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# Revisiting the effects of housing transfer taxes

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ABSTRACT

Housing transfer taxes are fiscally important in many countries despite evidence of substantial welfare costs. We argue that the welfare costs are larger than previously thought because previous studies ignore spillovers between treatment and control groups. We analyze the effect of transfer taxes on household mobility using a quasi-experiment arising from a tax reform. To account for spillovers between treatment and control groups, we use a housing market model calibrated to match the mobility rates in our micro data and our quasi-experimental mobility effect estimate. Ignoring the spillovers leads to a 20% underestimation of the negative mobility effect.

## 1. Introduction

Housing transfer taxes are typically seen as an inefficient form of taxation, but are nonetheless fiscally important in many countries (e.g. Mirrlees et al. 2011 and Andrews et al. 2011). Transfer taxes drive a wedge between the cost of buying a house and the price received by the seller and thereby reduce the likelihood of a mutually beneficial transaction. Because of the tax distortion, in some cases, no transaction occurs even though the prospective buyer values the house more highly than the current owner. In countries where most households own their home, such as the UK and the US, transfer taxes may also affect household mobility as moving requires the sale and purchase of houses. Through their effects on mobility, transfer taxes may influence not only the allocation of housing units to households, but also the allocation of jobs to employees.

We study the effects of the housing transfer tax on household mobility in Finland, a country with a high homeownership rate, using high-quality register data on the total population from 2005 to 2016. We exploit variation from a tax reform, which raised the transfer tax rate of apartments in multi-unit buildings but did not affect the tax rate on single-family detached houses. This quasi-experimental setting can be analyzed using a difference-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. However, in a housing market setting this type of design may be flawed due to spillovers between the treatment and control groups. If homeowners in the treatment group move less often because of the tax increase, homeowners in the control group may also be indirectly affected as they have less trading partners to interact with. As a result, the DID estimate would be biased towards zero as it is a combination of the true treatment effect and a spillover effect.

The key contribution of this paper is to quantify the bias caused by the spillover effect. We are able to do so because we observe in our data not only if a household moves, but also to which housing type it moves. This allows us to calculate the mobility rates between and within the treatment and control groups. We combine this additional information and our quasi-experimental DID estimate with a model of a housing market featuring two housing types with different tax treatments, realistic mobility costs and homeowner households who act as both buyers and sellers. More specifically, we calibrate the model to reproduce the pre-reform mobility rates in our micro data and our DID estimate when we implement the tax reform in the model. This allows us to back out the post-reform mobility rates in the model including the spillover effect.

Our main finding is that the transfer tax has a significant negative impact on mobility. Combining quasi-experimental analysis with a model-based analysis, we find a roughly 7.2% reduction in treatment group mobility due to a 0.5 percentage point increase in the transfer tax rate.

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Our DID estimate for the effect is roughly 5.6%, suggesting a 22% downward bias in the estimate. The bias arises because mobility also decreases by 1.6% in the control group. Our estimate for the cost of public funds of the reform taking the spillover effect into account is 2.3, while the estimate relying only on the DID estimate would be 1.3. Thus ignoring the spillover effect would lead to a substantial underestimation of the welfare costs of the transfer tax.

Prior empirical literature relies heavily on similar comparisons of treatment and control groups consisting of different segments of the same housing market. A number of papers use price notches in the tax schedule for identification. Besley et al. (2014), Hilber and Lyytikäinen (2017) and Best and Kleven (2018) study the effects of the UK Stamp Duty Land Tax using price notches in the tax schedule. 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. Slemrod et al. (2017) study a series of transfer tax reforms introducing discontinuous jumps in tax liability in Washington DC.

Dachis et al. (2012) and Fritzsche and Vandrei (2019) exploit both spatial and temporal variation in transfer taxes. Dachis et al. (2012) analyze the introduction of the Land Transfer Tax in the city of Toronto. The reform set a 1.1% tax rate on transactions in the city of Toronto, but no tax in other parts of the Greater Toronto housing market area, thus dividing the market into treatment and control groups. 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.

All the papers, with the exception of Slemrod et al., 2017, find that transfer taxes have a substantial negative effect on the number of transactions or moves. However, comparing housing transactions just below and above a tax notch or within and outside a geographic area consistently identifies a causal effect only if there is no trading across the tax notch or across regions. Our results suggest that standard quasi-experimental empirical strategies may lead to substantial underestimation of the adverse effects of transfer taxes.

In addition to the overall effects, our rich register data with detailed information on household characteristics and their housing units allow us to obtain a more complete picture of the effects of the tax reform in the labor and housing markets. First, we analyze outcomes related to the labor markets (e.g. Munch et al. 2006, Battu et al. 2008 and Yang 2019). We find that the transfer tax affects short-distance moves (less than 50 km) more strongly, but we also find negative effects on long-distance moves (more than 50 km), suggesting that the transfer tax also affects the labor market. This result is in contrast with the only previous paper studying mobility instead of transactions – Hilber and Lyytikäinen (2017) – which finds that the transfer tax only affects short-distance moves (10 km or less) in the UK.

Second, we analyze more closely the different margins of housing consumption adjustments highlighted in the literature on housing consumption over the life-cycle (e.g. Ortalo-Magné and Rady 2006, Flavin and Nakagawa 2008, Attanasio et al. 2012 and Li et al., 2016). As one would expect, the tax increase affected moves involving small adjustments in housing unit size most strongly. However, these effects are asymmetric so that upsizing became less frequent, but there were no effects on downsizing. This asymmetry is in line with a life-cycle model where credit-constrained households gradually climb the housing ladder by making small upgrades in unit size with multiple moves and downsize maybe only once towards the end of the life-cycle (e.g. Ortalo-Magné and Rady 2006 and Attanasio et al. 2012). Together these results suggest that when transaction costs increase, upsizing takes place through fewer moves over the life-cycle, but downsizing may be unaffected.

The paper is organized as follows. In the next section, we describe the Finnish transfer tax system and the reform that we exploit in the analysis. In Section 3, we present the data and the research design. Section 4 presents our main empirical results. In Section 5, we use a theoretical model to analyze the quantitative importance of spillovers. Section 6 presents further empirical results on different types of moves. Section 7 concludes.

## 2. Institutional setting and reform

In Finland, the housing transfer tax applies to all housing transactions, both new construction and resales. The buyer is responsible for paying the tax and officially becomes the owner of the housing unit only after the transfer tax payment has been received by the tax authorities. First-time buyers under the age of 40 are exempt from paying the tax.

As in many other countries, not all transactions face the same tax rate. In the Finnish system, the tax rate depends on the type of housing unit. Currently, the tax rate for housing units in housing co-operatives is 2% and 4% for properties, meaning single-family houses.

All residential buildings with multiple housing units are legally set up as housing co-operatives. The co-operatives own the building (or sometimes multiple buildings on the same lot) and often also the lot under the building. When buying a housing unit in a housing co-operative, one buys shares in the co-operative corresponding to a certain housing unit. Owning the shares in practice implies owning the unit.[2] Co-operatives often have outstanding loans taken out during the construction of the building or at some later stage for renovation. The loans are allocated to the shares and the owner of the shares is responsible for the corresponding portion of the loans. We refer to this type of housing units as *apartments*.

In the case of properties, the ownership structure is simpler: one directly owns the structure and typically also the lot under the structure. Because all properties are single-family detached houses, we refer to these housing units as *houses*.[3]

We exploit a tax reform that increased the transfer tax burden on apartments, while the tax treatment of houses remained unchanged. Until the end of February 2013, the transfer tax rate was 4% for houses and 1.6% for apartments. In both cases, the tax base was the transaction price. On March 1, 2013, the transfer tax rate for apartments was raised from 1.6% to 2% and the tax base was broadened to include housing co-operative loans linked to the apartment. For example, for an apartment with a transaction price of 200,000 euros and an outstanding housing co-operative loan of 20,000 euros, the transfer tax liability was 3200 euros ($1.6\% \times 200,000$) before the reform. After the reform, the tax liability increased to 4400 euros ($2\% \times (200,000 + 20,000)$). Personal housing loans do not affect the transfer tax.[4]

The main aim of the reform was to increase tax revenue and to bring the tax treatment of apartments and houses closer together. According to the government law proposal, the size of housing co-operative 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.

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, on December 5, 2012 it was announced that the reform would be post-

[2] For instance, the owner may sell the shares, renovate the apartment or rent it out without the consent of the other shareholders. In this respect the Finnish housing co-operatives have similarities with condominiums in the US.

[3] If several single-family houses are located on the same lot, their ownership structure may be organized as a housing co-operative. When buying such a housing unit, one buys shares in the co-operative, not the property. In our taxonomy, these are apartments.

[4] In Finland, housing loans are always full recourse loans.

**Table 1**
Summary statistics for homeowner households, 2005 – 2016.

| | House | | Apartment | |
|---|---|---|---|---|
| | 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 children | 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.

**Table 2**
Mobility rates of homeowners by origin and destination housing type.

| | House | | Apartment | |
|---|---|---|---|---|
| | Pre-reform | Post-reform | Pre-reform | Post-reform |
| Moved to house | 0.014 | 0.013 | 0.019 | 0.017 |
| Moved to apartment | 0.010 | 0.009 | 0.033 | 0.028 |
| Moved to rental | 0.015 | 0.015 | 0.022 | 0.023 |
| Total | 0.039 | 0.037 | 0.075 | 0.068 |

poned 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 on households, including households' end-of-year residence, whether the household is a renter or a homeowner and whether the unit is a house or an apartment.

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 (owned apartment, owned house or 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 comprise homeowners in houses (our control group) and the next two columns comprise homeowners in apartments (our treatment group). The homeowners in apartments differ somewhat from households living in houses. For example, they are more educated, often single and live in urban areas. They also move more often than homeowners living in houses as the average annual mobility rates over the time period are 7.2% and 3.8%.

In Table 2 we decompose the annual mobility rates according to housing type before and after the tax reform. This decomposition reveals that there are significant spillovers between market segments. Before the reform, 44% (0.033/0.075) of the moves by homeowners in apartments are to another apartment and 25% to a house. Correspondingly, 36% of the moves by homeowners in houses are to another house and 26% to an apartment. Table 2 also allows us to calculate a simple DID estimate, which according to the last row is −0.5 percentage points ((0.068 − 0.075) − (0.037 − 0.039)). Next, we describe our formal DID design.

### 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 it would be in the absence of the transfer tax.

In a housing market with a high homeownership rate, transactions are closely connected to mobility as for homeowners moving often requires the sale and purchase of 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, size or other characteristics.

In order to study the magnitude of these effects, we would ideally compare the mobility of households after the transfer tax increase to the mobility of these same households in a situation where the transfer tax was not increased. 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 apartments. Since the tax was increased for transactions involving apartments, we expect mutually beneficial trading opportunities to diminish. This would translate into lower mobility among homeowners living in apartments, our treatment group. As the tax on houses was not increased, we can construct the counterfactual using homeowners living in 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

$$\begin{aligned} move_{i,t} = {} & \alpha_t + \delta_1 apartment_{i,t-1} + \delta_2 after_{i,t} \\ & + \delta_3 apartment_{i,t-1} \times after_{i,t} + \beta' X_{i,t-1} + u_{i,t}, \end{aligned} \tag{1}$$

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 *apartment* indicates the treatment group, which consists of homeowners who lived in an apartment at the end of year $t-1$. The control group consists of homeowners who lived in a house at the end of year $t-1$. The dummy variable *after* indicates the time period after the tax increase. The vector $X$ denotes the control variables, which include household characteristics (see Table 1) and postcode fixed effects.

The parameter for the interaction term, $\delta_3$, has a causal interpretation under three assumptions. The first is the common trends assumption, which means that in the absence of treatment the mobility of homeowners living in apartments and 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 other reforms or events coinciding with the transfer tax reform and affecting the treatment and control groups differently. We are unaware of any such reforms or events taking place during this time period.

Finally, we need to assume that there are no spillovers between the treatment and control groups. That is, the mobility of households in the control group is not affected by the mobility decisions of households in the treatment group. This assumption fails if the two housing market segments are connected. Especially if apartments and houses are close substitutes, reduced mobility of homeowners living in apartments may also influence homeowners living in houses and looking for a new unit.

According to Table 2, we cannot rule out such spillovers. If the tax increase also reduces mobility among the control group of homeowners

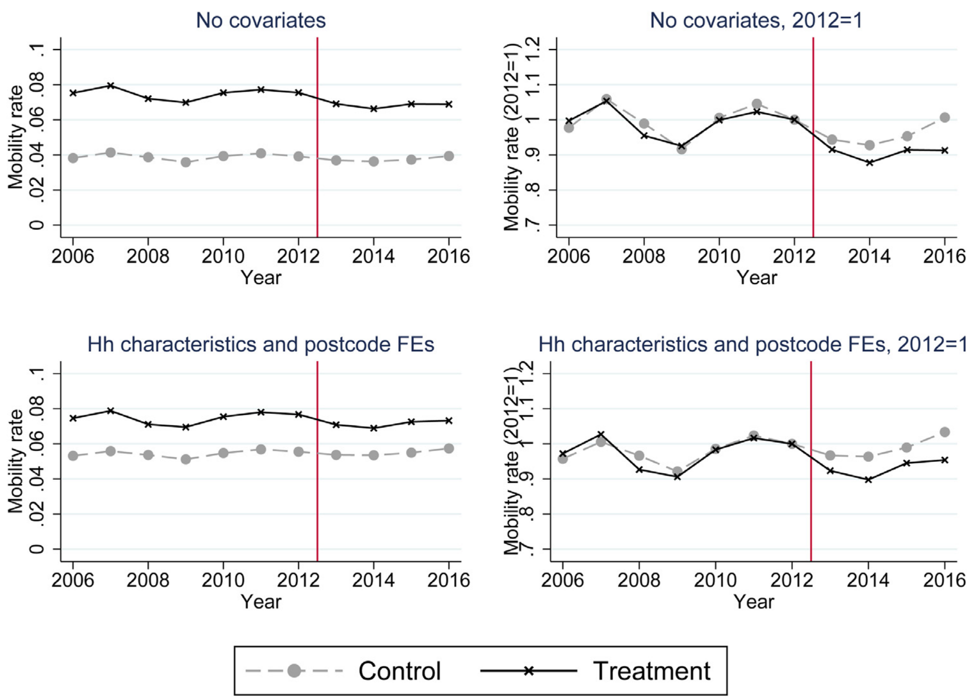


**Fig. 1.** Mobility rate for homeowners in apartments (treatment) and in houses (control). *Notes*: The left panel presents the group-specific mobility rates. In the right panel, the mobility rates are normalized to one in 2012. Mobility rate refers to the share of homeowners who move between the end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

in houses, our estimates will be biased towards zero. Thus, after presenting our baseline DID results, we build a housing market model and calibrate it to replicate the mobility rates between housing market segments in our data and the DID treatment effect. This allows us to identify the spillover effect and 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 the annual level and place of residence is recorded on 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 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 rate. This anticipation effect might also have induced them 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 conducting a number of robustness checks.

Finally, the nature of the policy reform has important implications for statistical inference. Although the data cover the entire population, there are actually only two relevant groups (apartment owners and house owners), which we compare in different years. First, failing to take into account the unobserved group-year effects would produce downward-biased standard errors, but standard clustering methods are not feasible with only two groups and 11 years. Second, when the number of groups is small in a DID setting, applying standard asymptotics leads to overstating the significance of the $t$-statistics. 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 $g \in \{apartment, house\}$.

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

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

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.[5] We use the year fixed effects $\alpha_t$ as additional 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. In Fig. 1, the left panel presents the group-specific mobility rates, and in the right panel the mobility rates are normalized to one in 2012.

Three observations stand out from Fig. 1. First, the mobility rate is clearly higher in the treatment group than in the control group throughout the time period (upper left panel). This is true even after controlling for household characteristics and adding postcode fixed effects (lower left panel). 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 crisis, but the groups develop very similarly during the last four pre-treatment years. This is especially clear after normalization, when we compare changes in the mobility rate relative to 2012 (right panel). Formal placebo tests also point to pre-treatment common trends (see Figures A1 and A2 in

[5] Donald and Lang (2007) propose weighting the second-step regression by the standard errors of $c_{g,t}$ to gain precision. In our data, weighting has no practical importance because the standard errors are almost identical. We therefore 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 this is a minor concern in our setting after controlling for common year effects.

**Table 3**
DID results for mobility.

| Panel A | (1) Mobility rate | (2) Mobility rate | (3) Mobility rate |
|---|---|---|---|
| Apartment | 0.0358*** | 0.0247*** | 0.0205*** |
| | (0.000583) | (0.000630) | (0.000631) |
| Apartment × 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 |
| Apartment | 0.651*** | 0.399*** | 0.319*** |
| | (0.00864) | (0.00739) | (0.00749) |
| Apartment × After | -0.0506*** | -0.0560*** | -0.0562*** |
| | (0.0143) | (0.0123) | (0.0124) |
| Year FE | Yes | Yes | Yes |
| HH characteristics ($t-1$) | No | Yes | Yes |
| Postcode FE ($t-1$) | No | No | Yes |

*Notes*: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. The sample size of the micro data used in the first step is approximately 18M. The 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: * $p < .1$, ** $p < .05$, *** $p < .01$. Household characteristics are reported in Table 1.

Online Appendix A.) Finally, after the tax increase, the mobility rate decreases in both groups, but clearly more so in the treatment group. The divergence also persists for four years after the reform.

Table 3 presents the DID regression results corresponding to Fig. 1 using the two-step procedure of Donald and Lang (2007). In the first column, the first-step 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 a specification where the dependent variable is the log of the mobility rate.

The regression results are in line with Fig. 1 and robust across specifications. The reduction in the mobility rate in the treatment group is roughly 0.4 percentage points. Compared to the pre-treatment mobility rate, this implies that the mobility rate decreased by 5.6%. This translates to roughly 3000 fewer moves per year ($-0.004 \times 743,355$).

### 4.2. Additional robustness and validity checks

In addition to the placebo tests, we conducted a number of robustness and validity checks. First, we tested 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 end-of-year situation, 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 Online Appendix A reports the monthly transaction volume of apartments 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.[6] Nonetheless, in order to check the robustness of our results to these timing issues, we estimate specifications where we omit years 2012 and 2013. As the comparison of Panel A and Panel B in Table A1 and Table A2 shows, the results are not affected by this omission.

Table A1 and Table A2 also report our main estimation for different time windows. One may argue that 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 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 using the procedure suggested by Goodman-Bacon (2021). We also estimate a specification where we replaced the separate postcode and year fixed effects with postcode-by-year fixed effects. Overall, although the point estimates in these specifications vary somewhat, they are consistently negative.

Finally, by increasing the cost of moving for those living in apartments, the tax reform may have induced less mobile households to sort into apartments. In Online Appendix A, Figure A4 and Table A3 indicate that, after the reform, the apartment homeowner group indeed becomes less mobile in terms of observable characteristics predicting mobility. However, the sorting is very gradual and small in magnitude. It cannot therefore explain the immediate and large decrease in mobility after the tax increase. Nonetheless, the gradual sorting is consistent with our overall observation that transfer taxes affect household behavior.

## 5. Accounting for spillovers

Our DID analysis is similar to several other studies in relying on comparison of treatment and control groups that consist of different segments of the housing market. This approach uncovers a causal effect if there are no spillovers between the segments. In our case, it assumes that the mobility rates of homeowners living in houses are not affected by the reform and therefore these homeowners constitute a reliable control group for the analysis. However, as was shown in Table 2, this assumption is not valid as moves happen between the housing market segments. If lower mobility among homeowners living in apartments has an effect on the mobility of homeowners living in houses, the DID estimate is a combination of the treatment effect and a spillover effect.

In order to separate the spillover and treatment effects, we impose more structure on mobility behavior by building a model with owner-occupied housing, two market segments (apartments and houses) and a competitive housing market. To identify the spillover effect within the model, we use the DID estimate and the data on mobility between market segments that was not used in our DID approach. More specifically, we discipline our model with two elements from our data. First, the model must reproduce the empirical mobility patterns between different market segments before the reform shown in Table 2. Second, the model must reproduce our DID estimate of 5.6% when the tax rate on apartments is increased from 1.5% to 2.0% while the tax rate for houses remains at 4%.

[6] 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.

### 5.1. Model

We consider a competitive housing market model with two housing market segments that correspond to our treatment and control groups: apartments and houses, $d = \{a, h\}$. In order to allow for mobility within and between housing market segments, both exist in two different qualities representing all other aspects of the housing quality distribution. For simplicity, we refer to them as locations, $l = \{1, 2\}$ (say, neighborhoods or cities).

We assume that the overall housing supply is perfectly inelastic. This is a reasonable approximation as we focus on the short run effects.[7] The stock of housing type $d$ in location $l$ is denoted by $n_{l,d}$ and

$$n_{1,a} + n_{1,h} + n_{2,a} + n_{2,h} = 1.$$

We focus on a symmetric case where $n_{1,a} = n_{2,a} = n_a$ and $n_{1,h} = n_{2,h} = n_h$. The symmetry assumption implies that in equilibrium prices are equal in both locations.[8]

Each household lives in one housing unit, and the number of households living in each housing type and location is equal to the number of housing units of that type in that location.

All households draw a monetary valuation for both housing types and locations, $u_{l,d}$. After that, each household makes a decision of whether to move or to stay in the current unit. All households own their housing. Hence, moving to a new unit involves selling the current unit and buying a new one.

Households take prices as given. If the household buys a new unit, it will have to pay a transfer tax. The tax rate is different for houses and apartments, but same in both locations. The after-tax price of a housing type $d$ in location $l$ is $(1 + \tau_d)p_{l,d}$ where $\tau_d$ is the transfer tax and $p_{l,d}$ is the price received by the seller in location $l$. In addition to the transaction tax, all transactions involve other costs related to mobility. This non-tax transaction cost is a fixed share ($\omega$) of the house price.

As our DID analysis focuses on homeowners, we do not explicitly model the decision to become a renter. Instead, we assume that a fixed share of homeowners move to rental housing. Their housing units are in turn assumed to be bought by former renters buying their own housing unit.

*Household's problem*

Consider first the problem of a household facing prices $p$ and living in housing type $d$ in location $l$. The household chooses unit $(l', d')$ to maximize its utility. Given preference shocks and prices, the best alternative for the household is

$$(l^*, d^*) = \arg\max_{l',d'} \{u_{l',d'} + p_{l,d} - p_{l',d'} - (\tau_{d'} p_{l',d'} + \omega p_{l,h}) 1_{(l' \neq l \text{ or } d' \neq d)}\}. \tag{4}$$

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

That is, the household prefers its current housing unit to any alternative if

$$u_{l,d} \geq u_{l',d'} + p_{l,d} - p_{l',d'} - (\tau_{d'} p_{l',d'} + \omega p_{l',h'}) \text{ for all } l' \neq l \text{ or } d' \neq d. \tag{5}$$

The overall valuation of a household living in $(l, t)$ for unit $(l', d')$ is determined by three different components

$$u_{l',d'} = v_{l',d'} + \varepsilon^{l,d}_{l',d'} + \kappa^{l,d}_{l',d'}$$

where $v_{l',d'}$ is a random component drawn from the standard normal distribution and independent of the current unit. Parameter $\varepsilon^{l,d}_{l',d'}$ reflects the value a household attaches to its current unit relative to all alternatives that require moving. Parameter $\kappa^{l,d}_{l',d'}$ in turn reflects the value a household attaches to its current housing type irrespective of location. More specifically,

$$\varepsilon^{l,d}_{l',d'} = \begin{cases} \varepsilon^h \geq 0 & \text{if } l' = l \text{ and } d' = d = h \\ \varepsilon^a \geq 0 & \text{if } l' = l \text{ and } d' = d = a \\ 0 & \text{otherwise} \end{cases}$$

and

$$\kappa^{l,d}_{l',d'} = \begin{cases} \kappa^h \geq 0 & \text{if } d' = d = h \\ \kappa^a \geq 0 & \text{if } d' = d = a \\ 0 & \text{otherwise} \end{cases}$$

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

$$d^{l,d}_{l',d'} = \begin{cases} 1 \text{ if } l' = l^* \text{ and } d' = d^* \\ 0 \text{ otherwise} \end{cases}.$$

*Equilibrium*

Given the preferences, the transaction costs and the distribution of different housing types in the two locations, we solve for prices such that the excess demand for both housing types in both locations is equal to zero.

With given prices $p$, the aggregate demand for housing type $d$ in location $l$ is

$$D_{l,d} = D^{1,a}_{l,d} + D^{2,a}_{l,d} + D^{1,h}_{l,d} + D^{2,h}_{l,d},$$

where, for instance, $D^{1,a}_{l,d}$ is the demand for housing type $d$ in location $l$ by all households living in housing type $a$ in location 1.

In equilibrium, all households choose the unit that maximizes their utility according to equation (4) taking prices as given and

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

that is, the demand for housing type $d$ in location $l$ equals its stock.

### 5.2. Solving the model

Because of the symmetry in the model, houses and apartments in the two locations will have the same equilibrium price. Therefore, in equilibrium, $p_{1,a} = p_{2,a} = p_a$ and $p_{1,h} = p_{2,h} = p_h$.

For solving the model, the relevant issue is the price difference between different types of units, not the price levels. Therefore, we set $p_h$ exogenously and then solve for $p_a$ that minimizes the excess demand for different housing types in different locations.

However, we must take into account that the level of $p_h$ determines the non-tax transaction cost ($\omega \times p_h$). If $p_h$ is low, the non-tax transaction cost is small relative to the valuation differences generated by the standard normal distribution and the mobility rates in the model will be high. In contrast, if the non-tax transaction cost is high, the mobility rates in the model will be low. To replicate the empirical mobility rates, $p_h$ must be set such that the transaction costs are reasonable relative to the benefits of moving. Therefore, the level of $p_h$ will be determined as part of the calibration process.[9]

We set the size of the population in the model so that it roughly matches the annual number of observations in our data. This means that there are 400,000 households in each housing type. We then draw valuations $v_{l',d'}$ for each household from the standard normal distribution, use equation (4) to determine excess demand for all units and solve for $p_a$ such that the excess demands are very close to zero.[10]

[7] In the longer run, transfer taxes may affect housing investment through their negative effects on prices (see e.g. Lyytikäinen 2009). However, because these indirect effects on mobility operate through the overall price level, they are likely to be small relative to the direct effects.

[8] To solve for the equilibrium with asymmetric housing stocks, we would have to target empirical mobility rates between houses and apartments in different locations. However, there is no natural dimension along which to divide our data (location or otherwise).

[9] Alternatively, we could fix $p_h$ and calibrate the standard deviation of the normal distribution.

[10] In Online Appendix B, we analyze the relevance of finite sample by drawing 1000 realizations for $v_{l',d'}$ and solving the model using the parameters of the baseline calibration.

**Table 4**
Mobility rates in the model before and after the reform.

| | House | | Apartment | |
|---|---|---|---|---|
| | Pre-reform | Post-reform | Pre-reform | Post-reform |
| Moved to house | 0.014 | 0.014 | 0.019 | 0.018 |
| Moved to apartment | 0.010 | 0.009 | 0.033 | 0.029 |
| Moved to rental | 0.015 | 0.015 | 0.022 | 0.022 |
| Total | 0.039 | 0.038 | 0.074 | 0.069 |

*Notes*: Mobility rates from houses and apartments to houses, apartments and rental units in the model when $\tau_a = 1.5\%$ (before reform) and $\tau_a = 2.0\%$ (after reform). Mobility to rental housing is constant by assumption.

### 5.3. Calibration

Before the reform, the transfer tax rates were $\tau_h = 4\%$ and $\tau_a = 1.5\%$.[11] We set the exogenous mobility rate from owner-housing to rental housing at 1.5% from houses and at 2.2% from apartments following Table 2. In the baseline calibration, the non-tax transaction cost parameter is set at $\omega = 0.03$.

We normalize $\kappa^a = 0$. The other preference parameters ($\varepsilon^h$, $\varepsilon^a$ and $\kappa^h$) and $p_h$ are chosen such that, given equilibrium prices, the model replicates the following targets as closely as possible:

1) The pre-reform mobility rates from owner-housing to owner-housing in Table 2.
2) The empirical estimate of the mobility effect of 5.6% when we increase the tax rate for apartments from 1.5% to 2.0%.

In the model, absolute levels of mobility from apartments to houses and vice versa are always equal. In the data, this need not be the case because of household formation, new housing construction etc. Therefore, we set the distribution of the housing stock so that the model is able to replicate the empirical mobility patterns. This requires that the share of houses in the model is 65% and the share of apartments is 35%.[12]

The calibrated preference parameter values are $\varepsilon^h = 1.9739$, $\varepsilon^a = 1.8224$ and $\kappa^h = 1.1385$. In addition, $p_h = 15.600$ and the equilibrium price of apartments is $p_a = 16.4713$.

In Online Appendix B, we vary the non-tax transaction cost parameter $\omega$ and show that the results are robust to these changes.

### 5.4. Results

Table 4 shows the mobility rates in the model by housing type before and after the reform. When the tax rate on apartments is increased from 1.5% to 2%, the mobility rate of those living in houses is reduced from 3.90% to 3.84% or by some 1.6%. At the same time, the mobility rate of those living in apartments is reduced from 7.40% to 6.86% or by some 7.2%.

In the model, the reform affects not only moves between apartments but also moves from apartments to houses and vice versa. After the reform, homeowners living in apartments are less likely to find welfare-improving trades. As a result, the effective supply of apartments relative to houses is reduced. This means that the price of apartments increases relative to houses. Consequently, homeowners living in houses are also less likely to find welfare-improving trades.

Overall, the reform therefore reduces the mobility rate of homeowners living in apartments by 7.2% in the model. As the model reproduces our DID estimate of 5.6%, this means that the DID estimate is biased downwards by 1.6 percentage points. Using 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 apartments by 22%.

These results are robust to the choice of the non-tax transaction cost parameter $\omega$, as Table B1 in Online Appendix B shows. When we vary $\omega$ from 0.01 to 0.05, the reform effect in the model for homeowners living in apartments is close to 7.2%. Of course, the calibrated $p_h$ and preference parameters change when $\omega$ changes. Given $\omega$, $p_h$ pins down the cost of moving while the preference parameters $\varepsilon^h$ and $\varepsilon^a$ determine the benefit of staying in the current unit. Since the model is calibrated to match the mobility rates in the data, a higher $\omega$ must be accompanied by a lower $p_h$ and/or a weaker preference for the current unit relative to all the alternatives, which require moving.

### 5.5. Welfare effects

When the transfer tax is increased, some moves are no longer mutually beneficial for the buyer and the seller. In addition, it is possible that changes in relative prices due to the tax increase alter the preferred alternative of some households. The aggregate welfare cost of the tax increase therefore consists of two different parts: 1) the utility loss related to the forgone moves and 2) the utility loss of those households who move regardless of the reform, but choose a less suitable unit after the reform because of changes in relative prices.[13]

The size of this welfare cost can be illustrated by relating the welfare loss 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 cost of public funds (CPF) is equal to one. The larger the welfare cost related to the tax, the larger the $CPF$.

The $CPF$ can be expressed as

$$CPF = \frac{\Delta W + \Delta R}{\Delta R} \tag{6}$$

where $\Delta W$ refers to the welfare loss resulting from increasing the tax rate from $\tau_a = 1.5\%$ to $\tau_a = 2.0\%$ and $\Delta R$ is the additional tax revenue collected. We use the model to calculate the $CPF$ by comparing the tax revenue and the aggregate welfare before and after the reform.

In our baseline calibration, $CPF = 2.3$. Further analysis of the distribution of trades in the model before and after the reform reveals that a very small share of those who move regardless of the reform move to a different type of unit. This means that the welfare costs are almost entirely due to forgone moves.

One useful benchmark for the value of the $CPF$ is a back-of-the-envelope calculation using the DID estimate. This is what a researcher recovering a quasi-experimental estimate of the mobility effect would be able to report. In our context, this back-of-the-envelope calculation using the DID estimate of 5.6% only gives the $CPF$ a range from 1.2 to 1.3.[14] This is a much lower figure as this calculation underestimates the amount of forgone moves from apartments and totally misses the forgone moves from houses.

Because of spillovers, the tax rate on houses ($\tau_h = 4.0\%$) also turns out to be an important factor contributing to the welfare cost. To see this, note first that all potential moves from apartment to apartment with a value lower than 1.5% of the transaction price are deterred by the transfer tax already before the reform. By the same token, all moves with a value less than 2.0% of the price will be deterred after the reform. Therefore, the value of forgone moves because of the reform must range from 1.5% to 2.0% of the apartment price.

[11] The tax rate on apartments is the effective tax rate on the overall value of apartments, i.e. taking into account the housing company loan associated with the unit.

[12] In the data in 2012, the shares of houses and apartments are 55% and 45%.

[13] We abstract from analyzing the division of surplus, which might depend on household characteristics in markets with differentiated goods (e.g. Harding et al., 2003 and Merlo and Ortalo-Magne, 2004). In our model, the housing market is perfectly competitive and households act as price-takers, and there is no heterogeneity in household type that would lead to idiosyncratic price differences.

[14] For details, see Online Appendix C.

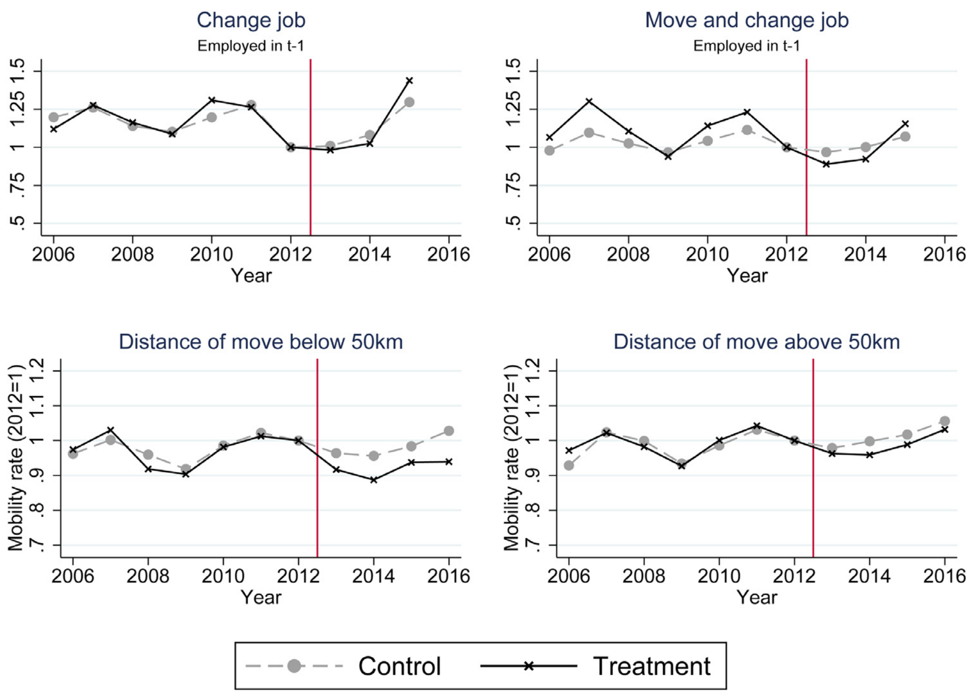


**Fig. 2.** Effects according to job change and distance of move. *Notes*: Mobility rate refers to the share of homeowners in each group who move between the end of year $t-1$ and the end of year $t$. The mobility rates are normalized to one in 2012. Group assignment is based on homeowners' housing type in year $t-1$. The vertical line indicates the timing of the reform..

Similarly, all potential moves from apartment to house with a value lower than 4% of the transaction price are deterred by the transfer tax before the reform. Because the tax rate on houses does not change after the reform, the value of any forgone moves due to the reform must exceed 4.0% of the house price. The capital losses created by the reduction in after-tax price set an upper bound for the value of the forgone moves. In practice, the value of these forgone moves is close to 4.0% of house value. Therefore, increasing the tax rate on apartments from 1.5% to 2.0% is more costly in welfare terms if the tax rate that applies to the substitute (houses) is high. In order to assess the quantitative importance of this effect, we solve the model using the parameters of our baseline calibration and assuming that $\tau_h = 1.5\%$. In this case, $CPF = 1.7$. That is, the relatively high tax rate on houses substantially increases the welfare cost of increasing the transfer tax on apartments.

Comparing our welfare cost estimate to other estimates in the literature is not straightforward.[15] The estimate from the model is relatively high given that the initial tax rate (1.5%) is towards the lower end of the tax rates considered previously, whereas the back-of-the-envelope estimate is closer to these previous estimates. We argue that the welfare cost estimates in the literature are downward-biased. Our analysis highlights that the size of the bias depends in subtle ways on the extent of spillovers between the treatment and control groups and the tax rate levels.

## 6. Effects on different types of moves

### 6.1. Labor market

Although the analysis in Section 5 shows that we underestimate the negative effects of the transfer tax using the DID design, it is nonetheless interesting to study different types of moves. We next consider the labor market and housing market implications of the tax reform more closely.

As suggested by Mirrlees et al. (2011), transfer taxes may influence labor market matching through decreased household mobility.[16] We use two complementary strategies to analyze labor market effects. First, we look at labor market outcomes directly by analyzing whether the reform had an effect on the probability of changing jobs, both with and without conditioning on moving. Second, we differentiate moves according to the distance of the move. The main motivation for analyzing the distance of move is that moving and changing jobs may not occur in the same year, and thus, our first strategy may miss some labor market related moves.

We report these results in Fig. 2 and Table 5. The upper panel in Fig. 2 shows the probability of changing job in the treatment and control groups and the lower panel shows moves according to distance. Based on the figure, there are no notable effects on job changes, although according to the regression results there is a marginally significant negative effect on job changes conditional on moving. However, the pre-treatment trends are not particularly clean for this outcome. At the same time, the standard errors are quite large for these results and we cannot rule out important effects relative to the size of the treatment.

The lower panel in Fig. 2 shows results where we divide the moves into short- and long-distance moves with a threshold of 50 km. This threshold is of course somewhat arbitrary, but as a point of reference, the median commuting distance in Finland in 2015 was roughly 14 km. The results show that while the effect is stronger for short-distance moves, the tax increase also lowered the mobility rate for long-distance moves. According Table 5, the effect on long-distance moves is roughly half the size of the effect on shorter moves. As long-distance moves are often related to the labor market, this suggests that the transfer tax has labor market effects. This result is in contrast with Hilber and Lyytikäinen (2017), who find that the transfer tax only affects short-distance moves (10 km or less) in the UK.

[15] Määttänen and Terviö, 2019 use a one-sided assignment model of the housing market to assess the welfare cost of transfer taxes using data from Finland. They also discuss different empirical estimates in the literature and compare the implied welfare cost in a systematic manner.

[16] In this respect, our study is related to the literature studying the relationship between homeownership, which involves higher moving costs compared to renting, and unemployment. See e.g. Oswald (1996), Munch et al. (2006), Battu et al. (2008), and Yang (2019).

**Table 5**
DID results according to job change and distance of move.

| | (1) Distance of move below 50km | (2) Distance of move above 50km | (3) Change job | (4) Move and change job |
|---|---|---|---|---|
| **Panel A: Levels** | | | | |
| Apartment × After | -0.00372*** | -0.000320** | 0.00168 | -0.00198 |
| | (0.000979) | (0.000111) | (0.00536) | (0.00114) |
| Pre mean | 0.0637 | 0.0112 | 0.130 | 0.0204 |
| **Panel B: Logs** | | | | |
| Apartment × After | -0.0608*** | -0.0333** | 0.00367 | -0.101* |
| | (0.0136) | (0.0109) | (0.0403) | (0.0508) |

*Notes*: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. The sample size of the micro data used in the first step is approximately 11M in columns (1) and (2) and 8M in columns (3) and (4). The sample size of the housing type-year data used in the second step is 22 in columns (1) and (2) and 20 in columns (3) and (4). Standard errors are in parentheses. Significance is based on t(9)-distribution and is denoted by asterisks: * $p < .1$, ** $p < .05$, *** $p < .01$. All the models include the household characteristics reported in Table 1 and postcode fixed effects in the first step and the apartment main effect and year dummies in the second step.

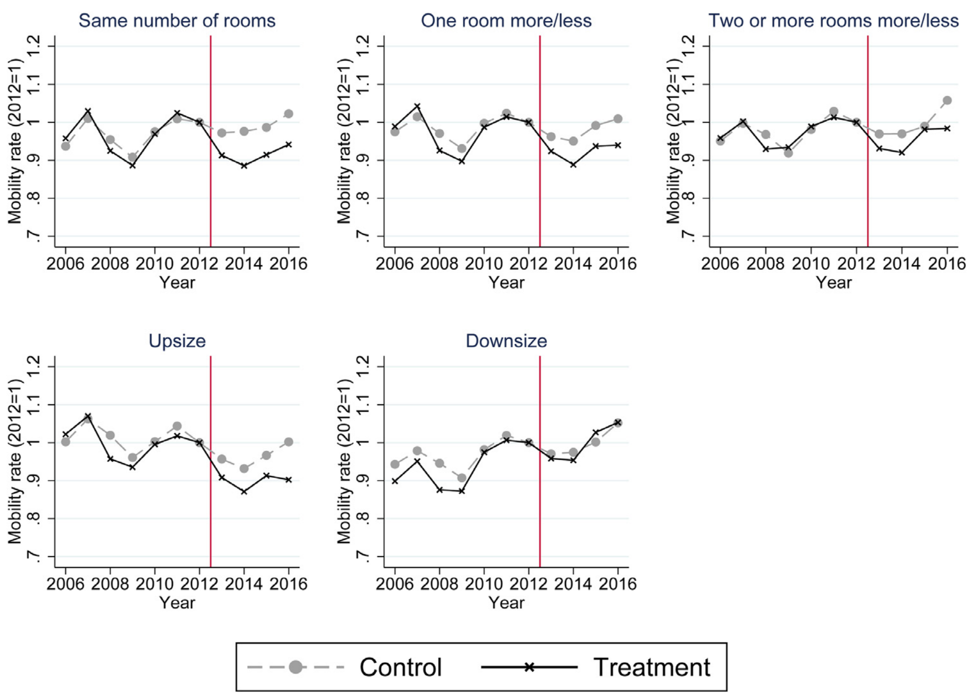


**Fig. 3.** Housing size adjustment. *Notes*: Mobility rate refers to the share of homeowners in each group who move to a housing unit of the specified size between the end of year $t-1$ and the end of year $t$. The mobility rates are normalized to one in 2012. Group assignment is based on homeowners' housing type in year $t-1$. The vertical line indicates the timing of the reform.

### 6.2. Housing consumption

The second important margin potentially affected by transfer taxes is adjustment of housing consumption, especially from a life-cycle perspective. Several issues are interesting in this regard.[17] First, by increasing the cost of housing consumption adjustments, transfer taxes may make adjustments less frequent and thereby influence the housing ladder, i.e. the idea that at different stages of their life-cycle homeowners will own different-sized homes (e.g. Ortalo-Magné and Rady 2006, Attanasio et al. 2012 and Bajari et al., 2013). Second, by creating an inaction region over which households do not adjust their stock of housing in response to income shocks, for example, transaction costs may create additional volatility in non-durable consumption and influence the desire for building liquid asset buffers to smooth consumption (e.g. Grossman and Laroque 1990, Flavin and Nakagawa 2008 and Yang 2009). Finally, prior literature has identified credit constraints as an important reason why young households move up the housing ladder gradually with several moves, whereas downsizing usually happens later in life with fewer or just a single large adjustment. Upsizing and downsizing might also be driven by different kinds of shocks. For example, downsizing could be more often related to "forced moves" due to unemployment, divorce or illness, where tax incentives may play a limited role.[18]

We can study these mechanisms, as our data allow us to divide moves into different margins of housing consumption adjustments. We first analyze the size of the adjustment in terms of number of rooms. We divide the moves into those where the number of rooms stays the same, changes by one and changes by two rooms or more. This allows us to study separately the effect on small and large adjustments. Based on the above discussion, large adjustments may even become more common if households take fewer, but larger steps when climbing the housing ladder. We also divide the moves into upsizing and downsizing. Given the above discussion, we would expect to observe a larger impact on upsizing compared to downsizing.

The results are shown in Fig. 3 and Table 6. As Fig. 3 shows, in general, the trends in mobility rates are quite similar in the treatment and control groups in the pre-treatment period. However, when looking at downsizing there are some differences in the development right after the financial crisis. According to columns (1)-(3) of Table 6, the tax

[17] See Piazzesi and Schneider (2016) for a survey of studies analyzing the life-cycle aspects and implications of housing consumption and homeownership.

[18] Fischer and Khorunzhina (2019) analyze how homeownership and housing demand is influenced by divorce risk.

**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** | | | | | |
| Apartment × 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** | | | | | |
| Apartment × 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. The sample size of the micro data used in the first step is approximately 18M. The 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: * $p < .1$, ** $p < .05$, *** $p < .01$. All models include the household characteristics reported in Table 1 and postcode fixed effects in the first step and the apartment main effect and year dummies in the second step.

reduces moves to same-size housing units by about 8%, moves to units 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 to be unaffected by the tax increase, which is consistent with these moves being larger or being driven by forced moves where tax incentives are relatively unimportant.

The asymmetry might also be, at least partly, explained by the spillovers between the different housing market segments discussed Section 5. In our control group, those upsizing are more likely to move to another house than to an apartment, whereas those downsizing are more likely to move to an apartment than to another house.[19] As a result, in the control group, those upsizing are probably less affected by the reform as they move from house to house. Those downsizing, in turn, may be indirectly affected by the reform due to the reduced mobility of homeowners living in apartments. If so, our DID estimates related to downsizing may be more biased towards zero.

## 7. Conclusions

We studied the effects of housing transfer taxes on household mobility using Finnish register data and quasi-experimental variation arising from a recent tax reform. Combining quasi-experimental analysis with a housing market model, we showed that transfer taxes have significant negative impacts on mobility. We also highlighted that the quasi-experimental 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 on mobility and welfare.

Our quantitative estimates may have limited external validity, because the magnitude of the biases depends on the institutional context, such as linkages between treatment and control groups and the level of transfer tax rates. Nevertheless, our findings should lead to a reassessment of the existing evidence and be of interest to policy makers as housing transfer taxes continue to be fiscally important in many countries.

In addition to the overall effects and the role of spillovers between market segments, we analyzed the effects of the tax reform in more detail. First, the tax increase affected both short- and long-distance moves (below and above 50 km), but the effect was larger on short-distance moves. The result for long-distance moves is novel in the literature and suggests that housing transfer taxes affect the functioning of the labor market when the homeownership rate is high. Second, the tax reform affected moves with small adjustments in housing unit size more strongly and had a larger effect on upsizing compared to downsizing. This asymmetry suggests that transfer taxes may distort the life-cycle profile of housing consumption and thereby savings and portfolio choices, and magnify the effects of income and house price risk.

[19] This pattern is probably mostly explained by differences in the size distributions: houses are often quite large while apartments also include studios or two-room apartments. For details on the mobility patterns, see Table A4 in Online Appendix A.

## Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2021.103367

## CRediT authorship contribution statement

**Essi Eerola:** Conceptualization, Methodology, Formal analysis, Investigation, Data curation, Writing - original draft, Writing - review & editing, Visualization, Funding acquisition. **Oskari Harjunen:** Conceptualization, Methodology, Investigation, Data curation, Writing - original draft, Writing - review & editing. **Teemu Lyytikäinen:** Conceptualization, Methodology, Formal analysis, Investigation, Data curation, Writing - original draft, Writing - review & editing, Visualization, Funding acquisition. **Tuukka Saarimaa:** Conceptualization, Methodology, Investigation, Data curation, Writing - original draft, Writing - review & editing.

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