# Online Appendix for 'Employment Structure and the Rise of the Modern Tax System'

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# A Cross-development material

## A.1 Data sources and construction of variables

In this sub-section, I provide additional details on the novel micro data-base used in Section 3. I first outline the underlying data and construction of variables in the cross-section of countries. I then outline the data and variables construction used in the historical US time-series.

### A.1.1 Cross-country: sources and methodology

The cross-country database contains micro-data collected from 100 countries around the world, to document changes in employment structure transformation in as many incremental stages over development as possible. I chose to focus on countries with at least 1 million citizens. The selection of a survey in a particular country had to satisfy three criteria. First, it must be nationally representative. Second, it must survey respondents in all forms of work arrangement as opposed to, per example, only salaried workers. Third, it must contain continuous information on all sources of income, instead of, say, only wage earnings.

Given these criteria, the preferred type is a living conditions survey. This type of survey will often dominate a 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. Basic health and demographics surveys are discarded, because they do not contain information on work arrangements and income.

The data collection effort resulted in 100 surveys, which are detailed in Table A.1, displaying for each country: the year of the survey; the per capita income group; the survey type; the coverage; the sample size; and, the original source. The income group corresponds to the World Bank classification of the country in the year of the survey. The micro data-base covers all levels of development: 20% of surveys from low-income countries; 28% from lower-middle income countries; 21% from upper-middle income countries; and, 31% from high-income countries. 93 out of the 100 data-sets are living condition surveys, 5 are labor force surveys, and the remaining 2 are censuses. In low and lower-middle countries, I obtain almost all surveys directly from the national statistics office, or the relevant government agency. In these countries, the average sample size is substantially larger than the corresponding Living Standards and Measurement Survey (LSMS) from the same country.

The construction of the employee variable is based on questions similar to the 'class of worker' question in the US Census. All cross-country surveys were chosen to ensure the highest possible international comparability. Two features in particular serve that purpose, and are common across all surveys. The first feature is the high level of detail in the categories of the 'worker-class' question. In all surveys, I can therefore distinguish between employees and employers. This removes the possibility that employers of large firms are counted as employees, in which case the comparison of employee versus self-employed would partially be confounded by a firm size comparison. In addition, I can systematically distinguish between employees and both family and non-family workers in household enterprises. I can also systematically distinguish between employees that work for a salary versus employees that work for in-kind payments. Finally, and related to the previous point, I can distinguish between casual daily wage laborers and 'regular' employees in the countries where seasonal work is arguably most prevalent. It is true, however, that I cannot systematically distinguish casual wage laborers, and non-regular wage earners more generally, from contract-based regular employees. Taken together, this discussion implies that, with the exception of daily wage laborers, I can construct employee and self-employment categories in a consistent and internationally comparable manner across all countries. The second advantageous feature of all surveys is that my definition of employee versus self-employed is systematically based on an 'objective' worker-class question. In contrast, certain surveys allow respondents to choose 'informal sector' in response to the worker-type question. As discussed in the main text, my employee classification is closely related to the ILO concept of formality. Nonetheless, the specific definition of formality embedded in surveys is likely to vary across countries in ways that are hard to measure, and relying on such responses would reduce the transparency of comparisons across countries. As such, I discard all surveys where I cannot construct the employee classification based on an detailed and objective 'class of worker' question.

I focus on calculating gross income from all sources in order to be conceptually consistent with the broadest possible income-definition in the tax code. This leads me to calculate four sources of income: wage income, self-employment income, capital income, and miscellaneous income (such as lottery receipts). Most importantly, I ensure that I can calculate both employee and self-employment income with precision. In this context, the most significant challenge is to calculate self-employment income in agriculture in less-developed countries. Agricultural earned revenue includes the value of crops sold to others. I do not attempt to create a monetary value of in-kind sales, as offering and receipt of in-kind goods and services is not subject to tax. Agricultural capital revenue includes the sale of livestock, income from rental of equipment, and share-cropping income. From this revenue I attempt to subtract costs, which include expenditure on inputs, wages paid out to workers, and new investments. In a limited number of countries, I do not observe any agricultural revenue for respondents that are self-employed in agriculture. These are most often contributing family workers on farms where the full output is consumed by the family. In this limited number of cases, I construct the income as the market value of the own-consumed output, as estimated by the respondent. In all surveys, I exclude two sources: social transfers, and in-kind goods and services. I exclude social transfers because it falls outside the concept of taxable income. The monetary value of in-kind goods and services are sometimes included in taxable income, often on a presumptive basis. However, apart from the mentioned case above, I exclude this source of income because I cannot measure it consistently across all surveys. Non-monetary income is often more important for less wealthy individuals, and is more prevalent in less developed countries. In the surveys where there exists systematic data on the monetary value of non-monetary income, I can confirm that the inclusion of these sources of income does not change the distributional employee-profile. That is because these sources of non-monetary income are too small in magnitude to overturn the decile-ranking of individual income.

In 7 countries, I cannot calculate gross individual income with precision. These countries, also reported in Table A.2, are: Democratic Republic of Congo, Liberia, Ethiopia, Malawi, Mali, Burkina Faso, Cambodia. In the case of DRC, Liberia, Ethiopia, and Malawi, I do not comprehensively observe either agriculture sales or costs, so I cannot calculate agricultural self-employment income. In Mali, Burkina Faso, and Cambodia, I do not comprehensively observe costs of non-agriculture own-account workers, so I cannot calculate non-agricultural self-employment income. In these 7 cases, which are among the poorest in the micro-database, I instead calculate total individual expenditure, and use it as a proxy for total income. There exists a set of low-income countries in which I have both good income and expenditure data. In results not reported, I can confirm that the employee-share profiles are very similar when using either income or expenditure to calculate the x-axis distribution. As mapping expenditure into income is difficult, I do not attempt to locate the income tax exemption threshold in these 7 countries.

While I define the employee-status based on the respondent's primary job activity, I attempt to calculate income from all activities reported during the reference period. The main issue that arises in this context is the allocation of income which is reported at the household, rather than individual, level. For sources of earned income that are not at the individual level, I assign equal portions of this income to each economically active member of the household that reports having undertaken this activity during the reference period. Per example, the value of sold crops will be distributed equally among all household members that report having contributed to the family farm, either as a first or secondary activity. For sources of non-earned income reported at the household level, I assign an equal portion to each economically active member, such as in the case of property rental income.

Whenever a country's tax code is based on annual amounts and the reference period in the country's survey module is not, I construct the annual income distribution. I multiply the regular amount by the number of periods in the year – e.g. if wage income was reported monthly, I multiply it by the number of months that the wage income is reported to have been received during the past year. In the case where no periodicity exists, I assume that the flow was occurring during the whole year with the same pattern as during the reference period.

In every country survey, I limit the sample to the economically active population, following the definition of employment from the U.N. System of National Accounts. This definition is also used in Bicks, Fuchs-Schundeln, & Lagakos (2018), and in Feng, Lagakos, & Rauch (2018), which study respectively how hours worked and unemployment vary with development. I code employment-type based on the primary job in the reference period. The primary job is often explicitly defined as the job in which the respondent spent most hours during the reference period. The reference period in the Luxembourg Income Study (LIS) is annual, while it is predominantly monthly in the remaining surveys. The extent to which the periodicity and the focus on the primary job introduce biases in the representativeness of my employment-categories is discussed in Section A.6.

In addition to income and employment-categories, the micro-database also contains variables on education, sector, and geographical location. The geographical location measures whether a respondent lives in an urban area or not. I do not attempt to harmonize this variable, and use the urban definition in the surveys, which may therefore vary from country to country. I use variables to indicate three levels of education completion: not completed primary; completed primary but not high school; completed high school. I chose to not distinguish further levels of education, in order to maximize the number of surveys where I could create consistent measures. Finally, I code the sector of the primary job. The aim was to create a set of sectoral categories which are consistent with the ISIC classification. I create four sectoral categories: agriculture; manufacturing ; services; and, public administration. I define these four categories in relation to the divisions of the ISIC 4.4 classification, where: agriculture contains Section A; manufacturing and construction contains Sections B to F; services contains Sections G to M, and S to U; and, public administration and education contains Sections N to R. As such, the manufacturing sector also contains mining and construction; the services sector also contains wholesale and retail trade, transportation, IT, finance, and activities of household enterprises; and, public administration also contains education, social work, and entertainment. Most of the industry codes in the surveys do not contain a pre-existing ISIC classification. To the best extent possible, I therefore first map the survey-categories to ISIC divisions, and then to my 4 sectoral categories. I do not include the sectoral variable if the survey has data only on a subset of the categories - per example, if a survey records that a job is not in the agricultural sector, but does not specify which non-agricultural sector it belongs to. The availability of the geography, education, and sector variables across surveys is described in Table A.2. These variables are used in the regression analysis in Section A.5.

#### A.1.2 Historical US time-series: sources and methodology

The historical federal profiles in the US between 1950 and 2010 were constructed using the decennial Census samples, extracted from the IPUMS USA database. I exclude all respondents that are not active in the labor force during the reference period. I calculate the individual income distribution, based on the measure of gross income at the individual level. To construct the income distribution, I use the measure of total, pre-tax, personal income. Farm and non-farm business income, as well as wage income, are consistently recorded in every Census sample. I use the detailed 'class of worker' question, which allows me to assign unpaid family workers to the self-employed category. Consequently, the self-employed category includes employers, own account workers, self employed that are not incorporated, and self-employed that are incorporated. Given the resemblance with the categories contained in the cross-country surveys, there is strong comparability between these US historical profiles and the cross-country profiles constructed in Figure 3. I apply individual weights to estimate the employee-share of every decile of the income distribution in every decade.

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. Instead, I use the 1935-36 Study of Consumer Purchases. The scope of the study was 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 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 a random sub-sample of approximately 5,000 families who only completed the first-wave 'labor force' component of the survey.[1] The ICPSR sub-sample 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." The employee classification is based on 'status of employment' question, which is identical to the (non-detailed) 'class of worker' question used in all US Censuses from 1950 onward. 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 exclude all respondents that are employed on work-relief projects in their primary job. As such, the sample closely resembles the economically active workforce definition used in the cross-country sample. Total gross income only exists at the household level. Rather than try to assign income at the individual level within the household, I focus on the work-type of the head of household. I then rank individuals based on the reported total income, and estimate the employee-share in each income decile.

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), which studies 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, Lindert & Williamson build estimates of 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 informa-

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

tion 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 social accounting matrices that were used in development economics in the 1970s and 1980s. The authors provide a significant effort to capture all occupation categories in a given space-time. They 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 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 derive measures of total income.

I construct a historical 1870 profile based on the data kindly provided by Peter Lindert. This cross-section builds upon the 1870 1 percent US Census sample delivered to the authors by IPUMS USA, which included 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. I use the detailed description of each occupation category to code employment-type. Per example, all occupations where a reference is made 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 reported separately from domestic servants." The occupation category only exists for the head of household. The measure of total income includes own labor earnings in agriculture and non-agriculture, farm and non-farm operating income, and property income. This is a comprehensive measure of gross income before taxes and transfers which is not identical to, but closely resembles, the measure used in the more recent Federal US and cross-country samples. I apply the sampling weights initially provided by IPUMS USA. I estimate the employee-share in every decile of the individual gross income distribution, for the population that is active in the labor force.

In all the profiles, I locate the Federal income tax exemption threshold in the income distribution. Note that there was no Federal income tax in 1870. In all profiles from 1935 onward, I use the historical IRS series which provide details on the nominal value of the standard deduction of a single filer.

Table A.1: Cross-Country Data Sources

| Country | Year | Per Capita Income Group | Survey type | Coverage | Sample Size | Original source |
|---|---|---|---|---|---|---|
| **Albania** | 2009 | Upper Middle | Labor Force | National | 18,997 | National Institute of Statistics |
| **Argentina** | 2009 | Upper Middle | Living Conditions | Urban | 47,862 | National Institute of Statistics and Census |
| **Australia** | 2014 | High | Living Conditions | National | 16,801 | Luxembourg Income Study |
| **Austria** | 2013 | High | Living Conditions | National | 5,102 | Luxembourg Income Study (LIS) |
| **Azerbaijan** | 1995 | Low | Living Conditions | National | 8,901 | Living Standards Measurement Study (LSMS) |
| **Bangladesh** | 2010 | Low | Living Conditions | National | 19,664 | Bangladesh Bureau of Statistics |
| **Belgium** | 2000 | High | Living Conditions | National | 2823 | Luxembourg Income Study (LIS) |
| **Belize** | 1999 | Lower Middle | Labor Force | National | 15,167 | Central Statistical Office |
| **Bolivia** | 2007 | Lower Middle | Living Conditions | National | 16,130 | National Institute of Statistics |
| **Brazil** | 2009 | Upper Middle | Living Conditions | National | 191, 810 | National Institute of Geographics and Statistics |
| **Bulgaria** | 2007 | Upper Middle | Living Conditions | National | 6,941 | National Institute of Statistics |
| **Burkina Faso** | 2014 | Low | Living Conditions | National | 32,023 | National Institute of Statistics and Demographics |
| **Cambodia** | 2009 | Low | Living Conditions | National | 31,959 | Ministry of Planning |
| **Cameroon** | 2007 | Lower Middle | Living Conditions | National | 51,836 | National Institute of Statistics |
| **Canada** | 2013 | High | Living Conditions | National | 27,344 | Luxembourg Income Study (LIS) |
| **Chile** | 2009 | Upper Middle | Living Conditions | National | 90,610 | Social Observatory, University Alberto Hurado |
| **China** | 2013 | Upper Middle | Living Conditions | National | 14,782 | Luxembourg Income Study (LIS) |
| **Colombia** | 2009 | Upper Middle | Living Conditions | National | 170,220 | National Directory of Statistics |
| **Costa Rica** | 2009 | Upper Middle | Living Conditions | National | 19,594 | National Institute of Statistics and Census |
| **Czech Republic** | 2013 | High | Living Conditions | National | 7,653 | Luxembourg Income Study (LIS) |
| **Cote d'Ivoire** | 2008 | Lower Middle | Living Conditions | National | 59,699 | National Institute of Statistics |
| **Dem. Rep. of the Congo** | 2004 | Low | Living Conditions | National | 72,685 | National Institute of Statistics |
| **Denmark** | 2013 | High | Living Conditions | National | 88,696 | Luxembourg Income Study (LIS) |
| **Dominican Republic** | 2009 | Upper Middle | Living Conditions | National | 30,430 | National Statistics Office |
| **Ecuador** | 2009 | Lower Middle | Living Conditions | National | 78,865 | National Institute of Staistics and Censuses |
| **Egypt** | 2010 | Lower Middle | Living Conditions | National | 34,069 | Economic Research Forum (ERF) |
| **El Salvador** | 2014 | Lower Middle | Living Conditions | National | 20,361 | Center for Labor and Social Studies (CEDLAS) |
| **Estonia** | 2013 | High | Living Conditions | National | 6,576 | Luxembourg Income Study (LIS) |
| **Ethiopia** | 2010 | Low | Living Conditions | National | 18,864 | Living Standards Measurement Study (LSMS) |
| **Finland** | 2013 | High | Living Conditions | National | 11,112 | Luxembourg Income Study (LIS) |
| **France** | 2010 | High | Living Conditions | National | 14,440 | Luxembourg Income Study (LIS) |

Table A.1: Cross-Country Data Sources (continued)

| Country | Year | Per Capita Income Group | Survey type | Coverage | Sample Size | Original source |
|---|---|---|---|---|---|---|
| **Georgia** | 2010 | Lower Middle | Living Conditions | National | 4,811 | Luxembourg Income Study (LIS) |
| **Germany** | 2014 | High | Living Conditions | National | 14,915 | Luxembourg Income Study (LIS) |
| **Ghana** | 2010 | Low | Living Conditions | National | 62,042 | Ghana Statistical Service |
| **Greece** | 2013 | High | Living Conditions | National | 6,115 | Luxembourg Income Study (LIS) |
| **Guatemala** | 2014 | Lower Middle | Living Conditions | National | 22,118 | Luxembourg Income Study (LIS) |
| **Honduras** | 2009 | Lower Middle | Living Conditions | National | 98,028 | National Institute of Statistics |
| **Hungary** | 2014 | High | Living Conditions | National | 2,718 | Luxembourg Income Study (LIS) |
| **Iceland** | 2010 | High | Living Conditions | National | 4,133 | Luxembourg Income Study (LIS) |
| **India** | 2004 | Low | Living Conditions | National | 59,487 | Luxembourg Income Study (LIS) |
| **Indonesia** | 2011 | Lower Middle | Living Conditions | National | 111,824 | Statistics Indonesia |
| **Iraq** | 2011 | Lower Middle | Living Conditions | National | 176,042 | Economic Research Forum (ERF) |
| **Ireland** | 2010 | High | Living Conditions | National | 3,508 | Luxembourg Income Study (LIS) |
| **Israel** | 2014 | High | Living Conditions | National | 11,770 | Luxembourg Income Study (LIS) |
| **Italy** | 2014 | High | Living Conditions | National | 6,258 | Luxembourg Income Study (LIS) |
| **Jamaica** | 2002 | Lower Middle | Living Conditions | National | 18,943 | Living Standards Measurement Study (LSMS) |
| **Japan** | 2008 | High | Living Conditions | National | 7,840 | Luxembourg Income Study (LIS) |
| **Jordan** | 2010 | Upper Middle | Living Conditions | National | 15,472 | Economic Research Forum (ERF) |
| **Kenya** | 2005 | Low | Living Conditions | National | 62,175 | National Bureau of Statistics |
| **Kosovo** | 2000 | Lower Middle | Living Conditions | National | 14,167 | Living Standards Measurement Survey (LSMS) |
| **Liberia** | 2014 | Low | Living Conditions | National | 18,089 | Institute for Statistics |
| **Lithuania** | 2008 | Upper Middle | Living Conditions | National | 15,837 | National Statistics Office |
| **Luxembourg** | 2013 | High | Living Conditions | National | 4,373 | Luxembourg Income Study (LIS) |
| **Malawi** | 2011 | Low | Living Conditions | National | 56,218 | National Statistical Office |
| **Mali** | 2014 | Low | Living Conditions | National | 37,175 | Living Standards Measurement Study |
| **Mexico** | 2011 | Upper Middle | Living Conditions | National | 17,682 | National Institute of Statistics and Geography |
| **Mongolia** | 2003 | Low | Labor Force | National | 49,948 | National Statistical Office |
| **Morocco** | 2009 | Lower Middle | Living Conditions | National | 10,769 | Ministry of Economy and General Affairs |
| **Mozambique** | 2014 | Low | Living Conditions | National | 9,128 | National Institute of Statistics |
| **Namibia** | 2009 | Upper Middle | Living Conditions | National | 44,614 | National Planning Commission |
| **Netherlands** | 2013 | High | Living Conditions | National | 23,935 | Luxembourg Income Study (LIS) |
| **Nicaragua** | 2014 | Lower Middle | Living Conditions | National | 9,250 | Center for Labor and Social Studies (CEDLAS) |
| **Niger** | 2011 | Low | Living Conditions | National | 3,859 | Living Standards Measurement Survey (LSMS) |
| **Nigeria** | 2011 | Lower Middle | Living Conditions | National | 23,289 | National Bureau of Statistics |
| **Norway** | 2013 | High | Living Conditions | National | 23,993 | Luxembourg Income Study (LIS) |

Table A.1: Cross-Country Data Sources (end)

| Country | Year | Per Capita Income Group | Survey type | Coverage | Sample Size | Original Source |
|---|---|---|---|---|---|---|
| **Pakistan** | 2001 | Lower Middle | Living Conditions | National | 75,519 | Federal Bureau of Statistics |
| **Palestine** | 2011 | Lower Middle | Living Conditions | National | 25,947 | Economic Research Forum (ERF) |
| **Panama** | 2010 | Upper Middle | Population and Housing Census | National | 314,118 | IPUMS-International |
| **Papua New Guinea** | 1996 | Lower Middle | Living Conditions | National | 8,660 | Living Standards Measurement Survey |
| **Paraguay** | 2009 | Lower Middle | Living Conditions | National | 18,419 | National Statistics Office |
| **Peru** | 2009 | Upper Middle | Living Conditions | National | 95,199 | National Institute of Statistics |
| **Poland** | 2013 | High | Living Conditions | National | 39,993 | Luxembourg Income Study (LIS) |
| **Puerto Rico** | 2005 | High | Population and Housing Census | National | 35,416 | IPUMS-International |
| **Romania** | 1997 | Lower Middle | Living Conditions | National | 35,995 | Luxembourg Income Study (LIS) |
| **Russia** | 2013 | High | Living Conditions | National | 6,079 | Luxembourg Income Study (LIS) |
| **Rwanda** | 2000 | Low | Living Conditions | National | 32,679 | National Institute of Statistics |
| **Serbia** | 2007 | Upper Middle | Living Conditions | National | 17,375 | Living Standards Measurement Survey (LSMS) |
| **Sierra Leone** | 2003 | Low | Living Conditions | National | 23,022 | National Office of Statistics |
| **Slovakia** | 2009 | High | Living Conditions | National | 4,704 | National Statistical Office |
| **South Africa** | 2012 | Upper Middle | Living Conditions | National | 7,105 | Luxembourg Income Study (LIS) |
| **South Korea** | 2006 | High | Living Conditions | National | 13,178 | Luxembourg Income Study (LIS) |
| **Spain** | 2013 | High | Living Conditions | National | 10,728 | Luxembourg Income Study (LIS) |
| **Sri Lanka** | 2008 | Lower Middle | Labor Force | National | 66,381 | Department of Census and Statistics |
| **Sudan** | 2009 | Lower Middle | Living Conditions | National | 48,845 | Economic Research Forum (ERF) |
| **Sweden** | 2005 | High | Living Conditions | National | 11,607 | Luxembourg Income Study (LIS) |
| **Switzerland** | 2013 | High | Living Conditions | National | 7,961 | Luxembourg Income Study (LIS) |
| **Taiwan** | 2013 | High | Living Conditions | National | 23,474 | Luxembourg Income Study (LIS) |
| **Tajikistan** | 2007 | Low | Living Conditions | National | 1,503 | State Statistical Agency |
| **Timor Leste** | 2007 | Lower Middle | Living Conditions | National | 9,094 | National Statistics Directorate |
| **Tunisia** | 2009 | Upper Middle | Living Conditions | National | 50,371 | Economic Research Forum (ERF) |
| **Turkey** | 2011 | Upper Middle | Labor Force | National | 37,121 | National Statistical Institute |
| **Tanzania** | 2010 | Low | Living Conditions | National | 20,559 | National Bureau of Statistics |
| **Uganda** | 2011 | Low | Living Conditions | National | 13,618 | National Bureau of Statistics |
| **Ukraine** | 2010 | Lower Middle | Living Conditions | National | 10,428 | State Statistics Service |
| **United Kingdom** | 2013 | High | Living Conditions | National | 20,002 | Luxembourg Income Study (LIS) |
| **United States** | 2013 | High | Living Conditions | National | 63,859 | Luxembourg Income Study (LIS) |
| **Uruguay** | 2009 | Upper Middle | Living Conditions | National | 132,559 | National Institute of Statistics |
| **Venezuela** | 2006 | Upper Middle | Living Conditions | National | 166,506 | National Institute of Statistics |
| **Zambia** | 2014 | Lower Middle | Living Conditions | National | 11,921 | Central Statistical Office |

**Notes**: for details on this table, please see Section A.1.

Table A.2: Cross-Country Data Variable Availability

| Country | Year | Income | Sector | Education | Location |
|---|---|---|---|---|---|
| **Albania** | 2009 | x | x | x | |
| **Argentina** | 2009 | x | x | x | x |
| **Australia** | 2014 | x | x | x | |
| **Austria** | 2013 | x | | x | x |
| **Azerbaijan** | 1995 | x | x | | x |
| **Bangladesh** | 2010 | x | x | x | x |
| **Belgium** | 2000 | x | x | x | x |
| **Belize** | 1999 | x | x | x | x |
| **Bolivia** | 2007 | x | x | x | x |
| **Brazil** | 2009 | x | x | x | x |
| **Bulgaria** | 2007 | x | | x | x |
| **Burkina Faso** | 2014 | | x | x | x |
| **Cambodia** | 2009 | | | x | x |
| **Cameroon** | 2007 | x | x | x | x |
| **Canada** | 2013 | x | | x | x |
| **Chile** | 2009 | x | x | x | x |
| **China** | 2013 | x | x | x | x |
| **Colombia** | 2009 | x | x | x | x |
| **Costa Rica** | 2009 | x | x | x | x |
| **Czech Republic** | 2013 | x | x | x | x |
| **Cote d'Ivoire** | 2008 | x | x | x | x |
| **Dem. Rep. of the Congo** | 2004 | | | x | x |
| **Denmark** | 2013 | x | x | x | x |
| **Dominican Republic** | 2009 | x | x | x | x |
| **Ecuador** | 2009 | x | x | x | x |
| **Egypt** | 2010 | x | x | x | x |
| **El Salvador** | 2014 | x | x | | |
| **Estonia** | 2013 | x | x | x | |
| **Ethiopia** | 2010 | | | x | x |
| **Finland** | 2013 | x | x | x | x |
| **France** | 2010 | x | x | x | x |

Table A.2: Cross-Country Data Variable Availability (continued)

| Country | Year | Income | Sector | Education | Location |
|---|---|---|---|---|---|
| **Georgia** | 2010 | x | x | x | x |
| **Germany** | 2014 | x | x | x | x |
| **Ghana** | 2010 | x | | x | x |
| **Greece** | 2013 | x | x | x | x |
| **Guatemala** | 2014 | x | x | x | x |
| **Honduras** | 2009 | x | x | x | x |
| **Hungary** | 2014 | x | x | x | x |
| **Iceland** | 2010 | x | x | x | x |
| **India** | 2004 | x | x | x | x |
| **Indonesia** | 2011 | x | x | x | x |
| **Iraq** | 2011 | x | x | x | x |
| **Ireland** | 2010 | x | x | x | x |
| **Israel** | 2014 | x | x | x | x |
| **Italy** | 2014 | x | x | x | x |
| **Jamaica** | 2002 | x | x | x | x |
| **Japan** | 2008 | x | x | x | x |
| **Jordan** | 2010 | x | x | x | x |
| **Kenya** | 2005 | x | x | x | x |
| **Kosovo** | 2000 | x | x | x | x |
| **Liberia** | 2014 | | | x | x |
| **Lithuania** | 2008 | x | | x | x |
| **Luxembourg** | 2013 | x | x | x | x |
| **Malawi** | 2011 | | x | x | x |
| **Mali** | 2014 | | x | x | x |
| **Mexico** | 2011 | x | x | x | x |
| **Mongolia** | 2003 | x | x | x | x |
| **Morocco** | 2009 | x | x | x | x |
| **Mozambique** | 2014 | x | x | x | x |
| **Namibia** | 2009 | x | x | x | x |
| **Netherlands** | 2013 | x | | x | x |
| **Nicaragua** | 2014 | x | x | | |
| **Niger** | 2011 | x | x | x | x |
| **Nigeria** | 2011 | x | x | x | x |
| **Norway** | 2013 | x | | | x |

Table A.2: Cross-Country Data Variable Availability (end)

| Country | Year | Income | Sector | Education | Location |
|---|---|---|---|---|---|
| **Pakistan** | 2001 | x | x | x | x |
| **Palestine** | 2011 | x | x | x | x |
| **Panama** | 2010 | x | x | | x |
| **Papua New Guinea** | 1996 | x | x | x | |
| **Paraguay** | 2009 | x | x | x | x |
| **Peru** | 2009 | x | x | x | x |
| **Poland** | 2013 | x | x | x | x |
| **Puerto Rico** | 2005 | x | | x | x |
| **Romania** | 1997 | x | x | x | x |
| **Russia** | 2013 | x | x | x | x |
| **Rwanda** | 2000 | x | x | x | x |
| **Serbia** | 2007 | x | x | x | x |
| **Sierra Leone** | 2003 | x | x | x | x |
| **Slovakia** | 2009 | x | | x | x |
| **South Africa** | 2012 | x | x | x | x |
| **South Korea** | 2006 | x | | | x |
| **Spain** | 2013 | x | x | x | x |
| **Sri Lanka** | 2008 | x | | x | x |
| **Sudan** | 2009 | x | x | x | x |
| **Sweden** | 2005 | x | x | x | |
| **Switzerland** | 2013 | x | x | x | |
| **Taiwan** | 2013 | x | x | x | |
| **Tajikistan** | 2007 | x | x | | x |
| **Timor Leste** | 2007 | x | x | x | x |
| **Tunisia** | 2009 | x | x | x | x |
| **Turkey** | 2011 | x | | x | x |
| **Tanzania** | 2010 | x | x | x | x |
| **Uganda** | 2011 | x | x | x | x |
| **Ukraine** | 2010 | x | x | | |
| **United Kingdom** | 2013 | x | x | x | |
| **United States** | 2013 | x | x | x | |
| **Uruguay** | 2009 | x | x | x | x |
| **Venezuela** | 2006 | x | x | x | x |
| **Zambia** | 2014 | x | x | x | x |

**Notes**: for details on this table, please see Section A.1.

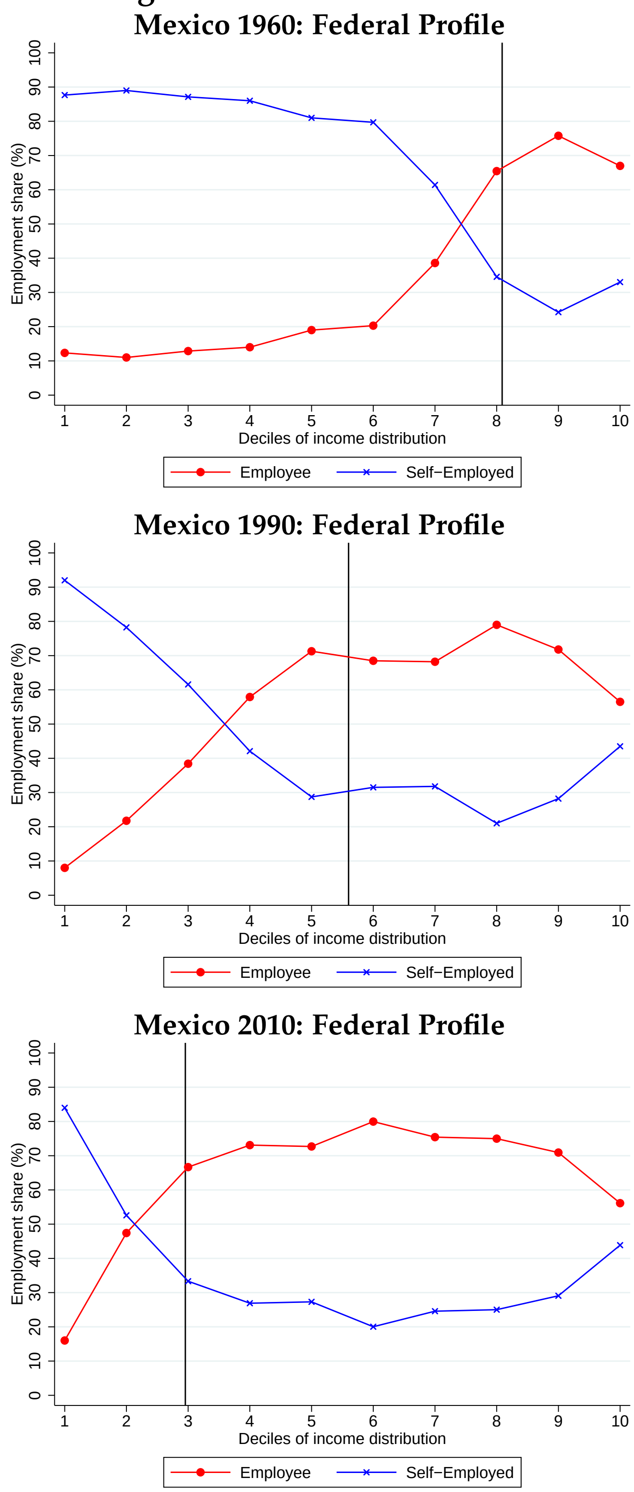
### A.2 Additional historical profile: Mexico 1960-2010

As a robustness check to the stylized facts, I show that they also hold over the long-run in a currently developing country, Mexico. I focus on Mexico because it has variables of income and employee-jobs that are consistently defined over a long period of time, namely 1960-2010. The data is extracted from IPUMS International. The disadvantage is that only earned income is measured consistently over this period - as opposed to total income, which further includes capital income and 'other' income. I use answers to the 'class of worker' question. The only inconsistency over time in this question is that the 2010 sample groups household assistants together with salaried workers, whereas in previous samples, these categories are separated. As such, I am over-estimating the true employee-share in the 2010 profile. Importantly, day laborers are separated from salaried workers, and I can assign the former to the self-employment category in all years. There also exists a category for unpaid family workers, which I assign to the self-employment category. I construct the sample of respondents that are economically active, and use survey weights to construct individual earned income distributions in 1960, 1990, and 2010. For the years 1990 and 2010, I code the value of the exemption threshold from OECD's Personal Taxes database. For 1960, I use the historical archives of the Mexican Tax Authority.[2]

The results from this exercise are displayed in Figure A.1. I uncover the same stylized facts that were found both in the cross-country sample and in the historical US series: the employee-share profile is upward-sloping and gradually moves leftward in the gross income distribution; the exemption threshold gradually moves down the distribution and expands the size of the income tax base; and, the employee-composition on the tax base is constantly maximized.

[2] Available at: http://www.dof.gob.mx/index.php.

**Figure A.1: Mexico 1960-2010**




**Notes**: The circle-line (small cross-line) indicate the employee-share of the economically active workforce in a decile of the Mexican personal gross income distribution. An employee-job is defined as a job whose activity generates an information trail that can be leveraged for income tax enforcement purposes. For more details on this variable, please see Section 3.2. In every profile, the vertical solid line denotes the location of the Federal individual income tax exemption threshold. This threshold is the nominal value of gross (pre-tax) income above which a single filer becomes liable to pay income tax. Each historical profile is built from the Census micro-data from IPUMS International. The values of the exemption thresholds are from the OECD's Personal Taxes database, and the official archives of the Mexican revenue service. Source: Section 3.4 and Appendix Section A.2.

### A.3 Redistributive targeting

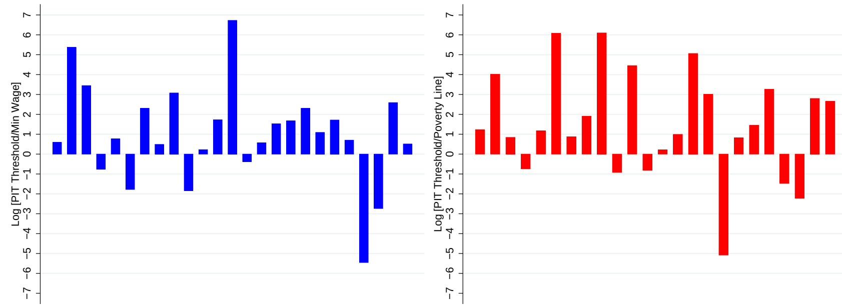
In this robustness check, I provide evidence to suggest that the exemption threshold is not set to target social assistance or anti-poverty in the income distribution. Governments define thresholds of income that are used as inputs in formulas to provide social assistance and anti-poverty relief. I use the national poverty line and the minimum wage values as proxies for the 'social redistribution' threshold. I first show that only very rarely is the income tax threshold explicitly defined to be either equal to, or a multiplicative of, this social redistribution threshold. In 5% of countries in the cross-sectional sample, the tax code defines the exemption threshold to be a multiple of this redistribution threshold. These countries are: Mozambique, Bolivia, Paraguay, Turkey, and Slovakia. As an example, in Mozambique the exemption threshold is equal to 36 times the minimum wage, while in Paraguay it is equal to 120 times the minimum wage. I use the country-specific IBFD tax summaries to document this pattern. There exists a much more frequent explicit relation between redistributive thresholds and social security contributions. Indeed, several countries use (a multiplicative of) the minimum wage to define an exemption threshold for employee contributions.

Even if there exists no explicit relation defined in the tax code, governments may nonetheless implicitly maintain an association between the tax threshold and the social assistance threshold. To investigate this, I collect data on the value of the national poverty line and the minimum wage in all countries in the cross-sectional sample. I try to collect the data in as close a year as possible to the survey and tax exemption threshold year. I use harmonized data from ILO on the statutory nominal gross monthly minimum wage. Data is missing in 8 countries: Austria, Denmark, Finland, France, Kosovo, Sweden, Switzerland, Palestine. There does not exist a similar harmonized database on the value of the national poverty line for my sample. The World Bank collects cross-country data on the share of the population that falls below both international and national poverty lines, but such data does not directly disclose the value of the national lines used. I was able to collect relevant data in 88 of the 100 countries in my sample. The missing countries are: Albania, Austria, Hungary, Kosovo, Panama, Papua New Guinea, Romania, Serbia, Slovakia, Timor Leste, Ukraine, and Venezuela. Importantly, I collect the poverty line that is set by the national government, rather than the value of the international poverty line in local currency. Some governments do incorporate international criteria to determine poverty lines. Per example, some low-income countries base their poverty calculations on the minimum nutritional intake concept used by the World Bank to define international poverty; and, some European countries adopt the EU-wide definition of poverty as 60% of median income. The important point is that the poverty lines I collect are based on an active decision made by the government, similarly to the definition of the tax exemption threshold. In some countries, the government defines several poverty lines, per example on a regional basis or on an urban-rural basis. I always pick the poverty line in each country with the highest value. Since poverty lines in developing countries are most often below the tax exemption threshold, this decreases the likelihood to observe that the two thresholds are far away from each other in value.

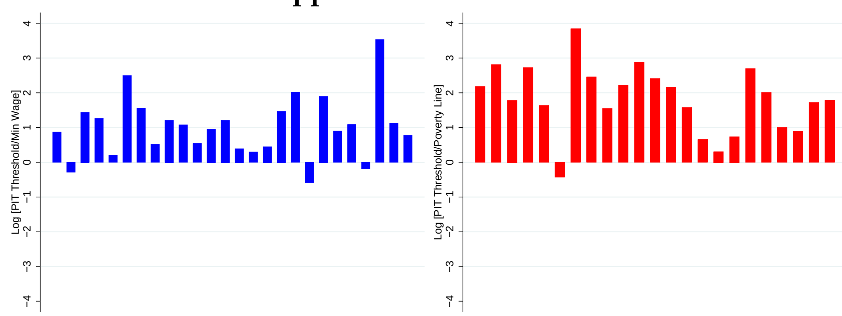
The results are displayed in Figure A.2. The three panels separate countries into development groups: low and lower middle income; higher middle income; and, high income. I construct the ratio of the income tax exemption threshold to the minimum wage, and of the exemption threshold to the poverty line. In the left-hand graphs, the bars represent country-specific ratios using the minimum wage, while the right-hand graphs display the ratio using the poverty line. Finally, within each graph, I sort the countries by GDP per capita. I take the log of the ratio, as this allows me to display all country-ratios on the same graph. Therefore, a bar-value below 0 means that the exemption threshold is located below the minimum wage/poverty line in the specific country. There is no obvious, confounding trend which emerges from Figure A.2. Within all development groups, countries with similar per capita income, and hence similar size of tax base (Figure 4), display very large variation in the relative value of the tax threshold to the redistribution threshold (note the log-scale of the y-axis). This holds even for countries at similar levels of development within the same region: the ratio for the minimum wage (poverty) is 0.48 log points (1.90 log points) in Burkina Faso, while it is 3.07 log points (6.09 log points) in Uganda; it is 2.09 log points (2.79 log points) in Bolivia, and 0.51 log points (2.65 log points) in Honduras. The highest-income countries often locate both the poverty and the minimum wage thresholds above the tax exemption threshold. But apart from this feature, there is not any systematic relationship between the relative location of tax and redistribution thresholds, and per capita income. Taken together, these findings suggest that the tax exemption threshold is not set to target social assistance in the income distribution.

## Figure A.2: Redistributive Targeting

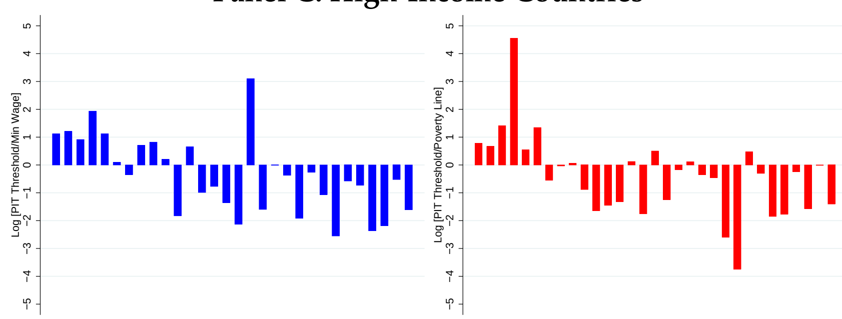
### Panel A: Low and Lower-Middle Income Countries




### Panel A: Upper-Middle Income Countries




### Panel C: High-Income Countries




**Notes**: In every graph, a bar represents a country-observation from the cross-country micro-database. The three panels demark countries according to their per capita income group: low and lower-middle; upper-middle; high income. Within each graph, countries are ranked in ascending order of per capita income. Within each group, the left-hand graph shows the log of the ratio of the income tax exemption threshold to the minimum wage; the right-hand graph shows the log of the ratio of the income tax exemption threshold to the poverty line. All thresholds are expressed in annual and local currency. Source: Section 3.4 and Appendix Section A.3.

### A.4 Sectoral distributional profiles

In this robustness check, I consider whether the location of the exemption threshold is targeting sectoral structure, rather than employment structure. I study whether the threshold appears to be set such as to avoid a 'hard to tax' sector, agriculture, or whether the threshold is set to capture the 'easy to tax' sectors of manufacturing and public administration (Musgrave, 1981). In order to investigate this confounding hypothesis, I first consider whether the tax exemption explicitly targets any sector. In particular, I use the IBFD country-reports in all countries in the cross-section, and report whenever income from agriculture is fully exempt from individual income taxation. I do not take into account instances where tax codes allow self-employed to deduct costs specifically related to agricultural work - per example, from the purchase of a tractor for farming. This is because my measure of the exemption threshold in all countries is the standard deduction, which is granted regardless of taxpayer behavior, and not the itemized deduction, which requires the taxpayer to itemize deductions. I chose the former measure because it can be constructed in a comparable way across space and time, as discussed in Section 3.2. I find that agricultural income is fully exempt only in 11% of low-income countries; 12% of middle-income countries; and, 5% of high-income countries. This list includes Mali, Morocco, and Sierra Leone in Africa; and, India and Pakistan in Asia.

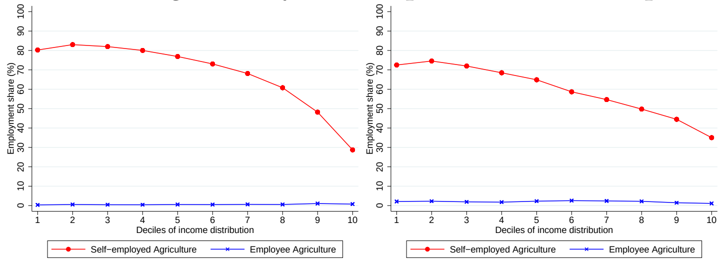
As an alternative approach, I consider whether changes in sectoral distributional profiles over development could account for the movement in the exemption threshold. I create four sectoral categories in all the surveys in the cross-section: agriculture; manufacturing and construction; trade and services; and, public administration. I define these four categories in relation to the divisions of the ISIC 4.4 classification. The construction of the sector variable is described in detail in Section A.1.1.

Using these harmonized sector variables, I first study the distributional profiles of agricultural employment. I construct these profiles in the same way as the employment profiles in the main text (Section 3.2). The results are displayed in Figure A.3. At lowest levels of development, agriculture is prevalent everywhere except for the top of the income distribution. And, in the transition from low-income to middle-income group, the downward-sloping agriculture-profile gradually shifts leftward in the distribution. This pattern is similar to stylized fact #1, such that stylized fact #2 would be consistent with a setting where the exemption threshold targets the non-agricultural sector which increases gradually further down the income distribution. However, Figure A.3 also reveals that in these same income groups, virtually all agricultural work is concentrated among self-employed with no information trails. On the other hand, Figure A.3 reveals that in the transition from middle-income to the high-income group, the agricultural profile has become very small in magnitude and almost entirely flat in the distribution. During this same transition, there continues to be an important transition between self-employment and employee-jobs outside of the agricultural sector, which is associated with further decreases in the location of the threshold. These facts suggest that movement out of agriculture could account for the expansion of the tax base, but only in a limited range of the development path, where it is fully confounded by movements out of self-employment. In contrast, movements out of self-employment can account for the expansion of the base over the full development path, including over a range of development where it cannot be confounded by movements out of agriculture.

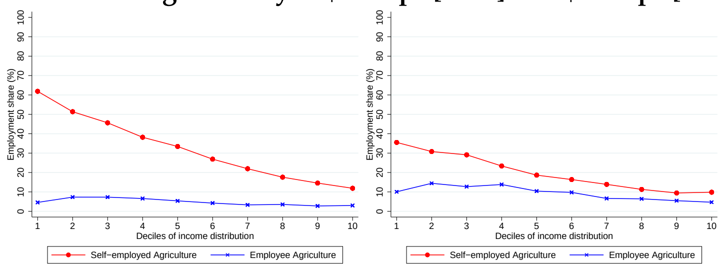
I now consider whether the movement of the exemption threshold is consistent with targeting of 'easy to tax' sectors. I focus on manufacturing and public administration. Since work in these sectors is strongly correlated with having an employee-job, I study the sectoral profiles conditional on employee-job. Results are displayed in Figure A.4. The distributional profile of easily taxable sectors would have to be upward-sloping in the income distribution, and move leftward as the country develops, in order to be a confounding factor. This is not borne out in the observed profiles. The public administration profile is upward-sloping at some development levels, but the magnitude of the slope is quantitatively small, and there is no consistent left-ward shift over development. The public sector share at the top of the income distribution is most likely driven by central administration workers, while the share towards the lower end of the distribution is probably made up in part by field-workers in health and education. While located at very different parts of the income distribution, these jobs share the common feature of being easy to tax - in the sense that the government, as the direct employer, perfectly observes the salaries. The manufacturing distributional profile is largely flat in the income distribution. The level-shift upward and then downward of the manufacturing profile is consistent with the inverse-U shaped aggregate importance of manufacturing over development that other work has documented. Taken together, these facts do not suggest that the stylized facts #1-#4 are confounded by sectoral transitions over the development path.

## Figure A.3: Distributional Profiles of 'Hard-to-Tax' Sectors

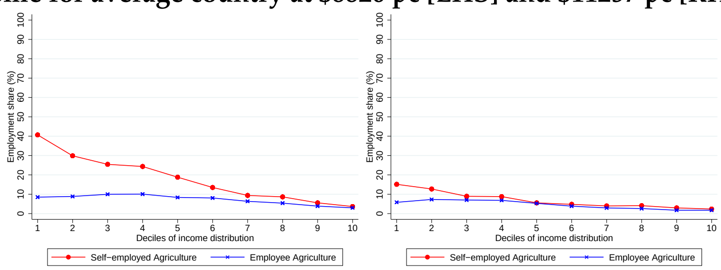
**Profile for average country at $1065 pc [LHS] and $2226 pc [RHS]**



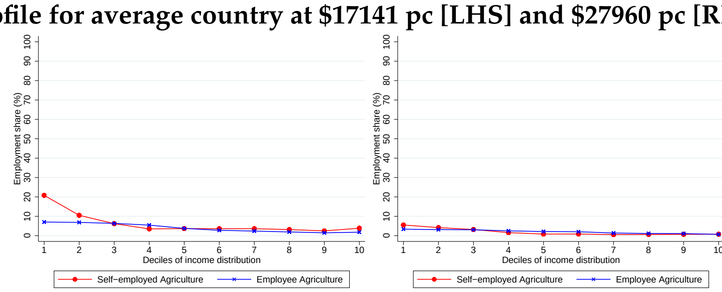

**Profile for average country at $3239 pc [LHS] and $5796 pc [RHS]**



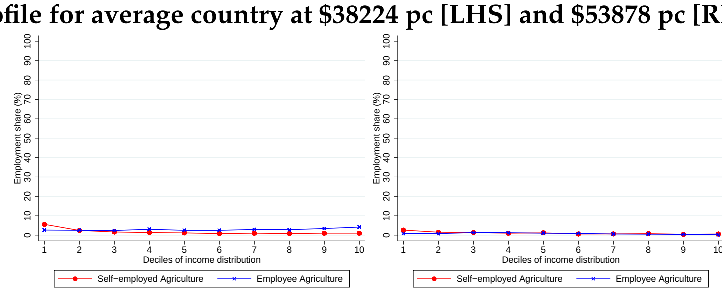

**Profile for average country at $8826 pc [LHS] and $11257 pc [RHS]**




**Profile for average country at $17141 pc [LHS] and $27960 pc [RHS]**



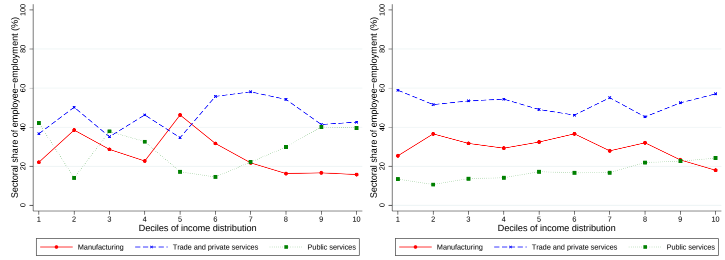

**Profile for average country at $38224 pc [LHS] and $53878 pc [RHS]**



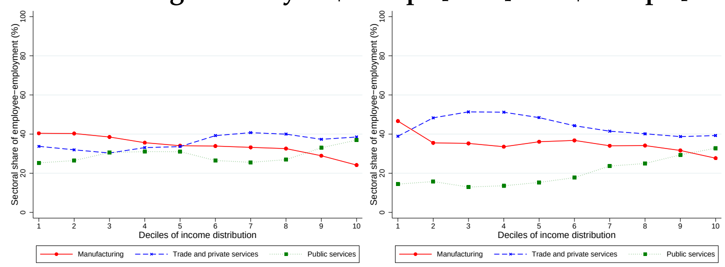

**Notes**: These figures plot the employment shares of self-employed agricultural workers and of employee agricultural workers, over deciles of the income distribution, for representative countries at different levels of per capita income. Employees (self-employed) are defined as individuals working in jobs which generate (no) information trails for the purposes of income tax enforcement. The share of each group is defined as the share of the total economically active workforce in the decile of the income distribution. To construct this graph, I partition the cross-country sample into ten groups of equal size, based on their level of per capita income. Note that I am limited to the group of countries where there exists sectoral data (see Table A.2). Within each group, I calculate the unweighted average employment-share of agricultural self-employed and agricultural employee. I plot this average profile for every group, and indicate the average per capita income of the group. I use expenditure-side real GDP at chained PPPs in 2011 US$ from the same year as the country-survey year. Source: Section 3.4 and Appendix Section A.4.

## Figure A.4: Distributional Profiles of 'Easy-to-Tax' Sectors

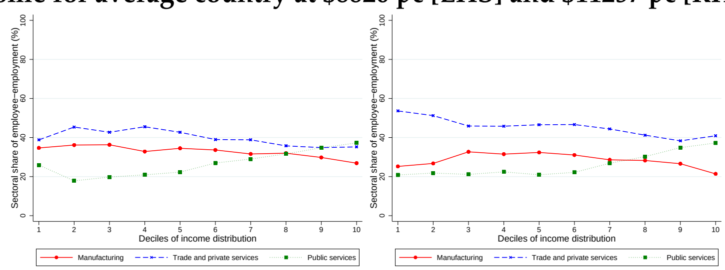
### Profile for average country at $1065 pc [LHS] and $2226 pc [RHS]



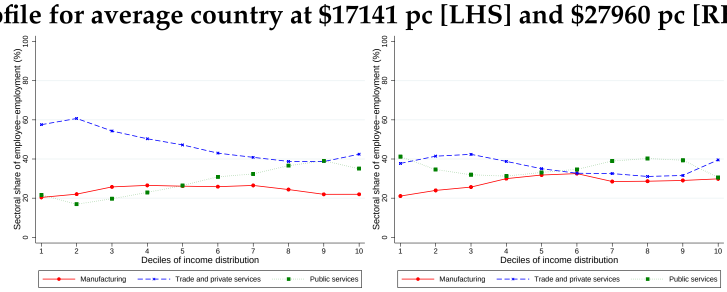

### Profile for average country at $3239 pc [LHS] and $5796 pc [RHS]



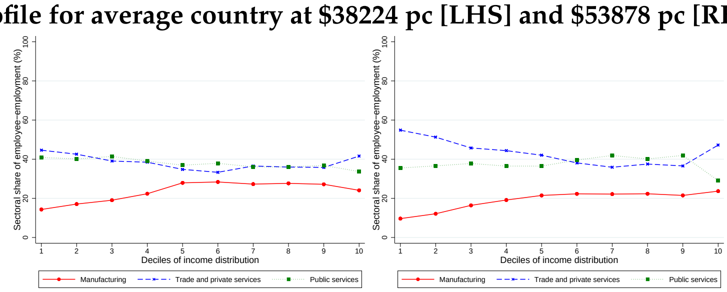

### Profile for average country at $8826 pc [LHS] and $11257 pc [RHS]




### Profile for average country at $17141 pc [LHS] and $27960 pc [RHS]




### Profile for average country at $38224 pc [LHS] and $53878 pc [RHS]




**Notes**: These figures plot the sectoral shares of employees over deciles of the income distribution, for representative countries at different levels of per capita income. Sectors are defined accordig to the ISIC classification (Section A.4). The share of each sector is defined as the share of the total employee workforce in the decile of the income distribution. To construct this graph, I partition the cross-country sample into ten groups of equal size, based on their level of per capita income. Note that I am limited to the group of countries where there exists sectoral data (see Table A.2). Within each group, I calculate the unweighted average sectoral shares by income decile. I plot this average profile for every group, and indicate the average per capita income of the group. I use expenditure-side real GDP at chained PPPs in 2011 US$ from the same year as the country-survey year. Source: Section 3.4 and Appendix Section A.4.

### A.5 Robustness of employee-income gradient in regression setting

In this subsection, I investigate the employee-income gradient in a regression setting. This serves two purposes. First, it provides a complementary method to the distributional profiles approach, to study the robustness of the employee-income gradient. Second, it provides a more formal setting to study which characteristics partially contribute to the steepness of the observed slope. I focus on three characteristics: sector, location, and education. These are individual characteristics that the government could, albeit imperfectly, seek to target for redistributive purposes. As such, if controlling for one such characteristic eliminates the employee-income gradient, this could suggest that the threshold in fact targets this confounding characteristic. At the same time, these are observable characteristics which vary over development, including from the sectoral movement from agriculture to manufacture to services; the rural-urban migration; and, the rise in higher education. As such, the partial reduction in magnitude due to controlling for a particular characteristic would be informative of the importance of this characteristic in quantitatively explaining the change in employee-income gradient over development.

I use the four sectoral categories described in the Section A.1.1. I further create a dummy variable equal to 1 if a respondent lives in an urban area. I do not attempt to harmonize this variable, and use the urban definition directly in the surveys. Finally, I use education variables to code four dummies, indicating if a respondent has: not completed primary; completed primary but not high school; completed high school. I chose to not distinguish further levels of education, in order to maximize the number of surveys where i could create consistent measures. The availability of these different variables is described in Table A.2.

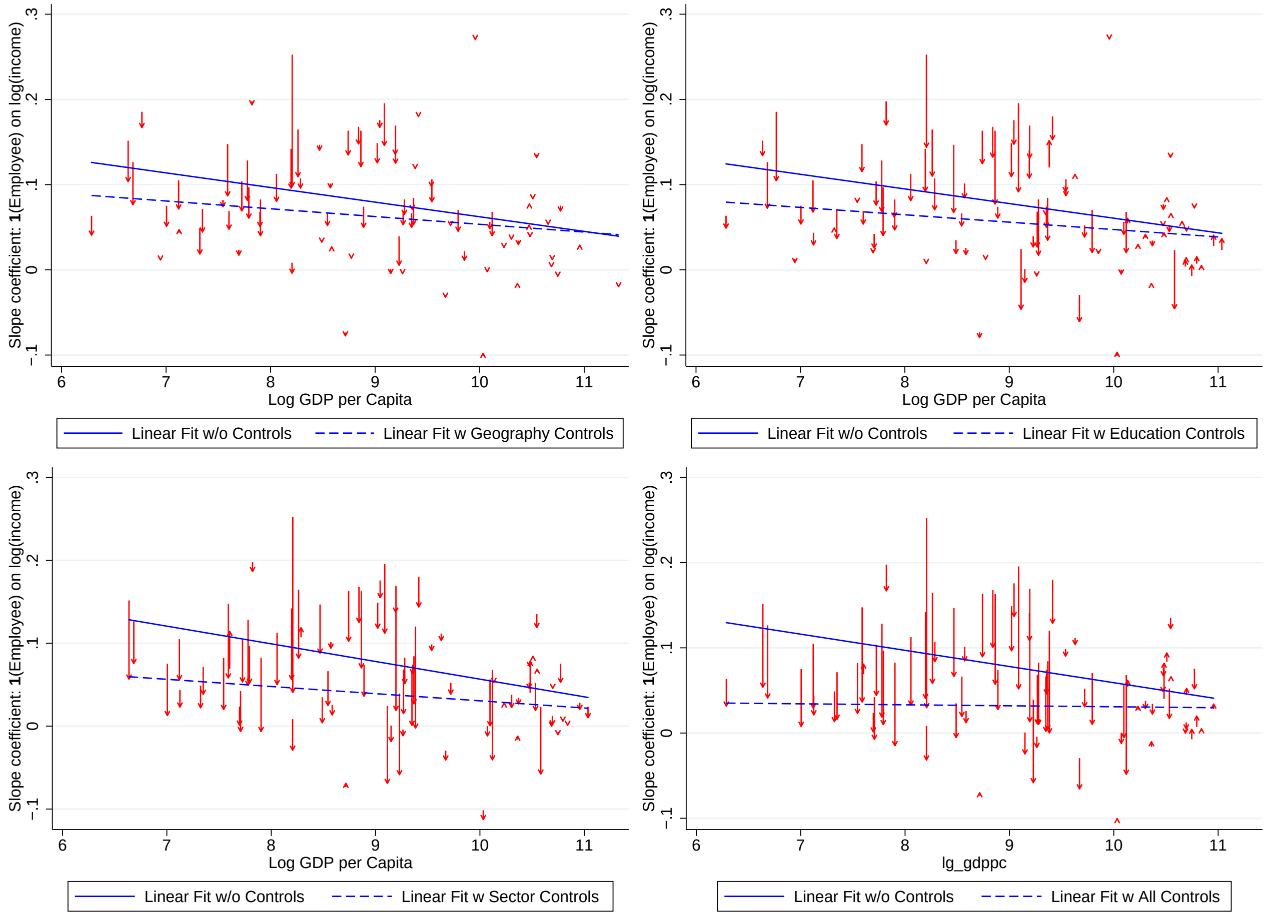
To visualize the impact of controlling for a characteristic on the employee-income gradient, I employ the methodology used in Bachas, Gadenne & Jensen (2019). In particular, in every country $c$, I estimate the following regression

$$\mathbf{1}(\text{Employee})_i = \alpha + \theta \boldsymbol{X}_i + \beta log(\text{income})_i + \varepsilon_i$$

where $\text{income}_i$ is the individual gross income of individual $i$ used to construct the income distribution (Section 3.2), $\mathbf{1}(\text{Employee})_i$ is a dummy equal to 1 if an individual is an employee (Section 3.2), and $\boldsymbol{X}_i$ contains the control indicator variables (sector, education, urban). I obtain a country-specific slope-coefficient $\beta^c$ from estimating this regression separately in every country. In every graph, I plot these coefficients $\beta^c_{with\,control}$ together with coefficients from estimating the regression without controls, $\beta^c_{no\,control}$, against log per capita income. The two coefficients for a particular country are denoted by the beginning ($\beta^c_{no\,control}$) and end ($\beta^c_{with\,control}$) of a vertical arrow. This regression is a linear probability model, which has the advantage that the slope-coefficient is directly interpretable. The disadvantage is that the slope-coefficient is not informative in settings where the relationship between employee and log(income) is strongly non-linear. This is the case in less (most) developed countries, where the likelihood of being an employee is very small (large) apart from the very top (top and bottom) of the income distribution (Figure 3). As an alternative, I can estimate the employee-share differential between the top and bottom deciles. This yields very similar qualitative results (not reported).

The results are displayed in Figure A.5. The top two panels control for geography (left graph) and education (right graph). The impact of geography is limited, but the inclusion of education significantly reduces the income-employee gradient especially in middle-income countries. The bottom left graph controls for sectors. This leads to the strongest reduction in magnitude, both in low-income and middle-income countries. It does not, however, fully eliminate the slope in most countries, and the potential confounding movement out of agriculture has been addressed in Appendix Section A.4. The bottom right graph includes all the control variables. This leads to a further reduction in slopes in most countries, compared to the sector control specification. This suggests that within sectors, location and, perhaps more likely, education, continues to be associated with higher income and employee-job status. Interestingly, the full set of controls almost fully eliminates the variation in the magnitude of employee-income gradient across development. This suggests that the joint movement over development of these three characteristics could drive the distributional employment patterns in stylized fact #1.

**Figure A.5: Employee-Income Gradients Across Countries Without and With Controls**




**Notes**: Each dot in every scatter-plot represents a country-specific slope coefficient based on estimating the regression in Section A.5. Each of the four graphs show slope-coefficients when including controls for: geography (North-West quadrant); education (NE); sectors (SW); geography, education, and sector (SE). In each graph, the start-point of an arrow represents the country-specific slope-coefficient without the control, and the end-point of an arrow represents the slope-coefficient after including the control. All slope-coefficients are plotted against log GDP per capita, measured using expenditure-side real GDP at chained PPPs in 2011 US$. In every graph, the solid (dashed) line represents the linear OLS fit of the slope-coefficients without control (with control). For more details on the construction of the different control variables, please see Section A.1.1. These graphs are constructed using the full cross-country survey sample. Source: Section 3.4 and Appendix Section A.5.

### A.6 Potential biases resulting from methodology

In this subsection, I discuss the potential biases that can arise from the survey methodology and the measurement and construction of variables. I code employment type based on the primary job in which the respondent spent the most hours during the reference period. Many individuals have many jobs at the same time (Banerjee & Duflo, 2007). But this will this will affect the representativeness of my estimates only to the extent that these jobs fall in different categories in my classification. An individual who contributes on the family farm while being an own-account worker within the same reference period would be classified as 'self-employed' in both jobs. In surveys where the reference period is not yearly, there may be bias in the measure of employment structure if the employment type in the reference period is not representative of the entire year. This is potentially important in developing countries, where there is strong seasonality in job type. This introduces bias to the extent that the jobs at different periods of the year fall in different employment structure categories, which I argue is unlikely in a developing country context. Indeed, individuals that are casual wage laborers during the harvest season are unlikely to be regular full-time employees in the non-harvest season. Rather, they are likely to be own-account workers or contributing family workers. In this case, the individual would be classified as self-employed during all periods of the year, despite the different jobs held at different periods of the year.

A second source of bias comes from the fact that I cannot systematically separate casual wage work from contract-based wage-work. I can always distinguish between working for someone for pay versus for in-kind payment, and I exclude the latter from the employee category. As such, casual wage-laborers that receive in-kind payment are systematically classified as self-employed. This leaves the group of casual workers that are not paid in-kind as the group that I potentially mis-classify as employee, whenever the survey answers do not provide sufficient precision about the nature of the employee-work. Since the transition over development involves a movement out of casual wage labor into contract-based wage labor, this mis-classification will lead me to under-state the true growth in employee-share along the development path.

Another potential source of bias arises from the possibility that self-employed misreport their true amount of income. This is unlikely to introduce a major bias, for three reasons. First, unlike on tax returns, self-employed do not directly have any incentive to mis-report their income to surveyors. Second, the model in Section 5 does predict under-reporting of income among self-employed locally around the exemption threshold. But while the standard bunching model predicts a steepened employee-share locally around the exemption threshold, it also predicts a decrease in employee-share further to the left of the threshold. This is not borne out in the data: instead, I observe a gradual increase in the employee-share over the full distribution. More generally, under-reporting of income by the self-employed would imply that the true self-employed distributional profile lies to the right of the observed one. If development is associated with increases in the ability to detect under-reporting among self-employed, this would generate gradual leftward shifts of the employee-share profile. Under-reporting of income by self-employed could also be due to by non-evasion motives. Woodruff et al. (2009) show that recall error, which is more present when the reference period is not annual, lead self-employed to under-estimate their income. If development is associated with a decrease in recall error, either due to changes in survey methodology or to an increase in accounting tools and book-keeping, this would similarly imply a rightward shift of the employee-profile at increasing levels of development. Both evasion detection capacity and measurement precision, which plausibly grow with development, therefore lead me to under-estimate the true progressive rightward shifts in the employee-profile due to structural transition out of self-employment.

Finally, bias could be introduced from the construction of the income tax base. I construct the tax base as the share of the individual income distribution that lies above the single-filer standard deduction (or allowance). As explained in the main text (Section 3.2), this choice is made to construct the tax base in the most transparent way without making any behavioral assumptions and in a way that can meaningfully be compared across countries. Notwithstanding, there exists features of tax systems which allow taxpayers to further reduce their tax liability, including deductible expenses. If a significant number of filers makes use of such additional features, this introduces a wedge between the size of base measured in this paper, and the size of the 'effective' base. The extent of existence of these features varies significantly across countries. Per example, there is a growing policy debate in the US on the large number of taxpayers that do not pay any Federal income tax. There exists no consistent evidence across countries at different levels of development on the extent to which the effective tax base is reduced through credits and deductions. Even if taxpayers in all countries in my sample made use of these deductions, it is likely that the size-wedge between my measured base and the effective base is larger in more developed countries. This is simply because the potential wedge in less-developed countries is bounded above by the small size of my measured base. In this case, I am overstating the variation in size of base across levels of development (Panel B, Figure 4). Perhaps more importantly, a size-wedge that increases with development means that I am understating the strength of the association between size of tax base and income tax collection (Panel B, Figure 5). This point is also supported by the observation that the variance in residual tax collection, controlling for the statutory size of tax base, is larger in more developed countries (Panel C, Figure 5). This discussion suggests that bias introduced by the wedge between my measure of the base and the effective size of tax base only strengthens the main finding of the tax base being a first-order determinant of tax collection across development.

# B US states material

## B.1 Data sources and construction of variables

In this sub-section, I describe the construction of variables used in the US states analysis (Section 4).

### B.1.1 Employment and earnings

I construct the aggregate employment-share variables using decennial Census data at the state level between 1930 and 2010. The data is extracted from IPUMS USA. In each decennial data-extract, I exclude from the sample any individual that is not economically active during the reference period and for whom the general class of worker variable is 0 ("N/A"). I also exclude, when possible, 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) a respondent who responds 'self-employed' ('works for wages') in the class of worker category. This classification in IPUMS USA is consistent with the classification used in the cross-development sample, in the sense that I code the employment-type based on the primary job of the respondent 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 employee respondents, and the total number of self-employed respondents. I then calculate the employee-share as the ratio of total number of employee respondents to the total number of employee and self-employed respondents. I interpolate the numerator and denominator between Census years using a natural cubic spline (Herriot & Reinsch, 1973).

I construct the employment shares by income decile of the income distribution of each state, in 1935 and in every decade between 1950 and 2010. The 1950-2010 data is 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, wage, farm and business components. I then apply person-weights and partition each state's income distribution into ten deciles (ten bins of equal sample size).

Within each decile, I estimate the conditional proportions of employees and self-employed to construct the employee-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 Section A.1.2. 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 again interpolate both the numerator and denominator between data-years.

The earning structure is constructed for all states and all years between 1929 and 2001 by combining the two historical series, namely 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 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', while the self-employed share of income uses line-item 70 'proprietors' income'. 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 income excludes transfers from all levels of government, similarly to the gross income variable used in the cross-development sample.

#### B.1.2 Tax revenue

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 tax-revenue collected to 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 a more standard construction of the variable, used in the cross-development sample, where the denominator use a measure of aggregate output. Unfortunately, continuous GDP data at the state-year level in the US is only available from 1963 onward. Instead, I follow previous papers studying growth in the US states (e.g. Barro and Sala-i-Martin, 1992; Besley et al., 2010) and use state personal income as a measure of state output. In the State Government Finances, T40 is the line-code corresponding to personal income tax; T41 corresponds to corporate net income tax; and, T09 corresponds to general sales tax.

#### B.1.3 Personal income tax structure: thresholds, rates, and 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 calculator 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 income tax threshold $K$ for an individual earner who files under the status of being single and who claims the standard deduction. This filng behavior is directly comparable to the filing behavior chosen to calculate the exemption threshold in the cross-development sample. As such, the measures of thresholds and income tax base are comparable between the US states time-series and the development cross-country series. The choice of a single earner, as opposed to household earnings, is also consistent with the income distribution which is calculated based on ranking of total personal earned income. Finally, an appealing feature of the standard deduction is that, unlike the itemized deduction, the filer does not deduct state personal income tax from her federal income tax liability. This provides additional incentives for the filer to under-report state income taxes, and makes the filing-choice more similar to the under-reporting model derived in Section 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 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. I use the marginal tax rate that applies on the first bracket in Table 3.

The measure for income tax reforms is coded in the following way. States began in the 1980s to automatically adjust the nominal values of the exemption threshold (and rate-brackets) for inflation. Prior to this period, no state provided inflation adjustments. Prior to the 1980s, the dollar value of the calculated threshold $K$ would therefore remain constant unless a legislative reform occurred which changes the value of the exemption threshold. I therefore code a year of reform as a year, before 1980, during which the nominal value of the threshold changed. I then construct the state-specific cumulative series of exemption reforms over time. I use this measure of reform likelihood in Panel A of Figure 7. In a graph that pools several States, the cumulative distribution measure has the advantage of controlling for cross-state heterogeneity in the frequency of threshold reforms.

#### B.1.4 Covariates

The poll tax and literacy test 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. Besley et al. use variation in these dummies to instrument for political competition, which they find to have a significant impact on the share of non-farm income and tax revenues. I also use the election year dummies from Besley et al.

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 stat corporate income tax is taken from Table 4.1 of Newman (1984). The dummy for the existence of right-to-work laws is 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 by the US Department of Labor. 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. 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 earlier 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 onward. I also wanted to code the employer UI-contribution, expressed as a percentage of wages, but this measure is not consistently reported throughout.

#### B.1.5 Additional outcome variables

I construct a proxy for tax administrative reforms based on the historical series of the Book of the State, published annually from 1993 until today by the Council of State Governments. 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. This variable is intended to proxy for investments in enforcement capacity, through consolidation of the number of tax agencies, and is used in the robustness checks (Table 3). I also collected state-year data from the same source on the annual salaries of the chief state administrative official in different departments: revenue-collection and taxation; treasury; attorney general. I then constructed the ratio of the annual salary in revenue-taxation relative to the salary in the Treasury and to the salary as Attorney General. These ratios were meant to proxy for investment in enforcement capacity through funding higher wages to tax administrators (relative to other state administrators). In results not reported, I do not find an impact of the upholding event on this measure of relative pay. These variables represent, to my knowledge, the first long-run time-series on proxies for tax administrative capacity of individual states in the US. As an additional proxy for enforcement capacity, I code the year when each state adopted withholding of state personal income taxes by employers. There exists both micro-evidence from Denmark (Kleven et al., 2011) and state-level evidence from US states (Dusek & Bagchi, 2017) on withholding's positive impacts on income tax collection. I use the historical IRS 'Annual Report' series to code the years of adoption. This variable is used in robustness checks (Table 3).

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. Further, I use the Democratic vote-share averaged across all state-wide elections, and the Democratic seat-share in the state House. These measures are used as outcome variables in robustness checks (Table 3).

In the robustness checks (Table 3), I also study the impact of upholding on the generosity of the state's unemployment benefits. In particular, I use the measure of state maximum unemployment benefits. This variable is taken from the 'Correlates of State Policy' database (Jordan & Grossman, 2017).

Finally, in the robustness checks (Table 3), I study the impact of upholding on level of income, and income inequality. I use the 'net earnings' measure of income from BEA, and the top 1 percent income share from Frank et al. (2015).

#### B.1.6 Exchange of information agreements

In the main heterogeneity analysis (Table 2), I study whether the impact of upholding on tax structure and collection differs according to whether a state has an exchange of information agreement in place by the time of the court upholding decision. I code the year of implementation of the agreement from the historical IRS series 'Annual Report.' The signature of the exchange of information acts has been found to increase income tax revenue (Troiano, 2017). Troiano's source for the year of implementation is Penniman (1980). There are only minor differences in the year of implementation between the annual IRS publication series and Penniman (1980), and my results are robust to using this alternative measure of implementation dates.

#### B.1.7 Cost of collection

I construct the measure of cost of collection used in Section 4.1 from the Book of the States. The earliest year where the required data exists is 1962. The cost-components of collecting state taxes are capital outlays, operating costs, and payroll. In 1962, these measures exist for the state's financial administration, which includes the revenue administration and the procurement administration. As such, my cost measure constitutes an upper bound, since I cannot separate the administrative costs of the revenue division from the procurement division. I divide this total cost by the total gross tax revenue collected within the same financial year. This measure of cost of collection is similar in construction to Jensen & Lagakos (2019), which studies variation in cost of national tax administrations across levels of development. Interestingly, I find that the cost of collection in the average US state in 1962 is slightly higher than the average low-income country's tax administration from Jensen & Lagakos (2019).

### B.2 Program details: Industrial Development Bonds

In this sub-section, I provide additional information on the Industrial Development Bonds program (IDB), and the legal uncertainty which generates variation at the state-level in the effective implementation date.

The IDB was a place-based local development program that sought to attract industrial facilities to predominantly rural areas characterized by 'surplus labor' concentrated among self-employed farmers (Advisory Commission on Intergovernmental Relations, 1962). The first state to implement an IDB program was Mississippi, when it launched 'Balancing Agriculture with Industry' in 1936. Practice of IDB did not, however, become a multi-State practice until the mid-1950s, when several other States decided to implement similar programs.

The official justification for government intervention was that these rural areas were "deficient in credit facilities" (ACIR, 1962), and capital for local firms was not readily available from conventional credit sources. Through the IDB, the local government therefore sought to relieve a local credit constraint. In the IDB program, sub-state government units (counties, boroughs, and cities) issue bonds to finance the acquisition or construction of facilities and equipment for lease to private firms. Importantly, IDB issuances were revenue bonds, which are secured exclusively by the revenues of the project. This is in contrast to general obligation bonds, which are secured by the credit of the issuer - in this case, the local government. This distinction implies that there is not a direct relationship between the issuance under the IDB program and increased tax revenue due to a need to solidify the local government's funding capacity.

The interest received from IDB securities was exempt from Federal income taxes. This meant that IDB securities commanded more favorable terms in the financial markets in relation to corporate securities with comparable risk. The Federal exemption is thought to have been one of the main reasons behind the growth of the IDB market. The growth of IDB issuances in the late 1960's implied an amount of forgone Federal government tax revenue which became intolerable. This triggered legislation in the early 1970s to remove the IDB exemption for Federal tax purposes and and to significantly limit the per issuance volume of IDB. These reforms also significantly widened the scope of projects that could be approved under municipal bond projects, with a shift away from rural industrialization towards public-goods projects in infrastructure and environmental conservation.

For identification purposes, I exploit the institutional features of implementation. In particular, the particular methods under IDB were unprecedented in the context of postwar state financing. The use of public credit for an otherwise private purpose was considered to be in direct violation of the public purpose doctrine, which prohibits such usage. Constitutions of many States explicitly contained such public purpose statutes. The implementation of IDB therefore required, in a first instance, a legislative vote of constitutional or statutory provision that authorizes industrial development financing.

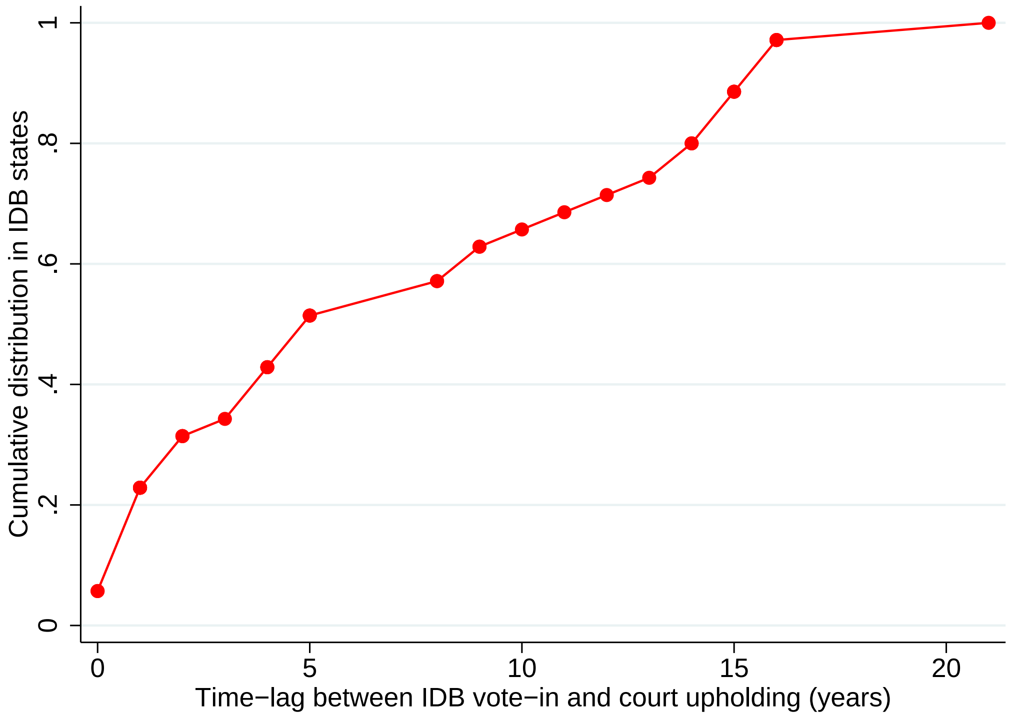
The lack of historical precedent, however, meant that the voted provision required judicial testing in order to be effectively implemented. Indeed, investors were reluctant to hold IDB securities in the period where the legality of the voted state provision had not been confirmed in the state's judicial system (Cobb, 1993). Judicial testing was most often delivered by a court case brought before the State's supreme court. This court case could be triggered in several ways. Most often, the issuance of an IDB required a significant amount of pre-issuance preparation, including a detailed description of the local workforce needs and a justification for why a particular candidate private firm would satisfy those needs. These preparations were often done by a local government agency, created specifically for this purpose. The case would then be brought against the legality of this local development agency. More generally, any legal step required to issue IDB could be targeted in a court case. In several States, including Tennessee, the IDB statute featured the requirement of a vote of approval by the relevant electors as a special municiapl election. The court case could also directly involve the issuance of an IDB bond itself. But as the graphical evidence in Panel A of Figure 7 shows, this was only very rarely the case.

In several instances, the fact that IDB were issued as revenue, rather than general obligation, bonds, was the basis of the argument for not violating the 'credit for public purpose' doctrine. In the case of Wayland v. Snapp, the Arkansas Supreme Court "(...) chose to uphold the issuance of the revenue bonds by invoking the doctrine that revenue bonds do not violate a credit clause because they are retired through lease revenues of the project, not out of tax funds" (Yale Law Journal, 1961).

I collect information on the dates of the legislative vote and the upholding from several sources. Importantly, I collect information from both administrative sources and legal reviews: Abbey (1965), ACIR (1963), Pinsky (1972), and Economic Development Administration (1978). The date for the vote is the year of appearance of the constitutional statute or provision authorizing local development financing. The date for the upholding event is the publication year of the leading case that upholds the constitutionality of the statute or provision. There is only little conflict in the reported dates of the vote and the upholding between the administrative and legal sources. In the case of upholding, there are sometimes several leading cases, when the first case upholds the constitutionality of the statute allowing cities to issue development bonds, and the second (later) case extends these powers to counties. I always choose the earliest date across sources for both the vote and the upholding events.

Table B.1 provides information for each IDB state in my time-period of study. The table reports the year of vote and the year of upholding that I use in the main analysis. In Figure B.1, I plot the cumulative distribution of the time-lag between the vote-year and the upholding-year. The average lag is 6.67 years, with a standard deviation of 6.77. In just under 40 percent of States, the time-lag exceeds 10 years. In the main analysis, my estimation is helped by the existence of a significant lag between the vote and the upholding events within state; the variance in lag across States; and, the differential timing of court upholding decisions across States.

**Figure B.1: Time-Lag Between Vote and Upholding Events Across IDB States**




**Notes**: This graph displays the empirical cumulative distribution function of the time-lag in years between the vote event and the upholding event within each State that has upheld IDB by 1980. The year of the vote is the year where the State legislature voted in a statute or provision authorizing IDB. The year of the uphold event is the year where the State supreme court upheld the legality of the voted IDB statute or provision through a leading court case. The time-lag is defined as the difference in years between these two events. Source: Section 4.1 and Appendix Section B.2.

**Table B.1: Industrial Development Bonds Program Legal Timing**

| State | Year vote | Year uphold | Leading court case |
|---|---|---|---|
| **Alabama** | 1949 | 1950 | Newberry v. City of Andalusia, 257 Ala. 49, 57 So. 2d 629 |
| **Arizona** | 1963 | 1973 | Industrial Development Authority of Pinal County v. Nelson, 109 Ariz. 368, 509 P. 2d 705 |
| **Arkansas** | 1958 | 1960 | Wayland v. Snapp, 232 Ark. 57, 334 S.W. 2d 633 |
| **Colorado** | 1955 | 1970 | Allardice v. Adams County, 173 Colo. 133, 476 P. 2d 982 |
| **Delaware** | 1961 | 1962 | In re Opinion of the Justices, 177 A. 2d 205 |
| **Georgia** | 1957 | 1970 | In re Opinion on Sub. H. B. 24 |
| **Illinois** | 1951 | 1972 | People ex rel. City of Salem v. McMackin, 53 Ill. 2d 347 |
| **Iowa** | 1963 | 1964 | Green v. City of Mount Pleasant, 131 N.W. 2d 5 |
| **Kansas** | 1961 | 1962 | State ex rel. Ferguson v. Pittsburgh, 364 P. 2d 71 |
| **Kentucky** | 1946 | 1950 | Faulconer v. City of Danville, 313 Ky. 468, 232 S.W. 2d 80 |
| **Louisiana** | 1952 | 1954 | Miller v. Washington Parish, 75 Southern So. 2d 394 |
| **Maine** | 1958 | 1966 | Northeast Shoe Company v. Industrial and Recreational Finance Approval Board, 233 A. 2d 423 |
| **Maryland** | 1960 | 1974 | Wilson v. Board of County Commissioners of Allegheny County, 273 Md. 30, 327 A. 2d 488 |
| **Michigan** | 1963 | 1966 | City of Gaylord v. Beckett, 144 N.W. 2d 460 |
| **Minnesota** | 1961 | 1970 | City of Pipestone v. Madsen, 178 N.W. 2d 594 |
| **Mississippi** | 1936 | 1944 | Albritton v. City of Winona, 178 So. 799 |
| **Missouri** | 1960 | 1975 | Atkinson v. Planned Industrial Expansion Authority of St. Louis, 517 S.W. 2d 36 |
| **Montana** | 1965 | 1970 | Fickles v. Missoula County, 470 P. 2d 287 |
| **Nebraska** | 1960 | 1962 | State ex rel. Meyer v. County of Lancaster, 113 N.W. 2d 63 |
| **Nevada** | 1959 | 1973 | State ex rel. Brennan v. Bowman, 512 P. 2d 1321 |
| **New Hampshire** | 1955 | 1971 | Opinion of the Justices, 278 A. 2d 357 |
| **New Mexico** | 1955 | 1956 | Village of Deming v. Hosdreg Co., 62 N.M. 18, 303 P. 2d 920 |
| **North Dakota** | 1955 | 1964 | Gripentrog v. City of Wahpeton, 126 N.W. 2d 230 |
| **Ohio** | 1955 | 1966 | State v. Greater Portsmouth Growth Corporation, 218 N.E. 2d 446 |
| **Oklahoma** | 1960 | 1961 | Application of The Oklahoma Industrial Financial Authority, 360 P. 2d 720 |

**Table B.1: Industrial Development Bonds Program Legal Timing (end)**

| State | Year vote | Year uphold | Leading court case |
|---|---|---|---|
| **Pennsylvania** | 1956 | 1968 | Basehore v. Hampden IDA and Walker v. Butler County IDA, 248 A. 2d 212 |
| **Rhode Island** | 1958 | 1974 | In re Advisory to Governor, 324 A. 2d 641 |
| **South Carolina** | 1962 | 1967 | Elliott v. McNair, 156 S.E. 2d 421 |
| **South Dakota** | 1964 | 1968 | Clem v. City of Yankton, 160 N.W. 2d 125 |
| **Tennessee** | 1951 | 1952 | Holly v. Elizabethon, 241 S.W. 2d 1001 |
| **Utah** | 1953 | 1968 | Allen v. Toole County, 445 P. 2d 994 |
| **Virginia** | 1962 | 1967 | Industrial Development Authority of the City of Chesapeake v. Suthers, 208 Va. 51 155 S.E. 2d 326 |
| **West Virginia** | 1963 | 1964 | State ex rel. Marion County v. Demus, 135 S.E. 2d 352 |
| **Wisconsin** | 1957 | 1973 | Hammermill Paper Co. v. LaPlante, 205 N.W. 2d 784 |
| **Wyoming** | 1963 | 1967 | Uhls v. State, 429 P. 2d 74 |

**Notes**: This table provides details on the legal timing of the IDB program in all States. The year of the vote is the year where the State legislature voted in a statute or provision authorizing IDB. The year of the uphold event is the year where the State supreme court upheld the legality of the voted IDB statute or provision through a leading court case. This leading court case is indicated in the final column of the table. The years of the vote and upholding event are drawn from administrative and legal reviews. Source: Section 4.1 and Appendix Section B.2.

### B.3 Robustness of main regression results

In this sub-section, I provide robustness checks for the main results in the US states regression setting. In Table B.2, I maintain the same specification as in the main text, but consider alternative measures of the main outcome variables. In the main text, I studied the impact of the upholding event on employment structure using the employee-share of the active workforce. The disadvantage of this variable is that it is interpolated between Census years. In Column 1, as an alternative I use the employee-share of income. This variable is drawn from the SA5H BEA series and is continuous throughout the sample period. The variable is constructed as the ratio of total wages and salaries to total resident income.[3] Column 1 indicates that the upholding event led to a large and significant increase in the employee-share of income, in line with the main finding of an increase in the employee-share in employment.

The final three columns of Table B.2 investigate the robustness of the absence of a per capita income effect. In column 2, I use the BEA 'net earnings by place of residence', which equals total earnings less contributions for government social insurance plus 'adjustment for residence'. In Column 3, I use the Census-based measure of total personal income. This income measure is interpolated between Census years. In Column 4, I use the IRS-based measure of income, adjusted gross income (AGI), drawn from the top-income share series in Frank et al. (2015). I find an insignificant impact of both the upholding event and the vote-in event across these three alternative measures of income. Both the BEA and Census measures suggest an insignificant positive impact, while the IRS measure suggests an insignificant, but negative impact. The absence of an impact on income at the state-level using various measures is consistent with the regressions in Section B.4 which also fail to detect a per capita income impact, but at the local county-level. Note that the absence of an impact on income in this context is not inconsistent with other place-based program evaluations which have found positive development impacts. Indeed, the findings in those studies, including Kline & Moretti (2014), are based on long-run estimates, while my estimates only capture the short-run program impacts.

In Table B.3, I consider the robustness of the impact on employment-structure to alternative specifications. Column 1 replicates the result from the main specification. In Column 2, I remove the vote-in dummy from the main specification. The counterfactual is now entirely built from states that uphold IDB at a later date. This has only a minor impact on the estimated coefficient, which changes from 1.7 percentage points to 1.5 percentage points. In Column 3, I remove the main covariates from the main specification, which are the first stage instruments used in Besley et al. (2010), and election year

[3] It was also used in the graphical evidence in Figure 7.

dummies. This has no effect on the estimated coefficient. In Column 4, I augment the main specification with additional controls. These additional controls are plausible determinants of employment-structure, but were not included in the main specification because of their potential endogeneity. They are: log per capita income; an indicator for the existence of right to work laws; an indicator for the existence of a corporate income tax; and, a firm-size measure of the state's unemployment insurance. The sources and construction of these variables is described in Section B.1. The inclusion of these controls has no impact on the main estimate.

In Column 5, I allow for the determinants of the time-lag between the vote-in and upholding event to have an independent impact. In particular, Table 1 showed that the time-lag was shorter in states with civil law origins, and longer in states that had witnessed defaults for a historical public-private funding initiative. While these are state-specific but time-invariant characteristics, they may nonetheless be correlated with state-time varying determinants of employment structure. This would confound the impact of the upholding event. I therefore allow civil law states and historical default states to be on fully non-parametric time-paths throughout the sample period. Formally, I estimate

$$\begin{aligned} y_{st} =& \beta + \alpha \mathbf{1}\left(\text{Vote-in}\right)_{st} + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \bar{}_t \left(\mathbf{1}\left(\text{Civil Law}\right)_{\mathbf{s}} \times \gamma_t\right) \\ &+ \text{ß}_t \left(\mathbf{1}\left(\text{Hist Default}\right)_{\mathbf{s}} \times \gamma_t\right) + \lambda X_{st} + \mu_s + \gamma_t + \varepsilon_{st} \end{aligned}$$

where all variables are defined as in the main text, and $\mathbf{1}\left(\text{Civil Law}\right)_{\mathbf{s}}$ and $\mathbf{1}\left(\text{Hist Default}\right)_{\mathbf{s}}$ are indicators taking a value of 1, respectively, if a state is has civil law origins or has experienced a historical default. The construction of these variables is described in the main text (Section 4.2). The inclusion of these time-paths marginally reduces the estimated coefficient on the upholding event, from 1.7 to 1.5 percentage points, which remains strongly statistically significant.

In Column 6, I investigate the possibility that my main control specification does not adequately capture differential convergence patterns in employment structure over time across states. Indeed, the IDB-implementation period was characterized by rapid structural convergence for the less-developed states in the US (Barro & Sala-i-Martin, 1992; Caselli and Coleman, 2001). To investigate this, I augment the main specification with an interaction between a linear time-trend and the cross-sectional level of state GDP per capita in 1940. That is, I estimate

$$y_{st} = \beta + \alpha \mathbf{1}\left(\text{Vote-in}\right)_{st} + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \lambda \mathbf{X}_{st} + \mu_s + \gamma_t + \text{ß}\left(\text{Initial\_Income}_{s1940} \times [t - 1940]\right) + \varepsilon_{st}$$

where variables are defined as in the main text, and where $\text{Initial\_Income}_{s1940}$ is the cross-section of initial GDP per capita in 1940, which is interacted with a linear time trend $[t - 1940]$. This leads to only a very marginal reduction in the magnitude of the estimated impact of upholding.

Finally, I show that the results are robust to excluding the set of IDB states which were implementing the Tennessee Valley Authority program (Kline and Moretti, 2013), a concurrent Federal place-based development program. The joint IDB-TVA states are: Alabama, Kentucky, Mississippi, and Tennessee. I remove these states from the sample, and re-estimate the main specification on the reduced sample. Column 7 shows that this leads to no meaningful change in the estimated impact of the upholding event.

**Table B.2: Alternative Measures of Employment and Income**

| | E-share of income | Avg Income (BEA) | Avg Income (Census) | Avg Income (IRS) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **1**(Vote) | .000 | 23.369 | 38.807 | -684.822 |
| | (.004) | (29.475) | (43.544) | (978.986) |
| **1**(Uphold) | .013 | .424 | 59.052 | -624.991 |
| | (.006)** | (33.114) | (55.131) | (1115.546) |
| Mean outcome variable | .707 | 1016 | 2003 | 1596 |
| State FE | x | x | x | x |
| Year FE | x | x | x | x |
| State-year controls | x | x | x | x |
| States | 28 | 28 | 28 | 28 |
| State-year Obs | 466 | 466 | 466 | 466 |

**Notes**: This table reports results from estimating the following regression

$$y_{st} = \beta + \alpha\mathbf{1}\left(\text{Vote-in}\right)_{st} + \theta\mathbf{1}\left(\text{Upheld}\right)_{st} + \beta\mathbf{1}\left(\text{Upheld}\right)_{st} \times \mathbf{1}\left(\text{EoI}\right)_{st} + \lambda\mathbf{X}_{st} + \mu_s + \gamma_t + \varepsilon_{st}$$

where $s$ denotes state and $t$ 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 State court system has upheld the constitutionality of the voted IDB statute or provision. The vote-in and upholding events are mutually exclusive. $\mathbf{1}\left(\text{EoI}\right)_{st}$ is an indicator variable taking a value of 1 when a State has passed an exchange of information agreement with the Federal Internal Revenue Service. In Columns 1, the outcome variable is the wage and salary share of of individual income, drawn from BEA historical data. In Column 2, the outcome variable is 'net earnings by place of residence', which is the BEA concept of personal income. It is equal to total earnings less contributions for government social insurance plans plus a residence adjustment. In Column 3, I use the measure of gross personal income from the decennial Census. This measure is interpolated between Census year, using a natural cubic spline. In Column 4, I use the IRS measure of income, adjusted gross income, which is drawn from the top income share series (Frank et al., 2015). The state-year controls, $\mathbf{X}_{st}$, are indicator variables for election year, and indicator variables for the existence of voting restrictions in the form of poll tax and literacy tests. These are the first stage instruments used by Besley et al. (2010) to study political competition and policy-making in US states. $*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. Source: Section 4.5 and Appendix Section B.3.

## Table B.3: Alternative Specifications

| | Employee-share of employment | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **1**(Vote) | .003 | | .004 | .003 | .004 | .004 | .004 |
| | (.005) | | (.005) | (.005) | (.006) | (.005) | (.006) |
| **1**(Uphold) | .017 | .015 | .017 | .017 | .015 | .016 | .018 |
| | (.005)*** | (.005)*** | (.005)*** | (.005)*** | (.006)** | (.005)*** | (.006)*** |
| Specification | Main | Cross-sectional only | No controls | Extensive controls | Time path civil law states | Initial income time-trend | Exclude TVA states |
| Mean outcome variable | .707 | .707 | .707 | .707 | .707 | .707 | .777 |
| State FE | x | x | x | x | x | x | x |
| Year FE | x | x | x | x | x | x | x |
| States | 28 | 28 | 28 | 28 | 28 | 28 | 24 |
| State-year Obs | 466 | 466 | 466 | 466 | 466 | 466 | 409 |

**Notes**: this table reports results from estimating alternative specifications, described in detail in Section B.3. In all regressions, the outcome variable is the employee-share of the economically active workforce. Column 1 replicates the central finding from estimating the main specification (1). Column 2 removes the indicator variable for the vote event from the main specification. Column 3 removes the controls from the main specification. Column 4 augments the main specification with additional controls: log per capita income; an indicator for the existence of right to work laws; an indicator for the existence of a corporate income tax; and, a firm-size measure of the state's unemployment insurance. Column 5 augments the main specification with a full set of year indicator interactions with both the indicator for civil law origins and the indicator for historical rail default. Column 6 augments the main specification with an interaction between a linear time-trend and the cross-section of GDP per capita in 1940. Finally, Column 7 estimates the main specification, but on a reduced sample which excludes the four States (Alabama, Kentucky, Mississippi, Tennessee) which were part of the Tennessee Valley Authority development program. $*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. Source: Section 4.5 and Appendix Section B.3.

### B.4 Local IDB impacts: evidence from county-level regressions

In this subsection, I study the impact of the IDB program at the county-level. While the effective start of the program is triggered at the level of the state supreme court (Sections 4.1- 4.2), the decision itself to issue IDB is predominantly made by counties within the state.[4] As such, a county-level analysis provides an assessment of the direct local economic impacts of the program. At the same time, the county-level analysis at the level of local implementation helps to shed light on the absence of an economically meaningful impact of IDB on non-employment outcomes.

In order to study the county-level impacts of IDB, I rely on two main data-sets. The first is the comprehensive county-level panel data-set ICPSR 2896 'Historical, Demographic, Economic, and Social Data: The United States, 1790-2002.' This data-set has been used in other studies of long-run impacts of local place-based development programs, including the Tennessee Valley Authority program (Kline & Moretti, 2013). I interpolate values between data-points in the ICPSR data-set. Note that during my period of interest, the primary source in ICPSR is the County Data Book. Since this source delivers data every five years, the interpolation period is smaller than between decennial Census which is used in the main analysis. Nonetheless, the data has the disadvantage that it does not contain a continuous measure of per capita income, which is the main object of interest in this county-level analysis. I therefore supplement it with a second county-year panel data-set. This data-set is the combination of the continuous BEA county-level per capita income data, which exists from 1969 onward; and, the 1959 Census module which measures per capita income in the cross-section of all counties. While I do have to interpolate per capita income between 1959 and 1969, this data-set nonetheless gives me a more naturally continuous measure of per capita income than the ICPSR data-set. The only disadvantage is that my sample only starts in 1959, while 7 states have voted in the IDB program before that date. The county-level analysis is therefore limited to the counties in the 21 states that vote in after 1959.

The aim is to investigate the impacts of the IDB by comparing counties with the program to counties without the program. The basis for this exercise remains the specification used in the main text, which assesses impacts by comparing changes before and after the upholding event, while controlling for any impact occurring during the vote in event. But without any additional modifications, this specification would rely on counties in different states as a counterfactual. Instead, I want to create a control county

[4] The decision could also be made by higher tiers of government, such as the the state government, or lower tiers of government, including cities. Data from Moody's Investor Service (1974) suggests that in practice, actual issuance was predominantly carried out by counties.

within the same state. This is a meaningful exercise since the IDB program was initiated at the county level and only a subset of counties in a given state would initiate IDB.

I assign treatment at the county-level within the state based on a list created by the federal government before IDB had become widely implemented. The Area Redevelopment Administration (ARA) is a federal agency that was created in 1961, with the aim of providing technical (data-driven) assistance to state and local governments to implement local development financing. For this purpose, the ARA created criteria that defined 'redevelopment areas.' These were predominantly rural geographical areas, characterized by "structural underemployment", where the encouragement of new industries was perceived as a solution to the stagnant levels of development (ARA, 1962). This characterization is effectively identical to the characterization of counties that IDB was targeted towards. In every state, the ARA compiled data from Census and the Departments of Health, Education, Welfare, and Agriculture, to establish a statistical profile of every county in 1961, and classify a subset of those as 'redevelopment areas.'

I digitize the list of 'redevelopment' counties based on the 'Statistical Profiles' in every state (ARA, 1961), and merge it with the main county panel data-set. This list has the appealing feature that it was created by a government entity which was not responsible for implementing IDB in the pre-IDB period. As such, the selection of counties into the list may be considered plausibly exogenous to unobservable county-time varying confounding determinants of local development.

I augment the empirical specification used in the main text with this list to create a difference-in-differences design. More specifically, I consider the ARA 'redevelopment' status to be a county-specific time-invariant assignment to program treatment. Since there exists counties on the ARA list that do not take up IDB, and there exists counties not on the ARA list that can take up IDB, this is an intent-to-treat design. The diff-in-diff evaluation will compare changes in outcome in ARA counties before and after the court upholding event to changes in outcomes in non-ARA counties within the same state, while controlling for any impacts that occurred during the vote-in event. Formally, I estimate

$$\begin{aligned} y_{cst} =& \beta + \alpha \mathbf{1}\left(\text{Vote-in}\right)_{st} + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \pi\left(\mathbf{1}\left(\text{Vote in}\right)_{st} \times \mathbf{1}\left(\text{ARA}\right)_{c}\right) \\ &+ \phi\left(\mathbf{1}\left(\text{Upheld}\right)_{st} \times \mathbf{1}\left(\text{ARA}\right)_{c}\right) + \mu_c + \gamma_t + \varepsilon_{cst} \end{aligned} \tag{1}$$

where $y_{cst}$ is the outcome of interest in county $c$, in state $s$, at time $t$, $\mathbf{1}\left(\text{Vote-in}\right)_{st}$ and $\mathbf{1}\left(\text{Upheld}\right)_{st}$ indicate whether a state has, respectively, voted in but not upheld or upheld the IDB program. $\mathbf{1}\left(\text{ARA}\right)_{c}$

is a county-specific, time-invariant indicator that takes value 1 if a county is on the ARA federal list of redevelopment areas, and $\mu_c$ and $\gamma_t$ are county and year fixed effects, respectively. I cluster the standard error at the state level, to allow for spill-over between ARA and non-ARA counties within the same state. The time-window is identical to the one used in the main estimation (Tables 2-3): in every IDB state that upholds before 1971, I consider the time-period that ranges from 5 years before the vote-in event to 5 years after the upholding event.

The results are displayed in Table B.4. In the first column, I study the employee-share of the active workforce as the outcome variable. I find that the large, positive impact is concentrated in the ARA counties in the upholding period. In the following two columns, I find no overall impacts on the level of employment and urbanization. The absence of impacts on these two outcomes is consistent with the interpretation that the IDB program achieved its stated objective of reducing underemployment in specifically targeted rural areas. Issuance of IDB required documenting the specific local industrial needs of a county and a justification for why the size and characteristics of the proposed IDB facility would achieve this local need. In comparison to other place-based development programs, IDB was therefore highly targeted in nature and narrow in scope, aiming to finance industrial development commensurate with the specific local workforce needs. The increase in the employee-share and the absence of an impact on the size of the workforce suggests IDB primarily provided a transition into employee-jobs of 'underemployed', self-employed farmers. The absence of any change in urbanization suggests that workers did not migrate to the predominantly rural areas where the IDB facilities were being opened. Consistent with the absence of generalized economic impacts, the final two columns find no statistically significant impacts per capita income. The fourth column uses family income, measured in the ICPSR data-set, while the fifth column uses the continuous BEA per capita income measure. The impact on the continuous measure of per capita income is particularly insignificant, both statistically and economically.

Taken together, these county-level results provide additional evidence to support the absence of any meaningful non-employment development impacts, in the short-run 5-year window considered in this estimation strategy. In particular, the IDB program seems to have led to a significant transition from self-employment to employee-jobs but only locally in the specifically targeted IDB counties. The absence of any spill-over to work structure or workforce attachment in non-treated counties suggests sectoral re-allocation was limited. The absence of any change to levels of urbanization suggests migration from non-treated to treated counties was also limited. The compensation for migration costs and the efficiency gains from sectoral re-allocation are two of the main mechanisms through which previous studies have found long-run positive income impacts of place-based programs. These mechanisms seem to not be significant forces in the IDB context in the 5-year short run.

**Table B.4: County-Level Evidence on Local Impacts of IDB**

| | E-share | Employment | Urbanization | Log(Family Income) | Log(Personal Income) |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **1**(Vote) | -.007 | -.003 | -.003 | -.016 | .031 |
| | (.006) | (.002) | (.003) | (.030) | (.043) |
| **1**(Vote)***1**(ARA) | .003 | -.001 | -.001 | -.007 | -.019 |
| | (.007) | (.003) | (.009) | (.033) | (.038) |
| **1**(Uphold) | -.009 | -.004 | -.003 | -.044 | -.003 |
| | (.008) | (.003) | (.005) | (.047) | (.072) |
| **1**(Uphold)***1**(ARA) | .031 | -.002 | .000 | .044 | -.008 |
| | (.010)*** | (.004) | (.010) | (.040) | (.051) |
| County FE | x | x | x | x | x |
| Year FE | x | x | x | x | x |
| States | 21 | 21 | 21 | 21 | 21 |
| County-year obs | 5140 | 5140 | 5140 | 5140 | 5140 |

**Notes**: This table reports the results from estimating the following regression

$$\begin{aligned} y_{cst} = &\beta + \alpha \mathbf{1}\left(\text{Vote in}\right)_{st} + \theta \mathbf{1}\left(\text{Upheld}\right)_{st} + \pi\left(\mathbf{1}\left(\text{Vote in}\right)_{st} \times \mathbf{1}\left(\text{ARA}\right)_c\right) \\ &+ \phi\left(\mathbf{1}\left(\text{Upheld}\right)_{st} \times \mathbf{1}\left(\text{ARA}\right)_c\right) + \mu_c + \gamma_t + \varepsilon_{cst} \end{aligned}$$

where $y_{cst}$ is the outcome of interest in county $c$, in state $s$, at time $t$, $\mathbf{1}\left(\text{Vote in}\right)_{st}$ and $\mathbf{1}\left(\text{Upheld}\right)_{st}$ indicate whether a state has, respectively, voted in but not upheld or upheld the IDB program. $\mathbf{1}\left(\text{ARA}\right)_c$ is a county-specific, time-invariant indicator that takes value 1 if a county is on the ARA federal list of redevelopment areas, and $\mu_c$ and $\gamma_t$ are county and year fixed effects, respectively. The outcome variables are: employee-share of employment; economically active employment share of population; urbanization share of population; log family income; and, log personal income. The first 5 outcomes are drawn from ICPSR 2896, while the final outcome variable is constructed from historical BEA series and the 1959 Census. The sample is limited to counties in the 21 IDB States that vote in IDB statutes or provisions after 1959. $*, **, ***$ denote significance at the 10%, 5%, 1% level. Robust standard errors clustered at the state level in parentheses. Sources: Appendix Section B.4.

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