# Mortgage Innovation and House Price Booms*

Claes Bäckman†and Chandler Lutz‡

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We study how mortgage product innovation can cause a housing boom even with a robust regulatory mortgage lending framework and strictly enforced recourse borrowing. We show that the introduction of IO mortgages in Denmark in 2003 ignited a housing boom and caused house prices to increase by 36 percent. In line with IO loans lowering debt-service payments and relaxing payment-to-income constraints, results show higher IO loan uptake and house price growth in areas with greater ex-ante benefits of such mortgages. Overall, our results are relevant for the many countries with elevated IO mortgage use and a robust regulatory framework.

*JEL Classification: R21, R30, G51, G21*

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†Corresponding author. Department of Economics and Business Economics, Aarhus University. Postal address: Fuglesangs Allé 4, 8210 Aarhus V, Denmark. Email: claes.backman@econ.au.dk.

‡Securities and Exchange Commission. Email: lutzc@sec.gov.

## 1 Introduction

After growing slowly between 2000 and 2003, Danish house prices boomed from 2003 to 2006 and increased by nearly 60 percent (Figure 1, orange line). The Danish mortgage market is broadly similar to the U.S. housing finance system (Campbell, 2013), but with strictly enforced recourse borrowing and a strong regulatory design that limits housing speculation for both banks and households. Mortgage banks (mortgage originators) are also fully liable for defaults on mortgages sold to investors, mitigating any concerns regarding asymmetric information in mortgage lending. Danish mortgage bonds performed well relative to comparable securities throughout the financial crisis (Gundersen *et al.*, 2011), and default rates in Denmark peaked at just 0.6 percent of outstanding mortgages. Institutional design thereby eliminates many potential causes of the 2000s U.S. housing boom (see Griffin *et al.*, 2020), and the incentives faced by banks and households limit other explanations. Even so, Figure 1 shows that Denmark experienced a larger housing boom than the U.S., UK, Ireland, Spain, and many other countries over this period.

In this paper, we document that the introduction of interest-only (IO) mortgages in October 2003 sparked the Danish housing boom. With IO mortgages, Danish borrowers could postpone amortization payments for up to 10 years, which at the time reduced mortgage expenses by 20 percent a year in the first ten years compared to a fixed rate mortgage. House price growth jumped following the legalization of IO mortgages, suggesting that their introduction and widespread uptake led to a dramatic shift in the Danish housing market.

Despite the appealing initial evidence in Figure 1, measuring the causal impact of IO mortgages on house prices is complicated by potential confounding factors, such as local labor market and income dynamics. To identify policy effects, we use ex-ante, within-Denmark, cross-sectional variation in the value of interest-only mortgages for borrowers to estimate their impact on house price growth. Specifically, we construct a measure of pre-treatment exposure using municipality house price levels (square meter price) five years before the reform. In line with this ex-ante measure signaling stronger subsequent treatment intensities, increased municipality-level exposure predicts higher IO loan use immediately following policy implementation and in later years. This result holds at the municipality level and for individual homebuyers with a battery of controls for income, liquid wealth, and demographic characteristics. We find that a one-standard-deviation increase in house price levels in 1998 predicts an 11 percent increase in IO

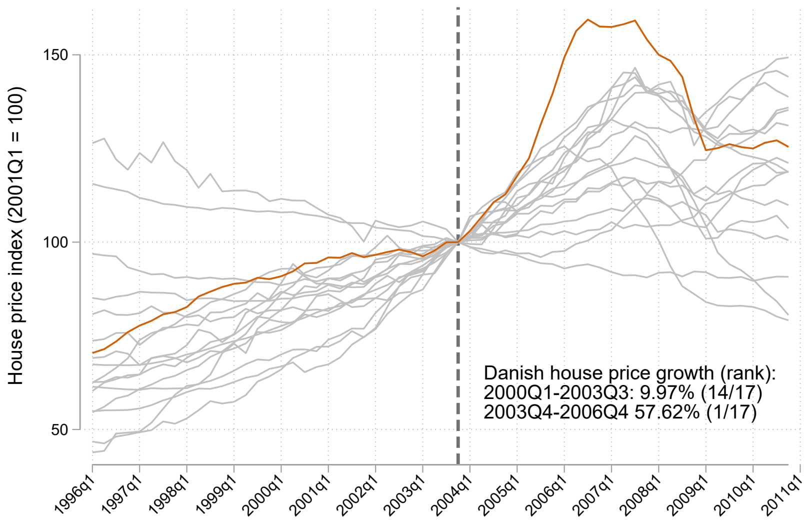



**Figure 1: House prices in Denmark and other countries, 1996-2010**

*Notes:* The solid orange line plots the real house price index for Denmark, and the solid gray lines plot house prices for all other countries in our sample. The sample includes Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Netherlands, Norway, Spain, Sweden, Switzerland, the United Kingdom, and the United States. The dashed line indicates 2003Q4, the quarter where Denmark introduced IO mortgages. Source: Bank for International Settlement, Detailed Residential Property Price Statistics, and authors' calculations.

mortgage use at the individual level. On the municipality level, we find a correlation of 0.73 between house price levels in 1998 and municipality IO mortgage share in 2009 after partialling out income levels in 1998. Intuitively, IO loans are more valuable for borrowers in higher-priced areas as they lead to larger dollar payment reductions relative to income in such areas, especially given Denmark's relatively compressed income distribution. Thus, higher exposure correlates directly with the borrower benefits of IO mortgages.

We estimate the causal impacts of IO loans on house prices using municipality-level house prices five years before policy implementation as a proxy for continuous treatment intensity (Mian & Sufi, 2012; Pierce & Schott, 2016; Berger *et al.*, 2020; Tracey & Van Horen, 2021). Since our exposure proxy is likely correlated with other cross-sectional characteristics important for house prices, we employ a comprehensive, fixed effects estimation approach within a rich panel data framework to isolate policy effects. We complement these estimates with several robustness and falsification tests to ensure that other local labor market and macroeconomic variables are not driving our results.

Specifically, we let the left-hand-side (LHS) variable in our main regressions be annual log changes in quarterly municipality-level house prices (year-over-year log differences). Our approach hence differences out any fixed, level differences across municipalities. In our regressions, we also include municipality and region × time fixed effects. Since the LHS variable is in log-differences, the included municipality fixed effects account for any differential long-run trends across municipalities over the sample period (Guren *et al.*, 2021). Such changes could stem, for example, from persistent housing or labor demand increases for superstar cities (Davidoff, 2016). The geographic regions in our sample aggregate municipalities to wider regions, which are approximately similar to U.S. MSAs (U.S. statistical aggregation for cities). The included region × time fixed effects thus account for time-varying shocks to house prices at the city level. Our regression models also include municipality-level controls measured pre-reform and interacted with time-fixed effects. These controls account for any time-varying impacts of pre-reform municipality-level differences in these variables on subsequent house price growth. Our results are similar with and without these controls. In total, the variation we use to identify the impact of the policy is thus orthogonal to fixed municipality-level differences, differential trends across municipalities, municipality ex-ante macro variables with time-specific coefficients, and any time-varying, city-level variation. In other words, we compare high- and low-exposure municipalities within each city and period, accounting for fixed and trending municipality differences and municipality macro controls. Our headline estimates are then a weighted average of these comparisons across cities.

Our preferred estimates show that a one-standard-deviation increase in exposure predicts a 2.3 percent (S.E. 0.4%) per quarter increase in year-over-year house price growth between 2003Q4 and 2006Q4. Aggregating these estimates over the entire boom period, we show that the introduction of IO mortgages explains over half of the rise in Danish house prices during the 2000s in treated municipalities versus those least affected by the reform.

The main threat to identification is time-varying shocks correlated with exposure. We address identification concerns in the following ways. First, as noted above, we implement a rich panel data setup so that our identifying variation is orthogonal differential municipality-level trends, ex-ante municipality macro variables with time-specific coefficients, and city-time fixed effects. Also, there are no pre-treatment differences in house price growth trends across high- and low-exposure municipalities, congruent with the difference-in-differences parallel pre-trends assumption. In falsification tests, we also find that ex-ante treatment intensity does not predict differences in pre-treatment changes in employment, unemployment, population, or income. The paths of these variables thus evolved in parallel across high and low treatment intensity municipalities during the pre-treatment period. Hence, the parallel pre-trends assumption within our regression framework also extends to local macroeconomic conditions. Fourth, the rapid implementation of the policy makes slower-moving explanations for the acceleration in Danish house price growth during the 2000s unlikely, especially since house price growth in ex-ante high exposure areas jumped immediately post-reform. Fifth, the reform was simultaneously introduced IO mortgages across Denmark. As all mortgage banks offered this product, it is unlikely that heterogeneous, regional credit supply-side factors drive our results. Sixth, we obtain similar results when we include controls for the mortgage interest rate. These results suggest that the steady decline in interest rates between 2000 and early 2005 does not explain our results.

Finally, we note that many countries experienced a housing inflection point in the early 2000s, but also that Denmark's house price growth was exceptional (Figure 1). While global credit supply shocks may have affected aggregate Danish house prices via lower interest rates, our robust within-Denmark research design and extensive controls ensure that global shocks are not driving our results. We also examine speculative investment activity but find little evidence that such activities increased following the introduction of interest-only mortgages. Our evidence thus suggests that the Danish housing boom started after IO mortgages were introduced and that house prices increased in areas where households found these mortgages more valuable.

What is the main economic mechanism driving identification? We argue that payment-to-income (PTI) constraints generate cross-sectional variation in the value of an IO mortgage. First, if borrowers or lenders include amortization payments in PTI calculations, an IO mortgage would naturally ease such assessments and enable more borrowing (see, e.g., Grodecka (2017) or Greenwald (2017) for models with payment-to-income constraints). Elevated housing demand due to relaxed PTI constraints would be especially acute if borrowers or lenders evaluate debt-service burdens based on initial payments. Second, the importance of PTI constraints varies across geographies depending on the size of the mortgage relative to income. If the mortgage and thus amortization payments are large relative to income with a fixed rate mortgage, then reducing those payments would lead to a notable relaxation of PTI constraints. As measured by our proxy for exposure, it is precisely in areas where the average price level is high that mortgage payments relative to income for homebuyers are also high. Backing up this assertion, Nissen *et al.* (2023) find that the share of payment-constrained borrowers is increasing in the square meter purchase price. Introducing IO mortgages thus lowers borrower mortgage expenses relatively more in these high-priced areas, causing a heterogeneous geographical increase in housing demand. Although the Danish mortgage banking sector promised to evaluate borrowers on their ability to afford a 30-year fixed-rate mortgage, a common view in Denmark is that mortgage banks did not follow this policy and instead assessed borrowers based on initial IO payments. Indeed, Danish law only mandated borrower assessments based on payments corresponding to a 30-year amortizing mortgage beginning in 2013 (Rangvid *et al.*, 2013, p.126). Altogether, these factors yield a causal role for IO mortgages in expanding credit: lenders relaxed credit assessments given that the borrower chose the newly introduced product. If PTI constraints are more likely to bind in municipalities with high price levels, house prices will increase relatively more in those areas following the introduction of IO loans.

Overall, introducing IO mortgages sparked the boom, and house prices would have been considerably lower without this mortgage reform. We can liken the house price impacts of mortgage payment reductions via IO loans to those stemming from interest rate declines. Assuming that interest rates and amortization payments have the same effect on house prices, the semi-elasticities of house prices with respect to interest rates in Glaeser *et al.* (2012) of between 7 and 20 suggest that the reduction in payments due to IO loans increased house prices by between 13 and 37 percent, in line with our main estimates. Yet this also leaves over 20 percentage points of the house price growth to be explained by other factors. Thus, our supposition is *not* that IO mortgages solely explain the boom. Rather, we contend that IO mortgages were the accelerant that ignited house price growth and led to other indirect effects, akin to a boom initially buoyed by falling interest rates.

Two further factors, in particular, are worth discussing. First, borrowers often combined IO mortgages with variable-rate mortgages. We show that the share of variable-rate mortgages increases dramatically throughout the boom, but IO mortgages drive the increase. Indeed, variable-rate mortgages with amortization payments *decline* as a share of outstanding mortgage debt. Our interpretation is that the introduction of IO mortgages sparked the adoption of variable-rate mortgages. Combining the two mortgage products further lowered mortgage payments and increased house prices. In this sense, our results are congruent with Dam *et al.* (2011), who estimate that variable-rate and IO mortgages explain approximately half of the rise in house prices. But since the Danish government introduced variable-rate mortgages in 1996, it is unlikely that the availability of variable-rate mortgages started the boom in 2003. Moreover, our exposure measure does not predict higher house price growth after the 1996 introduction of variable-rate mortgages, and the aggregate house price growth trend does not change after the variable mortgage reform. Second, rising house prices could affect house price expectations if such expectations depend on past growth rates. (Brueckner *et al.*, 2012; Case *et al.*, 2012; Dell'Ariccia *et al.*, 2012; Glaeser & Nathanson, 2017). An initial shock to house prices in areas with a high value of IO mortgages may experience an increase in house price expectations that fuel the boom, in line with the diagnostic expectations of Bordalo *et al.* (2020). Our results are thus consistent with recent work that finds that expectations did not initiate the 2000s housing boom (Griffin *et al.*, 2020), but also highlight how mortgage finance innovations may play a role in driving such expectations (see Foote *et al.*, 2012; Adelino *et al.*, 2016, 2020, on expectations and the financial crisis). While we lack time-series data on house price expectations before the crisis, survey evidence indicates that optimism rose in areas with larger IO mortgage-induced increases in house prices (Ministry of Economic and Business Affairs, 2005).

Our results suggest that financial innovation can have a considerable impact on housing markets even without asymmetric information in mortgage securitization, subprime lending, housing speculation, or non-recourse borrowing (see Conklin *et al.*, 2022, for a discussion of the role of subprime lending for house price appreciation and speculation). We differ from previous studies focusing on the United States (Barlevy & Fisher, 2021; Amromin *et al.*, 2018; Dokko *et al.*, 2019) by showing similar effects of mortgage innovation on house prices, while also arguing for different mechanisms. Fundamentally, our main contribution is to show how a mortgage innovation can cause a house price boom even within a well-regulated, often praised mortgage finance system with full recourse borrowing. By studying Denmark, our results are arguably more relevant for the many countries where IO mortgages were prominent in the 2000s. Scanlon *et al.* (2008) report that between 1995 and 2006, IO mortgages or similar products were introduced in Australia, Denmark, Finland, Greece, Korea, Portugal, and Spain.[1] In the post-crisis period, regulators in Sweden, Norway, and the Netherlands have moved to limit interest-only mortgages.

[1] In the United States, Amromin *et al.* (2018) find that mortgage products with lower initial payments constituted nearly 30 percent of U.S. mortgage origination volume in 2005, up from 2 percent in 2003, Justiniano *et al.* (2017) show that 44 percent of U.S. originations in 2005 were either interest-only, balloon mortgages, or Option ARMs, and Dokko *et al.* (2019) report that 60 percent of purchase mortgages contained at least one non-traditional feature. IO mortgages are currently relatively rare in the U.S. with the advent of the Qualified Mortgage (QM) and ability-to-repay rule but remain prevalent in other countries.

Our study also differs in the main mechanism that we emphasize: IO mortgages relax payment-to-income constraints. Barlevy & Fisher (2021), for example, emphasize the role of IO mortgages in feeding speculation, whereas we find a limited role for speculation. Greenwald (2017) shows how relaxed payment-to-income constraints can generate house price booms but focuses on changes in the PTI limit. A key contribution of this study is therefore to show how households can use mortgage innovation to lower amortization payments, thereby relaxing payment constraints. Our study is thus the first to link the relaxation of payment-to-income constraints caused by interest-only mortgages to house price growth. Finally, within the broader housing literature, our results also show that subprime borrowing, securitization, or flawed incentives in mortgage lending are not necessary conditions for starting housing booms. However, these factors may contribute to house price growth after the boom has begun or may be relevant for the start of a housing boom in other contexts.

## 2 Related literature

An extensive literature documents that a change in interest rates or credit availability has a sizable impact on house prices, but the effects of amortization payments on house prices have curiously received less attention until recently. Two related studies on Denmark are Karpestam & Johansson (2019) and Dam *et al.* (2011), who both estimate a version of the user-cost of housing model to explain the boom in aggregate house prices.[2] Karpestam & Johansson (2019) estimate an error-correction model for the Danish house price index and find that including amortization payments helps better predict house prices and mortgages relative to disposable incomes. However, they do not study heterogeneous effects across regions. Dam *et al.* (2011) use a user-cost model to decompose the Danish housing boom into various factors, including interest-only mortgages and variable-rate mortgages. The authors find that the introduction of new loan types significantly impacted aggregate house prices. They show that up to 46 percentage points of Denmark's average real house price increase is explained by the popularity of variable-rate and interest-only loans, with the two loan types playing roughly equal parts.

In a U.S. context, Barlevy & Fisher (2021) argue that IO mortgages facilitated housing speculation and raised house prices. Agents in their theoretical model have an incentive to use

[2] Other studies focusing on interest-only mortgages in Denmark include Larsen *et al.* (forthcoming), who study how interest-only mortgages affect consumption and savings over the life-cycle, Bäckman & Khorunzhina (2019), who study how the IO mortgage reform affects aggregate consumption, and Kuchler (2015), who finds that families hold a relatively large share of total interest-only mortgage debt with few liquid assets and high loan-to-value ratios. Finally, Andersen *et al.* (2019) find that consumption falls when the interest-only period expires.

IO mortgages to speculate on rising house prices and can default if prices fall. By increasing speculative demand, IO mortgages boost prices above their fundamental value. Dokko *et al.* (2019) find that increases in borrowing through mortgages with non-traditional features, such as initial interest-only payments, preceded the substantial house price growth in many U.S. counties during the 2000s. Both Barlevy & Fisher (2021) and Dokko *et al.* (2019) argue for a crucial role of speculation for rising house prices facilitated by interest-only mortgages.

## 3 Data description

We construct several datasets for the primary analysis. First, from FinansDanmark, we collect quarterly indices of average square meter prices for all regions and municipalities from 1992 based on Danish transaction-level data.[3] The square meter price data is available for apartment and single-family houses, which we combine to obtain a single square meter price series for each municipality.[4] We complement this dataset with municipality-level unemployment rates and income. Summary statistics are available in Table B2.

We also collect high-quality micro-data from Statistics Denmark on the entire population of households on an annual basis. All data registers come with unique personal and household identifiers that link different registers together contemporaneously and over time. The housing register comes with a unique property-level identifier. We collect data on housing ownership, property transactions, and detailed demographic and economic details on the universe of all Danish households from 1994 to 2010. We match data on property transactions to each household using ownership registers. We select transactions with one or two buyers and collect detailed demographic and financial data for each buyer. Individual- and household-level variables include household-level demographic information and financial information such as financial wealth and household income. If there are two buyers, we select the highest value of age, education, family size, and the number of children as the relevant value. We calculate the sum across buyers for

[3]The main advantage of these indices is that they provide continuous coverage at the municipality level both before and after the reform. Denmark went through a municipality reform in 2007 that reduced the number of municipalities from 271 to 98, which created a break in the house price index provided by Denmark Statistics. We have confirmed that the index from FinansDenmark is comparable to the index from Denmark Statistics for the period when both are available. See Appendix C.

[4]Specifically, we calculate a weighted average using the percentage of each transaction type as weights. The transaction-level data is only available starting in 2004 (after the reform), so we calculate the average share of housing transactions as a percentage of total transactions (single-family homes plus apartments between 2004 and 2006) and use that as weights. The correlation between single-family prices and apartment prices is 0.91, and we also validate that our results are robust to using the single-family square meter price. The square meter price index for apartments does not fully cover all municipality-quarter observations. There are very few available apartments for certain municipalities, and correspondingly there are not enough apartment transactions to construct reasonable quarterly square meter prices.

income, debt, and financial wealth.

We also collect detailed individual-level mortgage data from 2009. This dataset contains information about each mortgage, including maturity, interest rate, whether it is an IO mortgage, and origination date. We use the origination date to extrapolate backward in time, allowing us to examine IO mortgage penetration by year.[5]

## 4 Danish housing and mortgage markets

This section describes the housing market and mortgage system in Denmark. See Campbell (2013) for more information about Danish mortgage market design and Bäckman & Lutz (2020) for summary statistics regarding the Danish mortgage market after the introduction of IO mortgages.

The mortgage finance system in Denmark is highly rated internationally and similar in many ways to the U.S. (Campbell, 2013). Like in the U.S., Danish mortgages have historically consisted of long-term fixed-rate mortgages without prepayment penalties. Households can finance up to 80 percent of home purchases using mortgage loans with a legally mandated maximum maturity of 30 years and fund an additional 15 percent using higher interest bank debt. Denmark does not have a continuous credit-score system, and there are no requirements for positive equity for refinancing. There are no prepayment penalties, and households can legally refinance their mortgage loans to take advantage of lower interest rates, provided the principal balance does not increase. Borrowers can extract equity if they meet the loan-to-value limit, but this would involve a new credit assessment. To refinance into a new interest-only mortgage, a borrower would have to take out a new mortgage, which could be larger than the previous one as long as the loan-to-value ratio is met (see Bäckman & Khorunzhina, 2019).

Mortgage credit in Denmark is extended to borrowers through specialized lenders called mortgage credit banks (henceforth, mortgage banks), who act as intermediaries between borrowers and investors. There were seven mortgage banks in operation at the time of the reform. Danish regulations prohibit mortgage banks from offering new products without regulatory and legal approval, which has limited the number of mortgage products available for households. Mortgage banks are required to assess both the value of the underlying property and the borrower's abil-

[5] We mainly use this dataset to confirm that our proxy for the value of an IO mortgage subsequently captures IO mortgage penetration. Extrapolating backward generally provides a worse match further back in time. For example, if a borrower took out an IO mortgage in 2004 but refinanced it in 2008, we would only observe the mortgage from 2008.

ity to afford mortgage payments (International Monetary Fund, 2011). After extending credit to borrowers, mortgage banks sell the proceeds of the loans to investors via mortgage bonds. The market for mortgage bonds hence sets the interest rate for borrowers. Moreover, mortgage banks are legally mandated to hold each mortgage bond on their balance sheet throughout the loan period, thereby retaining any credit risk. If a borrower defaults, the mortgage bank must replace the defaulting mortgage with one with similar characteristics. Mortgage bonds, therefore, face no credit risk (in over 200 years of operation, no investor has lost money from mortgage bond defaults (Andersen *et al.*, 2020)), provided the issuing lender remains solvent. Investors in mortgage bonds instead assume interest rate and prepayment risk. Thus, mortgage bond investors do not price default risk. Instead, the mortgage bank charges an annual fee on top of the interest rate to cover default risk. There is no indication that mortgage banks adjust other parts of the contract to account for higher default risk.

Moreover, loss-given default is likely low in Denmark as the banks can garnish future income, making home equity a less important consideration for pricing default risk. Indeed, Larsen *et al.* (forthcoming) find that interest-only mortgage holders did not default to a greater extent during the Danish housing bust. Thus, IO loans and amortizing mortgages share similar (but not the same) credit risks for mortgage banks, even though IO borrowers have lower equity levels early in the loan term.

If a borrower defaults, the mortgage bank can trigger a forced sale of the underlying asset. Any residual claim is converted into an unsecured personal claim, where the interest rate is higher than the mortgage rate. In legal parlance, mortgage loans in Denmark are full recourse, which limits the incentive for strategic default (Gerardi *et al.*, 2018; Cunningham *et al.*, 2021, see). This feature of the Danish mortgage system makes default unattractive for borrowers (see Ghent & Kudlyak, 2011, for evidence on recourse laws and default from the United States), and default rates remained low throughout the boom and the bust. In Denmark, mortgage arrears peaked at 0.6 percent of outstanding amounts, and forced home sales remained low throughout the housing bust. Compare this to the United States, where comparable mortgage defaults peaked above 10 percent. Further, personal bankruptcy in Denmark is difficult and does not necessarily reduce the debt burden.

In total, the Danish mortgage system provides 1) strong incentives for the borrower to carefully assess the state of their future income and the state of the housing market and to not over-extend themselves and 2) a low-risk environment for investors in mortgage bonds (Campbell, 2013). Altogether, the Danish system's design minimizes concerns regarding asymmetric lending information, excessive points and fees, low documentation loans, and limited monitoring of new borrowers during the 2000s boom.

### 4.1 Mortgage Reform in 2003

Following a rapidly implemented law change, mortgage banks could offer IO mortgages beginning on October 1, 2003. The law was introduced to the Danish parliament on March 12, 2003, and passed on June 4, 2003, with a significant majority voting in favor of the proposal. Specifically, the law change allowed mortgage banks to offer IO loans. With these *deferred amortization* mortgages, principal repayments could be postponed for up to 10 years, even though the total amount still had to be repaid over the 30-year contract.[6] The government intended to increase the flexibility of mortgage financing, thereby improving affordability for cash-constrained households, such as students, young adults, and households on temporary leave from the labor market (e.g., certain marginal borrowers). The expectation was that IO loans would serve as a temporary niche solution. The government expected that penetration would be low with no long-term impact on house prices or consumption.[7]

Instead, IO loans rapidly became popular across all types of households (Bäckman & Lutz, 2020). Figure 2 plots the outstanding mortgage amounts by loan type. Before the reform, nearly all mortgages were fixed interest with amortization payments, but this quickly changed once IO mortgages were introduced. One year after the reform, 17 percent of all *outstanding mortgages* were IO loans. This number increased to 31 percent at the end of 2005 and 54 percent in 2010. Mortgage lending also expanded markedly following the reform, rising by nearly 40 percent between the reform and the end of 2006, with the bulk of this increase due to IO loans.

[6]The law technically allows the *mortgage* to have a ten-year interest-only period. Amortization payments can potentially be deferred forever by rolling over into a new mortgage contract after ten years, provided that the house value does not decrease. Danish media reported on this aspect of the new loans. See, e.g., Politiken (2003). Andersen *et al.* (2019) find that about 20 percent of borrowers whose interest-only period expires refinance to a new IO mortgage, 60 percent start repaying as scheduled, and 20 percent take out a new mortgage with amortization payments.

[7]The law proposal includes a rationale for the reform, along with the expected effects. The material is available in Danish at `https://www.retsinformation.dk/Forms/R0710.aspx?id=91430`.

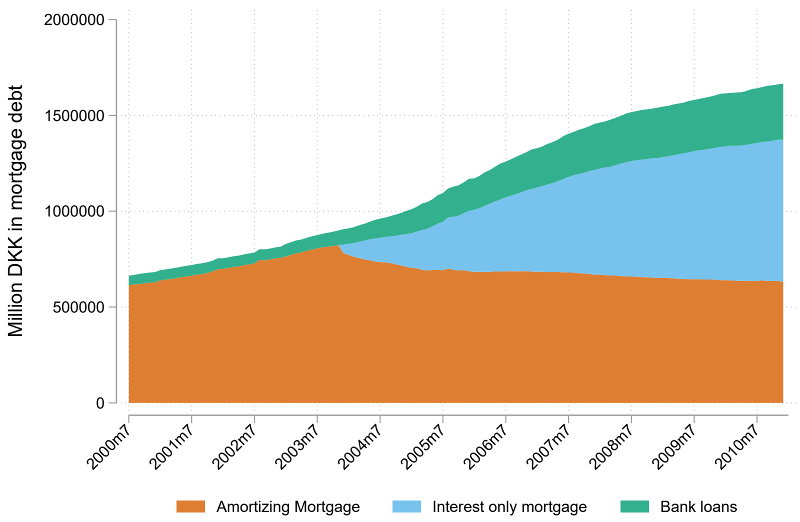



**Figure 2: Stock and Flow of Outstanding Mortgage Debt**

*Notes:* The figure plots outstanding mortgage debt divided into bank loans, interest-only mortgages, and amortizing mortgages. The figure combines fixed and variable-rate mortgages and includes loans for residential properties and vacation homes. A figure with all loan types separately can be found in Figure A1 in Appendix A. Source: Nationalbanken Statbank, Table DNEJER. https://nationalbanken.statistikbank.dk/DNEJER

## 5 Why amortization payments affect house prices

In a traditional user-cost of housing model, the cost of owning versus renting determines house prices (Poterba, 1984). Traditionally, the cost of ownership has not included amortization payments, as amortization payments are a form of savings. However, recent research (Amromin *et al.*, 2018; Bernstein & Koudijs, 2021; Bäckman *et al.*, 2023) suggests that amortization payments represent real costs for households. If amortization payments are indeed costly, we should include such payments in the user cost of housing.

Amortization payments can limit borrowing if they are a part of the credit assessment conducted by banks. The most obvious example is if amortization payments are a part of payment-to-income constraints, either enforced by the lender or perceived by the borrower. If total mortgage payments (interest plus amortization) must be lower than some fraction of income, reducing amortization payments raises borrowing capacity. Bäckman & Khorunzhina (2019) contend that interest-only mortgages relaxed payment-to-income constraints for existing homeowners in Denmark and caused an increase in consumption. Greenwald (2017) argues that relaxed payment-to-income constraints are vital for explaining the US housing boom. Bäckman *et al.* (2023) finds that households reduce their borrowing in response to higher amortization payments, an effect they attribute in part to payment constraints.

Alternatively, amortization payments are a cost if they force households to save more than desired or if amortization payments restrict portfolio allocation (Cocco, 2013). Suppose households are saving more than desired because of amortization payments (and they cannot undo higher savings by borrowing or refinancing (Svensson, 2016; Hull, 2017)). In that case, lower payments increase utility by moving the household closer to its optimal consumption level. The increased opportunities for consumption smoothing may affect property values, as buyers typically derive greater utility from owning. Moreover, amortization payments are also a cost if they affect portfolio choice. Saving by paying down the mortgage generally has a lower expected return than investing in stocks or pension savings (see Larsen *et al.*, forthcoming, for evidence on how Danish homeowners use IO mortgages). Fourth, households can mistake amortization payments for interest payments (see Argyle *et al.*, 2020; Shu, 2013). For example, in a survey in SBAB (2018), 37.7 percent of Swedish households stated that amortization payments were a cost, compared to 44.2 percent who said they were savings. Finally, Barlevy & Fisher (2021) argue that borrowers can use IO mortgages for speculative purposes with uncertain price appreciation and no-recourse borrowing.

Each of these factors highlights why IO mortgages may be valuable to borrowers. Notably, the channels mentioned above are *additive*, not exclusionary, and will be more or less relevant depending on the institutional setting. For example, gambling on house price growth is considerably riskier in Denmark than in the United States because of Danish (enforced) recourse borrowing. Thus, the IO speculation channel emphasized as a cause for the 2000s boom in Barlevy & Fisher (2021) is less relevant in Denmark than in the United States. Yet several of the other preceding reasons explain why IO mortgages may positively impact borrowing and thus housing demand in Denmark.

An IO-mortgage-induced increase in available credit can boost housing demand, provided the credit shock is large enough. The rise in housing demand consists of an extensive and intensive margin: The extensive margin effect of the reform is how many households chose an IO mortgage that otherwise would have chosen a fixed-rate mortgage or not purchased a home at all. The intensive margin is the reduction in payments for the borrowers who chose an IO mortgage. If new mortgage products were not widely available or only used by a small fraction of households, we would not expect housing demand to increase. Similarly, if the reduction in payments is small, we would not expect a considerable impact on demand. Below we provide evidence of a

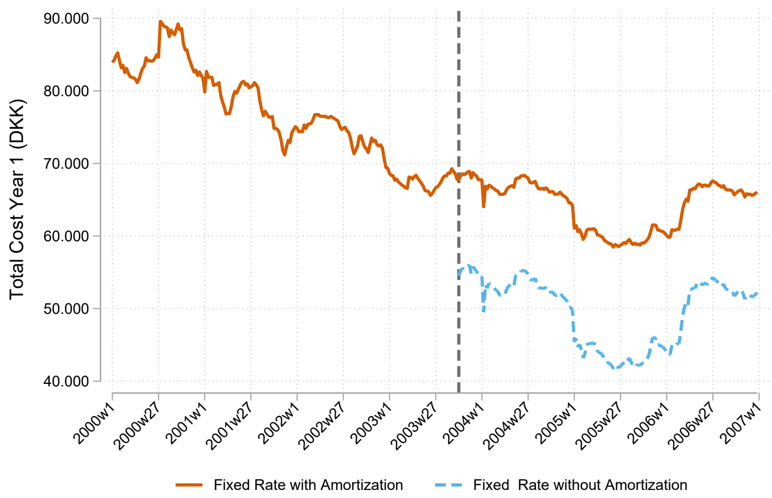



**Figure 3: First Year Payments**

*Notes:* The dashed vertical line indicates the introduction of interest-only mortgages. The figure plots the total first-year expense for a 1 million Danish Krone (DKK), fixed-rate mortgage contract, with amortization payments (solid line) and without amortization payments (dashed line). Both lines are calculated using the long-bond rate from the Association of Danish Mortgage Banks. Source: Association of Danish Mortgage Banks and authors' calculations.

significant shift in both the extensive and intensive margins. Since both of these margins were sizable, we expect to see a large increase in housing demand.

The increase in demand can affect either price of owner-occupied housing or the quantity of housing demanded, depending on the slope of the supply curve (Greenwald & Guren, 2021). If the supply curve is horizontal, any change in housing demand will cause an increase in either housing size or the homeownership rate. If the supply curve is instead vertical, changes in demand will appear in the price of housing. Bäckman & Lutz (2020) analyze both the house size and homeownership effect of the IO mortgage reform and ultimately conclude that there are no changes in either. The evidence therefore points towards a vertical supply curve. Note also that the rapid nature of Denmark's IO mortgage legalization process played a crucial role in the high subsequent house price growth, as supply could not quickly adjust to increased demand.

Even though an IO loan is considerably more expensive in total (e.g., total interest payments over the life of the loan), it allows for substantially smaller first-year payments. Figure 3 plots the first-year cost for a newly originated 1 million Danish Krone (approximately 150,000 USD) fixed-rate mortgage with (solid line) and without (dashed line) amortization payments over time. The calculations in Figure 3 are based on average fixed mortgage interest rates for each year-month.[8] In October 2003, choosing an IO mortgage reduced total first-year payments for a 1 million DKK loan by approximately 20 percent per year, or 13,259 DKK (USD 2,000).[9]

For the average home buyer in Copenhagen during 2003, the difference in annual payments for a fixed-rate mortgage amounts to 6.7 percent of annual disposable income or 11.9 percent of annual income for a variable interest-rate mortgage (see Dokko *et al.*, 2009, Table 3, for a comparison of payments with different mortgage types for the United States during the housing boom).[10]

Regarding the extensive margin, these new mortgages rapidly became the mortgage of choice for Danish homebuyers. Panel a) of Figure 4 shows that approximately 60 percent of homebuyers chose an IO mortgage in 2006. Panel b) shows that it was not only low-income buyers who chose an IO mortgage but rather that these mortgages were broadly popular among both high and low-income borrowers.[11] Bäckman & Lutz (2020) document similar results for age and wealth. While Figure 4 illustrates how popular IO mortgages became across the income distribution, the figure does not contain information about credit risk. We discuss credit risk for IO borrowers compared to traditional mortgage holders in Section 7.

## 6 Results

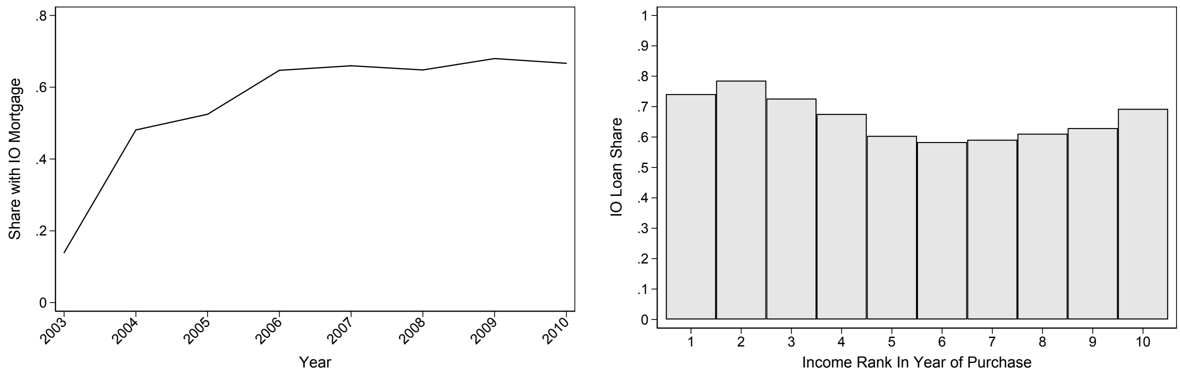
This section provides the methodology and main results of our study. We first provide evidence that our exposure measure, house price levels before the reform, strongly predicts subsequent IO mortgage use at the municipality and borrower levels for all years after the reform. We then describe our empirical methodology and discuss threats to identification. Since we are primarily interested in the total effects on local housing markets, we study house prices at the municipality

[8]See table B1 for further comparisons of mortgage payments under various annuity schedules and interest rates. Note that there are no or little differences in the interest rates for IO and amortizing mortgages (see Larsen *et al.*, forthcoming, page 8). However, the fee may differ between the two loan types, particularly in the post-crash period.

[9]Calculations are based on an annuity schedule, where the total monthly cost is constant across the loan period. The reduction in monthly payments would be higher with a straight amortization schedule. Total payments equal 67,759 DKK for a fixed-rate mortgage with amortization payments and 54,500 DKK for a fixed-rate mortgage without amortization payments. The difference equals $67,759 - 54,500 = 13,259$ DKK, or $13,259/67,579 = 20$ percent.

[10]The average sales price in Copenhagen was 1.3 million DKK, the average disposable income for buyers was 204,575 DKK per year, and the average difference in payments from the introduction of IO loans to the end of 2003 for a 1 million DKK mortgage was 13,259 DKK for a fixed-rate mortgage and 23,370 DKK for a variable interest mortgage. Assuming a down payment of 20 percent, the savings as a percentage of disposable income is 13,259*1.3*0.8 / 204,575 = 6.7% for a fixed-rate mortgage and 23,370*1.3*0.8 / 204,575 = 11.9% for a variable-rate mortgage.

[11]The sample is limited to homebuyers between 2004 and 2009. As discussed in Bäckman & Lutz (2020), this data is subject to survivorship bias because we assign mortgages back in time based on the origination date. The results are robust to focusing only on data from 2009 and onward when there is no survivorship bias. We observe a similar pattern if we focus on all households instead of homebuyers.




**a) Interest-only mortgages among buyers over time**

**b) Interest-only mortgages among buyers in the income distribution**

**Figure 4:** Interest-only mortgages use among homebuyers

*Notes:* The figure in panel a) plots the share of IO mortgages among homebuyers. The data only include observations where a buyer can be matched to a mortgage. Panel b) plots the IO loan share among homebuyers, dividing the sample into deciles based on disposable income. Source: Figures 6 and 7 of Bäckman & Lutz (2020).

level instead of buyer-level outcomes. Areas with low exposure act as a control group, which allows us to estimate the policy's causal impact on subsequent house price growth by looking at differences across local markets. We end this section by presenting our main results and an aggregation exercise that calculates the aggregate impact of the reform on house prices.

### 6.1 Exposure and IO mortgage use

Our identification strategy based on ex-ante geographical treatment intensity builds on previous studies that use a similar approach to estimate the cross-sectional impact of fiscal policy (Mian & Sufi, 2012; Chodorow-Reich *et al.*, 2012) and the effect of housing market policy (Berger *et al.*, 2020; Tracey & Van Horen, 2021). Specifically, we exploit cross-sectional differences in ex-ante exposure to the IO mortgage reform measured as municipality-level square meter prices in 1998, five years before the reform. Using other years as the base value yields similar results, but 1998 has the advantage of being far enough back in time to avoid any anticipation effects to confound our estimates.

Our exposure measure aims to capture the benefit of an IO mortgage for the *marginal* buyer. The value of an interest-only mortgage depends on the marginal borrower's valuation of the option to avoid amortization payments. The key idea is that this valuation depends on the size of amortization payments, which depends on the value of the purchased property and the mortgage. Since amortization payments for a \$100,000 mortgage are less onerous than amortization payments on a \$500,000 mortgage, holding income constant, the borrower with

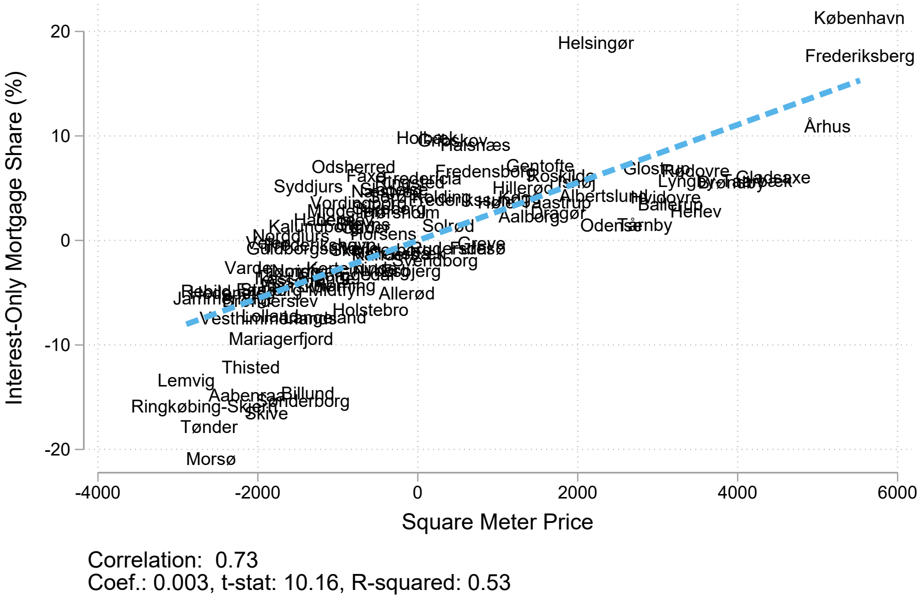



**Figure 5: Municipality-Level Ex-Ante Exposure and 2009 IO mortgage share, Controlling for Pre-Treatment Household Income**

*Notes:* This figure plots the average square meter price in 1998 against the IO mortgage share in 2009 for each municipality in Denmark. We calculate the IO mortgage share for 2009 by collapsing individual borrower data to the municipality level. The primary IO mortgage dataset covers all Danish mortgages and includes information about the location of the property used as collateral in the mortgage. We plot results after partialling out the average municipality income level in 1998. The coefficient, *t*-statistic, and R-squared are from a regression of the form: $IO_k = \alpha + \beta SquareMeterPrice_k + \gamma Income_k + \epsilon_k$, where $IO_k$ and $SquareMeterPrice_k$ denotes the IO share and square meter price in 1998 for municipality $k$.

the larger house should be more inclined to avoid such payments.

Using price levels captures the idea that the marginal buyer faces different mortgage payments depending on the size of the mortgage, which is well approximated with the square meter price. Using other proxies, such as mean municipality-level debt, would confound the marginal buyer with the municipality's average inhabitant, who has already paid off at least part of their mortgage. Moreover, the average owner is not usually equal to the marginal buyer, making even average debt levels for owners a potentially misleading measure of exposure to the reform. Furthermore, data on house price levels are publicly available in Denmark and elsewhere, whereas mortgage debt data at the municipal level is less readily available. Therefore, using house price levels makes it easier to replicate our results for Denmark and extend our analysis to other countries to test the external validity of our estimates. We also note that average price and debt levels strongly correlate with IO mortgage use (Bäckman & Khorunzhina, 2019; Bäckman & Lutz, 2020).

We begin by showing cross-sectional evidence consistent with the idea that ex-ante price levels capture the benefit of using an interest-only mortgage in Figure 5. The figure plots IO mortgage share against house price levels in 1998, five years before the reform, after partialling out average municipality income levels. The figure shows a clear positive relationship: the correlation between the two variables is 0.73. A regression of exposure on interest-only mortgage share yields a coefficient of 0.003 with a $t$-statistic of 10.16 and an R-squared of 0.53. Figure A3 in Appendix A shows similar results when we do not control for income levels (the correlation without controlling for income is then 0.81). Likewise, Figure A4 in Appendix A shows that while exposure is highest around Copenhagen, there is still considerable exposure in other parts of Denmark.

More formally, we estimate the following equation using transaction-level data:

$$InterestOnly_{i,k,t} = \alpha + \beta Exposure_k + \gamma \mathbf{X}'_{i,t} + \delta_t + \epsilon_{i,t} \tag{1}$$

where $InterestOnly_{i,k,t}$ is an indicator equal to one if the buyers involved in transaction $i$ in municipality $k$ use an IO mortgage to purchase a home in year $t$. $Exposure_k$ represents the square meter price in municipality $k$ in 1998, normalized by its standard deviation to ease interpretation. Thus, for a one standard deviation increase in *Exposure*, $\beta$, the coefficient of interest, represents the corresponding increase in the average probability that a buyer chooses an IO mortgage, holding controls constant. $\mathbf{X}'_{i,t}$ is a vector of controls for the buyers of transaction $i$ at time $t$. Controls include family size, number of children, a dummy equal to one if the buyer is retired, the employment ratio (the share of the year in full-time employment for the individual), gender, a dummy equal to one if the buyer owned property in the previous year, the log liquid wealth, and log total income as well as the lagged and future value of log total income. We also include year fixed effects, $\delta_t$. We cluster standard errors at the municipality level.

The results in Table 1 show that Exposure strongly predicts IO mortgage penetration. A one standard deviation increase in Exposure leads to a 9-11 percent increase in IO mortgage use. The coefficient on Exposure is significant at the 1% level, stable across specifications, and robust to the inclusion of controls. Columns 1-4 present results across transactions between 2003 and 2010, our full sample period. Yet the results are robust to the periods around the reform as well. Columns 5-8 restrict the sample to transactions from 2003 to 2006 without decreasing coefficient magnitude or significance. Thus, it is not the case that the financial

**Table 1:** Exposure and IO mortgage share

| | 2003-2010 | | | | 2003-2006 | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Exposure | 0.0924*** | 0.0948*** | 0.102*** | 0.101*** | 0.0996*** | 0.104*** | 0.112*** | 0.112*** |
| | (0.00850) | (0.00901) | (0.00899) | (0.00879) | (0.0109) | (0.0113) | (0.0114) | (0.0111) |
| Base controls | No | Yes | Yes | Yes | No | Yes | Yes | Yes |
| Wealth and income controls | No | No | Yes | Yes | No | No | Yes | Yes |
| Lagged controls | No | No | No | Yes | No | No | No | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Obs | 105,036 | 105,036 | 103,825 | 100,652 | 50,338 | 50,338 | 49,672 | 48,202 |
| Adjusted R-squared | 0.090 | 0.105 | 0.111 | 0.112 | 0.109 | 0.127 | 0.135 | 0.134 |

*Notes:* The table presents estimates of Equation (1). Exposure is defined as the square meter price in 1998. The sample consists of individual-level buyers, where we select transactions with one or two buyers. Base control variables include family size, number of children, a dummy equal to one if the buyer is retired, the employment ratio, gender, and a dummy equal to one if the buyer was a property owner in the previous year. Wealth and income control variables include the log liquid wealth and log total income. Lagged control variables include the lagged and future value of log total income. One, two, and three asterisks correspond to statistical significance at the 10, 5, and 1 percent levels, respectively.

crisis drove borrowers to IO mortgages because of a loss of earnings. Figure A5 in Appendix A further shows that the coefficient on Exposure is also robust to examining individual years and selecting only buyers who were not owners in the previous year. The reform's effect on house prices is thus not necessarily driven only by first-time homebuyers but also by existing buyers. Finally, Figure A6 in Appendix A shows similar results when we aggregate transactions to the municipality-level.

### 6.2 Methodology and threats to identification

We examine the effects of IO mortgages on house prices using a generalized difference-in-differences design that tests whether areas with ex-ante higher price levels (exposure) experienced higher subsequent house price growth increases. We estimate the following dynamic equation:

$$\begin{aligned} \Delta \ln HP_{kt} = & \sum_{y \neq 2003q3} \beta_t 1\{y = t\} \times Exposure_k \\ & + \sum_{y \neq 2003q3} \eta_t 1\{y = t\} \times \mathbf{X'}_k \\ & + \tau_k + \tau_{rt} + \alpha + \epsilon_{kt} \end{aligned} \quad (2)$$

The dependent variable is the annual growth rate in house prices for municipality $k$ in quarter $t$, $\Delta \ln HP_{kt}$, defined as the year-over-year log difference in house prices. $Exposure_k$ is a continuous variable equal to house price levels in 1998, five years before the reform, that measures exposure for municipality $k$. To ease interpretation, we normalize $Exposure$ by its standard deviation.

The key coefficients of interest are the time-varying difference-in-differences parameters, $\beta_t$, that measure the difference in house price growth across high and low exposure municipalities for each year-quarter (first difference) relative to the last pre-treatment period in 2003Q3 (second difference, corresponding to the omitted dummy). This dynamic setup also allows us to test the difference-in-differences parallel pre-trends assumption by examining $\beta_t$ for $t <$ 2003Q3. $\mathbf{X'}_k$ are municipality-level control variables, measured pre-reform as they are potentially endogenous to the reform in the post-reform periods. We include the income level in 1998 and the unemployment rate in 2000 as controls and interact each variable with time fixed effects to allow their impact on house price growth to vary with time.

Additionally, we include the growth in disposable income between 2002 and 2006 to account for a tax reform passed in 2003. The tax reform increased the threshold of the medium tax bracket, lowering the tax rate from 2004 for a considerable fraction of the population (Jakobsen & Søgaard, 2022). While disposable income is potentially endogenous to the reform (see, e.g. Bernstein & Koudijs, 2021, who show that increasing amortization payments led to a labor supply response), we include it to ensure that this tax reform does not drive our results. Our central estimates vary little with the inclusion of this control. $\tau_k$, $\tau_{rt}$, and $\alpha$ represent municipality dummies, region-time fixed effects, and the constant. As the dependent variable is in log differences, municipality fixed effects capture differential long-run trends across municipalities over the sample period. Note also that our aggregation of municipalities leads to regions that approximate U.S. MSAs.[12] Hence, region $\times$ time fixed effects control for time-varying, city-level shocks to house prices. Robust standard errors are clustered at the municipality level to account for correlation in within-municipality shocks over time. The regression is weighted by the number of transactions in 2004Q1 (the first quarter where transaction data are available). The results are similar if we use population weights instead.

The main threat to identification is time-varying shocks correlated with *Exposure* that also led to house price growth differences across geographies. We address potential endogeneity concerns in several ways. First, as noted above, regressions include municipality fixed effects and region $\times$ time fixed effects to account for differential municipality trends over the sample

[12]Regions ("landsdele") approximately correspond to U.S. MSAs. From 1970 to 2006, Denmark had 13 counties ("amter") and three municipalities with county status. After 2007, Denmark has five regions. We have decided to use our regions ("landsdele"), even though they do not perfectly correspond to administrative areas. Our house price data contain unbroken time series data across the municipality reform for this geographical division. Data from Statistics Denmark, for example, does not provide regional house price data for consistent geographical units before and after the 2007 reform.

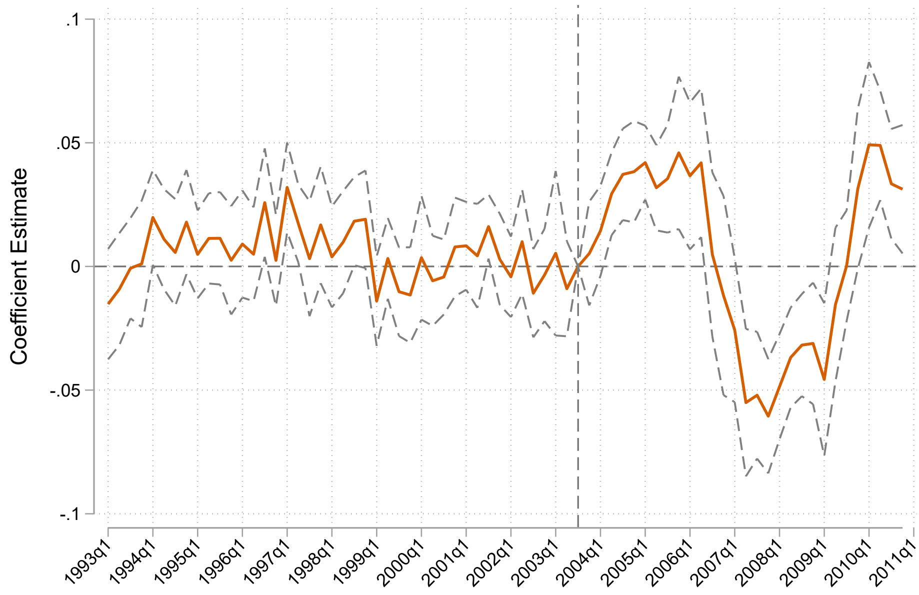



**Figure 6: The effect of IO mortgages on house price growth**

*Notes:* The figure plots the difference-in-differences coefficients, $\beta_t$, from Equation 2. Control variables include the average municipality average income in 1998, the municipality unemployment rate in 2000, and the growth in municipality-level disposable income between 2002 and 2006. We interact all controls with year-quarter dummies. The regression also includes municipality and region-time fixed effects. Observations are weighted by the number of transactions in 2004Q1, and the dashed lines show 99 percent confidence intervals computed using robust standard errors clustered at the municipality level.

and time-varying, city-level shocks to house prices, such as local labor market shocks. Hence, our identification scheme exploits ex-ante variation in house price levels across local administrative areas while accounting for municipality trends and time-varying regional differences to measure the impact of IO mortgages on house price growth. Second, we conduct several falsification tests and directly examine if other variables, such as income growth, differ depending on *Exposure*. Reassuringly, our pre-treatment exposure intensity measure does not predict changes in key local macroeconomic variables, supporting a causal interpretation of our results. Third, ex-ante treatment intensity does not predict trend differences in pre-reform house price growth, consistent with the difference-in-differences parallel pre-trends assumption. Moreover, if house price expectations depend on past growth rates (Brueckner *et al.*, 2012; Case *et al.*, 2012; Dell'Ariccia *et al.*, 2012; Glaeser & Nathanson, 2017), similar trends before the reform also imply similar pre-reform trends in house price expectations. Last, we interact municipality-level control variables, such as pre-treatment unemployment and income, with time fixed effects to allow pre-treatment variation in these variables to have differential impacts on subsequent house price growth over time. Our primary estimates vary little with the inclusion of these controls.

### 6.3 Main results

Figure 6 plots the difference-in-differences coefficient estimates corresponding to $\beta_t$, along with their 99 percent confidence intervals. Three key results are immediate. First, there is no statistically significant difference in pre-reform house price growth trends, congruent with the difference-in-differences parallel pre-trends assumption. The lack of pre-trends extends back to 1993, covering the period when Denmark introduced variable-rate mortgages. Therefore, the introduction of variable-rate mortgages had no significant effect on house price growth across high and low *Exposure* municipalities. The parallel pre-trends also speak directly to a potential confounder for the start of the boom, house price expectations. Assuming that past house price growth is a proxy for expectations of future growth (see Brueckner *et al.*, 2012; Case *et al.*, 2012; Dell'Ariccia *et al.*, 2012; Glaeser & Nathanson, 2017), the parallel pre-trends in Figure 6 allow us to rule out differential trends in house price expectations across high and low *Exposure* municipalities as a factor in the start of the Danish housing boom. Note that expectations may be a key contributor later in the boom, an idea we discuss further in section 7. Still, the evidence suggests that differences in expectations are not correlated with the subsequent changes in local house price growth and that expectations did not ignite the boom.

Second, municipalities with higher ex-ante *Exposure* experienced a larger spike in house price growth directly following the reform, highlighting the immediate impact of introducing IO mortgages on house prices in the areas where borrowers found these loans valuable. Differences in house price growth across high and low *Exposure* municipalities remain positive until late 2006. Figure A7 in Appendix A compares the estimates from equation 2 with and without the local macroeconomic controls, $\mathbf{X}'_k$. The figure shows that our estimates vary little with the inclusion of local macroeconomic controls, highlighting the robustness of our estimates.

Third, house prices in high *Exposure* municipalities revert to the mean in the subsequent housing bust, showing the short-term momentum and long-run mean reversion behavior common in housing markets (Glaeser & Nathanson, 2012). While deciphering the exact cause of the decline is beyond the scope of this paper, the rapid decline in prices can potentially be explained endogenously. First, if IO mortgages drove expectations in high *Exposure* areas later in the boom, the reversal of housing markets once the global financial crisis hit likely led to a reversal of these expectations. Second, affordability assessments after the crisis were tightened. For example, by 2009, mortgage banks had raised fees on IO mortgages (one channel through which Danish mortgage banks can adjust for credit risk). While fees on IO mortgages were similar to amortizing mortgages before 2009, the gap in fees was 0.27 percent by 2016 (The Danish Competition and Consumer Authority, 2017, page 57).[13] Third, a rapid increase in house prices is often associated with a large increase in construction, which would equalize prices between areas. Construction also boomed in Denmark and peaked in 2006 (Bäckman & Lutz, 2020). Higher house price growth in areas with higher *Exposure* would plausibly lead to higher construction activity and thus a more considerable price decline once housing supply adjusted. Overall, the decline in house prices in high *Exposure* municipalities suggests that either some factors behind the increase were temporary (which would be the case with house price expectations) or that other factors worked to equalize house prices across areas (which would be the case with construction).

Table 2 provides estimates for several different specifications for the pre-and post-reform periods. Specifically, we estimate the following equation:

$$\Delta HP_{kt} = \beta PostReform \times Exposure_k + \sum_{y \neq 2003q3} \eta_t 1\{y = t\} \times \mathbf{X'}_k + \tau_k + \tau_{rt} + \alpha + \epsilon_{kt} \quad (3)$$

where the *PostReform*-dummy is equal to one in the post-reform period. Depending on the column, the specification includes municipality-level controls interacted with quarter-year dummies (allowing the coefficients on control variables to vary by time), municipality fixed effects ($\tau_k$), and region-time fixed effects $\tau_{rt}$. The table divides the post-reform period into an early period (2003Q4-2006Q4) and a later period (2007Q1-2010Q4), as the boom-bust pattern otherwise cancels out the impact of the policy. The omitted period is between 1998Q1 and 2003Q2. In appendix B, we consider several alternate specifications to assess the robustness of the estimates in Table 2. These robustness checks use municipality income and unemployment changes instead of levels, add the continuous treatment as a control, exclude municipality fixed effects, and define treatment as a dummy equal to one when *Exposure* is above its median. The results match our main findings.

The difference-in-differences coefficient of interest, $\beta$, measures the difference in annualized quarterly house price growth across the high and low *Exposure* municipalities during the post-reform

[13]The fee for a traditional fixed-rate mortgage with amortization payments was 0.53 percent in 2009, increasing to 0.68 percent in 2016. From 2004 to 2009, fees were fixed. The fee for a fixed-rate IO mortgage was again 0.53 percent in 2009 but increased to 0.95 percent in 2016. Fees were unchanged between 2004 and 2009.

**Table 2:** The effect of IO mortgages on house price growth

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **a) Early post reform (2003Q3-2006Q4)** | | | | | |
| Exposure × Post-reform | 0.0234*** | 0.0210*** | 0.0240*** | 0.0229*** | 0.0154** |
| | (0.0018) | (0.0023) | (0.0036) | (0.0040) | (0.0077) |
| Exposure × Long-run Mortgage rate | | | | | -0.4478 |
| | | | | | (0.3221) |
| Municipality fixed effects | - | Yes | Yes | Yes | Yes |
| Region × year-quarter fixed effects | - | - | Yes | Yes | Yes |
| Municipality-level controls × year-quarter | - | - | - | Yes | Yes |
| Observations | 3348 | 3348 | 3348 | 3348 | 3348 |
| Adjusted $R^2$ | 0.231 | 0.325 | 0.706 | 0.708 | 0.709 |
| **b) Late post reform (2007Q1-2010Q4)** | | | | | |
| Exposure × Post-reform | -0.0297*** | -0.0317*** | -0.0099** | -0.0139*** | -0.0328*** |
| | (0.0034) | (0.0038) | (0.0043) | (0.0029) | (0.0037) |
| Exposure × Long-run Mortgage rate | | | | | -1.6849*** |
| | | | | | (0.2824) |
| Municipality fixed effects | - | Yes | Yes | Yes | Yes |
| Region × year-quarter fixed effects | - | - | Yes | Yes | Yes |
| Municipality-level controls × year-quarter | - | - | - | Yes | Yes |
| Observations | 3627 | 3627 | 3627 | 3627 | 3627 |
| Adjusted $R^2$ | 0.265 | 0.278 | 0.755 | 0.757 | 0.762 |

*Notes:* This table presents estimates of equation 3. Panel a) presents results from the early post-reform period (2003Q3-2006Q4), and panel b) displays results from the late post-reform period (2007Q1-2010Q4). Municipality level controls include the income level in 1998 and the unemployment rate in 2000. Region × year-quarter fixed effects are interactions between region and quarterly dummies. Robust standard errors clustered at the municipality level are in parenthesis. Regressions are weighted by the number of transactions in 2004Q1 (the first quarter where transaction data are available). The long mortgage rate is the mortgage rate on long-run mortgages provided by Finans Danmark. The weights are calculated using the share of variable-rate mortgages with the same data as in figure A1. One, two, and three asterisks correspond to statistical significance at the 10, 5, and 1 percent levels, respectively.

period, relative to this same difference during the pre-reform period. Panel a) of Table 2 presents results for the early post-reform period (2003Q4-2006Q4). The estimate of 0.0234 in column 1 shows that a one-standard-deviation increase in *Exposure* in the early post-reform period led to 2.34 percent higher quarterly year-over-year house price growth during the treatment period from 2003Q3-2006Q4. The coefficient is robust to controls for time-invariant municipality-level fixed effects (column 2), regional and temporal shocks through region × time fixed effects (column 3), and municipality-level for income, unemployment, and income growth between 2002 and 2006 (column 4). In our preferred specification in column 4, with both municipality-level macro controls and region-time fixed effects, a one standard deviation increase in *Exposure* during the post-reform period corresponds to a 2.29 percent increase in quarterly year-over-year house price growth during the 2003Q3-2006Q4 period.

In column 5, we are concerned with the downward trend in interest rates over the early course of the boom. Moreover, as the marginal buyer in higher *Exposure* areas is more exposed to interest rate changes due to the higher debt required to buy a fully-levered property, we are also concerned that mortgage rates may have a larger impact in high-exposure areas. We, therefore, control for the mortgage interest rate interacted with exposure. Note that the region × time dummies in the regressions are co-linear with the mortgage rate. The coefficient of interest remains highly statistically and economically significant.

Panel b) of Table 2 presents results for the late post-reform period (2007Q1-2010Q4). There is a reversal in house price growth in this period: higher *Exposure* municipalities experience lower relative house price growth in the late post-reform period.

What do these estimates imply about the aggregate impact of the reform on house prices? Following Mian & Sufi (2012) and Berger *et al.* (2020), we use the cross-sectional difference in *Exposure* and the estimates from Table 2 to compute an estimate of the reform's aggregate impact during the 2003-2006 boom relative to the least affected municipalities. We divide all municipalities into groups based on *Exposure*, choose the bottom group as the counterfactual, and compute the reform's effect relative to this group. Specifically, for each *Exposure* group $g$, we calculate the impact of the reform as a function of its (normalized) *Exposure* times the coefficient estimate in column 3 of Table 2:

$$(Exposure_g - Exposure_1) * \hat{\beta} = \text{Total Reform Impact on HP Growth}_g, \tag{4}$$

where *Total Reform Impact on HP Growth*$_g$ is the change in house price growth induced by the introduction of IO mortgages for group $g$, $\hat{\beta}$ is the coefficient estimate on $Exposure \times PostReform$ from column 3 in panel a) of Table 2, and $Exposure_g$ is the mean *Exposure* of municipalities in quintile group $g$ weighted by the number of transactions in 2004Q1. $Exposure_1$ represents the lowest *Exposure* group. We calculate equation 4 for each group and provide the results in Table 3, row 1. Each column in the table corresponds to a quintile group of municipalities based on their *Exposure*. The group in column 1 has the lowest *Exposure*, and the group in column 5 has the highest *Exposure*.

The results from the calculation in equation 4 are presented in the first row of Table 3, titled *"1. Total Reform Impact on HP Growth$_g$ (from Eqn (3)); log points."* The impact for the first group is zero by construction (column 1, row 1). For the highest *Exposure* group (column 5, row 1), the estimated impact of the IO loan introduction, relative to the lowest *Exposure* group, is 0.06 log points from 2003Q3 to 2006Q4.

The second row of Table 3, titled, "*2. Early Post-Reform Raw Mean House Price Growth less g=1; log points*", shows the raw average quarterly house price growth rate for the early post-reform period (2003Q3 to 2006Q4) less this same house price growth rate for $g = 1$. Specifically, this calculation subtracts the quarterly house price growth for the group with the lowest *Exposure* from each group's quarterly house price growth. The difference is again zero for the first group by construction. For the remaining groups, quarterly house price growth increases with *Exposure*. For the group with the highest *Exposure*, quarterly house price growth was 0.09 log points higher on average from 2003Q4-2006Q4 than quarterly house price growth for the municipalities in the lowest *Exposure* group.

To understand the degree to which the introduction of IO mortgages explains the housing boom, the third row, *"3. Share of Raw Mean House Price Growth Explained by reform,"* divides the first row by the second row for each group. For the highest *Exposure* group, the reform explains 67 percent of the post-reform house price growth not already accounted for by the lowest *Exposure group*. Next, row 4 of Table 3 indicates that the impact of the reform on house price growth for *Exposure* groups $g = 2, \ldots, 5$ was on average 0.03 log points higher than that for the lowest *Exposure* group, $g = 1$, during the early reform treatment period. Finally, Table 3, row 5 averages columns 2–5 from row 3 and documents that the IO mortgage reform explains 55 percent of the Danish house price growth from 2003Q3-2006Q4 not accounted for by changes in the lowest *Exposure*, control group. These results are large and economically meaningful.

Overall, the results show that introducing IO mortgages caused an initial increase in Danish house prices, followed by a reversion in 2007. The rise in house prices during the boom was substantial – the overall price increases in the Danish housing market exceeded the increases in the United States during the same period (Figure 1). The decline in prices that followed is also similar in magnitude to the United States. Finally, note that the fall in house price growth and eventual adverse price developments began before the financial crisis that roiled international financial markets, suggesting that there would have been a notable house price correction even in the absence of the financial crisis.

**Table 3:** Aggregate impact of reform

| | Group based on ex-ante Exposure | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| 1. Total Reform Impact on HP Growth$_g$(from Eqn (3)); log points | 0.00 | 0.01 | 0.02 | 0.04 | 0.06 |
| 2. Early Post-Reform Raw Mean HP Growth less g=1; log points | 0.00 | 0.03 | 0.03 | 0.07 | 0.09 |
| 3. Share of Raw Mean HP Growth Explained by Reform (Row 1 / Row 2) | 0.00 | 0.39 | 0.59 | 0.56 | 0.67 |
| **Average impact across exposure groups 2-5** | | | | | |
| 4. Average Impact of Reform on HP Growth$_g$ (mean row 1, columns 2-5) | | | | | 0.03 |
| 5. Mean Share of Raw Mean HP Growth Explained by Reform (mean row 3, columns 2-5) | | | | | 0.55 |

*Notes:* The table presents estimates of the aggregate impact of the reform. We divide the sample into quintiles based on *Exposure*, where *Exposure* equals the square meter price level in 1998 normalized by its standard deviation. When calculating average values for each group, we weight municipalities by the number of transactions in 2004Q1 (the first quarter where transaction data are available). Total Reform Impact on HP Growth$_g$ is calculated according to equation 4 – the impact for each group $g$ is measured as the difference in *Exposure* between each group and the first group multiplied by the coefficient on *Exposure* in column 3 of Table 2. 2. Early Post-Reform Raw Mean HP Growth less g=1 is calculated as the average growth in house prices ($\Delta HP_{kt}$) in the early-post reform period less than the house price growth of group 1. 3. Share of Raw Mean HP Growth Explained by Reform is calculated by dividing 1. Total Reform Impact on HP Growth$_g$ by 2. Early Post-Reform Raw Mean HP Growth less g=1. 4. Average Impact of Reform on HP Growth$_g$ is calculated as the average across 1. Total Reform Impact on HP Growth$_g$ for groups 2-5 and 5. Mean Share of Raw Mean HP Growth Explained by Reform is calculated as the average of 3. Share of Raw Mean HP Growth Explained by Reform for groups 2-5.

## 7 Mechanisms and Alternative Explanations

### 7.1 Mechanisms

Why did the introduction of interest-only mortgages lead to a rapid increase in house prices? The mechanism underpinning the reform's considerable impact stems from a notable reduction in initial payments and a corresponding relaxation of PTI constraints that resulted in an expansion of credit. A common view in Denmark is that IO mortgages led to an expansion of credit supply, as banks conducted credit assessments based on borrowers' ability to pay for the IO mortgage instead of their ability to repay a traditional, 30-year fixed-rate mortgage (Rangvid *et al.*, 2013, p. 126). This represents a relaxation of PTI constraints where amortization payments are a part of the credit affordability assessment. Moreover, borrowers could lower first-year expenses by approximately 20 percent with IO mortgages. Such initial payment reductions could ease debt-service burdens for the marginal borrower or yield portfolio allocation benefits (see section 5 for an overview of the benefits of IO mortgages). Together, these channels imply a causal role for interest-only mortgages – credit assessments eased when a borrower chose the newly introduced IO mortgage product, and higher income borrowers valued lower mortgage payments. As a substantial share of the population valued IO mortgages and such mortgages were readily available, housing demand and thus prices increased.

However, the IO mortgage-induced decline in borrower payments likely does not explain the full impact of the Danish housing boom by itself. Assuming that interest rates and amortization payments have the same effect on house prices (a strong assumption), the semi-elasticities of housing prices with respect to the interest rate presented in Glaeser *et al.* (2012) of between 7 and 20 suggest that the reduction in payments increased house prices by between 13 and 37 percent. However, since the elasticity is likely lower and house prices rose over 50 percent, the numbers above are insufficient to explain the entire course of the boom. Instead, we argue below that the introduction of IO mortgages created incentives for households to use variable rate mortgages and that the early boom in house prices following the reform caused house price expectations to increase. The latter idea is similar to the "diagnostic expectations" of Bordalo *et al.* (2020). These indirect effects of the reform are not captured by the direct effect of lower payments discussed in the paragraph above but are behavioral responses to the initial boom. We now discuss these two ideas in more detail.

Variable-rate mortgages were introduced in Denmark in 1996 (although the data for this loan type is only available from 2003) but became increasingly popular over the housing boom. Figure 7 shows that variable-rate IO mortgages were the main driver of the increased variable-rate share during the boom. Indeed, the share of variable-rate mortgages *with* amortization payments declines substantially during the boom. In addition to the lower amortization payments, variable-rate mortgages also feature a lower (but riskier) interest rate. Thus, the increasing share of variable-rate mortgages amplifies the reduction in payments from the IO mortgage reform. At the time of the reform, a borrower that chose a variable-rate mortgage with an IO amortization schedule could lower mortgage payments by 66 percent compared to an amortizing, fixed-rate mortgage (see Figure A8).

Is the boom explained solely by the availability of variable-rate mortgages? While they likely contribute to the run-up in prices, recall from Figure 6 that our *Exposure* measure does not predict higher house price growth after the introduction of variable rate mortgages in 1996. There is also no change in the trend of aggregate house prices with the introduction of variable-rate mortgages (Figure 1). Thus, the introduction of variable-rate mortgages in 1996 did not have the same effect as the introduction of IO mortgages in 2003. Hence, our interpretation is that IO mortgages started the boom and that IO mortgages led to higher use of variable-rate mortgages. These two products caused a rapid increase in house prices in areas where they were more valuable (see Dam *et al.*, 2011, for the aggregate effects).

The second factor that plausibly contributed to the Danish housing boom is house price expec-

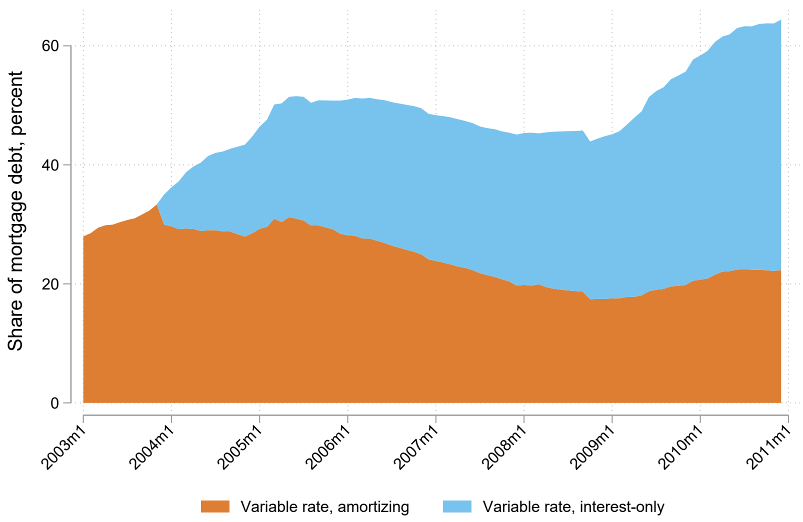



**Figure 7: variable-rate mortgages as a share of outstanding mortgage debt**
*Note:* The figure plots the share of variable-rate mortgages in outstanding mortgage debt for amortizing mortgages (solid-orange line) and IO mortgages (blue dashed line). Source: Finans Danmark.

tations. We view expectations as a mechanism behind the large impact of IO mortgages in high *Exposure* areas. The idea is in line with a "diagnostic bubble" (Bordalo *et al.*, 2020), where a beneficial economic innovation leads to good news about fundamentals and subsequently to high expectations. As households grow more optimistic, house prices increase. Abildgren *et al.* (2018) report evidence consistent with this idea. Consumer confidence became decoupled from economic fundamentals (income, real GDP growth, short-term interest rates, share prices) in 2003 in Denmark. Further, more optimistic households were more likely to purchase real estate than less optimistic households and used more leverage. The rise in optimism coincides with the introduction of IO mortgages and closely tracks house price growth. The Ministry of Economic and Business Affairs (2005) reports that in 2005, six out of ten Danes expected house prices to increase over the next 12 months. None of the survey's 1,000 respondents expected a significant decrease in house prices. House price expectations were also higher in areas with higher treatment exposure, like the capital region and Aarhus, the second largest city in Denmark. Eight out of ten respondents in the Capital region reported that they expected house prices to rise or rise considerably within the next 12 months. In contrast, four out of ten respondents reported that they expected house prices to rise or rise substantially in the rest of Jutland, areas with lower *Exposure*. In general, we do not discount the importance of expectations in driving the housing boom but question the role of expectations in *starting* the boom (see Griffin *et al.* (2020) for similar evidence in the U.S.). We instead see changes in expectations as a consequence of the introduction of IO mortgages and the corresponding relaxation of payment-to-income constraints.

### 7.2 Alternative explanations

Since most potential confounds move slowly, the sharp increase in house price growth after the introduction of IO mortgages, as documented in Figure 1 and Figure 6, represents strong evidence in support of the validity of our empirical design. However, time-varying shocks correlated with *Exposure* still represent a potential threat to identification, and thus we consider several additional alternative explanations for our results. These alternative factors include house price expectations, income growth, changes in non-IO mortgage credit standards, and tax rates. While these factors may be relevant in other contexts or even later in the Danish boom, we conclude that none can explain the rapid Danish house price increases that began in late 2003. We discuss each potential explanation in turn.

*House price expectations and speculation as the start of the housing boom* – We are not aware of any surveys of house price expectations conducted in Denmark before the treatment date (2003Q4). Thus, we follow Glaeser & Nathanson (2017), Brueckner *et al.* (2012), Dell'Ariccia *et al.* (2012) and Case *et al.* (2012) and use lagged house price growth as a proxy for house price expectations. Ma (2020) documents that subjective expectations display momentum that follows house price movements, validating this approach. Recall that there are no statistically significant pre-treatment differences in house price growth trends with increases in *Exposure* (Figure 6), suggesting no corresponding differences in the trends in ex-ante house price expectations.

The assertion of limited speculation is backed up by several *ex-post* outcomes such as forced home sales, non-performing loans, and mortgage defaults. Figure 8 shows that both mortgage arrears as a percentage of outstanding mortgage debt and the number of homes repossessed by mortgage banks remained low throughout the housing market downturn and the financial crisis.[14] Mortgage arrears peaked at 0.6 percent in Denmark (Association of Danish Mortgage Banks, 2016), a rate at which the mortgage banks covered themselves with no required government intervention. In comparison, delinquency rates in the U.S. on single-family residential mortgages peaked at 10 percent, and non-performing loans to gross loans peaked at slightly

[14]The mortgage arrears plot shows the percentage of loans where a large share of total payments have not been met 3.5 months after the latest due date.

below 5 percent (Board of Governors of the Federal Reserve System (US), 2016; World Bank, 2016). Similarly, forced sales were limited in Denmark, given the considerable drop in house prices. The blue dashed line in Figure 8, representing forced sales, peaks at a little above 600 per quarter, and even that peak is short-lived. Using individual-level data on borrowers, Larsen *et al.* (forthcoming) state that "[i]n spite of higher debt levels, debt-to-asset ratio, and loan-to-income ratio, IO borrowers in our sample did not default with a significantly higher frequency than repayment borrowers during the financial crisis." It is important to note that there is no incentive to strategically default to reduce high debt levels in Denmark. Indeed, given that the property will be sold and any remaining debt converted to higher interest, unsecured credit, the incentive for the borrower is often to keep making payments as long as possible.

Full recourse mortgages also change the incentives for the mortgage banks who issue loans and investors who purchase mortgage loans. Amortization payments and the associated reduction in leverage is a crucial component for default probabilities and loss given default in countries with no recourse mortgages (see Garmaise, 2013; Gerardi *et al.*, 2018), but less so in the case of full recourse mortgages. In Denmark, the bank can sell the property and garnish incomes until the full debt is repaid. The bank can therefore be reasonably certain that they will receive the full value of the loan. For the investor, the case is even more apparent – since the bank is assuming any default risk, the investor faces little risk of losing money due to default. A remarkable statistic is that no mortgage bonds have defaulted in Danish history (Andersen *et al.*, 2020). Lower amortization payments on behalf of borrowers, therefore, present little or no increase in risk for Danish mortgage banks and instead provide higher interest payments over the loan's lifetime.

*Mortgage interest rates* – Figure A2 in Appendix A plots the long (solid line) and short (dashed line) mortgage rates. The figure shows that mortgage rates gradually declined in mid-2000, making it unlikely that the decline in the mortgage rate started the boom. The long rate declined from 5.49 percent in 2003Q3 to 4.25 percent in 2005Q3 but increased to 5.33 percent in 2006Q2. Glaeser *et al.* (2012) argue that in practice and theory, a 100 basis point fall in the interest rate is associated with a 7 percent rise in house prices. The 124 basis point fall in interest rates from 2003Q3 to the lowest interest rate in 2005Q3 would imply an increase in *national* house prices by 8.68 percent. Lower interest rates cannot account for the large increase in house prices over this period. Alternatively, we use the variable-rate share to calculate an

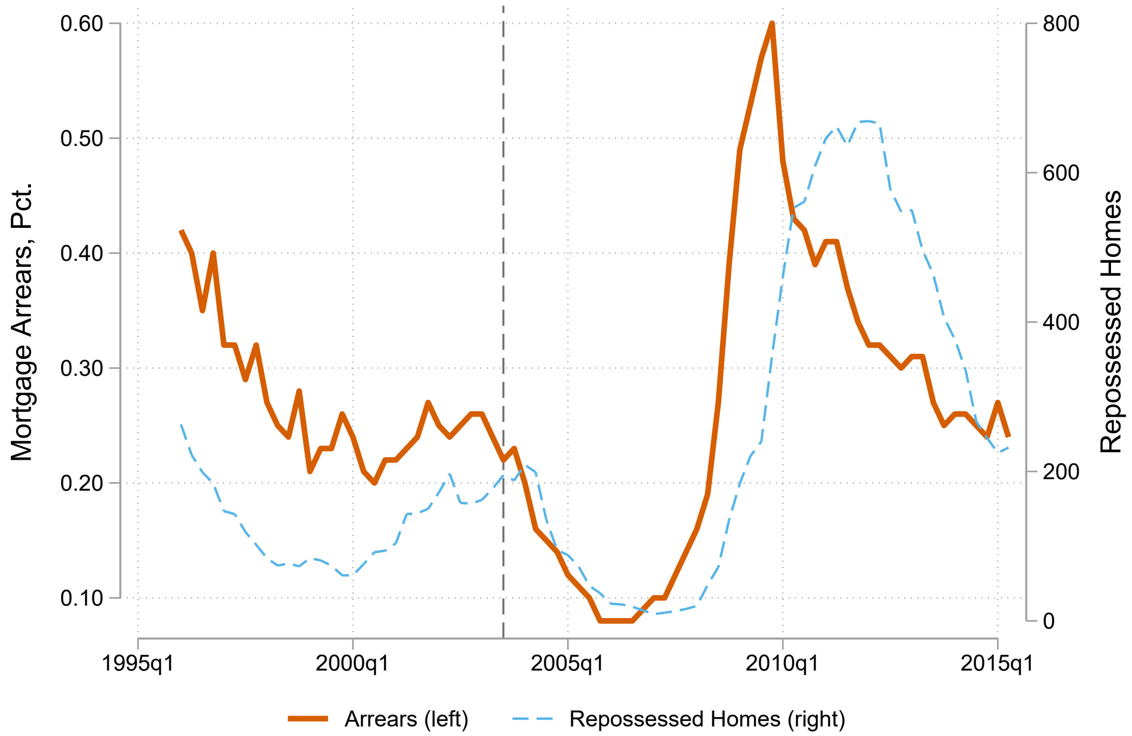



**Figure 8: Credit standards in Denmark**

*Notes:* The figure plots mortgage arrears (left axis) and repossessed homes (right axis) in Denmark over time. The vertical dashed line marks the introduction of IO mortgages. All data from Finans Danmark.

effective interest rate. The decline in the effective interest rate from 2003Q3 to the lowest point is 1.25 percent. Using the elasticity from Glaeser *et al.* (2012), this decline would lead to an 8.75 percent increase in house prices. Again, this is unlikely to explain the rise in Danish house prices over this period, especially considering that mortgage interest rates increased starting in 2005, well before the boom ended. Moreover, our empirical strategy differences out any aggregate changes in interest rates and controls for region-year fixed effects that account for any time-varying regional changes in interest rates. The combination of the empirical framework and these included controls makes it unlikely that lower interest rates can fully explain the house price boom in Denmark. Furthermore, our results are robust to including controls for the mortgage interest rate.

An alternative illustration of the effect of lower interest rates is estimating the impact of *Exposure* on the average interest rate borrowers pay in each municipality. While we lack data on the specifics of the mortgage debt before 2009, we can impute the mortgage rate by dividing mortgage interest payments by mortgage size. The imputed mortgage rate tracks the aggregate

a) *Exposure* and imputed mortgage rates    b) Imputed mortgage rate for treated and untreated

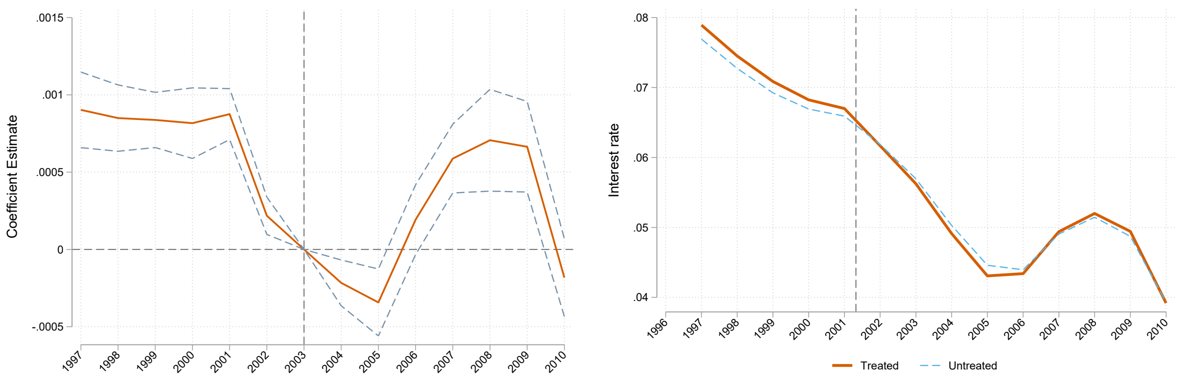



**Figure 9: Imputed mortgage rate and Exposure**
*Note:* Panel a) plots the difference-in-differences coefficients, $\sum_{y\neq 2003q1} \beta_t 1\{y=t\} \times Exposure_k$ from Equation 5. The dependent variable is the average imputed mortgage interest rate by municipality. The dashed lines show robust standard errors at the 1st and 99th percentile, clustered by municipality. Observations are weighted by the number of transactions in 2004Q1. Panel b) plots the average imputed mortgage interest rate for two groups based on *Exposure*: the Treated group has a value of *Exposure* above the median. The Untreated group has a value of *Exposure* less than or equal to the median value.

mortgage rate well, showing the same decline from 1996 to 2005 and the subsequent increase. When we estimate our baseline regression using interest rates as the dependent variable, we see in panel a) of Figure 9 that *Exposure* predicts a higher interest rate before the reform, followed by a lower interest rate in the immediate aftermath of the reform. At first glance, this suggests that different interest rate dynamics based on *Exposure* can help explain the boom. Note, however, that the coefficient is not economically significant: the coefficient of 0.01 is an order of magnitude smaller than the estimate in Figure 6. Panel b) divides the sample into high- and low *Exposure* groups, showing that the average difference between the two groups is minor.

*Income Growth and Macroeconomic Shocks* – Another plausible explanation for the origin of the boom in house prices is that income growth or expectations of higher income growth could cause an increase in demand. Since IO mortgages are valuable to households with rising incomes (Cocco, 2013), we may observe a relationship between IO mortgage share and house price growth due to omitted income expectations.

We proceed in several steps to evaluate this hypothesis. First, we estimate a similar dynamic regression as in equation 2 but use the change in income as the dependent variable.

$$\Delta Income_{kt} = \sum_{y\neq 2003q3} \beta_t 1\{y=t\} \times Exposure_k \\ +\tau_k + \tau_{rt} + \alpha + \epsilon_{kt} \tag{5}$$

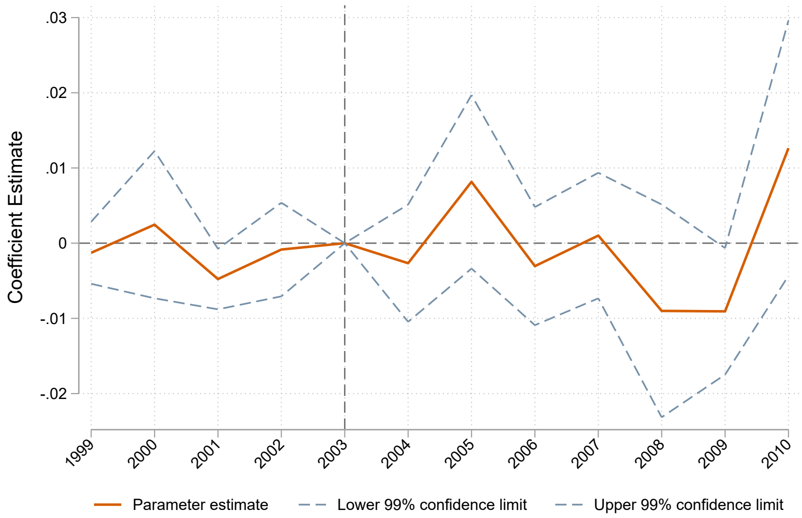



**Figure 10: Income Growth and Exposure**

*Notes:* The figure plots the difference-in-differences coefficients, $\sum_{y \neq 2003q1} \beta_t 1\{y = t\} \times Exposure_k$ from Equation 5. The dashed lines show robust standard errors at the 1st and 99th percentile, clustered by municipality. Observations are weighted by the number of transactions in 2004Q1.

This equation is equivalent to our main specification, where income growth replaces house price growth, and we do not include any control variables. Figure 10, panel a) plots the difference-in-differences coefficients and shows little correlation between income growth trends and *Exposure* before or after the reform. This test is similar to the one conducted in Barlevy & Fisher (2021) at the city-level and in Cocco (2013) at the individual level. The coefficients are statistically insignificant in the years before and after the reform. Figure A9 in Appendix A shows that similar results also hold for the income growth of property owners, unemployment, total employment, and population growth. There is little evidence that income growth or labor market shocks can explain the start of the boom.

*Changes in non-IO Credit Standards* – While interest-only mortgages represent a change in the type of mortgages available, other aspects of credit standards were unchanged by regulatory design over this period. For example, mortgage banks are legally obliged to retain credit risk and cannot sell this risk to investors.[15] The strict LTV limit of 80 percent was enforced throughout the sample period and did not change during the boom. Further, households appeared to understand the consequences of IO loans: in a 2011 survey of households with IO mortgages, 89 percent reported that they were "very well informed" or "well informed" on the implications of choosing IO mortgages (Association of Danish Mortgage Credit Banks, 2011).

[15] Also, there is no government intervention in the Danish mortgage market and no government insurance of mortgages, nullifying any concerns of government interference in the pricing of mortgages.

*Tax Rates* – Denmark introduced a freeze on nominal property taxes in 2002. The freeze fixed property taxes at 2002 levels; hence, property taxes did not increase as home values grew. This reform removes the stabilizing effect of higher taxes (in nominal terms) on house prices as it lowers the effective tax rate on properties when house prices rise. However, the announcement of the tax freeze occurred before 2002, meaning that it should have affected prices before the reform. Although it is difficult to argue that higher housing taxes would not have led to lower house price growth, the timing suggests that the tax freeze did not cause the boom. However, the counterfactual to the introduction of IO mortgages is not that there is no tax freeze and no IO mortgages. Instead, the counterfactual exercise for this paper is no IO mortgages and a tax freeze.

*Speculation by investors* – Above, we have argued that IO mortgages were valuable for many Danes, leading to rising prices. An alternative is that a small group of speculators started the Danish housing boom. The "speculator-induced boom" hypothesis posits that lower amortization payments allow households to speculate on rising prices while maintaining minimal home equity stakes using IO mortgages (Barlevy & Fisher, 2021). In the United States, Haughwout *et al.* (2011) estimate that 40-50 percent of housing purchases in such states were investment properties. Bayer *et al.* (2020) document two types of speculators. The first type acts as middlemen who purchase below-market prices and resell above throughout the cycle. The second type enters the market as speculators during the housing boom, buying and selling at market prices. Interest-only mortgages could facilitate speculation during times of high house price expectations (particularly by the second type defined in Bayer *et al.* (2020)), thereby allowing speculators to pay more for housing without changing initial debt-service payments. Note that this still implies a causal effect of introducing IO mortgages on house prices, but the mechanism is different.

There are, however, reasons to be skeptical of the "speculator-induced boom" argument in Denmark. Using ownership registers that link each individual to properties, we find that the share of individuals who own multiple properties changes little from 14.4 percent in 2003 to 14.9 percent in 2005. In addition, there was nearly no change in the share of out-of-town purchases, which only increased slightly from 8.7 percent in 2003 to 10 percent in 2005. None of the above numbers are indicative of heightened speculative activity. Indeed, the Danish institutional framework with full recourse mortgages makes this channel less applicable.

## 8 Conclusion

In this paper, we find the introduction of interest-only mortgages induced a wide-scale expansion in credit that ignited the Danish housing boom, leading to a run-up in house prices comparable to the U.S. during the 2000s. Fundamentally, our key contribution is that a housing boom and bust can occur with an ex-ante minor mortgage market change (e.g., the introduction of IO mortgages), even within a well-regulated, often praised mortgage finance system (Campbell, 2013). Our results show that a single and seemingly small country-level mortgage finance innovation can lead to sizable housing demand responses across local markets, in our case, varying with the ex-ante price level. The underlying mechanism driving our results thus departs from narratives of an aggregate housing demand or housing speculation shock having differing impacts depending on housing supply elasticities. Instead, as amortization payments are naturally a larger share of income in high-priced areas, an IO mortgage-induced credit expansion creates non-uniform changes in housing demand with outsized increases in regions where borrowers find these mortgages more valuable. This dynamic subsequently yields pronounced differences in local house price growth.

The Danish experience shows how credit is not merely about interest rates or borrower credit risk but is about other terms in the mortgage contract. Although an extensive literature documents that a change in interest rates or credit availability has significant effects on house prices, the role of changes in amortization payments for house prices has been largely neglected. And while a growing literature emphasizes the role of new mortgage products with lower initial repayments during the 2000s U.S. housing boom, the U.S. lacks a clean policy experiment to estimate the impact of IO mortgages on home prices (Foote *et al.*, 2012) and has a regulatory framework that differs widely from other countries where IO mortgages were prevalent. Nonetheless, several studies show that IO mortgages became extremely popular in the U.S. during the 2000s: Amromin *et al.* (2018) find that the share mortgage products with lower initial payments jumped from 2 percent of origination volume in 2003 to nearly 30 percent in 2005. Thus, understanding the causal impacts of IO mortgages is crucial for deciphering the causes of the 2000s U.S. and international run-up in house prices that led to the Great Recession. This paper shows that mortgage innovations, like IO loans, can lead to credit expansions and create a house price boom.

Within the broader housing literature, our results also show that overly optimistic expectations or speculation are not necessary conditions for starting housing booms. However, these factors may contribute to house price growth after the boom has started or may be relevant for creating a housing boom in other contexts. Our interpretation of the Danish experience is that a financial innovation shock led to rising house prices, which fueled house price expectations and the use of other, existing, alternative mortgage products. The implications are that policymakers should know how new mortgage products, like IO loans, can drive housing cycles even without more traditional factors.

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**Online Appendix: “Mortgage Innovation and House Price Booms”**

# A Online Appendix: Figures

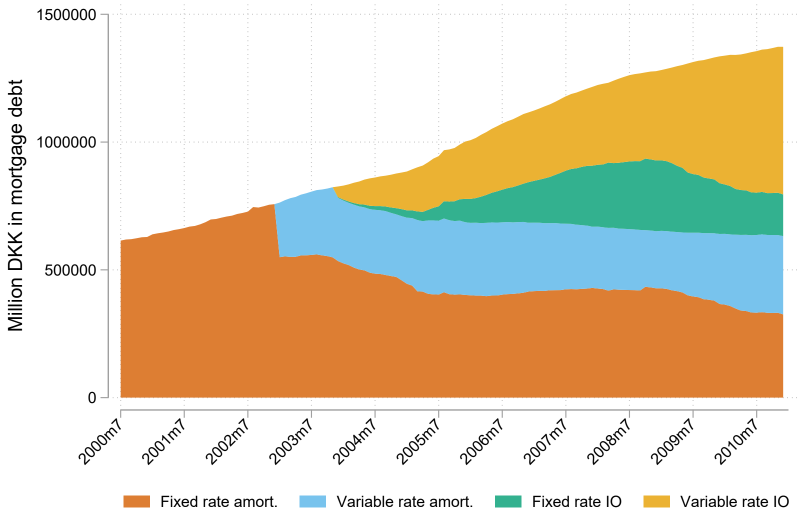



**Figure A1: Stock and flow of outstanding mortgage debt**
*Note:* The figure plots outstanding mortgage debt by loan type, including loans for residential properties and vacation homes. Source: Nationalbanken.

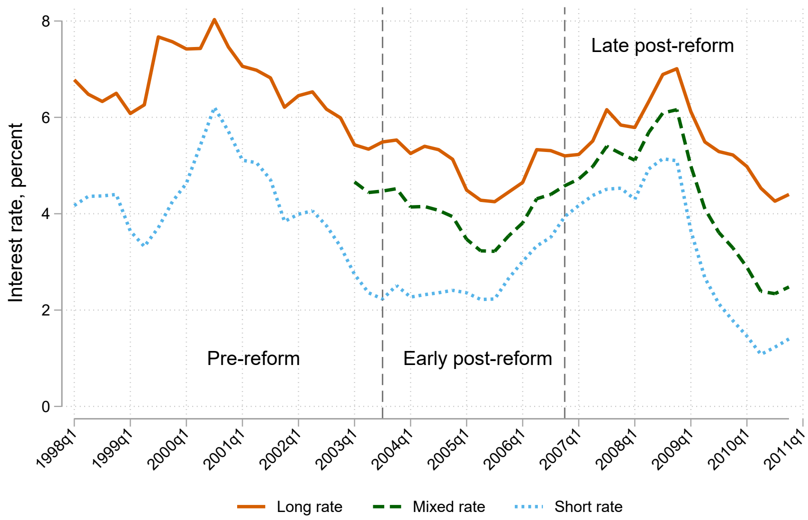



**Figure A2: Short and long run mortgage rates**
*Note:* The figure plots the effective long and short mortgage interest rate as well as the weighted average of the effective short and long mortgage interest rates for loans in Danish krona between 1998 and 2010. The weights are calculated using the share of variable rate mortgages with the same data as in figure A1. Weights are available from 2003 only. The dashed vertical line marks the introduction of IO mortgages in 2003. Source: Finans Danmark, Nationalbanken and authors' caluclations .


Correlation: 0.81
Coef.: 0.003, t-stat: 13.21, R-squared: 0.66

**Figure A3: Municipality-level ex-ante Exposure and 2009 IO mortgage share**

*Notes:* The figure plots the average square meter price in 1998 against the IO mortgage share in 2009 for each municipality in Denmark. The IO mortgage share is calculated for 2009 by collapsing individual, borrower level data to the municipality level. The main IO mortgage data-set covers all Danish mortgages and includes information about the location of the property used as collateral in the mortgage. The coefficient, $t$-statistic and R-squared are from a regression of the form: $IO_k = \alpha + \beta SquareMeterPrice_k + \epsilon_k$, where $IO_k$ and $SquareMeterPrice_k$ denotes the IO share and square meter price in 1998 for municipality $k$.

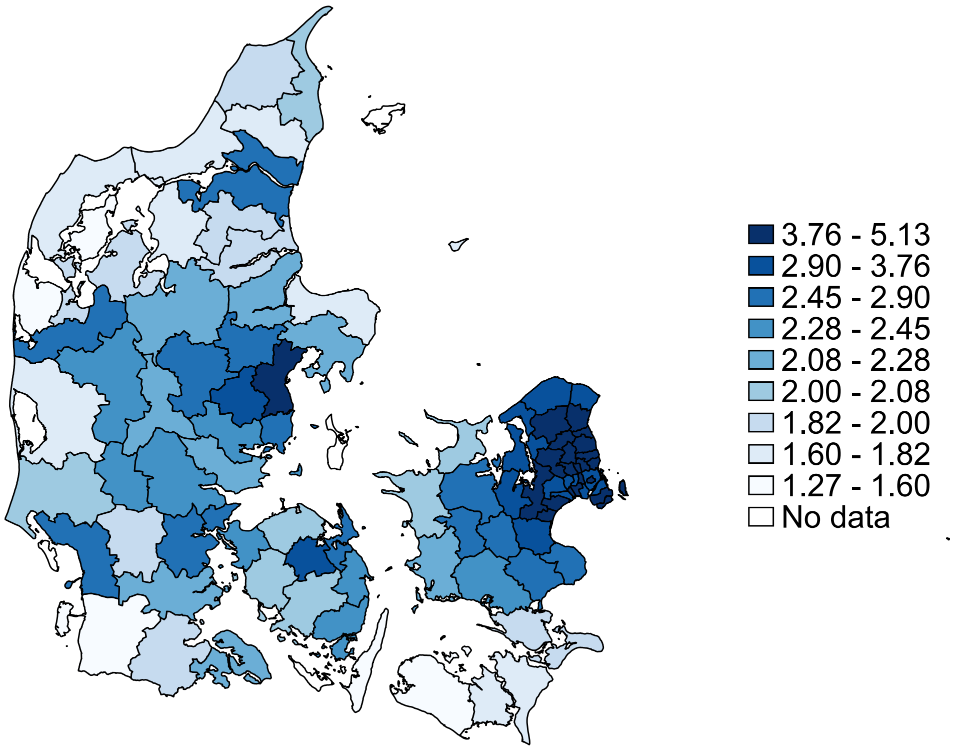



**Figure A4: Exposure across Denmark**

*Notes:* The figure plots values for Exposure for each municipality in Denmark.

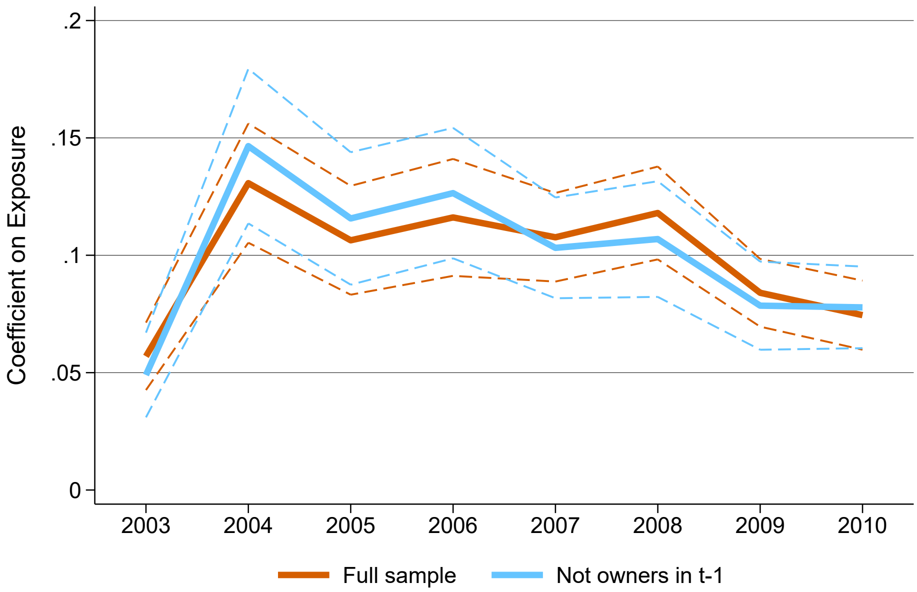



**Figure A5: Year-over-year coefficient on Exposure using transaction-level data**

*Notes:* The figure plots the coefficient on $Exposure_k$ from a regression of the form $InterestOnly_{it} = \alpha + \sum_{t=2003}^{2010} \beta_t 1\{z=t\} \times Exposure_k + \mathbf{X'}_{ti}\gamma + \epsilon_{kt}$. The sample consists of buyers in each year (orange line) and buyers who were not owners in the previous year (blue line). 95 percent confidence intervals shows by dashed lines. Controls include family size, number of children, a dummy equal to one if the buyer is retired, the employment ratio, gender, a dummy equal to one if the buyer was a property owner in the previous year, the log liquid wealth, and log total income as well as the lagged and future value of log total income.


**Figure A6: Year-over-year coefficient on Exposure using muncipality-level data**

*Notes:* The figure plots the coefficient on $Exposure_k$ from a regression of the form $InterestOnly_{kt} = \alpha + \sum_{t=2003}^{2010} \beta_t 1\{z = t\} \times Exposure_k + \mathbf{X'}_{tk}\gamma + \epsilon_{kt}$. We aggregate transactions to the $municipality \times year$ level. 95 percent confidence intervals shows by dashed lines. Controls include log liquid wealth, and log total income. Standard errors are clustered on the municipality-level.

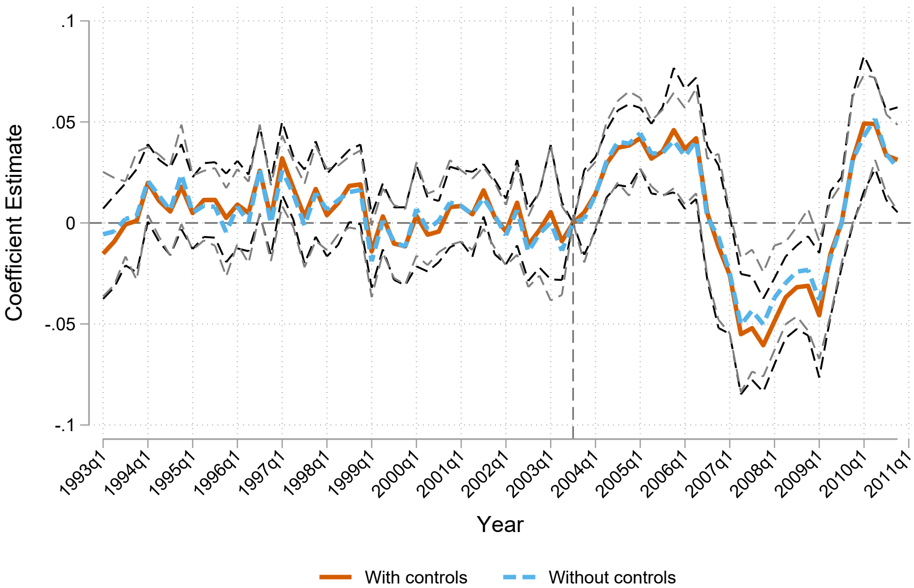



**Figure A7: The effect of IO mortgages on house price growth**

*Notes:* The figure plots the difference-in-differences coefficients, $\beta_t$, from Equation 2 for a specification with and without controls. Control variables include the municipality average income in 1998 and the municipality unemployment rate in 2002. All controls are interacted with quarterly dummies. Both specifications include municipality and region-time fixed effects. Observations are weighted by the number of transactions in 2004Q1, and robust standard errors are clustered by municipality.

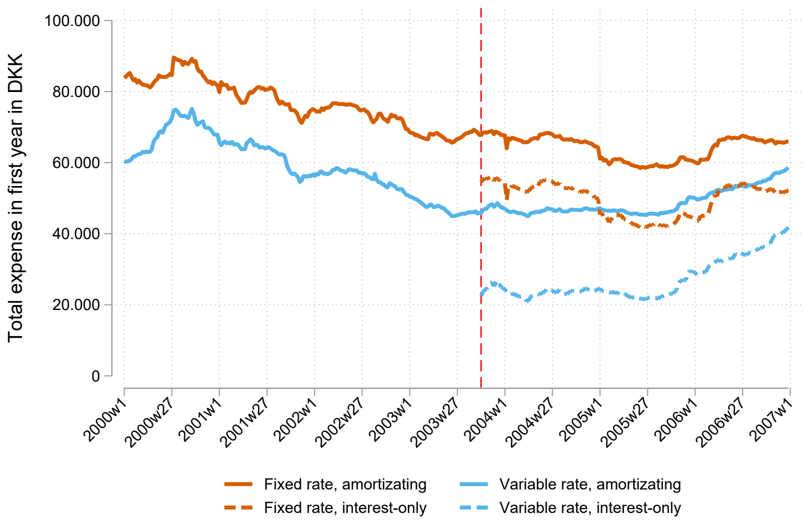
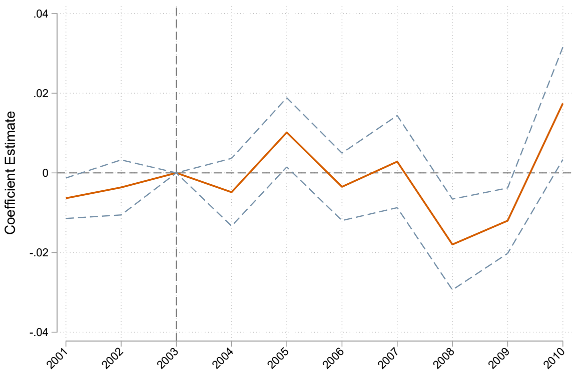
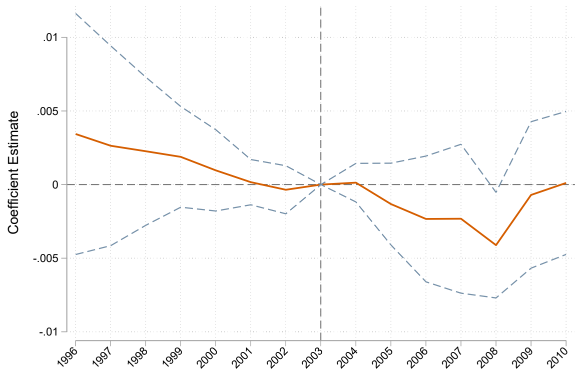



**Figure A8: First Year Payments for different mortgage products**

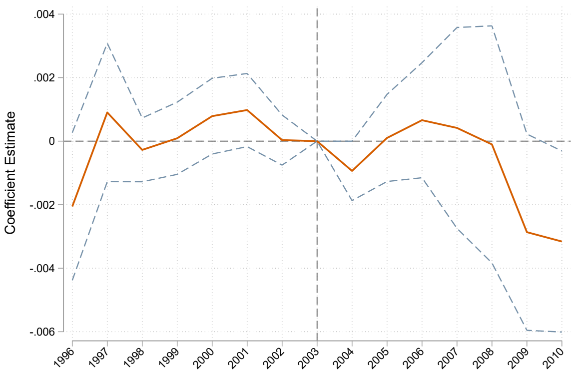
*Notes:* The dashed vertical line indicates the introduction of interest-only mortgages. The figure plots the total first-year expense for a 1 million Danish Krone (DKK) for different loan types. Fixed rate mortgages are plotted in orange and variable rate mortgages are plotted in blue. IO mortgages are marked with dashed lines. All calculations use the long- and short-bond rate from the Association of Danish Mortgage Banks. Source: Association of Danish Mortgage Banks and authors' calculations.



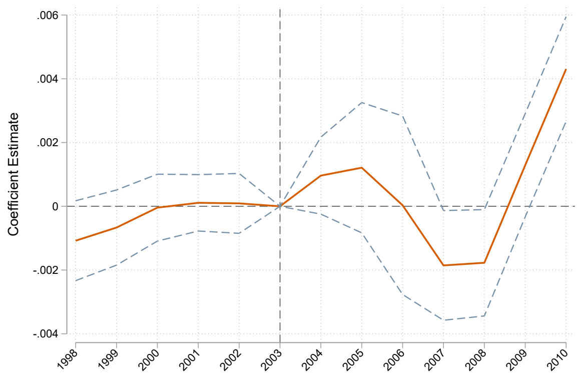

a) Exposure and income growth for house owners




b) Exposure and employment




c) Exposure and unemployment rates




d) Exposure and the first difference of log total population

**Figure A9: Exposure and labor market outcomes**

*Notes:* All panels plot the difference-in-difference coefficient $\sum_{y \neq 2003q1} \beta_t 1\{y = t\} \times Exposure_k$ from regressions similar to Equation 5. The dependent variable in each is listed under the panel.

# B Online Appendix: Tables

**Table B1:** Mortgage payments with and without amortization payments

| | Interest rate | | | | |
|---|---|---|---|---|---|
| | 1% | 1.5% | 3% | 5% | 10% |
| **Payments under each schedule** | | | | | |
| Annuity schedule | 38,597 | 41,414 | 50,592 | 64,419 | 105,309 |
| Interest-only mortgage | 10,000 | 15,000 | 30,000 | 50,000 | 100,000 |
| **Reduction in payments (%)** | | | | | |
| (IO - Annuity) / Annuity | -74 | -64 | -41 | -22 | -5 |

*Notes:* The table reports mortgage payments in the first year under different interest rates and repayment schedules. We calculate mortgage payments for a 1,000,000 mortgage, using the annual interest rate in the top row. All calculations assume that payments are made monthly. *Annuity schedule* is calculated using an annuity formula where the payments are the same in every period. For the annuity schedule the contract term is assumed to be 30 years. *Interest-only mortgage* is calculated as the mortgage amount times the effective annual interest rate. The last row reports the reduction in payments in percent, calculated as the percent reduction in total mortgage payments from choosing an IO mortgage.

**Table B2:** Summary Statistics

| | Mean | Median | Std. dev. | Min | Max |
|---|---|---|---|---|---|
| **Housing market statistics** | | | | | |
| House prices (Sq.m) | 8,852 | 7,475 | 3,635 | 3,618 | 16,812 |
| Apartment prices (Sq.m) | 10,268 | 9,153 | 3,667 | 3,275 | 17,764 |
| Property price | 9,068 | 7,724 | 3,721 | 3,794 | 16,801 |
| House price (Sq.m) in 1998 | 7,506 | 6,789 | 2,468 | 3,805 | 12,751 |
| Housing transactions as a share of total | 0.80 | 0.87 | 0.19 | 0.05 | 0.97 |
| Interest-only mortgage share | 57.14 | 58.00 | 8.09 | 37.00 | 76.00 |
| **Income and labor market** | | | | | |
| Income | 236,750 | 224,939 | 35,275 | 170,623 | 369,403 |
| Income for owners | 308,840 | 291,492 | 52,451 | 244,845 | 521,985 |
| Income for renters | 160,655 | 154,947 | 16,058 | 136,438 | 217,462 |
| Income growth 1998-2002 | 0.15 | 0.14 | 0.02 | 0.11 | 0.26 |
| Income growth 2000-2002 | 0.08 | 0.08 | 0.01 | 0.03 | 0.15 |
| Income growth for owners 2000-2002 | 0.07 | 0.07 | 0.02 | 0.02 | 0.17 |
| Income growth for renters 2000-2002 | 0.08 | 0.08 | 0.01 | 0.04 | 0.11 |
| Unemployment | 4.40 | 4.33 | 1.13 | 2.33 | 7.53 |

*Notes:* Summary Statistics for Danish Municipalities.

**Table B3:** Alternative specification for table 2

| | No mun. FE, cont. treat | | | Additional controls | |
|---|---|---|---|---|---|
| | (1)<br>Base | (2)<br>Region-Time FE | (3)<br>FE.+ controls | (4)<br>Changes | (5)<br>Abs + changes |
| Exposure × Post-reform | 0.018***<br>(0.002) | 0.021***<br>(0.003) | 0.021***<br>(0.003) | 0.019***<br>(0.003) | 0.020***<br>(0.003) |
| Exposure | 0.013***<br>(0.002) | 0.004***<br>(0.001) | 0.005***<br>(0.001) | 0.005***<br>(0.001) | 0.006***<br>(0.001) |
| Post reform=1 | 0.013*<br>(0.007) | | | | |
| Post reform=0 × Income in 1998 | | | -0.000***<br>(0.000) | | -0.000***<br>(0.000) |
| Post reform=1 × Income in 1998 | | | -0.000<br>(0.000) | | -0.000<br>(0.000) |
| Post reform=0 × Unemployment in 2000 | | | -0.003***<br>(0.001) | | -0.002**<br>(0.001) |
| Post reform=1 × Unemployment in 2000 | | | 0.003*<br>(0.001) | | 0.003*<br>(0.002) |
| Post reform=0 × income growth 98-02 | | | | 0.094***<br>(0.035) | 0.071**<br>(0.034) |
| Post reform=1 × income growth 98-02 | | | | 0.193***<br>(0.059) | 0.189***<br>(0.069) |
| Post reform=0 × Unemployment change 00-02 | | | | 0.004**<br>(0.002) | 0.003*<br>(0.002) |
| Post reform=1 × Unemployment change 00-02 | | | | -0.002<br>(0.004) | 0.002<br>(0.005) |
| Region-time fixed effects | No | Yes | Yes | Yes | Yes |
| Observations | 5208 | 5208 | 5208 | 5208 | 5208 |
| Adjusted $R^2$ | 0.214 | 0.232 | 0.233 | 0.233 | 0.233 |

*Notes:* See the notes in Table 2 for time periods and variable definitions.

**Table B4:** Alternative specification for table 2
Dummy for treatment status (above median Exposure)

| | No mun. FE, cont. treat | | | Additional controls | |
|---|---|---|---|---|---|
| | (1)<br>Base | (2)<br>Region-Time FE | (3)<br>FE.+ controls | (4)<br>Changes | (5)<br>Abs + changes |
| High exposure=1 × Post reform=1 | 0.028***<br>(0.006) | 0.021***<br>(0.006) | 0.021***<br>(0.006) | 0.017***<br>(0.005) | 0.020***<br>(0.005) |
| High exposure=1 | 0.023***<br>(0.005) | 0.008***<br>(0.002) | 0.008***<br>(0.002) | 0.010***<br>(0.002) | 0.009***<br>(0.002) |
| Post reform=1 | 0.052***<br>(0.004) | 0.056***<br>(0.007) | -0.018<br>(0.023) | 0.030<br>(0.021) | -0.061*<br>(0.032) |
| Post reform=0 × Income in 1998 | | | -0.000*<br>(0.000) | | -0.000<br>(0.000) |
| Post reform=1 × Income in 1998 | | | 0.000<br>(0.000) | | 0.000**<br>(0.000) |
| Post reform=0 × Unemployment in 2000 | | | -0.002**<br>(0.001) | | -0.001<br>(0.001) |
| Post reform=1 × Unemployment in 2000 | | | 0.005**<br>(0.002) | | 0.006***<br>(0.002) |
| Post reform=0 × income growth 98-02 | | | | 0.101***<br>(0.032) | 0.092***<br>(0.033) |
| Post reform=1 × income growth 98-02 | | | | 0.255***<br>(0.096) | 0.295***<br>(0.077) |
| Post reform=0 × Unemployment change 00-02 | | | | 0.006***<br>(0.002) | 0.005**<br>(0.002) |
| Post reform=1 × Unemployment change 00-02 | | | | -0.002<br>(0.005) | 0.003<br>(0.004) |
| Region-time fixed effects | No | Yes | Yes | Yes | Yes |
| Observations | 5208 | 5208 | 5208 | 5208 | 5208 |
| Adjusted $R^2$ | 0.193 | 0.229 | 0.229 | 0.230 | 0.230 |

*Notes:* See the notes in Table 2 for time periods and variable definitions.

## C Online Appendix: House price index comparison

Table C1 compares the FinansDanmark house price index with the house price index used by Denmark Statistics (DST). The DST index addresses concerns over differences in transacted property types by using property assessments, whereas the FinansDanmark index is based on square meter prices. We construct a FinansDanmark index for each municipality by scaling each observation by the price in 2006, the same base year as the DST index.

In the table below, the dependent variable is the Denmark Statistics house price index for each region and year-quarter, and the independent variable is the FinansDanmark price index for the same date and region. For both series, we select the single-family series.

**Table C1:** House Price Index Comparison

| | Levels | | | |
|---|---|---|---|---|
| | (1) All Years | (2) 1992-2003 | (3) 2004-2007 | (4) 2008-2012 |
| DST HPI | 1.02*** | 0.95*** | 1.00*** | 1.16*** |
| | (0.00) | (0.00) | (0.01) | (0.01) |
| Observations | 1344 | 768 | 256 | 320 |
| Adjusted $R^2$ | 0.993 | 0.992 | 0.993 | 0.962 |

*Notes:* Dependent variable is the Denmark Statistics house price index. Robust standard errors in parenthesis.

As the table shows, there is a very strong correlation between the two indices, with a coefficient close to one for the entire sample (column 1) and for the time periods before 2008 (columns 2-3).