FOR ONLINE PUBLICATION:

Hilber, C.A.L. and O. Schöni (2020). On the Economic Impact of Constraining Second Home Investments.

SUPPLEMENTARY MATERIAL: WEB-APPENDICES

Web-Appendix A: References to policies on second homes

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

TABLE W-A1
Second homes policies around the world

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

TABLE W-A1 *(cont.)*

Second homes policies around the world

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

FOR ONLINE PUBLICATION:

Web-Appendix B: Additional figures

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

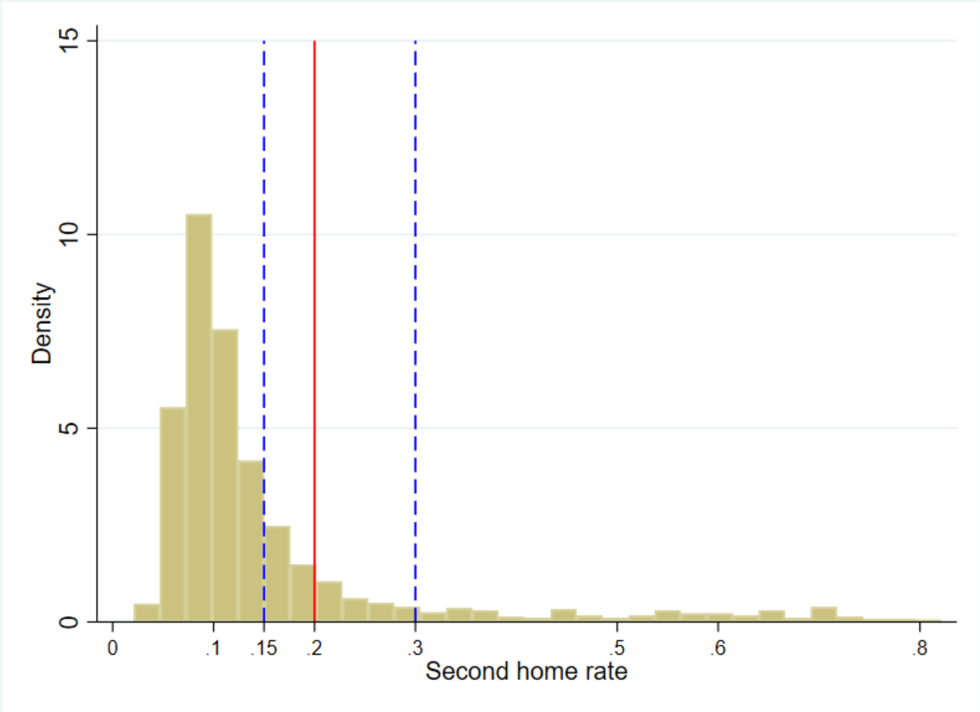
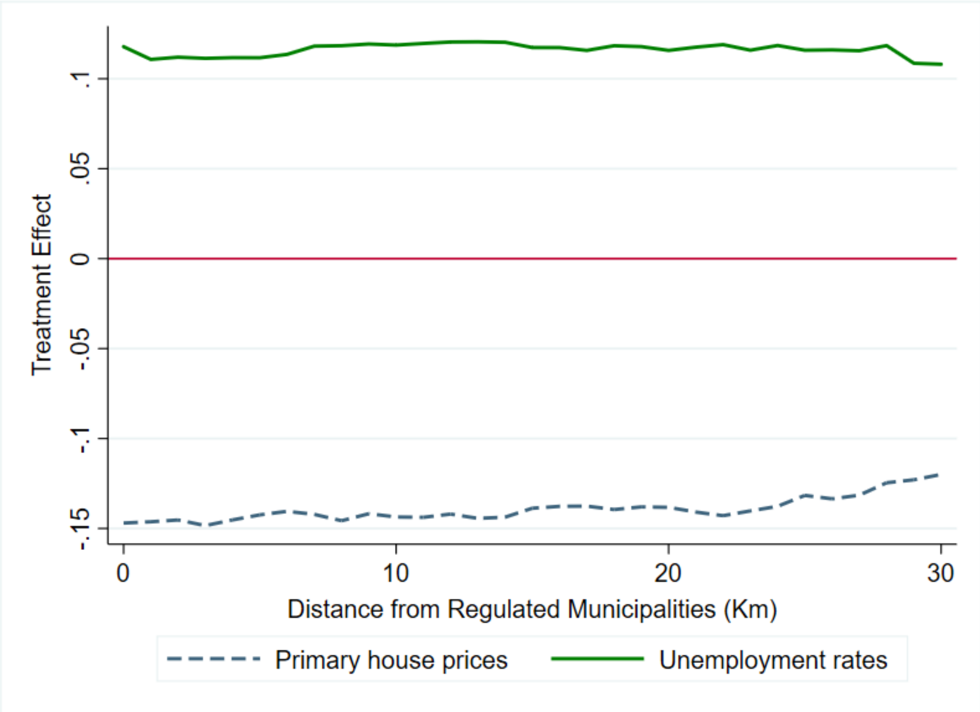


FIGURE W-B2
FD-IV treatment effects: excluding control municipalities within given distance from treated



Web-Appendix C: Theoretical results, extensions and simulations

C.1 Theoretical results and model extensions

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

Proof of Corollary 1

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

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

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

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

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

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

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

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

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

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

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

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

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

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

where we have used the notation $\frac{V_{t+1}}{V_t} = (1+g_V)$, $\frac{V_{t+1}^{\mathcal{S}}}{V_t^{\mathcal{S}}} = (1+g_{V^{\mathcal{S}}})$, $\frac{A_{it+1}}{A_{it}} = (1+g_{A_i})$, $\frac{W_{t+1}^{\mathcal{S}}}{W_t^{\mathcal{S}}} = \left(1+g_{W^{\mathcal{S}}}\right)$ 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 $\ln(1+g_V)$, $\ln\left(1+g_{V^{\mathcal{S}}}\right)$, $\ln(1+g_{W^{\mathcal{S}}})$, $\ln\left(1+g_{A_i}\right)$, $\ln\left(1+g_i^{c}\right)$, $\ln\left(1+g_i^{\mathcal{S},c}\right)$,

$a,\ b,,\eta,\ \epsilon,\ \rho,\beta,\gamma$. 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 W-C1 summarizes the impact of the ban on the endogenous variables of the system, with $c := -1+\epsilon+(-1+b+\epsilon)\rho-(-1+b)\gamma(1+\rho)+(-1+b)\beta(a-(1+\eta)(1+\rho))$.

TABLE W-C1

Treatment effects – No agglomeration economies

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

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

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

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

The effect of the ban on the number of primary residents is uncertain, as it depends on the magnitude of the parameter 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 $\eta \rightarrow 0$ 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

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

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

Deriving comparative static results when agglomeration economies are present is easy in our context. As the term N_{it}^{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 C1' and C6'. 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 W-C2.

We now investigate whether the sign of the impact of the ban on primary homes may be reversed and the implications for the price of second homes. The starting point is to investigate when the sign of the constant c is reversed by 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 $\bar{\alpha}:=\frac{-c(a,b,\epsilon,\eta,\rho,\beta,\gamma)}{(-1+b)(a-(1+\eta)(1+\rho))}$ denote a threshold value of agglomeration economies. This leads to the conditions

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

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

Case 2 can easily be dismissed, as it implies negative values of 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 W-C2

Treatment effects with agglomeration economies

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

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

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

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

C.2 Simulation

Figure W-C1 provides simulation graphs on the comparative static predictions with and without agglomeration economies. Different treatment effects corresponding to several agglomeration parameters are represented as a function of the disamenity parameter 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:

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

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-C1

Simulation results – Agglomeration economies and reversed effects

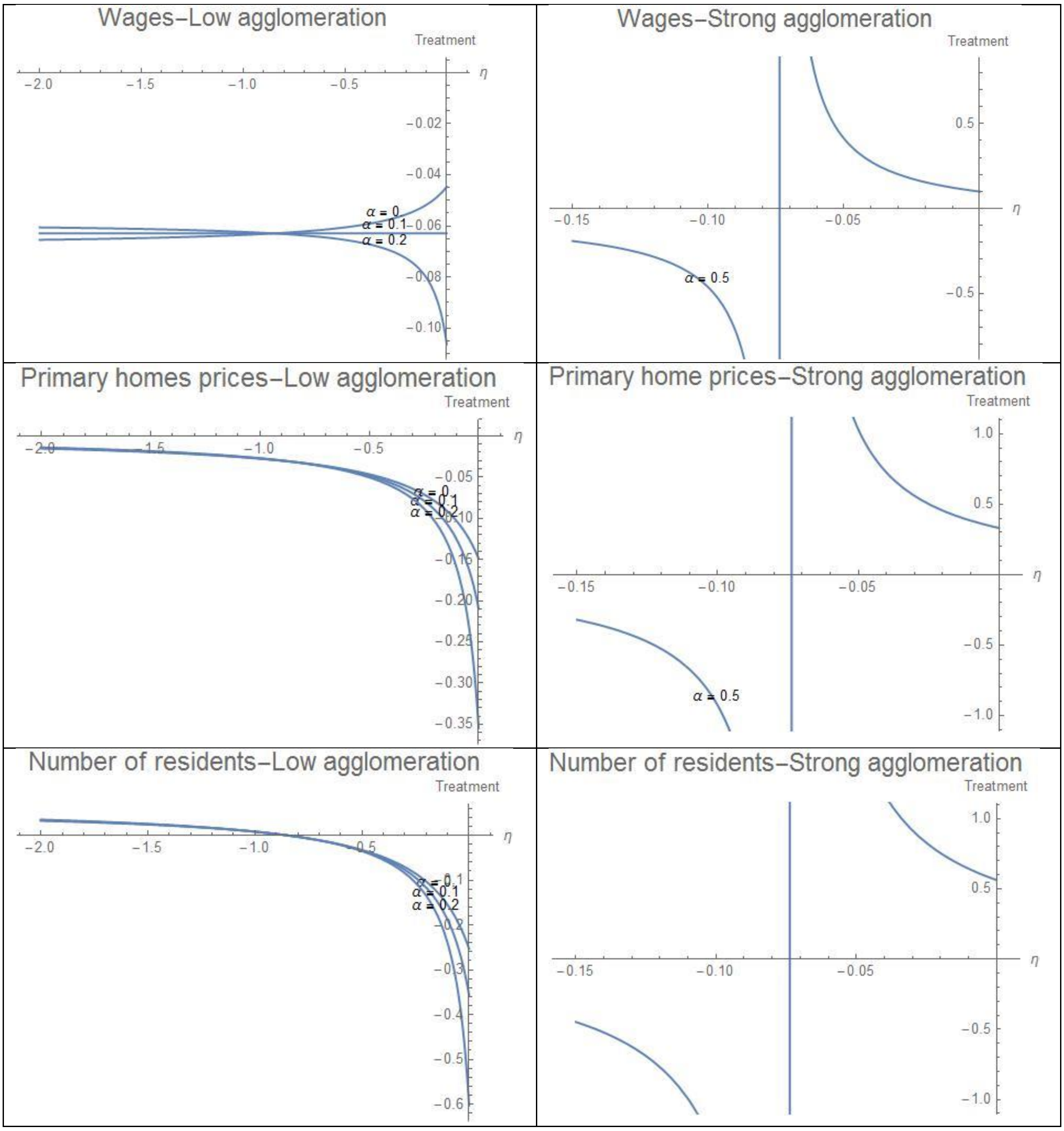
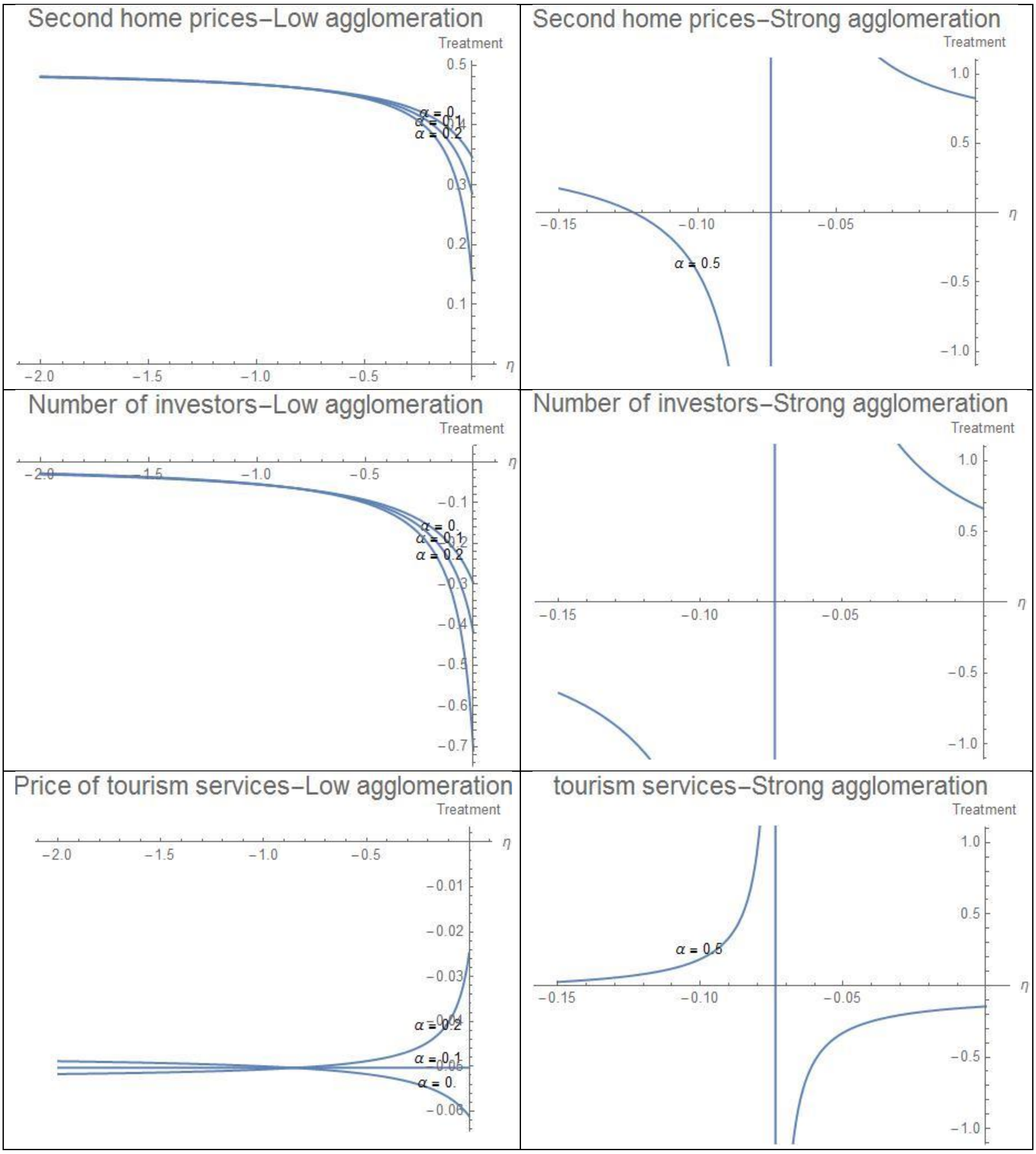


FIGURE W-C1 *(cont.)*



The above graphs show how investors' dislike and returns to scale affect the impact of the ban on the endogenous variables of the system. It can be seen that for the considered calibration the ban effects are reversed when the agglomeration parameter 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.

Web-Appendix D: Detailed description of data and sources

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

Housing transaction data

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

TABLE W-D1
Description of housing characteristics and data sources

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

Second home rates

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

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

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

Municipality-level characteristics

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

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

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

TABLE W-D2

Description of municipalities' characteristics and data sources

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

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

TABLE W-D3

Major urban centers (individual municipalities)

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

TABLE W-D4

Major ski resorts (individual municipalities)

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

Fiscal data

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

Web references

TABLE W-D5
Web references and links

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

Web-Appendix E: Robustness Checks and Detailed Estimation Results

TABLE W-E1

FD-IV estimates: Standard errors clustered at cantonal level

	Panel (a): TSLS: *Second stage*					
Dependent variable	Δ Log price of primary homes			Δ Log unemployment rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Observed treatment	-0.152***	-0.147***	-0.190***	0.121***	0.118***	0.111***
	(0.0549)	(0.0518)	(0.0633)	(0.0336)	(0.0334)	(0.0325)
Lagged difference of controls	No	Yes	Yes	No	Yes	Yes
Predetermined outcome level	No	No	Yes	No	No	Yes
Observations	1,406	1,406	1,406	1,406	1,406	1,406
Kleibergen-Paap F	870.2	981	755.7	870.2	981	897.9
	Panel (b): TSLS: *First stage*					
Dependent variable	Observed treatment					
Second home rates in 2000	2.066***	2.068***	2.043***	2.066***	2.068***	2.067***
	(0.0700)	(0.0660)	(0.0743)	(0.0700)	(0.0660)	(0.0690)
Lagged difference of controls	No	Yes	Yes	No	Yes	Yes
Predetermined outcome level	No	No	Yes	No	No	Yes

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

TABLE W-E2

DD estimates

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

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

TABLE W-E3

FD estimates

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

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

TABLE W-E4

FD covariates balance

	Control -	Treated -	Control CBD>10 km & Ski>0 km	Treated	Control CBD>10 km & 15%-30%	Treated	p-values		
	(1)	(2)	(3)	(4)	(5)	(6)	(1) vs. (2)	(3) vs. (4)	(5) vs. (6)
No. Observations	1,230	176	446	56	107	22	-	-	-
$\log(y_{10-11})$									
Price of primary homes	6.56	6.34	6.49	6.27	6.44	6.42	**0.00**	**0.00**	0.87
Unemployment rate	-4.36	-4.42	-4.40	-4.39	-4.31	-4.31	0.10	0.89	0.99
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) to (6) report the means of the outcome variables and controls used in Table 2 (Panel A) for the full sample of municipalities (columns 1-2), when municipalities within 10 km from major CBDs or adjacent to major ski resorts are dropped (columns 3-4), and when municipalities within 10 km from major CBDs and with a second home rate outside the [0.15, 0.3] interval are excluded. The last three columns report p-values for the test of difference in means between control and treated group according to the considered sample. The p-values lower than 0.1 are marked in bold.

TABLE W-E5

FD-IV estimates: Restricted Sample 1

(Excluding municipalities near major CBDs and ski resorts)

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

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

TABLE W-E6

FD-IV estimates: Restricted Sample 2

(Excluding municipalities near major CBDs and/or having a 2nd home rate below 15% or above 30%)

Panel (a): TSLS: *Second stage*						
Dependent variable	Δ Log price of primary homes			Δ Log unemployment rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Observed treatment	-0.561***	-0.370**	-0.353**	0.243*	0.291**	0.251**
	(0.169)	(0.149)	(0.149)	(0.125)	(0.116)	(0.105)
Lagged difference of controls	No	Yes	Yes	No	Yes	Yes
Predetermined outcome level	No	No	Yes	No	No	Yes
Observations	129	129	129	129	129	129
Kleibergen-Paap F	35.02	38.55	37.71	35.02	38.55	37.01

Panel (b): TSLS: *First stage*						
Dependent variable	Observed treatment					
Second home rates in 2000	2.689***	2.848***	2.868***	2.689***	2.848***	2.852***
	(0.454)	(0.459)	(0.467)	(0.454)	(0.459)	(0.469)
Lagged difference of controls	No	Yes	Yes	No	Yes	Yes
Predetermined outcome level	No	No	Yes	No	No	Yes

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

TABLE W-E7

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

Panel (a): TSLS: *Second stage*						
Dependent variable	Δ Log price of primary homes			Δ Log unemployment rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Observed treatment	-0.148***	-0.142***	-0.191***	0.113***	0.112***	0.105***
	(0.0459)	(0.0441)	(0.0441)	(0.0250)	(0.0251)	(0.0248)
Lagged difference of controls	No	Yes	Yes	No	Yes	Yes
Predetermined outcome level	No	No	Yes	No	No	Yes
Observations	1,027	1,027	1,027	1,027	1,027	1,027
Kleibergen-Paap F	1385	1375	1350	1385	1375	1374
Panel (b): TSLS: *First stage*						
Dependent variable	Observed treatment					
Second home rates in 2000	2.130***	2.128***	2.079***	2.130***	2.128***	2.126***
	(0.0572)	(0.0574)	(0.0566)	(0.0572)	(0.0574)	(0.0573)
Lagged difference of controls	No	Yes	Yes	No	Yes	Yes
Predetermined outcome level	No	No	Yes	No	No	Yes

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

TABLE W-E8

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

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

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

TABLE W-E9

FD-IV estimates: New constructions regressions

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

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

NEW TABLE W-E10

FD-IV estimates: Elderly regressions

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

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

TABLE W-E11

FD-IV estimates: Sorting of permanent residents

Panel (a): TSLS: *Second stage*									
Dependent variable	Δ Log population								
	Full sample			CBD >10 km & Ski>0 km			CBD >10 km & [0.15,0.3]		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Observed treatment	-0.00911	-0.00797	-0.00932	-0.00298	-0.000259	-0.00158	0.0182	0.0265	0.0261
	(0.00654)	(0.00650)	(0.00669)	(0.0150)	(0.0149)	(0.0153)	(0.0237)	(0.0206)	(0.0210)
Lagged difference of controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Predetermined outcome level	No	No	Yes	No	No	Yes	No	No	Yes
Observations	1,406	1,406	1,406	502	502	502	129	129	129
Kleibergen-Paap F	1623	1619	1626	536.8	524.9	523.8	35.02	38.55	37.68

Panel (b): TSLS: *First stage*									
Dependent variable	Observed treatment								
Second home rates in 2000	2.066***	2.068***	2.052***	2.150***	2.173***	2.160***	2.689***	2.848***	2.817***
	(0.0513)	(0.0514)	(0.0509)	(0.0928)	(0.0949)	(0.0944)	(0.454)	(0.459)	(0.459)
Lagged difference of controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Predetermined outcome level	No	No	Yes	No	No	Yes	No	No	Yes

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

TABLE W-E12

FD-IV estimates: Wage regressions

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

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

FOR ONLINE PUBLICATION:

Web-Appendix F: Additional analysis

F.1 Additional pre-trend analysis

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

The pre-trend analysis summarized in Tables W-F1 and W-F2, respectively, reveals that primary and second home prices do not display significantly different pre-trends.[2] For some periods, the price of *primary homes* displays some significant pre-trend differences in the full sample and in the Restricted Sample 1, but these differences disappear after the inclusion of controls, especially the lagged outcome level variables. Pre-trends of the price of *second homes* are never significant.

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

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

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

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

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

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

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

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

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

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

TABLE W-F1

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

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

TABLE W-F2

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

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

TABLE W-F3

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

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

F.2 Controlling for second home rate polynomial terms

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

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

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

TABLE W-F4

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

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

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

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

TABLE W-F5

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

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

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

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

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

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

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

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

F.3 Heterogeneous effects

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

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

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

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

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

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

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

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

TABLE W-F6

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

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

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