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Essays in Public Finance and Political Economy

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I confirm that Chapter 2 was jointly co-authored with Tim Besley, and I contributed 50% of this work.

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Abstract

This thesis consists of three independent chapters that explore aspects of tax policy and the determinants of responsiveness to a public health shock.

Chapter 1 explores the importance of tax revenue for future revenue collection capacity, spending, and growth of local governments. Using newly collected financial data of more than 300 U.S. cities over 1899-2000, I leverage source-specific variation in revenue through a shift-share research design. I report three main results. First, additional tax revenue causes greater spending on services than same-sized non-tax revenue, despite partial earmarking of non-tax revenue for this purpose. Second, tax revenue generates persistence in revenue. An initial increase in tax revenue has a 81% persistence on total revenue 10 years later, while similar non-tax revenue increase has dissipated after 3 years. This persistence can be explained by fiscal capacity improvements through higher effective tax rates, better enforcement and increased spending on revenue collection. Third, reliance on taxes has long-run effects on municipal finances and growth. Cities with initially higher tax revenue per capita in 1910-1936 have greater revenue and expenditure per capita throughout the next 60 years, as well as a higher median household income, larger population and migration.

Chapter 2 studies the role of free media in the responsiveness of governments and the public during the COVID-19 pandemic. Using a panel data of daily COVID-19 deaths, mobility and lockdown decisions for all countries, we show that, as the initial number of deaths increased, governments were more likely to impose a lockdown and citizens reduced their mobility. To account for inaccuracies in reporting deaths, we calibrate a SEIR model to create an instrument for reported deaths. Using this approach, we find that responsiveness to deaths was limited to governments and citizens in free-media countries, and differences in responsiveness account for 40% of the variation in lockdown decision and mobility. In support of the role of free media, we show that differences in responsiveness are not explained by a range of other country characteristics such as the level of income, education or democracy. We also find evidence that citizens with access to free media were better informed about the pandemic and had more responsive levels of online searches about COVID-19. These findings can be explained by a simple model of policy-making whereby free-media raises awareness about health risks and helps coordinate decisions.

Chapter 3 measures the role of salience in the behavioral response of taxpayers. I focus on the

2011 reform of the UK income tax that introduced both salient changes in the tax schedule (a new marginal tax rate of 50% on income above £150,000) as well as nonsalient changes (the withdrawal of tax-free personal allowance for income above £100,000). I develop a conceptual framework to account for inattention as a form of optimization friction when estimating the elasticity of taxation income, provide testable predictions, and derive a measure of attention. Empirical evidence from administrative data confirm the importance of tax inattention. I also find significant learning over time, reducing inattention by 40% up to 10 years after the introduction of the reform. These results have implications for measuring the elasticity of taxable income, and suggest only short-run gains from using salience as a policy parameter.

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Chapter 1

Local Fiscal Capacity: Historical Evidence from U.S. Cities

1.1. Introduction

At the heart of any effective government lies the ability to raise revenue. Collecting revenue through taxes is one of the fundamental characteristics of the state (Smith (1755). However, in reality, governments also raise non-tax revenue through natural resources, fees-for-services, or aid. The source of revenue carries important implications not only for the amount of revenue raised, but also for how public money is spent (Hines and Thaler (1995)) and the nature of the social contract between governments and citizens (Schumpeter and Swedberg, Levi, Besley (1918, 1988, 2020*b*)).

Empirical evidence on the causal effect of relying on taxes compared to other sources of revenue has been limited. Most of the literature has focused on the impact of increased non-tax revenue, and generally found contrasted results. While non-tax revenue may induce a greater propensity to spend (Hines and Thaler, Inman (1995, 2008)), this spending can be diverted or wasted (Van der Ploeg, Caselli and Michaels, Brollo et al., Singhal (2011, 2013, 2013, 2008)). Direct comparison of the effects of choice of source of revenue on spending or other outcome has been limited due to the scarcity of exogenous variation in tax revenue[1]

This paper uses newly collected municipal finance data to estimate the impact of the source

[1] Two exceptions are Gadenne (2017) and Martinez (2020), who examine the impact of a tax capacity program in Brazil and property tax variation in Colombia respectively.

of revenue on the revenue, spending and development of cities. I rely on historical data on the finances of more than 300 cities for over 100 years (1899-2000), with detailed information on 44 sources of revenues and 176 forms of public spending between 1910-1936. I exploit idiosyncratic variation in revenue sources and the high degree of path dependency in revenue composition by cities to through a shift-share instruments that predict tax and non-tax revenue for each city.

Historical evidence is particularly well suited to assess the importance of revenue sources on public goods provision and development. First, they allow to examine periods of large changes in revenue source mobilization that are not usually observed. The period of 1900-1940 is remarkable as new sources of municipal revenue became available, the local tax system was re-defined, and interdependence between levels of governments increased. This provides a host of variation suitable for examining the effects of sources of revenue.[2]. Second, U.S. cities were independent fiscal units with considerable independence on resource mobilization and spending decision, making this setting ideal to study the nexus between local revenue and spending. Third, observing a large window of time gives me the opportunity to estimate the causal impact of fiscal capacity changes on long-run urban development. To the best of my knowledge, this paper provides the first empirical validation of fiscal capacity as catalyst for local development.

Tax revenue accounts for 70% of municipal revenue on average, mainly derived from local property tax. Non-tax revenue is composed of intergovernmental transfers made up of outside grants and conditional subventions, earnings of public enterprises and special assessment charges in exchange for specified benefits. While taxes are compulsory, intended for general spending purpose and collected locally, non-tax revenue can be imposed in exchange for services, earmarked for specific purposes, and collected outside of the city.

Cities were in charge of delivering services that provided direct benefits to citizens. This included the running of primary and secondary education, public safety, providing access to water, health and sanitation as well as being in charge of welfare programs and the development of highway.

I first compare the effect of source of revenue on spending by cities, and find clear evidence of non-fungibility of local revenue. Despite non-tax revenue being partly earmarked on ser-

[2]Major changes in revenue were brought about by the Great Depression, Prohibition and improved enforcement of property tax. As mentioned above, changes affected each source of revenue separately. More details is provided in Section 1.2.

vices, the marginal propensity to spend on services is larger out of tax revenue than non-tax revenue (0.752 vs 0.0956 respectively) with the difference statistically significant. This larger effect is found across all categories of services. Consistent with the earlier literature, I find a larger effect on total spending from non-tax revenue than taxes (1.19 vs 0.897). Total spending includes spending on services, spending public enterprises , debt repayment and investment. While non-tax revenue tends to be initially spent more on public enterprises and investment.

Second, I examine the persistence of sources of revenue. Tax revenue exhibit a very high degree of persistence, with 81% of total revenue 10 years after the initial increase. In contrast, the effect of a non-tax revenue increase have disappeared after 3 years. I show that the persistence of tax revenue is due to an increase in fiscal capacity. An increase in tax revenue is associated with a persistent increase in effective tax rates on property, better enforcement measured by the amount of tax penalties, and increased revenue capacity indicated by spending on revenue collection. Taken together, this evidence shows that increased local tax revenue were associated with the development of revenue capacity. In comparison, non-tax revenue have no effect on enforcement, revenue collection capacity or tax rates which explains their low persistence. This difference in longevity of revenue translates in the observed spending effect. Tax revenue leads to persistent increases in spending and services, a feature not associated with non-tax revenue.

Third, I look at the long-run effects of tax revenue. I explore differences between cities with initially high vs low reliance on taxes in 1910-1936 on their future level of taxes, spending and outcomes. I find that cities with early increases in taxes have persistently higher revenues in the next 60 years (1940-2000). This revenue difference is due to higher tax revenue for cities with initially higher revenue from taxes. It is also specific to taxes, as I find no similar long-run differences on non-tax revenue such as transfers. This increased fiscal capacity is accompanied by greater local spending, higher median household income, larger population and higher migration and share of non-white[3].

I present a conceptual framework with source-specific asymmetric information to rationalize non-fungibility of revenue and provide testable predictions, building on Besley and Smart, Besley, Gadenne (2007, 2006, 2017). In this framework, voters are unaware of the revenue coming from non-tax sources [4] and vote to re-elect incumbents. Electoral mechanisms serve to discipline self-interested politicians. As taxes increase, this reduces agency problems and

[3] I find a weak positive effect on years of education and no discernible difference on direct measures of quantity of service provision such as the number of hospitals and teachers.

[4] Filimon, Romer and Rosenthal (1982) show that voters lack information on intergovernmental transfers, and this affects spending decisions.

leads to a greater share of spending on public goods.

I relate my work to several strands of the literature. This paper is closely related to the public finance literature on the local marginal propensity to spend in response to a revenue shock. Traditionally this literature has been centered around the role of the "flypaper effect" or trying to explain why federal transfer lead to greater increases in spending than own-revenue source increases (Courant, Gramlich and Rubinfeld, Hines and Thaler, Inman (1978, 1995, 2008)). Recent empirical evidence have contrasted this view, either showing that non-tax revenue may not improve public goods provision but instead are diverted or wasted (Reinikka and Svensson, Caselli and Michaels, Monteiro and Ferraz, Brollo et al. (2004, 2013, 2012, 2013)), while tax revenue instead have a positive impact on government spending (Gadenne, Martinez (2017, 2020)). I provide estimates of comparable sources of variation for tax and non-tax revenue coming from similar instruments, and find larger elasticity of spending from tax-revenue.

To the best of my knowledge, I provide the first evidence of causal impact of fiscal capacity on urban development. It is connected to recent studies on the link between taxation and development (Jensen (2016)). More recently, my approach is similar to in methods to estimates of the local fiscal multipliers that rely on exogenous variations in local revenue through federally-mandated programs Nakamura and Steinsson, Suárez Serrato and Wingender (2014, 2016). I also relate to new questions around fiscal federalism, urban public finance, and the provision of services at the local level (as reviewed by Gadenne Singhal 2014). Finally, I apply new insights from recent advances in the shift-share research methodology, for instance from Borusyak, Hull and Jaravel, Goldsmith-Pinkham, Sorkin and Swift, Adao, Kolesár and Morales, Broxterman and Larson, Jaeger, Ruist and Stuhler (2021, 2020, 2019, 2020, 2018), and provide an application of these tools in a new context, i.e. for a panel of municipal finances data.

The rest of the paper is organized as follows. Section 1.2 describes the data and the historical context. Section 1.3 outlines the identification strategy. Section 1.4 presents the main results on the non-fungibility of local revenue, persistence of tax revenue due to increased fiscal capacity, and long-term effects of tax revenues. Section 1.5 offers a discussion of additional results, including a conceptual framework of political agency that rationalizes the non-fungibility of revenue in the presence of source-specific asymmetric information. Section 1.6 concludes.

1.2. Context and Data

This section describes the context of urban growth in American cities between 1900-1940, provides a brief overview of their financial structure, and describes the data sources.

Historical background The United States in 1900 was the subject of spectacular urban growth. Fueled by innovations in transportation technology and mass migration, cities grew at an unprecedented rate. Transportation innovations include steam railroads, cable-car systems (pioneered in Chicago in 1887) and electric streetcars. These changes in rapid succession dramatically made cities linked to extensive regional networks and facilitated mobility.

Second, mass migration also transformed the size of cities. Mass migration started since 1865 but exploded between 1900-1917 with the arrival of "new immigrants" from southern and eastern Europe (e.g. from Italy and Greece), but also Japanese, Chinese and Mexican immigrants. By 1900, 30 million people, or 30 percent of the total population, lived in cities. By 1920, the urban population surpassed 50% for the first time. This movement stalled in the 1930s with a declining birthrate, immigration restrictions and the effects of the Great Depression

Increases in urban growth created new challenges, and paved the way for the Progressive Era Reform movement (1896-1916) in the early twentieth century. Emerging first at the city-level before becoming a national movement, this organic movement of organizations demanded that government engage in social reforms, and play a central role in social and economic affairs. This movement growth out of social changes: immigration, urban growth, widening of class division and corporate influence.

The massive influx of immigrants into slums tenements, made the need for urban changes dire, in particular the provision of housing, safe water, plumbing, garbage collection, and fire protection, as well as milk inspection, decent schools, recreation. This urban reform movement was also accompanied by calls for urban beautification projects such as the transformation of Chicago in 1910-1920 that costed more than $300 million on urban development projects through parks, museums, and public buildings.

The Progressive movement led to the birth of organized labor movements, such as the American Federation of Labor, women groups and socialist advocacy that demanded reforms and become elected as municipal officials. This was accompanied by structural changes in city government. A city-manager system, with experts running the city as professional managers like a business, became vogue after the disastrous flood of 1913 in Dayton, Ohio.

Another dimension of the progressive movement concerned moral values. This started with the founding of the Anti-Saloon League in 1895, and culminated with the adoption of the Eighteenth Amendment outlawing the sale of alcoholic beverages in 1919.

On the municipal revenue side, many technological innovations dramatically affected the revenue cities could expected to collect from some sources.

The 1920s democratization of the automobile (8 million in 1920 to more than 23 million in 1930, and 60% of households owned an automobile) increased revenue from highway privileges. Property taxes became more strongly enforced through the professionalization of tax assessors, exchange of information across jurisdictions and scientific methods of valuations brought about by the Progressive Era reforms

Prohibition, which took effect in 1920 and lasted until 1933, meant that cities could no longer count on liquor taxes as a source of revenue.

Intergovernmental transfers, already nascent in the early 1900-1920, became a mainstay of city revenue during the Great Depression and federal programs directly financed and commissioned cities in implementing them.

Data I use data from municipal finances collected from the largest U.S. cities between 1899 and 1938. This data comes from newly digitized reports from the U.S. Census Bureau that I compiled and harmonized. These reports contain annual financial information divided into: (1) municipal receipts by type of revenue sources (2) payments for expenses, interest, and outlays, and for each of the principal classes of expenses (3) the value of municipal properties (4) the amount of municipal indebtedness; and (5) the assessed value of property subject to taxation.

The sample of cities covered by these reports include cities with population above $30,000$ from 1899-1931 (310 cities in 1931), and above $100,000$ for 1932-1936 (95 cities). In order to maintain a consistent balanced sample, I focus the analysis on 88 cities that were sampled throughout this period.

These reports on the financial statistics of cities provide an unusually detailed source of information on the revenues and spending of cities. The data covers 45 categories of municipal receipts, including 26 tax revenue sources (e.g. property, license, or poll tax) and 19 non-tax revenue sources (e.g. highway privileges, intergovernmental transfers, fines, earnings from public service enterprises or municipal departments). The reports also detail types of spending divided into spending for public service enterprises, for the payment of interest on debt, for the payment of outlays, and for spending of municipal departments. The latter category provides the most detailed information, and is further divided into more than 170 spending categories. These categories are organized into 9 main themes: General Government expenses (spending related to legislative investigations, accounting and auditing, collection of revenue, judicial courts or the holding of elections), Public Order (mainly for police and fire departments,), Health (prevention of diseases, hospital expenses), Sanitation (street cleaning, sewage disposal), Highways, Public Assistance (care of poor or children, mothers' aid, old age assistance, veterans' relief), Education (funding of schools and libraries for primary education), Recreation (funding of recreation centers, expenses related to parks and trees), and Miscellaneous (unclassified or undistributed expenses, administration of public trust funds. Overall this accounts for 2% of total expenses of municipal departments).

Tax revenues constitute on average 70% of municipal revenue, mainly from property taxes. Taxes are defined as compulsory contributions collected by the city for the general purpose of raising revenue. Non-tax revenue in comparison is composed of different sources, each highlighting the specificity of taxes. First, about 20% of non-tax revenue comes from intergovernmental transfers. These transfers come from the federal government, the state or counties, and can take the form of subventions (with conditions on how the funds should be spent) or grants (no conditions). The majority of these transfers are taxes collected by states or counties and transferred for the exclusive benefits of city school districts. Second, about 1/3 of non-tax revenue are earnings of public service enterprises, mainly from water supply companies. Third, around 20% of non-tax revenue is derived from special assessment charges that are temporary charges on beneficiaries to cover the cost of providing specified public service or infrastructure. The remaining non-tax revenue is made up of highway franchising on corporation for the use of public highway ("highway privilege"), fees and charges of municipal departments, and fines.

Source of revenue used by cities was highly persistent. As shown in Figure A1.7, the share of revenue from each source has remained practically identical between the first and last observed period of the sample, i.e. over more than 25 years between 1910 and 1936.

Figures A1.2 and A1.5 show the evolution of revenue and spending by category. It highlights the general increase in size of city government as well as the idiosyncratic nature of revenue growth by source, also visible from Figure A1.3. A clear example is for license tax, which represents up to 11% of revenue of cities but was directly affected by the prohibition of alcoholic beverages during the Prohibition, removing any revenue from liquor taxes as shown in Figure A1.4.

I also complement this database with information on local measures of economic activity. First, I incorporated a measure of manufacturing output and value added collected by the Census of Manufactures between 1929-1935 measuring manufacturing output and value added at the county-level. Second, I added the number of patents filed in the city every year between 1899 and 1940 using the HistPat database from Petralia, Balland and Rigby (2016). Third, I relied on railway shapefiles from Sequeira, Nunn and Qian (2020) to measure the length of railroad between 1900 and 1921 in all the sampled cities.

1.3. Identification Strategy

1.3.1. Shift-Share Instrument

This section describes the shift-share research design used to provide variations on municipal revenues that are exogenous to spending decisions.

As shown in Figure A1.1, municipal spending steadily increased between 1918 and 1932, from about 400 to more than 1,000 current USD per capita. This increase coincided with the rise in municipal tax revenue over the same period and, to a lesser extent, non-tax revenue. The endogeneity challenge is to attribute the effect of increased revenue on changes in municipal spending. Typical concerns are twofold: (i) reverse causality, as cities that increased spending might have been more likely to raise more revenue as a consequence (ii) unobserved characteristics could explain both increase in revenue and spending, such as political leaning of cities.

To address these challenges, I propose a shift-share instrument that predicts municipal revenues by interacting a city's past reliance on sources of revenues with the national growth in the sources. To build intuition, I start from the following accounting identity regarding the level of revenue x_{it} in city i and year t :

$$x_{it} = x_{i0} \prod_{k=1}^{t} (\sum_{c} s_{ick} g_{ick})$$

where g denotes the growth rate in revenue source, s denotes the share of total revenue coming from source c, and x_{i0} is the initial revenue in city i and year t,

The shift-share instrument predicts revenue growth by replacing city-level growth rates in revenue category by the average growth rate measured in-sample across cities:

$$z_{it}^{S} = x_{i0}^{S} \prod_{k=1}^{t} (\sum_{c}^{S} \underbrace{s_{ic0}}_{\text{Shares}} \cdot \underbrace{g_{ck}}_{\text{Shifts}})$$

Where z_{it} is the predicted revenue for each type of revenue source $S \in$ {T, N} (T stands for tax revenue and N for non-tax revenue). I denote by s_{ic0} the initial share of revenue from revenue category c, and g_{ct} is the average (leave-one-out) city growth rate in revenue category c and year t. The instrument combines both the share of revenue from the initial period and compounded growth rates for each revenue source to predict revenue levels for each year. The choice of using a leave-one-out average follows the literature (Bartik and Bartik Timothy, Autor and Duggan (1991, 2003)) and ensures that the growth rate of revenue sources in the city does not factor into its predicted revenue. Similarly, I use as shares the first observed revenue decomposition of the city[5] so that this alleviates any concerns of endogeneity coming through contemporaneous changes in shares, as is done for similar shift-share instruments (Boustan, Derenoncourt (2010, 2019)).

This formulation of the shift-share instrument makes clear that it combines variation across cities (initial shares s_{ic0}) as well as common shock components to revenue categories (g_{ck}). Initial shares could be endogenous as they represent a city's choice of revenue instrument which could be influenced by access to natural resources, locations, policy preferences that also link to spending decisions. However, national changes in revenue sources g_{ct} capture national shocks to source mobilization that are unlikely to drive a city's own spending choice.

The identification strategy for the shift-share instrument requires that the instrument z_it be uncorrelated with unobserved factors affecting local spending, conditional on city and year fixed effects. The instrument aims to capture exogenous variation in revenue that is uncorrelated with local spending trends by (i) relying on national source-specific through change in tech-

[5]For the majority of cities, this is being observed in 1909, i.e. prior to the sample used for analysis.

nology (e.g. better enforcement capacity, more use of highway) and/or national policy change targeting a specific source of municipal revenue (e.g. ban on liquor sale, greater use of federal aid) (ii) distributing these shocks via shares that capture differential exposure to changes.

1.3.2. First Stage

The first stage equation for each type of revenue is

$$x^S_{i,t} = a + bz^T_{i,t} + cz^N_{i,t}\mu_i + \mu_t + \nu_{i,t} \tag{1.1}$$

where $z^S_{i,t}$ is the predicted tax/non-tax revenue for city i and year t. Both tax and non-tax revenue are separately estimated.

The second stage equation is

$$y_{i,t+k} = \alpha + \beta\hat{x}^T_i, t + \delta\hat{x}^N_{i,t} + \gamma_i + \gamma_t + \varepsilon_{i,t} \tag{1.2}$$

where $y_{i,t+k}$ is the log spending per capita in city i and year $t + k$, for $k \in [0, 10]$, $\widehat{x^T_{i,t}}$ and $\widehat{x^N_{i,t}}$ are the instrumented tax and non-tax revenue per capita respectively.

The coefficient of interest $\hat{\beta}$ captures the spending effects of an increase in revenue while keeping other sources of non-tax revenue fixed. Similarly, $\hat{\delta}$ captures the spending effects of an increase in non-tax revenue. The main results compares $\hat{\beta}$ and $\hat{\delta}$, i.e. the importance of revenue source on spending while keeping total revenue constant. as the effect could be having some persistence, therefore we will also estimate spending changes k years after the initial revenue shock, with k varying between 0 and 10 years.

I now evaluate the strength of the first stage. As detailed above, I consider both z_{it} as instruments as well as z^T_{it} for types of revenue sources.

Overall, predicting total revenue using the shift-share approach described above yield the first stage presented in Table A1.1. The F-statistics is large for both tax and non-tax revenue, both in levels and logs specifications.[6]

Additional evidence on the validity of the shift-share approach is provided in Appendix, including test of shock-level inference.

1.4. Main Results

This section presents the main findings of the paper. I start by describing the impact of revenue source on spending and service provision. Second, I look at how revenue source affects the persistence of revenue and spending. Third, I look at the long-term effect of revenue source on government finances and a wide range of outcomes.

1.4.1. Non-Fungibility of Revenue

This section reports results on the difference in elasticity of spending by the source of revenue shock. The empirical specification is the following:

$$y_{i,t} = \beta \widehat{x_{i,t}^T} + \delta \widehat{x_{i,t}^N} + \mathbf{X}_{i,t}\gamma + \varepsilon_{it} \quad (1.3)$$

where $y_{i,t+}$ is the per capita spending in city i and year t, $\widehat{x_{i,t}^T}$ is the instrumented tax revenue per capita, and $\widehat{x_{i,t}^N}$ the corresponding non-tax revenue per capita using the shift-share predicted revenue instrument described in Section 1.3. This specification controls for both city and year fixed effects. City fixed effects account for fixed differences between cities, such as geographical endowments and baseline levels of spending or revenue. Year fixed effects ensure that shocks affecting the spending of all cities, such as booms and busts or World War I, does not drive the results.Standard errors are clustered at the city level, and robustness results are also discussed below. [7]

Both β and δ capture the impact of revenue source on spending while keeping other revenue sources fixed. In other words, comparing β and δ will indicate the importance of revenue

[6] Additional evidence on the fit of the prediction is provided by Figure A1.8 who looks at the prediction for each type of revenue separately, as well as for a balanced panel of cities.

[7] I also provide robustness of the results by using a balanced sample of 88 cities that were included in all years by the Census between 1910 and 1936 when spending and revenue are observed and the instrument can be derived for the 88 cities. These cities were selected to have a population above $100,000$ in 1932, and thus represent large cities.

coming from tax vs non-tax revenue when keeping the overall revenue constant.

Table 1.1 presents the main findings. Results report the impact of revenue source on total revenue, total spending and spending on service provision which is the main activity of local governments[8]. Results are presented both in levels and in a log-log specification to estimate elasticity, and also show OLS and reduced-form estimates alongside the estimates from the Shift-Share IV regressions.

Columns 1-2 document the effect of revenue source on total revenue. Reassuringly, both tax revenue and non-tax revenue have a positive associated elasticity of total revenue, with a larger elasticity for tax revenue compared to non-tax revenue (0.68 vs 0.30 respectively) and the null hypothesis of similar elasticity rejected at the 5% level. By construction, level regressions presented in column 2 report an effect of 1 on total revenue from a 1$ increase in either revenue source controlling for other sources of revenue.

Columns 3-4 report the effect of revenue source on total spending. A 1% increase in tax revenue is associated with a .482% increase in total spending compared to 0.279% increase from non-tax revenue. All coefficients are highly precise and with a strong first stage, as confirmed by the Sanderson and Windmeijer (2016) F-statistics of 139 for both tax revenue and non-tax revenue instrument. What is more, results are very similar with the OLS regressions (elasticity of 0.480 and 0.339 for tax and non-tax revenue respectively). Level coefficients reported in column 4 show a larger effect of non-tax revenue, however the difference cannot be rejected at the 5% level, and as discussed below tax revenue effects on total spending are and remain larger after one period[9].

Columns 5-6 estimates the effect of revenue sources on spending on service provision. This constitutes the majority of spending (58% on average) and finance the provision of public goods in all sectors. Column 5 reports the log coefficients. A 1% increase in tax revenue is associated with a 0.8% increase in service provision compared to 0.08% from non-tax revenue. Equality of coefficients can be rejected at the 5% levels. Levels regressions report similar differences between tax and non-tax revenue: a 1$ increase in tax revenue is associated with a 0.75$ increase in service provision compared to a precisely estimated increase close to 0 and non-significant from non-tax revenue. OLS and reduced forms estimates report similar differences between tax and non-tax revenues. All coefficients are highly significant.

[8]Other forms of payments by the local government include spending on public service enterprises, payment of debt interest and payment for capital investment. The impact of revenue source on these spending categories is presented in Table A1.9.

[9]See Table 1.1.

Summary: This section has presented results showing that tax-revenue are associated with more spending and more service provision than a similar non-tax revenue increase. This result confirms the non-fungibility of public funds, i.e. that the source of revenue matters for local public spending.

1.4.2. Fiscal Capacity

This section reports estimates of the persistence of revenue by revenue source, as well as the persistence of associated spending effects.

The specification is

$$y_{i,t+k} = \beta \widehat{x_{i,t}^{T}} + \delta \widehat{x_{i,t}^{N}} + \mathbf{X}_{i,t}\gamma + \varepsilon_{it} \tag{1.4}$$

Compared to Equation (1.3), outcomes are measured in years t +k, with $k \in [0, 10]$ to capture the persistence of outcomes in response to tax/non-tax revenue increase.

Figure 1.2 shows the effect of a 1$ increase in year 0 from either tax or non-tax revenue on total revenue. Both effect start with a perfect pass-through in year 0 by construction as the specification controls for the other revenue source. However both revenue sources have quickly diverging patterns of persistence. While the non-tax revenue increase dissipates after 4 years, a similar tax revenue increase is associated with a 1$ increase in total revenue for up to 10 years after the initial increase.[10]

Figure 1.1 presents the evolution of spending effects by source of revenue. As shown, the effects of non-tax revenue on total spending (Figure 1.2(a)) rapidly dissipate to be insignificant after year +4, and non-tax revenue effects on service provision is not statistically significant from year 0 and up to 10 years after the initial increase in non-tax revenue (Figure 1.2(b)) . In contrast, tax revenue increase is associated with a constant increase in both total spending and service provision, respectively around 1 and .75 each year per 1$ of initial tax revenue increase.[11]

To explain the higher persistence coming from taxes, I explore whether increased tax revenue

[10] Figure A1.9 shows the revenue elasticity. While both tax and non-tax elasticities decline, tax revenue increases exhibit more persistence.

[11] Figure A1.10 report similar results for the log specification. While both tax revenue and non-tax revenue elasticity decline over time, this pattern is significantly more pronounced for non-tax revenue.

is associated with greater fiscal capacity. Fiscal capacity is defined as the ability for a city to collect more revenue through taxes. I measure fiscal capacity in three different ways: the level of effective tax rates on the value of property ; level of tax enforcement measured through the total amount of penalty on property tax ; revenue collection capacity measured as the amount of spending on the assessment and levy of revenue.

Figure 1.3 reports findings. An increase in tax revenue is associated with greater effective tax rates, better enforcement (more property tax penalty payments), and increased revenue collection.[12] Spending on revenue collection are initially larger at the time of the tax revenue increase, then remain higher than pre-increase but smaller in magnitude. Both effects on enforcement and revenue capacity are large in magnitude, given that tax penalty correspond to 0.6% of total revenue and the average spending on revenue collection is $.4\%$ of overall spending. No similar effect on either fiscal capacity or more general forms of revenue capacity from a non-tax revenue increase. This is consistent with the greater persistence of revenue from taxes explained by increased fiscal capacity.

1.4.3. Long-Run Effects

This section reports results on the long-run effects of revenue source. Having established that the source of revenue matters for levels of local revenue, spending and public goods provision, I now investigate whether the source of revenue (tax vs non-tax revenue) can explain patterns of public spending over the long-run, economic performance of cities and their urban attractiveness.

The specification is the following:

$$y_i^d = \beta \widehat{x_i^T} + \delta \widehat{x_i^N} + \gamma_s + \varepsilon_i \tag{1.5}$$

Compared to the regressions estimated in the previous results, the analysis is conducted on a cross-section of cities, with both actual and predicted revenue being the average revenue for 1910-1936. Outcomes are measured each decade d from 1940 to 2000 from the City Data Book

[12] A similar effect on effective rates and tax base is found when looking at elasticities. As shown in Figure A1.11, a 1% increase in tax revenue is associated with a .75 percentage point increase in tax rate and a .25% increase in property value. The persistence of tax revenue comes from tax rates, while the effect on property values becomes not statistically significant from zero after one year. In contrast, as expected, no effect on either tax rate or property value is detected following a non-tax revenue increase.

series. The regression controls for state fixed effects s and robust standard errors are reported. Tax and non-tax revenue are measured in per capita. Details on the outcomes is provided below.

Table 1.2 reports the results. Each panel corresponds to outcomes measured in a different decade. Outcomes are organized by relating to city government decisions, economic outcomes, and demographic measures as proxy for population and migration outcomes.

Columns 1-4 reports long-run effects on local government decisions. Tax revenue increases in 1910-1936 are associated with a very persistent increase in total revenue, taxes and government expenditures that can be observed throughout decades and up to 2000. Non-tax revenue increases do not lead to similarly persistent effects, with coefficients either non-significant or negative. We can reject the null hypothesis of tax and non-tax revenue effect for most regressions. An initial increase in either tax or non-tax is not associated with patterns of transfers, while an initial tax revenue increase leads to more taxes in the future. This highlights the persistence of tax revenue changes.

Columns 5-7 reports long-run effects on economic outcomes. The median household income is higher following an initial tax revenue increase, while non-tax revenue increases have no significant effects on future median income. No significant difference between tax and non-tax revenue is detected for unemployment or poverty rates.

Columns 8-10 reports long-run effects on migration and population. Initial tax revenue increase are associated with larger population, a greater share of foreigners and non-white. These effects on all three outcomes are positive and significant for all decades, while corresponding effects for non-tax revenue are non-significant or negative. Equality of tax and non-tax revenue coefficients can be rejected at the 5% level for 10 out of 14 regressions.

Table 1.3 reports the long-run effects of source of revenue for service provision. No effects are detected from either tax and non-tax revenue increase on mortality, number of hospital and hospital beds, the number of teacher or the school enrollment rates. One exception is the average years of education. An initial tax revenue increase is associated with more years of education in all subsequent decades for which data is available (1940-1970), however the difference with effects from non-tax revenue increase is not significant at the 5% level due to the effects from non-tax revenue being imprecisely estimated around 0. Finally, increases in tax revenue are initially associated with less corruption, as measured by the number of federal convictions of public officials (including local officials) for charges of corruption in the judicial district linked to the city in 1970. However, no discernible differences are found from 1980 on.

1.5. Additional Results

1.5.1. Conceptual Framework

In this section, I build on the political agency models developed by Besley (2006), Besley and Smart (2007), and Gadenne (2017) and present a conceptual framework that rationalizes the presence of non-fungibility of public funds. I also discuss alternative mechanisms.

Set-Up In this framework, a representative citizen votes to re-elect incumbents but do not observe some of their actions. The asymmetric information is focused on the level of public funds and varies by source of revenue. All politicians are self-interested in their pursuit of public office, so the model is focused on asymmetry of information derived from moral hazard in government.[13]

Public Revenues Public revenues are derived from two sources: taxes T, endogeneously determined each period, and non-tax revenues F which are exogenously determined. For simplicity, F can take two values: $F_H = \overline{F}(1+u)$ in the high state H and $F_L = \overline{F}(1-u)$ in the low state L. The probability of the high state occurring is given by $q \in (0,1)$ and we assume that $u \in (0,1)$.

Incumbent The incumbent politician faces the following government budget constraint

$$T + F = G + S$$

where $R \equiv T + F$ are total revenues, G represents spending on public goods, while S are rent or self-interested spending that incumbents extract from being in office. The incumbent maximizes the sum $S + \sigma Z$ where σZ is the expected utility value of being re-elected with σ the probability of re-election. All politicians are self-interested, so the role of the election is to discipline behavior in office and not uncover good vs bad types of politicians. Incumbents want

[13] This is similar to first generation political agency models, see Barro (1973) and Ferejohn (1986).

to divert all public resources for their own sake but are faced with institutional constraints that limit rents to being at most $\overline{S} = \alpha R$ with $\alpha < 1$.

Citizens Representative citizens obtain utility through the provision of public goods but face the cost of paying taxes. They maximize $W(G, T) = G - \phi C(T)$ with ϕ indicating the marginal utility cost of taxation, and $C(.)$ is a strictly increasing and convex function. I define $h(.) = C'^{-1}(.)$.

First Best Equilibrium In a situation with no information asymmetry, citizens fully observe T and F. They choose the re-election rule $\sigma(G_i, T_i) = \sigma_i$ that provides incentives for incumbents to provide the policy menu (G_i, T_i) that is welfare-maximising.

This contract must satisfies the participation constraint that it leaves the incumbent with enough rents today that they agree to provide the policy menu and seek re-election instead of extracting maximum rents today and running away. This is captured by the following incentive compatibility constraint:

$$T_i + F_i - G_i + \sigma_i Z \geq \alpha(T_i + F_i), \forall i = H, L \tag{1.6}$$

As there is no difference in type between politicians, in equilibrium $\sigma_i^* = 1$ in either high and low state whenever the incumbent chooses the following policy menu (G_i^*, T_i^*) :

$$G_i^* = (T_i^* + F_i)(1 - \alpha) + Z$$

We can also solve for the optimal tax level by maximizing the welfare of the citizen, and obtain $T^* = h(\frac{1-\alpha}{\phi})$

In the absence of asymmetry of information (citizens fully observe public revenue from all sources), the marginal propensity to spend on public goods is equal to $1 - \alpha$ irrespective of the source of revenue (tax or non-tax). There is full fungibility of public funds.

Equilibrium with asymmetric information Assume now that citizens imperfectly observe non-tax revenues: this could be due to ignorance over the amount of intergovernmental transfers, or because other forms of non-tax revenues are either not directly imposed on citizens (e.g. public franchise on corporations, revenue collected via special districts), or are paid by only a fraction of them (e.g. fees or charges). In this case, the incumbent can extract additional rents through this channel. By pretending to be in the low state while received high-state non-tax revenue, they can capture the difference in non-tax revenue between high and low states.

The new political contract must therefore also respect the following incentive compatibility constraints to ensure that incumbents have no incentives to lie about the true state:

$$T_H + \overline{F}(1+u) + \sigma_H Z - G_H \geq T_L + \overline{F}(1+u) + \sigma_L Z - G_L \tag{1.7}$$

and

$$T_L + \overline{F}(1-u) + \sigma_L Z - G_L \geq T_H + \overline{F}(1-u) + \sigma_H Z - G_H \tag{1.8}$$

There is only one set of values (G_H, G_L, T_H, T_L) that satisfies (1.7) and (1.8) together, which can be written as:

$$G_H = G_L + T_H - T_L + Z(\sigma_H - \sigma_L) \tag{1.9}$$

In the low state, citizens can expect the same first best equilibrium values of public goods and taxes paid given that no informational rent can be extracted. Using this and 1.9, we obtain

$$G_L^* = (T_L^* + \overline{F}(1-u))(1-\alpha) + \sigma_L^* Z \tag{1.10}$$

$$G_H^* = (T_H^* + \overline{F}(1-u))(1-\alpha) + \sigma_H^* Z + \alpha(T_H^* - T_L^*) \tag{1.11}$$

As in the first case, citizens will re-elect the incumbent whenever they respect the political contract, so $\sigma_H = \sigma_L = 1$. Maximizing citizen's welfare subject to (1.10) and (1.11) gives the optimal tax level

$$T_H^* = h(\frac{1}{\phi}) \tag{1.12}$$

$$T_L^* = \max(0, h(\frac{1-\alpha-q}{\phi(1-\alpha)}) \tag{1.13}$$

Tax revenues are lower in the low state as any increase in taxes in the low state menu would make this option more attractive to the incumbent in the high state.[14] As a result, less public good is being provided in the low state. From (1.13), we can see that this cost of lower public goods provision and lower taxes increases with q, i.e. as the low state becomes less likely.

Now the structure of revenue also affects the allocation of funds. Using (1.10) and (1.11), we can write the average amount of public goods provided $G^* \equiv qG_H^* + (1-q)G_L^*$ as:

$$G^* = (1-\alpha)(T^* + \overline{F}) - \overline{F}u(1-\alpha) + Z + \alpha q(T_H^* - T_L^*) \tag{1.14}$$

where $T^* \equiv qT_H^* + (1-q)T_L^*$

This makes clear that the fungibility of funds breaks down in the presence of moral hazard. A marginal increase in tax revenue still increases public goods provision by $(1-\alpha)$, assuming for simplicity that the tax differential between states remains unchanged. However, now the marginal propensity to spend on public goods from non-tax revenue has been decreased to $(1-\alpha)(1-u)$. This is capturing the presence of informational rents $\overline{F}u(1-\alpha)$ that incumbents can extract during high states in lieu of providing services to citizens. On the other hand, increasing taxes in the high state compared to the low state reduces the informational wedge and rent extraction, as captured by the term $\alpha q(T_H^* - T_L^*)$.

Summary: This simple model has provided a framework to understand the presence of higher marginal propensity to spend from taxes compared to non-tax revenue. The presence of asymmetric information on non-tax revenue and moral hazard from incumbent is sufficient to generate a greater marginal propensity to provide public goods from tax revenue. In the presence of asymmetric information where citizens do not fully observe non-tax revenue, incumbents have more incentives to extract resources away from services to citizens, leading to lower public goods provision. The non-fungibility of funds arise from the fact that asymmetric information is concentrated in non-tax revenue, a fact that is consistent with citizens being better informed about their direct contribution to public revenue. As a result of an increase in taxes, citizens are in a position to demand more from their elected officials as they better observe their actions.

[14] I restrict the cases to considering positive taxes, i.e. no subsidies, which would need to be financed elsewhere.

Alternative explanations Other mechanisms could lead to a difference in marginal propensity to provide services by source of funds outside of information limitation about public revenue. Related to the conceptual framework outlined above, political science has highlighted the increase in citizens' demand for accountability in response to higher taxes. Coined as the "no representation without taxation" hypothesis (Ross, Moore (2004, 2007)), it has been confirmed by recent experimental evidence (Weigel (2020)). An increase in tax revenue would increase monitoring and demand for accountability on public spending to a greater extent than a similar non-tax revenue increase. As pointed by Moore (2007), governments that do not have to resolve to tax revenues are financially independent from citizens. Dependence through taxes creates accountability. While the causes of non-fungibility of revenue are distinct from that outlined in the model above, predictions on spending are aligned. We would expect greater demand for accountability or increased scrutiny on public spending to increase spending on public services that serve citizens.

Second, spatial sorting of citizens with different preferences for public spending can lead to different spending by source of funds, as in Tiebout (1956). In this framework, an increase in local taxes would attract citizens with greater preferences for public good, increasing such local spending. However, the timing of these changes is likely to take several years to materialize, which does not conform to the results described below.

1.5.2. Spending Decomposition

Figure 1.4 reports the decomposition of spending effect on each service provision by source of revenue. Both log and level results are presented. Two findings stand out. First, an increase in tax revenue is associated with larger and more persistent spending on every type of service provision compared to non-tax revenue increase. Both level increase and spending elasticity are larger for spending on services as diverse as safety (police and fire protection), health, sanitation, education, highway or education. This is true at the time of the revenue increase as well as 5 years later and up to 10 years later (only detected through level results)[15]. Second, tax revenue leads to heterogeneous spending effects on service provision. In particular, spending on public assistance (encompassing spending on welfare programs and mental health institutions) are both more pronounced and more persistence than for other types of service provision. One interpretation of this finding is that social spending on welfare programs can be subject to ratchet effects, if these programs are not scaled down once the revenue

[15] Decomposition results for all years between 0 and +10 are presented in Figures A1.12 and A1.13.

increase dissipates[16].

1.5.3. Robustness Checks

This section presents robustness checks on the shift-share instruments results.

Table A1.5 reports results that mitigate concerns with exclusion restrictions violations. It shows that the instrument is not correlated with local economic trends that are determinants of local public spending. One concern of omitted variable bias is that shift-share predicted revenues could be correlated with time-varying city-specific economic performance that affect spending. To address this concern, I proxy for local economic conditions using (i) the amount of money in bank deposits measured annually by the Federal Deposit Insurance Corporation (FDIC) between 1920 and 1936 and (ii) city importance proxied by groups used by the Census to report statistics based on estimated population size. As shown in Table A1.5, while both deposits and city importance positively correlates with actual revenue and spending, they do not correlate with the instruments. It seems therefore unlikely that omitted variables for unobserved local economic trends bias my results. Table A1.7 reports results controlling for deposits and city importance and find qualitatively similar results to those presenting for the spending effects by revenue source in Table 1.1.

Table A1.6 reports results on the determinants of shares. As argued in Section 1.3, a necessary condition is for the predicted revenue to be uncorrelated with unobserved spending determinants, However, in support of this identifying assumption it is informative to understand patterns of evolution of shares and what might explain their significant persistence. Table A1.6 report regressions of tax revenue shares used in the instruments (measured in 1909) on preceding geographic characteristics and economic conditions of cities (household size, population size, share of labor force and share of population working in agriculture, age, share of whites). The findings are twofold. First, shares are best explained by long-run characteristics of cities from 1850 rather than recent conditions. This is evidenced by the R^2 from these regressions being twice as large when geographic and economic characteristics are measured in 1850 than 1880 or 1900. Second, geographic conditions play little role in explaining shares, while deep-rooted economic conditions best explain shares. The strongest determinants of larger tax revenue reliance are the household size in 1850, share of labor force in 1850 and lower

[16] This is supported by evidence shown in Figure A1.14 showing an increase in spending on welfare programs and public assistance over time despite constant tax revenue increase.

occupational income score in 1850.

Table A1.8 replicates main results on spending effects by separating transfers from other non-tax revenue. Both transfers and other-non tax revenue are associated with smaller increase in total spending and service provision than tax-revenue, consistent with the main results presented above.

1.6. Concluding Comments

This paper has made three contributions. First, it provided new evidence confirming the non-fungibility of public funds used by local governments using new data on municipal finances, and found clear evidence of differences in the amount and types of services provided by cities by source of revenue mobilized. Second, tax revenue provides a more persistent source of revenue through increased fiscal capacity, which in turn accounts for a more persistent effect on spending and services provided. These persistent spending also change the composition of spending, more concentrated on welfare, education and public safety. Third, an initial increase in tax revenue lead to greater increases in revenue and spending in the long-run, as well as a higher household income, a larger population and migration. These results all indicate that the source of public funds carries real consequences for policy and development. Investing in improving fiscal capacity can not only affects the level and composition of public spending, but also influence the development of local governments.

These results are derived from historical evidence and should therefore not be taken as providing estimates of elasticity of spending that are applicable today. However, they provide valuable lessons for local governments today. The historical context for this study comes from cities with large reliance on a single tax instrument (property tax), increasing intergovernmental dependence, and the potential for new sources of funding. Many of these characteristics are still in force today in many cities (Glaeser (2013)): (i) local governments having independent responsibility on revenue collection, spending decisions (ii) the possibility of local fiscal capacity improvements through increased enforcement and in large enough local jurisdictions[17] (iii) the availability of existing fiscal instruments such as property taxes.

This study aimed to offer new perspective into the mechanisms of fiscal capacity. While im-

[17] In smaller jurisdictions, the ability to improve enforcement might be limited, and one can expect larger behavioral responses to increased effective tax rates.

provements in the collection of taxes are often slow, their consequences on public spending and development are shown to be important and can explain past and present patterns of prosperity.

Tables

TABLE 1.1: SOURCE OF REVENUE AND LOCAL PUBLIC SPENDING

	Total Revenue		Total Spending		Service Provision	
	Log	Level	Log	Level	Log	Level
	(1)	(2)	(3)	(4)	(5)	(6)
			A. OLS			
Tax Revenue	0.657***	1.000***	0.480***	0.897***	0.500***	0.709***
	(0.013)	(0.000)	(0.038)	(0.030)	(0.040)	(0.012)
Non-Tax Revenue	0.299***	1.000***	0.339***	1.219***	0.111***	0.224***
	(0.009)	(0.000)	(0.016)	(0.138)	(0.013)	(0.033)
p-value H_0 : tax = non-tax	<0.01	.	<0.01	0.05	<0.01	<0.01
			B. REDUCED FORM			
Predicted Tax Revenue	0.237***	0.981***	0.167***	0.908***	0.282***	0.673***
	(0.039)	(0.073)	(0.054)	(0.076)	(0.037)	(0.039)
Predicted Non-Tax Revenue	0.169***	0.619***	0.149***	0.690***	0.0747***	0.165**
	(0.024)	(0.155)	(0.032)	(0.190)	(0.023)	(0.078)
p-value H_0 : tax = non-tax	0.10	0.06	0.76	0.36	<0.01	<0.01
			C. SHIFT-SHARE IV			
Tax Revenue	0.676***	1.000***	0.482***	0.897***	0.768***	0.752***
	(0.022)	(0.000)	(0.080)	(0.046)	(0.041)	(0.017)
Non-Tax Revenue	0.301***	1.000***	0.279***	1.190***	0.0830***	0.0956
	(0.016)	(0.000)	(0.050)	(0.226)	(0.030)	(0.077)
SW F-statistic Tax	139.5	70.0	139.5	70.0	139.5	70.0
SW F-statistic Non-Tax	139.0	20.1	139.0	20.1	139.0	20.1
p-value H_0 : tax = non-tax	<0.01	.	0.06	0.28	<0.01	<0.01
Observations	5098	5099	5098	5098	5098	5098
Cities	316	316	316	316	316	316
City fixed effect	X	X	X	X	X	X
Year fixed effect	X	X	X	X	X	X

Notes: Clustered standard errors at the city-level in parenthesis. Significance levels: * 10% ** 5% *** 1%. This table is based on the empirical specification from (1.3). Unit of observation: city-year. All regressions include city and year fixed effects. Sampled cities correspond to all cities with a population above 30,000 between 1910-1931, and above 100,000 for 1932-1936. Columns 1, 3, and 5 are based on log-log specifications, while columns 2, 4, and 6 are based on level-level specifications. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines. The shift-share research design is detailed in Section 1.3. SW F-statistics corresponds to the Sanderson and Windmeijer (2016) F-test for weak instruments in a model with multiple endogenous variables.

TABLE 1.2: LONG-RUN EFFECTS OF SOURCES OF REVENUE 1910-1936

	City Government				Economics			Demographics		
	Total Revenue	Taxes	Transfers	Expen-ditures	Median Income	Unemp-loyment	Poverty	Population	Share Foreign-ers	Share Non-white
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					A. 1940					
Tax Revenue (Thousand real USD p.c.)	512.54***	523.59***	42.14			4.34*		1.33*		8.48**
	(102.68)	(87.57)	(25.72)			(2.33)		(0.71)		(3.48)
Non-Tax Revenue (Thousand real USD p.c.)	-167.59	-254.72***	-54.03			8.08***		-1.23*		-8.83**
	(129.14)	(97.75)	(41.50)			(2.98)		(0.71)		(4.41)
Mean Dependent Variable	424.5	313.6	68.7			85.1		11.3		8.34
p-value H_0 : tax = non-tax	<0.01	<0.01	0.12			0.42		0.04		0.01
Observations	299	299	298			299		299		299
					B. 1950					
Tax Revenue (Thousand real USD p.c.)	424.62***	445.57***	13.03	508.61***	8.74***	1.07	-4.76	1.39**	0.65	8.81*
	(107.57)	(84.15)	(26.39)	(138.41)	(2.41)	(0.89)	(3.02)	(0.70)	(1.39)	(4.49)
Non-Tax Revenue (Thousand real USD p.c.)	-102.85	-190.17**	-39.40	-185.37	0.67	3.44**	-5.76	-1.10	1.82	-11.31*
	(103.61)	(77.48)	(34.73)	(123.59)	(3.24)	(1.54)	(5.25)	(0.70)	(3.67)	(6.22)
Mean Dependent Variable	443.3	302.9	83.5	486.0	26.1	94.7	20.9	11.4	6.13	9.30
p-value H_0 : tax = non-tax	<0.01	<0.01	0.36	<0.01	0.10	0.28	0.89	0.04	0.81	0.03
Observations	299	299	299	299	299	299	299	299	299	297
					C. 1960					
Tax Revenue (Thousand real USD p.c.)	471.47***	435.36***	61.50	418.36***	13.60***	-1.71	-4.76*	1.30*	8.56***	11.64*
	(181.10)	(75.21)	(50.88)	(161.19)	(3.26)	(1.06)	(2.74)	(0.66)	(3.12)	(6.24)
Non-Tax Revenue (Thousand real USD p.c.)	-36.05	-71.02	-100.04	-58.78	-0.69	-2.53*	-1.90	-0.91	-3.78	-16.37**
	(193.18)	(90.70)	(60.69)	(200.52)	(4.92)	(1.39)	(4.73)	(0.70)	(3.34)	(7.03)
Mean Dependent Variable	642.2	358.8	123.1	670.3	36.0	5.38	17.5	11.5	7.04	11.5
p-value H_0 : tax = non-tax	0.13	<0.01	0.12	0.14	0.05	0.71	0.66	0.06	0.04	0.01
Observations	299	299	299	299	299	299	299	299	299	298
					D. 1970					
Tax Revenue (Thousand real USD p.c.)	1518.53***	968.99***	-286.00	1480.59***	18.64***	-1.88**	-1.72	1.12*	18.41**	21.31*
	(528.76)	(332.29)	(542.46)	(482.54)	(5.00)	(0.89)	(2.12)	(0.63)	(8.03)	(11.29)
Non-Tax Revenue (Thousand real USD p.c.)	-839.72	-449.70	-93.33	-622.56	-0.71	-1.88	-6.90***	-0.84	-7.67	-22.98***
	(556.25)	(273.79)	(406.85)	(476.72)	(6.30)	(1.21)	(2.10)	(0.71)	(9.62)	(8.51)
Mean Dependent Variable	1543.9	731.0	581.4	1255.1	46.0	4.69	10.1	11.6	20.4	14.9
p-value H_0 : tax = non-tax	0.02	0.01	0.83	0.01	0.05	1.00	0.14	0.09	0.11	<0.01
Observations	299	299	299	299	299	299	299	298	299	299
					E. 1980					
Tax Revenue (Thousand real USD p.c.)	1362.42***	935.25***	365.77	1220.34***	15.43***	-2.84	-1.93	1.12*		26.28*
	(523.41)	(216.65)	(284.72)	(448.91)	(5.84)	(2.23)	(3.97)	(0.62)		(15.74)
Non-Tax Revenue (Thousand real USD p.c.)	-1262.59**	-550.37***	-543.80*	-1051.62**	-0.67	-1.60	-8.84***	-0.92		-27.95**
	(551.24)	(182.00)	(320.72)	(465.15)	(6.58)	(2.22)	(3.22)	(0.72)		(11.26)
Mean Dependent Variable	1381.3	554.2	522.6	1365.9	38.3	7.75	11.7	11.5		22.1
p-value H_0 : tax = non-tax	<0.01	<0.01	0.10	<0.01	0.14	0.75	0.28	0.07		0.02
Observations	283	283	283	283	298	298	298	299		298
					F. 1990					
Tax Revenue (Thousand real USD p.c.)	-896.29	-118.95	297.38	1541.02**	28.49***	-2.56	-6.16	1.25*	20.17***	
	(3844.23)	(2263.25)	(388.86)	(710.47)	(9.58)	(1.85)	(4.11)	(0.66)	(6.87)	
Non-Tax Revenue (Thousand real USD p.c.)	12426.91	7627.90	-505.24	-771.07	-1.46	-3.78**	-6.23	-1.09	-16.26	
	(15836.56)	(9310.09)	(377.41)	(637.68)	(9.12)	(1.82)	(4.50)	(0.75)	(12.63)	
Mean Dependent Variable	4602.3	2103.1	517.9	1721.4	52.0	8.20	13.9	11.6	8.45	
p-value H_0 : tax = non-tax	0.49	0.50	0.25	0.06	0.07	0.70	0.99	0.05	0.05	
Observations	278	278	258	278	284	284	284	284	283	
					G. 2000					
Tax Revenue (Thousand real USD p.c.)	2445.74***	1601.62***		2538.58***		-1.11		1.30**		15.39
	(904.57)	(357.95)		(940.81)		(1.34)		(0.65)		(15.11)
Non-Tax Revenue (Thousand real USD p.c.)	-1145.19	-576.27*		-1389.39		-2.24		-1.17		-20.29
	(889.51)	(315.28)		(926.01)		(1.57)		(0.77)		(12.85)
Mean Dependent Variable	1939.2	785.8		1920.0		4.81		11.6		33.9
p-value H_0 : tax = non-tax	0.03	<0.01		0.02		0.66		0.04		0.14
Observations	262	262		262		284		284		284
SW F-statistic Tax	765.7	765.7		765.7		988.2		988.2		988.2
SW F-statistic Non-Tax	478.6	478.6		478.6		471.0		471.0		471.0

Notes: Clustered standard errors at the city-level in parenthesis. Significance levels: * 10% ** 5% *** 1%. This table is based on the empirical specification from (1.5). Unit of observation: city. Each panel corresponds to outcomes measured in a different decade. All regressions include state fixed effects. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines. The instruments are the average shift-share predicted tax and non-tax revenue over 1910-1936. Details on data construction is provided in the Appendix.

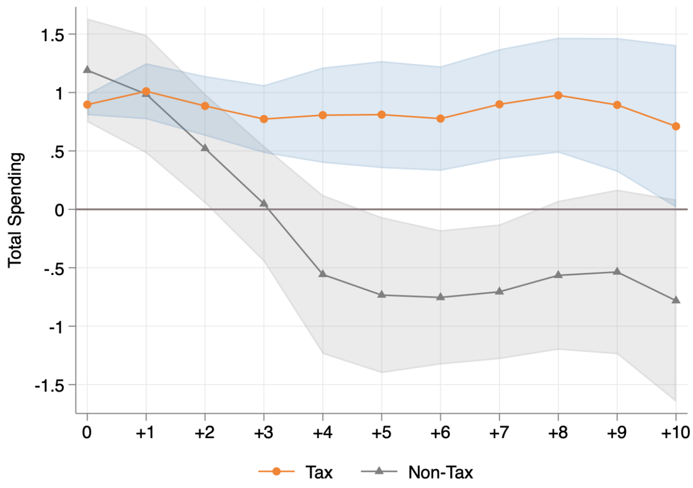
Table 1.3: Long-Run Effects of Sources of Revenue: Additional Outcomes

	Health			Education			Politics	
	Death rate	Hospitals	Hospital Beds	Number of Teachers	Years of Education	School enrollment	Democratic Newspapers	Corruption Public Officials
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. 1940				
Tax Revenue (Thousand real USD p.c.)	-3.37			0.73	2.21***	1.34	10.34	
	(2.16)			(0.69)	(0.84)	(2.03)	(24.17)	
Non-Tax Revenue (Thousand real USD p.c.)	1.28			-1.01	0.78	-3.81	-18.66	
	(3.52)			(1.41)	(0.72)	(4.40)	(24.89)	
p-value H_0 : tax = non-tax	0.37			0.37	0.28	0.40	0.52	
Observations	299			279	299	279	259	
				B. 1950				
Tax Revenue (Thousand real USD p.c.)	-1.28	0.02	-113.14	1.30	2.79***	1.04	-10.66	
	(1.15)	(0.18)	(110.32)	(1.12)	(0.71)	(2.25)	(24.42)	
Non-Tax Revenue (Thousand real USD p.c.)	1.09	-0.32	-109.31	-0.49	0.48	-2.96	-16.99	
	(2.14)	(0.24)	(155.10)	(2.18)	(0.73)	(5.54)	(27.05)	
p-value H_0 : tax = non-tax	0.42	0.38	0.99	0.55	0.07	0.59	0.89	
Observations	299	299	299	277	299	279	259	
				C. 1960				
Tax Revenue (Thousand real USD p.c.)	-2.43	-0.02	-202.23		2.51***	-1.11	11.23	
	(1.48)	(0.16)	(124.59)		(0.59)	(1.84)	(26.50)	
Non-Tax Revenue (Thousand real USD p.c.)	8.82*	-0.28	37.69		-0.09	-5.07**	-20.80	
	(5.01)	(0.24)	(165.88)		(0.66)	(2.02)	(25.64)	
p-value H_0 : tax = non-tax	0.07	0.49	0.38		0.02	0.23	0.50	
Observations	299	299	299		299	299	259	
				D. 1970				
Tax Revenue (Thousand real USD p.c.)	-1.17	-0.13	-201.67**		1.19**	-1.34	0.72	-2.66*
	(1.53)	(0.18)	(101.33)		(0.53)	(1.69)	(23.89)	(1.48)
Non-Tax Revenue (Thousand real USD p.c.)	5.26	-0.10	80.35		0.22	-2.81	-7.66	4.00
	(3.19)	(0.20)	(123.95)		(0.59)	(2.79)	(24.63)	(2.83)
p-value H_0 : tax = non-tax	0.13	0.94	0.18		0.34	0.71	0.85	0.09
Observations	299	298	298		299	298	259	290
				E. 1980				
Tax Revenue (Thousand real USD p.c.)	-0.42	-0.06	-98.14			0.55	1.92	3.45
	(1.43)	(0.18)	(60.66)			(2.20)	(24.31)	(2.97)
Non-Tax Revenue (Thousand real USD p.c.)	5.21**	-0.15	15.69			-8.42***	-8.10	0.65
	(2.44)	(0.22)	(78.68)			(2.94)	(23.92)	(4.71)
p-value H_0 : tax = non-tax	0.10	0.80	0.37			0.04	0.82	0.69
Observations	296	283	283			283	257	291
				F. 1990				
Tax Revenue (Thousand real USD p.c.)	-1.30	-0.14	-34.35			-0.24	14.46	1.21
	(1.37)	(0.13)	(32.49)			(2.02)	(28.32)	(6.31)
Non-Tax Revenue (Thousand real USD p.c.)	4.78**	0.03	6.57			-7.69***	11.58	24.31
	(2.17)	(0.14)	(52.03)			(2.80)	(31.21)	(17.16)
p-value H_0 : tax = non-tax	0.06	0.46	0.58			0.07	0.96	0.29
Observations	284	284	284			284	256	310
				G. 2000				
Tax Revenue (Thousand real USD p.c.)							10.26	12.32
							(36.65)	(7.79)
Non-Tax Revenue (Thousand real USD p.c.)							12.83	0.57
							(33.08)	(11.98)
p-value H_0 : tax = non-tax							0.97	0.51
Observations							242	309
SW F-statistic Tax							372.1	1032.7
SW F-statistic Non-Tax							475.6	521.2

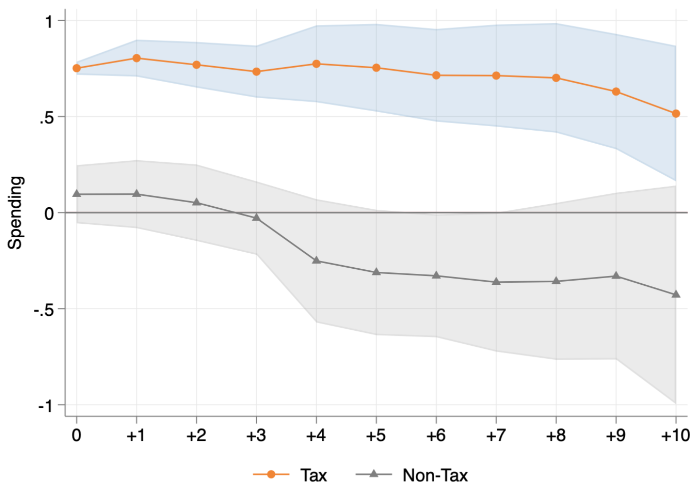
Notes: Clustered standard errors at the city-level in parenthesis. Significance levels: * 10% ** 5% *** 1%. This table is based on the empirical specification from (1.5). Unit of observation: city. Each panel corresponds to outcomes measured in a different decade. All regressions include state fixed effects. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines. The instruments are the average shift-share predicted tax and non-tax revenue over 1910-1936. Details on data construction is provided in the Appendix.

Figures

FIGURE 1.1: SOURCES OF REVENUE AND PERSISTENCE OF SPENDING



(a) Total Spending



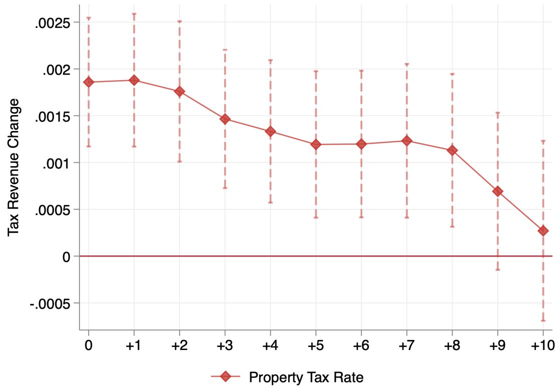
(b) Service Provision

Notes: Both figures compare the estimates of additional spending caused by an increase in tax vs non-tax revenue, based on Equation (1.4). Shaded areas correspond to the 95% confidence interval. Service provision corresponds to spending on services provided by departments of the city in the area of general government, education, health, sanitation, highway, public safety, welfare, and recreation. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines.

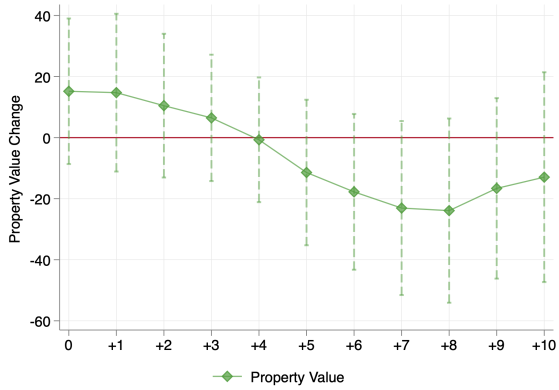
FIGURE 1.2: SOURCES OF REVENUE AND PERSISTENCE OF REVENUE

Notes: This figure shows the persistence in revenue coming out of tax revenue vs non-tax revenue. Shaded areas correspond to the 95% confidence interval. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines.

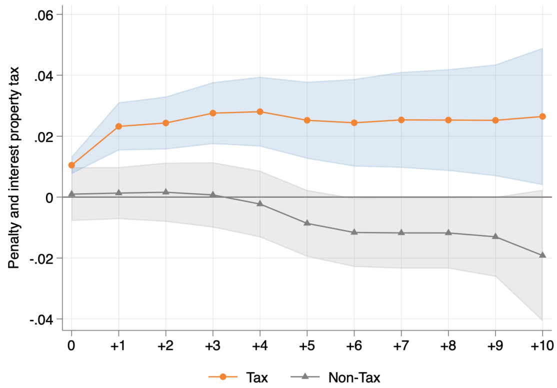
FIGURE 1.3: TAX REVENUE PERSISTENCE AND FISCAL CAPACITY



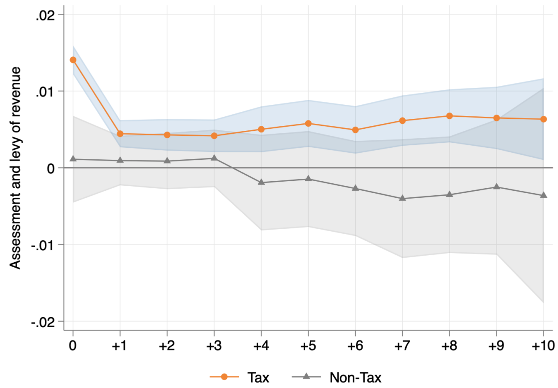
(a) Effective Tax Rate on Property



(b) Tax Base Value on Property



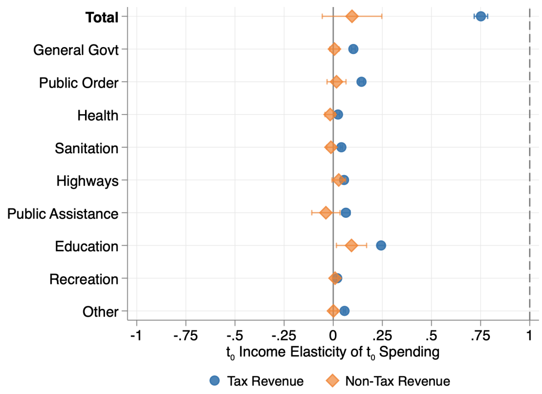
(c) Enforcement (Revenue from Tax Penalty on Property)



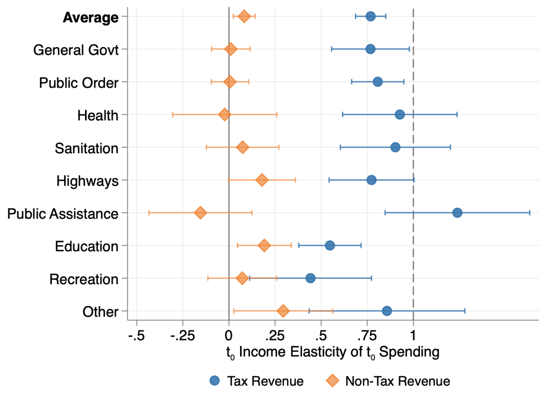
(d) Revenue Collection Capacity

Notes: These figures show the association between tax revenue increase and local fiscal capacity. Panels A and B provide indirect measures of increased fiscal capacity through effective tax rates and tax base value for the general property tax. Panel C measures fiscal capacity through enforcement, as the revenue from property tax penalty. Panel D measures the amount of public revenue spent for assessment and levy of revenue. Point estimates are derived from Equation (1.4). Shaded areas correspond to the 95% confidence interval. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines.

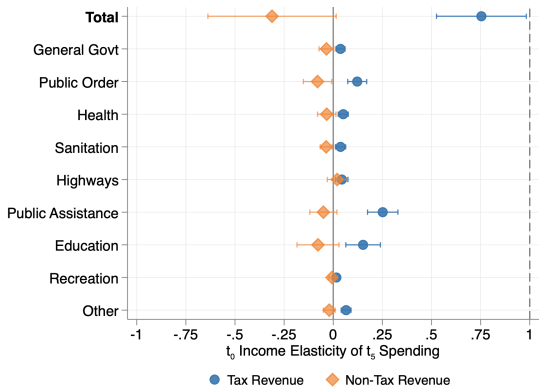
FIGURE 1.4: SOURCES OF REVENUE AND PERSISTENCE OF SERVICE PROVISION: DECOMPOSITION



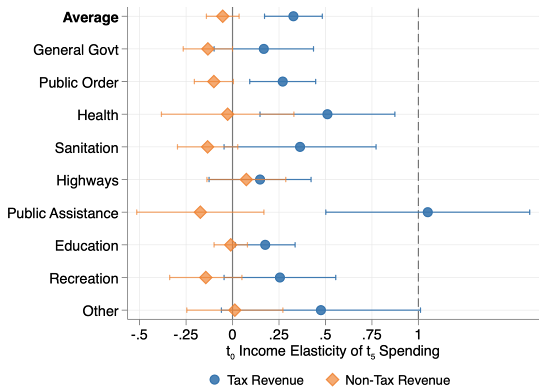
(a) At Time 0, Level



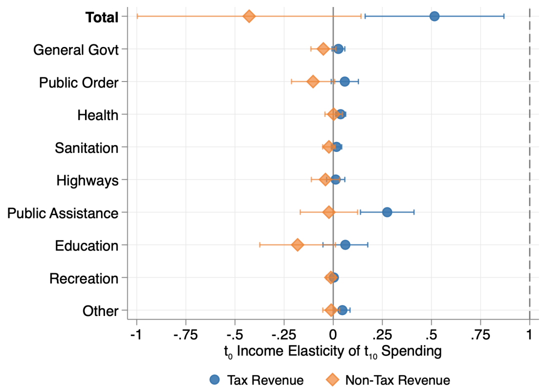
(b) At Time 0, Log



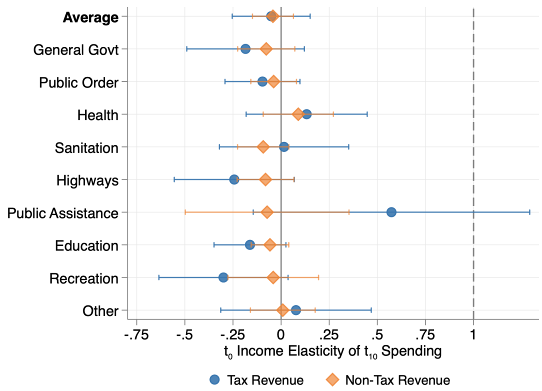
(c) At Time +5, Level



(d) At Time +5, Log



(e) At Time +10, Level



(f) At Time +10, Log

Notes: These figures show the spending effects on different services over time by source of revenue. Point estimates are derived from Equation (1.4). Shaded areas correspond to the 95% confidence interval. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines.

Appendix to Chapter 1

Appendix Tables and Figures

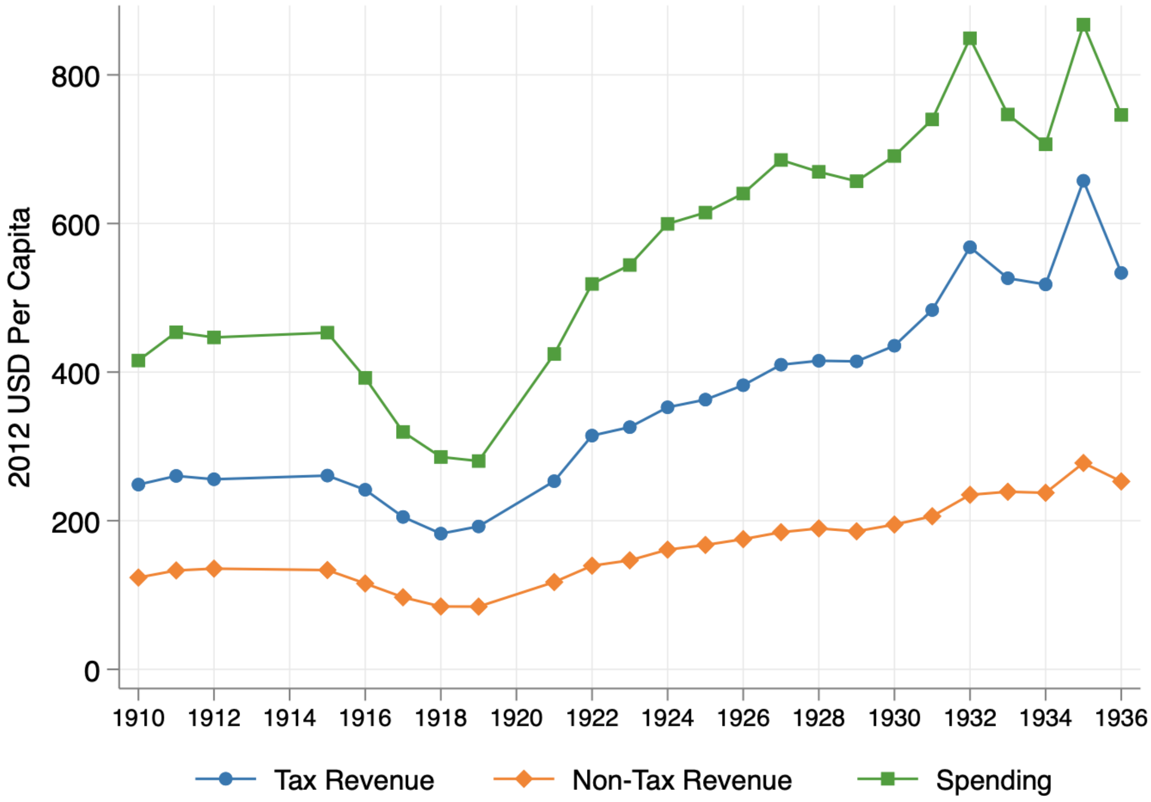


FIGURE A1.1: TRENDS IN MUNICIPAL REVENUE AND SPENDING

Notes: This graph shows the evolution of municipal spending and revenue, using the average value for cities sampled in the Financial Statistics of Cities. Sampled cities correspond to all cities with a population above 30,000 between 1910-1931, and above 100,000 for 1932-1936. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines.

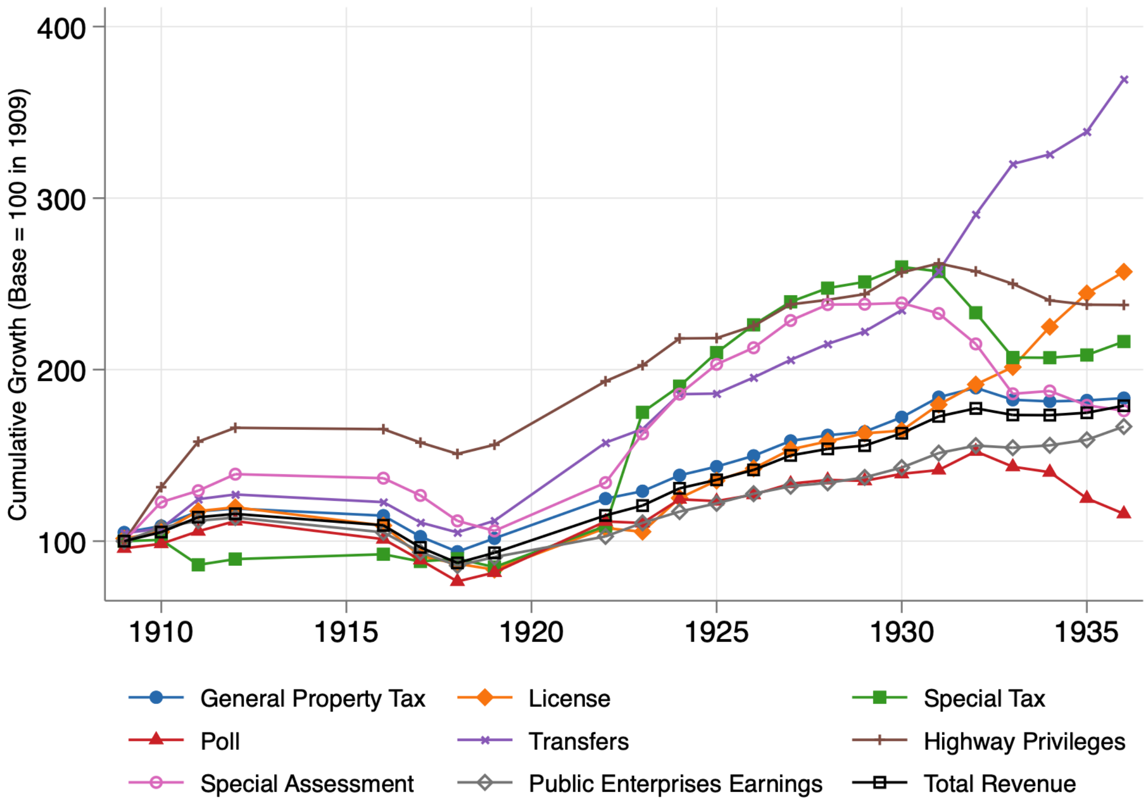


FIGURE A1.2: CUMULATIVE REVENUE CHANGE FROM MUNICIPAL REVENUE CATEGORIES

Notes: This graph shows the evolution of municipal revenue by category of revenue using the average value for cities sampled in the Financial Statistics of Cities. Sampled cities correspond to all cities with a population above 30,000 between 1910-1931, and above 100,000 for 1932-1936.

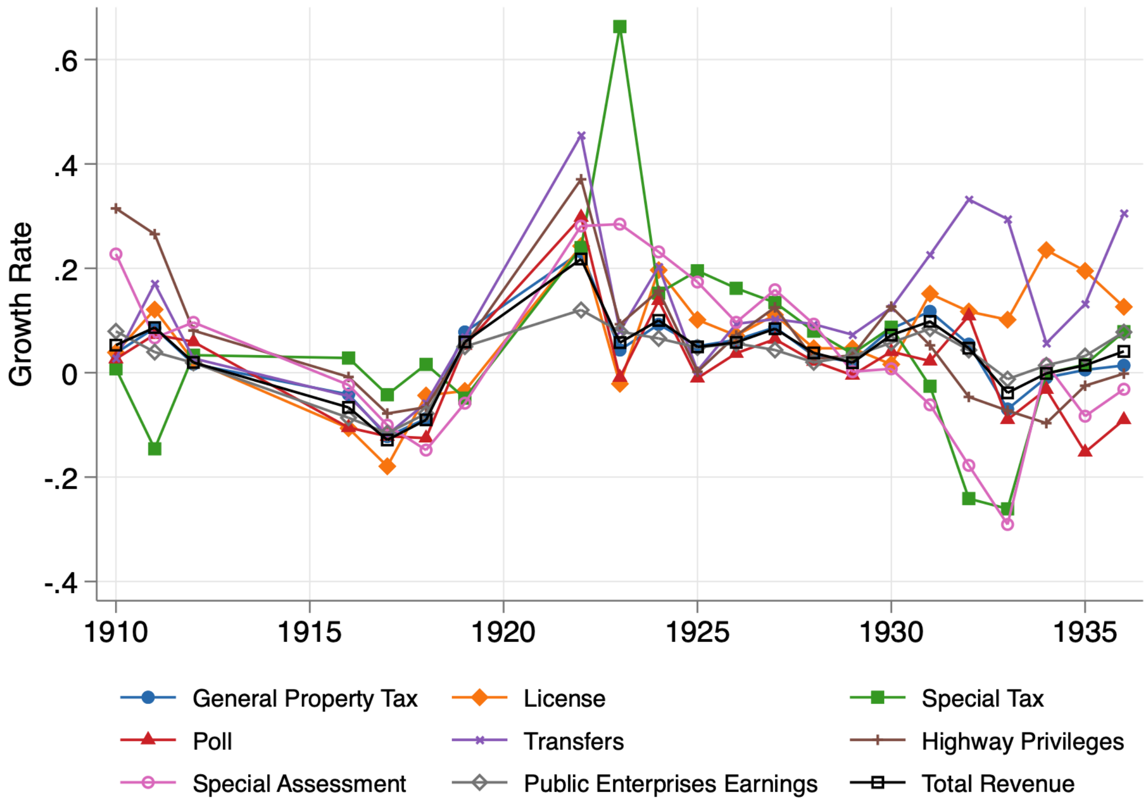


FIGURE A1.3: REVENUE CHANGE FROM MUNICIPAL REVENUE CATEGORIES

Notes: This graph shows the evolution of municipal revenue by category of revenue using the average value for cities sampled in the Financial Statistics of Cities. Sampled cities correspond to all cities with a population above 30,000 between 1910-1931, and above 100,000 for 1932-1936.

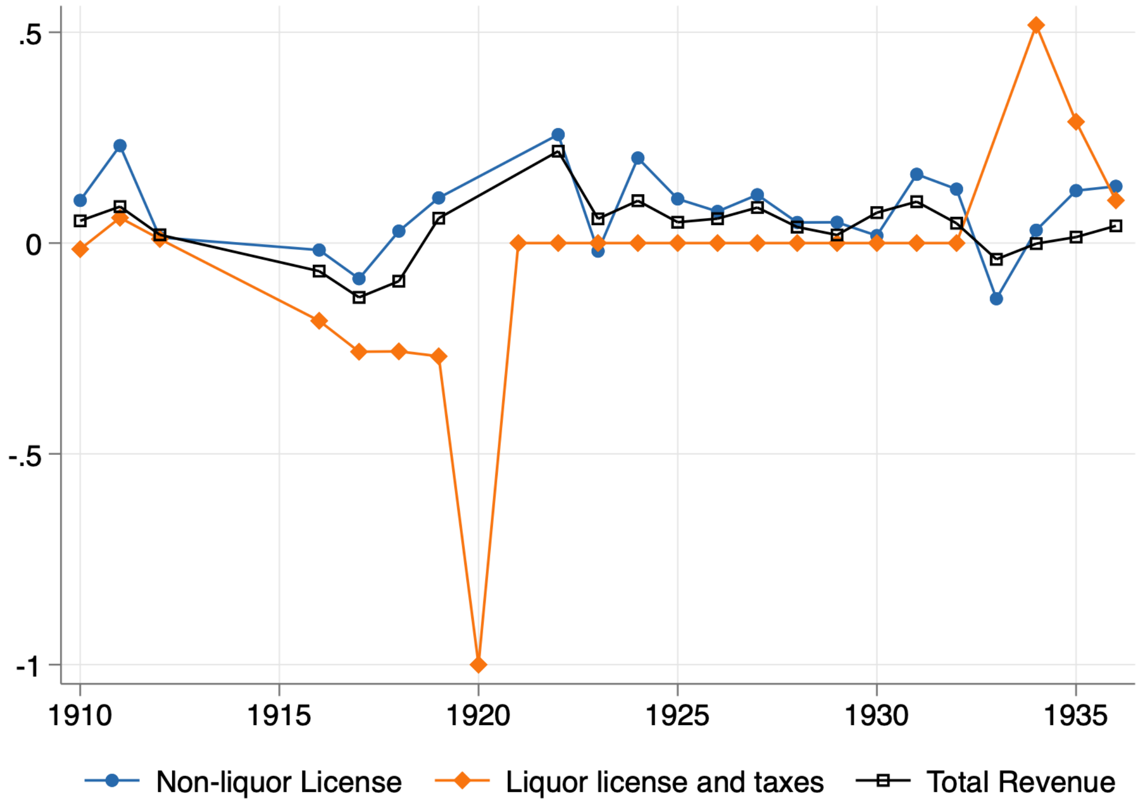


FIGURE A1.4: REVENUE CHANGE FROM MUNICIPAL LICENSE TAX

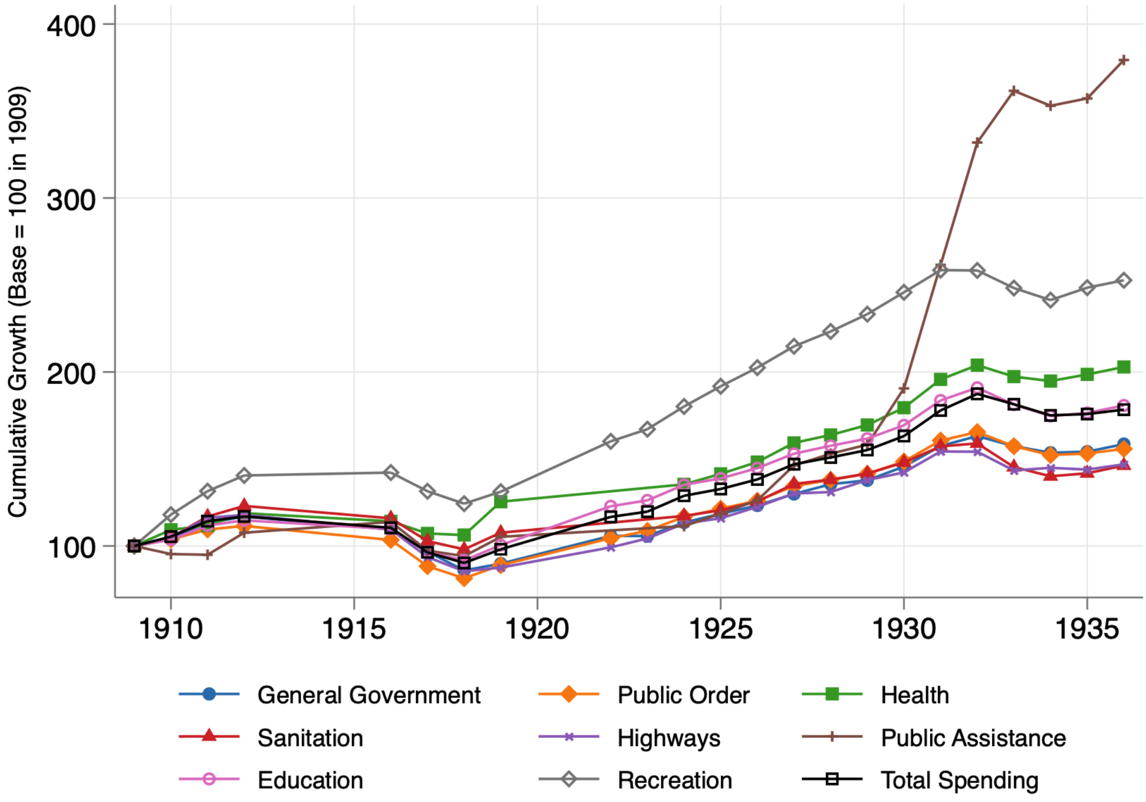


FIGURE A1.5: CUMULATIVE REVENUE CHANGE FROM MUNICIPAL SPENDING CATEGORIES

Notes: This graph shows the evolution of municipal spending by category of revenue using the average value for cities sampled in the Financial Statistics of Cities. Sampled cities correspond to all cities with a population above 30,000 between 1910-1931, and above 100,000 for 1932-1936.

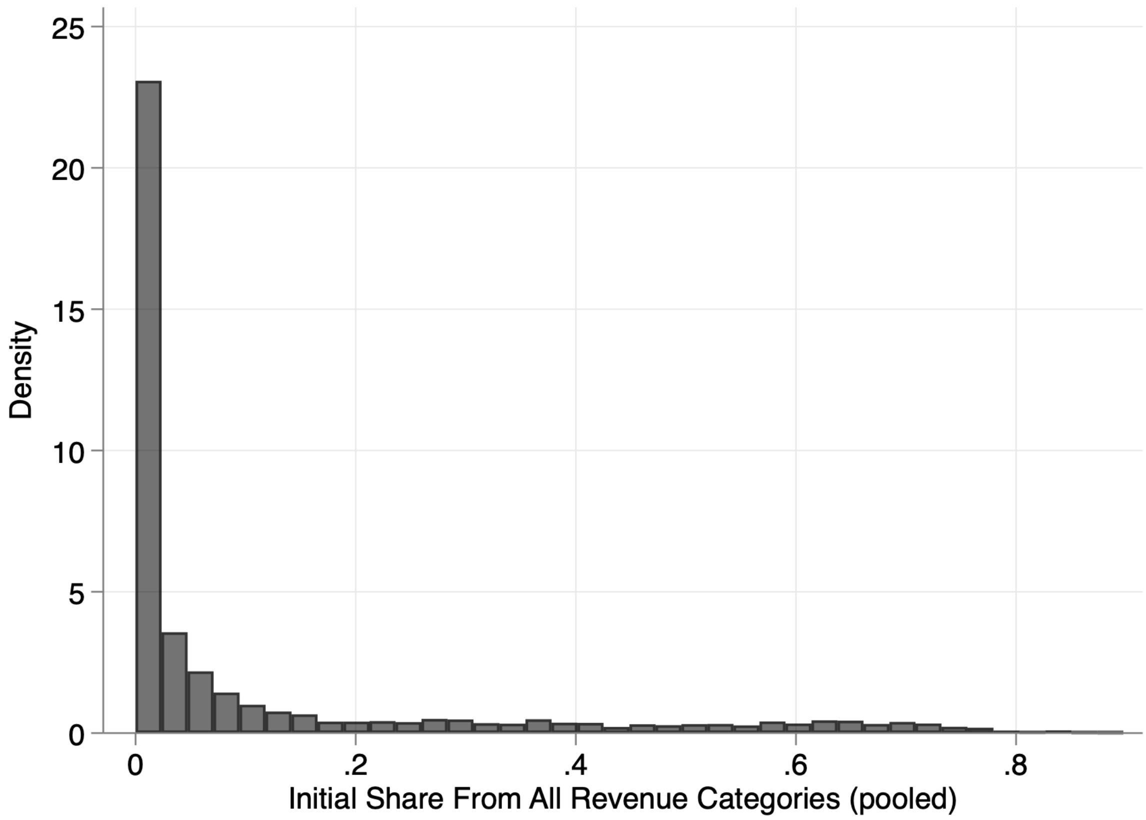


FIGURE A1.6: DISTRIBUTION OF EXPOSURE WEIGHTS

Notes: This graph shows the distribution of revenue share used in the shift-share instrument across all cities. The share is the first share observed for a city-revenue category, the majority of which are observed in 1909. See text for description of revenue categories.

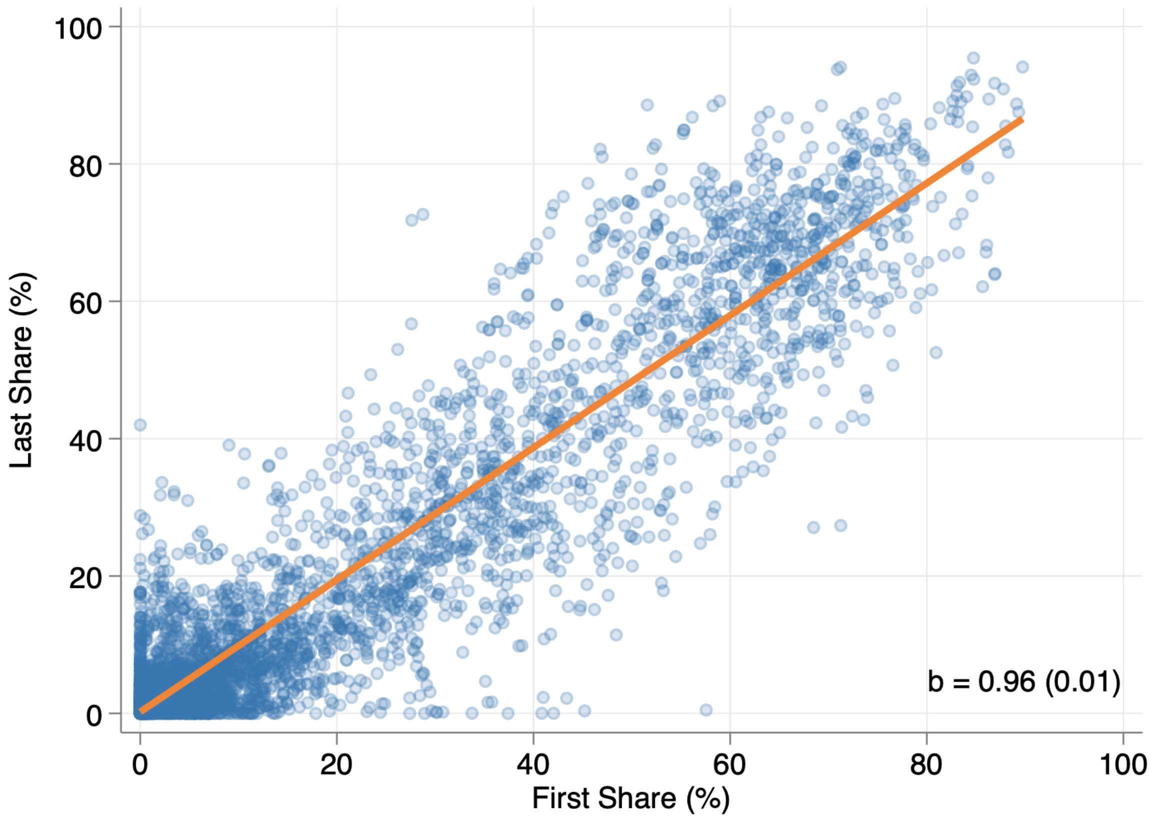
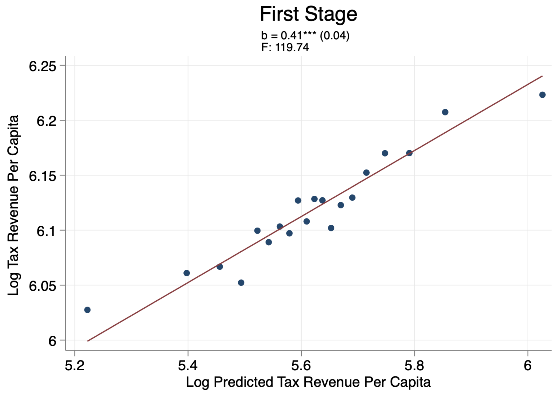


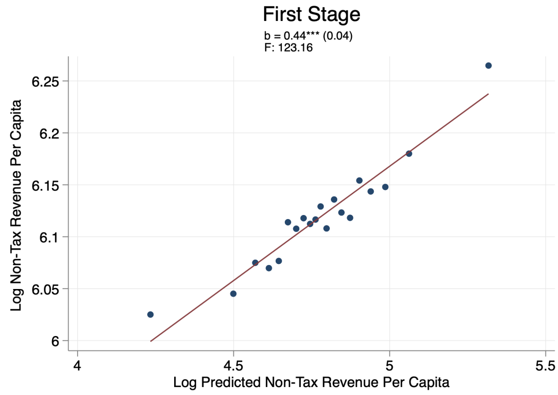
FIGURE A1.7: PERSISTENCE OF SHARES

Notes: This figure illustrates the persistence of revenue composition among cities. It plots the last observed share of revenue against the first observed share of revenue for all city-revenue categories.

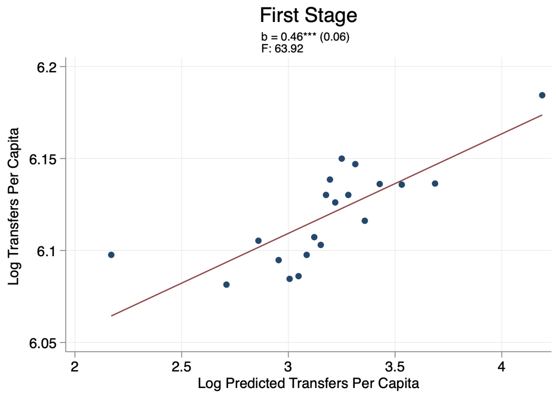
FIGURE A1.8: FIRST STAGE (USING ONLY ONE REVENUE CATEGORY AS INSTRUMENT)



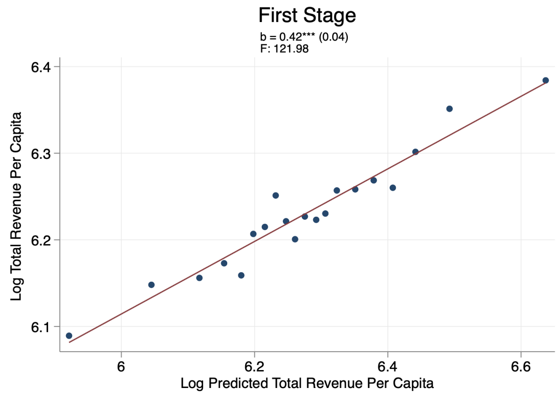
(a) Instrument = Tax Revenue



(b) Instrument = Non-Tax Revenue



(c) Instrument = Transfers



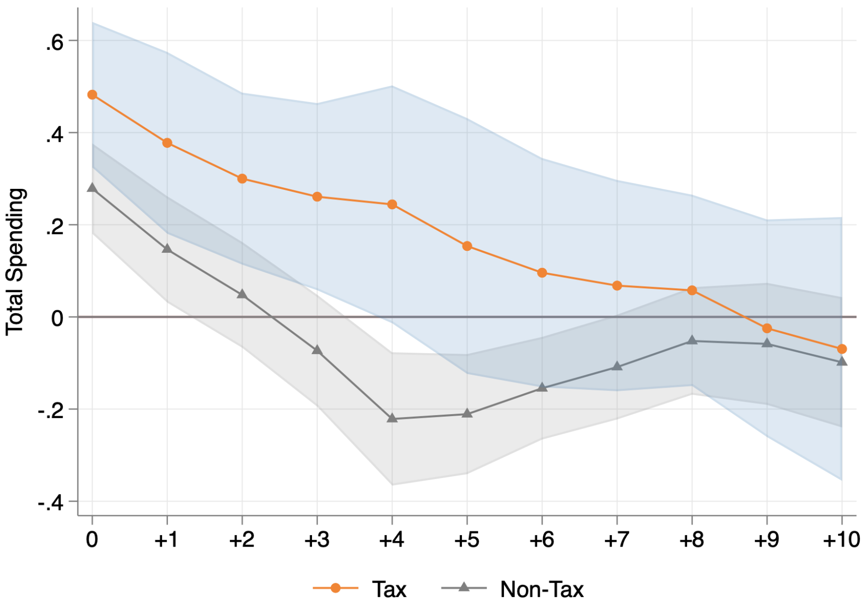
(d) Instrument = All Categories, Balanced Panel of 87 Cities

Notes: These figures illustrate the predictive power of the shift-share instrument by showing the relationship between actual and predicted revenue.

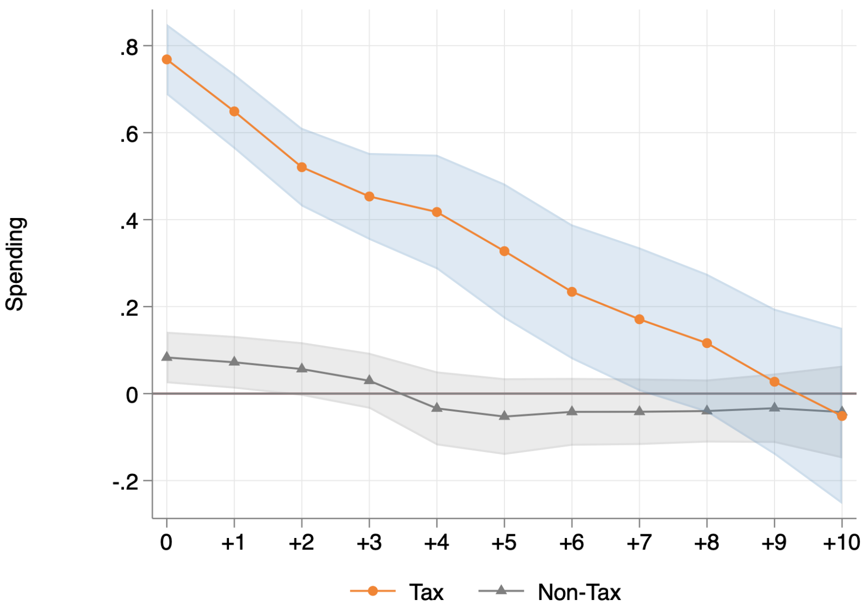
FIGURE A1.9: REVENUE PERSISTENCE BY SOURCE OF REVENUE: ELASTICITY

Notes: This graph shows the persistence of revenue. It depicts point estimates based on Equation (1.4) using a log-log specification. Shaded areas correspond to 95% confidence intervals. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines.

FIGURE A1.10: SOURCES OF REVENUE AND PERSISTENCE OF SPENDING: LOG



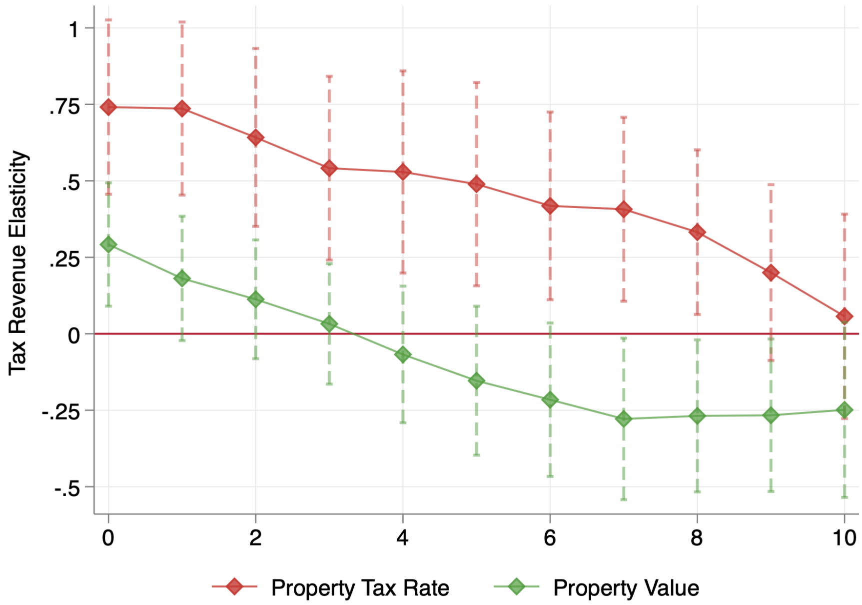
(a) Total Spending



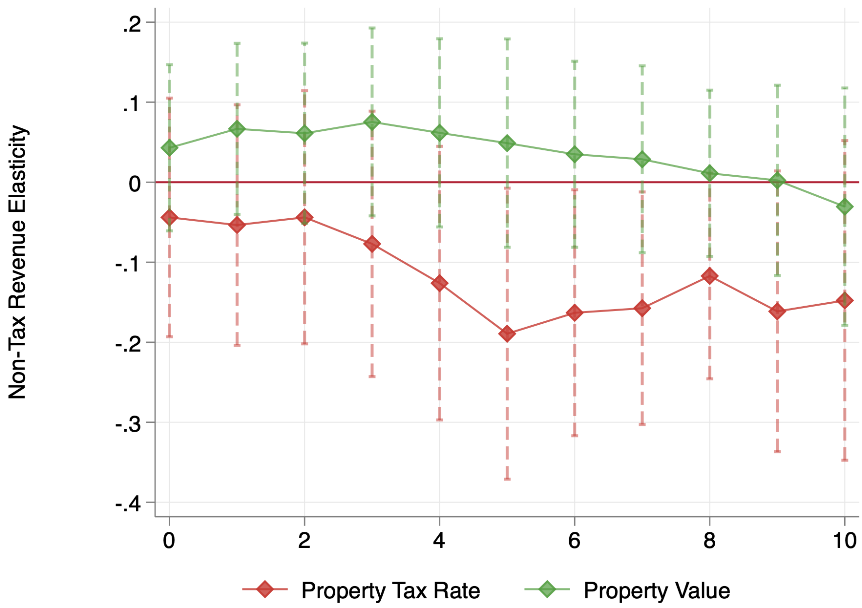
(b) Service Provision

Notes: This graph shows the difference in persistence of spending effects by source of revenue. It depicts point estimates based on Equation (1.4) using a log-log specification. Shaded areas correspond to 95% confidence intervals. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines.

FIGURE A1.11: MECHANISMS OF REVENUE PERSISTENCE



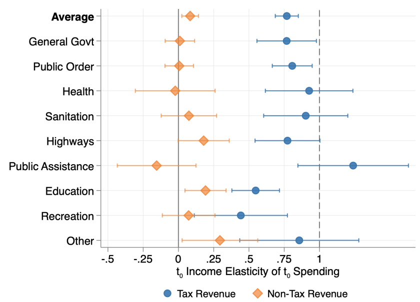
(a) Tax Increase



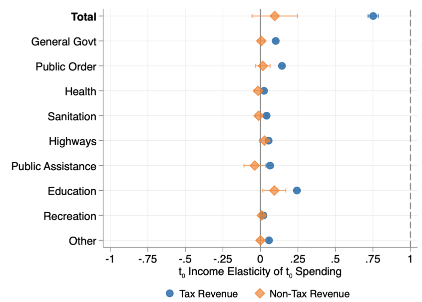
(b) Non-Tax Increasel

Notes: This graph shows the association between revenue increase and fiscal capacity. It depicts point estimates for effective tax revenue and tax base value of the property tax based on Equation (1.4) using a log-log specification. Dashed lines correspond to 95% confidence intervals. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines.

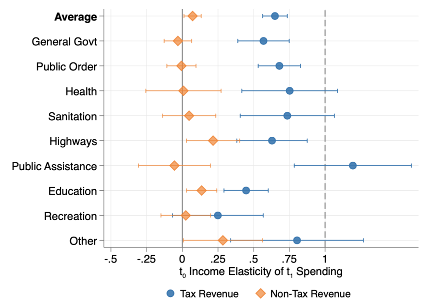
FIGURE A1.12: SPENDING DECOMPOSITION OVER TIME (0 TO +7)



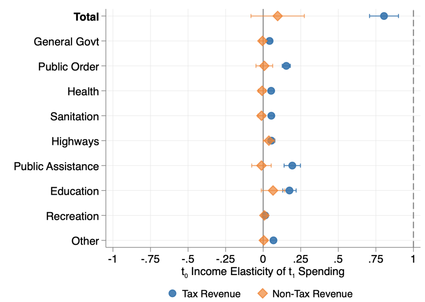
(a) At Time 0, Log



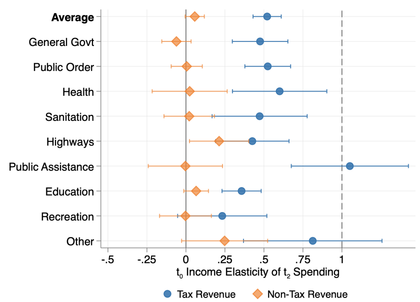
(b) At Time 0, Level



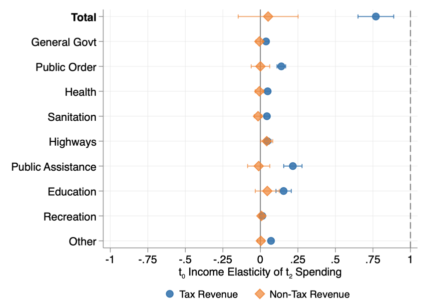
(c) At Time +1, Log



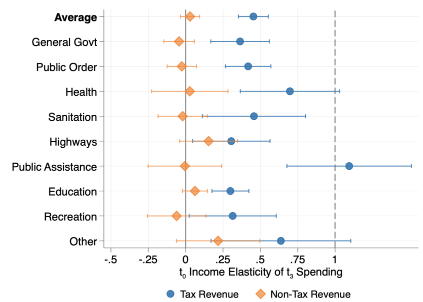
(d) At Time +1, Level



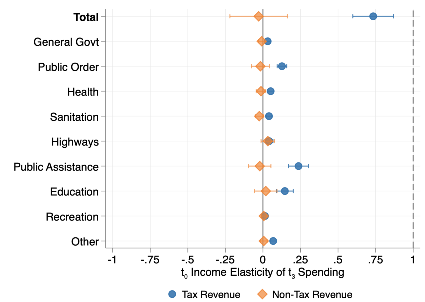
(e) At Time +2, Log



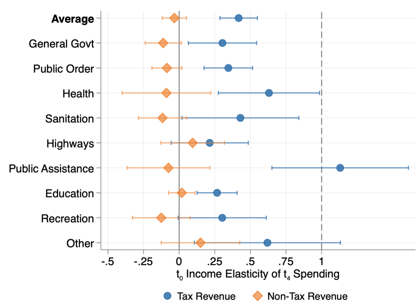
(f) At Time +2, Level



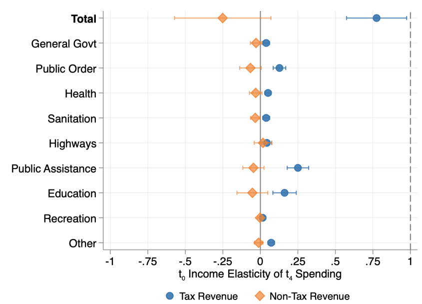
(g) At Time +3, Log



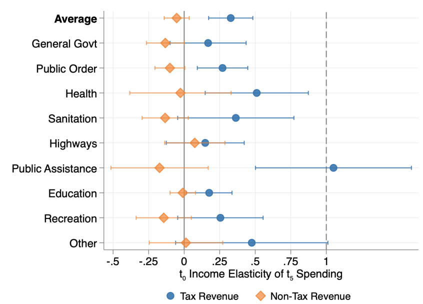
(h) At Time +3, Level



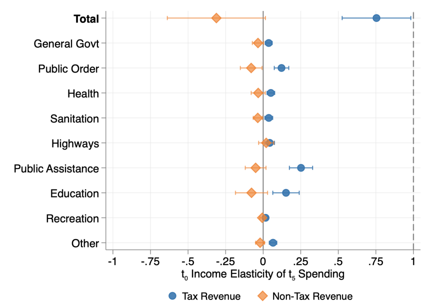
(i) At Time +4, Log



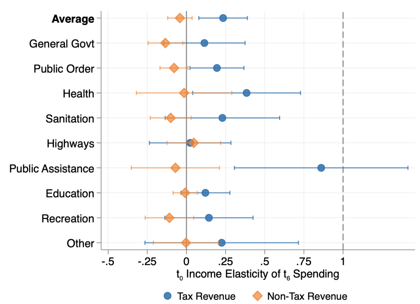
(j) At Time +4, Level

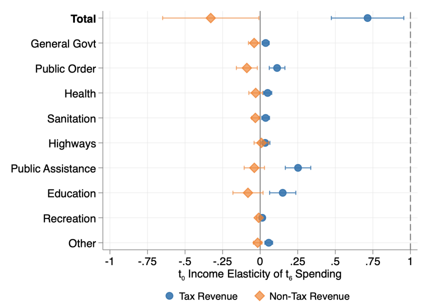


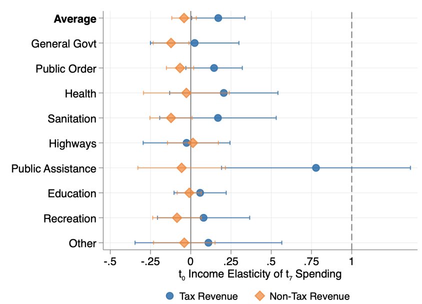
(k) At Time +5, Log



(l) At Time +5, Level







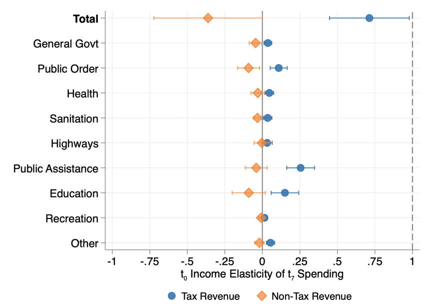
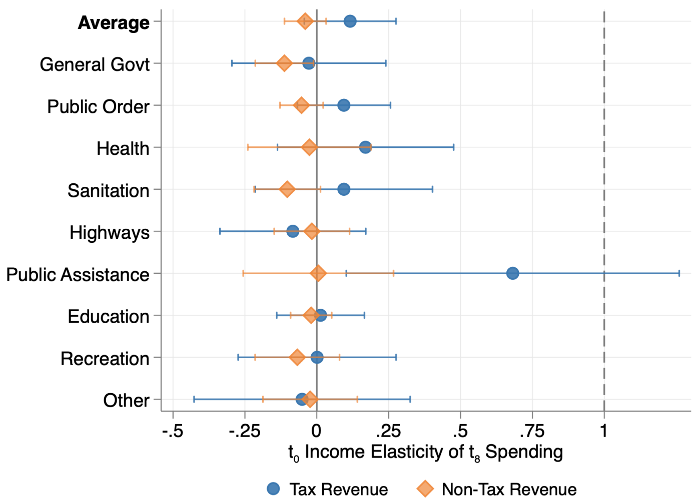
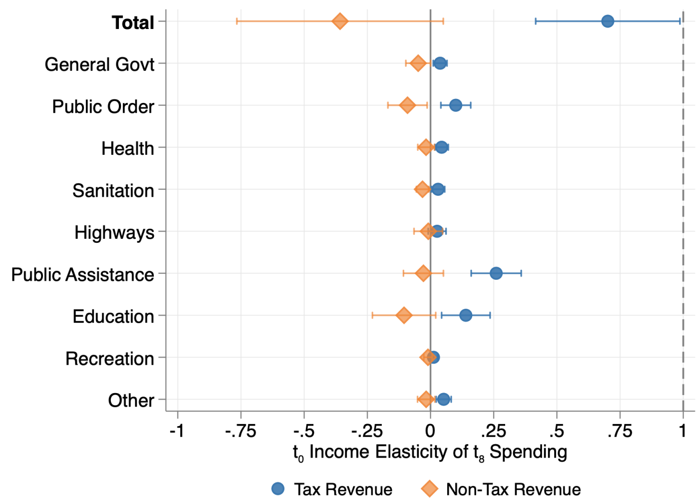


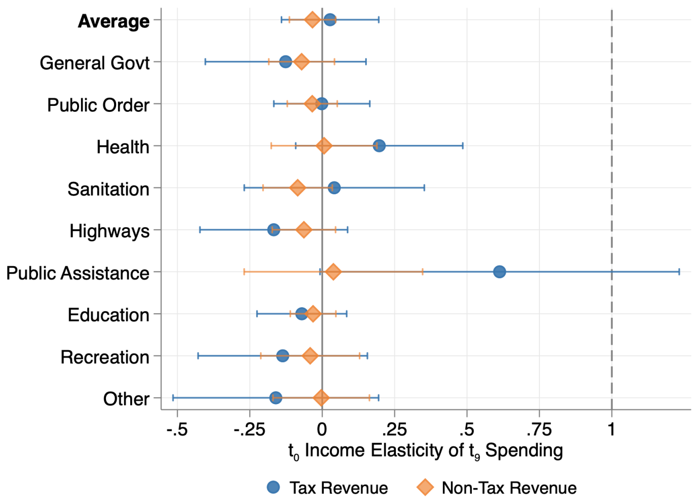
FIGURE A1.13: SPENDING DECOMPOSITION OVER TIME (+8 TO +10)



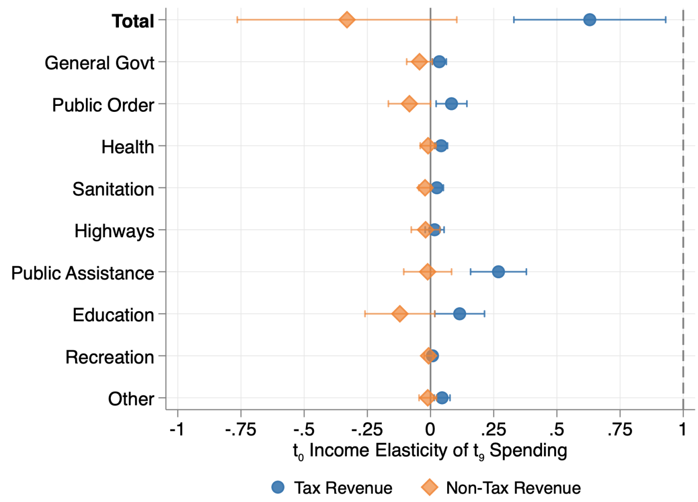
(a) At Time +8, Log



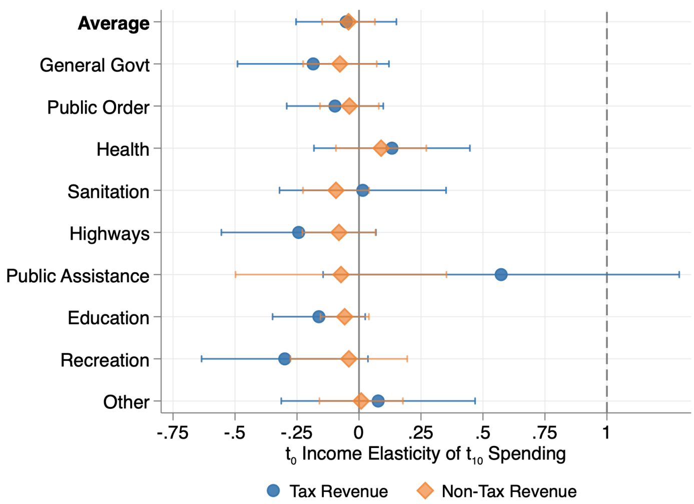
(b) At Time +8, Level



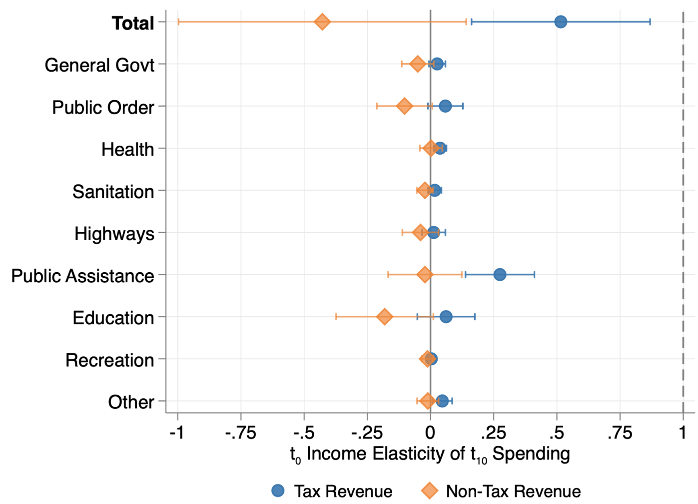
(c) At Time +9, Log



(d) At Time +9, Level



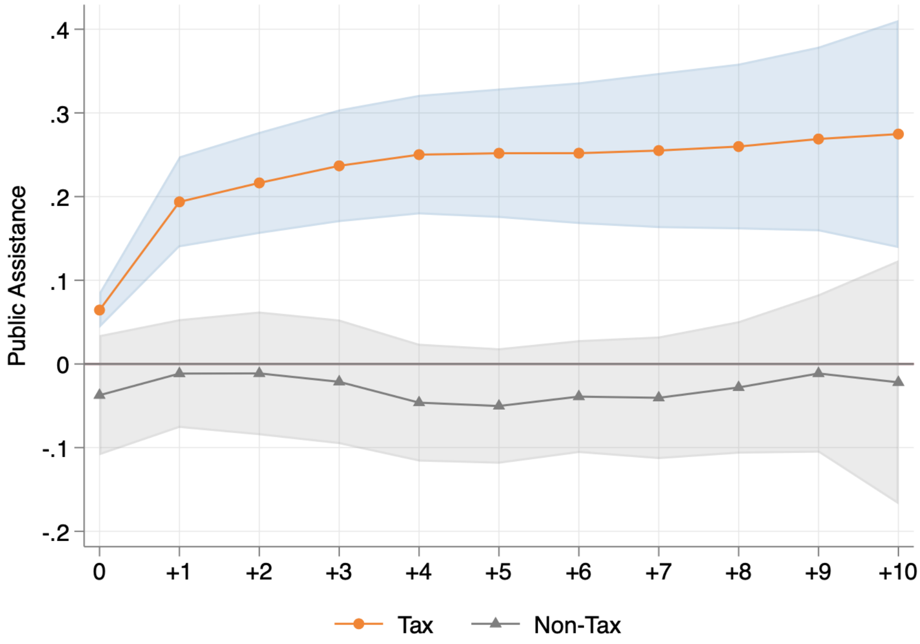
(e) At Time +10, Log



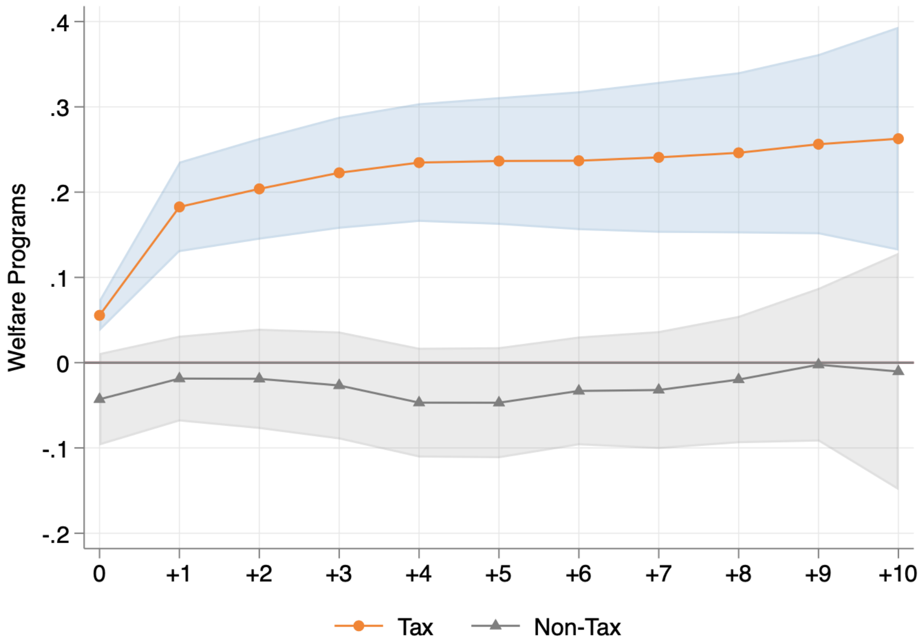
(f) At Time +10, Level

Notes: These figures show the decomposition of spending effects over time by source of revenue using Equation (1.4). Solid lines correspond to 95% confidence intervals. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines.

FIGURE A1.14: SOURCES OF REVENUE AND PERSISTENCE OF SPENDING: THE CASE OF PUBLIC ASSISTANCE



(a) Public Assistance Spending



(b) Public Assistance Spending on Welfare Programs

Notes: These figures show the spending effects on public assistance by source of revenue using Equation (1.4). Shaded areas correspond to 95% confidence intervals. Public assistance corresponds to all payments by the city for the care of children or of the insane, blind, or sick. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines.

TABLE A1.1: FIRST STAGE

	Tax Revenue		Non-Tax Revenue	
	Log	Level	Log	Level
	(1)	(2)	(3)	(4)
Log Predicted Tax Revenue	0.37***		-0.04	
	(0.04)		(0.05)	
Log Predicted Non-Tax Revenue	0.05**		0.45***	
	(0.02)		(0.04)	
Predicted Tax Revenue		0.88***		0.10***
		(0.05)		(0.03)
Predicted Non-Tax Revenue		0.16**		0.46***
		(0.07)		(0.10)
F-statistic	45.8	186.4	67.8	57.8
Sanderson-Windmeijer F-statistic	139.5	70.0	139.0	20.1
Observations	5098	5098	5098	5098
Cities	316	316	316	316
City fixed effect	X	X	X	X
Year fixed effect	X	X	X	X

Notes: Clustered standard errors at the city-level in parenthesis. Significance levels: * 10% ** 5% *** 1%. Unit of observation: city-year. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines.

Table A1.2: Balance Test

	Endogenous Variable Log Revenue Per Capita		Instrument Log Predicted Revenue Per Capita	
% Male	0.126**	(0.0489)	-0.0401	(0.0714)
% White	-0.403**	(0.173)	-0.521**	(0.248)
% Foreigner	0.133	(0.117)	-0.111	(0.131)
% Literate	0.140	(0.0872)	0.0404	(0.113)
% Aged 13-65	0.0971**	(0.0393)	0.0382	(0.0496)
Ratio Owned/Rented Households	0.0240	(0.0637)	0.00422	(0.0592)
Occupation Score	-0.0777***	(0.0287)	-0.0153	(0.0426)
% Employed in Agriculture	0.0653	(0.0790)	0.0908	(0.0865)
% Employed in Mining	0.00388	(0.0898)	-0.00142	(0.118)
Share Employed in Manufacturing	-0.217***	(0.0568)	-0.121*	(0.0667)
% Employed In Production of Non-Durable Goods	-0.0621	(0.0824)	-0.0607	(0.100)
% Employed In Transportation	0.0201	(0.0676)	0.0620	(0.0723)
% Employed In Retail	0.0303	(0.0698)	0.110	(0.0729)
% Employed In Finance	0.0795*	(0.0436)	-0.0425	(0.0648)
% Employed In Business	0.282***	(0.0908)	0.103	(0.112)
Railroad Length	-0.482	(0.377)	0.604	(0.576)
Observations	4492		4492	
Cities	288		288	
Period	1909-1936		1909-1936	
City fixed effect	X		X	
Year fixed effect	X		X	

Notes: Clustered standard errors at the city-level in parenthesis. Significance levels: * 10% ** 5% *** 1%. Independent variables are interpolated from Census years. See text for details on construction. Predicted revenue is based on a shift-share instrument using all revenue categories, see Section 1.3 for details.

Table A1.3: Shock Summary Statistics

	(1)	(2)	(3)
A. Revenue Source Shocks			
Mean	0.05	0.05	0.00
Standard Deviation	0.10	0.10	0.06
Interquartile Range	0.08	0.08	0.03
Intra-Cluster Correlation	0.04	0.04	0.08
	(0.03)	(0.03)	(0.05)
B. Exposure Weights			
Mean	0.00	0.00	0.00
Standard Deviation	0.00	0.00	0.00
Max	0.01	0.01	0.01
C. Shock Sample Size			
# of source-period shocks	698	698	698
# of revenue sources	28	28	28
Effective sample size (1/HHI of weights)	178	178	178
Excluding missing source		X	X
Residuals on year FE source			X

Table A1.4: Balance Test (Shock-Level)

	Endogenous Variable Log Revenue Per Capita		Shock National Growth in Revenue Source	
% Male	0.0695*	(0.0385)	-0.114	(0.477)
% White	-0.429***	(0.109)	2.569	(1.584)
% Foreigner	0.0375	(0.0598)	-0.0974	(0.737)
% Literate	0.141*	(0.0787)	0.189	(0.943)
% Aged 13-65	0.164***	(0.0405)	0.863**	(0.413)
Ratio Owned/Rented Households	-0.223***	(0.0408)	1.150*	(0.633)
Occupation Score	-0.0752**	(0.0381)	-0.906*	(0.474)
% Employed in Agriculture	0.235***	(0.0746)	-1.403	(0.904)
% Employed in Mining	0.0578	(0.0677)	0.871	(0.756)
% Employed in Manufacturing	0.0194	(0.0531)	0.803	(0.740)
% Employed in Non-Durable Production	0.200***	(0.0669)	1.566*	(0.900)
% Employed in Transportation	-0.00905	(0.0647)	1.739*	(0.926)
% Employed in Retail	-0.119**	(0.0517)	-1.151**	(0.548)
% Employed in Finance	-0.00802	(0.0478)	1.249*	(0.667)
% Employed in Business	0.296***	(0.0734)	1.163	(0.759)
Railroad Length (km)	-1.442***	(0.522)	-20.33**	(8.068)
Observations	698		247	
Period	1909-1936		1909-1936	
Unit fixed effect	X		X	
Year fixed effect	X		X	

Notes: Robust standard errors at the shock-level in parenthesis, weighted by exposure shares. Significance levels: * 10% ** 5% *** 1%.

Table A1.5: Correlation Between Local Economic Conditions, City Finances, and Instrument

	Service Provision (1)	Tax Revenue (2)	Non-Tax Revenue (3)	Predicted Tax Revenue (4)	Predicted Non-Tax Revenue (5)
Deposits	0.00641***	0.00673***	0.00585***	0.00130	-0.000492
Observations	3150	3150	3150	3150	3150
Cities	303	303	303	303	303
City FE	X	X	X	X	X
Year FE	X	X	X	X	X
Population Group FE	X	X	X	X	X

Notes: Robust standard errors in parenthesis. Significance levels: * 10% ** 5% *** 1%. Unit of observation: city-year. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines. Predicted tax and non-tax revenue are based on the shift-share instruments detailed in Section 1.3.

TABLE A1.6: DETERMINANTS OF TAX REVENUE SHARES

	First Share of Tax Revenue, 1909			
	Regressors measured in :			
	1850	1870	1880	1900
	(1)	(2)	(3)	(4)
Household Size	0.00191**	-0.000209	-0.000565	0.000556
	(0.001)	(0.001)	(0.001)	(0.000)
Share in Agriculture	-0.345	-0.138	-0.0408	0.710
	(0.222)	(0.784)	(1.158)	(0.943)
Share Labor Force	1.964***	-0.0583	0.441	
	(0.537)	(0.563)	(1.016)	
Occupational Income Score	-0.0369**	0.0296	-0.00436	0.0282
	(0.016)	(0.030)	(0.033)	(0.019)
Observations	88	64	78	88
Adjusted R^2	0.33	0.27	0.15	0.16
State FE	X	X	X	X
Geographic variables	X	X	X	X

Notes: Robust standard errors in parenthesis. Significance levels: * 10% ** 5% *** 1%. Unit of observation: city. Dependent variable is the share of tax revenue used in the shift-share research design, as the first observed share of revenue coming from taxes in 1909.

Table A1.7: Effect of Revenue Shock on Spending: Robustness

	Total Revenue		Total Spending		Service Provision	
	Log	Level	Log	Level	Log	Level
	(1)	(2)	(3)	(4)	(5)	(6)
	A. OLS					
Tax Revenue	0.630***	1.000***	0.358***	0.865***	0.381***	0.681***
	(0.021)	(0.000)	(0.063)	(0.056)	(0.073)	(0.018)
Non-Tax Revenue	0.296***	1.000***	0.348***	1.271***	0.145***	0.321***
	(0.012)	(0.000)	(0.024)	(0.247)	(0.022)	(0.047)
p-value H_0 : tax = non-tax	<0.01	.	0.88	0.18	<0.01	<0.01
	B. REDUCED FORM					
Predicted Tax Revenue	0.339***	1.000***	0.428***	0.857***	0.402***	0.701***
	(0.105)	(0.051)	(0.123)	(0.155)	(0.101)	(0.028)
Predicted Non-Tax Revenue	0.269***	0.820***	0.225**	1.199*	0.202**	0.195*
	(0.100)	(0.203)	(0.111)	(0.674)	(0.088)	(0.112)
p-value H_0 : tax = non-tax	0.57	0.48	0.22	0.68	0.10	<0.01
	C. SHIFT-SHARE IV					
Tax Revenue	0.727***	1.000***	0.956***	0.777***	0.901***	0.763***
	(0.041)	(0.000)	(0.173)	(0.249)	(0.117)	(0.031)
Non-Tax Revenue	0.249***	1.000***	0.0591	1.715	0.0354	0.0443
	(0.049)	(0.000)	(0.198)	(1.086)	(0.122)	(0.134)
SW F-statistic Tax	10.4	37.2	10.4	37.2	10.4	37.2
SW F-statistic Non-Tax	7.41	21.5	7.41	21.5	7.41	21.5
p-value H_0 : tax = non-tax	<0.01	.	0.01	0.48	<0.01	<0.01
Observations	3150	3150	3150	3150	3150	3150
Cities	303	303	303	303	303	303
City FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Deposits	X	X	X	X	X	X
Population Size FE	X	X	X	X	X	X

Notes: Clustered standard errors at the city-level in parenthesis. Significance levels: * 10% ** 5% *** 1%. This table is based on the empirical specification from (1.3). Unit of observation: city-year. All regressions include city, and year fixed effects. Compared to Table 1.1, all regressions additionally control for the amount of bank deposit held in the county, and population size group as defined by the Census Bureau. Sampled cities correspond to all cities with a population above 30,000 between 1910-1931, and above 100,000 for 1932-1936. "Log" and "Level" indicate a log-log and level-level specification respectively. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines. The shift-share research design is detailed in Section 1.3. SW F-statistics corresponds to the Sanderson and Windmeijer (2016) F-test for weak instruments in a model with multiple endogenous variables.

TABLE A1.8: EFFECT OF REVENUE SHOCK ON SPENDING WITH TRANSFERS

	Total Revenue		Total Spending		Cash Balance		Service Provision		Debt Interest Payment		Capital Investment		Public Enterprises		Total Debt	
	Log	Level	Log	Level	Log	Level	Log	Level	Log	Level	Log	Level	Log	Level	Log	Level
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
A. OLS																
General Property Tax	0.609***	1.000***	0.437***	0.908***	2.642***	0.092***	0.431***	0.685***	0.559***	0.138***	0.385***	0.078***	0.062	0.007	0.484***	3.236***
	(0.027)	(0.000)	(0.049)	(0.016)	(0.366)	(0.016)	(0.049)	(0.009)	(0.070)	(0.005)	(0.084)	(0.017)	(0.073)	(0.005)	(0.073)	(0.156)
Transfers	0.053***	1.000***	0.055***	1.066***	-0.075	-0.066	0.052***	0.803***	-0.018	0.071**	0.118***	0.197**	-0.038	-0.006	-0.010	2.062***
	(0.004)	(0.000)	(0.007)	(0.101)	(0.143)	(0.101)	(0.007)	(0.078)	(0.021)	(0.029)	(0.022)	(0.090)	(0.028)	(0.019)	(0.024)	(0.632)
Other Revenue	0.251***	1.000***	0.278***	1.140***	-0.998***	-0.140**	0.081***	0.133***	0.273***	0.083***	0.662***	0.724***	0.617***	0.200***	0.286***	1.676***
	(0.009)	(0.000)	(0.016)	(0.068)	(0.243)	(0.068)	(0.012)	(0.026)	(0.042)	(0.016)	(0.048)	(0.073)	(0.078)	(0.025)	(0.042)	(0.307)
p-value H_0 : property tax = transfers	<0.01	.	<0.01	0.15	<0.01	0.15	<0.01	0.16	<0.01	0.04	<0.01	0.24	0.19	0.54	<0.01	0.09
p-value H_0 : property tax = other	<0.01	.	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	0.01	<0.01	<0.01	<0.01	0.02	<0.01
p-value H_0 : transfers = other	<0.01	.	<0.01	0.59	<0.01	0.59	0.04	<0.01	<0.01	0.74	<0.01	<0.01	<0.01	<0.01	<0.01	0.61
B. REDUCED FORM																
Predicted General Property Tax	0.214***	1.021***	0.134**	0.960***	1.920***	0.061***	0.261***	0.690***	0.372***	0.137***	-0.212*	0.119***	0.065	0.014***	0.340***	3.273***
	(0.044)	(0.092)	(0.060)	(0.095)	(0.468)	(0.016)	(0.039)	(0.049)	(0.082)	(0.013)	(0.125)	(0.035)	(0.095)	(0.004)	(0.083)	(0.333)
Predicted Other Revenue	0.187***	0.538***	0.164***	0.502***	0.675*	0.036	0.083***	0.087	0.128*	0.036	0.217***	0.225**	0.456***	0.155***	0.082	0.476
	(0.030)	(0.106)	(0.040)	(0.145)	(0.370)	(0.086)	(0.026)	(0.068)	(0.065)	(0.027)	(0.074)	(0.090)	(0.107)	(0.025)	(0.070)	(0.552)
Predicted Transfers	0.014	0.313***	0.013	0.373***	-0.187	-0.061	0.021**	0.206**	0.024	0.046	0.016	0.112*	-0.036	0.009	0.040*	1.170
	(0.009)	(0.108)	(0.012)	(0.130)	(0.166)	(0.065)	(0.008)	(0.095)	(0.024)	(0.044)	(0.030)	(0.067)	(0.037)	(0.016)	(0.023)	(0.942)
p-value H_0 : property tax = transfers	<0.01	<0.01	0.04	<0.01	<0.01	0.09	<0.01	<0.01	<0.01	0.06	0.07	0.93	0.31	0.79	<0.01	0.05
p-value H_0 : property tax = other	0.61	<0.01	0.70	0.02	0.08	0.80	<0.01	<0.01	0.04	<0.01	<0.01	0.33	0.02	<0.01	0.03	<0.01
p-value H_0 : transfers = other	<0.01	0.18	<0.01	0.55	0.05	0.39	0.03	0.34	0.18	0.86	0.02	0.35	<0.01	<0.01	0.61	0.56
C. SHIFT-SHARE IV																
General Property Tax	0.738***	1.000***	0.518***	0.907***	5.135***	0.093*	0.749***	0.706***	1.071***	0.136***	-0.261	0.069	0.599***	-0.004	0.964***	3.263***
	(0.037)	(0.000)	(0.074)	(0.052)	(1.384)	(0.052)	(0.037)	(0.046)	(0.148)	(0.019)	(0.275)	(0.045)	(0.225)	(0.012)	(0.150)	(0.389)
Transfers	0.054***	1.000***	0.046**	1.392***	-0.252	-0.392	0.067***	0.772	0.085	0.174	0.032	0.474	-0.052	-0.026	0.115**	4.726
	(0.013)	(0.000)	(0.021)	(0.507)	(0.344)	(0.507)	(0.012)	(0.472)	(0.052)	(0.215)	(0.068)	(0.404)	(0.082)	(0.123)	(0.051)	(4.289)
Other Revenue	0.327***	1.000***	0.310***	0.950***	0.980	0.050	0.052	0.121	0.102	0.062	0.589***	0.457***	1.096***	0.311***	-0.012	0.741
	(0.035)	(0.000)	(0.082)	(0.173)	(1.161)	(0.173)	(0.043)	(0.076)	(0.158)	(0.044)	(0.206)	(0.161)	(0.261)	(0.041)	(0.183)	(0.888)
SW F-statistic Property Tax	82.8	42.2	82.8	42.2	82.8	42.2	82.8	42.2	82.8	42.2	82.8	42.2	82.8	42.2	82.8	42.2
SW F-statistic Transfers	50.6	6.56	50.6	6.56	50.6	6.56	50.6	6.56	50.6	6.56	50.6	6.56	50.6	6.56	50.6	6.56
SW F-statistic Other Revenue	47.3	89.3	47.3	89.3	47.3	89.3	47.3	89.3	47.3	89.3	47.3	89.3	47.3	89.3	47.3	89.3
p-value H_0 : property tax = transfers	<0.01	.	<0.01	0.38	<0.01	0.38	<0.01	0.90	<0.01	0.87	0.27	0.36	<0.01	0.86	<0.01	0.75
p-value H_0 : property tax = other	<0.01	.	0.10	0.83	0.04	0.83	<0.01	<0.01	<0.01	0.15	0.03	0.04	0.19	<0.01	<0.01	0.02
p-value H_0 : transfers = other	<0.01	.	<0.01	0.41	0.36	0.41	0.76	0.16	0.93	0.62	0.02	0.97	<0.01	0.01	0.56	0.37
Observations	4,981	4,981	4,981	4,981	4,981	4,981	4,981	4,981	4,981	4,981	4,981	4,981	4,981	4,981	4,981	4,981
Cities	270	270	270	270	270	270	270	270	270	270	270	270	270	270	270	270
City fixed effect	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Year fixed effect	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X

Notes: Clustered standard errors at the city-level in parenthesis. Significance levels: * 10% ** 5% *** 1%. This table is based on the empirical specification from (1.3). Unit of observation: city-year. All regressions include city, and year fixed effects. Compared to Table 1.1, non-tax revenue is split between the revenue from transfers and other non-tax revenue. The shift-share research design is detailed in Section 1.3. SW F-statistics corresponds to the Sanderson and Windmeijer (2016) F-test for weak instruments in a model with multiple endogenous variables.

Table A1.9: Source of revenue and local spending: additional spending categories

	Cash Balance		Debt Interest Payment		Capital Investment		Public Enterprises		Total Debt	
	Log	Level	Log	Level	Log	Level	Log	Level	Log	Level
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					A. OLS					
Tax Revenue	2.820***	0.103***	0.561***	0.131***	0.405***	0.0560	0.0406	0.00123	0.451***	3.055***
	(0.422)	(0.030)	(0.072)	(0.006)	(0.091)	(0.035)	(0.080)	(0.005)	(0.083)	(0.188)
Non-Tax Revenue	-0.986***	-0.219	0.324***	0.0995***	0.773***	0.729***	0.601***	0.167***	0.366***	2.270***
	(0.284)	(0.138)	(0.047)	(0.014)	(0.048)	(0.138)	(0.083)	(0.022)	(0.056)	(0.297)
p-value H_0 : tax = non-tax	<0.01	0.05	0.02	0.08	<0.01	<0.01	<0.01	<0.01	0.48	0.04
					B. REDUCED FORM					
Predicted Tax Revenue	1.784***	0.0727***	0.315***	0.125***	-0.116	0.103***	0.0269	0.00736	0.295***	3.010***
	(0.481)	(0.019)	(0.087)	(0.011)	(0.118)	(0.029)	(0.092)	(0.008)	(0.099)	(0.276)
Predicted Non-Tax Revenue	0.449	-0.0706	0.176***	0.0806***	0.178***	0.307***	0.479***	0.137***	0.144**	1.669***
	(0.336)	(0.095)	(0.053)	(0.024)	(0.066)	(0.110)	(0.086)	(0.036)	(0.058)	(0.489)
p-value H_0 : tax = non-tax	0.06	0.21	0.23	0.13	0.03	0.12	<0.01	<0.01	0.25	0.03
					C. SHIFT-SHARE IV					
Tax Revenue	4.846***	0.103**	0.882***	0.127***	-0.261	0.0437	0.197	-0.0260***	0.819***	3.126***
	(1.499)	(0.046)	(0.195)	(0.010)	(0.293)	(0.044)	(0.239)	(0.009)	(0.225)	(0.215)
Non-Tax Revenue	0.474	-0.190	0.295**	0.131***	0.422***	0.655***	1.039***	0.309***	0.231*	2.544***
	(0.842)	(0.226)	(0.115)	(0.044)	(0.142)	(0.217)	(0.178)	(0.041)	(0.131)	(0.914)
SW F-statistic Tax	139.5	70.0	139.5	70.0	139.5	70.0	139.5	70.0	139.5	70.0
SW F-statistic Non-Tax	139.0	20.1	139.0	20.1	139.0	20.1	139.0	20.1	139.0	20.1
p-value H_0 : tax = non-tax	0.03	0.28	0.04	0.94	0.06	0.02	0.02	<0.01	0.07	0.60
Observations	5098	5098	5098	5098	5098	5098	5098	5098	5098	5098
Cities	316	316	316	316	316	316	316	316	316	316
City fixed effect	X	X	X	X	X	X	X	X	X	X
Year fixed effect	X	X	X	X	X	X	X	X	X	X

Notes: Clustered standard errors at the city-level in parenthesis. Significance levels: * 10% ** 5% *** 1%. This table is based on the empirical specification from (1.3). Unit of observation: city-year. All regressions include city, and year fixed effects. Sampled cities correspond to all cities with a population above 30,000 between 1910-1931, and above 100,000 for 1932-1936. "Log" and "Level" indicate a log-log and level-level specification respectively. Tax revenue is composed of revenue from the general property tax, license tax, special taxes, and poll tax. Non-tax revenue is composed of revenue from transfers, public service enterprises, special assessment charges, highway privileges, fees from general departments, and fines. The shift-share research design is detailed in Section 1.3. SW F-statistics corresponds to the Sanderson and Windmeijer (2016) F-test for weak instruments in a model with multiple endogenous variables.

Additional Tests of Shift-Share Instruments

This section provides tests of shock-Level inference and testing for the shift-share instrument. The identification condition requires that predicted revenues are uncorrelated with contemporaneous spending, conditional on city and year fixed effects. Following Borusyak, Hull and Jaravel (2021), a sufficient condition for identification can be obtained by focusing on the exogeneity of the shifts only. Using their framework, the shift-share instrument variable estimator can be equivalently obtained from a shock-level IV regression using shares as weights. This intuitively comes from the fact that shocks g_n are assumed to be quasi-randomly assigned and provides exogeneity to the shift-share instrument. Therefore, following Borusyak, Hull and Jaravel (2021), this provides a set of sufficient shock-level orthogonality conditions for identification:

Assumption 1 (Quasi-random shock assignment) $\mathbb{E}[g_n|\hat{\varepsilon}, s] = \mu\ ,\ \forall n$.

Assumption 2 (Many uncorrelated shocks) $\mathbb{E}[\sum_n s_n^2] \rightarrow 0$ and $Cov(g_n, g_n'|\hat{\varepsilon}, s) = 0\ \forall (n, n')$ with $n' \neq n$

The first assumption ensures that each shock has the same expected value conditional on shock-level unobservables $\hat{\varepsilon}$ and average exposure s_n. The second assumption states that the expected Herfindhal index of average shares (or shock exposure) converge to 0 asymptotically and that shocks are mutually sufficiently uncorrelated. Assumption 2 implies that the number of observed shocks therefore grows with sample size, requiring a large effective sample size (an equivalent condition is that the largest exposure weights becomes vanishingly small, as we will test below). Together, Assumptions 1 and 2 are sufficient conditions to establish the consistency of SSIV estimator.

Next I evaluate the plausibility of the identification assumptions.

Many Uncorrelated shocks Table A1.3 presents summary statistics on shocks g_{nt} with exposure weights s_{nt} and provides evidence that these shocks are uncorrelated and sufficiently many. Column 1 reports summary statistics including the "missing source" in shocks and weights to account for the sum of source shares not summing to 1 in each period (Borusyak, Hull and Jaravel (2021)).[18] Column 2 and 3 exclude the missing source, while Column 3 additionally residualizes weights and shocks on year fixed effects. In Column 1 and 2, the distribution of shocks is very similar, with a mean of 0.04, a standard deviation of 0.10 and an interquartile range of 0.09. This implies a sizable degree of variation in shocks and weak correlation, consistent with Assumption 2. Looking at exposure weights, I find that these weights s_{nt} are generally small, and the largest weight is 0.03, which conforms to Assumption 2 that exposure weights become vanishingly small asymptotically. Similarly, the effective sample size is relatively high, as described by the inverse of the Herfindhal index of weights: 63 across all 3 Columns. Column 3 provides qualitatively similar findings, which confirms that Assumption 2is unlikely to be violated even when removing time variation captured by year fixed effects. Figure A1.6 also shows that shares tend to be small.

Shock-level Orthogonality Condition Table A1.4 provides a balance test for the instrument and endogenous variable at the shock-level to assess the plausibility of Assumption 1. Testing for the balance of the endogenous variable across baseline characteristics is a way of checking the validity of the balance test, as the endogenous variable need not be exogenous to satisfy the identification assumption. I first perform this test at the shock-level given that the underlying variation used by the shift-share instrument comes from shocks (Borusyak, Hull and Jaravel (2021)). All variables are standardized to ensure comparability of point estimates and standard errors. I also include unit and year fixed effects to mirror the empirical specification in the analysis.

In Column 1 of Table A1.4, I regress the endogenous variable on a set of baseline city characteristics weighted at the revenue source level. Given that city revenues are not as-good-as-randomly assigned, we should expect them to correlated strongly with demographics and local economic characteristics. As expected, revenue is correlated with several demographic and economic characteristics. To the contrary, when regressing the shock variable g_n on these same characteristics, I find that nearly all of them are uncorrelated with the shocks. In particular, the share of employment in commerce, mining and agriculture do not explain the average rate in revenue source. This supports the claim that these shocks are not driven by local economic characteristics. Taken together, this test of the orthogonality of the shock supports Assumption 1 of quasi-random shock assignment.[19]

[18] In my setting, the "incomplete shares" issue is not a main issue as the sum of revenue category shares is comprised between .9 and 1.01.

[19] Table A1.2 replicates the balance test at the city level. Although not necessary as the identification assumptions for the shift-share can be expressed at the shock-level (Borusyak, Hull and Jaravel (2021)), this can further assess the

Data Definition and Construction

Data Construction from City Databook (1944–2000)

Below is the steps to construct the measures of outcomes over 1940–2000. The data comes from the City Databook 1944, 1949, 1952, 1956, 1962, 1967, 1967, 1972, 1977, 1983, 1988, 1994, and 2000 originally obtained from the Census Bureau and available from ICPSR 2896. I harmonized all variables using the steps detailed below.

To increase coverage, I use outcomes measured at each decade, or the closest available measure within 5 years. All monetary values were converted to 2012 USD using the implicit price deflator from the Federal Reserve of St Louis[20].

Birth / Death rate: crude birth/death rate measured as the number of death (births) per 1,000 population (based on Census population for the preceding decade)

City employees: City government employees per 10,000 population

Crime rate: number of serious crimes reported to police per 100,000 population. For 1980-81, no information given on the seriousness of the crime. For 1998 and 1999, rate assumed to be per 100,0000.

Foreign: Share of population foreign-born. For 1949, share of population who lived in other country or abroad.

Hospitals: number of hospitals and hospital beds per 10,000 ; number of hospital admissions per 100,000; number of hospital personnels per 1,000.

Income: Median household income. For 1974 and 1985, income measured per capita. For 1979, median household income instead of family.

validity of Assumption 1. I similarly find that the instrument is uncorrelated with most baseline city characteristics while the endogenous variable is.

[20] https://fred.stlouisfed.org/series/A191RD3A086NBEA

Poverty: Share of families below poverty level, or below the lowest income category defined by the City Government Finances (for 1950 and 1959). This category is defined in current dollar terms. It is 2000 USD for 1950, 3000 USD for 1959.

School enrollment: Total population enrolled in school as percent of total population. For 1960, enrolled population must be aged 5-64. For 1970, it is aged 3-34. For 1980 and 1990, it is aged 3+.

Years of education: Median years of completed schooling, for people aged 25 or over.

Number of teachers: Number of teachers per 1,000 population, as the number of pupils not reported systematically by the City Databooks.

Total expenditure: total expenditures as reported by city government finances, then divided by population from Census in the preceding decade. For 1967, figures divided by 10, as they have been erroneously given in 100,000 instead of million of USD.

Other Data construction

Bank deposits: Data on bank deposits comes from the Federal Deposit Insurance Corporation (FDIC), a federal agency created in 1933 that collected information on total bank deposits, number of banks and bank suspensions on an annual basis between 1920 and 1936. The data was obtained from ICPSR 3.

Corruption of Public Officials: The data comes from annual reports of the Department of Justice's Public Integrity Section. Data was collected from the reports for 1989, 1999, and 2009. It corresponds to the total number of convictions of public officials by federal judicial districts. Cities were assigned the value of their corresponding judicial districts.

Democratic Newpapers: Data comes from the US Newspaper panel by Gentzkow, Shapiro and Sinkinson (ICPSR 30261). The fraction of democratic newspapers corresponds to the fraction of circulation of newspapers that have a democratic affiliation. Nearly all journals have a political affiliation.

TABLE A1.10: DESCRIPTION OF MAIN VARIABLES AND ACCOUNTING TERMINOLOGY

Type	Name	Description
Revenue	Taxes	Taxes are enforced proportional contributions of wealth levied and collected in the general interest of the community from individuals and corporations by virtue of the sovereignty of the Nation or state for the support of governments and for meeting all public needs, and levied without reference to the special benefits which the contributors may severally derive from the public purposes for which the taxes are required.
Revenue	General Property Tax	Property taxes, which constitute the most important source of American municipal revenue, are direct taxes upon property or upon persons, natural or corporate, owning property. Property taxes are divided by the Bureau of the Census into two subclasses, designated, respectively, general and special property taxes. General property taxes are those direct taxes assessed and collected by methods which are practically uniform for all kinds of property. All general property taxes are apportioned according to the value of the property, and therefore constitute ad valorem taxes.
Revenue	Special Property Tax	special property taxes are those assessed and collected upon selected classes of property by methods which are not applied to the assessment and collection of taxes upon property in general.
Revenue	Business Tax	Business taxes are taxes collected from persons, natural or corporate, by reason of their business, where such collection is not associated with the granting of a license or permit to carry on such business.

Table A1.10 continued from previous page

Type	Name	Description
Revenue	License Tax	License or permit taxes are taxes collected from persons, natural or corporate, by reason of their business, where such collection is associated with the granting of a license or permit to carry on such business, or where, without such license or permit, the individual or corporation has no legal right to engage in the business. These taxes are in some states called privilege taxes.
Revenue	Poll Tax	Poll taxes or capitation taxes are taxes assessed upon natural persons without regard to ownership of property. They may be levied uniformly upon all males of specified ages or graded according to occupation or otherwise. Some of them are levied at a fixed amount against all persons assessable therewith, and others are quasi property taxes based upon an arbitrary valuation of polls. Poll taxes graded according to occupation are also called occupation taxes.
Revenue	Special Assessment	Special assessments are compulsory contributions levied under the taxing or police power to defray the costs of specific public improvements or public services undertaken primarily in the interest of the public. They differ from general property taxes in that they are apportioned according to the assumed benefits to the property affected by the improvements, or the assumed benefits to individuals or corporations by reason of the services performed.
Revenue	Fines and forfeits	Fines and forfeits are enforced contributions of wealth received as punishment of individuals and corporations for violation of law or for failure to carry out the terms of specified agreements.

Table A1.10 continued from previous page

Type	Name	Description
Revenue	Escheats	Amounts of money received from the disposal of properties that have come into the ownership of the government because their owners can not be ascertained.
Revenue	Highway Privileges	The designation "privileges" is here applied as a technical term (1) to the special contract rights in and upon highways granted to specified individuals and corporations, and (2) to the general revenues that are paid or payable to the general treasury as compensation for such rights. These privileges are divided by the Bureau of the Census into two classes, called, respectively, major and minor. The major privileges are those which are exclusively enjoyed by public service corporations and which such corporations must possess in order to carry on their operations, while minor privileges are the right to utilize for business purposes specified portions of the highway, or the spaces above or below it, which are granted to private individuals as well as to public service and other corporations. It should be noted, however, that moneys derived from the use of streets in connection with the management of municipal markets and the regulation of sales of merchandise in the streets by its producers are in all cases to be considered as parts of the revenue of markets. The revenues last mentioned are not to be confounded with receipts from street peddlers or hucksters, which are reported as for "business licenses."

Table A1.10 continued from previous page

Type	Name	Description
Revenue	Fees and Charges	Fees and charges are compulsory contributions which are exacted to defray a part or all of the costs involved in some specific service rendered by the government. Fees are collected for services which are never performed except by governments, while charges are collected by governments for services which are similar in character to those performed by one individual for another. The amount of the fee for any given service is usually established by statute, and the fee is generally collected in advance. On the other hand, charges can be definitely determined only upon completion of the work or service, and advance payments thereof are made only to guarantee the payment of costs when determined. Not all municipal fees and charges are properly classed as revenues; only those are so classed that are incidental to the conduct of the ordinary municipal office or department. Those that are obtained in connection with the operation of a public service enterprise are to be classed as commercial revenues. Not only are fees and charges assigned to different classes of revenue according to the source, but city receipts from minor sales on revenue account and those from rent and interest are arranged in gi-oups according to their source. Those receipts

Table A1.10 continued from previous page

Type	Name	Description
Revenue	Subventions, Grants, Gifts and Donations	Gifts and donations are voluntary contributions received from private individuals, while grants and subventions are amounts received by one government from another. Gifts, donations, grants, and subventions are accepted either with or without specified conditions as to their use or investment. The greater portion of these receipts are, however, accepted upon conditions which make the amounts so received trust revenues. Subvention, grants, gifts and donation will be considered as general revenue whenever they are received for no specific purpose or subject to no specific conditions.
Spending	Governmental cost payment	The costs of city government are the accrued costs, paid and payable, of the services employed, property constructed, purchased, or rented, public improvements constructed or otherwise acquired, materials utilized, and interest on borrowed moneys, which are incurred for protecting person, property, and health, providing social necessities and conveniences, caring for the dependent and delinquent classes, bettering social conditions, informing other services, and carrying on other activities for which those governments have authority. These costs are readily separable into three principal classes, here referred to as expenses, interest charges, and outlays.

Table A1.10 continued from previous page

Type	Name	Description
Spending	Expenses	Municipal expenses are the costs and losses of cities from which no permanent or subsequently convertible value is received or receivable. They are (1) the accrued costs, paid or payable, exclusive of those arising in connection with the construction or acquisition of permanent properties and improvements, which are incurred by cities on account of services employed, property rented, and materials utilized in connection with the maintenance and operation of government, the conduct of municipal business undertakings, and the management of trusts; and (2) the losses occasioned by depreciation of permanent properties and otherwise.
Spending	Expenses – general	The general expenses of municipalities are those incurred by them in connection with the exercise of their general governmental functions.
Spending	Expenses – commercial	The commercial expenses of municipalities include (1) expenses of public service enterprises, or the costs of operating and maintaining public service enterprises or departments or offices, such as municipal waterworks and gas works, which are organized for the purpose of providing the public and the city with some public utility or service; and (2) expenses of general investments or costs of managing the properties held as general or free investments.
Spending	Expenses – trust	The trust expenses of municipalities are the costs of caring for and maintaining the property left in trust to cities for specified municipal purposes or uses, and for administering the trusts as directed by those establishing the same.

Table A1.10 continued from previous page

Type	Name	Description
Spending	Debt repayment	The term municipal interest charges or interest is a designation of the accrued interest paid or payable, incurred by municipalities for the use of credit capital.
Spending	Capital outlays	Municipal outlays are the accrued costs, paid or payable, of land and other properties and public improvements, more or less permanent in character, which are owned and used by municipalities in the exercise of their municipal functions, or in connection with the business undertakings conducted by them.
Spending	I. General Government	Under the division " General government " are included eight principal inquiries which are arranged under four principal subgroups designated "legislative," "general executive," "Judicial," and " general government buildings.
Spending	II. Public Order	Used for reporting the receipts and payments of cities that are inci- dental to the operation and maintenance of the departments and offices whose sole or principal duties are to protect person and property, including the enforcement of laws relating to crime, morals, safety of person, etc. Also includes expenses related to correction facilities

Table A1.10 continued from previous page

Type	Name	Description
Spending	III. Health	Expenses for municipal activities which seek primarily and directly to conserve or promote the public health by curing and preventing communicable diseases, lessening child mortality, promoting the efficiency of school children, guaranteeing the purity and healthfulness of the food, drink, and medicines of the public, and performing similar services. The activities referred to are those whose results are reflected and in a large degree measured by the statistics of mortality and of the progress of school children through the grades. Excluded: payment (1) for sanitation or promoting public and private cleanliness; (2) for providing facilities for outdoor and other athletics, baths, sports, and recreation; (3) for protecting persons from the physical and moral dangers that result from improperly constructed and regulated buildings, unscientific and careless plumbing, improperly guarded machinery, dust, and disease producing occupations; and (4) for providing the poor with free medical service in noncommunicable diseases.
Spending	IV. Sanitation	Payments incidental to the expenditures of the city for sanitation or the promotion of public and private cleanliness. Most of the expenditures to be reported after these inquiries promote health as well as cleanliness.

Table A1.10 continued from previous page

Type	Name	Description
Spending	V. Highways	Costs of the principal highway services and replacements and new constructions on the highways. Whenever practicable, the information should separately reports the cost of land for new highways and the cost of grading from the costs of maintaining the several classes of paved, improved, and unimproved streets. If public improvements or street repairs are made, or other street work is done by the labor of the inmates of charitable or penal institutions, the value of such labor should be entered as " service transfers," in the case of expenses; and in the case of outlays, and as "for outlays," and also in the column "service transfers". In like manner, if these institutions or this parks provide the highway or other department with hay or other produce grown by them, the value of such hay or produce should be reported among expenses as above directed.
Spending	VI. Charities	All payments by the city for the care of children or of the insane, blind, or sick. Payments to public institutions of other civil divisions and to private institutions should be care- fully segregated from payments for the purchase or construction and maintenance and operation of institutions owned and controlled by the city. Report all payments of the former class in the column " miscellaneous," and those for municipal institutions as set forth in general instructions. Payments for transportation to institutions should be reported on the line for the institution to which the person is taken. In the case of outdoor poor relief, such transportation charges should be reported separately.

Table A1.10 continued from previous page

Type	Name	Description
Spending	VII. Education	All payments for schools and libraries.
Spending	VIII. Recreation	All payments for public recreation, including those which are educational in their nature, as well as those which result from the establishment and maintenance of public parks.
Spending	IX. Other	All payments for expenses and outlays that do not strictly represent the cost of the functional or departmental activities of the city which are to be reported in categories II to VIII above, or the costs of the general municipal governments which are to be reported in category I.

Chapter 2

The Political Economy of Lockdown: Does Free Media Matter?

Introduction

The COVID-19 pandemic has highlighted the importance of responsiveness and access to truthful information during a public health crisis. Timely responses have been crucial to halt the spread of the virus by limiting community transmission and avoiding health system saturation. At the same time, knowledge of local peaks in infections was central to tailor responses to changes in circumstances such as the emergence of new variants. Non-pharmaceutical interventions, and in particular lockdowns, are dependent on responsiveness and access to trustworthy information for their effectiveness as they require widespread compliance.

This paper studies the role of free media in affecting lockdown decisions and social distancing during the early phase of the pandemic. We provide a conceptual framework and supporting evidence to motivate why free media increases responsiveness. Conceptually, free media makes citizens better informed about the severity of the pandemic, which can affect their compliance and the decision by governments to impose lockdowns.

We use a panel data of daily reported deaths, lockdown decisions and mobility trends during the initial phase of the pandemic (December 1 - May 1, 2020) in all countries affected by COVID-19. We measure government responsiveness as changes in the decision to impose a lockdown in response to an increase in COVID-19 deaths. Citizen responsiveness is measured as reduced mobility during lockdowns when COVID-19 deaths increase.[1]

Although deaths statistics are not the only trigger that could motivate governments and citizens to act, they were a salient indicator of the severity of the outbreak, especially in the early period with shortages of testing. That said, there are reasons to think that they are somewhat imprecise reflecting the competence of government and any incentive to misreport deaths for political reasons. Given the role of the media in reporting on COVID-19 and avoiding misinformation[2], we expect both death reporting and responsiveness to the pandemic to vary depending on whether countries have free or censored media.

[1]Responsiveness to a crisis is characterized by the speed and magnitude of the economic policy response, uncertainty about the nature of the threat and the importance of individual compliance. See Besley (2020*a*) for a general discussion of government responsiveness and preparedness.

[2]See e.g. Mian and Khan (2020) and Brennan et al. (2020). Fetzer et al. (2020) report that the majority of respondents find factual information provided by their government on COVID-19 to be untruthful based on an international survey of 73 countries conducted between March 29 and April 7, 2020.

We begin by documenting responsiveness to COVID-19 deaths. We show that a doubling of deaths early in the pandemic was associated with a 15.5 percentage points (p.p.) increase in the likelihood of imposing a lockdown by governments, as well as a 6 p.p reduction in time spent outside and a 2.8 p.p. increase in time at home during lockdowns by citizens. We find a similarly large level of responsiveness across all types of mobility, for alternative definitions of media freedom and death statistics, and when looking at responsiveness to cases instead of deaths.

To address concerns about misreporting of deaths, we develop an IV approach using simulated deaths from a calibrated Susceptible-Exposed-Infected-Recovered (SEIR) model as an instrument for reported death. To do so, we use a popular epidemiological model developed by Noll et al. (2020) and widely used at the time.[3] This model includes with age-specific parameters and several categories of infectious severity. The model has a range of inputs including population and age distribution, estimates of the spread and intensity of the disease from Noll et al. (2020), as well as country-level calibrated parameters of the initial time of the outbreak, basic reproduction number R_0, and healthcare capacity.[4]

We provide evidence that simulated deaths from the SEIR model are a good benchmark for the evolution of mortality in the initial phase of the pandemic, and serve as a valid instrument for reported deaths. The identification strategy requires that simulated deaths are exogenous to media freedom and lockdown decisions. In support of these assumptions, we show that the inputs used in the SEIR model are unrelated to the media status of a country and that, as expected by construction[5], death simulations are not affected by actual decisions in response to the pandemic.

The IV results show that *only* free-media countries are responsive to COVID-19 deaths. Specifically, citizens in free-media countries tend to reduce their time outside and increase time at home after COVID-19 death spikes, while we find no significant relationship between deaths and mobility in censored-media countries. We also find that governments in free-media countries are more likely to impose a lockdown following an increase in COVID-19, although the

[3]The website used to generate simulations from their model had on average 8 thousand page loads per day between March and May 2020 (Noll et al. (2020).)

[4]We take calibrated parameters from epidemiological simulations by Noll et al. (2020) and Walker et al. (2020). See full details on the SEIR model in Appendix Section 2.4.

[5]Our simulations do not take as input actual mitigation strategies by countries.

difference is not statistically significant. As a robustness test, we obtain similar results using excess mortality as an indicator of the true death toll of COVID-19 for a subset of 39 countries. Using a decomposition exercise to attribute differences in lockdown decisions and mobility between free- and censored-media countries, we find that responsiveness to deaths accounts for more than 40% of the difference in mobility and lockdown, and was quantitatively more important than the number of deaths in explaining mobility differences between free- and censored-media countries.

We then provide evidence that free media was an important mechanism behind higher responsiveness through the provision of information. First, we show that differences in responsiveness cannot be explained by other country characteristics such as income, education or democracy using a similar empirical strategy as described above. Second, we provide evidence from online searches that citizens in free-media countries were both better informed about the pandemic and more responsive in their COVID-19 online searches (they tended to increase their online searches for COVID-19 as deaths increased). This is consistent with responsiveness caused by being better informed about changes in health risks.

Although the context is specific, this paper relates to several existing strands of related literature. First, this paper contributes to a studies that explore drivers of government effectiveness. Authors such as Acemoglu and Robinson (2012) have emphasised differences in institutions, while Glaeser, Ponzetto and Shleifer (2007) focus on the importance of education. It is well known that there are large differences in observable measures of government quality across the world (La Porta et al. (1999)). Besley and Persson (2011) draws attention to how this reflects incentives to invest in state capacities, including public health systems. Related to this, is a large body of literature that has studied how political institutions shape policy incentives (e.g., Persson and Tabellini (2002)), in particular how health outcome are impacted by political institutions (Besley and Kudamatsu, Kudamatsu (2006, 2012)) as well as a wider range of societal, political and economic factors as argued by Case and Deaton (2020) in the case of the United States.[6]

Second, the paper contributes to the growing literature on media and politics as reviewed, for example, by Coyne and Leeson (2009), Prat and Strömberg (2013), and Strömberg (2015).

[6] Bosancianu et al. (2020) finds that governments with high levels of state capacity and responsiveness report the greatest death burdens from COVID-19, consistent with our findings on death reporting.

It is increasingly recognised that democratic accountability is enhanced when citizens have better information (e.g., Maskin and Tirole, Besley, Ferraz and Finan, Snyder Jr and Strömberg (2004, 2006, 2011, 2010)). Within this line of work, our findings are most closely related to Besley and Burgess (2002) and Eisensee and Strömberg (2007) who focus on how media make governments more responsive to shocks which ties in with debates about how democracy and free media have reduced the incidence of famine in India (Sen (1981)).

Third, the paper is linked to work on censorship and media bias. The fact that media freedom is sometimes censored is not an accident since, as discussed in, for example, Besley and Prat (2006), there are incentives for governments to silence the media in order to retain power. Such activities have been linked to media ownership patterns by Djankov et al. (2003). There are also reasons to believe that citizens sometimes receive biased and distorted views from media coverage (Mullainathan and Shleifer, Gentzkow and Shapiro, DellaVigna and Kaplan (2005, 2006, 2007)), including in the case of COVID-19 (see Bursztyn et al. (2020)) and attention has recently switched to role of social media in propagating and perpetuating misinformation (Allcott, Gentzkow and Yu, Enikolopov, Petrova and Zhuravskaya (2019, 2011)).

Finally, we contribute to three recent developments in the emerging literature on the economic analysis of COVID-19 pandemic. First, several studies have documented the importance of social norms and trust in compliance with social distancing (Bargain and Aminjonov, Barrios et al., Bazzi, Fiszbein and Gebresilasse, Besley and Dray, Campos-Mercade et al., Durante, Guiso and Gulino, Hensel et al. (2020, 2021, 2021, 2021, 2021, 2021, 2022)). For instance, Bargain and Aminjonov (2020) show that high-trust countries had higher levels of compliance with social distancing, and more efficiency of policy stringency. We contribute to this line of inquiry by highlighting the independent role of access to free media in increasing compliance with social distancing in response to higher COVID-19 mortality. Second, we contribute to documenting the role of (mis)information during the COVID-19 pandemic (Bursztyn et al., Mian and Khan (2020, 2020)). Our study is consistent with the idea that access to free media improved awareness of COVID-19 risks and increased public responsiveness during a public health crisis. Third, we contribute to the overall economic analysis of government responses to COVID-19. Early contributions by Acemoglu et al. (2020), Alvarez, Argente and Lippi (2020), Besley (2020*a*) and Kaplan, Moll and Violante (2020) integrate insights from economic and epidemiological models to study lockdowns. The economic cost of lockdowns has been studied using high-frequency financial market data in Carvalho et al. (2020) and Surico and Galeotti (2020), and consumption data in Coibion, Gorodnichenko and Weber (2020), while Brodeur et al. (2021*a*) uses internet searches to measure the impact of lockdowns on well-being.[7] We contribute by highlighting the importance of responsiveness by both governments and citizens on the dynamics of the pandemic.

The remainder of the paper is organised as follows. Section 2.1 presents the data and some core facts. Section 2.2 presents evidence on responsiveness to deaths in free-media and censored-media countries. Section 2.3 describes additional results and empirical support for the role of free media to inform citizens about the spread of COVID-19. Section 2.4 concludes. We describe additional results and provide a theoretical framework to motivate the empirical findings in the Appendix.

2.1. Data

This section presents the data used in the empirical analysis regarding reported and simulated COVID-19 deaths, lockdown decisions, mobility and search trends, and media characteristics.

Death statistics and lockdown Data on COVID-19 deaths come from the European Centre for Disease Prevention and Control (ECDC) and are based on national reports, mainly from health authorities. The ECDC collects and harmonises these reports on a daily basis worldwide. We focus on deaths as this is the most comparable statistics across countries. Data on lockdown decisions by countries comes from Lejeune (2020). We consider that a country is under a lockdown when national measures restricting movements are in place at a national level for at least part of the day.

Simulated SEIR Deaths We simulate daily COVID-19 deaths for each country using a SEIR model developed by Noll et al. (2020). Compared to the "standard" SIR model, our simulation includes a category for the exposed and allows for differences in healthcare availability and vulnerability of countries. Specifically, it takes as parameters the number of available hospital

[7] Brodeur et al. (2021*b*) provides an early review on the economic consequences of COVID-19.

and beds and intensive care units (ICUs), has additional categories of infectious individuals for those that are infectious but not hospitalised, hospitalised and hospitalised in critical care, and allows for age-specific transition rates. The inputs into the model include epidemiological parameters that are common across countries, hypothetical mitigation dates calibrated by income group, and epidemiological estimates to predict the initial onset of outbreaks and basic reproduction number in each country.[8] Further details on the calibration of the SEIR model can be found in Appendix Section 2.4.

The SEIR model allows us to generate a simulated death toll due to the spread of COVID-19 infections in each country over time. We use this measure as an instrument for the evolution of mortality for reported mortality and may help to deal with concerns that death rates are inaccurate due either to incompetent or deliberately misleading reporting.

Mobility trends Data on mobility trends comes from Google Community Mobility reports.[9] Mobility trends refer to changes in visits and length of stay at different places after lockdown compared to a median value, for the corresponding day of the week, during the 5-week period from January 3rd to February 6th, 2020. We normalise baseline mobility to be 0, so that a value indicates a percentage change in mobility compared to baseline. We focus on mobility trends after governments implemented a lockdown to capture compliance with lockdown measures.

Media freedom Media freedom is measured using data from the Varieties of Democracy project[10], which calculates a freedom of expression and alternative sources of information index which comprises: (i) the extent to which a government respects press and media freedom,(ii) the freedom ordinary people to discuss political matters at home and in the public sphere, and, (iii) the freedom of academic and cultural expression. It is based on indicators for media censorship effort, harassment of journalists, media bias, media self-censorship, print/broadcast media critical and print/broadcast media perspectives, freedom of discussion

[8] We use epidemiological parameters for the length of infectious period, length of hospital stay, length of ICU stay and severity of ICU overflow, and country-specific estimates of reproduction R_0, as well as estimated initial size and date of the outbreak from Noll et al. (2020). The number of hospital beds and intensive care units (ICUs) is estimated by Walker et al. (2020) which uses data from the World Bank and a systematic review. We account for mitigation interventions by governments using the average lockdown date by income group as the starting date and hypothesize that these measures reduce social contact rates by 60% following enhanced social distancing scenarios discussed by Ferguson et al. (2020) and Walker et al. (2020).

[9] www.google.com/covid19/mobility/

[10] From www.v-dem.net.

for men/women, and freedom of academic and cultural expression. We classify a country as having free media if it has a score above the median score of 0.7 out of 1. See Figure A2.3 for a list of countries according to their media freedom status. Free-media and censored-media countries differed in when they decided to impose lockdowns, and in the date of initial outbreak.[11]

As a robustness check, we also use the Media Freedom index from Freedom House[12] In addition, we use the World Press Freedom index.[13] We classify a country as having free media or free press if it has a score above or equal to 50 out of 100 on the corresponding index. These robustness results are discussed in Appendix Section 2.4.

Search trends We measure trends in internet searches about COVID-19 using Google search trends. Search trends indicate the share of Google searches including one of the following terms: "covid", "coronavirus", "covid19", "COVID-19", "covid 19", "ncov" , or "nCoV2019". Daily national search trends are normalised to be relative to the COVID-19 search share in the United States on January 30, 2020.[14] Any value therefore indicates the percentage change in search share compared to that baseline.

2.2. Evidence of Responsiveness and Role of Free Media

The core results on responsiveness focus on governments' decision to impose a lockdown in response to an increase in reported deaths along with citizens' compliance with such lockdowns.[15] We think of publicly available data on COVID-19 deaths as informing beliefs about

[11]Table A2.1 presents summary statistics for free-media and censored-media countries using a balanced panel of all countries with reported deaths statistics from COVID-19 between December 1st, 2019 and May 1st, 2020. We report average country characteristics, COVID-19 infections and mortality, lockdown and mobility changes by media freedom status.

[12]From www.freedomhouse.org. The score is from index D1 which assesses the extent to which there are free and independent media. Media in this case refers to all relevant sources of news and commentary—including formal print, broadcast, and online news outlets, as well as social media and communication applications when they are used to gather or disseminate news and commentary for the general public.

[13]From www.rsf.org. This scores the degree of freedom available to journalists in 180 countries using both the survey responses of experts and quantitative data on abuses and acts of violence against journalists during the period evaluated. The criteria evaluated in the questionnaire are pluralism, media independence, media environment and self- censorship, legislative framework, transparency, and the quality of the infrastructure that supports the production of news and information.

[14]On January 30, 2020, the WHO announced that COVID-19 was a Public Health Emergency of International Concern.

[15]Appendix 2.4 presents a conceptual framework on lockdown decisions and compliance that formalizes the idea that free media provides information on health risks.

the severity of the outbreak. And we look at how citizens comply with lockdown measures using data on daily mobility trends. We expect countries with free media to be be more responsive to deaths than censored-media countries.[16] All specifications will include country fixed effects, outbreak × time fixed effects and the global number of COVID-19 deaths.

2.2.1. OLS results

The empirical analysis aims to capture the determinants of the lockdown decision, as well as private compliance with lockdowns. We first report estimates of responsiveness using the following OLS specification:

$$Y_{it} = \alpha_i + \delta_{tM} + \beta D_{it} + \gamma D_{it} \times M_i + \varepsilon_{it} \tag{2.1}$$

The outcome of interest Y is either the government decision to impose a lockdown or mobility changes during periods of lockdown. M_i indicates whether a country enjoys media freedom. We denote α as country fixed effects, and δ are media-specific outbreak time fixed effects that are dummies for each day since a country reached a total of 10 deaths to pick up a typical progression of the disease across free-media and censored-media countries.[17] Further, The coefficient β estimates the magnitude of government and citizens responsiveness to deaths. Given our theoretical framework, we also expect responsiveness to be heterogeneous across free-media and censored-media countries, which is captured by γ. We also include country fixed effects, and outbreak time fixed effects which is the time since the first 10 cumulative deaths.

Table 2.1 reports our main estimates of responsiveness using the above OLS specification. We find evidence of responsiveness to COVID-19 deaths by both governments and citizens. As show in Column 1, a doubling of COVID-19 deaths is associated with a 15.5 percentage points increase in the likelihood of imposing a lockdown, which is statistically significant. Columns 2 to 8 provide evidence of responsiveness of citizens. We find that an increase is deaths is

[16] This argument is formalized in the model presented in Appendix 2.4

[17] Given significant differences in the timing of outbreaks between free- and censored-media countries, we allow these indicators to vary by media status.This allows us to capture the fact that outbreaks occurred at significantly later dates in censored-media countries (close to 13 days later, see Table A2.1). We report results without these flexible fixed effects in Appendix 2.4

associated with significantly more reductions in mobility outside across all types of outside places, and more time spent at home (shown by an increase in time spent in residential areas). For both governments and citizens, we find no evidence of differences in responsiveness between free-media and censored-media countries using OLS results. This is shown by the absence of statistical significance for the point estimate of the interaction between log deaths and a country media freedom status.

2.2.2. IV results

We now explore an IV approach to deal with concerns about potential misreporting of deaths due to either differences in competence or government incentives to accurately report deaths. We use country-specific simulated deaths from a SEIR model as instrument for reported deaths; we first discuss the results and then provide evidence of the validity of the instrument.

Simulated deaths using SEIR model The OLS gives an unbiased estimate of the impact of reported deaths on government and citizen responsiveness if D_{it} is uncorrelated with the error term ε_{it}. There are two natural concerns with this assumption: measurement error and endogenous reporting of deaths by governments. Measurement error could come from the misattribution of the cause of death, a common problem while pandemics are ongoing, and would lead to an attenuation bias towards zero. In the case of endogenous death reporting, the direction of bias is not as clear *a priori*. If governments face political costs from reporting higher death rates, this will tend to reduce responsiveness to deaths. But deaths themselves will also not be reported to justify a lockdown. Either way, this might induce a correlation between D_{it} and ε_{it}. Moreover, there are good reasons to think that the bias due to any strategic death reporting can vary depending on whether media is censored or free and lead to an overestimation of responsiveness for censored-media countries.

Our IV approach uses the simulated death rate from the SEIR model as an instrument for D_{it}. As discussed above, these simulations rely on a minimal set of country specific parameters, none of which are directly related to observed policy choices. Model parameters are R_0, the initial date of the outbreak, country population and age distribution, healthcare capacity and the average mitigation start and efficiency from a country's income group.[18] Additionally, we expect that the first stage relationship between SEIR-simulated deaths and reported deaths will be heterogeneous for free-media and censored-media countries.

Empirical Specification The second stage equation is

$$Y_{it} = \alpha_i + \delta_{tM} + \beta \widehat{D}_{it} + \gamma \widehat{D_{it} \times M_i} + \varepsilon_{it} \tag{2.2}$$

Compared to the OLS specification, we simultaneously instrument in the first stage for D_{it} (log deaths) and $D_{it} \times M_i$ (log deaths x media freedom status) using E_{it} (log SEIR deaths) and $E_{it} \times M_i$ (log SEIR deaths x media freedom status). $\widehat{D}_{it}$ and $\widehat{D_{it} \times M_i}$ are the fitted values from the first stage regressions.

Results Table 2.2 reports the main responsiveness results using the IV approach detailed above. Column 1 estimates the responsiveness of governments to COVID-19 deaths. We find no evidence that censored-media countries were responsive to deaths when deciding to impose a lockdown. A doubling of total deaths during the Great Lockdown led to only a 3 percentage points increase in the likelihood of imposing a lockdown, which is not statistically significant. For free-media countries, the responsiveness is 23 percentage points (as given by the sum of point estimates for both log deaths and its interaction with the media freedom indicator). On average, we estimate larger responsiveness for free-media countries but the difference with censored-media countries is not statistically significant at the 5% level.

Columns 2 to 8 focus on private compliance with lockdown, using mobility changes during lockdowns as outcomes. Citizens in censored-media countries are found to not be responsive to deaths when reducing their social contacts: an increase in deaths is not estimated to lead to more reduction in time outside or increase in time indoor (the point estimate are of reverse sign compared to the OLS results and not statistically significant). On the contrary, citizens in free-media countries are found to be highly responsive. An increase in deaths leads to less

[18] In particular, we do not use any observed measure of responsiveness for predicting the trajectory of deaths in a country. Instead, we impute a *hypothetical* date for severe mitigation measures in each country measured as the average time of implementing a lockdown relative to the start of the outbreak by country income group. We rely on epidemiological parameters and the estimated date of first case used in the epidemiological modelling by Noll et al. (2020). See details on the SEIR model in Appendix Section 2.4.

social contacts across all outside categories and more time at home.

Figure 2.2 illustrates the heterogeneity in mobility response to deaths between free-media and censored-media countries, giving the 90 percent and 95 percent confidence intervals for the two subsamples. The estimated elasticity response of mobility appears to be different in free-media and censored-media countries, and their associated confidence intervals barely overlap.

Taken together, these results suggest that only countries with free media have taken lockdown measures that are responsive to the severity of the outbreak.[19]

Validity of the Instrument We now provide evidence in support of the exclusion restriction needed for the IV results to be valid. First, we show that that model parameters used to predict deaths using the SEIR model are uncorrelated with media freedom in a country. Second, we show that, holding all other country parameters equal, having free media is *not* associated with significantly different *simulated* deaths.

Table 2.3 shows the correlation between media freedom and the seven country-specific parameters that enter the SEIR simulation: R_0, the simulated start date of the outbreak in the country, the simulated date of the first mitigation measure, number of hospital beds, number of ICU beds, log population and share of elderly population. We find no statistically significant correlations between the media freedom status of a country and any of these parameters, with the exception of R_0 with a difference significant at the 10% level and small in magnitude (0.46 i.e. 0.38 SD, see Table A2.9). Further, we also construct a SEIR death index that captures any country-specific differences in simulated deaths over time by regressing SEIR deaths per million on country fixed effects, after controlling for date, outbreak time and income groups. We then regress these country fixed effects coefficients on media freedom status. This aims to capture any residual differences in SEIR deaths attributable to country characteristics. Reassuringly, we find no significant correlation between SEIR death index and media freedom status as shown in column 8.

These results provide evidence that the instrument based on using the simulations from the

[19] We find corroborating evidence looking at responsiveness to simulated deaths as reported in Table A2.4, when instrumenting for COVID-19 cases or recent deaths instead of total deaths as reported in Tables A2.14 and A2.15. We also find similar results using excess mortality as an indicator of the true death toll in a subset of 39 countries as reported in 2.4.

SEIR model is not biased towards predicting more or fewer deaths in free- versus censored-media countries. What Table 2.3 indicates is that none of the parameters that enter the simulation differ significantly by media status, therefore any difference in simulated deaths reflects differential parameter values (such as a larger share of vulnerable population) but not differences in the media status of the country. [20]

Figure 2.1 visually confirms that the distribution of country-specific epidemiological parameters used to simulate COVID-19 deaths do not differ by media freedom status. For each of the parameters and for the SEIR death index, there is common support and limited differences between the distribution of parameters for free-media and censored-media countries. Reassuringly, there is little difference in the distribution of R_0. As detailed in section 2.4, parameters for the mitigation trigger and healthcare capacity (subpanels 6-8) are calibrated by income group following Walker et al. (2020).[21]

2.2.3. Mobility Decomposition

We now decompose differences in mobility in response to death between free- and censored-media countries into what is attributable to differences in responsiveness and what can be attributed to other factors. To do so, we us a standard Oaxaca-Blinder decomposition approach traditionally used in labor economics (see Fortin, Lemieux and Firpo (2011)). To this end, we use a subsample analysis as:

$$m_{ist}^{M} = \alpha^{M} \hat{D}_{ist}^{M} + \sum_{k} \beta_{k}^{M} X_{kist}^{M} + \epsilon_{ist}^{M} \tag{2.3}$$

where M denotes whether a country has free media (F) or censored media (C), m indicates lockdown mobility outcomes, $\hat{D}$ is the simulated death toll per million from the first stage of the IV approach and X captures all other covariates from equation (2.2).

The mean mobility difference is $\Delta \equiv \bar{m}^{F} - \bar{m}^{C}$ and can be written as:

[20] Censored-media countries have, in fact, reported fewer deaths than free-media countries, and this is also a feature of the simulations from the SEIR model as shown in Figure A2.1.

[21] This calibration choice is due to the lack of country-level data and to avoid any potential endogeneity between the instrument that would arise if we used actual dates of lockdown measures.

$$\hat{\Delta} = \underbrace{(\hat{\alpha}^F - \hat{\alpha}^C)\bar{\hat{D}}^F}_{\text{Responsiveness effect}} + \underbrace{\hat{\alpha}^C(\bar{\hat{D}}^F - \bar{\hat{D}}^C)}_{\text{Death toll effect}} + \underbrace{\sum_k (\hat{\beta}_k^F \bar{X}_{kist}^F - \hat{\beta}_k^C \bar{X}_{kist}^C)}_{\text{Other factors}} \tag{2.4}$$

The first term on the right-hand side represents average differences in responsiveness to death between free-media and censored-media countries. It refers to the "unexplained effect" in the language of the decomposition literature (difference in the responsiveness coefficients). The second term captures the average effect of differences in death toll that affect mobility (differences in values of $\hat{D}$ or "explained effect"). The last term represents the overall effect of other covariates, namely outbreak time dummies, country dummies and the log global death toll. Provided that the responsiveness effects are correctly identified by the IV approach (as argued above), our decomposition should be viewed as causal rather than pure correlations.

Table 2.4 presents results of the decomposition for both the decision to lockdown and mobility trends following lockdowns. The first part of the Table shows the average overall difference in outcomes by media freedom, while the second part decomposes these differences following Equation (2.4). Overall, free-media countries are on average more likely to lock down by 2 percentage points, while also more effectively reducing time outside (-1.75 percentage point). Turning to the decomposition, we see across all outcomes that differences in responsiveness are large and account for about 40% of the absolute decomposed difference. Differences attributed to responsiveness are much larger than the observed overall difference in mobility by an order of magnitude. This is due to other effects such as time and country differences reducing observed differences in mobility (e.g. censored-media countries having outbreaks at a later date). Interestingly, the number of fatalities alone explains very little in observed mobility differences between free-media and censored-media countries. In other words, responsiveness to deaths rather than the number of deaths explain more of the difference in lockdown behaviour between free-media and censored-media countries. As a result, controlling for other factors, censored media could have further reduced outdoor mobility by 15 percentage points had they been as responsive as free-media countries.

2.3. Additional Results

2.3.1. Evidence of Information Mechanism from Internet Searches

We now explore the information mechanism through which the presence of free media can influence government and public decisions on lockdowns. We look at how media freedom affects the magnitude of COVID-19 internet searches following spikes in COVID-19 reported deaths. We hypothesise that seeing more reporting in the media might lead citizens to search more intensively online for information on COVID-19. As such, free media would increases citizens' awareness of the severity of COVID-19, driving higher responsiveness from citizens and governments according to our model.

Our core specification to investigate this is:

$$S_{it} = \alpha_i + \delta_t + +\eta D_{it} + \phi \left(D_{it} \times M_i \right) + \varepsilon_{it} \tag{2.5}$$

This regresses daily Google searches for COVID-19 on log deaths in free-media and censored-media countries, including country and outbreak time fixed effects. We also include day fixed effects to capture differences in aggregate daily patterns in searches as the pandemic unfolded. We interact media freedom with log deaths instead of simulated deaths to capture whether citizens in free-media countries are differently aware of deaths spikes. Our framework would lead us to expect individuals to react more strongly to a public signal on the severity of the disease such as death reporting.

Table 2.7 reports the results and shows in Columns 1-3 that countries with free media experience more online searches about coronavirus and see a larger increase in COVID-19 searches in response to higher reported deaths compared to censored-media countries. In column 4, we include country fixed effects, and also find that citizens are more inclined to search online about COVID-19 follow death spikes in free-media countries. These findings suggest that, apart from the direct impact of media freedom, citizens may choose to find additional ways of becoming informed. This reinforces the channel posited in the model linking media freedom to better informed citizens.

2.3.2. Subsample analysis

We now show that the core findings are robust to a different choice of specification that separately estimates responsiveness for free-media countries and censored-media countries as follows:

$$Y_{it}^{M} = \alpha_i^{M} + \delta_t^{M} + \gamma^{M} D_{it}^{M} + \varepsilon_{it}^{M} \tag{2.6}$$

This more flexible specification allows fixed effect and first-stage estimates to vary by media freedom status. Estimating the first stage separately on free-media and censored-media countries, also mitigates concerns about the IV estimator being biased in the presence of parameter heterogeneity.

Table 2.5 reports both OLS and IV results using the specification above. Columns 1-8 shows the main OLS results when dividing the sample between free-media and censored-media countries. Similar to previous findings, we report evidence of responsiveness by governments and citizens. As reported in column 1, a doubling of deaths leads to a 12.2 percentage points increase in the likelihood of imposing a lockdown in free-media countries, compared to 15.9 percentage point increase in censored-media countries. While the responsiveness is statistically significant in both samples, we cannot reject that the difference in point estimate is different from 0 at the 5% level. As shown in columns 2 to 8, we find strong evidence of responsiveness in both samples of countries. An increase in deaths is associated with a significant reduction in time spent outside – and also specifically at retails shops, public transportation, workplaces, groceries or parks – and an increase in time at home. Here again, we find evidence of responsiveness for citizens in both samples and we reject the null hypothesis that private responsiveness differs by media status.

Columns 9-16 of Table 2.5 report IV results from the subsample analysis. We replicate our previous results and find that, when accounting for misreporting, only governments and citizens of free-media countries are responsive to COVID-19 deaths. A doubling of deaths leads to a 23 percentage points increase in the likelihood to impose a lockdown in free-media countries but has no effect in censored-media countries (although we cannot reject the null of equality of these two coefficients at the 5% level). Second, we find robust evidence of citizens in free-media countries being responsive to COVID-19 when complying with lockdowns, and no similar evidence in censored-media countries. This result is found across all the range of outcomes in Columns 10-16 that capture both time indoor (Residentials) and outdoor. Overall, we find similar results using a subsample analysis or our baseline IV specification in Section 2.2.2

2.3.3. Media Freedom versus Other Country Characteristics

One possibility is that media freedom is picking up other sources of heterogeneity across countreis givig a false impression of what drives differential responsiveness. One way to address this is to explore a wide range of ways to cut the data to see if we get similar results. Hence we employ a whole array of subsamples using:

$$Y_{it}^C = \alpha_i^C + \delta_t^C + \gamma^C D_{it}^C + \varepsilon_{it}^C \tag{2.7}$$

where C indicates a specific country characteristic. In each case, we split the data into into two sub-samples based on different characteristics, including democratic institutions, executive constraints, an index of general preparedness for a pandemic, measures of trust, the presence of social protection systems, economic conditions, age composition and income distribution.[22] Table 2.6 reports the p-value when we test the hypothesis that the responsiveness coefficients in each sub-sample created are equal. It is striking that we find no evidence of heterogeneity in responsiveness to deaths between countries other than that from media freedom documented above.[23] This is illustrated graphically in Figure 2.3 for the case of high- and low-income countries. A notable finding is that responsiveness is very similar for high- and low-income countries, and that the confidence intervals for these coefficients largely overlap.

While this approach does not *prove* that it is differences in media freedom that are driving the differences in responsiveness, it does add credence to this view given that there would be plausible reasons to believe that there would be heterogeneous responsiveness across different

[22]See a full list of these characteristics and data construction in Appendix 2.4

[23]We find some evidence of differences in responsiveness based on the holding of elections in 2020, and whether a country enjoys free and fair elections, as discussed in Appendix 2.4. These results are in line with several studies highlighting the role of electoral concerns in lockdown decisions by governments (Gonzalez-Eiras and Niepelt, Fernandez-Navia, Polo-Muro and Tercero-Lucas, Pulejo and Querubín (2022, 2021, 2021))

levels of development and political configuration.

2.4. Concluding Comments

This paper has explored the role of free media in the responsiveness of governments and the public to the COVID-19 pandemic. As the death toll from COVID-19 escalated, governments in free-media countries were more likely to impose a lockdown and their citizens were more responsive to the death toll when reducing their mobility. We find limited evidence of a similar pattern of responsiveness in countries with censored media. We interpret this as being due to free media serving to align beliefs by citizens and governments about the severity of the outbreak and hence coordinate actions.[24] We also show corroborating evidence that citizens in free-media countries are more aware of COVID-19 deaths.

We draw some general lessons from the evidence presented here. First, access to timely and trustworthy sources of information is likely to play a role during public health crises. Our results point to the potential importance of free media in ensuring early responses by governments and greater mobility restrictions in response to death spikes. This highlights the value of access to information, however imperfect, to respond to new and emerging threats.[25] Second, the correlation between free media and responsiveness suggests that trusted institutions can foster cooperation and increase compliance. This has proved crucial for lockdown measures in which widespread support is critical. Relatedly, the nature of behavioral changes needed for social distancing underscores the voluntary nature of non-pharmaceutical interventions. Governments can act in conventional ways by imposing penalties and regulating, but there is an increasing role for public acceptance of costly measures that are taken in situations of emergencies. More generally, our results suggest that "quasi-voluntary compliance" (Levi (1988)), where compliance depends on both the enforcement regime and beliefs about the legitimacy of government interventions, are likely to be of first-order importance in public health crises.

Future work could look at how media freedom is related to other dimensions of policy responsiveness such as fiscal support to encourage staying at home and/or testing for infection. It would also be interesting to look at responsiveness in different phases of the pandemic fol-

[24] This argument is developed formally in Appendix 2.4

[25] Das et al. (2021) made a similar point.

lowing the initial lockdown period studied here. There was an interesting learning process as new variants and views about the efficacy of different mitigation responses have emerged. Another area which merits further investigation relates to interdependent policy making due to learning across countries/jurisdictions. This raises the possibility of that there was a process of yardstick competition as suggested, for example, in Besley and Case (1995), and Salmon (2019). By increasing information flows, free media can play a role in intensifying such competition.

This paper addresses long-standing themes in political economy, emphasising the role of media freedom in affecting policy and behavior. While it is too early to provide a proper assessment of the costs and benefits of the wide-ranging responses taken across different phases of the pandemic, the early period when the pandemic was emerging constitutes an interesting period to investigate the role of free media on policy since communication was key in fashioning a response. Our findings reinforce the message that free media can be important in shaping how societies respond to emerging policy challenges.

Tables and Figures

TABLE 2.1: RESPONSIVENESS AND MEDIA FREEDOM: OLS RESULTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Log deaths	0.155***	-6.951**	-7.213*	-5.621**	-4.207**	-6.022**	2.846**	-6.009**
	(0.0538)	(3.362)	(3.680)	(2.740)	(2.003)	(2.589)	(1.267)	(2.839)
Log deaths × Media Freedom	-0.0328	-4.333	-3.406	-3.665	-2.197	-1.851	1.071	-2.987
	(0.0612)	(3.965)	(4.230)	(3.228)	(2.480)	(3.425)	(1.503)	(3.318)
Observations	23,549	22,874	22,874	22,876	22,874	22,876	22,872	22,953
Country FE	X	X	X	X	X	X	X	X
Outbreak time FE	X	X	X	X	X	X	X	X

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Each regression includes as covariate the log number of global COVID-19 deaths. Log deaths indicates the log cumulative deaths. Each regression includes country fixed effects and outbreak time fixed effects (time since first 10 cumulative deaths) that is allowed to vary by media status given differences in timing and evolution of outbreaks. Media freedom indicates a country with a value above the median value of 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018. The dependent variable for Column 1 is a dummy variable for the implementation of a national lockdown for at least part of the day. Columns 2 to 7 indicate percentage changes in visits and length of stay after lockdown compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020. Column 8 is the unweighted average of mobility change in outside categories.

TABLE 2.2: RESPONSIVENESS AND MEDIA FREEDOM: IV RESULTS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
$\widehat{\text{Log deaths}}$	0.0303	2.496	2.566	1.343	1.139	2.330	-1.685	1.979
	(0.121)	(7.406)	(7.614)	(5.879)	(4.155)	(5.679)	(2.710)	(6.056)
$\widehat{\text{Log deaths} \times \text{Media Freedom}}$	0.200	-21.57**	-21.45**	-16.99**	-13.50**	-18.89***	8.860***	-18.39**
	(0.132)	(8.385)	(8.710)	(6.851)	(5.210)	(7.002)	(3.185)	(7.053)
Observations	21,708	21,183	21,183	21,185	21,183	21,185	21,181	21,262
F-stat First Stage	17.30	13.98	13.98	13.97	13.98	13.97	13.98	13.98
F-statistic death	35.82	29.19	29.19	29.19	29.19	29.19	29.19	29.20
F-statistic death x Media Freedom	67.40	61.52	61.52	61.52	61.52	61.52	61.55	63.15
Country FE	X	X	X	X	X	X	X	X
Outbreak time FE	X	X	X	X	X	X	X	X

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Each regression includes as covariate the log number of global COVID-19 deaths. $\widehat{\text{Log deaths}}$ indicates the log cumulative deaths from a first stage regression of log deaths using as instruments log simulated deaths and log simulated deaths × media freedom status. Media freedom indicates a country with a value above the median value of 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018. Simulated deaths are based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group. Each regression includes country fixed effects and outbreak time fixed effects (time since first 10 cumulative deaths). We allow the outbreak time fixed effects to vary by media status given the difference in timing and evolution of outbreaks. F-stat First stage indicates the Kleibergen-Paap rk Wald F statistic of the excluded instrument in the first stage. The dependent variable for Column 1 is a dummy variable for the implementation of a national lockdown for at least part of the day. Columns 2 to 7 indicate percentage changes in visits and length of stay after lockdown at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020. Column 8 is the unweighted average of mobility change in outside categories.

TABLE 2.3: CORRELATIONS BETWEEN MEDIA FREEDOM AND SEIR PARAMETERS FOR SIMULATED DEATHS INSTRUMENT

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	R_0	Outbreak Start	Mitigation Trigger	Hospital Beds	ICU Beds	Log Population	Share 70+	SEIR death index
Media Freedom	0.464*	-4.616	3.690	0.842	0.364	-0.498	3.576	0.0421
	(0.174)	(2.866)	(2.065)	(0.515)	(0.261)	(0.239)	(3.020)	(0.119)
Observations	158	158	174	174	174	174	158	144
R^2	0.034	0.013	0.11	0.10	0.063	0.022	0.13	0.00088

Notes: Significance levels: * 10%, ** 5%, *** 1%. Media freedom indicates a country with a value above the median value of 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018. See Appendix Section 2.4 for full details on the calibration of the SEIR model.

TABLE 2.4: DECOMPOSITION OF RESPONSIVENESS BY MEDIA FREEDOM

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Overall								
Free media	0.17	-9.90	-9.75	-8.10	-5.96	-6.33	3.80	-8.07
Censored media	0.15	-7.69	-7.86	-6.08	-4.60	-5.37	2.74	-6.32
Difference	0.02	-2.21	-1.88	-2.02	-1.37	-0.97	1.06	-1.75
Decomposition								
Responsiveness to death	0.17	-17.13	-17.13	-13.46	-10.71	-15.17	6.89	-14.86
Number of fatalities	0.04	-3.32	-3.17	-2.84	-2.21	-2.69	1.29	-2.95
Other covariates	-0.19	18.24	18.42	14.27	11.55	16.90	-7.12	16.07
Observations	21,708	21,183	21,183	21,185	21,183	21,185	21,181	21,262

Notes: This table show the Oaxaca-Blinder decomposition based on Equation (2.4). The measure of COVID-19 deaths is the instrumented log number of deaths from the first stage regression in Table A2.3. Other covariates include outbreak time dummies , country dummies, and the log number of global deaths each day. Media freedom indicates a country with a value above the median value of 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018. The dependent variable for Column 1 is a dummy variable for the implementation of a national lockdown for at least part of the day. Columns 2 to 7 indicate percentage changes in visits and length of stay after lockdown at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020.

TABLE 2.5: RESPONSIVENESS AND MEDIA FREEDOM: SUBSAMPLE ANALYSIS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
				OLS estimates				
Panel A: Free-media countries								
Log deaths	0.122***	-11.29***	-10.62***	-9.287***	-6.405***	-7.874***	3.918***	-8.997***
	(0.0291)	(2.108)	(2.092)	(1.713)	(1.467)	(2.249)	(0.811)	(1.722)
Observations	11,822	11,547	11,547	11,547	11,547	11,547	11,545	11,624
Panel B: Censored-media countries								
Log deaths	0.155***	-6.930**	-7.190*	-5.601**	-4.188**	-6.006**	2.838**	-5.991**
	(0.0541)	(3.378)	(3.698)	(2.754)	(2.015)	(2.603)	(1.274)	(2.854)
Observations	11,727	11,327	11,327	11,329	11,327	11,329	11,327	11,329
Country fixed effect	X	X	X	X	X	X	X	X
Outbreak time fixed effect	X	X	X	X	X	X	X	X
H_0: Equality of coefficients (*p-value*)	0.73	0.13	0.24	0.11	0.20	0.37	0.27	0.19
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
				2SLS estimates				
Panel A: Free-media countries								
$\widehat{\text{Log deaths}}$	0.230***	-19.02***	-18.83***	-15.59***	-12.31***	-16.52***	7.150***	-16.37***
	(0.0540)	(3.929)	(4.225)	(3.515)	(3.140)	(4.093)	(1.670)	(3.613)
Observations	11,363	11,115	11,115	11,115	11,115	11,115	11,113	11,192
F-stat First Stage	33.27	34.22	34.22	34.22	34.22	34.22	34.25	35.89
Panel B: Censored-media countries								
$\widehat{\text{Log deaths}}$	0.0306	2.481	2.549	1.330	1.126	2.318	-1.678	1.965
	(0.122)	(7.443)	(7.653)	(5.910)	(4.177)	(5.706)	(2.722)	(6.088)
Observations	10,345	10,068	10,068	10,070	10,068	10,070	10,068	10,070
F-stat First Stage	34.47	27.83	27.83	27.82	27.83	27.82	27.83	27.82
Country fixed effect	X	X	X	X	X	X	X	X
Outbreak time fixed effect	X	X	X	X	X	X	X	X
H_0: Equality of coefficients (*p-value*)	0.10	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Each regression includes as covariate the log number of global COVID-19 deaths. $\widehat{Logdeaths}$ indicates the value of log deaths from a first stage regression of log reported deaths using as instrument log simulated deaths based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group. F-stat First stage indicates the Kleibergen-Paap rk Wald F statistic of the excluded instrument in the first stage. Media freedom indicates a country with a value above the median value of 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018. The dependent variable for Column 1 is a dummy variable for the implementation of a national lockdown for at least part of the day. Columns 2 to 7 indicate percentage changes in visits and length of stay after lockdown at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020.

TABLE 2.6: DRIVERS OF RESPONSIVENESS USING ALTERNATIVE SUBSAMPLE ANALYSIS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Media Freedom	[0.10]	[< 0.01]	[< 0.01]	[< 0.01]	[< 0.01]	[< 0.01]	[< 0.01]	[< 0.01]
Free and fair elections	[0.42]	[0.03]	[0.07]	[0.05]	[0.07]	[0.03]	[0.03]	[0.04]
Election in 2020	[0.27]	[0.03]	[0.10]	[0.04]	[0.10]	[0.01]	[0.08]	[0.05]
Most people can be trusted	[0.39]	[0.26]	[0.27]	[0.24]	[0.24]	[0.54]	[0.32]	[0.28]
Confidence in government	[0.81]	[0.60]	[0.56]	[0.73]	[0.88]	[0.40]	[0.61]	[0.59]
Confidence in the press	[0.89]	[0.46]	[0.44]	[0.50]	[0.52]	[0.38]	[0.50]	[0.43]
Satisfaction with democracy	[0.36]	[0.22]	[0.43]	[0.23]	[0.58]	[0.29]	[0.29]	[0.27]
Willingness to fight for country	[0.42]	[0.11]	[0.17]	[0.10]	[0.14]	[0.11]	[0.13]	[0.11]
Democracy	[0.75]	[0.30]	[0.39]	[0.36]	[0.48]	[0.23]	[0.34]	[0.32]
Executive constraints	[0.57]	[0.95]	[0.96]	[0.94]	[0.83]	[1.00]	[0.74]	[0.95]
Log GDP per capita > median	[0.93]	[0.63]	[0.96]	[0.56]	[0.99]	[0.90]	[0.95]	[0.82]
Education > median	[0.59]	[0.89]	[0.86]	[0.95]	[0.57]	[0.69]	[0.76]	[0.85]
Global Health Security index	[0.20]	[0.48]	[0.34]	[0.47]	[0.23]	[0.37]	[0.22]	[0.34]
Tax revenue > median	[0.29]	[0.37]	[0.57]	[0.46]	[0.98]	[0.71]	[0.38]	[0.60]
Access to handwashing facilities	[0.78]	[0.62]	[0.82]	[0.50]	[0.75]	[0.46]	[0.72]	[0.63]
Income inequality	[0.40]	[0.23]	[0.34]	[0.20]	[0.58]	[0.97]	[0.23]	[0.40]
Social protection	[0.57]	[0.60]	[0.50]	[0.72]	[0.56]	[0.52]	[0.68]	[0.57]

Notes: Each cell is the p-value from a separate IV regression based on Equation (2.2). The p-value tests the null hypothesis of equality of the responsiveness to death coefficient for two subsamples based on the category mentioned in the row. See Appendix Section 2.4 for definition of subsamples.

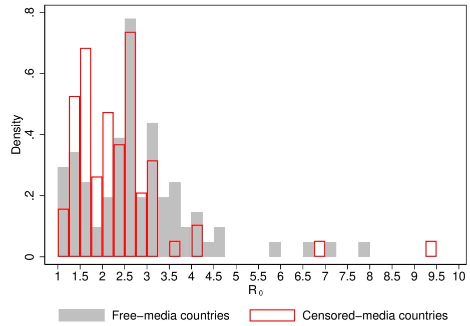
TABLE 2.7: COVID-19 DEATHS, ONLINE SEARCHES AND MEDIA FREEDOM

	(1)	(2)	(3)	(4)
	Dependent Variable: COVID-19 Online Searches			
Media Freedom	61.73***	54.25***	37.03**	
	(20.20)	(18.66)	(17.04)	
Log deaths	47.03***	40.40**	19.89	5.504
	(5.095)	(16.42)	(16.75)	(14.53)
Media Freedom × Log deaths			21.60**	17.85**
			(8.656)	(8.080)
Country fixed effect				X
Outbreak time fixed effect		X	X	X
Day fixed effect		X	X	X
Basic controls	X	X	X	
Observations	19,932	19,444	19,444	23,715
R^2	0.16	0.58	0.58	0.69

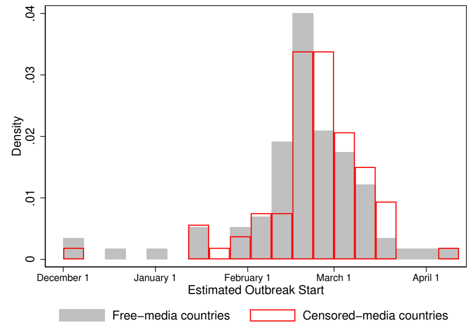
Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Columns 1-3 control for the share of internet users, years or education and log GDP per capita in a country (denoted basic controls). Covid search indicates the normalised share of Google searches including the word "COVID-19" or similar terms compared to the United States in January 30, 2020. Reported deaths are measured per million. Outbreak time indicates the days since a country reported its first 10 COVID-19 deaths. Simulated deaths represent simulated fatalities based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group.

FIGURE 2.1: PARAMETERS FOR SEIR DEATHS BY MEDIA FREEDOM STATUS

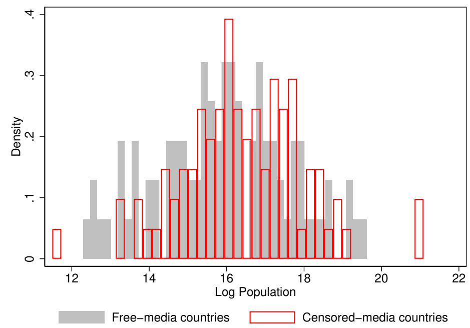
1: R_0



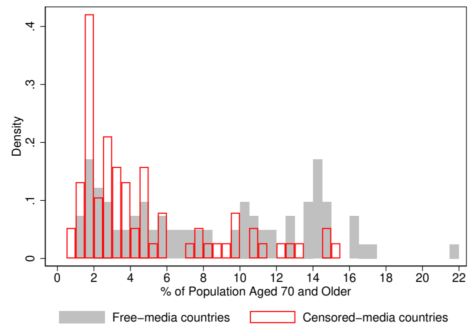
2: Estimated date of first outbreak in country



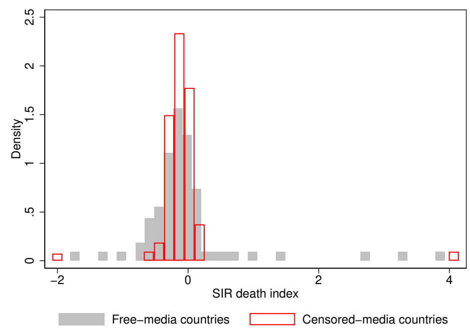
3: Population



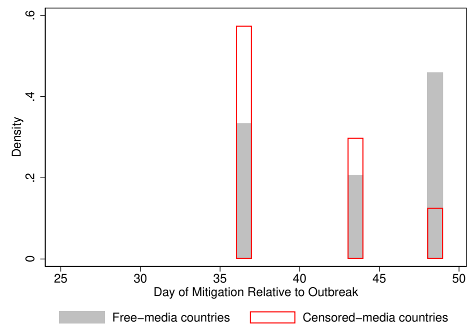
4: Age



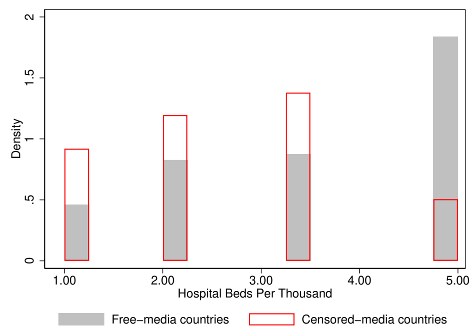
5: SEIR death index



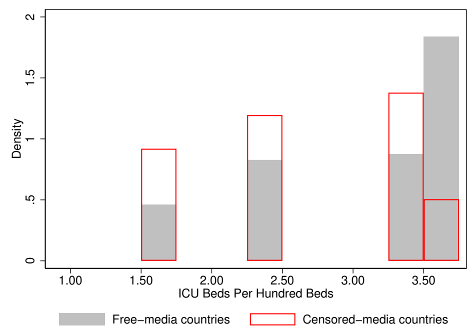
6: Day of Mitigation measures



7: Beds per thousand



8: ICU Beds per thousand



Notes: Day of mitigation measures relative to outbreak, beds and ICU beds are calibrated by income group following Walker et al. (2020). Country SEIR death index is obtained using the standardized country fixed effects when regressing SEIR deaths per million on country, date, outbreak time and income group fixed effects.

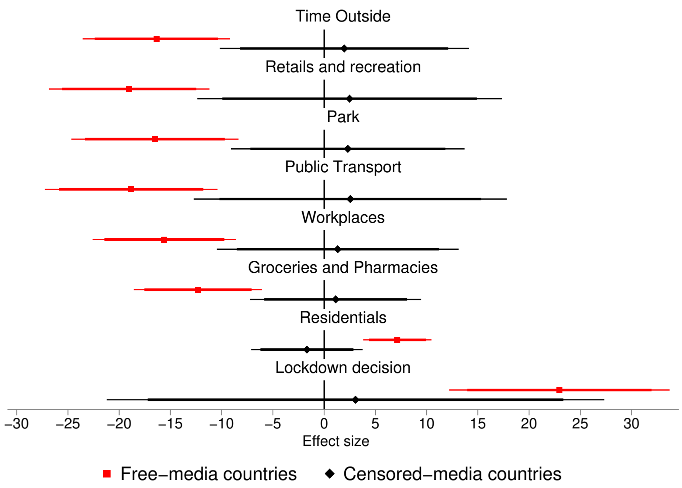


FIGURE 2.2: RESPONSIVENESS OF LOCKDOWN MOBILITY BY MEDIA FREEDOM

Notes: This figure shows the coefficients of deaths on mobility from Table 2.5. A thick line indicate a 90% confidence interval, a thin line indicates a 95% confidence interval. A free-media country has a score above the median value of 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018. Each panel indicates percentage changes in visits and length of stay after lockdown at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020.

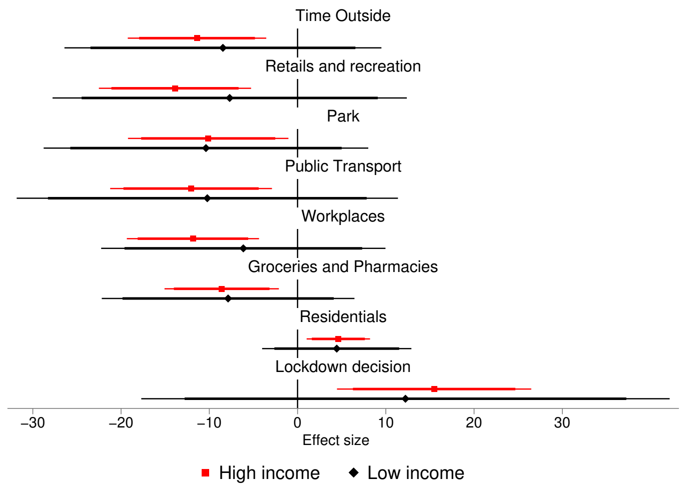


FIGURE 2.3: RESPONSIVENESS OF LOCKDOWN MOBILITY BY INCOME

Notes: This figure shows the coefficients of responsiveness on mobility from Equation (2.2). High income indicates countries with GDP per capita above the median value. A thick line indicate a 90% confidence interval, a thin line indicates a 95% confidence interval. Each panel indicates percentage changes in visits and length of stay after lockdown at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020.

Appendix to Chapter 2

Appendix Tables and Figures

TABLE A2.1: SUMMARY STATISTICS

	All			Free media			Censored media			Difference	
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD	Mean	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Country characteristics											
Media Freedom	0.69	0.74	(0.25)	0.88	0.88	(0.06)	0.49	0.59	(0.22)	0.39	[0.00]
GDP per capita	14,908	5,791	(20,686)	22,334	11,503	(24,809)	7,577	3,590	(11,780)	14,757	[0.00]
Years of education	8.00	8.03	(3.14)	8.85	9.07	(3.28)	6.97	6.77	(2.65)	1.88	[0.00]
Share of population 60 or older	0.14	0.11	(0.09)	0.17	0.16	(0.09)	0.11	0.07	(0.08)	0.06	[0.00]
Free and fair elections	0.53	1.00	(0.50)	0.82	1.00	(0.39)	0.24	0.00	(0.43)	0.57	[0.00]
Democracy	0.62	1.00	(0.49)	0.92	1.00	(0.27)	0.32	0.00	(0.47)	0.60	[0.00]
Trust in people	0.45	0.00	(0.50)	0.51	1.00	(0.50)	0.40	0.00	(0.49)	0.11	[0.26]
Access to handwashing facilities	0.40	0.00	(0.49)	0.52	1.00	(0.51)	0.32	0.00	(0.47)	0.20	[0.13]
Global Health Security index	43.30	41.30	(14.44)	49.08	47.90	(15.06)	37.43	35.95	(11.12)	11.65	[0.00]
COVID-19 Online Searches	60	-65	(259)	79	-55	(270)	42	-74	(247)	37	[0.03]
B. COVID-19 death toll											
First case	03/03	03/06	(20.04)	02/28	03/03	(19.23)	03/06	03/08	(20.44)	-6.41	[0.05]
First 10 deaths	04/24	04/10	(41.25)	04/18	04/04	(39.49)	04/30	04/20	(42.22)	-12.73	[0.05]
Deaths per million as of 05/01	34.04	3.42	(96.72)	61.99	5.72	(131.31)	6.46	1.75	(11.80)	55.53	[0.00]
Simulated deaths per million as of 05/01	360.27	7.87	(1,138)	614.52	33.54	(1,462)	83.59	2.30	(498)	530.93	[0.01]
C. Lockdown											
Date of lockdown	03/26	03/25	(8.07)	03/24	03/24	(6.76)	03/27	03/25	(8.97)	-3.52	[0.03]
Deaths per million at time of lockdown	0.83	0.06	(2.13)	1.32	0.06	(2.82)	0.32	0.06	(0.78)	1.00	[0.02]
Mobility Change (%): Retails and recreation	-63.71	-67.00	(18.37)	-66.30	-73.00	(20.23)	-60.52	-61.00	(15.19)	-5.78	[0.11]
Mobility Change (%): Groceries and pharmacies	-38.56	-37.00	(20.97)	-39.95	-38.00	(22.50)	-36.85	-36.00	(18.77)	-3.10	[0.37]
Mobility Change (%): Parks	-42.00	-45.00	(27.11)	-42.63	-48.00	(31.68)	-41.22	-44.00	(20.10)	-1.41	[0.79]
Mobility Change (%): Public transport	-64.00	-67.00	(16.07)	-65.16	-69.00	(16.54)	-62.55	-64.00	(15.37)	-2.61	[0.43]
Mobility Change (%): Workplaces	-51.19	-54.00	(18.64)	-53.95	-57.00	(20.20)	-47.81	-50.00	(15.90)	-6.14	[0.06]
Mobility Change (%): Residential	24.06	23.00	(8.42)	25.68	26.00	(8.90)	22.08	21.00	(7.33)	3.60	[0.02]
Number of countries		155			77			78			
Observations		23,715			11,781			11,934			

Notes: The sample includes all countries with reported COVID-19 cases between December 1, 2019 and May 1st, 2020. Media freedom indicates a country with a value above the median value of 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018. See Appendix Section 2.4 for definition of subsamples. Mobility indicates percentage changes in visits and length of stay at different places after lockdown compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020 using Google mobility trends data. Covid search indicates the share of Google searches featuring the term "COVID-19" or similar terms. Search trends are normalised to be percentage changes relative to the COVID-19 search share in the United States on January 30, 2020.

TABLE A2.2: RESPONSIVENESS AND MEDIA FREEDOM: REDUCED FORM AND FIRST STAGE

	(1)	(2)	(3)
	Time Outside	Log deaths	Log deaths × Media Freedom
Log SEIR deaths	0.584	0.296***	0.0000943
	(1.742)	(0.0559)	(0.000258)
Log SEIR deaths × Media Freedom	-6.251***	0.0499	0.345***
	(2.109)	(0.0803)	(0.0576)
Observations	21,262	21,262	21,262
F-stat First Stage		13.98	13.98
F-statistic death		29.20	
F-statistic death × Media Freedom			63.15
Country FE	X	X	X
Outbreak time FE	X	X	X
Outbreak time fixed effect × Media Freedom FE	X	X	X

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Reported and simulated deaths in log. Outbreak time indicates the days since a country reported its first 10 COVID-19 deaths. Simulated deaths represent simulated fatalities based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group.

TABLE A2.3: RESPONSIVENESS AND MEDIA FREEDOM: FIRST STAGE, SUBSAMPLE ANALYSIS

	(1)	(2)
	Free media	Censored media
Log SEIR deaths	0.346***	0.296***
	(0.0577)	(0.0561)
Country fixed effect	X	X
Outbreak time fixed effect	X	X
Kleibergen-Paap Wald rk F statistic	35.89	27.82
Observations	11,192	10,070

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Reported and simulated deaths in log. Outbreak time indicates the days since a country reported its first 10 COVID-19 deaths. Simulated deaths represent simulated fatalities based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group.

Table A2.4: Responsiveness and Media Freedom: Reduced Form, Subsample analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Panel A: Free-media countries								
Log SEIR deaths	0.0800***	-6.509***	-6.442***	-5.335***	-4.213***	-5.651***	2.447***	-5.658***
	(0.0165)	(1.370)	(1.318)	(1.221)	(0.961)	(1.364)	(0.507)	(1.196)
Observations	11,363	11,115	11,115	11,115	11,115	11,115	11,113	11,192
Panel B: Censored-media countries								
Log SEIR deaths	0.00952	0.734	0.754	0.393	0.333	0.686	-0.497	0.582
	(0.0385)	(2.141)	(2.201)	(1.714)	(1.212)	(1.624)	(0.760)	(1.751)
Observations	10,345	10,068	10,068	10,070	10,068	10,070	10,068	10,070
Country fixed effect	X	X	X	X	X	X	X	X
Outbreak time fixed effect	X	X	X	X	X	X	X	X
H_0: Equality of coefficients (*p-value*)	0.08	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01	< 0.01

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Each regression includes as covariate the log number of global COVID-19 deaths. Simulated deaths is the log number of fatalities based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group. The dependent variable for Column 1 is a dummy variable for the implementation of a national lockdown for at least part of the day. Columns 2 to 7 indicate percentage changes in visits and length of stay after lockdown at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020.

Table A2.5: COVID-19 Online Searches and Lockdown Mobility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Lockdown	-28.28***	-22.15***	-28.30***	-27.27***	-23.81***	9.614***	-25.95***
	(3.503)	(3.171)	(5.447)	(3.766)	(3.326)	(1.347)	(3.176)
COVID-19 searches × Lockdown	-0.0219***	-0.0239***	-0.0309***	-0.0234***	-0.0191***	0.0149***	-0.0238***
	(0.00667)	(0.00628)	(0.00910)	(0.00622)	(0.00642)	(0.00238)	(0.00583)
Country fixed effect	X	X	X	X	X	X	X
Day fixed effect	X	X	X	X	X	X	X
Outbreak time fixed effect	X	X	X	X	X	X	X
Mean Outcome ∣ Lockdown=1	-64.12	-38.90	-42.55	-64.77	-51.96	24.26	-52.45
Mean Covid search ∣ Lockdown=1	298.78	298.78	298.85	298.78	298.85	298.41	298.85
SE Covid search ∣ Lockdown=1	(249.54)	(249.54)	(249.49)	(249.54)	(249.49)	(249.69)	(249.49)
Observations	9,289	9,257	9,271	9,238	9,315	9,217	9,315

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Covid search measure the share of internet searches including one of the following terms: "covid", "coronavirus", "covid19", "covid-19", "covid 19", "ncov" , "nCoV2019". These national search trends are normalised to be relative to the COVID-19 search share in the United States on January 30, which takes a value of 0. Outbreak time indicates the days since a country reported its first 10 COVID-19 deaths. Columns 1 to 6 indicate changes in visits and length of stay at different places after lockdown compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020, which takes a value of 0.

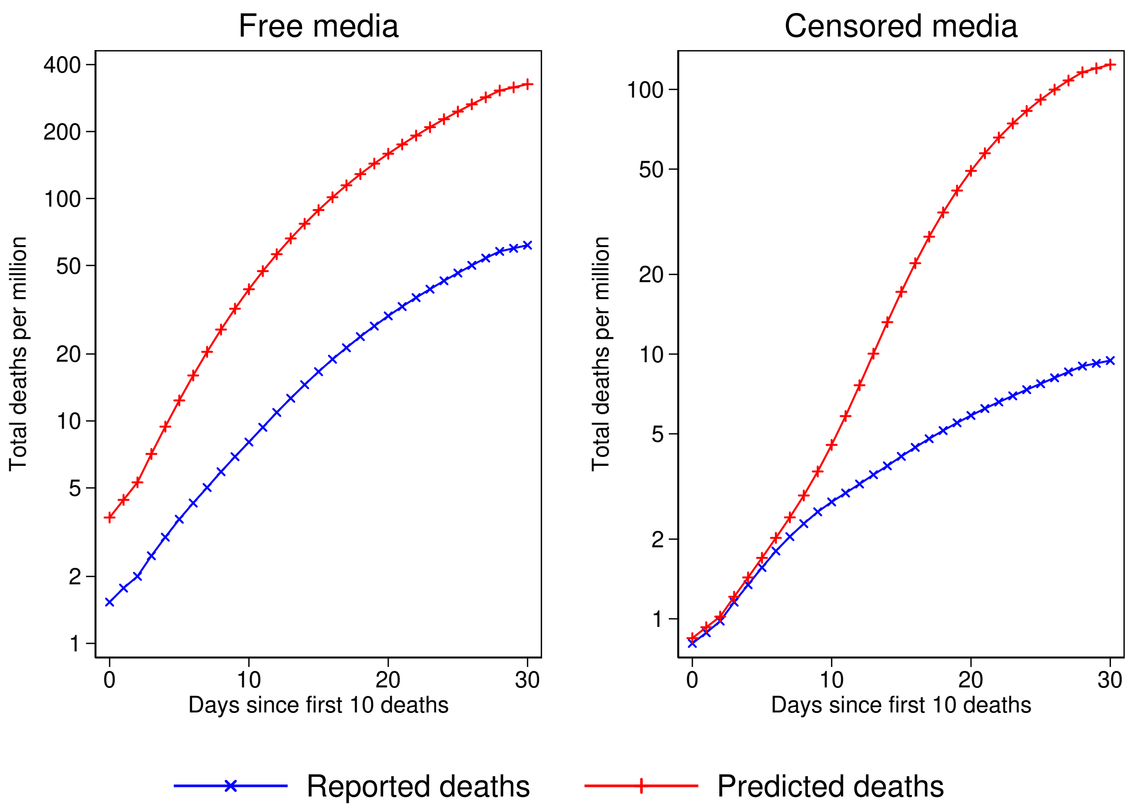
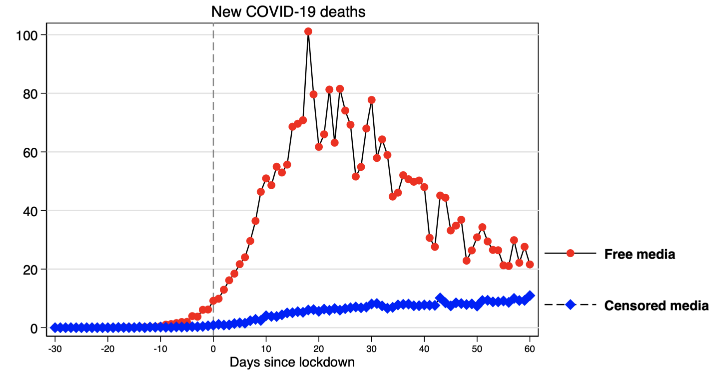


FIGURE A2.1: REPORTED VS SIMULATED DEATHS

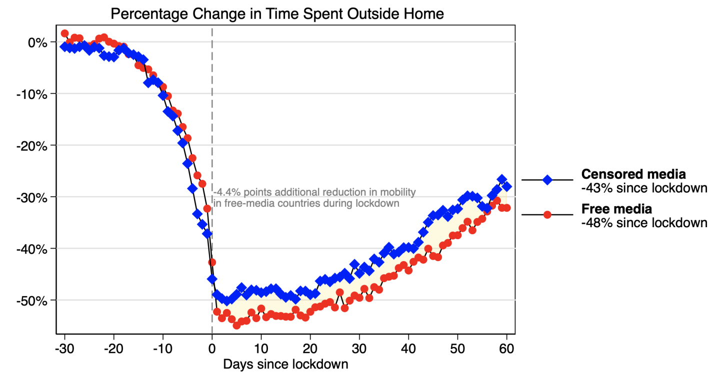
Notes: Simulated deaths is based on a SEIR model described in Section 2.1 with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group. Media freedom indicates a country with a value above the median value 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018. Both series report average cumulative death based on a balanced panel of countries who reported COVID-19 deaths for at least 30 days after their first 10 reported COVID-19 deaths. Values shown as 5-day moving averages using a log scale.

FIGURE A2.2: DESCRIPTIVE TRENDS IN COVID-19 MORTALITY, SEARCHES AND MOBILITY

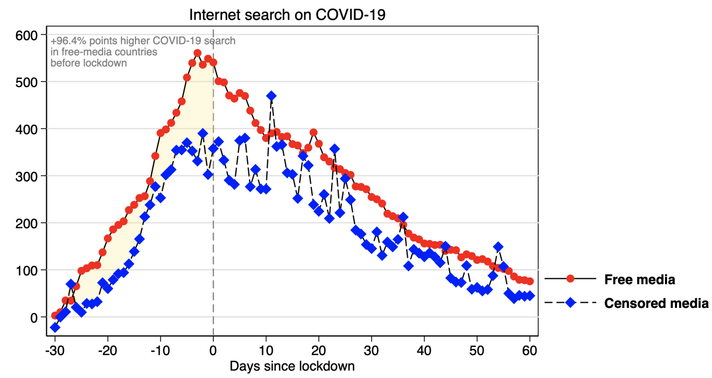
A. Evolution of COVID-19 mortality in free-media and censored-media countries



B. Evolution of mobility reduction in free-media and censored-media countries



C. Evolution of COVID-19 online searches in free-media and censored-media countries



Notes: Figure A shows the average number of new COVID-19 deaths. Figure B shows the average mobility change in time spent outside home compared to January 2020 using the google mobility data. Time outside home is the average of mobility changes across outdoor categories. Figure C displays the number of online searches about COVID-19 relative to the COVID-19 search share in the United States on January 30, 2020 (baseline of 100). All figures use a balanced sample of free- and censored-media countries that have imposed a lockdown for at least 60 days as of May 1, 2020. Free-media status is determined using the V-Dem Freedom of Expression and Alternative Source of Information index.

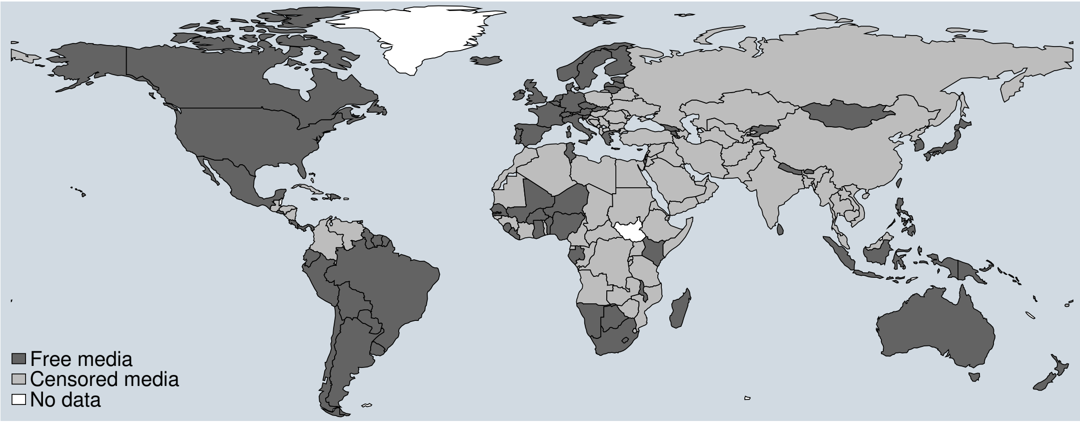


FIGURE A2.3: FREE-MEDIA AND CENSORED-MEDIA COUNTRIES

Notes: Free media indicates a country with a value above the median value 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018. **Free-media countries**: Argentina Armenia Australia Austria Belgium Benin Bolivia Brazil Burkina Faso Canada Cape Verde Chile Costa Rica Cyprus Czech Republic Denmark Dominican Republic Ecuador El Salvador Estonia Finland France Gabon Gambia Georgia Germany Ghana Greece Guyana Iceland Indonesia Ireland Israel Italy Jamaica Japan Kenya Kyrgyzstan Latvia Liberia Lithuania Luxembourg Madagascar Malawi Mali Malta Mauritius Mexico Namibia Nepal Netherlands New Zealand Niger Nigeria Norway Panama Paraguay Peru Philippines Portugal Sao Tome and Principe Senegal Sierra Leone Slovakia Slovenia South Africa South Korea Spain Sri Lanka Suriname Sweden Switzerland Trinidad and Tobago Tunisia United Kingdom United States Uruguay. **Censored-media countries**: Afghanistan Albania Algeria Angola Azerbaijan Bahrain Bangladesh Belarus Bosnia and Herzegovina Bulgaria Cameroon Central African Republic Chad China Colombia Congo Croatia Cuba Côte d'Ivoire DRC Djibouti Egypt Equatorial Guinea Ethiopia Guatemala Guinea Guinea-Bissau Haiti Honduras Hungary India Iran Iraq Jordan Kazakhstan Kosovo Kuwait Lebanon Lesotho Libya Macedonia Malaysia Maldives Mauritania Moldova Montenegro Morocco Mozambique Nicaragua Oman Pakistan Palestine Poland Qatar Romania Russia Rwanda Saudi Arabia Serbia Singapore Somalia Sudan Swaziland Syria Tajikistan Tanzania Thailand Togo Turkey Uganda Ukraine United Arab Emirates Uzbekistan Venezuela Vietnam Yemen Zambia Zimbabwe. Countries not in sample are those with less than 10 reported COVID-19 deaths or not included in the V-Dem media freedom index.

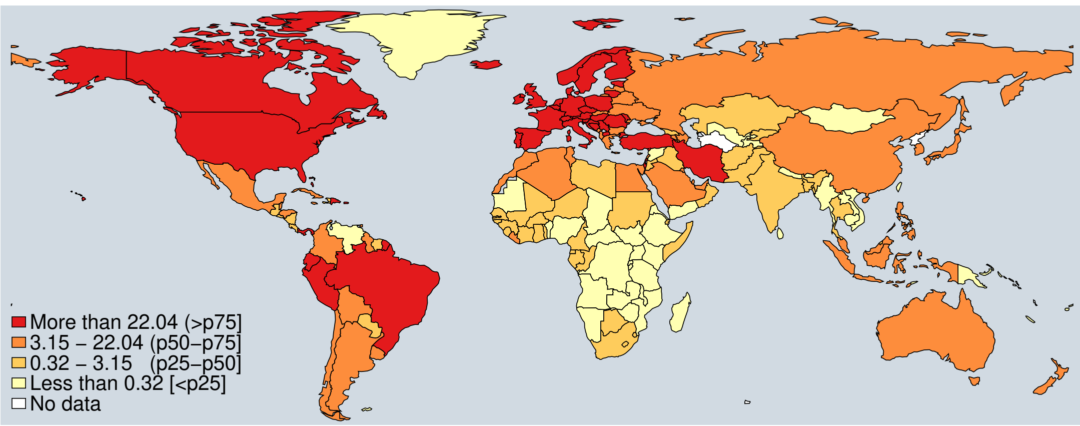


FIGURE A2.4: TOTAL REPORTED DEATHS PER MILLION AS OF MAY 1, 2020

Notes: Categories of countries constructed using as cutoff the 25^{th}, 50^{th} and 75^{th} percentile of total reported deaths per million as of May 1, 2020. *Source:* ECDC.

Conceptual Framework

Basics

We build a simple model where the government is choosing whether to lockdown the economy and citizens choose whether to take costly actions, such as social distancing, working from home or wearing face coverings that are intended to reduce the spread of a disease, in our case COVID-19. Whether or not such actions are needed depends on the state of the world, $\sigma \in \{0, 1\}$. The social benefit from fighting the threat in state σ is $\rho\Delta(\sigma)$ with $\Delta(1) > 0 > \Delta(0)$ where ρ is the proportion of the population who decide to mitigate the threat. This formulation implies that fighting the pandemic with lockdowns and social distancing is beneficial only if $\sigma = 1$.

Let π denote a common prior that $\sigma = 1$. This reflects the genuine uncertainty in the pandemic about whether lockdown and private mitigation measures were really necessary. Then the expected social benefit from fighting the pandemic is

$$\rho\hat{\Delta}(\pi) = \rho\left[\pi\Delta(1) + (1-\pi)\Delta(0)\right].$$

Define $\bar{\pi} = -\Delta(0) / [\Delta(1) - \Delta(0)]$. Then $\hat{\Delta}(\pi) > 0$ for all $\pi > \bar{\pi}$.

Citizens

There is a continuum of citizens indexed by $i \in [0, 1]$ and their social distancing decisions are denoted by $\delta(i) \in \{0, 1\}$ where $\delta(i) = 1$ denotes social distancing. The fraction of citizens who mitigate is therefore $\rho = \int_0^1 \delta(i)\, di$. Citizens face a private cost of social distancing denoted by $\varepsilon(i)$ which is uniformly distributed on $[0, E]$. The government can enact a lockdown $\lambda \in \{0, 1\}$ with a fine Φ for non-compliance. Citizens can be pro-socially motivated agents and care about the social costs and benefits as in Besley and Ghatak (2005). We assume that $\Phi / [E - \Delta(1)] < 1$ which implies that there there will always be limited compliance with a lockdown.

Putting these elements together the payoff of citizen i is

$$\delta(i)\left[\rho\hat{\Delta}(\pi) - \varepsilon(i)\right] - (1 - \delta(i))\lambda\Phi \tag{2.8}$$

where Φ is the expected penalty from breaking a lockdown. We will look for a Nash equilibrium in social distancing decisions where:

$$\hat{\delta}(i, \rho, \pi) = \arg\max_{\delta \in \{0,1\}} \left\{\delta\left[\rho\hat{\Delta}(\pi) + \lambda\Phi - \varepsilon(i)\right]\right\}. \tag{2.9}$$

The equilibrium level of social distancing is given by

$$\hat{\rho}(\pi, \lambda) = \frac{\lambda \Phi}{E - \hat{\Delta}(\pi)}. \tag{2.10}$$

This is determined by whether there is a lockdown, $\lambda\Phi$. There is also a social multiplier in the denominator of (2.10) which is increasing in π. This reflects the positive complementarity in the citizens' compliance decisions. There is more social distancing when there is a stronger belief that there are social benefits from doing so. Note that $\rho(\pi, 0) = 0$ so there will not be any private response to the pandemic unless there is a lockdown.[26]

Government

Government must decide whether to impose a lockdown. We assume that, just as with citizens, it has mixed motives. So, given a belief π, its objective function is

$$\rho \hat{\Delta}(\pi) + \lambda r \tag{2.11}$$

where $r \in \left[0, \frac{1}{\theta}\right]$ is a political cost from implementing a lockdown. Assume that $\theta \Delta(1) < 1$. This implies that the probability of a lockdown is

$$\Lambda = \begin{cases} \theta \rho \hat{\Delta}(\pi) & \text{if } \hat{\Delta}(\pi) > 0 \\ 0 & \text{otherwise.} \end{cases} \tag{2.12}$$

This gives a complementarity between compliance and the likelihood of observing a lockdown; more compliance encourages implementing a lockdown when $\hat{\Delta}(\pi) > 0$.

We assume that the government receives a signal about the severity of the disease, denoted by d, and that it updates its belief that $\sigma = 1$ to $\hat{\pi}(d) = \pi + \beta\left(d - \hat{d}\right)$.[27] This says that there is a threshold level, $\hat{d}$, such that for $d > \hat{d}$, the government will believe it more likely that $\sigma = 1$. We will be focusing the case which we believe is more relevant in our empirical example which is where $d > \hat{d}$ so that there is a strengthening belief that $\sigma = 1$.

Media

Suppose that the media determines whether the citizens are informed about d. Let $\alpha \in [0, 1]$ be the probability that they observe d. If they do not observe d, then we suppose that their beliefs are

$$\Pi(\lambda) = \begin{cases} \bar{\pi} & \text{if } \lambda = 1 \\ \underline{\pi} < \bar{\pi} & \text{if } \lambda = 0. \end{cases}$$

[26] We make this assumption for expositional simplicity. It would be straightforward to have private responses directly dependent on the severity of the threat, π.

[27] This could be derived from a standard Bayesian model but we do not need to be specific about this.

These are rational beliefs given the observed government action. This implies that the fraction of citizens who comply as a function of λ and α is:

$$\bar{\rho}\left(\lambda\right)=\begin{cases}\frac{\lambda\Phi}{E-\hat{\Delta}(\hat{\pi}(d))} & \text{with probability } \alpha\\ \frac{\lambda\Phi}{E-\hat{\Delta}(\Pi(\lambda))} & \text{with probability } 1-\alpha\end{cases}$$

As long as $\pi > \bar{\pi}$, then having more informed citizens increases compliance with the lockdown. This is driven by the social multiplier as the social benefits from the lockdown are now perceived to be higher.

Equilibrium

We now consider the equilibrium where citizens are choosing whether or not they will be comply with a lockdown alongside a decision of whether to lockdown. The timing that we consider is as follows:

1. Nature determines d.
2. Government makes a lockdown decision $\lambda \in \{0, 1\}$
3. Nature determines whether citizens become informed about d.
4. Citizens decide whether to comply with the lockdown.

We have already solved for stage 3, where the expected fraction of citizens who comply with a lockdown is

$$\hat{\rho}\left(\alpha,d\right)=\frac{\alpha\Phi}{E-\hat{\Delta}\left(\hat{\pi}\left(d\right)\right)}+\frac{\left(1-\alpha\right)\Phi}{E-\hat{\Delta}\left(\bar{\pi}\right)}.$$

It is straightforward to see that this is increasing in d if $d > \hat{d}$. Note that

$$\frac{\partial\hat{\rho}\left(\alpha,d\right)}{\partial d}=\frac{\alpha\beta\Phi\left[\Delta\left(1\right)-\Delta\left(0\right)\right]}{\left[E-\hat{\Delta}\left(\hat{\pi}\left(d\right)\right)\right]^{2}}>0$$

is a measure of how expected responsive varies with d. The probability of lockdown is

$$\hat{\Lambda}\left(\alpha,d\right)=\begin{cases}\theta\hat{\rho}\left(a,d\right)\hat{\Delta}\left(\hat{\pi}\left(d\right)\right) & \text{if } \hat{\Delta}\left(\hat{\pi}\left(d\right)\right)>0\\ 0 & \text{otherwise.}\end{cases} \tag{2.13}$$

Moreover, for $\hat{\Lambda}\left(\alpha,d\right)>0$,

$$\frac{\partial\hat{\Lambda}\left(\alpha,d\right)}{\partial d}=\frac{\partial\hat{\rho}\left(\alpha,d\right)}{\partial d}\hat{\Delta}\left(\hat{\pi}\left(d\right)\right)+\hat{\rho}\left(\alpha,d\right)\beta\Phi\left[\Delta\left(1\right)-\Delta\left(0\right)\right]>0.$$

This says that an increase in the progression of the disease is associated with a high probability of a lockdown. Our measure of responsiveness that we also take to the data is

$$m(\alpha, d) = \hat{\Lambda}(\alpha, d)\,\hat{\rho}(\alpha, d)$$

which is also increasing in d when $\hat{\Delta}(\hat{\pi}(d)) > 0$.

Less obvious, is how changes in α affect these results. In other words, how does responsiveness vary with media activism. We report this as follows.

Prediction: *Suppose that $\hat{\Delta}(\hat{\pi}(d)) > 0$, then, with more active media (higher α), governments and citizens are more responsive to the progression of the disease.*

Proof. First note that

$$\frac{\partial^2 \hat{\rho}(\alpha, d)}{\partial d \partial \alpha} = \frac{\beta \Phi [\Delta(1) - \Delta(0)]}{\left[E - \hat{\Delta}(\hat{\pi}(d))\right]^2} > 0.$$

And

$$\frac{\partial^2 \hat{\Lambda}(\alpha, d)}{\partial d \partial \alpha} = \frac{\partial^2 \hat{\rho}(\alpha, d)}{\partial d \partial \alpha}\hat{\Delta}(\hat{\pi}(d)) + \frac{\partial \hat{\rho}(\alpha, d)}{\partial \alpha}\beta \Phi [\Delta(1) - \Delta(0)] > 0.$$

And since $m(\alpha, d)$ is the product of two increasing supermodular functions, it is also supermodular, i.e. $\frac{\partial^2 m(\alpha,d)}{\partial d \partial \alpha} > 0$. ■

This prediction is what we take to the data by exploring how responsiveness varies between countries with and without free media which we think of as variation in α.

Link to Empirical Analysis

To link the model to the empirical analysis, note that we construct a variable linked to private compliance by recognizing that mobility is related to the number of citizens abiding by lockdown measures and can be represented by $1 - \hat{\rho}(\alpha_{it}, d_{it})$. Greater compliance during lockdown leads to lower mobility. Our key mobility variable is therefore $m_{it} = \lambda_{it}\rho_{it}$ which we have shown to be negatively related to media freedom for a given level of reported deaths due to the media effect on both λ_{it} and ρ_{it}.

Additional Empirical Results

Electoral Cycle

Our model suggests that governments facing a higher political cost of lockdown are more reluctant to implement such a policy. One possibility is that political accountability in the form of free and fair elections may push incumbents to respond more strongly to information about the severity of COVID-19. However, we do not know *a priori* how political accountability might affect responsiveness, and look for insights from empirical patterns.

Relatedly, we find differences in responsiveness according to whether countries conduct free and fair elections. As reported in Table A2.6, countries with systems of free and fair elections are more responsive to deaths when deciding to lock down, are see more reductions in mobility associated with a death rise during lock down. Conversely, no such pattern of responsiveness is detected for countries without systems of free and fair elections. This suggests that political costs of lockdown measures are lower in countries with free elections. While incumbents may fear the electoral consequences of locking down, they may be more inclined to decisively respond to the severity of the outbreak if they have more legitimacy for their actions and accountability mechanisms in place to rationalize their decisions. Taken together, these results suggests that political accountability strongly influences the determinants of responsiveness.

TABLE A2.6: RESPONSIVENESS AND FREE AND FAIR ELECTIONS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Panel A: Free and fair elections								
$\widehat{\text{Log deaths}}$	0.198***	-16.51***	-15.95***	-13.57***	-10.49***	-14.16***	6.339***	-14.06***
	(0.0512)	(3.689)	(3.875)	(3.264)	(2.904)	(3.831)	(1.521)	(3.367)
Observations	11,914	11,771	11,771	11,773	11,771	11,773	11,769	11,850
F-stat First Stage	35.80	36.81	36.81	36.80	36.81	36.80	36.83	38.51
Panel B: No free and fair elections								
$\widehat{\text{Log deaths}}$	0.0572	1.627	0.473	0.339	-0.569	1.712	-0.818	0.716
	(0.130)	(7.833)	(8.466)	(6.501)	(4.852)	(6.263)	(3.020)	(6.663)
Observations	9,798	9,416	9,416	9,416	9,416	9,416	9,416	9,416
F-stat First Stage	22.85	18.31	18.31	18.31	18.31	18.31	18.31	18.31
Country fixed effect	X	X	X	X	X	X	X	X
Outbreak time fixed effect	X	X	X	X	X	X	X	X
H_0: Equality of coefficients (*p-value*)	0.42	0.03	0.07	0.05	0.07	0.03	0.03	0.04

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Each regression includes as covariate the log number of global COVID-19 deaths. $\widehat{Deaths}$ indicates the value of log deaths from a first stage regression of log reported deaths using as instrument log simulated deaths based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group. F-stat First stage indicates the Kleibergen-Paap rk Wald F statistic of the excluded instrument in the first stage. The dependent variable for Column 1 is a dummy variable for the implementation of a national lockdown for at least part of the day. Columns 2 to 7 indicate percentage changes in visits and length of stay after lockdown at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020.

Excess mortality

In this section, we replicate our analysis using excess mortality instead of simulated death from a SEIR model as a benchmark for the evolution of the disease.

Excess mortality compares the weekly death counts from all causes to the average of the similar measure over 2015-2019. It has been argued that excess mortality is a more precise measure of the true impact of the pandemic on the number of deaths than what is being reported by countries or estimated by epidemiological models. However, excess mortality requires granular information on the current and historical number of deaths from all causes. To date, this measure is only available in 39 countries[28], including 30 in Europe and with variation in the presence of free media available only among European countries. For these countries, data on excess mortality is available on a weekly basis, and is derived by compared excess deaths from all causes in a week in 2020 compared to the average mortality in the corresponding week over the last 5 years. We use the a sample period of January 1, 2020 - September 6, 2020, the last week for which we have data. Since this period covers more than the Great Lockdown period, we look at the overall mobility trends as opposed to only mobility during lockdown. Data comes from the Human Mortality Database[29], completed with data on deaths by week from Eurostat for European countries, and excess mortality data compiled by the Financial Times for Brazil, Chile, Ecuador, Peru and South Africa.[30] A map of the countries for which excess mortality can be computed is shown in Figure A2.5. As all the censored-media countries with information on excess mortality are from Eastern Europe, we will restrict analysis using excess mortality to the countries from the Europe and Central Asia region.

Table A2.7 shows that under-reporting is systematically more pronounced in censored-media countries. Column 1 shows the significant correlation between reported deaths and excess mortality. In Columns 2, we include media freedom and find significantly higher reporting of deaths in free-media countries. Finally, in columns 3 and 4, we interact media freedom status with weekly excess mortality to detect heterogeneous patterns of reporting over time. We find that censored-media countries significantly under-reporting deaths for each increase in excess mortality (i.e. free-media countries report more deaths per thousand for each additional excess death per thousand, resulting in a positive coefficient for the interaction term). This pattern is also observed when focusing on countries in Europe and Central Asia only for which we have better coverage, as displayed in Column 4.

Table A2.8 explores responsiveness to excess mortality as a proxy for the severity of the disease. We focus only on countries from Europe and Central Asia as the coverage of excess mortality data gives us variation of media freedom in that region only. We find evidence of higher responsiveness during lockdowns on the part of citizens in countries with free media. In those countries, citizens further reduce their time outside in retails and recreation, parks and at groceries and pharmacies in response to higher excess mortality. Using the overall measure of time spent outside, we also find that only citizens in free-media countries reduce their time outdoor following an increase in deaths. We control for country fixed effects and week fixed effects to account for fixed country and time differences in our panel. We also find that an increase in excess mortality is associated with a greater increase in time spend in residential areas in free-media countries compared to censored-media countries, consistent with citizens in free-media countries complying more with lockdown measures. Although we see a greater probability of imposing a lockdown in response to excess death in free-media countries, this difference is not statistically significant.

[28] See Figure A2.5 for a map showing these countries. We are not using data on excess mortality when it is not available at the national level.

[29] https://www.mortality.org/

[30] https://github.com/Financial-Times/coronavirus-excess-mortality-data/

In sum, our results on excess mortality confirm our findings that free-media countries are more truthful about death reporting and more responsive to deaths when locking down for the period of January 1 - September 1, 2020.[31] Albeit data limitations force us to restrict our analysis to European countries, these findings are reinforcing our results found earlier on reporting and responsiveness.

TABLE A2.7: EXCESS MORTALITY AND REPORTING

	(1)	(2)	(3)	(4)
	Dependent variable: New deaths per thousand			
Excess deaths per thousand	0.00911***	0.00904***	0.000899	0.00180*
	(0.00123)	(0.00123)	(0.000934)	(0.000973)
Media Freedom		0.00289**		
		(0.00120)		
Media Freedom × Excess deaths per thousand			0.00734***	0.00627***
			(0.00149)	(0.00145)
Country fixed effect			X	X
Week fixed effect			X	X
Europe and Central Asia only				X
N	1,184	1,150	1,150	920

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Unit of observation: country-week. Excess mortality measured as the difference in deaths from all causes in a week compared to the average in the corresponding week between 2015-2019. A free-media country has a score above the median value of 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018.

TABLE A2.8: EXCESS MORTALITY AND RESPONSIVENESS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Excess deaths per thousand	-0.0235	3.521**	1.044	3.808	2.886*	1.631*	-1.790***	2.588**
	(0.0229)	(1.325)	(0.766)	(3.901)	(1.513)	(0.825)	(0.569)	(1.013)
Media Freedom × Excess deaths per thousand	0.0292	-4.230***	-1.320	-3.745	-3.295**	-2.251**	2.045***	-2.978**
	(0.0264)	(1.448)	(0.963)	(4.375)	(1.580)	(0.890)	(0.593)	(1.263)
Country fixed effect	X	X	X	X	X	X	X	X
Week fixed effect	X	X	X	X	X	X	X	X
N	1046	850	850	850	850	850	850	850

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Unit of observation: country-week. Excess mortality measured as the difference in deaths from all causes in a week compared to the average in the corresponding week between 2015-2019. Sample period: January 1 to September 6, 2020 (Weeks 1 to 36). A free-media country has a score above the median value of 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018. The dependent variable for Column 1 is a dummy variable for the implementation of a national lockdown for at least part of the day. Columns 2 to 7 indicate percentage changes in visits and length of stay at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020.

[31] In Table A2.20, we run the same analysis for the sample January-May 1, 2020 to be consistent with our main analysis. Although we are underpowered with a sample size of 648 observations, we find similar direction of the effects but not statistically significant at the 5% level.

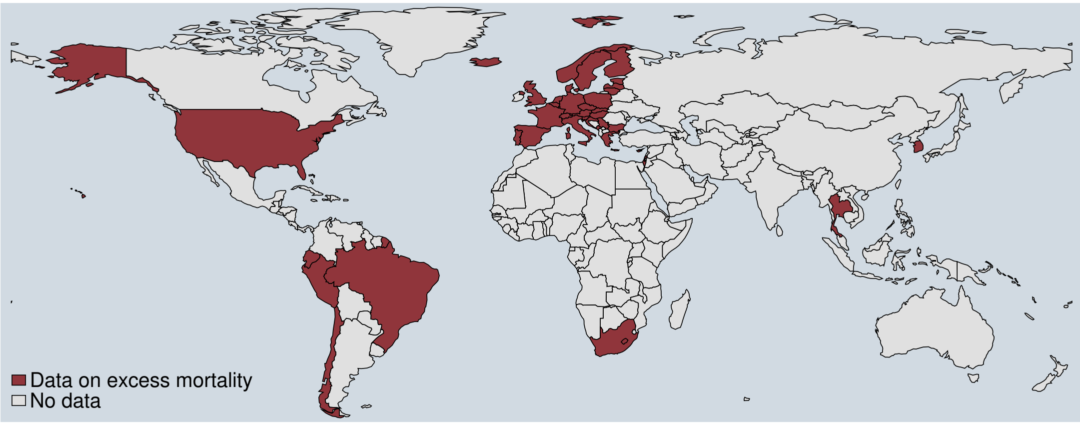


FIGURE A2.5: EXCESS MORTALITY COVERAGE

Source: Human Mortality Database, Eurostat and the Financial Times.

Calibration of the SEIR model

This section provides details on the calibration of the SEIR model used to construct simulated COVID-19 deaths.

The model takes as input the following parameters:

1. **Epidemiological parameters** come from Noll et al. (2020) : annual average reproduction number R_0, latency time, infectious period, seasonal variation in transmission, average duration of hospital stay, average duration of ICU stay, severity of ICU overflow. The value of each parameter is shown in Table A2.9. Most parameters are fixed across all countries, but three are country-specific and fit to observations. This is (i) R_0 (ii) the initial date of the outbreak (iii) the size of the initial outbreak (Noll et al. (2020) p.5). We use this calibration done by epidemiologists by fitting cumulative case counts and fatalities on data prior to any mitigation measures. As pointed by Noll et al. (2020), the simulation is not an inference tool, it relies on minimal fitting of the data to provide reasonable estimates, and does not use information on media freedom or actual mitigation measures of the country (e.g. lockdown) in any part of the simulation.

2. **Population and age distribution** from the UN's World Population Prospects 2019

3. **Healthcare capacity**: Number of hospital beds and number of hospital beds in Intensive Care Units (ICUs). Given the paucity of available recent statistics on hospital bed capacity, we rely on an estimate of hospital beds per 1,000 population by income group from Walker et al. (2020). Estimates from Walker et al. (2020) are modelled using data from the World Bank for hospital beds per 1,000 population, and a systematic review for data on ICU capacity. We use the World Bank classification of countries in income groups as low income, lower middle income, upper middle income or high income. Table A2.10 shows the estimates of healthcare and ICU capacity used in the SEIR model.

4. **Hypothetical mitigation**: We model a hypothetical mitigation scenario for each country reducing social contact rates by 60% and impute a start date using the average time to lockdown by country income group. This corresponds to an enhanced social distancing scenario as discussed by Walker et al. (2020) and Ferguson et al. (2020). See Table A2.10 on the parameter values of these hypothetical mitigation stratified by income group.

We have also considered alternative assumptions for healthcare availability and efficiency of the mitigation scenario as shown in Figure A2.6, which replicates Figure A2.1. Alternative estimates of hospital beds are available from Noll et al. (2020) and lead to a similar pattern of differences between reported and simulated deaths. Second, we consider a 75% efficiency of mitigation instead of a default 60% efficiency. As discussed in Walker et al. (2020) and Ferguson et al. (2020), this corresponds to a suppression scenario aimed at rapidly reducing transmission. Using this level of mitigation efficiency leads to a lower simulated fatality, although the gap in reported vs simulated deaths remains present and large in relative terms for censored-media countries. These alternative assumptions have little bearing on the results of the analysis in Section 2.2.

TABLE A2.9: SEIR MODEL PARAMETERS: EPIDEMIOLOGY

Parameter	Value	Source
R_0 (country est.)	2.51 (1.22)	Noll et al. (2020)
Infectious Period (days)	3	Noll et al. (2020)
Latency (days)	3	Noll et al. (2020)
Hospital Stay (days)	3	Noll et al. (2020)
ICU Stay (days)	14	Noll et al. (2020)
Severity of ICU Overflow	2	Noll et al. (2020)
Seasonal Peak	January	Noll et al. (2020)
Seasonal Forcing	0	Noll et al. (2020)
Outbreak Start (country est.)	February 20 (21)	Noll et al. (2020)

Notes: R_0 is the basic reproduction number. Seasonal Peak refers to the month of the year with peak transmission. Seasonal Forcing is the amplitude of seasonal variation in transmission. Latency is the time from infection to onset of infection. Infectious Period refers to the average number of days that a person is infectious. Hospital stay is the average number of days a severe case stays in a regular hospital bed. ICU stay refers to the average stay in Intensive Care Unit (ICU) for critical cases. Severity of ICU overflow is a multiplicative factor to death rate to patients that require but do not have access to an ICU bed relative to those who do. Outbreak start refers to the date of the epidemic in each country. Both R_0 and Outbreak Start are country-specific estimates from Noll et al. (2020). All other parameters are fixed for all countries.

TABLE A2.10: SEIR MODEL PARAMETERS: HEALTHCARE AND MITIGATION

	World Bank Income Group				
Parameter	Low income	Lower middle income	Upper middle income	High income	Source
Hospital Beds / 1,000	1.24	2.08	3.41	4.82	Walker et al. (2020)
ICU Beds /100 Beds	1.63	2.38	3.32	3.57	Walker et al. (2020)
Mitigation Efficiency	60	60	60	60	Walker et al. (2020)
Trigger for Mitigation (days since outbreak)	28	28	36	39	Average time of lockdown

Notes: Each parameter is assigned to a country based on its World Bank Income Group. Estimates of Hospital Bed and ICU Capacity are taken from Walker et al. (2020) who use data from the World Bank for hospital beds, and data from a systematic review for ICU capacity. Mitigation efficiency refers to the percentage reduction in social contact rates. A 60% reduction in social contact rates corresponds to enhanced social distancing as discussed in Walker et al. (2020). Trigger for mitigation refers to the time relative to the start of the outbreak from which simulated mitigation measures are in place.

TABLE A2.11: SEIR MODEL PARAMETERS: EPIDEMIOLOGY BY MEDIA FREEDOM STATUS

Parameter	Censored-media country	Free-media country	Source
R_0 (country est.)	2.33 (1.20)	2.79 (1.28)	Noll et al. (2020)
Infectious Period (days)	3	3	Noll et al. (2020)
Latency (days)	3	3	Noll et al. (2020)
Hospital Stay (days)	3	3	Noll et al. (2020)
ICU Stay (days)	14	14	Noll et al. (2020)
Severity of ICU Overflow	2	2	Noll et al. (2020)
Seasonal Peak	January	January	Noll et al. (2020)
Seasonal Forcing	0	0	Noll et al. (2020)
Outbreak Start (country est.)	February 23 (18)	February 18 (21)	Noll et al. (2020)

Notes: R_0 is the basic reproduction number. Seasonal Peak refers to the month of the year with peak transmission. Seasonal Forcing is the amplitude of seasonal variation in transmission. Latency is the time from infection to onset of infection. Infectious Period refers to the average number of days that a person is infectious. Hospital stay is the average number of days a severe case stays in a regular hospital bed. ICU stay refers to the average stay in Intensive Care Unit (ICU) for critical cases. Severity of ICU overflow is a multiplicative factor to death rate to patients that require but do not have access to an ICU bed relative to those who do. Outbreak start refers to the date of the epidemic in each country. Both R_0 and Outbreak Start are country-specific estimates from Noll et al. (2020). All other parameters are fixed for all countries.

TABLE A2.12: SEIR MODEL PARAMETERS: HEALTHCARE AND MITIGATION BY MEDIA FREEDOM STATUS

Parameter	Censored-media country	Free-media country	Source
Hospital Beds / 1,000	2.69	3.53	Walker et al. (2020)
ICU Beds /100 Beds	2.68	3.05	Walker et al. (2020)
Mitigation Efficiency	60	60	Walker et al. (2020)
Trigger for Mitigation (days since outbreak)	32	35	Average time of lockdown

Notes: Each parameter is assigned to a country based on its World Bank Income Group. Estimates of Hospital Bed and ICU Capacity are taken from Walker et al. (2020) who use data from the World Bank for hospital beds, and data from a systematic review for ICU capacity. Mitigation efficiency refers to the percentage reduction in social contact rates. A 60% reduction in social contact rates corresponds to enhanced social distancing as discussed in Walker et al. (2020). Trigger for mitigation refers to the time relative to the start of the outbreak from which simulated mitigation measures are in place.

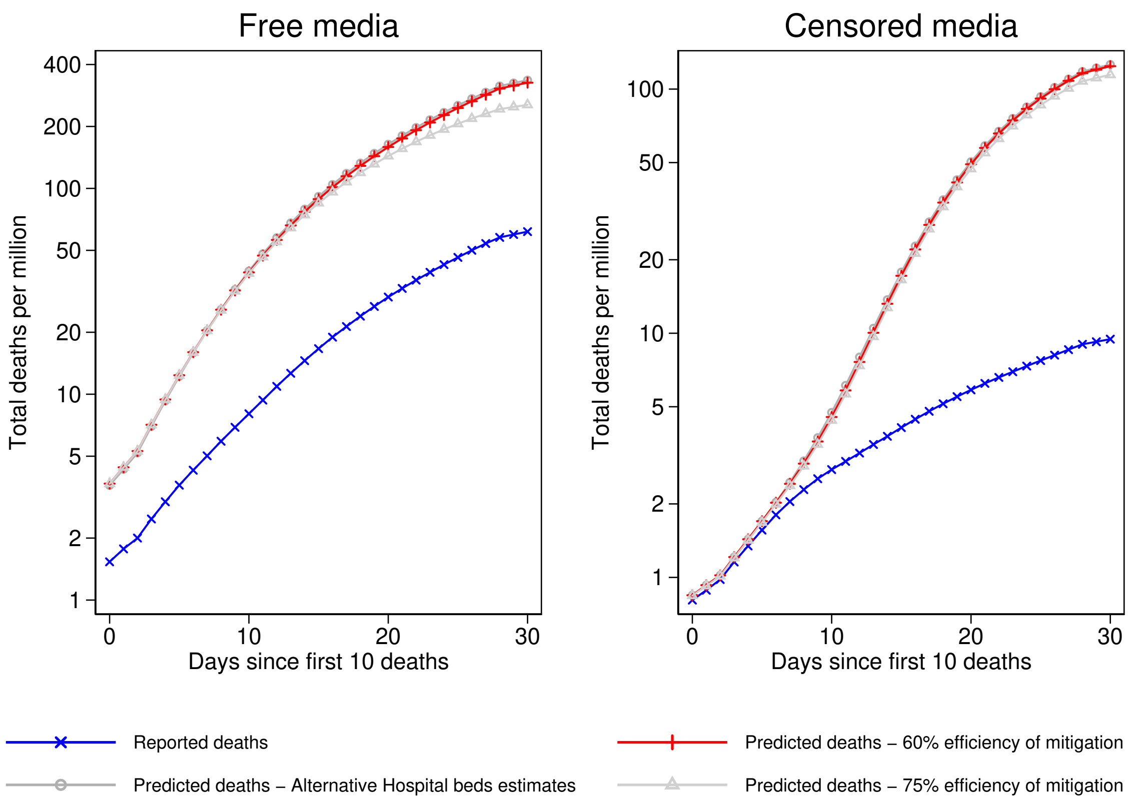


FIGURE A2.6: CALIBRATION OF SEIR MODEL

Notes: Simulated deaths is based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group. Preferred calibration is indicated as "Simulated deaths" in the legend. "Alternative Hospital beds estimates" uses estimates of hospital beds and ICU capacity from Noll et al. (2020) instead of Walker et al. (2020). "60% efficiency of mitigation" is our default calibration for mitigation efficiency and corresponds to enhanced social distancing measures. "75% efficiency of mitigation" models a 75% reduction of social contact rates corresponding to a suppression scenario discussed in Ferguson et al. (2020) and Walker et al. (2020). All series are average values from a balanced panel of countries who reported COVID-19 deaths for at least 30 days after reporting their first 10 confirmed COVID-19 deaths. Values are shown as 5-day moving averages.The y-axis uses a log scale.

Robustness results

This section presents robustness of the main results using different definitions of media freedom, responsiveness to other COVID-19 statistics, and alternative specifications.

First, we use alternative measures of media freedom provided by either the Freedom House or a measure of press freedom from the RSF World Press Freedom index. As shown in Table A2.13, we replicate our result of only citizens in free-media countries being responsive. Similar to our main Table 2.5, we find a larger responsiveness of free-media government for their lockdown decision, but the difference with censored-media governments is not statistically significant at the 5% level.

Second, we replicate our analysis but looking at responsiveness to either total cases or deaths over the last 7 days (the main analysis reports responsiveness estimates to total deaths). As shown in Tables A2.14 and A2.15 respectively, we find very similar estimates of responsiveness. Table A2.15 shows even stronger differences in responsiveness between free- and censored-media countries when looking at recent deaths instead of total deaths, and reports a significant difference in government responsiveness. Table A2.16 is the analogue of Table 2.6 and similarly shows that differences in responsiveness between free-media and censored-media countries cannot be attributed to other country characteristics such as income, the presence of democracy, or education.

Third, we test for alternative specifications in Tables A2.17 to A2.20. Specifically, compared to our core specification, in Tables A2.17, A2.18 and A2.19, we removed the fixed effects from the interaction between outbreak time (time since first 10 cumulative deaths) and media freedom status. Effectively, this forces the date of the country outbreak to have no impact on lockdown decision or mobility during lockdown by imposing a uniform coefficient of outbreak time for all countries despite outbreaks occurring at significantly later dates in censored-media countries (close to 13 days later, see Table A2.1). In Table A2.17, we replicate our findings from Table 2.1 that governments and citizens are responsive to COVID-19 deaths in an OLS specification. However, in that alternative specification, we fail to replicate our results from Table 2.2 and find no evidence of statistical difference in responsiveness between free- and censored-media countries using the IV approach. As explained above, this lack of difference can be explained by censored-media countries having access to more information as they were hit by the pandemic at a later date. Our preferred specification is one that accounts for such differences in outbreak characteristics between countries. In the main text, we discussed results using either an interaction between outbreak timing and media freedom status or a subsample approach that more flexibly accounts for timing differences. In both cases, we find evidence of significant differences in responsiveness between free- and censored-media countries. Finally, Table A2.20 reports our underpowered results from excess mortality if we restrict the sample period to be during the Great Lockdown period (until May 1) to be consistent with our main analysis. Given that excess mortality is only available for less than 40 countries and on a weekly basis rather than daily, we fail to find any significant point estimates.

TABLE A2.13: RESPONSIVENESS AND ALTERNATIVE MEASURES OF MEDIA FREEDOM

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Panel A: Free-media countries (Freedom House)								
$\widehat{\text{Log deaths}}$	0.167***	-14.49***	-13.84***	-11.88***	-9.459***	-12.80***	5.499***	-12.53***
	(0.0520)	(3.985)	(4.202)	(3.433)	(2.983)	(4.007)	(1.693)	(3.575)
Observations	15,283	14,894	14,894	14,896	14,894	14,896	14,885	15,065
F-stat First Stage	38.03	37.55	37.55	37.54	37.55	37.54	37.57	39.43
Panel B: Censored-media countries (Freedom House)								
$\widehat{\text{Log deaths}}$	0.0215	1.183	1.037	0.199	0.0267	2.380	-0.679	0.965
	(0.136)	(7.880)	(8.254)	(6.190)	(4.303)	(6.187)	(2.758)	(6.469)
Observations	6,891	6,708	6,708	6,708	6,708	6,708	6,708	6,708
F-stat First Stage	25.77	22.36	22.36	22.36	22.36	22.36	22.36	22.36
Country fixed effect	X	X	X	X	X	X	X	X
Outbreak time fixed effect	X	X	X	X	X	X	X	X
H_0: Equality of coefficients (*p-value*)	0.44	0.08	0.11	0.09	0.07	0.04	0.06	0.06
Panel C: Free press countries (Press Freedom index)								
$\widehat{\text{Log deaths}}$	0.184***	-15.93***	-15.48***	-13.10***	-10.65***	-14.19***	6.012***	-13.89***
	(0.0530)	(4.141)	(4.410)	(3.586)	(3.106)	(4.214)	(1.793)	(3.739)
Observations	13,505	13,094	13,094	13,096	13,094	13,096	13,085	13,250
F-stat First Stage	36.38	35.88	35.88	35.87	35.88	35.87	35.91	37.74
Panel D: Censored press countries (Press Freedom index)								
$\widehat{\text{Log deaths}}$	0.0107	0.979	0.799	0.000488	-0.148	1.173	-0.233	0.561
	(0.122)	(7.576)	(7.785)	(6.060)	(4.552)	(5.869)	(2.758)	(6.272)
Observations	8,356	8,195	8,195	8,195	8,195	8,195	8,195	8,195
F-stat First Stage	31.21	28.23	28.23	28.23	28.23	28.23	28.23	28.23
Country fixed effect	X	X	X	X	X	X	X	X
Outbreak time fixed effect	X	X	X	X	X	X	X	X
H_0: Equality of coefficients (*p-value*)	0.15	0.04	0.05	0.05	0.04	0.03	0.04	0.04

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Each regression includes as covariate the log number of global COVID-19 deaths. $\widehat{Deaths}$ indicates the value of log deaths from a first stage regression of log reported deaths using as instrument log simulated deaths based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group. Media freedom (Freedom House) indicates a country with a value above 50 out of 100 on the Media Freedom index from the Freedom House project. Press freedom indicates a country with a score above 50 out of 100 on the RSF World Press Freedom index. F-stat First stage indicates the Kleibergen-Paap rk Wald F statistic of the excluded instrument in the first stage. The dependent variable for Column 1 is a dummy variable for the implementation of a national lockdown for at least part of the day. Columns 2 to 7 indicate percentage changes in visits and length of stay after lockdown at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020.

TABLE A2.14: RESPONSIVENESS TO CASES AND MEDIA FREEDOM

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Panel A: Free-media countries								
$\widehat{\text{Log cases}}$	0.0900**	-9.637***	-9.344***	-7.915***	-6.296***	-7.500***	3.515***	-8.115***
	(0.0377)	(2.315)	(2.420)	(1.950)	(1.777)	(2.318)	(0.992)	(2.099)
Observations	11,822	11,547	11,547	11,547	11,547	11,547	11,545	11,624
F-stat First Stage	61.73	58.10	58.10	58.10	58.10	58.10	58.07	58.76
Panel B: Censored-media countries								
$\widehat{\text{Log cases}}$	0.0294	-3.455	-3.471	-3.082	-2.234	-2.514	0.811	-2.953
	(0.0461)	(2.688)	(2.929)	(2.225)	(1.731)	(2.024)	(1.051)	(2.289)
Observations	11,727	11,327	11,327	11,329	11,327	11,329	11,327	11,329
F-stat First Stage	39.18	36.33	36.33	36.39	36.33	36.39	36.33	36.39
Country fixed effect	X	X	X	X	X	X	X	X
Outbreak time fixed effect	X	X	X	X	X	X	X	X
H_0: Equality of coefficients (*p-value*)	0.27	0.07	0.11	0.09	0.08	0.09	0.05	0.08

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Each regression includes as covariate the log number of global COVID-19 cases. $\widehat{Cases}$ indicates the value of log cases from a first stage regression of log reported cases using as instrument log simulated cases based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group. F-stat First stage indicates the Kleibergen-Paap rk Wald F statistic of the excluded instrument in the first stage. The dependent variable for Column 1 is a dummy variable for the implementation of a national lockdown for at least part of the day. Columns 2 to 7 indicate percentage changes in visits and length of stay after lockdown at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020.

TABLE A2.15: RESPONSIVENESS TO DEATHS OVER LAST 7 DAYS AND MEDIA FREEDOM

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Panel A: Free-media countries								
$\widehat{\text{Log deaths last 7 days}}$	0.230**	-22.23***	-22.08***	-18.00***	-13.90**	-19.98***	8.017***	-19.12***
	(0.0903)	(7.302)	(7.850)	(6.230)	(5.501)	(7.156)	(3.031)	(6.539)
Observations	11,288	11,040	11,040	11,040	11,040	11,040	11,038	11,117
F-stat First Stage	11.79	10.68	10.68	10.68	10.68	10.68	10.69	11.38
Panel B: Censored-media countries								
$\widehat{\text{Log deaths last 7 days}}$	-0.134	8.788	9.311	5.652	4.697	7.320	-3.638	7.153
	(0.108)	(7.386)	(7.350)	(5.941)	(3.862)	(5.766)	(2.757)	(5.958)
Observations	10,277	10,000	10,000	10,002	10,000	10,002	10,000	10,002
F-stat First Stage	25.84	24.61	24.61	24.61	24.61	24.61	24.61	24.61
Country fixed effect	X	X	X	X	X	X	X	X
Outbreak time fixed effect	X	X	X	X	X	X	X	X
H_0: Equality of coefficients (*p-value*)	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Death count is a moving average over the last 7 days. Each regression includes as covariate the log number of global COVID-19 deaths. $\widehat{Cases}$ indicates the value of log deaths from a first stage regression of log reported deaths using as instrument log simulated deaths based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group. F-stat First stage indicates the Kleibergen-Paap rk Wald F statistic of the excluded instrument in the first stage. The dependent variable for Column 1 is a dummy variable for the implementation of a national lockdown for at least part of the day. Columns 2 to 7 indicate percentage changes in visits and length of stay after lockdown at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020.

TABLE A2.16: HETEROGENEITY IN RESPONSIVENESS TO DEATHS OVER LAST 7 DAYS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Media Freedom	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]	[<0.01]
Free and fair elections	[0.03]	[<0.01]	[0.01]	[0.01]	[0.02]	[<0.01]	[<0.01]	[<0.01]
Election in 2020	[0.12]	[0.02]	[0.07]	[0.03]	[0.08]	[0.01]	[0.09]	[0.05]
Most people can be trusted	[0.20]	[0.15]	[0.14]	[0.12]	[0.14]	[0.41]	[0.19]	[0.16]
Willingness to fight for country	[0.18]	[0.05]	[0.08]	[0.05]	[0.03]	[0.08]	[0.07]	[0.05]
Democracy	[0.24]	[0.10]	[0.14]	[0.19]	[0.31]	[0.08]	[0.20]	[0.13]
Executive constraints	[0.92]	[0.81]	[0.88]	[0.93]	[0.90]	[0.89]	[0.89]	[0.90]
Log GDP per capita > median	[0.51]	[0.50]	[0.74]	[0.50]	[0.84]	[0.96]	[0.89]	[0.69]
Education > median	[0.50]	[0.75]	[0.97]	[0.99]	[0.75]	[0.83]	[0.93]	[0.98]
Global Health Security index	[0.91]	[0.98]	[0.84]	[0.88]	[0.61]	[0.76]	[0.63]	[0.81]
Tax revenue > median	[0.50]	[0.47]	[0.62]	[0.58]	[0.91]	[0.68]	[0.44]	[0.67]
Access to handwashing facilities	[0.74]	[0.99]	[0.65]	[0.95]	[0.60]	[0.36]	[0.57]	[0.72]
Income inequality	[0.54]	[0.39]	[0.49]	[0.38]	[0.75]	[0.96]	[0.33]	[0.56]
Social protection	[0.88]	[0.87]	[0.62]	[0.80]	[0.77]	[0.55]	[0.83]	[0.72]

Notes: Each cell is the p-value from a separate IV regression based on Equation (2.2). The p-value tests the null hypothesis of equality of the responsiveness to death coefficient for two subsamples. Death count is a moving average over the last 7 days. See Appendix Section 2.4 for definition of subsamples.

TABLE A2.17: RESPONSIVENESS AND MEDIA FREEDOM: ALTERNATIVE SPECIFICATION, OLS, NO INTERACTED FE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Log deaths	0.152***	-10.16***	-10.05***	-8.239***	-5.874***	-7.902***	3.544***	-8.461***
	(0.0338)	(2.356)	(2.416)	(1.910)	(1.507)	(2.030)	(0.871)	(1.948)
Log deaths × Media Freedom	-0.0325	0.977	1.324	0.650	0.585	1.287	-0.131	0.980
	(0.0218)	(1.575)	(1.548)	(1.263)	(0.990)	(1.323)	(0.581)	(1.306)
Observations	23,570	22,895	22,895	22,897	22,895	22,897	22,893	22,974
Country FE	X	X	X	X	X	X	X	X
Outbreak time FE	X	X	X	X	X	X	X	X

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Reported and simulated deaths in log. Outbreak time indicates the days since a country reported its first 10 COVID-19 deaths. Simulated deaths represent simulated fatalities based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group.

TABLE A2.18: RESPONSIVENESS AND MEDIA FREEDOM: ALTERNATIVE SPECIFICATION, NO INTERACTED FE

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
$\widehat{\text{Log deaths}}$	0.170**	-11.66**	-11.73**	-9.800**	-7.829**	-10.26**	3.951*	-10.26**
	(0.0770)	(5.449)	(5.603)	(4.457)	(3.556)	(4.726)	(2.010)	(4.679)
$\widehat{\text{Log deaths}}$ × Media Freedom	-0.0226	0.321	0.653	0.193	0.264	0.717	0.210	0.461
	(0.0282)	(2.001)	(1.993)	(1.617)	(1.256)	(1.635)	(0.692)	(1.677)
Observations	21,734	21,209	21,209	21,211	21,209	21,211	21,207	21,288
F-stat First Stage	32.78	31.50	31.50	31.50	31.50	31.50	31.51	31.74
F-statistic death	79.81	74.08	74.08	74.07	74.08	74.07	74.09	72.90
F-statistic death x Media Freedom	225.99	175.07	175.07	175.04	175.07	175.04	175.11	166.64
Country FE	X	X	X	X	X	X	X	X
Outbreak time FE	X	X	X	X	X	X	X	X

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Reported and simulated deaths in log. Outbreak time indicates the days since a country reported its first 10 COVID-19 deaths. Simulated deaths represent simulated fatalities based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group.

TABLE A2.19: RESPONSIVENESS AND MEDIA FREEDOM: ALTERNATIVE SPECIFICATION, REDUCED FORM AND FIRST STAGE, NO INTERACTED FE

	(1)	(2)	(3)
	Time Outside	Log deaths	Log deaths × Media Freedom
Log SEIR deaths	-3.134*	0.291***	-0.325***
	(1.853)	(0.0448)	(0.0743)
Log SEIR deaths × Media Freedom	-0.130	0.0515	0.866***
	(1.325)	(0.0339)	(0.0417)
Observations	21,288	21,288	21,288
F-stat First Stage		31.74	31.74
F-statistic death		72.90	
F-statistic death × Media Freedom			166.64
Country FE	X	X	X
Outbreak time FE	X	X	X

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Reported and simulated deaths in log. Outbreak time indicates the days since a country reported its first 10 COVID-19 deaths. Simulated deaths represent simulated fatalities based on a SEIR model with several levels of infectious severity, age-specific transition rates, epidemiological parameters, and average mitigation measures and healthcare availability by income group.

TABLE A2.20: EXCESS MORTALITY AND RESPONSIVENESS - GREAT LOCKDOWN PERIOD

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lockdown decision	Retails and recreation	Public Transport	Workplaces	Groceries and Pharmacies	Parks	Residentials	Time Outside
Excess deaths per thousand	0.00685	1.338	0.754	1.156	0.435	2.036	-0.851	1.182
	(0.0395)	(2.240)	(2.142)	(2.050)	(1.371)	(2.425)	(0.867)	(1.694)
Media Freedom × Excess deaths per thousand	-0.00212	-3.157	-2.344	-2.417	-1.552	-4.045	1.379	-2.741
	(0.0425)	(2.460)	(2.321)	(2.147)	(1.410)	(2.590)	(0.876)	(1.831)
Country fixed effect	X	X	X	X	X	X	X	X
Week fixed effect	X	X	X	X	X	X	X	X
N	657	648	648	648	648	648	648	660

Notes: Clustered standard errors at the country-level in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Unit of observation: country-week. Excess mortality measured as the difference in deaths from all causes in a week compared to the average in the corresponding week between 2015-2019. Sample period: January 1 - May 3, 2020 (Weeks 1 to 18). A free-media country has a score above the median value of 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018. The dependent variable for Column 1 is a dummy variable for the implementation of a national lockdown for at least part of the day. Columns 2 to 7 indicate percentage changes in visits and length of stay after lockdown at different places compared to a median value, for the corresponding day of the week, during the 5-week period Jan 3–Feb 6, 2020.

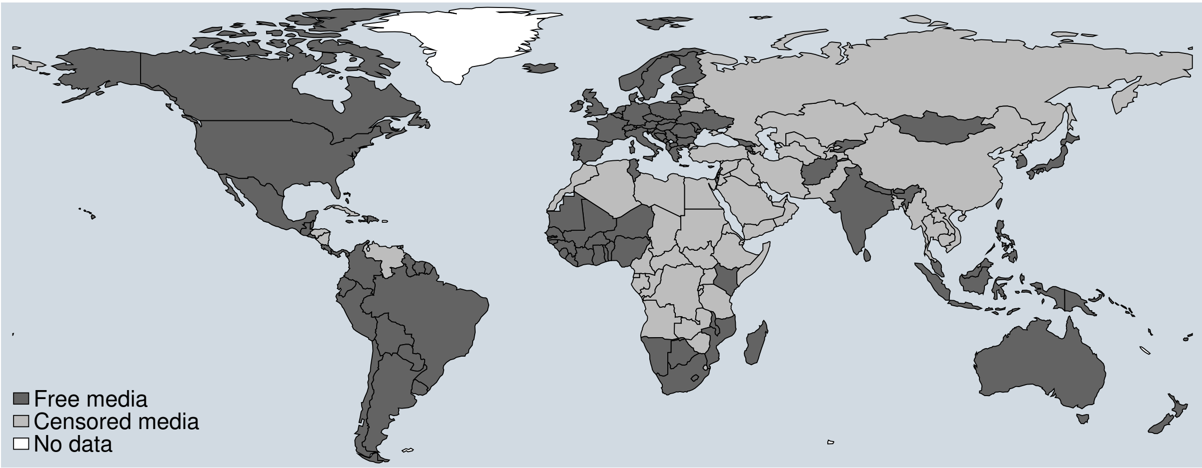


FIGURE A2.7: FREE-MEDIA AND CENSORED-MEDIA COUNTRIES (FREEDOM HOUSE)

Notes: Free media indicates a country with a value above above or equal to 50 out of 100 on the Freedom House media freedom index in 2020. **Free-media countries**: Afghanistan Albania Andorra Argentina Armenia Australia Austria Bahamas Belgium Belize Benin Bolivia Bosnia and Herzegovina Brazil Bulgaria Burkina Faso Canada Cape Verde Chile Colombia Costa Rica Croatia Cyprus Czech Republic Côte d'Ivoire Denmark Dominican Republic Ecuador El Salvador Estonia Finland France Gambia Georgia Germany Ghana Greece Guatemala Guinea Guinea-Bissau Guyana Haiti Hungary Iceland India Indonesia Ireland Israel Italy Jamaica Japan Kenya Kosovo Kyrgyzstan Latvia Lebanon Lesotho Liberia Lithuania Luxembourg Macedonia Madagascar Malawi Malaysia Mali Malta Mauritania Mauritius Mexico Moldova Montenegro Mozambique Namibia Nepal Netherlands New Zealand Niger Nigeria Norway Panama Paraguay Peru Philippines Poland Portugal Romania San Marino Sao Tome and Principe Senegal Serbia Sierra Leone Singapore Slovakia Slovenia South Africa South Korea Spain Sri Lanka Suriname Sweden Switzerland Togo Trinidad and Tobago Tunisia Ukraine United Kingdom United States Uruguay. **Censored-media countries**: Algeria Angola Azerbaijan Bahrain Bangladesh Belarus Cameroon Central African Republic Chad China Congo Cuba DRC Djibouti Egypt Equatorial Guinea Ethiopia Gabon Honduras Iran Iraq Jordan Kazakhstan Kuwait Libya Maldives Morocco Nicaragua Oman Pakistan Qatar Russia Rwanda Saudi Arabia Somalia South Sudan Sudan Swaziland Syria Tajikistan Tanzania Thailand Turkey Uganda United Arab Emirates Uzbekistan Venezuela Vietnam Yemen Zambia Zimbabwe. Countries not in sample are those with less than 10 reported COVID-19 deaths or not included in the Freedom House media freedom index.

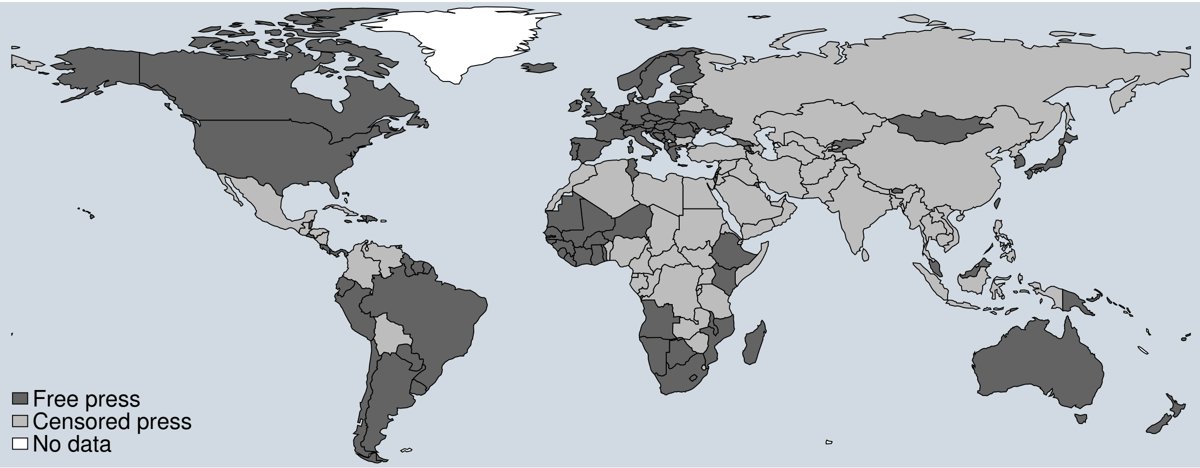


FIGURE A2.8: FREE-PRESS AND CENSORED-PRESS COUNTRIES

Notes: Free press indicates a country with a value above above or equal to 50 out of 100 on the World Press Freedom index in 2020. **Free-press countries**: Albania Andorra Angola Argentina Armenia Australia Austria Belgium Belize Bosnia and Herzegovina Brazil Burkina Faso Canada Cape Verde Chile Costa Rica Croatia Cyprus Czech Republic Côte d'Ivoire Denmark Dominican Republic Ecuador El Salvador Estonia Ethiopia Finland France Gambia Georgia Germany Ghana Greece Guinea Guinea-Bissau Guyana Haiti Hungary Iceland Ireland Israel Italy Jamaica Japan Kenya Kosovo Kuwait Kyrgyzstan Latvia Lebanon Lesotho Liberia Lithuania Luxembourg Macedonia Madagascar Malawi Malaysia Maldives Mali Malta Mauritania Mauritius Moldova Montenegro Mozambique Namibia Netherlands New Zealand Niger Norway Panama Paraguay Peru Poland Portugal Romania Senegal Serbia Sierra Leone Slovakia Slovenia South Africa South Korea Spain Suriname Sweden Switzerland Togo Trinidad and Tobago Tunisia Ukraine United Kingdom United States Uruguay. **Censored-press countries**: Afghanistan Algeria Azerbaijan Bahrain Bangladesh Belarus Benin Bolivia Bulgaria Cameroon Central African Republic Chad China Colombia Congo Cuba DRC Djibouti Egypt Equatorial Guinea Gabon Guatemala Honduras India Indonesia Iran Iraq Jordan Kazakhstan Libya Mexico Morocco Nepal Nicaragua Nigeria Oman Pakistan Palestine Philippines Qatar Russia Rwanda Saudi Arabia Singapore Somalia South Sudan Sri Lanka Sudan Swaziland Syria Tajikistan Tanzania Thailand Turkey Uganda United Arab Emirates Uzbekistan Venezuela Vietnam Yemen Zambia Zimbabwe. Countries not in sample are those with less than 10 reported COVID-19 deaths or not included in the World Press Freedom index.

Subsamples cutoffs

Free-media country indicates a country with a value above the median value 0.7 in the V-Dem Freedom of Expression and Alternative Sources of Information index in 2018.

Free-media country (Freedom House) indicates a country with a value above above or equal to 50 out of 100 on the Freedom House media freedom index in 2020.

Free-press country indicates a country with a value above above or equal to 50 out of 100 on the World Press Freedom index in 2020.

Free and fair elections indicates a country with a value above the median value 0.6 on the V-Dem index for free and fair elections in 2018.

"Most people can be trusted" takes a value of 1 if the average national share of respondents is above the median for the corresponding World Value Survey question between 2000-2014.

"Willingness to fight for country" takes a value of 1 if the average national share of respondents is above the median for the corresponding World Value Survey question between 2000-2014.

Democracy indicates a country with value above the median value 0.8 on the normalised polity2 score from Polity IV Project in 2018.

Executive constraints indicates a country with a value above the median value of 0.65 in the V-Dem Executive constraint index.

Global Health Security index is an assessment of countries health securities and capabilities in 2019 including their ability to prevent, detect and respond to the spread of an epidemic.

High income indicates a country with GDP per capita above the median value.

Tax revenue measures the share of taxes to GDP in 2016 from Besley and Persson (2011).

Access to hand-washing facilities takes the value of 1 for countries above the median share of population with basic hand-washing facilities taken from the 2015 World Development Indicator (WDI).

Income inequality indicates countries with a Gini coefficient above the median from the 2015 WDI.

Social protection indicates a country with a percentage of population participating in social insurance, social safety net, unemployment benefits and active labor market programs above the median, from the 2015 WDI.

Chapter 3

Out of Sight, Out of Mind ? Estimating Attention to Tax Reforms

3.1. Introduction

When tax systems become complex, or when some of its features are not fully salient, taxpayers may not pay attention. The UK Office of Tax Simplification estimated that the tax legislation in the United Kingdom contained 639 numerical thresholds in 2012, including 141 for the sole income tax (OTS (2013)). This complexity can come with significant economic cost of attention : for every dollar of revenue raised by their income tax system, U.S. taxpayers incur around ten cents in private compliance cost (Slemrod (1996*b*)). Yet this apparent cost on society might also be beneficial. Economists have long argued that keeping taxes less visible and salient could help governments raise taxes beyond what an informed citizenry might be willing to accept (Mill, Friedman and Friedman (1871, 1999)). Despite this apparent trade-off between compliance cost and reduced deadweight loss, there has been limited evidence on the impact of lack of salience on behavioral responses.

This paper estimates the extent of inattention that can be attributed to tax changes made non-salient to taxpayers[1] . In 2011, the UK introduced a reform to its income tax schedule that created both salient and nonsalient new kink points. Salient changes were made with the

[1] This paper focuses on measuring inattention to tax schedules. I use the term salience as defined by psychologists Taylor and Thompson (1982): "salience refers to the phenomenon that when one's attention is differentially directed to one portion on the environment rather than to others, the information contained in that portion will receive disproportionate weighing in subsequent judgments".

introduction of a new tax band – the additional tax band – for incomes above £150,00 with a marginal tax rate increased from 40% to 50%. At the same time, the reform introduced a phase-out of tax-free income known as the 'Personal Allowance'. The personal allowance meant that the first £6,475 of income were not taxable. From 2011, the personal allowance is progressively being withdrawn, such that the Personal Allowance goes down by £1 for every £2 of income above £100,000. This effectively created an additional nonsalient tax band of 60% at this threshold.

Building on Saez (2010), I develop a framework to incorporate optimization frictions coming from inattention, and derive three testable predictions to confirm the presence of inattention caused by salience effects and characterize its form. Using the variation in salience at kink points, I validate the predictions that (1) the elasticity of taxable income is smaller at non-salient kink points than at salient kink points (2) the elasticity of taxable income at nonsalient kink points increases with the level of attention as the nonsalient tax changes are more publicized. I find no evidence of bunching at misperceived kink points, which would be a particular form of misguided attention where individual incorrectly calculate their taxable income. Overall, I find evidence of significant levels of inattention to the non-salient withdrawal of the personal allowance, resulting in lower implied elasticity than those estimated at salient income thresholds. I also find limited heterogeneity in tax misperception due to salience.

This paper contributes to the literature in the following way. First, I contribute to the literature on tax misperception by providing empirical evidence of salience on income taxation. To the best of my knowledge, this is among the first studies to measure the salience effect on behavioral responses to the income tax, and measure the evolution of inattention over time. [2] Second, this paper contributes to the literature on estimating taxable income response by investigating the role of salience in optimization frictions. Third, this paper fits in the literature using sufficient statistics by providing an estimable parameter to account for behavioral agents in welfare analysis of taxation.

The rest of the paper is organized as follows. In section 3.2 I briefly summarize the related lit-

[2]A few studies have measured salience. For instance, Chetty, Looney and Kroft (2009) and Taubinsky and Rees-Jones (2017) have measured how consumers under-react to non-salient sales taxes in the United States, and Finkelstein (2009) analyzed whether the introduction of electronic toll collection made drivers less aware of toll fees. More recently, Lardeux (2022) showed evidence that some French taxpayers misperceive the tax schedule and can react to irrelevant tax thresholds. More generally, this paper is related to studies of how people understand the tax system (Stantcheva (2021)).

erature. In section 3.3 I provide an empirical framework. In section 3.4, I provide a description of the data and institution background. In section 3.5 I present the preliminary results. Section 3.6 concludes and describes the next steps.

3.2. Literature review

Taxable income elasticity The modern public finance literature has focused on the elasticity of taxable income (ETI) instead of forms of labor supply elasticity such as hours or participation elasticity to calculate the excess burden of income taxation. Feldstein, Feldstein (1995, 1999) has first argued that this approach captures all the channels through which income taxation might affect behavior (such as hours worked, effort, occupational choice). Second, this method has been widely popular thanks to the recent availability of taxable income in administrative data. This large and growing literature has been reviewed by Saez, Slemrod and Giertz (2012), noting that ETI estimates range from 0.12 - 0.40, which is higher than labor supply elasticity. There is also evidence of higher ETI estimates from high-income individuals and self-employed who have more access to evasion and avoidance opportunities.

A popular method to measure the ETI is to use the bunching estimation developed by Saez (2010), then further refined by Chetty et al. (2011) and Kleven and Waseem (2013). Kleven (2016) provides a review of the bunching literature and notes that "the observed amount of bunching is very small (or zero) in elasticity terms" in context with limited evasion and avoidance opportunities (where bunching would be coming from real earning responses). On the contrary, observed bunching can be large in elasticity terms when there is scope for evasion and avoidance. Blomquist et al. (2021) argues that further assumptions on the preference distribution are needed for bunching to identify the elasticity of taxable income.

Anatomy of behavioral responses A strand of the literature has focused on understanding the reasons for small observed ETI in most context. For instance, Saez (2010) finds large bunching at the first kink of the EITC schedule for self-employed but no bunching for wage-earners. The absence of bunching would imply an observed elasticity that is small or close to zero. The possible explanations for limited bunching that have been advanced include small structural elasticity, noise in the income generation process that prevents taxpayers from choosing their earnings with precision (Saez (2010)), optimization frictions (adjustment cost, inattention, etc) and the absence of evasion/avoidance opportunity. Slemrod, Slemrod (1990, 1995) proposed a three-tier hierarchy of behavioral response to taxation. Ranked from least to most responsive were real responses, avoidance responses, and timing responses. Evidence of shifting is provided by Slemrod, Goolsbee (1996*a*, 2000). Evidence of evasion/avoidance is provided for instance by Kleven et al. (2011).

Optimization frictions The literature has focused recently on optimization frictions. Direct evidence of frictions has been found by Ito (2014). Chetty et al. (2011) use a labor supply model with search costs and hours constraints and provide evidence of frictions due to adjustment costs. Gelber, Jones and Sacks (2013) provide evidence of frictions coming from adjustment cost as individuals continue to bunch at a kink even after not being subject to this change in tax rate. Frictions coming from knowledge of the tax schedule have also been used to identify the impact of the EITC on the earning distribution (Chetty, Friedman and Saez (2013)). Chetty (2012) shows that accounting for frictions can reconcile elasticity estimates from micro and macro studies.

ETI as a policy parameter Slemrod and Kopczuk (2002) have argued that the ETI is not a structural parameter as the behavioral response to taxation also depends on other policy parameters. In particular, the ETI is endogenous to the size of the tax base and the enforcement regime. Fack and Landais (2016) provide evidence of such endogeneity in the context of enforcement and charitable contribution in France. They exploit a natural experiment that tightened the requirements to claim charitable deductions. The reform led to a substantial drop in the amount of contributions reported to the administration, which is attributed to previous overreporting of charitable contributions rather than to a real change in giving behaviours. Further, the reform was also associated with a substantial decline in the absolute value of the elasticity of reported contributions, which shows that not accounting for other policy decisions can lead to significantly different ETI and policy conclusions. Finkelstein (2009) study the anatomy of behavioral response focusing on tax salience. She finds that under the less salient electronic toll collection (ETC), driving becomes less elastic with respect to the toll. This suggests that salience is also a policy parameter in this context.

Tax salience and inattention The role of salience on behavioral response to taxation has been studied by the literature. Early contributions focused on individual confusion about the tax schedule, and provided evidence of 'ironing', i.e. confusing the marginal and average tax rate (De Bartolome, Liebman and Zeckhauser, Feldman and Katuščák (1995, 2004, 2006)). There is also evidence of underreaction to nonsalient sales tax (Chetty, Looney and Kroft, Taubinsky and Rees-Jones (2009, 2017)), misperception between the marginal tax rate and lump-sum changes to tax liabilities (Feldman, Katuščák and Kawano (2016)), and of taxpayer search for information driven by exogenous shock to tax salience (Hoopes, Reck and Slemrod (2015)). Benzarti (2017) also finds evidence of misperception of the tax schedule using taxpayers' failure to itemize deductions. There is limited existing evidence from this literature on the level of inattention to nonsalient tax changes. Liebman and Zeckhauser (2004) find that 54 percent of U.S. taxpayers are "schmedulers" (inaccurately perceive their income tax schedule) using the introduction of the Child Tax Credit in 1998. De Bartolome (1995) deploys an experiment with MBA students and finds that, even in this population, 44% incorrectly calculate the marginal tax rate on income when the tax schedule does not explicitly provides information on marginal rates.[3]

Evidence from the UK There is limited evidence of ETI for the UK. Tazhitdinova (2015) provides evidence of small amount of bunching at kink points in the income and payroll tax schedule for wage earners but larger behavioral responses for self-employed and proprietors. She interprets the absence of bunching at the lowest earning level of the pension contributions schedule - the level above which individuals are entitled to receive social security benefits without being liable to national insurance contributions - as evidence for lack of knowledge of the pension contribution system. Adam et al. (2021) also study income and social security contributions and similarly find limited bunching for wage-earners but large behavioral responses by self-employed and company owners-managers. HMRC (2007) show that many claimants are not fully aware of the rules determining their tax credits awards, and in particular did not understand how the amount awarded is linked to their circumstances. Finally, a few studies have been interested in the change in tax policy implemented in 2010 used in this paper. Al-

[3]88 out of 125 (70%) study participants incorrectly derived the marginal tax rate and make the wrong choice between a taxable and tax-exempt investment when provided with a table format replicating that of the IRS that indicates taxable income and tax liabilities. In a control experiment with marginal rates explicitly given, 18 out of 69 study participants (26%) still chose the incorrect investment. Taking the difference, it appears that 44% of study participants chose the incorrect investment because they misperceive their marginal tax rate.

munia et al. (2020) use it to estimate the tax-price elasticity of giving. Heady (2017) measures the effect of personal allowance withdrawal on personal pension contributions.

3.3. Framework

Here I present a framework to guide my empirical analysis extending Saez (2010) to account for inattention caused by salience of the tax system. Consider a static model where the utility of agents depends on two goods, consumption c and earnings z. There is heterogeneity in ability n whose distribution is smooth and follows $F(n)$.

The utility function is quasi-linear and isoelastic of the form:

$$u(c, z/n) = c - n.\frac{(z/n)^{1+1/e}}{1+1/e}$$

and the budget constraint is $c = z - T(z) + R$ where $T(z)$ is the tax function and R represents untaxed earnings. This formulation implicitly assumes no income effect.

Baseline. The tax function is the following. Before the reform, it is a linear tax system $T(z) = t_0.z$. Assuming that the ability distribution and the tax system are smooth, agents optimization generates a smooth earnings distribution. We denote the earnings distribution before tax change as $G(z)$. From the first order condition, all agents will choose

$$z = n(1-t_0)^e$$

so $G(z) = Pr(n(1-t_0)^e < z) = F(z/(1-t_0)^e)$.

Reform. Suppose that a convex kink is introduced in the budget set at income level K. Above the earnings threshold z^*, the tax function is $T(z) = t_0.K + t_1(z-K)$. Note however that this change is nonsalient: this increase in the marginal tax rate is not part of the usual tax schedule. It may come from means-tested benefits or tax credits being withdrawn above an income threshold, or from temporary tax changes. Due to inattention or misperception, only a fraction θ of taxpayers correctly perceive this tax change.

For attentive taxpayers, the budget constraint is

$$c = \begin{cases} (1-t_0)z + R & \text{if } z < K \\ (1-t_0)K + (1-t_1)(z-K) + R & \text{if } z \geq K \end{cases}$$

For the inattentive agents, the budget constraint is

$$c = \begin{cases} (1-t_0)z + R & \text{if } z < \tilde{K} \\ (1-t_0)\tilde{K} + (1-\tilde{t}_1)(z-\tilde{K}) + R & \text{if } z \geq \tilde{K} \end{cases}$$

This formulation nests several forms of inattention. Individuals could not be aware of the reform entirely, this is captured by $\tilde{t}_1 = t_0$, which corresponds to a standard form of inattention. Second, there could be misperception of the threshold at this the tax change occur, with $\tilde{K} \neq K$. This corresponds to cases where individuals misperceive changes in their amount of taxable income (as in our empirical context), or miscalculate the level at which their tax credits are reduced.

For attentive taxpayers, the earning choice is given by

$$z^* = \begin{cases} n(1-t_0)^e & \text{if } n < \underline{n} \\ K & \text{if } n \in [\underline{n}, \overline{n}] \\ n(1-t_1)^e & \text{if } n > \overline{n} \end{cases}$$

with $\underline{n} = \frac{K}{(1-t_0)^e}$, $\overline{n} = \frac{K}{(1-t_1)^e}$. Only attentive individuals with $n \in [\underline{n}, \overline{n}]$ will bunch at the nonsalient kink point K. The elasticity of earnings with respect to the marginal net-of-tax rate can be inferred from the highest marginal buncher. This attentive taxpayer has an ability $\overline{n}$ and had income $K(\frac{1-t_0}{1-t_1})^e$ in the baseline scenario. We can recover the (compensated) elasticity using

$$dln(z) = e \cdot dln(1-t) \tag{3.1}$$

Let $B_K = \theta \cdot [F(\overline{n}) - F(\underline{n})]$ denote the fraction of attentive taxpayers who choose $z^* = K$. Let $g(K)$ denote the counterfactual density of hours in the absence of the tax change at the kink point. Under the approximation that the counterfactual income distribution is uniform around the kink, following Saez (2010) one can show that

$$e \simeq \frac{1}{\theta} \cdot \frac{B_K/g(K)}{Kln(\frac{1-t_0}{1-t_1})} = \frac{1}{\theta} \cdot \frac{b}{Kln(\frac{1-t_0}{1-t_1})} \tag{3.2}$$

where $b = B_K/g(K)$ is the excess mass at the kink point: the fraction of individuals who locate at the kink K normalized by counterfactual density. I denote e as the *structural* elasticity. However, the amount of bunching observed at kinks identifies the *observed* elasticity denoted by

$$\hat{e} \simeq \frac{B_K/g(K)}{Kln(\frac{1-t_0}{1-t_1})} = \frac{b}{Kln(\frac{1-t_0}{1-t_1})} \tag{3.3}$$

In the presence of nonsalient tax changes ($\theta \leq 1$), $\hat{e} \leq e$ and bunching does not identify the structural elasticity. This result leads to the first testable prediction:

Prediction 1 *Observed elasticity from bunching is larger at salient compared to nonsalient kink points:* $\hat{e}_{nonsalient} \leq \hat{e}_{salient}$

Proof. From (3.2) and (3.3), $\hat{e}_{nonsalient} = \theta \cdot \hat{e}_{salient}$ with $\theta \leq 1$. ■

Second, we can see directly from equation (3.3) and the expression for bunching that the observed elasticity $\hat{e}$ increases with attention. This could be the case with the accumulation of information over time about nonsalient changes. As $\theta \to 1$, observed and structural elasticity coincide. This leads to the second testable prediction:

Prediction 2 *Observed elasticity at nonsalient kink points increases with the level of attention and converges to e:*

$$\frac{d\hat{e}}{d\theta} > 0 \text{ , and } \lim_{\theta \to 1} \hat{e} = e$$

Proof. From equation (3.3):

$$\frac{d\hat{e}}{d\theta} = \frac{F(\overline{n}) - F(\underline{n})}{Kg(K)ln((1-t_0)/(1-t_1))} > 0$$

From (3.2) and (3.3), it follows directly that $\lim_{\theta \to 1} \hat{e} = e$. ■

Third, let us consider inattention caused by misperceiption of how taxable income is measured (such as miscalculating the amoung of tax-free allowances as in our case). With simple inattention to the tax schedule, none of the inattentive individuals will bunch. However, if there is misperception of the threshold used in the tax change, we should observe bunching at the point $\tilde{K}$. This form of misperception imposes a particular form of inattention (taxpayers are inattentive to the tax schedule, but only for some portions of it), and would not apply generally. This leads to our third testable prediction:

Prediction 3 *If inattentive taxpayers misperceive the threshold of nonsalient tax change, this produces bunching at the perceived threshold $\tilde{K}$:*

$$B_{\tilde{K}} = (1-\theta) \cdot [F(\overline{\tilde{n}}) - F(\underline{\tilde{n}})]$$

with $\underline{\tilde{n}} = \frac{\tilde{K}}{(1-t_0)^e}$, $\overline{\tilde{n}} = \frac{\tilde{K}}{(1-t_1)^e}$.

Proof. Similar to derivation of bunching from optimal choice of income for the attentive taxpayers. ■

3.4. Institutional Setting and Data

3.4.1. Context

In this section I provide a brief overview of the UK income tax system, focusing on 2000 - 2019. The income tax system operates through a system of tax bands and allowances. Each individual receives a personal allowance (PA) (£10,000 in 2014-15) which is deducted from total income to give the taxable income. Individuals aged 65-74 and 75 or older are also entitled to higher age-related allowances (ARA). Since the tax reform of April 2010 (in place since the fiscal year 2010-11) these allowances are withdrawn from taxpayers above a certain income. The personal allowance is reduced by 50 pence for every pound of income above £100,000. Age-related allowances (ARA) are also tapered away at higher income levels. The addition to the basic personal allowance is tapered away at a 50% withdrawal rate for every pound of income above £17,000 in 2000-01 (same threshold for both 65-74 and 74 or older taxpayers, threshold changing over time). The definition of income used for calculating deductions to the personal allowance is based on adjusted net income. It is equal to net income minus certain tax reliefs such as trading losses and the grossed-up amount of Gift Aid donations and pension contributions. Net income is the sum of all employment income, profits, pensions, and income from property, savings and dividends, after subtracting certain tax reliefs (e.g. trading losses and gross payments to pension schemes).[4]

Taxable income is subject to different marginal tax rate depending on the tax band upon which it falls (starting rate, basic rate, higher rate or additional rate). Starting rate limit, basic rate limit and higher rate limit determine the income bands. According to the IFS, of the 53.2 million UK adult population, it is estimated that there will be 30.1 million income tax payers in 2016–17. Of those, 4.4 million will pay income tax at the higher rate (but not the additional rate) and 333,000 taxpayers will pay income tax at the additional rate.

In summary, the following are the salient marginal tax rates that appear as official income tax bands in the tax schedule: starting rate (10-20% until 2007-08, then removed) ; basic rate (20-23%) ; higher rate (40%) ; additional rate (45-50% from 2010-11).

[4]The official definitions of both income measure by HMRC can be found online at https://www.gov.uk/guidance/adjusted-net-income

The nonsalient marginal tax rates caused by tapering of the personal allowance or tax credits are the following: 60 % at the PA withdrawal threshold of £100,000 [5]

Figure 3.1 and Table A3.1 summarize the evolution of the income tax schedule over the period.

3.4.2. Data

I rely on the Survey of Personal Income (SPI) provided by the Office of National Statistics. The SPI is a sample of tax records of taxpayers and individuals who could be liable to UK taxes. It covers the period 1985-86 to 2018-19. The sample gradually increases from 400,000 to 700,000 observations over time. The sample covers returns from PAYE and Self Assessment therefore contains information on both wage earners and self-employments. Moreover, it also includes claimants without tax liabilities. Sampling strategies vary based on the origin of the returns, and oversamples very high-income, with a sampling of about 10%.

The withdrawal of personal allowance is determined using the adjusted net income (ANI) instead of taxable income. I program a tax calculator to derive the adjusted net income for each individual using information on total income, deductions and tax reliefs provided in the SPI, and derive distance to the personal allowance threshold. The distribution of both taxable income and adjusted net income is shown in Figure A3.3. As seen, the taxable income distribution is shifted to the left of the ANI below 100,000 due to the presence of tax-free personal allowance between £93,525 and £112,950[6]. After this, both distributions coincide. I also show a proxy for the adjusted net income that does not rely on my own derivation of this definition of income (using values of taxable income and allowances directly provided by the SPI) which validates my calculation of the adjusted net income.

[5] 50% withdrawal i.e. overall 0.4 + 0.5*0.4 = 60% marginal tax rate

[6] £93,525 corresponds to a total income of £100,000 less the personal allowance, £112,950 is the point of full exhaustion of the personal allowance in 2011.

3.5. Results

3.5.1. Empirical Strategy

The identification strategy for estimating the fraction of attentive taxpayers θ is to assume that observed elasticity correctly estimated the structural elasticity at salient kink points (i.e. $\theta = 1$ at salient kink points). As a result, the fraction of attentive taxpayers is estimated by :

$$\hat{\theta} = \frac{\hat{e}_{nonsalient}}{\hat{e}_{salient}} \tag{3.4}$$

I assume that elasticity estimated at salient tax changes, i.e. at kink points with visible changes in the marginal tax rate, based on changes in the taxable income bands, and that are highly visible for individuals, will allow me to approximate the elasticity in the absence of inattention. I will estimate the salient elasticity at the kink point for the basic rate limit (around £30,000) or at the higher rate limit (at £150,000). Nonsalient elasticity is obtained based on indirect changes in the marginal tax rates faced by taxpayers due to a withdrawal of their tax-free personal allowance. Consequently, I obtain bounds on the fraction of inattentive taxpayers using both estimates of salient elasticity.

The bunching method to obtain elasticity estimates follows Chetty et al. (2011). Figure 3.2 provides graphical evidence of bunching at both nonsalient and nonsalient kink points. The observed elasticity implied by bunching is measured using Equation 3.3. Standard errors for these point estimates are obtained a bootstrap procedure by repeating the estimation procedure on 500 samples drawn with replacement from the empirical distribution, taking into account the SPI survey sampling procedure and applying a finite population correction.

I make the following specification choices for the bunching method. First, I rely on bins of £400. Second, the counterfactual density needed to derive observed elasticity is estimated by fitting a flexible three-degree polynomial to the frequency counts excluding the bunching region, and imposing the standard constraint the area under the counterfactual distribution equal the area under the empirical distribution. Third, I use an estimation region for the counterfactual density of £20,000 centered around the kink point. Fourth, given the visual presence of bunching near the kink point, I define a fixed bunching region of $[-£4,000, £800]$ for all estimations. A sensitivity analysis to all these choices is provided below, and find very robustness results to changes in specification.

3.5.2. Main estimates of inattention

The empirical analysis consists in measuring the fraction of attentive taxpayers and testing the three predictions listed in Section 3.3. The validity of these predictions will inform about whether salience plays a role in the behavior response to income taxation.

The main results are provided in Table 3.1 that separately measure the excess mass around salient thresholds (basic rate limit, higher rate limit) and nonsalient threshold (personal allowance withdrawal), estimate an elasticity of taxable income for each, and calculate bounds on the fraction of inattentive taxpayers.

These estimates are provided for each fiscal year since the 2011 reform that introduced the current income tax schedule. As shown in column (3), there is a significant excess mass at all three kink points significant at the 5% level, with standard errors obtained from the bootstrapping method. The fraction of taxpayers that bunch at the nonsalient tax change is large: it corresponds to 2.6 (s.e. 0.21) when all fiscal years 2011-2019 are pooled. Although this excess mass is larger than for other kink points, because the corresponding change in marginal tax rate is also larger than for other tax changes, it corresponds to smaller elasticity estimates than at either the basic rate limit or the higher rate limit kink points. I find that at the nonsalient tax change, the estimate of elasticity is 0.026, compared to 0.035 at the higher rate limit and 0.066 at the basic rate limit. Correspondingly, this gives a bound of inattentive taxpayers of $[27\%, 61\%]$, which is in line with U.S. estimates (De Bartolome, Liebman and Zeckhauser (1995, 2004)). These results validate the first testable prediction that observed elasticity at nonsalient kink points are smaller on average to that measured at salient kink points.

Second, Table 3.1 allows me to test the prediction that attention is increasing over time, and that observed elasticity at salient and nonsalient threshold would converge. As shown here and on Figure A3.1, bunching at the nonsalient personal allowance withdrawal has been progressively increasing over time, from an excess mass of 0.76 in 2012 to 3.05 in 2019. A similar pattern is not observed at salient kink points.[7] This is consistent with learning about nonsalient thresholds from taxpayers. Interestingly, years with higher excess mass than neighboring years correspond to the year of the reform in 2011 (excess mass of 1.61 compared to 0.78 in 2012) and 2016 when there was significant publicity around the personal allowance[8], which is consistent with our hypothesis on inattention. Similarly, bounds on inattention show an initially large fraction of inattentive taxpayers comprised between [76%, 83%] in 2012, which has declined to be [16%, 45%] in 2018 and [0%, 57%] in 2019.

Figure 3.3 tests the third prediction that inattentive taxpayers might bunch at the misperceived higher thresholds if they incorrectly calculate their taxable income. As shown, I find no evidence of bunching around this incorrect threshold. However, I cannot exclude that taxable income was provided by HMRC to taxpayers, therefore preventing them from making this mistake even though they were not aware of the withdrawal of their personal allowance. More generally, the second predictions imposes a particular form of mistakes on the part of taxpayers, and its violation does not imply absence of inattention.

3.5.3. Inattention by Taxpayer Characteristics

I now turn to describing heterogeneity in tax inattention. I apply a similar empirical strategy to that of the main analysis: I estimate the ETI at both nonsalient and salient tax thresholds, then provide bounds on the fraction of inattentive taxpayers provided by lower estimated elasticity at the nonsalient tax change.

I focus on examining heterogeneity in bunching by source of income and occupational group. The literature has documented larger bunching for self-employed individuals compared to wage earners, and linked it to greater tax evasion and avoidance responses (Saez, Chetty et al., Kleven and Waseem, Bastani and Selin (2010, 2011, 2013, 2014). In my context, I investigate heterogeneity between those that draw a majority of their income from wage earners (earning income) compared to investments (investment income), as nearly all taxpayers located around the nonsalient tax change must file a self-assessment return. Second, I am motivated to focus

[7] See Figure A3.2.

[8] There are clear seasonality in Google searches for "personal allowance", with a peak. March The year 2016 corresponds to the highest level of internet searches for "personal allowance" in the UK between 2004-2022 according to Google Trends, see here.

on heterogeneity by occupational group to further investigate whether groups with more tax evasion and avoidance opportunities (e.g. retail or construction) are more attentive to tax changes[9]. Third, I also investigate heterogeneity by geography to test whether individuals living closer to HMRC might be better informed and more attentive to tax changes.

The summary of these results is provided by Figure 3.4, with full details provided in Appendix[10] I focus on three main forms of heterogeneity. First, the lower panel investigates geographical heterogeneity based on a taxpayer's location. I find no difference in inattention based on the region of residence, and slightly higher levels of attention for taxpayers living in London although these differences are small and imprecisely estimated.

Second, I find that taxpayers with income primarily derived from investments or that are company directors are not more attentive. As shown, I find wide bounds on inattention for these two groups between 20 and 80% while taxpayers with a majority of earned income or that are not company directors are respectively 24% and 31% inattentive (compared to 44% for the full sample). However, this hides the fact that company directors and investors are much more reactive to tax changes overall, irrespective of the salience of the tax change. As shown in Figure 3.4, the fraction of bunchers is 6 to 9 times higher among these groups than for non-company directors or non-investors. A similar pattern emerges when we look at salient tax changes, as shown on Table A3.3. In other words, the level of responsiveness to tax changes is higher among investors and company directors, and they are less affected by the salience of the schedule.

Third, I investigate the heterogeneity in inattention based on the industry of the main occupation of the taxpayer. I find that taxpayers working in finance and retail are less affected by the salience of tax changes, while those working in the health sectors are significantly more likely to bunch at nonsalient tax changes than others. Similar to investors, the retail sector is comprised of highly responsive taxpayers irrespective of the salience of the tax change.

[9]There is anecdotal evidence that sectors such as construction are more responsive to tax changes Chetty et al. (2011).

[10]See Tables A3.3 and A3.4 for estimates around the personal allowance withdrawal and higher rate limit, and Tables A3.5 and A3.6 for estimates around the basic rate limit.

3.5.4. Robustness tests

In this section, I test the robustness of my results to changes in the specification used to estimate bunching. As pointed by Kleven (2016)), sensitivity analysis with respect to the estimation of the counterfactual or choices over the bunching region are important in addition to visual checks.

In Figure 3.5, I vary the value of parameter inputs used in the bunching estimation and confirm the robustness of my estimate of inattention. I vary sequentially the size of income bins, the estimation window to compute the counterfactual, the bunching region (in my context, there is significant visual bunching just below the kink point), and the number of degrees used in the polynomial function to derive a counterfactual distribution of income.

As shown, the fraction of inattentive taxpayers that I estimate is qualitatively similar for nearly all of the 20 specification changes that I perform.[11] Specification tests reported above show that my results are robust to changes in parameter values. The average fraction of inattentive taxpayers in my preferred specification is 44% ([27,61]), compared to 45% ([27,64]) if one takes the average over all specifications, and 44% ([26,62]) when excluding the specification with the first-degree polynomial.

Further, I conducted a series of additional robustness tests. First, a placebo exercise confirms the absence of bunching at new thresholds prior to the 2011 tax reform, as shown in Figure A3.4. This is reassuring for the interpretation of bunching as being related changes in the marginal tax rates introduced in 2011 and not other spurrious variations. Second, a worry with the bunching method is that it relies on localised estimates of elasticity that could rely on a few number of observations, which could be a worry when using an administrative survey as done here. Table A3.2 shows the number of taxpayers used for the estimates of elasticity at salient and nonsalient threshold underlying my estimates of attention. Overall, I rely on close to 140,593 observations for the estimation of nonsalient elasticity, as well as 391,374 and 116,845 for estimates of salient elasticity at the basic rate limit and higher rate limit respectively. This alleviates concern of underpowered estimation. Third, I test for whether heterogeneity of

[11] I do not show robustness to relying on a polynomial of degree 1. This is because the linear fit obtained with a first-degree polynomial does not account for the curvature of the densities around the kink, leading to an under-counting of excess mass at salient kink points.

bunching varies significantly at the three kink points. Similar patterns of heterogeneity at the salient and nonsalient kink points would be suggestive evidence that the utility function of agents is similar around these different thresholds. Tables A3.3 and A3.4 compare heterogeneous estimates of elasticity after the 2011 tax reform, and finds that the same groups of taxpayers disproportionaly bunch around either salient and nonsalient threshold (particularly company directors, investors, and those employed in retail). A similar pattern can be found around the salient basic rate limit (Tables A3.5 and A3.6). This evidence of absence of large differences in heterogeneity across thresholds is only suggestive but points to relative differences in preferences or behavioral response opportunities being similar along the income distribution. This would justify our approach of comparing observed elasticity at different points of the income distribution.

3.6. Conclusion

This paper has argued that nonsalient changes to the tax schedule have led to muted adjustments to taxable in the UK, and derive a measure of inattention to tax changes. I provide evidence that inattention caused by the lack of salience is large, with a significant fraction of taxpayers failing to react to higher marginal tax rates when they are not made salient. Second, there appears to be significant learning over time that reduces inattention in the years after the introduction of the reform. Additional, there are limited differences in levels of attention by geography, source of pay, or sector of work. These results are robust to a broad range of specifications and robustness checks.

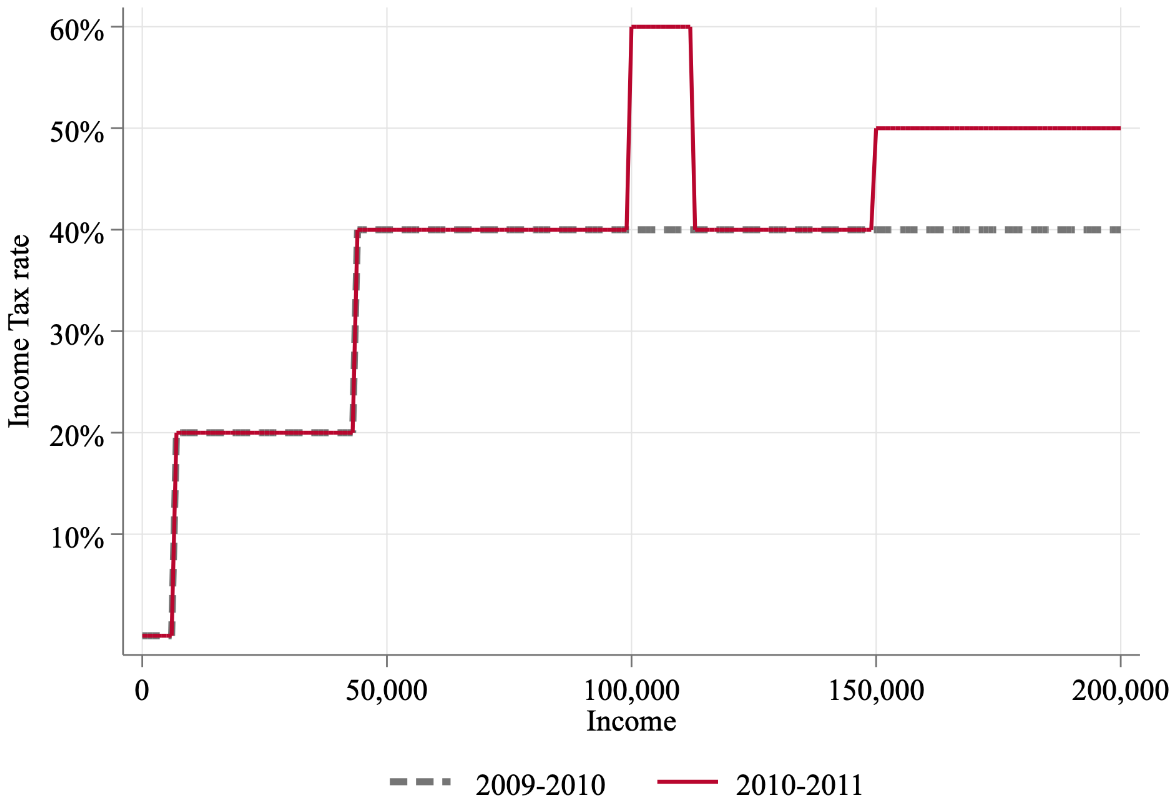
The results discussed here provide, to the best of my knowledge, the first measure of inattention caused by nonsalient tax reforms. These statistics are significant to quantify a form of optimization friction that could explain why observed responses by individuals are relatively limited in the short-run, resulting in smaller estimated elasticity than that from the macro-literature (Saez, Slemrod and Giertz, Chetty et al. (2012, 2011)). Accounting for tax salience could provide more representative estimates of structural elasticity.

Second, this paper has measured potential gains from inattention. By dampening behavioral response to tax rates, governments could reduce welfare costs associated with raising revenue.

It is interesting in that respect to compare responses to salient and nonsalient tax changes introduced in the same 2011 tax reform. Although nonsalient tax changes led to smaller income responses in the year of its introduction, after ten years the nonsalient tax change caused by tapering of personal allowance is causing greater behavioural response than more visible kink points such as the higher rate limit. Accounting for learning, we can conclude that complexity as a policy choice has only limited gains.

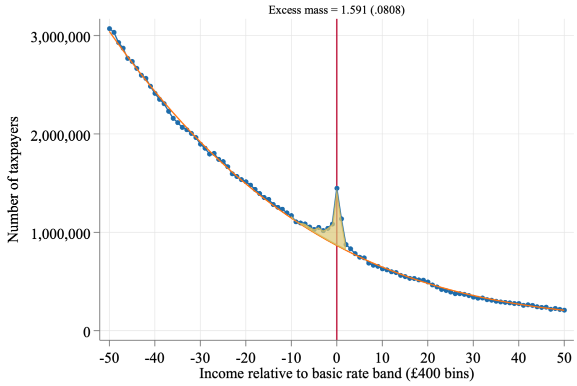
Tables and Figures

FIGURE 3.1: INCOME TAX SCHEDULE IN 2009-2010 AND 2010-2011

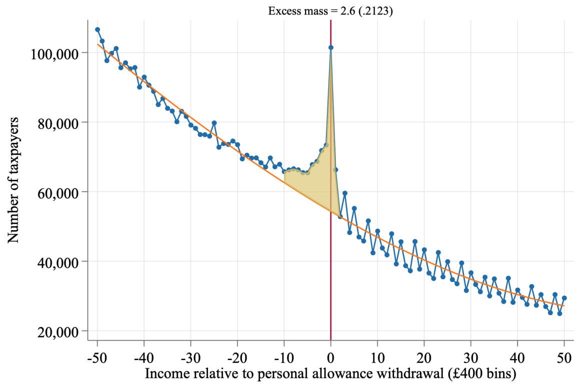


Notes: Income tax schedule excluding working and children tax credits. This does not include national insurance contributions. More details is provided in Table A3.1.

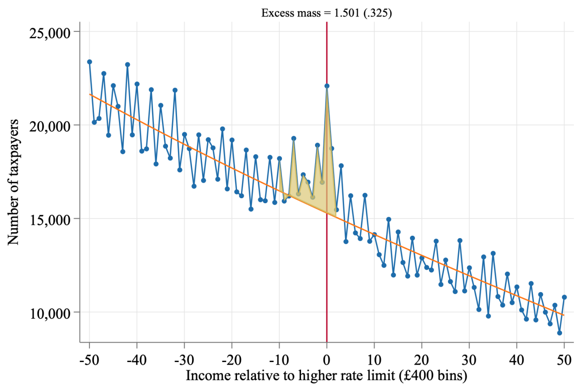
FIGURE 3.2: BUNCHING ESTIMATES AT SALIENT AND NONSALIENT TAX THRESHOLDS



(a) Basic rate limit



(b) Personal Allowance withdrawal



(c) Higher rate limit

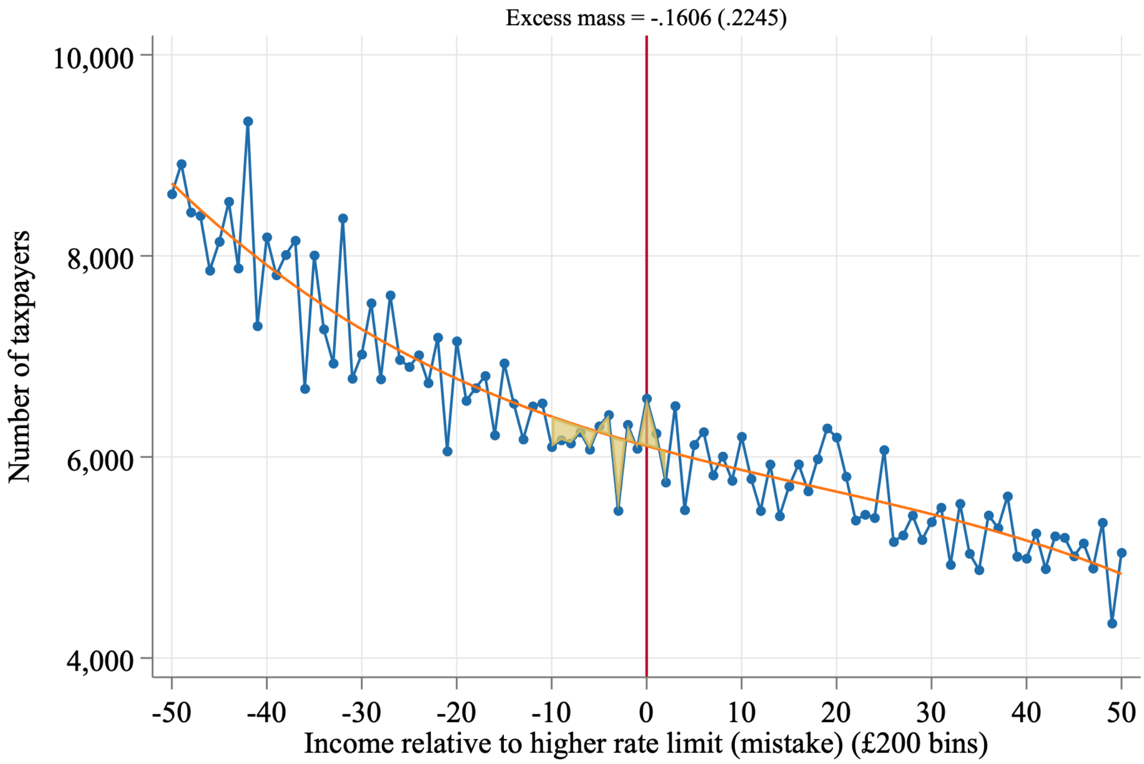
Notes: This figure shows the distribution of income bins around kink points using the pooled sample of tax returns for fiscal years 2011 to 2019. The yellow area corresponds to the excess mass in the bunching region. The red curve is the counterfactual income distribution excluding the bunching region, based on a three-degree polynomial.

TABLE 3.1: ESTIMATES OF INATTENTION

	Basic rate limit		Personal Allowance withdrawal		Higher rate limit		Inattentive taxpayers
	b	e	b	e	b	e	$1-\theta$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2011	1.02	0.038	1.61	0.016	1.71	0.025	37% - 58%
	(0.16)	(0.006)	(0.34)	(0.003)	(0.38)	(0.006)	
2012	1.14	0.045	0.78	0.008	2.23	0.033	76% - 83%
	(0.14)	(0.006)	(0.35)	(0.003)	(0.41)	(0.006)	
2013	1.39	0.056	2.02	0.020	2.08	0.030	34% - 64%
	(0.20)	(0.008)	(0.33)	(0.003)	(0.47)	(0.007)	
2014	1.81	0.079	2.92	0.029	1.54	0.047	39% - 63%
	(0.20)	(0.009)	(0.31)	(0.003)	(0.46)	(0.014)	
2015	1.95	0.085	2.76	0.027	1.60	0.049	44% - 68%
	(0.18)	(0.008)	(0.27)	(0.003)	(0.44)	(0.014)	
2016	1.99	0.087	3.51	0.035	1.84	0.056	39% - 60%
	(0.14)	(0.006)	(0.35)	(0.003)	(0.37)	(0.011)	
2017	1.61	0.070	2.41	0.024	1.25	0.038	38% - 66%
	(0.13)	(0.005)	(0.30)	(0.003)	(0.46)	(0.014)	
2018	1.46	0.061	3.36	0.033	1.29	0.040	16% - 45%
	(0.14)	(0.006)	(0.33)	(0.003)	(0.49)	(0.015)	
2019	1.73	0.070	3.05	0.030	0.36	0.011	0% - 57%
	(0.13)	(0.005)	(0.36)	(0.004)	(0.52)	(0.016)	
Pooled	1.59	0.066	2.60	0.026	1.50	0.035	27% - 61%
	(0.08)	(0.003)	(0.21)	(0.002)	(0.32)	(0.008)	

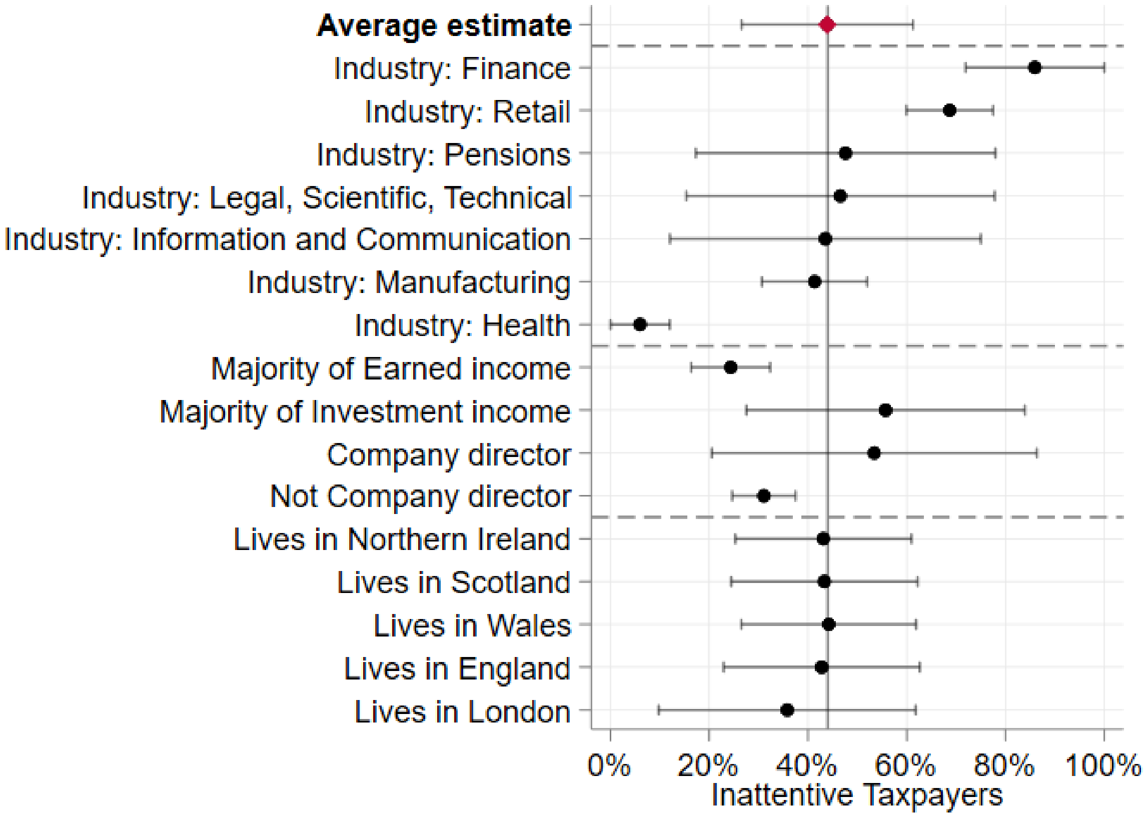
Notes: This table presents estimates of excess mass b from bunching at kink points in the income distribution, corresponding elasticity of income e, and the fraction of inattentive taxpayers $1-\theta$. Standard errors are indicated in parenthesis, based on bootstrapping of 500 samples.

FIGURE 3.3: BUNCHING AT INCORRECT HIGHER RATE LIMIT



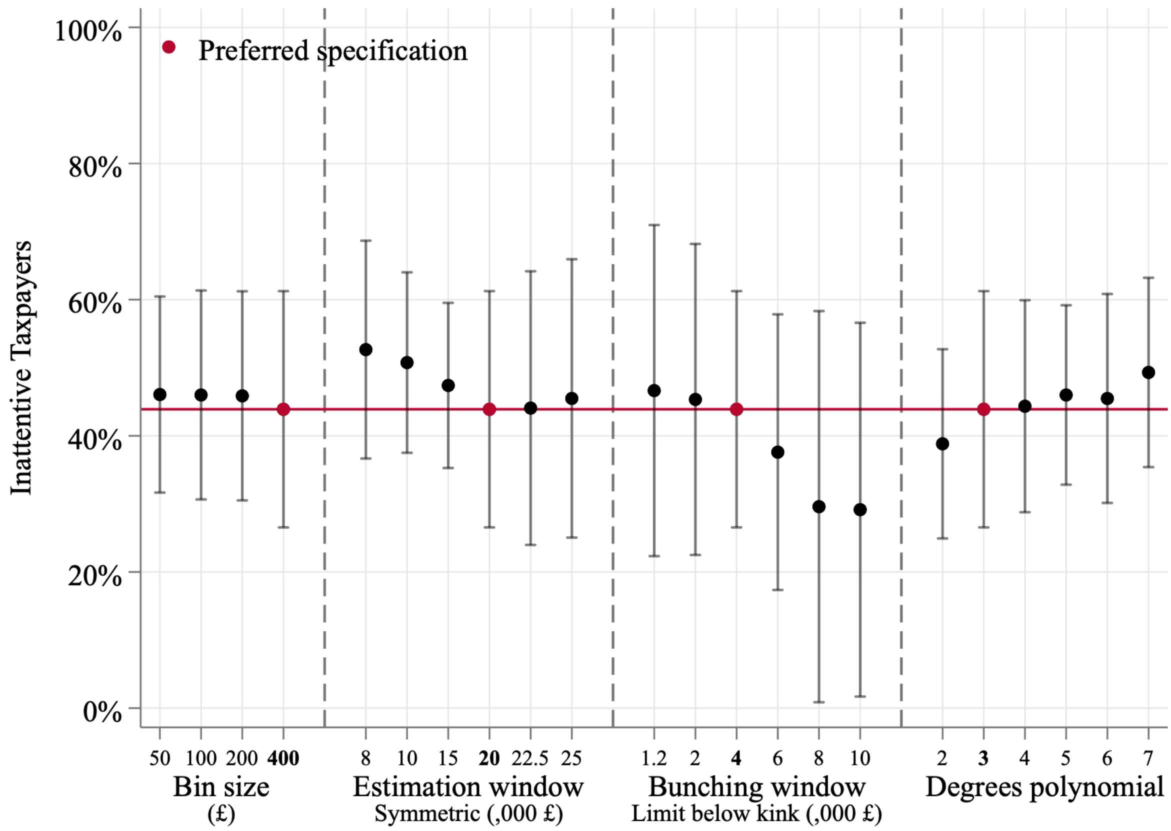
Notes: This figure shows the distribution of £200 bins of income around the threshold corresponding to the misperceived higher rate limit if taxpayers were to not account for the exhaustion of their personal allowance. The data is based on pooled tax returns for 2011-2019. The yellow area corresponds to the excess mass in the bunching region. The red curve is the counterfactual income distribution excluding the bunching region, based on a three-degree polynomial.

FIGURE 3.4: INATTENTIVE TAXPAYERS BY SUBGROUP



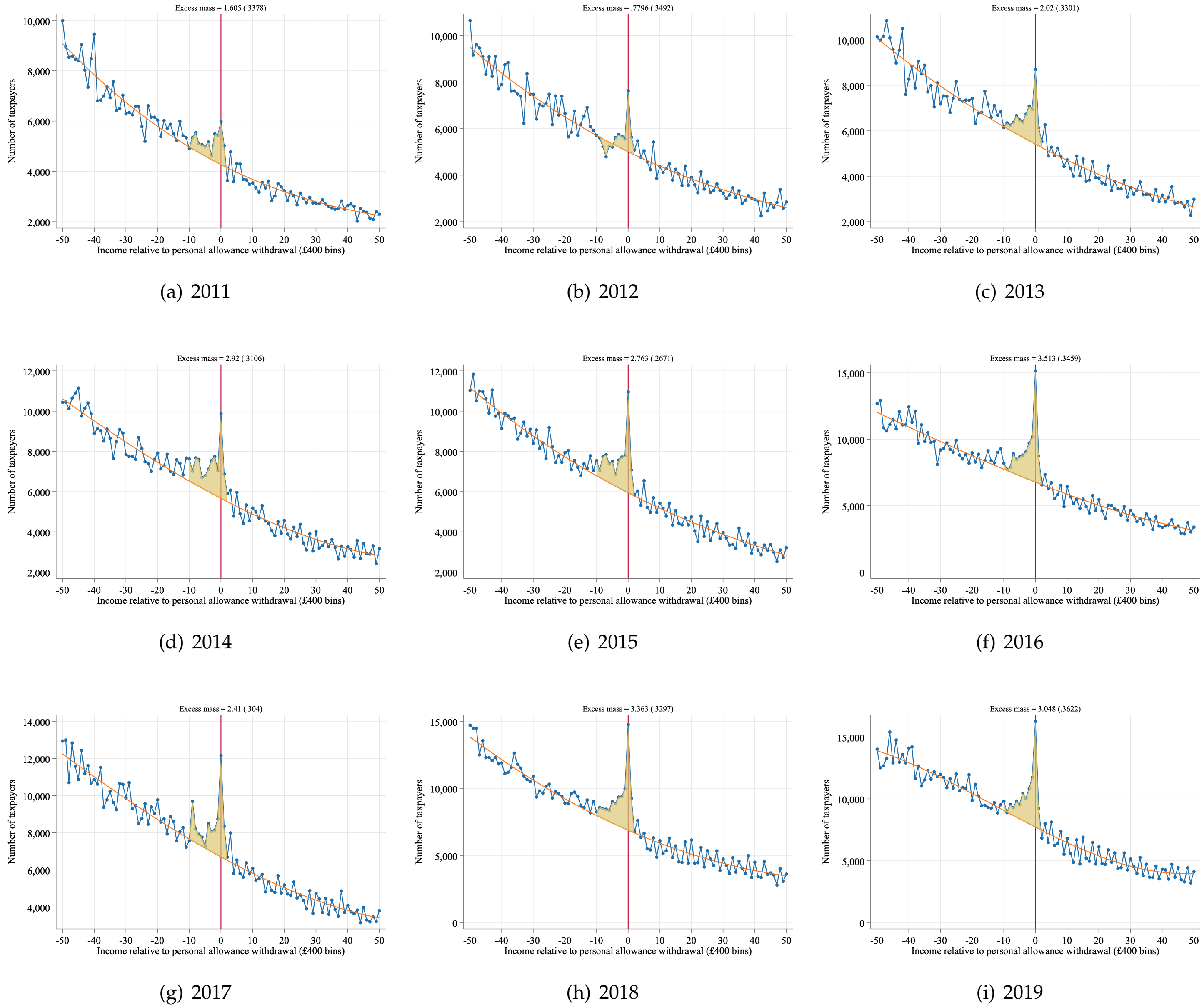
Notes: This figures shows the fraction of inattentive taxpayers by subgroup using the pooled sample of fiscal years 2011-19. See text for more details.

FIGURE 3.5: ROBUSTNESS OF BUNCHING ESTIMATION



Notes: This figures shows the robustness to alternative specification of bounds on the fraction of inattentive taxpayers, using the pooled sample of tax returns from 2011-19. See text for more details.

Appendix to Chapter 3



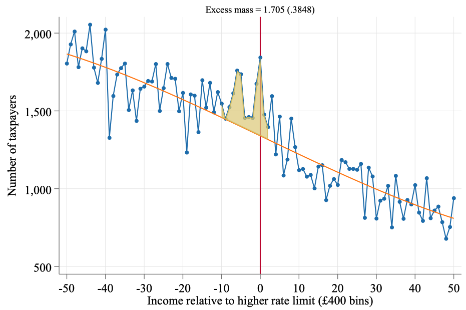
(a) 2011 (b) 2012 (c) 2013

(d) 2014 (e) 2015 (f) 2016

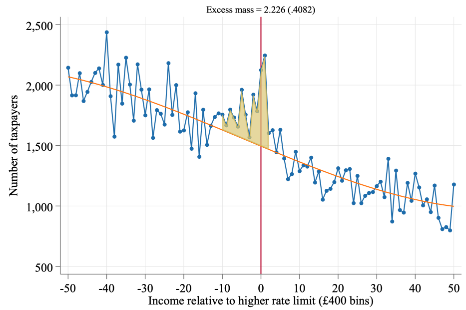
(g) 2017 (h) 2018 (i) 2019

FIGURE A3.1: BUNCHING AT NONSALIENT THRESHOLD OVER TIME

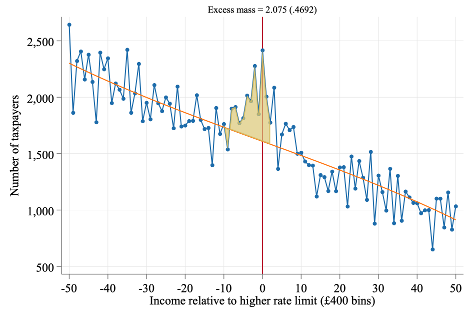
Notes: This figure shows the distribution of £400 bins of income around the kink point corresponding to the withdrawal of personal allowance. The yellow area corresponds to the excess mass in the bunching region. The red curve is the counterfactual income distribution excluding the bunching region, based on a three-degree polynomial.



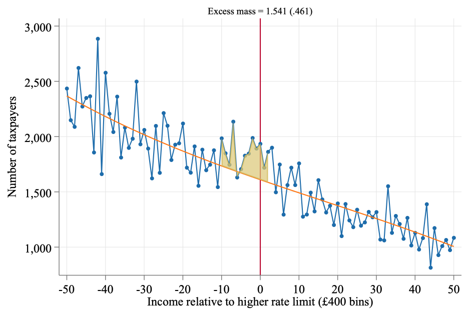
(a) 2011



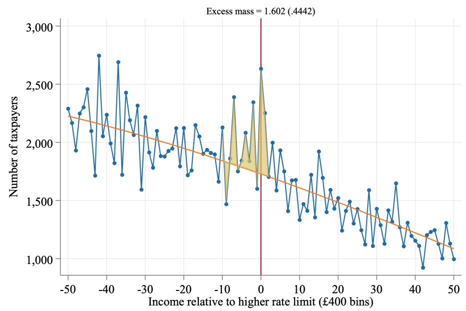
(b) 2012



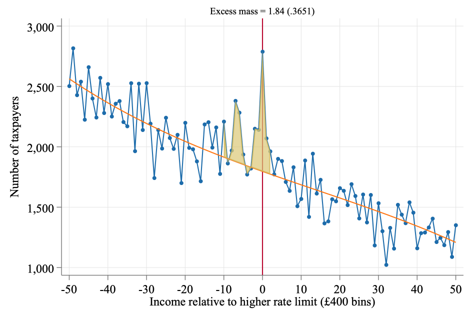
(c) 2013



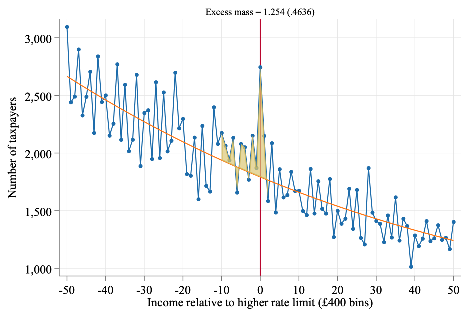
(d) 2014



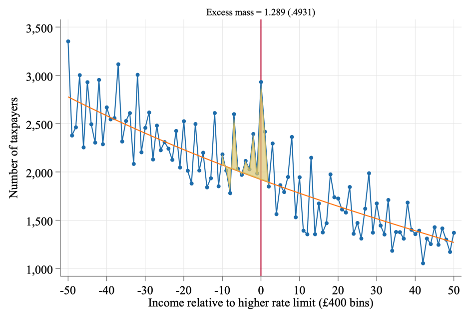
(e) 2015



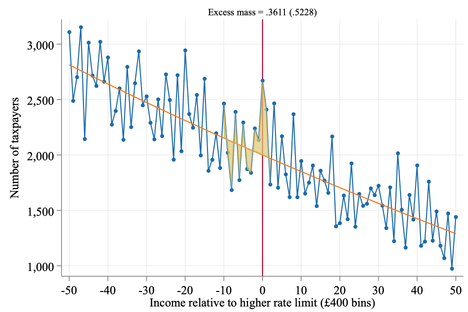
(f) 2016



(g) 2017



(h) 2018

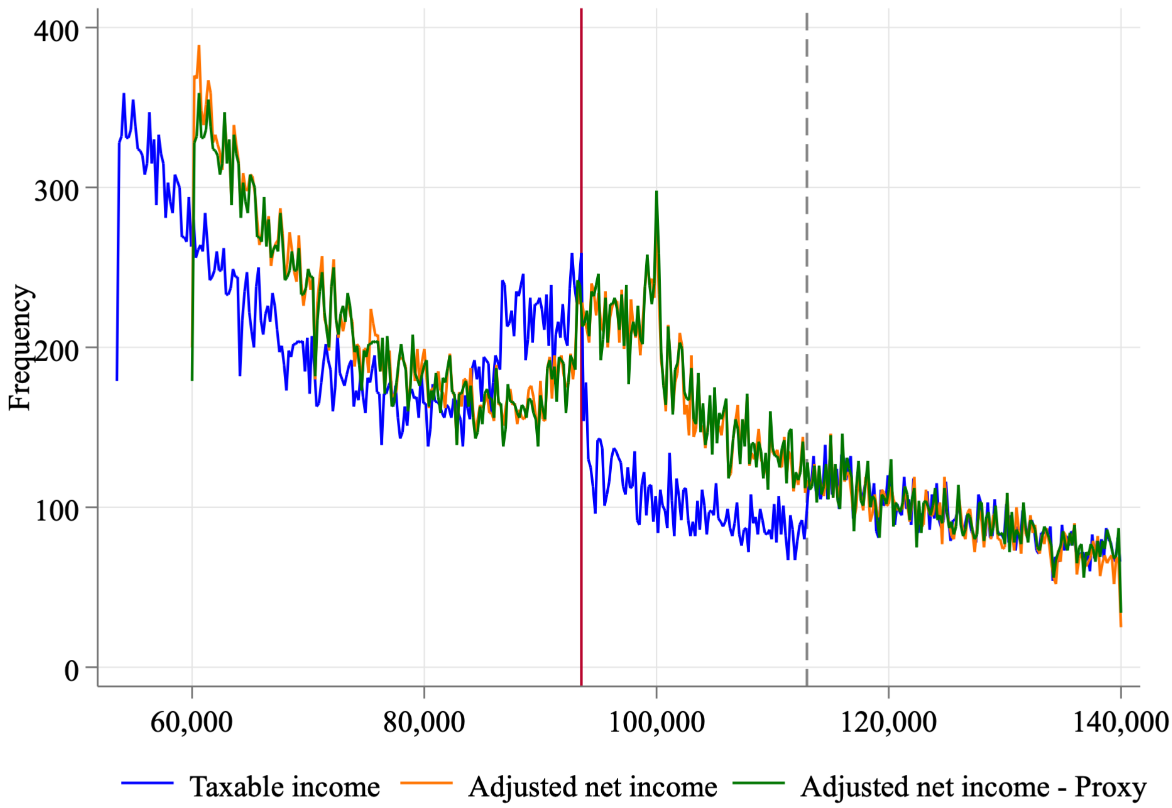


(i) 2019

FIGURE A3.2: BUNCHING AT SALIENT THRESHOLD OVER TIME

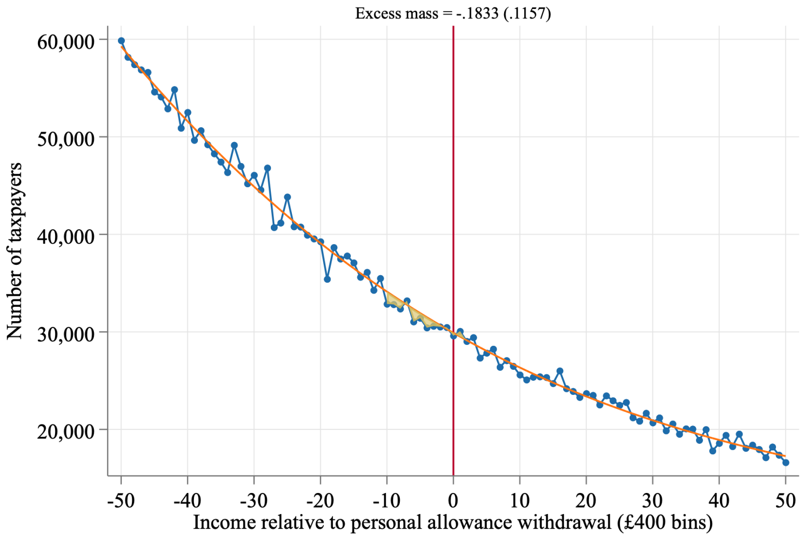
Notes: This figure shows the distribution of £400 bins of income around the higher rate limit. The yellow area corresponds to the excess mass in the bunching region. The red curve is the counterfactual income distribution excluding the bunching region, based on a three-degree polynomial.

FIGURE A3.3: DIFFERENT DEFINITIONS OF INCOME

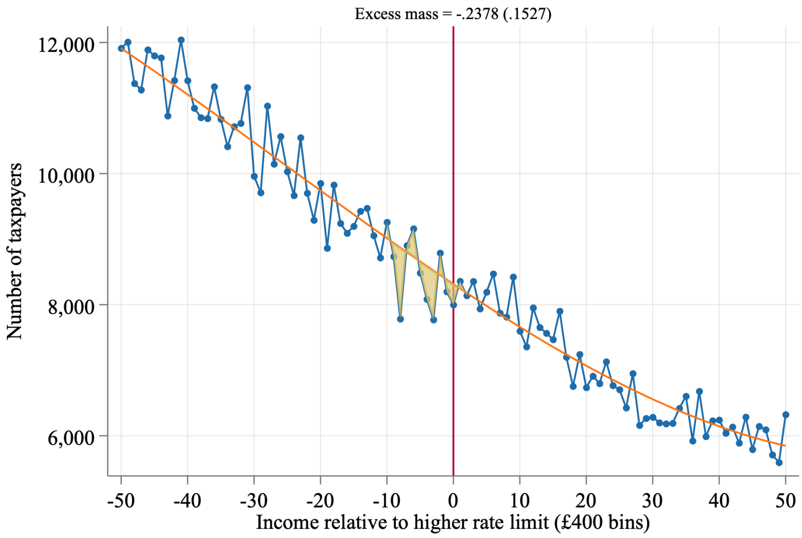


Notes: Taxable income is derived from SPI. The adjusted net income is calculated using available information on the level of personal allowance and tax reliefs provided by the SPI. The proxy measure is the amount of taxable income net of all allowances provided by the SPI. See text for more details.

FIGURE A3.4: BUNCHING ESTIMATES AT SALIENT AND NONSALIENT TAX THRESHOLDS PRIOR TO 2011 REFORM

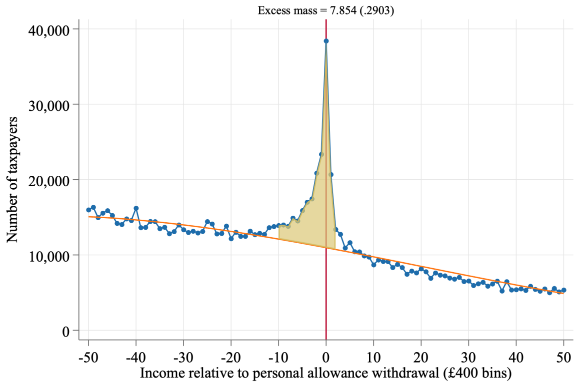


(a) Personal Allowance withdrawal

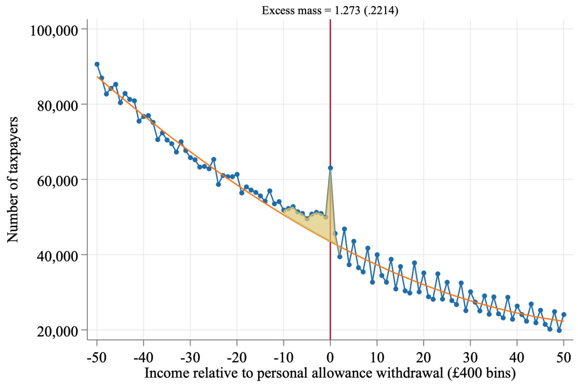


(b) Higher rate limit

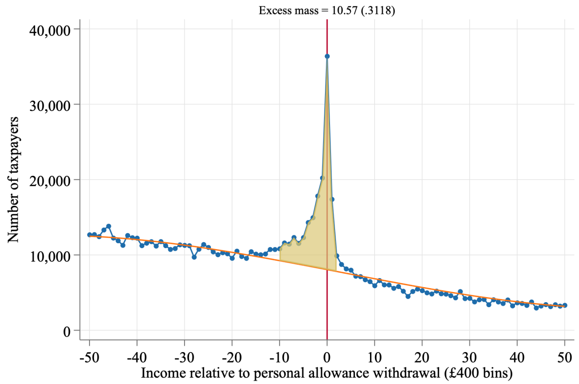
Notes: This figure shows the distribution of income bins around kink points using the pooled sample of tax returns for fiscal years 2000 to 2010, prior to the 2011 reform that introduced the additional income band and the withdrawal of personal allowance. The yellow area corresponds to the excess mass in the bunching region. The red curve is the counterfactual income distribution excluding the bunching region, based on a three-degree polynomial.



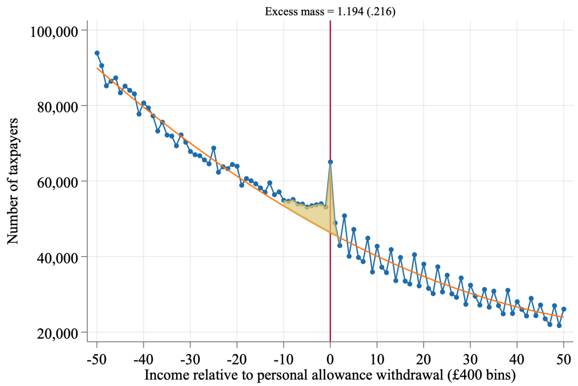
(c) Company Director



(d) Not Company Director



(e) Majority of Investment Income



(f) Majority of Earned Income

FIGURE A3.5: HETEROGENEITY OF BUNCHING AT NONSALIENT THRESHOLD

Notes: This figure shows the distribution of income bins around kink points using the pooled sample of tax returns for fiscal years 2011 to 2019. The yellow area corresponds to the excess mass in the bunching region. The red curve is the counterfactual income distribution excluding the bunching region, based on a three-degree polynomial.

TABLE A3.1: UK INCOME TAX SCHEDULE

	Income Tax rates				Income Tax Thresholds			Personal allowance		
Fiscal Year	Starting rate	Basic rate	Higher rate	Additional rate	Starting rate	Basic rate	Higher rate	Amount	Withdrawal rate	Threshold
1999-2000	10%	22%	40%	—	1,500	28,000	—	4,335	—	—
2000-2001	10%	22%	40%	—	1,520	28,400	—	4,385	—	—
2001-2002	10%	22%	40%	—	1,880	29,400	—	4,535	—	—
2002-2003	10%	22%	40%	—	1,920	29,900	—	4,615	—	—
2003-2004	10%	22%	40%	—	1,960	30,500	—	4,615	—	—
2004-2005	10%	22%	40%	—	2,020	31,400	—	4,745	—	—
2005-2006	10%	22%	40%	—	2,090	32,400	—	4,895	—	—
2006-2007	10%	22%	40%	—	2,150	33,300	—	5,035	—	—
2007-2008	10%	22%	40%	—	2,230	34,600	—	5,225	—	—
2008-2009	—	20%	40%	—	—	34,800	—	6,035	—	—
2009-2010	—	20%	40%	—	—	37,400	—	6,475	—	—
2010-2011	—	20%	40%	50%	—	37,400	150,000	6,475	50%	100,000
2011-2012	—	20%	40%	50%	—	35,000	150,000	7,475	50%	100,000
2012-2013	—	20%	40%	50%	—	34,370	150,000	8,105	50%	100,000
2013-2014	—	20%	40%	45%	—	32,010	150,000	9,440	50%	100,000
2014-2015	—	20%	40%	45%	—	31,865	150,000	10,000	50%	100,000
2015-2016	—	20%	40%	45%	—	31,785	150,000	10,600	50%	100,000
2016-2017	—	20%	40%	45%	—	32,000	150,000	11,000	50%	100,000
2017-2018	—	20%	40%	45%	—	33,500	150,000	11,500	50%	100,000
2018-2019	—	20%	40%	45%	—	34,500	150,000	11,850	50%	100,000

Notes: Tax Schedule taken from HMRC Tax structure and parameters statistics.

TABLE A3.2: NUMBER OF TAXPAYERS FOR ESTIMATES OF INATTENTION

	All Taxpayers	Basic rate limit	Personal Allowance withdrawal	Higher rate limit	Higher rate limit (incorrect)
	(1)	(2)	(3)	(4)	(5)
2011	31,482,946	10,308,613	486,084	137,152	59,864
	[546,321]	[200,492]	[32,599]	[12,302]	[10,855]
2012	31,020,526	11,586,371	545,250	154,911	66,707
	[532,785]	[192,664]	[34,922]	[13,816]	[11,889]
2013	30,816,142	11,989,002	589,653	164,665	69,905
	[538,703]	[206,685]	[38,925]	[14,465]	[12,275]
2014	30,623,776	13,590,468	623,824	166,913	67,320
	[563,357]	[245,641]	[40,488]	[14,495]	[12,093]
2015	30,860,862	13,824,054	651,423	174,037	71,907
	[573,918]	[253,717]	[44,884]	[15,660]	[13,230]
2016	31,441,488	14,173,440	734,603	186,928	74,782
	[580,114]	[258,264]	[49,425]	[16,733]	[13,804]
2017	31,976,824	14,122,243	729,947	188,260	73,131
	[578,285]	[251,516]	[48,273]	[16,693]	[13,464]
2018	31,950,538	12,967,811	778,629	199,353	76,548
	[557,586]	[240,499]	[49,245]	[17,513]	[14,134]
2019	32,071,482	12,356,078	843,589	204,222	78,707
	[580,349]	[236,227]	[52,613]	[18,916]	[15,101]
Pooled	282,244,584	114,918,081	5,983,001	1,576,441	638,871
	[5,051,418]	[2,085,705]	[391,374]	[140,593]	[116,845]

Notes: The table gives the weighted number of taxpayers for a given fiscal year and around kink points, using a centered estimation window around the kink point of £20,000. Raw SPI sample size is given between brackets.

TABLE A3.3: HETEROGENEOUS ESTIMATES OF ELASTICITY AT NONSALIENT AND SALIENT THRESHOLD

	All	London	England	Wales	Scotland	Northern Ireland	Company Director	Not Company Director	Investment Income	Earned Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					A. Personal Allowance Withdrawal (£100,000 threshold)					
2011	0.016	0.018	0.019	0.016	0.018	0.016	0.040	0.009	0.051	0.011
	(0.003)	(0.006)	(0.004)	(0.003)	(0.004)	(0.004)	(0.009)	(0.004)	(0.008)	(0.004)
2012	0.008	-0.006	0.007	0.007	0.007	0.008	0.049	-0.003	0.045	0.002
	(0.003)	(0.006)	(0.003)	(0.003)	(0.003)	(0.004)	(0.006)	(0.004)	(0.006)	(0.004)
2013	0.020	0.019	0.022	0.020	0.021	0.020	0.063	0.009	0.087	0.009
	(0.003)	(0.006)	(0.004)	(0.004)	(0.004)	(0.003)	(0.006)	(0.003)	(0.007)	(0.003)
2014	0.029	0.023	0.028	0.029	0.028	0.029	0.062	0.020	0.083	0.019
	(0.003)	(0.007)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)	(0.004)	(0.007)	(0.004)
2015	0.027	0.024	0.029	0.027	0.028	0.027	0.083	0.012	0.104	0.012
	(0.003)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.003)	(0.006)	(0.003)
2016	0.035	0.029	0.035	0.033	0.036	0.035	0.110	0.012	0.150	0.007
	(0.003)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.003)	(0.006)	(0.004)
2017	0.024	0.019	0.024	0.024	0.024	0.024	0.078	0.011	0.108	0.010
	(0.003)	(0.006)	(0.004)	(0.003)	(0.004)	(0.003)	(0.005)	(0.003)	(0.008)	(0.003)
2018	0.033	0.025	0.034	0.033	0.033	0.034	0.090	0.020	0.117	0.019
	(0.003)	(0.006)	(0.003)	(0.003)	(0.003)	(0.003)	(0.007)	(0.004)	(0.008)	(0.004)
2019	0.030	0.025	0.030	0.030	0.030	0.030	0.095	0.017	0.128	0.015
	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.007)	(0.004)	(0.006)	(0.004)
Pooled	0.026	0.020	0.026	0.025	0.026	0.026	0.077	0.013	0.104	0.012
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
					B. Higher Rate Limit (£150,000 threshold)					
2011	0.025	0.024	0.025	0.026	0.024	0.024	0.065	0.011	0.096	0.015
	(0.006)	(0.011)	(0.006)	(0.006)	(0.006)	(0.005)	(0.010)	(0.007)	(0.017)	(0.006)
2012	0.033	0.022	0.032	0.032	0.032	0.032	0.062	0.023	0.134	0.018
	(0.006)	(0.010)	(0.006)	(0.006)	(0.006)	(0.006)	(0.011)	(0.006)	(0.016)	(0.006)
2013	0.030	0.005	0.029	0.029	0.031	0.030	0.086	0.012	0.095	0.019
	(0.007)	(0.010)	(0.007)	(0.006)	(0.007)	(0.006)	(0.014)	(0.007)	(0.017)	(0.006)
2014	0.047	0.059	0.048	0.047	0.047	0.048	0.078	0.038	0.140	0.032
	(0.014)	(0.025)	(0.014)	(0.014)	(0.014)	(0.015)	(0.024)	(0.017)	(0.029)	(0.016)
2015	0.049	-0.001	0.042	0.049	0.045	0.046	0.143	0.022	0.209	0.021
	(0.014)	(0.016)	(0.013)	(0.014)	(0.013)	(0.015)	(0.022)	(0.016)	(0.031)	(0.013)
2016	0.056	0.037	0.052	0.055	0.053	0.057	0.196	0.015	0.243	0.018
	(0.011)	(0.020)	(0.011)	(0.011)	(0.012)	(0.011)	(0.020)	(0.012)	(0.028)	(0.012)
2017	0.038	0.028	0.038	0.038	0.039	0.038	0.102	0.021	0.157	0.021
	(0.014)	(0.024)	(0.015)	(0.015)	(0.015)	(0.015)	(0.027)	(0.016)	(0.027)	(0.015)
2018	0.040	0.019	0.041	0.038	0.043	0.040	0.127	0.017	0.155	0.021
	(0.015)	(0.026)	(0.017)	(0.016)	(0.017)	(0.016)	(0.022)	(0.017)	(0.033)	(0.016)
2019	0.011	0.032	0.014	0.011	0.014	0.011	0.061	-0.002	0.131	-0.006
	(0.016)	(0.026)	(0.017)	(0.016)	(0.016)	(0.015)	(0.032)	(0.017)	(0.025)	(0.016)
Pooled	0.035	0.023	0.034	0.035	0.035	0.035	0.098	0.017	0.144	0.017
	(0.008)	(0.011)	(0.008)	(0.007)	(0.008)	(0.007)	(0.009)	(0.008)	(0.010)	(0.008)

Notes: Bootsrapped standard errors in parenthesis based on 500 draws. Panel A gives elasticity estimates around the nonsalient kink point corresponding to the withdrawal of the personal allowance. Panel B gives elasticity estimates around the salient kink point corresponding to the higher rate limit threshold.

Table A3.4: Estimates of Elasticity by Industry

	All	Pension	Finance	Legal, Scientific, Technical	Health	Information and Communication	Retail	Manufacturing	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				A. Personal Allowance Withdrawal (£100,000 threshold)					
2011	0.016	0.024	-0.010	0.020	0.001	0.021	0.022	0.017	0.026
	(0.003)	(0.006)	(0.008)	(0.009)	(0.008)	(0.012)	(0.011)	(0.010)	(0.009)
2012	0.008	0.016	0.002	0.012	0.005	-0.005	0.005	0.024	0.006
	(0.003)	(0.006)	(0.008)	(0.008)	(0.008)	(0.012)	(0.010)	(0.011)	(0.006)
2013	0.020	0.028	0.009	0.031	0.023	0.014	0.027	0.030	0.043
	(0.003)	(0.007)	(0.009)	(0.007)	(0.006)	(0.007)	(0.008)	(0.009)	(0.008)
2014	0.029	0.021	0.024	0.039	0.034	0.036	0.033	0.045	0.022
	(0.003)	(0.005)	(0.009)	(0.008)	(0.007)	(0.009)	(0.010)	(0.012)	(0.006)
2015	0.027	0.030	0.008	0.036	0.015	0.025	0.034	0.026	0.032
	(0.003)	(0.005)	(0.007)	(0.007)	(0.006)	(0.010)	(0.009)	(0.011)	(0.006)
2016	0.035	0.035	0.009	0.047	0.034	0.037	0.028	0.038	0.038
	(0.003)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)	(0.013)	(0.006)
2017	0.024	0.016	0.006	0.034	0.018	0.020	0.031	0.027	0.032
	(0.003)	(0.005)	(0.008)	(0.008)	(0.007)	(0.008)	(0.009)	(0.013)	(0.006)
2018	0.033	0.025	0.022	0.035	0.026	0.035	0.040	0.020	0.044
	(0.003)	(0.005)	(0.009)	(0.006)	(0.008)	(0.010)	(0.009)	(0.014)	(0.009)
2019	0.030	0.035	0.030	0.035	0.032	0.043	0.023	0.010	0.029
	(0.004)	(0.005)	(0.010)	(0.007)	(0.008)	(0.009)	(0.010)	(0.010)	(0.007)
Pooled	0.026	0.026	0.013	0.033	0.021	0.027	0.028	0.026	0.031
	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)
				B. Higher Rate Limit (£150,000 threshold)					
2011	0.025	0.014	0.012	0.032	0.003	0.058	0.039	0.041	0.033
	(0.006)	(0.012)	(0.012)	(0.017)	(0.014)	(0.022)	(0.016)	(0.024)	(0.013)
2012	0.033	0.032	0.004	0.025	0.039	0.037	0.048	0.042	0.056
	(0.006)	(0.012)	(0.014)	(0.014)	(0.016)	(0.020)	(0.015)	(0.021)	(0.014)
2013	0.030	0.014	-0.007	0.031	0.009	0.032	0.078	0.074	0.052
	(0.007)	(0.012)	(0.015)	(0.011)	(0.014)	(0.018)	(0.020)	(0.023)	(0.014)
2014	0.047	0.039	-0.017	0.074	0.015	0.132	0.060	-0.110	0.096
	(0.014)	(0.024)	(0.037)	(0.027)	(0.029)	(0.041)	(0.039)	(0.034)	(0.031)
2015	0.049	0.074	-0.007	0.055	0.020	0.066	0.077	0.095	0.056
	(0.014)	(0.025)	(0.031)	(0.024)	(0.028)	(0.038)	(0.038)	(0.036)	(0.028)
2016	0.056	0.067	0.008	0.049	0.027	-0.015	0.160	0.003	0.118
	(0.011)	(0.025)	(0.026)	(0.023)	(0.029)	(0.026)	(0.036)	(0.038)	(0.021)
2017	0.038	0.037	0.007	0.055	0.014	-0.007	0.068	0.094	0.052
	(0.014)	(0.023)	(0.030)	(0.026)	(0.029)	(0.028)	(0.042)	(0.053)	(0.028)
2018	0.040	0.026	0.010	0.077	-0.028	-0.017	0.077	0.115	0.070
	(0.015)	(0.023)	(0.034)	(0.025)	(0.031)	(0.031)	(0.033)	(0.044)	(0.025)
2019	0.011	0.007	-0.036	-0.012	-0.006	0.036	0.039	0.132	0.018
	(0.016)	(0.020)	(0.034)	(0.026)	(0.029)	(0.033)	(0.031)	(0.047)	(0.034)
Pooled	0.035	0.031	-0.002	0.039	0.012	0.031	0.069	0.053	0.057
	(0.008)	(0.006)	(0.016)	(0.009)	(0.007)	(0.012)	(0.011)	(0.011)	(0.010)

Notes: Bootsrapped standard errors in parenthesis based on 500 draws. Panel A gives elasticity estimates around the nonsalient kink point corresponding to the withdrawal of the personal allowance. Panel B gives elasticity estimates around the salient kink point corresponding to the higher rate limit threshold.

TABLE A3.5: SUBGROUP ELASTICITY AROUND BASIC RATE THRESHOLD

	All	London	England	Wales	Scotland	Northern Ireland	Company Director	Not Company Director	Investment Income	Earned Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
2011	0.038	0.048	0.043	0.042	0.044	0.042	0.503	0.003	0.592	-0.000
	(0.006)	(0.012)	(0.007)	(0.006)	(0.007)	(0.006)	(0.025)	(0.007)	(0.028)	(0.007)
2012	0.045	0.042	0.051	0.048	0.049	0.048	0.576	0.004	0.668	-0.000
	(0.005)	(0.012)	(0.006)	(0.005)	(0.006)	(0.006)	(0.029)	(0.006)	(0.033)	(0.006)
2013	0.056	0.034	0.063	0.059	0.061	0.057	0.578	0.012	0.662	0.007
	(0.008)	(0.011)	(0.008)	(0.009)	(0.008)	(0.008)	(0.029)	(0.008)	(0.030)	(0.008)
2014	0.079	0.065	0.078	0.074	0.079	0.075	0.593	0.028	0.675	0.023
	(0.008)	(0.010)	(0.008)	(0.008)	(0.008)	(0.008)	(0.033)	(0.008)	(0.032)	(0.008)
2015	0.085	0.051	0.084	0.081	0.085	0.080	0.637	0.028	0.732	0.022
	(0.008)	(0.011)	(0.007)	(0.007)	(0.008)	(0.007)	(0.038)	(0.008)	(0.034)	(0.007)
2016	0.087	0.068	0.087	0.083	0.088	0.083	0.714	0.021	0.795	0.014
	(0.007)	(0.012)	(0.006)	(0.006)	(0.006)	(0.006)	(0.031)	(0.006)	(0.030)	(0.006)
2017	0.070	0.043	0.073	0.069	0.070	0.068	0.489	0.028	0.549	0.022
	(0.006)	(0.010)	(0.006)	(0.005)	(0.006)	(0.006)	(0.026)	(0.005)	(0.030)	(0.005)
2018	0.061	0.055	0.064	0.061	0.063	0.060	0.456	0.021	0.531	0.013
	(0.006)	(0.011)	(0.007)	(0.006)	(0.007)	(0.006)	(0.026)	(0.006)	(0.029)	(0.006)
2019	0.070	0.071	0.078	0.075	0.075	0.072	0.513	0.031	0.604	0.021
	(0.005)	(0.010)	(0.006)	(0.006)	(0.006)	(0.005)	(0.025)	(0.005)	(0.029)	(0.005)
Pooled	0.066	0.053	0.070	0.067	0.069	0.066	0.567	0.020	0.648	0.014
	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.023)	(0.003)	(0.022)	(0.003)

Notes: Bootsrapped standard errors in parenthesis based on 500 draws.

TABLE A3.6: ELASTICITY BY INDUSTRY AROUND BASIC RATE THRESHOLD

	All	Pension	Finance	Legal, Scientific, Technical	Health	Information and Communication	Retail	Manufacturing	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2011	0.038	0.020	0.032	0.109	-0.003	0.044	0.130	0.024	0.032
	(0.005)	(0.016)	(0.021)	(0.014)	(0.021)	(0.019)	(0.028)	(0.017)	(0.010)
2012	0.045	0.044	0.045	0.116	-0.006	0.097	0.106	0.005	0.038
	(0.005)	(0.018)	(0.021)	(0.016)	(0.018)	(0.015)	(0.022)	(0.014)	(0.010)
2013	0.056	0.080	0.179	0.387	0.180	0.339	0.283	0.269	0.225
	(0.008)	(0.017)	(0.040)	(0.023)	(0.025)	(0.028)	(0.031)	(0.029)	(0.015)
2014	0.079	0.142	0.011	0.145	0.013	0.093	0.135	0.043	0.047
	(0.008)	(0.039)	(0.022)	(0.013)	(0.021)	(0.015)	(0.023)	(0.012)	(0.010)
2015	0.085	0.151	0.082	0.144	0.018	0.100	0.161	0.026	0.053
	(0.007)	(0.025)	(0.020)	(0.013)	(0.020)	(0.017)	(0.024)	(0.014)	(0.010)
2016	0.087	0.168	0.029	0.194	0.014	0.114	0.136	0.040	0.043
	(0.007)	(0.018)	(0.020)	(0.017)	(0.018)	(0.019)	(0.025)	(0.014)	(0.011)
2017	0.070	0.151	0.090	0.116	0.014	0.069	0.071	0.048	0.035
	(0.006)	(0.017)	(0.023)	(0.012)	(0.017)	(0.016)	(0.020)	(0.013)	(0.009)
2018	0.061	0.130	0.009	0.125	0.052	0.136	0.076	0.034	0.018
	(0.006)	(0.019)	(0.022)	(0.013)	(0.018)	(0.017)	(0.023)	(0.012)	(0.008)
2019	0.070	0.145	0.035	0.162	0.068	0.143	0.127	0.040	0.016
	(0.005)	(0.018)	(0.023)	(0.016)	(0.023)	(0.016)	(0.023)	(0.016)	(0.009)
Pooled	0.066	0.117	0.045	0.150	0.024	0.109	0.122	0.037	0.040
	(0.003)	(0.012)	(0.008)	(0.006)	(0.007)	(0.007)	(0.011)	(0.005)	(0.004)

Notes: Bootsrapped standard errors in parenthesis based on 500 draws.

TABLE A3.7: NUMBER OF TAXPAYERS FOR ESTIMATES IN TABLE A3.3

	All	London	England	Wales	Scotland	Northern Ireland	Company Director	Not Company Director	Investment Income	Earned Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					A. Personal Allowance Withdrawal (£100,000 threshold)					
2011	486,084	105,348	433,542	474,473	451,880	479,358	98,285	387,799	59,592	426,492
	[32,599]	[6,989]	[29,035]	[832]	[2,269]	[463]	[6,863]	[25,736]	[4,018]	[28,581]
2012	545,250	121,064	488,106	533,199	507,028	538,378	105,625	439,625	72,749	472,501
	[34,922]	[7,640]	[31,182]	[803]	[2,464]	[473]	[7,395]	[27,527]	[4,861]	[30,061]
2013	589,653	128,241	526,595	577,158	546,507	582,236	113,867	475,786	80,793	508,859
	[38,925]	[8,486]	[34,769]	[847]	[2,817]	[492]	[7,969]	[30,956]	[5,457]	[33,468]
2014	623,824	137,371	555,487	610,099	577,011	616,026	121,933	501,891	94,016	529,808
	[40,488]	[8,854]	[36,123]	[885]	[2,975]	[505]	[8,344]	[32,144]	[6,266]	[34,222]
2015	651,423	143,226	580,826	637,028	602,650	643,994	128,665	522,758	103,724	547,699
	[44,884]	[9,922]	[40,064]	[983]	[3,300]	[537]	[9,199]	[35,685]	[7,187]	[37,697]
2016	734,603	166,768	658,244	718,138	684,514	724,797	158,423	576,180	138,678	595,925
	[49,425]	[11,371]	[44,207]	[1,086]	[3,494]	[638]	[11,347]	[38,078]	[9,814]	[39,611]
2017	729,947	167,511	655,049	714,724	679,276	720,942	135,701	594,246	103,802	626,144
	[48,273]	[11,356]	[43,314]	[1,035]	[3,316]	[608]	[9,440]	[38,833]	[7,043]	[41,230]
2018	778,629	179,089	700,745	761,569	728,077	768,357	142,666	635,964	114,416	664,213
	[49,245]	[11,556]	[44,296]	[1,084]	[3,224]	[641]	[9,823]	[39,422]	[7,599]	[41,646]
2019	843,589	202,531	762,510	825,556	790,307	833,825	139,930	703,659	113,631	729,958
	[52,613]	[12,775]	[47,503]	[1,122]	[3,333]	[655]	[10,382]	[42,231]	[8,319]	[44,294]
Pooled	5,983,001	1,351,150	5,361,104	5,851,944	5,567,250	5,907,911	1,145,094	4,837,907	881,401	5,101,600
	[391,374]	[88,949]	[350,493]	[8,677]	[27,192]	[5,012]	[80,762]	[310,612]	[60,564]	[330,810]
					B. Higher Rate Limit (£150,000 threshold)					
2011	137,152	33,906	124,340	134,714	128,491	135,439	33,215	103,937	16,426	120,726
	[12,302]	[3,078]	[11,168]	[218]	[762]	[154]	[2,921]	[9,381]	[1,430]	[10,872]
2012	154,911	38,856	140,850	152,000	145,275	153,398	36,420	118,491	19,562	135,349
	[13,816]	[3,490]	[12,566]	[260]	[856]	[134]	[3,187]	[10,629]	[1,706]	[12,110]
2013	164,665	40,502	148,870	161,270	154,073	162,857	38,991	125,674	22,695	141,970
	[14,465]	[3,614]	[13,068]	[294]	[944]	[159]	[3,328]	[11,137]	[1,961]	[12,504]
2014	166,913	42,227	150,234	163,596	155,311	165,152	36,744	130,169	24,066	142,847
	[14,495]	[3,679]	[13,064]	[279]	[1,004]	[148]	[3,144]	[11,351]	[2,052]	[12,443]
2015	174,037	45,160	156,489	170,699	161,930	171,935	38,605	135,433	26,636	147,401
	[15,660]	[4,079]	[14,087]	[303]	[1,085]	[185]	[3,407]	[12,253]	[2,338]	[13,322]
2016	186,928	50,708	169,034	183,245	174,750	184,894	42,594	144,334	31,432	155,496
	[16,733]	[4,574]	[15,144]	[324]	[1,088]	[177]	[3,752]	[12,981]	[2,752]	[13,981]
2017	188,260	51,907	172,349	184,948	177,727	186,193	38,877	149,382	23,395	164,865
	[16,693]	[4,662]	[15,286]	[296]	[929]	[182]	[3,343]	[13,350]	[2,021]	[14,672]
2018	199,353	54,173	182,868	195,703	188,678	197,194	41,055	158,298	26,332	173,021
	[17,513]	[4,811]	[16,064]	[315]	[949]	[185]	[3,542]	[13,971]	[2,252]	[15,261]
2019	204,222	58,504	186,852	200,319	192,964	202,013	39,898	164,324	24,044	180,178
	[18,916]	[5,485]	[17,292]	[362]	[1,054]	[208]	[3,530]	[15,386]	[2,272]	[16,644]
Pooled	1,576,441	415,944	1,431,885	1,546,494	1,479,198	1,559,076	346,399	1,230,042	214,589	1,361,853
	[140,593]	[37,472]	[127,739]	[2,651]	[8,671]	[1,532]	[30,154]	[110,439]	[18,784]	[121,809]

Notes: The table gives the weighted number of taxpayers for a given fiscal year and around kink points, using a centered estimation window around the kink point of £20,000. Raw SPI sample size is given between brackets.

TABLE A3.8: NUMBER OF TAXPAYERS FOR ESTIMATES IN TABLE A3.4

	All	Pension	Finance	Legal, Scientific, Technical	Health	Information and Communication	Retail	Manufacturing	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				A. Personal Allowance Withdrawal (£100,000 threshold)					
2011	486,084	79,929	56,644	70,919	47,417	44,621	43,029	32,154	100,746
	[32,599]	[5,394]	[3,679]	[4,860]	[3,639]	[2,926]	[2,791]	[2,080]	[6,478]
2012	545,250	81,272	66,281	82,756	49,739	52,372	46,755	36,271	114,564
	[34,922]	[5,551]	[4,236]	[5,263]	[3,752]	[3,219]	[2,838]	[2,141]	[6,907]
2013	589,653	88,017	47,570	67,129	43,511	40,378	36,924	27,085	90,893
	[38,925]	[6,082]	[3,660]	[4,970]	[3,459]	[2,983]	[2,734]	[1,963]	[6,370]
2014	623,824	97,618	71,634	93,607	52,306	62,917	53,972	41,892	134,689
	[40,488]	[6,483]	[4,756]	[6,201]	[3,891]	[3,924]	[3,355]	[2,620]	[8,305]
2015	651,423	98,418	74,639	102,491	52,098	64,616	54,990	44,304	142,129
	[44,884]	[6,818]	[5,256]	[7,030]	[4,023]	[4,396]	[3,780]	[2,906]	[9,471]
2016	734,603	110,109	83,357	121,528	54,484	76,469	62,179	45,701	160,240
	[49,425]	[7,509]	[5,730]	[8,237]	[4,214]	[5,056]	[3,998]	[2,909]	[10,404]
2017	729,947	108,329	82,726	115,806	56,244	74,380	61,652	48,969	162,379
	[48,273]	[7,219]	[5,749]	[7,809]	[4,240]	[4,860]	[3,938]	[2,983]	[10,194]
2018	778,629	111,557	87,509	123,455	60,023	77,944	65,386	51,410	180,228
	[49,245]	[7,226]	[5,795]	[7,932]	[4,215]	[5,040]	[4,023]	[2,956]	[10,754]
2019	843,589	110,980	98,582	131,243	64,910	88,172	69,450	58,707	199,900
	[52,613]	[7,638]	[6,103]	[8,279]	[4,554]	[5,375]	[4,242]	[3,309]	[11,777]
Pooled	5,983,001	886,229	668,943	908,935	480,733	581,869	494,337	386,494	1,285,768
	[391,374]	[59,920]	[44,964]	[60,581]	[35,987]	[37,779]	[31,699]	[23,867]	[80,660]
				B. Higher Rate Limit (£150,000 threshold)					
2011	137,152	21,493	20,831	20,948	16,060	10,657	11,588	7,441	24,323
	[12,302]	[1,930]	[1,901]	[1,890]	[1,468]	[969]	[1,042]	[670]	[2,096]
2012	154,911	23,595	25,496	24,345	15,989	12,465	12,648	8,458	26,823
	[13,816]	[2,101]	[2,322]	[2,157]	[1,437]	[1,120]	[1,147]	[755]	[2,334]
2013	164,665	25,808	25,168	25,062	15,906	13,167	13,156	9,310	29,781
	[14,465]	[2,278]	[2,257]	[2,219]	[1,421]	[1,170]	[1,169]	[812]	[2,530]
2014	166,913	26,030	25,528	26,784	15,191	14,480	13,913	9,131	30,434
	[14,495]	[2,275]	[2,267]	[2,327]	[1,340]	[1,242]	[1,217]	[799]	[2,576]
2015	174,037	26,391	26,315	28,411	15,789	15,372	14,129	9,673	32,559
	[15,660]	[2,369]	[2,404]	[2,571]	[1,435]	[1,405]	[1,265]	[874]	[2,852]
2016	186,928	28,492	28,798	31,283	16,155	17,647	15,108	9,642	34,046
	[16,733]	[2,551]	[2,616]	[2,815]	[1,450]	[1,601]	[1,357]	[853]	[2,985]
2017	188,260	27,699	29,627	31,802	16,327	18,349	15,260	9,693	34,533
	[16,693]	[2,460]	[2,674]	[2,848]	[1,455]	[1,657]	[1,366]	[853]	[2,941]
2018	199,353	29,139	29,634	33,936	18,192	19,776	15,565	9,947	37,884
	[17,513]	[2,551]	[2,643]	[3,017]	[1,603]	[1,744]	[1,384]	[882]	[3,236]
2019	204,222	28,485	30,972	33,303	17,072	21,314	16,169	10,029	41,272
	[18,916]	[2,703]	[2,908]	[3,132]	[1,626]	[1,985]	[1,510]	[942]	[3,597]
Pooled	1,576,441	237,130	242,369	255,874	146,680	143,226	127,536	83,324	291,655
	[140,593]	[21,218]	[21,992]	[22,976]	[13,235]	[12,893]	[11,457]	[7,440]	[25,147]

Notes: The table gives the weighted number of taxpayers for a given fiscal year and around kink points, using a centered estimation window around the kink point of £20,000. Raw SPI sample size is given between brackets.

TABLE A3.9: BUNCHING AT INCORRECT HIGHER RATE THRESHOLD

	All	London	England	Wales	Scotland	Northern Ireland	Company Director	Not Company Director	Investment Income	Earned Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
2011	-0.011	0.004	-0.012	-0.011	-0.012	-0.011	-0.017	-0.009	-0.003	-0.012
	(0.004)	(0.008)	(0.004)	(0.004)	(0.004)	(0.004)	(0.007)	(0.004)	(0.016)	(0.004)
2012	-0.008	-0.010	-0.008	-0.009	-0.008	-0.008	-0.003	-0.009	-0.001	-0.009
	(0.005)	(0.009)	(0.005)	(0.005)	(0.006)	(0.005)	(0.009)	(0.005)	(0.016)	(0.005)
2013	-0.002	-0.006	-0.006	-0.003	-0.005	-0.003	-0.003	-0.002	-0.021	0.001
	(0.005)	(0.009)	(0.005)	(0.005)	(0.005)	(0.005)	(0.011)	(0.005)	(0.012)	(0.006)
2014	-0.010	-0.011	-0.012	-0.009	-0.012	-0.011	-0.006	-0.010	-0.018	-0.008
	(0.009)	(0.016)	(0.009)	(0.009)	(0.009)	(0.008)	(0.014)	(0.011)	(0.026)	(0.009)
2015	0.001	-0.003	0.001	0.000	0.002	0.001	0.025	-0.005	0.006	0.000
	(0.011)	(0.014)	(0.011)	(0.011)	(0.011)	(0.011)	(0.024)	(0.010)	(0.034)	(0.010)
2016	0.010	-0.002	0.006	0.009	0.006	0.011	0.013	0.009	0.014	0.010
	(0.009)	(0.016)	(0.008)	(0.008)	(0.009)	(0.008)	(0.016)	(0.010)	(0.020)	(0.009)
2017	0.009	0.013	0.007	0.007	0.008	0.011	0.042	0.001	0.013	0.009
	(0.010)	(0.016)	(0.010)	(0.010)	(0.009)	(0.009)	(0.020)	(0.010)	(0.022)	(0.009)
2018	-0.004	-0.001	-0.003	-0.005	-0.003	-0.004	-0.008	-0.003	0.006	-0.006
	(0.012)	(0.019)	(0.012)	(0.011)	(0.012)	(0.012)	(0.016)	(0.014)	(0.020)	(0.013)
2019	0.009	0.023	0.010	0.009	0.010	0.009	-0.001	0.012	0.046	0.005
	(0.014)	(0.022)	(0.014)	(0.014)	(0.014)	(0.014)	(0.026)	(0.015)	(0.025)	(0.015)
Average	-0.002	0.000	-0.003	-0.002	-0.003	-0.002	0.001	-0.003	0.001	-0.002
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)	(0.005)	(0.003)	(0.006)	(0.003)

Notes: Bootsrapped standard errors in parenthesis based on 500 draws.

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