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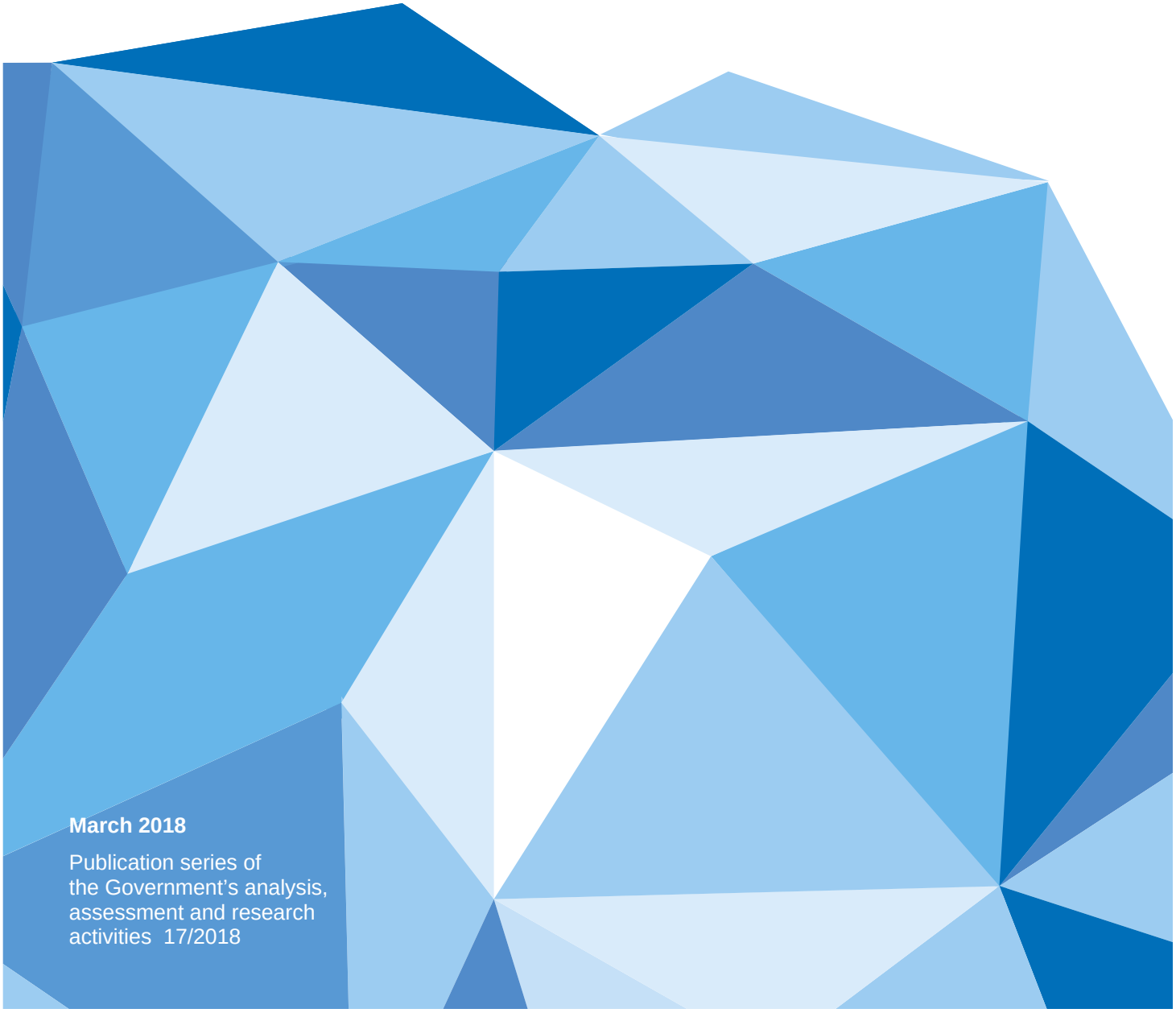
Effects of Real Estate Transfer Taxes: Evidence from a Natural Experiment



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Abstract

We study the effect of the transfer tax on housing transactions and household mobility in Finland using housing transaction data and micro data on the entire population in 2005–2015. In March 2013, the transfer tax rate was increased for housing co-operatives and the tax base was broadened to include housing co-operative loans, but the tax treatment of directly owned single-family houses remained unchanged. The reform allows a differences-in-differences design to be used.

We find that the tax reform was anticipated in the housing market. Some transactions that would have taken place after the reform were brought forward to late 2012 and early 2013 so as to avoid the tax increase. The anticipation effects were especially pronounced for new construction and for resales with relatively large housing co-operative loans. We also find that the tax increase on co-ops had a negative effect on the transaction volume of housing units in co-ops in the longer run, and may have influenced transactions of single-family houses.

Furthermore, the transfer tax reduces mobility, both within and across labor market areas. Our results suggest that the tax creates sizable welfare losses due to increased mismatch of housing units and households and workers and jobs.

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The content is the responsibility of the producers of the information and does not necessarily represent the view of the Government.

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Tiivistelmä

Tutkimuksessa tarkastellaan varainsiirtoveron vaikutuksia asuntokauppoihin sekä kotitalouksien muuttopäätöksiin. Vuoden 2013 maaliskuussa osakeasuntojen varainsiirtoveroprosentti korotettiin 1,6 prosentista 2,0 prosenttiin. Lisäksi osakeasuntojen veropohjaa laajennettiin lisäämällä siihen asunnon yhtiölainaosuus. Uudistus ei vaikuttanut kiinteistöjen, kuten omakotitalojen varainsiirtoveroon. Uudistus voidaan ajatella luonnollisena koeasetelmana, jossa koeryhmän muodostavat osakeasunnot ja kontrolliryhmän puolestaan omakotitalot. Tämä jaottelu mahdollistaa veron vaikutuksen eristämisen muista asuntokauppoihin ja muuttamiseen vaikuttavista tekijöistä.

Tulosten mukaan verouudistus ennakoitiin asuntomarkkinoilla. Osakeasuntojen kauppoja siirrettiin tapahtuvaksi ennen veronkorotusta. Ennakointia havaittiin etenkin uustuotannon puolella sekä sellaisten vanhojen osakkeiden osalta, joihin liittyi suuri yhtiölainaosuus. Uudistuksella oli myös pysyvä osakeasuntojen kauppamääriä laskeva vaikutus.

Tutkimuksessa havaittiin myös, että varainsiirto vähentää muuttamista. Varainsiirtoveron korotus vähensi osakehuoneistossa asuvien omistusasujien muuttoja noin 4,3 prosenttia verrattuna omakotiasujiin. Vero vähentää sekä työmarkkina-alueiden sisäisiä että niiden välisiä muuttoja.

Tämä julkaisu on toteutettu osana valtioneuvoston vuoden 2017 selvitys- ja tutkimussuunnitelman toimeenpanoa (www.tietokayttoon.fi).

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Sammandrag

Studien fokuserar på överlåtelseskattens inverkan på bostadsköp och hushållens flyttbeslut. I mars 2013 höjdes överlåtelseskatten på aktielägenheter från 1,6 till 2,0 procent. Dessutom utvidgades aktielägenheternas skattebas genom att till denna lägga till bostadens bolagslåneandel. Reformen påverkade inte överlåtelseskatten på fastigheter, såsom egnahemshus. Reformen kan betraktas som ett naturligt experiment där testgruppen utgörs av aktielägenheter och kontrollgruppen av egnahemshus. Denna indelning gör det möjligt att isolera beskattningens inverkan från andra faktorer som påverkar bostadsköp och flyttande.

Enligt resultaten beaktades skattereformen på bostadsmarknaden. Aktielägenhetsköp tidigarelades före skattehöjningen. Detta var fallet i synnerhet när det gällde nybyggen och sådana äldre aktier som hade stora bolagslåneandelar. Reformen minskade också permanent antalet köp av aktielägenheter.

Enligt studien minskar överlåtelseskatten även flyttandet. Höjningen av överlåtelseskatten minskade flyttandet bland ägare till aktielägenheter med cirka 4,3 procent jämfört med ägare till egnahemshus. Skatten minskar flyttandet både inom arbetsmarknadsområden och mellan dem.

Den här publikation är en del i genomförandet av statsrådets utrednings- och forskningsplan för 2017 (tietokayttoon.fi/sv).

SISÄLLYS

1. INTRODUCTION

Housing transfer taxes may 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, these effects are also closely linked to household mobility. Effects on mobility have in turn implications both on the allocation of housing to households as well as on the allocation of jobs to employees. Ultimately, all these will influence household welfare, and housing transfer taxes are typically considered as a very inefficient form of taxation (e.g. Mirrlees et al. 2011).

This study provides empirical evidence on the effects of the transfer tax on housing transactions and household mobility in Finland using a tax reform implemented in March 2013 as a plausibly exogenous source of variation. The reform raised the effective transfer tax rate by roughly 0.5 percentage points for housing co-operatives (henceforth co-ops), but did not affect directly owned single-family 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 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; and Dachis et al., 2012 for similar approaches).

In the analysis, we use transaction data with detailed information about the characteristics of the housing units being traded as well as registry data covering the total population of Finland. Therefore we are able to analyze both housing transactions and household mobility. This enables us to obtain a more complete picture of the effects of housing transfer taxes than in the previous literature. Furthermore, 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 tax reform was anticipated in the housing market. Some transactions that would have taken place after the reform were brought forward to late 2012 and early 2013 so as to avoid the tax increase. The anticipation effects were especially pronounced for new construction and for resales with relatively large co-op loans. We also find that the tax increase on co-ops also had a negative effect on the transaction volume of housing units in co-ops in the longer run, and may also have influenced transactions of single-family houses. In general, our results on transaction volume are consistent with most previous results using similar research designs (Dachis et al. 2012; Besley et al. 2014; Kopczuk and Munroe, 2015; and Best and Kleven, 2018).

Our findings also suggest that the transfer tax has a significant impact on mobility, both within and across labor markets. The latter result is novel in the literature and contrasts with previous findings, suggesting that transfer taxes only hinder short distance and housing related moves (Hilber and Lyytikäinen, 2017). Our results imply that the tax creates sizable welfare losses due to increased mismatch of housing units and households and of workers and jobs.

The report is organized as follows. In the next section, we present and discuss the previous literature on housing transfer taxes. After that we describe the Finnish transfer tax and the

reform that we exploit in the analysis. In section 4, we present evidence on the events in the housing market around the implementation of the reform. We mainly focus on transaction volume, but also discuss housing prices, co-op loans and sale times. In section 5, we present the data, the research design and the results related to household mobility. Section 6 presents some back-of-the-envelope welfare calculations and section 7 offers some discussion and conclusion.

2. LITERATURE REVIEW

The key challenge for empirical studies aiming to identify the effects of transfer taxes is created by the absence of exogenous variation in tax rates. In most countries, tax rates rarely change over time. In addition, when tax rates are changed, the changes often happen at a national level so that all transactions and households are affected by the reform in the same way. In these cases, the lack of a plausible counterfactual makes evaluation of the reform very difficult. However, in some cases, transfer tax reforms have been implemented in such a way that they create exogenous variation in the tax treatment of different households. These types of reforms have been exploited in some previous studies.

Another strategy used by researchers is based on different types of discontinuities in tax schedules. These discontinuities may 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.

In this section, we first review empirical literature on transfer taxes exploiting tax reforms and discontinuities in tax schedules as a source of identifying variation. We also discuss different approaches to studying the welfare effects of transfer taxes. Finally, we briefly discuss empirical results related to the effects of capital gains taxation, which has similar features to transfer taxes.

Best and Kleven (2018) study the UK Stamp Duty Land Tax (SDLT) using administrative tax data covering the universe of SDLT returns between November 2004 and October 2012. The data contain rich tax return information for each transaction. The authors exploit discrete jumps in tax liability at certain cutoff transaction prices. For instance, the tax rate jumps from 0 to 1% at a price of £125,000 and from 1% to 3% at a price of £250,000. The results indicate that transaction taxes are highly distortionary, causing large responses in the price, volume and timing of transactions.

Best and Kleven also study the effects of a temporary tax cut in 2008–2009 which abolished the SDLT for transactions in the £125,000–£175,000 price range without changing the tax in the other price categories. This feature of the reform allows its effects to be assessed using a difference-in-differences strategy.

The reform was largely unanticipated and was designed to be temporary. Therefore it potentially affected transaction volume in two ways. First, some households who would have traded a house sometime after the stamp duty holiday might have brought the transaction forward in order to benefit from the tax reduction. Second, some households who would not have traded in the absence of the tax holiday might have decided to trade because of the tax reduction.

Best and Kleven (2018) estimate the tax holiday to have increased the monthly transaction volume by 17%. Some 42% of this additional activity is attributed to a timing response by households bringing their transactions forward while the remaining 58% was estimated to be additional transactions compared to the *status quo*. The authors interpret these results using a theoretical model where household leverage amplifies the effects of transaction taxes on the transaction volume. The idea is that even small changes in the transaction tax may have a large impact on a household's decision to participate in the housing market if the household is close to being constrained by a formal down-payment requirement (i.e. has relatively little own savings).

Besley et al. (2014) exploit the same 2008–2009 stamp duty holiday to estimate the price and transaction volume effects, but using data from the UK financial regulator. The data include information on an independent surveyor's valuation of the property. This allows the authors to use independent house valuations (instead of the actual transaction prices) when dividing the transactions into treatment and control groups. According to the results, the stamp duty holiday increased transactions by about 8%. The average reduction in the after-tax transaction price was about £900, while the average tax reduction was about £1500. The price effect is not easy to interpret because additional transactions induced by the tax holiday may differ from other transactions in a systematic way. Using a simple bargaining model, the authors are able to divide the estimated price effect into a change in surplus sharing caused by the holiday and into a selection effect due to additional transactions taking place during the holiday period. Together with the empirical estimates of the transaction volume and prices, the authors conclude that roughly 60% of the increased surplus from trade went to the buyer. They also estimate the shadow cost of public funds to be between 1.02 and 1.15.

When interpreting the results related to the tax holiday, the following note is in order: If the stamp duty holiday increased the transaction volume over the price range directly affected by the tax reduction, it is also likely to have influenced transactions outside, but in close proximity to the price range. This is because units just below and above the price thresholds that determine the tax rate are likely to be close substitutes. Therefore, one would expect the control group to be indirectly affected by the reform. It is difficult to evaluate whether this is a serious concern, but if so, the effects on the transaction volume might be at least to some degree overestimated. Furthermore, the temporary nature of the tax change makes it very difficult to evaluate the long-run effects on transactions and mobility.

Hilber and Lyytikäinen (2017) study the effects of the UK SDLT exploiting a discontinuity in the tax schedule where the tax rate jumps from 1% to 3%. Unlike the two studies discussed above, they explicitly focus 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.

In the US, housing transfer taxes vary by state and in some states the tax rates change discontinuously, creating discontinuities in total tax liability. Kopczuk and Munroe (2015) utilize the discontinuity in tax liability induced by the so-called mansion tax applied in the states of New York and New Jersey. The tax rate is 1% on residential transactions of $1 million or more, while transactions at prices less than $1 million are not subject to the tax. Therefore, in both New York and New Jersey, the buyer's tax liability jumps from zero to $10,000 when the transaction price increases by one dollar from $999,999 to $1,000,000. The authors find that the tax distorts the price distribution and leads to significant bunching just below the threshold. The results also suggest that the impact of the tax is not limited to the proximity of the threshold, but extends much further, which indicates that the search and housing market matching process is affected by the tax notch.

Slemrod et al. (2017) in turn study changes in transfer tax policy in Washington DC, which introduced a discontinuous jump in tax liability. In January 2003, the transfer tax rate was raised from 2.2% to 3.0%, but only for houses with a transaction price greater than or equal to $250,000. This created an incentive for the transaction price not to exceed $249,999. This discontinuity was later eliminated in October 2004, but a new tax notch was introduced in October 2006, increasing the tax rate from 2.2% to 2.9% on houses with a transaction price above $399,999. The authors conclude that, especially after the 2006 tax increase, transaction prices around the tax notch were negotiated downwards so as to lower the tax burden. In addition, they observe a small timing response around the implementation of the tax increase so as to avoid the anticipated implementation of the higher tax rate.

In order to study the lock-in effects of the tax, the authors use transaction data from 1999 to 2010 to construct a panel data set in which each house appears every month whether it sold or not and impute the price for months in which the house does not sell. For houses that were sold at least once during the time period under study, the question of interest is then how much lower is the likelihood that they are sold after the tax increase. The authors do not find significant effects on the likelihood of selling around the tax notch after the reform compared to houses in the control group. As a result, they conclude that the welfare costs related to housing transaction taxes are likely to be small. One potential reason for this different result relative to those mentioned above is that the tax change was relatively small. The authors also conjecture that there may be large differences in the relative bargaining power of the seller and buyer across different housing markets and within the same housing market area over the real estate cycle.

Dachis et al. (2012) exploit the introduction of the Land Transfer Tax in the city of Toronto in early 2008. The authors estimate the effect of the tax by comparing the changes in the Greater Toronto housing market and the city of Toronto before and after the introduction of the tax. According to the results, the 1.1% tax caused a 15% decline in the number of sales and a decline in housing prices about equal to the tax. Assuming that the value of moving is uniformly distributed and using the value of an average house in Toronto, the authors evaluate the average value of a foregone transaction to be $6,559. Using the estimate on the reduction in transaction volume, the welfare loss is about $1 for every $8 in tax revenue.[1]

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 framework represents a single labor market and therefore

[1] Hilber and Lyytikäinen (2017) discuss why the deadweight loss is likely underestimated.

the focus is on the misallocation of houses caused by transaction costs. The model economy is calibrated to represent the Helsinki metropolitan region. The authors assess the welfare effects of a tax reform, where the transaction tax is replaced 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. Interestingly, despite clear aggregate welfare gains many households are worse off. The reason is that, especially when the transaction tax is high, many households will anticipate that they will not move and hence do not face the tax. However, in a setting where the tax revenue is collected using a property tax, these households would necessarily face their share of the tax burden. Therefore, there may be political support for transaction taxes even at very high and distortionary rates if some households are more likely to trade than others and households have sufficient knowledge about their future need to move.

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 main difference is that for transfer taxes the tax base is the entire value of the house, while for capital gains taxes the tax base is the appreciation in house value only. The empirical evidence on the effects of housing capital gains taxation is very limited. For the US, there are two studies exploiting the changes in the tax treatment of housing capital gains introduced by the Tax Relief Act of 1997. Shan (2011) and Cunningham and Engelhardt (2008) both conclude that the tax reduction raised the mobility rate among affected households. The estimated effects are relatively large, but at least Shan (2011) also finds that the short-term effect was much larger than the long-term effect.

Finally, the level of transaction taxation may also influence house price volatility. It is possible that transaction taxes reduce house price volatility by reducing speculative trading. On the other hand, the presence of large transaction costs can also work in the opposite direction. High transaction costs may limit the use of arbitrage possibilities and thereby increase price volatility. However, there is apparently no empirical evidence on the relationship between the level of transfer taxation and house price volatility.

All in all, most previous empirical studies find housing transfer taxes to have significant effects on housing transactions and household mobility. Welfare analyses point toward welfare losses, which increase rapidly with the tax rate.

3. INSTITUTIONAL SETTING AND REFORM

The Finnish transfer tax applies to three different categories of ownership transfers: 1) 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); 2) shares in 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 3) other shares (corporate stocks, such as shares in a telephone company). Shares and other securities sold on the Stock Exchange are exempt from the tax.

Co-ops are legal entities (typically limited-liability companies) that own residential buildings and in some cases also the lots of the buildings. In Finland, all multi-storey residential buildings and row houses are co-ops. In addition, the ownership of a single-family detached

house can also be organized as a co-op. In this case, the co-op usually includes several single-family 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 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 being bought.

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 single-family houses was 4%, while the tax rate for shares in a co-op was 1.6%. The tax base for both housing types 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 2400 euros (1.6%*150,000) before the reform. After the reform, the tax liability increased to 3300 euros (2%*165,000). The transfer tax treatment of directly owned single-family 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.

4. DESCRIPTIVE EVIDENCE OF HOUSING TRANSACTIONS

4.1 Transaction data

Our first data set comes from tax registers and was obtained from Statistics Finland. The data are at the transaction level and contain information on units in co-ops and single-family houses. The data include the transaction price, (the price of shares in a co-op or the price of a single-family house), the amount of co-op loans, the amount of transfer taxes paid, the location and size of the housing unit, and whether the buyer is a first-time homebuyer.

In principle, the data cover all transactions but with several shortcomings. First, a unit often has several owners. The data therefore contain transactions where one owner sells his or her share of the unit to the other owner(s). These “partial” transactions cannot be identified in the data. As a result, the prices obtained from these data may not be reliable. Second, for single-family houses the data only contain those transactions where the houses are located on an owned lot, i.e. the same person or persons own the house and the lot. All single-family house transactions where the house is located on a lot rented, for example, from the municipality are missing from the data. Municipalities have adopted fairly diverse policies on how much land they own. Therefore the data cover transactions in different regions to a varying degree. Third, except for the floor area of the unit, the data do not contain information about the characteristics of the unit or any identifier that would allow the data to be merged with other data sets containing information about the building or the housing units. This means, among other things, that the transactions cannot be divided into new construction and resales.

Because of these shortcomings, in this section we mostly rely on transaction data collected by the Central Federation of Finnish Real Estate Agencies (KVKL). As not all real estate agencies are members of the federation and some transactions do not involve an agent, the data set represents a sample of the total volume of transactions. The upshot of these data is that they contain extensive information about the unit (housing type, floor area, number of rooms, the storey, sauna, balcony etc.) and the building (including the location of the building). However, the data contain no information about the buyer or the seller.

4.2 Results

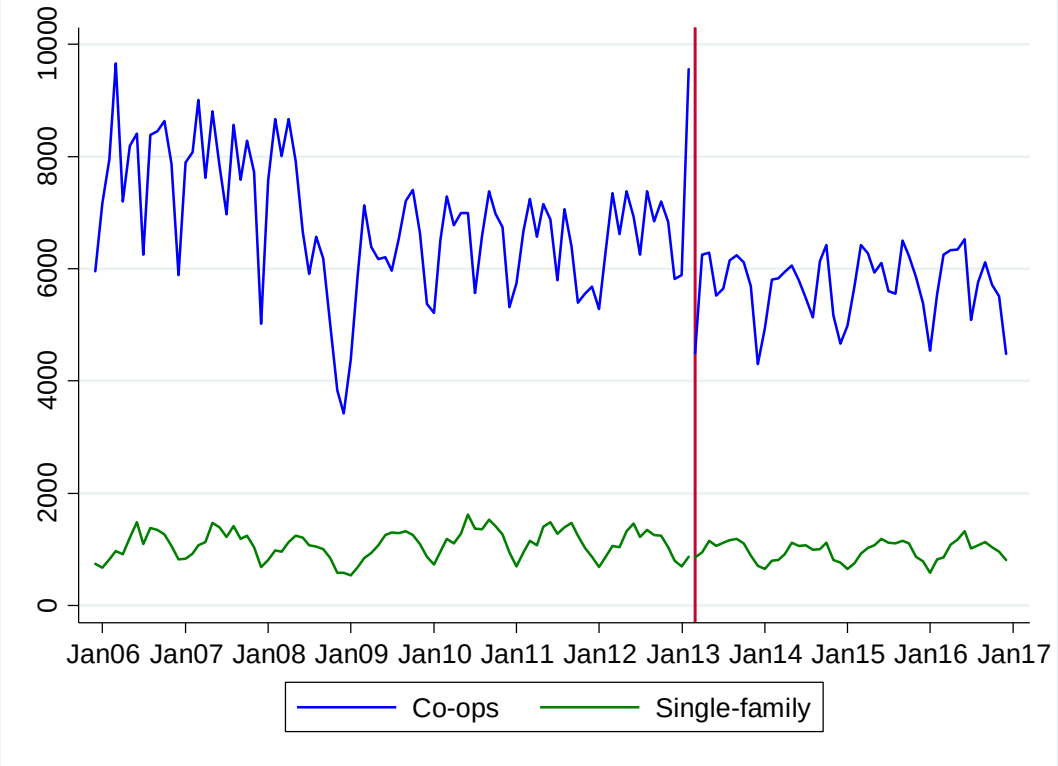
Figure 1 shows the monthly transaction volume of housing units in co-ops and single-family houses from January 2006 to December 2016 using tax register data from Statistics Finland. The figure shows substantial seasonal variation in transaction volumes. The uncertainty created by the financial crisis toward the end of 2008 is also clearly visible in the figure. There seems to be a downward trend in transaction volumes before the financial crisis, but no visible trend after the crisis.

With respect to co-ops, three observations can be made regarding the time around the tax reform. First, there is a clear spike before the reform, indicating substantial anticipation of the reform. Transaction volumes were roughly 50% higher in February 2013 than in Febru-

ary 2012. The monthly transaction volume in co-ops in October and November was roughly 30 and 20% higher, respectively, than in the corresponding months in 2011. Second, the monthly transaction volume in March 2013 was substantially lower than the typical transaction volume in March (before or after the reform). Third, the monthly transaction volume in general seems to be lower after the tax reform. The average monthly transaction volume is 16% lower in the three years following the reform than in the three years before the reform.

For directly owned single-family houses, there is much less action in the transaction volume around the reform (but also during the financial crisis). In particular, the figure does not reveal clear anticipation effects. This is as expected because bringing a transaction forward to take place before the reform offers no financial gains for directly owned single-family houses. However, the average monthly transaction volume is about 5% lower in the three years following the reform than in the three years before the reform. Therefore, it seems plausible that the reform lowered the transaction volume of housing units in co-ops, but may also have reduced the transaction volume of single-family houses.

Figure 1. Total number of transactions.



Notes: Co-ops include transactions of units in multi-storey buildings and row houses. The vertical red line shows the timing of the reform. Data: Statistics Finland.

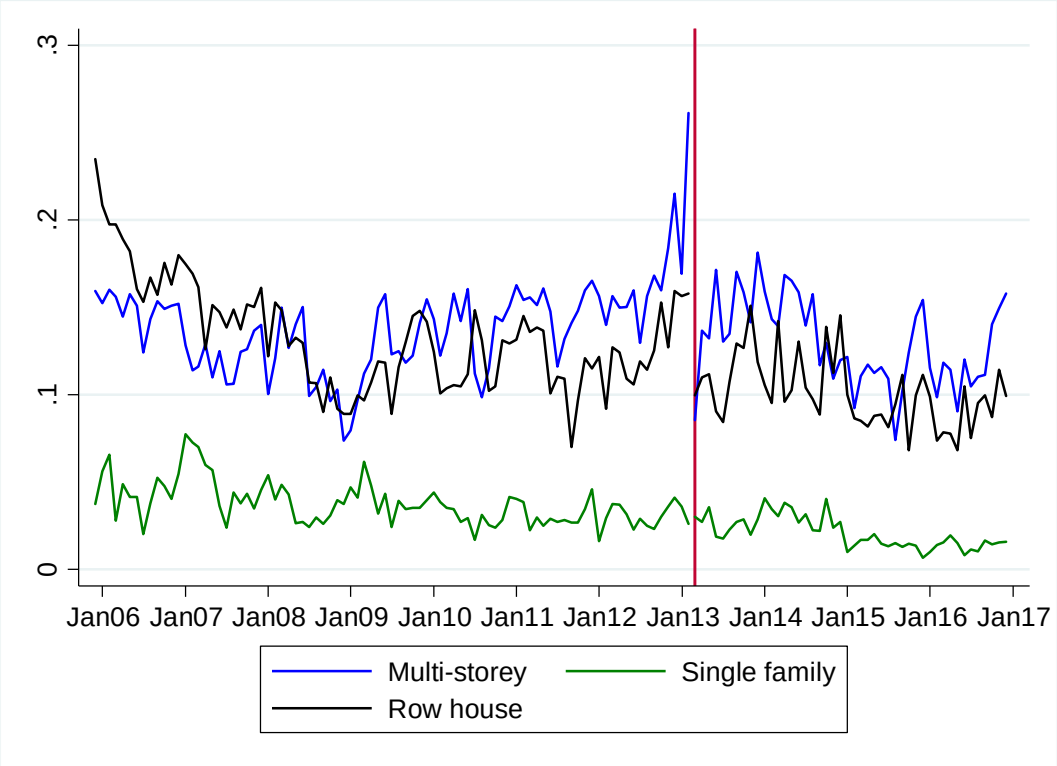
Figures 2–6 make use of transaction data from the Central Federation of Finnish Real Estate Agencies. All the figures display monthly averages of the variable of interest from January 2006 to December 2016.

These data allow us to divide transactions into housing units in multi-storey buildings, row houses and single-family houses. In addition, the data allow us to differentiate between new

construction and resales. This may be important as the time on the market and other market dynamics may be different for new construction and resales.

Figure 2 first shows the share of new construction of the total transaction volume for different housing types. In all cases, most transactions are always resales. This is particularly evident for single-family houses, for which the share of new construction is around 5% of all transactions. However, it also seems that the share of new construction increases before the reform for both co-op types subject to the reform.

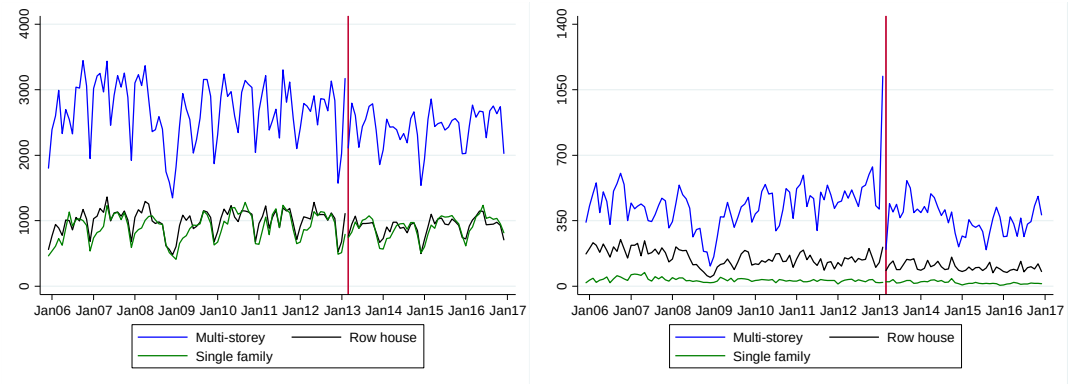
Figure 2. Share of new units out of all transactions by housing type.



Notes: Multi-storey buildings and row houses are co-ops. The vertical red line shows the timing of the reform. Data: KVKL.

Figure 3 shows the transaction volume for resales (left) and for new construction (right). Although the transaction volume for units in co-ops seems lower after the reform for both resales and new construction, the anticipation effect seems especially pronounced for new construction. This is consistent with Figure 2. All in all, the conclusion from Figure 3 is quite similar to that from Figure 1. This means that the KVKL data are representative enough to reflect the overall developments in the housing market around the time of the reform.

Figure 3. Number of transactions by housing type.

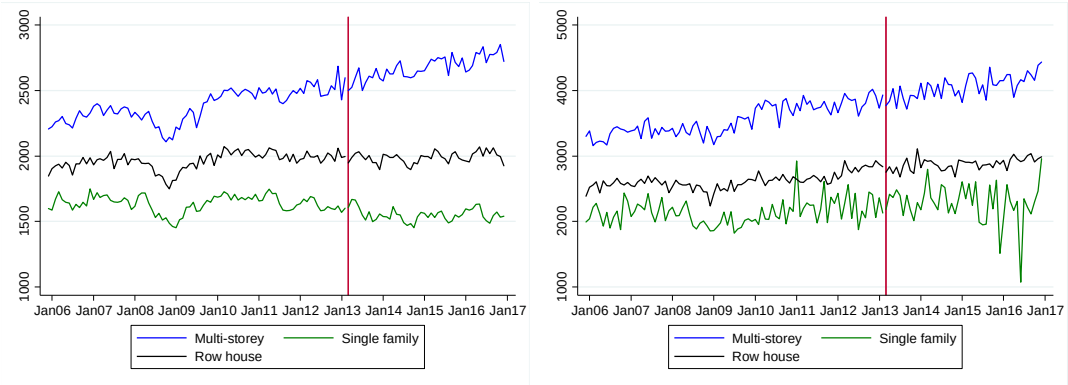


Notes: Left panel: resales. Right panel: new construction. Multi-storey buildings and row houses are co-ops. The vertical red line shows the timing of the reform. Data: KVKL.

Figure 4 shows the price development by housing type, again separately for resales (left) and new construction (right). The left panel shows divergence in the price developments starting roughly at the end of 2009 or early 2010. The average price per square meter of housing units in multi-storey co-ops was increasing and the price of single-family houses was decreasing, while the prices of units in row houses remained quite stable.

Because the price developments diverged before the reform, evaluating the price effects of the reform using a differences-in-differences strategy (with co-ops as a treatment group and single-family houses as a control) may not be warranted. Furthermore, even under full capitalization, the prize effect would be very difficult to detect from normal price variation. Therefore we do not analyze the price effects of the reform any further.

Figure 4. Average prices (€/m^2) by housing type.



Notes: Left panel: resales. Right panel: new construction. Multi-storey buildings and row houses are co-ops. The vertical red line shows the timing of the reform. Data: KVKL.

Next, we focus on the use of co-op loans in multi-storey buildings and row houses. Directly owned single-family houses are not in the figure as in these cases all housing related loans must be the owners' personal loans.

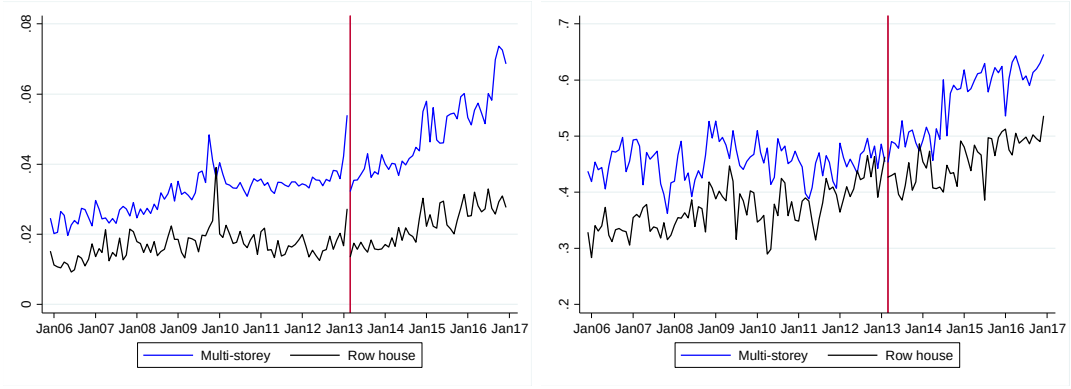
There are various reasons for holding a co-op loan. For example, a co-op loan may be lucrative if access to mortgages is restricted or if mortgage interest rates are high relative to co-op loan interest rates.[2] Before the reform, households also had incentives to hold co-op loans because they were not included in the transfer tax base. By broadening the tax base to include co-op loans, the tax reform eliminated this incentive.

Therefore, one would expect co-op loans to constitute a smaller share of the transaction price after the reform. In addition, as housing units with a large housing co-op loan attached to them were subject to higher tax increases, one would expect those units to be overrepresented in the transaction data just before the reform. This is because there was an incentive to bring the sale forward in order to avoid the tax increase.

Figure 5 shows the co-op loan-to-value ratios for resales (left) and for new construction (right), where value refers to the sum of the transaction price and the co-op loan. The co-op loan-to-value ratios are much lower for resales than for new construction. Also, a clear upward trend is visible in resales before the reform. For resales in particular, it seems that housing units with relatively large co-op loans were more likely to have been sold just before the reform, indicating anticipation.

For new construction, the co-op loan-to-value ratio seems rather stable before the reform in multi-storey buildings, with 40–50% average shares between 2006 and early 2013. In row houses the average loan shares were clearly increasing before the reform. Based on Figure 5, it is not possible to isolate the effect of the reform. However, it seems that reasons unrelated to the transfer tax are important for the use of co-op loans. One possible reason is the gradual phasing-out of mortgage interest deductibility starting in 2012. In 2016, 55% of mortgage interest was tax-deductible.

Figure 5. Housing co-operative loan-to-value ratio by housing type.



Notes: Left panel: resales. Right panel: new construction. The vertical red line shows the timing of the reform. Data: KVKL.

Finally, Figure 6 shows the average time on the market calculated for each transaction as the difference between the listing date and the sale date. On average, sale times seem to be substantially longer for new construction than for resales. In addition, there seems to be more variation between months for new construction. In both panels, selling times are longer around the financial crisis. However, any potential effect of the reform on selling times is not clearly visible in the figures.

[2] In Finland house loans are not assumable mortgages but personal loans. However, most Finnish house loans are secured by a home.

Figure 6. Time on the market (days) by housing type.

Notes: Left panel: resales Right panel: new construction. The vertical red line shows the timing of the reform. Data: KVKL.

5. EFFECTS ON MOBILITY

5.1 Household data

Our data on mobility come from Statistics Finland and include the entire population from 2005 to 2015. 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 single-family house or a co-op (single-family house, housing unit in a row house or in a multi-storey building).

Our measure of moving is based on the location and the characteristics of the housing unit. Under our definition, a household moved if any of the following changed between the end of year *t* and *t*–1: (i) home municipality, (ii) type of housing unit or (iii) number of rooms. This definition means that we are going to miss some short-distance moves within the municipality, 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 directly owned single-family houses, which are our control group. The next six columns include three alternative treatment groups, which are used throughout the empirical analysis. The first group includes all homeowners living in co-ops. In columns (5) and (6), the treatment group is further narrowed down to those living in row or single-family houses, and finally in columns (7) and (8) to those living in single-family houses.

The households in all co-op types are somewhat different from households living in directly owned single-family houses. For example, they are more mobile and more likely to be single, and they live in more urban areas. However, homeowners living either in row or single-family houses in co-ops are more similar to the control group in many respects. As these groups are more similar to the control group, the treatment effect estimates may be more reliable, but at the same time they may not generalize to the population of co-op homeowners.

Table 1. Summary statistics for households.

	Directly owned single-family house		Co-op, all house types		Co-op, row house or single-family house		Co-op, single-family house	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Move (*t*, *t*–1) (0/1)	0.038	0.191	0.067	0.251	0.073	0.261	0.065	0.246
Male hh head (0/1)	0.857	0.350	0.641	0.480	0.743	0.437	0.844	0.363
Taxable income (€/year)	30963	20856	32524	21418	36843	23473	45986	28385
Age	55.0	15.4	55.2	17.7	52.0	16.4	50.2	14.2
Single (0/1)	0.226	0.419	0.474	0.499	0.320	0.466	0.183	0.387
Number of children	0.823	1.135	0.363	0.750	0.590	0.912	0.891	1.057
Upper secondary education (0/1)	0.194	0.395	0.351	0.477	0.367	0.482	0.477	0.499
Employed (0/1)	0.580	0.494	0.546	0.498	0.645	0.479	0.735	0.442
Unemployed (0/1)	0.056	0.230	0.047	0.211	0.045	0.208	0.039	0.192
Student (0/1)	0.007	0.082	0.011	0.105	0.008	0.091	0.008	0.089
Pensioner (0/1)	0.349	0.477	0.388	0.487	0.295	0.456	0.212	0.409
Other (0/1)	0.009	0.092	0.007	0.085	0.006	0.078	0.007	0.084
Urban municipality	0.462	0.499	0.845	0.362	0.741	0.438	0.845	0.362
Semi-urban municipality	0.244	0.430	0.099	0.298	0.149	0.356	0.094	0.292
Rural municipality	0.294	0.455	0.056	0.230	0.110	0.313	0.061	0.239
N (all years)	8,874,187		7,307,203		2,813,476		683,120	
N (2012)	904,277		747,769		282,499		72,150	

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

In Table 2, we analyze the linkages with respect to moving between the homeowner groups in Table 1. The table shows the probability of moving for households in different types of housing units and the destination of the move. Conditional on moving, those owning a housing unit in a co-op are most likely to buy into another co-op (2.9%), but moves to directly owned single-family houses happen as well. Similarly, renters are most likely to move to another rental unit. In the case of owners living in a directly owned single-family house the differences with respect to move destination are smaller.

Table 2. Mobility by origin and destination housing type.

	No move	Move to		
		Directly owned single-family house	Co-op	Renting
Directly owned single-family house	0.962	0.013	0.010	0.016
Co-op, all house types	0.932	0.017	0.029	0.021
Co-op, row house or single family house	0.927	0.025	0.027	0.021
Co-op, single family house	0.935	0.019	0.026	0.020
Renting	0.808	0.028	0.034	0.130

5.2 Research design

The transfer tax increases moving costs. But whose moving costs are affected, depends on the extent to which the transfer tax capitalizes into prices. Under full capitalization, only sellers' moving costs increase, whereas with no capitalization only buyers' moving costs increase. If capitalization is somewhere in between these opposites, both buyers and sellers are affected by the tax increase.

We work with the full capitalization assumption, which is supported by the results in Dachis et al. (2012) and Kopczuk and Munroe (2015). Under this assumption, homeowner households living in co-ops are affected by the tax increase, whereas homeowners in directly owned single-family houses are unaffected. With less than full capitalization, moving costs change for prospective buyers as well. As some of these prospective buyers are in the control group, this would make it more difficult to find mobility effects.

Ideally, we would want to compare the mobility of households for whom the costs of moving increased due to 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.

We construct this counterfactual using homeowners living in single-family 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.

Our DID model takes the form

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

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 *co-op* indicates the treatment group, which consists of homeowners who at the end of year *t*–1 lived in a co-op. The control group consists of homeowners who lived in a directly owned single-family 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 and municipality fixed effects.

The parameter for the interaction term, δ_3, has a causal interpretation if two assumptions 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 single-family 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 at least to some extent because the two housing market segments are connected through transaction chains (see also Dachis et al. 2012). More specifically, if the tax increase also reduces mobility in the single-family houses not directly affected by the tax increase, our estimates will be biased towards zero.

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 of 2013. These households moved before the tax increase, but in our baseline specification these moves are misclassified as having taken place in the post-reform period. This may lead to a downward bias in our estimates. This is true if the tax increase reduces mobility after March 2013.

The second issue concerns the possibility that households planning to move bring their transaction forward in order to benefit from the lower pre-reform tax. This might also mean that they move before the end of 2012. In our baseline specification, this anticipation response would lead to a bias away from zero in our estimates when it comes to the longer-term effects of the tax. However, the average sale time for co-ops is over 60 days (see Figure 6). This means that households putting their house on the market in early October 2012 expect to sell the house sometime early December. Because there is no reason to bring the actual move forward once the transaction is completed before the tax increase, this may not be a serious concern in our setting. Nonetheless, in order to check the robustness of our results to these timing issues, we estimate specifications where we omit the year 2013 or both years 2012 and 2013.

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

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

where $g \in \{co\text{-}op,\ single\text{-}family\}$. In the second step, we use the annual group-level data on $c_{g,t}$ to estimate the DID model:

$$(3) \quad c_{g,t} = \alpha_t + \delta_1 * co\text{-}op_{g,t} + \delta_2 * after_{g,t} + \delta_3 * co\text{-}op_{g,t} * after_{g,t} + u_{g,t}.$$

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

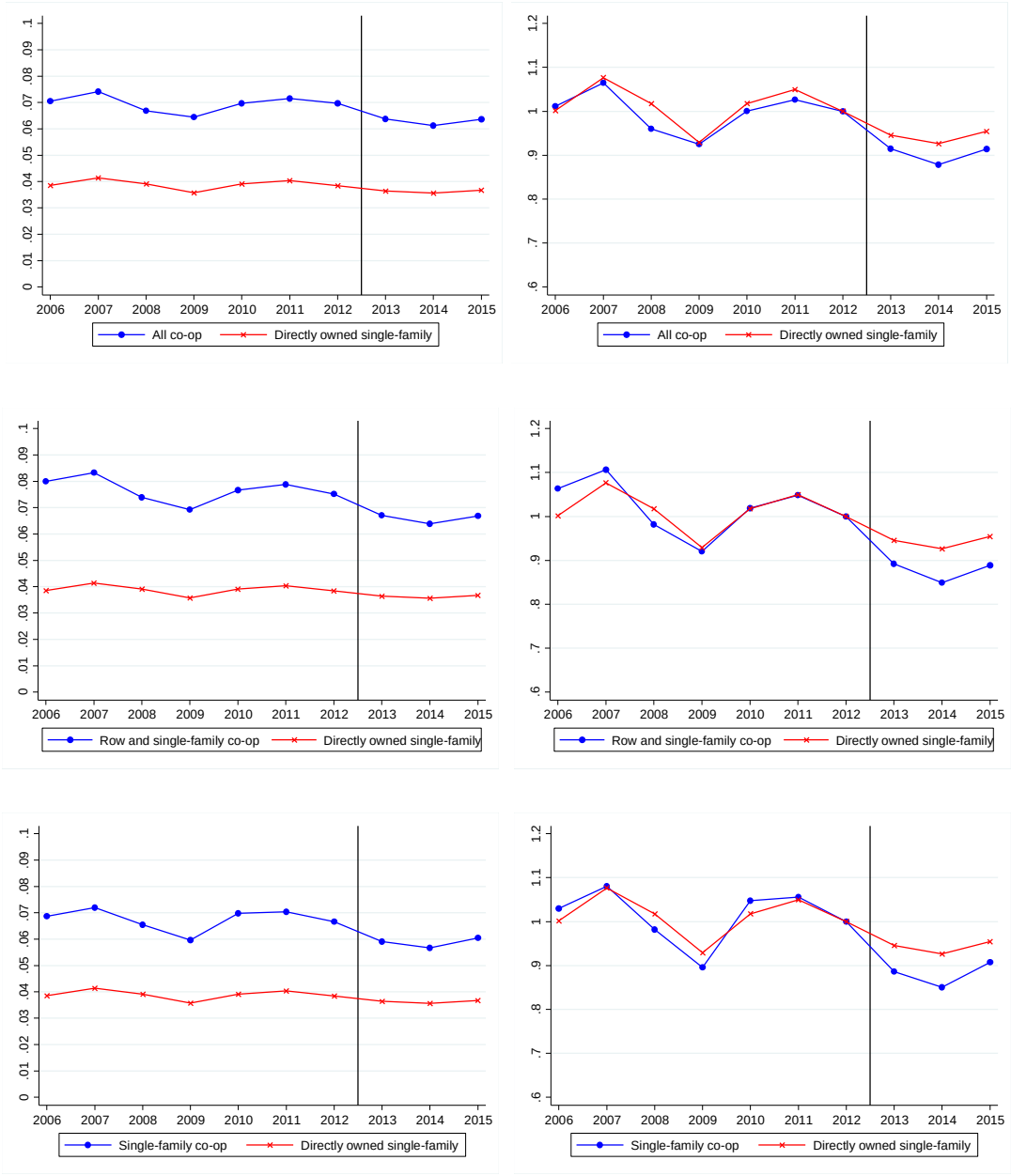
5.3 Results

Overall effects

We start by presenting graphical evidence on the mobility rate of homeowners in the treatment and control groups. This allows us not only to visually assess the plausibility of the common trends assumption, but also the size of the possible treatment effect. We consider three different treatment groups. The first group includes all homeowners living in co-ops (multi-storey, row house and single-family house). The second consists of homeowners living in row house or single-family house co-ops and the third group consists of homeowners living in single-family house co-ops only.

The left panel in Figure 7 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 7. First, the mobility rate is clearly higher in all of the treatment groups throughout the time period (left panel). Second, the trends are similar in the treatment and control groups in the pre-treatment period. This is especially clear after normalization, when we compare proportionate changes in the mobility rate relative to 2012 (right panel). We present the results of the formal pre-treatment placebo tests in Tables A1–A3 in the Appendix. Finally, after the tax increase, the mobility rate decreases in both groups, but clearly more so in the treatment group. This suggests that the transfer tax increase lowered the mobility of households in the treatment group.

Figure 7. Mobility rate for homeowners in co-ops (treatment) and in directly owned single-family houses (control).



Notes: The left panel in the figure presents the group-specific mobility rates and in the right panel the mobility rates are normalized to one in 2012. 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*. Group assignment is based on the homeowners' housing type in year *t*–1. There are three treatment groups (blue lines). The first group includes all homeowners in co-ops, the second homeowners in row and single-family house co-ops and the third single-family house co-ops. The control group (red line) includes homeowners in directly owned single-family houses.

Table 3 presents the DID regression results corresponding to Figure 7 using the two-step procedure of Donald and Lang (2007). In the first column, the first-stage regression does not include the household-level control variables. In the second column, we add the control variables to the first stage. In the third column, the specification is same as in column (2),

except that the dependent variable is the log of the mobility rate. All the model specifications include year dummies in the second step.

The regression results are in line with Figure 7 and robust across specifications. Interestingly, the effect is largest for the treatment group that includes both row and single-family houses (0.72 percentage points) and smallest for the treatment group that includes all co-op types (0.32 percentage points). Compared to the pre-treatment mobility rate, this implies that the mobility rate decreased by between 4.3% and 10% in different housing types. For all co-ops, this translates to roughly 2400 fewer moves per year (-0,0032*747,769).

Furthermore, given that the reform increased the transfer tax rate in the treatment group on average by only 0.5 percentage points (this includes the rate increase and the broadening of the tax base), the order of magnitude of the effect is comparable to prior studies.

Table 3. DID results for mobility.

	(1)	(2)	(3)
Dep. Var.	Mobility rate	Mobility rate	Ln(Mobility rate)
Panel A: All co-op types			
Co-op	0.0306***	0.0197***	0.370***
	[0.000607]	[0.000682]	[0.00913]
Co-op*After	-0.00395***	-0.00318**	-0.0426**
	[0.00111]	[0.00125]	[0.0167]
Pre-treatment mean	0.0696	0.0696	
Panel B: Row houses and single family houses			
Co-op	0.0378***	0.0240***	0.423***
	[0.00105]	[0.00106]	[0.0133]
Co-op*After	-0.00805***	-0.00724***	-0.101***
	[0.00192]	[0.00194]	[0.0242]
Pre-treatment mean	0.0767	0.0767	
Panel C: Only single family houses			
Co-op	0.0286***	0.0196***	0.374***
	[0.000877]	[0.000922]	[0.0125]
Co-op*After	-0.00606***	-0.00499**	-0.0736**
	[0.00160]	[0.00168]	[0.0228]
Pre-treatment mean	0.0675	0.0675	
Year dummies (2nd step)	X	X	X
Controls (1st step)		X	X
N	20	20	20

Notes: Table shows DID estimates using the Donald and Lang (2007) two-step procedure. Standard errors are in brackets. 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 municipality fixed effects (at *t*–1).

In Table 4, we test the robustness of the results with respect to anticipation effects. As discussed above, moves that were planned to take place in 2013 may have taken place already in late 2012 because of the 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 ex-

cessive moves in 2012 and fewer moves in 2013, respectively, leading to a bias away from zero in our DID estimates.

To check whether this is the case, we first drop 2013 and then both 2012 and 2013 from the analysis. As the table shows, the results are robust to the exclusion of these years, indicating that anticipatory behavior is not a major concern in our analysis. 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.

Table 4. Robustness with respect to anticipation effects.

	(1)	(2)	(3)	(4)
	2013 dropped	2012 and 2013 dropped	2013 dropped	2012 and 2013 dropped
Dep. Var.	Mobility rate	Mobility rate	Ln(Mobility rate)	Ln(Mobility rate)
Panel A: All co-op types				
Co-op*After	-0.00336*	-0.00322	-0.0465*	-0.0445*
	[0.00154]	[0.00167]	[0.0205]	[0.0221]
Panel B: Row houses and single family houses				
Co-op*After	-0.00758**	-0.00769**	-0.107***	-0.109**
	[0.00240]	[0.00263]	[0.0297]	[0.0325]
Panel C: Only single family houses				
Co-op*After	-0.00500**	-0.00496*	-0.0744**	-0.0739*
	[0.00209]	[0.00230]	[0.0283]	[0.0311]
N	18	16	18	16

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

We report additional robustness checks in Tables A4–A6 using the three alternative treatment groups. In the baseline specification, we use data for the entire time period 2005–2015. 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 2007–2015 to 2011–2015 (Panel A). In Panel B, we additionally drop year 2013 from the analysis. In Panel C, we allow for differential group-specific linear time trends. In Panel D, we both drop year 2013 and use group-specific trends.

Overall, the results seem robust to these changes in the specification. The point estimates are always negative and vary only slightly around our baseline estimate. The estimates become insignificant in some specifications with the narrower time windows, but this is mostly due to the fact that in these specifications we have very few remaining degrees of freedom.

Effects on mobility within and between labor markets

We are also interested in whether the transfer tax hinders moves between or within labor markets or both. We do not have a clear definition of what constitutes a labor market so we use two different regional divisions. The first regional division that we use is the county division (NUTS 3). There are 19 counties in Finland and they are quite large geographic areas. It is quite uncommon to live in one county and work in another. Thus, moves between counties are likely to be moves where the household's labor market changes. With this definition labor market related within county moves will be misclassified.

The second regional division that we use is the municipality division. In 2013, there were 320 municipalities. Most people live and work in the same municipality, but commuting across municipal borders is much more common than across county borders. So here we are likely to misclassify some within labor market moves as between labor market moves.

Table 5 presents the results for four types of moves. Columns (1) and (2) report results for between and within county moves, whereas columns (3) and (4) report between and within municipality moves. According to the pre-treatment means reported in the table, moving between labor markets is clearly less common than moving within a labor market, regardless of the labor market definition.

The transfer tax appears to hinder both between and within labor market moves. This finding is in contrast with the findings of Hilber and Lyytikäinen (2017), who report that in the UK transfer taxes only hinder short-distance and housing-related moves.

Table 5. Effects on between and within labor market moves.

	(1)	(2)	(3)	(4)
Type of move	Between counties	Within county	Between municipalities	Within municipality
Panel A: All co-op types				
Co-op*After	-0.000285*	-0.00289**	-0.00209***	-0.00086
	[0.000136]	[0.00118]	[0.000468]	[0.000869]
Pre-treatment mean	0.0100	0.0595	0.0267	0.0426
Panel B: Row houses and single family houses				
Co-op*After	-0.000907**	-0.00633***	-0.00347***	-0.00346**
	[0.000309]	[0.00171]	[0.000903]	[0.00115]
Pre-treatment mean	0.00985	0.0668	0.0283	0.0477
Panel C: Only single family houses				
Co-op*After	-0.000975**	-0.00402**	-0.00294***	-0.00188
	[0.000396]	[0.00142]	[0.000715]	[0.00115]
Pre-treatment mean	0.00829	0.0592	0.0259	0.0413
N	20	20	20	20

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

6. WELFARE EFFECTS

In this section, we evaluate the welfare costs of the transfer tax increase related to the mobility results presented in the previous section. When the transfer tax is raised, some moves that would have taken place in the absence of the increase will not take place. 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

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

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

$$\Delta R(t) = t_1 \times p \times (1-\gamma) m - t_0 \times p \times m,$$

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.[3] The parameter γ is the percentage change in mobility when the tax rate is raised from t_0 to t_1. The value of γ is based on our mobility estimates. Taking all co-op types together, the tax increase decreased the mobility rate by 4.3%. Therefore, in what follows γ = 0.043.

In our transaction data, the average co-op loan-to-value-ratio after 2013 for 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

$$\Delta W(t) \in \{\gamma \times m \times t_0 p,\ \gamma \times m \times t_1 p\}.$$

[3] Note that this expression assumes that the tax increase does not affect the transaction price. Furthermore, we can observe the total transfer tax revenue before and after the tax increase, but we do not know how much of the tax revenue is collected from moves and how much from other transactions (*e.g.* landlords trading) so we base our calculations of the *MCF* on our mobility estimates.

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

$$MCF \in \{1.17, 1.22\}.$$

Considering that the property tax, when levied on land value, is non-distortionary, eliminating the existing transfer tax in favor of a revenue-equivalent increase in the property tax on land value would increase aggregate welfare (see also Dachis et al. 2012). However, as only a small fraction of households move annually, the tax burden of these two tax forms is quite differently distributed (see also Määttänen and Terviö 2017).

Finally, it is interesting to see how our estimate of the *MCF* relates to comparable estimates obtained in prior literature. Dachis et al. (2012) report that the imposition of a 1.1% tax in the city of Toronto caused a welfare loss of $1 for every $8 in tax revenue, which implies a welfare loss of 13% relative to the tax revenue raised. Besley et al. (2014) use the UK stamp duty holiday which temporarily abolished the 1% tax rate and find the welfare loss to be between 2% and 15%. Hilber and Lyytikäinen (2017) exploit a discontinuity in the UK stamp duty schedule where the tax rate jumps from 1% to 3% and find the welfare cost to be 84% relative to the additional tax revenue raised.

On the other hand, Slemrod et al. (2017), when considering a tax rate increase from 2.2% to 2.9% for housing units with a transaction price at least $400,000 in Washington DC, conclude that the welfare loss from this increase was small. Kopczuck and Munroe (2015) and Best and Kleven (2018) also find sizable welfare losses, but they do not report numbers that would allow us to assess the *MCF* in their settings.

Taken together, this set of results from different institutional settings suggests that the welfare loss is relatively modest at low transfer tax rates, but increases quite rapidly with the tax rate. This conclusion is also drawn by Määttänen and Terviö (2017), who evaluate how the *MCF* depends on the transfer tax rate using a model economy calibrated to represent the Helsinki Metropolitan Area.

7. CONCLUSIONS

We study the effect of the Finnish transfer tax on housing market activity and 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 single-family houses remained unchanged. This reform enables the use of the differences-in-differences design.

Our descriptive analysis suggests that the tax reform was anticipated in the housing market and induced timing responses, whereby transactions that would have taken place after the reform were brought forward to late 2012 and early 2013 so as to avoid the tax increase. The anticipation effects were especially pronounced for new construction and for resales with relatively large co-op loans.

It also seems that the tax increase on co-ops had a negative effect on the transaction volume of housing units in co-ops over the long-run, and may have influenced transactions of single-family houses. Finally, although the reform eliminated the transfer tax-related incen-

tive to hold large co-op loans, the possible effects of this change were not large enough to lower average co-op loans after the reform. Instead, co-op loans have continued to increase after the reform both in new construction and resales.

In general, our results on transaction volume are consistent with most previous results using similar research designs (Dachis et al. 2012; Besley et al. 2014; Kopczuk and Munroe, 2015 and Best and Kleven, 2018), but in contrast with Slemrod et al. (2017), who do not find a transfer tax increase to have negative effects on transaction volumes in the long-run.

Our results, based on household data comprising the entire population of Finland for 2005–2015, suggest that the transfer tax has a significant negative impact on household mobility. Interestingly, the tax increase had a negative effect on both within and between labor market moves. The latter result is in contrast with previous findings in the literature which suggest that transfer taxes only hinder short-distance and housing-related moves. Our results imply that housing transfer taxes may lead to a mismatch not only of households and houses, but also to a mismatch between workers and jobs.

Our analysis of household mobility assumes that the mobility rates of households living in and owning 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. These spillovers may bias our results toward zero.

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APPENDIX: VALIDITY AND ROBUSTNESS CHECKS

Table A1. Placebo tests – all co-ops in treatment group.

	(1)	(2)	(3)	(4)
Placebo reform	2006	2007	2008	2009
Co-op*AfterPlacebo	-0.00145	-0.00255*	-0.00016	0.00103
	[0.00201]	[0.00130]	[0.00147]	[0.00142]
Placebo reform	2010	2011	2012	2014
Co-op*AfterPlacebo	0.00119	0.00103	-0.00055	0.00106
	[0.00155]	[0.00205]	[0.00235]	[0.00233]
N	20	20	20	20

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

Table A2. Placebo tests – co-op row houses and single-family houses in treatment group.

	(1)	(2)	(3)	(4)
Placebo reform	2006	2007	2008	2009
Co-op*AfterPlacebo	-0.00379	-0.00514**	-0.00221	0.000284
	[0.00292]	[0.00160]	[0.00214]	[0.00230]
Placebo reform	2010	2011	2012	2014
Co-op*AfterPlacebo	0.000332	-0.000759	-0.00102	0.00124
	[0.00252]	[0.00324]	[0.00367]	[0.00366]
N	20	20	20	20

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

Table A3. Placebo tests – only co-op single-family houses in treatment group.

	(1)	(2)	(3)	(4)
Placebo reform	2006	2007	2008	2009
Co-op*AfterPlacebo	-0.000532	-0.00146	0.000521	0.00304
	[0.00281]	[0.00211]	[0.00198]	[0.00163]
Placebo reform	2010	2011	2012	2014
Co-op*AfterPlacebo	0.00172	0.000321	0.000000	0.00228
	[0.00208]	[0.00281]	[0.00319]	[0.00307]
N	20	20	20	20

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

Table A4. Robustness to time window and inclusion of group-specific time trends – all co-ops in treatment group.

	(1)	(2)	(3)	(4)	(5)
Time window	2007-2015	2008-2015	2009-2015	2010-2015	2011-2015
Panel A: Varying time window					
DID estimate	-0.00297*	-0.00245*	-0.00311**	-0.00377***	-0.00403**
	[0.00132]	[0.00120]	[0.000927]	[0.000606]	[0.000704]
Panel B: Varying time window and 2013 dropped					
DID estimate	-0.00316	-0.00263	-0.00330**	-0.00395**	-0.00422**
	[0.00164]	[0.00149]	[0.00116]	[0.000745]	[0.000889]
Panel C: Varying time window and group-specific trends					
DID estimate	-0.00387	-0.00635***	-0.00566**	-0.00464**	-0.0044
	[0.00246]	[0.00132]	[0.00146]	[0.00135]	[0.00170]
Panel D: Varying time window, and group-specific trends and 2013 dropped					
DID estimate	-0.00439	-0.00789***	-0.00740**	-0.00634*	-0.00697
	[0.00307]	[0.00103]	[0.00129]	[0.00154]	[0.00272]
N	18	16	14	12	10
N (2013 dropped)	16	14	12	10	8
Pre-treatment mean	0.0635	0.0626	0.0632	0.0649	0.0655

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

Table A5. Robustness to time window and inclusion of group-specific time trends – co-op row houses and single-family houses in treatment group.

	(1)	(2)	(3)	(4)	(5)
Time window	2007-2015	2008-2015	2009-2015	2010-2015	2011-2015
Panel A: Varying time window					
DID estimate	-0.00670***	-0.00577***	-0.00630***	-0.00740***	-0.00748***
	[0.00191]	[0.00150]	[0.00152]	[0.000914]	[0.00118]
Panel B: Varying time window and 2013 dropped					
DID estimate	-0.00704**	-0.00611**	-0.00663**	-0.00774***	-0.00782**
	[0.00237]	[0.00185]	[0.00189]	[0.00110]	[0.00146]
Panel C: Varying time window and group-specific trends					
DID estimate	-0.00611	-0.00956***	-0.00936**	-0.00702**	-0.00674
	[0.00361]	[0.00232]	[0.00289]	[0.00219]	[0.00282]
Panel D: Varying time window, and group-specific trends and 2013 dropped					
DID estimate	-0.00661	-0.0114**	-0.0118**	-0.00844	-0.00865
	[0.00455]	[0.00255]	[0.00345]	[0.00348]	[0.00649]
N	18	16	14	12	10
N (2013 dropped)	16	14	12	10	8
Pre-treatment mean	0.0689	0.0676	0.0681	0.0702	0.0706

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

Table A6. Robustness to time window and inclusion of group-specific time trends – only co-op single-family houses in treatment group.

	(1)	(2)	(3)	(4)	(5)
Time window	2007-2015	2008-2015	2009-2015	2010-2015	2011-2015
Panel A: Varying time window					
DID estimate	-0.00492**	-0.00458*	-0.00522*	-0.00673***	-0.00622**
	[0.00184]	[0.00197]	[0.00204]	[0.00116]	[0.00135]
Panel B: Varying time windows and 2013 dropped					
DID estimate	-0.00493*	-0.00459	-0.00523	-0.00674**	-0.00623*
	[0.00229]	[0.00248]	[0.00258]	[0.00150]	[0.00181]
Panel C: Varying time windows and group-specific trends					
DID estimate	-0.00709*	-0.00941**	-0.00927*	-0.00599	-0.00677
	[0.00331]	[0.00313]	[0.00391]	[0.00276]	[0.00327]
Panel D: Varying time windows, and group-specific trends and 2013 dropped					
DID estimate	-0.00755	-0.0108*	-0.0111	-0.00541	-0.00798
	[0.00417]	[0.00394]	[0.00536]	[0.00471]	[0.00787]
N	18	16	14	12	10
N (2013 dropped)	16	14	12	10	8
Pre-treatment mean	0.0625	0.0619	0.0624	0.0649	0.0648

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

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