UNIVERSITÄT BERN

CRED
CENTER FOR REGIONAL ECONOMIC DEVELOPMENT

The Effect of a Second Home Construction Ban on Real Estate Prices*

Evidence from Switzerland using the Synthetic Control Method

CRED Research Paper No. 18

Daniel Steffen
University of Bern,
CRED

December 2017

Abstract

In 2012, a drastic regulation prohibiting the construction of holiday and investment homes in touristic municipalities in Switzerland was surprisingly introduced. I investigate the causal effect of such a construction limitation on real estate prices. The regulation does not affect all municipalities, which provides a unique possibility to separate the municipalities into treatment and control groups. I apply the synthetic control method to estimate the causal effect of the regulation. Unlike the classic synthetic control method literature, I deal with multiple heterogeneous treatment units. This allows me to compute statistical significance precisely and construct confidence intervals. I demonstrate a salient drop in real estate prices of between -10% and -18% three to five years after the intervention. These results are highly significant. However, no effect on prices in the first two years after the intervention is found. The decrease in prices is shown to be caused by indirect channels such as adverse effects on local economies and legal uncertainty.

CRED
Universität Bern
Schanzeneckstrasse 1
Postfach
CH-3001 Bern

Tel. +41 031 631 37 11
info@cred.unibe.ch
www.cred.unibe.ch

Keywords: Second homes, synthetic control, multiple treatment units, land use regulation, house prices.

JEL Codes: R21, R31, R52

*I am grateful to Blaise Melly for helpful suggestions and invaluable support. I thank all the Brown Bag seminar participants at the University of Bern as well as Thomas Rieder and Peter Ilg for their insightful comments. I also thank the Swiss Real Estate Datapool Association for allowing me to work with the housing data. I am especially grateful to Christian Studer for his invaluable help using the ArcGIS software.

1 Introduction

Housing markets are typically heavily regulated. However, the causal effects of these regulations are usually hard to estimate. Therefore, it often remains unknown whether the effect caused by a regulation is the effect regulators were seeking. On March 11, 2012, Swiss citizens accepted a very drastic regulation – the so-called Second Home Initiative (SHI) – in a popular vote. The SHI bans the construction of second homes[1] in all municipalities with a share of 20% of second homes or more. Accordingly, homes built after the vote in 2012 cannot be used as second homes at any point in the future (Federal Act on Second Homes of 2015).

Approximately one out of five municipalities in Switzerland has a second home share above the limit of 20%, and 17% of all homes in Switzerland are second homes. Thus, second homes are popular in Switzerland and play an important role in the real estate market. In some regions, second homes are even the main driver of the real estate market and have a notable impact on local economies in general. Particularly in the Alps, almost all municipalities are affected by the SHI (see figure 1) and second home shares of 50% and more are common in touristic municipalities. For instance, the SHI affects approximately 75% of all municipalities in the biggest mountain cantons, Graubünden and Valais. Therefore, the SHI was expected to cause major distortions in regional real estate markets and – as will be elaborated later – perhaps even to present some drawbacks for local economies. The goal of this paper is to find a way to isolate the causal effect of this drastic intervention on real estate prices.

As mentioned before, it is often very difficult to isolate the effect of such regulations. Since not all municipalities are affected by the SHI, the SHI offers a unique quasi-experimental research design to estimate the causal effect of the intervention. In 2012, 458 municipalities

[1]Second homes are broadly defined as homes not permanently used by persons who are either registered as permanent residents in this specific municipality or living in this municipality for work or educational reasons during the working week (Ordinance of Second Homes of 2012). Hence, second homes are mostly used as holiday or investment homes.

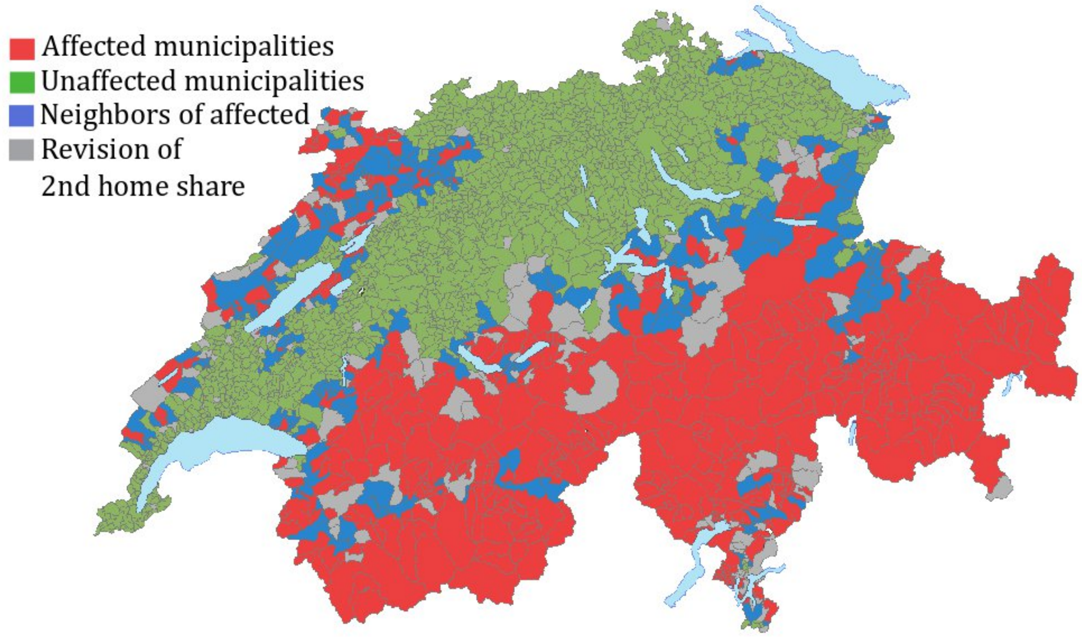


Figure 1: Municipalities of Switzerland marked as affected or unaffected municipalities, neighbors of affected municipalities and municipalities that asked for a revision of their official second home share (see section 5)

out of 2352 held a share of more than 20% of second homes. This enables a separation of municipalities into an unaffected control (less than 20% share of second homes) and an affected treatment group (all others). To make notation easier, I call municipalities with a share of at least 20% of second homes "affected" or "treated", whereas I call all others "unaffected" or "control municipalities". The possibility of separating municipalities into treatment and control groups might upon first consideration suggest applying a difference-in-difference estimation (DD). DD is becoming an increasingly popular approach to estimating the causal effect of such policy interventions or other events. However, DD is based on the restrictive assumption that the outcome variables of the control and treatment groups have parallel trends in the pre-intervention period (Angrist and Pischke, 2009), which is often not the situation.

In our case, municipalities affected by the SHI are very different from the unaffected municipalities, when it comes to location, economic structure and real estate markets. Almost all affected municipalities are rather small and remote towns located in the Alps, while unaffected municipalities are located in the densely populated Swiss Mittelland, which is dominated by major urban centers of the country. DD would roughly compare the average real estate market of the Alps with the average real estate market of the Swiss Mittelland. The average market structures of these two regions are not comparable. Therefore, the key assumption of parallel pre-intervention trends does not appear to be credible in the case of the SHI. Simple placebo DD estimations confirm the violation of the parallel trend assumptions (see table 3). The placebo DD estimations assume, for example, 2007 or any other pre-intervention year, to be the year of the SHI vote. Then, the DD method is used to test whether any significant effect of this placebo vote is found in the period 2007-2011. Because there was no vote in 2007, there should be no significant effect for the 2007-2011 period – unless the parallel trends assumption is violated. The placebo DD estimations show significant effects for placebo votes in 2007, 2008 and 2009. Consequently, simple DD regressions are not suitable to detect the effect of the SHI on housing prices. Hence, a first challenge of the paper is to find an identification strategy that handles this problem.

The synthetic control method (SCM), pioneered by Abadie and Gardeazabal (2003) and Abadie et al. (2010) relaxes the parallel trend assumption. Although treatment municipalities might be different on average, there exist control municipalities, which are similar to the treated municipalities in the Alps. The SCM assigns higher weights to control units that are similar and a weight of zero to control units that are very different from the treatment unit. Hence, this data-driven approach identifies the control municipalities that are similar to the treatment municipality instead of comparing plain averages. For that reason, the SCM is applied in this paper. While classic SCM literature usually deals with one or only a few treatment units, the SHI affects hundreds of local and heterogeneous housing markets (i.e., municipalities). A second challenge is, accordingly, to adapt the classic SCM to a setup with numerous treatment units.

I compute a synthetic control for each treated unit, re-weight the gaps between treatment units and synthetic controls and aggregate them to compute the overall effect of the SHI, comparable to Acemoglu et al. (2016) and Kreif et al. (2015). Above all, this extension allows rigorous inferences to be drawn by applying almost arbitrarily many placebo permutations, while the number of permutations and therefore, the power of statistical significance is limited in the classic synthetic control approach (e.g., Abadie et al., 2010). Insufficient power to detect statistical significance is commonly considered to be a weakness of classic SCM. The SCM with multiple treatments applied in this paper is able to overcome the issue of insufficient statistical power. I take this opportunity of multiple treatment units to introduce an innovative way to compute precise statistical significance.

Applying this SCM approach, I find no effect on prices in the first and second year after the intervention (2012-2013). However, I find a strong negative effect on prices of between -10% and -18% compared to the counterfactual in the third, fourth and fifth years after the vote (2014-2016). These negative effects are significant at a 99% level. The SHI might have caused this decline in prices via two channels: A loss of amenities in second home municipalities or legal uncertainty (see section 3). Data suggest that the SHI caused a change in the hedonic qualities of houses involved in post-treatment transactions: The quality and state of houses sold after the intervention clearly decreased in the treatment group. This drop in hedonic qualities indicates, as will be elaborated in section 3, that legal uncertainty might be the dominating channel. To investigate whether legal uncertainty is the dominating channel, I adjust for the varying hedonic qualities of houses sold by applying a novel three-staged estimation strategy, combining an approach to adjust for varying covariates pioneered by Athey and Imbens (2006) with the synthetic control method. This new estimation model demonstrates that legal uncertainty was the main driver for the decline in prices in the third and fourth year after the SHI vote (2014 and 2015), while the adverse effect on the amenity was the main driver for the negative effect on prices in the fifth year after the SHI vote (2016).

Hilber and Schöni (2016) estimated the effect of the SHI on real estate prices (and other variables) using the DD method and first differences. I take up that thread in this paper applying an alternative method and suggesting that the SCM might be a suitable approach in the situation of the SHI. Hilber and Schöni (2016) pool the years 2010 and 2011 to obtain a pre-intervention period and the years 2013 and 2014 to obtain one post-intervention period. They then estimate the average effect of the SHI on prices in this pooled period and find a strong negative and significant effect on primary housing prices of approximately -12%. As mentioned, I do not find any effect in the second year after the intervention, 2013. However, the effect found by Hilber and Schöni (2016) is comparable to the effect of -15% I found for the third year after the vote, 2014.

The rest of the paper is organized as follows: In section 2, the background of the SHI is discussed. Section 3 elaborates predicted effects of the SHI on real estate prices and transactions. Section 4 explains the SCM used in this paper and states why this method appears to be the right approach to estimate the effect of the second homes analysis. In section 5, the data and descriptive statistics are presented. Section 6 presents the results and finally, a conclusion is drawn in section 7.

2 Background of the SHI

In 2007, Helvetia Nostra handed in more than 100,000 signatures to the Federal Chancellery; in January 2008, Federal Chancellery validated those signatures and Federal Council authorized the initiative. In 2011, the parliament followed the Council's decision. Consequently, Swiss citizens voted on the SHI in March 2012[2]. The main goals of the initiators of the SHI are to protect the landscape, stop splinter development and keep housing affordable for locals.

Most major political parties, most known economic organizations and the Federal Council

[2]see Homepage of Swiss Federal Chancellery for more information

and parliament clearly recommended declining the SHI[3]. It is important to know that only a small minority of all popular initiatives held in Switzerland are accepted. Up to June 2016: only 22 of 206 initiatives have been accepted by popular vote[4]. Because of this broad resistance in politics and economics and the general tendency of initiatives to be turned down, most opponents of the initiative were quite confident that the SHI would be declined and, thus, did not start a vigorous campaign against the SHI (Dulio, 2012). In March 2012, a very narrow majority of 50.6% of all voters accepted the SHI. Although surveys predicted a tight race, the result was a surprise for most observers (as placebo studies confirm in figure 4a).

The SHI was applied immediately after the vote in March 2012. Hence, the Federal court declared all building permits for second homes in affected municipalities submitted after the vote on March 11, 2012, invalid in retrospect (Swiss Federal Court 2013). Although the Swiss government elaborated a provisional ordinance corresponding to the SHI in August 2012, it took almost three years for the parliament to work out the law. Parliament accepted the definitive law in March 2015 and began enforcing it on January 1st 2016. The ordinance of 2012 and the ultimate law of 2015 differ in some points, but they remained the same at their cores (see section 3). Nevertheless, the vote in favor of the SHI in 2012 meant an immediate building freeze for second homes in affected municipalities and, hence, a sharp cut in supply.

3 Predicted consequences and impact channels

As mentioned in the introduction, the SHI is a drastic intervention and affects particularly municipalities in the Alps regions (see figure 1). Predicted effects can be separated into *direct* and *indirect* effects. Direct effects suggest an increase in prices, while indirect effects propose a negative effect on prices. First, the mechanism of direct effects is discussed.

To understand the direct effects of the SHI, a closer look at the law is required (Federal

[3] see "Swiss Parliament" for more information (https://www.parlament.ch/de/services/volksabstimmungen/fruehere-volksabstimmungen/volksabstimmungen-2012/volksabstimmung-2012-03-11)

[4] see Federal Statistical Office (FSO): https://www.bfs.admin.ch/bfs/en/home/statistics/politics.html

Act on Second Homes of 2015, Ordinance on Second Homes of 2012). The second home law separates the real estate market into two different categories:

- *New first homes*: First homes, whose construction was still permitted after the vote in 2012. These homes can no longer be used as second homes. Their use is severely restricted.

- *Second homes* and *pre-law first homes*: Homes declared as second homes can arbitrarily be used as first or second homes. Their use is unrestricted in the future. Pre-law first homes are homes that were either built or whose construction was permitted before the vote in 2012. These homes can arbitrarily be used or sold as first or second homes according to the law of 2015. However, according to the ordinance of 2012, pre-law first homes can only be sold as second homes under the condition that the pre-law first home is not replaced by a new first home in the same municipality. This article in the ordinance of 2012 was very easy to avoid and was dropped in the final law. Hence, the use of pre-law first homes is virtually unrestricted by the SHI and legally equivalent to second homes.

This separation leads to different expected consequences for different groups of houses in affected municipalities. The price of new first homes should decrease because they can no longer be sold as second homes. Especially in tourist regions, real estate prices are driven by non-local buyers with a high willingness to pay (Kaufmann and Rieder, 2012). Since the option to sell new first homes as second homes to international buyers with a high willingness to pay is gone, a portion of the demand is gone. By contrast, there is no construction ban for new first homes and, therefore, supply can be extended if needed.

The use of second homes and pre-law first homes is unrestricted, but the building freeze has caused a cut in supply. Non-local potential buyers have a high willingness to pay for second homes, especially given the shortage of supply (Kaufmann and Rieder, 2012). The cut in supply by law should cause an increase in the prices of these homes given a stable non-local demand. Therefore, a separation of the market between second homes and pre-law first homes with increasing prices and new first homes with decreasing prices is expected. Since at least 92%[5] of all post-intervention transactions in treated municipalities involved pre-law first homes or second homes, the overall direct price effect of the SHI should increase housing prices in affected municipalities.

So far, the direct effects of the SHI on demand and supply have been considered. In a second step, the *indirect effects* of the SHI on prices will be discussed. Hilber and Schöni (2016) illustrated in detail how the SHI might have an adverse effect on local economies (e.g. unemployment and population movement). As mentioned in the introduction, second homes are of great economic importance in tourist regions. Not only do they guarantee many jobs in the locally very vital construction sector[6], but they are also an important source of income for hotels and mountain railway companies. Due to the building freeze, the number of construction contracts decreases, which leads to higher unemployment in this sector. Higher unemployment corresponds with a lower demand for housing and lower tax income for municipalities. There is some evidence that this indirect effect argumentation is not too far-fetched. The construction sector was indeed suffering because of the SHI. Fewer work contracts, the lowest ratio in twelve years of planned construction compared to building permission, redundancies and a salient rise in the bankruptcy petitions of construction firms in affected regions compared to the rest of Switzerland were observed after the vote (Flütsch, 2015; Credit Suisse, 2016b; Städler, 2016).

Moreover, an important source of income for investment in tourism infrastructure is gone because of the SHI. Hotels, mountain railways and other companies involved in the

[5]All first homes with construction finalized in 2013 or later are here considered new first homes. This is a very conservative estimate because only homes that received a construction permit after March 2012 are actually new first homes. A considerable number of homes finalized in 2013 or later may have received their building permit before the vote in March 2012.

[6]In 2015, 8.1% of the labor force in the mountain states (Uri, Obwalden, Nidwalden, Glarus, Graubünden, Ticino and Valais) was employed in the construction sector. The Swiss average share of the labor force employed in the construction sector is 6.5%. In the two states with the highest share of affected municipalities, 8.8% (Graubünden) and 8.7% (Valais) of the labor force is employed in the construction sector (see FSO Structural Survey 2015).

tourism industry sold second homes to cross-subsidize investments in infrastructure. Since this ability to cross-subsidize is gone, tourism resorts might no longer able to maintain their costly touristic infrastructure (Codoni and Grob, 2013; Kaufmann and Rieder, 2012). If tourism infrastructure is worsening, tourism demand and thereby demand for second homes will decrease. Because of the decrease in tourism demand and lower economic activity, the tax income of affected municipalities will drop further, and municipalities might face additional difficulties maintaining their infrastructure. Since taxes are paid on the primary residence, second home municipalities have by definition a small tax base compared to the number of houses and face high infrastructure costs (e.g., ski lifts). Therefore, municipalities affected by the SHI are especially vulnerable to such tax income reductions. In consequence, some municipalities were forced to introduce a second home tax in order to be able to maintain their ski lifts and other infrastructure projects (see, e.g., Knopf, 2013). The introduction of second home taxes increases the implicit price of a second home. Therefore, the SHI might have reduced the amenities of affected municipalities, which leads to a decline in prices. This indirect effect of the SHI on housing demand opposes the direct effect described.

Further, the SHI created insecurity in the local real estate markets. As stated in section 2, after the vote, it took parliament three years to reach agreement on the final second home law in 2015. Because it was not clear what the final law would look like, many market players might have been more conservative when selling or buying real estate in affected municipalities. This causes a decrease in transactions. Hence, the legal uncertainty might cause a "lock-in" effect, where second homeowners do not sell their homes if there is no immediate need to do so. Furthermore, homeowners know that it will be difficult to buy a second home in an affected municipality in the future due to the building freeze. With this in mind, homeowners might delay transactions in the hope that in the longer run, prices will increase due to the building freeze. This lock-in effect might lead to a situation in which only those in need of liquidity sell their homes. In general, only less prosperous people need to sell their homes immediately and, therefore, houses sold after the vote in 2012 might be of lower quality on average. Hence, average house prices decrease because the hedonic characteristics of houses changed.

In summary, it remains unclear whether the SHI is supposed to increase or decrease real estate prices, since there is a price increasing direct effect and opposing indirect and insecurity effects. Which of these effects dominates needs to be clarified empirically. However, insecurity should dampen market activity. Therefore, a decrease in transactions is expected.

4 Empirical strategy

Figure 1 demonstrates where the affected municipalities are located. Almost all treated municipalities are found in the Alps and barely any are in the Swiss Mittelland. Hence, as argued above and substantiated in the placebo DD estimation in table 3, a simple DD approach does not appear to be suitable. Therefore, the SCM developed by Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2015) is chosen. This method attempts to construct an optimal synthetic control by assigning different weights to different control units. I.e., the SCM assigns high weights to municipalities in the control unit pool that have real estate characteristics similar to those of the treatment municipality and a weight of zero to municipalities that are not at all similar to the primarily Alpine treatment municipalities. The method is based on the idea of finding a combination of untreated units by a data-driven procedure that provides the best possible match to the treatment group's price trend in the pre-intervention period. In the next subsections, this data-driven procedure is presented.

4.1 Classic synthetic control approach

The starting point is the classic SCM following Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2015). Suppose that our dataset contains J+1 different units (in our case, units are municipalities). One of these units (j=1) is the treatment unit, all other J units (j=2,..., J+1) are control units. Furthermore, the dataset contains T periods. T_0 of these periods are pre-treatment periods (t=1, 2,..., T_0), and T_1 periods are post-treatment periods (T_0+1,..., T). $\mathbf{W}$ is a (Jx1) vector $\mathbf{W}$=(w_2,..., w_{J+1}) of non-negative weights, such that $w_j \geq 0$ for all j and w_2 + w_3 + ... + w_{J+1} = 1. Each scalar w_j of this vector represents the weight of one control unit. The weights $\mathbf{W}$ shall be chosen to ensure that the synthetic control closely resembles the treatment unit before the intervention. $\mathbf{X}_1$ is a (Kx1) vector of K pre-treatment characteristics of treatment unit j=1 including the pre-treatment outcome variable. $\mathbf{X}_0$ is a (KxJ) matrix containing K pre-treatment characteristics of J control units. Further, $\mathbf{V}$ is a diagonal matrix containing the relative importance of each of these pre-treatment predictors. The goal is to find the $\mathbf{W}^*$ that assigns the optimal weight to every control municipality in order to minimize the pre-intervention distance metric of real estate characteristics, including the pre-intervention outcomes of the treatment unit and the synthetic control. To find $\mathbf{W}^*$, the problem

$$\underset{W \in \omega}{min} (\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})'\mathbf{V}(\mathbf{X}_1 - \mathbf{X}_0\mathbf{W}) \tag{1}$$

must be solved, where $\omega = \{(w_2, w_3, ..., w_{J+1})\}$ is subject to $w_j \geq 0$, and w_2 + w_3 + ... + w_{J+1} = 1. The $\mathbf{W}^*$ that minimizes formula (1) is the vector of weights, which gives each control unit a weight such that the synthetic control best resembles the treated unit in the pre-intervention period. In this paper, a data-driven approach is applied to select an optimal $\mathbf{V}^*$ that minimizes the root mean squared error (RMSE) of the outcome variable in the pre-intervention period, as done in Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2015). As soon as we obtain $\mathbf{W}^*$, the synthetic control can be computed:

$$\hat{Y}_{1t}^{SC} = \sum_{j=2}^{J+1} w_j^* * Y_{jt} \tag{2}$$

Y_{jt} is the outcome variable (i.e., the real estate price or number of transactions) in municipality j and time period t. $\widehat{Y}_1^{SC}t$ is the synthetic control and is supposed to be the counterfactual of the treatment unit. The gap between the treatment unit and the synthetic control is

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* * Y_{jt} = Y_{1t} - \hat{Y}_{1t}^{SC}. \tag{3}$$

Since $\mathbf{W}^*$ is minimizing the pre-intervention distance metric of real estate characteristics between the treatment unit and the synthetic control, pre-intervention gaps should be close to zero, i.e., $\widehat{\alpha}_{1t} \approx 0, \forall t \leq T_0$. Because only the treatment unit receives the intervention, the post-intervention gaps are supposed to be the causal effect of the intervention and significantly different from zero, i.e., $|\widehat{\alpha}_{1t}| \gg 0, \forall t > T_0$.

4.2 Multiple treatment units and exact inference with permutation tests

As mentioned in the introduction, I need to extend the classic SCM. The classic SCM only deals with one treatment unit. In this paper, approximately 100 treatment units have to be considered. There is not much literature dealing with that many treatment units. Abadie et al. (2010) suggest to simply aggregate the treated units into a single treated unit. Billmeier and Nannicini (2013) deal with several treatment units but look at effects for each treatment unit separately. Cavallo et al. (2013) also deal with several treated units, when estimating the causal effect of different natural disasters in different countries on GDP development. However, Cavallo et al. (2013) do not attempt to estimate one single intervention's overall effect on different units but many different treatments on many different units. Nevertheless, Cavallo et al. (2013)'s approach is comparable to that applied in this paper. Meanwhile, Kreif et al. (2015) and Acemoglu et al. (2016) came up with an approach closely related to the approach I am applying in this paper. They compute a synthetic control for each treatment unit and then calculate an aggregate effect.

The following approach is applied in this paper. Instead of having one treatment unit j=1, I have J_0 treatment units and J_1 control units (where $J_1 \gg J_0$). First, I compute a synthetic control and the corresponding gaps, $\hat{\alpha}_{jt}$, for all J_0 treatment units separately, as in the classic SCM. Hence, I obtain J_0 different gaps per year. To obtain an average effect per year, I compute the average of all J_0 gaps re-weighted by the number of transactions per year and municipality, L_{jt}:

$$\overline{\alpha}_t = \frac{\sum_{j=1}^{J_0} \hat{\alpha}_{jt} * L_{jt}}{\sum_{j=1}^{J_0} L_{jt}} \tag{4}$$

I weight the gaps to give municipalities with numerous transactions per year a higher weight than small municipalities with only a few transactions per year. Based on these weighted gaps $\overline{\alpha}_t$, I can reconstruct an aggregate weighted synthetic control $\hat{Y}_{1t}^{SC} = \overline{Y}_t - \overline{\alpha}_t$, where $\overline{Y}_t$ is the average price of the outcome variable of treatment units weighted by the number of transactions. Hence, by re-weighting the single gaps, I am able to construct aggregate gaps and aggregate synthetic control.

I further compute the ratio of the post- and the pre-intervention RMSE of the treatment units and the synthetic control:

$$RMSE\ ratio = \sqrt{\frac{(\sum_{t=T_0+1}^{T} \overline{\alpha}_t)^2}{T_1}} / \sqrt{\frac{(\sum_{t=0}^{T_0} \overline{\alpha}_t)^2}{T_0}}, \tag{5}$$

where the time periods (t=0,1,..., T_0) are pre-intervention and all post-intervention years. The ratio of the post- and pre-intervention RMSE should be clearly bigger than 1, since the gaps $\overline{\alpha}_t$ are supposed to be clearly bigger in the post-intervention period than in the pre-intervention period. The ratio of post- and pre-intervention RMSE is an important indicator, since it reflects the magnitude of the causal effect relative to pre-intervention fit. The worse the pre-intervention fit (the bigger the gaps) is, the higher are the expected gaps in the post-intervention period. The RMSE ratio takes the pre-intervention fit into account when assessing the magnitude of the intervention effect (see Abadie et al. (2010)). To evaluate the statistical significance of the results in the next step, the RMSE ratio is required.

Following Abadie et al. (2010), I assess the significance of the estimates by conducting a series of placebo studies. The difference from Abadie et al. (2010) is that my treatment group consists of J_0 treatment units instead of one treatment unit. This renders an opportunity to extend the approach of Abadie et al. (2010) slightly. I construct a placebo group consisting of J_0 randomly chosen control units and apply the SCM used to estimate the actual treatment effect of the SHI to this placebo group. Because none of the placebo units received the treatment, the pre- and post-intervention gaps should be similar. Therefore, the placebo RMSE ratio should be close to 1 or at least smaller than the treatment RMSE ratio. I can iterate this placebo study almost an arbitrary number of times (say N times) with different randomly chosen groups of J_0 control units, because $J_1 \gg J_0$, which renders it possible to obtain far more statistical power than in the classic SCM. This iterative placebo procedure provides me with a distribution of RMSE ratios for municipalities that never received treatment. This distribution can be used to compute the statistical significance of our treatment effect estimation by computing the corresponding p-value. The p-value reflects the probability of obtaining a placebo RMSE ratio larger or equal than the treatment RMSE ratio:

$$p - value = Pr(ratio_n^{plac} > ratio^{treat} | H_0) = \frac{\sum_{n=1}^{N} I(ratio_n^{plac} > ratio^{treat})}{N}, \tag{6}$$

where N is the number of iterations, H_0 is the null hypothesis that the SHI has no effect on prices or transactions, $ratio_n^{plac}$ is the RMSE ratio of placebo iteration n, and $ratio^{treat}$ is the original treatment RMSE ratio.

Multiple treatment units additionally allow confidence intervals (CI) to be computed as in Acemoglu et al. (2016). When conducting N placebo studies as described above, I obtain N weighted placebo gaps, $\overline{\alpha}_{nt}$, per year. I then compute the standard deviation, σ_t, of these N gaps:

$$\sigma_t = \sqrt{\frac{\sum_{n=1}^{N} \overline{\alpha}_{nt}^2}{N}} \tag{7}$$

Using σ_t, CI can easily be computed using average treatment prices as the basis.

4.3 Adjusting SCM for varying covariates

As will be shown later, the quality and state of houses sold decreased clearly in post-intervention period and treatment units. I adjust for these varying covariates to analyze whether the effect was caused by a decrease in the hedonic qualities of houses sold. I introduce an innovative three-stage estimation approach: In the first two stages, I adjust prices for covariates applying the approach of Athey and Imbens (2006). In a third stage, I combine this approach with the synthetic control method. The starting point is the following estimation equation:

$$Y_{ijt} = \alpha_{jt} + \mathbf{X}'_{ijt}\beta + \epsilon_{ijt} \tag{8}$$

where Y_{ijt} is the price of transaction i in municipality j and year t, α_{jt} is the municipality-year fixed effect, $\mathbf{X}_{ijt}$ is a vector of covariates, and ϵ_{ijt} is the error term. First, β can be estimated consistently using estimation equation (8). In the second stage, residuals with municipality-year effects can be constructed:

$$\widehat{Y}_{jt} = Y_{ijt} - \mathbf{X}'_{ijt}\beta = \alpha_{jt} + \widehat{\epsilon}_{ijt} \tag{9}$$

where $E(\widehat{\epsilon}_{ijt}) = 0$. Hence, $\widehat{Y}_{jt}$ is the real estate price per municipality and year adjusted for varying covariates. I use this $\widehat{Y}_{jt}$ instead of the average price per municipality in a third stage to conduct the SCM, adjusting for varying covariates as described in section 4.2. Notice that I first normalize all covariates to zero (i.e., $x_{ijt}^{normalized} = x_{ijt} - \overline{x}_t$) to obtain positive adjusted prices $\widehat{Y}_{jt}$.

5 Data and descriptive statistics

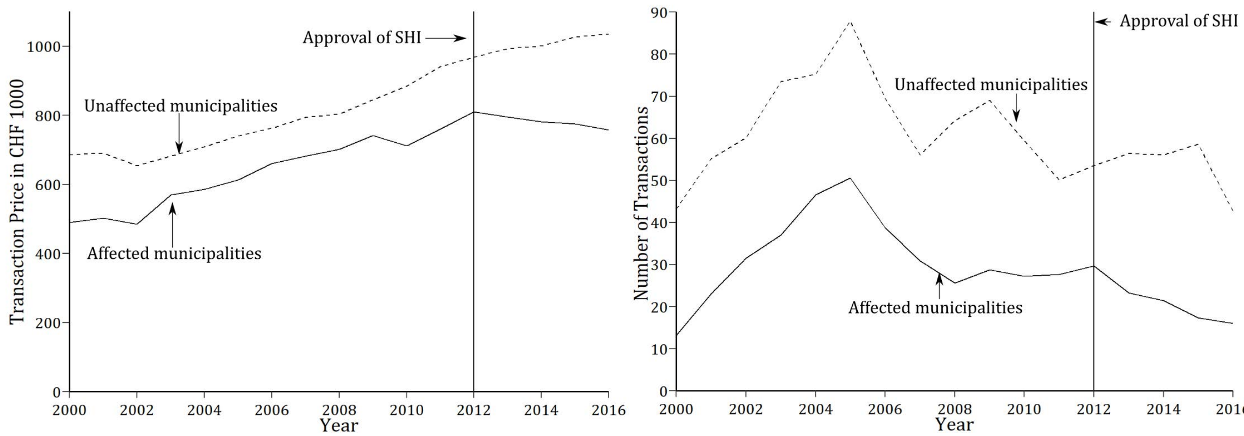
Swiss Real Estate Datapool (SRED)[7] collects the data used in this paper. SRED is an association founded by the three Swiss banks UBS, Credit Suisse and Zürcher Kantonal-

[7] see https://www.sred.ch/

bank. The SRED dataset contains information on real estate transactions executed by these three banks between 2000 and 2016. It contains information for more than 240,000 transactions completed during this period and includes transaction prices as well as other relevant attributes (listed in table 2 in the appendix A).

To define whether a transaction takes place in a treatment or a control unit, we need to know the second home share of each municipality. The Swiss Federal Spatial Development Office (ARE) provides the official second home share per municipality. However, the second home share of municipalities was unknown before the vote in 2012. Therefore, the ARE had to estimate these second home shares. Municipalities had the opportunity to ask for a revision of ARE's second home share estimation. When these municipalities were able to prove that their second home share was lower than that estimated by ARE, the ARE adjusted its original estimation. I dropped all municipalities that asked for such a revision of their estimated second home share because market players did not know whether municipalities that asked for revision end up as treated or untreated municipalities, until ARE accepts or rejects the objection (see figure 1 for municipalities that revised the original second home estimation).

Further, I only keep municipalities with at least one transaction in every year because SCM is not applicable otherwise. Finally, I dropped all municipalities with a second home share between 18% and 20%. This is done because market players might foresee that these municipalities are going to cross the threshold of 20% of second homes in the near future. Therefore, the SHI might affect these municipalities to a certain degree. Sensitivity calculations considering second homes build since 2012 show that only a few of the municipalities with a second home share between 18% and 20% are at risk of belonging to the treated group in the near future. Therefore, it is rather conservative to drop all municipalities in this bandwidth. The original dataset in total contains transactions in 2209 municipalities. After the data preparations mentioned above, 656 or approximately 30% of these 2209 municipalities remain in the final dataset. It is important to know that almost all municipalities



(a) Average transaction price in CHF 1000 (b) Average number of transactions per year

Figure 2: Trends in prices and number of transactions: Transactions in affected vs. transactions in unaffected municipalities

removed were dropped because no transaction took place in one or more years. This means that mainly very small municipalities were dropped. Therefore, the final dataset of 656 municipalities contains 186,508 or approximately 77% of all transactions in the original dataset. Because the vote on the SHI took place in mid-March 2012, I prepared the data so that every year starts in the second quarter and ends after the first quarter of the following year. For instance, the year 2000 starts in April 2000 and ends in end-March 2001. Hence, the post-intervention period (2012-2016) starts in April 2012, right after the vote took place.

Figure 2 and table 1 present descriptive statistics. Figure 2a clearly shows that prices in treated municipalities are decreasing right after the vote in 2012, while prices in unaffected municipalities continue to increase. Furthermore, the figure demonstrates that pre-intervention price trends in affected and unaffected municipalities are not permanently parallel. This suggests that a simple DD approach is probably not suitable to detect the causal effect of the SHI. Figure 2b shows the averages of the number of transactions in the treatment and control groups. The number of transactions decreases dramatically in the treatment group after the approval of the SHI. This might be caused by the legal uncertainty, as explained in section 3.

A summary of the most important housing characteristics is presented in table 1. A typi-

Table 1: Summary statistics, averages of transactions by treatment and control group

Period	Control group Pre-intervention	Control group Post-intervention	Treatment group Pre-intervention	Treatment group Post-intervention
Transaction price (CHF)	761'246	1'002'434	628'214	785'683
Transactions [a]	66.2	53.7	34.6	22.3
Number of rooms	4.78	4.35	3.79	3.60
Plumbing units	2.05	2.01	1.82	1.76
Number of garages	1.1	0.83	0.95	0.71
Micro-location[b,c]	2.87	2.71	3.08	2.93
Quality[c]	2.87	2.87	2.84	2.59
State[c]	3.17	2.99	2.88	2.56
Year of construction	1983	1988	1984	1984

[a] *Per year and municipality*

[b] *Micro-location is the quality of the location of a property within the municipality.*

[c] *Values between 1 (=poor) and 4 (=very good). See table 2 for more details.*

cal dwelling in an unaffected municipality is more expensive and bigger when it comes to the number of rooms, plumbing units or garages than a typical dwelling in affected municipalities. Furthermore, housing markets in affected municipalities, with 35 yearly transactions in the pre-treatment period, are clearly smaller than those in the unaffected municipalities, with 66.2 transactions in the same period. This underlines that the housing markets of the two groups on average differ clearly in size and characteristics. In both types of municipalities, the number of transactions and most other indicators such as the number of rooms decrease over time. The decrease in quality and state of the houses in affected municipalities is striking and will be discussed later.

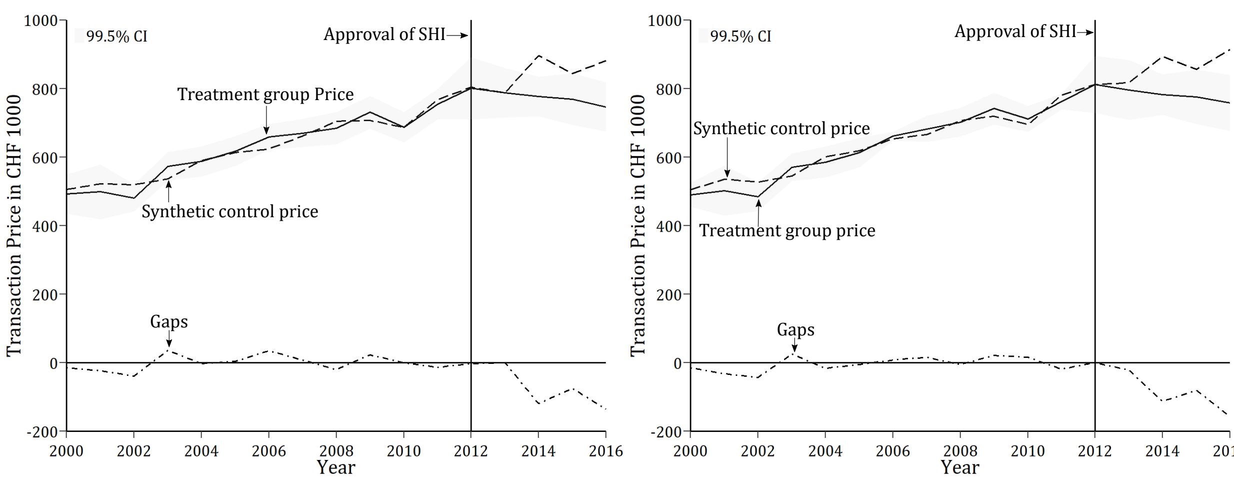
6 Results

6.1 Main results using extended SCM

First, I present the results of the SCM as illustrated in section 4.2, i.e., the results are not adjusted for varying covariates. As mentioned in section 4.1, I minimize the distance metric of real estate characteristics between the treatment unit and control municipalities.

Real estate characteristics include the number of transactions per year, number of rooms, plumbing units and garages, micro-location, quality and state of the property, municipality type, canton and finally the pre-intervention outcome variable, transaction price. Kaul et al. (2017) point out that using all pre-intervention outcomes could cause a bias. Therefore, I run the SCM two times: First, I include all pre-intervention outcome variables, and second, I include only pre-intervention outcome variables for the years 2000, 2006 and 2011. Figure 3 shows the effect of the SHI on the prices taking both approaches. Both approaches return almost identical results. Hence, including all prices does not cause a bias. Therefore, I focus on the approach including all pre-intervention outcome variables.

Figure 3a indicates that the SHI has a strong negative effect on housing prices, as the treatment and synthetic control prices diverge clearly in 2014 and later. The pre-intervention RMSE is very small, with 22,000 CHF or 3.6% of the average pre-intervention treatment price, which indicates that the synthetic control is an accurate counterfactual. Housing prices are in 2014 (-15%), 2015 (-10%) and 2016 (-18%) clearly lower than the corresponding synthetic control prices. For instance, in 2016, the average treatment housing price was CHF 135,000 lower than the synthetic control price. However, no effect in the post-intervention

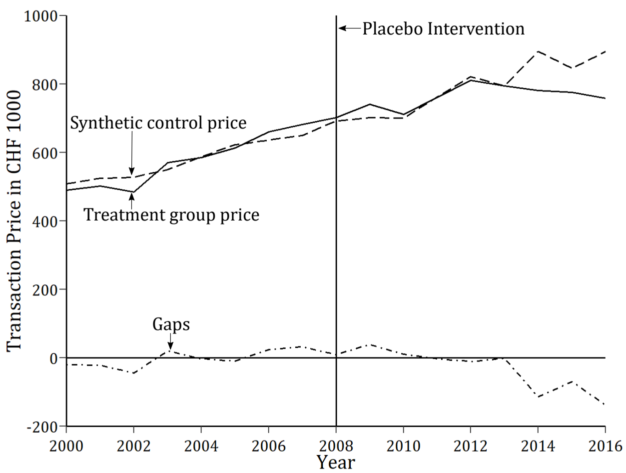


(a) All outcome variables included (b) Three outcome variables included

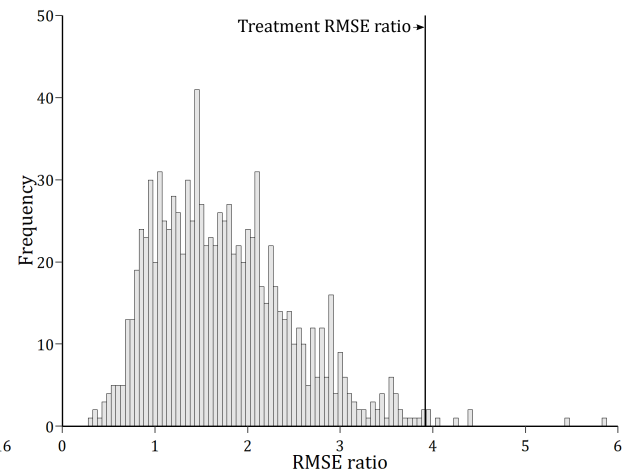
Figure 3: Price development of treatment group and synthetic control and corresponding gaps for both approaches including all and only three pre-intervention outcome variables to compute synthetic control; in gray, 99.5% confidence interval.

years 2012 and 2013 is visible. The RMSE of the post-intervention period is with 93,000 CHF or 11.9% of the average post-intervention price—clearly higher than in the pre-intervention period. This results in a RMSE-ratio of 3.9. In 2002, the synthetic is not able to reproduce the treatment price as closely as in other pre-treatment years. However, the gap in 2015 (the smallest gap of the 2014-2016 period) is still almost twice as that in 2002.

What is the probability of obtaining results of this magnitude by chance? To evaluate the significance of the results obtained above, two types of placebo tests are conducted. First, I run a placebo in-time test. In the placebo in-time test, I set the intervention year in 2008 instead of 2012 and run the exact same SCM approach used before. Because there was no intervention in 2008, the gaps in the period 2008-2011 should not be clearly larger than those in the pre-2008 period.



(a) Placebo in-time



(b) Distribution of placebo RMSE ratios

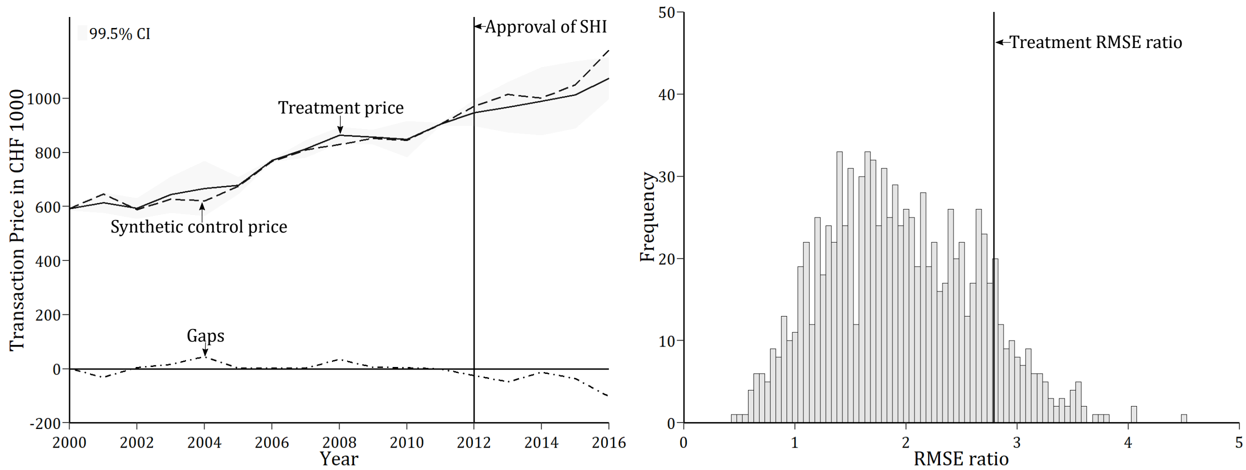
Figure 4: Placebo in-time robustness check with 2008 as placebo intervention year and distribution of placebo RMSE ratios with treatment RMSE ratio at 3.9.

Figure 4a shows that the pre-2008 RMSE (25,000) is larger than the RMSE of the period between 2008 and 2011 (21,000). This supports the claim that the results obtained are not driven by chance. Furthermore, it confirms that agents did not foresee the outcome of the vote in 2012 and did not adapt their behavior accordingly. However, to derive the precise statistical significance of the results, I need to run a second type of placebo test, i.e., a permutation test as described in section 4.2. I run 1000 permutation tests and correspondingly obtain 1000 placebo RMSE-ratios. The distribution of these placebo RMSE-ratios is presented in figure 4b. Only 8 placebo RMSE-ratios are greater than my treatment RMSE-ratio of 3.9, which corresponds to a p-value of 0.008 (see formula (6)). Hence, the results presented in figure 3 are highly significant, with a 99% significance level. Additionally, confidence intervals are constructed. The gaps between the treatment group and the synthetic control are outside of the 99.5% confidence interval in the years 2014-2016 (see figure 3).

6.2 Results using adjusted prices

Table 1 in section 5 indicates that especially quality, but also micro-location and the state of the houses, has decreased clearly in the treatment municipalities during the post-intervention period. DD regressions support this finding (see table 4). Also, the synthetic control approach using the quality of houses as an outcome variable instead of transaction prices while taking the same weights for control units as in figure 3a, indicates that the SHI might have caused a decrease in the quality of houses (see figure 6 in the appendix). This suggests that the decrease in housing prices found in figure 3 might occur mainly because houses with inferior hedonic attributes were sold in treatment municipalities after the vote. As argued in section 3, this drop in hedonic attributes might be driven by legal insecurity and a lock-in effect caused by the SHI. I adjust for these varying covariates in order to investigate whether prices decreased only due to a change in the quality of houses sold or also because of a reduced demand due to adverse effects on the amenity of tourism destinations. To adjust for the varying covariates, I apply the three-stage model introduced in section 4.3. Figure 7 in the appendix D shows the average prices of the treatment and control municipalities adjusted for time-variant covariates. In contrast to figure 2a, prices do not appear to diverge clearly in the post-intervention period.

I evaluate the effect of the SHI on these adjusted prices, applying the extended SCM as described in section 4.2. The only covariates considered to minimize the difference metric



(a) Results of SCM with adjusted prices (b) Placebo RMSE ratios with adjusted prices

Figure 5: Development of adjusted prices of the treatment group and the synthetic control and gaps; RMSE ratio with adjusted prices.

between the treatment and the synthetic controls are the adjusted prices of the years 2000, 2006 and 2011[8]. Figure 5a shows that there is no clear effect when using adjusted prices. Even though the synthetic control prices are still consistently higher than the treatment prices in the post-intervention period, the gaps of the post-intervention period are of similar magnitude as the pre-intervention gaps. Only the gap in 2016 with CHF 102,000 is clearly larger than all pre-intervention gaps. The gap in 2016 is the reason why the post-intervention RMSE (CHF 55,000) is much higher than the pre-intervention RMSE (CHF 20,000). Nevertheless, the corresponding RMSE-ratio of 2.8 is not significantly different from the placebo RMSE-ratios (see figure 5b). H_0, that the SHI did not have an effect on the corrected prices, cannot be rejected at a 90% significance level for the post-intervention period as a whole. CI show, however, that the prices in 2016 are significantly different from each other (see figure 5a). Hence, considering adjusted prices, the synthetic control post-intervention price still has the tendency to be higher than the treatment price, but only the price difference in 2016 is significantly larger than the placebo gaps. This indicates that legal uncertainty was the main driver of the price decrease in 2014 and 2015, while the adverse effect on the amenities of second home municipalities was the main driver of the negative effect in 2016. This makes

[8]Therefore, the CI in 5a are very narrow in 2000, 2006 and 2011.

sense, since the legal uncertainty disappeared in 2015 when the parliament agreed on a final second home law. The adverse effect on local economies is an indirect effect, and it takes time until the effect appears via unemployment and the loss of amenities. It remains unclear, however, why the legal uncertainty effect did not influence prices in 2013.

6.3 Robustness check using neighboring municipalities

In general, municipalities tend to be more similar to their neighboring municipalities than to municipalities located in different regions. Therefore, control municipalities next to treatment municipalities are likely to receive high weights when constructing the synthetic control. Since no more second homes can be built in treatment municipalities, it is possible that potential buyers are interested in purchasing a second home in the closest municipality not affected by SHI. Thus, the SHI could also affect the neighbors of treatment municipalities (see figure 1 for the location of neighbors of treated municipalities). This would bias the former results.

Only 10 neighboring municipalities (of 67 in the final dataset) receive a weight of more than 1% in the SCM estimation in the main analysis. Nevertheless, in total, almost 64% of the synthetic control in the main analysis consists of neighbors of the treatment units. Hence, if the SHI affects these neighboring municipalities, the results obtained earlier could be downwardly biased. Therefore, I estimate the effect of the SHI on neighboring municipalities using the SCM in almost the same way as before. The only difference is that I do not weight the gaps by number of transactions but by the weights assigned to the neighboring municipalities in the synthetic control of the main analysis in section 6.1. So, neighboring municipalities that are important for the synthetic control of the main analysis receive a higher weight than those that are of no importance for the construction of the synthetic control in the main analysis. The result of this robustness check can be found in figure 8 in appendix E. The pre-intervention fit of the neighboring municipalities and their synthetic control with an RMSE of 35,900 is greater than the RMSE of the main analysis. The post-intervention RMSE with 35,500 is slightly smaller than the pre-intervention RMSE. Hence, a RMSE-ratio of below 1 indicates that the SHI had no effect on the neighboring municipalities weighted by their contribution to the synthetic control of the main analysis. Accordingly, the main results obtained before should be unbiased.

7 Conclusion

Real estate prices in treated municipalities decrease noticeably compared to the synthetic control prices. I found that the SHI caused a sharp decline in prices of 15% in the third year, 10% in the fourth year and 18% in the fifth year after the vote. Hence, the predicted effects via direct channels described in section 3 cannot be the dominant driver of the intervention effect because they predict a positive effect. There are two possible indirect channels discussed in section 3 that could explain this drop in prices. Because indirect channels need some time until their effects come up, they would additionally explain why no effect was visible in the first two years after the intervention.

The first channel that could explain the drop in prices is the drawbacks presented by the SHI on local economies and the tourism industry. These drawbacks harmed the amenities of treated municipalities as second home residences and caused a decrease in demand. However, the results of the approach with prices adjusted for varying covariates shows that another channel might also be very important for the decline in prices: A drop in the hedonic qualities of properties sold might have influenced the decline of prices. Legal uncertainty caused by the SHI, the second possible channel, can explain the change in hedonic characteristics of affected real estate markets. Hence, both indirect channels are likely to be important for the salient price reduction. Legal uncertainty might have been the dominant reason why prices dropped in 2014 and 2015, while the drawbacks for the tourism industry were the main driver in 2016.

This paper examines the SCM in a context of multiple treatment units and covariates that vary in the post-intervention period between the treatment group and a synthetic control. The results show that the adapted SCM could be a very promising approach to estimate policy interventions or trends in a context of various and very heterogeneous real estate markets.

References

Abadie, A., Diamond, A., and Hainmueller, J. (2010). Synthetic Control Methods for Comparative Case studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association*, 105(490).

Abadie, A., Diamond, A., and Hainmueller, J. (2015). Comparative Politics and the Synthetic Control Method. *American Journal of Political Science*, 59.

Abadie, A. and Gardeazabal, J. (2003). The Economic Costs of Conflict: A Case Study of the Basque Country. *The American Economic Review*, 93(1):113–132.

Acemoglu, D., Johnson, S., Kwak, J., and Mitton, T. (2016). The Value of Connections in Turbulent Times: Evidence from the United States. *Journal of Financial Economics*.

Angrist, J. D. and Pischke, J.-S. (2009). *Mostly Harmless Econometrics*. Princeton University Press, 1st edition.

Athey, S. and Imbens, G. W. (2006). Idnetification and Inference in Nonlinear Difference-in-Difference Models. *Econometrica*, 74(2).

Billmeier, A. and Nannicini, T. (2013). Assesing Economic Liberalization Episodes: A Synthetic Control Approach. *The Review of Economics and Statistics*, 95(3).

Cavallo, E., Galiani, S., Noy, I., and Pantano, J. (2013). Catastrophic Natural Disasters and Economic Growth. *The Review of Economics and Statistics*, 95(5).

Codoni, D. and Grob, U. (2013). Auswirkungen der Zweitwohnungsinitiative auf den Tourismus im Schweizer Alpenraum. *Die Volkswirtschaft*, 4.

Credit Suisse (2016a). Was bringt das Jahr 2017? Published online: http://publications.credit-suisse.com/tasks/render/file/index.cfm? fileid=4E404B1B-0390-E621-588C5D276B340BAD [21.12.2017].

Credit Suisse (2016b). Weitere Senkung des Referenzzinssatzes 2017 wahrscheinlich. Published online: http://publications.credit-suisse.com/tasks/render/file/index.cfm? fileid=9F7B7892-0E67-C1D3-347D67A201B295A6 [21.12.2017].

Dulio, C. (2012). Die Zweitwohnungsinitiative unterschätzt. *Neue Zürcher Zeitung*.

Flütsch, A. (2015). Einbruch der Baubranche. *Sonntags Zeitung*.

Hilber, C. and Schöni, O. (2016). The Housing Market Impacts of Constraining Second Home Investments. CRED Working Paper No. 11.

Kaufmann, P. and Rieder, T. (2012). Zweitwohnungsstopp: Mögliche Auswirkungen auf die Immobilienpreise in den Tourismusregionen. *Die Volkswirtschaft*, 6.

Kaul, A., Klössner, S., Pfeifer, G., and Schieler, M. (2017). Synthetic Control Methods: Never Use All Pre-Intervention Outcomes as Economic Predictors. *mimeo*.

Knopf, S. (2013). Walliser Gemeinden bitten Besitzer von Zweitwohnungen zur Kasse. *Tages Anzeiger*.

Kreif, N., Grieve, R., Hangartner, D., Turner, A. J., Nikolova, S., and Sutton, M. (2015). Examination of the Synthetic Control Method for Evaluating Health Policies with Multiple Treated Units. *Health Economics*.

Städler, I. (2016). Baufirmen bangen um ihre Existenz. *Tages Anzeiger*.

A Appendix: Data Sources

Table 2: Description of Variables and Sources

Variable	Description/Remark	Source
Transaction price	in CHF	SRED
Transaction date	quarter and year	SRED
First/second home	dummy variable	SRED
Year of construction	starting in 1850	SRED
Number of rooms		SRED
Plumbing units		SRED
Number of garages	if more than 3 garages only > 3 indicated	SRED
State of the building	From 1 (=poor) to 4 (=very good)	SRED
Quality/ standard	From 1 (=poor/simple) to 4 (=luxury)	SRED
Micro-location	Location within the municipality; 1 (poor) to 4 (very good)	SRED
Municipality	Official FSO-ID and Name	SRED
Second home share	Per municipality	ARE
Municipality type as defined by FSO	9 categories (e.g., urban, suburban, touristic, rural, high-income)	SRED

B Appendix: Placebo Difference-in-Difference

Table 3: Placebo Difference-in-Difference

	Dependent variable: log price[a]				
	Placebo 2007[b]	Placebo 2008[b]	Placebo 2009[b]	Placebo 2010[b]	Real SHI in 2012[c]
Constant	11.062***	11.062***	11.061***	11.061***	11.038***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
Treatment	-0.081***	-0.078***	-0.077***	-0.075***	-0.067***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)
Treatment x post	0.022***	0.018***	0.015***	0.009	-0.019***
	(0.005)	(0.005)	(0.006)	(0.007)	(0.005)
Observations	170'889	170'889	170'889	170'889	189'986
R^2	0.669	0.669	0.669	0.669	0.69
Adjusted R^2	0.668	0.668	0.668	0.668	0.668

Notes: Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.
[a] Year effects, canton fixed effects, municipality type effects, time-variant controls (i.e., transactions per year and municipality, number of rooms, number of plumbing units, number of garages, quality, state and micro-location) and the second home share are used in every model as independent variables.
[b] Placebo interventions at the beginning of the indicated year. I.e the dummy *"post"* takes the value 1 after the placebo intervention (zero otherwise). Transactions from 2000-2011 (pre-intervention period) included.
[c] Considering the real intervention in 2012. I.e the dummy *"post"* takes the value 1 in 2012 and later (zero otherwise). Transactions from 2000-2016 included.

C Appendix: Varying Covariates

Table 4: Difference-in-Difference for Covariates

	Dependent variable:			
	Quality of the House		State of the House	
Constant	0.870***	2.893***	2.444***	3.24***
	(0.015)	(0.007)	(0.016)	(0.007)
Treatment	-0.009	-0.105***	-0.131***	-0.211***
	(0.012)	(0.007)	(0.013)	(0.007)
Treatment x post	-0.094***	-0.226***	-0.067***	-0.181***
	(0.012)	(0.015)	(0.013)	(0.015)
Time-variant controls[a]	yes	no	yes	no
Municipality type effects	yes	no	yes	no
Canton fixed effects	yes	no	yes	no
Year effects	yes	yes	yes	yes
Second home share	yes	no	yes	no
Observations	189'986	189'986	189'986	189'986
R^2	0.334	0.019	0.274	0.038
Adjusted R^2	0.334	0.019	0.273	0.038

Notes: Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Data from 2000-2016 included, where 2000-2011 are pre-intervention years.
[a] Includes the following covariates: price, transactions per year and municipality, number of rooms, number of plumbing units, number of garages, micro-location and quality as well as state (if not the dependent variable).

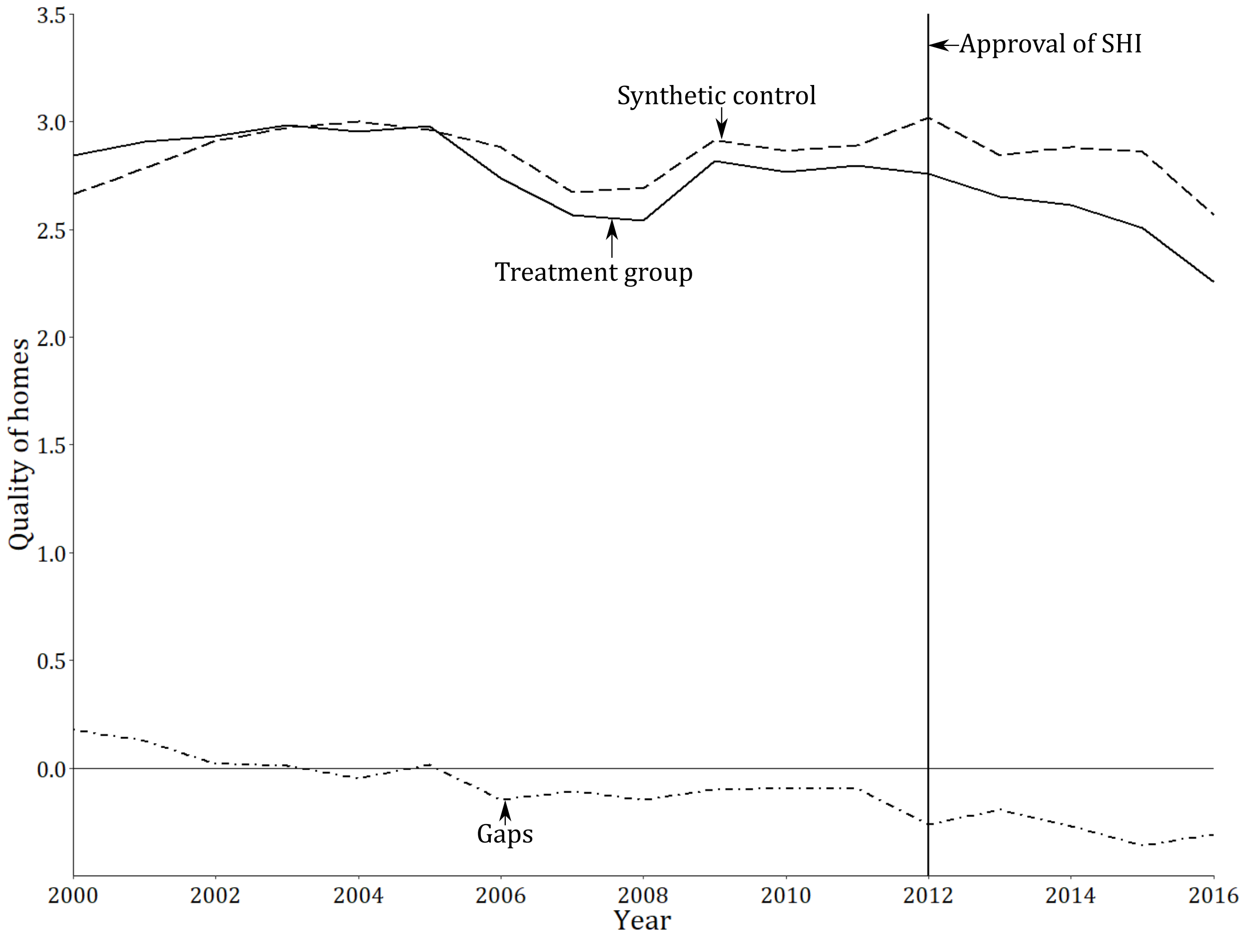


Figure 6: Development of the quality of houses and gaps between the treatment group and the synthetic control; vote was approved in 2012.

D Appendix: Three Stage Model to Adjust Prices

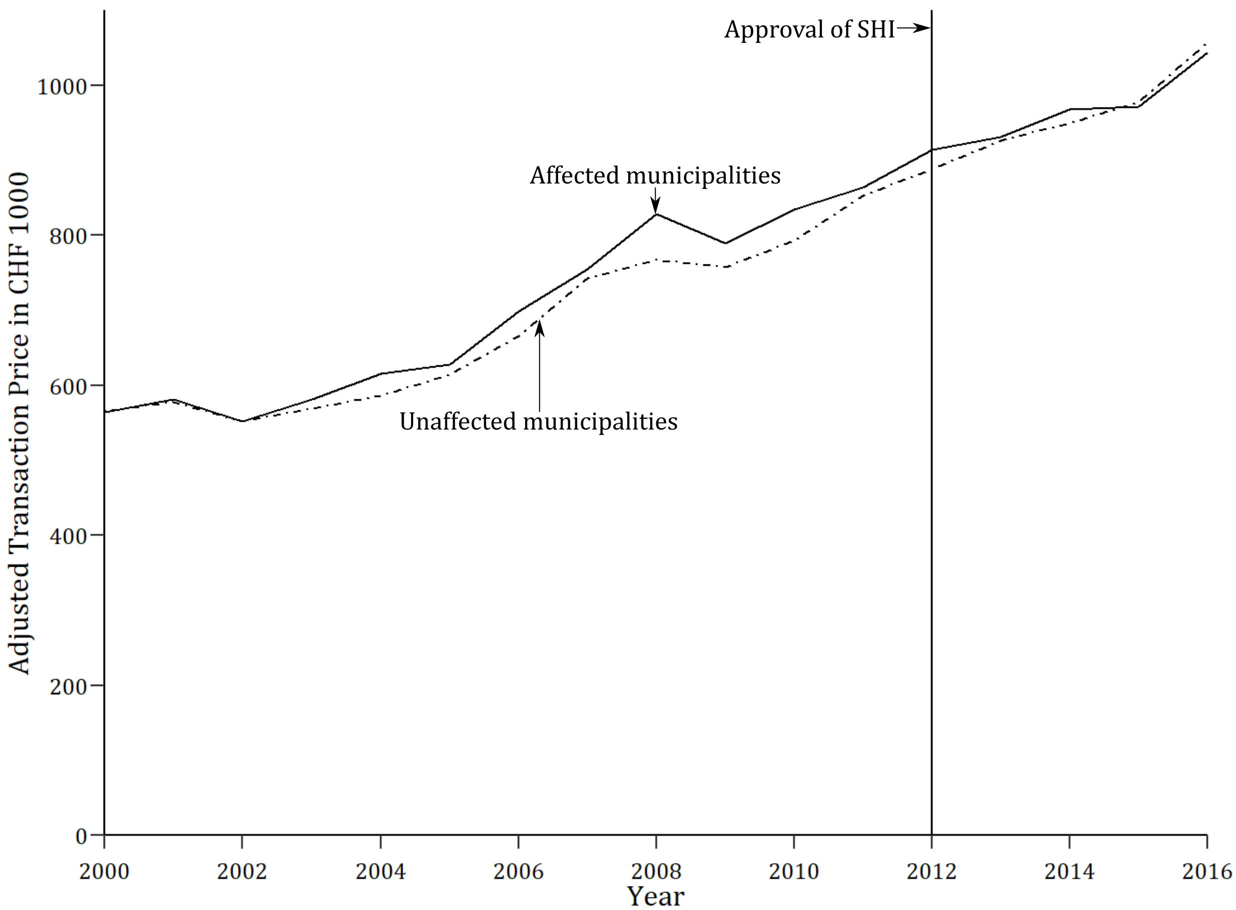


Figure 7: Trends in *adjusted* prices: transactions in affected vs. transactions in unaffected municipalities

E Appendix: Effect on Neighbors of Affected Municipalities

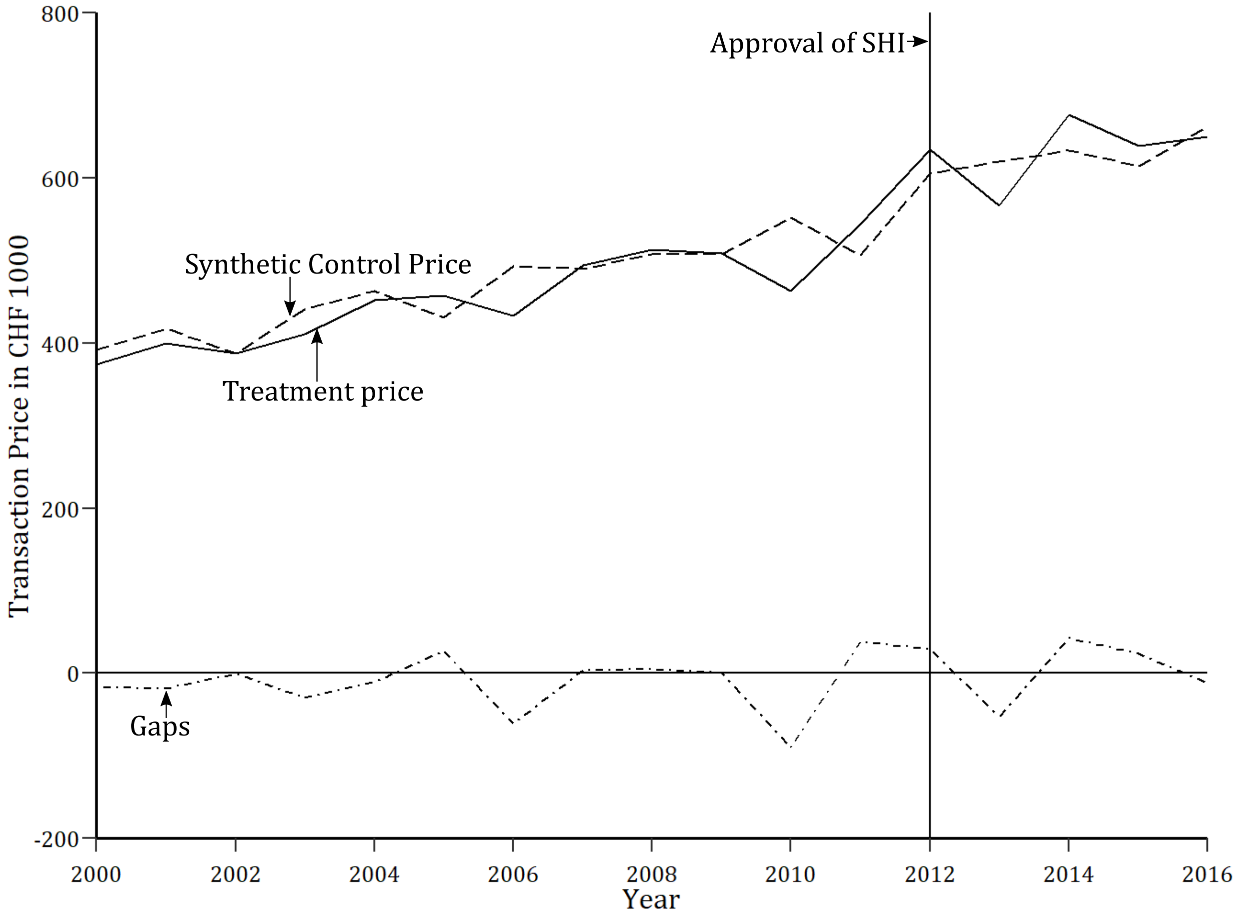


Figure 8: Price development of neighbors of affected municipalities and synthetic control

Center for Regional Economic Development (CRED)

University of Bern

Schanzeneckstrasse 1

P.O.Box

CH-3001 Bern

Telephone: +41 31 631 37 11

E-Mail: info@cred.unibe.ch

Website: http://www.cred.unibe.ch

The Center for Regional Economic Development (CRED) is an interdisciplinary hub for the scientific analysis of questions of regional economic development. The Center encompasses an association of scientists dedicated to examining regional development from an economic, geographic and business perspective.

Contact of the author:

Daniel Steffen

University of Bern

Schanzeneckstrasse 1

P.O.Box

CH-3001 Bern

Telephone: +41 31 631 33 84

Email: daniel.steffen@vwi.unibe.ch

This paper can be downloaded at:

http://www.cred.unibe.ch/forschung/publikationen/cred_research_papers/index_ger.html