Transfer Taxes and Household Mobility: Evidence from a Natural Experiment^{*}

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Abstract

We study the effect of the transfer tax on household mobility using Finnish register data on the entire population in 2005—2016. In 2013, the transfer tax rate was increased from 1.5% to 2% for co-ops (shares in housing cooperatives), but remained unchanged at 4% for directly-owned houses. Using the differences-indifferences design, we find that the transfer tax has a significant negative impact on household mobility. We complement the empirical analysis using a theoretical model which enables us to take into account spillovers between housing market segments through cross-segment mobility. Combining the two approaches implies a roughly 7% reduction in household mobility. Ignoring the spillovers would lead to a 20% underestimation of the negative effects of the transfer tax. 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. JEL: H21, R21

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1 Introduction

Housing transfer taxes influence the housing market in various ways. The most direct effects are related to transaction volume and prices. In countries where most households own their housing, such as the UK and the US, the changes in transaction volume and house prices are also closely linked to household mobility. Through household mobility transfer taxes may influence not only the allocation of housing units to households but also the allocation of jobs to employees. Typically, housing transfer taxes are considered as a very inefficient form of taxation (e.g. Mirrlees et al. (2011)). Nevertheless, empirical evidence on the importance of transfer tax on household mobility is very limited.

This study provides empirical evidence on the effects of the transfer tax on household mobility using a Finnish 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), but did not affect directly owned houses. As a result, the reform created a quasi-experimental setting which allows reliable estimation of the effects of the transfer tax 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 who were unaffected by the reform (see e.g. Best and Kleven (2018); Besley et al. (2014); Dachis et al. (2011) for similar approaches).

We complement the empirical analysis using a theoretical model which enables us to take into account household mobility from houses to co-ops and vice versa. The theoretical analysis allows us to take into account the spillovers between housing market segments. This is important especially if different housing types are close substitutes.

We use register data on the total population of Finland. This enables us to obtain a more complete picture of the effects of housing transfer taxes on mobility than in the previous literature. Most importantly, as the micro data contain a lot of information about the characteristics of the households, we are able to examine the heterogeneity of the effects and, in particular, to separate between housing related and labor market related moves.

We find that the transfer tax has a significant impact on mobility. Combining the empirical and the theoretical analysis 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. Ignoring the spillovers between different housing types would lead to an underestimation of the negative effects of the transfer tax. Our DID estimate of the effect of the tax increase is roughly 5.6%, suggesting a 20% bias in the DID estimate.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 geographical areas).

Previous studies on transfer taxes mostly focus on transactions and exploit tax reforms or discontinuities in tax schedules. Both create plausibly exogenous variation in tax rates and thereby enable researchers to isolate the effects of transfer taxes from other factors influencing housing market outcomes. Our paper is closely related especially to previous studies exploiting tax reforms which increase the tax rate on some types of houses without affecting the tax rate on others.

Best and Kleven (2018) study the effects of a temporary tax cut 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 prices. Using the 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. The data include information on an independent surveyor's valuation of the property. They also exploit the DID strategy and use independent 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%.

Dachis et al. (2011) 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, 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 effected 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. Whenever a state changes its tax rate, the remaining states function as control groups. The authors conclude that one-percentage-point increase in the tax rate is accompanied by 7% fewer transactions in the long run.

Papers exploiting discontinuties in the tax schedule include Best and Kleven (2018) and Hilber and Lyytikäinen (2017) for the UK and Kopczuk and Munroe (2015) for New York and New Jersey. All three studies conclude that transaction taxes are highly distortionary.

Hilber and Lyytikäinen (2017) is the only study in the previous literature that explicitly focuses on household mobility using British Household Panel Survey (BHPS) data. The data 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 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 (2017) 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.

The literature studying the effects of transfer taxes is also linked to studies on capital gains taxes and housing markets. The important common feature is that in both cases 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 reductin raised the mobility rate among affected households. The estimated effects are relatively large and at least Shan (2011) also finds that the short-term effect was much larger than the long-term effect.

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

The Finnish transfer tax applies to three different categories of ownership transfers: (i) real estate property including land and the residential structure (e.g. a lot with a detached house or other building or a piece of land with a summer house); (ii) shares in housing co-ops and in real estate companies (e.g. an apartment in a residential building, an office, a housing unit in a row house or a parking space); and (iii) other shares (corporate stocks, such as shares in a telephone company). Shares and other securities sold on the Stock Exchange are exempt.

Housing co-ops are legal entities (typically limited-liability companies) that own residential buildings and often also the lots under the buildings. In Finland, all multi-storey residential buildings and row houses are co-ops. In addition, the ownership of a detached house can also be organized as a co-op. In this case, the co-op usually includes several houses. 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 typically 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.

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

Until the end of February 2013, the tax rate for directly owned houses was 4%, while the tax rate for shares in co-ops was 1.6%. In both cases, the tax base was the transaction price.

In 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 housing co-op loans. For example, for a housing unit with a transaction price of 150,000 euros and an outstanding co-op loan of 15,000 euros, the transfer tax liability was 2,400 euros ($1.6\% \times 150,000$) before the reform. After the reform, the tax liability increased to 3,300 euros ($2\% \times 165,000$). The transfer tax treatment of directly owned houses remained unchanged.

The main aim of the reform was to increase tax revenue and to bring the tax treatment of co-ops and directly owned single-family 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. This trend effectively narrowed the tax base. The situation was considered undesirable as the tax burden related to a given transaction depended on how the construction was financed. In the case of resales, the co-op loans were substantially lower.

In 2012, total transfer tax revenue was roughly 580 million euros. According to the government proposal, the reform was expected to increase annual tax revenue by roughly 80 million euros. 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 on mobility come from Statistics Finland and include the entire Finnish population from 2005 to 2016. The data contain extensive information about households, including households' residence at the end of each year and whether the household is a renter or a homeowner. The data also include information on the type of the unit, i.e. 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, (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). 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.

	Single family house		Co-op		
	Mean	Std. Dev.	Mean	Std. Dev.	
Moved (t,t+1)	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	9,7	9,791,352		8,074,113	
Observations 2012	899,745		743,355		

Table 1: Summary statistics for homeowner households, 2006-2016.

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

Homeonwers living in co-ops are also more mobile than homeowners living in directlyowned houses (average annual mobility rates over the time period are 7.2% and 3.8%, respectively). In Table 2, we decompose the 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 2006-2016). For comparison, the table also reports the mobility rates for renters. At 19.2% the annual mobility rate of renters is considerably higher than the mobility rate of 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

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

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

seems fair to say that homeowners in co-ops predominantly trade with other co-op owners. However, it also seems that there exist spillovers from one market segment to the other.

3.2 Research Design

For any market transaction to occur, it needs to benefit both the buyer and the seller. The housing transfer tax drives a wedge between the cost of buying the unit and the price received by the seller of the unit. The tax therefore reduces the likelyhood that the buyer and the seller are able to settle on a mutually beneficial transaction price. This happens especially if the surplus from trade (buyer's valuation over and above the seller's valuation) is relatively small. As a result, the transaction volume is smaller than in the absence of a transfer tax and the housing units are more likely to owned by those who do not value them the most. This basic mechanism is well understood and much discussed in the literature (see, e.g. Mirrlees et al. (2011)).

In the housing market, at least if most households are homeowners, transactions are closely connected to mobility: If a homeowner chooses to sell its current housing unit, it most likely moves to another unit. Likewise, if a homeowner buys a new housing unit, it most likely moves to the new unit and sells its current one. In the absence of private households and individuals acting as landlords in the rental market, one would expect a one-to-one mapping between housing transactions and household mobility. Therefore, the transfer tax is expected to reduce household mobility and thereby lead to households living 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 the transfer tax increase to the mobility of these same households assuming that 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 feature of the Finnish transfer tax reform that the tax

was increased for shares in co-ops but not for directly-owned houses. This allows us to 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 difference-in-differences (DID) methods.

A potential problem with this approach is that all homeowners contemplating moving may be indirectly affected by the reform depending on the extent to which the transfer tax capitalizes into prices. Assume, for instance that tax inclusive co-op prices would do not increase when the tax rate on co-ops is increased. Then those living in co-ops would receive a lower price for their current housing units but would face the same the tax inclusive price of a potential new unit as before the reform. However, taking this reasoning further suggests that if mobility in the co-op owners is reduced because of the tax increase, there are fewer co-ops in the market after the reform. This would influence all households contemplating moving to a co-op.

These spillovers are probably especially likely if co-ops and houses are close substitutes and the two market segments are therefore closely linked to each other. If so, a differencesin-differences strategy may lead to downward biased estimates of the true effect of the reform.

Previous literature points towards substantial capitalization (see, e.g.Dachis et al. (2011)) and Kopczuk and Munroe (2015)). Therefore, based on the above discussion, we argue that homeowners living in co-ops are clearly affected by the reform. We therefore claim that they constitute a reasonable treatment group. In the same manner, homeowners living in directly-owned houses would seem to be a reasonable control group. In order to formally assess the validity of this assumption, we analyze the mobility effects of the tax increase in a model economy (see, Appendix B).

Our DID model takes the form

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

where *move* is equal to one if the household moved between the end of year t - 1and t and zero otherwise. The dummy variable co - op 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 if two as-

sumptions are met. 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 directlyowned 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 is likely to fail because the two housing market segments are connected through transaction chains. More specifically, if the tax increase also reduces mobility in the directly-owned houses not directly affected by the tax increase, our estimates will be biased towards zero. In Appendix B, we use a model economy to study the mobility effects. Using the model, we are able to separately analyse mobility in different housing types and thereby assess the validity of our DID assumption of no spillovers.

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 in turn was announced in October 2012 and eventually took place in March 2013. Hence, two additional issues regarding the timing of the treatment should be discussed.

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 downwards if the tax increase reduced mobility after March 2013.

The second concern is that households planning to move 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 argue that this is not a serious issue in our setting for three reasons.

First, even if a transaction was brought forward to the end of 2012, there was no incentive to expedite the move to the new house once the transaction was completed. Second, in the beginning of December the reform was postponed to March 2013 which also reduced the incentive to complete the transaction before the end of 2012. Third, based on Statistics Finland data on co-op resales the anticipation effect is very much concentrated on February 2013 (see Figure A1 in Appendix A).

Nonetheless, in order to check the robustness of our results to these timing issues, we estimate specifications where we omit both years 2012 and 2013 (see Appendix A for details).

The possible presence of group-level year effects causes problems for statistical infer-

ence in this type of models. With only two groups, standard clustering methods produce inconsistent standard errors (Wooldridge (2003)).

In order to address this issue, 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

$$move_{i,t} = c_{g,t} + v_{i,t} \tag{2}$$

where $g \in \{co - op, directly - owned\}$.

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

$$c_{g,t} = \alpha_t + \delta_1 co - op_{g,t-1} + \delta_2 after_{g,t} + \delta_3 co - op_{g,t-1} \times after_{g,t} + u_{g,t}$$
(3)

The statistical inference is based on the degrees of freedom in this group-level regression.

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 and in the right panel the mobility rates are normalized to one in 2012 just before the tax increase.

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 are similar in the treatment and control groups in the pretreatment period. This is especially clear after normalization, when we compare proportionate changes in the mobility rate relative to 2012 (right panel).¹ Finally, after the tax increase, the mobility rate decreases in both groups, but clearly more so in the treatment group.

¹We present the results of the formal pre-treatment placebo tests in Figure A2 and Figure A3 in Appendix A.



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.

Table 3 presents the DID regression results corresponding to Figure 1 using the twostep 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 an otherwise same specification except that the dependent variable is the log of mobility rate.

The regression results are in line with Figure 1 and robust across specifications. The reduction in the mobility rate in the treatment group is 0.40 percentage points. Compared

Panel A	(1)	(2)	(3)
	Mobility rate	Mobility rate	Mobility rate
Со-ор	0.0358***	0.0247***	0.0205***
	(0.000583)	(0.000630)	(0.000631)
Co-op \times 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
Со-ор	0.651^{***}	0.399***	0.319***
	(0.00864)	(0.00739)	(0.00749)
Co-op \times After	-0.0506***	-0.0560***	-0.0562***
	(0.0143)	(0.0123)	(0.0124)
HH characteristics	No	Yes	Yes
Postcode FE	No	No	Yes
Observations	22	22	22

Table 3: DID results for mobility.

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Standard errors are in parantheses. Significance is denoted by asterisks: * p < 0.1, ** p < 0.05, *** p < 0.01. The control variables include the household characteristics reported in Table 1 and postcode fixed effects (in t-1).

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 $(-0.0040 \times 743, 335)$.

We report three robustness checks in Table A1 and Table A2 in Appendix A. First, in the baseline specification, we use data for the entire time period 2006–2016. However, observations at the beginning of the time period may not provide as good a point of comparison for the post-reform years as observations closer to the reform. Therefore, we consider different specifications where we narrow the width of the time window around the reform.

Second, 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 future 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. To check whether this is the case, we drop years 2012 and 2013 from the analysis. Third, we allow for differential group-specific linear time trends.

Overall, the results seem 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.

4.2 Accounting for spillovers

Our empirical analysis of household mobility assumes that the mobility rates of homeowners living in directly-owned houses are not affected by the reform and therefore homeowners living in directly-owned houses constitute a reliable control group for the analysis. This assumption is quite standard in the literature. Several studies exploit different types of 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 or the state level real estate transfer tax in Germany) or the tax change may apply to houses in a certain price range (as in the UK stamp duty holiday). Then trade on houses located outside the geographic area or the price range is assumed not to be affected by the reform.

As we discussed in Section 3, it is unlikely that these different market segments are entirely independent from each other. This is especially true if most households also own their housing. Should they wish to move, the homeowner households are likely to be both selling and buying.

In order to analyze the role of the linkages between the market for directly-owned houses and co-ops, we build a simple theoretical model with owner-housing and mobility with two different housing types in two different locations. We use the model to uncover the mobility patterns from the different housing types before and after the tax increase. This enables us to assess the validity of our DID assumption.

In order to get an idea of the magnitude of the bias caused by the linkages to our DID estimation, we carefully calibrate the model so that it exactly replicates the empirical mobility matrix shown in Table 2 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 $2.0\%^2$

We find that a higher transfer tax for co-ops reduces especially moves from one co-op to another. However, also moves from co-ops to houses and vice versa are somewhat

²Details of the analysis are presented in Appendix B.

reduced. This is intuitive as reduced mobility in co-ops reduces effective supply in the market for also homeowners living in houses and willing to move to a co-op. On the other hand, mobility from houses to houses remains virtually unchanged. This is also to be expected as homeowners living in directly-owned houses who would have moved to a co-op before the reform are more likely to move to another house after the reform (due to reduced supply of co-ops).

Taking the reform effects generated by the model at face value would imply that the reduction in mobility rate of homeowners living in co-ops was 7.0%. That is, the DID estimate is biased downwards by 1.4 percentage points. By using the DID estimate only in assessing the effects of the reform, we would underestimate the negative effects of the reform on the mobility of homeowners living in co-ops by some 20%.

4.3 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

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

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

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, the average loan-to-value-ratio after 2013 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, the overall welfare loss lies within the interval

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

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

4.4 Different mobility types and heterogeneous responses

We next turn to studying different types of moves and heterogeneous responses. In Figure 2 and Table 4 we repeat the analysis presented in Section 4.1 by dividing the moves into different adjustments of housing consumption. The tax reduces both moves to same size (measured by the number of rooms) as well as moves to different size. Columns 4 and 5 in Table 4 show that this result follows from a clear reduction in upsizing.



Figure 2: 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.

	(1)	(2)	(3)	(4)	(5)
	Same size	1 br change	2 or more br change	Upsize	Downsize
Panel A: Mol	oility rate				
Co-op \times 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 ra	te			
Co-op \times After	-0.0788***	-0.0501***	-0.0407**	-0.0566***	0.0284
	(0.0120)	(0.0144)	(0.0139)	(0.0165)	(0.0156)
Observations	22	22	22	22	22

 Table 4: DID results for housing size adjustment.

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Standard errors are in parantheses. 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.

By affecting household mobility, the transfer tax may also influence the allocation of jobs to employees. In this respect, our study is related to the literature studying the relationship between homeownership and unemployment. For instance, Munch et al. (2006) show that in Denmark homeownership indeed lowers the propensity to move geographically for jobs while unemployed. However, homeownership also has a positive effect on the probability of finding employment in the local labour market.³

We are also interested in whether the transfer tax hinders moves between or within labor markets or both. In this regard, there are two complementary strategies: First, we can focus on the distance of the moves assuming that long-distance moves are more likely to be adjustments related to labor markets while short-distance moves (within a certain labor market) are more likely to be housing consumption adjustments. Second, we can look at the labor market outcomes directly.

The difficulty with the first approach is that we do not observe the actual distance of the move. Our data contain information about the location of the postcode area, but many moves are within the postcode area. In these cases the distance of the move is recorded to be zero which is a problem as the postcode areas vary dramatically in size.⁴

 $^{^{3}}$ See also Yang (2019) and references therein.

⁴Even if we did observe the actual distance of the move, there does not exist a clear cut measure for

Therefore, we consider two alternative definitions for a labor market move based on different regional divisions. The first regional division relies on counties (NUTS 3). There are 19 counties in Finland. As the counties are quite large geographic areas, moves between counties are likely to be moves where the household's labor market changes. Using this definition, across country moves are assumed to be labor market moves while within county moves to be housing adjustment moves.

The second definition uses the municipality division. In 2013, there were 320 municipalities in Finland. Most people live and work in the same municipality, but commuting across municipal borders is much more common than across county borders. Using this definition, across municipality moves are assumed to be labor market moves while within municipality moves to be housing adjustment moves.

Using the first definition, we are likely to misclassify some labor market related moves as housing consumption moves. Likewise, using the second definition, we are likely to missclassify some housing consumption adjustment moves as labor market related moves.

In Figure 3 shows the results using these two definitions. Focusing on regions (upper panel of the figure) suggests that mainly within region moves are affected by the tax increase. However, focusing on municipalities (lower panel of the figure) suggests that the tax increase affected mobility both between and within municipalities.

when the distance is long enough to be classified as a labor market move.



Figure 3: Mobility between and within labor markets.

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.

Focusing on regions (upper panel of the figure) suggests that mainly within region moves were affected by the tax increase. However, focusing on municipalities (lower panel of the figure) suggests that the tax increase affected mobility both between and within municipalities.

However, as discussed above, using distance to separate between labor market related and housing consumption related moves might be problematic. Therefore, as a complementary strategy we also look at labor market outcomes directly. Figure 4 reports the results.

The left panel in Figure 4 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 year 2012.

Based on the figure, there are no notable differences between the treatment and control group after the reform. However, the treatment and control groups seem to be developing differently also before the reform. Because of the absence of common trends before the reform, it is unlikely that the differences after the reform could be attributed to the reform.



Figure 4: 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.

5 Conclusion

We study the effect of the transfer tax on household mobility using Finnish micro data. In March 2013, the transfer tax rate was raised and the tax base broadened for co-ops (shares in housing co-operatives), but the tax treatment of directly-owned houses remained unchanged. This reform enables the use of the differences-in-differences design. Our results, based on household data comprising the entire population of Finland for 2005–2016, suggest that the transfer tax has a significant negative impact on household mobility. Furthermore, given that the reform increased the transfer tax rate in the treatment group on average by only 0.5 percentage points (this includes rate increase and broadening of tax base), the order of magnitude of the effect is comparable to prior studies. Our preliminary results on the heterogeneity of the responses suggest that the tax increase had a negative effect especially on housing consumption related moves.

Our empirical analysis of household mobility assumes that the mobility rates of homeowners living in single-family houses are not affected by the reform and hence they constitute a reliable control group for the analysis. This assumption is quite standard in the literature. However, it is also important to note that possible spillovers from the market for co-ops to the market for single-family houses cannot be ruled out. Based on an analysis using a model economy to uncover the mobility patterns after the tax increase, we are able to separately analyze mobility in different housing types and thereby assess the validity of our differences-in-differences assumption. When we calibrate the model to replicate the empirical mobility rates as well as the estimated reform effect, we find that a higher transfer tax for co-ops reduces especially moves from one co-op to another. However, also cross-moves (that is, moves from housing type to another) are affected by the tax increase. We therefore conjecture that our empirical strategy underestimates the effect of the increased tax rate on household mobility. Thus, it seems likely that the true mobility effect and welfare loss are somewhat larger than our DID results would indicate.

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Appendix

A Validity and Robustness Checks

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 in turn 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 A1 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 unsually high. However, it does not seem to be the case that announcement of the reform led to anticipation in the end or 2012.





Notes: 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 A1 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 years 2012 and 2013.

The results are reported in Table A1 and Table A2. The table also reports 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.

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	V			
Co-op \times After	-0.00399***	-0.00383***	-0.00329**	-0.00388***
	(0.00105)	(0.00112)	(0.001000)	(0.000856)
Panel B: Varying time window	v and $2012/20$	013 dropped		
Co-op \times After	-0.00409**	-0.00387**	-0.00316**	-0.00382**
	(0.00131)	(0.00141)	(0.00120)	(0.00110)
Panel C: Varying time window	v and group-s	pecific trends		
Co-op \times After	-0.00365	-0.00413	-0.00610**	-0.00529**
	(0.00202)	(0.00228)	(0.00171)	(0.00178)
Panel D: Varying time window	v, and group-s	specific trends	and 2012/2013	3 dropped
Co-op \times After	-0.00302	-0.00390	-0.00878**	-0.00783*
	(0.00322)	(0.00398)	(0.00250)	(0.00328)
Ν	22	20	18	16
N (2012 and 2013 dropped) \sim	18	16	14	12

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Standard errors are in parantheses. 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.

	(1)	(2)	(3)	(4)	
Time window	2006-2016	2007-2016	2008-2016	2009-2016	
Panel A: Varying time window	W				
Co-op \times After	-0.0562***	-0.0528***	-0.0471***	-0.0541***	
	(0.0124)	(0.0126)	(0.0115)	(0.00949)	
Panel B: Varying time window	w and $2012/2$	013 dropped			
Co-op \times 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 \times After	-0.0392	-0.0457	-0.0634**	-0.0477^{*}	
	(0.0230)	(0.0257)	(0.0237)	(0.0210)	
Panel D: Varying time window	w, and group-	-specific trend	ls and $2012/20$	13 dropped	
Co-op \times After	-0.0308	-0.0430	-0.0877*	-0.0570	
	(0.0354)	(0.0433)	(0.0384)	(0.0412)	
N	22	20	18	16	
N $(2012 \text{ and } 2013 \text{ dropped})$	18	16	14	12	

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

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Standard errors are in parantheses. 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.

Overall, the results seem robust to these changes in the specification. The point estimates are very close to those reported in Table 3, but become insignificant in some specifications with the narrower time windows, but this is mostly because in these specifications we have very few remaining degrees of freedom.

Finally, Figure A2 and Figure A3 present placebo treatments for years 2007 - 2016.
In Figure A2 the outcome variable is the mobility rate corresponding to Panel A of Table
In Figure A3 the outcome variable is the log of mobility rate corresponding to Panel B of Table 3.



Figure A2: 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 and year dummies in the second step.



Figure A3: 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 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). The aim is to understand the role of spillovers between market segments.

In the model, both housing units exist in two different varieties. One can think of these variaties as locating in different neighborhoods or cities. We calibrate the model so that it produces the empirical mobility rates as well as our empirical 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 units, co-ops (c) and houses (h). Both housing units are available in two different locations $l = \{a, b\}$. Hence, all together there are four housing types.

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 all housing types, $u_{l,t}$. 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 transaction tax liability for the buyer. The tax rate is 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 transaction 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 house type (l, t) chooses house (l', t') to maximise

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

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 continues to live in its current unit.

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

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

If

$$u_{l,t} \ge u_{l',t'} + p_{l,t} - p_{l',t'} - (\tau_{t'}p_{l',t'} + \omega)$$
 for all $l' \ne l$ or $t' \ne t$

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,

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

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 (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.⁵

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 (B1) to determine excess demand for all housing types with given co-op price p_c , and solve for a p_c which minimizes the excess demands.

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 transaction tax rates were $\tau_h = 4\%$ and $\tau_c = 1.5\%$.⁶

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

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

Table B1: Mobility rates in the data before the reform.

⁵This 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.

⁶The 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.

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 $p_c = 10.8852$.

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.

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.

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

