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Growth and Predictability of Urban Housing Rents

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Abstract

This paper studies urban rental prices for half a millennium (1500–2020) and seven cities: Amsterdam, Antwerp, Bruges, Brussels, Ghent, London, and Paris. Based on a dataset of 436,000 rental cash flow observations, we build continuous annual indices of housing rents, which we employ to study the long-term developments in rental cash flows, as well as their predictability. We find that real rent growth has been limited, but with large differences across cities: average annual growth rates range between 0.12 percent for the Belgian cities to 0.30 percent for Paris. At the market level, we show that sluggish supply adjustment implies that past population growth negatively predicts current rental growth. At the individual asset level, we find that past excess rental growth rates are predictive of future rent revisions, and that increasing steepness of the term structure of contract rents is predictive for future rent levels.

Keywords: rents, urban growth, predictability

JEL: R30, N93, N94

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Notes: This paper subsumes an earlier paper titled "500 Years of Housing Rents, Quality and Affordability"

Urban housing rents are both the largest component of urban household expenditure and the key return component for residential real estate investors, yet we know little of their evolution over time. In this paper, we assess the long-term performance of housing investments by studying urban rents in seven European cities since 1500. Based on observations of rental cash flows received by institutional investors, we estimate annual rent price indices for Amsterdam, Antwerp, Bruges, Brussels, Ghent, London, and Paris. We document that real housing rents have risen by limited amounts in the long run, while rental yields have been stable over the long term. However, real rental growth varied substantially over the shorter term. We show that past population changes negatively affect current rent price changes over horizons of several decades and attribute this to the slow process of urban development and redevelopment. At the level of individual rent contracts, we exploit variation in contract length to show that investors and tenants can anticipate future rental changes and that aggregate market information is informative for the future evolution of individual contract rents.

These new estimates of the evolution of rent prices and their determinants make several important contributions. First of all, we provide the first consistent overview and comparison of housing rents across cities and centuries, updated to current times. We collected more than 436,000 individual rent observations from archival sources and earlier studies, such as Clark (2002) for London, to estimate indices that go back until 1500. For investors, such estimates provide crucial information regarding the expected long-term growth rates of the rental cash flows they receive, which are a critical ingredient in the valuation and investment performance of residential real estate. For tenants, information on the evolution of housing rents is needed to make inferences on the urban cost of living. In the current public debate, there is much concern about rising urban housing costs but little information on the long-term evolution of rental prices.

Our paper provides annual-frequency rent price indices for seven cities, based on primary data, estimated using a state-of-the-art methodology that accounts for quality differences, consistently applied across cities. Existing papers have primarily focused on the long-term evolution of house prices in individual cities (e.g Eichholtz, 1997; Eitrheim and Erlandsen, 2004; Shiller, 2005; Edvinsson et al., 2021). Some papers have compiled existing long-term house price indices (Knoll et al., 2017) or rent price indices (Hoffman et al., 2002;

Knoll, 2017), but these combine indices constructed with different methods and from different geographies and cover shorter periods. Some of these indices do control for housing quality, but many do not. As these authors acknowledge this can have a large impact on the resulting indices and their comparability.

In the long term, we find an average growth rate of real housing rents around 0.1 percent for the Belgian cities and between 0.2 and 0.3 percent for Amsterdam, London, and Paris. These long-term numbers contribute to several debates.

First, existing estimates of long-term discount rates are based on leasehold prices and measures of past rental growth (Giglio et al., 2015; Bracke et al., 2018; Fesselmeyer et al., 2021; Giglio et al., 2021), as they require assumptions on the expected rental growth rate g to back out the long-term discount rate r. Absent long-term rent growth data, these studies used historical averages from shorter-term data, typically around 0.7%. For the UK estimates in Giglio et al. (2015); Bracke et al. (2018) and Giglio et al. (2021), our findings would reduce the implied long-term discount rate from 2.5% to around 2%.

Second, our findings relate to the wider literature on long-term house prices and returns. Existing work has established that long-term asset returns are largely driven by the cash flows they generate, both in stock markets (Dimson et al., 2009; Le Bris et al., 2019) and in housing markets (Jordà et al., 2019; Chambers et al., 2021; Eichholtz et al., 2021). Accordingly, differences in yields across properties and locations are a crucial driver of long-term housing returns (Amaral et al., 2021; Colonnello et al., 2021). If there are no structural trends in yields, rental growth is the key factor affecting long-term returns.

We compile data on actual property yields based on sold properties and establish that there is no evidence for any structural downward time trend in yields, in contrast to the study by Schmelzing (2020) on the development of interest rates since the Middle Ages. One possible reason for this finding is that the fundamental risk to rental housing investments could be more stationary over time than the risk to government debt. While the latter is currently associated with low default risk, that was not at all the case in previous centuries. These findings confirm that the rapid rise in urban house prices and the similar decline in yields since the 1980s is a recent phenomenon (Knoll et al., 2017; Knoll, 2017).

The second main contribution of this paper lies in the long-term perspective on rental markets and its implications for current and future housing markets. Housing markets in the 20th century have been heavily impacted by the World Wars and periods of government interference in prices and rents, either through housing subsidies or direct rent regulation (e.g. Arnott, 1995). Our longer sample covers extensive periods without any such inference, so providing a picture of the development of housing rents in free markets. This may shine some light on policy debates on rental affordability and the role of governments therein.

Besides that, most developed countries have experienced rapid population growth and urbanization since the onset of the Industrial Revolution, likely increasing rental prices. However, urban population growth is expected to slow down in the future. In our data, we observe episodes of urban growth followed by extended periods of decline, and this likely provides a more representative picture of future long-term rental growth.

Related, the cities we cover have experienced different growth trajectories. Most existing long-run data on housing prices or rents contain a strong bias towards modern "superstar cities": successful and fast-growing urban areas (Gyourko et al., 2013).[1] Similar to a 'survivorship bias' in studies of mutual fund returns (Brown et al., 1992), there might be a 'superstar bias' in studies of long-term housing returns. This might work in two directions: Amaral et al. (2021) show that modern superstar cities have experienced higher capital gains but lower total returns due to persistently lower yields. Some of the cities in our sample were very successful initially, such as Antwerp, Ghent, and Bruges, but then experienced long periods of stagnation and population decline. Others, such as Amsterdam and Brussels, rose to greatness over time, but the process of growth was never linear. Cities like London and Paris were consistently the largest in Western Europe and grew substantially. This is reflected in rental growth, which is skewed towards current-day superstar cities. Paris, for example, experienced a geometric average real rental growth rate of 0.30 percent per annum, but Ghent and Bruges experienced barely any long-term rental growth at all. Although such differences seem economically small, they amount to sizeable changes in relative prices over the long run.

While long-term rental growth has been modest in all cities, there are much more substantial differences in rental development over shorter horizons. Reconciling these two phenomena brings us to the third main contribution of the paper: exploring the dynamics and

[1] Examples include Eichholtz (1997); Eitrheim and Erlandsen (2004); Nicholas and Scherbina (2013); Shiller (2005); Knoll et al. (2017); Edvinsson et al. (2021).

predictability of housing rents over the medium term. (*decades*). Most importantly, we document that past shocks to urban population growth negatively predict current rent price growth, controlling for current demand shocks. We document that a one percent increase in the urban population over the previous 25-year period predicts a decrease in the current 25-year rent price growth of about 0.5 percent. We find comparable but slightly smaller effects for past decreases in the population growth.

We argue there is an urban economic explanation for our findings: sluggish supply adjustment to demand shocks. In the model of Glaeser and Gyourko (2005), urban growth translates into increased housing supply, but urban decline does not lead to a comparable reduction in supply as existing homes only very gradually deteriorate. We confirm the Glaeser and Gyourko (2005) predictions concerning house prices in growing and shrinking urban housing markets for long-term rental markets: aggregating the current and lagged effect of population growth on rental prices, we find a very small elasticity of housing rents to positive shocks to population growth, but a much larger elasticity to population declines.

However, the elasticity of rent price changes to current 25-year population changes is much larger than the net long-term effect, which we attribute to slow supply adjustment. We use historical evidence to show that in most European cities supply constraints are historically the norm rather than the exception, and these can still mitigate the housing supply response to growing populations over horizons of multiple years or even decades.

Our finding of predictability also relates to the findings of Combes et al. (2019) for modern France, who find the 'long-run' cross-sectional house price elasticity to urban growth to be substantially lower than the 'shorter-term' time-series elasticity and attribute this to the fact that property development in France can take many years. If supply responses to urban growth are limited in the short to medium term, rental price growth might overshoot its long-term equilibrium, implying negative rent predictability.

In line with this, we find that the negative relation between past population growth and current rent price growth is strongest over time horizons of 10-20 years but disappears at very long horizons when supply can adjust fully. For negative shocks to population growth, we do not find the effect to vary across time horizons, in line with the more gradual supply response over time due to gradual housing depreciation and demolitions.

To the best of our knowledge, our paper is the first to document that past shocks to housing demand influence current changes in rent prices over horizons of multiple years or decades. However, there is a large and related literature on the predictability of asset cash flows (see Koijen and Van Nieuwerburgh (2011) for a review), and various papers have studied the short-term predictability of rent prices. Plazzi et al. (2010) study commercial real estate but find limited consistent evidence for predictability of rent growth. For housing rents, Campbell et al. (2009) and Ambrose et al. (2013) find some positive persistence in rent-growth at (semi-)annual horizons. Gallin (2008) and Engsted and Pedersen (2015) also point to the predictability of rent growth using the rent-price ratio, but this pattern does not seem consistent across countries or time periods.

The final contribution of this paper is to explore rent predictability over shorter horizons at the level of individual contracts. We study both the predictability of rental price risk at the contract-level and whether the pricing of individual contracts of different lengths is indicative of future rental price development.

Starting with the latter, we study whether tenants and landlords are able to gauge where future rent prices are going, exploiting a unique feature of our Paris data which explicitly lists contract lengths. If market participants expect rents to go up in the future, then a long-term contract should be more expensive relative to a short-term contract, holding the property fixed.

For each contract, we use realized market rent price growth to compute the 'indifference' contract premium that would make a risk-neutral agent ex-post indifferent between a single long-term contract and multiple shorter-term contracts. We then compare these to the (ex-ante) premia in the actual contracts. Under a reasonable set of discount rates, we find that the observed premia for long-term contracts in the data strongly co-vary with the ex-post indifference premium, with a coefficient just below one. This suggests that market participants, on average, correctly gauge where rents are going in the future.

The term structure of office rents has been studied (see Aldana et al., 2020, for a survey), and our results for housing rents are in line with the key findings of that literature. We find evidence of an upward-sloping term structure of housing rents, with long-term contracts priced higher than short-term contracts, although that difference is only marginally significant. Existing work has not examined to what extent forward lease rates co-vary with actual realized prices. In contrast to the well-known literature in finance on interest rate spreads, where the expectations hypothesis has been rejected (e.g Pflueger and Viceira, 2011), we find that current spreads between long and short contracts do significantly predict future rent price growth.

Finally, we use our long panel of rental contract data for an exploratory investigation of rental price risk at the level of individual contracts. We show that past dispersion in rental prices both at the property-level and at the market-level predicts future contract-level price dispersion. This suggests that the idiosyncratic risk of property-level contract prices is persistent.

This paper is organized as follows. In the next section, we introduce and discuss the data and data sources, and explain the methods to estimate the indices of rent prices. In Section 2 we present the rent indices in nominal and in real terms and discuss the presence of trends in housing yields. Section 3 will provide a basic framework to analyze long-term rents, and Section 4 subsequently analyzes the dynamics in market rental prices and their predictability. Section 5 explores whether contract-level data can be used to infer future housing rental growth and cash flow risk. We end the paper with a summary and some conclusions.

1 Data and Index Estimation Method

1.1 Data

Tracking residential rents for seven cities and more than 500 years at annual frequency implies major data collection challenges. We compile rental cash flow and contract data from dozens of existing historical and contemporary studies, combined with hand-collected primary data from archives. This effort resulted in the collection of about 300,000 observations of housing rents, most of which originate from the archives of social institutions, such as churches, monasteries, orphanages, or hospitals. Beyond these sources, we collected additional primary and secondary data on estimated rents from tax registers, which we use to assess the representativeness of the institutional data. Including these, our database of primary rental data contains over 436,000 observations, about 30 percent of which we hand-collected from archival sources. In a few hundred cases, the rental data also provide information on the sales price.

Table 7 in Appendix A presents an overview of all these different data sources and the number of observations we obtained from each of them. That appendix also provides a very detailed discussion of all these sources. Except for Paris (1809–1860) and London (1909–1959), virtually all of our primary sources originate from the archives of social institutions. Such institutions were prevalent in most European cities and often had considerable housing portfolios, mostly resulting from bequests or donations over time. They used the rental cash flows of these homes to finance their activities. These institutions were the precursors of the modern-day institutional investors (Gelderblom and Jonker, 2009), and kept extensive archival records of their accounts, many of which have survived the test of time. Although renting from private landlords was more common than renting from such institutions, small-scale landlords did not keep archives. This limitation raises two essential questions regarding the validity of our dataset: did these institutions own a portfolio of housing that was representative of the housing stock of the city, and were these homes leased at market rates? In Appendix B we provide evidence showing that these institutions indeed rented their homes at market rates, and that the trajectories of these rents were representative for the cities as a whole.

The nature of the rental data varies slightly across cities due to differences in contracting and registration. For the Belgian cities and Amsterdam, most of the data specify the lease price per year at the property level but do not inform about the actual contract and its start and end dates. In Amsterdam, we have contract-length information for a small part of our data, and this indicates that most contracts were signed for one year. We do not have contract-length information from Belgian cities but contracts in Belgium ran typically for 9 years with changes possible after 3 years. That also holds for Paris, and for that city we observe actual contract length for 68 percent of the observations. We will use this to infer price premia on long-term contracts. The main exception in terms of data is our data from London before World War I, which varies wildly in terms of contract standards, mixing rentals with various forms of leaseholds and sometimes even freeholds (see Clark, 2002). We only use observations with a contract length of up to 21 years, implying our London sample is relatively thin before the late 19th century and should be interpreted with care.

Rather surprisingly, it was more difficult to obtain primary data on housing rents for the

20th and 21st centuries than for preceding centuries. There exist few commercial databases that track housing rents, and due to privacy reasons, it is not possible to obtain recent and contemporaneous rental contract data in archives. Therefore, we had to rely on secondary sources from the mid-20th century onward, ensuring to only select sources that (attempt to) control for housing quality.[2] In most cases, these series are based on the rent component of the CPI, often at the urban level but sometimes using national figures.

Although these indices do adjust for quality, there has been some debate about whether they accurately represent market developments. First, the existence of a complicated system of rent controls for a large part of the 20th century makes it by construction difficult to construct a representative index. Second, quality controls might be imperfect. For the United States, Gordon and VanGoethem (2005) argue that the rent component in the CPI from the early 20th century until the 1980s is biased downward, given that hedonic improvements in housing quality cannot fully make up for the increase in mean housing rents relative to the quality-controlled CPI figure. One potential reason for this bias is that renters are less likely to be included in the rental survey when they move, even though rent increases typically occur after signing a new contract. Ambrose et al. (2015) make a comparable point but find a bias in a different direction for the 2000s: their repeat-rent index, based only on newly signed contracts, increases much less than the rent component of the CPI. These limitations imply that our indices might be less precise for the short period in the 20th and 21st centuries when they rely on secondary rather than primary sources.

Beyond housing data, we also compiled primary and secondary data on consumer prices and wages to assess real rents and their development relative to other demand factors. For time consistency, we relied whenever possible on the evolution of day wages or annual wages of workers in the construction sector, which form the bulk of historical wage series (e.g. Allen, 2001). For the Belgian cities, we create a new consumer price index for the 1500–1830 period, while we rely on the existing series for the other cities. The data sources and construction method for our consumer price indices are discussed in Appendix C and for wages in Appendix D. We converted rents for each country into a single local currency (Dutch Guilder, French Franc, Belgian Franc, British Pound).

Last, we searched existing sources for population estimates and interpolated them lin-

[2] The only exception is Paris, where we rely on an existing rent index already from 1867.

early in case of missing data. We employ these population numbers to create population-weighted indices for the Belgian cities and, in our subsequent empirical analysis, to explain the growth of rental cash flows. The sources are in Appendix E.

1.2 Index Estimation Method

The literature regarding the estimation of rent indices has relied on hedonic models and repeated-measures models. We use the latter. The basic repeated measures methodology of Bailey et al. (1963) starts with the observation that the log price on any asset, in this case, the log rental price r_t for home i, can be represented as the sum of three components:

$$r_{i,t} = \alpha_i + \beta_t + \epsilon_{i,t} \tag{1}$$

The first term, α_i reflects the underlying value, and therefore quality, of the home: the key assumption is that this does not change over time, at least at the level of an individual home. The second term, β_t is the value of the log rental price index, while $\varepsilon_{i,t}$ reflects price noise and is assumed to be distributed as N(0,σ^2). Taking differences for any time periods $t = y$ and $t = x$, with $y > x$, the change in log rental price on any home i can be written as follows:

$$r_{i,t=y} - r_{i,t=x} = \sum_{t=1}^{T} \beta_t D_{t,i} + \tilde{\epsilon}_{i,t} \tag{2}$$

D refers to a set of dummy variables that take on the value of 1 if $t = y$ and -1 if $t = x$, and $\tilde{\epsilon}_{i,t}$ equals the difference in the two error terms. Equation (2) can be estimated using ordinary least squares (OLS), and subsequently converted to an index by exponentiation.

To satisfy the assumption of constant quality between rent reviews, homes in our sample were treated as new observations if there was any indication that the home had been rebuilt, renovated, or significantly affected in some other way. Still, it is unlikely that house quality does not change at all. First of all, we cannot account for the effect of aging on the properties as we do not know the years in which they were built. Second, minor quality improvements to the property might not have been registered. However, we believe the potential errors are small, as homes were kept in the portfolio and were well maintained for, in many cases, hundreds of years.[3]

Before proceeding to present and discuss our indices in the next section, it is important to consider the question of whether our repeated-rent indices are systematically influenced by potential depreciation or appreciation due to an unobserved deterioration or improvement in the quality of the underlying assets. In Appendix F we provide more historic context as well as statistical analyses to assess whether this is the case, and we conclude that it is unlikely that our indices systematically over- or underestimate rent developments due to unobserved quality changes.

Since rental contracts were typically signed for several years, we only include a rental observation in the index estimation in the year a new contract had been signed. For the Belgian cities, and most observations from Amsterdam, our rent data do not specify new contracts. For these observations, we only include observations where the rent changed, as this implies that a new contract had been signed. The main disadvantage of this approach is that it misses observations where the new contract is signed at the same price.

The use of repeated new contract rents rather than all rental cash flows implies that in some cities, in particular London and Bruges, the remaining number of observations is low. In such cases, noise in the rent prices can have a large impact on the resulting index. The literature has proposed several adaptations of the original model to improve the signal-to-noise ratio. Probably the most notable of these are the studies by Goetzmann (1992), proposing a Bayesian ridge estimator, and Francke (2010), who develops a generalization of Goetzmann's method that allows for general model specifications that can be compared using likelihood criteria. We follow the model of Francke (2010), and specify the betas in equation (2) not as fixed unknown parameters to be estimated using OLS, but by using a local level model:

$$\beta_{t+1} = \beta_t + \zeta_t, \qquad \zeta_t \sim N(0, q_\zeta \sigma^2) \tag{3}$$

The dependence between the betas is based on the signal-to-noise ratio q_ζ. If this ratio is low, the variance of the error terms of the index is low, and the dependence between the

[3] Some archival records also specify property-related expenses. For example, the Burgerweeshuis, the main Amsterdam orphanage and the most prominent institutional property owner in Amsterdam, spent about 26 percent of its rental revenue on maintenance between 1682 and 1806 (ACA 367.A, no. 141).

betas will be strong, resulting in a smoothing of the index compared to the standard case. Francke (2010) proposes an empirical Bayes procedure to estimate the index. Conditional on the variance parameters q_ζ and σ^2, estimates of the annual coefficients can be obtained using generalized least squares. The variance parameters are subsequently estimated by maximum likelihood. For more detail regarding the estimation method, see Francke (2010).

Table 1 contains the output of the estimations of the repeated-rent indices. Note that for Amsterdam, data was not available for the early part of the 16^{th} century, such that our index only starts in 1550. For Paris, we estimated the index including observations from 1400 onward, since this significantly increased the number of repeated observations available to estimate the growth of the index in the first part of the 16^{th} century. For London, we estimated the indices separately for the periods 1500-1903 and 1909-1959, due to the absence of data between 1903 and 1909, and the difference in data densities between the two samples. To compute a total Belgian city index, we used population-weighted averages. Consistent with the observations made earlier in the paper and Appendix B, the signal-to-noise ratio for London, and to a lesser extent Bruges, is significantly lower as compared to the other cities. This indicates these indices have been smoothed significantly.

– Insert Table 1 about here –

2 Housing Rents and Yields in the Long Run

2.1 Rent Indices

Figure 1 (a–g) reports the nominal rent indices for Amsterdam, Paris, London, and the four Belgian cities, with local CPI plotted for reference. The overall pattern for the very long-term rent development looks surprisingly similar across the seven cities. We observe a very gradual increase in nominal rents until the beginning of the 20^{th} century, after which they go up more quickly. Inflation follows a pattern that does not differ very much from nominal rents, and the graphs clearly show that the more rapid increase in nominal rents in the 16^{th} and 20^{th} century is associated with higher inflation. Overall, year-to-year changes in rents are less volatile than changes in consumer prices.

We deflate nominal to real rent based on consumer price indices. Although real rents can be compared in a meaningful way across cities (see Figure 2h), they exhibit excessive volatility due to the substantial short-term fluctuations in the consumer price indices, in particular before the 20th century. In early modern times, household expenditures have been dominated by the cost of food, especially bread. Prices of grain and other agricultural produce were intrinsically volatile as they depended on the richness of harvests and the conditions for trade.

Finally, Figure 2 presents the real rent indices, paired with corresponding population numbers. The indexed population numbers are based on municipal boundaries and therefore underestimate the total population due to suburbanization processes in the 20th century. In all cities, population numbers for their respective metropolitan areas continued to grow. The final panel in Figure 2 combines the real indices for all cities, pooling the Belgian cities using a weighted average until 1940 and national rent price data afterward.

– Insert Figure 1 and Figure 2 about here –

Summary statistics on the nominal and real rent indices are provided in Table 2, with all statistics using geometric (log) growth rates. The first striking conclusion is that real rental prices have shown very little growth in the long run. For both Amsterdam and Paris, the long-term growth rates in real rents have been about 0.3 percent per year. For London, the growth rate is a bit lower around 0.2 percent, because rents fell in the first part of the 16th century. We also want to point out again that the London data is very thin in the early period so that the developments are estimated less precisely than in other cities. Excluding the first part of the 16th century, for which no Amsterdam data is available, real long-term rent growth in each of these three cities has been remarkably similar with a growth rate around 0.3 percent per year.

The aggregate population-weighted index for the four Belgian cities shows more modest real growth at around 0.12 percent per year. There might be both an economic and a statistical reason for this. First, cities like Antwerp, Bruges, and Ghent were economically very prominent in the 16th century but fell behind the other cities later on, which likely translated into lower real rent growth: real rents fell substantially during the 16th century. Second, Table 2 uses a national index to compute rent growth for the Belgian cities post-1940, which might have evolved differently from a pure urban rent index.

One other way to assess the realism of these numbers is to compare the relative prices in these cities over time. Comparing absolute prices over time is not possible because housing standards have changed over time. The final column in Table 2 provides the current rent price per square meter for a property in the city center of these cities.[4] The Belgian estimate is based on an average for Antwerp, Ghent and Brussels. Currently, rent prices are highest in London and lowest in the Belgian cities. However, we can use the real rent indices for these cities to trace back the relative prices in 1550, the first year for which we have data on all cities. If the 2020 numbers reflect the current disparity in prices then our numbers imply that in 1550 relative prices in London, Paris and the Belgian cities were approximately the same and that prices in Amsterdam were about 20 percent lower. These values appear plausible, in particular given that Amsterdam was still a town of minor importance relative to the other cities in 1550.

– Insert Table 2 about here –

The conclusion is that over the very long term, rent growth in these cities has been similar. Over horizons of several decades, real rent growth across cities has also been significantly correlated, with pairwise correlations in 25-year real rent growth across cities in the range between 0.23 and 0.93.[5] Unsurprisingly, correlations are the highest among the Belgian cities (0.78–0.93), and the lowest between Amsterdam and Antwerp (0.23). Beyond these long-term conclusions, the short- to medium-term developments of the indices also offer important insights on the evolution of these cities and their rental markets. While a detailed discussion of the economic history of these cities is beyond the scope of this paper, we do want to point to some of the largest shocks to rent prices and the historical events they coincided with.

In terms of rent and consumer price development, the 16th and 20th centuries were probably the most turbulent. Rapidly rising consumer prices implied real rent reductions in most cities during the first half of the 16th century. In the second part of the 16th century, real rents started falling more quickly in both the Belgian cities and Amsterdam, following the start of

[4]Estimates are based on numbers from Savills for London, Pararius for Amsterdam, the OLAP for Paris and Numbeo for the Belgian cities.

[5]Supplementary Table 11 in the appendix provides exact statistics. For horizons over 10-years, correlations are similar.

the Eighty Year's War with the Spanish. Although this war was full of twists and turns, it induced an economic shift from the Southern Netherlands - Brussels, Antwerp, Ghent, and Bruges - to the Northern Netherlands, most notably Amsterdam. The key turning point was the Fall of Antwerp to the Spanish in 1585, when much of the protestant population fled the Belgian cities towards Amsterdam and other parts of Holland. In Amsterdam, this was the start of the famous Dutch Golden Age and went hand-in-hand with rapid increases in population and rent prices. This event also explains the low correlation between Amsterdam and Antwerp.

The Wars of Religion also affected Paris, culminating in the Siege of Paris of 1590. Around the Siege, nominal housing rents declined by as much as 75 percent, following the starvation and migration of a large part of the Parisian population, and it took almost 20 years for housing rents and population to recover fully. The population and rent reductions in the Belgian cities were more persistent, resulting in a significant period of urban decline. Brussels was the only city in the Low Countries sample that came out of the Eighty Year's War relatively unscathed. It did not experience population losses as significant as the Flemish cities and could sustain its political and economic status as the capital of the Southern Netherlands.

For the other cities, the population only recovered in the 18th century–early 19th century. In that period, Antwerp's housing rents recovered fast, as the city developed once again into one of Europe's leading port cities, in the wake of industrialization as well as the reopening of the Scheldt river, which had been blocked by the Dutch since the late 16th century and prevented the Antwerp port from growing. The 19th century was much less fortunate for Bruges, and its rental price growth was correspondingly much lower. The city did not industrialize like Ghent, Brussels, or Antwerp, and became one of the poorest cities in Belgium.

Industrialization and urban population growth also resulted in rising real rents in other cities in the 19th century, with real rents on average growing by around 1% per year: the most rapid increase in the data. The increase was particularly large in London. Beyond Bruges, Amsterdam was also late to industrialize. Amsterdam had remained one of the most important European cities for most of the 18th century but experienced an intense economic crisis following the start of the French period in the late 18th century and only recovered in the second part of the 19th century.

Just like the rent swings at the end of the 16th century, 20th-century rent developments were closely linked to the wars that ravaged Europe at that time. At the start of the century, urban rent levels were already at high levels and rose even further due to World War I housing shortages. Following World War I, each of the cities adopted strict rent controls. Because most rent controls fixed rents in nominal terms relative to their pre-war levels (Willis, 1950), varying levels of inflation after World War I amplified volatility in real rents, as Figures 1 and 2 show. Although real rents recovered in the late 1920s and 1930s, we observe the same pattern after World War II: real rents initially declined significantly due to strict rent controls, but then caught up in the 1950s and 1960s as governments allowed for larger rent hikes. While the degree of regulation varied over time, rent controls substantially weakened or were abolished in the 1980s (Kholodilin, 2020). In this period, most countries started to introduce more sophisticated policies for rent regulation and tenant protection (Arnott, 1995), which has likely had a dampening effect on real rent volatility. In the last part of the 20th century, real urban rents have started to rise again and this trend has continued in the 21st century, fueling renewed discussion about urban housing affordability, the lack of housing supply, and government policies to do something about it.

2.2 Housing Rents and Yields

Before we discuss medium-term rental price dynamics and predictability, we consider how our evaluation of long-term rent price growth informs us about the long-term evolution of housing costs and returns. The critical issue here is the evolution of the yield. If there is no structural trend in yields, long-term rent price growth equals long-term house price growth.

Recent evidence suggests a secular downward trend in interest rates since 1300 of about one basis point per year (Schmelzing, 2020), although it is unclear what is causing this exactly. If such a persistent trend would also be present in housing yields, our estimates of long-term housing rental growth and decline would not line up with trends in house prices and corresponding capital gains and losses.

We use two sources of data to examine this. First, we focus on Amsterdam, the only city for which long-term data on the evolution of house prices and rents is readily available (Korevaar et al., 2021), and for which we have a large set of actual yield observations for three sub-periods (Eichholtz et al., 2021; Korevaar, 2020). Second, we study a small subset of our rent data for the other six cities where we can pair the rent price with a transaction price. We have 305 transactions for which we simultaneously observe the rent and the sales price. These observations are spread across all six cities and cover the period between 1500 and 1900.

We plot these two sets of long-term housing yield evidence in Figure 3. The top panel depicts the benchmarked yield index for Amsterdam (red) between 1625 and 2020. This index depicts an imputed yield based on our rent index and the house price index from Korevaar et al. (2021). In the same graph, we also plot actual observations of average market yields (blue) based on simultaneous observations of rent and sales price for the same assets, for three sub-periods. In the bottom panel, we provide a scatterplot of yields for the other cities, together with an estimated time trend adjusted for city fixed effects, with Antwerp used as the baseline.

– Insert Figure 3 about here –

While the evidence from Amsterdam in Figure 3a shows that housing yields can deviate for extended periods of time from their long-term averages, in line with existing evidence (e.g Campbell et al., 2009; Ambrose et al., 2013), there is no structural trend in yields over time. Rental yields were relatively low before 1800, fluctuating between 2.5 and 5.0% for almost two centuries. During the two centuries after that, average yields were higher, but also more volatile. Their current low levels are unprecedented.

For the other six cities, provided in Figure 3b, the overall picture is similar. There is substantial variation in individual property yields, either driven by cross-sectional or time-series variation, but no structural trend. The estimated trend in housing yields is close to and not significantly different from zero (-0.1 bp per year) and precisely estimated (standard error: 0.2 bp). Full regression output for the trend line estimation, both for Amsterdam and the other cities, is provided in Appendix Table 12.

One reason for the absence of a trend in housing yields is that the risk profile of rental housing investments in these cities has remained more stable over time than those of government bonds. Although housing yields have indeed declined substantially in recent times, we find no evidence that rent price growth and house price growth differed structurally in the long run.

3 Rent Dynamics: A Basic Framework

In the previous section, we showed that long-term trends in rent price growth were very comparable across cities, but that economic shocks could substantially affect urban rent price dynamics over shorter horizons. In this section, we present a basic framework of the determinants of rent price growth to reconcile these two facts and to motivate our key hypothesis in this paper: the predictability of housing rents over medium horizons.

First, following the classic Rosen-Roback model, we start from the assumption that rental prices (r) in equilibrium equate the value of relative wages (W) and amenities (A) across cities. Thus, $r = W + A$.[6] Homes are being constructed if the present value of future rents exceeds construction costs. Thus, if a city experiences a positive demand shock $\Delta > 0$ pushing up wages and/or amenities, the city will attract more inhabitants, pushing up prices. New construction or redevelopment will occur until the present value of rents again equates to housing construction costs. Second, following Glaeser and Gyourko (2005), negative demand $\Delta < 0$ will push down wages and/or amenities, reducing the present value of rents below replacement cost. However, the short-term effect on the housing supply will be small, as housing is a durable good and will only deteriorate gradually.

The combination of these two findings motivates the Glaeser and Gyourko (2005) finding of a stronger house price elasticity with respect to urban decline than with respect to urban growth, measured over horizons of 10 years. As acknowledged in Glaeser and Gyourko (2005), such dynamics might be captured more directly in rent changes, because these reflect current demand-supply conditions and are not forward-looking towards expected future changes. In the history of our cities, the fall of Antwerp in 1585 and the subsequent migration wave to Amsterdam is an interesting example to see both mechanisms at play for rents: between 1585 and 1590 rents declined by -0.10 log points per year in Antwerp but rose by 0.05 log points per year in Amsterdam.

However, over the very long run, real rent development has not differed much across cities. Real wage developments across cities have been even more similar, growing at an annualized log growth rate of about 0.4 percent per year. This is not surprising: these regions were already economically integrated early on, with existing evidence pointing to significant

[6] Here, we assume for simplicity that all properties in a city have the same location amenity

international labor migration in the early-modern period (e.g. Van Lottum, 2007).

The question is thus why we observe substantial differences in rental prices even across horizons of multiple decades. The key suggestion we make in this paper is that this disparity over time horizons is driven by the fact that full supply adjustment back to equilibrium is slow. If the supply response in the short term is limited, it takes very long to restore equilibrium so that the short-term rent response to a population shock overshoots its long-term value. Thus, current population growth negatively predicts future rental growth, at least over medium horizons of about ten years to several decades. To illustrate this point, we will use examples from the planning histories of the cities studied in this paper, both for urban growth and decline.

3.1 Supply Adjustment in Growing Cities

Much modern literature argues that strict building regulations and zoning are a key cause of expensive housing and slow construction in modern cities (e.g., Glaeser and Gyourko, 2003; Quigley and Raphael, 2005; Hilber and Vermeulen, 2016). Although varying over time, such regulations have impacted urban development throughout history.

In pre-modern times many of our cities constrained construction outside of the city walls, typically for defense and tax reasons. The extent to which such legislation was enforced likely varied across cities; the *faubourgs* (suburbs) in Paris gradually became an integral part of the city (Descimon and Nagle, 1979), while similar developments were more actively prevented in Antwerp, London, and Amsterdam (e.g. Soly, 1977; Baer, 2007a; Abrahamse, 2010).

Some cities also took more proactive measures to restrict building activity. For example, London tried to limit urban growth and passed various laws in the 16th and 17th centuries that directly prohibited the construction of new housing in the city. Lawmakers appeared to believe that such laws were a solution to the problem of London's rapid growth, hoping that a tight housing market would deter migrants (Baer, 2007b). Only late in the 17th century did London gradually shift to a policy that accommodated rather than restricted urban growth. Baer (2007a) also notes swings in urban growth policies over time in other cities, most notably in Paris.

Many of the cities in our sample experimented early on with zoning and a centralized approach to urban planning. However, such processes and efforts were typically slow and complicated, and often met resistance. For example, when Antwerp's population grew rapidly in the 16th century, the city made efforts to expand and refortify, in particular after a major attack on the city in 1542. For years, this process barely moved forward until the city in 1549 commissioned a master builder to expand the city and strengthen its fortifications (Soly, 1977), but while he acted boldly, protesting property-owners delayed the project, and by the time this was settled housing demand had waned (Tijs, 1993; Baer, 2007a).

When Amsterdam's population grew quickly after 1585, the city's leaders started to make plans to significantly expand both the city itself and its fortifications, but it took until 1609 for the government to agree on a major expansion plan that was gradually executed in the 1610s (Abrahamse, 2010). Lawmakers agreed on the next major expansion in 1662, but by the time development was well on its way (in the 1670s), the Golden Age had ended, population growth had ceased, and many plots of land remained empty for decades.

There are also more recent examples. Paris' medieval city center had long been significantly overcrowded and very unhealthy (Francke and Korevaar, 2021), but modifying it was a hugely complex task that in the mid-19th century was given to Haussmann. His famous renovations substantially expanded the housing supply and improved health conditions in the city, but it took decades to complete and it experienced significant resistance. Scholarship on the benefit of his works is divided until today (e.g Freemark et al., 2021).

When Amsterdam's population started growing again in the 19th century, the government was slow to agree on a plan to extend the city. The first extension plan was rejected in 1866, accepted in revised form in 1877, and executed in the remainder of the 19th century (Smid, 2019). By that time, sustained population growth called for renewed urban expansion, and a plan was accepted in 1904, with execution starting only after 1917. Similarly, plans in the 1920s for a general extension plan were not signed into law until 1939 and then halted by World War II.

In short, a sluggish supply response to large increases in urban housing demand appears much more the norm than the exception. In the short term, building regulations slow down supply adjustment. Even within decades, plans for large-scale urban expansion are often slow and complex to realize.

3.2 Supply Adjustment in Declining Cities

While most of the cities in our sample have grown substantially over the long term, they have also experienced substantial periods of urban decline. Antwerp, Ghent, and Bruges lost substantial population in the 16^{th} century, Amsterdam at the end of the 18th century. Almost all of our cities lost population in the second part of the 20^{th} century, even when considering their entire urban areas, and have only resurged in the past decades.

In declining cities, redevelopment doesn't tend to be financially attractive (Glaeser and Gyourko, 2005; Rosenthal, 2008). Both today and in history, urban decline has typically been associated with a deteriorating or low-quality housing stock. However, such effects come with large negative externalities such that governments might want to counteract with revitalization policies (Rossi-Hansberg et al., 2010). For example, after decades of decline, Detroit has started to demolish a large amount of dilapidated housing, boosting local property values (Paredes and Skidmore, 2017). Similarly, if property owners cannot rent out their housing, it might be economically optimal to transform properties or assemble lots for better use.

Among our cities, urban decline has been studied most extensively in Bruges. Deneweth (2008) shows that between 1583 and 1667 the number of properties in Bruges declined by more than 16%, citing active renovations and gradual land assemblies due to lower population pressure. During the same time period, the number of non-housing units increased. The government also pro-actively took measures to 'hide' vacancies and improve the look of the city, for example by prohibiting the teardowns of facades but promoting redevelopment by changing walls and lot assembly (Deneweth et al., 2018). For Amsterdam, Lindenthal et al. (2017) have shown similar processes for the 19th and 20th centuries. In short, redevelopment and gradual demolitions imply that the housing supply will eventually adjust to structural negative shocks in housing demand, but it can take a long time.

4 Rent Growth Predictability

We now test whether past shocks to housing demand indeed influence current rent growth over short- to medium horizons. In this section, we investigate rent predictability at the market level, while the next section looks at predictability at the contract level.

We start by considering various descriptive regression models, which aim to explain current rental growth rates with past shocks to urban housing demand. To capture shocks to housing demand, we primarily focus on changes in population growth, in line with existing literature (Glaeser and Gyourko, 2005; Combes et al., 2019). Most changes in total urban housing demand are directly or indirectly linked to population changes and, importantly, using population changes allows for a clear distinction between periods of urban growth and urban decline. Alternative economic fundamentals, such as wages or lagged rent growth, are noisier, need to be converted into real terms, and lack the unambiguous classification into boom and decline states.

If supply is sluggish in responding to population shocks, then past changes in population numbers could still impact prices today. Most of the historical evidence in Section 2 indeed relates to the slow response of the housing supply to changing population numbers. In the first model, we regress changes in housing rents (r_{it}) on lagged values of population change, adjusting for city fixed effects (μ_i). We estimate the elasticity for positive shocks ($\Delta_s^+ pop_{it}$) and negative shocks ($\Delta_s^- pop_{it}$) separately. Here, $\Delta^+ / -_s\, pop_{it}$ reflect population growth rates interact with two dummy variables that take the value 1 in case of population growth (+) or decline (−), respectively, and 0 otherwise. As there might be persistence in shocks to urban housing demand, we also estimate a second model that controls for contemporaneous changes in demand variables, adding variables for changes in *current* population growth, wages, and consumer prices (in line with Glaeser and Gyourko, 2005). Again, we separately estimate the elasticity for positive population shocks ($\Delta_s^+ pop_{i,t}$) and negative population shocks ($\Delta_s^- pop_{i,t}$). In the most extensive model, we estimate the following equation, for each rent observation at time t in city i:

$$\Delta_{25} r_{it} = \mu_i + \beta_1 \Delta_{25}^+ pop_{i,t-1} + \beta_2 \Delta_{25}^- pop_{i,t-1} + \gamma_1 \Delta_{25}^+ pop_{i,t} + \gamma_2 \Delta_{25}^- pop_{i,t} + \gamma_3 \Delta_{25} w_{it} + \varepsilon_{i,t} \quad (4)$$

Since we focus on medium-term changes over several decades, we use overlapping 25-year changes for all our variables ($s = 25$). In later specifications, we assess the robustness of our findings to the time horizon ($s \in (5, ..., 75)$). To account for the serial correlation introduced by the overlapping observations, and the potential spatial auto-correlation across cities, we use standard errors based on Driscoll and Kraay (1998) with lag length equal to the time horizon plus five.[7]

In all estimates, we only include data up to 1913, the last year of peace before World War I. From World War I until roughly the 1980s, rents were regulated in most cities, implying that regulation and market forces were together setting prices. Finally, we exclude data from London before 1800 and Bruges after 1800 because in these periods our indices are smoothed and relatively imprecise due to thin data.

We estimate regressions in our baseline model using real changes in rents and wages. Real estimates are preferred because they allow adjusting for the presence of structural price inflation, as was common in the 16th century. However, strong volatility in historical food prices implies that some of our real rent changes will be driven by short-term consumer price volatility. For this reason, we also provide estimates using nominal changes in rents and wages.

– Insert Table 3 about here –

Table 3 provides the results of these different analyses. We start with the most basic specification in column 1, which tests for predictability of current 25-year changes in real rents with lagged 25-year changes in population growth. We find very weak evidence for predictability, with no effect for growth and a negative but weakly significant effect for population changes. The R^2-statistic of the purely predictive regression is also low.

In our cities, we find a strong correlation between past population growth and current population changes over 25-year horizons ($\rho = 0.54$) but a much weaker correlation between past population decline and current population changes ($\rho = 0.11$). This implies that if supply adjusts only gradually to demand growth, the negative effects of an increasing housing supply on the rent level may (partly) be offset by the positive effects of continued population growth. Thus, we need to control for current demand shocks to estimate the effect of past population shocks on current rent price growth correctly.

In Column 2 of Table 3, we show the results when adding controls for contemporaneous demand shocks. Controlling for these factors, we find that lagged changes in population strongly negatively predict current changes in rent prices, but that the effect of current population changes is stronger and pointing in the other direction. In line with Glaeser and

[7] Appendix Table 13 compares p-values for 25-year horizons based on overlapping and non-overlapping regressions. This results in comparable p-values and does not change our main results.

Gyourko (2005), we find that negative shocks to the urban population have a stronger price effect than positive shocks. This holds both when considering only current changes and when aggregating the effect of current and lagged changes. A one percent decrease in the current population reduces prices by 1.43 percent, but increases prices in the next period by 0.36%, resulting in a net effect of around 1 percent. For positive shocks, the net effect is much smaller. If we combine the effects of lagged and current population changes we document a long-term elasticity of housing rents that is close to zero. On top of this, we find a large effect of changes in the real wage level: a 1 percent increase in the real wage is associated with a 0.76 percent growth in the real rent.

The fact that the elasticity of urban growth is small in the long term is in line with the findings of Glaeser and Gyourko (2005) and Combes et al. (2019). The key addition of our paper is that we explicitly consider predictability and the dynamics over longer time horizons. In Column 3, we present a specification that only includes current changes in population. Again, the elasticity with respect to current urban growth is smaller than the elasticity of urban decline. Unsurprisingly, the effect is smaller compared to Column 2, because it does not explicitly control for the negative effect of previous population changes. Most importantly, adding lagged population growth rates substantially improves the fit of the model, adding about six percent in explanatory power. We cannot establish causality over these long time horizons, but the results are strongly in line with the basic framework we have described in the previous section.

We run a number of additional analyses to examine the robustness of these effects. First, we estimate equation 4 using the full sample of data, including the post-1914 period and the periods with more limited data for London (pre-1800) and Bruges (post-1800). The results are in Column 4 of Table 3. We consider the specification based on the restricted sample as more reliable, but including potentially less representative and more recent data does not alter the results substantially.

Next, we test whether the use of interpolated population data influences the results. To compute current and lagged population growth rates, we compare the current population to the population 25 years ago, and the population 25 years ago to the population 50 years ago. If the population number 25 years ago relies on interpolation, it will artificially generate a correlation between lagged and current population growth rates. To assess whether this impacts our findings, Column 5 in Table 3 computes the effects based on a sample of data that only includes observations where the population number 25 years ago was based on an actual price observation. Again, we do not find that this changes our estimates much.

Finally, we consider whether the results are sensitive to using real growth rates in wages and rents. Column 5 reports estimates based on nominal rather than real growth rates, additionally incorporating a control for consumer price changes. While the size of the coefficients changes a little bit, the main results remain unchanged.

The analysis in Table 3 relies on 25-year changes in rent prices. However, if supply adjustment is gradual, the impacts of past population shocks on current rent prices might depend on the time horizon. If the time horizon is long enough for supply to adjust fully, we would expect no effect of lagged changes in population growth on current prices. On the other hand, if it takes very long for cities to plan and execute urban expansions, supply adjustment might be slow initially but may speed up substantially over medium- to long horizons. This implies we might expect the strongest reversals in rent prices over medium horizons. For periods of population decline, the housing stock will deteriorate gradually and constantly, implying that the annualized *pace* of supply adjustment might not differ much across time horizons.

To test for this, we re-estimate equation 4 including all controls, varying the time horizon used to calculate growth rates from 5 years to 75 years. We do not use horizons shorter than 5 years because estimates of population changes are not available at such a high frequency. Figure 4 reports the results on the two key coefficients of interest: the impact of lagged population growth and lagged population decline on house prices.

– Insert Figure 4 about here –

We find that lagged negative shocks to population growth predict reversals in prices over all horizons. There do not appear to be strong differences in the magnitude of the effect across time horizons, suggesting the pace of supply adjustment is constant over time. The effect is not significant for all time horizons, which relates to the fact that the number of periods with population decline is comparatively small.

For past positive shocks to population growth, we find that the effect varies considerably across time horizons. For short horizons, we find no significant predictability. However, the magnitude of the price reversal increases substantially for time horizons up to 10–15 years. This implies that over horizons of 10–15 years, past 10–15 year population growth rates strongly negatively predict current rent price growth. Again, we should note that this only holds after controlling for current demand shocks. For longer horizons, the magnitude of the effects declines gradually and becomes insignificant for time horizons of over 50 years. Our interpretation is that over time horizons of more than half a century, the housing supply is able to adjust fully to the positive population shock so that lagged population growth rates do not predict current rental growth rates anymore.

5 Asset-Level Rents and Predictability

Until now, all our analysis has been focused on explaining city-level movements in rent prices using city-level data. However, we have neglected most of the information stored in individual-level contract data. In this section, we explore whether current contracts contain relevant information about movements in future rental prices and rental risk. We separately analyze each of these two components.

5.1 Expected rental growth

Our analysis in the previous section has shown that past shocks to housing demand still affect current rental growth. If both tenants and landlords realize that shocks today might have an impact on future rental price development, they could incorporate these expectations when setting rental prices. The question is whether they do.

We exploit an interesting future of our Paris data that enables us to observe rental prices for different contract lengths of 3, 6, and 9 years. We use these relative prices to back out the implied term structure of future rental prices and compare these to realized changes.[8] Our analysis starts from the basic assumption that the value of a long-term rental contract should be equal to the value of multiple shorter-term contracts. That is, the present value of future rental payments on multiple shorter-term contracts should equal the present value

[8]We also observe rental contract lengths for London and for a subset of Amsterdam data. For the London sample the number of repeated contracts is very limited, making it impossible to precisely separate long-term contract premia from differences in rental prices. For Amsterdam, nearly all contracts are of one-year length and the number of long-term contracts is exceedingly small.

of future rental payments on a single long-term contract. If investors and tenants are risk-neutral, the prices of a long-term contract should be equal to the current price of a short-term contract plus a premium that compensates for expected future changes in rental prices during the contract period.

$$\sum_{i=0}^{n} \frac{R_{t=0} + \pi}{(1+\delta)^t} = R_{t=0} + \sum_{i=1}^{n} \frac{\mathbb{E}[R_{t=i}]}{(1+\delta)^i} \tag{5}$$

Here $R_{t=i}$ is the price of a rental contract at time i, δ the discount rate and π the premium for a long-term contract. Clearly, the premium for such a contract will depend on the expected rate of rental growth during the duration of the contract. As such, we can rewrite equation 1 by adding a premium. That is, the rental price for a property i with contract length j signed at time t equals:

$$r_{i,j,t} = \alpha_i + \beta_t + \pi_{jt} + \epsilon_{it} \tag{6}$$

If we make assumptions on the discount rate δ, we can use the actual rent price indices for Paris to back out the theoretical long-term contract premia implied by the model for each rental contract at each point in time if investors had perfect foresight. Subsequently, we can estimate Equation 2 on all repeat-rent pairs with the change in the theoretical rental price premium added as the independent variable:

$$r_{i,t=y} - r_{i,t=x} = \sum_{t=1}^{T} \beta_t D_{t,i} + \gamma(\pi_{j=w,t=y} - \pi_{j=u,t=x}) + \tilde{\epsilon}_{it} \tag{7}$$

Here, we measure rent prices for a certain property at two time periods (t): time period x and time period y. The contract lengths (j) at these moments are respectively u and w. These contract lengths could be the same but they could also differ. If the coefficient on γ is close to one, rental price premia on long-term contracts on average equal the realized price difference between short- and long-term contracts, implying tenants and investors on average correctly anticipate future rent price changes. However, if investors and tenants cannot anticipate future rent price changes, for example, if they believe rents follow a random walk, we should expect a coefficient that is not different from zero.

We make use of all Paris repeat-rent pairs for which we both know the prices and the contract lengths to estimate this regression, which is a modification of the standard Bailey et al. (1963) repeated observations model. Contracts with lengths of over 21 years are excluded (14 contracts). Of the remaining 2,064 contracts, 93 percent have a length of either 3 years, 6 years, or 9 years. Their dates range from 1507 to 1788.

Because we do not know the discount rate and how it changes over time, we will use several different discount rates to back out the theoretical premium on long-term contracts, while still using realized prices. In Paris, 5 percent was the typical rental yield assumed for tax reasons (Eichholtz et al., 2021). We use this as benchmark discount rates and also report results using discount rates of 2.5 and 10 percent.

Table 4 reports the results. In the first three columns, we report the outcomes using discount rates of 2.5, 5, and 10 percent. In all three cases, we find a coefficient just below but not significantly different from one, so landlords and tenants seem to anticipate future rent price changes correctly, on average. It is not surprising that the impact of discount rate changes remains limited since we look at variation in rental contracts between 3 to 9 years so that the compounding effect remains small under a set of reasonable discount rates.

– Insert Table 4 about here –

One limitation is that our specifications in the first three columns do not allow for a general premium on long-term contracts, since we assumed tenants and landlords to be risk-neutral. Long-term rent contracts provide a hedge against rent price volatility for the duration of the contract for both tenants and landlords. However, for landlords, long-term contracts limit the option value of their investment by making it more difficult to sell or remodel the house. Additionally, we should expect tenants to care more about housing security than investors about rental price volatility, given that tenants tend to be more financially constrained than landlords.

To account for this, Column 4 adds a control for the change in contract length in the regression. This coefficient is about 2.7 percent and significant at the 10 percent level. This implies that a one log increase in contract length, for example from 3 years to 9 years, increased the rent price by about 3 percent. Adding this control reduces the size and significance of the coefficient on theoretical realized long-term contract premia, but the coefficient remains close to the estimates in the earlier three columns and is not statistically different from one.

Given the relatively limited set of data on repeated contracts, we do not have sufficient statistical power to investigate other drivers of this long-term contract premium. However, all our specifications suggest that tenants and investors can anticipate future changes in rental prices. Thus, even though it is difficult in our sample to identify statistical predictability of rents over short horizons of less than 10 years, realized contract rents suggest that, on average, investors and tenants do correctly predict where rents are going.

5.2 Property-level rental risk

We finally explore predictive components in rental risk at the asset-level. Most investors in the housing market only hold small portfolios, so they are strongly exposed to idiosyncratic risks. For long-term rental investors, property-level contract price risk is thus a significant contributor to total risk. We investigate to what extent past information about the riskiness rental contract prices is informative about the future riskiness of rental contract prices.

To measure contract-level risk we compare property-level rental prices to market-wide trends. To compute property-level idiosyncratic price risk we first measure the excess log growth rate for each rent revision we observe. Specifically, we define the excess growth rate Δ_e as the difference between the log rental growth rate at the asset- and at the market level:

$$\Delta_e R_{i,t_0,t_1} = \Delta R_{i,t_0,t_1} - \Delta R_{index,t_0,t_1}. \tag{8}$$

The market-level growth rate is based on the indices we have estimated and the asset-level growth rate based on the set of repeated rental contract prices. The time between revisions $(t_1 - t_0)$ can differ across properties and contracts. Mechanically, current property-level deviations from the index should predict reversals to the extent that they reflect transaction price errors in rental contracts. To measure risk, we therefore focus on the absolute value of Equation 8: Properties with higher idiosyncratic contract price risk should have higher average absolute log pricing errors: $\text{abs}(\Delta_e R_{i,t_0,t_1})$. Note that in our definition, this idiosyncratic risk might reflect both pure transaction price risk and potential segmentation across property type, location or property quality.

We test whether the absolute realized excess growth rate $\text{abs}(\Delta_e R_{i,t_{past},t_0})$ is predictive of the future absolute excess growth rate $(\text{abs}(\Delta_e R_{i,t_0,t_{future}})$. We also investigate whether the future absolute excess growth rate is predictable by market-level uncertainty in rental prices. To do so, we compute for each absolute future excess growth (abs($\Delta_e R_{i,t_0,t_{future}}$). the mean absolute error (MAE) for the excess growth rates for all *other* revisions, 1–3 years *prior* to t_0.

For each of our seven cities and for all contracts, we calculate these rates for all the years in which we observe individual rental cash flows. Table 5 provides statistics for these rates, as well as information regarding the periods for which we calculate these for each city. We find an average absolute error of 9.7 percent ($e^{0.093}$) for all cities. This estimate varies across cities, ranging from 3.4 percent for Amsterdam to 22 percent for London. Unsurprisingly, the pattern in cross-sectional mean absolute errors and property-level errors is the same.

– Insert Table 5 about here –

We then test whether the observed excess growth rates can be explained as a linear combination of prior deviations for the same asset and a market-wide measure of noise in rent setting in the three years *before* revision ($MAE_{3y\,before}$).[9] We use the following regression model to test this:

$$abs(\Delta_e R_{i,t_0,t_{future}}) = \alpha + \beta_1 abs(\Delta_e R_{i,t_{past,t_0}}) + \beta_2 MAE_{i,-3y} + \beta_3 abs(\Delta_e R_{i,t_{past,t_0}}) \times MAE_{i,-3y} + \epsilon_{i,t_0} \tag{9}$$

First, we pool the observations from all cities but then also estimate the regression equation independently for each city, using ordinary least squares (OLS) with robust standard errors.

Table 6 illustrates both asset-level prior excess growth rates and market-wide uncertainty levels are strong predictors of future excess growth rates. For all cities combined, the coefficient for the absolute error is 0.317, so an increase of one percent in the current absolute excess growth rate is associated with a 0.37 percent ($e^{0.317}$) higher absolute excess future growth rate. This suggests there is persistence in property-level rental price risk: properties whose prices are volatile relative to the market in previous periods will continue to be volatile relative to the market in future periods. The intuition here is that some specific properties are inherently more risky than others, resulting in persistently strong price movements relative to the market. Some might also reflect differential rent-setting practices across landlords.

[9] We also tried different periods for the market-wide rent revisions, and our result our robust.

Second, for the measure of market-wide uncertainty, we find an effect of an even greater relative magnitude ($e^{0.684}$), suggesting there is also persistence in rent-revision dispersion. This implies that if there was significant dispersion in the pricing of *other* contracts in the previous three years, the new contract on an individual property will also on average have more dispersed pricing relative to the market in the future. This persistence could for example be driven by increased segmentation of the market (e.g., across neighborhoods) or the fact that market-level uncertainty makes it more difficult for tenants and landlords to apply market pricing.

Interestingly, the interaction term between MAE and absolute excess growth rates is negative ($e^{-0.899}$). This suggests that if uncertainty in the market increases, it primarily does so because rental prices of properties that were initially less volatile relative to the market now become more volatile. In summary, our descriptive analysis suggests there are persistent differences in idiosyncratic risk across properties, but that increased uncertainty in the market as a whole erodes the advantage of seemingly low-risk properties just when it is needed most.

– Insert Table 6 about here –

6 Conclusion

In this paper, we have presented a long-term view of urban rental markets in Western Europe, relying on newly constructed indices of rents. For the first time, it is possible to trace the rental trajectories for various European cities from 1500 to the present on a continuous annual basis.

Until the 19th century, growth in real urban market rents was close to zero or even negative. Following sustained urban population growth, housing rents rose substantially during the 19th century. Importantly, direct government interference in the rent level did not exist in the first four centuries we study, and the interplay of market forces seems to have stabilized long-term real rent levels, despite substantial short-term volatility. In the 20th century, housing rents rose in aggregate, but growth has slowed down significantly.

We show that housing yields do not trend up- or downwards over the long term, suggesting that rental prices are the main determinant of housing returns over the long term.

Without our sample, we find that over the long term the real rental growth rates in each city have varied between 0 and 0.3 percent per year. In general, we find the lowest growth rates in cities that were economically leading in 1500 but did not maintain that status, such as Ghent and Bruges, whereas finding strong rent growth in cities that kept growing, such as Amsterdam, Paris, and London.

Over medium-horizons, rental growth rates across cities vary substantially. Our results indicate that future rent growth is predictable, both at the market level and at the individual level, and that landlords and tenants use these predictions when making choices regarding rent levels for different contract maturities. We find strong supporting evidence for predictions made by Glaeser and Gyourko (2005) and Rosenthal (2008) regarding growing and declining cities and the effects on housing rents, with much stronger rent elasticities for declining cities than for growing ones. However, we show this effect primarily plays out over very long horizons. Sluggish supply responses imply that the response of rent prices to positive demand shocks is much larger over the medium term so that prices revert over longer horizons.

The data collected here provide a valuable source for economists and economic historians. To our knowledge, the dataset presented in this study is the largest historical urban rental housing dataset constructed to date, and by providing the data and resulting indices to all interested researchers, we hope to have created a solid basis for future research on the long-term history of housing markets.

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Tables and Figures

Table 1: Variance Parameters and Signal-to-Noise Ratios

City	*Years*	*Obs.*	*Prop.*	σ	q_ζ	*Log likelihood*
Amsterdam	1550-1940	19,299	1,228	0.06	0.72	18,475.73
Antwerp	1500-1940	6,133	473	0.15	0.54	430.16
Bruges	1500-1920	3,115	592	0.20	0.25	-449.50
Brussels	1500-1940	4,304	894	0.17	0.40	-142.62
Ghent	1500-1940	6,495	1,278	0.21	0.34	-1,167.45
London	1500-1903	1,624	660	0.25	0.20	-400.32
London	1903-1959	3,165	1,141	0.08	0.56	1,469.50
Paris	1400-1870	8,712	2,364	0.15	0.53	416.00

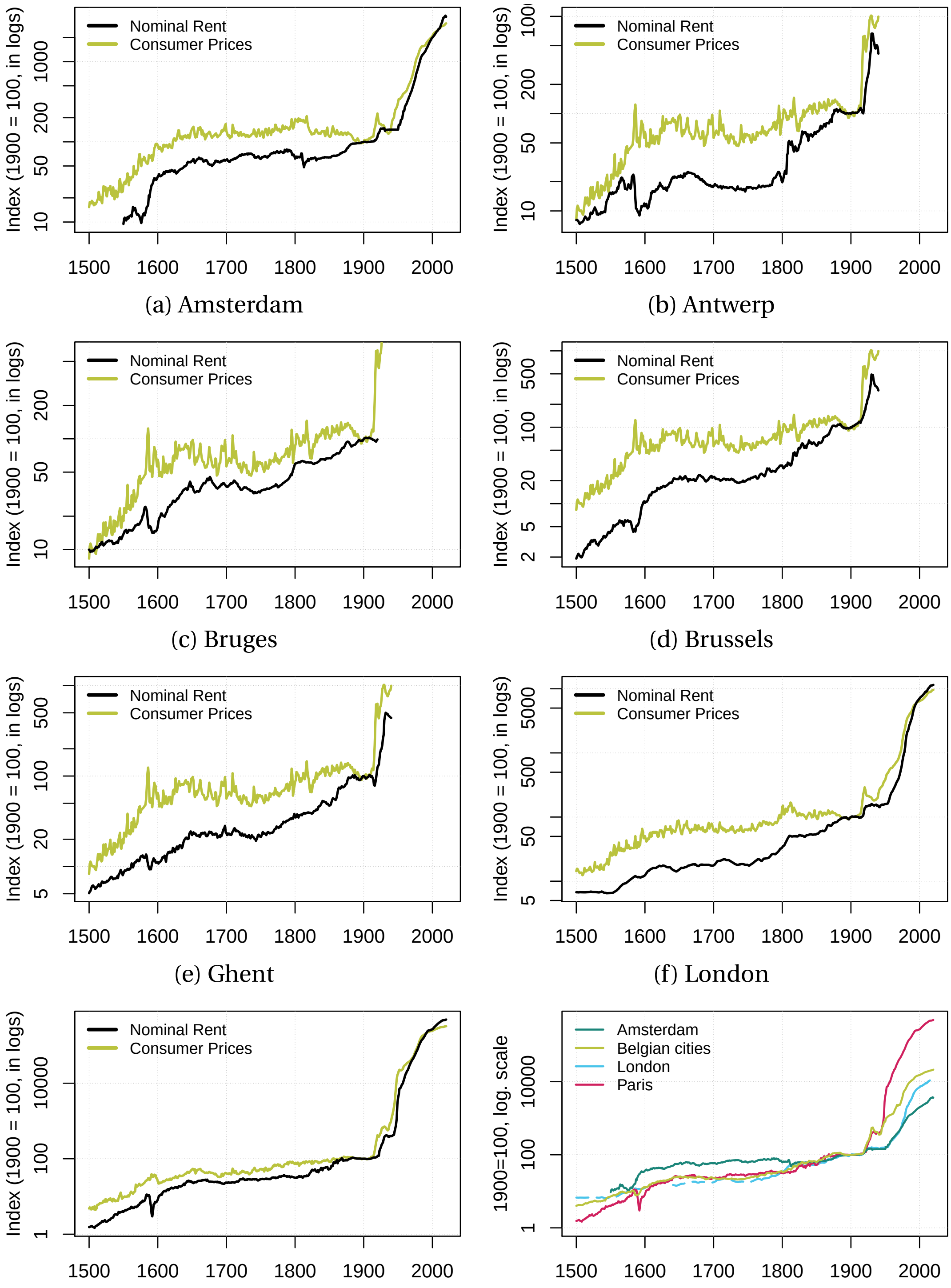
Notes: This table provides summary statistics for the estimated rent price indices, following the method of Francke (2010). q_ζ measures the signal-to-noise ratio, and σ measures the standard errors of the annual price movements of the index. Signal-to-noise ratios are lower for cities for which few data are available to estimate the index.

Table 2: Annual Geometric Rent Growth Rates

		Real growth		*Nominal growth*		*m2 price*
City	*Years*	μ	σ	μ	σ	2020, euros
Full sample						
Amsterdam	1550-2020	0.28%	7.7%	1.27%	4.0%	23.5
Belgian Cities	1500-2020	0.12%	12.6%	1.65%	4.2%	15.9
London	1500-2020	0.18%	8.5%	1.43%	3.1%	36.5
Paris	1500-2020	0.30%	9.8%	2.43%	7.9%	28.6
Belgian Cities						
Antwerp	1500-1940	-0.15%	14.4%	0.89%	6.0%	
Bruges	1500-1920	-0.44%	13.6%	0.55%	2.5%	
Brussels	1500-1940	0.11%	13.7%	1.12%	4.3%	
Ghent	1500-1940	-0.03%	12.6%	1.01%	4.5%	

Notes: This table provides summary statistics on the mean (μ) and standard deviations (σ) for the estimated rent price indices in both nominal and real terms. The top four rows show the long-run estimates until 2020, combining the primary index with secondary indices from the mid-20$^{\text{th}}$ century onward. They also display the rent price per square meter for a property in the city center (in euros) in 2021. The aggregate numbers for the Belgian cities are a population-weighted average of the city indices until 1940, afterward a national rent index is used.

Figure 1: Nominal Rent and Consumer Price Indices, 1500–2020



(a) Amsterdam (b) Antwerp

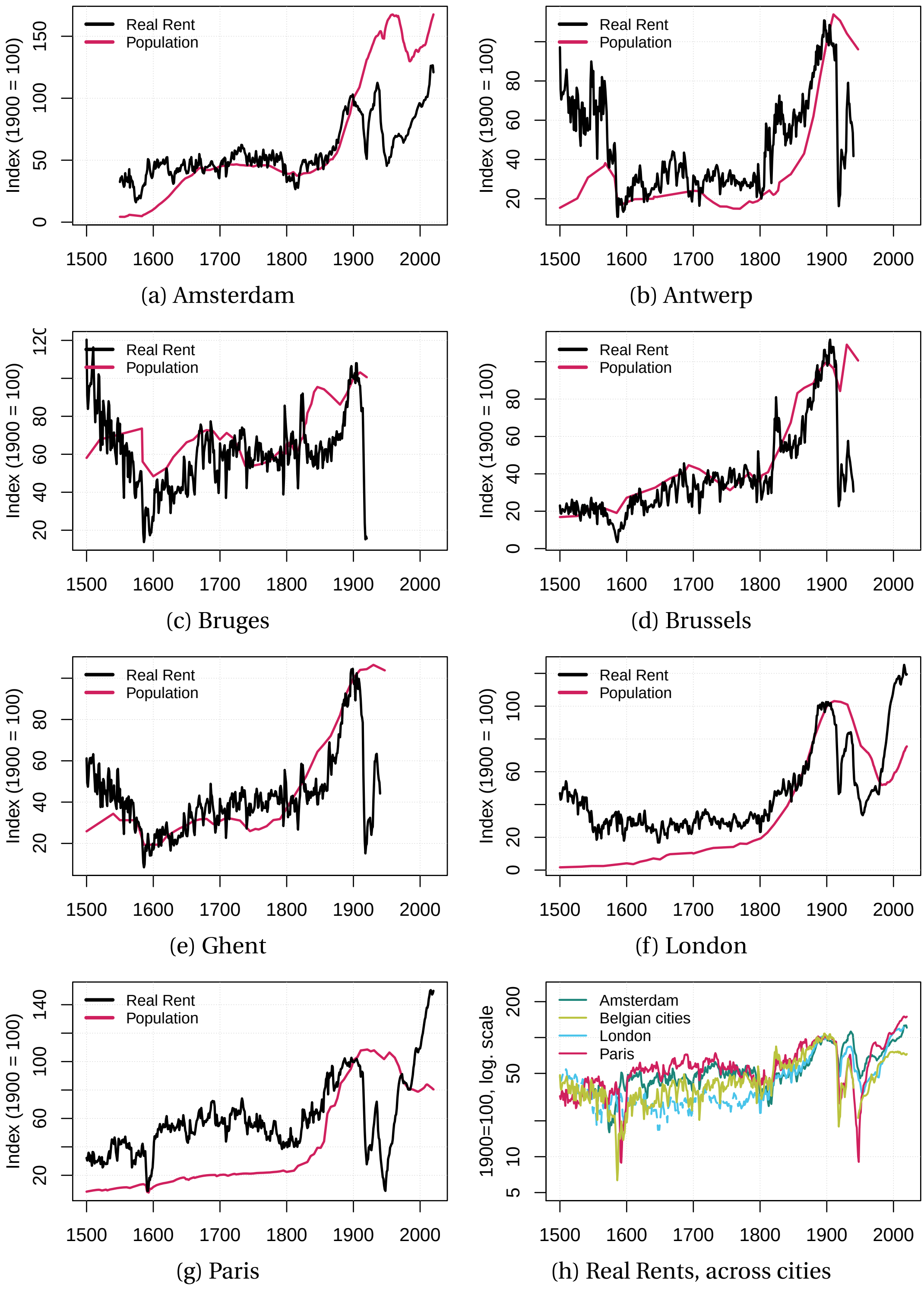
(c) Bruges (d) Brussels

(e) Ghent (f) London

(g) Paris (h) Nominal Rents, across cities

Notes: Scale of Y-axis in logs. The graphs provide rent price indices for 7 cities (black lines), compared to consumer price indices (green lines).

Figure 2: Real Rent and Population Indices, 1500–2020



(a) Amsterdam (b) Antwerp

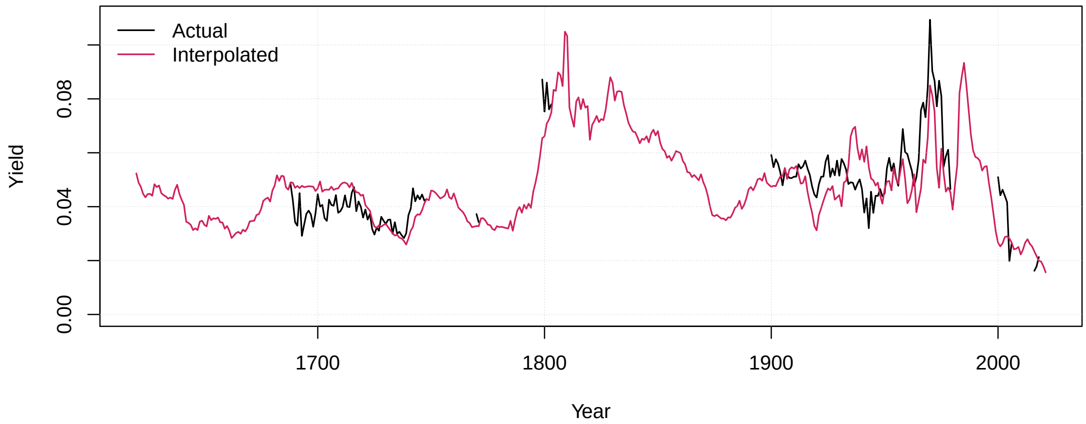
(c) Bruges (d) Brussels

(e) Ghent (f) London

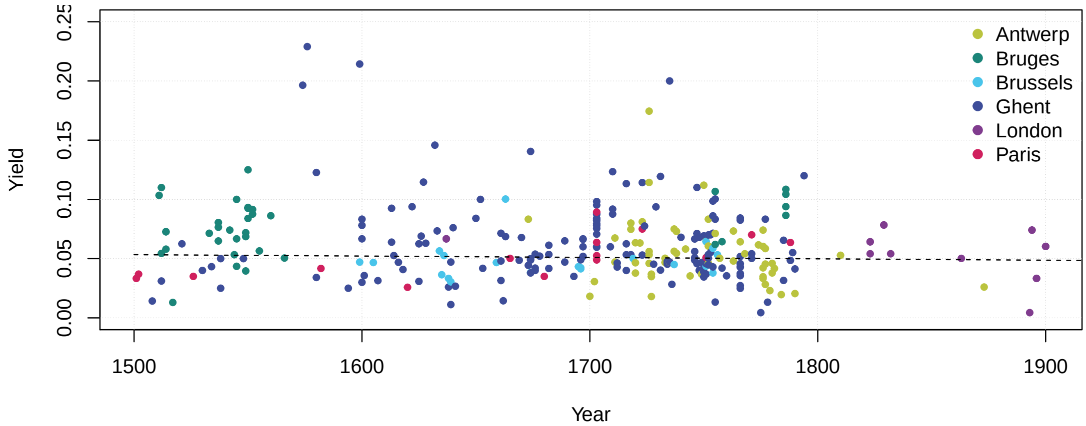
(g) Paris (h) Real Rents, across cities

Notes: This graph shows real rent indices (black lines), as well as population numbers (red lines) based on municipal boundaries. (h) compares the growth of real rental prices across cities. Data is aggregated for the Belgian cities by using a population-weighted average of the individual indices. From 1940, the Belgian index covers all urban areas.

Figure 3: Yields: House Prices and Rents



(a) Net Yield Amsterdam, 1625-2020



(b) Gross Yields, other cities

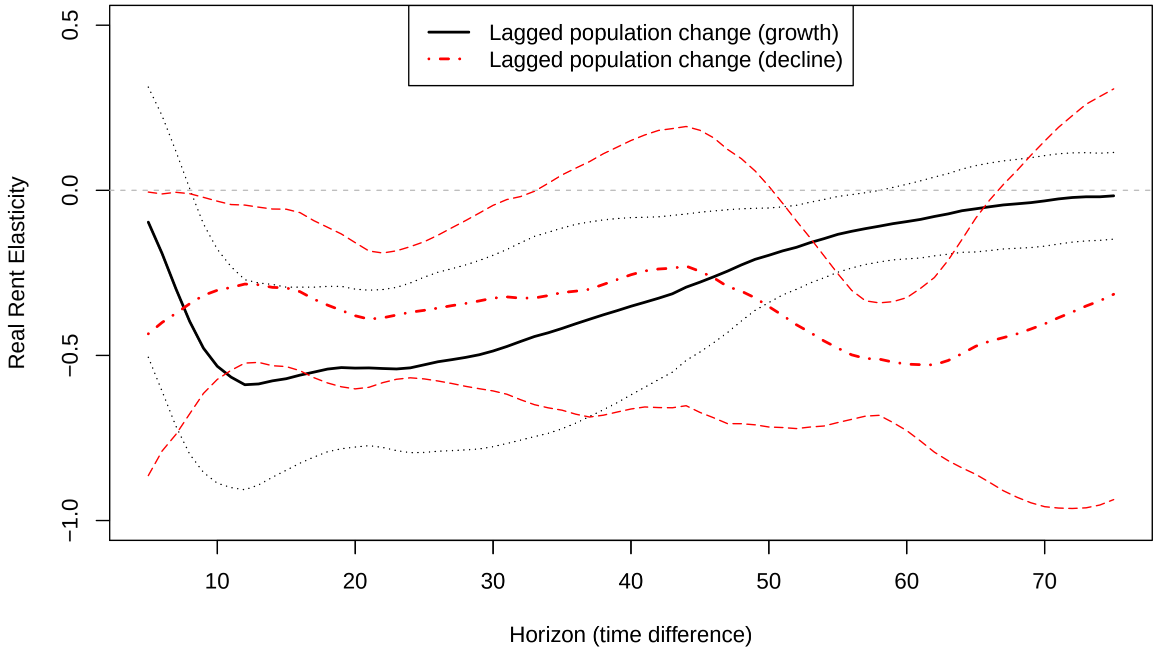
Notes: The top panel plots the estimated net yield for Amsterdam from 1625 to 2020. The red line is the imputed yield based on our rent price index and the house price index from Korevaar et al. (2021). The red line is calibrated based on data on actual housing yields from property auctions, net of costs, collected by Eichholtz et al. (2021); Korevaar (2020). The bottom panel plots gross yields for sold properties in our sample in other cities. The trend line is based on a regression of yields on a time trend with city fixed effects, with Antwerp used as baseline.

Table 3: Results: Market Rent Predictability

	Dependent variable:					
	Real: $\Delta_{25} r_t$					Nominal: $\Delta_{25} r_t$
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta_{25} pop^{+}_{t-1}$	0.183	-0.528^{***}		-0.728^{***}	-0.431^{***}	-0.373^{***}
	(0.174)	(0.135)		(0.244)	(0.110)	(0.122)
$\Delta_{25} pop^{-}_{t-1}$	-0.693^{*}	-0.363^{***}		-0.549^{***}	-0.461^{***}	-0.319^{***}
	(0.375)	(0.106)		(0.133)	(0.128)	(0.111)
$\Delta_{25} pop^{+}_{t}$		0.576^{***}	0.286^{**}	0.782^{***}	0.581^{***}	0.700^{***}
		(0.124)	(0.130)	(0.213)	(0.109)	(0.134)
$\Delta_{25} pop^{-}_{t}$		1.431^{***}	1.448^{***}	1.243^{***}	1.426^{***}	1.049^{***}
		(0.188)	(0.210)	(0.236)	(0.389)	(0.102)
$\Delta_{25} w_t$		0.763^{***}	0.755^{***}	0.712^{***}	0.705^{***}	0.303^{***}
		(0.063)	(0.072)	(0.084)	(0.080)	(0.080)
$\Delta_{25} p_t$						0.125^{***}
						(0.048)
Observations	2,083	2,083	2,083	2,904	281	2,083
R^2	0.032	0.673	0.615	0.482	0.640	0.495
Adjusted R^2	0.028	0.671	0.614	0.480	0.625	0.492
F Statistic	34.278	852.886	1,105.69	537.384	95.659	337.621

Notes: Table 3 reports the results of various estimations of Equation 4. Column 1 only includes lagged population growth rates, Column 2 includes the full specification, and Column 3 excludes the lagged population growth rates. Column 4 considers the full sample (1500-2020, all cities) instead of the restricted pre-1914 sample. Column 5 only includes observations for which population estimates 25 years ago use actual rather than interpolated data. Column 6 uses nominal changes; all other columns use real changes. Driscoll and Kraay (1998) standard errors are reported in parentheses, with a lag length of 30. *p<0.1; **p<0.05; ***p<0.01.

Figure 4: Rent Growth Predictability, 1500–1913



Notes: These plots show estimates of Equation 4 for different time horizons, using pre-1913 data for all seven cities, excluding London before 1800 and Bruges between 1800-1913. Lag lengths vary from 5 to 75 years. The graph depicts regression results of Equation 4 for the model in real terms, using the specifications in Column 2 of Table 3, separated for lagged population growth and decline. Standard errors for the confidence bands are based on Driscoll and Kraay (1998) with a lag length equal to the time horizon of the differences plus five.

Table 4: Premia for Long-Term Contracts

	Dependent variable:			
	Δr_i			
	$\delta = 2.5\%$	$\delta = 5\%$	$\delta = 10\%$	$\delta = 5\%$
$\Delta \pi_i$	0.798^{***}	0.848^{***}	0.960^{***}	0.604^{*}
	(0.294)	(0.311)	(0.346)	(0.343)
$\Delta log(ContractLength)_i$				0.027^{*}
				(0.016)
Time Dummies	Yes	Yes	Yes	Yes
Observations	1,560	1,560	1,560	1,560
R^2	0.606	0.606	0.606	0.607
Adjusted R^2	0.525	0.525	0.525	0.525
F Statistic	7.452	7.454	7.451	7.445

Notes: Table 4 reports the estimates of equation 2. These are based on the 2,064 rental observations that form 1,560 unique repeat-rental pairs and cover Paris from the 16th to the 18th century. To back out the theoretical premium for rent price contracts we use discount rates of 2.5 (Column 1), 5 (Column 2), and 10 (Column 3) percent. Column 4 adds a specific control for changes in contract length; independent of future rent price changes tenants might value long-term contracts higher than short-term contracts. Errors are adjusted for heteroskedasticity. *p<0.1; **p<0.05; ***p<0.01.

Table 5: Summary statistics for excess rental growth rates

Variable		All cities	Amsterdam	Antwerp	Bruges	Brussels	Ghent	London	Paris
Years	Range	1506–1942	1556–1942	1506–1940	1506–1920	1506–1940	1507–1940	1510–1903	1506–1878
abs($\Delta_e R_{i,t_0,t_{future}}$)	Mean	0.093	0.033	0.112	0.155	0.122	0.144	0.199	0.132
	SD	0.124	0.048	0.109	0.144	0.124	0.148	0.193	0.151
$MAE_{3y\ before}$	Mean	0.098	0.034	0.116	0.161	0.133	0.152	0.207	0.135
	SD	0.072	0.029	0.047	0.056	0.053	0.055	0.104	0.068
N		34,268	13,241	5,067	1,857	2,560	4,047	542	6,954

Notes: Absolute excess rental growth rates $abs(\Delta_e R_{i,t_0,t_{future}})$ are the absolute difference between rent changes (in logs) at the asset- and market-level, in logs. The time between revisions ($t_{future} - t_0$) can differ across properties and contracts. The mean absolute error (MAE) is calculated from excess growth rates for all *other* revisions, 1–3 years *prior* to t_0.

Table 6: Predictability of contract-level absolute excess rental growth rates

	Dep. Variable: abs($\Delta_e R_{i,t_0,t_{future}}$)							
	All cities	Amsterdam	Antwerp	Bruges	Brussels	Ghent	London	Paris
abs($\Delta_e R_{i,t_{past},t_0}$)	0.317***	0.374***	0.240***	0.160	0.410***	0.253***	0.325***	0.215***
	(0.014)	(0.028)	(0.042)	(0.099)	(0.059)	(0.052)	(0.101)	(0.027)
$MAE_{i,-3y}$	0.694***	0.807***	0.455***	0.308***	0.599***	0.473***	0.399***	0.503***
	(0.013)	(0.022)	(0.047)	(0.094)	(0.063)	(0.053)	(0.111)	(0.048)
abs($\Delta_e R_{i,t_{past},t_0}$)	−0.899***	−2.997***	−0.812**	0.399	−1.843***	−0.341	−1.022**	−0.381***
$\times MAE_{i,-3y}$	(0.085)	(0.416)	(0.317)	(0.537)	(0.378)	(0.315)	(0.432)	(0.145)
Constant	0.008***	−0.002***	0.044***	0.069***	0.023***	0.043***	0.095***	0.043***
	(0.001)	(0.001)	(0.005)	(0.015)	(0.008)	(0.008)	(0.022)	(0.006)
N	34,268	13,241	5,067	1,857	2,560	4,047	542	6,954
Adj. R^2	0.230	0.331	0.056	0.091	0.065	0.080	0.031	0.078

Notes: This table presents estimated regression coefficients for models where observed absolute excess growth rates are a linear combination of prior deviations from city-wide trends and a measure of city-wide tracking errors (*MAE*) in years *before* revision. Robust standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

APPENDICES

A Discussion of Rental Sources

This section provides an overview of all rental sources, organized per city. A summary of all sources can be found in Table 7.

A.1 Belgian Cities

Most Belgian historical rental studies follow a tradition that has been set up in the early 1960s, most notably with the work of Etienne Scholliers on Antwerp rents, also published in Verlinden (1972). The early works, done by Mason for Bruges (Verlinden, 1972), Van Ryssel (1967) for Ghent, Avondts (1971) for Brussels, and Scholliers (Verlinden, 1972) for Antwerp, focused on collecting housing rents for the largest possible number of representative homes. In each of these studies, representativeness was assessed in terms of location, ownership and fluctuations in rents. In each city, rental observations stem from homes spread all over the city. Due to data availability, practically all rents stem from institutional accounts, as explained in the main body of our paper. The main exception to this case is the study of Van Ryssel (1967) for Ghent, where 25 percent of homes stem from private investors and another 12.5 percent from city records. Homes that showed abnormal changes in the level of rents were excluded. In each study homes were only included in the database if rental observations were available for at least 7 years. If observations were available for less than 7 years, but the rent was revised within this period, the home was included as well.

Most rents in these studies were paid annually: monthly, quarterly or half-yearly payments were exceptional and seemed to occur only during very turbulent periods, such as the start of the Spanish occupation. Although the starting dates of the contracts are unknown, annual rents were mostly paid on various religious holidays, such as Christmas, Candlemas or Maria Ascension, which were spread evenly throughout the year. In the index estimation, it is therefore assumed that contracts start mid-year.

Works for the period after the Ancien Regime, from Avondts and Scholliers (1977), Van den Eeckhout and Scholliers (1979), Henau (1991, unpublished) and Segers (1999), vary slightly in methodology but rely on the same set of sources: social institutions. De 'Burelen van Weldadigheid' (offices of kindness) and 'Burgerlijke Godshuizen' (civil alms houses), were founded after the French revolution and operated like the institutions in place during the Ancien Regime. These institutions were merged in 1925 into a single organization that still

Table 7: Overview of Rental Data Sources

Source	*City*	*Type*	*I*	*Years*	*Obs.*
Primary sources, rents:					
Henau (1991)	Belgian cities	Rent prices	Y	1910-1940	11,711
Segers (1999)	Belgian cities	Rent prices	Y	1800-1920	33,088
Verlinden (1972)	Antwerp	Rent prices	Y	1500-1876	27,643
Verlinden (1972)	Bruges	Rent prices	Y	1500-1800	22,157
Avondts (1971)	Brussels	Rent prices	Y	1500-1800	19,150
Van den Eeckhout and Scholliers (1979)	Brussels	Rent prices	Y	1800-1940	14,977
Van Ryssel (1967)	Ghent	Rent prices	Y	1500-1796	41,492
Avondts and Scholliers (1977)	Ghent	Rent prices	Y	1796-1932	13,585
Lesger (1986)	Amsterdam	Rent prices	Y	1500-1869	48,860
ACA 367.A, no. 141-150	Amsterdam	Contracts	Y	1671-1805	7,537
ACA 367.C , no. 100, 1794, 1804-1805	Amsterdam	Contracts	Y	1833-1936	11,701
ACA 367.C, no 938, 947, 1498, 1798	Amsterdam	Rent prices	Y	1934-1940	348
ACA 201, no. 1973, 3596	Amsterdam	Contracts	Y	1849-1928	65
ACA 404, no. 156	Amsterdam	Contracts	Y	1843-1942	100
ACA 1120, no. 2087-2089, 2130	Amsterdam	Rent prices	Y	1845-1942	1,397
ACA 191, no. 979, 987, 991-992	Amsterdam	Contracts	Y	1840-1941	295
ACA 612, no. 432	Amsterdam	Contracts	Y	1853-1884	20
Clark (2002)	London / UK	Contracts	Y	1225-1914	19,246
LMA, CLC/B/216/MS144	London	Contracts	N	1909-1959	15,274
Archives Nationales, 66 AJ 2029-2035	Paris	Contracts	Y	1400-1792	9,221
Archives de l'APHP, 782 FOSS 1	Paris	Contracts	Y	1733-1820	1,047
Monin and Lazard (1920)	Paris	Contracts	Y	1766-1819	2,012
Archives de Paris, DQ18	Paris	Contracts	N	1803-1870	861
Primary sources, rental values:					
ACA 5044, no. 254, 273, 281, 284	Amsterdam	Rental value	N	1647-1650	14,549
ACA 5044, no. 402-405	Amsterdam	Rental value	N	1733	25,328
ACA 5045, no. 269-323	Amsterdam	Rent prices	N	1805	33,210
ACA 5045, no. 269-323	Amsterdam	Rental value	N	1805	17,777
ACA 5210, no. 69	Amsterdam	Rental value	N	1815	1,619
Fryske Akademy (2018)	Amsterdam	Rental value	N	1832	30,047
Felixarchief Antwerp, inv. 782 no 1-14	Antwerp	Rental value	N	1584	11,852
Secondary sources, rent indices:					
Henau (unpublished)	Belgian cities	Urban	N	1941-1961	
Banque Nationale de Belgique (1980)	Belgian cities	National	N	1975-1977	
Statistics Belgium (2021)	Belgian cities	National	N	1977-2020	
Gemeente Amsterdam (2018)	Amsterdam	City	N	1940-1994	
AFWC (2009)	Amsterdam	City	N	1994-1998	
Companen (2013)	Amsterdam	City	N	1998-2014	
Pararius (2021)	Amsterdam	City	N	2000-2020	
Samy (2015)	London	City	N	1903-1909	
ONS / National Archives RG 77/3	London	National	N	1959-1987	
Office for National Statistics (2021)	London	National	N	1987-2005	
Office for National Statistics (2021)	London	City	N	2005-2020	
Marnata (1961)	Paris	City	N	1867-1957	
Friggit, by courtesy	Paris	City	N	1957-2020	

Notes: This table shows all rent data sources. Details on each of the sources is provide in the remainder of Appendix A. The table uses the following abbreviations: ACA = Amsterdam City Archives, LMA = London Metropolitan Archives. Column *I* indicates whether the primary data were based on institutional sources. *Type* indicates whether data are actual paid rental prices, rent contracts or estimated rental values. For secondary sources, it names regional coverage.

exists nowadays in each Belgian municipality in the form of a Public Centre for Social Welfare (OCMW). Their archives formed the source for each of these studies. The work of Henau (1991) covers the period after the start of the World War I until 1940, whereas the others span from 1796 to the first half of the 20th century.

It is important to realize that the rental market was severely impacted by rent regulations introduced during World War I. In August 1914, a law was passed that gave the Belgian state the power to adapt contracts during wartime, including rental contracts. In 1919 and 1921 legislation was passed such that large groups of renters did not have to pay rent arrears built up during World War I. In some cases, actual market rents demanded might have therefore been higher than reported in our data, as we only observe the actual rent paid.

Rents were frequently re-capped relative to the rent level on January 1, 1914, with rent ceilings slowly increasing. There was significant variation in the imposition and revision of rent ceilings across municipalities, with the general trend being a relaxation of the regulations throughout the twenties and thirties. Following World War II, rent restrictions were re-imposed until the early fifties to deal with the housing shortages caused by the war.

We unfortunately do not possess underlying data for the unpublished study of Henau, which we have used from 1940 to 1961. Methodologically, this study is similar to Henau (1991), and covers the largest cities in Belgium. Between 1961 and 1975, no rental indices are available at the city level or national level. In order to splice our indices, we have used developments in house prices to proxy for rental prices from Knoll et al. (2017). From 1975, we rely on the rent component of the CPI. The first three years, we use a statistic published in Banque Nationale de Belgique (1980), while from 1977 we rely on the nation-wide CPI published by Statistics Belgium (2021). The rent component of the Belgian CPI is based on the average rent reported in a monthly survey of 1800 properties in the private sector. Properties remain in the sample for extended periods of time. Changes occur either when tenants do not want to participate in the survey anymore or when old homes are being replaced by newer dwellings to keep the sample representative.

A.2 Amsterdam

The work of Lesger (1986), our source for Amsterdam from 1550 to 1854, follows in the tradition of the Belgian rent studies, albeit with one significant difference: the selection of homes based on quality. Whereas the homes in the samples of the Belgian cities were well spread throughout the cities, there might have been a bias towards homes of a particular quality bracket in particular years. Lesger therefore categorized on the quality of the observed home, ensuring that in every year homes from each of the four defined quality categories were in the sample. Each category was defined based on a set of reference homes, for which quality characteristics were available such that a categorization could be made. Homes were subsequently classified based on their rental price relative to the rental prices of the reference homes.

Homes were only included in the sample if more than five years of rental data was available. If data was missing for less than two years, most likely because the home was not rented, the missing data would be filled with the rent that was paid after the gap. This strategy is somewhat unfortunate for our repeat-sales index, since rent revisions might occur one or two years earlier than they have occurred in reality. It was not possible to trace these observations, but fortunately these gaps were relatively rare.

We complement the data of Lesger with our own archival data collection, using data from various institutional archives kept in the Amsterdam City Archives. Our main additional source is the archive of the Burgerweeshuis, the Amsterdam orphanage, which has been discussed extensively in the work of McCants (1997). In addition we have collected data from the archive of the Roman-Catholic boys' orphanage, the Brants-Rus Almshouse and from various churches: the Walloon Reformed Church, the Remonstrants, and the Mennonites. For the majority of data, we have attempted to collect data on rental contracts, but for some cases it was only possible to rely on rent payments.

From 1940 onwards, we do not have sufficient primary sources to allow for the computation of a market rent index. However, this is not problematic since it coincides with a period of strict rent freezes. The first rent controls had been introduced in the Netherlands during World War I, following housing shortages and a broader set of government policies to control prices for basic needs during periods of large uncertainty. Initially, rents were fixed by the 'Huurcommisiewet' of 1917, but later rents could increase with the rate of inflation. In the early 1920s government's grip on rents had reduced already, but only in 1927 was this confirmed by law. The rent freeze after the start of World War II remained until 1950, when gradually a more sophisticated rent policy was introduced. The idea of the rent policy was to slowly bring rents of homes back to market level, while keeping rents affordable. While in many municipalities rents were already liberated in the late 1960s, Amsterdam, and most other big cities, remained under rent control until the late 1970s.

For rent prices in this period until 1994, we rely on a rent price index of the Amsterdam Statistical Office, which we retrieved from its annual yearbook. The methodology used for this statistic followed standards of the Dutch Central Bureau of Statistics. From 1994 to 1998, we rely on the average rent price increases of housing associations in Amsterdam, which own the large majority of rental property in the city (AFWC, 2009). For these years, there is no information available on rent price growth in the commercial sector, which our index primarily aims to track. From 1998 to 2014, we use an index of commercial rent price growth from Companen, a Dutch agency (Companen, 2013). After 2014, we use rents per square meter in Amsterdam from (Pararius, 2021), a listings platform that mostly operates in the higher-end of the commercial rental sector.

A.3 London

The main historical study we use for London is Clark (2002). He assembled a large dataset of rents, consisting of 19,246 observations spanning from 1225 until 1907.[10] As in the other cases, most rental observations stem from investigations into the activities of charities. Clark's sample consists of data from both Wales and England, but about a quarter of observations originate from London. Not all transactions in the sample of Clark correspond to actual rents. First of all, in about 10 percent of cases tenants had to pay fines or pay for repairs of the building. Since these are generally considered to be part of rental expenses, Clark (2002) annualized these fines and used these to adjust the rental values of the observations. Second, in another 10 percent of cases Clark estimated the rental values of homes from house prices, since no rental payments were mentioned.

Our index is only based on repeated observations for London, both within and outside the City, with rent contracts of 21 years or less. There are 1,624 observations left for the estimation of the index. Before 1770, there are very few observations and a significant number of years have no observations at all. As a result, the signal-to-noise ratio is very low, and hence the model smooths the index significantly.

From 1903 to 1909 we rely on the recent study of Samy (2015), who developed a house and rent price index for London for the period from 1895 until 1939, based on data from the London Auction Mart (1895-1922) and the mortgage registers of the Co-operative Permanent Building Societies (1920-1939). Absent repeat sales, Samy (2015) used the hedonic method to estimate the indices. Unfortunately, no structural characteristics are available for the London Auction Mart data, and only very basic ones (number of rooms, frontage size and property size) for the CPBS data. Hence, his index likely overstates rental price growth. However, since we only use six years of his data (with almost constant prices), this effect

[10]Note that the number of observations does not match the number of observations reported in the paper, since Clark added observations to the dataset after publication.

does not alter the London index significantly.

From 1909 until 1959, we have collected data on more than 30,000 rent observations from the archives of Trafalgar House Developments Ltd. We have collected data from seal books of two of its subsidiaries: Consolidated London Properties and City & West End Properties. These companies managed several apartment buildings, shops and offices all over London, and their seal books contain data on newly registered leases and renewals on existing ones, listing date, new rental price and old rental price. To identify repeat-sales, we first cleaned data on the unit identifiers per building. The unit numbers for each lease were not written down in a consistent way in the seal books, such that it was not always clear which unit exactly was let. After cleaning the unit numbers, we only matched rents as repeats in case the old rent matched the new rent on the previous observation on that unit. In total fifty percent of data could be matched. For the index, we only used residential rent observations. Devaney (2010) has used the same sources to estimate an office rent index for the City of London.

In London, rent controls were in place for the majority of time between 1915 and 1988u, but the strictness of these rent controls varied substantially over time (Wilson, 2017).

From 1959 until 1987 we use the nation-wide rent component of the CPI, as produced by the Office of National Statistics (ONS). The methodology behind this index has changed multiple times; from the early 1960s onwards the rent component also included the implied cost for owner-occupied housing. After 1987 we rely again on the rent component of the CPI, which is based on a representative sample of homes whose rents are tracked over time. As homes in both the private sector and the social sector are in the sample, the index is not a pure measure of changes in constant-quality market rents. After 2005 we use ONS's experimental index on private housing rents in London, which relies on the same sources as the rent component of the CPI, but only includes homes rented in the private sector.

A.4 Paris

The landmark study on the history of the Paris rental market is Le Roy Ladurie and Couperie (1970), covering the 1400 to 1789 period, and containing about 11,000 leases. Rental data does mostly come from actual lease contracts, stored in the archival records of 26 different social institutions; either religious institutions or hospitals. Only in a minority of cases data originate from accounting books for which the contract date is unknown. Since contracts were most commonly signed for nine years, rent payments from accounting books are not always representative of market rents. We therefore excluded these in the estimation of the index.

Unfortunately, the authors of the study did not preserve the punch card lists which contained the rents for every home. However, the authors did save transcriptions of the contracts and records, which are stored in the French National Archives. We collected and typed for each of these contracts the identifier and approximate location of the home, the contract date, the date of the accounting year and the rental price. All prices were converted to livre tournois.

Following the French Revolution and the dramatic state of the French public finances, all possessions of the institutions were nationalized in 1792, and only privatized again in 1811. Archival data is scarce for this period, and in order to continue our series we have combined several archival and non-archival sources. First, the French government registered the rent on each property and the contract date when all homes were nationalized, and these lists are published in the Sommier des Biens Nationaux de Paris Monin and Lazard (1920). Second, when the properties were returned in 1811, references were made to the underlying notary contracts, which in many cases can still be found in the Archives of the Assistance-Publique des Hôpitaux de Paris, the Paris hospital system. It is the latter archive from which we have collected additional archival data in order to combine data from before and after the Revolution.

From 1809 until 1870, we add data from the first register of the Parisian 'sommier foncier'. The sommier foncier is one of the registers that was part of the French *Enregistrement*, and contains data on contracts relating to all Parisian homes, such as inheritances, sales contracts, rental values or auctions. For the taxation of wealth, it was important to keep track of the owners of homes, as well as the value and revenue they generated with their real estate. In the first register, which spans the 1809 to 1860s period, rent contract data was included as well. We have collected a sample of this rent data for various streets across central Paris, and since observations are organized per house it allows for the identification of repeat rents. Note these rents are primarily for entire properties, and might thus not account for the presence of subrenting

From 1867 until 1957 we rely on a rent index from Marnata (1961). Marnata collected 11,800 different rents from lease management books from residential neighborhoods in Paris and subsequently used these observations to compute a chained index. Although his index is not a pure repeat sales index, it controls for quality as it follows the same residential units over long periods of time. The main disadvantage of his study is that most of the residential units in the sample are of relatively high quality, meant for the upper classes of society. Since rental developments might have differed in lower class rental units, the index cannot be considered completely representative for the city of Paris.

Between 1914 and 1948, rents were strictly regulated relative to the 1914 level of housing rents (Duon, 1946). After 1948, the rent regulation system changed substantially so that housing rents could gradually catch up to market levels (Bonneval, 2016).

From 1960 onwards, we make use of various rent indices compiled from data kindly provided by Jacques Friggit. Between 1960 and 1988, this index is based on the rent component of the CPI for the Paris region. From 1989 to 2020, it is based on the median rent per square meter in Paris from the Observatoire des Loyers de l'Agglomération Parisienne. The latter method likely overstates growth in quality controlled rents, since it only controls for quality improvements due to increased space, but does not take into the account that the quality of a given space has improved as well (e.g. due to better insulation).

B Representativeness of the Institutional Sample

The quality indices developed in this paper rely strongly on the assumption that the mean rent derived from our sample of institutional rents is representative for the general housing stock in the city, and that the institutional rents were true market rents rather than rents that were kept affordable because of the mission of the institutional owners.

To start with the latter concern, it is very likely that the institutional real estate owners on whose archives we depend indeed charged market rents, for a number of reasons. First, many institutions relied heavily, and some even exclusively, on housing rental streams to finance their core activities and could not afford to ask below-market rents (Le Roy Ladurie and Couperie, 1970). Correspondingly, the returns they made on these properties were of significant concern (Gelderblom and Jonker, 2009). For rural properties around Paris, owned by the Cathedral of the Notre-Dame de Paris, Hoffman et al. (2001) provides anecdotal evidence that these charitable organizations aimed to make profits from their property portfolios. Second, these institutions did not use their real estate portfolios to provide below-market rate housing to the poor or other vulnerable groups. In each city, there was considerable variety in the homes being leased, varying from sober tenements to urban mansions. In a few cases, we found evidence that homes were rented at low or no cost, for example to widows. Such cases were typically clearly indicated and organised separately, and we excluded them from our sample.

To assess the representativeness of the institutional housing portfolio for the housing stock in each city, we compared the mean level of rent in our sample to the mean level of rent obtained from historical fiscal sources or private rents. Plots of these estimates are provided in Figures 5 to 11. For the period before World War I, we could obtain such an estimate in 49 cases, spread over various cities and centuries. On average, institutional rents are about 2 percent higher than those obtained from other sources, indicating they are not systematically different from each other. However, in some periods, most notably Amsterdam and Bruges in the 19th and early 20th century, mean rent levels do not seem representative for the entire city. These differences are typically due to small-sample issues since they coincide with periods with lower numbers of rent observations and institutional owners in the sample. [11]

In the remainder of this appendix, we assess our claims concerning data representativeness in more detail by comparing the rent estimates from our sample with other estimates of rent prices in the cities we study. We also pair these sources to population data to make estimates of housing quality per capita.

Le Roy Ladurie and Couperie (1970) made an impressive effort to construct a sample representative for Paris. They collected an additional 12,000 leases from private contracts for 23 benchmark years to underline the representativeness of the charity rents: no differences in average rental prices were found in the private and charity samples. Additionally, they separated isolated and repeated observations and ensured renovated homes were treated as new observations. Last, properties in the sample were well spread across Paris: while each institution typically only owned real estate close the location of the institution, the large number of institutions covered ensures a sufficient geographic spread.

For Amsterdam and the Belgian cities, our main sources for these secondary estimates derive from property tax records. Prior to the 20th century, property taxation was the most common form of taxation and many cities. The Low Countries particularly had a developed system of property taxation already from the late medieval period onwards. Taxes were typically levied on the estimated capital or rental value of homes, sketching a fairly representative picture of the value of the housing stock in a city. Correspondingly, historians have already used these registers to make assessments of income inequality (e.g. Soltow and Van Zanden, 1998; Ryckbosch, 2016). From the early 19th century onwards, these systems were replaced by taxes on cadastral income.

[11] We could not formally assess the representativeness of the London sample. For the early 19th century, Clark (2002) used estimates of rents from tax records and found those to be closely correlated with the average level of rents in his sample for England and Wales. However, our London sample is likely the least representative due to the low number of observations. This is particularly the case before 1770, when our sample contains only 2.5 observations per year, on average.

These tax records also have several drawbacks. First of all, although the rental or cadastral value is typically aimed to proxy for actual rents, it is difficult to assess how precise these estimates are, particularly since they were rarely updated. If possible, we therefore only collected data in years when such an update took place. If that was not possible, we corrected the rental value for rent price changes that took place since the last correction, often employing the market rent indices estimated in this paper. Second, in various records it was not possible to separate non-residential property (most notably basements and warehouses) from residential property. However, the resulting error is likely small. For example, in 1805 non-residential property only constituted about 11 percent of total rental value in Amsterdam.

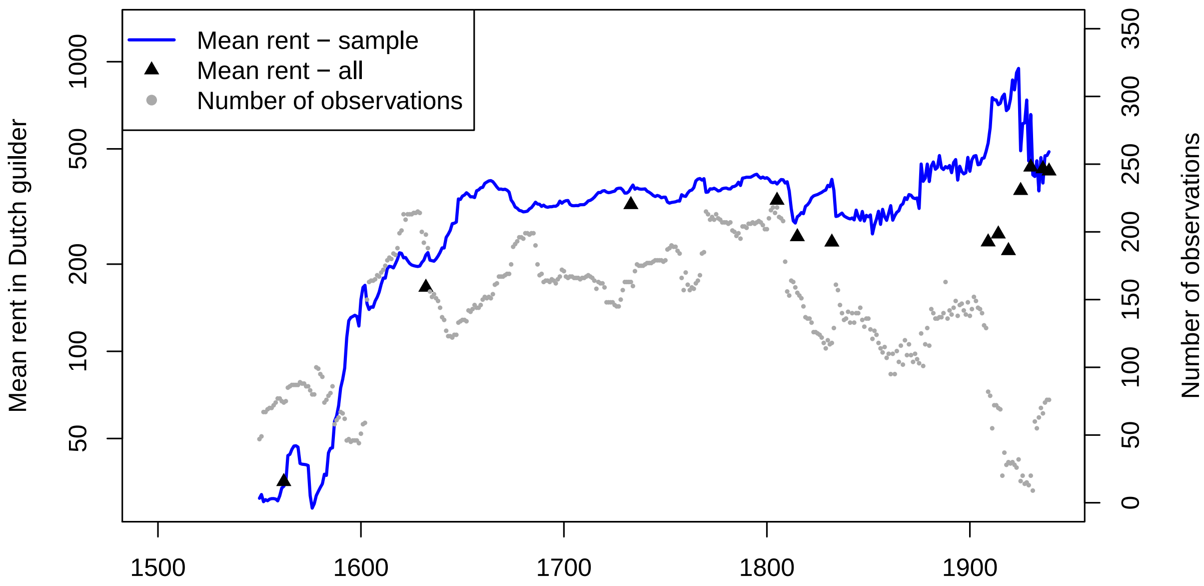
In total, we obtained data from 22 tax registers. For most of these, we were able to also collect data on the number of homes in the register, either by collecting all rents in the archival registers or through existing statistics.

Beyond these tax-based observations, data on the level of actual private rents was also available for Paris, Amsterdam and Brussels for different years. For Amsterdam, we computed the average level of rents for seven years between 1909 and 1939 based on census data. For Paris, Le Roy Ladurie and Couperie (1970) collected data on private rent contracts from the Paris notarial archives, covering 24 years between 1500 and 1788. For Brussels, we computed the average level of private rents in 1865 based on data from the Lokstat-PoppKad database. Overall, we obtained 53 points in time to compare levels of institutional rents to private rents.

In the figures below, we plot for each of our seven cities these points relative to developments in mean rents in our sample. For reference, we also plotted the number of observations. In each city, the level of mean housing rents is close to the level of housing rents obtained from our sample. Major differences mainly appear in Amsterdam in the early 20$^{\text{th}}$ century and Bruges in the 19$^{\text{th}}$ century.

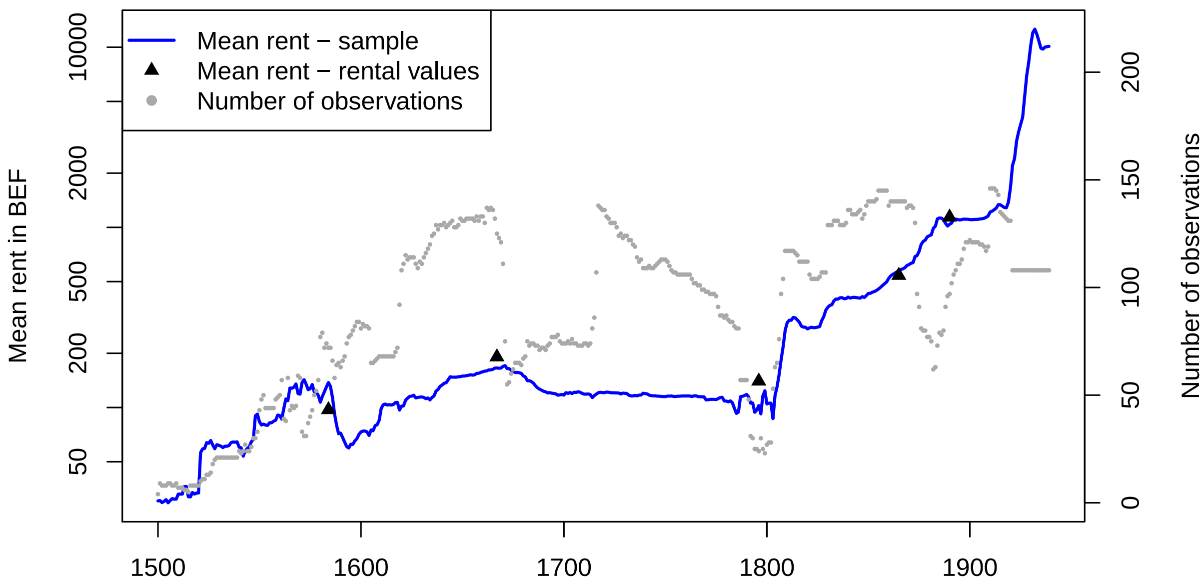
In the shorter term, substantial changes in the sample typically lead to significant volatility in the sample. This is particularly visible in Bruges around 1800, and to a lesser extent in Antwerp and Brussels. In each of these cases, the sample changes almost entirely. For London and Paris, developments in annual mean rent levels are substantially more volatile, since these samples are entirely based on rent contracts rather than rent payments. Due to the low number of observations, this issue is particularly severe for London.

Figure 5: Amsterdam Mean Rents



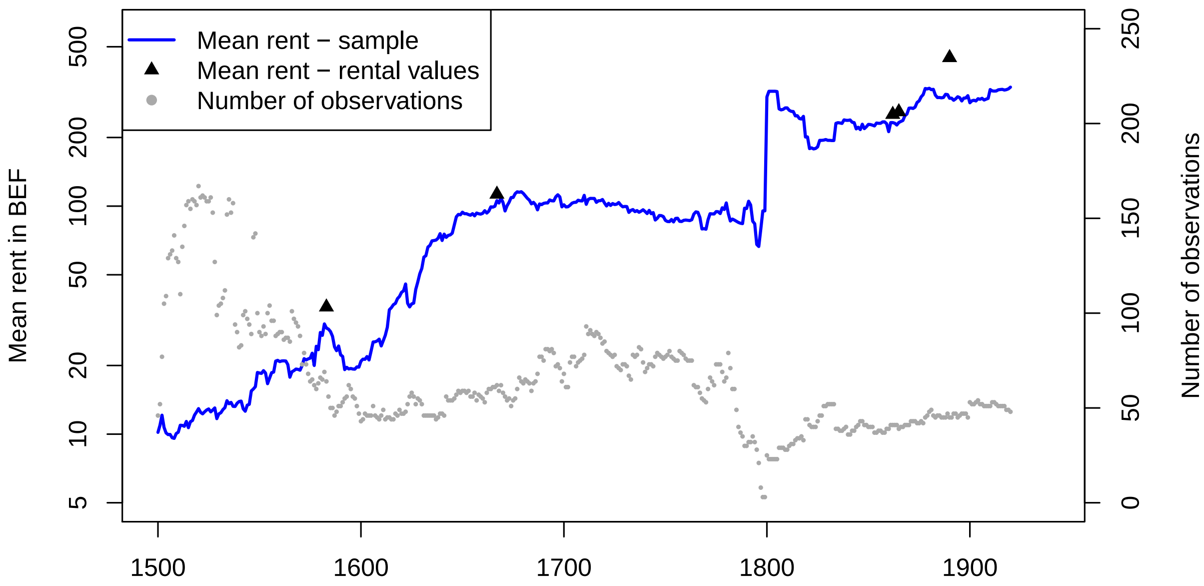
Notes: This figure plots the evolution of the average rent in our Amsterdam sample over time, relative to alternative estimates from sources reporting on rental values or rents in the entire city. The points for the 20th century reflect actual rents rather than rental values. To convert rental values to rents, we used data from the 1805 rent register listing both rental values and actual rents. The light-grey scatter reports the number of observations in the sample. *Sources alternative estimates:* Soltow and Van Zanden (1998); ACA 5044 no. 254, 273, 281 284, 402-405; ACA 5045 no. 269-323; ACA 5210 no. 69; Fryske Akademy (2018); Laloli (2018).

Figure 6: Antwerp Mean Rents



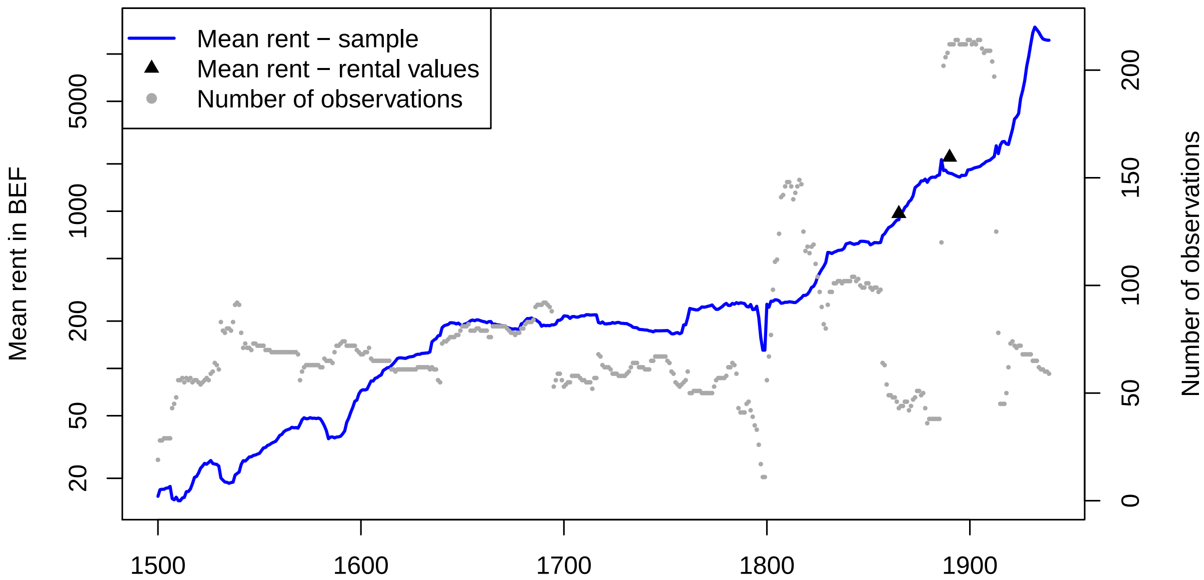
Notes: This figure plots the evolution of the average rent in our Antwerp sample over time, relative to alternative estimates from sources reporting on rental values or rents in the entire city. The light-grey scatter reports the number of rent observations in the sample. *Sources alternative estimates:* Felixarchief Antwerp 782 no. 1-14, Baetens (1976), De Belder (1977), LOKSTAT-POPPKAD.

Figure 7: Bruges Mean Rents



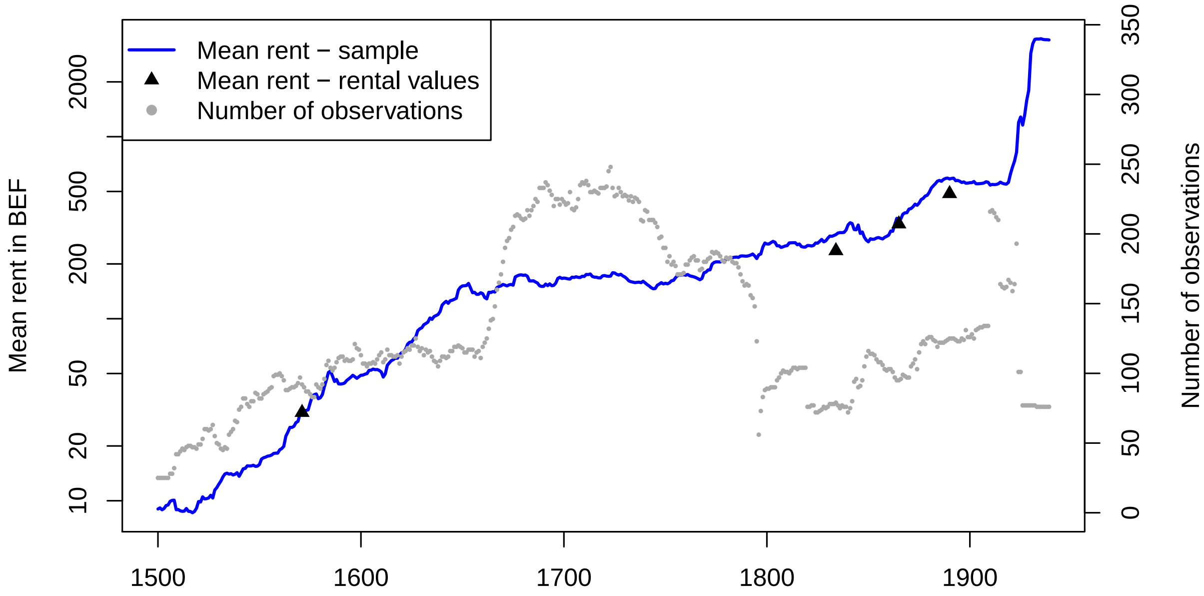
Notes: This figure plots the evolution of the average rent in our Bruges sample, relative to alternative estimates from sources reporting on rental values or rents in the entire city. The light-grey scatter reports the number of rent observations in the sample. *Sources alternative estimates:* Database Heidi Deneweth, LOKSTAT-POPPKAD, Quetelet Center.

Figure 8: Brussels Mean Rents



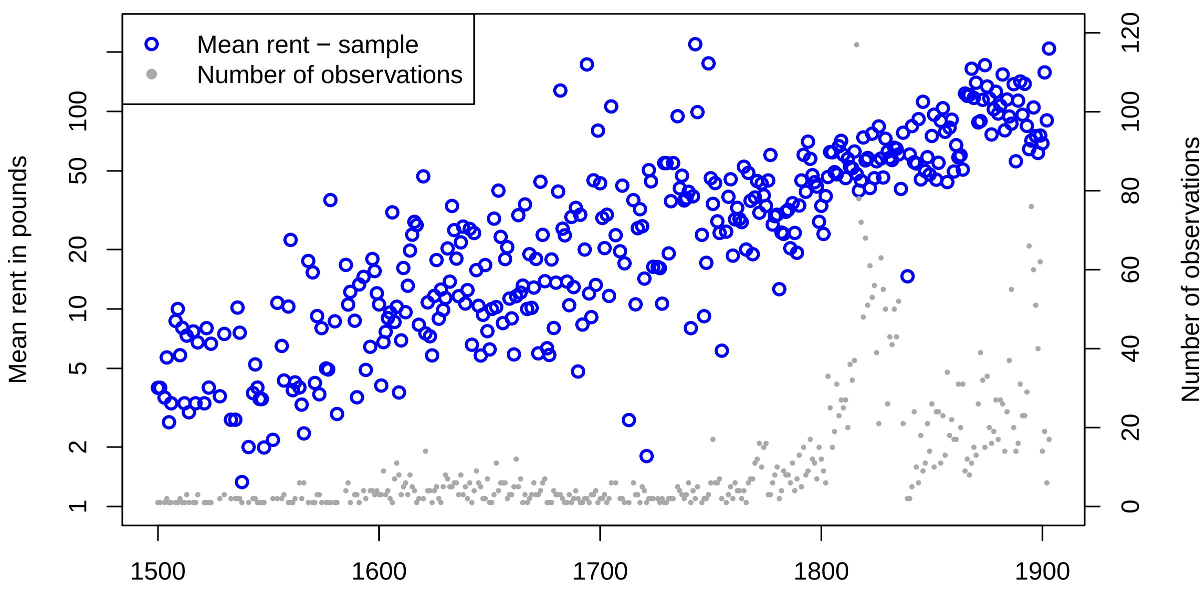
Notes: This figure plots the evolution of the average rent in our Brussels sample, relative to alternative estimates from sources reporting on rental values or rents in the entire city. The light-grey scatter reports the number of rent observations in the sample. Data from Vrielinck indicated the average ratio of cadastral income to average actual rents in 1865. We used this ratio to transform average cadastral income to actual rents for all other Belgian cities in 1865 and 1890. *Sources alternative estimates:* Database Sven Vrielinck, LOKSTAT-POPPKAD, Quetelet Center.

Figure 9: Ghent Mean Rents



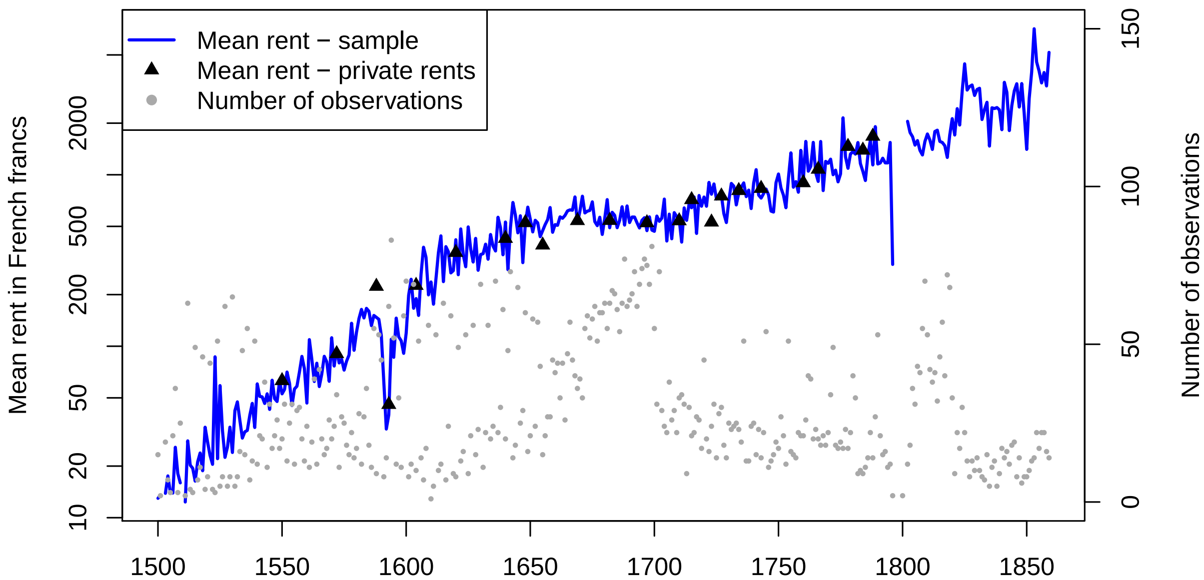
Notes: This figure plots the evolution of the average rent in our Ghent sample, relative to alternative estimates from sources reporting on rental values or rents in the entire city. The light-grey scatter reports the number of rent observations in the sample. The rental value for 1834 is an estimate based on rents from a decade earlier and, most likely, underestimated rents. We therefore correct the value by 25 percent. *Sources alternative estimates:* Dambruyne (2001), Vanhaute and Hannes (2007), LOKSTAT-POPPKAD, Quetelet Center.

Figure 10: London Mean Rents



Notes: This figure plots the evolution of the average rent in our London sample. The light-grey scatter reports the number of rent observations in the sample. Note that the number of observations in our London sample is very low, implying average rents are very noisy at annual level. For this reason, the plot provides a scatterplot of mean rents.

Figure 11: Paris Mean Rents



Notes: This figure plots the evolution of the average rent in our Bruges sample, relative to alternative estimates from sources reporting on rental values or rents in the entire city. The light-grey scatter reports the number of rent observations in the sample. *Source alternative estimates:* Le Roy Ladurie and Couperie (1970).

C Consumer Prices

Our sources for consumer price data are reported in table 8. For most periods, we rely on existing consumer price indices. For Belgium, from 1500 to 1830, we rely on indices constructed from primary consumer goods price data; the construction method is discussed in the next subsection. Primary price data on individual consumption goods are either based on actual purchase prices recorded by social institutions, or on fixed prices set for tax or exchange purposes. Governments levied small taxes on goods, which were either based on actual market prices paid for the goods or on so-called 'spijker prices', fixed prices set by counties based on prevailing market conditions. Institutions without tax-levying authority used similar practices to set prices for monetary contracts that were settled in kind, providing an additional source of price information. These fixed prices were not always accurate representations of average annual market prices. Prices of goods could fluctuate considerably within a calendar year, as harvests could significantly be affected by bad weather or political instability.

Table 8: Overview Consumer Price Sources

City/Country	*Study*	*Years*	*Coverage*	*Type*
Belgium	Michotte (1937)	1830-1913	National	Index
	Scholliers (1978)	1914-1920	Urban	Index
	Statistics Belgium (2021)	1913-2020	National	Index
Bruges	Verlinden (1972)	1500-1800	Urban	Raw prices
Ghent	Verlinden (1972)	1500-1800	Urban	Raw prices
Antwerpen	Van der Wee (1963)	1500-1600	Urban	Raw prices
	Verlinden (1972)	1500-1830	Urban	Raw prices
Brussels	Verlinden (1972)	1500-1800	Urban	Raw prices
Amsterdam	Van Zanden (2018)	1500-1800	Regional	Index
	van Riel (2021)	1800-1900	National	Index
	Statistics Netherlands (2021)	1900-2020	National	Index
Paris	Ridolfi (2019)	1500-1840	City	Index
	Singer-Kérel (1961)	1840-1958	City	Index
	CGEDD (2018)	1958-1990	National	Index
	INSEE (2021)	1990-2020	National	Index
London	Allen (2001)	1500-1913	City	Index
	Thomas and Dimsdale (2017)	1913-1988	National	Index
	Office for National Statistics (2021)	1988-2020	National	Index

Notes: The table reports for each city the different sources of consumer price data that we used. Most data covers a strictly urban sample, although data is in some cases at national level. Except for Belgium, we only use secondary time series.

For Antwerp, consumer price data is complemented with data from Van der Wee (1963). Prices are based on the consumer price index constructed by Michotte (1937) from 1830 until the World War I. For the period of World War I, we use an index for Brussels from Scholliers (1978). After World War I, a continuous consumer price index (1921-2018) is available from Statistics Belgium (2021), which uses 1914 as base year and is therefore spliced to the index of Michotte (1937).

Amsterdam consumption prices are from Van Zanden (2018), who computes a price index based on a representative basket of goods for Western Holland between 1500 and 1800. From 1800 to 1910, we use the price index constructed by van Riel (2021), which we deflate for rental expenses. Consumer prices after 1900 are based on the Dutch national consumer price index from Statistics Netherlands (2021).

For consumer prices in Paris we employ the index developed by Ridolfi (2019) for the period from 1500 to 1840. Annual figures for this index were kindly provided by Leonardo Rudolfi. This index is built on a wide array of primary and secondary sources, improving existing estimates of Allen (2001). For the period from 1840 to 1950, we use the price index for workers from Singer-Kérel (1961). After 1950, we rely on consumer price indices reported in CGEDD (2018) and INSEE (2021). Indices for consumer prices in London covering the 1500-1913 period are from Allen (2001). For the 20th century, we use data from the the Bank of England dataset "a millennium of macroeconomic data" (Thomas and Dimsdale, 2017), from which we used their preferred headline CPI measure. To extend to 2021, we use the standard CPI index of Office for National Statistics (2021).

C.1 Index Construction

We estimate a new Belgian consumer price index from 1500 to 1830, based on 128 different price series collected from the Verlinden volumes and Van der Wee (1963).[12] Even though Flanders and Brabant were separate states until 1795, with each having their own currency, we do not estimate a separate index for these regions. We have found no evidence that aggregate consumer prices within Flanders or Brabant were more strongly tied together. This was confirmed when looking at the individual price series.

We did attempt to construct price indices for each city, as in the short run prices for par-

[12] Allen (2001) has already estimated an annual consumer price index for Antwerp / Brabant from 1366-1913, but his index does not rely on a representative adjustable basket of goods and is likely to understate the true annual volatility in prices due to the strong reliance on interpolated data. As the majority of prices is missing, interpolation results in unrealistically smooth indices, in particular during the 18th century. This will make it much more difficult to identify to what extent nominal rents move with the general price level.

ticular goods could vary across cities, but this turned out to be infeasible. First, the number of series available per city is limited, in particular for Ghent and Brussels, causing their price indices to be unrealistically volatile relative to other cities. Second, the available sources are of varying quality, ranging from monthly averages of market prices to a single price fixed on the day before Christmas. Data quality considerations seem more important than differences across cities: high-quality series on the same good across cities tend to be more correlated than high- and low-quality series on the same good within a city.

Due to the lack of continuous price series, we have developed a pragmatic method to estimate the consumer price indices, making use of the available data as much as possible. Note that due to the data-driven index estimation strategy, the index developed in this section cannot be classified in standard price index categories; such as the well-known Laspeyres, Paasche or Fischer price indices. The method to construct our indices consists of three steps.

In the first step, the 128 collected price series were stacked into 14 different groups: wheat, rye, barley, peas, butter, egg, cheese, potatoes, buckwheat, beef, chicken, fish, energy, and oils. The first nine groups each contain only a single good, whereas the last five groups contain multiple goods representative of the group under consideration. To avoid sensitivity to size discounts or quality differences across cities, as each city had its own measures, we index the individual price series. Base years are chosen to be all years in which individual price series for a group overlap, which avoids strong base-year sensitivity. In case a series has no overlap, it is indexed relative to one or more high-quality series for the same good. Aggregate indices are constructed for each product group by taking averages of the most-representative series. Representativeness is assessed based on the nature of the prices (fixed versus market prices) and the frequency and timing of the observations within a year, with preference given to high-frequency market prices matching the calendar year.

In the second step the base weights of each good in the overall price index were determined. Weights are based on scarce information on expenditure patterns of Ghent households and Antwerp orphanages for a handful of years in the late 16$^{\text{th}}$ and 19$^{\text{th}}$ century, published in Scholliers (1960) and Avondts and Scholliers (1977). Weights are fixed before 1600, and from 1600 to 1830 interpolated. Potatoes and buckwheat are only included after 1800 due to data availability. It is important to realize that expenditure patterns vary significantly over time and across sources. The price of grain, which was the most important component of the household budget until the early 19$^{\text{th}}$ century, increased significantly in 1586 due to the uncertainty caused by the Fall of Antwerp to the Spanish in late 1585. Since cereals were, even at very high prices, the cheapest source of calories, inhabitants did not shift their consumption to other goods, but were forced to spend their money on cereals to avoid starvation.

The main problem with the selected base weights is that for some product groups no continuous price observations are available, in particular after 1800. In order to make use of the available data as much as possible, without engaging in excessive smoothing, we vary the weights across years depending on data availability.[13] In case prices for a product group are not available or of insufficient quality, its weight is redistributed to a group (or groups) that is (are) most correlated with the price index of the missing group. In the last step, the prices for each good are converted to index prices and multiplied with the weights to produce the consumer price index.

D Wages

D.1 Data Sources

An overview of all sources of wages data is presented in table 9.

Observations on daily wages of masons, carpenters, slaters and their helpers are obtained for Bruges (1500-1628), Ghent (1500-1799) and Antwerpen (1500-1840) from the Verlinden (1972) series. These are converted to a total index based on the methodology discussed in section D2. The study of Peeters (1939) provides us with an aggregate index of hourly wages in various Belgian industries from 1831-1913. For later periods we rely on a multitude of publications on industrial wages. Scholliers (1978) provides estimates for Brussels wages during World War I. Cassiers and Solar (1990) produce an index of gross hourly wages for the 1913-1959 period. From 1960 onward, we use the average hourly wage increases for all employees (the Belgian government makes a division between 'laborers' and 'service workers') from the official estimates of FOD-WASO (2018), the Belgian ministry of labor.

For Amsterdam, we use day wages in the construction sector from 1500 to 1815, which we have from De Vries and Van der Woude (1997). Wages from 1815 to 1913 are based on nominal day wages reported in the study of Horlings and Smits (1996). Wage data for the period from 1913-1939 from Schrage et al. (1989), and refer to average day wages across sectors. From 1939 onward, we rely on the average wage increases from collective labour

[13]The weighting schemes for each city are available upon request.

Table 9: Overview Wage Sources

City/country	*Study*	*Years*	*Coverage*	*Type*
Belgium	Peeters (1939)	1831-1913	National	Index
	Scholliers (1978)	1914-1919	City	Index
	Cassiers and Solar (1990)	1913-1959	National	Index
	FOD-WASO (2018)	1959-2020	National	Index
Bruges	Verlinden (1972)	1500-1628	City	Raw wages
Ghent	Verlinden (1972)	1500-1800	City	Raw wages
Antwerp	Van der Wee (1963)	1500-1605	City	Raw wages
	Verlinden (1972)	1606-1834	City	Raw wages
Amsterdam	De Vries and Van der Woude (1997)	1500-1815	Regional	Index
	Horlings and Smits (1996)	1816-1913	National	Index
	Schrage et al. (1989)	1913-1939	National	Index
	Statistics Netherlands (2021)	1939-2020	National	Index
Paris	Ridolfi (2019)	1500-1870	City	Index
	Singer-Kérel (1961)	1870-1946	City	Index
	Bayet (1997)	1913-1951	National	Index
	INSEE (2021)	1951-2020	National	Index
London	Allen (2001)	1500-1913	City	Index
	Thomas and Dimsdale (2017)	1914-2016	National	Index
	Office for National Statistics (2021)	2016-2020	National	Index

agreements, which cover most of the Dutch labor force. Given that this figure has not yet been updated to 2018, we use the Statistics Netherlands (2021) index on hourly cost of labour to extend to the present.

The wage index for Paris for the period 1500-1860 is based upon average day wages of laborers and craftsmen, from the indices reported in Ridolfi (2019). Between 1860 and 1920, we use the weekly wage index for Parisian workers from Singer-Kérel (1961). To correct for changes in the length of the working week, which were particularly prevalent in the early 20$^{\text{th}}$ century, we used national figures on nominal hourly wages reported in Bayet (1997) from 1914 to 1951. To fill the gaps in the war years, we still made use of the index of Singer-Kérel (1961). From 1950, we use INSEE (2021) indices on hourly pre-tax wage rates. Since these are not available for 2016-2018, we employ an INSEE index on hourly cost of labor in the construction for the period 2015-2020.

For London between 1500 and 1913, we use the standard day wage index from Allen (2001). From 1913 until 2020, we use a national index of weekly earnings derived from Thomas and Dimsdale (2017) and Office for National Statistics (2021). Since this does not control for changes in the number of hours worked per week, which likely declined, London wages probably slightly underestimate wage growth. This is confirmed by the fact that the London index increases the least of all cities during the 20th century.

D.2 Index Construction

Wage indices for the Belgian cities are created based on thousands of day wage observations from construction sector workers (1500-1830). No wage index is constructed for Brussels, given the lack of wage data. The wage index for Bruges only spans the period from 1500 to 1628; after 1628 Ghent wages are used for Bruges. An aggregate wage index for Belgium is constructed as well, based on wage data from all cities. Note that for Antwerp, our index is almost entirely the same as Allen (2001), who used the same sources to construct his index.

Wage data come from wage lists published in the Verlinden series; one for every job in every institution, containing the years in which workers were employed, the various salaries that were paid and the number of days a certain salary was paid. In most cases, wages of 'masters' are separated from the wages of 'helpers'. We have excluded observations that make note of special circumstances, such as risky jobs, the provision of beer money or the aggregation of helpers' and masters' salaries. Other large outliers have been removed as well, since these are likely the result of special provisions not identified in the records.

Annual averages of wages are computed based on the remaining observations. Contrary to the consumer price indices, we have interpolated average wages for years where data is missing. This can be justified since the level of wages is extremely stable: contracts show that sometimes workers were paid the same wages for as much as 60 years. Persistent increases in nominal wages occur in every city only in the second half of the 16th century. After interpolating, wages are indexed for each job and subsequently averaged across all jobs to construct the total wage index.

E Population Data

The population data sources for the Belgian cities after 1820 are census estimates reported in Segers (1999) and Statistics Belgium (2021). For individual cities, we used the following sources:

Antwerp: Quetelet (1846); Verbeemen (1956); Deprez (1957); Marnef (1996).

Bruges: Sentrie (2007); Deneweth (2010); Reba et al. (2016).

Ghent: Dambruyne (2001); Van Werveke (1948); Deprez (1957); Vermeulen (2002); Reba et al. (2016).

Brussels: Cosemans (1966), Avondts (1971), Lees and Hohenberg (1989), De Vries (2013), Reba et al. (2016) and Buringh (2021).

Amsterdam: Nusteling (1985), Van Leeuwen and Oeppen (1993), Gemeente Amsterdam (2018)

London: Harding (1990), Landers et al. (1993), Mayor of London (2017) and Reba et al. (2016).

Paris: Francke and Korevaar (2020) (combines multiple sources).

In rare cases when multiple conflicting values were found, we selected the most plausible values. Gaps in the population data were filled by linear interpolation.

F Robustness Repeat-Rent Indices

This appendix concerns the question whether unobserved changes in the quality of the real assets underlying our long-term rent indices may systematically affect our estimates.

First, if we assume that homes are new when they enter our sample, either due to new construction or significant renovation, we can test the assumption of constant quality based on the framework of Harding et al. (2007). To estimate net-of-maintenance depreciation, they suggest including the log difference in house age in the standard repeat-sales regression introduced in the methodology section. The non-linearity of the age effect avoids perfect collinearity with the length of the leases and corresponding dummy variables. Using this technique, Harding et al. (2007) estimate that US housing depreciates at an average rate of 2 percent per year.

Of course, the strength of this test is weakened when homes are not new when they enter the sample. Although it is difficult to verify the extent to which this is the case, there is strong evidence from Amsterdam that many of the homes were new or significantly renovated when they enter our sample. First, we found construction or renovation plans for many of these homes in the archives we consulted. Second, analysis of data from Korevaar (2020) on housing transactions in Amsterdam between 1563-1811 reveals that institutions were very inactive in purchasing property. For example, the Burgerweeshuis, the most important Amsterdam real estate owner, was only involved in 41 real estate purchases, while it was involved in 244 sales. However, some homes were certainly not new when they were leased for the first time: we could link some of these purchases to existing homes in our sample.

Taking note of this limitation, Table 10 contains the estimate of the ageing coefficient for each city, using the standard repeated-measures model. For both Paris and London, we estimated the regression separately for the institutional sample and the non-institutional sample, given that the upkeep of these properties might have been different. The aging coefficient is statistically insignificant in all but one case: London from 1909 to 1959. Of course, and absence of significance does not in itself provide evidence of the absence of an effect, but we find coefficients that are small in magnitude, sometimes positive and sometimes negative. Hence, it is very unlikely that our indices systematically under- or overestimate rental growth due to unobserved asset quality changes.

Table 10: Estimates of Log-Difference in House Age Coefficients

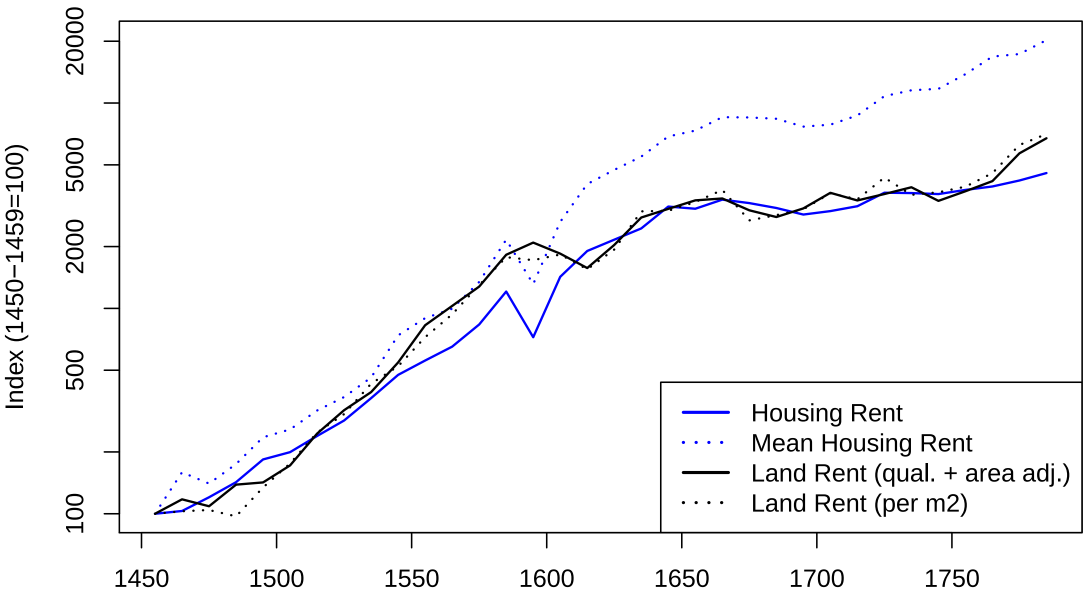
City	*Years*	*Coefficient*	*P-value*
Amsterdam	1550-1940	-0.00023	0.89
Antwerp	1500-1940	0.00421	0.49
Bruges	1500-1920	-0.00665	0.86
Brussels	1500-1940	-0.0014	0.86
Ghent	1500-1940	0.00306	0.61
London	1500-1903	0.0185	0.12
London	1909-1959	0.046	0.00
Paris	1400-1800	0.0011	0.77
Paris	1800-1870	0.0168	0.20

Notes: This table reports the results from a standard repeated-measures regression Bailey et al. (1963) that controls for the difference in the log age of the property between repeated rental contracts. The coefficient on the log-difference in house age is reported for each city. A negative and significant coefficient indicates unobserved depreciation, a positive and significant coefficient unobserved quality improvements.

A second way to assess the robustness of the assumption of constant quality in the underlying real assets is by comparing local housing rents to local land rents. Depreciation and quality improvements are aspects of the structures built on land, while the land itself does not depreciate. Hence, if quality is adjusted for properly, land rents should evolve similarly to housing rents over the longer run, at least in the period before 1800 when real wages and construction costs did not move much over time. Hoffman (2000) created such a land rent index for the Paris Basin, making use of land leases from the Cathedral of the Notre-Dame in Paris, an institution very similar to the other institutions in our Paris sample. He computed both a mean rent index per hectare, as well as a hedonic index that corrects for quality of the land (soil type, land use) and location.

Figure 12 compares his decennial indices to a decennial mean rent index based on our data, which does not control for quality, and a decennial repeat-rent index, which does control for quality. As can be seen, the various land rent indices closely track the repeat-rent index for Paris, while the mean rent index diverges from each of the indices as housing quality gradually improves. There are some periods where the repeat-rent index also diverges from the land rent index, most notably in the late 18th century, but this does not seem to result in mis-estimation, as the difference between the mean rent and repeat-rent index (the quality index) is not widening systematically in these periods. A second reassuring notion is that quality improvements seem to matter much less for farm rents. Although leased lands could still contain significant capital, for example in the form of land preparation, buildings or the plants and trees on the land, the hedonic indices suggest these did not affect farmland rents as much as housing rents.

Figure 12: Housing Rents and Land Rents, Paris area



Notes: This figure reports the evolution of our repeat-rent index for Paris and a mean rent index for Paris, relative to the land rent indices reported in Hoffman (2000). All indices are estimated for each decade.

We should note that these farmland rents are not perfectly comparable to housing rents, as urban-rural rent differences might have changed over time, even though most properties were very close to Paris. To complicate matters, land leases also contained the right to levy the tithe, which effectively reduced the rent (Hoffman (2000) adjusted for this). However, imperfections aside, both robustness checks supports our critical assumption that the repeat-rent indices that we estimate indeed adequately control for quality.

G Supplementary Tables and Figures

Table 11: Correlations in 25-year real rent growth

	Antwerp	Bruges	Brussels	Ghent	Amsterdam	Paris	London
Antwerp	1	0.865	0.775	0.863	0.231	0.631	0.504
Bruges	0.865	1	0.785	0.924	0.296	0.592	0.429
Brussels	0.775	0.785	1	0.844	0.536	0.711	0.404
Ghent	0.863	0.924	0.844	1	0.414	0.646	0.534
Amsterdam	0.231	0.296	0.536	0.414	1	0.362	0.509
Paris	0.631	0.592	0.711	0.646	0.362	1	0.404
London	0.504	0.429	0.404	0.534	0.509	0.404	1

Notes: Table 11 reports pair-wise correlations in 25-year real rent growth rates across different cities. Correlations are computed based on all years for which rent price data is available for both cities in each pair. For the Belgian cities, data stops in the early 20th century, the other cities continue until 2020. Correlations are highest among the Belgian cities, and lowest among Amsterdam and Antwerp.

Table 12: Time trends in Yields: Amsterdam (net) and other cities (gross)

	Dependent variable:	
	Other cities	Amsterdam
	(1)	(2)
Year	−0.00001	0.00002***
	(0.00003)	(0.00001)
Bruges	0.020**	
	(0.008)	
Brussel	−0.007	
	(0.008)	
Ghent	0.007	
	(0.005)	
London	0.003	
	(0.009)	
Paris	−0.005	
	(0.009)	
Constant	0.076*	0.042***
	(0.045)	(0.002)
Observations	305	402
R^2	0.057	0.029
Adjusted R^2	0.038	0.026
Residual Std. Error	0.030	0.015
F Statistic	3.015	11.744

Notes: Table 12 tests for a linear trend in yields, in line with the analysis in (Schmelzing, 2020) for bonds and land returns. Column 1 tests for the existence of a time trend based on gross individual property yields in cities outside Amsterdam, controlling for city fixed-effects with Antwerp used as baseline. Column 2 tests for a time trend in the net yields for Amsterdam, covering the period from 1550 to 2021 and using interpolated yield estimates based on a benchmarked rent and price index. In Amsterdam, there is an economically weak but statistically significant positive trend of 0.2 basispoint per year, primarily driven by high yields in the 20th century.

Table 13: Rent predictabilty, non-overlapping samples.

	Dependent variable:				
	Real: $\Delta_{25} r_t$				Nominal: $\Delta_{25} r_t$
	(1)	(2)	(3)	(4)	(5)
$\Delta_{25} pop^+_{t-1}$	0.183	−0.528		−0.728	−0.373
	(0.292)	(0.000)		(0.003)	(0.002)
	(0.363)	(0.000)		(0.002)	(0.000)
$\Delta_{25} pop^-_{t-1}$	−0.693	−0.363		−0.549	0.319
	(0.065)	(0.001)		(0.000)	(0.004)
	(0.042)	(0.081)		(0.011)	(0.055)
$\Delta_{25} pop^+_{t}$		0.576	0.286	0.782	0.700
		(0.000)	(0.028)	(0.000)	(0.000)
		(0.000)	(0.038)	(0.000)	(0.000)
$\Delta_{25} pop^-_{t}$		1.431	1.448	1.243	1.049
		(0.000)	(0.000)	(0.000)	(0.000)
		(0.000)	(0.000)	(0.000)	(0.000)
$\Delta_{25} w_t$		0.763	0.755	0.712	0.303
		(0.000)	(0.000)	(0.000)	(0.001)
		(0.000)	(0.000)	(0.000)	(0.000)
$\Delta_{25} p_t$					0.125
					(0.048)
					(0.011)
Restricted Sample	Yes	Yes	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes
Observations	2,083	2,083	2,083	2,904	2,083
R^2	0.032	0.673	0.615	0.482	0.495
Adjusted R^2	0.028	0.671	0.614	0.480	0.492
F Statistic	34.278	852.886	1,105.692	537.384	337.621

Notes: This table reports the estimates of Table 3 in the main paper but reports in parenthesis below the estimates p-values both based on regressions with overlapping and non-overlapping samples. The upper p-value is based on the Driscoll-Kraay standard errors with a lag length of 30, the bottom p-value is based on the median p-value in 25 OLS regressions with non-overlapping observations with the starting year shifting by one year in each regression. The latter are based on White standard errors.

In comparison to Table 3, Table 13 excludes the specification based on the restricted set of actual population data, because this analysis relies on too few observations (n=821) to compute non-overlapping estimates. In general, our selection of standard errors and of overlapping and non-overlapping samples does not alter the statistical significance of the coefficient on lagged population changes. For lagged population declines, we find slightly smaller coefficients in Columns 2 and 5, moving from siginfiicant at the 1 percent level to significant at the 10 percent level.

H Predictability of asset-level tracking errors

Table 14: Predictability of excess rental growth rates

	Dep. Variable: $\Delta_e R_{i,t_0,t_{future}}$							
	All cities	A'dam	Antwerp	Bruges	Brussels	Ghent	London	Paris
$\Delta_e R_{i,t_{past},t_0}$	-0.102^{***}	-0.142^{***}	-0.054	-0.204^{*}	-0.030	-0.157^{***}	-0.374^{***}	-0.168^{***}
	(0.018)	(0.033)	(0.047)	(0.105)	(0.065)	(0.060)	(0.098)	(0.031)
$MAE_{i,-3y}$	0.056^{***}	0.001	0.091^{*}	0.073	0.019	-0.122^{**}	0.245^{**}	0.063
	(0.015)	(0.024)	(0.048)	(0.101)	(0.061)	(0.058)	(0.104)	(0.045)
$\Delta_e R_{i,t_{past},t_0}$	-0.235^{**}	1.065^{**}	-0.313	0.232	-0.241	0.321	0.880^{**}	-0.235
$\times MAE_{i,-3y}$	(0.115)	(0.503)	(0.338)	(0.599)	(0.400)	(0.350)	(0.397)	(0.169)
Constant	-0.0004	-0.00001	0.007	-0.009	0.002	0.028^{***}	-0.029	-0.006
	(0.001)	(0.001)	(0.005)	(0.016)	(0.008)	(0.009)	(0.022)	(0.006)
N	34,268	13,241	5,067	1,857	2,560	4,047	542	6,954
Adj. R^2	0.022	0.006	0.010	0.026	0.004	0.011	0.043	0.046

Notes: The tables presents regression coefficient estimates for models where the observed asset-level growth rate in excess of market-wide rental growth are a linear combination of prior excess returns (for the same house) and a measure of market-wide rent revision noise (*MAE*) in years *before* revision. Robust standard errors in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

I Unpublished Data Sources

Deneweth, H. Database Heidi Deneweth based on "Huizen en mensen. Wonen, verbouwen, investeren en lenen in drie Brugse wijken van de late middeleeuwen tot de negentiende eeuw". Unpublished Ph.D. thesis, 2008. Courtesy of Heidi Deneweth

Henau, A. Rent index Belgian cities, 1940-1961. Courtesy of Katharina Knoll.

Historical Databases of Local and Cadastral Statistics (LOKSTAT-POPPKAD), Ghent University, Quetelet Center

Friggit, J. Rent index Paris, various INSEE / OLAP statistics. Courtesy of Jacques Friggit.

Vrielinck. S. Database relation cadastral income and rental value for 19th century Belgium. Courtesy of Sven Vrielinck.

J Archival Data Sources

Amsterdam City Archives, 191: Archief van het Rooms-Katholiek Jongensweeshuis, no 979, 987, 991, 992

Amsterdam City Archives, 201: Archief van de Waalsch Hervormde Gemeente, no. 1973 and 3596

Amsterdam City Archives, 367.A: Archief van het Burgerweeshuis, oud archief, no 143, 143A, 144, 145, 146

Amsterdam City Archives, 367.C: Archief van het Burgerweeshuis, nieuw-archief, no. 938, 947, 1421, 1794, 1798, 1804-1805

Amsterdam City Archives, 404: Brants-Rus Hofje en van Christoffel van Brants, no. 156

Amsterdam City Archives, 612: Archief van de Remonstrantse Gemeente, no. 432

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