Shocking Wealth:
The Long-Term Impact of Housing Wealth Taxation[1]

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Abstract

Housing is the main asset through which households accumulate wealth and its taxation is highly debated. We provide the first empirical estimates of the long-run effects of shocks to property taxation on lifetime wealth accumulation and investment. To do so, we examine a unique 18th-century tax reform in Holland which resulted in large and unanticipated changes in the effective tax rates on real estate wealth, plausibly exogenous to the owners and different for each property. We collect archival data on the wealth and home-ownership of all 18th-century Amsterdam inhabitants and determine their individual exposure to the shock. We find that the reform capitalized into house values in the short-run and had a large effect on long-run household wealth, with the effect growing over time. On average, a tax increase that implied a 1% drop in the property price led to a 3.5% decrease in wealth-at-death. We show that this large and growing effect is consistent with households not updating housing consumption in response to large tax changes: large positive or negative shocks had few impact on the likelihood of selling voluntarily, even in a liquid market with low transaction taxes. Instead, changes in taxation primarily affected annual saving. The shock had a large impact on foreclosure rates and still affected property-level vacancy and owner-occupancy rates 70 years after the reform. Our findings suggest that shocks to property taxation have large and persistent effects on household wealth and the housing stock, which extend far beyond their direct impact on house prices.

Keywords: housing wealth, wealth accumulation, housing policy, taxation, foreclosures

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1. Introduction

Housing is the main asset through which households accumulate wealth. Its taxation is highly debated. On the one hand, a wide set of tax policies affects the desirability and safety of homeownership and its returns, although the effectiveness of such policies is debated (e.g. Sommer & Sullivan 2018, Goodman & Mayer, 2018). Prominent examples include the exemption of (imputed) rental income from wealth taxation and mortgage-interest deductions. On the other hand, property tax revenue is a major source of revenue for local governments. Effective rates do not only vary across jurisdictions but also across properties within the same jurisdiction. In the United States, property taxes are typically regressive with expensive properties having lower effective tax rates (Amornsiripantich 2021, Berry 2021). In practice, there exist sizable differences in the effective tax rates property owners face on otherwise equivalent housing assets. Such differences might have substantial wealth effects as they directly affect the present value of the net-of-cost flows received by property owners.

In this paper, we investigate how tax-driven housing wealth shocks affect the long-term wealth accumulation of households. To identify this effect, we study a unique 18th-century reform of the property wealth tax in Holland that equalized the effective tax rates on properties, resulting in a large shock to tax rates and real estate wealth that was exogenous to the owners and different for each property. Using archival data from Amsterdam, we show that the 1732 reform had a large impact on the wealth of households that grew substantially over time. We show this is driven by the fact that households barely adjust their housing consumption in response to a large shock, instead adjusting non-housing savings. In line with this, we find the shock had a sizeable impact on foreclosure rates and a persistent impact on the quality of properties and their occupancy.

There are five reasons why this reform is an ideal experiment to measure the impact of tax-driven shocks on housing wealth. First, the tax shock exhibited substantial variation at the property-level and every property-owning household was affected differently. This allows for much more precise identification compared to estimates based on geographic or time-variation in taxes, which are common in the literature. Before the 1732 reform, property wealth taxes were levied based on the rental value in 1632. Taxation was uniform implying all types of real estate holdings were treated the same, although around 85% of property was residential.[2] Due to the enormous growth of Amsterdam in the mid-17th century and the resulting changes to the city and its properties, the 1632 values were soon outdated. Later in the 17th century, laws were passed to update tax values when properties were changed or improved but these updates were not consistently applied until the early 18th century. As a result, effective tax rates differed significantly and persistently across properties. Like today, these tax differences were widely considered unfair but also difficult to change because households priced them when purchasing a property. It took a century until a new tax register was designed based on current rental values, which equalized and updated tax rates across properties, resulting in a major but heterogeneous shock to housing wealth.

Second, conditional on the level of the property's actual rental value, the shock was arguably exogenous. Due to historical reasons, most of discrepancies between the tax register and actual rental values had already arisen before 1700, decades before the reform. An important driver was Amsterdam's expansion after the previous register of 1632. Newly constructed properties received a relatively low appraisal, while the more centrally located properties, already present in 1632, retained a relatively high appraisal. To control for pre-reform wealth, which incorporate some of these locational effects, we consistently including properties' actual rental value in our regressions. The remaining variation is driven by differences in local neighborhood development (roughly one third) and property-specific changes pre-1700 (roughly two-thirds). This was arguably exogenous to owners' economic decisions and outcomes after 1732. Including neighborhood fixed effects leaves our results largely unaffected, suggesting that local neighborhood differences are not driving our results.

[2] For the remainder of the paper, we will refer to 'housing' given that nearly all real estate wealth consisted of residential properties.

Third, the tax reform primarily redistributed the burden of property taxation rather than increasing total revenues, which increased only by a small amount at the provincial level. Relative to other cities, Amsterdam's tax burden increased because the city had become larger and relatively more expensive, but it did not benefit from the additional tax payments. Contrary to many modern systems, property tax revenues were not used to fund local expenditures but instead paid for provincial expenditures that mostly consisted of debt service and defense. Absent a social welfare system, there were no compensatory mechanisms in place for households that lost substantial amounts of wealth due to the shock. This implies that we can largely ignore general equilibrium effects and that any wealth effects we measure were purely the result of the shock.

Fourth, Amsterdam had advanced institutions for registering and taxing personal property and it had a well-functioning housing market. There exist plenty of administrative archival data to track the long-term impact of the shock. To the best of our knowledge, this paper is the first to empirically identify the long-term effects of housing wealth taxation. We have newly-digitized measures of wealth at both death and marriage for all inhabitants of Amsterdam in the 18th century, which we can link to the property tax shock for individuals with unique names. For some individuals, we can also observe their investment portfolios at death. To measure housing market effects, we link the tax shock data on all housing sales in Amsterdam in this period, including foreclosures. We also make use of data on occupancy from the rental census in 1805, allowing us to link the tax shock in 1732 to the long-term development of properties.

Fifth, although our shock happened centuries ago, it shares many characteristics with modern discussions about reforming housing taxation. For example, in New York City, a complicated system of exemptions and valuations implies that properties can face effective tax rates ranging from less than 0.01% to over 2% of market value per year.[3] Politicians and action groups have been calling for reform for decades, but actual reform is not yet in sight. The property tax systems in Germany and the U.K. are even more similar to historical Holland. The German *Grundsteuer* is

[3] See: "How a $2 million condo in Brooklyn ends up with a $157 tax bill", *Bloomberg,* October 14, 2021

based on highly outdated property values: from 1935 in the eastern states and from 1964 in the western states. This results in enormous differences in effective tax rates today.[4] In 2018, the federal court decided this was unconstitutional and forced the government to update the values and taxation. Values are currently being updated and taxation based on it is supposed to start in 2025. A similar situation exists in England, where the *Council Tax* is based on 1991 property values, while house prices appreciation has varied dramatically between different areas (Adam et al. 2020). Our historical experiment informs what happens if such a system does get replaced by a system that taxes housing wealth more equally across owners and properties.

We motivate our empirical analysis with a simple model to highlight the different adjustments property owners can make in response to the reform, and how wealth effects could grow over time. Suppose an owner needs to pay higher taxes on a given property. This has immediate negative wealth affects if the new tax rate is (fully) capitalized into the property value. Further, holding the property constant, the tax shock reduces the owner's disposable income, making it necessary to reduce non-housing savings or consumption. Alternatively, the owner can reduce its housing consumption by moving to a different property. If at least part of the adjustment falls on non-housing savings, the negative wealth effect will grow over time as the owner saves less each period. Finally, a lower subsequent level of non-housing savings might make the owner more vulnerable to other shocks. If the resulting financial distress has additional costs, the wealth effect will grow even stronger over time. Signs of distress could include insufficient upkeep of the property or even foreclosure, if the owner is indeed reluctant to move to a different property.

The main results of the paper are as follows: First, we use the records of the tax reform to determine the magnitudes of the tax shock. Relative to actual rental value net of taxes, Amsterdam households on average annually paid around 5.5% of rental value extra in taxes after the reform, with a standard deviation of 7% at the property and 6% at the household-level. Most properties experienced increasing taxes after the shock but a fifth paid less and thus gained value.

[4] See: "Frist zur Grundsteuererklarung wird verlangert", *Spiegel,* October 13, 2022.

In the paper, we will refer to the magnitude of the shock as the change in housing wealth predicted by full capitalization of the tax changes on rental value.

We then move on to study the effects of the reform on household wealth accumulation. We focus our main analysis on the subset of individuals for whom we can link their exposure to the estate tax record containing their exact wealth-at-death. We find that a one percent predicted decrease in house value due to the tax shock decreased wealth-at-death by approximately 3.5 percent. This indicates that the long-term wealth effect of the shock was much larger than the initial price effect. An individual experiencing the average predicted decline in house value (-5%) would thus lose 17 percent of wealth-at-death. An additional one standard deviation predicted decline in house value (-6%) would reduce wealth by an additional 21 percent. In line with growing wealth effects over time, we show that these effects are much larger for individuals that died long after the shock compared than for those that died shortly after.

The growing wealth effect of the shock over time suggests that households significantly adjusted their non-housing savings in response to the change in taxes. We show that the large impact of the shock on wealth accumulation was primarily driven by changes in non-housing savings: a one standard deviation predicted decline in house value reduced wealth in non-housing savings by about 33 percentage points. For the median individual in the sample holding 3600 guilders in housing and 1000 guilders in other assets at time of the shock and dying 15 years later, a one standard deviation shock implied a decrease of 300 guilders in non-housing savings at death relative to approximately a 350 guilder increase in paid taxes. In line with the growing effect over time, we again find the effect to be larger for individuals that died long after the shock.

Our estimates are all conditional on an individual leaving an estate and thus having registered real or financial assets. In the second part of the analysis, we investigate whether the shock also affected the probability of dying without any financial assets. We show that a one standard deviation predicted decline in house value reduced the likelihood of dying with any financial assets by 4 percentage points relative to base rate of about 60%.

We find suggestive evidence that the key driving factors are the limited adjustment of households' housing consumption and an increase in the likelihood of financial distress. Linking the wealth shock to the housing sale decisions of exposed households, we show that the length of tenure was only weakly affected by the shock, implying few households decided to move immediately in response to a negative shock. This effect is not driven by forced immobility: the Amsterdam housing market was relatively liquid, also compared to today: about 2.5 percent of the total housing stock changed hands per year relative to 2 percent today. At the same time, the shock did strongly affect the probability of foreclosing on the property, which was mostly caused by tax delinquency. Across all properties, a one standard deviation predicted decline in house value increased the probability of selling in foreclosure by around 2 percentage points relative to a base rate of about 8 percent. These effects could be due to the high fixed costs involved with moving, such that owners tried to postpone this as long as they could. The Amsterdam housing market did not feature a foreclosure discount, such that the strategy of waiting for better times rather than selling voluntarily is not necessarily irrational.

Further, we find evidence that higher taxes were associated with less upkeep of the property and lower quality of properties in the long-term. We link the shock to the occupancy of properties in the rental census of 1805. Our intuition is that properties receiving a negative shock should be more likely to be in a bad state in the long-term and thus more likely to be vacant. After the exposed generation passed away, it is unlikely that the new owners would have invested in renovation to undo this shock because Amsterdam experienced a major crisis after 1780 and only started growing in the mid-19th century. We find that a one standard deviation predicted decrease in house value increased the likelihood of vacancy 70 years later by about 1 percentage point, relative to a base rate of around 8 percent. Finally, we show that the shock also had a small but significant negative effect on home-ownership in 1805, in line with the shock significantly affecting foreclosure sales.

The findings of our paper link and contribute to various literatures. First, this paper contributes to the emerging literature on the impact of wealth taxation (e.g. Seim 2017, Jakobsen et al. 2020, Ring 2021, and Brulhardt et al. 2022). These papers show the large impacts of wealth taxation on reported wealth. Relative to these papers, we study the impacts of taxation on wealth accumulation over much longer horizons and focus specifically on the taxation of housing wealth. Housing is generally a middle-class asset implying that the effects of taxation might differ from general wealth taxes that primarily impact those at the top. More importantly, the lack of significant adjustment in housing consumption and investment in response to changes in housing taxation, a key driver of our long-term effects, is likely specific to housing assets. For other financial assets, the costs of such adjustments are arguably much lower. This suggests housing wealth taxes might have very different impacts relative to general wealth taxes. In line with this, existing literature also points to a distinct role for home-ownership in the process of wealth accumulation (Sodini et al. 2021, Bernstein and Koudijs 2021).

Our focus on the tax treatment of housing closely links to a large literature on the impact of fiscal subsidies and taxation on the housing market. Various theoretical papers argue that the mortgage interest deduction and limited taxation of (imputed) rental income are distortive so that tax reforms are generally welfare-improving for households (e.g. Floetotto et al. 2016, Sommer & Sullivan 2018, Boerma 2019). While our empirical analysis confirms the large distortive effects of differential tax treatments of housing investments, these same effects also imply there are large long-term effects when households face unanticipated increases in taxation, such as an increased likelihood of foreclosing and persistent property depreciation. As a result, the long-term impacts of reform are much larger than would be expected based on the short-term wealth shock alone. The does not invalidate the need for reform, but does suggest policy makers need to take adverse side-effects into account.

These findings also align with a large and growing literature on the impact of property taxation and tax delinquency. Various papers document that in many localities in the United States, property tax appraisals are biased, implying the tax system is regressive and also puts a higher burden on minorities (Hodge et al. 2017, Avenancio-Leon & Howard 2019, Amornsiripantich 2021, Berry 2021). LaPoint (2022) documents the system of tax lien sales (sales after tax delinquency) and shows how such sales might be a sizeable contributor to neighborhood gentrification. Wong (2020) and Fu (2022) show that property tax hikes increase the likelihood of respectively mortgage foreclosure and tax delinquency in the short-term, in line with our findings. Beyond our identification strategy, the main contribution of our paper is that we can study the long-term impact of tax-driven wealth shocks and link this to household-level outcomes.

2. Historical Background: The Tax Reform of 1732

During its existence, the Dutch Republic had an advanced system of wealth taxation in place (Fritschy, 2017). The government kept detailed records of property ownership and other personal wealth in *kohieren* that enabled the taxation of wealth. Wealth taxes were sometimes levied on general wealth (general property tax) but more typically (and systematically) on specific asset classes including real estate property. Most wealth was held in government bonds and real estate, which were taxed at similar rates for most of the 18th century. Taxes were generally levied on the cash flows provided by the assets rather than their total value. Relative to total value, taxes averaged around 1.5%.

The main property tax was called the *verponding* and it was levied on the annual rental value of a property. The Province of Holland, which included Amsterdam, developed its property tax register in 1632. For each property in the province, the government obtained the current rental price or appraised it in case the property was not (entirely) leased. Based on this price, the government levied an annual tax rate of 12.5%. Depending on government financing needs, tax rates were incidentally increased on a yearly basis. Further, rates increase secularly over time. By 1732, the year of the reform, the tax had been stable at 27.5% of the 1632 rental value for almost two decades.[5]

[5] Source: Fritschy (2017) and the Amsterdam archives (5044: Archief van de Thesaurieren Extraordinaris and 5039: Archief van de Thesaurieren Ordinaris).

Alike many property tax systems today, with the German and English cases as notable modern examples, the rental values in the tax register were not updated even if they changed. Over time, this resulted in substantial discrepancies in effective tax rates, both within and across cities. These were driven by three factors. First, in the words of contemporaries, there was the "increasing and decreasing states of the economy and welfare of the cities since the formation of the previous register", particularly the "city of Amsterdam that has increased so much in trade, wealth, population that it has become a wonder of the world".[6] Indeed, Amsterdam had grown enormously in the first decades after the 1632 register, with population doubling and the city engaging in an enormous planned expansion of the city between the 1630s and the 1670s (Abrahamse, 2010). This led to substantial discrepancies between the tax register's and the actual rental values. This process likely stopped by the end of the 17th century, when Amsterdam stopped expanding and actual rental values stabilized. Second, newly constructed properties were not appraised consistent with the 1632 valuations, causing further discrepancies (and significant concern among contemporaries). Third, property-level changes and improvements do not seem to have been registered until the early 18th century. Altogether, this led to significant discrepancies at the individual property-level, even over longer horizons.

Although discrepancies between tax and actual rental values were prevalent, reforming taxes was as politically complicated as it was today. Reform only became a serious point on the political agenda in the 18th century, when increases in public debt forced Holland to raise more wealth taxes to pay for debt service. On the 24th of May 1721, the States of Holland concluded that it was unlikely that the cities that fell behind on collecting property taxes would eventually be able to pay all of it, suggesting that "for the future, the tax registers should be strengthened and the losses supplemented with the value of the newly constructed buildings and reclaimed lands." The States of Holland started to request cities to accurately update the tax records for recent construction and improvement, especially the cities that had grown the most: Amsterdam,

[6] Source: Resolutie van de Heeren Staten van Hollandt en Westfrieslandt, vol. 93, p. 51-55, meeting of February 12, 1724.

Rotterdam and The Hague.[7] In the Amsterdam records, there are indeed many small updates to tax values in the 1720s, suggesting the Amsterdam aldermen did start recording smaller changes.[8] However, these updates were limited to properties in neighborhoods that saw substantial recent house price appreciation or construction. The large discrepancies that existed prior were not resolved. For the purpose of this paper, this implies that the remaining tax discrepancies that we observe in 1732 primarily came from property changes that happened long before the actual reform.

Because large tax discrepancies remained, the city of Gorinchem started to push for a complete renewal of the register around 1725.[9] In 1724, only half of the Gorinchem homes that were in the 1632 register still paid taxes: the other half had defaulted on taxation and most of these properties were vacated and in ruins. The city contrasted its experience to Rotterdam and Amsterdam, where strong population and rent increases during the 17th century had only resulted in small increases in tax revenues. Gorinchem's proposals initially did not get full support. The Gorinchem aldermen wrote in November 1726 that "the proposal to come to a renewal of the property tax register was supported by many members, but some have used the same arguments that were (fortunately unsuccessfully) used to prevent the previous renewal in 1632. That is, due to its effect on financial returns: many old homes are too high in the property tax, and many of these properties have been sold, and the price of the sale was affected by the amount of the property tax, so that any change will mean a profit to some owners and a loss to others." In short, reforming the tax implied substantial wealth redistribution, because discrepancies in the tax base been fully priced in. A reform would thus on average hurt the wealth of cities that had seen strong increases in actual rental values, such as Amsterdam.

[7] For example, Resolutien van de Heeren Staaten van Holland en Westfrieslandt, meeting on November 5, 1726 (vol. 95, page 787-788)
[8] Source: Amsterdam City Archives, 5044 Archief van de Thesaurieren Extraordinaris.
[9] Source: Resolutie van de Heeren Staten van Hollandt en Westfrieslandt, vol. 93, p. 51-55, meeting of February 12, 1724 and vol. 95, p. 788-791, meeting of November 6, 1726.

While more than half of Holland's real estate wealth consisted of Amsterdam real estate, it could not prevent reform because every city had one vote. After many reports and meetings, the States of Holland decided on the 24th of March 1729 "that a general update of the tax registers shall be designed, without reducing the total tax revenue". The decision remained hotly debated. Even the French philosopher Montesquieu in the memoirs of his travels to Holland in 1729, discussed the different proposals for tax reform.

The details of the tax reform remained unclear until he final plans were approved in May 1730. The States of Holland decided that all properties would be reassessed to determine their actual rental price. The assessment would be based on actual lease contracts or appraised rental values if a property was (partially) vacant or owner-occupied. In 1731 and 1732, all properties in Holland were assessed and by September 1733 the new tax register was available for inspection. The tax was reduced from an eighth to a one-twelfth of the newly updated rental value. Because properties had appreciated in price over time, overall tax income increased by 10% in Holland. From 1735 onwards, all cities levied taxes based on the new tax register. After the reform, tax rates only briefly increased following the Austrian Succession War in the late 1740s and early 1750s, as part of a general increase in wealth taxes, but stayed at the previous rates until the late 18th century.

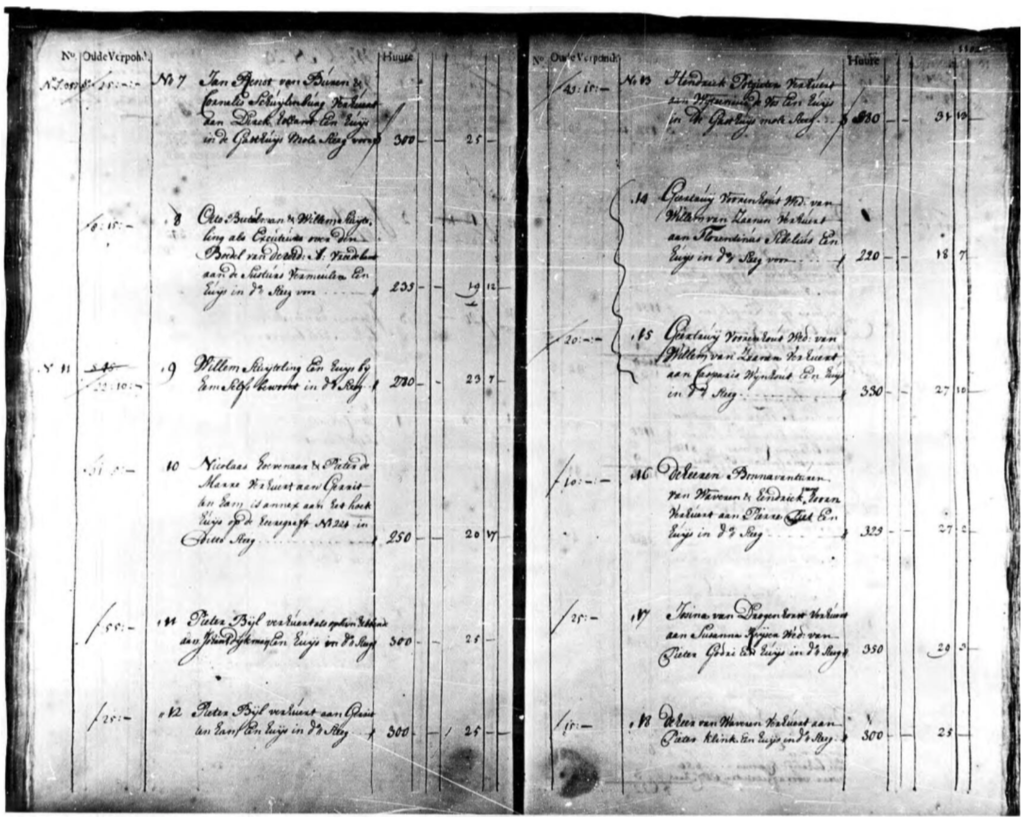
3. Data

Sources

Nearly all data used in this paper originate from hand-written administrative records kept in the Amsterdam City Archives. Part of this data has been digitized by the archives, and other sources we have transcribed ourselves from the original archival records. The first main source is the *kohier van redres*, a register made in 1731 and 1732 containing the name(s) of the owner(s), the value of the old tax and the value of the new tax for each of the 25,926 parcels in Amsterdam. We have digitized this register entirely and verified it with similar registers existing in the Dutch National Archives and the Amsterdam Archives. As a result, we can compute the tax shock for each property and owner. We explain this procedure in more detail in Appendix B, where we also explain how we deal with missing observations and parcels that changed over time.

In Figure 1, we show an extract from the *kohier van redres*. For each property, it lists the old tax, the tax identifier, the name(s) of the owner(s), the current rent or rental value and the new tax. In total, Amsterdam tax revenue increased substantially in the new register, with total taxes increasing by 32% relative to the old amounts. While the formal tax rate had been higher in the previous tax register (27.5%), it was in practice lower because most properties had a much higher rent in the 18th century relative to the old assessment.

Figure 1: Extract from the renewal of the property tax register



The second main source is a database containing the wealth-at-marriage and death for all individuals in Amsterdam in the 18th century. To construct this, we started from the database of all births, marriages and burials in Amsterdam which has been made available to us by the Amsterdam City Archives. Although this database includes millions of records from the period from 1554 until 1810, we focus on the period from the late 17th century onwards. For individuals that married or died between 1699 and 1805, we then digitized data from the register of a wealth tax at marriage and death that was levied during this period. There were five classes, with the lowest wealth class containing individuals without wealth, who paid no tax (class 0, *pro deo*), and the top class individual with wealth over 12,000 guilders. Individuals that held a formal office were assessed on income instead of wealth, except if their wealth put them in a higher tax bracket (Hart, 1973). 80% of couples and 85% of deceased individuals married or died without wealth (including children). Note that dying without wealth implied dying without any formally registered ownership of any financial assets (such as bonds, equity, real estate or accounts at the Bank of Amsterdam). Some of these individuals likely still had some cash savings. We transcribed the names of individuals that had to pay tax and their wealth class (51,403 grooms, 51,403 brides and 115,413 buried individuals) and linked these to the entire set burial and marriage records based on their names and dates of the event; all remaining marriages and burials thus belonged to individuals in the pro-deo class. Because the linking between the records can be done accurately, we can identify the wealth status of all people that died and married in this period with a high level of certainty.

The third main source is the set of housing transactions in this period, introduced in Korevaar (2022). The housing transactions dataset provides details on the names of buyers and sellers, transaction prices, and approximate locations for all properties sold in Amsterdam between the 17th century and 1810.

The fourth main source are estate tax records. These provide detailed information on wealth-at-death and its composition across assets. Estate taxes only had to be paid for individuals that left property to individuals that were not their children, implying they only cover about a quarter to a third of the population that died with any assets. While assessments for the burial tax and marriage tax were based on rough classifications because rates were relatively low, estate taxes were substantial and assessments precise. It included all forms of registered wealth such as real estate, bonds and other securities, loans, etc. Unregistered wealth, such as cash, private equity and movable property were not recorded.

Finally, to estimate the long-term impacts of the shock on the housing stock, we use the rental census of 1805, which we have digitized from the Amsterdam archives. This census indicates for every property the actual rental prices (if leased) and the number of units that are vacant and owner-occupied. They can be easily linked to the 1732 tax register based on their property identifier. Not all census data has survived: data are missing for approximately 25% of neighborhoods.

Data Linkage

The main challenge for our empirical analysis is to link individuals and their property across these different datasets. While we discuss the matching strategies and data processing in detail in Appendix C, we present the key intuition here. In general, all our matching is based on identifying unique matches between names of persons across databases using fuzzy string matching. We identify best matches and define a match if it is perfect or near-perfect and if the next-best match is substantially worse.

We start by linking the owners in the property tax register in 1732 to their marriage records. To do so, we compute Jaro-Winkler distances between each name in the property tax record and all marriages that happened in the years prior to it, as individuals generally did not purchase real estate before marrying. If we find a unique match, we include it in the data. We require matches to be very strict to minimize false positives and corresponding downward bias. [10] We use the same parameters to match marriage records to burial records and estate tax records. In case we cannot establish a unique match to the owner listed in the property tax register, we try to establish a unique match to the death record of the spouse. For matching individuals in the tax register to their marriage and estate tax records, we take less strict values for the match from

[10] We define a unique match if a match has a summed Jaro-Winkler distance based on the first name plus twice the last name of less than 0.10 and there are no other individuals with scores less than 0.10 away from the minimum score.

the tax register to the marriage record.[11] We can take less strict values here because conditional on owning real estate, the probability of having some wealth-at-death is much higher relative to that of the population. Thus, in case we can match an individual in the tax register to a couple that later leaves an estate, that match is likely to be correct.

In total, we can match 6,491 properties with positive rental value to 2,969 different owners, from which we remove 42 owners that own properties with incomplete data. For each owner, we can compute its real estate portfolio and tax shock by aggregating the rental values of all properties owned by the individual. Of these owners, we can link 852 owners to their wealth-at-death (or that of their spouse) and 356 individuals to their actual estate. These numbers only are for individuals that died after 1735, the first year in which taxation was based on the new records.

To link properties in the 1732 tax register to transactions of these properties, we follow the parameters used in Korevaar (2022). We establish a match if an owner only owns a single property on a given street in the 1732 tax register and the name of that owner appears once as a buyer before 1732 and/or once as a seller. For our estimation of sale probabilities, we directly use the repeat-sales dataset from Korevaar (2022), focusing on the subset of 18,573 repeat-sales pairs transacted in the five decades before and after the reform, covering the period from 1682 to 1781. Of these 18,573 pairs, we can link 3,299 pairs to the shock in taxes in 1732. 1,675 of these pairs cover transactions executed by the owners in the 1732 register; other pairs cover earlier or later owners of the same properties. For our analysis of foreclosure rates, we only need to establish a match to the sale of a property by its 1732 owner. Using the same parameters as Korevaar (2022) used to identify repeat-sales, this results in 3,502 linked property sales.

Data description

To obtain a better understanding of the data, Table 1 presents descriptive statistics on some of the key variables we use from (subsets of) the matched datasets. Starting with the entire

[11] The summed Jaro-Winkler distance based on the first name plus twice the last name needs to be less than 0.20. We still require strict matches for the match from the marriage record to burial record and estate tax record.

property tax register of 1732, the average rental price of a property was 312 guilders with a pre-reform annual tax of 38 guilders, which increased to 52 guilders afterwards. At the owner-level, the mean value of a real estate portfolio was 671 guilders and consisted of 2.2 different properties. 56% individuals owned only 1 property. Although we cannot identify whether a property was owner-occupied or not, it is most likely that an owner with multiple properties was living in the most expensive property he or she owned. In the matched sample, most expensive properties on average capture 79% of the total real estate wealth of individuals. This suggests that for the typical owner, the tax shock will fall primarily on owner-occupied property.

Table 1: Descriptive Statistics

Statistic	*Time*	*Obs.*	*Mean*	*St.Dev.*	*Min*	*Max*
Property Tax Register						
Rental Value, property	1732	25,926	311.9	299.2	0	9,270
Annual Tax, post-reform, property	1732	25,926	52	49.9	0	1,545
Annual Tax, pre-reform, property	1732	25,926	38	50.5	0	1,750
Rental value, matched portfolio	1732	2,927	671.1	883.4	12	11,635
Properties, matched portfolio	1732	2,927	2.2	2.3	1	35
Share 1st Property, matched portfolio	1732	2,927	0.79	0.27	0.09	1
Estate tax						
Wealth, with real estate	1733-1735	702	19,701	63,912	42	996,798
Real Estate Share	1733-1735	702	0.81	0.28	0.001	1
Wealth, with real estate, matched	1735-1781	356	29,093	73,594	125	767,524
Real Estate Share, matched	1735-1781	356	0.65	0.38	0	1
Burials and Marriage tax						
Wealthy at Death, matched	1735-1782	852	0.70	0.46	0	1
Years to Death, since 1735	1735-1782	852	14.6	10.8	0	47
Wealth-class-at-marriage, matched	1735-1782	737	1.30	1.43	0	4
Sales/Repeat-sales dataset						
Holding period, matched	1682-1781	1,675	33	17	1	91
Holding period, matched, excl. heirs	1682-1781	599	25.8	17.5	1	84
Foreclosure sale, matched	1735-1811	3,502	0.04	0.18	0	1
Foreclosure sale, matched, excl. heirs	1735-1811	1,118	0.10	0.30	0	1
Rental census						
Home-ownership rate, matched	1805	19,033	0.19	0.35	0	1
Vacancy rate, matched	1805	19,033	0.08	0.25	0	1

To understand the impact of a shock on housing wealth on overall wealth it is important to measure how important housing wealth was for the typical exposed owner in its overall portfolio at time of the shock. To measure this, we look at the average share of wealth in real estate for individuals that died between the construction of the register in 1732 and its introduction in 1735, and that owned any real estate. Conditional on owning real estate, individuals in the estate tax records in this period had on average 81% of their wealth invested in real estate. They had an average level of wealth of around 20,000 guilders. In the matched sample, we find somewhat higher levels of average wealth (29,000 guilders) and lower real estate shares of 65%. There are two reasons for this. First, individuals with unique names were more likely to be rich compared to the overall population of real estate owners, and richer individuals generally left less of their wealth in real estate. Second, for matched individuals we measure their real estate share at death, which generally happened after 1735. The average year of death in the matched sample is 1750. In this period, average real estate shares in the estate tax records were generally lower (77%), because house prices had fallen substantially.

In the broader sample of owners matched to their death records, we find that 70% of matched owners died in possession of any registered wealth. This is of course much higher than the overall rate in the population, because someone owning real estate in 1732 was very likely to die rich. We also find that these individuals were relatively wealthy when they married, with an average wealth class between 1 and 2, although we cannot translate these directly into guilder amounts.

Next, we report statistics on the holding periods and probability of selling in foreclosure for matched sales. On average, people held on to a property for 25 years. When we include the period that an heir held on to an inherited property, this increases to 33 years. On average 3.5% of the properties ended up in foreclosure. This increases to 9.6% when considering the holdings of heirs as well.

Finally, we have information for vacancy rates and home-ownership rates for properties that can be matched to the 1805 rental census. On average, 19% of properties were inhabited by their owners, while 8.4% was vacant (the remainder was rented out).

4. Conceptual framework and link to the data

In this section, we theoretically analyze the immediate and long-run wealth effects from an unanticipated tax shock.

Setup and assumptions

There is a group of property owners $i \in I$ who all have the same preferences, income and (initial) wealth. Each owner i is on the following intertemporal budget constraint (IBC):

$$y_t + \rho_t S_{i,t} = c_{i,t} + \tau_{i,t} + \Delta S_{i,t+1}, \tag{1}$$

with y_t wage income, $S_{i,t}$ non-housing savings at the beginning of period t that give return ρ_t, $c_{i,t}$ non-housing consumption, $\tau_{i,t}$ the tax paid on the property, and $\Delta S_{i,t+1}$ the change in non-housing savings over period t. The property tax is given by a fraction α_i of the rental value $r_{i,t}$: $\tau_{i,t} = \alpha_i r_{i,t}$. We assume that each owner fully consumes the property's rental value. Wealth W_t is given by

$$W_t = S_{i,t} + H_{i,t}, \tag{2}$$

where $H_{i,t}$ is the property value. There is no debt in the model.

We assume that all owners are on their optimal path, denoted by an $*$, with the same rent r_t^*, non-housing consumption c_t^*, and flow of non-housing savings ΔS_{t+1}^*. They only differ in the tax rates α_i that they pay on the property. Since all else is equal, this implies that certain owners have more of their wealth in non-housing savings such that, for any two owners $i \neq j$,

$$(\alpha_j - \alpha_i) r_{i,t}^* = \rho_t (S_{j,t}^* - S_{i,t}^*) = \rho_t (H_{i,t}^* - H_{j,t}^*), \tag{3}$$

where the second equality follows from the fact that each owner i has the same wealth. This identity is consistent with property prices being equal to:

$$H_{i,t} = \frac{(1-\alpha_i) r_{i,t}}{\rho_t} = \frac{r_{i,t} - \tau_{i,t}}{\rho_t}, \tag{4}$$

the net-present value of future rents minus taxes.

The effects of an unanticipated tax shock

Right before the beginning of period $t+n$, there is an unanticipated tax shock equalizing the tax rates on all properties to $\bar{\alpha}$. This is a negative shock for some, and a positive shock for others. We express the wealth effect as the difference between each owner's adjusted path, denoted by a hat (^), and its original optimal path: $\Delta_{i,t+n+\sigma} = \widehat{W}_{i,t+n+\sigma} - W^*_{t+n+\sigma}$, where σ is the time horizon over which we analyze the impact of the shock. Absent the tax shock, all owners would have been on the same optimal wealth path, and $\Delta_{i,t+n+\sigma}$ perfectly describes the shock's heterogenous effect.

The unanticipated tax shock has an immediate effect on wealth through the change in the property value:

$$\Delta_{i,t+n} = \widehat{W}_{i,t+n} - W^*_{t+n} = \frac{(\alpha_i - \bar{\alpha}) r^*_{t+n}}{\rho_{t+n}}. \tag{5}$$

Further, holding the property constant, a different tax rate means a change to disposable income equal to $(\alpha_i - \bar{\alpha}) r^*_{t+n}$. For expositional purposes, suppose owner i is faced with a higher tax rate. In response, the owner can reduce its non-housing savings or consumption. Alternatively, the owner can move to a house with a lower rental value. We assume this would happen right after the tax shock. This reduces taxes while the net sale proceeds sale can be invested in non-housing savings that generate a return, which further loosens the budget constraint. Finally, it can pick a combination of these possible responses. In sum:

$$(\alpha_i - \bar{\alpha}) r^*_{t+n} = \left(\widehat{\Delta S}_{i,t+n+1} - \Delta S^*_{t+n+1}\right) + \left(\hat{c}_{i,t+n} - c^*_{t+n}\right) + \left(\hat{r}_{i,t+n} - r^*_{t+n}\right), \tag{6}$$

where $\widehat{\Delta S}_{i,t+n+1}$ excludes the (possible) net sale proceeds from moving to a house with a lower rental value. (The proof is in Appendix A).

Subsequent differences in wealth accumulation depend on what dimension(s) owner i decides to adjust. Suppose that fraction $\zeta \in (0,1]$ of the decrease in resources is permanently compensated by a reduction in non-housing savings. Then, the wealth effect of the shock will grow over time as the owner now saves less than it otherwise would have. Under the assumption that it makes no further adjustments and does not experience any additional shocks (and holding ρ_{t+n} constant), the difference in wealth will develop over time as:

$$\Delta_{i,t+n+\sigma} = \widehat{W}_{i,t+n+\sigma} - W^{*}_{t+n+\sigma} = \frac{(\alpha_i - \bar{\alpha}) r^{*}_{t+n}}{\rho_{t+n}} \underbrace{\{1 + \zeta[(1 + \rho_{t+n})^{\sigma} - 1]\}}_{\mu_\sigma \geq 1}. \tag{7}$$

In what follows, we will refer to the second half of this expression as multiplier μ_σ, which will be larger than 1 and increasing over time as long as $\zeta > 0$.

Financial fragility and distress

After the initial unanticipated tax shock, property owners might be hit by other shocks. As long as owners are ex ante identical, the incidence and size of shocks should not systematically differ between owners who received different initial tax shocks. However, the response to these shocks might be different. For example, suppose that owner i had to pay a higher tax rate on its property to which it responded by lowering non-housing consumption and savings rather than moving to a lower rental value property. Compared to a different owner j who did not experience a tax increase, owner i will now be more vulnerable to further shocks. While owner j is able to absorb new shocks by using its stock of non-housing savings or reducing non-housing consumption, owner i is less able to do so. Its stock of non-housing savings will be lower and its non-housing consumption might already be close to some minimal level. If owner i remains unwilling to move to a property with a lower rental value, it might fall behind on its tax payments or become indebted, which could push the owner into financial distress. If this involved additional costs, and if a longer σ implied more shocks, multiplier μ_σ might be larger than suggested by equation (7). Signs of distress can include insufficient upkeep of the property or even foreclosure.

Link to the data

In the model, we assume that all property owners initially have the same wealth and own a property with the same rental value. Under this assumption, we can directly link differences in wealth effects $\Delta_{i,t+n+\sigma}$ between different owners i to differences in the unanticipated tax shock. For example, for any two owners $i \neq j$, the difference in wealth effects is given by:

$$\Delta_{i,t+n+\sigma} - \Delta_{j,t+n+\sigma} = \frac{(\alpha_i - \alpha_j) r^*_{t+n}}{\rho_{t+n}} \underbrace{\{1 + \zeta[(1 + \rho_{t+n})^\sigma - 1]\}}_{\mu_\sigma \geq 1}, \quad (8)$$

which follows directly from equation (7).

Though this assumption is unlikely to hold exactly, empirically we only need that initial differences in wealth, income, and the property's rental value are the same *on average* for owners receiving different shocks. Or framed differently, we need that differences in the characteristics of owners i are uncorrelated with the tax shock $\alpha_i - \bar{\alpha}$. A small fraction of the tax shock can be explained by the 1732 (updated) rental value, which correlates strongly with wealth. We can adjust for this by controlling for observable characteristics, in the particular the 1732 rental value or wealth-class-at-marriage fixed effects. Comparing our estimates with or without controlling for observable characteristics gives an indication how strong unobservable differences would need to explain the patterns we see in the data.

Econometric specifications

In our estimates, we define s_i as the predicted log-change in the property price assuming that taxes are fully capitalized, as suggested by the descriptive historical evidence.[12] Following the net present value relation from equation (4), this is given by

$$s_i = log\left(\frac{r_{i,1732} - \tau_{i,new}}{r_{i,1732} - \tau_{i,old}}\right) = log\left(1 + \frac{\tau_{i,old} - \tau_{i,new}}{r_{i,1732} - \tau_{i,old}}\right) \quad (9)$$

where $\tau_{i,old}$ are old tax payments, $\tau_{i,old}$ are new tax payments, and $r_{i,1732}$ is the property's updated 1732 rental value. To determine the wealth effect of the shock, we estimate various forms of the following cross-sectional regression for each individual *i:*

[12] In Appendix D, we provide suggestive evidence for capitalization based on the prices of properties sold around the reform; given that standard errors are large we cannot do any precise inference though.

$$Wealth_{i,t} = \beta\, s_i + \chi\, Wealth_{i,initial} + \lambda_i + \mu_t\ \ \varepsilon_i \tag{10}$$

We define the tax shock s_i similar to equation (9) except that *i* now refers to persons rather than properties, where we aggregate the tax shock over (possibly) multiple properties. In this regression, β is our main parameter of interest and captures the effect on wealth for individuals with different exposure to the shock, in line with equations (7) and (8) in the theoretical model. In our main specification, we use the log of wealth-at-death as dependent variable, where $t > 1734$, so we only include observations after taxation based on the new register started. Due to the spatial nature of the shock, the shock will correlate with local measures of property values which are likely indicative for wealth. To control for any potential correlations between the shock and individuals' initial wealth, we either include the (updated) rental value of their real estate in 1732, wealth-class-at-marriage fixed effects and/or neighborhood fixed effects (λ_i). The latter are based on sixty neighborhoods identified in the tax records.[13] To control for secular changes in the economic environment, we include year of death fixed effects in five-year bins, μ_t.

If we estimate equation (11) on a sample of individuals dying immediately after the shock, we would expect β to equal the capitalization factor from the regression in equation (10) scaled by the fraction of wealth those individuals held in Amsterdam real estate at time of the shock. For individuals dying later after the shock, we would expect β to be larger, scaled by multiplier μ_σ from equation (7) and (8) that grows over time. To test for this, we first report estimates separately for individuals dying before or after 1748 (the median year-of-death in our sample), where we expect the latter to have a bigger β. Second, we linearly interact β with the number of years between 1732 and the year-of-death. The first method is robust to possible non-linearities, while the second provides the average annual rate at which coefficient β increases.

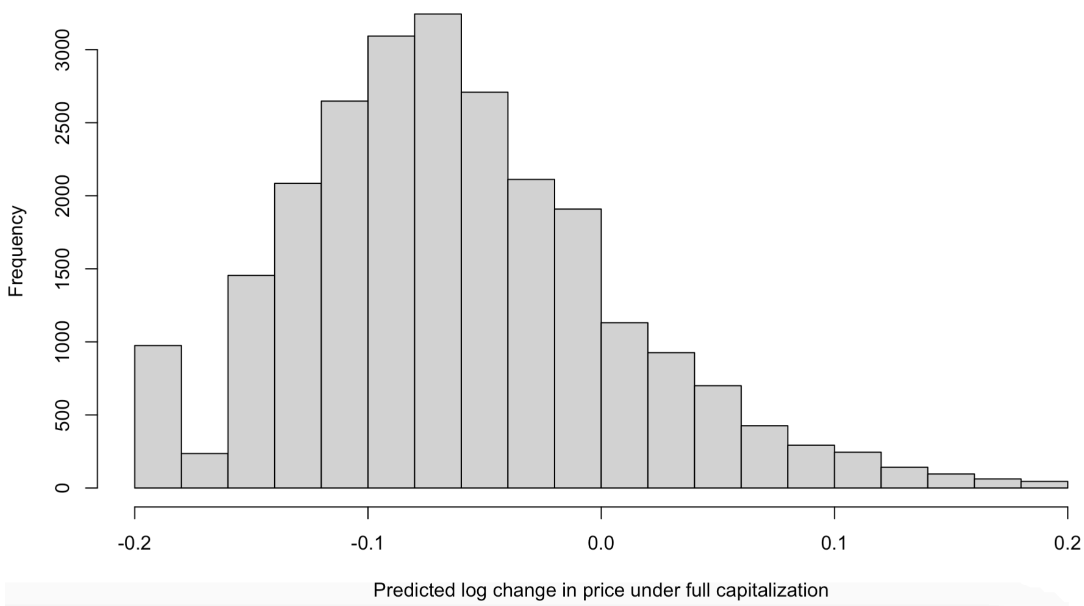
[13] In the rare case someone owns properties in different neighborhoods we use the neighborhood containing the most valuable property, since this likely corresponds to the neighborhood where the owner lives.

5. Results

The shock

In equation (9), we define the tax shock as the predicted log-change in the property price assuming that the change in taxes is fully capitalized. We plot the tax shock for every property in Figure 2. We exclude properties owned by institutions as well as 45 outlier properties where the pre-shock was more or very close to the 1732 rental value, leaving us with 24,766 properties. Note that Figure 2 does not depict the 1% of properties that experienced an implied value gain of more than 20% of rental value (though these properties remain in the dataset).

Figure 2: The tax shock (property level)



Under full capitalization, the average shock is 0.06 log points, the standard deviation of the shock is 0.08 log points. At the owner level, the standard deviation is slightly smaller (0.07) because some individuals owned multiple properties. In total, 18.4% of properties gained value and about 3.9% of properties lost the maximum value of -0.182 because they were untaxed in the previous register or not correctly identified. Overall, under full capitalization, the tax reform implied substantial immediate changes in total wealth, given the dominant position of real estate in households investment portfolios.

What drove the variation in the wealth shock? In Section 3, we argue that discrepancies between the tax register and actual rental values in 1732 had largely arisen before 1700. Part of the variation comes from Amsterdam's expansion during the 1600s. In particular, newly developed areas on the city's outskirts received artificially low appraisals, while properties in the old city center, which had been built before 1632, retained relatively high appraisals. Since these centrally located properties were generally worth more, this suggests that higher valued properties received a smaller negative shock. The remaining variation comes from differences in local neighborhood development and property-specific improvements pre-1700, both arguably exogenous to homeowners economic condition and decisions after 1732. In the empirical analysis, we always condition on a property's actual rental value to control for general neighborhood differences. In some specifications, we include highly granular neighborhood fixed effects. This leaves our results largely unaffected, suggesting that local neighborhood differences are not driving our results.

Table 2: Determinants property-level shocks

	Dependent variable:		
	Shock		
	(1)	(2)	(3)
log(Rental Value, 1732)	0.009^{***}	-0.0001	-0.012^{***}
	(0.001)	(0.001)	(0.001)
Shock, Immediate Neighbors			0.436^{***}
			(0.008)
Constant	-0.068^{***}		
	(0.002)		
Neighborhood FE	No	Yes	Yes
Street FE	No	No	Yes
Observations	24,770	24,770	24,768
R^2	0.01	0.157	0.359
Note:	$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$		

Table 2 illustrates where the variation in the tax shock comes from. Column 1 confirms the positive correlation between the rental value of a property in 1732 and the tax shock it received. Column 2 shows that this correlation mostly disappears after controlling for neighborhood fixed effects, confirming that part discrepancies between the tax register and actual rental values was driven by the expansion of Amsterdam and the differential impact this had on its neighborhoods. Column 3 combines the neighborhood fixed effects with the most granular location control: street name fixed effects and the tax change on immediately neighboring properties (which sometimes had the same owner). Unsurprisingly, we find a strong positive relation between these. Altogether, these highly granular location effects explain about 35% of the variation in the shock. The remainder is property specific. Adjusting for hyper-local location values turns the relation between rental value and the shock negative. This illustrates the impact of property-level changes: individual properties that were improved after the 1632 valuation likely had higher 1732 rental values relative to their 1632 rental values, and were thus more likely to receive a negative shock.

Wealth effects

We now explore wealth effects, estimating equation (10) on the sample of individuals that can be linked to their estate tax record. Table 3 reports the main results. Starting from Column 1, where we estimate the effect of the shock controlling for log rental value, we find that a one percent predicted decrease in property value due to the tax shock decreases wealth-at-death by approximately 3.4 percent. This effect is significantly larger than the immediate wealth effect of the shock, which under full capitalization and the real estate wealth shares in 1735 would be around 0.8 percent. This effect is also economically sizable: a one standard deviation decrease in the predicted property value decreases lifetime wealth by 20 percent, of which less than a third is driven by the capitalization effect. Unsurprisingly, we find a close correlation between wealth-at-death and real estate value in 1732. In Columns 2 and 3, we investigate how these estimates change as we add controls. First, we control for year of death using five-year bins (Column 2). We find this results in a very similar estimates. In Column 3, we report the most extensive specification that additionally includes neighborhood fixed effects. This does not result in significant changes to our main coefficient of interest, which suggest that secular trends in wealth accumulation across neighborhoods in combination with possibly different mortality rates are not driving our results.

Table 3: Wealth-at-death

	Dependent variable:							
	log(Wealth-at-death)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	*All*	*All*	*Strict Matching*	*Single Property*	*Marriage Match*	*All*	*All*
Shock	3.447***	3.728***	3.101**	2.675**	3.919**	3.557***	1.105	-0.224
	(1.185)	(1.190)	(1.438)	(1.041)	(1.754)	(1.200)	(1.583)	(1.947)
Shock x Death after 1748							5.594**	
							(2.247)	
Shock x Years since Shock								0.268**
								(0.105)
log(Rental Value, 1732)	0.821***	0.844***	0.908***	0.800***	0.925***		0.838***	0.840***
	(0.080)	(0.080)	(0.092)	(0.096)	(0.174)		(0.079)	(0.079)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Burial Year FE (5y)	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood FE	No	No	Yes	No	No	No	No	No
Marriage Class FE	No	No	No	No	No	Yes	No	No
Observations	356	356	356	252	170	315	356	356
R^2	0.215	0.254	0.442	0.278	0.271	0.368	0.307	0.308
Adjusted R^2	0.042	0.250	0.295	0.244	0.220	0.338	0.283	0.284
Note:						*p<0.10**p<0.05***p<0.01		

From Column 4, we estimate the effect separately for different subsamples or splits of the data. We exclude the neighborhood fixed effects in these estimates because including them does not materially affect our results while decreasing degrees of freedom significantly, which is particularly costly on subsamples. In Column 4, we use an even more strict matching approach to link persons that we use in the analysis in the next table; this reduces the number of observations substantially. In Column 5, we restrict the sample to owners that only held a single property or whose additional property was of minimal rental value (less than 20% of their real estate wealth). The effect in Column 4 is slightly smaller whereas it is slightly larger in Column 5. Although the difference is not significant, this is not surprising: individuals with more unique names are typically wealthier and hold less of their wealth in real estate, whereas individuals that only own one property are typically less wealthy. All else equal, we expect larger effects on wealth at death for individuals that invest most of their wealth in real estate. In Column 6, we control for pre-shock wealth using estimates of wealth-at-marriage (for marriages before 1735) instead of the log rental value in 1732. We find similar effects of the shock when using this measure to control for pre-shock wealth but the number of observations reduces slightly as some individuals married before the introduction of the marriage tax.

Finally, we test whether the effect of the shock was growing over time. We use two methods. First, we split the sample in two parts, covering individuals that died until 1748, the median year-of-death in the sample, and those that died after 1748. The basic intuition is that individuals in the first group died shortly after the shock, on average in 1741, and thus cannot have lost too much wealth on top of the direct capitalization effect. The story is different for those that died after 1748, who, on average, passed away on average in 1760 and thus paid higher-than-expected taxes for a longer period. So if $\zeta > 0$, the multiplier μ_σ in equation (8) will be substantially larger for these individuals. Further, individuals dying after 1748 all experienced major turmoil in the housing market. House prices declined substantially in the 1740s, bottoming out in the early 1750s, in response to the Austrian Succession War as well as to major increases in taxes on real estate and wealth more generally in the late 1740s and early 1750s (Korevaar, 2022). If the 1732 tax shock increases financial fragility, we expect a larger effect for individuals that experienced this turmoil. Second, we interact the shock with a simple linear time trend.

The coefficients in Columns 7 indicate that the impact of the shock on wealth-at-death for the group that died before 1748 is similar to the direct capitalization effect, although we cannot measure it precisely. We find an economically large and statistically significant additional effect for individuals that died after 1748. For this group, a one standard deviation decrease in the predicted property value decreased wealth-at-death by over 35%. In Column 7, we test for a linear time trend and find that the wealth effect is primarily driven by compounding over time rather than an immediate wealth effect: a 1% shock increases wealth by 0.27% per year. This is a useful estimate, as it gives some indication how fast the effect increases over time, but the effect is probably not exactly linear. The standard error on the interaction term is also relatively large. Further, the turmoil in the late 1740s and 1750s might have introduced non-linearities in the effect that are hard to capture with a single term.

To delve into more detail what is driving the growing effect over time we next separate the effect on housing wealth and non-housing wealth. In the model, we show the effect of the shock grows over time if households respond by adjusting non-housing savings rather than housing consumption. One challenge in estimating the response of non-housing savings is that 40% of individuals in the sample do not have any recorded non-housing assets, implying our estimates will be sensitive to how we deal with these zero observations. For housing assets, this is only the case in 15% of estates.

Table 4: Impact of the shock on housing and non-housing wealth

	Dependent variable:					
	log(Non-Housing Wealth)				log(Housing Wealth)	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mod.	*Mod.*	>0	>0	>0	>0
Shock	2.510	1.424	1.814	0.290	3.143^{**}	3.236^{*}
	(2.079)	(2.562)	(2.409)	(2.851)	(1.506)	(1.851)
Shock x Death after 1748	6.794^{**}		5.985^{*}		1.055	
	(2.952)		(3.386)		(2.023)	
Shock x Years since Shock		0.289^{**}		0.293^{**}		0.028
		(0.138)		(0.148)		(0.092)
log(Rental Value, 1732)	1.006^{***}	1.009^{***}	0.726^{***}	0.733^{***}	0.753^{***}	0.754^{***}
	(0.104)	(0.104)	(0.119)	(0.119)	(0.068)	(0.068)
Burial Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	356	356	214	214	314	314
R^2	0.282	0.281	0.233	0.236	0.336	0.335
Note:					$^{*}p<0.10^{**}p<0.05^{***}p<0.01$	

From Faber (1980), who studied Amsterdam probate records in the 18th century, we know individuals generally owned some cash and movable property that was not registered in the estate tax records. Because probate records are a selected sample, it is nonetheless difficult to assess how large such wealth was. In our baseline specification, we assume that for every individual, 15% of their total wealth was not registered and we add this to their non-housing wealth. In an alternative specification, we use registered housing- and non-housing wealth only but exclude individuals with zero values. Table 4 reports the results of the analysis. In all results, we control for rental value in 1732 as well as year-of-death fixed effects.

The first column indicates that a one percent predicted decrease in property value led to an average reduction log non-housing savings of about 2.5 percent for individuals that died before 1748 and about 9.3 percent for individuals dying after 1748. In the second column, we again use a linear term and show that a one percent predicted decrease in property value led to an average reduction of non-housing savings by about 0.29 percent per year. In Columns 3 and 4, we repeat the same specification but instead focus on the subset of individuals with non-zero non-housing wealth at death. This results in virtually identical estimates compared to Columns 1 and 2. Finally, Columns 5 and 6 repeat the specification in Columns 3 and 4 except using log housing wealth as dependent variable and focusing on the subset with positive housing wealth at death. On average, a one percent predicted decrease in property value let to an average decrease in housing wealth of about 3 percent. Although this is larger than the capitalization effect alone, the estimate is not significantly different from it. A larger effect could also be rationalized if the shock affected the upkeep of the property. More importantly, we find that the effect on housing wealth does not grow significantly over time and the economic effect is comparatively small. In short, this suggests most of the growing wealth effect over time is driven by adjustment in non-housing savings.

Both in our analysis in Table 3 and Table 4 we have conditioned on individuals that died with any registered wealth. We now move to analyze the larger sampled of individuals for whom we can match to the burial tax records, studying to what extent the shock also affected the probability of dying with any registered assets. The dependent variable is thus now a dummy for whether the individual paid burials tax. The results are in Table 5. The outline of Table 5 follows the outline of Table 3 exactly except that all results are based on strict matching. We do this because we have a larger sample and because the outcome variable is measured with more noise such that exact matching becomes more important.

Table 5: Wealthy-at-death

	Dependent variable:						
	Wealthy at Death (dummy)						
	(1)	(2)	(3)	(5)	(6)	(7)	(8)
	All	*All*	*All*	*Single Property*	*Marriage Match*	*All*	*All*
Shock	0.714***	0.750***	0.878***	0.552**	0.575**	0.649**	0.586
	(0.245)	(0.212)	(0.241)	(0.254)	(0.226)	(0.314)	(0.396)
Shock x Death after 1748						0.183	
						(0.415)	
Shock x Years since Shock							0.010
							(0.020)
log(Rental Value, 1732)	0.100***	0.105***	0.113***	0.176***		0.105***	0.105***
	(0.016)	(0.016)	(0.018)	(0.031)		(0.016)	(0.016)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Burial Year FE (5y)	No	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood FE	Yes	No	Yes	No	No	No	No
Marriage Class FE	No	No	No	No	Yes	No	No
Observations	852	852	852	456	737	852	852
R^2	0.100	0.058	0.150	0.099	0.105	0.067	0.067
Adjusted R^2	0.026	0.055	0.068	0.077	0.088	0.054	0.054
Note:					*p<0.10**p<0.05***p<0.01		

On average, a one percent predicted decrease in property value due to the tax shock decreases the probability of dying with any (registered) wealth by 0.7 percentage point. A one standard deviation decrease leads to a drop of 4.2 percentage points. Relative to the sample average of 64 percent, this is economically meaningful. This effect is somewhat but not significantly smaller when controlling for wealth using wealth-class-at-marriage or for individuals with a single property (or most wealth in a single property). In Columns (5) and (6), we find that the economic magnitude is somewhat larger for individuals dying later but differences are not statistically significant. This is perhaps not surprising because the dependent variable is naturally bounded. For example, if an individual receives a negative shock and loses registered property in period x, this effect will remain constant for periods $y > x$.

6. Mobility adjustment

Our results in the previous section show that the tax shock had a large effect on wealth-at-death. This effect exceeds the capitalization of the shock in property prices and is increasing over time. In this section, we empirically study which mechanisms could be driving these results using our theoretical discussion in Section 4 as guideline.

Adjusting housing consumption: evidence from property sales

First, we study to what extent owners move in response to the shock, implying they adjust their housing consumption, the third term in equation (6). The most direct way to test this is to look at the frequency of housing sales. The larger the absolute value of the shock, the farther an individuals is pushed away from its ex ante optimal consumption and savings path, implying we expect this to increase the likelihood of selling.

Before moving to the analysis, we should note that in most housing markets, only a small percentage of the housing stock trades hands each year. In general, the significant fixed cost of buying and selling a house might also prevent households from moving in response to shocks. Beyond costly housing search, transactions costs and moving costs can easily add up to a significant fraction in property value. Such costs were not particularly high in Amsterdam; the stamp duty on housing was about 2.5%, which is similar to the 2% on owner-occupied housing in the Netherlands today. Brokerage fees were low and homes could be sold at low costs in public auctions organized regularly by the city. Few households purchased their property with a mortgage and there were no fiscal frictions affecting the decision to buy or rent. Finally, the housing market was also fairly liquid, with turnover rates somewhat higher than the modern Amsterdam housing market. Conditional on ever observing a repeat-sale in our database, the average holding period was 20 years. This implies that for the mobility channel to be an economically important in equation (6), we would need to see very drastic increases in selling hazards due to the shock.

We use the set of repeat-sales that can be matched to individuals in the 1732 register and test whether the absolute value of the shock affected the selling hazard based on a Cox proportional hazards model. The main dependent variable is the holding period and the main independent variable of interest is the absolute value of the tax shock. We only focus on repeat-sales pairs of owners that were exposed to the shock ($|s_{i|exposed}|$) and exclude properties sold in foreclosure. This sample contains both individuals that actively sold their property before they died (554 individuals) and individuals whose heirs sold the property after death (1,070 individuals). Sales typically only list the name of the male owner and only mention the name of his wife if she sells the property after he has been deceased.

Results are reported in Table 6. The most basic specification is reported in Column 1, which shows that an increase in the absolute value of the shock increases the selling hazard of each property *i* with owner *j*, thus decreasing the holding period. This sample only includes owners that sold during their lifetime and who actively made the decision to sell themselves. The estimated hazard ratio of 5.8 implies that a predicted shock to the property value with an absolute value of 6 percent, the mean value in the sample, increases the likelihood of selling by 35%. This effect does not change much if we control for year of purchase and for the rental value of the property (Column 2) and if we exclude properties that saw their taxes decline (Column 3) but is generally insignificant. In Columns 4—6, we repeat the same specifications but we instead use the sample that also include sales by heirs, and estimate the effect on heirs separately with an interaction term. Adding these sales gives us more power, and we accordingly find slightly larger and more significant coefficients, with the hazard ratio increasing to 8, but the difference is not major relative to Columns 1—3. Unsurprisingly, properties of heirs have lower sale hazards and are bought earlier. Adding the coefficient on the interaction term with the main coefficient on the shock, we find no significant relation between sale hazards and the shock for inherited property.

This is not surprising: there is no clear reason why the decision to keep or sell an inherited property should depend on the taxes the heiress paid in the past. In line with our theoretical framework, this suggest the effect on the moving decision was primarily driven by those occupying the property at the time of the shock.

Table 6: Hazard rates of selling properties, Cox proportional hazard model

	Dependent variable:					
	Holding Period (hazard model)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Alive	Alive	Alive $s_i < 0$	All	All	All, $s_i < 0$
$\lvert s_{i\|exposed} \rvert$	1.754*	1.393	2.067	2.100**	2.085**	2.524*
	(0.968)	(0.884)	(1.281)	(1.006)	(0.921)	(1.315)
Hazard Ratio:	*5.78*	*4.03*	*7.90*	*8.17*	*8.05*	*12.48*
$log\ (RentalValue_{1732})$		-0.144**	-0.148**		-0.100***	-0.122***
		(0.063)	(0.075)		(0.036)	(0.044)
Hazard Ratio:		*0.87*	*0.86*		*0.90*	*0.89*
Year of Purchase		0.063***	0.069***		0.057***	0.059***
		(0.006)	(0.007)		(0.003)	(0.003)
Hazard Ratio:		*1.07*	*1.07*		*1.06*	*1.07*
$Heirs_j$				-0.364***	-0.156*	-0.165
				(0.097)	(0.093)	(0.131)
Hazard Ratio:				*0.69*	*0.86*	*0.85*
$s_{i\|exposed} \times Heirs_j$				-1.767	-2.251**	-1.996
				(1.174)	(1.093)	(1.603)
Hazard Ratio:				*0.17*	*0.11*	*0.14*
Observations	554	554	449	1,624	1,624	1,276
R^2	0.006	0.232	0.264	0.051	0.283	0.294
Wald Test	3.2090	122.070	111.130	90.650	462.650	372.580

In short, while the tax shock does seem to have resulted in a slight increase in the sale rate of, most households did not sell their property in response to the shock, given that sale rates were only a few percent year. This implies that most households adjusted other margins after facing changes in taxes.

Forced adjustment and financial fragility: effects on foreclosures

In the previous analysis, we only focused on the sale hazard for properties that eventually sold in a regular sale. However, households that experienced increasing property taxes but remained in their houses might have been increasingly unable to pay these taxes. If home-owners were delinquent on their taxes, the city would proceed to sell the property in a foreclosure procedure. The proceeds would be used to pay off outstanding tax payments and any other claims creditors had on the property. Table 7 tests whether an increase in property taxes indeed led to a higher probability of foreclosure. The dependent variable is a dummy for whether a sale is a foreclosure or not. We only include properties that we can match to a sale, since we cannot identify whether we were unable to match to a sale because it did not take place or because we cannot match it. Following our earlier analysis, we use samples both including and excluding sales from heirs, and only include sales were we can make a unique link between the tax register and the sale. The results are in Table 7.

Table 7: Foreclosure sales

	Dependent variable:				
	$Foreclosure_{i,j,t}$				
	(1)	(2)	(3)	(4)	(5)
	Alive	Alive	All	All	All
$s_{i\mid exposed}$	-0.263**	-0.263**	-0.263***	-0.236***	-0.218***
	(0.112)	(0.112)	(0.068)	(0.067)	(0.069)
$Heirs_j$			-0.077***	-0.083***	-0.081***
			(0.008)	(0.008)	(0.008)
$s_{i\mid exposed} \times Heirs_j$			0.277***	0.240***	0.252***
			(0.085)	(0.084)	(0.084)
$\log(RentalValue, 1732)$		-0.004	0.002	0.003	0.002
		(0.012)	(0.004)	(0.004)	(0.005)
Constant	0.083***	0.095**	0.078***		
	(0.010)	(0.040)	(0.015)		
Year of Sale FE	No	No	No	Yes	Yes
Neighbourhood FE	No	No	No	No	Yes
Observations	1,118	1,118	3,502	3,502	3,502
R^2	0.005	0.005	0.056	0.120	0.149
Note:			*p<0.10, **p<0.05, ***p<0.01		

Column (1) shows that properties with a one standard deviation predicted decrease in their value are about 1.8 percentage points more likely to be sold in foreclosure while the owner is still alive. This effect is economically significant: the baseline probability that a sale by the original owner is a foreclosure is 8.4 percent in the matched sample. Column (2) includes the 1732 rental value as control. The estimate remains the same. Most sales in the matched sample are executed by heirs of the same property, which we add to the sample in Column 3. Unsurprisingly, heirs seldom face foreclosure. The constant in the regression and the coefficient on the $Heirs_j$ dummy effectively sum up to zero. Further, the sum of the main coefficient on $s_{i|exposed}$ and the interaction effect is effectively zero. If heirs realized they would not be able to afford the taxes on the property, they would sell right away, rather than waiting for foreclosure. Because the probability of foreclosure was not independent of time and space, Columns 4 and 5 add year of sale and neighborhood fixed effects respectively, which do not lead to major changes in the coefficients.

Long-term effects on property occupancy

Beyond the effect of the shock on measured wealth accumulation and sales, there also might be effects on properties themselves. If households were facing increased tax burdens they might have reduced the amount of money they invested in the upkeep of the property. In the latter case, housing consumption adjusts a little bit over the long run because the property depreciates more over time, leaving households with less wealth and less housing consumption the longer they undermaintain the property. Relatedly, if the shock led to foreclosure, this distress itself might have accelerated depreciation of the property while also increasing the likelihood of investor ownership, who purchased distressed property in auctions.

To test for any such effects we link the shock to long-term vacancy rates of properties in 1805. In 1805, the city made a register that listed for every housing unit in the city whether it was vacant or not. If higher taxes imply less investment in improvements or maintenance, we would expect that the property depreciates more and has a higher likelihood of vacancy in 1805. This channel is in line with some of the anecdotal evidence provided by the Gorinchem aldermen, who argued that increasing real tax burdens had resulted in increased vacancy and depreciation of the housing stock before the reform. It is important to note that after 1780, when most of the exposed generation had died, Amsterdam house prices and rents started declining as the Dutch economy entered a major crisis. This makes it less likely that properties that had significantly depreciated in the decades after the shock were renovated by their subsequent owners.

Table 8: Long term impacts on occupancy in 1805

	Dependent variable:				
	Vacancy Rate, 1805		Owner-Occupancy Rate, 1805		
	(1)	(2)	(3)	(4)	(5)
s_i	-0.161***	-0.132***	0.119***	0.118***	0.127**
	(0.029)	(0.031)	(0.043)	(0.046)	(0.050)
log(Rental Value, 1732)	-0.051***	-0.050***	0.057***	0.061***	0.063***
	(0.003)	(0.003)	(0.005)	(0.005)	(0.006)
Constant	0.078	0.114	0.159	0.105	0.086
	(0.094)	(0.094)	(0.138)	(0.140)	(0.149)
Street & Neighborhood FE	Yes	Yes	Yes	Yes	Yes
Observations	19,233	18,727	19,233	18,727	16,658
R^2	0.152	0.153	0.118	0.119	0.117
Adjusted R^2	0.132	0.132	0.097	0.098	0.094
Residual Std. Error	0.228	0.224	0.335	0.336	0.353
F Statistic	7.591	7.436	5.685	5.597	4.991
Note:					*p<0.10,**p<0.05, ***p<0.01

The results of this analysis are in Table 8, Columns 1—2. We consistently control for the 1732 rental value given that it captures the pre-shock desirability of a property as well as granular location fixed effects to adjust for the spatial nature of the shock. The vacancy rate of a property is 0 if it is entirely occupied and 1 if it is entirely vacant. If a property contains multiple units, the number of vacancies is scaled by the number of units. The baseline effect in Column 1 suggests that a one standard deviation predicted decline in the property value due to higher taxes increased the likelihood of vacancy by approximately one percentage point. The average vacancy rate in the sample is 8.4 percent, so this is a sizeable economic effect. The effect only reduces slightly when we remove observations for which no tax was known in the old register, which might have been illegally erected and thus more likely to be vacant later (Column 2).

In Columns 3—5 we extend the analysis by looking at rates of owner-occupancy of properties. The most direct mechanism for this is that a negative shock increases the probability that a property is sold in foreclosure, implying the property is more likely to end in the hands of a long-term investor, who owned most properties in Amsterdam. Another potential channel is that subsequent owners of a property are less likely to be owner-occupants if it is in a bad condition.

Columns 3—4 follow the same outline as Columns 1—2 except for the change in the dependent variable. We find that an increase in taxes reduces the probability that a home is owner-occupied. Due to the geographic nature of the shock, homes experiencing a negative shock were more likely to be in newly-developed areas with lower shares of owner-occupied housing. The effect again does not change when we remove observations for which no tax was known in the old register (Column 4). Based on Columns 3—4, a one standard deviation decrease in predicted house value due to the shock increased the likelihood of owner-occupancy by 0.7%, relative to an average property-level rate of around 20%. In Column 7, we remove properties that are partially or entirely vacant to show that this effect is not driven by the fact that exposed properties are more likely to be vacant.

7. Conclusion

In this paper, we analyze the long-term consequences of shocks to housing wealth taxation on household wealth accumulation. We find large and long-lasting effects that far exceed the short-term impact of the shock on property values because current owners adjust savings instead of housing consumption in response to changes in taxes. This suggests that reforming property taxation has large wealth effects on incumbent owners. To combat such effects, policymakers might consider pairing reforms of property tax systems with policies that stimulate households to reoptimize housing consumption after the shock, for example by temporarily levying low or even negative transaction taxes.

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Appendix A: Proofs

Proof of equation (6):

The IBCs for owner i for the situations with or without a tax shock are given by:

$$y_{t+n} + \rho_{t+n} S^*_{i,t+n} = c^*_{t+n} + \alpha_i r^*_{t+n} + \Delta S^*_{t+n+1} \tag{A1}$$

$$y_{t+n} + \rho_{t+n} S^*_{i,t+n} + \rho_{t+n} V = \hat{c}_{i,t+n} + \bar{\alpha}\hat{r}_{i,t+n} + \widehat{\Delta S}_{i,t+n+1}, \tag{A2}$$

with V the net-proceeds of moving to a different house with rental value $\hat{r}_{i,t+n}$ right after the shock:

$$V = \frac{(1-\bar{\alpha})\left(r^*_{t+n} - \hat{r}_{i,t+n}\right)}{\rho_{t+n}}. \tag{A3}$$

Combining equations (A1) and (A2) gives:

$$(\alpha_i - \bar{\alpha}) r^*_{i,t+n} = \left(\widehat{\Delta S}_{i,t+1} - \Delta S^*_{t+n}\right) + \left(\hat{c}_{i,t+1} - c^*_{t+n}\right) + \bar{\alpha}\left(\hat{r}_{i,t+n} - r^*_{t+n}\right) - \rho_{t+n} V. \tag{A4}$$

Combining equations (A3) and (A4) yields equation (6) in the main text.

Appendix B: Connecting the Old and New Tax

Determining the property-level change in tax required information both on the old tax and the new tax. There are two registers in the archives that detail at property-level both the old tax and the new tax and the names of the owners.[14] There is a third register that contains the final assessment rental values for the new registers, together with the property identifiers (a tax number) and the name of the owner(s).[15] The registers are organized per neighborhood, with Amsterdam divided into 60 neighborhoods within the walls and 5 neighborhoods outside the walls. In total, there are 25,925 properties in the register.

We use the third register to identify the tax values after the tax change, the property identifiers, and the names of the owners. This register is most cleanly written and entirely complete. We use the other two registers to identify the level of the old taxes. There are two caveats. First, in both registers, there are some pages or neighborhoods missing. Second, some parcels that were registered as a single property in the old register would cover multiple parcels in the new register. In most cases this is indicated, but in some cases the old tax value is linked to only one of the parcels in the new register.

Our approach is to use the register that reports combined entries most clearly as a default (no. 33-40) and to resort to the other register (no. 203-268) in case of missing data or unclear entries. Comparing the entirely digitized registers, 10% of tax values differ across the two registers. Most of this is driven by differences in accounting for properties that cover multiple parcels in the new register and a single parcel in the old register. A small fraction of cases might also reflect true errors. Another 20% are missing in one of the two registers, so that we cannot do cross-checks.

[14] Amsterdam City Archives 5045: Archief van de Honderdste en Tweehonderdste Penningkamer of Commissarissen tot de Ontvangst van de Honderdste en Andere Penningen, no 33-40 and 203-268
[15] Amsterdam City Archives 5044: Archief van de Thesaurieren Extraordinaris, no 402-405

For properties covering multiple parcels *j = 1,…n* in the new register but a single one *i* in the old register we compute the old tax for property *j* in the new register based on the fraction of rental value that is attributable to the specific property *j:*

$$TaxOld_{i,j} = TaxOld_i \times \frac{RentalValue_j}{\sum_{j=1}^{n} RentalValue_j}$$

In total, we apply this procedure to the 13% of observations in the new register for which we have an old tax and a new tax value and for which the records indicate to which properties the old taxes belong. For some properties, it might be the case that none of the two records correctly link to the old tax: there remain 1176 observations in the data (4% of properties) for which we cannot link to the old tax. This either implied that the property was not taxed before the reform or that the assessors did not write this down. In the latter case, this would give us a biased estimate of the tax, but such cases were likely limited. Taking into account these potential errors and omissions together, our estimate is that for nearly all privately-owned properties in Amsterdam our data correctly identify both the old tax and the new tax payable.

Appendix C: Data Overview and Matching Strategies

General matching approach

A key element of our paper is to match individuals across different datasets. To do so requires identifying unique individuals across databases based on fuzzy string matching. Our general approach follows the approach outlined in Korevaar (2022) for comparable datasets from 17th-18th century Amsterdam, in line with similar procedures used on US census data (Abramitzky et al. 2022). To match individual *i* in dataset *x* to individual *i* in dataset *y,* we compute Jaro-Winkler distances between individual *i* in dataset *x* and a set of 'candidate' matches in dataset *y*. The individual in dataset *y* with the smallest JW-distance to that individual is selected as a match. To assess the uniqueness of a match, we also compute the distance to the second-best match and construct a score that increases in value if there are multiple *near* matches to that particular name. We vary the tuning parameters for the JW-distances depending on the characteristics of each dataset. In general, name distances are based on the JW-distance between first name plus twice the JW-distance between the last name. As tuning parameter, we set p=0.10 (see Winkler, 1999). In the remainder of this appendix, we briefly discuss each dataset and the various matching procedures.

B.1 Marriage Data

To reconstruct individual's marriages and their wealth-at-marriage we make use of three different register. We start by using digitized marriages banns provided by the Amsterdam City Archives, which contain information on 497,569 marriages between the 1565 and 1811. This data provides information on the date of the marriage, the name of the groom and bride and their witnesses, as well as whether it was a protestant or non-protestant wedding. The records also list the name of the previous partner in case the groom or bride was remarrying. Divorce was extremely rare: most remarried individuals were widowed. For a fifth of marriages, we can obtain more detailed data including information about background and the age of the groom and bride. This data comes from De Moor & Van Weeren (2021) and covers 94,303 marriages every five years between 1580 and 1810. Replacing the data from the Amsterdam City Archives with data from De Moor & Van Weeren (2021) provides data on 489,447 marriages between 1565 and 1811.

We clean the names of both grooms and brides to remove any special characters or letter combinations that can be written in multiple ways. We then search for duplicates in the data by computing JW-distances between the bride and groom names and bride and groom names in other records. Some marriages were recorded double, for example if it was registered both in the protestant or non-protestant register (e.g. in case only one of the weds was protestant) or in case a marriage was cancelled to be executed later. After removing duplicates, there remain 461,119 marriages in the dataset.

To obtain information about wealth-at-marriage, we collect data on mandatory marriage taxes that were wealth-dependent (and introduced in the main text). We only obtain information on 51,403 couples that paid tax between 1699 and 1805. For each couple, we obtain the name of the bride and groom, the year and month of registration and the wealth class of the couple. We only obtained the names of the couples that actually paid tax, which is 20.3% of the total number of newly married individuals in Amsterdam in this period.

We match these individuals to the entire set of marriage records based on their first and last names, adding the JW-scores of the couple (for both groom and bride 1x the score of the first name and 2x of the last name). This matching is relatively straightforward as long as the spelling of the names of the groom and bride is consistent across the records: couples paying marriage tax must appear in the marriage banns in the same period or slightly earlier. We match a couple in case the JW-score is below the value at which the match is more likely to be correct than false.

To determine this value, we compare the distribution of JW-scores including actual matches to a distribution of 'false' matches that compute for each couple in the marriage tax the nearest match to couples that married in a completely different year, and thus do not contain the actual couple.[16] As a cut-off score, we take the value where the expected number of false matches is more than half of the number of expected correct matches. In total, we can match 97% of couples that paid taxes to an entry in the marriage banns in this way. Nearly all of these matches are accurate: only in 0.1% of cases two couples in the marriage tax register are matched to the same individual marriage banns. In that case, we remove the match that has the highest JW-score. Of course, we only observe such double matches in case a couple that paid taxes is falsely matched to another marriage record that also paid marriage taxes. Because only 20% of married couples had to pay marriage tax, this implies about 0.5% of individuals that paid taxes were incorrectly matched. Thus, for 3.5% of married couples with wealth, we either have not identified a match in the records or identified the wrong match. Given that 80% of married couples did not possess any wealth, we in the end correctly observe wealth-at-marriage for >99% of the couples that married in Amsterdam and appear in the marriage banns.

B.2 Burials Data

For the burials data, we apply a very similar procedure as for the marriage data. To obtain information on the number of burials, we use digitized burial records from the Amsterdam City Archives covering the period from 1554 until 1810, containing in total 1,422,668 persons. For each burial, we know the date, the name of the registered person and the location of the burial site. Not each name in the burial records corresponds to the actual name of the person being buried. For example, when children were buried they were often registered under the name of the father or both parents ("child of …"). In some cases, this also applied to women ("housewife of …" or "…, partner of …"). If such a relationship status was explicitly mentioned, we identified this.

After cleaning all names and removing duplicate observations, we focus on the 755,126 individuals buried between 1701 and 1805, since we also have digitized data on the burials tax in this period. Similar to the marriage records, we only digitized data from the burials tax records

[16] Plots distributions are available upon request

for individuals that had to pay tax and thus possessed wealth. For each individual, we obtain the first and last name, the month and year of registration and the wealth class. In total, we digitized information for 115,413 individuals paying burials tax. Only 15% of individuals that died in the 1701-1805 period paid tax. This number is likely lower than the number in the marriage records because a large fraction of buried individuals were children. Some buried children were still taxable because they owned wealth through inheritance or because their parents held a certain office that was taxable based on income (Hart, 1973).

To match individuals in the burial tax register to individuals in the burial registration we use the same procedures as in the case of the marriage records. We compute JW-distance for individuals in the tax records with individuals in the burial records that died in the same month or in an earlier month and find the nearest match. The main difference is that burial records only list a single name whereas marriage records list two names, making it slightly more difficult to obtain a unique match. We thus use lower cut-off values to determine correct matches.

In total, we find a match for 85% of individuals in the burial tax records, lower than the marriage records. We also find much more cases where multiple tax records are matched to the same burial record (0.6% of matches). This implies that an estimated 82% of deceased individuals that paid taxes were matched to the correct burial record. Because 15% of households died with any wealth, this implies we observe the wealth class at death correctly for about 97% of buried individuals.

The lower match rate for the burials dataset relative to the marriage dataset is only for a small extent driven by the fact that is more difficult to find a unique match using a single rather than a double name. Only in about 2% to 3% of cases one or more individuals die in the same month with the same or a highly similar name. A much more important factor is that the name in the burial tax record can differ from the name in the burial record itself when it constitutes a child or partner. For example, a child might be identified by its true name in the burial record but by the name of its parent in the tax records (and vice versa). Duplicate matches are also partially explained by cases where multiple children or a child and wife die in the same month and all are identified as ‘child’ or ‘partner of’ the father/husband.

Although the fraction of false matches is small, any that are included in the analysis will bias down our coefficients towards zero. However, the probability of a match being correct is much larger conditional on being included in the sample. Our analysis focuses on individuals that are owning real estate at the time of the reform in 1732, which are mostly men, and who have sufficiently unique names. For this subset of individuals, the linkage between the tax records and the actual marriage and burial records is likely near-perfect.

B.3 Baptism Data

For the period from 1554 to 1811, records digitized by the Amsterdam City Archives provide information on 1,236,573 baptism in the city. For each of these baptisms, we have information on the name of the child, the date of the baptism or the birth date, the church and the name of the parents and witnesses. We focus on observations after 1698 because the marriage tax data no not start prior to 1699. After cleaning and removing duplicates, this leaves us with 707,944 children baptized between 1699 and 1810.

B.4 Matching Marriage Records to Burial Records and Estate Tax Records

To match individuals in their marriage records to their burial records we start by matching the marriage date to children that were born out of the marriage. Because the baptism records report both the name of the mother and father as well as the birth data, we can match the baptism records to the marriage records using similar strategies as presented in section B.2 for matching marriage records to marriage taxes. The main difference is that the pool of potential matches is larger, as we look at all marriages that happened in the preceding 30 years, when nearly every bride should have reached an infertile age.

For every birth, we compute JW-scores of the father and mother with the names of couples in earlier years (for both groom and bride 1x the score of the first name and 2x of the last name).

We match individuals when the summed JW scores are below a cut-off and in case there are no other near matches with similar scores. We select the cut-off (0.75) based on the value where a match is more likely to be a placebo match than a correct match. In this way, we can link 462,924 baptisms to the marriage records of their parents. The remaining 38% of newborns either have parents with a non-unique combination of names or were born to parents that did not marry in Amsterdam.

Next, we start matching marriages to earlier marriages. In case an individual remarries after his or her partner passed away, the new marriage record would stipulate the name of the previous partner. We again use approximate string matching to link couples, except that we now require the remarriage to happen within 40 years of the original marriage. 18% of grooms in the data were remarrying of which 57% could be matched to their previous marriage (and vice versa). Similarly, 15% of brides were remarrying of which 72% could be matched to previous marriages.

We then start matching the marriage data to the burials data. For a subset of women, the burial records also report the name of their husband. We can again use the name of the deceased women and her husband to match to the marriage records using the earlier presented strategies. We can match 27% of brides, about 67,000 individuals in total precisely to their wealth-at-death in this fashion. For grooms, we can only match 0.2% of individuals given that their death records rarely mention the name of their wife.

To match marriages to burials, we look for each bride or groom in the burial records and find the nearest match in the period up to 65 years after marriage, or below 85 years if the age of the groom or bride is known. As starting period for the set of potential matches we use the year of the marriage or, if available, the year in which the final child was born. If a groom or bride remarries, we use the remarriage date of the groom or bride as maximum death year for the earlier partner. For brides, we exclude burials and marriages that are already matched in the previous step. We then compute the best match using JW distances on the first name plus two times the last name (p=0.10) for the set of potential matches. We also compute the second best match to rule out other good matches. We define a match if the summed JW distance of the first best match is below 0.10 and the second best match at least 0.10 above that. We use the same strategy to match estate tax records to the burials and marriages.

Appendix D: Supplementary Tables

Table 7: Capitalization of tax discrepancies for properties in the register

	Dependent variable:			
	log(Purchase Price) 1720-1729		log(Sales Price) 1731-1739	
	(1)	(2)	(3)	(4)
log(Rental Value, 1732)	0.781***	0.425***	0.752***	0.481***
	(0.020)	(0.031)	(0.027)	(0.045)
log(Old Tax Rate + 1)	-0.614**	-0.918***	0.057	-0.085
	(0.288)	(0.344)	(0.161)	(0.153)
Year FE	Yes	Yes	Yes	Yes
Neighborhood & Street FE	No	Yes	No	Yes
Observations	1,997	1,997	1,016	1,016
R^2	0.437	0.671	0.498	0.753
Adjusted R^2	0.435	0.582	0.468	0.604
Residual Std. Error	0.719	0.619	0.682	0.589
F Statistic	154.394	7.520	16.662	5.068

Notes: *p<0.1; **p<0.05; ***p<0.01. This table provide suggestive evidence on whether tax discrepancies were priced. We match properties in the 1732 tax records to purchases of these properties and sales of these properties, with the JW-distance to the best match below 0.1 (and the next best match at least 0.375). We then run a hedonic analysis on the log sales price of the property controlling for 1732 log rental value as well as year fixed effects. In a second specification, we add street fixed effects and neigborhood fixed effects. The location fixed effects adjust for the fact that yields, and thus the relation between rental value and sales prices, plausibly varied across locations. We focus on properties bought shortly before the reform was announced (1720-1729) and properties sold quickly after the new taxes were announced (1731-1739). In the period around the reform, the 1732 rental value likely captures the consumption value of the property most accurately. For properties purchased in the decade before the reform was announced in 1730, a 1% increase in the old annual tax rate of rental value corresponds to a -0.92% decrease in price, which is close to full capitalization (Column 2). However, the magnitude of this effect is smaller without location fixed effects (Column 1) and standard errors are generally large, implying this evidence should be treated as suggestive rather than definitive evidence for full capitalization of taxes. Reassuringly, we do not find such a relationship for sales of properties in the records in the 1730s: because tax rates were equalized in the new register, differences in old tax rates should not matter for prices (Columns 3-4).