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The Housing Wealth Effect: Quasi-Experimental Evidence

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Abstract

We exploit a quasi-experiment to provide new evidence on the magnitude of the housing wealth effect. We estimate an immediate shock of approximately -15% to house prices close to one of Stockholm's airports after its operations were unexpectedly continued as a result of political bargaining. This source of price variation is ideal to identify housing wealth effects since it is local and unrelated to variation in macroeconomic conditions. Using a household data set with granular geographic information on location of primary residence, we find an elasticity of 0.45 among purchasers of new cars. Converting our estimate to an aggregate MPC on cars, it is however only 0.13 cents per dollar. The MPC is entirely concentrated to homeowners with a combined loan-to-value ratio between 0.6 and 0.8 which, on the one hand, confirms the key role of household balance sheets but on the other hand refutes a monotone relationship between response and household leverage.

JEL classification: D12, E21, E32, E44, E60.

Keywords: House prices, housing wealth, consumption, house price elasticity, marginal propensity to consume, collateral effect.

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1 Introduction

A fundamental topic in Economics that has received a lot of attention since the financial crisis is how the housing and housing finance 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.[1] Nevertheless, empirical studies found mixed results, depending on the use of aggregated data (e.g., Carroll et al. (2010, 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 how to interpret estimates. Overall, contributors to the literature remained skeptical of their own estimates due to poor identification.[2] 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)) or city-wide variation in sensitivity to regional house prices (Guren et al. (2018)). Such estimates make strong assumptions on consumption demand factors being either observed to the econometrician or uncorrelated with supply elasticities. Concerns have been raised that these assumptions are not met (Davidoff (2016); Liebersohn (2017)).[3]

This paper adds to this literature in three ways. The first contribution is that our estimates of the housing wealth effect is based on a quasi-natural experiment. 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 for instance

[1]See footnote 2 of Berger et al. (2018) for a literature review.

[2]See Appendix F (Table 20) for a literature review.

[3]Mian and Sufi (2014) and Charles et al. (2017) establish strong interaction effects between the housing and labor markets. Liebersohn (2017) finds that elasticities are reduced by 40% once he controls for regional industry composition. See also page 3433 of Aladangady (2017) for a discussion.

Chay and Greenstone (2005) and Currie et al. (2015). We use unanticipated news from political bargaining in Stockholm regarding the continued operation of Bromma Airport to isolate a casual effect on households' housing wealth as 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 it is capitalized into house prices within one quarter of the news announcement.[4] Using a data set on all transactions of single-family houses in Stockholm, we document a price decrease of 15% close to the airport's noise contour relative to further away. We use this finding in our rich household-level data set. This data set includes the geographic location of primary residence and we document a decrease in purchase prices of new cars of 6% close to the noise contour relative to households that reside further away. A two-sample IV approach establishes a sizeable elasticity of 0.45 among households that buy a new car. In contrast, we find little evidence for a response among purchasers of used cars or extensive margin effects. While aggregation is always tricky, and in principle state-dependent (see, e.g., Carroll and Dunn (1997) and Berger and Vavra (2015)), our estimates suggest that the housing wealth is about 4 times smaller than in the recent literature. Translated to an aggregate MPC out of housing wealth on cars, our elasticity amounts to only 0.13 cents per dollar. Nevertheless our estimate is in line with a simple life-cycle permanent income model.

The second contribution of this paper is that we exploit our rich household-level data set to document cross-sectional heterogeneity in the elasticity. Informed by recent developments in incomplete market macroeconomics, we argue that the nature of house price variation in our quasi-experimental design is identical to the partial equilibrium wealth shock which is analyzed in Berger et al. (2018) and discussed in Kaplan et al. (2017). That is, it is akin to a destruction of housing wealth stemming from a shift in preferences or beliefs rather

[4] Our means of identification is strikingly similar to the one proposed by Carroll et al. (2010), page 17: "[...] 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."

than a destruction stemming from shifts in labor income or in credit market conditions. We confirm recent research (Mian et al. (2013); Aladangady (2017)) that balance sheet conditions, in particular a household's combined loan-to-value (CLTV) ratio, is critical for a household's response. We find that homeowners finance new car purchases almost entirely by taking on additional debt and consistent with this, the entire elasticity is concentrated to homeowners with a CLTV ratio between 0.6 and 0.8. This implies that the response is hump-shaped in the CLTV ratio, rather than monotonically increasing as proposed from analysis of geographically aggregated data. Furthermore, we confirm that elasticity between house prices and new car purchases vary substantially across groups of different income, housing wealth, and net worth. This cross-sectional variation in our estimates is consistent with the theoretical predictions of Berger et al. (2018).

Finally, since our measure of household consumption is cars, our paper contributes to a large literature that estimates or tests the predictions of the (S, s) model. While households' car purchases have been used previously to estimate housing wealth effects, our data is of such high quality that it de facto represents a novel test of the of the (S, s) model applied to the market for cars.[5] We find weak evidence in favor of the simplest model in that we cannot rule out a zero extensive margin effect on car purchases. That is, the likelihood of households' buying a car appears largely unaffected. Our estimates suggest that a negative effect of a magnitude greater than -0.2% on the likelihood of purchasing a car can be ruled out with 97.5% probability. Our findings is thus in contrast to for instance Bar-Ilan and Blinder (1992) and instead suggestive in support of richer (S,s) models that allows for stochastics in the non-convex adjustment cost (Caballero and Engel (1993, 2007)) or strong time variation in the extensive margin response (Berger and Vavra (2015)).[6] This finding has important

[5]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). There is also important general equilibrium work – recent contributions include Gavazza et al. (2014).

[6]Foote et al. (2000) and Caplin and Leahy (2010) claim that the microeconomic evidence for the (S,s)

implications for inference on consumption based on observing only quantities of transacted cars at the regional level without further information about prices on those quantities.

The rest of the paper is structured as follows. Section 2 reviews the institutional setting and the quasi-experiment, Section 3 describes the data and Section 4 discusses the empirical strategy. Section 5 reports the results, and Section 6 concludes.

2 Institutional Setting and the Quasi-Experiment

This section provides an overview of Bromma Airport's history and explains the quasi-experiment.

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 airport. 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 establishment of the major international airport of Arlanda in 1959, Bromma airport saw a sharp decrease in traffic. By 1983 the traffic mainly consisted of private aviation. In 1992 the conservative-liberal national government opened up commercial airfare to competition and Bromma airport experienced an increase in traffic again. During the late 1990s and early 2000s, however, there was a general perception that Bromma airports would be closed – at latest 2011 when the operating contract would expire.[7]

model is surprisingly weak.

[7] There was a series of reports planning for the shutdown. In 1989 the municipality of Stockholm presented a major report proposing closing the airport by 1996 and using the centrally located area for housing (Stockholm, 1989). In 1994 the national government put together a commission tasked with the objective to figure out how fast Bromma airport could be phased out (Kommunikationsdepartementet, 1996). In 2000 Swedish Civil Aviation Administration presented a report on how they would phase out and eventually close Bromma airport by 2011 (Luftfartsverket, 2000).

The owner of the land where the airport is located is the municipality of Stockholm. The municipality has been letting it to the airport ever since 1936. The only political party in the municipality that during this period was consistently in favor of extending the contract beyond 2011 was the conservative party. In the 2006 election the conservative party in Stockholm increased their seats by more than 40 percent, from 27 to 41 out of 101 seats. This was the best result ever for the conservative party. The election outcome boosted the bargaining power of the conservative party in the negotiations with smaller parties in the center-right coalition. Rapidly, and behind closed doors, a new contract was negotiated. The new contract meant that the airport's operations would be extended to 2038. The new contract was disclosed at a press conference in September 2007. All the opposition parties issued minority reports before the council meeting where the new contract was debated, calling the process a coup. The news about the new contract was widely reported in local media.

2.2 Bromma Airport as a negative externality to residents

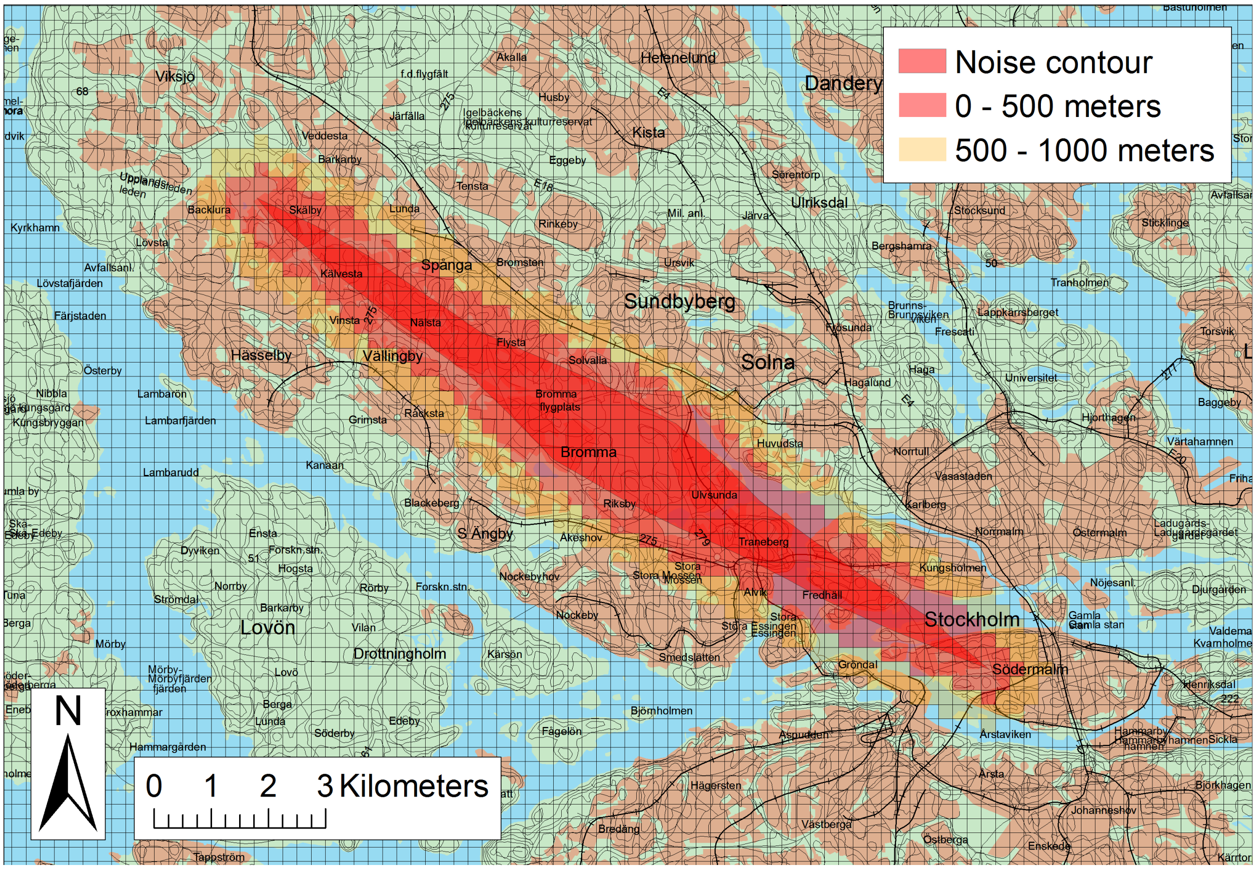
The reason for the political controversy surrounding Bromma Airport is its geographic location. The airport is surrounded by residential housing and it generates a substantial negative externality on its surroundings - not the least in terms of noise. Figure 1 displays the noise contour from Bromma Airport as a dark red ellipse. The area inside the noise contour is subject to noise levels of 70 decibel (henceforth referred to as Lmax).[8] The contour is regarded as the best measure for defining the geographical area that is exposed to hazardous noise, as confirmed in a case in the Land and Environment Court (Miljööverdomstolen, 2010).[9]

[8]The decibel scale is logarithmic. 60 decibels corresponds to a conversation in an office. 70 decibels are twice as loud. A vacuum cleaner is 70 decibel and makes it difficult to have a conversation. 90 decibel is four times as loud as 70 decibel and is the level of noise of a motorcycle at 8 meters distance. A Boeing 737 generates 97 decibels before landing at a distance of one nautical mile (1853 meters).

[9]The Land and Environment Court ruled that within this zone LFV, the regulatory body that oversees aviation in Sweden, must reimburse sound insulation.

The contour is based on a selected locations for noise measurement. The measurement data serves as input for interpolation and simulation in forming the contour. The measurement error of the border is +/- 100 meters.

Figure 1: **Noise propagation around Bromma Airport**



Note: The map shows the noise propagation around Bromma Airport along its runway which stretches from North-West to South-East. The dark red area is referred to as the noise contour. Within the noise contour the Land and Environment Court estimated the noise to exceed 70 decibles. The treatment region in our baseline specification is extended to also 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 $\times$ 250 meters. Source: Miljödomstolen (2006) and own analysis.

Though houses within the noise contour are entitled to sound insulation according to the court ruling, Bromma Airport has permission to service take-offs and landings that generate more than 70 decibels between 6 am in the morning and 10 pm in the evening. Each aircraft type that operates on Bromma Airport must be tested and the upper bound is 89 decibels.[10]

While noise is the obvious externality there are subtler negative externalities as well. Among those are transportation of fuel, the risk of accidents at take-off and landing, dumping of fuel etc. Thus, even though the established noise contour is the starting point for identifying geographical differences in negative externalities we acknowledge that the magnitude of the negative externality is not perfectly measured and that the noise contour of 70 decibels is only a proxy for exposure. In the following we discuss alternative definitions of the treatment and control group.

2.3 Definition of treatment and control group

Noise transcends continuously and does not stop at the boundary of the contour. We define our baseline treatment group as the area that is exposed to 60 decibels or more. If the noise is generated from a point source, it decreases by 6 decibels for every doubling of distance from the source. Let L_1 denote the decibel level at a distance of r_1 meters from the source. The noise level at a distance of r_2 meters is then given by L_2:

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

This equation implies that if the boundary of the noise contour is located on average 500 meters from the source (i.e., a distance of two grid squares on the map in Figure 1), we include residents located 1,000 meters further away in the baseline treatment group. This extended area, marked as yellow in Figure 1, is henceforth referred to as the noise area. Provided that the noise contour is approximated with an error of +/- 100 meters and household are located

[10] A noise level of 89 decibels is well above the threshold for mandatory ear protection must be used at Swedish work places. It is so loud that people will not be able to have a screaming conversation. In 2015 it was debated whether the noise calculations in the certificates had been manipulated and whether, in fact, some aircrafts generate more than 89 decibels. Thus, during day time the level of noise that some residents are exposed to may exceed 89 decibels.

with some error (within a grid square) it is arguably a reasonable baseline definition.

The control group consists of homeowners in the municipality of Stockholm that are located further away. In the following refer to real estate properties and households as being located either inside or outside the noise area. Later on we show that our estimates are robust to alternative definitions of the treatment and control group.

Based on the definition of treatment and control, we argue that the unexpected renewal of the airport contract in September 2007 is an exogenous shock to house prices for houses located inside the noise area. Thus, a standard difference-in-difference (DID) design can be used.[11] We return to details of the DID specification in Section 4.

3 Data

We use two data sets in the analysis. First, we establish a house price effect of the contract renewal in a data set on transactions of property. Second, we analyze consumption responses in a household data set.

3.1 Data on real estate transactions

Our transactions data is from the Swedish Land Surveying Agency. The Agency is responsible for registering title deeds, site-leasehold rights, and mortgage deeds. The data set covers all real estate transactions in the municipality of Stockholm and covers nine years; January 2004 to December 2012.

The data set includes the transaction price and the date of the transaction for each piece of real estate. We exclude pieces of real estate that are not intended to serve as a single family' primary residence, such as commercial properties, boat houses and fishing

[11]We are by no means 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. See discussion in Section 5.1.1.

huts. We do however include transactions of plots of land on which a single-family house has not yet been built. The data set includes a large number of characteristics for each piece of real estate. The set of variables include the geographic location in the form of GIS coordinates, the area of the plot of land, 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 to assess the value of the property. In total our sample covers 19,630 transactions.[12]

Table 1 reports summary statistics for these transactions. Panel A reports means and standard deviations of all transacted properties over the nine years. The average value per transaction is SEK 3.3 million and involves a single-family house with a living area of 117 square meters along with a total land area of 539 square meters. Panel B restricts the sample to transaction that took place up until 2008Q3 (i.e., up until the bankruptcy of Lehman Brothers). Since our aim is to isolate the effect of the contract's renewal in September 2007 we will focus on this time period in much of our analysis. Panel C and D compares transactions inside and outside the noise area (within 1,000 meters away from the noise contour versus further away). Real estate properties are essentially balanced before the renewal with two exceptions. Inside the noise buildings are older (78 versus 52 years) and the share of properties with site-leasehold rights (rather than outright ownership of the land through title deeds) is lower (12.5% versus 18.8%).[13]

[12]For 943 transactions some or all of the variables are missing. This is the case if the property is sold during the construction of house or if it is a non-residential property such as a boat house or a fishing hut. We have dropped these observations. We have also dropped two transactions with price zero and three transactions with a price in excess of SEK 40 million which we believe are miscoded (there are no such expensive houses in Stockholm municipality).

[13]Approximately five percent of all transactions involve real estate properties with a very low price. Most likely, these properties are plots of land with no houses on them. We choose to include them in our analysis of house prices since the effect on their market value is part of households' wealth shock. Nevertheless, we henceforth refer to house price effects of the renewal despite that a small share of properties only consist of land.

Table 1: **Real estate transactions in the municipality of Stockholm (2004–2012)**

	Price (kSEK)	Tax value (kSEK)	Age (Years)	Standard (Index)	Plot area (m^2)	Living area (m^2)	Non-living area (m^2)	Site-leasehold right (0/1)
Panel A. Full sample (2004Q1-2012Q4)								
Mean	3351	1751	77.16	28.37	539.5	116.7	47.67	0.155
SD	2244	822.9	209.2	4.320	367.2	38.12	59.74	0.362
N	19,777	19,777	19,666	19,666	19,777	19,666	19,666	19,777
Panel B. Before financial crises (2004Q1-2008Q3)								
Mean	2947	1762	71.35	28.61	540.6	117.2	47.81	0.174
SD	1955	857.6	189.3	4.323	349.4	38.52	35.93	0.379
N	11,321	11,321	11,308	11,308	11,321	11,321	11,308	11,308
Panel C. Inside noise area and before renewal of contract (2004Q1-2007Q3)								
Mean	2807	1729	51.47	28.97	563.5	120.1	48.17	0.125
SD	1517	725.0	61.00	4.386	329.2	36.49	40.12	0.331
N	2330	2330	2329	2329	2330	2329	2329	2330
Panel D. Outside noise area and before renewal of contract (2004Q1-2007Q3)								
Mean	2672	1766	78.17	28.32	533.1	115.6	48.19	0.188
SD	1893	837.2	217.0	4.173	363.1	39.29	34.40	0.390
N	6926	6926	6926	6926	6926	6926	6926	6926

Note: All values in 1000s of Swedish kronor (kSEK). The exchange rate is about 8 SEK/USD.

3.2 Data on households and their cars

The household data set, provided by Statistics Sweden, covers all households whose residential address is located in the municipality of Stockholm and is based on registers. Within the municipality of Stockholm there are about 410,000 dwellings. Approximately 90% of those are apartments and 10% are single-family residential houses. We focus on households that own single-family houses because it is difficult to assign market values to apartments based on Swedish registries. Our sample consists of 41,147 households (corresponding to 112,581 individuals). Statistics Sweden uses GIS coordinates to locate each household's residential address on the 250 times 250 meter grid illustrated in Figure 1. Previous studies that rely on the household as the unit of observation have typically relied on survey data with potentially selection in responses (e.g., Aladangady (2017)).

We have detailed balance sheet information for each household at annual frequency from the end of 1999 to the end of 2007.[14]

For each household the market value of residential real estate is reported. However, in principle the property may be located somewhere else, e.g. if it is a summer house that for tax purposes is as a property intended to serve as a permanent home. There are cases where the location of the residential address on the grid indicates that it is unlikely that the household owns the property in which it lives. We choose to exclude households whom Statistics Sweden locates outside of Stockholm municipality or in the city center where there is no single-family residential housing. We also choose to exclude households whose primary property is untaxed property because such properties are unlikely to be residential. We also exclude households that own several properties since we cannot infer which ones are located on the grid. Lastly we restrict attention to households owning at least 50% of a property. After implementation of these restrictions it is reasonable that households' housing wealth

[14]The balance sheet information was no longer collected upon the abolishment of the wealth tax in 2008. The balance sheet data module is described in detail in for instance Calvet et al. (2007) and Vestman (2018).

refers to a property which is located in the municipality of Stockholm and hence on the grid. We arrive at a sample of about 35,388 households that own single-family houses.[15] Since the municipality of Stockholm municipality had a total of 41,000 single-family houses in 2006 we cover 86% of them.

Table 2 reports statistics about the all households at the end of 2006. We focus on this year since it the most accurate information before the renewal of Bromma Airport's contract. Columns 1 and 2 report statistics for the full sample. The table also reports the statistics for those outside the noise area (Columns 3 and 4) and those inside (Columns 5 and 6). Panel A reports that households inside the noise area have on average 2.88 members. Households outside have marginally fewer (2.80). The average age of the oldest household member is 53 years inside the noise area and 54 outside. Panel B shows that in terms of wealth and other balance sheet metrics the two groups are very similar. The average financial wealth is SEK 959,000 versus SEK 979,000. By Swedish standards this is substantial amounts of financial wealth – Vestman (2018) reports that average financial wealth among home owners equals SEK 448,400 for 2000–2007. The difference indicates that the sample of single-family house owners in Stockholm is a positively selected group – we return to these implications when we discuss the aggregate implications. Based on Statistics Sweden's appraisal model for single-family houses both groups' average housing wealth is just over SEK 3.9 million, of which 3.4 million is tied to the single-family house located in the municipality of Stockholm and 0.5 million is tied to other forms of real estate. In our quasi-experiment we define the housing wealth shock as hitting the stock of single-family houses since we know where their geographic location through the household's official address.[16] At SEK 1.25 million, total

[15] A detailed description of sample restrictions can be found in Appendix A (Table 10).

[16] For each municipality, Statistics Sweden uses the ratio between observed transaction values and the tax value (which is based on the index for quality – see Table 1) to appraise the stock of single-family houses. Prior to the extension of the operating contract (i.e., up until the 2006 panel wave) this is an unbiased and quite accurate estimate of the market value. For the 2007 wave, the imputation method is too coarse geographically to fully detect the housing wealth shock.

household debt is also very similar for the two groups. We define the combined loan-to-value (CLTV) ratio as total debt divided by housing wealth.[17] The average CLTV ratio is somewhat lower inside the noise area (0.37) than outside (0.42). These CLTVs are quite moderate and consistent with households being middle-aged and fairly wealthy. That the CLTV ratios are low for many households in our sample implies that it is feasible to identify a pure housing wealth effect, isolated from the effect of collateral constraints. In our analysis we hence exploit heterogenous treatment effects across the CLTV distribution. Housing wealth to net worth is also very similar for households inside and outside (1.50 versus 1.43). The high standard deviations indicate the non-linear nature of the measure. Gross labor income and disposable income (gross labor income plus transfers minus taxes) are also very similar and higher than the average Swedish households'.

In addition to demographic and balance sheet information we observe car ownership. The data set includes model of the car, the year it was produced, how many previous owners it has had, and the mileage. For each year and each car we see the last three transactions, with each transaction's date and who sold it and who bought it. Panel C of Table 2 shows that car ownership is widespread. 81.4% of households own at least one car one average each household owns 1.14 cars. The two geographic groups are very similar also on car ownership; the average age of the cars is 4 years and average mileage is 124,000 kilometers. Since the properties of owned cars are important state variables in (S, s) models, and the aggregate response depends on the entire cross-sectional distributions of these state variables, Appendix A (Figure 5) validates that the entire distributions are identical in the two geographic groups.

In Appendix A (Table 11) we report population statistics for all Swedish owners of single-family houses in 2006.[18] Our sample is on average richer, which we account for in the

[17]The data only reports total debt and student loans at the household level so we cannot compute the actual LTV on the household's primary residence. First and second liens and other mortgage contract details are also unobservable.

[18]We thank Paolo Sodini for assisting in the calculations.

Table 2: **Households residing in single-family houses in Stockholm (2006)**

	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)	3099	2436	4055	2088	314.8	338.9
Age	53.95	14.46	54.19	14.51	53.25	14.28
Household size	2.817	1.338	2.795	1.333	2.880	1.354
Panel B. Balances sheets and income						
Financial wealth	974.5	2928	978.7	3042	959.2	2528
Single family housing wealth	3396	1625	3401	1651	3380	1548
Other real estate wealth	580.0	5388	588.3	4734	556.3	6924
Total wealth	4765	11671	4736	7263	4716	7615
Total debt	1256	1848	1253	1988	1264	1363
Combined LTV	0.407	2.752	0.421	3.168	0.365	0.567
Housing / Net worth	1.448	22.1	1.429	24.26	1.498	13.98
Labor income	570.7	580.3	567.4	606.6	579.6	494.1
Capital income	52.54	864.5	52.57	718.0	51.01	1185
Panel C. Car ownership						
Car owner (0/1)	0.814	0.390	0.807	0.393	0.852	0.355
Number of cars	1.144	0.910	1.134	0.906	1.252	0.944
Milage (km)	123900	241650	123820	241200	124710	246350
Age of car(s) (years)	4.094	5.158	4.071	5.126	4.331	5.447
Owner of premium brand (0/1)	0.047	0.212	0.047	0.213	0.047	0.211
N	35,388		26,410		8,978	

Note: All values in 1000s of Swedish kronor (kSEK). The combined loan-to-value ratio (CLTV) and the housing-net worth ratio is based on total debt which includes all kinds of debt, including students loans and consumer credits. The exchange rate is approximately 8 SEK/USD. Car owner is a dummy variable which takes on a value of one if the household owns at least one car. Mileage and Age of car(s) reports the average values in the household and is based on a total of 40,503 cars. For cars younger than 3 years in 2006 Statistics Sweden imputes mileage based on subsequent motor vehicle inspections. Premium brands are defined as Audi, BMW, and Mercedes.

subsequent analysis.

Table 3 reports on households' car transactions. 21.8% of households buys a car every year and approximately every fifth car purchasing households chooses to buy a new car.[19] The average price of a new car is SEK 248,000. For used cars, we do not observe the price and hence we exploit the characteristics of them instead. Those characteristics are milage, age, and whether the car belongs to a premium brand. At the time of transaction, the average age for a used car is 7 years. We define three brands as premium; Audi, BMW, and Mercedes. In our sample, 2.8% of households buy a used car from one of these brands. As indicated by the last four columns of Table 3, the two geographic groups display very similar behavior with regards to car purchases before renewal of the airport's contract.

3.3 Measurement issues

Overall, Swedish registry-based data is considered to be of very high quality. For our purpose there are two measurement issues that need to be addressed. The first issue is the date of the location. We observe where households live at the end of 2006 and use this location to form the treatment (noise) and control group. The announcement of the renewal of the contract, however, occurs at the end of the third quarter in 2007. In general there are not many movers in our sample (7% of our sample moved during 2007) so this classification error should not be large. As a robustness test we have excluded households that move during 2007 and the results stay the same.[20]

The second issue is that we observe ownership of the cars at the end of each year. At that time we see the three last transactions during the year for each car and the date of those transactions. This means that for cars that have changed ownership more than three

[19] These statistics line up quite well with aggregate statistics for Sweden. The national average for new (used) cars was about 6% (23%). That the likelihood of purchasing a car is somewhat lower in our sample is not surprising as households who live in a large city are less prone to own a car.

[20] The results are untabulated but are available on request.

Table 3: **Summary statistics of car purchases**

	Full sample		Outside noise area, before renewal		Inside noise area, before renewal	
	Mean	SD	Mean	SD	Mean	SD
Panel A. New and used cars						
Purchases	0.218	0.658	0.213	0.627	0.225	0.183
N	53166		32560		11762	
Panel B. New cars						
Purchases	0.042	0.212	0.042	0.213	0.042	0.209
Price	247.8	131.0	249.8	138.6	242.9	101.4
N	10252		6364		2195	
Panel C. Used cars						
Purchases	0.176	0.619	0.171	0.584	0.183	0.607
Milage	126770	181560	130150	191180	132030	198250
Age	7.026	5.484	7.015	5.464	7.059	5.538
Premium brand	0.028	0.200	0.028	0.203	0.028	0.176
N	42914		26286		9567	

Note: The sample consists of all purchases made by single-family house owners that reside in Stockholm. Full sample includes transactions from 2004Q1 to 2008Q3. The remaining columns include transactions from 2004Q1 to 2007Q3. The variable Purchases is a count variable that reports the number of car purchases per year in the household. Statistics Sweden can only provide price data for 2007 and later. For 2005 and 2006 the price of new cars is based upon our imputation using data from 2007 and hand collection. The coverage for 2005 and 2006 is 2,398 out of 3,019 transactions (79.4%). We choose to not impute prices for 2004. Premium brands refer to Audi, BMW, and Mercedes. All values in 1000s of Swedish kronor (kSEK). The exchange rate is about 8 SEK/USD.

times in a year we will not observe every purchaser. We will also not observe a household's purchase if the car is scrapped in the same year as it was bought. Such events are not common and in any case we have no reason to suspect that they should correlate with the extension of the airport contract.

4 Empirical strategy

4.1 Car consumption and housing wealth

The population function of interest if we were to relate changes in housing wealth to car consumption could be postulated as:

$$\text{Car}_{it} = \alpha + \beta \log(\text{House price}_{it}) + \varepsilon_{it} \quad (2)$$

where Cars_{it} represents some measure of car purchases of household i in time period t, such as the number of car purchases (Purchase_{it}) to measure extensive margin effects or the log of the car price to measure the intensive margin effect conditional on a car purchase. Since we only observe prices for new cars other characteristics that proxy for the price are considered as well. The variable $\log(\text{House price}_{it})$ is the house value of household i in timeperiod t. The error term ε_{it} represents all other factors related to the outcome. Our baseline specification focuses on house price changes rather than changes to housing net worth (Mian et al., 2013). See Kaplan et al. (2016) for a discussion of alternative covariates and specifications.

4.2 First stage, reduced form, and IV-estimator

We define the treatment period to start on 1^{st} of October 2007, also denoted as 2007Q4. The variable NoiseArea_i defines the treatment group in the sense that the variable takes on a value of one if the household 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 first stage equation reads:

$$\log(\text{House price}_{it}) = \gamma_0 + \theta\text{Noise area}_i + \delta(\text{Noise area}_i \times \text{Post}_t) + \gamma_t + \varepsilon_{it}^h \tag{3}$$

where γ_t indicates time effects and ε_{it}^h is an error term. Thus, θ measures the average (percentage) difference in house prices between the houses inside and outside the noise area. δ is the coefficient of interest and measures in the percentages change in house prices due to the extension of the contract. Throughout, standard errors are based on clustering of error terms at the level of the 250*250 meter grid.

The reduced form is identical to equation 3 with respect to the right-hand side but the outcome is our measure of car consumption as described in equation 2:

$$\text{Cars}_{it} = \phi_0 + \rho\text{Noise area}_i + \phi(\text{Noise area}_i \times \text{Post}_t) + \phi_t + \varepsilon_{it}^l \tag{4}$$

where ϕ_t indicates time effects, ε_{it}^l is an error term and ϕ is the parameter of interest. It measures the percentage change in purchase prices of new cars due to the renewal of the airport contract.

For δ and ϕ to have a causal interpretation the identifying assumption is the assumption of parallel trends prior to treatment between the treatment and control groups. Since equations (3) and (4) have causal interpretation only if that assumption holds, we will present dynamic estimates that allow for statistical hypothesis testing of the assumption. There should be no significant effects before the actual declaration of the extension in 2007Q3 (sometimes this is referred to as placebo tests). An immediate reaction after 2007Q3 adds further credibility to a casual interpretation.

4.3 IV estimates of the housing wealth effect

In order to obtain the instrumental variables effect we can weigh the reduced form with the first stage (i.e., $\hat{\beta}^{\text{IV}} = \hat{\phi}/\hat{\delta}$). This is a valid local average treatment effect, provided that the exclusion restriction holds. This second-stage IV estimates have the same interpretation as in our discussion of equation (2).

In practice, the effect of treatment on house prices is only observed in the data set of house transactions and not in the household data set. Therefore, we estimate the housing wealth effect using a two-sample instrumental variables approach analogous to Angrist and Krueger (1992). 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).[21]

4.4 Exclusion restriction and robustness

Our IV-estimator measures the causal effect of a change in housing wealth on car consumption if the exclusion restriction holds. Conversely, it does not measure the casual effect if the instrument (i.e., $\text{NoiseArea}_i \times \text{Post}_t$) has an independent effect on car consumption aside from the effect through housing wealth. In our baseline model our instrument is $\text{NoiseArea}_i \times \text{Post}_t$, which takes on the value one after 2007Q3 for the noise area and zero otherwise. Thus, a violation of the exclusion restriction amounts to no less than finding another event (e.g., another policy change) at the same time that affects car consumption in- and outside the noise area differentially. Clearly, building new houses on the land of the airport could cause new public transportation. However, building new houses on the premises of the airport would take at least ten years and before that more public transportation (e.g., buses) would not be set in.

[21] We thank Peter Fredriksson and Björn Öckert for sharing their Stata code with us.

Despite that we can think of no other event of this kind we note that since noise is decreasing with distance we expect to observe that the treatment effect is decreasing monotonically with the distance from the border of the noise contour. This would be compelling evidence of a causal effect as it is even more difficult to think of another event that would not occur in late 2007 and be monotonically related with the distance to the noise contour. We extend our regression model in ways to investigate this. First, we split our treatment group into three groups. One treatment group is defined as the within the noise contour, a second is located up to 500 meters away from the border of the noise contour and a third between 500 and 1000 meters away from the border of the noise contour. We refer to this analysis as a dose response analysis. We would expect a smaller effect for the groups furthest away and the largest for the group within the noise contour. In addition, we consider a continuous measure of distance, denoted by $\log^{10}(\text{distance}_i^2)$ consistent with the propagation of noise given by equation (1).

We consider three additional robustness tests. Firstly, a standard robustness test is to investigate compositional bias by adding controls for house characteristics. We consider the characteristics reported in Table 1 and parish fixed effects.[22] Secondly, we consider different coarser clustering schemes. In order to confirm that our way of clustering errors at the level of the 250 times 250 meter grid is innocuous we construct three coarser grids of 500 times 500, 1,000 times 1,000, and 2,000 times 2,000 meters, respectively. Statistical significance in key estimates are not altered. Finally, difference-in-difference estimates are known to be sensitive to functional form. In our baseline specification we use the natural logarithm of prices. However, we also consider linear specifications. In general we obtain very similar results. Lastly, house prices have a strong seasonal component even at the monthly frequency. Thus, we seasonally adjust the prices by adjusting for calendar month specific components.

[22]A parish is the smallest local government unit in Sweden. There were 18 parishes in the municipality of Stockholm in 2006. Five of those are partly overlapping with the noise area.

Since our model is robust to the above extensions and is showing clear signs of monotonically decreasing dose response, we argue that our results are likely to be causal and that it is very hard to understand what type of other channel that could violate the exclusion restriction.

In a difference-in-difference approach one might also worry about general equilibrium effects. In our case these effects are however likely small as 90% of households in Stockholm municipality reside in apartments as opposed to single-family houses. Moreover, only 17% of households residing in single-family houses reside inside the noise area.

5 Results

This section reports our results. First, we establish a robust a effect on house prices in the housing transaction data set. Second, we validate that this effect is present also in the household data set. Third, we present results on car purchases in the form of reduced form estimates as well as IV estimates. We then consider the role of initial leverage and the caveats of geographical aggregation.

5.1 Effect on house prices (first stage effect)

To validate our DID specification, we test for parallel trends in outcomes prior to 2007Q3. We augment equations (3) and (4) with yearly time dummy variables. We define a set of dummy variables based on time relative to treatment. Since the renewal of the operating contract was disclosed in late 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 NoiseArea_i and omit the dummy variable for 2006Q4–2007Q3 so that it serves as the reference level for prices outside noise area.[23] That is, for every 12-month period, we

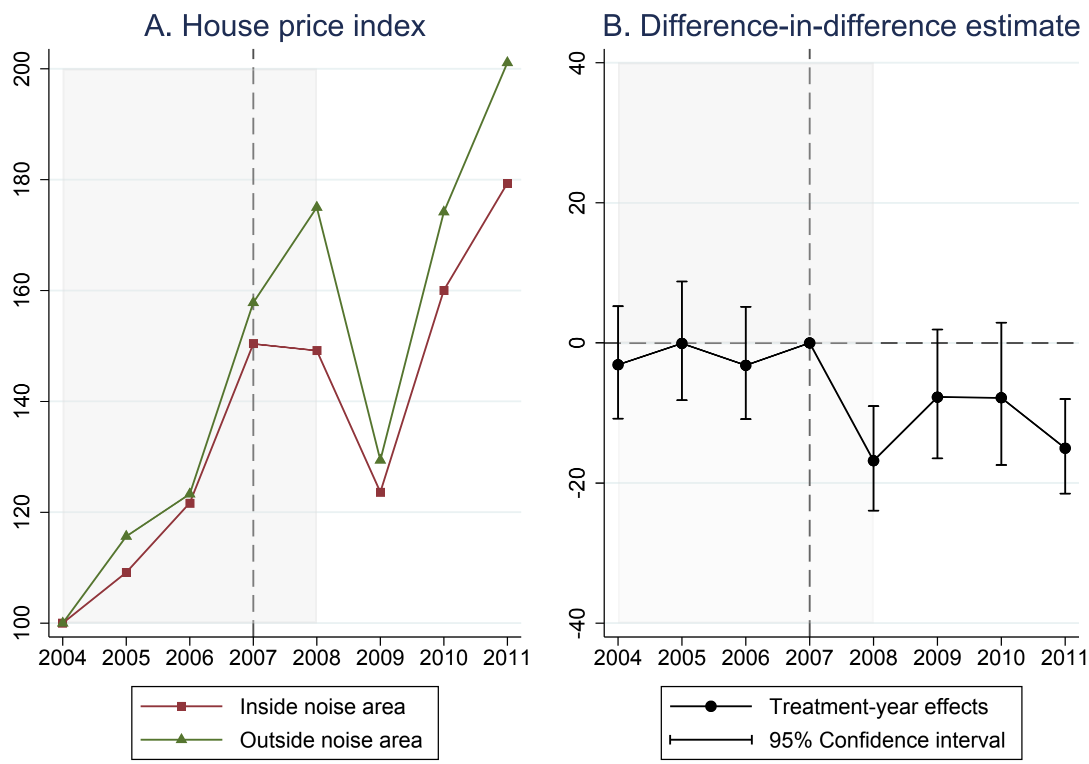
[23] In our graphical illustrations, 2006Q4-2007Q3 is referred to as 2007 and so forth unless explicitly stated otherwise.

estimate a treatment effect relative to the 12 months just before contract renewal. This is the standard test of parallel trends (Angrist and Pischke, 2009).

Moreover, this specification enables illustration of the dynamics after 2007Q3 (i.e., post contract renewal). We start by evaluating house prices (i.e., the first stage) in Figure 2. Panel A displays house price fluctuations inside and outside the noise area. The price series are indexed to 100 in 2004. The series follow the same trend from 2004Q1 to 2007Q3. Subsequently, there is a gap between them. Panel B displays the corresponding DID estimates on log house prices. Common pre-trends up to 2007Q3 cannot be rejected. However, the immediate effect in 2007Q4-2008Q3 is then -16.7 log points. Differences remain throughout up until 2011. However, since our aim is to isolate the effect of the contract's renewal in September 2007 we focus on the time period up until the major onset of the financial crisis in our main specifications, that is from 2004Q1 to 2008Q3. In Figure 2 and in all subsequent figures this area is shaded in grey. The focus on a short post-treatment time window should come at little cost from a causal point of view since a DID strategy is more credible if effects appear quickly. Appendix B discusses several noteworthy aspects of the housing market, such as how the house price index construction method underlying Panel A compares to official index providers' methods. It also reports the equivalent of Panel B in Figure 2 at quarterly frequency, investigates (and rejects) compositional effects of transacted properties, and illustrates how the culmination of the financial crisis in 2009 affected the Swedish housing market.

Table 4 reports estimates of the first stage. Columns (1) to (5) limit the sample to the pre-financial crisis period. Column (1) considers an unrestricted sample with no control variables and the estimated effect is -26% inside the noise area compared to outside. Column (2) restricts the sample to transactions of real estate for which the tax value is non-missing (which we interpret as excluding properties that are not intended to serve as single family housing). The estimate is stable at around -21%. Column (3) considers a

Figure 2: **Effect on house prices**



Note: Panel A shows house price indices outside and inside the noise area. Panel B shows the corresponding difference-in-difference estimates in log points. The timing on the horizontal axis is shifted one quarter. That is, 2007 refers to 2006Q4–2007Q3 and so forth. The shaded grey area indices the main sample period (2004Q1-2008Q3).

specification with basic control variables on property characteristics. We consider this to be our preferred specification since it controls for characteristics that serves as input to official index producers' methods. In this specification the magnitude of the effect to −16.7% but remains highly statistically significant. Adding additional control variables, such as the age of the house, plot area, and the change in property tax due to the new property tax code coming into effect in 2008. These additional control variables affect the point estimate by only 2 percentage points. The fifth column considers a shift of the dependent variable from the log of the transaction price to the transaction price. The house price effect amounts to −419 kSEK, or 12.4% of the pre-treatment mean inside the noise area. Finally, column (6) considers the full sample period (i.e., 2004–2012). The long-term effect is muted because of the turbulence in the housing market but remains large and statistically significant. We conclude that our instrument is strong and that the effect on house prices inside the noise are is robust across specifications and sample periods.

5.1.1 Dose response in house prices

We provide further credibility to the casual mechanism behind the decrease in house prices nearby Bromma Airport by considering dose responses. That is, we model the effect, or intensity of treatment, as a function of the distance to the noise contour. In terms of empirical methods, this brings our analysis close to the literature that uses quasi-experimental methods to quantify the negative externalities of local changes to environmental conditions (e.g., Currie et al., 2015).[24]

Column (1) of Table 5 presents results from dividing the treatment effect into three separate effects based on the distance from the noise contour. The first covariate, $\text{Post}_t \times 0$, measures the effect of the renewal inside the noise contour in comparison to house prices

[24]The specifications of this section rely on more continuous measures of distance. Panel A of Figure 14 in the Appendix confirms that transacted real estate properties are evenly distributed up to 10 kilometers away from the noise contour.

Table 4: **Effect on house prices**

	(1)	(2)	(3)	(4)	(5)	(6)
	log house price	log house price	log house price	log house price	house price	log house price
Noise area$_i$ × Post$_t$	-0.261***	-0.212***	-0.167***	-0.147***	-419.2***	-0.125***
	(0.051)	(0.039)	(0.038)	(0.032)	(120.0)	(0.026)
Noise area$_i$	0.190***	0.196***	0.081	0.087**	204.6***	0.114***
	(0.043)	(0.043)	(0.055)	(0.043)	(72.2)	(0.039)
N	11,696	11,321	11,321	11,308	11,321	19,777
R-squared	0.099	0.102	0.525	0.559	0.540	0.537
2004Q1-2008Q3	Yes	Yes	Yes	Yes	Yes	No
Restricted sample	No	Yes	Yes	Yes	Yes	Yes
Basic controls	No	No	Yes	Yes	Yes	Yes
Additional controls	No	No	No	Yes	No	No

Note: The restricted sample only includes transactions of real estate that have a non-missing tax value. The basic controls include the tax value, an indicator whether the property is a site-leasehold right, and an indicator for a low sales price (<250 kSEK). The set of additional control variables include age, standard, plot area, living area, and non-living area, and the change in amount of property tax to be paid due to the tax reform coming into effect in 2008. The specification in column (3) corresponds to Panel B of Figure 2. Clustering of standard errors at the level of the 250 × 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

further away than 1,000 meters from the border of the noise contour. Notably, the effect is of a large magnitude; −23%. The second covariate, $(0 \quad 500] \times \text{Post}_{it}$, captures the effect on prices just outside the noise contour but not more than 500 meters away. The effect in this geographical region is about half (−16%). The effect is further muted another 500 meters away from the noise contour (−11%). The monotone relationship (i.e., a dose response) is compelling evidence in favor of a casual interpretation of a price effect from the contract's renewal. Going forward, we refer to this measure of treatment as "segmented distance". Column (2) considers a continuous measure of treatment rather than a discrete one. Assuming that equation (1) is a good proxy for the net effect of all kinds of externalities (i.e., not only noise), the effect on house prices should be proportional to the logarithm of distance (or distance squared). We interact this intensity of treatment with the post contract renewal indicator. The point estimate (0.120) is statistically significant. It is also consistent with previous specifications because it implies that going from 1,000 meters away from the noise contour to just 100 meters away had a differential impact of 24.0% on house prices while the differential impact between 1,000 meters away and 500 meters away only is 7.2% $(0.6 \times 0,120)$. Likewise, going from 2,000 meters away to 1,000 meters away had a differential impact of 7.2%. Going forward, we refer to this measure as "log distance". The remaining five columns, (3) to (7), rely on specifications similar to the baseline but they split the sample into smaller geographical regions. The baseline model defines the noise area as the area which is up to 1,000 meters away from the noise contour. Columns 3 to 7 report estimates when the geographical area is restricted to any area between just the noise contour, the specification in Column (3), up to 800 meters away from it, the specification in Column (7). In all of the specifications the effect is large and statistically significant, illustrating that the analysis is not sensitive to a particular choice of treatment and control groups. Further, the effect decreases monotonically as the area widens, consistent with notion of a dose response. Taken together, Table 5 is evidence of a distinct effect on house prices, regardless of exactly how the treatment group is defined.

Table 5: **Effect on house prices: dose response**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	log house price	log house price	log house price	log house price	log house price	log house price	log house price
$\text{Post}_t \times 0$	-0.229***						
	(0.036)						
$\text{Post}_t \times (0 \quad 500]$	-0.162***						
	(0.077)						
$\text{Post}_t \times (500 \quad 1000]$	-0.111***						
	(0.046)						
$\text{Post}_t \times \log^{10}(\text{dist}^2)$		0.120***					
		(0.024)					
$\text{Post}_t \times 0$			-0.204***				
			(0.036)				
$\text{Post}_t \times [0 \quad 200]$				-0.226***			
				(0.050)			
$\text{Post}_t \times [0 \quad 400]$					-0.212***		
					(0.044)		
$\text{Post}_t \times [0 \quad 600]$						-0.173***	
						(0.042)	
$\text{Post}_t \times [0 \quad 800]$							-0.164***
							(0.039)
N	11,321	11,321	11,321	11,321	11,321	11,321	11,321
R-squared	0.526	0.53	0.524	0.559	0.525	0.524	0.525
2004Q1-2008Q3	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Restricted sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports dose responses as Post_t is interacted with variables that indicate the distance to the noise contour. A distance of 0 indicates inside the the noise contour. The noise area includes the noise contour plus up to 1,000 meters. The restricted sample only includes transactions of real estate that have a non-missing tax value. The basic controls include the tax value, an indicator whether the property is a site-leasehold right, and an indicator for a low sales price (<250 kSEK). Clustering of standard errors at the level of the 250 × 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B.6 (Table 13) illustrates that the estimates are robust to using alternative sub-sets of the control group, providing additional evidence for tight identification.[25]

[25] In untabulated results we have confirmed that changing the level of clustering to 500×500, 1,000×1,000 or 2,000×2,000 standard errors do not affect the level of statistical significance in any substantial way.

5.2 A cross-walk between the real estate transaction data set and the household data set

We now turn to analysis of consumption responses in the household data set. As a prologue to this analysis we establish that the effect on house prices of the contract renewal in September 2007 is present in the household data set too. As previously explained, the fundamental obstacle is that Statistics Sweden's model for appraising single-family houses in the household data set is too coarse, in the geographical sense, to be able pick up the treatment effect of the renewal of the airport contract.[26] That said, the house price effect ought to be present as a relative loss in realized capital gains from 2007Q4 and onwards for house sellers. In a DID regression with years 2004-2006 and 2008 we find a highly statistically significant decrease in realized capital gains inside the noise area, amounting to SEK 28,900 per household and year. This is the intent-to-treat (ITT) effect measured over all households inside the noise area, of which most do not sell. Our transaction data set suggests that there are about 2,400 transactions every year among the stock of 40,000 single-family houses in the Stockholm municipality which implies a rate of sales of 6%. After scaling we arrive at an approximate loss of 482 kSEK. This is very close to the estimates of Table 4.[27] We conclude that the estimated ITT effect in realized capital gains in the household data set confirms our findings on house price effects in the housing transaction data set.

In contrast to the effect on capital income, we find no effect on other kinds of income such as labor income, total income from employment, or labor income plus unemployment benefits, see Appendix D (Figures 15 and 16). This supports our view that the renewal of Bromma Airport's contract had no major economic consequences or that that the local house price shock in itself affected general economic conditions.[28]

[26] To be precise, Statistics Sweden employs the appraisal method municipality by municipality.

[27] Appendix B (Figure 11) reports on these estimates graphically, including a pre-treatment trend.

[28] While we do not challenge the view that airports are important for economic activity it would be surprising if Bromma Airport would be of vital importance to Stockholm. It is a smaller airport than

5.3 Effect on car purchases (reduced form)

We briefly consider reduced form estimates on car consumption, as specified by equation (4), before moving to the IV estimates.

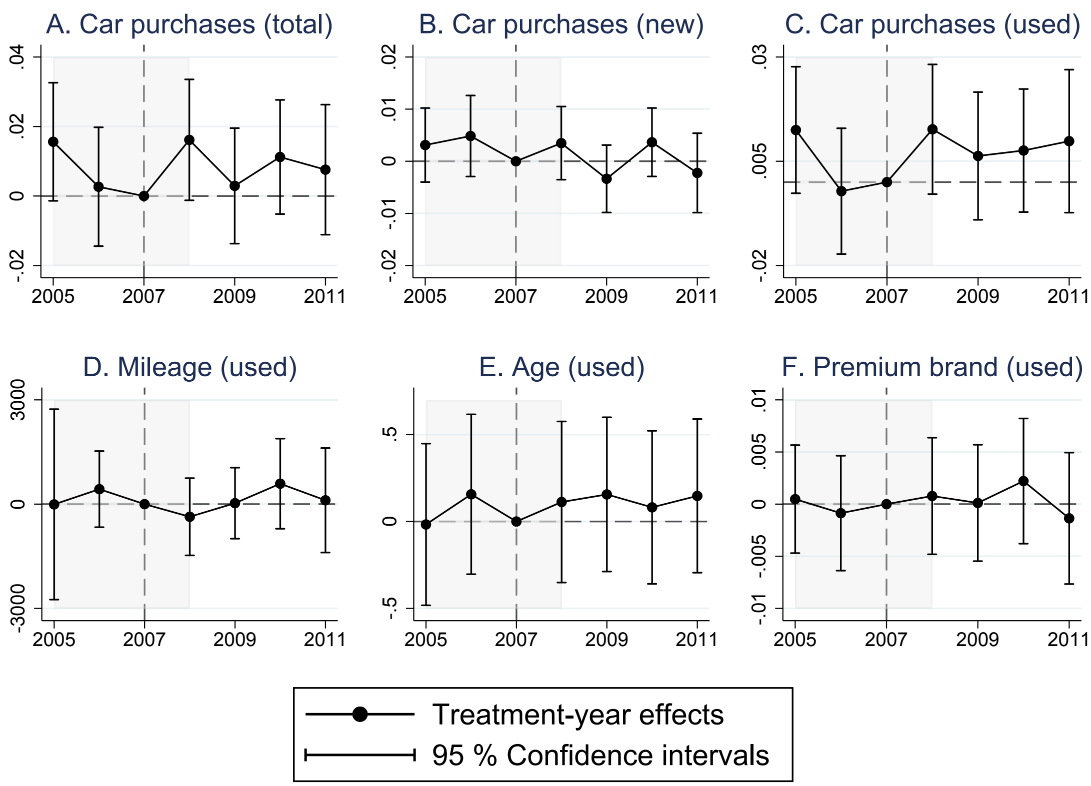
Figure 3 summarizes our findings on extensive margin effects (i.e., the likelihood that a car is purchased) and substitution effects between different kinds of used cars. We find no evidence of housing wealth effects on these margins. There is no effect on the likelihood of buying any kind of car (Panel A), a new car (Panel B), or a used car (Panel C). If anything, point estimates are in the positive range rather than in the negative range. We can rule out any negative effect on new cars in 2007Q4-2008Q3. Furthermore, we find no evidence that buyers of used cars inside the noise area substitute to cheaper cars because there is no effect on mileage (Panel D), age (Panel E), or on the likelihood of buying a used car with a premium brand (Panel F). Appendix C (Table 14) reports further details on the regressions corresponding to Figure 3. The estimates imply that a negative effect of a magnitude greater than -0.2% on the likelihood of purchasing a new or used car can be ruled out with 97.5% probability.

As a consequence of the null effects on these margins we focus on analyzing the substitution effect among new cars for which we observe the price. Figure 4 reports prices of new cars. Again, the assumption of parallel trends prior to contract renewal holds up very well. There are no statistically significant effects before the renewal and the point estimates show no systematic pattern. After the renewal there is a distinct and statistically significant negative effect inside the noise area. Relative to the reference period, the average decrease in car prices inside the noise area is 25 kSEK, or approximately -7%. After 2008Q3 the effect is somewhat muted by the financial crisis but the point estimate remains rather stable.

Figure 4 is based on a regression specification with no controls. Table 6 reports estimates

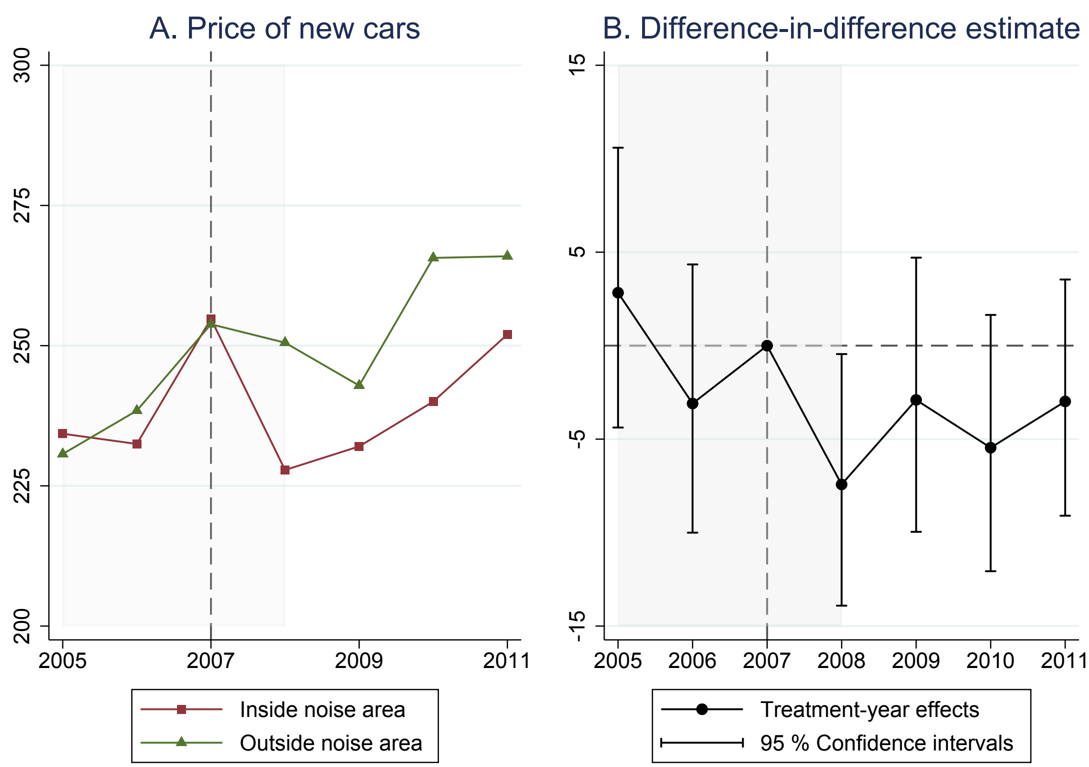
Arlanda Airport which is located only 30 kilometers away and is an obvious substitute. Further, according to wikipedia Bromma Airport itself only have 110 full-time employees, implying that it is not a major employer in Stockholm (https://sv.wikipedia.org/wiki/Stockholm-Bromma_flygplats).

Figure 3: **Car purchases: extensive margin and substitution effects**



Note: The figure shows estimates of 12-months effects inside the noise area relative to outside the noise area. 2006Q4-2007Q3 is omitted to serve as reference year (the vertical dashed line). The timing on the horizontal axis is shifted one quarter. That is, 2007 refers to 2006Q4–2007Q3 and so forth. Premium brands refer to Audi, BMW, and Mercedes. The shaded grey area indices the main sample period (2004Q1-2008Q3).

Figure 4: **Car purchases: price of new cars**



Note: The figure shows estimates of 12-months effects inside the noise area relative to outside the noise area. 2006Q4-2007Q3 is omitted to serve as reference year (the vertical dashed line). The timing on the horizontal axis is shifted one quarter. That is, 2007 refers to 2006Q4–2007Q3 and so forth. The shaded grey area indices the main sample period (2004Q1-2008Q3).

on the substitution effect for new cars based on alternative specifications. Column (1) reports the equivalent of the figure but restricted to primary time period of analysis. The effect on on prices of new cars is -7.2%. Column (2) corresponds to an addition of control variables that are likely to affect the preference for a new car. The control variables are household size, age of household head, labor income, housing wealth, loan-to-value ratio, and net worth. At -6.5% the point estimate is essentially unaffected. Column (3) considers the price amount as dependent variable rather than the log of the price. The estimate is -19 kSEK and is the equivalent of -7.8% given a mean price of 243 kSEK inside the noise area for the period 2005Q1-2007Q3. Finally, column (4) displays the estimated effect for a longer sample period, stretching up to 2011Q4. At -4.9% the effect is somewhat smaller but still statistically significant.

Table 6: **Effects on the price of new cars (intensive margin)**

	(1)	(2)	(3)	(4)
	log car price (New)	log car price (New)	car price (New)	log car price (New)
Noise area$_i$ × Post$_t$	-0.072**	-0.065**	-19.0**	-0.049*
	(0.029)	(0.030)	(8.7)	(0.025)
Noise area$_i$	-0.001	-0.001	-1.1	-0.018
	(0.017)	(0.029)	(5.2)	(0.012)
N	4,794	4,794	4,794	8,638
R-squared	0.010	0.077	0.082	0.052
Sample: 2005Q1-2008Q3	Yes	Yes	Yes	No
Controls	No	Yes	Yes	No

Note: The set of control variables includes household size, age of household head, labor income, housing wealth, loan-to-value ratio, and net worth. Column (3) reports a specification with the car price in kSEK instead of the log of car price. Clustering of standard errors at the level of the 250 × 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We conducted additional analysis that favors the interpretation of a direct casual effect from house prices to responses in car purchases. Analogous to the dose response analysis on house prices, we have investigated responses on new car purchases based on distance to the noise contour. The evidence is compelling in that the more continuous measures (i.e., the segmented distance and the log distance measures) display a monotonically decreasing relationship with distance to the contour. Furthermore, highly statistically significant estimates do not rely on our baseline definition of the noise area. See Appendix C (Table 15).[29]

5.4 IV estimates of the housing wealth effect

We shift to reporting IV estimates of the elasticity between house prices and consumption as specified by equation (2).[30] We have established that there is no effect for the extensive margin and no substitution effect for used cars so focus is on the substitution effect for new cars.

Table 7 reports IV estimates for the subset of households that purchase a new car. Columns (1) and (2) report elasticities based on the baseline instrument.[31] Column (1) reports an elasticity of 0.438 which is statistically significant at the 1%-level. Adding control variables (column (2)) does not materially affect the estimate.

We also consider using the instrument of column (1) in Table 5 which picks up finer variation in house prices (i.e., segmented distance). Column (3) reports the corresponding IV estimate of 0.421. Adding control variables (column (4)) does not make a difference. Finally, we consider the continuous measure of the instrument, corresponding to column (2) of Table 5 (i.e., log distance). The point estimate is 0.327 and at 0.34 the addition of control

[29] We also ensured ourselves that the geographical distribution of car purchasers is similar to the distributions of real estate transactions and households – see Panels A to C of Figure 14.

[30] Our implementation of the IV estimator is as follows. We first estimate equation (3) in the data set on real estate transactions. We then use the estimates to predict the quasi-random variation in house prices in the household data set. Different functional forms of the predictive variables are considered. The main specification utiliizes NoiseArea_i and $\text{NoiseArea}_i \times \text{Post}_t$, but we also consider the specifications of columns (1) and (2) of Table 5. The predicted house prices are used as covariate in equation (2). The IV estimates may deviate somewhat from $\hat{\phi}/\hat{\delta}$ if the set of control variables in the two specifications differ.

[31] The baseline instrument is $\text{Noise area}_i + \text{Noise area}_i \times \text{Post}_t$. We correct standard errors for pre-estimation as previously described.

Table 7: **Housing wealth elasticities**

	(1)	(2)	(3)	(4)	(5)	(6)
	log car price (New)	log car price (New)	log car price (New)	log car price (New)	log car price (New)	log car price (New)
log(House price$_{it}$)	0.438***	0.454**	0.421**	0.431**	0.327**	0.340**
	(0.203)	(0.231)	(0.189)	(0.216)	(0.143)	(0.153)
N	4,794	4,794	4,794	4,794	4,794	4,794
R-squared	0.010	0.055	0.011	0.055	0.010	0.054
Instrument	Noise area	Noise area	Segmented distance	Segmented distance	$\log^{10}$	$\log^{10}$
2005Q1-2008Q3	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes

Note: The table presents second-stage two-sample IV estimates using different functional forms of the instrument. Noise area refers to $\text{NoiseArea}_i + \text{NoiseArea}_i \times \text{Post}_t$. Segmented distance refers to the specification in column (1) of Table 5. $\log^{10}$ refers to the specification in column (2) of Table 5. Standard errors are corrected for first-stage estimation in the transaction data set. The set of control variables in the first stage are tax value, age, standard, plot area, living area, and non-living area and a control for the change in the property tax code in 2007. The set of control variables in the second stage are household size, the age of household head, labor income, housing wealth, loan-to-value ratio, and net worth. Clustering of standard errors at the level of the 250×250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

variables barely changes in the point estimate. We conclude that the elasticity is in the range from from 0.32 to 0.45.[32]

5.5 Heterogeneous effects

We exploit our rich micro data to explore the role of borrowing constraints, household characteristics and implications from the (S, s) models.

5.5.1 Balance sheets

We first explore how households with different balance sheet characteristics respond to the house price shock. A central concern in the housing wealth literature is how to distinguish a pure housing wealth effect from the effect of a binding collateral constraint. DeFusco (2018) reports sizeable MPC estimates when relaxing the borrowing constraint, holding expected wealth constant. Also Cooper (2013) finds a strong collateral effect. Relatedly, Benmelech et al. (2017) find substantial effect of a credit crunch in the asset-backed commercial paper market on auto purchases during the financial crisis, attributing it to tighter credit supply. Mian et al. (2013) find strong interaction effects between leverage and house price shocks whereas Kaplan et al. (2016) find no interaction effect.[33]

A simple way to investigate the role of housing as a collateral asset is to estimate heterogenous responses among households depending on pre-treatment balance sheet characteristics. That is, whether households with different (combined) loan-to-value ratios (CLTVs) respond differently. We divide our sample into subgroups based on their CLTV at the end of 2006 (i.e., prior to treatment). We create three groups based on whether it is likely that a 15-

[32] At a conceptual level, one could study the difference between the purchase price of the household's new car and the sales price of the household's old car. For some theoretical considerations, this would be the more relevant decision margin. This is however not the typical framing in the empirical literature and from the perspective of aggregate consumption, such transactions of used cars net out. Nevertheless, we have not found any evidence of an effect on households' car sales, see Appendix C (Figure 13).

[33] Laerkholm Jensen and Johannesen (2017) find effects on household consumption of tighter credit supply in Denmark.

percentage-point loss in housing wealth is likely to make a household borrowing constrained. The first group is one with low CLTVs which we define as being less than 0.6. The medium group is one with CLTVs between 0.60 and 0.80, and the third one consists of households with high CLTVs greater than 0.80. Households in the low group are unlikely to face binding constraints even in the presence of the shock whereas households in the high group are likely to have meagre possibilities to obtain additional mortgage loans using their house as collateral already before the the house price shock. For households in the medium group, however, the house price fall is likely to put them close to or at the borrowing constraint. If negative housing wealth shocks manifests into consumption through credit finance of car purchases, we expect to see large responses in this group.[34]

Panel A of Table 8 reports heterogenous effects by CLTV ratio. We previously reported no extensive margin effects and we confirm that there are no effects among households with different CLTVs either (Columns (1) to (3)). Columns (4) to (6) elaborate on the established intensive margin effects. In essence, they illustrate that the negative effect of the house price shock is entirely concentrated to households in the intermediate range of CLTVs, that is between 0.60 and 0.80. At an elasticity of 1.64, the magnitude is more than 3.5 times as large for this group compared to the previously reported baseline estimates. Furthermore, there is a distinct hump-shape – in unreported results we find that that $\log(\text{House price}_{it}) \times \text{CLTV}^2$ is negative and statistically significant, implying a hump-shaped response over the CLTV distribution. This is in contrast to previous estimates that rely on ZIP code level data. Mian et al. (2013) group ZIP codes by ratio of aggregate mortgage and home equity debt to aggregate value of owner-occupied homes and find an increasing and close to monotone relationship between leverage and MPC on autos. To further corroborate the tight link be-

[34]Since our IV estimates are based on two data sets and since we do not observe CLTV in the transaction data set, we rely on the assumption that first-stage effects are equal for all CLTV groups. There are no reason to believe that this would not hold. Likewise, in the subsequent analysis we assume that first-stage effects are homogenous also across groups sorted by DTI, labor income, housing wealth, net worth, and household age.

Table 8: **IV estimates by loan-to-value ratio and debt-to-income ratio**

	(1)	(2)	(3)	(4)	(5)	(6)
	Purchases (New & Used)	Purchases (New & Used)	Purchases (New & Used)	log car price (New)	log car price (New)	log car price (New)
Panel A. Estimates by CLTV ratio						
log(House $price_{it}$)	-0.013	0.013	0.003	0.348	1.640**	-0.490
	(0.015)	(0.009)	(0.021)	(0.245)	(0.743)	(0.713)
N	422, 530	52,925	46,345	3765	546	483
R-squared	0.013	0.004	0.007	0.071	0.076	0.131
CLTV ratio	0.0-0.6	0.6-0.8	0.8>	0.0-0.6	0.6-0.8	0.8>
Panel B. Estimates by debt-to-income ratio						
log(House $price_{it}$)	-0.019	0.003	0.000	0.345	1.431	0.639
	(0.016)	(0.023)	(0.012)	(0.240)	(1.022)	(0.700)
N	416,431	22,762	86,259	3,938	214	642
R-squared	0.015	0.004	0.009	0.085	0.234	0.175
DTI ratio	<1.5	1.5-2.5	>2.5	<1.5	1.5-2.5	>2.5

Note: The table reports IV estimates by subgroups. The instrumental variable is NoiseArea_i + $\text{NoiseArea}_i \times \text{Post}_t$. The set of control variables are as in Table 7. The time period is 2005-2008Q3. Clustering of standard errors at the level of the 250 × 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

tween leverage and car purchases, Appendix C (Table 16) reports the average response in debt for households who purchase a car in between 2004 and 2007. Upon the purchase a of new car, household debt increases by 90% of the average car value. We conclude that the pre-treatment CLTV at the household level is critical for determining the household's response. In other words, household-level data seems critical in unmasking the true relationship between housing wealth shocks and balance sheet conditions. Importantly, our estimate for the high CLTV households in fact indicates that the response to a house price shock among them may in fact be zero, at least in the short run.

To contrast our findings on the role of the CLTV ratio, Panel B reports heterogenous effects by DTI ratio. The cut-off values are 1.5 and 2.5. Unlike in Panel A, there are no statistically significant response response at any point in the cross-sectional distribution. This is consistent with the quasi-experiment operating through house prices which directly impacts the CLTV ratio of households exposed to the house price shock.

Overall, the findings indicate that the housing wealth effect is not a "pure" wealth effect. Rather, it manifests itself as collateral in the credit markets.

5.5.2 Other heterogeneity

Our rich micro data enables us to consider other dimensions of heterogeneity, consistent with the incomplete markets consumption-savings model of Berger et al. (2018) who derive a rule-of-thumb for the MPC and report substantial cross-sectional heterogeneity in the housing wealth elasticity. Table 9 reports elasticities along the labor income, housing wealth, net worth and age distributions.[35] Each distribution is cut in three thirds. Panel A reports elasticities for different income groups. None of the groups respond on the extensive margins (columns (1)-(3)) whereas the intensive margin response is concentrated to low income households. Panel B reports elasticities for households with different housing wealth. The

[35] The dimensions of the table correspond to the panels in Figure 3(b) of Berger et al. (2018).

intensive margin response is concentrated to households with the least housing wealth where it is estimated to be just over unity. This may appear counter to the effect of leverage. However, at 0.621 the CLTV ratio is the highest in this group (compared to 0.270 and 0.329 for the other two housing wealth groups). Panel C shows the equivalent pattern for net worth; the poorest one third responds the most. Finally, Panel D shows results for different age groups. There are no apparent differences. Taken together, these responses are qualitatively consistent with Berger et al. (2018) where the housing wealth effect is proportional to the MPC out of liquid wealth (or a transitory income shock).

5.6 Aggregation and MPCs

We now consider the aggregate implications on MPCs of our findings.

5.6.1 The MPC on cars out of housing wealth

We begin with a simple back-of-the-envelope calculation of the marginal propensity to consume cars out of housing wealth. The loss in housing wealth for households inside the noise are is approximately equal to 497 kSEK ($3,380 \times 14.7\%$). Our implied elasticity among purchasers of new cars implies that the cut-back is 16.2 kSEK ($0.454 \times 14.7\% \times 243$). This implies a sizeable response of every home owning household that purchase a new car; an MPC of 0.033 (i.e., 3.3 cents per dollar). To compare this MPC estimate to the estimates that rely on geographically aggregated data we must adjust for the share of households that purchase a new car which is 4.2% in our data set. This implies that the MPC on cars among all homeowners is 0.0013 (i.e., 0.13 cents per dollar). Mian et al. (2013) estimate an MPC on cars out of housing wealth to net worth equal to 0.023 which for comparison should be scaled by the average housing wealth to net worth in their sample which is 0.25–0.33, implying an MPC on cars out of housing wealth equal to 0.0058 (i.e., 0.58 cents per dollar).[36]

[36] Our adjustment for comparison to Mian et al. (2013) is identical to the one of Berger et al. (2018).

Table 9: **IV estimates – cross-sectional heterogeneity**

	(1)	(2)	(3)	(4)	(5)	(6)
	Purchases (New & Used)	Purchases (New & Used)	Purchases (New & Used)	log car price (New)	log car price (New)	log car price (New)
Panel A. Estimates by labor income						
log(House price$_{it}$)	-0.009	-0.008	0.004	0.879*	0.450	0.190
	(0.015)	(0.009)	(0.011)	(0.459)	(0.345)	(0.341)
N	176,940	168,455	168,675	982	1,664	2,148
R-squared	0.015	0.001	0,002	0.102	0.038	0.082
Labor income	<358	358–559	>559	<358	358–559	>559
Panel B. Estimates by housing wealth						
log(House price$_{it}$)	0.002	-0.010	-0.005	1.040**	0.380	0.079
	(0.017)	(0.009)	(0.011)	(0.409)	(0.337)	(0.345)
N	176,971	169,417	169,429	1,270	1,594	1,930
R-squared	0.013	0,001	0.004	0.098	0.045	0.075
Housing wealth	<2683	2683–3909	>3909	<2683	2683–3909	>3909
Panel C. Estimates by net worth						
log(House price$_{it}$)	-0.003	-0.009	-0.005	0.739*	0.606	0.016
	(0.020)	(0.009)	(0.009)	(0.389)	(0.391)	(0.355)
N	176,948	170,360	170,978	1,523	1,488	1,783
R-squared	0.012	0.001	0.004	0.073	0.067	0.109
Net worth	<3121	3121–4707	>4707	<3121	3121–4707	>4707
Panel D. Estimates by age						
log(House price$_{it}$)	-0.020	0.000	-0.002	0.638	0.359	0.351
	(0.024)	(0.011)	(0.009)	(0.405)	(0.380)	(0.374)
N	190,215	157,128	171,694	1,615	1,739	1, 440
R-squared	0.006	0.002	0.006	0.036	0.101	0.080
Age	<46	46–59	>59	<46	46–59	>59

Note: The table reports IV estimates corresponding to Table 8. The cut-off values for the respective variable corresponds to sample splits into thirds. The set of control variables are as in Table 7. The time period is 2005-2008Q3. Clustering of standard errors at the level of the 250 × 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

This estimate is a factor of 4.5 greater than ours and outside of our 95% confidence interval. Our elasticity estimate for households with a CLTV ratio between 0.6 and 0.8 does however imply an MPC of 0.118 (11.8 cents per dollar). Multiplying this response with the same factor as before (4.2%) would imply an aggregate MPC of 0.0050 and hence much closer and indistinguishable in statistical terms. Again, this highlights the critical role of household leverage.

We wish to put our estimate into a broader context to facilitate the interpretation of the difference in magnitudes. We highlight five important aspects.

First, it is of interest to compare the estimates to the implications of the simplest possible life-cycle permanent income hypothesis model. Cars' share of household consumption is 6.1% so the total life-time adjustment on cars is 30.3 kSEK (6.1% $\times$ 497 kSEK).[37] The replacement rate on cars is 21.8% per annum (see Table 3) and the remaining life of households in our sample is 31 years (assuming 85 years' life expectancy), meaning that the average household is to purchase 6.8 more cars. The cut-back on each purchase should then be 4.5 kSEK (34.4/6.8 kSEK) which implies an MPC of 0.009 (4.5/497), or 0.9 cents per dollar. This is is a factor of 3.6 smaller than our point estimate, which is considerably smaller than our's but inside our 95% confidence interval.[38]

Second, our estimate is comparable to recent theory-based partial equilibrium experiments of Berger et al. (2018) and Kaplan et al. (2016). This is in contrast to the estimates of Mian et al. (2013) and Aladangady (2017) which include an employment effect, as documented by Mian and Sufi (2014).[39] Also Charles et al. (2017) document strong interaction effects between the housing and labor markets. In contrast, the house price shock that we consider is local and there is no evidence of an effect on labor income.[40]

[37] Source: Statistics Sweden, Table NR0103B0.

[38] The lower (upper) bound on the 95% confidence interval of the intensive margin elasticity is 0.001 (0.907).

[39] See also page 3434 in Aladangady (2017) for a discussion of general equilibrium effects.

[40] There are also other noteworthy differences to Mian et al. (2013). The R.L. Polk data set for the U.S. reports the number of new car registrations. To arrive at an imputed value, the census expenditure weight is imposed. Consequently, they find a sizable effect driven by the extensive margin. Another noteworthy

Third, crisis estimates may be different to normal times estimates for many reasons. Time-variation in the response may be due to changes in beliefs about future house prices, as proposed by Kaplan et al. (2016), or simply due to the salience of house price movements in fragile economic times. To be specific, our experimental design only relies on a relative price fall. In fact, house prices in the noise area do not decrease in absolute terms during the five quarters that we consider. They remain flat while house prices outside continue to increase. This is in contrast to Mian et al. (2013) who include the 2006 U.S. house price bust which induces collateral effects.[41]

Fourth, as is well-known in (S,s) models and illustrated in Berger and Vavra (2015) the evolution of the cross-sectional distribution of car holdings may lead to sizable state dependent variation in the response.

Fifth, despite the advantages mentioned above our estimate may not reflect a representative population – not even the population of Swedish single family house owners. The heterogeneity in the responses are apparently substantial, which suggest that this is important to consider. We compare our sample of single-family house owners in Stockholm to the population of Swedish single-family house owners. Our sample deviates from the population of house owners in Sweden in that house owners in Stockholm are richer both in terms of income and wealth (see Appendix A, Table 11). We therefore re-produce our main elasticity estimates after re-weighting the sample using inverse probability weights.[42] The application of weights implies that point estimates increase, consistent with the heterogeneous effects,

difference is that we focus on homeowners exclusively whereas the aggregation to ZIP code level means that homeowners and renters are mixed in the analysis.

[41] See page 3420 of Aladangady (2017) for a discussion.

[42] We pin down the 33rd and 67th percentile cut-off in the 2006 distributions for labor income, housing wealth, and net worth and use them to compute population weights for 27 (3^3) cells. We then compute the sample weights for these cells. Notice that we only apply the weights in the reduced form and second stage estimation since we do not have the necessary variables for reweighing in the transaction data set. Hence we need to assume that first-stage effects (i.e., effects on house prices) are independent of the house owner's characteristics. There is no reason to believe that this would not be the case in our experiment – transaction volume of houses is unaffected and we observe no impact on the probability of selling the house.

but also that inference becomes less precise. Therefore we relegate the reported estimates to the Appendix (Tables 18–19).[43] That said, the reduced form intensive margin effect on new cars nearly doubles (and remains statistically significant). The re-weighing implies that an MPC on cars for Sweden may well be 0.28 cents per dollar, which is only half the size of the estimate of Mian et al. (2013) and statistically indistinguishable. In addition, the reweighing affects the interaction effect with the CLTV ratio. The Appendix (Table 19) indicates that elasticity in the group with medium CLTVs increases as well.[44] This further corroborates that household leverage is intimately related to economic stability (Mian and Sufi (2018)).[45]

6 Concluding Remarks

Long before the financial crisis economists suspected that the housing market is of importance for the evolution of the macro economy through the wealth effect it exposes households to. However, because of weak identification the credibility of available estimates of the elasticity between house prices and household consumption has been low.

We base our estimate on a quasi-experiment that occurred in Stockholm as a result of political bargaining behind closed doors. It was decided that Bromma Airport, located in a residential area, would not end its operations in 2011 as previously believed. Rather, its operating contract with the municipality was renewed and operations were allowed to

[43]We wish to thank Paolo Sodini for supplying the summary statistics as well as the cut-off values in the population.

[44]The Appendix (Table 18) shows that there is essentially no response on the extensive margin also when population weighing, possibly with the exception of a weak substitution effect to used cars.

[45]It is far from straightforward to convert our estimates on cars to one which represents all goods and services. Basically, one can proceed in two ways which gives quite different results. If one assumes equal MPC responses on all goods and services, including cars, one can rely on cars' expense share of total consumption (6.1%). This would imply an MPC of 0.038 on all goods and services. However, cars are well-known to be more cyclical than total consumption (see, e.g., Figure 17). Indeed, Mian et al. (2013) find that cars' MPC share of total consumption is 42.6% (0.023/0.054). This would imply an MPC of 0.0030 on all goods and services which is quite low even relative to the life-cycle PIH model. However, considering the upper bound of the 95% confidence interval and/or reweighing our sample to represent the Swedish economy, it would imply a low but theoretically reasonable value. Appendix F (Table 20) summarizes the literature and finds estimates in the range from 0.01 to 0.06.

continue until 2036. The event offers a unique possibility to analyze the consequence of local partial equilibrium housing wealth shock which we identify using a transaction data set of single-family houses. Prices of houses located close to the airport's noise contour fell by approximately 15% soon after the renewal of the operating contract. The closer to the airport's noise contour the greater impact.

Since the shock to house prices was local, unanticipated, and uncorrelated with changing macroeconomic conditions it provides a unique opportunity to study a partial equilibrium housing wealth effect as recently examined in theoretical work (Kaplan et al. (2016), Berger et al. (2018)). We estimate the elasticity between house prices and consumption, using a population data set of all homeowners that live in single-family houses in Stockholm and a two-sample instrumental variables approach.

Our first main finding is that we find a sizeable MPC of 3.3 cents per dollar among households that purchase a new car. Nevertheless, the implied aggregate MPCs are on the small side but plausible once the appropriate conversion has been applied. We speculate that differences to previous estimates may reflect a combination of differences in data granularity (i.e, household vs. geographically aggregated), identification strategies, and that our estimate reflects normal economic times. Our second main finding is that our analysis does not support a monotone relationship between the MPC and household leverage. Rather, the relationship is hump-shaped in our micro data set. The response is entirely concentrated to car purchasers that have a combined LTV ratio between 0.6 and 0.8. For this group, the response amounts to an MPC of 11.8 cents per dollar. Thus, our analysis confirms that households' combined LTV ratio is central also in normal times but that the relationship between LTV and response is not monotone. Our third main finding is that we confirm recent macroeconomic models that the heterogeneity in that the response to house prices is very large.

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A Details on the household data set

Table 10: **Restrictions on the household data set**

Sample restrictions	# Households
1. Households that are coded as living in Stockholm municipality and own their main property in Stockholm in the end of 2006	41,147
2. Drop households if at least one household member owns more than one property in Stockholm	39,519
3. Drop households that own only untaxed property*	37,917
4. Drop households that own more than one property in Stockholm	37,898
5. Drop households that whose residential address is in an area of Stockholm that has no single-family houses	36,580
6. Drop households with inconsistent geocoding**	36,075
7. Drop households that own less than 50% of a property	35,397
8. Drop extreme outliers in CLTV***	35,386

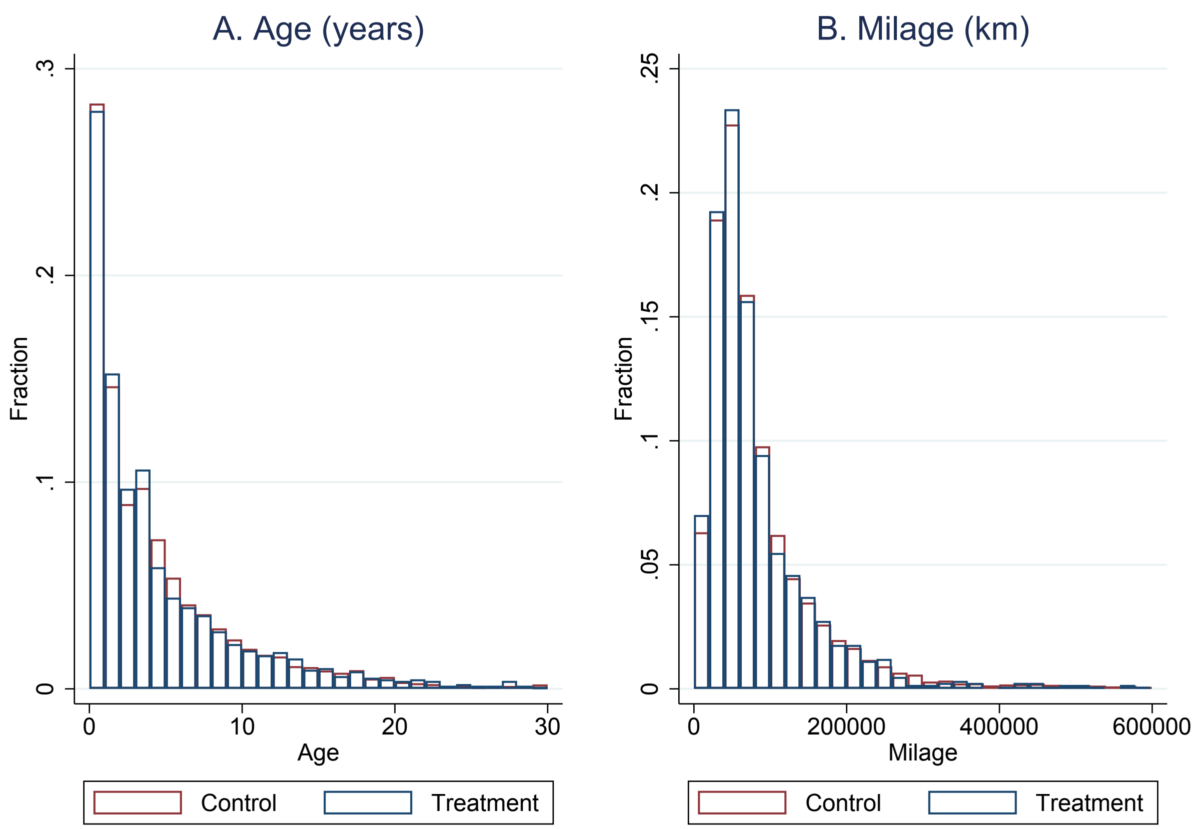
Note: The table presents the number of households that remain after imposing consecutive sample restrictions.
*) Examples of untaxed property classes are boat houses, churches, and small allotments for private farming.
**) Our initial sample of 41,147 households have Stockholm municipality as primary residence. In some cases the geocoding is inconsistent with this.
***) The cut-off in terms of combined loan-to-value ratio (CLTV) is 1,000. Approximately 0.6% of the households have a CLTV exceeding 2.0. 4.4% of the households have a CLTV exceeding 1.0. The CLTV includes student loans.

Table 11: **Households residing in single-family houses in Stockholm versus Sweden**

	Our sample		Stockholm		Sweden	
	Mean	SD	Mean	SD	Mean	SD
Panel A. Sociodemographics						
Age	53.95	14.46	53.25	14.39	53.85	14.99
Household size	2.817	1.338	2.662	1.360	2.537	1.335
Panel B. Balances sheets and income						
Financial wealth	974.5	2928	899.6	4384	537.9	2411
Housing wealth	3791	10884	3444	5909	1734	2549
Total wealth	4765	11671	4344	7744	2272	3821
Total debt	1256	1848	1237	2794	753.3	1239
CLTV	0.407	2.752	0.443	0.544	0.528	0.593
Housing / Net worth	1.448	22.1	1.286	64.8	2.122	963.2
Labor income	570.7	580.3	542.9	561.9	382.4	354.6
Capital income	52.54	864.5	51.00	950.2	17.87	541.0
N	35,388		42,681		1,419,159	

Note: All values in 1000s of Swedish kronor (kSEK) at the end of 2006. ‘Our sample’ refers to the sample in Table 2. ‘Stockholm’ and ‘Sweden’ refer to sample statistics provided by Paolo Sodini. ‘Stockholm’ are households in the municipality of Stockholm that own a single-family house but no co-op apartment. ‘Sweden’ imposes the same restrictions on ownership but no restriction on geographic location.

Figure 5: **Cross-sectional distributions related to car ownership (2006)**



Note: The figure displays the cross-sectional distributions of the variables Age and Mileage reported in Table 2.

B Details on the real estate transactions data set

B.1 Residualization of data

The data set on real estate transactions include approximately 2,500 transactions per year within the municipality of Stockholm. Statistics Sweden's official index FASTPI includes fewer transactions. The FASTPI index excludes properties with site-leasehold rights (rather than title deeds), transactions based on low prices, and transactions that display irregular discrepancies between tax value and transaction price. We choose to strike a balance between maintaining representativeness of Stockholm's home owners and comparability with FASTPI by first residualizing our data, rather than excluding observations all together. We first residualize the transaction price against the following covariates: tax value, a dummy variable that indicates site-leasehold right, and a dummy variable for transaction price below 250 kSEK. To these residuals we add the average price. Figure 6 displays the equivalent of Figure 2 in the main text without prior residualization.

B.2 Comparison to official house price indices

We compare our house price index of Panel A in Figure 2 to official providers' indices. Figure 7 displays our house price index ("Outside and inside") next to a version that imposes similar sample restrictions as Statistics Sweden (SCB) does when constructing their index FASTPI ("Outside and inside (restr.)").[46] We drop all transactions with a price below 250 kSEK and all transactions that involve site-leasehold rights. We then residualize the data with respect to tax value, only. Next to these two versions of our index, the figure reports SCB's index and Valueguard's index HOX Stockholm Villa. The restricted version of our index displays a very similar profile to Valueguard's HOX index. Among all indices, SCB's is the least

[46]We are grateful to Martin Verhage at Statistics Sweden for applying Statistics Sweden's method to the transactions in the municipality of Stockholm.

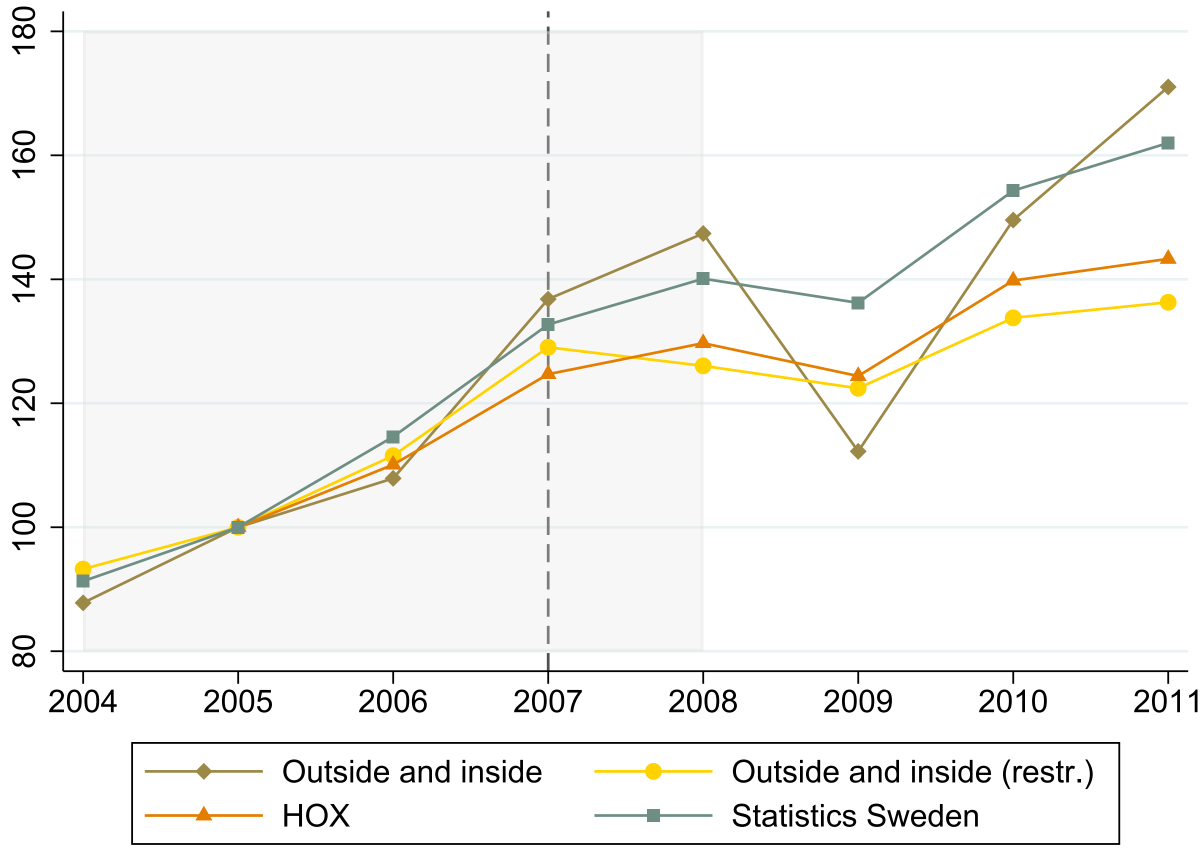
Figure 6: **Effect on house prices (non-residualized data)**

Note: The figure displays the equivalent of Figure 2 in the main text without prior residualization.

volatile one because of the heavy restrictions imposed on the input data.

The discrepancy is notably larger in the years 2009 and onwards. One reason may be that average characteristics of transacted properties shifts due to the financial crisis which makes index construction more difficult – see Section B.5 below. This is one reason to exclude 2008Q4 and later.

Figure 7: **Comparison to official house price indices**



Note: The figure displays Valueguard's index 'HOX Villa Stockholm' and Statistics Sweden's index 'FASTPI' for the municipality of Stockholm. The figure also depicts two indices based on our data set; the house price index of Figure 2 ('Outside and inside') and a more restricted version, in line with the intention of the restrictions of Statistics Sweden's FASTPI index. For all indices the timing is calendar year, as opposed to the timing in the main text.

Table 12 replicates our first-stage estimation of Table 4 on the restricted sample reported in Figure 7.

Table 12: **Effect on house prices (restricted sample)**

	(1)	(2)	(3)	(4)	(5)	(6)
	log house price	log house price	log house price	log house price	house price	log house price
Noise area$_i$ × Post$_t$	-0.244***	-0.199***	-0.196***	-0.206***	-551.1***	-0.177***
	(0.062)	(0.527)	(0.543)	(0.048)	(159.5)	(0.040)
Noise area$_i$	0.152**	0.152**	0.195***	0.157**	204.6***	0.198***
	(0.584)	(0.060)	(0.054)	(0.043)	(72.2)	(0.051)
N	9,103	8,759	8,759	8,759	8,759	15,875
R-squared	0.089	0.093	0.2380	0.306	0.464	0.537
2004Q1-2008Q3	Yes	Yes	Yes	Yes	Yes	No
Exclude non-missing tax value	No	Yes	Yes	Yes	Yes	Yes
Basic controls	No	No	Yes	Yes	Yes	Yes
Additional controls	No	No	No	Yes	No	No

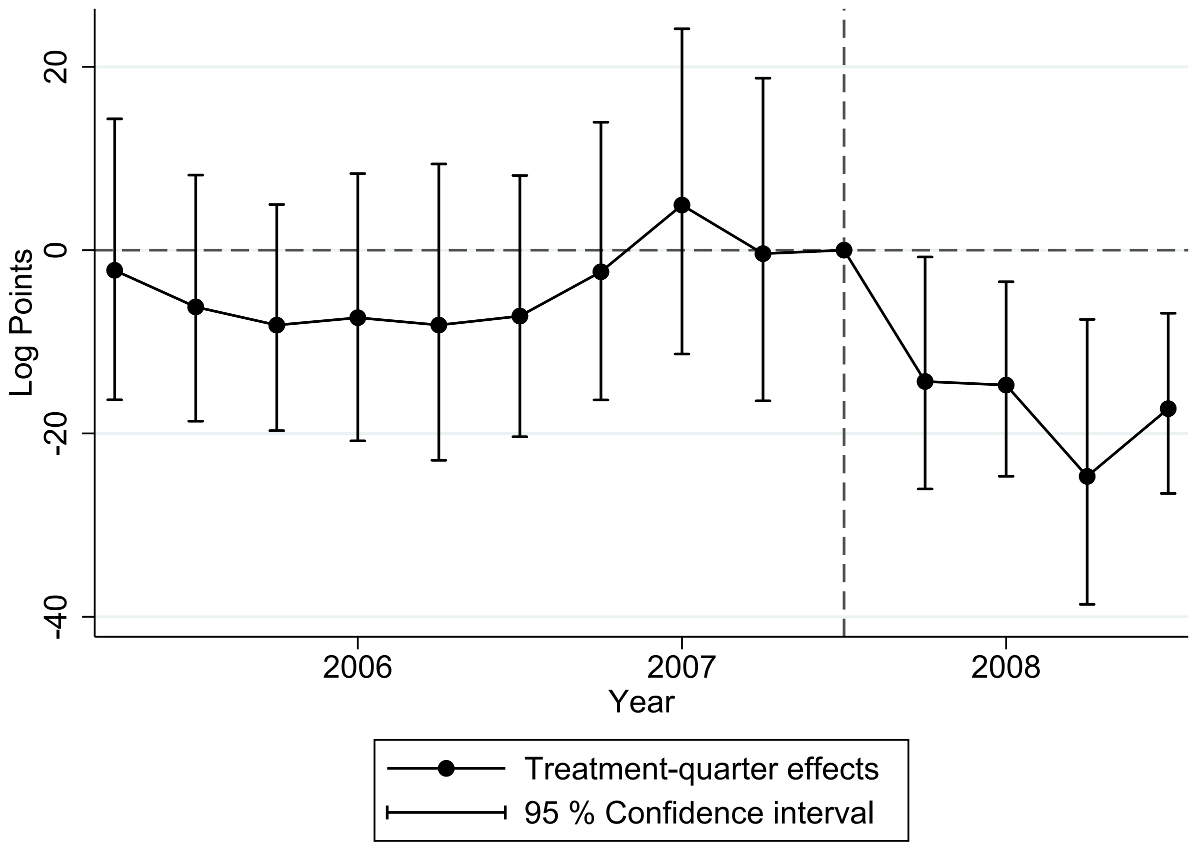
Note: This table is an analogue to Table 4 but the analysis is on a restricted sample of transactions is intended to capture Statistics Sweden's imposed restrictions of the FASTPI index (see also Figure 7). The basic controls include the tax value, an indicator whether the property is a site-leasehold right, and an indicator for a low sales price (<250 kSEK). The set of additional control variables include age, standard, plot area, living area, and non-living area, and the change in amount of property tax to be paid due to the tax reform coming into effect in 2008. Clustering of standard errors at the level of the 250 × 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B.3 First stage at quarterly frequency

B.4 No composition effects of contract renewal

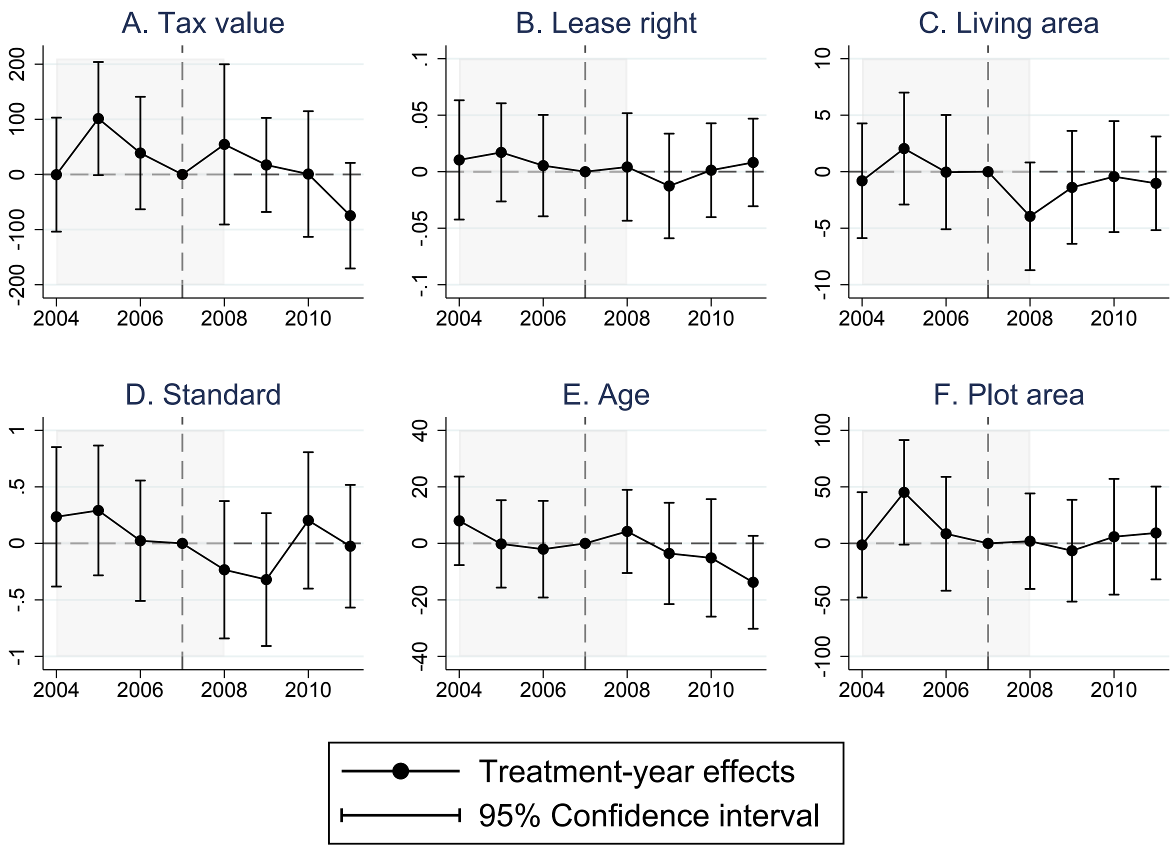
Figure 9 report DID estimates of the effect on properties' characteristics inside versus outside the noise area. The estimates indicate that the renewal of Bromma Airport's operating contract did not lead to compositional effects inside versus outside the noise area.

Figure 8: **Effect on house prices (quarterly frequency)**



Note: The figure displays the equivalent of Panel B, Figure 2, in the main text at quarterly frequency.

Figure 9: **Composition effects of contract renewal**



Note: The figure displays DID estimates analogous to Figure 2 for six property characteristics.

B.5 Fluctuations in average characteristics

Figure 10 show average characteristics of transacted properties (Panels A to E) and transaction volume (Panel F). There are both temporary and longer lasting effects of the financial crisis.

Figure 10: **Fluctuations in average characteristics**

Note: Panel A to E in the the figure display average characteristics of transacted properties. Panel F report transaction volume.

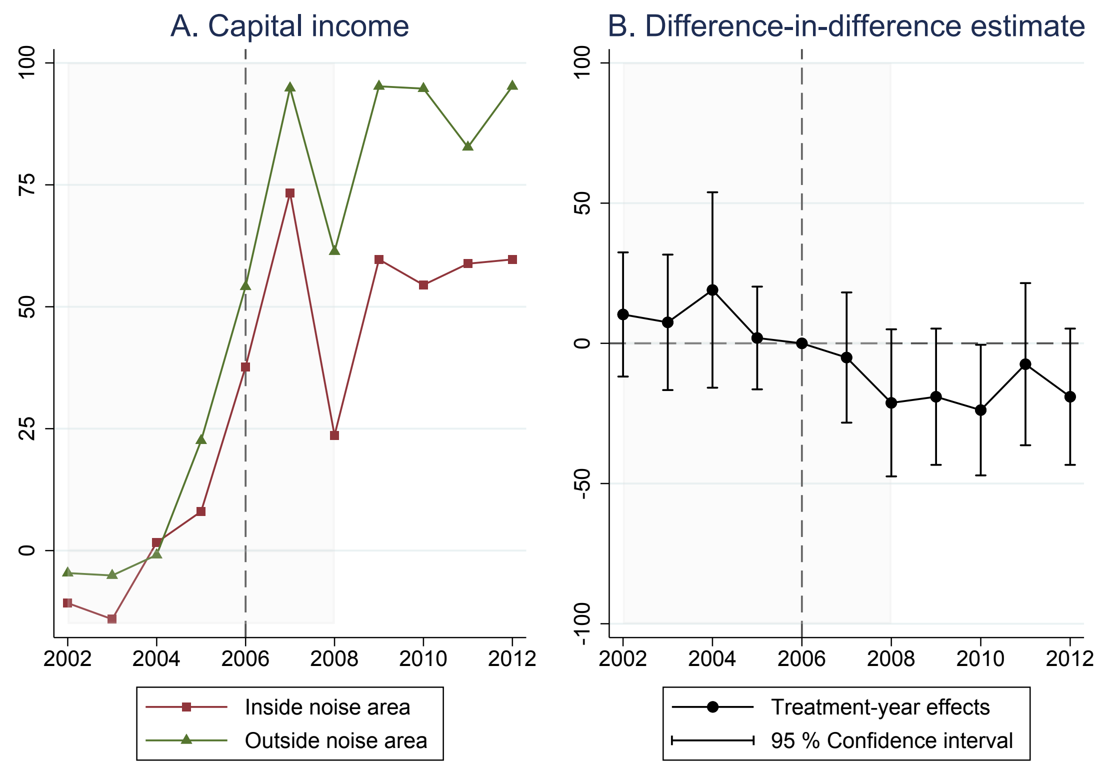
B.6 Alternative control groups

Table 13: **Alternative control groups**

	(1)	(2)	(3)	(4)	(5)
	log house price	log house price	log house price	log house price	log house price
Noise area$_i$ × Post$_t$	-0.198***	-0.143***	-0.054	-0.085*	-0.136***
	(0.033)	(0.037)	(0.046)	(0.033)	(0.034)
Noise area$_i$	0.087*	0.063	0.058	0.066	0.064
	(0.045)	(0.045)	(0.046)	(0.047)	(0.044)
Observations	10,599	8,210	5,897	5,471	9,547
R-squared	0.338	0.371	0.356	0.357	0.335
Control	South-E.	South-W.	North-E.	North-W.	<5,000m
Avg. distance of control	4,345m	3,438m	1,878m	1,858m	3,328m
2004Q1-2008Q3	Yes	Yes	Yes	Yes	Yes
Restricted	Yes	Yes	Yes	Yes	Yes
Parish FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Note: Estimates based on the full treatment group but different geographical control groups. Clustering of standard errors at the level of the 250 × 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

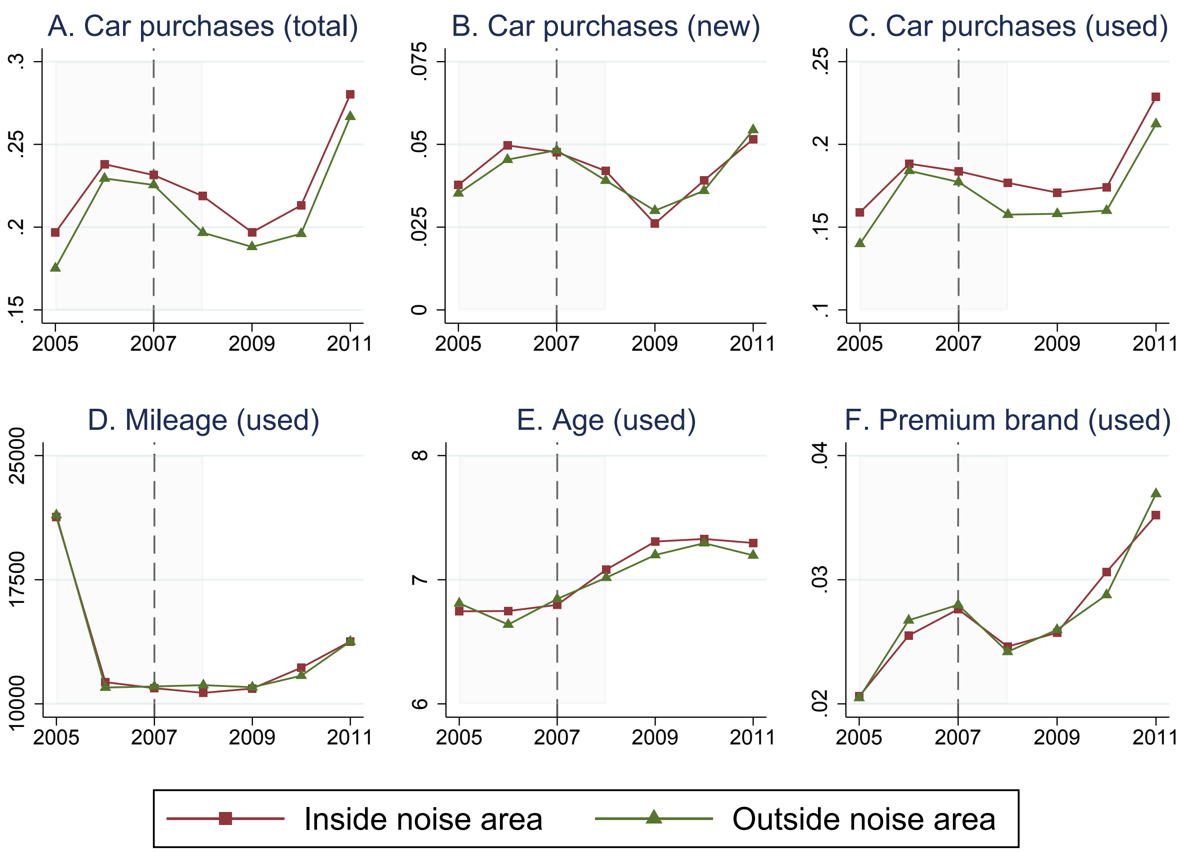
Figure 11: **Effect on capital income**



Note: The figure shows the capital income for households inside and outside of the noise area (Panel A) and the corresponding DID estimates (Panel B). The timing on the horizontal axis is calendar year. There are no control variables in the underlying regression.

C Details on car transactions

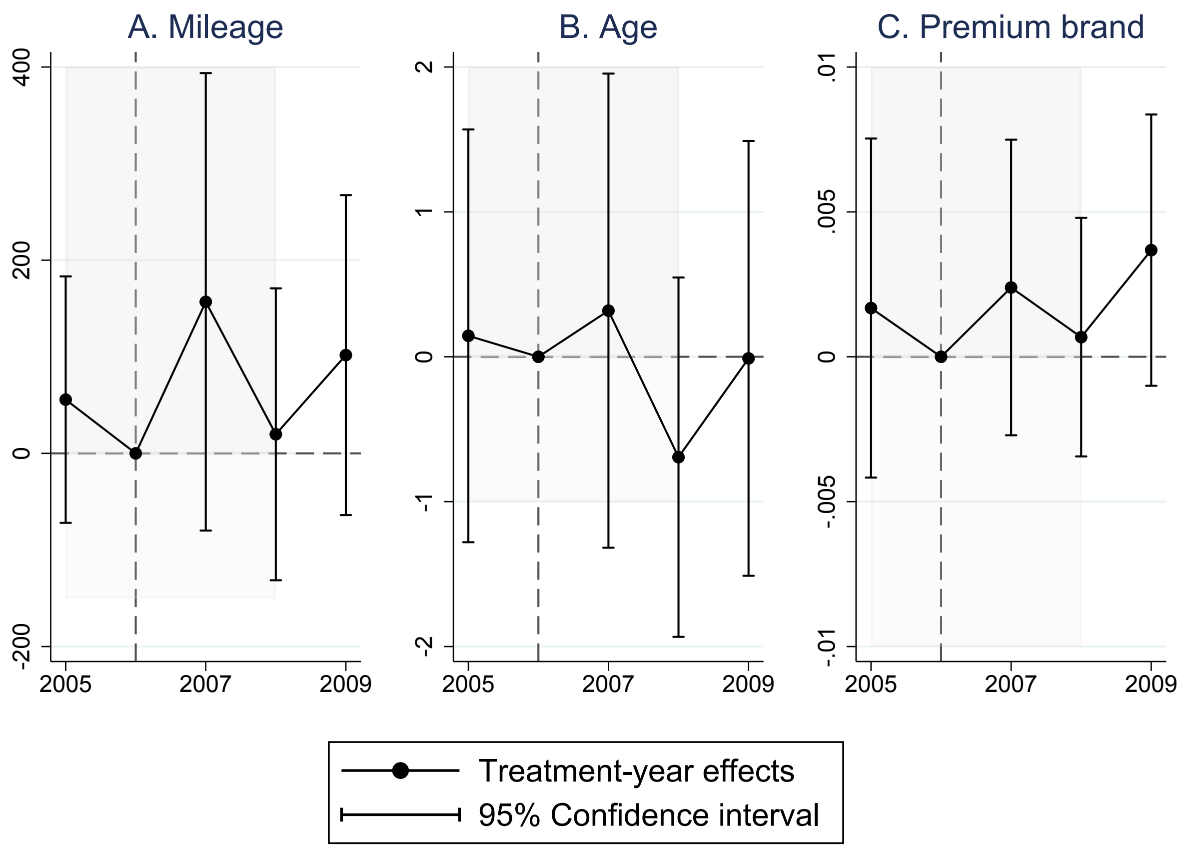
Figure 12: **Car purchases: extensive margin and substitution effects**



Note: The figure shows 12-months averages inside and outside the noise area, corresponding to the DID estimates in Figure 3. The timing on the horizontal axis is shifted one quarter. That is, 2007 refers to 2006Q4–2007Q3 and so forth. Premium brands refer to Audi, BMW, and Mercedes. The shaded grey area indices the main sample period (2004Q1-2008Q3).

Table 16 reports evidence of debt responses upon a car purchase. A purchase of a new car is associated with an average increase in debt of SEK 224,000 which is only SEK 24,000 less than the average price of a new car (Column (1)). Purchases of used cars leads to an average increase in debt of SEK 133,000 (Column (2)). A purchase of used car from a premium brand leads to an even greater increase in debt (Column (3)). Columns (4)-(7) illustrate that the

Figure 13: **Car sales: extensive margin and substitution effects**



Note: The figure shows 12-months averages inside and outside the noise area, analogous to the DID estimates in Figure 3 for households' sales. The timing on the horizontal axis is shifted one quarter. That is, 2007 refers to 2006Q4–2007Q3 and so forth. Premium brands refer to Audi, BMW, and Mercedes. The shaded grey area indices the main sample period (2004Q1-2008Q3).

Table 14: **Effects on cars – extensive margin and used cars**

	(1)	(2)	(3)	(4)	(5)	(6)
	Purchases (New & Used)	Purchases (New)	Purchases (Used)	Mileage (Used)	Age (Used)	Premium brand (Used)
Noise area$_i$ × Post$_t$	0.002	0.000	0.002	-612.1	0.001	0.000
	(0.002)	(0.001)	(0.002)	(524.1)	(0.194)	(0.001)
Noise area$_i$	0.001	0.000	0.000	54.150	0.214	-0.000
	(0.002)	(0.001)	(0.002)	(478.2)	(0.147)	(0.001)
N	530,820	530,820	530,820	21,927	21,972	530,820
R-squared	0.013	0.002	0.013	0.059	0.023	0.003
2005-2008Q3	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports DID estimates corresponding to Figure 3. The set of control variables includes household size, age of household head, labor income, housing wealth, loan-to-value ratio and net worth. Clustering of standard errors at the level of the 250 × 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

response in debt decreases with the perceived value of the car. As we have no price data of used cars this analysis supports our view that the available car characteristics for used cars captures a great deal of the market value. Overall, the table provides strong evidence that the households finance their car purchases by taking on additional debt. For new cars, approximately 90% (224,000/248,000) of the transaction is debt-financed.

As emphasized by the (S, s) literature on durable goods, the response to shocks is intimately linked to the mass of households close to the border of their adjustment range. We therefore investigate the response of households conditional on the time since their last car purchase in Table 17.

Table 15: **Effect on the price of new cars: dose response**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	log car price (New)	log car price (New)	log car price (New)	log car price (New)	log car price (New)	log car price (New)	log car price (New)
$\text{Post}_t \times 0$	-0.088*						
	(0.046)						
$\text{Post}_t \times (0 \quad 500]$	-0.054						
	(0.044)						
$\text{Post}_t \times (500 \quad 1000]$	-0.045						
	(0.048)						
$\text{Post}_t \times \log^{10}(\text{dist}^2)$		0.038**					
		(0.017)					
$\text{Post}_t \times 0$			-0.028				
			(0.023)				
$\text{Post}_t \times [0 \quad 200]$				-0.058*			
				(0.035)			
$\text{Post}_t \times [0 \quad 400]$					-0.047		
					(0.037)		
$\text{Post}_t \times [0 \quad 600]$						-0.078**	
						(0.033)	
$\text{Post}_t \times [0 \quad 800]$							-0.077***
							(0.031)
N	4,794	4,770	4,794	4,794	4,794	4,794	4,794
R-squared	0.055	0.054	0.054	0.055	0.055	0.055	0.055
2005Q1-2008Q3	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The set of control variables includes house hold size, age of household head, labor income, housing wealth, loan-to-value ratio, and net worth. Clustering of standard errors at the level of the 250 × 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 16: **Response in debt to a car purchase (2004–2007)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
New Car	224.3***						
	(15.8)						
Used Car		132.6***					
		(8.4)					
Used Car× (< 5years) × (< 100,000km)			168.6***				
			(24.4)				
Used Car× Premium brand				156.6***			
				(0.21)			
Used Car× (≥ 5years)					140***		
					(10.2)		
Used Car× (≥ 100,000km)						131.2***	
						(21.8)	
Used Car× (≥ 5years) × (≥ 100,000km)							8.6***
							(8.6)
N	105,979	105,980	105,981	105,985	105,982	105,983	105,984
R-squared	0.885	0.886	0.885	0.885	0.885	0.885	0.886

Note: The sample consists of all single-family house owners that reside in Stockholm. The years are 2004 to 2007. The dependent variable is total debt, measured in 1000s of Swedish kronor (kSEK). All the reported covariates are interacted with the count variable Purchases to obtain estimates on a per purchase basis. This variable is omitted from the table for brevity. All the regressions include household fixed effects and year fixed effects. The exchange rate is about 8 SEK/USD. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

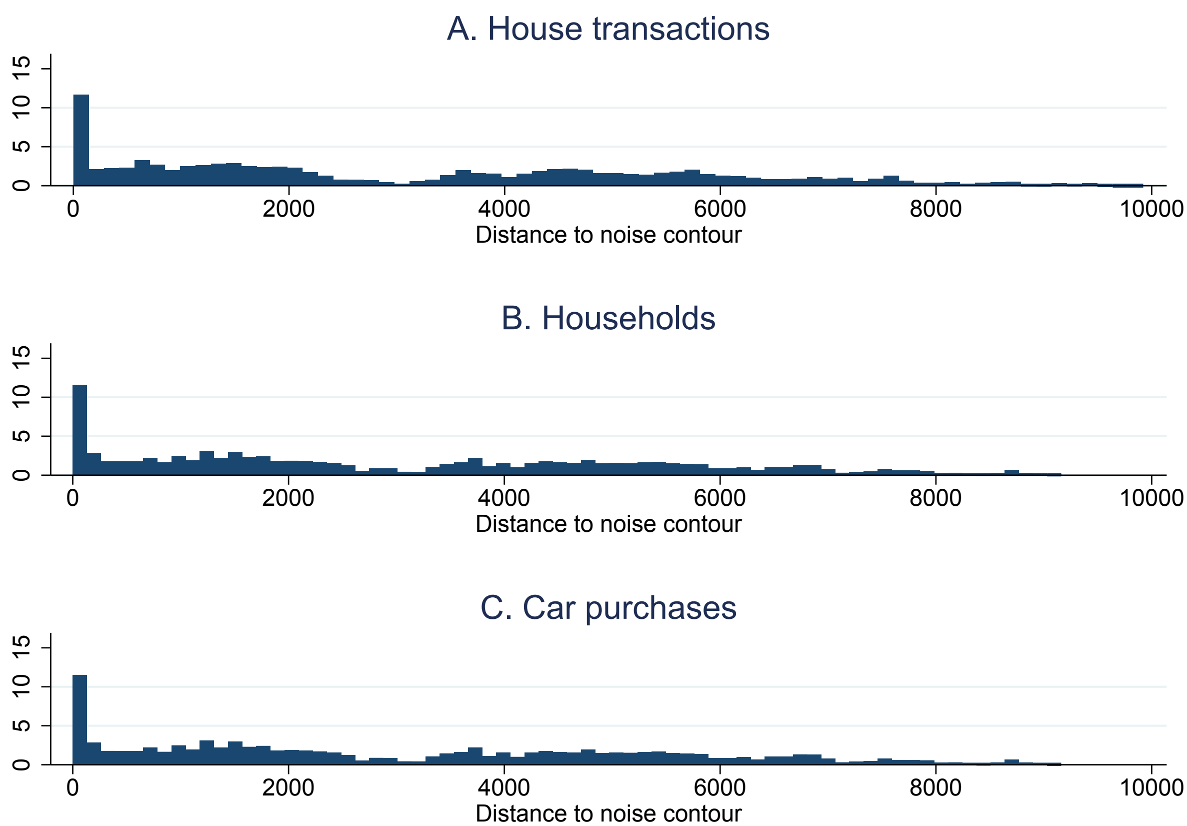
Table 17: **Effects by time since last car purchase**

	(1)	(2)	(3)	(4)	(5)	(6)
	Purchases (New & Used)	Purchases (New & Used)	Purchases (New & Used)	log car price (New)	log car price (New)	log car price (New)
log(House price$_{it}$)	-0.041	0.002	-0.009	0.846	0.637	1.119
	(0.036)	(0.010)	(0.010)	(0.549)	(0.363)	(2.741)
N	150,060	142,915	123,670	2,913	1,239	326
R-squared	0.039	0.018	0.007	0.077	0.069	0.133
Last purchase	<2	$2-4$	>4	<2	$2-4$	>4

Note: The table reports IV estimates for households who purchased their last car less than 2 years ago, 2–4 years ago, or more than 4 years ago. The set of control variables includes household size, age of household head, labor income, housing wealth, loan-to-value ratio, and net worth. Clustering of standard errors at the level of the 250 $\times$ 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

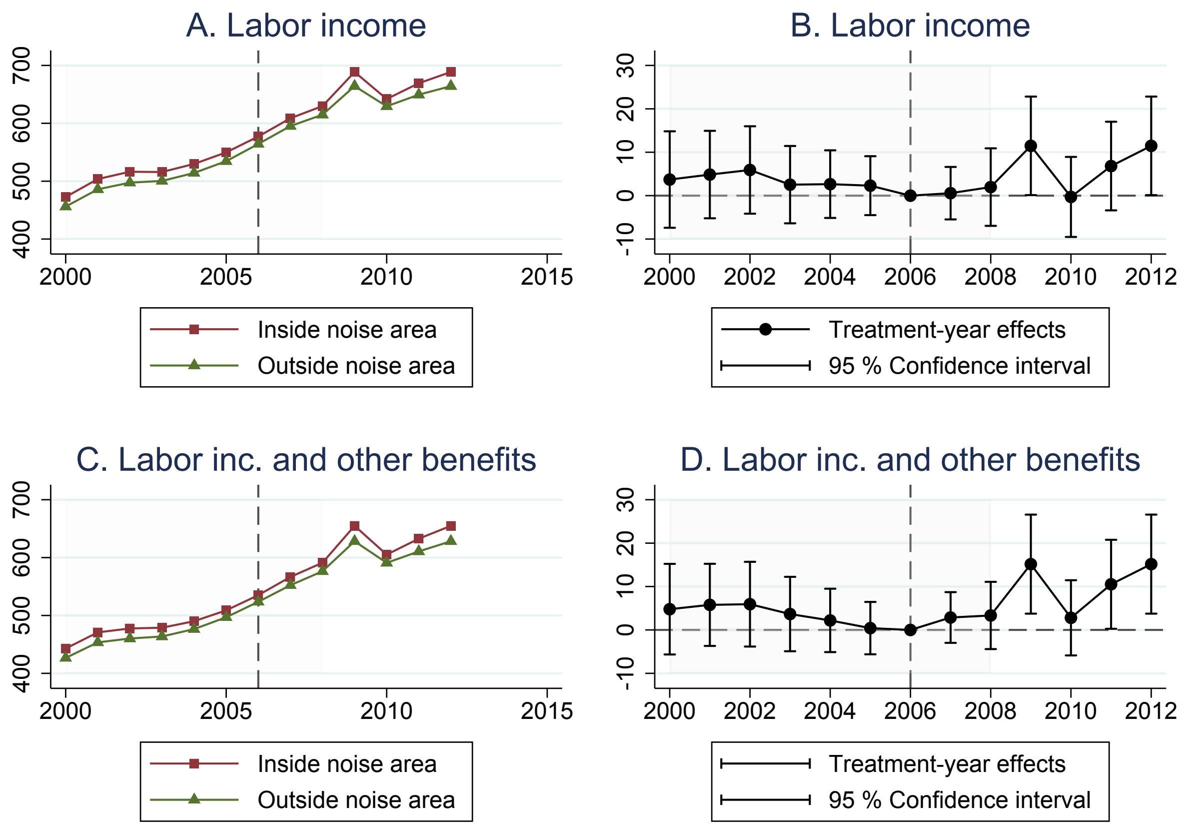
D Additional statistics

Figure 14: **Geographical distributions relative to the noise contour**



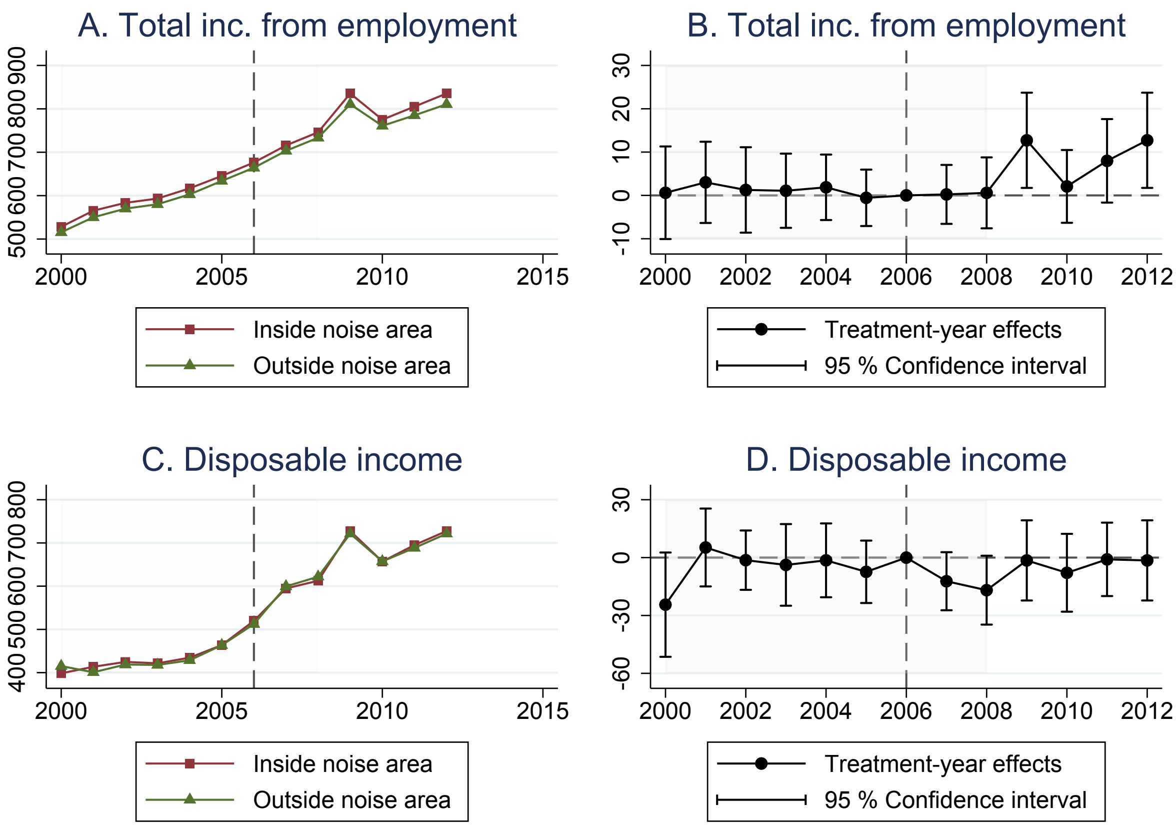
Note: The figure displays the geographical distributions of house transactions, households, and car transactions.

Figure 15: **Labor income and other employee benefits**



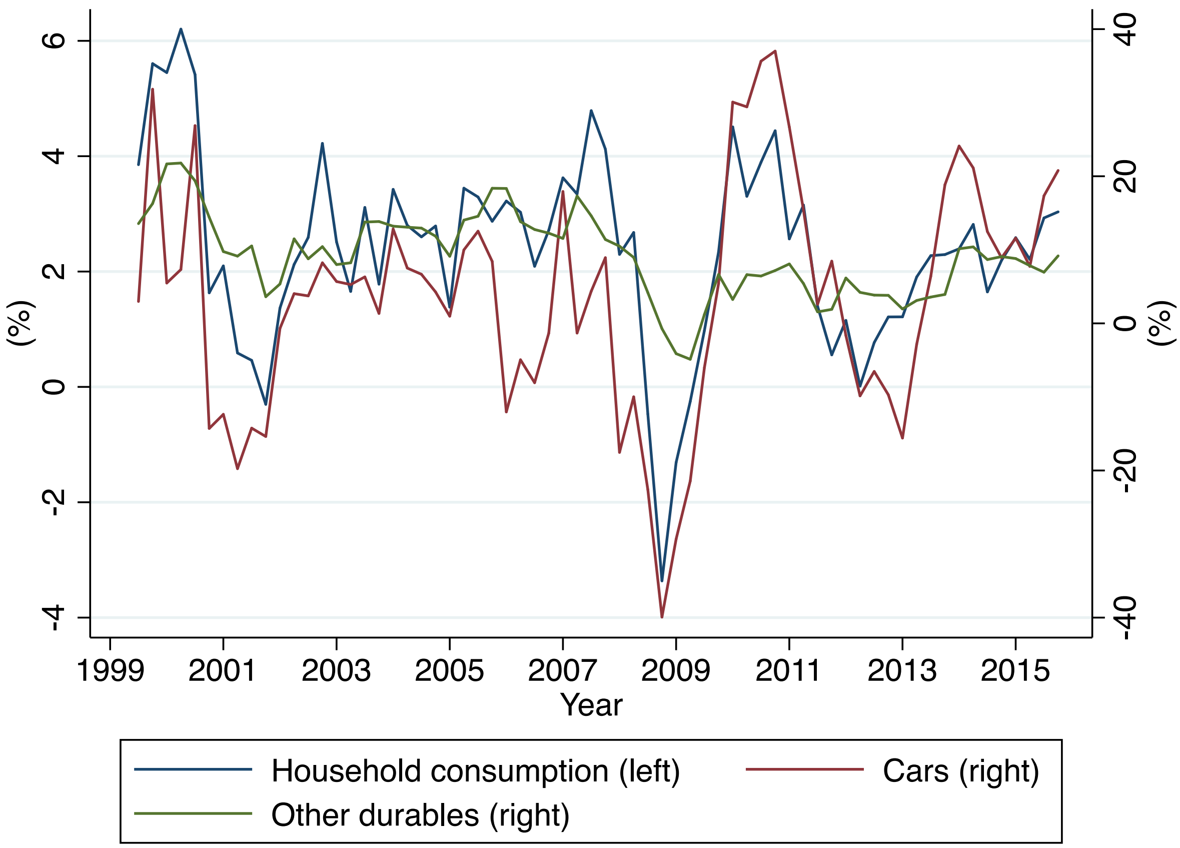
Note: The figure displays the evolution of labor income (Panels A and B) and labor income plus other employee benefits (Panels C and D).

Figure 16: **Total income from employment and disposable income**



Note: The figure displays the evolution of total income from employment (Panels A and B) and disposable income, defined as gross labor income plus capital income plus transfers minus taxes (Panels C and D).

Figure 17: **Household consumption growth (Sweden)**



Note: The figure displays year-on-year consumption growth for Swedish households.

E Population estimates

Table 18: **Effects on cars – extensive margin and used cars (population weighted)**

	(1)	(2)	(3)	(4)	(5)	(6)
	Purchases (New & Used)	Purchases (New)	Purchases (Used)	Mileage (Used)	Age (Used)	Premium brand (Used)
Panel A. Reduced form						
Noise area$_i$ × Post$_t$	0.012*	0.000	0.012*	-395.0	0.780	0.000
	(0.007)	(0.001)	(0.007)	(1814)	(0.937)	(0.001)
Noise area$_i$						
N	530,820	530,820	530,820	21,927	21,972	530,820
R-squared						
2005-2008Q3	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Panel B. IV estimates						
log(House price$_{it}$)	-0.085	-0.003	-0.082	2738	-5.400	0.000
	(0.105)	(0.008)	(0.108)	(12656)	(9.200)	(0.009)
N	530,820	530,820	530,820	21,927	21,972	530,820
R-squared						
2005-2008Q3	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports DID estimates corresponding to Table 14 but with population weights. The control variables are the same. Clustering of standard errors at the level of the 250 × 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 19: **Intensive margin effects by loan-to-value ratio (population weighted)**

	(1)	(2)	(3)	(4)
	log car price (New)	log car price (New)	log car price (New)	log car price (New)
Panel A. Reduced form estimates				
Noise area$_i$ × Post$_t$	-0.128*	-0.015	-0.349***	-0.062
	(0.074)	(0.055)	(0.105)	(0.138)
Panel B. IV estimates				
log(House price$_{it}$)	0.889	-0.101	2.422**	0.434
	(0.756)	(0.382)	(1.050)	(0.976)
N	4,794	3765	546	483
CLTV ratio	—	0.0–0.6	0.6–0.8	0.8>
2005Q1-2008Q3	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: The table presents reduced form and second-stage two-sample IV estimates corresponding to Table **??** but with population weights. The control variables are the same. Standard errors are corrected for first-stage estimation. Clustering of standard errors at the level of the 250 × 250 meter grid. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

F Estimates from previous literature

Table 20: **Estimates of housing wealth effects from previous literature**

Study	Country	Elasticity	MPC	MPC on cars
Attanasio et al. (2009)*	United Kingdom	0.00	–	–
Cambell and Cocco (2007)**	United Kingdom	0-1.7	–	–
Case, Quigley, and Shiller (2013)***	United States	0.10	–	–
Browning et al (2013)****	Denmark	0.13	0.05	–
Cooper (2013)	United States	0.06	0.06	–
Mian et al (2013)*****	United States	0.13-0.25	0.054	0.023
Disney et al (2010)	United Kingdom	–	0.01	–
Carrol (2011)******	United States	–	0.02-0.09	–
Kaplan et al (2016)	United States	0.24-0.36	–	–
Aladangady (2017)	United States	–	0.047	–
Guerrieri and Iacoviello (2017)	United States	–	0.24	–
DeFusco (2018)*******	United States	–	0.04-0.13	–

Note: The table presents estimates from previous studies.
*) Estimates reported in the Table 1 are positive but the authors refuse to interpret them as casual.
**) 1.7 is for older homeowners, 0 for young renters. Estimates need to be scaled with the proportion of the groups owning real estate for them to attain comparable aggregate estimates.
***) Elasticity refers to a decline in house prices.
****) Positive effects only for subsample (young and constraint), i.e. no pure wealth effect. Estimates should be scaled with the proportion of such households to attain comparable aggregate estimates.
*****) Scaled with H/NW, i.e. the housing wealth to net worth ratio.
******) Lower range of estimate is the direct effect, upper range is long run estimate
*******) Assuming 100% consumption out of an increase in debt.