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Essays in Public Finance

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

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Statement of conjoint work

I confirm that Chapter 2 was jointly co-authored with Tim Besley and Torsten Persson [professors respectively at London School of Economics and IIES, as of June 2016], and I contributed 33% of this work. I confirm that Chapter 3 was jointly co-authored with Pierre Bachas [PhD Candidate at UC Berkeley, as of June 2016] and I contributed 50% of this work.

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This thesis is dedicated to my sister, Astrid.

Abstract

This thesis explores the determinants of tax evasion and their implications for tax policy, with a special focus on taxation in developing countries.

Chapter 1 studies how the transition from self-employment to employee-jobs over the long run of development explains the rise of the modern tax system. I construct a new microdataset, covering 90 countries at all levels of development today and 140 years within the US between 1870 and 2010. Using these data, I provide new stylized facts: within country, the share of employees increases over the income distribution, and increases at all levels of income as a country develops; 2) the income tax exemption threshold moves down the income distribution as a country develops tracking employee growth. I provide a causal estimate of the impact of employee-share on the exemption threshold and on tax revenue, by studying a state-led development program which was implemented across US states in the 1950s-60s. I find that the exogenous increase in employee share is associated with a lower state income tax threshold and higher revenue.

Chapter 2 studies individual and social motives in tax evasion. We build a dynamic model that incorporates these motives, their interactions, and where social motives underpin the role of norms. Our empirical analysis exploits the adoption in 1990 of a poll tax to fund local government in the UK, which led to widespread evasion, and a series of natural experiments due to narrow election outcomes, which induce shifts into single-majority local governments and lead to more vigorous enforcement of local taxes. The econometric results are consistent with the model's main predictions on the dynamics of evasion.

Chapter 3 studies the impact of access to formal finance and firm size on tax inspection and tax compliance. We use firm-level data on 108,000 firms across 79 countries in the World Bank Enterprise Surveys. We instrument for finance and firm size at the industry-level using an out of sample extrapolation strategy related. We find a large and positive effect of firm size on both tax inspection and sales tax compliance, but no overall significant impact of reliance on external finance.

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1 Employment Structure and the Rise of the Modern Tax System[1]

Abstract

This paper studies how the transition from self-employment to employee-jobs over the long run of development can explain growth in income tax capacity. I construct a new database which covers 90 household surveys across countries at different income levels and 140 years of historical data within the US (1870-2010). Using these data, I first establish three new stylized facts: 1) within country, the share of employees increases over the income distribution, and increases at all levels of income as a country develops; 2) the income tax exemption threshold moves down the income distribution as a country develops tracking employee growth; 3) the employee share above the income tax threshold remains high and constant at 80-85 percent. These findings are consistent with a model where a high employee share is a necessary condition for taxation and where the rise in income covered by information trails through increases in employee shares drives expansion of the income tax base. To provide a causal estimate of the impact of employee share on the exemption threshold, I study a state-led US development program implemented in the 1950s-60s which shifted up the level of employee share. The identification strategy exploits within-state changes in court-litigation status which generates quasi-experimental variation in the effective implementation date of the program. I find that the exogenous increase in employee share is associated with an expansion of the state income tax base and an increase in state income tax revenue.

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1.1 Introduction

Tax capacity grows as economies develop. This is true across today's developing countries, and historically within today's advanced countries. In this paper, I show how the transition from self-employment to employee-jobs explains growth in tax capacity. This increase in employee share is a defining characteristic of changes to employment structure over the long run of development. Micro evidence shows that transitions into employee-jobs are associated with improved compliance at the individual level by creating third-party information trails (Kleven et al., 2011), but this evidence has little to say about state tax capacity over the long run. Macro evidence provide correlations between employee shares and tax take (Besley & Persson, 2014; Kleven, 2014; Kleven et al., 2015), but this evidence lacks clearly identified empirical channels. To build a bridge between the micro and macro contributions, I propose a research design which combines descriptive evidence and quasi-experimental evidence. In this design, I empirically identify a new channel through which employee share impacts tax capacity along the development path. To implement the design, I construct a new dataset with microdata for 90 countries at all levels of development and 140 years within the US (1870-2010).

The novel channel explains decreases in the income tax exemption threshold through increases in employee share that occur further and further down the income distribution. To motivate the channel, Panel A of Figure 1 shows four countries at increasing levels of development [India, Indonesia, Mexico, US]. Within each country, it plots employee share across deciles of the income distribution and the location of the exemption threshold above which earned income becomes liable for taxes. In India, the exemption threshold is located in the top decile of the income distribution, the only one where employee share is high. As countries reach higher levels of development, the threshold gradually moves down as the employee-share goes up in deciles further down the income distribution. This close co-movement is also observed within the US over time (Panel B, Fig 1). This paper explains the co-movements as the impact of increases in employee share on the exemption threshold.

Figure 1 shows that the statutory income tax base in US is 30 times larger than in India (Panel A). This large variation suggests the statutory threshold may be an important determinant of tax capacity. This observation complements previous papers that document a weak or zero correlation between statutory instruments and income tax capacity. While those studies have focused on variation in statutory tax *rates*, I study variation in the statutory tax *threshold*. This directly addresses the extensive margin of compliance of moving workers into the tax base. The focus on the threshold is consistent with development tax policy which emphasizes the extensive margin of compliance as an important driver of tax capacity (Keen, 2012)

I construct a new micro database containing information on type of work and income from nationally representative household surveys across 90 countries at all income levels and within the US between 1870 and 2010. This collection effort offers two main advantages. First, it allows me to shed new light on employment structures in countries at medium and lower levels of development (below $4000 per capita), where I source surveys directly from national statistics offices or government ministries. These surveys have a larger sample size and contain income data for a wider range of work-types than pre-existing surveys from public access licensed databases or external repositories. Second, it allows me to provide new evidence on long run changes to employment structure in the US pre 1900, where I draw on previously unexploited microdata produced by a joint effort between economic historians and IPUMS USA (Lindert & Williamson, forthcoming). My micro survey data offer the key advantage, relative to administrative records, that they permit studying employment structure for all types of work both above and below the exemption threshold.

In a first part of the paper, I provide three new stylized facts on employee shares and taxation. Stylized fact #1 shows that within country the employee-share is increasing through the income distribution, and over development the employee share is increasing in deciles locally further down the income distribution. I characterize stylized fact #1 in a quantitative exercise. I show that for each decile of the income distribution, there exists a level of per capita income beyond which employee growth in the decile stops and employee share reaches a steady state level. I estimate these per capita income levels for all deciles to find that: the income levels are decreasing in the deciles; and, the steady state employee share is constant across deciles at 80-85 percent.

The second stylized fact shows that the exemption threshold moves gradually down the income distribution in close co-movement with increase in employee share in the deciles locally to its left. The third stylized fact shows that the employee-share above the threshold remains constant and high at 82-85 percent at all levels of development. All three stylized facts are remarkably similar *in levels* when I compare a given US historical profile to a synthetic profile constructed from countries at similar levels of development. This robustness suggests a causal impact of employee share on the tax threshold.

In a second part of the paper I estimate the causal impact of employee share on the exemption threshold. Identification is based on exogenous timing in implementation dates of a US state-led development program. Through the Industrial Development Bonds program (IDB), states facilitated the transition into manufacturing employee-jobs by constructing debt-financed leasable industrial facilities in rural areas. Implementing IDB required the state House to vote in a legal amendment to exempt IDB from the constitutional 'public purpose' provision whereby the state may only enter the debt market for a public purpose. But the lack of historical precedent to the IDB amendment meant the program was considered legally uncertain until the highest state court would litigate to uphold it.

The estimation strategy uses within-state changes in court litigation status as identifying variation. It assesses impact by comparing changes in outcomes before and after the upholding event, relative to counterfactual changes before and after the vote in event within the same state. I provide two pieces of evidence to support this identification strategy. First, I confirm graphically that the effects I find are driven by sharp "on impact" changes around the upholding event with clear break from the vote in pre-trend. Pretrends are stable regardless of the length between vote in and upholding ($\geq$10 years in 40 percent of cases). Second, I show that, conditional on vote-in, the only significant predictor of faster timing to upholding is a time-invariant dummy for civil law origins. As an improvement upon cross sectional specifications, my empirical strategy remains identified even under time varying unobservable shocks *within state* to willingness or capacity to tax that coincide with the potentially endogenous policy period of vote in. Ultimately however, the causal interpretation of the results is driven by the exogenous timing of the upholding event, which I support by finding identical estimates in cross-sectional specifications (including paired synthetic matching) between IDB and non-IDB states.

I use changes in IDB court litigation status as an instrument to estimate the impact of employee share on the exemption threshold. I provide evidence to support the exclusion restriction that changes in IDB court litigation status impact the exemption threshold only through changing employee share. I first show changes in litigation status had no impact on proxies for other threshold determinants, including earnings structure, tax rate structure, enforcement capacity, demand for redistribution. Second, I find no impact of the upholding event on any other source of state tax revenue apart from income tax. Third, the program had no impact on labor force participation, migration, and sectoral spillovers. Fourth, I show that the impact on exemption threshold is sharp and occurs with a precise lag to the impact on employee share. Variation which violates the exclusion restriction would have to produce an immediate break from trend; occur with precise time lag to upholding event; not be captured by proxies; not impact any other tax instrument nor any other tax revenue.

In the first stage, I find that the program led to a large transition from self-employed to employee-jobs but had no impact on earnings structure, overall employment, rural migration, industry spillovers. I find that the IDB program led to a large reduced-form decrease in the tax exemption threshold and increase in (income tax/GDP). The instrumented estimates suggest that a one standard deviation increase in length of IDB program through exogenous changes in IDB court litigation status led to increase in employee share which account for 24 percent of the expansion of the income tax base and 10 percent of the rise in (income tax/GDP).

The US instrumented estimate is a weighted average of the causal impact of employee share on the tax policy over complier states at different initial levels of employee-share. It has predictive value in the cross-development setting for three reasons. First, the mechanisms underlying the instrumented estimate closely match the cross-development stylized facts: the IDB impact on employee-share led to a shift leftward of the employee-share profile in the income distribution (consistent with fact #1); the IDB decrease in threshold was driven by employee-share increases occurring locally to its left (fact #2); the employee-share above the threshold remained constant at 80-85 percent before and after IDB (fact #3). Second, I find that complying states' labor markets were characterized by large underemployment amongst self-employed in rural areas - thus matching a key characteristic of rural labor markets in developing countries (ILO, 2009). Third, growth in US states' tax take over time has been driven by the rise of personal income tax - thus matching the key cross-development tax fact. State-led industrial financing programs similar to IDB have been implemented in India, Mexico and South Africa, and are considered elsewhere in developing countries (UN Financing for Development, 2009)

The results can be rationalized in a model where government maximizes revenue from setting the income tax exemption threshold. Employees and self-employed differ in evasion cost due to differences in information trail coverage. An exogenous increase in employee-share around the threshold lowers the fiscal costs of a local threshold decrease. I predict the threshold location in the 90 countries of the cross-section in the database, allowing only the employee shares over the income distribution to vary across countries, and holding constant values of enforcement and administrative capacity, earnings, demand for redistribution and public goods. The predicted tax base can account for 62 percent of the observed growth in tax base size across the 90 countries.

The following section discusses related literatures. Section III describes the micro database and provides new stylized facts on employment structure and tax structure. Section IV identifies the impact of employee share on the exemption threshold. Section V provides a model to rationalize the results and quantify the importance of the employee-share channel. Section VI concludes.

1.2 Related literature

This paper is related to the micro and macro studies on information trails as a determinant of individual compliance and state tax capacity. Kleven et al. (2015) show theoretically that collusive behavior between employees and the employer is hard to sustain when there exist business records, making third-party information reporting by firms a powerful tool of tax enforcement. Gordon & Li (2009) show how information reporting by financial institutions can also improve tax enforcement. These models are supported by a large set of empirical studies in both developed and developing countries that show tax enforcement is affected by information reporting. Kleven et al. (2011) use Danish random audits to show that increases in information coverage associated with employee jobs dramatically improves income tax enforcement. Best (2014) shows, using matched employer-employee data from Pakistan, that third-party reporting also limits income tax evasion in a developing country. But third-party reporting coverage of transactions remains much less prevalent in developing countries. Carillo, Pomeranz and Singhal (2014) use a natural experiment from Ecuador to show limits to third-party information effectiveness when taxpayers can adjust on non verifiable margins. Kumler, Verhoogen and Frias (2015) show that third-party enforcement of Mexican payroll taxes works better in larger firms and when employees have stronger incentives to monitor employer wage reports. Naritomi (2015) uses a Brazilian reform to show that increased incentives and improved technology of firm monitoring by consumers leads to a a large increase in firms' reported revenues. In Chile, Pomeranz (2015) shows that randomized audit threats have less impact on transactions that are subject to double reporting from both buyers and sellers, indicating increased deterrence of evasion through double reporting. Finally, Bachas (2015) and Best et al. (2015) show, respectively in Costa Rica and Pakistan, that taxes based on turnover can be a useful alternative to corporate profit taxation because sales are easier to observe than profits. This paper provides micro evidence on changes in information trails related to employment-structure along the full development path.

The paper also relates to the literature on the determinants of government growth over development (see Cage and Gadenne (2015) for a comprehensive study of tax revenues in developing and developed countries). Demand side determinants of this growth include 'Wagner's law' whereby public goods have a income elasticity above one (see e.g. Musgrave, 1966); and, democratization and increased political power of the poor (Acemoglu and Robinson, 2000). Besley and Persson (2011, 2014) model investments in fiscal capacity as a response to demand for public goods and increased cohesiveness of institutions. This paper is more closely related to supply side studies that show how changes in economic structure impact the capacity to supply tax revenue (Bird and Oldman, 1964; Hinrichs, 1966; Kleven et al., 2015). I contribute by providing descriptive and identified evidence on a new tax policy channel through which economic structure affects the capacity to raise taxes.

The paper is related to the literature on changes in employment structure over development. Current evidence focuses on the cross-country stylized fact that self-employment declines over increasing levels of per capita income (Banerjee and Duflo, 2007; Gollin, 2008; 'Jobs' World Development Report, 2013; La Porta and Shleifer, 2014) Studies of structural transformation exploit both cross country and within country patterns, but focus on sectoral changes (review in Herrendorf et al., 2014). The exception is McNaig and Pavcnik (2014, 2015), who show that structural transformation in Vietnam 1990-2008 was accompanied by transitioning out of household businesses. This paper provides new evidence on the decline of self employment over development in larger cross-country and longer time series samples; and, at disaggregated levels over a country's income distribution. Using a previously unexploited US development program, I also contribute with identified evidence on an industrial policy's impact on sectoral and employment changes. I define self-employment versus employee-job based on whether the job generates an information trail relevant for tax enforcement. My self-employment category captures the informality category used by ILO (2009). Thus, this paper complements the literature on informality and development (La Porta and Shleifer, 2014), with micro based evidence on changes to informality over the income distribution along the development path. Finally, this paper's methodology relates to studies of macro economic changes using newly constructed micro evidence (Gollin, Waugh and Lagakos, 2013; Bick, Fuchs-Schundeln and Lagakos, 2015).

The paper also relates to studies of US states as laboratories in development and taxation. (Dincecco & Troiano, 2015; Gillitzer, 2013) While these papers study introductions of new tax instruments, I analyze determinants of state income tax exemption thresholds. I provide evidence on a previously unexploited state led development program, which differs from previous studies of US development programs, including Moretti & Kline (2013) in two dimensions. First, the program was narrow in scope as a stand-alone policy of industrial financing. Second, it was unique in its revenue-neutral funding scheme through issuance of revenue bonds. This type of state-led industrial financing is considered potentially important in developing countries, but there is little empirical evidence on its impacts (Platz, UN Financing for Development, 2009).

1.3 Descriptive evidence on employment structure and tax structure along the development path

I provide new stylized facts on changes to employee share and tax structure along the development path. I first describe the microdata and methodology. I then present descriptive and quantitative results.

1.3.1 Data and methodology

I construct the micro database from nationally representative household surveys. This represents two key advantages over alternative data sources. First, using household surveys allows a systematic study of changes to employee share over the income distribution which does not depend on the location of tax exemption threshold. This is in contrast to administrative records which typically measure earnings and type of work only for those above the exemption threshold. Second, using household surveys as opposed to firm surveys allows measures of types of work which are not restricted to activities for which individuals receive a wage, but cover all forms of self-employment and unpaid family work. This is especially important in less-developed countries where I document that the share of the workforce below the exemption threshold engaged in non-employee jobs is substantial.

I collected recent household surveys from 90 countries around the world at all levels of per capita income. I searched for surveys based on two main criteria: information on type of work and continuous measures of total gross earnings (as opposed to expenditure proxies) at the individual level; and, nationally representative coverage of all types of work. The first criteria allows construction of an income-distribution that is defined consistently across all countries. The second criteria ensures that all types of work are covered.

Many household surveys which are available as licensed data or through public access external repositories do not measure non-wage income.[2] This is the case, per example, for a significant number of Living Standards Measurement Surveys (LSMS). The absence of non-wage income measures in public access surveys is also more pronounced in least developed countries. Consequently, in these countries I sourced the surveys directly with the country's national statistics office, or in some cases, the Department for Planning. These surveys have an average sample size 6.5 times larger on average than the country's LSMS (and are on average much more recent). I chose a living conditions survey over a labor force survey whenever possible. This is because the latter sometimes exclude certain types of work such as casual daily wage labor or family businesses.

The result is 90 surveys in countries ranging from $125 per capita to $80250 per capita. In 2 cases, I rely on expenditure to measure income. In another 2 cases, the sample is only representative of the urban population. In all other cases, the survey is nationally representative and contains continuous measures of earned income from all types of jobs.

I construct the within country dimension of the database by combining new and previously used micro sources from the US. I collect data between 1950 and 2010 from Census microdata extracted via IPUMS USA. Before 1950, the Census did not record work type and continuous measures of income at the individual level.[3] I use the 1935-36 Study of Consumer Purchases, which was jointly con-

[2]Some datasets do contain step-function measures of non-wage income. I do not include such surveys in the database.

[3]Several US historical cross-sections of individual-level data contain binary measures indicating whether an individual earns non-wage income in excess of a given amount. These variables appear hard to map into a continuous measure of individual gross income, and for that reason I

ducted by Bureau of Labor Statistics, the Bureau of Home Economics and the Department of Agriculture. Considered the precursor to the Census methodology of data on income at the individual level, the survey was meant to "ascertain for the first time in a single national survey the earning and spending habits of inhabitants of large and small cities, villages, farms." (ICPSR study 8908, 2009) 300,000 households were interviewed based on sampling units chosen to represent the "demographic, regional, and economic characteristics of the United States." (ICPSR, 2009) Both the work type and income categories in the 1935 survey are consistent with the later Census-based definitions. In particular, the gross income variable contains continuous measures of wage-earnings, business income, and farm income. I provide further details on the ICPSR dataset in the appendix.

All national surveys carried out in the late 19th century and the end of the 1920s focused on sampling the work and living conditions of employed wage-earners.[4] To construct a historical pre-1900 profile of employment structure, I rely on previously unexploited data resulting from a collective effort between Williamson & Lindert (forthcoming) and IPUMS USA. Unlike previous pre-1900 estimates of US wealth which are built up from production or expenditure approaches (Berry, 1968; Gallman, 2000), Williamson & Lindert build their data from local personal income and work type records. To build the dataset, the authors used local tax assessments and occupational directories for 'registered occupations' and local censuses for 'unregistered occupations'. Labor force counts using the 1 percent US Census sample were provided specifically for the data-project by IPUMS USA. I use the Williamson & Lindert computations of gross earned income, which include wage income, farm income and non-farm business income. However, unlike the surveys from 1935 onwards which contain harmonized employee and self-employed variables, the 1870 data required building types of employment categories. I use a text search algorithm which exploits the highly detailed work titles from the enumerator instructions to the 1870 Census in order to construct self-employed and employee categories. Per example, the 1870 enumerators were explicitly instructed to "not call a man a 'shoemaker', 'bootmaker', unless he makes the entire boot or shoe in a small shop. If he works in a boot and shoe factory, say so (...) Cooks, waiters, etc., in hotels and restaurants will be reported separately from domestic servants." I discuss the US historical data in the appendix.

For each survey, I construct a nationally representative distribution of earned

choose not to use such datasets.

[4]Such as the "Cost of living in the United States, 1917-1919" (ICPSR 7711, 1986) and the "Cost of Living in the United States and Europe, 1888-1890" (Haines, 2006).

gross income over the subsample of respondents who declare being active in the labor force (ILO definition). I partition the distribution into ten deciles $s = 1, ..., 10$. Within each decile, I compute the agricultural share of total employment, ι_s, based on ISIC industry classification. Within non agriculture $1 - \iota_s$, I compute the self-employed and employee shares of non-agriculture employment, denoted respectively $(\varphi \mid 1 - \iota)_s$ and $(1 - \varphi \mid 1 - \iota)_s$. I define self-employed and employee shares such that φ and $1 - \varphi$ are mutually exclusive of $1 - \iota_s$. Together with ι_s , the three categories are jointly exhaustive and mutually exclusive in employment in decile s.

I focus on self-employed and employee shares outside the agricultural sector in order to study changes in employment structure amongst workers whose earnings are subject to income tax. The predominant practice in lower and middle income countries is to exclude agricultural earnings from the liable income tax base. I maintain the non-agriculture categories in high income countries where agriculture earnings are subject to income tax. I do this to ensure that variables studied remain the same across countries throughout the analysis of this section. Although in principle this is inconsistent with the definition of the relevant employment base for income tax in high income countries, in practice it has no impact. That is because agricultural employment is very small in high income countries. But importantly, as I document in appendix Figure 11, it is also because agricultural employment-share in high income countries is spread uniformly across all deciles.[5]

I code employment as self-employed versus employee on the basis of whether the work generates derivative information trails that can be used for income tax enforcement. In advanced countries, such information trails are mainly generated through third party reporting of employee wages by employers. In less developed countries where third-party coverage is limited, such information includes paper trails generated by contractual arrangements such as labor contract. This information trails definition of employees is conceptually consistent with the contractual definition of employees in Banerjee & Newman (1993).

Following this classification, I code an agent as self-employed if she responds working in a business without a registered employer. This category is mainly composed of small family businesses and domestic workers. I also code as self-employed any respondent who reports working as a casual daily-wage laborer. Naturally, I also include in the self-employment category all respondents who report working 'on their own account' or in a firm of size 1. This information trails classification produces a self-employment category which encompasses all

[5]It is interesting to consider why agricultural earnings are exempt from personal income tax at lower levels of development. One possibility would be that most agricultural production in less developed countries is carried out by self employed and very small units of production. According to the logic of this paper, the absence of information trails on the earnings in these units would make it worthwhile to exempt agricultural sources of income from taxation.

work categories in the ILO (2002) classification of informal employment: "self-employment in informal enterprises such as unregistered enterprises, employers and unpaid family workers; and paid employment from informal jobs such as casual or day labor and unregistered help for informal and formal enterprises, households or temporary employers."

A compelling feature of the methodology is that industry and type of employment categories are defined in a fully consistent way across all surveys in the cross-section and the within-section. In all surveys, I only code the type of work reported in the first job. This is usually defined as the the job defined towards which the largest number of working hours is used. In doing so, I omit variation in type of work status especially among respondents who may be best considered as having a portfolio of jobs (Banerjee & Duflo, 2007).

The interpretation is that employee work generates an information trail that can be used for income tax enforcement. To the extent that increases in enforceability is driven by movements into large firms, my employee category represents a consistent but 'fuzzy treatment' proxy for enforceable income.[6] If transition into employee jobs is associated with increases in gross income, then my employee share measure represents a lower bound on actual increase in enforceable income. On the other hand, if there is systematic under-reporting of earnings in the survey by self-employed, then the employee-share is an upper bound measure of enforceable mass in a given decile.[7] In the household surveys, I find no salient evidence of excess earnings mass locally below the threshold. I regard this as evidence that large misreporting of income the survey in response to location of the threshold is not a first order concern. This observation comes with the caveat that the sample sizes in the surveys do not have the power to fully rule out the existence of such underreporting behavior.

Finally, note that unobservable increases over development in the capacity to detect under-reporting of self-employment earnings would appear as a decrease in the employee share. Underlying growth in enforcement capacity thus works against my finding of gradual increases in employee share along the development path.

1.3.2 Results

I first document on changes to employee share using the cross-section of 90 countries. To build profiles of employee share representative at incremental de-

[6] Kleven et al. (2015) provide audit evidence from Denmark, which shows dramatic decrease in evasion rates on the 'extensive margin' between self-employed and employee groups, and on the 'intensive margin' between large and small firms.

[7] Or if there is systematic under-reporting in both categories, but self-employed under-report a larger fraction of true income.

velopment levels, I partition the sample of surveys into ten equal sized bins. The bins are based on the constant per capita income of the country-year in which the survey was collected. At each 'development level', I construct the profile of self employed share $(\varphi \mid 1-\iota)_s$, and employee share$(1-\varphi \mid 1-\iota)_s$, in each decile s of the income distribution as the weighted average over shares in decile s of countries that belong the the development bin. The country-weights are constructed as the country-survey representative sample share in the total sum of representative country samples in the development bin. These weights only vary across countries, not across country-decile. The resulting profile represents employee and self employed shares across the income distribution of the representative country at given development level.

Stylized fact #1: Within country employee share increases over the income distribution, and at all levels of income as a country develops

The results are reported in Figure 2. At the initially lowest level of development ($277 per capita), employee-share is concentrated in the top decile. Profiles for individual countries, such as India, suggest that even within the top percentiles, there is an extremely steep increase in the employee-share. As the first stylized fact, the panels show how transitions into employee jobs occurs in stages of development. Indeed, at each successive development level, the increase in employee share is concentrated in deciles gradually further down the income distribution (stylized fact #1). At low levels of development ($730 to $3286), increase in employee share is concentrated in the top third deciles. At middle levels of development, ($4638 to $13512), the increase in employee share is concentrated in the middle third deciles. Finally, at higher levels of development ($27596 to $53234), the increase in employee share is concentrated in the bottom third deciles.

The distributional patterns of increase in employee share are strikingly similar when comparing the long run historical evolution of the US to 'synthetic' profiles from the cross-section at similar levels of development. In Figure 3, I pool the historical US profiles and the cross-country profiles using the Maddison dataset. This results in a loss of 33 surveys from the cross-section for which the year is more recent than the latest Maddison year. For each historical US profile, I construct a paired synthetic country. This synthetic country is the average over countries for which the real per capita income in the survey-year lies within 10 percent of the per capita income of the historical US survey-year. Per example, India is included in the synthetic country that is paired with the US 1870 survey year. Remarkably, the US profile of employee share in 1870 on the eve of its second industrial revolution, matches closely the synthetic Indian-based profile in levels. Over the long run from 1870 to 2010, the US profiles systematically match the synthetic cross-country profiles in levels of employee share, and in trends of growth in employee share gradually further down the income distribution.

Stylized fact#2: Tax exemption threshold moves down the income distribution as a country develops in co-movement with increases in employee share

For all countries in the micro database, I extract the nominal value of the personal income tax exemption threshold from the tax code in the year of the survey. I locate it in the gross income distribution, as illustrated in Figure 1 using four different countries (panel A), and four points over time within the US (panel B). In all countries, I compute the size of the income tax base as 1 minus the percentile location of the exemption threshold. The size of the income tax base thus describes what fraction of the income distribution is liable in statutory terms to pay taxes. I calculate the employee share above the threshold as the employee-share over all percentiles that lie above the threshold. The second stylized fact describes the tight co-movement over development between increase in employee share occurring gradually further down the income-distribution and decreases in the exemption threshold (stylized fact #2). At lowest levels of development, the exemption threshold is systematically located in the top percentiles of the income distribution. At increasing levels of development, the increase in employee share to the left of the tenth decile is associated with decreases in the threshold. At highest levels of development, growth in employee share is concentrated in the bottom deciles where the threshold moves down. Through this co-movement with employee share, the threshold gradually decreases such that the size of the income tax base progressively increases over development. This is shown across 90 countries in the LHS of Panel B. Remarkably, the RHS of panel B shows that for a given level of development, the size of the income base is very similar when comparing US historical variation to cross country variation.

Stylized fact #3: Employee share above the tax exemption threshold remains constant and high at 80-85 percent as a country develops

The third stylized fact shows how despite large increases in the size of the income tax base, the employee-share on the income tax remains constant and high around 85 percent. In the LHS of Panel C, I cannot reject with statistical confidence that the employee share above the exemption threshold are the same in India and the US. This constant employee share on the tax base occurs despite the tax base being 30 times larger in the US than in India. The RHS of Panel C shows that, for a given level of development, the employee share on the tax base is the same in historical US and across countries.

These stylized facts are suggestive of the impact of employee share on the exemption threshold along the development path. In appendix Figure 12, I show that the three stylized facts also hold within Brazil between 1970 and 2010. They are consistent with a mechanism where employee-share proxies for enforceable income, and growth in enforceable income through increases in employee share drive expansions of the income tax base.

The tight match in the stylized facts provides a bridge of external validity between the suggestive cross country patterns and a within country identified estimate. In Section 1.4, I provide a within country identified estimate of the impact of the employee share on the exemption threshold.

To complement the descriptive facts, I quantify the relationship between employee share and levels of development, separately for each decile of the income distribution. I adapt a regression method used to quantify 'development-stages' of structural processes (Imbs & Wazciarg, 2003)

In order to quantify a fully flexible relationship, I attempt to impose as little structure on the functional form as possible. This motivates the use of a non-parametric regression method, which is locally robust[8] and allows me to recover the estimated coefficients that I use to for later estimation.[9]

The method is adapted from robust locally weighted scatterplot smoothing ('lowess'). In the cross country sample, I partition the data into S subsamples according to overlapping constant per capita income intervals of size $J = \$1,000$, with an overlap of size $\triangle = \$250$. Each interval thus has a midpoint which is \$250 away from the following midpoint. For each decile, I run a regression on each subsample of the decile-specific measure of employee-share on per capita income. I use the estimated coefficient on per capita income and constant values to plot the fitted value against the income midpoint of each estimation subsample.

The resulting curve for a given decile j yields a robust non-identified shape of the evolution of employee-share in every decile j over development. I plot the curves for all deciles in Appendix Figure 14. I can statistically confirm stylized fact #1. At low levels of development, growth in employee share is stronger in the top decile and is significantly different from growth in other deciles. Over increasing levels of per capita income, the statistically significant growth in employee share occurs in deciles gradually further down the income distribution.

More interestingly, the curves suggest that beyond a real per capita income level which is specific to each decile, growth of employee share in a given decile has come to a halt and employee-share in the decile has reached a steady-state level. The regression technique allows me to robustly estimate these decile-specific per capita income points. I calculate the decile-specific per capita income point by setting to 0 the derivative of the predicted decile-specific employee-share with respect to per capita income. Per example, at real \$7500 per capita, employee growth is estimated to have come to a halt in the 6th decile. At this

[8]This is unlike polynomial/semi-parametric functions where the shape of the curve at one point can be determined by shape of the curve at other extreme points.

[9]Other smoothing methods simply compute the mean of y for subsamples of data centered around x_s. This distinction is important because I am not only interested in the shape of the general relationship between employee share and per capita income (which is delivered by these other smoothing methods), but also in the sign and the statistical significance of the coefficients.

per capita income level, the employee share predicted by the statistical curves has reached 85 percent. At higher capita income levels, the predicted employee share remains constant at this steady state level. I estimate the 'steady state' per capita income specific to each decile. I find that the per capita income points are smoothly but steeply decreasing in all deciles. Remarkably, the fitted curves further suggest that the steady state employee share is constant across deciles, ranging between 80 and 85 percent.

I repeat the exercise for the sample of US states over time. The panel structure allows me to include state fixed effects into the subsample regressions. I can therefore calculate the 'steady state' real per capita incomes within the average state over time. I find that the estimated real per capita incomes are also smoothly decreasing in the deciles, but they exhibit less curvature than in the cross country setting. This difference in curvature is similar to the findings in Imbs & Wacziarg (2003) who finds that patterns of sectoral concentration are more pronounced across than within countries.

I use the fitted curves and estimated 'steady state' per capita levels in the model section.

1.4 Direct estimate of impact of employee share on exemption threshold and tax collection

In this subsection, I provide a causal estimate of the impact of employee share on the exemption threshold and income tax revenue. I first provide background information and details of the development program. I then discuss data and identification strategy. Finally, I present graphical and regression results.

1.4.1 Background and program details

US states are a compelling setting to study development of tax systems. Each state defines and enforces a wide range of tax bases. Historically, the large growth in state tax-to-GDP ratios (Wallis, 2000) has been entirely driven by an increase in personal income taxes (Panel A, Figure 5). The rise of the modern income tax system in individual states matches the key tax capacity stylized fact (Besley & Persson, 2014; Kleven et al., 2015).

In parallel to the rise of the modern income tax, individual states also witnessed large changes in employment structure over time. Panel B of Figure 5 shows the non agricultural employee-share of total employment along the income distribution of the average state over time between 1950 and 1980. The employee-share is increasing in the income distribution, and over time the employee share increases in all income deciles. This is consistent with stylized fact #1. Panel B shows the state exemption threshold gradually moves down the income distribution in co-movement with increases in employee share locally to its left, consistent with stylized fact #2. Finally, the employee-share above the threshold remains constant at 85 percent over time, consistent with stylized fact #3.

Throughout this section, employee and self-employed are calculated as shares of total employment that includes agriculture. I do this in order to follow the same criteria for calculating employee shares as in Section 1.3: in the US, agriculture has never been exempt from state income tax base. I focus, however, on changes in non-agricultural employee share to maximize comparability with employee share in Section 1.3. This choice also highlights that results are not confounded by movements out of agriculture. Indeed, I show that the development program led to no change in volume of agricultural employees, but a large movement out of self-employed farming. Including agricultural employees into the employee share variable does not affect results.

To establish a direction of causality from employee share to the exemption threshold, I exploit exogenous variation in implementation date of the Industrial Development Bonds (IDB) program. Through the IDB program, the state built leasable manufacturing facilities in rural counties characterized by underemployed self-employment. (Area Redevelopment Administration Commission, 1963; Cobb, 1993) The IDB program thus acted as a level-shifter in employee-share.

Financing of the IDB program was directly incompatible with the state 'public purpose' Constitutional provision, whereby government debt may only be issued for public purpose. Implementation therefore required the state House to vote in a legal act which exempted IDB from the public purpose provision. But there was no legal historical precedent to such development program. The voted act and by extension any program funding would therefore remain legally uncertain until the highest state court would litigate to uphold the legality of the IDB program through a specific court case (Pinsky, 1963; Abbey, 1966; Rollinson, 1976; Cobb, 1993).[10]

The time-lag between the vote-in and the upholding events was substantial. The lag has mean of 6.67 years and standard deviation of 6.77. In 40 percent of cases, the time-lag exceeded 10 years. I digitize archived Moody's state financial

[10] A large Federal reform in 1968 made large changes to IDB regulations. To maintain comparability, I therefore limit the treatment definition to the set of states that implemented the IDB program prior to the reform. Because my estimation strategy relies only on within-state variation, this sample choice has no bearing on the construction of a counterfactual and hence on the estimates. It does, however, condition results on the sample properties of the pre-reform IDB states.

records on issuance of IDB debt. I use this data in Figure 6 to show that upholding was a necessary condition for any issuance of IDB debt. This observation holds amongst states with the longest time lag. This suggests that timing of IDB implementation cannot be explained by systematic implementation delay since vote in due to constrained administrative capacity.

1.4.2 Data

I construct employee shares of employment from decennial Census between 1910-2010. I construct counts of the working labor force. I construct counts of non-agriculture and agriculture class of workers using the Census class of worker categories 'self-employed' and 'works for wages' and the Census 1950-based industry classification. I construct the employee share as the ratio of non-agriculture employees to agriculture and non-agriculture workers. The self-employed share is constructed as the ratio of all self-employed workers to agriculture and non-agriculture workers. I interpolate the numerator and denominator between Census years using a natural cubic spline (Herriot & Reinsch, 1973). The main results are robust to collapsing all data to a simple pre-post sample.

Continuous measures of employee counts are retrieved from 'State and Area Employment, Hours and Earnings" collected by the Bureau of Labor Statistics. The series provides number of employees in non-agriculture by industry categories in state-years 1939-2002. Volumes of earnings by industry and type of work (employee, self-employed non farming, and self-employed farming) is from the historical series "SA5H: Personal income by major component and earnings by industry" produced by BEA in all state-years 1929-2005. I use the BEA series to construct continuous measures of employee and self-employed shares of gross individual earnings. Gross earnings excludes all government transfers and taxes, and non-work income (dividends, interest). I combine the historical BEA and BLS series to construct continuous measures of average employee earnings, and average employee earnings by major industries. I combine historical BEA series with interpolated Census data to construct average workforce earnings, and average self-employed earnings.

Tax-to-GDP measures between 1929-2010 are based on state government finances published by US Census. GDP is proxied for by using state personal income. I use US historical state tax calculator 1900-2007, constructed by Bakija (2009), to construct measures of the state PIT-base and the state PIT-rate structure. I am grateful to Bakija for providing me input from the calculator. I calculate the state income tax exemption-threshold, K, for an individual earner who files as being single, reports having one dependent, and claims the standard deduction.

I measure dates of vote in and upholding of IDB-program independently across legal reviews and Federal administrative records. I define the year of IDB-upholding as the year where the highest court-instance in the state upholds IDB through a particular court case. Leading cases cited are consistent across legal reviews.

I digitize the full set of Book of the States 1935-2010 (Council of State Governments). I record the number of agencies administering major taxes: this ranges between 1 and 6. I construct the ratio of annual administrative salary in revenue-taxation relative to annual salary as Treasurer; as Attorney General; and, as average state government administrator. I uses these variables to proxy for investments in enforcement capacity. I use political outcome variables from Besley et al. (2010). I use the Democratic vote-share across all state elections, the Democratic seat share in the state Houses, the existence of a Democratic Governor as measures of increasing demand for redistribution. I use the Besley et al. (2010) measure of political competition.

I construct the distribution of average income across deciles of the state income-distribution by combining Census data 1950-2010 and the 1935-36 Consumer Survey. The income by decile is interpolated across missing years. I digitize the series of significant provisions of state unemployment insurance laws (Department of Labor, 1937-2009) to construct a measure of firm size coverage of state UI.

The sample used for the main set of regressions is a panel of the 48 continental states, between 1939 and 2005. A more detailed description of all variables and the data-sources can be found in the appendix.

1.4.3 Identification strategy

The estimation strategy exploits institutional features of IDB implementation. I use changes in court upholding litigation status as identifying variation, and assess program impact by comparing changes in outcomes before and after the upholding event, relative to counterfactual changes before and after the vote in event within the same state.

This specification estimates the causal impact of the IDB program on employee share under the identifying assumption that the vote-in period represents a valid counterfactual for the upholding period. That is, absent the IDB program, the outcome of interest would have been on parallel trends throughout the vote in and upholding periods within the same state. I provide two main pieces of evidence to support this identifying assumption.

First, I find that the only significant predictor of the time lag is a state specific time invariant dummy for civil law origins. Table 1 reports the results from non-parametric Cox proportional hazards models. These models use state time-varying and time-invariant regressors to predict the conditional probability of the upholding event occurring, conditional on the vote in event having occurred. The civil law dummy significantly predicts a higher conditional probability of upholding. None of the economic variables (manufacturing share of labor force, employee share of labor force, 'redevelopment'-share of the labor force, log per capita income, log population), political variables (political competition measure, existence of a poll tax and or literacy tests as voting restrictions), taxation variables (size of exemption threshold, share of income tax to GDP, share of total tax to GDP), geographical variables (dummy for Southern region) predict significant changes in the conditional probability, once the civil law origins dummy has been included. This variable is drawn from Berkowitz & Clay (2005), and codes a state with civil law origins if, by the time of American acquisition, its colonizers had a civil law legal system (as opposed to a common law system).[11] The faster time to uphold associated with civil law states is consistent with studies across US states (Berkowitz & Clay, 2005) and across countries (La Porta et al., 2008). [12] I show that the civil law residual time lag (appendix Figure 15), and the civil law dummy (appendix Table 5), are both uncorrelated with outcomes in a pre-IDB cross-section.

Second, I plot changes in outcomes before and after the upholding event and confirm that the effects I find are driven by sharp on-impact changes. I show that the outcome of interest is trending in a stable manner over the full pre-event interval regardless of the length of the time-lag between vote in. I show there is no discernible change in the years immediately preceding the upholding event. Any secular trends are largely eliminated by the inclusion of a state-specific linear trend in the empirical specification.

Using within state identifying variation alleviates identification issues related to cross-sectional estimates in the current setting where vote in is likely endogenous to the economic and political environment. Centering treatment around the upholding event also overcomes issues of fuzzy treatment due to legal uncertainty during vote in. The within-state specification in 1 controls for any state-time varying unobservable shocks to political and economic environments which

[11] Ten of the continental American states were settled by France, Mexico or Spain and had civil law legal systems by the time of the American Revolution. These ten states are: Alabama, Arizona, Arkansas, California, Florida, Louisiana, Mississippi, Missouri, New Mexico, Texas. The 38 other had a common law system or were unsettled. Note that an additional five states - Illinois, Indiana, Michigan, Ohio, Wisconsin - were also originally settled by a civil law country, but were acquired by Great Britain prior to the American Revolution.

[12] One interpretation of this result, drawing on Berkowitz & Clay, is that civil law produces a Constitution with more statutory components, rather than framework provisions, and that the existence of statutory laws created more frequent demand for constitutional change among affected groups as the political and economic climates change over time. This explanation is consistent with the difference in IDB-litigation procedures observed across states: civil law origins states were more likely to vote in statutes, as opposed to Acts, which was likely to being revised more quickly.

occur at time of vote in and which are common to vote in and upholding periods. Estimates are lower bound on the causal impact if there was any program impact during vote in.

The objective of this section is to obtain an estimate of the effect of employee share on the exemption threshold which is not affected by simultaneity or omitted variables. To achieve that goal, I instrument for employee share using changes in court IDB litigation status.

I show in the following subsection that court IDB litigation status has a large impact on employee share, with an break from trend immediately 'on impact'. The previous subsection showed that the only significant predictor of change in litigation status was a dummy for civil law origins. This evidence suggests changes in employee share caused by exogenous change in IDB litigation constitute a valid first stage.

In order for changes court IDB litigation status to be a valid instrument, it must also satisfy the exclusion restriction. That is, changes in court IDB litigation must only affect location of the exemption threshold through changes in the employee share. I provide several pieces of evidence to support this claim. First, I show that change in litigation status is not correlated with proxies for enforcement capacity, tax rate structure, earnings structure, demand for redistribution, political competition. These variables are chosen based on the exemption threshold model derived in the following section. I show that these proxies are meaningful confounders, in the sense that they strongly correlate with location threshold and tax revenue collections. Second, I show the program has no impact on revenue from any other tax base apart from the income tax. This suggests court litigation status did not lead to any change in revenue requirements from the issuing of IDB debt, which could also have caused a decrease in threshold. This is consistent with the nature of the IDB debt, which was issued as revenue bonds that only pledged repayment of principal and interest against the future income derived from the leasing of the IDB facilities, not against the 'full faith and credit' of the state. Third, the program had no impact on economic outcomes which are closely associated with changes in employment structure and correlate with tax capacity, including overall labor force attachment, migration rates, sectoral spillovers. Fourth, I show in the following subsection that the change in litigation status led to a sharp change in exemption threshold, with a consistent lag in timing relative to the impact on employee share.

Together, these pieces of evidence narrow the possible variation that could violate the exclusion restriction. Such variation would have to produce an immediate and sustained break from trend; impact the exemption threshold and income tax revenue with precise sequential time lags to the upholding event; not be captured by any of the proxies; have no impact on any other tax instrument nor any other tax revenue.

The baseline empirical specification is

$$y_{st} = \beta + \alpha \mathbf{1}\left(\text{Vote in}\right)_{st} + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \lambda \mathbf{X}_{st} + \mu_s + \gamma_t + \phi_s \cdot t + \varepsilon_{st} \qquad (1)$$

where s denotes state, t denotes time, $\mathbf{1}\left(\text{Vote in}\right)_{st}$ is a dummy indicating whether the state has voted in the constitutional amendment, and $\mathbf{1}\left(\text{Upheld}\right)_{st}$ is a dummy indicating whether the state-court has upheld the legality of the constitutional amendment. All main results reported include a state-specific linear trend, $\phi_s \cdot t$, and a set of political controls (dummies for the existence of voting rights restrictions and for election years), policy environment (dummy for the existence of right-to-work laws and continuous measure of the firm-size coverage of state unemployment insurance schemes) and log per capita income. Results are fully robust to replacing the state linear trend with interactions between three cross-sections of structural determinants calculated in 1930 and a linear trend.

All standard errors are clustered at the state level to allow for correlation over time within a state. I confirm that a block-bootstrap at the state level yields similar significance levels. I also implement non parametric tests of significance levels using permutation tests, and find similar results. Bertrand et al. (2004) Significance is also robust to ignoring all time series information by collapsing data to a two-period pre-post DiD specification.

The causal interpretation of the results is based on the exogenous timing of the upholding event. I confirm this interpretation by showing that results are identical in cross-sectional specifications, both standard and paired synthetic matching, which estimate program impacts off a non-IDB counterfactual.

In the main results, I use the full sample of years and states to allow precise estimation of covariates in 1. Results are robust in a sample of IDB states in a small interval around vote in and upholding.

1.4.4 Employment results

In this subsection, I show that court upholding of the IDB program led to an increase in the non-agriculture employee share, but had no impact on earnings structure. This provides the first stage of the IV.

I begin with some some graphical evidence. Figure 7 provides evidence on an immediate and large increase in employee-share upon upholding. The full set of IDB states have been grouped according to the time-lag between vote in and upholding: 0 to 5 years; 5 to 10 years; 10 to 15 years; in excess of 15 years. Relative to a normalized outcome value of 1 in the year of vote in, I calculate the average within state change in trend before and after the event, across the four groups. Centering the trend graphically around the year of vote in both provides a test for any endogenous impact around the vote in event where my data suggests the IDB program was not active. It also provides a simple but real test of any impact around the upholding event which is free from visual bias. In these graphs I use the employee-share of earned income.[13] I do this because it is available on a continuous basis before and after vote in and upholding. In the regressions, I find no impact of upholding on continuous measure of average employee earnings. This suggests an increase in employee share of earnings can be interpreted as increase in employee share of employment.[14]

Within each group, there is a systematic break in trend and increase of employee-share immediately following the upholding event. In contrast, within each group the employee-share is smoothly trending through the vote in event - with the exception of the the group for whom vote in and upholding coincide. There is no visible change in pretrend in the years immediately preceding the upholding event. This is neither the case when comparing the same group to itself over successive intervals of the vote in period, nor when comparing across groups in given years since vote in. Finally, the trend since vote in is small and almost identical across groups up to ten years after the vote, prior to upholding. This suggests confounding differences between early and late upholding groups in initial employee-share at time of upholding is not a first order concern.

In Figure 8 I provide a direct graphical equivalent of the estimation strategy in 1. I assess changes in outcome before and after the upholding event and compare them to changes before and after the vote in event within the same state. I focus on the subset of states in which the time-lag is in excess of 15 years, and define a post vote period of 10 years and a (non overlapping) pre upholding period of 5 years.[15] I make these choices in order to construct visually long periods around each event. I show in appendix Table 10 that the results hold when constructed using a subsample with smaller time lag (and hence a larger set of states). Panel A shows an immediate increase in employee share upon upholding. The pre-trend is stable up until the upholding event.

[13]This variable excluded all transfers received from Federal and state government. The denominator contains all sources of earned income: employee farming, employee non farming, self employed farming, self employed non farming. The denominator excluces all sources of non earned income, such as dividends and interest payments.

[14]In the regressions, I also find that the increase in employee share of employment and the increase in employee share of earnings are almost identical. This further suggests that the impact on employee share uncovered in the graphs is driven by a change in employment rather than average earnings.

[15]Which represents around 20 percent of the sample of IDB states.

The first 9 columns of Panel A of Table 2 present results from studying changes in log volume of total employment in specific employment-industry categories, using 1. The program led to no change in overall employment levels, but a large transition from self employment into manufacturing. I find no impact of the program on volume of employee jobs in construction, trade, government, and employee agriculture. I uncover a marginally significant decrease in volume of services, suggesting a small switch between industries within the employee share. These results suggest the specification is precisely picking up the program impacts with a movement out of self-employed into manufacturing. The results are inconsistent with secular sectoral changes due to structural transformation, which would also predict movement into construction, retail and services. In appendix Table 6 I show that the the program did not lead to any changes in net migration rates, also consistent with the characteristics of the program. The last two columns of Panel A show that these changes in employment volumes map into a 3.6 percentage point increase in the non-agricultural employee share of total employment. The increase is entirely accounted for by a corresponding decrease in self-employment share. Given the specification, note that these are lower bounds on the true program impact.

Panel B of Table 2 shows that the program was not accompanied by any discernible change to the gross earnings structure. It did however lead to an increase in volume of employee employee-related amenities. The program led to no change in gross overall earnings, nor in total volume neither per member of the workforce. The program led to an increase in volume but not in average earnings of employee-jobs, and a mirrored decrease in volume but not in average self-employment earnings. The program increased employee-related amenities, but had no impact on other state transfers. The historically relevant employee-specific amenities included state unemployment insurance.[16] Such employee-targeted amenities are consistent with current practices in developing countries (Gerard & Gonzaga, 2014). The last columns of Panel B show changes to volume and average earnings sources translate into a 4.29 percentage point increase in the employee-share of gross resident earnings.

The employment and earnings results are consistent with a classical Lewis

[16] At its onset in 1938, UI extended only to private firms employing eight or more persons at least 20 weeks a year. By 1958, coverage was broadened to include firms employing one to three employees. Only in 1978 was coverage extended to agricultural firms employing a minimum of 10 workers in at least 20 weeks a year or having a $20,000 quarterly payroll, and to employers paying a quarterly minimum of $1,000 to domestic workers (Bureau of Labor Statistics Handbook of Methods, 1997).

model (1954) in a setting of a local rural labor market. In this setting, the IDB program can be interpreted as an exogenous positive shock to available capital which shifts out initially constrained demand for manufacturing employee jobs. The setting is characterized by existence of large cohorts of potential workers relative to constrained capital stock. This generates 'underemployed' individuals who are self-employed in order to satisfy subsistence needs. The workers have infinitely elastic supply to the manufacturing sector. These characteristics fits the description of the the typical IDB-targeted areas. In this model, self-employed make the transition into employee-jobs even if the newly offered wage is no higher than pre-existing earnings. This is because the transition is associated with monetary gains due to available employee-specific (non taxable) amenities. It can also be due to non-monetary utility gains from moving to a more secure income stream. Consistent with my results, the model predicts that the IDB program will lead to a large movement from self-employment to low-skilled employee-jobs; have no impact on average earnings in manufacturing nor overall earnings; and, increase employee specific amenities. [17] Accounting for the equalization of average earnings, my employment and income results are fully consistent with other findings on development programs in the US, noticeably Kline & Moretti (2013) on the Tennessee Authority program.[18]

I find no change to overall volumes of total employment, suggesting the transition was not driven by movements into the labor force of initially unemployed. The finding of no significant spill over to other sectors, both in terms of employment and earnings, suggests limited importance of general equilibrium effects. This finding is consistent with Federal and academic reports which document that IDB spurred local plant expansion of pre-existing firms which operated in low-skill industries such as textile and food-processing. These low-skill firms mainly exported goods to out of state consumer markets. Finally, the simple model outlined above abstracts from effects due to income accruing to IDB plant owners. If owners consume outside local markets (perhaps because they physically reside elsewhere), one would expect no such impact. I find no impact on retail and services at the state level. This is consistent with anecdotal evidence

[17] One key test of the model is whether earnings in employee and self-employment jobs are equalized prior to the IDB program. I use county level data between 1940 and 1950 to confirm this to be the case. To fit the test of equal earnings as closely as possible to the model, I compare earnings in counties likely to be treated under the IDB program. I rely on Federal administrative reports (Area Redevelopment Administration, 1963), which coded all counties in the US as 'redevelopment areas' if they satisfied the two IDB criteria: excess under-employment of self-employed, farmers; lack of private credit supply. Using the 'redevelopment counties' as proxy for IDB counties, I find that in IDB counties a t-test cannot reject equality of average self-employed and average employee earnings.

[18] In particular, they also uncover a large movement from agriculture into manufacturing. They find that aggregate income does increase, arguing that this occurs because manufacturing paid higher wages than agriculture. Differences in income results between their study and mine could arise from underlying differences in types of counties studied - indeed, I find that in non-redevelopment counties, the equalization of earnings across self-employment and employee-jobs does not hold.

that the IDB-program was dominantly taken up by firms whose headquarters were located outside the IDB state. The local labor market model seems to account for effects of IDB at the level of the IDB state, but not at the national level.

1.4.5 Tax exemption threshold results

In this subsection, I study impacts on the exemption threshold. I first show a reduced form relationship between the IDB program and the threshold. I then estimate the impact of employee share on exemption threshold where I instrument for employee share using changes in IDB court litigation status (from Section 1.4.4)

The graphs in panel B of Figure 8 document on a reduced form impact of the program on the exemption threshold. In the LHS graph of Panel B I show that the program led to active tax policy change, through an increased likelihood of passing a legislative reform to the nominal value of the exemption threshold. [19] The break from trend is salient, and occurs with a two year lag relative to upholding. In the RHS graph of Panel B, I find a sharp decrease in the ratio of the exemption threshold to average earnings. This ratio proxies for the relative position of the exemption threshold in the income distribution, where a decrease implies the threshold has effectively moved down the distribution. The decrease in location of threshold could come both from a rightward shift in earnings for a constant nominal value of the threshold,[20] and from a real reform that lowered the nominal value of the threshold. The break from trend in threshold coincides exactly in timing with the year of break from trend increase in the likelihood of a legislative reform. This suggest the upholding event led state policy-makers to actively expand the income tax base through lowering of the exemption threshold. In the regressions, I find that upholding had no impact on earnings.

The break from trend impacts combined with stable pre-trends are simple evidence of a reduced form impact of IDB court upholding on a policy change to the location of the tax exemption threshold. The timing of sequential changes suggests the decrease in exemption threshold was driven in part through increase in employee share.

[19] This normalized likelihood is constructed as state specific empirical cumulative distribution of number of legislative reforms passed over the full sample period. I code a year as legislative reform whenever the nominal value of the exemption threshold breaks from the previous year. This coding is not confounded by inflation-indexing to the exemption threshold, which states started implementing in the late early 1980s, well beyond the average year of IDB upholding.

[20] Similar to the bracket creep phenomenon occurring in periods of inflation with non-indexed threshold values.

I modify the baseline regression specification to include a full set of average incomes z by decile j in state s in year t. The new specification controls in a flexible way for movements in the earnings distribution which may have impacted changes to the threshold. Formally, I estimate the following model

$$[K/y]_{st} = \beta + \alpha \mathbf{1}\,(\text{Vote in})_{st} + \theta \mathbf{1}\,(\text{Upheld})_{st} + \sum_{j=1}^{10} \omega_j z_{jst} + \lambda \mathbf{X}_{st} + \mu_s + \gamma_t + \phi_s \cdot t + \varepsilon_{st} \tag{2}$$

where $[K/y]_{st}$ is the ratio of the nominal value of the exemption threshold to the average gross resident earnings. Note that a decrease in the ratio implies a lowering of the threshold location in the income distribution. In Table 3, Col. 1, I find a large, statistically significant decrease of -.7218 in $[K/y]$, which translates into a 18.23 percentile decrease of the location of the threshold in the average IDB state's pre-period income distribution.

Maintaining the reduced-form specification, I provide three pieces of evidence which directly match the cross-development stylized facts from Section 1.3. I estimate the program impact on employee-share separately for all deciles j in the income distribution using 1 and study the coefficients $\widehat{\theta_j}$. Panel A of Figure 9 plots $\widehat{\theta_j}$. Panel B plots the implied post-IDB employee-shares $\theta_j^{POST} = \widehat{\theta_j} + \theta_j^{PRE}$ where θ_j^{PRE} is the average employee share in decile j calculated in the pre-IDB period in the IDB states. The panels compellingly show that the distributional impact of the program was to shift to the employee-share profile leftwards, resembling closely the stylized fact #1. The panels also locate the implied post IDB percentile location of the threshold calculated as $K^{POST} = K^{PRE} + \widehat{dK}$. $\widehat{dK} = -18.23$ corresponds to the reduced-form estimate. K^{PRE} is the average percentile location in the pre-period in the IDB states. Inspecting θ_j^{POST} relative to K^{PRE} makes clear that the program lead to increase in employee share exclusively *below* the pre-location of the threshold. Combining θ_j^{POST} and K^{POST} provides strong evidence that the threshold tracked growth in employee-share occurring locally to its left, which matches stylized fact #2. Finally, comparing K^{PRE} and $\theta_{j>K^{PRE}}^{PRE}$ with K^{POST} and $\theta_{j>K^{POST}}^{POST}$ suggests that before and after the large changes in employee share and location of the threshold, the employee share above the threshold remains constant, consistent with stylized fact #3. Col 4 of Table 3 confirms in a regression that employee-share above the threshold remained constant upon upholding.

Having demonstrated in the previous subsections a relationship between IDB court litigation status and employee share, as well as a reduced form relationship between such litigation changes and the exemption threshold, I now apply instrumental variables to estimate the elasticity of the exemption threshold with respect to employee share.

I assume that changes in exemption threshold and in employee share are determined according to

$$[K/y]_{st} = \beta + \varphi \text{Employee-share}_{st-1} + \sum_{j=1}^{10} \omega_j z_{jst} + \lambda \mathbf{X}_{st} + \mu_s + \gamma_t + \phi_s \cdot t + \varepsilon_{st} \quad (3)$$

The first stage estimates changes in employee share from changes in IDB court litigation status according to 1. If changes in employee share predicted by changes in IDB court litigation status provide a valid first stage, and if changes in court litigation status only affect the exemption threshold through employee share, then the IV-estimated φ^{IV} in 3 provides a causal estimate.

Table 3, Cols.2-3 show the instrumented value of employee-share is negative and significant. It is one-third larger in magnitude than non-instrumented employee-share, a sign of simultaneity.

I provide evidence for the exclusion restriction assumption that changes in IDB court litigation only impacted the exemption threshold through changes in employee share. I use the formula for the threshold determinants, derived in the following section, to construct proxies for confounding channels. I show in appendix Table 7 that the proxies proxies meaningfully correlate with K and tax revenue. I proxy for changes in enforcement capacity in two ways. First, a decrease in the number of tax departments responsible for collecting distinct sources tax revenues reflects improved administrative capacity to centralize and cross-check information sources in order to enhance enforcement. Second, measures of wages of tax administrators relative to other high ranking administration officials reflects improved incentives for tax collectors. (Khan et al., 2015) I construct approximations to the earnings hazard ratio by using measures of average income by decile of the income distribution and the methodology outlined in Saez (2001). I proxy for demand for redistribution by using measures of democratic vote share at the state level across all types of elections, measures of Democratic seat share in the state House, a dummy for democratic governor, and the political competition measure in Besley et al. (2010). I show in the following subsection that there was no impact of upholding on any other source of tax revenue apart from income tax. This suggest the program did not led to a change in common marginal value of funds. Finally, I show that none of a range of statutory tax rate measures change with upholding.

Cols. (5)-(8) show that the program had no impact on the marginal bottom tax rate, the earnings hazard ratio proxy, the number of tax agencies proxy for enforcement, or the democratic vote share proxy for redistribution. Appendix Table 6 shows the change in litigation status did not either correlate with any of the other proxies for confounding determinants. I find in the following subsection that there was no impact of the program on any other source of tax revenue apart from income tax, suggesting no change to a marginal value of funds. This combines with findings from the previous section that the program had no non-employment impact on local economic structure as evidence in favor of the exclusion restriction.

Based on the instrumented employee-share value, I derive an elasticity of the exemption threshold with respect to employee share. I find a value of -7.548. This is close in magnitude to the elasticity implied by the ratio of reduced form impacts on employee share and the exemption threshold, which equals -6.678 (Column 1, Table 3).

The φ^{IV} estimate captures the local average treatment effect of employee share on exemption threshold for states that implemented IDB. I use the Federal administration's classification of counties as 'redevelopment areas' (Area Redevelopment Administration, 1962) to calculate the state's share of labor force in the 'redevelopment counties' characterized by underemployment of self-employed in rural areas. The mean 'redevelopment' share in IDB states, 16.5, is three times larger than in non IDB states. This suggests the IV estimates carry external validity in labor markets of less developed countries. Indeed, ILO (2009) considers underemployment as a defining characteristic of labor markets at lower levels of development. Furthermore, ILO documents that in developing countries, underemployment tends to be concentrated in rural areas with large proportions of self employed workers. The IV estimates may also be relevant in developing countries such as India, Mexico and South Africa, which have implemented industrial financing programs like IDB. (Platz, UN Financing for Development, 2009)

I use the instrumented elasticity to understand what extent of observed variation within the average IDB state over time in size of income tax base can be explained by (policy-led) increase in employee share. A one standard deviation increase in length of IDB program due to random changes in court litigation status leads to an increase in size of tax base via an increase in employee share which can account for 26% of the average IDB within-state standard deviation growth in tax base.

1.4.6 Income tax capacity results

In this subsection, I proxy for tax capacity by using the ratio of tax revenue from a given base to total state GDP. I show graphical and regression evidence of a reduced form impact on income tax capacity. I then estimate an instrumented elasticity of income tax capacity with respect to employee share.

Panel C of Figure 8 shows a sharp increase in income tax to GDP, with a three year lag to the upholding event. The increase occurs in the year following the legislative reform to lower the exemption threshold (Panel B of Figure 8). This sequential timing suggests the rise in income tax capacity was in part channeled through the decrease of the exemption threshold.

Using specification 2, I find a large, positive reduced-form impact of the program on personal income tax. Column 1, Table 4) I proceed to estimate the elasticity of [income tax/GDP] with respect to employee-share. I assume the second stage follows 3. Employee-share is again instrumented using variation in court litigation status. I find an instrumented value of employee-share which is one-third larger than the non-instrumented one. The instrumented elasticity of (income tax/GDP) with respect to employee share is .90. The IV-elasticity is smaller than the elasticity implied by the ratio of IDB reduced form impacts on income tax and employee share, which equals 1.292 (Column 1, Table 4)

This elasticity is a consistent estimate under the exclusion restriction that changes in court IDB litigation status only affect (income tax/GDP) through its impact on employee share. To support this claim, Cols.4-7 show that changes in court litigation status had no impact on the four major other sources of state tax revenue: corporate income tax, general sales tax, selected sales tax, and license taxes. This suggests the program did not impact any general marginal value of funds, either per se or through increased revenue requirements following the issuance of IDB debt. As discussed earlier, this is consistent with the type of IDB debt that did not pledge repayment against the state's own revenue. I show in the appendix table 8 that for selective sales and license taxes, the null result holds when considering specific categories (motor vehicle, tobacco, public utilities, alcohol).[21] In the previous subsections, I found no impact of change in court litigation status on enforcement and administrative capacity, earnings structure, tax rate structure, demand for redistribution. I also found that the sharp increase in income taxes after the upholding event occurred with consistent time lag to the increase in employee share. Variation which violates the exclusion restriction would have to occur precisely around the time of upholding; produce an immediate and sustained break from trend; not be captured by any of the proxies; impact income tax revenue with precise time lag to employee share impact; have no impact on any other tax revenue.

This IV-estimate is the local average treatment effect of employee share on (income tax/GD) for those states that implemented the IDB program. The magnitude of the instrumented elasticity suggests a standard deviation increase in exposure to state-led increase in employee share through random court litiga-

[21] In appendix table 8, I show there was a decrease in the relative reliance within state on license taxes levied specifically on occupations and small businesses, which are effectively taxes on self employed. This decrease in revenue is consistent with a behavioural response induced by the IDB program out of self-employment. Relative to GDP, these specific licence taxes represented a trivial amount, with a mean of .0002 at the onset of the IDB program. When the outcome variable is [occupation licence taxes/GDP], the revenue loss estimated from upholding comes out insignificant, and represents 0.00008 percent of the estimated increase in income tax revenue.

tion changes can account for just under 10 percent of the average within state change in income tax effort over time.

I argue that the IV estimated effect of employee share on income tax effort was in part channeled through changes in the exemption threshold. Recall that there was no impact on employee-share in deciles of the income distribution that lied above the pre-IDB location of the exemption threshold. (Figure 9). Growth in employee share below the exemption threshold can lead to increase in income tax revenue through two channels. First, the employee share growth can cause the exemption threshold to decrease. Second, the employee share growth can lead to increased ability to collect income tax, conditional on an exogenous decrease in the threshold. I do not rule out the second channel, but interpret the results in Figure 9 as strong evidence of the first channel.

1.4.7 Robustness

The causal interpretation of the results is driven by the exogenous variation in timing of the court upholding decision. In the appendix Tables 11-12, I provide further evidence for this claim by showing that results are nearly identical in cross-sectional specifications which are based on very different counterfactual variation. For the key set of outcomes (employee share, exemption threshold, employee share above threshold, income tax rate, income tax to GDP ratio) and confounders (average earnings, average employee earnings, bottom and top marginal tax rates, political competition, democratic vote share), I find very similar magnitudes and significance in a simple cross-state specification between upholding and non upholding states. The results are also robust to using synthetic matching. In this specification, each IDB state is paired to a synthetic control state which is the weighted average over all non-IDB states that maximizes pre-upholding trends on employee-share. I use the the full set of economic and political covariates to predict the pretrend.

Results are robust to fully ignoring the time dimension of the panel. I do this in a specification which collapses all time series information into pre and post upholding periods. Standard errors block-bootstrapped by state deliver similar patterns of significance. To alleviate concerns in DiD specifications over bias in standard errors and over-rejection of the null of $\theta = 0$ (Bertrand et al., 2004), I implement a non-parametric permutation test for $\theta = 0$. I construct constructing placebo triplets of [IDB state]-[year of vote in]-[time lag to upholding]. I then re-estimate the main specification 1 using 500 random number generator seeds, for employee share and exemption threshold outcomes. The significance levels are obtained as the cumulative distribution of placebo effects below (above) the main specification employee (threshold) estimate, and remain very similar. [22]

[22] The non-parametric significance levels are nearly identical, in a permutation exercise which

The estimated magnitudes are not driven by events which occur long after the upholding event. Magnitudes remain very similar when I narrow the sample to the IDB states and a time window of 15 years before the vote in and 15 years after after upholding. Results are robust to interacting pre-IDB cross-sections of determinants of structural transformation (illiteracy rate; urbanization rate; population density) with time.

Results are robust to allowing non-parametrically for civil law origin states to be on a different time path.

1.5 Model

1.5.1 Setup and empirical prediction

I consider a fixed distribution of income z across workers with pdf $h(z)$ and cdf $H(z)$. I assume exogenous employment shares of self-employed and employees at each income level, denoted respectively φ_z and $1-\varphi_z$. I do not model the development process that leads to changes in employment shares, but assume it follows the patterns documented in stylized fact #1.

If the agent reports income $z \geq K$, then she is liable to pay $\tau(z-K)$. Otherwise, she is not liable for income tax. K is the exemption threshold and τ is the marginal tax rate. I assume linear utility to abstract from income effects. I assume agents have access to an evasion technology which allows them to pay c in order to report income at K and fully evade taxes. This evasion technology generates 'bunching' of reported income at K, in line with large set of evidence on evasion behavior. The cost is assumed to be infinite for employees: $c^E(z)=\infty$. For self-employed, the cost depends flexibly on total income z (due perhaps to a 'visibility' effect) and on the distance between income and the threshold $z-K$ such that $c^{SE} = c(z, z-K) > 0$. The cost is assumed to be increasing and convex in z. In this setting, there will exist a 'marginal buncher' at income $\bar{z}$ who is indifferent between bunching and full compliance: : $\bar{z}-c(\bar{z},\bar{z}-K) = K+(1-\tau)(\bar{z}-K)$. All self-employed with income $z : K \leq z \leq \bar{z}$ will under-report and bunch at the threshold. An increase in τ unambiguously leads to more evaders: $\frac{\partial \bar{z}}{\partial \tau} > 0$. An increase in the threshold will lead to less evaders if the marginal gain from compliance is larger than the marginal gain from under-reporting after the threshold decrease, that is

$$\frac{\partial \bar{z}}{\partial K} < 0 \text{ if } \tau > c_K(\bar{z}, \bar{z}-K) \tag{4}$$

I will assume the condition in 4 holds. The revenue base reflects evading

construct placebo [year of vote in]-[time lag to upholding] but which maintains the observed set of IDB states.

self-employed between K and $\bar{z}$:

$$R = \int_{z \geq \bar{z}} \tau (z - K) \varphi_z dH(z) + \int_{z \geq K} \tau (z - K)(1 - \varphi_z) dH(z)$$

Consider a reform which locally decreases the threshold: $dK < 0$. This reform will have two effects on revenue: a mechanical gain and a behavioral loss. The mechanical gain, dM, reflects the marginal increase in revenue collected due to the reform on the inframarginal agents, assuming no behavioral responses

$$\begin{aligned} dM &= -dK\tau \left[\int_{z \geq \bar{z}} \tau (z - K) \varphi_z dH(z) + \int_{z \geq K} \tau (z - K)(1 - \varphi_z) dH(z) \right] \\ &\geq 0 \quad \text{if } dK < 0 \end{aligned} \tag{5}$$

The behavioral loss, dB, reflects loss in revenue due to behavioral responses of the marginal agents

$$\begin{aligned} dB &= -\frac{\partial \bar{z}}{\partial K} dK \tau (\bar{z} - K) \varphi_K \\ &\leq 0 \quad \text{if } dK < 0 \end{aligned} \tag{6}$$

where I have used the local approximation that $\varphi_K \approx \varphi_{\bar{z}}$, which is plausible if the last buncher is not located too far above the threshold. At the revenue maximizing optimum, K^{Rev}, it must be that $dB + dM = 0$. This yields the characterization for the location of the threshold

$$\frac{K^{Rev}}{\bar{z}} = \frac{1}{\left[1 + \left[\frac{\text{Mech gain}}{\text{Beh loss}} \right] \cdot \left[\varepsilon_{\bar{z},K} \varphi_K \right]^{-1} \right]} \tag{7}$$

where Mech gain $= \int_{z \geq \bar{z}} \tau (z - K) \varphi_z dH(z) + \int_{z \geq K} \tau (z - K)(1 - \varphi_z) dH(z)$, Beh loss $= h(\bar{z})\, \bar{z}$, and where $\varepsilon_{\bar{z},K}$ denotes the elasticity of the marginal buncher with respect to the threshold. By changing the mass of agents who respond to the local reform, the model predicts the main empirical result of this paper

Empirical prediction: An increase in employee share (self-employed share) locally around the threshold leads to optimally lower (larger) size of threshold

$$\frac{\partial K^{Rev}}{\partial \varphi_K} > 0 \tag{8}$$

A policy literature in developing and developed countries (Tanzi, 1987; OECD, 2015; IMF, 2015) discusses differences in administrative costs between reconstructing information trails for self-employment earnings and aggregation of employee information trails by employers. I model the administrative cost of of taxing an income segment z as an increasing function of the self-employed share on the income segment, $c(z) = c(\varphi_z)$. Revenue net of administrative costs equals

$$R = \int_{z \geq \bar{z}} \tau (z - K) \varphi_z dH(z) + \int_{z \geq K} \tau (z - K)(1 - \varphi_z) dH(z) - \int_{z \geq K} c(\varphi_z) dH(z)$$

The local threshold decrease $dK < 0$ will lead to an additional administrative marginal cost $dC = dK \cdot c(\varphi_K) < 0$ if $dK < 0$. The revenue maximizing threshold now equals equal

$$\frac{K^{AdminRev}}{\bar{z}} = \frac{1}{\left[1 + \left[\frac{\text{Mech gain}_{-dC(\varphi_K)}}{\text{Beh loss}}\right] \cdot \left[\varepsilon_{\bar{z},K}\varphi_K\right]^{-1}\right]} \tag{9}$$

where the threshold is now predicted to increase due both to behavioral distortions and administrative costs that increase as the self-employed share goes up. The formula 9 is robust to any general social welfare function.

The full set of empirical results on the impact of employee share on the threshold and income tax revenue are consistent with an objective function of revenue maximization over the exemption threshold. An extension to the objective function which is also consistent with the full set of results is to include a social preference for a 'fair tax base'. Discussed especially in a setting of low enforcement capacity countries, fairness relates to the idea that the tax base should not discriminate against particular groups in terms of compliance. On the income tax base, such fairness would imply that a group's share in effective contribution to tax revenue should be equal to its share in statutory contribution. This channel can be formalized by modeling a 'mis-representation' index given by the ratio of employee-share of income on the statutory income tax base to the ratio of employee-share of income on the compliant income tax base. Society faces social loss with parameter μ from any deviation of this index from a situation of perfect representation (with index value 1)

$$\text{Horizontal inequity} = \mu \left(1 - \left|\frac{\text{Employee-share on statutory income tax base}}{\text{Employee-share on compliant income tax base}}\right|\right) \tag{10}$$

So long as self-employed evade more than employees, the inequity cost associated with a lowering of the income tax threshold, dE, will always be smaller when the employee-share at the local threshold is larger. The formula can be extended for the optimal threshold that solves $dM + dB + dC + dE = 0$. This horizontal equity channel delivers a non-trivial prediction for movements in exemption threshold driven by gradual increases in employee share (stylized fact #2) and for constant employee share above the threshold (stylized facts #2 & #3) in the simplest possible setting of costless full evasion by self-employed (unlike the behavioral distortions channel and the administrative cost channel).[23]

The empirical prediction 8 is also consistent with an objective function that minimizes social distortions across tax bases subject to a minimum revenue requirement.[24] But such a model predicts decreases in revenue collected from distortionary tax bases as employee share increases, which is not consistent with the US regression results.

1.5.2 Model fit and new proxy for fiscal capacity

I use the K^* derived under 9 to perform two simple quantitative exercises. First, I predict the location of the threshold in all 90 countries in the cross-country section of the micro database. I use the calculated distribution φ_j of employee shares across deciles j of the country's income distribution as the only source of cross-country variation, and assume constant values for ε, ω, λ, and the earnings-distribution which characterizes the mechanical gain and behavioral loss expressions.[25] I calculate the predicted size of the income tax base as the sum of percentiles that lie above the predicted percentile-location of the exemption threshold. I compare the predicted size of tax base to observed size of tax base, calculated as the sum of percentiles above the actual location of the exemption threshold. Panel A of Figure 10 shows results from this exercise. Variation across development in the employee-share predicted exemption threshold can account for 62 percent of the observed cross-country variation in exemption threshold (based on a simple R-squared). The predicted threshold lies within 3-5 percentiles of the actual threshold in countries at levels of development ranging between China, Indonesia and Mexico. At higher levels of income, the predicted threshold is on average ten percentiles lower than the actual threshold, due to the large increase in self-employed in the bottom decile. It is as large as 25 percentiles in countries like Italy and Spain where the self-employed share is prevalent in the the bottom third deciles. At lowest levels of development, the predicted threshold is on average 10 percentiles below the observed threshold. This masks heterogeneity between countries where the model performs very well (Rwanda: 1 percentile gap),

[23] In a setting where agents differ in skill level and in cost of avoidance, Kopczuk (2001) derives conditions under which high marginal tax rates can exacerbate horizontal inequity.

[24] In which case individual tax instruments will be set such that their marginal social costs are equalized.

[25] The distributions of φ_j in each country were used to construct the profiles for the stylized facts in Section 1.3.

fairly well (India: 6 percentile gap) and poorly (Kenya: 25 percentile gap). The average over-prediction in less-developed countries could be closed down by allowing enforcement capacity to exogenously increase over development. This highlights interactions between economic growth processes and investments in state capacity to explain the full variation in tax take along the development path. The gap could also be closed down by varying demand for redistribution or public goods.

In a second exercise, I construct the predicted distribution of employee shares at each of ten estimated steady state per capita income points from Section 1.3. I predict the location of K^* from 9 in these ten constructed income distributions using the formula from 9 . I plot the ten combined minimum income points and associated size of income tax base in Panel B of Figure 10. I interpret these changes as proxies for 'stages' of fiscal capacity to expand the tax base. At per capita income point associated with steady state employee share in decile j, a country has reached a level of development which is associated with an enforceable tax base of size $10 - j$.

1.6 Conclusion

This paper has provided evidence and supporting theory to show that transition into employee jobs over development is an important driver of tax capacity. The paper introduces a novel channel, in which increases in employee share occurring gradually further down the income distribution causes broadening of the base through lowering the exemption threshold.

By focusing on the exemption threshold, this paper has provided evidence on the interaction between the extensive margin of income tax compliance and a statutory tax instrument. While this margin of moving agents into the income tax net is potentially an important compliance margin to explain tax capacity, there currently exists no well identified micro evidence on it.[26] Providing an estimate of the extensive margin of income tax compliance is an area for future research (Agostini & Jensen, 2016). The large variation in the location of the tax threshold across development suggests statutory tax instruments may be important determinants of tax capacity. On the other hand, current literature has focused on enforcement technologies and political economy as the main determinants of tax capacity. On-going research (Abramovsky, Bachas & Jensen, 2015) attempts to build new measures of statutory tax instruments to investigate further their relative importance in explaining tax capacity.

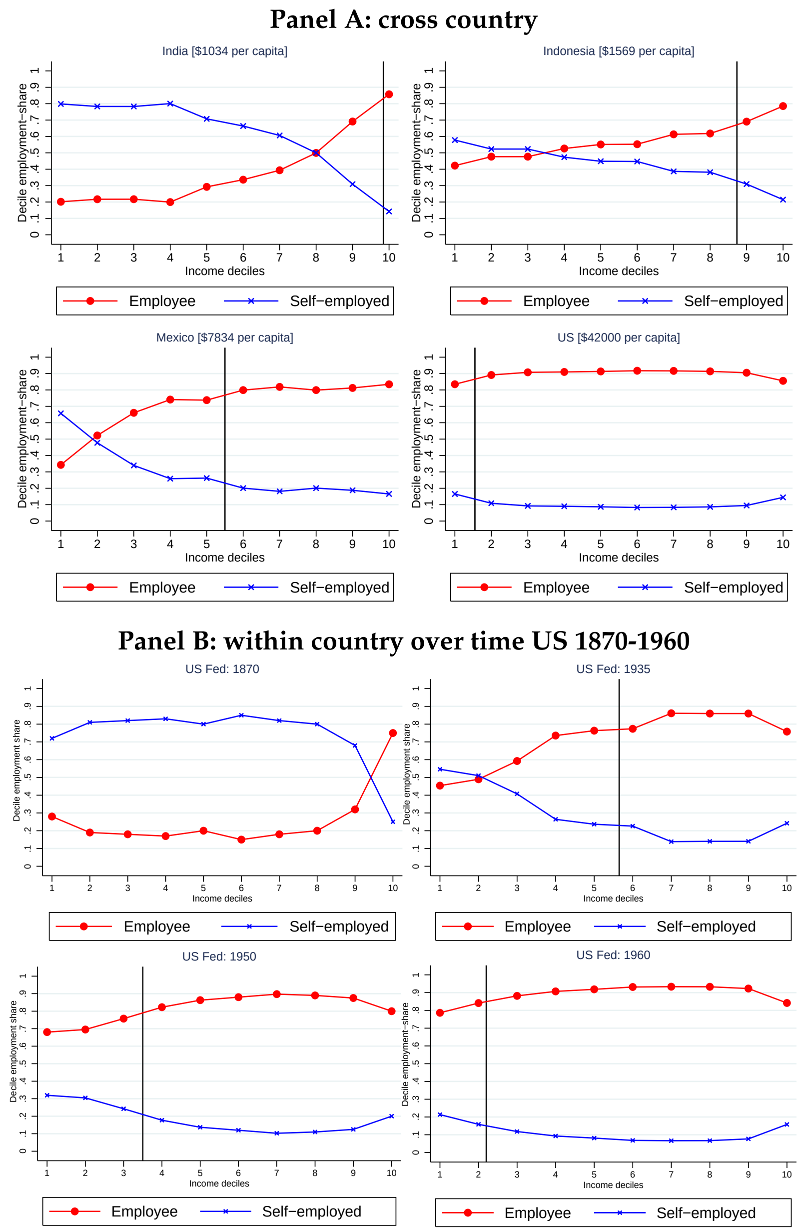
The evidence in this paper suggests the importance of studying jointly the drivers of development and their impacts on taxation. A simple but robust find-

[26] Field experiments in developing countries including Sri Lanka, Brazil, and Malawi study the impacts on business registration from varying the costs of formalization, but do not address any impact on labor income taxes (review of evidence in Bruhn and McKenzie, 2014).

ing has been the close match both between less developed countries and currently advanced countries at similar levels of development. This suggests that a small income tax base in a less developed country reflects the same factors which lead to a low employee share. Future research could study factors which explain the patterns of gradual increases in employee share over the income distribution.

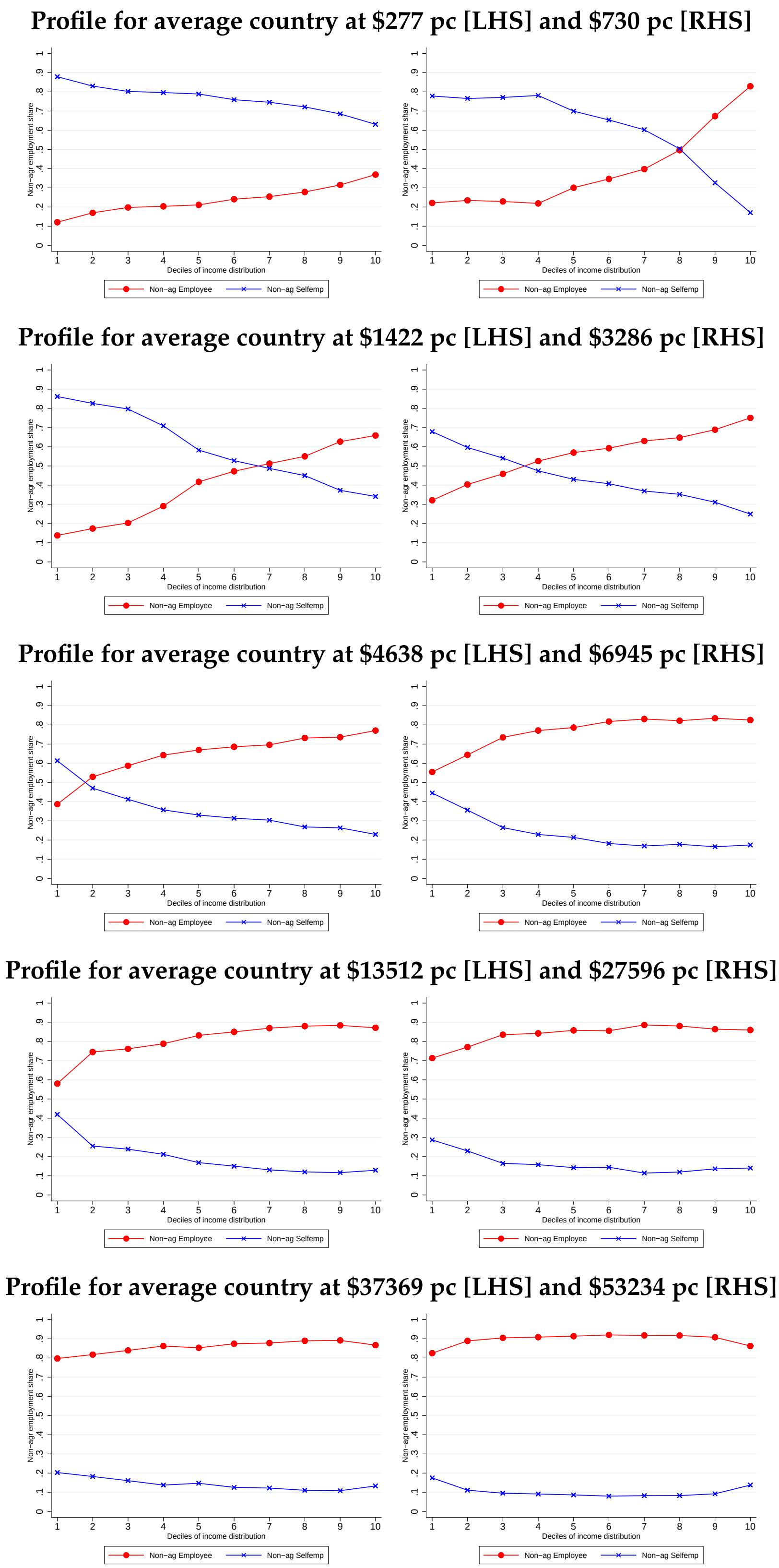
The research design of this paper has highlighted the usefulness of building micro evidence to answer macro questions. Such design could be applied to study other questions in taxation and development. There exists compelling micro evidence on the enforcement gains from exploiting sales connections between firms (Pomeranz, 2015). This evidence could be combined with the development-macro evidence on the growth in complexity and interconnectedness of firms (Poschke, 2011) to explain patterns of sales tax structure and revenue collection over development.

FIGURE 1: EMPLOYEE SHARE OVER INCOME DISTRIBUTION AND DECREASE IN INCOME TAX EXEMPTION THRESHOLD



These figures plot the employment-shares of employees and self-employed over deciles of the income-distribution, for different countries (Panel A) and within-country over time (Panel B). The share of each work-type is defined as the share of total non-agricultural employment in the decile of the income-distribution. Employees are defined as individuals working in a firm with size > 1; self-employed are defined as individuals who report working as own-account workers, or as employees in a firm of size 1, or in a family-business with no employer. In each graph, the black solid denotes the location of the personal income tax (PIT) exemption threshold, taken from the tax code of the relevant country-year. The PIT threshold is defined as the level of gross income above which an individual earner becomes liable to pay personal income tax. The source for each graph is a household micro-dataset containing a nationally representative sample; in all underlying household surveys, the work-type status is mutually exclusive at the level of the individual. Source: Appendix.

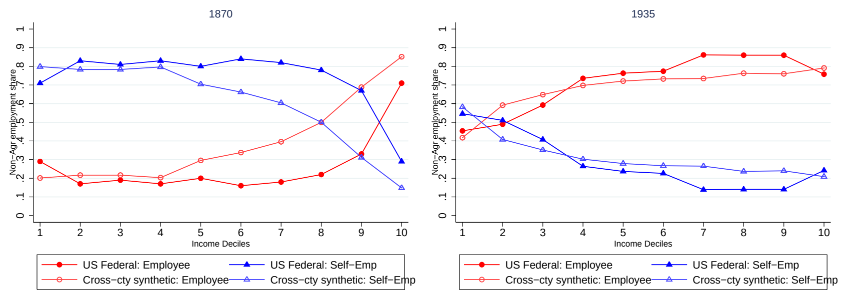
FIGURE 2: EMPLOYEE SHARE: REPRESENTATIVE DEVELOPMENT PROFILES



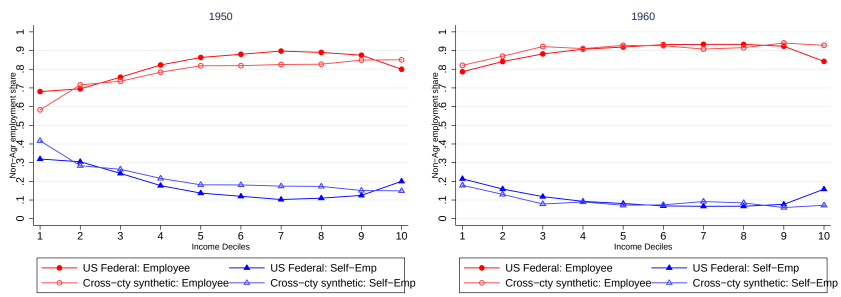
These panels depict the average employment structure profiles over deciles of the income-distribution. Red dotted (blue cross) observations indicate the employee (self-employed) share of non-agricultural employment in an income decile. A profile of employment shares is first construced for the 90 individual countries in the cross-section of the micro database. Then an average profile is constructed over the profiles of countries that lie in a bin with indicated average real per capita income. The bins correspond to deciles of the real per capita income distribution across the 90 countries. Source: Appendix.

FIGURE 3: EMPLOYEE SHARE: WITHIN US AND PAIRED SYNTHETIC CROSS-COUNTRY PROFILES

Federal US 1870 profile [$2445 per cap] and Federal US 1935 profile [$6212 per cap]

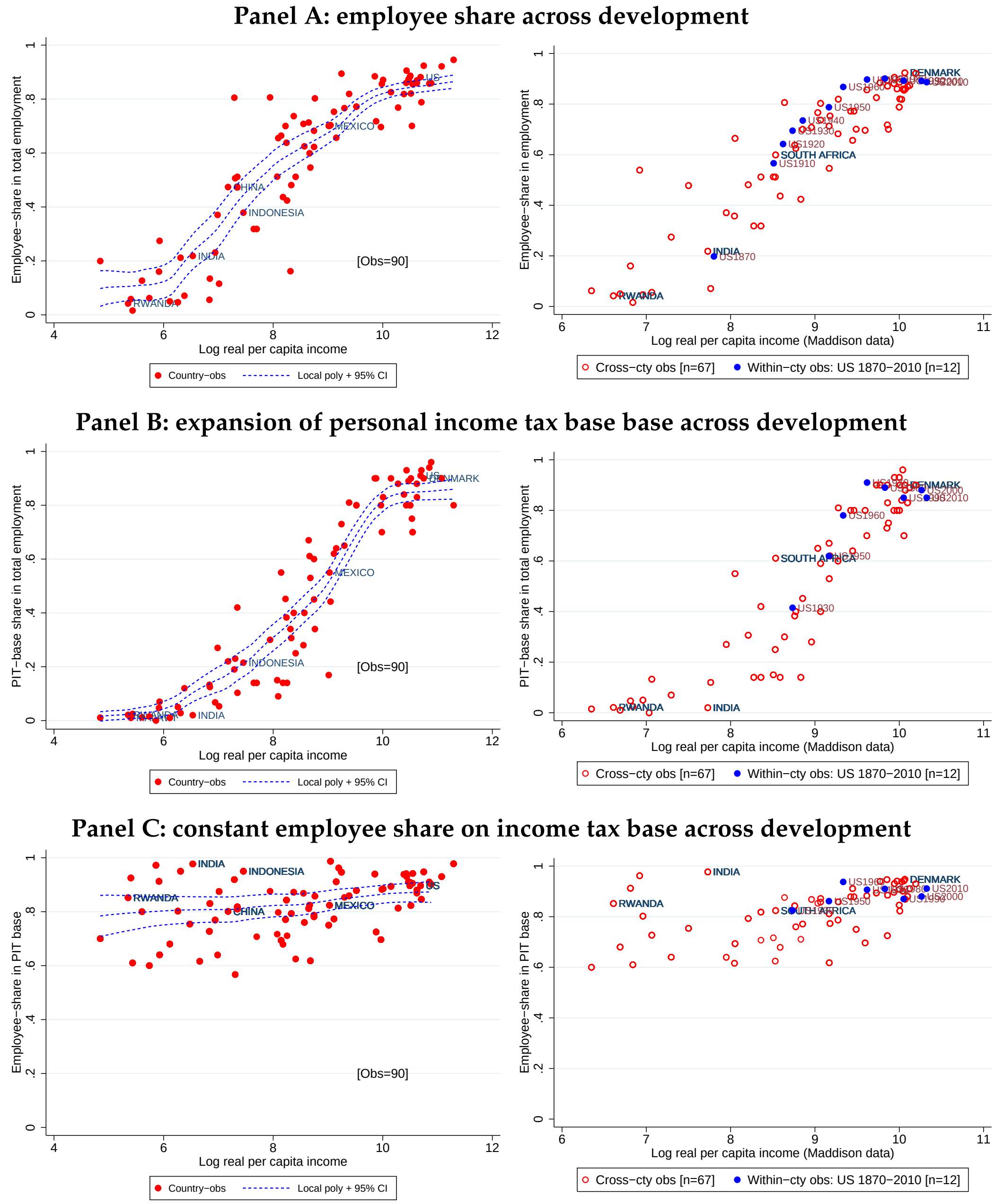


Federal US 1950 profile [$9561 per cap] and Federal US profile 1960 [$11338 per cap]



These panels plot employment structure shares over the income distribution in the US between 1870 and 1960. Each graph are constructed in exactly the same way as the profiles in Figure X, but using a nationally representative sample of the U.S. employed population in 1870, 1935, 1950 and 1960. In each panel, the solid circle and triangle deonote the employment shares in the US historical year. The hollow observations dentoe the employment-shares in the synthetic country based on the cross-country section of the micro databse. For each US profile, the paired synthetic cross-country profile is created by taking the average over profiles of countries whose real per capita income lies within $+/-$ 10 percent of the US real per capita income, using the Maddison dataset. The per capita income reported in brackets corresponds to the average income in the US in the year of the survey in the Maddison data. Source: Appendix.

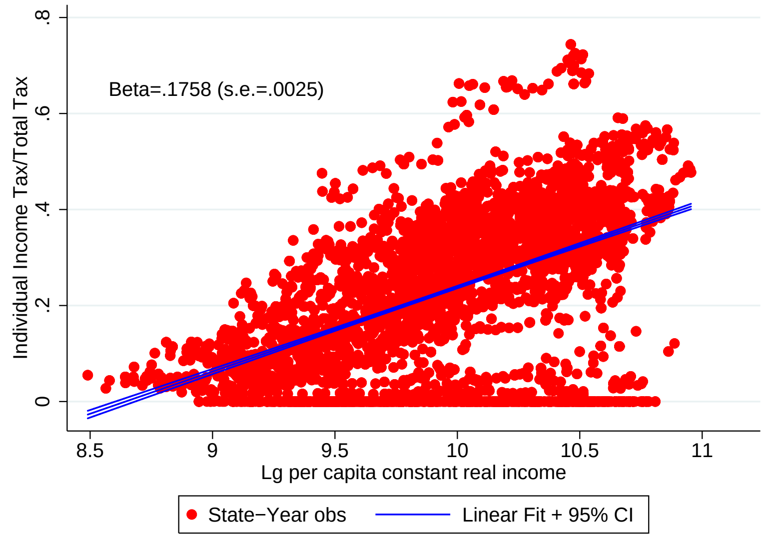
Figure 4: Employee share, size and employment-composition of income tax base



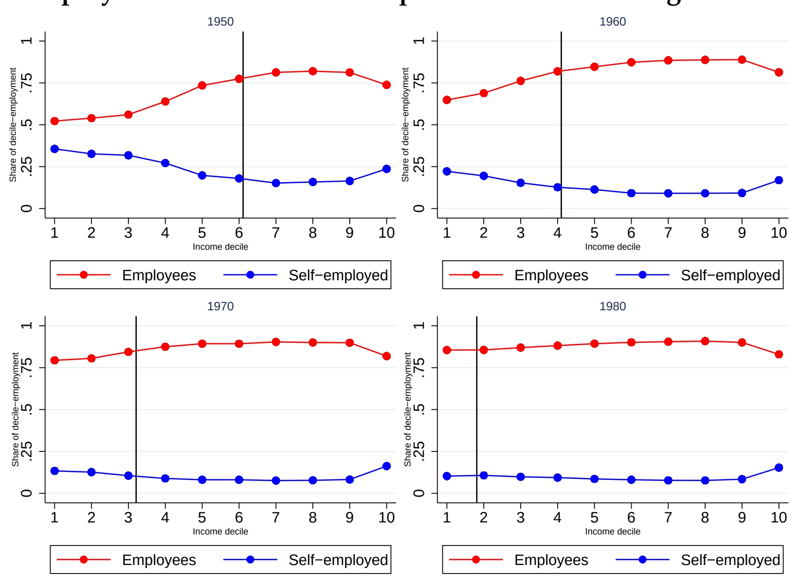
These figures plot correlations between log per capita income and: share of non-agriculture employees in total employment (Panel A); share of employment above the personal income tax threshold (PIT) in aggregate employment (Panel B); share of non-agriculture employees in employment above the PIT (Panel C). Each country-observation is calculated using a household micro-dataset containing a nationally representative sample; in all underlying household surveys, the work-type status is mutually exclusive at the level of the individual Within each panel, the LHS graph plots the correlation using the full sample of surveys in the cross-country section of the micro database; the RHS graph uses the full set of surveys from the within-country section of the database together with the subset of surveys from the cross-country section which could be appended using the Maddison real per capita income database. Dashed lines denote the local polynomial fit on the underlying observations together with a 95% confidence interval. In Panel A, the employee-share of employment is defined as the share of workers who report working in non-agriculture industries, and in firms of size > 1 and which are are not family units or casual daily-wage laborers. In Panel B, the PIT-base share in total employment is defined as the number of percentiles of the gross earnings income distribution which lies above the income tax exemption threshold. In Panel C, the employee-share in the PIT base is defined as the share of non-agriculture

FIGURE 5: RISE OF INCOME TAX, TAX BASE EXPANSION AND EMPLOYEE SHARE: US STATES

Panel A: State income tax share of total taxes: all states 1939-2010

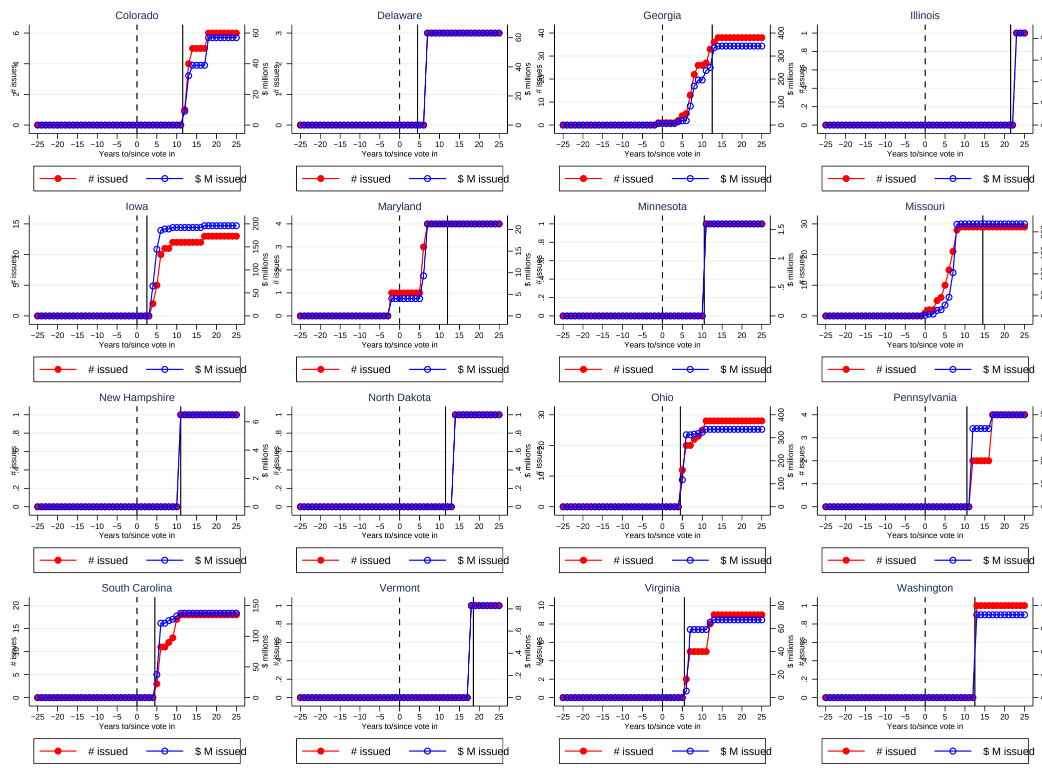


Panel B: employee share and state exemption threshold: average state 1950-1980



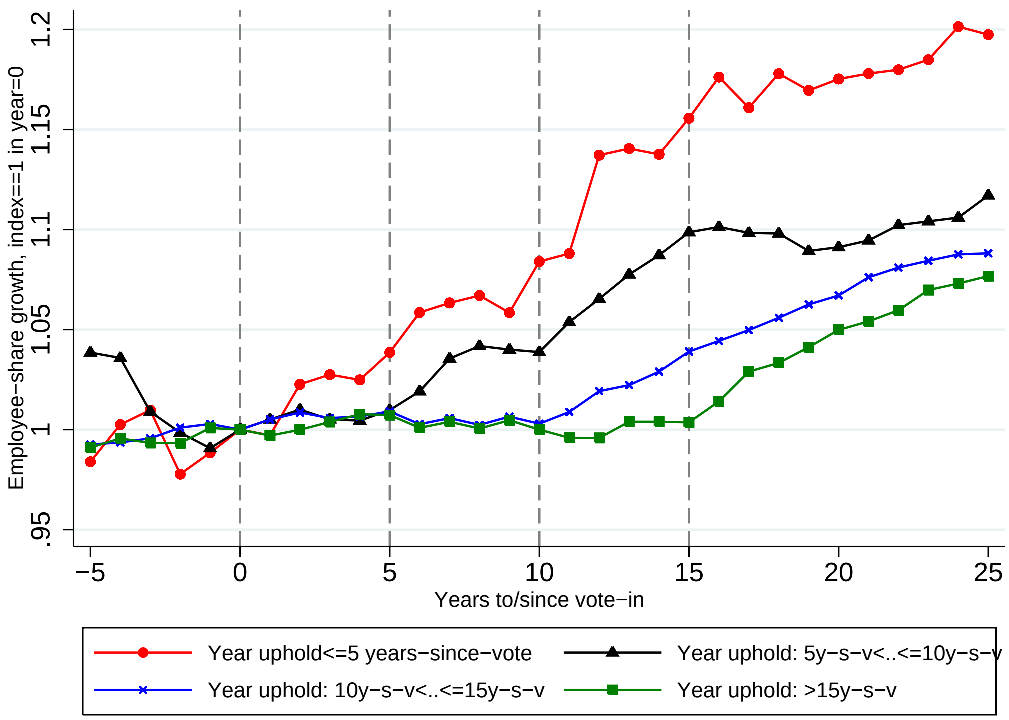
Panel A plots the state income tax share of total state taxes, using all state-year between 1939 and 2010, against real per capita income. The tax-mix observations are from Besley & Jensen (2015), originally sourced from historical Census records. The real per capita income is constructed as the per capita income in a state-year from the historical BEA series, deflated by the historical CPI. The solid lines denote the linear fit with a 95% confidence interval from the regression on the full underlying state-year observations.Panel B plots the employment-shares of employees and self-employed over deciles of the income-distribution, for the average state in the US between 1950 and 1980. The employee-share is defined as the share of employed agents who report being employed in a non-agricultural industry; the self-employed share is defined as the share of agents who report working on their own account, or as employee in a firm of size 1. The PIT threshold corresponds to the state PIT threshold (which in most cases differs from the Federal PIT threshold), and is calculated at the state-year level using the Bakija (2015) historical state-tax calculator. The income-decile distribution of employment-shares is first calculated for each state-year, then the average is taken over all the continental states (N=48) in a given year; the value of the exemption threshold is calculated in every state-year, then the average is taken over all threshold values. Source: appendix.

FIGURE 6: INDUSTRIAL DEVELOPMENT BOND PROGRAM: VOTE IN, COURT UPHOLDING AND ISSUANCE



This panel plots time-series of cumulative IDB-debt in number of issues and in millions of $ of principal, for the selected 16 states with the largest time lag between upholding and vote in. In each state, time is indicated as years to/since the vote in event, which is year 0 (black dashed line). In each state, the solid vertical line denotes the year of the court upholding of IDB. Source: appendix.

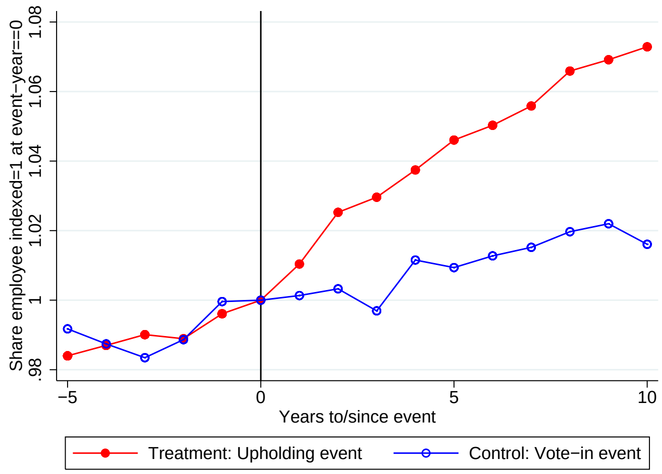
FIGURE 7: GRAPHICAL EVIDENCE ON IMPACT OF UPHOLDING EVENT ON EMPLOYEE SHARE



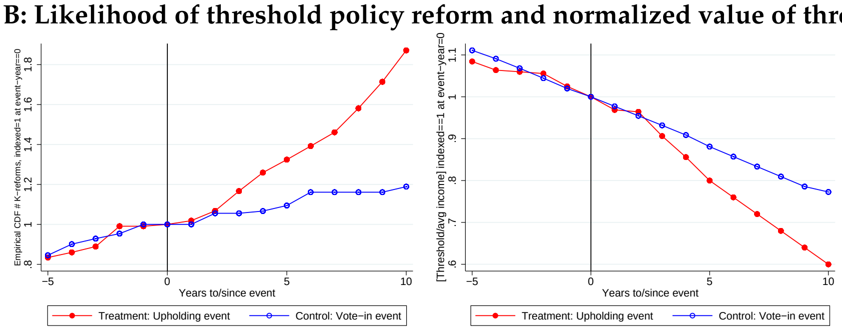
This figure shows the evolution of employee-share of earned income for groups of states for which the time-lag between the vote-in and the upholding event differed. For the circle-group, timelag $\in [0, 5]$; triangle group, timelag $\in (5, 10]$; cross group, timelag $\in (10, 15]$; square group, timelag $\in (15, 20]$. Each series shows the evolution of employee-share for the average state in the group, where employee-share is indexed to 1 in the year of the vote-in for all groups. Vertical dashed lines denote years since vote-in where a first upholding-decision occurs within a group. Source: appendix.

FIGURE 8: EVENT-STUDY WITHIN STATE AROUND VOTE IN AND UPHOLDING

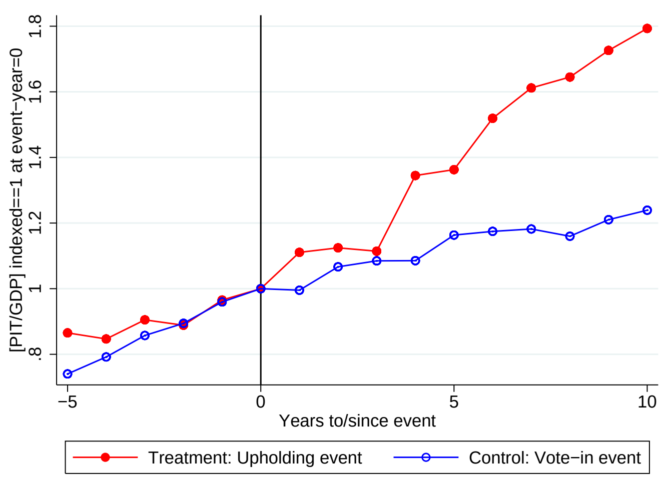
Panel A: Employee-share



Panel B: Likelihood of threshold policy reform and normalized value of threshold



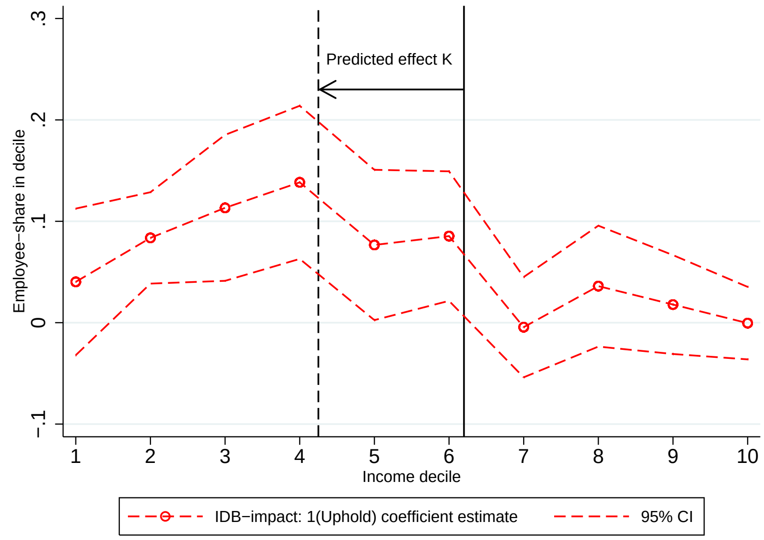
Panel C: Ratio [Personal income tax/GDP]



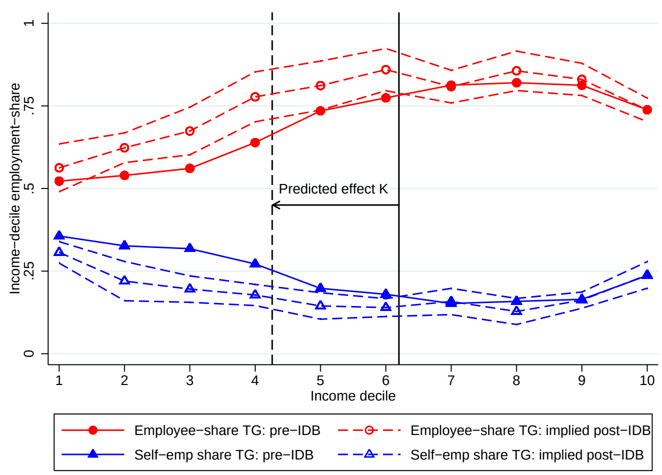
This figure shows the evolution of employee-share of earned income (panel A), size of PIT base and likelihood of PIT reform (panel B), and PIT to GDP ratio (panel C), within-state over time for the average treatment state around two events: vote-in, upholding. In the hollow-circle series (filled-circle series), the treatment-control is based on years before and after the event of state House vote-in of the Constitutional amendment to issue IDB (event of state-court upholding IDB). For each series, the evolution is normalized to 1 in the year of the event. In order to show the two events within-state for ten years after each event, these graph use only the subset of states for which the time-lag between vote-in and upholding exceeded 15 years, and the pre-event period is set to 5 years. These are arbitrary choices, and in the Appendix, I report the same exercise but for a shorter time-window to/since the event. In Panel B, the PIT-base is proxied for by the ratio of the state personal income tax exemption threshold to the state average earnings (right-hand side panel). The left-graph of Panel B shows the evolution of the state-specific empirical cumulative distribution of number of legislative reforms to the state exemption threshold: this measure controls for potential cross-state differences in frequency of tax-reform, and isolates any changes in likelihood that a reform to the state threshold will be passed around

FIGURE 9: DISTRIBUTIONAL IDB IMPACTS ON EMPLOYEE SHARES AND THRESHOLD LOCATION

Panel A: IDB impact on employee shares and threshold location in income distribution



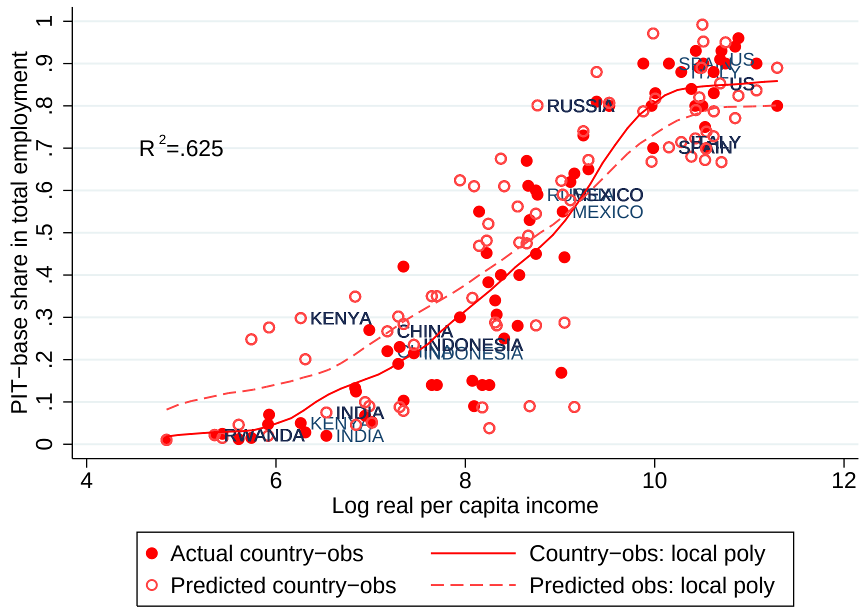
Panel B: implied IDB impact on employee shares and threshold location in income distribution



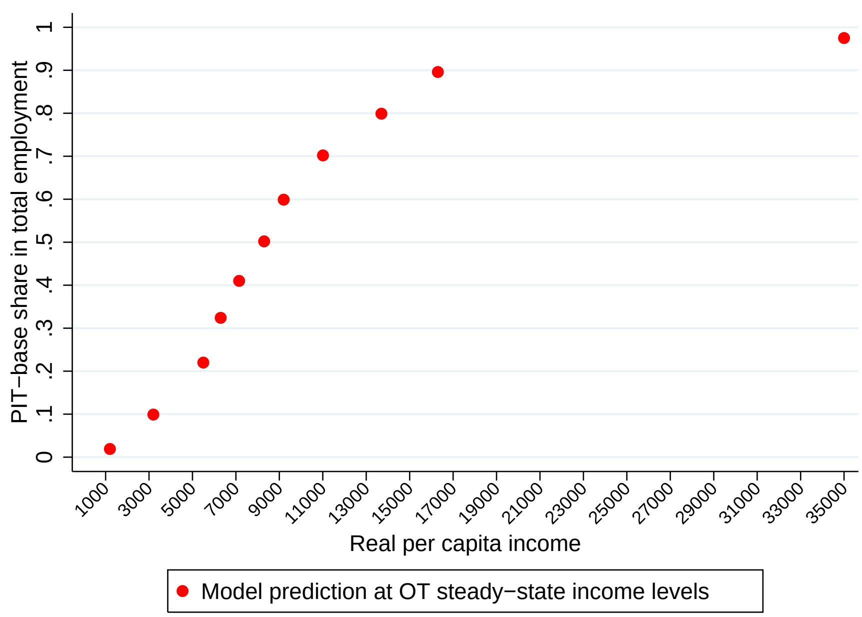
Panel A reports the coefficients $\widehat{\theta_j}$ on the 1(Uphold) dummy in a regression on employee-share in decile $j = 1, ..., 10$, using specification 1. Each hollow-circle denotes the decile-j point estimate $\widehat{\theta_j}$ and the dashed lines denote the 95% confidence interval of the point-estimate (robust standard errors clustered at the state level). The black solid line denotes the location of the average PIT exemption threshold in the IDB-treatment state in the pre-IDB period, K^{PRE}. The dashed line shows the predicted post IDB percentile location of the threshold calculated as $K^{POST} = K^{PRE} + \widehat{dK}$, where $\widehat{dK} = -18.43$ corresponds to the reduced-form estimate using 2. Panel B plots the implied post-IDB employee-shares and self-employed shares. The implied employee-share in a decile j, θ_j^{POST}, is calculated as $\theta_j^{POST} = \widehat{\theta_j} + \theta_j^{PRE}$ where θ_j^{PRE} is the average employee share in decile j calculated in the pre-IDB period. The construction of the implied self-employed shares is similar. The solid circle series denotes the pre-IDB distribution of employee shares, θ_j^{PRE}, while the hollow circle series denotes the post-IDB implied distribution θ_j^{POST} . Similarly for self-employed shares denotes with triangle symbols. Source: appendix.

FIGURE 10: MODEL FIT

Panel A: Model predicted versus actual size of income tax base in 90 countries



Panel B: Model predicted size of income tax base at steady state employee share stages of development



Panel A plots the cross country actual (solid circle) and predicted (hollow circle) size of income tax base, for the 90 countries in the cross section of the micro database. The predicted size of income tax base is calculated based on the model derived in Section V, and predicts the location of the exemption threshold in all countries using the measured employment shares in the deciles of the country's income distribution as the only source of cross-country variation. That is, values of enforcement capacity, administrative costs, earnings structure, demand for redistribution, marginal value of public goods are all assumed to be constant across countries. The highlighted countries are meant to illustrate cases of good and poor model fit. The cross country model fit is repoted as the R-square from an OLS regression of the actual PIT base share in employment on the predicted PIT base share in employment. Panel B plots the predicted size of income tax base, using the model derived in Section V and the predicted decile distributions of employment shares based estimated at each of the ten development stages discussed in Section V. Similary to Panel A, the model predicts the location of the threshold assuming constant values for all other threshold

TABLE 1: DETERMINANTS OF CHANGE TO IDB LITIGATION-STATUS

	LHS=1(Upheld) Non-parametric Cox proportional hazard model, hazard rate reported										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1(Civil law origins)	5.4351	4.6122	4.4784	6.7249	6.8493	7.0552	6.8491	5.4314	5.9501	9.2531	5.3948
	(3.2017)***	(3.4472)**	(3.3070)**	(4.7508)***	(5.0120)***	(5.3059)***	(4.8204)***	(4.6463)**	(4.7442)**	(9.3976)**	(4.4142)**
Redev-share lab force		9.0393	9.2889	2.6910	6.9380	1.4096	1.7451	11.9789	254.7608	.6733	16.5004
		(31.4626)	(34.6653)	(12.0902)	(22.0226)	(6.5460)	(8.0408)	(74.1761)	(948.501)	(4.1576)	(129.3488)
1(Poll tax)			1.0184	.9983	1.2002	1.3098	1.3129	1.3968	1.5858	1.5628	1.329
			(.7572)	(.6685)	(1.0741)	(1.1022)	(1.1176)	(1.2716)	(1.7699)	(1.5572)	(1.5791)
1(Literacy test)			1.3063	1.2828	1.2042	.9580	.9745	1.2009	.0372	.8025	1.2751
			(.7206)	(.6659)	(.5564)	(.4748)	(.5073)	(1.1384)	(.0856)	(.7165)	(1.6888)
Log(population)				.6759	.5863	.6045	.6118	.6416	.5925	.5516	.6417
				(.2843)*	(.2467)	(.1880)	(.2023)	(.2100)	(.2283)	(.2010)	(.2123)
Manuf share lab force					7.4211						
					(33.6242)						
Employee share lab force						16.8598	11.9849	10.0700	1794.923	95.3021	9.6009
						(82.1034)	(81.8919)	(70.7272)	(16589.73)	(702.837)	(70.2367)
Lg(per cap inc)							1.2221	.9257	.1472	.2766	.9216
							(3.3499)	(2.3310)	(.5211)	(.7398)	(2.3287)
1(Southern state)								.6464	.1562	.6113	.6367
								(.6681)	(.2125)	(.5602)	(.6839)
(K/y)									5.6413		
									(7.0495)		
(Pers income tax/GDP)										2.1414	
										(4.7415)	
(Total income tax/GDP)											.0626
											(2.2671)
Obs	134	134	134	134	130	130	130	130	123	123	123

$*, **, ***$ denote significance at the 10 percent, 5 percent, 1 percent level. Standard errors robust to clustering at the state level. This table reports the results of estimating non-parametric Cox proportional hazard models, where hazard rates are reported. Hence tests for significance relate to the null that the coefficient is equal to one. The unit of observation is state-year. A state enters the sample in the year where the Constitutional amendment allowing issuance of IDBs is voted in. The state drops the sample once the highest instance of the state court system has upheld the legality of the IDB-program. In Column 1, the baseline model includes a dummy for civil law origins. In the columns onwards, the baseline model is augmented with additional controls: col. (2) includes the state-share of labor force in redevelopment counties (time-invariant: TI) ; col.(3) includes indicators for whether the state has a poll tax and/or a literacy test for voting (time-varying: TV); col.(4) includes the log of state-population (TV); col.(5) includes manufacturing share of employment (TV); col.(6) includes the employee-share of total employment (TV); col.(7) includes the log of per capita income (TV); col.(8) includes a dummy for Southern states according to US Census definition (TI); col.(9) includes the ratio of state income tax threshold to average earnings (TV); col.(10) includes the ratio of personal income tax to state GDP (TV); col.(11) includes the ratio of total tax to GDP (TV). Sources: appendix.

TABLE 2: IDB IMPACT ON EMPLOYMENT AND EARNINGS STRUCTURE

Panel A: Employment	Log(volume of employment)									Share of employment	
	Total	Non agric employee	Manufacturing	Construction	Trade	Services	Government	Self employed	Agric employee	Non agric employee	Self employed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1(Vote in)	-0.0047	0.0089	-0.004	0.0246	-0.0145	0.0023	-0.0054	.0084	-.0322	.0029	-.0042
	(0.0123)	(0.0111)	(0.0248)	(0.0375)	(0.0131)	(0.0185)	(0.0201)	(.0203)	(.0309)	(.0035)	(.0029)
1(Uphold)	-0.0243	0.0394	0.0763	-0.036	0.0252	-0.0478	.0054	-.0756	-.0085	.0360	-.0368
	(0.0241)	(.0191)**	(.0238)***	(0.0544)	(0.0215)	(.0277)*	(.0338)	(.0240)***	(.0496)	(.0073)***	(.0052)***
R squared	0.9808	0.9878	0.9150	0.9067	0.9871	0.9939	0.9867	0.9422	0.9569	0.9810	0.9559
Number of states	48	48	48	48	48	48	48	48	48	48	48
Number of state-years	2890	2890	2890	2890	2890	2455	2890	2890	2890	2890	2890

Panel B: Earnings

	Log(volume of earnings)					Log(average earnings)					Share of earnings	
	Total	Non agric employee	Self employed	Employee-related transfers	Other transfers	Total	Non agric employee	Self employed	Employee-related transfers	Other transfers	Non agric employee	Self employed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1(Vote in)	-.0089	-.0040	.0192	-.0101	-.0113	-.0004	.0022	.0215	.0000	.0049	.0048	.0000
	(.0139)	(.0133)	(.0216)	(.0185)	(.0236)	(.0117)	(.0062)	(.0309)	(.0170)	(.0240)	(.0043)	(.0044)
1(Uphold)	-.0031	.0508	-.0891	.0880	.0514	.0251	-.0004	-.0025	.0843	.0435	.0429	-.0407
	(.0209)	(.0215)**	(.0420)**	(.0412)**	(.0429)	(.0200)	(.0157)	(.0361)	(.0472)*	(.0477)	(.0137)***	(.0115)***
R squared	0.9988	0.9984	0.9896	0.9975	0.9982	0.9960	0.9989	0.9694	0.9959	0.9961	0.8913	0.8138
Number of states	48	48	48	48	48	48	48	48	48	48	48	48
Number of state-years	2855	2855	2855	2855	2855	2855	2855	2855	2855	2855	2855	2855

$*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. Time period is 1939-2005. This table reports OLS estimates $\widehat{\alpha}$ and $\widehat{\theta}$

$$y_{st} = \beta + \alpha \mathbf{1}\,(\text{Vote in})_{st} + \theta \mathbf{1}\,(\text{Upheld})_{st} + \lambda \mathbf{X}_{st} + \mu_s + \gamma_t + \phi_s \cdot t + \varepsilon_{st}$$

where s denotes state, t denotes time, $\mathbf{1}\,(\text{Vote in})_{st}$ indicates whether a vote has occured in the state-House to allow issuance of IDB but the IDB has not yet been upheld, $\mathbf{1}\,(\text{Upheld})_{st}$ indicates whether the court-system has upheld the legality of IDB. The vote-in and upholding events are mutually exclusive events. The set of state-time varying controls $\mathbf{X}_{st}$ includes: log average resident earnings; dummies for the existence of a poll tax and a literacy test, both used for voting restrictions; dummies for state election years; dummies for the existence of a state corporate income tax and of right-to-work laws; a continuous measure of the firm-size coverage of state unemployment insurance laws. All regressions include a state-specific linear trend: $\phi_s \cdot t$. Source: appendix.

Table 3: IDB reduced form impact and employee share impact on income tax base

	Employee share impact on tax base: Size of tax base			Employee share impact on tax base: Composition of tax base	Confounding channels: Tax rates	Confounding channels: Earnings distribution	Confounding channels: Enforcement	Confounding channels: Demand for redistribution
	(PIT exemption threshold/average earnings)			[Employee-share above threshold]	Bottom MTR	Hazard ratio	Number tax agencies	Democratic vote share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Vote in)	-.1400			-.0052	.0018	.0484	.0401	.0072
	(.1140)			(.0119)	(.0065)	(.0870)	(.0577)	(.0101)
1(Upheld)	-.7218			-.0054	.0035	.0263	.0326	.0108
	(.3296)**			(.0185)	(.0075)	(.1473)	(.0965)	(.0178)
Employee-share		-20.318	-29.588					
		(5.832)***	(12.157)**					
1st stage F-test			7.79					
p-value			(.0012)					
Implied elasticity tax base - emp share	-6.678	-5.200	-7.548					
Number of states	48	48	48	48	48	48	48	48
Number of state-years	2931	2931	2931	2931	2931	2901	2815	2931
R-squared	0.7869	0.8111		0.7737	0.6762	0.3035	0.6719	0.6619
Method	OLS	OLS	IV	OLS	OLS	OLS	OLS	OLS

$*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. Time period is 1939-2005. This table reports OLS estimates $\widehat{\alpha}$ and $\widehat{\theta}$

$$y_{st} = \beta + \alpha \mathbf{1}\left(\text{Vote in}\right)_{st} + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \sum_{j=1}^{10} \omega_j z_{jst} + \lambda \mathbf{X}_{st} + \mu_s + \gamma_t + \phi_s \cdot t + \varepsilon_{st}$$

where s denotes state, t denotes time, $\mathbf{1}\left(\text{Vote in}\right)_{st}$ indicates whether a vote has occured in the state-House to allow issuance of IDB but the IDB has not yet been upheld, $\mathbf{1}\left(\text{Upheld}\right)_{st}$ indicates whether the court-system has upheld the legality of IDB. The regression includes average income in all ten deciles of the state-year income-distribution, z_{jst}. The set of state-time varying controls $\mathbf{X}_{st}$ include dummies for the existence of a poll tax and a literacy test, both used for voting restrictions; dummies for state election years; dummies for the existence of a state corporate income tax and of right-to-work laws; a continuous measure of the firm-size coverage of state unemployment insurance laws. All regressions include a state-specific linear trend: $\phi_s \cdot t$. In Col. (1), the implied elasticity of [exemption threshold/average earnings] is calculated based on the ratio of reduced-form estimates of 1(Uphold); in Col.(3), the implied elasticity of [exemption threshold/average earnings] is calculated based on the IV-estimated impact of employee-share on $[K/y]$. Source: appendix.

TABLE 4: IDB IMPACT ON TAX TAKES

	Personal Income Tax/GDP			CorpIncT/GDP	GenSalesT/GDP	SelectSalesT/GDP	LicenceT/GDP
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1(Vote in)	.0007			-.0001	-.0007	-.0005	.0003
	(.0009)			(.0002)	(.0008)	(.0004)	(.0002)
1(Upheld)	.0017			.0001	-.0002	.0001	-.0004
	(.0007)**			(.0004)	(.0010)	(.0010)	(.0003)
Employee-share		.0166	.0248				
		(.007)**	(.0109)**				
1st stage F-test			7.96				
p-value			(.001)				
Implied elasticity	1.292	.6225	.900				
Number of states	48	48	48	48	48	48	48
Number of state-years	2931	2931	2931	2931	2931	2931	2931
R-squared	0.9195	0.9280		0.7081	0.8653	0.8805	0.8414
Method	OLS	OLS	IV	OLS	OLS	OLS	OLS

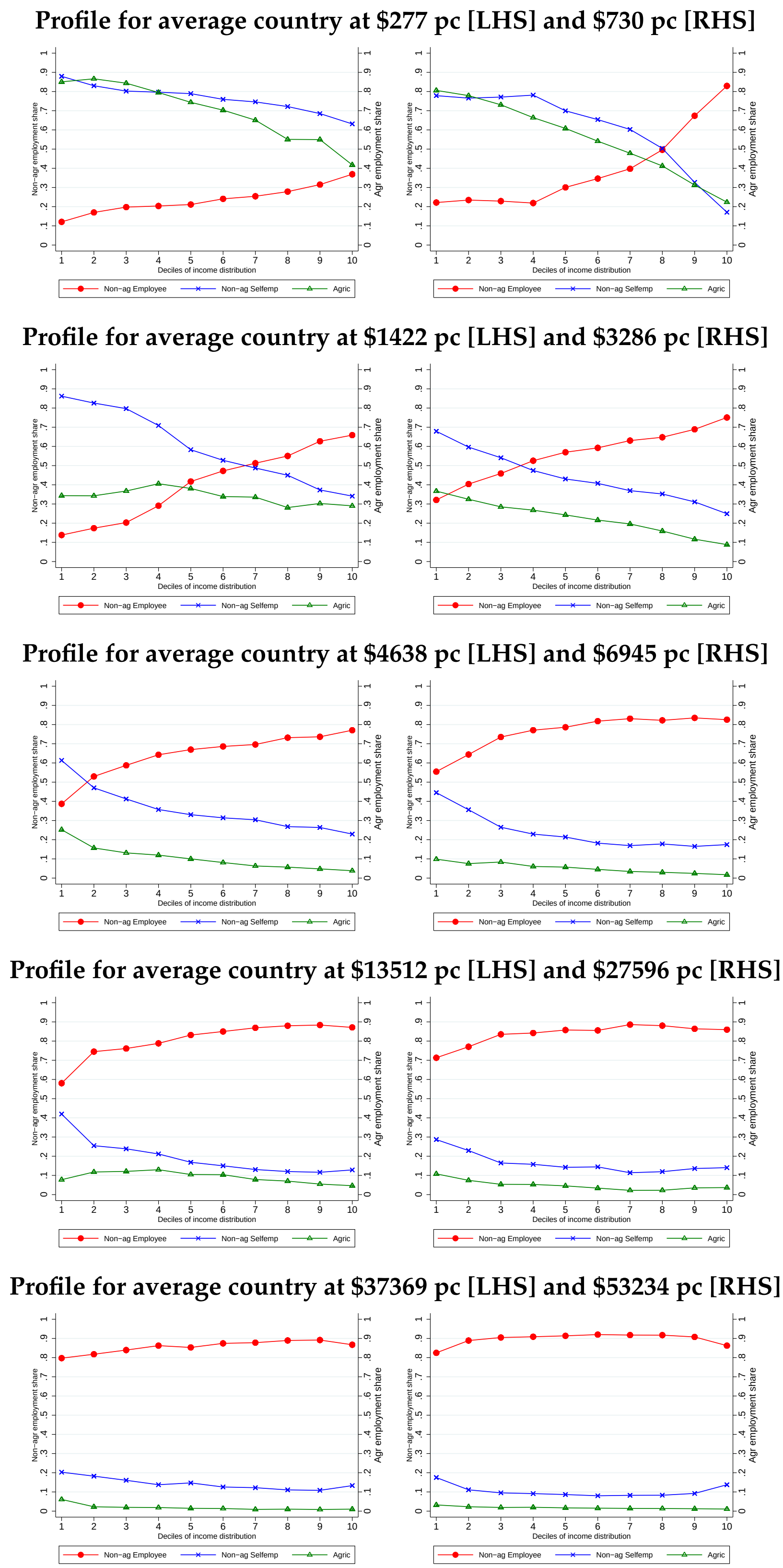
$*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. Time period is 1939-2005. This table reports OLS estimates $\widehat{\alpha}$ and $\widehat{\theta}$

$$y_{st} = \beta + \alpha \mathbf{1}\left(\text{Vote in}\right)_{st} + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \sum_{j=1}^{10} \omega_j z_{jst} + \lambda \mathbf{X}_{st} + \mu_s + \gamma_t + \phi_s \cdot t + \varepsilon_{st}$$

where s denotes state, t denotes time, $\mathbf{1}\left(\text{Vote in}\right)_{st}$ indicates whether a vote has occured in the state-House to allow issuance of IDB but the IDB has not yet been upheld, $\mathbf{1}\left(\text{Upheld}\right)_{st}$ indicates whether the court-system has upheld the legality of IDB. The regression includes average income in all ten deciles of the state-year income-distribution, z_{jst}. State-time varying controls $\mathbf{X}_{st}$ include dummies for the existence of a poll tax and a literacy test, both used for voting restrictions; dummies for state election years; dummies for the existence of a state corporate income tax and of right-to-work laws; a continuous measure of the firm-size coverage of state unemployment insurance laws. All regressions include a state-specific linear trend: $\phi_s \cdot t$. In Col. (1), the implied elasticity of [PIT/GDP] is calculated based on the ratio of reduced-form estimates of 1(Uphold); in Col.(3), the implied elasticity is calculated based on the IV-estimated impact of employee-share on [PIT/GDP]. Source: appendix.

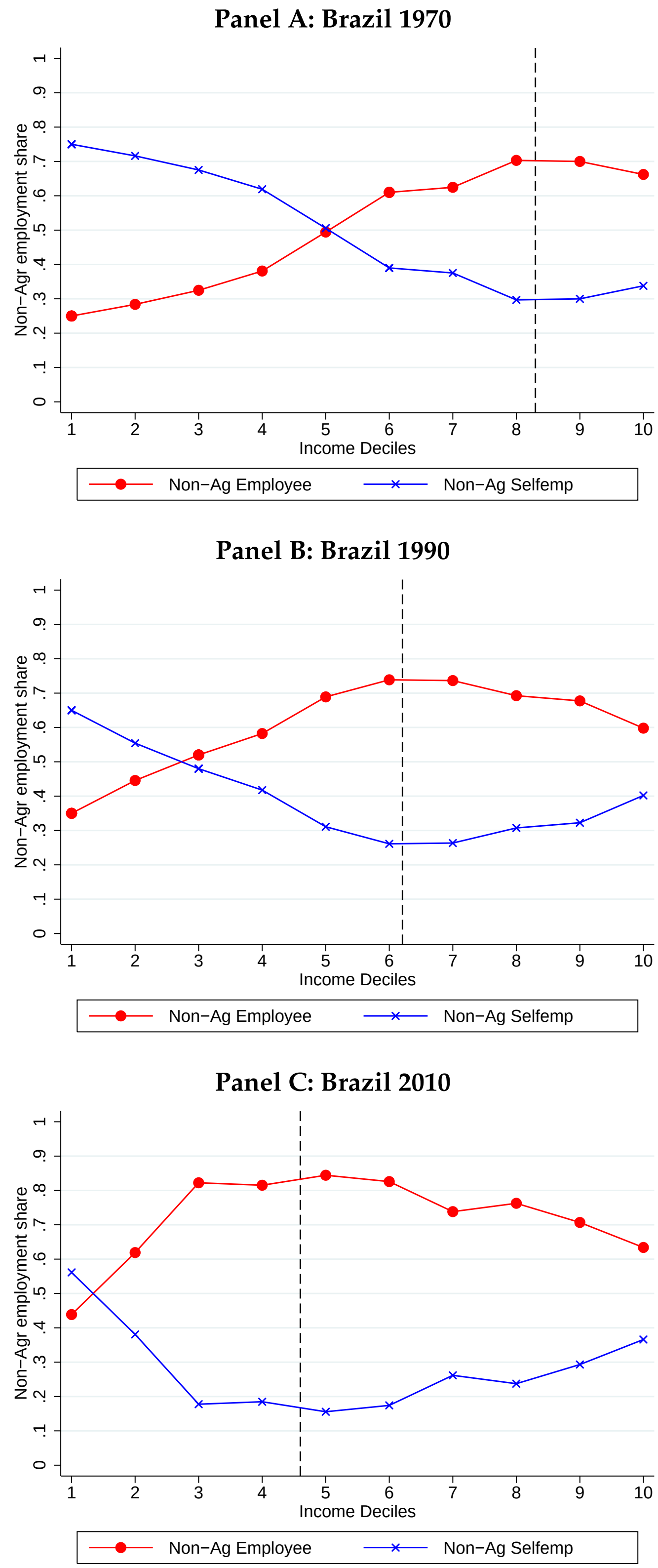
1.7 Appendix

Figure 11: Employee share and agriculture share: representative development profiles



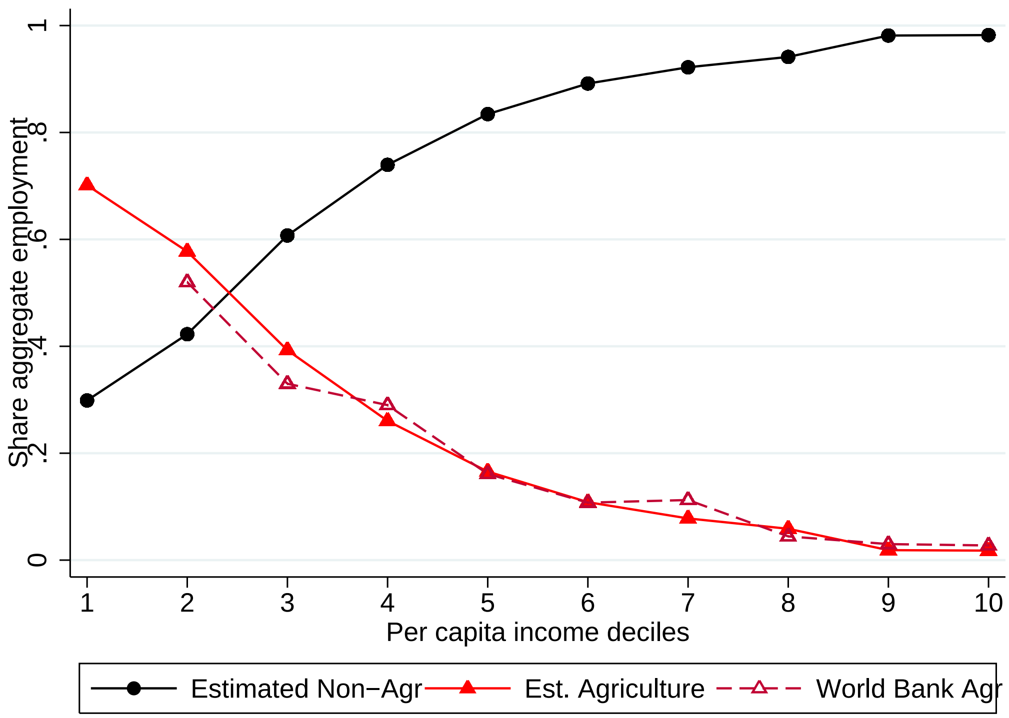
These panels depict the average employment-shares over deciles of the income-distribution. Red dotted (blue cross) observations indicate the employee (self-employed) share of non-agricultural employment in an income decile. Green triangles indicate the agricultural share of total employment in the income decile. A profile is first construced for the 90 individual countries in the cross-section of the micro database. Then an average profile is constructed over the profiles of countries that lie in a bin with indicated average real per capita income. The bins correspond to deciles of the real per capita income distribution across the 90 countries. Source: Appendix.

FIGURE 12: DISTRIBUTION OF EMPLOYEE SHARES AND INCOME TAX EXEMPTION THRESOHLD: BRAZIL 1970-2010



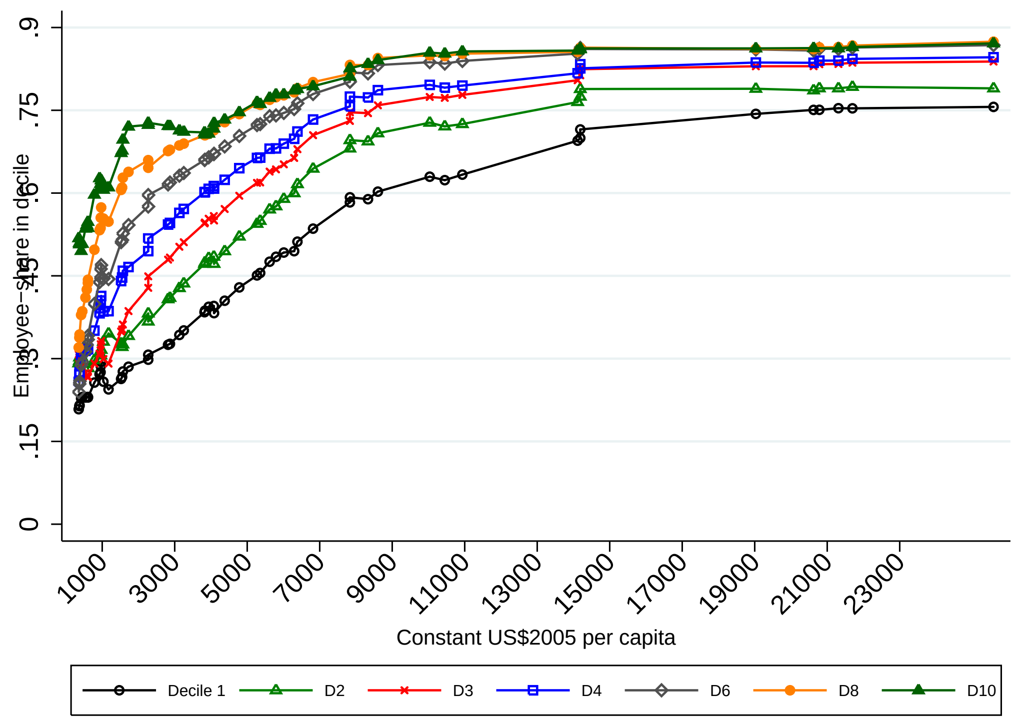
These panels depict the employment shares over deciles of the income-distribution, within Brazil over time 1970-2010. Red dotted (blue) observations indicate the employee (self-employed) share of non-agricultural employment in an income decile.The income-distribution is constructed for the subsample of agents that report being active in the labor force. Each graph is constructed from micro-data and applies populaiton weights to construct a nationally representative sample. Source: Appendix.

FIGURE 13: ROBUSTNESS: OWN AGRICULTURE ESTIMATES VS WORLD BANK ESTIMATES



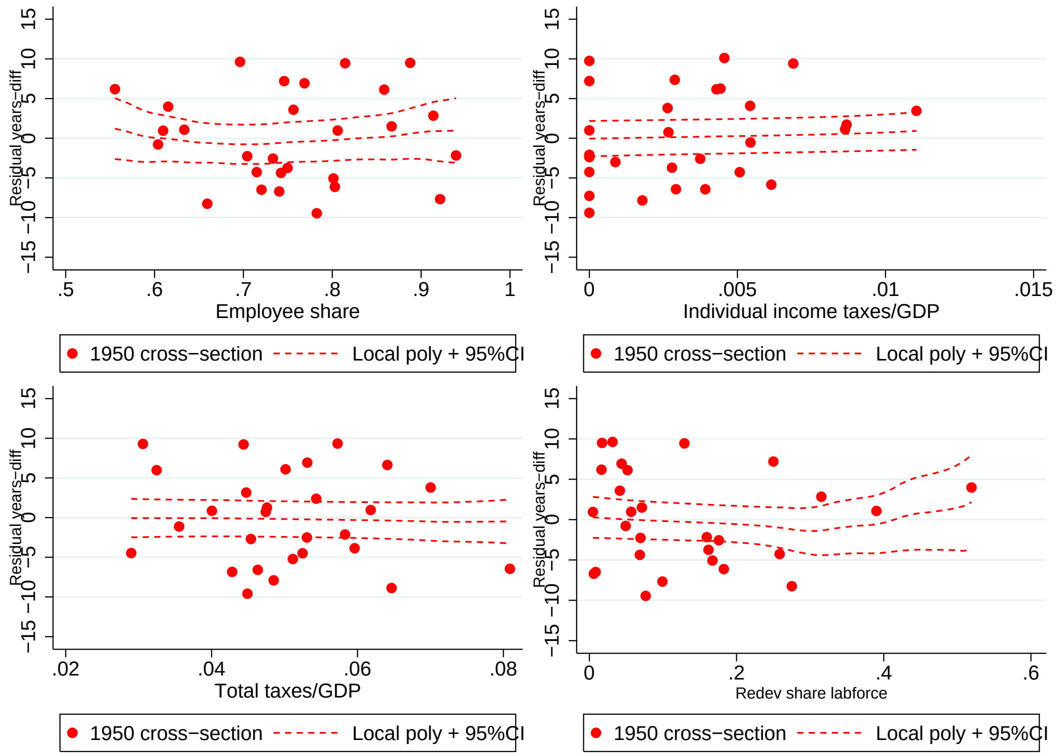
This graph shows estimated weighted employment shares of agriculture and non-agriculture employment-shares across per capita income deciles, based on the cross-country sample of 90 household surveys. Within each decile, the weight assigned to a country is equal to the country-household survey's representative sample share of total household representative sample-size in the decile. The exercise is repeated using country-level aggregate estimates of agriculture share of employment, extraxted from the World Bank World Development Indicators database. In all deciles from the 2nd to the 10th, there is a 100 percent match on countries between my data and the World Bank data. For the countries in decile 1 of my data, there does not exist World Bank agriculture estimates. In the World Bank decile-series, the weight associated to each country is equal to the country's population-share of total population in the decile. Source: appendix.

FIGURE 14: QUANTITATIVE ANALYSIS OF DECILE-SPECIFIC EMPLOYEE SHARE GROWTH: CROSS-COUNTRY



Each curve in this figure provides the shape of the evolution of employee share in decile d throughout the development path, when development is proxied for by cross-country variation, for deciles of the country income-distribution $d \in \{1, 2, 3, 4, 6, 8\}$. The construction of the curves is explained in Section 1.3.

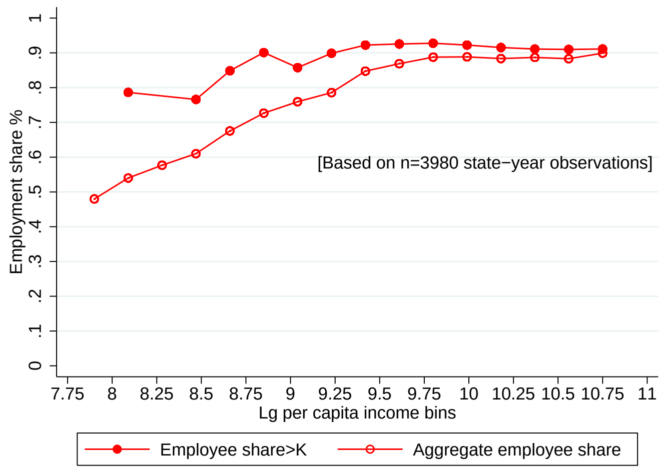
FIGURE 15: CROSS-STATE CORRELATIONS BETWEEN VOTE-UPHOLDING TIME-LAG AND OUTCOMES OF INTEREST



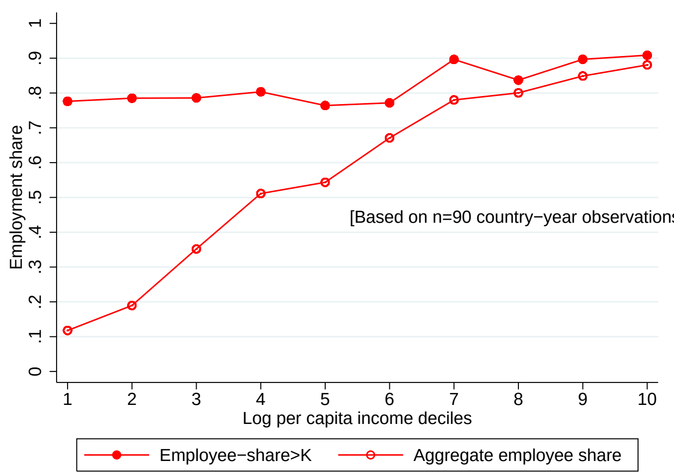
This figure plots the cross-sectional correlation between outcomes of interest in the pre-IDB period and the residual time-lag between vote-in and upholding of IDB program. The residual time-lag is obtained as the residual from a cross-state regression of the time-lag on region dummies and an indicator for civil law origins. The residual is correlated with the (pre-IDB) average employee-share in North-West quadrant; pre-IDB individual income tax to GDP ratio in N-E quadrant; pre-IDB total tax to GDP ratio in S-W quadrant; and, pre-IDB redevelopment-share of the labor force in S-E quadrant. The dashed lines denote the 95% confidence-interval and a fitted local polynomial on the underlying data. The redevelopment-share of the labor force is defined as the total employment-share of counties classified as 'redevelopment areas' by the Area Redevelopment Administration (under the US Department of Commerce). An important characteristic of redevelopment areas was the presence of 'underemployed agents' usually working in 'self-employmrnt and agriculture.' (ARA, 1962)

FIGURE 16: EMPLOYEE-SHARE IN AGGREGATE AND IN PIT BASE: SIMILAR PATTERN ACROSS US STATES AND CROSS-COUNTRY

Panel A: Employee-share in aggregate and on PIT-base: US individual states variation



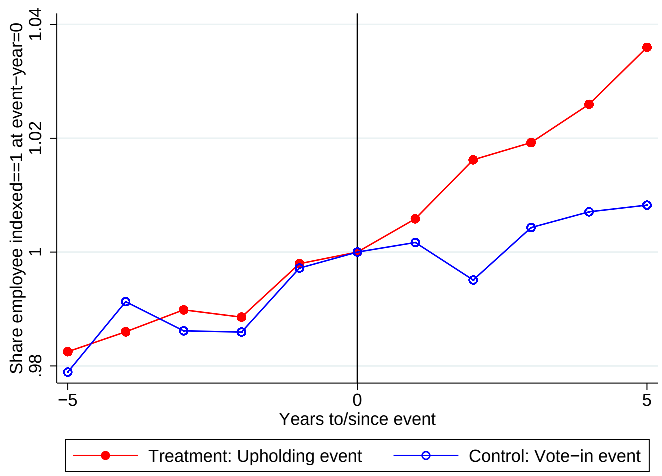
Panel B: Employee-share in aggregate and on PIT-base: cross-country variation



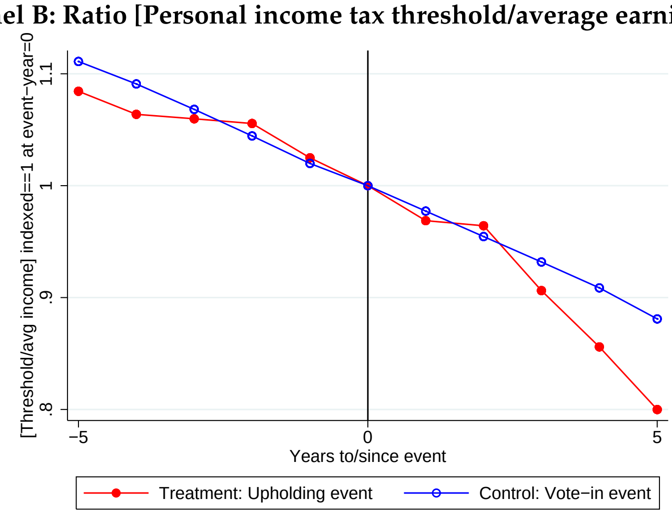
Panel A plots the average share of employees in total employment (hollow-circles) and in employment above the state PIT threshold, in bins of real per capita income, using the full sample of years 1930-2010 for the 48 continental states. Panel B plots the average share of employees in total employment (hollow-circles) and in employment above the country state PIT, using the cross-country sample of 90 countries, in deciles of real per capita income. Employee-share of employment above the state (or country) PIT threshold is defined as the employee-share of employment in the income-distribution which lies above the value of the exemption threshold. Source: appendix.

FIGURE 17: IDB DID RAW DATA IMPACT: DIFFERENT WITHIN-STATE VARIATION

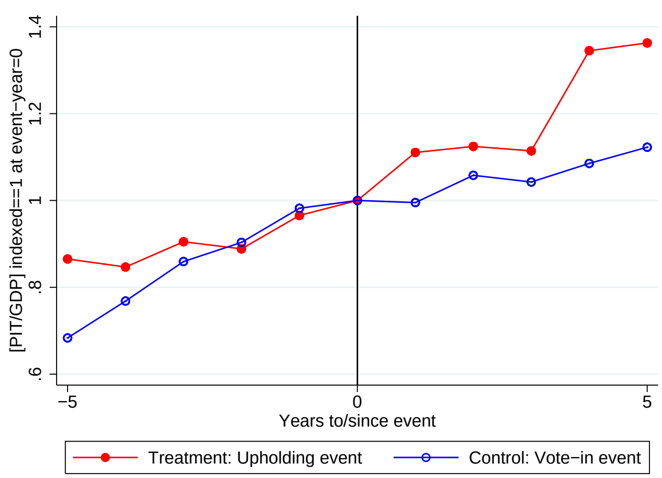
Panel A: Employee-share



Panel B: Ratio [Personal income tax threshold/average earnings]

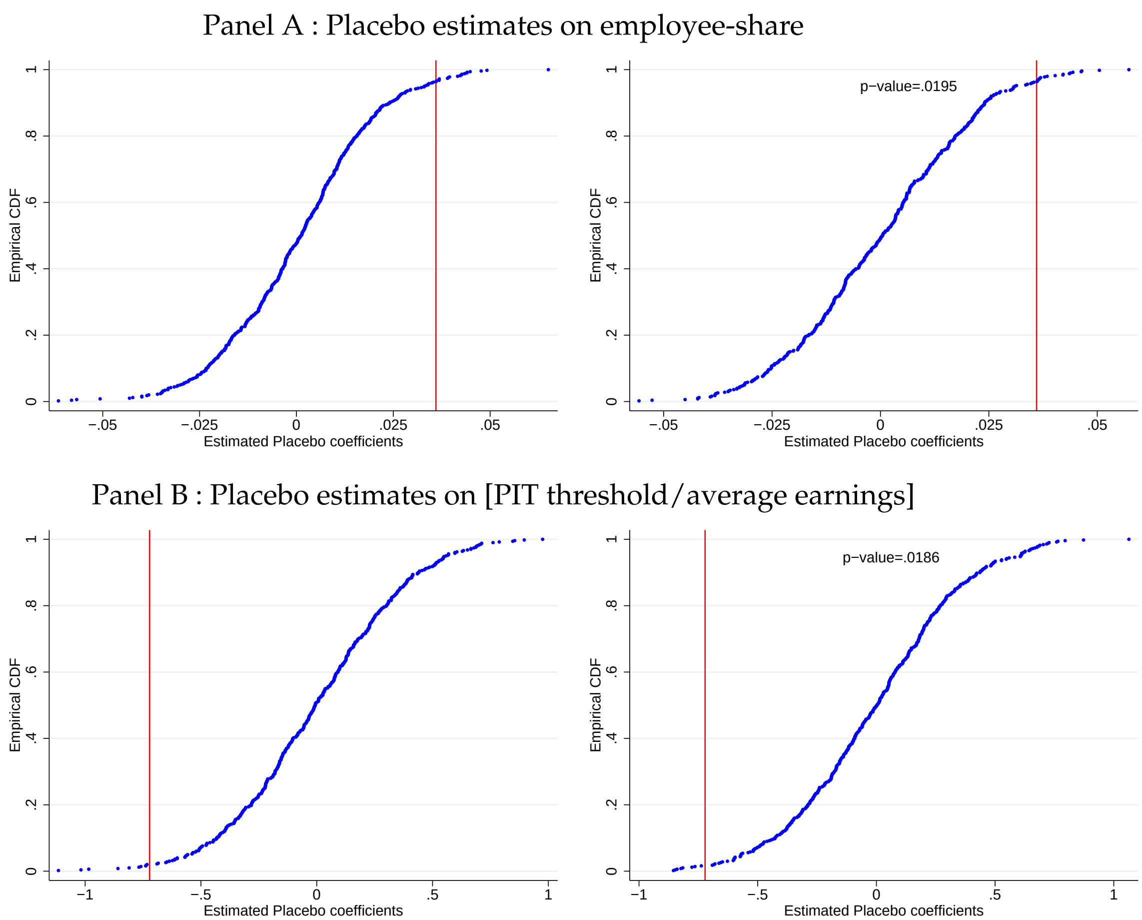


Panel C: Ratio [Personal income tax/GDP]



This figure shows the evolution of employee-share of earned income (panel A), size of PIT base (panel B), and PIT to GDP ratio (panel C), within-state over time for the average treatment state around two events: vote-in, upholding. In the hollow-circle series (filled-circle series), the treatment-control is based on years before and after the event of state House vote-in of the Constitutional amendment to issue IDB (event of state-court upholding IDB). For each series, the evolution is normalized to 1 in the year of the event. In order to show the two events within-state for 5 years after each event, these graph use only the subset of states for which the time-lag between vote-in and upholding exceeded 10 years, and the pre-event period is set to 5 years. In Panel

FIGURE 18: DISTRIBUTION OF PLACEBO ESTIMATES: $\mathbb{1}$ **(UPHOLD)**



These figure plot the empirical distribution of placebo effects for the impact of the change in litigation status, $\mathbb{1}$(Uphold), on the employee-share of employment (Panel A) and on the size of the PIT base (Panel B). To construct the CDF, a placebo consists of a triplet [state]-[year of vote]-[time-lag to upholding] IDB-program (or placebo-twin [year-of-vote]-[time-lag to upholding] for RHS figures within each panel). The specification for employee-share and exemption threshold are the same as in respectively Table 2 and 3. The specification is re-estimated using 500 different random number generator seeds and the point estimates $\hat{\theta}$ of $\mathbb{1}$ (Uphold) are plotted. The vertical line shows the treatment effect estimates on employee-share (PIT base) reported in Table 2 (Table 3). The cumulative percent of placebo-triplet or placebo-twin estimates that exceed (lies below) the actual estimate of employee share (exemption threshold) give a non parametric p-value for test of significance of $\theta = 0$.

TABLE 5: CROSS-SECTIONAL CORRELATIONS CIVIL LAW ORIGINS AND INITIAL CONDITIONS

	Share of employment						PIT base	Tax-take/GDP					
	Employee	Manuf	Services	Govt	Self-emp	Agric	K/y	TotalT	IndincT	CorpIncT	GensalesT	SelectST	LicenceT
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
1(Civil law origin)	.0409	.0524	.0073	-.0051	-.0298	-.0111	-.0096	.0037	-.0017	-.0018	.0057	-.0021	-.0018
	(.0577)	(.0532)	(.0185)	(.0144)	(.0394)	(.0211)	(.1283)	(.0040)	(.0012)	(.0013)	(.0041)	(.0020)	(.0013)
Lg(per cap income)	.4176	.2353	.1355	.0231	-.2559	-.1616	-.4766	-.0240	-.0032	-.0082	-.0042	-.0186	-.0082
	(.1658)**	(.1528)	(.0532)**	(.0414)	(.1131)**	(.0605)**	(.3684)	(.0117)*	(.0036)	(.0039)**	(.0119)	(.0059)***	(.0039)**
Region FE	x	x	x	x	x	x	x	x	x	x	x	x	x
Observations	25	25	25	25	25	25	25	25	25	25	25	25	25

$*, **, ***$ denote significance at the 10%, 5%, 1% level. This table displays correlations between the state time-invarying indicator for civil law origins and a set of outcomes of interest, in the pre-IDB treatment period, for the subsample of states that upheld the IDB program at some point. In all columns, a control for log per capita income is included as well as region fixed effects (4 regions, based on US Census definition). Columns 1-6 display correlations between the civil law dummy and employment-shares. In column 7, K/y is the ratio of state personal income tax exemption threshold to average resident earnings. Columns 8-13 display correlations between the civil law dummy and ratios of tax-revenue to GDP: total tax, individual income tax, corporate income tax, general sales tax, selective sales tax, and licence tax. Source: appendix.

TABLE 6: IDB IMPACT ON CONFOUNDING DETERMINANTS OF EXEMPTION THRESHOLD

	Marginal tax rates			Earnings-structure	Demand for redistribution				Enforcement				
	1st-bracket MTR	Avg (MTR>0)	Top MTR	(Earnings hazard at threshold)	Democratic vote-share	Dem seat-share	1(Dem governor)	Political competition	Number tax agencies	Tax admin earnings relative to avg earnings of			
										Resident	Govt	Attorney Gen	Treasury
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
1(Vote in)	.0018	.0109	.0084	.0484	.0072	-.0166	.0389	-.0015	.0401	.2372	.0274	.0300	.0648
	(.0065)	(.0070)	(.0054)	(.0870)	(.0101)	(.0159)	(.0757)	(.0079)	(.0577)	(.1315)*	(.0374)	(.0337)	(.0540)
1(Upheld)	-.0054	-.0002	.0115	.0326	.0108	.0234	.1049	-.0138	.0326	-.1259	.0544	.0859	.0812
	(.0185)	(.0083)	(.0097)	(.1473)	(.0178)	(.0279)	(.1041)	(.0133)	(.0965)	(.3296)	(.0659)	(.0851)	(.1094)
State-years	2931	2931	2931	2931	2931	2879	2931	2931	2815	2592	2592	2592	2592
States	48	48	48	48	48	47	48	48	48	48	48	48	48

$*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. Time period is 1939-2005. This table reports OLS estimates $\widehat{\alpha}$ and $\widehat{\theta}$ based on the difference-in-difference specification

$$y_{st} = \beta + \alpha \mathbf{1}\left(\text{Vote in}\right)_{st} + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \sum_{j=1}^{10} \omega_j z_{jst} + \lambda \mathbf{X}_{st} + \mu_s + \gamma_t + \phi_s \cdot t + \varepsilon_{st}$$

where s denotes state, t denotes time, $\mathbf{1}\left(\text{Vote in}\right)_{st}$ indicates whether a vote has occured in the state-House to allow issuance of IDB but the IDB has not yet been upheld, $\mathbf{1}\left(\text{Upheld}\right)_{st}$ indicates whether the court-system has upheld the legality of IDB. The vote-in and upholding events are mutually exclusive events. The regression includes average income in all ten deciles of the state-year income-distribution, z_{jst}. The set of state-time varying controls $\mathbf{X}_{st}$ include dummies for the existence of a poll tax and a literacy test, both used for voting restrictions; dummies for state election years; dummies for the existence of a state corporate income tax and of right-to-work laws; a continuous measure of the firm-size coverage of state unemployment insurance laws. All regressions include a state-specific linear trend: $\phi_s \cdot t$.

TABLE 7: DETERMINANTS OF EXEMPTION THRESHOLD AND TAX REVENUE

	Lg(PIT Threshold/average earnings)	Lg(Personal income tax/GDP)	Lg(Corp inc tax/GDP)	Lg(Gen sales tax/GDP)	Lg(Select sales tax/GDP)	Lg(Licence tax/GDP)
Each coefficient corresponds to a regression of the outcome on the given variable	OLS (1)	OLS (2)	(3)	(4)	(5)	(6)
MTR 1st bracket	-.1755	.0260	-.0003	-.0038	.0106	-.0067
	(1.1417)	(.0103)**	(.0025)	(.0066)	(.0043)**	(.0048)
Avg (MTR>0)	.0663	.0398	.0013	-.0053	.0122	-.0029
	(1.2594)	(.0149)**	(.0035)	(.0071)	(.0046)**	(.0025)
Top MTR	-.0403	.0451	.0040	-.0092	.0056	-.0008
	(.8616)	(.0136)***	(.0031)	(.0073)	(.0045)	(.0017)
Earnings-hazard at threshold	-.0541	.0008	.0000	-.0003	-.0003	-.0001
	(.0175)***	(.0002)***	(.0000)	(.0003)	(.0001)***	(.0000)*
Dem vote-share	.5588	.0103	.0025	-.0041	.0053	-.0013
	(.3314)*	(.0048)**	(.0009)***	(.0028)	(.0018)***	(.0008)
Dem seat-share	.1630	.0082	.0019	-.0014	.0053	-.0012
	(.2292)	(.0029)***	(.0005)***	(.0023)	(.0018)***	(.0009)
1(Dem governor)	-.0014	.0000	.0000	-.0003	.0001	-.0000
	(.0298)	(.0004)	(.0001)	(.0003)	(.0001)	(.0001)
Political Comp						
	-.8888	-.0062	-.0013	.0009	-.0049	.0017
	(.3545)**	(.0052)	(.0010)	(.0033)	(.0023)**	(.0009)*
1(Tax-agency consolidation)	-.2294	.0006	.0003	-.0008	-.0004	-.0002
	(.1130)**	(.0003)*	(.0001)**	(.0003)**	(.0001)**	(.0001)*
Earnings-ratio (Tax-administrator/Avg Resident)	.0035	-.0009	.0000	-.0005	-.0000	.0002
	(.0043)	(.0004)	(.0001)	(.0004)	(.0002)	(.0001)
Earnings-ratio (Tax-admininistrator/Avg Govt)	-.1170	-.0034	-.0003	-.0024	-.0011	.0005
	(.0946)	(.0017)*	(.0005)	(.0012)*	(.0008)	(.0005)
Earnings-ratio (Tax-admininistrator/Attorney General)	-.1178	-.0017	-.0007	-.0004	.0002	.0003
	(.1006)	(.0020)	(.0009)	(.0018)	(.0008)	.0005
Earnings-ratio (Tax-admininistrator/Treasury official)	.0118	-.0001	.0000	.0002	-.0002	-.0000
	(.0184)	(.0002)	(.0001)	(.0002)	(.0001)*	(.0000)
State FE	x	x	x	x	x	x
Time FE	x	x	x	x	x	x
State-time controls	x	x	x	x	x	x
States	48	48	48	48	48	48

$*, **, ***$ denote significance at the 10%, 5%, 1% level. Standard errors in parentheses robust to clustering at the state level. This table reports OLS estimates $\widehat{\eta}$ based on the specification

$$y_{st} = \beta + \eta X_{st-1} + \omega \text{income}_{st-1} + \mu_s + \lambda_t + \varepsilon_{st}$$

where s denotes state, t denotes time. income_{st-1} is average per capita income, lagged one period. All regressions include state fixed effects and year fixed effects. Each cell reports the coefficient $\widehat{\eta}$ from a separate regression of the outcome in a given column on the regressor in a given row. Source: appendix.

TABLE 8: IDB IMPACT ON LICENCE TAXES AND SELECTIVE SALES TAXES

	Decile of (Licence tax/GDP)				(Selective sales tax/GDP)			
	Occupation and small-business	Motor-vehicle	Alcohol	Public utility	Motor-vehicle	Alcohol	Public utility	Tobacco
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Vote in)	.1134	.0203	.1339	.1376	-.0004	.0001	.0000	-.0001
	(.2426)	(.3105)	(.1469)	(.5172)	(.0003)	(.0001)	(.0001)	(.0001)
1(Upheld)	-.3350	.3401	-.1816	.5920	.0006	-.0001	-.0002	.0002
	(.1603)**	(.2587)	(.3228)	(.5483)	(.0007)	(.0001)	(.0001)	(.0002)
State-years	2986	2986	2986	2986	2986	2986	2986	2986
States	48	48	48	48	48	48	48	48

$*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. Time period is 1939-2005. This table reports OLS estimates $\widehat{\alpha}$ and $\widehat{\theta}$ based on the difference-in-difference specification

$$y_{st} = \beta + \alpha \mathbf{1}\left(\text{Vote in}\right)_{st} + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \sum_{j=1}^{10} \omega_j z_{jst} + X_{st} + \mu_s + \gamma_t + \phi_s \cdot t + \varepsilon_{st}$$

where s denotes state, t denotes time, $\mathbf{1}\left(\text{Vote in}\right)_{st}$ indicates whether a vote has occured in the state-House to allow issuance of IDB but the IDB has not yet been upheld, $\mathbf{1}\left(\text{Upheld}\right)_{st}$ indicates whether the court-system has upheld the legality of IDB. The vote-in and upholding events are mutually exclusive events. The regression includes average income in all ten deciles of the state-year income-distribution, z_{jst}. The set of state-time varying controls $\mathbf{X}_{st}$ includes dummies for the existence of a poll tax and a literacy test, both used for voting restrictions; dummies for state election years; dummies for the existence of a state corporate income tax and of right-to-work laws; a continuous measure of the firm-size coverage of state unemployment insurance laws. All regressions include a state-specific linear trend: $\phi_s \cdot t$. In columns (1) to (4), the outcome variable is the decile of the ratio [specific license-tax/GDP], where the deciles are constructed using the full state-year sample. In columns (5) to (8), the outcome variable is the ratio [specific selective sales tax/GDP]. Source: appendix.

TABLE 9: IDB IMPACT IN COLLAPSED PRE-POST DID SPECIFICATION

	Employee share channel				Confounding channels					
	Employee share (1)	[Threshold/avg inc] (2)	[Emp-share above threshold] (3)	[IncTax/GDP] (4)	Avg inc (5)	Avg employee inc (6)	Bottom MTR (7)	Top MTR (8)	Pol Comp (9)	Dem vote share (10)
1(Uphold)	.0336	-.7875	.0166	.0019	.0070	-.0416	-.0041	.0096	-.0030	.0091
	(.0101)***	(.3590)**	(.0152)	(.0007)**	(.0098)	(.0281)	(.0059)	(.0065)	(.0152)	(.0218)
States	25	25	25	25	25	25	25	25	25	25
State-years	50	50	50	50	50	50	50	50	50	50

$*, **, ***$ denote significance at the 10%, 5%, 1% level. Standard errors in parentheses robust to clustering at the state level. This table reports OLS estimates $\widehat{\theta}$ based on the difference-in-difference specification

$$\tilde{y}_{st} = \beta + \theta \mathbf{1} \left(\text{Upheld}\right)_{st} + \varepsilon_{st}$$

where s denotes state, t denotes time. $\tilde{y}_{st}$is the residual outcome from: first, running the regression of y_{st} on state fixed effects, year fixed effects, average income in all ten deciles of the state-year income-distribution, dummies for the existence of a poll tax and a literacy test, dummies for state election years, dummies for the existence of a state corporate income tax and of right-to-work laws, a continuous measure of the firm-size coverage of state unemployment insurance laws, but omitting the $1(\text{Vote-in})$ and $1(\text{Uphold})$ dummies; second, constructing the residual outcome from this regression; third, collapsing the outcome by state and pre-post upholding periods. This is a diff-in-diff on the sample of treated states (states that uphold IDB at some point), over the 2-period interval before/after upholding. Source: appendix.

TABLE 10: IDB IMPACT IN SAMPLE OF IDB STATES AND NARROW WINDOW AROUND VOTE IN AND UPHOLDING EVENTS

	Employee share channel				Confounding channels					
	Employee share (1)	[Threshold/avg inc] (2)	[Emp-share above threshold] (3)	[IncTax/GDP] (4)	Avg inc (5)	Avg employee inc (6)	Bottom MTR (7)	Top MTR (8)	Pol Comp (9)	Dem vote share (10)
1(Vote in)	.0036	.3385	.0067	.0004	-.0101	-.0025	-.0001	-.0017	.0075	.0113
	(.0082)	(.2342)	(.0111)	(.0006)	(.0079)	(.0078)	(.0025)	(.0018)	(.0155)	(.0132)
1(Uphold)	.0393	-1.009	.0056	.0019	-.0162	.0043	.0040	-.0041	-.0167	.0071
	(.0167)**	(.3898)**	(.0141)	(.0007)**	(.0151)	(.0079)	(.0071)	(.0027)	(.0215)	(.0191)
States	25	25	25	25	25	25	25	25	25	25
State-years	910	910	910	910	910	891	925	925	910	925

$*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. In this table, the sample is limited to the set of states in which the court system upholds IDB. The sample is also limited to state-year observations which lie between 15 years prior to the vote in event and 15 years after the upholding event. The rows report OLS estimates $\widehat{\alpha}$ and $\widehat{\theta}$ based on the difference-in-difference specification

$$y_{st} = \beta + \alpha \mathbf{1}\left(\text{Vote in}\right)_{st} + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \sum_{j=1}^{10} \omega_j z_{jst} + X_{st} + \mu_s + \gamma_t + \varepsilon_{st}$$

where s denotes state, t denotes time, $\mathbf{1}\left(\text{Vote in}\right)_{st}$ indicates whether a vote has occurred in the state-House to allow issuance of IDB but the IDB has not yet been upheld, $\mathbf{1}\left(\text{Upheld}\right)_{st}$ indicates whether the court-system has upheld the legality of IDB. The vote-in and upholding events are mutually exclusive events. The regression includes average income in all ten deciles of the state-year income-distribution, z_{jst}. The set of state-time varying controls $\mathbf{X}_{st}$ includes dummies for the existence of a poll tax and a literacy test, both used for voting restrictions; dummies for state election years; dummies for the existence of a state corporate income tax and of right-to-work laws; a continuous measure of the firm-size coverage of state unemployment insurance laws. All regressions include a state-specific linear trend: $\phi_s \cdot t$. Source: appendix.

TABLE 11: IDB IMPACT IN CROSS-STATE DID SPECIFICATION

	Employee share channel				Confounding channels					
	Employee share (1)	[Threshold/avg inc] (2)	[Emp-share above threshold] (3)	[IncTax/GDP] (4)	Avg inc (5)	Avg employee inc (6)	Bottom MTR (7)	Top MTR (8)	Pol Comp (9)	Dem vote share (10)
1(Uphold)	.0397	-.7336	.0001	.0014	.0252	-.0008	.0030	.0102	-.0128	.0097
	(.0142)***	(.3279)**	(.0172)	(.0005)**	(.0193)	(.0156)	(.0069)	(.0095)	(.0114)	(.0175)
States	48	48	48	48	48	48	48	48	48	48
State-years	2931	2931	2931	2931	2855	2905	2931	2931	2931	2931

$*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. This table reports OLS estimates $\widehat{\theta}$ based on the difference-in-difference specification

$$y_{st} = \beta + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \sum_{j=1}^{10} \omega_j z_{jst} + \lambda \mathbf{X}_{st} + \mu_s + \gamma_t + \phi_s \cdot t + \varepsilon_{st}$$

where s denotes state, t denotes time, $\mathbf{1}\left(\text{Upheld}\right)_{st}$ indicates whether the court-system has upheld the legality of IDB. The vote-in and upholding events are mutually exclusive events. The regression includes average income in all ten deciles of the state-year income-distribution, z_{jst}. The set of state-time varying controls $\mathbf{X}_{st}$ includes dummies for the existence of a poll tax and a literacy test, both used for voting restrictions; dummies for state election years; dummies for the existence of a state corporate income tax and of right-to-work laws; a continuous measure of the firm-size coverage of state unemployment insurance laws. All regressions include a state-specific linear trend: $\phi_s \cdot t$. Source: appendix.

TABLE 12: IDB IMPACT IN SYNTHETIC MATCHING PAIRED DID SPECIFICATION

	Employee share channel				Confounding channels					
	Employee share (1)	[Threshold/avg inc] (2)	[Emp-share above threshold] (3)	[IncTax/GDP] (4)	Avg inc (5)	Avg employee inc (6)	Bottom MTR (7)	Top MTR (8)	Pol Comp (9)	Dem vote share (10)
1(Uphold)	.0328 (.0074)***	-.6314 (.2519)***	.0692 (.0672)	.0014 (.0006)**	.0460 (.0239)*	1.2381 (.7328)	-.0024 (.0035)	-.0074 (.0072)	-.0187 (.0147)	.0301 (.0369)
States	25	25	25	25	25	25	25	25	25	25
State-years	3050	3050	3050	3050	2955	2964	2964	3050	3050	3050

$*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the level of state synthetic pair in parentheses. Time period is 1939-2005. This table reports OLS estimates based on a difference in difference specification. The regression is done on the sample of IDB states and the synthetic control state paired to each IDB state. The synthetic control is constructed as the weighted average over all non IDB states which maximizes the fit on pre-upholding employee share in the IDB state. The full set of economic covariates and political covariates are used as variables to predict the fit. The regressions include a full set of state FE, year FE, synthetic pair FE. Source: appendix.

TABLE 13: IDB IMPACT USING ALTERNATIVE CONTROLS FOR TIME

	Employee share channel				Confounding channels					
	Employee share (1)	[Threshold/avg inc] (2)	[Emp-share above threshold] (3)	[IncTax/GDP] (4)	Avg inc (5)	Avg employee inc (6)	Bottom MTR (7)	Top MTR (8)	Pol Comp (9)	Dem vote share (10)
1(Vote in)	.0013	.2179	-.0132	.0008	.0811	.0024	.0092	.0000	.0006	.0044
	(.0066)	(.1711)	(.0167)	(.0015)	(.0483)*	(.0088)	(.0061)	(.0000)	(.0083)	(.0116)
1(Uphold)	.0338	-.8188	.0280	.0015	-.0021	-.0128	-.0073	.0094	-.0118	.0223
	(.0097)***	(.3470)**	(.0211)	(.0006)**	(.0158)	(.0114)	(.0119)	(.0120)	(.0153)	(.0186)
States	48	48	48	48	48	48	48	48	48	48
State-years	2931	2931	2931	2931	2931	2854	2931	2931	2931	2931

$*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. Time period is 1939-2005. This table reports OLS estimates $\widehat{\alpha}$ and $\widehat{\theta}$ based on the difference-in-difference specification

$$y_{st} = \beta + \alpha \mathbf{1} \left(\text{Vote in}\right)_{st} + \theta \mathbf{1} \left(\text{Upheld}\right)_{st} + \sum_{j=1}^{10} \omega_j z_{jst} + \lambda \mathbf{X}_{st} + \mu_s + \gamma_t + \text{ß} \left(\mathbf{D}_{s1930} \times [t - 1930]\right) + \varepsilon_{st}$$

where s denotes state, t denotes time, $\mathbf{1}\left(\text{Vote in}\right)_{st}$ indicates whether a vote has occured in the state-House to allow issuance of IDB but the IDB has not yet been upheld, $\mathbf{1}\left(\text{Upheld}\right)_{st}$ indicates whether the court-system has upheld the legality of IDB. The vote-in and upholding events are mutually exclusive events. The regression includes average income in all ten deciles of the state-year income-distribution, z_{jst}. The set of state-time varying controls $\mathbf{X}_{st}$ includes dummies for the existence of a poll tax and a literacy test, both used for voting restrictions; dummies for state election years; dummies for the existence of a state corporate income tax and of right-to-work laws; a continuous measure of the firm-size coverage of state unemployment insurance laws. $\mathbf{D}_{s1930}$ is a vector of 'structural' determinants of employment: illiteracy rate, population density, and urban share of population. They are obtained in 1930, prior to IDB program, and interacted with linear time trends $[t - 1930]$ to allow their impact to vary over time.

TABLE 14: IDB IMPACT ALLOWING FOR INDEPENDENT PATH OF CIVIL LAW STATES

	Employee share channel				Confounding channels					
	Employee share (1)	[Threshold/avg inc] (2)	[Emp-share above threshold] (3)	[IncTax/GDP] (4)	Avg inc (5)	Avg employee inc (6)	Bottom MTR (7)	Top MTR (8)	Pol Comp (9)	Dem vote share (10)
1(Vote in)	.0160	.1802	-.0099	.0005	-.0174	.0035	.0034	.0110	.0016	.0037
	(.0099)	(.2091)	(.0192)	(.0015)	(.0129)	(.0107)	(.0058)	(.0075)	(.0079)	(.0113)
1(Uphold)	.0463	-.9814	.0305	.0015	.0480	-.0076	-.0087	.0068	-.0029	.0271
	(.0116)	(.3080)***	(.0250)	(.0006)**	(.0488)	(.0130)	(.0108)	(.0106)	(.0154)	(.0197)
States	48	48	48	48	48	48	48	48	48	48
State-years	2931	2931	2931	2931	2931	2854	2931	2931	2931	2931

$*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. Time period is 1939-2005. This table reports OLS estimates $\widehat{\alpha}$ and $\widehat{\theta}$ based on the difference-in-difference specification

$$y_{st} = \beta + \alpha \mathbf{1}\left(\text{Vote in}\right)_{st} + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \sum_{j=1}^{10} \omega_j z_{jst} + X_{st} + \mu_s + \gamma_t + \check{}_t \left(\mathbf{1}\left(\text{Civil Law}\right)_{\mathbf{s}} \times \gamma_t\right) + \varepsilon_{st}$$

where s denotes state, t denotes time, $\mathbf{1}\left(\text{Vote in}\right)_{st}$ indicates whether a vote has occurred in the state-House to allow issuance of IDB but the IDB has not yet been upheld, $\mathbf{1}\left(\text{Upheld}\right)_{st}$ indicates whether the court-system has upheld the legality of IDB. The vote-in and upholding events are mutually exclusive events. The regression includes average income in all ten deciles of the state-year income-distribution, z_{jst}. The set of state-time varying controls $\mathbf{X}_{st}$ includes dummies for the existence of a poll tax and a literacy test, both used for voting restrictions; dummies for state election years; dummies for the existence of a state corporate income tax and of right-to-work laws; a continuous measure of the firm-size coverage of state unemployment insurance laws. $\mathbf{1}\left(\text{Civil Law}\right)_s$ is a state specific time invariant dummy which takes the value of 1 if, by the time of American acquisition, the colonizers of the state had civil law legal systems. The civil law dummy is fully interacted with the set of year dummies γ_t. Source: appendix.

Employment structure: cross-country and within-country data

I discuss in turn the data-collection for the cross-country set of household surveys and for the historical surveys within-US over time and within-Brazil over time.

Cross country household-surveys

The database contains microdata collected from 90 countries around the world, to document on changes in employment structure transformation in as many incremental stages over development as possible. Construction of the data-set was built using three criteria: to obtain household surveys with information on income (as opposed to only expenditure) and employment for as large a sample as possible; to obtain one survey per country in the most recent year possible; to obtain surveys across all levels of development. Basic demographic and health surveys were discarded because they lack the required information on employment and income. Labor force surveys were used, which contain basic demographic information, labor force attachment information, employment information, and detailed sources of income. Living conditions surveys were also used, which in addition to the content of the labor force survey, contained information on expenditure and possibly health and education. Whenever both types existed, the living conditions survey is preferred over the labor force survey, for three reasons. First, the living conditions survey usually contains information on a broader range of income-sources which, especially in the context of less-developed countries, can be quite important in order to construct the lower deciles of the country's income-distribution. Second, it is not always clear what the underlying sample-design is for the labor force survey, and it could potentially omit individuals which in the context of this study should be included in the survey, such as casual wage-day laborers and household family workers; on the other hand, the scope of a living conditions survey is usually to assess the conditions of a nationally representative sample of individuals, which should include all the alternative work type patterns. Third, the sample-size of a living condition survey is typically larger than that for a labor force survey, which does not have to imply better quality of data, but usually is due to sampling-design which attempts to survey all geographical areas in the country.

Based on these criteria, the resulting data-collection resulted in 90 household surveys, which are summarized in Table . 64 of the 90 surveys are in the form of living condition household surveys, the remainder in the form of labor force surveys. The emphasis in the data-collection on the ability to construct personal income measures based on reported sources of income rather than expenditure resulted in only two surveys using expenditure as the underlying variable for construction of the earned income-distribution.

All surveys are referenced in the International Household Survey Network.

Within-country survey data: US 1870-2010, Brazil 1970-2010

The historical Federal profiles in the US between 1950 and 2010 were constructed using the decennial Census samples, extracted from the IPUMS USA database. Each Federal profile between 1950 and 2010 was constructed in the same way as for the profiles in the individual states in the same year, discussed in the section below.

Before 1950, the decennial Census does not report total personal income at the individual level. The 1940 1 percent sample does contain wage and salary income, but no business income nor farm income, which are required to construct a personal gross income distribution. I therefore resort to the 1935-36 Study of Consumer Purchases. The scope of the study was to "ascertain for the first time in a singe national survey the earning and spending habits of inhabitants of large and small cities, villages, and farms" (ICPSR Study 8908, 2009). The survey was the result of a joint effort by the Bureau of Labor Statistics and the Bureau of Home Economics of the Department of Agriculture, and is meant to have been the sampling-methodology predecessor for the income-component in Census. The survey contains both a labor force component, where respondents gave information on income and housing, and for a subset of the total sample, a living conditions component where respondents gave additional information on expenditure. The primary sampling units were chosen to represent "the demographic, regional, and economic characteristics of the United States". (ICPSR, 2009) From these areas, a randomly selected group of approximately 700,000 families were screened in a first wave. From this first wave, 300,000 families were chosen to supply basic income and housing info, and a subset of 61,000 families were selected to provide additional expenditure information. It is important to understand the selection criteria into the different waves. The ICPSR accompanying documentation explains that in order to be selected out of the first wave, the requirements were: "families include at least two members, with husband and wife married for at least one year, and with no more than the equivalent of ten boarders for the survey year (...) farm families had to live in a setting that met the Census definition of a farm; the family itself must operate the farm (or in the southeast, be a sharecropper) and have conducted farming activities for at least one year" (ICPSR Codebook, 2009). Families were admitted to the first wave "without restriction in terms of occupation, income, employment status, or whether they were drawing or had drawn relief during the year." Selection into the second-wave where the survey included expenditure components, was based on the following criteria: "non-farm families must have had at least one wage earner in a clerical, professional, or business occupation. A minimum income for the survey year of $500 was required in the largest cities and $250 in the smaller cities and rural areas (...) Families that had received relief were excluded from this third wave." These criteria produce a highly selected sample for the second-wave respondents, and hence I base the analysis on the sample of first-wave respondents.

The ICPRS data-sample that I use for the 1935 Federal profile is based on random subsamples of approximately 5,000 families who only completed the first-wave 'labor force' component of the survey.[27] The ICPSR subsample was created in the following way: "a sampling fraction of 1 schedule for entry for every 83 schedules counted was chosen" from the urban sample, creating 3200 schedules from the larger urban areas and 1800 schedules from the more rural areas"; the ICPSR-sample consists of schedules "spread across both the rural and urban portions of the original investigation." I code as an employee any individual respondent who reports being a "salaried worker/wage earner". I code as self-employed any respondent who reports being "self-employed", and any respondent who does not specify a type of work but declares to be working, is above age 20 and who has substantial work-related income. I rank invidual based on the reported total income, in order to construct the Federal income-distribution. I then estimate the employee and self-employment employment-shares, together with the agriculture industry-share, in all deciles of the Federal income-distribution. The procedure is further explained in Section 1.4.2.

The 1935-36 survey marked a clear shift in focus of the surveys conducted by the Bureau of Labor Statistics. Indeed, the surveys carried out prior to the 1930s focused on measuring family income and expenditure patterns of the U.S. employed workers and their families. Consequently, the available surveys, including the "Cost of living in the United States, 1917-1919" (ICPRS 7711, 1986) and the "Cost of living of industrial workers in the United States and Europe, 1888-1890" (Haines, 2006) contain data from families of wage earners or salaried workers in industrial locales scattered throughout the U.S. In order to construct a historical profile before the 1930s, I use data from Lindert & Williamson (2016), an on-going data-collection project to assess incomes in the U.S. between 1650 and 1870. Unlike previous work which approaches the measurement of income during this historical period from the production-side or the expenditure-side (including Berry, 1968; Gallman, 2000), Lindert & Williamson build estimates of

27 The ICPSR data available from the 1935-36 survey has also been used in Collins & Wanamaker (2014), Costa (2001), Margo (1993).

income based on personal income records, assembling nominal earnings from free labor and property income.

The approach to estimating income in Lindert & Williamson derives from combining information about income and labor force participation counts across occupation-space-time. This amounts to building 'social tables' across occupations within a given space-time frame, and the approach is conceptually similar to a social accounting matrix, which was used in studies of development economics (and at the World Bank) in the 1970s and 1980s. The authors provide a tremendous effort to capture all occupation categories in a given space-time, and draw on data from local tax assessments and occupational directories for 'registered' occupations, and local censuses for 'unregistered occupations'. These same data-sources usually provide counts of the total number of individuals across the different occupations. The authors combine previous work (including Blodget, 1806; Main, 1965) with new estimates from local sources to derive personal earned income across occupation-space-time. In some instances, the occupation-space-time income reported was not at the annual level, and the authors bring the estimates to such level by making assumptions on the full-time number of hours spent (the assumptions are discussed in Lindert & Williamson, 2016). The authors also collect data on property income by assuming rates of return on wealth estimates that vary across occupation-space-time, and combine this with earned income to derives measures of total income. I construct a historical 1870 profile based on the data kindly provided by Peter Lindert.[28] This cross-section builds upon the 1870 1 percent US Census sample delivered to the authors by IPUMS USA, which also delivered the authors with sampling weights at the individual-level. The 1 percent sample contains space-occupation counts, which are then merged with the authors' estimate of total income at the same level. I extend their analysis and classify all available occupation categories as either self-employed or employee, and code agriculture versus non-agriculture, which in this setting amounts to farming versus non-farming. I use a text-algorithm which assigns self-employed vs employee status according to keyword-search in the occupation title. Per example, all occupations where a reference is made to 'manufacturing' or to 'manager' are coded as employee cells. The enumerator instructions for the sample-design are particularly useful for my exercise in that they highlight very clearly the need to distinguish between self-employed and employee status: "Do not call a man a 'shoemaker', 'bootmaker', unless he makes the entire boot or shoe in a small shop. If he works in a boot and shoe factory, say so (...) Cooks, waiters, etc., in hotels and restaurants will be

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Results from the project relating to earlier historical periods are forthcoming in Lindert & Williamson (2015). The data for 1870 will be used in their forthcoming book Williamson & Lindert (2016).

reported separately from domestic servants." Coding workers attached to farming was based on a keyword-search that would turn up the words 'agriculture' or 'farming'. I use this classification to construct employee and self-employed shares of employment in the ten deciles of the income-distribution, applying the sampling-weights provided by IPUMS USA. The income-distribution reported in Panel B of Figure 1, and Panel D of Figure 13 compute total income as the sum of earned income and property income, but the results are robust to constructing the distribution based only on earned income.

As a robustness check to the US within-country over time results on the changes in the distribution of employment-shares, I construct a within-country profile over time for Brazil between 1970 and 2010. The data is extracted from IPUMS International. My selection-criteria was to choose a country with a lover level of per capita income, to document on employment structure transformation within-country at lower initial levels of development. IPUMS International also has long-run data from Indonesia (1976-2010) and Mexico (1960-20100, but unlike Brazil, the personal income variable is not consistently recorded throughout the data-period. For Brazil, I do not include the 1960 estimate because the reporting-basis for total personal income changes substantially between 1960 and 1970 (the 1960 income is only reported in broad income ranges). Between 1970 and 2010, the definition of individual total income has remained fairly constant, including the constant reference across surveys to monthly earned income. The employment variables are constructed based on the class of worker categories, where I code an individual as employee if she reports 'wage/salary' as her main occupation, and as self-employed if she reports 'self-employed' or 'unpaid worker' as the main occupation. I drop from the sample all respondents who respond 'not in universe' to the class of worker question. I build the income-distribution based on total earned income, and estimate employment-shares across the income-deciles using individual population weights. The results from this exercise are reported for 1970, 1990, 2010 in appendix Figure 18.

US state-year data

Employment-shares at level of state-year

I construct the aggregate employment-share variables using decennial data at the state level between 1910 and 2010. The data was extracted from IPUMS USA. In each decennial data-extract, I exclude from the sample any individual for whom the general class of worker variable is 0 ("N/A"), and, if the variable exists, any individual who reports total personal income either equal to 9999999 ("N/A") or strictly negative. In the IPUMS USA data, total personal income corresponds to the respondent's total pre-tax personal income or losses from all sources for the previous year. I code as self-employed (employee) in non-agriculture a respondent who responds 'self-employed' ('works for wages') in the class of worker category and who does not respond working in 'agriculture, forestry and fishing' using the 1950-based industry classification. Note how the occupation-classification in IPUMS USA is consistent with the classification used in the cross-development sample, in the sense that if a respondent has multiple sources of employment, the occupation-category concerns the work in which they spent the most time during the reference day or week.

Within each decennial extract, I apply person-weights to estimate, for each state, the representative total number of respondents, the total number of agriculture respondents, the total number of non-agriculture employee respondents, and the total number of non-agriculture self-employed respondents. These variables permit the construction of employment-structure variables. Per example, the employee-share variable is constructed as the ratio of total number of non-agriculture employee respondents to the total number of respondents. In addition, I construct the employment-shares by industry, using the 1950-based industry classification.

I construct continuous measures of employee counts using the historical series 'State and Area Employment, Hours and Earnings" collected by the Bureau of Labor Statistics. State agencies report data from a sample of establishments in all nonagricultural activities, including government. The series provides a breakdown of employees by consistently defined major industry categories in all states and years between 1939 and 2002. Unfortunately this series only provides data on average earnings and average hours worked beginning in the 1970s.

Employment-shares at level of income decile-state-year

I construct the employment-shares by income-decile of the income-distribution of each state, in years 1935 and 1950-2010. The 1950-2010 data is again extracted from the IPUMS USA database. The definitions of type of work and industry are the same as those used to construct the state-year aggregate employment-shares. I rank all respondents within a given state according to the reported total personal income. The personal income reported measures each respondent's total pre-tax personal income. Importantly, throughout the sample-period, this measure is largely comparable: it includes in all samples, the wage, farm and business components. I then apply person-weights and partition each state's income-distribution into ten deciles, that is, into ten bins of equal sample-size. Within each decile, I estimate the conditional proportions of employees, self-employed and agriculture workers to construct the employment-shares by income-decile.

In years before 1950, the decennial US Census does not provide reported income and occupation-category at the level of the individual. I use the 1935-36 Study of Consumer Purchases in the United States, which had the scope to 'ascertain for the first time in a single national survey the earning and spending habits of inhabitants of large and small cities, villages, and farm. I access this data under the ICPSR data-archive reference #08908. I discuss the 1935 data-sample and construction of variables in more detail in thr sub-section above. I construct the deciles of the state-specific income-distribution and estimate the employment-shares specific to each decile-state. I use these data to construct the profile of employment-share and self-employment share over deciles of each state's income-distribution, for all continental states, between 1935 and 2010.

I use this data to construct the variable measuring the employee-share above the PIT-threshold K. I first locate the decile of the state-year income-distribution in which the state-year PIT threshold is located in. I then recompute the non-agriculture employee-share of all respondents whose income lie in a decile weakly above the PIT-decile.

Earnings-shares

The earning-structure is constructed for all states and all years between 1929 and 2001 by combining the two historical series SA5H and SA5 'Personal Income by Major Components and Earnings by Industry' published by the US Bureau of Economic Analysis. The denominator for earnings-structure is systematically line-item 45 'Net earnings by place of residence', which equals total earnings less contributions for government social insurance plus 'adjustment for residence'. The employee-share uses in the numerator line-item 90 'private non-farm earnings'; the self-employed share of income uses line-item 70 'proprietors' income'; the manufacturing-share of income uses line-item 400 'Manufacturing'; the services share of income uses line-item 800 'Services'; the government-share of income uses line-item 900 'Government and government enterprises'.

The line-item 45 is also used as the denominator y to construct the ratio of the PIT-threshold K to average earnings, K/y. Importantly, this measure y of personal earnings excludes transfers from all levels of government.

State tax-revenues

The tax-revenue sources by state and year are based on the historical series on state government finances published by the US Census Bureau. The State Government Finances series publishes series on yearly tax-revenue collected over the fiscal year of each state. I proxy for tax-take by constructing the ratio of a given total tax-revenue collected to the total personal income in the state, where the denominator is based on the BEA historical series of state personal income. This tax-take ratio differs from the usual construction of the variable, where the denominator use a measure of aggregate output; however, GDP data at the state-level in the US is only available from 1963 onwards, and I follow the US states literature (e.g. Barro and Sala-i-Martin, 1992; Besley et al., 2010) in using state personal income as a measure of state output. The line-codes corresponding to the various tax-takes constructed are: personal income tax, T40; corporate net income tax T41; General sales tax, T09; selective sales tax, 'total select sales tax', sum of T10, T11, T12, T13, T14, T15, T16, T19) ; license taxes, 'total licence taxes', sum of T20, T21, T22, T23, T24, T25, T27, T28, T29.

State personal income tax structure: K, τ, and K-reforms

To construct measures of the state PIT-base and state PIT-rate structure, I use data from the Bakija (2009) historical U.S. Federal and state income tax calculator program. I thank Jon Bakija for kindly providing me access to the calculator. The data models federal and state personal income taxes based on legal text, covering the period from 1900 to 2007 for state income tax laws. I construct the PIT-exemption threshold K for an individual earner who files under the status of being single, who reports having one dependent, and who claims the standard deduction. I choose a single-earner as the filing-type for two reasons: first, because I construct the deciles of the income-distribution based on ranking of total personal earned income; second, to avoid dealing with a large set of rules concerning interactions between spouses in terms of exemption values when filing under married status. Under the standard deduction (as opposed to the itemized deduction), the filer does not deduct state personal income tax from her federal income tax liability, which provides additional incentives for the filer to under-report state income taxes: I choose this filing-choice to more closely fit the under-reporting model in Section 1.5. Evidence from IRS statistics suggest that standard deduction filers are systematically more prevalent at lower levels of gross income (the Statistics of Income series on individual income tax returns regularly documents on this: see e.g. IRS, 1982). I set K to 0 in all years where the state did not have a personal income tax. I then construct the ratio K/y where y is the state-year per capita personal income, extracted from the historical US BEA series.

I use the same state tax calculator to construct measures of the tax-rate structure. The calculator provides data on the number of brackets for the specific filing-type, and the marginal tax rate which applies to each bracket. Some states have multi-bracketed structure with progressive marginal tax rates, other states apply a single-rate flat income tax over all taxable income. Some states also feature a 'zero-th bracket' on which there is a zero percent tax rate, but this is a very rare event. I construct the 1st bracket marginal tax rate, the average of all marginal tax rates strictly greater than zero, and the top-bracket marginal tax rate. All these variables are set to 0 if the state does not levy a personal income tax.

The measure for PIT-threshold reforms is coded in the following way. Before the early 1990s, following the move to inflation-adjust the income-tax brackets of the Federal schedule, no state provided inflation-adjustment to its nominal-valued (bracketed) own income tax. Thus the dollar value of the calculated threshold K would remain constant unless a reform occured to change K. I therefore code a year of reform as a year, before 1990, during which the calculated nominal value of K changed. Second, I construct the state-specific empirical cumulative distribution of K-reforms over time. This cumulative distribution controls for the the large cross-state heterogeneity in frequency of K-reforms.

State-time covariates

The poll tax and literacy test dummies and election-year dummies are taken from Besley et al. (2010). They provide state-time varying measures of the share of the state population subject to either a literacy test or a poll tax. Prior to the 1965 Voting Rights Act, such measures were in place in predominantly Southern states. The 1965 VRA gave the Attorney General the authority to appoint federal examiners to oversee voter registration in states using literacy or qualification tests, and the power to seek legal action against poll taxes as a prerequisite for voting in state elections. The authors use variation in these dummies to instrument for political competition, which they find to have a positive impact on the share of non-farm income.

I construct proxies for the state-year policy environment. These different proxies are meant to capture variation in state-policies which may have affected location decisions of private firms. The choice of proxies is based on historical readings which provide qualitative evidence that these policies contributed to the workforce transition into manufacturing and services jobs, especially in Southern and Midwestern states (Cobb, 1993; Newman, 1984). First, a dummy for the existence of a corporate income tax is constructed, which takes value 1 in all years in a state where there exists such a tax-base. The date of creation of state-CIT is taken from Table 4.1 of Newman (1984). The dummy for existence of right-to-work laws was extracted from Besley et al. (2010). Right-to-work laws make it illegal to demand that employees join a union, or to automatically deduct union fees from wages. The continuous measure of state unemployment insurance firm-size coverage is taken from the historical publication series 'Significant Provisions of UI State Laws" published y the US Department of Labor

(Dept Labor, 1937). I download all publications between 1937 and 1979. In each state-year, I code the firm-size coverage, that is the lower-bound on firm-size above which an employee in a given firm is entitled to receive state UI benefits. This measure is defined consistently over the entire series. I also wanted to code the employer UI-contribution, expressed as a percentage of wages, but this measure is not consistently reported throughout. Federal-time varying regulation provided an upper-bound on the allowed firm-size, but states were free to legislate in order to define a firm-size below the Federally mandated size. Some states chose to lower the firm-size coverage early on, ahead of Federal regulations, while some states followed the Federal upper-bound throughout time. After 1979, Federal regulations extended coverage to all firms with one employee or more, and I code the state-time coverage as equal to 1 from 1979 onwards.

I control several proxies for income. I use either the log of per capita personal income based on the BEA historical series, or I use the ten measures of average personal income in all deciles of the state-year income distribution, constructed using the same methodology as for employment-shares at decile-state-year level.

Finally, in the appendix I provide controls for time which interact cross-sections of three structural variables in 1930 with a linear time trend. The first structural variable is population density, which is defined as the average population per square mile. This variable is extracted from the US Census resident population data. The second structural variable is the illiteracy rate, by state. This variable is defined as the share of population 14 years old and over who is unable to read and write a simple message in English or another language. The cross-section is extracted from the 1962 Current Population Report, published by the Department of Commerce. The third structural variable is the rural share of total population, based on the US Census data on rural and urban populations. The US Census defines urban areas as densely developed territory which encompasses residential, commercial and non-residential urban land uses.

State-year outcome variables

I proxy for the earnings-hazard at the PIT-threshold in the following way. First, I locate the income-decile of PIT-K in a given state-year. I then calculate the mass of income above K, in terms of deciles, and the average income in the decile of K. Total mass of income over the income-distribution is normalized to 1, as in the model of Section 1.5, and thus each decile represents a mass of 0.1. The sum of decile-mass above K is a proxy for $1 - H(z_K)$. I multiply the average income in the decile of K by 0.01: this is the proxy for $h\left(z_K\right) \cdot z_K$. The ratio of the proxies of $1 - H(z_K)$ to $h\left(z_K\right) \cdot z_K$ is the empirical proxy for the hazard-ratio.

I construct proxies for tax enforcement capacity in two ways. The data are based on the historical series of the Book of the State, published annually from

1993 until today by the Council of State Governments. First, I collect data at the state-year level on the number of agencies administering major taxes: property, income, sales, gasoline, motor vehicle, tobacco, death, liquor. I code the total number of state tax agencies in operation in every state-year. This variable is available from 1939 to 2009. Second, I collect state-year data on the annual salaries of the chief state administrative official in different departments: revenue-collection and taxation; treasury; Attorney general. I construct the ratio of the annual salary in revenue-taxation relative to the salary in the Treasury and relative to the salary as Attorney General. I also construct the ratio of annual administrative salary in revenue-taxation relative to average government-employed earnings, using the historical earning-shares data described above. These relative-earnings ratios are available between 1948 and 2009. These variables are intended to proxy for investments in enforcement capacity, through consolidation of the number of tax agencies, and by funding higher wages to tax administrators. These variables represent, to my knowledge, the first long-run time-series evidence on the long-run evolution of proxies for tax administrative capacity of individual states in the US.

Finally, I use data from Besley et al. (2010) to build proxies for political outcome-variables. I use their measure of party-neutral political competition, which is defined as minus the absolute value of the deviation of the democratic vote-share from 50 percent, where the vote-share is the average vote-share over all state-wide races. I also use a dummy for whether the governor in the state-year is a Democrat. Further, I use the Democratic vote-share averaged across all state-wide elections, and the Democratic seat-share in the state House.

Industrial Development Bonds program: litigation status, volume issued

The data on the timing of vote-in and upholding decisions is built from four main sources: legal reviews in Abbey (1966) and Pinksy (1963), administrative lists (US ACIG, 1963 and 1974), and administrative reports (Institute of International Law and Economic Development, 1978).

I collect data on number of IDB issues and the dollar amount of each IDB-issue using the historical records of Moody's Municipal and Government Manual of Issuers. Though IDB-issues do appear in earlier series such as the 1970 series and the 1972 series, I focus on the 1974 series because it effectively supersedes the previous series in terms of recorded issues for the historical period that I am interested in. I cannot use publications from 1975 onwards because the reported issues bundles together pollution control bonds and IDB-bonds. Using the Special Features section of the 1974 publication, I code at the state-year level, the total number of issues and the total nominal dollar value that correspond to these issues. I then construct the cumulative series of IDB number of issues and of IDB volume of issues over time, by state. The constructed data-series represents, to the best of my knowledge, the first complete historical time-series on issuance of IDB-debt by state.

TABLE 15: CROSS-COUNTRY HOUSEHOLD SURVEYS

	$ Per cap income	Survey type	Sampling properties	Distribution
Argentina	5700	Living conditions	Urban representative	Income
Australia	32443	Living conditions	Nationally representative	Income
Austria	36446	Labor force	Nationally representative	Income
Azerbaijan	651	Living conditions	Nationally representative	Income
Bangladesh	371	Living conditions	Nationally representative	Income
Belgium	34009	Labor force	Nationally representative	Income
Belize	3269	Labor force	Nationally representative	Income
Bolivia	1081	Living conditions	Nationally representative	Income
Brazil	5271	Living conditions	Nationally representative	Income
Bulgaria	4339	Living conditions	Nationally representative	Income
Cambodia	590	Living conditions	Nationally representative	Income
Cameroon	931	Living conditions	Nationally representative	Income
Canada	35277	Living conditions	Nationally representative	Income
Chile	8217	Living conditions	Nationally representative	Income
China	1308	Living conditions	Urban representaitve	Income
Colombia	3841	Living conditions	Nationally Representative	Income
Costa Rica	5180	Living conditions	Nationally Representative	Income
Cote d'Ivoire	939	Living conditions	Nationally representative	Income
Czech Republic	14640	Labor force	Nationally representative	Income
Dem Rep Congo	127	Living conditions	Nationally representative	Income
Denmark	46540	Labor force	Nationally representative	Income
Dominican Republic	4493	Labor force	Nationally representative	Income
Ecuador	3210	Living conditions	Nationally representative	Income
Egypt	1550	Living conditions	Nationally representative	Income
El Salvador	3005	Living conditions	Nationally representative	Income
Estonia	10370	Living conditions	Nationally representative	Income
Ethiopia	229	Living conditions	Non-capital representative	Income
Finland	38065	Labor force	Nationally representative	Income
France	33819	Living conditions	Nationally representative	Income
Germany	36127	Labor force	Nationally representative	Income
Ghana	501	Living conditions	Nationally representative	Income
Greece	21310	Labor force	Nationally representative	Income
Guatemala	2092	Living conditions	Nationally representative	Income
Honduras	1490	Living conditions	Nationally representative	Income
Hungary	10937	Living conditions	Nationally representative	Income
Iceland	51528	Labor force	Nationally representative	Income
India	687	Living conditions	Nationally representative	Income
Indonesia	1731	Living conditions	Nationally representative	Income
Ireland	44583	Labor force	Nationally representative	Income
Israel	22169	Living conditions	Nationally representative	Income
Italy	29163	Labor force	Nationally representative	Income
Jamaica	4150	Living conditions	Nationally representative	Income
Japan	36817	Living conditions	Nationally representative	Income
Jordan	2817	Living conditions	Nationally representative	Income
Kenya	524	Living conditions	Nationally representative	Income

Per capita income is in $US constant. Labor-force surveys contain information on demographics, income and employment; living condition surveys in addition contain information on expenditure, and possibly health and education. Distribution refers to the variable at the individual level used to construct the country income-distribution.

TABLE 16: CROSS-COUNTRY HOUSEHOLD SURVEYS (CONTINUED)

	$ Per cap income	Survey type	Sampling properties	Distribution
Lithuania	9426	Living conditions	Nationally representative	Income
Luxembourg	80276	Labor force	Nationally representative	Income
Malawi	222	Living conditions	Nationally representative	Income
Mexico	8336	Living conditions	Nationally representative	Income
Mongolia	861	Labor force	Nationally representative	Income
Morocco	2315	Living conditions	Nationally representative	Income
Namibia	4086	Living conditions	Nationally representative	Income
Netherlands	41110	Labor force	Nationally representative	Income
Nepal	351	Labor force	Nationally representative	Income
Nicaragua	1175	Living conditions	Nationally representative	Income
Niger	272	Living conditions	Nationally representative	Expenditure
Nigeria	1034	Living conditions	Nationally representative	Income
Norway	64545	Labor force	Nationally representative	Income
Pakistan	765	Labor force	Nationally representative	Income
Panama	6287	Labor force	Nationally representative	Income
Papua New Guinea	1111	Living conditions	Nationally representative	Income
Paraguay	1553	Living conditions	Nationally representative	Income
Peru	3565	Living conditions	Nationally representative	Income
Poland	9044	Living conditions	Nationally representative	Income
Puerto Rico	21959	Labor force	Nationally representative	Income
Romania	3447	Living conditions	Nationally representative	Income
Russia	6386	Living conditions	Nationally representative	Income
Rwanda	211	Living conditions	Nationally representative	Income
Serbia	3733	Living conditions	Nationally representative	Income
Sierra Leone	311	Living conditions	Nationally representative	Income
Slovakia	14162	Labor force	Nationally representative	Income
Slovenia	19054	Living conditions	Nationally representative	Income
South Africa	5794	Living conditions	Nationally representative	Income
South Korea	19528	Labor force	Nationally representative	Income
Spain	25596	Labor force	Nationally representative	Income
Sri Lanka	1486	Labor force	Nationally representative	Income
Sudan	780	Living conditions	Nationally representative	Income
Sweden	41041	Labor force	Nationally representative	Income
Switzerland	53340	Living conditions	Nationally representative	Income
Tanzania	452	Living conditions	Nationally representative	Income
Tajikistan	375	Living conditions	Nationally representative	Income
Taiwan	37500	Living conditions	Nationally representative	Income
Tunisia	3847	Living conditions	Nationally representative	Expenditure
Turkey	8493	Living conditions	Nationally representative	Income
Timor-Leste	550	Living conditions	Nationally representative	Income
Ukraine	1974	Living conditions	Nationally representative	Income
United Kingdom	37899	Labor force	Nationally representative	Income
United States	43952	Living conditions	Nationally representative	Income
Uruguay	6280	Living conditions	Nationally representative	Income
Venezuela	5880	Labor force	Nationally representative	Income

Per capita income is in $US constant. Labor-force surveys contain information on demographics, income and employment; living condition surveys in addition contain information on expenditure, and possibly health and education. Distribution refers to the variable at the individual level used to construct the country income-distribution.

2 Norms, Enforcement, and Tax Evasion[29]

Abstract

This paper studies individual and social motives in tax evasion. We build a simple dynamic model that incorporates these motives and their interaction. The social motives underpin the role of norms and is the source of the dynamics that we study. Our empirical analysis exploits the adoption in 1990 of a poll tax to fund local government in the UK, which led to widespread evasion. We also exploit a series of natural experiments due to narrow election outcomes, which induce shifts into single-majority local governments and lead to more vigorous enforcement of local taxes. The econometric results are consistent with the model's main predictions on the dynamics of evasion.

[29] We are grateful to Juan Pablo Atal, Pierre Bachas, Richard Blundell, Tom Cunningham, Gabriel Zucman, and a number of seminar participants for helpful comments, to Dave Donaldson, Greg Kullman and Gordon Ferrier for help with data, and to the ERC, the ESRC, Martin Newson and the Torsten and Ragnar Söderberg Foundations for financial support.

2.1 Introduction

Sustaining the high government spending levels typical of most advanced economies requires high fiscal capacity. The latter depends not only on institutions to detect and punish tax non-compliance, but also on intrinsic motives that curb individual desires to cheat the government. Individual taxpayers may also care about how their tax compliance is perceived by others, leaving a role for social interactions to shape norms of compliance. Even though this idea has been widely discussed, it remains poorly understood from a theoretical and empirical point of view. One issue concerns the robustness of social tax-compliance norms: can they be eroded by shocks and how do norms persist over time? Another concerns the interaction between the individual and social motives to comply with taxes: do these weaken or reinforce each other? This paper attempts to make progress on both sets of issues by exploiting a unique natural experiment in fiscal history, the poll tax introduced by the Thatcher government in the early 1990s and which led to high levels of evasion.

The first contribution of the paper is theoretical. It lays out a model in which individuals can evade taxes, and where the incentives depend on public tax enforcement (detection and fines), intrinsic motivation, and how not paying taxes affects their reputation. The latter creates a role for social interactions and yields a micro-foundation for social norms in tax compliance. The model is used to study the equilibrium dynamics of norms and tax evasion. We derive responses of tax compliance to a temporary shock to intrinsic motivation and a permanent shock to tax enforcement. The main role of the model is to guide the empirical analysis.

The second contribution of the paper is empirical. We exploit two kinds of natural experiments in the United Kingdom. The first is the poll tax itself which replaced a long-standing system of taxation by local governments (councils) based on rental values of properties. Officially named the Community Charge, the poll tax was levied on an equal basis within a jurisdiction for all citizens of voting age. The tax was deemed unfair by many, triggered mass evasion, and lead to a U-turn which restored a property-based tax only three years later. As the breakdown in compliance was heterogeneous across councils, it can be interpreted as an array of (council-specific) temporary shocks to the intrinsic motive to pay taxes. The second set of natural experiments exploits narrow election victories in non-synchronous council elections in the period following the poll tax. Since shifts in and out of single-party majority are systematically associated with higher tax enforcement and lower tax evasion, they correspond to (council-specific) permanent shocks to tax enforcement.

Section 2.2 of the paper formulates our model. We build on insights from several pieces of literature. The traditional economic literature starts with Allingham and Sandmo (1972), who examine the gamble citizens take by not complying with their taxes, given the probability to be detected in an audit and the existing legal penalties (see e.g., Cowell 1990 and Slemrod and Yitzhaki 2002 for surveys). Our model incorporates such material compliance motives.

Tax compliance also depends on intrinsic motives. A variety of different labels are used for this in the existing literature: Gordon (1989) refers to "individual morality", Cowell (1990) to "stigma", Erard and Feinstein (1994) to feelings of "guilt and shame", and Torgler (2007) to "tax morale"[30]

Social interactions may also be important in the creation of norms of compliance.[31] This is highlighted in the quote above by Posner (2000). Although concrete applications to tax compliance are few, social scientists have developed different approaches to social norms. One simple way of modeling them is to put a desire to conform with others directly into preferences. A literature in social psychology — started by the experiments reported in Asch (1955) — suggests such an interpretation. Economists have taken a similar approach, e.g., Akerlof and Yellen (1990) in their study of efficiency wages as a reciprocal norm of fair effort for a fair wage. Another approach to micro-founding norms, as in Kotlikoff, Persson and Svensson (1988) or Kandori (1992), is to embed behavior in a repeated game where the threat of dynamic punishments for norm-violation play a key role.

The approach taken here is based on the desire of individuals to build a reputation. In this we follow Benabou and Tirole (2011).[32] The approach gives way to a range of comparative statics which does not assume a priori whether social norms crowd in (complement) or crowd out (substitute) standard economic incentives. The model in section 2.2 extends Benabou and Tirole's model to a dynamic setting to study the interplay among extrinsic, intrinsic and social motives in tax evasion. We then study the comparative statics and dynamics in response to temporary shocks to the intrinsic motives to pay taxes, and to a permanent shock to tax enforcement.[33]

Section 2.3 of the paper describes the empirical context and our panel data on tax evasion, enforcement, and political majorities over thirty years (1980-2009) in the 346 councils of England and Wales. Since we do not have individual data on compliance, we focus on the average level of compliance at the council level.[34] We construct a consistent measure of tax evasion across three regimes:

[30]Luttmer and Singhal (2014) provide a review of the literature on intrinsic motivation in the context of tax compliance. A recent contribution by Dwenger et al. (2014) studies compliance with the local Church tax in Germany; despite a lack of both perceived and actual enforcement, the authors find that 20 percent of individuals pay at least as much tax as is owed, providing strong evidence of intrinsically motivated compliance behaviour.

[31]See, for example, Myles and Naylor (1996).

[32]A somewhat different signalling approach is taken in Posner (2000).

[33]The dynamic model we formulate has some similarities with Lindbeck, Nyberg and Weibull's (2009) model of individual incentives and social norms in unemployment insurance.

[34]Del Carpio (2013) uses household level data to study the determinants of evasion on the prop-

the (property-based) domestic rates from 1980 to 1989, the (person-based) poll-tax from 1990 to 1992, and the (property-based) council-tax from 1993 and onwards. Figure 19 shows average tax evasion across all councils for each year in our sample. Evasion before the poll tax was around 3% and on a declining trend. The poll-tax period saw an abrupt upward shift, as average evasion reached between 10 and 15%. After the return to property-based taxes in 1993, evasion returned gradually towards pre-poll-tax levels. This time pattern squares well with the idea that shocks to intrinsic motives to pay taxes might have quite persistent effects, due to the dynamics of social norms.[35]

Since elections are staggered across councils and years, we get many close election outcomes in each calendar year. We exploit the fact that these close elections trigger shifts into or out of single-party control that are as good as random. Moreover, single-party majorities are systematically associated with less tax evasion. Figure 20 shows a standard Regression Discontinuity Design (RDD) diagram, where each dot represents half a percent of the sample (about 50 council-election years), and the horizontal axis shows the seat share of the largest political party minus the 50-percent cutoff. The quadratic control functions left and right of the cutoff suggest that a narrow shift into a single-party majority decreases tax evasion by 1-2 percentage points. While this is a reduced-form relation, we also consider how discretionary tax enforcement varies with majority shifts and show that the drop in tax evasion most likely reflects more vigorous enforcement by single-party council majorities.

Section 2.4 of the paper presents our econometric evidence, beginning with tax-evasion changes in the poll-tax regime. These are heterogeneous across councils which can be attributed to different demographic, economic and political compositions of any particular council. Non-parametric estimates of tax-evasion dynamics after the poll-tax show clearly that evasion falls more slowly in councils where it was high during the poll-tax period — as predicted by the theory in Section 2.2. Moreover, this result does not reflect pre-trends and is robust to alternative empirical specifications.

Next, we analyze the effects of narrow shifts into single-party majorities after 1993. Our RDD approach shows that these shifts are associated with stricter enforcement effort by councils and lower tax evasion. There is both an immediate "impact effect" and a sustained effect over time, suggesting that we can indeed interpret a narrow political shift as a permanent enforcement shock. We show that the results are robust to a number of alternative RDD specifications and that the RDD is properly identified.

Finally, we exploit the earlier persistence findings and ask if the level of poll-

erty tax in Peru. She conducts an RCT at the municipality level, where the treatment is information disclosure about average compliance, enforcement or both. She finds a positive impact for all three treatments, but no statistical significance for any pairwise difference in estimated effects.

[35] This persistence is consistent with the evidence presented in Helliwell, Wang and Xu (2013).

tax evasion shapes the effects on tax evasion in the council-tax period. We find that an enforcement shock has a smaller effect in councils with high levels of poll-tax evasion — a result that confirms the predicted interaction effect between individual and social motives for tax compliance.[36] As with the results on persistence, these findings are robust to alternative measurements and definitions. Section 2.5 concludes the paper. Some auxiliary empirical findings which do not appear in the main text are available in the Appendix.

2.2 Theory

2.2.1 The model

Our theoretical framework is based on Benabou and Tirole's (2011) model of social norms, but augments that model to include some adaptive dynamics. Time is measured in discrete periods, indexed by t, that correspond to years in the data. There are C councils, indexed by c, each of which is populated by a continuum of agents of size one. Agents in council c at date t have the same exogenous constant income y and tax liability $x_{c,t}$ and must decide whether or not to comply: $e \in \{0, 1\}$ where $e = 1$ denotes evasion. As in the classic Allingham-Sandmo framework, the standard incentive to pay taxes $m_{c,t}$, is given by the expected cost of getting caught (probability times punishment), which is determined by the council.

Agents may also be intrinsically motivated to pay their taxes. The mean level of such motivation, denoted $i_{c,t}$, may vary between councils and over time. However, agents also vary in their intrinsic motivation. We let higher values of ν denote a greater proclivity to pay taxes – i.e., a higher intrinsic motivation. For example Helliwell (2003) reports a positive correlation with expressing a desire never to cheat on taxes and subjective well-being. We assume that v has a symmetric distribution with a single mode (median and mean) at zero, which is the same in all councils and time periods. The p.d.f and c.d.f. of this distribution are denoted by $g(v)$ and $G(v)$, respectively.

In addition to intrinsic motivation ν, agents care about their reputation with a desire to be perceived as more intrinsically motivated. Following Benabou and Tirole (2011), this component of their utility depends on the signal that compliance sends about their type. In our setting, it becomes a source of social interaction within a council since the effect on the signal of complying depends on the behavior of others.

[36] The existing empirical literature on norms and tax compliance has mostly relied on attitudinal data from surveys (see, for example, Wenzel, 2004). Such data has allowed researchers to investigate a wide variety of factors which support the willingness to comply with taxes (see, Hoffman et al, 2008, for a review). Among these, perceptions of fairness is frequently invoked as a crucial factor together with knowledge of the tax system.

Summarizing this discussion, the preferences of a type v agent are given by:

$$y - x_{c,t}\,(1-e) - (m_{c,t} + i_{c,t} + v)e + \mu E(v \mid e)\,. \tag{11}$$

The final term in (11) is the signalling term, which represents the influence on the agent of his/her reputation (or self image) – i.e., how society views her (or how she views herself) given her evasion decision, e. It depends on the average value of the intrinsic motivation parameter ν in the parts of the population that either evades or pays their taxes faithfully. The agents who pay their taxes will be among those with a high value of v. The key assumption is that people in society are highly regarded when they are good citizens, in this case tax-payers, and hence appear to have a high value of ν. Parameter μ is the weight that agents place on their social reputation relative to their individual well-being.

The agent chooses e to maximize (11). The resulting decision rule is is characterized by a cutoff value, $v^*_{c,t}$ for each council which is defined by

$$m_{c,t} - x_{c,t} + i_{c,t} + v^*_{c,t} - \mu\left[E\left(v|e=1\right) - E\left(v|e=0\right)\right] = 0\,. \tag{12}$$

Everybody with $v < v^*_{c,t}$ chooses to evade. Hence, the fraction of agents that evade their taxes in council c in year t is given by $G(v^*_{c,t})$.

There are three parts to (12). The term $m_{c,t} - x_{c,t}$ represents the material cost/benefit from compliance while the second term $i_{c,t} + v^*_{c,t}$ is the critical level of intrinsic motivation. The third term, $\mu\left[E\left(v|e=1\right) - E\left(v|e=0\right)\right]$, represents the reputational cost of evading, which depends on how such acts are perceived. This is where social interactions enter the picture. Another way to read the cutoff condition is that the individual marginal cost of evading (the first two terms) exactly balances the social benefit of complying (the third term).

We assume that the reputation cost of evasion is updated only with a one-period lag. This is justified by the reasonable conjecture that individuals can observe evasion behavior only in the previous year. Then, the reputational cost depends on the lagged cutoff $v^*_{c,t-1}$ which determines the fraction of evaders in period $t-1$. This will allow us to study the dynamic paths of evasion with state dependence.

Formally, we have

$$\begin{aligned} -\mu\left[E\left(v|e=1\right) - E\left(v|e=0\right)\right] &= \mu[E(v \mid v > v^*_{c,t-1}) - E(v \mid v < v^*_{c,t-1})] \\ &\equiv \mu\Delta\left(v^*_{c,t-1}\right)\,. \end{aligned} \tag{13}$$

By definition of the truncated means, the value of $\Delta\left(v^*_{c,t-1}\right)$ is always positive, i.e., there is a positive gain in reputation from paying taxes faithfully.[37] All of the model dynamics, as well as the social interaction that shapes how the norm varies over time, is embodied in (13).

Substituting (13) into (12), yields a non-linear first-order difference equation:

$$v^*_{c,t} = x_{c,t} - m_{c,t} - i_{c,t} - \mu\Delta(v^*_{c,t-1}) \; . \tag{14}$$

Standard arguments show that the equilibrium dynamics are determined by the derivative Δ_v, which is (minus) the root of the difference equation. We will assume throughout that $-1 < -\Delta_v < 1$ which implies that if x, m, and i are constant, then tax evasion will converge to a steady state implicitly defined by

$$\hat{v}(x, m, i) = x - m - i - \mu\Delta(\hat{v}(x, m, i)) \; .$$

If $\Delta_v < 0$, convergence is monotonic, while if $\Delta_v > 0$ it is oscillatory. The derivative Δ_v thus determines both the slope and steepness of the "impulse-response function"of tax evasion. This will give us a way to interpret some of the dynamics in the data.

To understand the sign of Δ_v, suppose that $v^*_{c,t-1}$ goes up so that more people evade in year $t-1$. Then, the two truncated means that enter into (13) both go up, so the effect on the reputational term $\Delta(\cdot)$ is ambiguous in sign. Since the density is single peaked, the results in Jewitt (2004) imply that Δ has a unique interior minimum, which is located at zero (due to the symmetry of the distribution). Hence $\Delta_v < 0$ for low values of $v^*_{c,t-1}$, a case where few people are evaders, and $\Delta_v > 0$ for high values of $v^*_{c,t-1}$, the case of high evasion.[38]

Using (14), we see that, when $\Delta_v < 0$, then individual evasion decisions across years are strategic complements – i.e., if more people evade in council c in year $t-1$ ($v^*_{c,t-1}$ goes up) then this leads to even more people in this council evading in the subsequent year t (so that $v^*_{c,t}$ goes up as well). This is the case that most earlier models of tax evasion focused on by assumption. However, it is quite possible that $\Delta_v > 0$ in which case tax-evasion decisions are strategic substitutes. This implies that higher evasion in year $t-1$ leads to lower evasion at t.

When it comes to empirical estimation, the model property that the effects of social norms on individual behavior in the current period are related to behavior

[37] However, for the council population as a whole, social reputation is a "zero-sum game". Specifically, summing the reputational terms across all individuals in equilibrium, we obtain:

$$\int^{v^*} E(v \mid v < v^*_{c,t-1})dv + \int_{v^*} E(v \mid v > v^*_{c,t-1})dv = 0$$

[38] Benabou and Tirole (2011) refer to paying taxes as a "respectable act" in the first case, and an "honorable act" in the latter case. In between it is a "modal act".

within the group in the previous period means that we do not have to deal with the reflection problem uncovered and discussed by Manski (1993).

2.2.2 Comparative Dynamics

To illustrate the model's comparative dynamics, we now consider the adjustment path to two different shocks: (i) a permanent change in enforcement, $m_{c,t}$ and (ii) a temporary shock to the intrinsic motivation within a council district, $i_{c,t}$.

Suppose we begin from a steady state $\hat{v}_c$ and then $m_{c,t}$ increases permanently from some year T onwards, i.e.,

$$m_{c,t} = \begin{cases} \hat{m}_c \text{ for } t < T \\ m'_c > \hat{m}_c \text{ for } t \geq T \end{cases}$$

In the data, this experiment will correspond to a positive enforcement shock, which is triggered by a shift towards a one-party majority due to a narrow majority for the largest party. With $\mu = 0$, so that there is no role for norms, the model would predict an immediate adjustment to a new steady state level of compliance. However, with $\mu > 0$, this adjustment will be more gradual.

To simplify the analysis, we assume that the enforcement shock is small enough that we stay in the same region where evasion activities are either strategic complements ($\Delta_v < 0$) or strategic substitutes ($\Delta_v > 0$) throughout the adjustment towards the new steady state. Then, we have the following result:

Proposition 1 *The effect over time – the impulse-response function – of permanently stricter tax enforcement depends on the sign of Δ_v. If $\Delta_v < 0$, the proportion of tax evaders declines monotonically from $G(\hat{v}_c)$ to a new value $G(\hat{v}_c^-)$* with $\hat{v}_c^- < \hat{v}_c$. *If $\Delta_v > 0$, the proportion of tax evaders, undershoots its new steady-state value and displays oscillating fluctuations towards its new steady state value, which is lower than initially but higher than the one with $\Delta_v < 0$, i.e., $G(\hat{v}_c) > G(\hat{v}_c^+) > G(\hat{v}_c^-)$.*

[begin Proof] Repeated iteration on the difference equation (14) gives the following first-order approximation for the impulse-response function – i.e., the year-on-year change in the cutoff value

$$v^*_{c,t} - v^*_{c,t-1} = -(-\mu)^{t-1-T}[\Pi_{s=T}^{s=t-2}\Delta_v(v^*_{c,s})](m'_c - \hat{m}_c) \,. \tag{15}$$

If $\Delta_v(v^*_{c,t}) < 0$ for all t, it follows that $v^*_{c,t} - v^*_{c,t-1}$ is always negative with $\mid v^*_{c,t} - v^*_{c,t-1} \mid$ declining in t. If $\Delta_v(v^*_{c,t}) > 0$ for all s, the sign of $v^*_{c,t} - v^*_{c,t-1}$ takes positive values for even $t - T$ and negative values for odd $t - T$.

Since $m'_c - \hat{m}_c$ is small, we get the following first-order approximation to the difference in steady-state cutoffs:

$$-\frac{1}{1 + \mu\Delta_v(\hat{v}_c)}(m'_c - \hat{m}_c) \,. \tag{16}$$

Because the social multiplier in (16) is larger or smaller than 1 depending on the sign of $\Delta_v(\hat{v}_c)$, we get $\hat{v}^+_c > v^-_c$. The undershooting result (for $v^*_{c,t}$) in the strategic-substitutes case is obtained by evaluating (15) $t = T + 1$ and $t = T + 2$ and comparing the results to (16). The results for the share of tax evaders follow trivially, because c.d.f. G is increasing in $v^*_{c,t}$ [end Proof]

This proposition characterizes the adjustment from a given initial condition, depending on the sign of Δ_v, and forms our first empirical prediction. We now consider what can be said about the adjustment in different councils that start out with different levels of tax evasion. Under the (relatively) weak assumption that $\Delta_{vv} > 0$,[39] we obtain a simple corollary:

Corollary 1 *Suppose we compare two councils with different initial share of tax evaders* $G(\hat{v}^{low}_c) < G(\hat{v}^{high}_c)$. If $\Delta_{vv} > 0$, *the same permanent enforcement shock has an ambiguous/a larger effect on the incidence of tax evasion in the council with the lower share of evaders if both* $\hat{v}^{low}_c$ *and* $\hat{v}^{high}_c$ *are low/high.*

To see this, note that the enforcement effect on the share of tax avoiders is given by

$$-g(\hat{v}_c)\frac{1}{1 + \mu\Delta_v(\hat{v}_c)}(m'_c - \hat{m}_c) \,.$$

A lower share of tax evaders is associated with a lower initial cutoff value $v^*_{c,t}$ which, in turn, makes Δ_v lower and the 'social multiplier' for the cutoff value $\frac{1}{1+\mu\Delta_v(\hat{v}_c)}$ correspondingly higher. But the enforcement effect also depends on the fraction of agents who find themselves around the shifts in cutoffs $\hat{v}^{low}_c$ and $\hat{v}^{high}_c$, as measured by the respective densities $g(\hat{v}_c)$. If the share of tax evaders in both councils is large – so both cutoffs are higher than the minimum where $\Delta_v = 0$ – a lower share of tax avoiders is associated with a higher density.[40] This reinforces the effect of the larger social multiplier.

However, in the empirically more relevant case when the initial share of tax evaders is small (lower than one half), a lower share of tax avoiders is associated

[39] This basically rules out initial values of $\hat{v}_c$ in the very tails of the distribution for v – see Figure 1b in Benabou and Tirole (2011).

[40] Because the underlying distribution of v is unimodal and symmetric around zero, the minimum value of Δ_v occurs at $v = 0$. Thus the density $g(\hat{v}_c)$ is declining (increasing) in $\hat{v}_c$ for cutoffs above (below) zero.

with a lower density, which makes the overall effect ambiguous. However, if we find a larger effect – or even the same effect – on tax evasion at $\hat{v}_c^{low}$ compared to $\hat{v}_c^{high}$ in the data, then this provides compelling evidence for a varying social multiplier.

To evaluate empirically the effect of different social multipliers on the adjustment to an enforcement shock, we will exploit different initial conditions for social norms in the period following the abolition of the poll tax. In the results section, this will be referred to as the second empirical prediction.

We now investigate the response to a temporary fall in the in the average level of intrinsic motivation $i_{c,t}$. In the data, we will argue that this corresponds to council-specific shifts in evasion norms that were triggered by the Thatcher government's introduction of a poll tax and associated with reduced tax compliance. Many taxpayers perceived the poll tax to be unfair because there was no link between the tax liability and the ability to pay.[41] This lead to a reduction in the intrinsic motivation to pay taxes, but the underlying shock was temporary: the poll tax was abolished in 1993 and replaced by essentially the same property-value based system as the one that had prevailed before 1990.

To capture the poll-tax episode, we consider the following path for mean intrinsic motivation:

$$i_{c,t} = \begin{cases} \hat{\imath}_c \ \text{ for } t < \underline{T} \ \text{ and } t > \overline{T} \\ i'_c < \hat{\imath}_c \ \text{ for } \overline{T} \geq t \geq \underline{T} \ . \end{cases}$$

Note that if $\mu = 0$, so that social norms were not important, there would be discrete jump down during the period when motivation falls followed by a return to the previous level of compliance. However, since $\mu > 0$, there is a dynamic path with persistence which we now study.

As before, denote the initial steady-state value cutoff by $\hat{v}_c$. Moreover, define an interim value

$$\widetilde{v}_c = \hat{v}_c + \frac{1}{1 + \mu \Delta_v(\hat{v}_c)}(i'_c - \hat{\imath}_c) > \hat{v}_c \ ,$$

which is the hypothetical new steady-state cutoff, had the shock to $i_{c,t}$ been permanent. Then, we have the following result:

Proposition 2 *The dynamic effect on tax evasion – the impulse-response function – of a temporary decline in intrinsic motivation depends on the sign of* Δ_v. *If* $\Delta_v < 0$,

[41] This is consistent with the ideas in Cummings et al (2009) who show that there is link between willingness to pay taxes and perceptions of the quality of government. The evidence discussed in Hoffman et al. (2008) supports the idea that perceptions of fairness of the tax system shape attitudes towards tax compliance.

the proportion of tax evaders increases monotonically from $G(\hat{v}_c)$ and stays in the range between $G(\hat{v}_c + \hat{\imath}_c - i'_c)$ and $G(\widetilde{v}_c^-)$ as long as $\overline{T} \geq t \geq \underline{T}$. Then, the proportion of tax evaders starts to fall monotonically back to $G(\hat{v}_c)$. If $\Delta_v > 0$, the proportion of tax evaders, rises to $G(\hat{v}_c + \hat{\imath}_c - i'_c)$ in $t = \underline{T} + 1$, and then oscillates in a range from $G(\hat{v}_c + \hat{\imath}_c - i'_c) < G(\widetilde{v}_c^-)$ to *$G(\widetilde{v}_c^+) < G(\hat{v}_c + \hat{\imath}_c - i'_c)$ as long as $\overline{T} \geq t \geq \underline{T}$. Then, the proportion of tax evaders starts to oscillate back towards $G(\hat{v}_c)$.*

[beign Proof] These results follow from the fact that the model can be solved recursively. Thus, as long as $\overline{T} \geq t \geq \underline{T}$ – i.e., the lower value of i_c is in place – the dynamics are the same as they would have been if the shock to i had been permanent. The impulse-response function $v^*_{c,t} - v^*_{c,t-1} = -(-\mu)^{t-1-T}[\Pi_{s=T}^{s=t-2}\Delta_v(v^*_{c,s})](i'_c - \hat{\imath}_c)$ over these years is thus analogous to that in (15) When i returns to its former value, $v^*_{c,t}$ returns back to $\hat{v}_c$ in a monotonic (oscillating) way, when $\Delta_v < 0$ ($\Delta_v > 0$). The results for $G(v^*_{c,t})$ again follow from the monotonicity of G. [end Proof]

This provides a useful guide for the empirical analysis below. Since the variation in tax evasion prior to the introduction of the poll tax experiment was small, we hypothesize there being a common starting value for tax evasion. However, we postulate that councils experienced heterogeneous shocks $i'_c - \hat{\imath}_c$, reflecting the different socioeconomic makeup of the relevant population. Proposition 2, which becomes the third empirical prediction, says that councils with larger increases in evasion in the poll-tax years 1990-1992 should return more slowly to pre-poll tax levels of evasion. Moreover, their evasion rate should stay above that in councils with smaller poll-tax shocks to evasion throughout the adjustment towards the new steady state.

Putting the results on permanent enforcement shocks and heterogeneous shocks to intrinsic motivation together provides a precise characterization for the second empirical prediction. Specifically, we can consider the impact of the shifts in tax enforcement in the post-poll-tax period triggered by changes in the political majority controlling a council. We can then compare the evasion responses to these enforcement shocks in councils that had different levels of tax evasion due to heterogeneous shocks to norms during the poll-tax period.

So far, tax enforcement has been exogenous. We now sketch an extension where enforcement is set according to a simple adaptive rule. To motivate this, observe that the revenue raised by council c in year t is given by

$$r_{c,t} = (1 - G(v^*_{c,t}))x_{c,t}\ . \tag{17}$$

Suppose that the council has a target level of revenue $\overline{r}_{c,t}$ with a quadratic cost of deviating from this target. Stronger enforcement at a quadratic cost indexed by $\theta_{c,t}$ can increase revenues. One can think about the changes in political control that we exploit empirically as affecting these parameters, which govern the priority the council attaches to tax evasion. Specifically, a shift into single-party majority control can be interpreted as a (permanently) higher value of $\bar{r}_{c,t}$ (or a lower value of $\theta_{c,t}$).

The council government sets $m_{c,t}$ to minimize total costs in period t,[42] taking the social norm in the previous period as given. Thus, we obtain

$$m^*_{c,t} = \text{argmin}_m \left\{ \frac{1}{2}[(r_{c,t} - \overline{r}_{c,t})^2 + \theta_{c,t} m^2] \right\} = \frac{(\overline{r}_{c,t} - r_{c,t}) g(v^*_{c,t}) x_{c,t}}{\theta_{c,t}} . \tag{18}$$

Thus, if $r_{c,t} < \overline{r}_{c,t}$, the council responds to the gap between its revenue and the target. The response is more aggressive if the tax liability $(x_{c,t})$ is higher, if the response of the tax base (represented by $g(v^*_{c,t})$) is more elastic, or if the marginal enforcement cost $\theta_{c,t}$ is lower.

Using this simple policy rule, we can show that the contemporaneous response, as well as the dynamic response, of the cutoff $v^*_{c,t}$ have the same signs but smaller magnitudes than in the absence of enforcement.[43] Therefore, the qualitative predictions in Proposition 1 (empirical prediction 1), Corollary 1 (empirical prediction 2), and Proposition 2 (empirical prediction 3) remain valid even when enforcement is endogenous.

[42] This is a purely static objective – a more ambitious model would also take into acount the dynamic effects, via changing future social norms, of today's policy.

[43] Consider a shift in $i_{c,t}$.Using (17), (18), and (14), the contemporaneous response of the cutoff to this shift is given by:

$$\begin{aligned} \frac{dv^*_{c,t}}{di_{c,t}} &= \frac{\partial v^*_{c,t}}{\partial i_{c,t}} + \frac{\partial v^*_{c,t}}{\partial m^*_{c,t}} \cdot \frac{\partial m^*_{c,t}}{\partial i_{c,t}} = \\ & \quad - \left(1 + \frac{\left[g\left(v^*_{c,t}\right) x_{c,t} \right]^2}{\theta_{c,t}} \left[1 - \left(r_{c,t} - \bar{r}_{c,t}\right) \frac{g'\left(v^*_{c,t}\right)}{g\left(v^*_{c,t}\right)} \right] \right)^{-1} . \end{aligned}$$

The second-order condition associated with the minimization in (18) implies that this expression is less than -1, the effect in the absence of an enforcement response. Thus the endogenous enforcement response dampens the response to the shift in norms. The dynamic responses are similarly dampened by endogenous enforcement with:

$$\begin{aligned} \frac{dv^*_{c,t}}{dv^*_{c,t-1}} &= \frac{\partial v^*_{c,t}}{\partial v^*_{c,t-1}} + \frac{\partial v^*_{c,t}}{\partial m^*_{c,t}} \cdot \frac{\partial m^*_{c,t}}{\partial v_{c,t-1}} \\ &= -\mu \Delta_v (v^*_{t-1}) \left(1 + \frac{\left[g\left(v^*_{c,t}\right) x_{c,t} \right]^2}{\theta_{c,t}} \left[1 - \left(r_{c,t} - \bar{r}_{c,t}\right) \frac{g'\left(v^*_{c,t}\right)}{g\left(v^*_{c,t}\right)} \right] \right)^{-1} . \end{aligned}$$

The expression on the right-hand side is smaller in absolute magnitude than $-\mu\Delta_v(v^*_{t-1})$, which was the response in the exogenous enforcement model.

2.3 Data

This section gives our sources and defines our measures of tax evasion, electoral outcomes and tax enforcement. Our data forms an unbalanced panel of 346 local authorities (councils) in England and Wales over 30 years between 1980 and 2009, so that each observation is a council-year. Councils are separated into classes which differ in several dimensions, including their electoral structure. However, all councils share two important characteristics: they are responsible for collecting the taxes that we study, and their policies are determined by a legislative assembly of locally elected councillors.

2.3.1 Tax Evasion

Although the tax base changed during our sample period, the local council has retained responsibility for enforcing and spending the revenue it collects from taxes levied on households.15 Prior to the introduction of the poll tax, a system of local rates had been in use since 1601 with minor exceptions. Rates were levied on all properties based on a measure of their rental value. This was assessed by the Valuation Office, which would uprate the value in line with improvements. The occupants were liable to pay tax whether a property was used for domestic or business purposes.

In 1990, domestic rates were replaced by the community charge, popularly referred to as "the poll tax".[44] This was levied at a flat-rate per-head tax. A few groups — including nuns, criminals and recipients of income support — were exempted. Other low-income groups, such as students and unemployed, were liable for 20% of the standard amount. Otherwise, the poll tax was levied independently of an individual's income and wealth. Ostensibly, this reform was to improve political accountability by creating equal stakes for every citizen. But the tax was deemed unfair since it was not linked to individual circumstances — it broke the link between a property's value and the tax levy, a hallmark of the earlier regime and a feature of almost every existing system of local taxation. The perceived unfairness resulted in major protests and riots accompanied by unprecedented levels of tax evasion by UK standards.[45]

In 1993, the poll tax was abolished and replaced by the present council tax. It is based on the value a property would have sold for in the open market on April 1st 1991. The Valuation Office individually assessed each property and assigned it to one of a given set of preassigned valuation brackets. The council

[44] See Butler, Adonis and Travers (1994) for a discussion of the factors leading up to the introduction of the poll tax and its subsequent abolition.

[45] It was not the first time in British history that a poll tax had triggered a mass protest — more than 600 years before, in 1381, the poll tax is considered to have a had central place in triggering the peasants' revolt.

sets the council tax rate, which implies a liability for each bracket. Thus, the council tax results in one bill for each household that occupies a property. This reintroduced the link between the taxes and property values, thus restoring some semblance of fairness in the local tax system. However, no revaluations have taken place after 1991 and no new bands have been introduced with increasing property prices.[46] As the council tax has become decreasingly detached from actual property values, its fairness have come under debate.

There is no simple way of comparing tax levels across the three tax regimes due to the different tax bases. However, we can make a rough guess of the level of taxation per dwelling.[47] This suggests that domestic rates per dwelling in 1989 were around £501 (std. 110), the poll tax per dwelling in 1990 was £677 (std. 214) and the council tax per dwelling in 1993 was £509 (std. 289). However, poll tax number is somewhat misleading, because of cuts in 1991 and 1992 — a per-head reduction by £110 in 1991 brought the poll tax per dwelling down to almost exactly the same liability level as under the domestic rates and the council tax. This exercise suggests that we may want to condition on each council's poll-tax level when analyzing evasion from the this tax.

We calculate a measure of yearly average tax evasion for each council and year as the difference between net collected tax revenue and net tax liability, expressed as a percentage of net liability. This is the measure of evasion used in Figure 19 for the different tax bases in the periods 1980-89, 1990-92 and 1993-2009. It is also our main measure of evasion in the paper.[48]

Under the present council-tax system (from 1993), as well as the rates system (in 1980-89), councils combine a registry list of all properties with independently assessed valuations of these properties to draw up a tax liability for all households. Under the poll tax (during the years 1990-92), councils relied on population registers used in the rates system to count the number of adult individuals liable for the tax. This makes the total liability per household a straightforward calculation. Since no deductions are allowed against other taxes, yearly household payments are known to the councils. This makes it straightforward — for the councils and for research purposes — to measure and track tax eva-

[46]There have been talks of re-valuation of properties in England, but these have systematically been postponed. However, in Wales, re-valuation of properties occured in April 2003.

[47]The methodology from CIPFA (1993, page 8, rows 12 and 16-17) is used to calculate the poll tax per dwelling in 1990. However, using this method, poll tax per dwelling is missing for approximately 30% of the councils. CIPFA (1993) has data on domestic rates per dwelling in 1989 and the council tax per dwelling in 1993.

[48]We calculate tax evasion separately for each year. Collected revenue is measured net of any tax that was collected from outstanding arrears from previous years. Similarly, net liability is calculated as gross liability minus all exemptions and outstanding arrears. We are thus reasonably confident that our measure of evasion is net of any lagged evasion-related error component, which is important for interpreting our decay and dynamic-path results.

sion. No publicly available long-run administrative estimates of evasion rates exist for any of the three systems. However, the Department for Communities and Local Government, together with the Office of National Statistics, published estimates of collection and evasion rates for the council tax over the period 2006-2011 (Communities and Local Government, 2011). For 2009, our average measure of evasion for the UK is 2.69%, against theirs of 2.90%. It is reassuring that the correlation is 0.99 at the council-matched level.[49] Our data on evasion is constructed from two series produced by the Chartered Institute for Public Finance and Accountancy (CIPFA).[50] We have digitized CIPFA's series for all years prior to 1996, with a resulting sample size of 8,220 council-year evasion observations.[51] To the best of our knowledge, this dataset is the first to measure tax evasion in a consistent way for the three regimes of local household property taxation in the UK.[52]

By our measure, the pre-1990 rates system had high compliance with mean evasion at 2.8%. Compared to the last two years of the rates system (1988-89), average evasion in the first two years of the poll tax went up by nearly 550%. At the same time, the distribution of evasion across councils shifted notably rightward with a flattening out of the distribution. This is shown in the marginal density distributions in Figure 21. This shift in dispersion illustrates a heterogeneous change in evasion behavior, which can be readily interpreted as a set of heterogenous shifts in intrinsic motives.[53] The most plausible interpretation of this heterogeneity is that different areas in the UK have different socioeconomic make ups. At the shift to the council tax in 1993-94, the distribution of evasion starts moving back to the left with a large relative decrease in the spread. Figure 19 shows that the average evasion on the council tax base in these two transition years is close to 6.3%. But this is still 125% higher than average evasion in the rates system. The distribution of evasion across councils during the remaining years of our sample (1995-2009) more closely resembles the pre-poll tax distribution, but a higher mean as well as a larger spread suggest persistent effects of the

[49] Council-level evasion measures in the administrative data were only available in 2008-11.

[50] 22CIPFA is a professional accountancy body which collects a large set of statistics on the functioning on the councils. CIPFAStats produces the Revenue Collection series and has been producing local government data for over 100 years.

[51] For years 1980-1989 we relied on the annual "Rate Collection Statistics, Actual". From 1990 to 2009, we use the annual "Revenue Collection Statistics, Actual."

[52] Besley, Preston and Ridge (1997) study the determinants of evasion during the poll-tax era and our data are consistent with theirs during this period.

[53] When the poll tax was introduced in 1990, there was also a significant increase in average local tax rates. But the increase in VAT-rates in 1991 allowed for a large average reduction in poll tax liability of 110 pounds (relative to an average of 340 pounds). Nevertheless, evasion continued to steeply increase in 1991 (Figure 19).

poll-tax shock on evasion.[54]

2.3.2 Electoral outcomes

To explore the relationship between majority control and evasion (recall Figure 20), we collect data on electoral outcomes in all councils between 1980 and 2009. First-past-the-post elections are held at the level of the ward, a smaller unit than the council (on average 23 wards per council). Each ward returns between one and three members to the local council. The distribution of council size is roughly bell-shaped, with a mean and standard deviation of 49 and 12 seats, respectively.

Our data include a breakdown of council seats by political party in all of the councils in all years.[55] Based on 10,434 council-year composition observations, we construct a binary indicator of single-party majority control, which we set equal to one whenever one of the political parties controls 50% or more of the council's total seats. This rests on the idea that policies are more cohesive when a single party rather than a coalition runs the council. In particular, we hypothesize that more cohesion might facilitate greater agreement on the use of tax revenue and therefore a stronger motive to enforce the payment of outstanding tax liabilities.

The electoral cycle varies depending on the type of authority. London boroughs elect all members at a single election every four years, while metropolitan districts return a third of their members on a rotating basis in three out of every four years. Unitaries and non-metropolitan districts can opt for either system and may change between them. This heterogeneity in returned seats and timing of elections suits well our empirical Regression Discontinuity Design (RDD). We use narrow shifts in political majority control of the council, and the number of years this tight majority remains in place, as sources of identifying variation. The specific definitions of close elections are presented in Section 2.4.2.

To the best of our knowledge, this paper is the first to use data on UK local electoral outcomes up until 2009. Besley and Preston (2007) use local council seat-share data over the 1973-98 period to study how districting bias in favor of a party impacts electoral incentives and policy outcomes. Eggers et al. (2013) use such data over the 1945-2003 period in their meta-study of close election outcomes.[56] However, none of these papers studies the impact of electoral outcomes

[54] Another difference between the poll tax and the council tax/rates is that the poll tax was levied on individuals rather than properties. It is difficult to translate our evasion measures to a per capita basis consistently. However, our efforts to measure evasion on a per capita basis yielded similar results.

[55] These data were obtained from the Elections Centre at Plymouth University.

[56] That paper is motivated by critiques of RDD studies of U.S House elections, on the argument that the previous vote share remains highly correlated with victory even in close elections. How-

on local tax evasion.

2.3.3 Enforcement

We collected measures of enforcement in order to test the relationship between switches in majority control and council-tax enforcement. This relationship is intended to support our use of majority switches as shocks to tax enforcement of the kind that were studied theoretically in the previous section.

The data source is the same series of CIPFA publications used to construct the evasion measure (Section 2.3.1). If a household does not comply with council-tax payments, the council's first action is to send out a reminder. If non-payment persists, or payment in full is not received, the council can summon the household to attend a court hearing. Only when a summons order has been issued may the council proceed to other methods to recover the debt, including (in order of severity) taking money directly from wages and benefits, ordering bailiffs to collect the amount, placing a lien on the property, and starting proceedings for a prison sentence. In practice, the debt is usually recovered before the final steps are reached. Thus, reminders constitute a 'soft' signal of enforcement while issuing a summons is a more directed and costly effort by the council.

Based on this, we use the ratio of the number of court summons relative to the number of reminders in a council-year as our core proxy for enforcement. In our sample, the council sends out an average of 0.31 summons per reminder (std. 0.28, 25th percentile 0.17, 75th percentile 0.35). If this measure proxies for exogenous enforcement it should predict decreases in tax evasion. On the other hand, if it reflects an endogenous response to evasion, we would observe a positive correlation with evasion. In a cross-sectional regression, summons over reminders is positively correlated with evasion. However, in a within-council regression — i.e., including council fixed effects — summons over reminders is negatively correlated with evasion, with an elasticity of –0.61 (std. 0.16). This suggests that our core measure is a good proxy of enforcement effort. Below, we interpret changes in the summons/reminders ratio induced by majority switches as exogenous shocks to enforcement corresponding to those in Prediction 1.

2.4 Evidence

We now use the data introduced in Section 2.3 to shed light on the predictions from Section 2.2. In a first subsection, we test Prediction 3 on the persistence of social motives in the wake of temporary shocks to the intrinsic motives to pay. We do this by highlighting the heterogenous evasion responses to the "poll-tax shock" and by following the evolution of tax evasion in the council-tax regime.

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evere, Eggers et al. (2013) find no evidence of such sorting in the case of close UK local elections, which supports the identifying assumptions for our RDD framework.

In the second subsection, we consider the effects of permanent shocks to enforcement. We do this by considering the impact on tax evasion via shifts in enforcement generated by random switches in and out of single-party majority in local councils. Combining this analysis with the findings from the first subsection, we can evaluate Predictions 1 and 2. Specifically, we gauge whether the effect of the narrow political shifts on tax evasion is indeed heterogenous depending on the initial level of evasion.

2.4.1 Persistence of Social Motives

Figure 19 discussed in the introduction shows that average tax evasion goes up to between 10% and 15% during the poll-tax years and then begins to decline immediately after the shift to the council tax in 1993. We will interpret the changes in evasion during the poll-tax period as induced by council-specific shifts in the average intrinsic motivation to comply with taxes. Then, we can study the dynamics using Prediction 3 about the impulse-response function. In particular, we expect councils with the largest increases in tax evasion between 1990 and 1992 to have a larger share of tax evaders in the council-tax era. Moreover, we expect this share to fall over time but to stay above the share of tax evaders in councils with less poll-tax evasion.

A natural split is between councils with above and below median poll-tax evasion. The left-hand panel of Figure 22 compares these sub-groups with the above-median poll-tax evading councils marked in red and the below-median poll-tax evasion councils marked in blue. The graph is striking. We see no difference in tax evasion in the decade preceding the poll-tax experiment. However, following the experiment, the share of tax evaders in the high poll-tax evasion councils lies everywhere above that in the low poll-tax evasion councils. This is consistent with the basic idea in our model of a dynamically evolving social norm for evasion.

A plausible criticism is that the council-specific shifts in poll-tax evasion need not only reflect variation in intrinsic motives to comply with taxes. In particular, they may vary systematically with economic, social and political variables at the local authority level. For example, people in Labour-dominated councils may have been more upset about the Thatcher government's poll-tax policy and thus more motivated to evade taxes as a form of protest. Table 17 explores this possibility by reporting the results of a cross-council regression for poll-tax evasion on a range of variables: the size of the poll-tax liability, (log) per-capita income, (log) population, the seat shares of the Conservative and Labour parties, dummies for Conservative and Labour majority control, the proportion of houses in the top council tax band compared to the bottom band (a measure of housing-value inequality), and the share of renters broken down by private and council landlords (social housing). The correlation patterns in Table 17 make sense. A higher poll tax liability is positively correlated with evasion while higher income is associated with lower evasion. There are insignificant correlations with the political variables except for Labour control. Greater inequality in the value of the housing stock is negatively correlated with poll-tax evasion. The proportion of private renters is positively correlated with evasion (which is plausible if such renters are particularly mobile and more willing/able to move rather than pay).

We use the regression in the final column of Table 17 to construct a measure of conditional poll tax evasion, as the residual from this regression. In this way, our measure is not purely a proxy for these observable sources of heterogeneity across authorities. Dividing the sample based on this conditional measure of evasion, we obtain the right-hand panel of Figure 22. The results for conditional evasion are very comparable to those for gross tax evasion. From now on, whenever we divide the sample, we focus on the results for our conditional measure of poll-tax evasion based on Table 17.

We now examine the persistence of evasion in relation to evasion in the poll-tax era more formally by within-council non-parametric estimation. Specifically, we regress evasion in the council-tax period on an indicator for above-median conditional poll-tax evasion interacted with a full set of year indicators from 1993 to 2008 (2009 is the left-out indicator).[57] In effect, we are estimating separate year effects for the two sub-groups in the right-hand panel of Figure 22. The inclusion of council fixed effects and year fixed effects capture a plethora of fixed sociodemographic factors which are likely to affect evasion at all points in time and thereby capture the 'normal' value of intrinsic motives.

All year dummies for high conditional poll-tax evasion councils are significantly different from zero between 1993 and 2002. This suggests a persistent effect up to ten years after the poll tax is abolished. The estimated coefficients are plotted in Figure 23 together with their 95% confidence intervals.

If the poll tax disrupted a tax-paying social norm, we should not expect a similar pattern in the period before the introduction of the poll tax. If we did observe a similar pattern, this would be analogous to the assumption of parallel pre-trends being violated in a difference-in-difference estimation.

To investigate this, we repeat the analysis underlying Figure 23, but for the 1980-89 period. Thus, we interact year indicators for 1981-1989 (leaving out the 1980 dummy) with the indicator for above-median conditional poll-tax evasion

[57] Appendix Tables 20 and 21 report the regression estimates for conditional poll tax evasion and raw gross poll-tax evasion and Appendix Figures 29 and 30 display graphically the respective sets of regression results.

and plot the estimated coefficients in Figure 24. None of these is statistically different from zero. In other words, whatever source of heterogeneity is uncovered by poll-tax compliance to persist for ten years, it was not observed in the prior period. This makes it plausible to attribute the evasion patterns to a break-down of social norms following the introduction of the poll tax.[58]

Taken together, the results show persuasive evidence in line with the idea that the poll tax shifted the intrinsic motives to pay tax, and that these shifts spilled over into the prevailing social norm, which then exerted a significant but declining effect on tax evasion for around a decade after the poll tax. Specifically, councils with high poll-tax evasion had significantly higher tax evasion throughout this decade compared to councils with low poll-tax evasion.[59]

To quantify the amount of evasion implied by our results, we use the average tax evasion over councils and years in the domestic rate period as a counterfactual evasion rate. We then apply this evasion rate to all councils from 1990 onwards and compute the actual evasion less the counterfactual evasion rate multiplied by the tax liability for each council. Finally, to get an aggregate measure, we sum over councils and years. On this basis, we estimate the cumulative tax foregone due to the poll-tax experiment at 4,980,000,000 in 2009 pounds, which is around 26% of the total value of the 2009 council tax liability. This number suggests a fairly weighty "echo effect" from the poll-tax period on local government tax revenue.

2.4.2 Enforcement

One concern with the results presented so far is that they do not consider council enforcement decisions. Ideally, we would like to find a factor which plausibly influences enforcement without any other effects on evasion. This is where the narrow shifts in political control come in. When elections outcomes are close, we can plausibly assume that the resulting shifts in political majority are as good as random and orthogonal to the determinants of individual evasion decisions.

[58]Appendix Figure 31 finds no statistical significance for the set of interactions between year-indicators and the indicator for above-median poll-tax evasion.

[59]As shown in Appendix Figure 36, all the results go through if we instead split the sample of councils into those below and above the 75th percentile of conditional evasion during the poll-tax tax period. Finally, Appendix Figure 37 shows the results are robust to controlling for the difference in household tax-liability between the last year of Domestic Rates and the first year of the Poll Tax, in the poll-tax regression (Table 17, Col.5). This measure is not available for all councils in the required years, and leads to a 30 percent decrease in sample-size relative to the sample used in the main text.

We study how narrow shifts into, or out of, single-party control (recall Section 2.3.2) affect tax evasion and our proxy for enforcement (recall Section 2.3.3). Let us first define close elections as those that lie within the optimal bandwidth for RDD estimation with a linear control function proposed by Imbens and Kalanyaramanan (2012) (IK in what follows). When applied to our data, their algorithm returns a bandwidth just above 3 percentage points. We also consider alternative definitions with half and double the optimal IK bandwidth. The number of close elections is sizeable with all three definitions — 708, 1533 and 2804 elections respectively over the entire sample period.[60]

Do narrow shifts in and out of single-party council majority systematically affect the levels of tax evasion and enforcement? Figure 20 already gave some evidence in favor of this possibility, which we now explore more systematically. To that end, Table 18 presents RDD estimates of the impact effects of a random switch into single-party majority. The upper panel shows estimates for tax evasion while the lower panel shows estimates for enforcement. The whole table is based on local-linear regression within narrow intervals, namely the optimal IK bandwidth, as well as half and double this bandwidth. In each panel, we show four sets of estimates, with (i) no controls, (ii) council fixed effects, (iii) these plus year fixed effects, and (iv) these plus controls for tax liability and (log) per-capita income.

The reduced-form results for tax evasion in the upper panel are strong. The estimates for the optimal IK bandwidth suggest that a random switch into single-party majority reduces tax evasion by about 2 percentage points, which is just above 50 percent of its sample-mean. This effect is quite precisely estimated and changes little as we include different sets of controls. The estimates for the smaller bandwidth are (considerably) higher, while those for the larger bandwidth are a little lower.

The results for the ratio of summons to reminders — our proxy for tax enforcement — are somewhat weaker. The point estimates with the optimal IK bandwidth suggest that a switch into a single-party majority raises this ratio by about 0.06, which is about 20% of its mean (0.29). But only two out of the four estimates are statistically significant at the 10% level. For smaller as well as larger bandwidths, the point estimates are generally lower, noisier, and less stable when we vary the set of controls.

[60] Appendix Figure 35 plots the number of close elections in all sample years according to the IK and half the IK bandwidth sizes.

To assess whether these results are robust, Table 19 provides estimates with some alternative RDD specifications. Here, we vary the interval for the running variable — i.e., the seat share percentage for the largest party — to be ±1.5%,±3%,±5%, and ±10% from the switchpoint at 50%. All regressions include a control function, which is either linear or quadratic. For tax evasion in the upper panel, we only show the estimates without any controls (as the results in Table 18 were robust to including controls). But for the enforcement proxy in the lower panel, we show the estimates with and without controls.

The earlier estimates for tax evasion hold up well. The estimates are larger in absolute terms as the bandwidth is lower, and generally larger with the quadratic than the linear specification. The effect of a switch into single-party majority control varies around the earlier estimate of about −2 percentage points. As in Table 18, the estimates for tax enforcement are less precise. They are significant only when we include the full set of controls, both for the linear and the quadratic control functions.

As another robustness test, we show how the RDD estimates for tax evasion change with the interval for the running variable, always using the same local linear specification as in Table 18 (without any controls). Figure 25 shows the resulting estimates for small stepwise changes of the width of the estimation interval around its switchpoint at 50%. The estimate stabilizes just above −2 percentage points as the interval grows.[61][62][63]

The results above only concern the impact effects on enforcement and evasion. But Predictions 1 and 2 in the theory refer to the impulse-response function of tax evasion, following a permanent change in enforcement. We now ask whether a political shock produces a permanent change in enforcement. To do this, we estimate a non-parametric regression where the enforcement proxy or the evasion measure is regressed on an indicator for a switch into majority control in year t interacted with a set of year-since-election indicators for

[61] Appendix Figure 32 shows the equivalent estimates for enforcement when the interval for the running variable changes. The specification does however include controls. The point-estimate remains broadly stable in the range between the optimal bandwidth and three times the optimal bandwidth.

[62] As another robustness check, Appendix Figure 28 shows that the core evasion RD-result of Figure 20 holds when we restrict the bins of the running-variable to contain equal numbers of observations. This alleviates concerns that the RD evasion result could be driven by a small number of outlier observations situated very locally around the majority cut-off.

[63] Following the standard practice in papers based on RDD (see e.g., Imbens and Lemieux, 2008), we also present some tests of the identifying assumptions that underpin the empirical design. Thus, Appendix Figure 33 shows a diagram, which corresponds to the McCrary (2008) test for continuity of the running variable around the switch point. There is no visible discontinuity of the single-majority seat share margin. We also carry out placebo tests to ensure that switches in the running variable are not associated with jumps in any predetermined variable — see Appendix Table 22.

$t+s, s=0,...,6$. The regression also includes council fixed effects, year-since-election dummies, a one-period lag of council-tax evasion and a quadratic control function in the largest seat share.

The estimated coefficients for the enforcement proxy are displayed in the left-hand panel of Figure 26, where we have re-scaled the point-estimate relative to their sample-means (to allow direct comparison across the two panels). The estimates suggest that enforcement (the ratio of summons to reminders) increases in the year of the narrow single-majority switch and then stays higher for the subsequent six years. Apart from a dip in year 4, the point estimates vary around 10% of the mean (the point-estimate vary around 0.05, relative to the summons-reminders mean of 0.31). Four out of seven of them are statistically significant at the 5% level. While these results are not overly strong, they are still consistent with a permanent effect on enforcement from a shift in political control.

The right-hand panel of Figure 26 shows the estimates with tax evasion as the dependent variable. We see a clear sign of a cumulatively larger effect over time: a significant negative impact effect just short of minus 20 percent in year 0, which more than doubles (in absolute value) to year 6. The downward jump and subsequent gradual decrease in evasion are entirely consistent with Prediction 1.[64]

The final question we pose concerns the heterogenous effect of permanent enforcement shocks — via different social multipliers at different levels of tax evasion — in Prediction 2.[65] As in Section 2.4.1, we split the sample into councils above and below median conditional poll-tax evasion. The coefficients are displayed in Figure 27.[66]

All the estimated coefficients on the interacted majority-switch and year in-

[64]The estimates in the right panel of Figure 26 correspond to the non-parametric regression in Column 1 of Appendix Table 23, which refers to the entire Council-tax period 1993-2009. Column 2 adds to the council fixed effects a set of controls — tax liability, (log) per-capita income, (log) population, a set of seat-shares for labor or conservative and a set of dummies for labor or conservative political control. The point estimates become smaller in absolute value and some lose statistical significance, but their qualitative features with a cumulatively larger effect over time remains.

[65]The static RDD within-council estimates of the impact of random switch into majority on enforcement (Table 18, lower panel) do not differ across samples of above and below-median conditional poll-tax evasion nor across samples of above/below median gross poll-tax evasion. Results not reported here show that the 95% confidence interval of the point estimates for the sub-samples systematically overlap.

[66]These results are based on Column 1 and Column 3 in Appendix Table 24, which show estimates similar to those in Appendix Table 24, when we include council fixed effects and six-year fixed effects. We do not include single-year fixed effects, as they would absorb most of the year-to-year variation due to uneven majority swiches. Columns 2 and 4 show that the same results obtain when we also include the standard set of council-specific controls — tax liability, (log) per-capita income, (log) population, seat-shares for labor and conservative, and dummies for labor and conservative political control.

dicators are larger in absolute value for the councils with low poll-tax evasion than for those with high poll-tax evasion. The F-statistic when testing the joint significance of the estimated coefficients is much higher for low poll-tax evasion councils (3.24, p-value .003), than for high poll-tax evasion councils (1.17, p-value .323).[67]

Our results on permanent enforcement shocks triggered by switches in and out of single-party majority square well with the model predictions summarized in Prediction 2. That an enforcement shock has a larger effect on council-tax evasion for councils with smaller poll-tax tax evasion means that, around the initial cutpoints for evasion, a larger social multiplier outweighs the effect of a smaller density.

Through the lens of the model, we have thus found prima facie evidence for an interaction of individual and social motives in tax evasion. In line with that interpretation, social norms exercise a larger crowding in of enforcement at lower levels of tax evasion, which shows up as a larger social multiplier.

2.5 Conclusion

This paper has studied the persistence of social motives in tax evasion and the interaction between these motives and traditional individual motives tied e.g., to enforcement. We have built a model of the dynamics of tax evasion, by extending the approach in Benabou and Tirole (2011) to incorporate adaptive dynamics. The framework has helped us design our empirical specifications and permitted a sharper interpretation of the results.

The empirical analysis revolves around a unique natural experiment: the introduction and abolition of the poll tax in English and Welsh councils in the early 1990s. This induced a breakdown in tax compliance in an otherwise law-abiding environment, with much higher levels of evasion than could be seen in a field experiment. This gives an instructive case, where we can study and interpret the dynamics of tax evasion in response to a set of specific identifiable shocks which undermined the intrinsic motivation to comply due to the perceived unfairness of the tax. Non-parametric estimates suggest that these shocks to compliance

[67] The results are qualitatively similar, when we replace the conditional level of poll-tax tax evasion with the gross level of tax evasion discussed in Section 2.4.1. Regression output, with and without controls, is reported in the Online Appendix Table 25 and the estimated coefficients corresponding to Figure 27 are shown in Online Appendix Figure 34. As in Section 2.4.1, we can also perform the analysis by changing the sample split from councils below and above median conditional evasion in the poll-tax era to those below and above the 75th percentile of conditional poll-tax. Appendix Figure 36 shows that the results based on this alternative sample split are similar to the resutls reported in the main text.

exerted significant upward pressure on tax evasion for about a decade after the abolition of the poll tax. Specifically, councils with high poll-tax evasion had higher tax evasion throughout this decade compared to councils with low poll-tax evasion. This is in line with the impulse-response function predicted by the theory for a temporary shock to the intrinsic motives to pay taxes. The findings are robust to common concerns such as pre-trends, omitted variables, and alternative ways of measuring the key variables.

We also provide evidence for social multipliers in tax-evasion behavior and interactions between incentives due to enforcement and social-norm dynamics. Estimating dynamic paths of enforcement and evasion triggered by close-election switches into majority control of a single party, our results suggest that a permanent increase in enforcement induces a cumulative negative effect on tax evasion. We also find a heterogenous crowding-in effect from enforcement: the cumulative effect on tax evasion is larger in the councils with smaller poll-tax evasion, in line with the predicted impulse-response functions. To the best of our knowledge these empirical results are the first to show explicit evidence for interactions between individual and social motives in tax compliance.[68]

Our approach has been to consider social influence within a local authority. An interesting issue for further research is to consider the possibility of social spillovers across jurisdictional boundaries.[69]

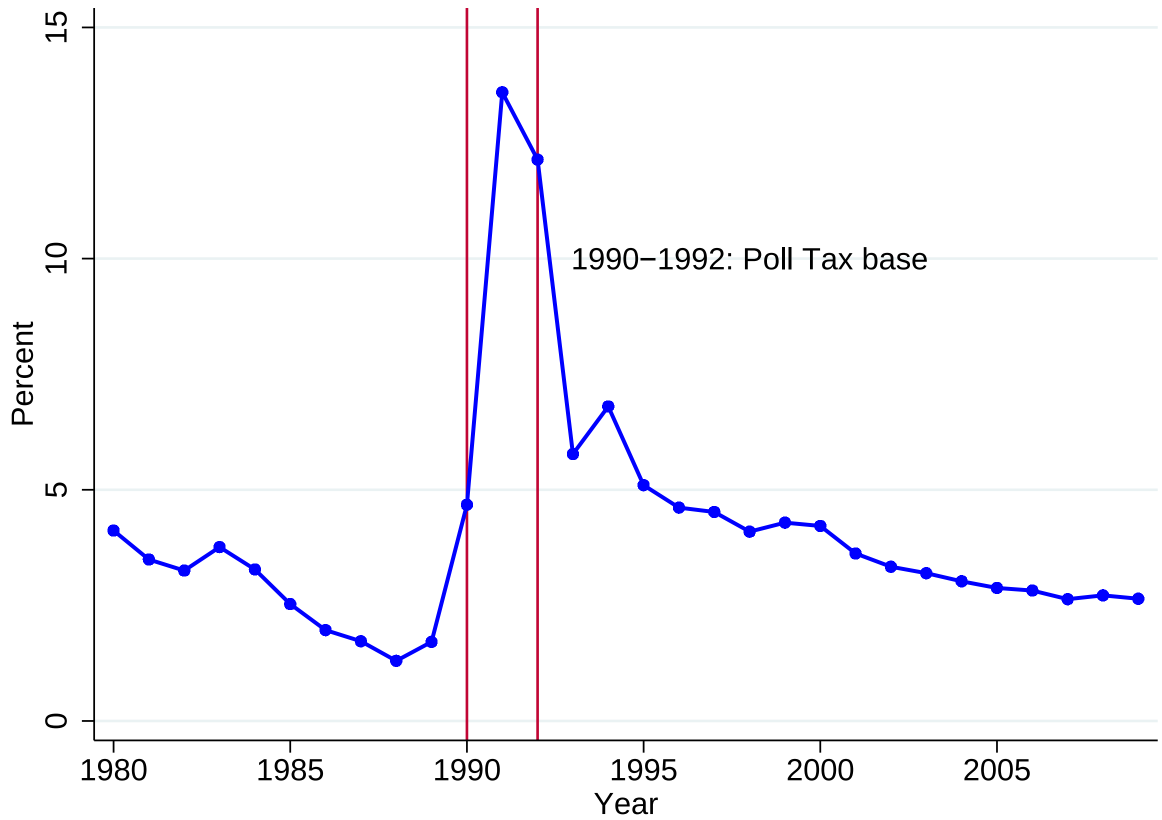
Although we have focused on the positive implications of social motives, the join between theory and data leaves open the possibility of a normative analysis based on a sufficient-statistics approach (see Chetty 2009). Our empirical analysis uses the theory to identify two sets of critical effects: (i) the impulse-response function on tax evasion of a temporary change in the intrinsic incentives to pay taxes, (ii) the impulseresponse- function on tax evasion of a permanent change in enforcement, at different points of tax evasion. These imply key elasticties that would enter an analysis of optimal investments in tax enforcement in the presence of social motives. However, our observation that social norms may adjust slowly implies that the reduced-form elasticties are time-varying equilibrium responses rather than structural parameters.[70] Exploring normative models of tax compliance in the presence of social motives is also an important topic for future research.

[68] See also Jia and Persson (2014) for related findings in a very different context.

[69] As suggestive evidence we do find, in a within-council regression, that the evasion in contiguous neighboring jurisdictions are positively correlated with that in a given jurisdiction. Moreover, this within-council effect does seem to decay in the period following the abolition of the poll tax.

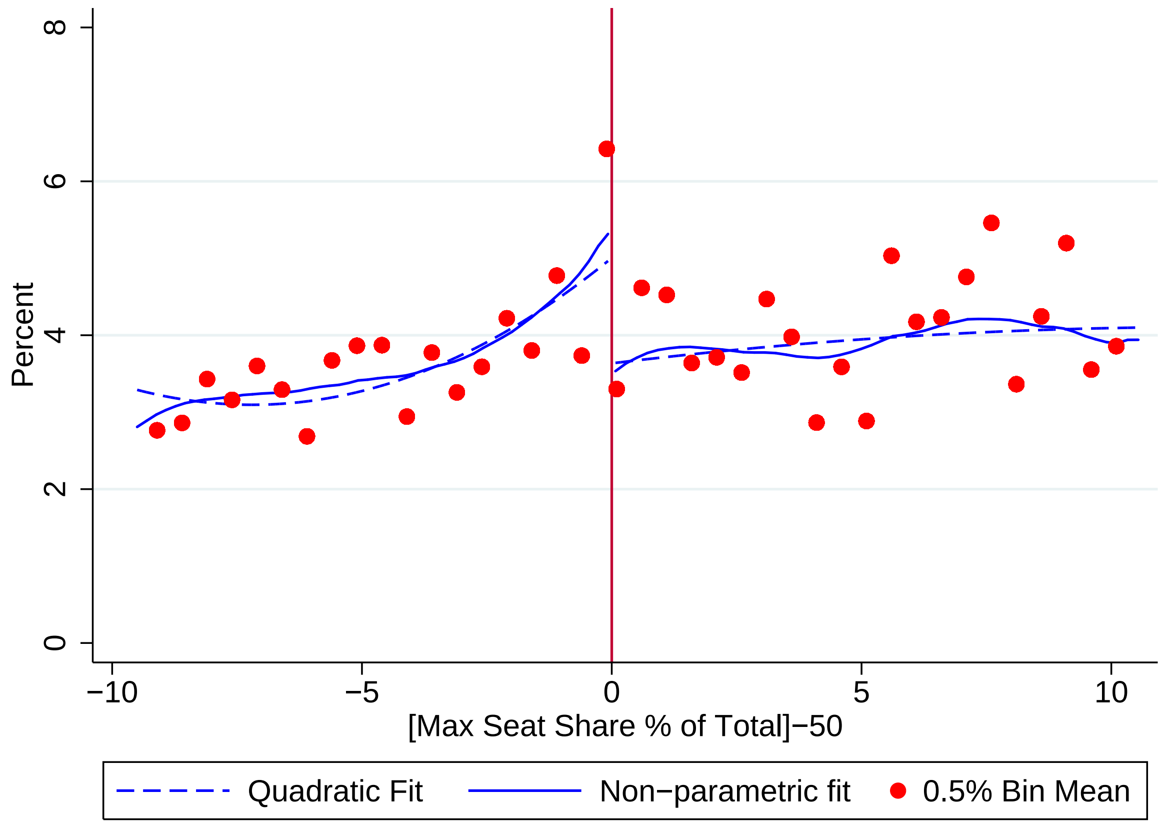
[70] This observation is related to the point in Slemrod and Kopczuk (2002) the the taxable income elasticity can depend on government enforcement policies.

FIGURE 19: TAX EVASION 1980-2009



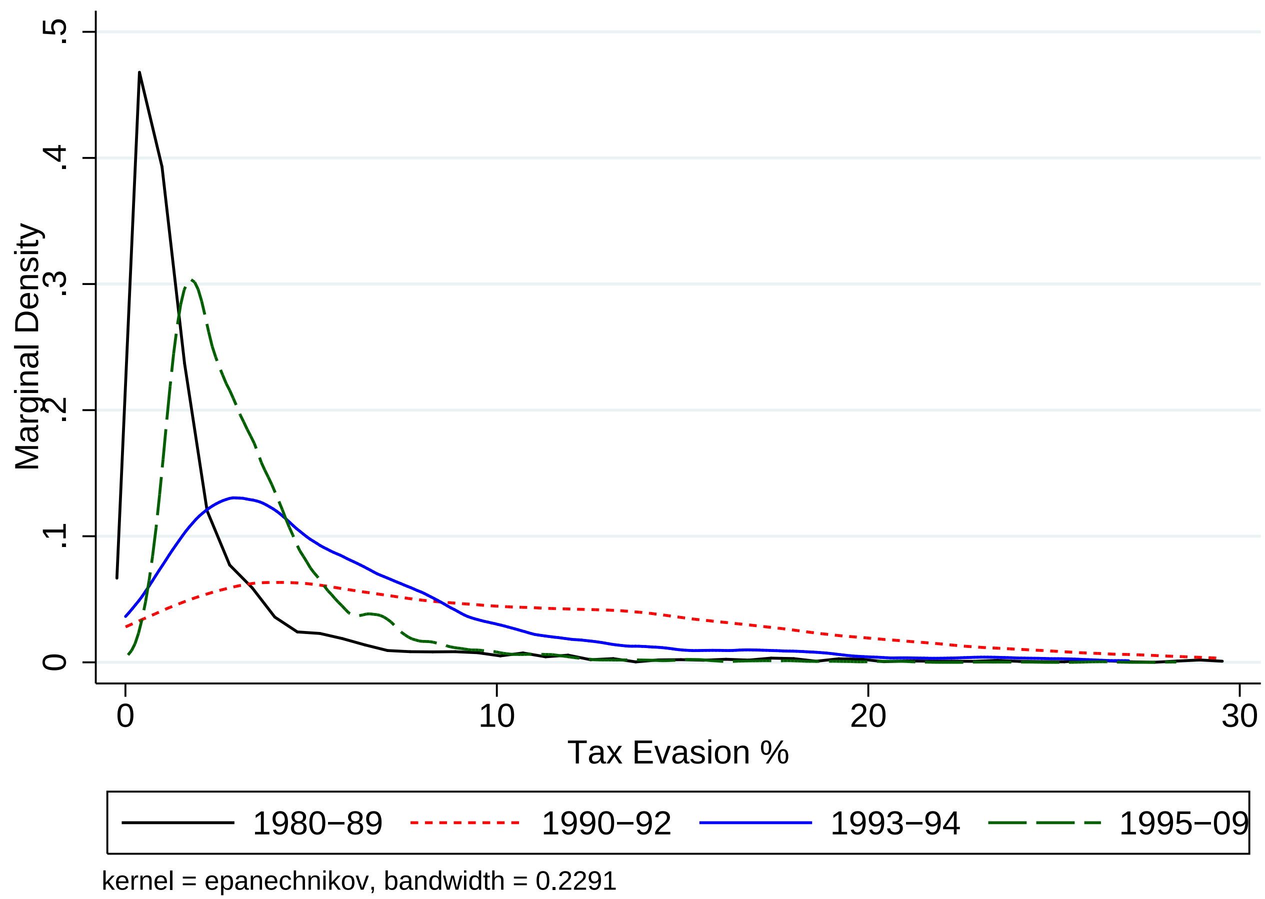
Notes: Each observation is a yearly average across all councils of our main measure of evasion, the difference between net collected tax revenue and net tax liability on the local tax base. During 1990-1992, a property tax base was replaced by the poll tax, which was levied at a flat rate per head. See Section 2.3 of the text for further details.

FIGURE 20: SINGLE-PARTY MAJORITY AND TAX EVASION



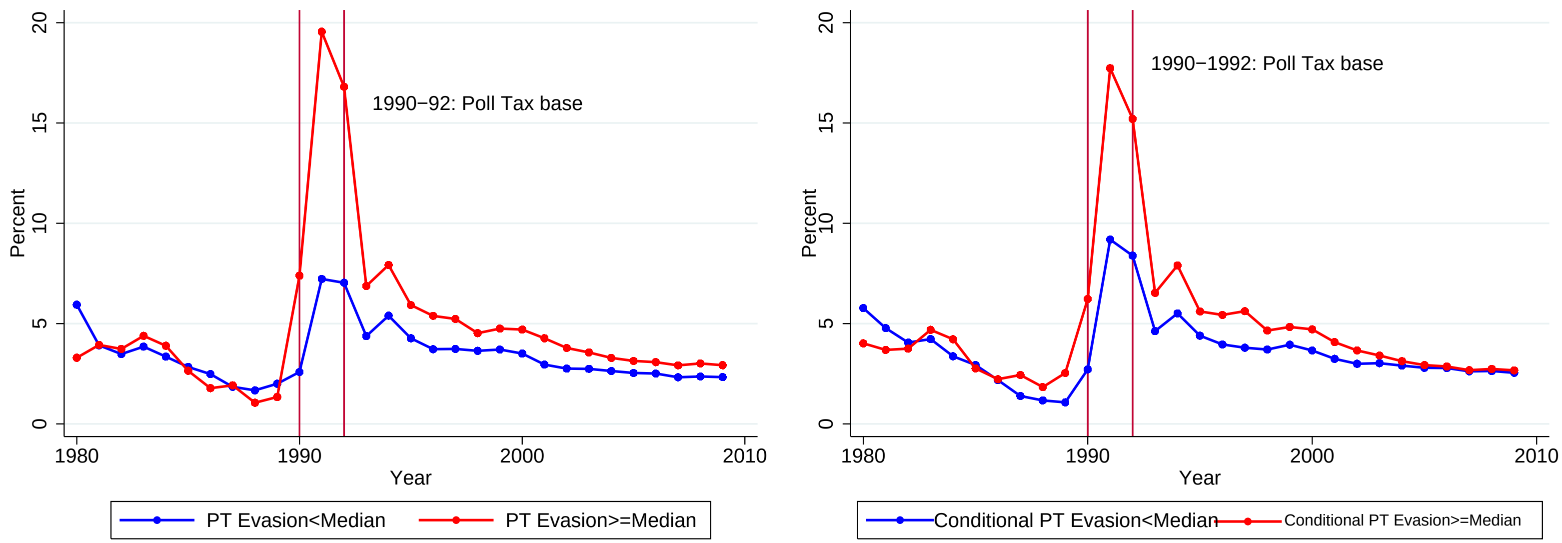
Notes: Each dot represents average tax evasion in a 0.5% bin of the difference between the largest party seat share and 50% in a given council-year, over the full sample 1980-2009. The vertical line is the single-party majority cut-off at 0. Dashed lines are quadratic fits, separately estimated on each side of the cut-off and the underlying council-level data. Solid lines are non-parametric fits from a local linear regression that uses triangular kernels with a bandwidth of 3% separately estimated on either side of the majority threshold, and on the underlying council-level data. The 3% bandwidth was determined as the optimal bandwidth using the Imbens and Kalyanaraman (2009) algorithm.

FIGURE 21: MARGINAL DENSITY DISTRIBUTION OF TAX EVASION ACROSS TAX-BASES



Notes: This graph plots the marginal density distribution of tax evasion across 4 time-periods: 1980-89 (Domestic Rates tax base); 1990-92 (Poll Tax base); 1993-94 (first 2 years of Council Tax base); 1995-2009 (remaining sample years of Council Tax base). Tax evasion is truncated at 30%, which equals (almost exactly) the 99th percentile for all time-periods except 1995-2009 where it equals the 99.9th percentile. See Section 2.3.1 for further details on the construction of the tax evasion measure, and description of tax evasion under the separate time-periods.

FIGURE 22: TAX EVASION BY RAW POLL-TAX EVASION AND CONDITIONAL POLL-TAX EVASION

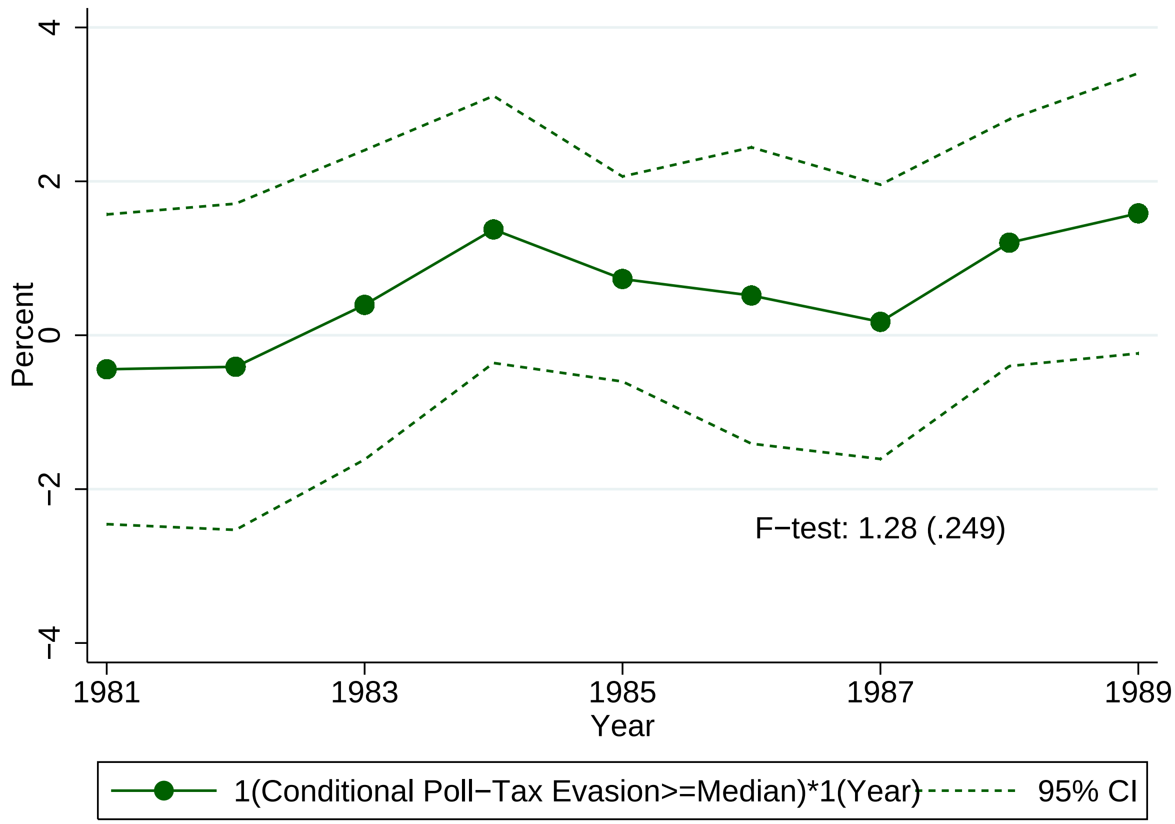


Notes: Each yearly observation in the left-side (right-side) panel is an average of tax evasion across all councils in one of two subsamples: the blue line refers to the councils where average tax evasion (average conditional tax evasion) in the poll-tax period was below the median; the red line refers to councils where poll-tax evasion (conditional poll-tax evasion) was above the median. See main text (Section 2.4.1) and Table 17 for the construction of the conditional poll-tax evasion variable.

FIGURE 23: NON-PARAMETRIC ESTIMATES OF COUNCIL-TAX EVASION BY CONDITIONAL POLL-TAX EVASION

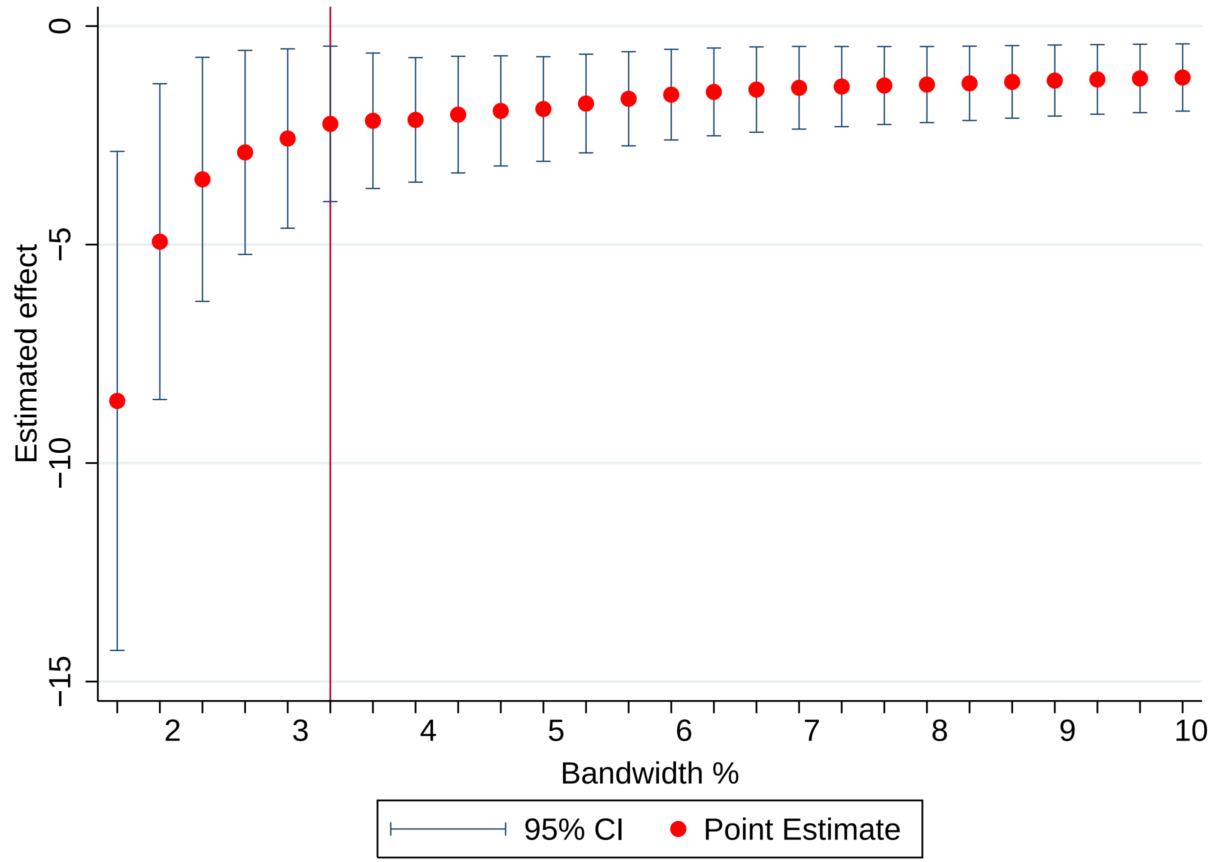
Notes: This graph plots the **1**(Year)***1**(Conditional Poll-Tax Evasion>=Median) coefficients from a regression of council-tax evasion on a set of year dummies, year-dummies interacted with a dummy for high conditional poll-tax evasion, and council fixed effects. The sample period is 1993-2009, which corresponds to the council-tax period. The omitted year-dummy is 2009. Dashed lines denote the 95% confidence interval of the interaction term. The F-test (and its p-value) refers to the joint significance of all interactions **1**(Year)***1**(Conditional Poll-Tax Evasion>=Median). Full regression output is given in column 1 of Appendix Table 20. See the main text (Section 2.4.1) for details on construction of **1**(Conditional Poll-Tax-Evasion>=Median).

FIGURE 24: DOMESTIC-RATES EVASION BY CONDITIONAL POLL-TAX EVASION



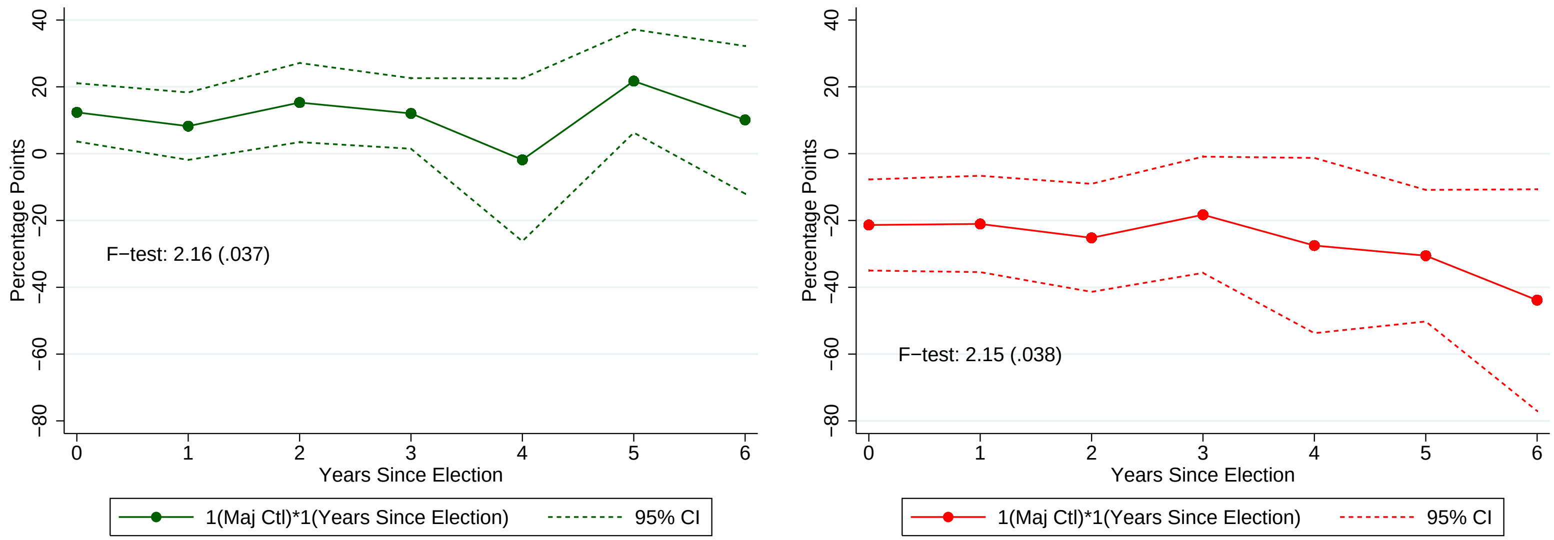
Notes: This graph plots the **1**(Year)***1**(Conditional Poll-Tax Evasion>=Median) coefficients from a regression of domestic-rates evasion on a set of year dummies, year-dummies interacted with a dumy for high conditional poll-tax evasion, and council fixed effects. The sample period is 1980-89, which corresponds to the domestic-rates period of our sample. Omitted year-dummy is 1980. Dashed lines denote the 95% confidence interval of the interaction term. The F-test (and its p-value) refers to the joint significance of all interactions **1**(Year)***1**(Conditional Poll-Tax Evasion>=Median). The underlying regression model is the same as that in the Notes to Appendix Table 20.

FIGURE 25: SINGLE-MAJORITY IMPACT EFFECT ON TAX EVASION BY ESTIMATION BANDWIDTH



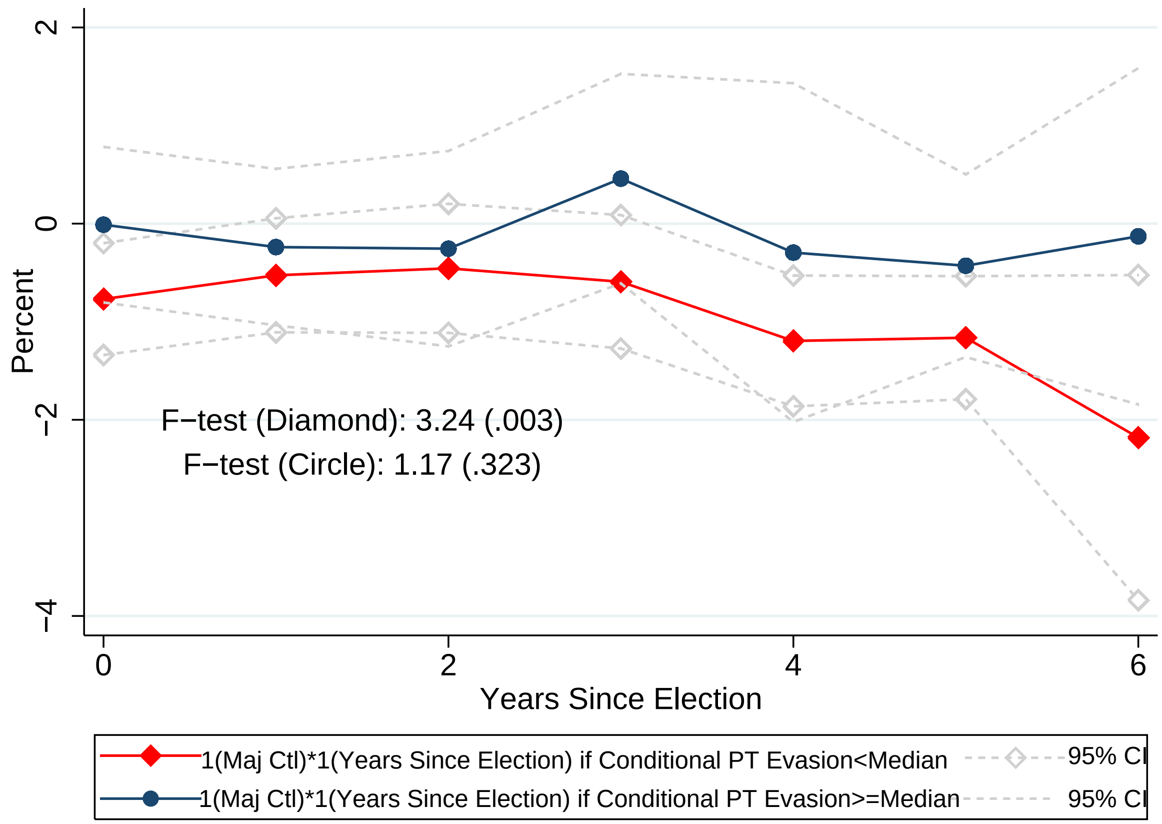
Notes: This graph plots the point estimates from an RDD for the impact effect on tax evasion of a switch into single-party majority. The vertical line at 3.32% shows the optimal bandwidth for the running variable determined by the Imbens and Kalyanaraman (2009) algorithm – here, the difference between the seat share for the largest party in the council and 50%. Each point is the estimate of a separate local linear RDD regression, as the bandwidth varies from half the I-K optimum (at 1.66%) to 3 times (at 10%) this optimum. Vertical blue lines denote the 95% confidence intervals for the point estimates. The RDD estimates for bandwidths corresponding to one half, equal to, and twice the I-K optimum are also reported in the top left corner of Panel A, Table 18.

FIGURE 26: DYNAMIC IMPACT OF SINGLE-PARTY MAJORITY ON ENFORCEMENT AND ON TAX EVASION



Notes: The right-hand side (left-hand side) graph plots the **1**(Majority Control)***1**(Years since election) coefficients from a regression of our enforcement measure (tax evasion measure) on the indicator for switch into majority, **1**(Majority Control), seven years-since-election dummies, **1**(Years since election), their interactions, lagged council-specific tax evasion, and a quadratic control function in the largest political seat-share. The enforcement measure is the ratio [#Summons]/[#Reminders]. The sample period is 1993-2009, which corresponds to the council-tax period. Dashed lines denote the 95% confidence interval on the interaction term. The F-test (and its p-value) refers to the joint significance of the (seven) interaction terms. The underlying regression model is in the Notes of Appendix Table 23. See the text (Section 2.4.2) for further details.

FIGURE 27: DYNAMIC IMPACT ON COUNCIL-TAX EVASION BY CONDITIONAL POLL-TAX EVASION



Notes: This graph plots the **1**(Majority Control)***1**(Years Since Election) coefficients from a regression of our measure of tax evasion on the indicator for switch into majority, **1**(Majority Control), seven years-since-election dummies, **1**(Years Since Election), their interactions, lagged council-specific tax evasion, and a quadratic control function in the largest political seat-share. The red line with diamonds shows the interaction coefficients for the councils that had conditional poll-tax evasion below the median conditional poll-tax evasion, while the blue line with circles shows the same estimates for the councils with above median conditional poll-tax evasion. The F-test (and its p-value) refers to the joint significance of the set of seven interaction terms. Full regression output for the graph is given in columns 1 and 3 of Appendix Table 24. The model estimated is in notes of Appendix Table 24, conditioning the sample on the high conditional poll-tax evasion dummy. See the text (Sections 2.4.1 and 2.4.2) for further details and the construction of the high and low conditional poll-tax evasion subsamples. The sample runs from 1993 to 2009.

TABLE 17: DETERMINANTS OF POLL-TAX EVASION

	(1)	(2)	(3)	(4)	(5)
	Poll Tax Evasion=[Net Collectable - Net Collected]/[Net Collectable]				
Poll Tax Liability	.032***	.028***	.029***	.023***	.025***
	(.006)	(.006)	(.006)	(.006)	(.006)
Lg(per capita income)		-28.470***	-29.917***	-21.988***	-23.863**
		(3.764)	(4.070)	(4.605)	(5.210)
Log(population)			-1.193*	-1.499**	-1.443**
			(.636)	(.601)	(.731)
Cons Seat Share				-.021	-.027
				(.021)	(.021)
Cons Control				-1.908	-.441
				(.872)	(.024)
Labour Seat Share				-.026	-.018
				(.022)	(.024)
Labour Control				3.239***	4.328***
				(.702)	(.823)
(Top Band Houses)/(Bottom Band Houses)					-.247***
					(.081)
Share households renting from private					37.108***
					(7.838)
Share households renting from Council					-.939
					(5.088)
Year Dummies	Y	Y	Y	Y	Y
Observations	684	684	684	684	624

Notes: This table estimates a cross-council model of determinants of poll-tax evasion. The unit of observation is council-year. Standard errors corrected for heteroskedasticity. *, **, *** denote significance at the 10%, 5%, and 1% level respectively. Conditional poll-tax evasion in the main text is defined as the residual component of poll-tax evasion based on the model in Column 5. Sample years: 1990-1992, which correspond to the years where the poll tax was in place.

TABLE 18: RDD ESTIMATES OF THE IMPACT EFFECTS OF SINGLE MAJORITY ON TAX EVASION AND ENFORCEMENT

Panel A												
	LHS: Tax Evasion=[Net Collectable - Net Collected]/[Net Collectable]											
Bandwidth	1.66	3.32	6.64	1.66	3.32	6.64	1.66	3.32	6.64	1.66	3.32	6.64
1(Maj Ctl)	-8.57***	-2.23**	-1.45***	-8.53***	-2.24**	-1.45***	-8.39***	-1.82**	-1.12**	-8.57***	-1.98**	-1.06**
	(2.91)	(.90)	(.49)	(2.85)	(.90)	(.49)	(3.04)	(.89)	(.47)	(3.05)	(.88)	(.47)
Controls	None			Council FE			Council Fe, Year FE			Council FE, Year FE, ctls		
Observations	708	1533	2804	708	1533	2804	708	1533	2804	708	1533	2804
Mean LHS	4.152	4.031	3.828	4.152	4.031	3.828	4.152	4.031	3.828	4.152	4.031	3.828
Panel B												
	LHS Enforcement=[Summons]/[Reminders]											
Bandwidth	1.061	2.123	4.246	1.061	2.123	4.246	1.061	2.123	4.246	1.061	2.123	4.246
1(Maj Ctl)	.042	.062*	.044*	.004	.059*	.044*	.027	.060	.020	.199	.055	.017
	(.105)	(.035)	(.024)	(.113)	(.035)	(.024)	(.176)	(.039)	(.024)	(.195)	(.040)	(.024)
Controls	None			Council FE			Council FE, Year FE			Council FE, Year FE, ctls		
Observations	393	859	1662	393	859	1662	393	859	1662	393	859	1662
Mean LHS	.302	.295	.303	.302	.295	.302	.302	.295	.303	.302	.295	.303

Notes: This table presents RDD estimates of the impact effects of a random switch into single-party majority. Panel A shows estimates for tax evasion while Panel B shows estimates for enforcement, defined as the ratio of summons to reminders. All estimations are based on a local linear regression. Each panel shows the RDD estimates across four specifications (left to right): (i) no controls, (ii) council fixed effects, (iii) these plus year fixed effects, and (iv) these plus controls for tax liability and (log) per-capita income. Within each specification, point estimates are presented for bandwidths from (left to right): (i) half, (ii) equal to, (iii) double the optimum chosen by the Imbens and Kalyanaraman (2009 algorithm, which minimizes squared bias plus variance. Underneath each specification, we report the number of observations that lie within the given bandwidth, and the sample mean of the dependent variable. See the main text (Section 2.4.2) for further details. Use of Stata rd-package. Standard errors in parenthesis. *, **, *** denote significance at the 10%, 5%, 1% level, respectively.

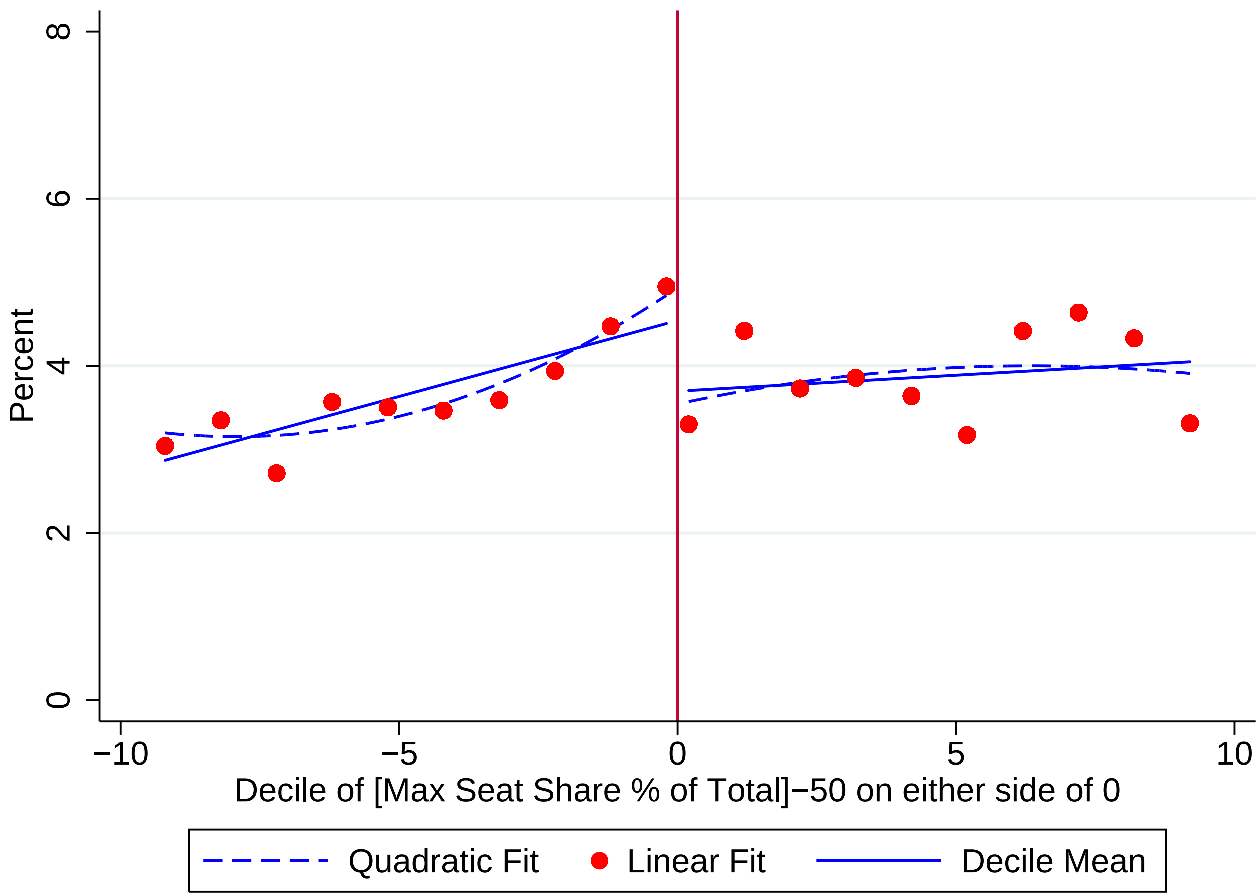
TABLE 19: ALTERNATIVE RDD SPECIFCATIONS AND BANDWIDTHS

Panel A								
	LHS: Tax Evasion=[Net Collectable - Net Collected]/[Net Collectable]							
Bandwidth	1.5		3		5		10	
Control Function	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
1(Maj Ctl)								
w/o controls	-10.595***	-50.292***	-2.565**	-7.614**	-1.895***	-3.086**	-1.174***	-2.333***
	(3.385)	(16.052)	(1.043)	(3.147)	(.609)	(1.343)	(.392)	(.651)
Observations	664		1384		2287		3933	
Panel B								
	LHS: Enforcement=[Summons]/[Reminders]							
Bandwidth	1.5		3		5		10	
Control Function	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
1(Maj Ctl)								
w/o controls	.027	-.021	.042	.078	.040	.047	-.049	.040
	(.055)	(.028)	(.205)	(.064)	(.022)	(.039)	(.060)	(.028)
w controls	.203**	.557**	.069**	.177**	.050**	.091**	-.045	.077**
	(.203)	(.274)	(.032)	(.073)	(.023)	(.042)	(.058)	(.039)
Observations	574		1185		1948		3327	

Notes: this table presents RDD estimates of the impact effects of a random majority switch on tax evasion and enforcement. Panel A shows estimates for tax evasion, while Panel B shows estimates for our enforcement measure, the ratio of summons to reminders. Each panel shows RDD estimates from two specifications with a bandwidth of (left to right): (i) 1.5%, (ii) 3%, (iii) 5%, (iv) 10%. In each specification, we present point estimates with linear and quadratic control functions in the running variable, i.e. the difference between the seat share of the largest party and 50%. Panel B shows results without controls, and with council-specific controls: log per capita income, tax liability, and log population. The number of observations used are reported below each specification. Use of Stata-rd package. Standard errors in parenthesis. *, **, *** denote significance at the 10%, 5%, 1% level respectively. See main text (Section 2.4.2) for further details.

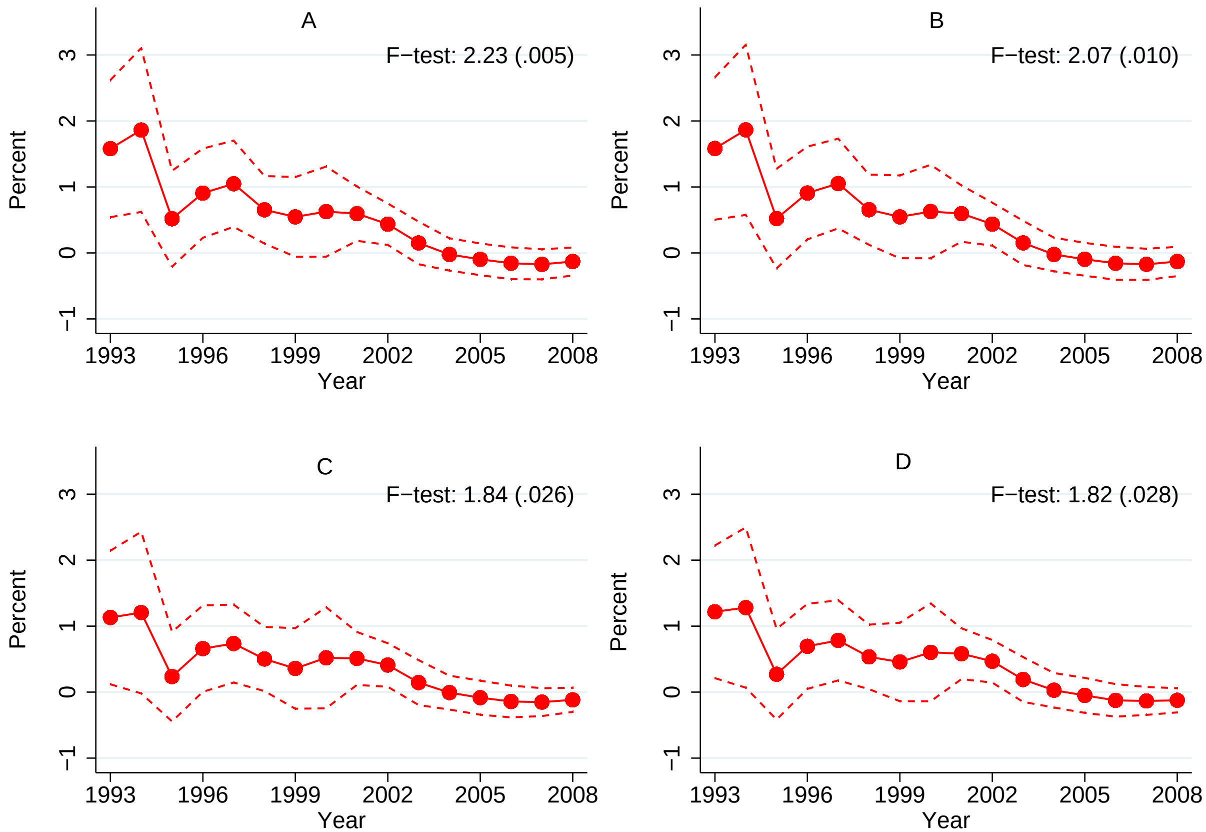
2.6 Appendix

FIGURE 28: SINGLE-PARTY MAJORITY AND TAX EVASION: ROBUSTNESS



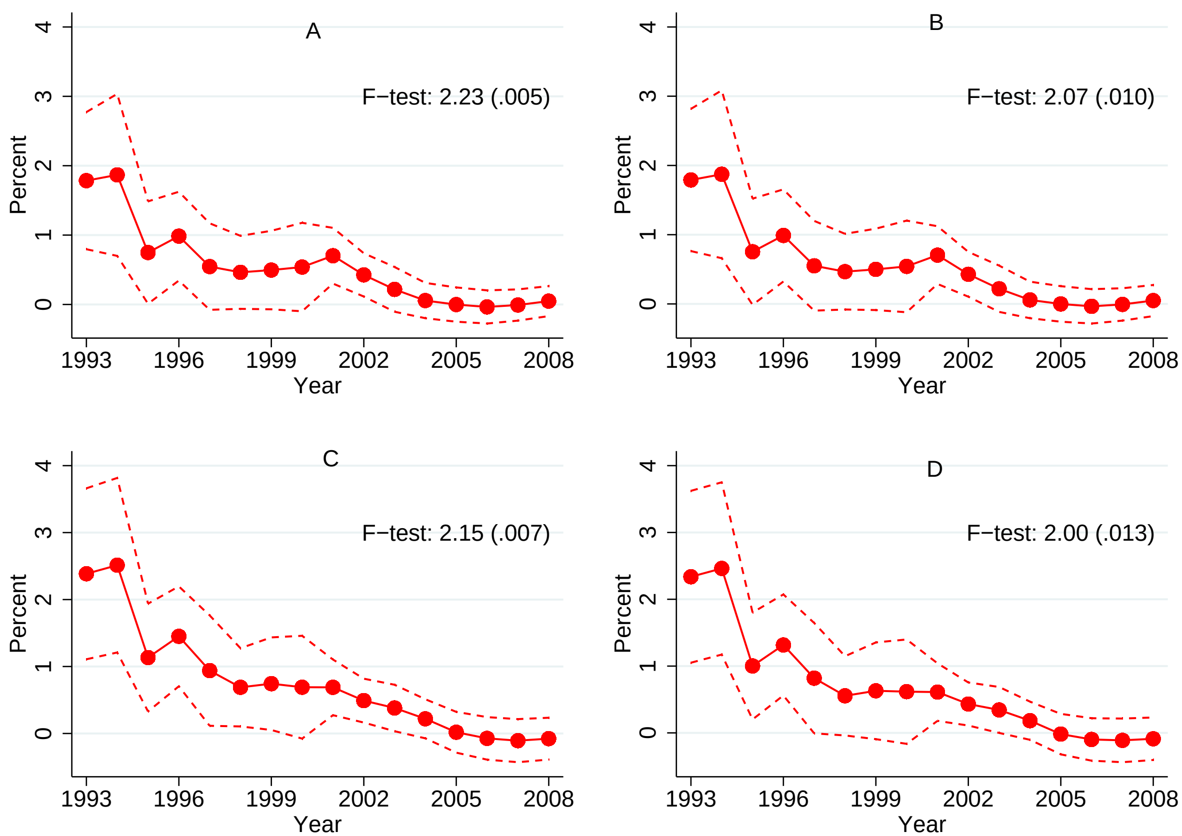
Notes: In this graph, separate decile-distributions were computed on either side of the single-party majority cut-off of 0, when the running variable is the difference between the largest party and 50% in a given council-year. Each dot represents average tax evasion in a decile of the sample between 0% and 10% (or 0% and -10%) for the running variable. The vertical line is the single-party majority cut-off at 0. Dashed (solid) lines are quadratic (linear) fits, separately estimated on each side of the cut-off and on the underlying council-level data. Sample: 1980-2009.

FIGURE 29: COUNCIL-TAX EVASION BY CONDITIONAL POLL-TAX EVASION: ROBUSTNESS



Notes: Panel A plots the **1**(Year)***1**(Conditional Poll-Tax Evasion>=Median) coefficients from a regression of council-tax evasion on a set of year dummies, year-dummies interacted with a high conditional poll-tax evasion dummy, and council fixed effects. Panel B plots the coefficients when the Panel A regression model is augmented by a council-specific linear-trend. Panel C reports the coefficients of Panel A model augmented with a full set of **1**[Region]***1**[Year] dummies. Panel D reports the coefficients of Panel A model augmented with a full set of **1**[Region]***1**[Year] dummies and the interactive quadratic control function in max-seat share. Full regression output for the respective panels is given in Columns (1), (2), (3) and (4) of Appendix Table 20. Sample period is 1993-2009, which corresponds to the council-tax period. The omitted year-dummy is 2009. Dashed lines denote the 95% confidence interval of the interaction term. The F-test (and its p-value) refers to the joint significance of all interactions **1**(Year)***1**(Conditional Poll-Tax Evasion>=Median). See the main text (Section 2.4.1) for details on construction of **1**(Conditional Poll-Tax Evasion>=Median).

FIGURE 30: COUNCIL-TAX EVASION BY POLL-TAX EVASION: ROBUSTNESS

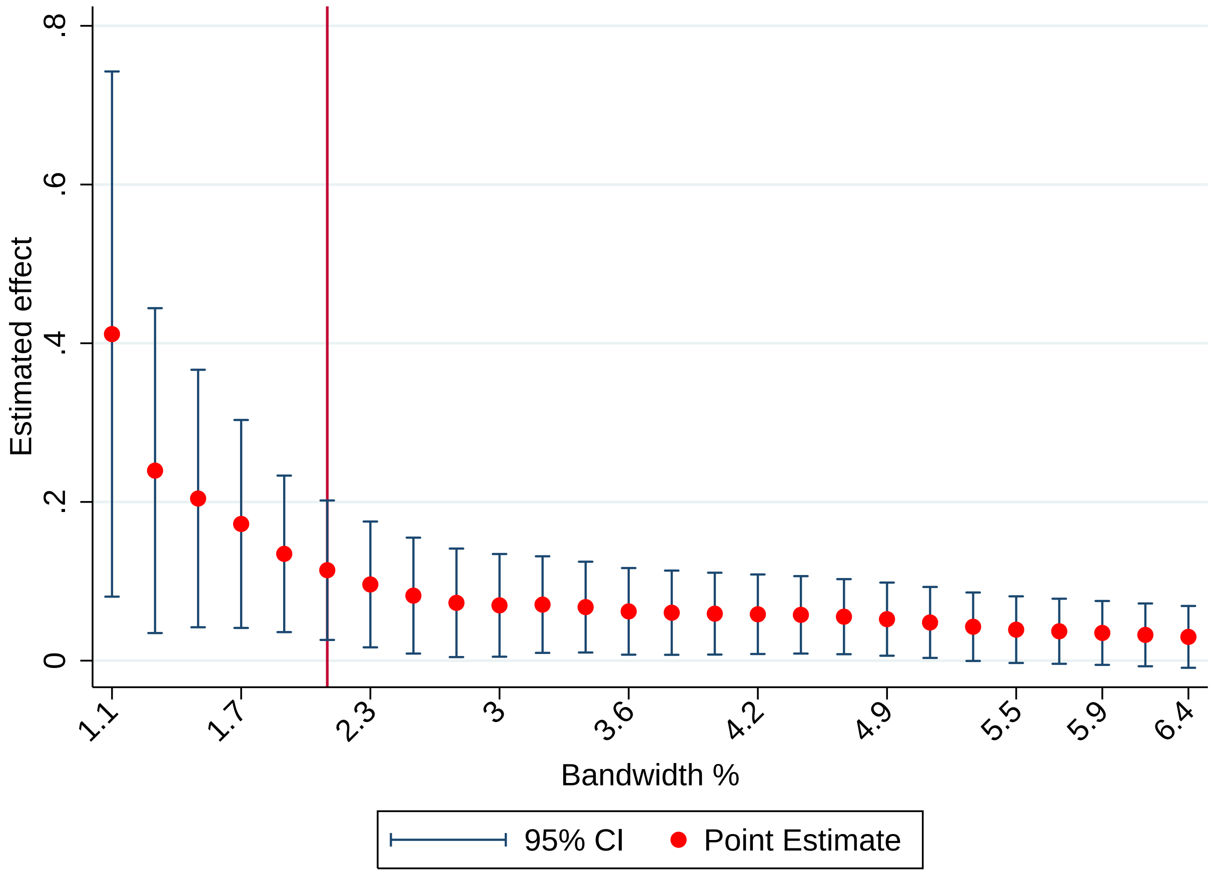


Notes: Panel A plots the **1**(Year)***1**(Poll-Tax Evasion>=Median) coefficients from a regression of council-tax evasion on a set of year dummies, year-dummies interacted with a high poll-tax evasion dummy, and council fixed effects. Panel B plots the coefficients when the Panel A regression model is augmented by a council-specific linear-trend. Panel C reports the coefficients of Panel A model augmented with a full set of **1**[Region]***1**[Year] dummies. Panel D reports the coefficients of Panel A model augmented with a full set of **1**[Region]***1**[Year] dummies and the interactive quadratic control function in max-seat share. Full regression output for the respective panels is given in Columns (1), (2), (3) and (4) of Appendix Table 21. Sample period is 1993-2009, which corresponds to the council-tax period. The omitted year-dummy is 2009. Dashed lines denote the 95% confidence interval of the interaction term. The F-test (and its p-value) refers to the joint significance of all interactions **1**(Year)***1**(Poll-Tax Evasion>=Median). See the main text (Section 2.4.1) for details on construction of **1**(Poll-Tax Evasion>=Median).

FIGURE 31: DOMESTIC-RATES EVASION BY POLL-TAX EVASION

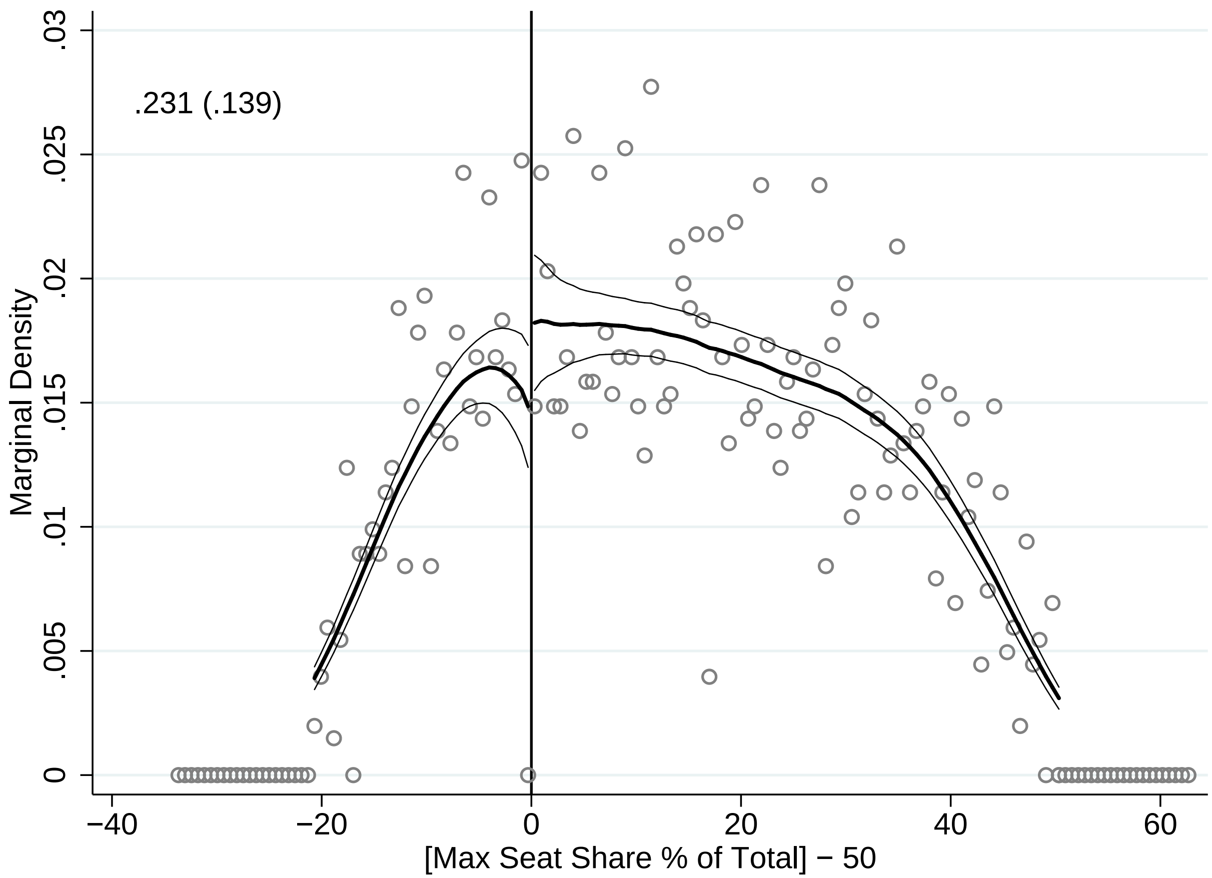
Notes: This graph plots the **1**(Year)***1**(Poll-Tax Evasion>=Median) coefficients from a regression of domestic-rates evasion on a set of year dummies, year-dummies interacted with a high poll-tax evasion dummy, and council fixed effects. The sample period is 1980-89, which corresponds to the domestic-rates period of our sample. Omitted year-dummy is 1980. Dashed lines denote the 95% confidence interval of the interaction term. The F-test (and its p-value) refers to the joint significance of all interactions **1**(Year)***1**(Poll-Tax Evasion>=Median). The underlying regression model is the same as that in the Notes to Appendix Table 21.

FIGURE 32: SINGLE-MAJORITY IMPACT EFFECT ON TAX ENFORCEMENT BY ESTIMATION BANDWIDTH



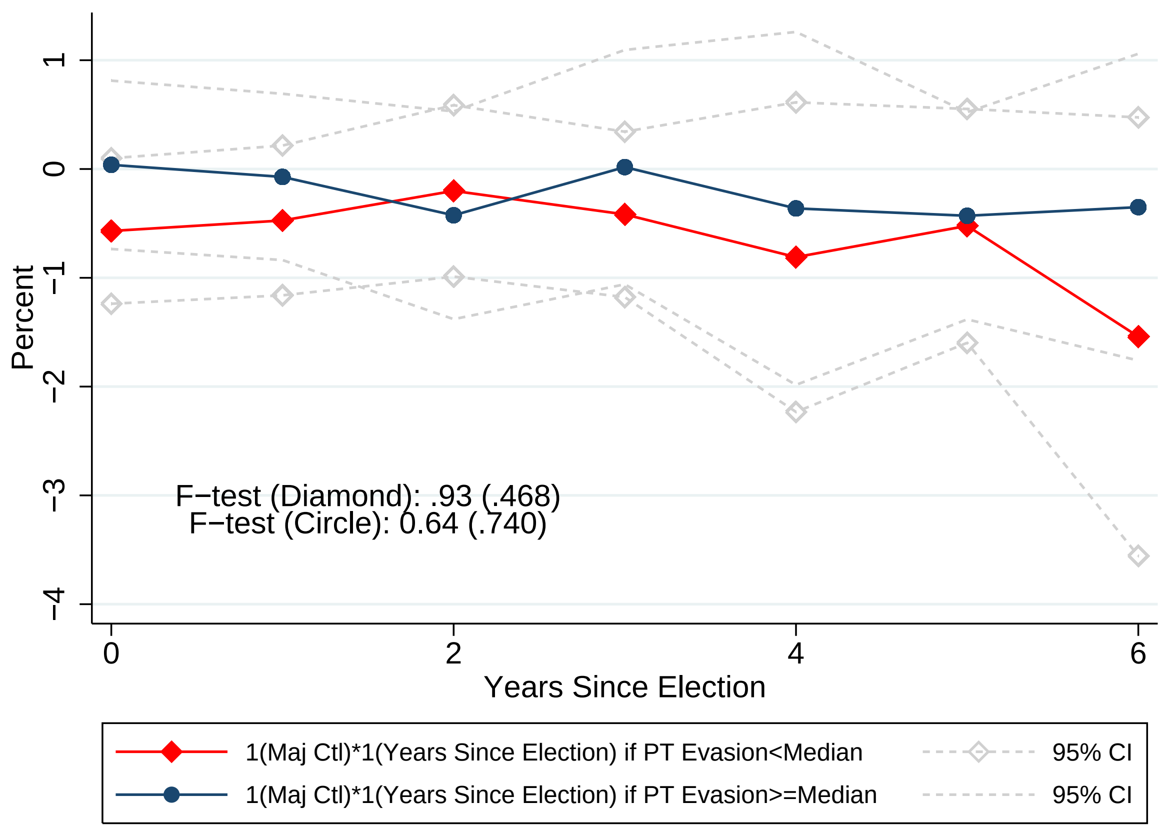
Notes: This graph plots the point estimates from an RDD for the impact effect on tax enforcement of a switch into single-party majority. The vertical line at 2.12% shows the optimal bandwidth for the running variable determined by the Imbens and Kalyanaraman (2009) algorithm – here, the difference between the seat share for the largest party in the council and 50%. Each point is the estimate of a separate local linear RDD regression, as the bandwidth varies from half the I-K optimum (at 1.1%) to 3 times (at 6.4%) this optimum. Log population, tax liability per dwelling and log per capita income are included as controls. Vertical blue lines denote the 95% confidence intervals for the point estimates.

FIGURE 33: MCCRARY TEST AROUND MAJORITY THRESHOLD



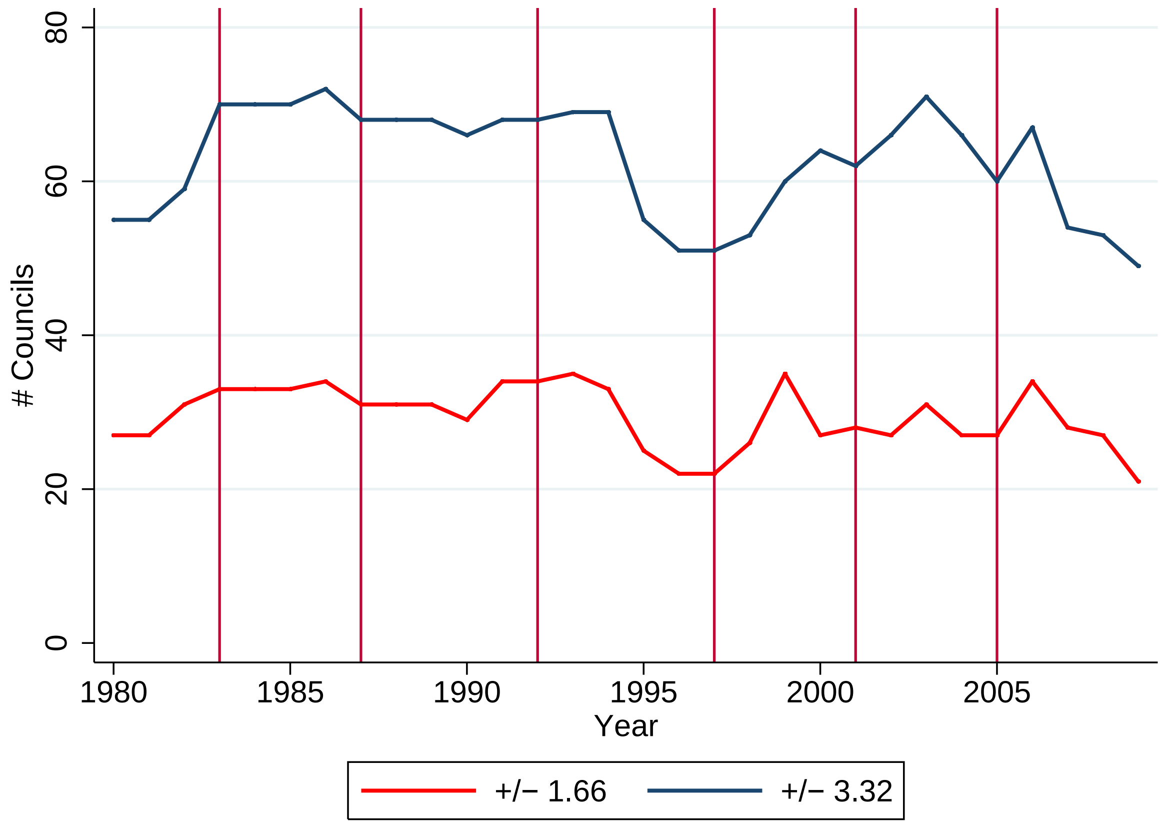
Notes: This graph displays the result of the McCrary (2008) test for continuity in the running variable, i.e. the difference between the largest seat-share on the council and 50%. The vertical line denotes the threshold value of 0. The bandwidth is optimally selected. The test employs a local-linear density estimator and delivers a test statistic for the null hypothesis that the difference in marginal densities between the two sides of the threshold is zero. This statistic (its p-value) is reported in upper left corner. The sample consists of council-years between 1980 and 2009.

FIGURE 34: DYNAMIC IMPACT ON COUNCIL-TAX EVASION BY POLL-TAX EVASION



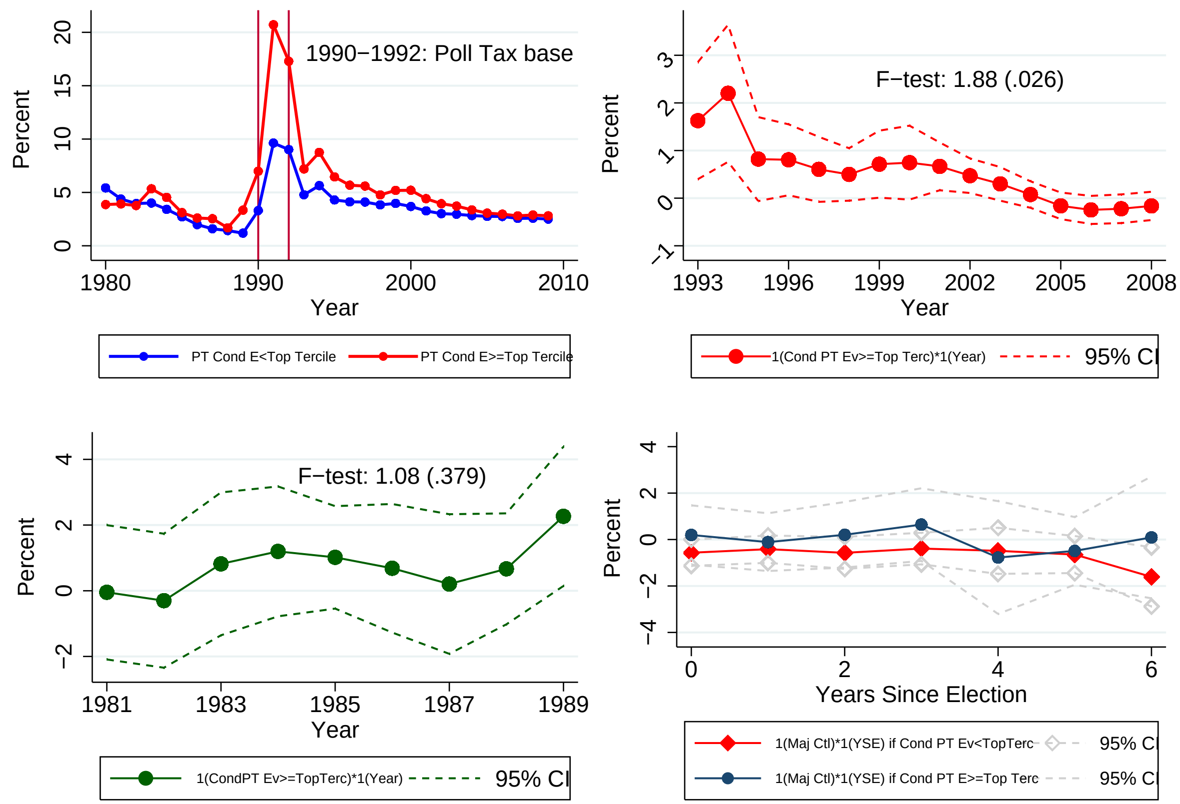
Notes: This graph plots the **1**(Majority Control)***1**(Years Since Election) coefficients from a regression of our measure of tax evasion on the indicator for switch into majority, **1**(Majority Control), seven years-since-election dummies, **1**(Years Since Election), their interactions, lagged council-specific tax evasion, and a quadratic control function in the largest political seat-share. The red line with diamonds shows the interaction coefficients for the councils that had poll-tax evasion below the median poll tax evasion, while the blue line with circles shows the same estimates for the councils with above median poll-tax evasion. The F-test (and its p-value) refers to the joint significance of the set of seven interaction terms. Full regression output for the graph is given in columns 1 and 3 of Appendix Table 25. The model conditions the sample on the high poll-tax evasion dummy. See the text (Sections 2.4.1 and 4.2) for further details and the construction of the high and low poll-tax evasion subsamples. The sample runs from 1993 to 2009.

Figure 35: Number of Close Elections over Time



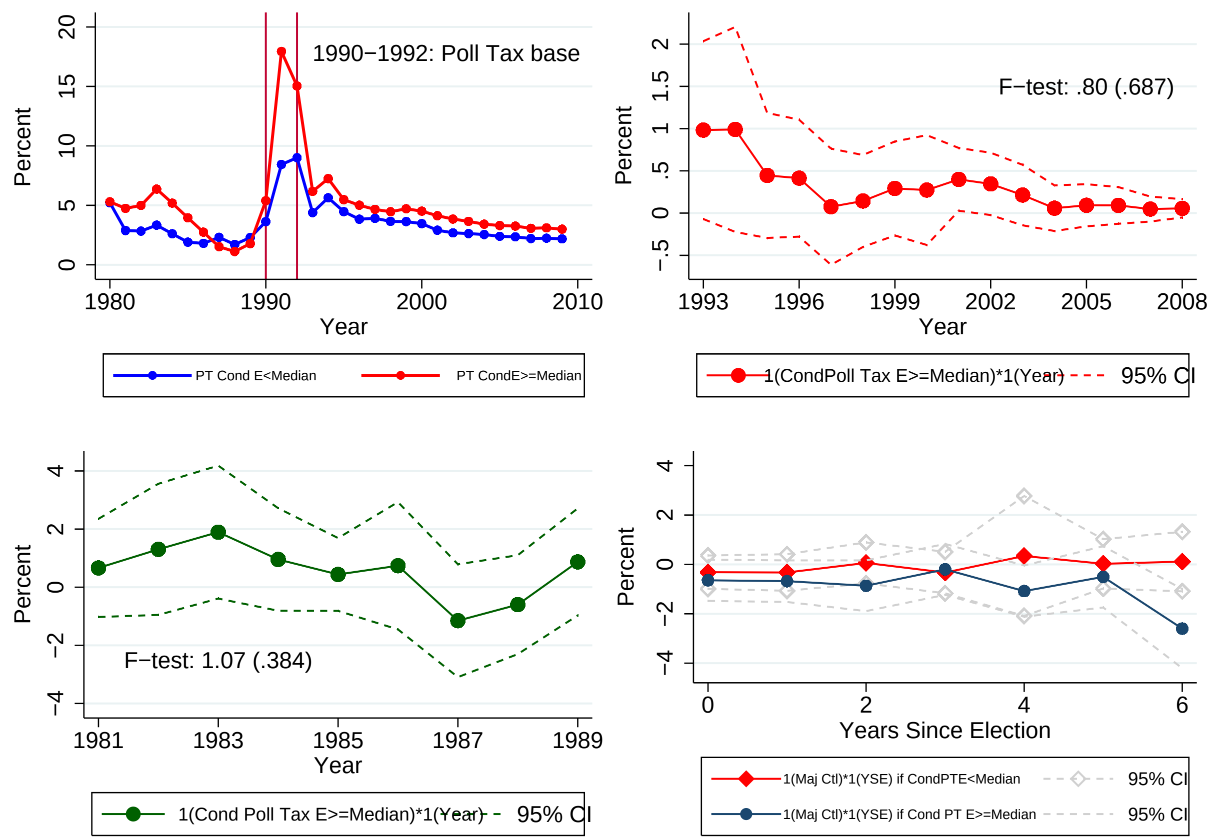
Notes: This graph shows the number of council-specific close elections in each year between 1980 and 2009 for different definitions of 'close'. The blue line defines 'close' as the optimal bandwidth for RDD proposed by Imbens and Kalanyaramanan (2012) applied to tax evasion, and is equal to 3.32 percentage points. The red line corresponds to half this optimum bandwidth, 1.66 percentage points. Vertical red lines denote years of UK general elections.

FIGURE 36: CORE RESULTS BY TOP TERCILE CONDITIONAL POLL-TAX EVASION



Notes: In these panels, councils are classified according to whether their average conditional Poll-Tax evasion lied above or below the top tercile value of the Poll-Tax conditional evasion distribution. Conditional Poll-Tax is the residual component of poll-tax evasion based on the model in Column 5 of Table 17. Using this new treatment dummy, Panels A, B, C, and D display results using the same empirical setting as underlying respectively Figs.22, 23, 24, and 27.

FIGURE 37: CORE RESULTS BY CONDITIONAL POLL-TAX EVASION: ROBUSTNESS



Notes: In these panels, councils are classified according to whether their average conditional Poll-Tax evasion lied above or below the median value of the conditional Poll-Tax evasion distribution. Conditional Poll-Tax is the residual component of Poll-Tax evasion based on the model in Column 5 of Table 17, augmented with an estimated measure of the difference in household tax-liability between the last year of Domestic Rates and the first year of the Poll Tax. Using this new treatment dummy, Panels A, B, C, and D display results using the same empirical setting as underlying respectively Figs.Figs.22, 23, 24, and 27. The measure is not available for all councils in the required years, and leads to a 30 percent decrease in sample-size relative to the sample used in the results of Figs.19-27.

TABLE 20: COUNCIL-TAX EVASION BY CONDITIONAL POLL-TAX EVASION: ROBUSTNESS

LHS: Tax Evasion=[Net Collectable - Net Collected]/[Net Collectable]

	(1)	(2)	(3)	(4)
1(PT Conditional Evasion>Median)				
*1(Year==1993)	1.581***	1.582***	1.131***	1.217**
	(.532)	(.533)	(.519)	(.515)
*1(Year==1994)	1.863***	1.866***	1.206*	1.281**
	(.636)	(.661)	(.628)	(.622)
*1(Year==1995)	.518	.520	.235	.270
	(.372)	(.387)	(.348)	(.352)
*1(Year==1996)	.906***	.908**	.659**	.694**
	(.346)	(.360)	(.335)	(.330)
1(Year==1997)	1.049***	1.052***	.736**	.785**
	(.335)	(.348)	(.302)	(.311)
1(Year==1998)	.653**	.655**	.502**	.534**
	(.262)	(.272)	(.249)	(.250)
1(Year==1999)	.546*	.547*	.358	.457
	(.309)	(.321)	(.313)	(.304)
1(Year==2000)	.626*	.627*	.520	.603
	(.350)	(.363)	(.392)	(.380)
*1(Year==2001)	.595***	.596***	.511**	.582**
	(.210)	(.218)	(.205)	(.197)
*1(Year==2002)	.435***	.436***	.411**	.467**
	(.160)	(.166)	(.169)	(.165)
1(Year==2003)	.151	.152	.144	.191
	(.165)	(.171)	(.174)	(.174)
1(Year==2004)	-.022	-.022	-.006	.028
	(.125)	(.130)	(.131)	(.133)
1(Year==2005)	-.096	-.096	-.084	-.049
	(.123)	(.127)	(.132)	(.135)
1(Year==2006)	-.156	-.156	-.141	-.124
	(.123)	(.128)	(.123)	(.126)
1(Year==2007)	-.172	-.172	-.151	-.133
	(.116)	(.121)	(.108)	(.108)
1(Year==2008)	-.129	-.129	-.116	-.123
	(.109)	(.113)	(.093)	(.094)
F-test	2.23	2.07	1.84	1.82
	(.004)	(.009)	(.026)	(.028)
Council FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Council linear time trend	N	Y	N	N
1(Region)*1(Year) FE	N	N	Y	Y
Max Seat Share Quadratic Control	N	N	N	Y
Observations	3828	3828	3828	3818
Mean LHS	3.725	3.725	3.725	3.725

Notes: Standard errors clustered at the council level. *, **, *** denote significance at the 10%, 5%, 1% level respectively. Sample years: 1993-2009. We estimate the following model

$$e_{it} = \alpha + \beta_t \left[1(\text{Conditional PT Evasion>Median})_i \cdot \delta_t\right] + \delta_t + \mu_i + \varepsilon_{it};$$

where e_{it} is the council-year measure of tax evasion, $1(\text{Conditional PT Evasion>Median})_i$ is a council-specific dummy equal to 1 if the council had average conditional Poll-Tax evasion above median conditional Poll-Tax evasion. 2009 is the omitted year category. Reported F-test value is from a F-test on the joint significance on the set of all β_t coefficients being equal to zero.

TABLE 21: COUNCIL-TAX EVASION BY POLL-TAX EVASION: ROBUSTNESS

LHS: Tax Evasion=[Net Collectable - Net Collected]/[Net Collectable]				
	(1)	(2)	(3)	(4)
1(PT Conditional Evasion>Median)				
*1(Year==1993)	1.784***	1.790***	2.385***	2.337***
	(.503)	(.526)	(.654)	(.659)
*1(Year==1994)	1.867***	1.872***	2.513***	2.462***
	(.598)	(.621)	(.668)	(.661)
*1(Year==1995)	.748**	.753*	1.134***	1.002*
	(.378)	(.393)	(.412)	(.410)
*1(Year==1996)	.984***	.989***	1.451**	1.316***
	(.328)	(.341)	(.382)	(.387)
1(Year==1997)	.546*	.551*	.939**	.818*
	(.319)	(.332)	(.422)	(.423)
1(Year==1998)	.463*	.466*	.689**	.554*
	(.269)	(.280)	(.299)	(.304)
1(Year==1999)	.496*	.499*	.744**	.630*
	(.290)	(.301)	(.354)	(.371)
1(Year==2000)	.539*	.542	.691	.617
	(.327)	(.339)	(.393)	(.400)
*1(Year==2001)	.702***	.705***	.689***	.611***
	(.206)	(.213)	(.213)	(.220)
*1(Year==2002)	.426***	.428***	.491**	.432***
	(.159)	(.165)	(.168)	(.165)
1(Year==2003)	.217	.219	.381**	.343*
	(.165)	(.171)	(.177)	(.176)
1(Year==2004)	.056	.057	.219	.183
	(.130)	(.135)	(.149)	(.146)
1(Year==2005)	-.001	-.000	.018	-.017
	(.126)	(.131)	(.156)	(.155)
1(Year==2006)	-.035	-.034	-.072	-.097
	(.122)	(.127)	(.162)	(.162)
1(Year==2007)	-.007	-.006	-.106	-.111
	(.115)	(.119)	(.165)	(.163)
1(Year==2008)	.048	.049	-.076	-.087
	(.110)	(.115)	(.159)	(.163)
F-test	2.23	2.07	2.15	2.00
	(.004)	(.009)	(.006)	(.013)
Council FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Council linear time trend	N	Y	N	N
1(Region)*1(Year) FE	N	N	Y	Y
Max Seat Share Quadratic Control	N	N	N	Y
Observations	4223	4223	4223	4223
Mean LHS	3.725	3.725	3.725	3.725

Notes: Standard errors clustered at the council level. *, **, *** denote significance at the 10%, 5%, 1% level respectively. Sample years: 1993-2009. We estimate the following model

$$e_{it} = \alpha + \beta_t \left[1(\text{PT Evasion}>\text{Median})_i \cdot \delta_t\right] + \delta_t + \mu_i + \varepsilon_{it};$$

where e_{it} is the council-year measure of tax evasion, $1(\text{PT Evasion} > \text{Median})_i$ is a council-specific dummy equal to 1 if the council had average Poll-Tax evasion above median Poll-Tax evasion. 2009 is the omitted year category. Reported F-test value is from a F-test on the joint significance on the set of all β_t coefficients being equal to zero.

TABLE 22: PLACEBO RDD ON TAX LIABILITY AND LOG PER-CAPITA INCOME

Panel A												
	LHS: Domestic Tax Liability per Household											
Bandwidth	4.41	8.83	17.66	4.41	8.83	17.66	4.41	8.83	17.66	4.41	8.83	17.66
1(Maj Ctl)	42.41	29.09	38.09	42.70	29.03	37.08	-3.92	9.99	26.58	-14.15	-.802	21.093
	(37.33)	(24.84)	(25.44)	(37.34)	(24.84)	(25.06)	(10.34)	(6.57)	(18.48)	(9.58)	(6.10)	(21.79)
Controls		None			Council FE			Council Fe, Year FE			Council FE, Year FE, ctls	
Observations	2375	4189	6686	2375	4189	6686	2375	4189	6686	2375	4189	6686
Mean LHS	566.80	569.95	581.52	566.80	569.95	581.52	566.80	569.95	581.52	566.80	569.95	581.52
Panel B												
	LHS Log(per Capita Income)											
Bandwidth	.851	1.703	3.406	.851	1.703	3.406	.851	1.703	3.406	.851	1.703	3.406
1(Maj Ctl)	-2.421	.102	.170*	-2.453	.095	.169*	.197	-.089	.026	-.191	.017	.048**
	(1.609)	(.268)	(.090)	(1.628)	(.268)	(.090)	(.327)	(.061)	(.020)	(.334)	(.061)	(.048)
Controls		None			Council FE			Council FE, Year FE			Council FE, Year FE, ctls	
Observations	400	959	2045	400	959	2045	400	959	2045	400	959	2045
Mean LHS	9.130	9.093	9.103	9.130	9.093	9.103	9.130	9.093	9.103	9.130	9.093	9.103

Notes: This table presents RDD estimates of the impact effects of a random switch into single-party majority. Panel A shows estimates for local household tax liability, while Panel B shows estimates for log per capita income. All estimates are based on a local linear regression. Each panel shows results across four specifications (left to right): (i) no controls, (ii) council fixed effects, (iii) these plus year fixed effects, and (iv) these plus controls for log population and log per capita income (log population and tax liability in Panel B). In each specification, we present estimates as the bandwidth varies from (left to right): (i) half of, (ii) equal to, (iii) double the optimal bandwidth calculated from Imbens and Kalyanaraman (2009) algorithm, which minimizes squared bias plus variance. We also show the number of observations corresponding to these bandwidths, and the sample mean of the dependent variable. Standard errors in parenthesis. *, **, *** denote significance at the 10%, 5%, 1% level respectively. Stata rd-package. See the main text (Section 2.4.2) for further details.

TABLE 23: IMPULSE-RESPONSE RESULTS

LHS: Tax Evasion=[Net Collectable - Net Collected]/[Net Collectable]		
	(1)	(2)
1(Maj Control)		
*1(Year Since Election=0)	-.857***	-.415*
	(.280)	(.238)
*1(Year Since Election=1)	-.844***	-.395
	(.296)	(.250)
*1(Year Since Election=2)	-1.012***	-.560**
	(.333)	(.285)
*1(Year Since Election=3)	-.734**	-.259
	(.358)	(.305)
*1(Year Since Election=4)	-1.104**	-.531
	(.540)	(.498)
*1(Year Since Election=5)	-1.226***	-.583*
	(.405)	(.350)
*1(Year Since Election=6)	-1.761***	-1.104*
	(.683)	(.616)
F-test	2.15	1.00
	(.038)	(.427)
Council FE	Y	Y
6-year FE	Y	Y
Council Controls	N	Y
Observations	4308	4243
Mean LHS	3.725	3.724

Notes: The estimated model is

$$\begin{aligned} e_{it} &= \kappa + \alpha e_{it-1} + \sum_{j=0}^{6} \tau_{ij} + \sum_{j=0}^{6} \theta_j \left[\tau_{ij} \cdot 1\left(\text{MajCtl}\right)_{ij} \right] + \beta f\left(\text{max-share}\right)_{it} \\ &\quad + \phi \left[f\left(\text{max-share}\right)_{it} \cdot 1\left(\text{MajCtl}\right) \right] + \delta_t + \mu_i + \varepsilon_{it} \end{aligned}$$

where τ_{ij} is a council-specific years-since-election dummy, $1\left(\text{MajCtl}\right)_{ij}$ is a dummy for single-party majority in the council-year, $f\left(\text{max-share}\right)_{it}$ is a second-order control function in the largest seat-share on the council, and δ_t are six-year-period dummies. Council controls are log per capita income, tax liability per dwelling, log population, Conservative seat-share and dummy for majority control, Labour seat-share and dummy for majority control. Standard errors are clustered at the council level. *, **, *** denote significance at the 10%, 5% and 1% level respectively. Sample: 1993 to 2009.

TABLE 24: HETEROGENEOUS IMPULSE RESPONSES BY CONDITIONAL POLL-TAX EVASION

LHS: Tax Evasion=[Net Collectable - Net Collected]/[Net Collectable]				
Sample restriction	PT Cond Evasion$^{<}$Median		PT Cond Evasion$^{>=}$Median	
	(1)	(2)	(3)	(4)
1(Maj Control)				
*1(Year Since Election=0)	-.769***	-.706***	-.010	.037
	(.292)	(.267)	(.406)	(.425)
*1(Year Since Election=1)	-.526*	-.488*	-.239	-.214
	(.298)	(.266)	(.408)	(.426)
*1(Year Since Election=2)	-.455	-.440	-.255	-.267
	(.337)	(.314)	(.510)	(.525)
*1(Year Since Election=3)	-.592*	-.534	.458	.552
	(.348)	(.329)	(.547)	(.576)
*1(Year Since Election=4)	-1.195***	-.920***	-.295	-.476
	(.341)	(.313)	(.885)	(.945)
*1(Year Since Election=5)	-1.163***	-.795**	-.429	-.580
	(.321)	(.307)	(.477)	(.487)
*1(Year Since Election=6)	-2.181**	-1.953**	-.129	-.177
	(.849)	(.824)	(.879)	(.865)
F-test	3.24	2.17	1.17	1.50
	(.003)	(.040)	(.323)	(.172)
Council FE	Y	Y	Y	Y
6-year FEs	Y	Y	Y	Y
Council Controls	N	Y	N	Y
Observations	1892	1773	1709	1690
Mean LHS	3.153	3.153	4.247	4.247

Notes: The estimated model is exactly the same as in the notes to Appendix Table 23, but where the sample is conditioned on whether the council has low (Cols.1-2) or high (Cols.3-4) conditional Poll-Tax evasion. See the text (Sections 2.4.1 and 2.4.2) for further details and for the construction of the high and low poll-tax conditional evasion subsamples.

TABLE 25: HETEROGENEOUS IMPULSE RESPONSES BY POLL-TAX EVASION

LHS: Tax Evasion=[Net Collectable - Net Collected]/[Net Collectable]				
Sample restriction	PT Evasion<Median		PT Evasion>=Median	
	(1)	(2)	(3)	(4)
1(Maj Control)				
*1(Year Since Election=0)	-.569*	-.499	.038	.017
	(.342)	(.332)	(.396)	(.368)
*1(Year Since Election=1)	-.472	-.427	-.072	-.076
	(.353)	(.342)	(.391)	(.362)
*1(Year Since Election=2)	-.200	-.164	-.425	-.454
	(.403)	(.394)	(.489)	(.458)
*1(Year Since Election=3)	-.417	-.339	.017	.101
	(.389)	(.384)	(.552)	(.537)
*1(Year Since Election=4)	-.809	-.687	-.362	-.487
	(.729)	(.692)	(.831)	(.911)
*1(Year Since Election=5)	-.522	-.374	-.429	-.551
	(.551)	(.456)	(.488)	(.481)
*1(Year Since Election=6)	-1.540	-1.267	-.350	-.363
	(1.033)	(1.000)	(.722)	(.709)
F-test	0.93	0.77	0.62	0.80
	(.486)	(.615)	(.740)	(.585)
Council FE	Y	Y	Y	Y
6-year FEs	Y	Y	Y	Y
Council Controls	N	Y	N	Y
Observations	1972	1939	1886	1863
Mean LHS	3.153	3.153	4.245	4.245

Notes: The estimated model is exactly the same as in the notes to Appendix Table 23, but where the sample is conditioned on whether the council has low (Cols.1-2) or high (Cols.3-4) Poll-Tax evasion. See the text (Sections 2.4.1 and 2.4.2) for further details and for the construction of the high and low Poll-Tax evasion subsamples.

3 An Empirical Test of Information Trails[71]

Abstract

Firm level tax compliance depends on the stock of information accessible to the government. Theoretical work (Gordon and Li, 2009; Kleven, Kreiner and Saez, 2009) highlights two particular information trails: access to formal finance and the number of employees. In this article we test whether firms with more employees and with more formal finance are more likely to be audited and less prone to evading taxes, using firm-level data on 108,000 firms across 79 countries in the World Bank Enterprise Surveys. Instruments for finance and workers are constructed at the industry-level, using an out of sample extrapolation strategy related to Rajan and Zingales (1998). The instruments isolate variation in industry technological demand for labour and formal finance by taking the US industry distributions as undistorted benchmarks. We find a large positive effect of the firm-size information trail on both tax inspection and tax compliance, but no significant impact of reliance on external finance. We frame the empirical strategies with a simple model where a firm chooses a percentage of sales taxes to evade, its funding-composition between internal and external finance and its number of workers, where only external financing and the number of employees generate information trails visible to the tax authorities.

[71] We are grateful to Alan Auerbach, Tim Besley, Michael Best, Tom Cunningham, Roger Gordon, Michael Keen, Henrik Kleven, Camille Landais, Florian Misch, Ted Miguel, Torsten Persson, Andres Rodriguez-Clare, Emmanuel Saez, Johannes Spinnewijn, Owen Zidar and seminar participants at the LSE, the 69th IIPF Congress and the 2014 ZEW Public Finance conference for valuable comments. Financial support from the ESRC is gratefully acknowledged.

3.1 Introduction

Governments enforcing taxes are constrained by the stock and flows of information on transactions in the economy. Third-party reporting, verifiable paper trails, and whistle-blowing mechanisms have been shown empirically to create important information flows that facilitate tax enforcement (Kopczuk and Slemrod 2006, Kleven et al. 2009, 2010, Kumler et al. 2012, Pomeranz 2012, Carillo, Pomeranz and Singhal 2014).

In the theoretical literature, two settings in particular have been emphasized as powerful generators of tax-relevant information: the firm's interaction with the financial system and the firm's worker size. The financial trail is based on the insight that when accesing formal banking firms generate a paper trail of information, often including financial statements and lists of customers (Gordon and Li, 2009). If this financial information trail is shared by the bank with the tax authority, it may constitute the basis for stronger tax enforcement on financially connected firms. The firm-size information trail is related to the idea that firms with more employees are more complex and thus require more accurate book-keeping (Kleven, Kreiner and Saez, 2009). In the hands of a tax auditor, such book-keeping consitutes the paper trail necesary to enforce tax liability. Larger firms can still evade by keeping parallel books (one for their own use, one for the tax authority) but evasion will be more costly and collusion to under-report activities between employer and employees more likely to break down. This could be due either to random shocks that reveal the true internal records or to whistle-blowing.

Despite the importance of the worker-size and the financial information trails in the theoretical literature no studies has tested their empirical relevance. We provide the first empirical test to study whether larger-sized firms and more financially externally reliant firms face higher tax inspection and comply more with sales taxes. To motivate the empirical specification, we formulate a reduced-form model in which firm behaviour generates information trails that tax authorities can use to enforce tax collection.

We use the World Bank Enterprise Surveys (WBES) 2002-2010, a firm level survey with information on firm-size, external financial reliance, tax inspection and reported sales tax compliance for 108,000 firms in 79 countries. In Fig.38, pooling all countries together, we group firms in twenty equal-sized groups (vingtiles) of firm-size distribution, and plot mean tax inspection and sales tax compliance across the twenty vingtiles. Enforcement and compliance clearly increase monotonically by firm-size. In Fig.39 we construct the same graph, with twenty quantiles of external financial reliance. The upwards-sloping relationship is much less clear, both with regards tax inspection and sales tax compliance. These figures summarize a general trend in all our regressions results: we systematically uncover a strong positive effect of the firm-size information trail on tax inspection and sales tax compliance and an insignificant effect of the external finance trail.

In the regression setting, we isolate exogenous demand-driven variation in external funding and firm-size at the industry-level, by adopting an identification strategy related to Rajan and Zingales (1998, RZ hereafter). The identification strategy consists in predicting the size-ranks and the external finance-ranks of an industry within a WBES country fom the size and external finance rankings of the same set of industries in the U.S. This out-of-sample extrapolation technique relies on the assumption that the labor and finance markets of the U.S. are relatively undistorted and thus that the ranking of industries in external reliance and number of workers in the U.S. reflects "benchmark" technological differences in demand for labour and funding inputs. The first stage shows the extent to which US industry-ranks in firm-size and funding choice explain the WBES countries' corresponding industry-ranks. As RZ did not have data on external funding for firms in developing countries, we believe our study is the first to test the hypotheses of industry-specific country-invariant demand technologies for finance and firm-size in a large dataset of both developed and developing countries. We find strong evidence in favor of the industry-specific technology for workers, but weak evidence in the case of financial external reliance. In a double IV specification, which simultaneously instruments external reliance rank and firm-size rank by the U.S. rankings in external reliance and the US firm-size, we uncover strong effects for the worker-size trail on both tax inspection and sales tax compliance, but no discernible impact of external reliance on either inspection or compliance. These results speak to the importance of information trails and third party information in understanding firm level tax compliance behavior.

The results also relate to the revenue versus production efficiency trade-off the tax authority faces when choosing enforcement instruments.[72] On the one hand, the tax administration may consciously choose to leave aside relevant information trails generated from firm-size and external funding, since any enforcement based on firm size and funding will distort the firm input choices and generate socially costly production inefficiencies. On the other hand, using the information trails would help to maximize revenue for a given amount of enforcement effort: tax collection yield will be higher for the information-intensive firms since their paper trails allow the discovery of more evaded taxes. Empirically we find a significant firm-size effect on the probability of tax inspection, suggesting tax administrations do actively levy the size-information trail via targeted enforcement on larger firms. The positive association between exogenous

[72] Kleven et al. (2013) study the revenue versus production efficiency trade-off of a tax authority in setting the optimal tax base between pure-profits and a turnover tax

firm-size and tax revenue may in part be channeled through the size-based inspection policy.

The non-results concerning the financial trail are interesting in their own right. The insignificance in all regressions could point to the lack of willingness to use financial information by tax administrations or to the lack of access to such information. Despite an important push for the availability of bank information to tax authorities (OECD 2000, 2006) bank secrecy laws have often prevented significant improvements. In many countries, even in the case of an audit, accessing bank information can be long and tedious and typically requires the signature of a judge.

This paper contributes to the literature focusing on the importance of administrations' information constrains in understanding tax enforcement and tax structure policy. Kleven et al. (2013) study the minimum tax scheme in Pakistan where firms are taxed either on profits or turnover depending on whichever liability is larger. The authors show that this production inefficient policy can be rationalized by revenue-efficiency considerations, since it is typically harder to evade on the broader turnover tax base than on the profit tax base. Wingender's (2008) test of the relation between finance and evasion at the industry level, using an empirical strategy related to ours, uncovers a much larger impact of the finance information trail than we do. Other information trails have also been discussed. For example, Pomeranz (2013) studies the paper trail generated by VAT payments among firms in Chile. She shows that an audit threat to a firm producing at the end of the supply chain affects tax payments for the whole chain of suppliers. In addition she shows that firms already covered by a paper trail are not affected by the audit threat. She concludes that the VAT chain acts as a substitute for the paper trail in case of an audit. Artavanis, Morse and Tsoutsoura (2012) show that a large Greek bank had informally embedded into its credit models total income instead of reported income. If the formal lending books of the Greek bank are based on reported income, then this banking behavior could explain our finding of a non-significant external finance trail, since the books would be uninformative to the Greek tax authority. Interestingly though, the authors argue that the lack of tax enforcement does not arise because of a lack of information but because of a lack of political will. In this case the information trail is left on the side, not for production-efficiency reasons, but due to political factors. Finally, Besley & Persson (2012) develop a series of models which study the government's choice of investment in tax capacity, recognizing the practical limits imposed on administration policy, including information constraints and political instability.

Section 3.2 outlines an industry-representative firm model of external reliance, firm-size, tax enforcement and tax compliance, and derives the two sets of empirical predictions. Section 3.3 discusses the data and the identification strategy.

Section 3.4 presents the results for tax inspection and sales tax evasion and then performs robustness checks. Section 3.5 concludes.

3.2 Theory

3.2.1 Industry-representative firm specification

In order to study the relationship between information trails and tax under-reporting, we model the reduced-form interactions between access to external funds and tax enforcement on the one hand, and between firm-size and tax enforcement on the other hand.

The number of workers channel, formalized by Kleven, Kreiner and Saez (2009), highlights random shocks and whistle blowing as two mechanisms which could reveal the true business records to the tax authority. The model generates a critical threshold firm-size, N, below which full evasion can be the optimal strategy. Further, the optimal amount of under-reporting on the intensive margin will be decreasing in N above the threshold: in the whistle-blowing case, the reward to the whistle blower will be a share of total uncovered revenue, which is increasing in the size of the firm; in the random shocks case, the probability that any employee reveals the true, internal records, is also increasing in the total number of workers employed.

The second information channel is generated by a firm's interaction with the financial markets. The important distinction is between the use of internal and external sources of funding, where the latter is characterized to generate an information trail on firm's real activities. We model the choice of internal and external cash flow based on Kaplan and Zingales (1997), a workhorse model in the finance literature. In a one-period setting, a firm chooses a level of investment W to maximize (net-of-tax) profits. Any investment W can be financed with internal and with external funds, denoted respectively I and E: $W \equiv I + E$. The cost of internal funds is its opportunity cost in capital markets, which we normalize to 1. There exists a wedge, r, is between the firm's internal and external cost of funds: $r \geq 1$. This wedge could arise due to information or agency problems, which are passed on as costs to the firm. Investment financed internally is limited by the upper bound of available internal funds, $\bar{I}$. The firm should therefore use external finance only once it has exhausted its internal funds and reached the $\bar{I}$ limit.

Our model incorporates the internal and external funding choice and number of workers choice into a standard representative-firm setting of under-reporting

(Chetty, 2009). It models the firms tax-gain benefit of evasion e weighted against the evasion cost $c(e)$. Later in the text we discuss some possible functional forms for $c(e)$. Under convexity of $c(e)$ there will be an interior solution for evasion e.

Based on our above discussion, and a novel feature to this type of reduced-form evasion-cost models, we model the probability and cost of getting detected $c(e; E; N)$ as increasing in the level of evasion e; in the amount of external funds E; and in the number of workers N weakly above the threshold $\bar{N}$. The industry-representative firm faces the optimization problem

$$\underset{\{I,E,N,e\}}{max}\ (1-t)\left(f(W,N)-e\right)+e-c\left(e,E,1\left(N\geq\bar{N}\right)\cdot N\right)-(rE+I)-wN \tag{19}$$

$subject\, to$

$W = I + E$

$I \leq \bar{I}$

$e \leq f\left(W,N\right)$

We define two empirical variables directly observable in the data and which will allow us to derive the empirical predictions:

- α is external finance reliance, the share of a firm's finance that is not met by internal funds: $\alpha = 1 - \frac{I}{W} = \frac{E}{E+I}$. This is equivalent to the Rajan and Zingales empirical measure of external financial reliance.
- γ is the share of production reported for tax purposes: $\gamma = \frac{f(W,N)-e}{f(W,N)}$.

Given the above structure, we can characterize the optimal choice of internal and external funds, number of workers, and evasion. The representative firm makes optimal choices based on three first-order conditions, which are characterized by

$$\text{FOC wrt } e\text{: } t = c_e\left(e,E,N\right) \tag{20}$$

$$\text{FOC wrt } E\text{: } (1-t)f_W\left(W,N\right) = r + c_E\left(e,E,N\right) \tag{21}$$

$$\text{FOC wrt } N\text{: } (1-t)f_N\left(W,N\right) = w + c_N\left(e,E,N\right) \tag{22}$$

The reduced-form evasion-cost $c(e, E, N)$ contains the first central prediction of our study, namely that firms with more workers and with more external finance face higher tax enforcement.

There are a number of distinct ways to model these enforcement information trails. One possibility is to specify a form for $c(\cdot)$ as the expected monetary loss under a tax audit, such as

$$c\left(e, E, N\right) = p\left(E, N\right) \cdot \left(\theta e\right) \tag{23}$$

where $p\left(E, N\right)$ is the probability a firm will receive a tax inspection - assumed to be independent of evasion - and θ is the amount of evaded taxes that have to be paid in fines. Under this formulation, a firm which is perfectly compliant ($e = 0$) faces no compliance-cost, independently of how large and externally reliant it is. On the other hand, it may be that larger and more externally dependent firms face higher administrative compliance costs simply from the time spent dealing with tax inspectors, such that $c(0, E, N) > 0$. This could be modeled by introducing an firm administrative cost of compliance φ, such that

$$c\left(e, E, N; \varphi\right) = p\left(E, N\right) \cdot \left(\varphi + \theta e\right) \tag{24}$$

Finally, the amount evaded may also directly enter the probability of tax inspection, $p\left(e, E, N\right)$, so that evading is more costly both because detection is more likely and because fines are larger.

One policy that a large number of administrations have followed to manage enforcement of taxes on subsets of the tax-payer population is the establishment of special dedicated units, usually refereed to as Large Taxpayer Units (LTU hereafter). A firm's inclusion in the LTU implies an increase in the auditing probability and intensity, corresponding to a discrete upwards jump of $p(\cdot)$ in our model. As anecdotal policy-evidence to support modelling $p = p\left(N\right)$, firm size N seems to constitute one of the main criteria to assign a firm to the LTU. Pooled survey responses from administrative revenue authiorities in 67 countries[73] indicate that a threshold criteria in terms of number of employees is amongst the 6 most common criteria for allocating a firm to the country LTU.[74] In both Denmark and the United Kingdom a firm is assigned to the LTU once it employs in excess of 250 people; in Ghana, the threshold lies at 500, while in Sweden it is at 800; in Lithuania, allocation to the LTU is based on jointly meeting the criteria of 10+ employees and large sales revenue.

Other countries seem in practice to also target audits based on size. Goyette (2012) shows using firm-level data in Uganda that sales tax audits are effectively based on number of employees rather thant the official cut-off rule which is in terms of sales. In the Finnish setting, Harju et al. (2014) discuss how the desk audit inspection probability for VAT returns is increasing in size for labor intensive firms. Similarly information on finance is used to select audit cases. The

[73] Survey answers provided in OECD Tax Administration (2013), totalling 52 surveyed countries, and the International Tax Dialogue Information Series (2010), totalling 15 Sub-Saharan African countries.

[74] Other criteria cited include annual gross turnover; potential revenue contribution of the firm; size of assets; and significant international business activity.

World Bank Risk-based tax audits report discusses the case of Turkey which automatically cross checks VAT receipts with credit card information. (Khwaja et al., 2011)

3.2.2 Empirical predictions of the model

There are two sets of empirical testable implications that the model delivers. The first concerns the relationship between tax inspection probability and the firm information trails, E and N.

The industry-representative firm tax inspection probability is increasing in firm-size and the amount of external funds

$$\frac{\partial p\left(E,N\right)}{\partial N} \geq 0 \, \textit{and} \, \frac{\partial p\left(E,N\right)}{\partial E} \geq 0 \tag{25}$$

The positive derivative of the tax inspection probability with respect to the two information trails is motivated by the discussions in Section 3.2.1.

In the case where N and E are chosen as interior solutions (i.e. $N > \bar{N}$ and E> 0), the first-order conditions allow us to directly derive the second set of empirical predictions of our model. Supposing some form of underlying heterogeneity cause some firms to optimally choose higher levels of funding and size, this will impact the firm's compliance decision towards larger reliance on external financing, α, and towards an increase in the optimal size of the firm, N; these changes lead to higher tax-compliance, partly through increased tax inspection intensity. Formally

An exogenous increase in firm-demand for labor input causes sales tax compliance to increase in general

$$\frac{\partial \gamma}{\partial N} \geq 0 \tag{26}$$

and in particular through the channel of tax inspection

$$\frac{\partial \gamma}{\partial p\left(E,N\right)} \frac{\partial p\left(E,N\right)}{\partial N} \geq 0 \tag{27}$$

Similarly, the total impact of an exogenous increase in firm reliance on external fi-

nance is for sales tax compliance to increase

$$\frac{\partial\gamma}{\partial\alpha} \geq 0 \tag{28}$$

and a specific channel is through increased tax inspection

$$\frac{\partial\gamma}{\partial p\left(E,N\right)}\frac{\partial p\left(E,N\right)}{\partial E} \geq 0 \tag{29}$$

In our empirical setting we will try to isolate exogenous changes in demand for labor and external finance input which should drive sales tax inspection and sales tax compliance through the information trails and the change to the cost of evasion.[75]

In both empirical predictions, we simply assume that there exists heterogeneity at the industry-level causing variation in input demands for size and external funding. One way to model this is to suppose that there exists an industry-specific productivity-parameter θ_i, and that industries differ in the following sense

$$\theta_i > \theta_j \Rightarrow f_W\left(W,N;\theta_i\right) > f_W\left(W,N;\theta_j\right) \text{and} f_N\left(W,N;\theta_i\right) > f_N\left(W,N;\theta_j\right) \tag{30}$$

In the empirical exercise, we will treat differences in demand for labor and external reliance between two U.S. industries i and j as reflective of underlying technological differences.

3.3 Empirics: data and identification strategy

3.3.1 Data description and main variables

Throughout the analysis the unit of observation is a country-industry, where industries are classified using the ISIC 3 digit methodology, produced by the U.N. Statistics Division. The World Bank Enterprise Survey is the primary data

[75] In terms of our model, the underlying derivations for these predictions are

$$\frac{\partial\gamma}{\partial N} = \frac{\left(\frac{\partial f}{\partial N}e - \frac{\partial e}{\partial N}f\right)}{\left(f\right)^2} \geq 0 \text{and}$$

$$\frac{\partial\gamma}{\partial\alpha} = \frac{\frac{\partial\gamma}{\partial E}}{\frac{\partial\alpha}{\partial E}} = \frac{\left(\frac{\partial f}{\partial E}e - \frac{\partial e}{\partial E}f\right)}{\left(f\right)^2}\frac{\left(E+I\right)^2}{I} \geq 0$$

source: it is a firm level survey collected by the World bank between 2002 and 2010 in more than a hundred countries. To our knowledge this is the only standardized firm data set spanning countries across the development spectrum. Since the survey was clearly administered by a third party not related to the government or the statistical office it could ask unique questions on tax behavior and corruption.

We rely on two data sources to construct our instrumental variables: the Compustat database of publicly listed firms in the U.S. and the U.S. Bureau of Labour Statistics Quarterly Census of Employment and Wages. The final sample is 2597 country-ISIC3 observations from 77 countries.[76]

To operationalise a measure of tax enforcement, we use the firm's reported answer to the question: "Total days spent with officials from: tax inspectorate." In particular we constructed a tax inspection dummy equals to one if the firm has been visited for at least one day by tax auditors. This proxies for the extensive margin of tax inspection. The industry-average tax inspection probability is .71, with a standard deviation of .35. This is the measure of tax enforcement which we used to produce Figs.38-39 in the introduction. In , we also report the results from the 'intensive margin' measure of total number of days. These results using the 'intensive margin' number of days visited by tax inspectors are discussed in Section 3.4.3.[77]

In a second stage of the analysis we use the WB survey question on sales tax evasion: "Recognizing the difficulties many enterprises face in fully complying with taxes and regulations, what percentage of total sales would you estimate the typical establishment in your area of activity reports for tax purposes?" There are obvious difficulties in assuming that the firm's answer to this question reflects its own industry tax evasion and even harder that it is the firm's own evasion level, but since our theory suggests that information trails ultimately impact tax compliance, we also use this measure of sales tax compliance (in Section 3.3.4) The average share of sales reported for tax purpose is 84.21% with a standard deviation of 20.79%.

To construct a measure of external reliance, we follow the methodology presented in Rajan and Zingales (1998). The measure of external dependence proxies for the amount of investment that is not be financed through internal sources such as retained earnings. For the U.S. values of this measure, we use Compus-

[76]For details on construction of the dataset, please see Appendix.

[77]Regression results from using the continuous number of days measures are in Appendix Table 30.

tat over the years 2000-2012.[78] In the World Bank sample, firms' balance sheets give information on the shares of financing from different sources of earnings. The measure of external reliance is defined as the ratio of external sources' share over the sum of external and internal shares:

$$share\,ext_i = \frac{external_i}{external_i + internal_i}$$

Internal finance is the sum of retained earnings, friends and family and informal finance. External finance is defined as banking finance which incudes loans, overdrafts and credit card finance.[79] . In the Appendix we provide some summary statistics on the different sources of financing. Retained earnings is the dominant method of financing. A compelling feature of our data is the possibility to construct the same measure of external reliance in both the World Bank and the Compustat sample

We choose as number of workers the Enterprise Survey firm's reported average number of permanent workers. Workers which are permanent employees are more likely to have access to the internal books of the firm and thus are more able than temporary employees to 'blow the whistle' and report tax underreporting. The sample-wide distribution of firms has a long right tail: while the median is at 17 workers, the mean of 105 is largely influenced by a few very large firms. Given the shape of the marginal density, we always use the natural log of firm size in the regressions. U.S. data is taken from the 2002 Census of Employment and Wages.

Figure 40 uses the pooled Enterprise Survey firm-level observations to plot the density distributions of firm-size in tercils of the external financial reliance distribution: zero, low, and high. The size-distribution appears increasingly right skewed as external funding reliance decreases which could suggest nonzero cross-derivatives of the production function and of the evasion-cost function with respect to inputs N and E. Though less clear, this 'interaction' between the inputs is also reflected in the density distributions of external reliance across terciles of the firm-size distribution, plotted in Figure 41. The possibility of interactions in the production and evasion functions highlights the importance of estimating jointly the two first stages to predict exogeous variation in firm-size and external reliance, as detailed in the following section.

[78] Detailed description of this ratio is in Appendix.

[79] We believe that these sources all generate an information trail in the case of external funding, and are non-visible in the case of internal funds. The main findings are robust to only including retained earnings as internal funding source and local bank access as external funding source.

3.3.2 Identification strategy

A key identification issue is that a firm's choice of workers and finance in our sample of surveyed countries could be distorted by the information trails. Our identification strategy relies on out-of-sample extrapolation techniques at the industry-level: if firms across countries, but within the same industry, share a common underlying technological demand for inputs, then we can use the demand for workers and finance of firms in a relatively undistorted market, like the US, as instruments for firms' demand for workers and external funding in our sample. This idea was developed by Rajan and Zingales (1998), who studied whether sectors reliant on external finance grew faster in countries with more developed financial institutions.

The between-industry regressions thus rely on two assumptions for identification. First, the US labor and financial markets must represent a benchmark, where firm-choices of workers and external funding are undistorted, in particular with respect to the information-trail enforcement channels. Rajan and Zingales (1998) provide a series of arguments to support this assumption, which ultimately remains hard to test.

The second assumption is that there exist an industry-specific technological demand for external finance and workers. In a regression with country fixed effects, the identifying assumption holds if the following type of statement is true: "If Drugs and Pharmaceuticals require more external funding than Tobacco in the US, then this rank-condition also holds in Bangladesh." Figure 42 provides some evidence to support this assumption. For four selected countries from the World Bank sample across all levels of per capita income (Burkina Faso, Argentina, Slovenia, Germany), the graph plots the firm-size rank of a given industry in the U.S. sample against the industry's rank in the surveyed country sample. In each country, the upward-slope is clear with a linear trend, although there are some outlier industries. The identifying assumption is equivalent to a significant upward-slope being empirically verified in the set of surveyed countries.

We estimate jointly the two first-stages

$$(\text{Firm Size Rank})_{i,c,t} = \gamma\,(\text{Firm Size Rank})_{i-US,c,t} + \beta\,(\text{External Reliance Rank})_{i-US,c,t} + \delta_c + \sigma_t + \varepsilon_{ict} \tag{31}$$

$$(\text{External Reliance Rank})_{i,c,t} = \omega\,(\text{External Reliance Rank})_{i-US,c,t} + \pi\,(\text{Firm Size Rank})_{i-US,c,t} + \iota_c + \tau_t + \tag{32}$$

where e.g. $(\text{Firm Size Rank})_{i,c,t}$ is the firm-size rank of ISIC3 industry i in country c in year t, $(\text{Firm Size Rank})_{i-US,c,t}$ is the US-rank of the same industry, and δ_c and σ_t are country and year fixed effects. Significant $\gamma > 0$ and $\omega > 0$ coefficients provide support for the assumption of country-invariant but industry-dependent demand functions for labor and finance inputs, with coefficients equal to 1 implying perfect rank-ordering of country c's industries relative to U.S. industries. To the best of our knowledge, this study is the first to provide direct evidence on the assumption underlying the RZ study and the ensuing literature in finance and development which builds on RZ results.

In a second stage, we study the impact of demand for labor and external finance on the probability of tax enforcement. That is, we estimate

$$1\left(\text{TaxInspection}\right)_{ict} = \beta \cdot \left(\text{Firm Size Rank}\right)_{ict} + \alpha \cdot \left(\text{External Reliance Rank}\right)_{ict} + \vartheta_c + \chi_t + \varepsilon_{ic} \tag{33}$$

where the rank of firm size and external reliance may be replaced by their predicted values from the first stage.

Using the rank of firm size as opposed to actual firm size values to predict tax enforcement is motivated by the idea that in the absence of administrative and information constraints within a country, tax authorities will effectively target enforcement and auditing towards industry-firms in descending order of these industries' reliance on external finance and labor inputs. Thus actual firm-size and external finance only matter to the extent that they determine an industry's relative location in terms of the country-wide distributions of labor input and external dependency. A rank-rank regression also provides a more direct test of the identification assumption than a regression using actual values.[80]

3.4 Regression results

In this section we present the impact of information trails on the probability of tax inspection and on sales tax compliance, both in reduced form and in a double-IV strategy. We also provide robustness checks for the main results.

3.4.1 Main results

Table 26 presents the results of information trails on tax inspection. Column 1 presents the linear regression of tax inspection probability on the surveyed country's industry-rank in terms of workers and finance.[81] The results show that while the rank of firm-size positively predicts the probability of tax inspection, there is no effect for the external reliance channel. The joint explanatory power of the two trails in determining tax enforcement is strong (F-test equal to 16.07).

[80] In Appendix Tables 33-34, we show that using actual industry-average values of size and external reliance yield very similar results.

[81] All standard errors are clustered at the country level, to account for country-wide correlation. We use as analytical weights the ratio of country-industry number of firm observations to the total number of firm observations in the raw Enterprise Survey sample; country-industry averages which reflect a larger underlying number of firms, and thus are likely to be more important and more accurate, receive higher weight in the regression.

The non-impact of external reliance may arise if the true relationship between external funding and tax inspection is highly non-linear, while our OLS assumes a linear relationship. To get at this, Figure 43 illustrates graphically the regression result of Col.1 of Table 26 using binned scatter plots. To construct the left (right) plot, we first residualize the tax-inspection variable and the firm-size rank (external reliance rank) variable with respect to external reliance rank (firm-size rank) and country fixed effects. We then divide the residual firm-size rank (external reliance rank) into twenty equal-sized groups (vingtiles) and plot the means of the tax-inspection residual within each bin against the mean value of residual firm-size rank (external reliance rank). The solid line shows the best linear fit estimated on the underlying microdate using OLS and corresponds to the regression coefficient for firm-size (external reliance) in Col.1 Table 26.[82] The scatter-plot provides no evidence of a non-linear underlying relationship, and the flat linear OLS provides a good fit. The linear OLS also seems to fit well the size-tax inspection scatter plot. This linear relationship substantiates our choice of modelling the inspection dummy outcome as a linear probability model over size and external finance inputs.

In column 2, we present the reduced form results based on the extrapolation technique where the US-industry ranks of firm size and external reliance are used to directly predict cross-industry variation in tax enforcement in the WBES country. The coefficient on firm size is small but highly significant at the 1% level; a one standard deviation increase in the US firm-size industry-ranking leads to a 2.38 percentage points increase in the probability of tax inspection for that industry in the surveyed country. However, the coefficient on external reliance again fails to explain tax inspection.

In columns 3 and 4, we study the identifying assumption that industry-differences in US-ranks of firm size and external reliance predict differences in the industry-ranks of the World Bank countries. Column 3 shows that a one-rank increase in the US-ladder of a given industry predicts a 0.24 rank increase in the industry-ladder of the WB surveyed country, significant at 1%. The U.S. industry of firm-size rank also seems to positively predict external reliance in the World Bank country industry-rank, again significant at 1%. The US-industry rank in external dependency explains positively, albeit with a small coefficient, firm-size, and even negatively external reliance; this last result however, is no longer as significant when controls are added. The joint F-test on the U.S. values in predicting cross-industry ranking in firm size and external reliance remain large, respectively at 43.34 and 10.13, implying a strong first stage.

We estimate the joint instrumental variables model in column 5. The firm-size information trail is again significant at 1% while the external reliance channel is not significant, despite a first stage F-test in excess of 10. This estimate suggests

[82] The same methodology is used in Chetty et al. (2011)

that a one standard deviation in the surveyed country size-ranking caused by exogenous industry-differences in size and external finance leads to an 8.34 percentage points industry average tax auditing probability, or a 12% increase at a sample mean tax inspection probability of 71%.

In Appendix Tables 28-29, we report the results for Tables 26-27 when controlling for age, legal status (a dummy for sole proprietorship), exporter status, and reported difficulty in accessing finance.[83] The first stage estimates continue to be very strong (1st stage F-statistics of 26.54 (9.10) when the outcome variable is WB rank of worker (external reliance). The reduced form results continue to suggest a strong and positive association between firm-size and tax inspection, and a nill relation with external finance. There is a small loss in significance in the double-IV coefficients, but the qualitative patterns remain.

3.4.2 Extension to sales tax compliance

As an extension, we study the impact of the information trails on sales tax compliance, both directly and through tax inspection. The industry-average measure of sales tax compliance comes with the caveats discussed in Section 3.3; on the other hand, it should be noted that studies of corruption and informality (Svensson, 2003; Almeida and Carneiro, 2012) have used the same survey formulation of the question and interpreted the values as the true measure of own firm behavior.

The results from this extension are presented in Table 27.[84] In column 1, the simple OLS specification shows that within country industry-ranked firm-size significantly predicts sales tax compliance, while external reliance does not. Again, the non-significance on the external finance trail may arise if the true relationship is highly non-linear. Constructed similarly to Figure 43, in Figure 44 we present the graphical results of the regression in Column 1. The external reliance - tax compliance relationship seems to be well approximated by a linear OLS, alleviating concerns of mis-specification in the econometric model.

In the reduced-form specification of column 2, US-industry rank differences in firm size are found to have a significant reduced-form impact on sales tax compliance, while there is no pattern across external reliance ranks. In Column 3, which uses a double IV estimation, a one standard deviation increase in the surveyed country's size-industry rank, caused by exogenous industry-differences in demand for size and finance, leads to a 4.22 percentage points increase in sales

[83] We do not include the log of sales for several reasons: first, it is likely to be endogenous to our setting; second, log workers and external finance are arguably amongst the primitives which determine sales volume; third, total sales is not reported for a large set of firms which do disclose worker size and external reliance.

[84] In the firm-level survey data, there are 3330 ISIC3-matched observations which do not report an answer to the sales tax compliance question; this leads to a loss of 88 ISIC3-country observations. Average tax inspection in the two groups are not significantly different at the 5% level.

tax compliance, significant at 5%.

3.4.3 Robustness checks

As a continuous measure of tax enforcement, this intensive margin variable of the firm's reported number of days spent with tax inspectors provides a robustness check to the tax inspection and compliance results. The mean number of days spent with tax inspectors is at 2.23, with a standard deviation of 2.27.

The results carry over using this intensive margin variable.[85] Full results are reported in Appendix Table 30. The double-IV estimates show that from exogenous changes to technological demand for labour and finance input, increased firm-size leads to an increase of 1.18 tax inspection days, significant at 5%.

It may be the case that the US industries of the 1990's are better proxies for the current position of developing countries' industries in their finance life-cycles. If so, then using older samples of industry-variation of U.S. firm-financing pattern may reveal a more significant impact of external reliance on tax inspection. We therefore re-compute our measure of ISIC-3 external reliance, but using instead the 1990-1999 sample of Compustat US listed firms.

Results are reported in the Appendix Tables 31-32. Both the results on the significant impact of the firm-size information trail both on tax enforcement and tax inspection and on the non-impact of external reliance hold.

Appendix Tables 33-34 provide results from specifications which measure firm size (external reliance) using logs (percent share) instead of rankings. The first stage estimates continue to be very strong, with first stage F-statistic of 37.90 (9.75) in the case of log WB firm size (share WB external reliance). The reduced form results also carry over to this alternative specification. The main difference lies in a drop in significance in the double-IV specification (although the coefficients remain sizeable, and in line with the rank-based specification).

[85] In this double instrumental variables model, the second stage is now

$$1\ (\#\ \text{Days spent with tax inspectors})_{ic} = \beta \cdot (\text{Firm Size Rank})_{ic} + \alpha \cdot (\text{External Reliance Rank})_{ic} + \vartheta_c + \varepsilon_{ic} \tag{34}$$

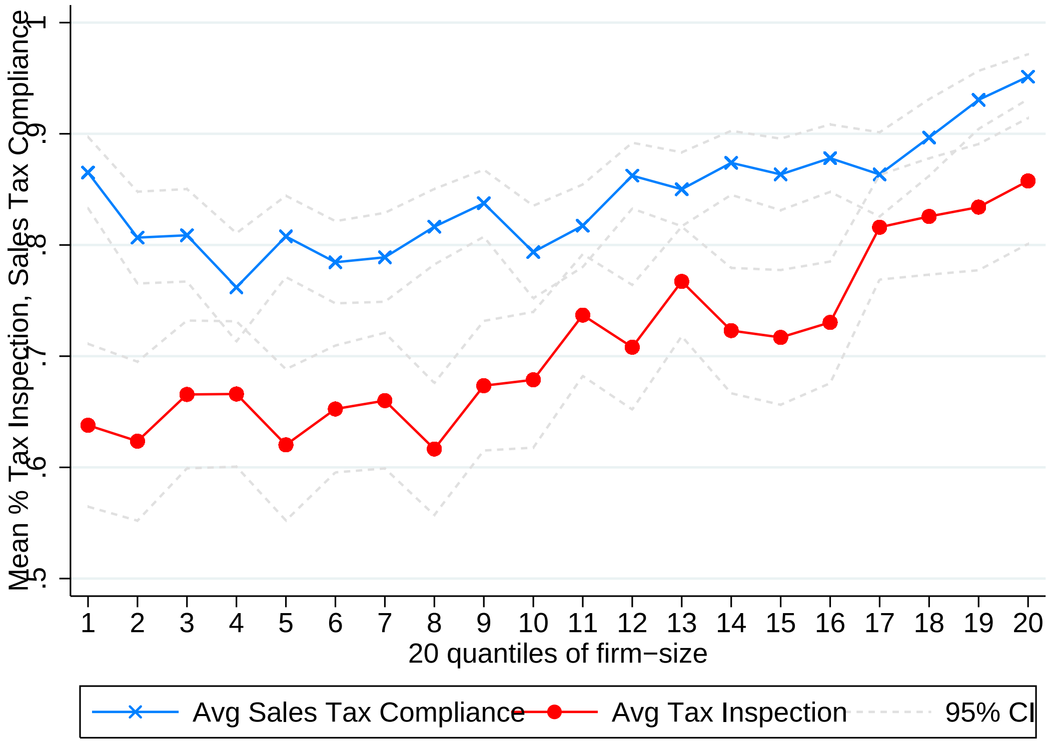
3.5 Conclusion

Due to the rising internal demand for public goods and transfers, increasing tax revenue has become a priority for developing countries. Taxing firms is particularly important, not only because firms directly collect an important share of revenue through sales and profit taxes, but also due to their role in monitoring employees and transactions in the economy. The theoretical literature (Gordon and Li 2009, Kleven, Kreiner and Saez 2009) has focused on firms and suggested two important information channels: the use of finance and the number of workers. As a firm grows in size and uses external sources of finance it generates trails about its operations and becomes more visible to the tax authorities. We construct a simple model of tax evasion and tax enforcement which provides the reduced-form predictions that tax enforcement and tax compliance should increase for these firms which are larger in employee-size and more externally reliant.

We test the importance of the finance and worker channels by using 80 country-surveys from the World Bank Enterprise Survey, covering 108,500 firms, with data on size, source of funding, tax inspection and tax compliance. Identification relies on within-country between-industry variation, which uses the U.S. distribution of firm size and external finance as an instrument for undistorted and exogenous variation in technological demand for workers and finance (a technique similar to Rajan and Zingales, 1998). We find that a larger firm-size affects significantly the probability of tax inspection both in the OLS and IV specifications: a one standard deviation exogenous increase in the industry size-ranking leads to a 8.34 percentage points higher industry tax auditing probability. External finance does not seem to have an impact on tax inspection. In a second part we study the impact of information trails on sales tax compliance: here, we also find a large effect of size , but no effect of extenal financial dependence.

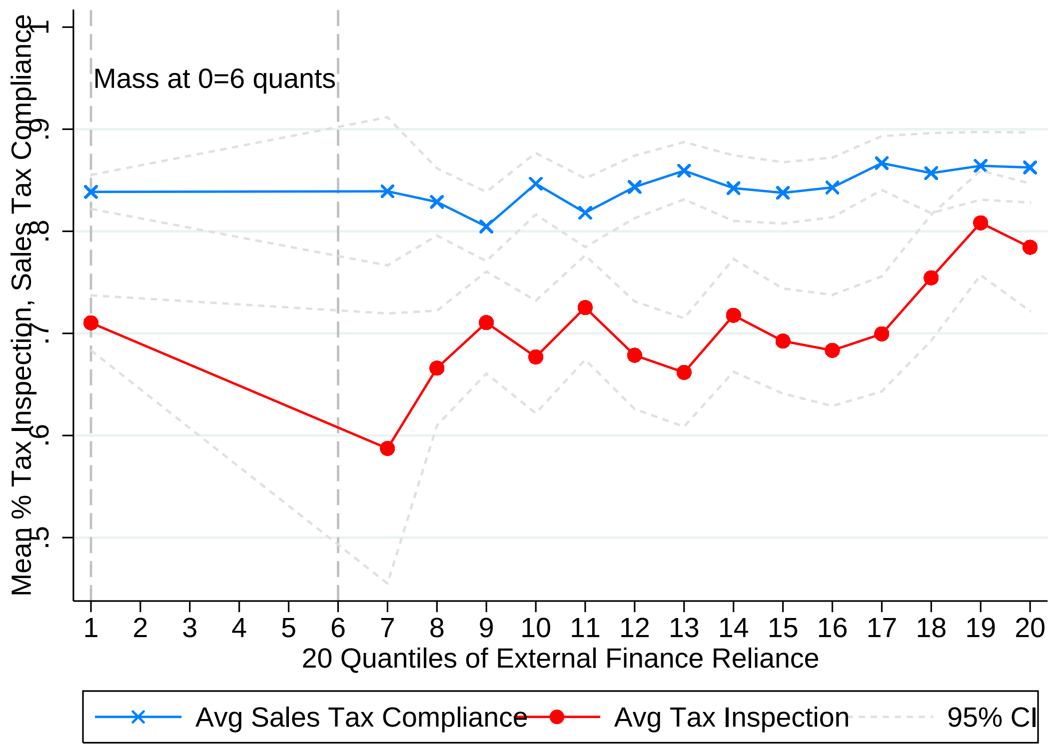
The non-result regarding external funding reliance is interesting in its own right, and could suggest tax administrations do not leverage financial information. One reason for doing so may be that the induced misallocation from levying the finance information trail is prohibitively large. Another is that political economy factors prevent tax administrations from systematically accessing this information. A final possibility is that tax authorities of developing countries have not yet made the investments in tax capacity to efficiently manage financial information. Untangling these different factors is an area of interest for future research.

FIGURE 38: TAX INSPECTION AND SALES TAX COMPLIANCE ACROSS FIRM-SIZE



This graph plots averages of sales tax compliance (X-series) and tax inspection (circle-series) across 20 quantiles of firm size. The quantiles are constructed based on the pooled sample of country-ISIC3 industry observations in the World Bank Enterprise database (2,597 observations). Firm size is measured by the reported number of permanent employees in the firm. Tax inspection is the dummy indicator for whether the firm had a tax inspection visit over the last year. Sales tax compliance is the share of sales that a firm estimates its competitors report for tax purposes. See section 3.3.1 for further details.

FIGURE 39: TAX INSPECTION AND SALES TAX COMPLIANCE ACROSS EXTERNAL FINANCIAL RELIANCE

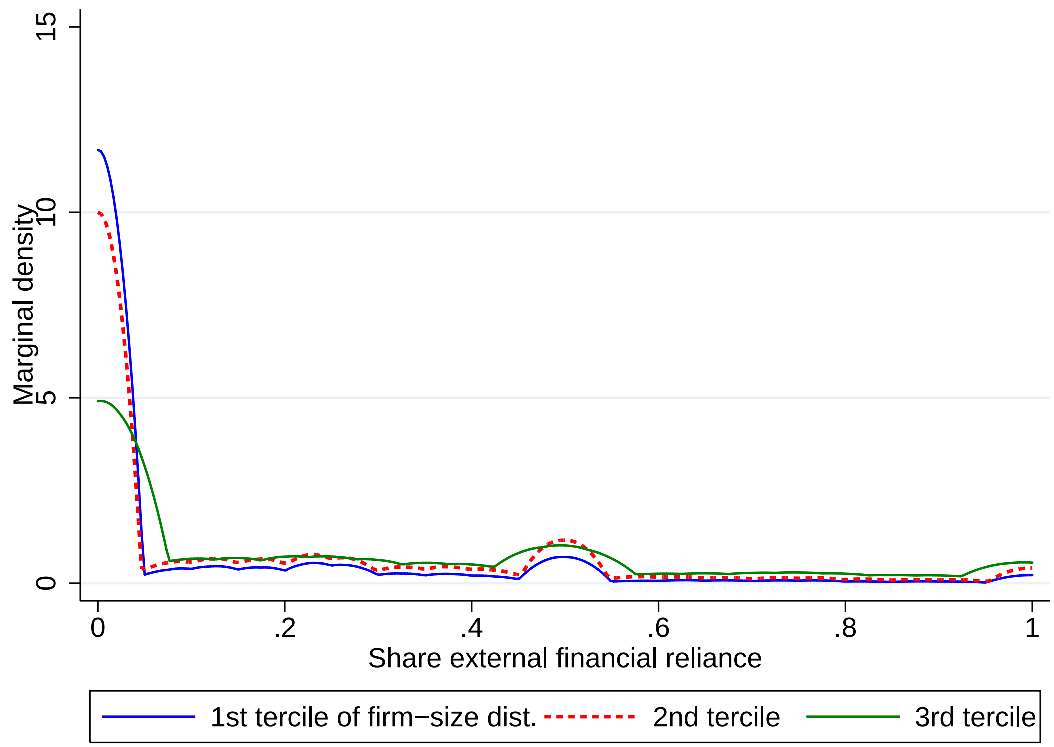


This graph plots averages of sales tax compliance (X-series) and tax inspection (circle-series) across 20 quantiles of share of external financial reliance. The quantiles are constructed based on the pooled sample of country-ISIC3 industry observations in the World Bank Enterprise database (2,597 observations). External reliance is measured as the percent share of financing which is sourced in retained earnings, family and friends and informal finance. Firms with zero external reliance represent 34% of the total number of firms. Tax inspection is the dummy indicator for whether the firm had a tax inspection visit over the last year. Sales tax compliance is the share of sales that a firm estimates its competitors report for tax purposes. See section 3.3.1 for details.

FIGURE 40: DENSITY DISTRIBUTIONS OF FIRM-SIZE CONDITIONAL ON EXTERNAL FINANCIAL RELIANCE

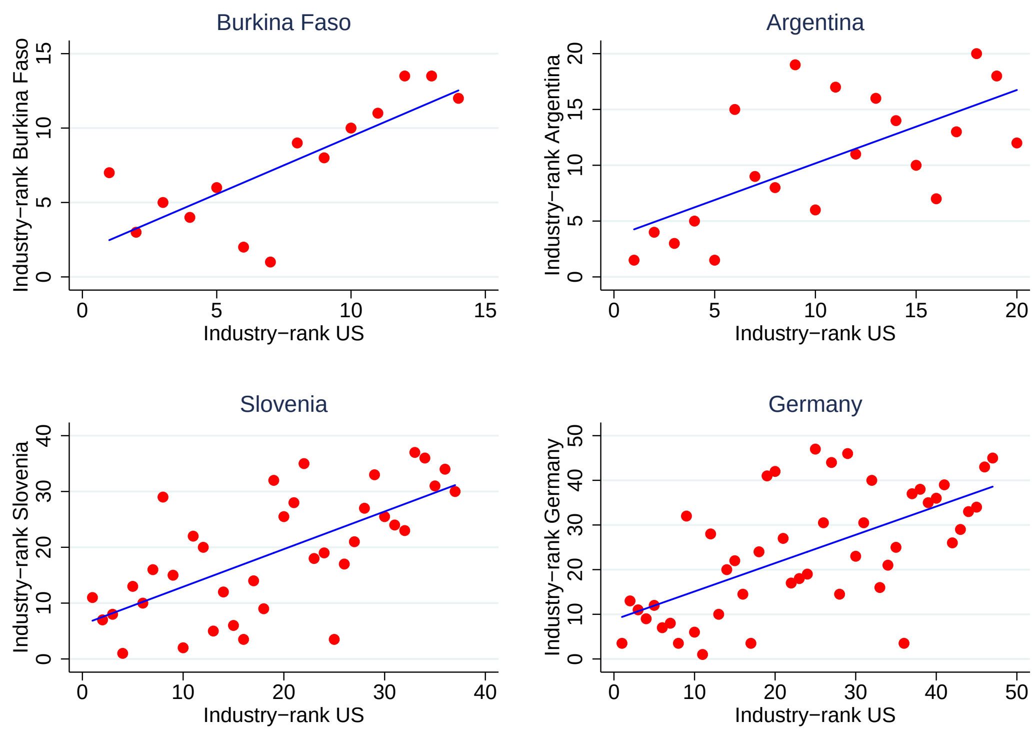
This graph superposes firm-size kernel density distributions where sample-restriction is based on external financial reliance at the firm-level. Respectively the blue, red dashed, and green lines depict the firm-size density distribution for firms in the first, second, and third terciles of the external reliance distribution. The first group has zero percent external reliance, the second external reliance strictly positive but below 34%, and the third group has external reliance greater than 34%. External reliance is measured as the percent share of financing which is sourced in retained earnings, family and friends and informal finance. Total firm-level sample-size: 108,538.

Figure 41: Density distributions of external financial reliance conditional on firm-size



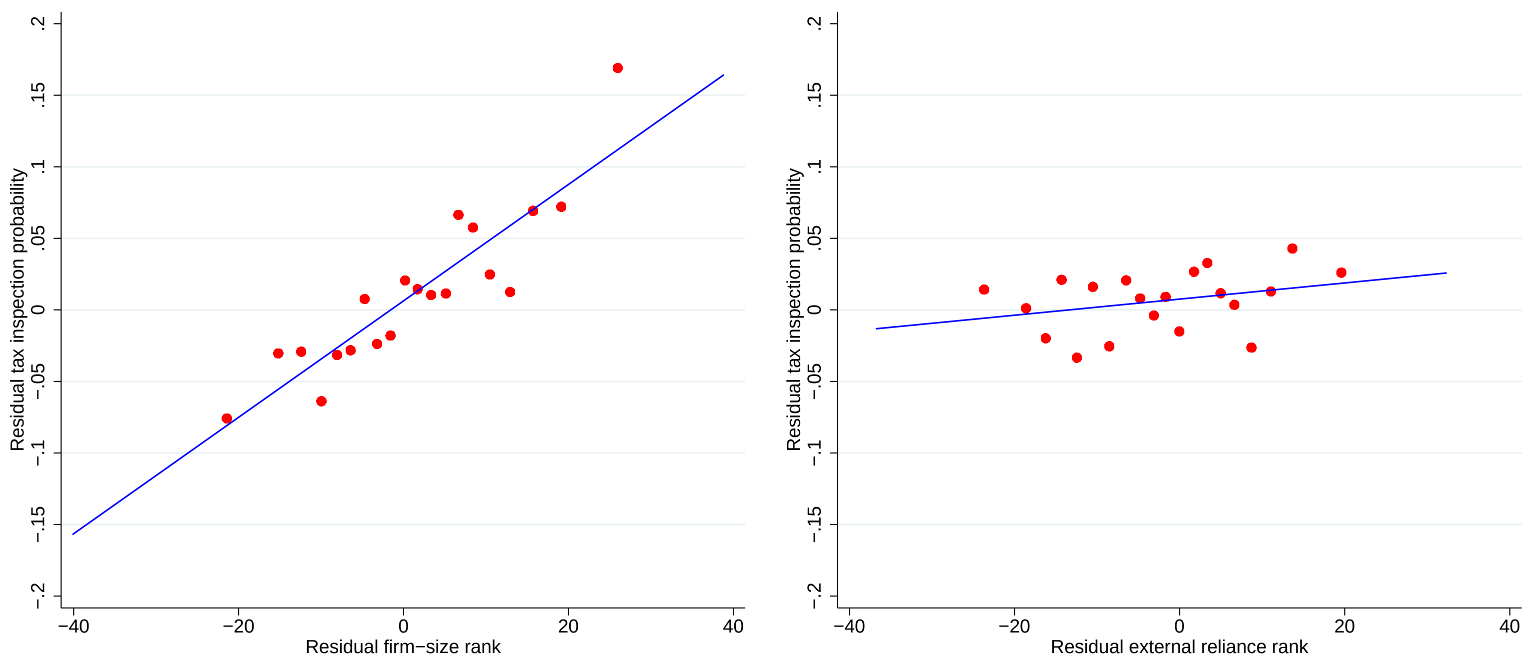
This graph superposes external financial reliance kernel density distributions where sample-restriction is based on the number of permanent employees at the firm-level. Respectively the blue, red dashed, and green lines depict the external reliance density distribution in the first, second and third tercile of the firm-size distribution. External reliance is measured as the percent share of financing which is sourced in retained earnings, family and friends and informal finance. Total firm-level sample-size: 108,538.

Figure 42: Firm-size rank-rank relationships: U.S. industries against 4 selected WB survey countries



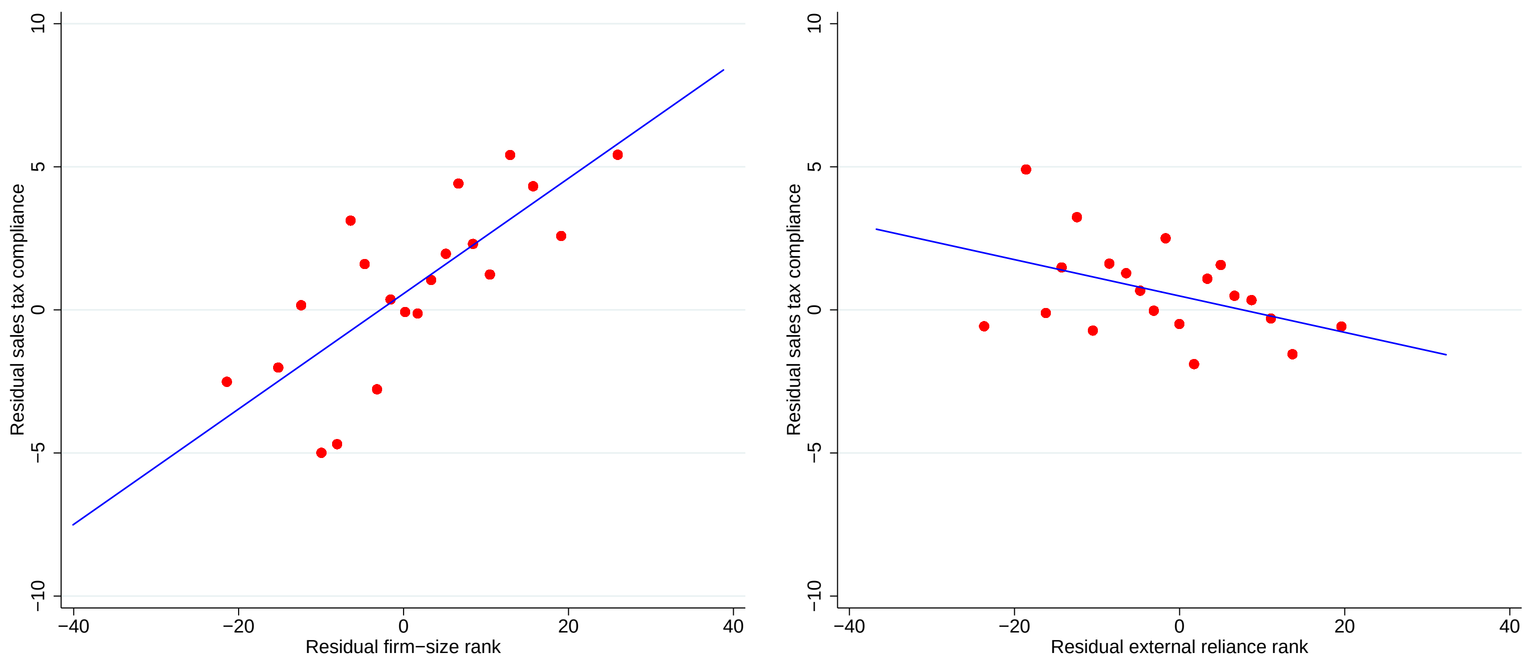
This set of graphs plots the rank-rank relation for firm-size in 4 selected WB surveyed countries: Burkina Faso, Argentina; Slovenia; and Germany. Each observation represents the ranking-position of a given ISIC-3 industry, in the WB country industry-rank and in the U.S. industry-rank. Blue lines are linear fits. See Section 3.4.2 for details.

FIGURE 43: IMPACT OF FIRM-SIZE RANK AND EXTERNAL FINANCE RANK ON TAX INSPECTION



The right and left panels are binned scatter plots of tax inspection versus respectively firm-size rank and external financial reliance rank. These plots correspond to regression Col.1 of Table 26 and use the same sample and variable definitions. To construct the left (right) plot, we first residualize the tax-inspection variable and the firm-size rank (external reliance rank) variable with respect to external reliance rank (firm-size rank) and country fixed effects. We then divide the residual firm-size rank (external reliance rank) into twenty equal-sized groups (vingtiles) and plot the means of the tax-inspection residual within each bin against the mean value of residual firm-size rank (external reliance rank). The solid line shows the best linear fit estimated on the underlying microdate using OLS and corresponds to the regression coefficient for firm-size (external reliance) in Col.1 Table 26.

Figure 44: Impact of firm-size rank and external finance rank on sales tax compliance



The right and left panels are binned scatter plots of sales tax compliance versus respectively firm-size rank and external financial reliance rank. These plots correspond to regression Col.1 of Table 27 and use the same sample and variable definitions. To construct the left (right) plot, we first residualize the sales tax compliance variable and the firm-size rank (external reliance rank) variable with respect to external reliance rank (firm-size rank) and country fixed effects. We then divide the residual firm-size rank (external reliance rank) into twenty equal-sized groups (vingtiles) and plot the means of the sales tax complianxe residual within each bin against the mean value of residual firm-size rank (external reliance rank). The solid line shows the best linear fit estimated on the underlying microdate using OLS, and corresponds to the regression coefficient for firm-size (external reliance) in Col.1 Table 27.

TABLE 26: TAX INSPECTION RESULTS

	Reduced Forms		First Stages		IV
	Tax Inspection		Rank WB Worker	Rank WB ExtRel	Tax Inspection
	OLS	OLS	OLS	OLS	IV
	(1)	(2)	(3)	(4)	(5)
Rank WB Worker	.003				.006
	(.000) ***				(.002)***
Rank WB External Reliance	.000				.003
	(.000)				(.006)
Rank US Worker		.002	.248	.085	
		(.000)***	(.033)***	(.021) ***	
Rank US External Reliance		.000	.075	-.039	
		(.000)	(.035)**	(.019)**	
F test joint significance	16.07	8.02	43.43	10.13	8.08
(p-value)	(.000)	(.000)	(.000)	(.000)	(.000)
Observations	2597	2597	2597	2597	2597
Clusters	78	78	78	78	78

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions include country and year fixed effects

Standard errors clustered at the country level.

Column (5) is a double IV which uses Cols. (3)-(4) as first stages

TABLE 27: SALES TAX COMPLIANCE RESULTS

	Sales Tax Compliance (Percent)		
	OLS	OLS	IV
	(1)	(2)	(3)
Rank WB Worker	.202		.317
	(.034)***		(.145)**
Rank WB Ext Rel	-.031		-.032
	(.027)		(.300)
Rank US Worker		.076	
		(.026)***	
Rank US Ext Rel		.025	
		(.028)	
F test joint significance	17.28	4.64	4.62
(p-value)	(.000)	(.012)	(.012)
Observations	2509	2509	2509
Clusters	77	77	77

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions include country and year fixed effects
Standard errors clustered at the country level
Column (3) is a double IV which uses Cols. (3)-(4) Table 26 as first stages

3.6 Appendix

TABLE 28: TAX INSPECTION RESULTS WITH CONTROLS

	Reduced Forms		First Stages		Double IV
	Tax Inspection		Rank WB Worker	Rank WB ExtRel	Tax Inspection
	OLS	OLS	OLS	OLS	IV
	(1)	(2)	(3)	(4)	(5)
Rank WB Worker	.003				.007
	(.000)***				(.004)*
Rank WB External Reliance	.000				.001
	(.000)				(.007)
Rank US Worker		.001	.166	.089	
		(.000)***	(.028)***	(.022)***	
Rank US External Reliance		.000	.049	-.037	
		(.000)	(.028)*	(.020)*	
F test joint significance	10.34	4.35	26.54	9.10	4.01
(p-value)	(.000)	(.016)	(.000)	(.000)	(.022)
Controls included	Age, legal status, exporter status, reported financial constraints				
Observations	2532	2532	2532	2532	2532
Clusters	77	77	77	77	77

Standard errors clustered at the country level. All regressions include country and year fixed effects

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 29: SALES TAX COMPLIANCE RESULTS WITH CONTROLS

	Sales Tax Compliance (Percent)		
	OLS	OLS	IV
	(1)	(2)	(3)
Rank WB Worker	.161		.291
	(.042)***		(.221)
Rank WB Ext Rel	-.019		.072
	(.028)		(.369)
Rank US Worker		.056	
		(.025)**	
Rank US Ext Rel		.010	
		(.026)	
F test joint significance	7.52	2.42	2.61
(p-value)	(.001)	(.095)	(.080)
Controls included	Age, legal status, exporter status, reported financial constraints		
Observations	2449	2449	2449
Clusters	76	76	76

Standard errors clustered at the country level. All regressions include country and year fixed effects

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 30: TAX INSPECTION NR. OF DAYS

	Reduced Forms		First Stages		Double IV
	Tax Inspection nr. Days		Rank WB Worker	Rank WB ExtRel	Tax Inspection nr. days
	OLS	OLS	OLS	OLS	IV
	(1)	(2)	(3)	(4)	(5)
Rank WB Worker	.047				.089
	(.009)***				(.037)**
Rank WB External Reliance	.010				.135
	(.009)				(.127)
Rank US Worker		.033	.248	.085	
		(.007)***	(.033)***	(.021)***	
Rank US External Reliance		.001	.075	-.039	
		(.005)	(.035)**	(.019)**	
F test joint significance	15.05	10.02	43.34	10.13	11.27
	(.000)	(.016)	(.000)	(.000)	(.022)
Observations	2597	2597	2597	2597	2597
Clusters	78	78	78	78	78

Standard errors clustered at the country level. All regressions include country and year fixed effects

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 31: TAX INSPECTION RESULTS USING ALTERNATIVE SAMPLE FOR US EXTERNAL RELIANCE

	Reduced Forms		First Stages		Double IV
	Tax Inspection		Rank WB Worker	Rank WB ExtRel	Tax Inspection
	OLS	OLS	OLS	OLS	IV
	(1)	(2)	(3)	(4)	(5)
Rank WB Worker	.003				.005
	(.000) ***				(.002)*
Rank WB External Reliance	.000				.005
	(.000)				(.010)
Rank US Worker		.001	.274	.078	
		(.000)***	(.031)***	(.022) ***	
Rank US Ext Rel 1990-99		.000	.132	-.010	
		(.000)	(.031)***	(.031)	
F test joint significance	16.07	11.60	44.98	7.45	9.47
	(.000)	(.000)	(.000)	(.001)	(.000)
Observations	2597	2597	2597	2597	2597
Clusters	78	78	78	78	78

Standard errors clustered at the country level. All regressions include country and year fixed effects

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 32: SALES TAX COMPLIANCE RESULTS USING ALTERNATIVE SAMPLE FOR US EXTERNAL RELIANCE

	Sales Tax Compliance (Percent)		
	OLS	OLS	IV
	(1)	(2)	(3)
Rank WB Worker	.202		.119
	(.034)***		(.177)
Rank WB Ext Rel	-.031		.603
	(.027)		(.694)
Rank US Worker		.081	
		(.026)***	
Rank US Ext Rel 1990-99		.011	
		(.022)	
F test joint significance	17.28	4.71	5.31
(p-value)	(.000)	(.011)	(.006)
Observations	2509	2509	2509
Clusters	77	77	77

Standard errors clustered at the country level. All regressions include country and year fixed effects
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 33: TAX INSPECTION RESULTS WITH NON-RANK MEASURES OF FIRM SIZE AND EXTERNAL RELIANCE

	Reduced Forms		First Stages		Double IV
	Tax Inspection		Lg Firm Size WB	Share Ext Rel WB	Tax Inspection
	OLS	OLS	OLS	OLS	IV
	(1)	(2)	(3)	(4)	(5)
Lg Firm Size WB	.040				.172
	(.006) ***				(.112)
Share Ext Rel WB	.018				-.781
	(.017)				(1.04)
Lg Firm Size US		.025	.303	.034	
		(.007)***	(.039)***	(.007) ***	
Share Ext Rel US		.006	.021	-.003	
		(.003)	(.015)	(.005)	
F test joint significance	19.23	8.82	37.90	9.75	3.93
	(.000)	(.000)	(.000)	(.000)	(.023)
Observations	2597	2597	2597	2597	2597
Clusters	78	78	78	78	78

Standard errors clustered at the country level. All regressions include country and year fixed effects

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 34: SALES TAX COMPLIANCE RESULTS WITH ALTERNATIVE MEASURES OF FIRM SIZE AND EXTERNAL RELIANCE

	Sales Tax Compliance (Percent)		
	OLS	OLS	IV
	(1)	(2)	(3)
Lg Firm Size WB	2.415		.527
	(.371)***		(.423)
Share Ext Rel WB	-.367		-93.43
	(.902)		(88.38)
Lg Firm Size US		.753	
		(.483)***	
Share Ext Rel US		.677	
		(.207)***	
F test joint significance	21.97	6.82	1.04
(p-value)	(.000)	(.001)	(.359)
Observations	2509	2509	2509
Clusters	77	77	77

Standard errors clustered at the country level. All regressions include country and year fixed effects
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Throughout the analysis the unit of observation is a country-industry, where industries are classified using the ISIC 3 digit methodology, produced by the U.N. Statistics Division. The ISIC3 methodology classifies distinct and refined groups; per example, 'Manufacture of dairy products' is classified as ISIC152, while 'Manufacture of grain mill products, starches and starch products, and prepared animal feed' is classified as ISIC153.

The World Bank Enterprise Survey is the primary data source: this is a firm level survey collected by the World bank between 2002 and 2010 in more than a hundred countries. To our knowledge this is the only standardized firm data set spanning countries across the development spectrum. Since the survey was clearly administered by a third party not related to the government or the statistical office it could ask unique questions on tax behavior and corruption.

We also rely on two other data sources to construct our instrumental variables: the Compustat database of firms listed in the stock exchange in the US and the U.S. Bureau of Labour Statistics Quarterly Census of Employment and Wages. We mapped the U.S. industry coding, NAICS, into the ISIC classification using the appropriate U.N. tables and merged U.S. industry-averages with the industry averages from the Enterprise Survey based on common ISIC3 codes. The final sample is 2597 country-ISIC3 observations from 77 countries.

We use the firm's reported answer to the question: "Total days spent with officials from: tax inspectorate." In particular we construct a tax inspection dummy equal to one if the firm has been visited for at least one day by tax auditors. The industry-average tax inspection probability is .71, with a standard deviation of .35. This is the main measure of tax enforcement that we use throughout the main text.

We also use the question on sales tax evasion: "Recognizing the difficulties many enterprises face in fully complying with taxes and regulations, what percentage of total sales would you estimate the typical establishment in your area of activity reports for tax purposes?" There are obvious difficulties in assuming that the firm's answer to this question reflects its own industry tax evasion and even harder that it is the firm's own evasion level.

To construct a measure of external reliance, we strictly follow the methodology presented in Rajan and Zingales (1998, RZ hereafter). The measure of external dependence proxies for the amount of investment that cannot be financed through internal sources, i.e. the retained earnings generated by the business. For the U.S. values of this measure, we use Compustat over the years 2000-2012, and compute external reliance as the ratio of capital expenditures minus cash flow from operations divided by capital expenditures. For each firm, we sum the numerator and denominator over time before dividing, with the intention of smoothing temporal fluctuations. As in RZ we use the industry median, to reduce the impact of larger firms. For the decades after 1990, the reporting in Compustat changes from the original RZ sample. We follow the instructions in RZ and the conventions in the finance literature to construct the measure of external reliance.

In the World Bank sample, firm's balance sheets give information on the shares of financing from different sources of earnings. The measure of external reliance is defined as the ratio of external sources' share over the sum of external and internal shares:

$$share\, ext_i = \frac{external_i}{external_i + internal_i}$$

Internal finance is the sum of retained earnings, friends and family and informal finance. External finance is defined as banking finance which incudes loans, overdrafts and credit card finance. An important advantage of the Enterprise Surveys is the possibility to construct the same measure of external reliance in both the World Bank and the Compustat sample. In the Enterprise Survey, the industry-average external reliance is .38, with a standard deviation of .47. Table 1 provides some summary statistics for sources of firm-financing, distinguishing between finance-sources for working capital and for new investment. In both cases, retained earnings is the dominant method of financing, with respectively 63.9 and 63.8 percent of working capital and new investment financed through the internal cash flow of the firm. Bank finance comes second for both capital and investment, at respectively 15.4 and 19.6 percent.

For some sources of financing it is ambiguous whether they generate any tax-relevant information trail (such as trade credit and leasing arrangements). In all results presented, they are excluded when constructing our measure of external reliance.

TABLE 35: FINANCING SOURCES SUMMARY

	Working Capital		New Investment	
	mean	sd	mean	sd
Retained earnings	63.9	38.0	63.8	42.2
Bank finance	15.4	27.5	19.6	34.3
Trade credit	11.8	22.5	4.3	16.5
Friends	3.7	13.8	3.5	15.1
Non bank finance	1.6	9.3	1.9	11.5
Other	3.3	14.2	5.6	19.8
N	69985		43869	

We choose as number of workers the Enterprise Survey firm's reported average number of permanent workers. Workers which are permanent employees are more likely to have access to the internal books of the firm and thus are more able than temporary employees to 'blow the whistle' and report tax underreporting. The sample-wide distribution of firms has heavy negative skew, with a long right tail: while the median is at 17 workers, the mean of 105 is largely influenced by a few very large firms. Given the shape of the marginal density, we always use the natural log of firm size in the regressions. We retrieve data in the U.S. from the 2002 version of the Census of Employment and Wages to construct U.S. ISIC3-specific average firm-size.

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