Effects of Housing Transfer Taxes on Household Mobility*

Essi Eerola[†] Oskari Harjunen[‡] Teemu Lyytikäinen[§]
Tuukka Saarimaa[¶]

May 2019

Abstract

We study the effect of the housing transfer tax on household mobility using Finnish register data. In 2013, the transfer tax rate was increased from 1.5% to 2% for apartments in multi-unit buildings, but remained unchanged at 4% for detached houses, which facilitates a differences-in-differences design. However, the treatment and control groups in our analysis consist of different segments of the same housing market and there may be spillovers across groups due to cross-segment mobility. To account for these spillovers, we complement the empirical analysis with a theoretical model. Combining the two approaches implies a roughly 7% reduction in household mobility due to the tax increase. Ignoring the spillovers would lead to a 20% underestimation of the effects. Similar sources of bias are likely to be present in prior empirical literature, which relies on similar research designs comparing segments of the same housing market. This suggests that the welfare costs of transfer taxes are underestimated in the literature.

JEL: H21, R21

Keywords: Transfer tax, household mobility, dead-weight loss, difference-in-differences.

*We thank the participants at the UEA meetings in Düsseldorf and New York, MaTax Conference in Mannheim, IIPF Conference in Tampere, JSBE seminar in Jyväskylä, VATT seminar in Helsinki, the FEA meeting in Turku and IEB workshop in Barcelona for comments and the Government Plan for Analysis, Assessment and Research (VN-TEAS) and the Academy of Finland (grant number 315591) for funding.

†essi.eerola@vatt.fi, VATT Institute for Economic Research.

‡oskari.harjunen@hel.fi, Aalto University and City of Helsinki – Urban Research and Statistics.

§teemu.lyytikainen@vatt.fi, VATT Institute for Economic Research.

¶tuukka.saarimaa@aalto.fi, Aalto University and Helsinki Graduate School of Economics.

1 Introduction

Housing transfer taxes are typically considered a very inefficient form of taxation, but are nonetheless fiscally important in many countries (e.g. Mirrlees et al. (2011)). The most direct effects of transfer taxes are related to transaction volume and prices. In countries where most households own their home, such as the UK and the US, transfer taxes may also affect household mobility as moving often requires selling and buying a house. Through their effects on household mobility, transfer taxes may influence not only the allocation of housing units to households, but also the allocation of jobs to employees. However, empirical evidence on the importance of transfer taxes on household mobility is still very limited.

We study the effects of the housing transfer tax on household mobility in Finland, a country with a high homeownership rate, using a tax reform implemented in March 2013 as a plausibly exogenous source of variation. The reform raised the effective transfer tax rate by roughly 0.5 percentage points for housing co-operatives (henceforth co-ops) without affecting the tax rate on directly-owned single-family detached houses. This quasi-experimental setting can be analyzed using a differences-in-differences (DID) design where the treatment group consists of homeowners living in housing units subject to the tax increase and the control group of homeowners whose housing units were unaffected by the reform. The analysis is based on high-quality register data on the total population of Finland from 2005–2016.

A similar control and treatment group approach has been used in a number of papers studying the effects of transfer taxes (e.g. Best and Kleven (2018); Besley et al. (2014); Dachis et al. (2012)), but in a housing market setting this type of design may be flawed due to spillover effects between the treatment and control groups. For example, if homeowners in the treatment group move less because of the tax increase, the control group may also be indirectly affected, especially if housing units in the treatment and control groups are close substitutes.

To analyze this issue further, we complement our empirical analysis with a theoretical model of a one-sided housing market where all households are homeowners, and thus, act as both buyers and sellers. We calibrate the model to replicate the mobility rates in our micro data and our DID estimate. This enables us to take into account household mobility across different housing market segments and assess the potential bias in our DID estimates.

Our main finding is that the transfer tax has a significant impact on mobility. Combining the empirical and the theoretical analysis and taking into account spillovers between housing market segments implies a roughly 7% reduction in household mobility due to a 0.5 percentage point increase in the transfer tax. Our DID estimate of the effect is roughly 5.6%, suggesting a 20% bias in the DID estimate. Ignoring the spillovers between different housing types would lead to a substantial underestimation of the negative effects of the transfer tax. As similar sources of bias may also be present in the previous empirical studies relying on treatment and control groups consisting of different segments of the same housing market (e.g. price ranges in notched tax schedules or adjacent geographic areas), this suggests that the adverse effects of transfer taxes are underestimated in the literature.

Our rich register data contain detailed of information on the characteristics of households and their housing units allowing us to obtain a more complete picture of the effects of the tax reform. Firstly, mobility decreased mainly within labor market areas, although we also detect a negative effect on moves across municipal boundaries, suggesting that we cannot rule out labor market effects. At the same time, we do not find clear effects on job changes or on employment status. Secondly, the tax increase affected more strongly moves with small housing unit size adjustments (number of rooms stay the same or change by one room). We also find that these effects are asymmetric so that upsizing becomes less frequent, but there are no effects on downsizing.

Previous studies on transfer taxes mostly focus on transaction volume and exploit tax reforms or discontinuities in tax schedules. Best and Kleven (2018) study the effects of a temporary tax holiday in 2008–2009 in the UK Stamp Duty Land Tax (SDLT) using administrative tax data covering the universe of SDLT returns between November 2004 and October 2012. The tax holiday abolished the SDLT for transactions in the £125,000–£175,000 price range without changing the tax in the other price ranges. Using a DID strategy, the authors estimate that the tax holiday increased the monthly transaction volume by 17%. Some 42% of this additional activity is attributed to a timing response while the remaining 58% was estimated to be additional transactions compared to the *status quo*.

Besley et al. (2014) exploit the same 2008–2009 tax holiday, but use data from the UK financial regulator that includes information on an independent surveyor's valuation of the property. They also exploit a DID strategy and use surveyor's house valuations (instead of the actual transaction prices) to divide the transactions into treatment and control groups. According to the results, the tax holiday increased transactions by about 8%. One possible reason why the results in Besley et al. (2014) differ from the results in Best and Kleven (2018) is measurement error in the surveyor's house valuations.

Kopczuk and Munroe (2015) utilize the discontinuity in tax liability induced by the so-called mansion tax applied in the states of New York and New Jersey. The tax rate is 1% on residential transactions of $1 million or more, while transactions at prices less than $1 million are not subject to the tax. The authors find that the tax distorts the price distribution and leads to significant bunching just below the threshold. The results also suggest that the impact of the tax is not limited to the proximity of the threshold, but extends much further, which indicates that the search and housing market matching process is affected by the tax notch.

Dachis et al. (2012) in turn exploit the introduction of the Land Transfer Tax in the city of Toronto in early 2008. The reform set a 1.1% tax rate on transactions in the city of Toronto, but no tax on other parts of the Greater Toronto housing market area. According to the results derived from a DID design, the 1.1% tax caused a 15% decline in the number of sales and a welfare loss of about $1 for every $8 in tax revenue in the city of Toronto.

Slemrod et al. (2017) study a series of transfer tax reforms introducing discontinuous jumps in tax liability in Washington DC. In order to study the long term effects of the tax, the authors use transaction data from 1999 to 2010 to construct a monthly panel data of repeat sales to study how the likelihood of a transaction is affected by the tax changes. The authors do not find significant effects on the likelihood of selling around the tax notch after the reform compared to the control group. As a result, they conclude that the welfare costs related to housing transaction taxes are likely to be small.

Fritzsche and Vandrei (2019) exploit state level variation in the transfer tax rate in Germany where state governments have been able to set their own tax rates since September 2006. The data feature multiple tax rate changes in different states. The authors conclude that a one percentage point increase in the tax rate is accompanied by 7% fewer transactions in the long run.

Hilber and Lyytikäinen (2017) is the only study thus far in the literature that explicitly focuses on household mobility.[1] They use the British Household Panel Survey (BHPS) data, which contain homeowners' own assessment of the value of their house and information on whether the household moved the following year as well as a rich set of household characteristics. As the data also include information on the reason for moving (e.g. employment or housing reasons) and the distance of the move, different types

[1] This literature is linked to studies on capital gains taxes and housing markets. The important common feature is that the tax payment is triggered by a transaction. The empirical evidence on the effects of housing capital gains taxation is very limited and mainly related to the Tax Relief Act of 1997 in the US. Shan (2011) and Cunningham and Engelhardt (2008) both conclude that the tax reduction raised the mobility rate among affected households.

of moves can be studied separately. The authors find that a higher SDLT has a strong negative impact on short distance, housing-related moves, but does not adversely affect job-induced or long-distance mobility. The tax increase from 1% to 3% reduces household mobility by 2.6 percentage points, implying a reduction in mobility of about 37%. With additional assumptions on the value of foregone transactions, this implies a welfare loss of roughly 80% of additional revenue raised.

Määttänen and Terviö (2019) examine the welfare effects of transaction taxes using a one-sided assignment model with transaction costs and imperfectly transferable utility where households are heterogeneous by incomes, houses are heterogeneous by quality, and housing is a normal good. The model economy is calibrated to represent the Helsinki metropolitan region in Finland. The authors assess the welfare effects of replacing the transfer tax by a revenue-equivalent property tax. The aggregate welfare gain would be 13% of the tax revenue at the current 2% tax rate, but increases rapidly with the tax rate.

We contribute to this literature in three ways. First, we combine a quasi-experimental research design with a theoretical model that allows us to assess the bias arising from spillovers across the treatment and control groups that may be prevalent in most published work to date. We show that in our case this bias is substantial indicating that the negative effects of the transfer tax may have been underestimated in the prior literature. Second, we are only the second paper to analyze the effects of the transfer tax on household mobility in a country where most households are homeowners. Finally, our rich register data allows us to analyze in more detail which type of moves are affected by the tax reform and also to analyze direct labor market measures.

The paper is organized as follows. In the next section, we describe the Finnish transfer tax and the reform that we exploit in the analysis. In section 3, we present the data and the research design. Section 4 presents the empirical results and section 5 offers discussion and conclusions.

2 Institutional Setting and Reform

In Finland, home ownership can be attained either by directly owning a single-family detached house (henceforth directly owned house) or through a housing co-op. Typically, housing co-ops are limited-liability companies that own residential buildings and often the lots under the buildings. Owning shares in a co-op corresponding to a certain apartment in practice implies owning the apartment. For instance, the owner may renovate the apartment and the shares can be sold or the apartment rented out without the consent of the other shareholders. Housing co-ops often have outstanding loans obtained during the construction of the building or at some later stage for renovation. When buying shares for a particular apartment, the buyer becomes responsible for any co-op loans linked to the shares. All multi-unit residential buildings are co-ops, but the ownership of a single-family detached house can also be organized as a co-op, although this is rare. In the latter case, the co-op usually includes several houses.

The transfer tax is paid by the buyer and the buyer officially becomes a shareholder of the co-op or the owner of the house only after the transfer tax has been paid. First-time buyers under the age of 40 are exempt from paying the tax.

We exploit a tax reform that affected co-op apartments, while the tax treatment of directly owned houses remained unchanged. Until the end of February 2013, the transfer tax rate for was 4% for directly owned houses and 1.6% for for co-op apartments. In both cases, the tax base was the transaction price. On March 1, 2013 the transfer tax rate for co-ops was raised from 1.6% to 2% and the tax base was broadened to include the housing co-op loan linked to the apartment. For example, for a housing unit with a transaction price of 200,000 euros and an outstanding co-op loan of 20,000 euros, the transfer tax liability was 3,200 euros ($1.6\% \times 200,000$) before the reform. After the reform, the tax liability increased to 4,400 euros ($2\% \times (200,000 + 20,000)$). Personal mortgages do not affect the transfer tax.

The main aim of the reform was to increase tax revenue and to bring the tax treatment of co-ops and directly owned houses closer together. According to the government proposal, the size of co-op loans had been increasing before the reform, especially in newly built housing, effectively eroding the tax base. The situation was considered undesirable as the tax burden of a given transaction depended on how the construction of the building was financed. Moreover, the co-op loans were often substantially lower in the case of resales. According to the government proposal, the reform was expected to increase annual tax revenue by roughly 80 million euros, from the 580 million euros (0.3% of GDP) collected in 2012. Slightly more than 50% of this increase was expected to result from the tax rate increase and the rest from the broadening of the tax base.

The reform was initially announced in the beginning of October 2012 and was supposed to become effective on January 1, 2013. However, in December 5, 2012 it was announced that the reform would be postponed to March 1, 2013. The delay was due to technical issues in the tax administration.

3 Data and Research Design

3.1 Data

Our data come from Statistics Finland and include the entire Finnish population from 2005 to 2016. The data contain extensive register information about households, including households' residence at the end of each year and whether the household is a renter or a homeowner and whether the unit is a directly owned house or a co-op.

Our measure of moving is based on the location and the characteristics of the housing unit. Under our definition, a household moved if at least one of the following changed between the end of year $t-1$ and t: (i) postcode, (ii) type of housing unit (co-op, directly owned house and rented unit), (iii) number of rooms. This definition means that we are going to miss some very short-distance moves within the postcode area, where the number of rooms and the type of unit did not change.

Table 1 reports summary statistics for the homeowner households in our data. The first two columns include homeowners in houses (our control group) and the next two columns include homeowners in co-ops (our treatment group).

The homeowner households in co-ops are somewhat different from households living in directly-owned houses. For example, they are more likely to be single and to live in urban areas. Homeowners living in co-ops are also more mobile than homeowners living in directly-owned houses (average annual mobility rates over the time period are 7.2% and 3.8%, respectively).

Table 1: Summary statistics for homeowner households, 2005-2016.

	Single family house		Co-op	
	Mean	Std. Dev.	Mean	Std. Dev.
Moved (t-1,t)	0.038	0.192	0.072	0.259
Male hh head	0.858	0.349	0.642	0.480
Taxable income	31,358	21,132	32,885	21,673
Age	56.1	15.4	56.2	17.7
Single	0.228	0.419	0.475	0.499
Number of kids	0.817	1.133	0.362	0.750
Upper secondary education	0.197	0.398	0.355	0.478
Employed	0.578	0.494	0.544	0.498
Unemployed	0.056	0.231	0.047	0.213
Pensioner	0.350	0.477	0.390	0.488
Urban municipality	0.475	0.499	0.843	0.363
Semi-urban municipality	0.241	0.428	0.099	0.299
Rural municipality	0.281	0.450	0.056	0.230
Observations	9,791,352		8,074,113	
Observations 2012	899,745		743,355	

Notes: Taxable income, age, education level and labor market status refer to the head of the household.

In Table 2, we decompose the annual mobility rates according to destination of the move. The table shows the probability of moving for households in different types of housing units and the destination of the move (pooled data for years 2005-2016). For comparison, the table also reports the mobility rates for renters who are more mobile than homeowners.

Conditional on moving, homeowners living in co-ops are most likely to buy into another co-op (2.9%). Similarly, renters are most likely to move to another rental unit (13.0%). In the case of homeowners living in a directly-owned house the differences are smaller. It seems fair to say that homeowners in co-ops predominantly trade with other co-op owners. However, there are also spillovers from one market segment to the other.

Table 2: Mobility rates by origin and destination housing type.

	Move to		
Current unit	House	Co-op	Rental
House	0.013	0.010	0.016
Co-op	0.017	0.029	0.021
Rental	0.028	0.034	0.130

3.2 Research Design

A market transaction occurs when it benefits both the buyer and the seller. The housing transfer tax drives a wedge between the cost of buying a unit and the price received by the seller of the unit. The tax therefore reduces the likelihood that the buyer and the seller are able to settle on a mutually beneficial transaction price. As a result, the transaction volume is smaller than in the absence of the transfer tax and the housing units are more likely to be owned by those who do not value them the most. This basic mechanism is well understood and extensively discussed in the literature (e.g. Mirrlees et al. (2011)).

In a housing market with a high homeownership rate, transactions are closely connected to mobility as moving often requires selling and buying a house. Therefore, the transfer tax is expected to reduce household mobility and lead to households living in housing units that are less suitable for them in terms of location or other characteristics.

In order to study the magnitude of these effects, ideally, we would compare the mobility of households after a transfer tax increase to the mobility of these same households in a situation where the transfer tax was not raised. Obviously, we never observe both outcomes for the same households and we need to impute a credible counterfactual that serves as the baseline when estimating the causal effect of the transfer tax increase.

To this end, we exploit the Finnish transfer tax reform where the tax was increased for shares in co-ops. Since the tax was increased in transactions involving co-ops, we expect mutually beneficial trading opportunities to diminish. This would translate into lower mobility among homeowners living in co-ops, which is our treatment group. As the tax for directly-owned houses was not increased, we can construct the counterfactual using homeowners living in directly-owned houses as a control group. Having data for the treatment and control groups before and after the tax increase facilitates the use of DID methods.

Our DID model takes the form

$$move_{i,t} = \alpha + \delta_1 coop_{i,t-1} + \delta_2 after_{i,t} + \delta_3 coop_{i,t-1} \times after_{i,t} + \beta' X_{i,t-1} + u_{i,t}, \qquad (1)$$

where *move* is equal to one if the household moved between the end of year $t-1$ and t and zero otherwise. The dummy variable *coop* indicates the treatment group, which consists of homeowners who lived in a co-op at the end of year $t-1$. The control group consists of homeowners who lived in a directly-owned house at the end of year $t-1$. Dummy variable $after$ indicates the time period after the tax increase. Vector X denotes the control variables, which include household characteristics (see Table 1) and postcode fixed effects.

The parameter for the interaction term, δ_3, has a causal interpretation under two assumptions. The first is the common trends assumption, which means that in the absence of the treatment the mobility of homeowners living in co-ops and directly-owned houses would have developed similarly. This assumption can be tested indirectly by analyzing the pre-treatment trends in mobility in the treatment and control groups.

The second assumption is that there are no spillovers across the treatment and control groups. That is, the mobility of households in the control group is not affected by the mobility decisions of the households in the treatment group.

This assumption fails if the two housing market segments are connected. This can be illustrated with a simple example. Assume full capitalization so that the tax inclusive co-op prices remain the same when the tax on co-ops is increased. Co-op homeowners would receive a lower price for their current housing units, but would face the same tax inclusive price of a potential new co-op unit as before the reform making a move less appealing to them and decreasing their mobility. If the mobility of the co-op homeowners is reduced, there are fewer co-ops in the market, which in turn may influence all households contemplating moving to a co-op.

These spillovers are likely if co-ops and houses are close substitutes linking the two market segments and according to Table 2 we cannot rule them out. If the tax increase also reduces mobility among among the control group of homeowners in directly owned houses, our estimates will be biased towards zero. After presenting our baseline DID results, we use a theoretical model, calibrated to replicate the mobility rates in our data, to assess the magnitude of the potential bias in our estimates.

In addition to the group assignment, we need to discuss two issues related to the timing of the treatment. Our household data are at annual level and the place of residence is recorded at the last day of the year. The tax increase in turn was announced in October 2012 and eventually took place in March 2013.

The first issue concerns those households who moved in January or February 2013. These households moved before the tax increase, but in our baseline specification the moves are misclassified as having taken place after the reform. This will bias our estimates towards zero if the tax increase reduced mobility after March 2013. The second concern is that households planning to move may have brought their transaction forward in order to benefit from the lower pre-reform tax. This anticipation effect might have induced them also to move before the end of 2012. In our baseline specification, this anticipation response would bias our estimates away from zero. We address these issues by reporting a number of robustness checks.

The nature of the policy reform has important implications for statistical inference. Although the data covers the entire population there are actually only two relevant groups (co-op owners and direct owners) which we compare in different years. Firstly, failing to take account of unobserved group-year effects would produce downward biased standard errors, but standard clustering methods are not feasible with only two groups and eleven years. Secondly, Donald and Lang (2007) argue that, when the number of groups is small in a DID setting, applying standard asymptotics implies that the significance of the t-statistics is overstated. In order to address these issues, we use the two-step procedure proposed by Donald and Lang (2007), which effectively treats the number of group-years as the number of observations.

Instead of estimating equation (1) directly, we first use the household-level data to estimate yearly group-specific intercepts, $c_{g,t}$, from the following model:

$$move_{i,t} = c_{g,t} + \beta' X_{i,t-1} + v_{i,t}, \tag{2}$$

where $g \in \{coop, house\}$.

In the second step, we use the annual group-level data with 22 observations of $c_{g,t}$ to estimate the DID model:

$$c_{g,t} = \alpha_t + \delta_1 coop_{g,t-1} + \delta_3 coop_{g,t-1} \times after_{g,t} + u_{g,t}. \tag{3}$$

This regression gives the same point estimates as the OLS regression using micro data, but corrects standard errors for correlation within housing type year cells, and uses the t-distribution with only 9 degrees of freedom.[2] We use year fixed effects α_t as additional

[2]Donald and Lang (2007) propose weighing the second step regression by the standard errors of the $c_{g,t}$ to gain precision. In our data, weighing has no practical importance because the standard errors are almost identical, and thus, we report the unweighted estimates. The time series nature of the data raises the additional issue of serial correlation of the error terms (Bertrand et al. (2004)) but visual inspection of the data suggests that this is a minor concern in our setting when we control for common year effects.

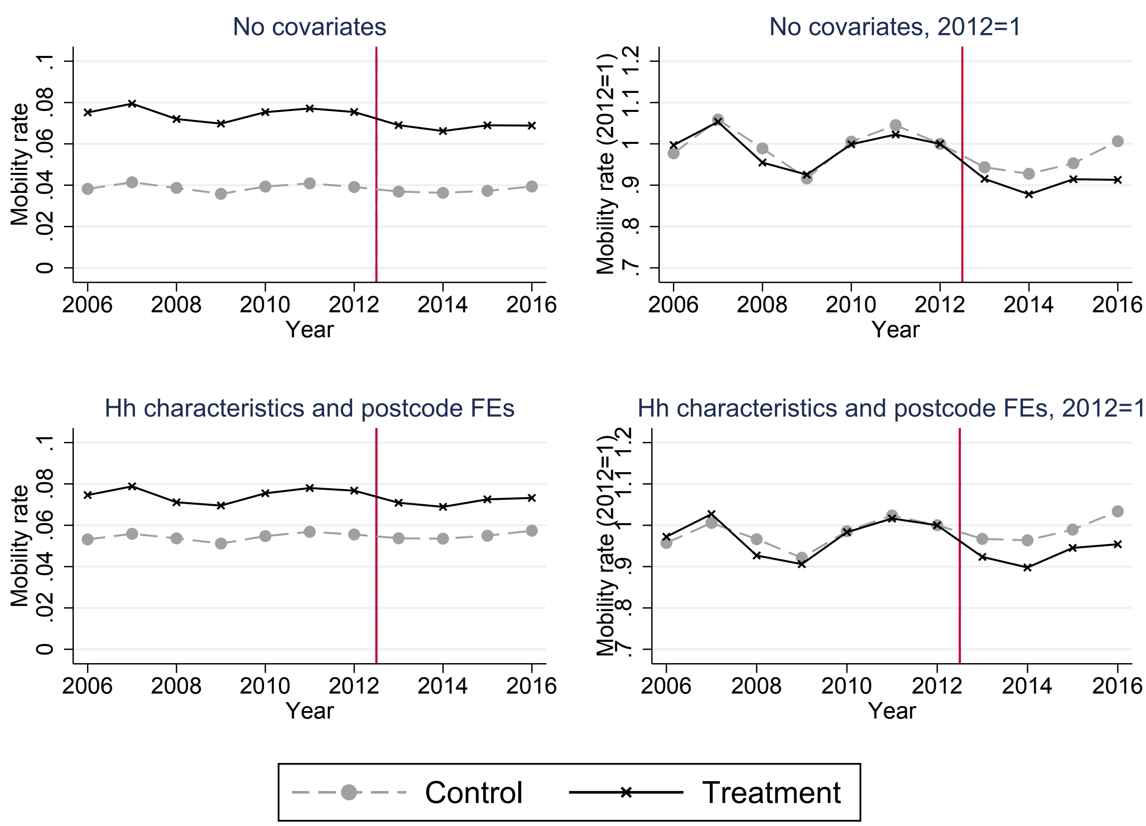
controls so that the main effect of $after_{g,t}$ is absorbed by them.

4 Results

4.1 Baseline mobility effects

We start by presenting graphical evidence on the mobility rate of homeowners in the treatment and control groups. This allows us to visually assess the plausibility of the common trends assumption and the size of the possible treatment effect. The left panel in Figure 1 presents the group-specific mobility rates, whereas in the right panel the mobility rates are normalized to one in 2012 just before the tax increase.

Figure 1: Mobility rate for homeowners in co-ops (treatment) and in directly owned houses (control).



Notes: The left panel presents the group-specific mobility rates. In the right panel, the mobility rates are normalized to one in 2012. Mobility rate refers to the share of homeowners who move between the end of year t–1 and the end of year t. Group assignment is based on the homeowners' housing type in year t–1. The vertical line indicates the timing of the reform.

Three observations stand out from Figure 1. First, the mobility rate is clearly higher in the treatment group than in the control group throughout the time period (left panel). This is true even after controlling for household characteristics and adding postcode fixed effects. Second, the trends in mobility rates are similar in the treatment and control groups in the pre-treatment period. There seem to be some differences in the development during the financial crises, but the groups develop very similarly during the last four pre-treatment years. This is especially clear after normalization, when we compare proportionate changes in the mobility rate relative to 2012 (right panel). Formal pre-treatment placebo tests also point to pre-treatment common trends (see Figures A1 and A2 in Appendix A.) Finally, after the tax increase, the mobility rate decreases in both groups, but clearly more so in the treatment group. This divergence is also permanent.

Table 3 presents the DID regression results corresponding to Figure 1 using the two-step procedure of Donald and Lang (2007). In the first column, the first-stage regression does not include any additional control variables. In the second column, we add the household-level control variables shown in Table 1. In the third column, we further add postcode fixed effects. All model specifications include year dummies in the second step. Panel A reports the results for a specification where the dependent variable is the mobility rate and Panel B for the same specification except that the dependent variable is the log of mobility rate.

Table 3: DID results for mobility.

Panel A	(1)	(2)	(3)
	Mobility rate	Mobility rate	Mobility rate
Coop	0.0358***	0.0247***	0.0205***
	(0.000583)	(0.000630)	(0.000631)
Coop × After	-0.00503***	-0.00401***	-0.00399***
	(0.000967)	(0.00104)	(0.00105)
Pre mean	0.0749	0.0749	0.0749
Panel B			
	Log mobility rate	Log mobility rate	Log mobility rate
Coop	0.651***	0.399***	0.319***
	(0.00864)	(0.00739)	(0.00749)
Coop × After	-0.0506***	-0.0560***	-0.0562***
	(0.0143)	(0.0123)	(0.0124)
Year FE	Yes	Yes	Yes
HH characteristics	No	Yes	Yes
Postcode FE	No	No	Yes

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Sample size of the micro data used in the first step is approximately 18m. Sample size of the housing type-year data used in the second step is 22. Standard errors are in parentheses. Significance is based on t(9)-distribution and is denoted by asterisks: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The control variables include year fixed effects, household characteristics reported in Table 1 and postcode fixed effects (in t–1).

The regression results are in line with Figure 1 and robust across specifications. The reduction in the mobility rate in the treatment group is roughly 0.40 percentage points. Compared to the pre-treatment mobility rate, this implies that the mobility rate decreased by 5.6%. This translates to roughly 3,000 fewer moves per year ($-0.0040 \times 743,335$).

From the table it is also clear that more mobile households tend to sort into co-ops. The main effect of *Coop* in Panel A diminishes from approximately 3.6% to 2% when we include household characteristics and postcode fixed effects as controls in the first stage. Interestingly, using the treatment effect estimate of 0.4%-points and extrapolating to a hypothetical reform that would eliminate the remaining 2%-point tax rate difference between co-ops and directly owned houses suggests that almost all of the remaining difference in mobility rates can be explained by the tax rate difference.

4.2 Additional robustness and validity checks

In addition to the pre-treatment placebo test, we have conducted a number of robustness and validity checks. First, we test the robustness of the results with respect to anticipation effects. As discussed in Section 3, moves that were planned to take place in 2013 may have been brought forward to the end of 2012 because of the anticipated tax increase. As our measure of moving is based on the situation at the end of each year, this anticipation effect would show up in our data as excessive moves in 2012 and fewer moves in 2013, leading our DID estimates to be biased away from zero.

Figure A3 in Appendix A reports the monthly transaction volume of co-ops from January 2010 to December 2017. As the figure shows, the reform was clearly anticipated: the transaction volume in February 2013 is unusually high. However, the announcement of the reform did not lead to anticipation at the end of 2012. Based on Figure A3 it seems that anticipation is not a serious concern in our setting.[3] Nonetheless, in order to check the robustness of our results to these timing issues, we estimate specifications where we omit the years 2012 and 2013. The results are reported in Tables A1 and A2 and they show that the results are not affected by this omission.

Tables A1 and A2 also report our main estimation for different time windows. One may argue that observations at the beginning of the period far from the tax reform may not provide as good a point of comparison for the post-reform years as observations closer to the reform. Therefore we vary the width of the time window around the reform from 2007–2016 to 2009–2016. In addition, we allow for differential group-specific linear time trends. Overall, the results are robust to these changes in the specification. The point estimates are very close to those reported in Table 3, but in some cases the statistical significance is weaker due to fewer degrees of freedom.

The fact that moving costs increased in the co-op segment may lead to sorting so that households who are inherently less mobile are more likely to occupy a co-op unit after the tax increase. We have analyzed the mobility rate in the control and treatment groups based on how observable household characteristics predict mobility. Results in Figure A4 and Table A3 indicate that there is some gradual sorting happening so that co-op homeowner group becomes less mobile in terms of observable characteristics that predict mobility after the reform. However, the sorting is gradual and small in magnitude, which means that it cannot explain the immediate and large decrease in mobility due to the tax increase. Nonetheless, this is consistent with our overall message that transfer taxes

[3] Figure A3 also shows that there is a permanent downward shift in transaction volume after the tax increase supporting our main findings with respect to household mobility.

affect household behavior.

4.3 Accounting for spillovers

Our empirical analysis assumes that the mobility rates of homeowners living in directly-owned houses are not affected by the reform and therefore these homeowners constitute a reliable control group for the analysis. This approach is quite standard in the literature. Several studies also exploit similar reforms which increase the transaction tax for certain types of houses without affecting the tax rate for other houses. For instance, the transfer tax change may apply to a certain geographic area (as in the Toronto Land Transfer Tax (Dachis et al. (2012)) or to houses in a certain price range (as in the UK stamp duty holiday (Besley et al. (2014) and Best and Kleven (2018)). The studies analyzing the effects of these reforms assume that trading of houses located outside the geographic area or the price range is not to be affected by the change.

As we discussed in Section 3, it is unlikely that these different market segments are entirely independent from each other. In order to analyze the role of the linkages between the different market segments, we build a theoretical model with owner-occupied housing and two different housing types in two different locations (say, different neighborhoods or cities). The housing types are different in terms of some characteristics which make them less than perfect substitutes (say, having a yard or not), and they are also subject to different transfer tax rates. In the model, each household has a preference for one housing type and one location, but they may also face a preference shock which makes the current unit less suitable for them. Given the preferences, the costs related to moving to another housing type and location and the fixed distribution of housing stock, we solve for a competitive equilibrium, i.e. house prices such that the excess demand for all housing types is equal to zero.

We use the model to analyze the effects of changing the tax treatment of one housing type without changing that of the other type, thus mimicking the actual Finnish tax reform. The aim is to uncover the mobility patterns between different housing types before and after the tax reform and thereby understand how and to what extent our control group is affected by the reform.

In order to be useful, the model needs to reproduce the empirical mobility patterns between and within the different market segments before the reform. This is important as the bias in our DID estimate presumably depends on how strong the linkages between the two market segments are. In addition, the model needs to reproduce our DID estimate as an outcome to a reform which increases the tax rate on co-ops by 0.5 percentage points.

This is important in order to pin down the house prices in the model.

We calibrate the model so that it exactly replicates the empirical mobility patterns between different market segments in our data before the reform and produces our DID estimate of a 5.6% reduction in mobility when we increase the tax rate for co-ops in the model from 1.5% to 2.0%.[4]

In the model, the reform affects not only moves between co-ops but also moves from co-ops to houses and vice versa. The main reason is that after the reform homeowners living in co-ops are less likely to find welfare improving trades. As a result the effective supply of co-ops is reduced from the point of view of homeowners living in houses and contemplating moving to co-ops.

Overall, the reform reduces the mobility rate of homeowners living in co-ops by 7.0% in the model. As the model reproduces our DID estimate of 5.6%, this means that the DID estimate is biased downwards by 1.4 percentage points. The bias arises from the fact that also the homeowners in the control group are indirectly affected by the reform. The use of the DID estimate only in assessing the effects of the reform, would underestimate the negative effects of the reform on the mobility of homeowners living in co-ops by some 20%.

4.4 Welfare

When the transfer tax is increased, some moves that would have taken place in the absence of the increase are no longer mutually beneficial for the buyer and the seller. The welfare cost of the tax increase is the overall utility loss related to these foregone moves.

The size of the welfare cost can be illustrated by calculating the marginal cost of public funds (MCF), which relates the welfare loss of a tax increase to the additional tax revenue raised. For a non-distortionary tax, one tax-euro collected from the private sector is worth exactly one euro for the private sector and the MCF is equal to one. The larger the welfare cost related to the tax, the larger the MCF.

The MCF can be approximated by

$$MCF = \frac{W(t_0) - W(t_1) + R(t_1) - R(t_0)}{R(t_1) - R(t_0)} = \frac{\Delta W(t) + \Delta R(t)}{\Delta R(t)} \tag{4}$$

where ΔW refers to the welfare loss resulting from increasing the tax rate from t_0 to t_1 and ΔR is the additional tax revenue.

The additional tax revenue raised can be expressed as

[4] The technical details of the analysis are presented in Appendix B.

$$\Delta R(t) = t_1 \times p \times (1-\gamma) \times m - t_0 \times p \times m \tag{5}$$

where p is the average price (transaction price including any co-op loan) and m is the number of moves prior to the tax increase. Parameter γ is the percentage change in mobility when the tax rate is raised from t_0 to t_1.

In our transaction data, after 2013 the average loan-to-value-ratio for co-op resales was roughly 5%. This means that the average effective tax rate on the transaction price including any co-op loan was 1.52% before the reform and 2% after the reform. Hence, in our MCF calculations we set $t_0 = 0.0152$ and $t_1 = 0.02$.

We cannot directly observe the welfare loss related to the foregone moves. However, we can conjecture that before the tax increase, trades involving housing units in co-ops with a welfare gain smaller than 1.52% of the price (i.e. transaction price including any co-op loan) did not take place. In the same way, we know that the welfare loss related to the foregone moves cannot exceed 2% of the price after the tax increase. Therefore the welfare loss related to a foregone move is somewhere between 1.52% and 2% of the price. Thus, MCF lies within the interval

$$MCF = \left\{ \frac{\gamma \times t_0 + t_1 \times (1-\gamma) - t_0}{t_1 \times (1-\gamma) - t_0}, \frac{\gamma \times t_1 + t_1 \times (1-\gamma) - t_0}{t_1 \times (1-\gamma) - t_0} \right\} \tag{6}$$

Finally, based on our results on mobility, we set $\gamma = 0.07$. This figure takes into account that the DID estimate is downward biased because the reform also reduced mobility in the control group.

Plugging the tax rates and the estimated effect on the mobility rate into the above formulas gives a range of MCF values of

$$MCF = \{1.31, 1.41\}$$

4.5 Effects on different types of moves

We next turn to studying different types of moves and labor market outcomes. By affecting household mobility, the transfer tax may influence the allocation of jobs to employees. In this respect, our study is related to the literature studying the relationship between home ownership, which involves higher moving costs compared to renting, and unemployment. For instance, Munch et al. (2006) show that in Denmark home ownership indeed lowers the propensity to move geographically for jobs while unemployed. However, home ownership also has a positive effect on the probability of finding employment in the local labour market.[5]

In analyzing whether the transfer tax hinders labor markets, we use two complementary strategies. Firstly, we try to differentiate between moves within and across labor markets, and secondly, we look at labor market outcomes directly.

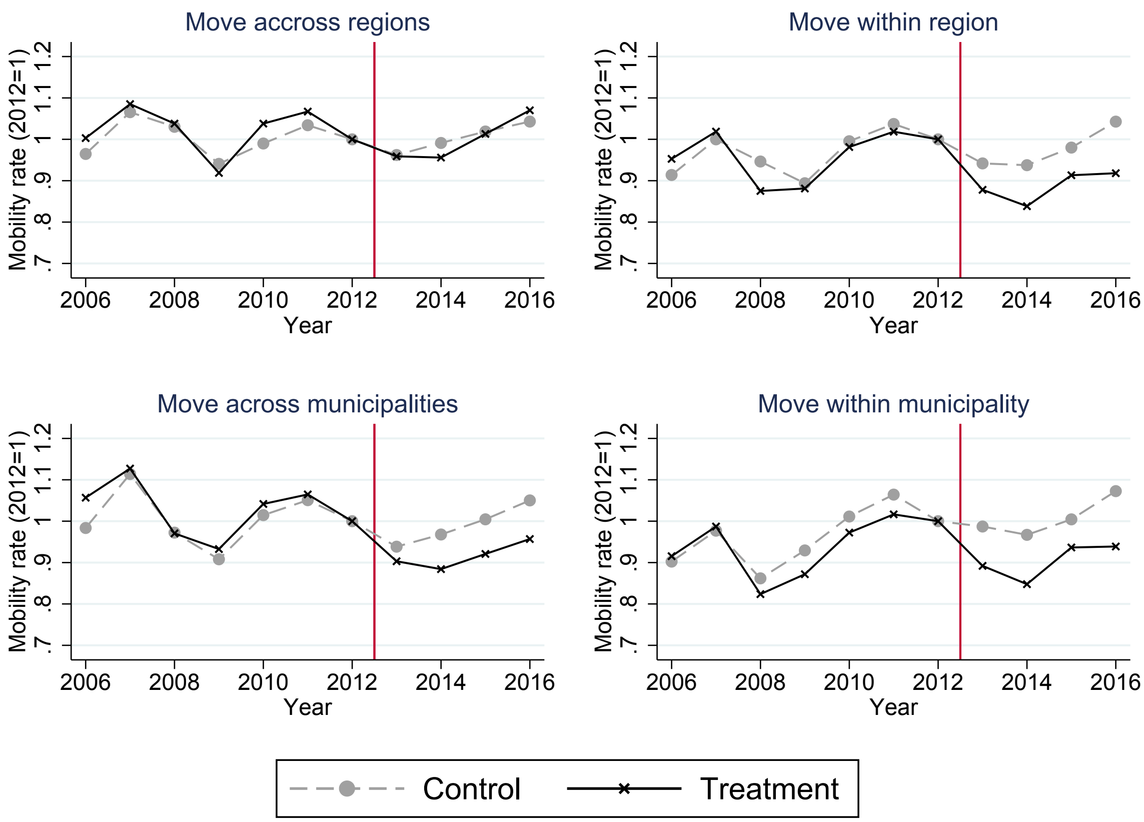
Since we do not directly observe, which moves are related to labor markets, we consider two alternative definitions for a labor market move based on different administrative regional divisions: provinces (n = 19) and municipalities (n = 320). Provinces are quite large geographic areas (level 3 in NUTS classification) and commuting across province borders is rare. Most people live and work in the same municipality as well, but commuting across municipal borders is much more common than across province borders.

Using these geographic divisions, moves that take place across regional boundaries (province or municipality) are assumed to be labor market moves while moves within a region are assumed to be motivated by housing consumption adjustment. Because provinces are so large geographically, we are likely to miss-classify some labor market related moves as housing consumption moves, i.e., some moves that take place within a province are actually labor market related. With the municipality division, the potential miss-classification runs in the other direction as some moves that take place across municipal borders are actually within labor market moves related to housing consumption adjustment.

With these caveats in mind, Figure 2 and Table 4 show the results using these two definitions. The sample includes only those who belong to the labor force (pensioners are excluded). The results are not conclusive about the effects of transfer tax on labor related moves. Moves across provinces do not seem to be affected by the tax increase so our overall results are driven by reduced mobility within provinces. On the other hand, the effect of the tax increase is quite similar for moves between and within municipalities. To the extent that moves across municipal borders include labor related moves, this would indicate that the transfer tax has some labor market effects. However, we cannot be sure whether this is the case.

[5] See also Yang (2019) and references therein.

Figure 2: Mobility between and within provinces and municipalities.



Notes: Mobility rate refers to the share of homeowners in each group who move between the end of year t–1 and the end of year t. The mobility rates are normalized to one in 2012. Group assignment is based on the homeowners' housing type in year t–1. The vertical line indicates the timing of the reform.

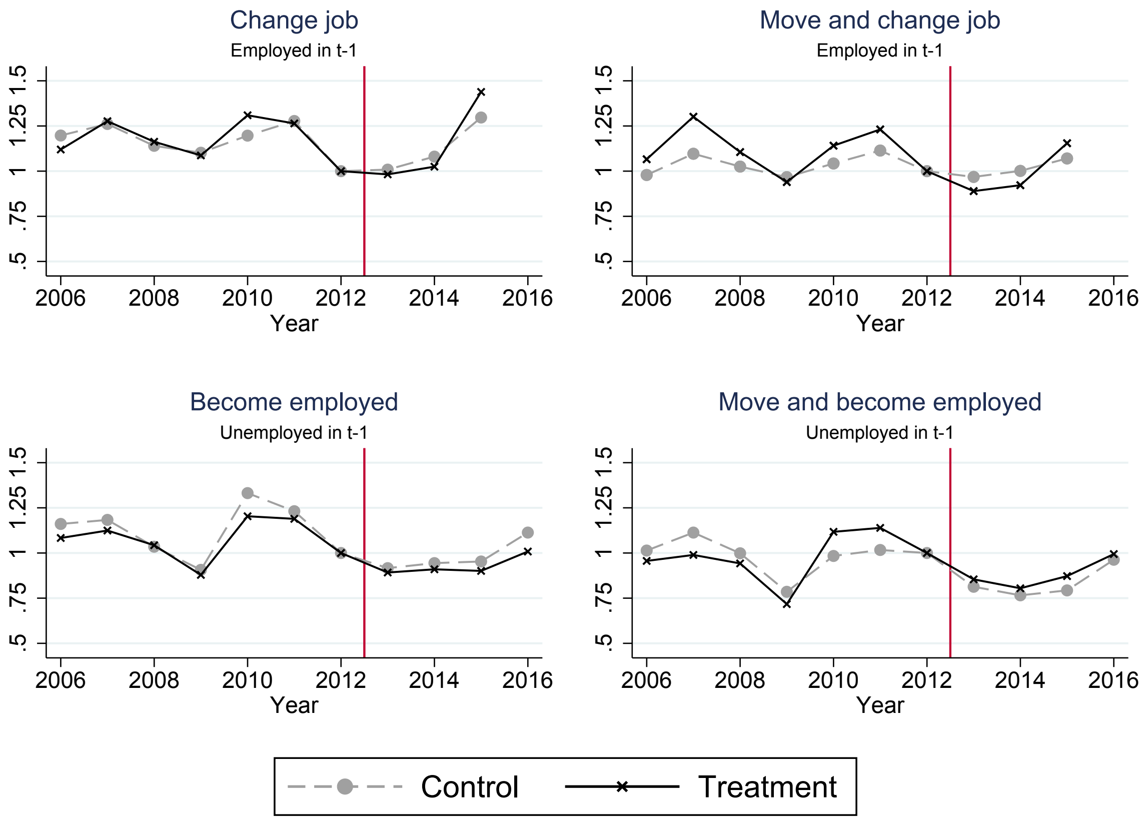
Table 4: DID results for mobility between and within provinces and municipalities.

	(1) Move across regions	(2) Move within region	(3) Move across municipalities	(4) Move within municipality
Panel A: Mobility rate				
Co-op × After	-0.000221	-0.00520***	-0.00266***	-0.00297**
	(0.000180)	(0.00153)	(0.000434)	(0.00123)
Pre mean	0.0102	0.0647	0.0271	0.0474
Panel B: Log mobility rate				
Co-op × After	-0.0221	-0.0857***	-0.0983***	-0.0848***
	(0.0156)	(0.00218)	(0.0159)	(0.0194)

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Sample size of the micro data used in the first step is approximately 11m. Sample size of the housing type-year data used in the second step is 22. Standard errors are in parentheses. Significance is based on t(9)-distribution and is denoted by asterisks: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The control variables include year fixed effects, household characteristics reported in Table 1 and postcode fixed effects (in t–1).

As a complementary strategy we look at labor market outcomes directly. We analyze the probability of changing job and becoming employed directly and conditional on moving. Figure 3 and Table 5 report the results. The left panel in Figure 3 shows the probability of changing the job (upper panel) or becoming employed (lower panel) in the treatment and control group. The right panel shows the probability of both changing job and moving to a different housing unit (upper panel) and the probability of becoming employed and moving to a different housing unit (lower panel). In all cases, the probabilities are reported relative to 2012. Based on the figure, there are no notable effects on these outcomes. This is confirmed by the corresponding DID regression results in Table 5. On the other hand, standard errors are quite large and we can not rule out important effects relative to the size of the treatment.

Figure 3: Labor market outcomes.



Notes: Mobility rate refers to the share of homeowners in each group who move between the end of year t–1 and the end of year t. The mobility rates are normalized to one in 2012. Group assignment is based on the homeowners' housing type in year t–1. The vertical line indicates the timing of the reform.

Table 5: DID results for labor market outcomes.

	(1) Change job	(2) Move and change job	(3) Become employed	(4) Move and become employed
Panel A: Levels				
Co-op × After	0.00168	-0.00198	-0.00384	0.000684
	(0.00536)	(0.00114)	(0.00451)	(0.00142)
Pre mean	0.130	0.0204	0.219	0.0267
Panel B: Logs				
Co-op × After	0.00367	-0.101*	-0.0147	0.0685
	(0.0403)	(0.0508)	(0.0227)	(0.0508)

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Sample size of the micro data used in the first step is approximately 8m in columns (1) and (2) and 900,000 in columns (3) and (4). Sample size of the housing type-year data used in the second step is 20 in columns (1) and (2) and 22 in columns (3) and (4). Standard errors are in parentheses. Significance is based on t(9)-distribution and is denoted by asterisks: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The control variables include year fixed effects, household characteristics reported in Table 1 and postcode fixed effects (in t–1).

The other important margin potentially affected by transfer taxes is the adjustment of housing consumption. Overall, transaction costs are important in determining when and how often households buy owner-occupied homes. Because size adjustments are costly and therefore only made on a very infrequent basis, homeowners effectively make quite long-term commitments to housing unit size when moving.

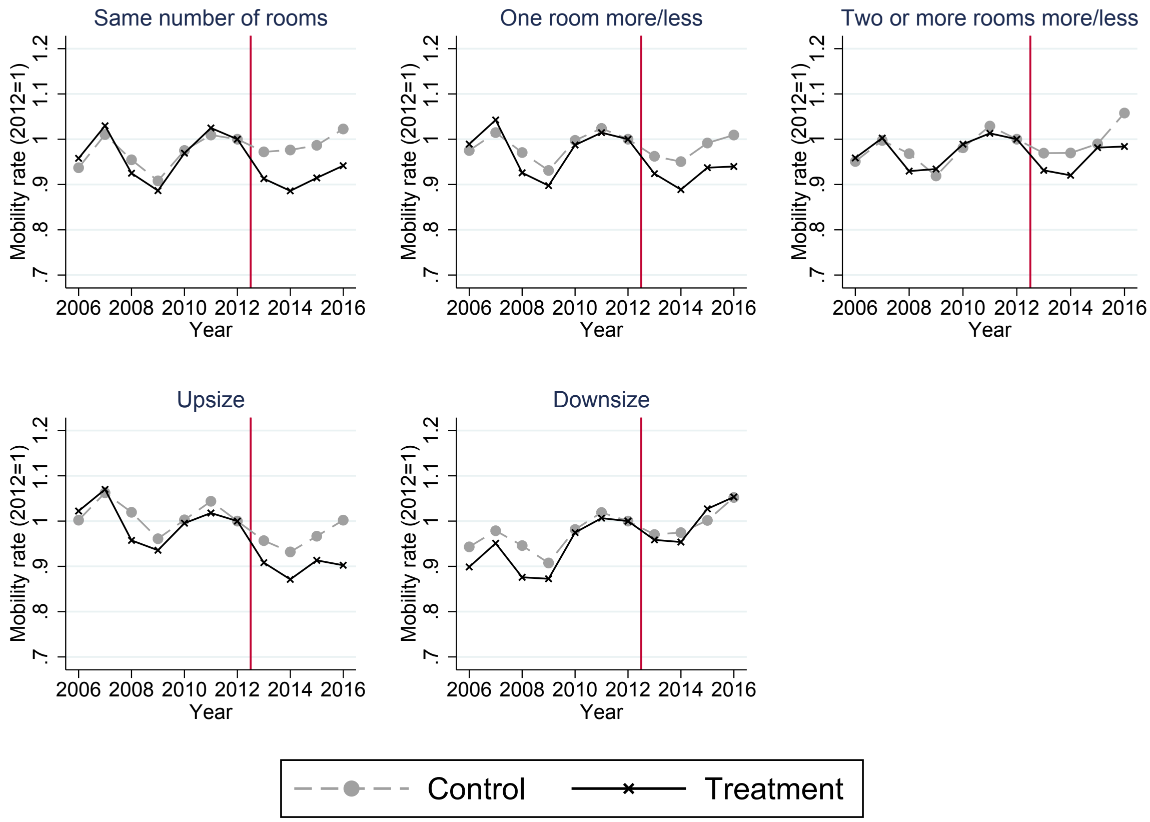
Several issues are interesting in this regard. Firstly, by increasing the transaction costs, transfer taxes affect overall welfare by affecting the equilibrium allocation of heterogeneous houses across households (see, Määttänen and Terviö (2019)). Secondly, by creating an ”inaction region” over which households do not adjust their stock of housing in response to income shocks, for example, the transaction costs also create additional volatility of non-durable consumption and influence the desire for building liquid asset buffers to smooth consumption (see, Gruber and Martin (2004)). Thirdly, by increasing the cost of housing consumption adjustments, transfer taxes may make adjustments less frequent, but larger and thereby influence the housing ladder, i.e. the idea that in different stages of their life cycle homeowners will own different homes according to their needs (see, Ortalo-Magné and Rady (2006)).

In order to understand how the transfer tax affects housing consumption adjustment, we divide the moves into different margins of housing consumption adjustments. This is of interest because one could postulate that moves that involve a small adjustment of housing consumption (measured in the number of rooms) could be more strongly affected by the reform than larger adjustments of housing consumption. Large adjustments may even become more common if households substitute many small adjustments with one large adjustment (e.g. two bedrooms more with a single move, instead of one additional bedroom at a time with two moves). Upsizing and downsizing might also be differently affected by moving costs if they are driven by different kinds of shocks to housing demand. For example, downsizing could be more often related to "forced moves", due to unemployment, divorce, illness etc., where tax incentives may play a limited role.

The results are shown in Figure 4 and Table 6. In the figure and the table moves are divided into moves where the number of rooms stays the same, to moves with a one room difference and to those with a two or more room difference. In addition, moves are divided simply as upsizing and downsizing moves.

The results indicate that the negative effect is weaker for larger adjustments in housing consumption. According to the estimates in columns 1-3 of Table 6 the tax reduces moves to same size apartments by about 8%, moves to apartments with one room more or less by 5%, and other moves by 4%. Columns 4 and 5 in Table 6 show that this result follows from a clear reduction in upsizing. Downsizing seems unaffected by the reform, which is consistent with downsizing being driven by "forced moves" where tax incentives are relatively unimportant. We acknowledge that there might be other potential explanations and note that this finding is only indicative as pre-trends for downsizing in Figure 4 are somewhat different before 2010.

Figure 4: Housing size adjustment.



Notes: Mobility rate refers to the share of homeowners in each group who move to a housing unit of the specified size between the end of year t–1 and the end of year t. The mobility rates are normalized to one in 2012. Group assignment is based on the homeowners' housing type in year t–1. The vertical line indicates the timing of the reform.

Table 6: DID results for housing size adjustment.

	(1) Same size	(2) 1 br change	(3) 2 or more br change	(4) Upsize	(5) Downsize
Panel A: Mobility rate					
Co-op × After	-0.00123***	-0.00179**	-0.000976**	-0.00302***	0.000250
	(0.000335)	(0.000628)	(0.000346)	(0.000688)	(0.000254)
Pre mean	0.0180	0.0338	0.0231	0.0368	0.0201
Panel B: Log mobility rate					
Co-op × After	-0.0788***	-0.0501***	-0.0407**	-0.0566***	0.0284
	(0.0120)	(0.0144)	(0.0139)	(0.0165)	(0.0156)

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Sample size of the micro data used in the first step is approximately 18m. Sample size of the housing type-year data used in the second step is 22. Standard errors are in parentheses. Significance is based on t(9)-distribution and is denoted by asterisks: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models include household characteristics reported in Table 1 and postcode fixed effects in the first step and the co-op main effect and year dummies in the second step.

5 Conclusions

We studied the effects of the housing transfer tax on household mobility using Finnish register data and quasi-experimental variation arising from a recent tax reform. Combining our quasi-experiment with a theoretical model of a housing market consisting of homeowner households, we showed that the transfer tax has a significant impact on mobility. We also highlighted that the quasi-experimental empirical approaches prevalent in the literature of using control and treatment groups from the same housing market can lead to substantial underestimation of the adverse effects of transfer taxes.

In addition to overall effects and the role of spillovers across market segments, we analyzed the effects of the tax reform in more detail. First, the tax reform affected more strongly moves with small housing unit size adjustment. Second, mobility decreased mainly within labor markets. Third, we did not find clear effects on job changes or on employment status. Although the difference-in-differences results are probably biased toward zero also in these cases, we can rule out large labor market effects from this tax reform. This result is in line with UK evidence reported in Hilber and Lyytikäinen (2017), but naturally, we cannot rule out labor market effects if the tax rate is increased further.

Our results should be of interest to policy-makers as transfer and transaction taxes continue to be fiscally important in many countries. We argue that the efficiency costs of these taxes are larger than previously thought. This reinforces the call to substitute away from transfer taxes towards property taxes, as highlighted by Dachis et al. (2012) and more recently by Määttänen and Terviö (2019).

References

Bertrand, M., Duflo, E., Mullainathan, S., 2004. How much should we trust differences-in-differences estimates? Quarterly Journal of Economics 119 (1), 249–275.

Besley, T., Meads, N., Surico, P., 2014. The incidence of transaction taxes: Evidence from a stamp duty holiday. Journal of Public Economics 119, 61–70.

Best, M., Kleven, H., 2018. Housing market responses to transaction taxes: Evidence from notches and stimulus in the UK. The Review of Economic Studies 85 (1), 157–193.

Cunningham, C. R., Engelhardt, G. V., 2008. Housing capital gains taxation and home owner mobility: evidence from the taxpayer relief act of 1997. Journal of Urban Economics 63, 803–815.

Dachis, B., Duranton, G., Turner, M. A., 2012. The effects of land transfer taxes on real estate markets: evidence from a natural experiment in Toronto. Journal of Economic Geography 12 (2), 327–354.

Donald, S. G., Lang, K., 2007. Inference with difference-in-differences and other panel data. Review of Economics Statistics 89, 221–233.

Fritzsche, C., Vandrei, L., 2019. The german real estate transfer tax: Evidence for single-family home transactions. Regional Science and Urban Economics 74, 131–143.

Gruber, J., Martin, R., 2004. Does housing wealth make us less equal? the role of durable goods in the distribution of wealth. Econometric Society 2004 North American Summer Meetings 15.

Hilber, C. A., Lyytikäinen, T., 2017. Transfer taxes and household mobility: distortion on the housing or labor market? Journal of Urban Economics 101, 57–73.

Kopczuk, W., Munroe, D., 2015. Mansion tax: The effect of transfer taxes on the residential real estate market. American Economic Journal: Economic Policy 7 (2), 214–57.

Määttänen, N., Terviö, M., 2019. Welfare effects of housing transaction taxes: A quantitative analysis with an assignment model. Working Paper.

Mirrlees, J., Adam, S., Besley, T., Bell, R., Bond, S., Chote, R., Gammie, M., Johnson, P., Myles, G., Poterba, J. M., Sep 2011. Tax by design. Oxford University Press.

Munch, J. R., Rosholm, M., Svarer, M., 2006. Are homeowners really more unemployed? The Economic Journal 116 (514), 991–1013.

Ortalo-Magné, F., Rady, S., 2006. Housing market dynamics: On the contribution of income shocks and credit constraints. The Review of Economic Studies 73 (2), 459–485.

Shan, H., 2011. The effect of capital gains taxation on home sales: evidence from the taxpayer relief act of 1997. Journal Public Economics 95, 177–188.

Slemrod, J., Weber, C., Shan, H., 2017. The behavioral response to housing transfer taxes: Evidence from a notched change in dc policy. Journal of Urban Economics 100, 137–153.

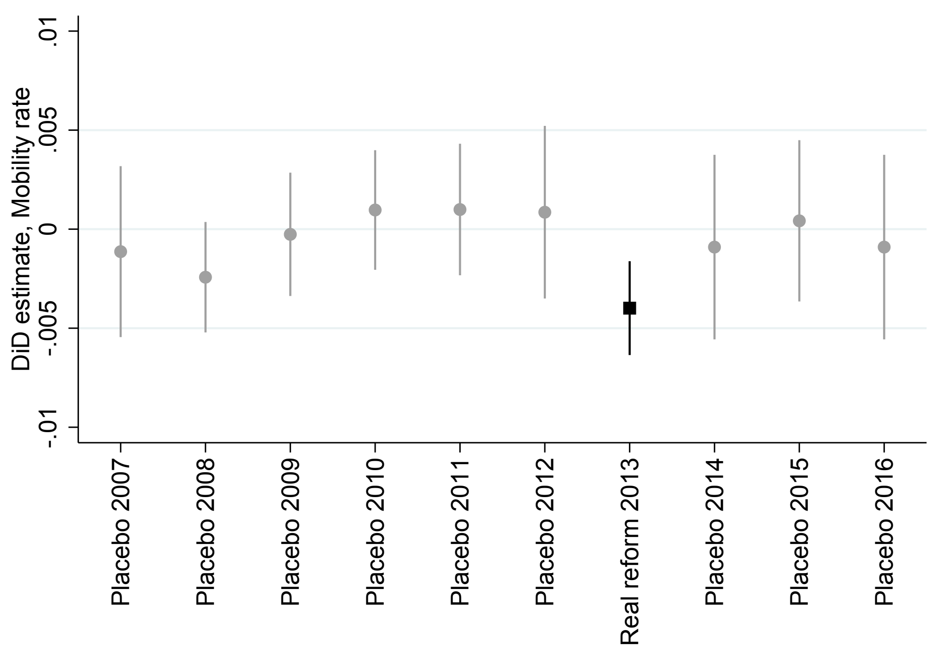
Yang, X., 2019. The effects of home ownership on post-unemployment wages. Regional Science and Urban Economics 74, 1–17.

Appendix

A Robustness and Validity Checks

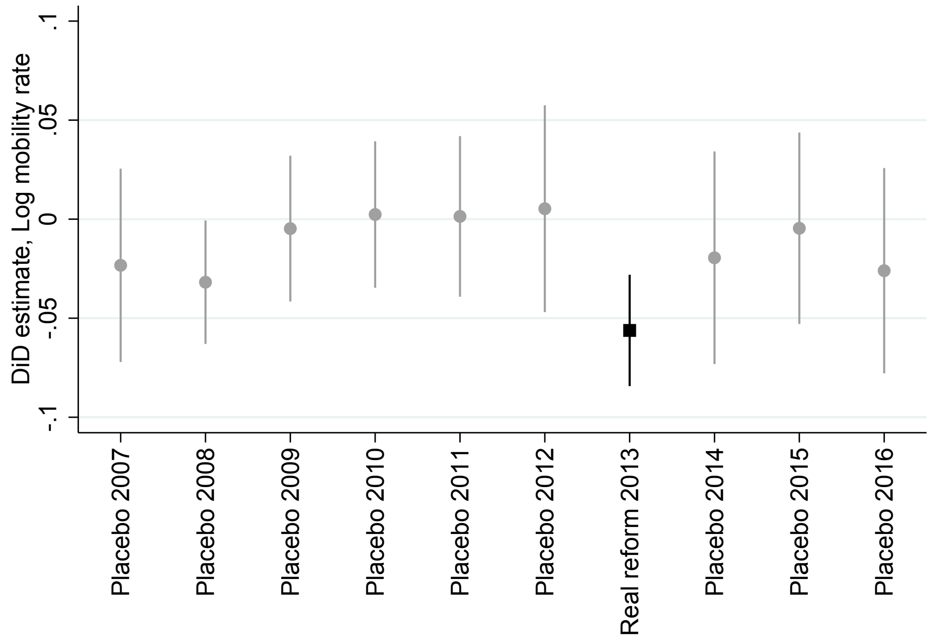
In this Appendix, we report the results for the robustness and validity checks discussed in the main text. First, Figures A1 and A2 present placebo treatments for years $2007-2016$. In Figure A1 the outcome variable is the mobility rate corresponding to Panel A of Table 3. In Figure A2 the outcome variable is the log of mobility rate corresponding to Panel B of Table 3. The placebo treatment effects come from a model that uses data for the whole period, and adds the placebo reform in our baseline model (in addition to the actual reform). The figures also report our baseline treatment effect. None of the placebo treatments are statistically significant before or after the actual reform. The latter reinforces the fact that the reform had lasting effects on the mobility rate.

Figure A1: Placebo reforms (Outcome: Mobility rate).



Notes: Placebo DID estimates using the Donald and Lang (2007) two-step procedure. All models include household characteristics and postcode fixed effects in the first step and the co-op main effect, the interaction term for the actual reform, and year dummies in the second step. Placebo reforms are included in the model one by one to in addition to the actual reform.

Figure A2: Placebo reforms (Outcome: Log mobility rate).

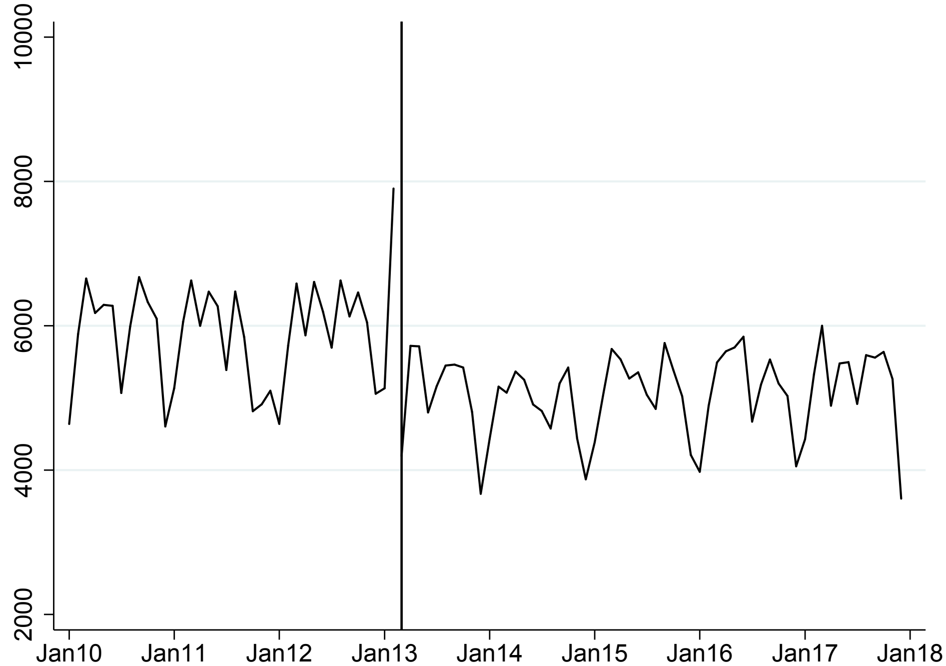


Notes: Placebo DID estimates using the Donald and Lang (2007) two-step procedure. All models include household characteristics and postcode fixed effects in the first step and the co-op main effect, the interaction term for the actual reform, and year dummies in the second step. Placebo reforms are included in the model one by one to in addition to the actual reform.

Second, our household data are at an annual level and the place of residence is recorded at the last day of the year. The tax increase was announced in October 2012 and eventually took place in March 2013. Clearly, households that were planning to move in the near future, faced an incentive to bring forward their transaction after the announcement of the reform. This anticipation effect is a problem for our estimation if the households also moved before the end of 2012.

Figure A3 reports the monthly transaction volume of co-ops from January 2010 to December 2017. As the figure shows, the reform was clearly anticipated: the transaction volume in February 2013 is unusually high. However, there seems to be no anticipation at the end of 2012. The figure also shows that there is a permanent downward shift in transaction volume after the tax increase supporting our main findings with respect to household mobility.

Figure A3: Number of transactions in co-ops (monthly).



Notes: Total transaction volume of resale co-ops based on monthly data published by Statistics Finland from Jan 2010 to Dec 2017. The vertical line indicates the timing of the reform.

Based on Figure A3 it seems that anticipation is not a serious concern in our setting. Nonetheless, in order to check the robustness of our results to these timing issues, we estimate specifications where we omit the years 2012 and 2013. The results are reported in Tables A1 and A2. Reassuringly, the results are very similar compared to our main results.

Tables A1 and A2 also reports results using different time windows. The motivation for these estimations is that observations at the beginning of the period far from the tax reform may not provide as good a point of comparison for the post-reform years as observations closer to the reform. Therefore, we vary the width of the time window around the reform from 2007–2016 to 2009–2016. Finally, we allow for differential group-specific linear time trends across the different specifications. Overall, the results seem robust to these changes in model specification. The point estimates are very close to those reported in Table 3, but become insignificant in some specifications with the narrower time windows. This is due to increased imprecision as standard errors roughly double in size, not due to changes in the size of the treatment effect.

Table A1: Robustness to time window, donut hole estimation and group-specific time trends (outcome: Mobility rate).

	(1)	(2)	(3)	(4)
Time window	2006-2016	2007-2016	2008-2016	2009-2016
Panel A: Varying time window				
Co-op × After	-0.00399***	-0.00383***	-0.00329**	-0.00388***
	(0.00105)	(0.00112)	(0.001000)	(0.000856)
Panel B: Varying time window and 2012/2013 dropped				
Co-op × After	-0.00409**	-0.00387**	-0.00316**	-0.00382**
	(0.00131)	(0.00141)	(0.00120)	(0.00110)
Panel C: Varying time window and group-specific trends				
Co-op × After	-0.00365	-0.00413	-0.00610**	-0.00529**
	(0.00202)	(0.00228)	(0.00171)	(0.00178)
Panel D: Varying time window, and group-specific trends and 2012/2013 dropped				
Co-op × After	-0.00302	-0.00390	-0.00878**	-0.00783*
	(0.00322)	(0.00398)	(0.00250)	(0.00328)
N	22	20	18	16
N (2012 and 2013 dropped)	18	16	14	12

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Standard errors are in parentheses. Significance is denoted by asterisks: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All the models include household characteristics reported in Table 1 and postcode fixed effects in the first step and the co-op main effect and year dummies in the second step.

Table A2: Robustness to time window, donut hole estimation and group-specific time trends (outcome: Log mobility rate).

	(1)	(2)	(3)	(4)
Time window	2006-2016	2007-2016	2008-2016	2009-2016
Panel A: Varying time window				
Co-op × After	-0.0562***	-0.0528***	-0.0471***	-0.0541***
	(0.0124)	(0.0126)	(0.0115)	(0.00949)
Panel B: Varying time window and 2012/2013 dropped				
Co-op × After	-0.0603***	-0.0562**	-0.0486**	-0.0568***
	(0.0152)	(0.0155)	(0.0135)	(0.0111)
Panel C: Varying time window and group-specific trends				
Co-op × After	-0.0392	-0.0457	-0.0634**	-0.0477*
	(0.0230)	(0.0257)	(0.0237)	(0.0210)
Panel D: Varying time window, and group-specific trends and 2012/2013 dropped				
Co-op × After	-0.0308	-0.0430	-0.0877*	-0.0570
	(0.0354)	(0.0433)	(0.0384)	(0.0412)
N	22	20	18	16
N (2012 and 2013 dropped)	18	16	14	12

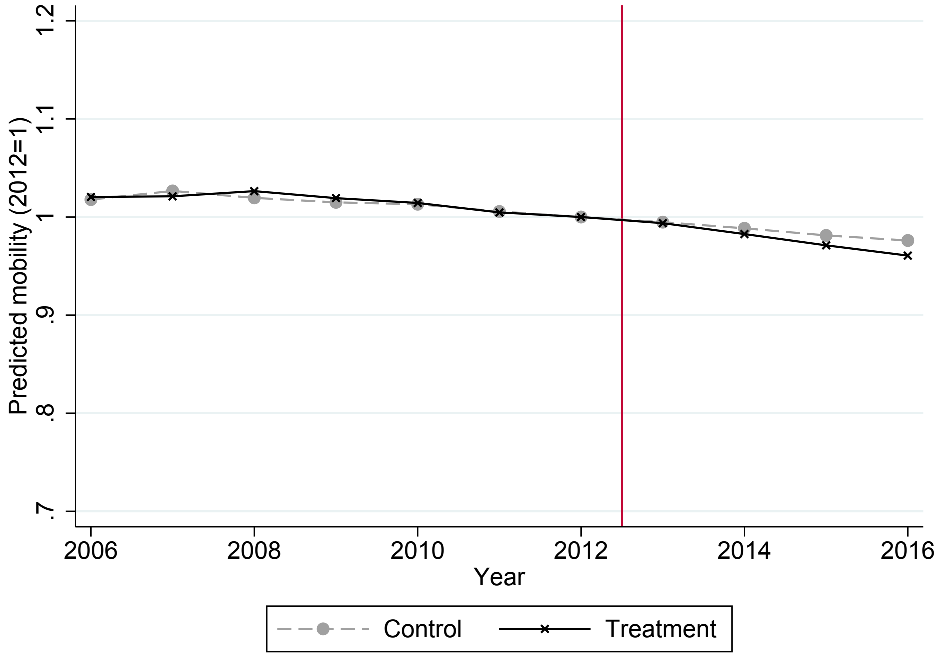
Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Standard errors are in parentheses. Significance is denoted by asterisks: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models include household characteristics reported in Table 1 and postcode fixed effects in the first step and the co-op main effect and year dummies in the second step.

Third, a potential concern is that our results might be driven by sorting of less mobile households to co-ops after the reform. Such sorting could be analyzed through balancing tests that use household charateristics as outcome variables in the DID model. Instead of balancing tests for individual characteristics, we construct and index of these characteristics that relates them to propensity to move. The index is constructed by using pre-reform data to regress the mobility dummy on all the household characteristics we use as controls, postcode fixed effects and the co-op dummy. We perform the balancing test for household and year specific predicted moving propensity from this model (holding postcode and housing type constant at base level). The results are reported in Figure A4 and Table A3.

Figure A4 shows that predicted mobility develops almost identically in the treatment and the control group before the reform, but start to gradually diverge after the reform. This is consistent with less mobile households moving in to co-ops after the reform. However, the magnitude of the divergence is small relative to our treatment effects in Section 4.

In Table A3 we provide estimates of the effect of the reform on sorting. We find a 0.1%-point or 1% reduction in predicted mobility in co-ops in post-reform years relative to the control group. The estimates are statistically significant but economically small compared to the DID estimates for the mobility effects in Table 2 (0.4%-point or 5.6% reduction in mobility). Note that we control for the household characteristics used in the prediction model in our baseline DID model. Thus, sorting on these observable characteristics should not bias our DID estimates. Sorting on unobservable characteristics not adequately proxied by our controls could affect our estimates, but we argue that this is unlikely to be a major concern in our setting as sorting can occur only gradually through mobility. Moreover, the fact that we find only very small divergence of the mobility indices based on a rich set of observable characteristics suggests that sorting on unobservables is unlikely to be an important driver of our results.

Figure A4: Sorting on observable household characteristics.



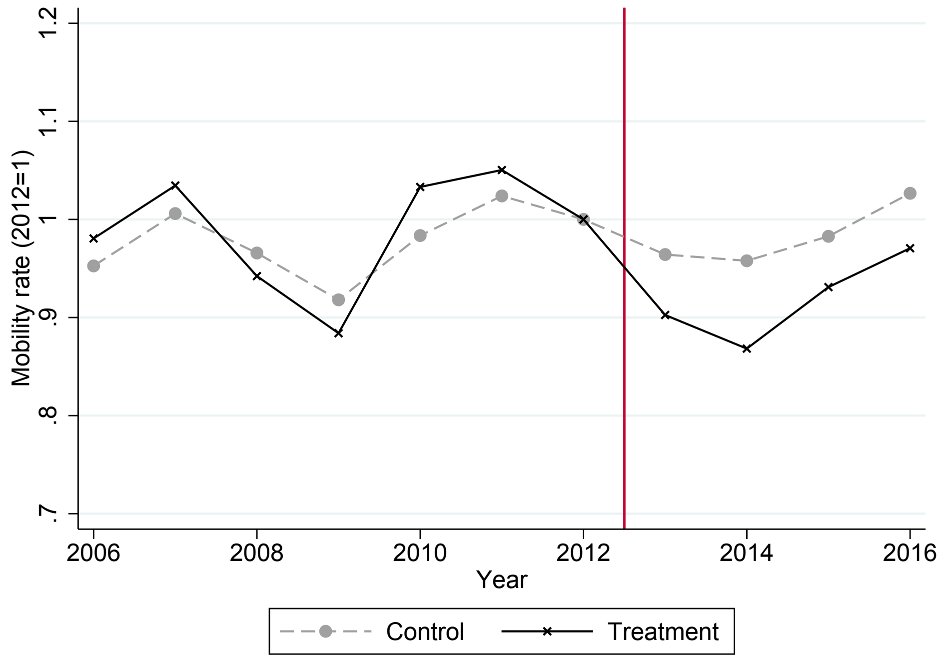
Notes: Predicted mobility rate is based on a regression of actual mobility on household characteristics and postcode FEs using pre-reform data. The results are used to calculate moving probabilities for all households in all years. The prediction is only affected by household characteristics. Postcode is fixed to reference postcode. Predicted mobility refers to the share of homeowners in each group who are expected to move between the end of year t–1 and the end of year t. The mobility rates are normalized to one in 2012. Group assignment is based on the homeowners' housing type in year t–1. The vertical line indicates the timing of the reform.

Table A3: Sorting on observable household characteristics.

	(1)	(2)
	Predicted mobility	Log predicted mobility
Co-op × After	-0.000986***	-0.00954**
	(0.000248)	(0.00303)
Pre mean	0.0749	

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Standard errors are in parentheses. Significance is denoted by asterisks: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models include the co-op main effect and year dummies in the second step.

Figure A5: Mobility rate for owners of co-op single family homes (treatment) and directly owned single family homes (control).



Notes: Mobility rate refers to the share of homeowners in each group who move between the end of year t–1 and the end of year t. The mobility rates are normalized to one in 2012. Group assignment is based on the homeowners' housing type in year t–1. The vertical line indicates the timing of the reform.

Table A4: DID results for single family homeowners.

	(1) Mobility rate	(2) Log mobility rate
Co-op × After	-0.00525***	-0.0785***
	(0.00147)	(0.0178)
Pre mean	0.0699	

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Standard errors are in parentheses. Significance is denoted by asterisks: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All models include the co-op main effect and year dummies in the second step.

B Theoretical Analysis

In this appendix, we analyze the effect of the transfer tax on household mobility using a simple model with two different types of housing units (houses and co-operatives). In the model, both housing units exist in two different varieties. One can think of these varieties as locating in different neighborhoods or cities. We calibrate the model so that it produces the empirical mobility rates before the reform as well as our DID estimate of the effect of the reform. The question we wish to address is whether and how the control group is affected by the reform.

Model There are two different housing types, co-ops (c) and houses (h). Both housing types are available in two different locations $l = \{a, b\}$.

The stock of housing type (l, t) is denoted by $n_{l,t}$. The total housing stock in then

$$n_{a,c} + n_{a,h} + n_{b,c} + n_{b,h} = 1.$$

We focus on a symmetric case where $n_{a,c} = n_{b,c} = n_c$ and $n_{a,h} = n_{b,h} = n_h$ and

$$2n_c + 2n_h = 1.$$

Initially, each household lives in one housing type. The mass of households living in each housing type is equal to the stock of that housing type.

All households then draw a monetary valuation for both housing types and locations, $u_{l,t}$. After having observed the valuations, each household makes a decision of whether to move or to stay in the current unit.

Households take prices $p = (p_{a,c}, p_{a,h}, p_{b,c}, p_{b,h})$ as given. A transaction triggers a transfer tax liability for the buyer. The tax rate may be different for houses and co-ops but the same in both locations. The after-tax price of housing type (l, t) is $(1 + \tau_t)\, p_{l,t}$ where τ_t is the transfer tax and $p_{l,t}$ is the price received by the seller. All transactions also involve a fixed non-tax transaction cost ω.

Household problem Consider first the problem of an individual household facing price vector p. The household currently living in housing unit (l, t) chooses unit (l', t') to maximize

$$u_{l',t'} + p_{l,t} - p_{l',t'} - (\tau_{t'} p_{l',t'} + \omega)\, 1_{(l' \neq l \text{ or } t' \neq t)}$$

where $u_{l',t'}$ is the value of living in housing type (l',t') and the indicator function $1_{(l' \neq l \text{ or } t' \neq t)} = 1$ if the household moves to a new unit and $1_{(l' \neq l \text{ or } t' \neq t)} = 0$ if the household stays in its current unit.

Given preferences, the best alternative for a household living in a housing type (l,t) is

$$(l^*, t^*) = \arg\max_{l',t'} \left\{ u_{l',t'} + p_{l,t} - p_{l',t'} - (\tau_{t'} p_{l',t'} + \omega)\, 1_{(l' \neq l \text{ or } t' \neq t)} \right\}. \tag{B1}$$

If

$$u_{l,t} \geq u_{l',t'} + p_{l,t} - p_{l',t'} - (\tau_{t'} p_{l',t'} + \omega) \text{ for all } l' \neq l \text{ or } t' \neq t$$

the household prefers its current house to any other alternative with the given prices.

In order to replicate the empirical mobility rates, we assume that the valuation for (l',t') of a household living in (l,t) is determined by three different components

$$u_{l',t'} = v_{l',t'} + \kappa^{l,t}_{l',t'} + \varepsilon^{l,t}_{l',t'}$$

where $v_{l',t'}$ is a random component drawn from the standard normal distribution. This component is independent of the current unit. In addition,

$$\begin{aligned} u_{A,F} &= v_{A,F} + \kappa^F + \varepsilon^F \\ u_{A,H} &= v_{A,H} \\ u_{B,F} &= v_{B,F} + \kappa^F \\ u_{B,H} &= v_{B,H} \end{aligned}$$

where

$$\kappa^{l,t}_{l',t'} = \begin{cases} \kappa^h & \text{if } t' = t = h \\ \kappa^c & \text{if } t' = t = c \\ 0 & \text{otherwise} \end{cases}$$

and

$$\varepsilon^{l,t}_{l',t'} = \begin{cases} \varepsilon^h & \text{if } l' = l \text{ and } t' = t = h \\ \varepsilon^c & \text{if } l' = l \text{ and } t' = t = c \\ 0 & \text{otherwise} \end{cases}$$

Parameters κ^h and κ^c reflect the value households living in housing unit h and c attach to units of the same type irrespective of location. In the same manner, ε^h and ε^c reflect the value a household attaches to his current unit relative to all alternatives that require moving.

The demand for housing type (l', t') by a household currently living in (l, t) is

$$d_{l',t'}^{l,t} = \begin{cases} 1 \text{ if } l^{'} = l^{*} \text{ and } t' = t^{*} \\ 0 \text{ otherwise} \end{cases} .$$

Equilibrium With given prices p, the aggregate demand for housing type (l, t) is

$$D_{l,t} = D_{l,t}^{a,c} + D_{l,t}^{a,h} + D_{l,t}^{b,c} + D_{l,t}^{b,h},$$

where $D_{l,t}^{a,c}$ is the demand for housing type (l, t) by all households living in housing type (c, a). That is, the aggregate demand for housing type (l, t) equals the demand by all households living in different housing types (including those living currently in house (l, t) and not moving).

In equilibrium, all households choose the house that maximizes their utility according to equation (B1) taking house prices as given and

$$D_{l,t} = n_{l,t}$$

that is, the demand for housing type (l, t) equals the stock of housing type (l, t).

Solving the model Because of the symmetry in the model, houses and co-ops in the two locations will have the same equilibrium price. Therefore, in equilibrium, $p_{a,c} = p_{b,c} = p_c$ and $p_{a,h} = p_{b,h} = p_h$.

The price of houses, p_h, is pinned down by the size of the transaction costs relative to the valuation shocks drawn from the standard normal distribution. If house prices are very low, the transaction costs are small relative to the valuation differences generated by the standard normal distribution. Therefore, p_h must be set such that the transactions costs are reasonable relative to the benefits of moving.[6]

We discretize the model by assuming that there are $1,000,000$ households living in each housing type. We then draw valuations $v_{l',t'}$ for each household, use equation (B1) to determine excess demand for both housing types with given co-op price p_c, and solve for a p_c which minimizes the excess demands.

[6]This is because the model features only housing consumption and no other consumption. As a result, the price level as such does not reflect the cost of housing.

Calibration We calibrate the model so that it produces the same difference-in-difference effect we found where households living in a co-op are the treatment group and households living in a house are the control group.

Before the reform, the transfer tax rates were $\tau_h = 4\%$ and $\tau_c = 1.5\%$.[7]

The mobility rates of households living in houses and co-ops in our data before the reform are reported in Table B1. Those moving to rental housing have been excluded from the figures reported in the table.

Table B1: Mobility rates in the data in 2012.

	Move to	
	House	Co-op
House	1.4	1.1
Co-op	2.0	3.4

In 2012, roughly 54% of all housing units in our data were houses and 46% co-ops. However, using these housing stocks together with the mobility rates in Table B1 would imply that, in absolute numbers, more households are moving from co-ops to houses than vice versa. As a result, we would not be able to replicate the empirical mobility rates in the model.

Therefore, we set the relative sizes of the different types of housing stocks so that absolute levels of mobility from different types of houses are equal. This requires assuming that the share of houses in the model is 66.7% and the share of co-ops is 33.3%.

The preference parameters (ε^h, ε^c, κ^h, and κ^c), the pre-reform house price, p_h, and the non-tax transaction cost parameter, ω, are chosen such that, given equilibrium prices, the model replicates the following targets:

1) The mobility rates in Table B1.

2) The empirical estimate of the mobility effect of the reform, 5.6%.

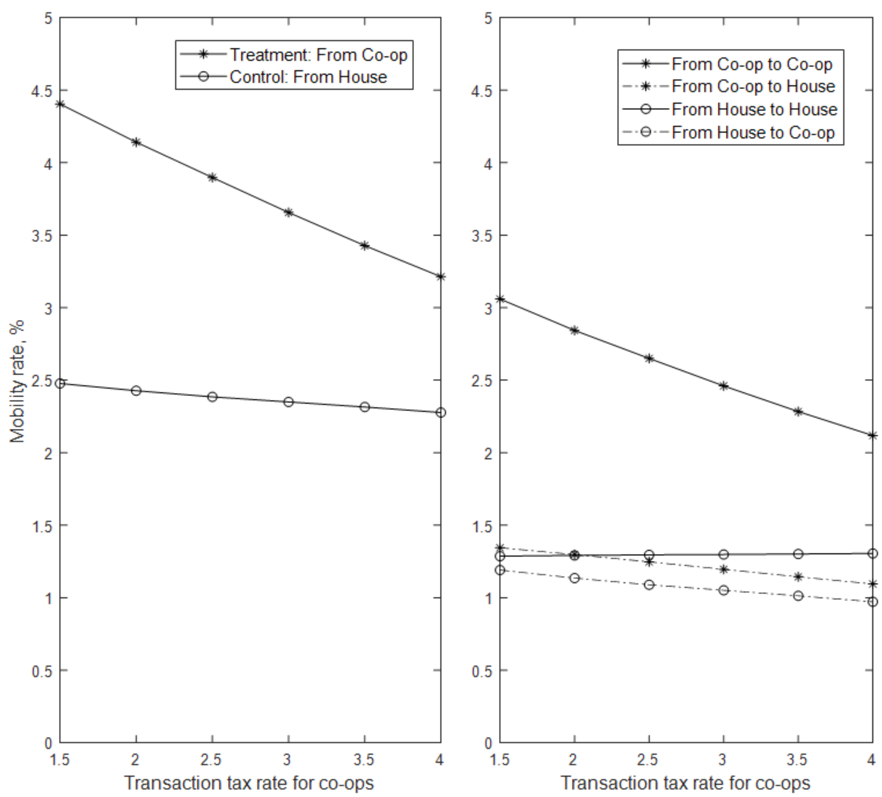
3) The non-tax transaction cost is 3% of the equilibrium house price before the reform.

The calibrated preference parameter values are $\varepsilon^h = 2.3333$, $\varepsilon^c = 2.0444$, $\kappa^h = 0.3667$, and $\kappa^c = 0.7222$. In addition, $p_h = 10.5333$ and $\omega = 0.3160$. The equilibrium price of co-ops is $p_c = 10.8852$.

[7]The tax rate on co-ops is the effective tax rate on the overall value of the co-op, that is, taking into account the housing company loan associated with the unit.

Results Figure B1 shows the mobility rates in the model in different sub-groups for six different tax regimes where the tax rate on co-ops increases from 1.5% up to 4.0% and the tax rate on houses is always 4.0%. The left panel shows the mobility rate in the treatment group homeowners living in co-ops) and control group (homeowners living in houses). The right panel in turn divides the two groups into two sub-groups according to the destination of the moves. The solid lines show the mobility rate from one housing type to the same type while the dashed lines show the mobility rate from one housing type to the other type.

Figure B1: Mobility rates in treatment and control groups (left panel) and by destination housing type (right panel).



The left panel shows that changing the tax rate on co-ops also effects mobility rate in among those homeowners living in houses (our control group). When the tax rate on co-ops is increased from 1.5% to 2%, the mobility rate of those living in houses is reduced from 2.47% to 2.44% or by some 1.4%. At the same time, the mobility rate those living in co-ops (our treatment group) is reduced from 5.47% to 5.09% or by some 7.0%.

The right panel of the figure shows the reduced mobility among those living in houses is driven by reduction in cross-moving. Moves from houses to co-ops are slightly hindered by a higher tax rate on co-ops while moves from houses to houses are not affected at all. The reason is the link between the two market segments: if those living in co-ops are less willing to move, those living in houses have fewer opportunities to move to a co-op.