Identifying the Benefits from Home Ownership: A Swedish Experiment

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Abstract

Home ownership is widely stimulated by policy yet its effects are poorly understood. Exploiting privatization decisions of municipally-owned apartment buildings, we obtain random variation in home ownership for otherwise similar buildings with similar tenants. Granular data on demographics, income, housing and financial wealth, and debt allow us to construct high-quality measures of consumption expenditures. Home ownership leads households to increase spending and to smooth consumption in the wake of an adverse income shock. We also find a positive but short-lived effect on labor supply.

Keywords: home ownership, housing wealth, MPC, collateral effect
JEL codes: D12, D31, E21, G11, H31, J22, R21, R23, R51

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Developed and developing economies alike deploy a myriad of housing policies to encourage home ownership. The United States alone spends roughly $200 billion per year in pursuit of this policy objective.[1] Policies supporting home ownership typically enjoy broad support across the political spectrum, offering a rare instance of policy agreement.[2] Conventional wisdom confers many benefits to home ownership accruing both to the individual households and to society. Despite the importance of a good understanding of how housing contributes to wealth accumulation and wealth inequality, and its obvious policy relevance, there is little empirical evidence for these alleged benefits of home ownership. Moreover, the costs of home ownership have become more salient in the wake of the foreclosure crisis of 2008-2012 in several countries, e.g., the U.S., Ireland, and Spain.

This paper studies two alleged household-level effects. First, home ownership stimulates wealth accumulation. We find no evidence for the wealth-building effect. Rather, households reduce savings and increase consumption after home ownership. Second, housing is a prime source of collateral for households to borrow against in the wake of an adverse shock. We find strong evidence that housing collateral enables households to smooth consumption after a large labor income decline.

To measure the economic effects of home ownership at the household level, the ideal experiment is one where identical households are randomly assigned into renters and owners and housing services are offered at the same cost to owners and renters. The households' 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. Hitherto, the literature has mostly resorted to simple comparison of outcomes for owners and renters. Two key endogeneity issues plague such comparisons. First, household characteristics are different for owners and renters. Owners are older, married and with children, better educated, and have higher income and financial wealth. These differences in characteristics

[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, implicit and since 2008 explicit 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 home ownership across the world (Westin et al., 2011; Cerutti, Dagher and Dell'Ariccia, 2015).

[2] This is notwithstanding the fact that such policies are often regressive. See Poterba and Sinai (2008), Jeske, Krueger and Mitman (2013), Sommer and Sullivan (2013), and Elenev, Landvoigt and Van Nieuwerburgh (2016) for studies on the distributional aspects of existing policies that favor home ownership and the consequences of repealing them. Glaeser (2011) emphasizes that policies promoting home ownership distort the rental housing market especially in dense urban areas.

correlate with tenure status, making it difficult to separate out the effect of home ownership from that of the underlying characteristics. Second, the properties that are owned and rented have different characteristics. Single-family versus multi-family building, floor area, number of bedrooms, age of the building, heating methods, neighborhood density and socio-economic make-up, and school quality can all differ. While a subset of these household- and building-level characteristics may be observable and can be controlled for, fully unbundling tenure choice and these characteristics is an uphill battle.

This paper overcomes these endogeneity issues by using a quasi-experiment which randomly assigns home ownership. In the early 2000s, tenants of municipally-owned apartment buildings in Stockholm were given the option to purchase their unit and become home owners. 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.[3]

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. What results is a complete financial picture, in terms of household balance sheet and cash-flow information, from (up to) four years before until (up to) four years after privatization. The income and wealth data enable us to construct a high-quality measure of consumption. Our focus is on estimating the causal effects of home ownership on consumption, savings, and their components. Our sample contains all 46 buildings affected by Stopplag. They collectively house 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

[3]Insights from this study may carry over to similar privatization programs carried out in the United States, 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 episodes using micro data or has exploited a quasi-natural experiment like ours.

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 equal to the net-present value of rents minus operating expenses. Tenants could purchase their apartment at a conversion fee below the market value in the ownership market. This discount allowed them to obtain personal mortgage financing for the entire amount of the conversion fee.

We refer to the initial purchase discount as the "naive windfall." A simple conceptual framework clarifies that it is only one component of the total windfall. The second component is the opportunity cost households face of giving up the rental apartment. This cost makes the total windfall substantially smaller than the naive windfall, especially for younger households. To explore how privatization effects vary by total windfall, we study exogenous variation in the windfall driven by household age and building location.

The first finding is that the take-up rate, conditional on approval to privatize, is very high. Fully 93% of tenants in approved buildings exercise their option to buy their apartment. The treatment effect on home ownership is large and persistent. While some households subsequently sell their apartment and move elsewhere, about two-thirds of households stay in place four years after the privatization. Of the movers, about two-thirds remain owner occupiers. Once conferred, home ownership remains the desired tenure status for eight out of nine households.

Our main results study the effects of home ownership on consumption and savings. We find a negative but statistically insignificant treatment effect on consumption and a positive treatment effect on savings in the year of the privatization. Households make a sizeable downpayment on the apartment they buy; they borrow less than the price they pay to acquire the unit. The downpayment is financed by a reduction in financial wealth, but also with an increase in after-tax labor income and a reduction in consumption. The initial increase in savings and decline in consumption are not driven by binding financial constraints. Treated households were far away from standard mortgage underwriting limits. We find a positive income effect, consistent with a debt-induced labor supply response.

More interesting is the response of consumption in the years following home ownership. We find that the treated increase consumption by SEK 16,500 (USD 2,200) in each of the four years following privatization. This represents 10% of average annual pre-treatment consumption in each of those four years, and the effect is precisely estimated. Average savings fall by nearly the same amount. In sum, home ownership does not result in increased savings, in contrast to the alleged wealth building benefits associated with home ownership.

The treatment effect is larger for households who are younger and who live farther from the city center. Those households receive a smaller windfall, implying that they display a much higher consumption response per unit of housing wealth. This is consistent with a pure home ownership effect, as well as with a stronger consumption effect of additional housing wealth for lower-wealth households.

The second major alleged benefits of home ownership is that housing is a collateral asset that households can draw upon in times of need. To study the use of the house as a collateral asset, we analyze how households respond to a large labor income shock (a reduction of at least 25%). We find strong evidence for the housing collateral effect. Households who become home owners as part of the privatization experiment and receive an adverse labor income shock increase borrowing to smooth consumption. Households who were denied privatization do not have this possibility, and their consumption falls nearly as much as their after-tax labor income. The collateral effect is stronger the more housing collateral a household has, and it is robust to different definitions of the income shock.

In addition to consuming more in the wake of a negative income shock, we find evidence that households consume more upon the realization of their windfall. We find a much stronger consumption response for households who sell their privatized apartment and move than for households who stay in their privatized apartment. While stayers also have the opportunity to tap into their housing wealth, they choose to do so to a much lesser extent.

Our paper relates to the empirical literature on the effects of home ownership. The earlier branch of this literature used regression control to deal with endogeneity concerns. Much of this literature studies social benefits of home ownership.[4] This paper focuses on the personal benefits from home ownership, leaving a detailed study of the social benefits for future work. A much smaller branch of this literature uses survey methods or quasi-experiments to study

[4]This literature has been inconclusive on whether or not ownership leads to better property maintenance, better outcomes for children, and more involvement with the local community. See e.g., Rossi-Hansberg, Sarte and Owens (2010), Green and White (1997), Rossi and Weber (1996), Haurin, Parcel and Haurin (2002), and DiPasquale and Glaeser (1999), respectively. Di Tella, Galiant and Schargrodsky (2007) find that giving households ownership rights to the land they inhabit affects their beliefs in free market ideals. Autor, Palmer and Pathak (2014) studies the elimination of rent control and the effect on property values in Cambridge, MA.

the causal effects of home ownership.[5] 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, measures economic 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.[6] Our paper is one of the first to trace out how an adverse labor income shock affects consumption for a household that owns a home versus one that does not. The random variation in housing wealth we observe as a result of the privatization experiment contributes a new source of identification.

Third, our study contributes to the literature on the marginal propensity to consume out of housing wealth.[7] Our MPC estimates are in line with evidence from the Great Recession and richer life-cycle models with financial constraints and risky labor income. Consistent with Berger et al. (2017), we find higher MPCs for younger, lower-income, and lower-wealth households. More generally, our study relates to a growing literature that investigates consumption and labor supply responses to windfall gains in terms in the form of cash prizes from lotteries. Fagereng, Holm and Natvik (2016) find that household balance sheet composition matters for the MPC and that the MPC is greater for smaller windfall gains. Cesarini et al. (2017) study labor supply responses to lottery winnings and find a relatively small response. Our study is complementary to theirs in that we study consumption and labor supply responses out of windfall gains received in the form of illiquid housing wealth. In related work, Browning, Gørtz and Leth-Petersen (2013) impute consumption in Danish data and investigate the impact of shocks to house prices, and Bach, Calvet and Sodini (2017) show that house price dynamics are a key force in explaining the dynamics of wealth inequality.

[5]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 home owners -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.

[6]The role of housing as a collateral asset was emphasized by Lustig and Van Nieuwerburgh (2005, 2010), Markwardt, Martinello and Sándor (2014), Leth-Petersen (2010), and deFusco (2016).

[7]See, 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. (2017), and Paiella and Pistaferri (2017). The home equity extraction channel that was operational in the United States over the same years of our study is studied in Greenspan and Kennedy (2008) and Laufer (2013).

The rest of this paper is organized as follows. Section 1 discusses the privatization experiment and the institutional background. Section 2 provides a simple framework that conceptualizes the experiment and its implications. Section 3 discusses data and estimation methodology. Section 4 contains the main causal estimates of privatization for consumption and its components as well as the housing collateral results. Section 5 concludes. The appendix contains detailed variable descriptions, additional summary statistics, and additional empirical results.

1 The Privatization Experiment

In this section, we describe the key features of the privatization experiment and the institutional background in which it took place.

1.1 The Swedish rental market

Between 1965 and 1974, Social Democrat governments in Sweden embarked on an ambitious public housing construction program (The "Million Program") which aimed to provide modern, high-quality housing to a million working- and middle-class households. Three quarters of all construction in this period was municipally-owned public housing with federal financial backing.

In 1974, the current rent-setting mechanism was introduced. Rents are set by negotiations between landlord and tenant associations. All private and public landlords are bound by the resulting rent. The law states that the rent should be set based on the location and characteristics of the apartment. Rent-setting is implemented at high granularity: by narrow geographic area, by apartment type, and by quality of finish. Rents set by municipal landlords serve as the benchmark in economy-wide rent negotiations. Given their special role 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 will exploit the institutional role of the municipal landlords, as detailed below.

1.2 Co-op privatizations

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

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. While some early experiments took place in the late 1980s and early 1990s, large-scale privatization started only after the September 1998 general election. A center-right wing coalition took power in Stockholm and one of its chief political aims was to sell residential real estate owned by the municipal landlords. In total, 12,200 apartments were privatized between 1999 and 2004. Privatizations ramped up dramatically in the year 2000 and peaked in the year 2001. These privatizations took place in the context of a broader co-op conversion process where most conversions involved private rather than public landlords. Appendix A.1 provides detailed statistics.

1.3 The Stopplag

In November 2001, the federal 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, 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. It gave substantial latitude to the County Board. Stopplag resulted in a dramatic slowdown in the pace of privatizations of municipally-owned apartments in 2003 and 2004. A careful reading of all County Board meeting minutes shows that denials were based on the argument that there would not be enough housing units of a particular type (e.g., studios in a certain neighborhood) remaining in the municipal landlord portfolios if privatization proceeded. Usually, the unit type at issue (e.g., large studios or courtyard apartments) made up only a small part of the co-op's apartment mix. Appendix A.2 describes the steps of the privatization process and Appendix A.3 provides examples of County Board denials. The randomness of the denials is well illustrated by the Akalla co-op case detailed in Appendix A.4. Our identification strategy is based on the observation that virtually identical buildings were close to randomly split into treatment (privatization) and control (denial) groups after Stopplag came into effect. As we show below, this leads to parallel pre-trends, technically the identification assumption we require.

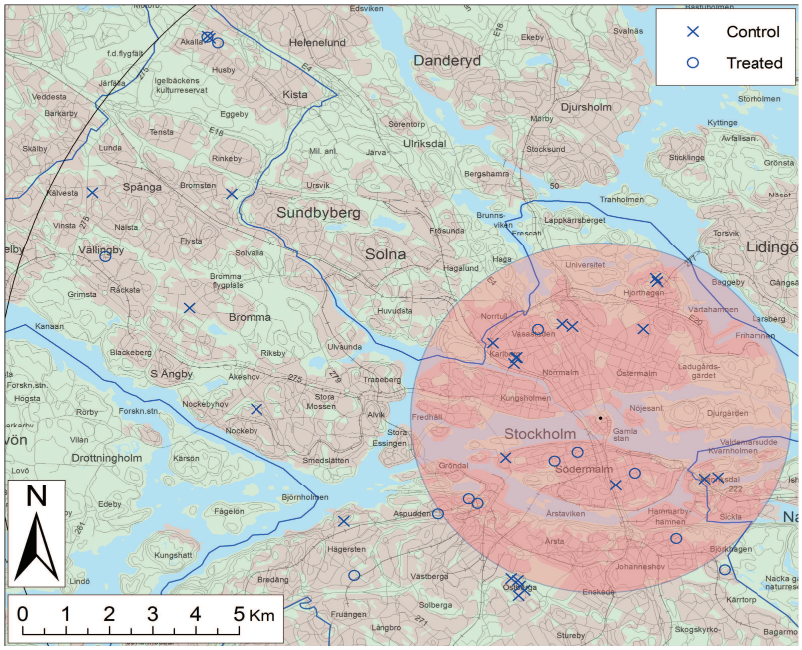
The general election of September 2002 saw the Social Democrats hold on to their majority in parliament. They upheld the Stopplag in the face of opposition. The Stopplag was abolished in June 2007, after the liberal-conservative political coalition came to power in September 2006, both nationally and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

1.4 Stopplag sample

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.[8] 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

[8]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.

Figure 1: Location of the Stopplag Sample



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

equal and high expectations of becoming home owners. 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.

Figure 1 plots the 38 co-ops on a map of the municipality of Stockholm; with circles denoting approvals and crosses denials. It also plots a shaded circle of five kilometer distance from the Royal Castle. In subsequent analysis we call the shaded area the inner city and the area outside the circle the outer city. Approvals and denials are approximately equally split between inner and outer city.

2 Conceptualizing the Privatization

This section provides a simple framework to illustrate the most basic implications of the privatization for a household.

2.1 The ideal experiment

Any reasonable experiment must involve voluntary take-up of treatment. Treated households must be made better off for two reasons. First, after privatization, treated households can choose to remain renters. Second, they have access to the treatment outcome (home ownership) prior to (and in the absence of) treatment. Treatment thus necessarily involves both home ownership as well as a wealth transfer to ensure take-up. In our context, we argue that a sizable share of the wealth transfer already took place at the time that the household began renting its apartment from their municipal landlord. The long queues to get into the municipal rental housing system corroborate its large financial benefits. Thus, entitlement to the rental contract can be viewed as the first step in two-stage treatment. This first step is a wealth transfer with a restriction on ownership. Our experiment studies random assignment in the second stage of treatment, which involves lifting the restriction on ownership, along with a smaller additional wealth transfer.[9]

The ideal experiment does not affect the per period housing expenditures. And it does not trigger binding borrowing constraints for debt associated with home ownership. We argue below that our experiment approximates the ideal setting to infer the causal effect of home ownership.

2.2 Budget implications of privatization

The landlord's perspective Prior to privatization, the landlord receives an annual rent ω_t and incurs an annual maintenance cost ϕ_t for the average apartment unit. Let the cost of capital of the landlord equal r, where $R = (1 + r)$. The political directive to the municipal

[9]Every policy that promotes home ownership is associated with a transfer. Mortgage interest deductibility, for example, redistributes wealth from all taxpayers to present and prospective home owners. Attempting to distinguish a pure home ownership effect from a pure windfall effect is therefore of little interest if the goal is to shed light on the costs of policy interventions intended to promote home ownership. That said, we will study extensively how treatment effects differ by the size of the windfall.

landlords was to set the asking price for the building such that the landlord breaks even:

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

where $(1-\tau)P_0$ is the conversion price set by the landlord, P_0 is the apartment's value on the private market for co-op shares, and $\tau > 0$ is a fractional privatization discount.

The household's perspective Consider a household that lives (in Stockholm) from $t = -1$ to $t = T \leq \infty$. The household can save and borrow in an 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 a_{-1}.

If the household is denied privatization at the start of year 0 and remains a renter until T, its per-period budget constraint is:

$$c_t^r + \omega_t + a_t = y_t + a_{t-1}R, \quad \forall t = 0, \cdots, T. \tag{2}$$

Without loss of generality, we can choose a consumption path for the renter such that financial wealth at the end of period T is $a_T = 0$. Aggregating budget constraints yields:

$$\sum_{t=0}^{T} c_t^r R^{-t} + \sum_{t=0}^{T} \omega_t R^{-t} = \sum_{t=0}^{T} y_t R^{-t} + a_{-1}R. \tag{3}$$

If instead the household is approved for privatization in year 0 and becomes a home owner, its initial budget constraint is:

$$c_0^o + \phi_0 + a_0 + (1-\tau)P_0 = y_0 + a_{-1}R, \tag{4}$$

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.[10]

[10]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 conversion process and the co-op's role in Appendix A.2.

The budget constraint from period 1 onwards reads:

$$c_t^o + \phi_t + a_t = y_t + a_{t-1}R \quad \forall t = 1, \cdots, T-1 \tag{5}$$

At the end of period T, the household sells the house for $p_{T+1}R^{-1}$:

$$c_T^o + \phi_T + a_T = y_T + a_{T-1}R + p_{T+1}R^{-1} \tag{6}$$

Aggregating budget constraints yields:

$$\sum_{t=0}^{T} c_t^o R^{-t} + \sum_{t=0}^{T} \phi_t R^{-t} = \sum_{t=0}^{T} y_t R^{-t} + a_{-1}R + P_{T+1}R^{-T-1} - (1-\tau)P_0 \tag{7}$$

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). This ensures that the household ends up with the same financial resources at the end of period T regardless of tenure status between 0 and T.

Windfall gain The windfall gain measured at the time of privatization, W_0, is the difference between the consumption stream of the owner in (7) and that of the renter in (3):

$$W_0 = \sum_{t=0}^{T} c_t^o R^{-t} - \sum_{t=0}^{T} c_t^r R^{-t} = \sum_{t=0}^{T} \omega_t R^{-t} - \sum_{t=0}^{T} \phi_t R^{-t} + P_{T+1}R^{-T-1} - (1-\tau)P_0, \tag{8}$$

Substituting in for the conversion value $(1-\tau)P_0$ from (1), we obtain:

$$W_0 = R^{-T-1}P_{T+1} - \sum_{t=T+1}^{\infty} (\omega_t - \phi_t)R^{-t} = \tau R^{-T-1}P_{T+1}. \tag{9}$$

The first equality in (9) makes clear that the owner gains the sale price of the privatized apartment discounted back to today, but effectively gives up the present value of regulated rents net of maintenance costs after time T, since their value is embedded into the landlord's conversion price set at time 0. The second equality follows from applying (1) at time $T+1$, assuming the rent regulation system remains in place. The second equality expresses the windfall as the discount fraction τ of the present-value of the apartment. It is the valuation gap between the value of the apartment in the private market and the value to the landlord, discounted back to today.

Assume that house price growth is $P_{t+1}/P_t = R_h$.[11] The difference $R - R_h > 0$ measures the dividend yield of housing, i.e., the service flow divided by the price. Then the windfall can be rewritten as:

$$W_0 = \tau P_0 \left(\frac{R_h}{R} \right)^{T+1}, \tag{10}$$

We refer to τP_0 as the "naive" windfall. 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. Equation (10) makes clear that the naive windfall overstates the true windfall because the last term is strictly smaller than 1. The longer the horizon T, the smaller the windfall. As T approaches infinity, the windfall goes to zero. The naive windfall ignores that the home owner would need to buy a new apartment at the prevailing market price after the sale, to live in until she leaves the housing market at time T. The relevant notion of the horizon is the remaining time until the household leaves the Stockholm housing market.

For realistic T, the naive windfall is a substantially upward biased estimate of the true windfall. For example, if the cum-dividend return on housing is $R = 1.07$, the capital gain component is $R_h = 1.02$, and the household stays in Stockholm for $T = 20$ years, the true windfall is only 37% of the naive windfall. If $T = 60$, for example for a 25-year old planning to remain in Stockholm until death at age 85, the windfall is only 5.4% of the naive windfall.[12] We conclude that the total windfall is much smaller than any immediately realized capital gain.

2.3 Empirical implementation

In our empirical work, we exploit cross-sectional variation in (10) to disentangle the pure home ownership effect from the windfall. We measure the naive windfall at the household level by comparing the conversion price to the market price in the same year. As long as at

[11] Given the pricing policy in equation (1), R_h is also the gross growth rate of rent ω_t and maintenance ϕ_t.

[12] 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 practice, 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. For couples, there may be a way to prevent the queueing time reset to zero by having one of the two spouses retain its position in the queue while the other privatizes. Still, the average cost of re-entry in the rental market is strictly positive, and the relevant horizon T strictly greater than the time of sale.

least one treated household in the building sells within the year, we have a market price. We apply the per square foot price of that transaction to the square footage of all apartments in the building.[13]

Measuring the total windfall W_0 requires us to take a stand on R_h/R and T. Appropriate numbers for the dividend yield on housing and real price appreciation are 5% and 2% per year ($R_h = 1.02$), for a total housing return of $R = 1.07$.[14] As a proxy for T, we use expected age at death (85) minus age at the time of the privatization.

The simple framework makes several assumptions: no risk (hence equal discount rates on all financial instruments, no portfolio choice, and risk neutrality), same maintenance costs for landlords and owners, preservation of the rent regulation system, and known horizon. In a richer framework, the windfall would take into account (income, house price, rental rate, institutional, moving) risk and discount the consumption streams of owners and renters at their stochastic discount factor (capturing risk aversion). Rather than relying on a potentially poor proxy when the true windfall is generated from a much more complex model, our strategy is to exploit easily measurable sources of heterogeneity in windfall, informed by equation (10).

The first one is whether the co-op is located in the inner or outer city; recall Figure 1. Appendix Table A6, discussed below, shows that co-ops in the inner city received much larger naive and total windfalls. This simple measure of geography captures some of the variation in τP_0. The second proxy is age. It captures some of the variation in horizon T, and hence in the second term in (10). Since younger households tend to live in smaller apartments, and the windfall is linear in square footage, age also captures variation in τP_0. The younger the household, the smaller the predicted windfall for both reasons. The third proxy is predicted windfall; the predicted value of a regression of the windfall on location, age, and age squared. Another advantage to using these three windfall proxies is that we can measure them also for the control group. Comparing households of the same age, living in the same location, and with the same predicted windfall results in cleaner inference. A final advantage is that, while the incidence of the windfall is random by virtue of our experiment, the size of the windfall may not be. The windfall proxies are measured before the treatment decision, and instrument

[13] In the absence of a transaction, we use later transactions in the same building and discount them back using a parish-level house price index, as described in the appendix.

[14] Long-run average real house price growth (1981-2008) in Sweden is 2.5% (SCB). Average rental yields ($R - R_h$) in Sweden are 5%, implying annual price-rent ratio of 20 (Global Property Guide). Our results are nearly identical if we use $R_h = 1.01$ and $R = 1.06$.

for the windfall.

Two more comments are in order. First, we have verified that the per period costs of owning and renting are indeed very similar in the data. This equivalence implies that there are no mechanical cash-flow implications from the privatization experiment. Second, financial constraints do not affect our experiment because households were able to buy their apartment at conversion prices that were far below the prevailing market price, i.e., the naive windfall was large. Every treated household in our sample has a combined loan to market value (CLTV) ratio below 70% and nearly all of them had debt-to-income ratios below 30%. Mortgages with those underwriting criteria were widely available in Stockholm during our sample period.

3 Data and Estimation

This section reports our data sources and summary statistics. Details are in Appendix B.

3.1 Sources

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 three main sources. First, we obtain County Board meeting minutes, meeting dates, and Stopplag decisions for each co-op.

The second source of data are 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 location and important dates in the privatization process (first contact between the parties, sales contract, transfer of the building if approved by the County Board). At our request, landlords also sent excerpts from their database of tenants directly to Statistics Sweden. These excerpts contain information about the rent and the size of each apartment (square meters and number of rooms) as well as the identity (social security number) of the tenant.

The third source is household-level data from Statistics Sweden. We use the tenant data bases to link the tenants to their demographic, income, and wealth information. 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). Fagereng, Guiso, Malacrino, and Pistaferri (2016) use Norwegian and Calvet, Campbell and Sodini (2007) and Bach, Calvet and Sodini (2017) Swedish wealth data to measure the returns to wealth. Data on after-tax and transfer 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:

$$Cons = Income - Savings = Income + dDebt - dHousing - dFin \tag{11}$$

Variable definitions are detailed in Appendix B.1. Consumption measures total spending. 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) to allow for housing and changes in tenure status over time, a crucial extension for our purposes. Because the wealth data are only available until 2007, our analysis spans the period 1999 to 2007. All nominal variables are deflated by the Swedish consumer price index with base year 2007.

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. We hand-collect data on these residual tenants.[15]

3.2 Household formation

There are two important dates for our experiment: the privatization year, which we call relative year 0 (RY0), and the household formation year. For privatizations approved by the County Board, RY0 is the year in which the property transfer takes place. For the co-ops that were denied, RY0 is typically set 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 through end of December, 10 remaining cases), the next calendar year is chosen to be RY0. In sum, RY0 is the first year in which our outcome variables can be expected to show a response to the conversion decision. The years after the decision year are indicated as

[15]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% of the treatment group.

RY(+k), the years before as RY(-k), for $k = 1, \cdots, 4$.[16]

The household formation year is the year in which we form our sample of tenants. This tenant sample contains the set of individuals we will track both before and after the conversion decision. The household formation year is the last year in which there is still substantial uncertainty over the outcome of the approval process. Usually, we set the household formation year equal to RY(-1), one year before the decision year.[17] 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. For simplicity we define the household head to be the oldest member of the household.

We track changes in household composition. For brevity, we focus on the sample of household-year observations where the adult composition is the same as in the household formation year.[18] In unreported results, we confirm that treatment has no effects on marriage or divorce rates, nor on the number of children in the household, justifying this focus. The sample has 1,865 households and 15,076 household-year observations; 534 households and 4,298 observations are for households in the treatment group (successful privatization attempts) while 1,331 households and 10,778 observations are in the control group (failed attempts).

3.3 Summary statistics

Table 1 reports summary statistics, measured in the household formation year. The full sample is reported in column 1, the treatment group in column 2, and the control group in column 3. The average household head is 44 years old; 42% of household heads have at most a high school degree. One third of the households have a partner and the average number of workers in a household is 1.34. The treated are more likely to be in a partnership, and correspondingly have a higher number of workers. We will control for age and partnership

[16]Our panel is unbalanced. For the co-ops with decision in 2002, RY+4 refers to the years 2006 and 2007 and we do not have RY-4. For the co-ops with decision in 2004, RY-4 refers to the combination of 1999 and 2000 and we do not have RY+4.

[17]For four co-ops we make exceptions to this rule. In these cases, the conversions 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.

[18]Appendix B.2 describes the details. Our results are similar for a larger sample of 18,281 household-year observations where we include households with changing adult composition before or after the household formation year.

in all our regressions below, and we express all nominal amounts per adult equivalent and in Swedish krona.[19] The likelihood that at least one household member is unemployed for some time during the household formation year is 15 percent for the control and 14% for the treatment group.

Table 1: Averages Characteristics Before Treatment

	All	Treated	Control
Panel A: Sociodemographics			
Number of households	1865	534	1331
Age	44.22	45.08	43.88
High school	0.42	0.39	0.43
Post high school	0.26	0.28	0.25
University	0.20	0.23	0.19
Ph.D.	0.02	0.02	0.02
Partner	0.33	0.40	0.31
Number of workers per hh	1.34	1.42	1.31
Unemployed	0.15	0.14	0.15
Panel B: Balance sheets			
Homeowner	0.04	0.03	0.05
Housing wealth	28.85	24.97	30.40
Financial wealth	106.12	118.61	101.11
Debt	103.47	104.75	102.95
Net worth	83.22	105.76	74.17
Panel C: Cash-flows			
Labor income per adult	202.66	214.44	197.92
Disposable income	174.15	177.93	172.64
Consumption	164.3	168.76	162.51
Panel D: Apartments			
Distance to center (km)	7.25	7.76	7.05
Area (sqm)	74.16	72.57	74.80
Number of rooms	2.88	2.97	2.83
Rent per year	44.48	41.82	45.54
Vote share	0.73	0.73	0.73

Notes: The table presents averages of variables for all households (first columns) and separately for households in successful privatization attempts (treated; second column) and failed attempts (control; third column) in the household formation year $RY(-1)$. Age and education refer to the highest age or education level among the household members. Partner refers to households with two adults who are married, have a civil partnership, or at least one child together. Unemployed refers to a dummy variable that indicates if any unemployment insurance was received by any household member during the year. With the exception of labor income per adult, all variables are denominated in 1000 SEK per adult equivalent according to the OECD formula and deflated by the consumer price index.

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 29,000). On average, households have SEK 106,000 in financial wealth.[20] Total debt of households equals SEK 103,500. Since there are few homeowners, debt mainly reflects student loans and unsecured debt rather than

[19] We use the OECD adult equivalence scale: 1+ (Adults-1)·0.7 + (Children)·0.5. In the household formation year the average number of adult equivalents is 1.6 (all), 1.68 (treated) and 1.57 (control). The exchange rate is approximately 7.5 SEK per USD over our sample period.

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

mortgages. Treated and control households are similar for all balance sheet variables.

Panel C shows cash flows. The average adult with positive labor income earns SEK 202,700 before tax. Our analysis also relies on non-financial income, a comprehensive measure of gross labor income plus unemployment benefits plus pension income plus transfers minus taxes. The typical household has a disposable income of SEK 174,000. Average consumption is SEK 164,000. Again, treated and control are well balanced.

Panel D compares apartment characteristics. Households live on average 7.3 kilometers from the centre of Stockholm. Treated households live, on average, only 700 meters further away. Apartments have an average floor plan of 74 square meters (about 800 square feet) and average three rooms (counting bedrooms and living room). Households pay SEK 44,500 in rent every year. Consistent with the U.S. evidence, this represents about twenty-five percent of total consumption. The last row shows that 73 percent of tenants vote in favor of a privatization, with no difference between treated and control.

A formal balance test does not reject the null of equal means of treated and control households for most variables reported in the table. What matters for our empirical strategy below is not so much a perfectly balanced sample, but rather parallel trends before the experiment.

Appendix Table A4 reports the same summary statistics broken down by co-ops located in the outer city versus the inner city.

Appendix Table A5 shows that our Stopplag sample is representative of a larger sample of all 250 co-op privatization attempts that took place during 2000-2005. It also shows that our sample is representative of the broader population of Stockholm renters. Furthermore, Appendix Figure A2 shows that the disposable income distribution of our sample households fits comfortably in the body of the Stockholm-wide distribution. This evidence suggests that our analysis is externally valid.

3.4 Estimation methodology

For a household-level outcome variable y measured in year t, we estimate:

$$y_{it} = \alpha + Private_i \sum_k \delta_k RY_i(t = k) + \sum_k \gamma_k RY_i(t = k) + X_{it} + \psi_t + \omega_b + \varepsilon_{it}, \tag{12}$$

where α is the intercept of the regression. $Private_i$ is an indicator variable which is one if household i lives in a building that was approved for privatization. Since tenants in privatized buildings are free to remain renters, (12) estimates "intention-to-treat" (ITT) effects. Recall that the decision year is not the same for all households so this is a staggered treatment. The indicator variables $RY_i(t = k)$ indicate the time relative to the conversion decision. Because of our unbalanced panel, we have fewer observations in the early years and in the later years. We employ two specifications. For the dynamic specification (reported in the figures), we bundle the years -4 and -3 into an indicator variable $RY(t = -3)$ and we bundle the years +3, +4, and +5 into an indicator variable $RY(t = +3)$. For the parsimonious specification (reported in the tables), we collapse relative years -4, -3, and -2 into one $RY(pre)$ variable, and relative years +1, +2, ..., +5 into a $RY(post)$ variable.

The coefficients γ trace out the dynamics of the outcome variable for the control group. The main coefficients of interest are $\delta_0, ..., \delta_3$. They measure the ITT effect in the conversion year and the years that follow. The assumption on parallel trends in the pre-treatment period can be evaluated by inspecting that the pre-treatment estimates $\delta_{-3}, \delta_{-2}, \delta_{-1}$ are not different from zero. Calendar year fixed effects, ψ_t, control for the aggregate trends in the outcome variables. Building fixed effects, ω_b, control for constant differences in building characteristics and the characteristics of their tenants. Control variables X_{it} allow us to control for household-specific characteristics. We include Age, Partnership, and Education in the control vector. We cluster standard errors at the co-op level because randomization occurred at the co-op level.[21]

As is standard in difference-in-difference specifications like (12), one interaction term and one RY term are not identified. We drop the terms $Private_i RY_i(t = -1)$ and $RY_i(t = -1)$. This allows us to interpret all δ estimates relative to the household formation year. The treatment and control groups have the same outcome variable in $RY(t = -1)$, conditional on the controls.

[21] Using co-op rather than building fixed effects makes almost no differences since most co-ops consist of only one building. We prefer the finer building-level fixed effects. Our results are also robust to using household fixed effects instead of co-op fixed effects.

4 Main results

This section reports estimates of (12) for our main outcome variables y_i: consumption and its components. But first, we analyze "first-stage" effects on home ownership.

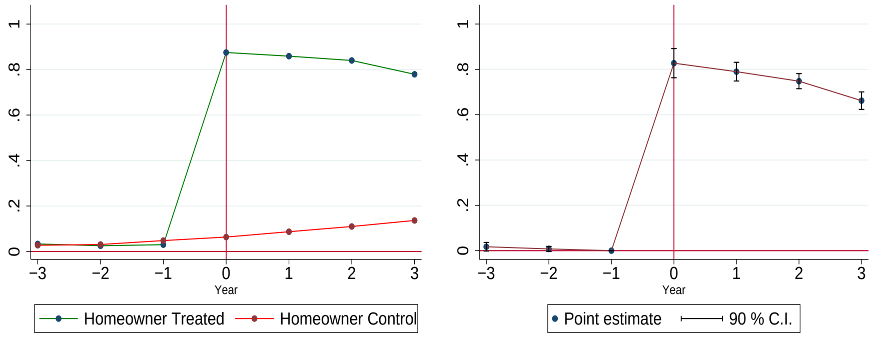
4.1 Home ownership

The left panel of Figure 2 plots the raw home ownership rate for the treatment and control groups for the years before and after privatization. The right panel plots the dynamic ITT estimates from equation (12) with an indicator variable for home ownership as the outcome variable. Home ownership is extremely low for treatment and control group pre-treatment (left panel) and shows parallel pre-trends (right panel). There is a large jump in the home ownership rate in the decision year for the treated relative to the control and relative to the household formation year RY(t=-1). The effect on home ownership persists for many years. The left panel shows that the ownership rate of the treatment group gradually falls from about 80% to about 65% over the years following privatization. About one in nine treated households sell the privatized apartment and return to rentership elsewhere. The home ownership rate among the control group rises to just below 20%. With the uncertainty of the privatization resolved, some of the tenants who are denied choose to move out and buy an apartment or house elsewhere. Nevertheless, the difference in home ownership remains above 65% three or more years after treatment. These results suggest that, once acquired, home ownership remains the preferred status for most treated households.

4.2 Consumption and savings

Two alleged benefits of home ownership are that it induces households to save and that the house is an important source of collateral that facilitates consumption smoothing in the wake of an adverse shock. We investigate those claims empirically using our quasi-experimental variation in home ownership.

Figure 2: Home Ownership around Treatment



Left panel: home ownership rate for the treatment and control group; raw data. Right panel: dynamic estimated ITT effect with standard error bands; equation (12) with home ownership as the dependent variable. Relative years -4 and -3 are combined in the -3 estimate and relative years +3, +4, and +5 are combined in the +3 term.

4.2.1 Initial consumption and savings response

Table 2 displays the treatment effects (δ in equation 12) on consumption (column 1), its four components (columns 2-5) from the budget constraint (11), and on savings (column 6). It is for the parsimonious specification; Appendix Table A7 presents the estimation results for the dynamic specification. The first row indicates that consumption, savings, and their components are not statistically different for treatment and control in the pre-period. Table A7 confirms the parallel pre-trends.

Second, we see a massive increase in housing wealth and debt in the year of privatization. The average treated household pays a conversion fee of SEK 376,500 (*dHousing*) and takes on SEK 342,800 in additional (mortgage) debt (*dDebt*). The SEK 33,700 difference between the two reflects home equity, i.e., the downpayment. This downpayment is partly financed by reducing financial wealth to the tune of SEK 12,000. The SEK 21,600 *Savings* effect is the sum of the treatment effects of home equity and the change in financial wealth. While we certainly expect a large portfolio reallocation towards housing wealth and away from financial wealth upon home ownership, the net increase in total wealth (positive savings) is surprising. It is also large, about three times pre-treatment annual savings.

The savings effect in RY0 is generated in equal measure from an increase in disposable income and from a reduction in spending. The drop in consumption is SEK 11,700 or 7.3% of pre-treatment consumption. While economically meaningful, the initial consumption effect is too imprecisely measured to be statistically different from zero. We turn to the income effect below.

All treated households have initial total debt representing less than 70% of the market value of the home they bought ($CLTV < .7$) due to the large "naive windfall." Given prevailing CLTV standards at that time in Stockholm, all treated households could have borrowed the entire conversion fee. Debt-to-income ratio constraints were also unlikely to be binding. Total debt service is below 30% of disposable income for 95% of the treated households. This suggests that households were making the downpayment and the associated savings and consumption decision voluntarily. They could have borrowed more to avoid the initial consumption drop. This choice could be rationalized by beliefs of superior expected returns on home equity relative to financial assets. Case, Shiller and Thompson (2012), Foote, Gerardi and Willen (2012), and Kaplan, Mitman and Violante (2016) have argued for high expected housing returns, based on U.S. evidence from around the same time as our privatization experiment. Alternatively, it could be consistent with debt aversion, as in Caetano, Palacios and Patrinos (2011). Whether motivated by rational or irrational beliefs, the initial downpayment and savings effects we find are consistent with the notion that home ownership induces households to save, at least initially.

4.2.2 Initial labor income response

Column 2 shows that treated households earn SEK 10,000 higher after-tax income than the control group in RY0, relative to RY(-1). It represents a 5.9% increase over average pre-treatment income and is measured precisely. Appendix Table A8 investigates the income increase further by studying pre-tax labor income. The latter increases even more, by SEK 15,400 or 8.2% of the pre-treatment average. Both the number of adults working (+0.03 on a baseline of 1.34) and the income per working adult contribute to the increase and are significantly different from zero.

There are several potential explanations for the increase in labor income: increased hours worked, a return from part-time to full-time work, a return from parental leave to full-time employment, or an increase in income reported to tax authorities possibly connected to having to obtain a mortgage from a bank. Our result is consistent with a debt-service induced increase in labor supply (Fortin, 1995; Del Boca and Lusardi, 2003). The treatment effect on labor income is stronger among treated households who take on more debt upon privatization. The

Table 2: Consumption and Savings Effects

LHS var:	(1) Consumption	(2) Income	(3) dHousing	(4) dDebt	(5) dFin	(6) Savings
RY(pre)	-4585.9	108.0	822.4	-3845.5	26.11	4694.0
	(-0.53)	(0.04)	(0.16)	(-0.63)	(0.00)	(0.65)
RY(0)	-11736.2	9874.5**	376494.3***	342772.5***	-12111.1**	21610.7**
	(-1.36)	(3.10)	(5.20)	(4.84)	(-2.55)	(2.62)
RY(post)	16456.3**	2126.1	-14091.1	719.3	480.2	-14330.2**
	(2.41)	(0.57)	(-1.63)	(0.12)	(0.10)	(-2.87)
PT-Mean	159,689	165,960	1865	4,868	9,273	6,270
PT-SD	117,169	84,593	49,845	70,092	77,076	92,537
N	13,372	13,372	13,372	13,372	13,372	13,372
R^2	0.0673	0.14	0.208	0.199	0.0125	0.0156

Notes: t statistics in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$. Standard errors are clustered at the building level. The table reports the coefficients δ_k on the interaction between the treatment dummy and the relative year (RY) vis-a-vis treatment. The coefficients on the relative year dummies are not reported. Building fixed effects and calendar year fixed effects are included but not reported. Age, Education, and Partnership are included as control variables in all columns. The last four rows report the mean and standard deviation of the dependent variable of all treatment and control group household-year observations in the years before RY0, the number of household-year observations, and the R^2 of the regression. All variables are expressed in SEK, per adult equivalent, and in real terms. Relative years -4 through -2 are collapsed into the RY(pre) term and relative years +1, ..., +5 are collapsed in the RY(post) term. We loose one year of data in the construction of $dDebt$, $dHousing$, $dFin$, and therefore in savings and consumption; all regressions use the same sample.

debt-service effect offsets a presumably negative labor supply effect from higher wealth. In related work, Bernstein (2017) finds strong labor supply responses to mortgage modification programs.

4.2.3 Subsequent effect on consumption and saving

Arguably more consequential than the initial response is the consumption effect in the years after home ownership. Column (1) of Table 2 shows a large and precisely estimated consumption effect of SEK 16,500. This is the average annual consumption differential between the treated and the control group one to four years after treatment. It represents a 10% increase over the average pre-treatment consumption level. The cumulative effect is SEK 65,800 or about USD 8,800.

The SEK 16,500 consumption increase is paid for by a SEK 2,000 increase in *Income* and a SEK 14,500 reduction in *Savings*. The income increase is not different from zero and economically small. Appendix Table A8 confirms that the labor income effect is confined to the treatment year.[22] The reduction in savings results from a reduction in home equity. The

[22] Cesarini et al. (2017) find a small reduction in labor income in response to lottery winnings; labor income is SEK 89 lower per SEK 10,000 won in lotteries each year for the next five years. One difference with our setting is that lottery winnings are in the form of liquid wealth whereas our windfall is in the form of less liquid housing wealth. This distinction matters, as explained by Kaplan, Violante and Weidner (2014). Section 4.4 explores the empirical implications of turning illiquid housing into liquid financial wealth.

home equity reduction is an average effect reflecting some households who take on additional debt against the same house, and some households who reduce their housing wealth by more than their debt. For example, we show below that low-windfall households increase debt and leave real estate wealth constant. The reduction in housing wealth is a relative effect of treatment versus control. Some households in the control group become homeowners while some households in the treatment group sell their apartments.

In sum, we find a substantial increase in spending in the four years following home ownership.[23] This evidence is inconsistent with the view that home ownership promotes savings. We now investigate how this spending response varies with the windfall.

4.2.4 Heterogeneity in consumption response by windfall

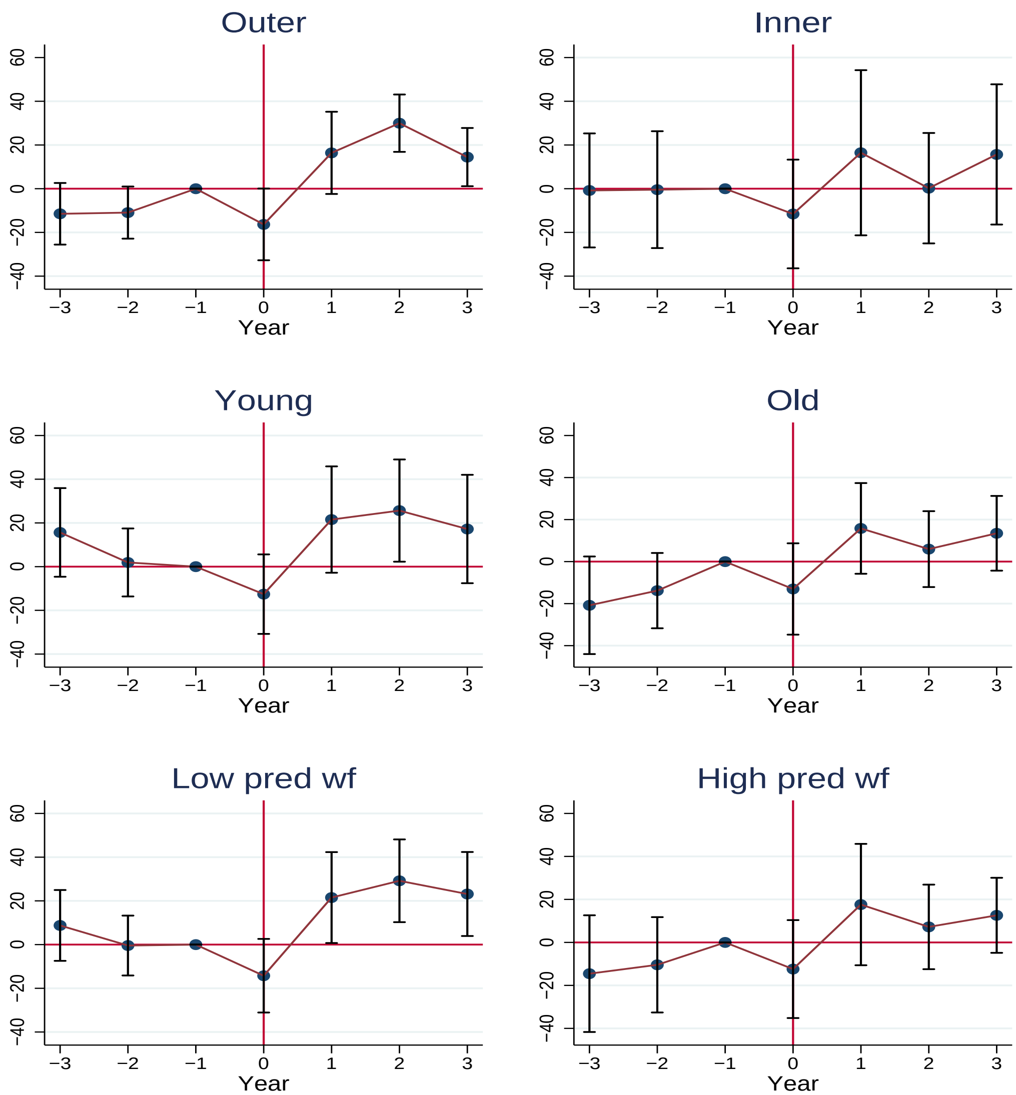
A first source of variation in windfall is the location of the apartment. Privatized apartments in the inner city receive a larger naive windfall, the term τP_0 in equation (10), than treated households in the outer city by virtue of the much higher per square foot house prices in the inner city. Appendix Table A6 and Figure A3 provide the windfall distribution for treated households, broken down by location, and confirm that the windfall distribution in the inner city is shifted to the right of that in the outer city for both naive and total windfall measures.

The top row of Figure 3 shows the dynamic treatment effect of consumption around the privatization. The left panel is for the outer city, the right panel is for the inner city. By comparing treated households in the inner (outer) city to control households in the inner (outer) city, we take selection into a location into account. Appendix Table A10 shows the regression estimates in parsimonious format for consumption and its components. As before, there are no differences between treatment and control prior to treatment. The main finding is that both the initial consumption decline and –more importantly– the subsequent consumption increase are stronger for the outer city group. The post-treatment consumption response for treated households in the inner city is not statistically different from zero. The households with the smallest windfall show the largest consumption response.

We find similar results when we split the sample by age; younger and older than 45. Age

[23] About 7% of tenants in buildings approved for privatization chose not to exercise their home ownership option. We have also studied treatment effects on the treated (ToT), looking only at those who actually become home owner. Appendix Table A9 shows the ToT estimation results for consumption and its components. The ToT estimates are about 7% larger in magnitude than the ITT estimates, but qualitatively the same.

Figure 3: Consumption Around Privatization: Heterogeneous Effects



Each panel shows the dynamic treatment effect of consumption around the privatization. The top row splits the sample into outer city (left panel) and inner city (right panel) households. The middle row shows the consumption response by age, with the young on the left and the old on the right. The bottom row shows the consumption response by predicted windfall, with low predicted windfall on the left and high predicted windfall on the right. Relative years -4 and -3 are combined in the -3 estimate and relative years +3, +4, and +5 are combined in the +3 term.

affects the windfall in two ways. First, since the naive windfall scales by the size of the apartment, the naive windfall is smaller for younger households who are more likely to live in a smaller apartment. Second, the horizon effect $(R_h/R)^T$ in equation (10) implies that younger households have a lower horizon effect since they are likely to live in the area for more years (recall $R_H/R < 1$). Younger households have a smaller windfall for both reasons. The middle row of Figure 3 shows the consumption response by age, with the young on the left and the old on the right. Appendix Table A11 presents the regression coefficient estimates. We find the largest post-treatment consumption responses for the young. They increase consumption by SEK 20,900 per year relative to the under-45 households in the control group, an economically and statistically large response. In contrast, the over-45 treatment effect is only half as large at SEK 11,500 and not statistically different from zero. Yet, it is the over-45 treatment group that has the largest windfall.

The findings are the same when we group households into a low and high predicted windfall group. The predicted windfall is formed from a regression of windfall on distance to center, age, and age squared.[24] The bottom row of Figure 3 shows the consumption response by predicted windfall, with low predicted windfall on the left and high predicted windfall on the right. Appendix Table A12 shows the regression coefficients. Households with the low predicted windfall have the biggest post-treatment consumption response (SEK 22,800); the effect is precisely estimated. The consumption response for the high-predicted windfall group is half as large (SEK 12,300) and not statistically different from zero.

Whether we instrument the windfall by location, age, or predicted windfall, we find larger consumption responses for households who receive a smaller windfall. One interpretation is that a pure home ownership effect accounts for the consumption response. Another, complementary, interpretation is that low-windfall households have a higher marginal propensity to consume (MPC) out of housing wealth than high-windfall households. When utility is concave in wealth, the lower pre-treatment wealth of the young, outer-city, and low predicted windfall households predicts the observed pattern.

4.2.5 MPC out of housing wealth

Our quasi-experimental variation in housing wealth is helpful for identifying the MPC. We define the MPC as the estimated treatment effect on annual consumption in the post period for those who privatize divided by either the naive or total windfall. We also divide by

[24] The windfall on the left-hand side of that predictive regression is formed as the product of the naive windfall and the horizon effect $(R_h/R)^T$, where we set $R_h = 1.02$, $R = 1.07$, and T is 85 minus age in the household formation year. The regression coefficients are estimated on the sample of treated households; the R^2 is 73%. These coefficients are then used to form a predicted windfall for both treated and control households. Households are split into low and high predicted windfall groups at the median.

Table 3: Marginal Propensity to Consume Out of Housing Wealth

	(1) **All**	(2) **Outer**	(3) **Inner**	(4) **Young**	(5) **Old**	(6) **Low pred wf**	(7) **High pred wf**
Consumption response	16,456	17,535	11,149	20,933	11,457	22,848	12,324
Naive windfall	493,353	404,995	602,955	424,833	545,924	366,928	614,938
Total windfall	88,202	67,975	113,292	41,509	124,027	38,376	136,121
Home ownership rate	0.88	0.82	0.95	0.83	0.91	0.83	0.92
MPC (naive windfall)	**3.8%**	**5.3%**	**2.0%**	**5.9%**	**2.3%**	**7.5%**	**2.2%**
MPC (total windfall)	**21.3%**	**31.3%**	**10.4%**	**60.6%**	**10.2%**	**71.7%**	**9.8%**

Notes: The Consumption Response is the estimated annual treatment effect $\beta(post)$ in the regression (12) with consumption as the outcome variable. The naive windfall is the term τP_0 in equation (10) and the total windfall is the entire expression in equation (10). Both are measured as of the treatment year RY0. Consumption and the windfall measures are expressed in real SEK and per adult equivalent. The home ownership rate is measured in RY0. The MPC is measured as the Consumption response divided by the windfall and divided by the home ownership rate. Column 1 is for the full sample. Columns 2 and 3 split the sample by Outer versus Inner city (distance from center more or less than 5km driving, respectively). Columns 4 and 5 into Young (under 45) and Old (over 45). Columns 6 and 7 split the sample into Low predicted windfall and High predicted windfall, based on a specification with distance to the center, age, and age squared.

the home ownership rate in RY0 to only consider those who actually became home owner.[25] Column (1) of Table 3 finds a MPC of 3.8% out of the naive windfall and a MPC of 21.3% out of the total windfall. The 3.8% number is similar to what has been found in literature using aggregate data (Case, Quigley and Shiller, 2005) and more recently in Italian micro-data by Paiella and Pistaferri (2017). The 21.3% MPC is in line with the roughly 20% estimates obtained from the literature that aims to explain U.S. households' consumption response in the Great Recession (e.g., Mian, Rao and Sufi, 2013; Berger et al., 2017; Kaplan, Mitman and Violante, 2016). The second number is arguably the more natural one since the naive windfall fails to take into account that treated households who sell right away will need to buy more expensive housing for the future than those who stay.

Since privatization was anticipated by both treated and control groups, but the control group was unexpectedly denied, our findings document a strong response of consumption to unexpected changes in (housing) wealth. Campbell and Cocco (2007) and Paiella and Pistaferri (2017) find that consumption responds to both unexpected and expected wealth changes.

The other columns of Table 3 provide three ways of splitting the sample by our various windfall instruments. They confirm the much larger MPC out of housing wealth for households with smaller windfall gains: the young, those living farther from the city center, and those with low predicted windfalls. This is not only because the denominator in the MPC

[25] Alternatively, treatment effects on the treated can be estimated by instrumenting for take-up of treatment, with similar MPC results.

is lower for those households but also because the consumption response is stronger. This heterogeneity suggests that policies aimed at boosting home ownership may prompt very different consumption responses depending on the part of the wealth distribution they operate on.

The large consumption response and the decline in savings in the four years post home ownership suggest that home ownership does not bring the alleged savings benefits.

4.3 Housing collateral effect

The second major alleged benefits of home ownership is that housing is a collateral asset that households can draw upon in times of need. To study the use of the house as a collateral asset, we analyze how households respond to a large labor income shock. We focus on a decline in household labor income of at least 25% to eliminate concerns about the possible endogeneity of the fall in income. The average shock is close to -40%.[26] We ask whether the consumption response differs between home owners and renters. What makes our setting an attractive laboratory for testing the housing collateral effect is that we have exogenous variation in home ownership.

Let Z_{it} be an indicator variable that takes on the value of 1 if the ratio of household labor income in period t to labor income in period $t-1$ for household i is below 0.75, and 0 otherwise. The average pre-tax labor income decline is SEK 94,800. Panel A of Table 4 shows the effect of a labor income shock on consumption and savings. The shock results in a post-tax disposable income decline of SEK 51,400. The decline is smaller than that in labor income due to progressive taxation and automatic stabilizers such as unemployment benefits. The labor income decline is associated with an average fall in consumption of SEK 35,000. The average consumption decline represents 22% of pre-treatment average annual consumption. Households smooth consumption by reducing savings, financial wealth, and net housing wealth.

Do home owners respond differentially to such a large income shock? To investigate this,

[26]We also make sure the drop is not due to retirement by excluding retirees. It is difficult to find well-identified income shocks or measures of income risk. See Fagereng, Guiso and Pistaferri (2017*a*,*b*) for a discussion.

Table 4: Housing Collateral Effect

LHS var:	(1) Consumption	(2) Income	(3) dHousing	(4) dDebt	(5) dFin	(6) Savings
			Panel A			
Z	-35216.7***	-51349.2***	-8331.9*	-2872.4	-10673.0***	-16132.5***
	(-10.28)	(-20.92)	(-1.71)	(-0.67)	(-3.74)	(-4.26)
R^2	0.0722	0.170	0.0360	0.0422	0.0125	0.0148
			Panel B			
$\delta(pre)$	-3026.3	1197.2	347.9	-4226.9	-351.3	4223.5
	(-0.35)	(0.48)	(0.07)	(-0.69)	(-0.05)	(0.56)
$\delta(0)$	-7997.4	10011.9**	378721.3***	345985.8***	-14726.2**	18009.4**
	(-0.90)	(2.96)	(5.14)	(4.82)	(-2.96)	(2.13)
$\delta(post)$	12839.0*	1752.7	-14187.9	-2878.8	222.8	-11086.3*
	(1.74)	(0.47)	(-1.66)	(-0.45)	(0.04)	(-1.89)
$\beta(pre)$	-9840.4	-1390.9	5191.9	3720.7	6978.3	8449.6
	(-0.79)	(-0.21)	(1.60)	(0.61)	(0.63)	(0.65)
$\beta(0)$	-23632.3	6575.0	-15573.2	-23427.3	22353.1**	30207.3
	(-0.99)	(0.78)	(-0.43)	(-0.51)	(2.45)	(1.30)
$\beta(post)$	38175.0**	4166.5	940.5	37328.9**	2379.9	-34008.5*
	(2.48)	(0.39)	(0.03)	(2.43)	(0.16)	(-1.78)
Z	-34207.0***	-44266.2***	-7477.8**	-4592.2	-7173.6*	-10059.1**
	(-5.37)	(-8.57)	(-2.53)	(-0.87)	(-1.69)	(-2.49)
R^2	0.0744	0.171	0.209	0.200	0.0143	0.0188
N	13372	13372	13372	13372	13372	13372

Notes: Panel A reports estimates and R^2 statistics of a simple regression of the dependent variable in the top row on an indicator variable Z which is one in a period in which the household experiences at least a 25% decline in labor income relative to the prior year. The specification includes building and year fixed effects. Panel B reports estimates of equation A3. The δ terms are the coefficients on the $Private \times RY$ indicator. The β terms are the coefficients on the $private \times RY \times Z$ indicator. The γ coefficients on the RY indicator and the λ coefficients on the $RY \times Z$ indicator are not reported in the table for brevity. All outcome variables are expressed in real SEK and per adult equivalent.

we estimate:

$$\begin{aligned} y_{it} &= \alpha + Private_i \sum_k \delta_k RY_i(t=k) + Private_i \sum_k \beta_k RY_i(t=k) Z_{it} \\ &+ \sum_k \gamma_k RY_i(t=k) + \sum_k \lambda_k RY_i(t=k) Z_{it} + Z_{it} + X_{it} + \psi_t + \omega_b + \varepsilon_{it}. \end{aligned} \tag{13}$$

The main coefficients of interest are the triple-difference coefficients β, which measure the consumption response of the treated households experiencing a negative income shock, relative to the control households and relative to those who do not get a negative income shock. Panel B of Table 4 shows the main collateral effect estimation for consumption in column 1. As we saw in panel A, the large decline in labor income coincides with a large average consumption decline. This is captured by the -34,200 estimate on the Z_{it} term. If this income shock hits a home owner after privatization, she fully offsets that baseline consumption decline. The estimate $\beta(post)$ is SEK 38,200 and estimated precisely. As the column for *dDebt* shows, the consumption smoothing is accomplished by increasing debt by nearly the same amount (SEK 37,300). If that same household had received that same income shock prior to treatment, without a house to borrow against, her consumption response would have been no different than that of a household in the control group; $\beta(pre)$ is not different from zero and, if anything, negative. This is strong evidence that owners respond very differently to an income shock than renters, and that we can interpret that differential response causally to home ownership since ownership was randomly assigned.

The table also reports the treatment effect for those who do not get a negative income shock, measured by the δ coefficients. Post-privatization, these treated households consume SEK 12,800 more per year than the households in the control group without an income shock. This consumption response is smaller than the overall SEK 16,500 consumption effect discussed in Table 2. Put differently, the strong consumption response of the treated households with a negative income shock increases the average response of all treated households. The average consumption response and the collateral effect are intertwined. Treated households who do not receive an income shock gradually pay back their debt, while those with the income shock strongly increase it.

If households that are hit by an adverse income shock use the house as a collateral asset to borrow against in order to smooth consumption, then we expect them to pay back that debt and reduce consumption in the period that follows the shock. Appendix Table A13 investigates this implication of the housing collateral effect, and indeed finds the predicted reversal in borrowing and consumption, for home owners, in the period after the income shock.

The housing collateral results are not sensitive to the definition of the income shock. Appendix Table A14 explores income shocks of at least 15%, 20%, and 30% rather than at least 25%, with similar results.

We also explore heterogeneity in the collateral effect by size of the windfall, using sample splits by distance to center, age, and predicted windfall. Appendix Table A15 shows that the groups with the largest windfall and therefore the largest amount of housing collateral wealth (Inner city, Old households, High predicted windfall) display the strongest consumption offset to a labor income decline. The relatively weaker housing collateral effect for the young (and more generally low-windfall) households, combined with their strong average post-treatment consumption response, suggests a strong consumption wealth effect for this group.

4.4 Realized windfall

Having established that the treatment effect on consumption works (in large part) through the housing collateral channel, we ask whether there are any other circumstances besides adverse labor income shocks that are associated with higher consumption. Specifically, we distinguish between households who move and those who stay in their same apartment. A mover is someone who changes their official address at any point between the treatment year and the last year they are observed. When estimating the treatment effects, we compare movers in the treatment group to movers in the control group and treated stayers to stayers in the control group.[27] Two-thirds of household-year observations are stayer observations, while the remaining third are mover observations. Of the one-third of movers, about one third reverts to renting, while the rest purchases a new primary residence elsewhere. The moving decision may very well be an endogenous outcome. However, for this exercise, we are only interested in understanding the heterogeneity in the consumption responses between those who decided to move for whatever reason and those who chose to stay.

Table 5 shows that the consumption effect is more than twice as large for movers than for stayers: SEK 28,700 versus SEK 12,800 or 18% versus 8% of annual average pre-treatment spending. The cumulative consumption response of SEK 114,800 SEK for movers vastly exceeds the SEK 51,000 increase for stayers. For savings, the opposite is true. Movers significantly reduce savings in the post-privatization period, compared to movers in the control group, while treated stayers do not save differently from stayers in the control group. Since the income effect is close to zero in the post-period, movers finance the consumption increase by reducing home equity. This takes the form of reduced housing wealth and increased debt. The reduced housing wealth arises because some movers in the treatment sample revert to renting and others buy a smaller house, and also because some movers in the control group become home owner for the first time. Treated movers have higher financial wealth than

[27] We estimate a fully saturated regression, equivalent to a sample split. Results are similar when we compare movers and stayers in the treatment group to the full control sample. That specification would be a simple split of the treatment group. Analyzing average household characteristics indicates that movers (stayers) in the control group are similar to movers (stayers) in the treatment group, and hence a better point of comparison.

Table 5: Consumption and Savings Effects: Movers versus Stayers

LHS var:	(1) Consumption	(2) Income	(3) dHousing	(4) dDebt	(5) dFin	(6) Savings
RY(pre) Stayer	-3333.8	-4125.2	-4331.7	-7728.0	-4187.7	-791.4
	(-0.40)	(-1.02)	(-0.82)	(-1.11)	(-0.74)	(-0.12)
RY(0) Stayer	-9745.8	6786.8**	373108.9***	335261.6***	-21314.7***	16532.6
	(-0.78)	(2.23)	(4.91)	(4.47)	(-3.84)	(1.47)
RY(post) Stayer	12763.2**	3753.4	-3223.9	-4711.8	-10497.6**	-9009.7**
	(2.12)	(0.81)	(-0.55)	(-0.86)	(-2.64)	(-2.19)
RY(pre) Mover	-5596.4	6689.7	8905.2	2067.1	5448.1	12286.1
	(-0.43)	(1.35)	(1.04)	(0.22)	(0.40)	(0.87)
RY(0) Mover	-11351.2	15482.5**	379253.4***	353501.8***	1082.1	26833.7**
	(-0.92)	(2.94)	(5.39)	(5.31)	(0.09)	(2.03)
RY(post) Mover	28707.6**	-2729.5	-45124.2*	8163.2	21850.4**	-31437.1**
	(2.09)	(-0.48)	(-1.79)	(0.46)	(2.32)	(-2.77)
PT-Mean	159,689	165,960	1,865	4,868	9,273	6,270
PT-SD	117,169	84,593	49,845	70,092	77,076	92,537
N	13,372	13,372	13,372	13,372	13,372	13,372
R^2	0.0784	0.16	0.221	0.209	0.0227	0.0285

Notes: t statistics in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$. Each household-year observation is either coded as Mover or Stayer. The Mover indicator variable is 1 if the household changes official address in or after RY0, and 0 otherwise. The Stayer indicator variable is the opposite. Relative year variables, relative year times Private variables, controls, and fixed effects are all interacted with both Mover and Stayer indicators. this is a fully saturated regression, equivalent to a sample split.

treated stayers in the post period, indicating a large portfolio shift from housing to financial wealth. In contrast, stayers do not reduce housing wealth, pay down some debt, and reduce financial wealth to fund the higher consumption.

In sum, home equity extraction is much stronger for movers than for stayers, even though home equity lines of credit were available for all treated households, and all treated households had plenty of home equity to tap into. Second, the different consumption responses of movers and stayers suggest that realized windfall gains, in liquid form, trigger larger spending responses than when they are in the form of illiquid housing wealth. Households spend more not only when they face adverse income shocks, but also when they realize capital gains.

5 Conclusion

Unearthing the effects of home ownership is difficult since ownership status is correlated with many other household and housing characteristics. Our quasi-experimental setting overcomes this endogeneity problem and is the first to study the causal effects of home ownership on consumption and savings. Home owners not only change the composition of their savings, they also increase total savings, increase labor supply, and cut consumption in the year of the home purchase.

In the four years following home ownership, home owners consume significantly more than renters. There are no additional labor supply effects. The consumption response is stronger for households for whom the housing wealth gain is the smallest. We uncover new causal estimates of the marginal propensity to consume out of housing wealth. The average MPC is 21.3%, but hides substantial cross-sectional variation by age and location. Our results show that home ownership does not promote savings in the medium run. But, it provides strong collateral benefits allowing households to smooth consumption in the wake of an adverse shock. We also document higher propensity to consume out of wealth after housing wealth is transformed into liquid financial wealth.

In follow-up work we plan to study how home ownership affects stock market participation and the portfolio composition among financial assets, conditional on participation. We also plan to explore how home ownership affects mobility, contrasting the housing ladder and housing lock views. Chetty, Hendren and Katz (2016) find positive effects on the educational and labor market outcomes for the children of poor households who were moved to better neighborhoods. An interesting question is whether exposure to home ownership during childhood affects educational and labor market outcomes, mobility, home ownership, and wealth accumulation in adulthood. Finally, we plan to study social outcome variables such community and political engagement, school quality, and crime, using our unique data and identification procedure.

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Online Appendix "Identifying the Benefits from Home Ownership: A Swedish Experiment"

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A Privatization Process

A.1 Market-wide Conversion Statistics

To illustrate the size of the coop conversion movement, Table A1 reports on the composition of the stock of apartments in the municipality of Stockholm in 1990, 2000 and 2004. Between 1990 and 2000, the stock of municipally-owned apartments declined by 8,000 units. Privatizations accelerated between the years 2000 and 2004 with another 8,000 units converted into co-ops. In addition to the three large municipal landlords, private landlords also massively converted apartment, accounting for three-quarters of the co-op conversions (31,000 out of 47,000). Between 2000 and 2004, co-op-owned apartments increased by 34,400 units. Over the longer 1990 to 2004 period, the ownership share of co-ops increased from 25% to 43%. Table A2 zooms in on co-op conversions in the period 1999-2004. Municipal landlords privatized 12,200 apartments in Stockholm. Municipal landlord conversions ramped up dramatically in the year 2000 and peaked in 2001 at 5,500 units.

Table A1: Apartments by ownership, 1990-2004, Municipality of Stockholm

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

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

Table A2: Transactions of apartments by ownership, 1999-2004, Municipality of Stockholm

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

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

A.2 The Steps of the Privatization Process

The process of co-op conversion requires a series of formal steps. The first step is for the tenant association to register a home owner co-operative with Bolagsverket, the agency responsible for registering all limited liability companies in Sweden. A co-op needs at least three members. The co-op board consists of at least three and at most seven board members.

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

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

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

Once the mortgage loan and the economic plan are in place, the tenants meet and vote on the proposed conversion. At least 2/3 of all eligible votes must be in favor for the conversion to go ahead. It is possible to submit a written vote. Only primary renters are allowed to vote, subtenants are not. The municipal landlord verifies that only eligible votes are taken into account. In a few instances, the landlord stopped the process and asked for a re-vote because some votes were deemed eligible by the tenant association but not by the landlord. The 2/3 majority is a minimum requirement. We have some observations where the vote exceeded 2/3, yet the purchase did not go through. Presumably, some co-op board decided it wanted or needed an even larger majority to go ahead. Upon a favorable vote, the co-op board communicates the vote tally and the minutes of the meeting to the landlord. Unfortunately, we cannot use this 2/3 threshold as an alternative RDD-based identification strategy, as we observe bunching on the right hand side of the threshold. This bunching might reflect unobserved heterogeneity across co-ops and their tenants that is possibly correlated with our outcome variables of interest.

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

A.3 Denials by the County Board

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

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

- The 4 bed room apartments in the building are unique to the neighborhood.
- The studios of size 17 to 25 square meters in the building are unique.
- The only remaining muncipal building in the neighborhood has no elevator and has 2 floors less.
- The 2 bed room apartments in the building are unique to the neighborhood.
- The studios in the building are unique to the neighborhood.
- Two 5 bed room apartments in the building are unique to the neighborhood.
- There is one very large one-bedroom apartment in the building (54 square meters) which is unique to the neighborhood.

A.4 Example: Akalla Conversion

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

Each of the four co-ops consists of several low- and mid-rise buildings adjacent to each other. Figure A1 shows aerial and street views of the four properties, showing their geographic proximity. The entire Akalla complex was constructed in 1976, one year after the subway line to Akalla opened. All properties are owned by Svenska Bostäder, one of the large municipal landlords in Stockholm. Table A3 provides details on the four properties. In addition to their extreme geographic proximity, identical year of construction, and identical ownership, the four co-ops' properties share several more characteristics. All co-ops have about the same floor area, with the vast majority of square meterage going to apartments and only a small fraction devoted to commercial use. They also have about the same distribution of apartments in terms of number of rooms, with the vast majority 3- and 4-room apartments (i.e., one- and two-bedroom apartments).

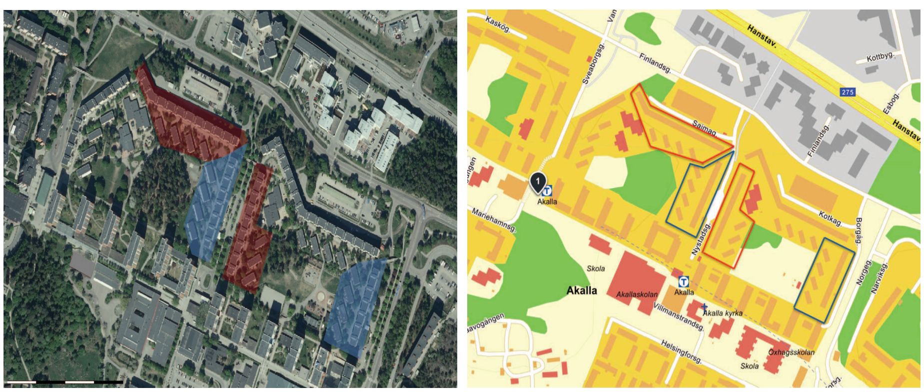


Figure A1: Akalla Complex

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

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

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

Table A3: Akalla Coop Conversions

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

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

B Data Appendix

B.1 Variable definitions

The following main variables of interest are available to us from Statistics Sweden.

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

Income – We consider two different income concepts. *Labincind* measures a household's labor income per adult. It is a comprehensive measure of all income derived from work: wages, salaries, income from sole proprietorships and active business activity, unemployment benefits, and employer-provided benefits such as a company car, sick leave, and continued education. *Numwork* is the number of adults in the workforce. *Labinchh* is total household income, the product of the labor income per adult (intensive margin) and the number of working adults (extensive margin). Our second income variable *Income* is a broader measure of income that enters the household budget constraint; it is after-tax. The construction procedure for *Income* is below in appendix B.1.1.

Debt – We observe total household-level debt. We only have data for total debt, *Debt*, but no separate information on mortgage debt. Mortgage debt accounts for 2/3 of total household debt in Sweden in the 2002-04 period according to the Riksbank's 2004 Financial Stability Report. *Interest* is the interest paid on *Debt*. When a household converts, buys her apartment and increases debt to do so, the increase in housing wealth and in debt does not always occur in the same year. This timing issue occurs when the real estate transaction occurs around year-end. Appendix B.1.3 describes our algorithm for adjusting the timing of debt, and it describes how exactly we construct the variable *dDebt*.

Housing wealth – From the wealth registry data, we observe the value of single-family houses owned, second homes, investment properties, and commercial real estate. The value of owned apartments is imputed by the SCB, with substantial measurement error. Whenever available we rely on another database for the value of apartments, the Transfer of Condominium Registry (KU55). KU55 contains all sales of apartments. Conditional upon a sale, it records not only the current sale date and price but also the date and price of the *preceding* purchase. We obtain KU55 data for the years 1999-2000 and 2003-2014. Thus, for any household in our treated co-ops that sold their apartment after conversion and before the end of 2014, we know the price for which they obtained the apartment, i.e. the conversion fee. The inference problem is for households that lived in the converted co-ops but for which we do not observe a sale by the end of 2014. They are either owners who have not sold or residual renters. Statistics Sweden imputes housing wealth for all of them, as if they are all owners. We improve the precision of Statistics Sweden's imputation as follows. We calculate a precise estimate of what the conversion fee would have been for each tenant had they bought. We multiply the size of each tenant's apartment in square meters with the median price per square meter, calculated from the conversion fees per square meter paid by households in the same building who sold their apartment prior to the end of 2014. From KU55, we know what they bought the apartment for upon conversion. We assume that if the household's total debt increase in the conversion year is less than 20% of the estimated transfer fee, then the household is a residual tenant. Otherwise, we assume they are owner and impute the transfer fee for them. We test this procedure on the four Akalla co-ops for which we have high quality tenant lists that identify the residual tenants. Reassuringly, the LTV procedure correctly identifies all residual tenants, including the residual tenants we are missing based on the KU55 data alone. We end up with 40 residual tenants out of 1,864 households (2%) or out of 533 treated households (7.5%).

We define a variable *Housing* as the sum of apartment and single-family housing wealth. It only contains the primary residential property. All additional residential or commercial real estate is called *Nonhouse* and part of financial wealth. The change in housing wealth (other real estate wealth), *dHousing* (*dNonhouse*), is zero unless *Housing* (*Nonhouse*) switches from a positive number to zero or vice versa or unless the household moves. Appendix B.1.2 provides the details of how we construct the change in primary residential wealth. We do not consider unrealized gains or losses in property value as part of the change in real estate wealth. We measure home ownership, *HomeOwn*, as having positive *Housing* wealth.

Financial wealth – A unique feature of the Swedish data is the granular financial asset information. We have information for every stock, mutual fund, and money market fund for every individual in our sample. We also have information on the total value invested in bonds for each individual. End-of-year values of each asset are administratively reported (not self-reported) for the computation of the wealth tax. Because the wealth tax was abolished starting in 2008, we end our sample in 2007. We label the sum of these risky financial assets *Risky*. Financial wealth *Financial* contains four more components: *Nonhouse*, *Bank*, *CapIns*, and *Pension*. *Bank* is the balance of all bank accounts. Reporting requirements on bank accounts vary across time, depending on interest earned between 1999 and 2005 and on bank balance in 2006-07. Appendix B.1.4 provides more detail on our bank account imputation procedure, which further improves on Calvet, Campbell and Sodini (2007). For the capital insurance accounts, we observe the year-end balance but not the asset mix. We assume a 50-50 mix of equity and bonds. Regarding pension accounts, we observe contributions made in the year. Withdrawals are included in $Income$.

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

Consumption – As explained below, the wealth and income data are so comprehensive and detailed that they allow us to compute high-quality measures of household-level consumption spending, a rarity in this literature that usually relies on proxies for consumption (car or credit card purchases) or -in the best case scenario- on noisy survey-based measures of consumption. Because of a change in the wealth tax, detailed holdings of financial instruments were no longer collected after 2007. Therefore, we follow households from 1999 until 2007. Consumption is measured as the right-hand side of the budget constraint:

$$Cons = dDebt - dHousing - dFin + Income \tag{A1}$$

Consumption is high when households increase borrowing, sell housing or financial assets, or earn high income, all else equal. A purchase of an apartment which is fully funded with a mortgage has no implications for consumption. Our consumption measure is registry-based, and therefore precisely measured and comprehensive. The four (minor) sources of measurement error are: imputation of apartment real estate wealth for stayers, measurement issues with bank accounts, coarse imputation of returns on bonds based on a bond index, and lack of knowledge of the exact asset mix of the capital insurance accounts.

Our consumption measure is a measure of total annual spending. As such, it includes outlays on durables rather than the service component from durable spending. The method does not allow us to break down consumption any further into its subcategories. Koijen, Van Nieuwerburgh and Vestman (2014) discuss the benefits and drawbacks of our consumption data in detail and compare them to the standard survey measures of consumption typically used in micro-level analysis for the same set of households. One possibility we cannot exclude is that home ownership prompts inter-vivos transfers from family members or friends. By linking generations to each other in Swedish data, Englund, Jansson and Sinai (2014) provide some evidence for intergenerational giving at the time of home purchase. We define *Savings* as *Income* minus *Cons*.

The rest of this appendix describes in detail how each of the four consumption components is constructed.

Naive Windfall We calculate the naive windfall for each treated household as the product of the market price per square meter times the number of square meters of their apartment. The market price per square meter is calculated at the building level from house price transactions that took place in the year of privatization. If there are no housing transactions in the year of privatization in a given building, we impute the square meter market value by using transactions in that same building in future years, discounting them back to the year of privatization using a parish-level square meter house price index. The latter is obtained as the yearly average sale prices for condominiums from SCB.

B.1.1 Construction of Income

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

B.1.2 Construction of dHousing

Because of the detailed nature of the Swedish data, we are able to observe the real estate wealth of individuals in great detail. In order to construct an accurate measure of change in real estate, we include information on several types of properties taken from the Wealth Registry (Förmögenhetsregistret). These properties are grouped into residential and non-residential real estate and are treated separately. Consumption decreases with positive changes in real estate (acquisitions) and increases with negative changes in real estate (sales).

dHousing, our measure of primary real estate investment, only includes residential real estate. Changes in non-residential real estate are treated separately and are included in dFin. Residential real estate consists of houses and apartments. We observe the imputed market value for these two types of properties at the end of any given year in our sample.

In order to calculate the change in wealth invested in houses, we turn to the wealth registry. We consider that a house is acquired if the house real estate wealth changes from zero in the past year to a positive value at the end of the current year, and the opposite in the case of a sale. In addition, we consider another special case for transactions with houses if the house real estate was positive at the end of both the past year and the current year and if the individual moved during the current year. In this scenario we assume that the individual sold a house at last years market value and bought a new house, spending an amount equal to the market value at the end of the current year. The change in house real estate is defined as the difference between the value in the current year and past year.

Regarding apartments, we use real transaction and acquisition values from the Transfer of Condominium Registry (KU55 - Överlåtelse av bostadsrätt). This registry consists of all sales of apartments for the years 1999-2000 and 2003-2014. In the case of a recorded sale, we know the exact date of the transaction and the price, but also the acquisition date and the acquisition price of the apartment.

We construct the change in apartment wealth as the difference between the value of acquisitions and the value of sales. We only consider standard sales where individuals transfer their entire ownership share of an apartment, thus excluding donations, transfers between spouses, inheritances, etc. Similarly, we only consider standard and complete acquisitions. In addition, whenever an individual buys an apartment according the Wealth Registry, but there is no information in the Transfer of Condominium Registry, for example if the apartment is sold in 2002, we use the imputed apartment value for the acquisition.

Some small adjustments are necessary to reconcile the information from these two sources. For instance, if a household buys an apartment but only moves in the next year, the KU55 registry marks the exact day when the acquisition took place while the Wealth Registry is updated only the next year. In this case, only the accurate KU55 acquisition is considered in order to avoid including the same apartment acquisition in two consecutive years.

Because KU55 is not available for years 2001 and 2002, we apply the same method as we do for houses and non-residential real estate for these two years. Whenever available, we improve by using information from acquisitions of apartments that were bought in this period and sold in the following years, thus appearing in KU55. In addition, we also calculate change in apartment real estate for households that have positive apartment values both in the current year and the previous one, but have moved during the current year. In this case the change is calculated as the difference between the current and previous market value.

After identifying all sales and acquisitions of houses and apartments, we perform a check on the timing of the transactions. Because we are not always able to observe the bank account balance, we try to match transactions that happen in consecutive years to improve the accuracy of our imputation. This means that, if in the current year a house or an apartment is sold and nothing is acquired, but a house or an apartment is bought in the next year, the acquisition is moved to the current year as most likely the proceeds from the initial sale were used. When imputing consumption for the next year, this acquisition is disregarded.

Because the other major source of financing a real estate acquisition is debt, we employ a simple unaccounted cash minimization algorithm in order to decide if a similar timing correction should be applied to the debt level in this situation. This is described below.

B.1.3 Construction of dDebt

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

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

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

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

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

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

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

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

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

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

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

Compare A_1 and A_2 and decide:

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

B.1.4 Construction of dFin

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

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

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

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

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

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

Finally, the total change in the risky asset portfolio is calculated as the sum of the active changes in the stocks, mutual funds, money market funds and bonds portfolios. Consumption decreases when the change in risky assets is positive.

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

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

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

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

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

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

Calvet, Campbell and Sodini (2007) report that the imputation problem affects 2 million of the 4.8 million households in 2002. The imputation methodology relies on the subsample of individuals for which we observe the bank account balance.

We start by dropping the extra bank accounts that become visible in 2006 after the regulation change in order to have a consistent imputation across all years (i.e. we drop visible accounts that earn less than 100 SEK interest). We regress the log bank account balance on the following characteristics: log of financial assets other than bank account balances and Swedish money market funds, log of Swedish money market fund holdings, log of residential real estate, log of non-residential real estate, household size, log of debt, square of log debt, disposable income decile dummies, parish decile dummies ranked on average disposable income, 5-year wide age group dummies, education level dummies and a series of demographics dummies such as married man, married woman, single individual, single father and single mother.

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

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

B.2 Changes in household composition

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

We dynamically adjust household composition to account for four major life changes, both before and after the household formation year. First, children are added as they are born into a household. Second, if a grown child leaves the house and forms its own single or married household, we add a household to the sample. Third, if a married couple divorces, two new households are formed each with a new household identifier. The old household unit is dropped starting in the year of the divorce. Fourth, if two singles marry or have a first child together, the single households are dropped from the sample and a new married household is added. This approach conforms with how Statistics Sweden defines and follows households. It results in strictly more household observations in every year before and every year after the household formation year than in the household formation year itself. We refer to this as the sample of *All* households. New households that are added to the sample due to life changes after (before) RY0 inherit the treatment flag of their predecessor (successor) household unit. The *All* sample consists of 2,464 unique households in the household formation year. After removing those who are older than 65 in the household formation year, we are left with 1,864 households. Of these 533 are in the treatment group.

Our main sample of study starts from the *All* sample and drops all household-year observations for households whose adult composition changes before or after the household formation year. In this *Fixed* household subsample, no new households are added before or after the household formation year. The number of households is the same in the *Fixed* and *All* samples in the household formation year. In all years before and after that year, the number of households in the *Fixed* sample is strictly smaller than in the household formation year (while it is strictly larger in the *All* sample). The *Fixed* sample drops all singles who marry before the household formation year and all married households who divorce after the household formation year. Specifically, if they are single in the household formation year, the Fixed sample drops all household-year observations when they are married. If instead they are married in the household formation year, the Fixed sample drops all household-year observations when they are single. If two adults who are not married co-habit, unbeknownst to us, the *All* sample misclassifies them as two separate households until they get married or have a child together. The *Fixed* household sample drops such households (and avoids the mistake) because their adult composition changes during the sample. Finally, the Fixed sample does not consider the households formed by grown children who leave the house. While this sample design prevents us from studying the effect of co-op conversion on life outcomes such as marriage and divorce, it focuses on a more stable sample for which results are easier to interpret.

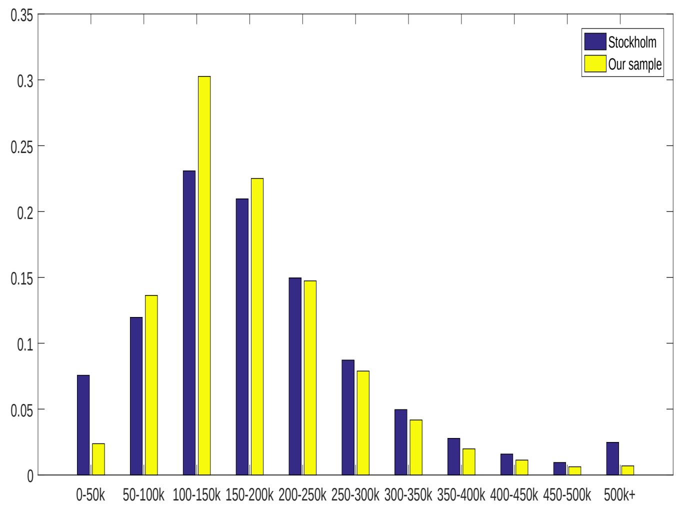
B.3 Additional summary statistics

Table A4: Summary Statistics in Treatment Year By Location

	All	**Outer City**	**Inner City**
Panel A: Sociodemographics			
Observations	1615	894	721
Age	44.95	44.43	45.58
High school	0.43	0.46	0.40
Post high school	0.25	0.25	0.26
University	0.19	0.16	0.23
Ph.D.	0.02	0.01	0.02
Partner	0.32	0.37	0.25
Number of workers	1.32	1.41	1.20
Unemployed	0.15	0.16	0.14
Panel B: Balance sheets			
Homeowner	0.03	0.03	0.04
Housing wealth	19.98	14.33	26.99
Financial wealth	106.33	77.67	141.86
Debt	89.72	77.20	105.24
Net worth	88.74	46.66	140.91
Panel C: Cash-flows			
Labor income per adult	196.28	188.98	205.41
Disposable income	171.95	164.02	181.78
Consumption	160.74	150.09	173.94
Panel D: Apartments			
Distance (km)	7.30	10.34	3.53
Area (sqm)	74.61	78.35	69.98
Number of rooms	2.90	3.12	2.38
Rent per year	42.49	40.24	45.26
Vote share	0.73	0.72	0.73

Notes: The table presents averages of variables for all households (column 1) and separately for households in suburbs (distance to center > 5km; column 2) and center (distance to center ≤ 5km; column 3) one year before treatment. 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. With the exception of Labor income per adult, all SEK variables are expressed in 1000 SEK per adult equivalent and in real terms. Labor income per adult is expressed in 1000 SEK and in real terms.

Figure A2: External validity: Income Distribution



The figure plots the distribution of disposable income (the variable $Income$) for the households in our Stopplag sample in the pre-treatment period (yellow histogram) and compares it to the distribution of disposable income for all households in the municipalities of Stockholm and Nacka during the same 1999-2002 period (blue histogram). All households included in the graph have constant adult composition over the 1999-2002 period under consideration in this table. Both samples are winsorized at the 1% and 99% levels. Income is expressed in thousands of Swedish krona, deflated by the 2007 consumer price index, and expressed per adult equivalent.

Table A5: External Validity

	(1)	(2)	(3)	(4)
	Stopplag sample		Large co-op sample	Stockholm
	Treated	Control		Renters
Homeowner	.033	.037	.048	0
	(.180)	(.190)	(.215)	(0)
Age	44.536	43.403	44.203	39.064
	(9.656)	(10.630)	(10.545)	(11.609)
Edu	1.789	1.611	1.786	1.612
	(.994)	(.979)	(1.027)	(1.051)
Partner	.372	.296	.323	.175
	(.483)	(.456)	(.467)	(.380)
Numwork	1.424	1.308	1.350	1.069
	(.766)	(.782)	(.818)	(.684)
Labincind (kSEK)	217.8	200.2	219.1	187.9
	(141.3)	(148.7)	(174.0)	(179.8)
Income (kSEK)	180.0	173.7	191.8	165.1
	(103.7)	(88.6)	(658.2)	(1060.1)
Debt (kSEK)	108.3	102.0	118.4	129.2
	(235.4)	(179.2)	(232.2)	(572.6)
House (kSEK)	26.4	22.5	34.3	0
	(181.8)	(172.8)	(219.2)	(0)
Nonhouse (kSEK)	56.9	47.2	78.4	76.2
	(186.8)	(190.0)	(336.2)	(1172.9)
Risky (kSEK)	67.0	45.0	107.0	87.1
	(338.1)	(205.7)	(576.8)	(3326.7)
Consumption (kSEK)	163.8	159.8	165.7	
	(116.9)	(119.2)	(158.6)	
N	1,451	2,884	15,493	830,832

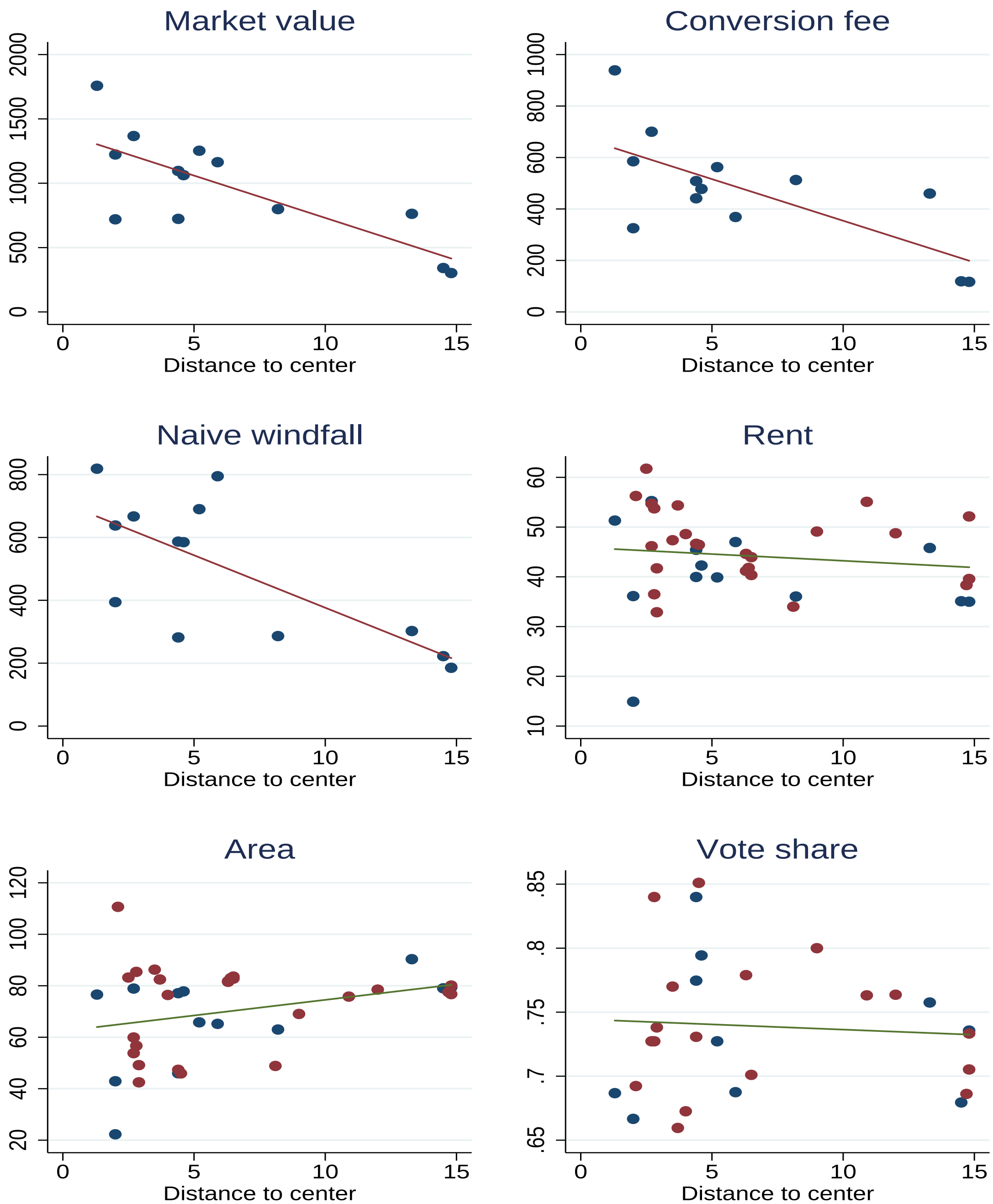
Notes: Columns 1 and 2 contain average pre-treatment household characteristics for our main estimation sample (Stopplag sample of 40 co-ops). Column 3 contains average household characteristics over the same time period (1999-2002) for a much larger sample of 247 buildings (belonging to 182 co-ops) owned by the municipal landlords Svenska Böstader and Stockholmshem. This is the universe of co-ops that were subject to a privatization attempt. They contain our 40 Stopplag co-ops. Column 4 reports average characteristics of all renters in the Stockholm and Nacka municipalities over the same time period 1999-2002. Those are the municipalities in which all 40 of our Stopplag co-ops are located. Since this sample contains of renters, the home ownership rate is zero as is housing wealth. Consumption is not available for this sample. The last row reports the number of household-year observations N included in the calculation of this table. All households included in the table have constant adult composition over the 1999-2002 period under consideration in this table. We also consistently applied the criterion that all households must have a head of household aged under 64 in the year 2002 to be included. All variables indicated by (kSEK) are expressed in thousands of Swedish krona, deflated by the 2007 consumer price index, and expressed per adult equivalent, where the adult equivalents is given by the OECD formula: 1+ (Adults-1)*.7 + (Children)*0.5. Standard deviations within the sample are reported in parentheses below the sample average.

Table A6: Detailed Statistics for Treated Households

	10%	20%	50%	75%	90%	Mean
Panel A: All Treated						
Number of households	448	448	448	448	448	448
Distance	2.00	4.40	5.90	14.50	14.80	8.03
Area	46.00	69.00	73.00	88.50	92.00	72.01
Number of rooms	1.00	3.00	3.00	4.00	4.00	2.97
Rent	20.92	26.70	38.12	55.08	62.05	41.02
Vote share	0.68	0.69	0.74	0.77	0.79	0.73
Downpayment	-84.53	-30.16	1.19	76.15	227.90	49.12
Mortgage rate	1.58	2.87	3.73	4.62	6.62	5.63
Conversion fee	90.78	199.71	412.03	608.89	755.94	428.79
Market value	263.70	487.42	866.29	1313.81	1528.37	922.14
Naive windfall	153.82	229.54	436.93	719.34	915.00	493.35
Total windfall	18.66	31.76	60.11	126.01	205.43	88.20
Total windfall per sqm	0.25	0.43	0.86	1.77	2.96	1.29
Panel B: Treated in Outer City						
Number of households	260	260	260	260	260	260
Distance	5.90	5.90	13.30	14.50	14.80	11.47
Area	54.00	72.00	73.00	92.00	92.00	75.97
Number of rooms	2.00	3.00	3.00	4.00	4.00	3.27
Rent	20.95	26.00	36.68	54.90	57.81	39.67
Vote share	0.68	0.68	0.73	0.74	0.76	0.71
Downpayment	-62.73	-16.00	5.26	83.68	202.86	43.39
Mortgage rate	1.72	3.00	3.82	4.97	6.87	6.49
Conversion fee	78.65	115.93	271.77	437.09	578.96	300.77
Market value	214.86	304.17	579.05	1107.74	1378.90	705.76
Naive windfall	141.21	178.65	292.18	568.71	906.93	405.00
Total windfall	15.19	23.48	45.10	87.71	167.98	67.98
Total windfall per sqm	0.20	0.30	0.59	1.28	2.44	0.96
Panel C: Treated in Inner City						
Number of households	188	188	188	188	188	188
Distance	1.30	2.00	4.40	4.60	4.60	3.28
Area	41.00	46.00	74.00	78.50	88.00	66.53
Number of rooms	1.00	2.00	3.00	3.00	4.00	2.59
Rent	20.92	32.06	39.72	57.94	62.41	42.89
Vote share	0.69	0.69	0.77	0.79	0.84	0.76
Downpayment	-116.31	-48.76	-0.16	47.83	324.21	57.04
Mortgage rate	0.81	2.75	3.62	4.47	6.34	4.76
Conversion fee	280.28	404.96	574.61	682.89	972.23	587.59
Market value	604.77	845.48	1194.35	1482.72	1883.32	1190.54
Naive windfall	324.48	394.13	587.88	795.54	940.16	602.95
Total windfall	31.96	46.70	90.94	162.02	232.25	113.29
Total windfall per sqm	0.52	0.79	1.46	2.34	3.44	1.71

Notes: The table presents summary statistics, measured in the year of treatment, for all treated households (Panel A), treated households in suburbs (distance $>$ 5km, Panel B), and treated households in the city center (distance $\leq$ 5km, Panel C). Distance is expressed in kilometers. Rent, Downpayment, Conversion fee, Market value, Naive windfall, Total windfall, and Total windfall per sqm are all expressed in thousands of real (2007) SEK and per adult equivalent.

Figure A3: Conversion and Co-op Characteristics by Distance to Center



The scatter plots display co-op characteristics. Household-level variables have been collapsed to averages by co-op. Blue dots indicate treated co-ops and red dots indicate control co-ops. Red lines are indicate regression line based on the sample of treated co-ops. Green lines indicate regression line based on the sample treated and control co-ops. Market value, co-op fee, and naive windfall are based on selling households' tax records and only available for treated co-ops. All variables are expressed in 1000 SEK, per adult equivalent, and in real terms. The vote share is available for 28 co-ops.

Table A7: Consumption and Savings: Dynamic ITT Effects

LHS var:	(1) Consumption	(2) Income	(3) dHousing	(4) dDebt	(5) dFin	(6) Savings
RY(-3)	-3017.8	-99.44	1275.8	-2025.8	-383.3	2918.3
	(-0.31)	(-0.03)	(0.25)	(-0.35)	(-0.05)	(0.36)
RY(-2)	-6401.6	335.4	1471.3	-4965.0	300.7	6737.0
	(-0.74)	(0.10)	(0.27)	(-0.73)	(0.04)	(0.89)
RY(0)	-11798.2	9756.4**	376469.8***	342773.0***	-12142.2**	21554.5**
	(-1.36)	(3.10)	(5.18)	(4.83)	(-2.52)	(2.61)
RY(+1)	17643.2	3229.9	-22846.7	835.3	9268.7	-14413.3
	(1.53)	(0.82)	(-1.12)	(0.06)	(1.05)	(-1.57)
RY(+2)	15470.6	3020.7	-987.1	11357.3	-105.5	-12449.9
	(1.66)	(0.78)	(-0.08)	(1.15)	(-0.02)	(-1.58)
RY(+3)	16787.1*	502.4	-14693.9	-4575.4	-6166.3	-16284.8**
	(1.79)	(0.10)	(-1.45)	(-0.59)	(-1.24)	(-2.39)
PT-Mean	159,689	165,960	1865	4,868	9,273	6,270
PT-SD	117,169	84,593	49,845	70,092	77,076	92,537
N	13,372	13,372	13,372	13,372	13,372	13,372
R^2	0.0673	0.14	0.208	0.199	0.0125	0.0156

Notes: t statistics in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.01$. Standard errors are clustered at the building level. The table reports the coefficients δ_k on the interaction between the treatment dummy and the relative year (RY) vis-a-vis treatment. The coefficients on the relative year dummies are not reported. Building fixed effects and calendar year fixed effects are included but not reported. Age, Education, and Partnership are included as control variables in all columns. The last four rows report the mean and standard deviation of the dependent variable of all treatment and control group household-year observations in the years before RY0, the number of household-year observations, and the R^2 of the regression. All variables are expressed in SEK, per adult equivalent, and in real terms. Relative years -5 through -3 are collapsed into the RY(-3) term and relative years +3, ..., +5 are collapsed in the RY(+3) term. We loose one year of data in the construction of $dDebt$, $dHousing$, $dFin$, and therefore in savings and consumption; all regressions use the same sample.

C Additional Estimation Results

Table A8: Earnings and Labor Force Participation

	(1) Labinchh	(2) Numwork	(3) Labincind
$\beta(pre)$	5308.2	0.0207	5534.1
	(1.05)	(0.88)	(1.26)
$\beta(0)$	15354.1**	0.0287*	15038.4**
	(3.28)	(1.88)	(3.14)
$\beta(post)$	4413.8	0.0230	4879.5
	(0.64)	(0.71)	(0.77)
PT-Mean	186,943	1.34	193,924
PT-SD	151,239	0.78	143,468
N	14,536	14,536	14,536
R^2	0.107	0.4	0.114

Notes: See Table 2. Labinchh in column (1) is household-level labor income, expressed per adult equivalent in real Swedish krona. Numwork in column (2) is the number of adults that are working in the household. Labincind in column (3) is individual labor income per working adult in real Swedish krona. Since it is already expressed per adult, there is no further scaling. For this table only, we focus on the subsample of adults under the age of 64 in order to eliminate retirees who have little or no control over their labor income.

Table A9: Treatment-of-the-Treated Estimation

LHS var:	(1) Consumption	(2) Income	(3) dHousing	(4) dDebt	(5) dFin	(6) Savings
RY(pre)	-4945.5	153.1	2131.5	-2979.2	-11.97	5098.6
	(-0.54)	(0.05)	(0.43)	(-0.49)	(-0.00)	(0.66)
RY(0)	-12951.1	10826.9**	413188.6***	376119.0***	-13291.6**	23778.1**
	(-1.40)	(3.22)	(6.16)	(5.62)	(-2.49)	(2.79)
RY(post)	17861.0**	2312.1	-15142.3*	922.9	516.3	-15548.8**
	(2.40)	(0.57)	(-1.71)	(0.15)	(0.10)	(-2.94)
PT-Mean	159,689	165,960	1,865	4,868	9,273	6,270
PT-SD	117,169	84,593	49,845	70,092	77,076	92,537
N	13,372	13,372	13,372	13,372	13,372	13,372
R^2	0.0679	0.14	0.225	0.213	0.0125	0.0164

Notes: t statistics in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.001$. The table reports treatment effects of the treated δ_k^{TOT} in:

$$y_{it} = \alpha + TakeUpPrivate_i \sum_k \delta_k^{TOT} RY_i(t = k) + \sum_k \gamma_k RY_i(t = k) + X_{it} + \psi_t + \omega_b + \varepsilon_{it} \tag{A2}$$

where $TakeUpPrivate_i$ is an indicator that is one if the household actually participates in the privatization and becomes an owner. Since $TakeUpPrivate$ is an endogenous variable, we instrument for it using the randomly assigned treatment group indicator $Private$ and estimate (A2) using two-stage least squares. Under the assumption that privatization offers only affect outcomes through the actual use of the privatization option, δ^{TOT} can be interpreted as the causal effect of exercising the privatization option and becoming home owner (Angrist, Imbens and Rubin, 1996). Building fixed effects and calendar year fixed effects are included but not reported. Age, Education, and Partnership are included as control variables in all columns. The coefficients on the controls are not reported. The last four rows report the mean and standard deviation of the dependent variable of all treatment and control group household-year observations in the years before RY0, the number of household-year observations, and the R^2 of the regression. All variables are expressed in real Swedish krona and expressed per adult equivalent. Standard errors are clustered at the co-op level.

Table A10: Consumption Response by Location

LHS var:	(1) Consumption	(2) Income	(3) dHousing	(4) dDebt	(5) dFin	(6) Savings
RY(pre) Inner	-1335.5	3192.1	8939.2	2050.1	-2361.6	4527.6
	(-0.09)	(0.62)	(1.20)	(0.22)	(-0.16)	(0.35)
RY(0) Inner	-11357.2	17481.9**	563883.0***	517898.9***	-17145.0*	28839.0**
	(-0.74)	(2.92)	(7.86)	(6.34)	(-1.97)	(2.47)
RY(post) Inner	11148.8	399.3	-16353.1	-3179.6	2424.0	-10749.5
	(0.96)	(0.08)	(-1.26)	(-0.30)	(0.25)	(-1.36)
RY(pre) Outer	-9741.2	-3021.8	10718.4	7227.3	3228.2	6719.4
	(-1.35)	(-0.86)	(1.04)	(0.69)	(0.84)	(1.23)
RY(0) Outer	-16506.6	4483.5*	258361.6***	232113.5***	-5258.1	20990.0*
	(-1.64)	(1.96)	(3.81)	(3.74)	(-0.96)	(2.00)
RY(post) Outer	17535.0**	3789.1	-7007.1	8693.8	1955.0	-13745.9**
	(2.94)	(0.96)	(-0.49)	(0.89)	(0.55)	(-2.33)
N	13372	13372	13372	13372	13372	13372
R^2	0.0716	0.146	0.236	0.226	0.0142	0.0178

Notes: See Table 2. The specification interacts an indicator variable for whether the building is located in the inner city (within 5 km from city center) and an indicator variable for outer city (more than 5 km) with all the terms in equation (12).

Table A11: Consumption Response by Age

LHS var:	(1) Consumption	(2) Income	(3) dHousing	(4) dDebt	(5) dFin	(6) Savings
RY(pre) Young	9377.7	4868.8	2536.0	3783.2	-3261.6	-4508.9
	(0.89)	(1.21)	(0.56)	(0.61)	(-0.45)	(-0.53)
RY(0) Young	-12627.7	9168.8**	297973.3***	270421.9***	-5754.9	21796.5*
	(-1.14)	(2.17)	(5.39)	(5.11)	(-0.91)	(1.95)
RY(post) Young	20933.0**	-6962.8	-6948.0	18395.9*	-2551.8	-27895.8**
	(2.34)	(-1.45)	(-0.67)	(1.91)	(-0.40)	(-3.19)
RY(pre) Old	-17419.2	-3747.5	599.2	-9904.6	3167.9	13671.7
	(-1.56)	(-0.85)	(0.08)	(-0.92)	(0.26)	(1.19)
RY(0) Old	-12997.9	7966.2	440206.6***	401139.7***	-18102.7**	20964.2*
	(-1.00)	(1.57)	(4.90)	(4.52)	(-2.50)	(1.86)
RY(post) Old	11456.7	4494.8	-14000.2	-5893.5	1144.7	-6961.9
	(1.26)	(0.75)	(-1.32)	(-0.74)	(0.19)	(-1.00)
N	13372	13372	13372	13372	13372	13372
R^2	0.0794	0.167	0.219	0.211	0.0165	0.0203

Notes: See Table 2. The specification interacts an indicator variable for whether the household head is under or over age 45 with all the terms in equation (12).

Table A12: Consumption Response by Predicted Windfall

LHS var:	(1) Consumption	(2) Income	(3) dHousing	(4) dDebt	(5) dFin	(6) Savings
RY(pre) Low	4687.5	1279.0	4445.5	5469.1	-2384.9	-3408.5
	(0.55)	(0.36)	(0.81)	(0.79)	(-0.37)	(-0.44)
RY(0) Low	-14468.8	7064.6**	257771.3***	229091.0***	-7147.0	21533.4*
	(-1.41)	(2.12)	(4.51)	(4.29)	(-1.11)	(1.99)
RY(post) Low	22848.2**	70.68	-4327.5	19523.1**	1073.1	-22777.6**
	(2.90)	(0.02)	(-0.43)	(2.37)	(0.19)	(-2.91)
RY(pre) High	-12233.0	963.0	735.0	-9931.6	2529.3	13195.9
	(-0.90)	(0.18)	(0.08)	(-0.84)	(0.18)	(1.06)
RY(0) High	-12209.1	10607.0	503804.0***	463191.4***	-17796.4*	22816.1**
	(-0.88)	(1.55)	(6.74)	(5.86)	(-1.91)	(2.12)
RY(post) High	12324.0	1954.7	-14530.8	-6216.4	-2054.8	-10369.3
	(1.35)	(0.29)	(-1.29)	(-0.74)	(-0.28)	(-1.54)
N	13372	13372	13372	13372	13372	13372
R^2	0.0796	0.166	0.232	0.225	0.0163	0.0206

Notes: See Table 2. The specification interacts an indicator variable for whether the household head is under or over age 45 with all the terms in equation (12).

Table A13: Housing Collateral Effect: Reversal of Consumption and Debt

LHS var:	(1) Consumption	(2) Income	(3) dHousing	(4) dDebt	(5) dFin	(6) Savings
$\eta(pre)$	12373.9	4813.8	3017.6	7150.7	-3427.0	-7560.1
	(1.25)	(0.58)	(0.75)	(0.93)	(-0.60)	(-0.86)
$\eta(0)$	-35777.7	-6848.7	17807.9	-3717.1	7403.9	28929.0*
	(-1.44)	(-0.56)	(0.45)	(-0.07)	(0.33)	(1.82)
$\eta(post)$	-21854.1	5450.1	6258.5	-14198.7	6847.0	27304.2**
	(-1.42)	(0.85)	(0.41)	(-1.01)	(0.77)	(2.37)
LZ	-52941.7***	-37782.7***	-1641.4	-12470.9**	4329.5	15159.0**
	(-7.30)	(-8.71)	(-0.31)	(-2.40)	(0.95)	(3.13)
$\beta(pre)$	-11203.5	-2523.5	5113.1	3474.4	7041.3	8680.0
	(-0.88)	(-0.38)	(1.62)	(0.58)	(0.63)	(0.67)
$\beta(0)$	-15337.8	10949.8	-17857.7	-22088.9	22056.4**	26287.5
	(-0.70)	(1.37)	(-0.54)	(-0.48)	(2.08)	(1.18)
$\beta(post)$	41130.6**	2615.7	-105.3	39638.8**	1229.1	-38515.0*
	(2.49)	(0.27)	(-0.00)	(2.53)	(0.08)	(-1.91)
Z	-29372.5***	-40926.9***	-7326.0**	-3422.8	-7651.3*	-11554.4**
	(-4.15)	(-8.07)	(-2.52)	(-0.61)	(-1.81)	(-2.50)
N	13372	13372	13372	13372	13372	13372
R^2	0.0810	0.184	0.209	0.200	0.0145	0.0197

Notes: This table reports the estimates from the regression

$$\begin{aligned} y_{it} \quad = \quad & \alpha + \sum_k \mu_k RY_i(t=k)LZ_{it} + Private_i \sum_k \eta_k RY_i(t=k)LZ_{it} + LZ_{i,t} \\ & + \sum_k \lambda_k RY_i(t=k)Z_{it} + Private_i \sum_k \beta_k RY_i(t=k)Z_{it} + Z_{it} \\ & + \sum_k \gamma_k RY_i(t=k) + Private_i \sum_k \delta_k RY_i(t=k) + X_{it} + \psi_t + \omega_b + \varepsilon_{it}, \end{aligned} \tag{A3}$$

The dependent variable is listed in the top row. The indicator variable Z_{it} is one in a period in which the household experiences a decline in labor income relative to the prior year of at least 25%, and zero otherwise. The indicator variable LZ_{it} is one if the household experiences a decline in labor income of at least 25% between periods $t-1$ and $t-2$. Thus, $LZ_{it} = Z_{i,t-1}$. The specification includes building and year fixed effects. The η terms are the coefficients on the $Private \times RY \times LZ$ indicator. The β terms are the coefficients on the $Private \times RY \times Z$ indicator. The other regression coefficients are not reported in the table for brevity.

Table A14: Housing Collateral Effect: Robustness Income Shock Definition

	(1)	(2)	(3)	(4)	(5)	(6)
	A: Inc Shock $\geq$ 30%		B: Inc Shock $\geq$ 20%		C: Inc Shock $\geq$ 15%	
LHS var:	Cons	dDebt	Cons	dDebt	Cons	dDebt
$\delta(pre)$	-2913.1	-3907.8	-3599.4	-3906.1	-4127.0	-4557.4
	(-0.34)	(-0.65)	(-0.44)	(-0.64)	(-0.49)	(-0.74)
$\delta(0)$	-6861.2	346088.6***	-8341.1	344046.5***	-6786.7	345106.3***
	(-0.77)	(4.84)	(-0.92)	(4.73)	(-0.76)	(4.69)
$\delta(post)$	13709.4*	-2133.5	12659.9*	-2842.2	13102.3*	-2223.4
	(2.00)	(-0.33)	(1.70)	(-0.45)	(1.76)	(-0.35)
$\beta(pre)$	-15256.9	370.9	-4076.6	590.6	236.6	5615.6
	(-1.47)	(0.06)	(-0.36)	(0.10)	(0.02)	(0.93)
$\beta(0)$	-43427.1*	-30555.9	-17715.1	-6820.3	-22053.3	-10848.0
	(-1.90)	(-0.73)	(-0.77)	(-0.15)	(-0.97)	(-0.25)
$\beta(post)$	30776.1**	32268.5*	35578.3**	31130.1**	26629.9**	21690.4
	(2.22)	(1.77)	(2.61)	(2.17)	(2.27)	(1.59)
Z	-37467.8***	-4492.9	-31951.1***	-4049.3	-28906.4***	-4419.2
	(-4.97)	(-0.85)	(-5.42)	(-0.65)	(-5.43)	(-0.78)
N	13372	13372	13372	13372	13372	13372
R^2	0.0752	0.199	0.0742	0.199	0.0728	0.199

Notes: The table reports estimates of equation A3 for outcome variables Consumption and dDebt. The indicator variable Z is one in a period in which the household experiences a decline in labor income relative to the prior year of at least 30% (column 1-2), 20% (columns 3-4), or 15% (column 5-6). The δ terms are the coefficients on the $Private \times RY$ indicator. The β terms are the coefficients on the $Private \times RY \times Z$ indicator. The γ coefficients on the RY indicator and the λ coefficients on the $RY \times Z$ indicator are not reported in the table for brevity.

Table A15: Housing Collateral Effect: Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)
	A: Distance to center		B: Age		C: Predicted Windfall	
	Inner	Outer	Young	Old	Low	High
$\delta(pre)$	294.0	-8585.9	12894.8	-16932.9	7799.7	-11463.4
	(0.02)	(-1.10)	(1.31)	(-1.46)	(0.97)	(-0.80)
$\delta(0)$	-7572.0	-13646.3	297.6	-14928.2	-4899.5	-14173.7
	(-0.49)	(-1.28)	(0.03)	(-1.09)	(-0.46)	(-1.01)
$\delta(post)$	6715.6	14325.9**	21230.1**	6118.3	21715.4**	8799.3
	(0.52)	(2.34)	(2.39)	(0.68)	(2.95)	(0.91)
$\beta(pre)$	-15767.2	-432.6	-9662.4	-8791.5	-8691.2	-11738.7
	(-0.90)	(-0.02)	(-0.81)	(-0.28)	(-0.74)	(-0.38)
$\beta(0)$	-22960.0	-17963.9	-72834.3**	24787.4	-49891.3	24067.4
	(-0.47)	(-1.20)	(-2.28)	(0.67)	(-1.54)	(0.62)
$\beta(post)$	55327.6	28379.0*	5319.0	75084.1**	16377.8	47136.7
	(1.71)	(2.02)	(0.27)	(2.34)	(1.00)	(1.32)
Z	-32605.9**	-37332.2***	-52893.8***	1247.6	-58799.2***	11001.6
	(-3.60)	(-4.27)	(-6.45)	(0.07)	(-6.97)	(0.70)
N	6033	7339	6178	7194	6909	6463
R^2	0.0605	0.0921	0.0769	0.0651	0.0809	0.0597

Notes: The table reports estimates of equation A3 for outcome variables Consumption, and for various sample splits. Columns 1 and 2 split the sample by Inner versus Outer city (distance from center less or more than 5km driving). Columns 3 and 4 into Young (under 45) and Old (over 45). Columns 5 and 6 split the sample into Low predicted windfall and High predicted windfall, based on a specification with distance to the center, age, and age squared. The indicator variable Z is one in a period in which the household experiences a decline in labor income relative to the prior year of at least 25%. The δ terms are the coefficients on the $Private \times RY$ indicator. The β terms are the coefficients on the $Private \times RY \times Z$ indicator. The γ coefficients on the RY indicator and the λ coefficients on the $RY \times Z$ indicator are not reported in the table for brevity.