^2 f(\pi_{12}) s_1(t+\phi)}_{B_2}$$

where we denote the part representing the mechanical effect from increased detection, M, the behavioural effect from firms moving into full compliance, B_1, and the behavioural effect from firms shifting between the two technologies, B_2. It is clear that $\frac{dR}{dp_1}$ is negative if and only if $M + B_1 < B_2$, and this is exactly the case when,

$$\int_{\pi_{10}}^{\pi_{12}} \pi f(\pi) d\pi + f(\pi_{10})\pi_{10}^2 < f(\pi_{12})\pi_{12}^2$$

First, it is directly clear that c_2 increases M by driving up the threshold π_{12} and hence creating a situation where more firms use the simple and less use the sophisticated technology. The cost c_2 has no impact on the behavioural effect B_1 but has an ambiguous effect on B_2 depending on the shape of the density function, f. The parameter θ_2 has the exact opposite effect to the cost c_2 by driving down the threshold π_{12}.

To say more about the importance of c_2 and θ_2 for this inequality we need to make assumptions on the distribution of profits. First, we abstract from the shape of this distribution and look only at the importance of the primitive parameters through other channels. We illustrate this by assuming that profit is uniformly distributed on the interval $[\pi_L; \pi_H]$. This means that the density is constant and equal to $f(\pi) = \frac{1}{\pi_H - \pi_L}$ on the entire interval. The inequality above then becomes,

$$\frac{1}{2}(\pi_H - \pi_L) < \pi_{12} + \pi_{10}$$

We can insert the definitions of π_{10} and π_{12} to get,

$$\frac{1}{2}(\pi_H - \pi_L) < \frac{c_2 - c_1}{\theta_2 - \theta_1} + \frac{c_1}{\theta_1} = \frac{c_2 + c_1\left(\frac{\theta_2}{\theta_1} - 2\right)}{\theta_2 - \theta_1} = g(c_1, c_2, \theta_1, \theta_2)$$

and hence,

$$\frac{dg}{dc_2} > 0 \qquad \text{and} \qquad \frac{dg}{d\theta_2} < 0$$

This essentially means that the inequality is more likely to hold if c_2 is large while θ_2 is small. The fixed cost c_2 drives up the threshold π_{12} and hence the shifting between technologies, when increasing p_1, happens among higher-profit firms. This makes the cost of this shifting effect large and therefore the effect of increasing p_1 on revenue is more likely to be negative in this case. The benefit parameter θ_2 has exactly the opposite effect by driving down the threshold π_{12}.

Consequently, if we disregard all effects going through the shape of the profit-distribution, then a high c_2 and a low θ_2 will unambiguously make the inequality more likely to hold and hence make it more likely that the effect on revenue from increasing enforcement on the simple technology is negative. If we now consider the significance of the distribution, then it is clear that if c_2 becomes so large (or θ_2 so small) that π_{12} goes to the tail of the profit distribution, then $f(\pi_{12})$ will drag down B_2 while M keeps increasing as π_{12} becomes larger.

The fixed cost on the simple technology, c_1, drives up the threshold π_{10} but drags down the threshold π_{12} and thereby unambiguously decreases the mechanical effect M. On the other hand c_1 has an ambiguous effect on both B_1 and B_2 which depends on the shape of the profit distribution. Again the parameter θ_1 has exactly the opposite effect by decreasing π_{10} and increasing π_{12}. The signs of $\frac{dg}{dc_1}$ and $\frac{dg}{d\theta_1}$ are both ambiguous and depends on the relative values of c_1, c_2, θ_1 and θ_2.

Initial enforcement levels

We now consider the significance of the initial enforcement levels for the size and sign of the effect on revenue of a further increase in enforcement. Above we divided the marginal effect on revenue into a mechanical effect M, and two behavioural effects B_1 and B_2. Looking at each of these contributions separately, we first have

$$\frac{dM}{dp_1} = s_1(t+\phi)\pi_{12} f(\pi_{12})\frac{d\pi_{12}}{dp_1} - s_1(t+\phi)\pi_{10} f(\pi_{10})\frac{d\pi_{10}}{dp_1} \qquad < 0$$

i.e. the mechanical effect M is decreasing in p_1 meaning that if you increase p_1 from an initially high level, then the mechanical effect is smaller than if you increase it from an initially lower level. This is because π_{10} is increasing while π_{12} is decreasing in p_1, and hence as p_1 increases the range of the profit distribution containing firms using technology 1 shrinks. In other words, when enforcement is initially high only few firms will use the evasion technology and therefore the effect on revenue from discovering more evading firms will be small when increasing enforcement even further.

We also have,

$$\frac{dB_1}{dp_1} = 2s_1(t+\phi)\pi_{10}f(\pi_{10})\underbrace{\frac{d\pi_{10}}{dp_1}}_{>0} + \pi_{10}^2 s_1(t+\phi)\underbrace{\frac{df(\pi_{10})}{d\pi_{10}}}_{\lesseqgtr 0}\underbrace{\frac{d\pi_{10}}{dp_1}}_{>0} \qquad \gtreqless 0$$

The effect of p_1 on B_1 depends on the shape of the distribution of profits, and since we have made no assumptions on this distribution the sign of dB_1/dp_1 is ambiguous. However, if we assume that we are dealing with a relatively flat distribution, or at least a distribution which is flat around the range of possible values of π_{10}, then $df(\pi_{10})/d\pi_{10} \approx 0$ and dB_1/dp_1 is unambiguously positive. I.e. in this case the behavioural revenue gain from pushing firms into compliance is larger at initially higher levels of enforcement. This is because at higher levels of enforcement π_{10} is placed higher in the distribution and hence when we increase p_1 further, we push higher-profit companies out of avoidance technology 1.

Lastly,

$$\frac{dB_2}{dp_1} = 2s_1(t+\phi)\pi_{12}f(\pi_{12})\underbrace{\frac{d\pi_{12}}{dp_1}}_{<0} + s_1(t+\phi)\pi_{12}^2\underbrace{\frac{df(\pi_{12})}{d\pi_{12}}}_{\lesseqgtr 0}\underbrace{\frac{d\pi_{12}}{dp_1}}_{<0} \qquad \gtreqless 0$$

Again we do not know the shape of f and therefore the sign of dB_2/dp_1 becomes ambiguous. However, if we again assume a flat distribution such that $df(\pi_{12})/d\pi_{12} \approx 0$, then the sign of dB_2/dp_1 becomes unambiguously negative. I.e. in this case the behavioural revenue loss from pushing firms from avoidance technology 1 to technology 2 is smaller when the level of enforcement is initially high. This is because π_{12} is decreasing in p_1 and hence when enforcement is initially high the substitution between technology 1 and 2 happens at a lower level in the profit distribution. In other words, the firms at the margin between the two technologies are smaller when enforcement (p_1) is initially high and therefore the loss in government revenue from this shifting effect is smaller.

Now assume as above that $df(\pi_{12})/d\pi_{12} \approx 0$ and $df(\pi_{10})/d\pi_{10} \approx 0$ such that M and B_2 are decreasing in p_1 while B_1 is increasing in p_1. Note that the first part of dM/dp_1 is exactly equal to half of the first part of dB_2/dp_1 while the second part of dM/dp_1 is exactly equal to half of the first part of dB_1/dp_1. In other words the increase in the gain from B_1 and the decrease in the loss from B_2 more than compensates for the decrease in the gain from M. Hence, if we assume a relatively flat distribution of profits (or an increasing distribution) in the range where these thresholds are feasibly placed, then the government is less likely to get a negative effect on revenue from an increase in enforcement (p_1) if enforcement is already high. Conversely the government is more likely to get a negative revenue effect if increasing enforcement from an initially low level.

When we assume a flat profit distribution around π_{10} and π_{12}, we are essentially ignoring the fact that the mass of firms moving across these thresholds when p_1 changes might depend on where in the distribution the threshold is placed. If the density function has a large slope in the vicinity of one of the thresholds then this is not a good approximation and one should naturally take account of this change in mass.

Now consider the level of enforcement within the sophisticated technology when enforcement is increased within the simple technology. Note that,

$$\frac{dM}{dp_2} = s_1(t+\phi)\pi_{12}f(\pi_{12})\frac{d\pi_{12}}{dp_2} \qquad > 0$$

I.e. the mechanical effect M of increasing p_1 is increasing in p_2. Consequently, if you increase p_1 from a situation where p_2 is relatively high, then the mechanical revenue gain is larger than if you increase p_1 in a situation where p_2 is relatively low. This result emerges because p_2 increases π_{12} and hence it also increases the number of firms employing the simple technology.

Further we have that $dB_1/dp_2 = 0$. Since p_2 does not have an effect on the first threshold π_{10}, the level of p_2 does not matter for the size of the behavioural effect coming from firms shifting out of technology 1 and into no avoidance when p_1 is increased.

Lastly,

$$\frac{dB_2}{dp_2} = 2s_1(t+\phi)\pi_{12}f(\pi_{12})\underbrace{\frac{d\pi_{12}}{dp_2}}_{>0} + s_1(t+\phi)\pi_{12}^2\underbrace{\frac{df(\pi_{12})}{d\pi_{12}}}_{\lesseqgtr 0}\underbrace{\frac{d\pi_{12}}{dp_2}}_{>0} \qquad \gtreqless 0$$

If $df(\pi_{12})/d\pi_{12} \approx 0$ then dB_2/dp_2 is unambiguously positive, meaning that in a situation where p_2 is relatively large the revenue loss from shifting between the two technologies when increasing p_1 will be large compared to a situation where p_2 is smaller. This stems from the fact that π_{12} is increasing in p_2 and hence if p_2 is large then an increase in p_1 will push high-profit firms into technology 2. Further, if $df(\pi_{12})/d\pi_{12} \approx 0$ then $dB_2/dp_2 > dM/dp_2$, and hence in this situation it must hold that the revenue effect from an increase in p_1 is more likely to be small, or even negative, if p_2 is already relatively high.

In conclusion, the primitive parameters of the model matter for the placement of the essential thresholds determining which firms employ each technology. This matters for the effect on revenue from an increase in enforcement because π_{10} determines the size of the firms that shift to compliance, while π_{12} determines the size of the firms that shift between the two technologies as a result of this enforcement-change. Further, the distance between the thresholds dictates the range of firms employing the simple technology and consequently also the mechanical gain from discovering more evasion when increasing enforcement (p_1). However, the total size of these effects essentially also depends on the mass of companies around and between these thresholds. This ultimately depends on the shape of the distribution of profits one is considering.

4 Robustness to model assumptions and simplifications

In the following we investigate the robustness of our main result to model-assumptions made primarily to assure simplicity.

The probability of detection depends on profits hidden

In the basic model above we simply assume that the government fixes a level for the probability of detection which is independent of the intensive margin choice of the firm of how much to evade. However, in real life it might very well be the case that the probability of detection depends positively on the share of profits shielded from taxation. A large firm that pays very little taxes may draw specific attention from the government. If the evasion technology revolves around either over- or understating transactions, it might be easier to misrepresent transactions slightly compared to substantial over- or understatements.

To factor in these considerations we let the probability of detection in the two technologies depend on the fractions s_1 and s_2. Set $p_1 = q_1 s_1$ and $p_2 = q_2 s_2$ i.e. firms that shield a larger fraction of their profits from taxation are more likely to be detected. The parameters determined by the government are now q_1 and q_2.

In this case the expected tax saving from technology i as a fraction of profits is given by,

$$\theta_i = s_i t - q_i s_i^2 (t + \phi)$$

Firms that choose to use technology i will choose s_i to maximize θ_i. Using the first-order condition we get the following expression for the optimal choice of s_i,

$$s_i^* = \frac{t}{2q_i(t + \phi)}$$

Hence naturally we have $\frac{ds_i^*}{dq_i} < 0$, and therefore if $q_1 > q_2$ then $s_1^* < s_2^*$.

We can write revenue on the same form as earlier and completely analogously we now get,

$$\frac{dR}{dq_1} = -\int_{\pi_{10}}^{\pi_{12}} \frac{d\theta_1}{dq_1} \pi f(\pi) d\pi + \theta_1 \pi_{10} f(\pi_{10}) \frac{d\pi_{10}}{dq_1} + (\theta_2 - \theta_1) \pi_{12} f(\pi_{12}) \frac{d\pi_{12}}{dq_1}$$

where,

$$\frac{d\theta_1}{dq_1} = -s_1^* \frac{t}{2q_1} < 0, \qquad \frac{d\pi_{10}}{dq_1} = -\frac{\pi_{10}}{\theta_1} \cdot \frac{d\theta_1}{dq_1} > 0 \quad \text{and} \qquad \frac{d\pi_{12}}{dq_1} = \frac{\pi_{12}}{\theta_2 - \theta_1} \cdot \frac{d\theta_1}{dq_1} < 0$$

Hence, we get the same three effects as before: a positive mechanical effect, a positive effect from firms moving out of evasion and a negative effect from firms shifting between technologies. One difference relative to the basic setup is that $\frac{d\theta_1}{dq_1}$ is now a function of q_1 because the optimal intensive margin choice s_1^* changes with q_1.

Firms can use both technologies simultaneously

In the basic model we impose the restriction that firms can only choose one technology to evade taxation on part of their profits. Whether this is realistic probably depends on the situation and type of evasion one has in mind. However, for completeness we now relax this restriction and allow firms to endogenously decide whether to use one or both technologies. Say again that firms can evade taxes on a fraction s_1 of profits if they use technology 1 and a fraction s_2 if they use technology 2, and that p_1 and p_2 are fixed and set by the government. Assume further that there exists some overall upper limit s on the fraction of profit a firm can hide from taxation without getting detected with certainty. This upper limit could, for instance, be the result of general audits which the government conducts if the level of profit reaches some lower bound relative to the size of the firm. If a firm uses both technologies then it will hide s_2 with technology 2 and $s - s_2$ with technology 1 because $\theta_2 > \theta_1$. We assume here that $s_1 + s_2 > s$. If $s_2 > s$, then no firm will use both technologies and we get exactly the same result as in the basic model above. However, if $s_1, s_2 < s$, then we get a new threshold determining the use of both technologies simultaneously. Define $\theta_{11} = (s-s_2)t-p_1(s-s_2)(t+\phi)$ and analogous to earlier define the threshold $\pi_b \equiv \frac{c_1}{\theta_{11}}$. If $s - s_2$ is close to s_1 then π_b is close to π_{10} and we get,

$$\text{Choice of the firm} \begin{cases} \text{No evasion} & \text{if } \pi \in (\pi_L; \pi_{10}) \\ \text{Evasion technology 1} & \text{if } \pi \in (\pi_{10}; \pi_{20}) \\ \text{Both evasion technologies} & \text{if } \pi \in (\pi_{20}; \pi_H) \end{cases}$$

However, if $(s - s_2)$ is small, meaning that there is not a lot of evasion potential left after investing in technology 2, then $\pi_b > \pi_{12}$, and π_b becomes the threshold determining when it is (weakly) preferred to use both technologies relative to only technology 2. In this case we get,

$$\text{Choice of the firm}\begin{cases}\text{No evasion} & \text{if } \pi\in(\pi_L;\pi_{10})\\ \text{Evasion technology 1} & \text{if } \pi\in(\pi_{10};\pi_{12})\\ \text{Evasion technology 2} & \text{if } \pi\in(\pi_{12};\pi_b)\\ \text{Both evasion technologies} & \text{if } \pi\in(\pi_b;\pi_H)\end{cases}$$

Assume now that $(s-s_2)$ is relatively small such that we are in the last scenario. Define $\theta_{12}=st-p_1(s-s_2)(t+\phi)-p_2s_2(t+\phi)$ i.e. the expected tax savings as a share of profits when using both technologies. Then we can define government revenue as,

$$R=\int_{\pi_L}^{\pi_H}t\pi f(\pi)d\pi-\int_{\pi_{10}}^{\pi_{12}}\theta_1\pi f(\pi)d\pi-\int_{\pi_{12}}^{\pi_b}\theta_2\pi f(\pi)d\pi-\int_{\pi_b}^{\pi_H}\theta_{12}\pi f(\pi)d\pi$$

and we get,

$$\frac{dR}{dp_1}=\int_{\pi_{10}}^{\pi_{12}}s_1(t+\phi)\pi f(\pi)d\pi+\int_{\pi_b}^{\pi_H}(s-s_2)(t+\phi)\pi f(\pi)d\pi+\theta_1\pi_{10}f(\pi_{10})\frac{d\pi_{10}}{dp_1}$$
$$+(\theta_2-\theta_1)\pi_{12}f(\pi_{12})\frac{d\pi_{12}}{dp_1}+(\theta_{12}-\theta_2)\pi_b f(\pi_b)\frac{d\pi_b}{dp_1}$$

where

$$\frac{d\pi_b}{dp_1}=\frac{\pi_b}{\theta_{11}}(s-s_2)(t+\phi)\qquad>0$$

Hence, the first term in dR/dp_1 is the mechanical revenue gain from discovering more technology 1 evasion from the group using only this technology. The second term is the mechanical revenue gain from discovering more technology 1 evasion from the group using both technologies. The third term is the behavioural revenue gain from firms moving from technology 1 to no evasion (because π_{10} increases). The fourth term is the revenue loss from firms shifting from technology 1 to technology 2 (because π_{12} decreases). Lastly, the fifth term is the revenue gain from firms moving from both technologies to only using technology 2 (because π_b increases).

Hence, relative to the basic setup we now get two additional positive terms. We get an additional mechanical revenue gain because more firms in the economy use the simple evasion technology, and further we get an additional positive effect coming from firms shifting from using both technologies to using only technology 2. However, if the shifting between technologies is sufficiently strong revenue and consequently welfare may still drop when p_1 is increased.

5 Conclusion

Using a very simple theoretical framework we show that the presence of multiple heterogeneous tax evasion technologies changes the unambiguous prediction found in previous work that increased enforcement leads to more compliance and more collected revenue. If the substitution between technologies is sufficiently strong increased enforcement on one technology might provoke enough substitution to the other technology to make total evasion increase and tax revenue fall.

This result firstly highlights the importance of considering the system of possible evasion techniques and not a single technology in isolation when planning enforcement efforts. If two technologies are substitutable to a large degree any effective policy probably has to target both techniques simultaneously. Further, it calls for more research on the magnitude of shifting effects between technologies, and a classification of more and less substitutable strategies. Such information could be vital for the design of effective policies to combat tax evasion.

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3 | Enforcement and the importance of tax policy design for elasticities: Evidence from Cypriot reforms

Enforcement and the importance of tax policy design for elasticities: evidence from Cypriot reforms

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Abstract

We study behavioural responses to tax enforcement and the importance of enforcement policy design for behavioural elasticities, using the context of charitable contributions in the Republic of Cyprus. We show three sets of results. First, exploiting salary-dependent thresholds governing the documentation requirements when claiming deductions for contributions, we estimate that reported donations increase by 0.7 pounds when taxpayers can claim 1 pound more without providing documentation. Second, using a reform that retroactively shifted the reporting threshold, we estimate that at least 36 percent of these observed responses are purely due to changes in reporting. Third, we estimate a tax price elasticity of reported donations of −0.5, and show that this parameter is highly sensitive to policy design features such as thresholds determining reporting standards.

Keywords: *Charitable giving* · *Tax enforcement* · *Tax design*

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1 Introduction

Tax enforcement is an essential dimension of any tax system and has therefore been the subject of an extensive literature. The enforcement environment consists of several components such as auditing, self- versus third-party reporting, penalties and documentation requirements. Each element is arguably important, but in recent decades many countries have moved specifically towards a heavy reliance on stricter documentation requirements and third-party reporting to assure compliance. Given the significant role of this policy dimension, assessing the behavioural responses to the reporting environment is important both to understand the magnitude of misreporting and to be able to invest efficiently in enforcement initiatives.

Moreover, the enforcement environment can have a significant effect on how other traditional policy instruments, such as tax subsidies, affect the behaviour of taxpayers. Understanding the size and composition of such behavioural responses is essential for designing optimal policies but also for evaluating the efficiency of a tax system. If elasticities are not structural parameters determined by preferences, but instead parameters controlled by policy choices of the government, such as the level of enforcement, this affects how we should interpret and utilise such parameters. Further, it affects the external validity of estimates across the literature that are dependent on a specific form of tax policy design.

In this paper, we address these important dimensions of a tax system. We focus on charitable contributions in the Republic of Cyprus,[1] which are subsidised through a tax deduction, and have been subject to varying levels of enforcement. The enforcement environment in this context is characterised by a set of thresholds determining a level of claimed deductions for which no documentation is necessary. For claimed deductions above this level, documentation from a third party must be provided.[2] Within this setup we present three sets of results. We start by identifying substantial reactions to changes in the enforcement environment. Exploiting salary-dependent thresholds governing the documentation requirements for claiming

[1]Henceforth, we simply use the term Cyprus to refer to the Republic of Cyprus.

[2]Note that this type of enforcement initiative is very closely related to the standard concept of *third party reporting*. While charities are not required to report directly to the tax authority on behalf of the tax-payer, the authority here still requires documentation from a third party, but collected and filed by the tax-payer himself.

deductions, we show clear discontinuities at exactly these threshold values. Using a regression discontinuity approach, we find that individuals increase reporting by 0.7 pounds when 1 pound more can be claimed without providing documentation from a third party.

Next, we use a reform that retroactively shifted the location of a reporting threshold to separate real from reporting responses. Exploiting the time-profile of responses using bunching techniques, we find that at least 36 percent of responses to such threshold changes are purely changes in reporting behaviour.[3] This separation of reporting and real responses is crucial in a setup where we expect positive externalities from real behaviour, such as expenditures on charitable contributions, investments in education, professional training, retirement savings, etc. In such cases, the goal of the fiscal authority is not only limited to raising tax revenue, but also to encourage real responses through tax incentives.

In the final section, we turn to estimating the elasticity of charitable contributions with respect to the price of giving. Using quasi-experimental variation in tax prices generated by reforms to the income tax schedule, we find a price elasticity of about -0.5. We then show that the presence of reporting thresholds has a very significant effect on our estimates. Price elasticities double if we disregard individuals who bunch around these thresholds. Further, we estimate elasticities across different sub-periods with varying enforcement levels, and show suggestive evidence that this parameter is sensitive to even minor changes in the strictness of the enforcement environment. Our findings show that elasticities are sensitive to a design feature which is relatively common to many tax systems, reporting thresholds, because people who bunch around these thresholds are very insensitive to price changes.

Our paper contributes to several strands of literature. First, we contribute to a growing literature measuring behavioural responses to enforcement initiatives. Using our unique setup, we can show clear discontinuities around thresholds in the reporting environment. While responses to enforcement initiatives have been documented earlier (Kleven et al. 2011, Phillips 2014, Alm et al. 2009, Skov & Gillitzer 2018)[4] we do not have to rely on comparisons between

[3]This wedge can be driven by over-reporting due to the discontinuity in detection probabilities, or under-reporting due to discontinuity in the filing hassle cost such thresholds generate.

[4]Skov & Gillitzer (2018) consider a similar tax design to us, but examine the effect of moving from no third-party reporting (unlimited reporting) to automatic third-party reporting. Our variation does not change the self-reporting feature of the system, but affects the requirement for receipts and hence examines pure

different types of income or potentially selected audit data. Further, we are able to exploit a unique reform which retroactively changed the reporting environment on earlier years' contributions, to credibly show that a large fraction of responses is pure reporting. Carrillo et al. (2017) also disentangle pure reporting responses by analysing a policy intervention concerning the reporting environment of firms in Ecuador. However, the case of firm profit is very different from our setup, because firms can offset any change in income with changes in costs. We consider deductions in the personal income tax schedule, and hence study a very different tax environment.

Second, we contribute to a small and emerging empirical literature on the importance of tax system design features for estimated elasticities. The concept that features of the tax system, such as the size of the tax base, matters for the size of behavioural elasticities has been addressed theoretically by Slemrod (1994) and Slemrod & Kopczuk (2002). Kopczuk (2005) shows empirically that the elasticity of reported income with respect to tax rates depends on the level of deductions in the tax system. Mishra et al. (2008) look at the effect of tariffs on evasion of customs duties and find evidence that this elasticity is affected by characteristics correlated with enforcement possibilities. Fack & Landais (2016) show that both the elasticity of reported income with respect to the tax rate and the elasticity of reported charitable contributions with respect to price are sensitive to the level of enforcement.[5] Much in line with Fack & Landais (2016) we show suggestive evidence that changes in the strictness of the enforcement environment affect estimated elasticities. However, we show such evidence in a situation where only minor changes are made to the reporting environment, where Fack & Landais (2016) use a large reform severely changing reporting standards. These results hence suggest that elasticities might be very sensitive to even small changes in enforcement. Further we include a new dimension of tax system design in this literature, by showing that the existence of reporting thresholds in the tax system directly affects elasticities. This tax design feature is not uncommon - both in the case of charitable contributions but also in other dimensions of tax systems. In the United Kingdom such a threshold exists for on-the-

enforcement variation.

[5] A related paper in this context is Doerrenberg et al. (2017) which documents responsiveness of total deductions to tax changes in Germany. Further they show a significant difference in the elasticity of taxable income and the elasticity of gross income.

job mileage expenses, Germany has a reporting threshold on work-related expenses and the US has a threshold for charitable contributions.

Lastly, we contribute to a literature estimating the elasticity of charitable contributions with respect to the price of giving. This literature has been very focused on the US case and recently also a few other developed countries (Almunia et al. 2017, Fack & Landais 2010, Duquette 2016).[6] Given the importance of tax system design for elasticities, these reported elasticities might not be very informative for less developed economies or economies generally characterised by more informality or less enforcement. We contribute to this literature by providing estimates for a semi-formal economy with tax rules concerning charitable contributions which are representative of other semi-formal economies.

The rest of the paper is structured as follows: Section 2 describes the institutional environment and the data we use in the empirical analysis. Section 3 goes through our results from the regression discontinuity analysis investigating behavioural responses to enforcement. In section 4 we use bunching techniques to separate real and reporting responses. Section 5 presents our results on the tax price elasticity of charitable contributions, and Section 6 concludes.

2 Institutional Context and Data

In this section, we describe the institutional context of charitable giving in Cyprus, the associated reforms we exploit, and the administrative dataset we use to implement our identification strategies.

2.1 Institutional Details

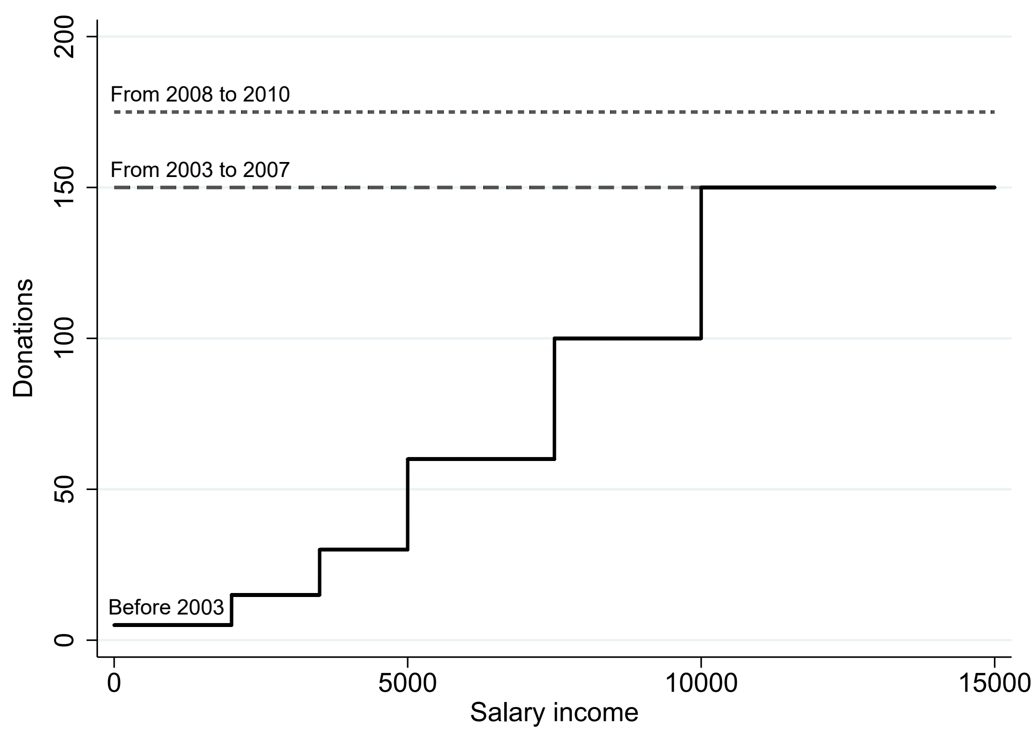
As in most countries, charitable giving in Cyprus is subsidised through tax incentives. Specifically, the amount donated is deducted from taxable income, reducing the effective price of giving to $(1 - \tau)$, where τ is the marginal tax rate. Due to feasibility constraints, there is no automatic third-party reporting by charities to the tax authority. Instead, tax filers are

[6] This is a non-exhaustive list of contributions to this extensive literature. Other contributions for instance include Bakija & Heim (2011), Auten et al. (2002), Karlan & List (2007) and Adena (2014).

required to provide receipts of donations if claimed deductions exceed some specified level. To reduce both hassle and administrative costs, a threshold has been set, up to which no receipts are necessary. For any amounts claimed beyond this threshold, receipts must be provided. We exploit several sources of exogenous variation in filing thresholds and marginal income tax rates, which allow us to examine how contributions respond to both the filing environment and to the price of giving. We first explain the reforms associated with the filing environment. The filing threshold schedule, determining at which donation level receipts are necessary, features several discontinuities and reforms between 1999-2010. This is illustrated in Figure 1. Prior to 2003 the maximum amount one could declare without providing receipts was a

Figure 1:

Reporting thresholds over the sample period



Notes: The graph illustrates the thresholds up to which people could claim deductions for charitable contributions without providing receipts. In the years 1999-2002 this threshold was dependent on salary income with 5 different notches in the schedule. From 2003 the threshold became independent of income and was set at CYP 150. In 2008 this threshold was changed again to CYP 175.

function of salary income. For salary earnings above CYP 10,000, this threshold was CYP 150, for earnings between CYP 7,500-10,000 it was CYP 100, etc. These salary-dependent cutoffs, introduced in 1989, were abolished by the Regulatory Administrative Act No. 823 of 2003. No new law or regulatory act set any new threshold; rather, the tax authority created a de-facto threshold at CYP 150 for everyone by clearly stating the following on the 2003 tax form: "*For donations above £150 please attach receipts"* (shown in Figure 6b). This is the first tax form that denotes a specific threshold; up to 2002 the tax form simply stated: "*attach relevant receipts*" (Figure 6a). The 2003 wording was kept the same up to 2007.

This threshold was changed again when Cyprus switched currency and adopted the Euro. The Euro was phased in during 2008, and the tax return for the 2008 fiscal year (which coincides with the calendar year) had to be filed in Euros. The tax return now stated *"attach receipts only for donations above €300 euros"* (Figure 6c). Given the locked exchange rate[7] of CYP0.585274 = €1, this was equivalent to CYP 175. Tax returns are published after the end of the fiscal year and need to be submitted by the end of April. Therefore, this new threshold was published *after* the end of the 2008 fiscal year, precluding any real responses in contributions during 2008.

Besides threshold discontinuities and reforms, we are also able to exploit exogenous variation in the tax price of giving generated by marginal tax rate reforms. Figure 7 shows the income tax schedule in Cyprus between 1999-2010, where marginal tax rates were changed six times in total, affecting all parts of the income distribution.

Our empirical strategy draws on three sources of variation generated by the institutional setting. We start by focusing on the pre-2003 salary-based discontinuities in the filing threshold to establish that donations respond strongly to the filing environment. We then exploit the unique timing of the 2008 reform of this threshold to set bounds on the real and pure reporting components of the response. Lastly we draw on the variation in marginal tax rates to estimate the tax price elasticity of donations and examine whether this is sensitive to the filing environment.

[7]This became legally binding by the Regulatory Administrative Act No. 311/2007.

2.2 Data

The data come from first-time access to the administrative records of the Tax Department of the Republic of Cyprus. It covers the universe of tax filers between 1999-2010, and includes information from the main fields of the I.R. 1A tax return, as well as basic demographic and firm-related characteristics. All employees are required to file taxes, unless they earn a gross amount below some tax free level. The self-employed are required to file regardless of the amount earned. Besides individuals with earnings from labour, the dataset also includes pensioners and individuals out of the labour force who may be filing because it is a requirement for accessing government welfare programmes.

To create our working dataset, we impose the following restrictions. First, we consider only individuals with a single employer, who report at least some positive salary income and are aged between 25-54.[8] Second, we drop individuals in the top 0.1% of donations. Our working dataset contains about 1.5 million observations and 225,000 unique individuals. Table 6 shows summary statistics for our sample.

It is important to note that due to the way the tax administration provides the tax data, our variable measuring donations also includes trade union subscription fees, which is another tax deduction that appears on the same section of the tax return. Prior to 2003, it also includes a so-called "professional" tax. This is a lump-sum tax that was a step-function of earnings (Figure 8 shows the exact schedule). We deal with the professional tax by simply removing the amount from our variable, as we know the exact amount individuals had to pay based on their salaries.

The only remaining issue is that of trade union membership fees. This does not affect our first two empirical strategies, because we use variation where adding random noise to the measure should not affect any results. For instance, in the case of the regression discontinuity design this should only shift the level on both sides of the cut-off, but not affect the size of the discontinuity.[9] Similarly, for our bunching estimates fees should affect both the level of

[8]As is explained below, we need to know workers' sector and salary to determine their potential union membership fees. In our data, we can observe individual salaries, but not salaries per employer. We therefore drop the 3% of our sample having more than one job, to ensure we can do this accurately.

[9]Importantly, union fee rates are fixed across all salary ranges and did not change following any tax or threshold reforms.

bunching and the counterfactual similarly leaving the bunching estimate unchanged. Further, we are not interested in the level of bunching per se, but in the changes in bunching across years. However for the last part of the analysis, where we consider elasticities, these union fees play a role and we need to correct for them. Since we don't directly observe trade union membership in our data, we tackle this issue by using detailed sectoral information available in our dataset, which we combine with information on union fee rates we have collected directly from trade unions and from the Ministry of Labour and Social Insurance. Union fees are a fixed proportion of salaries, deducted every month from employers through the PAYE system. We use the information on union rates, salaries and sectors to residualise our donation measure from union fees in highly (or fully) unionised sectors where we can be sure we are correctly accounting for them.

To our advantage, union membership in Cyprus, which has about 50% coverage in our study period (Ioannou and Sonan 2014) is highly concentrated in just a few sectors: commercial banking, the public sector, hotel services and construction. Due to industry-wide agreements and automatic enrolment upon employment, both the public sector and commercial banking have nearly 100% coverage (Ioannou and Sonan 2014). They are also relatively large sectors, and together make up about 43% of our estimation sample (7% banks, 36% public sector). In comparison, hotel and construction have unionisation rates of about 75%, and make up 3% and 7% of our sample respectively. Besides these, union membership is very low for the remaining sectors. Our raw donation measure will therefore only be significantly affected for workers in the highly unionised sectors, for which we can accurately correct.[10] For each of our empirical strategies, we run a battery of robustness checks to show that our results are not affected by the way we deal with union fees.

[10]In the few cases where we cannot, we simply drop them from our analysis. For instance, we exclude the public sector whenever we run specifications using our adjusted measure because our data does not distinguish between different types of workers in the public sector which can be subject to different union fee rates within the sector.

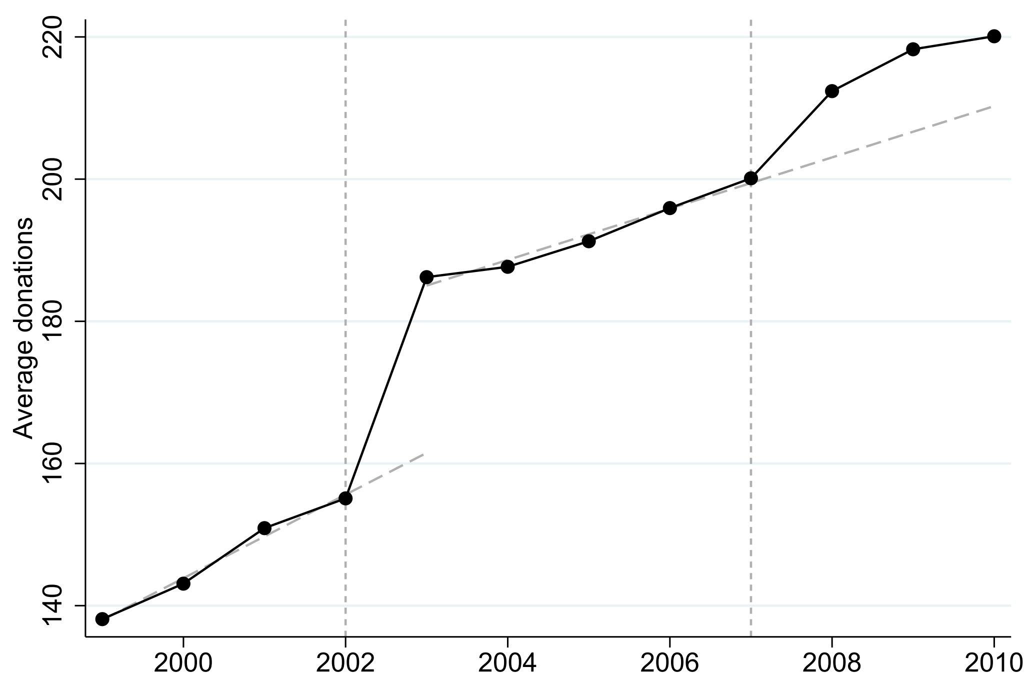
3 Behavioural responses to the enforcement environment

As explained above reforms changing the level of the reporting thresholds was implemented in 2003 and 2008. In both years thresholds were exclusively increased. In Figure 2 we clearly see the effect of these reforms in the evolution of average donations over time. The figure shows average donations among donors across the sample period. Aside from a clear increasing time trend we see jumps exactly in the two reform years. This initial time-profile of donations suggests that these reforms, which uniformly relaxed the enforcement environment, caused a substantial increase in reported donations in Cyprus.

In this section, we look further into behavioural responses to enforcement by exploiting the

Figure 2:

Yearly average donations among donors



Notes: The graph shows the yearly average of donations using only observations where donations are positive. We remove the top 0.01% of donations within each year. The sample includes all tax filers in the age range 25-54, with some positive salary income and only one job within a given year.

salary-based discontinuities in the amount of donations tax filers can report without receipts.

3.1 Regression Discontinuity estimate

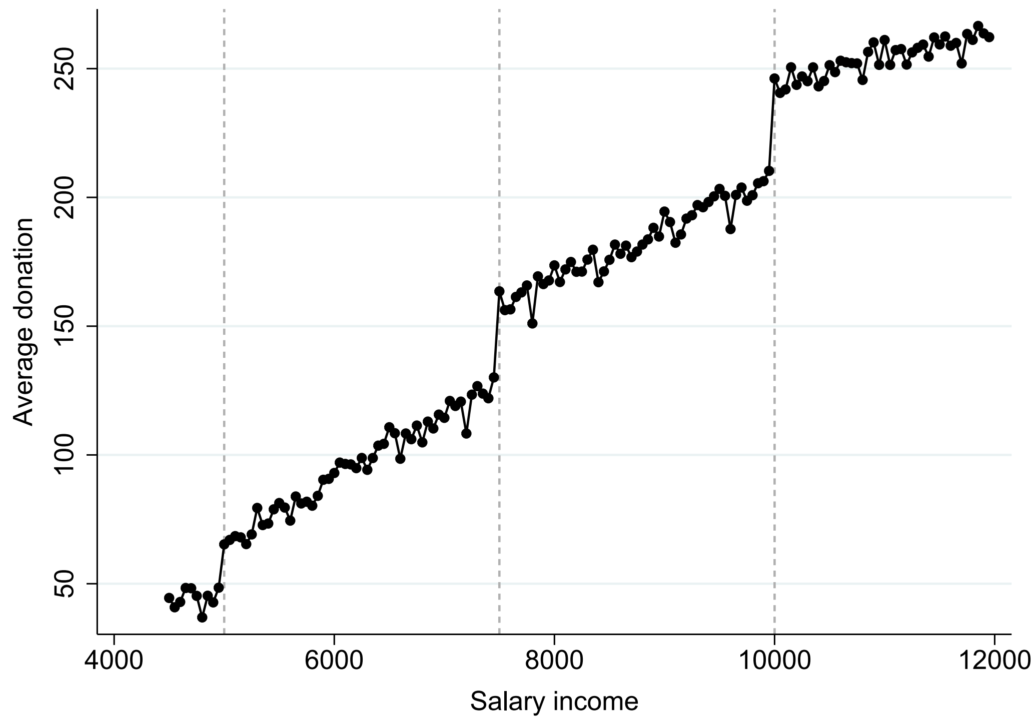
As shown in Figure 1, before 2003 the reporting threshold was a function of gross salaries. This setup lends itself to a regression discontinuity design. For our main RD estimates, we use years 1999-2001 and restrict our sample to those with only salary income. We exclude 2002 because a reform in that year shifted the first income tax threshold to CYP 9,000, meaning that individuals with salaries below this level cease to be a reliable sample as they had no obligation to file a tax return and no tax incentive to claim deductions. We also exclude individuals with non-salary income because the threshold we want to exploit is a function of salary income, and we want to preserve the income trend in donations.[11] We focus on two discontinuities: the jump from CYP 100 to CYP 150 at the CYP 10,000 salary threshold, and the jump from CYP 60 to CYP 100 at the CYP 7,500 salary threshold. We do not consider lower thresholds because they are located at income levels where individuals have no tax filing obligation.

To motivate our approach, Figure 3 plots the average donation by salary bins of width 50 between 1999-2001. As is clearly seen, donations jumps at exactly the income thresholds associated with different reporting standards, but otherwise evolves smoothly. Note that our measure here also includes professional taxes and union fees. We do not remove these, since neither involve any discontinuities at our thresholds of interest. This is seen in Figure 8, which shows that the professional tax indeed evolves smoothly across these thresholds. Likewise, union fees are always set at fixed percentage of salary, and hence do not jump at different income levels.

Our aim is to estimate the jumps in reported donations using an RDD, treating individual salaries s_i as our assignment variable. Before doing so, we check that our identification strategy is valid. The identifying assumption is that there is no precise manipulation of the assignment variable, i.e. workers cannot precisely choose their salaries in order to manipulate the different thresholds. For instance, if workers just to the right of a threshold strategically

[11] For robustness, we also run our main specification including 2002, and including individuals with non-salary income, and find very similar results.

Figure 3: **Average donations by income 1999-2001**



Notes: The graph shows average donations by income pooled in the years 1999-2001. We use income bins of 50 including the left-hand value. We include people with only salary income and only one job within a single year and remove people with an income at an exact round number (multiples of 500).

placed themselves there in order to be able to report more, then workers with salaries just below the threshold would not provide a good counterfactual. The possibility that workers specifically search for wage-hours packages in order to respond to the thresholds associated with charitable giving is however very unlikely. More importantly, even if this scenario were true, it is highly unlikely that they would be successful in doing so *precisely*. There are significant labour market frictions associated with searching for wage-hours packages. Indeed, a public finance literature on taxable income bunching (Chetty et al. 2011; Kleven and Waseem 2013; Gelber et al. 2017) and work hours constraints (Dickens and Lundberg 1993; Blundell et al. 2008) shows that there are significant frictions associated with precisely choosing earnings. We check for evidence of manipulation by examining the density of salaries, as shown in Figure 9. A sign of manipulation would be significant amounts of bunching or sorting around the donation-related salary thresholds, but not elsewhere. In particular, we would expect individuals to sort just to the right of these cutoffs, in order to take advantage of the higher reporting thresholds. As we can see, this is not the case. Figure 9a shows some bunching at our thresholds, but there is also (much larger) bunching at many other levels. Specifically, this comes from round-number bunching at *all* multiples of CYP 500, which is characteristic of the fact that salaries have a high propensity to be set at round numbers. This is also confirmed by Figure 9b, which shows the density of salaries when we drop individuals with a salary that is an *exact* multiple of CYP 500, i.e. $s_i \bmod 500 = 0$. In this case, the bunching at our thresholds disappears, as it does at all other round numbers. We also use a McCrary test to formally test for the existence of any significant discontinuities in the density around each cutoff, the results of which are also reported in the figure. In line with visual evidence, the null of no discontinuity cannot be rejected, supporting our identifying assumption of no precise manipulation of s_i.

We proceed by dropping the rounders from our estimation sample, but show as a robustness check that their inclusion makes no difference. To estimate the jumps, we treat individual salary s_i as our assignment variable, and run regressions of the form:

$$Y_i = \alpha_0 + \alpha_1 Treated_i + f(s_i, \beta) + Treated_i \times f(s_i, \gamma) + X_i'\delta + \epsilon_i \tag{1}$$

where we define, for each threshold $T \in \{7500, 10000\}$ separately, $Treated_i = \mathbb{1}\{s_i \geq T\}$. $f(s_i, ;)$ is a polynomial function with parameter vector β that controls for the salary trend and γ that controls for the interaction between the salary trend and treatment status. α_1 measures the jump in average donations due to the change in the reporting threshold. Some specifications also include a vector of controls X (sex, year and sector fixed effects). Lastly, we only consider bandwidths of up to 2000 to ensure that no estimation sample includes more than one threshold.

Table 1 shows our results split into two panels: A for the CYP 10,000 threshold and B for the CYP 7,500 threshold. To check robustness of our estimates, we present results from eight different specifications of (1): with a first and second order polynomial of the assignment variable,[12] with and without controls, and with different bandwidths. Each panel reports two estimates: (a) the size of the discontinuity, and (b) the implied takeup, which scales our estimate by the size of the notch in the donation schedule.

Starting with the 10,000 cutoff, column (1) of Panel A shows that the effect of increasing

Table 1:

Regression discontinuity estimates at notches in donation schedule

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above 10k	36.64***	33.80***	34.40***	32.51***	35.08***	35.12***	33.79***	34.15***
	(0.95)	(1.41)	(0.97)	(1.45)	(1.33)	(2.04)	(1.38)	(2.12)
Implied takeup	0.73	0.68	0.69	0.65	0.70	0.70	0.69	0.69
Observations	84 255	84 255	68 697	68 697	43 095	43 095	35 404	35 404
R^2	0.21	0.21	0.28	0.28	0.13	0.13	0.20	0.20
Panel B:								
Above 7.5k	29.64***	31.77***	30.48***	30.91***	30.78***	26.50***	30.21***	26.30***
	(0.74)	(1.08)	(0.79)	(1.14)	(1.01)	(1.51)	(1.06)	(1.58)
Implied takeup	0.74	0.79	0.76	0.77	0.77	0.66	0.76	0.66
Observations	92 433	92 433	71 095	71 095	51 850	51 850	40 123	40 123
R^2	0.29	0.29	0.37	0.37	0.17	0.18	0.27	0.27
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows the results from estimating specification (1) on our main sample pooled over 1999-2001. The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

[12] As is clear from Figure 3 a linear specification allowing for different slopes on each side of each threshold should be sufficient. For robustness we also report results using second-order polynomials. We have tried running specifications with higher order polynomials, and this does not change any results.

the threshold from CYP 100 to CYP 150 leads to a CYP 36.64 increase in donations, with this effect estimated with very high precision. This estimate implies a takeup of 73%, i.e. that workers increase their donations by 0.73 for every unit increase in the amount of donations that can be reported without providing receipts. These results are highly robust to the choice of polynomial order, inclusion of controls, and bandwidth. As columns (2) - (8) show, the estimated effect is on average CYP 34.5 and the implied takeup is hence about 70%. Very similar results are found when we consider the second cutoff at CYP 7,500 (panel B). The increase in donations is estimated at about CYP 30. This is lower than the effect estimated at the higher cutoff, which is expected given that the discontinuity in reporting threshold is also smaller in magnitude. When we scale the effect by the size of the notch, we find a very similar implied takeup, estimated at about 74%, which suggests that the behavioural responses are highly comparable across the two cutoffs. Again, estimates are extremely robust to the alternative specifications across (1)-(8).

We next present robustness tests of our selected estimation sample. First, we include individuals with round-number salaries. Second, we include the year 2002. Third, we include individuals who also have non-salary income (conditional on having some salary income). Fourth, we use our variable cleaned from professional taxes and union fees.[13] Lastly, we run separate regressions for individuals working in highly and not highly unionised sectors. The results are shown in Appendix Tables 7-13. Our estimates are extremely robust to every variation we consider.

As a last robustness test we also check for any discontinuities in covariates around our thresholds. Non-smoothness in covariates could suggest non-smoothness in the distribution of unobserved heterogeneity, thereby casting doubt on the validity of our method. We consider four covariates: age, the probability of being female, the probability of working in a highly unionised sector, and the level of other deductions. We also check the first and last price of giving, where these are defined as the prices before and after donations respectively.[14] Figure 10 shows plots of each of these cases as a function of our assignment variable. All plots confirm

[13]In this case, we drop the public sector from Table 10 because we cannot correct the trade union payments with high precision.

[14]These definitions are explained in detail in Section 5.

smoothness around our two cutoffs.

Finally we investigate heterogeneity in these responses by sex and age. Table 14 shows results for males, and Table 15 for females. We do not find any notable heterogeneity around the 10,000 threshold, but do find a somewhat larger response around the 7,500 threshold among females. The implied takeup for females is around 0.82, compared to 0.66 among males.[15] Table 16 shows results for taxpayers aged 25-39 and Table 17 those aged 40-54. Younger taxpayers seem to respond slightly more to the lower threshold, but slightly less to the higher threshold, compared to older taxpayers. The implied takeup rate among the younger group is about 0.62 at 10,000 and 0.77 at the 7,500 threshold. Among the older group, it is 0.75 and 0.68 respectively. Overall, these results do not reveal substantial heterogeneity, and importantly all groups seem to exhibit large responses independent of specification or threshold choice. Even the lowest implied takeups are quite substantial, confirming the large impact these reporting thresholds have on taxpayer behaviour.

4 Separating real and reporting responses

Having established that reported donations respond strongly to the filing environment, we now focus on separating real and reporting responses by exploiting the timing of the 2008 reform. As explained, the threshold up to which no receipts were necessary was moved from CYP 150 to CYP 175 (300 Euros) in 2008, but this change was only announced after the end of the 2008 fiscal year. Therefore, any response to the new threshold for 2008 can only be a change in reporting behaviour.

Our strategy exploits changes in bunching patterns, generated by the discontinuities associated with the filing threshold, around the 2008 reform. To do this, we implement standard bunching techniques (Saez 2010; Chetty et al. 2011; Kleven 2016) and estimate the excess mass of individuals located at each threshold between 2003-2010. We group donations in bins of width 5 and fit an 11th order polynomial to estimate the counterfactual mass of filers in the absence of these thresholds. The difference between the actual and counterfactual count is therefore the excess mass ascribed to the discontinuities in filing requirements. To assure comparability of bunching across different thresholds, we estimate the normalised excess mass

[15] These are the averages of the implied takeups across specifications.

b by scaling the excess mass by the height of the counterfactual. In our estimation, we also control for round number bunching in multiples of 50 and 100 (thereby flexibly allowing for different levels of roundedness for each).[16]

Our bunching results then allow us to indirectly estimate what proportion of observed responses to raising such thresholds are pure reporting effects by comparing the bunching in 2008 at the CYP 175 new threshold, which can only be driven by a pure reporting response, to the total bunching in 2007 at the CYP 150 threshold:

$$\mathrm{L}_R = \frac{b_{175}^{2008}}{b_{150}^{2007}} \tag{2}$$

L_R provides a lower bound, since bunching and responses in subsequent years can include both a real and a reporting dimension. In words, L_R reports the fraction of the excess mass at the previous threshold that moves to the new threshold before real responses are feasible.

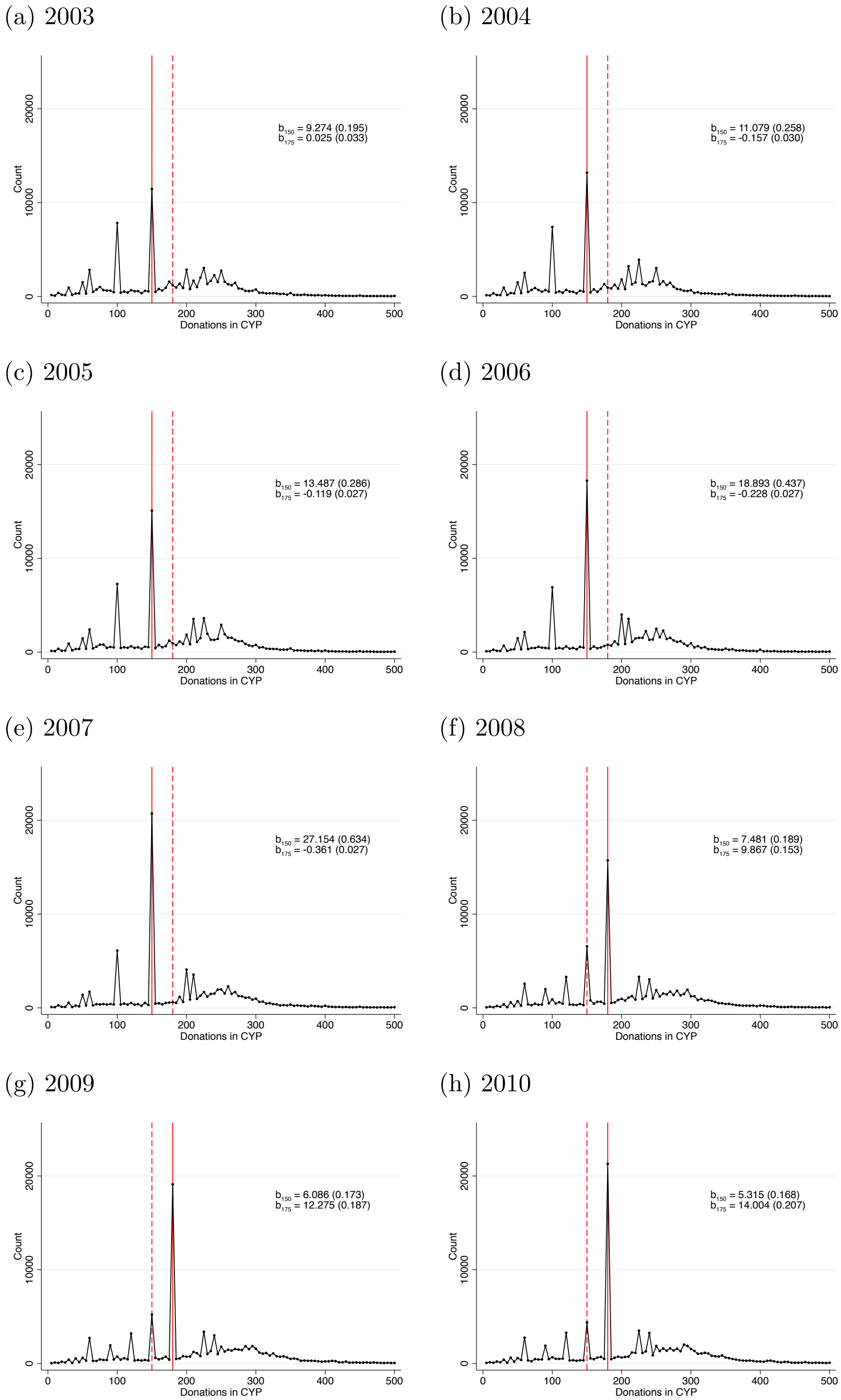
4.1 Bunching Results

We start by focusing on our main sample, defined in the same way as in the RD section. We do not remove union fees because we want to preserve the raw patterns in the data. This of course means that we are identifying our effects from the non-unionised sample. We show in the next section that our results are highly robust to accounting for union fees.

Figure 4 shows the empirical density of reported donations between 2003-2010 for our main sample. Each sub-figure plots the density in bins of CYP 5, and reports the normalised excess mass at each threshold, b_{150} and b_{175}, with bootsrapped standard errors shown in parentheses. To highlight how the bunching moves across the two thresholds, each sub-figure also demarcates the threshold in place in a given year by a solid vertical line, and the other threshold by a dashed vertical line. We do not include the estimated counterfactuals here to avoid cluttering, but show these separately for each threshold in Appendix Figures 11 and 12.

[16]For years 2008-2010, we have converted the currency from Euros to CYP using the official exchange rate. In this case, we control for round numbers by using the CYP converted amounts of the round numbers in Euros, since that was the actual currency used to file the tax return.

Figure 4: **Bunching around reporting thresholds**



Notes: This figure shows the bunching dynamics of donations among salary earners between 2003-2010, by plotting the yearly empirical distributions in bins of width CYP 5. Each sub-figure reports the normalised excess bunching mass b around the CYP 150 and CYP 175 thresholds, with bootstrapped standard errors in parentheses. Vertical solid lines mark the relevant threshold that is in place in a given year (CYP 150 during 2003-2007 and CYP 175 during 2008-2010), while dashed lines mark the other threshold that has either been eliminated, or has not been yet introduced.

We find that bunching at the CYP 150 threshold is very large in magnitude and sharp (i.e. there is no diffuse bunching around the threshold). The normalised excess mass steadily increases between 2003 and 2007, starting from a level of 9.3 and peaking at 27.2 in the last year this threshold is effective. By 2007 therefore, there are 27 times as many individuals at CYP 150 than what there would be absent the filing discontinuity. At the same time, there is no excess bunching at CYP 175 throughout this period (marked with a dashed vertical line). These patterns are followed by a dramatic change in 2008. The bunching at CYP 150 stops growing and instead exhibits a large drop to 7.5, and continues decreasing thereafter. Bunching at CYP 175 now appears, producing a normalised excess mass of 9.9 in 2008. In a symmetrically opposite way to the bunching at CYP 150, bunching at the new threshold exhibits further growth in years 2009-2010. What is striking is that while there is no bunching in any year before 2007 at CYP 175, a very large spike appears in 2008, even though there was no knowledge of this new threshold, and hence no real response possible during the 2008 fiscal year.

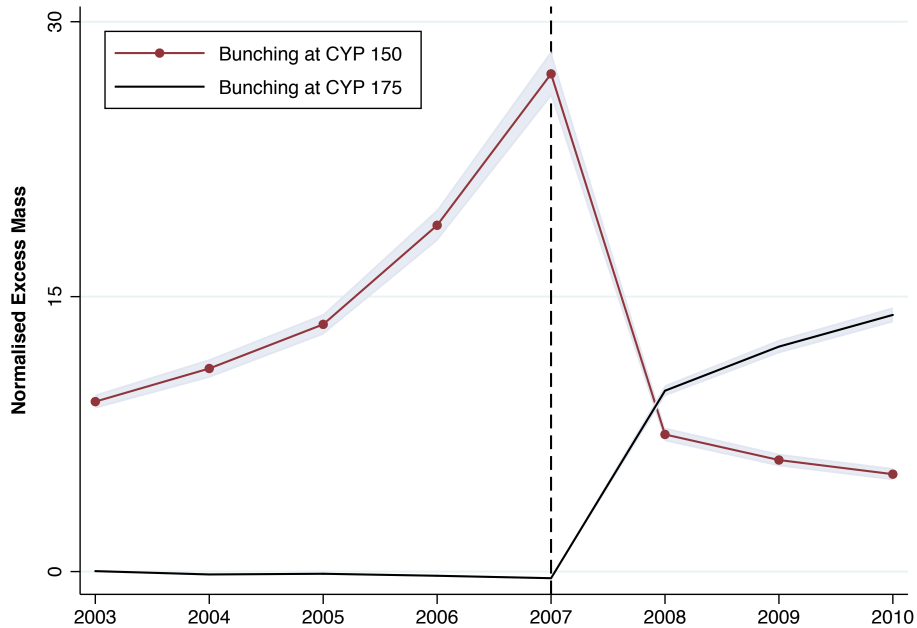
To visualise these patterns Figure 5 plots our bunching estimates at each threshold across time, with the shaded areas demarcating our 95% confidence intervals. The bunching dynamics suggests learning, as it seems to take time for individuals to understand the incentives created by the thresholds and respond to them over the years.

Given our estimates, we find that the lower bound of the reporting response is $L_R = 36\%$. This implies that at least 36% of the response to an increase in the reporting threshold is due to changes in reporting, rather than real changes in contributions. To the extent that individuals take time to learn about, and understand, the changes in the reporting environment, the responses in 2009 and 2010 may also capture reporting responses, which would increase our estimate.

Next, we exploit the panel dimension of our data to check whether the patterns we observe is indeed driven by individuals moving from the old threshold to the new one. In Appendix Figure 13, we plot the empirical density of reported donations between 2003-2010, for the sample bunching at CYP 150 in 2007. We find that the overwhelming majority of the 2007 bunchers are "repeat" bunchers, locating at CYP150 for up to 4 years earlier. They also overwhelmingly shift to the new threshold in 2008, while some take a further one or two years

Figure 5:

Bunching Estimates over Time



Notes: This figure shows for our main sample of salary earners, the estimates of the normalised excess mass around both the CYP 150 and 175 thresholds, between 2008-2010. The shaded areas demarcate 95% confidence intervals.

to complete the shift. Appendix Figure 14 repeats the analysis for those bunching at the new threshold in 2008. Again, we find the same patterns; these are individuals who were previously bunching at the old threshold for up to 5 years before, with the shift being nearly complete by 2010.

Figure 16 plots the fraction of individuals with reported donations at (1) the 150 threshold, (2) the 175 threshold, (3) above 150 and (4) above 175 throughout the entire sample period. The trends in the fraction of individuals filing 150 and 175 is in line with our previous results. The fraction of individuals at 150 is increasing from 2003 and peaks at 2007, before dropping sharply in 2008. Conversely, there are very few at 175 until 2008, when it increases sharply. What is more interesting is the trend in the proportion of people filing more than 150 and more than 175. While they exhibit parallel trends up to 2008, there is a sharp increase in the fraction of individuals filing above 150, but no change in the fraction of those filing above 175.

This confirms that the movement is purely between these two thresholds, and emphasises how important the filing environment is for taxpayer behaviour.

4.2 Robustness Analysis

We next discuss the robustness of these results and conduct a battery of checks on our main results. First, it is very important for the validity of the lower bound that the 2008 threshold change was not anticipated or somehow made public before the end of the fiscal year. Before the introduction of the Euro there was a large government campaign informing citizens that during the transition they should simply use the official locked exchange rate to convert prices, salaries etc. Following this, filers should have expected the threshold to remain unchanged at a converted value of €250, not €300. Further, tax returns are not published before the end of the fiscal year. Even if it was published early through unofficial channels, it is highly unlikely that filers would be so keen to obtain their tax return before the end of the fiscal year that they would search for it. The fiscal year ends four months in advance of the tax return submission deadline and looking at the data on tax return submission dates we see that the vast majority of tax filers procrastinate, and submit their return just before the deadline of April 30th of the following year (see Figure 15). Most file in the last week of the deadline. This behaviour is clearly at odds with active tax return search and filing behaviour.

Second, we consider whether our bunching patterns could be be affected by the existence of union fees. This would be implied by two, extremely unlikely, scenarios. The first is that we are picking up bunching at thresholds that is driven purely by fees which coincidentally place individuals at the thresholds. The second is that we are picking up the sum of union fees and donations, which again happen to consistently sum to these thresholds. Both are implausible. The first case would require that the salaries of such bunchers were at a level prior to 2008 that, when the fixed % of salary paid as union fees was applied, would place them at the filing threshold. At the same time, their salary in 2008 would also have to grow by a rate exactly large enough to move them to the new threshold (a 16.67% increase). Thereafter, they would again have to revert back to a zero growth rate in order to stay at the new threshold. We can in fact check this. Figure 17 plots the salary growth rate of the 2007 bunchers and shows that this is not the case;[17] salary growth rates are far from what this extreme scenario would imply. The second case would require a very high level of sophistication and meticulous tax planning. Specifically, individuals would have to predict their exact yearly salary and union fees, and dynamically adjust their donations to crowd-out union fees one-for-one, so that these would always sum to the exact threshold by the end of each fiscal year. Not only is this implausible, but importantly taxpayers have no financial incentive to engage in such a form of planning in the first place.

Nevertheless, we conduct robustness checks where we repeat all of our previous analysis, but restrict the sample to only workers not in highly unionised sectors. In this case, the contribution of any union fees will be minimal because the proportion of unionised members in this sample will also be limited. Our full set of results is presented in Appendix Figures 18 - 21. The bunching patterns are nearly identical to our main findings, both in terms of magnitudes and dynamics. As shown in Figure 21, bunching at the CYP 150 threshold grows steadily from 13.6 in 2003 to 31.2 by 2007, falls sharply in 2008 to 7.5 and keeps decreasing thereafter. In the exact same way as before, a normalised bunching mass of 12.1 first appears at the new threshold in 2008, and grows thereafter. The lower bound estimate of the reporting response is now 39%, which is extremely close to our previous estimate (of 36%). One difference worth mentioning is that the height of the counterfactual to the right of the thresholds now seems lower than before (shown in Figures 23 and 24). This is exactly what we would expect when we remove the highly unionised, if they are also bunching at the donation threshold because their union fees would thereby place them above it.

Our main results are therefore highly robust to the presence of union fees. This of course means that we are identifying the effect of the 2008 reform from non-union members, since union members would be scattered around these thresholds with their donations, and as the previous analysis revealed, their union fees would place them to the right of them. This does not affect our analysis, since systematic union fees would only potentially affect the *level* of bunching. We are not however interested in this level per se, but rather in the *ratio* of bunching, between 2008 and 2007. While this is identified from the non-unionised, our result is generalisable to the extent that union members behave in a similar way. We check this

[17] There is a large drop from 2008, driven by the financial crisis.

by next repeating our main analysis after removing union fees among the highly unionised sectors.[18] The results are shown in Appendix Figures 22 - 25. Again, we find the exact same patterns. Our lower bound estimate of the reporting response is 42%, which is very similar to our previous estimates.

As a final robustness test, we check whether our main results are sensitive to the choices we have made regarding the polynomial order used to estimate the counterfactual and the bandwidth used. Figure 26 shows the bunching estimates for different specifications of these, and shows that our results are highly robust.

4.3 Heterogeneity Analysis

We have also tested for heterogeneity by running our main specification on different sub-samples by sex and age. Appendix Figure 27 shows the bunching dynamics by sex and Figure 28 by age groups. Overall, we do not find any evidence of significant heterogeneity. Results are extremely similar regardless of the sample split we consider.

5 The elasticity of giving and tax policy design

In this section we turn to our second source of quasi-experimental variation: changes in the price of giving generated by tax rate reforms and our second question: the importance of tax policy design for elasticities. Most importantly we look at how the thresholds studied extensively above impact the elasticity of reported donations with respect to price. The tax rate reforms in our sample period are illustrated in Figure 7. We can exploit these reforms as a source of variation in the price of giving, since the tax rate essentially determines the size of the tax subsidy to charitable donations. The typical approach in the literature on the elasticity of the tax price of giving is to run log-specifications of the form:

$$ln(d_{it}) = \beta_1 ln(1 - \tau_{it}) + \beta_2 ln(y_{it}) + \beta_3' X_{it} + \Gamma_i + \Gamma_t + \varepsilon_{it} \quad (3)$$

where d_{it} is the donation amount and y_{it} is disposable income before donations, of individual i at time t. τ_{it} is the marginal tax rate and hence $1 - \tau_{it}$ is the price of giving. X_{it} is a vector

[18] In this case, we exclude the public sector.

of other controls. Specifications estimated using panel data can also include individual and time fixed effects, Γ_i and Γ_t. The price elasticity is then given by β_1.

Estimating this equation using standard OLS leads to an endogeneity problem. Charitable donations can affect the price of giving because these may shift taxpayers to lower tax brackets, thereby reducing the tax price and causing an upward bias in the estimated elasticities. This is a well-known endogeneity problem in the literature, and has been typically dealt with by instrumenting the "last-pound" (observed) price of giving with the "first-pound" price of giving ($1 - \tau_{it|d_{it}=0} \equiv 1 - \tau^*_{it}$). In words, this is the price a taxpayer faces for the first pound of charitable contribution. This removes any price variation due to charitable giving, and results in a very strong first-stage because the first- and last-pound prices are mechanically very highly correlated.

For the exclusion restriction to hold, the relationship between the first-pound price and the level of donations must solely go through the last-pound-price of giving. As argued by Almunia et. al (2017), this exclusion restriction is violated when using price variation from tax reforms because such reforms create a second source of endogeneity. Specifically, changes in marginal tax rates can also affect other choices such as individual labour supply and earnings more generally. Tax reforms therefore affect taxable earnings choices and hence can determine the first-pound price of charitable giving because they determine which tax bracket a taxpayer is in. In other words, tax reforms can affect choices which both enter the donation decision and directly affect the first-pound price, violating the exclusion restriction.

Almunia et al. (2017) propose a solution that leverages the availability of panel data, based on the Gruber and Saez (2002) IV strategy that is widely used in the literature on the elasticity of taxable income (for a review, see Saez et al. 2012). Their instrument uses lagged income values to predict the reform-induced change in the price of giving. Concretely they propose estimating the following differenced equation:

$$\Delta ln(d_{it}) = \beta_1 \Delta ln(1 - \tau^*_{it}) + \beta_2 \Delta ln(y_{it}) + \beta'_3 \Delta X_{it} + \Delta\varepsilon_{it} \tag{4}$$

where $\Delta ln(x_{it}) = ln\left(\frac{x_{it}}{x_{i,t-k}}\right)$ for $x_{it} = d_{it}, 1 - \tau^*_{it}, y_{it}$, and the log change in the first-pound price is instrumented by:

$$ln\left(\frac{1-\tau^*_{i,t}\left(y^*_{i,t-k}\right)}{1-\tau^*_{i,t-k}\left(y^*_{i,t-k}\right)}\right) \quad (5)$$

The variable k determines the time horizon of the difference. In words, the instrument is the change in the price of giving from time $t-k$ to time t if taxable income at zero donations (y^*_{it}) remained unchanged. This instrument solves the endogeneity problem because it uses past (pre-tax) income which should not be affected by future reform-related choices. The only threat to this exclusion restriction is potential anticipation responses to tax reforms. For this reason, we consider lags of both $k = 1$ and $k = 2$, as the longer the lag, the less realistic such a violation becomes.[19]

In our empirical application, we mainly follow Almunia et al. (2017) and implement the differenced-IV specification (4). To compare our findings with existing practice, we also report results for both OLS and IV estimates of specification (3).

As mentioned earlier our measure of donations also include a union membership fee for some subset of workers. We cannot directly observe the size of this fee or who is a member of a union. However, using information on sector we can to some extent back it out. The banking sector in Cyprus is a large sector, and it is (almost) fully unionised meaning that all workers in this sector are members of a union and pays the same fraction of salary in membership fee. Consequently for the banking sector we can completely separate donations from union membership fees. Apart from the banking sector a small number of other sectors, such as the construction and hotel sectors, are highly but not fully unionised. Consequently for these sectors we can correct for fee payments with some error coming from the small fraction of workers who are not in a union.[20] Lastly Cyprus has a number of sectors with very low unionisation. In these sectors the donation measure will be somewhat noisy due to the small number of workers who are anyway in a union. Following these considerations we use the

[19] We have also tried longer lags and find very similar results, confirming the lack of anticipatory effects.

[20] In this sample we do not include the public sector even though this sector is highly unionised. This is due to the existence of two different rates for the union membership fee in the public sector. Since we cannot observe which workers are subject to which rate we cannot correct the measure for membership fees in this sector.

sample of workers from the banking sector as our main sample in the following analysis, but for all results we show robustness to using either the full sample or the sample of highly unionised sectors instead.

5.1 Elasticity results for the whole period 1999-2010

In this section we estimate the elasticity of donations with respect to price using the methods described above and the full sample period. Table 2 reports results for different specifications using the sample of salary earners in the banking sector. We only include individuals with a tax filing obligation in the sample period. Since we are dealing exclusively with the banking sector we can correct for union fee payments and hence obtain an accurate measure of charitable contributions for each individual.

In Table 2 we begin with the benchmark case where we estimate specification (3) by OLS. Column (1) shows results without controls, while column (2) also includes age squared and a dummy for whether a worker switched job as controls. All specifications include individual and year fixed effects. When including controls the OLS specification produces an estimate of -0.38 for β_1. We next turn to the IV version of specification (3) in columns (3) and (4), where we instrument the last-pound price using the first-pound price. As expected, β_1 becomes much larger in absolute value (-0.56), which is in line with the upward bias of OLS. Finally, the last four columns turn to our preferred differenced-IV specification. Column (6) and (8) shows that the estimate of β_1 is -0.51 when using controls and $k = 1$, and nearly unchanged when using $k = 2$. Consequently the estimate of β_1 is very stable across our three different IV specifications at a value around -0.5 when including controls. Estimates are slightly higher, but comparable when excluding controls.

These results are robust to including individuals for whom we cannot be sure that we can correct precisely for union fee payments in the donation measure. In Table 18 in the Appendix we include other sectors which are highly unionised - for instance the hotel sector and the construction sector. These sectors all have a high degree of unionisation and therefore we can correct the donation measure with some noise coming from the small fraction which is not in a union. Results from this table are very much in line with what we found above. In Table 19 we include the whole sample and correct the donation measure only for those sectors with

Table 2:

Elasticity of donations with respect to price (1999-2010)

Price variable	(1) OLS	(2) OLS	(3) IV	(4) IV	(5) IV$\Delta_{k=1}$	(6) IV$\Delta_{k=1}$	(7) IV$\Delta_{k=2}$	(8) IV$\Delta_{k=2}$
$ln(1-\tau)$	-0.556^{***} (0.051)	-0.375^{***} (0.056)						
$ln(1-\tau^*)$			-0.716^{***} (0.057)	-0.556^{***} (0.065)				
$\Delta ln(1-\tau^*)$					-0.715^{***} (0.066)	-0.506^{***} (0.062)	-0.747^{***} (0.074)	-0.475^{***} (0.072)
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	-	✓	-	✓	-	✓	-	✓
Observations	50 789	50 789	50 783	50 783	43 094	43 094	37 598	37 598
R^2	0.50	0.51	0.50	0.51	0.13	0.14	0.18	0.19

Notes: The sample includes all years in the data. In all specifications we control for income. Additional controls include age squared and a dummy for changing employer. We remove professional taxes from the donation measure (before 2003), and we drop people below the first tax bracket (i.e. people with no tax liability). We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

a high degree of unionisation. In this case the donation measure will be somewhat noisy. The results from this sample are nevertheless still very much in line with our main results above. Reassuringly, all findings are not sensitive to the potential noise created by the union fees in our measure. This is also not surprising, given that the level of union fees is very low.

In the results above we do not explicitly deal with censoring coming from people reporting zero donations. Given the logarithmic specification such observations are excluded from the estimation and this can potentially create bias. However, in our setup this is not a serious concern since 87 % of tax filers report positive donations.[21] Following previous literature we nevertheless check for robustness of our results to this margin by estimating a Poisson model of the form,

$$d_{it} = exp(\theta_1 log(1-\tau^*_{it}) + \theta_2 log(y_{it}) + \theta_3 X_{it} + \alpha_i + \delta_t + u_{it}) \quad (6)$$

[21]In our main estimation sample of bankers, this is even higher (91 %). In contrast, this number is only 10% of the estimation sample in Almunia et al. (2017) and 15% in Fack & Landais (2015).

This model can be estimated using a pseudo-maximum likelihood method. The benefit of this approach is that it circumvents the selection issue by allowing the inclusion of both positive and zero values of the dependent variable.[22]

Table 20 in the Appendix shows the results from this approach. Both column (1) and (2) use the first pound price of giving as the regressor, and these results are hence comparable to columns (3) and (4) of Table 2. Especially when including controls the elasticity resulting from this approach is very similar in magnitude to our main estimates from Table 2. Hence we conclude that our results are not biased by censoring issues around zero donations.

5.2 Elasticities and reporting thresholds

The first sections of this paper investigated in detail the behavioural responses to thresholds determining reporting standards. We now investigate whether the existence of such thresholds affects people's responsiveness to price changes and hence affects the elasticity estimated above. We have shown that people respond strongly to these thresholds by bunching at the exact threshold values. If such bunching behaviour is sticky when prices change, then this design feature could potentially have a strong effect on elasticities and consequently the effectiveness of providing subsidies. This is an important issue as this feature is not uncommon across the world, nor is it restricted to the treatment of charitable contributions. Moreover, tax authorities may set such thresholds for a variety of reasons including concerns about administrative costs.

To get a picture of the sensitivity of elasticities to this tax design feature, we split our sample into two types of workers. The first group consists of those workers who at some point bunch, meaning that at some point in the sample period we observe them exactly at a threshold value. The second group consists of those workers who never bunch meaning that we never observe them at a threshold value. We then estimate elasticities separately for these two groups of workers using the methods introduced above. We look only at the period 2003-2007 since this is the longest period in the data for which the reporting environment remains

[22]It is also preferred to the Tobit model as it does not suffer from the incidental parameters problem, relies on weaker functional form assumptions, and allows for constant elasticities, which the Tobit specification does not. For more details, see Almunia et al. (2017).

constant. In both year 2003 and year 2008 we observe changes in both marginal tax rates and hence prices, and also in the placement of the reporting thresholds. Since we are specifically interested in the stickiness of bunching behaviour around these thresholds, we want to clearly separate responses to prices from movements caused by changes in thresholds.[23]

In Table 3 panel A, we first estimate the elasticity for both groups together in the period in question 2003-2007. In column (1)-(2) we report estimates using the first-pound price IV strategy, and in columns (3)-(4) we report results using the differenced IV strategy with a time horizon of one year. Since results in the previous section were very insensitive to the time horizon of the difference, we focus only on the case $k = 1$ moving forward. The results from panel A using both groups are unsurprisingly very similar to the results from the previous section. In panel B we look only at the group of workers who never bunch at a reporting threshold. Results from both estimation strategies show that this group displays much more sensitivity to price changes than the collective group of workers. Estimated elasticities double in magnitude compared to panel A. In panel C we now look exclusively at the group of workers who at some point in the period 2003-2007 bunch at a threshold value. This group is much less responsive to price changes and estimated elasticities are around half the size of the estimates for the entire group and about a fourth of the size of the estimates for the non-bunchers. If we instead use the sample of all highly unionised sectors or the sample of all sectors we see exactly the same patterns (Appendix Tables 21 and 22). In these samples the estimated differences are somewhat smaller consistent with the fact that we cannot precisely separate bunchers from non-bunchers given the noise in the donation measure.

These results are in line with a situation where bunching behaviour around reporting thresholds is sticky and hence workers who bunch at the thresholds need strong incentives to change behaviour. If these differences are driven by stickiness around reporting thresholds then the differences above should be driven by bunchers being less likely to react to price changes and not from bunchers reacting less to a fixed price change compared to non-bunchers. If we look at the average fraction of workers who change the amount they report in donations from one year to the next, then this fraction is 38 % for people who bunch at a threshold the previous

[23]Note that we cannot look exclusively at the last period 2008-2010 since we have no variation in tax rates in this period. Focusing instead on the period before 2003 does not change the main conclusions of this section.

Table 3:

Elasticity of donations with respect to price - bunchers vs. non-bunchers (2003-2007)

	(1)	(2)	(3)	(4)
Price variable	IV	IV	IV$\Delta_{k=1}$	IV$\Delta_{k=1}$
Panel A: All workers				
$ln(1-\tau^*)$	-0.642^{***} (0.169)	-0.658^{***} (0.180)		
$\Delta ln(1-\tau^*)$			-0.419^{***} (0.160)	-0.375^{***} (0.154)
Observations	17 955	17 955	15 957	15 957
R^2	0.62	0.62	0.20	0.21
Panel B: Excluding bunchers				
$ln(1-\tau^*)$	-1.462^{***} (0.335)	-1.517^{***} (0.372)		
$\Delta ln(1-\tau^*)$			-0.818^{***} (0.261)	-0.786^{***} (0.247)
Observations	5 336	5 336	4 586	4 586
R^2	0.72	0.72	0.27	0.28
Panel C: Only bunchers				
$ln(1-\tau^*)$	-0.294 (0.188)	-0.306 (0.191)		
$\Delta ln(1-\tau^*)$			-0.253 (0.203)	-0.207 (0.198)
Observations	12 619	12 619	11 371	11 371
R^2	0.52	0.52	0.17	0.18
Individual FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Controls	-	✓	-	✓

Notes: The sample includes all workers from the banking sectors in years 2003-2007. In all specifications we control for income. Additional controls include age squared and a dummy for changing employer. We drop people below the first tax bracket (i.e. people with no tax liability). We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

year, while it is 79 % for people who did not bunch at a threshold the previous year. If we only look at workers in years where they experience a change in the first-pound price then these fractions increase to 44 % and 91 % respectively. These numbers support the idea of stickiness in behaviour around thresholds since both unconditional and conditional on a price change bunchers are much less likely to change reporting behaviour from one year to the next. Beyond these non-parametric measures, we can implement this analysis in a regression framework and include controls. Table 4 report results from a simple linear probability model

Table 4:

Buncher stickiness - Linear probability model

	(1) All	(2) All	(3) $\Delta(1-\tau^*) \neq 0$	(4) $\Delta(1-\tau^*) \neq 0$
$\mathbb{1}_{(\text{buncher})}$	-0.403^{***} (0.007)	-0.391^{***} (0.007)	-0.474^{***} (0.013)	-0.458^{***} (0.013)
Constant	0.785^{***} (0.004)	0.813^{***} (0.011)	0.913^{***} (0.004)	0.895^{***} (0.015)
Observations	20 621	20 621	6 567	6 567
R^2	0.16	0.20	0.27	0.28
Year FE	-	✓	-	✓
Controls	-	✓	-	✓

Notes: The sample includes workers from the banking sector in years 2003-2007. Controls include age squared, salary and a dummy for changing employer. We drop people below the first tax bracket (i.e. people with no tax liability). In columns (3) and (4) we restrict only to workers experiencing a change in the first-pound price. We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

where the left hand side is an indicator for changing reporting from one year to the next. On the right hand side we include an indicator for being located at a threshold value i.e. being a buncher, a constant and potential controls. Column (1) simply shows the unconditional average fractions reported above - the constant shows that 79 % of non-bunchers change reporting from one year to the next, while the coefficient on the buncher-indicator shows that this percentage is $78.5\% - 40.3\% = 38.2\%$ for bunchers. In column (2) we control for year fixed effects, age squared, salary and a dummy for changing employer and results remain unchanged. Column (3) reports the conditional average fractions discussed above, where we only look at individuals with a change in price. Also in this case adding controls makes little difference to the results. Tables 23 and 24 in the Appendix reports results using the alternative samples of either all highly unionised sectors or all sectors. Both tables show results very much in line with the main results discussed above. Consequently we observe a large and statistically significant difference between bunchers and non-bunchers in the probability of changing reporting behaviour and reacting to price-changes. This lends support to the idea that the large effect of these reporting thresholds on elasticities, comes from sticky bunching behaviour around the threshold values. Such behaviour should hence be taken into account when trying to estimate elasticities in an environment characterised by discontinuous changes in reporting standards around threshold values.

5.3 Elasticities and enforcement

Next we look at the importance of the overall enforcement strictness for estimated elasticities. In short we have three different sub-periods characterised by differences in the enforcement environment. Before 2003 reporting thresholds were salary dependent and relatively low for many income groups. In 2003 this threshold was increased for individuals with income below CYP 10,000, and the threshold was made independent of income. Hence between 2003 and 2007 the enforcement environment was laxer compared to the earlier period because a subgroup experienced a higher threshold. In 2008 the threshold was moved again - this time it was increased for everyone and hence again the enforcement environment became laxer. To investigate the importance of these enforcement changes for elasticities we divide our sample period into sub-periods and perform separate estimations in each period. In the last period 2008-2010 we don't have any marginal tax rate reforms and hence we do not have variation in the price of giving. Therefore we only estimate on two different periods: 1999-2002 representing the stricter enforcement environment and 2003-2010 representing the laxer enforcement environment with higher thresholds.

Results for workers in the banking sector are given in Table 5. We report results both for the first-price IV strategy and for the differenced IV strategy, but estimated elasticities are very similar across these specifications and hence we focus on the preferred differenced strategy. In column (4) we find an estimated elasticity of -0.29 for the stricter period when including

Table 5:

Elasticity of donations with respect to price by period

	1999-2002:				**2003-2010:**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price variable	IV	IV	IV$\Delta_{k=1}$	IV$\Delta_{k=1}$	IV	IV	IV$\Delta_{k=1}$	IV$\Delta_{k=1}$
$ln(1-\tau^*)$	-0.235^{***} (0.088)	-0.229^{**} (0.098)			-0.580^{***} (0.125)	-0.554^{***} (0.141)		
$\Delta ln(1-\tau^*)$			-0.288^{**} (0.131)	-0.285^{**} (0.130)			-0.613^{***} (0.127)	-0.513^{***} (0.124)
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	-	✓	-	✓	-	✓	-	✓
Observations	12 110	12 110	8 198	8 198	33 855	33 855	30 260	30 260
R^2	0.71	0.72	0.34	0.34	0.55	0.55	0.12	0.12

Notes: The sample includes all years in the data. In all specifications we control for income. Additional controls include age squared and a dummy for changing employer. We remove professional taxes from the donation measure (before 2003), and we drop people below the first tax bracket (i.e. people with no tax liability). We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

controls. In column (8) we find an estimated elasticity of −0.51 for the laxer period when including controls. Consequently we see a much larger elasticity in the laxer enforcement environment suggesting that enforcement matters for the size of elasticities. Importantly this large difference is present in all four different specifications of Table 5. Note further that standard errors are small in both periods and hence these elasticities are precisely estimated. These results are again robust to including individuals for whom we cannot be sure that we can correct precisely for union fee payments in the donation measure. In Table 25 we show analogous results for the larger sample of all highly unionised sectors corrected for union fees. Again we see large differences across periods, with elasticities being larger in the laxer enforcement period. In Table 26 we show results for the whole sample where we correct the highly unionised sectors, but not the sectors with low unionisation. Again we see the same differences across periods.

6 Conclusion

This paper studies behavioural responses to enforcement and the importance of tax policy design for elasticities, using the context of charitable contributions in the Republic of Cyprus. We use multiple sources of quasi-experimental variation in reporting requirements and tax price subsidies to present several policy-relevant results.

First, we show evidence of substantial reactions to changes in reporting environment. Exploiting salary-dependent thresholds for third party reporting of charitable contributions and a regression discontinuity approach, we estimate that reported donations increase by about 0.7 pounds when taxpayers are given 1 pound more in non-third-party reported deductions. Second, we separate real from reporting responses by utilising a reform that retrospectively shifted the location of the threshold determining third party reporting. Our bunching analysis reveals that at least 36 percent of the responses to changes in thresholds are purely due to changes in reporting.

Finally, using quasi-experimental variation in tax prices generated by income tax reforms, we find that the tax price elasticity of reported charitable donations is about -0.5. Importantly, we show that this policy parameter is highly dependent on the reporting environment, partly because the existence of thresholds generates strong bunching responses which mitigates responses to tax price changes. Price elasticities double if we remove individuals who at some point in the sample period bunch at a threshold. Further, we estimate elasticities across different sub-periods with varying enforcement strictness, and show suggestive evidence that this parameter is sensitive to even minor changes in reporting environment.

Our findings have important policy implications. The very strong behavioural responses to reporting thresholds imply that this policy instrument severely affects taxpayers' behaviour and consequently government revenue. Further, it affects people's responsiveness to prices and therefore potentially the effectiveness of providing subsidies such as for instance the globally widespread subsidy to charitable contributions. To the extent that the fiscal authority wants to incentivise certain forms of behaviours that generate positive externalities, it is crucial to understand the conditions under which tax subsidies cannot achieve this goal. Given that such thresholds are common in tax systems of many countries, this aspect of tax design warrants a more direct incorporation in optimal tax theory.

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7 Appendix - Tables

Table 6: **Summary Statistics**

	Mean	Std. Dev.
Salary Earnings Only	0.845	0.362
Ratio of Salary to Total Earnings	0.960	0.141
Taxable Income	12646.498	11139.126
Job Switches	0.028	0.164
Marginal Tax Rate	0.182	0.133
Positive Donations	0.873	0.333
Donations (cond. positive)	171.711	118.089
Positive Donations (Net of Union Fees)	129.87	103.436
Donations (Net of Union Fees, cond. positive)	0.788	0.409
Age	40.437	8.070
Female	0.385	0.487
Agriculture	0.005	0.069
Mining	0.003	0.051
Manufacturing	0.086	0.281
Construction	0.074	0.262
Utilities	0.021	0.142
Trade	0.120	0.325
Hotel Services	0.033	0.180
Other Services	0.186	0.389
Commercial Banking	0.066	0.248
Other Financial Services	0.033	0.179
Public Sector	0.360	0.480
Other	0.011	0.102
Observations	1,462,409	

Notes: This table displays summary statistics for our sample. We distinguish between positive donations, and positive donations net of union fees, where we residualise our measure in the latter case from union fees. Both measures have professional taxes already removed.

Table 7:

RD estimates - including rounders

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above 10k	38.43***	36.35***	35.81***	34.10***	37.06***	37.40***	34.85***	35.29***
	(0.93)	(1.39)	(0.95)	(1.42)	(1.28)	(2.00)	(1.33)	(2.09)
Implied takeup	0.77	0.73	0.72	0.68	0.74	0.75	0.70	0.71
Observations	91 768	91 768	74 064	74 064	46 676	46 676	38 012	38 012
R^2	0.26	0.26	0.32	0.32	0.20	0.20	0.26	0.26
Panel B:								
Above 7.5k	29.50***	31.79***	30.10***	30.82***	31.72***	27.66***	30.98***	26.56***
	(0.70)	(1.02)	(0.75)	(1.09)	(0.95)	(1.42)	(1.01)	(1.50)
Implied takeup	0.74	0.79	0.75	0.77	0.79	0.69	0.77	0.66
Observations	107 684	107 684	81 197	81 197	60 945	60 945	46 272	46 272
R^2	0.33	0.33	0.39	0.39	0.22	0.22	0.30	0.30
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows robustness checks from estimating specification (1) when we include rounders to our main sample. In this case, the specification also includes round number fixed effects. The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 8:

RD estimates - years 1999-2002

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above10k	38.18***	34.43***	36.28***	33.41***	35.89***	34.91***	34.74***	35.09***
	(0.83)	(1.25)	(0.84)	(1.27)	(1.17)	(1.80)	(1.19)	(1.84)
Implied takeup	0.76	0.69	0.73	0.69	0.72	0.70	0.69	0.70
Observations	114 588	114 588	93 830	93 830	59 023	59 023	48 459	48 459
R^2	0.20	0.20	0.29	0.29	0.13	0.13	0.22	0.22
Panel B:								
Above 7.5k	28.75***	30.70***	29.21***	29.50***	29.76***	26.76***	28.78***	26.37***
	(0.69)	(1.00)	(0.71)	(1.04)	(0.94)	(1.41)	(0.96)	(1.44)
Implied takeup	0.72	0.77	0.73	0.74	0.74	0.67	0.72	0.66
Observations	116 837	116 837	90 641	90 641	64 941	64 941	50 592	50 592
R^2	0.26	0.26	0.36	0.36	0.15	0.15	0.27	0.27
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows robustness checks from estimating specification (1) when we also include year 2002 to our main sample. The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 9:

RD estimates - including individuals with some non-salary income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above 10k	35.07***	31.84***	33.11***	31.07***	33.17***	32.36***	32.34***	31.78***
	(0.89)	(1.35)	(0.91)	(1.39)	(1.26)	(1.95)	(1.30)	(2.03)
Implied takeup	0.70	0.64	0.66	0.62	0.66	0.65	0.65	0.64
Observations	96 826	96 826	79 271	79 271	49 475	49 475	40 784	40 784
R^2	0.20	0.20	0.27	0.27	0.12	0.12	0.20	0.20
Panel B:								
Above 7.5k	27.63***	29.46***	28.66***	28.80***	28.65***	24.49***	28.30***	24.79***
	(0.72)	(1.05)	(0.76)	(1.11)	(0.98)	(1.48)	(1.03)	(1.54)
Implied takeup	0.69	0.74	0.72	0.72	0.72	0.61	0.71	0.62
Observations	105 388	105 388	81 401	81 401	59 031	59 031	45 849	45 849
R^2	0.27	0.27	0.35	0.35	0.16	0.16	0.25	0.25
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows robustness checks from estimating specification (1) when we include individuals with income from both salary and non-salary earnings to our main sample. The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 10:

RD estimates - outcome variable corrected

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above 10k	36.58***	35.19***	35.64***	33.45***	35.06***	35.51***	33.55***	32.96***
	(1.17)	(1.72)	(1.22)	(1.79)	(1.62)	(2.48)	(1.70)	(2.58)
Implied takeup	0.73	0.70	0.71	0.67	0.70	0.71	0.67	0.66
Observations	62 960	62 960	47 402	47 402	32 269	32 269	24 578	24 578
R^2	0.11	0.11	0.24	0.24	0.08	0.08	0.22	0.22
Panel B:								
Above 7.5k	28.28***	27.25***	27.98***	25.68***	26.25***	23.67***	25.11***	22.60***
	(0.86)	(1.25)	(0.92)	(1.34)	(1.18)	(1.78)	(1.25)	(1.88)
Implied takeup	0.71	0.68	0.70	0.64	0.66	0.59	0.63	0.57
Observations	75 557	75 557	54 219	54 219	41 914	41 914	30 187	30 187
R^2	0.14	0.14	0.26	0.26	0.10	0.10	0.23	0.23
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows robustness checks from estimating specification (1) when we net out professional taxes and union payments from our outcome variable. In this case, we exclude individuals working in the public sector. The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 11:

RD estimates - only highly unionised sectors with corrected outcome variable

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above 10k	38.77***	33.67***	38.74***	33.88***	35.09***	34.97***	34.75***	34.43***
	(2.01)	(2.81)	(1.97)	(2.75)	(2.67)	(3.94)	(2.63)	(3.87)
Implied takeup	0.78	0.67	0.77	0.68	0.70	0.70	0.70	0.69
Observations	13 805	13 805	13 805	13 805	7 496	7 496	7 496	7 496
R^2	0.13	0.13	0.16	0.16	0.11	0.11	0.14	0.14
Panel B:								
Above 7.5k	25.80***	25.70***	27.26***	25.25***	25.63***	20.40***	25.38***	21.82***
	(1.76)	(2.67)	(1.67)	(2.54)	(2.45)	(3.88)	(2.34)	(3.70)
Implied takeup	0.65	0.64	0.68	0.63	0.64	0.51	0.63	0.55
Observations	13 531	13 531	13 531	13 531	7 638	7 638	7 638	7 638
R^2	0.11	0.11	0.19	0.19	0.09	0.09	0.17	0.17
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows robustness checks from estimating specification (1) when we net out professional taxes and union payments from our outcome variable, and restrict our sample to workers in highly unionised sectors (excluding the public sector). The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 12:

RD estimates - only highly unionised sectors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above 10k	34.81*** (1.29)	31.21*** (1.92)	35.52*** (1.27)	31.38*** (1.89)	32.72*** (1.82)	34.62*** (2.81)	33.38*** (1.80)	33.92*** (2.75)
Implied takeup	0.70	0.62	0.71	0.63	0.65	0.69	0.67	0.68
Observations	35 100	35 100	35 100	35 100	18 322	18 322	18 322	18 322
R^2	0.22	0.22	0.25	0.25	0.13	0.13	0.16	0.16
Panel B:								
Above 7.5k	31.52*** (1.17)	34.75*** (1.70)	32.44*** (1.15)	35.10*** (1.67)	33.07*** (1.57)	26.87*** (2.36)	33.19*** (1.53)	27.23*** (2.31)
Implied takeup	0.79	0.87	0.81	0.88	0.83	0.67	0.83	0.68
Observations	30 407	30 407	30 407	30 407	17 574	17 574	17 574	17 574
R^2	0.29	0.29	0.33	0.33	0.19	0.19	0.23	0.24
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows robustness checks from estimating specification (1) when we restrict our sample to those working in highly unionised sectors (including the public sector). The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 13:

RD estimates - only sectors with low unionisation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above 10k	33.08***	33.04***	33.55***	33.60***	33.88***	32.84***	34.57***	33.81***
	(1.54)	(2.29)	(1.49)	(2.22)	(2.17)	(3.34)	(2.11)	(3.23)
Implied takeup	0.66	0.66	0.67	0.67	0.68	0.66	0.69	0.68
Observations	33 700	33 700	33 597	33 597	17 134	17 134	17 082	17 082
R^2	0.19	0.19	0.25	0.25	0.11	0.11	0.18	0.18
Panel B:								
Above 7.5k	28.99***	27.89***	28.76***	27.60***	27.79***	24.08***	27.51***	25.19***
	(1.12)	(1.61)	(1.08)	(1.55)	(1.51)	(2.25)	(1.45)	(2.15)
Implied takeup	0.72	0.70	0.72	0.69	0.69	0.60	0.69	0.63
Observations	40 831	40 831	40 688	40 688	22 635	22 635	22 549	22 549
R^2	0.29	0.29	0.35	0.35	0.18	0.18	0.24	0.24
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows robustness checks from estimating specification (1) when we restrict our sample to those not working in highly unionised sectors. The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 14:

RD estimates - Only males

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above 10k	34.76***	33.04***	34.15***	32.74***	34.16***	32.76***	33.67***	32.78***
	(1.23)	(1.80)	(1.17)	(1.71)	(1.71)	(2.56)	(1.63)	(2.44)
Implied takeup	0.70	0.66	0.68	0.65	0.68	0.66	0.67	0.66
Observations	47 705	47 705	47 632	47 632	25 220	25 220	25 184	25 184
R^2	0.21	0.21	0.28	0.28	0.13	0.13	0.21	0.21
Panel B:								
Above 7.5k	27.93***	28.64***	27.18***	27.76***	27.96***	22.78***	27.01***	22.91***
	(1.13)	(1.64)	(1.07)	(1.54)	(1.53)	(2.29)	(1.44)	(2.13)
Implied takeup	0.70	0.72	0.68	0.69	0.70	0.57	0.68	0.57
Observations	43 398	43 398	43 316	43 316	24 150	24 150	24 097	24 097
R^2	0.30	0.30	0.37	0.37	0.18	0.18	0.27	0.27
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows the results from estimating specification (1) on our main sample restricting only to males. We pool years 1999-2001. The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 15:

RD estimates - Only females

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above 10k	34.56*** (1.78)	30.41*** (2.81)	35.06*** (1.72)	32.43*** (2.69)	32.45*** (2.63)	33.08*** (4.39)	34.64*** (2.53)	37.30*** (4.21)
Implied takeup	0.69	0.61	0.70	0.65	0.65	0.66	0.69	0.75
Observations	21 095	21 095	21 065	21 065	10 236	10 236	10 220	10 220
R^2	0.21	0.21	0.29	0.29	0.12	0.12	0.21	0.21
Panel B:								
Above 7.5k	32.97*** (1.21)	34.20*** (1.73)	34.41*** (1.15)	34.14*** (1.66)	33.71*** (1.59)	28.53*** (2.39)	33.56*** (1.52)	29.40*** (2.30)
Implied takeup	0.82	0.86	0.86	0.85	0.84	0.71	0.84	0.74
Observations	27 840	27 840	27 779	27 779	16 059	16 059	16 026	16 026
R^2	0.32	0.32	0.40	0.40	0.21	0.21	0.30	0.30
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows the results from estimating specification (1) on our main sample restricting only to females. We pool years 1999-2001. The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 16:

RD estimates - ages 25-39

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above 10k	31.50*** (1.29)	29.63*** (1.97)	31.43*** (1.24)	29.97*** (1.90)	31.37*** (1.88)	30.97*** (2.90)	31.90*** (1.82)	32.17*** (2.77)
Implied takeup	0.63	0.59	0.63	0.60	0.63	0.62	0.64	0.64
Observations	42 068	42 068	42 014	42 014	21 096	21 096	21 069	21 069
R^2	0.19	0.19	0.26	0.26	0.11	0.11	0.18	0.18
Panel B:								
Above 7.5k	29.92*** (0.99)	33.50*** (1.43)	30.95*** (0.94)	33.01*** (1.37)	32.37*** (1.33)	26.22*** (1.99)	32.05*** (1.26)	26.93*** (1.90)
Implied takeup	0.75	0.84	0.77	0.83	0.81	0.66	0.86	0.67
Observations	45 534	45 534	45 446	45 446	26 164	26 164	26 106	26 106
R^2	0.29	0.29	0.36	0.36	0.18	0.19	0.27	0.27
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows the results from estimating specification (1) on our main sample restricting only to ages 25 to 39. We pool years 1999-2001. The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 17:

RD estimates - ages 40-54

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:								
Above 10k	39.58***	36.19***	38.80***	37.24***	37.37***	35.88***	37.73***	36.95***
	(1.63)	(2.36)	(1.52)	(2.23)	(2.20)	(3.40)	(2.08)	(3.25)
Implied takeup	0.79	0.72	0.78	0.74	0.74	0.72	0.75	0.74
Observations	26 732	26 732	26 683	26 683	14 360	14 360	14 335	14 335
R^2	0.24	0.24	0.32	0.32	0.16	0.16	0.27	0.27
Panel B:								
Above 7.5k	29.98***	27.48***	30.12***	26.80***	27.50***	23.57***	26.28***	24.38***
	(1.52)	(2.16)	(1.41)	(2.01)	(2.02)	(3.01)	(1.88)	(2.77)
Implied takeup	0.75	0.69	0.75	0.67	0.69	0.59	0.66	0.61
Observations	25 704	25 704	25 649	25 649	14 045	14 045	14 017	14 017
R^2	0.31	0.31	0.42	0.42	0.18	0.18	0.30	0.30
Polynomial	p_1	p_2	p_1	p_2	p_1	p_2	p_1	p_2
Controls	-	-	✓	✓	-	-	✓	✓
Bandwidth	2 000	2 000	2 000	2 000	1 000	1 000	1 000	1 000

Notes: This table shows the results from estimating specification (1) on our main sample restricting only to ages 40 to 54. We pool years 1999-2001. The *Implied takeup* is calculated as the parameter estimate divided by the notch size in the donation schedule (i.e. in panel A the parameter estimate is divided by 50 while in panel B the parameter estimate is divided by 40). p_s indicates that we fit a polynomial of order s on each side of the notch, while controls include sex, year and sector fixed effects. Robust standard errors clustered at the individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 18:

Elasticity of donations with respect to price (1999-2010)

(Sample: All highly unionised sectors)

Price variable	(1) OLS	(2) OLS	(3) IV	(4) IV	(5) IV$\Delta_{k=1}$	(6) IV$\Delta_{k=1}$	(7) IV$\Delta_{k=2}$	(8) IV$\Delta_{k=2}$
$ln(1-\tau)$	-0.402^{***} (0.040)	-0.279^{***} (0.041)						
$ln(1-\tau^*)$			-0.598^{***} (0.045)	-0.481^{***} (0.047)				
$\Delta ln(1-\tau^*)$					-0.554^{***} (0.050)	-0.499^{***} (0.050)	-0.556^{***} (0.059)	-0.459^{***} (0.059)
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	-	✓	-	✓	-	✓	-	✓
Observations	84 697	84 697	84 683	84 683	67 852	67 852	57 556	57 556
R^2	0.57	0.57	0.57	0.57	0.15	0.15	0.21	0.21

Notes: The sample includes all years in the data. In all specifications we control for income. Additional controls include age squared and a dummy for changing employer. We remove professional taxes from the donation measure (before 2003), and we drop people below the first tax bracket (i.e. people with no tax liability). We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

Table 19:

Elasticity of donations with respect to price (1999-2010)

(Sample: All)

Price variable	(1) OLS	(2) OLS	(3) IV	(4) IV	(5) IV$\Delta_{k=1}$	(6) IV$\Delta_{k=1}$	(7) IV$\Delta_{k=2}$	(8) IV$\Delta_{k=2}$
$ln(1-\tau)$	-0.392^{***} (0.020)	-0.344^{***} (0.020)						
$ln(1-\tau^*)$			-0.606^{***} (0.024)	-0.557^{***} (0.024)				
$\Delta ln(1-\tau^*)$					-0.440^{***} (0.026)	-0.412^{***} (0.026)	-0.421^{***} (0.031)	-0.364^{***} (0.031)
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	-	✓	-	✓	-	✓	-	✓
Observations	219 732	219 732	219 706	219 706	168 999	168 999	139 480	139 480
R^2	0.65	0.65	0.65	0.65	0.16	0.16	0.23	0.23

Notes: The sample includes all years in the data. In all specifications we control for income. Additional controls include age squared and a dummy for changing employer. We remove professional taxes from the donation measure (before 2003), and we drop people below the first tax bracket (i.e. people with no tax liability). We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

Table 20:

Poisson regression (1999-2010)

	(1)	(2)
$ln(1-\tau^*)$	-0.557^{***} (0.052)	-0.470^{***} (0.057)
Observations	55 091	55 091
Individual FE	✓	✓
Year FE	✓	✓
Controls	-	✓

Notes: The sample includes all workers in the banking sector and all years. All specifications control for income and additional controls include age squared and a dummy for changing employer. We drop people below the first tax bracket (i.e. people with no tax liability). We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

Table 21:

Elasticity of donations with respect to price - bunchers vs. non-bunchers (2003-2007)

(Sample: All highly unionised sectors)

	(1)	(2)	(3)	(4)
Price variable	IV	IV	IV$\Delta_{k=1}$	IV$\Delta_{k=1}$
Panel A: All workers				
$ln(1-\tau^*)$	-0.428^{***} (0.131)	-0.426^{***} (0.148)		
$\Delta ln(1-\tau^*)$			-0.345^{***} (0.126)	-0.363^{***} (0.126)
Observations	25 822	25 822	22 644	22 644
R^2	0.69	0.69	0.26	0.27
Panel B: Excluding bunchers				
$ln(1-\tau^*)$	-0.725^{***} (0.208)	-0.774^{***} (0.213)		
$\Delta ln(1-\tau^*)$			-0.630^{***} (0.199)	-0.669^{***} (0.194)
Observations	9 110	9 110	7 708	7 708
R^2	0.75	0.75	0.32	0.32
Panel C: Only bunchers				
$ln(1-\tau^*)$	-0.361^{**} (0.154)	-0.342^{**} (0.160)		
$\Delta ln(1-\tau^*)$			-0.241 (0.162)	-0.232 (0.162)
Observations	16 712	16 712	14 936	14 936
R^2	0.64	0.64	0.23	0.24
Individual FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Controls	-	✓	-	✓

Notes: The sample includes workers from all hihgly unionised sectors in years 2003-2007. In all specifications we control for income. Additional controls include age squared and a dummy for changing employer. We drop people below the first tax bracket (i.e. people with no tax liability). We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

Table 22:

Elasticity of donations with respect to price - bunchers vs. non-bunchers (2003-2007)

(Sample: All)

	(1)	(2)	(3)	(4)
Price variable	IV	IV	IV$\Delta_{k=1}$	IV$\Delta_{k=1}$
Panel A: All workers				
$ln(1-\tau^{*})$	-0.432^{***} (0.064)	-0.409^{***} (0.065)		
$\Delta ln(1-\tau^{*})$			-0.411^{***} (0.068)	-0.394^{***} (0.068)
Observations	55 416	55 416	47 574	47 574
R^2	0.74	0.74	0.27	0.27
Panel B: Excluding bunchers				
$ln(1-\tau^{*})$	-0.555^{***} (0.105)	-0.523^{***} (0.107)		
$\Delta ln(1-\tau^{*})$			-0.593^{***} (0.101)	-0.580^{***} (0.099)
Observations	23 604	23 604	20 729	20 729
R^2	0.81	0.81	0.30	0.30
Panel C: Only bunchers				
$ln(1-\tau^{*})$	-0.360^{***} (0.080)	-0.344^{***} (0.081)		
$\Delta ln(1-\tau^{*})$			-0.306^{***} (0.094)	-0.282^{***} (0.094)
Observations	31 812	31 812	26 845	26 845
R^2	0.64	0.64	0.25	0.25
Individual FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Controls	-	✓	-	✓

Notes: The sample includes workers from all sectors in years 2003-2007. In all specifications we control for income. Additional controls include age squared and a dummy for changing employer. We drop people below the first tax bracket (i.e. people with no tax liability). We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

Table 23:

Buncher stickiness - Linear probability model

(Sample: All highly unionised sectors)

	(1) All	(2) All	(3) $\Delta(1-\tau^*) \neq 0$	(4) $\Delta(1-\tau^*) \neq 0$
$\mathbb{1}_{(\text{buncher})}$	-0.381^{***} (0.006)	-0.370^{***} (0.006)	-0.428^{***} (0.011)	-0.421^{***} (0.011)
Constant	0.791^{***} (0.003)	0.816^{***} (0.009)	0.930^{***} (0.003)	0.885^{***} (0.011)
Observations	33 031	33 031	11 548	11 548
R^2	0.14	0.15	0.23	0.24
Year FE	-	✓	-	✓
Controls	-	✓	-	✓

Notes: The sample includes workers from all highly unionised sectors in years 2003-2007. Controls include age squared, salary and a dummy for changing employer. We drop people below the first tax bracket (i.e. people with no tax liability). In columns (3) and (4) we restrict only to workers experiencing a change in the first-pound price. We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

Table 24:

Buncher stickiness - Linear probability model

(Sample: All)

	(1) All	(2) All	(3) $\Delta(1-\tau^*) \neq 0$	(4) $\Delta(1-\tau^*) \neq 0$
$\mathbb{1}_{\text{(buncher)}}$	-0.425^{***} (0.004)	-0.423^{***} (0.004)	-0.473^{***} (0.007)	-0.468^{***} (0.007)
Constant	0.746^{***} (0.002)	0.777^{***} (0.006)	0.850^{***} (0.003)	0.863^{***} (0.009)
Observations	77 269	77 269	27 872	27 872
R^2	0.15	0.15	0.19	0.20
Year FE	-	✓	-	✓
Controls	-	✓	-	✓

Notes: The sample includes workers from all sectors in years 2003-2007. Controls include age squared, salary and a dummy for changing employer. We drop people below the first tax bracket (i.e. people with no tax liability). In columns (3) and (4) we restrict only to workers experiencing a change in the first-pound price. We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

Table 25:

Elasticity of donations with respect to price by period

(Sample: All highly unionised sectors)

	1999-2002:				**2003-2010:**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price variable	IV	IV	IV$\Delta_{k=1}$	IV$\Delta_{k=1}$	IV	IV	IV$\Delta_{k=1}$	IV$\Delta_{k=1}$
$ln(1-\tau^*)$	-0.118^* (0.069)	-0.067 (0.072)			-0.343^{***} (0.093)	-0.342^{***} (0.106)		
$\Delta ln(1-\tau^*)$			-0.168^* (0.097)	-0.168^* (0.096)			-0.434^{***} (0.096)	-0.394^{***} (0.097)
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	-	✓	-	✓	-	✓	-	✓
Observations	25 298	25 298	15 732	15 732	50 461	50 461	44 091	44 091
R^2	0.70	0.70	0.27	0.27	0.63	0.63	0.18	0.18

Notes: The sample includes all years in the data. In all specifications we control for income. Additional controls include age squared and a dummy for changing employer. We remove professional taxes from the donation measure (before 2003), and we drop people below the first tax bracket (i.e. people with no tax liability). We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

Table 26:

Elasticity of donations with respect to price by period

(Sample: All)

	1999-2002:				**2003-2010:**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price variable	IV	IV	IV$\Delta_{k=1}$	IV$\Delta_{k=1}$	IV	IV	IV$\Delta_{k=1}$	IV$\Delta_{k=1}$
$ln(1-\tau^*)$	-0.131^{***} (0.037)	-0.119^{***} (0.037)			-0.421^{***} (0.041)	-0.403^{***} (0.042)		
$\Delta ln(1-\tau^*)$			-0.241^{***} (0.051)	-0.244^{***} (0.051)			-0.413^{***} (0.042)	-0.384^{***} (0.041)
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	-	✓	-	✓	-	✓	-	✓
Observations	72 314	72 314	44 058	44 058	121 218	121 218	101 912	101 912
R^2	0.76	0.76	0.27	0.27	0.69	0.69	0.18	0.18

Notes: The sample includes all years in the data. In all specifications we control for income. Additional controls include age squared and a dummy for changing employer. We remove professional taxes from the donation measure (before 2003), and we drop people below the first tax bracket (i.e. people with no tax liability). We report robust standard errors clustered at the individual level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

8 Appendix - Figures

Figure 6: **Information on Tax Returns Regarding Thresholds**

(a) 2002

C MISCELLANEOUS DEDUCTIONS (attach necessary certificates)

	1 DESCRIPTION	2 AMOUNT £		1 DESCRIPTION	2 AMOUNT £
1	Professional licence / Tax		4	Donations to approved Charities	
2	Contributions to trade unions		5	Deposits under the specific savings scheme of the Housing Finance Corporation	
3	Subscriptions		6	Any other deduction	
	TOTAL				

(b) 2003

B **MISCELLANEOUS DEDUCTIONS**
(For donations over £150 please attach certificates / receipts. For donations of a lesser amount you should keep the certificates / receipts to submit when requested).

	1 DESCRIPTION	2 AMOUNT £
1	TRADE UNION CONTRIBUTIONS	
2	PROFESSIONAL SUBSCRIPTIONS	
3	DONATIONS TO APPROVED CHARITABLE ORGANISATIONS	
4	ANY OTHER DEDUCTION	
5	**TOTAL**	

(c) 2008

PART 5 – DEDUCTIONS / ALLOWANCES

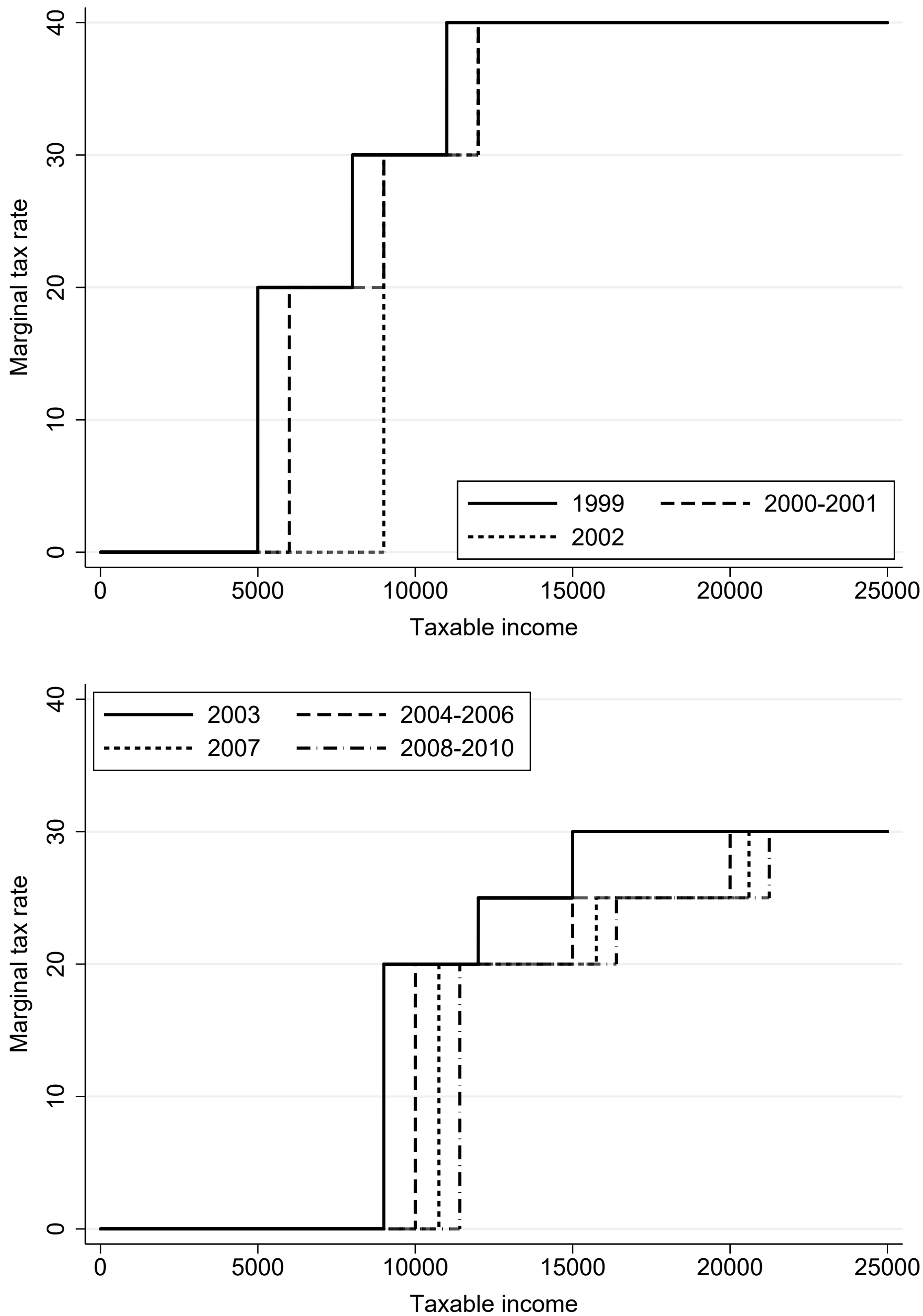
A **MISCELLANEOUS DEDUCTIONS**
(Attach certificates / receipts **only** for donations over €300. For donations of a lesser amount you should keep the certificates / receipts to submit when requested).

	1 DESCRIPTION	2 AMOUNT
1	TRADE UNION CONTRIBUTIONS	
2	PROFESSIONAL SUBSCRIPTIONS	
3	DONATIONS TO APPROVED CHARITABLE ORGANISATIONS	
4	ANY OTHER DEDUCTION	
5	**TOTAL**	

Notes: This set of figures shows the information provided on the tax return regarding filing thresholds for different years.

Figure 7:

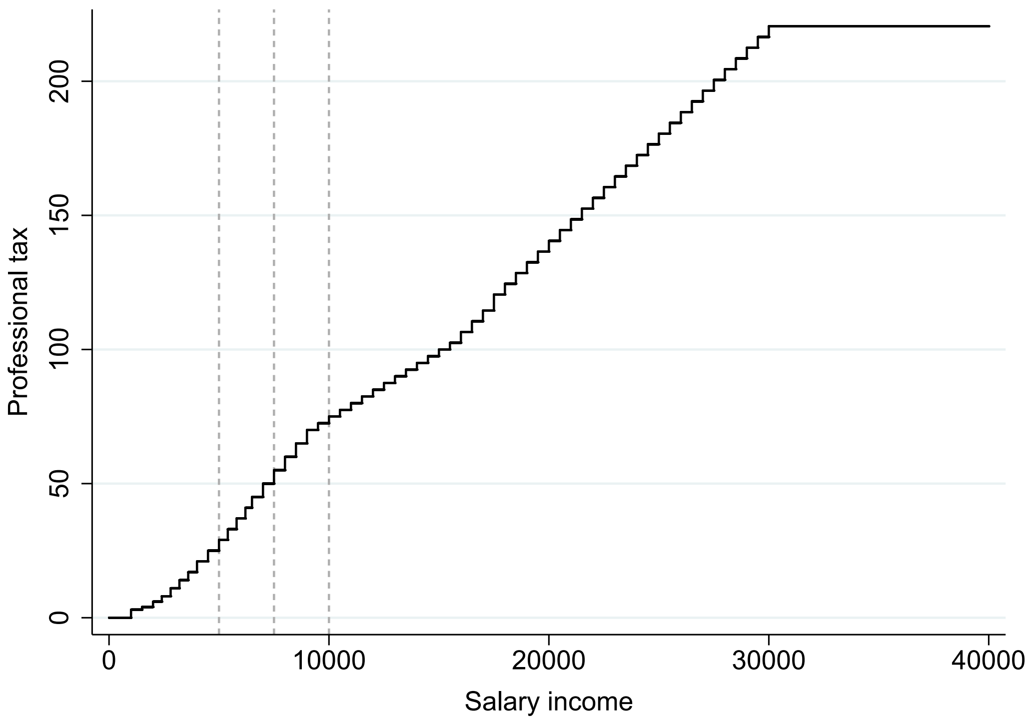
Schedule of marginal tax rates (years 1999-2010)



Notes: The figure shows the schedule of marginal tax rates in place in the Republic Of Cyprus in the years 1999-2010.

Figure 8:

Schedule of professional taxes (before 2003)

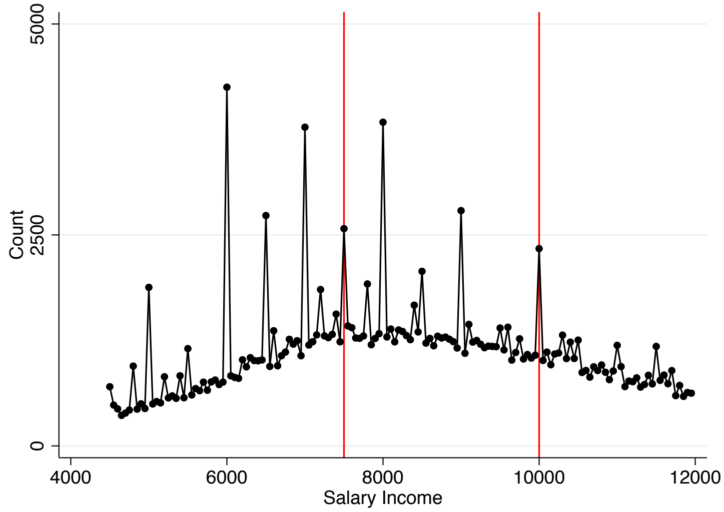


Notes: The graph shows the schedule of professional taxes in place in the years prior to 2003. The tax was dependent on salary income, with incremental steps until 30001. Vertical lines indicate the notches in the schedule for deductible donations without the provision of receipts.

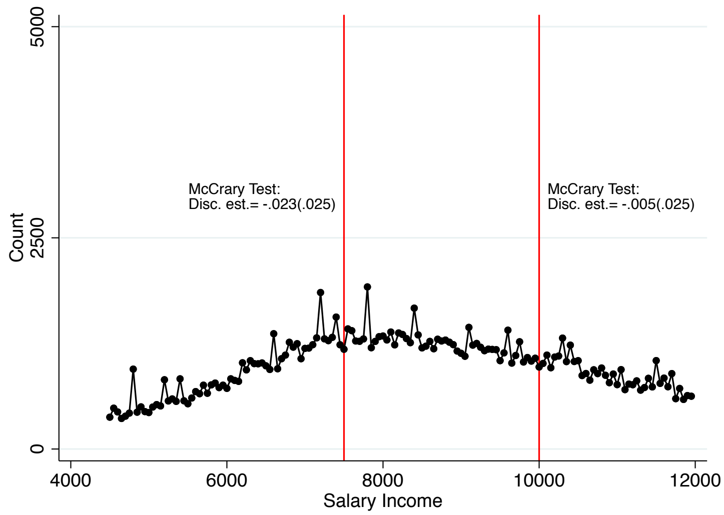
Figure 9:

Density of Salary Income Between 1999-2001

(a) Salary Income



(b) Salary Income Excluding Salaries at Exact Multiples of CYP 500

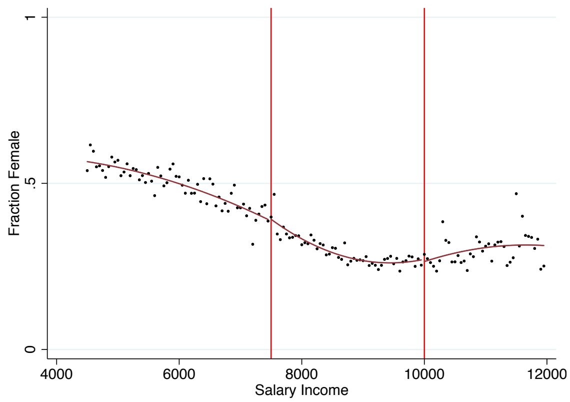


Notes: This figure shows the density of salaries pooled over the years 1999-2001, for two samples: (a) all salary earners and (b) all salary earners excluding those earning at exact multiples of CYP500. In each case, the two earnings thresholds that we focus on (7,500 and 10,000) are marked with vertical lines. Panel (b) also reports the results from a McCrary test for discontinuities in the density of the assignment variable (the estimated log difference in height). The null of no discontinuity cannot be rejected at any of the two earnings cutoffs, in support of the assumptions of our RD design.

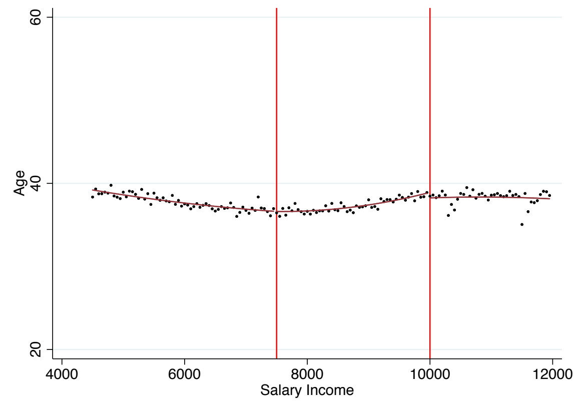
Figure 10:

Robustness check: Smoothness of covariates

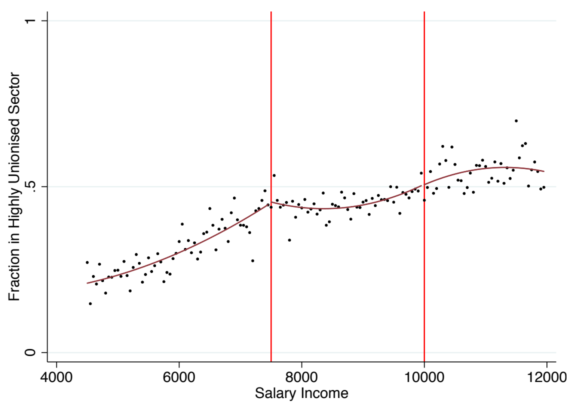
(a) Female



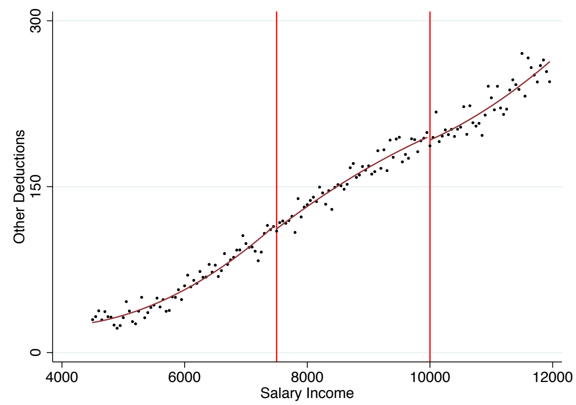
(b) Age



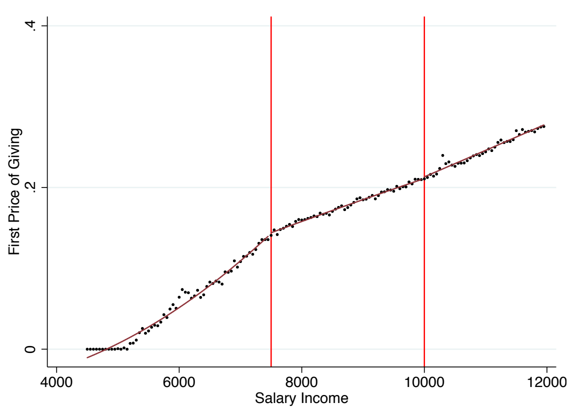
(c) Highly Unionised Sectors



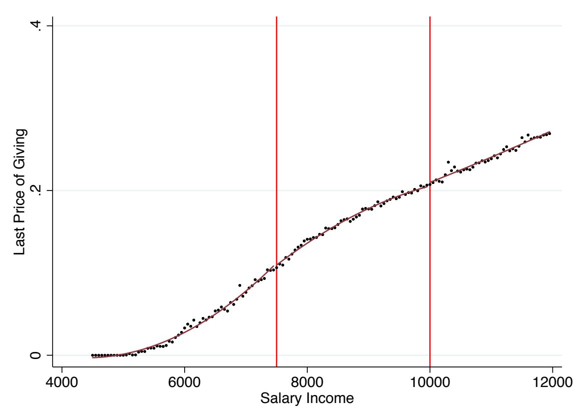
(d) Other Deductions



(e) First Price of Giving

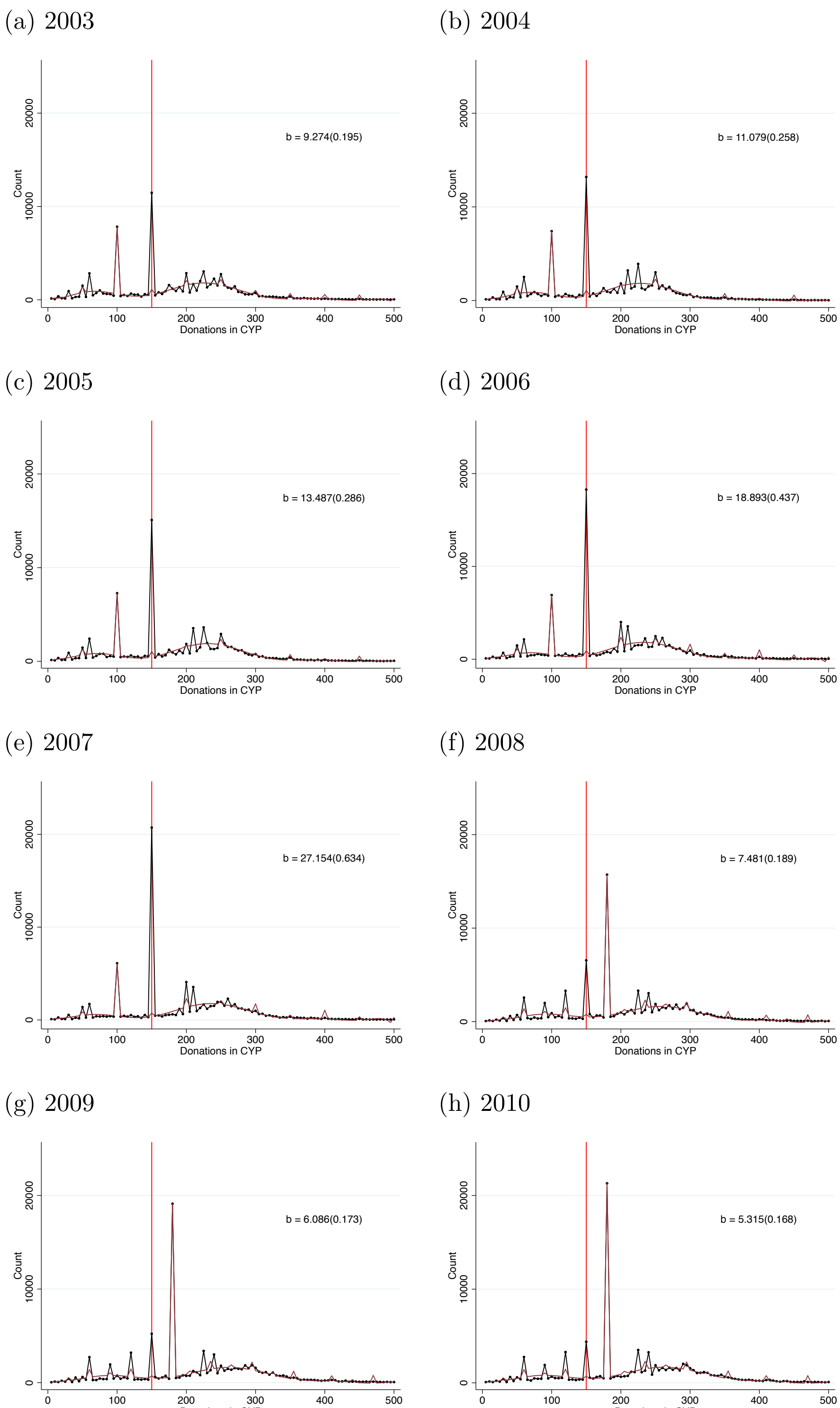


(f) Last Price of Giving



Notes: This figure shows evidence in support of the RD identifying assumption. Each sub-figure shows the mean value of the given covariate in bins of width 50 of the assignment variable around each salary threshold. The sample is the same as in our main specification (pooled over 1999-2001).

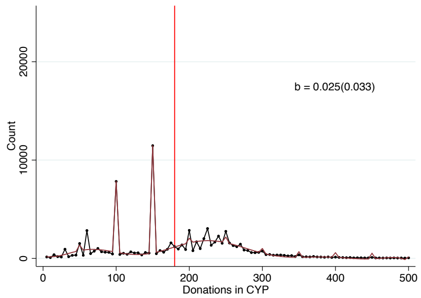
Figure 11: **Bunching at CYP 150 with Estimated Counterfactual, Main Sample**



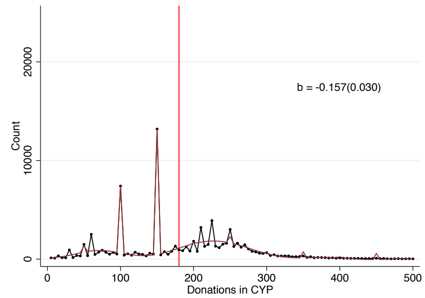
Notes: This figure shows the bunching dynamics of positive donations among salary earners between 2003-2010. It plots the yearly empirical distribution in bins of width CYP 5, together with the estimated counterfactual. Each sub-figure reports the normalised excess bunching mass b around the CYP 150 threshold. Bootstrapped standard errors are in parentheses.

Figure 12: **Bunching at CYP 175 with Estimated Counterfactual, Main Sample**

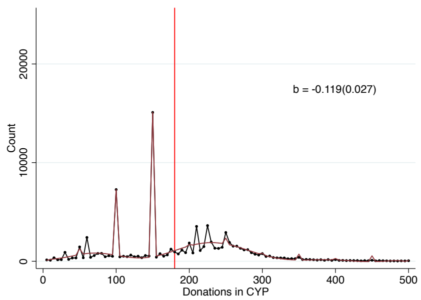
(a) 2003



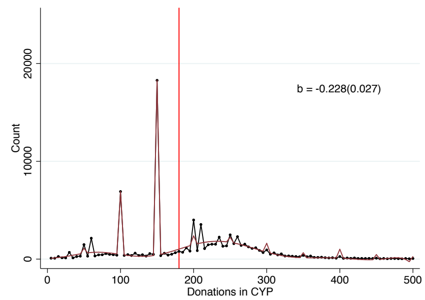
(b) 2004



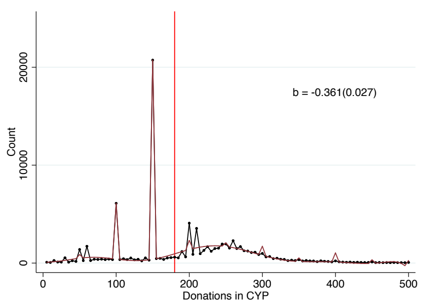
(c) 2005



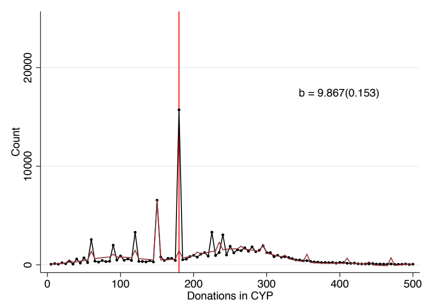
(d) 2006



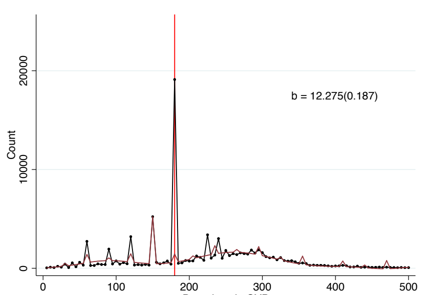
(e) 2007



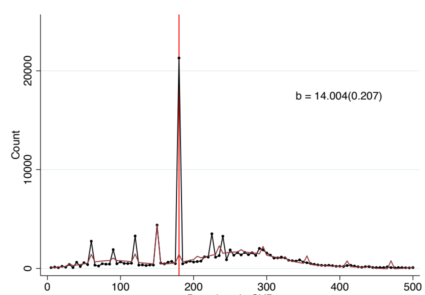
(f) 2008



(g) 2009

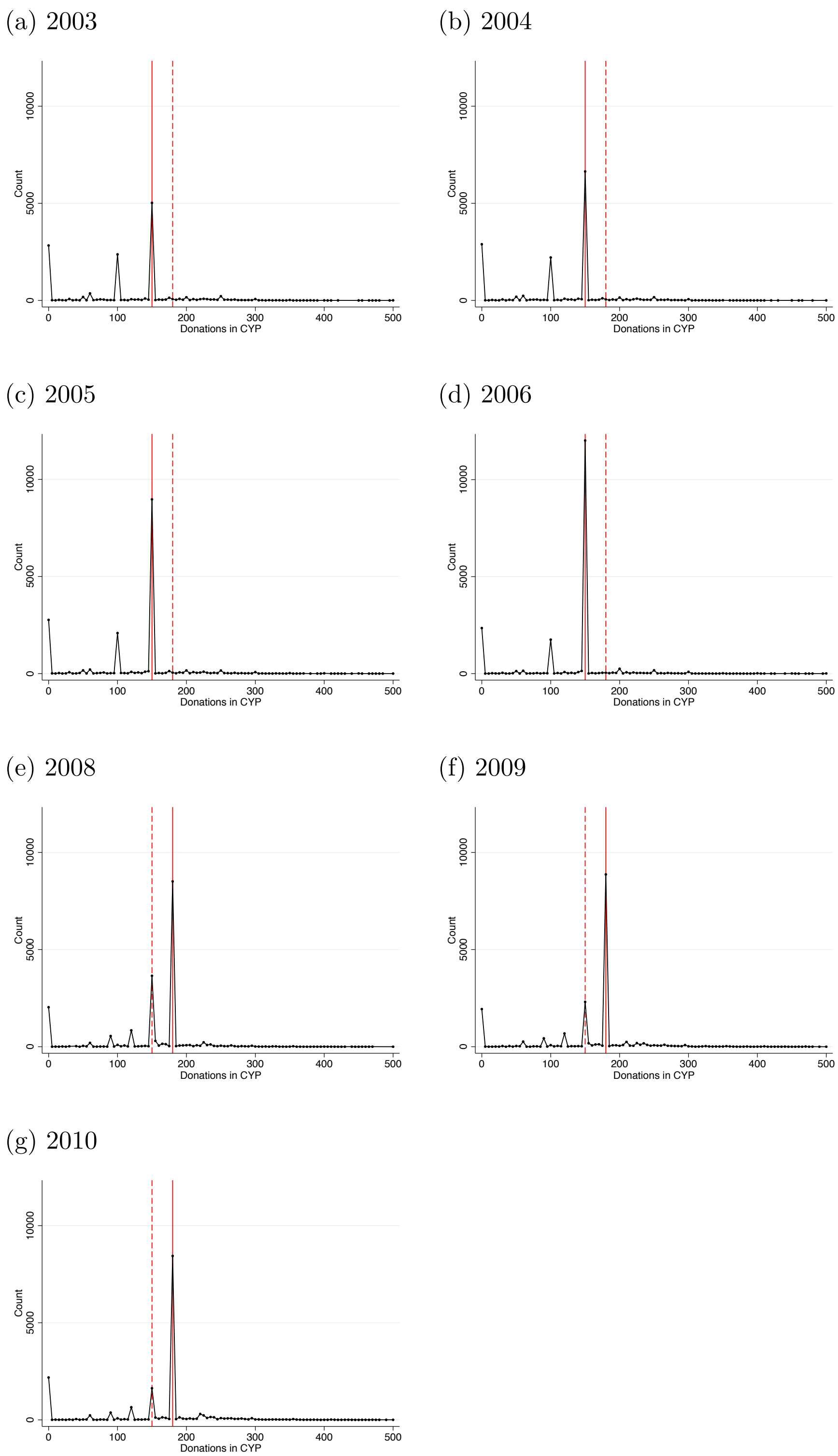


(h) 2010



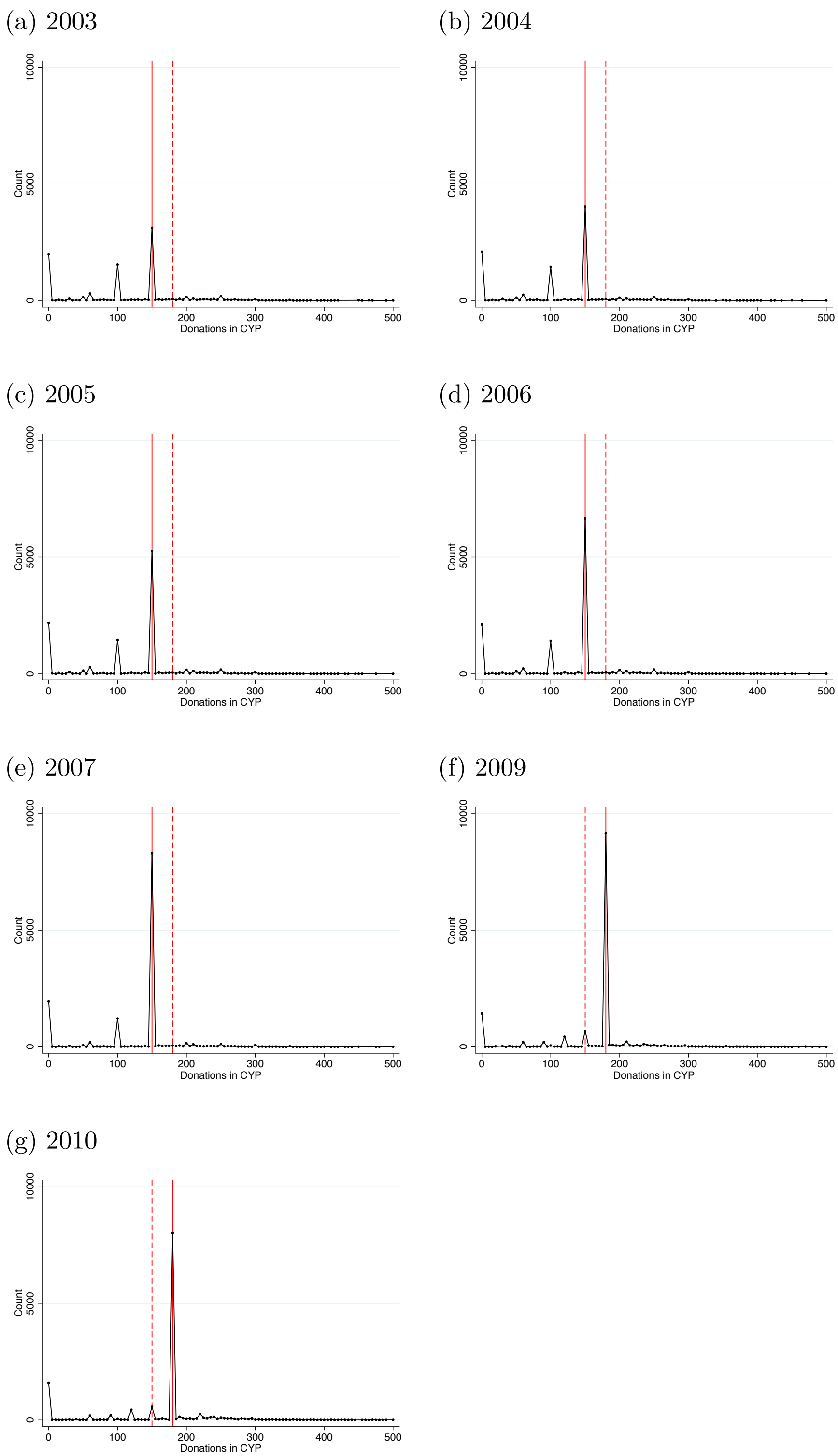
Notes: This figure shows the bunching dynamics of positive donations among salary earners between 2003-2010. It plots the yearly empirical distribution in bins of width CYP 5, together with the estimated counterfactual. Each sub-figure reports the normalised excess bunching mass b around the CYP 175 threshold. Bootstrapped standard errors are in parentheses.

Figure 13: **Donations Between 2003-2010 of Those Bunching at CYP 150 in 2007**



Notes: This figure shows the empirical distribution of donations before and after 2007, for the sample of salary earners who bunched at CYP 150 in 2007. Vertical solid lines mark the relevant threshold that is in place in a given year (CYP 150 during 2003-2006 and CYP 175 during 2008-2010), while dashed lines mark the other threshold that has either been eliminated, or has not been yet introduced.

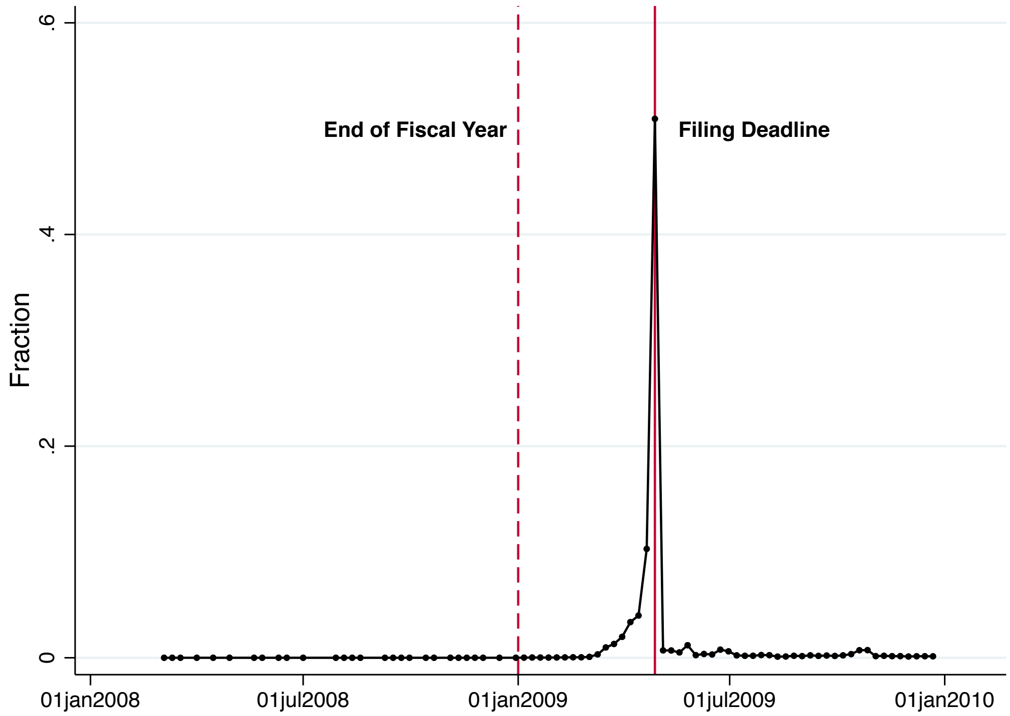
Figure 14: **Donations Between 2003-2010 of Those Bunching at CYP 175 in 2008**



Notes: This figure shows the empirical distribution of donations before and after 2008, for the sample of salary earners who bunched at CYP 175 in 2008. Vertical solid lines mark the relevant threshold that is in place in a given year (CYP 150 during 2003-2007 and CYP 175 during 2009-2010), while dashed lines mark the other threshold that has either been eliminated, or has not been yet introduced.

Figure 15:

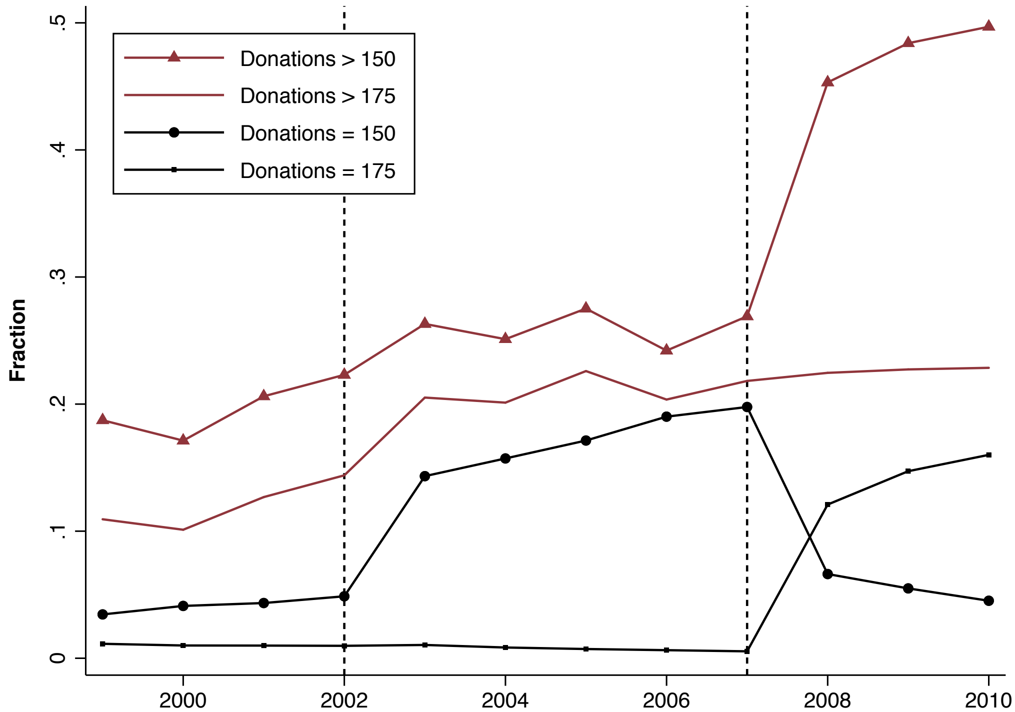
When did people file their taxes for 2008?



Notes: The graph shows, in weekly bins, the fraction of people filing their taxes for the fiscal year 2008. Vertical dashed and solid lines mark the end of the the fiscal year (31 December 2008) and filing deadline (30 April 2009) respectively.

Figure 16:

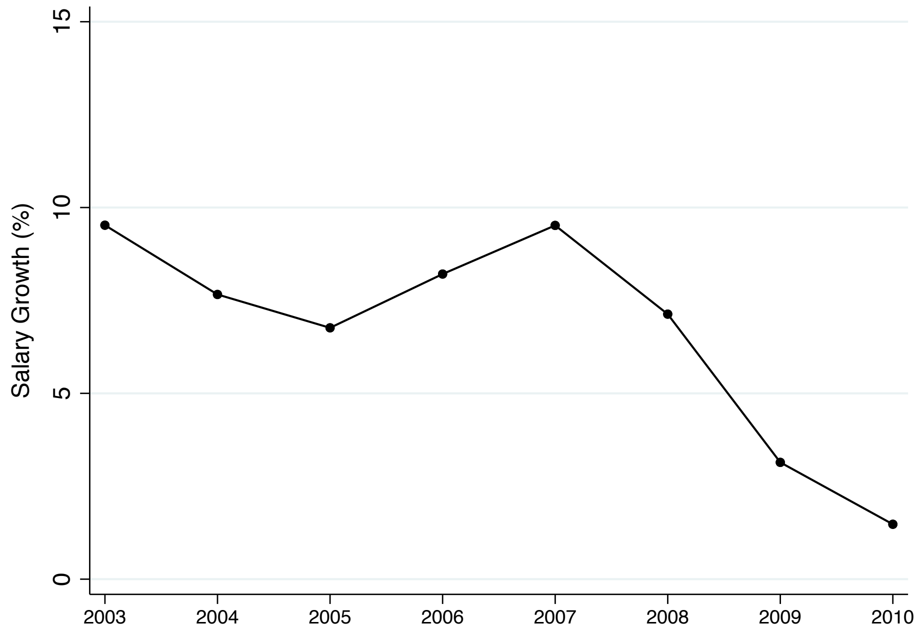
Fraction Reporting Specific Amounts of Donations Over Time



Notes: This figure shows the fraction of individuals reporting each of the following amounts of donations over time: over 175, over 150, 150 and 175.

Figure 17:

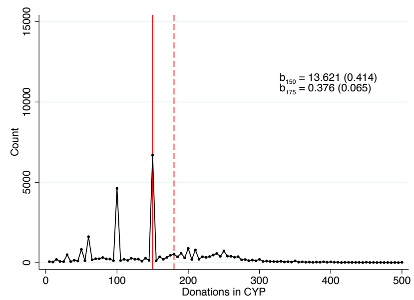
Salary Growth Rates of 2007 Bunchers



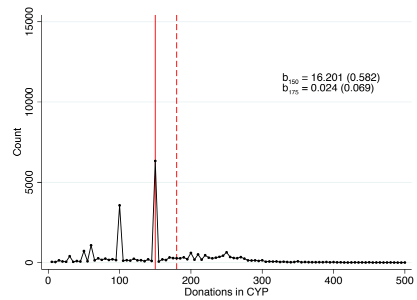
Notes: This figure shows the yearly salary growth rate between 2003-2010 of salary earners bunching at the CYP 150 threshold in 2007.

Figure 18: **Bunching around reporting thresholds, Excl. Highly Unionised Sectors**

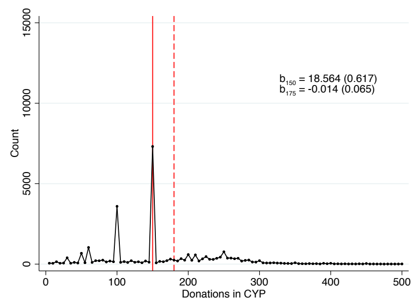
(a) 2003



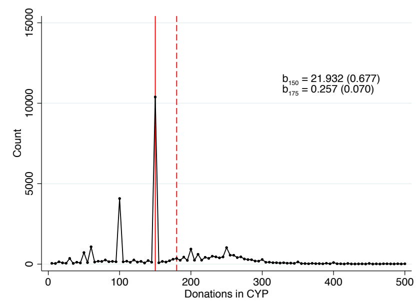
(b) 2004



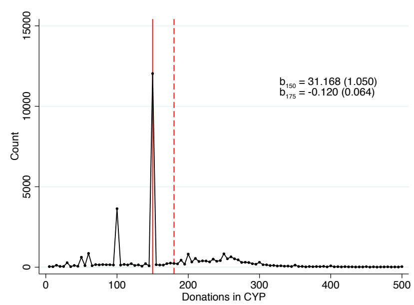
(c) 2005



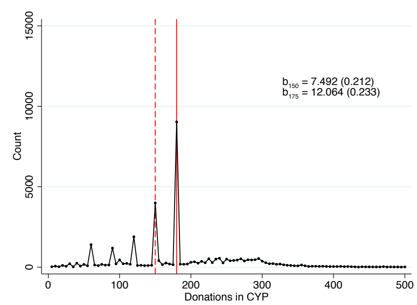
(d) 2006



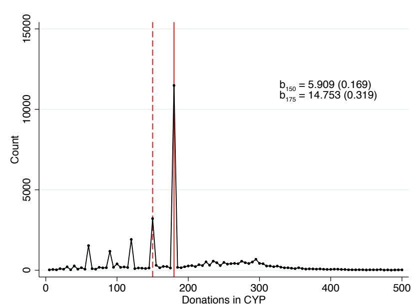
(e) 2007



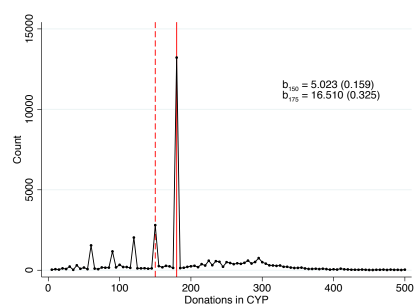
(f) 2008



(g) 2009

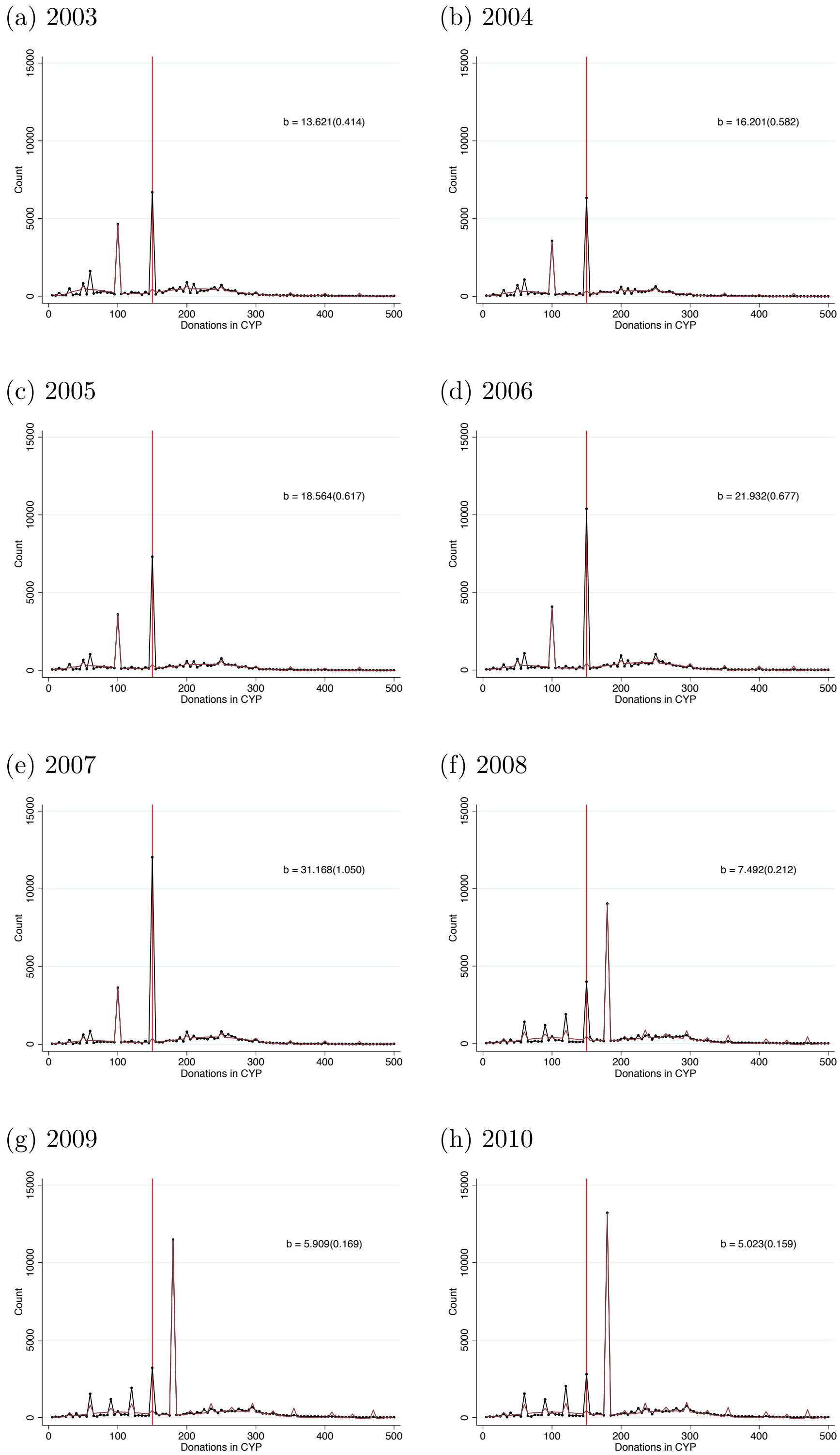


(h) 2010



Notes: This figure shows the bunching dynamics of positive donations among salary earners between 2003-2010 by plotting the yearly empirical distributions in bins of width CYP 5. The sample is restricted to those not in highly unionised sectors and drops those whose sector is not observed. Each sub-figure reports the normalised excess bunching mass b around the CYP 150 and CYP 175 thresholds, with bootstrapped standard errors in parentheses. Vertical solid lines mark the threshold that is in place in a given year, while dashed lines mark the other threshold that has either been eliminated, or has not been yet introduced.

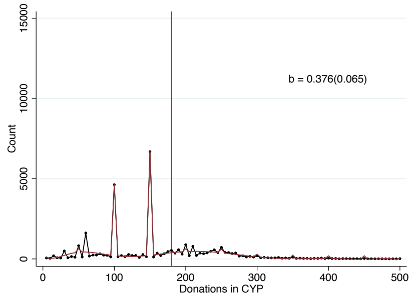
Figure 19: **Bunching at CYP 150 w. Counterfactual, Excl. Highly Unionised Sectors**



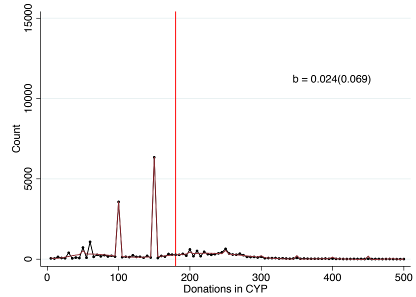
Notes: This figure shows the bunching dynamics of positive donations among salary earners between 2003-2010, restricting the sample to those not in highly unionised sectors and dropping those whose sector is not observed. It plots the yearly empirical distribution in bins of width CYP 5, together with the estimated counterfactual. Each sub-figure reports the normalised excess bunching mass b around the CYP 150 threshold. Bootstrapped standard errors are in parentheses.

Figure 20: **Bunching at CYP 175 with Counterfactual, Excl. Highly Unionised Sectors**

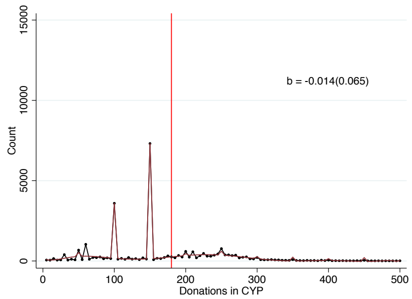
(a) 2003



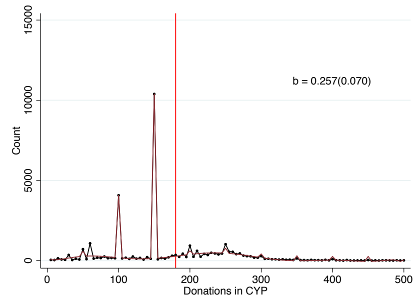
(b) 2004



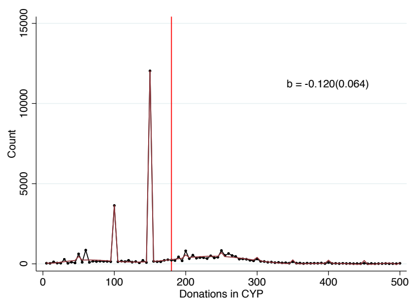
(c) 2005



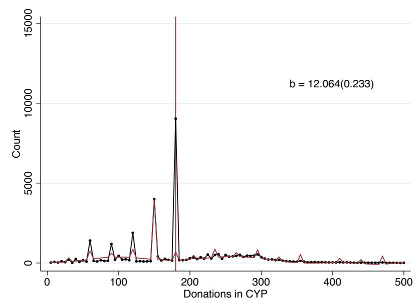
(d) 2006



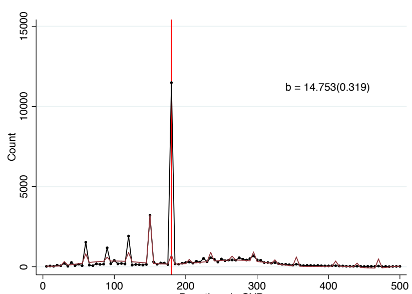
(e) 2007



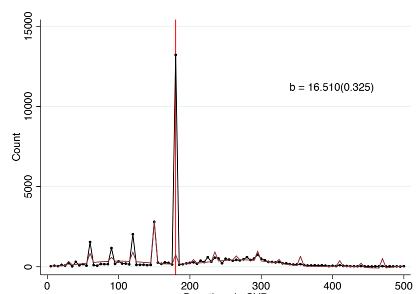
(f) 2008



(g) 2009



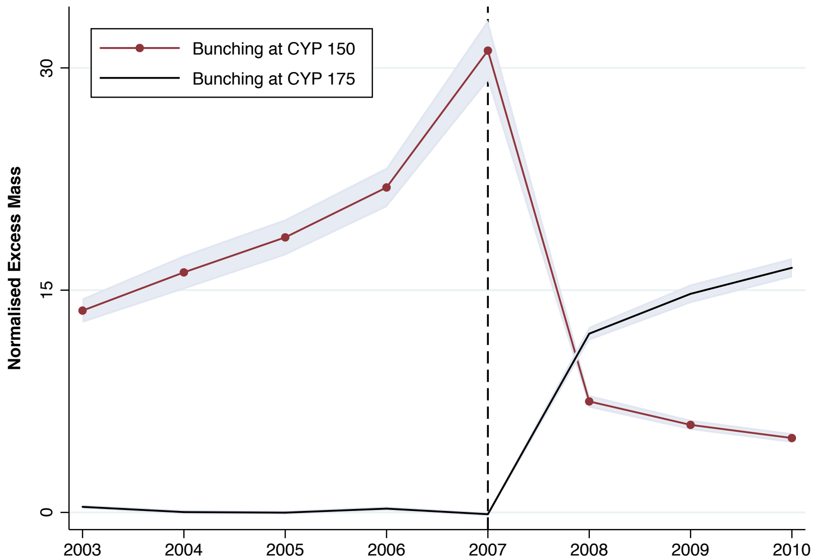
(h) 2010



Notes: This figure shows the bunching dynamics of positive donations among salary earners between 2003-2010, restricting the sample to those not in highly unionised sectors and dropping those whose sector is not observed. It plots the yearly empirical distribution in bins of width CYP 5, together with the estimated counterfactual. Each sub-figure reports the normalised excess bunching mass b around the CYP 175 threshold. Bootstrapped standard errors are in parentheses.

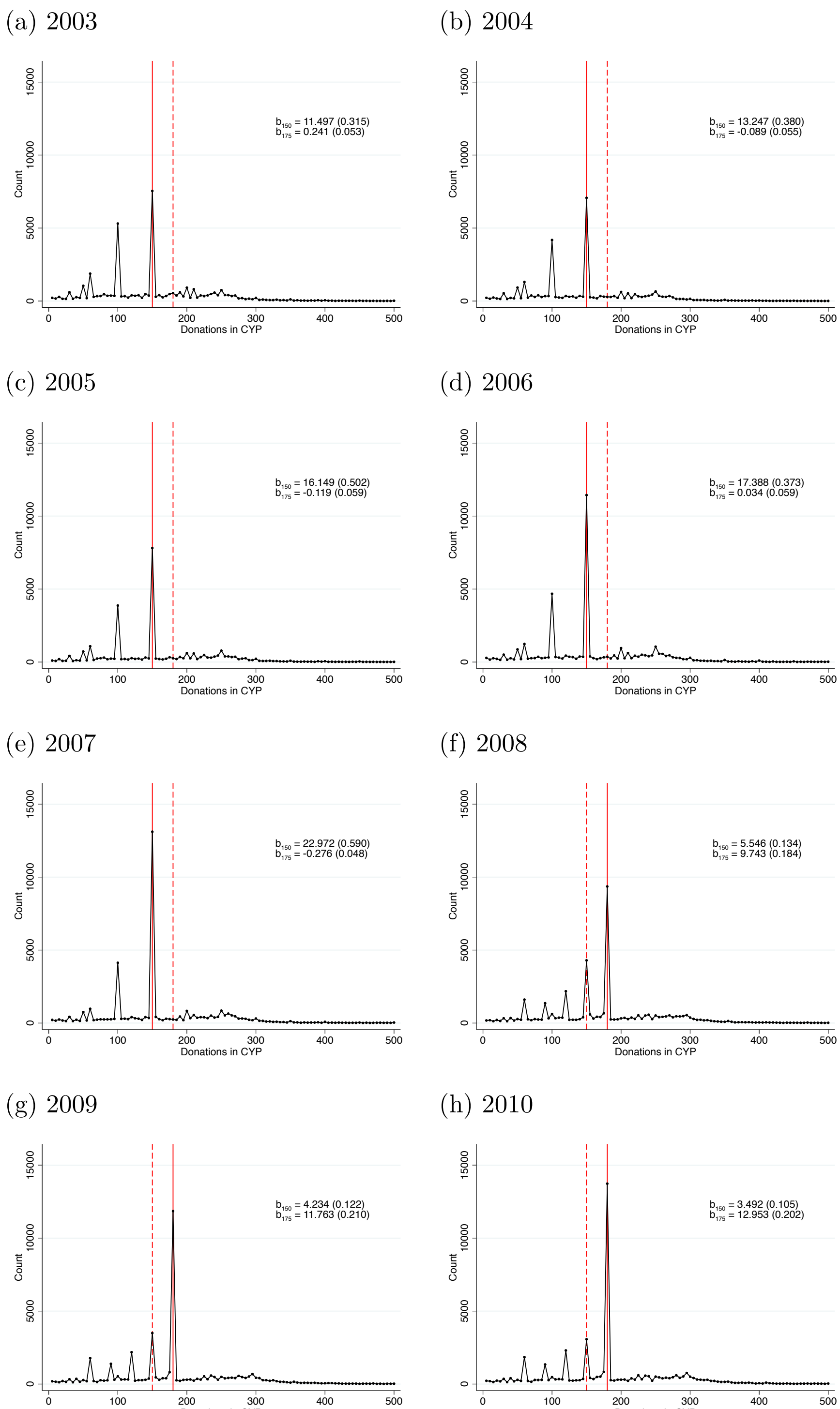
Figure 21:

Bunching Estimates over Time, Excluding Highly Unionised Sectors



Notes: This figure shows the estimates of the normalised excess mass around both the CYP 150 and 175 thresholds between 2008-2010, restricting the sample to those not in highly unionised sectors. The shaded areas demarcate 95% confidence intervals.

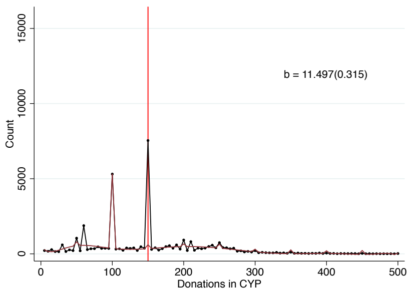
Figure 22: **Bunching of Donations, Removing Union Fees**



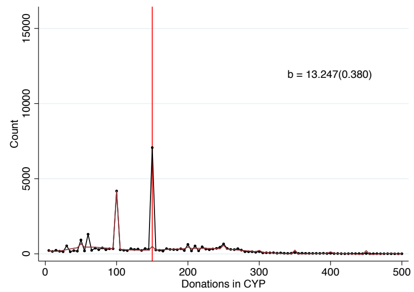
Notes: This figure shows the bunching dynamics of positive donations among salary earners between 2003-2010, by plotting the yearly empirical distributions in bins of width CYP 5. The sample is restricted to only those whose sector can be observed. For those in highly unionised sectors, the union fees have been removed. Each sub-figure reports the normalised excess bunching mass b around the CYP 150 and CYP 175 thresholds, with bootstrapped standard errors in parentheses. Vertical solid lines mark the threshold that is in place in a given year, while dashed lines mark the other threshold that has either been eliminated, or has not been yet introduced.

Figure 23: **Bunching at CYP 150 with Counterfactual, Removing Union Fees**

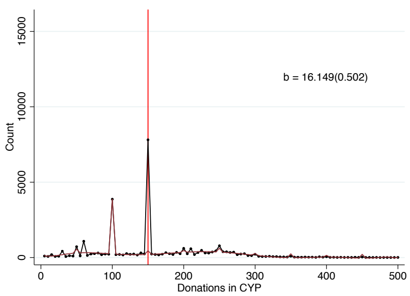
(a) 2003



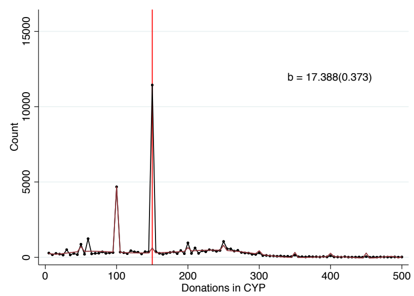
(b) 2004



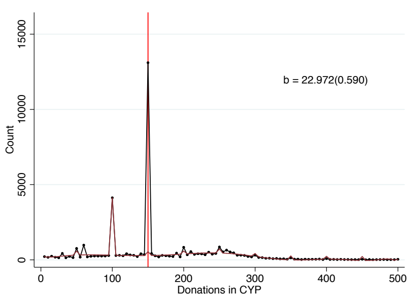
(c) 2005



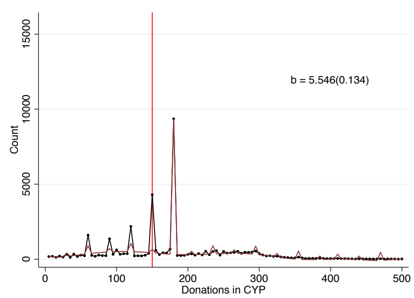
(d) 2006



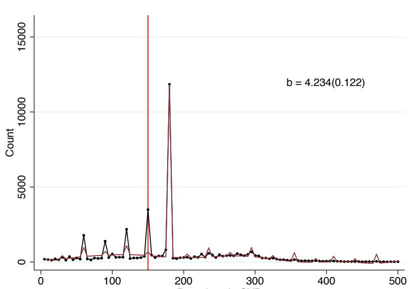
(e) 2007



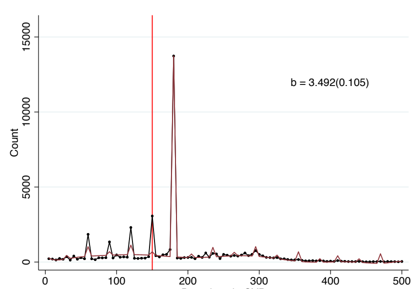
(f) 2008



(g) 2009



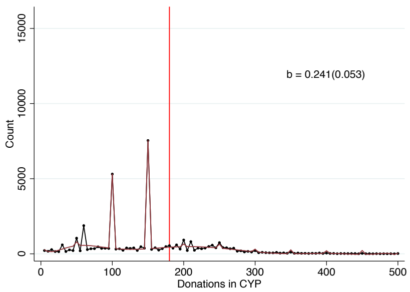
(h) 2010



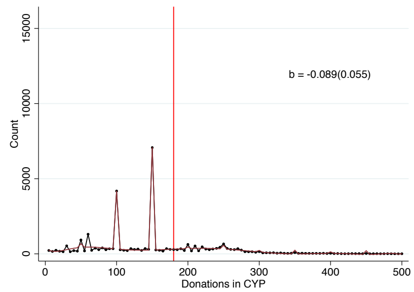
Notes: This figure shows the bunching dynamics of positive donations among salary earners between 2003-2010, for the main sample but restricted to only those whose sector can be observed. For those in highly unionised sectors, the union fees have been removed. It plots the yearly empirical distribution in bins of width CYP 5, together with the estimated counterfactual. Each sub-figure reports the normalised excess bunching mass b around the CYP 150 threshold. Bootstrapped standard errors are in parentheses.

Figure 24: **Bunching at CYP 175 with Counterfactual, Removing Union Fees**

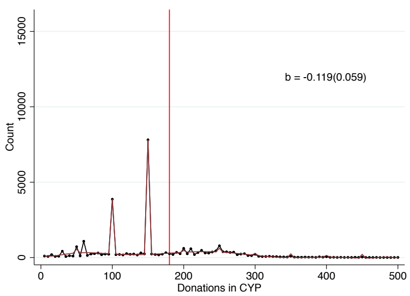
(a) 2003



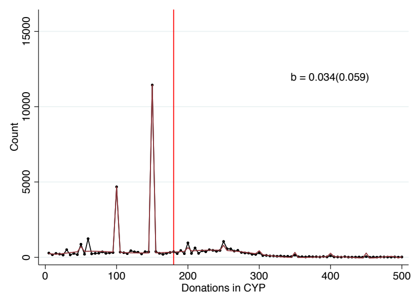
(b) 2004



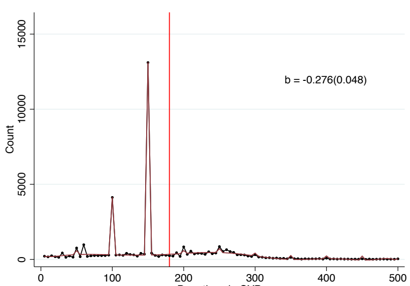
(c) 2005



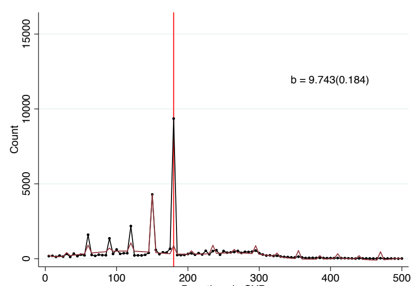
(d) 2006



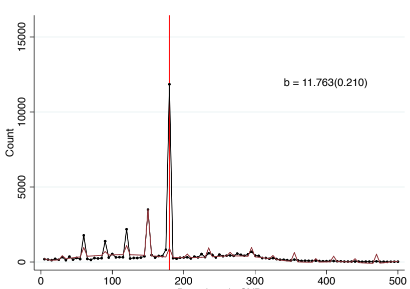
(e) 2007



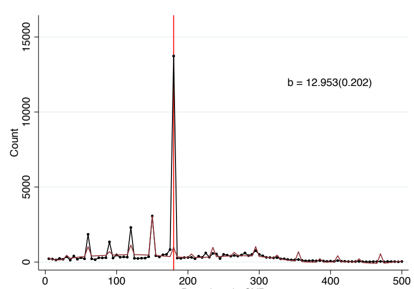
(f) 2008



(g) 2009



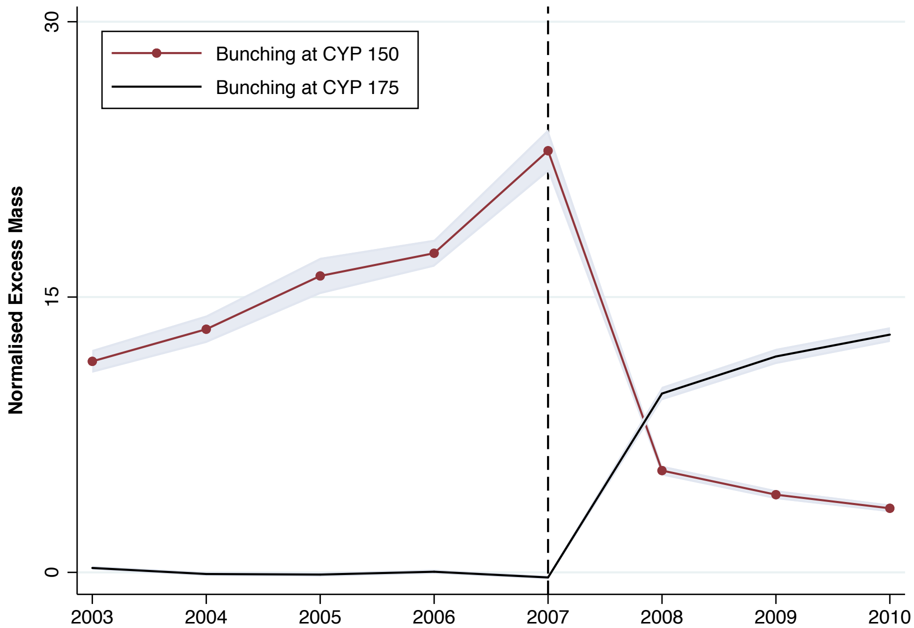
(h) 2010



Notes: This figure shows the bunching dynamics of positive donations among salary earners between 2003-2010, for the main sample but restricted to only those whose sector can be observed. For those in highly unionised sectors, the union fees have been removed. It plots the yearly empirical distribution in bins of width CYP 5, together with the estimated counterfactual. Each sub-figure reports the normalised excess bunching mass b around the CYP 175 threshold. Bootstrapped standard errors are in parentheses.

Figure 25:

Bunching Estimates over Time, Removing Union Fees

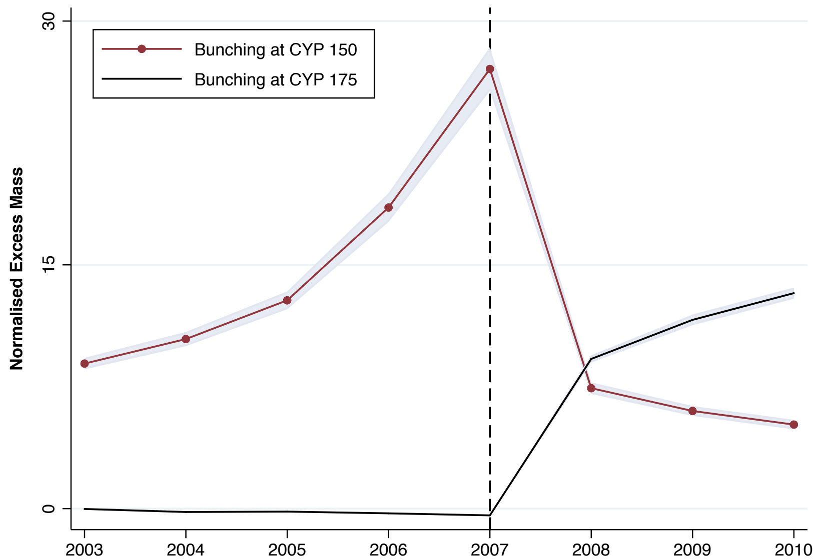


Notes: This figure shows the estimates of the normalised excess mass around both the CYP 150 and 175 thresholds between 2008-2010. The shaded areas demarcate 95% confidence intervals. The sample is restricted to salary earners whose sector is observed (but excludes the public sector), and the outcome variable has been adjusted for union fees among those in highly unionised sectors.

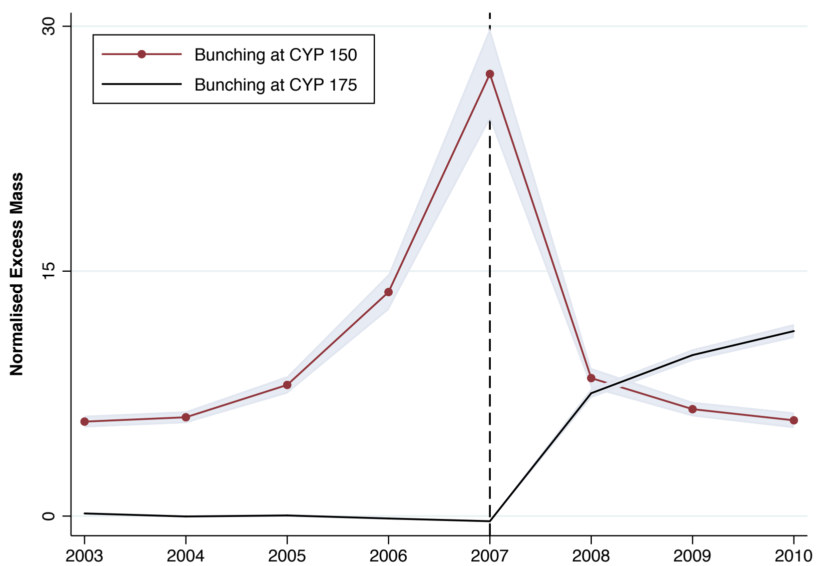
Figure 26:

Bunching Estimates over Time, Main Sample, Robustness Checks

(a) 9th order Polynomial



(b) Smaller Bandwidth

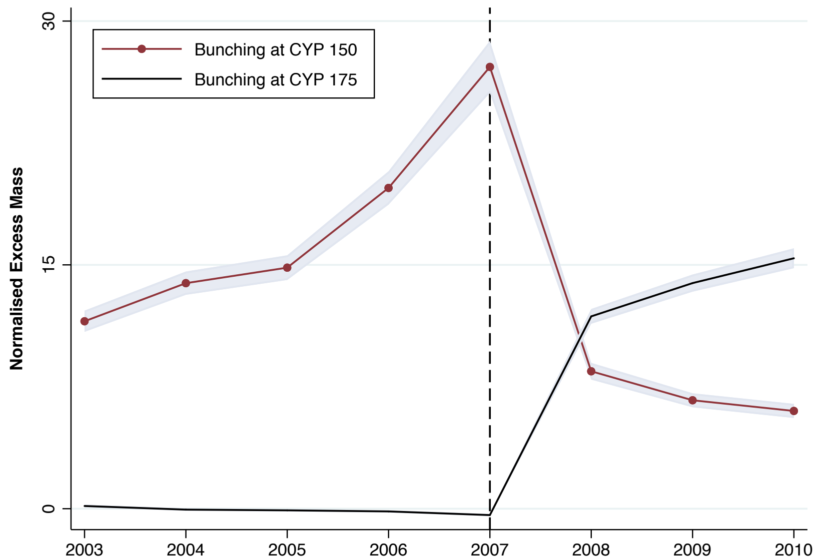


Notes: This figure shows robustness checks for the estimates of the normalised excess mass around both the CYP 150 and 175 thresholds, between 2008-2010. The shaded areas demarcate 95% confidence intervals. Sub-figure (a) shows results using a 9th order polynomial and (b) restricting the bandwidth to between CYP 50-350.

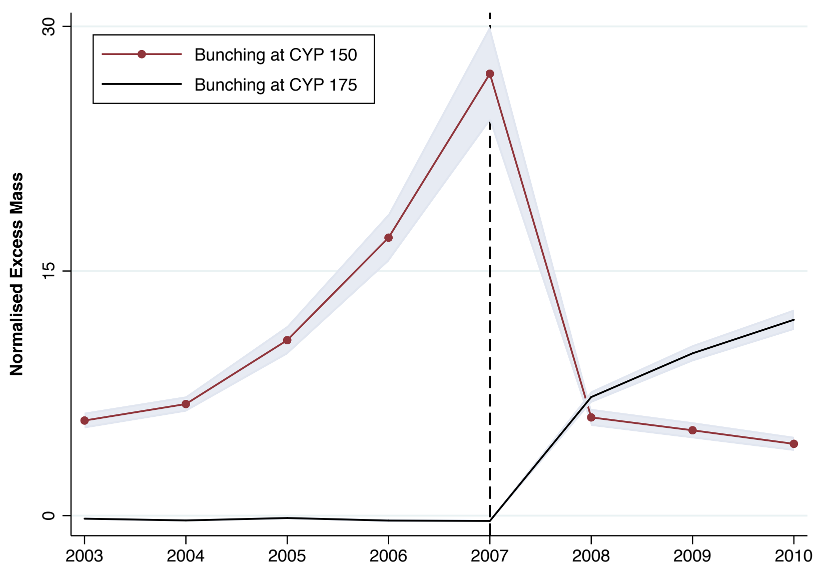
Figure 27:

Bunching Estimates over Time, Heterogeneity Analysis by Sex

(a) Males



(b) Females

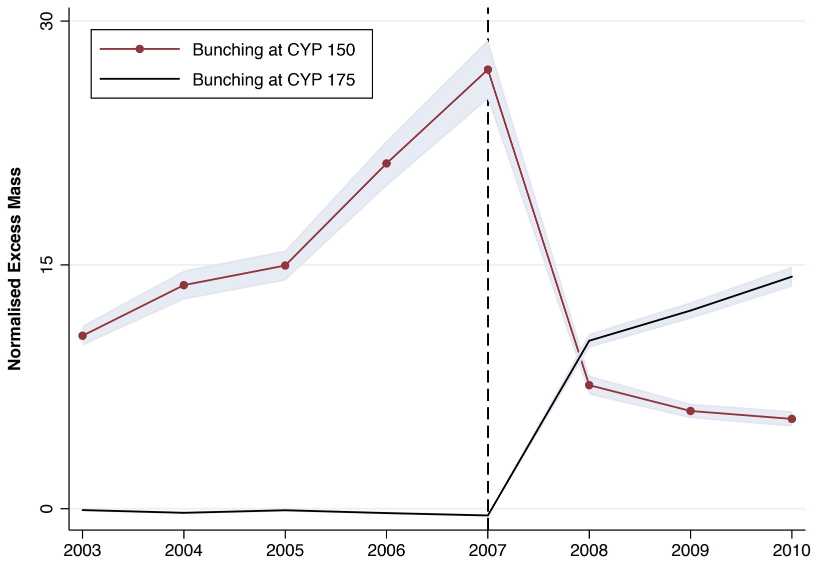


Notes: This figure shows the estimates of the normalised excess mass around both the CYP 150 and 175 thresholds, between 2008-2010. The shaded areas demarcate 95% confidence intervals. Sub-figure (a) restricts our main sample of salary earners to males and (b) to females.

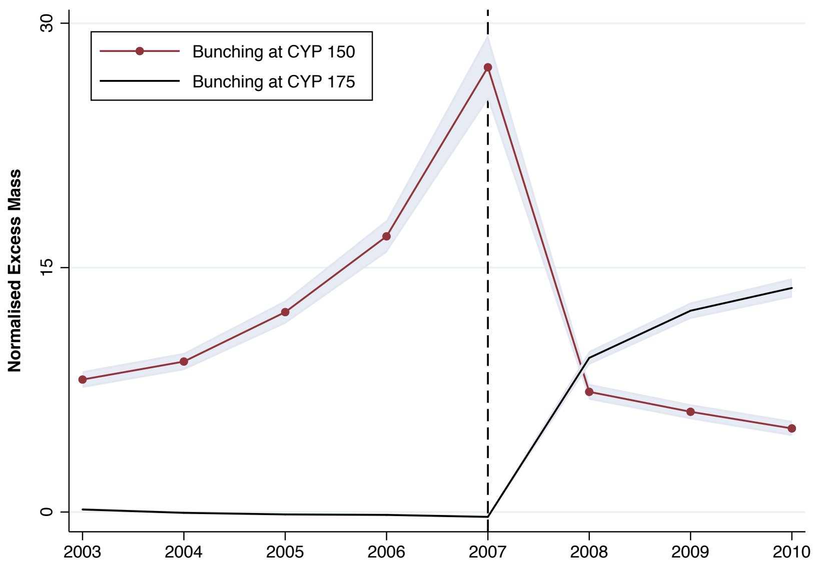
Figure 28:

Bunching Estimates over Time, Heterogeneity Analysis by Age

(a) Ages 25-39



(b) Ages 40-54



Notes: This figure shows the estimates of the normalised excess mass around both the CYP 150 and 175 thresholds, between 2008-2010. The shaded areas demarcate 95% confidence intervals. Sub-figure (a) restricts our main sample of salary earners aged 25-39 and (b) to those aged 40-54.