Identifying the Benefits from Homeownership: A Swedish Experiment

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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 accumulation via house price appreciation, increases consumption, and improves consumption smoothing across time and states of the world. 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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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]

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[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).

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. 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 resembles 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 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 model in which homeowners frictionlessly smooth consumption over the life cycle, 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 borrow against their housing wealth, allowing them to consume more than young renters. Treated households increase borrowing against their home equity in the wake of an adverse income shock. While renters' consumption falls by as much as the income decline, owners are able to fully offset the impact by borrowing and keep their 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 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, larger financial wealth position, and lower moving probability 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 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 (44,500 at a maximum LTV of 90%). In sum, the experiment creates a non-trivial increase in wealth (10,800 USD) but a much larger increase in housing collateral (44,500 USD), 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 some mortgage amortization leave the typical homeowner with substantial, illiquid home equity. This 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.[2] 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. The cross-section of consumption responses, then, points away from a pure housing wealth effect.

The mismatch between the observed consumption response and the response predicted by the frictionless life-cycle model indicates the presence of market incompleteness. Treated households who take up homeownership lose the present value of future implicit rental subsidies, reducing their wealth gain. Their consumption responses are abnormally large relative to the wealth gain, whereas they are normal relative to the value of homeowners' home equity increase. Indeed, our estimated consumption responses line up well with the response predicted by the incomplete markets model of Berger et al. (2018) and other empirical estimates in the literature. Simply put, homeownership increases borrowing capacity more than it does wealth (2.75 times more in our experiment).

The quasi-experiment moves the average treated household from the 54^{th} to the 71^{st} percentile of the Stockholm wealth distribution, where she stays over the next several years as subsequent capital gains allow her to maintain her position in the wealth distribution, and even as she enjoys higher consumption. Using the estimated consumption response, we quantify counter-factual wealth changes under alternative scenarios for house price growth.

Finally, we explore to what extent the consumption response is driven by additional spending on renovation, furniture, and home appliances. 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 expenditure survey shows that buyers of apartments increase their spending on maintenance, furniture, and appliances somewhat, consistent with the U.S. evidence in Benmelech, Guren and Melzer (2021). The survey also shows a strong response in non-housing related spending categories. The spending response upon homeownership appears to be broad-based.

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.[3] A much smaller branch of this

[2]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.

[3]See e.g., Rossi-Hansberg, Sarte and Owens (2010), Green and White (1997), Rossi and Weber (1996),

literature uses survey methods or quasi-experiments to study the causal effects of homeownership.[4] 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.[5] 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 young respond more to a deregulation of the credit market. The setting in DeFusco (2018) shares with ours the feature that the shock to wealth is small relative to the shock to borrowing capacity.

Third, our study contributes to the literature on the marginal propensity to consume (MPC) out of housing wealth.[6] 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. In the cross-section, the MPC out of housing wealth is declining in income, housing wealth, net worth, and age, consistent with the incomplete-markets 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; Cesarini et al., 2017). We find a labor supply response only among households who move and liquify their gain in a setting where wealth shocks are in the form of illiquid housing wealth.

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.

[4]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.

[5]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).

[6]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. Throughout we use the (standard) terminology MPC although our imputation method for consumption expenditure implies that we measure the marginal propensity to expenditure, see Laibson, Maxted and Moll (2022).

Fifth, we contribute to the literature on portfolio choice in the presence of housing.[7] 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.[8] 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.

Another branch of this literature studies rent regulation.[9] 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.

The rest of this paper is organized as follows. Section I discusses the privatization experiment and the institutional background. Section II provides a simple model that conceptualizes the experiment, the wealth shock, and its implications for consumption. Section III discusses the data. Section IV presents the estimation methodology. Section V contains the main results. Section VI discusses renovation expenditures and evidence on spending items from survey data. Section VII concludes. The Online Appendix contains further detail on the experiment, the model, data construction, and additional empirical results.

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

[8]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.

[9]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.

I. The Privatization Experiment

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

A. 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 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.

B. 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.[10] A center-right wing coalition took power in Stockholm and one of its political aims was to sell residential real estate controlled 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.

[10]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.

C. *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 ownership 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.[11] 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.[12] The practice of using municipally-owned properties for rent benchmarking was used as a device for a politically-motivated decision to halt conversions since the specific usually made up a very small portion of the complexes in question. 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.[13]

[11] Appendix A.A3 provides more information and additional examples of similar cases.

[12] Appendix A.A4 studies this Akalla case in detail.

[13] 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.

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.[14] 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.[15]

D. *Wealth transfers in quasi-experiments on homeownership*

Our privatization quasi-experiment involves voluntary take-up of homeownership. Treated households who take up homeownership must be made better off. 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. Moreover, households have access to the treatment outcome (homeownership) prior to (and in the absence of) treatment. Take-up of treatment thus involves both homeownership as well as a wealth transfer. This feature is shared with real-life policies that promote homeownership. 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 conceptualizes the wealth effect.

II. 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.

A. *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

[14] 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.

[15] Figure A1 of Appendix A.A3 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. We denote this as distance to center and use this variable to construct some of our other variables.

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:

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

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, 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.[16] 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).

B. 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

[16]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.G3 shows that rents before approval and interest expenses plus co-ops' fees after approval are approximately equal.

difference of the renter's and homeowner's consolidating budget constraints[17] 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.[18] 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 a renter household is entitled to regulated rents for the next T years, a benefit the owner forgoes. While τP_0 measures the instantaneous increase in home equity on the household's balance sheet, the net wealth effect is strictly smaller as long as $R_h < R$ and $T > 0$.[19] This setup assumes that everyone understand the value of the implicit rental subsidy.

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:[20]

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

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

[17] 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.

[18] In this model, the return on housing equals the return on financial welath. Appendix B.B5 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 $\frac{R_h}{R} = 1 - (\omega - \phi)$. A large literature shows that the rental yield is the largest component of the housing return while the capital gais return is modest (Jordà et al., 2019; Giglio et al., 2021), i.e., $R > R_h$.

[19] 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. In practise, a household that privatizes and later sells and wants to re-enter the rental 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.

[20] 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}.

$\widetilde{W}$. It is an important variable in our empirical analysis.[21] 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).[22] 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.[23]

III. 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.[24] 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 (Svenska Bostäder, 2004; Stockholmshem, 2004). 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 (Statistics Sweden, 2007*a*,*c*,*d*,*e*,*f*, 2015, 2017). 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 asset-level return data, we can construct the rate of return on a household's portfolio (Calvet, Campbell and Sodini, 2007).[25] 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:

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

[21]Notice that the wealth shock is adjusted for differences in time horizon T across households. Figure B1 in Appendix B illustrates the model-implied spending responses for different values of τP_0 and T.

[22]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.

[23]Below, we show that our main results are robust to alternative choices for R and T.

[24]See data replication package (Sodini et al., 2023).

[25]Stock and fund price data come from OMX (2009); NGM (2009); Citygate (2009); Morningstar (2009); Datastream (2009, 2016); FinBas (2016); Bach, Calvet and Sodini (2020).

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 here adds 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 (Statistics Sweden, 2016).

Fourth, we contacted treated co-op boards who, in some instances, sent information directly to Statistics Sweden about which households did not participate in the privatization but continued to rent (Co-op boards, 2014). Appendix E describes our method for identifying these residual tenants.[26]

Fifth, we digitalize co-op annual reports (Bolagsverket, 2018). 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.

A. *Event time*

We label event time as relative year k. For approved co-ops, $k = 0$ is the year in which the property transfer takes place. The median time between County Board approval and transfer is three months. 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 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 $k = 1, ..., 4$.[27]

[26]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.

[27]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 $k = 4$.

The household formation year $k = -1$ is the year in which we form our sample of tenants.[28] 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.

B. 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.[29] 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 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.[30] Total debt of

[28] 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.

[29] 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.

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

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.[31] 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.

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.[32] 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.[33] Based on the conversion price and market price

60.

[31]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.

[32]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.

[33]As long as at least one treated household in the building sells within the year, we have a market

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.[34] 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 P_0, the same notation we used in the model. The average discount for each household, τP_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. 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.[35] 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.[36] 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 $\widetilde{W}^{\mathrm{nbd}}$. The correlation between $\widetilde{W}$ and $\widetilde{W}^{\mathrm{nbd}}$ 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 P_0^{nbd} and its average value in the treatment group is SEK 866,990. The correlation between P_0 and P_0^{nbd} in the treatment group is 0.934.

C. External validity

Appendix Table K1 shows that our Stopplag sample is representative of the broader population of Stockholm renters. Furthermore, Appendix Figure K1 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

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.

[34]The wealth shock $\widetilde{W}$ includes the adjustment for the household's time horizon. The correlation between $\widetilde{W}$ and W_0 is 0.997. The mean of W_0 is 75.3 kSEK. $\widetilde{W}$ 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).

[35]The small difference reflects a high R-squared of 0.77. See Appendix H.H4 for details.

[36]We exclude all other transactions that are not at arms' length. See Appendix H.H3 for details.

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.[37] 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. That said, social insurance is generous in Sweden compared to other developed economies, and the consumption responses should be interpreted against that background. The fact that we find an important insurance role for housing collateral in Sweden suggests that the effects might be even stronger in other countries where households face more residual risk.

IV. Empirical Strategy

To establish our baseline results, we pursue a standard event study design. To get at the economic mechanisms at play, we consider heterogeneous treatment effects as well as an instrumental variables approach.

A. *Empirical specifications – baseline results*

For our baseline results, we estimate the following event study 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 privatization (1, treatment) or denial (0, control), $\mathbf{X_{it}}$ 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

[37] 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.

by the household fixed effects. The main event study coefficients of interest are the δ^k, either for relative years $K = \{-4, -3, -2, 0, 1, 2, 3, 4\}$ or a more parsimonious specification with $K = \{\text{Pre}, 0, \text{Post}\}$ 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 an event study 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. Since we expect the magnitudes of these effects to be larger than in subsequent years, it is appropriate to separate the effects in relative year $k = 0$ and relative years $k > 0$.

B. 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.

Heterogeneous 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 equal to 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 the overall effect on the old). This sample split is useful to study the question 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. In an ideal experiment, we would assign homeownership randomly and give wealth shocks of random sizes. We could then investigate whether the response to the joint homeownership-cum-wealth-shock treatment differed by the magnitude of the wealth shock. If the response varied little with the level of the wealth shock, we would learn that the response was mainly driven by homeownership. To mimic this ideal experiment, we calculate a hypothetical wealth shock based on pre-determined characteristics (mainly regional house prices, available for both treatment and control group) and test whether high wealth-shock households respond more. Concretely, among two households randomly assigned to homeownership, we test whether the one with the larger hypothetical wealth shock displays a stronger consumption response, relative to the control group. Given that the hypothetical wealth shock is based on pre-determined characteristics, we can control for possibly different time trends in these characteristics.

Mathematically, 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 II predicts that $\beta_1 = 0$ and $\beta_2 = r/(1+r)$ when consumption is the outcome variable. The literature that focuses on homeowners' economic responses to housing wealth fluctuations omits the own_i term.

Instrumental Variables Approach

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.

Our solution is to construct a hypothetical wealth shock for both treated and control households based on neighborhood-level prices. As explained in Section III, we first construct neighborhood apartment wealth $P_{0,i}^{nbd}$ as the product of apartment 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 apartment 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 in the first line of (9) are then used in the second-stage regression:[38]

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

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.

V. Main results

A. *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,

[38]The estimation proceeds in one step to ensure that the correct standard errors are used reflecting the uncertainty associated with generated regressors.

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 consume 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.[39] This suggests that the wealth building benefits from homeownership we find apply broadly.

B. *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 L1, 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 model of Section II, a model whose purpose is to help understand the impact of the wealth shock inherent in our experiment. Equation (4) predicts an average response of 5.6 kSEK ($r/(1+r) \cdot \widetilde{W}$ with $r = .07$). These quantitative differences indicate that a simple model of the wealth effect cannot account for the observed consumption response.[40] Below we discuss additional mechanisms that contribute to the large response, notably collateral constraints.

[39] Appendix L.L9 reports the wealth accumulation of our sample relative to co-op owners in general.

[40] The estimated consumption response is about 5.3 times higher than the response predicted by our simple permanent income hypothesis (PIH) model. Similarly, Berger et al. (2018) find that a calibrated PIH model underestimates consumption elasticities of a richer incomplete-markets model with borrowing constraints by a factor of 4.9.

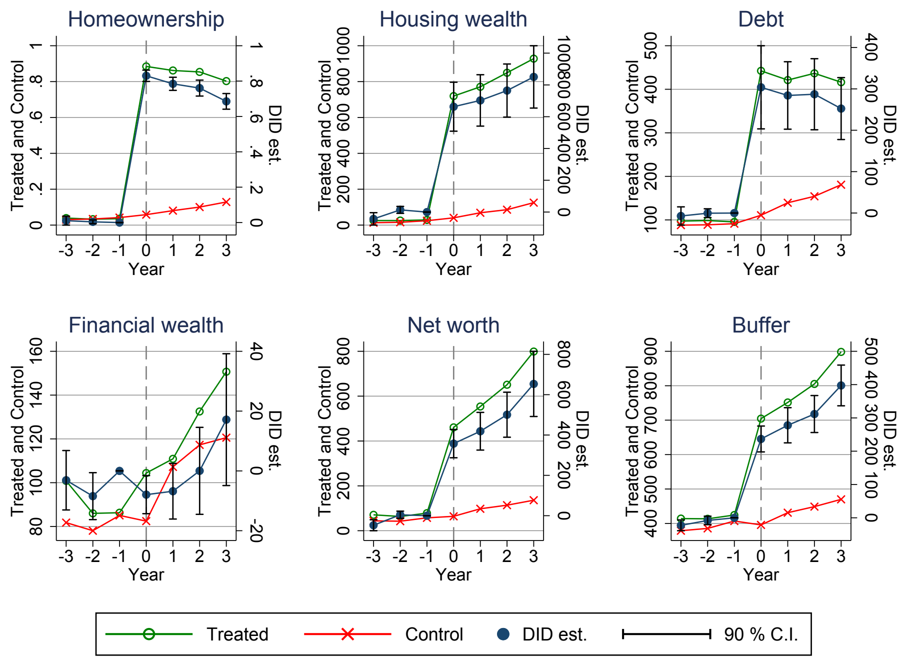

Figure 1. : Effects on Homeownership and Balance Sheets

Note: 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.

Columns (3)-(6) analyze how the consumption increase is financed. We find no effect of homeownership on disposable income. Appendix Table L3 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.[41] 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 around 30 kSEK, twice the size of the initial response and equal to more than 20% of pre-treatment consumption. On average, the consumption increase 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.

C. *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 two 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 cross-sectional 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, τP_0. The latter measures the immediate capital gain in apartment wealth at market prices and should be thought of as the increase in housing equity on households' balance sheets.[42] 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.

Our second and preferred approach is the IV estimation, detailed in Section IV. Table 3 reports the second-stage estimation results. The main result is in column (4). It displays how consumption responds to a change in homeownership and to the wealth shock; recall equation (9). The second row shows a strong effect of homeownership in the post years, 32.9 kSEK.[43] The fourth row reports a slope effect on the instrumented wealth shock of -.006 with a standard error of .08.

[41] 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 44,500 kUSD in remaining debt capacity in the year of treatment on average in the raw data.

[42] Recall that the landlord discount can be interpreted as the wealth shock in the special case where $R_h/R = 1$.

[43] This estimate is close to the 34.4 kSEK point estimate for the intercept (t-statistic of 5.25) obtained

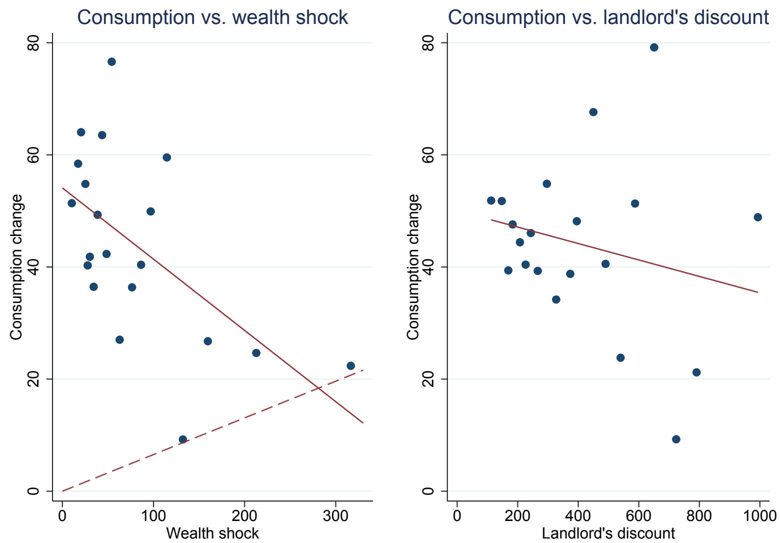

Figure 2. : Consumption changes vs. wealth shocks and landlord's discount

Note: 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: $c_{i,k} - c_{i,k-1} = 50.06 - 0.015 \cdot \tau P_{0,i}$, 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.

Consistent with the first two approaches, the IV approach indicates that there is no evidence of a positive 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.[44]

For comparison, columns (1) and (2) of Table 3 only include the homeownership dummy variable in an OLS and IV regression, respectively. The estimated

from a reduced-form estimation of equation (7) with $D_i = \widetilde{W}_i^{nbd}$. As Table L4 shows, 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.

[44]Appendix L.L5 shows that the negative cross-sectional relationship between consumption changes and the wealth shock continues to hold for alternative values of R_h/R and T that go into the construction of the wealth shock $\widetilde{W}$.

consumption response in the post period is 32.5 and 32.4 kSEK, respectively.[45] The OLS and IV estimates are very close in the post period.[46]

To illustrate the consequences of omitting homeownership from the regression (8), column (3) 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 if the model of Section 3 does not apply, for instance because consumption responses depend on borrowing constraints.

To make closer contact with the literatures on the MPC out of housing wealth, we also consider an IV regression where we instrument the landlord's discount τP_0 instead of the wealth shock $\widetilde{W}$, using neighborhood-level housing wealth $\tau^{nbd} P_0^{nbd}$ instead of $\widetilde{W}^{nbd}$ in the formation of the instruments. Studying the (instrumented) consumption response to an increase in τP_0, which measures the immediate increase in home equity on households' balance sheets, is informative for uncovering mechanisms beyond a pure wealth effect such as binding borrowing constraints or collateral effects.

Typical MPC out of housing wealth estimates recently estimated for the U.S. range from 0.03 to 0.07 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 (5) 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 (6) remedies the problem. Consistent with the findings in column (4), 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 (6) 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 home equity (τP_0) 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 τP_0 to the marginal propensity to borrow out of housing collateral. DeFusco (2018) finds

[45]The OLS estimate in column (1) is about 10% higher than the 29.7 kSEK reduced-form estimate in column (2) of Table 2, reflecting the fact that about 10% of households in the treatment group continue to rent. This is the difference between estimating the TOT effect in Table 3 versus the ITT effect in Table 2.

[46]They differ a bit more in the year of treatment, which we attribute to endogeneity. Households in the control group that choose to buy are likely to be positively selected and households in the treatment group that become residual tenants are likely to be negatively selected. This biases the OLS coefficient upwards compared to the IV estimate in the privatization year.

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 simply dividing the average treatment response (29.7 kSEK) by the average landlord discount (356 kSEK) may be more appropriate. This implies an average marginal propensity to consume of 0.083. Adding capital gains from house price appreciation in the four years after treatment increases the wealth gain by 336 kSEK, and results in an average MPC of 0.0429 (see Appendix L.L9).

To summarize, observed consumption responses are much larger than what the simple model of Section II, 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. We find substantial cross-sectional heterogeneity in the consumption response. Figure L2 shows that the consumption response is declining in the household's income, apartment wealth, net worth, and age. For low-income and young households the MPC is well above 0.10 whereas it is below 0.05 for high-income and older households. These results line up well with the incomplete-markets model of Berger et al. (2018).[47] 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 provide evidence that the large response is consistent with homeownership relaxing borrowing constraints.

D. *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.

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

[47] Consistent with these cross-sectional patterns, we find that the average consumption response to subsequent capital gains is smaller. After the initial treatment, treated households have higher housing wealth and net worth which reduces their subsequent response, consistent with the model of Berger et al. (2018).

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 out of $\widetilde{W}$ greater than unity and an MPC out of τP_0 of 0.112.[48]

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 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, the cross-sectional variation in consumption responses 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) reports that young households respond more than old households to a credit market reform.

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.

[48]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 L1 for summary statistics of $\widetilde{W}$, τ, and P_0 by subgroup.

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 L12 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 L12 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 the post years increase debt by 32.0 kSEK, about the same amount as 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.[49]

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.

Taking stock.

Homeownership allows households to meaningfully increase consumption (Tables 2 and 3). The consumption increase partly reflects consumption smoothing motives over the life-cycle or across states of nature. Yet the consumption increases are not so large that they undo the wealth-building effects of homeownership (Figure 1). While the effect on net worth depends on the in-sample house price appreciation (HPA) rate, HPA was not uncommonly large compared to other times or countries as argued in section III.C. The transition into homeownership moves the average treated household from the 54^{th} to the 71^{st} percentile of the

[49]Table L14 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 L15 shows that consumption smoothing across states is not statistically different for young and old households.

Stockholm wealth distribution in 2003. Capital gains over the next four years, net of additional consumption, allow treated households to maintain that position in 2007 (see Figure L3).[50]

We can use our estimated consumption and balance sheet response coefficients to quantify how much smaller the wealth accumulation for the average treated household would have been had HPA in the years after treatment been smaller. The calculations are in Appendix L.L9. A 0% (5%) house price appreciation during the post period would have lowered the increase in net worth during the post period by 283 (170) kSEK relative to the baseline. Because of the consumption adjustment, this reduction is smaller than the reduction in the housing capital gain. Increasing the mortgage rate from 5% to 7% would have lowered the wealth increase during the post period by 55 kSEK relative to the baseline.

E. (Upward) Mobility

Homeowners are generally thought to be less mobile than renters. But those results suffer from 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 L16 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 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 L16 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.[51]

[50] The control group is at the 52^{nd} percentile of the net worth distribution in 2003 and remains there in 2007. Among Stockholm homeowners, the treatment group is at the 36^{th} percentile in 2003 and at the 38^{th} percentile in 2007. In terms of amounts, net worth of the treated increases by 367.8 kSEK in RY(0) and by an additional 227.6 kSEK over the next four years, relative to the control group.

[51] Table L16 also shows that in the full Stockholm population, young renters are 0.6% point less likely to move than young homeowners (9.1% versus 9.7%) while old renters are 1.1% points more likely to move than old owners (4.4% versus 3.3%). Both the substantial levels of mobility among renters and the

Table L17 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 results on upward mobility add an interesting spatial aspect to the wealth-building effects of homeownership emphasized in Section V.A. The capital gains that accompany homeownership in a normal, rising housing market allow households to use the sale proceeds from their privatized apartment to make a downpayment on their next home (Ortalo-Magne and Rady, 2006).[52] They complement a literature on moving-to-opportunity (Chetty, Hendren and Katz, 2016).

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

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.[53] 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 τP_0 is .023. The housing collateral effect contributes to the strong consumption response for stayers. Table L13 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.

similar relative mobility rates of owners and renters suggest that the specifics of the Stockholm rental market do not deter mobility among renters in general. Renters can and do move without losing their position in the rent registry queue via bilateral and triangular apartment swaps.

[52]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. While one source of the wealth gain in our experiment is the capitalization of an implicit rent subsidy, there is no reason to believe that the mobility effects would be different for a wealth effect whose source was regular house price appreciation.

[53]Table L22 displays the evolution of balance sheet items for stayers and movers.

Stable homeownership provides insurance to income shocks through increased debt capacity.

Second, movers experience larger consumption increases than stayers in the post years, despite 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.[54] 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 of cash-flow over wealth changes for explaining household behavior.

G. *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 L20).[55]

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.[56]

These results show that a substantial fraction of treated households meaning-

[54]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.

[55]Figure L5 reports the raw data on the risky share for treated and control households as well as the dynamic difference-in-difference estimates.

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

fully 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 the financial wealth portfolio.

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.[57]

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 L1, 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, 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

[57] 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.

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 and HPA, 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).[58] Like the old, stayers face less uncertainty in their housing commitment, which may explain their larger portfolio responses.

VI. 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.L13 discusses the details. Table L23 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 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 G2 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.G3).

Third, we investigates consumption responses in the Swedish household expenditure survey in Appendix J. Table J1 shows that households who just purchased a new apartment meaningfully increase home maintenance, furniture, and appliance purchases. 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. However, non-housing related expenditure categories account for half of

[58]Table L21 shows that the old increase home equity by 576.1 KSEK or 72,000 USD, which implies a predicted increase of 4.9% points.

the consumption increase in the year of the apartment purchase. This evidence also suggests a consumption response beyond housing expenditure categories.

VII. 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 to 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.

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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 (P_0)		813.14		
Panel F: Neighborhoods				
Predicted conv. price per m^2 ($p_0^{\text{c,nbd}}$)	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 ($\widetilde{W}^{\text{nbd}}$)	87.93	86.06	88.73	0.90
Predicted apartment value (P_0^{nbd})	954.98	866.99	992.67	0.48
Number of households	1764	529	1235	

Note: 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 L1 reports the same statistics for sub-groups of the sample.

Table 2—: Consumption and Its Components

	(1) Log cons.	(2) Cons.	(3) Income	(4) dHouse	(5) dDebt	(6) dFin
$\text{Priv.}_{\cdot i} \times \text{RY}_{it}(\text{Pre})$	0.032	2.431	-1.425	-6.661	-2.391	0.369
	(0.04)	(5.40)	(2.39)	(4.43)	(6.11)	(6.07)
$\text{Priv.}_{\cdot i} \times \text{RY}_{it}(0)$	0.078**	14.462**	2.281	319.737***	321.203***	-10.738**
	(0.04)	(5.23)	(1.64)	(57.68)	(61.78)	(4.77)
$\text{Priv.}_{\cdot i} \times \text{RY}_{it}(\text{Post})$	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

Note: 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 L2. Standard errors are clustered at the co-op level and reported in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Table 3—: OLS and IV estimates on consumption

	(1)	(2)	(3)	(4)	(5)	(6)
$\text{own}_i \times \text{RY}_{it}(0)$	24.932***	14.775**		1.560		-15.384
	(6.53)	(6.47)		(10.95)		(15.31)
$\text{own}_i \times \text{RY}_{it}(\text{Post})$	32.552***	32.439***		32.906***		20.054
	(5.31)	(6.63)		(8.99)		(16.69)
$\widetilde{W} \times \text{RY}_{it}(0)$			0.157**	0.152		
			(0.08)	(0.12)		
$\widetilde{W} \times \text{RY}_{it}(\text{Post})$			0.208***	-0.006		
			(0.06)	(0.08)		
$\tau P_{0,i} \times \text{RY}_{it}(0)$					0.039**	0.067*
					(0.01)	(0.03)
$\tau P_{0,i} \times \text{RY}_{it}(\text{Post})$					0.062***	0.025
					(0.02)	(0.04)
Observations	12857	12857	12857	12857	12857	12857
Kleibergen-Paap F-stat		329.75	35.44	39.70	32.55	13.92

Note: Column (1) presents OLS estimates. The remaining columns present second-stage instrumental variable estimates on consumption. 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 (4) and (6) are reported in Tables L5 and L6. The complete second-stage regression estimates with all interactions are reported in Table L7. Standard errors are clustered at the co-op level. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

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
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)
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)
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

Note: 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 L10. Standard errors are clustered at the co-op level and reported in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

Table 5—: Consumption Smoothing Across States of the World

	(1)	(2)	(3)	(4)	(5)	(6)
	Log cons.	Cons.	Income	dHousing	dDebt	dFin
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)
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

Note: 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 L11. Standard errors are clustered at the co-op level and reported in parentheses.

Table 6—: Heterogenous Treatment Effects for Stayers and Movers

	(1) Log cons.	(2) Cons.	(3) Income	(4) dHouse	(5) dDebt	(6) dFin
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

Note: 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 L18. Standard errors, clustered at the co-op level, in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.

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

Note: 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 $D_i = \text{D(Old)}_i$ in the columns (1)-(2), an indicator which is one for households with head above median age, and $D_i = \text{D(Mover)}_i$ 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. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$.