



# Home-ownership and the Labour Market: Evidence from Rental Housing Market Deregulation

Jani-Petri Laamanen

University of Tampere, FI-33014 University of Tampere, Finland

## ARTICLE INFO

### Article history:

Received 30 September 2015

Received in revised form

13 June 2017

Accepted 10 August 2017

Available online 12 August 2017

### Keywords:

Home-ownership

Unemployment

Instrumental variables (IV) estimation

### JEL classification:

J64

R31

C26

## ABSTRACT

Perhaps the most common finding relating housing to the labour market is that high home-ownership rates are associated with higher unemployment. In contrast, recent micro-evidence suggests that home-owners have relatively favourable labour market outcomes. We explore the effect of home-ownership on unemployment exploiting a rental housing market deregulation reform which created exogenous variation in home-ownership across regions, allowing us to avoid the endogeneity problem in earlier studies. While home-owners are less likely to experience unemployment, an increase in the home-ownership rate causes unemployment to rise. Externalities arising from consumption reductions and increased job competition may explain the conflicting evidence.

© 2017 Elsevier B.V. All rights reserved.

## 1. Introduction

The roles of housing markets and household credit in the economy have received increased attention after the onset of the recent economic crisis. It seems that the housing and mortgage markets play a more important role in macroeconomic fluctuations than previously thought (see e.g. the discussion in Mian and Sufi, 2010). However, not much is known about the relationships between the housing and mortgage markets and macroeconomic outcomes. Even less is known about the mechanisms underlying these relationships. One of the earliest and most often observed relationships is the positive correlation between the rate of home-ownership and unemployment. Since Oswald's (1996) influential paper documenting this relationship, several studies have either replicated Oswald's empirical analyses with other data sets or tested the theoretical hypotheses using microeconomic data. Several studies using regional or cross-country data lend at least some support to the claim that a higher regional home-ownership rate leads to a higher rate of unemployment (Blanchflower and Oswald, 2013; Costain and Reiter, 2008; Coulson and Fisher, 2009; Di Tella and MacCulloch, 2005; Green and Hendershott, 2001; Isebaert et al., 2015; Munch et al., 2006; Nickell, 1998; Wolf and Caruana-Galizia, 2015). Oswald (1996) hypothesises that this is caused by lower geographical mobility of home-owners relative to

renters. Indeed, Battu et al. (2008) find that homeowners in the United Kingdom are less likely to experience a job change associated with a non-local residential move than renters. Munch et al. (2008) find that Danish homeowners have fewer local and non-local job-to-job changes than renters. The above findings have resulted in a call for policies that discourage home-ownership and encourage mobility. Recent research has found that the effects of deductibility of mortgage interest payments on home-ownership rates are probably small (see e.g. Hilber and Turner, 2014 or Bourassa et al., 2013 and references therein). An extensively studied policy which has the potential to affect mobility is transfer taxation. The evidence on the effects of transfer taxes on housing transactions and mobility, and especially the relevance of these effects from the point of view of labour market is somewhat mixed (see e.g. Slemrod et al., 2017 and references therein).

The evidence on negative mobility effects of home-ownership is in line with Oswald's (1996) hypothesis. However, several studies show that despite being less mobile, homeowners have more favourable labour market outcomes than renters. Owning one's home is found to be associated with a lower unemployment probability (Coulson and Fisher, 2009), smaller risk of becoming unemployed (Leuvensteijn and Koning, 2004; Munch et al., 2008), shorter unemployment durations (Flatau et al., 2003; Munch et al., 2006) and higher wages (Munch et al., 2008). All of the aforementioned individual-level results are obtained when correcting for the presumed endogeneity of housing tenure status. Therefore, the findings of the micro-level studies seem to be in conflict with

E-mail address: [jani-petri.laamanen@uta.fi](mailto:jani-petri.laamanen@uta.fi)

Oswald's empirical results and the results of the other papers that use aggregate data. Since the labour market outcomes of home-owners are generally more favourable than those of renters, regions with higher home-ownership rates should experience lower unemployment rates. This is generally not true, which means that there might be some other mechanisms at work than those identified by most studies thus far. In particular, the conflicting earlier micro-level and aggregate evidence points to externalities of home-ownership on the labour market.

In this paper, we use Finnish individual-level data to study the effects of home-ownership on unemployment experience and, more generally, the labour market. We allow home-ownership to have external labour market effects. More specifically, we allow labour market outcomes of individuals to be affected by the overall home-ownership rate in their region. Tests based on recent research are used to test new hypotheses on the mechanisms through which the externalities may work. To identify the causal effect of regional home-ownership on individual labour market outcomes, we exploit a rental housing market deregulation reform in the early 1990s. The reform produced a natural experiment that provides regional and time variation in home-ownership. Our results show that home-ownership has a significant positive external effect on unemployment experience, whereas, at the same time, homeowners are less likely to experience unemployment than non-owners. Our results are, thus, consistent with both the harmful and the beneficial labour market effects of home-ownership found in the earlier literature. In the light of the additional analyses, it is likely that debt-financed home-ownership hurts the local labour market by causing reductions in consumption demand. Although home-ownership has the potential to boost the labour supply of homeowners, the positive effects may be at least partly offset by displacement effects in the short-run.

The remainder of the paper is organised as follows. [Section 2](#) provides some background information on Finnish housing and labour markets in the early 1990s. [Section 3](#) describes the econometric model and the data, and [Section 4](#) presents the results. [Section 5](#) includes a discussion of the results and robustness checks. Finally, [Section 6](#) concludes.

## 2. Finnish housing and labour markets in the early 1990s

Over the recent decades, the home-ownership rate in Finland has been close to the average when compared to other European countries. Home-ownership rose during the 1970s and 1980s, reached a peak of about 72 % in 1988, and stayed high for a couple of years. Rent control laws had been effective for many decades (for a history of the rent control laws in Finland, see [Lyytikäinen, 2006](#)), probably contributing to increasing home-ownership through less supply of rental housing. In the 1980s, home-ownership rate rose for at least two additional reasons. First, household income increases were large due to strong economic growth. Second, financial liberalisation increased the availability of mortgages. Home purchases were typically financed by variable rate mortgages, and the mortgage interest payments were deductible from taxable income. [Scanlon and Whitehead \(2004\)](#) note that Finnish mortgage loan terms were exceptionally short by international standards in the early 1990s. This means that households, especially those with large mortgages, had high monthly repayment obligations.

There were large changes in the Finnish labour market in the early 1990s. Unemployment had fallen to its low since the late 1970s crisis by the end of 1980s but started rising rapidly in the early 1990s. The unemployment rate had risen from about 3.5 % in 1989 and 1990 to about 13 % in 1992. In the early 1990s, Finland had strong trade unions and high degree of unionisation. Wage

risers were determined in centralised negotiations and were universally binding. The 1991 contract still included wage rises but in the 1992 contract, wages were frozen due to increasing unemployment. Based on [Böckerman et al. \(2010\)](#), it seems that the contracts in our period of investigation were at least partly binding. The resulting downward wage rigidity together with the fact that centralised contracts do not take into account regional differences in labour market prospects makes it likely that the effects of home-ownership on unemployment and employment were large and the effects on wages were small.

## 3. Econometric model and data

Most of the earlier studies on the association between home-ownership and unemployment have used either aggregate or individual-level data. We combine an individual-level data set with region-level information on home-ownership to estimate probit models for whether an individual experienced unemployment during the year:

$$U_{ijt}^* = \alpha \text{hor}_{jt} + \beta' X_{ijt} + \delta_{jt} + \epsilon_{ijt}, \quad (1)$$

and

$$U_{ijt} = \begin{cases} 1, & \text{if } U_{ijt}^* > 0. \\ 0, & \text{otherwise.} \end{cases}$$

where  $U_{ijt}$  is the unemployment dummy variable, indicating whether individual  $i$  residing in county  $j$  experienced (any number of months of) unemployment during year  $t$ . Variable  $\text{hor}$  is the regional proportion of home-ownership. Further, we control for various individual characteristics  $X$ , including a dummy variable for living in an owner-occupied dwelling and a dummy for a mortgage loan to capture the impact of individual housing tenure.<sup>1</sup> We also include year dummies as well as county dummies. The error term of the model has two components:  $\delta_{jt}$  is a region-level error common to all individuals residing in county  $j$  in year  $t$ , and  $\epsilon_{ijt}$  is an individual-level error. Conditional on the region-level error, the expectation of the individual-level error is assumed to be zero.

By including regional-level home-ownership in our model, we allow regional home-ownership to have an effect on unemployment spell probability of an individual, given her own housing tenure. Although the origin of this external effect is unclear, there may be several different reasons for it. After first identifying the externality, we discuss the possible interpretations of it and perform analyses that shed light on the mechanisms involved.<sup>2</sup> Since we are interested in the causal effect of regional home-ownership on unemployment experience, we need to take into account possible endogeneity of regional home-ownership. The results in [Oswald \(1996\)](#) come from simple regional-level regressions, and the author argues that his coefficient estimates may understate the positive causal effects. Assuming exogenous regional home-ownership would yield similarly biased estimates in our study as well.

Theoretically, regional home-ownership depends on the supply of and demand for owner-occupied housing. The endogeneity of

<sup>1</sup> Since the data do not include mortgage loan information the mortgage dummy equals one if an individual has claimed mortgage interest deduction and zero otherwise.

<sup>2</sup> Attempts to empirically identify externalities have been made in the literature on the effects of education (see e.g. [Acemoglu and Angrist, 2000](#)). Externalities in the labour market have been discussed and estimated by [Crépon et al., 2013](#), [Lalive et al. \(2015\)](#), [Ferracci et al. \(2014\)](#), [Gautier et al. \(forthcoming\)](#), and [Blundell et al., 2004](#).

**Table 1**  
Model of Unemployment Experience.

<i>Regional home-ownership</i>			
County home-ownership proportion	4.17	***	(.153)
<i>Personal characteristics</i>			
Mortgage	-0.0443	***	(.006)
Owner-occupier	-0.0393	***	(.004)
Male	0.0402	***	(.005)
Age	0.0094	***	(.001)
Age squared	-0.0001	***	(.000)
<i>Marital status</i>			
Single	-0.0294	***	(.010)
Married	-0.0616	***	(.009)
Separated	-0.0172		(.026)
Divorced	-0.0236		(.015)
Unknown	-0.0151		(.013)
Household size	0.0047	*	(.002)
Number of children	-0.0198	***	(.003)
<i>Education</i>			
Lower secondary	0.0306	***	(.004)
Higher secondary	-0.0039		(.005)
Vocational college	-0.0402	***	(.012)
Lower University	-0.0620	***	(.015)
Higher University	-0.0790	***	(.012)
Graduate school	-0.1722	***	(.051)
<i>Type of municipality</i>			
Capital region	-0.0284		(.018)
Urban	-0.0084		(.006)
Semi-urban	-0.0047		(.007)

Notes: Marginal effects calculated at sample means from a probit regression including county dummies and year dummies.  $N = 30,316$ . Robust clustered (county-year) standard errors in parentheses. \*, \*\* and \*\*\* denote significance at 10%, 5% and 1% levels, respectively. The omitted category is widowed females with a basic or no degree residing in a rural region.

the home-ownership proportion probably arises from the fact that the regional demand for owner-occupied housing depends positively on the employment of individuals residing in the region. Regional labour supply and demand shocks ( $\delta_{jt}$ s in our model) are, thus, likely to induce a negative association between home-ownership and unemployment experience. Controlling for labour supply and demand factors would alleviate the endogeneity problem and reduce the downward bias in the coefficient of regional home-ownership. Thus, it is not surprising that many of the earlier studies that include a broad range of regressors in their unemployment equation estimate a positive coefficient on the home-ownership variable (Costain and Reiter, 2008; DiTella and MacCulloch, 2005; Nickell, 1998). However, as Oswald (1996) points out, instrumental variables are needed to obtain an unbiased estimate of the causal effect. Appropriate instruments for regional home-ownership are rare, and, therefore, it is likely that the earlier literature has been unable to identify the causal effect reliably. To our knowledge, the papers by Coulson and Fisher (2009) and Wolf and Caruana-Galizia (2015) are the only studies that use instrumental variables for regional home-ownership rate. Coulson and Fisher (2009) use both microdata and regional-level data from the United States. Their instrument in regional-level analyses is the state marginal tax rate applied to mortgage interest deduction.<sup>3</sup> Wolf and Caruana-Galizia (2015) analyse German regional data and use regional variation in WWII bombings as a source of identification.

Our paper is the first to use a policy reform to identify the causal effect of regional home-ownership on unemployment. Since the rental housing market deregulation reform that we

exploit does not create groups of eligible and ineligible individuals within regions, we are unable to identify the causal individual-level effect.<sup>4</sup> However, the reform allows us to reliably answer a more novel question, namely that related to the possible externalities caused by home-ownership.<sup>5</sup> We begin by estimating a model assuming exogenous regional home-ownership proportions ( $hor_{jt}$  is independent of  $\delta_{jt}$ ). We then proceed and relax this assumption and use the deregulation reform to construct instrumental variables to deal with endogeneity.

We employ a Finnish register-based data set augmented with information on regional housing markets and the rental housing market deregulation reform. The individual-level data set used is a service file of annual Income Distribution Statistics (IDS) for years 1990–2, which includes a rich set of register and survey variables on more than 30,000 individuals in more than 11,000 households per year. IDS contains information on individuals' labour market outcomes during the year. Specifically, we know the number of unemployment and employment months reported by the individual. The data include information on individuals' housing tenure and important control variables, such as sex, education, age and household composition. Information on the county of residence is included for each respondent, which allows us to match county-level home-ownership proportions to the data and to divide the sample individuals into those affected by the deregulation reform and those not affected.<sup>6</sup>

## 4. Empirical analysis and results

### 4.1. Descriptive analysis

To conduct a descriptive analysis, we first estimate the unemployment experience model for the years 1990–2 using county-level home-ownership as an explanatory variable. To reflect the average home-ownership, the proportion of home-ownership is calculated as the average of the year-end value and the previous year-end value. We have restricted the sample to individuals of official working age (from 15 to 64 years old). Descriptive statistics for the sample can be found in the Appendix. County dummies and year dummies as well as individual-level control variables are included in the model. Results of this exercise are presented in Table 1. Because the model includes an aggregate-level regressor, we need to account for the possibility of a correlation between the error terms of individuals within a county. We use robust standard errors that allow for such correlation (see Moulton, 1986).

The coefficient for regional home-ownership is positive and statistically significant, and, thus, we are able to get a result in line with Oswald's (1996) results using individual-level data and controlling for characteristics of individuals.<sup>7</sup> Regional home-

<sup>4</sup> It is rarely the case in the labour market spillover literature that both individual-level and group-level effects can be identified. See e.g. Blundell et al. (2004) and Crépon et al. (2013) for studies in which analyses of both effects are conducted.

<sup>5</sup> It is natural that many of the earlier studies have focused on the individual-level effects, because the only studies that have acknowledged the possibility of externalities are those of Coulson and Fisher (2009) and Blanchflower and Oswald (2013).

<sup>6</sup> Counties were the second tier of government, the third tier being the municipalities. The average size of a county in the end of year 1990 was 416,540 inhabitants, which is about 8.3 % of the total population. The largest county was the one around the capital region with 25 % of the total population and the largest one was the island of Åland, with 0.5 % of the total population. The advantage of using counties instead of smaller regional units is that the likelihood of spillovers between regions is smaller. In general, it is crucial that the regions are sufficiently large so that the assumption of no spillovers between them can be justified.

<sup>7</sup> The estimated coefficient of regional home-ownership might seem to be relatively large, because a one percentage point increase in home-ownership is

<sup>3</sup> In their individual-level analyses, Coulson and Fisher (2009) use the tax rate instrument, the percentage of households in the region living in multifamily housing, and sex composition of the children in the family as instruments for individual-level home-ownership.

ownership is positively associated with unemployment experience, while owner-occupiers (especially those with mortgage loans) are less likely to experience unemployment than other individuals.<sup>8</sup> It seems that the endogeneity bias discussed in the previous section is not large enough to cause the coefficient estimate of the regional home-ownership variable to be negative. It is possible that financial liberalisation in the 1980s and the following substantial mortgage credit expansion created regional variation in home-ownership in the sample period. To the extent that this variation is exogenous, it alleviates the endogeneity of the regional home-ownership variable.

We have tried estimating our models using different definitions of the dependent variable. The overall effect mostly comes from short-term or occasional unemployment, i.e. probability of experiencing a small number of months of unemployment. This observation is in line with the results by Nickell (1998), who finds that there are statistically significant impacts from home-ownership to total unemployment and short-term unemployment but not to long-term unemployment. One possible explanation is that home-ownership affects groups that typically spend at least some of their months outside the labour force and, therefore, experience only limited number of months of unemployment (instead of employment) due to high home-ownership. Another explanation in our case may be that, because the time span of our data is short, home-ownership changes occur during the year and thus cause entries to unemployment in all months of the year. The result on the association between having a mortgage loan and being less likely to experience unemployment is in line with results reported by Flatau et al. (2003), who find that homeowners with mortgages experience shorter unemployment spells than homeowners without mortgages and individuals in other tenures.

At this point, it is worth noting that there is a potential danger in including the regional proportion of home-ownership and individual home-ownership variable (as well as the mortgage variable) in the same model.<sup>9</sup> Namely, variation in the regional proportion of home-ownership captures part of the variation in individual home-ownership and can be thought of as a result from a first-stage regression of individual home-ownership on region-year interaction dummies.<sup>10</sup> Angrist (2014) notes that running a model with both this kind of instrumented variable and the original variable may lead one to conclude that there are externalities in cases where they are, in fact, absent. This is the case if the original variable is endogenous for any reason. In turn, excluding the group-average variable from the regression leads to omitted variable bias in the estimate of the individual-level variable if there are externalities. Thus, including both variables is a correct strategy when externalities are suspected (as in our case) but may lead to false conclusions if the individual-level variable is endogenous.<sup>11</sup> To make sure that our results on the externalities are not biased because of endogeneity of individual-level home-ownership, we conduct robustness checks in the discussion section of the paper.

(footnote continued)

associated with about a four percentage point increase in the probability of an unemployment spell during the year. However, one has to bear in mind that our dependent variable is a dummy variable for any unemployment experience during the year, and, thus, the coefficient estimate cannot be interpreted as the effect on the unemployment rate.

<sup>8</sup> Our mortgage variable is at the individual level and the owner-occupancy variable at the household level. We also constructed a variable indicating a mortgage by another household member but it was not statistically significant in the regressions.

<sup>9</sup> We are grateful to Bas van der Klaauw for pointing this out to us.

<sup>10</sup> Although the regional home-ownership proportions do not exactly equal the sample averages because the proportions data come from a different source, it is obvious that the same logic applies.

<sup>11</sup> For more discussion on these issues, see Angrist (2014).

#### 4.2. The rental housing market deregulation reform

To investigate whether causality runs from regional home-ownership to unemployment experience, we use a rental housing market deregulation reform to estimate instrumental variables models of unemployment experience. The Finnish rental housing market was deregulated in the early 1990s. The most important feature of the reform was that rent-ceilings and exact limits on rent increases were removed. In addition, eviction without specifying its grounds was made easier. There was a serious shortage of rental housing in the country, and the goal of the reform was to encourage supply in the private rental sector. The opposition was worried that the reform would lead to significant price increases, and, therefore, the government wanted to experiment with deregulation in parts of the country. Specifically, the bill was a proposal to relax some of the existing rental housing market regulations in regions where 'demand and supply of rental housing are in approximate balance'. The seven counties of northern and central Finland were chosen as the region for the experiment. To exclude regions of large supply deficiency in the rental housing market, all university cities were left out of the experiment. However, the opposition argued that there were at least some localities that suffered from a shortage of rental housing in the region of the experiment and that the regulations were, therefore, at least partly binding. It should be noted that the experiment was implemented in a large part of the country. At the end of 1991, about one-third of all housing units in the country were located in the experiment regions. Theoretically, the reform had an increasing effect on the supply of rental housing and, thus, decreased the rate of home-ownership. This reason is that the regulations had constrained the income accrued to landlords and made eviction relatively difficult. Thus, deregulation increased the supply of rental dwellings by making renting more profitable for landlords. As we later show, the reform had a negative effect on home-ownership rates of the reform regions during our sample period.

The reform was implemented during the 1991–4 period, and the market was deregulated gradually. Different buildings and rental contracts were subject to deregulation in different years and, important to our identification strategy, the timing of the first phase of the reform differed between geographical areas. In the 'target regions', the seven counties in northern and central Finland (excluding the six largest cities), markets were deregulated earlier than those in the five remaining counties. In the first phase, in the beginning of 1991, new apartments and houses in the target region were subject to deregulation. The second change in the legislation, effective 1 Feb. 1992, deregulated all new contracts for private rental dwellings in the whole country. Finally, in the beginning of 1994, all rental contracts were deregulated. Thus, dwellings completed and rented between 1 Jan. 1991 and 1 Feb. 1992 were free from regulation in the target region but not in other regions. This means that in the target region only, some dwellings were free from regulation for 13 months. At the end of 1991, dwellings completed during that year constituted approximately 2.3 % of the housing stock in the country.

To serve as a relevant instrument, the reform needs to have had an effect on regional home-ownership. To get an idea of the effect, we have calculated time series of home-ownership rates from aggregate housing data. Comparisons between the development of home-ownership in the target regions and elsewhere reveal that the reform decreased home-ownership. Fig. 1 plots aggregate home-ownership rates in experiment regions and other regions four years prior to and four years after the reform. The vertical line indicates the beginning of the experiment in the beginning of 1991. Since the experiment counties were comprised of municipalities with fewer than 15,415 dwelling units, we have excluded municipalities larger than this from the calculation of the home-ownership rate of the other regions as well. In 1991, when the reform was implemented in the target regions only, the home-ownership rate of these regions





Fig. 1. Trends in home-ownership in experiment (solid line) and other (dashed line) regions.

decreased relative to the home-ownership rate elsewhere. Both before and after 1991, the home-ownership rates of the two regions followed an approximately similar trend. A closer look at the home-ownership rates of the regions reveals that the effect varies between municipalities of different sizes and types. Fig. 2 plots the home-ownership rates by municipality type. The effect is most pronounced in rural municipalities, although there is a visible effect in all municipality groups. In our instrumental variables analysis, we exploit the fact that it was only the new housing stock that was subject to the reform, and, thus, there were larger changes in the areas with more new dwellings. It is shown in the following section that, indeed, relevant instruments can be constructed using the reform.

It is also important to our analysis that the choice of the target regions was independent of the labour market prospects of the regions. It appears from the bill and the preceding committee report that the choice of regions was based on rental housing market conditions only. References to labour markets are virtually absent from these documents. This absence might be because the labour market was not a concern, because of very low unemployment after strong economic growth in the 1980s. As mentioned previously, the government wanted to experiment with deregulation in regions where rental housing markets were in approximate balance. This strategy was probably chosen because it would reduce the magnitude of unwanted consequences (rental price increases). However, neither a systematic analysis of regional markets was done nor did the committee suggest choosing any specific regions or any criteria. The government may have wanted to choose a set of administrative regions (counties) that constitute a single contiguous area. Based on the presumption that the housing markets were closer to equilibrium in the north than in the south, northern and central counties (excluding the university cities) were chosen. In light of the previous discussion, it seems clear that the choice of the reform regions was not related to labour market concerns. To check whether the choice of regions was related to pre-experiment changes in the outcome variables, we estimate difference-in-differences models using the 1989 and 1990 IDS data. It appears that pre-experiment changes in the probability of unemployment experience were virtually the same in the experiment region and elsewhere, both unconditionally (p-value 0.981) and conditional on our personal control variables (p-value 0.974). No statistically significant differences are found in other outcomes or for population sub-groups either.<sup>12</sup>

#### 4.3. Instrumental variables analyses

In constructing the instruments for the regional home-ownership variable, we take into account that two separate phases (1991 and 1992) of the rental housing market reform took place during the period of investigation. Furthermore, in six counties, the reform excluded the largest cities, whereas in one county, all municipalities were included. We construct four dummies that reflect the two phases and two different scales of the reform. The first dummy (a) is for the county that was fully exposed to the reform in 1991. The second dummy (b) is for this county in 1992. The third dummy (c) is for the other six reform counties where largest cities were excluded in 1991. The fourth dummy (d) is for these counties in 1992. Separate dummies are constructed for years 1991 and 1992 because of different phases of the reform but also because we wish to allow the effects of the reform on home-ownership to evolve over time (see the previous section). For 30 % of the observations in our sample, one of the dummies is non-zero. Since only new dwellings were deregulated in the reform, dummies a, b, c, and d are interacted with the share of deregulated multifamily dwellings in the county to construct instruments A, B, C, and D.<sup>13</sup> Since we use a midyear measure of the home-ownership proportion, the relevant new housing stocks are half the number of dwellings completed in 1991 and dwellings completed in January 1992 for the instruments of 1991 and 1992, respectively.<sup>14</sup> We expect that each of the instruments has a negative effect on regional home-ownership.

(footnote continued)

including the personal control variables. The sample is restricted to working age individuals as it is in all our analyses. All p-values based on county-year clustered standard errors are larger than 0.123 and half of them are larger than 0.440. Unfortunately, the three type-of-municipality dummies are not available in the year 1989 data. To see how important these variables are, we re-run all our models without controlling for type of municipality and the results are robust to doing this. These results are available on request.

<sup>13</sup> Interacting with the share of multifamily (rather than all) dwellings improves the strength of the instruments because renting single-family houses is rare. We also estimated models with dummy instruments and dummies interacted with all new dwellings. These alternative instruments appeared to have satisfactory explanatory power as well, and the qualitative results did not differ from those presented here.

<sup>14</sup> Our dwelling data are annual, but we have quarterly data on buildings by building type. The number of dwellings completed in January 1992 is measured as one-third of the share of buildings completed in the first quarter multiplied by the number of dwellings completed during the whole year. The exact way of measuring the new housing stock has only minor impacts on the estimates and does not influence the qualitative results.

<sup>12</sup> We run a difference-in-differences model for each combination of outcomes and population sub-groups used in our analyses, both unconditionally and

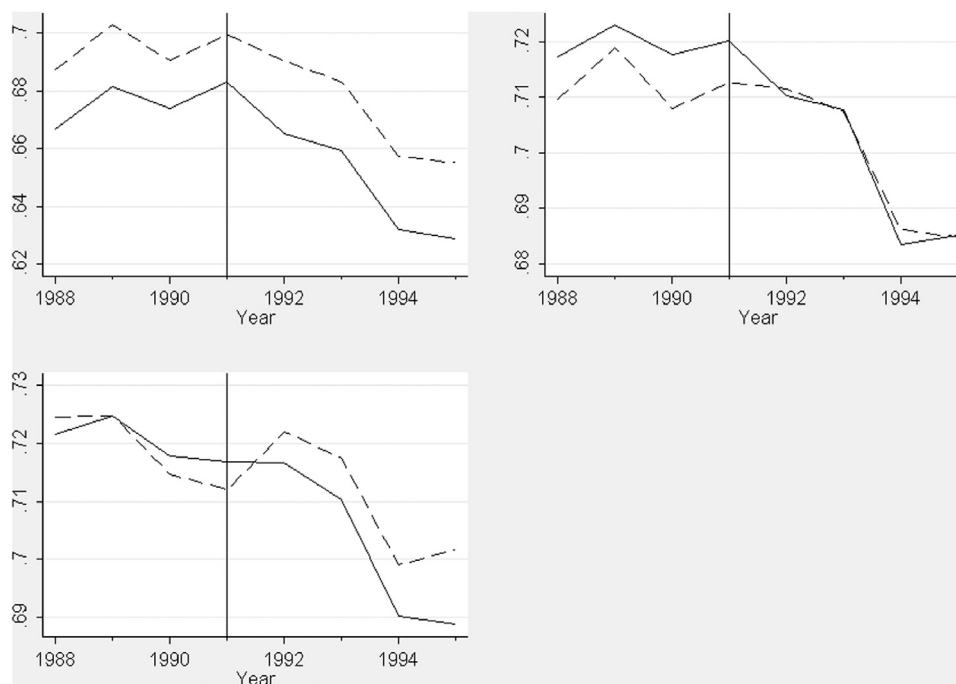


Fig. 2. Trends in home-ownership in experiment (solid line) and other (dashed line) regions by municipality type (top left: urban, top right: semi-urban, bottom: rural).

Table 2 presents the results from estimating an instrumental variables probit model of unemployment experience. The first-stage results indicate that, indeed, the reform had a negative effect on home-ownership in the target regions. The F-statistic on the excluded instruments is 22.13, indicating that the maximal second-stage bias and test size distortion are small (Stock and Yogo, 2002). The effect of regional home-ownership on unemployment experience is positive and statistically significant.<sup>15</sup> This finding confirms the result of the descriptive analysis in Table 1. As expected, the estimated coefficient is much larger when instrumental variables are used. This is because the estimate in Table 1 is contaminated by the endogenous variation in the proportion of home-ownership. Our results are consistent both with earlier results obtained by using microdata and the results of Oswald (1996) and other studies using aggregate data. Home-ownership seems to be positively associated with individual owner-occupiers' labour market outcomes and, at the same time, increase the overall unemployment experience probability.

#### 4.4. Mechanisms

Although the main aim of our analysis is to study the causal effect of home-ownership on unemployment experience, our data also permit us to explore some possible mechanisms through which the effect might work. Oswald (1999) and Blanchflower and Oswald (2013) discuss various potential labour market effects of home-ownership, some of which involve external impacts. The externalities are related to labour immobility and not-in-my-backyard (NIMBY) behaviour. Immobility of homeowners makes it more difficult for other people to move close to ideal jobs as well, and the labour market, as a whole, becomes less efficient. Less job creation may follow from homeowners' NIMBY sentiment if they

<sup>15</sup> Given that we have four instruments, the overidentifying restrictions could be tested to examine the validity of the instruments. However, such a test is not available for IV probit models with clustered errors. We thus re-estimated all our IV models as linear probability IV models with clustered errors and conducted Hansen's J tests. The tests were rejected in none of the cases. Moreover, the results were found to be robust to using linear probability models instead of probit models.

try to influence zoning to prevent businesses from locating to their home region. Blanchflower and Oswald (2013) find evidence in favour of the aforementioned mechanisms in the longer run. Indeed, these mechanisms are more likely to be more relevant in the longer run, and we focus on the immediate effects of home-ownership. In what follows, we conduct analyses that shed light on the relevance of two new hypotheses based on the idea that households' decisions about labour supply and consumption may be linked to changes in their housing tenure. This happens because most home purchases, typically financed by mortgage loans, are associated with increased housing expenditures.<sup>16</sup> Our two hypotheses might be relevant in light of recent research and can be tested using our data, but we do not claim that there are no other (positive or negative, short-run or long-run) impacts of home-ownership on the labour market.

It has been argued by some authors that homeowners' favourable labour market outcomes are caused by homeowners' lower reservation wages and relatively active local job search, which arise from the need to meet high mortgage payments and the reluctance to move to another region (e.g. Flatau et al., 2003 and Munch et al., 2006). Recent research on displacement effects in the labour market suggests that increases in job search activity and labour supply may produce negative externalities in the local labour market. As one group of individuals increases its labour supply and working, other individuals may be displaced from jobs as a result. This situation is especially likely in the short-run, since labour demand can be assumed to be less elastic in the short-run than in the long-run. Crépon et al. (2013), Ferracci et al. (2014), Gautier et al. (forthcoming) and Blundell et al. (2004) have studied displacement effects associated with labour market programmes. So far the displacement literature has focused mainly on labour market programmes but Lalive et al. (2015) argue that displacement may occur as a result of a change in unemployment insurance generosity as well. The literature review by Abbring and

<sup>16</sup> In our sample, about 73 % of the households who bought their homes had a mortgage loan. Scanlon and Whitehead (2004) have documented that the average expenditures on housing have been higher for owners than for renters in most countries both in the 1990s and in the 2000s.

**Table 2**  
IV model of unemployment experience.

<i>Regional home-ownership</i>			
County home-ownership proportion	9.51	***	(3.14)
<i>Personal characteristics</i>			
Mortgage	-0.0443	***	(.006)
Owner-occupier	-0.0393	***	(.004)
Male	0.0403	***	(.005)
Age	0.0094	***	(.001)
Age squared	-0.0001	***	(.000)
<i>Marital status</i>			
Single	-0.0296	***	(.010)
Married	-0.0616	***	(.009)
Separated	-0.0171		(.026)
Divorced	-0.0228		(.015)
Unknown	-0.0153		(.013)
Household size	0.0048	*	(.002)
Number of children	-0.0198	***	(.003)
<i>Education</i>			
Lower secondary	0.0304	***	(.004)
Higher secondary	-0.0041		(.005)
Vocational college	-0.0402	***	(.012)
Lower University	-0.0624	***	(.015)
Higher University	-0.0789	***	(.012)
Graduate school	-0.1728	***	(.051)
<i>Type of municipality</i>			
Capital region	-0.0282		(.018)
Urban	-0.0081		(.006)
Semi-urban	-0.0046		(.007)
<i>First-stage results: Excluded instruments</i>			
Instrument A	-0.1661	***	(.043)
Instrument B	-0.6135	***	(.188)
Instrument C	-0.1821	***	(.021)
Instrument D	-0.4877	***	(.119)
F-statistic	22.13	***	

Notes: Marginal effects calculated at sample means from a probit regression including county dummies and year dummies. N = 30,316. Robust clustered (county-year) standard errors in parentheses. \*, \*\* and \*\*\* denote significance at 10%, 5% and 1% levels, respectively. The omitted category is widowed females with a basic or no degree residing in a rural region.

Heckman (2007) includes a thorough discussion on displacement and various other types of external effects in the labour market. Since, in theory, displacement may be associated with any increase in labour supply, we argue that high job search intensity and low reservation wages of new homeowners may lead to displacement of other workers in the same region. In sum, our displacement hypothesis is that higher home-ownership leads to higher job search intensity and lower reservation wages. Under the assumption of less than perfectly elastic labour demand, these lead to externalities which are captured by the estimated effect of regional home-ownership.

In addition to boosting homeowners' labour supply, mortgage loans associated with home purchases may also affect consumption. Some recent studies have examined the effects of household credit on consumption behaviour. It has been argued that credit-constrained households with debt are forced to cut back on spending when house prices are declining (Dynan, 2012; Mian et al., 2013; Mian and Sufi, 2010). However, debt may, under some circumstances, be negatively linked to consumption, even in the absence of unexpected changes in asset values. Stephens (2008) shows that repayment of a vehicle loan leads to an increase in nondurable consumption. Coulibaly and Li (2006) examine the effect of the final mortgage payment and find that it is associated with an increase in durable consumption (house furnishings and entertainment equipment). Although the credit-constraint explanation of these results does not imply that individuals would cut back on consumption when they become borrowers, the possibility that home buying and associated mortgage borrowing decrease consumption cannot be ruled out *a priori*.<sup>17</sup> For example, one

can imagine reasons why households do not want or are unable to save as much before they buy their home as after the purchase. Many countries have policies that favour home-ownership and make it a relatively profitable method of saving. Thus, it may be optimal for households to start saving more (and consuming less) after a home purchase either by committing to high mortgage principal repayments or by making extra repayments. Another possible mechanism that would lead to decreased consumption is related to self-control problems that prevent individuals from saving as much as they would like. Laibson (1997) has studied self-control problems and has mentioned mortgage contracts as an example of commitment devices that help individuals force themselves to save more.<sup>18</sup> A recent study by Mian and Sufi (2014) links consumption decreases caused by house price reductions to decreases in local employment. Similarly, if an increase in home-ownership and associated mortgage borrowing leads to a reduction in household spending, this may be negatively reflected in the local labour market. Our consumption hypothesis thus is that higher home-ownership leads to less spending which, through lower aggregate demand, leads to externalities that are captured by the estimated effect of regional home-ownership.<sup>19</sup>

Both the displacement hypothesis and the consumption hypothesis lead to empirically testable predictions about the labour market externalities of home-ownership. In Table 3, we start by presenting coefficient estimates on regional home-ownership separately for homeowners with mortgages, owner-occupiers who do not have a mortgage loan and non-owners.<sup>20</sup> It appears that only homeowners with a mortgage are not affected by the external impact, and that the effect is larger for non-owners than for owner-occupiers. In the light of the displacement story, non-owners may be more likely to be displaced than owners. This happens because their job search is not intensified and their reservation wages are not lowered by their housing tenure, which arguably makes non-owners more vulnerable to changes in local competition for jobs.<sup>21</sup>

We next see if results in line with the displacement hypothesis can be found. Clearly, displacement is strongest for individuals whose labour is a close substitute for the labour of individuals who recently bought their homes and, as a result, increased their labour supply. As a simple test, we estimate the unemployment experience model separately for individuals whose personal characteristics are similar to those who bought their home during the year and for individuals who are dissimilar to them. To do this, we first estimate a probit model of the probability of buying a home. The explanatory variables are the

(footnote continued)

consumer's optimisation problem. Thus, if there is a change in the amount of labour supplied because of a change in housing tenure, consumption is likely to change as well and *vice versa*. Thus, the claim that housing tenure affects consumption is closely linked to the claim that housing tenure affects labour supply.

<sup>18</sup> Ashraf et al. (2006) find empirical support for the claim that individuals are willing to use commitment devices to be able to save more.

<sup>19</sup> Home purchase may also be associated with (at least temporary) spending increases due to spending on home utilities, maintenance, or new durables for the new home. We would like thank an anonymous reviewer for pointing this out. It is worth noting that such consumption increases would make the consumption effects smaller or even reverse them, making detection of the effects of consumption reductions less likely.

<sup>20</sup> Notice that we assume that the instruments are equally valid in the analyses presented in Table 3. The F-statistics on the excluded instruments are presented for the models in which the sample differs from the one used earlier (the footnote for Table 3).

<sup>21</sup> In analyses not presented here, we also found that the effect is more pronounced on groups whose labour market attachment is likely to be weak and who, therefore, are more likely to be affected by job competition. These groups include students, mothers of pre-school children and individuals younger than 25 years old. We also found that the effects on unemployment experience are largest for people living in semi-urban municipalities (Statistics Finland defines a semi-urban municipality as a municipality in which 'at least 60 per cent but less than 90 per cent of the population lives in urban settlements and in which the population of the largest urban settlement is at least 4,000 but less than 15,000'). These results are available on request.

<sup>17</sup> Notice also that labour supply and consumption choices are both part of the

**Table 3**  
IV models of unemployment experience and employment experience.

<i>Effect on sub-group unemployment</i>				
Home-owners with mortgage	2.49		(3.79)	N = 6,682
Owner-occupiers, no mortgage	9.89	***	(3.77)	N = 17,625
Non-owners	15.99	***	(3.17)	N = 5,998
Similar to buyers	14.59	***	(3.99)	N = 15,140
Dissimilar to buyers	4.43		(2.88)	N = 15,157
<i>Effect on employment</i>				
All employment	-6.21	***	(1.79)	N = 30,316
<i>Employment in</i>				
buyers' sectors	-4.61	**	(2.02)	N = 30,316
other sectors	1.60		(3.16)	N = 30,316
non-tradable sectors	-4.58	**	(2.02)	N = 30,316
tradable sectors	1.33		(3.13)	N = 30,316
non-tradable & buyers' sectors	-1.33	*	(.805)	N = 30,316
non-tradable & other sectors	-3.00	*	(1.60)	N = 30,316
tradable & buyers' sectors	-3.13		(1.91)	N = 30,316
tradable & other sectors	4.78		(3.70)	N = 30,316

Notes: Marginal effects calculated at sample means from probit regressions including county dummies and year dummies. Robust clustered (county-year) standard errors in parentheses. \*, \*\* and \*\*\* denote significance at 10%, 5% and 1% levels, respectively. Control variables as in Tables 1 and 2. The first-stage F-statistics for the first five models are 25.18, 18.90, 30.90, 26.10 and 18.63.

same personal characteristics as in our unemployment experience model (gender, age, age squared, marital status, household size, number of children and education). We then divide the sample in two based on the predicted probability of being a buyer. This gives us one group with personal characteristics similar to buyers and another group with characteristics less similar to buyers. Estimating the unemployment experience model for the individuals who are similar to buyers yields a large and statistically significant estimate, whereas the estimate for the dissimilar half of the sample is much smaller and not statistically significant.<sup>22</sup> The result that individuals who are similar to buyers are more affected by regional home-ownership is in line with the displacement hypothesis.

Next, we perform another analysis related to displacement. Crépon et al., 2013 note that the more there are individuals whose job search is positively affected in a market, the larger the displacement is likely to be among the job seekers in that market. We do not have data on job search, but we know the sector of all sample individuals who work. Applying the idea of Crépon et al. (2013) to our case implies that the more there are buyers working in a given sector, the fewer chances there are that other workers find employment in that sector. Thus, we calculate the number of buyers working in each sector and, based on this figure, divide sectors in two groups with both representing about half of the total employment. It should be noted that displacement may be due to both intensive margin and extensive margin changes in the labour supply of buyers. Number of buyers is a proxy for the total displacement potential in a sector because it includes both the number of individuals who started working in that sector and the number of individuals who already worked in that sector and may have increased their working hours.

In Table 3, we first present the overall effect of regional home-ownership on the probability of working during the year and then estimate the model separately for working in the two groups of sectors. The overall effect of regional home-ownership on employment experience is negative and, thus, in line with the estimated effect on unemployment experience. We find a negative employment effect in sectors where a large number of buyers are working ('Buyers' sectors'),

whereas the estimated effect is not statistically significantly different from zero for sectors with relatively few buyers ('Other sectors'). This result is in line with the displacement hypothesis, and it seems that individuals whose labour supply is increased because of a home purchase may displace other workers in the sectors in which they work.

The two tests that we have performed seem to support the displacement hypothesis. We next turn to the externality mechanism that works through a reduction in consumption resulting from home-ownership. Mian and Sufi (2014) argue that the employment changes that are caused by consumption changes can be identified by examining employment changes separately in non-tradable and tradable sectors. Tradable sectors produce goods that can be consumed outside the region where they are produced. Non-tradable goods, in turn, are consumed locally. Therefore, a local change in consumption demand has a local impact on producers of non-tradable goods, but the impact on tradable sectors is spread over regions. We use a strategy similar to that of Mian and Sufi (2014) and test whether the local employment effect of a region's home-ownership differs between non-tradable and tradable sectors. We classify a sector as non-tradable if it produces mostly goods that are consumed locally.<sup>23</sup> Other sectors are classified as tradable. The results presented in Table 3 show that home-ownership has a negative effect on employment experience in non-tradable sectors but has no effect on employment experience in tradable sectors. These results are in line with the idea that home-ownership decreases employment by decreasing consumption. The advantage of the test is that it is hard to imagine any other mechanism that would result in similarly differential effects on non-tradable and tradable sectors (Mian and Sufi, 2014).

It is not possible to reliably disentangle the displacement effect and the consumption effect with our data. We perform one more test to shed some light on which of the two effects is likely to be of more importance. We divide the sectors into four groups based on the two previous classifications. If there were displacement but no consumption effect, we would find an effect only on sectors with large number of buyers (non-tradable/buyers' and tradable/buyers'). If instead there was only the consumption effect but no displacement, we would find an effect only on non-tradable sectors (non-tradable/buyers' and non-tradable/other). Finding an effect on all groups of sectors except the tradable/other group would indicate the coexistence of the two effects. The results in Table 3 indicate that there is only the consumption effect. However, the effect on tradable/buyers' sectors is very close to significance. Based on these analyses, it would be safe to conclude that home-ownership might influence employment through both displacement and consumption effects.

## 5. Discussion

It was mentioned earlier that our strategy of including both regional-level home-ownership and individual-level home-ownership in our models may lead to biased estimates of externalities. In particular, endogeneity of the individual-level home-ownership variable could cause us to conclude that there are externalities in the case where they are absent (see Angrist, 2014). The danger applies to all analyses in this paper, and we want to make sure that our conclusions about the externalities are not false. Thus, we have estimated alternative versions

<sup>22</sup> We lose some observations in the sample of individuals similar to buyers because graduate schooling predicts non-unemployment perfectly in this subsample. We have tried combining the two highest educational categories and including all observations and the results did not change.

<sup>23</sup> The sectors classified as non-tradable include retail and wholesale, restaurants, bars, canteens and catering, taxis, motion picture and video production and distribution, motion picture projection, arts performances, concerts and artistic creation and interpretation, libraries, archives, museums and exhibitions, sporting activities and operation of sport arenas and stadiums, dance halls and dancing schools, gambling, circuses, amusement parks, other recreational activities, hair-dressers, beauty salons, photo portraits, day care, funerals and other personal and household services. As Mian and Sufi (2014) point out, there are sectors that could be classified as either non-tradable or tradable. We tried different classifications and the results were qualitatively similar.



of all models. In these models, the individual home-ownership variable (as well as the mortgage variable, which is likely to be endogenous as well) is excluded. In such a model, what is being estimated is simply the effect of regional home-ownership on the outcome variable. Notice that the estimated effect now includes the direct effect on those whose housing tenure changes *and* the indirect, external effect on the residents of the region. Thus, we do not get separate estimates of the direct effects and externalities. One way of interpreting the resulting coefficients is that we get biased estimates of the external effects. The bias is due to omitted individual-level home-ownership variable. The sign of the bias is negative in the unemployment models and negative in the employment models because the direct effect of home-ownership is likely to be negative on unemployment and positive on employment.<sup>24</sup>

The differences between the results of the robustness checks to the results presented earlier are negligible. The only exception is the estimated effect of regional home-ownership on employment in the non-tradable sectors where a large number of buyers are working (Table 3, employment in non-tradable & buyers' sectors), which loses its statistical significance (the p-value is 0.112). Notice that since a large number of buyers are working in these sectors, the direct effect through buyers' increased labour supply is likely to be large. Therefore, the upward bias in the estimate of the indirect effect on employment is large as well. Our robustness checks suggest not only that our findings are robust, but also that the indirect effect (externality) dominates the direct individual-level effect.

Although the way the experiment regions were selected, the pre-experiment outcome changes, and the overidentifying tests all point to validity of the instruments, they may, by chance, be correlated with some important factors that are not controlled for. As mentioned earlier, Finland experienced large negative labour market changes, including those caused by the collapse of the Soviet Union, during the period of investigation. Thus, one important factor may be the regional sectoral employment structure which may have affected the exposure of a region to the macroeconomic labour market shocks. To check robustness, we calculated regional employment shares of some sectors just before the beginning of the analysis period (year-end 1989) and interacted them with year dummies for the post-experiment years (1991 and 1992). Such control variables allow the effects of the macroeconomic labour market shocks on counties' labour market outcomes to depend on cross-county differences in sectoral employment shares. First, to directly capture the impact of Soviet trade exposure in counties, we collected national-level sectoral data on exports to the Soviet Union, total exports, and gross output from OECD databases.<sup>25</sup> Following (Gorodnichenko et al., 2012), we calculated 'Soviet sector' employment share for each county by multiplying sectors' county-level employment figures by the shares of those sectors' Soviet exports out of all exports. By dividing this by the total county-level employment, we got the county-level share of Soviet sector employment. An alternative Soviet sector measure was constructed by using the value of gross output instead of total exports. Second, we estimated models in which we controlled for employment shares of primary and secondary sectors in a similar fashion. Our key result on unemployment experience and the findings on sub-groups' unemployment experience are robust to including the controls. The results on employment experience, especially sector-specific experience, are less robust. On one hand, the results on the mechanisms should thus be taken with

some caution. On the other hand, it is difficult to accurately classify sectors to non-tradable/tradable etc., which arguably makes these results more vulnerable. The relatively small number of county-year pairs from which the results on county-level regressors are identified makes it natural that, as we do find, adding more county-level control variables (especially non-significant ones) results in multicollinearity problems and, in many cases, mostly non-significant coefficients on regional-level variables.

We have presented evidence on two new mechanisms that may at least partly explain the externalities. It should be noted that the displacement effect would have to be very large for it alone to generate a positive relationship between the rate of home-ownership and the unemployment rate. It would require that more than one worker becomes unemployed by every newly employed homeowner. This would be possible only if homeowners worked longer hours than the displaced workers. We do not think that it is a credible claim that each homeowner displaces more than one other worker. Since our dependent variables measure any experience of unemployment and employment, from one month to 12 months, more than one-to-one displacement is more likely. It appears from additional analyses (not presented here) that the effects on experiencing only a few months of unemployment or employment are more pronounced while the effects on full-year unemployment and employment are smaller or negligible. Home-owners with mortgage and those who bought their homes in our data are less likely to experience small numbers of unemployment or employment months. Thus, many displacements are likely to involve a permanent job resulting in a loss of many short-term jobs. In the extreme, a displacement in our data may mean a displacement of 12 one-month jobs by one 12-month job.

There are no obvious bounds on the magnitude of the consumption effect, because it depends on the reduction of money spent and on its effect on employment. It should be noted that the total employment effect of home-ownership may differ from the estimated effect on non-tradable sectors' employment. This is because home-ownership may result in differential changes in consumption of different goods. For example, if home buying is not only associated with a reduction in spending on non-tradable goods, but also with an increase in spending on tradable goods, the effect on employment is negative locally but may even be positive globally. On the other hand, the estimated effect on the local labour market understates the global effect if there is a reduction in consumption of tradable goods as well. Further, there may be differences between employment effects of demand changes between sectors.<sup>26</sup> Thus, further research on the effects of housing tenure on total consumption and consumption of different goods is needed to shed light on the consumption effect of home-ownership. Given that home purchases are usually associated with higher housing costs, the negative local effect on non-tradable goods is very unlikely to be fully compensated by a positive global effect on other goods. In such a case consumption would simply shift from non-tradable to tradable goods. The positive relationship between home-ownership and unemployment can often be found using cross-country data and it does not seem likely that home-ownership would merely cause a shift from domestic products to imported products.

A final note should be made about the consumption effect. During our period of analysis, house prices were rapidly falling and interest rates were rising in the wake of a major financial crisis. These developments were likely to cause a reduction in household spending because of unexpected household balance sheet changes and increases in mortgage payments.<sup>27</sup> Although more home-ownership means that there are more households susceptible to

<sup>24</sup> Here, we rely on the findings of earlier micro-level studies listed in the introduction of the paper. There are no studies that document negative employment effects of home-ownership at the individual-level.

<sup>25</sup> Similarly to Gorodnichenko et al. (2012) we used OECD's International Trade by Commodity Statistics data on Soviet exports, STAN Bilateral Trade data on total exports, and STAN Database for Structural Analysis data on gross output. World Bank's concordance tables were used to convert product data into sectoral data.

<sup>26</sup> Mian and Sufi (2014) use theoretical assumptions to be able to extrapolate the employment changes in non-tradable sectors to other sectors.

<sup>27</sup> Most mortgages in Finland were, and still are, variable-rate mortgages.

the changes and, thus, more households which cut back on consumption, we believe that this may have only a small impact on our estimates. This is because we study the immediate effects of changes in home-ownership. It is unlikely that there was sufficient time for the house price and interest rate shocks to affect the consumption decisions of households which had bought their homes in the same year.

## 6. Conclusions

The earlier evidence on the labour market effects of home-ownership is mixed. Many studies have found that a higher prevalence of home-ownership is associated with higher aggregate unemployment, whereas studies using microdata suggest that homeowners have relatively favourable labour market outcomes. We find that home-ownership has effects above and beyond the direct effects on individuals. Namely, although homeowners are less likely to experience unemployment, significant externalities counteract the positive effects of home-ownership at the ag-

gregate geographical level. The externalities may be because of consumption reductions and increased local job competition caused by home purchases, especially if the purchases are financed by debt. More theoretical and empirical research is needed to better understand the mechanisms at work.

## Acknowledgements

We would like to thank Lars-Erik Borge, Petri Böckerman, Matz Dahlberg, Bruno Decreuse, Matti Hovi, Bas van der Klaauw, Kaisa Kotakorpi, Andrew Oswald, Tuomas Pekkarinen, Jukka Pirttilä, Tuukka Saarimaa, Matti Sarvimäki, Hannu Tervo, Jari Vainiomäki and participants of the EEA congress in Glasgow, IIPF congress in Dresden, PEARL conference in Helsinki and seminars in Rennes and Tampere for helpful comments and discussions. This work was supported by the Academy of Finland (project number 252369) and the Finnish Doctoral Programme in Economics.

## Appendix

**Table A1**  
Descriptive statistics.

		Mean	St. dev.	Min	Max
<i>Dependent variables</i>					
Unemployment experience		0.128	0.334	0	1
Employment experience		0.789	0.408	0	1
Non-tradable industries		0.153	0.360	0	1
Tradable industries		0.637	0.481	0	1
Buyers' industries		0.393	0.488	0	1
Other industries		0.397	0.489	0	1
<i>Explanatory variables</i>					
County home-ownership proportion		0.668	0.035	0.603	0.718
Mortgage		0.220	0.415	0	1
Owner-occupier		0.802	0.399	0	1
Male		0.508	0.500	0	1
Age (year-end)		39.363	13.502	16	64.917
Marital status	Single	0.309	0.462	0	1
	Married	0.584	0.493	0	1
	Separated	0.003	0.053	0	1
	Widow	0.038	0.190	0	1
	Divorced	0.024	0.154	0	1
	Unknown	0.043	0.202	0	1
Household size		3.269	1.434	1	16
Number of children		0.909	1.194	0	13
Education	Basic or no degree	0.414	0.493	0	1
	Lower secondary	0.262	0.440	0	1
	Higher secondary	0.197	0.398	0	1
	Vocational college	0.047	0.211	0	1
	Lower university	0.025	0.156	0	1
	Higher University	0.049	0.216	0	1
	Graduate school	0.006	0.079	0	1
	Capital region	0.143	0.350	0	1
	Urban	0.380	0.486	0	1
Type of municipality	Semi-urban	0.166	0.372	0	1
	Rural	0.311	0.463	0	1

## References

- Abbring, J.H., Heckman, J.J., 2007. Econometric evaluation of social programs. In: Heckman, Leamer (Eds.), Part III: Distributional Treatment Effects, Dynamic Treatment Effects, Dynamic Discrete Choice, and General Equilibrium Policy Evaluation, *Handbook of Econometrics* Edition 1. Vol. 6B. Elsevier.
- Acemoglu, D., Angrist, J., 2000. How large are the social returns to education: Evidence from compulsory schooling laws. *NBER Macroeconomics Annual* 2000, 9–74.
- Angrist, J., 2014. The perils of peer effects. *Labour Economics* 30, 98–108.
- Ashraf, N., Karlan, D., Yin, W., 2006. Tying Odysseus to the mast: Evidence from a commitment savings product in the philippines. *Quarterly Journal of Economics* 121 (2), 635–672.
- Battu, H., Ma, A., Phimister, E., 2008. Housing tenure, job mobility and unemployment in the u.k. *The Economic Journal* 118, 311–328.
- Blanchflower, D.G., Oswald, A.J., 2013. Does high home-ownership impair the labor market? NBER Working Paper No. 19079.
- Blundell, R., Costa Dias, M., Meghir, C., Van Reenen, J., 2004. Evaluating the employment impact of a mandatory job search program. *Journal of the European Economic Association* 2 (4), 569–606.
- Bourassa, S., Haurin, D., Hendershott, P., Hoesli, M., 2013. Mortgage interest deductions and homeownership: An international survey. *Journal of Real Estate Literature* 21 (2), 181–203.
- Böckerman, P., Laaksonen, S., Vainiomäki, J., 2010. Micro and macro level wage rigidity: Lessons from finland. *Finnish Economic Papers* 23 (1), 27–42.
- Costain, J.S., Reiter, M., 2008. Business cycles, unemployment insurance, and the calibration of matching models. *Journal of Economic Dynamics and Control* 32 (4), 1120–1155.
- Coulbaly, B., Li, G., 2006. Do homeowners increase consumption after the last mortgage payment? an alternative test of the permanent income hypothesis. *Review of Economics and Statistics* 88 (1), 10–19.
- Coulson, N.E., Fisher, L.M., 2009. Housing tenure and labor market impacts: The search goes on. *Journal of Urban Economics* 65, 252–264.
- Crépon, B., Duflo, E., Gurgand, M., Rathelot, R., Zamora, P., 2013. Do labor market policies have displacement effects? evidence from a clustered randomized experiment. *Quarterly Journal of Economics* 128 (2), 531–580.
- Di Tella, R., MacCulloch, R., 2005. The consequences of labor market flexibility: Panel evidence based on survey data. *European Economic Review* 49 (5), 1225–1259.
- Dynan, K., 2012. Is a household debt overhang holding back consumption? *Brookings Papers on Economic Activity*, 299–362.
- Ferracci, Jolivet, Van den Berg, 2014. Evidence of treatment spillovers within markets. *Review of Economics and Statistics* 96 (5), 812–823.
- Flatau, P., Forbes, M., Hendershott, P.H., Wood, G., 2003. Homeownership and unemployment: The roles of leverage and public housing. NBER Working Paper No. 10021.
- Gautier P., Muller P., Van der Klaauw B., Rosholm M. and Svarer M., Estimating equilibrium effects of job search assistance. *Journal of Labor Economics*, forthcoming.
- Gorodnichenko, Y., Mendoza, E.G., Tesar, L.L., 2012. The Finnish great depression: From Russia with love. *The American Economic Review* 102 (4), 1619–1643.
- Green, R.K., Hendershott, P.H., 2001. Home-ownership and unemployment in the u.s. *Urban Studies* 38, 1501–1520.
- Hilber, C.A., Turner, T.M., 2014. The mortgage interest deduction and its impact on homeownership decisions. *Review of Economics and Statistics* 96 (4), 618–637.
- Isebaert, D., Heylen, F., Smolders, C., 2015. Houses and/or jobs: Ownership and the labour market in belgian districts. *Regional Studies* 49 (8), 1387–1406.
- Laibson, D., 1997. Golden eggs and hyperbolic discounting. *Quarterly Journal of Economics* 112 (2), 443–478.
- Lalive, R., Landais, C., Zweimüller, J., 2015. Market externalities of large unemployment insurance extension programs. *American Economic Review* 105 (12), 3564–3596.
- Van Leuvensteijn, M., Koning, P., 2004. The effects of home-ownership on labour mobility in the netherlands. *Journal of Urban Economics* 55, 580–596.
- Lyytikäinen, T., 2006. Rent control and tenants welfare: the effects of deregulating rental markets in finland. VATT Discussion Papers 385.
- Government Institute for Economic Research (VATT).
- Mian, A., Rao, K., Sufi, A., 2013. Household balance sheets, consumption, and the economic slump. *Quarterly Journal of Economics* 128 (4), 1687–1726.
- Mian, A., Sufi, A., 2010. The great recession: Lessons from microeconomic data. *American Economic Review, Papers & Proceedings* 100 (2), 51–56.
- Mian, A., Sufi, A., 2014. What explains the 2007–2009 drop in employment? *Econometrica* 82 (6), 2197–2223.
- Moulton, B.R., 1986. Random group effects and the precision of regression estimates. *Journal of Econometrics* 32, 385–397.
- Munch, J.R., Rosholm, M., Svarer, M., 2006. Are homeowners really more unemployed? *The Economic Journal* 116, 991–1013.
- Munch, J.R., Rosholm, M., Svarer, M., 2008. Home ownership, job duration, and wages. *Journal of Urban Economics* 63, 130–145.
- Nickell, S.J., 1998. Unemployment: Questions and some answers. *The Economic Journal* 108, 802–816.
- Oswald, A.J., 1996. A conjecture on the explanation for high unemployment in the industrialized nations: Part i. University of Warwick Working Paper No. 475.
- Oswald, A.J., 1999. The housing market and europe's unemployment: A non-technical paper. Available at <http://www.andrewoswald.com>.
- Scanlon, K., Whitehead, C., 2004. International Trends in Housing Tenure and Mortgage Finance. Council of Mortgage Lenders, London.
- Slemrod, J., Weber, C., Shan, H., 2017. The behavioral response to housing transfer taxes: Evidence from a notched change in DC policy. *Journal of Urban Economics* 100, 137–153.
- Stephens, M., 2008. The consumption response to predictable changes in discretionary income: Evidence from the repayment of vehicle loans. *Review of Economics and Statistics* 90 (2), 241–252.
- Stock, J.H., Yogo, M., 2002. Testing for weak instruments in linear IV regression. NBER Technical Working Paper No. 284.
- Wolf, N., Caruana-Galizia, P., 2015. Bombs, homes, and jobs: Revisiting the Oswald hypothesis for germany. *Economics Letters* 135, 65–68.