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THE HOUSING WEALTH EFFECT: QUASI-EXPERIMENTAL EVIDENCE

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Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

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JEL Classification: D12, E21, E32, E44, E60

Keywords: Consumption, House prices, Marginal propensity to consume, Housing wealth, Collateral effect, House price elasticity

Roine Vestman - roine.vestman@ne.su.se
Stockholm University and CEPR

Jesper Bojeryd - bojeryd91@gmail.com
University of California, Los Angeles

Björn Tyrefors - bjorn.tyrefors@ifn.se
Research Institute of Industrial Economics (IFN)

Dany Kessel - dany_kessel@hotmail.com
Södertörn University, Sweden

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March 24, 2023

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*Vestman (corresponding author): Stockholm University and CEPR; email: roine.vestman@su.se. Bojeryd: University of California, Los Angeles; Tyrefors: Research Institute of Industrial Economics (IFN). We thank Peter Fredriksson, Soren Leth-Petersen, Kurt Mitman, Rodney Ramcharan, Morten Ravn, Kathrin Schlafmann, Amir Sufi, and seminar participants at Danmarks Nationalbank, Lund University, Norges Bank, Stockholm University, the Greater Stockholm Macro Group, the Research Institute of Industrial Economics (IFN), the 2019 SED Annual Meeting, the Swedish House of Finance, Sveriges Riksbank, and UCLA. We are grateful for generous funding from Jan Wallanders och Tom Hedelius stiftelse, Marianne and Marcus Wallenberg Foundation, the Royal Swedish Academy of Sciences, and Vinnova. The computations were enabled by resources in project SNIC 2021/22-584 provided by the Swedish National Infrastructure for Computing (SNIC) at UPPMAX, partially funded by the Swedish Research Council through grant agreement no. 2018-05973. All data used in this research have passed ethical vetting at the Stockholm ethical review board and have been approved by Statistics Sweden. Karin Ek, Mambuna Njie, and Niklas Nordfors provided excellent research assistance.

1 Introduction

A fundamental topic in economics that has received a great deal of attention since the global financial crisis is how the housing and mortgage markets interact with the macroeconomy. In particular, there is a rich literature on how housing booms and busts affect household consumption—commonly referred to as housing wealth effects.

The early theoretical literature argued that housing was a particular asset that would generate no or small effects. Nevertheless, empirical studies have found mixed results, depending on the use of aggregate data (e.g., Carroll et al., 2011; Case et al., 2013; Guerrieri and Iacoviello, 2017) or household-level data (e.g., Campbell and Cocco, 2007; Attanasio et al., 2009; Disney et al., 2010; Browning et al., 2013) or on the interpretation of estimates.[1] In fact, many of the early contributors to the literature remained skeptical of their estimates due to weak identification.

To date, the most credible estimates are based on instrumental variable regressions that rely on regional variation in the elasticity of housing supply (Mian et al., 2013; Aladangady, 2017; Kaplan et al., 2020b; Aruoba et al., 2022) or city-wide variation in sensitivity to regional house prices (Guren et al., 2021). Such estimates make fairly strong assumptions about consumption demand factors being either observed by the econometrician or uncorrelated with supply elasticities (Davidoff, 2016).

This paper adds to this literature in three ways. The first contribution is that our estimates of the housing wealth effect are based on a novel identification—a quasi-natural experiment. We use unanticipated news from political bargaining in Stockholm, Sweden, regarding the continued operation of the city airport Bromma (or Bromma Airport) to isolate a causal effect from a relative change in house prices, which is a function of distance from the airport's noise contour. It is well-documented that the airport is a negative externality to its closest surrounding, and we show that this is capitalized into house prices within one quarter of the news announcement. Using a data set on all transactions of single-family houses in

[1] See Footnote 2 of Berger et al. (2018) for a literature review and Table A.1 for a review of estimates.

Stockholm, we document a price divergence, or a relative price fall, of 19.4 percent close to the airport's noise contour relative to further away. Our identification is novel in that it relies on an exogenous change of a negative externality that capitalizes locally into house prices. The source of variation is thus conceptually similar to e.g., Chay and Greenstone (2005) and Currie et al. (2015), but novel in the context of households' consumption response. The announcement is ideal to measure households' consumption response because the effect is contained in a geographically granular area and the timing does not coincide with any important events that would differentially affect the treatment versus the control group.[2] We cannot find support that any other policy or event would have improved the wealth and income of households in proportion to the distance to the noise zone of the airport. Furthermore, the shock to wealth was as good as permanent because of the length of the extension of the airport. This is important for a wealth effect; it has to be permanent in nature to influence current consumption. These features of the quasi-experiment set our study apart from other studies.

We use the source of house price variation together with a rich household-level data set. This data set includes the geographic location of primary residence, household balance sheet information such as loan-to-value ratio and types of loans, and purchases of new cars at a quarterly frequency. We find that single-family house owners close to the airport reduce their purchase values of new cars by 7.7–8.5 percent relative to homeowners who reside further away.[3] A two-sample IV approach establishes a sizeable elasticity of 0.39 among households that buy a new car, which translates into a marginal propensity for expenditures on cars (what we call a "car MPX") of 2.5 cents per dollar loss in housing wealth per new car purchase.[4] This implies an aggregate MPX in new cars of 0.12 cents per year for each dollar

[2]We limit our experiment to before the financial crisis became global (before the bankruptcy of Lehman Brothers) to avoid any disruptions in credit conditions that are not due to the collateral value of homes.

[3]We use the term "homeowner" interchangeably with "single-family house owner."

[4]We adopt the terminology of Laibson et al. (2022) and let MPX stand for the marginal propensity for expenditure and denote by "car MPX" the marginal propensity for expenditure on cars. Analogously, we use the terms non-car MPX and total MPX in the model section. We clarify in the text when we refer to other concepts of marginal propensities. For a recent discussion on these, see Kaplan and Violante (2022).

lost in housing wealth and, assuming an equal response in used cars, implies an overall car MPX of 0.38 cents per year per dollar loss. The response of homeowners decreases with the distance to the airport's noise contour, and placebo tests show no response for households that live in apartments—two features that support our identifying assumption.

Our second contribution is that our data set enables analysis of the heterogeneous responses and the financing decisions of car purchases. This allows us to separate between the channels of the housing wealth shock. We find that homeowners with a loan-to-value (LTV) ratio above 50 percent respond twice as much, and that it is households with small bank deposits that respond. This heterogeneity is consistent with general borrowing behavior when purchasing a car. Forty-seven percent of a new car's value is financed with some kind of credit, and mortgages make up about 71 percent of these credits. However, there is substantial cross-sectional variation depending on households' balance sheets. Households with high LTV ratios borrow one-third less (per dollar car), and the difference is almost entirely explained by a reduction in the use of mortgages. These findings support the view that binding borrowing constraints and the collateral channel are important for the observed total effect. Our results are complementary to Mian et al. (2013), who find that housing wealth shock responses in autos vary with the level of net worth at the ZIP code level, and to Aladangady (2017), Graham and Makridis (2023), and Aruoba et al. (2022), who find larger responses among credit-constrained households.

Our third contribution is that we relate our empirical findings to economic theory. Our estimates of housing wealth effects are the first ones generated from a quasi-experiment that resembles a partial equilibrium house price shock in the sense of Berger et al. (2018) and Guren et al. (2020). This is because the shock is geographically local (even granular); thus, general equilibrium effects are likely absent.[5] We use a state-of-the-art life-cycle consumption

[5] Our identification is strikingly similar to the one proposed by Carroll et al. (2011), page 71: "[...] to isolate a 'pure' housing wealth effect, one would want data on spending by individual households before and after some truly exogenous change in their house values, caused for example by the unexpected discovery of neighborhood sources of pollution. The perfect experiment observed in the perfect microeconomic dataset is however not available."

savings model that builds on Berger and Vavra (2015), Berger et al. (2018), McKay and Wieland (2021), and Attanasio et al. (2022).[6] The model includes elements that are relevant to our empirical setting: costly adjustment of cars, long-term mortgages, and an information friction for house prices.

We use the model to investigate responses in total consumption expenditure and cars to a partial equilibrium house-price shock. In simulations, we find that a shock of 19.4 percent to house prices leads to a reduction in the value of cars purchased by 6.1 percent in the next four quarters. This corresponds to a new-car MPX of 0.20 cents for each dollar change in housing wealth. This is close to our empirical estimate of 0.12 cents per dollar and inside the 95-percent confidence interval.

We establish a version of the intertemporal smoothing of durables demonstrated by McKay and Wieland (2021), applied to the context of housing wealth shocks (instead of monetary policy). The response in cars, as a share of the total expenditure response, is high at first: 45–72 percent in the first four quarters after the house-price shock and then gradually falls in the following years. The spending pattern in durables relative to total expenditure is a complementary finding to Laibson et al. (2022), who deduce the relationship between the marginal propensity to consume and the marginal propensity for expenditures.

The explicit modeling of a long-term mortgage enables us to use the model to distinguish between the role of a change in housing wealth and the role of changes to borrowing capacity. We find that changes to borrowing capacity account for 93 percent of the car MPX and 83 percent of the total MPX. In other words, if all households would be unconstrained (and remained unconstrained after the shock), the short-term expenditure response would only be one-sixth as large. This finding is consistent with our heterogeneity analysis and several previous studies that have emphasized collateral effects (e.g., Leth-Petersen, 2010; DeFusco, 2018; Aydin, 2022; Sodini et al., 2022). Finally, the model allows us to pinpoint several

[6]Other seminal contributions to the literature on (S,s) models and their applications to households' durable goods or car purchases are Lam (1991), Eberly (1994), Bar-Ilan and Blinder (1992), Caballero (1993), Adda and Cooper (2000), Attanasio (2000), Hassler (2001), Bertola et al. (2005), and Schiraldi (2011).

important aspects of different empirical settings: the shock size, the measurement period, the regression specification, and whether it is conducted in normal times or crisis times, where the latter is likely to influence households' beliefs and awareness. If households are immediately aware of a house price shock, the consumption response is twice as large compared to our setting. In the first year, the total MPX can even be three times as large. These factors are important to reach a consensus view on the housing market's role in fluctuations in aggregate demand.

The rest of the paper is organized as follows. Section 2 reviews the institutional setting, the quasi-experiment, and the reaction of house prices to the announcement; Section 3 describes the data and discusses the empirical strategy. Section 4 reports our empirical results. Section 5 presents the model and our insights from it, and Section 6 concludes.

2 The Quasi-Experiment

This section describes the political process leading to the renewal of Bromma Airport's operating contract and establishes that it caused a decrease in house prices close to the airport.

2.1 History and political governance

Bromma Airport is the city airport of Stockholm. The airport has one runway and is located close to the city in an area that is otherwise dominated by single-family housing. It is Sweden's third-largest airport in terms of takeoffs and landings.[7] Since the late 1990s and early 2000s, however, there was a general perception that Bromma Airport would be closed, at the latest in 2011 when the operating contract would expire.[8]

[7]In the years between 2006 and 2015, it had about 60,000 takeoffs and landings per year. In the early years, Bromma was Sweden's largest airport, but after the Arlanda airport opened in 1959, Bromma Airport saw a sharp decrease in traffic. In 1992, the center-right national government deregulated commercial airfare, and Bromma Airport increased in importance again.

[8]There was a series of reports planning for the shutdown. In 1989, Stockholm Municipality presented a major report proposing the closure of the airport by 1996 and the use of the land for housing (Stockholm,

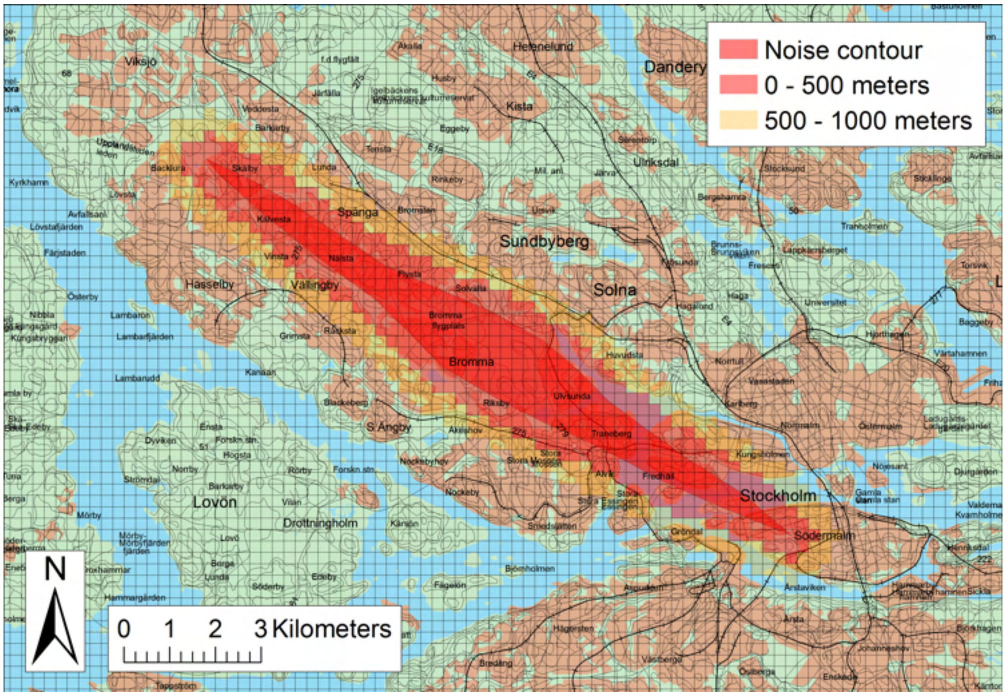
Stockholm Municipality is the owner of the land on which the airport is located. The municipality has been leasing the land to the airport since 1936. The only political party in the municipality that consistently has been in favor of renewing the contract beyond 2011 is the conservative party (*Moderaterna*). In the 2006 election, this party increased their seats by more than 50 percent in Stockholm Municipality, from 27 to 41 seats out of 101. This was the best result ever for the party. The election outcome boosted the bargaining power of the party in the negotiations with the other parties in the center-right coalition. Rapidly, and behind closed doors, the municipality negotiated a new contract with the airport, extending the airport's operations to 2038. The contract was announced at a press conference in September 2007. All the opposition parties issued minority reports before the municipal council, calling the process a coup. The news about the new contract was widely reported in local media.

The reason for the political controversy over Bromma Airport's existence is its geographic location. The airport is surrounded by residential housing and is a substantial negative externality on its surroundings—not the least in terms of noise. Figure 1 displays a map of the noise contour around Bromma Airport as a dark red ellipse. The area inside the noise contour is frequently exposed to noise levels of at least 70 decibels.[9] The contour is regarded as the best approximation of the area that is exposed to hazardous noise, as confirmed in a case in the Land and Environment Court (Miljööverdomstolen, 2010). The court ruled that, within this area, the regulatory agency that oversees aviation in Sweden (LFV) must reimburse sound insulation to homeowners. The measurement error of the border is +/− 100

1989). In 1994, the national government put together a commission to evaluate how fast Bromma Airport could be phased out (Kommunikationsdepartementet, 1996). In 2000, the Swedish Civil Aviation Administration presented a report on how it would eventually close Bromma Airport by 2011 (Luftfartsverket, 2000).

[9] The decibel scale is logarithmic. Sixty decibels correspond to a conversation in an office. Seventy decibels are twice as loud. A vacuum cleaner is 70 decibels, which makes it difficult to have a conversation. A Boeing 737 generates 97 decibels before landing at a distance of one nautical mile (1,853 meters). Bromma Airport has permission to service takeoffs and landings that generate more than 70 decibels between 6 a.m. in the morning and 10 p.m. in the evening. Each aircraft type that operates at Bromma Airport must be noise tested, and the upper bound is 89 decibels. A noise level of 89 decibels is well above the threshold to mandate ear protection at Swedish workplaces. It is so loud that people would not be able to have a screaming conversation.

Figure 1: Noise propagation around Bromma Airport



Note: The map shows the noise propagation around Bromma Airport along its runway which stretches from northwest to southeast. The dark red area is referred to as the noise contour. Inside the noise contour, the Land and Environment Court estimated the noise to exceed 70 decibels. The treatment region in our baseline specification is extended also to include the area which is less than 1,000 meters away from the noise contour. We call the treatment region the noise area. We locate house transactions and households on the grid. Each square is 250 × 250 meters. Source: Miljödomstolen (2006) and own analysis.

meters.

Externalities from airports are known to be severe. Many studies find that the noise from aircraft is more hazardous than, for example, noise from trains or cars for a given daily average decibel level (Guski et al., 2017; Miedema and Oudshoorn, 2001).[10] This is because the house facades are not able to shield the noise from above, but also because the aircraft noise is intermittent and unpredictable. Furthermore, there may be fear of dumping of fuel and accidents at takeoff and landing.

We acknowledge that the magnitude of the negative externality is not perfectly proxied by the noise contour that corresponds to 70 decibels. To limit misclassification of the treatment

[10] We are not the first to use the fact that a negative externality capitalizes into real estate prices. Chay and Greenstone (2005) and Currie et al. (2015) use it to measure the cost of nearby air pollution and toxic plants.

group, we choose to include the area up to 1,000 meters away from the border of the contour in our baseline definition of the treated area. Households residing in this area are exposed to at least 60 decibels following the transmission formula for noise:

$$L_2 = L_1 + 10 \times \log_{10}\left(\frac{r_1^2}{r_2^2}\right), \tag{1}$$

where L_1 denotes the decibel level at a distance of r_1 meters from the source and L_2 is the decibel level at distance r_2.

The baseline treatment area, marked as yellow in Figure 1, is henceforth referred to as "the noise area." In the following, we refer to single-family houses as simply "houses" and households as being located either inside or outside the noise area. We argue that the negative externalities should be negligible outside this area, enabling us to construct a control group of unaffected households.

2.2 Data on transactions of houses and apartments

To measure the treatment effect on house prices, we acquire transaction data on houses and apartments. First, we obtain data on all transactions of houses in Stockholm Municipality from the Land Survey Agency (*Lantmäteriet*). The data set covers all transactions and prices in the municipality from January 2004 to December 2012. The data set includes a large number of characteristics for each house, such as the transaction date, the geographic location in the form of GIS coordinates, the area of the building plot, and information about the houses, such as living area, supplementary area, age, and an index of the attractiveness of the location and the house standard. This index is used by the Swedish Tax Agency (*Skatteverket*) to assess the value of the property. The GIS coordinates allow us to compute the distance between the house and the noise contour.

Stockholm Municipality has about 410,000 dwellings, of which approximately 90 percent are apartments, either rentals or co-op shares,[11] and 10 percent are houses. Therefore, we also collect data on transactions of co-op apartments from *Mäklarstatistik* (a data-collection company owned by the Association of Swedish Real Estate Agents). We have data from 2005 to 2010. In this data, we have the transaction date, GIS coordinates, living area, number of rooms, and price.

It is important to distinguish between co-op apartments and single-family houses. Apartments are different because they have no private outdoor area, which makes them less exposed to noise. Furthermore, the co-op buildings are at least three stories high and have thicker walls and more insulation due to fire safety regulations. They are solid concrete buildings and not made of wood like many single-family houses. Therefore apartment owners are expected to suffer less from noise, and hence apartment prices should not react as much upon the announcement of the renewal of the airport contract.

Table 1 shows summary statistics of the transaction price and the living area, the two variables that exist in both data sets. Panel A reports statistics for the full sample period. The average transaction value of a single-family house is SEK 3.4 million, and the average living area is 117 square meters. Apartments are cheaper, about SEK 2.4 million, and are smaller; the living area is 63 square meters on average.

The full sample period includes the global financial crisis and the transaction volumes dropped at that time. Since we aim to isolate the effect of the airport contract's renewal in September 2007, we focus on the period up until the bankruptcy of Lehman Brothers in 2008Q3. Panel B reports the statistics for this period. The mean and standard deviations of the living areas are similar when comparing the two periods, but prices are lower for the pre-crisis period, which is consistent with the secular increase in Swedish home prices. Table B.1 reports statistics of additional variables in the data sets. Tables B.2 and B.3 compare statistics for transactions inside and outside the noise area before the renewal of the airport

[11] In Sweden, apartment buildings and the units are co-owned through associations. When a household buys an apartment, they buy a co-op share in that association linked to a specific apartment. According to Statistics Sweden, all tenant-owned dwellings in multi-dwelling buildings in Stockholm were co-op apartments during the period of interest. This distinction is not of importance for our analysis.

Table 1: Summary statistics for single-family house and co-op apartment transactions

	Panel A: Full sample			
	Single-family houses		Co-op apartments	
	Price	Living area	Price	Living area
Mean	3351	117	2409	62.6
Std. dev.	2244	38.1	1698	29.3
Obs.	19,777	19,666	85,168	85,168

	Panel B: Before 2008Q3			
	Single-family houses		Co-op apartments	
	Price	Living area	Price	Living area
Mean	2947	117	2292	62.3
Std. dev.	1955	38.5	1620	29.6
Obs.	11,321	11,308	50,312	50,312

Note: Transactions of single-family houses start in 2004Q1 and end in 2012Q4. Transactions of co-op apartments start in 2005Q1 and end in 2010Q4. Amounts are in SEK 1,000 and living area in square meters. For additional variables see Table B.1.

contract. Prices of both houses and apartments are similar in the two areas before the renewal.

2.3 Empirical strategy for measuring the house-price shock

The identifying variation comes from the unexpected renewal of the airport contract in combination with the location of the residence. Dwellings close to the airport suddenly faced at least another 30 years of negative externalities. Our outcome variable is the natural logarithm of prices of dwellings sold for household i in time period t, $\log(\text{Price}_{it})$. We define the treatment period to start on October 1, 2007, also denoted as 2007Q4. The variable Noise Area_i defines the treatment group in the sense that the variable takes on a value of one if the dwelling is located within 1,000 meters from the noise contour (and otherwise zero). The variable Post_t is zero up until and including 2007Q3 and one thereafter. A standard difference-in-difference equation reads

$$\log(\text{Price}_{it}) = \alpha + \delta \, \text{Noise area}_i \times \text{Post}_t + \theta \, \text{Noise area}_i + \eta \, Z_{it} + \gamma_t + \varepsilon_{it}, \tag{2}$$

where α is an intercept. γ_t indicates year-quarter-time fixed effects, Z_{it} is a vector of data on the transacted unit, and ε_{it} is an error term. Thus, θ measures the average log point (percent) difference in prices between the dwellings inside and outside the noise area. The coefficient δ is of primary interest. It measures the log point (percent) change in prices due to the renewal of the contract. Standard errors are based on clustering of error terms at the level of the 250×250 meters grid.

2.4 Estimates of house-price effects

Table 2 reports estimates of equation (2). Starting with the effect on prices of single-family houses, Column (1) reports the results for the pre-global financial crisis period with no control variables. The estimated price decrease for the houses within the noise zone is −21.4 percent. Adding control variables has no impact on the estimate.[12] Thus, there is no evidence of any compositional bias (i.e., different types of houses being sold before due to the airport continuation). If we use the longer sample period, we see a slight decrease in the estimate to −19.4 percent (Column (3)). We take this to be our baseline first-stage effect. We conclude that the effect on house prices is highly significant (t-stats > 5 in all specifications) and robust across specifications. Columns (4) to (6) report the same estimates for co-op apartment prices. In contrast to the effects on single-family houses, there are no statistically significant effects, and the estimates are close to zero.

To validate our DiD specification, we test for parallel trends in outcomes prior to 2007Q3. We augment equation (2) with yearly time-dummy variables and define them based on time relative to treatment. Since the renewal of the operating contract was disclosed in late

[12] We lose 13 observations due to missing data in the control variables.

Table 2: Effect on house prices

	Log of house prices			Log of apartment prices		
	(1)	(2)	(3)	(4)	(5)	(6)
Noise area_i	-0.214^{***}	-0.214^{***}	-0.194^{***}	0.027	0.002	-0.019
$\times$ Post_t	(0.040)	(0.035)	(0.028)	(0.018)	(0.014)	(0.014)
Noise area_i	0.202^{***}	0.211^{***}	0.222^{***}	0.042	0.135^{***}	0.135^{***}
	(0.043)	(0.035)	(0.034)	(0.037)	(0.031)	(0.032)
Observations	11,321	11,308	19,666	50,312	50,248	85,048
R-squared	0.102	0.330	0.374	0.049	0.430	0.437
Pre-GFC	Yes	Yes	No	Yes	Yes	No
Controls	No	Yes	Yes	No	Yes	Yes

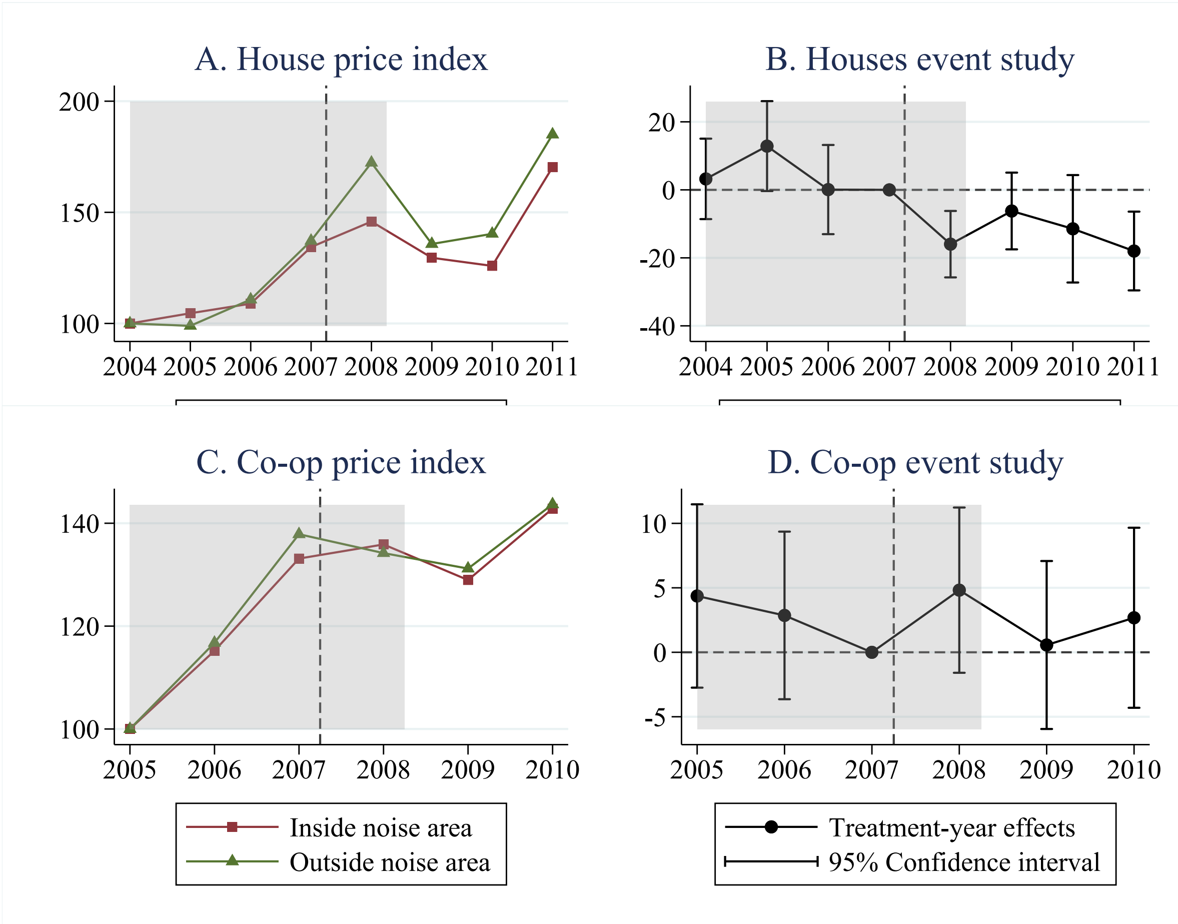
Note: The set of control variables for the housing prices regression includes a polynomial in age, standard, plot area, living area, and non-living area, and the change in the amount of property tax to be paid due to the tax reform coming into effect in 2008. For the apartment prices, regressions controls are living area and number of rooms. Errors cluster robust at the level of the 250 × 250 meters grid. The baseline time window is 2004Q1–2008Q3 for single-family houses and 2005Q1–2008Q3 for co-op apartments (pre-GFC). The long time window extends the baseline time window to 2012Q4 and 2010Q4, respectively. ${}^{*}p < 0.1$, ${}^{**}p < 0.05$, ${}^{***}p < 0.01$.

September 2007, we define every 12-month period as running from October 1 of year $t-1$ to September 30 of year t. We interact this set of time-dummy variables with Noise Area_i and omit the dummy variable for 2006Q4–2007Q3 so it serves as the reference level for prices outside the noise area.[13] That is, for every 12-month period, we estimate a treatment effect relative to the 12 months just before the contract renewal.

The top panels of Figure 2 show single-family house prices. Panel A displays house-price fluctuations inside and outside the noise area. The price series are indexed to 100 in 2004. The series follows the same trend from 2004Q1 to 2007Q3 but after the new contract, the series diverge. Panel B displays the corresponding event study estimates on house prices using the specification in Column (3) in Table 2. Common pre-trends up to 2007Q3 cannot be rejected, and the immediate effect in 2007Q4–2008Q3 is -20 percent. The price difference remains up until 2011. However, since our aim is to isolate the effect of the contract's renewal in September 2007, we focus on the period before the financial crisis became global—that is, from 2004Q1 to 2008Q3 (the area shaded in gray). For completeness, the bottom panels of

[13] In our graphical illustrations, 2006Q4–2007Q3 is referred to as 2007 and so forth unless explicitly stated otherwise.

Figure 2: Effect on house prices



Note: Panels A and C show house and apartment prices indices outside and inside the noise area, respectively. Panels B and D show the corresponding annual treatment effects in log points for house and apartment prices, respectively. The timing on the horizontal axis is shifted by one quarter. That is, 2007 refers to 2006Q4–2007Q3 and so forth. The shaded grey area marks the main sample period (2004Q1–2008Q3). The set of control variables for the housing prices regression includes age, standard, plot area, living area, and non-living area, and the change in the amount of property tax to be paid due to the tax reform coming into effect in 2008. For the apartment prices, regressions controls are living area and number of rooms. The regression specification is an augmented version of equation (2) and is described in the text.

Figure 2 show co-op apartment prices, for which there is no treatment effect.

To support the credibility of our identifying assumption that the announcement was unanticipated, Figure B.1 reports a version of Panel B of Figure 2 estimated at quarterly frequency. The figure shows that the response of single-family house prices was immediate in the fourth quarter of 2007.

To conclude, the renewal of the contract had a direct impact on single-family house prices but not on apartment prices. The results are robust when we account for compositional changes in transacted units, and several additional aspects speak in favor of a causal interpretation. Going forward, single-family house owners are the focus of our analysis.

3 Household data

Our main analysis uses registry-based panel data provided by Statistics Sweden that covers all households whose residential address is located in the municipality of Stockholm. This data set has information on age, household size, balance sheet items, and car transactions. Most of the information has an annual frequency, but for cars we have exact transaction dates.

Table 3 reports statistics on Stockholm's house owners at the end of 2006, just before the renewal of Bromma Airport's contract.[14] The first two columns report statistics for the full sample. The table also reports the statistics for those outside the noise area (Columns (3)–(4)) and those inside (Columns (5)–(6)). Panel A reports that house owners inside the noise area have on average 2.89 members. Households outside have marginally fewer (2.83). The average age of the oldest household member is 53 years. Panel B shows that in terms of wealth and other balance sheet metrics, the two groups are very similar. Average financial wealth is SEK 1,020,000 versus SEK 1,090,000. By Swedish standards, these are substantial amounts of financial wealth; Vestman (2019) reports that average financial wealth among house owners equals SEK 448,400 for the period 2000–2007.

Based on Statistics Sweden's appraisal model for single-family houses, both groups' housing wealth is just over SEK 3.9 million. We know the houses' location on the 250 × 250 meters geographical grid and can thus infer the housing wealth shock implied by the quasi-experiment.

Financial institutions report to the Swedish Tax Agency each credit that households

[14] We impose five sample restrictions and cover 86 percent of the house owners in our analysis. A detailed description of the sample restrictions can be found in Table C.1.

Table 3: Summary statistics for single-family house owners

	Full sample		Outside noise area		Inside noise area	
	Mean	SD	Mean	SD	Mean	SD
Panel A. Geographic location and sociodemographics						
Distance from noise contour (meters)	3070	2450	4020	2120	321	338
Age	53.1	13.5	53.3	13.5	52.6	13.4
Household size	2.85	1.33	2.83	1.33	2.89	1.34
Panel B. Balances sheets and income						
Total wealth	5060	9150	5090	9580	4960	7750
Housing wealth	3990	7230	4000	7390	3940	6750
Financial wealth	1070	4620	1090	5080	1020	2880
Bank deposits	306	954	308	1014	308	1010
Total debt	1300	2700	1300	3030	1280	1350
Mortgage	1180	2590	1180	2920	1160	1230
LTV (%)	39.0	75.6	39.3	84.5	38.2	39.6
CLTV (%)	40.5	64.8	40.6	69.6	40.2	48.1
Labor income	605	692	604	734	610	551
Capital income	72.5	990	75.7	935	63.1	1130
Panel C. Car variables						
Ownership (%)	81.7	38.7	81.0	39.2	83.5	37.1
Cars	1.10	0.753	1.08	0.750	1.15	0.759
Purchase freq. (new)	0.049	0.215	0.048	0.214	0.050	0.217
Car value (new)	254	125	255	125	252	124
Households	39,342		29,218		10,124	

Note: The full sample consists of all single-family house owners in Stockholm fulfilling our selection criteria and who are observed in years before 2007. Income variables are for 2006; all amounts are in SEK 1,000. The loan-to-value ratio (LTV) is defined as mortgage debt divided by housing wealth and the combined loan-to-value ratio (CLTV) is all debt divided by housing wealth. Net worth is the difference between the value of all the household's assets and its total debt. The exchange rate is approximately 8 SEK/USD. Ownership is a dummy variable that takes on a value of one if the household owns at least one car. The purchase frequency for cars is annual. Car value refers to the value of new cars at the time of purchase.

have. We categorize each institution by the type of credit they offer: mortgage, consumer lending, auto lending, agricultural and forestry lending ("Ag lenders", loans collateralized by farmland and property, and forestland), debt collection, and debt to government agencies. Institutions that have not specialized in one type of credit are marked as "mixed" (17 percent of the number of credit items). Unidentified credit suppliers (less than 0.1 percent) are also included in this category.[15] We are able to identify mortgages and auto loans with high precision.[16]

Panel B of Table 3 shows that total debt and mortgage debt are similar for the two groups. We define the loan-to-value (LTV) ratio as total mortgage debt divided by housing wealth, and the combined loan-to-value (CLTV) ratio as total debt divided by housing wealth. These measures of indebtedness are also similar across groups, with averages of 39.0 and 40.5 percent, respectively.

The variables that differ the most between the inside and outside groups are the car ownership rate (81 percent vs. 84 percent) and capital income (SEK 75,700 vs. SEK 63,100). The latter has a very thick right tail, as reflected by the high standard deviation.

Statistics Sweden's car registry contains annual observations of each car with variables such as model, brand, the exact date of purchase, current owner, the two former owners, and the value if it is a new car. We can thus observe essentially every car transaction that involves a household in our sample.[17]

[15] If a financial institution is marked as mixed, we look at its share of outstanding loans that have a balance above SEK 100,000 (approximate USD 12,500). If this share is above 70 percent a year, we categorize the bank as a mortgage lender. This affects about 0.1 percent of all observed credits. We assume that they are not Ag lenders because Ag lenders are highly specialized and easily identified. Student debt falls under debt to government agencies, and this category includes CSN, the agency that provides very advantageous student loans at a low interest rate and with a very long duration. No credit history check is required to get a student loan, and no private alternatives exist in Sweden.

[16] Most Swedish banks have special subsidiaries that manage mortgage lending (so-called *hypoteksbolag*), so we are confident in our identification of mortgage lenders in particular. In December 2007, the outstanding stock of mortgages to Swedish households was SEK 1,534 billion (Source: Statistics Sweden); when we use our categorization on the total stock of Swedish household debt, mortgages amount to SEK 1,565 billion. Auto lenders are also easy to identify based on their names.

[17] In cases where the value of a new car is missing, we use data from the Swedish Tax Agency. We match the car models by fuzzy string searching using the user-written Stata command `reclink2` (Wasi and Flaaen, 2015). Fifteen percent of the new cars have been assigned a value in this way.

Based on this data, we construct two data sets: an annual panel for all Stockholm homeowners to study the credit financing of car purchases and a quarterly panel for the main analysis to track the response of households to the renewal of Bromma Airport's contract.

3.1 Credit financing of car purchases

To set the stage for our main analysis, we use our registry-based data set to document household borrowing behavior at the time of car purchases. It is well-documented that U.S. households frequently use credit to finance car purchases. McCully et al. (2019) find that more than 70 percent of new cars are funded by either home equity or auto loans. We are unaware of any documentation of Swedish households' borrowing behavior in conjunction with car purchases and therefore establish basic facts on borrowing to finance car purchases among Swedish single-family house owners.[18]

We focus on how borrowing behavior varies with the LTV ratio because it is most likely to determine a household's ability to use its house as collateral.[19] To estimate marginal propensities to borrow (MPBs), we run the following regression:

$$\begin{aligned}\frac{\Delta \text{credit}_{it}^{k}}{\text{car value bought}_{it}} = {} & \beta_l \, \mathbb{1}(\text{LTV}_{it-1} < 50\%) \\ & + \beta_m \, \mathbb{1}(\text{LTV}_{it-1} \in [50\%, 100\%)) \\ & + \beta_h \, \mathbb{1}(\text{LTV}_{it-1} \geq 100\%) + \eta \, X_{it} + \epsilon_{it},\end{aligned} \tag{3}$$

where the outcome variable is the change in the credit of household i from the end of year $t-1$ to the end of year t divided by the value of all the cars bought during t. The k indicates the type of credit, which can be all credit, mortgage, consumer credit, or mixed credit. The dummy variables $\mathbb{1}(\cdot)$ indicate the household's LTV ratio at the end of $t-1$, and X_{it} is a

[18] Grodecka-Messi et al. (2022) use a tailored data set from the Swedish Credit Bureau (UC) to document that Swedish households to a high degree reoptimize their debt portfolio when they make equity withdrawals in response to growth in house prices.

[19] We restrict the sample to house owners that do not move during the year and that purchase at least one car during the year.

vector of control variables, standardized to mean zero and unit variance. The coefficients of interest are β_l, β_m, and β_h, which are estimates of the MPBs for households in three different groups based on their LTV ratios. The cutoffs for the categorization were chosen to represent groups that are likely to face different borrowing constraints. The first group has an LTV below 50 percent. Households in this group should be able to take out a substantial second mortgage, regardless of house price fluctuations. The second group comprises households with an LTV between 50 and 100 percent and hence are probably close to the maximum they can borrow using a mortgage. We conjecture that this group's borrowing capacity is influenced the most by house price fluctuations. The last group comprises households with a very high LTV; they should be limited in taking out additional mortgage debt regardless of house price fluctuations.[20] Note that the regression is demanding because it requires information about both car values and credit and that the data set has a panel dimension. Throughout our analysis, we focus on the purchase value of the car rather than the net of purchases and sales. This is for three reasons. The first one is conceptual—in the aggregate purchases and sales of used cars would net out and hence it is appropriate to focus on new cars. Secondly, even if one is able to account for sales with an accurate appraisal model for used cars, the timing of purchases and sales of the replacement in the household would potentially add a lot of noise. Thirdly, this approach is consistent with the majority of micro-level studies and it facilitates comparisons with studies using regional data.

We consider different kinds of credit k to be able to make more precise statements about household borrowing. If $\Delta \text{credit}_{it}^{k}$ is the change in total debt, then MPBs smaller than 1 indicate that households also use proceeds from sales of cars and financial wealth (e.g., bank deposits) to finance their car purchases.[21]

[20] In 2006–2008, banks set their LTV constraints in this range. During this period, there was no legal LTV cap. See Finansinspektionen (2010).

[21] Since we only observe the change in debt at year-end, we underestimate debt outtake for the specific purchase; we expect that households amortize a non-trivial amount of the loan over the year, and this introduces a downward bias in the MPBs. On the other hand, we also do not observe amortization payments before the purchase. If households that plan to buy a car pay off more debt leading up to the purchase, then our estimates are upward biased.

Table 4 presents our estimates. We find that the MPB on average is 0.47 (Column (1)). However, Column (2) shows that there is considerable variation in borrowing behavior depending on the LTV ratio. Households in the highest LTV category borrow a substantially smaller share of their cars' value. Central to our analysis is to what extent home equity is used as a source of collateral for mortgage loans. A comparison of Columns (1) and (3) shows that, on average, 71 percent (0.333/0.467) of the borrowing associated with a car purchase is a mortgage. Furthermore, the cross-sectional variation in borrowing propensities is even greater when focusing on mortgage borrowing. Column 4 shows that while households with an LTV ratio below 50 percent have an MPB of 0.35, households with an LTV ratio above 100 percent have no tendency to take on additional mortgage debt.

The estimated MPBs in Column (4) are consistent with the difference in the MPB in total debt; the MPB difference between the lowest LTV groups is insignificant, but the reduction in MPB as we move into the top LTV bin is very similar for total debt and mortgages. We interpret this as binding restrictions on supply as the LTV ratio increases and that households understand that mortgage debt is the cheapest form of credit. In Table B.4, we present other credit types as well, showing that households with below-median bank deposits use more credit financing when buying cars.

The cross-sectional variation in MPBs leads us to conclude that house price fluctuations are likely to influence households' means of financing car purchases. A big divergence in house prices of the kind we have documented can imply that the borrowing capacity of house owners far away from the airport increases more than the capacity of house owners closer to the airport. In the next section, we estimate the housing wealth effects.[22]

[22] An additional implication of the large cross-sectional variation in borrowing behavior is that there may be limitations on analyses that proxy car purchases since borrowing behavior depends on balance sheet characteristics.

Table 4: Marginal propensity to borrow by LTV group

	All credit		Mortgage	
	(1)	(2)	(3)	(4)
Intercept	0.467***		0.333***	
	(0.031)		(0.029)	
Low LTV, < 50%		0.458***		0.354***
		(0.033)		(0.031)
Mid LTV, 50–100%		0.524***		0.310***
		(0.044)		(0.040)
High LTV, > 100%		0.323***		0.116
		(0.094)		(0.087)
Low LTV – Mid LTV		−0.066		0.044
High LTV – Mid LTV		−0.201**		−0.194**
Observations	6647	6647	6647	6647
Controls	Yes	Yes	Yes	Yes

Note: This table presents the results from estimating model (3) for homeowners in Stockholm who do not move in years of purchasing a new car. The controls are 4th-order polynomials in household size, age, and disposable income. Each control variable is standardized. Standard errors are cluster-robust at the household level. The middle panel displays the F-tests of the differences between the estimated coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4 Results

This section presents estimates of housing wealth effects based on our quasi-experiment. The population function of interest is

$$\text{Car outcome}_{it} = \alpha + \beta \log(\text{House price}_{it}) + \varepsilon_{it}, \tag{4}$$

where β is an elasticity. It measures how households' spending on cars responds to a change in house prices. The common issue when estimating β is that house prices change when economic circumstances relevant to decisions on cars change. Ordinary least squares therefore lead to biased estimates. We address this by applying a difference-in-difference (DiD) regression and ultimately obtain a two-sample instrumental variables estimate of β. We first present reduced form DiD estimates.

4.1 Reduced form

For reduced form estimates, we use the same difference-in-difference model as when we estimated the house price effect:

$$\text{Car outcome}_{it} = \phi \,\text{Noise area}_i \times \text{Post}_t + \rho \,\text{Noise area}_i + \eta \, X_{it} + \gamma_t + \varepsilon_{it}, \tag{5}$$

where ϕ is the coefficient of interest. It measures the change in car outcomes inside the noise area relative to outside after the renewal of the airport contract. The coefficients γ_t indicate year-quarter effects, X_{it} is a vector of household controls, and ε_{it} is an error term.[23]

The results are presented in Table 5. Columns (1) and (2) present estimates for the extensive margin effect: the number of cars purchased per household and quarter. On this margin, there is no effect of the renewal. The probability of buying a car in the treatment group is imprecisely estimated at 0.029 percentage points per quarter, which is minuscule relative to a baseline probability of approximately 1.2 percent (4.9 percent per annum). Columns (3) and (4) present effects on the intensive margin: the effect on the log car value conditional on the household buying a new car. Households close to the airport respond by reducing car values by 8.5 log points. Adding control variables reduces the estimate only a little, to 7.7 log points.[24]

The lack of an extensive margin response combined with a substantial intensive margin response is somewhat surprising but not implausible. A strong extensive margin response is considered standard in (S,s) models (e.g., Bar-Ilan and Blinder, 1992; Caballero, 1993; Eberly, 1994). However, Attanasio et al. (2022) illustrate that different combinations of shocks can lead to such responses, for instance in the US during the Great Recession. When we present the results from our model in Section 5, we will return to this discussion.

Having established that there are household responses to the renewal, we turn to IV estimation to obtain elasticity estimates.

[23] All estimates are based on the Stata command `reghdfe` by Correia (2016).

[24] Figure B.3 reports tests of parallel pre-trends of outcome variables.

Table 5: Reduced form results – extensive and intensive margin

	New cars		log car value	
	(1)	(2)	(3)	(4)
Noise area$_i$ × Post$_t$	0.000 29	0.000 29	−0.085***	−0.077***
	(0.000 54)	(0.000 54)	(0.019)	(0.021)
Noise area$_i$	0.000 14	0.000 12	0.000	0.004
	(0.000 31)	(0.000 31)	(0.012)	(0.012)
Controls	No	Yes	No	Yes
Observations	531,105	531,105	6045	6045
R-squared	0.001	0.001	0.011	0.042
Age	53	53	52	52
LTV (%)	39	39	42	42
Net worth	3400	3400	3860	3860

Note: The table presents estimates of the coefficients in equation (5). The outcome variable in Columns (1) and (2) is the number of new cars bought (i.e., extensive margin response). The outcome variable in Columns (3) and (4) is the log value of these cars (i.e., intensive margin response). We control for household size, age of household head, disposable income, and net worth, all in 2006. All regressions include year-quarter fixed effects. Errors are two-way cluster robust at the household and year-quarter level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Two-sample instrumental variable estimates

Reduced form estimates from the difference-in-difference model established that house owners respond to the renewal. We now estimate the elasticity to housing wealth using the two-sample instrumental variable approach of Angrist and Krueger (1992).[25] Obtaining a credible partial equilibrium estimate of β has a strong appeal because it is closely connected to economic theory, as discussed in Berger et al. (2018) and Guren et al. (2020). Our quasi-experimental design, with granular geographic divergence in house prices, makes our estimates particularly credible. For instance, Orchard et al. (2022) argues that households'

[25] Inoue and Solon (2010) show that the inference problem is solved using a generated regressor correction as proposed by Murphy and Topel (2002). We follow the implementation of Fredriksson and Öckert (2013). We first estimate the equation (2) in the transaction data set. We then use the estimates to predict the quasi-random variation in house prices in the household data set. The predicted house prices are used as a covariate in equation (4). Different functional forms for the predictive variables are considered. The main specification utilizes Noise Area$_i$ and Noise Area$_i$ × Post$_t$, but we have also considered the continuous "dose specification"—see Section 4.4.3. The IV estimates, $\widehat{\beta}$, may deviate somewhat from $\hat{\phi}/\hat{\delta}$ because the set of control variables in the two specifications differ. We thank Peter Fredriksson and Björn Öckert for sharing their Stata code with us.

responses are dampened in general equilibrium because of price effects, implied by non-perfectly elastic car supply (see also Gavazza and Lanteri, 2021, who analyze the price effect of shocks to credit market conditions). But it is unlikely that our geographically granular setting gives rise to such effects (see also Section 4.4). Furthermore, the econometric issues concerning difference-in-difference regressions raised by Orchard et al. (2022) and Borusyak et al. (2021) do not apply.[26]

Table 6 reports the instrumental variable estimates for the subset of households that purchase a new car. We focus on the intensive margin responses since we found no responses on the extensive margin. Columns (1) and (2) report baseline elasticities. Without additional control variables, the elasticity is 0.398, which is statistically significant at the one-percent level. Adding control variables barely reduces the estimate, which remains at 0.39.

There is a strong theoretical foundation for the amplification of the housing wealth effect due to balance sheet characteristics. Berger et al. (2018) and Kaplan et al. (2020b) derive analytical expressions for the response to non-durable consumption in standard incomplete market models; the response is a function of the household's marginal propensity to consume out of transitory income shocks, which depends on the household's liquid savings buffer and the ratio between housing wealth and net worth.

Since our data set includes detailed balance sheet items and car transactions, we are in a good position to explore these relationships in the context of durable goods. We split households into two groups based on their balance sheet characteristics and explore variation in the intensive margin response. First, we split the households by their LTV ratios. Given our findings of differential MPBs across the LTV distribution, it seems plausible that a house price shock that affects LTV ratios will amplify the house owners' response on car consumption. We find strong heterogeneity in responses. Columns (3) and (4) of Table 6 show that house owners with an LTV ratio greater than 50 percent respond almost twice as strongly to the house price shock than house owners with an LTV ratio below 50 percent

[26] Carroll et al. (2011) page 71 also discuss the ideal quasi-experimental design.

Table 6: Housing wealth elasticities

	Full sample		LTV		Bank deposits	
			$\leq 50\%$	$> 50\%$	$\leq$ P50	$>$ P50
	(1)	(2)	(3)	(4)	(5)	(6)
log(House price$_{it}$)	0.398***	0.393***	0.269**	0.526***	0.694***	0.123
	(0.108)	(0.124)	(0.124)	(0.188)	(0.183)	(0.138)
Controls	No	Yes	Yes	Yes	Yes	Yes
Observations	6045	6045	3945	2100	2748	3297
Age	52	52	56	45	50	54
LTV	42	42	22	80	51	35
Net worth	3860	3860	4950	1830	2440	5050
Financial wealth	1517	1517	1860	879	508	2360

Note: The table presents the second-stage two-sample IV estimates of β in equation (4). The dependent variable is the log of car values. Standard errors are corrected for first-stage estimation in the house transaction data set. In the first stage, we control for taxation value, building age, living area, and non-living area, and a control variable for the change in the property tax code in 2007. In the second stage we control for household size, age of household head, disposable income, and net worth, all in 2006. Errors are two-way cluster robust at the household and year-quarter levels. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

(0.526 versus 0.269). We also split house owners by their amount of bank deposits, which is the most liquid form of financial wealth and hence a good proxy for the households' marginal propensity to consume out of transitory income shocks. For house owners with small deposits, the response is 0.694, whereas for the other house owners, the response is only one-fifth as large. Such strong heterogeneity in responses along this dimension is consistent with previous general findings on responses to income shocks (e.g., Baker, 2018, who find particular strong heterogeneity in responses of durables) or lottery prize gains (Fagereng et al., 2021) but has to our knowledge not been documented before in the context of a well-identified partial equilibrium house price shock.[27] Section 5 compares these findings to a quantitative model adapted specifically to our setting. Before turning to the model, we convert our estimates to MPXs.

[27] We present related reduced form estimates from triple differences in Table B.5. This allows us to formally test the statistical significance of differences between groups. We also investigate differential responses along additional dimensions. Broader measures of financial wealth (that include mutual funds and direct stock ownership) reveal no significant difference.

4.3 The marginal propensity of expenditures (MPX)

What are the aggregate implications of our findings in terms of car MPXs? The loss in housing wealth for households inside the noise area is approximately equal to SEK 774,060 (SEK 3,990,000 × 19.4%). The elasticity among purchasers of new cars implies that the reduction in spending is SEK 19,061 (0.393 × 19.4% × 250,000). This is a substantial response among house owners that purchase a new car; a car MPX of 0.025 (i.e., 2.5 cents per dollar). To compare this MPX to estimates that rely on geographically aggregated data, we must adjust for the number of new cars households buy per year, which is 0.049. This implies that the MPX on cars among all house owners is 0.0012 (i.e., 0.12 cents per dollar). Mian et al. (2013) estimate an MPX on cars of 1.8–2.3 cents per dollar and Aruoba et al. (2022) 1.2 cents per dollar.[28]

4.4 Robustness

We have undertaken some tests to further strengthen the credibility of our quasi-experimental design.

4.4.1 Tests of the exclusion restriction

For the IV estimates to be consistent, the exclusion restriction must hold. What circumstances would lead to violations?

One such circumstance would be if the renewal of the airport contract affects car purchases for other reasons. Since the obvious alternative use of the land is to build residential housing and connect public transport to the neighborhood, the demand for cars among prospective residents could conceivably be affected. However, we argue that the plans to convert the land were too diffuse at the time. For instance, due to soil pollution, it was estimated that it would take many years to clean the area, which would delay new construction. Another

[28] We report estimates from Figure 4 and Table 5 in Mian et al. (2013), consistent with Aruoba et al. (2022). Berger et al. (2018) start from elasticities to housing net worth shocks in Table 2 of Mian et al. (2013) and scale them by 0.25–0.33 to obtain housing wealth elasticities.

such circumstance would be tax reforms, but they had already been announced and both groups benefited equally. A third circumstance would be if the income of current residents is affected. This could happen in two ways. It could be the result of an indirect effect (in the sense of Guren et al., 2020) where demand adjustments of households spill over to income or the result of a direct effect on job opportunities at the airport. However, Figure B.2 shows that there are no such effects. Indeed, any income effects would seem implausible given both the geographical granularity of the house price shock relative to the overall Stockholm labor market and that we study responses over just five quarters. We find neither an effect on the probability to move in the short or long run, so it seems unlikely that any housing externalities, in the sense of Rossi-Hansberg et al. (2010), materialize.[29]

4.4.2 Placebo tests

We have confirmed that co-op owners and renters respond differently than house owners. These reduced form results are reported in Table B.6 and B.7.

4.4.3 Dose response

We have also performed additional analyses that strengthen the credibility of our quasi-experimental design. If noise is the fundamental cause of the local divergence in house prices, there is an expectation that the house price shock would be muted monotonically with distance to the noise contour. Table B.8 confirms a monotone relationship in terms of relative house prices and distance but also the house owners' responses and distance.

5 Model

We set up a model to compare empirical estimates of MPXs out of housing wealth with their theoretical counterpart. We also use the model to generalize the insights of Berger et al.

[29]The baseline probability of moving in our sample is 8.3 percent per year and the treatment effect is estimated to be -0.0024 and is insignificant.

(2018) on responses to house price shocks, taking into account durable goods, long-term mortgages, and information frictions. We provide an informal overview of our model and refer to Appendix D for details.

5.1 Overview

Our model is closest to Attanasio et al. (2022) and Berger et al. (2018). Time is discrete and at a quarterly frequency. Households live from age 30 to 85 and retire at 65.[30] Income is exogenous and has a hump-shaped profile during working life. It is exposed to transitory i.i.d. shocks. Upon retirement, the household receives a predetermined fixed income that corresponds on average to a replacement rate of 70 percent.

Households have preferences for a non-durable and a durable good, where the latter is the car. The goods form a Cobb-Douglas consumption basket from which the household receives utility. When the household purchases a car, it pays an adjustment cost and chooses a new level of its car stock. Between time periods, the car depreciates.

From the start of life, the household is endowed with a house and a long-term fixed-rate mortgage (FRM). The house is used as collateral for the mortgage. In the last period of life, the house is sold and its value added to cash-on-hand.[31] In the last period, the household consumes as per usual and then receives utility from a bequest motive of what remains of their wealth. The car depreciates one more period, is sold and is added to the bequest. At any point in life, the household can adjust its mortgage amount subject to satisfying a down-payment constraint and pay an adjustment cost. The sum of interest and amortization is constant per period, as in, for instance, Campbell and Cocco (2003).

There is a second financial asset that households use to save or borrow. Borrowing in this asset does not require collateral and is limited by an exogenous borrowing constraint. There are interest rate spreads: the interest rate for borrowing is greater than for saving.

[30] We motivate the age span based on our empirical setting. Only 2.1 percent of our sample of house owners have a household head that is younger or older.

[31] It is not possible for households to sell the house and become renters. We motivate this simplification from a low moving rate and the absence of treatment effect.

Furthermore, the interest rate on the mortgage is lower than the interest on other borrowing. Unlike Attanasio et al. (2022), cars cannot be used as collateral for a loan. Instead, the household can use equity withdrawal through mortgage adjustments.

The adjustment costs for cars and mortgages make adjustments infrequent. In particular, there are inaction regions in the state space; (S,s) bounds. This feature can give rise to strong extensive margin responses to large shocks. However, recent additions to the literature have argued that strong immediate extensive margin responses do not always occur in the market for cars. This was the case in the Great Recession (see in particular Attanasio et al., 2022). There may also be a delay in response to aggregate shocks, such as monetary policy (McKay and Wieland, 2021). We therefore allow for an information friction. Households are not immediately aware of the spatial divergence in house prices. They only conceive the change to their housing wealth once they attempt to adjust their mortgage or trade cars or if they are hit by a random shock to their information set. We argue that this information friction is reasonable, in particular in "normal" economic times when our quasi-experiment occurred (recall that this was just before the US financial crisis became a global crisis). Furthermore, we find it plausible that households do not frequently stay up to date on aggregate house prices as long as they do not fall. In this perspective, the friction appears to be a weaker assumption than in e.g., McKay and Wieland (2021), where households are unaware of aggregate variables.[32] We return to the implications of the information friction and its plausibility in different settings in Section 5.5.

5.2 Design of the quasi-experiment

We set up a replica of the quasi-experiment as follows. We calibrate the model to the sample of single-family house owners in Stockholm in 2006.[33] We simulate life-cycle paths

[32] Carroll et al. (2020) and Auclert et al. (2020) have similar frictions to produce hump-shaped aggregate responses.

[33] To limit the support and size of the state space, we remove the top and bottom quartiles with respect to labor income when we compute our target moments. Therefore, several numbers will not coincide with the summary statistics in Table 3.

for 200,000 households in the treatment and control groups. Each household has its identical twin in the other group, meaning that the twins have the same initial states and experience an identical sequence of idiosyncratic shocks to income. The difference between them is that in a random quarter, households in one group unexpectedly receive an increase of 19.4 percent in their housing wealth (from w^h to $w^{h'}$); an MIT shock.[34] The housing wealth shock is permanent. The shock affects both end-of-life wealth and the household's capacity to increase its mortgage balance. The spending response of the household depends on its other state variables: age, mortgage balance, cash-on-hand, and the current stock of cars. Our analysis of the consumption responses to this kind of partial equilibrium house price shock resembles the analysis of Berger et al. (2018), though our model includes endogenous responses to a durable good in the presence of long-term mortgages. These aspects are the focus of our analysis.

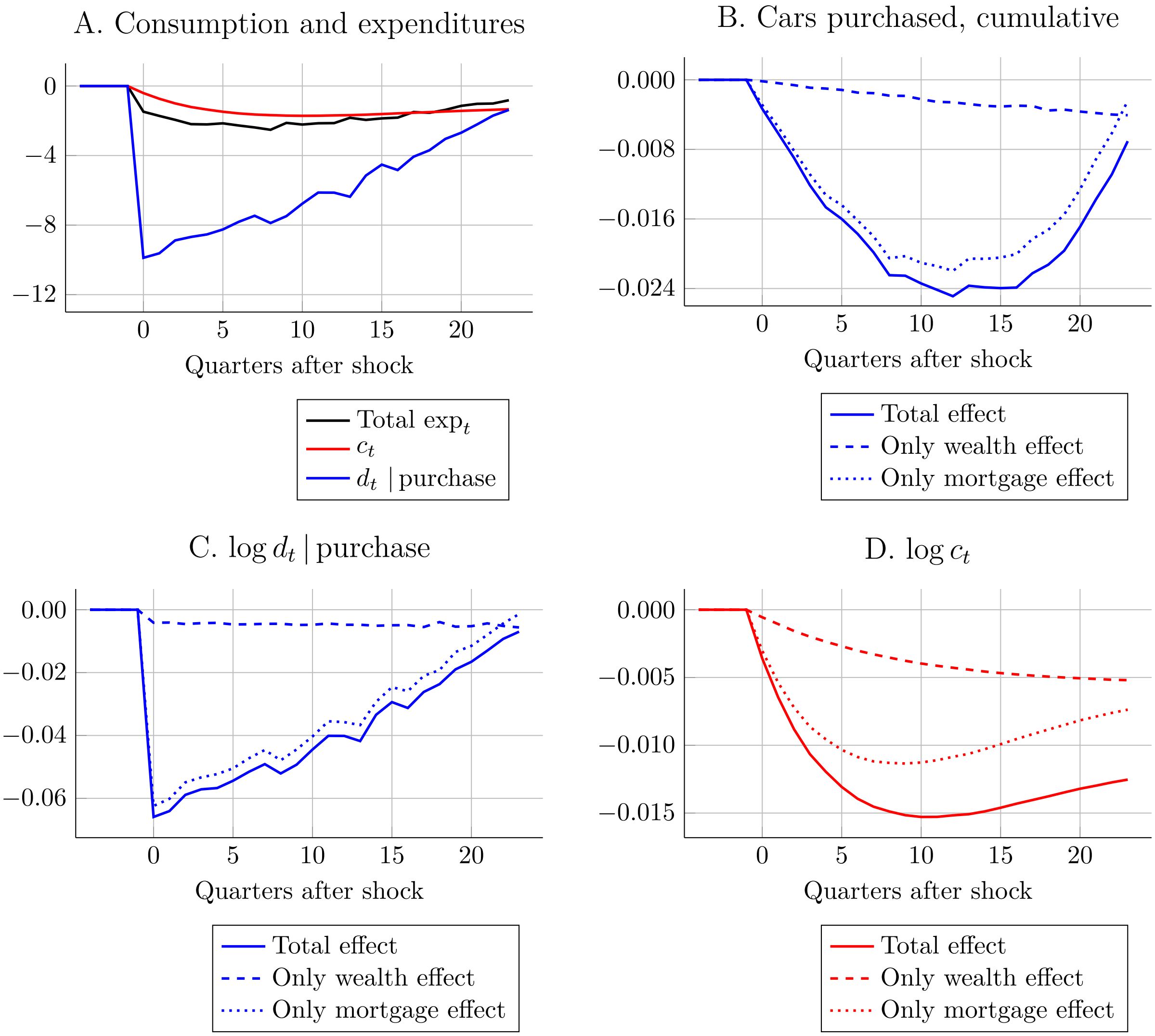
5.3 Consumption response

Figure 3 presents the average difference in consumption and spending responses (treatment group minus control group). Panel A illustrates the fall in total expenditures, non-durable consumption, and cars. Despite the Cobb-Douglas expenditure share for cars being only 5.5 percent, the reduction in expenditure in the short term is mostly due to a response in cars. Conditional on a car purchase in the first four quarters, homeowners reduce the car value by SEK 8,700–9,900. This translates to an unconditional car expense of SEK 1,000 per quarter, which in turn amounts to 45–72 percent of the total response. The total MPX in the first year is 1.2 cents per dollar and the car MPX is 0.64 cents per dollar which implies a new car MPX of 0.20 cents per dollar (see Appendix D.5).

Panels B, C, and D show other aspects of the response: the cumulative probability of a car purchase; the log car expense conditional on a purchase; and the log of non-durable

[34] We label the group that experiences the shock as the control group. As in our empirical setting, the treatment group (close to the noise area) experienced flat prices and the control group (i.e., further away) experienced increasing prices.

Figure 3: Model simulations – difference between treatment and control group



Note: The figure shows households' responses to the shock to housing wealth at event time $t = 0$. The plots are based on simulations of 200,000 households. The shock happens at a random age and every shocked household is compared to its benchmark counterpart. For each household, the response is calculated as the difference between the choice in presence of the shock minus the choice in absence of the shock. The values in the top left panel are in SEK 1,000. The other panels display a decomposition of the shock into shocks to wealth and the mortgage cap separately. The top-right panel shows the difference in the cumulative number of cars purchased (extensive margin); the bottom-left panel shows the difference in the car value bought conditional on buying a car (intensive margin, in logs); and the bottom-right panel shows the difference in non-car consumption (in logs).

consumption. We first focus on the solid lines, labeled "Total effect." Two features are noteworthy. First, Panel B shows that the extensive margin response is gradual as opposed to most (S, s) models. The second feature is illustrated by the solid line in Panel C. On impact, the response is 6.6 log points, and the average over the first four quarters is 6.1 log points. This translates to an elasticity of 0.31 (0.061/0.194), which is close to our empirical estimate and well inside the 95-percent confidence interval.[35] But the elasticity depends heavily on the measurement period. For instance, eight quarters after the shock, it is 20 percent smaller.[36] Finally, Panel D illustrates that the maximum elasticity in non-durables is attained only after ten quarters when it is 0.079 (0.0153/0.194). This translates to an MPC in non-durables of 1.1 cents per dollar.

There are two main takeaways from our discussion so far. First, the total MPX and the MPC in non-durables are consistent with many estimates in the literature (e.g., Disney et al., 2010; Guren et al., 2021; Graham and Makridis, 2023), albeit in the lower range. Second, we think that the dynamic aspect of the responses and its implications for the empirical design have not been given sufficient attention when comparing empirical estimates. We return to this matter in Section 5.5.

5.4 Understanding variation across households

Motivated by the uncovered heterogeneity in responses from different balance sheet characteristics, we use the model to decompose the total response into a wealth effect and a collateral effect. We modify the baseline experiment and run it twice. In the first modification, to investigate the role of wealth, we shock the housing value but hold the borrowing capacity constant. In the second modification, to investigate the role of housing as collateral, we do not shock house prices but only households' borrowing constraints so the borrowing capacity in mortgages increases by as much as in the main experiment.[37] The results are presented as the dashed and dotted lines in Panels B, C, and D of Figure 3.

We find that the collateral effect dominates, and it is particularly strong for cars. The response in car expenditures (Panel C) in the first four quarters is 93 percent of the baseline

[35] Based on Column (2) of Table 6, the confidence interval is 0.150–0.636.

[36] See Table D.4 for average responses over measurement periods of 4, 16, and 32 quarters.

[37] We shock the downpayment constraint from $\phi = 0.85$, which corresponds to a minimum downpayment of 15 percent, to ϕ'. In the first experiment $\phi' = 0.7119$. In the second experiment $\phi' = 1.0149$.

response, whereas in the case of a pure wealth shock, the response is 7.5 percent of the baseline. After 20 quarters, the collateral effect is 69 percent of the baseline while the wealth effect is 32 percent. For non-durable consumption, the difference in force between the wealth and the collateral channel is somewhat less stark. The short-run collateral effect is 81 percent of the baseline, and the short-run wealth effect is 19 percent. In summary, the effects of wealth and collateral are consistent with the differential response in the data. Our empirical and theoretical results are also consistent with important contributions that emphasize collateral effects (e.g., Leth-Petersen, 2010; DeFusco, 2018; Aydin, 2022; Sodini et al., 2022).

From the point of view of macroeconomic stabilization policy, total expenditure is perhaps the most interesting, and the dynamics of cars strongly influence the short-run expenditure statistics: the collateral effect on total expenditure is 83 percent of the baseline in the first four quarters, and the wealth effect on total expenditure is 19 percent. This illustrates how credit market freezes can have strong immediate effects on aggregate demand (see e.g., Benmelech et al., 2017; Bernanke, 2018).

Relatedly, it is worth pointing out that while our model exhibits intertemporal shifting as in McKay and Wieland (2021), it is not due to monetary policy but a shift in households' credit portfolios. In our model, homeowners' effective interest rates on loans increase as they shift from mortgage financing to uncollateralized borrowing, and households gradually recover access to cheap credit, and at that point the value of their stock of cars recovers, too.[38]

Another important takeaway from this analysis is that the characteristics of consumption goods matter for households' expenditure responses to shocks. One such characteristic is the ability to postpone purchases—intertemporal shifting due to durability. Another one is if the good is credit-financed. Under a restriction of studying short-term housing wealth effects based on a single consumption item, cars are the preferable item.[39]

[38] Attanasio et al. (2022) achieve a similar effect by shocking the risk premium on auto loans.

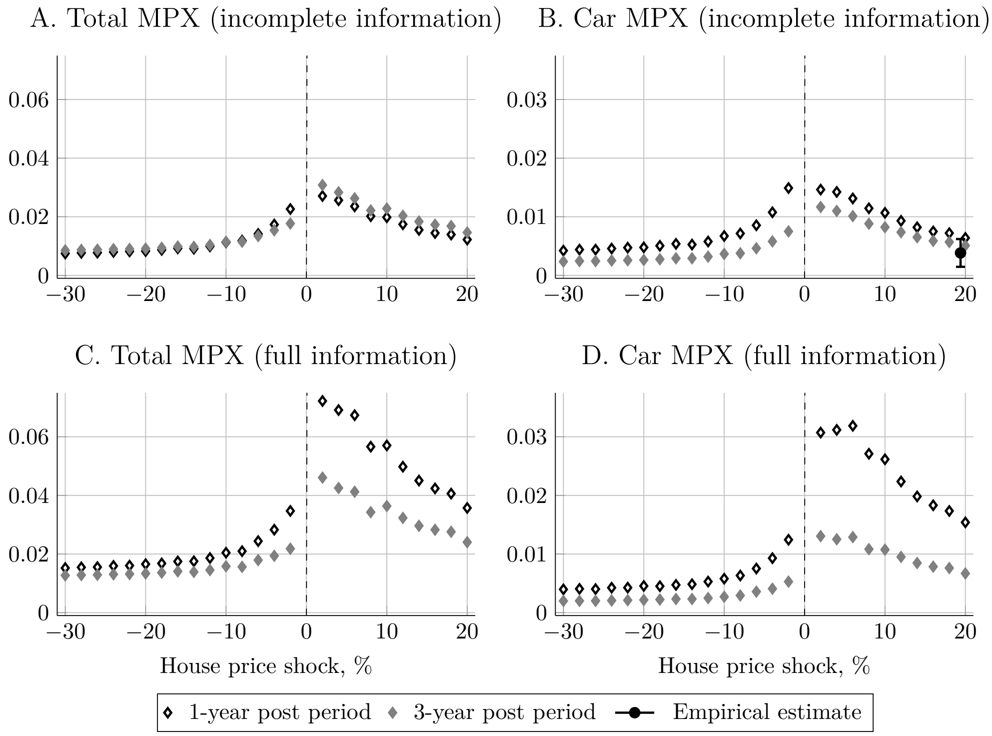
[39] Table A.1 reports car MPXs and car elasticities from previous housing wealth studies.

5.5 Understanding variation across studies

We have set up a state-of-the-art model that matches our empirical evidence. However, the empirical literature has found a wide range of estimates of elasticities and MPXs (see Table A.1 for a summary). Based on the combination of our model analysis and the conclusions of previous studies, we identify three factors that contribute to this variation. Figure 4 illustrates these factors.

Empirical setting and regression specification. Several features related to the empirical studies' design can give rise to variation in estimates. One such feature is the shock magnitude. Panels A and B of Figure 4 plot the Total MPX and Car MPX in our model for different shock magnitudes in the interval from −30 percent to +20 percent (i.e., shocks to w^h—recall that our experiment implies +19.4 percent—see the point estimate and confidence interval in Panel B). The panels show that the greater the magnitude, the smaller the partial equilibrium MPX. Car and total MPXs may be in the range of 1 to 2 cents per dollar. The reason is that as the shock size increases, fewer and fewer households' immediate consumption decisions are affected as they become constrained. Another empirical feature that gives rise to variation is the measurement period relative to the shock. The differently colored diamonds illustrate this. Short-run MPXs (say, 1-year: white diamonds) are greater than long-run ones (say, 3-year: gray diamonds). This is particularly relevant for the lumpy spending on durables (Panel B), but also for total spending (Panel A). An aspect related to the measurement period is the regression specification of different studies. Because of the intertemporal dynamics of durable spending, the use of "long differences" in regressions—that is, outcome variables based on differences over several years—implies a downward bias in estimates. Such regression specifications do not capture that the initial response is greater, whereas a difference-in-difference specification identifies the average effect over the post-period. We illustrate this in Appendix D.6 using data generated by our simulation. We find that for estimates of the car MPX, the bias can be 50 percent or more.

Figure 4: The role of the shock magnitude and the measurement period



Note: The figure shows households' responses to different partial equilibrium housing wealth shocks. Each diamond represents the average across 200,000 simulated households. We mimic our other simulations by shocking households at a random stage in the life-cycle and compare them to identical households that experience no shock. The left panels display the marginal propensity for total expenditures. The right panels display the marginal propensity for car expenditures. Panels A and B present our baseline model with an information friction (incomplete information). Panels C and D present responses in the absence of this friction (full information). The black circle in Panel B indicates the empirical estimate from our quasi-experiment, adjusted so that it reflects the total of new and used car purchases. The error bars around the circle indicates a confidence interval of 95 percent. See Appendix D.5 for details.

The bias on total MPX is much smaller, in particular for large housing wealth shocks.

The state of the economy. The focus of our analysis is a partial equilibrium shock to house prices, but the underlying reasons behind the movement in house prices matter for the response. We have documented ample use of mortgage debt among car purchasers and matched that feature in our model.[40] Consequently, many households in our simulation are borrowing constrained and respond strongly to small positive house price shocks. In

[40]The product of the subjective discount factor and the rate of return is less than 1, which implies that households borrow to front-load consumption.

contrast, the US financial crisis can be thought of as a classic boom-bust episode where credit supply was ample in the boom phase (Mian and Sufi, 2018; Adelino et al., 2016). In such an episode, the bust would imply that many homeowners become constrained, and thus the MPXs would be greater for negative shocks than positive ones. That is, such a state would yield the opposite asymmetry compared to Figure 4. Indeed, Guerrieri and Iacoviello (2017) report a car elasticity of 0.24 in the boom phase of the US financial crisis and 0.49 in the bust phase. It is also plausible that households are more attentive to housing market developments in bad times and to falls in house prices (rather than increases). This reasoning is in line with Kaplan et al. (2020a), who argue that changing household beliefs was a critical component of the US financial crisis. We consider the effect of higher awareness by relaxing the information friction in our model: Panels C and D of Figure 4 report the resulting MPXs. The 1-year MPX to small positive shocks is nearly three times as large, with a car MPX of 3 cents per dollar and a total MPX well above 6 cents. Additional factors can amplify further the variation across studies: Attanasio et al. (2022) argue that factors such as shocks to expected income growth and to the risk premium on auto loans mattered for the responses of US households during the crisis.[41]

General equilibrium effects. Finally, we wish to highlight the role of general equilibrium effects, discussed extensively in Guren et al. (2020). We argue that the longer the measurement period, the more likely it is that estimates are biased upward due to feedback loops and spill-over from other markets to the housing market. In combination with the use of long differences in car spending as the outcome variable, the net effect is unclear.

With these factors in mind, how do our empirical and model results relate to the literature?[42] Our empirical estimate of the car MPX and the fitted model's MPXs is clearly in the lower range relative to the literature. Yet, housing wealth MPCs of this magnitude are not unique;

[41] Relatedly, Berger and Vavra (2015) argue that durable expenditures do not respond as strongly to stimulus in recessions as in expansions. The mechanism is that more households remain inside their (S,s) bound even after the stimulus.

[42] See Table A.1 for a literature review.

Browning et al. (2013) report an MPC of 0.0003–0.05 and Graham and Makridis (2023) an interval of 0.0078–0.0092. Adam Guren and his co-authors explicitly adjust for general equilibrium effects and report a partial equilibrium MPC of 0.018 and an MPC including equilibrium effects of 0.033 (Guren et al., 2020, 2021).[43] If anything, later studies tend to report smaller estimates.

We see two main reasons why our estimates are on the low side. The first reason is that we view our estimate as a "normal" times estimate. Notice that the time window of our analysis entails no absolute fall in house prices but a mere price divergence, or relative house price fall. House prices in the noise area do not decrease during the five quarters that we consider. They remain flat, while house prices outside the noise area continue to increase. In contrast, responses in crisis times may be different as households become more attentive to economic conditions.[44] The second reason is that our experimental design is probably most conceptually similar to recent theory-based partial equilibrium experiments. The US financial crisis includes a credit supply effect as well as an employment effect.[45]

6 Concluding remarks

Long before the global financial crisis, economists debated the extent to which the housing market influences the evolution of the macroeconomy through its roles as a store of value and collateral for borrowing. Recent studies have found such housing wealth effects using a variety of methods. However, there is substantial variation in empirical estimates.

We bring to the table a novel identification method—based on a quasi-experiment and household level data—that enables us to identify a partial equilibrium housing wealth effect, as recently defined and examined in Guren et al. (2020); Berger et al. (2018), with the

[43] Many studies report only elasticities and not MPCs. We omit them from this discussion, although it is noteworthy that some of those studies report low elasticities.

[44] Guren et al. (2021) and Guerrieri and Iacoviello (2017) indeed argue that the housing wealth effect has varied over time.

[45] See Mian and Sufi (2014). See also page 3434 in Aladangady (2017) and footnote 12 of Berger et al. (2018) for discussions of general equilibrium effects. Also, Charles et al. (2017) document strong interaction effects between the housing and labor markets.

important difference that our outcome variable is car expenditure.

Our estimated elasticity implies a new car MPX of 0.12 cents per dollar, which is small. Yet, conditional on purchasing a new car, house owners' response is substantial, around 8 percent, and house owners with an LTV ratio above 50 percent or with bank deposits below the median value respond much stronger than other house owners. We use a state-of-the-art model to argue that it is consistent with a short-run response to durable consumption in "normal times" in the presence of information frictions and absent general equilibrium effects. Further, the model enables us to highlight several factors that most likely explain some of the variation in estimates: measurement period, regression specification, and the state of the economy. To make progress on a consensus view on the housing market's role in aggregate demand fluctuations, future empirical studies should highlight each of these aspects.

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Appendix

A Estimates from previous literature

Table A.1: Estimates of housing wealth effects from previous literature

Study	Country	Elasticity	MPC	Car elasticity	Car MPX
Aladangady (2017)[a]	U.S.	–	0.047	–	–
Andersen and Leth-Petersen (2020)[b]	DK	–	0.03–0.05	–	–
Aruoba et al. (2022)	U.S.	–	–	–	0.012
Attanasio et al. (2009)[c]	U.K.	0.0	–	–	–
Browning et al. (2013)[d]	DK	0.0–0.13	0.003–0.05	–	–
Calomiris et al. (2013)	U.S.	0.163–0.270	0.049–0.081	–	–
Campbell and Cocco (2007)[e]	U.K.	0–1.7	–	–	–
Carroll et al. (2011)[f]	U.S.	–	0.02–0.09	–	–
Case et al. (2013)	U.S.	0.065–0.068	–	–	–
Cloyne et al. (2019)[g]	U.K.	0.2–0.3	–	–	–
Cooper (2013)	U.S.	0.06	0.06	–	–
DeFusco (2018)[h]	U.S.	–	0.04–0.13	–	–
Disney et al. (2010)[i]	U.K.	0.087–0.120	0.01	–	–
Graham and Makridis (2023)	U.S.	0.10	0.0078–0.0092	–	–
Guerrieri and Iacoviello (2017)[j]	U.S.	–	–	0.24–0.49	–
Guren et al. (2020)[k]	U.S.	0.040	0.018	–	–
Guren et al. (2021)[l]	U.S.	0.072	0.033	–	–
Kaplan et al. (2020b)[m]	U.S.	0.06–0.12	–	–	–
Mian et al. (2013)[n]	U.S.	0.13–0.26	0.054	0.33–0.43	0.018–0.023

Note: The table presents estimates from previous studies. Elasticities and MPCs either refer to total expenditure or non-durables.
[a] Aladangady in addition reports a zero effect for renters.
[b] Estimates a marginal propensity to borrow on mortgage debt.
[c] Estimates reported in Table 1 are positive but the authors refuse to interpret them as casual.
[d] Significant effect only for subsamples (young and constrained, the upper bound), i.e., no pure wealth effect.
[e] 1.7 is for older homeowners, 0 for young renters. I.e., pure wealth effect.
[f] The lower end of the estimates is the direct effect and the upper end is long run estimate.
[g] The reported number is an elasticity of borrowing with respect to house prices.
[h] Same interpretation as above.
[i] For elasticities, the lower end is for old homeowners and the upper for young homeowners. They also report result for young renters.
[j] They report two estimates of car elasticities: 0.24 in 2002–2006, and 0.49 in 2006–2010.
[k] The estimates are the partial equilibrium housing wealth effect computed using results in Guren et al. (2021).
[l] We only report the estimates of their sensitivity instrument. See the paper for other estimates.
[m] Elasticities have been multiplied by the mean housing wealth to net worth ratio (H/NW); see footnote 12 in Berger et al. (2018). The elasticities are for non-durable consumption.
[n] Rescaled by H/NW as above. The auto sale elasticity is presented in the online appendix where they also report non-durable spending elasticities of 0.09–0.13 (all rescaled by H/NW).

B Additional empirical results

Table B.1: Summary statistics for other variables for single-family house and co-op apartment transactions

	Panel A: Full sample					
	Single-family houses					Co-op apartments
	Tax value	Age	Score	Lot area	Non-living area	Rooms
Mean	1751	77.2	28.4	540	47.7	2.30
Std. dev.	823	210	4.32	367	59.7	1.07
Obs.	19,777	19,666	19,666	19,777	19,666	85,048

	Panel B: Before 2008Q3					
	Single-family houses					Co-op apartments
	Tax value	Age	Score	Lot area	Non-living area	Rooms
Mean	1762	71.4	28.6	541	47.8	2.27
Std. dev.	858	189	4.32	349	35.9	1.07
Obs.	11,321	11,308	11,308	11,321	11,308	50,248

Note: This table complements Table 1. Transactions of single-family houses start in 2004Q1 and end in 2012Q4. Transactions of co-op apartments start in 2005Q1 and end in 2010Q4. All amounts are in SEK 1,000.

Table B.2: Summary statistics for single-family houses inside and outside the noise area before renewal

	Panel A: Inside noise area						
	Price	Living area	Tax value	Age	Score	Lot area	Non-living area
Mean	2807	120	1729	51.5	29.0	563	48.2
Std. dev.	1517	36.5	725	61.0	4.39	329	40.1
Obs.	2330	2329	2330	2329	2329	2330	2329

	Panel B: Outside noise area						
	Price	Living area	Tax value	Age	Score	Lot area	Non-living area
Mean	2672	116	1766	78.2	28.3	533	48.2
Std. dev.	1893	39.3	837	271	4.17	363	34.4
Obs.	6926	6926	6926	6926	6926	6926	6926

Note: Transactions of single-family houses start in 2004Q1 and end in 2007Q2. Tax value refers to the value assigned by The Swedish Tax Authority. Score is a hedonic variable that determines the tax value. Amounts are in SEK 1,000 and areas are in square meters.

Table B.3: Summary statistics for co-op apartments inside and outside the noise area before renewal

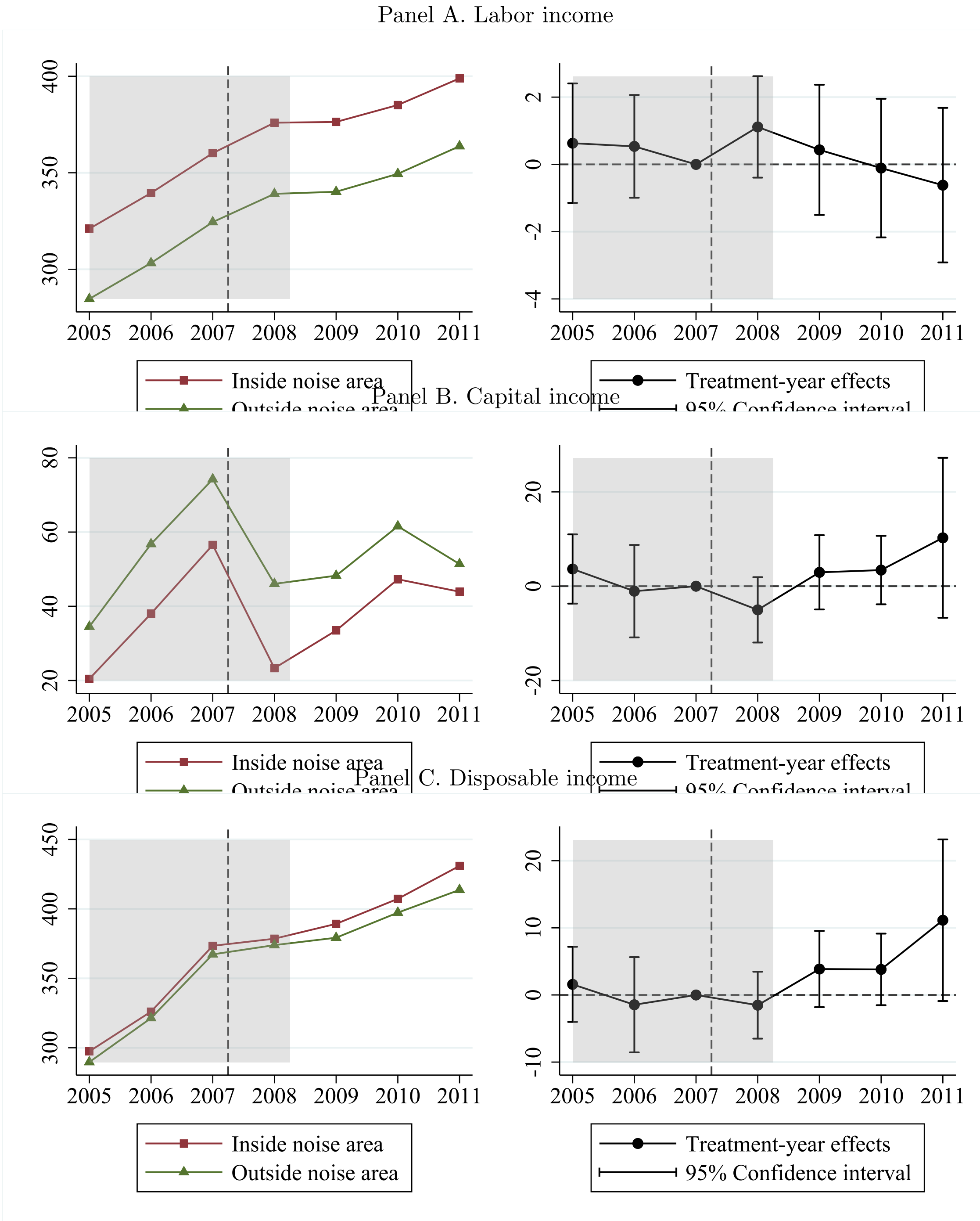
	Panel A: Inside noise area		
	Price	Living area	Rooms
Mean	2172	56.8	2.07
Standard deviation	1282	26.1	0.982
Observations	9764	9764	9740
	Panel B: Outside noise area		
	Price	Living area	Rooms
Mean	2240	64.3	2.33
Standard deviation	1700	31.2	1.10
Observations	26,608	26,608	26,579

Note: Transactions of apartments start in 2005Q1 and end in 2007Q2. Values in SEK 1,000 and area in square meters.

Figure B.1: Effect on house prices, quarterly

Note: This figure plots the quarterly house price indices outside and inside the noise area, respectively. No shifting of quarters is used. The gray-shaded region indicates the time period used in the empirical analysis.

Figure B.2: The evolution of economic variables inside and outside the noise area



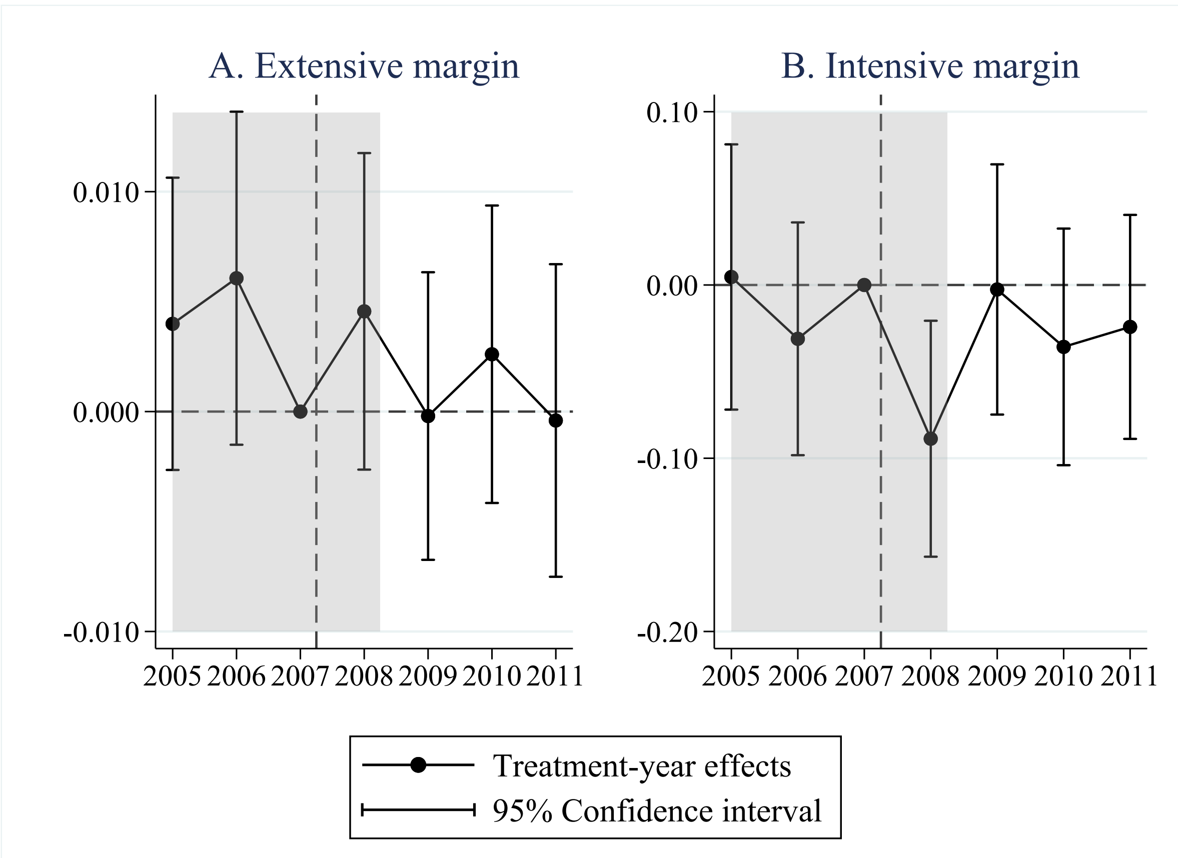
Note: This figure presents different income measures for single-family homeowners living inside and outside the noise area. All amounts are in SEK 1,000. The left graphs plot the mean level of each measure by group and the right graphs plot the differences between the groups, using a regression model as in Figure 2. No additional household controls are used in the regression but household fixed effects are included. Errors are cluster-robust at the household level. The gray-shaded region indicates the time period used in the empirical analysis.

Table B.4: Marginal propensity to borrow for all credit types

	Panel A: By LTV group			
	All credit (1)	Mortgage (2)	Consumer credit (3)	Mixed credit (4)
Low LTV, < 50%	0.458***	0.354***	0.019***	0.018**
	(0.033)	(0.031)	(0.003)	(0.007)
Mid LTV, 50–100%	0.524***	0.310***	0.036***	0.030***
	(0.044)	(0.040)	(0.005)	(0.010)
High LTV, > 100%	0.323***	0.116	0.020**	0.047***
	(0.094)	(0.087)	(0.009)	(0.017)
Low LTV – Mid LTV	−0.066	0.044	−0.017***	−0.012
High LTV – Mid LTV	−0.201**	−0.194**	−0.016*	0.017
	Panel B: By bank-deposit group			
	All credit (5)	Mortgage (6)	Consumer credit (7)	Mixed credit (8)
Bank deposits, P0–P25	0.579***	0.389***	0.029***	0.031***
	(0.041)	(0.038)	(0.005)	(0.010)
Bank deposits, P25–P50	0.619***	0.436***	0.037***	0.033***
	(0.040)	(0.039)	(0.005)	(0.009)
Bank deposits, P50–P75	0.497***	0.369***	0.019***	0.024***
	(0.040)	(0.038)	(0.004)	(0.009)
Bank deposits, P75–P100	0.199***	0.149***	0.009**	0.003
	(0.044)	(0.042)	(0.004)	(0.008)
Controls	Yes	Yes	Yes	Yes
Observations	6647	6647	6647	6647

Note: This table presents the results from estimating model (3) for homeowners in Stockholm who don't move in years when they purchase a car. The controls are 4th-order polynomials in household size, age, and disposable income. Each control variable is standardized. Errors are cluster-robust at the household level. The lower part of panel A displays the F-tests of the differences between the estimated coefficients above. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure B.3: Car outcomes



Note: This figure presents the difference in car outcomes for homeowners inside and outside the noise zone. To the left is the difference in the extensive margin, the car buying probability. To the right is the difference in the intensive margin, the logarithm of the car value at purchase. The error bands indicate the 95-percent confidence intervals.

Table B.5: Triple differences – Effects on the value of new cars by groups (intensive margin)

	Loan-to-value (1)	Bank deposits (2)	Net worth (3)	Age (4)	Financial wealth (5)
Noise area$_i$ × Post$_t$	−0.054**	−0.023	−0.063***	−0.069**	−0.053
	(0.022)	(0.027)	(0.024)		(0.041)
Noise area$_i$ × Post$_t$ × $\mathbb{1}$(LTV$_{it}$ ≥ 50%)	−0.061*				
	(0.029)				
Noise area$_i$ × Post$_t$ × $\mathbb{1}$(BD$_{it}$ ≤ P50)		−0.115***			
		(0.031)			
Noise area$_i$ × Post$_t$ × $\mathbb{1}$(NW$_{it}$ ≤ P25)			−0.058*		
			(0.029)		
Noise area$_i$ × Post$_t$ × $\mathbb{1}$(Age$_{it}$ ≤ P25)				−0.027	
				(0.040)	
Noise area$_i$ × Post$_t$ × $\mathbb{1}$(FW$_{it}$ ≤ P50)					−0.051
					(0.048)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	6045	6045	6045	6045	6045
R-squared	0.043	0.047	0.043	0.043	0.048

Note: This table presents the estimates of our main specification (5) with log(car value$_{it}$) as the dependent variable. BD, NW, and FW are short for bank deposits, net worth, and financial wealth. We control for household size, household head age, and labor income, all measured in 2006. All non-collinear interactions of Noise area$_i$, Post$_t$, and $\mathbb{1}(\cdot)$ are included in the model but not presented above. P25 and P50 stand for percentile 25 and 50, respectively. The standard errors are two-way cluster-robust at the household and quarter levels. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6: Effects on number new cars bought, by housing tenure (extensive margin)

	Single-family home owners		Co-op owners		Renters	
	(1)	(2)	(3)	(4)	(5)	(6)
Noise area$_i$ × Post$_t$	0.000 29	0.000 29	0.000 59	0.000 59	0.000 01	0.000 01
	(0.000 54)	(0.000 54)	(0.000 38)	(0.000 38)	(0.000 10)	(0.000 10)
Noise area$_i$	0.000 14	0.000 12	−0.000 77***	−0.000 50**	−0.000 47***	−0.000 30***
	(0.000 31)	(0.000 31)	(0.000 21)	(0.000 20)	(0.000 09)	(0.000 10)
Controls	No	Yes	No	Yes	No	Yes
Observations	531,105	531,105	1,837,905	1,837,905	2,959,005	2,959,005
R-squared	0.001	0.001	0.000	0.001	0.000	0.002

Note: This table presents the estimates of our main specification (5) with the number of cars bought as the dependent variable. We control for household size, household head age, and labor income, all measured in 2006. Errors are two-way cluster-robust at the household and quarter levels. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7: Effects on the value of new cars, by housing tenure (intensive margin)

	Single-family home owners		Co-op owners		Renters	
	(1)	(2)	(3)	(4)	(5)	(6)
Noise area$_i$ × Post$_t$	−0.085***	−0.077***	0.007	0.009	−0.012	−0.006
	(0.019)	(0.021)	(0.019)	(0.018)	(0.039)	(0.038)
Noise area$_i$	0.000	0.004	−0.029*	−0.026*	−0.018	−0.008
	(0.012)	(0.012)	(0.015)	(0.015)	(0.013)	(0.011)
Controls	No	Yes	No	Yes	No	Yes
Observations	6045	6045	11,065	11,065	9334	9334
R-squared	0.011	0.042	0.007	0.043	0.006	0.051

Note: This table presents the estimates of our main specification (5) with log car value as the dependent variable. We control for household size, household head age, and labor income, all measured in 2006. Errors are two-way cluster-robust at the household and quarter levels. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.8: Intensity of treatment on home prices and the intensive margin

	Single-family homes			
	log single-family home prices		log car value	
	(1)	(2)	(3)	(4)
$\text{Post}_t \times 0$	−0.288***		−0.105***	
	(0.041)		(0.028)	
$\text{Post}_t \times (0, 500]$	−0.216***		−0.052**	
	(0.055)		(0.023)	
$\text{Post}_t \times (500, 1000]$	−0.143***		−0.069	
	(0.052)		(0.042)	
$\text{Post}_t \times$ $-\log_{10}((1 + \text{dist}_i)^2)$		−0.053***		−0.015***
		(0.007)		(0.003)
Controls	Yes	Yes	Yes	Yes
Observations	11,308	11,308	6045	6045
R-squared	0.331	0.334	0.043	0.042

Note: This table presents the estimates of our main specifications (2) and (5). Columns (1) and (2) use samples of house transactions, while Columns (3) and (4) use samples of households that buy cars. When estimating model (2) we use the controls as listed in Table 2, and in model (5) as listed in Table 5. Errors are cluster-robust as in Tables 2 and 5. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C Details on the household data set

Table C.1: Restrictions on the household data set

Sample restrictions		# households
1.	Start with households that are coded as living in Stockholm municipality and own their main property in Stockholm at the end of 2006.	43,975
2.	Keep households that live in Stockholm throughout 2004– 2011.	39,700
3.	Keep households we cannot geo-locate.	39,578
4.	Keep households who own at least 50% of their home that has a positive taxation value.[a]	37,105
5.	Keep only households whose housing wealth is tied mostly in their home in Stockholm (above 50%).[b]	35,612
6.	Keep households who own less than 6 cars in one quarter.[c]	35,407

Note: The table presents the number of households that remain after imposing consecutive sample restrictions.

[a] Non-positive taxation value can indicate that the house is not used for residential purposes.

[b] This makes more certain that the household can be influenced by a wealth or collateral effect and that the household does not own significant housing outside of Stockholm.

[c] In Sweden so-called "goalkeepers" are registered owners of many cars that are in reality owned and used by other people. By being the registered owner, the goalkeeper is responsible to pay car-related taxes, fees, and fines and usually does not have income that can be confiscated to finance the costs; thus, the actual owner can escape these costs.

D The life-cycle model

This section provides a mathematical formulation of the model in Section 5, our solution method, and the calibration, and uses the model to illustrate insights discussed in the paper.

D.1 Preferences, endowments and asset markets

The household lives from $t = 1$ to $t = 221$, where t denotes a quarter. Beginning of life should be thought of as 30 years of age. The household is born with a house worth w_1^h. The household has a per-period utility function

$$U(c_t,d_t) = \frac{({c_t}^{\nu} d_t^{1-\nu})^{1-\gamma}}{1-\gamma},$$

where c_t denotes non-durable consumption goods, d_t the value of cars, ν the Cobb-Douglas expenditure share on non-durables, and γ the coefficient of relative risk aversion.

Disposable income is exogenous and stochastic and follows a standard process, akin to Carroll and Samwick (1997). A working-age individual receives disposable income y_t that follows a deterministic hump-shaped life-cycle trend, $\bar{y}_t$, and is exposed to a transitory idiosyncratic income shock, ω_{it}. Disposable income cannot be less than $\underline{y}$, which is a parsimonious way to account for welfare and transfers. For $t < 140$ (for ages younger than 65),

$$y_{it} = \bar{y}_t \exp(\omega_{it}). \tag{6}$$

The transitory random variable ω_{it} is distributed

$$\omega_{it} \sim N\left(-\sigma_\omega^2/2, \sigma_\omega^2\right). \tag{7}$$

Notice that the process abstracts from permanent income shocks to economize on state variables.

Upon retirement, which happens at $t = 140$, individuals have a safe pension income. It is modeled as a deterministic replacement rate, κ, relative to permanent labor income at 64.75 years (139 quarters):

$$y_{it} = \kappa \times \bar{y}_{139}, \quad t \geq 140. \tag{8}$$

There are two financial assets: a mortgage and liquid financial savings. The current period control variables associated with these assets are denoted by $\tilde{m}_t$ and $\tilde{s}_t$, respectively. A negative value for $\tilde{s}_t$ means uncollateralized borrowing. The beginning-of-period balance, including returns, is denoted by m_t and s_t. A mortgage payment has to be made at the end of the period. The amount depends on the end-of-period mortgage balance, $\tilde{m}_t$. The function $mp_t(m)$, defined below, determines the balance.

During the period, the household chooses to adjust their mortgage and/or stock of cars and then derives utility from non-durable consumption and cars. There is an exogenous state variable that indicates a match-quality shock denoted by ζ_t that follows a Bernoulli distribution. The probability of the shock happening is ρ. The shock makes the household want to adjust d_t. After its current period decision on the car, the car value is denoted by $\tilde{d}_t$. The cost of transacting the car is $A^d(d_t)$.

The laws of motion for the endogenous state variables are

$$d_{t+1} = \tilde{d}_t(1 - \delta), \tag{9}$$
$$m_{t+1} = \tilde{m}_t(1 + r^m) - mp_t(\tilde{m}_t), \tag{10}$$
$$s_{t+1} = (1 + r^s(\tilde{s}_t))\tilde{s}_t, \tag{11}$$

where the installment $mp_t(m)$ covers both interest and amortization so the mortgage is zero in the last period T, as in Campbell and Cocco (2003):

$$mp_t(m) = m\left(\sum_{j=1}^{T+1-t}\left(\frac{1}{1+r^m}\right)^j\right)^{-1} = m\frac{r^m(1+r^m)^{T+1-t}}{(1+r^m)^{T+1-t}-1}. \tag{12}$$

Furthermore, there are return spreads between uncollateralized borrowing, savings, and mortgage borrowing. The interest rates are ordered $r^m < r^s < r^b$, where r^m is the interest rate on the mortgage, r^s is the return on savings ($s_t > 0$), and r^b is the cost of borrowing ($s_t < 0$).

The borrowing constraints are

$$\tilde{s}_t \geq \underline{s}, \tag{13}$$
$$\tilde{m}_t \in [0, \phi\, w_t^h], \tag{14}$$

where (13) holds in every time period and $\underline{s}$ is a borrowing limit. Equation (14) holds only at the time of refinancing. The variable w_t^h is the value of the house in period t, and ϕ is a requirement on home equity at the time of refinancing. Refinancing the mortgage is associated with cost $A^m(m_t)$.

D.2 Dynamic programming problem

The household maximizes the value function $V_t(d_t, m_t, s_t, y_t, \zeta_t)$, which is defined by four cases:

1. No adjustment of either cars or the mortgage (no adj.),
2. Adjustment of the car stock (d adj.),
3. Adjustment of the mortgage (m adj.), and
4. Adjustment of both car and mortgage (d&m adj.).

Each case is associated with its own value function and Bellman equation.

The value function also depends on whether the household is hit by the car-purchase shock. Let $V_t(d_t, m_t, s_t, y_t, 0)$ denote the value function if the household is not hit by this shock:

$$\begin{aligned} V_t(d_t, m_t, s_t, y_t, 0) = \max\big\{&V_t^{\text{no adj.}}(d_t, m_t, s_t, y_t),\ V_t^{d\text{ adj.}}(d_t, m_t, s_t, y_t),\\ &V_t^{m\text{ adj.}}(d_t, m_t, s_t, y_t),\ V_t^{d\&m\text{ adj.}}(d_t, m_t, s_t, y_t)\big\}. \end{aligned}$$

Let $V_t(d_t, m_t, s_t, y_t, 1)$ denote the value function if the household is hit by the shock:

$$V_t(d_t, m_t, s_t, y_t, 1) = \max\big\{V_t^{d\text{ adj.}}(d_t, m_t, s_t, y_t),\ V_t^{d\&m\text{ adj.}}(d_t, m_t, s_t, y_t)\big\}.$$

The Bellman equations and associated budget constraints are as follows for periods $t < T$:

D.2.1 No adjustment

$$V_t^{\text{no adj.}}(d_t, m_t, s_t, y_t) = \max_{\tilde{c}_t, \tilde{s}_t} \frac{(\tilde{c}_t^{\nu} d_t^{1-\nu})^{1-\gamma}}{1-\gamma} + \beta\, \mathbb{E}_t[V_{t+1}(d_{t+1}, m_{t+1}, s_{t+1}, y_{t+1}, \zeta_{t+1})]$$

subject to (6–13) and $\tilde{c}_t + \tilde{s}_t = s_t + y_t - mp_t(m_t)$.

D.2.2 Adjusting only the car

$$V_t^{d\text{ adj.}}(d_t, m_t, s_t, y_t) = \max_{\tilde{c}_t, \tilde{d}_t, \tilde{s}_t} \frac{(\tilde{c}_t^{\nu} \tilde{d}_t^{1-\nu})^{1-\gamma}}{1-\gamma} + \beta\, \mathbb{E}_t[V_{t+1}(d_{t+1}, m_{t+1}, s_{t+1}, y_{t+1}, \zeta_{t+1})],$$

subject to (6–13) and $\tilde{c}_t + \tilde{s}_t + p\,\tilde{d}_t = s_t + y_t - mp_t(m_t) + p\,d_t - A^d(d_t)$.

D.2.3 Adjusting only the mortgage

$$V_t^{m\text{ adj.}}(d_t, m_t, s_t, y_t) = \max_{\tilde{c}_t, \tilde{m}_t, \tilde{s}_t} \frac{(\tilde{c}_t^{\nu} d_t^{1-\nu})^{1-\gamma}}{1-\gamma} + \beta\, \mathbb{E}_t[V_{t+1}(d_{t+1}, m_{t+1}, s_{t+1}, y_{t+1}, \zeta_{t+1})],$$

subject to (6–14) and $\tilde{c}_t + \tilde{s}_t = s_t + y_t - m_t + \tilde{m}_t - mp_t(\tilde{m}_t) - A^m(m_t)$.

D.2.4 Adjusting both the car and the mortgage

Lastly, we consider the case to adjust both the stock of cars and the mortgage:

$$V_t^{d\&m\text{ adj.}}(d_t, m_t, s_t, y_t) = \max_{\tilde{c}_t, \tilde{d}_t, \tilde{m}_t, \tilde{s}_t} \frac{(\tilde{c}_t^{\nu} \tilde{d}_t^{1-\nu})^{1-\gamma}}{1-\gamma} + \beta\, \mathbb{E}_t[V_{t+1}(d_{t+1}, m_{t+1}, s_{t+1}, y_{t+1}, \zeta_{t+1})],$$

subject to (6–14) and

$$\begin{aligned} \tilde{c}_t + \tilde{s}_t + p\, \tilde{d}_t = {} & s_t + y_t + p\, d_t - A^d(d_t) \\ & - m_t + \tilde{m}_t - A^m(m_t) - mp_t(\tilde{m}_t). \end{aligned}$$

D.2.5 The last time period

In the last time period, $t = T$, the household derives utility from a bequest motive, $\Psi(b)$. The household also sells the house:

$$\Psi(b) = \Psi_0 \frac{b^{1-\gamma}}{1-\gamma}.$$

It is not possible to take out a mortgage, and the car can be used in the last period and is then sold (depreciated by one more period). The value functions are as follows:

No adjustment in the last time period

$$\begin{aligned} V_T^{\text{no adj.}}(d_T, m_T, s_T, y_T) = \max_{\tilde{c}_T, \tilde{b}_T} & \frac{(\tilde{c}_T^{\nu} d_T^{1-\nu})^{1-\gamma}}{1-\gamma} + \Psi(\tilde{b}_T + p\, d_T(1-\delta)), \\ \text{subject to} \quad & \tilde{c}_T + \tilde{b}_T = s_T + y_T - mp_T(m_T) + w_T^h. \end{aligned}.$$

Adjusting the car in the last time period

$$\begin{aligned} V_T^{d\text{ adj.}}(d_T, m_T, s_T, y_T) = \max_{\tilde{c}_T, \tilde{d}_T, \tilde{b}_T} & \frac{(\tilde{c}_T^{\nu} \tilde{d}_T^{1-\nu})^{1-\gamma}}{1-\gamma} + \Psi(\tilde{b}_T + p\, \tilde{d}_T(1-\delta)), \\ \text{subject to} \quad & \tilde{c}_T + p\, \tilde{d}_T + \tilde{b}_T = s_T + y_T + p\, d_T - mp_T(m_T) + w_T^h - A^d(d_T). \end{aligned}$$

D.3 Solution method

We solve the model using nested value function iteration (Druedahl, 2021).[46] The technique exploits a nested structure of value functions: once we have solved for $V_t^{\text{no adj.}}(d_t, m_t, s_t, y_t, \zeta_t)$ over a grid, we can quickly compute $V_t^{d \text{ adj.}}(d_t, m_t, s_t, y_t, \zeta_t)$ by solving

$$V_t^{d \text{ adj.}}(d_t, m_t, s_t, y_t, \zeta_t) = \max_{\tilde{d}} \; V_t^{\text{no adj.}}(\tilde{d}, m_t, s_t + p\, d_t - A^d(d_t) - p\, \tilde{d}, y_t, \zeta_t)$$

by interpolation. If the optimal new car level is $\tilde{d}^*$, the other optimal decisions are given by the interpolations

$$\begin{aligned}
\tilde{c}^{d \text{ adj.}}(d_t, m_t, s_t, y_t, \zeta_t) &= \tilde{c}^{\text{no adj.}}(\tilde{d}^*, m_t, s_t + p\, d_t - A^d(d_t) - p\, \tilde{d}^*, y_t, \zeta_t), \qquad \forall t, \\
\tilde{s}^{d \text{ adj.}}(d_t, m_t, s_t, y_t, \zeta_t) &= \tilde{s}^{\text{no adj.}}(\tilde{d}^*, m_t, s_t + p\, d_t - A^d(d_t) - p\, \tilde{d}^*, y_t, \zeta_t), \qquad \forall t, \\
\tilde{b}^{d \text{ adj.}}(d_T, m_T, s_T, y_T, \zeta_T) &= \tilde{b}^{\text{no adj.}}(\tilde{d}^*, m_T, s_T + p\, d_T - A^d(d_T) - p\, \tilde{d}^*, y_T, \zeta_T).
\end{aligned}$$

We can solve the case of adjusting the mortgage and the case of adjusting both cars and the mortgage in a similar way.

Furthermore, we follow Druedahl (2021) in that we reduce the dimensionality of the state space by summing savings, uncollateralized borrowing, and income into the variable cash-on-hand, $x_{t+1} \equiv \tilde{s}_t(1 + r(\tilde{s}_t)) + y_{t+1}$. Each case of adjusting one variable at a cost also makes it possible to reduce the state space by one state: when changing the car stock, the car is sold, and the adjustment cost is paid (since it only depends on today's initial car stock), and what is left is added to that period's cash-on-hand, x_t^d. In this case, we solve over a grid over (x_t^d, m_t); the initial d_t only affects the decision $\tilde{d}_t$ through its contribution to the budget constraint. Correspondingly, when adjusting the mortgage, we solve over a grid (x_t^m, d_t) where $x_t^m \equiv x_t - m_t - A^m(m_t)$. In the case of adjusting both the level of cars and the mortgage, we define $x_t^{d,m} \equiv x_t - m_t - A^m(m_t) + p\, d_t - A^d(d_t)$ and solve over a grid over only this measure of cash-on-hand (i.e., both states d_t and m_t can be dropped).

D.4 Calibration

For most parameters, we use standard values or values consistent with the Swedish institutional setting. Those parameter values are reported in Table D.1. The adjustment costs for the durable good and the mortgage are worth discussing.

For cars, we have studied the pricing model of used car dealers and noted that they discount their procurement price by a relative amount.[47] Thus, we assume that the adjustment cost of cars is proportional to the car value, $A^d(d) = \eta_1 d$, and we set that value to 15 percent. For mortgages, we assume that the adjustment cost is constant as in e.g., Eichenbaum et al. (2022); Berger et al. (2021): $A^m(m) = \mu_0 =$ SEK 18,400.

[46]Because it is a life-cycle model, we actually use backward induction and not iteration, but the method is known as nested value function *iteration*.

[47]Our source is a 2006 catalog produced by Autodata that published valuations of used cars in Sweden.

Table D.1: Calibration

Description	Parameter	Value	Source
Preferences and income			
Risk aversion	γ	2.0	Standard
Income profile	$\bar{y}_t$	–	Dahlquist et al. (2018)
Transitory income risk	σ_ω	0.178	Micro data
Financial savings and uncollateralized borrowing			
Interest rate on savings	r^s	1.337%	Sveriges Riksbank
Interest rate on borrowing	r^b	1.692%	Statistics Sweden
Borrowing constraint	$\underline{s}$	−200.0	Sodini et al. (2022)
Cars			
Relative price	p	1.0	Attanasio et al. (2022)
Depreciation rate	δ	3.599%	Autodata
Adjustment cost	η_1	0.15	Autodata
Mortgages			
Interest rate	r^m	1.103%	Statistics Sweden
Borrowing constraint	ϕ	85%	Swedish bank norm
Adjustment cost	μ_0	18.4	Eichenbaum et al. (2022) ($2,100) and Berger et al. (2021) ($2,500).
Housing wealth			
Before announcement	w^h	3218.0	Micro data
After announcement	$w^{h'}$	3842.3	Micro data

Note: This table presents parameter values determined by historical values and institutional features. The average income per period, $\bar{y}_t$, follows the age profile in Dahlquist et al. (2018), scaled to match the average of our sample. We calculate the adjustment cost and depreciation rate for cars from Autodata price catalogs. Interest rates are reported at a quarterly frequency. Amounts are in SEK 1,000.

D.4.1 Fitted parameters

We fit five parameter values to target moments. The time discount factor (β) and the bequest parameter (Ψ_0) are chosen to fit liquid financial wealth (i.e., financial wealth outside the pension system) during the working phase and the retirement phase. The Cobb-Douglas expenditure share (ν) is chosen to match an approximate value of the car stock, including used and new cars. Based on car advertisements, we approximate the average value of used cars in 2006 to SEK 75,274. For new cars, our micro data set indicates a value of SEK 220,200. Given the average transaction frequencies of used and new cars, we match a target value of SEK 100,030. We compute the car transaction probability in the data set, 5.365 percent per quarter, and set the probability of exogenous match quality shocks, ρ, to match the overall frequency in the data. About half of the car adjustments are determined by the exogenous shock. The probability of information arrival, λ, is set in accordance with McKay and Wieland (2021) and Coibion and Gorodnichenko (2012).

The parameter values are reported in Table D.2 and the target moments in Table D.3. Below, we provide further details on how we fit the model.

D.4.2 Method

Fitting the model is a two-stage process. The first stage consists of the following steps:

We make an initial guess of the parameters (step 0), solve the model at the initial housing wealth w^h (step 1), and simulate the model for a large sample of households (step 2). After this, we compute the moments of the simulated data that we are targeting (step 3), and test if the total error is below a tolerance level (step 4). If it is, we continue to stage 2; otherwise, we make a new guess (step 5).

The new guess is computed by studying the simulated error of each moment and the percentage deviation. We associate each moment with a particular parameter. The level of savings of younger households are associated with the time discount factor β; the savings of older households are associated with the bequest motive Ψ_0; the average level of the car stock with the non-car preference parameter ν; and the car purchasing frequency with the match-quality shock frequency ρ. If the average car stock is above the life-cycle target, we increase $\nu^{\text{new guess}}$ by some relative amount proportional to the percentage deviation of the moment. (An increase in the non-car preference parameter makes cars less desirable, and the household will thus on average buy less cars). Likewise, if young savings are too high, we decrease the time discount factor β; if the saving of the old is too high, we decrease the bequest motive Ψ_0; if the frequency of car purchases is too high, we decrease ρ.

Then we return to step 1 and continue from there. Once the test in step 4 is passed, the first stage is completed, and we continue to stage 2.

In stage 2, we set up a simplex in the parameter space around the set of values obtained from stage 1. We pass the simplex to an optimizer that uses the Nelder-Mead method that goes through steps 1 to 4 above using a lower tolerance level. Step 5 is replaced by the internal updating method of the Nelder-Mead method.

The value for one parameter requires an even more elaborate process. The frequency for information arrival, λ, requires a change in the house price to be identified. Using the bisection method, we compute the λ that makes the households' average time to update

Table D.2: Fitted parameters

Description	Parameter	Value
Time discount factor	β	0.981
Bequest parameter	Ψ_0	533.2
Preference parameter	ν	0.945
Probability of match shock	ρ	0.029
Probability of information arrival	λ	0.063

Note: This table presents the fitted parameters (see Section D.4.1).

Table D.3: Target moments

Summary statistic	Target value	Simulated value
Financial wealth (s_t), age 35–65	918.59	922.88
Financial wealth (s_t), age 65–85	1478.12	1468.77
Value of car stock (d_t)	100.30	100.09
Car purchasing freq. (%)	5.385	5.365
Time to update after house price shock	6.000	6.006

Note: This table presents the targets of the estimation using the method outlined in Section D.4.1. Savings and car stock are in SEK 1,000; the car purchasing frequency is quarterly; and the time to update information after the house price shock is stated in quarters and represents the average time. This statistic is based on McKay and Wieland (2021) and Coibion and Gorodnichenko (2012).

their information six quarters (as in McKay and Wieland, 2021; Coibion and Gorodnichenko, 2012). At a random time, we shock the household by changing the current housing wealth from the initial value to $w^{h'}$; the household is not aware of this until they update their information set. There are now three cases when they update: when deciding it is optimal and using their 'un-updated' information set, (1) to buy a new car, (2) to refinance the mortgage, or (3) when they are hit by the information-arrival process. The parameter λ controls the frequency of the latter and is tuned to make the average time for an update six quarters after the shock. Given that the quasi-experiment occurs in normal times, just before the U.S. financial crisis spread globally, we find this frequency to be reasonable.

D.5 Computing the marginal propensity for car expenditures

While the model makes no distinction between new and used cars, we only observe values for new cars in our empirical analysis. Therefore, we have to make an assumption about the new-car share of total car expenditures to be able to compare the new car MPX in the data with the car MPX in the model.

We assume that households reduce expenditures of each type of car proportionally to their expenditure share. To approximate the share of expenditure spent on new cars we rely on statistics from webcar2000.com.[48] We find that the average value in used car ads in 2006 was SEK 75,274. New cars were about SEK 220,200 (computed from our micro data set). Given the probabilities of buying used and new cars, the total share of new cars out of all car expenditures is 0.315.

The black circle in Panel B of Figure 4 relies on this share. It is a conversion of our new-car MPX estimate to an all-car MPX of 0.38 cents per dollar (0.0012/0.315). The 95-percent confidence interval is 0.146–0.620 cents per dollar, which we obtain from a conversion of the 95-percent confidence interval of the elasticity estimate, 0.150–0.636, in Column (2) of Table 6.

D.6 The role of the measurement period and the regression specification

In the main text, we emphasize that the temporal dynamics of car purchases have important implications for the appropriate empirical design. Below, we expand on this.

D.6.1 The role of the measurement period

To illustrate the role of the length of the measurement period, we estimate reduced form responses based on the simulated paths:

$$y_{it} = \alpha + \phi \,\text{Treated}_i \times \text{Post}_t + \gamma \,\text{Post}_t + \rho \,\text{Treated}_i + \varepsilon_{it}, \tag{15}$$

where y_{it} denotes one of the outcome variables, displayed in Figure 3. This regression specification corresponds to the empirical difference-in-difference model (5). We present estimates of ϕ in Table D.4 for different lengths of the post-period. Panel A is based on a post-period of four quarters, as in our empirical setting. Columns (1) and (2) show that the total spending response in the first year is SEK 1,836 per quarter and the car spending response is SEK 996 per quarter, respectively. Despite cars' small expenditure share on average (6.9 percent in the simulation), they dominate the change in household expenditures in the first year, with a response share of 54 percent. Column (3) shows a small extensive margin response (-0.3 percentage points per quarter relative to a baseline probability of 5.37 percent per quarter).[49] We also see in Column (4) that the intensive margin response over the four following quarters is 6.1 log points, close to our empirical estimates of 7.7–8.5. The house price divergence between the treatment and control groups corresponds to a relative loss in housing wealth of SEK 624,200. This implies that the (per-year) car MPX in the first year is 0.64 cents per dollar ($996 \times 4/624{,}200$). Given that 31.5 percent of all car expenditure

[48] Source: http://www.webcar2000.com/countries/sweden/car/statistics.phtml.

[49] We find no extensive margin response in the empirical analysis. This can be due to a lack of statistical power. If we compute the standard error of the model estimate for a simulated sample of the same size as the empirical sample, the t-statistic of a Welch's t-test is -1.84. We estimate model (5) with the outcome being a dummy variable $\mathbb{1}(\text{num. bought cars}_{it} > 0)$. The estimate is 0.00071, and the standard error is 0.00109. The re-scaled standard error from the model when using a sample of the size of the empirical sample is 0.00171, which is similar. The empirical outcome is not unlikely enough to be rejected by the model outcome using conventional levels of statistical significance.

is on new cars (see Appendix D.5), the model gives a new-car MPX in the first year of 0.20 (0.64×0.315) cents per dollar. This is close to our empirical estimate of 0.12 cents per dollar.

The four-quarter results of Panel A are the most relevant for a comparison with our quasi-experiment. However, to complete the picture of the model dynamics, Panel B of Table D.4 reports estimates for longer post-periods. The estimates align with the dynamics in Figure 3; the change in total expenditures does not peak until 6–8 quarters after the shock, while cars' expenditure share falls. The reversion of car expenditures is driven by the intensive margin as extensive margin responses are minute relative to the baseline. We will discuss this reversal below.

D.6.2 The role of the regression specification

The regression specification of Mian et al. (2013) and Aruoba et al. (2022) is different from ours. The outcome variable is the three-year difference (2009 versus 2006) in county-level expenditures: $\Delta_q c_{jt_0} \equiv c_{jt_0+q} - c_{jt_0}$, where j denotes the county and q the length between the observations at t_0 and $t_0 + q$. Their regression specification can be written

$$\Delta_q c_{jt_0} = \beta\, \Delta_q \text{home prices}_{jt_0} + \eta\, X_{jt_0} + \varepsilon_{jt_0},$$

where X_{jt_0} is a set of control variables and β is an estimate of the MPC. In Mian et al. (2013), $t_0 = 2006$, $q = 12$ (i.e., 3 years). They also instrument for $\Delta_q \text{home prices}_{jt_0}$. In our model-based regression, the specification translates to

$$\Delta_q c_{t_0} = \beta\, \Delta_q w^h_{t_0} + \varepsilon_{t_0}. \tag{16}$$

In contrast, our specification measures the average change over the measurement period. Table D.5 illustrates the role of the regression specification by reporting estimates on simulated data based on regression specification (15) and (16).

Table D.4: Reduced form estimates for different post measurement periods (model)

	Panel A. 4 quarters post period			
	Total expenditure (1)	Car expenditure (2)	Prob. to buy car (3)	log car value | car purch. (4)
$\text{Treated}_i \times \text{Post}_t$	-1.836^{***} (0.085)	-0.996^{***} (0.081)	-0.003^{***} (0.001)	-0.061^{***} (0.001)
Observations	3,200,000	3,200,000	3,200,000	173,181

	Panel B. Longer post periods			
	16 quarters		32 quarters	
	Total expenditure (5)	Car expenditure (6)	Total expenditure (7)	Car expenditure (8)
$\text{Treated}_i \times \text{Post}_t$	-2.071^{***} (0.068)	-0.643^{***} (0.064)	-1.556^{***} (0.063)	-0.168^{***} (0.063)
Observations	8,000,000	8,000,000	14,400,000	14,400,000

Note: This table presents the estimated coefficients of model (15) using simulated data. The length of the post-period used after the shock to house prices is indicated by the column head; the length of the pre-period is irrelevant but we use 4 periods. For each outcome, we simulate 200,000 households twice (one treated- and one control-copy of each). All amounts are in SEK 1,000. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D.5: MPXs based on different regression specifications

	Panel A. Total expenditures					
	Housing wealth shock −30%			Housing wealth shock −10%		
Post-period length q	4	8	16	4	8	16
$\text{Treated}_i \times \text{Post}_{t,q}/\Delta_q w^h$	0.0075	0.0079	0.0091	0.0113	0.0111	0.0116
$\Delta_q w^h_{jt_0}$	0.0065	0.0089	0.0110	0.0099	0.0115	0.0109

	Panel B. Car expenditures					
	Housing wealth shock −30%			Housing wealth shock −10%		
Post-period length q	4	8	16	4	8	16
$\text{Treated}_i \times \text{Post}_{t,q}/\Delta_q w^h$	0.0042	0.0029	0.0021	0.0067	0.0045	0.0030
$\Delta_q w^h_{jt_0}$	0.0016	0.0014	0.0010	0.0032	0.0020	−0.0001

Note: This table presents the different estimates of our model (15), and the alternative model (16), using simulated data with different housing wealth shocks and different post-period lengths.