ISSN 2042-2695

CENTRE *for* ECONOMIC PERFORMANCE

CEP Discussion Paper No 1556

July 2018

The Economic Impacts of Constraining Second Home Investments

Christian A.L. Hilber
Olivier Schöni

THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE

E·S·R·C ECONOMIC & SOCIAL RESEARCH COUNCIL

Abstract
We investigate how political backlash against wealthy investors in high-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 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. Constraining second home investments in locations where primary and second homes are not close substitutes may reinforce wealth inequality.

Key words: second homes, wealth inequality, land use regulation, housing policy, house prices, unemployment
JEL: D63; G12; R11; R21; R31; R52

This paper was produced as part of the Centre's Urban and Spatial Programme. The Centre for Economic Performance is financed by the Economic and Social Research Council.

We are grateful to Gabriel Ahlfeldt, John Clapp, Steve Gibbons, Lu Han, Blaise Melly, Henry Overman, Yu Qin, Stuart Rosenthal, Maria Sanchez, Corentin Trevien, and Maximilian von Ehrlich for insightful comments and suggestions. We thank participants at the 2018 RIETI Research Meeting in Tokyo, the 2017 North American UEA sessions of the NARSC conference in Vancouver, the 2017 International AREUEA conference in Amsterdam, the 2017 European UEA meetings in Copenhagen, the 2016 IV IEB Workshop on Urban Economics in Barcelona, the 2016 Weimer School of Advanced Studies in Real Estate and Land Economics, and the 2015 North American UEA sessions of the NARSC conference in Portland. We also thank seminar participants at the LSE (SERC), University of Bern, University College Dublin (Centre for Financial Markets), CREST in Paris, Lancaster University, University of Würzburg, University of Nuremberg, University of Durham, University College London, Kraks Fond in Copenhagen, and University of Barcelona/IEB for helpful comments. Olivier Schöni is grateful to Laurent Donzé for his support and to the Swiss National Science Foundation (P2FRP1_155187) for providing research funding. We thank the Swiss Real Estate Datapool Association for providing the housing data. All errors are the sole responsibility of the authors.
Filippo Di Mauro, National University of Singapore and CompNet. Fadi Hassan, Trinity College Dublin, Bank of Italy and Centre for Economic Performance, London School of Economics. Gianmarco I.P. Ottaviano, Bocconi University, London School of Economics, Centre for Economic Performance, London School of Economics and CEPR.

Published by
Centre for Economic Performance
London School of Economics and Political Science
Houghton Street
London WC2A 2AE

1 Introduction

In this paper we explore a recent global phenomenon; the surge of investment in 'second homes' – properties that are not used as primary residence – and the subsequent political backlash against wealthy investors in such properties. To identify the local housing and labor market impacts of constraining second home investments, we exploit a unique quasi-natural experiment in Switzerland – the 'Second Home Initiative' (SHI) that was narrowly approved by voters in March 2012 and effectively banned the construction of new second homes in touristic areas with a high share of such homes. Our findings imply that the adverse 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 preferred estimates suggest that the ban lowered price growth of primary homes in affected areas by around 15%, increased price growth of second homes by about 26%, and increased the growth in local unemployment rates by around 12%.

Fueled by low interest rates and a staggering wealth accumulation among a growing cohort of 'top earners', over the last two decades countries such as the United States, Canada, the United Kingdom, France, China, or Singapore have seen a dramatic increase in wealthy individuals investing in second homes. Within these countries, major cities – such as New York or London – and popular tourist regions observed the most pronounced booms.

In the United States the number of second homes increased by about 20% to 6.8 million between 1995 and 2005 alone (Belsky *et al.*, 2007). In Canada, according to estimates derived from the Survey of Financial Security, the number of second homes increased even slightly more during the same time period; by about 22% to 1.1 million. In the United Kingdom, according to the English Housing Survey, the number of second homes more than doubled between 1995 and 2013. The rise of the market for second homes is perhaps most dramatic in China. In 2002, 6.6% of all urban households owned a second home. By 2007, this share surged to 15% (Huang and Yi, 2011). Finally, in France, according to the French National Institute of Statistics and Economic Studies, INSEE, by 2014, second homes represented 9.3% of the total housing stock.

The surge in second home investments has triggered a serious political backlash in many countries, reflecting a diverse array of concerns.[1] Critically, wealthy second home investors – especially foreign ones – are being blamed for the dramatic house price increases in many desirable high-amenity locations – tourist places as well as superstar cities. [2] Antipathy to new second home investors may also reflect "an ugly dislike of outsiders" or in some cases even "NIMBYism of second home investors themselves, keen to preserve the exclusiveness of their

[1] We provide newspaper references documenting second home policies implemented across the globe in Web-Appendix A.

[2] While house prices in desirable tourist locations and superstar cities tend to increase strongly in the long-run due to a combination of strong demand and tight supply constraints (Hilber and Vermeulen, 2016), 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.

holiday patch" (The Economist, 2016). This antipathy may be further reinforced by a growing number of wealthy at the top end and rising wealth inequality (Rognlie, 2014) in conjunction with envy motives. Other concerns relate more directly to the impact of owners of second homes in the affected localities: The uncontrolled construction of second homes may blight the beautiful landscape in touristic areas. Moreover, second homes typically stay empty for much of the year, creating a ghost town atmosphere outside tourist seasons, yet often generating little or no local tax revenue.

One example of political backlash is New York City. In 2012, the city abolished a 20% tax-abatement for owners of condos that were not primary residences. More recently, the Fiscal Policy Institute proposed a steep property tax surcharge on expensive pied-à-terre residences. In a similar vein, too curb soaring housing prices, Vancouver introduced a transaction tax of 15% on purchases made by not permanent residents in 2016. The government of Ontario adopted a similar tax in 2017 for transactions occurring in the Greater Golden Horseshoe region, a high-amenity area particularly popular among foreign real estate investors.

The political backlash has arguably been even more pronounced in Europe, in certain Asian countries, and in Australia. For example, in 2016 the UK introduced a 3% 'stamp duty' (transfer tax) hike on second homes. Popular tourist destinations located on the South West coast of the country – such as St. Ives or Carbis Bay – went a step further and adopted complete bans on second homes to limit the investors' footprint.[3] In France, the national government approved a law in 2014, allowing municipalities with overheating housing markets to introduce a property tax on second homes of up to 20%.[4]

The Chinese government announced a whole series of measures to curb second home investments. These include drastic minimum requirements on down-payments in the entire country (although recently somewhat relaxed) and even more drastic measures in certain cities such as Beijing, where single-person households were banned from buying more than one residence and where a 20% capital gains tax on property was imposed. In a similar vein, Singapore's government introduced several measures between 2010 and 2015 including an additional stamp duty tax and increased compulsory down-payments to discourage second home investments. Lastly, in Australia, a review board ensures that the purchase of existing properties by foreign investors benefits local communities. It precludes purchases by foreign buyers for investment motives (buy-to-let or expected capital gains) or for recreational use.

To date we know little about the consequences of these policies and, in particular, we lack evidence on the impact of restrictions on the construction or ownership of second homes (vis-à-vis instruments that tax non-primary residences).

---

[3] Denmark has a similar policy. Municipalities grant building permits for summer cottages only if projects meet stringent planning requirements – mainly intended to preserve the coastline. These restrictions were imposed mainly to prevent an inflow of (foreign) investors.

[4] In a similar vein, Israel introduced a property tax increase on second homes in 2015 with the intent to fight so called 'phantom apartments'.

In our empirical analysis, we exploit a unique quasi-natural experiment – the SHI – to explore the impact of a constraint on the construction of new second homes in high-amenity places. The SHI stipulates that in municipalities with a share of second homes of 20% or more, 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.[5] 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).[6]

We faced two main challenges. The first is of a theoretical nature: to understand the mechanisms through which a constraint on second homes may affect local housing and labor markets, and, the residential location choices of primary residents and investors.

To shed light on these mechanisms and resulting outcomes, we develop a simple dynamic general equilibrium framework and consider two contrary settings. The first assumes that primary and second homes effectively trade in separate markets (i.e., are poor substitutes). We assume nevertheless that the two housing markets are linked via labor markets. A growing number of wealthy second home investors positively affects local economies, driving up local wages, and thus demand for primary housing. However, second home investments are also assumed to adversely influence the primary residents' valuation of local amenities, negatively affecting local housing demand. Our model yields two main propositions. Proposition 1 states that constraining second home investments (i) negatively impacts the housing market of primary residences (lowering the price growth of primary homes), (ii) adversely affects local labor markets (lowering wage growth – or in a setting with sticky wages; increasing the unemployment growth rate), and (iii) creates an ambiguous sorting reaction depending on how much local residents dislike second home investors. Proposition 2 asserts that the ban positively affects the equilibrium price growth of second homes.

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

Our empirical findings are consistent with the former setting, suggesting that – consistent with our priors – in Swiss tourist areas primary and second homes are poor substitutes.

[5] 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.

[6] 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.

The second main challenge is the unbiased estimation of treatment effects. Although the treatment assignment variable – which is a deterministic function of second home rates – is pre-determined in the standard difference-in-differences setting, it may correlate with unobserved time-invariant variables at the municipality level. More specifically, our control municipalities typically belong to major urban/suburban areas, whereas treated municipalities are usually located in mountainous touristic places. As such, the estimated impacts of the SHI on (pre and post ban) outcomes may potentially capture unobservable differences between treatment and control group.

An additional source of endogeneity comes from the practical implementation of the SHI. Municipalities were allowed to challenge the official second home rate, which was used to determine the applicability of the SHI, and ask for a downward revision. Municipalities that were allowed to revise their second home rate downward were not subject to the ban imposed by the SHI. This might lead to out-of-treatment selection bias, as municipalities might have decided to challenge their second home rate according to economic incentives that correlate with unobserved outcome dynamics.

A last source of endogeneity arises from unobserved dynamics, which likely differ between treated and control municipalities.

To counter these potential sources of endogeneity, we employ three different strategies. First, to partial out time-invariant unobservables we estimate a first-difference model.

Second, to address potential out-of-treatment selection bias, we instrument the observed treatment dummy – which is a function of second home rates as defined in 2012 but after revisions were taken into account – with the second home rates as measured by the 2000 Federal Population Census. The instrument strongly correlates with the observed treatment (measured in 2012), solving potential endogeneity issues linked to out-of-treatment selection.

Third, to address the concern that "historic" second home rates may reflect permanent differences in unobserved outcome dynamics between treatment and control group (i.e., the instrument exogeneity may be questionable), we restrict the sample of municipalities such that their observed covariates are balanced, and then instrument the observed treatment dummy with second home rates in 2000. We use two alternative sample restrictions. The first sample restriction drops all municipalities located within a distance of 10 km from the boundary of one of the 15 major Swiss CBDs, and/or adjacent to a municipality containing one of the 53 major Swiss ski resorts. The second sample restriction follows Greenstone and Gallagher (2008) and is akin to a fuzzy regression discontinuity design: We drop municipalities located within a distance of 10 km from major CBDs and having a second home rate outside the narrow interval [0.15, 0.3]. Both sample restrictions allow us to focus on "similar" municipalities by way of excluding most major urban areas and highly touristic municipalities. Reassuringly, the estimated effects are robust to this check.

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. To begin with, Haughwout *et al.*

(2014) investigate the role of investors in the recent U.S. housing crisis. Three main findings arise from their analysis. First, investors are overrepresented in states that display the strongest boom-bust cycles. Second, investors misreporting their occupancy status to obtain better credit conditions had the tendency to bid more aggressively during the boom than owner-occupiers and admitted investors. Third, investors defaulted at a higher rate during the bust phase than owner-occupiers.

Chinco and Mayer (2016) compare local second home buyers to out-of-town investors. They find that out-of-town buyers – unlike local second home buyers – behave as misinformed speculators, increasing future house prices and the implied-to-actual rent ratio. They develop an estimation strategy taking into account the possible reverse causality between housing prices and the out-of-town demand of investors. In a related paper, Bayer *et al.* (2015) 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.

Four recent papers focus on international second home investments in major world cities. Cvijanovic and Spaenjers (2015) explore the effect of international demand for luxury secondary residences in Paris. They point out how investors concentrate in specific areas, 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 (2016) 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.[7] 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 home buyers. 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 can lead to welfare gains to the extent fiscal revenues are used to finance local public goods.

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

[7] 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.

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. 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 presence of short-term, often inexperienced, speculators may only be one of the ultimate symptoms associated with overheating local housing markets.

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 a simple dynamic general equilibrium 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 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 counter-proposal, often leading proponents to withdraw the initiative.

Supporters of the SHI collected enough validated signatures by January 2008. The Federal Council, the Parliament, most of the political parties and economic organizations recommended to vote against the initiative. 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 1st, 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. 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.[8]

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[9] – 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. The positive association between the voting outcome and the anticipated treatment status is also apparent at cantonal level. Figure 2 documents that in most cantons where a sizeable share of the population is likely affected by the ban, the initiative was rejected.

In Appendix Table A1 (Appendix A) 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. 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, the more strongly permanent residents in treated areas were opposed to the SHI.

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-touristic 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

[8] Initially the 'conversion option' was confined 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. 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 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.

[9] Homeownership rates at the local level are available only from the decennial Census until 2000. Since 2010 the Federal Statistical Office only draws a sample of the population, not allowing us to explore the effect of the SHI on homeownership attainment in treated areas.

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 a general spatial equilibrium setting for the structural analysis of housing prices, wages, and population growth in the presence of agglomeration economies.[10]

We consider a system of local jurisdictions that differ in the quality of major natural amenities, such as mountains or lakes, but an amenity could also be interpreted e.g. as the touristic or consumer center of a superstar city.[11] 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. 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.[12]

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 does not seem farfetched. It arises when second home investors and primary residents differ in their preferences for the micro-location within municipalities or for the layout of a property. 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

[10] Our theoretical framework also relates to recent work by Desmet and Rossi-Hansberg (2013), Hsieh and Moretti (2015), and Gaubert (2015).

[11] While the theoretical considerations are in large parts similar for highly touristic areas and large superstar cities, one important distinction is the fact that the latter have much more diversified labor markets and are less dependent on second home investors. Thus, the predictions for labor market effects may differ. We discuss this point and its policy implications in the concluding section.

[12] In our setting we do not model the likely negative effect of the ban on wages through the local construction industry. The negative wage effect in the construction industry is redundant with the negative wage impact of the ban on the tourism industry. In interest of parsimony, we model only this latter mechanism.

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 restauration 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 (2015), the output of firms is characterized by a Cobb-Douglas production function that displays decreasing returns to scale at the aggregate level:

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

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

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

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 θ_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 η 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:

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

where, similar to the case of primary residents, the amenity index $\theta_i^{\mathcal{S}} N_{it}^{\mathcal{S}\,\epsilon}$ reflects the potential dislike of an investor for the presence of other investors. (When ϵ 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 $W_t^{\mathcal{S}}$ and $r_{it}^{\mathcal{S}}$ 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.[13] The parameter b is the constant expenditure share on housing of second home investors.

3.4 *Housing developers*

We describe the problem of developers of primary residences following Glaeser (2008). (Developers of second homes solve a similar optimization problem. See the right-hand side of the market clearing condition B5 in Appendix B.) 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

[13] The wage $W_t^{\mathcal{S}}$ can be thought of as the share of wage investors spend where their second home is located. This is the case, for example, if second home investors – which consume composite goods and services c_{it} and housing H_{it} where their primary residence is located (location i) and touristic services c_{lt} and housing H_{lt} where they own a second home (location l) – have preferences according to a nested Cobb-Douglas function of the form $U^{\mathcal{S}}(u_{it}, u_{lt}) = (u_{it})^{s_1} (u_{lt})^{1-s_1}$ with $u_{kt} = \theta_{kt} (c_{kt})^{1-s_k} (H_{kt})^{s_k}, k = i, l$. Then investors spend a constant share $(1-s_1)$ of their "total" wage W_t in location l, i.e. $W_t^{\mathcal{S}} = (1-s_1) W_t$.

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

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

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:

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

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 $\left(\frac{W_{it+1}}{W_{it}},\frac{P_{it+1}}{P_{it}},\frac{N_{it+1}}{N_{it}},\frac{P_{it+1}^{\mathcal{S}}}{P_{it}^{\mathcal{S}}},\frac{N_{it+1}^{\mathcal{S}}}{N_{it}^{\mathcal{S}}},\frac{p_{it+1}}{p_{it}}\right)$ 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 Appendix B.

We can use the dynamic equilibrium to make comparative static predictions about the impact of constraining second home investments (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

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

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 Appendix B and Appendix Table B1.

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 affect). 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. In Appendix B, 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 B – suggest that such a threshold may be unrealistically high.

PROPOSITION 2. If primary and second homes are not substitutable, the average price growth effect on second homes of constraining new second home investments is positive.

Proof. See Appendix B and Appendix Table B1.

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 of 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 second home investments on local residents and, more generally, wealth inequality. Proposition 1 implies that constraining second home investments imposes a significant economic cost on local homeowners in the form of both, lower house price and lower wage growth, making local homeowners unambiguously worse off.[14] Since prices are measured as the present value of imputed rents, constraining second home investments 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 a decrease local wages. In a spatial equilibrium setting without relocation costs, renters should be neither better nor worse off. Proposition 2 implies that *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 new second home investments, hurting local homeowners and favoring absentee second home investors.

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.

[14] We would not expect a negative effect of the ban on the price of primary homes in a setting with exogenously determined incomes. Consider a retirement community where retirees receive an exogenously determined pension income. Retirees will welcome the preservation effects of the ban on the local landscape, whereas local labor market considerations are, in the extreme, irrelevant. These considerations could explain the popularity of banning second homes in British sea resorts.

In contrast to the separate market case, 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 residents 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 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. We provide more detail on the sources and data in Web-Appendix C.

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 for 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). 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.[15] 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).

Elderly residents — The Federal Population Census provided by the FSO offers data on the age structure of the resident population 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 outcome variable to measure the amenity effect. We use the number of elderly to measure the local amenity effect, as their sorting behavior should not be affected by labor-related decisions. If the SHI does indeed create a positive amenity effect, we would expect the number of elderly moving to a treated municipality to increase post ban.

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 the municipality second home rate is greater or equal than 20%, and takes value zero if the municipality is below the 20% threshold. Additionally, we use 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 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 (time-invariant or predetermined) — To proxy for 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

[15] One might be concerned that the SHI affected our unemployment rate measure through total population rather than the number of unemployed individuals. However, the findings reported in Appendix Table A3 reveal that the second home ban did not have any meaningful effect – both in a statistical and economic sense – on the total (permanent) population.

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 and on the number of newly constructed residences from 2008 to 2011. This latter variable allows us to control for the predetermined importance of the residential real estate sector in the municipality. Finally, the FSO also provides data on the predetermined number of primary residents in the municipality.

4.2 Descriptive statistics of control and treated municipalities

For the purpose of our regression analysis, we aggregate the data at 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.[16] We provide summary statistics in Tables 1A (control group) and 1B (treatment 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.

A comparison of Tables 1A and 1B reveals that the threshold imposed by the initiative essentially divides areas with major urban centers (control) from mountainous ones (treatment). Below the threshold, municipalities are nearer to major urban centers and more distant to major ski resorts, whereas the opposite is true for treated municipalities. Control municipalities thus have – on average – a larger population and higher salaries. Elderly prefer to live in municipalities belonging to the control group, likely due to a 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 3 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

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

Alps, treated municipalities have more natural amenities, as measured by the share of unproductive surface, compared to the control group.

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. However, whereas the control places have a positive price trend in the traded primary properties pre and post the SHI-approval, the price trend in the treated locations is reversed post implementation of the policy. In treated places, traded properties are older, although the difference is not statistically significant. Interestingly, we observe an increase in this age differential after the SHI-approval: The average age of transacted properties in the control group remained stable. In the treatment group however it increased by more than four years. Similarly, the aggregate housing stock quality of traded properties in treated municipalities appears to have been adversely affected by the ban.

Two remaining points are worth noting. First the threshold imposed by the SHI is situated at the tail of the second home rate distribution as depicted in Figure 4. This makes sample restrictions around the threshold – such as those implemented in a regression discontinuity design – extremely challenging. As expected, we do not observe any bunching of municipalities around the 20% threshold set by the initiative. The absence of bunching suggests that requesting a revision of the official second home rate in 2012 was the only way for municipalities to modify their treatment status. Second, as illustrated in Figure 5, 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, Table 1A and 1B show that the average number of transacted primary homes has not been significantly affected by the policy in treated municipalities.

5 Empirical research design

5.1 Econometric framework and endogeneity issues

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 approval of the SHI allows us to empirically identify theoretical mechanisms of the ban that might disappear in the longer run.[17]

To empirically test our model's predictions, we consider prices of primary and secondary residences, wages and unemployment rates, and the number of elderly as outcome variables. These outcome variables capture wealth, local economy, and amenity effects, respectively. We start by estimating the following two-period difference-in-differences (DD) model:

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

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

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.[18] The variable u_{it} is a stochastic error term.

Unbiased estimation of the coefficient of interest δ is obtained if $E(u_{it}|sr_i) = 0$.[19] 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:

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

where the outcome variable is given by $\Delta\ln(y_{i13-14}) = \ln(y_{i13-14}) - \ln(y_{i10-11})$, 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

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

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 $1\{sr_i \geq 0.2\}$ – making it a relevant instrument – and could not have been manipulated by municipalities according to 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 dynamics of major CBDs and suburban areas – which usually have low historical second home rates – differ in a sensible way from those of touristic 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 ($d_t \cdot y_{i10-11}$ in the case of the DD model). This variable allows us

[18] Data on the share of individuals and firms active in the service sector was available only for 2011 and thus included in the "fixed" effects category for ease of exposition. See Web-Appendix C for further details.
[19] The reader may want to refer to the results' section for a discussion of the parallel trends assumption.

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%.[20] 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 $E(\Delta u_{i13-14}|z_{i00}) \neq 0$.[21] 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 Constraining second home investments: impacts on local residents

Table 2 documents standard cross-sectional DD estimates according to equation (9).[22] To test the predictions of our theoretical model, we consider the price of primary homes (columns 1-3), unemployment rates[23] (columns 4-6), and the number of elderly living in the municipality (columns 7-9) as outcomes variables. For each of these outcome variables, we progressively increase the set of controls. To ease the comparison between DD and first-differenced models, we include in our sample only municipalities for which housing transactions occurred both before and after the SHI approval.

[20] 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.

[21] We do not include second home rates (or polynomials thereof) in our specifications. This is because only a few municipalities in the treated group have second home rates in the 20%-30% interval. Using the Frisch-Waugh theorem it is easy to show that, when including second home rates as control, treatment effect estimates are driven by data points where the second home rate doesn't predict treatment assignment well, i.e. by those points close to the threshold. This considerably increases the standard errors of the estimated treatment effect.

[22] 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 the results in Web Appendix D. However, standard errors may not be reliable due to the small number of clusters.

[23] We report wage results separately in Section 6.3. 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.

DD estimates suggest 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 12 to 15%. The estimated average treatment effect is highly significant, independent of the set of included controls. The stability of treatment estimates to the inclusion of the pre-determined level of the dependent variable (interacted with time FEs) suggests that pre-policy differences in the price of primary homes do not strongly affect post-policy prices (the increase in the adjusted R squared is quite modest).

Unemployment growth rates also seem to have been negatively affected by the SHI, with a relative increase of about 8-10%. Results are less statistically significant than in the case of the price of primary homes, but remain extremely stable to the inclusion of additional controls. Indeed, the lack of statistical significance of the treatment effect in column 4 seems to be due to lack of precision, which is lessened when adding controls (the adjusted R squared increases considerably when including controls). Remarkably, pre-existing patterns of the outcome variable hardly affect the magnitude of the treatment estimates.

Estimation results are less clear-cut for the growth in the number of elderly living in the municipality. The first two specifications show statistically insignificant treatment effects close to zero, suggesting that the SHI had no effect on the sorting behavior of the elderly. However, adding pre-determined levels of this outcome variable strongly affects the estimated treatment effect, which becomes strongly positive and highly significant. Despite being in line with our model prediction, we interpret this finding with some skepticism, as it likely hints more to an omitted variable bias problem than at a true effect. In fact, sorting of the elderly likely depends on factors not measured by our controls, such as family ties and access to healthcare services.

Table 3 illustrates the estimated average treatment effects when estimating our FD-model (equation (10)) that partials out unobserved municipality heterogeneity. The estimated impact on the price growth of primary residences remains negative and highly significant, with the most conservative estimate suggesting a drop in price growth of 14%. The role played by the pre-determined level of primary housing prices seems to become more relevant, although all estimates remain about 1.5 standard deviations from each other across specifications.

Magnitudes of FD estimates reported in Table 3 for unemployment rates are similar to those of Table 2, with an increase in unemployment growth of about 7 to 8% across specifications. First differencing allow us to increase the precision of our estimates, which become highly significant (at the 1% level) across all specifications.

The impact of the SHI on the sorting behavior of elderly remains insignificant and close to zero for the first two specifications, as shown in columns (7) and (8) of Table 3. Adding predetermined levels of the outcome variable turns the treatment effect coefficient weakly statistically significant and *negative,* contrary to our model prediction. The change of sign with respect to the DD estimate reported in column (9) of Table 2 supports the hypothesis that endogeneity problems may still be present. We try to address them in the following models.

Table 4 reports treatment effects when equation (10) is estimated using our IV approach outlined in equation (11). This allows us to partially address endogeneity concern related to potential omitted variable bias and out-of-treatment selection. The FD-IV model is our preferred approach and its estimates are used as benchmark in subsequent robustness checks. Panel A shows second-stage results and Panel B first-stage ones.

Reassuringly, instrumenting second home rates in 2012 with their lagged value in 2000 hardly affects the estimated treatment effects for the price of primary homes. The estimated drop in the price growth of primary homes as a consequence of the second home ban is about 15% (preferred estimate reported in column (2)). The FD-IV estimates for unemployment rates increase in magnitude with respect to the FD estimates and are similar to the DD ones, suggesting a relative increase in the growth of the unemployment rate of about 12% (preferred specification reported in column (5)). The fact that the FD and the FD-IV results for the price of primary homes and for the unemployment rate are quite similar implies that municipalities may not have used the option to revise their second home rate endogenously according to labor and housing market conditions. Lastly, as columns (7) to (9) of Table 4 reveal, the treatment effect of the ban on the growth of the elderly population is statistically insignificant and close to zero in all specifications, suggesting that the second home ban did not affect sorting of the elderly. The Kleibergen-Paap F statistics are extremely high for all specifications, suggesting that weak identification is not a problem in any of the estimated models.

To summarize, we find compelling evidence of a strong negative effect of the SHI on the price growth of primary homes (adverse wealth effect) and a strong positive effect on the unemployment growth rate (adverse economy effect), both of which are in line with the predictions of our theoretical model. We do not find a statistically significant effect of the SHI on sorting of the elderly in our preferred FD-IV specifications casting some doubt on a positive anticipated amenity effect of the SHI.[24] All in all, our results seem to suggest that the negative unintended consequences of restricting second home investments far outweigh the positive intended ones.

6.2 Impact of unobserved dynamics on treatment estimates for primary residents

One concern about the validity of FD-IV estimates is whether such estimates are affected by intrinsic differences between control and treatment groups. 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 groups by using two alternative sample restrictions, discussed above. Of course, balancing observable covariates

[24] 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 is 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. This also implies that if there is a positive amenity effect, the expectations of this should be positively capitalized into house prices today, at least partially offsetting the negative economy effect on house prices.

does not ensure that unobservable ones are balanced as well, but likely reduces considerably the bias coming from omitted variables (Altonji *et al.* 2005). Additionally, as pointed out by Greenstone and Gallagher (2008), balancing covariates makes irrelevant the (linear) functional-form assumption between an outcome variable and the covariates.

Table 5 documents the results when testing whether observed covariates are orthogonal to the observed treatment status. More specifically, we test the orthogonality of two groups of controls included in equation (10): *levels* of pre-policy outcome variables and dynamic (lagged first-differenced) controls. Columns 1-6 report the mean of the covariates in the control and treated group i) for the full sample used in Tables 2-4 (columns 1-2), ii) when we drop municipalities situated within a 10 km radius from major CBDs and/or are adjacent to a major ski resort (columns 3-4), and iii) when we exclude municipalities within a 10 km radius from major CBDs and/or having a second home rate below 15 or above 30% (columns 5-6).[25] The last three columns show the p-values of the difference in mean test for each one of the considered samples. We mark in bold p-values below 10%.

The first three columns in Table 5 reveal that several covariates are not balanced in the full sample: pre SHI the average number of parking places was decreasing in control municipalities while increasing in treated ones, and the inequality of the net income distribution was increasing more markedly in treated municipalities than in control ones. A highly significant difference in means (p-value<1%) is also observed for the price of primary homes. This is hardly surprising. As predicted by the monocentric city model, locations near major CBDs (contained in the control group) have significantly higher house prices than those further out. Additionally, elderly were much more present in the control group, likely due to the proximity to better health care services and family ties. Interestingly, we do not observe any marked difference in the predetermined patterns of unemployment rates. This is likely due to long-run sorting mechanisms, which tend to equalize unemployment across municipalities: people migrate to where they can find work. However, importantly, dropping major urban areas and ski resorts from our sample balances all the covariates except the pre-determined price of primary homes, and combining CBD distance with second home rate restrictions balances all covariates.

Table 6 and 7 report results for the FD-IV model when implementing the two sample restrictions. Dropping major CBDs and highly touristic places makes the negative impact of the initiative on the growth price of primary homes somewhat stronger, with estimates ranging from about 17 to 24%. The negative impact on unemployment growth becomes slightly less pronounced – the treatment effect is estimated to be between 9 and 10% – and its statistical significance is reduced with respect to the FD-IV estimates without sample restrictions. The impact on the growth rate of the number of elderly is slightly more positive across all specifications but remains statistically insignificant. The even stricter sample restriction –

[25] As apparent from the number of observations reported in Table 5, this corresponds to one of the strictest restrictions allowing us to provide stable FD estimates. Figure 3 indeed shows that we are selecting municipalities at the tail of the second home rate distribution.

dropping locations near major CBDs and confining second home rates to between 15 and 30% – 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. The positive effect for the number of elderly remains not significant. We interpret the magnitude of the estimated effects in the most stringent sample restriction with due caution, as the sample size – and in particular the number of municipalities belonging to the treatment group – becomes extremely low, thus considerably increasing the variance of our estimates.

We draw two conclusions from our analysis thus far. First, including unbalanced covariates as controls seems to neither affect full sample estimates (Table 4) nor estimates of restricted samples (Tables 6 and 7). Second, making the covariates more balanced does not affect the direction and statistical significance of the estimated treatment effects, although estimated magnitudes become somewhat inflated in our most rigorous specification with the most constrained sample. Overall, these conclusions strongly suggest that predetermined differences between treatment and control groups – which could invalidate our IV approach – do not strongly affect the FD-IV estimates.

6.3 Constraining second home investments: impact on existing second home investors

Another pertinent question is whether the SHI affected the price growth of second homes. 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 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.[26] 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 8. 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 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 touristic 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 during the past, leaving only properties without conversion potential in the stock of primary residences.

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

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 5).[27]

6.4 Other results and robustness checks

Negative economy effect and wages

The results of the previous sections suggest that the SHI negatively affects local economies of treated municipalities by increasing the 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. To test this proposition we report results for employees' wages in Appendix Table A2, employing a FD-IV model. The ban does not seem to significantly affect wage growth once pre-trends in wages are accounted for and when the control and the treated group are balanced. In line with our model, the impact of the ban becomes negative for the model with the strictest sample restriction. However, the effect is small in magnitude and not statistically significant.

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 a ten year (or more) employment relationship by giving a three month-notice and without providing any justification for it. Thus, it would appear to be much easier for firms to fire workers to counter an expected negative shock to the local economy than to lower wages.

Sorting and heterogeneous treatment effects

Our theoretical framework predicts heterogeneous treatment effects δ_i for a given outcome to the extent that the growth in the number of second home investors varies across municipalities. Workers might sort into or out of a treated municipality according to the expected gain/loss in order to maximize their utility (selection on gains) and second home investors may shift their housing demand to the nearest control municipalities (i.e., the closest substitutes). What does the causal effect δ represent when heterogeneous effects are present? To answer this question,

[27] Municipalities had to ascertain that the conversion of primary residences into secondary ones was not driven by pure 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.

we must assess whether sorting of households according to the treatment status did occur. Let us consider the random coefficient version of equation (10):

$$\Delta \ln(y_{i13-14}) = \tau + \delta_i D_i + \boldsymbol{\beta}_1 \Delta \boldsymbol{x}_{i10-11} + \Delta u_{i13-14} =$$
$$\tau + \delta D_i + \boldsymbol{\beta}_1 \Delta \boldsymbol{x}_{i10-11} + (\delta_i - \delta) D_i + \Delta u_{i13-14}, \tag{12}$$

where δ_i represents the heterogeneous effect of the SHI on municipality i. The treatment effect δ estimated by model (12) is biased if the heterogeneous effect δ_i correlates with the treatment assignment D_i, i.e. $E((\delta_i - \delta)D_i) \neq 0$. This corresponds to sorting of households across municipalities with respect to potential gains.

However, our findings so far are indicative that sorting may not be of primary importance in our empirical setting. In fact, if sorting of households and investors were present, we should observe a shift in the distribution of primary and second home transactions with respect to the municipalities' second home rates. This, however, is not the case. The histogram of transactions presented in Figure 5 shows no evident change in the distribution of transacted primary and second homes pre- and post-SHI, suggesting that sorting from treated municipalities to control ones – and vice versa – did not take place.

To test more formally whether sorting of households occurred, we estimate the impact of the SHI on the growth of the resident population – including both homeowners and renters – for the full sample and for the two sample restrictions discussed in the previous section. Appendix Table A3 reveals that the impact of the ban on the resident population is not statistically significant and close to zero in magnitude across all specifications, suggesting that the SHI did not affect population growth in treated municipalities.

To further verify the robustness of our previous estimates to potential sorting effects, we estimate the FD-IV model for the price of primary homes, unemployment rates, and the number of elderly when we use as control group municipalities situated more than 5 kilometers away from the nearest treated ones (see Figure 3 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 major natural amenities. Appendix Table A4 documents the results. Reassuringly, the estimated impacts are virtually identical to our baseline estimates reported in Table 4.[28] The choice of a 5 km distance band is arbitrary. In a next step, we thus vary this distance band continuously to document that the estimated effects of our FD-IV specifications are robust to the choice of the distance band. The results are

[28] To the extent that second home rates capture the proximity to major natural amenities better than physical distance, municipalities in the control group that have a second home rate close to the threshold might also have been impacted by the ban through a shift of the demand. We thus experimented by dropping municipalities in the control group that have a second home rate between 15% and 20%. The restriction leaves the baseline estimates of Table 4 unchanged.

illustrated in Figure 6. 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.[29]

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 as a disamenity, which would eliminate any positive effect of the ban. Our voting analysis indeed seems to suggest that the SHI was approved for social envy reasons of primary residents in non-affected (control) areas more than anything else. Third, the SHI reinforced the price differential of primary residences located in control and treated municipalities. This implies lower asset values of 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.[30] 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).

Conversion option and the impact of age-related characteristics

In our main analysis we dropped primary houses built after 2012 (i.e., houses that no longer possess a conversion option) from our regression sample to be able to compare 'like with like' housing units pre and post the ban.

To disentangle the impact of the SHI on age-related characteristics from its 'direct' effect on the price of primary homes, in a first step, we re-estimate our FD-IV model including primary houses built after 2012 back into our regression sample. The obtained treatment effect can be interpreted as the 'total' effect of the SHI – the sum of a compositional effect (more older, less valuable properties may be traded post ban) and a direct effect (i.e., the effect we are primarily interested in). In a second step, we construct a hedonic price index for age-related characteristics and analyze how this price index was affected by the ban. More precisely, we estimate the following equation using the price of primary homes over the 2008-2009 period as outcome variable:

$$\ln(P_{i08-09}) = \alpha_0 + \alpha_1 age_{i08-09} + \alpha_2 condition_{i08-09} + \alpha_3 quality_{i08-09} + \epsilon_{i08-09}, \quad (13)$$

[29] 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.

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

where *age*, *condition*, and *quality* are age-related characteristics for the same period. The estimated parameters $\hat{\alpha}_i$, $i = 0,1,2,3$ allow us to predict (the log of) the price of primary homes $\widehat{\ln(P_{it})}$ before (2010-2011) and after (2013-2014) the ban while keeping the valuation of these characteristics fixed. We then estimate the FD-IV model using the predicted log prices as outcome variables (without including age-related characteristics as controls). The estimated effect describes the composition effect (i.e., the effect of the ban on primary house prices via affecting age-related characteristics). The difference between the estimates of the first and second step corresponds to the direct effect of the ban on the price of primary homes.

Appendix Tables A5 and A6 document FD-IV estimates for total and composition effects, respectively. To deal with potential endogeneity linked to unobserved trends, each table reports the robustness of the estimated treatment effects for the sample restrictions discussed in the previous section. Table A5 reports treatment estimates of the total effect of the ban on the price growth of primary homes that are very similar to those reported in Tables 4, 6 and 7. Table A6 reveals that the composition effect is statistically significant and economically meaningful (around -5% in the preferred estimate) for the full sample, however becomes statistically completely insignificant when imposing sample restrictions with the effect going close to zero for the more rigorous of the two restrictions.

The above results are indicative that i) excluding houses without a conversion option from our sample does not greatly affect our main FD-IV treatment estimates, and ii) composition effects of age-related characteristic are not of significant importance in our setting: the total treatment effect of the ban on the price growth of primary homes appears to be similar to the direct effect. Interestingly, these conclusions add a further piece of evidence in favor of the poor substitutability assumption between primary and second homes. In fact, if the value of the conversion option approaches zero due to poor substitutability, there is no economic incentive for residents to buy older houses that potentially can still be converted, making the composition effect irrelevant.

Placebo test and the parallel trends assumption

Finally, we conduct a placebo test to verify that no treatment effect was present before the policy implementation. Specifically, we use the years 2006-2007 and 2008-2009 as pre-policy periods, and 2010-2011 as post-policy period. As no data on the number of elderly living in a municipality are available before 2010, we only report placebo tests for the price of primary homes and unemployment rates.[31] Appendix Table A7 reports the estimation results for the FD-IV model. The treatment effect is statistically insignificant and close to zero in all cases.

These results suggests that i) pre-policy growth rates of the price of primary homes and of the unemployment rate are orthogonal to the observed treatment status, and ii) individuals did not anticipate the effect of the ban. Our placebo test is also a test for parallel trends in the pre-policy periods. As parallel trends is the main identifying assumption in DD models, these results help explain why the treatment effect estimates presented in Table 2 are similar to those reported in Tables 3 and 4. The fact that pre-ban outcome dynamics are not different adds further credibility

[31] We also note that because no data on the share of foreign residents was available prior to 2008, we had to exclude this variable from our estimated model.

to our main FD-IV estimates, as historic second home rates do not seem to capture permanent differences between treatment and control groups 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.

7 Conclusion

Rising inequality has led to a global political backlash against wealthy elites. One increasingly popular policy is to constrain or impose an outright ban on new second home investments in high-amenity places (highly touristic places or superstar cities). We propose a dynamic general equilibrium model that describes the mechanisms through which this policy may affect primary residents and existing second home investors.

Local residents face a basic trade-off. Constraints on second home investments hurt the local economy but provide benefits in the form of landscape preservation effects. Theory suggests that the predicted impact on the price growth of primary homes depends on a number of factors: whether primary and second homes are close substitutes, whether local residents attach a strong disamenity value to the presence of second home investors, and the local labor output elasticity in the tourism sector.

Exploiting the unique empirical setting provided by the unexpected approval of the SHI in March 2012, we find that the ban on the construction of new second homes reduced the price growth of primary homes in the areas that were affected by the ban by around 15% and increased the growth in the unemployment rate by about 12%. We do not find a positive (anticipated) landscape preservation effect. Estimating the effect of the ban on the price growth of second homes is challenging due to a small sample size issue. This caveat aside, our DD results suggest that the ban increased the price growth of second homes by about 26%.

Our findings are consistent with the view that primary homes in Swiss tourist areas are poor substitutes for second homes. In a setting with poor substitutability, in the extreme, the option to convert a primary residence into a second home is worthless and it does not provide a hedge against the negative impact of banning investors.

Constraining new second home investments hurts local homeowners via higher unemployment growth rates and lower price growth of primary homes. Renters benefit from lower rents but overall they are not better off. This is because the fall in rents is likely commensurate to the negative local economy effect. 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 owners of such homes.

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 second home investments may reinforce rather than reduce wealth inequality in highly touristic areas. While bans do nothing to improve local economies, local taxes on the value of land or (investment) property could potentially help local economies whilst at the same time preserve the landscape.

To what extent our findings also apply to superstar cities is less clear-cut. Labor markets of large superstar cities are much more diversified and less dependent on second home buyers. If a ban on second home investments reduces upward pressure on housing rents and prices, then both local labor supply and local wages may go up. This is because in the case of superstar cities, labor markets may not depend much on the consumption of goods and services by the investors in second homes. Instead, lower housing costs will attract more labor to superstar cities. In the presence of agglomeration externalities, this may raise local wages and may lead to an increase in the aggregate productivity of an entire country, as in Hsieh and Moretti (2017). We leave the analysis of these effects for future research.

References

Altonji, J.G., Todd E. E., and Taber, C.R. (2005). Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools. *Journal of Political Economy* 113(1): 151-184.

Badarinza, C., Ramadorai, T. (2016). Home Away From Home? Foreign Demand and London House Prices. Working Paper.

Baron, R.M., Kenny, D.A. (1986). The moderator–mediator variable distinctionin social psychological research: Conceptual, strategic, and statistical considerations. *Journal of Personality and Social Psychology* 51: 1173-1182.

Bayer, P., Geissler, C., Mangum, K., & Roberts, J.W. (2015). Speculators and Middlemen: The Role of Intermediaries in the Housing Market. Economic Research Initiatives at Duke (ERID) Working Paper No. 93.

Belsky, E., Di, Z. X., McCue, D. (2007). Multiple-home ownership and the income elasticity of housing demand. In G. K. Ingram, & Y. Hong, Land Policies and their Outcomes. Cambridge (MA): Lincoln Institute of Land Policy.

Cellini, S.R., Ferreira, F., Rothstein, J. (2010). The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design, *Quarterly Journal of Economics* 125(1): 215-261.

Chay, K., Greenstone, M., (2005). Does Air Quality Matter? Evidence from the Housing Market, *Journal of Political Economy* 113(2): 376-424.

Chinco, A., Mayer, C. (2016). Misinformed Speculators and Mispricing in the Housing Market, *Review of Financial Studies* 29(2): 486-522.

Clapp, J.M., Eichholtz, P. and Lindenthal, T. (2013). Real Option Value and the Dynamics of House Prices, *Regional Science and Urban Economics* 43(6): 862-874.

Clapp, J.M., Salavei, K., (2010). Hedonic pricing with redevelopment options: A new approach to estimating depreciation effects, *Journal of Urban Economics* 67: 362-377.

Clapp, J.M., Jou, J.B. and Lee, T., (2012). Hedonic Models with Redevelopment Options under Uncertainty. *Real Estate Economics* 40: 197-216

Cvijanovic, D., Spaenjers, C. (2015). Real Estate as a Luxury Good: Non-Resident Demand and Property Prices in Paris. HEC Paris Research Paper No. FIN-2015-1073.

Desmet, K. and Rossi-Hansberg, E. (2013). Urban Accounting and Welfare. *American Economic Review*, 103(6): 2296-2327.

Economist, The (2016). Second homes – Stay Away. There's a new villain in town – at weekends and during the summer. May 14: 12-13.

Favilukis, J.Y. and Van Nieuwerburgh, S. 2017. Out-of-Town Home Buyers and City Welfare. CEPR Discussion Paper, No. 12283.

Gaubert, C. (2015). Firm Sorting and Agglomeration. Unpublished.

Glaeser, E.L. (2008). Cities, Agglomeration, and Spatial Equilibrium. *Oxford University Press*.

Glaeser, E.L. and Gottlieb, J.D. (2009). The Wealth of Cities: Agglomeration Economies and Spatial Equilibrium in the United States. *Journal of Economic Literature*, 47(4): 983-1028.

Greenstone, M., Gallagher, J. (2008). Does Hazardous Waste Matter? Evidence from the Housing Market and the Superfund Program, *Quarterly Journal of Economics* 123(3): 951-1003.

Gyourko, J., Mayer, C. and Sinai, T. (2013). Superstar Cities. *American Economic Journal: Economic Policy*, 5(4): 167-99.

Haughwout, A., Lee, D., Tracy, J., van der Klaauw, W. (2014). Real Estate Investors, the Leverage Cycle, and the Housing Market Crisis. Federal Reserve Bank of New York Staff Reports, 514.

Hilber, C.A.L. and Vermeulen, W. (2016). The Impact of Supply Constraints on House Prices in England. *Economic Journal*, 126(591): 358-405.

Hsieh, C.T, and Moretti, E. (2017). Housing Constraints and Spatial Misallocation. NBER Working Paper No. 21154.

Huang, Y., Yi, C. (2011). Second Home Ownership in Transitional Urban China. *Housing Studies*, 26(03), 423-447.

Roback, J. (1982). Wages, Rents, and the Quality of Life. *Journal of Political Economy*, 90(6): 1257–78.

Rognlie, M. (2014). A note on Piketty and diminishing returns to capital. MIT Working Paper.

Rosen, S. (1979). "Wage-Based Indexes of Urban Quality of Life." In Current Issues in Urban Economics, ed. Peter Mieszkowski and Mahlon Straszheim, 74–104. Baltimore and London: Johns Hopkins University Press.

Sá, F. (2016). The Effect of Foreign Investors on Local Housing Markets: Evidence from the UK. Centre for Macroeconomics Discussion Paper, No. 1639.

Suher, M. (2016). Is Anybody Home? The Impact of Taxation of Non-Resident Buyers. NYU Furman Center, mimeo, February.

TABLES

TABLE 1A

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
Wages (1’000 CHF)	38.21	195.48	67.95	16.00	40.75	203.23	69.01	15.97
Nb. of elderly (1'000)	0.01	62.45	0.77	2.37	0.01	62.23	0.84	2.42
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 building 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				
Resident population (1'000)	0.13	374.92	4.54	13.69				
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 new residential buildings (1'000)	0	1.75	0.03	0.07				
Number of municipalities		1556				1524		

Note † Unemployment rates are expressed relative to the *total* population of a municipality. †† The age of the building at the 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 age variable can take negative values.

TABLE 1B

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
Wages (1’000 CHF)	35.05	99.79	55.66	9.00	32.85	325.21	58.30	19.37
Nb. of elderly (1'000)	0.01	4.60	0.36	0.48	0.01	4.88	0.42	0.53
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 building 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				
Resident population (1'000)	0.03	24.89	1.87	2.58				
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 new residential buildings (1'000)	0	0.15	0.01	0.02				
Number of municipalities		276				255		

Note: † Unemployment rates are expressed relative to the *total* population of a municipality. †† The age of the building at the 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 age variable can take negative values.

TABLE 2

DD estimates

Dependent variable	Log price of primary homes			Log unemployment rate			Log elderly		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Observed treatment × Post	-0.142**	-0.152***	-0.119***	0.0787	0.0823*	0.0969**	-0.00725	0.0121	0.184***
	(0.0571)	(0.0450)	(0.0456)	(0.0602)	(0.0428)	(0.0396)	(0.102)	(0.0855)	(0.0693)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FE and lagged controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Predetermined outcome level	No	No	Yes	No	No	Yes	No	No	Yes
Observations	2,812	2,812	2,812	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	0.014	0.649	0.737

Notes: We report heteroscedastic-robust standard errors 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 having 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 lagged controls. Data is aggregated at the municipality level by computing two-year averages in these periods. The considered sample pools data on municipalities for which housing transactions were available pre and post the implementation of the SHI. Houses built after 2012, and not having a conversion option anymore, have been excluded from the sample before aggregation.

TABLE 3

FD estimates

Dependent variable	Δ Log price of primary homes			Δ Log unemployment rate			Δ Log elderly		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Observed treatment	-0.142***	-0.140***	-0.191***	0.0787***	0.0757***	0.0651***	-0.00725	-0.00676	-0.0116*
	(0.0386)	(0.0376)	(0.0365)	(0.0231)	(0.0236)	(0.0230)	(0.00645)	(0.00653)	(0.00667)
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
R-squared	0.020	0.128	0.196	0.012	0.023	0.122	0.001	0.012	0.065

Notes: We report heteroscedastic-robust standard errors 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 having 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 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, and not having a conversion option anymore, have been excluded from the sample before aggregation.

TABLE 4

FD-IV estimates

Panel (a): TSLS: *Second stage*									
Dependent variable	Δ Log price of primary homes			Δ Log unemployment rate			Δ Log elderly		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Observed treatment	-0.152***	-0.147***	-0.190***	0.121***	0.118***	0.111***	0.00246	0.00322	-0.00205
	(0.0461)	(0.0448)	(0.0443)	(0.0252)	(0.0257)	(0.0254)	(0.00839)	(0.00840)	(0.00849)
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	1623	1619	1632	1623	1619	1620	1623	1619	1627
Panel (b): TSLS: *First stage*									
Dependent variable	Observed treatment								
2000 second home rate	2.066***	2.068***	2.043***	2.066***	2.068***	2.067***	2.066***	2.068***	2.063***
	(0.0513)	(0.0514)	(0.0506)	(0.0513)	(0.0514)	(0.0513)	(0.0513)	(0.0514)	(0.0511)
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: We report heteroscedastic-robust standard errors 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 having 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 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, and not having a conversion option anymore, 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 5

FD covariates balance

	Control	Treated	Control	Treated	Control	Treated	p-values		
	-	-	CBD>10 km & Ski>0 km		CBD>10 km & 15%-30%				
	(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
No of elderly	6.05	5.70	5.83	5.62	5.97	5.66	**0.00**	0.13	0.23
Δx_{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)-(6) report the means of FD and FD-IV controls for the full sample of municipalities considered in Tables 2, 3, and 4 (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 6

FD-IV estimates: Exclusion of municipalities near major CBDs and ski resorts

Panel (a): TSLS: *Second stage*									
Dependent variable	Δ Log price of primary homes			Δ Log unemployment rate			Δ Log elderly		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Observed treatment	-0.172**	-0.195***	-0.237***	0.0962*	0.0931*	0.105*	0.0144	0.0174	0.0145
	(0.0734)	(0.0703)	(0.0661)	(0.0568)	(0.0546)	(0.0563)	(0.0184)	(0.0181)	(0.0181)
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	502	502	502	502	502	502	502	502	502
Kleibergen-Paap F	536.8	524.9	517.4	536.8	524.9	520	536.8	524.9	526.7
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***	2.150***	2.173***	2.171***
	(0.0928)	(0.0949)	(0.0943)	(0.0928)	(0.0949)	(0.0954)	(0.0928)	(0.0949)	(0.0946)
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: We report heteroscedastic-robust standard errors 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 having 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 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, and not having a conversion option anymore, 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 7

FD-IV estimates: Fuzzy RDD

Panel (a): TSLS: *Second stage*									
Dependent variable	Δ Log price of primary homes			Δ Log unemployment rate			Δ Log elderly		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Observed treatment	-0.561***	-0.370**	-0.353**	0.243*	0.291**	0.251**	0.0197	0.0279	0.0265
	(0.169)	(0.149)	(0.149)	(0.125)	(0.116)	(0.105)	(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	129	129	129	129	129	129	129	129	129
Kleibergen-Paap F	35.02	38.55	37.71	35.02	38.55	37.01	35.02	38.55	37.15
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***	2.689***	2.848***	2.814***
	(0.454)	(0.459)	(0.467)	(0.454)	(0.459)	(0.469)	(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: We report heteroscedastic-robust standard errors 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 having 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 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, and not having a conversion option anymore, 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 8

DD estimates: Impact on the price growth of second homes

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)
Time fixed effects	Yes	Yes	Yes
FE and lagged controls	No	Yes	Yes
Predetermined outcome level	No	No	Yes
Observations	323	323	323
R-squared	0.015	0.562	0.562

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). The two-period analysis is structured similarly to the one of Table 2. Data available for all municipalities has been pooled for the pre (2010-2011) and post (2013-2014) periods. The average price of second homes in the full sample was about 597’000CHF pre and 637’000CHF post SHI in not treated municipalities. In those municipalities, the average number of transactions was 2.26 (pre) and 1.54 (post). In treated municipalities, the average price was about 629’000 (pre) and 649’000 (post), with an average number of transaction equal to 7.5 (pre) and 7.38 (post). Full summary statistics for all variables (including controls) are available from the authors upon request.

FIGURES

FIGURE 1

Voting results at the municipality level with respect to second home percentage

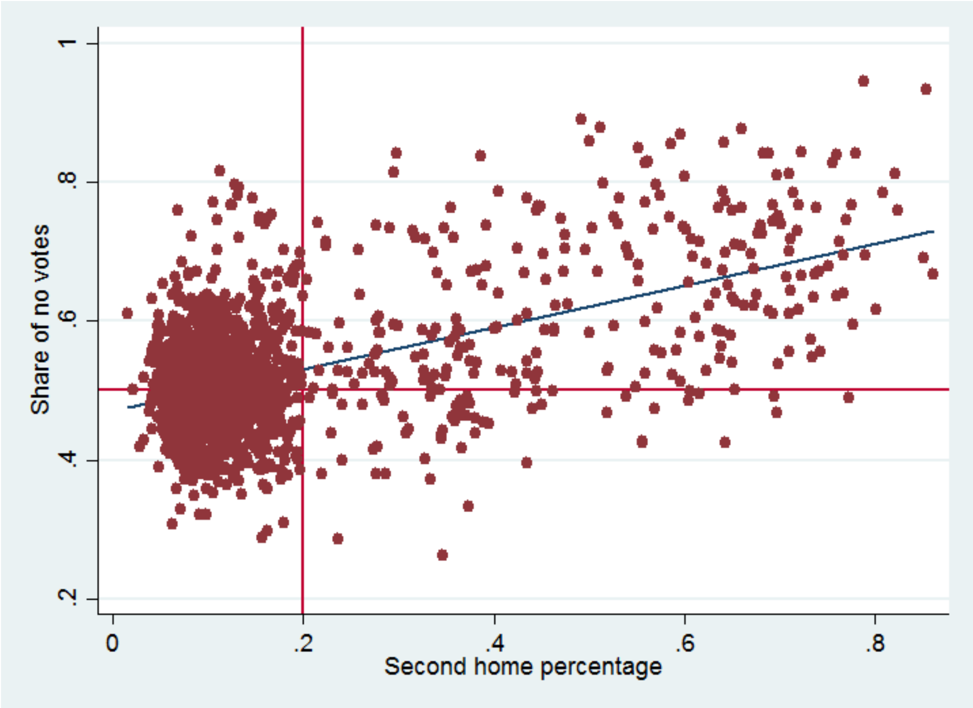
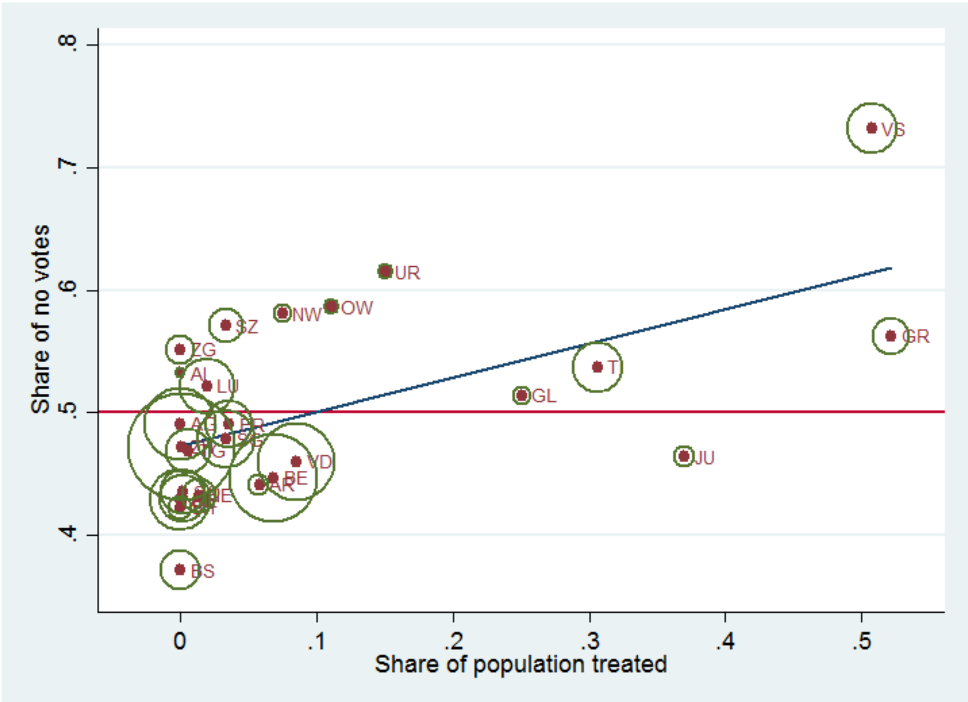


FIGURE 2

Voting results at the cantonal level with respect to share of treated population



Note: circles are proportional to the resident population of each canton.

FIGURE 3

Treatment and control groups

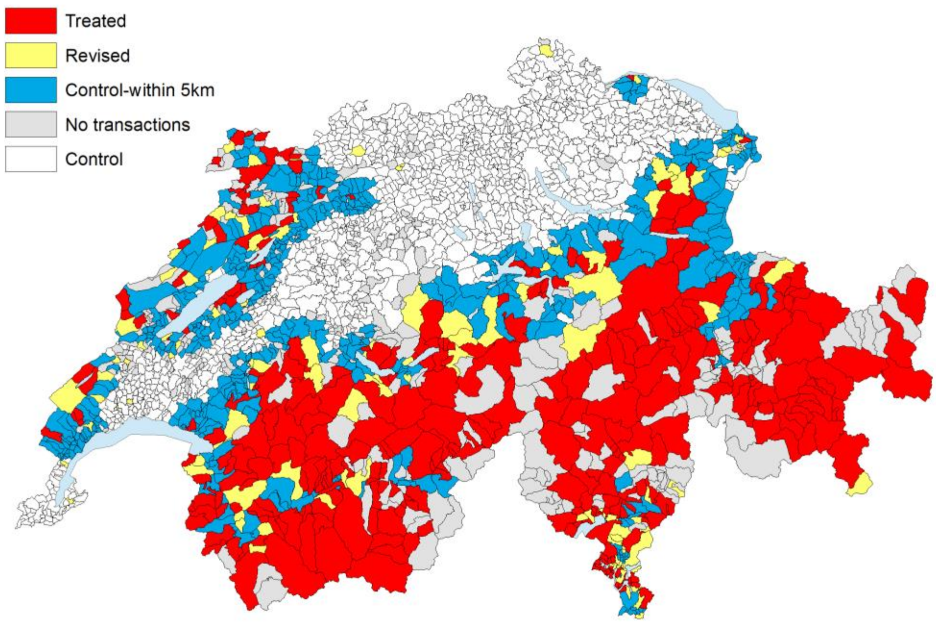


FIGURE 4

Second home rate distribution at the municipality level

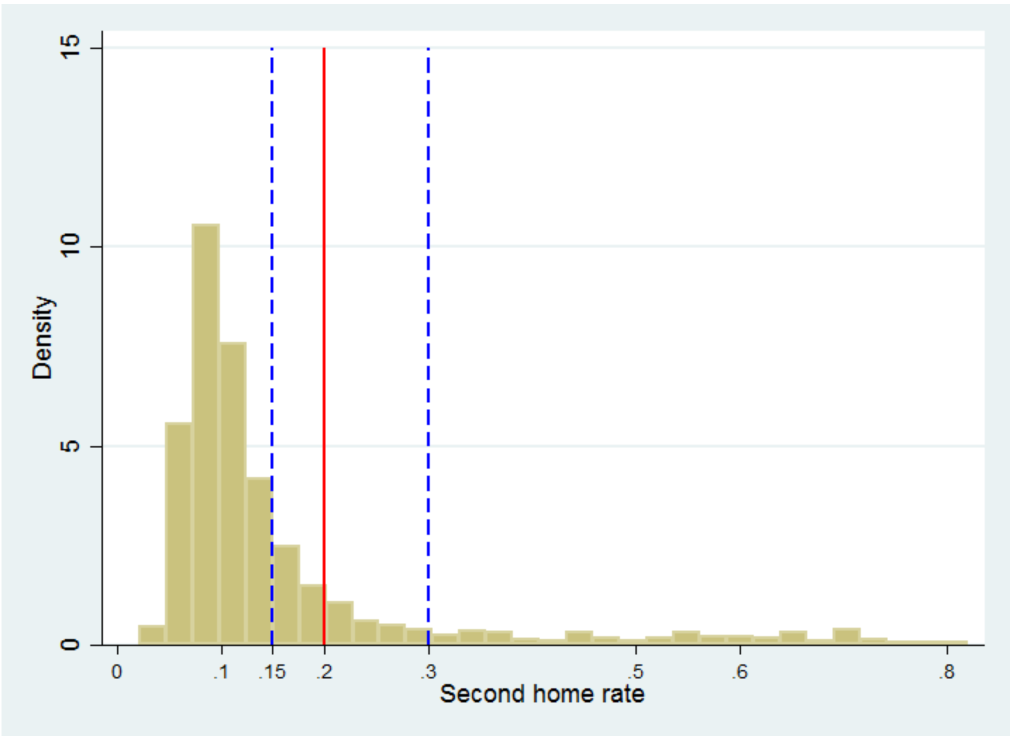


FIGURE 5

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

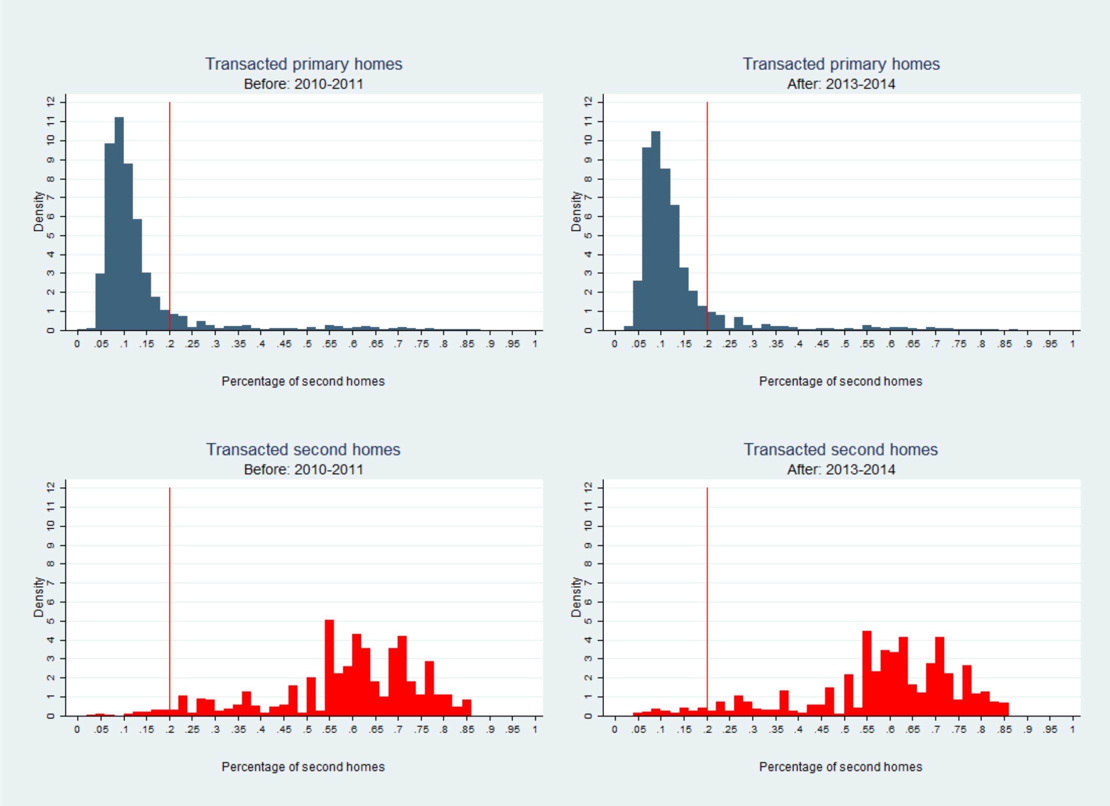
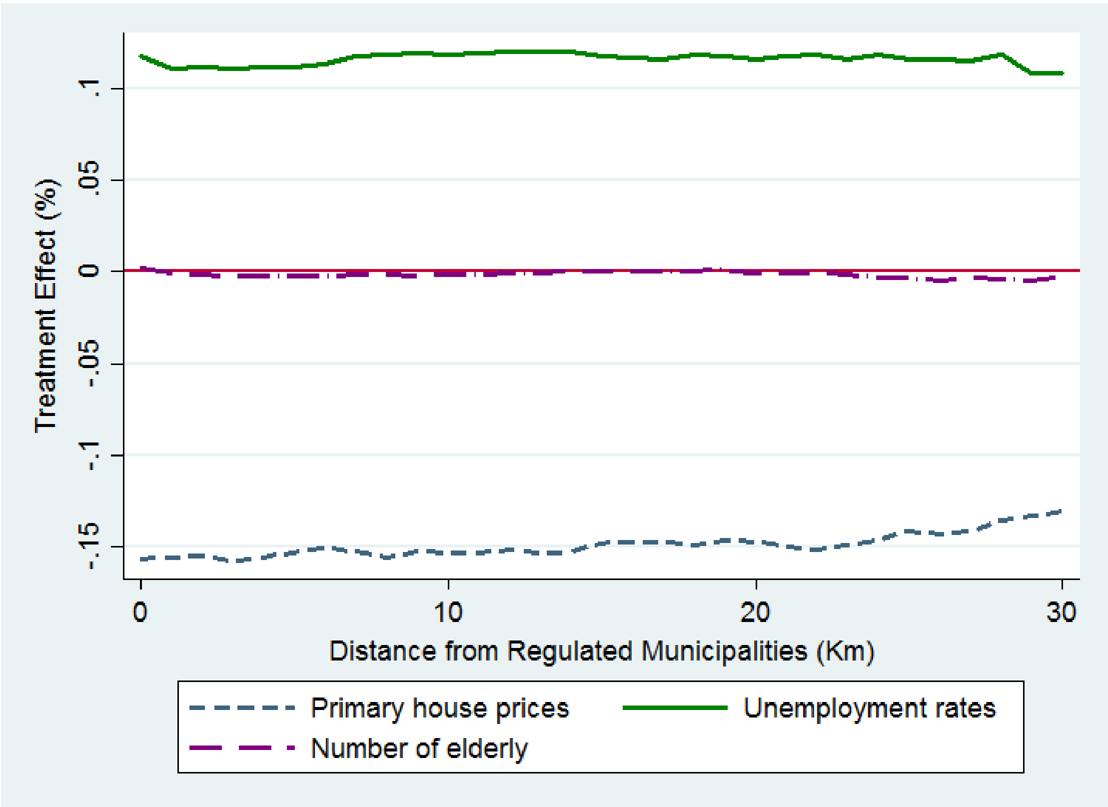


FIGURE 6

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



APPENDICES

Appendix A: Voting Results and Robustness Checks

TABLE A1
Voting results

Dependent variable	Share of no votes		
	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 in parentheses (*** p<0.01, ** p<0.05, * p<0.1). All municipalities for which second home rates, voting results, and included control were available in 2010-2011 are included in the sample. Municipalities having revised their second home rate are not included.

TABLE A2

FD-IV estimates: Wage regressions

Panel (a): TSLS: *Second stage*									
Dependent variable	Δ Employee wages								
	Full sample			CBD >10 km & Ski>0 km			CBD >10 km & [0.15,0.3]		
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: We report heteroscedastic-robust standard errors 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 having 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 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, and not having a conversion option anymore, 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 A3

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]		
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: We report heteroscedastic-robust standard errors 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 having 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 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, and not having a conversion option anymore, 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 A4

FD-IV estimates: Excluding municipalities within 5 km from treated ones (impact of households and investors' sorting)

Panel (a): TSLS: *Second stage*									
Dependent variable	Δ Log price of primary homes			Δ Log unemployment rate			Δ Log elderly		
Observed treatment	-0.148***	-0.142***	-0.191***	0.113***	0.112***	0.105***	-0.000813	-0.000846	-0.00581
	(0.0459)	(0.0441)	(0.0441)	(0.0250)	(0.0251)	(0.0248)	(0.00840)	(0.00841)	(0.00851)
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	1,027	1,027	1,027	1,027	1,027	1,027	1,027	1,027	1,027
Kleibergen-Paap F	1385	1375	1350	1385	1375	1374	1385	1375	1376
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***	2.130***	2.128***	2.120***
	(0.0572)	(0.0574)	(0.0566)	(0.0572)	(0.0574)	(0.0573)	(0.0572)	(0.0574)	(0.0572)
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: We report heteroscedastic-robust standard errors 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 having 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 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, and not having a conversion option anymore, 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 A5

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]		
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: We report heteroscedastic-robust standard errors 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 having 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 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. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000.

TABLE A6

FD-IV estimates: Composition effect

Panel (a): TSLS: *Second stage*									
Dependent variable	Δ Log hedonic prices of primary residences								
	Full sample			CBD >10 km & Ski>0 km			CBD >10 km & [0.15,0.3]		
Observed treatment	-0.0549**	-0.0473*	-0.0712***	-0.0582	-0.0498	-0.0787	-0.153	0.00341	0.0161
	(0.0272)	(0.0269)	(0.0270)	(0.0638)	(0.0647)	(0.0629)	(0.200)	(0.174)	(0.173)
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	1662	568.2	558.6	556.1	32.12	34.64	34.17
Panel (b): TSLS: *First stage*									
Dependent variable	Observed treatment								
Second home rates in 2000	2.041***	2.043***	2.033***	2.142***	2.162***	2.149***	2.558***	2.702***	2.750***
	(0.0497)	(0.0499)	(0.0498)	(0.0898)	(0.0915)	(0.0907)	(0.451)	(0.459)	(0.457)
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: We report heteroscedastic-robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Each numbered column describes the impact of the SHI on first-differenced (log) hedonic 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 having 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 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. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000. Hedonic prices used as outcome variable pre and post the ban are estimated in each period using a predetermined valuation (in the 2008-2009) of age-related characteristics.

TABLE A7

FD-IV estimates: Placebo tests

Panel (a): TSLS: *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)
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
Panel (b): TSLS: *First stage*						
Dependent variable	Observed treatment			Observed treatment		
Second home rates in 2000	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

Notes: We report heteroscedastic-robust standard errors 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. 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 lagged difference of controls. Data is aggregated at the municipality level by computing two-year averages in these periods. The observed treatment dummy is instrumented using second home rates as measured by the Federal Population Census in 2000.

Appendix B: 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: $N_{it} = \beta^{\frac{\gamma-1}{1-\beta-\gamma}}\gamma^{\frac{\gamma}{1-\beta-\gamma}}\bar{Z}p_{it}^{\frac{1}{1-\beta-\gamma}}A_{it}^{\frac{1}{1-\beta-\gamma}}W_{it}^{\frac{\gamma-1}{1-\beta-\gamma}}$ (B1)

Primary residents' spatial equilibrium: $V_t = \theta_i N_{it}^{\mathcal{S}\,\eta}\frac{W_{it}}{r_{it}^a}$ (B2)

Investors' spatial equilibrium: $V_t^{\mathcal{S}} = \theta_i^{\mathcal{S}} N_{it}^{\mathcal{S}\,\epsilon}\frac{W_t^{\mathcal{S}}}{p_{it}^{1-b}r_{it}^{s,b}}$ (B3)

Primary residences housing market clearing: $\frac{aN_{it}W_{it}}{r_{it}} = H\left(\frac{(r-g_i)P_{it}}{(r-g_i^c)(1+g_i^c)^t}\right)^{\rho_i}$ (B4)

Secondary residences housing market clearing: $\frac{bN_{it}^{\mathcal{S}}W_t^{\mathcal{S}}}{r_{it}^{\mathcal{S}}} = H^{\mathcal{S}}\left(\frac{(r-g_i^{\mathcal{S}})P_{it}^{\mathcal{S}}}{\left(r-g_i^{\mathcal{S},c}\right)\left(1+g_i^{\mathcal{S},c}\right)^t}\right)^{\rho_i}$ (B5)

Tourism services clearing: $\beta^{\frac{\beta}{1-\beta-\gamma}}\gamma^{\frac{\gamma}{1-\beta-\gamma}}p_{it}^{\frac{\beta+\gamma}{1-\beta-\gamma}}A_{it}^{\frac{1}{1-\beta-\gamma}}W_{it}^{\frac{-\beta}{1-\beta-\gamma}} = N_{it}^{\mathcal{S}}(1-b)\frac{W_t^{\mathcal{S}}}{p_{it}}$ (B6)

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

$$\ln\left(\frac{N_{it+1}}{N_{it}}\right) = \frac{1}{1-\beta-\gamma}\ln\left(\frac{p_{it+1}}{p_{it}}\right) + \frac{1}{1-\beta-\gamma}\ln\left(1+g_{A_i}\right) + \frac{\gamma-1}{1-\beta-\gamma}\ln\left(\frac{W_{it+1}}{W_{it}}\right) \quad \text{(B1')}$$

$$\ln(1+g_V) + a\ln\left(\frac{P_{it+1}}{P_{it}}\right) = \eta\ln\left(\frac{N_{it+1}^{\mathcal{S}}}{N_{it}^{\mathcal{S}}}\right) + \ln\left(\frac{W_{it+1}}{W_{it}}\right) \quad \text{(B2')}$$

$$\ln(1+g_{V^{\mathcal{S}}}) + b\ln\left(\frac{P_{it+1}^{\mathcal{S}}}{P_{it}^{\mathcal{S}}}\right) + (1-b)\ln\left(\frac{p_{it+1}}{p_{it}}\right) = \epsilon\ln\left(\frac{N_{it+1}^{\mathcal{S}}}{N_{it}^{\mathcal{S}}}\right) + \ln(1+g_{W^{\mathcal{S}}}) \quad \text{(B3')}$$

$$\ln\left(\frac{N_{it+1}}{N_{it}}\right) + \ln\left(\frac{W_{it+1}}{W_{it}}\right) = (\rho+1)\ln\left(\frac{P_{it+1}}{P_{it}}\right) - \rho\ln(1+g_c) \quad \text{(B4')}$$

$$\ln\left(\frac{N_{it+1}^{\mathcal{S}}}{N_{it}^{\mathcal{S}}}\right) + \ln(1+g_{W^{\mathcal{S}}}) = (\rho+1)\ln\left(\frac{P_{it+1}^{\mathcal{S}}}{P_{it}^{\mathcal{S}}}\right) - \rho\ln(1+g_c^{\mathcal{S}}) \quad \text{(B5')}$$

$$\frac{1}{1-\beta-\gamma}\ln\left(\frac{p_{it+1}}{p_{it}}\right) + \frac{1}{1-\beta-\gamma}\ln\left(1+g_{A_i}\right) - \frac{\beta}{1-\beta-\gamma}\ln\left(\frac{W_{it+1}}{W_{it}}\right) = \ln\left(\frac{N_{it+1}^{\mathcal{S}}}{N_{it}^{\mathcal{S}}}\right) + \ln(1+g_{W^{\mathcal{S}}}), \quad \text{(B6')}$$

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 $\ln(1+g_V), \ln(1+g_{V^{\mathcal{S}}}), \ln(1+g_{W^{\mathcal{S}}})$, $\ln(1+g_{A_i})$, $\ln(1+g_i^c)$, $\ln\left(1+g_i^{\mathcal{S},c}\right)$,

$a,\ b,,\eta,\ \epsilon,\ \rho,\beta,\gamma$. 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 B1 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 B1
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 B1 (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 η 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) ϵ 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

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

where $\tilde{N}_{it}^{\alpha}$ 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}^{β} 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 β) drops out from the system of equations in changes, we can simply substitute β with $\alpha + \beta$ in equations B1' and B6'. The new dynamic equilibrium is thus equal to the one in the absence of agglomeration economies with β replaced with $\alpha + \beta$. The resulting comparative static results are shown in Table B2.

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 α, i.e., when $c(a, b, \epsilon, \eta, \rho, \alpha + \beta, \gamma) > 0$. One can show that

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

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

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

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

Case 2 can easily be dismissed, as it implies negative values of α. 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 α 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 η: 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 B2
Treatment effects with agglomeration economies

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

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

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

However, as $\bar{\alpha}'=\bar{\alpha}-\frac{b}{(-1+b)\left(a-(1+\eta)(1+\rho)\right)}$, 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.

FOR ONLINE PUBLICATION:

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 described in the introduction. 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
UK	HM Treasury and George Osborne (2015). Spending Review and Autumn Statement 2015, Cm 9162. Morris, S. (2014). St. Ives council toys with banning outsiders buying holiday homes. *Guardian*, November 17. Swerling, G. (2014). St. Ives aims to turn tide on city dwellers with second home ban. *The Times*, November 7. The Economist (2016). To the lighthouse. April 2016. The Economist (2016). Stay away. May 2016. The Guardian (2016). St. Ives backs residents-only home ownership plan in referendum. May 2016.
New York	Barbanel, J. (2014). New Yourk City Mayor De Blasio Weighs Pied-à-Terre Tax. *Wall Street Journal*, September 23. Higgins, M. (2013). Tax-Abatement Changes Affect Many Unit Owners. *The New York Times*, March 26.
Israel	Gross, Judah Ari. (2015). Bid to make housing affordable sends buyers scrambling, but will it work? *The Times of Israel*. June 21.
Singapore	Harper, J. (2013). Singapore gets tough on foreign property buyers, *The Telegraph*, Jan 16. Shamim, A. (2011). Singapore Extends Housing Measures; Developers Drop. *BloombergBusiness*, January 14.
France	Le Parisien (2014). Résidences secondaires: l'Assemblé a voté la hausse de la taxe d'habitation. December 3. Samuel, H. (2014). Britons face tax hike on coveted French second homes. *Telegraph*, November 4.
China	Bloomberg. (2013). Beijing Curbs Second Home Buying as China Cools Property Market. *Bloomberg News*, 30 March 2013. Fung, E. (2015). China Lowers Down Payments for Buyers of Second Homes. *Wall Street Journal*, 30 March.

FOR ONLINE PUBLICATION:

Web-Appendix B: Simulations

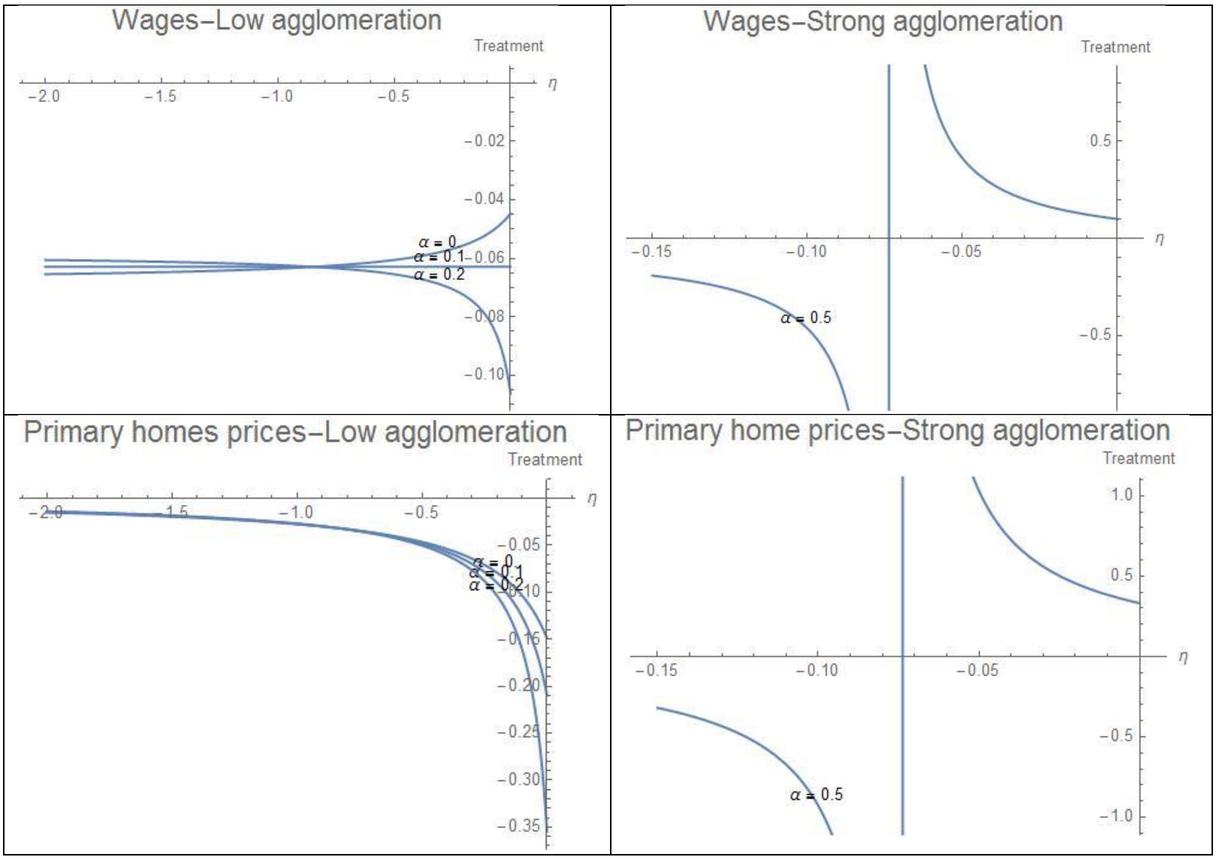
Figure W-B1 provides simulation graphs on the comparative static predictions with and without agglomeration economies. Different treatment effects corresponding to several agglomeration parameters are represented as a function of the disamenity parameter η of primary residents. In particular, we show that for α above a given value, the effect of the ban is reversed. To this end, we calibrate our model as follows:

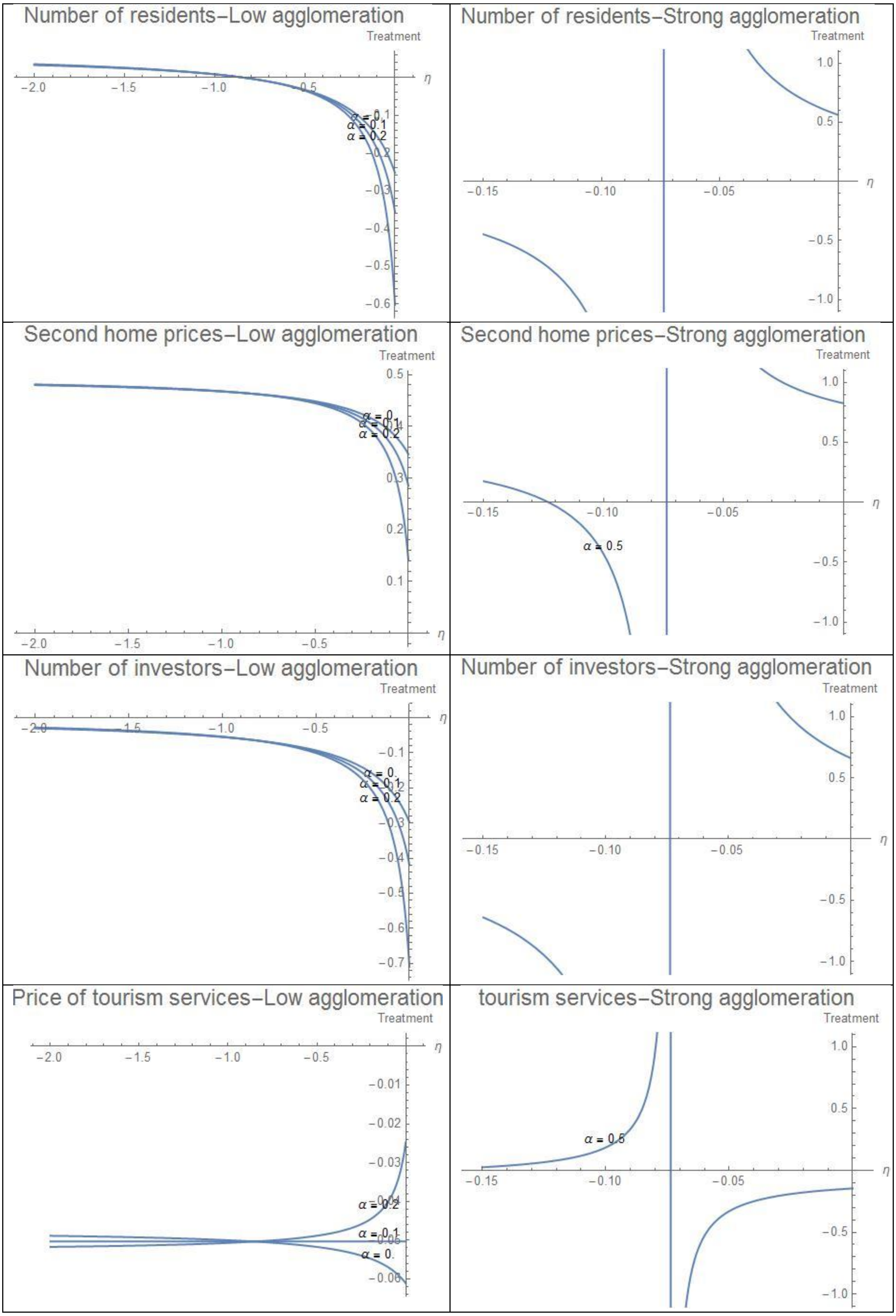
$a = 0.3,\ b = 0.15,\ \rho = 1,\ \beta = 0.7,\ \gamma = 0.2,\ \mathrm{g}_c^S = 0.01.$

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 α 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-B1

Simulation results – Agglomeration economies and reversed effects




Number of residents–Low agglomeration
Treatment
α = 0.1
α = 0.1
α = 0.2
η
Number of residents–Strong agglomeration
Treatment
η
Second home prices–Low agglomeration
Treatment
α = 0
α = 0.1
α = 0.2
η
Second home prices–Strong agglomeration
Treatment
α = 0.5
η
Number of investors–Low agglomeration
Treatment
α = 0.
α = 0.1
α = 0.2
η
Number of investors–Strong agglomeration
Treatment
η
Price of tourism services–Low agglomeration
Treatment
α = 0.2
α = 0.1
α = 0.
η
tourism services–Strong agglomeration
Treatment
α = 0.5
η

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 α 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.

FOR ONLINE PUBLICATION:

Web-Appendix C: 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.

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-C1 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-C1
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-C2 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-C3 contains a list of the major CBDs we used in our analysis.

TABLE W-C2

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-C4 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-C3

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-C4

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

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

FOR ONLINE PUBLICATION:

Web-Appendix D: Additional Robustness Checks

TABLE W1

FD-IV estimates: Clustered standard errors

Panel (a): TSLS: *Second stage*									
Dependent variable	Δ Log price of primary homes			Δ Log unemployment rate			Δ Log elderly		
Observed treatment	-0.152***	-0.147***	-0.190***	0.121***	0.118***	0.111***	0.00246	0.00322	-0.00205
	(0.0549)	(0.0518)	(0.0633)	(0.0336)	(0.0334)	(0.0325)	(0.00584)	(0.00609)	(0.00607)
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	870.2	981	755.7	870.2	981	897.9	870.2	981	980.6
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***	2.066***	2.068***	2.063***
	(0.0700)	(0.0660)	(0.0743)	(0.0700)	(0.0660)	(0.0690)	(0.0700)	(0.0660)	(0.0659)
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: We report standard errors clustered at the cantonal level 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 having 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 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, and not having a conversion option anymore, 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.

CENTRE FOR ECONOMIC PERFORMANCE
Recent Discussion Papers

1555	Filippo Di Mauro Fadi Hassan Gianmarco I.P. Ottaviano	Financial Markets and the Allocation of Capital: The Role of Productivity
1554	Keith Head Yao Amber Li Asier Minondo	Geography, Ties and Knowledge Flows: Evidence from Citations in Mathematics
1553	Nicholas Bloom Kalina Manova Stephen Teng Sun John Van Reenen Zhihong Yu	Managing Trade: Evidence from China and the US
1552	Gabriel M. Ahlfeldt Wolfgang Maennig Steffen Q. Mueller	The Generation Gap in Direct Democracy
1551	Keith Head Tierry Mayer	Brands in Motion: How Frictions Shape Multinational Production
1550	Natalie Chen Dennis Novy	Currency Unions, Trade and Heterogeneity
1549	Felipe Carozzi	The Role of Demand in Land Re-Development
1548	Nick Bloom Brian Lucking John Van Reenen	Have R&D Spillovers Changed?
1547	Laura Alfaro Nick Bloom Paola Conconi Harald Fadinger Patrick Legros Andrew F. Newman Raffaella Sadun John Van Reenen	Come Together: Firm Boundaries and Delegation

1546	Jörg Kalbfuß Reto Odermatt Alois Stutzer	Medical Marijuana Laws and Mental Health in the United States
1545	Maria Molina-Domene	Specialization Matters in the Firm Size-Wage Gap
1544	Jonathan Colmer	Weather, Labor Reallocation and Industrial Production: Evidence from India
1543	Andrew B. Bernard Esther Ann Bøler Swati Dhingra	Firm-to-Firm Connections in Colombian Imports
1542	Gerard H. Dericks Hans R.A. Koster	The Billion Pound Drop: The Blitz and Agglomeration Economics in London
1541	Andrew B. Bernard Andreas Moxnes	Networks and Trade
1540	Does Gentrification Displace Poor Households? An 'Identification-Via-Interaction' Approach	Sevrin Waights
1539	Zhuan Pei Jörn-Steffen Pischke Hannes Schwandt	Poorly Measured Confounders Are More Useful On the Left than On the Right
1538	Konstantin Büchel Stephan Kyburz	Fast Track to Growth? Railway Access, Population Growth and Local Displacement in 19th Century Switzerland
1537	Stephen Gibbons Maria Sánchez-Vidal Olmo Silva	The Bedroom Tax

The Centre for Economic Performance Publications Unit
Tel: +44 (0)20 7955 7673 Email info@cep.lse.ac.uk
Website: http://cep.lse.ac.uk Twitter: @CEP_LSE