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# On the Economic Impacts of Constraining Second Home Investments

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**Key words:** Second homes; wealth inequality; land use regulation; housing policy; house prices; unemployment
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# On the Economic Impacts of Constraining Second Home Investments


## Abstract

We investigate how political backlash against wealthy second home investors in high natural amenity places affects local residents. We exploit a quasi-natural experiment: the 'Swiss Second Home Initiative', which banned the construction of new second homes in desirable seasonal tourist locations. Consistent with our model, we find that the ban substantially lowered (increased) the price growth of primary (second) homes and increased the unemployment growth rate in the affected areas. Our findings suggest that the negative effect on local economies dominated the positive amenity-preservation effect. We conclude that constraining second home construction in seasonal tourist locations where primary and second homes are not close substitutes may reinforce wealth inequality.



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# 1 Introduction

Over the last two decades, fueled by a staggering amount of wealth accumulation among a growing cohort of high earners, countries all over the world have seen a dramatic increase in wealthy individuals investing in 'second homes' – properties that are not used as primary residence – with a particular concentration in seasonal tourist locations and desirable (superstar) cities. This surge in second home investments has triggered a serious political backlash in many countries.[1]

In this paper, we explore the local housing and labor market impacts of one form of such political backlash: *constraints or outright bans on the construction of new second homes in seasonal tourist locations*. While in most countries far fewer people live and work in seasonal tourist locations than in superstar cities, when it comes to analyzing the market for second homes, the former locations are arguably economically at least as important as the latter.

Seasonal tourist locations rich in natural amenities differ from high-productivity superstar cities in two important respects that are relevant for both, our theoretical and empirical analysis. First, unlike in superstar cities, in seasonal tourist locations, the tourist sector is typically the dominant industry. Second, while in superstar cities primary and second homes tend to be close substitutes, in seasonal tourist locations this is usually not the case. For example, holiday homes at the beach often do not possess heating required for the winter season and wooden chalets in the mountains are in specific micro-locations, typically near ski lifts, and are of a style that is not suitable for year-round living.

To shed light on the mechanisms through which a constraint on second homes in seasonal tourist locations may affect local housing and labor markets, we develop a simple dynamic general equilibrium framework, where bans on second home investments have two opposing effects. They adversely affect local labor markets (negative 'local economy effect') but positively influence the primary residents' valuation of local amenities (positive 'local amenity effect').

We consider two alternative theoretical settings. The first assumes that primary and second homes are poor substitutes and therefore trade in separate markets. The model with this setting yields three empirically testable predictions. Constraining second home construction (i) negatively impacts the price of primary homes, (ii) adversely affects local labor markets, and (iii) increases the price growth of second homes in the constrained areas.

In contrast, the second setting assumes that the two types of homes are perfect substitutes. In this case, the price of existing primary and second homes must move in the same direction. Whether the direction is positive or negative is theoretically ambiguous.

[1] Countries that have implemented stringent policies to curb second home construction and/or investments include Australia, Canada, China, Denmark, France, Germany, Israel, New Zealand, Singapore, Switzerland, the United Kingdom, and the United States. We provide newspaper references documenting some second home policies implemented across the globe in Web-Appendix Table W-A1. We also note that resentment can turn into support in places that are confronted with severe house price busts. A case in point is Spain's Golden Visa program, introduced in 2013, after the collapse of its real estate market. The intention of the program has been to stimulate the housing market by attracting property investment into Spain through facilitating a path towards residency.

To empirically identify the local housing and labor market impacts of constraining the construction of new second homes, we exploit a unique quasi-natural experiment in Switzerland – the 'Second Home Initiative' (SHI). Voters narrowly approved this popular initiative in March 2012 and effectively banned the construction of new second homes in municipalities with a share of such homes of 20% or more.

Our empirical analysis builds on a standard difference-in-differences (DD) setting and addresses concerns of omitted variable bias and out-of-treatment selection by first-differencing the DD-equation and instrumenting the observed treatment assignment. Our preferred estimates suggest that the SHI-ban lowered price growth of primary homes in affected areas by 15%, increased the growth in local unemployment rates by 12%, and increased price growth of second homes by 26%. Our empirical findings for Switzerland are thus consistent with a theoretical setting where primary and second homes are poor substitutes.

Overall, our empirical findings imply that the adverse local labor market effects dominated any anticipated positive landscape preservation effects. In fact, we do not observe any significant positive sorting response from residents to the alleged benefits of the ban. Our results suggest that in seasonal tourist locations, like in Switzerland, where primary and second homes are not close substitutes, bans on the construction of second homes may reinforce rather than reduce wealth inequality.

Our paper relates to a relatively small but growing recent literature that focuses on the role played by residential real estate investors in housing markets. Haughwout *et al.* (2011) investigate the role of investors during the Great Financial Crisis in the United States, documenting that investors were heavily overrepresented in states that experienced the largest housing booms and busts. In a related study, Chinco and Mayer (2016) compare local second homebuyers to out-of-town investors. They find that out-of-town buyers – unlike local second homebuyers – behave as misinformed speculators, increasing future house prices and the implied-to-actual rent ratio. Finally, Bayer *et al.* (2020) classify investors into two categories according to their observed investment strategies: middlemen and speculators. The former group aims to make profit by buying from motivated sellers at prices below the market value and re-selling quickly, whereas the latter group times their investments to markets displaying strong price increases. By excluding the possibility that speculators possess superior information on housing price dynamics, they indirectly establish a causal link between speculative behavior and housing price bubbles.

A number of recent papers focus on international second home investments in superstar cities. Cvijanovic and Spaenjers (2020) explore the effect of international demand for luxury secondary residences in Paris. They point out how investors concentrate in specific areas, thereby increasing local housing prices. In line with Chinco and Mayer (2016), they find that foreign investors realize lower capital gains compared to local ones. Badarinza and Ramadorai (2018) focus on London and document how foreign real estate investors possess a "home bias abroad". They invest in areas displaying high shares of residents of the same country thus affecting housing prices and transaction volumes. In a similar vein, Sá (2016) finds that the volume-share of residential real estate investments in England and Wales performed by overseas companies increases house prices and decreases homeownership rates. Suher (2016) explores the response of non-resident owners of second homes in New York City to targeted annual property taxes. Using the city's 2013 change in the property tax treatment of condominiums, he documents that non-resident buyers have a significant impact on house prices within a subset of highly desirable neighborhoods, but no impact outside of these areas. Finally, Favilukis and Van Nieuwerburgh (2017) develop and calibrate a spatial equilibrium model for the New York and Vancouver metro areas to investigate the welfare effects of out-of-town homebuyers. Their findings suggest that higher levels of out-of-town buyers are associated with higher house prices and lower welfare. However, taxing purchases made by foreign investors may lead to welfare gains to the extent fiscal revenues are used to finance local public goods.

Studies on the economic impacts of restrictions on non-resident buyers are still rare and have so far focused on China. Somerville *et al.* (2020) document that purchase restrictions in China significantly reduced the housing transaction volume in restricted areas in the short run but that these effects diminished over time. Interestingly, they do not find any differential price effects between restricted and unrestricted areas. The underlying mechanisms that drive these results are quite different, however, from those proposed in this paper. This is because the institutional settings differ starkly. In China, unlike in Switzerland or other Western countries, land supply is determined by government-controlled land auctions.

Overall, the literature appears to support the widespread concern that non-resident investors into residential real estate increase local house prices and fuel market instability. This gives potential legitimacy to policies that aim to constrain non-resident real estate investments, either by imposing higher local taxes on non-primary owners or by constraining the quantity of such investments. To date, however, we know little about the economic effects of such investment constraints on local housing and labor market outcomes, and on the location decisions of primary residents, especially in Western advanced economies. This paper aims to fill this gap. In particular, our analysis considers mid- and long-term investors and does not exclusively focus on short-term speculators. The latter do not fully capture the significance of the global second home investment phenomenon.

The remainder of this article is structured as follows. Section 2 discusses the institutional setting and the specifics of the SHI. In section 3 we present the model and derive predictions for the empirical analysis. Section 4 discusses the data and provides descriptive statistics. We outline our empirical setup in Section 5 and present the main results and robustness checks in Section 6. The final section concludes.

## 2 Institutional background and the Second Home Initiative (SHI)

Popular initiatives like the SHI are an instrument of direct democracy that allows Swiss citizens to modify the country's constitution. Supporters of an initiative are required to collect 100'000 valid signatures in favor of the initiative within 18 months. In order to avoid undue influence of populous regions (in Switzerland called ‘cantons’ and ‘half-cantons’), the initiative must be approved by the majority of voters *and* cantons. Popular initiatives have a low approval rate: up to April 2015 only 22 out of 198 initiatives obtained dual majority. This is for two reasons. First, popular initiatives are often considered extreme and meant to send a signal to policy makers rather than being intended to actually modify the constitution. Second, authorities are allowed to formulate a more moderate counterproposal, often leading proponents to withdraw the initiative.

Supporters of the SHI, who argued a ban on the construction of new second homes is necessary to protect the natural landscape in tourist areas and prevent ghost towns, collected enough validated signatures by January 2008. The Federal Council, the Parliament, most of the political parties and economic organizations recommended voting against the initiative, mainly for economic reasons. Thus it came as a surprise when in March 2012 Swiss voters approved the SHI with the narrowest of margins; 50.6% of the votes and 13.5 (12 cantons and 3 half-cantons) of the 26 cantons (23 cantons and 6 half-cantons). Although voting polls suggested that a tight majority in favor of the initiative is feasible, its approval by the majority of cantons was a complete bolt from the blue.

On January 1, 2013, the SHI ordinance came into force, banning construction of new second homes in municipalities where such homes represented 20% or more of the total housing stock. The SHI stipulated that in the treated municipalities investors are not allowed to plan and build any new second homes going forward, though primary residences built prior to 2013 can still be converted into second homes. Fiscal authorities in Switzerland legally categorize all housing units as either ‘primary’ or ‘second’ homes depending on whether or not a household uses a housing unit as primary residence.[2] There is certainty about whether a unit is a primary residence because households only pay local income taxes in their primary place of residence (i.e., in the place where they live more than half of the year).[3]

Two elements of the ordinance are particularly relevant for our analysis. First, second homes that had obtained a construction permit prior to the vote were still allowed to be built after the ordinance came into force. This prevented the number of newly built second homes above the threshold to fall to zero in the years just after the approval of the initiative. Second, primary homes built – or possessing a construction permit issued – before the ordinance came into force (i.e., before 2013) may still be converted into second homes, but those planned and built after the ordinance was enacted lost their conversion option.[4]

[2] The second home status does not depend on the tenure (owner-occupied vs. renter-occupied) of the unit. Developers can still build rental properties – sometimes labelled ‘investment properties’ – post 2012 but, crucially, renter-occupiers must live in these new units permanently, not just during the tourist season.

[3] Cantonal inspectors can monitor an occupier’s presence in a second home. They can also conduct surprise visits for control purposes if they suspect misconduct. In a similar vein, in Israel authorities check the water usage of properties to determine whether an occupier may falsely claim to use a property as second home.

[4] Initially authorities confined the ‘conversion option’ to sales that did not trigger the construction of a new primary home in the treated or another nearby municipality. This measure intended to avoid speculative behavior of primary homeowners, thus limiting arbitrage strategies over the period of our analysis. However, the restriction was not included in the final law – implemented in January 2016 – because policy makers deemed it ineffective. This is allegedly for two reasons. First, mobile skilled individuals are likely to move over longer distances, so the restriction

Both elements of the ordinance were defined after the approval of the initiative, thus they were unknown to the voters prior to August 2012. Although the wording of the initiative had to be introduced into the Swiss constitution, implementation-specifics (and conformity with existing laws) were open to debate. In fact, the final text of a popular initiative is usually an arm-wrestled compromise between politicians supporting the initiative and those representing lobbies' interests. Therefore, the uncertainty concerning the specific implementation of the SHI made anticipation strategies extremely unlikely even after the voting results were known.

Treated areas in our setting – mountainous and other areas near lakes with shares of second homes above 20% – typically possess local economies that are reliant on tourism. A majority of voters in these areas, on balance, benefit substantially from the second home industry, directly or indirectly. It is therefore no surprise that the majority of local residents – especially in municipalities with very high shares of second homes and high homeownership rates – were strongly opposed to the SHI. The strong positive correlation between the SHI-share of no votes and the share of second homes in a municipality is illustrated in Figure 1.

In Appendix Table A1 we go one step further and present the results of a simple voting analysis, controlling for confounding factors, and reporting separate findings for the full sample of municipalities, the control and the treatment group. Focusing on treated tourist areas first, we find that – consistent with our main results – permanent local residents in the affected areas weighed the adverse economic effects of the SHI much more strongly than the anticipated positive effects highlighted forcefully by the supporters of the initiative. Permanent residents in treated areas were more strongly opposed to the SHI, the higher the share of second homes, the higher the homeownership rate, the closer a municipality to a major ski resort, and the higher the voter turnout.

Despite their strong opposition and turnout, however, voters in the treated areas did not succeed in preventing the approval of the SHI. This is because voters in populous and non-tourist control areas also had a say. A simple analysis of the voting behavior in these non-treated areas indicates that the overall support may have been mainly driven by envy motives of voters with little wealth: the higher the share of renters and the lower the income in a non-treated municipality, the stronger was the support in favor of the SHI. Moreover, perhaps driven by an 'existence value' associated with the preserved landscape, the further away voters lived from high amenity places, and therefore the higher the travel costs associated with a second home, the greater is the likelihood that they supported the SHI.

## 3 The model

In this section, we present a simple dynamic general equilibrium model in the spirit of Rosen (1979) and Roback (1982). We build on recent work by Glaeser and Gottlieb (2009) who provide

would not prevent them from moving away and pocketing the proceeds from the conversion option. Second, implementation (coordination across local jurisdictions) would have been very difficult and costly to monitor.

a general spatial equilibrium setting for the structural analysis of housing prices, wages, and population growth in the presence of agglomeration economies.[5]

We consider a system of local jurisdictions that differ in the quality of major natural amenities, such as mountains or lakes.[6] High quality amenities attract second home investors and increase the production efficiency of firms that exploit these amenities, leading local economies to exclusively specialize in the tourism sector.[7] Mobile workers choose their primary residence by sorting across local jurisdictions according to wages, housing prices, natural amenities, and the negative externalities caused by second home investors. Investors generate such externalities via adversely affecting the landscape and creating ghost towns.

One key assumption in our model is that primary and second homes trade in two distinct markets within each local jurisdiction, that is, the two markets have separate demand and supply functions. This implies that primary and second homes are *poor substitutes*. In section 3.6 we discuss the contrasting case of *perfect substitutability* along with predictions.

The assumption of poor substitutability is not far-fetched. It arises when second home investors and primary residents differ in their preferences for the micro-location within municipalities, the layout of a property, or the quality of construction. For example, second home investors tend to have strong preferences for nice views onto mountaintops, lakes or cityscapes or for quick access to ski lifts. These micro-locations are typically scarce. Vice versa, primary residents tend to strongly value good access to employment opportunities, local schools or supermarkets. Moreover, the layout of permanent homes often differs starkly from that of second homes. Differences in preferences for micro-locations and layouts, within municipality heterogeneity in locational access to amenities and services, and differences in the layouts of properties may thus effectively create separate markets. Strong wealth differentials between well-off second home investors and less well-off primary residents may further reinforce this market separation.

### *3.1 Tourism industry*

The local tourism industry produces non-tradable goods and services such as local ski lifts or food services that are sold to second home investors. We assume that residents in the municipality supply one unit of labor inelastically and we ignore cross-commuting, such that the number of local residents corresponds to local employment. Following Glaeser and Gottlieb (2009) and Hsieh and Moretti (2019), the output of firms is characterized by a Cobb-Douglas production function that displays decreasing returns to scale at the aggregate level:

$$Y_{it} = A_{it} N_{it}^{\beta} K_{it}^{\gamma} \bar{Z}_i^{1-\beta-\gamma}, \quad 0 < \beta, \gamma < 1, \ \beta + \gamma < 1 \qquad (1)$$

[5] Our theoretical framework also relates to recent work by Desmet and Rossi-Hansberg (2013), Gaubert (2018), and Hsieh and Moretti (2019).

[6] We briefly discuss the generalization of our framework to superstar cities in Section 6.5.

[7] In the interest of parsimony, we assume that the local economies of tourist locations solely consist of the tourism industry. A similar interpretation of the model would hold if construction were the sole industry. We refrain from interpreting the main local industry as being construction for two reasons. First, the construction industry is arguably not fully localized in tourist places. Second, the negative wage effect in the construction industry is likely of second order importance relative to the one in the tourism industry.

where $Y_{it}, A_{it}, N_{it},$ and $K_{it}$ represent output, total factor productivity, employment, and traded capital in municipality $i$ at time $t$, respectively; $\bar{Z}_i$ represents the municipality fixed stock of non-traded capital (e.g. land) that makes returns to scale decreasing at the municipality level but constant for individual firms. The industry is assumed to be perfectly competitive and firms choose the level of the factors of production to maximize their profits. Traded capital is supplied with infinite elasticity at an exogenous price set equal to 1. Labor and capital first order conditions lead to the labor demand equation:

$$N_{it} \propto A_{it}^{\frac{1}{1-\beta-\gamma}} p_{it}^{\frac{1}{1-\beta-\gamma}} W_{it}^{\frac{\gamma-1}{1-\beta-\gamma}}. \tag{2}$$

where $p_{it}$ and $W_{it}$ denote, respectively, the price of tourism services and the wages paid by the local tourism industry.

### 3.2 *Local residents*

Local residents are perfectly mobile and equalize their indirect Cobb-Douglas utility function

$$V_t = \theta_i N_{it}^{S\,\eta} \frac{W_{it}}{r_{it}^{a}}, \qquad 0 < a < 1,\ \theta_i > 0,\ \eta < 0 \tag{3}$$

across municipalities, where the term $\theta_i N_{it}^{S\,\eta}$ denotes an endogenous amenity index that decreases as the number of second home investors $N_{it}^{S}$ in the municipality increases. In our context, the factor $\theta_i$ reflects either the exogenously given value of natural amenities or the quality of the social life in the municipality. The value primary residents attach to this index evolves dynamically according to the negative externalities imposed by second home investors. The factor $\eta$ captures the extent to which local residents care about the disamenity caused by the presence of investors. The term $r_{it}$ represents the cost of local housing in the considered time period – i.e. the rental cost or the periodical cost of homeownership. The parameter $a$ is the constant expenditure share on housing.

### 3.3 *Second home investors*

Second home investors sort across municipalities to maximize their indirect Cobb-Douglas utility, which we assume depends on the optimal consumption of natural amenities, tourism services, and housing:

$$V_t^{S} = \theta_i^{S} N_{it}^{S\,\epsilon} \frac{W_t^{S}}{p_{it}^{1-b} r_{it}^{S\,b}}, \qquad 0 < b < 1,\ \theta_i^{S} > 0,\ \epsilon \leq 0, \tag{4}$$

where, similar to the case of primary residents, the amenity index $\theta_i^{S} N_{it}^{S\,\epsilon}$ reflects the potential dislike of an investor for the presence of other investors. (When $\epsilon$ is strictly negative, the endogenous amenity index could also be interpreted as congestion costs associated with the consumption of tourism services such as the use of ski lifts.) The terms $W_t^{S}$ and $r_{it}^{S}$ represent, respectively, the local second home market housing costs and the exogenous wages of second home investors that are determined outside our system of municipalities.[8] The parameter $b$ is the constant expenditure share on housing of second home investors.

[8] The wage $W_t^{S}$ can be thought of as the share of wage that investors spend in the place where their second home is

### 3.4 Housing developers

We describe the problem of developers of primary residences following Glaeser (2008).[9] Let us assume that in every municipality at an arbitrary point in time $t_0 < t$ there is a fixed supply of housing units $H_i C_{it_0}^{\rho_i}$ – where $H_i, \rho_i > 0$ are parameters affecting the supply elasticity – that can be built at a unitary cost of $C_{it_0}$ or less and sold at the market price $P_{it_0}$. Prices and heterogeneous construction costs are assumed to grow or shrink at steady-state rates $g_i$ and $g_i^c$, respectively, prior to the ban. Both rates are lower than the interest rate $r$. Profit maximizing developers choose the optimal period $t$ in which to develop and sell a property. The profit at $t_0$ of developing a plot of land is given by the discounted value of the future property price $P_{it} = (1+g)^{t-t_0} P_{it_0}$ less the discounted value of its future unit cost $C_{it} = (1+g^c)^{t-t_0} C_{it_0}$:

$$\max_t \left( (1+r)^{-(t-t_0)} \left( (1+g_i)^{t-t_0} P_{it_0} - (1+\ g_i^c)^{t-t_0} C_{it_0} \right) \right), \ \ t \geq t_0. \tag{5}$$

Marginal development in period $t$ occurs when the optimal stopping rule – obtained by setting the derivative of the continuous version of (5) equal to zero – is satisfied. Waiting to develop after the period implied by the stopping rule decreases the profit function of developers, thus harming them.

As we assume that primary ($\mathcal{P}$) and secondary ($\mathcal{S}$) residences are produced by two distinct supply functions, the housing supply of each type of residence is then given by

$$H_i^j \left( \frac{r-g_i^j}{\left(1+g_i^{j,c}\right)^{t-t_0} (r-g_i^{j,c})} P_{it}^j \right)^{\rho_i}, \ \ j \in \{\mathcal{P}, \mathcal{S}\}. \tag{6}$$

For ease of exposition, in what follows we only report the $\mathcal{S}$ superscript to distinguish second homes from primary ones.

We model a ban on second homes as the limiting case of an increase in the cost of producing such houses. By exogenously increasing $g_i^{\mathcal{S},c}$, the second home supply becomes more inelastic. If the increase in costs is large enough, the supply will become perfectly inelastic, which corresponds to a ban on second homes. Comparative static results based on the growth of construction costs of second homes thus correspond to those of a ban of such homes.

### 3.5 Equilibrium outcomes (when primary and second homes are traded in separate markets)

Having stated the problem of firms in the tourism sector, primary residents, second home investors, and housing developers, we can solve for the equilibrium solution of the system. To link the endogenous stock price of primary and secondary residences to the value of their housing flows, we use the standard dynamic price equation:

located. The wage $W_t^{\mathcal{S}}$ can easily be modified to incorporate ad hoc taxes targeting second home investors, which would shift their demand downwards. Adding such taxes, however, would require modelling the public good provision of local governments and/or the tax revenue redistribution from higher-tier political units, a task beyond the aim of the present framework.

[9] Developers of second homes solve a similar optimization problem. See the right-hand side of the market-clearing condition C5 in Web-Appendix C.1.

$$P_{it}^{j} = \sum_{l=0}^{+\infty} \frac{r_{it+l}^{j}}{(1+r)^{l}} = \frac{1+r}{r-g_{i}^{j}} r_{it}^{j}\ ,\ j \in \{\mathcal{P}, \mathcal{S}\}\ , \quad (7)$$

where we assume that rents grow at a steady state rate $g_i^j$. We can now define the concept of dynamic equilibrium:

**DEFINITION 1.** A dynamic equilibrium is a vector $(\frac{W_{it+1}}{W_{it}}, \frac{P_{it+1}}{P_{it}}, \frac{N_{it+1}}{N_{it}}, \frac{P_{it+1}^{S}}{P_{it}^{S}}, \frac{N_{it+1}^{S}}{N_{it}^{S}}, \frac{p_{it+1}}{p_{it}})$ such that for every municipality $i$ and every time period $t$:

i) Local labor markets clear according to equation (2).

ii) Primary residents and second home investors equalize their indirect utilities across municipalities according to equations (3) and (4), respectively.

iii) Housing markets of primary and secondary residences clear.

iv) The market of tourism services clears.

As the dynamic system of equations characterizing local economies can be linearized, we have

**COROLLARY 1.** There exists a unique dynamic equilibrium.

**Proof**. See Web-Appendix C.1.

We can use the dynamic equilibrium to make comparative static predictions about the impact of constraining the construction of new second homes (i.e. increase their construction costs) on the outcome variables of our model. Let $y_{it+1}^{0,j}$ and $y_{it+1}^{1,j}$ denote a given post-ban outcome variable if the ban would not have been/is enacted, respectively. We can express the average treatment effect on the treated as

$$E\left(\ln\left(y_{it+1}^{1,j}\right) - \ln\left(y_{it+1}^{0,j}\right) | D = 1\right) = E\left(\ln\left(\frac{y_{it+1}^{1,j}}{y_{it}^{j}}\right) - \ln\left(\frac{y_{it+1}^{0,j}}{y_{it}^{j}}\right) | D = 1\right),\quad j \in \{\mathcal{P}, \mathcal{S}\} \quad (8)$$

where $y_{it}^{j}$ denotes pre-ban outcomes and $D$ an observed treatment dummy variable equal to 1 if the municipality is subject to the ban and 0 otherwise. We obtain the following propositions for primary residents and second home investors, which we test in the empirical analysis below:

**PROPOSITION 1.** If primary and second homes are not substitutable, then constraining the construction of new second homes

i) reduces the price growth of primary homes,

ii) reduces wage growth, and

iii) has an ambiguous effect on the growth of the local population. The sign depends on the extent to which local residents dislike second home investors.

**Proof.** See Web-Appendix C.1 and Web-Appendix Table C1.

To understand the intuition behind Proposition 1, consider the effects of a constraint (or outright ban) on new second homes on the local landscape and the local economy. If local residents don't care much about the disamenity caused by the presence of investors ($\eta \approx 0$), the constraint hurts the local tourism industry without providing any benefit to primary residents, causing the growth in wages and the number of residents to be lower in the new equilibrium. This negatively impacts the aggregate housing demand for primary homes, leading to a negative equilibrium price effect.

Now consider the other extreme where local residents care a lot about the negative externality imposed by investors ($\eta \ll 0$). In this case, the predictions of Proposition 1 hinge on the decreasing returns to scale assumption, which would seem plausible for the local tourism industry. That is, the constraint can be expected to attract local residents into treated municipalities relative to the counterfactual (positive amenity effect). However, in a setting with decreasing returns to scale in the tourism industry, the constraint also reinforces the negative effect on local wage growth (deterring primary residents). In equilibrium, in our setting with decreasing returns to scale, the effect on local demand for primary homes and primary house prices is unambiguously negative, whereas the effect on the total number of primary residents is theoretically ambiguous.[10]

**PROPOSITION 2.** If primary and second homes are not substitutable, the average price growth effect on second homes of constraining their construction is positive.

**Proof**. See Web-Appendix C.1 and Appendix Table C1.

The intuition behind Proposition 2 is straightforward: A constraint (or outright ban) on new second homes makes supply more price inelastic, thus capitalizing future demand growth for second homes into comparatively higher equilibrium prices (and price growth). More inelastic supply also implies fewer second home investors and this in turn reduces demand for tourism services, lowering prices for such services.

Propositions 1 and 2 also have distributional implications, allowing us to speculate about the impact of constraining the construction of new second homes on local residents and, more generally, wealth inequality. Proposition 1 implies that constraining the construction of new second homes imposes a significant economic cost on local homeowners in the form of both, lower primary house price and wage growth, making local homeowners unambiguously worse off. Since prices are measured as the present value of imputed rents, constraining the construction of new second homes is also expected to lower future rent levels. But this does not mean that renters are better-off. This is because the fall in rents is commensurate to lower local wages. In a spatial equilibrium setting without relocation costs, renters should be neither better nor worse off. Proposition 2 implies that (typically wealthy) *existing* second home investors in treated locations should be better off as their investments become more valuable. Overall, these predicted distributional effects imply an increase in wealth inequality as a consequence of constraining the construction of new second homes, hurting local homeowners and favoring absentee second home investors.

[10] In Web-Appendix C.1, we explore whether Proposition 1 still holds when we instead assume agglomeration economies (increasing returns to scale) in the local tourism industry. We demonstrate that if agglomeration forces become very strong and exceed a certain threshold, a constraint on new second homes may increase the price growth of primary homes and wages. However, simulations – documented in Web-Appendix C.2 – suggest that such a threshold may be unrealistically high.

### *3.6 Equilibrium outcomes when primary and second homes are perfect substitutes*

In a setting where existing primary and second homes are *perfect substitutes* (both have a conversion option in both directions), the price of the two types must be the same and, by implication, the impact of the ban on the price must go in the same direction and must be of the same magnitude as well. Although the ban prevents the construction of new second homes, it does not prevent second home investors from entering the location. This is because existing primary residents have the valuable option to sell their property to second home investors and either move away or build a new – cheaper – primary home *without conversion option* at the outskirts of the location. Nevertheless, the expected growth rate of the number of second home investors should decrease post-ban. This is because eventually the municipality will run out of existing primary homes with a conversion option, at which point the ban puts an absolute upper limit on the number of second homes.

In our setting, if the expected growth rate of the number of new second home investors decreases, this has a negative feedback effect on local residents via the local labor market. Aggregate demand for housing in the local jurisdiction decreases, yet, at the same time, supply of second homes (or primary homes with a conversion option respectively) becomes more inelastic at the point in time of the ban. The net impact of these two opposing effects on the equilibrium price growth of houses with a conversion option is theoretically ambiguous.

In contrast to the separate market case, here primary homeowners retain a 'conversion option' to sell their property to second home investors post-ban. How valuable this option for existing owners is, depends on their moving costs. In the extreme of 'excessively high moving costs' the option to convert is worthless. However, in reality the option may at least partially hedge primary homeowners against the adverse effects on the local economy. Put differently, ignoring moving costs, primary homeowners may not be worse off compared to existing second home investors.

Interestingly, from a policy point of view, in a setting with perfect substitutability, banning second homes is likely to reinforce some of the key concerns of the policy it is supposed to tackle: The ban reduces the willingness-to-pay for housing of local residents due to the adverse effect on local wages. The ban thus creates incentives for primary homeowners to sell their properties to second home investors, whose willingness-to-pay has not changed post-ban. Some primary residents may sell and move away, which would mean that the share of second home investors relative to the total local population rises and the 'ghost town' problem worsens. Some primary residents may sell their homes in the most desirable micro-locations and purchase newly constructed primary dwellings that do not have a conversion option at the outskirts of the location, in effect creating a new separate market of 'properties without a conversion option' for primary residents. To the extent that existing primary homes are clustered mainly in the center of municipalities and new primary homes have to be built at the outskirts, this could reduce social cohesion and may even increase sprawl – because a ban on second homes does not prevent construction of primary homes at the outskirts.

# 4 Data and descriptive statistics

We combine housing data provided by the Swiss Real Estate Datapool Association (SRED) with municipality-level data from various sources discussed below.[11]

### *4.1 Data sources and variables*

**Housing transaction data** — The SRED collects and pools transaction data from various mortgage lenders – both private and cantonal banks. The SRED provided us data on individual transaction prices and corresponding housing characteristics for all of Switzerland and from 2000q1 to 2015q1. For each housing unit, in addition to the transaction price, we know whether the buyer intends to use the unit as primary or secondary residence, the physical characteristics of the unit (number of rooms, number of bathrooms, number of parking places, micro-location quality, housing unit quality, housing condition, construction year, and an indicator of whether the unit is a single-family house or an apartment) and the unit's location (municipal and cantonal identification codes).

**Unemployment and wage data** — We use yearly data on unemployment at municipality level pre and post approval of the SHI provided by the State Secretariat for Economic Affairs (SECO).[12] Our measure of local unemployment is the number of unemployed individuals in a municipality divided by its total population. We use total population as denominator rather than total employment, as the latter is not available at municipality level. As a consequence, our 'unemployment rate' measure is lower than that published by official sources for more aggregate geographical levels. Average yearly wages of employees at the municipality level have been computed by merging the Population and Household Statistics of the Swiss Federal Statistical Office (FSO) with social-security data provided by the Central Compensation Office (CCO).

**Second home rates** — We obtained the municipality-level second home rate from the Swiss Federal Office for Spatial Development (ARE). Using data from the Federal Register of Buildings and Dwellings of 2012, ARE computes the number of second homes per municipality as the total housing stock less the number of primary homes. Second home rates are thus fixed over the period of our analysis, although some municipalities – upon request – were allowed to revise their rates downwards. We use the second home rates after revisions were taken into account to compute the observed treatment dummy, which equals one if a municipality's second home rate is greater or equal than 20%, and takes value zero if the municipality is below the 20% threshold or asked for a revision. Additionally, we use ('historic') second home rates provided by the 2000 Federal Population Census as an instrument for second home rates in 2012.

**Fiscal data** — Fiscal data at municipality level comes from the Swiss Federal Tax Administration (FTA). In our analysis, we use the pre-policy municipality average net income after taxes, the municipality's Gini index based on the same underlying income measure, and the predetermined share of foreign residents in the municipality represented by foreign individuals paying local

[11] We provide more detail on the sources and data in Web-Appendix D.
[12] Unemployment data by industry is not available at the municipality level.

taxes. We note that predetermined values of these variables reflect not only the fiscal status of the municipality, but may also capture a social amenity value: households may prefer to live in a municipality whose residents share a similar socio-economic background as their own.

**Other municipality characteristics**—The Federal Population Census provided by the FSO offers data on the number of residents and its age structure at the municipality level from 2010. We use the number of local residents over 65 years – thus not working anymore according to the Swiss mandatory retirement age – as an additional outcome variable to measure the amenity effect (we provide a rationale for this in Section 6.4). To proxy for time-invariant local natural amenities, we use the time-invariant share of undevelopable land – including lakes, glaciers, and bedrock – provided by land use data sourced from the FSO. Geographical Information System (GIS) data on the boundaries of administrative units at national, cantonal, and municipal level comes from the Federal Office of Topography (Swisstopo). GIS data allows us to compute the distance of each municipality from 15 major Swiss urban centers and 53 major ski resorts. These two measures capture how households value the proximity to major labor markets and labor markets linked to the tourist industry in high natural amenity places, respectively. We collected data from the FSO on the number of workers and firms active in the service sector as measured in 2011. From the Housing Construction Statistic published by the FSO we collected the number of newly constructed residences from 2008 to 2014. This latter variable allows us to investigate the impact of the SHI on the local residential real estate sector.

### *4.2 Descriptive statistics of control and treated municipalities*

For the purpose of our regression analysis, we aggregate the data at the municipality level and compute two-year averages for the pre-ban (2010-2011) and the post-ban (2013-2014) period. We consider an additional pre-period (2008-2009) to include lagged controls. Computing two-year averages allows us to increase the number of transactions observed in a given municipality and to include a greater number of municipalities in our sample. In our less restrictive specifications we retain approximately 60% of all Swiss municipalities.[13] We provide summary statistics in Tables 1A (treatment group) and 1B (control group) for the pre (2010-2011) and post (2013-2014) SHI-approval periods.

Because there was great uncertainty concerning the practical application of the initiative until August 2012, individuals may or may not have anticipated its effects during this year despite the ordinance not being in force, making its evaluation difficult. In our empirical analysis, we thus drop 2012 observations from our sample. Finally, in order to compare only primary homes that possess a conversion option before and after the SHI-approval (i.e., to compare 'like with like'), we drop primary residences *built after 2012* from our sample when investigating primary house price dynamics.

A comparison of Tables 1A and 1B reveals that the threshold imposed by the initiative broadly divides mountainous locations (treatment) from areas with major urban centers (control). Below

[13] We excluded new municipalities that were created from mergers of existing municipalities during the post-ban period from our analysis.

the threshold, municipalities are nearer to major urban centers and more distant to major ski resorts. Control municipalities thus have – on average – a larger population, more newly constructed housing units, and higher wages. Elderly people are more prone to live in municipalities belonging to the control group, likely due to better access to healthcare services. The percentage of individuals and firms active in the service sector is similar for the two groups, suggesting that local economies in treated places mostly rely on tourism and that agriculture may only play a marginal role. Interestingly, we do not observe any marked difference in unemployment rates between treatment and control municipalities.

Figure 2 illustrates the geographic distribution of treated municipalities: most of them are situated in or near the Alps, further supporting our claim that for these municipalities the tourist industry is the main pillar of their local economies, consistent with our model. Given this proximity to the Alps, treated municipalities have more natural amenities, as measured by the share of unproductive surface, compared to the control group.

Focusing next on the housing stock and house prices, Tables 1A and 1B reveal that treated municipalities have lower average house prices, both before and after the approval of the initiative. House prices are lower in treated municipalities in part because they are further from major urban areas, but in part also because of lower housing quality.

Figure 3 depicts pre-trends of our three main outcome variables – the log price of primary and second homes and local unemployment rates – providing visual support for the common trend hypothesis. We compute bi-annual averages of the three measures pre and post approval of the SHI, consistent with the bi-annual averages we use in our empirical-analysis (outlined below). While all three outcome variables display similar pre-trends, consistent with our theoretical priors, post acceptance of the SHI the trends of the treatment and control group go in opposite directions. In Section 6, we test more formally for differences in the pre-trends of the main outcome variables.

Two remaining points are worth noting. First, as illustrated in Figure 4, the SHI did not noticeably affect the pattern of primary housing transactions with respect to second home rates: primary homes are mainly transacted in and nearby major urban centers, which typically possess second home rates between 10% and 15%. Similarly, very little of the second home demand from the above-20%-municipalities appears to have shifted to control municipalities just below the 20% threshold. Consistent with this, Tables 1A and 1B show that the average number of transacted primary homes has not been significantly affected by the policy in treated municipalities. Second, the threshold imposed by the SHI is situated at the tail of the second home rate distribution, making sample restrictions around the threshold extremely challenging.[14]

## 5 Empirical research design

Let $y_{i10-11}$ and $y_{i13-14}$ denote the outcome variable in municipality $i$ in 2010-2011 (pre-period) and 2013-2014 (post-period), respectively. Focusing on the two years directly following the

[14] See Web-Appendix Figure W-B1 for an illustration of this point.

approval of the SHI allows us to empirically identify theoretical mechanisms of the ban that might disappear in the longer run.[15]

To empirically test our model predictions, we consider three main outcome variables: the local price of primary and second homes as well as the local unemployment rate (in Section 6.4 we investigate additional outcome measures). We start by estimating the following two-period difference-in-differences (DD) model:

$$\ln(y_{it}) = \alpha + \gamma D_i + \tau d_t + \delta\, d_t \times D_i + \boldsymbol{\beta}_1 \boldsymbol{x}_{it-1} + \boldsymbol{\beta}_2 \boldsymbol{c}_i + u_{it}, \quad (9)$$

where $D_i$ represents the observed treatment assignment defined according to the second home rate $sr_i$ (after revisions were taken into account), $d_t$ is a time dummy equal to 1 for post-initiative observations and zero otherwise, $\boldsymbol{x}_{it-1}$ is a vector of pre-determined covariates including information on local housing markets and fiscal variables, and $\boldsymbol{c}_i$ is a vector of time-invariant variables that captures locational and geographic features of the municipality, including canton fixed effects. The variable $u_{it}$ is a stochastic error term.

Unbiased estimation of the coefficient of interest $\delta$ is obtained if $E(u_{it}|sr_i) = 0$. Two main sources of endogeneity may invalidate this assumption in our setting, namely omitted variable bias and out-of-treatment selection. To partially address the former, in a first step we partial out unobserved municipality heterogeneity by estimating the following first-difference (FD) model:

$$\Delta\ln(y_{i13-14}) = \tau + \delta D_i + \boldsymbol{\beta}_1 \Delta\boldsymbol{x}_{i10-11} + \Delta u_{i13-14}, \quad (10)$$

where the outcome variable is given by $\Delta\ln(\mathrm{y}_{i13-14}) = \ln(y_{i13-14}) - \ln(y_{i10-11})$, the term $\Delta\boldsymbol{x}_{i10-11} = \boldsymbol{x}_{i10-11} - \boldsymbol{x}_{i08-09}$ captures pre-determined dynamics, and $\Delta u_{i13-14} = u_{i13-14} - u_{i10-11}$ denotes contemporaneous unobserved dynamics.

To address the latter, in a second step we rely on an instrumental variable (IV) approach and estimate model (10) by 2SLS (FD-IV). More precisely, we instrument the observed treatment assignment as

$$D_i = \gamma_0 + \pi z_{i00} + \boldsymbol{\gamma}_1 \Delta\boldsymbol{x}_{i10-11} + \mathrm{v}_i, \quad (11)$$

where the instrument $z_{i00}$ is given by the second home rate as measured in the 2000 Federal Population Census. This 'historic' measure of second home rates is strongly correlated with the observed treatment dummy – making it a relevant instrument – and could not have been manipulated by municipalities according to the treatment assignment, thus removing endogeneity issues linked to out-of-treatment selection.

The 2SLS estimate of the treatment effect is thus consistent if $E(\Delta u_{i13-14}|z_{i00}) = 0$ and if the instrument affects outcome variables only through the first-stage equation (11). These two conditions may not be satisfied if the instrument captures permanent differences in the unobserved outcome dynamics between the control and treatment group after the effect of other control variables has been partialled out. In fact, we might worry that short-term outcome

[15] For example, one might expect the positive impact of the SHI on unemployment rates in treated areas to decrease over time, as local residents may move to non-treated regions to access better employment opportunities.

dynamics of major CBDs and suburban areas (which usually have low historical second home rates) differ from those of tourist areas (which have high historic second home rates).

To partially solve this problem, we examine the robustness of our treatment estimates when we include the natural log of the pre-determined outcome variable $y_{i10-11}$ among our controls in the FD and FD-IV models ($d_t \cdot \ln(y_{i10-11})$ in the case of the DD model). This variable allows us to control for pre-policy differences in outcome *levels*, likely making the direct effect of 'historic' second home rates on short-term outcome dynamics irrelevant. For example, municipalities with high initial levels of house prices or unemployment rates – such as CBDs – might have outcome dynamics that differ from those with low initial levels. This approach also allows us to control for mean reversion in the outcome variables.

We further investigate the robustness of our FD-IV estimates by balancing treatment and control group. Specifically, we drop municipalities near major CBDs and highly touristic places from our sample. We employ two strategies. The first relies on directly excluding those municipalities situated within a 10 km radius from major CBDs and those adjacent to a major ski resort. The second follows Greenstone and Gallagher (2008) and is akin to a fuzzy regression discontinuity design: We drop municipalities within a 10 km radius from major CBDs while restricting the sample to municipalities that have a second home rate between 15 and 30%.[16] To the extent that dynamic unobservables are balanced in our restricted samples – Altonji *et al.* (2005) suggest that balancing according to observed covariates may indeed reduce omitted variable bias – the two approaches provide consistent estimates of the treatment effect, even when the instrument is not exogenous for the whole sample, i.e. even when $E(\Delta u_{i13-14}|z_{i00}) \neq 0$. Additionally, the exclusion restriction is likely satisfied for the restricted samples, as permanent differences between control and treatment group have been removed. The two approaches are data demanding – the sample size is considerably reduced – which translates into a higher variance of the estimated treatment effect.

# 6 Results

## *6.1 Main results: Impact of ban on price of primary homes and local unemployment*

In Panel A of Table 2 we report treatment effects estimates of Equation 10 using the FD-IV approach outlined in the previous section.[17] To test the predictions of our theoretical model, we consider the price of primary homes (columns 1-3) and unemployment rates[18] (columns 4-6). For each of these two outcome variables, we progressively increase the set of controls. The FD-IV approach allows us to partially address endogeneity concerns related to potential omitted variable

[16] We combine a sample restriction based on second home rates with CBD exclusion because some major urban areas in the control group – such as Geneva and Bern – have second home rates in the narrow band of 15%-20% below the threshold set by the SHI.

[17] We report heteroscedasticity-robust standard errors. Clustering standard errors by cantons – which are the "most aggregate" institutional entities in Switzerland – does not alter the statistical significance of our main results. See Web-Appendix Table W-E1. However, standard errors may not be reliable due to the small number of clusters.

[18] We report wage results, as well as results for other outcome variables, separately in Section 6.4. We motivate our focus on unemployment rates to capture the negative local economy effect with the fact that in Switzerland wages are extremely sticky downwards.

bias and out-of-treatment selection. This is our preferred approach to evaluate the impact of the SHI on local residents and its estimates are used as benchmark in subsequent robustness checks.

The FD-IV estimates suggest, consistent with Proposition 1, a strong negative impact of the second home ban on the price growth of primary homes: on average, the SHI lowered the price growth of primary homes by about 15% (preferred estimate reported in column (2)). To give an idea of the magnitude of this effect in levels, this equates to about 12% lower house prices over a 20 year horizon.[19] The estimated average treatment effect is highly significant, independent of the set of included controls. The stability of the treatment estimates to the inclusion of the pre-determined outcome level suggests that pre-policy differences in the price of primary homes do not strongly affect post-policy price dynamics.

Table 2 (columns 4-6) further reveals that the SHI increased the unemployment growth rate by about 12% in the treated compared to the control areas (preferred specification reported in column (5)). The results are strongly statistically significant and remain extremely stable to the inclusion of additional controls, as in the case of the price of primary homes. Remarkably, pre-existing patterns of the outcome variable hardly affect the magnitude of the treatment estimates.

First stage coefficients of our instrument have the expected sign, denoting a strong and highly significant relationship between 'historic' second home rates and those measured more than a decade later. The Kleibergen-Paap F statistics are extremely high for all specifications, suggesting that weak identification is not a problem in any of the estimated specifications.

To verify that no treatment effect was present before the policy implementation, we conduct a (placebo-)pre-trend analysis for the periods immediately pre-dating the SHI approval. Specifically, we use the years 2006-2007 and 2008-2009 as pre-policy periods, and 2010-2011 as post-policy period. We report the corresponding estimation results in Panel B of Table 2. The (placebo-)treatment effect is statistically insignificant and close to zero for both primary home prices and unemployment rates. First-stage results are unchanged.

The fact that pre-ban outcome dynamics are not different, adds further credibility to our main FD-IV estimates, as 'historic' second home rates do not appear to capture permanent differences between treatment and control group through the first-stage equation. Put differently, if 'historic' second home rates were simply dividing major CBDs from highly touristic places through the treatment assignment, and these areas have permanently different outcome dynamics, then the pre-ban treatment effect should be significant. This, however, is not the case.

### *6.2 Main results: Impact of ban on price of second homes*

Another pertinent question is whether the SHI positively affected the price growth of second homes (Proposition 2). Only a small percentage of second homes are traded below the threshold set by the SHI and these are traded only in a small number of control municipalities. This lack of

[19] House prices grew roughly 4% annually during the 10 years preceding the SHI. Using this number as a benchmark, our preferred estimate implies that post SHI-approval and as a direct consequence of the ban, going forward primary house prices grew 0.6% percentage points less annually. This equates to around 12% lower primary house prices in 20 years from the approval, compared to the counterfactual scenario without a ban.

data makes estimating the treatment effect on second homes extremely challenging. In particular, we cannot reliably estimate FD and FD-IV models because very few municipalities are present in the control group in these samples.[20] These caveats aside, in an attempt to nevertheless shed some light on the impact of the SHI on the price growth of second homes, we estimate a DD model as in equation (9), but to increase sample size, we do not restrict the sample to municipalities for which housing transactions were observed both before and after the SHI ordinance came into force. We report results in Table 3 (Panel A). The sign of the treatment effect is positive and fairly stable across specifications. Once controls are included in the model, the effect becomes statistically significant, although only weakly so.

This finding is consistent with our theoretical model that assumes poor substitutability between primary and second homes. This should not be too surprising in the case of Switzerland's tourist areas. Second homes are usually located where access to ski resorts is easiest, are built using specific materials – wood-built chalets – and usually lack some of the comforts of primary residences, such as access to broadband connection and covered parking garages. Additionally, it may be that primary homes that were good substitutes for second homes were already converted into second homes in the past, leaving only properties without conversion potential in the stock of primary residences.

Another possible explanation is that post SHI-implementation, primary residences that retained a conversion option systematically dropped out from our sample – as they were sold as second homes – thus causing a selection bias. This seems unlikely for two reasons. First, primary homes built before 2012 do retain a conversion option. If they are systematically sold as second homes, it means that potential primary residents prefer to buy properties that do not have a conversion option, an unlikely case. Second, if primary residences that have a conversion option are systematically converted post policy, we should observe a significant drop in the number of transacted primary residences in treated municipalities, and this did not happen (see Figure 4).[21]

As in the case of the price of primary homes and unemployment rates, we also conduct a (placebo-)pre-trend analysis for the periods immediately pre-dating the SHI approval. Panel B of Table 3 shows that the estimated (placebo-)treatment effect is statistically insignificant across all specifications.

### *6.3 Results for alternative identification strategies and robustness checks*

Table 4 summarizes the results for alternative strategies of identifying the impact of the SHI on the price of primary homes and local unemployment as well as some additional robustness checks.[22] In Panel A of Table 4 we replicate our main specifications from Table 2, but employ a

[20] Even in the less restrictive FD specification, estimates become erratic when including predetermined controls.

[21] Municipalities had to ascertain that the conversion of primary residences into secondary ones was not driven purely by speculative motives. For example, primary homeowners were not allowed to convert their residence and directly build/buy a new one in the same (or nearby) municipality.

[22] The Web-Appendix Tables W-E2, W-E3 and W-E5 to W-E8 provide detailed estimation results. Additionally, in Web-Appendix F we report further robustness checks and results, which include investigating the parallel trend assumption over older time-periods (Table W-F1 to W-F3), controlling for second home rate polynomials (Table W-F4 and W-F5), and the estimation of heterogeneous treatment effects (Table W-F6).

standard DD and FD estimator, respectively, instead of our FD-IV approach. The estimated effects for the price of primary homes are virtually identical to our main specifications. The estimates for local unemployment rates are qualitatively similar, but somewhat smaller in magnitude and statistically less significant. The fact that the FD results for the price of primary homes are quite similar to our main results, reported in Table 2, implies that municipalities may not have made use of the option to revise their second home rate endogenously according to local housing market conditions.

Panel B of Table 4 reports results for a number of additional checks. To begin with, one concern with our FD-IV estimates is that they might be affected by intrinsic differences between treatment and control group. To the extent that our "historic" instrument captures persistent differences between the two groups – which in turn correlate with short-term dynamics – treatment effect estimates may not be consistent. To mitigate this concern, we balance observed covariates in the treatment and control group. We use two alternative sample restrictions. The first drops municipalities situated within a 10 km radius from major CBDs and/or adjacent to a major ski resort (Restricted Sample 1). The second excludes municipalities within a 10 km radius from major CBDs and/or having a second home rate below 15 or above 30% (Restricted Sample 2).[23]

Dropping major CBDs and highly touristic places makes the negative impact of the initiative on the price growth of primary homes somewhat stronger, with estimates ranging from 17-24%. The impact on unemployment growth becomes slightly less pronounced (between 9-10% increase compared to around 12% in our preferred specification reported in column (5) of Table 2). The even stricter sample restriction further amplifies the negative effect of the ban on the price growth of primary homes and the positive effect on the unemployment growth rate. Both effects are highly statistically significant. We interpret the magnitude of the estimated effects in the most stringent sample restriction with due caution, however, as the sample size – and in particular the number of municipalities belonging to the treatment group – becomes very low, thus considerably increasing the variance of our estimates.

To further verify the robustness of our estimates to potential sorting effects, we estimate the FD-IV model for the price of primary homes and the local unemployment rate when we use as control group municipalities situated more than 5 kilometers away from the nearest treated ones (see Figure 2 for a visual representation of dropped municipalities). Excluding municipalities near treated ones allows us to exclude those places where households and investors are most likely to sort into, according to the incentives created by the initiative. For example, households may move to the nearest municipality not affected by the ban to find a job. Similarly, second home investors may shift their housing demand to those non-restricted municipalities in closest proximity to

[23] Web-Appendix Table W-E4 shows that these two sample restrictions balance treatment and control group. Of course, balancing observable covariates does not ensure that unobservable ones are balanced, however, it likely reduces considerably the bias coming from omitted variables (Altonji *et al.* 2005). Additionally, as pointed out by Greenstone and Gallagher (2008), balancing covariates renders the (linear) functional-form assumption between an outcome variable and the covariates irrelevant.

major natural amenities. Reassuringly, the estimated impacts are virtually identical to our baseline estimates reported in Table 2.[24]

We explain the absence of sorting of households across municipalities as follows. First, as argued by Glaeser and Gottlieb (2009), sorting of individuals in response to economic incentives is likely to occur in the long-run. As our analysis takes place right after the implementation of the SHI ordinance, sorting mechanisms may simply not have had enough time yet to materialize. Second, local residents may not consider second home investors a disamenity, which would eliminate any localized positive effect of the ban. The voting results in Appendix Table A1 support this view.[25]

Third, the SHI reinforced the price differential of primary residences located in control and treated municipalities. This implies lower asset values for primary homeowners in treated locations post-ban and suggests that they may no longer have had sufficient wealth to buy a similar property in a control-location.[26] Fourth, the entire second home demand in municipalities that did not exceed the threshold is very small (less than 0.5% of the total transactions of primary residences), thus hardly affecting local price growth of primary homes in non-treated areas. Fifth, investors may value the close proximity to amenities – such as ski resorts – and would rather invest in a neighboring country (e.g. Austria or France) than losing the benefit of this proximity (i.e., even nearby municipalities may not be sufficiently close substitutes).

The final row in Panel B of Table 4 reports results for the effect of the ban on the price growth of primary homes using a sample that includes primary homes built after 2012. In our main specifications, reported in Table 2, we dropped these observations because our aim is to compare ‘like with like’ housing units pre and post ban (and primary homes built after 2012, in contrast to those built earlier, no longer possess a conversion option). Including primary homes built after the ban, allows us to estimate the ‘total’ effect of the ban – the sum of a compositional effect (properties without a conversion option may be traded post ban) and a direct effect (i.e., the effect we are primarily interested in). The results reveal that the ‘total’ effect is similar to our main results reported in Table 2, suggesting that the compositional effect may not be important quantitatively.

[24] The choice of a 5 km distance band is arbitrary. In a further robustness check, we thus vary the distance band continuously to document that the estimated effects of our FD-IV specifications are robust to the choice of the distance. The results are illustrated in Web-Appendix Figure W-B2. The estimates are extremely stable over a wide range of distance bands used to exclude the nearest-to-treated control municipalities, providing further evidence that the potential spatial sorting of individuals across municipalities is not relevant in our setup. These results suggest that the demand of second home investors may not have shifted from treated- to control-municipalities post-SHI but, instead, the fixed shares of income that ‘marginal’ investors spent for second homes and tourism services pre-SHI may have shifted to a reservation locale outside Switzerland post-SHI, consistent with our theoretical framework.

[25] The voting results are indicative that the SHI was approved at least in part for social envy reasons of primary residents in non-affected (largely urban) areas, although landscape preservation-considerations might also have played a role to swing the decision of voters in these areas.

[26] The scenario in which homeowners sell their properties to become renters in non-restricted municipalities seems highly unlikely.

### *6.4 Impact of ban on other outcomes*

In Table 5 we report the FD-IV estimates of the impact of the SHI on several additional outcome variables: new residential construction, number of elderly, population size and wages (all measures are in logs and first differenced).[27]

First, we explore the impact of the ban on residential construction in the treated municipalities. As expected, the impact on new construction is negative and statistically significant. The effect is also economically meaningful, with the ban reducing residential construction growth by between 19 and 23 percent, depending on the specification. This is despite the fact that several residential projects were approved prior to the SHI and therefore had permission to go ahead during the post-period (2013-2014). To the extent that the local construction industry employs local residents and is more strongly adversely affected in the longer run, our unemployment results thus provide a conservative estimate of the negative impact of the ban on local economies.[28]

Our second outcome measure is the number of elderly. We focus on the elderly, as their mobility decisions can be expected to be affected by local amenities in the treated areas rather than by the local labor market conditions. If the SHI had a positive amenity effect, we would expect more elderly to move to the treated locations, all else equal. Table 5 reveals however that the impact of the SHI on the sorting behavior of elderly remains insignificant and close to zero. This may be for two reasons. First, sorting of the elderly likely depends on factors not measured by our controls, such as family ties (making relocation particularly costly) and access to healthcare services. Second, a positive amenity effect may not materialize for a few years to come. This is because the ban did not apply to already approved second home projects and construction of these projects takes time. However, if the ban on second homes was indeed perceived to positively affect the landscape in the medium and longer run, one would expect that the elderly move to the treated areas in anticipation of this effect and that this should be reflected in higher house prices, all else equal, at least partially offsetting the negative economy effect. Given that our overall effect of the SHI on the price of primary homes is negative is thus indicative that, locally, the negative economy effect outweighs any potential positive amenity effects.

Our findings so far are indicative that sorting may not be of primary importance in our empirical setting. In a next step, we test more formally whether sorting of households occurred, by estimating the effect of the SHI on the growth of the resident population. The coefficient of the treatment dummy is statistically insignificant and close to zero in magnitude in all specifications, providing further support for the view that there was no noticeable sorting in response to the SHI.

Our last alternative outcome measure is local wages. The results reported in Table 2 strongly suggest that the SHI negatively affects local economies of treated municipalities by increasing

[27] We provide detailed estimation results, including first stage results and results for the Restricted Samples 1 and 2 (discussed in Section 6.3), in Web-Appendix Tables W-E9 to W-E12.

[28] We note however, that the estimated effect on new construction becomes statistically insignificant when we progressively balance the sample. See Web-Appendix Table W-E9. The finding of an adverse short-term effect on construction should therefore be interpreted with some caution.

the local unemployment growth rate. This finding is consistent with a setting where wages are sticky downwards. In our theoretical framework, however, we assume that wages are flexible, thus predicting a negative impact of the ban on local wage growth. As we document in Table 5, however, the ban does not significantly affect wage growth once pre-trends in wages are accounted for.[29] Our wage results seem sensible in the context of the Swiss institutional setting. This is for two reasons. First, it is extremely uncommon for employers, due to de facto 'upward-only' wage adjustments at industry level, to be able to renegotiate wages for existing workers downwards. Second, by international standards Switzerland has one of the most liberal labor laws. For example, employers can terminate an employment relationship lasting ten years (or more) by giving a three months' notice and without providing any justification for it. Thus, to counter an unexpected negative shock to the local economy, it would appear to be much easier for firms to fire workers or not rehire certain seasonal workers rather than to lower wages.

### *6.5 Contextualization of results*

The upside of our empirical analysis is a clean quasi-natural setting that allows us to rigorously study the impact of a ban on the construction of new second homes. Our findings are, however, to some extent context-specific.

While in seasonal tourist locations like ski or beach resorts, primary and second homes are often poor substitutes as in the Swiss setting, there are many tourist places where the two types of properties are close substitutes. In these latter locations, we would expect the price of primary and second homes to move in the same direction and the option to convert an existing primary into a second home to be valuable.

Anecdotal evidence supporting this assertion stems from a ban on the construction of second homes that was introduced in St. Ives and a few other smaller British seaside towns in 2016. Interestingly, this ban was approved by local voters. Data on transaction prices suggests that the ban in St. Ives caused the demand of second home investors to shift from newly built to existing homes, thereby intensifying the seasonal ghost town character. This shift drove up the price of existing homes, slashing construction levels and the price of newly built homes, adversely affecting local tourism and construction businesses (Economist 2019). The only potential beneficiaries of the ban have been already existing owners of housing in St. Ives, including many retirees who welcome landscape preservation effects but may care little about the local labor market. Young would-be buyers, lower income renters and the local workforce in the tourism and construction sectors are the ones who lose out.

We would also expect the effects of a ban to be different in *superstar cities* such as London or New York, where labor markets are much more diversified and less dependent on buyers of second homes. The negative effects of a ban on the local economy may therefore be more muted. The price effects would again depend on the degree of substitutability of primary and second

[29] Somewhat surprisingly the coefficient of the treatment dummy is positive, albeit statistically insignificant in the most rigorous specification reported in column (3). Reassuringly, the statistical significance further deteriorates as we balance the treatment and control group. In fact, the impact of the ban becomes negative for the specification with the strictest sample restriction. See Web-Appendix Table W-E12 for details.

homes. If the two types of housing are close substitutes, then demand of investors should shift from newly built to existing homes, further accentuating the housing affordability crises in superstar cities (although this effect may not be very important quantitatively). In contrast, in the case of poor substitutability, a ban may somewhat dampen the upward pressure on housing rents and primary house prices. Lower housing costs compared to the counterfactual in turn may attract more labor to superstar cities. In the presence of agglomeration externalities, this may raise local wages in non-tourism industries and may lead to an increase in the aggregate productivity, as in Hsieh and Moretti (2019).

Finally, the overall distributional impact of a ban depends crucially on who owns real estate assets in the affected areas. Second home owners may be foreign investors, domestic 'out-of-town' buyers, or, in fact, local residents who possess a second home in their own municipality that they rent out during holiday seasons only (if a property is rented out on a permanent basis, it is not classified as a 'second home'). In the case of Switzerland, it is quite rare that local residents possess vacation homes locally. Rather, wealthy local residents tend to own undeveloped land locally or they rent out on a permanent basis. In both cases, they will be negatively affected by the ban due to the adverse effect of the ban on the market for primary homes and, by implication, the market for undeveloped land (as the ban removes the option to build second homes). Thus, in Switzerland, most local homeowners in treated areas are likely worse off. However, this does not necessarily apply to other countries and settings.

# 7 Conclusion

Rising inequality has led to a political backlash against wealthy elites in many countries. One increasingly popular policy is to constrain or impose an outright ban on the construction of new second homes in seasonal tourist places. The Swiss Alps may be the most prominent example, but it is by no means the only one.

In this paper, we explore the economic impacts of an outright ban on the construction of new second homes. We do so by exploiting the unique empirical setting provided by the unexpected approval of the Swiss SHI in March 2012. We find that the SHI-induced ban substantially reduced the price growth of primary homes, increased local unemployment, and increased the price growth of already existing second homes.

Our findings are consistent with the predictions derived from a general equilibrium model that treats primary and second homes as poor substitutes that are traded in separate markets. In such a setting, the option to convert a primary residence into a second home is worthless and thus does not provide a hedge against the negative impact of banning new second homes.

Constraining the construction of new second homes hurts local (typically immobile) homeowners via lower primary house prices and adverse effects on the local labor market. Renters benefit from lower rents but, overall, they are likely not better off because the fall in rents is commensurate to the negative effects on the local economy. In a spatial equilibrium setting without relocation costs, renters should be neither better nor worse off. Our empirical findings indicate that *existing* second home investors were the real beneficiaries in the treated areas: The estimated effect of the ban on the price growth of second homes is consistently positive, representing a positive wealth effect for existing owners of such homes.

Whether the landscape preservation effect of the ban for residents living in unaffected (urban) areas compensates the documented negative effects for local residents in treated areas, is an open question. The aggregate welfare effect of banning second home investors thus remains uncertain. We leave the further theoretical and empirical analysis of this question for future research.

Our findings hold important lessons for other countries with highly touristic areas, in which inequality has led to a political backlash against the wealthy and, in particular, against (foreign) second home investors. Overall, our findings are indicative that constraining the construction of new second homes may reinforce rather than reduce wealth inequality in highly touristic areas. While bans do nothing to improve local economies, local annual taxes on the value of land or second homes could potentially help local economies (via increasing local tax revenue and reducing the ghost town character), whilst at the same time preserving the landscape.

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# TABLES

TABLE 1A

Summary statistics – Municipalities with share of second homes at or above 20%-threshold (treatment group)

| | 2010-2011 | | | | 2013-2014 | | | |
|---|---|---|---|---|---|---|---|---|
| VARIABLES (municipality level averages) | Min | Max | Mean | Sd | Min | Max | Mean | Sd |
| Price of primary homes (1'000 CHF) | 100 | 3'366.67 | 608.77 | 366.37 | 100 | 2'396.67 | 592.07 | 312.74 |
| Unemployment rate (%)† | 0.21 | 4.13 | 1.27 | 0.66 | 0.14 | 4.44 | 1.35 | 0.65 |
| Number of new residential units (1'000) | 0 | 0.15 | 0.01 | 0.02 | 0 | 0.20 | 0.02 | 0.03 |
| Nb. of elderly (1'000) | 0.01 | 4.60 | 0.36 | 0.48 | 0.01 | 4.88 | 0.42 | 0.53 |
| Resident population (1'000) | 0.03 | 24.89 | 1.87 | 2.58 | 0.07 | 26.09 | 2.03 | 2.73 |
| Wages (1'000 CHF) | 35.05 | 99.79 | 55.66 | 9.00 | 32.85 | 325.21 | 58.30 | 19.37 |
| **Housing characteristics** (primary homes) | | | | | | | | |
| Number of rooms | 2 | 10 | 4.25 | 1.19 | 1 | 9 | 4.09 | 1.18 |
| Number of bathrooms | 1 | 4 | 1.85 | 0.47 | 1 | 4 | 1.79 | 0.52 |
| Number of parking places | 0 | 2 | 0.61 | 0.50 | 0 | 2 | 0.58 | 0.50 |
| Micro-location (1 to 4' bad to excellent) | 1 | 4 | 3.09 | 0.48 | 1 | 4 | 2.89 | 0.52 |
| Quality (standard of finishing) (1 to 4' bad to excellent) | 1 | 4 | 2.73 | 0.67 | 1 | 4 | 2.52 | 0.64 |
| Condition (1 to 4' bad to excellent) | 1 | 4 | 2.68 | 0.71 | 1 | 4 | 2.50 | 0.75 |
| Age of housing unit at time of transaction†† | -0.83 | 161 | 32.57 | 28.64 | 0 | 164 | 36.91 | 29.65 |
| Single-family house (yes/no) | 0 | 1 | 0.49 | 0.40 | 0 | 1 | 0.50 | 0.41 |
| Number of transactions | 1 | 121 | 7.12 | 12.85 | 1 | 148 | 6.25 | 12.46 |
| **Fiscal variables** | | | | | | | | |
| Foreign residents (%) | 0.00 | 61.18 | 15.90 | 10.26 | 1.79 | 60.75 | 17.14 | 10.25 |
| Mean net income (1'000 CHF) | 26.05 | 96.82 | 50.80 | 11.29 | | | | |
| Net income Gini index | 0.38 | 0.71 | 0.49 | 0.07 | | | | |
| **Other municipality characteristics** (time-invariant or predetermined) | | | | | | | | |
| Second home rate (%) | 20.30 | 86.10 | 47.88 | 17.21 | | | | |
| Voting No (%) | 26.20 | 88.90 | 60.99 | 12.47 | | | | |
| Unproductive surface (%) | 0.00 | 95.00 | 22.73 | 22.27 | | | | |
| Distance to major city (km) | 0 | 102.52 | 36.82 | 24.78 | | | | |
| Distance to major ski resort (km) | 0 | 81.03 | 15.33 | 22.10 | | | | |
| Pct. of workers in the 3rd sector (%) | 0.00 | 95.00 | 61.63 | 18.41 | | | | |
| Pct. of firms in the 3rd sector (%) | 0.00 | 94.00 | 62.93 | 15.07 | | | | |
| Number of municipalities | | 276 | | | | 255 | | |

*Note* † Unemployment rates are expressed relative to *total* population. †† The age of the housing unit at time of transaction is defined as the year in which the transaction takes place minus the construction year. Since some dwellings are sold before being constructed, the variable can take negative values. Summary statistics for the price of 2nd homes are reported in the note to Table 3.

TABLE 1B

Summary statistics – Municipalities with share of second homes below 20%-threshold (control group)

| | 2010-2011 | | | | 2013-2014 | | | |
|---|---|---|---|---|---|---|---|---|
| VARIABLES (municipality level averages) | Min | Max | Mean | Sd | Min | Max | Mean | Sd |
| Price of primary homes (1'000 CHF) | 120 | 3'040 | 745.46 | 333.35 | 120 | 2'880 | 805.33 | 332.31 |
| Unemployment rate (%)† | 0.00 | 4.14 | 1.32 | 0.61 | 0.16 | 3.99 | 1.31 | 0.58 |
| Number of new residential units (1'000) | 0 | 1.75 | 0.03 | 0.07 | 0 | 0.66 | 0.03 | 0.05 |
| Nb. of elderly (1'000) | 0.01 | 62.45 | 0.77 | 2.37 | 0.01 | 62.23 | 0.84 | 2.42 |
| Resident population (1'000) | 0.13 | 374.92 | 4.54 | 13.69 | 0.11 | 388.07 | 4.80 | 14.24 |
| Wages (1'000 CHF) | 38.21 | 195.48 | 67.95 | 16.00 | 40.75 | 203.23 | 69.01 | 15.97 |
| **Housing characteristics** (primary homes) | | | | | | | | |
| Number of rooms | 2 | 12 | 4.85 | 0.84 | 2 | 11 | 4.74 | 0.88 |
| Number of bathrooms | 1 | 4 | 2.05 | 0.43 | 1 | 4 | 2.03 | 0.44 |
| Number of parking places | 0 | 3 | 0.87 | 0.52 | 0 | 3 | 0.82 | 0.52 |
| Micro-location (1 to 4' bad to excellent) | 1 | 4 | 2.92 | 0.40 | 1 | 4 | 2.76 | 0.40 |
| Quality (standard of finishing) (1 to 4' bad to excellent) | 1 | 4 | 2.96 | 0.54 | 1 | 4 | 2.85 | 0.55 |
| Condition (1 to 4' bad to excellent) | 1 | 4 | 2.91 | 0.58 | 1 | 4 | 2.82 | 0.62 |
| Age of housing unit at time of transaction†† | -1 | 161 | 28.39 | 25.44 | -1 | 164 | 29.62 | 26.26 |
| Single-family house (yes/no) | 0 | 1 | 0.61 | 0.32 | 0 | 1 | 0.59 | 0.34 |
| Number of transactions | 1 | 798 | 14.94 | 33.85 | 1 | 855 | 13.23 | 32.17 |
| **Fiscal variables** | | | | | | | | |
| Foreign residents (%) | 0.62 | 51.67 | 16.09 | 9.40 | 0.24 | 55.09 | 17.48 | 9.62 |
| Mean net income (1'000 CHF) | 40.16 | 341.34 | 68.54 | 23.33 | | | | |
| Net income Gini index | 0.31 | 0.81 | 0.44 | 0.06 | | | | |
| **Other municipality characteristics** (time-invariant or predetermined) | | | | | | | | |
| Second home rate (%) | 1.60 | 34.30 | 11.32 | 4.70 | | | | |
| Voting No (%) | 28.70 | 84.20 | 50.38 | 7.12 | | | | |
| Unproductive surface (%) | 0.00 | 86.70 | 2.90 | 6.36 | | | | |
| Distance to major city (km) | 0 | 75.79 | 10.88 | 11.09 | | | | |
| Distance to major ski resort (km) | 0 | 78.91 | 34.44 | 19.80 | | | | |
| Pct. of workers in the 3rd sector (%) | 5.00 | 99.00 | 57.77 | 17.73 | | | | |
| Pct. of firms in the 3rd sector (%) | 15.00 | 94.00 | 64.65 | 14.45 | | | | |
| Number of municipalities | | 1556 | | | | 1524 | | |

*Note* † Unemployment rates are expressed relative to *total* population. †† The age of the housing unit at time of transaction is defined as the year in which the transaction takes place minus the construction year. Since some dwellings are sold before being constructed, the variable can take negative values. Summary statistics for the price of 2nd homes are reported in the note to Table 3.

TABLE 2

Impact of SHI on price growth of primary homes and unemployment rates: FD-IV estimates

| Panel A: *Pre and post - Second stage* | | | | | | |
|---|---|---|---|---|---|---|
| Dependent variable | Δ Log price of primary homes | | | Δ Log unemployment rate | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Observed treatment | **-0.152***** | **-0.147***** | **-0.190***** | **0.121***** | **0.118***** | **0.111***** |
| | **(0.0461)** | **(0.0448)** | **(0.0443)** | **(0.0252)** | **(0.0257)** | **(0.0254)** |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |
| Observations | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 |
| Kleibergen-Paap F | 1623 | 1619 | 1632 | 1623 | 1619 | 1620 |
| *First stage* | | | | | | |
| Dependent variable | Observed treatment | | | | | |
| | 2.066*** | 2.068*** | 2.043*** | 2.066*** | 2.068*** | 2.067*** |
| | (0.0513) | (0.0514) | (0.0506) | (0.0513) | (0.0514) | (0.0513) |
| Panel B: *Parallel pre-trend (placebo test) - Second stage* | | | | | | |
| Dependent variable | Δ Log price of primary homes | | | Δ Log unemployment rate | | |
| Observed treatment | **0.0272** | **0.0118** | **-0.0288** | **-0.0189** | **-0.0249** | **-0.0253** |
| | **(0.0346)** | **(0.0319)** | **(0.0313)** | **(0.0213)** | **(0.0219)** | **(0.0219)** |
| *Parallel pre-trend (placebo test) - First stage* | | | | | | |
| Dependent variable | Observed treatment | | | | | |
| | 2.048*** | 2.061*** | 2.039*** | 2.048*** | 2.061*** | 2.061*** |
| | (0.0478) | (0.0477) | (0.0478) | (0.0478) | (0.0477) | (0.0477) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |
| Observations | 1,462 | 1,462 | 1,462 | 1,462 | 1,462 | 1,462 |
| Kleibergen-Paap F | 1840 | 1869 | 1818 | 1840 | 1869 | 1867 |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on a given outcome variable for a given set of controls. Municipalities that have missing values for a given set of controls are excluded from all specifications. In Panel A, the two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. In Panel B, the two-period analysis is carried out by dividing the data into pre (2008-2009) and post (2010-2011) periods. We consider an additional pre period (2006-2007) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The sample includes municipalities for which housing transactions were available pre and post implementation of the SHI. Houses built after 2012 are excluded. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000. In Panel B, we do not control for lagged changes in foreign residents and new construction in columns 2-3 and 5-6 due to lack of available data.

TABLE 3

Impact of SHI on price growth of second homes: DD estimates

| Panel A: *Pre and post* | | | |
|---|---|---|---|
| Dependent variable | Log price of second homes | | |
| | (1) | (2) | (3) |
| Observed treatment × Post | **0.259** | **0.256*** | **0.252*** |
| | **(0.184)** | **(0.146)** | **(0.146)** |
| Observed treatment | Yes | Yes | Yes |
| Time fixed effects | Yes | Yes | Yes |
| Lagged and time-invariant controls | No | Yes | Yes |
| Predetermined outcome level × Post | No | No | Yes |
| Observations | 323 | 323 | 323 |
| R-squared | 0.015 | 0.562 | 0.562 |
| Panel B: *Parallel pre-trend (placebo test)* | | | |
| Observed treatment × Post | **-0.0498** | **-0.121** | **-0.157** |
| | **(0.200)** | **(0.160)** | **(0.159)** |
| Observed treatment | Yes | Yes | Yes |
| Time fixed effects | Yes | Yes | Yes |
| Lagged and time invariant controls | No | Yes | Yes |
| Predetermined outcome level × Post | No | No | Yes |
| Observations | 324 | 324 | 324 |
| R-squared | 0.004 | 0.557 | 0.570 |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). The two-period analysis is structured similarly to the one of Table 2. In Panel A, data available for all municipalities has been pooled for the pre (2010-2011) and post (2013-2014) periods. We consider an additional pre period (2008-2009) to include lagged controls. In Panel B, the two-period analysis is carried out by dividing the data into pre (2008-2009) and post (2010-2011) periods. We consider an additional pre period (2006-2007) to include the lagged difference of controls. The average price of second homes in the full sample was about 597'000 CHF in 2010-2011 and 638'000 CHF in 2013-2014 in not treated municipalities. In these municipalities, the average number of transactions was 2.26 (2010-2011) and 1.54 (2013-2014), respectively. In treated municipalities, the average price was about 630'000 (2010-2011) and 647'000 (2013-2014), with an average number of transactions equal to 7.5 (2010-2011) and 7.38 (2013-2014), respectively. Full summary statistics for all variables (including controls) are available from the authors upon request.

TABLE 4

Summary of alternative identification strategies and robustness checks

| Dependent variable | Δ Log price of primary homes | | | Δ Log unemployment rate | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | | | Panel A: *Standard strategies (non-IV)* | | | |
| DD estimates | -0.142** | -0.152*** | -0.119*** | 0.0787 | 0.0823* | 0.0969** |
| | (0.0571) | (0.0450) | (0.0456) | (0.0602) | (0.0428) | (0.0396) |
| FD estimates | -0.142*** | -0.140*** | -0.191*** | 0.0787*** | 0.0757*** | 0.0651*** |
| | (0.0386) | (0.0376) | (0.0365) | (0.0231) | (0.0236) | (0.0230) |
| | | | Panel B: *Alternative FD-IV estimates, 2nd stage only* | | | |
| Restricted Sample 1[a)] | -0.172** | -0.195*** | -0.237*** | 0.0962* | 0.0931* | 0.105* |
| | (0.0734) | (0.0703) | (0.0661) | (0.0568) | (0.0546) | (0.0563) |
| Restricted Sample 2[b)] | -0.561*** | -0.370** | -0.353** | 0.243* | 0.292** | 0.252** |
| | (0.169) | (0.149) | (0.149) | (0.125) | (0.116) | (0.105) |
| Excluding close to | -0.148*** | -0.142*** | -0.191*** | 0.113*** | 0.112*** | 0.105*** |
| treated (within 5km) | (0.0459) | (0.0441) | (0.0441) | (0.0250) | (0.0251) | (0.0248) |
| Including primary homes built after 2012 | -0.135*** | -0.130*** | -0.180*** | | | |
| | (0.0441) | (0.0430) | (0.0426) | | | |
| Lagged diff. of controls[c)] | No | Yes | Yes | No | Yes | Yes |
| Predeterm. outcome level [c)] | No | No | Yes | No | Yes | Yes |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Web-Appendix Tables W-E2, W-E3 and W-E5 to W-E8 provide detailed estimation results. Web-Appendix Table W-E4 reports balancing tests for the two restricted samples (Tables W-E5 and W-E6). The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000. [a)] We exclude municipalities situated within a 10 km radius from major CBDs and/or are adjacent to a major ski resort. [b)] We exclude municipalities within a 10 km radius from major CBDs and/or having a second home rate below 15% or above 30%. [c)] For DD estimates the corresponding set of controls are FEs and lagged controls.

TABLE 5

Impact of SHI on other outcome measures (FD-IV estimates, 2nd stage only)

| Dependent variable | (1) | (2) | (3) |
|---|---|---|---|
| Δ Log newly built residential units | -0.187* | -0.197* | -0.231** |
| | (0.107) | (0.107) | (0.101) |
| Δ Log of number of elderly | 0.00246 | 0.00322 | -0.00205 |
| | (0.00839) | (0.00840) | (0.00849) |
| Δ Log of population | -0.00911 | -0.00797 | -0.00932 |
| | (0.00654) | (0.00650) | (0.00669) |
| Δ Log of wages | 0.0124*** | 0.0137*** | 0.00612 |
| | (0.00380) | (0.00380) | (0.00419) |
| Lagged difference of controls | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Web-Appendix Tables W-E9 to W-E12 provide detailed estimation results. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000.

# FIGURES

FIGURE 1

SHI-voting results at municipality level with respect to second home percentage

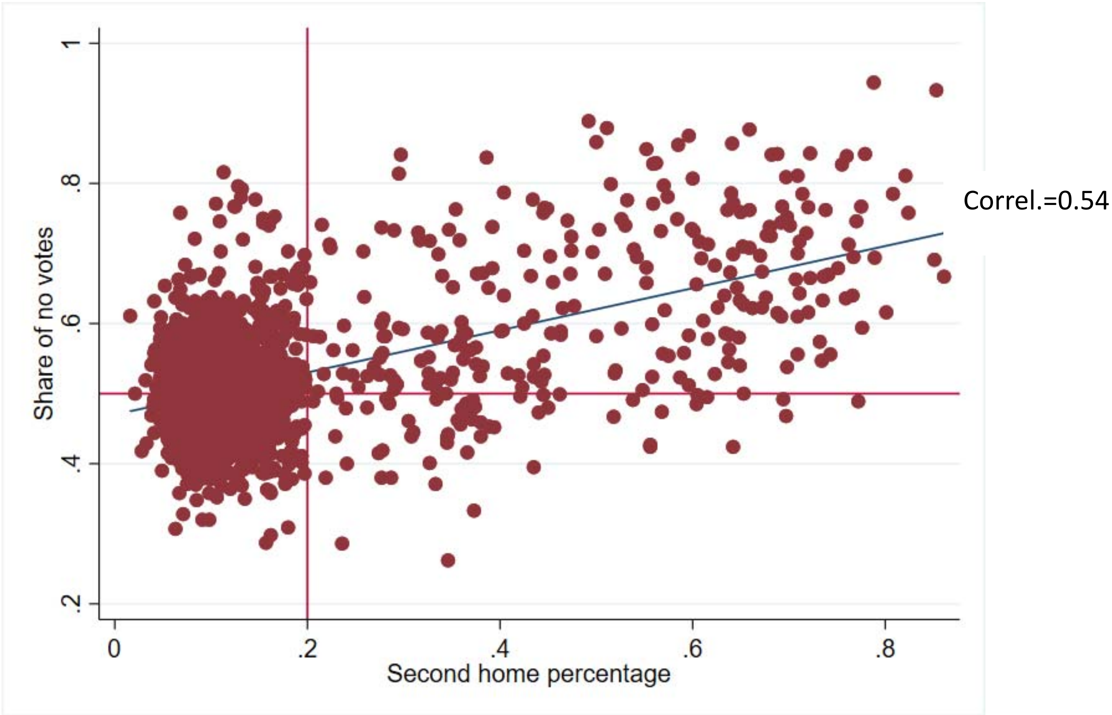



FIGURE 2

Treatment and control group

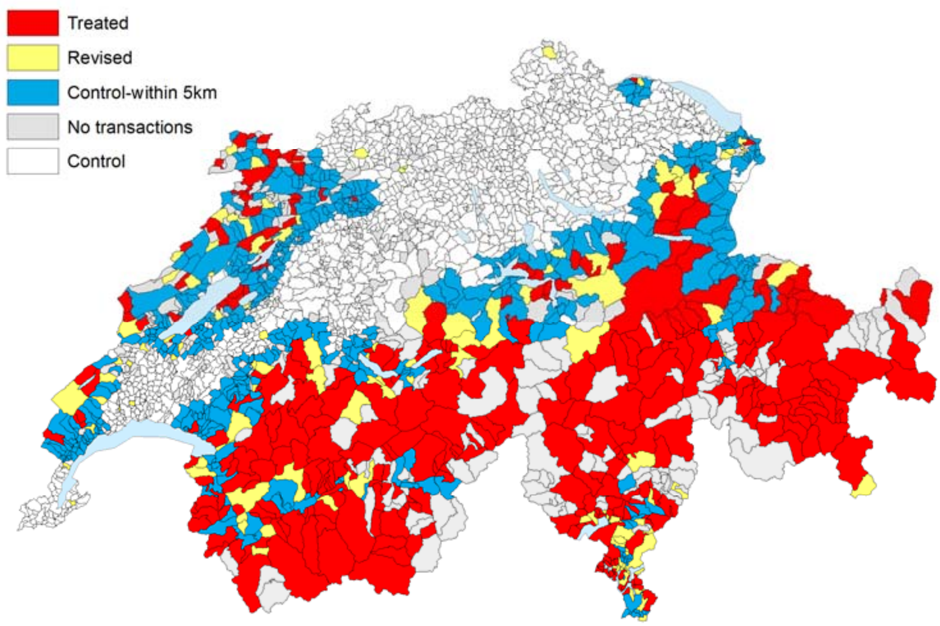


FIGURE 3
Parallel trend graphs for main outcome measures

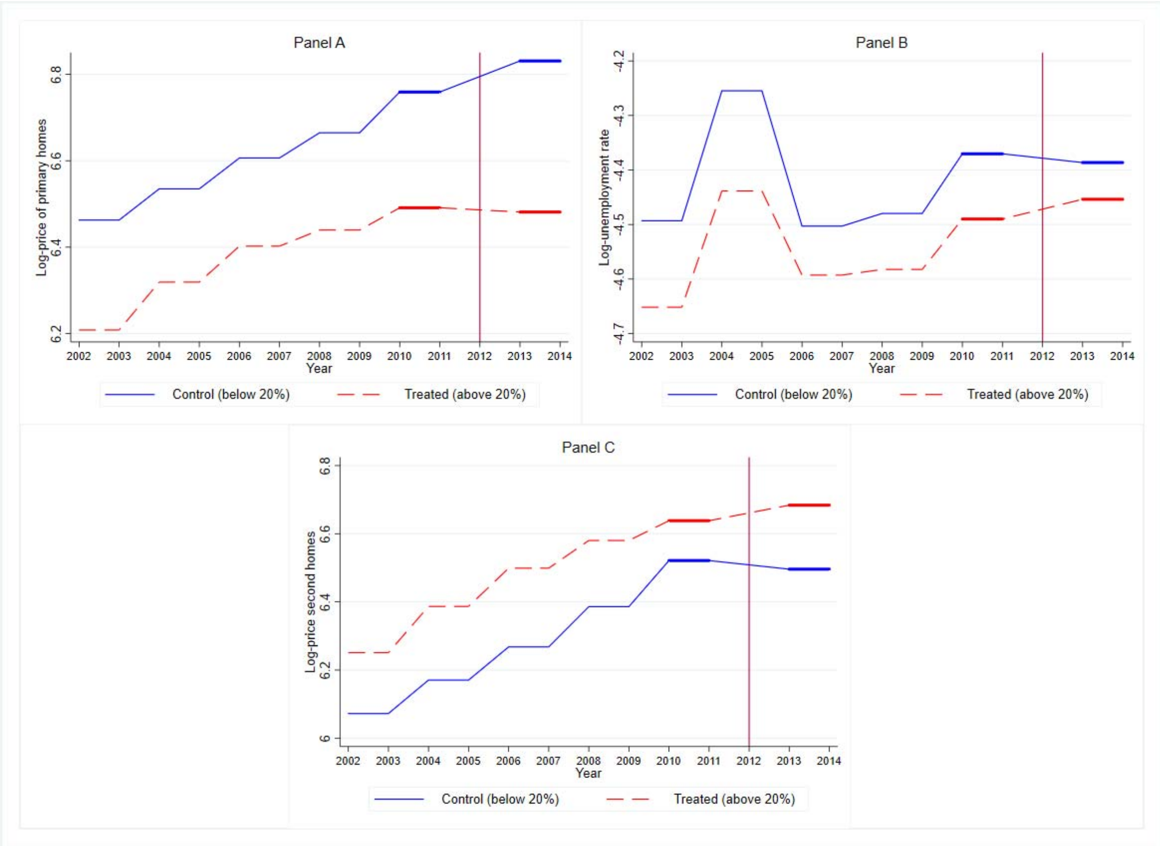
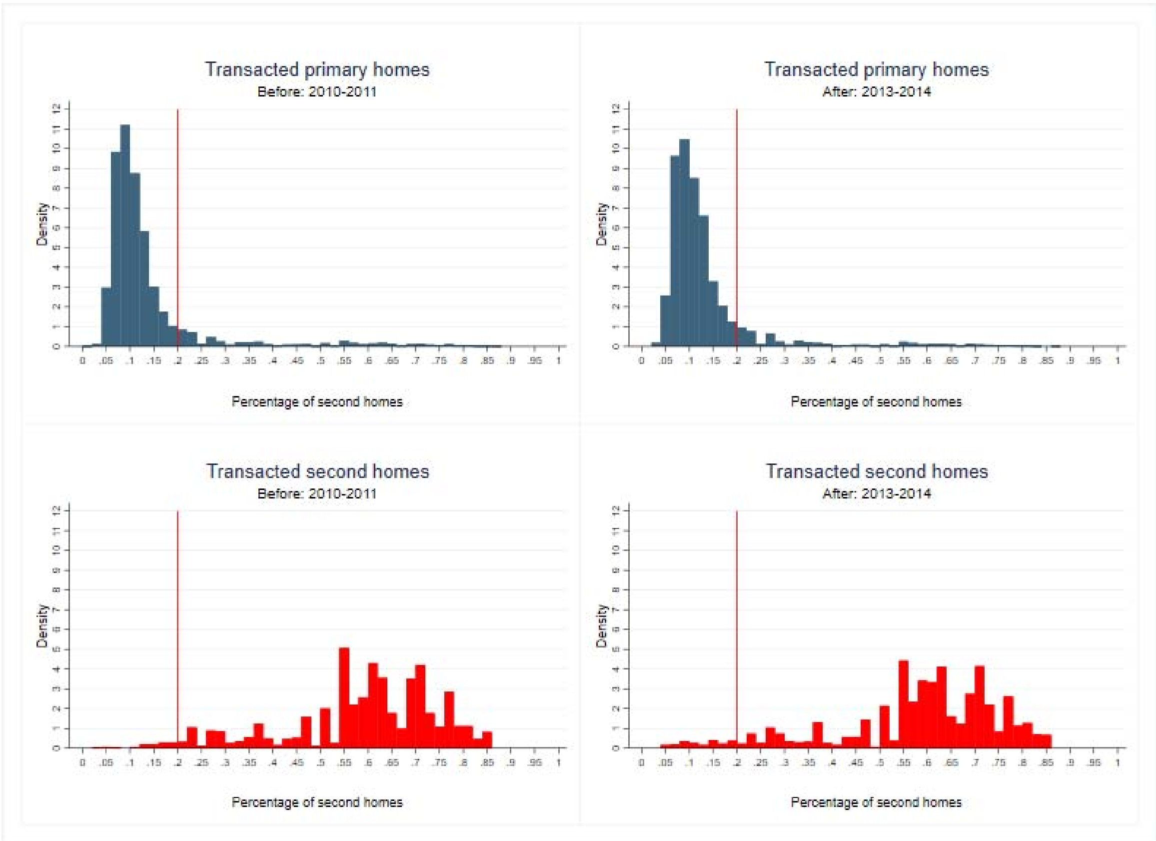



FIGURE 4
Histogram of transacted primary and second homes according to second home percentage



# Appendix

APPENDIX TABLE A1
SHI-voting results

| Dependent variable | Share of no votes | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | All | Only control | Only treated |
| Second home rate | 0.1225*** | -0.0246 | 0.1961*** |
| | (0.0270) | (0.0454) | (0.0596) |
| Voting turnout | 0.0837** | 0.0241 | 0.2347*** |
| | (0.0327) | (0.0296) | (0.0592) |
| Average net income | 0.0009*** | 0.0006*** | 0.0012 |
| | (0.0002) | (0.0002) | (0.0007) |
| Gini coefficient for net income | -0.0607 | 0.1145* | -0.1893 |
| | (0.0644) | (0.0592) | (0.1289) |
| Number of primary residents | -0.0003*** | -0.0004*** | 0.0056** |
| | (0.0001) | (0.0001) | (0.0026) |
| Share of foreign residents | 0.0206 | 0.0305 | -0.0670 |
| | (0.0291) | (0.0250) | (0.0715) |
| Unproductive surface | 0.0335 | 0.0476* | -0.0020 |
| | (0.0266) | (0.0281) | (0.0311) |
| Share of residents in the service sector | -0.0070 | -0.0010 | -0.0061 |
| | (0.0118) | (0.0113) | (0.0452) |
| Share of firms in the service sector | -0.0692*** | -0.0754*** | -0.0985 |
| | (0.0207) | (0.0193) | (0.0825) |
| Homeownership rate | 0.0841*** | 0.0610*** | 0.3199*** |
| | (0.0173) | (0.0154) | (0.0687) |
| Distance from major CBD | -0.0002 | 0.0000 | -0.0012*** |
| | (0.0002) | (0.0002) | (0.0004) |
| Distance from major ski resort | -0.0010*** | -0.0004*** | -0.0032*** |
| | (0.0002) | (0.0001) | (0.0005) |
| Cantonal FE | Yes | Yes | Yes |
| Observations | 1,688 | 1,422 | 266 |
| R-squared | 0.6297 | 0.5858 | 0.6441 |

*Notes:* Robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). All municipalities for which second home rates, voting results, and included controls were available in 2010-2011 are included in the sample. Municipalities that have revised their second home rate are not included.

# SUPPLEMENTARY MATERIAL: WEB-APPENDICES

## Web-Appendix A: References to policies on second homes

In this section we provide a small selection of non-academic references on second homes policies implemented around the globe. The list is by no means exhaustive. Rather, the cited references provide a brief description of the implemented policies and how they were welcomed by the press.

TABLE W-A1

Second homes policies around the world

| Country | Reference |
|---|---|
| | ***Constraints or bans on the construction of new second homes*** |
| Denmark | Global Property Guide (2018). Danish house prices continue to surge! June 9. |
| Switzerland | Franz Weber Foundation (https://www.ffw.ch/projekte/zweitwohnungsinitiative/ )<br>Investorproperty.com (2017). The Weber Law: The End for Swiss Second Homes. March 2017. |
| UK | Morris, S. (2014). St. Ives council toys with banning outsiders buying holiday homes. *Guardian*, November 17.<br>Swerling, G. (2014). St. Ives aims to turn tide on city dwellers with second home ban. *The Times*, November 7.<br>The Economist (2016). To the lighthouse. April 2016.<br>The Economist (2016). Stay away. May 2016.<br>The Guardian (2016). St. Ives backs residents-only home ownership plan in referendum. May 2016.<br>Wilkinson, G. (2017). More places in Cornwall follow St Ives second homes ban as High Court challenge dismissed. November 2017.<br>BBC (2018). Voters back new-build second homes ban in Northumberland. May 2018. |
| | ***Constraints on second homve investments*** |
| Australia | Macken, L. and Razaghi, T. (2018). Foreign buyers of Australian real estate plummet, Foreign Investment Review Board figures show. Domain. May 29. |
| New Zealand | Agerholm, H. (2018). New Zealand bans sale of homes to foreign buyers. Independent. August 15.<br>Ainge Roy, E. (2018). 'Tenants on our own land': New Zealand bans sale of homes to foreign buyers. The Guardian. August 15.<br>The Guardian (2018). New Zealand ban on foreign home buyers begins amid doubts it will ease crisis. October 22. |
| | ***Tax supplements or penalties on second homes/second home investors*** |
| Canada | Alini, E. (2017). The Vancouver foreign homebuyer tax is one year old. Here's what Canada can learn from it. Global News. August 1.<br>Non-Resident Speculation Tax, Ontario, Ministry of Finance. https://www.fin.gov.on.ca/en/bulletins/nrst/<br>The Canadian Press (2017). Home sales to foreign buyers decreasing in the Greater Golden Horseshoe area. September 14.<br>Giovannetti and Mahoney (2017). Toronto housing market feels effect of foreign-buyers tax. The Globe and Mail, September 15. |
| France | Le Parisien (2014). Résidences secondaires: l'Assemblé a voté la hausse de la taxe d'habitation. December 3.<br>Samuel, H. (2014). Britons face tax hike on coveted French second homes. *Telegraph*, November 4. |

TABLE W-A1 *(cont.)*
Second homes policies around the world

| Country | Reference |
|---|---|
| ***Tax supplements or penalties on second homes/second home investors** (cont.)* | |
| Israel | Gross, Judah Ari. (2015). Bid to make housing affordable sends buyers scrambling, but will it work? *The Times of Israel*. June 21. |
| UK | HM Treasury and George Osborne (2015). Spending Review and Autumn Statement 2015, Cm 9162. |
| Singapore | Harper, J. (2013). Singapore gets tough on foreign property buyers, *The Telegraph*, Jan 16.<br>Shamim, A. (2011). Singapore Extends Housing Measures; Developers Drop. *BloombergBusiness*, January 14. |
| United States (New York) | Barbanel, J. (2014). New Yourk City Mayor De Blasio Weighs Pied-à-Terre Tax. *Wall Street Journal*, September 23.<br>Higgins, M. (2013). Tax-Abatement Changes Affect Many Unit Owners. *The New York Times*, March 26. |
| ***Various constraints on second home investments including credit constraints*** | |
| China | Bloomberg. (2013). Beijing Curbs Second Home Buying as China Cools Property Market. *Bloomberg News*, 30 March 2013.<br>Fung, E. (2015). China Lowers Down Payments for Buyers of Second Homes. *Wall Street Journal*, 30 March. |

## Web-Appendix B: Additional figures

FIGURE W-B1
Second home rate distribution at the municipality level

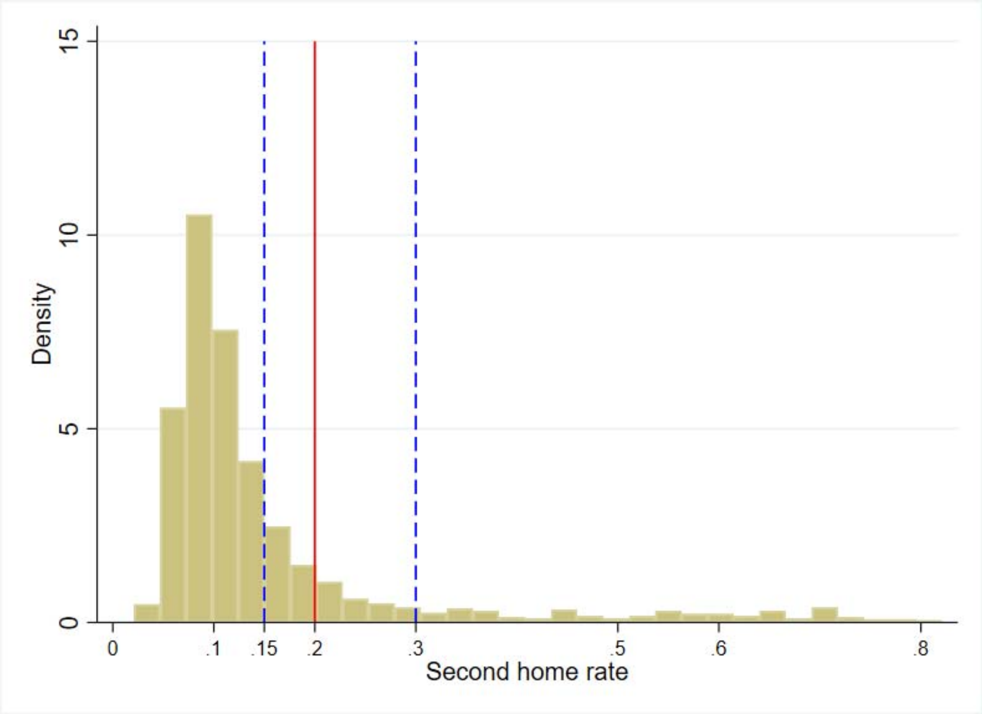
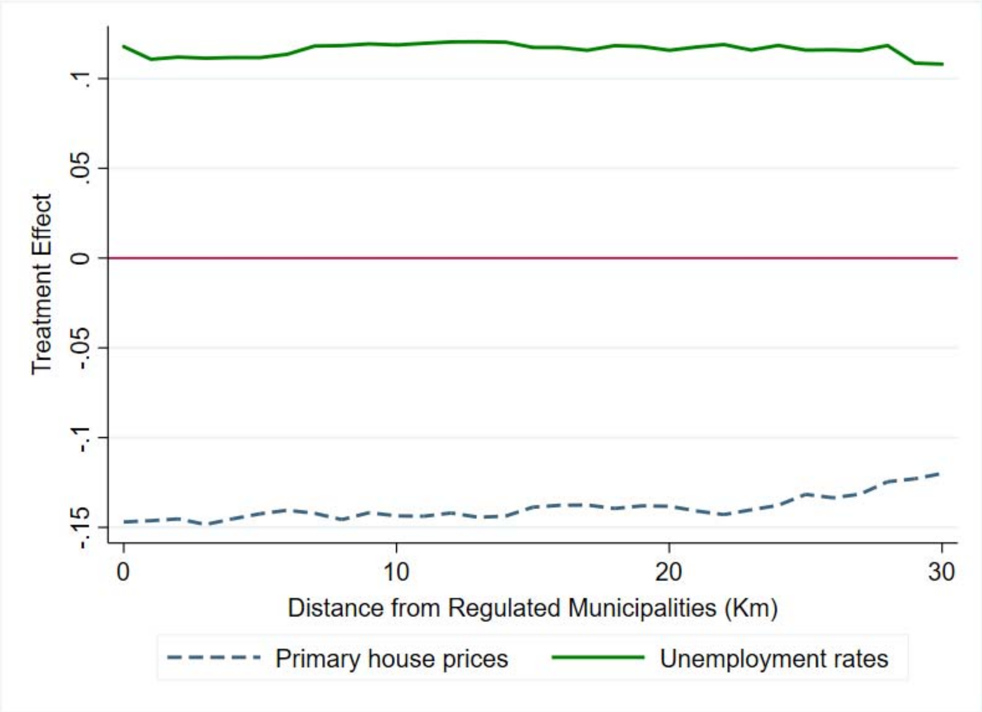



FIGURE W-B2
FD-IV treatment effects: excluding control municipalities within given distance from treated



## Web-Appendix C: Theoretical results, extensions and simulations

*C.1 Theoretical results and model extensions*

Symbolic computations presented in this section have been made using Mathematica.

*Proof of Corollary 1*

We prove the existence and uniqueness of the dynamic equilibrium. We start by explicitly stating the equations defining the equilibrium according to Definition 1.

Labor market clearing: $N_{it} = \beta^{\frac{\gamma-1}{1-\beta-\gamma}}\gamma^{\frac{\gamma}{1-\beta-\gamma}}\bar{Z}p_{it}^{\frac{1}{1-\beta-\gamma}}A_{it}^{\frac{1}{1-\beta-\gamma}}W_{it}^{\frac{\gamma-1}{1-\beta-\gamma}}$ (C1)

Primary residents' spatial equilibrium: $V_t = \theta_i N_{it}^{\mathcal{S}\,\eta}\frac{W_{it}}{r_{it}^{a}}$ (C2)

Investors' spatial equilibrium: $V_t^{\mathcal{S}} = \theta_i^{\mathcal{S}} N_{it}^{\mathcal{S}\,\epsilon}\frac{W_t^{\mathcal{S}}}{p_{it}^{1-b}r_{it}^{\mathcal{S},b}}$ (C3)

Primary residences housing market clearing: $\frac{aN_{it}W_{it}}{r_{it}} = H\left(\frac{(r-g_i)P_{it}}{(r-g_i^c)(1+g_i^c)^t}\right)^{\rho_i}$ (C4)

Secondary residences housing market clearing: $\frac{bN_{it}^{\mathcal{S}}W_t^{\mathcal{S}}}{r_{it}^{\mathcal{S}}} = H^{\mathcal{S}}\left(\frac{(r-g_i^{\mathcal{S}})P_{it}^{\mathcal{S}}}{(r-g_i^{\mathcal{S},c})(1+g_i^{\mathcal{S},c})^t}\right)^{\rho_i}$ (C5)

Tourism services clearing: $\beta^{\frac{\beta}{1-\beta-\gamma}}\gamma^{\frac{\gamma}{1-\beta-\gamma}}p_{it}^{\frac{\beta+\gamma}{1-\beta-\gamma}}A_{it}^{\frac{1}{1-\beta-\gamma}}W_{it}^{\frac{-\beta}{1-\beta-\gamma}} = N_{it}^{\mathcal{S}}(1-b)\frac{W_t^{\mathcal{S}}}{p_{it}}$ (C6)

Using the dynamic price equation $r_{it}^{j} = (r-g_i^{j})P_{it}^{j}/(1+r)$, $j \in \{\mathcal{P},\mathcal{S}\}$, expressing the system of equations in changes, and applying a log-transformation we obtain

$$\ln\left(\frac{N_{it+1}}{N_{it}}\right) = \frac{1}{1-\beta-\gamma}\ln\left(\frac{p_{it+1}}{p_{it}}\right) + \frac{1}{1-\beta-\gamma}\ln\left(1+g_{A_i}\right) + \frac{\gamma-1}{1-\beta-\gamma}\ln\left(\frac{W_{it+1}}{W_{it}}\right) \quad \text{(C1')}$$

$$\ln(1+g_V) + a\ln\left(\frac{P_{it+1}}{P_{it}}\right) = \eta\ln\left(\frac{N_{it+1}^{\mathcal{S}}}{N_{it}^{\mathcal{S}}}\right) + \ln\left(\frac{W_{it+1}}{W_{it}}\right) \quad \text{(C2')}$$

$$\ln(1+g_{V^{\mathcal{S}}}) + b\ln\left(\frac{P_{it+1}^{\mathcal{S}}}{P_{it}^{\mathcal{S}}}\right) + (1-b)\ln\left(\frac{p_{it+1}}{p_{it}}\right) = \epsilon\ln\left(\frac{N_{it+1}^{\mathcal{S}}}{N_{it}^{\mathcal{S}}}\right) + \ln(1+g_{W^{\mathcal{S}}}) \quad \text{(C3')}$$

$$\ln\left(\frac{N_{it+1}}{N_{it}}\right) + \ln\left(\frac{W_{it+1}}{W_{it}}\right) = (\rho+1)\ln\left(\frac{P_{it+1}}{P_{it}}\right) - \rho\ln(1+g_c) \quad \text{(C4')}$$

$$\ln\left(\frac{N_{it+1}^{\mathcal{S}}}{N_{it}^{\mathcal{S}}}\right) + \ln(1+g_{W^{\mathcal{S}}}) = (\rho+1)\ln\left(\frac{P_{it+1}^{\mathcal{S}}}{P_{it}^{\mathcal{S}}}\right) - \rho\ln(1+g_c^{\mathcal{S}}) \quad \text{(C5')}$$

$$\frac{1}{1-\beta-\gamma}\ln\left(\frac{p_{it+1}}{p_{it}}\right) + \frac{1}{1-\beta-\gamma}\ln\left(1+g_{A_i}\right) - \frac{\beta}{1-\beta-\gamma}\ln\left(\frac{W_{it+1}}{W_{it}}\right) = \ln\left(\frac{N_{it+1}^{\mathcal{S}}}{N_{it}^{\mathcal{S}}}\right) + \ln(1+g_{W^{\mathcal{S}}}), \quad \text{(C6')}$$

where we have used the notation $\frac{V_{t+1}}{V_t} = (1+g_V)$, $\frac{V_{t+1}^{\mathcal{S}}}{V_t^{\mathcal{S}}} = (1+g_{V^{\mathcal{S}}})$, $\frac{A_{it+1}}{A_{it}} = (1+g_{A_i})$, $\frac{W_{t+1}^{\mathcal{S}}}{W_t^{\mathcal{S}}} = (1+g_{W^{\mathcal{S}}})$ for the exogenous parameters' growth.

As the system is linear in the endogenous quantities $\ln\left(\frac{W_{it+1}}{W_{it}}\right), \ln\left(\frac{P_{it+1}}{P_{it}}\right), \ln\left(\frac{N_{it+1}}{N_{it}}\right)$, $\ln\left(\frac{P_{it+1}^{\mathcal{S}}}{P_{it}^{\mathcal{S}}}\right), \ln\left(\frac{N_{it+1}^{\mathcal{S}}}{N_{it}^{\mathcal{S}}}\right), \ln\left(\frac{p_{it+1}}{p_{it}}\right)$ we can solve it with respect to the exogenous parameters $\ln(1+g_V), \ln(1+g_{V^{\mathcal{S}}}), \ln(1+g_{W^{\mathcal{S}}}),\ \ln(1+g_{A_i}),\ \ln(1+g_i^c),\ \ln(1+g_i^{\mathcal{S},c})$, $a,\ b,,\eta,\ \epsilon,\ \rho,\beta,\gamma$. Assuming parameters do not take degenerate values, the existence and uniqueness of the solution follows from standard linear algebra.

*Proof of Propositions 1 and 2*

In the previous section we have shown the existence and uniqueness of the equilibrium describing local economies. We make comparative static predictions about the effect of banning second homes (i.e. making their housing supply more/perfectly inelastic) by computing the derivative of the equilibrium solution with respect to $g_i^{s,c}$. In fact, the post-ban costs of providing new second homes increased due to the imposed constraints. Table W-C1 summarizes the impact of the ban on the endogenous variables of the system, with $c := -1 + \epsilon + (-1 + b + \epsilon)\rho - (-1 + b)\gamma(1 + \rho) + (-1 + b)\beta(a - (1 + \eta)(1 + \rho))$.

TABLE W-C1

Treatment effects – No agglomeration economies

| **Outcome variable** | **Comparative static treatment effect** | **Sign** |
|---|---|---|
| Wages | $-\frac{b\rho(-a + \eta + \eta\rho)}{(1 + \rho)c(a, b, \epsilon, \eta, \rho, \beta, \gamma)(1 + g_{s,c})}$ | $< 0$ |
| Price of primary homes | $\frac{b\rho}{(1 + \rho)c(a, b, \epsilon, \eta, \rho, \beta, \gamma)(1 + g_{s,c})}$ | $< 0$ |
| Number of primary residents | $\frac{b\rho(1 - a + \eta + \rho + \eta\rho)}{(1 + \rho)c(a, b, \epsilon, \eta, \rho, \beta, \gamma)(1 + g_{s,c})}$ | $\lesseqgtr 0$ |
| Price of second homes | $-\frac{\rho(-b - c(a, b, \epsilon, \eta, \rho, \beta, \gamma))}{(1 + \rho)c(a, b, \epsilon, \eta, \rho, \beta, \gamma)(1 + g_{s,c})}$ | $> 0$ |
| Number of investors | $\frac{b\rho}{c(a, b, \epsilon, \eta, \rho, \beta, \gamma)(1 + g_{s,c})}$ | $< 0$ |
| Price of tourism services | $-\frac{b\rho((-1 + \gamma)(1 + \rho) + \beta(1 - a + \eta + \rho + \eta\rho))}{(1 + \rho)c(a, b, \epsilon, \eta, \rho, \beta, \gamma)(1 + g_{s,c})}$ | $< 0$ |

The assumptions on our model's parameters are $\beta, \gamma, \rho > 0$ (output elasticities of input factors and housing supply are positive), $0 < a, b < 1$ (housing consumption of primary residents and investors are positive but housing does not consume their entire budget), $\eta, \epsilon < 0$ (primary residents and investors are subject to a disamenity effect caused by the presence of these latter), and $\beta + \gamma < 1$ (decreasing returns to scale).

These assumptions determine the sign of the impact of the ban on each outcome variable reported in the last column of Table W-C1 (see the Mathematica code for further details). In particular, we have that $c < 0$. This makes it trivial to show that the price of primary homes subject to the ban is lower than its counterfactual (point i) of Proposition 1), that wages are comparatively lower (point ii) of Proposition 1), and that the number of second home investors naturally decreases post-ban.

It is slightly less trivial to show the sign for the remaining outcome variables. Let us start with the price of second homes. We have that $\rho\big(-b - c(a, b, \epsilon, \eta, \rho, \beta, \gamma)\big) = \rho(1 - b)(1 - \beta - \gamma - \beta\eta)(1 + \rho) - \epsilon\rho(1 + \rho) + \rho(1 - b)\beta a > 0$, as each term of the sum is positive by assumption. The overall price effect is thus positive, which proves Proposition 2.

The effect of the ban on the number of primary residents is uncertain, as it depends on the magnitude of the parameter $\eta$ describing the dislike of primary residents for investors. If primary residents strongly dislike investors, the ban may succeed in attracting more new primary residents than in the counterfactual case due to the comparative increase in the endogenous amenity value of the municipality. On the other hand it's easy to show that if we let $\eta \to 0$ the effect of the ban on the number primary residents is unambiguously negative with respect to its counterfactual: while hurting the local economy, the ban provides no incentive for them to move into the municipality (point iii) of Proposition 1). The sign of the other endogenous variables is the same.

Finally, let us consider prices of tourism services. We have that $-b\rho\big((-1+\gamma)(1+\rho)-\beta(1-a+\eta+\rho+\eta\rho)\big)=-b\rho(-1+\beta+\gamma)(1+\rho)-b\rho\beta(-a+\eta+\eta\rho)>0$ as each term of the sum is positive. The overall price effect on tourism services is thus negative.

Note that the above comparative static results remain unchanged if we set $\epsilon=0$, i.e. if investors are indifferent to each other. This can easily be verified, as i) $\epsilon$ enters our system of equations only through $c$, which remains negative for $\epsilon=0$, and ii) every term of the numerator of second home prices treatment effect is positive: setting one of them equal to zero does not change the sign of the sum.

*Agglomeration economies and reverse effects*

In the previous sections we have assumed that no agglomeration economies were present and, in particular, that returns to scale at the aggregate level were decreasing. We now consider the case in which agglomeration economies are present, possibly leading to increasing returns to scale in the tourism sector. In particular, we investigate how agglomeration forces may reverse the predictions of Propositions 1 and 2. Following Glaser and Gottlieb (2009), the most straightforward way to introduce agglomeration economies in the model is to modify the aggregate production function as follows

$$Y_{it}=A_{it}\widetilde{N}_{it}^{\alpha}N_{it}^{\beta}K_{it}^{\gamma}\bar{Z}_{i}^{1-\beta-\gamma},\quad 0<\alpha,\beta,\gamma<1,\qquad \beta+\gamma<1,$$

where $\widetilde{N}_{it}^{\alpha}$ denotes an agglomeration term depending on the total number of primary residents (workers) in the municipality which increases total factor productivity. Importantly, this factor is treated as parametrically given to individual firms. We maintain the hypothesis of decreasing returns to scale in absence of agglomeration economies.

Deriving comparative static results when agglomeration economies are present is easy in our context. As the term $N_{it}^{\beta}$ is replaced by $N_{it}^{\alpha+\beta}$ in the industry first order conditions and noting that non-traded capital $\bar{Z}$ (the only other term involving the output elasticity $\beta$) drops out from the system of equations in changes, we can simply substitute $\beta$ with $\alpha+\beta$ in equations C1' and C6'. The new dynamic equilibrium is thus equal to the one in the absence of agglomeration economies with $\beta$ replaced with $\alpha+\beta$. The resulting comparative static results are shown in Table W-C2.

We now investigate whether the sign of the impact of the ban on primary homes may be reversed and the implications for the price of second homes. The starting point is to investigate when the sign of the constant $c$ is reversed by $\alpha$, i.e., when $c(a,b,\epsilon,\eta,\rho,\alpha+\beta,\gamma)>0$. One can show that

$$c(a,b,\epsilon,\eta,\rho,\alpha+\beta,\gamma)>0\ \Leftrightarrow\ (-1+b)\alpha\big(a-(1+\eta)(1+\rho)\big)>-c(a,b,\epsilon,\eta,\rho,\beta,\gamma).$$

Let $\bar{\alpha}:=\frac{-c(a,b,\epsilon,\eta,\rho,\beta,\gamma)}{(-1+b)\left(a-(1+\eta)(1+\rho)\right)}$ denote a threshold value of agglomeration economies. This leads to the conditions

$$\alpha>\bar{\alpha}\ \text{ if }\ a-(1+\eta)(1+\rho)<0 \qquad \text{(Case 1)}$$

$$\alpha<\bar{\alpha}\ \text{ if }\ a-(1+\eta)(1+\rho)>0. \qquad \text{(Case 2)}$$

Case 2 can easily be dismissed, as it implies negative values of $\alpha$. In fact, from the previous section we know that $c(a,b,\epsilon,\eta,\rho,\beta,\gamma) < 0$. If $a-(1+\eta)(1+\rho) > 0$ this would imply a negative threshold $\bar{\alpha}$. As the agglomeration parameter $\alpha$ is assumed to be positive, we discard Case 2. This implies that the effect of the ban on the price of primary homes (and on wages, and the number of second home investors) is reversed only if the agglomeration economies are strong enough. Interestingly, the threshold $\bar{\alpha}$ decreases with $\eta$: the more primary residents (comparatively) benefit from the ban, the weaker the agglomeration forces must be to create a positive effect of the ban on the price of primary homes.

TABLE W-C2

Treatment effects with agglomeration economies

| **Outcome variable** | **Comparative static treatment effect** |
|---|---|
| Wages | $-\frac{b\rho(-a+\eta+\eta\rho)}{(1+\rho)c(a,b,\epsilon,\eta,\rho,\alpha+\beta,\gamma)(1+g_{s,c})}$ |
| Price of primary homes | $\frac{b\rho}{(1+\rho)c(a,b,\epsilon,\eta,\rho,\alpha+\beta,\gamma)(1+g_{s,c})}$ |
| Number of primary residents | $\frac{b\rho(1-a+\eta+\rho+\eta\rho)}{(1+\rho)c(a,b,\epsilon,\eta,\rho,\alpha+\beta,\gamma)(1+g_{s,c})}$ |
| Price of second homes | $-\frac{\rho(-b-c(a,b,\epsilon,\eta,\rho,\alpha+\beta,\gamma))}{(1+\rho)c(a,b,\epsilon,\eta,\rho,\alpha+\beta,\gamma)(1+g_{s,c})}$ |
| Number of investors | $\frac{b\rho}{c(a,b,\epsilon,\eta,\rho,\alpha+\beta,\gamma)(1+g_{s,c})}$ |
| Price of tourism services | $-\frac{b\rho((-1+\gamma)(1+\rho)+(\alpha+\beta)(1-a+\eta+\rho+\eta\rho))}{(1+\rho)c(a,b,\epsilon,\eta,\rho,\alpha+\beta,\gamma)(1+g_{s,c})}$ |

Let us now consider the effect of the ban on the price of second homes when the effect on the price of primary homes is reversed, i.e. when $\alpha > \bar{\alpha}$. The sign of the effect is reversed if $-\rho\big(-b-c(a,b,\epsilon,\eta,\rho,\alpha+\beta,\gamma)\big) < 0$. One can show that

$$-\rho\big(-b-c(a,b,\epsilon,\eta,\rho,\alpha+\beta,\gamma)\big) < 0 \iff \alpha < -\frac{b+c(a,b,\epsilon,\eta,\rho,\beta,\gamma)}{(-1+b)\big(a-(1+\eta)(1+\rho)\big)} =: \bar{\alpha}'.$$

However, as $\bar{\alpha}' = \bar{\alpha} - \frac{b}{(-1+b)\big(a-(1+\eta)(1+\rho)\big)}$, we have that $\bar{\alpha}' < \bar{\alpha}$. Therefore, it is not possible to reverse the price effect on second homes if it is already reversed for primary ones. In other words, in the presence of strong agglomeration economies causing the ban to comparatively increase the price of primary homes, the price of second homes must also be comparatively higher.

*C.2 Simulation*

Figure W-C1 provides simulation graphs on the comparative static predictions with and without agglomeration economies. Different treatment effects corresponding to several agglomeration parameters are represented as a function of the disamenity parameter $\eta$ of primary residents. In particular, we show that for $\alpha$ above a given value, the effect of the ban is reversed. To this end, we calibrate our model as follows:

$a = 0.3,\ b = 0.15,\ \rho = 1,\ \beta = 0.7,\ \gamma = 0.2,\ g_c^S = 0.01.$

The share of housing consumption for primary residents corresponds to rough rule of thumb used by mortgage lenders to finance house purchases. We assume second home investors spend half of that share for their secondary residences. To simplify we assume a linear housing supply function. The assumed output elasticities' values are standard in the literature. Growth of construction costs of second homes is arbitrarily assumed to increase 1% from one period to another. Finally, we assume that investors are less negatively affected by their own presence and set $\epsilon = 0.5\eta$. The considered values of the agglomeration parameter $\alpha$ are 0 (decreasing returns to scale), 0.1 (constant returns to scale), 0.2 (increasing returns to scale but below the reverse threshold), 0.5 (increasing returns to scale and above the reverse threshold).

FIGURE W-C1

Simulation results – Agglomeration economies and reversed effects

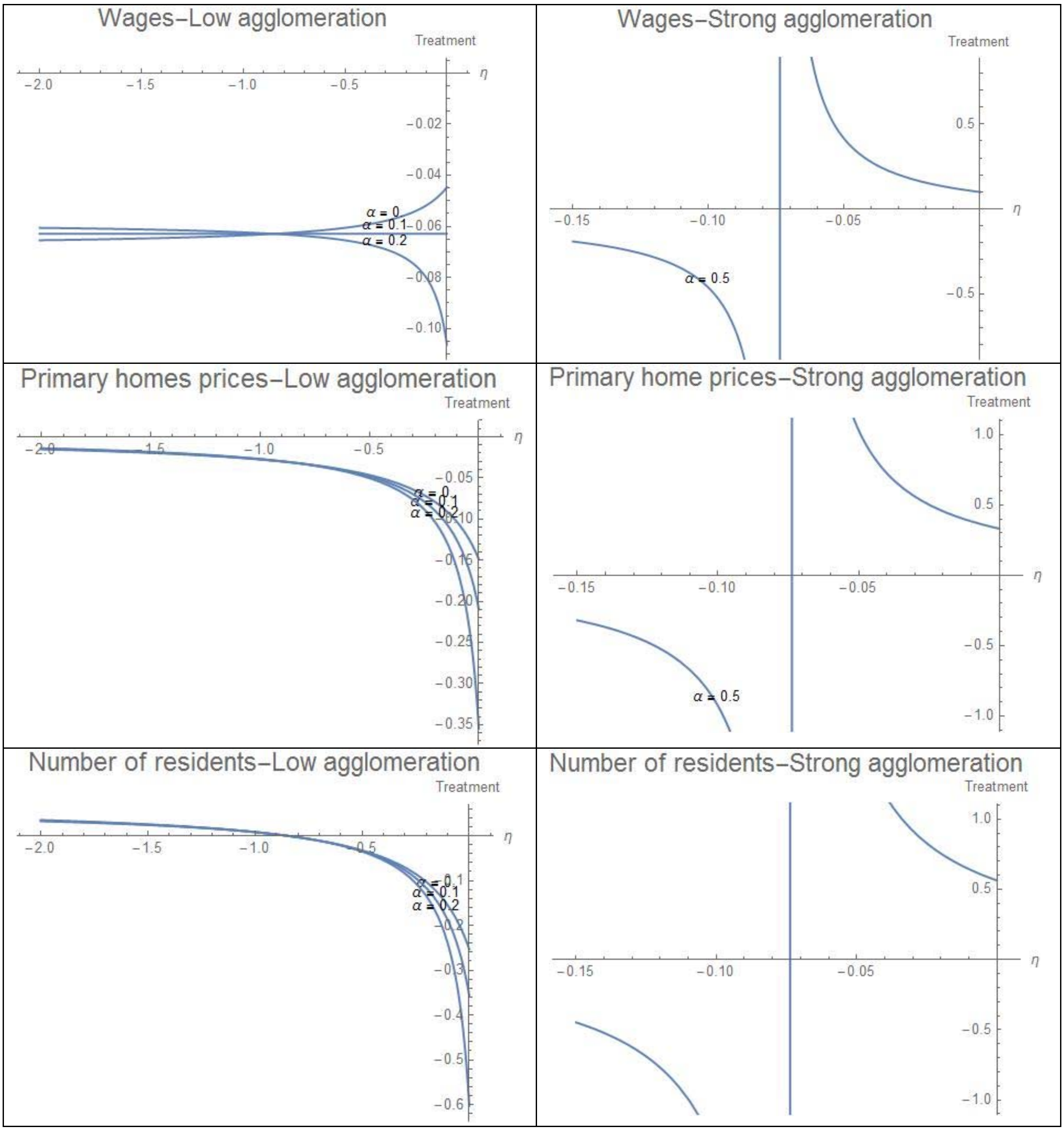
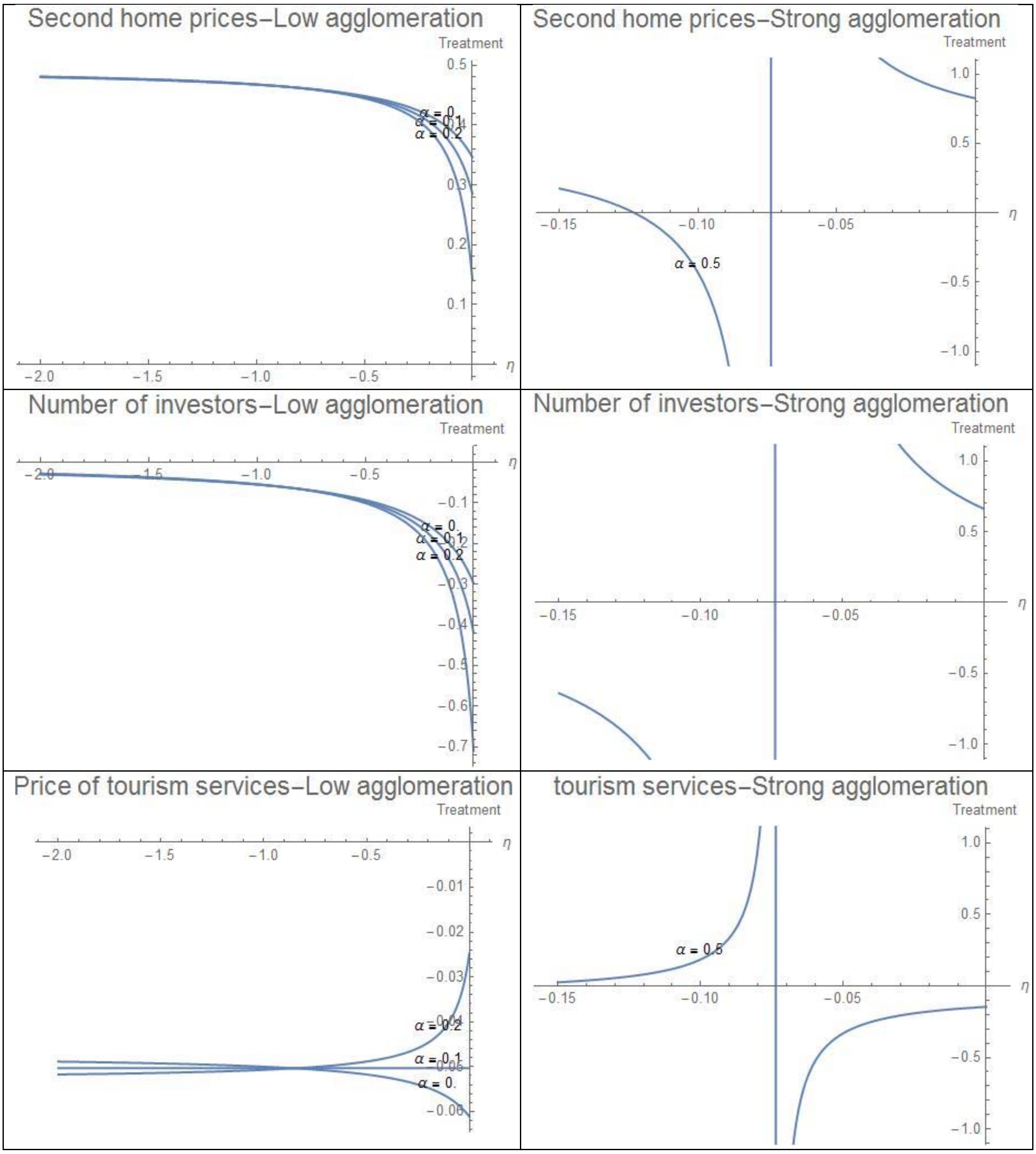


FIGURE W-C1 *(cont.)*




The above graphs show how investors' dislike and returns to scale affect the impact of the ban on the endogenous variables of the system. It can be seen that for the considered calibration the ban effects are reversed when the agglomeration parameter $\alpha$ is above a given threshold (right hand side graphs). This threshold is apparently extremely high for the considered calibration – for $\alpha = 0.2$ the ban effects remain stable – and it seems plausible to assume that in the real world agglomeration forces are not that strong. We thus discuss only left hand side graphs in detail.

In line with Proposition 1, the policy effect is unambiguously negative (resp. positive) for primary (resp. secondary) residences and local labor markets. Interestingly, we can see how returns to scale of local tourism industries magnify or decrease the effect of the ban on local economies depending on its effect on the number of residents. For example, if primary residents don't dislike investors much – and their number is comparatively lower post ban – the wage effect of the regulation will be more negative in the case of increasing returns to scale ($\alpha = 0.2$) than for constant or decreasing ones ($\alpha = 0, 0.1$). The opposite is true for the price of tourism services. On the other hand, if primary residents strongly dislike investors – and their number is comparatively higher after the ban – the negative wage (price of tourism services) effect for decreasing returns to scale will be stronger (weaker) than in the case of increasing return to scale.

# Web-Appendix D: Detailed description of data and sources

The present appendix contains detailed information on the sources and definitions of the data used in the paper. Web links to data sources are provided at the end of the section in Table W-D5.

*Housing transaction data*

Individual transaction data has been provided by the Swiss Real Estate Datapool Association (SRED). The proprietary data can be obtained against payment from the association, see reference [1] below. Table W-D1 reports the definition of the variables used in the empirical part before being aggregated at the municipality level over given time periods or used to subset the data.

TABLE W-D1
Description of housing characteristics and data sources

| **Variable name** | **Description** | **Values** |
|---|---|---|
| Number of rooms | Self-explanatory. To aggregate. | 1, 2, 3… |
| Number of bathrooms | Self-explanatory. To aggregate. | 1, 2, 3… |
| Number of parking places | Self-explanatory. To aggregate. | 1, 2, 3… |
| Quality | The property standard: bad, average, good, very good. To aggregate. | 1, 2, 3, 4 |
| Condition | The property condition: bad, average, good, very good. It implicitly describes whether the property needs major renovations. To aggregate. | 1, 2, 3, 4 |
| Micro-location | The micro-location of the property inside the municipality: bad, average, good, very good. It depends, for example, whether the property has an open view, is situated in a spot with a lot of sun hours, etc. To aggregate. | 1, 2, 3, 4 |
| Age | Age of the property at the moment of the transaction. Has been computed by subtracting from the transaction year the year in which the property has been built. To aggregate. Negative values represent properties having been sold before being constructed. | …,-2, -1, 0, 1, 2, 3… |
| House type | House versus flat indicator. To aggregate. | 0,1 |
| Primary | Primary versus secondary residence indicator. Used to subset the data. | 0,1 |
| Municipality | FSO identifier for municipalities. More detailed information is available at [2]. Used to compute geographic distances (see below). | 1, 2, 3… |
| Canton | FSO identifier for cantons. More detailed information is available at [5]. Used as categorical variable. | 1, 2, 3…,26 |

*Second home rates*

The text of the SHI ordinance, as well as the methodology used to measure municipalities' second home rates are available on the website of the Federal Office for Spatial Development (ARE), see [6]. ARE computes second home rates as total housing stock less primary residences, which may overestimate the second home number in some municipalities, since not all housing units that are not primary homes are necessarily second homes. However, the ordinance was applied according to this approximated measure, independently of a municipality's "true" second home rate.

When the draft of the ordinance – that listed all affected (treated) municipalities – was made public in August 2012 – municipalities were allowed to request a revision of their second home rate if they could document that the one published by the ARE was incorrect. Municipalities that opted to propose a revision of their second home rate did not have to comply with the restriction imposed by the initiative. Only about 6% of Swiss municipalities requested a revision of their second home rate and all of them were able to provide proof that their second home rate was indeed below 20%. ARE continues to systematically verify and update the second home rate of all municipalities.

ARE points out that a comparison of the Federal Population Census of 2000 and the Federal Register of Buildings and Dwellings reveals only minor differences between the two data sets, in the sense that the classification of municipalities into below and above 20% second homes does not vary too much across the two data sets.

*Municipality-level characteristics*

Data on municipality-level characteristics are freely provided by the Federal Statistical Office (FSO). The indicators used in the present paper can be directly downloaded using the interactive statistical atlas of Switzerland – available only in French and German – see [7]. Table W-D2 describes the considered variables and the corresponding data sources. When necessary, we provide additional information on how data were computed.

The share of undevelopable land has been computed using land use data measured from 2004 to 2009. This time interval corresponds to the time necessary to take areal pictures by overflying the whole country's territory. More up-to-date measurements are presently underway and will be available in 2018. The FSO classifies municipalities' surface into four main categories: urban, wood, agriculture, and unproductive surfaces. This latter category mainly corresponds to lakes, rivers, glaciers, and bedrock surfaces. Additional information on the methodology used to measure and classify land surfaces is available at [9].

Distances to major city centers and ski resorts have been computed using GIS data provided by the Federal Office of Topography, see [10]. Geographic boundaries updated to 2014 were used. In particular, distances were computed as the minimal planar distance between the two closest points of the considered municipalities' boundaries. For example, if a municipality is adjacent to a major urban center/ski resort, the corresponding distance is equal to zero. The 15 major urban centers were identified using FSO information on major agglomerations, see [11]. Table W-D3 contains a list of the major CBDs we used in our analysis.

TABLE W-D2

Description of municipalities' characteristics and data sources

| Variable name | Description | Values |
|---|---|---|
| Vote No | Share of voters having rejected the SHI on the 11 March 2012. Provided by the FSO, see [8]. | [0,1] |
| Unproductive surface | Surface of lakes, mountains, glaciers, etc. present in a municipality. Provided by the FSO, see [7]. See below for further details. | [0,1] |
| Distance to major city | Distance to one of the 15 major urban centers of Switzerland. See below for further details. | km |
| Distance to major ski resort | Distance to one of the 53 major ski resorts of Switzerland. See below for further details. | km |
| Percentage working in 3rd sector | Share of firms and individuals working in the third sector. Provided by the FSO, see [7] | [0,1] |

The 52 major ski resorts were identified using Google results obtained by searching 'Switzerland + ski resorts', to which we added the municipalities of Ste Croix, St Cergue, and Le Lieu to represent ski resorts belonging to the district of Jura-Nord Vaudois. Table W-D4 contains the list of the considered ski resorts. Some of the considered ski resorts belong to the same municipality and thus have the same FSO identification number.

TABLE W-D3

Major urban centers (individual municipalities)

| FSO number | City Name | FSO number | City Name |
|---|---|---|---|
| 261 | Zürich | 230 | Winterthur |
| 6621 | Genf | 1711 | Zug |
| 2701 | Basel | 4021 | Baden |
| 351 | Bern | 371 | Biel |
| 5586 | Lausanne | 2196 | Fribourg |
| 1061 | Luzern | 2581 | Olten |
| 3203 | St. Gallen | 6458 | Neuchatel |
| 5192 | Lugano | | |

TABLE W-D4

Major ski resorts (individual municipalities)

| **FSO number** | **City Name** | **FSO number** | **City Name** |
|---|---|---|---|
| 1202 | Andermatt | 3612 | Obersaxen |
| 6031 | Verbier | 6139 | La Tzoumaz |
| 3851 | Davos | 3539 | Savognin |
| 5409 | Villars-sur-Ollon | 6252 | Zinal |
| 584 | Mürren | 6252 | Grimentz |
| 6300 | Zermatt | 3982 | Disentis |
| 584 | Wengen | 1631 | Elm |
| 3575 | Laax | 1004 | Flühli |
| 6243 | Crans-Montana | 5411 | Les Diablerets |
| 6290 | Saas-Fee | 6151 | Champéry |
| 1402 | Engelberg | 6285 | Grächen |
| 3787 | St. Moritz | 5061 | Airolo |
| 3871 | Kloster-Serneus | 6252 | Saint-Luc |
| 3921 | Arosa | 6252 | Chandolin |
| 6024 | Nendaz | 6193 | Bürchen |
| 561 | Adelboden | 3981 | Brigels |
| 3506 | Lenzerheide | 6135 | Ovronnaz |
| 576 | Grindelwald | 1501 | Beckenried |
| 3752 | Samnau | 794 | Zweisimmen |
| 5407 | Leysin | 6111 | Leukerbad |
| 3732 | Flims | 6156 | Morgins |
| 783 | Hasliberg | 584 | Mürren |
| 3357 | Wildhaus | 3311 | Amden |
| 3986 | Tujetsch | 5568 | Ste Croix |
| 792 | Lenk im Simmental | 5727 | St. Cergue |
| 3762 | Scuol | 5873 | Le Lieu |
| 6082 | Anzère | | |

*Fiscal data*

Data on municipalities' fiscal data are freely available on the website of the Swiss Federal Tax Administration (FTA), see [12]. Based on individuals liable to pay the Federal Tax, we used the average net income and the corresponding Gini index at the municipality level computed including both married and not married individuals. We supplemented this data by adding the share of foreign residents available at [7].

*Web references*

TABLE W-D5
Web references and links

| Reference | Link |
|---|---|
| [1] | http://www.sred.ch/ |
| [2] | http://www.bfs.admin.ch/bfs/portal/de/index/infothek/nomenklaturen/blank/blank/gem_liste/03.html |
| [3] | http://www.bfs.admin.ch/bfs/portal/de/index/infothek/nomenklaturen/blank/blank/gemtyp/01.html |
| [4] | http://www.bfs.admin.ch/bfs/portal/de/index/regionen/11/geo/raeumliche_typologien/01.html |
| [5] | http://www.bfs.admin.ch/bfs/portal/en/index/regionen/thematische_karten/maps/raumgliederung/institutionelle_gliederungen.parsys.0002.PhotogalleryDownloadFile2.tmp/k00.22s.pdf |
| [6] | http://www.are.admin.ch/themen/raumplanung/00236/04094/index.html?lang=fr |
| [7] | http://www.bfs.admin.ch/bfs/portal/en/index/regionen/thematische_karten/02.html |
| [8] | http://www.bfs.admin.ch/bfs/portal/de/index/themen/17/03/blank/key/2012/011.html |
| [9] | http://www.bfs.admin.ch/bfs/portal/fr/index/themen/02/03.html |
| [10] | https://shop.swisstopo.admin.ch/fr/products/landscape/boundaries3D |
| [11] | http://www.bfs.admin.ch/bfs/portal/fr/index/themen/01/02/blank/key/raeumliche_verteilung/agglomerationen.html |
| [12] | https://www.estv.admin.ch/estv/de/home/allgemein/dokumentation/zahlen-und-fakten/steuerstatistiken/direkte-bundessteuer.html |

## Web-Appendix E: Robustness Checks and Detailed Estimation Results

TABLE W-E1

FD-IV estimates: Standard errors clustered at cantonal level

| Panel (a): TSLS: *Second stage* | | | | | | |
|---|---|---|---|---|---|---|
| Dependent variable | Δ Log price of primary homes | | | Δ Log unemployment rate | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Observed treatment | -0.152*** | -0.147*** | -0.190*** | 0.121*** | 0.118*** | 0.111*** |
| | (0.0549) | (0.0518) | (0.0633) | (0.0336) | (0.0334) | (0.0325) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |
| Observations | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 |
| Kleibergen-Paap F | 870.2 | 981 | 755.7 | 870.2 | 981 | 897.9 |

| Panel (b): TSLS: *First stage* | | | | | | |
|---|---|---|---|---|---|---|
| Dependent variable | Observed treatment | | | | | |
| Second home rates in 2000 | 2.066*** | 2.068*** | 2.043*** | 2.066*** | 2.068*** | 2.067*** |
| | (0.0700) | (0.0660) | (0.0743) | (0.0700) | (0.0660) | (0.0690) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |

*Notes:* Standard errors clustered at the cantonal level are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on a given outcome variable for a given set of controls. Municipalities that have missing values for a given set of controls are excluded from all specifications. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages in these periods. The sample includes municipalities for which housing transactions were available pre and post the implementation of the SHI. Houses built after 2012, which no longer have a conversion option, have been excluded from the sample before aggregation. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000.

TABLE W-E2

DD estimates

| Dependent variable | Log price of primary homes | | | Log unemployment rate | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Observed treatment × Post | -0.142** | -0.152*** | -0.119*** | 0.0787 | 0.0823* | 0.0969** |
| | (0.0571) | (0.0450) | (0.0456) | (0.0602) | (0.0428) | (0.0396) |
| Time fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| FE and lagged controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |
| Observations | 2,812 | 2,812 | 2,812 | 2,812 | 2,812 | 2,812 |
| R-squared | 0.054 | 0.571 | 0.577 | 0.001 | 0.670 | 0.693 |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on a given outcome variable for a given set of controls. Municipalities that have missing values for a given set of controls are excluded from all specifications. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The final sample pools data on municipalities for which housing transactions were available pre and post the implementation of the SHI. Houses built after 2012, which no longer have a conversion option, have been excluded from the sample before aggregation.

TABLE W-E3

FD estimates

| Dependent variable | Δ Log price of primary homes | | | Δ Log unemployment rate | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Observed treatment | -0.142*** | -0.140*** | -0.191*** | 0.0787*** | 0.0757*** | 0.0651*** |
| | (0.0386) | (0.0376) | (0.0365) | (0.0231) | (0.0236) | (0.0230) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |
| Observations | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 |
| R-squared | 0.020 | 0.128 | 0.196 | 0.012 | 0.023 | 0.122 |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on a given outcome variable for a given set of controls. Municipalities that have missing values for a given set of controls are excluded from all specifications. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The sample includes municipalities for which housing transactions were available pre and post the implementation of the SHI. Houses built after 2012, which no longer have a conversion option, have been excluded from the sample before aggregation.

TABLE W-E4

FD covariates balance

| | Control<br>- | Treated<br>- | Control<br>CBD>10 km & Ski>0 km | Treated | Control<br>CBD>10 km & 15%-30% | Treated | p-values | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (1) vs. (2) | (3) vs. (4) | (5) vs. (6) |
| No. Observations | 1,230 | 176 | 446 | 56 | 107 | 22 | - | - | - |
| $\log(y_{10-11})$ | | | | | | | | | |
| Price of primary homes | 6.56 | 6.34 | 6.49 | 6.27 | 6.44 | 6.42 | **0.00** | **0.00** | 0.87 |
| Unemployment rate | -4.36 | -4.42 | -4.40 | -4.39 | -4.31 | -4.31 | 0.10 | 0.89 | 0.99 |
| $\Delta x_{10-11}$ | | | | | | | | | |
| No. of rooms | -0.07 | -0.05 | -0.10 | -0.09 | -0.12 | 0.00 | 0.75 | 0.96 | 0.64 |
| No. of bathrooms | 0.02 | 0.05 | 0.00 | 0.09 | 0.06 | 0.08 | 0.44 | 0.22 | 0.88 |
| No. of park places | -0.03 | 0.08 | -0.03 | 0.07 | -0.09 | 0.09 | **0.02** | 0.28 | 0.27 |
| Quality | 0.23 | 0.22 | 0.26 | 0.17 | 0.30 | 0.46 | 0.77 | 0.34 | 0.40 |
| Condition | -0.03 | 0.00 | -0.01 | -0.04 | 0.03 | 0.33 | 0.49 | 0.76 | 0.10 |
| Micro location | 0.08 | 0.05 | 0.07 | 0.09 | 0.04 | 0.09 | 0.48 | 0.74 | 0.72 |
| Age | 1.25 | -0.05 | 0.46 | -1.90 | -5.02 | 0.27 | 0.52 | 0.57 | 0.51 |
| House | -0.01 | -0.00 | -0.03 | -0.08 | -0.01 | -0.04 | 0.69 | 0.30 | 0.78 |
| Average net income | 1.06 | 1.00 | 0.91 | 1.13 | 1.20 | 1.00 | 0.93 | 0.64 | 0.80 |
| Gini net income | 0.00 | 0.01 | 0.00 | 0.01 | 0.01 | 0.01 | **0.04** | 0.33 | 0.36 |
| No. transactions | -0.43 | -0.16 | -0.14 | -0.46 | -0.14 | -0.91 | 0.75 | 0.74 | 0.65 |
| Foreign share | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 | 0.76 | 0.41 | 0.10 |
| No. of new residences | 2.84 | -0.27 | 2.87 | 1.22 | 5.11 | 8.00 | 0.31 | 0.66 | 0.68 |

*Notes:* Columns (1) to (6) report the means of the outcome variables and controls used in Table 2 (Panel A) for the full sample of municipalities (columns 1-2), when municipalities within 10 km from major CBDs or adjacent to major ski resorts are dropped (columns 3-4), and when municipalities within 10 km from major CBDs and with a second home rate outside the [0.15, 0.3] interval are excluded. The last three columns report p-values for the test of difference in means between control and treated group according to the considered sample. The p-values lower than 0.1 are marked in bold.

TABLE W-E5
FD-IV estimates: Restricted Sample 1
(Excluding municipalities near major CBDs and ski resorts)

| Panel (a): TSLS: *Second stage* | | | | | | |
|---|---|---|---|---|---|---|
| Dependent variable | Δ Log price of primary homes | | | Δ Log unemployment rate | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Observed treatment | -0.172** | -0.195*** | -0.237*** | 0.0962* | 0.0931* | 0.105* |
| | (0.0734) | (0.0703) | (0.0661) | (0.0568) | (0.0546) | (0.0563) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |
| Observations | 502 | 502 | 502 | 502 | 502 | 502 |
| Kleibergen-Paap F | 536.8 | 524.9 | 517.4 | 536.8 | 524.9 | 520 |
| Panel (b): TSLS: *First stage* | | | | | | |
| Dependent variable | Observed treatment | | | | | |
| Second home rates in 2000 | 2.150*** | 2.173*** | 2.146*** | 2.150*** | 2.173*** | 2.175*** |
| | (0.0928) | (0.0949) | (0.0943) | (0.0928) | (0.0949) | (0.0954) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on a given outcome variable for a given set of controls. Municipalities that have missing values for a given set of controls are excluded from all specifications. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The sample includes municipalities for which housing transactions were available pre and post the implementation of the SHI. Houses built after 2012, which no longer have a conversion option, have been excluded from the sample before aggregation. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000. Municipalities within 10 km from major CBDs or adjacent to major ski resorts are dropped.

TABLE W-E6
FD-IV estimates: Restricted Sample 2
(Excluding municipalities near major CBDs and/or having a 2nd home rate below 15% or above 30%)

| Panel (a): TSLS: *Second stage* | | | | | | |
|---|---|---|---|---|---|---|
| Dependent variable | Δ Log price of primary homes | | | Δ Log unemployment rate | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Observed treatment | -0.561*** | -0.370** | -0.353** | 0.243* | 0.291** | 0.251** |
| | (0.169) | (0.149) | (0.149) | (0.125) | (0.116) | (0.105) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |
| Observations | 129 | 129 | 129 | 129 | 129 | 129 |
| Kleibergen-Paap F | 35.02 | 38.55 | 37.71 | 35.02 | 38.55 | 37.01 |
| Panel (b): TSLS: *First stage* | | | | | | |
| Dependent variable | Observed treatment | | | | | |
| Second home rates in 2000 | 2.689*** | 2.848*** | 2.868*** | 2.689*** | 2.848*** | 2.852*** |
| | (0.454) | (0.459) | (0.467) | (0.454) | (0.459) | (0.469) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on a given outcome variable for a given set of controls. Municipalities that have missing values for a given set of controls are excluded from all specifications. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The sample includes municipalities for which housing transactions were available pre and post the implementation of the SHI. Houses built after 2012, which no longer have a conversion option, have been excluded from the sample before aggregation. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000. Municipalities within 10 km from major CBDs and/or having a second home rate outside the [0.15, 0.3] interval are dropped.

TABLE W-E7

FD-IV estimates: Excluding close to treated (5km)

| Panel (a): TSLS: *Second stage* | | | | | | |
|---|---|---|---|---|---|---|
| Dependent variable | Δ Log price of primary homes | | | Δ Log unemployment rate | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Observed treatment | -0.148*** | -0.142*** | -0.191*** | 0.113*** | 0.112*** | 0.105*** |
| | (0.0459) | (0.0441) | (0.0441) | (0.0250) | (0.0251) | (0.0248) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |
| Observations | 1,027 | 1,027 | 1,027 | 1,027 | 1,027 | 1,027 |
| Kleibergen-Paap F | 1385 | 1375 | 1350 | 1385 | 1375 | 1374 |

| Panel (b): TSLS: *First stage* | | | | | | |
|---|---|---|---|---|---|---|
| Dependent variable | Observed treatment | | | | | |
| Second home rates in 2000 | 2.130*** | 2.128*** | 2.079*** | 2.130*** | 2.128*** | 2.126*** |
| | (0.0572) | (0.0574) | (0.0566) | (0.0572) | (0.0574) | (0.0573) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on a given outcome variable for a given set of controls. Municipalities that have missing values for a given set of controls are excluded from all specifications. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The sample includes municipalities for which housing transactions were available pre and post the implementation of the SHI. Houses built after 2012, which no longer have a conversion option, have been excluded from the sample before aggregation. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000.

TABLE W-E8

FD-IV estimates: Total effect when including residences built after 2012

| Panel (a): TSLS: *Second stage* | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Dependent variable | Δ Log price of primary homes | | | | | | | | |
| | Full sample | | | CBD >10 km & Ski>0 km | | | CBD >10 km & [0.15,0.3] | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Observed treatment | -0.135*** | -0.130*** | -0.180*** | -0.123* | -0.143** | -0.188*** | -0.514*** | -0.328** | -0.292* |
| | (0.0441) | (0.0430) | (0.0426) | (0.0698) | (0.0652) | (0.0611) | (0.176) | (0.150) | (0.150) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |
| Observations | 1,454 | 1,454 | 1,454 | 525 | 525 | 525 | 134 | 134 | 134 |
| Kleibergen-Paap F | 1684 | 1676 | 1667 | 568.2 | 556.9 | 548.8 | 32.12 | 36.73 | 36.27 |
| Panel (b): TSLS: *First stage* | | | | | | | | | |
| Dependent variable | Observed treatment | | | | | | | | |
| Second home rates in 2000 | 2.041*** | 2.043*** | 2.019*** | 2.142*** | 2.168*** | 2.142*** | 2.558*** | 2.739*** | 2.772*** |
| | (0.0497) | (0.0499) | (0.0494) | (0.0898) | (0.0919) | (0.0914) | (0.451) | (0.452) | (0.460) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on first-differenced log-prices of primary residences for a given set of controls and for three different samples. The considered samples are the full sample of Tables 2-4, and the restricted samples of Tables 6 and 7, respectively. Municipalities that have missing values for a given set of controls are excluded from all specifications. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The sample includes municipalities for which housing transactions were available pre and post the implementation of the SHI. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000.

TABLE W-E9

FD-IV estimates: New constructions regressions

| Panel (a): TSLS: *Second stage* | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Dependent variable | Δ Log number new housing units | | | | | | | | |
| | Full sample | | | CBD >10 km & Ski>0 km | | | CBD >10 km & [0.15,0.3] | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Observed treatment | -0.187* | -0.197* | -0.231** | -0.283 | -0.317 | -0.426** | -0.554 | -0.630 | -0.555 |
| | (0.107) | (0.107) | (0.101) | (0.207) | (0.212) | (0.196) | (0.448) | (0.406) | (0.373) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |
| Observations | 1,330 | 1,330 | 1,330 | 475 | 475 | 475 | 122 | 122 | 122 |
| Kleibergen-Paap F | 1574 | 1561 | 1563 | 542.5 | 522.7 | 516.8 | 36.50 | 42.52 | 42.18 |
| Panel (b): TSLS: *First stage* | | | | | | | | | |
| Dependent variable | Observed treatment | | | | | | | | |
| Second home rates in 2000 | 2.053*** | 2.052*** | 2.050*** | 2.134*** | 2.143*** | 2.137*** | 2.790*** | 2.935*** | 2.956*** |
| | (0.0518) | (0.0519) | (0.0519) | (0.0916) | (0.0937) | (0.0940) | (0.462) | (0.450) | (0.455) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on the first-differenced log-new residential construction (in number of units) for a given set of controls and for three different samples. The considered samples are the full sample of Tables 2-4, and the restricted samples of Tables 6 and 7, respectively. Municipalities that have missing values for a given set of controls are excluded from all specifications. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The sample includes municipalities for which housing transactions were available pre and post the implementation of the SHI. Houses built after 2012, which no longer have a conversion option, have been excluded from the sample before aggregation. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000.

NEW TABLE W-E10

FD-IV estimates: Elderly regressions

| Panel (a): TSLS: *Second stage* | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Dependent variable | Δ Log elderly | | | | | | | | |
| | Full sample | | | CBD >10 km & Ski>0 km | | | CBD >10 km & [0.15,0.3] | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Observed treatment | 0.00246 | 0.00322 | -0.00205 | 0.0144 | 0.0174 | 0.0145 | 0.0197 | 0.0279 | 0.0265 |
| | (0.00839) | (0.00840) | (0.00849) | (0.0184) | (0.0181) | (0.0181) | (0.0283) | (0.0305) | (0.0303) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |
| Observations | 1,406 | 1,406 | 1,406 | 502 | 502 | 502 | 129 | 129 | 129 |
| Kleibergen-Paap F | 1623 | 1619 | 1627 | 536.8 | 524.9 | 526.7 | 35.02 | 38.55 | 37.15 |
| Panel (b): TSLS: *First stage* | | | | | | | | | |
| Dependent variable | Observed treatment | | | | | | | | |
| Second home rates in 2000 | 2.066*** | 2.068*** | 2.063*** | 2.150*** | 2.173*** | 2.171*** | 2.689*** | 2.848*** | 2.814*** |
| | (0.0513) | (0.0514) | (0.0512) | (0.0928) | (0.0949) | (0.0946) | (0.454) | (0.459) | (0.462) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on the first-differenced log-number of elderly residents (65 years or older) for a given set of controls and for three different samples. The considered samples are the full sample of Tables 2-4, and the restricted samples of Tables 6 and 7, respectively. Municipalities that have missing values for a given set of controls are excluded from all specifications. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The sample includes municipalities for which housing transactions were available pre and post the implementation of the SHI. Houses built after 2012, which no longer have a conversion option, have been excluded from the sample before aggregation. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000.

TABLE W-E11

FD-IV estimates: Sorting of permanent residents

| Panel (a): TSLS: *Second stage* | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Dependent variable | Δ Log population | | | | | | | | |
| | Full sample | | | CBD >10 km & Ski>0 km | | | CBD >10 km & [0.15,0.3] | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Observed treatment | -0.00911 | -0.00797 | -0.00932 | -0.00298 | -0.000259 | -0.00158 | 0.0182 | 0.0265 | 0.0261 |
| | (0.00654) | (0.00650) | (0.00669) | (0.0150) | (0.0149) | (0.0153) | (0.0237) | (0.0206) | (0.0210) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |
| Observations | 1,406 | 1,406 | 1,406 | 502 | 502 | 502 | 129 | 129 | 129 |
| Kleibergen-Paap F | 1623 | 1619 | 1626 | 536.8 | 524.9 | 523.8 | 35.02 | 38.55 | 37.68 |
| Panel (b): TSLS: *First stage* | | | | | | | | | |
| Dependent variable | Observed treatment | | | | | | | | |
| Second home rates in 2000 | 2.066*** | 2.068*** | 2.052*** | 2.150*** | 2.173*** | 2.160*** | 2.689*** | 2.848*** | 2.817*** |
| | (0.0513) | (0.0514) | (0.0509) | (0.0928) | (0.0949) | (0.0944) | (0.454) | (0.459) | (0.459) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on the first-differenced log-population for a given set of controls and for three different samples. The considered samples are the full sample of Tables 2-4, and the restricted samples of Tables 6 and 7, respectively. Municipalities that have missing values for a given set of controls are excluded from all specifications. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The sample includes municipalities for which housing transactions were available pre and post the implementation of the SHI. Houses built after 2012, which no longer have a conversion option, have been excluded from the sample before aggregation. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000.

TABLE W-E12

FD-IV estimates: Wage regressions

| Panel (a): TSLS: *Second stage* | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Dependent variable | Δ Log employee wages | | | | | | | | |
| | Full sample | | | CBD >10 km & Ski>0 km | | | CBD >10 km & [0.15,0.3] | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Observed treatment | 0.0124*** | 0.0137*** | 0.00612 | 0.00533 | 0.00610 | 0.00173 | -0.0206 | -0.0160 | -0.0186 |
| | (0.00380) | (0.00380) | (0.00419) | (0.00646) | (0.00625) | (0.00665) | (0.0174) | (0.0145) | (0.0143) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |
| Observations | 1,406 | 1,406 | 1,406 | 502 | 502 | 502 | 129 | 129 | 129 |
| Kleibergen-Paap F | 1623 | 1619 | 1553 | 536.8 | 524.9 | 526.2 | 35.02 | 38.55 | 37.92 |
| Panel (b): TSLS: *First stage* | | | | | | | | | |
| Dependent variable | Observed treatment | | | | | | | | |
| Second home rates in 2000 | 2.066*** | 2.068*** | 2.017*** | 2.150*** | 2.173*** | 2.120*** | 2.689*** | 2.848*** | 2.819*** |
| | (0.0513) | (0.0514) | (0.0512) | (0.0928) | (0.0949) | (0.0924) | (0.454) | (0.459) | (0.458) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |

*Notes:* Heteroscedastic-robust standard errors are reported in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on the first-differenced log-wages of employees for a given set of controls and for three different samples. The considered samples are the full sample of Tables 2-4, and the restricted samples of Tables 6 and 7, respectively. Municipalities that have missing values for a given set of controls are excluded from all specifications. The two-period analysis is carried out by dividing the data into pre (2010-2011) and post (2013-2014) approval of the SHI. We consider an additional pre period (2008-2009) to include the lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages for these periods. The sample includes municipalities for which housing transactions were available pre and post the implementation of the SHI. Houses built after 2012, which no longer have a conversion option, have been excluded from the sample before aggregation. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000.

## Web-Appendix F: Additional analysis

*F.1 Additional pre-trend analysis*

To investigate pre-trends even further, we collect additional historical data on unemployment and population statistics and proceed as follows.[30] We partition the decade pre-dating the SHI approval in two-year intervals and carry out pre-trend tests similar to the ones presented in Table 2 and Table 3 by progressively rolling back two years from the acceptance of the SHI. In this way, we reduce the sample friction of municipalities for which housing transaction and unemployment data is available (i.e., we limit the loss of municipalities), which makes the empirical estimation of pre-trends more reliable. Additionally, we investigate how the pre-trend assumption holds for the two sample restrictions that aim to balance the treatment and control groups, namely the one excluding major urban areas and ski resorts (Restricted Sample 1) and the one excluding major urban areas and restricting the sample around the threshold set by the policy (Restricted Sample 2).

The pre-trend analysis summarized in Tables W-F1 and W-F2, respectively, reveals that primary and second home prices do not display significantly different pre-trends.[31] For some periods, the price of *primary homes* displays some significant pre-trend differences in the full sample and in the Restricted Sample 1, but these differences disappear after the inclusion of controls, especially the lagged outcome level variables. Pre-trends of the price of *second homes* are never significant.

The interpretation of pre-trend estimates for unemployment rates (Table W-F3) warrants a more in-depth discussion. In the earliest period and in the period immediately pre-dating the acceptance of the SHI, pre-trends are not significantly different for the full sample and the two sample restrictions once controls are included. However, pre-trends are significant for the full sample and the Restricted Sample 1 over the periods 2004-2005 (pre)/ 2006-2007 (post) and 2006-2007 (pre) / 2008-2009 (post).

We conjecture that these two pre-post periods capture massive one-time shocks to the regulation of the Swiss labor market. Specifically, in 2002 a Bilateral Agreement between Switzerland and states of the European Union (EU) entered into force that aimed to guarantee the free movement of people. In 2004, the agreement was followed by flanking measures aimed at protecting the national labor market from an undercut of salaries and a deterioration of the

[30] Due to backward revisions and multiple data sources for population statistics, this additional historical data does not perfectly match the sample of municipalities used in our main analysis.

[31] For sake of consistency, we also replicate the full sample parallel trend analysis for primary home prices and unemployment rates reported in Panel B of Table 2. In the case of primary home prices, using the alternative sample of municipalities stemming from the new population and unemployment data does not significantly alter the results. Similarly, full sample pre-trend results of unemployment rates relying on newly collected data do not change.

working conditions.[32] The shock of the Bilateral Agreement, and subsequent flanking measures, to the Swiss labor market is apparent in Figure 3 (Panel B) and in line with economic intuition. Only a couple of years after the introduction of the agreement, unemployment rates sharply increased (2004-2005), followed by a sharp drop (2006-2007) subsequent to the adoption of the flanking measures. This is true for both treated and control municipalities.

In 2008 Switzerland entered the Schengen Area, which further facilitated immigration and cross-border commuting from countries belonging to the area. In this case too, the policy change is in line with economic intuition. The effect of the shock is apparent in Figure 3 (Panel B), with the figure depicting a moderate increase in unemployment during the 2008-2009 period, in both the treated and control municipalities. In the case of the Schengen Area agreement, no strong measures were undertaken to significantly counter its impact on the labor market, partly due to pressures from the European Union.

Despite the arbitrariness of the 20%-threshold set by the SHI, the flanking measures following the Bilateral Agreement and the adoption the Schengen Area affected our control and treatment group differentially. The estimated impact of these two policies is documented in the pre-trend tests of Table W-F2 and is in line with the pre-trend graph for the unemployment rate shown in Figure 3 (Panel B). Over the period 2004-2005 (pre) and 2006-2007 (post), the flanking measures reduced unemployment rates more effectively in the control group than in treated areas (significant positive coefficient). Conversely, from 2006-2007 (pre) to 2008-2009 (post), unemployment rates increased more in the control group than in treated areas (significant negative coefficient).

We argue that this differential impact of the two policies is because flanking measures were designed to protect the bulk of Swiss workers, which is located in cities, and entering the Schengen Area mostly increased commuting inflows from neighboring countries. Indeed, foreign workers tend to disproportionally supply labor in the larger urban areas, often cross-border commuting from neighboring countries (mainly from Germany, France, and Italy). All major cities with the exception of Bern are located within commuting distance to the country border, facilitating cross-border commuting. This is particularly true for Geneva, Basel and all the main cities in the Italian speaking part of the country. Over the last decade, Switzerland has experienced a steady increase in the number of cross-border commuters driven by strong wage and house price differentials (wages and house prices are both significantly higher in Switzerland). As a consequence of this, cross-border commuters increase the supply of labor without directly affecting housing demand. This also may explain why the labor supply shock caused by the two agreements does not show up in the price of primary residences.

[32] See https://www.eda.admin.ch/missions/mission-eu-brussels/en/home/key-issues/free-movement-persons.html for further details.

However, we should stress a couple of important points. First, despite the fact that the agreements impact the control and treatment group differently in two of our four pre-trend tests, unemployment trends of the two groups continue to move in the same direction in all test-years, as shown in Figure 3 (Panel B). Second, before and after the shock caused by the agreements, unemployment dynamics of the treatment and control group become similar again, suggesting that in equilibrium the unemployment trend of the control and treated group are the same. Third, pre-trend differentials do vanish completely once we employ the most rigorous specification (Restricted Sample 2), supporting the hypothesis that major urban and tourist places were impacted differently by the Bilateral and Schengen Area agreements. Seen through this lens, our main results for unemployment rates presented in Table 2 (Panel A), might actually represent conservative estimates of the negative impact of the SHI on the local labor market. This is because the estimated treatment effect for the Restricted Sample 2 (see Table W-E6) is much higher, although slightly less statistically significant due to the lower number of observations.

TABLE W-F1

Parallel trend of price of primary homes (FD-IV estimates, 2nd stage only)

| Dependent variable | | Δ Log price of primary homes | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Pre | Post | Full sample | | | CBD >10 km & Ski>0 km | | | CBD >10 km & [0.15,0.3] | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| 2008-2009 | 2010-2011 | 0.0108 | -0.00377 | -0.0406 | 0.0429 | 0.0454 | 0.0274 | 0.155 | 0.343 | 0.355 |
| | | (0.0356) | (0.0331) | (0.0327) | (0.0716) | (0.0645) | (0.0648) | (0.259) | (0.232) | (0.228) |
| 2006-2007 | 2008-2009 | 0.0736* | 0.0737** | 0.0135 | 0.0403 | 0.0501 | -0.000842 | 0.371 | 0.303 | 0.306 |
| | | (0.0378) | (0.0353) | (0.0340) | (0.0713) | (0.0737) | (0.0696) | (0.301) | (0.264) | (0.262) |
| 2004-2005 | 2006-2007 | 0.0244 | 0.0336 | -0.00194 | 0.0688 | 0.0682 | 0.0337 | -0.0921 | -0.0272 | 0.0316 |
| | | (0.0323) | (0.0318) | (0.0307) | (0.0583) | (0.0568) | (0.0543) | (0.235) | (0.221) | (0.199) |
| 2002-2003 | 2004-2005 | 0.0254 | 0.0464 | 0.0255 | 0.158* | 0.168** | 0.130 | -0.124 | -0.463 | -0.239 |
| | | (0.0502) | (0.0443) | (0.0455) | (0.0905) | (0.0791) | (0.0883) | (0.242) | (0.300) | (0.227) |
| Lagged difference of controls | | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | | No | No | Yes | No | No | Yes | No | No | Yes |

TABLE W-F2

Parallel trend of unemployment rates (FD-IV estimates, 2nd stage only)

| Dependent variable | | Δ Log unemployment rate | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Pre | Post | Full sample | | | CBD >10 km & Ski>0 km | | | CBD >10 km & [0.15,0.3] | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| 2008-2009 | 2010-2011 | -0.0203 | -0.0264 | -0.0309 | 0.0412 | 0.0273 | 0.0314 | -0.127 | -0.156 | -0.171 |
| | | (0.0209) | (0.0215) | (0.0216) | (0.0429) | (0.0439) | (0.0438) | (0.158) | (0.148) | (0.142) |
| 2006-2007 | 2008-2009 | -0.105*** | -0.107*** | -0.104*** | -0.144*** | -0.138*** | -0.124** | -0.228 | -0.171 | -0.155 |
| (Schengen Area agreem., 2008) | | (0.0254) | (0.0251) | (0.0246) | (0.0528) | (0.0528) | (0.0487) | (0.182) | (0.195) | (0.193) |
| 2004-2005 | 2006-2007 | 0.181*** | 0.191*** | 0.180*** | 0.258*** | 0.262*** | 0.244*** | 0.259 | 0.274 | 0.259 |
| (Flanking agreement, 2004) | | (0.0259) | (0.0270) | (0.0262) | (0.0605) | (0.0621) | (0.0552) | (0.186) | (0.187) | (0.190) |
| 2002-2003 | 2004-2005 | 0.117** | 0.112** | 0.0273 | 0.0194 | 0.0327 | -0.0435 | -0.0765 | -0.116 | -0.301 |
| | | (0.0550) | (0.0528) | (0.0522) | (0.131) | (0.129) | (0.132) | (0.189) | (0.258) | (0.224) |
| Lagged difference of controls | | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | | No | No | Yes | No | No | Yes | No | No | Yes |

TABLE W-F3

Parallel trend of second home prices (FD-IV estimates, 2nd stage only)

| Dependent variable | | Δ Log price of second homes | | |
|---|---|---|---|---|
| Pre | Post | Full sample | | |
| | | (1) | (2) | (3) |
| 2008-2009 | 2010-2011 | -0.0498 | -0.121 | -0.157 |
| | | (0.200) | (0.160) | (0.159) |
| 2006-2007 | 2008-2009 | -0.0839 | -0.0845 | -0.0903 |
| | | (0.170) | (0.147) | (0.144) |
| 2004-2005 | 2006-2007 | 0.0653 | 0.0784 | 0.0797 |
| | | (0.155) | (0.126) | (0.126) |
| 2002-2003 | 2004-2005 | 0.0526 | -0.0922 | -0.105 |
| | | (0.197) | (0.218) | (0.214) |
| Observed treatment | | Yes | Yes | Yes |
| Time fixed effects | | Yes | Yes | Yes |
| Lagged and time invariant controls | | No | Yes | Yes |
| Predetermined outcome level × Post | | No | No | Yes |

*F.2 Controlling for second home rate polynomial terms*

An alternative approach to account for the fact that our "historic" instrument may capture intrinsic differences between the treatment and the control group that correlate with short-term dynamics of the outcome variables, is to include polynomial terms of second homes rates (i.e., our running variable) in the full sample case. Thereby we allow for different polynomial-coefficients for the treatment and the control group.

We do this by centering second home rates at the threshold set by the policy (20%), computing the corresponding linear, quadratic, and cubic polynomial terms, and finally interacting them with the observed treatment dummy. Polynomial terms that are not interacted with the observed treatment dummy are partialled out by first differencing, as the policy defines treated areas based on time-invariant second home rates measured in 2012.

The interaction terms with the observed treatment dummy are also endogenous due to the fact that municipalities have the option to request a revision of their second home rates and thus are not being treated. Therefore, we instrument each of these interactions by interacting our instrument ('historic' share of second homes) with the corresponding second home rate polynomial term. It is highly problematic to restrict the sample around the threshold set by the initiative, as there is not enough variation left that can be exploited by the instrument.

TABLE W-F4

Primary house prices: Including second home rate polynomials (FD-IV, 2nd stage only)

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | *Pre and post - Second stage* | | | | | | | | |
| Dependent variable: Δ Log price of primary homes | | | | | | | | | |
| | Linear | | | Quadratic | | | Cubic | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Observed treatment | -0.210** | -0.166 | -0.168* | -0.178** | -0.155** | -0.195*** | -0.169*** | -0.151** | -0.193*** |
| | (0.106) | (0.105) | (0.0995) | (0.0729) | (0.0731) | (0.0708) | (0.0655) | (0.0658) | (0.0643) |
| Observed treatment × Second home rate | 0.158<br>(0.290) | 0.0524<br>(0.289) | -0.0589<br>(0.277) | | | | | | |
| Observed treatment × Second home rate$^2$ | | | | 0.164<br>(0.442) | 0.0531<br>(0.453) | 0.0372<br>(0.443) | | | |
| Observed treatment × Second home rate$^3$ | | | | | | | 0.236<br>(0.805) | 0.0577<br>(0.844) | 0.0483<br>(0.834) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |
| Observations | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 |
| Kleibergen-Paap F | 111.1 | 112.4 | 113.7 | 300.5 | 303.2 | 301.1 | 420.3 | 422.2 | 421.7 |

*Notes:* Second home rates polynomial terms of the interaction terms are centered at the threshold set by the policy.

We report the estimation results for the price of primary residences in Table W-F4 and for local unemployment rates in Table W-F5. We report results including linear, quadratic, and cubic polynomial terms individually. Given the distribution of second home rates, these polynomial terms are strongly correlated, with correlations above 0.8. Therefore, we only consider the impact of one polynomial term at a time, and refrain from including several polynomial terms jointly.

TABLE W-F5

Unemployment rate: Including second home rate polynomials (FD-IV, 2nd stage only)

| | *Pre and post - Second stage* | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Dependent variable: Δ Log unemployment rate | | | | | | | | | |
| | Linear | | | Quadratic | | | Cubic | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Observed treatment | 0.213*** | 0.203*** | 0.236*** | 0.134*** | 0.128*** | 0.133*** | 0.121*** | 0.116*** | 0.116*** |
| | (0.0673) | (0.0672) | (0.0652) | (0.0404) | (0.0409) | (0.0394) | (0.0350) | (0.0356) | (0.0346) |
| Observed treatment × | -0.250 | -0.232 | -0.342** | | | | | | |
| Second home rate | (0.171) | (0.170) | (0.166) | | | | | | |
| Observed treatment × | | | | -0.0796 | -0.0635 | -0.140 | | | |
| Second home rate$^2$ | | | | (0.206) | (0.209) | (0.200) | | | |
| Observed treatment × | | | | | | | 0.000537 | 0.0267 | -0.0777 |
| Second home rate$^3$ | | | | | | | (0.329) | (0.336) | (0.320) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes | No | No | Yes |
| Observations | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 | 1,406 |
| Kleibergen-Paap F | 111.1 | 112.4 | 112.7 | 300.5 | 303.2 | 304.3 | 420.3 | 422.2 | 424.6 |

*Notes:* Second home rates polynomial terms of the interaction terms are centered at the threshold set by the policy.

In the case of the price of primary homes, results contained in Table W-F4 show that including polynomial terms interacted with observed treatment dummies does not significantly alter the magnitude of the impact of the observed treatment (main effect), although we lose statistical significance in the case of linear polynomial terms in column (2). The interactions of the observed treatment dummy with second home rate-polynomial terms are always completely statistically insignificant.

We attribute the loss in significance in the case of the linear polynomial term to two factors. First, we notice a sharp drop in the Kleibergen-Paap F statistic compared to our baseline specification in Table 2 and when using quadratic and cubic polynomial terms in columns (4) to (6) and (7) to (9) of Table W-F4, respectively. This is to be expected, as the share of contemporaneous and 'historic' second home rates are strongly correlated (with a correlation of about 0.95 in our sample).

Second, the interaction term of the observed treatment with the linear second home rate is largely insignificant, with the standard deviation of the interaction terms amounting to several times the coefficient value. Put differently, we give up variation that can be exploited by the instrument to introduce a noisy term that is largely insignificant to describe the growth of primary home prices. As soon as we reduce the loss of variation by including quadratic or cubic polynomials terms, which correlate less with the instrument, we obtain again highly significant results without the magnitude of the main treatment effect being significantly affected.

In the case of unemployment rates, the impact of the main treatment effect remains positive and highly significant, as shown in Table W-F5. Similar to primary home prices, interactions of the observed treatment with quadratic and cubic polynomial terms are also largely insignificant. However, in the case of the linear polynomial term, the interaction term is more significant than in the case of primary home prices and even turns significant at the 5% level when controlling for the predetermined outcome level (column 3 of Table W-F5). As such, the impact of the main treatment effect remains highly significant when including an interaction with a linear polynomial term.

To summarize, we find evidence that controlling for polynomial terms of second home rates interacted with the observed treatment does not systematically affect the magnitude and significance of our main results. Additionally, these interaction terms are usually largely insignificant and, in the case of the linear polynomial term, reduce the variation exploited by our instrumental variable.

*F.3 Heterogeneous effects*

We investigate potential heterogeneous treatment effects along two dimensions: the importance of the hotel industry and household mobility.

We discuss results only for the price of primary homes and unemployment rates, as we did not obtain any significant results for the price of second homes. Indeed, the variables we use to perform the heterogeneity analysis seem mostly relevant for primary residents. Unfortunately, there is no data available to investigate the heterogeneous impact of the policy according to the characteristics of second home investors. For example, we do not know who owns second homes, whether they are local residents, Swiss citizens living in an urban area, or foreign investors living abroad.

We proceed as follows. First, exploiting the 2000 Swiss Census and related surveys, we have collected data on (i) the proportion of beds in hotels relative to the local population, (ii) homeownership rates, (iii) housing vacancy rates, and (iv) the share of families with young children (aged between 0-6 years). We use data measured in 2000, because they are largely predetermined with respect to the policy and arguably unrelated to the dynamics of our main outcome variables. Second, we mean-center these 'historic' variables and interact them with the observed treatment dummy. When first differencing, the main effect of these time-invariant variables is partialled out. Third, we instrument these interaction terms by interacting each variable that potentially causes heterogeneous treatment effects with our instrument. We report the results of this analysis in Table W-F6.

In what follows, we only discuss the results for the interaction effects. The main treatment effects are always statistically significant and fairly stable with the expected sign.

We find weak evidence that in municipalities where the hotel industry is important the negative impact of the ban on the local economy is weaker. That is, the interaction term for the price of primary homes is positive and relatively stable to the inclusion of controls and becomes significant when we control for the predetermined outcome level. The sign of the interaction terms is stable and negative in the case of the unemployment rate, albeit never significant. This weaker impact of the SHI on the economy of tourist places might be due to a shift of investors from buying second homes to consuming tourism services, thus negatively affecting the local economy less strongly. However, the lack of strong evidence suggests that second home buyers do not consider tourism services as a good substitute, likely due to the fact that the investment component is missing.

We find weak evidence that the SHI had a stronger negative impact on the price of primary homes and increased unemployment more in municipalities that have a higher homeownership rate. The sign of the interaction term is always negative (positive) for the price of primary homes (unemployment rates). The negative impact of the interaction term on the price of primary homes is only significant when controlling for the predetermined outcome level, whereas it becomes insignificant in the case of the local unemployment rate. We attribute the positive coefficient of the interaction term in the case of the unemployment rate to the fact that homeowners are usually less mobile than renters. A higher share of homeowners means that local residents are more likely to stick around in the municipality as unemployed and will not leave (as renters may). This increases the unemployment rate, all else equal.

We also find weak evidence that municipalities that have a historically higher housing vacancy rate were more negatively affected by the SHI. As in the case of homeownership, the sign of the interaction term is always negative (positive) for the price of primary homes (the unemployment rate), although its coefficient is significant only when we control for the predetermined outcome level for both outcome variables. We explain this as follows. Places with historically high vacancy rates tend to be declining places with weak demand for housing. If such places are hit by a negative economic shock (i.e., the demand curve shifts downwards), the demand curve is shifted to the (nearly) perfectly inelastic part of the supply curve (kinked supply curve argument due to the durability of the housing stock). This leads to a stronger negative capitalization of the SHI in primary house prices. Similarly, in places with high vacancy rates it is more difficult to sell a property. Thus, the price response to a negative demand shock may be more pronounced.

Finally, we find some weak evidence that the SHI increased the local unemployment rate more strongly in places with high shares of families with little children. The explanation is very similar to the one for the homeownership rate and the vacancy rate; young families with children tend to be less mobile than other demographic groups and therefore cannot easily escape unemployment by moving to other areas.

TABLE W-F6

Heterogeneous treatment effects (FD-IV estimates, 2nd stage only)

| Dependent variable | Δ Log price of primary homes | | | Δ Log unemployment rate | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Observed treatment | -0.203*** | -0.196*** | -0.254*** | 0.135*** | 0.134*** | 0.127*** |
| | (0.0578) | (0.0544) | (0.0533) | (0.0328) | (0.0328) | (0.0318) |
| Observed treatment × Hotel beds/population | 0.191 | 0.180 | 0.239** | -0.0535 | -0.0665 | -0.0594 |
| | (0.125) | (0.113) | (0.106) | (0.0827) | (0.0780) | (0.0725) |
| Observed treatment | -0.100* | -0.0921* | -0.1000** | 0.0683** | 0.0554* | 0.103*** |
| | (0.0517) | (0.0514) | (0.0493) | (0.0311) | (0.0312) | (0.0310) |
| Observed treatment × Homeownership rate | -0.435 | -0.479 | -0.841*** | 0.530*** | 0.609*** | 0.0457 |
| | (0.302) | (0.295) | (0.288) | (0.203) | (0.199) | (0.188) |
| Observed treatment | -0.152*** | -0.150*** | -0.201*** | 0.119*** | 0.115*** | 0.113*** |
| | (0.0513) | (0.0496) | (0.0472) | (0.0257) | (0.0262) | (0.0257) |
| Observed treatment × Vacancy rate | -3.752 | -4.412 | -7.612** | 0.412 | 0.615 | 2.081* |
| | (4.193) | (3.999) | (3.071) | (1.094) | (1.139) | (1.158) |
| Observed treatment | -0.126** | -0.128** | -0.178*** | 0.171*** | 0.169*** | 0.141*** |
| | (0.0538) | (0.0528) | (0.0519) | (0.0360) | (0.0361) | (0.0355) |
| Observed treatment × % Family with young children | 0.815 | 0.488 | -0.0388 | 2.868** | 3.056** | 1.815 |
| | (1.685) | (1.593) | (1.485) | (1.180) | (1.188) | (1.142) |
| Lagged difference of controls | No | Yes | Yes | No | Yes | Yes |
| Predetermined outcome level | No | No | Yes | No | No | Yes |

*Notes:* All interaction variables are at municipality-level and are derived from the 2000 Swiss Population Census. The average value of the share of beds in hotels relative to the municipality's population is 4.58%, with values ranging from 0% to 212% (SD=14.88%). The average value of the homeownership rate is 50.77%, with values ranging from 3.6% to 88.1% (SD=15.10%). The average value of the housing vacancy rate is 1.54%, with values ranging from 0% to 13.06% (SD=1.65%). The average value of the share of families with young children (aged between) 0 and 7 years old is 14.04%, with values ranging from 5.3% to 27.4% (SD=3%). We mean-center these variables before interacting them with the observed treatment dummy.