

Mortgage Innovation and House Price Booms*

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We study how mortgage innovation can cause a housing boom even within a robust regulatory framework and strictly enforced recourse borrowing. Specifically, we find that the 2003 introduction of interest-only (IO) mortgages in Denmark ignited a housing boom that increased house prices 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 where IO loans play a sizable role in mortgage finance.

JEL Classification: R21, R30, G51, G21

Keywords: Interest Only Mortgages, Housing Booms; Mortgage Lending

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After growing slowly between 2000 and 2003, Danish house prices increased nearly 60 percent from 2003 to 2006 (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 robust regulatory design that limits housing speculation for banks and households. Mortgage banks (mortgage originators) are also fully liable for defaults on mortgages sold to investors, mitigating 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, reducing mortgage expenses by 20 percent for these first ten years relative 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. 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. 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.

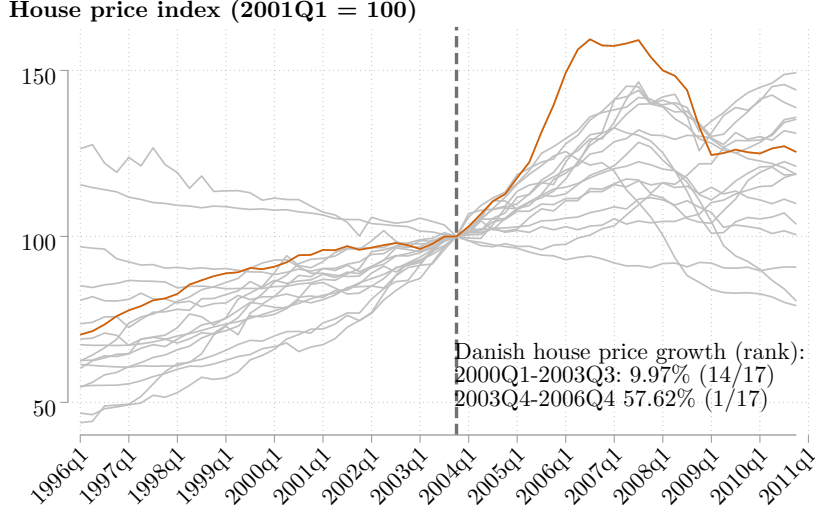


Figure 1: House prices in Denmark and other countries, 1996-2010

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

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). 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 document 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. We implement a rich panel data setup so that our identifying variation is orthogonal to differential municipality-level trends, ex-ante municipality macro variables with time-specific coefficients, and city-time fixed effects. 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 and our results are robust to controls for the mortgage interest rate. The paths of these variables thus evolved in parallel across high and low treatment intensity municipalities during the pre-treatment pe-

riod. Hence, the parallel pre-trends assumption within our regression framework also extends to local macroeconomic conditions. In placebo tests, we show that exposure does not predict house price growth during other boom periods or following other mortgage market reforms, such as during Covid-19 or with introduction of variable-rate mortgages.

The rapid implementation of the policy also 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. As all mortgage banks offered this product, heterogeneous, regional credit supply-side factors are unlikely to drive our results. Finally, we note that many countries experienced a housing inflection point in the early 2000s and 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 but find little evidence that such activity increased following the introduction of interest-only mortgages. Our evidence thus suggests that the Danish housing boom started with the introduction of IO mortgages 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 (Grodecka, 2017; Greenwald, 2017). 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. To support this assertion, we show that the share of payment-constrained borrowers correlates strongly with exposure at the municipality-level.¹

Introducing IO mortgages thus lowers initial borrower mortgage obligations more in these high-

¹Similarly, Nissen *et al.* (2023) find that the share of payment-constrained borrowers is increasing in the square meter purchase price, our exposure measure.

priced areas, causing a heterogeneous increase in housing demand across geographies. IO loan use then becomes a self-reinforcing mechanism. After IO mortgages initially boost property values, subsequent buyers choose an IO mortgage to satisfy binding payment-to-income constraints amid elevated prices. 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.

Our supposition is *not* that IO mortgages solely explain the full extent of the boom in house prices. 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 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 (see also [Karpestam & Johansson, 2019](#)). 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 trend in house price growth does not change after the variable mortgage reform.

Second, an initial shock to house prices stemming from IO mortgages may spark an increase in house price expectations that subsequently fuels a boom, as expectations depend on past growth rates² and 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

²See [Brueckner *et al.* \(2012\)](#); [Case *et al.* \(2012\)](#); [Dell’Ariccia *et al.* \(2012\)](#); [Glaeser & Nathanson \(2017\)](#).

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 substantial impacts 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, house price appreciation, and speculation). 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. Likewise, IO mortgage originations were prominent in the U.S. during the 2000s boom.³ In the post-crisis period, regulators in Sweden, Norway, and the Netherlands have moved to limit interest-only mortgages.

Our study also differs from others in the underlying mechanisms: IO mortgages relax payment-to-income constraints that subsequently increase house prices. [Barlevy & Fisher \(2021\)](#) emphasize the role of IO mortgages in feeding speculation, whereas we find a limited role for speculation. [Greenwald \(2017\)](#) shows how relaxed PTI constraints can generate house price booms but focuses on changes in the PTI limit. Our study is thus the first to link the relaxation of PTI constraints caused by IO mortgages to house price growth.

1 Data description

We construct several datasets for the primary analysis. A replication package is available in [Bäckman & Lutz \(2024\)](#). 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.⁴ The square meter price data is available for apartment and single-family houses,

³In 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 less common in the U.S. with the advent of the Qualified Mortgage (QM) and ability-to-repay rule but remain prevalent in other countries.

⁴The 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.

which we combine to obtain a single square meter price series for each municipality.⁵ We complement this dataset with municipality-level unemployment rates and income. Summary statistics are available in Table B2.

We 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 an array of demographic and financial information, including 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 income, debt, and financial wealth.

The dataset also includes 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.⁶

2 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

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.

⁵Specifically, 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 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.

⁶We 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.

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. Refinancing into an IO mortgage requires a new mortgage. Borrowers can also cash-out refinance into an IO mortgage, as long as the new mortgage does not exceed loan-to-value thresholds (see [Bäckman & Khorunzhina, 2023](#)).

In Denmark, borrowers obtain credit 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 ability 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.

Danish 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.

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). 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.

An implication of the mortgage market design in Denmark is that loss-given default is likely low, as the banks can garnish future income. This makes 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.

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.

2.1 The 2003 Danish mortgage reform

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.⁷ The government

⁷The 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

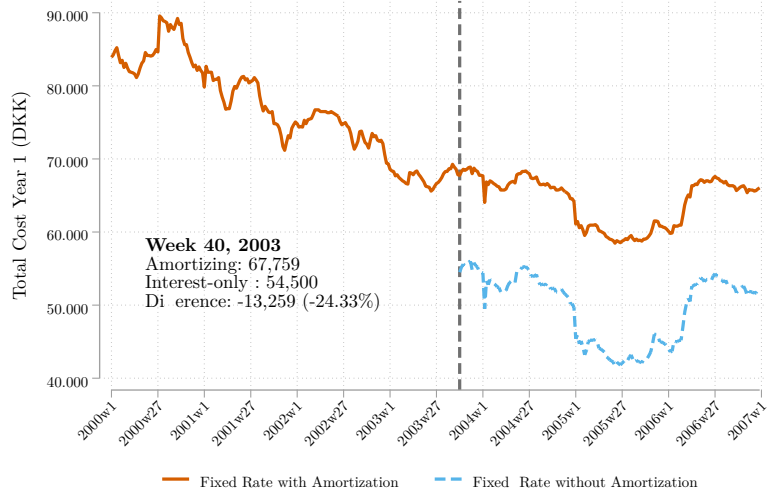


Figure 2: First Year Mortgage Payments

Notes: The vertical dashed-line indicates the Danish introduction of interest-only mortgages in 2003Q4. The figure plots the total first-year payments for a 1 million Danish Krone (DKK), fixed-rate mortgage contract, with amortization payments (orange solid-line) and without amortization payments (blue 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.

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 without long-term impact on house prices or consumption.⁸

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 2 plots the first-year cost for a newly originated 1 million Danish Krone (approximately \$150,000) fixed-rate mortgage with (orange solid-line) and without (blue dashed-line) amortization payments over time. The calculations in Figure 2 are based on average fixed-rate mortgage interest rates for each year-month.⁹ 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 (approximately \$2,000). For the average home buyer in Copenhagen during 2003, the difference in annual

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.

⁸The 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>.

⁹See 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.

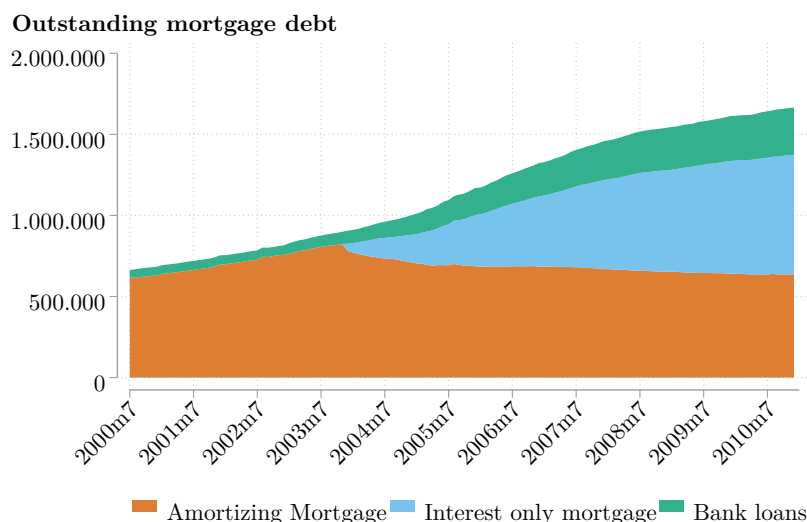


Figure 3: Stock of Outstanding Mortgage Debt

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>.

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).

Interest-only mortgages rapidly became the mortgage of choice for Danish homebuyers. Bäckman & Lutz (2020) document that approximately 60 percent of homebuyers chose an IO mortgage in 2006, and it was not only low-income buyers who preferred IO mortgages. Instead, interest-only mortgages were broadly popular among high and low-income borrowers, the young and the old, and wealthy and non-wealthy households. Figure 3 plots 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.

3 Results

This section provides our empirical methodology and main results. We first document that our exposure measure, municipality-level house price levels five years before the reform, strongly

predicts subsequent IO mortgage use at the municipality and borrower levels. 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 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.

3.1 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 that may confound our estimates.

Our exposure measure captures 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 hinges on the size of amortization payments and thus the value of the purchased property and the mortgage. Since amortization payments are less onerous for a \$100,000 mortgage than for a \$500,000 mortgage, holding income constant, the borrower with the larger mortgage 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

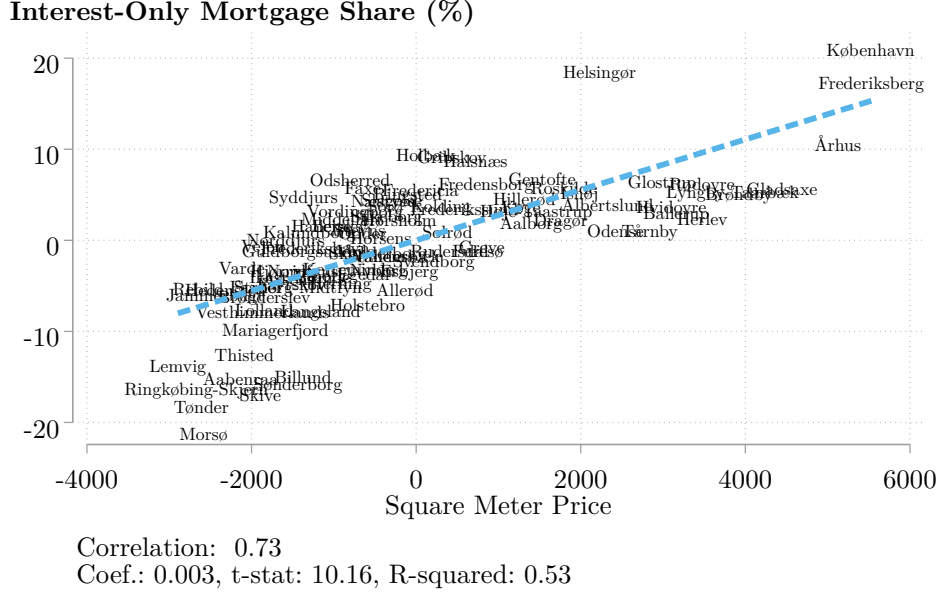


Figure 4: Municipality-Level Ex-Ante Exposure and 2009 IO mortgage share, controlling for Pre-Treatment Household Income

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 the following regression: $IO_k = \alpha + \beta \text{SquareMeterPrice}_k + \gamma \text{Income}_k + \epsilon_k$, where IO_k and $\text{SquareMeterPrice}_k$ denote the IO share and square meter price in 1998 for municipality k .

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, 2023; Bäckman & Lutz, 2020).

We first show cross-sectional evidence that ex-ante price levels capture the benefits of IO mortgage use in Figure 4. 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 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:

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

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. We observe the interest-only mortgage status in 2009, and use the loan origination year to track the mortgage back in time. Thus, for a one standard deviation increase in $Exposure$, β , 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, δ_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 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. Finally, Figure A6 in Appendix A shows similar results when we aggregate transactions to the municipality-level.

3.2 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) expe-

Table 1: Exposure and IO mortgage share

	2003-2010				2003-2006			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposure	0.0924*** (0.00850)	0.0948*** (0.00901)	0.102*** (0.00899)	0.101*** (0.00879)	0.0996*** (0.0109)	0.104*** (0.0113)	0.112*** (0.0114)	0.112*** (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). The dependent variable is a dummy equal to one if the individual buyer uses an interest-only mortgage. We observe the interest-only mortgage status in 2009 and use the loan origination year to track the mortgage back in time. 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.

rienced higher subsequent house price growth increases. We estimate the following dynamic equation:

$$\begin{aligned}
\Delta \ln HP_{kt} = & \sum_{y \neq 2003Q3} \beta_t 1\{y = t\} \times Exposure_k \\
& + \sum_{y \neq 2003Q3} \eta_t 1\{y = t\} \times \mathbf{X}'_k \\
& + \tau_k + \tau_{rt} + \alpha + \epsilon_{kt}
\end{aligned} \tag{2}$$

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, β_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 β_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.¹⁰

Additionally, we include the growth in disposable income between 2002 and 2006 to account for an income tax reform passed in 2003. The income tax reform increased the threshold of the medium tax bracket, lowering the income tax rate beginning in 2004 for a considerable fraction of the population (Jakobsen & Sogaard, 2022). We discuss the impact of this reform in more detail in Section 5. 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 income tax reform does not drive our results. Our central estimates vary little with the inclusion of this control. τ_k , τ_{rt} , and α 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.¹¹ 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 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 *Ex-*

¹⁰ The choice of income level in 1998 and unemployment in 2000 is guided by data availability. In robustness checks, we have confirmed that our main results are not sensitive to the choice of years for control variables.

¹¹ 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.

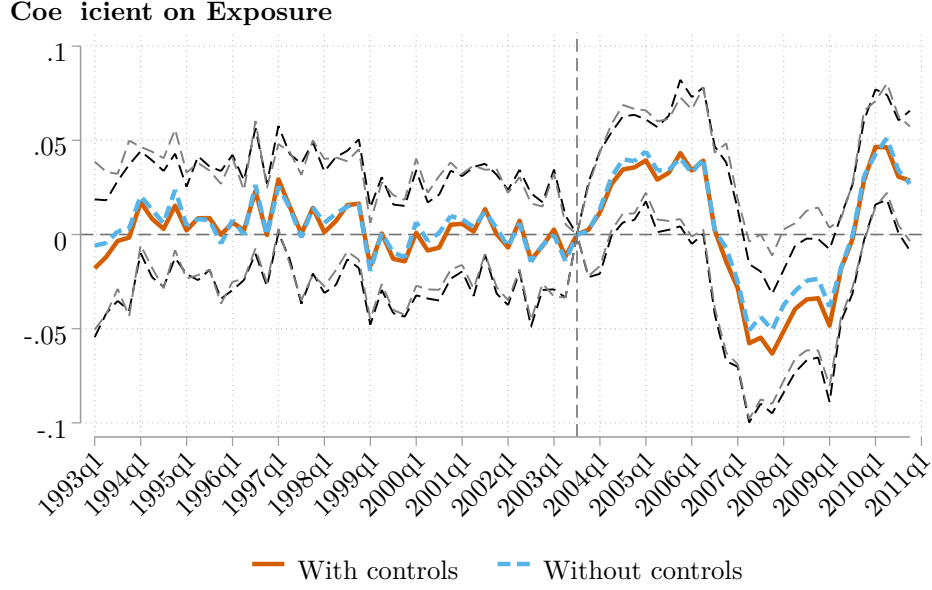


Figure 5: The effect of IO mortgages on house price growth

Notes: The figure plots the difference-in-differences coefficients, β_t , from Equation 2 for a specification with and without controls. Control variables include the municipality average income in 1998, the growth in average municipality income between 2002 and 2006 the municipality unemployment rate in 2002. All controls are interacted with year-quarter 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.

posure. 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. Last, we interact municipality-level control variables (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.

3.3 Main results

Figure 5 plots the difference-in-differences coefficient estimates corresponding to β_t , along with their 99 percent confidence intervals. We provide results with and without control variables, highlighting the robustness of our estimates. There is no statistically significant difference in pre-reform house price growth trends, congruent with the difference-in-differences parallel pre-trends assumption. 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 (Brueckner *et al.*, 2012; Case *et al.*, 2012; Dell’Ariccia

et al., 2012; Glaeser & Nathanson, 2017), the parallel pre-trends in Figure 5 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 4. 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. The lack of pre-trends extends back to 1993, covering a period of high house price growth in 1994 and the introduction of variable-rate mortgages in 1996. In placebo tests, we later show that our exposure measure does not predict house price growth during these two episodes or during the Covid-19 pandemic.

Following the introduction of interest-only mortgages, Figure 5 shows that municipalities with higher ex-ante *Exposure* experienced a larger spike in house price growth directly following the reform. Thus, introducing IO mortgages immediately impacted 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. House prices in high *Exposure* municipalities then turn negative 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).¹² 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

¹²The 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.

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 (τ_k), and region-time fixed effects τ_{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 Tables B3 and B4 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, β , measures the difference in annualized quarterly house price growth across the high and low *Exposure* municipalities during the post-reform 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). For the early post-reform treatment period from 2003Q3-2006Q4, the estimate of 0.0234 in column 1 shows that a one-standard-deviation increase in *Exposure* led to 2.34 percent higher quarterly year-over-year house price growth. The coefficient is robust to controls for time-invariant municipality-level fixed effects (column 2), regional and temporal shocks through region \times time fixed effects (column 3), and municipality-level for income, unemployment, and income growth (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

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

	(1)	(2)	(3)	(4)	(5)	(6)
a) Early post reform (2003Q3-2006Q4)						
Exposure \times Post-reform	0.0234*** (0.0018)	0.0210*** (0.0023)	0.0240*** (0.0036)	0.0235*** (0.0042)	0.0149** (0.0073)	0.0232*** (0.0082)
Exposure \times Long Mortgage rate					-0.5130* (0.2854)	
Exposure \times Short Mortgage rate						-0.0195 (0.3170)
Municipality fixed effects	-	Yes	Yes	Yes	Yes	Yes
Region \times year-quarter fixed effects	-	-	Yes	Yes	Yes	Yes
Municipality-level controls \times year-quarter	-	-	-	Yes	Yes	Yes
Observations	3348	3348	3348	3348	3348	3348
Adjusted R^2	0.231	0.325	0.706	0.708	0.709	0.708
b) Late post reform (2007Q1-2010Q4)						
Exposure \times Post-reform	-0.0297*** (0.0034)	-0.0317*** (0.0038)	-0.0099** (0.0043)	-0.0139*** (0.0029)	-0.0328*** (0.0037)	-0.0286*** (0.0030)
Exposure \times Long Mortgage rate					-1.6849*** (0.2824)	
Exposure \times Short Mortgage rate						-1.4231*** (0.2159)
Municipality fixed effects	-	Yes	Yes	Yes	Yes	Yes
Region \times year-quarter fixed effects	-	-	Yes	Yes	Yes	Yes
Municipality-level controls \times year-quarter	-	-	-	Yes	Yes	Yes
Observations	3627	3627	3627	3627	3627	3627
Adjusted R^2	0.265	0.278	0.755	0.757	0.762	0.767

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). Control variables include the municipality average income in 1998, the growth in average municipality income between 2002 and 2006, and the municipality unemployment rate in 2000, all interacted with year-quarter dummies. Region \times 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 corresponds to a 2.35 percent increase in quarterly year-over-year house price growth during 2003Q3-2006Q4.

In columns 5 and 6, we are concerned with the downward trend in interest rates over the early part of the boom, 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 therefore interact exposure with the long mortgage interest rate in column 5 and the short mortgage rate in column 6. Note that the region \times time dummies in the regressions are co-linear with the mortgage rate. The coefficient of interest remains highly statistically and economically significant in both specifications. 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.

3.4 Aggregate impact

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 2003–06 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:

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

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 6 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. *Exposure₁* 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

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	0.40	0.60	0.57	0.68
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)					0.56

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 6 of Table 2. 2. Early Post-Reform Raw Mean HP Growth less g=1 is calculated as the average growth in house prices (ΔHP_{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.

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 68 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 $g = 2, \dots, 5$ 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 56 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 that of 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.

3.5 Spillovers and treatment effect estimation

Finally, two empirical concerns are worth noting. First, difference-in-differences estimation assumes no spillover between units. This assumption, called the Stable Unit Treatment Value Assumption (SUTVA), is violated in the presence of general-equilibrium effects or spillovers. This is a common yet under-appreciated concern in the literature and is a clear challenge in spatial analysis. In our setting, spillovers could occur if demand spills over into areas with a low value of interest-only mortgages, perhaps because households selling in high Exposure areas subsequently buy in low Exposure areas.

The important question is what spillovers imply for our main estimates of how interest-only mortgages affect house price growth. Spillovers, if present, are likely positive (e.g., demand spilling over from high to low exposure areas). In this case, our estimates would be biased toward zero and underestimate the effect of interest-only mortgages on price growth, making our results conservative. Yet in robustness checks below, we find no significant change in the trends in income, population, unemployment, and employment across high and low Exposure municipalities immediately following the reform (Figures 12 and A9). Spillovers across regions within Denmark are thus likely limited and not a notable source of potential bias.

Regarding general-equilibrium effects, recall that the Danish IO reform was introduced nationally and that even low-exposure areas experienced IO mortgage use. As IO mortgages relax debt-service constraints, they make homeownership more affordable. Therefore, an uptick in IO loan use would likely be capitalized into house prices, like a decline in interest rates, even in low exposure areas, all else equal. Thus, any general-equilibrium effects would bias estimates toward zero, suggesting that our main results near a lower bound.

To assess the impact of spillover and general-equilibrium effects, we report estimates from a synthetic control design, where the potential control units are regions in Canada. Canada has a mortgage market similar to many Western countries but did not allow IO loans during the 2000s.¹³ With this methodology, we remove the concern that the Danish IO loan reform

¹³See [Traclet \(2005\)](#), [Han et al. \(2021\)](#), and the references therein for an overview of the Canadian housing

affected the control group, while fixed effects inherent in the synthetic control design account for static differences between countries. Note that there may be spillover effects within Denmark in this setup, but the control group is not affected by these spillovers. The results in Appendix D are consistent with our main results in that a) the introduction of interest-only mortgages increased house price growth in Denmark, and b) the effect was stronger in areas with high exposure.

Second, there is a recent discussion around the assumptions underlying continuous-treatment difference-in-differences estimation within a linear two-way fixed-effect (TWFE) specification (Callaway *et al.*, 2024). In particular, treatment effect heterogeneity may lead to difficulties in interpreting differences in parameter values across different values of the treatment. With a continuous treatment, Callaway *et al.* (2024) discusses two distinct causal parameters: a *level treatment effect* and a *causal response*. In our context, the level treatment effect measures the difference in house price growth for a municipality for a given treatment dose d and its untreated potential outcome, in the absence of the introduction of interest-only mortgages. The causal response measures the difference in the potential outcome for a marginal increase in the treatment dose d , or a marginal increase in Exposure. Comparison between treated and untreated units identify the average level treatment effects parameter under parallel trends. We are primarily interested in the level-effect, which requires a parallel trend assumption and a discussion of heterogeneous treatment effects associated with the TWFE estimator.

We assess homogeneous treatment effects by examining estimates for different levels of Exposure using a saturated set of dummy variables. Panel a) of Figure 6 plots a local polynomial across Exposure for the difference in house price growth between the pre-reform and early post-reform periods. The polynomial shows a monotonic and linear relationship between Exposure and the difference in house price growth through the reform. This result is reassuring, as it suggests a homogeneous treatment effect across different levels of Exposure.

Panel b) of Figure 6 plots the estimates from a regression where we create ten dummy variables based on Exposure and estimate regressions for the early-post-reform period. The figure also includes the value of Exposure for each group. Specifically, we estimate the following regression and plot the coefficients on Post-reform for each treatment group j , while controlling for the market.

same variables as in columns 1, 4, and 6 of Table 2:

$$\Delta HP_{kt} = \beta_j PostReform \times \sum_{j=2}^J 1D_k = d_j + \sum_{y \neq 2003q3} \eta_t 1\{y = t\} \times \mathbf{X}'_k + \tau_k + \tau_{rt} + \alpha + \epsilon_{kt} \quad (5)$$

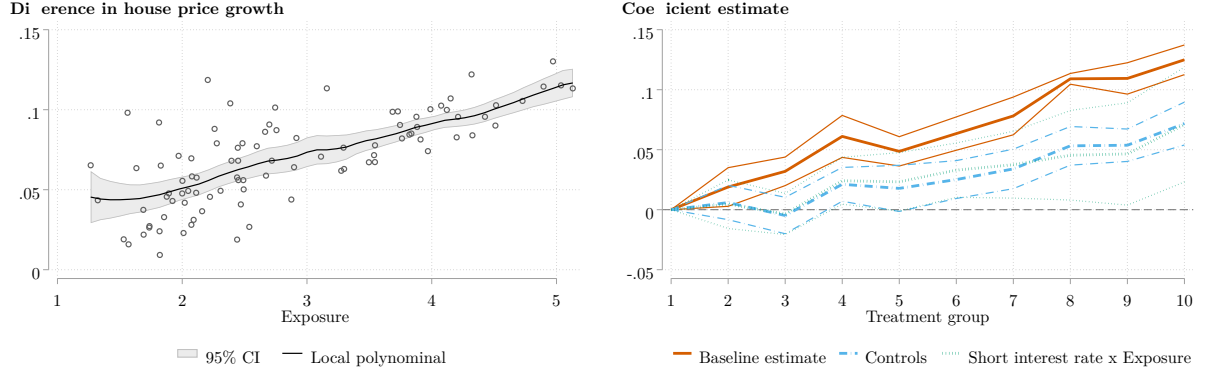
The estimated coefficients are all relative to the house price growth in the lowest treatment group, group 1. The figure shows that the estimated treatment effect are linearly increasing in Exposure, which indicates homogeneity in treatment effects. Under parallel trends assumption, the coefficient β_j identifies the ATT for each group m (Callaway *et al.*, 2024), with coefficients for $\hat{ATT}(m|m)$ that range from 0.018 for group $m = 2$ and 0.125 for group $m = 10$ for the baseline estimates. The average across groups 2-10 is 0.072 for the baseline, 0.031 for both the estimates with control variables and where we control for the interest rate. In comparison, the TWFE estimate in Table 2 was 0.0234 for the baseline results in column 1, 0.0235 for the results with controls in column 4, and 0.0232 when we control for the short mortgage rate in column 6. While the TWFE estimator has the correct sign, the average of the treatment effects in Figure 6 is thus larger than the estimates in Table 2. We also provide binned scatterplots with and without control variables in panels c) and d), again showing the linear trend in house price growth by exposure.

Overall, the concerns over spillovers and the continuous difference-in-difference suggest, if anything, that our main estimates are conservative.

4 Mechanisms

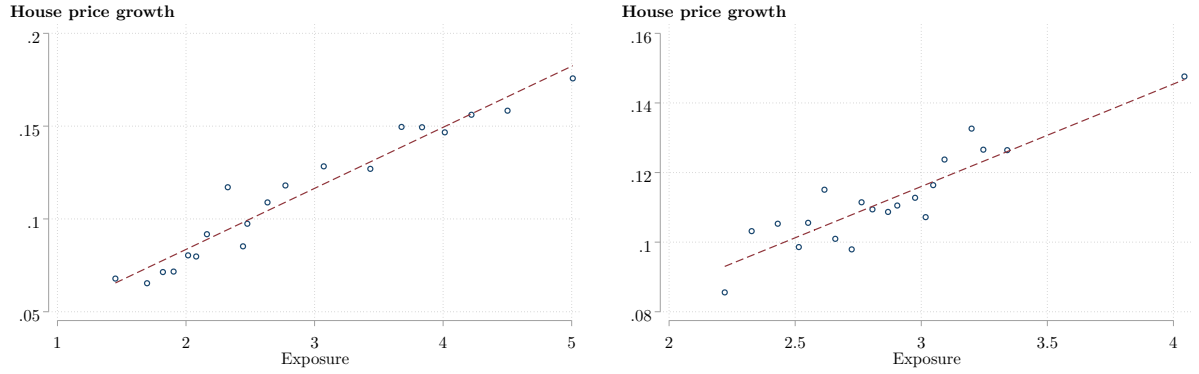
Why did the introduction of interest-only mortgages lead to a rapid increase in house prices? Recent research suggests that amortization payments represent real costs for households (Amromin *et al.*, 2018; Bernstein & Koudijs, 2021; Bäckman & Khorunzhina, 2023) or that households are targeting low initial monthly payments (see Argyle *et al.*, 2020; Shu, 2013; Bäckman *et al.*, 2023). Alternatively, interest-only mortgages can relax payment-to-income constraints on the bank side. Both mechanisms imply a causal role for IO mortgages to impact house prices; we discuss them in more detail below.

IO mortgages created a sizable housing demand shock in Denmark, given their notable popularity among borrowers and the corresponding steep reduction in payments. This is not sufficient for a large increase in house prices, however, since an increase in demand can affect either the price of owner-occupied housing or the quantity of housing demanded, depending on the slope



(a) Local polynomial in house price growth and exposure

(b) Estimated coefficients by groups of Exposure



(c) Binscatter, no controls

(d) Binscatter, with controls

Figure 6: Non-linear effects of Exposure

Notes: Panel a) plots a scatter plot of the difference in house price growth between the pre-reform and early post-reform and Exposure, where we also estimate a local polynomial. The difference in house price growth is calculated as the average house price growth in the early post-reform period minus the average house price growth in the pre-reform period for each municipality. In Panel b), we divide municipalities into ten groups based on Exposure and estimate $\Delta HP_{kt} = \beta_0 + \sum_{j=1}^J \beta_j \times Post + \epsilon_k$. We omit the group with the lowest Exposure. The orange solid line provides the baseline results without any control variables, the blue line adds the same control variables as in Table 2 (municipality average income in 1998, the growth in average municipality income between 2002 and 2006, and the municipality unemployment rate in 2000, all interacted with year-quarter dummies). The green line adds a control for the mixed interest rate interacted with Exposure. Panels c) and d) provide binned scatterplots, where we plot average house price growth in the early post-reform period against Exposure. Panel c) provides results with no control variables, and panel d) includes region fixed effects and controls for municipality average income in 1998, the growth in average municipality income between 2002 and 2006, and the municipality unemployment rate in 2000, all interacted with year-quarter dummies.

of the supply curve (Greenwald & Guren, 2021; Lutz & Sand, 2023). Bäckman & Lutz (2020) analyze both the house size and homeownership effect of the IO mortgage reform and ultimately conclude that neither change. The evidence therefore points towards a vertical supply curve, implying that the reform impacted house prices. 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.

The IO mortgage-induced decline in borrower payments likely does not explain the full extent of the Danish housing boom. We argue that introducing 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 impact of lower payments discussed in the paragraph above but are behavioral responses to the initial boom. We now discuss the mechanism behind our results in more detail.

Relaxation of payment-to-income constraints – 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 (Cocco, 2013; Larsen *et al.*, forthcoming).

How important are mortgage payments relative to income across Danish municipalities, and how do they vary with exposure? To answer this question, we use micro-data on household income, property size, and wealth, as well as transaction prices (grouped into quartiles based on square meter size) for each municipality and year. We then estimate the price each household would have to pay to buy a property of a similar size to their current dwelling. This calculation computes mortgage payments using the mortgage rate for each year and an annuity formula, assuming that the household is either taking out a mortgage with the maximum LTV of 80% or using their wealth, based on registry data, for the down payment.¹⁴ For each household in

¹⁴Wealth includes financial wealth in the form of stocks, bonds or cash deposits, or housing wealth calculated as the housing wealth minus mortgage debt for homeowners.

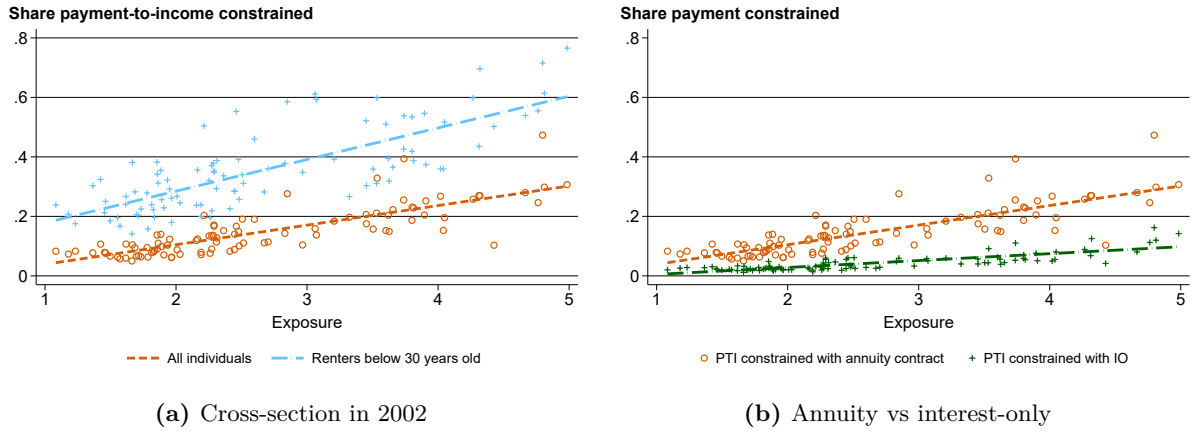


Figure 7: Share of payment-constrained households against exposure

Notes: The figure plots the share of households facing binding payment-to-income constraints against exposure on the municipality level in 2002. All calculations assume a 30-year fixed mortgage, a mortgage interest rate of 6.29 (the average mortgage rate in 2002), and an LTV of 80 percent or that borrowers use their available financial wealth for the down payment. The PTI limit is set to 40 percent of income. In panel b, we plot the share of borrowers in 2002 with payments above 40 percent under an annuity contract (orange line) and an interest-only contract (green line) against Exposure. The unit of observation is a municipality.

the dataset, payment-to-income (PTI) then equals mortgage payments divided by household income. We set the PTI constraint to 40% in 2002 and calculate the share of constrained households ($PTI > 40$) at the municipality level.

In Panel a) of Figure 7, we relate the share of payment-constrained borrowers in 2002 to Exposure, accounting for the individuals' own wealth in the calculations. The figure provides results for all households and for young renters. Young are defined as households where the maximum age among household members is less than 30 years old. As expected, renters below the age of 30 are generally more payment-constrained. The plot also documents a clear positive relationship between Exposure and the share of payment-constrained borrowers for all individuals and the subset of young renters. These results are consistent with the idea that payment-constraints are more prevalent in high Exposure municipalities, suggesting that interest-only mortgages have a larger impact precisely in these areas. Then, as shown above, prices increased in these high Exposure areas as the lower mortgage payments associated with substantial IO loan uptake were capitalized into house prices. Panel b) shows that calculating the share of payment constrained borrowers falls substantially when calculating maximum monthly payments using an interest-only mortgage (green line) instead of an annuity contract (orange line, a fully amortizing mortgage). Note that in panel b), like in panel a), the computations accounts for the borrowers' own wealth, so that the share of payment constrained borrowers under an annuity

contract (orange line) is the same across panels a) and b).

We next examine how the relationship between Exposure and the share of payment constrained borrowers evolves over time. To do so, we estimate the following equation:

$$ShareConstrained_{kt} = \alpha + \sum \beta_t 1\{y = t\} \times Exposure_{1998,k} + \epsilon_{kt}, \quad (6)$$

where the coefficients β_t summarizes the relationship between Exposure and the share of payment constrained borrowers, $ShareConstrained_{kt}$, for each year. Figure A8 in the appendix plots the resulting coefficients. Intuitively, each coefficient corresponds to the slope of the line in panel a). The figure shows that the relationship between Exposure and the share of payment-constrained borrowers is relatively stable over time but then increases sharply during the housing boom. This result implies that housing became increasingly unaffordable for households using standard, amortizing mortgages. Such unaffordability may have encouraged borrowers to look for other products with lower payments, like interest-only mortgages.

Indeed, the expansion of IO mortgages can become a self-reinforcing mechanism, leading to increased use that lifts house prices over time. The initial introduction of IO mortgages boosts house prices, subsequently forcing new potential buyers to choose an IO mortgage to satisfy binding PTI constraints amid elevated prices. This then leads to further home demand, propelling property values (Figure 1) and further IO loan uptake (Figure 3).

The approach in Figure 7 assumes that each household is seeking to purchase a property that is the same size as their current dwelling. Clearly, some households would seek to buy a larger property and move up the property ladder, and some would seek to move down the property ladder. We focus on young potential buyers since they make up most housing transactions (Bäckman & Lutz, 2020) and are thus most relevant for understanding house price movements. Young households typically prefer to move up the property ladder and buy a larger or more expensive property than their current dwelling. If so in aggregate, our calculations here would *underestimate* the price for the next dwelling.

Moreover, we focus on the prevalence of payment-to-income constraints in the local housing market. In a housing market with more than one buyer, the amount that the *second* buyer is willing to pay will impact what the actual buyer will have to pay. In our context, interest-only mortgages may raise the amount that the marginal buyer will have to pay even if that

marginal buyer does not use an interest-only mortgage. The implication is that only market-level outcomes reveal the total impact of interest-only mortgages.

Variable-rate mortgages – 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 8 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 who 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 A7).

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 5 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 introducing 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).

Diagnostic expectations – The second factor that plausibly contributed to the Danish housing boom is house price expectations. We view expectations as a contributor to the marked impact of IO mortgages in high Exposure areas. The idea coincides 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.

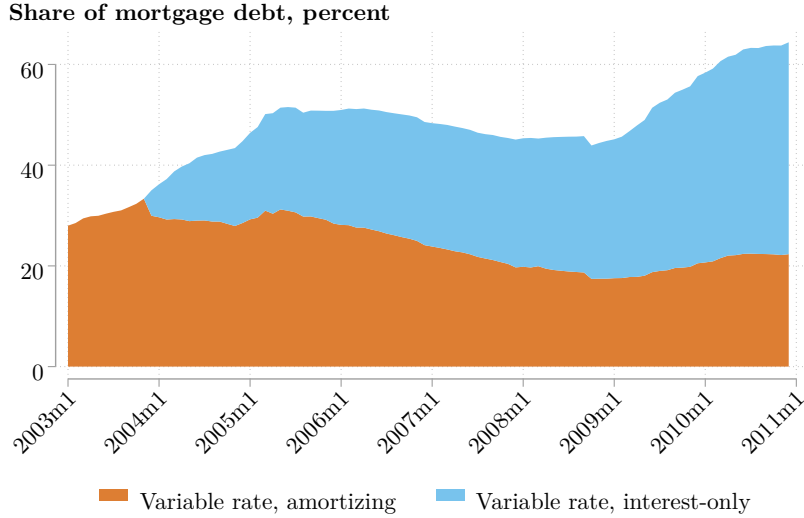


Figure 8: Variable-rate mortgages as a share of outstanding mortgage debt

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). Data for variable-rate mortgages is not available before 2003M1. Source: Finans Danmark.

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 around Jutland, an area 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.

5 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 5](#), 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, reduc-

tions in income tax rates, and a property tax freeze. 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 several potential explanations in turn.

Placebo Estimates – We conduct several placebo tests to examine whether areas with higher Exposure are more prone to booms and busts. To examine this potential concern, we conduct several placebo tests and check whether areas we define as having high Exposure also experience elevated returns in other periods. Specifically, we let placebo interventions occur in 1994 (a period of high house price growth), 1996 (when variable-rate mortgages were introduced), 2000 (when mortgage rates started to decline but IO loans were unavailable), and 2020 (the Covid-19-era house price boom).

For each placebo test, we use the same Exposure measure as in the main analysis and control for region \times year-quarter and municipality dummies. We omit control variables from the regression because of missing data at the municipality level pre-1993. Note that control variables only had a small impact on our estimates in the baseline specification. Regression weights match our main analysis, although our results are unchanged without weights.

Figure 9 presents the results for the placebo tests. In the first panel, we include our main specification for comparison. Overall, we find little evidence that our Exposure measure predicts house price growth around other periods of rapid house price growth, other mortgage market reforms, or periods of declining mortgage rates, implying only the introduction of IO loans created differences in house price returns across high and low Exposure areas.

Speculation and Credit Standards – Several *ex-post* outcomes, including those for forced home sales, non-performing loans, and mortgage defaults, imply limited speculation during the 2000s Danish housing boom. Figure 10 shows that both mortgage arrears as a percentage of outstanding mortgage debt and the number of homes reposessed by mortgage banks remained low throughout the housing market downturn and the financial crisis.¹⁵ 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

¹⁵The 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.

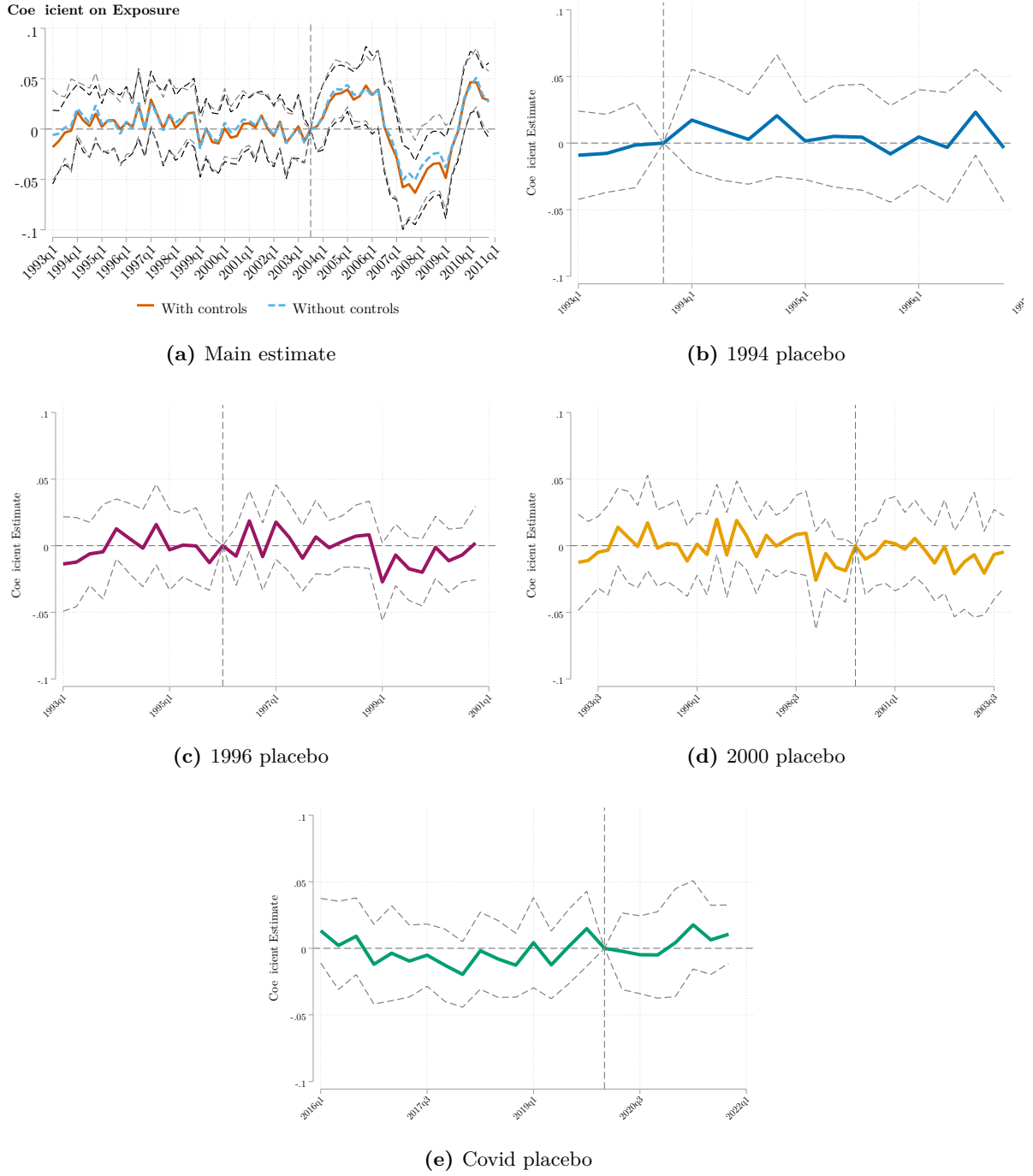


Figure 9: Placebo estimates

Note: The figure plots the difference-in-differences coefficients, β_t , from Equation 2 for different placebo checks. The specifications include municipality and region-time fixed effects. Observations are weighted by the number of transactions in 2004Q1. Standard errors are clustered by municipality.

non-performing loans to gross loans peaked at slightly 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 10, representing forced sales (right-axis), peaks at a little above

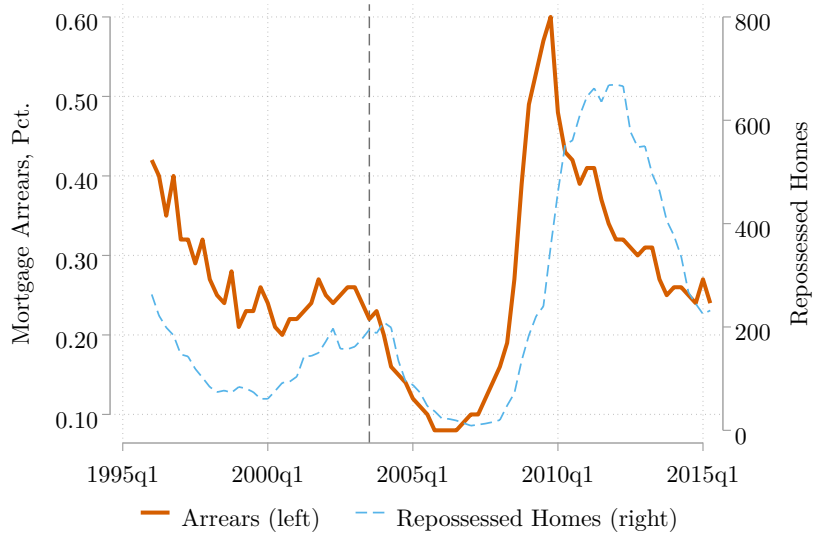


Figure 10: Credit standards in Denmark

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.

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, given that mortgage banks have full recourse against borrowers and can garnish wages.

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. Thus, spec-

ulation may represent an alternative causal link between IO mortgages and house prices.

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.

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.¹⁶ 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](#)).

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 dropped 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 substantial rise in house prices over this period. In addition, estimates of the semi-elasticity are typically lower than 7 ([Davis et al., 2020](#); [Adelino et al., 2012](#)). With the estimated elasticity of 3.4 in [Davis et al. \(2020\)](#) the fall in the interest rate implies a 4.2 percent increase in national house prices.

Alternatively, we use the variable rate share to calculate an effective interest rate. The decline

¹⁶Also, 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.

in the effective interest rate from 2003Q3 to the lowest point is 1.25 percent. Using the elasticity estimate 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. Finally, the placebo estimates associated with a synthetic intervention around 2000, when interests began to fall but before the introduction of IO mortgages, in Figure 9, panel d, show that *Exposure* does not predict regional differences in house price growth during this period.

Estimating the impact of *Exposure* on the average interest rate that new buyers pay in each municipality yields an alternative illustration of the effects of lower interest rates. 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 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 11 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 0.0004207 in 2004 is equivalent to 1.2 percent of the mean imputed mortgage rate (or 0.34 percent of the standard deviation). Moreover, panel b) shows that the difference in absolute numbers between high and low *Exposure* municipalities is low: the average difference from 2003–08 is 10 basis points.

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.

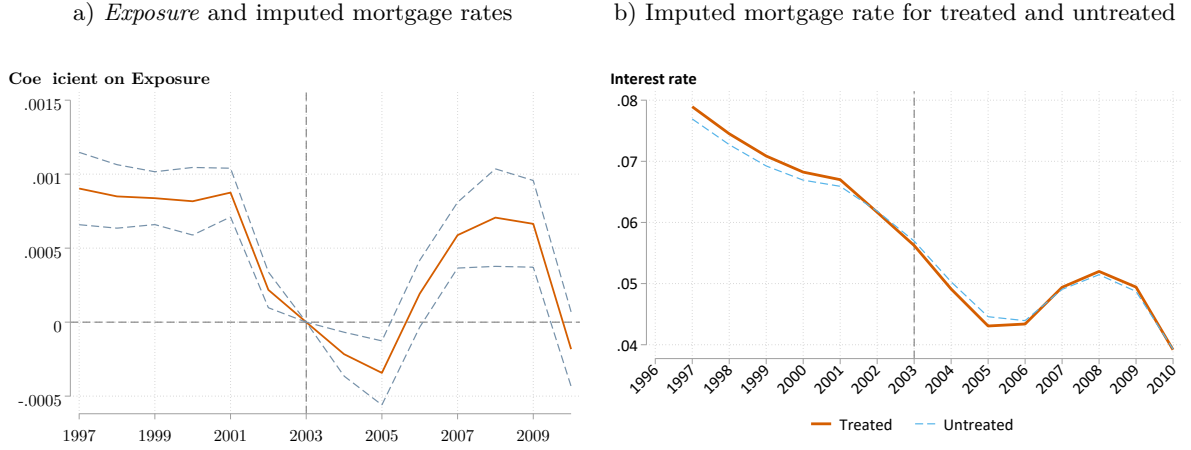


Figure 11: Imputed mortgage rate and Exposure

Note: Panel a) plots the difference-in-differences coefficients, $\sum_{y \neq 2003q1} \beta_t 1\{y = t\} \times Exposure_k$ from Equation 7. 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.

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.

$$\Delta Income_{kt} = \sum_{y \neq 2003q3} \beta_t 1\{y = t\} \times Exposure_k + \tau_k + \tau_{rt} + \alpha + \epsilon_{kt} \quad (7)$$

This equation is equivalent to our main specification, where income growth replaces house price growth, and we do not include any control variables. Figure 12 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 thus little evidence that income growth or labor market shocks can explain the start of the boom.

Income Tax Reform in 2003 – The income tax reform in 2003 represents a plausible threat to identification. Although our results are robust to including a measure of after-tax disposable income, we are concerned that the benefits from the income tax reform accrue disproportionately to high-income areas. To address this concern, we first provide additional detail on the income

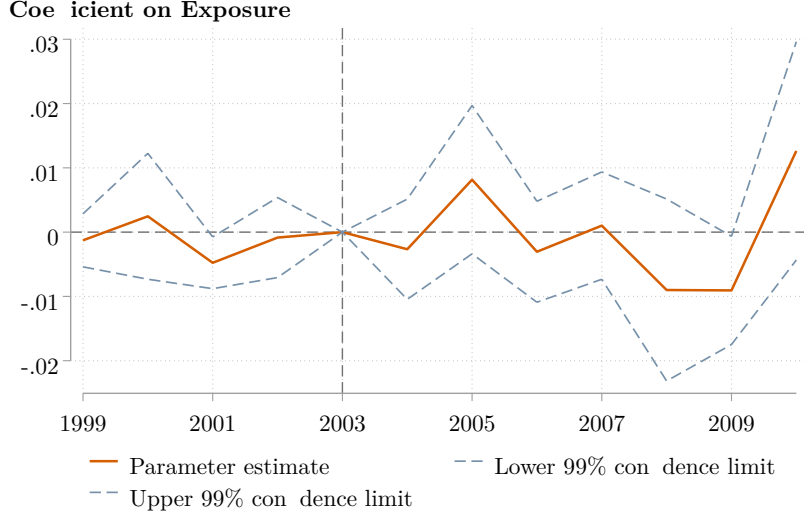


Figure 12: Income Growth and Exposure

Notes: The figure plots the difference-in-differences coefficients, $\sum_{y \neq 2003q1} \beta_t 1\{y = t\} \times Exposure_k$ from Equation 7. 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.

tax reform itself and then use the micro-data to determine who benefits.

The income tax reform did not change the tax rates but significantly increased the size of the middle-income tax bracket. The cutoff for the middle-income tax bracket rose gradually by 12,000 DKK per year for four years. Consequently, the middle-income tax bracket cutoff increased from 198,000 DKK in 2003 to 246,000 DKK in 2007. The annual decrease in taxes paid equals $12,000 * 0.06 = 720\text{DKK}$ or approximately \$105 per year. The tax savings from the reform equals an increase in before-tax income of $720/198,000 = 0.003$ for individuals who earn 198,000 DKK. The tax reform also introduced a new deduction for individuals with taxable income. After 2004, individuals can deduct 2.5 percent of any income below 210,000 DKK (the middle-income tax bracket). The deduction is capped at 5,800 DKK. An individual could already deduct 8 percent (from labor market contributions, “AM-bidrag”), and [Jakobsen & Sogaard \(2022\)](#) report that the average regional tax rate was 32.6 percent. The monthly savings for an individual earning 210,000 DKK (the middle-income tax bracket in 2004) equals $0.025 * 210,000 * 0.92 * 0.326 = 1,574$ DKK per year, or 131 DKK per month. In dollars, this is just \$18.90 per month.

The threat to identification is that the income tax reform had different effects across municipalities. To examine this concern, we calculate the impact of the income tax reform at the municipality level. We select all individuals between age 18 and 65 in 2003 and use a measure

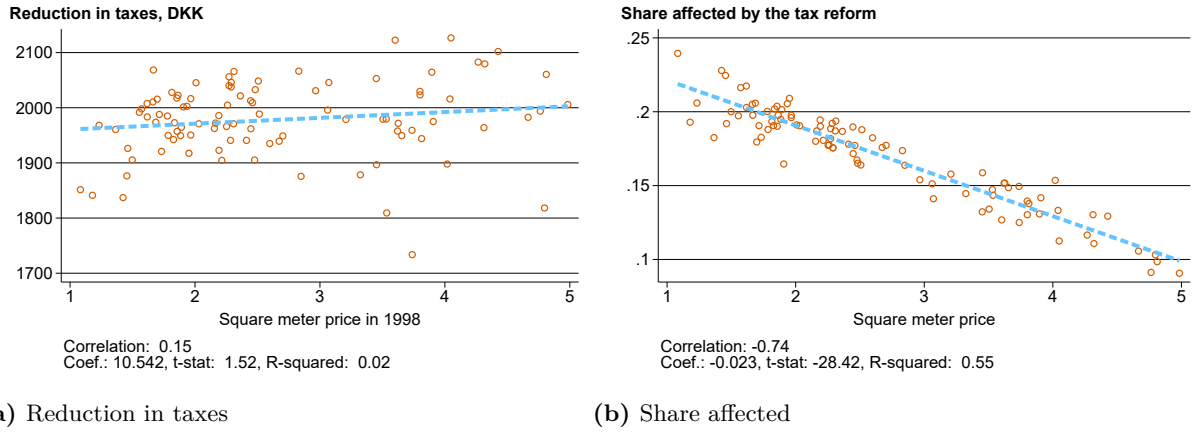


Figure 13: Income tax reform and Exposure

Notes: The figure plots the average reduction in taxes from both the change in the middle-income tax bracket and the new deduction against exposure for each municipality.

of total income provided by Denmark Statistics. The deduction equals total income times 2.5%, up until a maximum deduction of 5,800 DKK. The reduction in taxes is then the deduction times 0.92 times the regional tax rate, which we take from Denmark Statistics. To compute the tax reduction due to the change in the middle-income tax bracket, we calculate the distance to the middle-income tax bracket as total income minus 198,000, up to a maximum of 12,000 DKK. The reduction in taxes equals the distance times 6%. For example, an individual who earns 205,000 DKK would experience a tax reduction of $(205,000 - 198,000) * 0.06 = 420 \text{ DKK}$. All individuals who earn above $198,000 + 12,000$ experience a tax reduction of $12,000 * 0.06 = 720 \text{ DKK}$. We add these tax reductions, calculate the average reduction by municipality, and plot them against exposure.

The results in Panel a) of Figure 13 show that the average reduction in income taxes is larger in high Exposure municipalities. The difference is small at only 40 DKK (approximately \$6) when we compare the lowest and highest quintiles. For young renters, the difference reverses: young renters in high exposure municipalities benefit *less* from the income reform than young renters in low exposure municipalities. As a share of income, the tax reduction negatively correlates with Exposure. We can also compare the effect of the income tax reform to the decrease in payments from choosing an interest-only mortgage across municipalities. Comparing the reduction in payments across municipalities, we find that interest-only mortgages reduce first-year payments by 4,551 DKK in low exposure municipalities and by 14,206 DKK in high

exposure municipalities, a difference of 9,655 DKK (approximately \$1,500).¹⁷ We conclude that the income tax reductions, while significant at the aggregate level, are not sufficiently different across high and low Exposure areas and thus cannot explain the regional variation in house price growth following the introduction of IO mortgages.

We also calculate the “Share affected by the income tax reform” as the share of individuals who earned between 198,000 and 246,000 DKK in 2003 by municipality. We find that Exposure is *negatively* correlated with the share of individuals who would benefit from the tax reform. Panel b) of Figure 13 provides the results. The negative correlation is robust to narrowing the share of affected individuals to 198,000 to $198,000 + 12,000 = 210,000$ (the income range affected in the first year), to using three-year average income (to avoid mean-reversion in income) and to examining only individuals below 35 years of age.

Overall, evidence suggests that the income tax reform did not drive cross-sectional Danish house price growth during the 2000s boom.

Property Tax Freeze – 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 property 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 likely affected prices before the introduction of interest-only mortgages. 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.

6 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

¹⁷To calculate this number, we take the average square meter price in 2002 by municipality and multiply by 113, the average square meter size for purchases in 2002. This calculation gives us a price for a representative property for each municipality. Exposure is not systematically correlated with square meter size, and, for simplicity, we chose the average square meter size across all transactions. We then calculate first-year payments for a mortgage of 80% of the property price for each municipality for both an annuity contract and an interest-only contract. High Exposure municipalities are then municipalities in the top quintile of Exposure, and low exposure municipalities are in the bottom quintile of Exposure.

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 significantly affects 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 boosted house prices, fueling house price expectations and the further use

of 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

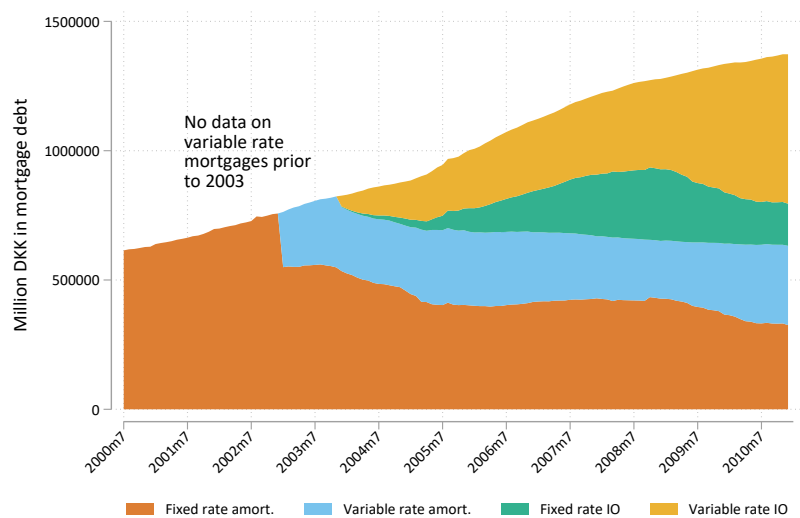


Figure A1: Stock of outstanding mortgage debt

Note: The figure plots outstanding mortgage debt by loan type, including loans for residential properties and vacation homes. Data on variable rate mortgages begin in 2002, which creates the jump in the figure. Variable rate mortgages were introduced in 1996. Source: Nationalbanken.

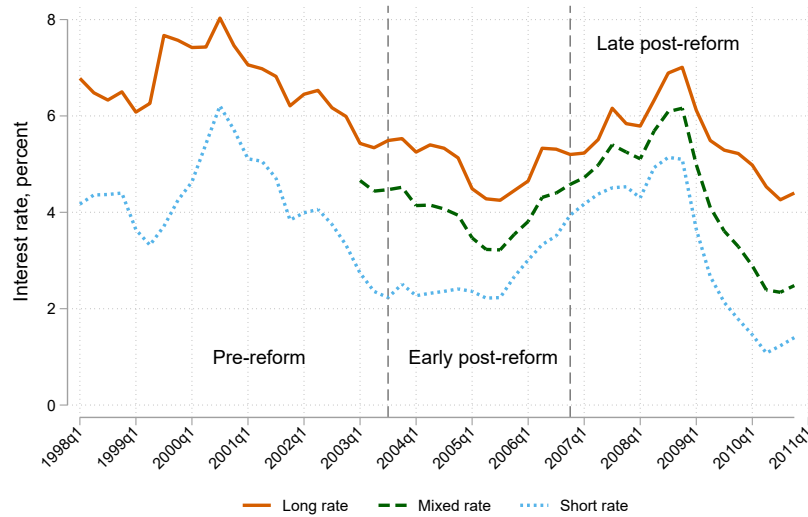


Figure A2: Short and long run mortgage rates

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' calculations .

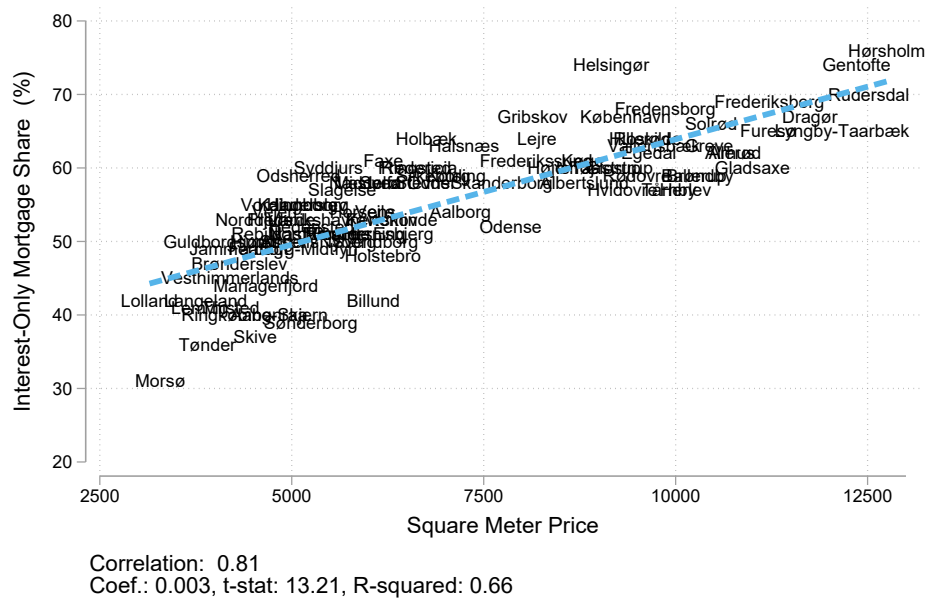


Figure A3: Municipality-level ex-ante Exposure and 2009 IO mortgage share

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 .

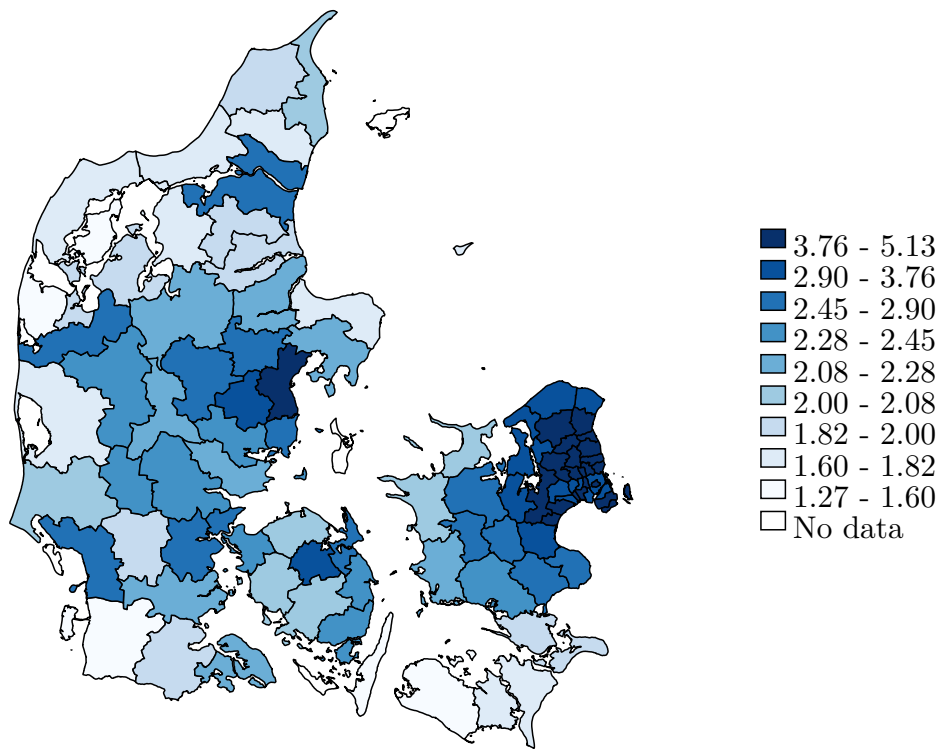


Figure A4: Exposure across Denmark

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

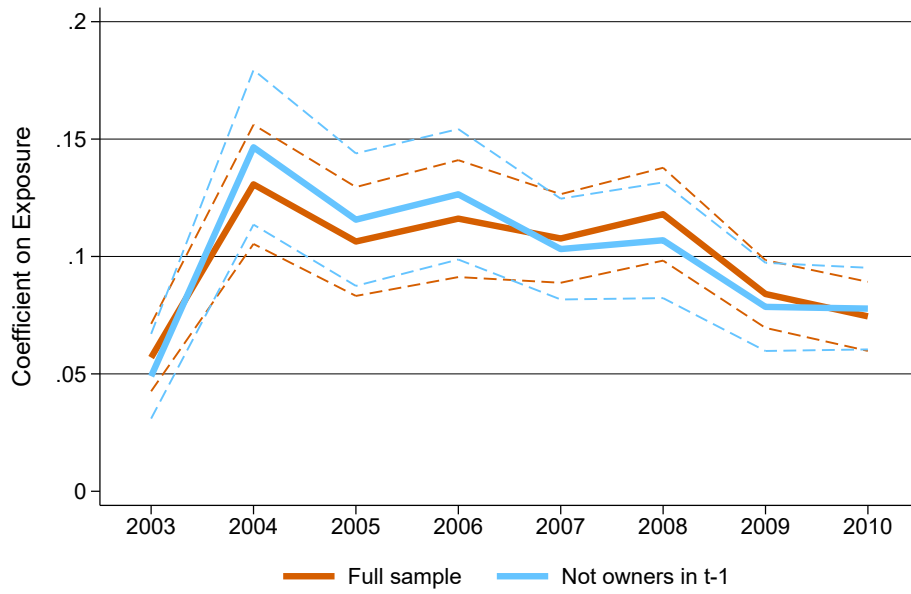


Figure A5: Year-over-year coefficient on Exposure using transaction-level data

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.

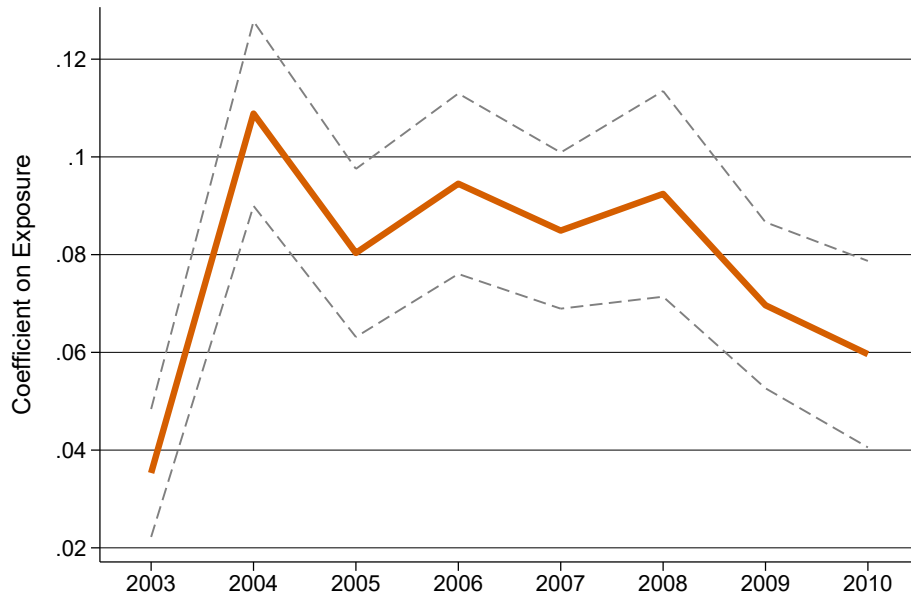


Figure A6: Year-over-year coefficient on Exposure using municipality-level data

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.

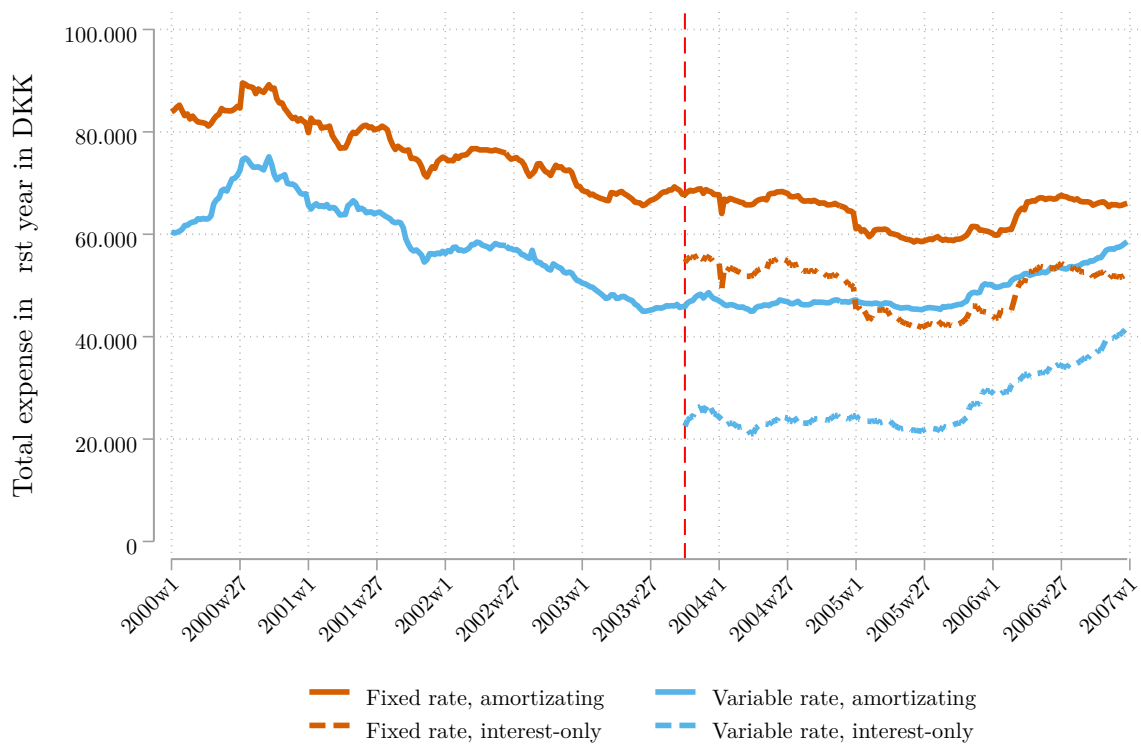


Figure A7: First Year Payments for different mortgage products

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.

Coefficient on Exposure

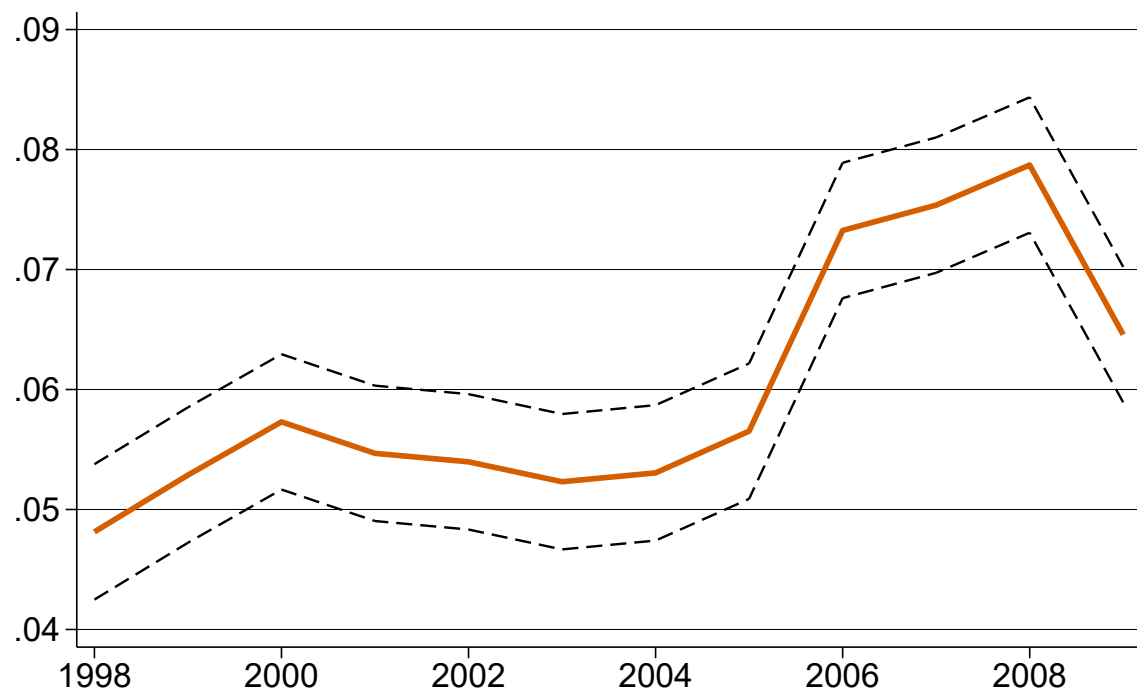
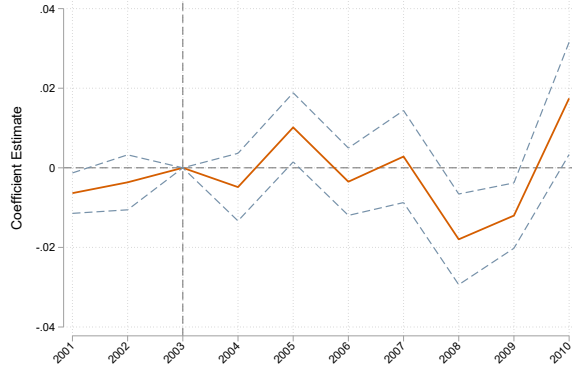
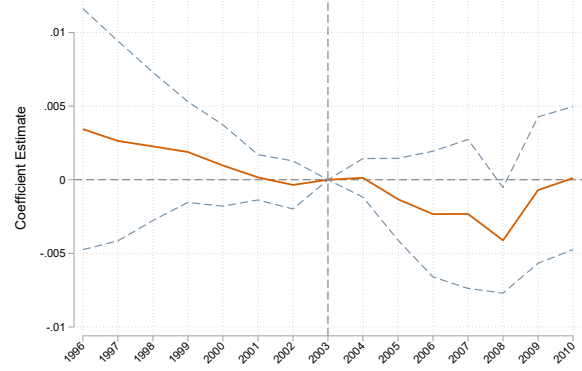


Figure A8: Coefficient on Exposure

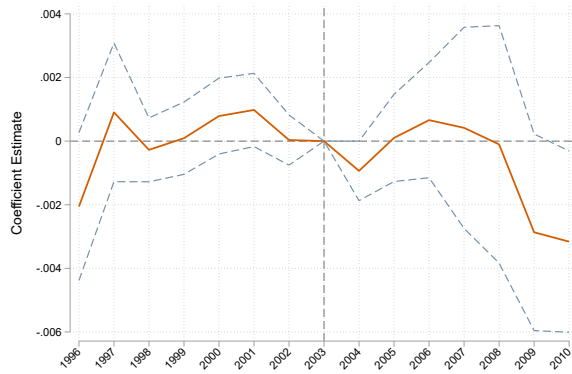
Notes: The figure plots the coefficient on β_t from estimating equation $ShareConstrained_{kt} = \alpha + \sum \beta_t 1\{y = t\} \times Exposure_{1998,k} + \epsilon_{kt}$.



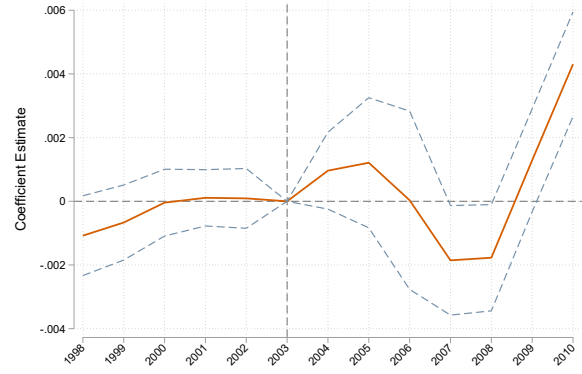
a) Exposure and income growth for house owners



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 7. 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	224,938	35,275	170,623	369,403
Income for owners	308,840	291,492	52,451	244,845	521,985
Income for renters	160,655	154,946	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) Base	(2) Region-Time FE	(3) FE.+ controls	(4) Changes	(5) Abs + changes
Exposure \times Post-reform	0.018*** (0.002)	0.021*** (0.003)	0.021*** (0.003)	0.019*** (0.003)	0.020*** (0.003)
Exposure	0.013*** (0.002)	0.004*** (0.001)	0.005*** (0.001)	0.005*** (0.001)	0.006*** (0.001)
Post reform=1	0.013* (0.007)				
Post reform=0 \times Income in 1998			-0.000*** (0.000)		-0.000*** (0.000)
Post reform=1 \times Income in 1998			-0.000 (0.000)		-0.000 (0.000)
Post reform=0 \times Unemployment in 2000			-0.003*** (0.001)		-0.002** (0.001)
Post reform=1 \times Unemployment in 2000			0.003* (0.001)		0.003* (0.002)
Post reform=0 \times income growth 98-02				0.094*** (0.035)	0.071** (0.034)
Post reform=1 \times income growth 98-02				0.193*** (0.059)	0.189*** (0.069)
Post reform=0 \times Unemployment change 00-02				0.004** (0.002)	0.003* (0.002)
Post reform=1 \times Unemployment change 00-02				-0.002 (0.004)	0.002 (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) Base	(2) Region-Time FE	(3) FE.+ controls	(4) Changes	(5) Abs + changes
High exposure=1 \times Post reform=1	0.028*** (0.006)	0.021*** (0.006)	0.021*** (0.006)	0.017*** (0.005)	0.020*** (0.005)
High exposure=1	0.023*** (0.005)	0.008*** (0.002)	0.008*** (0.002)	0.010*** (0.002)	0.009*** (0.002)
Post reform=1	0.052*** (0.004)	0.056*** (0.007)	-0.018 (0.023)	0.030 (0.021)	-0.061* (0.032)
Post reform=0 \times Income in 1998			-0.000* (0.000)		-0.000 (0.000)
Post reform=1 \times Income in 1998			0.000 (0.000)		0.000** (0.000)
Post reform=0 \times Unemployment in 2000			-0.002** (0.001)		-0.001 (0.001)
Post reform=1 \times Unemployment in 2000			0.005** (0.002)		0.006*** (0.002)
Post reform=0 \times income growth 98-02				0.101*** (0.032)	0.092*** (0.033)
Post reform=1 \times income growth 98-02				0.255*** (0.096)	0.295*** (0.077)
Post reform=0 \times Unemployment change 00-02				0.006*** (0.002)	0.005** (0.002)
Post reform=1 \times Unemployment change 00-02				-0.002 (0.005)	0.003 (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*** (0.00)	0.95*** (0.00)	1.00*** (0.01)	1.16*** (0.01)
Observations	1344	768	256	320
Adjusted R^2	0.993	0.992	0.993	0.962

Notes: The dependent variable is the Denmark Statistics house price index. Robust standard errors are in parenthesis.

As the table shows, the two indices are highly correlated, with a coefficient close to one for the entire sample (column 1) and before 2008 (columns 2-3).

D Online Appendix: Synthetic control analysis

By comparing high and low Exposure municipalities, the above within-Denmark analysis differences out any aggregate policy effects. Yet unbiased policy estimates from such a difference-in-differences design require that general equilibrium effects, as well as spillover or other policy-induced effects, do not impact house price growth in low exposure municipalities. In our case, as IO mortgage penetration was considerable even in low exposure municipalities (see Figure A3), the foregoing assumptions for unbiasedness may not hold. Indeed, suppose the IO mortgage reform positively impacted house price growth in low exposure municipalities. Then our main within-Denmark estimates may be biased downwards, meaning that we would have underestimated the overall true policy effects. In contrast, if the policy increased affordability in high exposure municipalities, for example, and households moved from low to high exposure areas, house prices in low exposure areas may have fallen. These spillover effects may then bias our estimates upwards. While in Figure A9 we show that ex-ante treatment intensity does not predict subsequent population growth changes, other unobserved factors may similarly affect low exposure areas. Such concerns naturally cannot be addressed using only within-Denmark variation. This issue is common in studies that use cross-sectional variation to study national-level policies.¹⁸

To address these issues and provide an estimate of the IO mortgage reform that is free from potential bias due to general equilibrium or other spillover effects within Denmark, we conduct a regional-level analysis of the impact of interest-only mortgages on house prices using the Synthetic control method (SCM) of Abadie & Gardeazabal (2003) and Abadie *et al.* (2010). Researchers are increasingly using the SCM to identify national and regional level policy effects in the housing, finance, and broader macroeconomic literatures in cases where they can construct treatment and control groups from aggregated units (Acemoglu *et al.*, 2016; Born *et al.*, 2019; Gabriel *et al.*, 2020). We employ the SCM to construct counterfactual house price indices for each Danish region using regional housing markets outside of Denmark unaffected by interest-only mortgages.

More formally, the SCM implements a data-driven procedure for comparative case studies to estimate a policy intervention’s causal impact at the aggregate level. The SCM generalizes the

¹⁸For example, previous studies that focus on the U.S. are unable to address such general equilibrium effects due to the lack of a natural counterfactual for the United States (Mian & Sufi, 2012; Berger *et al.*, 2020).

fixed effects (difference-in-differences) estimator as the unobserved unit-specific heterogeneity can vary over time (Abadie *et al.*, 2010), which makes it well-suited to evaluate the regional effects of the introduction of IO mortgages in Denmark. We construct a Synthetic control unit for each Danish region (treated units) from a convex combination of available control units (the donor pool) that best represents each Danish region’s most relevant characteristics during the pre-intervention period. In the wake of the policy, the path of the Synthetic control unit represents the counterfactual estimate of the treated unit in the absence of the policy change. The policy’s causal impact is then calculated as the difference between each Danish region and its Synthetic counterpart (the so-called “Gap” in SCM literature).

Inferential techniques in the SCM can be carried out through placebo studies. A placebo study iteratively assigns the treatment to all other units of the donor pool.¹⁹ Iteratively applying the treatment to members of the control group is comparable to a permutation test where a test statistic is calculated under random permutations of the treatment and control group. The magnitude and rarity of the treatment effect can be assessed relative to the set of estimated placebo effects.²⁰

We apply the SCM approach iteratively using each of the 10 Danish regions as the treated unit.²¹ The donor pool consists of Canadian non-resource (coastal) regions. We select regions from Canada, as Canada had a highly regulated mortgage market without IO mortgages during the sample period.

For ease of exposition and visualization of the results, the outcome variable is now the house price index. Results are unchanged if we use the year-over-year house price growth as in the main analysis. Pre-treatment predictor variables include the year-over-year percentage change in income, the pre-boom house price growth, and the average unemployment rate between 1996 and 2002. In robustness checks, we use different permutations of these predictor variables, including standardizing income growth by country and including the full time series of house prices as a predictor. The results are generally stable across different variations of predictor

¹⁹Placebo studies can also be carried out by assigning the treatment to a random point in time. We follow the literature and assign the treatment to the control units.

²⁰As in Abadie *et al.* (2010, 2011), we will discard any placebo studies where the mean squared prediction error during the pre-intervention period between the treated unit and its Synthetic control in the placebo experiment is more than five times larger than that for the observed experiment.

²¹These Danish regions are Copenhagen City, Copenhagen Surroundings, Northern Zealand, Eastern Zealand, Eastern Jutland, Fyn, West South Zealand, Southern Jutland, Northern Jutland, and Western Jutland. We consider these 10 Danish regions, rather than the 98 Danish Municipalities used in our above within-Denmark analysis, so that the Danish data are comparable to regional housing market data from other countries. The SCM is well suited to estimate causal policy effects in aggregated data.

variables.

Appendix Table D1 provides weights on the potential controls that constitute the Synthetic counterfactual for each treated Danish region. The Synthetic control units for the Danish regions mostly consist of a combination of Newfoundland-Labrador and Quebec. For example, the Synthetic matches for Copenhagen City (CphCity) and the Surrounding Areas of Copenhagen (CphSurroundings) consist solely of Quebec. In general, we view these matches as reasonable. The respective Synthetic control units also generally provide a reasonable match in terms of predictor variables for most regions (see Appendix Table D2).

Moreover, the SCM can approximate the path of house prices for Danish regions before the reform. After IO mortgages are introduced in Denmark, house prices for all Danish regions diverge from their Synthetic counterparts. The scale of this divergence, however, differs across regions. Figure D1 illustrates these dynamics by plotting the results for a high Exposure region, Copenhagen Surroundings, and a low Exposure region, Northern Jutland. In the top panel, we plot the path for each treated region and the path for its Synthetic unit, while in the bottom panel we plot the difference between the treated unit and its Synthetic (gap estimate). Note that the scale of the vertical axis differs across the top and bottom panels of the figure. The bottom panel also includes the largest estimated placebo effects from the permutation tests, marked by gray-dashed lines. The corresponding gap plots for all Danish regions are in Appendix Figure D2. While the sizable impact of the reform is apparent in Copenhagen Surroundings, the gap estimate for house prices in Northern Jutland only marginally extends outside the largest estimated placebo effects from the permutation test after 2006. In other words, the notable heterogeneous house price impacts of the reform readily surface through comparisons of the treated Danish units versus their Synthetic counterfactuals as in Figure D1. Yet the policy also positively affected house prices in Denmark’s low Exposure regions. Below, we further examine the correlation between ex-ante Exposure and subsequent policy-induced house price growth as measured by the SCM gap estimates.

Table D3 provides all of the main numerical Synthetic control estimates by Danish region, including the quarter when the house price index for each treated unit peaked (column 2); the peak value of the house price index between 2003Q4 and 2010Q4 (column 3, noting that the house price indices are normalized to be 100 in 2003Q4, the quarter of policy implementation); the increase in house price growth from 2003Q4 to the peak for each treated Danish region minus

the corresponding change for each of their Synthetic counterfactuals over this same period (column 4); and the share of total house price growth from 2003Q4 to each region’s peak explained by the difference between each treated unit and its Synthetic as measured by column 4 (column 5). In Copenhagen Surroundings, for example, the house price index was 50.36 percent higher than that for its Synthetic counterfactual (see column 4) at the peak of the housing boom. In contrast, Northern Jutland was only 22.39 percent higher than its Synthetic counterpart at its peak. Copenhagen Surroundings also peaked faster than Northern Jutland (2006q2 compared to 2007q3). In column 5, we divide column 4 by the total growth rate for each Danish region between the time of the reform and the peak quarter (column 2) to calculate the share of the total growth explained by the reform. For Copenhagen Surroundings, IO mortgages explain 71.94 percent of the growth in house prices, whereas for Northern Jutland, the share explained by the reform is 52.07 percent. These results thus indicate that the impact of the IO mortgage reform is heterogeneous across regions, as in our previous baseline results.

Figure D3 further documents the heterogeneous impacts of the reform by plotting “Percent above Synthetic at Peak” (Table D3, column 4) against the normalized square meter price in 1998, our previous measure of Exposure, for each region (Table D3, column 1). The figure shows that the estimated impact is positively related to Exposure, consistent with our within-Denmark estimates. Yet the effect of the reform on areas with low Exposure was not trivial. Indeed, the house price indices for regions with the lowest Exposure were still 20-30 percent higher than their Synthetic counterfactuals at the peak in these respective housing markets (see also Table D3, column 4). This suggests that the reform also positively impacted low Exposure areas. A key implication of this result is that the calculations of the policy effects documented in Table 3 from our within-Denmark analysis are likely biased downward and thus conservative in nature.

Overall, the aggregate policy estimates from the within-Denmark and SCM methodologies are similar, with the SCM results being slightly larger. Recall from our within-Denmark analysis (bottom row of Table 3) that the introduction of IO mortgages explains 56 percent of the increase in Danish house prices in aggregate. Using a transaction-weighted average of column 5 of Table D3 (“Share explained by reform”), we now find that the introduction of IO mortgages explains 65 percent of the increase in house prices during the Danish boom. In other words, the two approaches yield similar estimates of the policy’s aggregate impact on Danish housing markets,

where the SCM results are slightly larger. This result, combined with positive policy impacts in low exposure regions documented in the SCM analysis, indicates that any bias emanating from our within-Denmark analysis will make those estimates conservative. Yet the magnitude of any bias is likely small.

As a final check on the similarity between the two approaches, we estimate an equation similar to the within-Denmark equation 2. The dependent variable is a measure of *abnormal housing returns*, calculated as the year-over-year house price growth for each region minus the year-over-year house price growth for its Synthetic unit. This yields a triple-differences research design. We estimate the following dynamic equation at the regional level:

$$\Delta \ln HP_{rt}^{Abnormal} = \sum_{y \neq 2003q3} \beta_t 1\{y = t\} \times Exposure_r + \tau_t + \tau_r + \alpha + \epsilon_{rt}, \quad (8)$$

where $\Delta \ln HP_{rt}^{Abnormal}$ is the abnormal housing returns for region r for time period t , τ_t are time dummies, τ_r are region dummies, and α is a constant. The results are in Figure D4. Standard errors are either clustered by region (gray dashed lines) or computed using wild-bootstrap standard errors clustered by region (black dashed lines). Similar to before, Exposure predicts higher house price growth almost immediately after the reform. The figure shows that a one-standard deviation increase in Exposure predicts 5.3 percent higher quarterly year-over-year house price growth during the peak. The corresponding estimate from the within-Denmark analysis in Figure 5 is 4.2. Overall, the Synthetic control analysis confirms the within-Denmark analysis results – Danish house prices grew more in regions with higher exposure to IO mortgages.

Tables

Table D1: Synthetic Control Weights, Danish Regions

Treated unit	Synthetic Control Region Weight
CphCity	Quebec - 1.00;
CphSurroundings	Quebec - 1.00;
EasternJutland	NewfoundlandLabrador - 1.00;
EasternZealand	NovaScotia - 0.25; Quebec - 0.75;
Fyn	NewBrunswick - 0.74; NewfoundlandLabrador - 0.26;
NorthernJutland	NewBrunswick - 0.51; NewfoundlandLabrador - 0.49;
NorthernZealand	NovaScotia - 0.30; Quebec - 0.70;
SouthernJutland	NewBrunswick - 0.90; NewfoundlandLabrador - 0.10;
WestSouthZealand	Quebec - 1.00;
WesternJutland	NewBrunswick - 1.00;

Notes: Synthetic control unit weights. Treated units consist of Danish regions. The donor pool consists of Canadian regions. Only regions from the donor pool with positive weights are listed.

Table D2: Average predictor variables, Danish regions

	CphCity		CphSurroundings		EasternJutland		EasternZealand	
	Treated	Synthetic	Treated	Synthetic	Treated	Synthetic	Treated	Synthetic
Pre-boom HP growth	44.15	18.1	27.45	18.1	9.74	9.93	23.34	17.21
Income growth	.04	.02	.04	.02	.04	.02	.04	.02
Average Unemployment	.11	.1	.07	.1	.08	.17	.05	.1
	Fyn		NorthernJutland		NorthernZealand		SouthernJutland	
	Treated	Synthetic	Treated	Synthetic	Synthetic	Synthetic	Synthetic	Synthetic
Pre-boom HP growth	3.72	5.09	5.52	6.61	23.4	17.02	2.98	4.05
Income growth	.03	.02	.03	.02	.04	.02	.03	.02
Average Unemployment	.08	.13	.08	.14	.05	.1	.06	.12
	WestSouthZealand		WesternJutland					
	Treated	Synthetic	Treated	Synthetic				
Pre-boom HP growth	22.43	18.1	2.3	3.39				
Income growth	.04	.02	.03	.02				
Average Unemployment	.08	.1	.05	.11				

Notes: Synthetic control average predictor values. Treated units consist of Danish regions. The donor pool consists of Canadian regions.

Table D3: Summary of Results - Gap in House Prices

Treated unit	(1) Regional Exposure	(2) Peak House Price Quarter	(3) Peak HP Index (2003Q4=100)	(4) Percent above Synthetic at Peak	(5) Share explained by reform
CphCity	4.45	2006q2	175.4	54.62*	72.83
CphSurroundings	4.80	2006q2	170.6	50.36*	71.94
EasternJutland	3.45	2007q1	162.3	40.06*	64.61
EasternZealand	4.34	2006q4	159.9	40.65*	64.52
Fyn	2.87	2007q4	158.2	38.57*	68.88
NorthernJutland	2.44	2007q3	140.8	22.39	52.07
NorthernZealand	4.52	2007q1	168.3	46.82*	69.88
SouthernJutland	2.58	2007q4	148.0	31.93*	69.41
WestSouthZealand	2.46	2007q3	148.0	24.55	50.10
WesternJutland	2.38	2008q1	144.0	29.16*	69.43
Weighted Mean (Average) across regions					65.24

Notes: The table shows Exposure (column (1), defined as the house price level in 1998 divided by its standard deviation), Peak House Price Quarter (column (2); the quarter when the house price index for each treated unit reached its highest value), the Peak House Price (HP) Index (column 3; the peak value of the house price index between 2003Q4 and 2010Q4, where the house price indices are normalized to by 100 in 2003Q4, the quarter of policy implementation), the Percent above Synthetic at Peak (column 4; defined as the ratio between the house price index of the treated unit and the Synthetic unit at the peak quarter minus 1), and the share of average house price growth explained by the reform (column (5), defined as Percent above Synthetic at Peak (column 4) divided by the total growth rate for the treated unit between the reform and the peak). An asterisk in column 4 indicates that the “Percent Above Synthetic at Peak” is larger than all estimated placebo effects. The donor pool consists of Canadian regions.

Figures

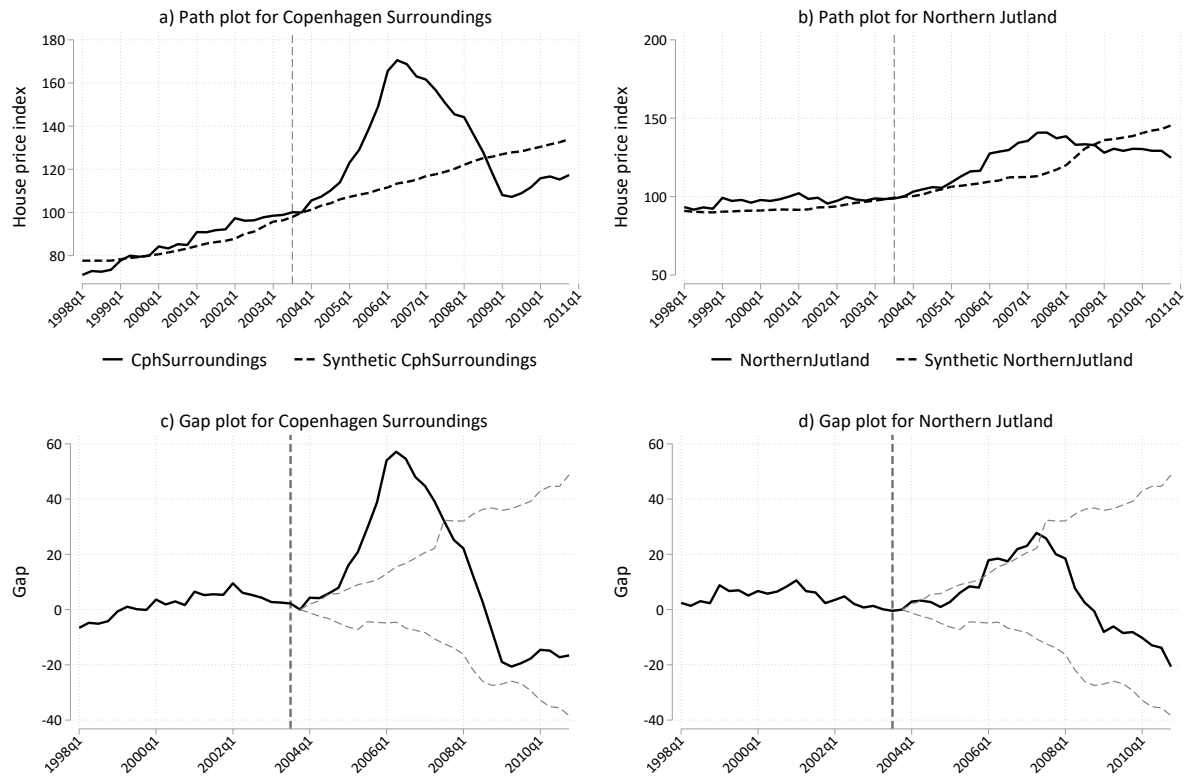


Figure D1: Synthetic Control – House price indices for Copenhagen Surroundings and Northern Jutland

Notes: House price growth for different regions. In the top half, panels a) and b) plot the evolution of the house price indices for the treated and Synthetic units of Copenhagen Surroundings and Northern Jutland (house price indices are normalized to be 100 in 2003Q4, the year-quarter of policy implementation). In the bottom half, panels c) and d) plot the difference between the treated unit and its Synthetic counterfactual, where the gray-dashed lines represent the largest estimated placebo effects for every time period when the treatment is iteratively applied to each member of the donor pool. The dashed vertical line signifies the policy implementation date in 2003Q4. The donor pool consists of all regions in our final sample.

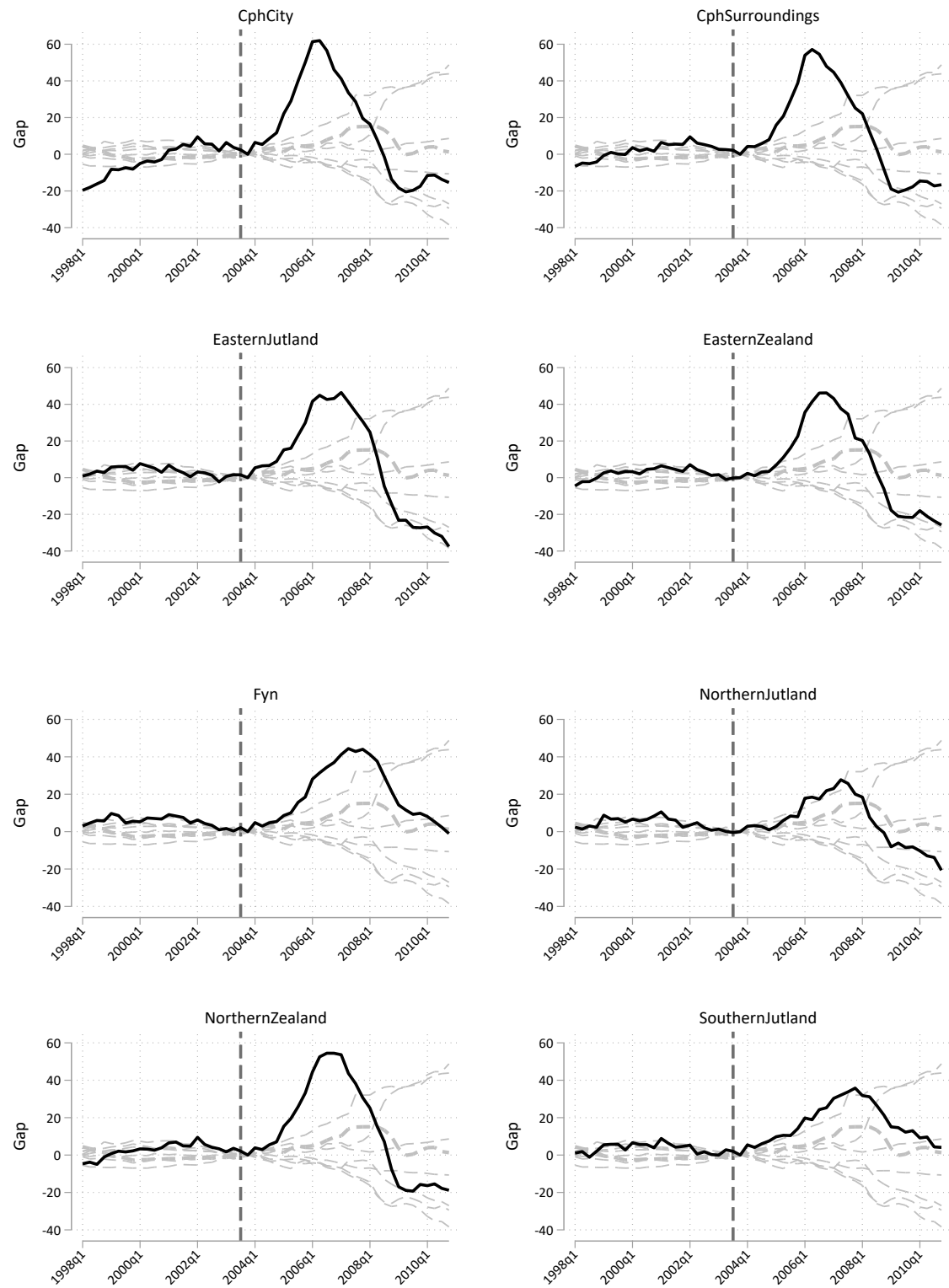


Figure D2: Synthetic control results for Danish regions – Canadian regions in the donor pool

Notes: The figure plots Synthetic control gap plot, including all permutation tests, for Danish regions. The outcome variable is the house price index. The donor pool consists of Canadian regions regions/sub-regions.

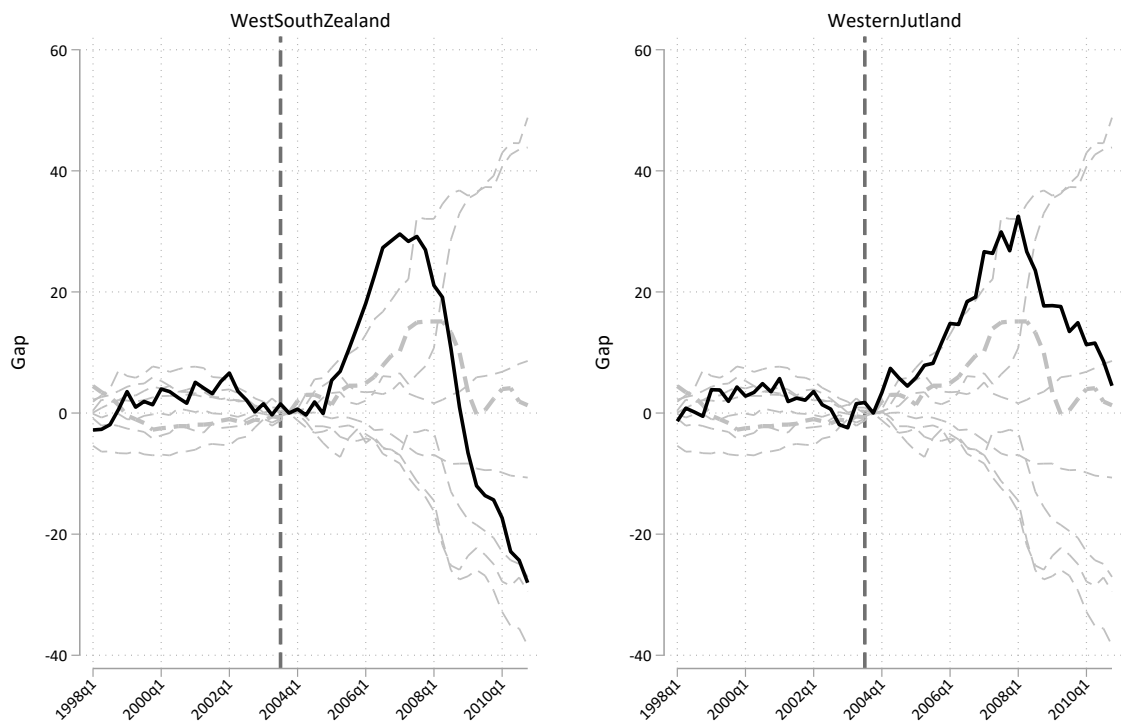


Figure D2 continued: Synthetic control results for Danish regions – Canadian regions in the donor pool

Notes: The figure plots the Synthetic control gap plot, including all permutation tests, for Danish regions. The outcome variable is the house price index. The donor pool consists of Canadian regions regions/sub-regions.

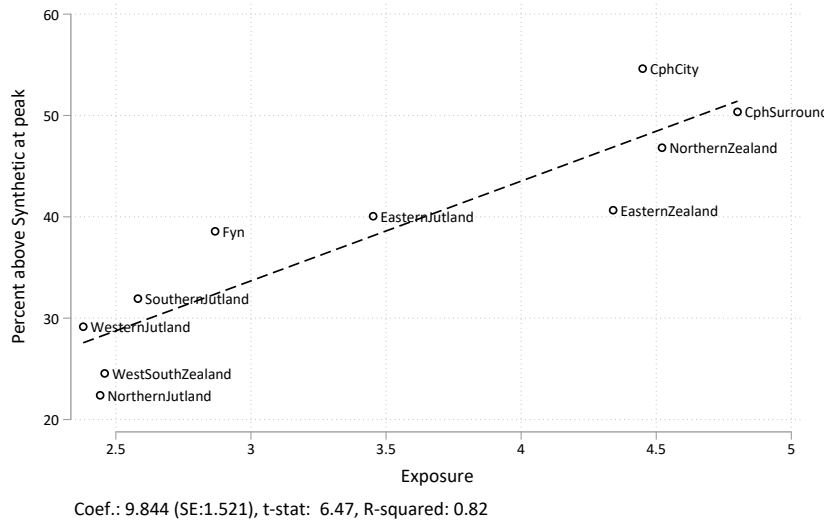


Figure D3: Percent above Synthetic against Exposure

Notes: The figure plots the average regional square-meter price in 1998 divided by its standard deviation (Exposure) against Percent above Synthetic from Table D3 for each Danish region. The coefficient, robust standard errors, t -stat and R-squared are from a regression of: $PercentAbove_r = \beta Exposure_r + \epsilon_r$, where $PercentAbove_k$ denotes the Percent above the Synthetic at the peak of the housing boom for region r , $Exposure_r$ is the average regional square-meter price in 1998 divided by its standard deviation and ϵ_r is an error term.

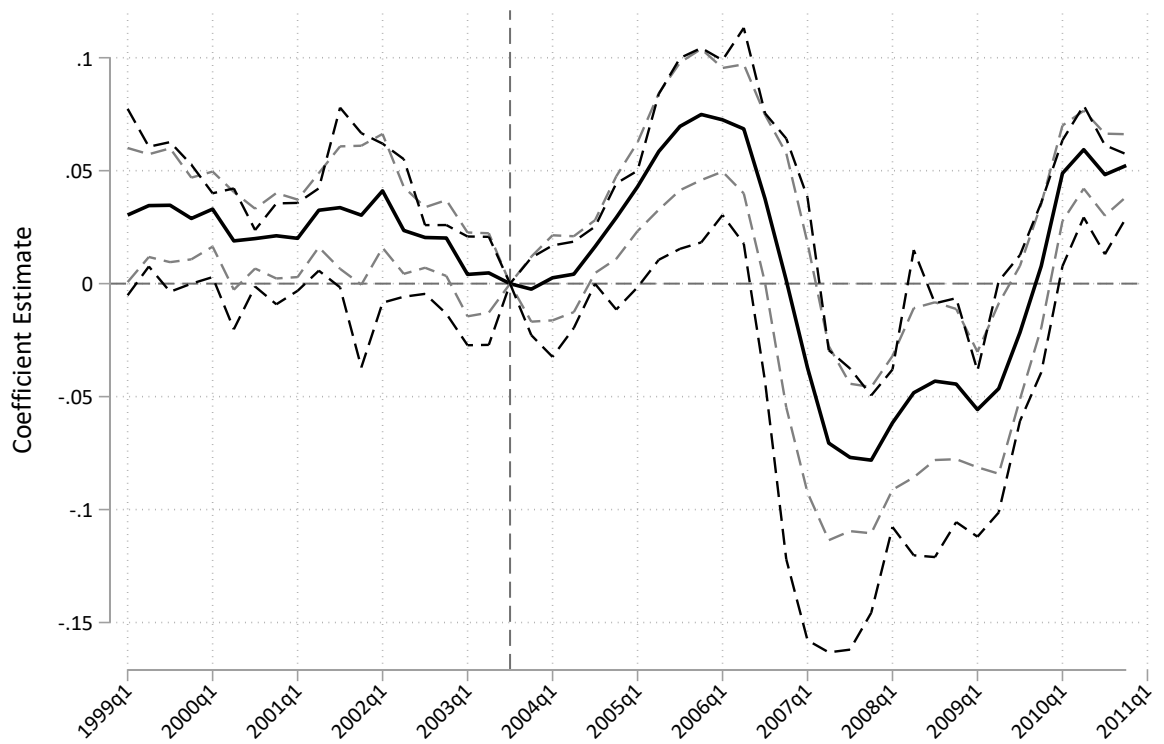


Figure D4: Abnormal housing returns and exposure

Notes: The figure plots the difference-in-differences coefficients, β_t , from Equation 8. The dependent variable is the abnormal housing return, calculated as $\Delta \ln HP_{kt}^{Treated} - \Delta \ln HP_{kt}^{Synthetic}$. The gray-dashed lines show 99 percent confidence intervals computed using robust standard errors clustered by region, and the black-dashed lines indicate 99 percent confidence intervals computed using wild-bootstrap standard errors clustered by region as in [Roodman et al. \(2019\)](#).