Identifying the Benefits from Home Ownership: A Swedish Experiment

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December 2, 2016

Abstract

This paper studies the economic benefits of home ownership. Exploiting a quasi-experiment surrounding privatization decisions of municipally-owned apartment buildings, we obtain random variation in home ownership for otherwise similar buildings with similar tenants. We link the tenants to their tax records to obtain information on demographics, income, mobility patterns, housing wealth, financial wealth, and debt. These data allow us to construct high-quality measures of consumption expenditures. Home ownership causes households to move up the housing ladder, work harder, and save more. Consumption increases out of housing wealth are concentrated among the home owners who sell subsequent to privatization and among those who receive negative income shocks, evidencing a collateral effect.

Keywords: home ownership, housing wealth, MPC, collateral effect, mobility

JEL codes: D12, D31, E21, G11, H31, J22, R21, R23, R51

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1 Introduction

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] Yet, the rationale for such policies is vague. Conventional wisdom has it that home ownership confers benefits for the individual and for society. The main individual benefits are faster wealth accumulation –through the accumulation of home equity– and improved ability to maintain spending in the wake of an adverse income or expenditure shock –through the use of the home as a collateral asset against which to borrow. Examples of societal benefits are a stable community of responsible neighbors invested in their local institutions and a reduction in crime.[3] Despite the importance of the question and its obvious policy relevance, there is little solid empirical evidence for the 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 countries like the U.S., Ireland, and Spain.

To measure the economic cost and benefits of home ownership at the household level, the ideal experiment is one where identical households are randomly assigned into renters and owners. The households' economic decisions are then 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 resorted to simply comparing owners to renters. Two key endogeneity issues plague such comparisons. First, home owners are different from renters. Owners are older, more likely to be white, married, and with children, better educated, have higher income and financial wealth, as well as higher future earnings potential. These differences in characteristics correlate with tenure status

[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.

[3] E.g., The Economist (2009) .

(owning versus renting), making it difficult to separate out the effect of home ownership from the effect of these underlying characteristics. While the literature has tried to control for household-level characteristics, the approach ultimately fails to resolve the endogeneity problem: characteristics unobservable to the researcher could be driving both the tenure decisions and the outcome variable.

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, etc. could all differ. Neighborhood characteristics also differ since rental properties are more likely located in densely-populated urban areas while owned properties are more likely to be in suburban areas. Neighborhood density, its racial or ethnic makeup, distance to work, quality of the local school system, etc. are all likely to differ. One can control for such observable property and neighborhood characteristics, but fully unbundling tenure choice and dwelling characteristics is an uphill battle. It is impossible to rule out that unobserved differences in property characteristics affect both the tenure choice and the outcome variable of interest.

In recognition of these challenges, a small literature has used survey methods or quasi-experiments to study the causal effects of home ownership.[4] The few studies there are have small samples, focus on a small set of non-economic outcome variables (like life satisfaction), and the survey data they use may not carry over to actual market behavior.

This paper provides new evidence on the benefits and costs to home ownership, focusing on the economic effects to individual households. We overcome key challenges that have plagued the literature to date by using a quasi-experiment which randomly assigns home ownership. We consider a larger sample. We track more outcome variables over a longer period of time. And since our data are based on tax registries, they measure actual decisions (rather than survey responses) and are more granular and of higher quality than survey-based data.

Our study addresses the endogeneity problems by exploiting a unique setting. In the early

[4]Shlay (1985, 1986) elicits the preferences for renting versus owning of a small sample of households in Syracuse, NY. Property characteristics, including tenure status, were assigned randomly to fictitious housing choices and respondents rank houses according to their desirability. The paper finds that tenure status does not affect the desirability of the property. Rohe and Stegman (1994) and Rohe and Basolo (1997) report on a quasi experiment of low-income households who became 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.

2000s, tenants of municipally-owned apartment buildings were given the option to convert from rentership to home ownership. The private ownership rate of co-ops increased from 34% in 2000 to 43% in 2004. Private and municipality-owned landlords combined sold 42,000 apartments of which 12,000 were municipality-owned and hence privatized.[5] Since privatization attempts are endogenous, we limit our analysis to a sample of tenants in municipality-owned apartment buildings whose attempt was affected by a new law known as *Stopplag*. The law introduced an additional layer of approval by the Stockholm County Board. While the political intent of the national law was to slow down privatizations in Stockholm, it was presented as a tool to preserve the mechanism that regulates Sweden's rental market rather than an outright ban. This design of the law introduced arbitrariness in the approval process. Our experiment exploits that random variation in the outcome of privatization attempts of otherwise similar buildings with similar tenants.

Our sample consists of about 5,000 individuals that make up about 2,500 households living in 46 buildings in the Stockholm metropolitan area. This is the universe of buildings subject to Stopplag. In each building, tenants formed a co-op association, petitioned their municipal landlord to acquire the building, and voted on the acquisition. All co-op associations approved the acquisition by about the same margin at around the same time. All 46 buildings would have become privately owned were it not for Stopplag.

To understand the nature of the random variation, we highlight one example. In Akalla, four adjacent co-ops with very similar tenant population and building characteristics applied for privatization at the same time. Because of the four buildings served as benchmark in the rent setting, the Stockholm County Board decided in adherence with the Stopplag that not all four co-ops could be privatized. The Board arbitrarily decided that two of the four co-ops could be approved. Moreover, the assignment of the four co-ops into the treatment group (approval of privatization) and the control group (denial of privatization) was as good as random.

Overall, our sample consists of all 46 buildings that were subject to the additional County Board approval layer during the years that Stopplag was in effect. Ultimately, 13 of the buildings were approved for privatization. All tenants in the 13 buildings that were approved are in

[5] Several other countries like the United Kingdom, the Netherlands, and Germany went through similar privatization programs in the 1980s and 1990s (Elsinga, Stephens and Knorr-Siedow (2014)). Hong Kong provides a more recent example. We are not aware of any other work that has studied these episodes using micro data or has exploited a natural experiment like ours.

our treatment group, while all tenants in the 33 buildings that were denied are in our control group. The creation of a control group of denied tenants enables us to estimate household level effects of home ownership in a standard difference-in-difference regression framework. We show that the groups are balanced in terms of building and household characteristics. More importantly for identification, we show that all outcome variables of interest display parallel trends prior to the Board's decision.

Assisted by Statistics Sweden, we are able to track down all residents in these 46 buildings by matching on address and manually consulting original back-dated tenant lists provided by the landlords. We fix the set of households to all those who lived in the 46 buildings in the year before the County Board decision. We dynamically track all members of these households for up to four years before the decision year and up to five years after the decision year. Statistics Sweden provides us with their detailed demographic, income, financial wealth, and housing wealth data from tax records for the period 1999–2007. As explained in Calvet, Campbell and Sodini (2009), the Swedish data contain full detail on every stock, bond, and mutual fund the household owns and every source of income. The tax registry data is rich enough to construct a precise savings measure. Combining income and savings, we impute total consumption expenditures as a residual from the budget constraint. We improve on the consumption construction, first used by Koijen, Van Nieuwerburgh and Vestman (2014), to deal with changes in real estate wealth.

Our experiment has several nice features. First, privatizations were cash-flow neutral because the monthly co-op dues plus the mortgage payment were about the same as the monthly subsidized rent tenants paid prior to privatization. Second, landlords did not set out to maximize profits. Landlords set the asking price equal to the net-present value of rents minus operating expenses. Because Swedish rental markets are regulated, converters could purchase their apartment at a discount from the prevailing market value in the ownership market. This discount, in turn, allowed them to obtain 100% personal mortgage financing. For example, at a 30% discount, the mortgage principal would only amount to 70% of the market value of the property. Thus, financial constraints played no role in the conversion decision.

The first basic finding is that the take-up rate of conversion, conditional on approval to privatize, is very high. 93% of tenants in approved co-ops exercise their option to buy their apartment. The treatment effect on home ownership is large and persistent. While some households subsequently sell their co-op 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. This finding indicates a latent desire for home ownership. Once conferred, home ownership remains the desired tenure status for the vast majority of households.

Our main results use the detailed wealth data and the consumption imputation to study the consumption response to home ownership. We highlight three effects.

First, we document an intertemporal shift in consumption. We find an initially negative treatment effect on consumption. In the year of the privatization, treated households choose to reduce consumption (and sell financial assets) to make a sizeable downpayment. This is arguably a surprising finding since they could have easily obtained a larger mortgage; treated households' LTV ratios only ranged between 30% and 70%. The sharp reduction in consumption in the initial year of home ownership could be driven by an expectation of high house price appreciation (a smaller mortgage is equivalent to a larger housing investment) or by an aversion to high household leverage, as argued by the literature on debt aversion.[6]

In the years following conversion, we find a surprisingly weak effect on consumption. The average treated household does not borrow against her considerable housing wealth to boost spending, but rather gradually pays off the mortgage and accumulates home equity. This behavior is consistent with the wealth building advantages often associated with home ownership.

However, the weak consumption response masks substantial heterogeneity. In particular, households who stay in their apartment after privatization refrain from borrowing against their home equity to fuel consumption, but rather pay off their mortgage.Household who move, in contrast, increase spending considerably. Thus, we find that consumption responses are concentrated on those who monetize/liquify their illiquid housing wealth. This finding holds up whether we compare treated movers to the entire control group or to movers in the control group, and if we instrument the moving choice by pre-determined demographic variables.

Second, we contribute to the literature that studies the marginal propensity to consume out of housing wealth. The discount offered by municipal landlords implies that the experiment not only bestowed home ownership status upon treated households, but also a windfall

[6]E.g., Caetano, Palacios and Patrinos (2011).

in the form of illiquid housing wealth. We find a 2.1% MPC out of this housing wealth windfall per year for the four years after privatization. This is a low estimate, even relative to the evidence from aggregate data and the typical MPC numbers arising from models with complete insurance. It is far below more recent estimates that use evidence from the Great Recession and richer life-cycle models with financial constraints and risky labor income.[7] The MPC estimate for movers (6.7%) is an order of magnitude larger than that for stayers (0.6%).

Third, we document new evidence for a housing collateral effect.[8] Treated households who suffer a large labor income shock smooth consumption by borrowing against their housing collateral. In contrast, the control group sees consumption fall by about as much as income. We observe this collateral effect even among stayers, suggesting that adverse circumstanced trigger home equity extraction.

Aside from consumption and savings, we study three other margins of household behavior. We find a positive treatment effect on labor income. Home ownership induces households to work harder. The effect occurs mostly at the intensive margin, but there is a small extensive margin effect through increased labor force participation. This effect is surprising to the extent that it overcomes the decrease in labor supply that is predicted by the increase in wealth from the windfall. While we cannot rule out alternative explanations, we find that the treatment effect on labor income is stronger among movers who take on more debt upon conversion, as in Fortin (1995) and Del Boca and Lusardi (2003).

We also study the composition of savings, in particular participation in risky asset markets. We find a positive treatment effect on stock market participation (extensive margin) and on the share of risky assets in the financial portfolio, conditional on participation. These findings are consistent with the intensive margin effects documented by Vestman (2016) and Chetty, Sándor and Szeidl (2016). Chetty et al. argue that an increase in home equity, as opposed to an increase in mortgage debt holding fixed home equity, increases the risky asset share conditional on participation because it makes households effectively less risk averse. We confirm their results in a quasi-natural experiment in Sweden, show that the home equity effects

[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), and Berger et al. (2015). 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).

[8]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).

dominates in our context, and extend their results to the extensive participation margin.[9]

Finally, we study mobility. We find that treated households become more mobile. They are more likely to move to a different address, move to a different parish (ZIP code), or to a different municipality. When they move, they are more likely to trade up to "better" areas where real estate is more expensive or disposable income is higher. Higher geographic and economic mobility is consistent with the housing ladder hypothesis, whereby households use the capital gains made in the sale of one property to make a downpayment on another one, of better quality/size or in a better neighborhood. The increased mobility finding is inconsistent with the "housing lock" sometimes associated with home ownership. We find that mobility increases weakly in the windfall conferred by the privatization process, but is strongly present in all quartiles of the windfall distribution.[10]

The treatment effect in our study is the combined effect of home ownership and a windfall. Every policy that promotes home ownership is associated with a windfall. Such policies redistribute wealth from all taxpayers to present and prospective home owners. Moreover, in the aftermath of a transition from rentership to ownership, house prices change and cause a positive or negative wealth effect through market mechanisms. 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, last part of our analysis studies how treatment effects differ across groups sorted by the size of the windfall. By and large, the evidence points to similar effects across all groups. The finding that our results do not differ much across windfall groups is consistent with the view that these results are mainly a home ownership effect and less of a windfall effect. Along the same line, we also explore how treatment effects differ by age, labor income, and financial wealth.

This paper relates to several strands of the literature. There is a large literature on the social benefits from home ownership. This literature has been inconclusive on whether or not ownership leads to better property maintenance, better outcomes for children, and more

[9]See Cocco (2005) for a theoretical framework and Davis and Van Nieuwerburgh (2015) for a review of the literature on housing and portfolio choice. Calvet and Sodini (2014) study the determinants of financial risk taking using data on the portfolios of twins. Briggs et al. (2015) study the effect of lottery winnings on stock market participation in Sweden.

[10]We find high and similar degrees of mobility among both renters and owners in Stockholm, suggesting that the institutional features of the Swedish rental market do not create barriers to mobility, and cannot account for these results.

involvement with the local community.[11] 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. This paper focuses on the personal benefits from home ownership, leaving a detailed study of the social benefits for future work.

Our work also relates to work that studies the effect of subsidies given to poor households for moving to better neighborhoods, the moving-to-opportunity (MTO) program. Chetty, Hendren and Katz (2016) and Kling, Liebman and Katz (2007) find positive effects on the educational and labor market outcomes for the children of the treated households. The MTO program is a rental subsidy aimed at the poor while our experiment is aimed at ownership and affects a broader cross-section of the population. It focuses on the treated households rather than their offspring. Nevertheless, our upward mobility results are consistent. Like in our experiment, the MTO experiment has a windfall component.

The rest of this paper is organized as follows. In Section 2, we discuss the institutional context in which the co-op conversions took place. In Section 3, we discuss our data sources and construction in detail and we present a balance test for treatment and control groups. Section 4 contains our empirical specification. Section 5 shows the treatment effect on home ownership and household stability. Section 6 contains the main results on consumption and savings. Section 7 studies stock market participation. Section 8 contains the results on mobility. Section 9 studies how the treatment effects differ by windfall, age, income, and financial wealth. Section 10 discusses treatment of the treated estimation, and Section 11 concludes.

2 The Privatization Experiment

In this section, we briefly summarize the key features of the privatization experiment. Co-operatives (co-ops) are legal entities of individuals that collectively own the apartment building. By *co-op conversion* we mean the transfer of legal ownership of the property from a landlord (private or public) to the co-op association. By *privatization* we mean a co-op conversion that involves a public (municipal) landlord. Individual members of the co-op association own

[11] 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.

co-op shares representing the ownership of their apartment unit.

2.1 Background and Stopplag

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. In short, the rental market in Sweden is regulated, as discussed below. While some early experiments with privatization took place in the late 1980s and early 1990s, the privatization program started in earnest only after the September 1998 general election. In Stockholm, a center-right wing coalition took power and one of its chief political aims was to sell residential real estate owned by the three large Stockholm municipal landlords (Svenska Bostäder, Stockholmshem, and Familjebostäder) to its tenants. These three municipal landlords owned about 110,000 apartments or 30% of the apartment stock in Stockholm. They privatized 12,200 apartments 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 cop-op conversion process that included private landlords. Appendix A provides detailed statistics.

In November 2001, the federal Social Democratic-led coalition government proposed a law, known as *Stopplag*, which was passed by the parliament in March 2002 and went into effect on April 1, 2002.[12] The underlying purpose of the law was to halt or at least slow down the co-op privatizations. For political reasons, it went about this in a roundabout way. Since 1974, rents in Sweden are set by negotiation between landlord and tenant associations. If negotiations fail, a regional board determines the rent.[13]

[12]The Swedish name of the law is Lag om allmännyttiga bostadsföretag, SFS 2002:102.

[13]If negotiations fail, the law states that the rent should we set according to a set of criteria based on the location and characteristics of the apartment, (bruksvärdereglerna). The rents are set at a fine level of granularity: by narrow geographic area, by apartment type, and by quality of finish. Prior to 2010, only municipal landlords' apartments could be used in benchmarking and comparisons by these regional boards. Effectively, this meant that rents were closely synced to the cost of maintenance of the municipal landlords. The regional board includes representatives of landlord associations (e.g., SABO and Fastighetsägarna) and tenant associations (e.g., Hyresgästföreningen). Even (public or private) landlords that are non-members of landlord associations are bound by the rent-setting decisions of the regional board. Thus, while there is private ownership of for-rent multi-family properties, there is no free rental “market” in Sweden because private landlords must not escalate rents faster than the increase mandated by the regional board.

Between 1974 and 2010, only the housing stock owned by municipal landlords could serve as the reference object by these regional boards. It was thus desirable that municipal landlords had a diverse housing stock consisting of apartments in all geographies, of all sizes, and qualities in order to fulfill their role as yardstick. With the passing of the Stopplag the municipal landlords became obliged to seek approval from the County Board to sell any part of their residential housing stock. The Board became responsible to determine if sufficient benchmarking objects would remain if the transaction would be approved.[14] It gave substantial latitude to the County Board in determining the approval process. Stopplag resulted in a dramatic slowdown in the pace of privatizations of municipally-owned apartments in 2003 and 2004. 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 made up only a small part of the co-op's apartment mix. A detailed reading of all minutes of the County Board meetings shows a large degree of arbitrariness in the approval process. Below, we provide the example of the Akalla complex, with more detail in Appendix B. Importantly, the Akalla example shows how virtually identical buildings were randomly split into the treatment and control groups.

The general election of September 2002 meant that the Social Democrats continued to be the majority party in the government. 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. The conservatives rekindled the co-op conversion program and a second wave of privatization started in 2007-08, after our sample ends.

2.2 Co-op Conversion 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.[15] Once registered, the co-op can

[14]Prior to Stopplag's passing, the County Board had not been involved in overseeing the municipal housing stock and had no role in the rent-setting process.

[15]A co-op needs at least three members. The co-op board consists of at least three and at most seven board members.

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. 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 develop 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 submitted votes must be in favor for the conversion to go ahead.[16] Upon a favorable vote, the co-op board communicates the vote tally and the minutes of the meeting to the landlord.[17]

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.

[16] 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.

[17] 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.

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.[18] The conversion attempt of the Akalla complex, described in detail in Appendix B, serves as a good example of the random nature of the County Board decision. Four co-ops with buildings adjacent to each other in the suburb Akalla, owned by the same municipal landlord, constructed in the same year go through the conversion process at the same time. All four co-op's tenant associations vote for conversion by nearly the same margin. All four are approved by the landlord. The County Board considers all four conversion attempts in one single meeting. It establishes that it cannot privatize all four co-ops because then it would no longer retain sufficiently many low-rise buildings, which all four co-ops have in their courtyards as a small part of their overall footprint. However, the County Board decides that it should allow the municipal landlord privatize two out of the four co-ops without compromising the latter's ability to serve as a yardstick for the rental market. The County Board is not guided by the law, nor has established procedures for choosing between the co-ops. It decides to prioritize the two buildings whose tenant associations voted first. All four votes were spaced very close in time so that the approval/denial is essentially random. Furthermore, different rules the County Board could have chosen, such as the date of approval of the landlord or the highest voting share in favor of privatization would have resulted in a different outcome.

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, the likelihood of success was reduced from 50 percent to 17 percent.[19]

[18] 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.

[19] 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.

2.3 Budget Implications of Conversion

The economic plan and the appraisal report contain detailed information on the financial implications for participants in the conversion. Because the conversion program was politically motivated, the Stockholm municipal landlords did not set out to maximize profit. The appraisal reports and the sale prices make clear that the buildings were valued at the present discounted value of rental income minus operating expenses, using a standard interest rate. The properties were valued as if the buyer would be another landlord, subject to the same rent regulation as the selling landlord.

Because of the law on the determination of rents, as well as tight zoning laws and other restrictions on construction, apartments are scarce in Stockholm. Apartments-for-sale are expensive relative to the net present value of rents. Thus, the buildings were sold to the co-ops at a discount to their private market value under ownership.[20]

Tenants who live in co-ops approved for conversion have a choice of whether to buy their unit or not. If they do not buy, they remain as *residual tenants*. They keep their old rent which they now pay to the co-op. Tenants who convert pay the one-time conversion fee as well as monthly co-op dues. In order to finance the conversion fee, the household typically needs to obtain a personal mortgage. One of the nice features of our experiment is that, because the one-time conversion fee is (far) below the market value of the unit on the private ownership market, financial constraints play no role in the conversion decision. That is, households who want to convert qualify for a mortgage principal equal to the full conversion fee.[21]

A second nice feature of our experiment is that conversion has no implications for the monthly user cost of housing. The monthly rent that converters used to pay is about equal to the monthly co-op dues plus the personal mortgage payment. Combined with 100% financing of the conversion fee, this cash-flow equivalence implies that there are no mechanical cash

[20] The rent regulation and the limited supply lead to a net excess demand for rentals. Households queue with municipal and private landlords, often for many years, to obtain a rental apartment. Households in the municipal queue can apply for vacant apartments and the apartment is assigned based on queuing time among the applicants. However, we note that the rent is not subsidized. The private rental market must charge the same rent for the same apartment in the same neighborhood. We also note that there is substantial mobility within the rental system. An active, online exchange platform enables households to trade apartments. Finally, households who purchase their apartment in a privatization have ways of remaining in the municipal landlord queue, should they decide to return to the rental system at a later date. For example, one adult in a household could purchase the apartment while the other spouse remains in the queue.

[21] In our sample, the conversion fees paid for the co-op shares are between 30% and 70% of their market value. Put differently, an 80%-LTV limit would have qualified all converters to a mortgage with principal at least equal to 100% of the conversion fee. Such mortgages were freely available in Stockholm at the time.

flow implications from privatization. Appendix C works through a numerical example for one of the co-ops in our sample.

The main implications from conversion are therefore that (a) the converters become home owners and (b) they receive a windfall in the form of illiquid housing wealth. Converters can liquefy the housing wealth windfall by selling their unit on the co-op market and moving. Unless they do so immediately, the financial benefit from owning over renting depends on the length of stay and the evolution of house prices and rents. Appendix C compares the cost of owning versus renting for multiple horizons in the concrete example of Akalla.

3 Data

Our data comes from four main sources: Statistics Sweden files containing federal tax records of every single tenant, the archives of the municipal landlords in Stockholm, the archives of the County Board, and individual co-op associations.

3.1 Sources

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

The second source of data are the archives of the municipal landlords in Stockholm. This is hand-collected data in the form of pdf files for each co-op. For all co-ops affected by the Stopplag, we obtain the date of first contact between the co-op and the landlord, the appraisal report, the economic plan that the co-op has to file with the landlord, and the rent for each unit. We ask the landlords to send excerpts from their database of tenants directly to Statistics Sweden to preserve anonymity. These excerpts contain information about the size of the apartment that the household rents in square meters, as well as the identity of the households.

Third, we link the properties that were subject to a conversion attempt to their tenants and their demographic and financial information. From the Statistics Sweden dataset we obtain detailed micro-level information on all individuals that lived in these buildings at any point between 1999 and 2013. The data contain detailed demographics, income data, wealth data, and all car transactions. These wealth data are so detailed that, when combined with asset-level return data, we can construct the rate of return on an individual's portfolio. Combining all income, asset, and liability data, this allows us to compute a high-quality registry-based measure of consumption and savings. Because the wealth data are only available until 2007, our analysis is for the period 1999 to 2007.

Fourth, we hand collect information about residual tenants in the co-ops that successfully privatized. For eight of the thirteen 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.

3.2 Sample of co-ops

We focus on the subsample of 38 co-ops affected by Stopplag.[22] They combine for 46 buildings. Of these, 13 co-ops with 13 buildings convert. This is the treatment group. The other 25 co-ops with 33 buildings are denied conversion and constitute the control group. Of the 38 co-ops, 29 are initially owned by Svenska Bostäder, the other 9 by Stockholmshem. The co-op registration range from January 1999 to April 2002. The date of first contact between the co-op and the landlord is typically shortly after co-op registration and ranges form May 1999 to April 2002. For all but one co-op, the date of first contact is before the passage of Stopplag in March 2002. In that one case, it is just 10 days after the law is approved. In 35 out of 38 cases, the date of first contact is well before the Stopplag was even proposed (November 2001). We have tenant association voting dates on the conversion for 28 co-ops. They range from April to September 2002, except for one vote which takes place in February 2003. All of these 28 co-ops vote in favor of conversion, with voting shares ranging from 67.3% to 84.2%. Because all 38 co-ops received approval for conversion from their municipal landlord after April 1^{st} 2002, all were subject to the additional approval decision by the County Board under Stopplag. The County Board decisions took place between September 2002 and June 2004, with one exception; 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.

The 46 buildings range in size: the smallest 5 have 21 apartments or fewer while the largest

[22] There are an additional ten co-ops denied by the County Board that privatize in the year 2007, immediately upon the abolition of the Stopplag. Since we observe no data after 2007, we choose to drop these co-ops.

Table 1: Balance Test at Co-op Level

	Control	Treated	Treated-Control
Total floor area (m^2)	5,226	5,282	56
	(4,995)	(3,958)	(1,656)
Number of apartments	68.4	70.1	1.7
	(61.9)	(39.9)	(20.4)
Average apartment size (m^2)	75.0	75.3	0.3
	(15.6)	(26.6)	(7.1)
Year of construction	1958	1954	-4
	(23.1)	(24.8)	(8.3)

Notes: Building characteristics for control group of co-ops (column 1) and treated group of co-ops (column 2). Standard deviation is in parentheses. Column 3 reports regression coefficients of the characteristic on an indicator of being treated. The regression coefficient's standard error is in parentheses.

5 have more than 100 apartments. The smallest co-op has 12 units, the largest 273. Table 1 presents key features of the co-ops in the treatment and control groups. The last column shows that there are no significant differences between the two groups in terms of total floor area, number of apartments, average apartment size, and year of construction.

There are two important dates for our experiment: the conversion decision year, which we call relative year 0 (RY0), and the household formation year. For conversions that were 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). When 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 as Relative Year 0. 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 RY(+k), the years before as RY(-k), for $k = 1, \cdots, 4$.[23]

The household formation year is the year in which we form our sample of tenants. This is the set of individuals we will track both before and after the conversion decision. We want the household formation year to be a year in which there is still substantial uncertainty over the outcome of the approval process. We set the household formation year equal to RY-1, one year before the decision year, for all co-ops except for four where we set it to RY-2. These are four cases where the conversion is 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

[23]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.

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. We will sometimes refer to the household formation year as RY-1 even though that is slightly inaccurate.

3.3 Household Formation

Our dataset 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.[24] The 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.

We also study a second sample of households which starts from the *All* sample but drops all household-year observations for households whose adult composition changes before or

[24] The alternative approach is to define a household as the constant union between its members in the household formation year, regardless of the life changes that take place before and after household formation. We think this approach is unappealing. Two adults that were married pre-conversion but divorce post-conversion are presumably no longer making joint decisions. Also, two adults who are single at household formation, but who marry post-conversion would be assumed to still be making their separate decisions.

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 RY0 and all married households who divorce after RY0. 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.[25] The *Fixed* household sample drops such households (and avoids the mistake) because their adult composition changes during the sample.[26] 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.

Finally, within the *Fixed* household sample, we study two subsamples of *Stayers* and *Movers*. We define *Stayers* as those households who do not move between the conversion date and the end of the sample in 2007. We define *Movers* as those households who do move at some point between the conversion date and 2007.[27] Each household is in one group only, and together the two groups make up the Fixed sample. In each group, we follow the same households back in time pre-treatment. While the decision to stay in place or move to another address is obviously endogenous, studying these two groups separately helps to shed light on the economic mechanisms at play. To overcome this endogeneity concern, we also report results in which we instrument the moving decision with pre-determined variables and our results carry over to this setting.

[25]We do not observe the exact household structure for all individuals living in a building. We only know that two adults live in the same apartment and belong to the same household unit if they are married or if they have a child together (in which case they must register their partnership).

[26]Specifically, if they are single in RY0, the Fixed sample drops all observations where they are married. If instead they are married in RY0, the Fixed sample drops all observations where they are single.

[27]Moving is defined based on the population registry. We have a (first) moving in and a (last) moving out date for each individual and building. The household's moving in date is the earliest one among the household members and the moving out date is the latest.

3.4 Outcome Variables

Our ability to match the tenants in co-op conversions with household-level characteristics is what makes our paper's data unique. The following main variables of interest are available to us from Statistics Sweden. All nominal variables are deflated by the Swedish consumer price index based in 2007.

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 the household formation year. *Partner* takes on the value of one for married individuals, those with registered partnerships, and for unmarried couples with a child. *Anymove* takes on the value of one if one of its adult household members changes its official registered address. We also construct an indicator variable *Parishmove* that is one if an adult household member moves its official address to a different parish, akin to a U.S. zip code, and *Municipmove* if an adult member changes municipalities, a larger geographic unit akin to a U.S. county.

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 disposable income. It is the measure that enters the household budget constraint. It includes both labor income and financial income (including income from real estate) and is after-tax.

Debt – We observe total household-level debt. We only have data for total debt, *Debt*, but no separate information on mortgage debt.[28] *Interest* is the interest paid on *Debt*. *dDebt* is the difference between total debt at the end of the current and the previous year minus *Interest*. 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 D describes

[28] 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.

our algorithm for adjusting the timing of debt.

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, the Transfer of Condominium Registry (KURU55), for the value of apartments. KURU55 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 KURU55 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 transfer 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 transfer fee would have been for each tenant had they bought.[29] 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.[30] 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 (*Anymove* is one). 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

[29] We multiply the size of each tenant's apartment in square meters with the median price per square meter, calculated from the transfer fees per square meter paid by households in the same building who sold their apartment prior to the end of 2014. From KURU55, we know what they bought the apartment for upon conversion.

[30] 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 KURU55 data alone. We end up with 40 residual tenants out of 1,864 households (2%) or out of 533 treated households (7.5%).

every individual in our sample. We also have information on the total value invested in bonds for each individual. Individuals must report the end-of-year value of each asset they own 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.[31] For the capital insurance accounts, we observe the year-end balance but not the asset mix. We assume it is a 50-50 mix of equity and bonds. Regarding pension accounts, we observe contributions made in the year. Withdrawals are included in disposable 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 price appreciation over the course of the current tax year. This requires pulling in price appreciation data on thousands of individual financial assets.[32] 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 ultimately 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:

$$Cons = dDebt - dHousing - dFin + Income \tag{1}$$

[31] Reporting requirements on bank accounts vary across time, depending on interest earned between 1999 and 2005 and on bank balance in 2006-07. Appendix D provides more detail on our imputation procedure, which further improves on Calvet, Campbell and Sodini (2007).

[32] For bonds, we do not have such price information, and we apply a bond index return to the individual bond positions to calculate the passive value. All dividend and interest income is part of the disposable income measure.

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.[33] It is a measure of total annual spending. As such, it includes durable spending 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.[34]

Separately, we obtain information on car purchases from the Swedish car registry. We label this measure *Cars*. It allows for comparison with the prior literature which has often only had car spending as a crude proxy for total consumption. We define *Savings* as *Income* minus *Cons*.

3.5 Balance Test

Table 2 reports summary statistics and balance tests for our main covariates, once for the All sample (columns 1-3) and once for the Fixed sample of households with stable adult composition (column 4-6). The table reports averages over the four pre-treatment years. The summary statistics show that the treatment and control groups are quite similar in the pre-treatment period in terms of demographics and socio-economic characteristics. Both groups are unlikely to own real estate (3.8-3.9% ownership rates).[35] The oldest adult in the household is 43-44 years of age in the pre-treatment period in both groups. The treated are more likely to be married or in a partnership, but the 7.6% point difference is not statistically different from zero. The treated are 1.2% point less likely to move in the pre-treatment period, but this difference is again not statistically different from zero. The higher partnership rate results in a larger average number of employed adults in the treatment group: 1.4 versus 1.3, a difference which is statistically significant. Labor income per adult in the household and total

[33]The four (minor) sources of measurement error we mentioned above 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 with the exact asset mix of the capital insurance accounts.

[34]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.

[35]This is property they own but where they are not registered.

Table 2: Balance Test at Household Level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		All			Fixed		Larger co-op sample
	Treated	Control	T-C	Treated	Control	T-C	All
Homeowner	.039	.038	.000	.033	.037	-.004	0.051
	(.193)	(.192)	(.010)	(.180)	(.190)	(.009)	(.221)
Age	44.11	43.09	1.02	44.54	43.41	1.13	43.94
	(9.78)	(10.71)	(.76)	(9.66)	(10.63)	(.78)	(10.62)
Partner	.371	.294	.076	.372	.296	.075	.321
	(.483)	(.456)	(.047)	(.483)	(.456)	(.047)	(.466)
Anymove	.071	.083	-.012	.060	.078	-.018*	.088
	(.257)	(.277)	(.009)	(.238)	(.269)	(.009)	(.284)
Numwork	1.416	1.310	.106*	1.424	1.308	.116**	1.346
	(.756)	(.786)	(.053)	(.766)	(.782)	(.054)	(.815)
Cars	.126	.140	-.013	.126	.141	-.014	.115
	(.332)	(.347)	(.012)	(.332)	(.348)	(.012)	(.319)
Labincind (kSEK)	216.5	201.1	15.4	217.8	200.3	17.4	218.6
	(141.9)	(149.5)	(12.8)	(141.3)	(148.9)	(13.0)	(173.2)
Income (kSEK)	178.7	173.8	4.9	180.0	173.9	6.0	190.7
	(103.4)	(89.4)	(9.1)	(103.7)	(88.8)	(9.6)	(645.4)
Debt (kSEK)	110.7	103.4	7.3	108.3	102.0	6.3	121.7
	(232.3)	(180.5)	(15.4)	(235.4)	(179.1)	(15.4)	(268.6)
House (kSEK)	31.4	24.3	7.1	26.4	22.5	3.9	37.6
	(198.7)	(183.7)	(11.5)	(181.8)	(172.8)	(9.5)	(230.1)
Nonhouse (kSEK)	55.3	47.3	7.9	56.9	47.2	9.7	79.7
	(182.0)	(188.8)	(15.0)	(186.8)	(189.9)	(15.7)	(401.5)
Risky (kSEK)	65.7	45.0	20.6	67.0	45.3	21.7	107.6
	(329.1)	(202.8)	(16.1)	(338.1)	(206.4)	(17.1)	(578.2)
Cons (kSEK)	164.4	160.6	3.7	164.6	160.5	4.1	161.7
	(120.2)	(120.8)	(9.6)	(117.3)	(119.8)	(9.9)	(158.9)
N	1,560	3,014		1,451	2,885		16,131

Notes: Pre-treatment average household characteristics for the treated (columns 1 and 4) and control group (columns 2 and 5), with standard deviation in parentheses. Columns 3 and 6 report regression coefficients of the characteristic on an indicator of being treated. The regression coefficient's standard error is in parentheses. Standard errors are clustered at the co-op level. The first three columns are for the sample of All households and the next three columns for the Fixed sample with stable adult composition. Column 7 is for a much larger sample of all 186 co-op conversion attempts of 259 buildings owned by the municipal landlords Svenska Böstader and Stockholmshem; it is an All household sample. Income, Debt, House, Nonhouse, Risky, Cons are all expressed per adult equivalent, where the adult equivalents is given by the OECD formula: 1+ (Adults-1)*.7 + (Children)*0.5. The last row reports the number of household-year observations the balance tests are based upon.

household disposable income per adult equivalent, expressed in thousands of SEK, are no different between treatment and control. Debt, housing wealth, non-residential real estate wealth, financial asset wealth, and consumption are all statistically indistinguishable for the two groups in the pre-treatment period.

The last column of Table 2 reports the same characteristics for a much larger sample of 186 co-cop conversion attempts of 259 buildings owned by the municipal landlords Svenska Böstader and Stockholmshem. It includes all the buildings in our main Stopplag sample; the other buildings were not affected by Stopplag. Like the other columns, the data refer to the pre-privatization period 2000-2002. It shows very similar average household characteristics than in our main sample of 38 co-ops/46 buildings. In other words, the co-ops conversion attempts we study are a representative sample of all municipal co-op conversion attempts at that time.

4 Methodology

In this section, we discuss our quasi-natural experiment where due to the introduction of Stopplag several co-ops were allowed to convert while others were not. We estimate "intent-to-treat" (ITT) effects of the conversion treatment. For a household-level outcome variable y measured in year t, we have:

$$y_{it} = \alpha + Convert_i \sum_k \delta_k RY_i(t = k) + \sum_k \gamma_k RY_i(t = k) + X_{it} + \psi_t + \omega_b + \varepsilon_{it}, \tag{2}$$

where α is the intercept of the regression. $Convert_i$ is an indicator variable which is one if household i lives in a building that was approved for conversion. 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 fully dynamic specification, 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 our main tables, we consider a more parsimonious specification where 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 intent-to-treat 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 the pre-treatment estimates $\delta_{-3}, \delta_{-2}, \delta_{-1}$. 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 (allowing for common error components across tenants of the same co-op) because randomization occurred at the co-op level.[36]

As is standard in difference-in-difference specifications like (2), one interaction term and one RY term are not identified. We drop the terms $Convert_i RY_i(t=-1)$ and $RY_i(t=-1)$. This allows us to interpret all δ estimates relative to the household formation year. Put differently, all δ coefficients are relative to the household formation year, a natural choice for base year. The treatment and control groups have the same outcome variable in $RY(t=-1)$, conditional on the controls.[37]

Sections 5-9 report estimates of (2) for various outcomes y_i. We begin by analyzing "first-stage" effects on actual conversion rates, i.e., home ownership. We then turn to the impact on outcomes related to mobility, labor supply and income, savings behavior, and consumption. In section 10, we study the treatment effect on the treated, looking at those who actually take up the offer to convert.

5 Home Ownership and Demographics

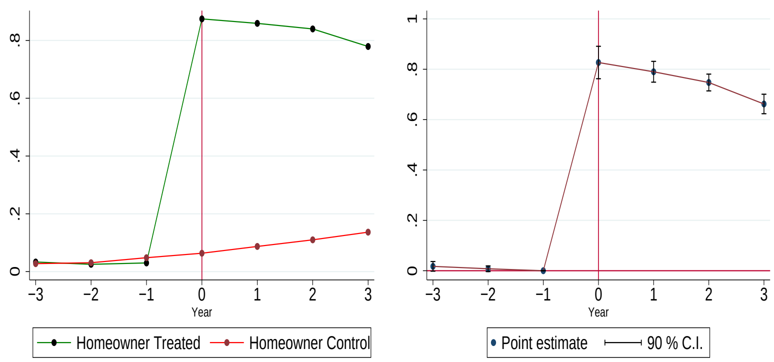
5.1 Home Ownership

As a first-stage effect, we investigate the effect of treatment on home ownership. The left panel of Figure 1 plots the raw home ownership rate for the treatment and control group for the years before and after privatization. The right panel plots the dynamic ITT effect estimates from equation (2) with home ownership as the outcome variable. The figures are for the Fixed sample of households. As explained above, we combine the early years into the RY(t=-3) variable and the late years into the RY(t=+3) variable. The figures confirm that the home ownership rate is extremely low for treatment and control group pre-treatment, and not significantly different. There are parallel pre-trends in home ownership rates. Both panels of Figure 1 show a large jump in the home ownership rate in the year of treatment for the treated relative to the control and relative to the household formation year RY(t=-1). The treatment effect on home ownership persists for many years. The left panel of Figure 1 shows

[36]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.

[37]We have also estimated all our results under a different normalization, where we rescale all δ estimates so that the sum of $\delta_{-4}, \delta_{-3}, \cdots, \delta_{-1}$ is zero. The results are similar.

Figure 1: Home Ownership



Left panel: home ownership rate for the treatment and control group; raw data. The sample is the sample of Fixed households. Right panel: dynamic ITT effect estimated for the Fixed sample. Relative years -4 and -3 are combined in the -3 estimate and relative years +3, +4, and +5 are combined in the +3 term.

that the raw home ownership rate of the treatment group gradually falls from about 80% to about 65% over the years following privatization. Some households who privatize decide to sell and return to rentership. Over the same period, the home ownership rate among the control group of households rises to just below 20%. With the uncertainty of the Stopplag decision 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 45% even four years after treatment. The right panel of Figure 1 confirms these persistent treatment effects after taking into account year and building fixed effects and after controlling for age, partnership , and education. For the fixed sample, the gap in home ownership rates remains at 65% three years or more after treatment. For both samples, we have a very large and persistent “first-stage” effect on home ownership.

The first four columns of Table 3 shows the estimated ITT effects in table format. For parsimony, the tables focus on the specification where we collapse all pre-treatment periods (except for the excluded RY-1) in a RY(pre) variable and all the post-treatment effects in the RY(post) variable. In addition to confirming the absence of a pre-trend, the first column shows a 75% point difference in the home ownership rate between the treatment and the control group, and relative to the household formation year, in the sample of All households. The treatment effect for the sample with fixed adult composition, reported in column (2), is 83% in the year of conversion. The treatment effect is persistent. The home ownership rate differences are 61% and 72%, for All and Fixed samples, respectively, in the average

Table 3: ITT estimation - Home Ownership and Demographics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Home Ownership				Number of adults				Number of Children			
Sample	All	Fixed	Stayers	Movers	All	Fixed	Stayers	Movers	All	Fixed	Stayers	Movers
RY(pre)	0.0140	0.0137	0.00689	0.0263**	0.0125	0.0207**	0.0125	0.0388**	-0.0108	-0.0222	-0.00725	-0.0372
	(1.51)	(1.48)	(0.72)	(2.06)	(0.78)	(2.22)	(1.26)	(2.69)	(-0.31)	(-0.81)	(-0.29)	(-0.72)
RY0	0.751***	0.827***	0.880***	0.734***	0.000940	0.0155	0.0314**	-0.0159	-0.0387	-0.0322	0.000437	-0.0895
	(20.70)	(20.88)	(24.42)	(11.23)	(0.06)	(1.43)	(2.34)	(-1.11)	(-1.08)	(-1.47)	(0.02)	(-1.60)
RY(post)	0.610***	0.721***	0.838***	0.466***	0.00774	0.0170*	0.0264**	-0.0128	-0.00591	0.00129	0.0198	-0.0595
	(24.87)	(32.72)	(30.38)	(12.87)	(0.34)	(1.75)	(2.38)	(-0.74)	(-0.09)	(0.04)	(0.52)	(-1.19)
PT-Mean	.03	.03	.02	.04	1.32	1.32	1.32	1.30	.69	.70	.71	.68
PT-SD	.19	.17	.16	.19	.46	.46	.46	.46	1.01	1.01	1.03	.97
N	18284	15076	10273	4803	18284	15076	10273	4803	18284	15076	10273	4803
R^2	0.422	0.535	0.672	0.395	0.105	0.121	0.136	0.156	0.104	0.169	0.184	0.190

Notes: t statistics in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.001$. Standard errors are clustered at the co-op 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 omitted relative year is the household formation year RY-1. The coefficients on the relative year dummies themselves are not reported. Building fixed effects and calendar year fixed effects are included but not reported. Age and Education are included as control variables in all columns, while columns (1)-(4) additionally control for partnership. 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.

post-privatization year.

5.2 Demographics

Next, we investigate whether home ownership affects family composition. Columns (5)-(8) of Table 3 have the number of adults in the household as the outcome variable and Columns (9)-(12) report on the number of children.[38] In the All sample (column 5), there are no significant differences in the number of adults between treatment and control groups before or after treatment. Home ownership does not cause treated households to divorce, singles to get married, or adult children to move out of the house at different rates than their counterparts in the control group. We confirm this by estimating separate regressions with the divorce rate and marriage rate as the dependent variable.[39] Home ownership does not affect the "stability" of the household.

In the Fixed sample, the adult composition of the household is fixed by construction. Therefore, the regression in column (6) only serves as a diagnostic on how the family *composition* of treatment and control groups fluctuates over time and shows that our sample composition remains well balanced throughout the full estimation window. Nevertheless, in

[38]For these two outcome variables only, we omit partnership as a control variable.
[39]These results are available upon request.

all subsequent regressions we will control for the partnership rate.

Columns (9)-(12) of Table 3 report the effect of conversion on the number of children in the household. We find no significant differences before treatment in the All or Fixed so that the parallel trends assumption is satisfied. Home ownership does not spur child birth. If anything, we find a small negative treatment effect of -.03 children in RY0, but the effect is too imprecisely estimated. The modest overall effects of home ownership on child birth could be related to the fact that the average age (of the oldest adult) in RY0 is 45. Many of our households are beyond prime child bearing years.

Combined, the results in columns (5)-(12) indicate that the overall family composition remains balanced throughout the experiment. They suggest that we can focus our discussion on the Fixed sample without loss.

6 Main Results on Consumption and Savings

Our main variables of interest are spending and savings. Two key benefits of home ownership that are often references are that home ownership induces households to save, and that the house is an important source of collateral that can be borrowed against to smooth consumption across states of the world. Investigating these benefits from home ownership requires high-quality household-level consumption data. Much of the literature lacks such data. Sometimes aggregate data is used instead of household-level data, household consumption is measured based on surveys, or approximated by car purchases or credit card spending.[40] We build high-quality consumption from administrative data, substantially extending the procedure outlined in Koijen, Van Nieuwerburgh and Vestman (2014), and relate that consumption to home ownership and housing wealth at the level of the household in the context of a quasi-experiment.[41]

[40]The only exception we are aware of is the *mint.com* data employed by Baker (2015). Koijen, Van Nieuwerburgh and Vestman (2014) discuss major issues with survey-based measures of consumption.

[41]In related work, Browning, Gørtz and Leth-Petersen (2013) impute consumption in Danish data and investigate the impact of shocks to house prices.

6.1 Initial Effect on Consumption and Saving

Table 4 displays the treatment effects on total consumption expenditures (column 1), the other components of the budget constraint (columns 2-5), and on total savings (column 6). The budget constraint (1) states that consumption equals income minus savings, where savings is defined as the change in financial plus the change in real estate wealth minus the change in household debt. An increase in the value of a household's assets that is not fully offset by an increase in liabilities or in current income leads to lower consumption. A decrease in household net worth (dissaving) generates an increase in consumption, absent a change in income. All results for consumption and its components are expressed per adult equivalent. The sample is the Fixed sample. The results for the All sample are very similar and are omitted for brevity. First, we find that consumption, savings, and their components all show parallel pre-trends.

Second, the initial treatment effect (in RY0) on consumption is negative and significant. The point estimate of -16,475 SEK represents a drop of 10.3% of pre-treatment average annual consumption. Since consumption is measured per adult equivalent, the total effect on household consumption for a family of four is 2.7 times greater, or about 5,000 USD. The raw data show a modest decline in consumption for the treatment group, but a substantial increase for the control group. Put differently, absent the home purchase, the treated would have spent like their peers in the control group, and their consumption would have been 16,500 SEK higher than we observed. Column (6) shows that the treated save 27,375 SEK more than the control group. This represents a nearly five-fold increase over the pre-treatment average savings level. Why does home ownership prompt an initial decline in spending and increase in savings?

Columns (2)-(5) present a four-way breakdown of the -16.5k SEK treatment effect on consumption in RY0. The fall in consumption results from a 337k SEK increase in debt, a 376k SEK increase in housing wealth, a 12k SEK decrease in financial wealth, and a treatment effect on disposable income of 10.9k SEK.[42] Combining the increase in housing wealth and debt, and assuming that the entire increase in debt is attributable to debt collateralized by

[42]The RY0 increase in housing wealth is measured as the conversion fee paid to the co-op. It is the book value of the co-op shares and the actual outlay of the household in RY0. Households only get credited with the market value of that real estate upon a future sale. This is to avoid mechanical valuation effects on consumption.

Table 4: ITT Estimation - Consumption and Savings

LHS var:	(1) Consumption	(2) Income	(3) dHousing	(4) dDebt	(5) dFin	(6) Savings
RY(pre)	-4281.1	233.2	861.3	-3735.6	-82.63	4514.3
	(-0.49)	(0.08)	(0.16)	(-0.63)	(-0.01)	(0.62)
RY0	-16475.2*	10899.8**	376403.5***	337065.5***	-11963.0**	27375.0**
	(-1.89)	(3.24)	(5.18)	(4.83)	(-2.51)	(3.15)
RY(post)	8075.8	1905.0	-14287.1	-7748.8	367.4	-6170.8
	(1.19)	(0.52)	(-1.66)	(-1.12)	(0.07)	(-1.29)
PT-Mean	160,517	166,894	1,865	4,867	9,380	6,378
PT-SD	117,627	85,380	49,841	70,086	77,498	92,889
N	13370	13370	13370	13370	13370	13370
R^2	0.0646	0.141	0.208	0.196	0.0124	0.0142

Notes: t statistics in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.001$. 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 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. Consumption and Savings are divided by the adult equivalent scale 1+ (Adults-1)*.7+Children*0.5. The sample is the Fixed sample of households with constant adult composition. Relative years -4 through -2 are collapsed into the RY(pre) term and relative years +1, ..., +5are collapsed in the RY(post) term.

real estate, the treatment effect on home equity is +39.2k SEK. To make this downpayment, households reduce their financial wealth (draw down bank accounts, sell stocks, mutual funds, and bonds) by 12k SEK. The 27.2k SEK difference between the increase in home equity and the decline in financial wealth equals the estimated increase in savings. The difference between the treatment effect on disposable income (10.8k) and that on savings (27.2k) accounts for the estimated effect on consumption (-16.5k). The treatment effects in RY0 are highly significant for all four consumption components, as well as for savings.

Importantly, the reduction in consumption in the conversion year is voluntary. Because converters were able to buy property at prices below the resale value –the windfall averaged about 400k SEK per adult equivalent– they could have obtained a much larger mortgage if they had wished. In particular, they could have easily borrowed the full amount of the increase in real estate wealth which equals the conversion fee. The reason is that banks would have been able to make a regular 80% LTV mortgage against the market value of the apartment, which amounts to a 100%+ LTV mortgage relative to the conversion fee. This would have eliminated the entire drop in consumption in RY0.[43] Instead, converters chose to limit the size of the mortgage and pay for the downpayment by reducing financial wealth and by

[43]This statement is not only true on average, but holds for *all* converters. We calculate the distribution of the ratio of conversion fee to market value and find that it lies between 0.3 and 0.7. Assuming an (overly conservative) LTV limit for Stockholm at the time of conversion of 0.8, all households could have financed all of the conversion fee with a personal mortgage.

reducing consumption. This behavior appears at odds with standard consumption smoothing motives. It may be consistent with a notion of leverage- or debt-aversion. Alternatively, the consumption drop could reflect expectations of high house price appreciation that justify the housing investment (positive downpayment). Households may believe that they can earn a much higher return on housing than on other financial assets, and so much so that it is worth temporarily cutting consumption.[44]

6.2 Effect on Income

The positive treatment effect on disposal income in the year of privatization is noteworthy (column (2) of Table 4). It represents about a 6.5% increase over average pre-treatment income. Appendix E.1 investigates the income increase further by studying *labor* income, which is by far the largest component of disposable income. We find an even larger 8.5% increase in labor income in the treatment year. We then decompose household labor income into labor income per working adult and the number of adults working in the household. The former increases by 8.5% while the latter increases by 2.1% in RY0. Both changes are statistically different from zero. While the intensive margin effect is large, the extensive margin effect is modest in size. The number of working adults in the households increases by 0.03 adults, on a baseline level of 1.3 working adults per household. Potential explanations for the increase in labor income and (hence in disposable income) are 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. The increased income effect is consistent with a debt-service induced increase in labor supply.

6.3 Subsequent Effect on Consumption and Saving

In the years after conversion, our main finding is a small positive treatment effect on consumption. Column (1) of Table 4 shows an average annual consumption expenditure response of 8,076 SEK in the four years after privatization. The increase represents 5% of average

[44]Research has similarly argued that U.S. households had high house price expectations in the years leading up to 2007 (Foote, Gerardi and Willen (2012), Kaplan, Mitman and Violante (2016)). Our sample period also covers the housing boom in Sweden which had many similarities with that in the U.S.

annual pre-treatment consumption. This amounts to increased spending of 2,400 USD per household per year. While non-trivial, the increase is too imprecisely estimated to deliver statistical significance.

After privatization, the treated households earn 2k SEK more and save 6k SEK less per year than the control group. The lower savings are the result of a relative decrease in home equity of 7k and a relative increase in financial wealth of 1k. The decline in home equity itself is accounted for by a relative decline in housing wealth exceeding the decline in debt. In the post-privatization period, some households in the control group also move into home ownership while some of the treated households move out of home ownership. This explains why the treated see smaller changes in housing wealth than the control group in this period, as well as smaller increases in debt. The control group catches up with the treatment group, and much of the *differential* increase in savings in the year of privatization is reversed in the years after privatization. The opposite is true for the consumption response: the treatment group consumes more than the control group post-privatization, reversing the initial relative consumption drop.

Evidence from car purchases in Appendix E.2 confirms the weak positive consumption response.

6.4 Heterogeneity between Stayers and Movers

The treatment effect on consumption and savings differs in important ways between those who stay in their privatized apartment (until the end of our sample in 2007 or for as long as we observe them) and those who move out (at some point between the treatment year and 2007 or when we last observe them). To explore this heterogeneity we split all household-year observations into one of two groups: those belonging to Stayers (2/3 of observations) and those belonging to Movers (1/3).[45] Below we explore alternative ways of defining the control group or splitting the sample.

Columns (3) and (4) of Table 3 shows that the initial home ownership effect is stronger for Stayers (88%) than for Movers (73%). Naturally, the effect is quasi-permanent for the Stayers, while it declines strongly for Movers. Nevertheless, the treated Movers are still 23% points

[45] In unreported analysis, we study the household characteristics listed in Table 2. They indicate that a good control group for the treated households who stay is households in the control group who stay. Similarly, we compare treated Movers to Movers in the control group.

more likely to own four years later, and 47% on average in the post-period, relative to the control group of Movers and relative to household formation year RY-1. Some movers in the treatment group sell their newly obtained apartment and revert to rentership (about 1/3 of the treated movers do) and some movers in the control group become home owners. Nevertheless, a large treatment effect remains several years later even among mobile households.

Table 5 shows the treatment effect on consumption, its four components, and savings. Panel A compares Stayers in the treatment group with Stayers in the control group, while Panel B does the same for the Movers. The initial decline in consumption is not that different for Stayers (-13.7k SEK) and Movers (-17.4k SEK). The treated Stayers take out smaller mortgages than the treated Movers and choose a larger home equity position (43k for Stayers versus 33k for Movers). The initial income effect is also much larger for treated Movers than it is for treated Stayers (16k versus 8k). This is directionally consistent with the Movers' larger mortgage debt, and our conjectured debt service-induced labor supply response.

Most interestingly, the post-privatization consumption response is essentially zero for the Stayers (+2.3k SEK per year) while it is large and statistically significant for the Movers (+25k SEK). The latter annual increase represents 15% of annual pre-treatment consumption. Put differently, the cumulative consumption response of 100k SEK for Movers in the four-years post-privatization is about ten times larger than the 10k SEK 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 than Stayers in the control group. Figure 2 visualizes the stark difference in post-privatization consumption responses for Stayers (left panel) and Movers (right panel).

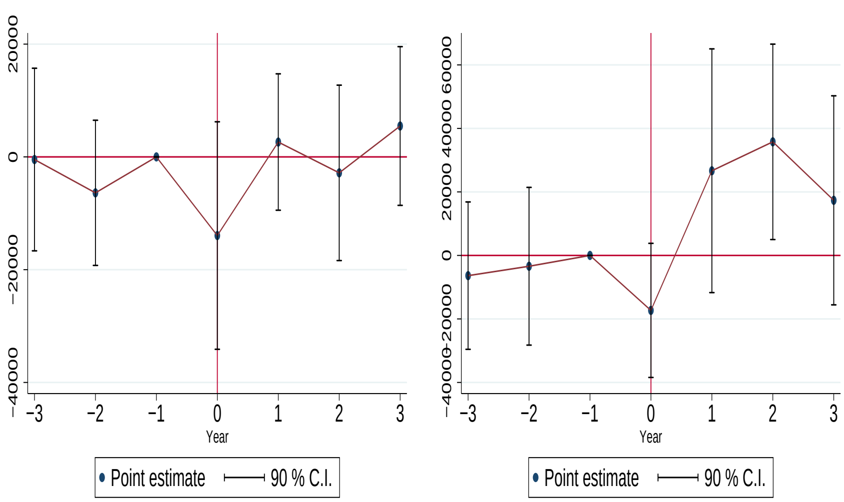
The zero consumption response for Stayers post-privatization arises because they reduce financial wealth to pay off their debt, and experience little change in housing wealth or income (Columns 2-4 of Table 5). This evidence suggests that, for the average Stayer, there is no pure housing wealth effect on spending. Stayers could have borrowed against their home equity, especially in light of the large windfall tied up in their property and in light of the ensuing property price appreciation they experienced. Home equity lines of credit were widely used in Sweden at the time. Yet, Stayers chose not to tap into their home equity. Not only did they leave the home equity wealth tied up in the house, they grew it by paying off their debt. Stayers behave like the stereotypical "forced saver," often associated with home ownership.

Table 5: Consumption and Savings - Stayers versus Movers

	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Stayers			
LHS var:	Consumption	Income	dHousing	dDebt	dFin	Savings
Pre	-3024.1	-4198.6	-4358.0	-7563.7	-4380.3	-1174.5
	(-0.36)	(-1.02)	(-0.83)	(-1.11)	(-0.78)	(-0.18)
RY0	-13663.2	7906.2**	372403.7***	329743.2***	-21091.1***	21569.4*
	(-1.12)	(2.52)	(4.89)	(4.46)	(-3.79)	(1.93)
Post	2338.4	3043.5	-3238.4	-14384.3**	-10440.9**	705.1
	(0.37)	(0.67)	(-0.55)	(-2.39)	(-2.64)	(0.17)
PT-Mean	158,565	165,613	671	2,914	9,291	7,048
PT-SD	112,600	81,059	29,623	52,388	73,746	86,163
N	9165	9165	9165	9165	9165	9165
R^2	0.0760	0.152	0.371	0.292	0.0196	0.0197
			Panel B: Movers			
LHS var:	Consumption	Income	dHousing	dDebt	dFin	Savings
Pre	-5258.5	7129.9	9029.1	2198.5	5557.7	12388.4
	(-0.40)	(1.46)	(1.06)	(0.23)	(0.41)	(0.87)
RY0	-17429.6	16272.8**	380240.7***	347653.5***	1115.0	33702.3**
	(-1.36)	(3.03)	(5.38)	(5.30)	(0.09)	(2.41)
RY(post)	24979.0*	-1955.6	-45804.3*	2495.7	21365.5**	-26934.5**
	(1.82)	(-0.34)	(-1.83)	(0.14)	(2.23)	(-2.45)
PT-Mean	164,479	169,495	4,287	8,833	9,562	5,016
PT-SD	127,159	93,508	75,754	96,374	84,624	105,240
N	4205	4205	4205	4205	4205	4205
R^2	0.0751	0.177	0.136	0.138	0.0263	0.0357

Notes: See table 4. Panel A is for the Stayer sample (the Stayers in treatment and in control groups), Panel B is for the Mover sample (the Movers in treatment and in control groups).

Figure 2: Consumption for Stayers and Movers



Left panel: dynamic treatment effect on annual consumption expenditures from the dynamic ITT difference-in-difference estimation for the Stayer sample. Right panel: same specification for the Mover sample.

In sharp contrast, treated Movers reduce housing wealth relative to the control group of Movers but they do not reduce debt. On average, they reduce home equity. The proceeds from (net) real estate sales go towards accumulating financial assets and towards boosting consumption. This suggests that home equity extraction does takes place, but only when the property is sold, and the gains are in liquid form. Realized, rather than unrealized, gains trigger spending.

Since the moving decision is endogenous, one may be worried that there are important differences between Stayers in the treatment group and Stayers in the control group, and similarly for the Movers. Such differences might affect the heterogeneous treatment estimates in Table 5. To investigate the robustness of the results, we conduct two additional exercises in Appendix E.3. First, we estimate a specification where we compare Stayers and Movers both to the same set of all households in the control group, in a one-pass regression. This amounts to simply splitting our Fixed sample estimates of the treatment effect into a component attributable to Stayers and a component attributable to Movers. Second, we instrument moving with variables that are pre-determined. We label households with high moving probability as Movers and those with low probability as Stayers, and repeat the one-pass estimation. For both exercises, we find the same result: Movers have much stronger post-privatization consumption responses than Stayers. Stayers behave like "forced savers."

6.5 Propensity to Consume Out of Housing Wealth

Our results shed light on the literature that studies marginal propensities of consumption (MPC) out of housing wealth. We have quasi-experimental variation in housing wealth which is very helpful in identifying the MPC. Our windfall is a one-time shock to housing wealth, akin to a one-time income shock. We define the MPC as the estimated treatment effect on annual consumption in the post privatization period for those who privatize divided by the average windfall. Both consumption and windfall are expressed per adult equivalent. We find a MPC of 2.1% for the Fixed sample. This estimate is on the low end of the estimated MPCs, obtained using different methodologies and in different contexts. It is close to the income response predicted by the permanent income hypothesis combined with a high degree of patience. At the same time, it is much lower than the roughly 20% estimates obtained from the literature that aims to explain the consumption response in the Great Recession (e.g., Mian, Rao and Sufi (2013) and Berger et al. (2015)).

6.6 Collateral Effect of Housing

One of the alleged benefits of home ownership is that housing is a collateral asset that households can draw upon in times of need. To get more directly at the use of the house as a collateral asset, we study 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%. We ask whether the 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. We estimate:

$$\begin{aligned} y_{it} &= \alpha + Convert_i \sum_k \delta_k RY_i(t=k) + Convert_i \sum_k \beta_k RY_i(t=k) Z_{it} \\ &+ \sum_k \gamma_k RY_i(t=k) + \sum_k \lambda_k RY_i(t=k) Z_{it} + Z_{it} + X_{it} + \psi_t + \omega_b + \varepsilon_{it}, \end{aligned} \tag{3}$$

If y_{it} is the consumption of household i at time t, then β_k measures the consumption of a household in the treatment sample that received a negative income shock in the current period. It can be compared to consumption response of a non-treated household to the same labor income shock, λ_k, and to the consumption of a treated household that did not receive the income shock (δ_k). We are mostly interested in labor income shocks that occur after privatization. Appendix E.4 contains the estimation results. We find parallel trends for all groups prior to treatment.

As a first sanity check of our empirical setting, we confirm that a large negative income shock leads to a significant reduction in consumption (the Z_{it} term). Households facing a negative income shock cut back their consumption by 35,571 SEK (t-stat of -6.39). All λ coefficients are insignificant which implies that the timing of the income shock is irrelevant for the control group. Tenants cut back their consumption due to a large income shock, in the year prior to privatization, the year of privatization, or the years following the privatization.

Results are remarkably different for home owners. If the income shock occurs in the post-privatization phase, home owners can use their house as collateral and smooth consumption considerable. The estimated coefficient for collateral effect $\beta(post)$ is 32,903 SEK (t-stat of 2.11). This implies that the collateral effect almost completely offsets the negative income shock effect.

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 it is randomly assigned. These point estimates also clarify that the modest positive consumption response post-privatization is largely driven by those home owners who received a negative income shock. Treated households not receiving the income shock display a consumption response which is not different from that of the control group.

Looking at the change in debt ($dDebt$ as left-hand side variable in (3)), we find that the treated who do not receive an income shock reduce debt by 10k SEK on average per year in the post period, while the treated that do receive an income shock increase debt by 21k SEK. The increase in debt does not stem from increased real estate purchases because real estate wealth actually falls. Renters again behave quite differently and reduce debt by 6k SEK. Thus, home owners use their housing equity to borrow more in the face of a large income shock. This allows them to offset the fall in income and smooth consumption.

We find a strong housing collateral effect both for Stayers and for Movers. Recalling the weak consumption response in the average post-privatization year for Stayers, we can conclude that the only time Stayers tap into their housing wealth is when they are hit with an adverse income shock. The consumption response for Stayers whose income did not fall is zero.[46]

7 Stock Market Participation

We saw differences between Movers and Stayers in terms of savings in the form of financial vs. housing wealth. Now, we turn to the effects of home ownership on the composition of the financial portfolio itself. We investigate how the privatization experiment affects the decision to participate in risky financial asset markets, and conditional on participation, how it affects the share of risky assets in the total financial wealth portfolio. Home ownership adds a large, idiosyncratic asset to the asset side and a large mortgage to the liability side of households'

[46]The collateral effect for Movers is present because many of the household-year observations pertain to pre-moving years, and because even after they move, 2/3 buy a new house.

Table 6: ITT Estimation - Risky Asset Market Participation and Risky Share

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Participation			**Cond. Risky Share**			**Risky Share**		
Samples	Fixed	Stayers	Movers	Fixed	Stayers	Movers	Fixed	Stayers	Movers
RY(pre)	-0.0221	-0.0280	-0.0149	0.0188	0.0299**	0.00216	0.00681	0.0127	-0.00611
	(-1.43)	(-1.39)	(-0.41)	(1.27)	(2.10)	(0.08)	(0.56)	(1.08)	(-0.28)
RY0	0.0229*	0.00579	0.0539**	0.0291*	0.0423**	-0.00482	0.0289**	0.0312**	0.0234
	(1.74)	(0.50)	(2.04)	(1.99)	(2.59)	(-0.21)	(2.87)	(2.98)	(1.54)
RY(post)	0.0371**	0.0237	0.0598**	0.0122	0.0204	-0.00235	0.0263**	0.0278**	0.0204
	(2.86)	(1.59)	(2.47)	(0.78)	(1.27)	(-0.07)	(2.84)	(2.79)	(1.09)
PT-Mean	.51	.51	.53	.39	.39	.38	.20	.20	.20
PT-SD	.49	.49	.49	.28	.28	.28	.28	.28	.28
N	15076	10273	4803	7728	5156	2572	15076	10273	4803
R^2	0.0915	0.110	0.106	0.0793	0.0978	0.134	0.0769	0.0992	0.0945

Notes: See Table 4. Participation is an indicator variable for whether the household has any stocks or mutual funds. Cond. Risky Share measures the ratio of the SEK holdings in stocks and mutual funds to the sum of SEK holdings in stocks, mutual funds, bonds, and bank accounts, conditional on participation in the risky asset market. Risky Share is measured the same way as Cond. Risky Share, but does not condition on participation. I.e., it includes the zeroes.

balance sheets.

We define risky assets to be direct holdings of stocks and indirect holdings through mutual funds (equity, bond, and mixed funds). The risky asset share is the ratio of stocks and mutual funds to the sum of stocks, mutual funds, money market funds, and bank accounts. Since we do not observe the composition of pension accounts, they are left out of the definition of both variables. We distinguish between the conditional risky share, which conditions on participation (strictly positive holdings of stocks or mutual funds) and the (unconditional) risky share which does not condition and includes the zeroes from non-participants. The pre-treatment mean of the stock market participation rate is 51%. The PT-mean of the conditional risky share is 39%, and the unconditional risky share averages to 20% pre-treatment. For brevity, we focus on the Fixed sample, and verify that the results for the All sample are similar.

Table 6 displays the ITT effects. In the Fixed sample (columns 1, 4, and 7), we find no statistically significant pre-trend differences for all three variables. The increase in stock market participation after treatment is economically meaningful and represents about a 7% increase from the average pre-treatment level. The effect is concentrated among the Movers for whom it represents a 10% increase.

Turning to column (4), we find an increase in the risky share, conditional on participation, of 3% points in RY0. It is significant at the 10% level and represents a 7% increase over the pre-treatment mean. The effect is short-lived. The increase in the conditional risk share is consistent with Chetty, Sándor and Szeidl (2016). In the context of a simple life-cycle consumption-savings model, they explain that an increase in housing wealth, holding fixed the size of the mortgage, should lead to an increase in risky share. Conversely, an increase in the mortgage, holding fixed home equity, should lead to a reduction in the risky share. In our experiment both mortgage and home equity increase. Our results in columns (4)-(6) indicate that the home equity effect dominates, and leads to an increase in risky share. In contrast to the extensive margin, the intensive margin effect is concentrated among the Stayers. Movers see a larger increase in leverage and a smaller increase in home equity, as we pointed out above, which is consistent with a weaker risky share response. Our paper confirms the Chetty et al. findings using quasi-experimental evidence from Sweden. It also extends their findings by investigating the extensive margin. We find the latter effects to be more persistent.

In columns (7)-(9), we combine the extensive and intensive margin effects by studying the (unconditional) risky share. We find a large and persistent treatment effect which remains significant after treatment. The point estimate of 3% represents a 15% increase over the pre-treatment average. Total effects, as measured by the risky share, are stronger for Stayers than for Movers.

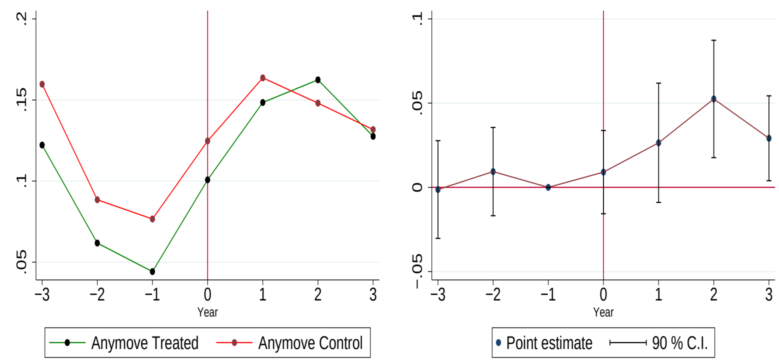
In sum, these results are consistent with the notion that home ownership associated with the accumulation of home equity (which is stronger for Stayers than for Movers) leads to increased portfolio allocations to risky assets, and may strengthen that wealth accumulation.

8 Mobility

Does home ownership reduce geographic mobility? We study three different definitions of geographic mobility, based on changes in exact address (Anymove), parish of residence (Parishmove), and municipality of residence (Municipmove). We find that our treatment increases both geographic and economic mobility.

Columns (1)-(6) of Table 7 report the parsimonious ITT estimation results for the All and the Fixed samples. Starting with the broadest measure of mobility in columns (1) and (2), we find no significant pre-trends. The left panel of Figure 3 confirms the parallel pre-trends in the raw data. In the year of treatment, RY0, there is slightly lower mobility for the treated,

Figure 3: Moving Rates



Left panel: annual moving rate based on changes in address. Raw mobility rates for treatment and control groups; All sample. Right panel: Dynamic difference-in-difference estimation for annual moving rate based on changes in address. The sample is the Fixed sample.

Table 7: ITT estimation - Mobility Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Anymove		Parishmove		Municipmove		Moving Up P		Moving Up Y	
Samples	All	Fixed	All	Fixed	All	Fixed	All	Fixed	All	Fixed
RY(pre)	0.00166	0.0119	0.00261	0.0149	0.00467	0.0107*	0.000395	0.00231	-0.00123	0.00176
	(0.11)	(0.80)	(0.25)	(1.65)	(0.67)	(1.81)	(0.06)	(0.39)	(-0.15)	(0.24)
RY0	0.00883	-0.00883	0.00134	-0.00419	-0.000785	-0.000274	0.0109	0.00761	-0.00161	0.00549
	(0.60)	(-0.99)	(0.08)	(-0.40)	(-0.09)	(-0.04)	(1.02)	(0.99)	(-0.15)	(0.77)
RY(post)	0.0344**	0.0562***	0.0326**	0.0502***	0.0200**	0.0361***	0.0345**	0.0377**	0.0311**	0.0388***
	(2.46)	(3.93)	(2.82)	(4.31)	(2.76)	(4.40)	(3.14)	(3.37)	(3.00)	(3.66)
PT-Mean	.11	.10	.04	.04	.02	.01	.02	.02	.02	.02
PT-SD	.31	.30	.21	.20	.14	.13	.14	.14	.14	.14
N	18284	15076	18284	15076	18284	15076	18284	15076	18284	15076
R^2	0.0749	0.0434	0.0695	0.0391	0.0396	0.0244	0.0263	0.0194	0.0298	0.0234

at least in the Fixed sample. This seems natural if the treated are preoccupied with the conversion process (obtaining a mortgage, etc.). The effect is not different from zero. In contrast, we find large and significant positive effects of treatment on mobility in the years following conversion, both in the All and in the Fixed sample. Returning to the table, on a baseline moving rate of 10% points per year in the pre-treatment years, the moving rate for the Fixed sample increases by 56% (5.6% points) for the treated in the post-period. The effects are measured precisely. The right panel of Figure 3 shows the results for the Fixed sample graphically in the fully dynamic specification. It confirms the long-lived effects of increased mobility.

Columns (3) and (4) show large and significant effects on inter-parish (similar to zip code) mobility in the post-treatment years. The 3.3% and 5.0% points higher moving rates are economically large since the average inter-parish moving rate in the pre-treatment years is only 4% points per year. The effects on inter-municipality moving rates in Columns (5) and (6) are larger still, at least in light of the much lower 1-2% baseline moving rate across municipalities. We estimate a doubling in mobility rates for the All and a tripling for the Fixed sample.

To understand these results better, it is informative to examine the pattern in raw mobility rates in Figure 3. It shows high mobility rates for both the treated and control groups in the period before the conversion process was set in motion (years -4 and -3). As the privatization decision approaches, mobility rates start to fall. This anticipation effect occurs in parallel for control and treatment groups. When the privatization decision is made in RY0, moving rates increase for both the treatment and control group, explaining the lack of treatment effect in RY0. Both groups' mobility rates return to the levels observed before the conversion was on the horizon and then decline two or more years after the decision. However, the decline is smaller for the treated than for the control group, explaining the large treatment effect we find in the post-period.

What this graph makes clear is that, notwithstanding the specific institutional features of the Stockholm rental market, there is high mobility among renters both before and after the conversion.[47] As mentioned earlier (footnote 20), there exists a fairly liquid market for moving within the municipal rental system. And owners of course face moving costs as well, such as fees for realtors and mortgage brokers. Hence, our result that home ownership persistently improves mobility is not due to the low liquidity of the Stockholm rental market.

The increased mobility effect associated with ownership matters because it may improve the spatial allocation of labor, and ultimately the potential output for the economy. These results are surprising. One may have expected the opposite effect: becoming a home owner makes one less likely to move, a form of "housing lock" effect. Below, we investigate how the treatment effect on mobility varies across different levels of the windfall received upon conversion.

Having established higher inter-parish mobility rates post-conversion, we now study the

[47]We have studied mobility rates among the entire population of Stockholm and even of Sweden and confirm that renters have similar mobility rates than owners, even after controlling for demographics, income, and wealth.

destination of the movers. We construct an indicator variable, *Moving Up P*, that is one when a household moves to a parish that has higher average real estate prices than the parish of origin. Similarly, we define an indicator variable, *Moving Up Y*, that is one if the household moves to a parish with a higher average disposable income. Columns (7)-(10) of Table 7 show that treatment increases upward economic mobility. The probability that a household moves to a parish with higher house prices (income) post-privatization is 3.5-3.8% (3.1-3.9%) points higher for treatment than for control. This is a large economic effect in light of the pre-treatment average upward mobility rate of 2% points per year.[48] In other words, the privatization process helps households climb up the property ladder. Once converters own their apartment, they can sell it and use the proceeds to make a downpayment on a new property elsewhere. We find that they take advantage of this opportunity to move to better areas. The windfall associated with the initial conversion obviously boosts this process. So do subsequent capital gains; our sample period was one of rising house prices in the Stockholm area.

9 Heterogeneous Treatment Effects

This section explores how our ITT effects differ across groups of households that differ by the size of the windfall, age, labor income, and financial wealth.

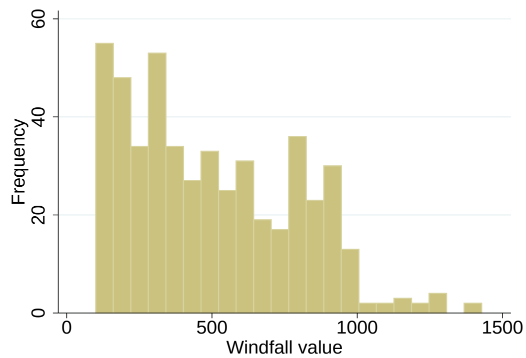
9.1 By Windfall

We measure the windfall for a given household in a given building as the difference between the market value of the apartment in RY0 and the conversion fee paid by the converting household. The market value in RY0 is computed from sales transactions that take place in that building. We apply the median price per square meter across those sales and multiply it by the square meters of that household's apartment to obtain the market value.[49] Within

[48] In unreported results, we find similar effects for moving to higher-house-price and higher-income municipalities rather than parishes. We also find that the probability of moving to *lower* price or income parishes is lower for treatment group, relative to the control group and relative to the pre-treatment period.

[49] Across the 13 treated buildings, we have 5 with at least one sale in RY0, 5 with at least one sale in RY+1, 2 with at least one sale in RY+2, and 1 building with at least one sale in RY+3. If we have no sales in RY0, we take the median transacted price per square meter in RY+a and deflate it by the ratio of the parish-level real estate price index in RY+a and RY0.

Figure 4: Windfall from conversion



The figure plots the distribution of the windfall upon conversion (in RY0) for all 493 households who converted. Measured in 2007 kSEK. Windfall in the graph is measured per adult equivalent.

a building, the size of the windfall grows with the size of the apartment. Figure 4 shows substantial cross-sectional variation in the size of the windfall.

To study how treatment effects depend on the size of the windfall, we group each household in one of five groups. Groups 1 through 4 are the quartiles of the windfall distribution. Since all of our outcome variables are measured per adult equivalent, we also classify households in groups based on windfall per adult equivalent. The mean of that scaled windfall distribution is 501k SEK. The 25^{th} percentile of the scaled windfall distribution is 249k, the 50^{th} percentile is 446k, and the 75^{th} percentile is 740k SEK. Group 0 consists of the residual tenants who are treated by our definition of treatment, but do not receive any windfall. There are 40 such households in the residual tenant group and about 123 households in each windfall quintile. Let $WF_i(n) = 1$ if household i has an initial windfall in group n, for $n = 0, 1, \cdots, 4$. The households in the control group are not included in any of the bins, and of course have a windfall of zero. We estimate the following piecewise-linear specification:

$$y_{it} = \alpha + Convert_i \sum_k \sum_n \delta_{k,n} RY_i(t=k) WF_i(n) + \sum_k \gamma_k RY_i(t=k) + WF_i(n) + X_{it} + \psi_t + \omega_b + \varepsilon_{it}, \quad (4)$$

Essentially, we estimate dynamic ITT effects $\{\delta_{k,n}\}$ for each windfall group.[50]

Table 8 summarizes our main findings by windfall group for key outcome variables. It reports the point estimates and t-statistics in $RY0$ and in the years after privatization $RY(post)$

[50]As in the main specification, we drop the terms in $RY(-1)$ so that all treatment effects (for each windfall group) are to be interpreted as differences relative to the household formation year RY(-1) and relative to the control group. Since the control group has zero windfall, every treatment group in a given windfall bin is compared to the same control group. For brevity, we focus on the parsimonious specification where we collapse pre and post RY variables.

Table 8: ITT Estimation - By Windfall Groups

Windfall Bins	1-250k	250k-445k	445k-740k	>740k
	Home Ownership			
RY0	0.867***	0.901***	0.951***	0.929***
	(47.54)	(37.92)	(47.19)	(43.64)
RY(post)	0.809***	0.785***	0.794***	0.754***
	(21.50)	(23.73)	(24.07)	(21.81)
	Consumption			
RY0	2831.5	-14752.5	321.8	-63884.6***
	(0.42)	(-1.24)	(0.02)	(-4.21)
RY(post)	30743.5***	8517.3	18286.6*	-12694.1
	(4.79)	(1.31)	(1.69)	(-0.73)
	Savings			
RY0	4276.7	26577.3*	10232.4	75729.5***
	(0.61)	(1.91)	(0.55)	(6.17)
RY(post)	-11979.5**	-10713.5*	-18525.1	-6076.3
	(-2.30)	(-1.97)	(-1.68)	(-0.51)
	MPC out of Housing Wealth			
MPC	19.40%	2.52%	3.23%	-1.44%
	Household Labor Income			
RY0	9295.6**	22256.3**	9867.3*	21129.0**
	(2.06)	(3.17)	(1.70)	(3.32)
RY(post)	17970.9*	-693.7	7952.9	-19763.0
	(1.97)	(-0.06)	(0.79)	(-0.68)
	Stock Market Participation			
RY0	0.0224	-0.00425	0.0435**	0.0430
	(1.02)	(-0.22)	(2.46)	(1.00)
RY(post)	0.0307	0.0645**	0.0526**	-0.0322
	(1.42)	(3.36)	(2.25)	(-1.32)
	Anymove			
RY0	-0.0191	0.00318	-0.00114	-0.0257*
	(-1.47)	(0.18)	(-0.11)	(-1.76)
RY(post)	0.0382**	0.0636**	0.0808***	0.0520**
	(2.14)	(2.67)	(5.02)	(2.10)

Notes: See Table 4. Treatment effects are estimated by windfall bin according to equation (4).

for households in the four bins with positive windfall. The windfall bounds for each bin are listed in the first row. The first panel shows that take-up of the privatization option was very high (87-93%) in all windfall bins. The largest windfall-group sees the largest initial home ownership rate and the largest decline post-privatization, reflecting the higher incentive to sell and liquefy the illiquid windfall.

The initial drop in spending and initial increase in savings is most pronounced for the largest windfall group. This group makes a much larger downpayment than any of the other groups and has the lowest leverage (mortgage debt-to-conversion fee ratio). The lower windfall groups show a smaller initial consumption decline or no decline at all. Post-privatization, the consumption increase is largest and most significant for the smallest windfall group. The lower windfall group contains more lower-income and lower-wealth households who have a higher propensity to consume out of the windfall. Indeed, this group has a MPC out of the windfall of 19.4%. The low-windfall group shows economically meaningful spending out of home equity, unlike the average household in the treatment group. The MPC is strongly declining in the size of the windfall. It is large for the lowest quartile of the windfall distribution, modestly positive at 2.5-3% for the middle half of the windfall distribution, and turns negative for the largest 25% of the windfall distribution. The savings effects mirror the consumption effect.

The fifth panel shows the results for total household labor income. While the initial effect is largest for the high-windfall group, increased labor income effect is found for all groups. The Post effect, and also the average effects over all years including RY0, is largest for the lowest-windfall group. This is consistent with the lower windfall groups taking on more housing leverage, and the higher debt service leading to higher long-term labor supply. The sixth panel shows that the stock market participation effects are concentrated in the middle windfall groups. The last panel shows strong mobility effects in all windfall groups post-privatization.

Appendix E.5 presents the results from a specification that imposes linearity-in-windfall on the treatment effects. The findings are consistent with the bin analysis. With the exception of the initial effect on consumption and savings, the "intercept" effects are large and significant while the "slope" effects often are not. In particular, we do not find a differential response on post-privatization consumption by windfall. The same is true for labor income, stock market participation, and mobility. The evidence is consistent with our results being mostly about the "pure home ownership" effect, rather than being driven by the size of the windfall.

9.2 By Age, Labor Income, and Financial Wealth

Similarly, we explore how our treatment effects differ by age, labor income, and financial wealth. We employ a piecewise-linear specification based on splitting the sample into quartiles. The results are in Appendix E.5.[51] The main take-away from this exercise is that our main findings hold across age, income, and financial wealth groups. Of course, we find some heterogeneity in treatment effects. The initial consumption decline and initial increase in savings are larger for the youngest, the low-income, and the lowest financial wealth group. The subsequent consumption increase is largest for the youngest as well as the 45-53 year

[51] In unreported results, we find the results to be robust to a linear-in-characteristics specification as well.

olds, and for the third labor income quartile. The youngest group has a higher MPC than the oldest group. The same is true for the lowest income and lowest financial wealth groups versus the highest income and financial wealth groups. These results are consistent with the intuition that the young, low income, low wealth households have the highest marginal utility of consumption.

10 Treatment Effect on the Treated

Our discussion thus far has focused on the intention-to-treat (ITT) estimation. Since not all households who are given the opportunity to move to home ownership actually take up the offer (and remain as residual tenants), the ITT estimates δ underestimate the causal effect of actually converting and becoming home owner. We now estimate the impact of conversion –the impact of "treatment on the treated" (TOT)– by instrumenting for conversion take-up with treatment assignment indicators as in e.g. Chetty, Hendren and Katz (2016). Formally:

$$y_{it} = \alpha_T + TakeConv_i \sum_k \delta_k^{TOT} RY_i(t=k) + \sum_k \gamma_k RY_i(t=k) + X_{it} + \psi_t + \omega_b + \varepsilon_{it} \tag{5}$$

where $TakeConv_i$ is an indicator that is one if the household actually converts and becomes an owner. Since $TakeConv$ is an endogenous variable, we instrument for it using the randomly assigned treatment group indicator $Convert$ and estimate (5) using two-stage least squares. Under the assumption that conversion offers only affect outcomes through the actual use of the conversion option, δ^{TOT} can be interpreted as the causal effect of exercising the conversion option and becoming home owner (Angrist, Imbens and Rubin (1996)). All TOT point estimates increase by about 10% relative to the ITT estimates, consistent with the fact that about 10% of all households that were given the opportunity to privatize did not do so. The statistical significance of the results is not affected. Appendix E.6 presents our main consumption table for the TOT estimation.

11 Conclusion

Our paper exploits a quasi-experimental setting in Sweden where one group of tenants were randomly allowed to buy the apartment they had been renting while another group was prevented from doing so. The two groups of households and buildings were similar in terms of their characteristics and these characteristics evolved similarly before the conversion. Over 90% of households given the chance to buy their apartment chose to do so. Even four years later, the experiment caused a large difference in home ownership rates.

We find that new home owners cut consumption in the year of their home purchase. We find mildly positive effects on consumption in the years following the home purchase. Households who do not sell their home show weak consumption responses. They do not use their newly gained home equity as a piggy bank but rather pay off their mortgage. Only when faced with a severely negative labor income shock do they tap into their housing collateral. Home owners who sell and move, in contrast, increase spending considerably even absent an income shock. Consumption responses seem to require the monetization of illiquid housing wealth. We estimate a marginal propensity to consume out of the housing wealth windfall of 2.1%. It is an order of magnitude smaller for stayers than for movers. Home ownership temporarily induces higher labor supply. It also spurs mobility, giving households the opportunity to move to better neighborhoods.

In follow-up work we plan to study outcome variables relating to educational achievement of the children of treated households. We also plan to study social outcome variables such as measures of community and political engagement and civility, school quality, and crime.

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

P. Sodini, S. Van Nieuwerburgh, R. Vestman, U. von Lillienfeld

A 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).

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.

B 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).

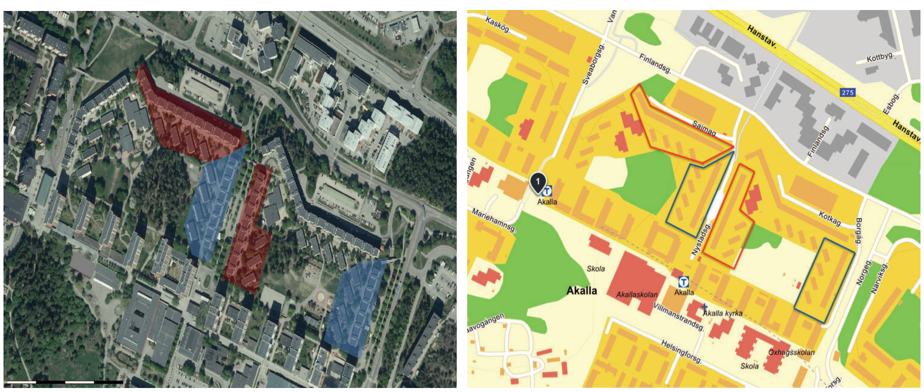

Figure A1: Akalla Complex

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.

Figure A2 plots all 38 co-op attempts in our Stopplag sample on a map of greater Stockholm. It shows that there is no systematic pattern in the geographic distribution of approved versus denied attempts. A detailed reading of the County Board minutes reveals that denials arose whenever the municipal landlords would be left with too few units of a particular type in a specific geographic area. More often that not, the apartment type in question would be only a small part of the co-op under review. For example, a 100 unit co-op building may have 5 studio apartments. If municipal landlords own too few other studio apartments in that neighborhood, the County Board would deny the privatization.

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).

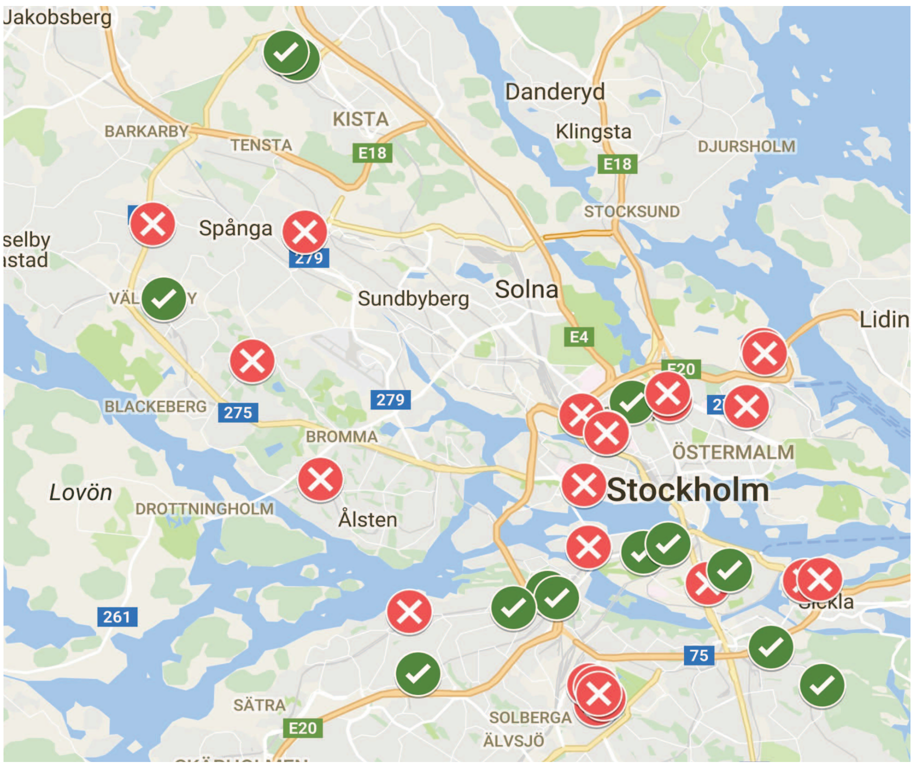

Figure A2: All Stopplagen Co-op Privatization Attempts

The dots with a green check mark are approved privatization attempts in our Stopplagen sample while the circles with red crosses are denied attempts.

C Appendix on the windfall from converting

A current renter in a municipally-owned building pays an annual rent r_t^s per square meter. Denote the present value of these rents by:

$$\Pi[r_t^s] = \sum_{i=1}^{\infty} \frac{r_{t+i}^s(1+g)^i}{(1+R)^i} = \frac{r_t^s(1+g)}{R-g},$$

where $\Pi(\cdot)$ denotes the present value operator. We assume a constant discount rate $R > 0$ and a constant growth rate in rents $g > 0$, as well as $R > g$. The discount rate reflects the cost of capital to the municipal landlord. Operating the building (irrespective of ownership) costs c_t per square meter. This cost includes all operating expenses, the ground lease, and taxes. These costs are assumed to grow at the same rate g as rents. The (accounting) value to the municipal landlord of the building *per square meter* is:

$$V_t^s = \Pi[r_t^s] - \Pi[c_t] = \frac{(r_t^s - c_t)(1+g)}{R-g}.$$

Let S be the total number of square meters of the entire building. Thus the value of the building to the landlord is SV^s.

C.1 Conversion from the perspective of the co-op

To understand the value transfer from privatization, it is useful to progress in four steps.

C.1.1 No debt, full conversion, no other rental income

First, assume that all tenants participate in the co-op conversion and that the building has no commercial space and hence no other revenues from residential nor commercial rent.

The building After conversion, the co-op must pay for the operational costs. We assume that there is no gain in operational efficiency from private ownership but also no increase in costs (such as from more high-end amenities or renovations of common areas). The value of the building equals the present discounted value of the *shadow market* rent minus the PDV of the operational costs. Thus the value in private hands per square meter is:

$$V_t^m = \Pi[r_t^m] - \Pi[c_t],$$

where r_t^m is the shadow market rent. We call it the shadow market rent since there is real market rent. Private landlords must charge the same rent as the municipal landlords for the same type of unit, of the same quality, in the same location. The capitalized shadow market rent, then, reflects the value of the unit under ownership rather than rentership. We assume that shadow market rents grow at the same rate as the regulated rents, but can differ in the level: $r_t^m \geq r_t^s$. If the co-op is able to buy the building from the municipal landlord for V_t^s per square meter, it receives a windfall of

$$W_t = V_t^m - V_t^s = \Pi[r_t^m] - \Pi[r_t^s]$$

per square meter.

The co-op Because the co-op takes on no debt, the total market value of the building equals the total market value of the co-op shares (equity). As a matter of cash flow accounting, the conversion price (inlag) per square meter that is paid by the initial co-op owners, X, equals the price asked by the landlord:

$$X_t = V_t^s$$

The co-op pays the operating expenses via co-op fees (avgift), which we denote by f_t:

$$f_t = c_t$$

Normalize the total number of co-op shares to the total number of square meters in the entire building S. The market value of the building is SV_t^m. The total windfall from conversion is SW_t. If tenant i occupies an apartment of x_i square meters, her number of shares is: $s_i = x_i$, where $\sum_i s_i = S$. The value of her co-op shares is $s_i V_t^m$. Her windfall from conversion is $s_i W_t$.

The market price of one share (in this case one square meter) in the co-op is:

$$V_t^e = V_t^m = \Pi[r_t^m] - \Pi[f_t] = W_t + X_t - \Pi[f_t]$$

C.1.2 No debt, full conversion, other rental income

Second, suppose that the building has a fraction $1-\alpha$ of total square meters S devoted to commercial space. This area is rented out for an annual rent of r_t^c per square meter. For simplicity, assume that the operating expenses (including taxes) are the same per square meter for residential and commercial, irrespective of ownership. Assume that both the municipal authority and the co-op can charge the same commercial rent. That is, commercial rents do not change upon transfer of ownership.

The building The value of the building in private hands (market value) is now:

$$V_t^m = \alpha\Pi[r_t^m] + (1-\alpha)\Pi[r_t^c] - \Pi[c_t],$$

The windfall is

$$W_t = V_t^m - V_t^s = \alpha\left(\Pi[r_t^m] - \Pi[r_t^s]\right),$$

which is lower than without commercial real estate. Intuitively, the overall windfall from converting the building is lower since part of the building (the commercial part) does not participate in converting, leaving less upside from conversion (only the residential part).

The co-op Since the co-op has no debt, the total market value of the co-op's equity equals the total market value of the building. The market value of the building is SV_t^m. The total windfall from conversion is SW_t. The co-op uses the conversion fee (inlag) to pay for the building: $X = V_t^s$ per square meter.

To calculate the market value of an individual tenant i's stake in the co-op, start by calculating her shares in the co-op. If i occupies an apartment of x_i square meters, her number of shares is:

$$s_i = \frac{x_i}{\alpha} > x_i.$$

Note that we still have $\sum_i s_i = S$. Basically, the commercial square meters are proportionately reallocated across all residential tenants so that it is as if each tenant owns more square meters. The total value of i's share in the co-op is the effective number of square meters times the market value per square meter $s_i V_t^m = x_i V_t^m/\alpha$. The value of her windfall is $s_i W$. Note that the SEK windfall for tenant i is identical in the case with commercial rent and without. The conversion fee for tenant i is $s_i X = s_i V_t^s > X$.

The annual fee (avgift), f_t, for the co-op owners is now:

$$\alpha f_t + (1-\alpha) r_t^c = c_t \Rightarrow f_t = \frac{c_t - (1-\alpha) r_t^c}{\alpha}$$

per square meter. The operational expenses increase because there are fewer owners contributing (division of c_t by α), but that is more than offset by the rental income stream from the commercial space.

Thus, the presence of commercial space lowers the yearly co-op fees, raises the initial conversion fee, and keeps the windfall unchanged.

C.1.3 No debt, partial conversion, other rental income

As before, suppose that the co-op has a fraction $1-\alpha$ of square meters devoted to commercial space. The assumptions on the commercial space are the same as above. Now assume that only a fraction β of the residential square meters participates in the co-op conversion. The remainder fraction continues to pay the same subsidized rent as before, but now to the co-op owners.

The building The value of the building in private hands is now:

$$V_t^m = \alpha\beta \Pi[r_t^m] + \alpha(1-\beta)\Pi[r_t^s] + (1-\alpha)\Pi[r_t^c] - \Pi[c_t],$$

The windfall is

$$W_t = V_t^m - V_t^s = \alpha\beta \left(\Pi[r_t^m] - \Pi[r_t^s]\right)$$

The non-participation in the conversion of some residential tenants further shrinks the overall value creation from privatization.

The co-op Since the co-op has no debt, the total market value of the co-op's equity equals the total market value of the building. The market value of the building is SV_t^m. The total windfall from conversion is SW_t. The co-op uses the conversion (inlag) to pay for the building: $X = V_t^s$ per square meter.

To calculate the market value of an individual participating tenant i's stake in the co-op, start by calculating her shares in the co-op. If i occupies an apartment of x_i square meters, her number of shares is:

$$s_i = \frac{x_i}{\alpha\beta} > x_i.$$

Note that we still have $\sum_i s_i = S$. Basically, the commercial square meters and the residential square meters of non-participants are proportionately reallocated across all participating tenants so that it is as if each participating tenant owns more square meters. The total value of i's share in the co-op is the effective number of square meters times the market value per square meter $s_i V_t^m = x_i V_t^m/\alpha\beta$. The value of her windfall is $s_i W$. Note that the SEK windfall for tenant i is identical to the two previous cases. The conversion fee for tenant i is $s_i X = s_i V_t^s > X$. The conversion fee per occupied residential square meter increases for owners since they alone (a fraction $\alpha\beta$ of the building) must raise the amount the municipal landlord asks for the building.

The annual fees (avgift), f_t, for the co-op owners are now:

$$\alpha\beta f_t + \alpha(1-\beta)r_t^s + (1-\alpha)r_t^c = c_t \Rightarrow f_t = \frac{c_t - (1-\alpha)r_t^c - \alpha(1-\beta)r_t^s}{\alpha\beta}$$

per square meter. Let $r_t^c = \phi r_t^s$. Then we can write:

$$f_t = \frac{c_t - [1-(\phi+\beta-1)\alpha]r_t^s}{\alpha\beta}.$$

C.1.4 Debt, partial conversion, other rental income

Now consider the most realistic case where a fraction $1-\alpha$ of square meters is devoted to commercial space, a fraction $\alpha(1-\beta)$ to non-participating tenants, and a fraction $\alpha\beta$ to participating tenants. The co-op issues debt to partially pay for the high conversion fee we saw in the previous case.

The building The value of the building in private hands is the same as in the previous example:

$$V_t^m = \alpha\beta\Pi[r_t^m] + \alpha(1-\beta)\Pi[r_t^s] + (1-\alpha)\Pi[r_t^c] - \Pi[c_t],$$

as is the windfall:

$$W_t = V_t^m - V_t^s = \alpha\beta\left(\Pi[r_t^m] - \Pi[r_t^s]\right).$$

The total windfall from conversion is SW_t.

The co-op The co-op now issues debt so that the total market value of the co-op's equity plus the market value of the co-ops debt equals the total market value of the building. Assume that the co-op takes out a T-year fixed rate mortgage with annual debt service d_t per square meter. Express the yearly co-op fee (avgift) that the co-op owners pay as a fraction χ of the subsidized rent they paid prior to conversion: $f_t = \chi r_t^s$. Then the debt service equals:

$$\begin{aligned} c_t + d_t &= \alpha\beta\chi r_t^s + \alpha(1-\beta)r_t^s + (1-\alpha)r_t^c \\ d_t &= [\phi + \alpha(1-\phi) - \alpha\beta(1-\chi)]r_t^s - c_t \end{aligned}$$

where the second equality follows under the assumption that $r_t^c = \phi r_t^s$. The market value of the debt D_t that can be raised with an annual fixed rate mortgage payment of d_t is given by the annuity formula:

$$D_t = \frac{d_t}{R^m}\left[1 - \frac{1}{(1+R^m)^T}\right]$$

where the annual mortgage rate is R^m. Note that because the debt is a level payment while the costs and rental revenues grow, the annual co-op fee will grow at a slower rate than costs and rental revenues until the debt is paid off. It will then fall discretely. It will grow at rate g from that point forward.

Given the market value of the building is V_t^m, the value of the co-op's equity is $V_t^e = V_t^m - D_t$ per square meter.

The co-op uses the conversion fee (inlag) as well as the debt it raised to pay for the building:

$X = V_t^s - D_t$ per square meter.

If i occupies an apartment of x_i square meters, her number of shares is as before:

$$s_i = \frac{x_i}{\alpha\beta}.$$

The total value of i's share in the co-op is the effective number of square meters times the market value of the co-op per square meter: $s_i V_t^e = s_i(V_t^m - D_t)$. Owner i's conversion fee is $s_i X$. Her annual fees (avgift) are equal to a fraction of the subsidized rent by assumption:

$$f_t = \chi r_t^s = \frac{c_t + d_t - [\alpha(1-\beta) + (1-\alpha)\phi] r_t^s}{1 - \alpha\beta}.$$

The presence of debt does not affect the economic value of the windfall from conversion. That is still W_t, and owner i's share of that is still $s_i W$. However, the presence of co-op debt means that the co-op owners had to pay only $s_i X$ to acquire their shares. Define the profit from participating in the co-op at X and immediately selling the co-op shares for V_t^e:

$$\Pi_t = V_t^e - X = V_t^m - D_t - (V_t^s - D_t) = V_t^m - V_t^s = W_t$$

This profit/windfall is invariant to the amount of leverage.

C.2 The perspective of the household

When thinking about participating in the co-op conversion, the household must trade off the utility of staying in the subsidized rental apartment with the utility from converting. These utilities depend on the horizon of occupancy in both scenarios, and these may be different. That is, the length of stay is a choice variable. For example, a young household may want to convert because she plans to sell the apartment soon and use the proceeds as a downpayment for a larger dwelling, possibly in a better neighborhood. She moves up on the housing ladder. An older tenant may decide to stay put in the apartment because the (implicit) cost of moving is too high relative to the benefit. By revealed preference, those households who convert must be better off converting (in utility terms) than renting, and vice versa for those who do not convert. In sum, the decision to rent or convert depends on household characteristics (age, income, wealth), on preference parameters (patience, risk aversion, jolt from owning, and bequest motive), and on expectations about future house price growth, financial asset returns, and income growth. Absent a structural model, it is impossible to due full justice to the complexity of this choice and to the heterogeneity driving it.

Nevertheless, if we are willing to make additional assumptions, we can compare the costs of converting to the cost of renting and relate that cost difference to the windfall discussed above. These assumptions are risk neutrality, frictionless financing, known length of stay, no uncertainty elsewhere.

C.2.1 Cost of renting

The cost of renting to a household depends on her length of stay $\hat{T}$ and her discount rate $\hat{R}$. Denote the present value of the cost of renting per square meter by Π_i^{rent}:

$$\Pi_i^{rent}(\hat{T}) = \sum_{j=1}^{\hat{T}} \frac{r_{t+j}^s (1+g)^j}{(1+\hat{R})^j} = \frac{r_t^s (1+g)}{\hat{R}-g} \left[1 - \left(\frac{1+g}{1+\hat{R}} \right)^{\hat{T}+1} \right].$$

For a household with an apartment of size x_i, the present value of the cost is $x_i \Pi_i^{rent}$.

C.2.2 Cost of converting

The cost of converting is the discounted value of the payments of the co-op fee (avgift), plus a personal mortgage payment on a mortgage that was taken to pay for the initial conversion fee. As discussed above, the co-op fee covers the operating expenses (c_t) plus the debt service on the mortgage the co-op took out (d_t) minus the rental revenue from the commercial and residential spaces of non-converters. Per square meter, the co-op fee to the owners is:

$$f_t = c_t + d_t - [\alpha(1-\beta) + (1-\alpha)\phi] r_t^s$$

The co-op fees total $s_i f_t = x_i f_t / \alpha\beta$ for an owner of x_i square meters. The co-op fee changes over time because c_t and r_t^s grow at rate g while d_t is fixed for the first 30 years and then falls to zero thereafter.

$$f_{t+j} = c_t (1+g)^j + d_{t+j} - [\alpha(1-\beta) + (1-\alpha)\phi] r_t^s (1+g)^j$$

where $d_{t+j} = d_t$ for $j \leq T$ and $d_{t+j} = 0$ for $j > T$. This formula assumes that there will be no changes in the ownership structure of the building over time (i.e., no additional conversions or no conversions between rented commercial space and owned residential space).

The personal mortgage is assumed to cover the conversion fee $s_i X$ and to be a T-year fixed-rate mortgage with interest rate $\tilde{R}^m$. The interest rate could in principle be different from the one on the co-op's mortgage (R^m) and/or the discount rate of the household ($\hat{R}$). The monthly payment required to cover the co-op conversion cost is:

$$\tilde{d}_{t+j} = \frac{s_i X \tilde{R}^m}{1 - \frac{1}{(1+\tilde{R}^m)^T}}, \forall j \leq T, \quad \tilde{d}_{t+j} = 0, \forall j > T$$

The outstanding personal mortgage balance evolves as follows:

$$\tilde{D}_{t+j} = \tilde{D}_{t+j-1}(1+\tilde{R}^m) - \tilde{d}_{t+j}, \forall j \leq T, \qquad \tilde{D}_{t+j} = 0, \forall j > T$$

The interest component of the personal mortgage payment is $\tilde{R}^m \tilde{D}_{t+j-1}$. This interest expense is tax deductible at the marginal income tax rate. The interest portion of this loan is highest in the early periods while the principal payment increases over time. Thus, the value of the tax shield decreases over time. Let $\tilde{\tau}$ be the relevant marginal income tax rate of the owner in question, then the tax shield equals:

$$ts_{t+j} = \tilde{\tau} \tilde{R}^m \tilde{D}_{t+j-1}$$

In sum, the payments associated with ownership (r_t^o) are:

$$r_{t+j}^o = s_i f_{t+j} + \tilde{d}_{t+j} - ts_{t+j}$$

The cost of owning to a household depends on her length of stay $\tilde{T}$ and her discount rate $\hat{R}$. Denote the present value of the cost of owning *per square meter* by Π_i^{own}:

$$\Pi_i^{own}(\tilde{T}) = \frac{1}{x_i} \sum_{j=1}^{\tilde{T}} \frac{r_{t+j}^o}{(1+\hat{R})^j}.$$

For a household with an apartment of size x_i, the present value of the cost is $x_i \Pi_i^{own}$.

C.2.3 The Cost Differential

The cost differential between owning (converting) and renting also depends on the value of the outside option after the length of stay has terminated. It is reasonable to assume that this outside option is the same for renters and owners. The renter who leaves the subsidized apartment and the owner who sells her co-op apartment face the same outside housing market at a given point in time. Let the per-period cost of accessing the outside housing market at time $t+j$ be H_{t+j}. Let T be the last year a person lives and assume no mortality risk, then $T-t$ is residual life expectancy. Finally, the owner sells her co-op shares at time $t+\tilde{T}$ for their market value and must use the funds to pay back the outstanding balance on the personal mortgage. Then the cost differential between renting and owning at time t, or equivalently the windfall (cost savings) from owning, is:

$$\tilde{W}_t(\tilde{T}, \hat{T}, \hat{R}) = x_i \Pi_i^{rent}(\hat{T}) - x_i \Pi_i^{own}(\tilde{T}) + \sum_{j=\hat{T}+1}^{T} \frac{H_{t+j}}{(1+\hat{R})^j} - \sum_{j=\tilde{T}+1}^{T} \frac{H_{t+j}}{(1+\hat{R})^j} + \frac{s_i V_{t+\tilde{T}}^e - \tilde{D}_{t+\tilde{T}}}{(1+\hat{R})^{\tilde{T}}}$$

In the case where $\tilde{T} = \hat{T}$, this simplifies to:

$$\tilde{W}_t(\hat{T}, \hat{R}) = x_i \Pi_i^{rent}(\hat{T}) - x_i \Pi_i^{own}(\hat{T}) + \frac{s_i V_{t+\hat{T}}^e - \tilde{D}_{t+\hat{T}}}{(1+\hat{R})^{\hat{T}}}$$

In the case where $\tilde{T} < \hat{T}$, we get:

$$\tilde{W}_t(\tilde{T}, \hat{T}, \hat{R}) = x_i \Pi_i^{rent}(\hat{T}) - x_i \Pi_i^{own}(\tilde{T}) - \sum_{j=\tilde{T}+1}^{\hat{T}} \frac{H_{t+j}}{(1+\hat{R})^j} + \frac{s_i V_{t+\tilde{T}}^e - \tilde{D}_{t+\tilde{T}}}{(1+\hat{R})^{\tilde{T}}}$$

The outside option (third term) no longer disappears, but it is small if either $\tilde{T}$ and $\hat{T}$ are close, or if $\tilde{T}$ is sufficiently large.

One special case obtains when the trade-off is between renting for one year or converting and selling at the end of year one. Then the windfall from owning to the household is:

$$\tilde{W}_t(1, \hat{R}) = \frac{x_i r_t^s (1+g) - s_i f_{t+1} + s_i V_{t+1}^e - \tilde{D}_t (1 + \tilde{R}^m (1-\tilde{\tau}))}{(1+\hat{R})}$$

C.3 Numerical example for Sveaborg 5

The numerical example approximates the realities at Sveaborg 5. One exception is that I assume that all debt is fully amortizing. The size of the building is $S = 7002$ square meters. We will study the entire building, the co-op, and one owner of the co-op, the tenant of apartment no 802.

Assume that $\alpha = 0.9634$ (256 out of 7002 square meters) and $\beta = 0.6727$ (4538 out of 6746 residential square meters participates in conversion). Assume a 737SEK subsidized rent per year per square meter, and assume that the same rent applies to the commercial tenants: $r_t^c = \phi r_t^s$, with $\phi = 1$. Both of these assumptions are true in Sveaborg 5. Assume a 434SEK cost per square meter, and a discount rate of 8% per year. Then the accounting value to the municipal landlord is $V_t^s = 5,160$SEK per square meter or 36.135 million SEK for the entire building. These are the exact values used in the appraisal report and economic plan for Sveaborg 5.

In order to pay for the building, the co-op raises mortgage debt. We assume it pays an average mortgage rate of 5% for the debt, consistent with the observed interest cost of Sveaborg 5. We have no information on the type and maturity of the debt but we assume a fixed rate mortgage with a $T = 28.5$ year average maturity.

We target annual co-op fees are $f_t = 636.6$SEK per square meter per year, which is the observed value. This is a fraction $\chi = 0.8638$ of subsidized rents. Given income from commercial and residual tenant rents, this co-op fee pins down the monthly debt service d. Given the terms of the mortgage, it therefore also pins down the size of the debt D. We obtain a 25.1 million SEK mortgage, which corresponds to a debt to purchase price ratio of 69.4%. This exactly matches the ratio in the economic plan, because that is how we chose the maturity of the mortgage T.

Given the purchase price and the debt raised, the conversion fee is $X = 1577.8$SEK per square meter or 11.0 million SEK in total.

The key parameter we need to take a stance on next is the market rent per square meter r_t^m. This will ultimately determine the windfall the tenants receive. For illustration purposes, let us assume that the free market rent is 12.7% above the subsidized rent, or 830.6SEK per year per square meter. Then the building's market value is 43.348 million SEK, or exactly 20% above the sale price of 36.135 million SEK. This market value amounts to 6190.7 SEK per square meter. The windfall to the co-op is $W = 1031.2$ per square meter or 7.22 million SEK. The market value of a co-op share is $V_t^e = 2609.1$SEK per square meter, while the market value of all co-op shares is 18.27 million, which is the difference between the market value of the building and the face value of the debt the co-op took on. based on the market value of the building, the co-op's leverage ratio is not 69.4% but only 57.9%.

Tenant i owns an apartment of $x_i = 89$ square meters. This amounts to $s_i = 137.3$ shares in the co-op. Owner i pays a conversion fee of $s_i X = 216,672$ SEK and an annual co-op fee of $x_i f = 56,659$ SEK. The market value of her co-op share is $s_i V_t^e = 358,287$ SEK. If the tenant participates in the co-op conversion and immediately turns around and sells her co-op shares, she would have made a profit of: $s_i(V^e - X) = 141,615$ SEK. Note that this equals her share of the windfall $s_i W$. My initial conversion fee matches that in the data closely (217kSEK versus 221kSEK). Thus, this owner gets a windfall of about $14,000 in the form of illiquid housing wealth.

Next, we take the household's perspective (the same tenant with a 89 square meter apartment) and compare the cost of staying in subsidized rental housing to the cost of converting. We assume that the household's discount rate $\hat{R} = 0.08$, the same as that we used for the municipal landlord. We assume that the household finances the entire conversion fee with a personal mortgage. This personal mortgage has the same maturity $T = 28.5$ and the same interest rate $\tilde{R}^m = 0.05$ as the mortgage that the co-op obtained. The marginal tax rate is 42%. The principal balance of the loan is thus 216,672 SEK. The first period mortgage payment is 14,425 and the first-period tax shield is worth 4550 SEK. Therefore, the personal mortgage payment after tax shield in the first year is 9,874 SEK. The first-year cost of ownership is this amount plus the co-op fee of 56659 SEK, for a total of 66,533.7 SEK. For comparison, the first year cost of staying in subsidized housing (and not converting) is 65,593 SEK, which is very similar. Because the personal mortgage payments are constant, the cost of ownership drops below the cost of renting from year 3 onwards. As before, we assume that rents, costs, and house prices all grow at 2% per year.

We now compare the PDV of renting and owning, assuming that the length of stay is the same for both ($\hat{T} = \tilde{T}$). In the case of ownership, we assume that the house can be sold at its market value minus a 5% sales commission and tax. When the holding period is 1 year, the cost of renting is 65,593 SEK, while the cost of owning is -68,364 SEK. The latter is the difference of the annual cost of owning of 66,533.7 SEK and the capital gain from selling the co-op shares after one year which is 134,898 after transfer taxes. Thus, the cost differential between renting and owning is 133,957 SEK in present value terms. (Note that this is close to the windfall we derived above of 141,615 SEK, which was the benefit of selling immediately as opposed to after 1 year). Hence, owning vastly dominates renting due to the initial subsidy.

We repeat this analysis assuming a 5-year horizon. Owning bests renting by a total present value margin of 180,903 SEK. For 10, 20, and 30 year lengths of stay, the present value of owning minus renting is 215,652 SEK, 238,885 SEK, and 234,657 SEK. The non-monotonicity occurs by virtue of the high household discount rate used to discount the capital gain upon sale of the co-op shares. This analysis shows that it is not difficult to justify immediate sales of the converted property, but also sales that take place much later.

D Detail on Consumption Imputation

This appendix describes in detail how consumption is imputed. We recall the household budget constraint:

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

Where Cons is imputed consumption, dDebt is change in debt, dHousing is change in residential real estate, dFin is change in financial wealth and non-primary real estate and Income is labour income after taxes and transfers. Consumption is calculated at the individual level and total household consumption is obtained by summing up the individual consumptions. We deflate consumption and all its components by the consumer price index to express them in real terms. We also scale consumption and its components by the household equivalent scale, which is computed form the number of adults and children in the household, and applied household by household.

D.1 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.[52] 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.[53]

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,

[52] The wealth registry only records housing wealth for adult individuals, thus we disregard children or other family members. In the case of married couples, the value of a property is equally split between the two.

[53] As we cannot observe the actual change in address, we disregard this case if the household owned or transacted apartments/coop shares during the current year or past year.

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.

Because the value of apartments in the Wealth Registry is only updated when an individual moves to a new address, 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 KURU 55 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.

D.2 Construction of dDebt

The debt level is observed in the wealth registry for all individuals and at the end of each year. Debt refers to student loans, mortgages and consumer loans. Because student loan cash flows are already included in disposable income, we deduct these cash flows from our measure of income.

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.). The total amount of interest paid for loans is observed and we subtract this amount in order to obtain our measure of borrowing.[54] 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).

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:

[54]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.

- 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.

D.3 Construction of dFin

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

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 bond and equity 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:

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

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 households 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.

D.4 Construction of Income

Disposable income already includes interest income from fixed income securities, dividend income from stocks and mutual funds, rental income from properties, as well as capital gains realized from the sale of financial assets and real estate properties. Since financial income (interest, dividends, rents) and capital gains are part of our measure of financial and housing wealth we must subtract them from disposable income, lest we double-account 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 labor income after taxes and transfers. Consumption increases with income.

E Additional Estimation Results

E.1 Labor Income and Labor Force Participation

We investigate whether home ownership affects households' labor force participation and earnings from work. There are several reasons why we could expect to see an effect. First, households may want to work extra in order to save for a downpayment. One might expect this effect to be stronger as the possibility of home ownership approaches. However, given that both treatment and control group were equally uncertain about the possibility of ownership, we do not expect this anticipation channel to differentially affect the treated and the control groups in the pre-conversion period. Second, post-conversion, the treated may be compelled to work harder in order to service the mortgage debt they took on to finance the home purchase, or to sustain the same consumption in the face of increased debt service (Fortin (1995), Del Boca and Lusardi (2003)). This would be especially true for those households who see the largest increases in debt. Third, since the conversion coincides with a windfall gain, a wealth effect may reduce the desire to work, and with it labor income. Since movers liquify this windfall gain but stayers do not, the wealth effect might be stronger for movers than for stayers.

Our first outcome variable is household labor income (Labinchh, columns 1-3 of Table A4), which combines the extensive margin effect on the number of adults that are working (Numwork, columns 4-6) with the intensive margin effect on labor income per working adult (Labincind, columns 7-9). Labinchh and Labincind are both expressed per adult equivalent so that their magnitudes are similar.

Table A4: ITT Estimation - Labor Force Participation and Earnings Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Labinchh under age 64			Numwork under age 64			Labincind under age 64		
Samples	Fixed	Stayers	Movers	Fixed	Stayers	Movers	Fixed	Stayers	Movers
RY-3	7690.9	1929.8	14482.0	0.0128	-0.0398	0.106**	7136.6	1790.7	13814.5
	(1.38)	(0.26)	(1.52)	(0.48)	(-1.02)	(2.48)	(1.42)	(0.25)	(1.47)
RY-2	260.3	-7281.4	10971.0	0.0400*	0.0265	0.0542	2186.8	-2515.9	7890.1
	(0.05)	(-1.23)	(1.24)	(1.70)	(0.85)	(1.48)	(0.48)	(-0.49)	(0.94)
RY0	15909.5**	11295.4**	23166.5**	0.0284*	0.0281	0.0208	15797.2**	10875.5**	23660.9**
	(3.41)	(2.55)	(2.76)	(1.87)	(1.40)	(0.70)	(3.33)	(2.41)	(2.81)
RY+1	9689.2	6596.2	14077.4*	0.0337	0.00636	0.0722	10866.1*	7373.5	15868.2*
	(1.62)	(0.92)	(1.75)	(1.28)	(0.21)	(1.35)	(1.96)	(1.16)	(1.91)
RY+2	1636.3	4645.9	-5096.5	0.0502	0.0450	0.0396	1984.6	4700.0	-5507.6
	(0.20)	(0.44)	(-0.41)	(1.33)	(1.09)	(0.63)	(0.25)	(0.50)	(-0.42)
RY+3	1880.5	-3693.2	9523.5	0.000354	-0.0611	0.100*	2148.0	-869.6	3562.5
	(0.23)	(-0.34)	(0.76)	(0.01)	(-1.09)	(1.87)	(0.27)	(-0.09)	(0.27)
PT-Mean	186999.4	182161	196850.7	1.34	1.34	1.32	193978.6	187839.9	206477.5
PT-SD	151304.8	144957.9	163055.6	.78	.81	.71	143536.4	137187.1	154945
N	14536	9835	4701	14536	9835	4701	14536	9835	4701
R^2	0.107	0.119	0.151	0.400	0.402	0.426	0.114	0.122	0.163

Notes: t statistics in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.001$. 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 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. Labinchh is household-level labor income divided by the adult equivalent scale 1+ (Adults-1)*.7+Children*0.5. Labincind is individual labor income. 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.[55] For brevity, we focus on the Fixed sample. Columns (1)-(2) show a positive treatment effect on total household labor income (per adult equivalent) in the treatment year RY0. The effect in the Fixed sample is an increase of 15,910 SEK or about 2,000 USD per person in household labor income. The effect represents a 8.5% increase over the average annual pre-treatment household labor income. Labor income remains higher for treatment than for control in RY+1 but the effect is no longer statistically significant.

Both extensive and intensive margins contribute to the labor earnings effect. Column (4) of Table A4 show a positive treatment effect in RY0 on the number of adults that work in the household. The initial effect in RY0 is statistically significant. The treatment effect represents a 2.5% increase for the Fixed sample over its average pre-treatment level. The increase in labor force participation dies out quickly. Column (7) shows an increase in labor income per working adult of 15,800 SEK for the Fixed sample in RY0, representing 8.2% of annual pre-treatment labor income per adult. The effect is significant at the 5% level. The effect remains significant in RY+1 and disappears afterwards.

The labor income effects are stronger among Movers, both at the intensive and extensive margin. This helps distinguish between the economic channels at work. Movers in the treatment group liquefy their housing wealth, and some of them return to rentership. If more liquid financial wealth depresses labor supply through a wealth effect, then we should find weaker labor income effects for Movers. But we do not. The data are consistent with the alternative hypothesis that Movers work

[55] This extra filter makes little difference since our sample contains no households over the age of 64 in RY-1. If one adult in the household is below 64 and the other above 64, we form household labor income from the labor income of the adult under 64 only.

harder because they must repay more debt. We show below that the relative increase in household debt is about 20k SEK larger for the treated Movers than for the treated Stayers.

E.2 Car Purchases

Researchers have turned to car purchase data as a proxy for household spending. This makes car purchases a natural starting point for our investigation into the consumption responses to an exogenous change in home ownership and housing wealth. Rather than relying on auto loan data to infer car purchases, we use data on actual car purchases (both new and second hand) from administrative records of the Swedish car registry.

Table A5 show the results. We find only weak evidence for a positive treatment effect on car spending. The largest effect for the All sample occurs in RY0 when the treated group's car buying rate is 2.9% points higher than that of the control group and relative to RY-1. This is a 20% increase on the pre-treatment baseline level of 14% per year. While the effect is economically meaningful, the effect is too imprecisely measured to deliver statistical significance. The effects are weaker for the Fixed sample in Column (2) in years RY0 to RY+2 and somewhat stronger in year RY+3 (three or more years after privatization), but never significant.

The treatment effects on car spending in the years post treatment are stronger for Movers than for Stayers. The treatment effect is 4.9% points in RY+2 and 9.4% points in RY+3 among the Movers. The RY+3 effect is significant at the 10% level. This estimate is large relative to the baseline car purchase rate for Movers of 15%. Stayers could in principal use their newly gained housing wealth and borrow against it to purchase a car. n Sweden it is quite common to obtain a bank loan to purchase a car with the house as collateral. The interest rate on such loans is substantially below that on regular car loans. Since we only observe total household debt we cannot directly investigate the rise in home equity debt related to a car purchase. We essentially find no evidence for such a housing collateral effect on car purchases. The point estimate for Stayers is 2.0% in RY0 and imprecisely estimated (t-stat of 0.7) and there is nothing further in the later years. The higher car purchase rates among Movers is consistent with the fact that some treated Movers liquefy their housing wealth and spend some of it on cars.

E.3 Alternative Breakdowns Stayers vs Movers

In the main text, we discussed a specification where Stayers in the treatment group are compared to Stayers in the control group, and where Movers in the treatment group are compared to Movers in the control group. We recall that all household-year observations in the Fixed sample go into either the Stayer or the Mover group, and that Moving is defined based on the last known address of a household in the post-privatization period. Unreported analysis on household characteristics of treated Stayers and treated Movers suggests that control Stayers and control Movers, respectively, are the closest comparison groups.

Nevertheless, it is useful to also compare treated Stayers to all households in the control group, and do the same for the treated Movers. Such a specification amounts to splitting up the main treatment effects into those coming from the Stayers and those coming from the Movers. Effectively, the $Convert$ indicator is split up into two indicators $ConvertxMover$ and $ConvertxStayer$. These two indicators sum to the original $Convert$ indicator, so that every household-year observation in the treatment sample belongs to one and only one type of converter, Stayer or Mover. Table A6 presents the results. Relative to the average household in the control sample, Stayer households

Table A5: ITT Estimation - Car Purchases

Samples	(1) All	(2) Fixed	(3) Stayers	(4) Movers
RY-3	-0.0166	-0.0198	-0.0319**	0.00552
	(-1.05)	(-1.18)	(-2.06)	(0.16)
RY-2	0.00637	-0.00104	-0.0209	0.0411
	(0.19)	(-0.03)	(-0.69)	(0.76)
RY0	0.0288	0.0189	0.0201	0.0203
	(1.15)	(0.74)	(0.72)	(0.50)
RY+1	0.0176	0.00731	-0.00319	0.0287
	(0.78)	(0.31)	(-0.12)	(0.67)
RY+2	-0.000352	-0.00341	-0.0261	0.0493
	(-0.02)	(-0.17)	(-1.29)	(1.03)
RY+3	0.0140	0.0308	0.00195	0.0944*
	(0.74)	(1.44)	(0.11)	(1.84)
PT-Mean	.14	.13	.12	.15
PT-SD	.34	.34	.33	.36
N	18284	15076	10273	4803
R^2	0.0366	0.0414	0.0466	0.0535

Notes: t statistics in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.001$. 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 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.

in the treatment sample show a much lower consumption in the treatment year (Stayer RY0), but no differential consumption response in the post-privatization period (Stayer RY(post)). Treated Movers show about the same consumption response in the initial year of private ownership as treated Stayers, but a large, positive subsequent consumption response. Treated Movers consume 30.4k SEK more per year than the control group (and 34k more than the treated Stayers). This difference is economically large and statistically significant. The opposite is true for savings. Treated Movers save 38.6k SEK less per year than the control group and 47k SEK per year less than treated Stayers, post-privatization. Treated Stayers save slightly more than the control group (8.5k SEK per year).

The moving decision is endogenous. Treatment may interact in complicated ways with that moving decision. Therefore, splitting the sample into Movers and Stayers may inadvertently be comparing different types of households in the treatment and in the control sample. The previous exercise showed that using the full control group delivered similar results than using stayers as a control group for the treated stayers and movers as a control group for treated movers. As a second robustness check on the results we define movers and stayers based on pre-determined variables. Rather than sorting households based on realized moving choices in the post-privatization period, we sort them based on predicted moving probability where the predictors are all known in the household formations year, i.e., before privatization. That is, we instrument moving in a first-stage regression, and then use the predicted mobility rates from the first stage to form a group of “Stayers” with low predicted mobility and “Movers” with high predicted mobility which we use in the second stage in lieu of the realized staying and moving indicator variables.

Specifically, we regress the Moving indicator variable on age of the oldest adult in the household, number of children, partnership dummy, years the household has lived in the building, and apartment size in an OLS specification. All rhs variables are measured as of the household formation year RY(-1), whereas the lhs variable tracks the incidence of moving between RY(0) and RY(+4), so that the

Table A6: ITT Estimation - Stayers vs. Movers, Control Group is Fixed

LHS var:	Consumption	Income	dHousing	dDebt	dFin	Savings
Stayer RY(pre)	-10651.3	-3561.6	2190.1	-4367.3	532.2	7089.7
	(-1.14)	(-1.09)	(0.40)	(-0.65)	(0.07)	(0.92)
Stayer RY0	-16173.7	9928.0**	369413.3***	326081.4***	-17230.2***	26101.7**
	(-1.29)	(2.45)	(4.89)	(4.45)	(-3.71)	(2.42)
Stayer RY(post)	-2031.0	6421.4	-4809.8	-16288.3**	-3026.1	8452.4
	(-0.26)	(1.46)	(-0.90)	(-2.86)	(-0.47)	(1.35)
Mover RY(pre)	6780.3	7049.0*	-1391.5	-3001.1	-1341.0	268.6
	(0.74)	(1.91)	(-0.24)	(-0.47)	(-0.18)	(0.03)
Mover RY0	-17043.8	12486.7*	388633.2***	356446.0***	-2656.8	29530.4**
	(-1.50)	(2.00)	(5.48)	(5.40)	(-0.34)	(2.30)
Mover RY(post)	30373.9**	-8211.9	-35342.1	11124.8	7881.1	-38585.8***
	(2.93)	(-1.37)	(-1.54)	(0.69)	(1.19)	(-4.63)
PT-Mean	160516.6	166894.1	1864.67	4867.42	9380.30	6377.56
PT-SD	117626.5	85380.2	49840.65	70086.34	77498.23	92889.09
N	13370	13370	13370	13370	13370	13370
R^2	0.0664	0.142	0.209	0.197	0.0130	0.0190

Notes: The table reports ITT estimates of a specification that separates out the treatment effect $ConvertxRY$ into separate treatment effects for Stayers and Movers. That is, it includes $ConvertxStayerxRY$ terms $ConvertxMoverxRY$ terms. The rest of the specification is as in the benchmark specification described in the notes to Table 4.

covariates are all pre-determined. A one-standard deviation increase in each of the right-hand side variables affects the moving rate by -9.9%, -1.9%, +2.5%, -3.3%, and -3.9%, respectively. All five covariates are statistically significant. The average predicted moving rate is 34.2% and the standard deviation of 12.5%. In other words, the regression generates substantial variation in predicted moving rates. We then use the predicted values of this regression to classify each household into a high, middle, and low moving probability group. The three groups' average predicted moving rates are 20%, 34%, and 48%. We define "Pred. Stayers" as those in the lowest predicted mobility group and "Pred. Movers" as those households in the highest predicted mobility group.

Table A7 contains the ITT estimates for consumption and its components. The treated households in the middle-mobility group are included in the estimation but the coefficients are omitted for brevity. As before, we find that Movers have a much higher consumption response than Stayers in the post-privatization period. The opposite is true for their savings response to home ownership. The initial consumption response is larger here for Movers, whereas in Table A6 it was no different between Movers and Stayers. The initial labor income effect is concentrated among the likely Stayers, consistent with Table A6. Post-privatization, we see that likely Movers build up debt while likely Stayers pay down debt. The difference is economically large and statistically significant.

We conclude that our findings on the heterogeneity in the post-privatization consumption response between Savers and Movers hold irrespective of what control group is used and irrespective of whether Movers are defined based on realized or predicted mobility.

E.4 Collateral Effect Estimation

Table A8 reports the results from the ITT estimation of equation (3) in the main text. The main coefficients of interest are the triple-difference coefficients β which measure the consumption response of the treated households with the negative income shock, relative to the control households and

Table A7: ITT Estimation - Stayers vs. Movers, Predicted Mobility

LHS var:	Consumption	Income	dHousing	dDebt	dFin	Savings
Pred. Stayer RY(pre)	12828.0	17786.3**	5461.2	-3309.4	-3812.4	4958.2
	(0.92)	(3.14)	(0.99)	(-0.50)	(-0.39)	(0.46)
Pred. Stayer RY0	1926.1	22305.7**	449816.1***	400408.4***	-29028.1**	20379.6
	(0.12)	(3.38)	(4.46)	(4.17)	(-2.82)	(1.48)
Pred. Stayer RY(post)	4491.3	12487.3	-24190.9**	-26324.7**	5862.2	7996.0
	(0.43)	(1.34)	(-2.20)	(-2.72)	(1.04)	(1.03)
Pred. Mover RY(pre)	9374.5	-1480.5	-3775.4	-2323.6	-9403.2*	-10855.0
	(0.90)	(-0.25)	(-0.63)	(-0.31)	(-1.78)	(-1.47)
Pred. Mover RY0	-25598.2*	4511.5	303138.4***	270903.4***	-2125.3	30109.7**
	(-2.00)	(0.66)	(5.53)	(5.47)	(-0.31)	(2.12)
Pred. Mover RY(post)	12425.4	-12919.7	683.3	21892.0**	-4136.4	-25345.1**
	(1.34)	(-1.62)	(0.07)	(2.58)	(-0.73)	(-3.38)
PreTreat_Mean	160516.6	166894.1	1864.67	4867.42	9380.30	6377.56
PreTreat_SD	117626.5	85380.2	49840.65	70086.34	77498.23	92889.09
N	13144	13144	13144	13144	13144	13144
R^2	0.0679	0.144	0.212	0.204	0.0123	0.0149

Notes: The table reports ITT estimates of a specification that separates out the treatment effect *ConvertxRY* into separate treatment effects for Stayers and Movers. That is, it includes *ConvertxStayerxRY* terms *ConvertxMoverxRY* terms. Stayers are defined as those with low *predicted mobility*, based on pre-determined characteristics, while Movers are defined as those with *high predicted* mobility. The rest of the specification is as in the benchmark specification described in the notes to Table 4.

relative to those who do not get a negative income shock. The δ coefficients measure the treatment effect for those who do not get a negative income shock. Post-privatization, the treated households only consume 5.0k SEK more per year than the households in the control group without an income shock. This weak consumption response is not statistically different from zero, and smaller than the average 8k SEK effect discussed in the main text.

In sharp contrast, those treated households who do receive a negative income shock consume 32.9k SEK extra ($\beta(post)$) relative to the treated households who do not get a negative income shock. Treated households with a negative income shock display a vastly stronger and statistically significant consumption response. The 8k SEK average treatment effect on consumption post-privatization is largely driven by this group.

To get at the collateral effect, we can compare the difference in consumption response to the income shock between the treated and the control. The former have a house to borrow against while the latter don't. The $\lambda(post)$ shows that the consumption of the control group hit by a negative income shock is almost 8k SEK lower than that of the control group households not hit by the shock. Relative to the control group hit with a negative income shock, the treated hit with the same income shock consume 38k SEK ($\delta(post) + \beta(post)$) more per year. To put this number in perspective, 38k SEK fully offsets the -35.6k SEK level effect from lower income (the Z term). This is strong evidence for the housing collateral effect.

Column (2) shows that the treated indeed increase their debt to pay for this extra consumption. The triple-difference is 21k SEK. In other words, while treated households who do not receive the income shock pay back their mortgage in the average post-privatization year, those hit with a large negative income shock do the reverse. Those without housing collateral have no assets to borrow against and cannot increase borrowing ($\gamma(post) + \lambda(post)$ is very negative).

Column (3)-(6) repeat the analysis separately for Stayers and Movers. The findings are similar to the full Fixed sample. While the overall consumption response post-privatization is weak for Stayers

Table A8: ITT Estimation - Collateral Effects of Housing

	(1)	(2)	(3)	(4)	(5)	(6)
	Fixed		Stayers		Movers	
LHS var:	Consumption	dDebt	Consumption	dDebt	Consumption	dDebt
$\delta(pre)$	-1361.4	-3908.7	-812.2	-7617.0	-104.6	1597.1
	(-0.16)	(-0.65)	(-0.09)	(-1.13)	(-0.01)	(0.16)
δ_0	-11640.5	340609.7***	-10153.5	325968.0***	-10477.0	364312.3***
	(-1.29)	(4.79)	(-0.77)	(4.42)	(-0.80)	(5.34)
$\delta(post)$	4960.6	-10256.7	1348.5	-17709.4**	18020.3	713.4
	(0.65)	(-1.42)	(0.18)	(-2.68)	(1.08)	(0.04)
$\beta(pre)$	-12974.2	1407.2	-5040.7	-1099.3	-30996.1*	4444.8
	(-1.14)	(0.30)	(-0.28)	(-0.23)	(-1.78)	(0.45)
β_0	-27867.9	-25275.2	-12222.6	27709.0	-56707.3*	-120412.4**
	(-1.22)	(-0.59)	(-0.45)	(0.68)	(-1.82)	(-2.05)
$\beta(post)$	32903.5**	20973.0	21867.8	26683.8**	48476.7	13369.4
	(2.11)	(1.67)	(1.09)	(2.16)	(1.32)	(0.37)
$\gamma(pre)$	-6186.1	-5007.7	-2043.1	-4044.5	-17376.9	-5860.0
	(-1.10)	(-0.60)	(-0.35)	(-0.62)	(-1.34)	(-0.30)
γ_0	1421.2	16516.8	9023.7	15701.0	-15531.0	19822.4
	(0.20)	(1.35)	(1.22)	(1.17)	(-1.09)	(1.26)
$\gamma(post)$	-9590.9	-24254.5	2076.8	-22034.5	-36845.6	-28416.4
	(-1.03)	(-0.85)	(0.22)	(-0.69)	(-1.60)	(-1.00)
$\lambda(pre)$	7711.5	4293.7	6937.9	-1350.2	10503.9	6431.0
	(0.94)	(0.73)	(0.67)	(-0.18)	(0.67)	(0.63)
λ_0	7566.6	-14459.2	14275.9	-23116.8**	1191.5	-7990.9
	(0.68)	(-1.51)	(0.97)	(-2.55)	(0.08)	(-0.37)
$\lambda(post)$	-7925.9	-5845.5	-1257.2	-13098.7	-18322.9	-1774.1
	(-1.27)	(-0.76)	(-0.12)	(-1.51)	(-1.34)	(-0.11)
Z	-35571.9***	-2792.8	-35375.3***	3947.1	-36950.9***	-10152.6
	(-6.39)	(-0.64)	(-5.06)	(0.70)	(-3.61)	(-1.49)
N	13370	13370	9165	9165	4205	4205
R^2	0.0725	0.197	0.0825	0.293	0.0862	0.140

Notes: The table reports ITT estimates of equation 3. The δ terms are the coefficients on the $Convert$xRY indicator. The β terms are the coefficients on the $Convert$xRYx$Shock$ indicator, where Shock indicates a large labor income shock of at least 25%. The γ terms are the coefficients on the RY indicator. The λ terms are the coefficients on the RYx$Shock$ indicator.

and fairly strong for Movers, both Stayers and Movers affected by a negative income shock display a surge in consumption and take out additional debt. The treatment effect on consumption is nearly zero for Stayers who do not receive a negative income shock, but large for those who do.

E.5 Heterogeneous Treatment Effects

E.5.1 Linear Specification in the Windfall

We estimate treatment effects that vary linearly in the size of the windfall:

$$y_{it} = \alpha + Convert_i \sum_k (\tilde{\delta}_{k,0} + \tilde{\delta}_{k,1}\widetilde{WF}_i)RY_i(t=k) + \sum_k \tilde{\gamma}_k RY_i(t=k) + \widetilde{WF}_i + X_{it} + \psi_t + \omega_b + \varepsilon_{it}, \quad \text{(A2)}$$

Table A9: ITT Estimation - Treatment Effects Linear in Windfall

Windfall terms:	intercept	slope
	Home Ownership	
RY0	0.859***	0.124***
	(38.35)	(3.68)
RY(post)	0.734***	0.0692**
	(34.36)	(2.62)
	Consumption	
RY0	-20761.3**	-16650.7**
	(-2.71)	(-2.22)
RY(post)	8481.0	-2254.7
	(1.31)	(-0.48)
	Savings	
RY0	31146.4***	20123.4***
	(4.77)	(4.17)
RY(post)	-9738.1**	-2315.2
	(-2.18)	(-0.70)
	Household Labor Income	
RY0	15341.1***	5122.9
	(3.90)	(1.46)
RY(post)	-200.2	-1989.5
	(-0.02)	(-0.23)
	Stock Market Participation	
RY0	0.0280*	0.0209
	(1.72)	(1.09)
RY(post)	0.0292*	-0.0217
	(1.97)	(-1.54)
	Anymove	
RY0	-0.0120	-0.00566
	(-1.36)	(-0.85)
RY(post)	0.0559***	0.00270
	(4.05)	(0.53)

Notes: This table reports ITT estimates of $\tilde{\delta}_{k,0}$ (first column) and $\tilde{\delta}_{k,1}$ (second column) from equation (A2). It collapses the relative year effects in the post-privatization period $k \geq 1$.

where $\widetilde{WF}_i$ is the windfall of household i, normalized by subtracting the cross-sectional mean of the windfall and dividing through by the cross-sectional standard deviation. The coefficients $\tilde{\delta}_{k,0}$ measure the ITT effect at the average windfall, while the slope coefficients $\tilde{\delta}_{k,1}$ measure the sensitivity of the treatment effect to a one standard deviation increase in the windfall. Table A9 reports ITT estimates of $\tilde{\delta}_{k,0}$ (first column) and $\tilde{\delta}_{k,1}$ (second column) from equation (A2). It collapses the relative year effects in the post-privatization period $k \geq 1$. The windfall is standardized to be mean-zero, standard deviation 1. Consistent with the evidence from the piece-wise linear specification reported in the main text, we find that initial home ownership response and initial consumption response are increasing in the windfall.

E.5.2 Treatment Effects that Vary by Age, Income, and Wealth Bin

Denote by $Z_{it}(n)$ an indicator variable which is 1 if household i is in bin n for variable Z at time t, where Z is either Age, or Labor Income, or Financial Wealth. Because all agents in the control group have an age, labor income, and financial wealth, we can let their dynamic response ($\gamma_{k,n}$) to not being privatized depend on their age, labor income, and financial wealth:

$$y_{it} = \alpha + Convert_i \sum_k \sum_n \delta_{k,n} RY_i(t=k) Z_{it}(n) + \sum_k \sum_n \gamma_{k,n} RY_i(t=k) Z_{it}(n) + Z_{it}(n) + X_{it} + \psi_t + \omega_b + \varepsilon_{it}, \tag{A3}$$

Table A10 reports the $\delta_{k,n}$ ITT estimates of equation (A3) in the conversion year ($k = 0$) and the post-privatization period ($k > 0$). The first four columns are for the age bins, the next four for labor household income bins, and the last four for financial wealth bins. The bin cut-offs are reported in the second row. Bins are quartiles, except for financial wealth, where we group all households with zero financial wealth in the first group (31.2% of the sample) and split the rest in three equal groups of 22.7% each. The main take-away from this table is our main findings hold across age, income, and financial wealth groups. But, there is some interesting variation as well. The treatment effect on mobility is strongest among the young, the low-income, and the low-wealth. The positive labor income effects from privatization are only present for the 45-53 year olds and the highest financial wealth group. The initial consumption decline and initial increase in savings are strongest for the youngest, the low-income, and the lowest financial wealth group. The subsequent consumption increase is largest for the youngest as well as the 45-53 year olds, and for the third labor income quartile. The stock market participation effects are concentrated among the 37-54 year olds, and the bottom half of the labor income and financial wealth distribution. Linear specifications convey a similar message.

E.6 Treatment Effect on the Treated

Table A11 repeats our main consumption table using the Treatment Effect on the Treated estimation described in equation (5) as opposed to the ITT results reported in the main text. All point estimates are about 7% larger than the ITT estimates and are estimated equally precisely.

Table A10: ITT Estimation - By Age, Labor Income, and Financial Wealth Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Age Bins				Labor Income Bins				Financial Wealth Bins			
Bins	<37	37-44	45-53	>53	<86k	86k-176k	176k-281k	>281k	0	0-13k	13k-70k	>70k
	Homeowner											
RY0	0.758***	0.850***	0.874***	0.845***	0.765***	0.888***	0.866***	0.791***	0.715***	0.823***	0.908***	0.873***
	(11.45)	(21.56)	(25.90)	(15.12)	(10.47)	(20.75)	(25.13)	(19.98)	(10.41)	(16.01)	(25.50)	(41.52)
RY(post)	0.620***	0.751***	0.773***	0.752***	0.596***	0.811***	0.764***	0.706***	0.649***	0.724***	0.766***	0.752***
	(11.06)	(23.53)	(26.02)	(14.73)	(11.13)	(25.92)	(19.81)	(20.34)	(18.46)	(21.40)	(23.84)	(22.62)
	Consumption											
RY0	-31744.6	-13139.5	4688.8	-25958.5	-45013.5**	-20022.4	-9428.2	-6021.5	-36273.2**	2059.4	1745.1	-24005.1
	(-1.54)	(-1.25)	(0.46)	(-1.26)	(-2.21)	(-1.47)	(-0.67)	(-0.42)	(-2.28)	(0.16)	(0.10)	(-1.54)
RY(post)	17864.4	-333.8	25944.9**	-9088.0	12559.9	2995.6	18293.8*	12157.3	14986.2	14527.0	3393.2	6782.5
	(1.63)	(-0.03)	(2.68)	(-1.10)	(1.31)	(0.42)	(1.71)	(0.92)	(1.67)	(1.18)	(0.33)	(0.64)
	Savings											
RY0	39038.2*	10659.4	19876.3**	28471.5*	46272.6**	23458.0*	24840.5*	6059.2	43408.7**	6687.9	-7228.0	47280.1**
	(1.79)	(1.07)	(2.15)	(1.74)	(2.25)	(1.84)	(1.87)	(0.45)	(3.04)	(0.58)	(-0.53)	(3.23)
RY(post)	-26006.6**	-9611.0	-9157.3	7097.1	-10088.7	2818.4	-9046.7	-19185.0*	-15175.5	-21940.6**	-9074.4	10226.1
	(-3.20)	(-1.49)	(-1.24)	(0.81)	(-1.04)	(0.58)	(-1.43)	(-1.76)	(-1.64)	(-2.60)	(-0.99)	(1.43)
	Propensity to Consume Out of Housing Wealth											
Windfall	413,074	374,423	451,716	545,615	318,258	348,330	467,687	577,016	332,427	398,440	474,114	548,838
MPC	5.47%	-0.10%	6.18%	-1.87%	5.26%	0.97%	4.35%	2.32%	6.35%	4.29%	0.77%	1.26%
	Household Labor Income											
RY0	-1113.2	-15708.3	50857.4***	10831.0	12241.7	1033.5	13757.9	-7437.1	12722.9	14251.3	-17370.0	39229.7**
	(-0.08)	(-1.00)	(4.88)	(0.54)	(1.59)	(0.21)	(1.66)	(-0.50)	(1.41)	(1.13)	(-1.28)	(2.98)
RY(post)	-22279.8	-31213.7**	34027.9**	26420.7	13880.4	9376.2	7470.5	-3008.0	1823.9	-17192.6	-16134.4	46666.4**
	(-1.36)	(-2.04)	(2.05)	(1.00)	(1.04)	(1.10)	(0.59)	(-0.15)	(0.15)	(-1.42)	(-1.28)	(2.51)
	Stock Market Participation											
RY0	0.00191	0.0732**	0.0252	0.000927	0.0181	0.0791*	0.0160	-0.0389	0.0197	0.0491	-0.0770**	0.0412
	(0.04)	(2.04)	(0.58)	(0.02)	(0.35)	(2.02)	(0.32)	(-1.04)	(0.80)	(1.14)	(-2.05)	(1.45)
RY(post)	-0.00251	0.0889**	0.0572	0.0110	0.0539	0.0859	0.0250	-0.000243	0.0842**	0.0795**	-0.0357	0.0538**
	(-0.05)	(2.54)	(1.36)	(0.24)	(1.62)	(1.55)	(0.61)	(-0.01)	(2.16)	(2.52)	(-0.86)	(2.09)
	Anymove											
RY0	-0.0535**	0.0280*	-0.0187	0.00998	-0.0153	-0.00764	-0.0186	0.00159	-0.00000943	-0.0311*	-0.0254	0.0162
	(-2.93)	(1.70)	(-1.40)	(0.49)	(-0.84)	(-0.51)	(-1.13)	(0.10)	(-0.00)	(-1.84)	(-1.29)	(0.90)
RY(post)	0.0750**	0.0545**	0.0455**	0.0612**	0.0714**	0.0489**	0.0525**	0.0582**	0.0777***	0.0541**	0.0351*	0.0590**
	(3.27)	(2.80)	(3.10)	(3.31)	(3.37)	(2.36)	(2.30)	(2.77)	(4.02)	(2.33)	(1.82)	(2.66)

Notes: t statistics in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.001$. 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 coefficients on the controls are not reported.

Table A11: TOT Estimation - Consumption and Savings

LHS var:	(1) Consumption	(2) Income	(3) dHousing	(4) dDebt	(5) dFin	(6) Savings
RY-3	-2877.1	-59.04	1672.9	-1630.4	-485.3	2818.0
	(-0.27)	(-0.02)	(0.32)	(-0.28)	(-0.06)	(0.31)
RY-2	-6762.2	754.7	2852.7	-4163.7	500.6	7516.9
	(-0.74)	(0.20)	(0.54)	(-0.60)	(0.06)	(0.93)
RY0	-18187.4*	11823.5**	413184.4***	369990.0***	-13183.5**	30010.9**
	(-1.95)	(3.38)	(6.11)	(5.59)	(-2.43)	(3.38)
RY+1	10173.8	3140.4	-26169.2	-9445.6	9690.3	-7033.4
	(0.85)	(0.73)	(-1.22)	(-0.58)	(1.00)	(-0.75)
RY+2	8088.7	3447.2	-147.1	4137.2	-357.1	-4641.4
	(0.80)	(0.84)	(-0.01)	(0.39)	(-0.06)	(-0.54)
RY+3	8447.7	241.6	-17058.0	-14784.8*	-5932.9	-8206.1
	(0.78)	(0.05)	(-1.67)	(-1.73)	(-1.10)	(-1.08)
PT-Mean	160,517	166,894	1,865	4,867	9,380	6,378
PT-SD	117,627	85,380	49,841	70,086	77,498	92,889
N	13370	13370	13370	13370	13370	13370
R^2	0.0651	0.142	0.225	0.211	0.0137	0.0150

Notes: t statistics in parentheses. $* = p < 0.10$, $** = p < 0.05$, $*** = p < 0.001$. Standard errors are clustered at the building level. The table reports the coefficients δ_k^{TOT} 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 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. Consumption and Savings are divided by the adult equivalent scale 1+ (Adults-1)*.7+Children*0.5. The sample is the Fixed sample of households with constant adult composition. Relative years -4 through -3 are collapsed into the RY(-3) term and relative years +3, +4, and +5are collapsed in the RY(+3) term. The table reports Treatment effects of the treated.