# Identifying the Benefits from Home Ownership: A Swedish Experiment

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**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 high-quality information on demographics, income, mobility patterns, housing wealth, financial wealth, and debt. This 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. We find little effect for a housing collateral effect, whereby home owners who stay in place tap into their home equity to boost spending. Spending only increases for those who move and realize the capital gain embedded in their house. For this group, the marginal propensity to consume is large.

*Keywords:* home ownership, housing wealth, marginal propensity to consume out of housing wealth, privatization of social housing

*JEL codes:* D12, D31, E21, G11, H31, J22, $R^2$1, $R^2$3, 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, for example, spends roughly $300 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. 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 (owning versus renting), making it difficult to separate out the effect of home ownership

[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.

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 structure, floor area, number of bedrooms, age of the structure, 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.[3] The studies are usually on a small sample. They focus on a small set of outcome variables centering on life satisfaction rather than economic decisions. Survey responses 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. It overcomes key challenges that have plagued the literature to date by using a quasi-experiment which randomly assign home ownership. It considers a larger sample. It tracks more outcome variables over a longer period of time. And since our data are based on tax registries, they are much more granular and of much higher quality than survey-based data.

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

Our study exploits a unique setting to overcome these issues. In the early 2000s, Sweden went through a privatization wave of municipally-owned multi-family housing which ultimately turned many of its tenants from renters into home owners.[4] Our sample consists of about 5,000 individuals that make up about 2,500 households living in 46 buildings in the Stockholm metropolitan area. In each building, tenants formed a co-op association, petitioned their municipal landlord to acquire the building, and voted on the acquisition after having received an ask price from their landlord. 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 that a new law was passed in the middle to the co-op conversion process. This law, *Stopplag*, introduced an additional layer of approval by a County Board. Ultimately only 13 of the buildings were approved for conversion. The goal of the law was to prevent a situation whereby so few buildings would remain in municipal ownership that the municipal authorities could no longer reliably calculate the local rental price index. The law operated at a fine geographic and building-type level. We argue that Stopplag led to a random approval or denial decision on otherwise similar buildings, granting some households the option to own while confining others to rentership.

The Akalla complex serves as a good example. The complex comprises of four adjacent buildings with similar layout, identical construction year, and similar tenant demographic. The four tenant associations separately applied for co-op conversion in June 2001. After having received the ask price calculated using the same methodology from their common municipal landlord, all four associations voted on the purchase in April-June 2002. All four approved the acquisition by essentially the same margin. The municipal landlord approved all four transactions in the same week in September 2002. Because Stopplag went into effect on April $1^{st}$, 2002, long after the co-op conversion process was initiated but before the four votes, the County Board had to give its final blessing. In the same meeting in February 2003, it approved two out of the four buildings for privatization. Because all four co-ops had a type of courtyard unit which was fairly rare in the area, allowing all four co-ops convert would have impaired its ability to determine the rental price for that type of courtyard unit. Absent a rule for how to choose between the four co-ops, the County Board improvised, and

[4]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)). We are not aware of any other work that has studied these episodes using micro data or has exploited a natural experiment like ours.

ended up approving the two buildings with the earlier vote date. Given that the four votes took place within two months of each other, this was an essentially random assignment.

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. All tenants in the 13 buildings that were approved for co-op conversion are in our treatment group, while all tenants in the buildings that were denied are in our control group. We show that the groups are balanced in terms of building and household characteristics, and that there are no noticeable pre-trends in our main outcome variables of interest. The creation of a control group of denied tenants enables us to estimate household level effects of home ownership in difference-in-difference regressions.

We are able to track down all residents in these 46 buildings by matching on address and manually consulting tenant lists. 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. We match the tenants and their family members to their social security number and obtain 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 obtain total consumption expenditures as a residual from the budget constraint. We refine the consumption construction method of Koijen, Van Nieuwerburgh and Vestman (2014) to deal with changes in real estate wealth.

Our experiment has several nice features. The transaction was roughly cash-flow neutral in that the monthly co-op dues plus the mortgage payment were about the same as the monthly subsidized rent tenants paid prior to conversion. Since tenants had the option to remain tenants, the conversion price was based on the net-present value of rents. Due to rent control, rents were low relative to the costs of buying an apartment on the open market. As a result, converters could purchase their apartment at a discount, which meant that they could obtain 100% mortgage financing. This lead to another nice feature of the experiment in that financial constraints played no role in the conversion decision. It also implies that

the experiment not only bestowed home ownership status upon the converters, but also a windfall in the form of illiquid housing wealth.

Our first main result is that the vast majority (about 80%) of tenants who are approved for conversion also 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.

Our second main result is that the treated households, those approved for co-op conversion, are more mobile. They are more likely to move at all, move to a different parish (ZIP code), or to a different municipality. When they move, they are more likely to move 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 sale of one property to make a down payment 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. Naturally, we find that mobility is higher the larger the windfall conferred in the privatization process.

Third, we find a positive treatment effect on labor income and a weaker but positive effect on labor force participation. Home ownership induces households to work harder, both through an intensive and an extensive margin effect. This effect is surprising since the windfall confers a wealth increase which could have had a negative effect on labor supply. The treatment effects are stronger among movers who take on more debt upon conversion. We conclude that the labor supply response seems driven by the need to service a larger mortgage, consistent with Fortin (1995) and Del Boca and Lusardi (2003).

Fourth and most importantly, we study total consumption expenditures. We find an initially negative treatment effects on consumption. In the year of the co-op conversion, treated households choose to reduce consumption rather than taking a larger mortgage or reducing financial wealth by more. While this is inconsistent with standard consumption smoothing, this choice may be prompted by an aversion to high leverage. In the years following conversion, we find weak effects on spending for the stayers and strong positive effects on spending for movers. Stayers keep their illiquid housing wealth windfall illiquid. They refrain from borrowing against their ample home equity to fuel consumption, but rather pay off their mortgage. Movers, in contrast, increase spending considerably. Thus, we find

that consumption responses require liquidation of illiquid housing wealth. We estimate a marginal propensity to consume out of the (one-time) housing wealth windfall of 5% for stayers and 38% for movers. The stayers' MPC is consistent with MPC estimates from aggregate data and from models with complete insurance, while the MPC we find for movers is consistent with recent estimates from micro data for levered households and with richer life-cycle models with financial constraints and risky labor income.[5] We also find a small treatment effect on car purchases, again concentrated among the movers.

Our work relates to several strands of the literature. As mentioned before, 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 (Rossi-Hansberg, Sarte and Owens (2010)), better outcomes for children (Green and White (1997), Haurin, Parcel and Haurin (2002)), and more involvement with the local community (DiPasquale and Glaeser (1999)). Di Tella, Galiant and Schargrodsky (2007) find that giving households ownership rights to the land they inhabit affects their beliefs in free market ideals.

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. Nevertheless, our upward mobility results are consistent.

Finally, Vestman (2014) and Chetty, Sándor and Szeidl (2016) study how households' financial portfolios change around the time of house purchase. They interpret their results as households selling risky financial assets to provide for the downpayment on a house, which itself is a risky asset.[6]

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

[5]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).

[6]See Cocco (2005) for a theoretical framework and Davis and Van Nieuwerburgh (2015) for a review of the literature on housing and portfolio choice.

and construction in detail and we present a balance test for treatment and control groups. Section 4 contains our main difference-in-difference estimates. Section 5 studies how the main results vary by the size of the windfall households obtained. Section 6 concludes.

# 2 The Privatization Experiment

In this section, we briefly summarize the key features of the multi-family housing privatization experiment. Co-operatives, or co-ops, are legal entities of individuals that collectively own the multi-family 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 or city) landlord. Individual members of the co-op association own 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. 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 privatized 12,200 apartments in Stockholm 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 market-wide statistics.

In November 2001, the government proposed a law, known as *Stopplag*, which was passed by the parliament in March 2002 and went into effect on April 1, 2002. The law affected attempts to convert buildings owned by public landlords (municipalities or cities). It re-

quired the municipal landlords to ask for permission from the County Board to sell any part of their residential real estate.[7] One of the peculiarities of Sweden's rental housing market is that rental prices are set by bilateral negotiation between the landlord and the association of renters.[8] Until 2011, rental increases on apartments owned by municipal landlords served as the sole benchmark for the private rental market. Private landlords were not allowed to escalate rents faster than the increase in the municipal index.[9] The municipally-set benchmark rent index is specific to the neighborhood and the property type (e.g., 2-bedroom midrise apartment, 3-bedroom townhouse). As municipal co-op conversions increased dramatically in number in 2000 and 2001, the authorities became worried that too few municipally-owned properties might be left over to reliably calculate the rental price index for each neighborhood-property type combination. The intention of the *Stopplag* was to safeguard municipal landlords' ability to serve as yardstick for the rental market, area by area and property type by property type. Stopplag resulted in a dramatic slowdown in the pace of conversion of municipally-owned apartments in 2003 and 2004. Private landlords were not affected by the Stopplag and private conversions continued unabatedly.

The general election of September 2002 brought the return of the Social Democrats both nationwide and in Stockholm. 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. Our focus is on the first privatization wave because our high-quality wealth data end in 2007.

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

[8]Landlords often delegate the negotiation to an association of apartment owners , such as *SABO* or *Fastighetsä garna*.

[9]In general, the rent should equal the costs of maintaining the apartment (bruksvärdereglerna). The relevant part of the law is: "tillstånd till en överlåtelse skall inte lämnas om det kan befaras att de kommunala bostadsföretagens hyror till följd av överlåtelsen inte kommer att fåtillräckligt genomslag vid tillämpningen av bruksvärdereglerna i 12 kap. 55 jordabalken eller de hyresförhandlingar som förs enligt hyresförhandlingslagen (1978:304)." Since 2012 (which is after our sample ends), privately-owned apartments can also serve as a benchmark for rental price increases. In general, the rent should equal the costs of maintaining the apartment (bruksvärdereglerna). The relevant part of the law is: "tillstånd till en överlåtelse skall inte lämnas om det kan befaras att de kommunala bostadsföretagens hyror till följd av överlåtelsen inte kommer att fåtillräckligt genomslag vid tillämpningen av bruksvärdereglerna i 12 kap. 55 jordabalken eller de hyresförhandlingar som förs enligt hyresförhandlingslagen (1978:304)."

## 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.[10] Once registered, the co-op can submit a letter to the district court indicating its interest in purchasing the property. This will give the co-op a right of first-purchase for two years. Around the same time, the co-op will contact the landlord and 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.[11]

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.[12] 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. The co-op is under no obligation to reply to the offer and for some co-ops in our sample, the deadline expires without the landlord ever hearing back from the co-op association.

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).

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

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

[11] Private landlords generally had more flexibility in determining the asking price for co-op conversions.

[12] If the seller is one of the large Stockholm municipal landlords, the final asking price is determined by the Board of Directors of the municipal landlord based on input provided by the employees of the landlord and the external appraisal experts.

conversion to go ahead.[13] Upon a favorable vote, the co-op board communicates the vote tally and the minutes of the meeting to the landlord.[14]

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 additional approval from the County Board to sell the property.

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, while other conversions were able to go ahead. The example of the conversion attempts 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 loose its ability to determine rents on the kind of 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 can safely privatize two out of the four co-ops without jeopardizing that ability. The County Board is not guided by the law, nor does it have an established procedure for choosing between the buildings. 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 been chosen, such as approval based on the date when the contract with the landlord was signed or based on the highest voting share in favor among the tenants, would have resulted in a different outcome.

[13]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.

[14]Unfortunately, we cannot use this 2/3 threshold as an alternative RDD-based identification strategy as we observe substantial bunching on the right hand side of the threshold.

We use the passage of the Stopplag as an exogenous shock to the likelihood of approval of a co-op conversion, and hence to the likelihood of home ownership. Conditional on having signed a contract with the landlord, the Stopplag reduced the likelihood of conversion from 100 percent to 25 percent. Unconditionally, the likelihood of success was reduced from 50 percent to 12 percent.[15]

## 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 net operating income, rental income minus operating expenses, using a standard interest rate. Since municipal landlords charge their tenants below-market rents, the buildings were sold to the co-ops at a discount to their private market value. Rather the properties were valued as if the buyer would be another landlord subject to the same rent regulation as the selling landlord. The discount equals the difference between the private market value and the present value of regulated rents.

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* and keep their old regulated rent, which they now pay to the co-op. If they convert, tenants 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 the experiment is that, because the one-time conversion fee is below the market value of the unit on the private co-op 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 transfer fee. Also, conversion had typically no cash-flow implications. The monthly regulated rent was about equal to the monthly co-op dues plus the personal mortgage payment. Combined

[15]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 prior to April 1st 2002 when the Stopplag became effective.

with 100% financing of the conversion fee, this cash-flow equivalence implies that there are no mechanical cash flow implications from conversion. 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 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 verusus renting for multiple horizons in a concrete example.

# 3 Data

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

## 3.1 Sources

The Hitta BRF datset contains information on the universe of all co-ops that register with the Bolagsverket agency between 1995 and 2013. Each co-op has a unique identifier which is used in any official transaction of the co-op. The data set we assemble contains co-op level information such as date of registration and the names and addresses of the co-op's board members. For those co-ops that acquire a property, the dataset contains the property identifier from the Swedish real property register. For our main sample of municipally-owned properties, we have exact addresses of the property that the co-op attempted but ultimately failed to acquire.

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.

Third, we obtain County Board protocols and dates of Stopplag decisions for each co-op.

Fourth, we link the properties that were subject to a conversion attempt to their tenants using the exact property address. 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, combined with asset 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.

## 3.2 Sample of co-ops

We focus on the subsample of 38 co-ops affected by Stopplag.[16] 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 Administrative Board under Stopplag. The County Board decisions took place be-

[16]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.

tween 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 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 $k = 1, \cdots, 4$.[17]

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

[17]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.

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 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.[18] The new households that are added to the sample due to life changes after (before) RY0 inherit the treatment flag of their predecessor

[18]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.

(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 households whose adult composition changes before or after the household formation year. In this *Fixed* household subsample, no new households are added before or after the household formation year. The number of households is the same in the *Fixed* and *All* samples in the household formation year. In all years before and after that year, the number of households in the *Fixed* sample is strictly smaller than in the household formation year (while it is strictly larger in the *All* sample). The *Fixed* sample drops all singles who marry before RY0 and all married households who divorce after RY0. If two adults who are not married co-habit, unbeknownst to us, the *All* sample missclassifies them as two separate households until they get married or have a child together.[19] The *Fixed* household sample drops such households (and avoids the mistake) because their adult composition changes during the sample.[20] 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.[21] 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.

[19]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).

[20]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.

[21]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.[22] *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

[22]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.[23] 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.[24] 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

[23]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.

[24]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%).

information. We have information for every stock, mutual fund, and money market fund for every individual in our sample. We also have information on the total value invested in bonds for each individual. 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.[25] 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.[26] 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

[25]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).

[26]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.

2007. Consumption is measured as the right-hand side of the budget constraint:

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

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.[27] 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.[28]

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). 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

[27]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.

[28]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.

Table 2: Balance Test at Household Level

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | All | | | Fixed | |
| | Treated | Control | T-C | Treated | Control | T-C |
| Homeowner | .039 | .038 | .000 | .033 | .037 | -.004 |
| | (.193) | (.192) | (.010) | (.180) | (.190) | (.009) |
| Age | 44.110 | 43.094 | 1.016 | 44.536 | 43.408 | 1.128 |
| | (9.777) | (10.710) | (.758) | (9.656) | (10.631) | (.779) |
| Partner | .371 | .294 | .076 | .372 | .296 | .075 |
| | (.483) | (.456) | (.047) | (.483) | (.456) | (.047) |
| Anymove | .071 | .083 | -.012 | .060 | .078 | -.018* |
| | (.257) | (.277) | (.009) | (.238) | (.269) | (.009) |
| Numwork | 1.416 | 1.310 | .106* | 1.424 | 1.308 | .116** |
| | (.756) | (.786) | (.053) | (.766) | (.782) | (.054) |
| Cars | .126 | .140 | -.013 | .126 | .141 | -.014 |
| | (.332) | (.347) | (.012) | (.332) | (.348) | (.012) |
| Labincind (kSEK) | 216.5 | 201.1 | 15.4 | 217.8 | 200.3 | 17.4 |
| | (141.9) | (149.5) | (12.8) | (141.3) | (148.9) | (13.0) |
| Income (kSEK) | 178.7 | 173.8 | 4.9 | 180.0 | 173.9 | 6.0 |
| | (103.4) | (89.4) | (9.1) | (103.7) | (88.8) | (9.6) |
| Debt (kSEK) | 110.7 | 103.4 | 7.3 | 108.3 | 102.0 | 6.3 |
| | (232.3) | (180.5) | (15.4) | (235.4) | (179.1) | (15.4) |
| House (kSEK) | 31.4 | 24.3 | 7.1 | 26.4 | 22.5 | 3.9 |
| | (198.7) | (183.7) | (11.5) | (181.8) | (172.8) | (9.5) |
| Nonhouse (kSEK) | 55.3 | 47.3 | 7.9 | 56.9 | 47.2 | 9.7 |
| | (182.0) | (188.8) | (15.0) | (186.8) | (189.9) | (15.7) |
| Risky (kSEK) | 65.7 | 45.0 | 20.6 | 67.0 | 45.3 | 21.7 |
| | (329.1) | (202.8) | (16.1) | (338.1) | (206.4) | (17.1) |
| Cons (kSEK) | 164.4 | 160.6 | 3.7 | 164.6 | 160.5 | 4.1 |
| | (120.2) | (120.8) | (9.6) | (117.3) | (119.8) | (9.9) |
| N | 1560 | 3014 | | 1451 | 2885 | |

*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 last three columns for the Fixed sample with stable adult composition. 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.

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 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.

# 4 Analysis and Results

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:

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

where $\alpha$ 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. The indicator variables $RY_i(t=k)$ indicate the year relative to the conversion decision. The coefficients $\gamma$ trace out the dynamics of the outcome variable for the control group. The main coefficients of interest are $\delta_0, ..., \delta_3, \delta_4$. They measure the treatment effect in the conversion year and the four years that follow. Recall that the decision year is not the same for all households so this is a staggered treatment. The assumption on parallel trends in the pre-treatment period can be evaluated by inspecting the pre-treatment estimates $\delta_{-4}, \delta_{-3}, \cdots, \delta_{-1}$. Calendar year fixed effects, $\psi_t$, control for the aggregate trends in the outcome variables. Building fixed effects, $\omega_b$, 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.[29]

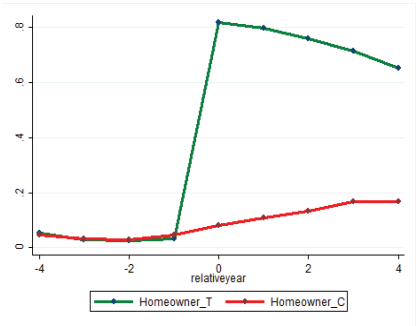
As is standard in difference-in-difference specifications like (2), one interaction term and one RY term are not identified. We drop the terms $Convert_i RY_i(t=-1)$ and $RY_i(t=-1)$. This allows us to interpret all $\delta$ estimates relative to the household formation year. Put differently, the $\delta$ coefficients are rescaled so that the treatment and control groups have the same outcome variable in RY-1, conditional on the controls.[30]

This section reports estimates of (2) for various outcomes $y_i$. We begin by analyzing

[29] 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.

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

Figure 1: Home Ownership




Raw home ownership rates for treatment and control groups. The sample is the All sample.

"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 6, we study the treatment effect on the treated, looking at those who actually take up the offer to convert.

## 4.1 Home Ownership

As a first-stage effect, we investigate the effect of treatment on home ownership. Figure 1 plots the raw home ownership rate for treatment and control groups for the All sample. It shows essentially identical home ownership rates pre-treatment, confirming the results of the balance test. The home ownership rate in the household formation year RY-1 is slightly lower for the treatment group. Since the regression results express all treatment effects relative to the effect in RY-1, this leads to slightly positive treatment effects in RY-4, RY-3, and RY-2. Column 1 of Table 3 shows that the ITT estimates for RY-4, RY-3, and RY-2 are economically small and statistically insignificant. Furthermore, any positive differences in home ownership pre-treatment would bias us against finding a strong treatment effect.

Figure 1 shows the massive jump in home ownership in the conversion year RY0. The treatment group reaches a home ownership rate of nearly 80%. In the four years following treatment, home ownership gradually reverses back to about 65%. The control group sees a gradual increase in home ownership, consistent with these households buying property as they age and have children, and consistent with the rising home ownership rate in Sweden

during this period. Four years after the decision, the control group's home ownership rate remains below 20%.

Column (1) of Table 3 shows the same results more formally, controlling for year and building fixed effects as well as for household age, partnership, and education, all of which are naturally associated with home ownership. It shows the treated increase home ownership by 75% points in RY0, relative to the control group and relative to the prior year RY-1. The treatment effect gradually attenuates to a 50% point difference in home ownership by RY+4. The attenuation is driven by gradually increasing home ownership rates among the control group as well as by treated households who sell their apartment after conversion and return to rentership. In other words, our experiment has a strong first stage and causes massive and persistent differences in home ownership between treatment and control groups.

Column (2) of Table 3 shows that the treatment effect on home ownership is even stronger among households whose adult composition does not change over the course of our sample period. This sample eliminates households who go through a divorce and households whose adult children who form their own household, explaining the higher home ownership effects. The initial effect is a relative increase in home ownership of 83% points, which attenuates to 63% points over the next four years.

Columns (3) and (4) break the Fixed household sample into subsamples of Stayers and Movers. Stayers represent 68% of the Fixed sample household-year observations; Movers account for the rest. We recall that Movers are all those households who move at some point during the RY0 to RY+4 period. While the moving decision is clearly endogenous, it is an interesting way to split the sample. We find that the initial home ownership effect is stronger for Stayers (88%) than Movers (73%). The effect is quasi-permanent for the Stayers, while it declines strongly for Movers. Nevertheless, the treated Movers are still 23% points more likely to own four years later, relative to the control group of Movers and relative to household formation year RY-1. Thus, while some movers in the treatment group sell their newly obtained apartment and revert to rentership and some movers in the control group become home owners, a large treatment effect remains several years later even among the most mobile of households.

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-4 | 0.0209 | 0.0217 | 0.00918 | 0.0470** | 0.000996 | 0.0211 | 0.0150 | 0.0393** | -0.0527 | -0.0413 | -0.0299 | -0.0372 |
| | (1.35) | (1.38) | (0.60) | (2.11) | (0.05) | (1.53) | (0.95) | (2.14) | (-1.22) | (-1.17) | (-1.07) | (-0.47) |
| RY-3 | 0.0134 | 0.0130 | 0.0116 | 0.0145 | 0.0256* | 0.0278** | 0.0193* | 0.0447** | 0.00977 | -0.0206 | -0.00527 | -0.0371 |
| | (1.51) | (1.56) | (1.42) | (0.99) | (1.78) | (3.26) | (1.88) | (3.17) | (0.29) | (-0.67) | (-0.21) | (-0.62) |
| RY-2 | 0.00909 | 0.00770 | 0.000287 | 0.0204* | 0.0161 | 0.0136** | 0.00290 | 0.0325** | 0.0279 | -0.00186 | 0.0183 | -0.0377 |
| | (1.35) | (1.13) | (0.04) | (1.94) | (1.59) | (2.15) | (0.44) | (2.46) | (1.29) | (-0.10) | (1.01) | (-1.36) |
| RY0 | 0.750*** | 0.827*** | 0.880*** | 0.733*** | 0.00248 | 0.0159 | 0.0315** | -0.0152 | -0.0339 | -0.0299 | 0.00464 | -0.0894 |
| | (21.04) | (21.12) | (24.61) | (11.25) | (0.17) | (1.46) | (2.37) | (-1.06) | (-1.01) | (-1.43) | (0.25) | (-1.60) |
| RY+1 | 0.706*** | 0.790*** | 0.865*** | 0.635*** | 0.000503 | 0.0124 | 0.0156 | -0.00409 | -0.00464 | 0.000831 | 0.0187 | -0.0474 |
| | (30.99) | (31.42) | (29.23) | (13.40) | (0.04) | (1.45) | (1.46) | (-0.28) | (-0.13) | (0.05) | (0.79) | (-0.83) |
| RY+2 | 0.645*** | 0.747*** | 0.847*** | 0.523*** | 0.0214 | 0.0209** | 0.0319** | -0.0125 | 0.0138 | 0.0131 | 0.0288 | -0.0578 |
| | (28.88) | (37.13) | (30.62) | (14.66) | (1.02) | (2.12) | (2.99) | (-0.65) | (0.27) | (0.48) | (0.81) | (-0.97) |
| RY+3 | 0.567*** | 0.685*** | 0.821*** | 0.379*** | 0.0103 | 0.0210* | 0.0328** | -0.0114 | 0.0101 | 0.0189 | 0.0475 | -0.0633 |
| | (22.68) | (30.73) | (29.25) | (11.45) | (0.49) | (1.91) | (2.78) | (-0.54) | (0.17) | (0.55) | (1.18) | (-0.97) |
| RY+4 | 0.502*** | 0.631*** | 0.809*** | 0.229*** | 0.0000186 | 0.0153 | 0.0247 | -0.0133 | -0.0339 | -0.0210 | -0.0119 | -0.0676 |
| | (15.68) | (22.41) | (25.18) | (4.36) | (0.00) | (1.09) | (1.64) | (-0.47) | (-0.35) | (-0.36) | (-0.19) | (-0.81) |
| 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 |
| $R^2$ | 0.423 | 0.535 | 0.672 | 0.397 | 0.106 | 0.121 | 0.136 | 0.157 | 0.105 | 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 building level. The table reports the coefficients $\delta_k$ 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 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.

## 4.2 Demographics

Next, we investigate whether conversion 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.[31] In the All sample (column 5), there are no significant differences at the 5% level 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.[32] 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) with number of adults as dependent variable is of little interest, except to serve as a diagnostic on how the family *composition* of treatment and control groups fluctuates over the nine sample years. Relative to the household formation year RY-1, the households in the treatment group have a slightly higher average number of adults than the households in the treatment group both before and after the household formation year. The differences are never higher than 0.028 adults, which is small compared to a pre-treatment sample average of 1.32 adults per household with a standard deviation of 0.46. The differences after conversion are coming from the Stayers. Among the Movers, there are more adults per household (fewer singles) in the treatment group than in the control group before treatment, while the reverse is true after treatment (more singles). Again, the differences are small. We conclude that our sample composition remains nicely balanced throughout the full estimation window. Nevertheless, in 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 .034 children in RY0, but the effect is imprecisely estimated. The initial negative effect on children is driven by the Movers for whom the 0.089 drop constitutes 13% of the average number of children in the pre-

[31] For these two outcome variables only, we omit partnership as a control variable.

[32] We confirm this by estimating separate regressions with the divorce rate and marriage rate as the dependent variable. These results are available upon request.

treatment period (Column 12). It is consistent with the finding that there are fewer adults per household among treated movers than control movers in RY0 and beyond (Column 8). 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. Most of our households are beyond prime child bearing years. If so, a more appropriate interpretation of the results in columns (5)-(12) is that the overall family composition remains balanced throughout the experiment.

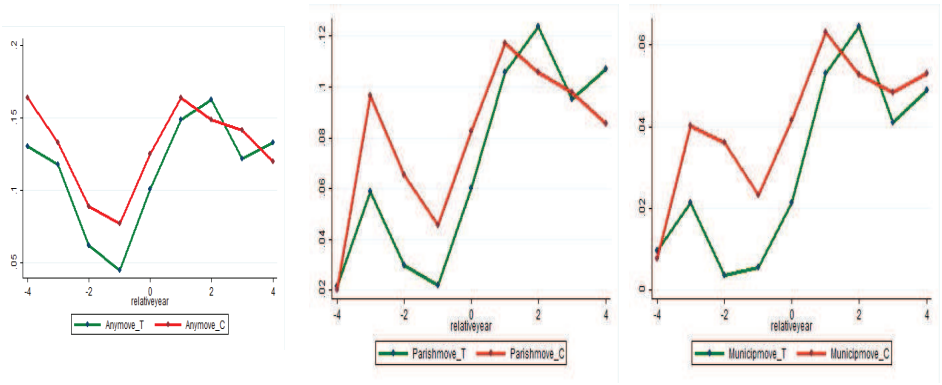
## 4.3 Mobility

Does home ownership increase household geographic mobility? Columns (1)-(6) of Table 4 find that it does. We study three different definitions of geographic mobility, based on exact address (Anymove), parish of residence (Parishmove), and municipality of residence (Municipmove). Starting with the broadest measure of mobility in columns (1) and (2), we find no significant pre-trends. The left panel of Figure 2 confirms the parallel pre-trends. In contrast, we find positive and significant effects of treatment (home ownership) on mobility in the years following conversion, both in the All and in the Fixed sample. The effects are large. On a baseline moving rate of 11% points per year in the pre-treatment years, the moving rate increases by as much as 47% (5.2% points) for the treated in RY+2. The effects in the Fixed sample are even larger with a 68% increase in mobility (6.8% points) relative to a pre-treatment moving rate of 10% in this sample. Even in RY+4, there is a large effect of 5.2% points in the All and 7.5% points in the Fixed sample. In the year of treatment, RY0, there is lower mobility for the treated, at least in the Fixed sample. This seems natural if the treated are preoccupied with the conversion process (getting a mortgage, etc.). Figure 2 clarifies that both treated and control increase mobility in the conversion year and beyond relative to RY-1, but the overall increase is larger for the treated.

Columns (3) and (4) show large and significant effects on inter-parish mobility in several post-treatment years. The 4.6% and 5.7% point higher moving rates in RY+1 and RY+2 for the Fixed sample are economically large since the average inter-parish moving rate in the pre-treatment years is only 4% points per year. The effect is also significant in RY+3 and RY+4 years in the Fixed sample.

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

Figure 2: Moving Rates: Any, Parish, Municipality




Left panel: annual moving rate based on address (narrow geography). Middle panel: annual moving rate to different parish (intermediate geography). Right panel: Moving rate to different municipality (broadest geography). The sample is the All sample.

estimate a treatment effect of 3.4% in the All sample and 5.3% in the Fixed sample in RY+2. The treatment effect on municipal moving rates for the Fixed sample represents a five-fold increase over the baseline level, and relative to the control group. The effect is also significant in RY+1 and RY+4 in the Fixed sample.

Our results indicate that home ownership strongly and persistently promotes household mobility. This effect of 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. We later investigate how the treatment effect on mobility differs across various levels of the windfall received upon conversion.

Having established higher inter-parish mobility rates post-conversion, we now study *where* movers move to. 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 4 show that treatment increases upward economic mobility. The probability that a household moves to a parish with higher house house prices in RY+1 or RY+2 is 2.6-4.4% points higher for treatment than for control in every year between RY+1 and RY+4 in the All sample. This is a large economic effect in light of the pre-treatment average upward mobility rate

Table 4: 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-4 | -0.0197 | -0.0163 | 0.0147 | 0.0215** | 0.0167** | 0.0202** | 0.00900 | 0.00834 | 0.0123 | 0.0140 |
| | (-1.04) | (-0.82) | (1.38) | (2.46) | (2.29) | (2.94) | (1.11) | (1.06) | (1.29) | (1.54) |
| RY-3 | 0.0189 | 0.0330 | -0.00295 | 0.0175 | 0.00271 | 0.0132 | -0.00250 | 0.00106 | -0.0132 | -0.00995 |
| | (0.81) | (1.39) | (-0.17) | (1.06) | (0.27) | (1.35) | (-0.23) | (0.10) | (-1.13) | (-0.95) |
| RY-2 | 0.00882 | 0.0220 | -0.00860 | 0.00424 | -0.00954 | -0.00268 | -0.00857 | -0.00366 | -0.00656 | -0.000898 |
| | (0.56) | (1.43) | (-0.76) | (0.43) | (-1.34) | (-0.46) | (-1.03) | (-0.47) | (-0.66) | (-0.10) |
| RY0 | 0.00901 | -0.00890 | 0.00127 | -0.00447 | -0.000846 | -0.000504 | 0.0110 | 0.00764 | -0.00110 | 0.00585 |
| | (0.60) | (-1.01) | (0.08) | (-0.43) | (-0.10) | (-0.08) | (1.01) | (0.98) | (-0.10) | (0.82) |
| RY+1 | 0.0264 | 0.0537** | 0.0218 | 0.0457** | 0.0138 | 0.0338*** | 0.0440** | 0.0472** | 0.0372** | 0.0396** |
| | (1.23) | (2.66) | (1.21) | (2.85) | (1.43) | (3.73) | (2.38) | (2.76) | (2.31) | (2.54) |
| RY+2 | 0.0524** | 0.0681** | 0.0459* | 0.0571** | 0.0343** | 0.0527** | 0.0314** | 0.0361** | 0.0338** | 0.0403** |
| | (2.47) | (3.27) | (2.00) | (3.18) | (2.29) | (3.52) | (2.54) | (2.54) | (2.40) | (2.68) |
| RY+3 | 0.0159 | 0.0362 | 0.0229 | 0.0353** | 0.0132 | 0.0209 | 0.0266* | 0.0245* | 0.0202 | 0.0299** |
| | (0.82) | (1.48) | (1.49) | (2.06) | (1.44) | (1.68) | (1.98) | (1.93) | (1.56) | (2.34) |
| RY+4 | 0.0515** | 0.0747*** | 0.0415** | 0.0661*** | 0.0169* | 0.0345** | 0.0379** | 0.0430** | 0.0366** | 0.0495** |
| | (2.87) | (4.86) | (3.10) | (4.45) | (1.86) | (3.40) | (3.13) | (3.47) | (2.47) | (3.28) |
| 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 |
| $R^2$ | 0.0756 | 0.0445 | 0.0699 | 0.0394 | 0.0401 | 0.0251 | 0.0269 | 0.0201 | 0.0308 | 0.0241 |

*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 $\delta_k$ 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.

of 2% points per year. The treatment effects for the Fixed sample are even larger, with the RY+1 effect representing a 235% increase in upward mobility over the pre-treatment level. The effects are qualitatively and quantitatively similar if we define upward economic mobility in terms of the average income of the destination parish.[33] In other words, the conversion process helps households climb up the property ladder. Once converters own their apartment, they can sell it and use the proceeds to make a down payment 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. We study later how much the upward mobility effect varies with the size of the windfall.

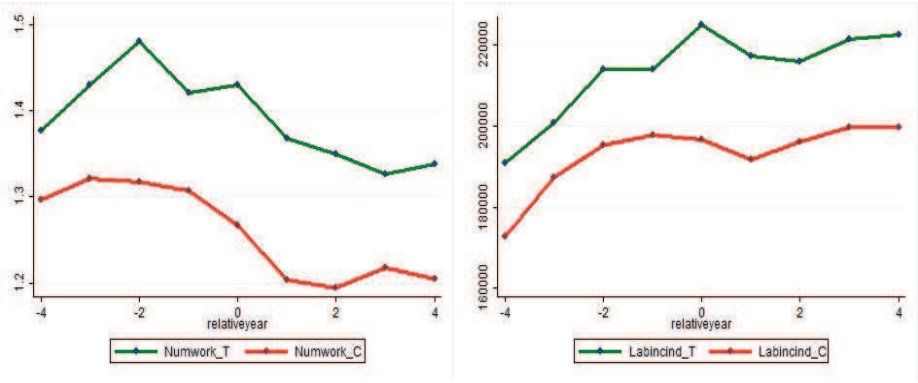
## 4.4 Labor Income and Labor Supply

Next, we ask 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 down payment. 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 1-4 of Table 5), which combines the extensive margin effect on the number of adults that are working (Numwork) with the intensive margin effect on labor income per adult (Labincind). For this

[33] 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.

Figure 3: Labor Force Participation and Labor Income Per Adult




Left panel: Number of workers per household (Numwork). Right panel: Labor income per adult (Labincind). The sample is the All sample.

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.[34] 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 All sample is an increase of 12,250 SEK or about 1,500 USD per person in household labor income. and one year after treatment. The effect represents a 6.5% 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 falls by half and is no longer statistically significant. In the Fixed sample, the treatment effect in RY0 is larger than in the All sample at almost 16,000 SEK or 2,000 USD. The positive labor income effect is short-lived; it loses statistical significance in RY+1 and all but disappears in years RY+2 and beyond.

Both extensive and intensive margins contribute to the labor earnings effect. Columns (5) and (6) of Table 5 show a positive treatment effect in RY0, RY+1, and RY+2 on the number of adults. The initial effect in RY0 is statistically significant in both the All and Fixed samples. The treatment effect in RY+1 represents a 3.4% increase for the All and a 2.5% increase for the Fixed sample over their respective average pre-treatment levels. The increase in labor force participation post-conversion lasts for two years before attenuating. The post-conversion effects are too imprecisely measured to be statistically significant. The

[34]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.

Table 5: ITT Estimation - Labor Force Participation and Earnings Results

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Labinchh under age 64 | | | | Numwork under age 64 | | | | Labincind under age 64 | | | |
| Samples | All | Fixed | Stayers | Movers | All | Fixed | Stayers | Movers | All | Fixed | Stayers | Movers |
| RY-4 | 5831.5 | 13479.5* | 7542.2 | 20875.9 | -0.0104 | 0.0152 | -0.0447 | 0.125** | 5408.4 | 11319.4 | 4889.2 | 20661.2* |
| | (0.93) | (1.73) | (0.80) | (1.57) | (-0.34) | (0.48) | (-1.16) | (2.06) | (1.02) | (1.66) | (0.54) | (1.77) |
| RY-3 | -3866.9 | 847.7 | -4786.8 | 7113.7 | -0.0101 | 0.00867 | -0.0351 | 0.0827** | 169.1 | 2151.6 | -1986.2 | 6089.0 |
| | (-0.98) | (0.19) | (-0.76) | (0.90) | (-0.34) | (0.30) | (-0.76) | (2.46) | (0.04) | (0.49) | (-0.34) | (0.61) |
| RY-2 | -1438.8 | 360.9 | -7115.8 | 10974.7 | 0.0349 | 0.0395 | 0.0257 | 0.0541 | 1387.2 | 2222.8 | -2495.0 | 7962.9 |
| | (-0.27) | (0.07) | (-1.20) | (1.23) | (1.58) | (1.68) | (0.83) | (1.47) | (0.28) | (0.48) | (-0.49) | (0.94) |
| RY0 | 12249.6** | 15931.5** | 11334.1** | 23164.1** | 0.0383* | 0.0285* | 0.0280 | 0.0209 | 12810.4** | 15814.7** | 10902.2** | 23654.3** |
| | (3.03) | (3.41) | (2.55) | (2.76) | (1.99) | (1.87) | (1.40) | (0.70) | (3.03) | (3.33) | (2.41) | (2.81) |
| RY+1 | 5981.1 | 9714.9 | 6644.6 | 14071.4* | 0.0447 | 0.0337 | 0.00648 | 0.0723 | 7653.4 | 10878.0* | 7383.7 | 15858.0* |
| | (1.06) | (1.63) | (0.93) | (1.75) | (1.57) | (1.28) | (0.21) | (1.35) | (1.36) | (1.97) | (1.16) | (1.91) |
| RY+2 | 62.52 | 1559.0 | 4510.0 | -5101.8 | 0.0257 | 0.0508 | 0.0446 | 0.0402 | 2191.9 | 2024.4 | 4835.9 | -5532.6 |
| | (0.01) | (0.19) | (0.43) | (-0.41) | (0.75) | (1.35) | (1.08) | (0.64) | (0.33) | (0.26) | (0.52) | (-0.42) |
| RY+3 | -1363.5 | 1380.0 | -4102.1 | 9477.3 | -0.00581 | -0.00150 | -0.0346 | 0.0410 | -329.3 | 1612.9 | -1372.5 | 3884.7 |
| | (-0.18) | (0.15) | (-0.34) | (0.77) | (-0.16) | (-0.04) | (-0.67) | (0.67) | (-0.04) | (0.18) | (-0.12) | (0.30) |
| RY+4 | -5311.2 | 2833.1 | -2572.0 | 9567.7 | 0.0124 | 0.00599 | -0.0910 | 0.185** | -6516.2 | 3079.6 | -41.80 | 2518.6 |
| | (-0.67) | (0.33) | (-0.22) | (0.57) | (0.34) | (0.14) | (-1.35) | (2.50) | (-0.86) | (0.37) | (-0.00) | (0.16) |
| PT-Mean | 187430.7 | 186999.4 | 182161 | 196850.7 | 1.34 | 1.34 | 1.34 | 1.32 | 193846.4 | 193978.6 | 187839.9 | 206477.5 |
| PT-SD | 156323.3 | 151304.8 | 144957.9 | 163055.6 | .78 | .78 | .81 | .71 | 147829.7 | 143536.4 | 137187.1 | 154945 |
| N | 17703 | 14536 | 9835 | 4701 | 17703 | 14536 | 9835 | 4701 | 17703 | 14536 | 9835 | 4701 |
| $R^2$ | 0.108 | 0.107 | 0.119 | 0.151 | 0.405 | 0.400 | 0.403 | 0.427 | 0.105 | 0.115 | 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 $\delta_k$ 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.

left panel of Figure 3 shows the raw labor force participation data for treatment and control. The treatment group's labor supply does not fall as much as that of the control group between RY-1 and RY+2.

Columns (9) and (10) show an increase in labor income per working adult of 12,800 and 15,800 SEK, for All and Fixed samples resp. in RY0, representing 6.5-8.2% of annual pre-treatment labor income per adult. Both effects are significant at the 5% level. For the All sample, the effect loses significance in RY+1 while for the Fixed sample it remains significant in RY+1 and disappears afterwards. The right panel of Figure 3 shows the raw labor income per adult comparison for treatment and control groups. The graph shows parallel pre-trends, an increase in income for treatment group between RY-1 and RY+1, but a decrease for the control group over the same period. Overall, the intensive margin effect contributes more to the overall positive labor income effect of home ownership than the extensive margin effect.

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 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.

## 4.5 Consumption

Our main variable of interest is spending and savings. Two key benefits of home ownership that are often pointed out are, first, that home ownership induces households to save, and second, that the house is an important source of collateral that can be borrowed against to smooth consumption across states of the world. There is a long line on research on the propensity of households to consume out of housing wealth. Much of that literature suffers from a lack of identification since both housing wealth and consumption are endogenous objects. Nearly all of the literature suffers from a lack of high-quality consumption data. Often aggregate consumption and income are used instead of household-level data. Household-level consumption is often approximated by car purchases or credit card spending or impre-

cisely measured based on survey data.[35] We are the first to build high-quality consumption from registry-based data, following the procedure outlined in our earlier work Koijen, Van Nieuwerburgh and Vestman (2014), and to relate that consumption to home ownership and housing wealth at the level of the household in the context of a quasi-experiment.[36]

**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. Columns (1)-(4) of Table 6 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 years RY+3 and RY+4 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, 11.5% points in RY+3, and 7.1% points in RY+4 among the Movers. THe RY+2 effect is significant at the 5% level. These are 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.[37] We find only the weakest 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.

[35]The only exception we are aware of is the *mint.com* data employed by Baker (2015).

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

[37]In 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.

Table 6: ITT Estimation - Car Purchases, Consumption, and Savings

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Car purchases | | | | Consumption | | | | Savings | | | |
| Samples | All | Fixed | Stayers | Movers | All | Fixed | Stayers | Movers | All | Fixed | Stayers | Movers |
| RY-4 | -0.0335* | -0.0327 | -0.0317** | -0.0370 | | | | | | | | |
| | (-1.77) | (-1.66) | (-2.33) | (-0.77) | | | | | | | | |
| RY-3 | 0.00420 | -0.00571 | -0.0331 | 0.0532 | -4058.3 | -2959.7 | -69.42 | -7686.8 | 2797.0 | 2834.0 | -2288.1 | 9883.9 |
| | (0.21) | (-0.28) | (-1.36) | (1.52) | (-0.46) | (-0.30) | (-0.01) | (-0.54) | (0.34) | (0.34) | (-0.31) | (0.62) |
| RY-2 | 0.00598 | -0.00163 | -0.0211 | 0.0403 | -4218.9 | -6118.1 | -6375.9 | -3081.6 | 5030.6 | 6783.3 | -139.9 | 15977.2 |
| | (0.18) | (-0.05) | (-0.70) | (0.74) | (-0.53) | (-0.71) | (-0.81) | (-0.20) | (0.72) | (0.89) | (-0.02) | (1.05) |
| RY0 | 0.0287 | 0.0189 | 0.0201 | 0.0203 | -14540.9* | -16475.5* | -13913.9 | -17267.7 | 23386.5** | 27245.4** | 21611.5* | 33536.1** |
| | (1.15) | (0.74) | (0.73) | (0.50) | (-1.73) | (-1.87) | (-1.14) | (-1.34) | (2.77) | (3.12) | (1.93) | (2.39) |
| RY+1 | 0.0175 | 0.00728 | -0.00323 | 0.0287 | 13434.6 | 9344.9 | 2647.9 | 26730.6 | -12246.5 | -6446.3 | 507.5 | -24579.7 |
| | (0.77) | (0.30) | (-0.12) | (0.67) | (1.34) | (0.85) | (0.36) | (1.15) | (-1.48) | (-0.75) | (0.10) | (-1.21) |
| RY+2 | -0.000419 | -0.00334 | -0.0256 | 0.0492 | 7618.8 | 7578.7 | -2858.3 | 35795.0* | -6224.0 | -4416.1 | 6743.0 | -35613.7** |
| | (-0.02) | (-0.16) | (-1.27) | (1.03) | (0.93) | (0.82) | (-0.30) | (1.91) | (-0.93) | (-0.56) | (0.89) | (-2.16) |
| RY+3 | 0.0168 | 0.0324 | -0.00600 | 0.115** | -977.6 | -1997.1 | 4130.2 | -12137.9 | 213.2 | 3624.7 | -2067.4 | 11275.7 |
| | (0.78) | (1.33) | (-0.31) | (2.28) | (-0.12) | (-0.20) | (0.45) | (-0.54) | (0.04) | (0.44) | (-0.24) | (0.62) |
| RY+4 | 0.0112 | 0.0313 | 0.0135 | 0.0708 | 14695.3 | 22101.1* | 9096.4 | 52474.6** | -19225.4** | -24091.0** | -7130.1 | -65094.7*** |
| | (0.55) | (1.41) | (0.59) | (1.15) | (1.39) | (2.02) | (0.83) | (2.25) | (-2.63) | (-3.30) | (-0.77) | (-3.58) |
| PT-Mean | .14 | .13 | .12 | .15 | 160564.3 | 160516.6 | 158564.7 | 164478.6 | 5744.4 | 6377.56 | 7048.26 | 5016.12 |
| PT-SD | .34 | .34 | .33 | .36 | 119201.4 | 117626.5 | 112600.3 | 127158.6 | 93471.05 | 92889.09 | 86163.09 | 105239.5 |
| N | 18284 | 15076 | 10273 | 4803 | 16199 | 13370 | 9165 | 4205 | 16199 | 13370 | 9165 | 4205 |
| $R^2$ | 0.0367 | 0.0416 | 0.0468 | 0.0547 | 0.0620 | 0.0652 | 0.0764 | 0.0782 | 0.0138 | 0.0154 | 0.0207 | 0.0417 |

*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 $\delta_k$ 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.

**Initial Effect on Consumption and Saving** Columns (5)-(8) of Table 6 display the treatment effects on total consumption spending. Because consumption is constructed from variables that involve changes, we lose one year of observations (RY-4). The budget constraint (1) states that consumption equals income minus savings, where savings is the change in financial and 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 current income lowers consumption. A decrease in household net worth (dissaving) generates an increase in consumption, absent a change in income. The ITT effects on Savings are reported in Columns (9)-(12) of Table 6. Table 7 breaks consumption down into its four components: changes in debt, changes in residential real estate wealth, changes in financial wealth (including commercial real estate and second homes), and income. For parsimony, this table only reports results for the Fixed sample. All results for consumption and its components are expressed per adult equivalent. Pre-treatment consumption and its four components all show parallel pre-trends.

The RY0 effect on consumption is negative and significant in the All and Fixed samples. The point estimate of -14,500 SEK represents a drop of 9.1% 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 initial treatment effect in the Fixed sample is larger still at -16,500 SEK. Columns (9) and (10) show that the treated save 23,400-27,200 SEK more than the control group in the All and Fixed samples, respectively. This represents a nearly five-fold increase over the pre-treatment average savings. Why does home ownership prompt an initial decline in spending and increase in savings?

Table 7 presents a four-way breakdown of the -16.5k SEK treatment effect on consumption for the Fixed sample in RY0. It results from a 337k SEK increase in debt, a 376k SEK increase in housing wealth, a 12k SEK decrease in financial wealth, and disposable income of 10.8k SEK.[38] Combining the increase in housing wealth and debt, and assuming that the entire increase in debt is attributable to debt collateralized by real estate, the treatment effect on home equity is 39.2k SEK. To make this down payment, households reduce their financial

[38]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 sale. This is to avoid mechanical valuation effects on consumption.

wealth (draw down bank accounts, sell stocks, mutual funds, and bonds) by 12k SEK. The 27.2k SEK difference between the increase in net housing wealth and the decline in financial wealth is the aforementioned increase in savings. The difference between the treatment effect on disposable income (10.8k) and that on savings (27.2k) is another way to account for the treatment effect on consumption (-16.5k). The treatment effects in RY0 are significant for all four consumption components.

It is important to note that the reduction in consumption in the conversion year is voluntary. Because converters were able to buy property at prices below the market value -below we detail that the windfall averaged 650k SEK- they could have obtained a much larger mortgage if they had wished. For example, 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. Instead, converters chose to limit the size of the mortgage and pay for the down payment by reducing financial wealth and by reducing consumption. This clearly illustrates some notion of leverage- or debt-aversion, at odds with standard consumption smoothing motives.

The RY0 decline in consumption is not that different for Stayers (-13.9k SEK) than for Movers (-17.3k SEK). Treated Stayers increase savings by less (+21.6k) than the treated Movers (+33.5k). Stayers choose a larger down payment than Movers (42.6k vs. 32.6k) but reduce financial wealth by more (-21k vs. +1k), and have a much smaller treatment effect on disposable income (+7.7k vs 16.3k). The higher leverage Movers choose in RY0, or equivalently the lower share of housing equity in net worth, signals their intention to Move in RY0 or later.

**Subsequent Effect on Consumption and Saving** In the years after conversion, our main finding is a positive effect on consumption, driven by the Movers. The treatment effect in the All sample is +13.4k SEK in RY+1 and +7.6k SEK in RY+2 (Column 5 of Table 6). These represent 8.4% and 4.7% of average pre-treatment consumption. We also find a positive consumption effect of +14.7k SEK in RY+4. The cumulative consumption response in years RY+1 to RY+4 is 35k SEK. The treatment effects are similar for the Fixed sample, except for the +22k effect in RY+4 which turns significant (column 6). The cumulative

Table 7: ITT Estimation - Consumption Components

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Change in Debt | | | Change in Residential RE Wealth | | | Change in Financial Wealth | | | Income | | |
| Samples | Fixed | Stayers | Movers | Fixed | Stayers | Movers | Fixed | Stayers | Movers | Fixed | Stayers | Movers |
| RY-3 | -2090.2 | -8122.9 | 8448.8 | 1514.8 | -3875.1 | 10106.5 | -771.0 | -6535.8 | 8226.2 | -125.7 | -2357.5 | 2197.1 |
| | (-0.37) | (-1.25) | (0.87) | (0.29) | (-0.67) | (1.33) | (-0.09) | (-1.07) | (0.52) | (-0.04) | (-0.47) | (0.33) |
| RY-2 | -4819.6 | -6025.1 | -3944.4 | 1489.4 | -4445.6 | 10250.3 | 474.3 | -1719.4 | 1782.4 | 665.2 | -6515.7* | 12895.5** |
| | (-0.71) | (-0.81) | (-0.34) | (0.27) | (-0.86) | (0.97) | (0.06) | (-0.26) | (0.13) | (0.19) | (-1.83) | (2.43) |
| RY0 | 337087.1*** | 329954.3*** | 347653.6*** | 376335.9*** | 372547.5*** | 380232.9*** | -12003.3** | -20981.7*** | 956.8 | 10769.9** | 7697.6** | 16268.5** |
| | (4.82) | (4.45) | (5.29) | (5.17) | (4.89) | (5.36) | (-2.49) | (-3.77) | (0.08) | (3.23) | (2.51) | (3.01) |
| RY+1 | -8163.4 | -9509.3 | -12799.6 | -23455.7 | -3231.9 | -74307.4 | 8846.0 | -5769.9 | 36928.1* | 2898.7 | 3155.4 | 2150.9 |
| | (-0.54) | (-0.98) | (-0.34) | (-1.16) | (-0.36) | (-1.35) | (0.99) | (-0.95) | (1.81) | (0.74) | (0.77) | (0.34) |
| RY+2 | 3126.0 | -13948.1* | 37799.3 | -1198.4 | -1832.3 | -6655.2 | -91.72 | -5372.9 | 8840.8 | 3162.6 | 3884.7 | 181.3 |
| | (0.32) | (-1.82) | (1.24) | (-0.10) | (-0.25) | (-0.18) | (-0.02) | (-0.87) | (0.86) | (0.83) | (0.90) | (0.02) |
| RY+3 | -17206.1** | -24499.3** | -3553.3 | -4507.8 | -11785.1 | 6066.4 | -9073.6 | -14781.6** | 1656.0 | 1627.6 | 2062.8 | -862.2 |
| | (-2.28) | (-3.27) | (-0.22) | (-0.55) | (-1.48) | (0.28) | (-1.37) | (-2.13) | (0.13) | (0.37) | (0.35) | (-0.12) |
| RY+4 | -6864.3 | -5126.8 | -12547.0 | -28728.1 | 9495.5 | -114563.3** | -2227.2 | -21752.5** | 36921.6** | -1989.9 | 1966.3 | -12620.1 |
| | (-0.46) | (-0.55) | (-0.33) | (-1.59) | (1.13) | (-2.58) | (-0.43) | (-3.33) | (2.37) | (-0.29) | (0.25) | (-1.12) |
| PT-Mean | 4867.42 | 2913.66 | 8833.28 | 1864.67 | 671.15 | 4287.36 | 9380.30 | 9290.77 | 9562.04 | 166894.1 | 165613 | 169494.7 |
| PT-SD | 70086.34 | 52387.75 | 96396.62 | 49840.65 | 29623.04 | 75753.51 | 77498.23 | 73746.39 | 84623.94 | 85380.2 | 81058.93 | 93508.39 |
| N | 13370 | 9165 | 4205 | 13370 | 9165 | 4205 | 13370 | 9165 | 4205 | 13370 | 9165 | 4205 |
| $R^2$ | 0.197 | 0.293 | 0.140 | 0.209 | 0.372 | 0.143 | 0.0138 | 0.0214 | 0.0288 | 0.142 | 0.153 | 0.178 |

*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 $\delta_k$ 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.

consumption response is 37k SEK. The effects on savings in columns (9) and (10) mirror those on consumption. Home ownership on average reduces savings in the years following treatment. The effect in RY+1 through RY+4 more than offset the initial increase in savings in RY0.

Interestingly, we find essentially no effect on consumption and savings for Stayers but large effects for Movers. The treatment effects on spending for Movers are +27k, +36k, and +53k in RY1, RY+2, and RY+4, respectively. These represent 16%, 22%, and 32% of average annual consumption pre-treatment. The cumulative consumption response is 103k SEK for Movers compared to 13k SEK for Stayers.

The zero result for Stayers arises because they reduce financial wealth to pay off their debt. There is little change in housing wealth and a zero income effect. Thus, we find little evidence for a pure illiquid housing wealth effect. Stayers could borrow against their home equity, especially in light of the large windfall tied up in their property. Home equity lines of credit were widely used in Sweden at the time. Yet, the treated Stayers chose not to tap into their home equity but rather leave the home equity wealth tied up in the house.

Treated movers reduce housing wealth relative to the control group of movers. These relative reductions are especially significant in RY+1 and RY+4. However, they do not reduce debt over these four years. Instead, 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.

**Marginal Propensity to Consume** Our results shed light on the literature, cited in the introduction, that studies marginal propensities of consumption 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. Over the four years between RY+1 and RY+4, treated movers' consumption increases by 103k SEK cumulatively, relative to the control group of movers and relative to RY-1. This amounts to 14.7% of the average windfall among movers of 702k SEK. In sharp contrast, the treated stayers' consumption increases by a cumulative 13k SEK relative to the stayers in the control group. This is 1.8% of the 724k SEK windfall stayers receive. These effects are

ITT, not TOT effects. They should be scaled up by a factor 1/0.90 for stayers since only 90% of the treated stayers become home owners and by a factor 1/0.773 for movers since only 77.3% of movers become home owner. We obtain a marginal propensity to consume out of a temporary wealth shock of 2% for Stayers and 19% for Movers. The estimate for Stayers is consistent with the evidence from aggregate data and close to what the full-insurance literature obtains. The estimate for movers is close to what the recent empirical literature obtains and what comes out of a richer model with life-cycle, labor income risk, and financial constraints (Berger et al. (2015), Mian, Rao and Sufi (2013)).

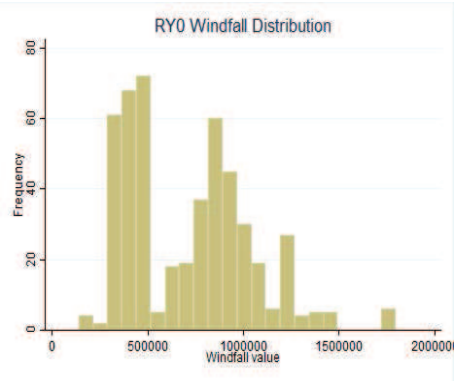
# 5 Heterogeneous Treatment Effects By Windfall

The conversion experiment confers not only home ownership but is associated with a financial windfall. Households can purchase their apartment for a conversion fee which is below the fair market value. Essentially, the municipal landlord passes on the present value of the subsidized rents to the new owner. 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.[39] Within 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. The average across the 493 converting households is 716k SEK with a standard deviation of 320k SEK. The $20^{th}$ percentile of the windfall distribution is 385k, the $40^{th}$ percentile is 505k, the $60^{th}$ percentile is 824k, and the $80^{th}$ percentile is 940k SEK.

To study how treatment effects depend on the size of the windfall, we group each household in one of six groups. Groups 1 through 5 are the quintiles of the windfall distribution whose cut-off points we reported above. Group zero consists of the residual tenants who are treated by our definition of treatment, but do not receive any windfall in RY0. There are 40 such households in the residual tenant group and about 100 in each windfall quintile.

[39] 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 2012 SEK.

Let $WF_i(n) = 1$ if household $i$ has an initial windfall in group $n$, for $n = 0, 1, \cdots, 5$. We estimate the following specification:

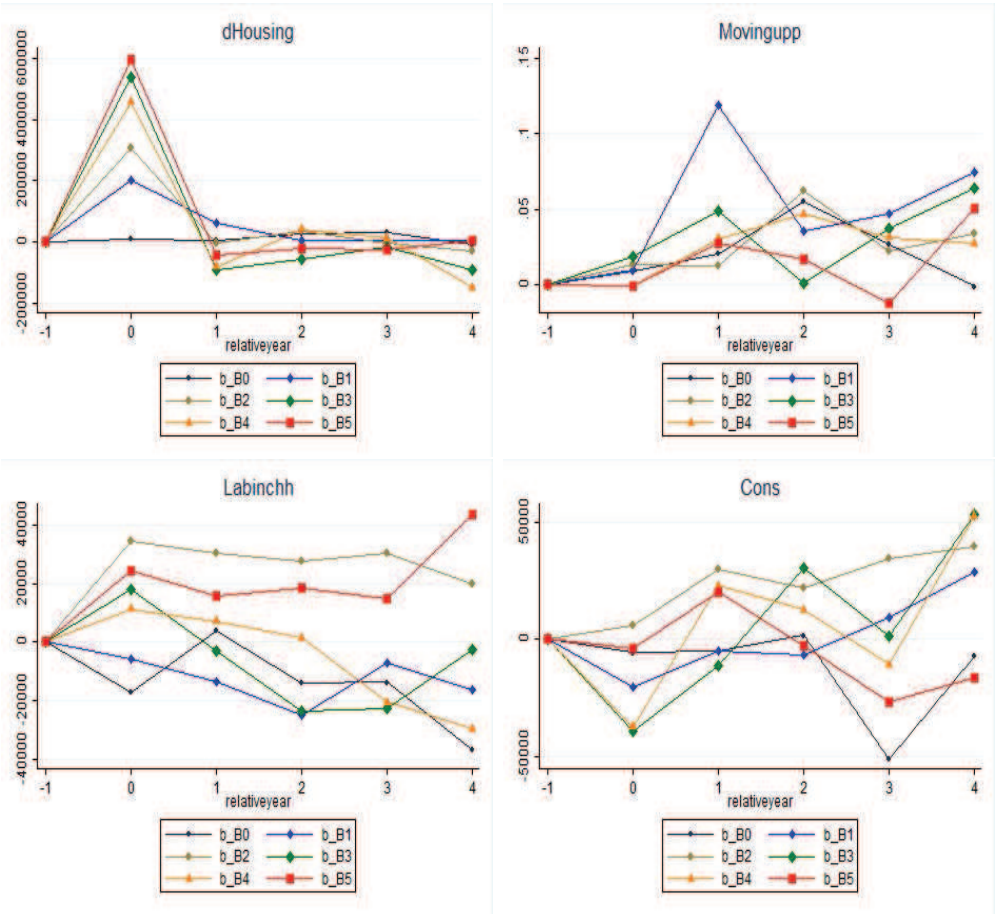
$$y_{it} = \alpha + Convert_i \sum_k \sum_n \delta_{k,n}^{ITT} RY_i(t=k) WF_i(n) + \sum_k \gamma_k RY_i(t=k) + X_{it} + \psi_t + \omega_b + \varepsilon_{it}, \quad (3)$$

Essentially, we estimate dynamic treatment effects $\{\delta_{k,n}^{ITT}\}$ for each windfall group. 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.

Figure 5 summarizes our main findings by windfall group. The top left panel shows that those with the largest windfall increase housing the most. This is natural given that the windfall is proportional to house size. In unreported results, we find that the pattern for the increase in debt tracks that for the increase in housing wealth closely. The increase in home ownership is generally larger for those with the higher windfall, though the effect is not monotonic. The RY0 treatment effect is 83.9% in the first windfall quintile, 94.8% in the second, 93.3% in the third, and 92.2% in quintiles 4 and 5.

The second panel shows that upward economic mobility increases the most for the lowest quintile, suggesting that either the windfall they received was enough to get them to move to a better area and/or that some of the higher windfall groups were already living in better areas in the first place. Generally speaking, there is no strong monotonic relationship here,

Figure 5: Heterogeneous Treatment Effects: By Windfall Group




Each panel plots the difference-in-difference coefficient estimates $\delta$ from RY-1 until RY+4 for the Fixed sample. The top left left panel is for the outcome variable *dHousing*, the top right panel for *Moving Up P*, the bottom left panel for *Labinchh*, and the bottom right panel. Bin 0 (B0) is the group of residual tenants who do not convert. Bins 1 (B1) through 5 (B5) are the five quintiles of the windfall distribution, measured as of RY0, in increasing order.

suggesting that the exact size of the windfall was not that important for the moving decision.

The bottom left panel shows treatment effects for total household labor income. They are highest for quintiles 2, 3, and 5 suggesting a modest positive correlation between the size of the windfall and the labor supply response. This is consistent with the higher windfall groups taking on more housing debt, and the higher debt service leading to higher labor supply.

Finally, the bottom right panel shows the consumption response by windfall bin. The lower windfall bins tend to have the greater initial decline in consumption. They have less financial wealth to make the down payment and cut spending by more. The subsequent spending response is biggest for the fourth and fifth quintiles. In all, the results show surprisingly little variation by windfall bin, suggesting that it is home ownership (and at least some windfall) that accounts for most of our effects.

# 6 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 $\delta$ 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 Chetty, Hendren and Katz (2016). Formally:

$$y_{it} = \alpha_T + TakeConv_i \sum_k \delta_k^{TOT} RY_i(t=k) + \sum_k \gamma_k RY_i(t=k) + X_{it} + \psi_t + \omega_b + \varepsilon_{it} \quad (4)$$

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 (4) using two-stage least squares. Under the assumption that conversion offers only affect outcomes through the actual use of the conversion option, $\delta^{TOT}$ can be interpreted as the causal effect of exercising the conversion option and becoming home owner (Angrist, Imbens and Rubin (1996)).

In the interest of space, we relegate the full set of TOT estimates to Appendix E. All point

estimates are about 10% larger in absolute value in the TOT than in the ITT estimation. This makes sense given that about 90% of all households that were allowed to convert also converted. While the economic significance of all our results is increased by about 10%, none of the statistical significance is affected.

# 7 Conclusion

Our paper exploits a quasi-experimental setting in Sweden whereby the tenants of one set of apartment buildings 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 are similar in terms of their characteristics and these characteristics display similar dynamics before the conversion.Over 90% of those households given the chance to own their apartment choose to do so. Even four years later, the experiment causes a large difference in home ownership rates between those that were approved for conversion and those that were denied. We find that home ownership has little effect on household stability (marriage or divorce rate, number of children). Home ownership causes a positive increase in mobility, giving households the opportunity to move to better neighborhoods. Home ownership also has a positive effect on labor supply, both on the extensive and the intensive margin. Finally, we turn to consumption where our data are of especially high quality relative to the extant literature. We find that new home owners cut consumption in the year of their home purchase. We find 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. Movers, in contrast, increase spending considerably. Thus, we find that consumption responses require liquidation of illiquid housing wealth. We estimate a marginal propensity to consume out of the (one-time) housing wealth windfall of only 2% for stayers but 19% for movers.

Several interesting questions remain for future work. 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 engagement and civility, school quality, political engagement, and crime.

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

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# A Market-wide Conversion Statistics

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

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

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

*Notes:* The table reports the number and share of apartments in the municipality of Stockholm by type of ownership. Source: Utrednings- och statistikkontoret i Stockholms stad (2005, p. 11).

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

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

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

# 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 1 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 3 provides details on the four properties. In addition to their extreme geographic proximity, identical year of construction, and identical ownership, the four co-ops' properties share several more characteristics. All co-ops have about the same floor area, with the vast majority of square meterage going to apartments and only a small fraction devoted to commercial use. They also have about the same distribution of apartments in terms of number of rooms, with the vast majority 3- and 4-room apartments (i.e., one- and two-bedroom apartments).

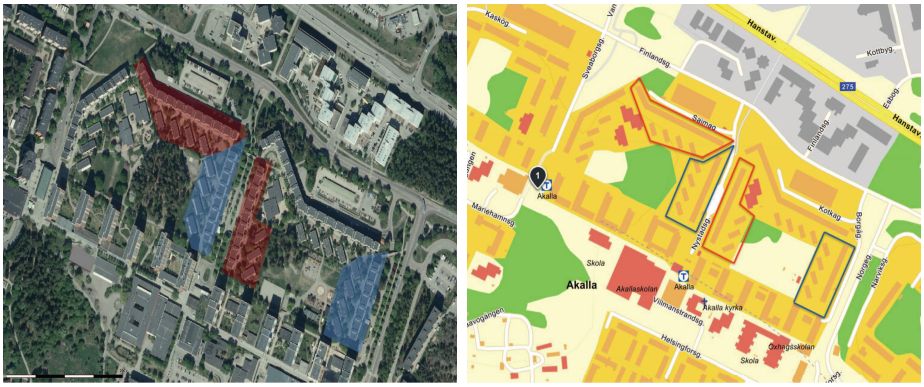


Figure 1: Akalla Complex

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, the Board decided that it could not let all four co-ops convert, lest it lose its ability to calculate the rental price index for that type of property. It decided that only two of the four transactions could be approved. There was no established rule for which of the co-ops to give priority. The Board had to make up a rule at the meeting and decided to give priority to the two co-ops that voted first. Different rules could have been employed, such as approval based on the date when the contract was signed or the voting share among the tenants. Either of these two alternative rules would have resulted in a different outcome. Practically, this decision meant that the two co-ops that voted in April 2002 (ten months before the decision of the Board) won approval while the two that had voted in June 2002 (eight months before the decision of the Board) were denied. We argue that the decision to approve conversion was random in nature, since (i) the dates of the vote where within two months of each other, (ii) Stopplag was not even being discussed when the co-ops first registered in June 2001 and therefore could not have been anticipated, (iii) any other rule applied by the Board would have resulted in a different outcome, and (iv) the number of townhouse apartments was essentially the same in each co-op. The transfer of the property title for the buildings that gained approval took place at the end of May in 2003.

Table 3: Akalla Coop Conversions

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

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

# C Appendix on the windfall from converting

A current renter in a municipally-owned building pays a subsidized annual rent $r_t^s$ per square meter. Denote the present value of these *subsidized* 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 *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 assume that private market rents grow at the same rate as subsidized rents, but can differ in the initial level: $r_t^m \geq r_t^s$. This assumption on growth rates of private rents reflects the reality in Sweden that the private rental market must benchmark its rents to those set by the municipal government. 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 $\alpha$), 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, raisins 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 $\beta$ 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 $\chi$ 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 $\Pi_i^{rent}$:

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

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 $\Pi_i^{own}$:

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

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 tansfer 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. The household budget constraint implies:

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

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.[40] 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.[41]

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

[40] 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.

[41] 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.

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

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.[42] 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

[42]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.

algorithm is described below. We use the following notation:

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

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

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

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

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

Compare $A_1$ and $A_2$ and decide:

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

## 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 $\times$ a regional factor which is based on transaction values in the region during the year.

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

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

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

**Change in bank accounts**. We observe the total amount 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 houseing 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 Treatment Effect on the Treated

This appendix reports a full list of results for the Treatment Effect on the Treated estimation descibed in equation (4). Tables 4-8 represent the full set of TOT estimates, keeping the format of the ITT tables in the main text.

Table 4: TOT 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-4 | 0.0255 | 0.0277 | 0.0148 | 0.0539** | 0.00118 | 0.0227 | 0.0163 | 0.0423** | -0.0563 | -0.0442 | -0.0317 | -0.0403 |
| | (1.44) | (1.67) | (0.89) | (2.30) | (0.05) | (1.50) | (0.94) | (2.10) | (-1.21) | (-1.17) | (-1.07) | (-0.47) |
| RY-3 | 0.0150 | 0.0161* | 0.0147 | 0.0172 | 0.0277* | 0.0299** | 0.0208* | 0.0482** | 0.0107 | -0.0222 | -0.00557 | -0.0402 |
| | (1.57) | (1.85) | (1.67) | (1.11) | (1.81) | (3.17) | (1.88) | (3.10) | (0.30) | (-0.67) | (-0.21) | (-0.61) |
| RY-2 | 0.0110 | 0.0109 | 0.00398 | 0.0230** | 0.0174 | 0.0147** | 0.00323 | 0.0348** | 0.0301 | -0.00204 | 0.0199 | -0.0406 |
| | (1.60) | (1.65) | (0.64) | (2.05) | (1.56) | (2.09) | (0.45) | (2.39) | (1.30) | (-0.10) | (0.99) | (-1.34) |
| RY0 | 0.814*** | 0.904*** | 0.962*** | 0.802*** | 0.00272 | 0.0174 | 0.0344** | -0.0166 | -0.0368 | -0.0327 | 0.00507 | -0.0979 |
| | (43.20) | (56.68) | (94.65) | (17.13) | (0.17) | (1.47) | (2.46) | (-1.06) | (-1.02) | (-1.44) | (0.25) | (-1.57) |
| RY+1 | 0.759*** | 0.857*** | 0.936*** | 0.692*** | 0.000520 | 0.0134 | 0.0168 | -0.00441 | -0.00487 | 0.000953 | 0.0203 | -0.0516 |
| | (39.18) | (52.74) | (47.46) | (17.65) | (0.04) | (1.44) | (1.45) | (-0.27) | (-0.13) | (0.05) | (0.80) | (-0.83) |
| RY+2 | 0.696*** | 0.809*** | 0.917*** | 0.567*** | 0.0231 | 0.0226** | 0.0346** | -0.0135 | 0.0150 | 0.0142 | 0.0312 | -0.0627 |
| | (26.32) | (35.85) | (45.61) | (14.07) | (1.04) | (2.13) | (3.02) | (-0.65) | (0.27) | (0.48) | (0.81) | (-0.98) |
| RY+3 | 0.611*** | 0.741*** | 0.891*** | 0.406*** | 0.0110 | 0.0228* | 0.0356** | -0.0123 | 0.0109 | 0.0205 | 0.0518 | -0.0680 |
| | (22.06) | (33.19) | (41.06) | (11.38) | (0.49) | (1.90) | (2.76) | (-0.54) | (0.17) | (0.56) | (1.19) | (-0.97) |
| RY+4 | 0.543*** | 0.691*** | 0.893*** | 0.244*** | -0.000117 | 0.0165 | 0.0270 | -0.0150 | -0.0366 | -0.0230 | -0.0135 | -0.0726 |
| | (13.71) | (19.48) | (31.49) | (4.27) | (-0.00) | (1.06) | (1.63) | (-0.48) | (-0.35) | (-0.36) | (-0.19) | (-0.80) |
| 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 |
| $R^2$ | 0.455 | 0.584 | 0.739 | 0.420 | 0.106 | 0.122 | 0.137 | 0.157 | 0.105 | 0.169 | 0.184 | 0.189 |

*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 $\delta_k$ 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 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.

Table 5: TOT 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-4 | -0.0211 | -0.0174 | 0.0157 | 0.0230** | 0.0178** | 0.0217** | 0.00961 | 0.00898 | 0.0131 | 0.0150 |
| | (-1.04) | (-0.82) | (1.35) | (2.39) | (2.27) | (2.93) | (1.09) | (1.05) | (1.27) | (1.53) |
| RY-3 | 0.0205 | 0.0357 | -0.00328 | 0.0188 | 0.00284 | 0.0142 | -0.00278 | 0.00114 | -0.0144 | -0.0107 |
| | (0.82) | (1.40) | (-0.17) | (1.07) | (0.26) | (1.36) | (-0.24) | (0.10) | (-1.12) | (-0.95) |
| RY-2 | 0.00948 | 0.0237 | -0.00936 | 0.00455 | -0.0104 | -0.00292 | -0.00932 | -0.00394 | -0.00711 | -0.000897 |
| | (0.56) | (1.46) | (-0.76) | (0.43) | (-1.35) | (-0.47) | (-1.03) | (-0.47) | (-0.66) | (-0.09) |
| RY0 | 0.00971 | -0.00994 | 0.00124 | -0.00512 | -0.00104 | -0.000726 | 0.0117 | 0.00818 | -0.00138 | 0.00625 |
| | (0.60) | (-1.04) | (0.07) | (-0.45) | (-0.11) | (-0.11) | (0.99) | (0.96) | (-0.12) | (0.80) |
| RY+1 | 0.0284 | 0.0584** | 0.0233 | 0.0496** | 0.0148 | 0.0367*** | 0.0473** | 0.0512** | 0.0400** | 0.0430** |
| | (1.23) | (2.70) | (1.20) | (2.85) | (1.42) | (3.76) | (2.32) | (2.68) | (2.26) | (2.47) |
| RY+2 | 0.0566** | 0.0738** | 0.0495* | 0.0618** | 0.0370** | 0.0571** | 0.0338** | 0.0390** | 0.0364** | 0.0436** |
| | (2.48) | (3.27) | (1.98) | (3.06) | (2.26) | (3.35) | (2.44) | (2.42) | (2.32) | (2.56) |
| RY+3 | 0.0170 | 0.0390 | 0.0244 | 0.0380** | 0.0140 | 0.0225 | 0.0285* | 0.0264* | 0.0216 | 0.0322** |
| | (0.83) | (1.50) | (1.51) | (2.10) | (1.44) | (1.68) | (1.98) | (1.93) | (1.55) | (2.37) |
| RY+4 | 0.0563** | 0.0824*** | 0.0450** | 0.0726*** | 0.0181* | 0.0377** | 0.0410** | 0.0472** | 0.0397** | 0.0544** |
| | (2.96) | (5.05) | (3.02) | (4.17) | (1.82) | (3.23) | (3.01) | (3.29) | (2.42) | (3.11) |
| 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 |
| $R^2$ | 0.0756 | 0.0443 | 0.0698 | 0.0391 | 0.0398 | 0.0240 | 0.0270 | 0.0202 | 0.0308 | 0.0236 |

*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 $\delta_k$ 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.

Table 6: TOT Estimation - Labor Force Participation and Earnings Results

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Labinchh under age 64 | | | | Numwork under age 64 | | | | Labincind under age 64 | | | |
| Samples | All | Fixed | Stayers | Movers | All | Fixed | Stayers | Movers | All | Fixed | Stayers | Movers |
| RY-4 | 6297.9 | 14514.9* | 8101.4 | 22510.7 | -0.0108 | 0.0167 | -0.0473 | 0.135** | 5867.2 | 12211.7 | 5285.3 | 22273.7* |
| | (0.95) | (1.74) | (0.80) | (1.57) | (-0.33) | (0.49) | (-1.13) | (2.03) | (1.04) | (1.68) | (0.54) | (1.79) |
| RY-3 | -4188.2 | 917.0 | -5163.8 | 7707.3 | -0.0108 | 0.00946 | -0.0376 | 0.0894** | 183.8 | 2332.2 | -2130.1 | 6591.5 |
| | (-0.98) | (0.19) | (-0.76) | (0.90) | (-0.34) | (0.30) | (-0.75) | (2.33) | (0.04) | (0.49) | (-0.34) | (0.61) |
| RY-2 | -1516.3 | 443.2 | -7674.2 | 11829.8 | 0.0381 | 0.0429* | 0.0282 | 0.0581 | 1536.1 | 2453.9 | -2659.6 | 8590.5 |
| | (-0.26) | (0.07) | (-1.18) | (1.23) | (1.62) | (1.70) | (0.86) | (1.44) | (0.29) | (0.49) | (-0.48) | (0.94) |
| RY0 | 13347.8** | 17469.0*** | 12428.7** | 25383.1** | 0.0418* | 0.0313* | 0.0311 | 0.0226 | 13956.8** | 17342.1** | 11959.2** | 25919.6** |
| | (3.10) | (3.60) | (2.68) | (2.83) | (2.01) | (1.89) | (1.46) | (0.70) | (3.09) | (3.54) | (2.55) | (2.86) |
| RY+1 | 6430.8 | 10556.0 | 7193.1 | 15249.9* | 0.0482 | 0.0367 | 0.00705 | 0.0785 | 8236.0 | 11828.6* | 8015.6 | 17191.2* |
| | (1.05) | (1.63) | (0.92) | (1.76) | (1.58) | (1.28) | (0.21) | (1.34) | (1.34) | (1.96) | (1.16) | (1.90) |
| RY+2 | 63.10 | 1677.9 | 4919.1 | -5583.7 | 0.0280 | 0.0553 | 0.0490 | 0.0432 | 2368.7 | 2184.6 | 5276.8 | -6058.0 |
| | (0.01) | (0.19) | (0.43) | (-0.41) | (0.75) | (1.33) | (1.09) | (0.63) | (0.33) | (0.26) | (0.52) | (-0.43) |
| RY+3 | -1451.2 | 1494.6 | -4535.7 | 10075.1 | -0.00622 | -0.00176 | -0.0378 | 0.0432 | -332.9 | 1750.9 | -1526.5 | 4103.5 |
| | (-0.18) | (0.15) | (-0.34) | (0.77) | (-0.16) | (-0.04) | (-0.67) | (0.66) | (-0.04) | (0.18) | (-0.13) | (0.30) |
| RY+4 | -5868.9 | 2955.2 | -2943.9 | 10039.4 | 0.0136 | 0.00629 | -0.101 | 0.199** | -7202.8 | 3240.9 | -116.5 | 2393.2 |
| | (-0.69) | (0.31) | (-0.22) | (0.55) | (0.35) | (0.13) | (-1.37) | (2.55) | (-0.89) | (0.36) | (-0.01) | (0.14) |
| PT-Mean | 187430.7 | 186999.4 | 182161 | 196850.7 | 1.34 | 1.34 | 1.34 | 1.32 | 193846.4 | 193978.6 | 187839.9 | 206477.5 |
| PT-SD | 156323.3 | 151304.8 | 144957.9 | 163055.6 | .78 | .78 | .81 | .71 | 147829.7 | 143536.4 | 137187.1 | 154945 |
| N | 17703 | 14536 | 9835 | 4701 | 17703 | 14536 | 9835 | 4701 | 17703 | 14536 | 9835 | 4701 |
| $R^2$ | 0.108 | 0.107 | 0.119 | 0.151 | 0.405 | 0.400 | 0.403 | 0.428 | 0.105 | 0.115 | 0.122 | 0.164 |

*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 $\delta_k$ 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.

Table 7: TOT Estimation - Car Purchases, Consumption, and Savings

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Car purchases | | | | Consumption | | | | Savings | | | |
| Samples | All | Fixed | Stayers | Movers | All | Fixed | Stayers | Movers | All | Fixed | Stayers | Movers |
| RY-4 | -0.0357* | -0.0350 | -0.0341** | -0.0393 | | | | | | | | |
| | (-1.71) | (-1.61) | (-2.32) | (-0.75) | | | | | | | | |
| RY-3 | 0.00471 | -0.00602 | -0.0356 | 0.0577 | -4416.7 | -3238.2 | -150.3 | -8246.5 | 3098.6 | 3149.9 | -2356.4 | 10699.3 |
| | (0.22) | (-0.27) | (-1.37) | (1.52) | (-0.46) | (-0.31) | (-0.01) | (-0.54) | (0.36) | (0.36) | (-0.30) | (0.62) |
| RY-2 | 0.00653 | -0.00170 | -0.0228 | 0.0432 | -4615.7 | -6659.9 | -6986.0 | -3302.5 | 5524.4 | 7412.0 | -61.84 | 17192.8 |
| | (0.18) | (-0.05) | (-0.70) | (0.74) | (-0.54) | (-0.72) | (-0.82) | (-0.20) | (0.74) | (0.91) | (-0.01) | (1.06) |
| RY0 | 0.0312 | 0.0207 | 0.0221 | 0.0223 | -15916.6* | -18137.4* | -15317.5 | -18995.8 | 25561.4** | 29950.8** | 23730.3* | 36874.9** |
| | (1.15) | (0.74) | (0.73) | (0.50) | (-1.79) | (-1.95) | (-1.15) | (-1.37) | (2.94) | (3.37) | (1.99) | (2.50) |
| RY+1 | 0.0189 | 0.00792 | -0.00351 | 0.0314 | 14509.9 | 10177.9 | 2873.4 | 29424.3 | -13243.1 | -7038.5 | 510.4 | -27083.3 |
| | (0.77) | (0.30) | (-0.12) | (0.67) | (1.35) | (0.85) | (0.36) | (1.17) | (-1.49) | (-0.75) | (0.09) | (-1.24) |
| RY+2 | -0.000466 | -0.00366 | -0.0277 | 0.0534 | 8129.9 | 8129.1 | -3187.9 | 38452.0* | -6604.9 | -4694.3 | 7386.0 | -38229.5** |
| | (-0.02) | (-0.17) | (-1.28) | (1.05) | (0.92) | (0.81) | (-0.31) | (1.94) | (-0.92) | (-0.55) | (0.91) | (-2.21) |
| RY+3 | 0.0182 | 0.0352 | -0.00646 | 0.124** | -1153.0 | -2283.2 | 4434.0 | -13417.8 | 350.0 | 4060.9 | -2210.3 | 12437.1 |
| | (0.79) | (1.34) | (-0.31) | (2.34) | (-0.13) | (-0.21) | (0.44) | (-0.55) | (0.06) | (0.45) | (-0.24) | (0.63) |
| RY+4 | 0.0125 | 0.0348 | 0.0155 | 0.0767 | 16154.0 | 24630.1* | 10201.2 | 57370.5** | -21147.8** | -26906.7** | -8033.4 | -71273.3** |
| | (0.56) | (1.42) | (0.60) | (1.17) | (1.38) | (1.99) | (0.84) | (2.16) | (-2.69) | (-3.39) | (-0.78) | (-3.37) |
| PT-Mean | .14 | .13 | .12 | .15 | 160564.3 | 160516.6 | 158564.7 | 164478.6 | 5744.4 | 6377.56 | 7048.26 | 5016.12 |
| PT-SD | .34 | .34 | .33 | .36 | 119201.4 | 117626.5 | 112600.3 | 127158.6 | 93471.05 | 92889.09 | 86163.09 | 105239.5 |
| N | 18284 | 15076 | 10273 | 4803 | 16199 | 13370 | 9165 | 4205 | 16199 | 13370 | 9165 | 4205 |
| $R^2$ | 0.0366 | 0.0416 | 0.0468 | 0.0556 | 0.0623 | 0.0654 | 0.0767 | 0.0783 | 0.0141 | 0.0156 | 0.0208 | 0.0418 |

*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 $\delta_k$ 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.

Table 8: TOT Estimation - Consumption Components

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Change in Debt | | | Change in Residential RE Wealth | | | Change in Financial Wealth | | | Income | | |
| Samples | Fixed | Stayers | Movers | Fixed | Stayers | Movers | Fixed | Stayers | Movers | Fixed | Stayers | Movers |
| RY-3 | -1115.1 | -7462.3 | 10000.5 | 2900.8 | -2728.9 | 11821.7 | -866.0 | -7089.8 | 8878.0 | -88.26 | -2506.7 | 2452.7 |
| | (-0.20) | (-1.16) | (0.97) | (0.58) | (-0.49) | (1.46) | (-0.10) | (-1.08) | (0.52) | (-0.02) | (-0.46) | (0.34) |
| RY-2 | -4240.6 | -5262.7 | -3813.9 | 2695.7 | -3382.0 | 11480.4 | 475.7 | -1942.6 | 1898.5 | 752.1 | -7047.9* | 13890.3** |
| | (-0.61) | (-0.69) | (-0.30) | (0.51) | (-0.73) | (1.01) | (0.06) | (-0.27) | (0.13) | (0.20) | (-1.80) | (2.40) |
| RY0 | 370026.1*** | 362260.3*** | 381510.3*** | 413170.8*** | 408966.1*** | 417399.4*** | -13193.9** | -22975.6*** | 985.9 | 11813.4** | 8412.8** | 17879.2** |
| | (5.59) | (5.13) | (6.19) | (6.11) | (5.79) | (6.28) | (-2.43) | (-3.80) | (0.07) | (3.37) | (2.57) | (3.11) |
| RY+1 | -9445.7 | -10645.4 | -15079.7 | -26143.6 | -3899.6 | -82748.2 | 9659.4 | -6235.4 | 40585.1* | 3139.3 | 3383.7 | 2341.0 |
| | (-0.58) | (-1.07) | (-0.37) | (-1.22) | (-0.43) | (-1.43) | (1.00) | (-0.95) | (1.83) | (0.73) | (0.76) | (0.34) |
| RY+2 | 4237.8 | -14004.4* | 40972.4 | -307.6 | -769.6 | -6651.4 | -148.8 | -5848.8 | 9394.3 | 3434.8 | 4198.1 | 222.4 |
| | (0.40) | (-1.84) | (1.24) | (-0.02) | (-0.11) | (-0.16) | (-0.02) | (-0.87) | (0.85) | (0.83) | (0.89) | (0.03) |
| RY+3 | -18308.1** | -25828.1*** | -4709.9 | -4343.1 | -11944.2 | 6114.4 | -9904.1 | -16094.1** | 1612.8 | 1777.7 | 2223.7 | -980.8 |
| | (-2.46) | (-3.61) | (-0.27) | (-0.52) | (-1.50) | (0.27) | (-1.38) | (-2.16) | (0.12) | (0.37) | (0.35) | (-0.13) |
| RY+4 | -8199.8 | -5879.8 | -15111.6 | -32692.0* | 10256.9 | -126480.6** | -2414.5 | -24170.1** | 40095.7** | -2276.7 | 2167.8 | -13902.8 |
| | (-0.52) | (-0.59) | (-0.38) | (-1.76) | (1.12) | (-2.73) | (-0.42) | (-3.51) | (2.32) | (-0.30) | (0.25) | (-1.15) |
| PT-Mean | 4867.42 | 2913.66 | 8833.28 | 1864.67 | 671.15 | 4287.36 | 9380.30 | 9290.77 | 9562.04 | 166894.1 | 165613 | 169494.7 |
| PT-SD | 70086.34 | 52387.75 | 96396.62 | 49840.65 | 29623.04 | 75753.51 | 77498.23 | 73746.39 | 84623.94 | 85380.2 | 81058.93 | 93508.39 |
| N | 13370 | 9165 | 4205 | 13370 | 9165 | 4205 | 13370 | 9165 | 4205 | 13370 | 9165 | 4205 |
| $R^2$ | 0.211 | 0.313 | 0.151 | 0.225 | 0.399 | 0.153 | 0.0140 | 0.0220 | 0.0284 | 0.142 | 0.153 | 0.178 |

*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 $\delta_k$ 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.