Identifying the Benefits from Homeownership: A Swedish Experiment*

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Abstract

Homeownership is widely stimulated by policy yet its economic effects are poorly understood. We exploit quasi-random variation in homeownership generated by privatization decisions of municipally-owned buildings, and use granular data on demographics, income, housing, financial wealth, and debt that allow us to construct high-quality measures of spending. Homeownership causes wealth building via house price appreciation, increases consumption, and improves consumption smoothing across time and states of the world through a collateral effect. It increases mobility for young households, who move up the property ladder, and amplifies wealth accumulation for older households, who take more risk in their financial portfolio.

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1 Introduction

Developed and developing economies alike deploy a myriad of housing policies to encourage homeownership, which enjoy broad political support. The United States alone spends roughly $200 billion per year stimulating homeownership.[1] Conventional wisdom is that homeownership confers many benefits to households (Goodman and Mayer, 2018, and the references therein). Despite the policy relevance, there is little empirical evidence for the hypothesized benefits of homeownership.

Finding convincing evidence for the economic effects of homeownership is challenging for two major reasons. First, a simple comparison of outcomes for owners and renters is plagued by severe endogeneity issues. Owners differ from renters in a myriad of ways (age, education, income, financial wealth, etc.). Furthermore, properties that are owned have different attributes than properties that are rented (location, size, age, neighborhood quality, etc.). While some household- and building-level characteristics are observable to the researcher and can be controlled for, the problem of omitted characteristics is hard to avoid. The second challenge is the lack of high-quality data allowing the researcher to track households' tenure status, wealth, and consumption over time. The ideal experiment is one where identical households are randomly assigned into owners and renters of identical housing units offered at the same cost, and their economic decisions are measured for multiple years before and after the experiment and compared. For obvious fiscal, technical, and ethical reasons, such random experiments do not exist.

This paper makes progress on these two challenges. We explore a quasi-experiment that approximates the ideal experiment. An unexpected change to the privatization process of rental apartments provides plausibly exogenous variation in homeownership. High-quality data from administrative registries enable us to track the tenants of the buildings that were affected by this change for multiple years before and after the experiment.

We focus on the effects of homeownership on the following five household outcomes. First, we find that homeownership causes substantial wealth accumulation. The wealth-building effect of homeownership depends on house price growth, which was strong during our sample period, but not unusual by international or historical standards. Leverage amplifies these gains when the return on housing exceeds the mortgage rate.

Second, we find that homeownership causes an increase in consumption. Households "treated with homeownership" increase consumption by about 3,700 USD per year in the four years after treatment. This represents an increase of 18.5% relative to the consumption level in the four years before treatment. This response is much stronger than what is implied by a simple life-cycle

[1] The main policy instruments are the income tax deductibility of mortgage interest payments and property taxes, the tax exemption of the rental service flow from owned housing, (limited) tax exemption of capital gains on primary dwelling, support to the government-sponsored enterprizes Fannie Mae and Freddie Mac and to the FHA and its securitizer Ginnie Mae, first-time home buyer tax credits, etc. The IMF documents support for homeownership across the world (Westin et al., 2011; Cerutti, Dagher and Dell'Ariccia, 2015).

model, but smaller than the capital gains on housing so as to not eliminate the passive accumulation of wealth.

Third, homeownership gives access to housing collateral which allows households to better smooth consumption in the presence of binding borrowing constraints. Households in our sample have limited financial wealth before treatment, making them likely to be constrained. Because the treated were able to purchase their apartment at a discount to market values, the experiment bestowed substantial remaining debt capacity, after accounting for the mortgage to finance the apartment purchase. We show that households use this debt capacity to smooth consumption both intertemporally and across states of the world. Young homeowners bring forward future labor income by borrowing against their housing wealth, allowing them to consume much more than young renters. Moreover, we find that treated households increase borrowing against their home equity in the wake of an adverse income fluctuation. While renters' consumption falls by as much as the income decline, owners are able to fully offset the impact and keep consumption flat. Homeownership provides insurance.

Fourth, homeownership promotes mobility. The treated young households are substantially more likely to move as well as to move to a neighborhood with higher house prices. The capital gains that accrue in normal housing markets, as long as owners do not consume all of their home equity, create the downpayment needed to purchase the next home and climb the housing ladder.

Fifth, homeownership affects portfolio choice. We find that older homeowners increase the share of financial wealth invested in risky assets compared to older renters. A larger home equity position, smaller mortgage, larger debt capacity, and larger financial wealth position for the old relative to the young help explain this result. The evidence implies that homeowners gain further wealth-building advantages by exploiting the equity risk premium in their financial portfolio.

Our quasi-experiment takes place in Sweden between 1999 and 2007. In the early 2000s, tenants of municipally-owned apartment buildings in Stockholm were given the option to purchase their unit and become homeowners. Scores of such privatizations took place. Then, a change in the political environment resulted in the passage of a new law—the Stopplag—aimed at slowing down privatizations. The implementation of the Stopplag created random variation in the outcome of privatization attempts of otherwise similar buildings with similar tenants. This random variation is the source of our identification. We sort households in a treatment and control group and study their outcomes before, in the year of, and after the privatization decision in a standard difference-in-differences framework with household fixed effects.

We collect data on the identity of the tenants of all buildings affected by Stopplag, as well as the building and apartment characteristics of their dwellings. We merge this data with registry-based data on tenant demographics and comprehensive income and wealth data. A complete financial picture of the households' balance sheet emerges together with the components of their budget constraint (cash-flows) from four years before until four years after privatization. The income and wealth data enable us to construct a high-quality measure of consumption. Registry-based data on

housing market transactions allow us to construct a precise valuation of the housing units as well as neighborhood-level house prices. Our sample contains all 46 buildings affected by Stopplag. They collectively house about 5,000 individuals in 2,500 households, whom we track over time. We show that buildings and their tenants approved for privatization are similar to those that are denied. More importantly, the variables of interest follow parallel trends prior to the privatization decision.

Our experiment has several desirable features. First, privatizations were cash-flow neutral. The monthly building dues plus the mortgage payment post-privatization were about the same as the monthly rent tenants paid prior to privatization. Second, financial constraints played no role in the privatization decision. Since the privatizations were politically motivated, landlords did not set out to maximize profits. The building's asking price was set equal to the net-present value of rents minus operating expenses. Tenants could purchase their apartment at a price below the market value in the private market for co-op apartments. This discount allowed them to obtain mortgage financing for the entire amount of the purchase price.

The experiment's impact on housing wealth is more complicated. While households could purchase their apartment at a discount, they had to give up their rental contract in return. Due to universal rent regulation, there is a long queue for obtaining a rental unit in Stockholm. The wealth shock is the difference between the landlord's discount and the opportunity cost of giving up a desirable rental unit. We use a simple model to conceptualize and quantify the wealth shock. The average privatized co-op apartment has a market value of about 103,000 USD. The average purchase price upon privatization is about 50,000 USD. We calculate that the model-implied wealth shock is only about 10,800 USD. The 42,200 USD difference with the landlord discount of 53,000 USD reflects the opportunity cost of giving up the rental unit. Given that households borrow 48,200 USD to finance the purchase but the apartment is worth 103,000 USD, the experiment creates substantial remaining debt capacity (44,600 at a maximum LTV of 90%). In sum, the experiment creates a non-trivial increase in wealth but a much larger increase in housing collateral, which supports our finding of strong collateral effects.

The balance sheet situation of our treated households is similar to that of a typical homeowner, contributing to the external validity of our findings. Several years of house price appreciation coupled with modest mortgage amortization leave the typical homeowner with substantial, illiquid housing wealth. The home equity is available to borrow against in the event of an adverse income shock or for life-cycle reasons.

To asses the importance of the wealth shock for the consumption response, we exploit cross-sectional variation in the size of the wealth shock. The raw data for the treated households show a strongly negative relationship between the wealth shock and the consumption response. This is the opposite pattern as what the simple model (without borrowing constraints among other things) predicts. To address the possibility that the size of the wealth shock is endogenous conditional on treatment, we propose an instrumental variable approach. It instruments the wealth

shock with a hypothetical wealth shock based on neighborhood-level house prices. This hypothetical wealth shock is also available for households in the control group, allowing us to control for differences across households with different wealth shocks. The IV estimates an intercept effect, which captures the effects of homeownership on consumption for a household with a zero wealth shock, and a slope effect, which captures the marginal propensity to consume out of an additional dollar of wealth. Our identifying assumption is that the interaction term between a dummy variable that indicates successful privatization and the wealth shock is uncorrelated with other factors of consumption. We argue that our experiment is random, and if true, an interaction term of treatment will also be random. In particular, we note that the predicted wealth shock (as well as predicted square meter prices) are not different across the treatment and control groups. The intercept is large and precisely estimated. The slope has a negative point estimate and is imprecisely estimated. We cannot reject the null hypothesis that the slope is zero, but can rule out that the majority of the consumption response reflects a wealth effect.

Our paper contributes, first, to the empirical literature on the effects of homeownership on a range of household- and community-level outcomes. The earlier literature uses regression analysis, and includes control variables to deal with endogeneity concerns. This literature has been inconclusive on whether homeownership leads to more property maintenance, better outcomes for children, and more involvement with the local community.[2] A much smaller branch of this literature uses survey methods or quasi-experiments to study the causal effects of homeownership.[3] The few studies have small samples, focus mostly on non-economic outcome variables, and the survey data they use may not carry over to actual market behavior. Our quasi-experiment is much larger in scale, focuses on personal outcomes such as wealth building, consumption, and mobility, measures outcome variables using administrative data, and tracks households for a much longer period of time.

Second, we provide new evidence on the importance of the housing collateral effect.[4] Our results show powerful consumption smoothing benefits at the household level, accomplished by taking on additional debt. The random variation in housing wealth we observe as a result of the privatization experiment contributes a new source of identification. Our differential consumption responses for young households are consistent with Leth-Petersen (2010) who reports that the

[2]See e.g., Rossi-Hansberg, Sarte and Owens (2010), Green and White (1997), Rossi and Weber (1996), Haurin, Parcel and Haurin (2002), and DiPasquale and Glaeser (1999), respectively. Di Tella, Galiant and Schargrodsky (2007) find that ownership affects household beliefs in free market ideals.

[3]Shlay (1985, 1986) elicits the preferences for renting versus owning of a small sample of households in Syracuse, NY. Property characteristics, including tenure status, were assigned randomly to fictitious housing choices and respondents rank houses according to their desirability. The paper finds that tenure status does not affect the desirability of the property. Rohe and Stegman (1994) and Rohe and Basolo (1997) report on a quasi-experiment of low-income households who became homeowners—with the aid of deep subsidies provided by a foundation and the city of Baltimore—and a comparison group of low-income renters. Both groups filled out surveys concerning life satisfaction, self-esteem, and perceived control over their lives. After a year in their residences, owners were significantly different only on life satisfaction and showed positive, but not significant, effects on the other measures.

[4]The role of housing as a collateral asset was emphasized by Hurst and Stafford (2004), Lustig and Van Nieuwerburgh (2005, 2010), Leth-Petersen (2010), Mian and Sufi (2011), Kaplan, Mitman and Violante (2020), DeFusco (2018), and Cloyne et al. (2019).

young respond more to a deregulation of the credit market. The quasi-experimental setting in DeFusco (2018) shares with ours the feature that the shock to wealth is small relative to the shock to borrowing capacity. The magnitude of the average propensity to consume out of collateral in our experiment, 0.079, falls within the range of marginal propensities to borrow that he reports.

Third, our study contributes to the literature on the marginal propensity to consume out of housing wealth.[5] Standard MPC estimates exploit variation at the intensive margin (housing wealth). If there is also extensive margin variation (homeownership), as in our setting, the standard MPC regression is misspecified. Once the extensive-margin variation is considered, we find no intensive-margin effect out of the instrumented wealth shock or out of the immediate capital gain (i.e., the landlord's discount). The stronger consumption response for younger and poorer households is consistent with the model of Berger et al. (2018).

Fourth, our study relates to a literature that investigates consumption and labor supply responses to lottery wins. Fagereng, Holm Blomhoff and Natvik (2021) find that younger households and households with smaller windfall gains display higher MPCs. Cesarini et al. (2017) find a labor supply response to lottery gains. In our sample, the labor supply response is zero on average. We find a response among households who move and liquify their gain. Our study is complementary in that we study consumption and labor supply responses out of wealth shocks received in the form of illiquid housing wealth.

Fifth, we contribute to the literature on portfolio choice in the presence of housing.[6] The increase in the risky share among homeowners is consistent with the diversification argument in Yao and Zhang (2005). The differential responses for young versus old and stayers versus movers point to the importance of housing collateral, which acts like a reduction in effective risk aversion (Lustig and Van Nieuwerburgh, 2005), and the likelihood of moving, which ties in with the question whether the current home is a good hedge for future housing costs (Sinai and Souleles, 2005). Housing consumption commitments, as in Chetty, Sándor and Szeidl (2017), can help explain strong responses for older households and stayers.

Sixth, our work speaks to the literature that studies housing policy. One branch of this literature studies the distributional and general equilibrium effects of policies that subsidize homeownership.[7] Recently, research has shown that portfolio composition shapes the evolution of the wealth distribution (Kuhn, Schularick and Steins, 2020; Bach, Calvet and Sodini, 2017). Our results on the causal effects of homeownership on wealth-building underscore the importance of homeownership in this debate.

[5] See, e.g., Case, Quigley and Shiller (2005), Case, Quigley and Shiller (2013), Campbell and Cocco (2007), Carroll, Otsuka and Slacalek (2011), Mian, Rao and Sufi (2013), Berger et al. (2018), Paiella and Pistaferri (2017), and Aladangady (2017). In related work, Browning, Gørtz and Leth-Petersen (2013) impute consumption in Danish data and investigate the impact of shocks to house prices. Guren et al. (2020) provide a good summary of the literature.

[6] E.g., Yao and Zhang (2005), Cocco (2005), Vestman (2019), summarized in Davis and Van Nieuwerburgh (2015).

[7] See Poterba and Sinai (2008), Jeske, Krueger and Mitman (2013), Sommer and Sullivan (2013), and Elenev, Landvoigt and Van Nieuwerburgh (2016). Glaeser (2011) emphasizes that policies promoting homeownership distort the rental market especially in urban areas.

Another branch of this literature studies rent regulation.[8] Insights from this paper, which studies the conversion of rent-regulated into owner-occupied units, may carry over to similar privatization programs carried out in the United States, the United Kingdom, the Netherlands, and Germany in the 1980s and 1990s (Elsinga, Stephens and Knorr-Siedow, 2014), and in Hong Kong more recently. We are not aware of any other work that has studied these privatizations using micro data or has exploited a quasi-natural experiment like ours. More broadly, our results are relevant to understand the consequences of an expansion of rent regulation. In light of their housing affordability issues, many cities and states have recently embarked on such an expansion.

Finally, we explore to what extent the consumption response is driven by additional spending on renovation, furniture, and home appliances. Based on the available data from the tax registry and co-op annual reports, we find evidence for a small response in home improvements, but it cannot account for the bulk of the increase in household spending. Non-experimental evidence from the Swedish housing expenditure survey shows that buyers of new apartments increase their spending on maintenance, furniture, and appliances, somewhat, consistent with the U.S. evidence in Benmelech, Guren and Melzer (2021). But, the survey also shows a strong response in non-housing related spending categories. We conclude that the spending response upon homeownership is broad-based.

The rest of this paper is organized as follows. Section 2 discusses the privatization experiment and the institutional background. Section 3 provides a simple model that conceptualizes the experiment, the wealth shock, and its implications for consumption. Section 4 discusses the data. Section 5 presents the estimation methodology. Section 6 contains the main results. Section 7 discusses renovation expenditures and evidence on spending items from survey data. Section 8 concludes. The appendix contains further detail on the experiment, the model, data construction, and additional empirical results.

2 The Privatization Experiment

In this section, we describe the privatization quasi-experiment and the institutional background in which it took place.

2.1 The Swedish rental market

Starting in 1974, all rents are set in a negotiation process between landlord and tenant associations. All landlords, private and public, are bound by the resulting rent levels. The law states that the

[8] Autor, Palmer and Pathak (2014) studies the effect of the elimination of rent control on property values in Cambridge, MA. Diamond, McQuade and Qian (2019) study the effect of an expansion of rent control in San Francisco, CA on housing supply. Favilukis, Mabille and Van Nieuwerburgh (2021) study changes to rent regulation in New York in a dynamic spatial equilibrium model.

rent should be set based on the location and characteristics of the apartment. Rent-setting is highly granular: by narrow geographic area, by apartment type, and by quality of finish.

As large owners of housing units, public landlords owned by municipalities play a central role in the Swedish rental market. At the time of our quasi-experiment, rents set for the apartment stock of municipal landlords serve as the benchmark in all the rent negotiations. Given their role as a yardstick in the rent-setting process, it is deemed desirable that municipal landlords maintain a diverse housing stock, consisting of apartments in all geographies and of all sizes and qualities. Our quasi-experiment exploits the institutional role of the municipal landlords.

2.2 Co-op privatizations

Apartments make up 89% of the housing stock of the municipality of Stockholm. Apartment owners can be co-operatives (co-ops), municipal landlords, and private landlords. Each type owns approximately one third of the apartment stock. Co-ops are legal entities made up of individuals that collectively own their apartment building. The co-op shares of each member represent the ownership of its apartment unit. The three municipal landlords (Svenska Bostäder, Stockholmshem, and Familjebostäder) are owned and controlled by the municipality of Stockholm. Their role in the housing market has been an important political issue. Parties on the right of the political spectrum have strived for a smaller footprint, while the parties on the left have been in favor of the status quo.

The late 1980s and early 1990s saw some early experimentation with privatization of municipal landlord's properties into co-ops, but large-scale privatization started only after the September 1998 general election.[9] A center-right wing coalition took power in Stockholm and one of its political aims was to sell residential real estate owned by the municipal landlords. In total, 12,200 apartments were privatized between 1999 and 2004. Privatizations ramped up dramatically in the year 2000 and peaked in the year 2001. Appendix A describes the general steps of the privatization process and provides detailed statistics.

2.3 The Stopplag

In November 2001, the national Social Democratic-led coalition government proposed a law, known as *Stopplag*. This law was passed by the parliament in March 2002 and went into effect on April 1, 2002. The purpose of the law was to halt or at least slow down co-op privatizations. For political reasons, it went about this in a roundabout way.

Under Stopplag, municipal landlords became obliged to seek final approval to sell apartment buildings from an administrative body, the County Board. Prior to April 1, 2002, building own-

[9] By *co-op conversion* we mean the transfer of legal ownership of the property from a landlord (private or municipal) to the co-op association. By *privatization* we mean a co-op conversion that involves a municipal landlord.

ership would be transferred to the co-op after co-op and landlord had signed a sales contract, ratifying that the co-op had voted to accept the take-it-or-leave-it asking price and submitted a viable financial plan. After April 1, 2002, when Stopplag had come into effect, an additional County Board approval was necessary after the signing of the (provisional) sales contract. Stopplag instructed the County Board to determine if the sale would compromise the ability of the municipal landlords to serve as a benchmark in the rent-setting process. A similar law had not been in place before so there was no established practice for these judgements. Consequently, County Boards were given substantial latitude.

Stopplag resulted in a dramatic slowdown in the pace of privatizations of municipally-owned apartments in 2003 and 2004. A careful reading of all meeting minutes of the County Board in Stockholm shows that denials were based on the argument that there would not be enough housing units of a particular type in the neighborhood. Usually, the unit type at issue made up only a small part of the co-op's apartment mix (e.g., large studios, courtyard apartments, etc.). In one denial, two five-bed room apartments in large building complex were considered unique to the neighborhood, constituting ground (or excuse) for denial. In another denial, the studios in a building were considered unique to the neighborhood. In a third case, four very similar and geographically adjacent co-ops each consisted of a high-rise building with many units and a few low-rise buildings with few units each. The low-rise units were considered unique. The County Board denied the privatization of two of these four co-ops, deciding between them in random fashion.[10] Our identification strategy is based on the observation that virtually identical buildings were essentially randomly split into treatment (privatization) and control (denial) groups after Stopplag came into effect.[11]

We study the universe of co-ops affected by Stopplag. The 38 co-ops combine for 46 buildings. Of these, 13 co-ops with 13 buildings are approved for privatization; the treatment group. The other 25 co-ops with 33 buildings are denied by the County Board; the control group.[12] With one exception, all privatization processes were initiated prior to April 1, 2002. In most cases, the privatizations were initiated long before Stopplag was on the horizon. These co-ops had signed contracts with the landlords and would have privatized had it not been for the Stopplag. Prior to the County Board decisions, households in both treatment and control groups had equal and high expectations of becoming homeowners. The County Board decisions mostly took place between September 2002 and June 2004; 12 decisions were taken in 2002, 20 in 2003, 5 in 2004, and the last one in April 2005. For the 13 co-ops that were approved, the transfer of the property took place between November 2002 and September 2004.[13]

[10] Appendix A.4 studies this Akalla case in detail.

[11] The general election of September 2002 saw the Social Democrats hold on to their majority in parliament. They upheld the Stopplag in the face of opposition. The Stopplag was abolished in June 2007, after a liberal-conservative political coalition came to power in September 2006, both nationally and in Stockholm. They rekindled the co-op conversion program and a second privatization wave started after our sample ends.

[12] Of the 38 co-ops, 29 are owned by Svenska Bostäder, the other 9 by Stockholmshem. Familjebostäder signed no (provisional) sales contracts with co-ops after April 1, 2002.

[13] Figure A.1 of Appendix A.3 plots the 38 co-ops on a map of the municipality of Stockholm; with circles denoting

2.4 Wealth transfers in quasi-experiments on homeownership

Any reasonable quasi-experiment on homeownership must involve voluntary take-up of treatment. In our setting, treated households must be made better off for two reasons. First, after privatization of a building, a household can choose to remain a renter and continue to rent from the newly established co-op at their old rental rate. Second, households have access to the treatment outcome (homeownership) prior to (and in the absence of) treatment. Take-up of treatment thus necessarily involves both homeownership as well as a wealth transfer. Every policy that promotes homeownership is associated with a transfer. Mortgage interest deductibility, for example, redistributes wealth from all taxpayers to current homeowners. In the next section we embed the quasi-experiment into a model that provides a testable implication of the wealth effect.

3 Model

This section sets up a simple consumption-savings problem to conceptualize the quasi-experiment, the associated wealth transfer, and the consumption response. The derivations are in Appendix B.

3.1 Budget implications of privatization

The landlord's perspective Prior to privatization, the landlord receives an annual rent ω_t and incurs an annual maintenance cost ϕ_t for the average apartment unit. Let the cost of capital of the landlord equal r, where $R = (1 + r)$. Let P_0 be the apartment's value on the private market for co-op shares in year $t = 0$. The political directive to the municipal landlords was to set the asking price for the building, $(1 - \tau)P_0$, such that the landlord breaks even:

$$(1 - \tau)P_0 = \sum_{t=0}^{\infty} \omega_t R^{-t} - \sum_{t=0}^{\infty} \phi_t R^{-t} \tag{1}$$

The parameter $\tau > 0$ is the landlord's fractional discount offered to co-ops.

The household's perspective Consider a household that lives (in Stockholm) from $t = 0$ to $t = T$. The household can save and borrow in a financial asset a_t with rate of return r, equal to the landlord's cost of capital. Every period the household receives income y_t and consumes non-housing consumption c_t. Let initial financial wealth be $\hat{a}$.

If the household is denied privatization at the start of year 0 and remains a renter until T, its per-period budget constraint is $c_t^r + \omega_t + a_t = y_t + \hat{a}$ for all $t = 0, \cdots, T$. Without loss of generality,

approvals and crosses denials. It also plots a shaded circle of five kilometer distance from the Royal Castle. We denote this as distance to center and use this variable to construct some of our other variables.

we can choose a consumption path for the renter such that financial wealth at the end of period T is $a_T = 0$.

If instead the household is approved for privatization in year 0 and becomes a homeowner, its initial budget constraint is $c_0^o + \phi_0 + a_0 + (1-\tau)P_0 = y_0 + \hat{a}$, where the annual maintenance is the same as it was for the landlord. The home purchase is financed with a mortgage with interest rate r. If the mortgage interest rate is r, the mortgage debt can be folded into a and the fraction of the house that is financed with debt is irrelevant.[14] The budget constraint from period 1 onwards reads $c_t^o + \phi_t + a_t = y_t + + a_{t-1}R$ for all $t = 1, \cdots, T-1$. At the end of period T, the household sells the house for $p_{T+1}R^{-1}$ which enters T budget constraint $c_T^o + \phi_T + a_T = y_T + a_{T-1}R + p_{T+1}R^{-1}$. Just as for the renter, we choose a consumption path for the owner such that end-of-period net financial wealth $a_T = 0$ (after the home sale and repayment of debt).

3.2 The household's wealth shock and spending response

The wealth shock, W_0, is the difference between the life-time budget constraint of a household that chooses to own and the same household that chooses to rent, measured in present value. In other words, we assume that by $t = T$, a household that undergoes "treatment" has consumed the additional wealth. By taking the difference of the renter's and homeowner's consolidating budget constraints[15] and substituting in the pricing policy, (1), it can be shown that:

$$W_0 = \tau P_{T+1} R^{-(T+1)}, \tag{2}$$

$$= \tau P_0 \left(\frac{R_h}{R}\right)^{T+1}. \tag{3}$$

We refer to τP_0 as the landlord's discount. It measures how much the household would gain if it bought the apartment at the conversion price $(1-\tau)P_0$ and immediately sold it at the prevailing market price P_0. The variable R_h denotes house price appreciation and the ratio $\frac{R_h}{R}$ should be thought of as the net rental yield. It captures the difference between house price appreciation and the return on financial wealth.[16] Equation (3) makes clear that the landlord's discount overstates the wealth shock because the net rental yield is strictly smaller than 1. The downward adjustment $\left(\frac{R_h}{R}\right)^{T+1}$ reflects the fact that the household is entitled to regulated rents for the next T years.[17]

[14] For simplicity, we abstract from the co-op and its financing choices. In reality both the co-op and the household obtain mortgages. The co-op fee includes not only the maintenance but the debt service on the co-op mortgage. As long as the co-op and the household borrow at the same rate, the mortgage debt split between co-op and co-op member is irrelevant. We discuss the privatization process and the co-op's role in Appendix A. Appendix G.3 shows that rents before approval and interest expenses plus co-ops' fees after approval are approximately equal.

[15] Consolidating budget constraints yields $\sum_{t=0}^{T} c_t^r R^{-t} + \sum_{t=0}^{T} \omega_t R^{-t} = \sum_{t=0}^{T} y_t R^{-t} + \hat{a}$ for renters and $\sum_{t=0}^{T} c_t^o R^{-t} + \sum_{t=0}^{T} \phi_t R^{-t} = \sum_{t=0}^{T} y_t R^{-t} + \hat{a} + P_{T+1} R^{-T-1} - (1-\tau)P_0$ for owners.

[16] Appendix B.5 shows that if owners' and renters' first-order conditions are satisfied, and rent-to-price and maintenance-to-price ratios are constant at ω and ϕ, respectively, then $\frac{R^h}{R} = 1 - (\omega - \phi)$.

[17] As long as there is a cost to returning to the regulated rent system, the relevant horizon is strictly greater than the time of sale of the privatized apartment. A household that privatizes and later sells and wants to re-enter the rental

If households desire a flat consumption profile before as well as after conversion, the spending response, $c^o - c^r$, upon privatization depends on four factors:[18]

$$c^o - c^r = \left(\frac{r}{1+r}\right)\left(1 - \frac{1}{(1+r)^{T+1}}\right)^{-1} \tau P_0 \left(\frac{R_h}{R}\right)^{T+1} = \left(\frac{r}{1+r}\right)\widetilde{W}. \tag{4}$$

The first factor is a perpetuity factor. The second factor is an adjustment to account for the finite horizon T. The product of these two factors is a standard annuity factor. The product of the third and fourth factor is W_0. Going forward, we define the product of the second, third, and fourth factors as the wealth shock $\widetilde{W}$. It is an important variable in our empirical analysis.[19] To compute $\widetilde{W}$ we rely on parameter values for R and $\frac{R_h}{R}$. We set R=1.07 and $R^h = 1.02$. These values are broadly consistent with the post-1950s and post-1980s equity return and net rental yield values for Sweden (Jordà et al., 2019).[20] We set T equal to 85 minus the age of the oldest household member if the age is less than 85. We describe measurement of τ and P_0 in the following section.

4 Data

What makes our paper's data unique is our ability to match the tenants in co-op privatizations to their demographic and financial characteristics and the characteristics of the homes they live in. Our data comes from five sources. First, we obtain Stockholm County Board meeting minutes and Stopplag decisions for each co-op.

Second, we use the archives of the municipal landlords in Stockholm. We obtain the entire correspondence between the co-op and the landlord associated with each privatization attempt. For each co-op, we collect information on exact geographic location and important dates in the privatization process (first contact between the parties, the landlord's asking price and other details of the sales contract, and transfer date of the building if approved by the County Board). At our request, landlords sent their database of tenants for our period of study directly to Statistics Sweden. This includes the personal identification number of the tenant and information about the rent and the size (square meters and number of rooms) of each tenant's apartment.

The third source is household-level data from tax registries obtained via Statistics Sweden. Using the personal identification number, we link the tenant data to Statistics Sweden's demographic, income, and wealth data. We collect data on all individuals that lived in these buildings at any point between 1999 and 2013. The wealth data are so detailed that, when combined with

market needs to apply and start at the beginning of the rental housing queue. The relevant horizon T is the time of exit from the Stockholm housing market, which is greater than the time of sale.

[18] The standard assumptions for this condition to be met is decreasing marginal utility in consumption and that the subjective discount factor of households is equal to R^{-1}.

[19] Notice that the wealth shock is adjusted for differences in time horizon T across households. Figure A.3 in Appendix B illustrates the model-implied spending responses for different values of τP_0 and T.

[20] Our parameter values are also consistent with Statistics Sweden who report a long-run average real house price growth (1981–2008) in Sweden of 2.5 percent and Global Property Guide who report an annual price-rent ratio of 20.

asset-level return data, we can construct the rate of return on a household's portfolio (Calvet, Campbell and Sodini, 2007). Data on after-tax and transfer disposable income, changes in debt, changes in housing wealth, and changes in financial wealth allows us to compute a high-quality registry-based measure of consumption and savings:

$$Cons = Income - Savings = Income + dDebt - dHousing - dFin \tag{5}$$

Definitions of the terms of the budget constraint are detailed in Appendix C. Consumption measures total spending at annual frequency. It includes housing consumption, measured as rent for renters and maintenance plus debt service for owners. Our consumption measure extends Koijen, Van Nieuwerburgh and Vestman (2014) in several important ways. The method newly measures apartment wealth and changes in tenure status over time. A crucial issue for our purposes is accurate measurement of the apartments' value on the private co-op market. We do not rely on Statistics Sweden's assignment of co-op apartment ownership nor valuation of co-op apartments, which are known to be noisy. Rather, we use exact apartment transaction prices, obtained from tax registries filed after the sale of an apartment. This allows us to compute square meter prices for all co-op apartments in our sample and combine it with square meter information from the tenant lists. We also compute house prices at the fine geographic neighborhood level for use in the IV analysis described below. Appendix D provides details. Because the wealth data are only available until 2007, our analysis spans the nine-year period from 1999 until 2007. All nominal variables are deflated by the Swedish consumer price index with base year 2007.

Fourth, we hand-collect data on tenants that live in successfully privatized buildings but do not participate in ownership themselves. Appendix E describes our method for identifying these residual tenants.[21]

Fifth, we digitalize co-op annual reports. Co-op revenues are based on its members' monthly co-op fees. From this data, we can infer the monthly housing cost for households after treatment. From the co-op's book equity we can also verify the landlord's asking price; details are in Appendix G.

4.1 Event time

We label event time as relative year $RY(k)$. For approved co-ops, $k = 0$ is the year in which the property transfer takes place. For denied co-ops, we typically set $k = 0$ to the year of the County Board decision (15 out of the 25 denied co-ops). In cases where that decision takes place very late in the year (end of November to end of December, 10 remaining cases), the calendar year after

[21] Tenants who live in co-ops that successfully privatize are allowed to remain as renters, at their old rental rate which they now pay to the co-op association. For eight of the thirteen treated co-ops, we find information about the number of residual tenants in annual co-op reports. In addition, four co-ops sent social security numbers of their residual tenants to Statistics Sweden for matching. This allows us to identify forty residual tenants among the treated households, about 7 percent of the treatment group.

the denial is defined to be relative year 0 since that year lines up better with the consumption measurement. The years after the decision are indicated by $k = 1, ..., 4$.[22]

The household formation year $k = -1$ is the year in which we form our sample of tenants.[23] It is the last year in which there is still substantial uncertainty over the outcome of the approval process. Our data set starts from all individuals who live in the co-ops of interest in the household formation year. We form households from the individual data and aggregate across all the household members. This tenant sample contains the set of individuals that we track consistently before and after the privatization decision. For simplicity we define the household head to be the oldest member of the household.

We track changes in household composition. We focus on the sample of household-year observations where the adult composition is the same as in the household formation year. The sample is an unbalanced panel data set. It has 1,911 households and 12,859 household-year observations; 567 households and 3,769 observations are for households in the treatment group (successful privatizations) while 1,344 households and 9,090 observations are for in the control group (failed attempts). Appendix F describes the details of household formation.

4.2 Summary statistics

Table 1 reports summary statistics, measured in relative year -1. The full sample is reported in column 1, the treatment group in column 2, and the control group in column 3. Column 4 reports p-values of a formal balance test, which does not reject the null of equal means for the treated and control households at the 5% level for any of the covariates. In addition, we show below that the two groups display parallel trends before treatment.

The average household head is 44 years old; 44 percent of household heads have at most a high school degree. One third of the households have a partner (married or cohabiting) and the average number of workers in a household is 1.36. The treated are more likely to be in a partnership, and correspondingly have a higher number of workers. Therefore we express all amounts as per adult equivalents.[24] The likelihood that at least one household member is unemployed for some time during the household formation year is 16 percent for the control and 14 percent for the treatment group. Households in both groups have a low moving rate of 1 percent just before treatment, most

[22] Our panel is unbalanced. For the co-ops with decision in 2003, we see all nine years from 1999 until 2007. For the co-ops with decision in 2002, we have data on five years after treatment and $k = 4$ refers to the combined years 2006 and 2007; we do not have $k = -4$. For the co-ops with decision in 2004, $k = -4$ refers to the combination of 1999 and 2000 and we do not have $k = 4$.

[23] For four co-ops we make an exception to this rule. In these cases, the privatizations were approved in late 2002 or early 2003, but the actual transfer of the building does not take place until 2004. Forming households in 2003 rather than 2002 would open us up to the criticism that households already knew they were approved in 2003 and were already making economic decisions with knowledge of the approval decision.

[24] We use the OECD adult equivalence scale: 1+ (Adults-1)·0.7 + (Children)·0.5. In relative year -1 the average number of adult equivalents is 1.71 (all), 1.79 (treated) and 1.67 (control). All values are reported in SEK 1000s. The exchange rate is approximately 8.0 SEK per USD over our sample period.

likely reflecting anticipation of privatization.

Turning to balance sheet information in Panel B, only four percent of households own any real estate (co-op shares or single-family houses including vacation homes or cabins) prior to treatment so average housing wealth is small (SEK 25,900). On average, households have SEK 85,400 in financial wealth.[25] Total debt of households, including student loans, equals SEK 92,600. Since there are few homeowners, debt mainly reflects student loans and unsecured debt rather than mortgages. At SEK 63,700 average net worth is low.

We define a buffer variable, which captures the household's ability to insure against idiosyncratic shocks. The buffer is the sum of financial wealth and remaining debt capacity, where the latter is debt capacity minus current debt outstanding. Debt capacity depends on a debt-to-income ratio constraint, as well as on a loan-to-value ratio for collateralized debt. Appendix I provides the details.[26] The definition and meaning of the buffer is similar to the one in Kaplan, Violante and Weidner (2014). The average buffer is SEK 412,300 in the household formation year.

Panel B also reports two portfolio choice variables. The risky share is defined as the share of financial wealth invested in stocks, equity mutual funds and risky bonds. The unconditional risky share reflects both the extensive margin (participation) and the intensive margin, and is 19%. The conditional risky share, conditional on participation, is 34%, and reflects that participation in risky asset markets is slightly above 55%.

Panel C shows consumption (from equation (5)) and income. Average consumption is SEK 145,300. Income includes gross labor income plus unemployment benefits plus pension income plus transfers minus taxes. It excludes capital incomes. The typical household has an income of SEK 161,200. Approved and denied households are well balanced.

Panel D reports apartment characteristics. Households live on average 7.3 kilometers from the city center. Treated households live, on average, only 0.88 kilometers farther. Apartments have an average size of 74 square meters (about 800 square feet). Households pay SEK 41,500 in rent every year. Consistent with the U.S. evidence, this represents about 28% of total spending. 74% of tenants vote in favor of a privatization, with less than one percentage point difference between treated and control.

[25] We do not count financial wealth tied to pension plans, which remains inaccessible at least until age 60.

[26] For our sample, a 10% downpayment requirement is common for regular housing market transactions, and the debt-to-income constraint limits debt to a maximum of two years of income. The latter is the main determinant of the buffer before treatment.

Table 1: Averages Before Treatment

	All	Treated	Control	p-value
Panel A: Sociodemographics				
Age	44.28	45.06	43.95	0.24
High school	0.44	0.43	0.44	0.65
Post high school	0.44	0.48	0.42	0.17
Partner	0.34	0.40	0.31	0.09
Number of workers per hh	1.36	1.44	1.32	0.09
Unemployed	0.15	0.14	0.16	0.56
Income shock 25% (Z_{it})	0.10	0.09	0.10	0.68
Move	0.01	0.01	0.01	0.80
Panel B: Balancesheets				
Homeowner (D(Own)$_i$)	0.04	0.04	0.04	0.56
Housing wealth	25.85	29.03	24.48	0.70
Financial wealth	85.43	86.28	85.06	0.93
Debt	92.58	95.48	91.34	0.82
Net worth	63.65	78.35	57.35	0.40
Buffer	412.26	424.46	407.03	0.62
Risky share (uncond.)	0.19	0.21	0.19	0.29
Risky share (cond.)	0.34	0.35	0.34	0.59
Panel C: Cashflows				
Income	161.24	161.51	161.13	0.97
Consumption	145.25	143.17	146.14	0.79
Panel D: Apartments				
Distance to center (km)	7.27	7.89	7.01	0.66
Area (m^2)	74.04	72.40	74.75	0.58
Rent per year	41.54	38.80	42.71	0.09
Vote share	0.74	0.73	0.74	0.83
Panel E: Approved coop				
Conversion price per m^2 (p_0^c)		8.67		
Market price per m^2 (p_0)		18.21		
Discount fraction (τ)		0.54		
Wealth shock ($\widetilde{W}$)		85.16		
Apartment value (HW_0)		813.14		
Panel F: Neighborhoods				
Predicted conv. price per m^2 ($p_0^{c,nbd}$)	9.57	9.08	9.78	0.66
Predicted market price per m^2 (p_0^{nbd})	19.33	18.79	19.57	0.81
Predicted wealth shock ($\widetilde{W}^{nbd}$)	87.93	86.06	88.73	0.90
Predicted apartment value (HW_0^{nbd})	954.98	866.99	992.67	0.48
Number of households	1764	529	1235	

Notes: The table presents averages of variables for all sample households (first columns) and separately for households in successful privatization attempts (treated; second column) and failed attempts (control; third column) in relative year $k = -1$. The fourth column reports balance test p-values based on standard errors clustered at co-op level. Age and education refer to the highest age or education level among the household members. Partner refers to households with two adults who are married, have a civil partnership, or at least one child together. Unemployed refers to a dummy variable that indicates if any unemployment insurance was received by any household member during the year. Risky share (cond.) refers to the share of risky assets out of financial wealth conditional on stock market participation in the year of household formation. Panel E excludes residual tenants. The construction of the neighborhood variables in Panel F is described in Appendix H. With the exception of variables per individual or in ratios, all variables are denominated in SEK 1,000 per adult equivalent according to the OECD formula and deflated by the consumer price index. Table A.14 reports the same statistics for sub-groups of the sample.

Panel E reports statistics that are related to privatization, which exist only for the approved co-ops. We measure the conversion price, p_0^c, from tax records of households that sell their co-

ops shares subsequent to completion of the transaction between the landlord and the co-op. The average conversion price per square meter is SEK 8,700.[27] In the tax records of sellers, we also find the apartment price on the private co-op market, p_0. In relative year 0 ($k = 0$), the average price per square meter is SEK 18,200.[28] Based on the conversion price and market price we construct the fractional discount, τ, which on average is 0.54 in our sample (average ratio; ratio of averages is 0.51). We construct the wealth shock $\widetilde{W}$ by applying equation (4). It has a mean of SEK 85,200 and a median of 47,180.[29] We can use the market price and apartment size to construct the market value of the apartment in relative year 0, which is SEK 813,100 on average. We denote this variable by HW_0 – it is the model equivalent to P_0. The average discount for each household, τHW_0, is SEK 412,200 (per adult equivalent, excluding residual tenants). This variables is closely related to the literature's notion of a housing wealth shock.[30] We note that the wealth shock $\widetilde{W}$, which takes into account the lost value of the rental contract, on average is five times smaller than the landlord's discount.

Panel F reports four variables that we construct for both approved and denied co-ops. These variables are important inputs in the IV analysis discussed in the next section. First, we predict conversion prices per square meter using distance to center in a regression. The average value for treated co-ops is SEK 9,080 per square meter which can be compared to the actual of 8,720 SEK.[31] We also predict the market price per square meter using data on each co-op apartment transaction in a granular geographic neighborhood surrounding each building in our sample, excluding the building itself.[32] The average predicted market price for treated co-ops is SEK 18,790 per square meter, which can be compared to the actual average of SEK 18,280 in Panel E. These neighborhood-level prices, together with information about square meters, enable us to employ equation (4) once more to construct a predicted wealth shock for households in approved and denied co-ops, denoted by $\widetilde{W}^{\text{nbd}}$. The correlation between $\widetilde{W}$ and $\widetilde{W}^{\text{nbd}}$ is 0.925. We can also use neighborhood prices to construct the predicted market value of the apartment on the co-op market. We denote it by HW_0^{nbd} and its average value in the treatment group is SEK 866,990. The correlation between HW_0 and HW_0^{nbd} in the treatment group is 0.934.

[27] The conversion price is not the same as the landlord's asking price. The conversion price is affected by co-op level mortgage debt, the number of vacant apartments at the time of the transaction, and the number of households that choose to not buy their co-op share. Appendix E discusses how we identify these residual tenants. Appendix G.1 shows that correlation between the conversion price and the landlord's asking price on a book equity basis is 0.989.

[28] As long as at least one treated household in the building sells within the year, we have a market price. We apply the per square foot price of that transaction to the square footage of all apartments in the building. See Appendices D and H for details.

[29] $\widetilde{W}$ has the following cross-sectional distribution in the treatment group: 0.00 (P5), 25.72 (P25), 47.18 (P50), 99.80 (P75), 278.69 (P95).

[30] The model-equivalent to τHW_0 is τP_0.

[31] The small difference reflects a high R-squared of 0.77. See Appendix H.4 for details.

[32] We exclude all other transactions that are not at arms' length. See Appendix H.3 for details.

4.3 External validity

Appendix Table A.13 shows that our Stopplag sample is representative of the broader population of Stockholm renters. Furthermore, Appendix Figure A.9 shows that the distribution of disposable income, defined consistently with equation (5), of our sample households is similar to the Stockholm-wide distribution of renters.

Appendix J compares our consumption measurement to that of new apartment purchasers in the Swedish household budget survey and finds a similar consumption distribution.

Furthermore, Appendix K argues that the context in which our quasi-experiment takes place is relevant for housing markets in other countries and at different times. Homeownership rates and house price growth in Sweden were average among OECD countries in our time of study, and similar to the strong house price appreciation most OECD countries have seen in the last decade from 2012 until 2021. While our sample households had more home equity than other new homeowners by virtue of the experiment, they are actually more representative of the average homeowner. For example, U.S. homeowners collectively own about two-thirds of housing wealth. The Swedish mortgage market functions similarly to that in other European countries in terms of mortgage product composition; underwriting standards and amortization were not unusual at the time of our study. Rental markets are also regulated in most other European countries, and rent regulation is seeing a revival in the U.S. since 2019. Similar privatization experiments took place around the world since the 1980s.[33] Finally, income inequality after taxes and transfers is not unusually low among European countries, nor is the difference between inequality after and before taxes and transfers.

5 Empirical Strategy

To establish our baseline results, we estimate a standard (dynamic) diff-in-diff regression. To get at the economic mechanisms at play, we consider heterogeneous treatment effects as well as an instrumental variables approach.

[33] In particular, there were similar privatizations in the U.K., in Eastern Europe, in Hong Kong and in China. Germany chose to privatize municipally owned apartments by selling large portfolios of rental properties to institutional investors. Closely related are Tenant Opportunity to Purchase Acts which give tenants the right to purchase their property using a coop structure should it be up for sale.

5.1 Empirical specifications - baseline results

For our baseline results, we estimate the following reduced-form dynamic difference-in-difference regression:

$$y_{it} = \sum_{k \in K} \delta^k \underbrace{RY_{it}(k) \times \text{Priv}_i}_{\text{Exogenous}} + \phi \mathbf{X_{it}} + \psi_{\mathbf{t}} + \omega_{\mathbf{i}} + \nu_{\mathbf{it}}, \tag{6}$$

where Priv_i indicates treatment or denial (0), δ^k are the coefficients of interest, $\mathbf{X_{it}}$ includes baseline relative year effects $RY_{it}(k), \forall k \in K$, ψ_t are time fixed effects, and ω_i are household fixed effects. The simple treatment effect term Priv_i is absorbed by the household fixed effects. Our dynamic diff-in-diff estimators either capture relative years $K = \{-4, -3, -2, 0, 1, 2, 3, 4\}$ or a more parsimonious specification $K = \{\text{Pre}, 0, \text{Post}\}$ which combines the years $k < -1$ in Pre and $k > 0$ in $Post$. The results are measured relative to the household formation $k = -1$ which the summation omits. Throughout, we cluster standard errors at the co-op level. Focusing on a dynamic diff-in-diff estimation has two advantages. First, we can immediately check whether the identifying assumption of no differences in pre-trends between treatment and control group is satisfied. Second, we expect interesting dynamics in the treatment effects. In relative year 0, households from the treatment group purchase their apartment and take on large amounts of debt. We expect the magnitudes of these effects to be larger than in subsequent years. Thus, it is appropriate to separate the effects in relative year $k = 0$ and relative years $k > 0$.

5.2 Empirical specifications - economic mechanism

Equation (6) establishes the dynamic impact of the quasi-experiment. In the next step, we are interested in uncovering the different economic channels behind the responses, in particular the collateral channel and the wealth shock channel.

Heterogenous Treatment Effects To understand which channels are at play, we estimate (fully saturated) interaction term variants of equation (6):

$$\begin{aligned} y_{it} &= \sum_{k \in K} \delta^k RY_{it}(k) \times \text{Priv}_i + \phi \mathbf{X_{it}} + \psi_{\mathbf{t}} + \omega_{\mathbf{i}} \\ &+ D_i \times \left\{ \sum_{k \in K} \tilde{\delta}^k RY_{it}(k) \times \text{Priv}_i + \tilde{\phi} \mathbf{X_{it}} + \psi_{\mathbf{t}} + \omega_{\mathbf{i}} \right\} + \nu_{it}. \end{aligned} \tag{7}$$

In order to investigate whether young or old are differentially affected by our natural experiment, we set $D_i = D_i(Old)$ where $D_i(Old)$ is an indicator variable which is one for households with head above age 40, and zero otherwise. The estimates of δ^k for $k \geq 0$ denote the treatment effect for the young, while $\tilde{\delta}^k$ denotes the differential impact of the experiment on the old relative to

the young; $\delta^k + \tilde{\delta}^k$ is thus the overall effect on the old. This sample split is useful to study the role of whether housing collateral helps relax borrowing constraints that prevent the young from smoothing consumption intertemporally.

Wealth Shock Since our treatment implies a joint shock to homeownership and wealth, we are interested in understanding the importance of the wealth shock for our results. At a conceptual level, an outcome variable y_i for household i could be associated with homeownership, own_i, and/or the wealth shock $\widetilde{W}_i$:

$$y_i = \beta_1 \text{own}_i + \beta_2 \widetilde{W}_i + u_i, \tag{8}$$

where u_i is an error term. The coefficients of interest are β_1, which denotes the effect of homeownership on y_i, which is independent of $\widetilde{W}_i$, and β_2 which denotes the effect of the wealth shock on y_i, independent of homeownership. The simple model of Section 3 predicts that $\hat{\beta}_1 = 0$ and $\hat{\beta}_2 = r/(1+r)$ when consumption is the outcome variable. The literature that focuses on homeowners' economic responses to housing wealth fluctuations omits the own_i term.

One could estimate (8) for treated households. While homeownership is exogenous by virtue of treatment, one concern may be that households with a large wealth shock are systematically different from households with a small wealth shock based on observables (location, age, income, etc.) and unobservables, and possibly be subject to different time trends. These differences may impact the consumption response to the wealth shock and its dynamics for reasons unrelated to treatment. To address this concern, we would like to compare high- and low-wealth shock households to similar households in the control group. But by the nature of the experiment, the wealth shock $\widetilde{W}_i$ is zero for households in the control group.

Instrumental Variables Approach Our solution is to construct a hypothetical wealth shock for both treated and control households based on neighborhood-level prices. As explained in Section 4, we first construct neighborhood housing wealth $HW_{0,i}^{nbd}$ as the product of housing unit size (m_i^2 in square meters) and the neighborhood house price level in the year of treatment (or denial) p_0^{nbd}. We then compute the wealth shock $\widetilde{W}_i^{nbd}$ from (3) using neighborhood-level housing wealth.

With $\widetilde{W}_i^{nbd}$ in hand for both treatment and control groups, a first (reduced-form) way to implement (8) is to estimate equation (7), setting $D_i = \widetilde{W}_i^{nbd}$. The coefficient δ^k is then an intercept effect, measuring the effect of homeownership at a zero wealth shock, while $\tilde{\delta}^k$ captures the marginal impact of additional wealth. Since it includes interaction effects of the wealth shock with time fixed effects, this specification compares treated and control households with the same hypothetical wealth shock and allows for time trends (macro-economic effects) that differ by the level of wealth shock.

A second way to implement (8) is to estimate an instrumental variable (IV) regression. The first stage of the IV estimates equation (7) where the dependent variable y_{it} equals homeownership in

relative year 0 ($own_i \times RY_{it}(0)$), homeownership in the post years ($own_i \times RY_{it}(Post)$), the wealth shock in relative year 0 ($\widetilde{W}_i \times RY_{it}(0)$), or the wealth shock in the post years ($\widetilde{W}_i \times RY_{it}(Post)$). In each of the four first-stage regressions, the instruments are $Priv_i \times RY_{it}(0)$, $Priv_i \times RY_{it}(Post)$, $\widetilde{W}_i^{nbd} \times Priv_i \times RY_{it}(0)$, and $\widetilde{W}_i^{nbd} \times Priv_i \times RY_{it}(0)$. The first-stage regressions again control for the hypothetical wealth shock $\widetilde{W}_i^{nbd}$ and its interactions with relative year.

The instrumented values of the interaction terms are then used in the second-stage regression:

$$y_{it} = \sum_{k \in K} \alpha^k \times \widehat{\text{own}_i \times RY_{it}(k)} + \sum_{k \in K} \tilde{\alpha}^k \times \widehat{\widetilde{W}_i \times RY_{it}(k)} + \phi \mathbf{X_{it}} + \psi_{\mathbf{t}} + \omega_{\mathbf{i}} + \widetilde{\mathbf{W}}_{\mathbf{i}}^{\mathbf{nbd}} \times \left\{ \tilde{\phi} \mathbf{X_{it}} + \psi_{\mathbf{t}} + \omega_{\mathbf{i}} \right\} + \nu_{\mathbf{it}}. \quad (9)$$

The coefficient α^k measures the effect of homeownership on y_{it} in relative year k, and the coefficient $\tilde{\alpha}^k$ measures the relative year k effect of the wealth shock on y_{it}. The IV approach thus allows us to track the (dynamic) impact of homeownership and the wealth shock, while accounting for endogeneity concerns with both homeownership and the size of the wealth shock conditional on homeownership. The coefficient α^k in (9) is a treatment-of-the-treated effect while δ^k in (6) or (7) is an intention-to-treat effect.

6 Main results

6.1 Homeownership and Wealth Accumulation

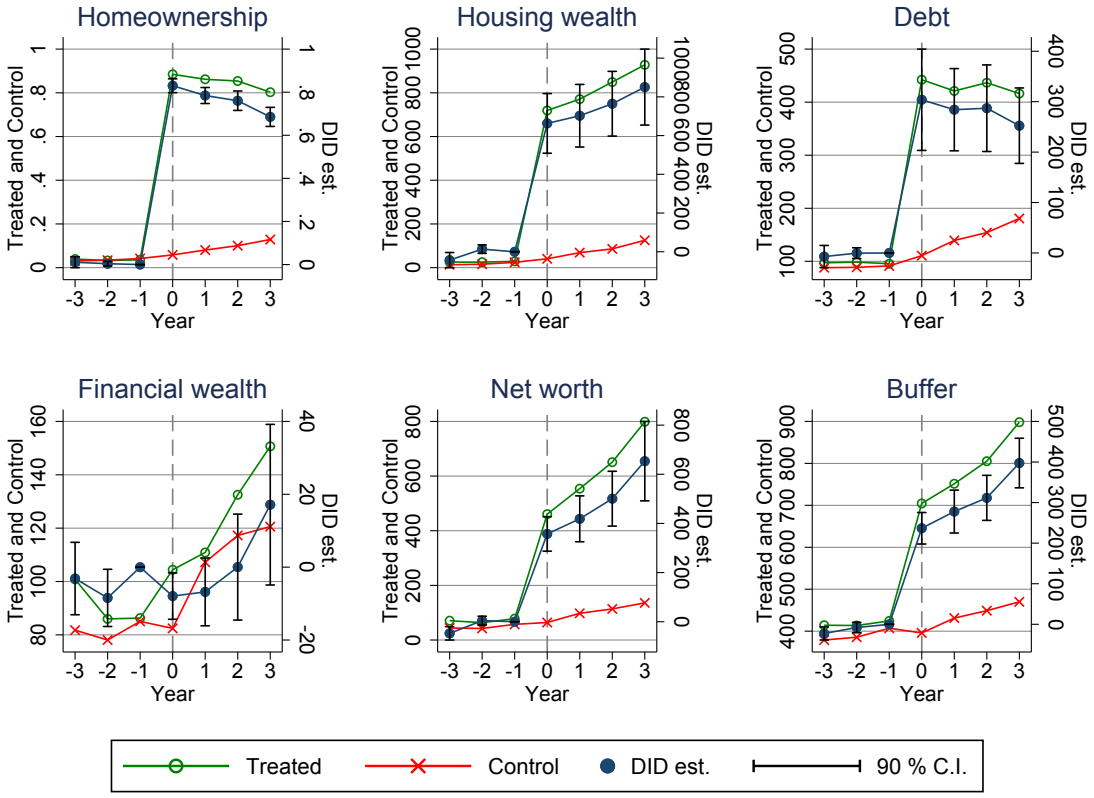
Figure 1 reports responses to homeownership, housing wealth, and net worth upon treatment. The raw averages for the treatment and control groups are plotted against the left axes while the difference-in-difference estimates are plotted against the right axes. Homeownership (own_{ik}) increases dramatically among the treated from a few percentage points to 88 percentage points in relative year 0. Homeownership declines in subsequent years as some treated households sell their apartments and revert back to renting. Some households in the control group, who were denied, achieve homeownership on their own in the years after the decision. The difference-in-difference estimate reflects the net effect.

Homeownership results in substantial wealth accumulation. The top middle panel reports housing wealth measured at market prices. Even though the treated on average borrow the entire purchase price of their apartment, housing wealth jumps in the year of privatization because the landlord's asking price is below the market price. The increase in home equity boosts net worth in the year of treatment. We recall that the actual wealth shock $\widetilde{W}$ is much smaller than the effect on net worth measured at market values.

The continued wealth accumulation in the years after arises because homeowners have a levered position in housing and house price growth is positive. As long as households do not con-

sume too much out of these gains in housing wealth, either by reducing financial wealth or by borrowing more, their net worth increases. Passive homeownership financed with debt earns high returns in a rising housing market. A unique advantage of our empirical setting is that we are comparing similar households who were randomly allocated into homeownership. We do not suffer from the traditional selection issues into homeownership that obscure the true relationship between homeownership, consumption, and wealth accumulation. As argued, our sample period saw strong but not unusual house price appreciation. This suggests that the wealth building benefits from homeownership we find apply broadly.

Figure 1: Effects on Homeownership and Balance Sheets



Notes: The figure depicts the effects on balance sheets for the treatment and control groups (left vertical axes) and difference-in-difference estimates (right vertical axes). The difference-in-difference estimates are based on the regression specification in equation (6). Homeownership is defined as ownership of co-op apartments or single-family houses. Housing wealth refers to the market value of apartments and single-family houses. Net worth is the sum of real estate, apartments and financial wealth minus debt. Buffer is the sum of financial wealth and remaining debt capacity. All values are in SEK 1,000 and scaled by adult equivalents. Confidence intervals based on clustering at the co-op level.

Table 2: Consumption and Its Components

	(1) Log cons.	(2) Cons.	(3) Income	(4) dHouse	(5) dDebt	(6) dFin
Priv.$_i$ × RY$_{it}$(Pre)	0.032	2.431	-1.425	-6.661	-2.391	0.369
	(0.04)	(5.40)	(2.39)	(4.43)	(6.11)	(6.07)
Priv.$_i$ × RY$_{it}$(0)	0.078**	14.462**	2.281	319.737***	321.203***	-10.738**
	(0.04)	(5.23)	(1.64)	(57.68)	(61.78)	(4.77)
Priv.$_i$ × RY$_{it}$(Post)	0.185***	29.680***	0.784	-31.284**	-0.603	1.821
	(0.05)	(5.61)	(2.80)	(12.11)	(7.03)	(5.06)
PreTreat_Mean	4.78	142.49	157.03	-1.18	4.61	20.26
PreTreat_SD	0.64	88.63	75.44	52.99	60.84	69.00
Observations	12857	12857	12857	12857	12857	12857
R^2	0.45	0.43	0.80	0.27	0.30	0.31

Notes: The table presents estimates based on the regression specification in equation (6). Outcomes are the consumption components of equation (5). All values are in SEK 1,000 and expressed per adult equivalent. The complete regression estimates with all interactions are reported in Table A.15. Standard errors are clustered at the co-op level and reported in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

6.2 Homeownership and Consumption

Table 2 reports the estimated treatment effects on consumption and its components using equation (5). The table shows that there are no significant differences between treated and control in the years before treatment ("Pre"). A dynamic DiD specification, plotted in Appendix Figure A.10, confirms that the parallel trends assumption is satisfied for all outcome variables reported in the table.

We observe a large increase in consumption in the treatment year (7.8%, 14.5 kSEK) and particularly in the post years (18.5%, 29.7 kSEK). The consumption response substantially exceeds the response implied by the simple life-cycle model. Equation (4) predicts an average response of 5.6 kSEK ($r/(1+r) \cdot \widetilde{W}$ with $r = .07$). Below we discuss several mechanisms that contribute to the large response.

Columns (3)-(6) analyze how the consumption increase is financed. We find no effect of homeownership on disposable income. Appendix Table A.16 confirms this null result for a broad set of labor supply outcomes, including labor income per adult, labor force participation, parental and sick leave benefits, unemployment benefits, distance from work, and transitions to more or less volatile industries.

Naturally, housing wealth and debt increase substantially as part of the initial apartment purchase. For the treated, the change in housing wealth in relative year 0 is measured using the apartment conversion price, p_0^c , to avoid mechanical increases in consumption from valuation effects. The average treated household borrows the entire purchase price of the house, about 320 kSEK. Yet, the loan-to-value ratio, which is based on market values, remains modest. Assuming that all incremental debt is mortgage debt, we calculate a mean LTV for mortgage debt of 45% among the treated with an interquartile range of 36–58%. Treated households have spare debt capacity. As shown in the bottom right panel of Figure 1, the buffer increases by more than 200

kSEK in the year of treatment. On average, the treated do not tap into the available home equity in the year of treatment.[34] Instead, most of the initial consumption increase is financed by reducing financial wealth, which falls by 10.7 kSEK.

In the post years, the average yearly consumption increase is twice the size of the initial response. On average, it is entirely financed by tapping into housing wealth. Subsequent analysis will show substantial heterogeneity in the consumption response and how it is financed. First, we study how the consumption response varies with the size of the wealth shock.

6.3 Disentangling the Wealth Effect from Other Effects of Homeownership

Our natural experiment involves a joint change in homeownership and housing wealth, as laid out in equation (8). To get at the economic mechanism behind the consumption response, this section pursues three approaches to gauge the impact of the wealth shock itself.

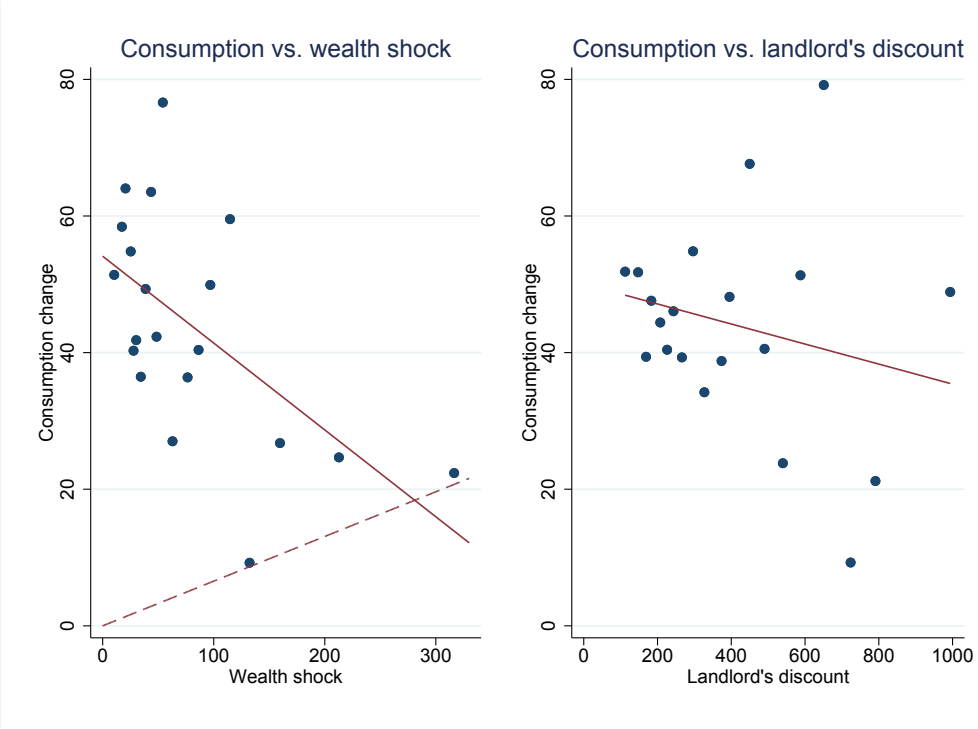
In the first approach, we confine ourselves to the response of households that take up treatment. The left panel of Figure 2 plots the consumption change against the wealth shock $\widetilde{W}_i$ in a bin scatter plot. The plot shows a strongly negative relationship. For each additional 100 SEK of $\widetilde{W}$, the consumption response decreases by 12.7 SEK. This result stands in sharp contrast to the predictions of the life-cycle model in equation (4) which implies a linearly increasing relationship between the wealth shock and the consumption response, as indicated by the dashed line in the plot. The null hypothesis that the estimated slope is equal to zero is rejected ($p = 0.01$). The right panel plots the same consumption response among the treated against the landlord's discount, τHW_0. The latter measures the immediate capital gain in apartment wealth at market prices. The gain in apartment wealth displays a negative relationship with consumption as well; the slope estimate is not statistically different from zero. We have verified similarly negative cross-sectional relationships between gross housing wealth and consumption changes, and between home equity and consumption changes.

The second approach estimates heterogeneous treatment effects by estimating equation (7) with $D_i = \widetilde{W}_i^{nbd}$. We relegate these results to Table A.17. The key finding is that there is no marginal effect of the wealth shock on consumption in the treatment year nor in the post period. The slope effect in the post period is estimated to be -0.075 with a t-statistic of -1.25. The intercept effect is estimated at 34.4 kSEK with a t-statistic of 5.25.

Our third and preferred approach is the IV estimation, detailed in Section 5. Table A.18 reports first-stage results; the Kleibergen-Paap F-statistic indicates strong instruments. Table 3 reports the second-stage estimation results. The main result is in column (3). It displays how consumption responds to a change in homeownership and to the wealth shock. The second row shows a strong

[34] As noted in the introduction, the increase in debt in the raw data for treated households who take up treatment is 385.3 kSEK. These households have 44,600 kUSD in remaining debt capacity in the year of treatment on average in the raw data.

Figure 2: Consumption changes vs. wealth shocks and landlord's discount



Notes: The panels depict bin scatter plots of consumption changes from relative year -1 to relative year k: $c_{i,k} - c_{i,k-1}$ for $k \geq 0$. The sample is the treatment group excluding residual tenants ($N = 1,824$). The left panel plots consumption changes against the wealth shock ($\widetilde{W}_i$). The solid line is based on a fitted OLS regression: $c_{i,k} - c_{i,k-1} = 54.11 - 0.127 \cdot \widetilde{W}$, where the standard error on the slope coefficient is 0.055 ($p = 0.022$) and the 95% confidence interval is $[-0.24, -0.019]$. The dashed line indicates the model-implied response from equation (4) with $r = .07$. The right panel depicts a bin scatter plot with the landlord's discount in relative year 0 on the horizontal axes. The solid line is based on a fitted OLS regression: $c_{i,k} - c_{i,k-1} = 50.06 - 0.015 \cdot \tau HW_i$, where the standard error on the slope coefficient is 0.020 and the 95% confidence interval is $[-0.054, 0.025]$. Bins in the panels are based on the respective variable on the horizontal axis. Standard errors are clustered at the household level. All values are in thousands of SEK and scaled by adult equivalent units.

effect of homeownership in the post years, 32.9 kSEK, which is close to the 34.9 point estimate from the second approach. The fourth row reports a slope effect on the instrumented wealth shock of -.006 with a standard error of .08. Consistent with the first two approaches, the IV approach indicates that there is no evidence of a positive marginal propensity to consume out of marginal increases in $\widetilde{W}$. Even at the upper bound of the 95% confidence interval for the slope estimate, we can bound the pure wealth effect at 13.6 kSEK ($0.16 \cdot 85.16$ kSEK) in the post years for the average household, which represents 46% (41%) of the average ITT (TOT) consumption response.

For comparison, column (1) of Table 3 only includes the instrumented homeownership rate. The estimated consumption response in the post period is 32.4 kSEK, which is greater than the 29.7 kSEK reduced-form estimate of Table 2, reflecting the fact that some households in the treatment group continue to rent (TOT versus ITT estimate).

To illustrate the consequences of omitting homeownership, column (2) estimates a specification that only includes the instrumented wealth shock. It shows a strong response of consumption to the wealth shock, with a slope estimate of 0.208 in the post years. This specification is misspecified since it does not account allow for homeownership to exert an effect on consumption that is unrelated to the wealth changes triggered by the experiment.

Table 3: Instrumental variable estimates on consumption

	(1)	(2)	(3)	(4)	(5)
$\widehat{\text{own}_i \times \text{RY}_{it}(0)}$	14.775**		1.560		-15.384
	(6.47)		(10.95)		(15.31)
	[1.66 27.9]		[-20.6 23.7]		[-46.4 15.6]
$\widehat{\text{own}_i \times \text{RY}_{it}(\text{Post})}$	32.439***		32.906***		20.054
	(6.63)		(8.99)		(16.69)
	[19.0 45.9]		[14.7 51.1]		[-13.8 53.9]
$\widehat{\widetilde{W} \times \text{RY}_{it}(0)}$		0.157**	0.152		
		(0.08)	(0.12)		
		[0.00077 0.31]	[-0.10 0.40]		
$\widehat{\widetilde{W} \times \text{RY}_{it}(\text{Post})}$		0.208***	-0.006		
		(0.06)	(0.08)		
		[0.093 0.32]	[-0.17 0.16]		
$\widehat{\tau HW_i \times \text{RY}_{it}(0)}$				0.039**	0.067*
				(0.01)	(0.03)
				[0.011 0.067]	[-0.0026 0.14]
$\widehat{\tau HW_i \times \text{RY}_{it}(\text{Post})}$				0.062***	0.025
				(0.02)	(0.04)
				[0.031 0.092]	[-0.054 0.10]
Observations	12857	12857	12857	12857	12857
R^2	0.01	0.01	0.01	0.01	0.01
Kleibergen-Paap F-stat	329.75	35.44	39.70	32.55	13.92

Notes: The table presents second-stage instrumental variable estimates on consumption. Estimates are based on the regression specification in equation (9). Standard errors in parentheses and 95-percent confidence intervals in brackets. The Kleibergen-Papp F-statistic reports on the test for weak instruments (see Kleibergen and Paap (2006) and Andrews, Stock and Sun (2020) for discussion). First-stage estimates for columns (3) and (5) are reported in Tables A.18 and A.19. The complete second-stage regression estimates with all interactions are reported in Table A.20. Standard errors are clustered at the co-op level. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

To make closer contact with the literatures on the MPC out of housing wealth and on the housing collateral effect, we also consider an IV regression where we instrument the landlord's discount τHW instead of the wealth shock $\widetilde{W}$, using neighborhood-level housing wealth $\tau^{nbd} HW^{nbd}$ instead of $\widetilde{W}^{nbd}$ in the formation of the instruments. Studying the (instrumented) consumption response to an increase in τHW is informative for uncovering mechanisms beyond a pure wealth effect, such as a collateral effect.

Typical MPC out of housing wealth estimates, recently estimated for the U.S., are in the .03–.07 range (e.g., Aladangady, 2017; Mian, Rao and Sufi, 2013; Guren et al., 2020). Since the typical estimates are on a sample of homeowners, they have no variation at the extensive margin. Column (4) of Table 3 estimates a MPC out of housing wealth of .062 per year in the post years, in a specification that ignores variation in homeownership. In our context, this slope estimate is biased. The specification in column (5) remedies the problem. Consistent with the findings in column (3),

the slope estimate is not statistically different from zero in the post years once the intercept is included. The evidence suggests that other factors besides a pure housing wealth effect dominate the consumption response of homeowners. The point estimate for the MPC in column (5) is .025. Paiella and Pistaferri (2017) estimate a similar .03 wealth effect on consumption, which they argue is driven by housing wealth. The 95%-confidence interval allows us to bound the MPC out of a marginal increase in housing wealth from above at .10.

As noted, the change in housing wealth (τHW) is much larger than the wealth shock ($\widetilde{W}$) since the former includes valuation effects and does not consider that privatizing households give up a subsidized rental apartment. The market value of housing wealth is relevant when considering the additional collateral effect. Therefore, it is useful to also compare our MPC estimate out of τHW to the marginal propensity to borrow out of housing collateral. DeFusco (2018) finds estimates ranging from 0.04 to 0.13, holding wealth fixed. Our estimate of .025 is lower, while our confidence interval allows for estimates in this range. More importantly, our experiment generates discrete (rather than marginal) changes in both homeownership status and wealth, so that an average propensity to consume number may be more appropriate than a MPC. Dividing the average treatment response of the treated (32.4 kSEK) by the average landlord discount (412.2 kSEK) implies an average propensity to consume of 0.079.

To summarize, observed consumption responses are much larger than what the simple model of Section 3, which only considers a wealth effect, implies. In sharp contrast with that model, the raw data for treated households display a negative relationship between additional wealth and the magnitude of the response. In other words, small wealth shocks are associated with large responses and vice versa. The IV estimation, which additionally instruments for the size of the wealth shock, confirms the absence of a positive link between the wealth shock (or the change in the market value of housing wealth) and consumption in the cross-section. These results suggest that incremental increases in wealth are not the primary driver of the consumption response in our quasi-experiment. In the next section, we argue that the large intercept effect is consistent with homeownership relaxing borrowing constraints.

6.4 The Housing Collateral Effect

In this section, we argue that the pattern of consumption responses is consistent with housing collateral alleviating borrowing constraints by allowing young households to tap into housing equity in order to bring consumption forward in time. Housing collateral also allows homeowners to smooth consumption across states of the world. Faced with large negative income fluctuations, homeowners are able to borrow to smooth consumption whereas renters cannot.

6.4.1 Consumption smoothing over the life-cycle

Table 4 splits households into those below and above age 40. Column (1) shows that households younger than 40 years in the treatment group increase consumption in the post period by 30.9% relative to the young in the control group. In contrast, the older households increase consumption by only 13.3% (i.e., a difference of 17.6%). The smaller consumption response of the old occurs despite the fact that the old experience a larger wealth shock than the young, consistent with Figure 2 and the IV regression evidence. The decomposition by age also highlights how far off young households are from the model's prediction. Their average consumption response is 47.6 kSEK, which is larger than their average wealth shock $\widetilde{W}$ of 37.26 kSEK, implying a MPC greater than unity.[35]

These results are consistent with the young wanting to bring consumption forward in time, given that they are on the upward sloping part of life-cycle income profile. In the absence of sufficient liquid wealth, this intertemporal consumption smoothing requires the ability to borrow. As noted, the experiment creates a large increase in borrowing capacity, the buffer, both in the treatment year and in the years thereafter by virtue of rising house prices. This gives young homeowners access to housing collateral to borrow against. Indeed, columns (4)–(6) show that the young increase borrowing substantially to pay for the consumption increase. In the treatment year, their borrowing exceeds the cost of the apartment. In the post years, they increase borrowing despite a decrease in housing wealth, thereby reducing home equity by 34 kSEK. Young renters in the control group do not have this buffer to smooth consumption intertemporally.

In contrast with the young, the old do not wish to bring consumption forward. Columns (4)–(6) show that the old indeed choose to borrow less initially, relative to the value of their apartment, and pay down debt in the post years. They are able to simultaneously increase consumption and financial savings by reducing net housing wealth.

The evidence that households use home equity to better smooth consumption over the life-cycle is consistent with responses to lottery shocks (i.e., shocks to financial wealth) reported in Fagereng, Holm Blomhoff and Natvik (2021), but stands in contrast with response to positive housing wealth shocks reported in Christelis et al. (2019). Overall, these results point to the importance of borrowing constraints, in line with the model of Berger et al. (2018) and the empirical results in Leth-Petersen (2010), Paiella and Pistaferri (2017), Baker (2018), DeFusco (2018), and Ganong and Noel (2020). In particular, Leth-Petersen (2010) report that young households respond more than old households to a credit market reform in Denmark.

[35] In contrast, the one-year MPC out of the wealth shock for the old is approximately 0.21 $((47.56 - 24.90)/107.1)$. See Table A.14 for summary statistics of $\widetilde{W}$ by subgroups.

Table 4: Consumption Smoothing Across Time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Cash-flows				Mobility	
	Log cons.	Cons.	Income	dHouse	dDebt	dFin	Move	Move up
$\text{Priv.}_i \times \text{RY}_{it}$(Pre)	0.082	6.681	-0.327	6.384	3.722	-9.609	0.016	-0.004
	(0.07)	(8.30)	(5.15)	(7.70)	(7.83)	(6.45)	(0.01)	(0.01)
$\text{Priv.}_i \times \text{RY}_{it}$(0)	0.065	14.007*	2.398	247.714***	254.699***	-4.645	-0.030	-0.023**
	(0.06)	(8.24)	(4.10)	(43.41)	(46.28)	(6.94)	(0.02)	(0.01)
$\text{Priv.}_i \times \text{RY}_{it}$(Post)	0.309***	47.562***	-2.546	-24.422	19.350*	-6.281	0.047**	0.044***
	(0.08)	(8.22)	(4.27)	(14.67)	(10.22)	(5.18)	(0.02)	(0.01)
$\text{Priv.}_i \times \text{RY}_{it}$(Pre)	-0.070	-6.183	-1.832	-19.058**	-8.528	14.724	-0.021	0.006
D(Old)_i	(0.07)	(8.22)	(4.97)	(8.55)	(8.71)	(9.10)	(0.01)	(0.01)
$\text{Priv.}_i \times \text{RY}_{it}$(0)	0.021	1.365	-0.216	107.066*	99.870*	-8.782	-0.001	0.022**
D(Old)_i	(0.07)	(10.46)	(4.51)	(56.23)	(54.79)	(8.32)	(0.02)	(0.01)
$\text{Priv.}_i \times \text{RY}_{it}$(Post)	-0.176**	-24.897**	4.878	-8.380	-25.909**	12.214	-0.046**	-0.037**
D(Old)_i	(0.07)	(8.65)	(4.44)	(16.78)	(12.20)	(7.51)	(0.02)	(0.01)
Observations	12857	12857	12857	12857	12857	12857	12857	12857
R^2	0.4503	0.4284	0.8042	0.2768	0.3082	0.3065	0.1585	0.1671

Notes: The table presents reduced form effects on the consumption components (cash-flows) in columns (1)-(6) and two mobility outcomes in columns (7)-(8). The regression corresponds to equation (7) where $D = \text{D(Old)}_i$ which indicates whether the household head is older than 40 in relative year -1. The variable Move is equal to one in the year that household moves out from the original apartment. Move up is equal to one if the household moves out of the original apartment to a parish with higher average housing wealth per household and is zero otherwise. The moving variables are one at most once during the sample period. The average age conditional on being younger than the cut-off value is 33 years. The average age conditional on being older than the cut-off value is 51 years. The complete regression estimates with all interactions are reported in Table A.21. Standard errors are clustered at the co-op level and reported in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

6.4.2 Consumption smoothing across states of the world

Next, we show that homeowners are better able to smooth consumption in the wake of adverse income fluctuations.

We define an indicator variable Z_{it} which takes a value of one if household i experiences an income decline of 25% or more in year t. Table 1 shows that about 10% of households experience such fluctuations in the pre-treatment period. As Fagereng, Guiso and Pistaferri (2017, 2018) discuss, it is often difficult to separate endogenous income fluctuations from exogenous income shocks. Panel A of Table A.23 shows that Z_{it} is driven by a range of different events, including unemployment. The indicator variable Unemployment is 0.40 if $Z_{it} = 1$ and 0.12 if $Z_{it} = 0$. Importantly, Panel B of Table A.23 shows that Z_{it} does not respond to treatment. For the purpose of our analysis, this justifies labeling $Z_{it} = 1$ as an exogenous income shock.

We estimate regression equation (6) interacting all covariates but the fixed effects with the indicator Z_{it}. Table 5 reports the results. For households with $Z_{it} = 1$, the average income change is -27.4 kSEK and is not different between the control and treatment groups (column 3).

There are two key results. First, homeowners who are hit with the negative income shock in

Table 5: Consumption Smoothing Across States of the World

	(1) Log cons.	(2) Cons.	(3) Income	(4) dHousing	(5) dDebt	(6) dFin
$Z_{it} \times \text{Private}_i \times \text{RY}_{it}(\text{Pre})$	0.073	-0.897	1.315	-16.320	-2.588	15.988
	(0.11)	(13.29)	(6.51)	(20.36)	(15.53)	(19.45)
$Z_{it} \times \text{Private}_i \times \text{RY}_{it}(0)$	0.135	21.318	3.174	29.203	68.218	20.866
	(0.14)	(20.23)	(8.34)	(47.01)	(52.97)	(13.15)
$Z_{it} \times \text{Private}_i \times \text{RY}_{it}(\text{Post})$	0.192*	29.940*	-3.746	-2.743	31.950**	0.916
	(0.10)	(16.20)	(8.82)	(26.70)	(11.25)	(13.80)
Z_{it}	-0.174**	-18.187**	-27.390***	6.241	3.836	-11.617**
	(0.05)	(5.29)	(4.25)	(6.05)	(6.71)	(5.57)
Observations	12857	12857	12857	12857	12857	12857
R^2	0.45	0.43	0.81	0.27	0.30	0.31

Notes: The table presents reduced form effects on the consumption components of equation (5). The dummy variable Z_{it} takes on the value one if the income fluctuation is -25% or greater in magnitude. Estimates are based on the regression specification in equation (6), extended so that all covariates are interacted with Z_{it}. The complete regression estimates with all interactions are reported in Table A.22. Standard errors are clustered at the co-op level and reported in parentheses.

the post years increase debt by 32.0 kSEK, about the same amount of the income shock. Households in the control group (predominantly renters) that are hit with the same income shock display no change in debt. Second, the difference in response to debt leads to a large difference in consumption between owners and renters. Renters hit with the income shock reduce consumption by 18.2 kSEK on average, a clear marker of incomplete consumption insurance. In sharp contrast, homeowners are able to fully off-set the loss in income by using their debt capacity. The difference in the consumption response between treatment and control groups is 29.9 kSEK or 19.2% points.

In related work, Hurst and Stafford (2004) find that liquidity-constrained households that become unemployed are 25% more likely than otherwise similar households to refinance their mortgage, and that sixty percent of the increase in debt is used to boost consumption. Homeowners in our experiment, who have access to substantial home equity, are able to borrow and use all of the increase in debt to avoid a consumption drop.

Table A.25 shows that the use of debt capacity among homeowners is robust to alternative definitions of Z_{it} where the threshold for the income shock ranges from -10% to -30%. Table A.26 shows that consumption smoothing across state is not statistically different for young and old households.

6.5 (Upward) Mobility

Homeowners are generally thought to be less mobile than renters. But those results suffer from severe endogeneity issues since owners differ from renters in myriad ways that are hard to fully control for. We study the causal effect of homeownership on household mobility.

Column (7) of Table 4 finds that homeownership increases mobility for young treated households. The probability that a treated household moves in the four years after treatment is 4.7%

points higher than for young households in the control group. Given a moving rate for young households in the control group of 7.8% points in the post period, this is a large treatment effect. Similarly, Table A.27 shows that for the typical renter household in Stockholm below 40 years of age, the likelihood of moving at least once during our sample period is 9.1%. The treatment effect constitutes a 50% increase relative to this baseline rate. In contrast, the old show no increase in mobility; the differential effect between young and old is statistically significant.

Column (8) of Table 4 shows that young owners are 4.4% points more likely to move to a better neighborhood than young renters. A better neighborhood is defined as a parish with higher housing house prices (housing wealth per household). Given a moving-up rate for young households in the control group of 1.7% points in the post period, this is a large treatment effect. Table A.27 shows that the typical young renter household in Stockholm experiences a likelihood of moving up to a parish with higher average housing wealth of 2.8%. Our treatment effect represents a 150% increase relative to this baseline level. In other words, homeownership strongly promotes upward mobility.

Table A.28 reports additional moving outcomes. For instance, the probability that a young treated household moves and buys a new apartment or single-family house increases by 6.4% points. The effect is large compared to baseline mobility rates. Overall, treated households have a greater opportunity to move than control households, and it is the younger households that take advantage of this opportunity.

Our quasi-experimental evidence suggests that homeownership promotes mobility. The capital gains that accompany homeownership in a normal, rising housing market allow households to use the sale proceeds from their privatized apartment make a downpayment on their next home (Ortalo-Magne and Rady, 2006).[36] The results on upward mobility add an interesting spatial aspect to the wealth-building effects of homeownership emphasized in Section 6.1. They complement a literature on moving-to-opportunity (Chetty, Hendren and Katz, 2016), focused on subsidies to low-income renters to facilitate their mobility to better neighborhoods. With incomes similar to those of other renters in Stockholm, our subjects are richer than the average renter in the MTO experiment.

6.6 Heterogeneity in responses among stayers and movers

Table 6 studies heterogeneous responses for three groups of households in the treatment group: stayers, mover-renters, and mover-owners. Mover-owners are those who are homeowners at the end of the year of the move. Each subgroup in the treatment group is measured against the full control group in this analysis. While we showed in the previous section that moves are at least partly caused by treatment, an important caveat to this analysis is that moving is an endogenous

[36] The opposite scenario where a homeowner is underwater on her mortgage may result in a reduction of mobility. Bernstein and Struyven (2021) refer to this phenomenon as housing lock.

decision. The results are best interpreted as a sample split that sheds further light on the economic mechanisms at work.

Table 6: Heterogenous Treatment Effects for Stayers and Movers

	(1) Log cons.	(2) Cons.	(3) Income	(4) dHouse	(5) dDebt	(6) dFin
$\text{Priv.}_i \times \text{RY}_{it}$(Pre)	0.029	4.494	-1.160	-5.668	-1.854	-1.855
	(0.04)	(4.48)	(2.67)	(3.61)	(5.56)	(3.99)
$\text{Priv.}_i \times \text{RY}_{it}$(0)	0.069*	13.909**	1.698	327.804***	325.017***	-15.004**
	(0.04)	(4.85)	(1.95)	(59.65)	(63.23)	(4.44)
$\text{Priv.}_i \times \text{RY}_{it}$(Post)	0.144**	18.363**	4.788	-2.173	3.417	-7.953**
	(0.04)	(5.18)	(3.12)	(4.32)	(4.74)	(2.91)
$\text{Priv.}_i \times \text{RY}_{it}$(Pre)	0.073	-2.291	1.161	-3.549	-4.468	2.139
$\times \text{D(MoveRent)}_i$	(0.10)	(10.09)	(5.78)	(10.72)	(8.73)	(15.61)
$\text{Priv.}_i \times \text{RY}_{it}$(0)	0.057	-0.721	-3.880	25.991	40.683	11.388
$\times \text{D(MoveRent)}_i$	(0.06)	(6.69)	(4.59)	(77.15)	(75.41)	(11.31)
$\text{Priv.}_i \times \text{RY}_{it}$(Post)	0.019	18.813	-30.628**	-182.497***	-70.927**	62.167**
$\times \text{D(MoveRent)}_i$	(0.09)	(12.31)	(9.12)	(48.84)	(27.36)	(21.02)
$\text{Priv.}_i \times \text{RY}_{it}$(Pre)	-0.042	-12.038	0.175	16.900	15.957	11.308
$\times \text{D(MoveOwn)}_i$	(0.10)	(11.47)	(6.64)	(18.52)	(14.21)	(17.47)
$\text{Priv.}_i \times \text{RY}_{it}$(0)	-0.058	-9.532	8.708**	-72.717	-70.848	20.100
$\times \text{D(MoveOwn)}_i$	(0.11)	(15.92)	(4.08)	(73.29)	(67.21)	(16.51)
$\text{Priv.}_i \times \text{RY}_{it}$(Post)	0.184**	41.868**	-1.019	-78.582**	-17.143	18.529
$\times \text{D(MoveOwn)}_i$	(0.09)	(14.65)	(6.30)	(29.41)	(23.98)	(12.56)
PreTreat_Mean	4.78	142.49	157.03	-1.18	4.61	20.26
PreTreat_SD	0.64	88.63	75.44	52.99	60.84	69.00
Observations	12857	12857	12857	12857	12857	12857
R^2	0.45	0.43	0.81	0.29	0.32	0.31

Notes: The table presents reduced form effects on cash-flows and portfolio choice. The variable D(MoveRent)_i is equal to 1 in the year that household moves out from the original apartment if the household does not own an apartment or single-family house at the end of that year. The variable D(MoveOwn)_i is equal to 1 in the year that household moves out from the original apartment if the household does own an apartment or a single-family house at the end of that year. The complete regression estimates with all interactions are reported in Table A.29. Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

First, stayers increase consumption significantly. The initial increase is 6.9% (13.9 kSEK) and the post-years response is 14.4% (18.4 kSEK). Stayers finance their consumption increases by actively reducing their savings in financial wealth relative to the control group. The patterns among stayers suggest that even those with stable homeownership, whose housing wealth, net worth, and buffer grow as a result of house price appreciation, increase consumption.[37] The average wealth shock among stayers is 90.2 kSEK so the post-year response implies a yearly MPC out of $\widetilde{W}$ of .20. The MPC out of apartment wealth HW_0 is .023. The housing collateral effect contributes to the strong consumption response for stayers. Table A.24 finds that stayers who experience a negative income shock increase debt by as much as in the full sample, and similarly managing to avoid a consumption drop. Stable homeownership provides insurance to income shocks through increased debt capacity.

Second, movers experience larger consumption increases than stayers in the post years, de-

[37] Table A.33 displays the evolution of balance sheet items for stayers and movers.

spite receiving a smaller average wealth shock ($\widetilde{W}$ of 74.1 kSEK). Indeed, the point estimate for mover-renter's consumption response in the post years is 18.8 kSEK higher than for the stayers (imprecisely estimated) while the consumption response for mover-owners is 41.9 kSEK higher per year (statistically significant). Movers account for 26.6% of treated households but 55.2% of the total spending increase.[38] Consumption increases are financed by a reduction in home equity for both types of movers, but naturally much more so for mover-renters. Mover-renters also reduce labor supply, but this may be a selection or reverse causality effect due to the endogenous nature of the moving decision. The stronger consumption response of both sets of movers suggests that turning illiquid housing wealth into liquid financial wealth boosts the spending response. This is consistent with (Ganong and Noel, 2020) who emphasize the importance over cash-flow over wealth changes for explaining household behavior.

6.7 Portfolio Choice

The final outcome variable we study is the composition of the financial asset portfolio. In the full sample, we find that the unconditional risky share of the treated increases by 2.1% points in the post years relative to the control group. This is a sizeable effect relative to the pre-treatment mean of 23% points. The conditional risky share increases by 2.7% points but the latter effect is imprecisely measured (see Table A.31).[39]

The average effect hides interesting cross-sectional variation. The effect is fully driven by the old, who increase their unconditional risky share by 5.3% points and their conditional risky share by 8.2% points in the post period, both precisely measured. The first two columns of Table 7 report the estimates. We also find large and statistically significant increases in the unconditional (3.7% points) and conditional (5.1% points) risky share for stayers, but not for movers. The last two columns of Table 7 report the estimates for stayers versus movers. For context, pre-treatment averages for our sample households are 19% for the unconditional and 34% for the conditional risky share.[40]

These results show that a substantial fraction of treated households meaningfully increase their allocation to risky assets. By taking advantage of the equity risk premium, they extend the wealth-building benefits of homeownership to their financial portfolio and magnify them. To the best of our knowledge, we are the first to establish a causal link between homeownership and returns on

[38] Moving costs and broker fees cannot account for the differential consumption response. In a separate specification where we compare treated movers to control movers, we find a similar consumption increase for treated movers. Since control and treated movers face the same moving costs, moving costs do not explain the consumption increase. Treated movers face a 3% brokerage fee on the sale price of their housing unit. Control movers do not since they do not own their apartment unit. When split over the four treatment years, the one-time broker fee can only account for about 10% of the consumption response.

[39] Figure A.12 reports the raw data on the risky share for treated and control households as well as the dynamic difference-in-difference estimates.

[40] Effects are small and insignficant among households that are not participating already in relative year -1, consistent with the differences between the unconditional and conditional risky share results.

the financial wealth portfolio.

Table 7: Portfolio Choice Depending on Age and Moves

	(1)	(2)	(3)	(4)
	Young/Old		Stayer/Mover	
	RS (uncond.)	RS (cond.)	RS (uncond.)	RS (cond.)
Priv.$_i$ × RY$_{it}$(Pre)	0.004	-0.000	0.004	0.009
	(0.02)	(0.03)	(0.01)	(0.02)
Priv.$_i$ × RY$_{it}$(0)	-0.007	-0.008	0.012	0.015
	(0.01)	(0.02)	(0.01)	(0.02)
Priv.$_i$ × RY$_{it}$(Post)	-0.007	-0.015	0.037**	0.051**
	(0.01)	(0.02)	(0.01)	(0.02)
Priv.$_i$ × RY$_{it}$(Pre)	0.006	0.027	0.011	0.018
D(Old)$_i$/D(Move)$_i$	(0.02)	(0.04)	(0.02)	(0.03)
Priv.$_i$ × RY$_{it}$(0)	0.028	0.046	-0.008	0.005
D(Old)$_i$/D(Move)$_i$	(0.02)	(0.04)	(0.02)	(0.03)
Priv.$_i$ × RY$_{it}$(Post)	0.053**	0.082**	-0.055**	-0.081**
D(Old)$_i$/D(Move)$_i$	(0.02)	(0.03)	(0.02)	(0.03)
Observations	12857	7232	12857	7232
R^2	0.76	0.65	0.76	0.65

Notes: The outcome variables are the unconditional risky share and the conditional risky share, conditional on a strictly positive risky share. The regression corresponds to equation (7) where D_i = D(Old)$_i$ in the columns (1)-(2), an indicator which is one for households with head above median age, and D_i = D(Mover)$_i$ in columns (3)-(4), an indicator that is one for households who move after treatment. Standard errors are clustered at the co-op level and reported in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Several economic theories predict differences in optimal financial portfolios for homeowners and renters. Some of the mechanisms they emphasize differ for young and old households and for stayers and movers.

The first mechanism is portfolio diversification. Yao and Zhang (2005) use a life-cycle model to argue that homeowners increase their risky share, all else equal, because financial wealth is a smaller share of their total net wealth. Our results are consistent with this mechanism.[41]

Second, the housing collateral effect may contribute to the sizeable response in risk-taking for older households and stayers. Ample housing collateral improves risk sharing and acts like a reduction in risk aversion (Lustig and Van Nieuwerburgh, 2005), which naturally encourages more risk-taking. The old and stayers have particularly large buffers, as shown in Table A.14, which may drive their larger responses. The young have little financial wealth prior to treatment, and are borrowing against their housing collateral to bring consumption forward. They have less financial wealth to invest.

Third, the mobility results may also help explain the smaller effects on portfolio risk-taking by the young and the movers. Facing a higher probability of moving and having to buy a new home,

[41] A substantial literature on quantitative life-cycle portfolio choice models studies the link between homeownership and optimal portfolio choice. Cocco (2005) argues that homeownership crowds out stock holdings. Vestman (2019) show that the gap in stock market participation rates between renters and homeowners is linked to wealth inequality but find no evidence of crowding out.

the young and the movers face house price risk when selling the current home and buying a new one. This risk is larger in the treatment than in the control group since households in the control group do not have a house to sell. The differential housing market risk exposure may make the young treated, who display a high propensity to move (up), and the movers more reluctant to invest their savings in risky assets such as stocks. Among the treated, the young and the movers have a more incomplete hedge against future housing cost changes than the old and the stayers. This is consistent with the model of Sinai and Souleles (2005), which shows that the risk of owning declines with the correlation between house prices in the current house and housing costs in future locations.

Finally, homeownership acts as a commitment to housing expenses, which affects optimal portfolio choice (Calvet and Sodini, 2014). In a stylized model that abstracts from borrowing constraints and life-cycle considerations, Chetty, Sándor and Szeidl (2017) show that an increase in home equity—an increase in housing wealth holding fixed the size of the mortgage—should lead to an increase in the risky share. Conversely, an increase in the mortgage, holding fixed home equity, should lead to a reduction in the risky share. Using different sources of variation in housing wealth, they estimate an increase in the unconditional risky share of 6.8% points for every 100 kUSD increase in home equity. In our experiment both mortgage and home equity increase, but because of the landlord's discount, home equity increases more. Applying their estimate to our setting gives a good fit for the old (4.9% versus 4.6% points predicted versus observed increase) but not for the young (3.4% versus -0.7% points).[42] Like the old, stayers face less uncertainty in their housing commitment, which may explain their larger portfolio responses.

7 Spending on Home Improvements

Some of the consumption response could be due to home renovations or purchases of furniture or home appliances, as was found for the U.S. (Benmelech, Guren and Melzer, 2021). Our consumption measurement does not allow for a breakdown by spending category. However, we pursue three avenues to investigate the extent to which the consumption response may reflect renovation expenditures or increases in housing costs.

First, tax registry-based data are available on some categories of renovation expenditures for our sample households. Appendix L.10 discusses the details. Table A.34 analyzes the response of the renovation spending variables for treated relative to control households. The spending response is positive and significant, but economically small compared to the overall consumption response.

Second, we study renovation expenditures at the co-op level. It is possible that upon privatization, the co-op board engages in extensive renovations of common areas, paid for by higher co-op

[42] Table A.32 shows that the old increase home equity by 576.1 KSEK or 72,000 USD, which implies a predicted increase of 4.9% points.

fees. If so, the increase in consumption could largely reflect an increase in housing consumption. We measure common area improvements at the co-op level and their pass-through into higher co-op fees. The data come from co-op annual reports, as discussed in Appendix G. Table A.8 shows no relationship between co-op improvements and co-op fees. The increases in co-op fees in treated buildings in the year of treatment and the following four years are de minimis. There is no evidence that renovation or housing spending account for a large share of the consumption increase. Furthermore, treated households' housing cost, measured by rent prior to treatment and co-op fees plus interest expenses after, do not increase (see Appendix G.3).

Third, we investigates consumption responses in the Swedish household expenditure survey in Appendix J. Table A.11 shows that households who just purchased a new apartment meaningfully increase home maintenance, furniture, and appliance purchases. However, non-housing related expenditure categories account for half of the consumption increase in the year of the apartment purchase. This evidence also suggests a consumption response beyond housing expenditure categories.

8 Conclusion

Unearthing the economic effects of homeownership is challenging because ownership status is a choice that is correlated with many household and housing characteristics. Our quasi-experimental setting, which exploits the privatization of rental apartments in Stockholm, overcomes this endogeneity problem. By using registry-based panel data on consumption and wealth, it is the first to study the causal effects of homeownership on consumption and savings.

We find that homeownership builds wealth and increases consumption. While some of the consumption increase may reflect a standard wealth effect, there is strong evidence that other economic mechanisms are at work. First, consumption responses are, if anything, stronger for households that experience a smaller wealth increase. Second, we find that a housing collateral channel allows households to borrow against their housing wealth to smooth consumption in the face of an adverse income shock, and allows young homeowners borrow to smooth consumption intertemporally. All results hold for a subsample of households who stay in their housing unit for at least four years after the privatization.

Homeownership promotes geographic mobility. Moving rates of young households increase substantially after privatization. Some of these households use the accumulated housing wealth to make a downpayment on a house in a better neighborhood, climbing the property ladder.

Finally, we find that homeownership interacts with portolio choice. Older households increase the risky share of assets, consistent with the rise in home equity they experience. The concomitant increase in borrowing capacity effectively makes them less risk averse.

An interesting question left for future work is whether exposure to homeownership during

childhood affects educational and labor market outcomes, mobility, home ownership, and wealth accumulation in adulthood. Chetty, Hendren and Katz (2016) find positive effects on the educational and labor market outcomes for the children of poor households who were moved to better neighborhoods. Studying neighborhood-level outcome variables such community and political engagement, school quality, and crime is another fruitful direction for exploration.

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Appendix for Online Publication

Identifying the Benefits from Home Ownership: A Swedish Experiment

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A Privatization Process

A.1 Market-wide Conversion Statistics

To illustrate the size of the co-op conversion movement, Table A.1 reports on the composition of the stock of apartments in the municipality of Stockholm in 1990, 2000 and 2004. Between 1990 and 2000, the stock of municipally-owned apartments declined by 8,000 units. Privatizations accelerated between the years 2000 and 2004 with another 8,000 units converted into co-ops. In addition to the three large municipal landlords, private landlords also massively converted apartment, accounting for three-quarters of the co-op conversions (31,000 out of 47,000). Between 2000 and 2004, co-op-owned apartments increased by 34,400 units. Over the longer 1990 to 2004 period, the ownership share of co-ops increased from 25% to 43%. Table A.2 zooms in on co-op conversions in the period 1999-2004. Municipal landlords privatized 12,200 apartments in Stockholm. Municipal landlord conversions ramped up dramatically in the year 2000 and peaked in 2001 at 5,500 units.

Table A.1: Apartments by ownership, 1990-2004, Municipality of Stockholm

Year	Co-ops	Municipal landlords	Private landlords	Total
1990	84,200	118,000	141,700	343,900
	25%	34%	41%	100%
2000	125,000	110,600	126,300	361,900
	34%	31%	35%	100%
2004	159,400	102,500	110,900	372,800
	43%	27%	30%	100%

Notes: The table reports the number and share of apartments in the municipality of Stockholm by type of ownership. Source: Utrednings- och statistikkontoret i Stockholms stad (2005, p. 11) and *http://statistik.stockholm.se/images/stories/excel/b085.htm*.

A.2 The Steps of the Privatization Process

The process of co-op conversion requires a series of formal steps. The first step is for the tenant association to register a home owner co-operative with Bolagsverket, the agency responsible for

Table A.2: Transactions of apartments by ownership, 1999-2004, Municipality of Stockholm

	1999	2000	2001	2002	2003	2004	1999-2004
Municipal landlords	200	3,500	5,500	2,100	400	500	12,200
Other landlords	5,300	4,700	5,300	4,900	5,000	4,100	29,300
Total	5,500	8,200	10,800	7,000	5,400	4,600	41,500

Notes: The table reports the number of apartment sales by year by type of ownership. Source: Utrednings- och statistikkontoret i Stockholms stad, 2005.

registering all limited liability companies in Sweden. A co-op needs at least three members. The co-op board consists of at least three and at most seven board members.

Once registered, the co-op can submit a letter to the district court indicating its interest in purchasing the property. This gives the co-op a right of first purchase for two years. Around the same time, the co-op contacts the landlord to express interest in acquiring the property. We refer to this date as the date of first contact. Below we describe the price formation process for privatizations executed by the three municipal landlords.

If the landlord is interested in selling the property, she must decide on an asking price. The landlord hires an appraisal firm to value the property and orders a technical inspection. Based on the inspector's and appraiser's reports, the landlord settles on an asking price for the property as a whole. This is a take-it-or-leave-it offer. How each individual apartment is priced is left to the discretion of the co-op. The landlord communicates the asking price to the co-op, along with a deadline.

Upon a favorable reply, the co-op has to submit an "economic plan," detailing how it will finance the purchase. Typically, the purchase is financed through a combination of one-time conversion fees paid in by co-op members, and a mortgage. The mortgage is a liability of the co-op and collateralized by the property. After conversion, the co-op uses the cash flows generated by the building to service the mortgage. The cash flows consist of co-op dues, rents from apartments from tenants who did not participate in the conversion and whose apartment is now owned by the co-op, and rental income from commercial tenants (e.g., retail or offices located in the building) if applicable.

Once the mortgage loan and the economic plan are in place, the tenants meet and vote on the proposed conversion. At least 2/3 of all eligible votes must be in favor for the conversion to go ahead. It is possible to submit a written vote. Only primary renters are allowed to vote, subtenants are not. The municipal landlord verifies that only eligible votes are taken into account. In a few instances, the landlord stopped the process and asked for a re-vote because some votes were deemed eligible by the tenant association but not by the landlord. The 2/3 majority is a minimum requirement. We have some observations where the vote exceeded 2/3, yet the purchase did not go through. Presumably, some co-op board decided it wanted or needed an even larger majority

to go ahead. Upon a favorable vote, the co-op board communicates the vote tally and the minutes of the meeting to the landlord. Unfortunately, we cannot use this 2/3 threshold as an alternative RDD-based identification strategy, as we observe bunching on the right hand side of the threshold. This bunching might reflect unobserved heterogeneity across co-ops and their tenants that is possibly correlated with our outcome variables of interest.

At this point, a private landlord would be free to approve the contract and sell the real estate. Until April 1^{st} 2002, the same was true for municipal landlords. After that date, the Stopplag applies, and municipal landlords must seek approval for the sale from the County Board.

A.3 Denials by the County Board

We use the passage of the Stopplag as an exogenous shock to the likelihood of approval of a co-op conversion. Conditional on having signed a contract with the landlord, the Stopplag reduced the likelihood of conversion from 100 percent to 33 percent. Unconditionally (taking the sample of all initiated privatization attempts), the likelihood of success was reduced from 50 percent to 17 percent. These numbers are calculated as follows. The municipal landlord Svenska Bostäder reports that 244 co-op associations initiated the conversion process during 1998-2002. Of those, 117 were sold representing a success rate of 48 percent. Among the 244 properties, 38 contracts were screened by the County Board. The Board approved 10, a success rate of 26 percent. Stockholmshem reports similar statistics: 59 conversions out of 120 applications. Nine properties with sales contracts were subject to the Stopplag and the County Board approved three. Familjebostäder finished privatizations prior to April 1st 2002 when the Stopplag became effective.

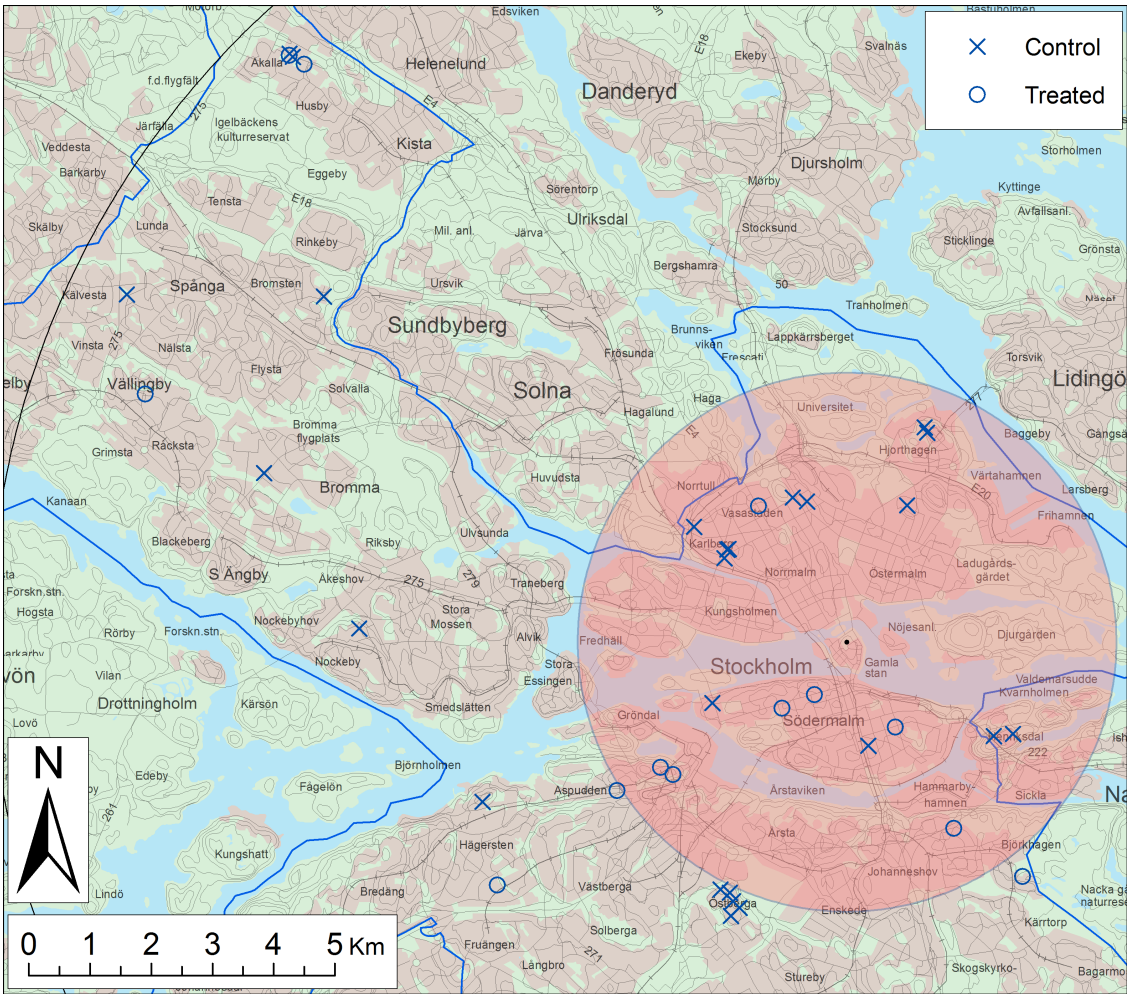
Stopplag resulted in the random denial of some co-op conversion attempts that were (i) initiated well before Stopplag was on the horizon, and (ii) fully approved by the municipal landlord and the tenant association. Out of 46 buildings (38 co-ops), 44 (36) of the attempts were initiated before November 2001. The other two were initiated before Stopplag became effective in April 2002. The conversion attempt of the Akalla complex, described in detail in Appendix A.4, serves as a good example of the random nature of the County Board decision. A detailed reading of minutes from the County Board confirms that the other denials were predominantly because a small share of apartments in the co-op had unique characteristics. Aside from the Akalla complex, reasons for denial in our sample include:

- The four-bedroom apartments in the building are unique to the neighborhood.
- The studios of size 17 to 25 square meters in the building are unique.
- The only remaining municipal building in the neighborhood has no elevator and has two fewer floors.
- The two-bedroom apartments in the building are unique to the neighborhood.

- The studios in the building are unique to the neighborhood.
- Two five-bedroom apartments in the building are unique to the neighborhood.
- There is one very large one-bedroom apartment in the building (54 square meters) which is unique to the neighborhood.

Figure A.1 plots the 38 co-ops on a map of the municipality of Stockholm; with circles denoting approvals and crosses denials. It also plots a shaded circle of five kilometer distance from the Royal Castle. Distance to the city center is measured as distance to the Royal Castle.

Figure A.1: Location of the Stopplag Sample



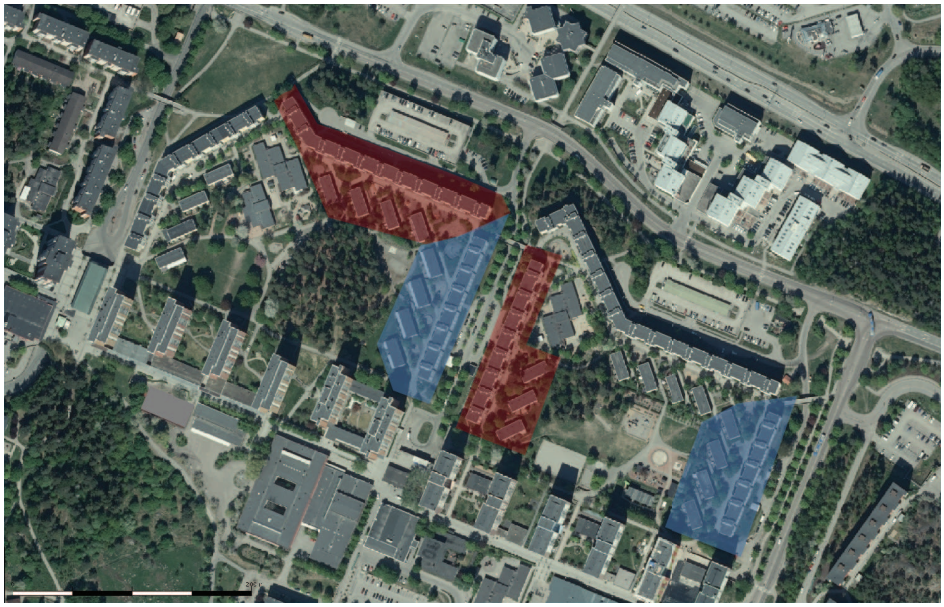
Notes: The map displays the location of the 38 privatization attempts in our Stopplag sample. Circles indicate approved co-ops (treated) and crosses indicate denied co-ops (control). The red circle has a radius of 5 kilometers distance from the center of Stockholm. The center is defined as the Royal Castle in the Old Town and it is indicated by a small black dot. The blue border indicates the municipality of Stockholm.

A.4 Example: Akalla Conversion

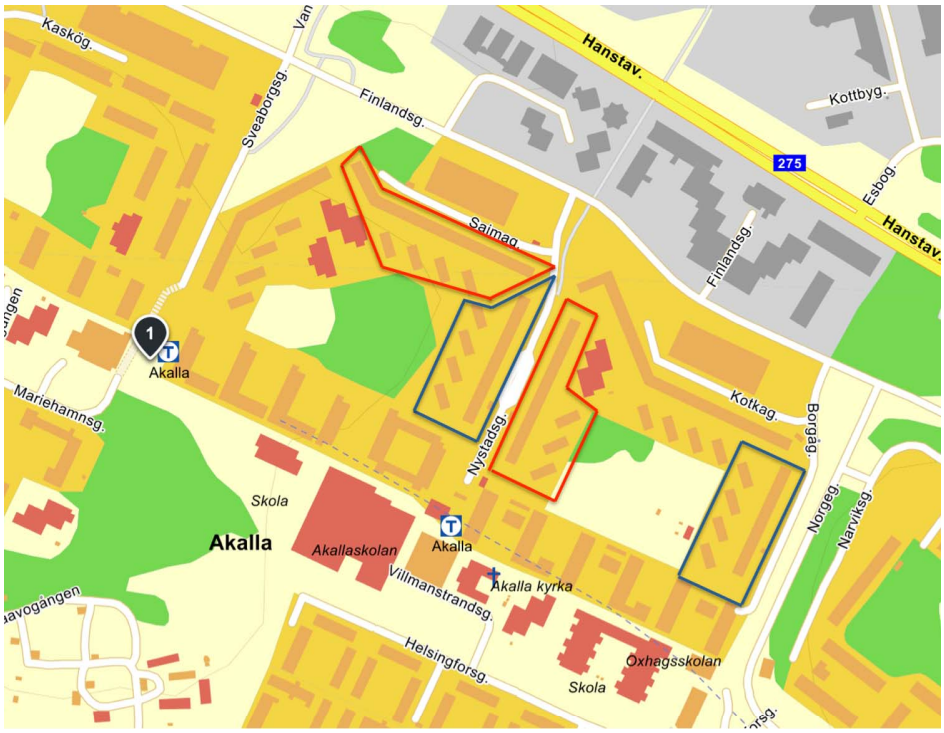
An example may help to further clarify the main quasi-experiment in home ownership that this paper studies. The Akalla complex consists of four co-ops located in a northern suburb of Stockholm, Akalla. Akalla is located in the district Kista, which is part of the Stockholm metropolitan area. Located only ten miles from the city center, it is served by the subway. It takes under 25 minutes to get to Stockholm's central train station by metro and about 35 minutes by car. The subway stop is a five minute walk from the co-ops. The district Kista was initially a working-class area, but starting in the 1970s an industrial section was constructed that housed several large IT companies which later became units of Ericsson and IBM. Ericsson has had its headquarters in Kista since 2003. Kista hosts departments of both the Royal Institute of Technology and Stockholm University. It is sometimes referred to as the Silicon Valley of Sweden. The area where the co-ops are located is a middle-class area at the time of our experiment.

Each of the four co-ops consists of several low- and mid-rise buildings adjacent to each other. Figure A.2 shows aerial and street views of the four properties, showing their geographic proximity. The entire Akalla complex was constructed in 1976, one year after the subway line to Akalla opened. All properties are owned by Svenska Bostäder, one of the large municipal landlords in Stockholm. Table A.3 provides details on the four properties. In addition to their extreme geographic proximity, identical year of construction, and identical ownership, the four co-ops' properties share several more characteristics. All co-ops have about the same floor area, with the vast majority of square meterage going to apartments and only a small fraction devoted to commercial use. They also have about the same distribution of apartments in terms of number of rooms, with the vast majority 3- and 4-room apartments (i.e., one- and two-bedroom apartments).

Figure A.2: Akalla Complex



(a) Aerial photograph



(b) Street view

Notes: Panel (a) shows an aerial photograph and panel (b) a street view of the Akalla complex where the buildings colored/boxed blue were accepted and the buildings colored/boxed red were denied for co-op conversion. From northwest to southeast, the buildings are Sveaborg 4, Sveaborg 5, Nystad 2, and Nystad 5, respectively. The T with a circle indicates the nearest metro stop. The townhouse apartments are the buildings in the courtyard.

The four co-op conversion attempts display striking similarity, as shown in Table A.3. All co-ops registered around the same time. The date of initial contact is the date on which the co-op sends a letter to the landlord indicating interest in the purchase of the building, thereby starting the conversion process. The first two co-ops approached Svenska Bostäder within two weeks from one another in June 2001. The last two co-ops sent their request within one week at the end of September 2001. After the requests were made, the landlord hired an appraisal firm to determine the value of the property. The appraisals for all four buildings were done by the same appraisal firm, around the same time (September and November 2001), and using the exact same methodology. The landlord then made the formal offer with the asking price to the co-op. The co-ops voted on the offer at their tenant association meeting. The meetings at the first two co-ops took place on the same day, April 21, 2002. The next two votes took place less than two months later on June 17 and 19, 2002. All four tenant associations voted for conversion, i.e., for accepting the price offered by the landlord, by essentially the same margin: 68-74% of the vote in favor. Having exceeded the voting threshold of 2/3, all four co-ops decided to go ahead with the conversion. Upon verification of the vote, the landlord conditionally approved all four votes and the sale of all four buildings on September 5 and 9, 2002. If Stopplag had not been in effect yet, that approval would have been the end of the process, and all four conversions would have gone ahead.

However, given that the Stopplag was approved just a few months earlier (in March 2002, going into effect on April 1, 2002), the sale to the four Akalla co-ops required an additional layer of approval from the County Administrative Board of Stockholm. The County Board ruled on all four co-ops on the same day, February 21, 2003. The Board ruled that the inner courtyard of the Akalla complex, which contained townhouses belonging to each of the four co-ops, represented a unique kind of residential housing among the municipal landlords' overall stock of housing. For the purposes of determining the rent on those types of units in that geography, the Board decided that it could not let all four co-ops convert. It decided that only two of the four transactions could be approved. There was no established rule for which of the co-ops to give priority. The Board had to make up a rule at the meeting and decided to give priority to the two co-ops that voted first. Different rules could have been employed, such as approval based on the date when the contract was signed or the voting share among the tenants. Either of these two alternative rules would have resulted in a different outcome. Practically, this decision meant that the two co-ops that voted in April 2002 (ten months before the decision of the Board) won approval while the two that had voted in June 2002 (eight months before the decision of the Board) were denied. We argue that the decision to approve conversion was random in nature, since (i) the dates of the vote where within two months of each other, (ii) Stopplag was not even being discussed when the co-ops first registered in June 2001 and therefore could not have been anticipated, (iii) any other rule applied by the Board would have resulted in a different outcome, and (iv) the number of townhouse apartments was essentially the same in each co-op. The transfer of the property title for the buildings that gained approval took place at the end of May in 2003.

Table A.3: Akalla Coop Conversions

	Panel A: Property Details								
Property	built	sqm comm	sqm apts	apt units	1/2	3	4	4 TH	5 TH
Nystad 5	1976	228	6055	77	1	50	10	16	0
Sveaborg 5	1976	227	6775	87	1	60	10	16	0
Sveaborg 4	1976	254	10321	133	0	103	13	16	1
Nystad 2	1976	97	7204	95	8	65	10	12	0
	Panel B: Conversion Process								
Property	registration	contact	appraisal	vote	vote %	accepted	County	decision	transfer
Nystad 5	16-May-01	14-Jun-01	24-Sep-01	21-Apr-02	67.9%	9-Sep-02	21-Feb-03	approval	26-May-03
Sveaborg 5	27-Sep-00	28-Jun-01	14-Sep-01	21-Apr-02	73.6%	9-Sep-02	21-Feb-03	approval	27-May-03
Sveaborg 4	27-Sep-00	26-Sep-01	5-Nov-01	17-Jun-02	68.6%	9-Sep-02	21-Feb-03	denial	– –
Nystad 2	17-Jul-01	1-Oct-01	5-Nov-01	19-Jun-02	70.5%	5-Sep-02	21-Feb-03	denial	– –

Notes: The table reports property characteristics (Panel A) and details on the co-op conversion process (Panel B) for the four buildings in the Akalla sample. Nystad 5 is located at Borgagatan 2-44, Sveaborg 5 is located at Nystadsgatan 2-46, Sveaborg 4 is located at Saimagatan 1-53, and Nystad 2 is located at Nystadsgatan 1-39. Panel A reports the name of the co-op, the name of the property, the address of the property, the year of construction, the total square meters of commercial space, the total square meters of apartments, the number of apartment units, and a breakdown of the number of apartments into 1- or 2-room, 3-room, 4-room, 4-room townhouse (TH), and 5-room TH units. Panel B lists the date of registration of the co-op, the date of initial contact between the co-op and the landlord (initiation of the conversion process), the date of appraisal, the date of the vote of the tenant association to approve the conversion, the fraction of votes that voted for conversion, the date the landlord approved the sale conditional on District approval, the date of the District approval decision, and the actual decision, and finally the date of the transfer of the property (closing) from the landlord to the co-op (for the approved conversions only).

B Model

B.1 The landlord's pricing policy

The political directive of the municipality of Stockholm is to set the conversion price equal to the net present value of the operational profit of the landlord. Let ω_t denote the rent and ϕ_t the operating expenses. The landlord's asking price is given by:

$$(1-\tau)P_0 = \sum_{t=0}^{\infty} \omega_t R^{-t} - \sum_{t=0}^{\infty} \phi_t R^{-t} \tag{10}$$

where P_0 denotes the co-op market price and τ denotes the discount offered to the tenants relative to that market price.

B.2 The household's consolidated budget constraint

Consider a household that lives from $t = 0$ to $t = T \leq \infty$. The household can save and borrow in the asset a_t with rate of return r and where $R = (1 + r)$. Every period the household receives income y_t. The household consumes non-housing consumption c_t.

B.2.1 The renter's consolidated budget constraint

If a household does not partake in a privatization, it remains in its apartment from period 1 to period T. Denote a renter's consumption series by c_t^r. The period-0 budget constraint reads:

$$c_0^r + a_0 + \omega_0 = y_0 + \hat{a} \tag{11}$$

where $\hat{a}$ denotes net wealth in the beginning of period 0 and where ω_0 denotes the rent paid to the landlord. The intertemporal budget constraint from $t = 1$ to $t = T - 1$ reads:

$$c_t^r + a_t + \omega_t = y_t + a_{t-1}R \tag{12}$$

An equivalent formulation is:

$$a_{t-1} = (c_t^r + a_t + \omega_t - y_t)R^{-1} \tag{13}$$

In the terminal period T the renter cannot leave any debt, so that $a_T = 0$. Hence, the terminal period's budget constraint reads:

$$a_{T-1} = (c_T^r + \omega_T - y_T)R^{-1} \tag{14}$$

Consolidating the one-period budget constraints (11) and (13), one obtains:

$$\sum_{t=0}^{T-1} c_t^r R^{-t} + \sum_{t=0}^{T-1} \omega_t R^{-t} + a_{T-1} R^{-T+1} = \sum_{t=0}^{T-1} y_t R^{-t} + \hat{a} \tag{15}$$

Substituting (14) into (15) delivers:

$$\sum_{t=0}^{T} c_t^r R^{-t} + \sum_{t=0}^{T} \omega_t R^{-t} = \sum_{t=0}^{T} y_t R^{-t} + \hat{a} \tag{16}$$

where $\sum_{t=0}^{T} \omega_t R^{-t}$ denotes the net present value of rents.

B.2.2 The homeowner's consolidated budget constraint

Let P_0 denote the price of the apartment at $t = 0$ on the private co-op market. The conversion price is $(1-\tau)P_0$. Conditional on privatizing in year 0, the budget constraint reads:

$$c_0^o + \phi_0 + a_0 + (1-\tau)P_0 = y_0 + \hat{a} \tag{17}$$

where $\hat{a}$ denotes net wealth in the beginning of period 0 and ϕ_0 denotes the co-op fee, which the owner pays to the co-op to pay for operational expenditures. Homeowner consumption is denoted by c_t^o.

From period 1 to T, the household remains in its housing unit as a homeowner. The budget constraint reads:

$$c_t^o + \phi_t + a_t = y_t + a_{t-1} R \tag{18}$$

An equivalent formulation is:

$$a_{t-1} = (c_t^o + \phi_t + a_t - y_t) R^{-1} \tag{19}$$

The homeowner consumes and pays the co-op fee for the last time in T. The household sells its apartment in the beginning of $T+1$ at price P_{T+1}. The terminal condition is $a_T = 0$ ($a_T > 0$ is sub-optimal, $a_T < 0$ is not allowed). Hence, the terminal budget constraint reads:

$$a_{T-1} = (c_T^o + \phi_T - y_T - P_{T+1} R^{-1}) R^{-1}, \tag{20}$$

where the proceeds from the apartment sale is discounted to T.

Consolidating intra-temporal budget constraints (17) and (19), we obtain:

$$(1-\tau)P_0 + \sum_{t=0}^{T-1} c_t^o R^{-t} + \sum_{t=0}^{T-1} \phi_t R^{-t} + a_{T-1} R^{-T+1} = \sum_{t=0}^{T-1} y_t R^{-t} + \hat{a} \tag{21}$$

Substituting (20) into (21):

$$(1-\tau)P_0 + \sum_{t=0}^{T-1} c_t^o R^{-t} + \sum_{t=0}^{T-1} \phi_t R^{-t} + (c_T^o + \phi_T - y_T - P_{T+1}R^{-1})R^{-T} = \sum_{t=0}^{T-1} y_t R^{-t} + \hat{a} \tag{22}$$

Rearranging:

$$(1-\tau)P_0 + \sum_{t=0}^{T} c_t^o R^{-t} + \sum_{t=0}^{T} \phi_t R^{-t} = \sum_{t=0}^{T} y_t R^{-t} + \hat{a} + P_{T+1}R^{-T-1} \tag{23}$$

B.2.3 Defining the wealth shock

The wealth shock, W_0, is the difference between the present discounted value of consumption for a household that participates in the privatization and becomes a homeowner in year 0 and for that same household that is denied privatization and rents.

$$W_0 = \sum_{t=0}^{T} c_t^o R^{-t} - \sum_{t=0}^{T} c_t^r R^{-t}$$

Subtracting equation (16) from (23) we obtain:

$$(1-\tau)P_0 + \sum_{t=0}^{T} c_t^o R^{-t} - \sum_{t=0}^{T} c_t^r R^{-t} + \sum_{t=0}^{T} \phi_t R^{-t} - \sum_{t=0}^{T} \omega_t R^{-t} = \sum_{t=0}^{T} y_t R^{-t} - \sum_{t=0}^{T} y_t R^{-t} + P_{T+1}R^{-T-1} \tag{24}$$

Under the (empirically validated) assumption that income is unaffected by treatment, the difference simplifies to:

$$(1-\tau)P_0 + \sum_{t=0}^{T} c_t^o R^{-t} - \sum_{t=0}^{T} c_t^r R^{-t} + \sum_{t=0}^{T} \phi_t R^{-t} - \sum_{t=0}^{T} \omega_t R^{-t} = P_{T+1}R^{-T-1} \tag{25}$$

Hence, the wealth shock equals:

$$W_0 = P_{T+1}R^{-T-1} - (1-\tau)P_0 + \sum_{t=0}^{T} (\omega_t - \phi_t)R^{-t} \tag{26}$$

Imposing the pricing policy Substituting the pricing policy of the landlord, as given by equation (10), into (26), we obtain:

$$W_0 = P_{T+1}R^{-T-1} - \sum_{t=0}^{\infty} (\omega_t - \phi_t)R^{-t} + \sum_{t=0}^{T} (\omega_t - \phi_t)R^{-t}, \tag{27}$$

which simplifies to:

$$W_0 = P_{T+1}R^{-T-1} - \sum_{t=T+1}^{\infty} (\omega_t - \phi_t)R^{-t} \tag{28}$$

Put differently,

$$W_0 = R^{-T-1}(P_{T+1} - \sum_{t=T+1}^{\infty} (\omega_t - \phi_t)R^{T+1-t}) \tag{29}$$

Rearranging once more:

$$W_0 = R^{-T-1}(P_{T+1} + \sum_{t=T+1}^{\infty} \phi_t R^{T+1-t} - \sum_{t=T+1}^{\infty} \omega_t R^{T+1-t}) \tag{30}$$

which is the difference between the cost of owning and the cost of renting from period $T+1$ onwards, discounted back to today. Equation (30) says that the wealth shock today depends on the evolution of the rent control system. If it were announced at time 0 that the rent control system would be abolished at time T, the wealth shock W_0 would be 0 (by equation (10) applied at $T+1$ with $\tau = 0$).

Assuming that the rent regulation remains in place until $T+1$, we can evaluate equation (10) at time $T+1$ and compute its present value as of $t = 0$, and then substitute into (30):

$$W_0 = R^{-T-1}\left(P_{T+1} - (1-\tau)P_{T+1}\right), \tag{31}$$

or

$$W_0 = \tau P_{T+1}R^{-T-1}. \tag{32}$$

The wealth shock is the wedge between the value of the building in the private market and in the hands of the municipal landlord at time $T+1$, discounted back to time 0. The reason that the wealth shock is lower than the wedge measured at time zero is that the renter enjoys discounted rents, which are lower than the market rents (as implied by the market price), and are subtracted out.

Imposing a house price process House prices evolve as follows:

$$P_{t+1} = P_t R_h, \tag{33}$$

where $R_h = 1 + r_h$. From period 0 to $T+1$, house prices appreciates as follows:

$$P_{T+1} = P_0 R_h^{T+1}, \tag{34}$$

implying that equation (32) can be written as:

$$W_0 = \tau P_0 \left(\frac{R_h}{R}\right)^{T+1}. \tag{35}$$

This equation illustrates that there is cross-sectional variation to be explored for τ at the co-op level and for T at the household level.

B.3 Solving for the spending response

We assume that the household has decreasing marginal utility and that its subjective discount factor, β, is equal to R^{-1}. This ensures that households desire to hold per-period consumption constant over time. Renters' consumption is $c_t^r = c^r$ and homeowners' consumption is $c_t^o = c^o$. Then the wealth shock

$$\begin{aligned} W_0 &= (c^o - c^r)\sum_{t=0}^{T} R^{-t} \\ &= (c^o - c^r)\sum_{t=0}^{T} \frac{1}{(1+r)^t} \\ &= (c^o - c^r)\frac{1+r}{r}\left(1 - \frac{1}{(1+r)^{T+1}}\right) \end{aligned} \tag{36}$$

So the consumption response is the annuity value of the wealth shock

$$c^o - c^r = W_0 \left(\frac{r}{1+r}\right)\left(1 - \frac{1}{(1+r)^{T+1}}\right)^{-1}. \tag{37}$$

Substituting in equation (35) we see that the consumption response depends on four factors:

$$c^o - c^r = \left(\frac{r}{1+r}\right)\left(1 - \frac{1}{(1+r)^{T+1}}\right)^{-1} \tau P_0 \left(\frac{R_h}{R}\right)^{T+1}. \tag{38}$$

The first factor is the perpetuity factor $r/(1+r)$. The second factor is an adjustment of the first factor for a finite horizon of T years. That is, the annuity factor for T years is the product of the first two factors. The third factor is the landlord's discount relative to the co-op market price. The fourth factor is an adjustment taking into account that the household is entitled to regulated rents for the next T years. This is equation (4) in the main text. We define the product of the first three terms as $\widetilde{W}$.

B.4 Numerical Example

To solve for the consumption levels of renters and homeowners, we make additional assumptions. The operationalize the model, we choose parameter values as detailed below.

B.4.1 Assumptions

Assume that rent to price and net rent to price ratios are constant, implying that $\omega_t = \omega P_t^h h_t$ and $\phi_t = \phi P_t^h h_t$, where P_t^h is the price per square meter and h_t the number of square meters. Assume that the household stays in its current apartment ($h_t = h_0,\ \forall t \in \{0, \cdots, T\}$) and that the household has constant income, $y_t = y$ over the same period.

Renters' consumption can be computed from (16). After some algebra, we get:

$$c^r = y + \left(\frac{r}{1+r}\right)\left(1 - \frac{1}{(1+r)^{T+1}}\right)^{-1}\left[\hat{a} - \omega P_0^h h_0 \frac{1 - \left(\frac{R_h}{R}\right)^{T+1}}{1 - \left(\frac{R_h}{R}\right)}\right] \tag{39}$$

Owners' consumption can be computed from (23). It follows that:

$$c^o = y + \left(\frac{r}{1+r}\right)\left(1 - \frac{1}{(1+r)^{T+1}}\right)^{-1}\left[\hat{a} - \phi P_0^h h_0 \frac{1 - \left(\frac{R_h}{R}\right)^{T+1}}{1 - \left(\frac{R_h}{R}\right)}\right] + P_0^h h_0 \left(\frac{R_h}{R}\right)^{T+1} - (1-\tau)P_0^h h_0 \tag{40}$$

B.4.2 Calibration

We calibrate the model as reported in Table A.4. The return on financial wealth, house price growth, the net rental yield, and the net rent to price ratio are consistent with the post-1950s and post-1980s equity return and net rental yield values for Sweden reported in Jordà et al. (2019). The values for disposable income, apartment values, and initial financial wealth are consistent with averages reported in Table 1 for our quasi-experiment.

Disposable income, apartment value, and initial financial wealth are only required to solve for the level of the renter's and homeowner's consumption (equations (39) and 40).

Figure A.3 illustrates the model implications based on equation (38). Panel (a) shows spending responses for four different time horizons T, which can be thought of as four different households leaving the Stockholm housing market after differing lengths of residency. Spending responses are stronger for shorter T. Panel (b) shows the spending response out of the wealth shock, $\widetilde{W}$, which is household- and building-specific. The response coefficient is the perpetuity factor $r/(1+r)$. In our empirical work, we exploit cross-sectional variation in $\widetilde{W}$ to disentangle the effect of home

Table A.4: Model parameters

	Notation	Value
Gross return on financial wealth	R	1.0700
Subjective discount factor	β	0.9346
House price growth	R_h	1.0200
Rent to price ratio	ω	0.0678
Maintenance to price ratio	ϕ	0.0210
Net rent to price ratio	$\omega - \phi$	0.0468
Time horizon	T	40.00
Fractional discount	τ	0.4600
Disposable income	y	163.87
Apartment value on co-op market	$P_0^h h_0$	1353.45
Initial financial wealth	$\hat{a}$	85.43

Notes: The table presents the parameter values of the model. Values for the last three rows are in SEK 1,000s.

ownership from the wealth shock.

B.5 Equilibrium net rental yield

This section derives a parameterized expression for $\left(\frac{R_h}{R}\right)$ using the first order conditions of owner and renter. We use this expression in equation (35), as well as in the calibration of the model. We assume a per-period utility function $u(c_t, h_t)$ and that households live until $T \leq \infty$. Let u_c denote the partial derivative with respect to the first argument and u_h the partial derivate with respect to the second argument.

The renter's problem In the renter's budget constraint ω is redefined as the rent-to-price ratio. The budget constraint reads:

$$c_t + a_t + \omega P_t^h h_t = y_t + a_{t-1} R \tag{41}$$

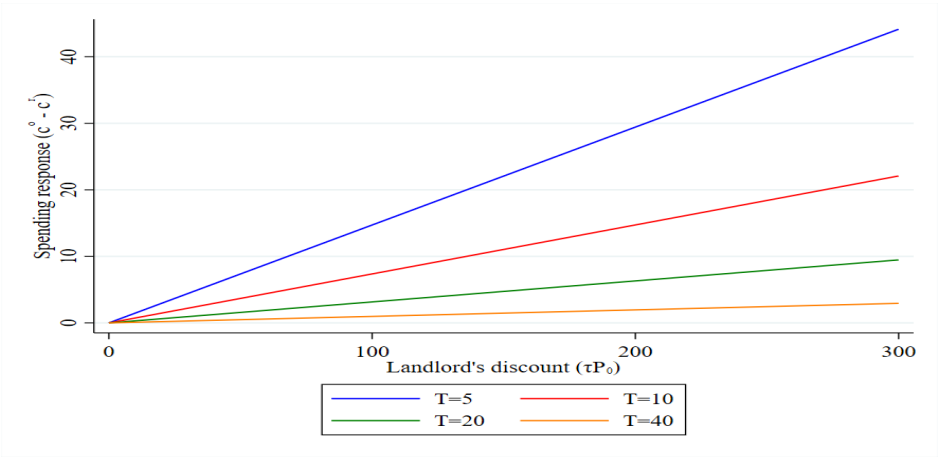
with $a_T = 0$ so that the budget constraint is analogous to equations (11)-(14).

The renter's problem is:

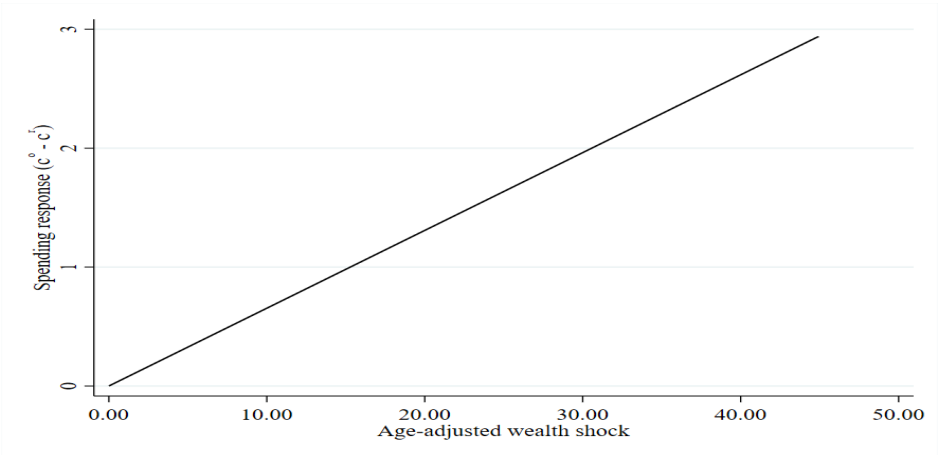
$$\max_{c_t, a_t, h_t} \sum_{t=0}^{T} \beta^t u(c_t, h_t)$$

subject to (41).

Figure A.3: Model implications



(a) Spending response as a function of the discount



(b) Spending response as a function of the wealth shock

Notes: Panel (a) depicts the spending response ($c^o - c^r$) as a function of the landlord's discount, τP_0, for four different time horizons, T. Panel (b) depicts the spending response as a function of thewealth shock, $\widetilde{W}$. The slope of that line is $r/(1+r)$.

The first-order condition with respect to h_t is:

$$\frac{u_h^t}{u_c^t} = \omega P_t^h. \tag{42}$$

Notice that for the renter this optimally condition is static. The first-order condition with respect to a_t is the standard Euler equation:

$$R^{-1} = \beta \frac{u_c^{t+1}}{u_c^t} \tag{43}$$

The homeowner's problem Let us assume that the homeowner optimizes housing consumption every period so that the first-order condition always holds. That is, the homeowner purchases and

sells housing every period. Let ϕ define the maintenance-to-price ratio. Let R^h define house price appreciation, $P_t^h = P_{t-1}^h R^h$. The budget constraint of the homeowner reads:

$$c_t + a_t + (1+\phi)P_t^h h_t = y_t + a_{t-1}R + P_{t-1}^h h_{t-1} R^h \tag{44}$$

There are no transaction costs so one can think of the owner as buying its house every period and paying one period of maintenance ($\phi P_t^h h_t$) upfront. In the next period the house is sold at $P_{t-1}^h h_{t-1} R^h = P_t^h h_{t-1}$. Equation (44) is the analogue to equations (17)-(20) aside from the housing terms which appear every period and which is going to be a choice variable.

The homeowner's problem is:

$$\max_{c_t, a_t, h_t} \sum_{t=0}^{T} \beta^t u(c_t, h_t)$$

subject to (44).

The first-order condition w.r.t. h_t is

$$P_t^h = \frac{u_h^t}{u_c^t} - \phi P_t^h + \beta \frac{u_c^{t+1}}{u_c^t} P_{t+1}^h. \tag{45}$$

If owner and renter agree on the (shadow) value of housing services, equation (42) can be substituted into (45) to obtain:

$$1 = \omega - \phi + \beta \frac{u_c^{t+1}}{u_c^t} R^h \tag{46}$$

The homeowner's first-order condition w.r.t. to a_t is identical to equation (43). Substitute (43) into (46) and rearrange:

$$\frac{R^h}{R} = 1 + \phi - \omega = 1 - (\omega - \phi) \tag{47}$$

This equation provides a parameterized expression for $\frac{R^h}{R}$. It is determined by the wedge between the rent and the operating cost, $\omega - \phi$. Notice that it should be interpreted as the net rental yield.

C Construction of variables

This appendix describes construction of all variables except for housing wealth, $dHousing$, and the buffer.

Demographics For each tenant, we obtain data on age, gender, number of children, total family size, marital status, and location. The *Age* of the household is the age of the oldest adult in the household. We limit our sample to households whose *Age* is less than 65 in RY(-1). *Partner* takes on the value of one for married individuals, those with registered partnerships, and for unmarried couples with a child.

Income We consider two different income concepts. *Labincind* measures a household's labor income per adult. It is a comprehensive measure of all income derived from work: wages, salaries, income from sole proprietorships and active business activity, unemployment benefits, and employer-provided benefits such as a company car, sick leave, and continued education. *Numwork* is the number of adults in the workforce. *Labinchh* is total household income, the product of the labor income per adult (intensive margin) and the number of working adults (extensive margin). Our second income variable *Income* is a broader measure of income that enters the household budget constraint; it is after-tax. The construction procedure for *Income* is described below in Section C.1.

Debt We observe total household-level debt, *Debt*. We have no separate information on mortgage debt. Mortgage debt accounts for 2/3 of total household debt in Sweden in the 2002-04 period according to the Riksbank's 2004 Financial Stability Report. *Interest* is the interest paid on *Debt*. When a household participates in a co-op conversion, buys her apartment and increases debt to do so, the increase in housing wealth and in debt does not always occur in the same year. This timing issue occurs when the real estate transaction occurs around year-end. Appendix C.2 describes our algorithm for adjusting the timing of debt, and it describes how exactly we construct the variable $dDebt$.

Financial wealth A unique feature of the Swedish data is the granular financial asset information. We have information for every stock, mutual fund, and money market fund for every individual in our sample. We also have information on the total value invested in bonds for each individual. End-of-year values of each asset are reported administratively (not self-reported) for the computation of the wealth tax. Because the wealth tax was abolished starting in 2008, we end our sample in 2007. We label the sum of these risky financial assets *Risky*. Financial wealth *Financial* contains four more components: *Nonhouse*, *Bank*, *CapIns*, and *Pension*. *Bank* is the balance of all bank accounts. Reporting requirements on bank accounts vary over time, and are based on interest income earned for the period from 1999 to 2005 and based on bank balances in 2006–07.

Appendix C.3 provides more detail on our bank account imputation procedure, which further improves on Calvet, Campbell and Sodini (2007). For the capital insurance accounts, we observe the year-end balance but not the asset mix. We assume a 50-50 mix of equity and bonds. For pension accounts, we observe contributions made in the year. Withdrawals are included in $Income$.

Changes in risky assets *dRisky* measure only active changes. For each asset, we take the invested amount at the end of the prior tax year and apply the cum-dividend return over the course of the current tax year. Constructing $dRisky$ requires collecting price appreciation and dividend data on thousands of individual financial assets. For bonds, we do not have such price information, and we apply a (cum-coupon) bond index return to the individual bond positions to calculate the passive value. If the value at the end of the current tax year deviates from the passive value, we label the difference an active change. We aggregate active changes across all risky assets in *dRisky*. Like for real estate, this ensures that unrealized gains and losses do not affect the change-in-wealth measure and therefore consumption. The change in financial wealth *dFin* is the sum of *dRisky*, *dBank*, *dCapIns*, *dPension*, and *dNonhouse*. A positive value for *dFin* measures household savings, while a negative value measures dissaving.

Appendix D describes how we construct $dHousing$.

Consumption As explained below, the wealth and income data are so comprehensive and precisely measured that they allow us to compute high-quality measures of household-level consumption spending, a rarity in this literature that usually relies on proxies for consumption (car or credit card purchases) or on survey-based measures of consumption. Consumption is measured as the right-hand side of the budget constraint:

$$Cons = dDebt - dHousing - dFin + Income \tag{48}$$

Consumption is high when households increase borrowing, sell housing or financial assets, or earn high income, all else equal. A purchase of an apartment which is fully funded with a mortgage has no implications for consumption. We define *Savings* as *Income* minus *Cons*.

Our consumption measure is a measure of total annual spending. As such, it includes outlays on durables rather than the service component from durable spending. The method does not allow us to break down consumption any further into its subcategories. Appendix J compares our consumption measure to that in the Swedish household budget survey. It also discusses a breakdown of consumption categories for households who newly purchased an apartment. Koijen, Van Nieuwerburgh and Vestman (2014) discuss the benefits and drawbacks of registry-based consumption data and compare registry-based consumption to standard survey-based consumption for the same set of households. Four minor sources of measurement error are: imputation of apartment wealth for stayers, measurement issues with bank accounts, coarse imputation of returns on bonds based on a bond index, and lack of knowledge of the exact asset mix of the capital

insurance accounts.

The rest of this appendix describes in detail how $dDebt$, $dFin$, and $Income$ are constructed.

C.1 Construction of Income

Disposable income includes interest income from fixed income securities, dividend income from stocks and mutual funds, rental income from properties, as well as realized capital gains from the sale of financial assets and real estate properties. Since financial income and capital gains are part of our measure of financial wealth we subtract them from disposable income to avoid double counting these items. From disposable income we also deduct net increases in student loans, which are part of the change in debt. The tax values for each of these types of income are also reported separately and are added back in the calculation. We are left with a broad measure of mostly labor income after taxes and transfers, which we call $Income$. Consumption increases with $Income$.

C.2 Construction of dDebt

The debt level is observed in the wealth registry for all individuals and at the end of each year. Debt refers to student loans, mortgages and consumer loans. Consumption increases with a positive change in debt (when an individual borrows more) and decreases with a negative change in debt (when loans are paid off).

Simple debt change for the current year is calculated as the difference between the level of debt at the end of the current year and the value at the end of the previous year; call this variable dD. The variable $dDebt$ is constructed as:

$$dDebt = dD - Interest + \underbrace{0.7 \times Interest \times Adjfactor}_{after-tax\ mortgage\ interest} \tag{49}$$

Prior to the treatment year RY0, the adjustment factor $Adjfactor = 0$. That is, the amount of interest paid on loans is subtracted from the simple debt change to obtain dDebt. Conceptually, this prevents interest payments made for past (durable) expenditures to be counted as consumption in the current year. Since our consumption measure is a total expenditure measure, we account for the (durable) expenditures fully in the year of the outlay. Including the interest expense on the debt would lead one to overstate the true consumption expenditure. On SCB's server, interest expenses are not available for years 2001 and 2002. In this case we calculate the average interest rate individuals paid for their loans in 2000 and 2003 and we apply this rate to the debt levels in 2001 and 2002.

After the treatment year RY0, we proceed the same way ($Adjfactor = 0$) as long as a household is not a home owner. For households that become homeowners after RY0, things become

more complicated. For housing, we want to measure the service flow of owned housing because we do not want to treat renters and owners asymmetrically. Failing to capture this service flow would systematically understate consumption for homeowners and thus create mechanical effects in the measurement of consumption for the treatment versus the control groups. Our consumption measure automatically includes housing consumption for renters (rent payments). If we do not include the mortgage interest expense for owners, total consumption for owners would only reflect part of housing consumption, namely home maintenance expenses and co-op fees. Therefore, for all household-year observations after RY0 in which a household in the treatment or in the control group is a home owner, we add back the mortgage interest debt service. This ensures that this component of housing consumption for owners is included in *Consumption*. A complication is that we only see total debt, which is the sum or mortgage debt, student loans, and consumer loans. We proxy the share of mortgage debt in total debt as $Adjfactor = [Debt_k - Debt_{-1}]/Debt_k$, where $k \geq 0$, and apply this mortgage share to the total interest expense to proxy for the mortgage interest expense. A final detail is that we only want to add back 70% of the mortgage interest expense since 30% of the mortgage interest expense can be deducted from income for tax purposes. A similar approach is followed by Eika, Mogstad and Vestad (2017).

Adjusting Timing of Debt around Housing Transaction For the cases when we modify the timing of residential real estate acquisition in order for it to match a sale during the current year, we employ a simple two-step *unaccounted cash minimization* algorithm in order to decide if a similar timing correction should be applied to the debt level. This algorithm is described below. We use the following notation:

- $UC_t =$ unaccounted cash at time t
- $dDebt_t = Debt_t - Debt_{t-1}$
- $dFin_t = Fin_t - Fin_{t-1}$ where Fin stands for financial wealth
- P_t^S = Price at which the apartment/house was Sold
- P_t^B = Price at which the apartment/house was Bought

Step 1. Compute the sum of absolute values of unaccounted cash during the current year and the next year, leaving the debt levels unchanged.

$$\begin{aligned} UC_t &= dDebt_t - dFin_t + P_t^S - P_{t+1}^B \\ UC_{t+1} &= dDebt_{t+1} - dFin_{t+1} \\ A_1 &= abs(UC_t) + abs(UC_{t+1}) \end{aligned}$$

Step 2. Compute the sum of absolute values of unaccounted cash during the current year and the next year, after moving the debt level of the next year to the end of the current year.

$$\begin{aligned} UC_t &= dDebt_t + dDebt_{t+1} - dFin_t + P_t^S - P_{t+1}^B \\ UC_{t+1} &= -dFin_{t+1} \\ A_2 &= abs(UC_t) + abs(UC_{t+1}) \end{aligned}$$

Compare A_1 and A_2 and decide:

- If $A_2 < A_1$, move the debt level from the end of the next year (t+1) to the end of the current year (t).
- Else, leave the debt where it is.
- If the debt level is moved backwards, when imputing consumption for the next year (t+1) the change in debt will be overwritten to zero.

C.3 Construction of dFin

The change in financial wealth is the sum of changes in the risky portfolio, capital insurance accounts, non-residential real estate, and imputed bank accounts, plus contributions made to pension accounts.

The yearly change in the *risky asset portfolio* is calculated as the sum of active changes in the stocks, mutual funds, Swedish money market funds and bonds individual portfolios. End of year holdings are observable and thus we construct a measure that only considers active rebalancing of these portfolios.

We treat stocks, mutual funds and Swedish money market funds separately and we calculate the current year return of each portfolio based on the holdings at the end of the previous year. The active change is thus calculated as the difference between the portfolio's value at the end of year and last year's value multiplied by the weighted portfolio return, or:

$$Pv_t - Pv_{t-1} R_{holdings\, in\, t-1,t}$$

where Pv is the portfolio value and $R_{holdings\, in\, t-1,t}$ is the cum-dividend portfolio return calculated using last year's asset weights. If an asset does not have prices during the next year (i.e. delisting, mergers), we assume that the asset value is distributed proportionally to the other assets in the portfolio and the weights are scaled accordingly.

For the portfolio of bonds, we replace the return from the holdings with the return of a one year bond index. This return is cum-dividend, that is, inclusive of coupon income.

Finally, the total change in the risky asset portfolio is calculated as the sum of the active changes

in the stocks, mutual funds, money market funds and bonds portfolios. Consumption decreases when the change in risky assets is positive.

For *capital insurance accounts* we observe the end of year level of the account without knowing how the assets are allocated. We assume that the portfolio allocation is a 50-50 mix of bonds and stocks and we calculate the change in capital insurance accounts using benchmark Swedish bond market and equity market index returns.

Non-residential real estate consists of different kinds of property, such as farm houses, vacation homes, apartment buildings, real estate abroad, industrial real estate, agricultural real estate, land for own home, land for vacation home and real estate holdings classified as "other". For any given year in our sample period we can observe the market value for each of these kinds of property. The market value is imputed by Statistics Sweden and is calculated as the tax value $\times$ a regional factor which is based on transaction values in the region during the year.

We consider that a property is sold during the current year if it appears in the wealth registry with zero market value and the market value at the end of the previous year was positive. Alternatively, a property is bought if its market value in the current year is positive, while its corresponding value was zero in the previous year. Thus, the change in real estate wealth for a type of property can be equal to either the market value of the current year in the case of an acquisition, or to minus last year's value in the case of a sale. To identify transactions each kind of property is tracked by itself from year to year. Thereafter, we sum the market values of all kinds to obtain the total change in non-residential real estate:

$$dNonhouse = \sum_j Hnr_{j,t} - Hnr_{j,t-1},\ \textit{if}\ Hnr_{j,t} = 0\ \textit{or}\ Hnr_{j,t-1} = 0$$

where $Hnr_{j,t}$ is the market value of non-residential real estate type j at time t.

Change in bank accounts. We observe the total amount individuals have in their bank accounts at the end of the year when this amount exceeds a certain level. For years 1999 to 2005, bank accounts are reported if the earned interest is greater than 100 SEK, while for years 2006 and 2007 they are reported if the total balance of an account is greater than 10,000 SEK. The change in 2006 results in significantly more visible accounts. If the level or interest earned condition is not met, the observed balance is zero. In these cases we use an improved version of the bank account imputation procedure developed first by Calvet, Campbell and Sodini (2007), which relies on the subsample of individuals for which we observe the bank account balance.

We start by dropping the extra bank accounts that become visible in 2006 after the regulation change in order to have a consistent imputation across all years (i.e. we drop visible accounts that earn less than 100 SEK interest). We regress the log bank account balance on the following characteristics: log of financial assets other than bank account balances and Swedish money market funds, log of Swedish money market fund holdings, log of residential real estate, log of non-residential real estate, household size, log of debt, square of log debt, disposable income decile

dummies, parish decile dummies ranked on average disposable income, 5-year wide age group dummies, education level dummies and a series of demographics dummies such as married man, married woman, single individual, single father and single mother.

We use the regression to estimate the account balances of each individual. In this procedure, we adjust the intercept of the imputation regression so that the average value of observed and imputed bank account balances in our population matches the average bank account balance of the household sector reported by Statistics Sweden.

The yearly change in bank accounts is calculated as the difference between the balance at the end of the current year and the balance at the end of the previous years. Consumption decreases with the change in bank accounts.

D Construction of Housing Wealth and $dHousing$

From the Wealth Registry (Förmögenhetsregistret) data, we observe the market value of apartments, single-family houses, second homes, investment properties, and commercial real estate. We define the variable housing wealth as the sum of the apartment and single-family housing wealth. It only contains properties that are intented for permanent residency according to the tax authority (Skatteverket), that is co-op apartments and single-family houses (småhus). All additional residential or commercial real estate is part of financial wealth. Because the value of owned apartments is imputed by Statistics Sweden with substantial measurement error, we construct apartment wealth using our own methodology, as described below.

D.1 Apartment wealth

We proceed in two stages for each apartment: identification of apartment ownership and the corresponding apartment value. We use a different methodology depending on whether the apartment has been sold by 2017, or not.

D.1.1 Valuation of apartments prior to the last sale

We are able to identify the ownership of apartments whose sales appear in the tax form for sale of a co-op apartment (Överlåtelse av bostadsrätt, KU55). The KU55 data record the seller, the organization number of the co-op, and the date, sale price, prior purchase price, ownership share, and transaction type of both the prior acquisition and the current sale. Conditional upon a sale, we know the exact period during which the seller owned the apartment. We can also tie the apartment to its co-op, including the 13 treated co-ops. To impute the corresponding apartment value, we first use the price and ownership share information to calculate the value of the apartment at date of purchase and at date of sale. Second, we use the apartment price index at the most detailed geographical level available (either county, municipality or parish; see Appendix H) to impute the value of the apartment after the purchase date and prior to the sale date.

D.1.2 Valuation of apartments never transacted or after the last sale

Identifying ownership We identify apartment ownership after the apartment has been last sold in KU55 (or if the apartment never appears in KU55) utilizing information from the Total Population Register (Register över totalbefolkningen, RTB, 1999-2017), Apartment Registry (Lägenhetsregistret, 2012-2017), and the tax form for property values of co-op shares (Förmögenhetsvärde för bostadsrätt, KU56, 2003-2007). The Total Population Register contains the official address of each individual and a property identifier. The properties can be classified into different categories such as rental units, co-op ownership, and private ownership (Hyresrätt/Bostadsrätt/Äganderätt)

using the Apartment Registry. Thus, we have a list of individuals residing in co-op properties up to the end of 2017. We can identify owners and renters by using KU56, which only contains information on owners of apartments. We assume the ownership started from the first year that the individual was registered in the property. Finally, the individual ownership share is distributed according to the number of registered adults in the household (100 percent if single adult, 50 percent if two adults).

Valuation The value of ownership of unsold apartment is imputed using the apartment floor area in square meters multiplied by the apartment price per square meter at the smallest geographical area available (see Appendix H). Apartment size is retrieved from the Apartment Registry when available. For owners not observed in the Apartment Registry, we predict the apartment square meters using KU56, since this registry reports the co-op ownership share which is typically proportional to the size of the apartment, and we are able to retrieve the total square meters in the co-op association using the Apartment Registry.

D.1.3 Improvements for apartments in our privatized buildings

Improvements on classifying ownership For households in the 13 converted co-ops that did not sell their apartment before 2017 according to KU55, we use the residual tenants' classification methodology, see Appendix E.

Improvements on valuation In our sample we have additional information on each apartment's size (i.e, square meters) from the tenant lists obtained from landlords. We use this information together with every transaction in each building to calculate the price per square meter for each individual building. We use the mean square meter price per building in every year to value the apartments in our sample. For years when there are no transactions, we calculate the appreciation using the apartment price index for each parish to value the apartments (see Appendix H). This is how we obtain square meter prices in all years after the privatization, including relative year $k = 0$, denoted by p_0 in Section 4. The product of square meters and p_0 is denoted by HW_0. (Its model-equivalent is denoted by P_0.)

D.2 Construction of dHousing

Because of the detailed nature of the Swedish data, we are able to observe the real estate wealth of individuals in great detail. In order to construct an accurate measure of the change in real estate wealth, we include information on several types of properties taken from the Wealth Registry (Förmögenhetsregistret). These properties are grouped into properties intended for permanent residency (i.e, single-family houses and co-op apartments) and other real estate and are treated separately.

Consumption decreases with positive changes in real estate wealth (acquisitions) and increases with negative changes in real estate (sales). dHousing includes houses and apartments and is the sum of dHouse and dApartments. Other forms of real estate are included in dFin.

D.2.1 dHouse

In order to calculate the change in wealth invested in houses, we turn to the Wealth Registry. We observe the total imputed market value of an individual's owned single-family houses. We also observe the geographic location (municipality) of upto two single-family houses for each individual in our sample.

We define a house as acquired if housing wealth changes from zero in the past year to a positive value at the end of the current year, and the opposite in the case of a sale. When housing wealth is positive both at the end of current and the prior year, we consider there to be an acquisition of housing wealth during the year if the number of houses owned increases, and vice versa. In addition, we consider there to be both an acquisition and a sale during the year if the number of house(s) owned is the same but the municipality of at least one of the housing properties changes. Similarly, we identify transactions when we observe people moving to a different property in the Total Population Register, even though they have positive wealth in residential house(s) located in the same municipality in the current and the prior year. This special case captures acquisitions and sales when house owners move within the municipality. In such scenarios of both a sale and a new purchase, we assume that the houses are sold at last year's market value and the houses are bought at the market value at the end of the current year. The change in house real estate, dHouse, is defined as the difference between the value in the current year and the past year.

When we do not observe housing transactions, either because homeowners continue to own or renters continue to rent during the year, dHouse is zero.

D.2.2 dApartments

We use values from KU55 for the acquisitions and sales of apartments sold before the end of 2017 as described in section D.1. We only include arm's length transactions in dApartment, i.e. entries in KU55 in which individuals transfer their entire ownership share of an apartment, thus excluding donations, transfers between spouses, and inheritances. For owners that did not sell their apartment before the end of 2017, we use the imputed apartment wealth value for the acquisition.

Improvements for apartments in our privatized buildings For apartments that belong to our privatized buildings we are especially careful to account for the cash-flow generated by the privatization itself. From KU55 tax forms of sellers, we calculate the conversion price per square meter, denoted by p_0^{ς}. We compute the median value for each building and apply it to every household.

E Classification of residual tenants

We use four pieces of information to classify residual tenants: co-ops' annual reports, co-ops' lists of residual tenants submitted to Statistics Sweden, the change in debt to income of a household, and information about households' sales of co-op apartments up until 2017.

From the co-ops' annual reports we collect the total number of apartments in the building, the number of apartments owned by co-op members, and the number of apartments that the co-op rents out. According to the earliest available reports just after the privatization and transfer of ownership title from the landlord to the co-op, there are 848 apartments of which 736 are owned by co-op members and 111 are rented out (i.e., 13 percent).[43]

We proceed in two main steps in the classification of purchasers and residual tenants, described in Sections E.1 and E.2.

E.1 Procedure to classify purchasers and residual tenants in a provisional sample

First, we construct a provisional sample of households that only is used to classify residual tenants. The sample represents the households whose official address coincides with the building address at the time of the transaction between the landlord and the co-op. That is, we select households that according to their recorded address live in the building at the time of the transaction.[44] For these households we construct the change in debt from the year before the transaction to the end of the transaction year. We scale the change by disposable income at the end of the transaction year to obtain a proxy for the change in debt-to-income. We rank households co-op by co-op by their change in debt-to-income, from high to low, and use the largest changes to provisionally classify the set of purchasers within each co-op. That is, we define a cut-off value such that the number of households above the cut-off (i.e., the number of households with large changes in debt-to-income) are equal to the number of apartments owned by co-op members according to the annual report. The cut-off is co-op-specific.

Table A.5 reports diagnostics at the co-op level based on this procedure. Two sources of information are used in the validation. First, six co-ops have supplied complete or partial lists of their residual tenants.[45] For these co-ops, we count how many households that are above the cut-off

[43] In 2 out of 13 instances the first annual report is more than one year later than the transaction year which makes the analysis less reliable (Bondesonen 21, Stencilen 2). In one instance, the building contains a hotel with studios rented out. The hotel was sold to the co-op as well but the tenants of the hotel were not allowed to buy their studio. We report statistics separately for this building.

[44] For households that appear in the tenant lists and hence are part of the fixed sample we form households according to the fixed sample. For other households we use Statistics Sweden's household identifier. Hence, there are more households in the sample than apartments as reported in the co-ops' annual reports.

[45] In one case (Nystad 5) the residual tenant list is contradictory to sales information in KU55. We take KU55 as the truth and modify the residual tenant lists accordingly. One Nystad 5 tenant observed in KU55 actually belongs to a HH in another building. This individual is excluded from the sample to avoid complexity. This means that one household fewer should be classified as a residual tenant compared to the annual report. In three cases (Korpen 13, Slalomsvängen 2, and Mullvaden Andra 30), the residual tenant lists are incomplete because the number of households are fewer than

value do not appear on the residual tenant lists (column (6)) and do appear (column (7)). In total, 207 households are above the cut-off and only 7 of them appear on the residual tenant lists. We also check how many households that are below the cut-off value appear on the residual tenant list (column (9)). There are 21 households of this kind. Hence only 7 out of 28 residual tenants are misclassified. We conclude that the procedure is accurate at classifying purchasers and residual tenants.

E.2 Classification of purchasers and residual tenants in the fixed sample

In a second step, we classify purchasers and residual tenants in the fixed sample. The fixed sample is based on the tenant lists provided by the landlords and date stamped around the time when the privatization attempts are initialized (i.e., when tenants contact the landlord and express interest in buying the building).

Since the fixed sample is formed 1–2 years before the transactions – and since not all of the households in the fixed sample remain in the buildings until the time of the transaction, it is impossible to know exactly how many residual tenants there are in it. Our method does however ensure that there are approximately as many residual tenants as reported in the annual reports and that all possible corrections have been made. Table A.6 summarizes the procedure.

As reported in column (1), there are in total 747 households in the fixed sample.[46] This represents 88 percent (747/848) of all apartments.

To classify households as purchasers or residual tenants in the fixed sample we proceed in the following steps. First, we sort households by change in debt-to-income ratio in the transaction year and apply the same cut-off values as in the provisional sample of the first step. At this stage, there are 139 households below the cut-off values compared to 111 residual tenants according to the co-ops' annual reports (column (3) versus column (4)).

We then apply five corrections:

1. We use the residual tenant lists and the KU55 forms to correct our classification. This reduces the number of residual tenants from 139 to 110 (column (4) versus column (5)).

2. For Reservoaren 2, we identify two late buyers that subsequently sold their co-op shares. Since the first annual report states that there are two residual tenants at the beginning we classify the late buyers as being the two residual tenants.

3. We notice that one household in Reservoaren 2, Reservoaren 4 and Mimer 9 have negative

the number of apartments rented out according to the annual reports.

[46] The fixed sample reported in Table A.6 includes 12 more households than the final sample in the analysis. These 12 additional households are not present in the final sample because their primary address is not equal the co-ops' address between 1999 and the household formation year.

(adjusted) disposable income but large increases in debt. We reclassify these three households as purchasers.

4. Finally, we correct our statistics in column (5) for households that move out of the building before the transaction year. For the purpose of comparison with annual reports they should neither be counted as purchasers or residual tenants.

5. For Reservoren 4, we notice that two classified residual tenant households have large decrease in financial wealth and bank account balance. They are reclassified as purchasers.

After these corrections we arrive at our final sample of residual tenants, reported in column (6). We classify 93 households as residual tenants in the fixed sample. Relative to the total number of residual tenants, this represents 84 percent of the number of all residual tenants (93/111). Hence, the shares of purchasers and residual tenants in the fixed sample are very close to the shares in the annual reports.

Table A.5: Summary statistics and response upon apartment purchase

Building names	Apartments (reports) (1)	Residual tenants (reports) (2)	Purchasers (reports) (3)	Cut-off value (4)	HHs above cut-off (5)	HHs above cut-off & not in residual tenant list (6)	HHs above cut-off & in residual tenant list (7)	HHs in sample that are on residual tenant list (8)	HHs below cut-off & in residual tenant list & not in KU55 (9)
Nystad 5*	77	16	60	0.21	59	58	1	11	10
Sveaborg 5*	88	12	76	0.00	76	71	5	11	6
NATTSLÄNDAN 3*	24	4	20	0.17	20	20	0	3	3
Total	189	32	156	-	155	149	6	25	19
Korpen 13**	31	3	28	-3336.00	28	27	1	1	0
Slalomsvängen 2**	30	6	24	1.17	24	24	0	2	2
Total	61	9	52	-	52	51	1	3	2
MULLVADEN ANDRA 30	25	8	17	0.12	17	-	-	-	-
ROSENDAL MINDRE 30	78	21	57	0.16	57	-	-	-	-
MIMER 3 / MIMER 9	59	8	51	0.00	51	-	-	-	-
RESERVOAREN 2	135	2	133	0.00	133	-	-	-	-
DUGGREGNET 8	79	12	67	0.09	67	-	-	-	-
Total	376	51	325	-	325	-	-	-	-
BONDESONEN 21***	30	0	30	0.25	30	-	-	-	-
Stencilen 2***	68	9	59	0.06	59	-	-	-	-
Total	98	9	89	-	89	-	-	-	-
RESERVOAREN 4****	124	10	114	0.60	114	-	-	-	-
Total	848	111	736	-	735	200	7	28	21

Notes: (*) Complete residual tenant lists. (**) Incomplete residual tenant lists. (***) First annual report of co-op is more than one year after the transaction. (****) Has about 100 studios in the form of a hotel.

Table A.6: Summary statistics for the fixed sample using the DTI method

Building name	Total households (1)	Apartments (reports) (2)	Residual tenants (reports) (3)	Classified residual tenants before corrections (4)	Classified residual tenants (5)	Classified residual tenants (additional corrections) (6)
Nystad 5*	71	77	16	22	9	9
Sveaborg 5*	80	88	12	9	11	11
NATTSLÄNDAN 3*	20	24	4	4	3	3
Total	171	189	32	35	23	23
Korpen 13**	29	31	3	0	1	1
Slalomsvängen 2**	28	30	6	11	8	7
Total	57	61	9	11	9	8
MULLVADEN ANDRA 30	21	25	8	6	6	5
ROSENDAL MINDRE 30	73	78	21	23	23	23
MIMER 3 / MIMER 9	46	59	8	6	6	3
RESERVOAREN 2	112	135	2	7	6	2
DUGGREGNET 8	74	79	12	11	10	8
Total	326	376	51	53	51	41
BONDESONEN 21***	28	30	0	4	4	3
Stencilen 2***	61	68	9	10	8	7
Total	89	98	9	14	12	10
RESERVOAREN 4****	104	124	10	30	18	11
Total	747	848	111	143	113	93

Notes: (*) Complete residual tenant lists. (**) Incomplete residual tenant lists. (***) First annual report of co-op is more than one year after the transaction. (****) Has about 100 studios in the form of a hotel.

F Household Formation

Our data set starts from the sample of all individuals who live in the co-ops of interest in the household formation year. The household, not the individual, is the relevant unit for consumption, housing, and savings decisions. Thus, we form households from the individual data. Household income, consumption, wealth, debt, etc. in a given year are aggregated up across all the household members in that year.

We primarily use Statistics Sweden's original family id variable (familjeidentitet) available in Total Population Register (Swedish: "Register över totalbefolkningen, RTB") to identify households. According to Statistics Sweden's definition, household composition is dynamically adjusted to account for four major life changes, both before and after the household formation year. First, children are added as they are born into a household. Second, if a grown child leaves the house and forms its own single or married household, we add a household to the sample and adjust the original household. Third, if a married couple divorces, two new households are formed each with a new household identifier. The old household unit is dropped starting in the year of the divorce. Fourth, if two singles marry or have a first child together, the single households are dropped from the sample in the years after marriage or child birth and a new married household is added in those years. This approach results in strictly more household observations in every year before and every year after the household formation year than in the household formation year itself.

Our sample of study starts from all households and their members in the household formation year and drops all household-year observations for households whose adult composition changes before or after the household formation year. In this fixed household subsample, no new households are added before or after the household formation year. In all years before and after that year, the number of households in the Fixed sample is strictly smaller than in the household formation year. The Fixed sample drops all singles who marry before the household formation year and all married households who divorce after the household formation year. Specifically, if they are single in the household formation year, the Fixed sample drops all household-year observations when they are married. If instead they are married in the household formation year, the Fixed sample drops all household-year observations when they are single. While this sample design prevents us from studying the effect of co-op conversion on life outcomes such as marriage and divorce, it focuses on a more stable sample for which results are easier to interpret. In previous versions of this paper, we found no evidence of significant treatment effects on such life outcomes.

Since, Statistic Sweden's family id variable is derived from family relationships and registered location at a building , not apartment, we cannot identify whether adults that do not have children together and are living in the same building are cohabiting in the same apartment or not.[47] By using the tenant lists and rental contracts, we are able to correct Statistic's Sweden's household

[47] See "Bakgrundsfakta - Historic population register, Statistics Sweden, 2006".

composition under the principle that one household should only have one rental contract and one rental contract should only correspond to one household. The corrections are as follows.

First, when two single adults with different family identifiers rent the same apartment according to tenant list, we assume these two adults belong to one household in the formation year and combine them together. Second, when more than one adult with the same family identifier in the formation year have different rental contracts, we assume each tenant belongs to a separate household and split the original households. Finally, we eliminate from the sample the parent of one registered tenant, since she appears to be living only temporarily at her daughter's apartment. Indeed she is not in the rental list of the building and moves out after formation year. We keep the daughter as single household in the sample, as she is a registered tenant in the household formation year. We end up with a fixed sample of 2,455 rental apartments and unique households in the household formation year.

G Co-op annual reports

We extract information from co-ops' annual reports for those co-ops that privatize as part of our quasi-experiment. We use this information for three purposes.

First, we perform a simple validation of the conversion price per square meter (p_0^c), which is calculated based on households' tax forms but which also can be approximated from from co-ops' book equity value and residential area. Second, we use information about the co-ops' monthly fee per square meter together with other information to compute the housing cost of staying households before and after treatment. Third, we investigate the relationship between renovations that the co-op undertakes and households' monthly co-op fee.

G.1 Validation of the conversion price per square meter

We use information about the co-ops' book equity and residential area to compute book equity per square meter. Table A.7 summarizes our analysis. We take book equity and co-ops' mortgages from the earliest annual report (columns (3) and (4)). The sum of this value is similar to landlord's asking price which we obtained from minutes of the landlords' boards (column (2)).[48] Co-ops typically report how many square meters belong to the members. if not, we approximate based on the total residential area and the number of residual tenants. We report the total residential area in column (5) and the area that belongs to co-op members in column (6). We then compute book equity per co-op members' total residential area (column (9)). This value can be compared with the conversion price obtained from KU55 tax forms (column (10)). We report the difference between these values in column (11). The largest deviation is 16 percent. The correlation between the two measures is 0.989.

[48] There can be differences between the actual transaction value and the value stipulated in the minutes of landlords' board meetings for several reasons, such as vacancies and information about the state of the building that is revealed in the due diligence process.

G.2 Co-ops' renovations and the households' monthly fee

We consider relative year 0 to relative year 4 for the 13 co-ops that belong in the treatment group. This means that we sometimes include 2008 to obtain more observations. We obtain 52 co-op year observations on co-op fees per square meter, depreciation, and repairs. this sample is smaller than the theoretical maximum sample of 65 co-op-year observations:

1. We are able to obtain 56 out of 65 annual reports.
2. We exclude relative year 0 if the expense item "Depreciation" is set to zero in the first annual report.
3. We exclude relative year 0 if the transaction happens very late in the year (December) because then it is difficult to reliably annualize the co-op fee per sqm.
4. It is quite often the case that the first report covers more than 12 months. If the first report considers relative year 0 and 1 in combination then data is annualized and only data for RY1 is used when doing so.

The average annual co-op fee per square meter for relative years 1 to 4 is as follows: 532 SEK, 564 SEK, 561 SEK, and 575 SEK. The co-op fee increases only by 8% points cumulatively during our sample period. Given the typical apartment size of around 70 square meters, it cannot account for much of the observed increase in consumption.

Nevertheless, to investigate further whether there is any indication that co-ops' renovation of common areas (such as gardens and staircases, storage space, laundry rooms, basement and attic spaces, etc) contributes to higher co-op fees, we estimate the following regression:

$$\Delta \log(\text{co-op fee}_{it+k}) = \beta_0 + \beta_1 \Delta \log(\text{Improvements}_{it}) + \beta_2 X_{it} + \varepsilon_{it}, \tag{50}$$

where co-op fee_{it} is the co-op fee per square meter and year and Improvements_{it} is a measure of the co-ops' expense. It is important to use the fee per square meter and not the co-op's total revenues from fees because many co-ops gradually expand, either as residual tenants move or buy or by converting common areas to new apartments. The variable Improvements_{it} is the sum of depreciation of assets and repairs. Depreciation measures the annual cost of capital expenditures, as they are written off according to the accounting rules. The variable X_{it} is the log change in the co-op's total number of square meters for its members apartments (i.e., total residential area minus the area of residual tenants). It is an important control variable since expanding the co-op's total number of apartments is the most common investment.

Table A.8 reports the estimates corresponding to equation (50) for $k = \{1, 2, 3\}$. The overall finding is that improvements display no relationship with increases in co-ops' monthly fees. The elasticity is small and insignificant over 1, 2, and 3 years. In contrast, a one percentage point

Table A.7: Book equity and the conversion price

Building name	Transfer date (1)	Landlord's ask (2)	Equity (3)	Debt (4)	Residential area (5)	Area of co-op members (6)	Landlord's ask per sqm (7)	Debt per sqm (8)	Equity per sqm (9)	Conversion price per sqm (p_0^c) (10)	% Difference (11)
Nattsländan 3	2003-02-18	16 800	10 120	7 727	1 172	920	18,26	8,40	11,00	11,00	0,0%
Korpen 13	2004-09-01	23 965	20 183	8 431	1 964	1 719	13,95	4,91	11,74	11,19	-4,7%
Slalomsvängen 2	2003-06-04	34 650	15 916	20 662	1 941	1 553	22,31	13,31	10,25	9,85	-3,9%
Bondesonen 21	2003-12-03	23 500	17 132	10 568	1 307	1 307	17,98	8,09	13,11	13,40	2,2%
Duggregnet 8	2003-05-21	54 000	33 025	24 726	5 006	4 246	12,72	5,82	7,78	7,75	-0,4%
Mimer 3 / Mimer 9	2004-06-21	93 000	52 125	45 621	4 009	3 465	26,84	13,16	15,04	14,14	-6,0%
Mullvaden Andra 30	2003-12-16	9 000	6 458	4 174	693	471	19,11	8,86	13,71	15,01	9,5%
Nystad 5	2003-05-26	33 000	13 193	25 764	6 055	4 797	6,88	5,37	2,75	2,61	-5,2%
Reservoaren 2	2004-04-27	95 000	74 981	38 725	10 342	10 342	9,19	3,74	7,25	8,41	16,0%
Reservoaren 4	2004-05-04	135 300	71 878	74 501	9 389	8 632	15,67	8,63	8,33	8,38	0,6%
Rosendal Mindre 30	2003-05-16	126 300	65 826	65 279	5 802	4 240	29,79	15,40	15,53	15,51	-0,1%
Stencilen 2**	2002-11-10	55 000	39 612	21 006	5 936	5 150	10,68	4,08	7,69	7,93	3,1%
Sveaborg 5	2003-05-27	36 000	15 761	25 599	6 775	5 826	6,18	4,39	2,71	2,58	-4,5%

Notes:
The landlord's asking price is taken from the landlords' board meeting minutes. The actual transaction value is sometimes adjusted for vacancies, and new information (such as the state of the building). Per square meter values have been divided by the area of co-op members. The conversion price per square meter is obtained from KU55 tax forms (see Appendix D). All values in SEK 1,000s.
*) The first annual report is in 2006.
**) This first annual report is in 2005.

Table A.8: Co-op fees and co-op improvements

	(1)	(2)	(3)	(4)	(5)	(6)
	$k=1$		$k=2$		$k=3$	
$\Delta_k$log(Improvements$_{it}$)	-0.035	-0.024	0.054	-0.001	-0.122	0.026
	(1.23)	(-0.85)	(-1.14)	(-0.02)	(-1.32)	(0.26)
$\Delta_k$log(Co-op sqm$_{it}$)		-0.932**		-1.486**		-2.22**
		(-2.08)		(-2.72)		(-2.46)
Constant	0.035	0.048	0.080	0.118	0.115	0.217
	(3.32)	(4.04)	(4.44)	(5.54)	(3.46)	(4.34)
N	39	39	26	26	14	14
R-squared	0.039	0.142	0.052	0.282	0.127	0.436

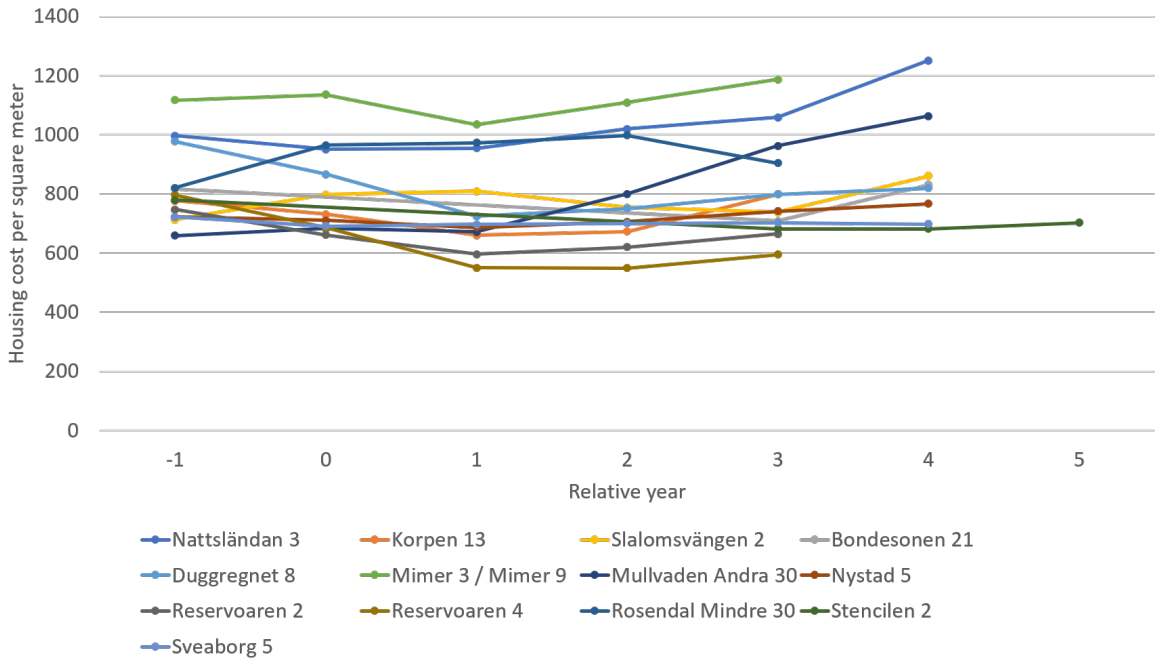
Notes: The table presents estimates based on the regression specification in equation (50). The dependent variable is the k-year difference in the logarithm of the co-op's fee per square meter. The covariates are k-year differences over the same time period. t-statistics in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

increase in the co-ops' residential area is associated with a decrease of similar magnitude in the monthly fee per square meter.

G.3 Stayers' housing cost

We construct the housing cost for households that stay in their housing unit throughout the treatment period. We use information about (i) rents and square meters from tenant lists, (ii) monthly fees per square meter from annual reports, and (iii) the after-tax mortgage interest expense derived in Appendix C. The housing cost for relative year 0 is based on a linear interpolation between the co-op fee and the rent, factoring in the transaction date of each co-op. Figure A.4 reports the housing cost per square meter for each relative year. Overall, housing costs show little change before and after treatment. In particular, there is no evidence that the monthly housing cost of treated households increases.

Figure A.4: Housing costs



Notes: The figure displays the housing cost per square meter and year for each approved co-op. The housing cost consists of the rent and co-op fee per square meter for each approved co-op. For relative year 0 and onwards, the after-tax mortgage interest expense is added as well. Rents are obtained from landlords' tenant lists. Co-op fees are obtained from annual reports. The value for relative year 0 is based on a linear interpolation between the co-op fee and the rent, factoring in the transaction date. See Appendix C for construction of the after-tax mortgage interest expense. For the figure, we summed up all stayers' mortgage interest expenses per co-op and divided by the sum of the square meters that the stayers own.

H Construction of price indices

The detailed nature of the Swedish data enables us to construct local real estate price indices and estimates of average price per square meter at various levels of geographical aggregation. We create three variables defined at the apartment level – value, geographical location and number of square meters, using two datasets provided by Statistics Sweden: the KU55 register (Överlåtelse av Bostadsrätt), and the Apartment Register (Lägenhetsregistret). In addition, we use a commercial dataset maintained by Svensk Mäklarstatistik, which has information on real estate transactions in Sweden obtained directly from real estate agents.

H.1 Data selection

In Sweden, apartment buildings are owned by co-op associations whose shares give right to live in the apartment units. All transfers of co-op shares are reported to the tax authorities through form SKV 2324 and are available in the KU55 register. The data reports observations of the type

of transaction, the acquisition price, the sale price, the date of sale and the ownership share being transacted. In order to build price indexes, we apply a number of filters to the data. First, we keep only sales of the entire property, and thus exclude partial sales, gifts or bequests. Second, we exclude all observations for which form SKV 2324 is not filled in accordance with the official rules, as outlined in the taxation brochures published annually by Skatteverket (SKV 378). For example, we exclude observations with missing fields (e.g. missing acquisition date or transaction price), or when the sum of the transferred ownership shares does not sum up to 100%. Third, we drop transactions which took place less than a year after the conversion of a rental building to co-op (bostadrätt), as acquisition prices at the time of conversion do not represent market valuations. Fourth, we exclude transactions with abnormally low prices (less than 1,000 SEK) and with holding periods of fewer than 90 days to filter out non arm's length transactions, distressed sales, and transactions by professional flippers. Finally, we exclude observations which have experienced extreme price growth in a short period of time, as they are likely to have transaction prices which do not reflect market values.[49]

H.2 Index estimation

We use the cleaned KU55 dataset to create local apartment price indices from 1999 to 2017. The local price indices are estimated using a standard repeat sales methodology by running an OLS regression of log purchase and log sale prices on area-by-year fixed effects and KU55 observation fixed effects. The area-by-year fixed effect coefficients are then converted into local annual index levels. We build indices at the county, municipality, and parish level. Parish and municipality indices are missing when we do not have enough observations to estimate the area-year fixed effect for all years between 1999 and 2017.

We also use the cleaned data to create price per square meter estimates. Sweden is divided into 250×250 meter geographical units called rutor that are linked to apartment property identifiers (lägenhetsbeteckning). Since property identifiers are available in the Apartment Register and not in the KU55 register, we link the two databases by using a matching procedure that uses the personal identifiers (personnummer) of transaction parties, and the organization number of the co-op association. In turn, we merge the KU55 register and the Svensk Mäklarstatistik dataset by using the co-op association organization number, the transaction price, and the transaction date. The combined data set allows us to assign apartment size to KU55 observations from the Svensk Mäklarstatistik dataset and, when missing, the Apartment Registry.

[49] More precisely, we run a regression of holding log returns on holding periods and exclude observations with Cook's distance that exceeds 4/(n-1), where n is the sample size. In total, we drop 4.45 percent of the observations due to extreme price growth.

H.3 Predicted neighborhood prices – p_0^{nbd}

Our instrumental variable regression relies on the construction of the neighborhood market price for both approved and denied co-ops. Section 4 denotes this variable by p_0^{nbd}. We apply the method below to construct the variable.

Price per square meter data for each neighborhood, containing a building in the treated or control group, are created by centered expansions around the 250×250 meter block (rutor) where the properties are located. If the block contains fewer than 10 co-op share transactions, then it is enlarged by 250 meters in each direction. We exclude from the calculation of average prices the buildings that are part of the quasi-natural experiment considered in this study either as part of the treatment or as part of the control group. The enlarged square sizes are 750×750 meter, 1250×1250 meter, and 2250×2250 meter. The average price per square meter is computed as the equally-weighted average of square meter prices among all apartments located in the square. Table A.9 reports summary statistics on square size for buildings in the treatment and control groups. Panel A reports the smallest neighborhood size with any transaction and panel B the selected neighborhood size after the criterion that 10 transactions exist has been applied. Figure A.5 displays the selected neighborhood size, as a shaded square, for each co-op in our sample.

Table A.9: Apartment transactions in neighborhoods (2001–2007)

Panel A: Smallest neighborhood sizes with transactions

	250 × 250		**750× 750**		**>750× 750**		**Total**
	Co-ops	Transact.	Co-ops	Transact.	Co-ops	Transact.	Co-ops
Treatment group	8	134	5	83	0	-	13
Control group	20	224	10	83	3	226	33

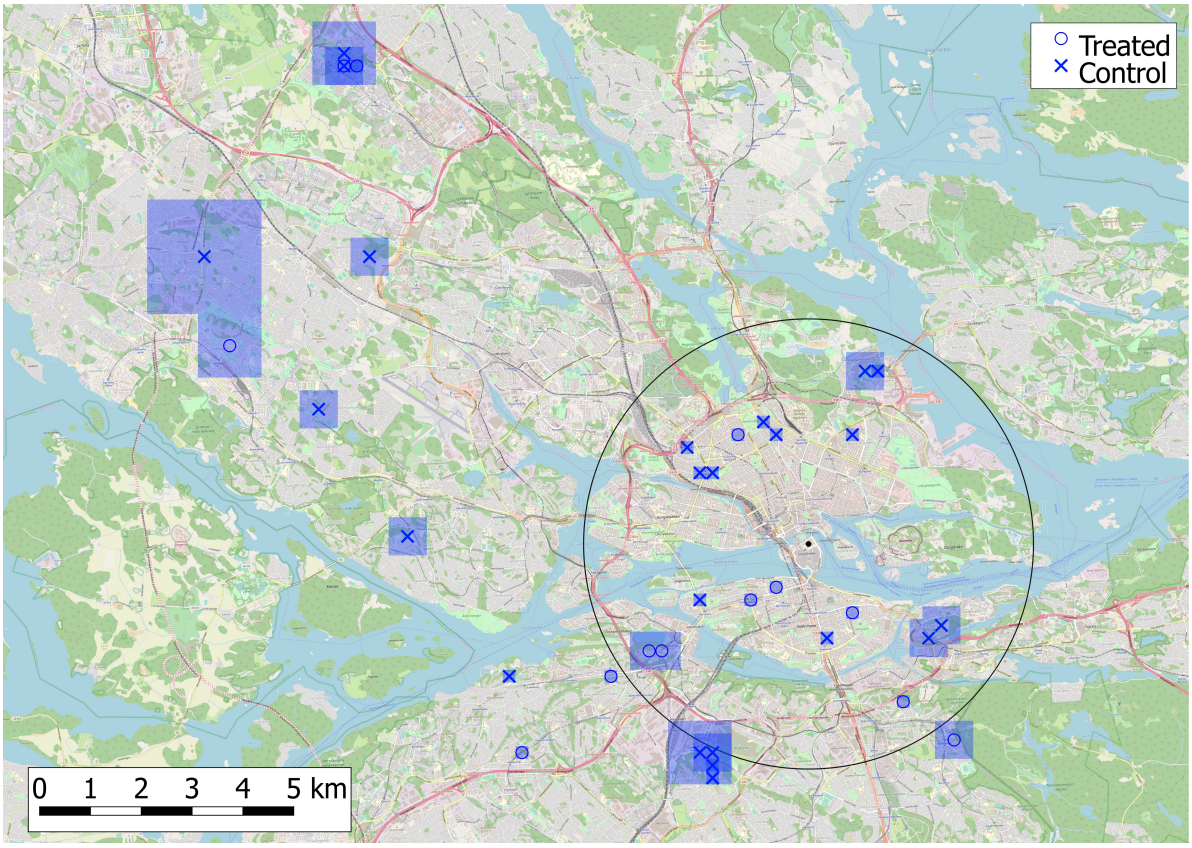
Panel B: Selected neighborhood sizes for price level estimation

	250 × 250		**750× 750**		**>750× 750**		**Total**
	Co-ops	Transact.	Co-ops	Transact.	Co-ops	Transact.	Co-ops
Treatment group	8	134	4	103	1	72	13
Control group	19	235	11	77	3	244	33

Notes: The table reports the average number of transactions per neighborhood. For estimation a minimum of 10 apartment transactions in the neighborhood is required (see Panel B). Otherwise, the neighborhood area is expanded by another 250 meters in all directions. The maximum neighborhood size in Panel A is 1750×1750 meters. The maximum neighborhood size in Panel B is 2250× 2250 meters.

To estimate a price index for the neighborhoods we run the following OLS regression on these

Figure A.5: Neighborhood co-op prices



Notes: The map displays the location of the 38 privatization attempts in our Stopplag sample. Circles indicate approved co-ops (treated) and crosses indicate denied co-ops (control). Each shaded square illustrate the neighborhood from which we retrieve transactions.

transaction data:

$$p^h_{t,i} = \beta_0 + \sum_{k=2002}^{2007} \beta_k D_{it}(t = k) + \gamma \text{Distance to Center}_i + \varepsilon_{a,t} \tag{51}$$

where $p^h_{t,i}$ is the log of the square meter market price of apartment i that transacts in year t, $D_{it}(t = k)$ is a calendar year fixed effect indicating the year of the transaction, and Distance to Center$_i$ measures the distance between the apartment and the city center (Royal Palace). The coefficients on the year dummy variables, β_k, measure price growth relative to the year 2001. Table A.10 reports the results. Apartment prices increase steadily over the sample period, displaying a cumulative rate of growth of 55.8 log points between 2001 and 2007. Apartment prices are 10.4 log points lower for each kilometer further away from the city center. With a R^2-statistic of 0.59, a substantial share of the variation in log price per square meter is explained. We use this regression to construct the neighborhood market price per square meter for the housing units in our sample,

denoted by $p_{0,i}^{\text{nbd}}$ in Section 4:

$$p_{0,i}^{\text{nbd}} = \exp\left\{\hat{\beta}_0 + \hat{\beta}_k D_{it}(t=0) + \hat{\gamma}\text{Distance to Center}\right\}, \tag{52}$$

where $D_{it}(t=0)$ is the calendar year of treatment for household i (approvale/denial).

Table A.10: Estimation of log square meter prices based on selected neighborhoods (2001–2007)

	(1)
Constant	10.43***
	(0.028)
$D_{it}(t=2002)$	0.0262
	(0.0354)
$D_{it}(t=2003)$	0.0883***
	(0.0313)
$D_{it}(t=2004)$	0.165***
	(0.0305)
$D_{it}(t=2005)$	0.236***
	(0.0288)
$D_{it}(t=2006)$	0.447***
	(0.0285)
$D_{it}(t=2007)$	0.558***
	(0.0283)
Distance to Center	−0.104***
	(0.002)
Observations	4,429
R^2	0.590

Notes: The table reports the regression results for neighborhood price level estimation as reported in equation (51). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

H.4 Predicted conversion prices – $p_0^{c,\text{nbd}}$

In addition to the neighborhood-level market prices per square meter, p_0^{nbd}, our instrumental variable regression relies on the construction of predicted conversion prices per square meter for both approved and denied co-ops, denoted by $p_0^{c,\text{nbd}}$. The prediction is based on the following regression:

$$p_{0,j}^{c} = \beta_0 + \gamma\text{Distance to Center}_j + \varepsilon_j \tag{53}$$

where $p_{0,j}^{c}$ denotes the median conversion price per square meter (in kSEK) for approved co-op j and Distance to Center$_j$ is the distance to center of co-op j measured in kilometers. Figure A.6 shows the fit. The R^2-statistic is 0.771, $\hat{\beta}_0$ is 15.33 and $\hat{\gamma}$ is -0.792 ($p < 0.01$), meaning that the conversion price declines by 792 SEK per kilometer further away from the center.

We use these estimates for constructing $p_{0,j}^{\text{c,co-op}}$ in an (out-of-sample) prediction for both apart-

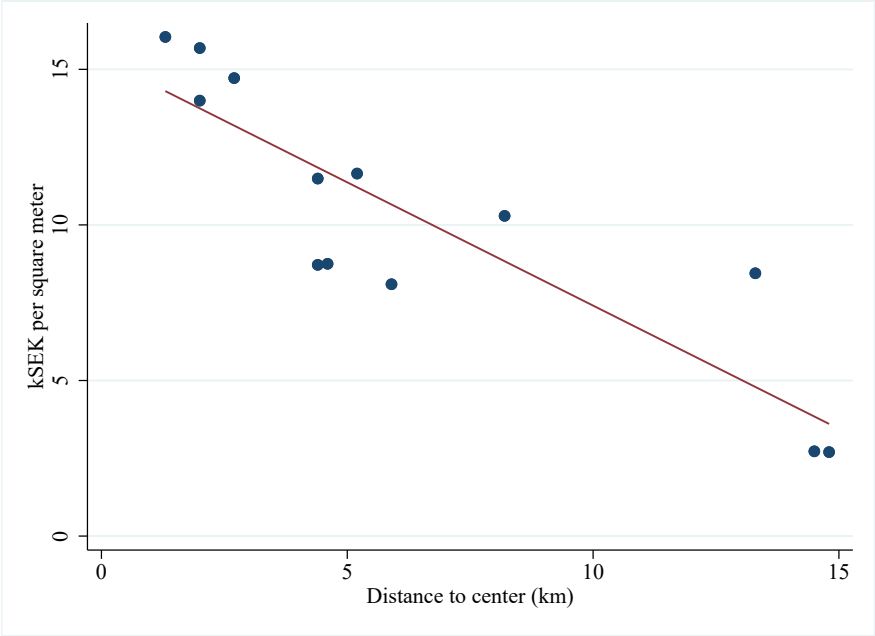
ments in the treatment and control groups:

$$p_{0,j}^{c,co-op} = \exp\left\{\hat{\beta}_0 + \hat{\gamma}\text{Distance to Center}\right\}. \tag{54}$$

In a last step, we assign the predicted co-op level conversion price to the households living in that co-op, $p_{0,i}^{c,nbd} = p_{0,j}^{c,co-op} \mid (i \texttt{ lives in } j)$ for each household i and co-op j. Finally, we calculate the hypothetical fractional discount in the neighborhood:

$$\tau_i^{nbd} = (p_{0,i}^{\text{nbd}} - p_{0,i}^{c,nbd})/p_{0,i}^{\text{nbd}}. \tag{55}$$

Figure A.6: Conversion price per square meter for approved co-ops



Notes: The figure shows a scatter plot of the conversion price per square meter in SEK 1,000, p_0^c, against distance to center for the 13 approved co-ops. The regression line corresponds to equation (53).

I Construction of the household's buffer

This appendix defines and details the construction of the household's buffer. Section I.1 define the relevant borrowing constraints. Section I.2 describes the implementation in the data. Section I.3 provides institutional details and parameter values.

I.1 Borrowing constraints

The borrowing capacity for collateralized debt is determined by a LTV constraint and an affordability constraint. The borrowing capacity for uncollateralized debt is determined only by the affordability constraint.

I.1.1 The affordability constraint

The affordability constraint is also known as a "left-to-live-on" constraint. It is similar to a debt service-to-income constraint. See Section I.3 for institutional details.

The affordability constraint is common to both collaeralized and uncollateralized debt. It states that, after a new debt contract is initiated, the household should be left with weakly positive resources after essential consumption expenditures have been met :

$$y_{it} - \underline{c}_{it} - r^{M}_{\text{DSTI}} M_{it} - r^{D}_{\text{DSTI}} D_{it} \geq 0 \tag{56}$$

where y_{it} is disposable income, $\underline{c}_{it}$ denotes the level of subsistence consumption which depends on household size (see Section I.3 for details), M_{it} denotes mortgage debt, D_{it} denotes uncollateralized debt, and r^{M}_{DSTI} and r^{D}_{DSTI} denote stressed interest rates plus amortization rates.

I.1.2 The LTV constraint

For collateralized debt (i.e., mortgages) there is a standard LTV constraint. It says that at the time of mortgage origination the mortgage balance must satisfy:

$$\mathrm{M}_{it} \leq (1-\delta) \cdot P^{h}_{t} h_{it}, \tag{57}$$

where $P^{h}h_{it}$ is the market value per square meter of the housing unit at t, h_{it} is the size of the apartment in square meters, and δ is the minimum down-payment. We set $\delta = 0.1$ which is consistent with lending standards at the time of our quasi-experiment.

For mortgage debt, both affordability and LTV constraint must be satisfied. This implies that

at the time of origination the maximum mortgage balance is given by:

$$\overline{M}_{it} = \min\left\{\frac{y_{it} - \underline{c}_{it}}{r^M_{\text{DSTI}}}, (1-\delta)\cdot P^h_t h_{it}\right\}. \tag{58}$$

I.2 Implementation in the data

In the data, we observe only total debt, denoted by $M_{it} + D_{it}$. We therefore assume that homeowners exhaust their collateralized borrowing capacity before they take on any uncollateralized debt.

I.2.1 Homeowners

Given the assumption that homeowners exhaust their collateralized debt capacity the two cases for the homeowner are as follows.

Case 1 If $(1-\delta)\cdot P^h_t h_{it} < \frac{y_{it}-\underline{c}_{it}}{r^M_{\text{DSTI}}}$ then the homeowner's total debt capacity is:

$$\overline{M}_{it} = (1-\delta)\cdot P^h_t h_{it} \tag{59}$$

$$\overline{D}_{it} = \min\left\{\max\left\{\frac{y_{it} - \underline{c}_{it} - r^M_{\text{DSTI}}\cdot \overline{M}_{it}}{r^D_{\text{DSTI}}}, 0\right\}, 2\cdot y_{it}\right\}. \tag{60}$$

where $\underline{c}_{it}$ depends on the composition of the household (see Section I.3 for further details). The outer bracket in (60) imposes that the homeowner can borrow at most twice her disposable income in additional uncollateralized debt.

Case 2 If $(1-\delta)\cdot P^h_t h_{it} \geq \frac{y_{it}-\underline{c}_{it}}{r^M_{\text{DSTI}}}$ then the homeowner's total debt capacity is:

$$\overline{M}_{it} = \max\left\{\frac{y_{it} - \underline{c}_{it}}{r^M_{\text{DSTI}}}, 0\right\} \tag{61}$$

$$\overline{D}_{it} = 0. \tag{62}$$

I.2.2 Renters

The renter's total debt capacity is given by:

$$\overline{M}_{it} = 0 \tag{63}$$

$$\overline{D}_{it} = \min\left\{\max\left\{\frac{y_{it} - \underline{c}_{it}}{r^D_{\text{DSTI}}}, 0\right\}, 2 \cdot y_{it}\right\}. \tag{64}$$

where the outer bracket in (64) implies that the renter, at origination, can borrow at most twice her disposable income in uncollateralized debt.

I.2.3 Remaining debt capacity

For both renters and homeowners, let $M_{it} + D_{it}$ denote total debt which is observed in the data. Remaining debt capacity is then for both homeowners and renters given by:

$$\text{Remaining debt capacity}_{it} = \max\left\{\overline{M}_{it} + \overline{D}_{it} - (M_{it} + D_{it}), 0\right\}. \tag{65}$$

I.2.4 The buffer

For all households the buffer, denoted by Buffer_{it}, equals the remaining debt capacity, as defined above, plus liquid financial wealth:

$$\text{Buffer}_{it} = \text{Financial wealth}_{it} + \text{Remaining debt capacity}_{it}. \tag{66}$$

I.3 The affordability constraint – institutional details

The DSTI constraint in Sweden is also known as an affordability constraint. The Swedish acronym for the constraint is KALP as in "Kvar Att Leva På." By law, mortgage lenders are required to calculate borrowers' debt capacity and as a practical consequence of this requirement, mortgage lenders perform a standardized calculation to check if the DSTI constraint is violated. This constraint only needs to hold at the time of origination of the mortgage. Banks' algorithm for this calculation has been monitored by the Swedish Financial Supervisory Authority (Finansinspektionen) since 2008. Since 2008, Finansinspektionen publishes reports on their monitoring activities. Correspondence with bank representatives indicates that the algorithm has remained essentially unchanged since at least the early 2000s.[50] The components in the algorithm are: (i) the stressed interest rate, (ii) required amortization rates, (iii) housing cost, and (iv) cost of living, which depends on household composition.

[50] E-mail conversations with Anne-Charlotte Ringberger at Nordea and phone conversation with Klas Flodmark, controller at Handelsbanken.

I.3.1 Parameter values in 2008

Finansinspektionen (2008) reports the input parameters in 2008 for the eleven largest banks in Sweden:[51]

- Average stressed interest rate: 7.5% (Minimum: 6.25%, Maximum: 8.8%)
- Average required amortization: 0.9% (Minimum: 0%, Maximum: 2.0%)
- Estimated housing cost for single-family house: SEK 3,600 (Minimum: 3,000, Maximum: 4,000)
- Cost of living for two adults and two children: SEK 15,900 (Minimum: 15,000, Maximum: 17,500)

I.3.2 Cost of living from the Swedish Consumer Agency

For the cost of living, all banks more or less follow the recommended budgets from the Swedish Consumer Agency. Their budgets are updated each year.[52] The budgets should be viewed as quite tight, leaving no room for luxuries.

I.3.3 Mortgage lending at Nordea in 2002–2007

In an e-mail, Nordea provided the following information on their inputs to the calculation in years 2002–2007:

- Stressed interest rate: the 5-year fixed mortgage rate plus three percentage points with a floor at 8%.
- Mortgage principals has an amortization rate of 2% per year (principal paid back in 50 years) and uncollateralized debt an amortization rate of 10% per year (credit paid back in 10 years).
- Estimated housing cost varies by the type of home. Over a long period of time the following inputs were used: SEK 4,000 per month for houses and SEK 750 plus the co-op's monthly fee for apartments.
- Cost of living according to the Swedish Consumer Agency. It includes the standard costs (e.g. food, hygiene, medicine, etc.) as well as the cost for a car (if the household has one), costs for day care (if there are children), commuting costs, and costs for having lunch out if household members are of working age.

[51] Finansinspektionen (2008), Utvecklingen påbolånemarknaden, Report 2008:6, Dnr 07-12625-399.

[52] See for instance Konsumentverket (2004), Skäliga levnadskostnader – Beräkningar av Konsumentverket, PM 2004:10. We also obtained scanned budget tables for 2002–2007 from Ingrid Eriksson at the Swedish Consumer Agency.

I.3.4 Uncollateralized debt

With regards to uncollateralized debt, the lenders are required to employ a similar calculation but there is greater variation in their input parameters. The algorithms are not as closely monitored by Finansinspektionen. Typically, higher stressed interest rates are employed but the exact rate often depends on the credit amount. Today, websites offer uncollateralized debt contracts of up to SEK 500,000 or 600,000.[53] But in the time period of our study, unsecured credit was not as easy to obtain. Based on our conversations with bankers and regulators, a restriction that limits uncolateralized debt at twice annual income is a good assumption for our sample period.

I.3.5 Our input parameters

Based on the above discussion, we choose the following parameters for the calculation of the buffer for our sample households.

Mortgages

- Minimum down-payment at origination (δ) of 10%
- Stressed interest rate of 8%.
- Amortization rate of 2% per year.
- Subsistence consumption consists of housing cost (rent or co-op fee) and cost of living according to the Swedish Consumer Agency.

Uncollateralized debt

- Stressed interest rate of 10%.
- Amortization rate of 5% per year.
- Subsistence consumption determined as it is for mortgage debt.
- In addition, mortgage debt burdens calculation with stressed interest rate and amortization rate as stated above.

[53] See for instance www.advisa.se, https://www.freedomfinance.se, or https://www.icabanken.se.

J Consumption in the Household Survey and Sample Restrictions

J.1 First-time apartment buyers

Table A.11 reports summary statistics from the consumption expenditure survey (HUT). The data are for annual waves between 2003 and 2007, corresponding to our treatment years. In the full sample average consumption expenditure equals 181 kSEK with a minimum at 11.8 kSEK and a maximum at 1121 kSEK per adult equivalent. The table breaks down total spending into various consumption subcategories of interest.

The table also reports estimates from the regression:

$$c_{it} = \omega_0 + \sum_{k=0}^{2} \delta_k \text{ApartmentBuyer}_{it-k} + \omega X_{it} + \psi_t + \varepsilon_{it} \tag{67}$$

where c_{it} denotes an expense item in kSEK per adult equivalent, X_{it} is vector of dummy variables that characterizes the household head in terms of age, civic status, education, country of residence, as well as the disposable income of the household. The variable $\text{ApartmentBuyer}_{it-k}$ is equal to one if the household changed primary address in year $t-k$ and owns a co-op apartment in t. The fifth to seventh columns of Table A.11 report the coefficients δ_k. The eighth to tenth columns report the same coefficients for households that did not own an apartment before the change of primary address. These households are referred to as first-time apartment buyers. This latter group is most similar to the treated households in our sample, who were not home owners at the time of treatment.

First-time apartment buyers spend 39 kSEK more in the year of the purchase and 29 kSEK in the subsequent year. Two years after the purchase there is no noticeable difference with other households. Reassuringly, these consumption responses are broadly in line with the estimates from our experiment. This helps with external validity.

Furniture, household appliances, and regular maintenance of the home account for 27 percent of the total expenditure increase in both the year of purchase and the year after, substantially above their 6.2% average expenditure share.

Interest expenses account for about as much in the year after the purchase but less in the year of purchase or two years after. Expenditures on rent, which include monthly co-op fees for owners, and utilities increase in the year of purchase. Both interest expenses and rent/utilities are housing-related expenditures, separate from renovations or furniture purchases.

Importantly, there are increases in consumption categories such as clothes and footwear, health care, communication, and restaurants and hotels. All told, non-housing related spending categories account for more than half of the consumption increase (52%) in the year of purchase and more than one-third in the year after (34.6%).

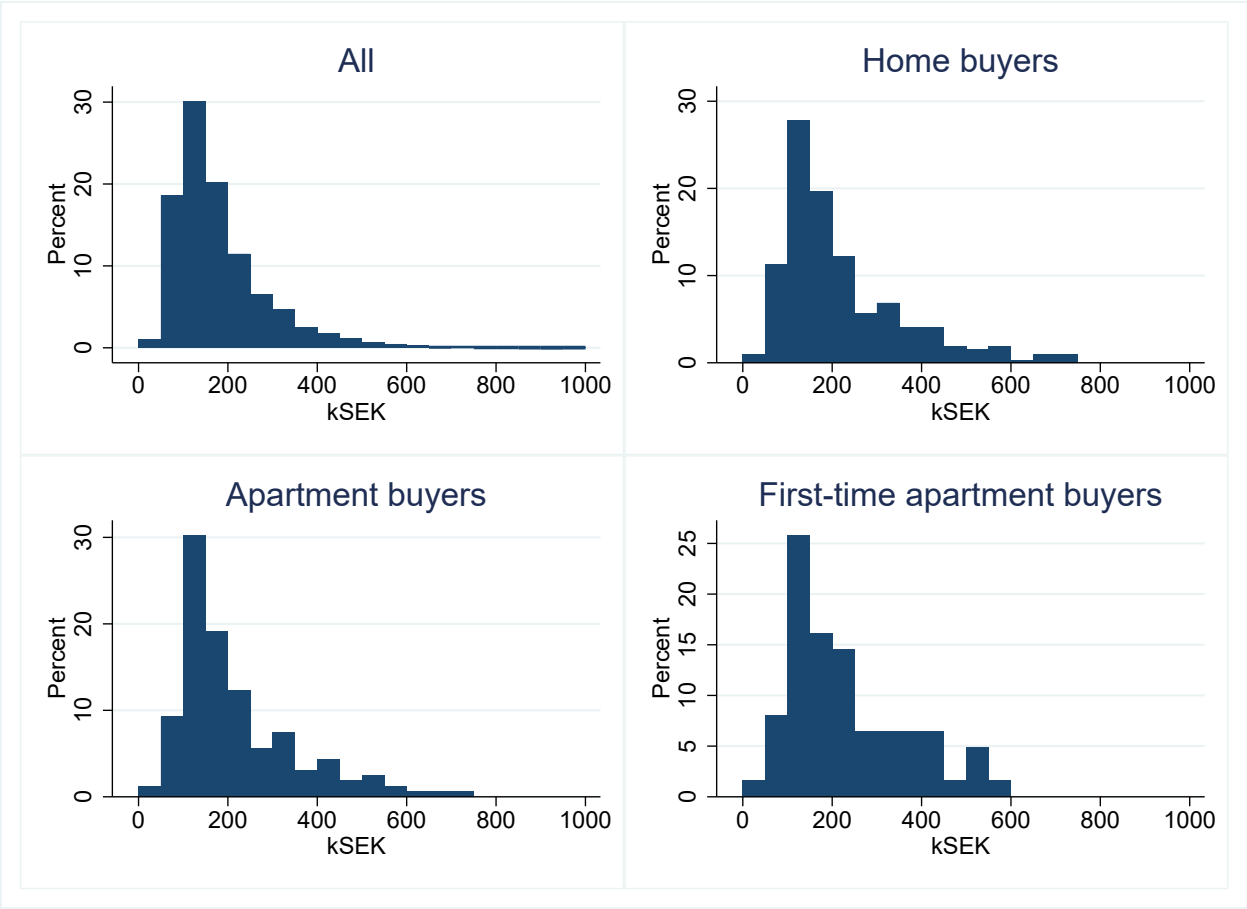
Table A.11: Summary statistics and response upon apartment purchase

					Apt. buyer in relative year $-k$			First-time apt. buyer in relative year $-k$		
	Mean	Std.	Min	Max	$k=0$	$k=1$	$k=2$	$k=0$	$k=1$	$k=2$
01. Food and nonalcoholic beverages	21.77	12.69	0.00	147.49	-0.32	-2.34	0.04	1.48	-1.62	0.30
02. Alcoholic beverages and tobacco	4.25	6.87	0.00	90.60	-0.14	-0.31	-0.05	-0.58	-0.55	-1.07
03. Clothes and footwear	9.53	17.10	0.00	255.63	1.65	3.04	2.47	6.72**	0.91	0.02
04. Rent and utilities	34.82	24.09	0.00	628.88	3.06	3.15	0.26	7.31*	2.94	5.80
05. Furniture, appliances, and maintenance	11.17	18.83	-55.00	334,81	5.38**	5.96**	4.50**	10.51***	8.12**	-2.45
051. Furniture	4.37	10.36	-60.00	232.06	5.26***	4.18***	1.10	7.93***	4.64**	-1.21
053. Household appliances	1.72	4.81	-5.88	132.50	0.41	1.54**	1.17**	0.09	2.32***	0.14
056. Regular maintenance of the home (goods and services)	1.28	2.02	0.00	53.42	-0.19	-0.03	0.39*	-0.16	0.14	-0.16
06. Health care	4.20	13.02	0.00	260.00	-1.47	3.15*	-1.01	-0.23	4.02*	-1.16
07. Transportation	26.81	44.61	-172.35	699.67	-1.47	3.32	-2.24	1.63	3.59	0.24
08. Communication	6.00	4.68	0.00	51.60	0.46	-0.07	0.81*	1.24*	-0.04	0.96
09. Recreation and culture	25.40	31.09	-63.60	508.16	-5.30	2.67	3.19	2.71	4.57	5.60
10. Education	0.08	2.15	0.00	144.68	-0.02	-0.07	0.01	-0.01	-0.11	-0.17
11. Restaurants and hotels	7.80	12.80	0.00	174.23	1.90	3.44**	5.22***	4.39*	-0.10	0.26
12. Other goods and services	10.95	12.28	0.00	409.34	-1.98	-0.15	-0.05	0.87	1.31	-1.74
21. Unemployment insurance and union membership fee	2.09	2.71	0.00	82.02	-0.19	-0.16	-0.08	0.32	-0.20	0.04
22. Interest expenses	9.58	16.72	0.00	360.00	-0.71	6.55***	5.01***	0.87	8.08***	3.33
23. Tax on vehicles	1.15	1.20	0.00	18.40	-0.13	-0.00	-0.05	-0.25	-0.00	-0.10
24. Gifts	1.43	8.25	0.00	377.00	-0.10	-0.36	-0.14	0.12	-1.01	0.10
25. Second homes (cabins)	1.29	4.97	0.00	144.23	-0.20	0.52	0.35	0.08	0.86	-0.55
30. Taxable benefits	0.83	3.46	0.00	65.15	-0.52	-0.24	-0.22	-0.22	-0.72	-0.13
Total expenditure	181.43	111.21	11.77	1,121.24	0.55	26.53**	17.88*	38.85**	29.25*	8.80

Notes: The table reports summary statistics and regression estimates from the household expenditure survey for 2003–2007. All values are in SEK 1000 per adult equivalent. The total number of observations is 5581. Four observations with negative total expenditure have been dropped. The last six columns report estimates of γ_0, γ_{-1} and γ_{-2} from regression equation (**??**) using each item as the dependent variable. $* = p < 0.10, ** = p < 0.05, *** = p < 0.01$

Figure A.7 displays histograms for four samples from the household expenditure survey. The top left panel reports the distribution for all households in the survey for 2003–2007. The mode is at only 100-150 kSEK but the distribution is highly right-skewed. Households that buy a single-family house or co-op apartment (top right panel) are a lot more likely to spend more than 300 kSEK compared to households in the full sample. A thick right tail in the consumption distribution is also present for apartment purchasers (bottom left panel) and first-time apartment purchasers (bottom right panel).

Figure A.7: Spending According to the Survey



Notes: Top left panel: all households in the household expenditure survey for 2003–2007. The total number of observations is 5,581. Four observations with negative total expenditure have been dropped. Top right panel: households that change primary address and own a single-family house or co-op apartment at the end of the year. Bottom left panel: households that change primary address and own a co-op apartment at the end of the year. Bottom right panel: households that change primary address and own a co-op apartment at the end of the year and that did not own a single-family house or apartment before the change of address. All values are in SEK 1,000 per adult equivalent.

J.2 Sample restrictions based on the distribution of imputed spending

Table A.12 reports percentiles for spending in the survey and for the imputed spending measure in our sample. Approximately five percent of the observations for the imputed spending measure are negative. This is in line with the statistics reported in Koijen, Van Nieuwerburgh and Vestman (2014). Furthermore, the right tail of the distribution is a bit more skewed than in the survey. About 0.5 percent of the observations exceed 1,000 kSEK. Consequently we choose to exclude observations for which spending exceeds percentile 99.5 (1,010 kSEK). We also choose to exclude observations with negative values to facilitate the transition between levels and logs of the variable.

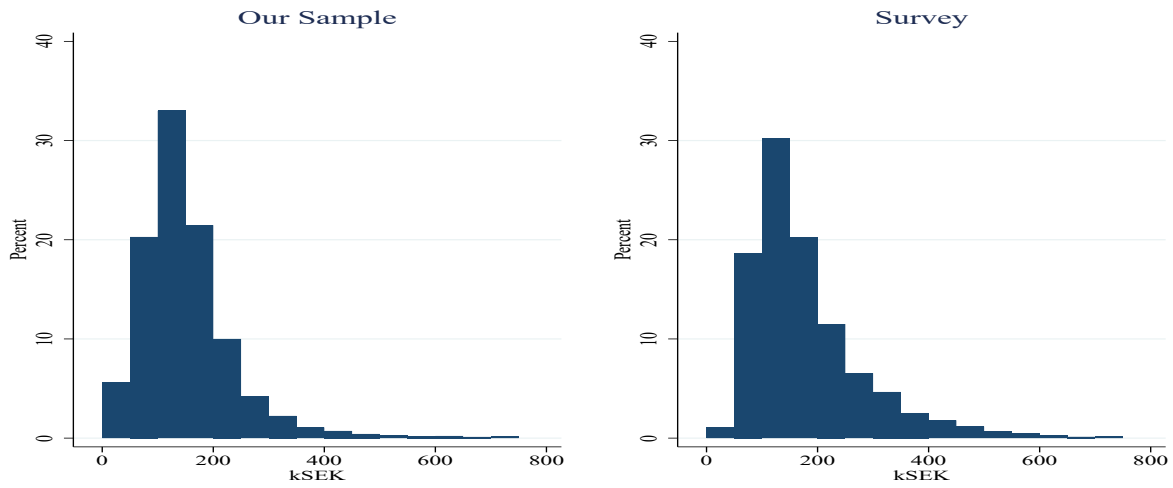
Table A.12: Distributions of Spending – Survey and Imputed Measure

Percentile	Survey	Imputed spending
0.5	39.68	-715.30
1	49.84	-429.06
2.5	59.59	-145.61
3	61.02	-100.73
5	69.01	-18.50
10	82.77	44.50
25	108.20	92.66
50	150.68	131.61
75	221.01	179.81
90	318.47	246.02
95	399.46	316.16
97	454.55	387.87
97.5	472.62	419.57
98	501.95	463.95
98.5	536.53	559.66
99	581.02	712.14
99,5	692.55	1,010.79
N	5,581	17,640

Notes: The table reports percentile values for total expenses according to the household expenditure survey for 2003–2007 and for imputed spending in our sample, including those that are 65 years and older. All values are in SEK 1000 per adult equivalent.

Figure A.8 displays two histograms, one for consumption in our sample after the sample restrictions have been imposed and one for consumption in the survey. The distributions are similar.

Figure A.8: Consumption in Our Sample and in the Survey



Notes: Left panel: imputed consumption in our sample. Right panel: consumption in the survey. All values are in SEK 1,000 per adult equivalent.

K External Validity

We discuss external validity in several realms. The first two exercises compare the income distributions of our sample households to the broader Stockholm renters and the consumption distribution of our households to that in the Swedish household budget survey. The next sections discuss Sweden in international context with regards to its homeownership and house price growth, mortgage market and home equity dynamics, rent regulation, and the broader welfare state.

K.1 Comparisons with Stockholm renters

Table A.13 reports averages of a selection of socioeconomic variables reported in Table 1 for households in our Stopplag sample and for all renter households in the municipalities of Stockholm and Nacka.

Figure A.9 shows the distribution of income, as defined in equation (5) and Table 1, for households in our Stopplag sample and for all renter households in the municipalities of Stockholm and Nacka.

Table A.13: Socioeconomic Variables for the Stopplag Sample and Stockholm Renters

	Stopplagen Sample	Stockholm Renters
Age	43.46	38.97
	(10.36)	(11.87)
High school	0.43	0.39
	(0.50)	(0.49)
Post high school	0.45	0.47
	(0.50)	(0.50)
Partner	0.33	0.18
	(0.47)	(0.39)
Unemployed	0.16	0.13
	(0.37)	(0.33)
Income shock -25% (Z_{it})	0.09	0.07
	(0.28)	(0.25)
Homeowner	0.04	0
	(0.19)	(0)
Apartment wealth	8.68	0
	(105.09)	(0)
Housing wealth	20.37	0
	(165.86)	(0)
Financial wealth	84.62	138.88
	(222.95)	(3249.34)
Debt	91.63	126.85
	(172.62)	(527.02)
Net worth	54.69	86.54
	(326.13)	(3514.45)
Risky share (uncond.)	0.23	0.25
	(0.30)	(0.33)
Income	157.04	143.11
	(75.45)	(157.79)
Observations	5548	969988

Notes: The table presents averages of variables for the Stopplagen sample and for all renter households in the municipalities of Stockholm and Nacka. For the Stopplagen sample, the average is taken for relative years -1 and earlier (2000–2004). For the broader sample of Stockholm renters, the years included are 1999–2002. In this sample, a household is defined as a renter if it has no apartment wealth and no single-family house. Standard deviations are in parentheses. With the exception of variables per individual or in ratios, all variables are denominated in SEK 1,000 per adult equivalent according to the OECD formula and deflated by the consumer price index.

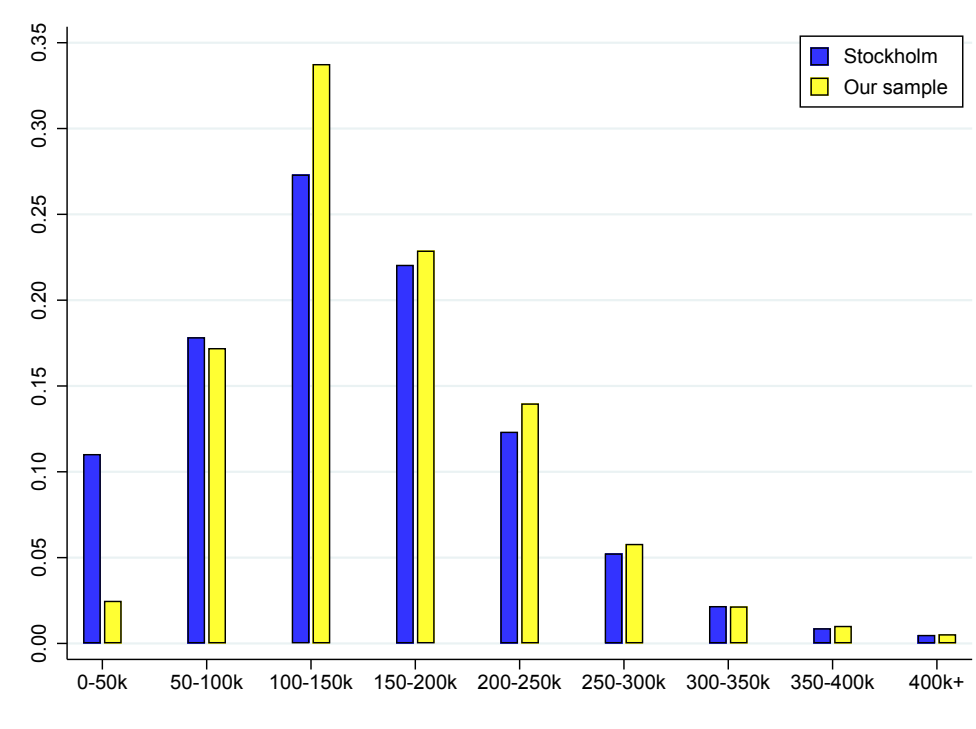
K.2 Consumption Distribution

Appendix J.2 compares the consumption distribution in our sample to that in the Swedish household budget survey.

K.3 Homeownership and House Price Growth

During our period of study (1999-2007), the Swedish home ownership rate was nearly identical to that in the United States, at 66.5%. Just like in the U.S., it peaked in 2006-07 (69.5% in Sweden and 69% in the U.S.), and then fell back down during the GFC (to 64.1% in 2018 in Sweden and 63%

Figure A.9: Distribution of income in our sample and among all renters in Stockholm



Notes: The histogram depicts income, as defined in equation (5) and Table 1, for households in our sample and for all renter households in the municipalities of Stockholm and Nacka. A household is defined as a renter if it has no apartment wealth and no single-family house. For the Stopplagen sample, the average is taken for relative years -1 and earlier (2000–2004). For the broader sample of Stockholm renters, the years included are 1999–2002. All values are in thousands of SEK and scaled by adult equivalents.

in 2016 in the U.S.). The Stockholm housing market for owner-occupied housing is also similar to that in other major European cities with strict supply restrictions due to zoning (e.g., height restrictions, historical districting, etc.).

The Swedish housing market displayed robust house price growth during our period of study, like most countries around the world at that time. According to the BIS comparative international house price data set, annual nominal HPA between 2000.Q1 and 2007.Q4 was 9.1% in Sweden. This is close to HPA in the United States (7.8%), Canada (8.2%), the U.K. (10.7%), Australia (10.7%), as well as several European countries such as Denmark (8.6%), Norway (7.8%), Finland (6.8%), Belgium (8.4%), France (10.7%), Ireland (8.8%), and Spain (12.2%) over the same period. Unlike in the U.S., Spain, Ireland, and Denmark, but like in several other European countries, Swedish house prices slowed but did not crash in the GFC. Over the period 2007.Q4-2010.Q4, nominal HPA in Sweden was 3.3% per year, similar to Norway (3.5%), Finland (2.0%), and Belgium (3.5%), to name a few. Our study period seems like a good period to investigate the effect of home ownership and housing wealth changes on consumption. It is relevant for the past decade from 2010–2020 which, just like 1999–2007 period, also witnessed strong house price growth. Swedish house price

growth was 5.1% per year over this period.

K.4 Mortgage Market

The next point of comparison concerns the mortgage markets at the time of our study. The typical first-lien mortgage in Sweden is up to about 70% of the value of the house and interest-only (non-amortizing). The second (junior) mortgage covers an additional 10-25% of the value of the house and must be amortized over 10-15 years. Those 80-95% combined LTV (CLTV) levels and less-than-full amortization schedules were common in the U.S. and in many other countries at that time. Indeed, Amromin et al. (2018) shows that in the U.S. 18% of mortgages were interest-only or negative amortization (4%). Many more subprime mortgages had low initial interest rates (teaser rates) for an initial period before full amortization started. Lea (2010) documents that IO mortgages were available in 10 European countries, with 79% of all mortgages outstanding in 2009 that were IO in the Netherlands, 50% in Denmark, and 43% in the U.K. and in South Korea. In sum, the amortization schedule of Swedish mortgages was not that unusual.

Like in the U.K. and in most European countries, but unlike Denmark and the U.S., the typical mortgage is an adjustable-rate mortgage.

Mortgage underwriting considers debt service coverage, loan-to-value, and credit scores just like in any other market. Lending standards were loose at the time of our experiment, just like in many other countries, but not quite as lax as in the U.S.

How can there be much tapping into home equity when mortgages do not amortize much? The answer is twofold: Our treated households had a lot of free debt capacity to borrow against as a result of the experiment, and subsequent house price growth created substantial additional home equity. We refer to the available debt capacity plus financial wealth as the buffer; it is constructed as detailed in Appendix I. As shown in the bottom right panel of Figure 1, the buffer increases substantially in the year of treatment as well as afterwards due to healthy house price appreciation. The initial buffer arises because households can purchase their home at a substantial discount to the market value. While the typical new homeowner with a mortgage during our period of study in Sweden almost certainly did not have as much home equity as our treated households, the situation of ample and rising home equity is actually quite representative of the average homeowner in the average country in the typical rising house price environment. For example, U.S. households collectively own 66% of the value of their homes at the end of 2020, a number that has expanded from about 50% in 2010, as house prices have risen over the past decade. The same rising house price environment created home equity for Swedish home owners both during our sample of study and in the more recent housing market expansion. Paradoxically, our experiment is more relevant to the typical homeowner in the average place and time than it would have been if we had observed the average new home buyer in Stockholm in our period of study.

K.5 Rental Market and Privatization

The Stockholm rental market has substantial rent regulation. But so do many other OECD countries. Looking at the 20 richest OECD countries from 2002, a majority of countries have rent controls today; see the OECD Affordable Housing Database (PH6.1.1) and the *Tenancy Law and Housing Policy in Multi-level Europe* report. Ex-ante rent control comes in two different varieties. A small number of countries, such as Luxembourg and Switzerland, have absolute level controls based on maximum rental yields. The majority of countries tie rents to those on comparable apartments, in Germany via *Der Mietspiegel* or in Sweden with the point system. The OECD Affordable Housing Database (PH6.1.1) reports that 14 OECD members have caps on rent increases while 8 have caps on initial rent levels. Nearly all European countries have strong protections for tenants in place, making it difficult for landlords to evict tenants. Large U.S. cities such as New York and San Francisco have long had strong rent control; see Favilukis, Mabille and Van Nieuwerburgh (2021) and Diamond and McQuade (2019) for additional discussion.

Our privatization experiment is not unique. In other countries like the U.K., large privatization waves under Thatcher in the early 1980s are similar to the experiment we study in Sweden in the early 2000s. The U.S. is undergoing major rent regulation reform right now. California and Oregon passed state-wide rent control laws limiting the annual percentage increase in rents in 2019. Similarly, New York State passed the largest rent regulation reform since 1974 in June 2019. California, New York, New Jersey, Maryland, Oregon, and the District of Columbia now all have rent control laws with similar legislative pushes underway elsewhere. Our paper, which studies the effects of a privatization——and hence a weakening of rent control— can be informative about the effects of a strengthening of rent regulation observed in the U.S. today. The strong push towards expansion of rent control over the past three years in the United States has made our setting, if anything, more relevant.

K.6 Social Insurance and Self-Insurance

The Swedish “welfare state" provides strong social insurance. That said, Sweden is similar to various other developed countries. Among the 20 richest OECD countries, Sweden is ranked 12th by GDP per capita, 6th by life expectancy, 10th by health care expenditures, and 9th by employment protection. There are several other European countries with similar levels of income inequality after government taxes and transfers. Data for 2013 on the income Gini coefficient across OECD countries from the 2017 OECD publication “Government at a Glance" show that Sweden is right in the middle of the European countries when it comes to income inequality after taxes and transfers. Nine countries have lower inequality after transfers and taxes, including all other Scandinavian countries (Norway, Denmark, Finland, and Island), but also Belgium, Austria, Slovenia, and the Czech Republic. Luxembourg, the Netherlands, Germany, and France are all close. Sweden’s Gini coefficient of 0.281 is not that far below the OECD average of 0.311. Also, before tax-and-transfer

inequality is not that unusual in Sweden. Again, nine OECD countries have lower before tax-and-transfer inequality than Sweden. Finally, if we define social insurance provision by the log difference between the pre-government and post-government income Gini coefficient, 17 OECD countries provide more social insurance than Sweden. The presence of robust social insurance in Sweden makes our housing collateral effect–where we show that owners are able to smooth through an income shock whereas renters are not–all the more interesting.

Finally, when it comes to the importance of self-insurance, Kaplan, Violante and Weidner (2014) compare the share of hand-to-mouth households in the United States, Canada, Australia, the United Kingdom, Germany, France, Italy, and Spain. This share is around 30%. Andersson and Vestman (2021) extend this comparison to Sweden and find a similar share of hand-to-mouth households. This shows that Swedes are not in better financial shape than households in other countries in terms of financial wealth that they have at their disposal to insure against events and shocks not covered by social insurance.

L Additional results

This sections provides further details on our results.

L.1 Summary statistics for sub-groups

Table A.14 reports the same kind of summary statistics as Table 1 for additional sub-groups of the sample.

L.2 Main consumption results

Table A.15 reports all coefficient estimates corresponding to Table 2.

Table A.14: Averages Characteristics Before Treatment

	Treated	Control	Young	Old	Stayers	Movers	Movers (Owner)	Movers (Renter)
Panel A: Sociodemographics								
Age	45.06	43.95	33.28	50.88	45.54	40.83	39.67	41.61
High school	0.43	0.44	0.44	0.44	0.45	0.41	0.36	0.44
Post high school	0.48	0.42	0.46	0.43	0.42	0.49	0.56	0.44
Partner	0.40	0.31	0.30	0.36	0.33	0.34	0.48	0.25
Number of workers per hh	1.44	1.32	1.28	1.40	1.33	1.42	1.56	1.33
Unemployed	0.14	0.16	0.19	0.13	0.15	0.15	0.12	0.18
Income shock 25% (Z_{it})	0.09	0.10	0.13	0.08	0.09	0.11	0.08	0.12
Move	0.01	0.01	0.02	0.01	0.00	0.05	0.01	0.07
Panel B: Balancesheets								
Homeowner (D(Own)$_i$)	0.04	0.04	0.04	0.04	0.04	0.05	0.11	0.01
Housing wealth	29.03	24.48	32.41	21.92	22.55	34.92	77.66	5.92
Financial wealth	86.28	85.06	64.50	97.97	89.73	73.59	92.23	60.95
Debt	95.48	91.34	107.20	83.82	81.55	122.95	156.71	100.04
Net worth	78.35	57.35	10.39	95.57	72.33	39.75	82.62	10.65
Buffer	424.46	407.03	372.07	436.35	415.91	402.20	454.87	366.47
Risky share (uncond.)	0.21	0.19	0.20	0.19	0.19	0.20	0.26	0.16
Risky share (cond.)	0.35	0.34	0.35	0.34	0.35	0.34	0.37	0.32
Panel C: Cashflows								
Income	161.51	161.13	154.12	165.51	159.95	164.79	173.25	159.05
Consumption	143.17	146.14	140.99	147.80	140.59	158.08	166.00	152.71
Panel D: Apartments								
Distance to center (km)	7.89	7.01	6.97	7.45	7.28	7.25	7.64	6.99
Area (m^2)	72.40	74.75	69.53	76.74	74.94	71.56	73.89	69.97
Rent per year	38.80	42.71	39.81	42.58	42.19	39.75	37.90	41.01
Vote share	0.73	0.74	0.74	0.73	0.74	0.74	0.74	0.74
Panel E: Approved coop								
Conversion price per m^2 (p_0^c)	8.67		8.68	8.67	8.50	9.05	8.59	9.96
Market price per m^2 (p_0)	18.21		18.87	17.92	17.87	18.97	18.16	20.57
Discount fraction (τ)	0.54		0.55	0.53	0.54	0.54	0.55	0.52
Wealth shock ($\widetilde{W}$)	85.16		37.26	107.08	90.18	74.06	62.21	97.51
Apartment value (HW_0)	813.14		770.99	833.50	803.81	834.82	795.15	905.76
Panel F: Neighborhoods								
Predicted conv. price per m^2 ($p_0^{c,nbd}$)	9.08	9.78	9.81	9.42	9.56	9.59	9.28	9.79
Predicted market price per m^2 (p_0^{nbd})	18.79	19.57	19.68	19.13	19.34	19.31	18.86	19.61
Predicted wealth shock ($\widetilde{W}^{nbd}$)	86.06	88.73	38.76	117.40	94.08	71.00	62.61	76.69
Predicted apartment value (HW_0^{nbd})	866.99	992.67	923.23	974.01	971.89	908.43	858.03	942.63
Number of households	529	1235	661	1103	1294	470	190	280

Notes: The table presents averages of variables for the Fixed sample, as Table 1, but broken down into various subgroups. all Fixed sample households (first columns) and separately for households in successful privatization attempts (treated; second column) and failed attempts (control; third column) in the household formation year $k = -1$. Age and education refer to the highest age or education level among the household members. Partner refers to households with two adults who are married, have a civil partnership, or at least one child together. Unemployed refers to a dummy variable that indicates if any unemployment insurance was received by any household member during the year. Risky share (cond.) refers to the share of risky assets out of financial wealth conditional on stock market participation in the year of household formation. The construction of the neighborhood variables in Panel F is described in Appendix H. With the exception of variables per individual or in ratios, all variables are denominated in SEK 1,000 per adult equivalent according to the OECD formula and deflated by the consumer price index.

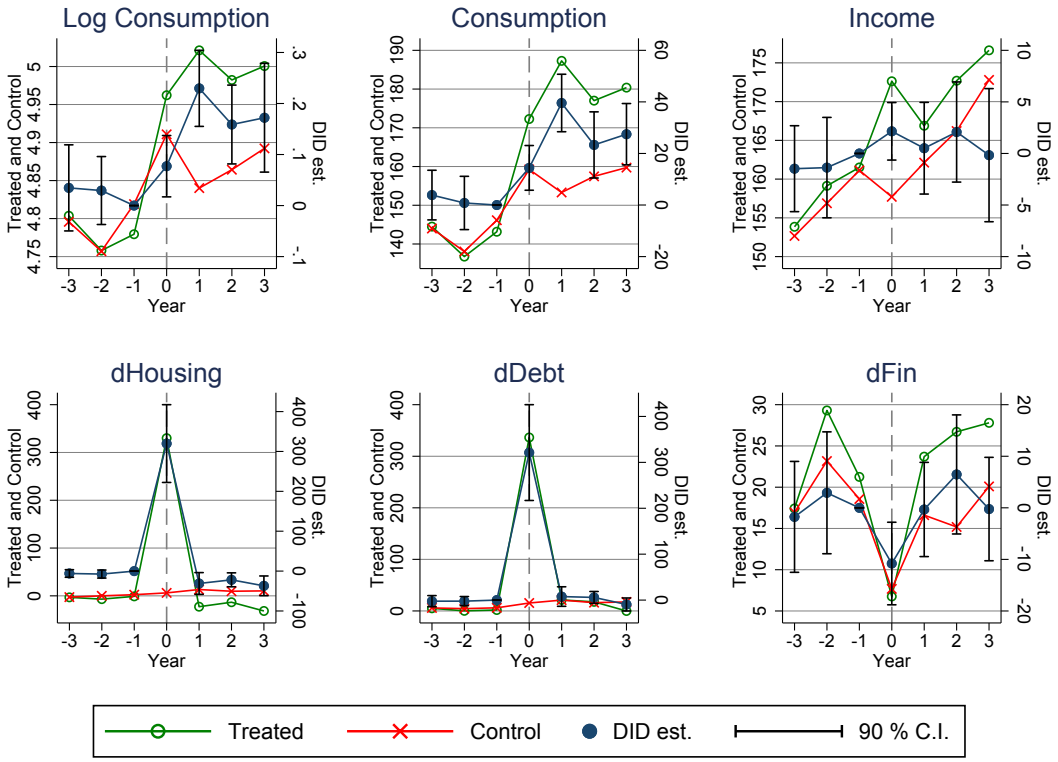
Table A.15: Reduced form estimates corresponding to Table 2

	(1) Log cons.	(2) Cons.	(3) Income	(4) dHouse	(5) dDebt	(6) dFin
Priv.$_i$ × RY$_{it}$(Pre)	0.032	2.431	-1.425	-6.661	-2.391	0.369
	(0.04)	(5.40)	(2.39)	(4.43)	(6.11)	(6.07)
Priv.$_i$ × RY$_{it}$(0)	0.078**	14.462**	2.281	319.737***	321.203***	-10.738**
	(0.04)	(5.23)	(1.64)	(57.68)	(61.78)	(4.77)
Priv.$_i$ × RY$_{it}$(Post)	0.185***	29.680***	0.784	-31.284**	-0.603	1.821
	(0.05)	(5.61)	(2.80)	(12.11)	(7.03)	(5.06)
RY$_{it}$(Pre)	-0.031	-5.881*	2.103	-5.864	-8.463	5.305
	(0.03)	(3.38)	(2.02)	(6.06)	(5.70)	(4.76)
RY$_{it}$(0)	0.025	2.407	0.888	4.370	6.546	0.719
	(0.03)	(5.54)	(1.53)	(6.39)	(8.60)	(4.27)
RY$_{it}$(Post)	-0.001	3.109	2.217	-16.217	-14.460	0.947
	(0.05)	(9.57)	(2.50)	(23.33)	(22.94)	(6.23)
PreTreat_Mean	4.78	142.49	157.03	-1.18	4.61	20.26
PreTreat_SD	0.64	88.63	75.44	52.99	60.84	69.00
Observations	12857	12857	12857	12857	12857	12857
R^2	0.45	0.43	0.80	0.27	0.30	0.31

Notes: The table presents all coefficient estimates of Table 2. Year and household fixed effects are included but not reported. Outcomes are the consumption components of equation (5). All values are in SEK 1,000 and expressed per adult equivalent. Standard errors are clustered at the co-op level and reported in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Figure A.10 displays difference-in-difference estimates corresponding to the cash-flow components of Table 2

Figure A.10: Difference-in-Difference Estimates of Cash-Flows



Notes: The figure displays estimates corresponding to the cash-flow responses reported in Table 2. The panels depict the effects on cash-flows for the treatment and control groups (left vertical axes) and difference-in-difference estimates (right vertical axes). The difference-in-difference estimates are based on the regression specification in equation (6). All values are in SEK 1,000 and scaled by adult equivalents. Confidence intervals based on clustering at the co-op level.

L.3 Labor supply

Table A.16 reports estimates for labor income and various labor market-related outcome variables (gross labor income, parental and sick leave benefits, unemployment benefits, distance from work, transitions to more or less volatile industries, etc.).

L.4 The instrumental variable regressions

Table A.17 reports the reduced-form estimates with the neighborhood wealth shock as the instrument, as in equation (7).

Table A.16: Labor supply responses

	Panel A: Labor income and transfers				
	Labor income	Number of workers	D(Unemployment)	D(Sick leave or parental leave)	D(Adult in educ.)
Priv.$_i$ × RY$_{it}$(Pre)	0.598	0.011	-0.002	-0.013	0.010
	(3.71)	(0.02)	(0.02)	(0.03)	(0.01)
Priv.$_i$ × RY$_{it}$(0)	3.316	0.012	-0.010	0.014	-0.025**
	(4.20)	(0.02)	(0.02)	(0.03)	(0.01)
Priv.$_i$ × RY$_{it}$(Post)	2.832	0.020	0.001	-0.053	-0.017
	(5.08)	(0.04)	(0.02)	(0.04)	(0.01)
PreTreat_Mean	192.86	1.36	0.16	0.34	0.07
N	12426	12857	12857	12857	12857
r2	0.80	0.77	0.49	0.47	0.67

	Panel B: Employers				
	D(Change of employer)	D(Entrepreneur)	Distance between work-home (km)	D(Change to more volatile sector)	D(Change to less volatile sector)
Priv.$_i$ × RY$_{it}$(Pre)	0.014	0.013	0.203	0.003	0.011
	(0.02)	(0.01)	(2.79)	(0.01)	(0.01)
Priv.$_i$ × RY$_{it}$(0)	0.051*	-0.010	-3.475	0.002	0.012
	(0.02)	(0.01)	(2.84)	(0.01)	(0.02)
Priv.$_i$ × RY$_{it}$(Post)	0.026	0.013	-2.968	-0.003	0.005
	(0.02)	(0.01)	(3.11)	(0.01)	(0.01)
PreTreat_Mean	0.15	0.08	12.64	0.03	0.03
Observations	12857	12857	9040	12857	12857
R^2	0.26	0.76	0.47	0.16	0.16

Notes: The table presents reduced form effects on labor income, transfers and the likelihood of going to post-high school training (Panel A), and effects on matches with employers (Panel B). The regression estimates a complete difference-in-difference specification (equation (6)), but only the estimates associated with the treated are reported. Standard errors, clustered at the level of the co-op, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Table A.17: Reduced form estimates: neighborhood wealth shock

	(1)	(2)	(3)	(4)	(5)	(6)
	Log cons.	Cons.	Income	dHouse	dDebt	dFin
Priv.$_i$ × RY$_{it}$(Pre)	0.055	10.095*	-1.209	8.573	4.673	-15.208**
	(0.04)	(5.49)	(3.01)	(5.71)	(7.21)	(4.50)
Priv.$_i$ × RY$_{it}$(0)	0.030	10.494	2.323	176.063***	183.581***	-0.687
	(0.05)	(7.58)	(2.14)	(42.55)	(46.31)	(5.03)
Priv.$_i$ × RY$_{it}$(Post)	0.225***	34.385***	0.463	-8.252	15.018**	-10.583**
	(0.05)	(6.55)	(2.98)	(8.91)	(5.84)	(4.51)
$\widetilde{W}^{\text{nbd}}$ × Priv.$_i$ × RY$_{it}$(Pre)	-0.000	-0.097**	-0.013	-0.180***	-0.065	0.199**
	(0.00)	(0.04)	(0.02)	(0.04)	(0.05)	(0.07)
$\widetilde{W}^{\text{nbd}}$ × Priv.$_i$ × RY$_{it}$(0)	0.000	0.035	0.004	1.702***	1.656***	-0.076
	(0.00)	(0.08)	(0.02)	(0.20)	(0.25)	(0.06)
$\widetilde{W}^{\text{nbd}}$ × Priv.$_i$ × RY$_{it}$(Post)	-0.001	-0.075	-0.008	-0.303**	-0.194***	0.175**
	(0.00)	(0.06)	(0.03)	(0.11)	(0.05)	(0.07)
PreTreat_Mean	4.78	142.49	157.03	-1.18	4.61	20.26
PreTreat_SD	0.64	88.63	75.44	52.99	60.84	69.00
Observations	12857	12857	12857	12857	12857	12857
R^2	0.454	0.430	0.806	0.312	0.343	0.310

Notes: The table presents estimates based on the regression specification in equation (7). All values are in SEK 1,000 and scaled by adult equivalents. Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Table A.18 reports first-stage estimates for column (3) of Table 3. Table A.19 reports first-stage estimatess for column (5) of Table 3. Table A.20 reports all coefficient estimates corresponding to Table 3.

Table A.18: First-stage estimation for column (3) of Table 3

	(1)	(2)	(3)	(4)
Dependent variable:	$\text{own}_i \times \text{RY}_{it}(0)$	$\text{own}_i \times \text{RY}_{it}(\text{Post})$	$\widetilde{W} \times \text{RY}_{it}(0)$	$\widetilde{W} \times \text{RY}_{it}(\text{Post})$
$\widetilde{W}^{\text{nbd}} \times \text{Private}_i \times \text{RY}_{it}(\text{Pre})$	-0.000	0.000	-0.002	-0.006
	(0.00)	(0.00)	(0.00)	(0.02)
$\widetilde{W}^{\text{nbd}} \times \text{Private}_i \times \text{RY}_{it}(\text{R0})$	-0.000	0.000	0.668***	0.012*
	(0.00)	(0.00)	(0.07)	(0.01)
$\widetilde{W}^{\text{nbd}} \times \text{Private}_i \times \text{RY}_{it}(\text{Post})$	-0.000	-0.000	0.004	0.673***
	(0.00)	(0.00)	(0.00)	(0.08)
$\text{Priv.}_i \times \text{RY}_{it}(\text{Pre})$	0.001	0.010*	0.025	-0.061
	(0.00)	(0.01)	(0.37)	(0.63)
$\text{Priv.}_i \times \text{RY}_{it}(\text{R0})$	0.837***	0.009*	15.816***	-0.215
	(0.02)	(0.00)	(3.93)	(0.44)
$\text{Priv.}_i \times \text{RY}_{it}(\text{Post})$	0.002	0.852***	-0.270	16.282***
	(0.00)	(0.02)	(0.29)	(4.23)
$\widetilde{W}^{\text{nbd}} \times \text{RY}_{it}(\text{Pre})$	-0.000	0.000*	-0.001	-0.011
	(0.00)	(0.00)	(0.00)	(0.03)
$\widetilde{W}^{\text{nbd}} \times \text{RY}_{it}(\text{R0})$	0.000	-0.000	-0.000	0.019
	(0.00)	(0.00)	(0.01)	(0.03)
$\widetilde{W}^{\text{nbd}} \times \text{RY}_{it}(\text{Post})$	0.000*	-0.000	-0.046	0.044
	(0.00)	(0.00)	(0.06)	(0.06)
$\text{RY}_{it}(\text{Pre})$	-0.000	-0.032**	0.384	-0.417
	(0.00)	(0.01)	(0.31)	(2.21)
$\text{RY}_{it}(\text{R0})$	0.036**	0.031**	-0.920	-0.409
	(0.01)	(0.01)	(1.52)	(2.01)
$\text{RY}_{it}(\text{Post})$	-0.059**	0.098***	1.404	-1.976
	(0.02)	(0.02)	(3.37)	(2.86)
Observations	12857	12857	12857	12857
F-stat on excluded instruments	337.57	457.36	38.75	44.55

Notes: The table reports the first-stage estimates corresponding to column (3) of Table 3. Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Table A.19: First-stage estimation for column (5) of Table 3

	(1)	(2)	(3)	(4)
Dependent variable:	$\tau HW_i \times \text{RY}_{it}(0)$	$\tau HW_i \times \text{RY}_{it}(\text{Post})$	$\text{own}_i \times \text{RY}_{it}(0)$	$\text{own}_i \times \text{RY}_{it}(\text{Post})$
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times \text{Priv.}_i \times \text{RY}_{it}(\text{Pre})$	0.009	0.006	0.000	-0.000
	(0.01)	(0.01)	(0.00)	(0.00)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times \text{Priv.}_i \times \text{RY}_{it}(\text{R0})$	0.559***	0.010	-0.000*	0.000
	(0.09)	(0.01)	(0.00)	(0.00)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times \text{Priv.}_i \times \text{RY}_{it}(\text{Post})$	0.012*	0.555***	0.000	-0.000
	(0.01)	(0.09)	(0.00)	(0.00)
$\text{Priv.}_i \times \text{RY}_{it}(\text{Pre})$	-5.405	-3.528	-0.002	0.010
	(4.58)	(4.25)	(0.00)	(0.01)
$\text{Priv.}_i \times \text{RY}_{it}(\text{R0})$	122.444***	1.502	0.857***	0.004
	(34.04)	(5.46)	(0.03)	(0.01)
$\text{Priv.}_i \times \text{RY}_{it}(\text{Post})$	-4.309	126.036***	-0.000	0.867***
	(4.16)	(34.80)	(0.00)	(0.03)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times \text{RY}_{it}(\text{Pre})$	-0.007	0.014	-0.000	0.000
	(0.01)	(0.04)	(0.00)	(0.00)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times \text{RY}_{it}(\text{R0})$	0.024	0.002	0.000	-0.000
	(0.02)	(0.04)	(0.00)	(0.00)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times \text{RY}_{it}(\text{Post})$	0.005	0.027	0.000	0.000
	(0.06)	(0.05)	(0.00)	(0.00)
$\text{RY}_{it}(\text{Pre})$	5.728	-18.264	0.002	-0.026
	(4.47)	(21.40)	(0.00)	(0.02)
$\text{RY}_{it}(\text{R0})$	-19.694	7.394	0.032*	0.028
	(16.28)	(16.40)	(0.02)	(0.02)
$\text{RY}_{it}(\text{Post})$	-17.279	-2.917	-0.046*	0.087**
	(24.56)	(19.86)	(0.03)	(0.03)
Observations	12857	12857	12857	12857
F-stat on excluded instruments	37.38	49.78	385.72	410.90

Notes: The table reports the first-stage estimates corresponding to column (5) of Table 3. Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Table A.20: Instrumental variable estimates on consumption (corresponding to Table 3)

	(1)	(2)	(3)	(4)	(5)
$\widehat{\text{own}_i \times \text{RY}_{it}(0)}$	14.775**		1.560		-15.384
	(6.47)		(10.95)		(15.31)
$\widehat{\text{own}_i \times \text{RY}_{it}(\text{Post})}$	32.439***		32.906***		20.054
	(6.63)		(8.99)		(16.69)
$\widetilde{W}^{\text{nbd}} \times \text{RY}_{it}(\text{Pre})$	-0.141***	-0.137**	-0.140***		
	(0.04)	(0.05)	(0.04)		
$\widetilde{W}^{\text{nbd}} \times \text{RY}_{it}(0)$	0.028	-0.007	0.005		
	(0.07)	(0.08)	(0.07)		
$\widetilde{W}^{\text{nbd}} \times \text{RY}_{it}(\text{Post})$	0.054	0.025	0.092		
	(0.09)	(0.09)	(0.08)		
$\text{RY}_{it}(\text{Pre})$	7.936	8.989	7.785	17.946**	15.096*
	(5.00)	(6.26)	(5.01)	(8.57)	(8.63)
$\text{RY}_{it}(0)$	-0.346	3.756	2.990	-3.966	2.676
	(6.87)	(6.91)	(6.74)	(9.26)	(11.03)
$\text{RY}_{it}(\text{Post})$	-2.427	6.300	-4.329	-10.519	-13.493
	(12.54)	(12.78)	(12.85)	(19.72)	(20.08)
$\widetilde{W}^{\text{nbd}} \times D(t = 2000)$	0.361**	0.371**	0.398***		
	(0.11)	(0.11)	(0.10)		
$\widetilde{W}^{\text{nbd}} \times D(t = 2001)$	0.300**	0.310**	0.337**		
	(0.10)	(0.11)	(0.10)		
$\widetilde{W}^{\text{nbd}} \times D(t = 2002)$	0.198**	0.211**	0.235**		
	(0.09)	(0.09)	(0.09)		
$\widetilde{W}^{\text{nbd}} \times D(t = 2003)$	0.142*	0.164**	0.182**		
	(0.07)	(0.08)	(0.08)		
$\widetilde{W}^{\text{nbd}} \times D(t = 2004)$	0.116**	0.117**	0.116**		
	(0.04)	(0.04)	(0.04)		
$\widetilde{W}^{\text{nbd}} \times D(t = 2005)$	0.097**	0.096**	0.097**		
	(0.03)	(0.03)	(0.03)		
$\widetilde{W}^{\text{nbd}} \times D(t = 2006)$	-0.012	-0.012	-0.011		
	(0.04)	(0.04)	(0.04)		
$\widehat{\widetilde{W} \times \text{RY}_{it}(0)}$		0.157**	0.152		
		(0.08)	(0.12)		
$\widehat{\widetilde{W} \times \text{RY}_{it}(\text{Post})}$		0.208***	-0.006		
		(0.06)	(0.08)		
$\widehat{\tau HW_i \times \text{RY}_{it}(0)}$				0.039**	0.067*
				(0.01)	(0.03)
$\widehat{\tau HW_i \times \text{RY}_{it}(\text{Post})}$				0.062***	0.025
				(0.02)	(0.04)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times \text{RY}_{it}(\text{Pre})$				-0.044**	-0.041**
				(0.02)	(0.02)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times \text{RY}_{it}(0)$				0.012	0.004
				(0.02)	(0.02)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times \text{RY}_{it}(\text{Post})$				0.034	0.039
				(0.04)	(0.04)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times D(t = 2000)$				0.116**	0.110**
				(0.04)	(0.04)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times D(t = 2001)$				0.105**	0.099**
				(0.04)	(0.04)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times D(t = 2002)$				0.077**	0.073*
				(0.04)	(0.04)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times D(t = 2003)$				0.046*	0.048*
				(0.03)	(0.02)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times D(t = 2004)$				0.027*	0.028*
				(0.02)	(0.02)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times D(t = 2005)$				0.021*	0.022*
				(0.01)	(0.01)
$\tau^{\text{nbd}} HW_0^{\text{nbd}} \times D(t = 2006)$				-0.019*	-0.019*
				(0.01)	(0.01)
Observations	12857	12857	12857	12857	12857
R^2	0.01	0.01	0.01	0.01	0.01
Kleibergen-Paap F-stat	329.75	35.44	39.70	32.55	13.92

Notes: The table presents all coefficient estimates of Table 3. Year and household fixed effects are included but not reported. Estimates are based on the regression specification in equation (9). Standard errors in parentheses.The Kleibergen-Papp F-statistic reports on the test for weak instruments (see Kleibergen and Paap (2006) and Andrews, Stock and Sun (2020) for discussion). First-stage estimates for columns (3) and (5) are reported in Tables A.18 and A.19. Standard errors are clustered at the co-op level. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

L.5 Consumption smoothing across time

Table A.21 reports all coefficient estimates corresponding to Table 4.

Table A.21: Consumption Smoothing Across Time (corresponding to Table 4)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Cash-flows				Mobility	
	Log cons.	Cons.	Income	dHouse	dDebt	dFin	Move	Move up
Priv.$_i$ × RY$_{it}$(Pre)	0.082	6.681	-0.327	6.384	3.722	-9.609	0.016	-0.004
	(0.07)	(8.30)	(5.15)	(7.70)	(7.83)	(6.45)	(0.01)	(0.01)
Priv.$_i$ × RY$_{it}$(0)	0.065	14.007*	2.398	247.714***	254.699***	-4.645	-0.030	-0.023**
	(0.06)	(8.24)	(4.10)	(43.41)	(46.28)	(6.94)	(0.02)	(0.01)
Priv.$_i$ × RY$_{it}$(Post)	0.309***	47.562***	-2.546	-24.422	19.350*	-6.281	0.047**	0.044***
	(0.08)	(8.22)	(4.27)	(14.67)	(10.22)	(5.18)	(0.02)	(0.01)
RY$_{it}$(Pre)	-0.025	-10.394	1.463	-11.751	-19.942**	3.503	-0.014	-0.006
	(0.05)	(6.65)	(3.41)	(7.01)	(8.82)	(6.40)	(0.01)	(0.01)
RY$_{it}$(0)	0.086	12.264*	-1.004	6.544	18.695*	-0.983	0.002	0.015*
	(0.05)	(6.66)	(3.16)	(7.75)	(9.71)	(4.88)	(0.02)	(0.01)
RY$_{it}$(Post)	0.036	13.153	-0.887	-27.164	-9.475	3.828	0.029	0.008
	(0.07)	(17.65)	(4.56)	(28.36)	(29.25)	(8.82)	(0.04)	(0.02)
Priv.$_i$ × RY$_{it}$(Pre)	-0.070	-6.183	-1.832	-19.058**	-8.528	14.724	-0.021	0.006
D(Old)$_i$	(0.07)	(8.22)	(4.97)	(8.55)	(8.71)	(9.10)	(0.01)	(0.01)
Priv.$_i$ × RY$_{it}$(0)	0.021	1.365	-0.216	107.066*	99.870*	-8.782	-0.001	0.022**
D(Old)$_i$	(0.07)	(10.46)	(4.51)	(56.23)	(54.79)	(8.32)	(0.02)	(0.01)
Priv.$_i$ × RY$_{it}$(Post)	-0.176**	-24.897**	4.878	-8.380	-25.909**	12.214	-0.046**	-0.037**
D(Old)$_i$	(0.07)	(8.65)	(4.44)	(16.78)	(12.20)	(7.51)	(0.02)	(0.01)
D(Old)$_i$ × RY$_{it}$(Pre)	-0.011	6.332	1.122	8.873	15.957*	2.000	0.016	0.002
	(0.06)	(8.54)	(3.92)	(7.17)	(9.31)	(9.84)	(0.01)	(0.01)
D(Old)$_i$ × RY$_{it}$(0)	-0.089	-14.329	2.890	-3.322	-18.248*	2.189	0.023	-0.009
	(0.08)	(9.70)	(3.72)	(11.15)	(10.80)	(7.81)	(0.03)	(0.01)
D(Old)$_i$ × RY$_{it}$(Post)	-0.045	-14.100	4.539	20.755	-3.356	-5.605	0.008	-0.004
	(0.08)	(14.92)	(5.46)	(33.64)	(30.34)	(12.76)	(0.04)	(0.02)
Observations	12857	12857	12857	12857	12857	12857	12857	12857
R^2	0.4503	0.4284	0.8042	0.2768	0.3082	0.3065	0.1585	0.1671

Notes: The table presents all coefficient estimates of Table 4. Year and household fixed effects are included but not reported. Move is an indicator variable that takes the value of one if a household changes address in the current year and is zero otherwise. Move up is an indicator variable that takes the value of one if the household moves to a parish with higher average housing wealth per household and is zero otherwise. The regression corresponds to equation (7) where D = D(Old)$_i$ which indicates whether the household head is older than 40. The average age conditional on being younger than the cut-off value is 33 years. The average age conditional on being older than the cut-off value is 51 years. Additional mobility outcomes are reported in Table A.28. Standard errors are clustered at the co-op level and reported in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

L.6 Consumption smoothing across states

Table A.22 reports all coefficient estimates corresponding to Table 5.

Table A.22: Consumption Smoothing Across States: All Households (corresponding to Table 5)

	(1)	(2)	(3)	(4)	(5)	(6)
	Log cons.	Cons.	Income	dHousing	dDebt	dFin
Priv.$_i$ × RY$_{it}$(Pre)	0.026	2.564	-1.579	-5.388	-2.154	-0.957
	(0.04)	(5.44)	(2.02)	(4.66)	(6.60)	(6.09)
Priv.$_i$ × RY$_{it}$(0)	0.064	12.200*	2.226	317.011***	314.299***	-12.708**
	(0.04)	(6.11)	(1.68)	(58.50)	(62.12)	(5.50)
Priv.$_i$ × RY$_{it}$(Post)	0.167***	26.921***	1.238	-31.138**	-3.770	1.728
	(0.05)	(5.06)	(2.71)	(11.82)	(7.42)	(5.00)
Z_{it} × Private$_i$ × RY$_{it}$(Pre)	0.073	-0.897	1.315	-16.320	-2.588	15.988
	(0.11)	(13.29)	(6.51)	(20.36)	(15.53)	(19.45)
Z_{it} × Private$_i$ × RY$_{it}$(0)	0.135	21.318	3.174	29.203	68.218	20.866
	(0.14)	(20.23)	(8.34)	(47.01)	(52.97)	(13.15)
Z_{it} × Private$_i$ × RY$_{it}$(Post)	0.192*	29.940*	-3.746	-2.743	31.950**	0.916
	(0.10)	(16.20)	(8.82)	(26.70)	(11.25)	(13.80)
RY$_{it}$(Pre)	-0.039	-7.590**	1.862	-5.202	-8.552	6.012
	(0.03)	(3.63)	(1.86)	(6.34)	(5.68)	(5.09)
RY$_{it}$(0)	0.034	4.059	1.002	5.600	9.394	0.796
	(0.04)	(6.25)	(1.38)	(6.45)	(8.98)	(4.32)
RY$_{it}$(Post)	0.001	3.108	2.557	-14.169	-13.298	0.399
	(0.05)	(9.41)	(2.19)	(23.12)	(22.55)	(6.20)
Z_{it} × RY$_{it}$(Pre)	0.103	19.093**	5.176	-7.027	1.039	-5.763
	(0.09)	(8.00)	(4.69)	(7.89)	(8.86)	(6.97)
Z_{it} × RY$_{it}$(0)	-0.112	-17.544	-3.759	-10.922	-25.235*	-0.485
	(0.10)	(11.46)	(4.99)	(9.35)	(13.17)	(8.61)
Z_{it} × RY$_{it}$(Post)	-0.067	-5.913	-8.011	-15.087*	-6.252	6.775
	(0.06)	(6.86)	(5.53)	(8.69)	(8.06)	(8.73)
Z_{it}	-0.174**	-18.187**	-27.390***	6.241	3.836	-11.617**
	(0.05)	(5.29)	(4.25)	(6.05)	(6.71)	(5.57)
Observations	12857	12857	12857	12857	12857	12857
R^2	0.45	0.43	0.81	0.27	0.30	0.31

Notes: The table presents all coefficient estimates of Table 5. Year and household fixed effects are included but not reported. The dummy variable Z_{it} takes on the value one if the income fluctuation is -25 percent or greater in magnitude. Estimates are based on the regression specification in equation (6), extended so that all covariates are interacted with Z_{it}. Standard errors, clustered at the co-op level, in parenthesis. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

L.6.1 Details on the income fluctuations

This sections provides further details on the analysis of responses to income fluctuations.

Panel A of Table A.23 reports estimates from the regression

$$\begin{aligned} Z_{it} = \gamma_0 + \gamma_1 \text{D(Parental leave)}_{it} + \gamma_2 \text{D(Sick leave)}_{it} + \gamma_3 \text{ D(In education)}_{it} \\ + \gamma_4 \text{D(Change of employer)}_{it} + \gamma_5 \text{D(Unemployed)}_{it} + \beta X_{it} + \nu_{it}, \end{aligned} \tag{68}$$

where Z_{it} is the dummy variable that indicates whether household i experiences a negative income fluctuation in year t. The coefficients γ_1, γ_2,...,γ_5 pick up common variation between parental

leave benefits, sick leave benefits, whether an adult household member is in schooling, whether an adult household member changed employer since last year, and whether a household member receives unemployment benefits. Columns (1) to (5) of report estimates. Parental leave, sick leave, schooling and unemployment contribute to the smaller income fluctuations (up to -15%) . Schooling, change of employer, and unemployment contribute to the larger income fluctuations (-30%).

Panel B of Table A.23 reports estimates from the regression specified in equation (6) with Z_{it} as the dependent variable. The table shows no evidence of treatment effects, meaning that the income fluctuations do not seem to depend on treatment.

Table A.23: Income fluctuations – relation to observables and test of treatment response

	A. Relation to observables				
Income fluctuation:	$\Delta Y \leq -10\%$	$\Delta Y \leq -15\%$	$\Delta Y \leq -20\%$	$\Delta Y \leq -25\%$	$\Delta Y \leq -30\%$
Parental Leave	0.057**	0.043**	0.035**	0.018	0.004
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Sick leave	0.032**	0.018*	0.002	-0.008	-0.015**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Adult in Education	0.090**	0.084**	0.091***	0.094***	0.086***
	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)
Change of Employer	-0.012	-0.016	-0.016	-0.012	-0.016**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Unemployment	0.195***	0.193***	0.177***	0.163***	0.146***
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Observations	12857	12857	12857	12857	12857
R^2	0.238	0.246	0.243	0.244	0.247
	B. Reduced form responses				
Income fluctuation:	$\Delta Y \leq -10\%$	$\Delta Y \leq -15\%$	$\Delta Y \leq -20\%$	$\Delta Y \leq -25\%$	$\Delta Y \leq -30\%$
Priv.$_i$ $\times$ RY$_{it}$(Pre)	0.032	0.012	0.012	-0.002	-0.005
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Priv.$_i$ $\times$ RY$_{it}$(0)	0.015	0.020	0.012	0.010	-0.001
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Priv.$_i$ $\times$ RY$_{it}$(Post)	0.026	0.019	0.019	0.007	0.006
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Pre-treatment mean	0.15	0.12	0.10	0.08	0.07
Observations	12857	12857	12857	12857	12857
R^2	0.22	0.22	0.22	0.22	0.22

Notes: Panel A reports regressions estimates from equation (68). Panel B reports regression estimates from equation (6) with Z_{it} as the dependent variable, constructed based on different threshold values for the income shock (ΔY). Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

L.6.2 Smoothing across states for stayers

Table A.24 reports estimation results corresponding to Table 5 when the sample is restricted to households that stay.

Table A.24: Consumption Smoothing Across States: Stayers

	(1)	(2)	(3)	(4)	(5)	(6)
	Log cons.	Cons.	Income	dHousing	dDebt	dFin
Priv.$_i$ × RY$_{it}$(Pre)	0.007	0.978	-2.219	-6.452	-2.868	0.365
	(0.04)	(4.72)	(2.52)	(3.87)	(5.82)	(4.69)
Priv.$_i$ × RY$_{it}$(0)	0.038	7.924	-0.230	323.639***	315.798***	-16.011**
	(0.04)	(6.38)	(2.18)	(58.84)	(62.26)	(4.68)
Priv.$_i$ × RY$_{it}$(Post)	0.115**	15.640**	3.162	-1.283	2.685	-8.463**
	(0.04)	(5.23)	(3.10)	(4.15)	(4.54)	(3.19)
Z_{it} × Private$_i$ × RY$_{it}$(Pre)	0.011	-1.936	3.702	13.410*	2.578	-5.160
	(0.14)	(19.17)	(5.04)	(7.42)	(10.45)	(15.51)
Z_{it} × Private$_i$ × RY$_{it}$(0)	0.221*	28.230	13.064	36.367	73.961	22.532*
	(0.12)	(17.38)	(9.08)	(48.21)	(49.23)	(12.06)
Z_{it} × Private$_i$ × RY$_{it}$(Post)	0.127	14.812	9.072	15.101*	33.762**	12.859
	(0.09)	(10.51)	(6.21)	(7.65)	(15.07)	(8.60)
RY$_{it}$(Pre)	-0.012	-2.776	2.311	-1.526	-4.824	1.716
	(0.04)	(3.87)	(1.58)	(3.54)	(5.35)	(3.94)
RY$_{it}$(0)	0.064*	9.385	3.214*	-4.785	4.267	2.944
	(0.04)	(6.30)	(1.61)	(8.00)	(9.63)	(5.31)
RY$_{it}$(Post)	0.042	7.487	3.697	-39.782	-30.607	5.452
	(0.05)	(7.10)	(2.51)	(24.13)	(25.93)	(7.11)
Z_{it} × RY$_{it}$(Pre)	0.058	9.656	3.635	3.764	3.772	-5.949
	(0.10)	(9.12)	(5.98)	(4.94)	(8.42)	(8.07)
Z_{it} × RY$_{it}$(0)	-0.193*	-23.397**	-10.575	-7.726	-23.225**	-2.741
	(0.10)	(11.19)	(6.86)	(7.02)	(8.36)	(10.89)
Z_{it} × RY$_{it}$(Post)	-0.085	-11.832	-6.951	3.019	-2.593	-0.724
	(0.08)	(8.80)	(6.58)	(5.10)	(8.82)	(5.93)
Z_{it}	-0.154**	-11.918**	-25.950***	-6.162**	-3.052	-10.914**
	(0.06)	(5.53)	(5.49)	(2.94)	(5.31)	(4.93)
Observations	9438	9438	9438	9438	9438	9438
R^2	0.46	0.44	0.83	0.47	0.42	0.40

Notes: The table presents reduced form effects on the consumption components of equation (5) for the Stayers sample. It is based on the same regressions as Table 5. The dummy variable Z_{it} takes on the value one if the income fluctuation is -25% or greater in magnitude. Estimates are based on the regression specification in equation (6), extended so that all covariates are interacted with Z_{it}. Standard errors, clustered at the co-op level, in parenthesis. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

L.6.3 Robustness to shock size

Table A.25 reports estimates equivalent to Table 5 for different threshold values for Z_{it}.

L.6.4 Smoothing across states for young and old

Table A.26 reports heterogenous effects of consumption smoothing across states for young and old. All covariates of the regression specification in Table 5 have been interacted with the dummy variable D(Old)$_i$ which is equal to 1 if the household is older than 40 years.

Table A.25: Income fluctuations – robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta Y \leq -10\%$		$\Delta Y \leq -15\%$		$\Delta Y \leq -20\%$		$\Delta Y \leq -30\%$	
	Cons	dDebt	Cons	dDebt	Cons	dDebt	Cons	dDebt
$Z_{it} \times \text{Private}_i \times \text{RY}_{it}(\text{Pre})$	-6.075	-5.715	2.842	-3.407	-1.054	-2.740	-11.667	-5.101
	(10.85)	(9.88)	(13.71)	(11.58)	(10.99)	(13.97)	(11.74)	(16.74)
$Z_{it} \times \text{Private}_i \times \text{RY}_{it}(0)$	4.569	-14.589	3.069	39.157	15.406	81.975	5.318	65.237
	(14.96)	(56.80)	(16.26)	(40.53)	(17.28)	(48.84)	(20.40)	(45.83)
$Z_{it} \times \text{Private}_i \times \text{RY}_{it}(\text{Post})$	12.092	18.064*	16.415	14.533	24.060	29.539**	27.843	25.436**
	(10.09)	(9.56)	(11.08)	(8.81)	(14.60)	(11.11)	(17.29)	(12.31)
Z_{it}	-15.313**	-1.972	-12.730**	0.141	-14.943**	4.560	-16.452**	5.439
	(4.55)	(4.33)	(4.97)	(6.29)	(5.18)	(6.26)	(5.29)	(6.83)
Observations	12857	12857	12857	12857	12857	12857	12857	12857
R^2	0.43	0.30	0.43	0.30	0.43	0.30	0.43	0.30

Notes: The table presents estimates analogous to Table 5 for alternative definitions of Z_{it} (i.e., alternative threshold values of the magnitude of the income fluctuation ΔY). Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

L.7 Mobility

L.7.1 Moving probabilities for households in Stockholm

Table A.27 reports estimates from the following OLS regression for the muncipalities of Stockholm and Nacka where the Stopplag sample is located:

$$Move_{it} = \beta_0 + \beta_1 D(\text{Old})_i + \beta_2 D(\text{Own})_i + \beta_3 D(\text{Old})_i \times D(\text{Own})_i + \varepsilon_{it} \tag{69}$$

where $Move_{it}$ is an indicator variable that is equal to 1 if the household undertakes a specific kind of move, $D(\text{Old})_i$ is equal to 1 if the oldest household member is 40 years or more, and $D(\text{Own})_i$ is equal to 1 if the household owns an apartment or a single-family house.

L.7.2 Additional moving outcomes in our sample

The last two columns of Table A.21, with outcome variables Move and Move up, report all coefficient estimates corresponding to the last two columns of Table 4.

Table A.28 reports additional mobility outcomes, starting with the two main outcome variables from the main text, Move and Move up.

Table A.26: Heterogenous responses to income fluctuations depending on age

	(1)	(2)	(3)	(4)	(5)	(6)
	Log cons.	Cons.	Income	dHousing	dDebt	dFin
Z_{it}	-0.007	-6.188	-17.251***	0.400	9.660	-1.840
	(0.05)	(7.63)	(2.95)	(10.75)	(10.76)	(7.50)
$Z_{it} \times D(Old)_i$	-0.282**	-20.117*	-17.317**	13.177	-7.132	-17.460*
	(0.09)	(10.35)	(7.75)	(13.11)	(12.79)	(9.75)
Priv.$_i \times$ RY$_{it}$(0)	0.218	22.732	-2.193	66.788	116.877**	25.070
$\times Z_{it}$	(0.18)	(23.27)	(12.69)	(46.40)	(52.18)	(19.68)
Priv.$_i \times$ RY$_{it}$(Post)	0.188	41.559	-8.037	-8.674	41.474**	0.436
$\times Z_{it}$	(0.16)	(27.00)	(9.63)	(37.66)	(16.79)	(22.99)
Priv.$_i \times$ RY$_{it}$(0)	-0.127	-1.426	9.635	-103.575	-135.669**	-20.899
$\times Z_{it} \times D(Old)_i$	(0.28)	(37.04)	(20.56)	(73.70)	(62.05)	(21.99)
Priv.$_i \times$ RY$_{it}$(Post)	-0.025	-25.652	7.290	7.822	-23.065	2.115
$\times Z_{it} \times D(Old)_i$	(0.17)	(29.38)	(13.69)	(59.49)	(30.46)	(35.30)
Observations	12857	12857	12857	12857	12857	12857
R^2	0.46	0.43	0.82	0.28	0.31	0.31

Notes: The table presents estimates analogous to Table 5 but extended to include interactions with a dummy variable, $D(Old)_i$, that indicates age above age 40 in relative year -1. The variable Z_{it} is one if the income fluctuation is -25 percent or greater in magnitude. Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Table A.27: Mobility in Stockholm (1999–2007)

	(1)	(2)	(3)	(4)
	Move*	Move	Move up*	Move up
$D(\text{Old})_i$	-0.069***	-0.047***	-0.024***	-0.016***
	(0.00)	(0.00)	(0.00)	(0.00)
$D(\text{Own})_i$	0.010***	0.006***	0.004***	0.003***
	(0.00)	(0.00)	(0.00)	(0.00)
$D(\text{Old})_i \times D(\text{Own})_i$	-0.027***	-0.017***	-0.009***	-0.006***
	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.127***	0.091***	0.040***	0.028***
	(0.00)	(0.00)	(0.00)	(0.00)
Observations	3585368	3585368	3585368	3585368
R^2	0.02	0.01	0.01	0.00

Notes: The table presents OLS estimates based on regression equation (69) for every household that resides in the Stockholm and Nacka municipalities (1999–2007). Move* is equal to 1 if all household members change official address during the year, and 0 otherwise. Move is equal to 1 the first time all household members change official address, and 0 otherwise. Move up* is equal to 1 if all household members move to a parish where households' housing wealth on average is greater, and 0 otherwise. Move up is equal to 1 the first time all household members move to a parish where households' housing wealth on average is greater, and 0 otherwise. $D(\text{Old})_i$ is equal to 1 if the oldest household member is 40 years or older. $D(\text{Own})_i$ is equal to 1 if the household owns an apartment or a single-family house. Standard errors, clustered at the household level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Table A.28: Mobility for young and old

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Move	Move up (parish)	Move all HH	Move up Y (muni)	Move up Y (parish)	Move up (muni)	First move (owner)	First move (renter)
Priv.$_i$ × RY$_{it}$(Pre)	0.016	-0.004	0.025	-0.001	-0.005	-0.002	0.006	0.010
	(0.01)	(0.01)	(0.03)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
Priv.$_i$ × RY$_{it}$(0)	-0.030	-0.023**	-0.019	-0.015**	-0.027**	-0.010*	-0.009	-0.021
	(0.02)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Priv.$_i$ × RY$_{it}$(Post)	0.047**	0.044***	0.041	0.024**	0.039**	0.021**	0.064***	-0.017
	(0.02)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Priv.$_i$ × RY$_{it}$(Pre)	-0.021	0.006	-0.029	0.002	0.008	0.003*	-0.007	-0.014
D(Old)$_i$	(0.01)	(0.01)	(0.04)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
Priv.$_i$ × RY$_{it}$(0)	-0.001	0.022**	-0.001	0.014**	0.026**	0.009*	0.013	-0.014
D(Old)$_i$	(0.02)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Priv.$_i$ × RY$_{it}$(Post)	-0.046**	-0.037**	-0.031	-0.022**	-0.033**	-0.019**	-0.050***	0.005
D(Old)$_i$	(0.02)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Observations	12857	12857	12857	12857	12857	12857	12857	12857
R^2	0.1585	0.1671	0.2066	0.1756	0.1687	0.1788	0.1672	0.1526
PreTreat_Mean	0.00	0.00	0.07	0.00	0.00	0.00	0.00	0.00
F_TR0_B0_B2	0.00	0.95	0.13	0.2	0.93	0.21	0.34	0.00
F_TPost_B0_B2	0.92	0.11	0.49	0.33	0.16	0.25	0.02	0.15

Notes: The table presents reduced form effects on mobility for young and old households. All terms of regression equation (7) have been interacted with a dummy variable, D(Old)$_i$, that indicates whether the household head is older than 40. The average age conditional on being younger than the cut-off value is 33 years. The average age conditional on being older than the cut-off value is 51 years. The variable Move is equal to 1 in the year that household moves out from the original apartment. Move up (muni) and Move up (parish) is a dummy variable equal to 1 if the household moves out of the original apartment and if the move involves a move to a neighborhood, defined as municipality or parish, with higher average disposable income. The variable Move all HH is a dummy variable equal to 1 in every year when the household moves. Move up (muni) is a dummy variable equal to 1 if the household moves out of the original apartment and if the move involves a move to a munipality with higher average housing wealth per household. Move up Y is a dummy variable equal to 1 if the household moves out of the original apartment and if the move involves a move to a munipality or parish with higher average disposable income per household. First move (owner) is a dummy variable equal to 1 if the household moves out from the original apartment and if it at the end of that year continues to be an owner of an apartment or single-family house (i.e., Move$_i$ × D(Own)$_i$). First move (renter) is dummy variable equal to 1 if the household moves out from the original apartment and if it at the end of that year does not own an apartment or single-family house (i.e., Move$_i$ × $(1 - \text{D(Own)}_i)$). Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

L.8 Stayer versus Mover results

Table A.29 reports all coefficient estimates corresponding to Table 6.

Table A.29: Heterogenous Treatment Effects for Stayers and Movers (corresponding to Table 6)

	(1) Log cons.	(2) Cons.	(3) Income	(4) dHouse	(5) dDebt	(6) dFin
RY_{it}(Pre)	-0.031	-6.187*	2.208	-5.156	-8.585	4.889
	(0.03)	(3.47)	(2.03)	(5.72)	(5.43)	(4.61)
RY_{it}(0)	0.028	3.010	0.652	2.601	5.895	1.000
	(0.03)	(5.46)	(1.50)	(5.93)	(8.12)	(4.19)
RY_{it}(Post)	0.001	3.521	1.936	-17.990	-15.200	1.290
	(0.05)	(9.49)	(2.39)	(22.14)	(21.68)	(6.08)
Priv.$_i$ × RY_{it}(Pre)	0.029	4.494	-1.160	-5.668	-1.854	-1.855
	(0.04)	(4.48)	(2.67)	(3.61)	(5.56)	(3.99)
Priv.$_i$ × RY_{it}(0)	0.069*	13.909**	1.698	327.804***	325.017***	-15.004**
	(0.04)	(4.85)	(1.95)	(59.65)	(63.23)	(4.44)
Priv.$_i$ × RY_{it}(Post)	0.144**	18.363**	4.788	-2.173	3.417	-7.953**
	(0.04)	(5.18)	(3.12)	(4.32)	(4.74)	(2.91)
Priv.$_i$ × RY_{it}(Pre)	0.073	-2.291	1.161	-3.549	-4.468	2.139
×D(MoveRent)$_i$	(0.10)	(10.09)	(5.78)	(10.72)	(8.73)	(15.61)
Priv.$_i$ × RY_{it}(0)	0.057	-0.721	-3.880	25.991	40.683	11.388
×D(MoveRent)$_i$	(0.06)	(6.69)	(4.59)	(77.15)	(75.41)	(11.31)
Priv.$_i$ × RY_{it}(Post)	0.019	18.813	-30.628**	-182.497***	-70.927**	62.167**
×D(MoveRent)$_i$	(0.09)	(12.31)	(9.12)	(48.84)	(27.36)	(21.02)
Priv.$_i$ × RY_{it}(Pre)	-0.042	-12.038	0.175	16.900	15.957	11.308
×D(MoveOwn)$_i$	(0.10)	(11.47)	(6.64)	(18.52)	(14.21)	(17.47)
Priv.$_i$ × RY_{it}(0)	-0.058	-9.532	8.708**	-72.717	-70.848	20.100
×D(MoveOwn)$_i$	(0.11)	(15.92)	(4.08)	(73.29)	(67.21)	(16.51)
Priv.$_i$ × RY_{it}(Post)	0.184**	41.868**	-1.019	-78.582**	-17.143	18.529
×D(MoveOwn)$_i$	(0.09)	(14.65)	(6.30)	(29.41)	(23.98)	(12.56)
PreTreat_Mean	4.78	142.49	157.03	-1.18	4.61	20.26
PreTreat_SD	0.64	88.63	75.44	52.99	60.84	69.00
Observations	12857	12857	12857	12857	12857	12857
R^2	0.45	0.43	0.81	0.29	0.32	0.31

Notes: The table presents all coefficient estimates of Table 6. Year and household fixed effects are included but not reported. The variable D(MoveRent)$_i$ is equal to 1 in the year that household moves out from the original apartment. The variable D(MoveOwn)$_i$ is consistent with the definition of homeownership, i.e. the variable is equal to 1 if the household owns an apartment or single-family house at the end of the year. Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Table A.30 reports a specification with a simple Stayer/Mover split, where we also include the indicator variable D(Move)$_i$ interacted with the relative year effects and Priv.$_i$, so that stayers in the treatment group can be compared to stayers in the control group, and similarly for movers. Columns (7) and (8) correspond to columns (3) and (4) in Table 7.

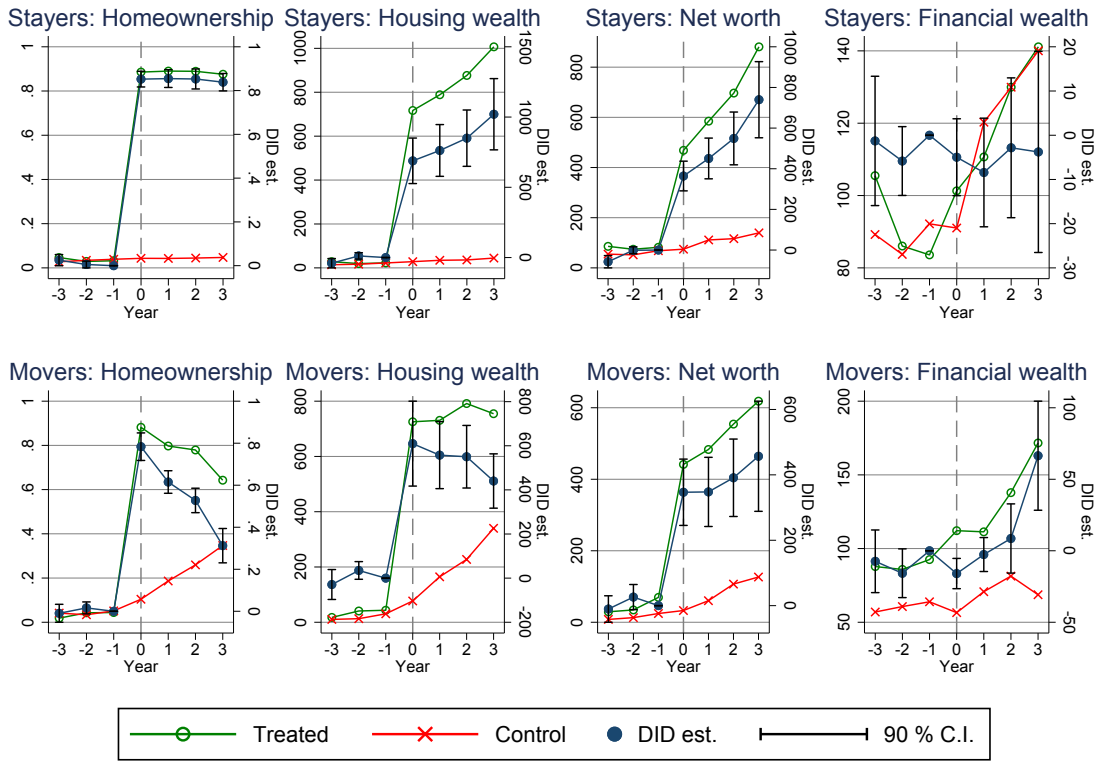
Figure A.11 reports the raw data on stayers' and movers' balance sheet items as well as the dynamic difference-in-difference estimates.

Table A.30: Cash-flows and Portfolio Choice for Stayers and Movers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cash-flows						Portfolio choice	
	Log cons.	Cons.	Income	dHousing	dDebt	dFin	RS (uncond.)	RS (cond.)
Priv.$_i$ × RY$_{it}$(Pre)	0.008	0.786	-1.942	-5.410	-2.624	0.037	0.004	0.009
	(0.04)	(4.53)	(2.72)	(3.83)	(5.78)	(4.22)	(0.01)	(0.02)
Priv.$_i$ × RY$_{it}$(0)	0.056	10.408*	0.376	327.257***	323.104***	-14.190**	0.012	0.015
	(0.04)	(5.77)	(2.18)	(59.43)	(63.19)	(4.39)	(0.01)	(0.02)
Priv.$_i$ × RY$_{it}$(Post)	0.125**	16.924**	3.732	0.145	5.758	-7.538**	0.037**	0.051**
	(0.04)	(5.34)	(3.25)	(3.96)	(4.72)	(3.06)	(0.01)	(0.02)
RY$_{it}$(Pre)	-0.009	-2.031	2.291	-1.373	-4.547	1.081	-0.012	-0.019
	(0.04)	(3.57)	(1.88)	(3.77)	(5.60)	(3.71)	(0.01)	(0.02)
RY$_{it}$(0)	0.049	7.459	2.819	-5.518	1.942	2.877	0.004	0.002
	(0.04)	(5.71)	(1.83)	(8.40)	(9.82)	(5.06)	(0.01)	(0.02)
RY$_{it}$(Post)	0.040	6.841	3.796	-39.555	-31.148	5.430	0.000	-0.019
	(0.05)	(7.25)	(2.87)	(24.42)	(26.32)	(7.05)	(0.02)	(0.03)
Priv.$_i$ × RY$_{it}$(Pre)	0.085	5.888	2.402	1.599	3.490	-1.682	0.011	0.018
×D(Move)$_i$	(0.07)	(8.75)	(4.82)	(11.81)	(11.44)	(12.61)	(0.02)	(0.03)
Priv.$_i$ × RY$_{it}$(0)	0.065	13.118	7.455*	-23.119	-6.963	10.429	-0.008	0.005
×D(Move)$_i$	(0.09)	(11.32)	(4.14)	(58.68)	(57.45)	(10.57)	(0.02)	(0.03)
Priv.$_i$ × RY$_{it}$(Post)	0.196**	40.553***	-9.047	-103.804**	-24.705	29.470*	-0.055**	-0.081**
×D(Move)$_i$	(0.07)	(9.92)	(5.90)	(34.09)	(21.18)	(15.01)	(0.02)	(0.03)
D(Move)$_i$ × RY$_{it}$(Pre)	-0.094	-17.887	0.356	-18.902	-18.796	18.280	-0.014	-0.013
	(0.06)	(11.36)	(6.63)	(22.58)	(17.50)	(14.07)	(0.02)	(0.02)
D(Move)$_i$ × RY$_{it}$(0)	-0.100	-20.792	-8.728	39.093*	18.298	-8.707	-0.013	-0.039
	(0.07)	(12.94)	(5.22)	(22.78)	(20.58)	(13.40)	(0.02)	(0.03)
D(Move)$_i$ × RY$_{it}$(Post)	-0.173*	-16.809	-8.538	89.191	63.761	-17.096	0.000	-0.016
	(0.09)	(19.33)	(6.56)	(59.42)	(52.32)	(24.64)	(0.03)	(0.04)
Observations	12857	12857	12857	12857	12857	12857	12857	7232
R^2	0.4502	0.4302	0.8044	0.2779	0.3045	0.3077	0.7590	0.6506

Notes: The table presents reduced form effects on cash-flows and portfolio choice for stayers and movers. The regression corresponds to equation (7) where $D_i = \text{D(Move)}_i$, which indicates whether the household is moving in the post years. Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Figure A.11: Effects on Homeownership and Balance Sheets for Stayers and Movers



Notes: The top panels depicts the effects on balance sheets for stayers in the treatment and control groups (left vertical axes) and difference-in-difference estimates for them (right vertical axes). The difference-in-difference estimates are based on the regression specification in equation (6). The bottom panels shows the corresponding statistics for movers in the treatment and control groups. All values are in SEK 1,000 and scaled by adult equivalents. Confidence intervals are based on clustering at the co-op level.

L.9 Balance sheets and portfolio choice

Table A.31 reports estimated effects on balance sheet items and portfolio choice based on equation (7) (i.e., corresponding to Table 2).

Table A.31: Balance Sheet Variables and Portfolio Choice

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Balance sheets					Portfolio choice	
	Own	Housing w.	Debt	Fin. w.	Buffer	RS (uncond.)	RS (cond.)
Priv.$_i$ × RY$_{it}$(Pre)	0.007	-16.842	-4.345	-5.496	-15.538**	0.007	0.014
	(0.01)	(17.70)	(9.95)	(4.62)	(7.02)	(0.01)	(0.02)
Priv.$_i$ × RY$_{it}$(0)	0.831***	659.263***	303.233***	-8.079**	235.182***	0.010	0.017
	(0.02)	(90.55)	(59.69)	(3.76)	(23.22)	(0.01)	(0.01)
Priv.$_i$ × RY$_{it}$(Post)	0.735***	780.179***	270.353***	5.539	338.257***	0.021*	0.027
	(0.02)	(104.91)	(47.25)	(8.59)	(32.51)	(0.01)	(0.02)
Pre-treatment mean	0.03	20.36	91.63	84.61	398.02	0.23	0.39
Observations	12857	12857	12857	12857	12857	12857	7232
R^2	0.81	0.74	0.69	0.89	0.87	0.76	0.65

Notes: The table presents reduced form estimates based on the regression specification in equation (6). Outcomes are balance sheet items and portfolio choice. All values are in SEK 1,000 and scaled by adult equivalents. Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Table A.32 reports estimated effects on balance sheet items for young and old households. The regression specification corresponds to columns (1) and (2) of Table 7. Table A.33 reports estimated effects on balance sheet items for stayers and movers. The regression specification corresponds to columns (3) and (4) of Table 7.

Table A.32: Balance sheets for young and old

	(1)	(2)	(3)	(4)	(5)	(6)
	Own	Housing w.	Debt	Fin. w.	Buffer	Home equity
Priv.$_i$ × RY$_{it}$(Pre)	0.000	-7.460	-1.879	-4.395	-2.647	-5.580
	(0.01)	(13.43)	(13.12)	(6.15)	(14.29)	(10.58)
Priv.$_i$ × RY$_{it}$(0)	0.781***	551.854***	243.893***	-0.709	210.419***	307.960***
	(0.03)	(75.96)	(48.47)	(5.44)	(26.79)	(35.17)
Priv.$_i$ × RY$_{it}$(Post)	0.657***	651.331***	232.640***	1.539	264.288***	418.691***
	(0.03)	(103.21)	(44.28)	(8.47)	(38.41)	(63.91)
Priv.$_i$ × RY$_{it}$(Pre)	0.016	-10.842	-1.193	-3.139	-24.175	-9.649
×D(Old)$_i$	(0.01)	(30.24)	(20.19)	(10.99)	(18.65)	(20.69)
Priv.$_i$ × RY$_{it}$(0)	0.093**	192.958**	110.143**	-13.843	40.034*	82.816**
×D(Old)$_i$	(0.04)	(78.96)	(51.45)	(10.29)	(20.37)	(31.95)
Priv.$_i$ × RY$_{it}$(Post)	0.144**	232.110**	74.688**	4.946	124.070**	157.422***
×D(Old)$_i$	(0.04)	(68.38)	(36.62)	(11.43)	(39.94)	(43.67)
Observations	12857	12857	12857	12857	12857	12857
R^2	0.8120	0.7443	0.6935	0.8944	0.8718	0.7246

Notes: The table presents reduced form effects on mobility for young and old households. The regression corresponds to equation (7) where D = D(Old)$_i$, which indicates whether the household is older than median in relative year -1. Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Figure A.12 reports the raw data on the risky share for treated and control households as well

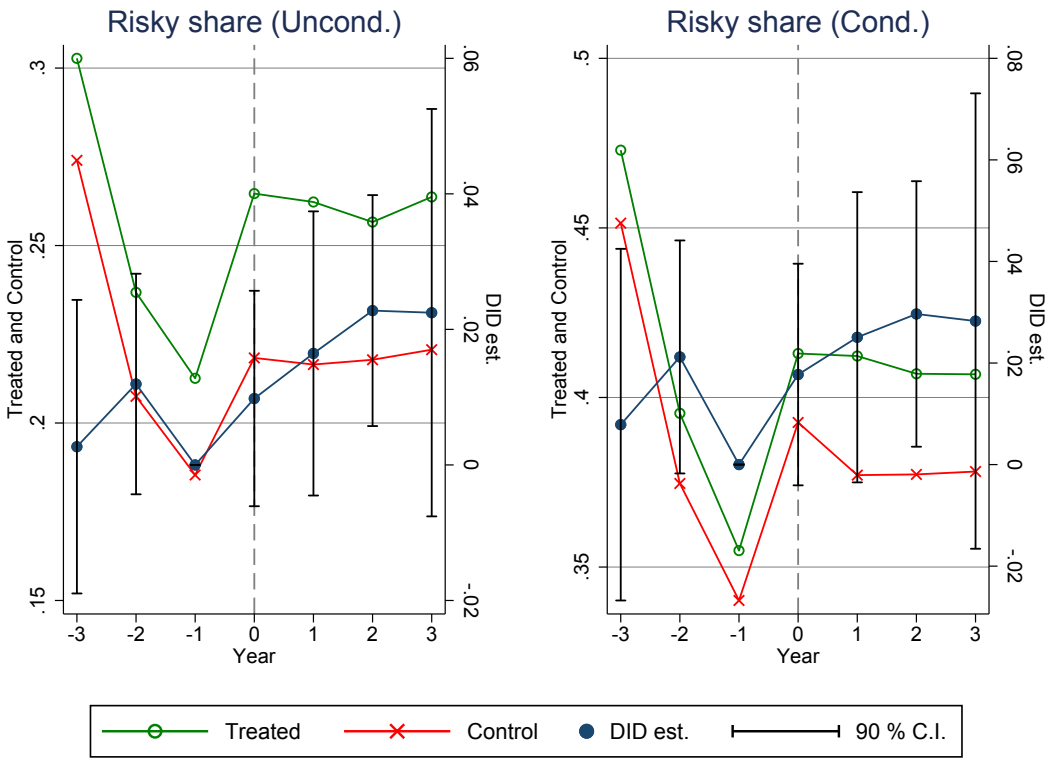
Table A.33: Balance Sheets for Stayers and Movers

	(1)	(2)	(3)	(4)	(5)
	Own	Housing w.	Debt	Fin. w.	Buffer
Priv.$_i$ × RY$_{it}$(Pre)	0.016	-16.053	4.213	-3.354	-20.621**
	(0.01)	(16.65)	(8.96)	(6.25)	(8.10)
Priv.$_i$ × RY$_{it}$(0)	0.852***	682.916***	316.457***	-4.969	244.117***
	(0.02)	(94.00)	(58.86)	(5.19)	(24.88)
Priv.$_i$ × RY$_{it}$(Post)	0.847***	893.999***	304.913***	-4.794	384.887***
	(0.02)	(128.17)	(59.62)	(9.73)	(40.95)
Priv.$_i$ × RY$_{it}$(Pre)	-0.015	15.442	-22.778	-7.937	26.575
D(Move)$_i$	(0.02)	(21.30)	(18.12)	(14.48)	(18.44)
Priv.$_i$ × RY$_{it}$(0)	-0.069	-74.969	-47.213	-11.085	-22.891
D(Move)$_i$	(0.04)	(91.80)	(58.13)	(9.17)	(24.74)
Priv.$_i$ × RY$_{it}$(Post)	-0.389***	-393.672***	-133.503**	35.427**	-146.860***
D(Move)$_i$	(0.04)	(95.20)	(63.10)	(14.66)	(35.61)
Observations	12857	12857	12857	12857	12857
R^2	0.8206	0.7494	0.6990	0.8943	0.8719

Notes: The table presents reduced form effects on balance sheets for stayers and movers. The regression corresponds to equation (7) where D_i = D(Move)$_i$, which indicates whether the household is moving in the post years. Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

as the dynamic difference-in-difference estimates.

Figure A.12: Effects on Portfolio Choice



Notes: The figure depicts the effects on the unconditional and conditional risky share for the treatment and control groups (left vertical axes) and difference-in-difference estimates (right vertical axes). The difference-in-difference estimates are based on the regression specification in equation (6). Confidence intervals are based on clustering at the co-op level.

L.10 Tax-deductible home improvements and renovations

In the household registry-based data we have two measures of home improvements and renovations. First, households that sell are taxed on their realized capital gains. Home improvements in the last five years are deductible (on tax form K6). We therefore observe home improvements for each household that sell up until 2007. Second, Sweden introduced subsidies for households that hire craftsmen such as carpenters and painters for maintenance and renovation of homes. The labor cost of craftsmen was subsidized by 50 percent up to SEK 5,000.[54] The program was called ROT and was in place between April 15 2004 and June 30 2005. Table A.34 reports reduced form effects for these outcome variables. The responses on theses variables are small relative to the overall consumption response reported in Table 2. In the Fixed sample, the average effect on ROT is 0.0461 kSEK, or 46 SEK per household and year. This can be contrasted to the 29.68 kSEK which

[54]Source: https://sv.wikipedia.org/wiki/Rot-programmet

is our estimated response on total consumption in the post years. Even if we were to adjust for the limited duration of the ROT program (15 months), the conclusion would still be that the expense on craftsmen upon treatment is small relative to the total consumption response. Tax deductible home improvments is concentrated to the sample movers (column (5)). In this group, home improvements amount to 2.47 kSEK per year in the post years. Again, this amount, less than ten percent, relative to the total consumption.

Table A.34: Renovations and home improvements

	(1)	(2)	(3)	(4)	(5)
	Fixed sample		Stayers	Movers	
	ROT	K6	ROT	ROT	K6
Priv.$_i$ × RY$_{it}$(Pre)	-0.001	-0.016	-0.002	-0.002	-0.402*
	(0.00)	(0.33)	(0.00)	(0.00)	(0.20)
Priv.$_i$ × RY$_{it}$(0)	0.002	-2.224	0.001	0.003	4.012***
	(0.01)	(3.75)	(0.01)	(0.01)	(0.86)
Priv.$_i$ × RY$_{it}$(Post)	0.046**	0.291	0.034**	0.070**	2.474**
	(0.02)	(0.72)	(0.02)	(0.03)	(0.70)
Observations	12857	12857	9438	3419	3419
R^2	0.16	0.20	0.14	0.17	0.48

Notes: The table presents reduced form effects for subsidies for craftsmen (ROT) and costs for home improvments in the last five years which are tax deductible upon a sale of the apartment (tax form K6). All values are in SEK 1,000 and scaled by adult equivalents. Standard errors, clustered at the co-op level, in parenthesis. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.