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

Quasi-Experimental Evidence Using the Synthetic Control Method

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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. First, I show that the parallel trends assumption of the difference-in-differences method is likely to be violated and then 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 percent and -19 percent three to five years after the intervention.

JEL classification: R21, R31, R52, R58.

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

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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. Currently, there are worldwide efforts for such regulations that attempt to restrict the construction or purchase of second homes by non-local buyers. The stock of second homes has risen sharply in a number of countries such as the USA, United Kingdom, France, Switzerland or China.[1] This strong raise of non-local demand for residential properties leads to growing resistance against second home investors. One concern is that this demand for second homes leads to price increases, which the local population can no longer afford. Another concern is that second homes aggravate urban sprawl. Thus, politicians are seeking regulations that limit the second home construction without entailing any unintended effects on local real estate markets.

An example of such a regulation is the the so-called Second Home Initiative (SHI) in Switzerland. On March 11, 2012, Swiss citizens accepted this very drastic regulation in a popular vote. The SHI bans the construction of second homes[2] in all municipalities with a share of 20 percent 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 percent, and 17 percent of all homes 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 percent and more are common in touristic municipalities. For instance, the SHI affects more than 70 percent 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 estimate the (unintended) causal effects of this 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] See Hilber and Schöni (2020) for an overview.

[2] 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.

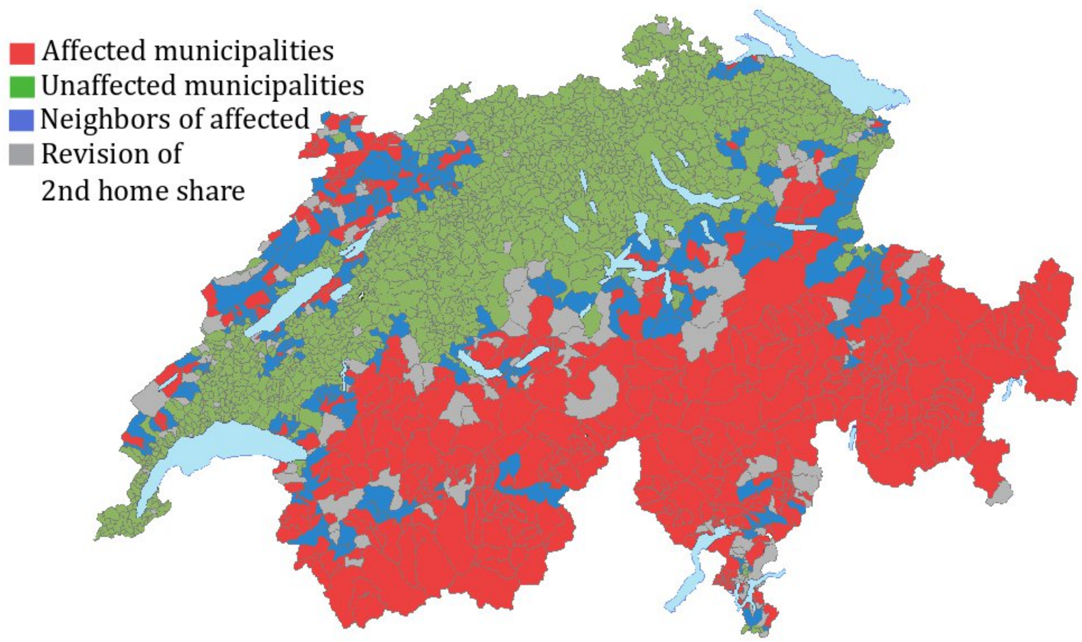


Fig. 1 Municipalities of Switzerland marked as affected or unaffected municipalities, neighbors of affected municipalities and municipalities that asked for a revision of their official second home share.

out of 2352 held a share of more than 20 percent of second homes. This enables a separation of municipalities into an unaffected control (less than 20 percent share of second homes) and an affected treatment group (all others). The possibility of separating municipalities into treatment and control groups might upon first consideration suggest applying a difference-in-differences estimation (DD). However, DD is based on the assumption that the outcome variables of the control and treatment groups have parallel trends in the pre-intervention period (Angrist and Pischke, 2008), which is often not the situation.

In this case, almost all affected municipalities are rather small and remote towns located in the Alps, while most unaffected municipalities are located in the densely populated Swiss Mittelland dominated by major urban centers of the country (see Figure 1). Thus, DD would roughly compare the real estate market of the Alps with a real estate market dominated by major urban centers 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. In a first step, I demonstrate with DD placebo tests, the inclusion of group-specific time trends and causality tests in the spirit of Granger that the parallel trend assumption is unlikely to hold in this context (see Section

6.1). 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, Diamond and Hainmueller (2010) relaxes the parallel trend assumption. Although treatment and control municipalities are different on average, there exist control municipalities, which are similar to the treated municipalities in the Alps. I.e. almost all affected municipalities are located in the Alps, but not all municipalities in the Alps (or municipalities comparable to municipalities in the Alps) are treated. Hence, there are municipalities in the donor pool that are fairly similar to affected municipalities. 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 instead of comparing plain averages. For that reason, the SCM is applied in this paper. While the classic SCM literature usually deals with one or only a few treatment units, the SHI affects numerous 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.[3]

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 more precise 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, Diamond and Hainmueller, 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 levels.

This paper connects to an emerging strand of literature studying the relationship between non-local demand and local real estate markets. Favilukis and Van Nieuwerburgh (2017) and Chao and Yu (2014) develop theoretical models to demonstrate how non-local demand decreases the affordability of residential properties for local buyers. Favilukis and Van Nieuwerburgh (2017) calibrate the model for typical US metropolitan areas and find that rents increase by 19 percent and housing prices by 10 percent if non-local investors buy 10 percent of a city's housing supply. Badarinza and Ramadorai (2018) are using political shocks in a source country as exogenous instrument to address the question whether foreign

[3]Chen, Jain and Yang (2019) conduct a literature review for studies using the SCM with multiple treatment units. Including this study they have found 7 very recent studies using SCM with multiple treatment units. While I am dealing with 89 treatment units in the final data, all studies but one deal with less than 30 treatment units.

capital is responsible for real estate price movements. They find that non-local demand strongly affects London's housing prices. Similarly, Cvijanovic and Spaenjers (2015), Chinco and Mayer (2015) and Sá (2016) all find evidence that non-local demand is linked with higher local housing prices in different contexts. Thus, this strand of literature demonstrates the existence of a relationship between non-local demand and rising housing prices as well as a corresponding decrease in affordability of residential properties for local buyers. These results show that the concerns that real estate is no longer affordable for local buyers due to non-local demand may well be justified. Furthermore, the emergence and growth of this strand of literature in recent years shows the relevance of the topic. However, evidence on the effects of interventions that aim at restricting non-local demand in order to stabilize prices and stop urban sprawl is scarce.

There is a series of studies examining non-local buyer restrictions in China. Du and Zhang (2015) find that purchase restrictions in Beijing reduced the annual growth rate of real estate prices by 7.69 percent, while they find only smaller or no effects for the trial property tax rate in Chongqing and Shanghai. Yan and Ouyang (2018) find as well a substantial negative effect of house-sale restrictions on housing prices. Somerville, Wang and Yang (2020) exploit within city instead of inter-city variation on a purchase restriction and find no relative changes in housing prices between restricted and unrestricted areas. In general, all studies find much larger declines in volume than in prices. In contrast to the SHI in Switzerland, these interventions in China restrict the demand (an individual can only buy a fixed number of residential properties) instead of the supply. Consequently, the mechanism of the SHI is different from the Chinese purchase restrictions: The aim of the SHI is that free building land is only available for local people and thus all new buildings are no longer subject to non-local demand. Moreover, the focus of the initiators of the SHI is also very much on preventing splinter development.

In simultaneous and independent work, Hilber and Schöni (2020) examine as well the effects of the SHI. They estimate the short-run effect of the SHI on real estate prices using a different method than I do – the DD method. Hilber and Schöni (2020) pool the years 2010 and 2011 to obtain a pre-intervention period and the years 2013 and 2014 to obtain a single post-intervention period, while they drop the year of intervention, 2012. I use a longer time period from the year 2000 until 2018 to estimate the yearly effect in the longer-run instead and I am able to estimate whether the effects varies over time. Hilber and Schöni (2020) find a strong negative and significant effect on primary housing prices of approximately -15 percent in the pooled period of the second and third year after the vote (2013 and 2014).

Applying the SCM approach, I find no effect on prices in the first and second year after the intervention (2012 to 2013). However, I find a strong negative effect on prices of between -10 percent and -19 percent compared to the counterfactual in the third, fourth and fifth year after the vote (2014 to 2016). These negative effects are significant at a 99 percent level. Prices in affected municipalities remain below the counterfactual prices in the following years (2017 to 2018). However, it depends on the specification whether price differences in 2017 and 2018 are significantly different from zero. The analysis of the impact channels suggests that the second home initiative has led to lower demand through adverse effects on economic activity and legal uncertainty. The effect found by Hilber and Schöni (2020) in the pooled second and third year after the SHI is comparable to the effect of -10 percent to -19 percent I found for the third to fifth year after the SHI. However, I do not find any effect in the second year after intervention. Further, I show indirectly that the effect on second homes must be negative and very similar to the effect on pre-law first homes not affected by the SHI, while Hilber and Schöni (2020) find a positive price effect on second homes.

This study makes mainly two contributions to the literature. *First*, it adds to the scarce evidence on the effects of interventions that aim at restricting the global emergence of second homes. Worldwide there is increasing resistance to second homes. This is leading increasingly to regulations designed to limit the potentially negative effects of second homes on the local population. This study contributes to understanding the (unexpected) effects of such regulations, which is important for implementing more targeted and effective solutions in the future.

Second, this study is among the first studies to implement the SCM with multiple treatments units and extends the classic SCM in order to compute (more) precise statistical inference. Acemoglu et al. (2016) and Kreif et al. (2015) apply comparable techniques to compute statistical inference. This study shows that the SCM might be a very attractive method to examine interventions in real estate economics with numerous and heterogeneous housing markets.

2 Background of the Second Home Initiative

In 2006, a committee called Helvetia Nostra started to collect signatures for the SHI. In 2007, Helvetia Nostra handed in more than 100,000 signatures to the Federal Chancellery and in January 2008, the Federal Chancellery validated those signatures and the Federal Council authorized the initiative. In 2011, the parliament followed the Council's decision. Consequently, Swiss citizens voted on the SHI in March 2012.[4] 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, the Federal Council,

[4]see Swiss Federal Chancellery for more information, URL: `www.bk.admin.ch/`

and parliament clearly recommended declining the SHI.[5] It is important to know that only a small minority of all popular initiatives held in Switzerland are accepted. Up to April 2020: only 22 of 217 initiatives have been accepted by popular vote.[6] 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.[7] In March 2012, a very narrow majority of 50.6 percent of all voters accepted the SHI. Although surveys predicted a tight race, the result was a surprise for most observers (as placebo studies in Figure 5 or submissions of construction permits in Figure 2 confirm).

The SHI was applied immediately after the vote in March 2012 (Ordinance on Second Homes 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. I.e. building permits for second homes submitted after the SHI can be prevented by objections. 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 Conceptual Framework 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. In the next subsections, the mechanisms of direct and indirect effects are discussed. But first, a closer look at the law is required (Federal Act on Second Homes of 2015, Ordinance on Second Homes of 2012) to understand the impact channels.

The second home law separates the real estate market into two different categories:

1. *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

[5]see "Swiss Parliament" for more information, URL: `www.parlament.ch/`

[6]see Federal Statistical Office (FSO), URL: `www.bfs.admin.ch/`

[7]see Dulio, Claudio, "Die Zweitwohnungsinitiative unterschätzt", *Neue Zürcher Zeitung*, February 24, 2012.

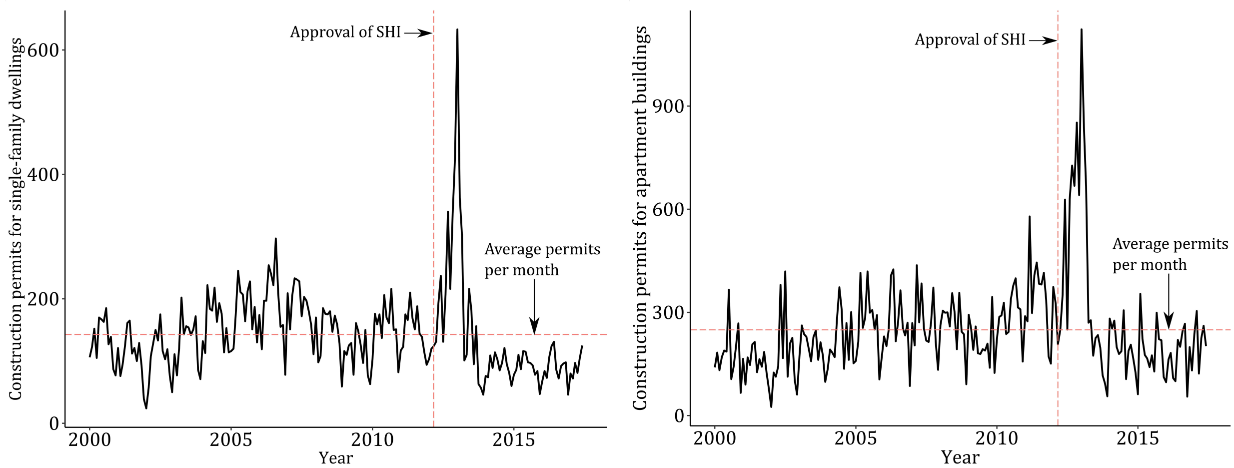
restricted.

2. *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, sold or even rebuilt and enlarged as first or second homes according to the law of 2015. However, according to the ordinance of 2012 (in effect until 2015), 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. Because second homes and pre-law first homes are unrestricted in their use, I assume that these types of homes are substitutes. There is no reason why these two types of homes should vary substantially in layout. If this should be the case, owners always have the possibility to renovate, alter the layout or even rebuild and enlarge properties. Finally, since almost all affected municipalities are small towns in the Alps with highly restricted building zones, first and second homes are not located in different quarters (especially in high-amenity places with typical second home rates of more than 50 percent). Unfortunately, the Swiss Real Estate Datapool (SRED) data used in this paper does not allow to estimate the effect on first and second homes separately, because the information on the second home status of houses is too deficient to deliver reasonable results (see Section 6.3.4 for a discussion). However, estimates in Section 6.3.4 suggest that the SHI affects first and second homes similarly. This finding supports the argument that pre-law first and second homes are rather close substitutes.

3.1 Direct Effects

First, the *direct effects* of the SHI on demand and supply for real estate are considered. The separation in homes restricted (new first homes) and unrestricted in use (all others) 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.

On the other hand, is the use of second homes and pre-law first homes unrestricted,



(a) Permits for single-family dwellings **(b)** Permits for apartment buildings

Fig. 2 Monthly construction permits in affected municipalities (*Source*: Baublatt/Credit Suisse).

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 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 percent[8] 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.

Although the SHI was supposed to cut the supply in affected municipalities, practitioners often argue that the SHI caused a last-minute glut of new construction project submissions: Many land owners became aware that they cannot build second homes on their land anymore as soon as the new second home law is enforced. Therefore, they applied for last-minute building permits shortly after the SHI vote in 2012. Even though these building applications submitted after the vote should not have been approved, there was a glut of building applications accepted right after the SHI vote took place. The number of approved building permits right after the vote in March 2012 was more than three times higher than the long-term average (see Figure 2). Two years after this panic-stricken submission of construction projects, the corresponding apartments came on the market and provided a one-time boost to supply. Although objections[9] prevented all the planned objects from being built, the

[8] 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.

[9] The Federal Court declared that buildings permits for applications submitted after the SHI vote are

initiative nonetheless might have sparked a short-term boost in supply and lead to a delayed fall in prices when these newly built homes came on the market.[10] In Section 7, I check whether there is any evidence for this impact channel.

3.2 Indirect Effects

3.2.1 Adverse Effects on Local Economies

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. As mentioned in the introduction, second homes are of great economic importance in affected regions. Not only do they guarantee many jobs in the locally very vital construction sector,[11] but they are also an important source of income for hotels and mountain railway companies. Hotels, mountain railways and other companies involved in the tourism industry sold second homes or land to cross-subsidize investments in infrastructure. Since this ability to cross-subsidize is gone, tourism resorts might no longer be 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. The introduction of second home taxes increases the implicit price of a second home. Therefore, the SHI might have reduced local and non-local demand in affected municipalities, which leads to a decline in prices.

Hilber and Schöni (2020) develop a formal model that explores the housing and labor market impacts of a ban on second homes in a general dynamic equilibrium setting and formalizes some of the arguments mentioned above. The predictions of the model crucially

invalid. Hence, objections against these permits prevent the construction of such buildings.

[10]See for example Kohler, Franziska, "Wo die Wohnungspreise in den Bergen jetzt tiefer sind", *Tages-Anzeiger*, December 11, 2016 or Martel, Andrea, "Kein Run mehr auf Ferienwohnungen", *Neue Zürcher Zeitung*, July 17, 2017 for a discussion of the one-time boost in supply due to the SHI.

[11]In 2015, 8.1 percent of the labor force in the mountain cantons (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 percent. In the two cantons with the highest share of affected municipalities, 8.8 percent (Graubünden) and 8.7 percent (Valais) of the labor force is employed in the construction sector (see FSO Structural Survey 2015).

depend on whether pre-law first and second homes are poor substitutes or not. The effect of the SHI on real estate prices is ambiguous, if first and second homes are close substitutes: There is a negative effect on local wages that decreases the aggregate demand for housing on the one hand side, while the SHI causes a cut in supply of pre-law first homes and second homes on the other hand side. If first homes and second homes are poor substitutes the model predicts a decrease in first home prices and an increase in second home prices. As discussed above, I assume pre-law first and second homes to be close substitutes. I show that the effect on first and second homes is very similar (see Section 6.3.4), what supports the assumption that pre-law first homes and second homes are rather close substitutes.

3.2.2 Legal Uncertainty and the Lock-In Effect

Another very important point is that 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 an 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 and do not sell their properties at all. 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 primarily those in need of liquidity sell their homes. Therefore, houses sold after the vote in 2012 might be of lower quality on average. Hence, average housing 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 effect via a drawback on local economies or a lock-in effect. Furthermore, there might even be a negative effect due to a one-time surge in supply. Which of these effects dominates needs to be clarified empirically.

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 Section 6.1, a DD approach does not appear to be suitable in this case. Therefore, the SCM developed by Abadie and Gardeazabal (2003) and Abadie, Diamond and Hainmueller (2010, 2015) is chosen. The point is that almost all affected municipalities are located in the Alps, but not all municipalities in the Alps are affected. Further, there exist as well other control municipalities which are not located in the Alps that are comparable to treatment municipalities. Hence, there are still control municipalities in the donor pool that are similar to affected municipalities. The SCM attempts to construct an optimal synthetic control by assigning different weights to different control units. In the next subsections, this data-driven procedure is presented.

4.1 Classic Synthetic Control Method

The starting point is the classic SCM following Abadie and Gardeazabal (2003) and Abadie, Diamond and Hainmueller (2010, 2015). Suppose that our dataset contains J+1 different units (in this 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). **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 **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, **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 outcome of the treatment unit and the synthetic control of one year. 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 $w_2 + w_3 + \ldots + w_{J+1} = 1$. The $\mathbf{W}^*$ that solves (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, Diamond and Hainmueller (2010, 2015).

As soon as I 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. $\widehat{Y}_{1t}^{SC}$ 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.

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, 89 treatment units have to be considered. There is not much literature dealing with that many treatment units. Abadie, Diamond and Hainmueller (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) come 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 $\hat{Y}_t^{SC} = \overline{Y}_t - \overline{\alpha}_t$, 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 an 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 periods (t=T_0,..., T) post-intervention years. The ratio of the post- and pre-intervention RMSE should be larger than 1, since the gaps $\overline{\alpha}_t$ are supposed to be substantially larger 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 larger the gaps), 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, Diamond and Hainmueller, 2010). To evaluate the statistical significance of the results in the next step, the RMSE-ratio is required.

Following Abadie, Diamond and Hainmueller (2010), I assess the significance of the estimates by conducting a series of placebo studies. The difference from Abadie, Diamond and Hainmueller (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, Diamond and Hainmueller (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 $J_1 \gg J_0$. This 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 of these N gaps. Using the standard deviation, CI can easily be computed using average treatment prices as the basis.

5 Data and Descriptive Statistics

Swiss Real Estate Datapool (SRED)[12] collects the data used in this paper. SRED is an association founded by the three Swiss banks UBS, Credit Suisse and Zürcher Kantonalbank. The SRED dataset contains information on real estate transactions executed by these three banks between 2000 and 2018. It contains information for more than 240,000 transactions completed during this period and includes transaction prices as well as other relevant attributes.

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). Furthermore, I use the administrative data of the "Gebäude- und Wohnungregister" (GWR)

[12]See `https://www.sred.ch/`

provided by the FSO to estimate the effect of the SHI on the housing stock and data on unemployment provided by the State Secretariat for Economic Affairs (SECO) to estimate the effect of the SHI on unemployment rates. Finally, the "Baublatt" in cooperation with the Credit Suisse collects all building permits on a monthly basis. I use these data in order to see if there actually was an increase in building permits right after the vote (Figure 2).

In order to apply the SCM I only keep municipalities with at least one transaction in every year. Furthermore, I dropped all municipalities with a second home share between 18 percent and 20 percent. This is done because market players might foresee that these municipalities are going to cross the threshold of 20 percent 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 percent and 20 percent 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, 613 or approximately 30 percent of these 2209 municipalities remain in the final dataset. It is important to know that almost all of the removed municipalities 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 613 municipalities contains 186,508 or approximately 77 percent 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.[13] Hence, the post-intervention period (2012 to 2018) starts in April 2012, right after the vote took place.

A summary of the most important housing characteristics is presented in Table 1. A typical 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 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. Figure 10 presents the price development in control and treatment municipalities. Prices in treated municipalities are stagnating after the vote in 2012, while prices in unaffected municipalities continue to increase.

[13]For instance, the year 2000 starts in April 2000 and ends in end-March 2001.

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

	Control group		Treatment group	
	Pre-interv.	Post-interv.	Pre-interv.	Post-interv.
Transaction price[a]	761,246	1,002,434	628,214	785,683
Transactions [b]	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[c,d]	2.87	2.71	3.08	2.93
Quality[d]	2.87	2.87	2.84	2.59
State[d]	3.17	2.99	2.88	2.56
Year of construction	1983	1988	1984	1984

Notes: *a* In Swiss Francs (CHF)
b Per year and municipality
c Micro-location is the quality of the location of a property within the municipality
d Values between 1 (=poor) and 4 (=very good)

6 Results

6.1 Robustness Checks for the DD Approach

As discussed in Section 1, municipalities affected by the SHI are placed in the Alps and unaffected municipalities are mostly located in the Swiss Mittelland. Therefore, the average control municipality does not seem to be a suitable counterfactual for affected municipalities and the parallel trend assumption of the DD strategy is likely to be violated. While it is not possible to directly test the parallel trends assumption, there are several checks that are able to narrow down whether the key identification assumption holds or not. In this section, all tests commonly mentioned in the literature (see e.g. Angrist and Pischke, 2008; Wing, Simon and Bello-Gomez, 2018, for an overview) are applied to further investigate whether the parallel trends assumption holds in the case of the SHI context.[14]

One possible check is to estimate the effects of placebo interventions before the actual SHI vote took place. 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 remaining pre-intervention period 2007 to 2011 (the actual vote took place in 2012). Because there was no vote in 2007, there should be no significant effect for the 2007 to 2011 period. If there are significant effects of such placebo interventions, the common trend assumption is not credible. I conducted

[14]These tests are all quite closely related to each other and test very similar properties. Nevertheless, I conduct all tests in order to provide a complete picture.

such placebo tests for the pre-intervention years 2006, 2007, 2008, 2009, 2010. Results in Tables 2 and 3 in Appendix B show that these placebo intervention effects are significant and, hence, that the DD is not likely to be a suitable identification strategy for the SHI context.[15]

Further, there are two common checks for the validity of the common trend assumption, the inclusion of group-specific time trends and causality tests in the spirit of Granger. If the group-specific time trends are significantly different from each other or if their inclusion notably changes the estimated effect of the intervention, this is discouraging for the parallel trends assumption (see Besley and Burgess, 2004, for an application). As shown in Tables 4 and 5 in Appendix C the difference in group-specific time trends is significant and the estimated treatment effect changes clearly when different group-specific time trends are allowed. Allowing treatment and control group to follow different trends changes the effect of the SHI from a significant negative effect to a (significant) positive effect when using transaction-level data (see Table 4). When aggregating the data on municipality level, as well a strong change of the estimated effect is found (see Table 5). Hence, the inclusion of group-specific time trends shows in a revealing way that the parallel trends assumption is likely to be violated.

Finally, I compute lags and leads of the effect similar as in a Granger causality test. The idea is to check, whether past interventions predict outcomes while future interventions do not (see Angrist and Pischke, 2008 or for an application Autor, 2003). Table 6 in Appendix D shows that effects in several of the five years before the SHI took place are significantly different from zero for transaction-level data (see columns 1 and 2). Effects in the years before the SHI took place are as well significantly different form zero when using municipality level data (see columns 3 and 4).

In conclusion, the validity of the common trend assumption was already doubted, because treatment group municipalities are located in the Alps and are mostly remote municipalities. Control municipalities are partly as well located in the Alps, but the control group is clearly dominated by the densely populated Mittelland region including all Swiss major cities. All three checks conducted in this section, placebo tests, inclusion of group-specific time trends and causality test in the spirit of Granger, indicate that the common trend assumption is violated in the context of the SHI. Therefore, DD seems not to be suitable to estimate the effect of the SHI. For this reason, the SCM is applied in the following Sections.

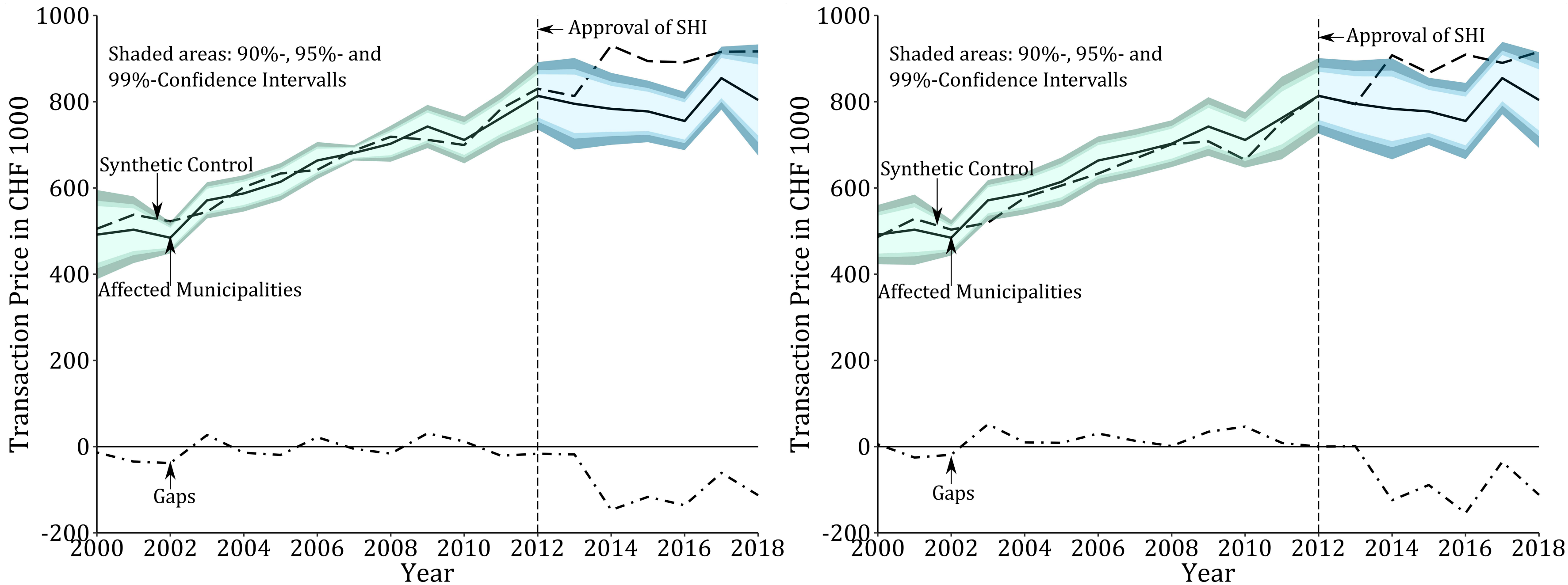
[15]Once I ran the DD placebo tests with transaction level data in order to keep as much information as possible in the data (see Table 2). However, in order to conduct the SCM method I have to aggregate the observations on municipality level and make sure that the data is balanced, i.e. make sure that every municipality has at least one observation per year. Hence, I ran additional DD placebo tests with data containing municipality level observations, which is the same as the data used in the SCM (see Table 3).

6.2 Main Results Using the Synthetic Control Method

In this section, results of the SCM as illustrated in Section 4 are presented. 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, garages, micro-location, quality and state of the property as well as the pre-intervention outcome variable data point of one year, i.e. the transaction price of 2007. Kaul et al. (2017) point out that using too many pre-intervention outcomes could cause a bias. Therefore, I run the SCM two times: First, I include only the price level of 2007, and second, I do not include any pre-intervention outcome variables. Figure 3 shows the effect of the SHI on the prices taking both approaches. Both approaches return very similar results. Because the approach including one pre-intervention outcome variable data point offers a better pre-intervention fit, I focus on the approach including the pre-intervention outcome of 2007.

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 about 23,000 Swiss Francs (CHF) or 3.56 percent of the average pre-intervention treatment price, which indicates that the synthetic control is an accurate counterfactual. Housing prices are in 2014 (-19 percent), 2015 (-14 percent) and 2016 (-18 percent) clearly lower than the corresponding synthetic control prices. For instance, in 2016, the average treatment housing price was CHF 136,000 lower than the synthetic control price. However, no effect in the post-intervention years 2012 and 2013 is visible. There seems to be a rebound in prices in 2017, where the gap is only -7 percent of the treatment group price. However, the gap widens again in 2018 (-14 percent) and is still significant on the 95 percent level for both years, 2017 and 2018. Hence, prices in treatment regions did not yet recover from the drop in 2014. The RMSE of the post-intervention period is with CHF 101,000 or 11 percent of the average post-intervention price clearly higher than in the pre-intervention period. This results in a RMSE-ratio of 4.33. In 2002, the synthetic control 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 to 2016 period) is still almost twice as that in 2002.

In Figure 3b, we observe a very similar effect for the approach without using the pre-intervention outcome of 2007. In fact, most numbers are very close to those discussed in Figure 3a. However, the rebound in prices in 2017 is stronger in this approach. The gap in 2017 is with -4 percent considerably smaller than in Figure 3a and not significant anymore. However, the gap increases again in 2018 (-14 percent) and becomes significant on a 95 percent level.

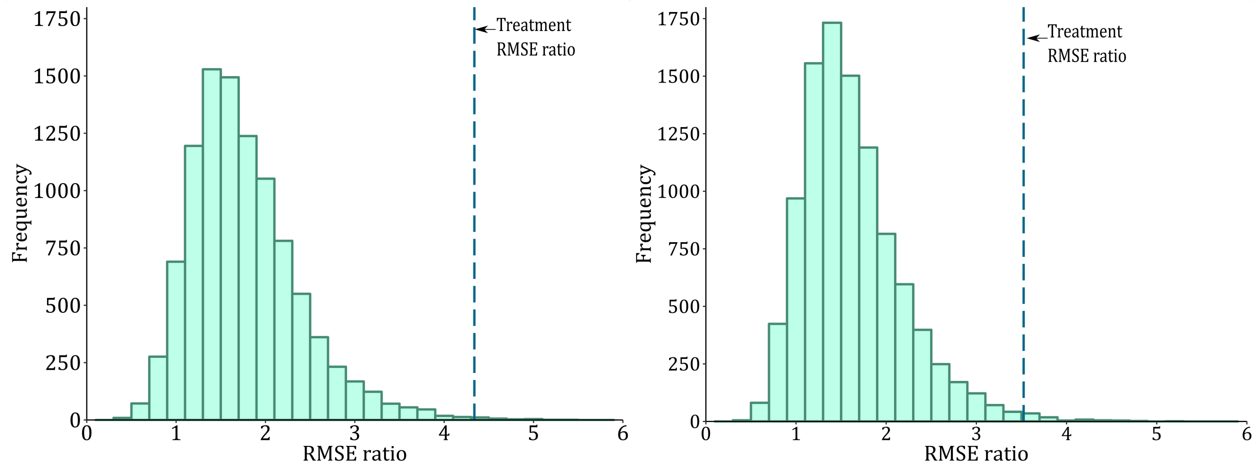


(a) Outcome of 2007 included

(b) No outcome variable included

Fig. 3 Price development of treatment group and synthetic control and corresponding gaps for both approaches including one and no pre-intervention outcome variable data point to compute synthetic control.

Notes: CI in both approaches are based on 10,000 placebo runs.



(a) Outcome of 2007 included **(b)** No outcome variable included

Fig. 4 Distribution of 10,000 placebo RMSE-ratios.

What is the probability of obtaining results of this magnitude by chance? To evaluate the significance of the results obtained above, I run placebo tests as described in Section 4.2. I run 10,000 permutation tests and correspondingly obtain 10,000 placebo RMSE-ratios. These 10,000 placebo iterations can be used to construct CI as described in Section 4.2. In Figures 3a and 3b these CI are reflected in the shaded areas. The synthetic control prices in 2014 to 2016 are outside the 99 percent CI and thus, the price decrease in affected municipalities compared to the synthetic control is highly significant on a 99 percent significance level in those years. As mentioned before, there is no effect on prices in the first two years after the approval of the SHI.

As explained in Section 4.2, we are going to look as well at the RMSE-ratio distribution of the placebo permutations. This approach is considering the significance of the total post-intervention period instead of individual years as in the CI approach. Because there was no effect in the first two years after the approval of the SHI, the significance of the effect might be lower according to the RMSE-ratio approach. The distribution of these placebo RMSE-ratios is presented in Figures 4a and 4b. When including one outcome data point in the synthetic control computation only 23 placebo RMSE-ratios are greater than the treatment RMSE-ratio of 4.33 (see Figure 4a), which corresponds to a p-value of 0.002 (see Equation 6 for more information on the calculation). Hence, the the overall post-intervention effect in Figure 3a is significant with a 99 percent significance level, although there was no effect found in two of five post-intervention years. When not including the outcome variable, only 70 placebo RMSE-ratios are bigger than the treatment ratio of 3.52 (see Figure 4b). Hence, in this case the overall post-intervention effect is as well significant on a 99 percent level.

6.3 Robustness Checks

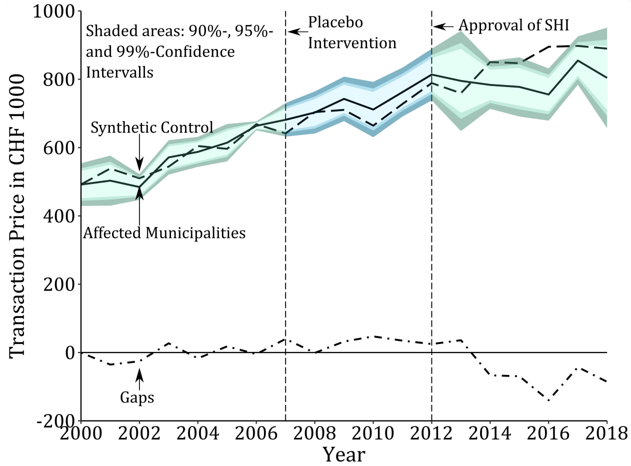
6.3.1 Placebo In-Time

The idea behind the placebo in-time approach is that I introduce a placebo intervention before the actual intervention took place.[16] Therefore, there are three periods in this approach: The pre-placebo period, the between placebo intervention and actual intervention period and the post-intervention period. With this approach I can answer two questions: First, I can check whether the SHI was approved surprisingly or whether it was foreseen by market agents (see 2 for a short discussion). If the SHI approval was no surprise, there should be an effect before the actual approval in 2012. Furthermore, I can check whether the gaps of the magnitude found in the baseline estimation above occur as well after a placebo intervention.

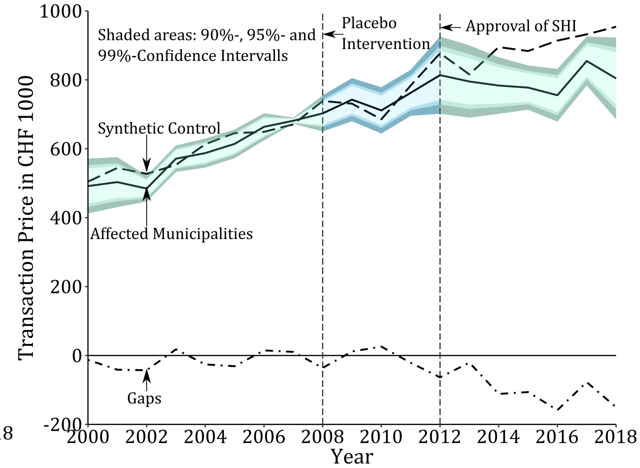
Figure 5 shows the results of four different placebo interventions from 2007 to 2010. We are mainly interested in the period between placebo intervention and actual intervention (blue-shaded part in Figure 5). Because there was no actual treatment in these placebo years, there should be no effect. Therefore, we expect the counterfactual price to be within the inner CI band before the the SHI was accepted in 2012. Using the example of the placebo intervention in 2008 (see Figure 5b), we see that the pre-placebo RMSE (CHF 27,000) is larger than the RMSE of the period between placebo intervention and actual intervention (CHF 25,000). This corresponds with a p-value of 0.92 under the null hypothesis that prices between treatment and synthetic control do not differ in the period between placebo and actual intervention. Moreover, synthetic control price trend never leaves the inner CI band in the between-period. Hence, there is no significant placebo effect in the period between the placebo and the actual intervention. The same applies to all four placebo interventions in Figure 5. This supports the claim that the results obtained do not occur simply because I stop minimizing the distance between synthetic control and actual treatments in 2012. Furthermore, it confirms that agents did not foresee the outcome of the vote in 2012 and did not adapt their behavior accordingly.

As in the benchmark approach, there is no significant effect in the first two years after the actual intervention in 2012 and a significant negative effect three to five years after the intervention in all placebo approaches in Figure 5. Thus, post-treatment effects are very similar to the benchmark results.

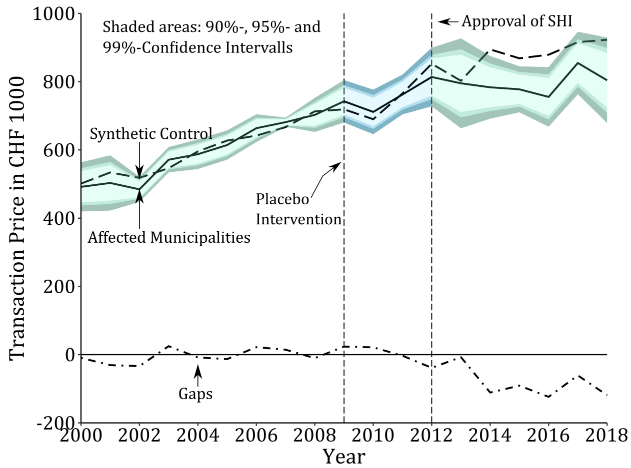
[16] The idea behind this kind of placebo tests is the same as behind the DD placebo tests in Appendix B.



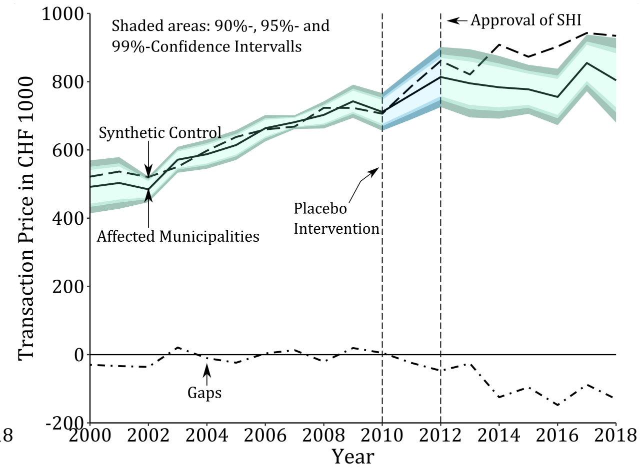
(a) Placebo intervention in 2007



(b) Placebo intervention in 2008



(c) Placebo intervention in 2009



(d) Placebo intervention in 2010

Fig. 5 Placebo in-time approach with placebo interventions in 2007 to 2010.
Notes: CI in all approaches are based on 10,000 placebo runs and the outcome of 2004 is included in the construction of the synthetic control.

6.3.2 Only Alpine Municipalities

As shown in Figure 1, almost all affected municipalities are located in the Alps. This is the reason, why I do not use the DD approach but SCM to make sure our treatment units and synthetic controls are comparable. However, one might argue that there are events (e.g. adverse effects on tourism industry) which hit Alpine regions harder than any other municipalities.[17] Therefore, I keep only those municipalities in the dataset, which are officially declared as Alpine municipalities according to the FSO.[18]

[17] Note, that the synthetic control of the baseline estimation consists mainly of municipalities located in the Alps (weight of about 65 percent) and/or municipalities for which tourism is an important sector.

85 of 89 municipalities of the treatment group remain in the dataset when I include only Alpine regions. This shows again, that the SHI affected almost only this one region. On the other hand only 82 of formerly 524 control municipalities remain in the donor pool. Since the donor pool is smaller, we expect a less accurate fit in the pre-intervention period. However, the 82 Alpine control municipalities that remain in the donor pool are likely to be similar to the treatment municipalities, since they are located in the same region. The result is shown in Figure 6a. The pre-intervention RMSE is with CHF 35,000 higher than in approaches with bigger donor pools. However, the pattern is very similar to the baseline approaches including all municipalities. There is no effect in the first two years after the intervention, but a strong negative effect three to five years after the intervention (2014 to 2016). The magnitude of the effects in these years is almost the same as in the benchmark approach with -14 percent in 2014, -9 percent in 2015 and -17 percent in 2016. As well as in the benchmark estimation there is a rebound in 2017 (-6 percent), but in 2018 the gap widens again (-15 percent). Hence, the effect of the SHI remains mostly the same, if we include only Alpine regions and the effect seems not to be caused by an adverse effect other than the SHI that only affects the Alpine region. There are no CI computed, because there are less units in the donor pool (82) than in the treatment pool (85). Nevertheless, I used the 82 control municipalities as placebo units and computed a single synthetic control for them. The gaps between this placebo group and its synthetic control are shown in red ("Placebo Gaps") in Figure 6a. These post-intervention placebo gaps are clearly smaller than the original gaps.

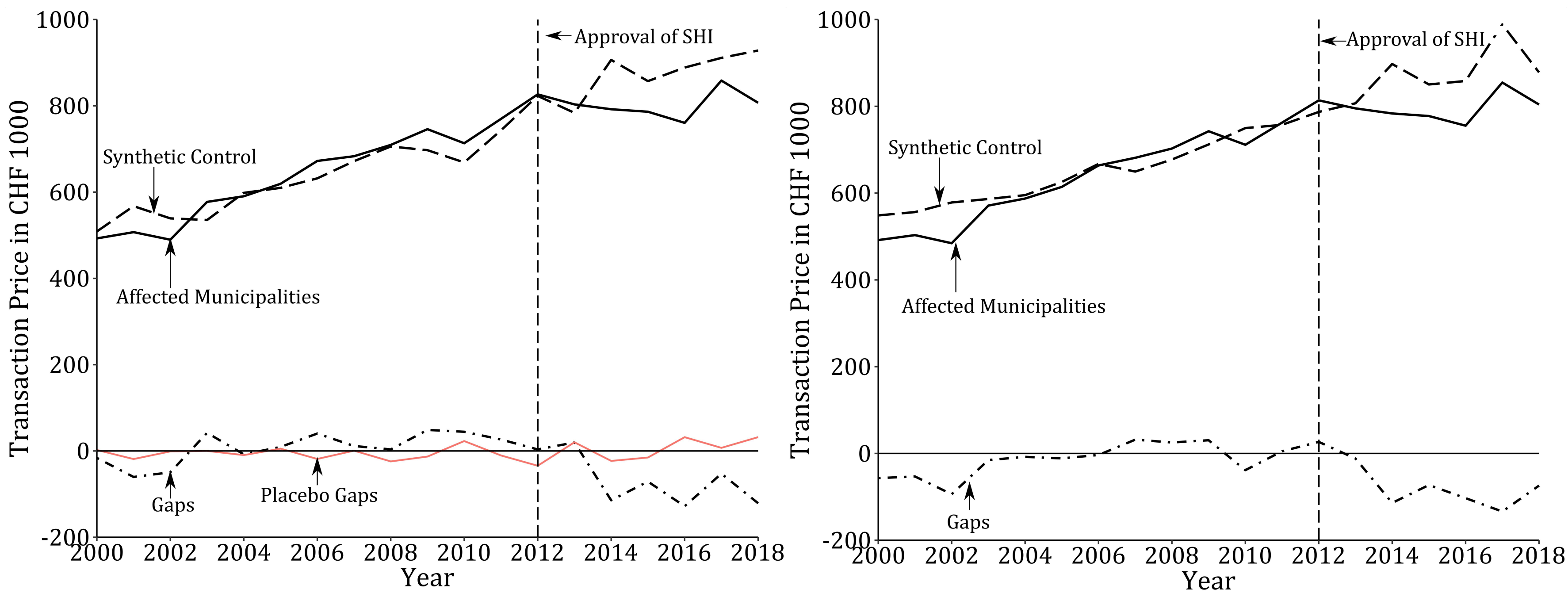
6.3.3 Neighbor Municipalities

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 the 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 and violate the stable unit treatment value assumption (SUTVA).

Only 10 neighboring municipalities (of 56 in the final dataset) receive a weight of more than 1 percent in the SCM estimation in the main analysis. Nevertheless, in total, almost 64 percent of the synthetic control in the main analysis consists of neighbors of the treatment

Thus, it is unlikely that there are shocks which affect only the treatment municipalities but not the synthetic control.

[18] The FSO follows the European mountain areas delineation. I include in this robustness check only municipalities which are classified as "Alpine". See FSO "Räumliche Typologien", URL: `www.agvchapp.bfs.admin.ch/de/typologies/query`



(a) Only Alpine Municipalities

(b) Excluding neighbor municipalities

Fig. 6 Robustness checks: Approach using only Alpine regions as defined by FSO and estimation excluding all municipalities next to affected municipalities from the donor pool.

Notes: Outcome of 2007 is included in both estimations.

units. Hence, if the SHI affects these neighboring municipalities, the results obtained earlier could be biased.

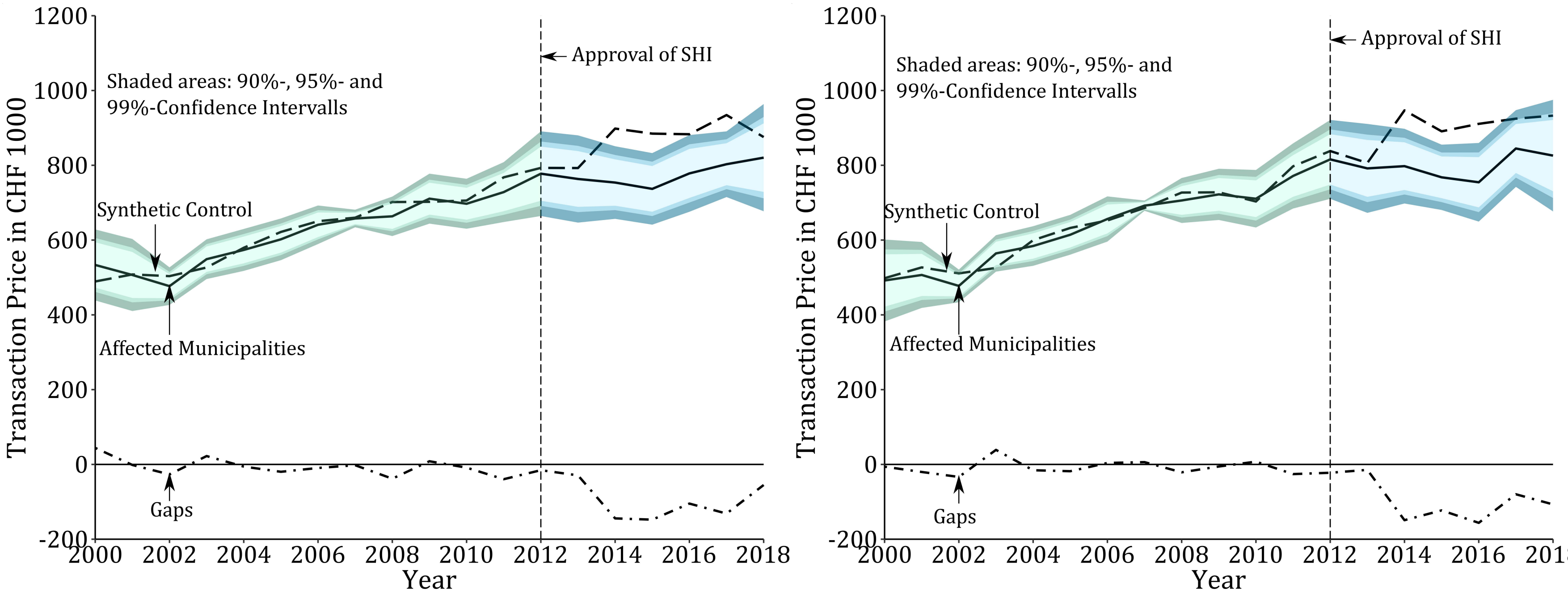
In a further robustness check I exclude all neighbor municipalities of the treatment group from the sample to make sure that spillovers on neighbor municipalities do not harm my estimation. Because I reduce the number of municipalities in the donor pool and these neighbor municipalities are likely to be very similar to the treatment group, the pre-intervention fit is expected to be less accurate than in the baseline estimations with bigger donor pools. The results of this approach can be found in Figure 6b. The pre-intervention RMSE is with CHF 38,500 bigger than in the baseline estimations. But again, the pattern remains very similar: There is no effect found in the first two years after intervention, but a strong and negative effect in all later years. While the effect is very similar to the benchmark effect in 2014, the magnitude of the effect in the years 2015 to 2016 is negligibly lower than in the benchmark estimation (between -16 percent and -12 percent in 2015 and 2016 compared to -18 percent to -14 percent in the baseline estimation). However, in contrast to the benchmark estimations, there is no rebound effect in 2017. Because the effects without neighbors are very similar to those in the baseline estimation, except for the effect in 2017, I conclude that my estimation is not significantly biased by spillovers.[19]

6.3.4 Heterogeneity of the Effect: First Homes vs. Second Homes

As mentioned in Section 3, the effect on second homes and pre-law first homes is supposed to be different, if second homes and pre-law first homes are poor substitutes: We would expect an increase in prices of second homes and a decrease in prices for pre-law first homes. I assume that second homes and pre-law first homes are rather good substitutes (see Section 3). This assumption could indirectly be tested by estimating the effect separately for second and first homes.

However, the information on the second home status of transactions in control municipalities is not reliable enough to estimate the effect on first and second homes: Only 0.37 percent of all pre-intervention transactions in control municipalities are declared as second homes, i.e. not even 47 of about 12,000 transactions per year in *all* control municipalities in Switzerland. Given the average administrative second home rate of more than 10 percent in control municipalities this information does not appear to be reliable. This does not change much after the vote: In the first three years after the vote (2012 to 2014) only 0.3 percent of all transactions in control municipalities involved second homes (i.e. 30 observations per year in *all* 524 control municipalities in Switzerland). There is no unaffected municipality with

[19]Hilber and Schöni (2020) conduct a similar robustness check. This check does not change their results and indicates that including municipalities close to treated municipalities does not bias the estimated effects in the baseline model.



(a) Effect on first homes only

(b) Effect on first and second homes

Fig. 7 Heterogeneity of the effect: Effect on first homes only vs. effect on first and second homes.
Notes: Only municipalities with at least one first home transaction in every year included (48); CI are based on 10,000 placebo runs and the outcome of 2007 is included in both estimations.

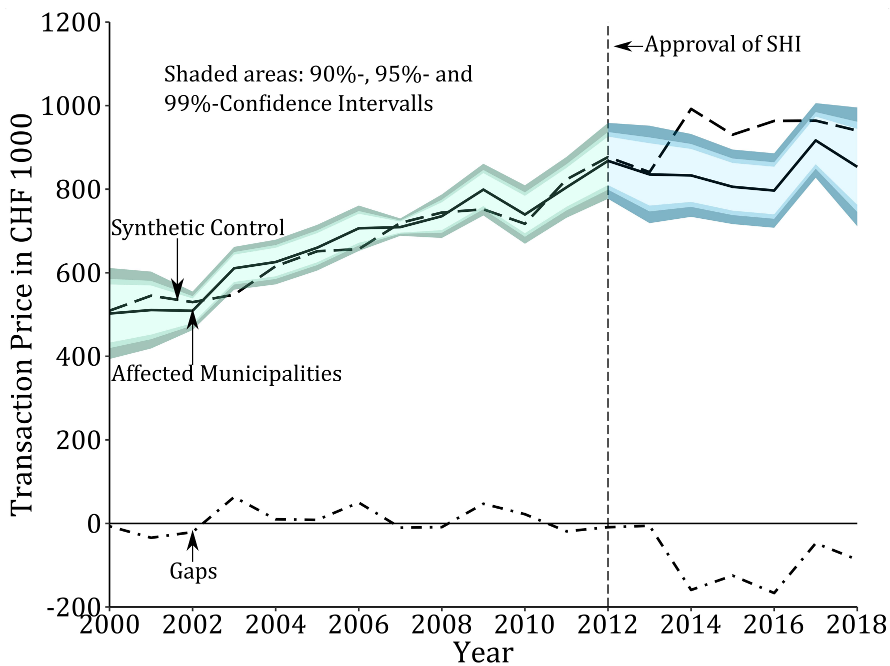
at least one second home transaction in every year. Hence, there are two major problems when separating first and second homes in control municipalities. First, the information on second home status does not appear to be correct nor reliable for control municipalities. Second, because there are only very few transactions in control municipalities, the aggregate on municipality level contains only a single observation for most of the few municipalities left. Because aggregates consist usually only on one observation, there is a huge volatility in prices and due to these erratic price changes unaffected municipalities do not serve as sensible control group anymore. Therefore, focusing only on second home transactions does not make sense and should be neglected.

In order to shed some more light on this matter, I run the estimation procedure only for first homes. In this case, only 48 affected municipalities are left with at least one first home transaction in every year. I repeat the estimation for first *and* second homes with the same 48 affected municipalities. If second homes and first homes are poor substitutes, we expect a negative effect on first homes and a positive effect on second homes. 54 percent of all transactions in treatment municipalities are second homes. Hence, if first and second homes are poor substitutes and the effect on second homes is positive, the effect including only first homes must be substantially more negative than the effect including first and second homes.

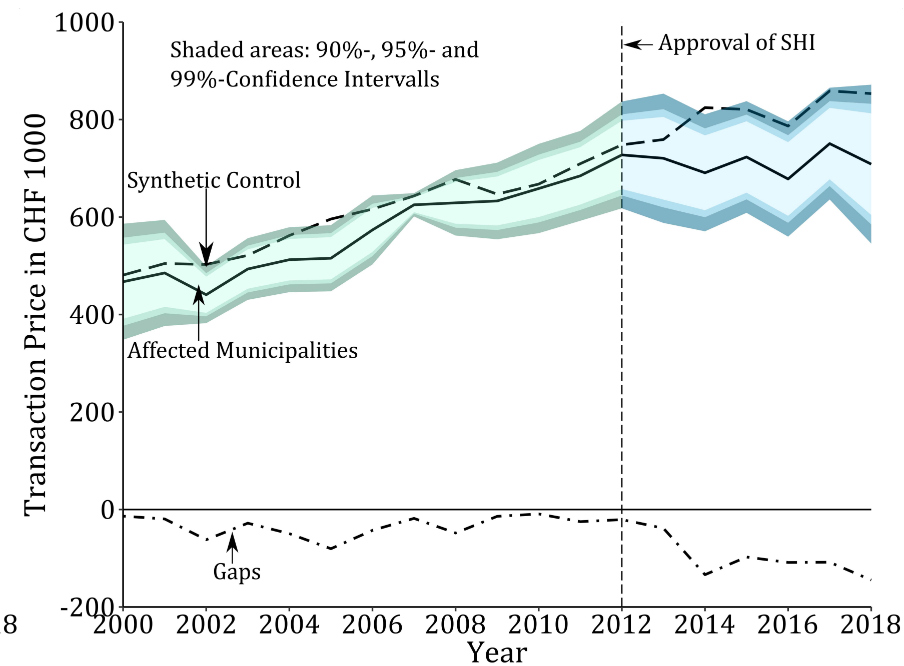
The results of both estimations can be found in Figure 7. The effect is negative and of a similar magnitude in both approaches. A closer look to the data reveals that the effect including first and second homes is even slightly more negative than the approach including only first homes: While there is no significant effect in the first two years after the vote, the average effect in three to five years after the vote is with -15.6 percent for first and second homes lower than for first homes only with -15.2 percent. These results indicate that the effect on second homes is negative and similar to the effect on first homes. This finding supports the assumption that first and second homes are rather good substitutes. This result contradicts the results of Hilber and Schöni (2020), who find a positive effect for second homes. Hilber and Schöni (2020) estimate the effect separately for second homes.

6.3.5 Heterogeneity of Effect: Touristic vs. Less Touristic Municipalities

In this Section, the heterogeneity of the effect is examined in more detail. The best known touristic municipalities with secondary housing rates above 50 percent are considered separately from the other less touristic municipalities. There are 56 affected municipalities with a share of second homes of more than 50 percent and 33 affected municipalities with a lower share. Figure 8a shows the effect of the SHI on housing prices in the best-known touristic municipalities. The effect is very similar to the effect in the benchmark estimations: No effect in the first two years after the intervention and then a strong negative effect of



(a) Effect on most touristic places



(b) Effect on less touristic places

Fig. 8 Heterogeneity of the effect: Effect on more touristic places (second home share above 50 percent) and less touristic places (second home share between 20 percent and 50 percent).

Notes: CI in both approaches are based on 10,000 placebo runs and the outcome of 2007 is included in both estimations.

-15 percent to -20 percent three to five years after the vote. In the sixth (-5 percent) and seventh (-10 percent) year after the vote, the prices in affected municipalities are still below the synthetic control's prices, however, the effect is not significant anymore. Hence, the effect seems to be slightly stronger in these high-amenity places in years 2014 to 2016, but prices seem to recover more sustainable than in the benchmark estimations. However, differences to the benchmark estimation are very small and should not be over-interpreted.

Figure 8b shows the effect on less touristic municipalities. The pre-intervention fit is less accurate for this approach (RMSE of CHF 40,000). Post-intervention RMSE is with CHF 103,000 still clearly higher. Post-intervention prices in treatment regions are between 13 percent and 20 percent lower in years 2014 to 2018 and significant on the 95 percent level or more. Hence, less touristic places seem not at all to recover from the price drop in 2014. However, because the pre-intervention fit is less accurate (often outside of the 90 percent CI bounds) this result should be interpreted with caution.

6.4 Case Studies of Most Affected Cantons

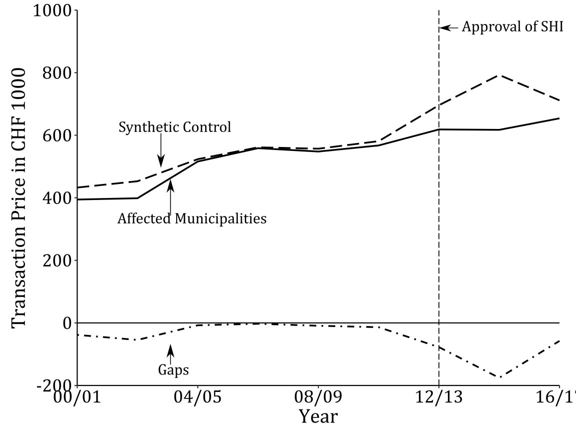
In this Section three case studies of three cantons are discussed, the cantons of Valais, Graubünden and Ticino. These three cantons are located in the Alps and are clearly the most affected regions by the SHI. More than 70 percent of all municipalities are affected in the cantons Valais and Graubünden and 60 percent in the canton of Ticino.[20] The absolute number of municipalities affected is as well clearly the highest in these cantons. Therefore, I estimate the effect of the SHI for these cantons separately. Because this approach is considering regions separately, the number of affected municipalities with at least one observation in each year is too small. Therefore, the transactions of two years are pooled in order to increase the number of municipalities per region.[21]

In opposite to the benchmark estimations or the other case studies, there is a negative effect in the first two years after the vote in the Canton of Valais. The price of affected municipalities is 12.5 percent below the counterfactual price in the years 2012/13. The strongest negative effect is found in 2014/15 with -28.4 percent compared to the control municipalities. Prices seem to recover in 2016/17, where the actual prices are only 8.8 percent below the synthetic control.

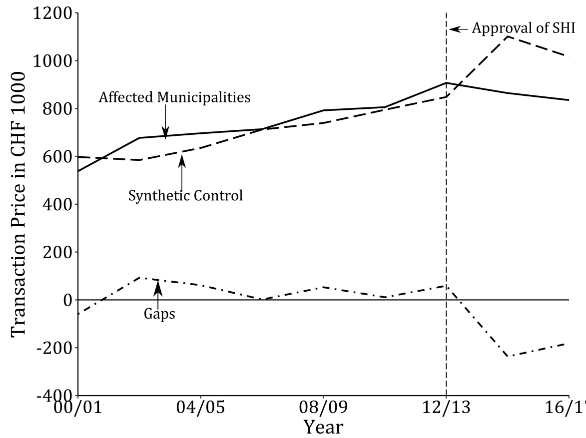
There is no effect visible in the first two years after the vote in Graubünden. However, similarly to the Valais, a tremendous negative effect of -27.4 percent can be found in 2014/15. Unlike the Valais, prices do not recover in 2016/17 with a negative effect of -21.7 percent.
Real estate prices in Ticino are as well negatively affected by the SHI. However, the magni-

[20] The group of affected municipalities in these cantons is consequently larger than the control group. This prevents to compute statistical significance and CI as discussed in Section 4.2.

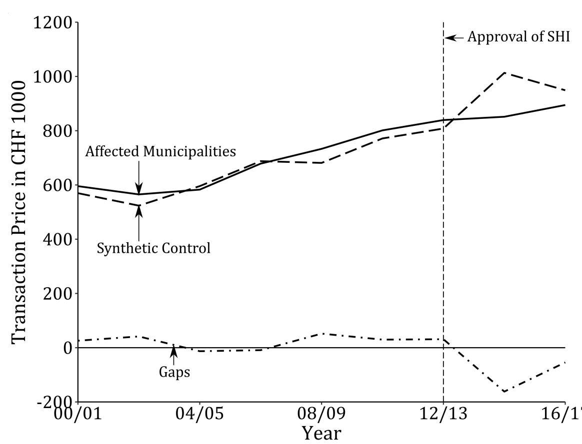
[21] Due to this pooling, all municipalities with at least one observation in every second year is included.



(a) Effect in the canton of Valais



(b) Effect in the canton of Graubünden



(c) Effect in the canton of Ticino

Fig. 9 Case studies for the three most affected cantons Valais, Graubünden and Ticino.
Notes: Transactions of two years were pooled and the outcome of 2006/07 is included.

tude of the effect is smaller than in Valais and Graubünden. Again, there is no effect in the first two years after vote, but a strong negative of -19 percent in 2014/15. Prices recover in 2016/17 with a negative effect of -6 percent.

In summary, the two most affected cantons, Valais and Graubünden, experience a tremendous negative price effect of about -28 percent in 2014/15. The magnitude of this effect is greater than the magnitude found in benchmark results including all regions. The Ticino is as well affected negatively, however, the effect in Ticino is comparable to the benchmark effect including all regions.

7 Evidence for Impact Channels

The results show that the SHI caused a drastic decrease in prices. This decrease in prices can either be explained by an extension in housing supply *or* a fall in demand in affected municipalities. In Section 3, different mechanisms through which the SHI might cause such an extension in supply or a fall in demand are discussed. While a boost in supply can only be explained by a one-time glut of last-minute construction permits, there are two different channels that might cause a fall in demand – adverse effects on local economies and a lock-in effect. In this section, I examine whether there is evidence for these three impact channels. This exercise represents an attempt to narrow down whether the different impact channels exist, but it cannot draw definitive conclusions.

7.1 Impact Channel: Increase in Housing Supply

As discussed in Section 3, it is possible that a one-time glut of newly build homes might have caused the drop in real estate prices in affected municipalities. This would as well explain the two year delay of the effect after the intervention, because it took time until the homes permitted right after the vote in 2012 were built and came on the market. According to this argument, there must be an increase in housing supply in 2014 and afterward in affected municipalities relative to the synthetic control municipalities. I test this argument by running the same synthetic control procedure as above with housing stock instead of prices as outcome variable. The result can be found in Figure 11 in Appendix E. It is clearly visible that the housing stock development of the counterfactual municipalities is not significantly different from the development in the affected municipalities. Compared to the control housing stock, the SHI rather caused a decrease of the housing stock in affected municipalities. Hence, there is no evidence for the argument that a one-time increase in housing supply caused the drop in prices. Consequently, the SHI seems to affect housing prices via a drop in demand.

7.2 Impact Channel: Adverse Effects on Local Economies

We discussed indirect channels that might have caused a drop in housing demand in affected municipalities. One reason that can explain this drop are adverse effects on local economies. The SHI was a setback for the locally very vital construction industry and the SHI prevents the use of second homes or free construction land to cross-subsidize (see Section 3 for more details). Such adverse effects on local economies should be reflected in the unemployment rates of affected municipalities. Therefore, I test whether the SHI had an effect on local unemployment rates using the synthetic control method. In order to do that I construct an unemployment rate on municipality level by dividing the number of unemployed by the population of each municipality.[22] The data on number of unemployed by municipality is collected by the SECO and available from 2004 onwards. The SECO strongly recommends to drop municipalities, if the standard deviation of unemployed is greater than a quarter of the average number of unemployed.[23] Following this procedure, 58 percent of all municipalities in Switzerland need to be dropped. In our sample, only 24 affected municipalities and 445 control municipalities remain in the data.

So far I used predictors of real estate prices in order to construct the synthetic control. However, these real estate characteristics are poor predictors for unemployment rates. Further, many important predictors of unemployment are not available on municipality level. Therefore, I compute the synthetic control of the unemployment rate slightly different than the synthetic control of real estate related outcomes. Following Doudchenko and Imbens (2016) I run constrained regressions in order to compute the synthetic control. Constrained regressions are a special case of the synthetic control method, where the predictor is the full vector of pre-intervention outcomes.[24]

Results are shown in Figure 12 in Appendix E. While the pre-intervention fit is very good (RMSE=0.12), the unemployment rate is consistently higher in affected municipalities after the intervention: In the first four years after the intervention (2012 to 2015) the unemployment rate was 9 to 11 percent (0.3 percentage points) higher in affected municipalities compared to the synthetic control. However, these differences in the unemployment rate are not significant. In 2017 and 2018, the unemployment rate is significantly higher in affected municipalities (13 to 14 percent). The post-intervention RMSE is with 0.32 2.8 times

[22]Since there is no data on the working population on municipality level, I divide the number of unemployed by the municipality's population. Population data on municipality level is not available from 2001 to 2006. Therefore, I impute population by municipality for the years 2004 to 2006 by linear interpolation.

[23]If the standard deviation is too big relative to the mean, there is too much noise in order to be able to interpret the change of unemployed.

[24]In this special case of the synthetic control method, the diagonal matrix $\mathbf{V}$ is a N x N identity matrix and consequently $\underset{W \in \omega}{min}(\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})'(\mathbf{X}_1 - \mathbf{X}_0\mathbf{W})$ must be solved, where $\mathbf{X}_1$ and $\mathbf{X}_0$ contain only the pre-intervention outcome of the treatment municipality in $\mathbf{X}_1$ and of the control municipalities in $\mathbf{X}_0$. See Section 4 for more information.

greater than in the pre-intervention period, which results in an overall p-value of 0.11 based on the RMSE-ratio distribution of 10,000 placebo iterations. Although gaps in unemployment rates are just above of the conventional 0.1-threshold for statistical significance, the results still indicate that unemployment is likely to have increased as a result of the SHI and that local economic activity has cooled down. I interpret this as suggestive evidence that the decrease in real estate prices was partially driven by adverse effects on local economies. As a robustness test, I run the same routine for the full sample (not shown), i.e. I do not exclude municipalities with a standard deviation greater or equal to a quarter of the average of the unemployment rate. The pattern remains similar to the result based on the restricted sample: Unemployment rate is between 7 and 13 percent higher in affected municipalities in the first four years after the intervention. However, these differences are only significant in year 2012. Unemployment rates in 2016 and later are very similar for both groups.

7.3 Impact Channel: Legal Uncertainty and the Lock-In Effect

Another impact channel through which the SHI might cause a fall in prices is the legal uncertainty and a general lock-in effect. As discussed in Section 3, the final law was agreed upon in 2015 and implemented in 2016. While it is difficult to quantify this legal uncertainty directly, it might have caused a lock-in effect, in which the quality and state of properties sold decreased. Further, the SHI might as well have caused a general lock-in effect irrespective of the legal uncertainty with similar consequences (see Section 3). I examine the effect of the SHI on the quality and condition of the houses sold to show whether there is evidence for the existence of the lock-in impact channel. If the quality and condition of the houses decreases due to the SHI, this is an indication that the channel exists.[25]

In this section, I run the SCM as in the benchmark estimations using quality and state of homes as outcome variables. Figure 13a shows the results for the quality of homes. Although the pre-intervention fit seems to be good, CI bands are very narrow and the quality of property sold is significantly lower in affected municipalities as of 2006. Therefore, the synthetic control might not be a reliable counterfactual in this case. The quality of houses remains lower in affected municipalities than in control municipalities in the post-intervention period and the gaps are larger in the post-intervention period compared to the 2006–2011 period. Further, the post-intervention RMSE is with 0.22 about 2.4 larger than in the pre-intervention period with 0.09 resulting in a p-value of 0.03 based on 10,000 placebo iterations. Although these findings suggest that there might be a decrease in quality of homes, these results should be interpreted wit caution since the synthetic control is not able

[25]Note that the SCM minimizes the pre-intervention distance metric of real estate characteristics. Hence, these characteristics used in the SCM are pre-determined and do not respond to the SHI. This is important since covariates that respond to the treatment are likely to be *bad controls*.

to replicate affected municipalities in the 2006–2011 period.

Results for the state of real estate sold are presented in Figure 13b. The synthetic control fits the original housing state well in the pre-intervention with the exception of the years 2006 and 2011. The state of real estate sold in the post-intervention period is significantly lower in affected municipalities compared to control municipalities – especially in the years 2013 to 2015. The gaps in 2013 are for example two to three times larger than in 2011. In 2016, the difference is not significant anymore, however, the fact that the gap widens again in 2017 and 2018 indicates that the lock-in effect might prevail as well for later years. A similar pattern is observed in Figure 13a where the quality of homes sold in affected municipalities remains significantly lower in affected municipalities even after the final law was implemented. This may be because second home owners are aware that it will be almost impossible to re-buy a second home in affected municipalities in the future due to the supply freeze and are therefore more reluctant to sell their second homes. Hence, only those in need tend so sell their home and therefore, quality might be lower (see Section 3).

Summing up, there is no evidence for a one-time boost in supply that might explain the drop in prices in the aftermath of the SHI. Thus, the falling prices must take place through a fall in demand. The evidence for the adverse effect on local economies and the lock-in effect are not clear cut and must be interpreted with caution. However, results point toward the existence of both channels. Hence, it is likely that the decrease in prices caused by the SHI realized through a cooling down of local economic activity and a lock-in effect.

8 Conclusion

I estimate the causal effect of a drastic second home construction ban in Switzerland by exploiting a quasi-experimental setup. First, I show that simple DD estimations are not suitable for the context and, therefore, apply the synthetic control method in an innovative way. Results show that there was no short-run effect on real estate prices in the first two years after the intervention (2012 to 2013). However, I found a sharp decrease in prices of -19 percent in the third year, -10 percent in the fourth year and -18 percent in the fifth year after the vote. These results are robust as several checks show. While it is not possible to conclusively pin down the impact channel, data points toward an adverse effect on local economies and a lock-in effect that caused the drop in prices.

The goals of the regulation were to stop splinter development and keep housing affordable for the local population. Estimations show that the SHI had no significant effect on housing supply. However, housing supply was (insignificantly) lower in affected regions compared to the synthetic control, which indicates that the intervention might have contributed to a decrease of urban sprawl. Moreover, estimates clearly show that housing prices decreased in affected municipalities. However, this came at a price: An economic drawback in affected regions as well as a lock-in effect are the probable reasons for the drop in prices. This adverse effect on local economies is likely to have increased local unemployment. Furthermore, local land owners suffer a strong devaluation of their land, because it cannot be used for the construction of second homes anymore. Hence, housing might have become more affordable for locals, however, the devaluation of the land price and economic drawback on these local economies are unintended consequences which harm the local population. Thus, in the light of the global tendency to regulate the second home market this paper shows that regulations should be made more flexible to take account of regional peculiarities and contexts. Drastic regulations may often exhibit unwanted effects as it is the case with the SHI.

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A Appendix: Price Trends in Affected and Unaffected Municipalities

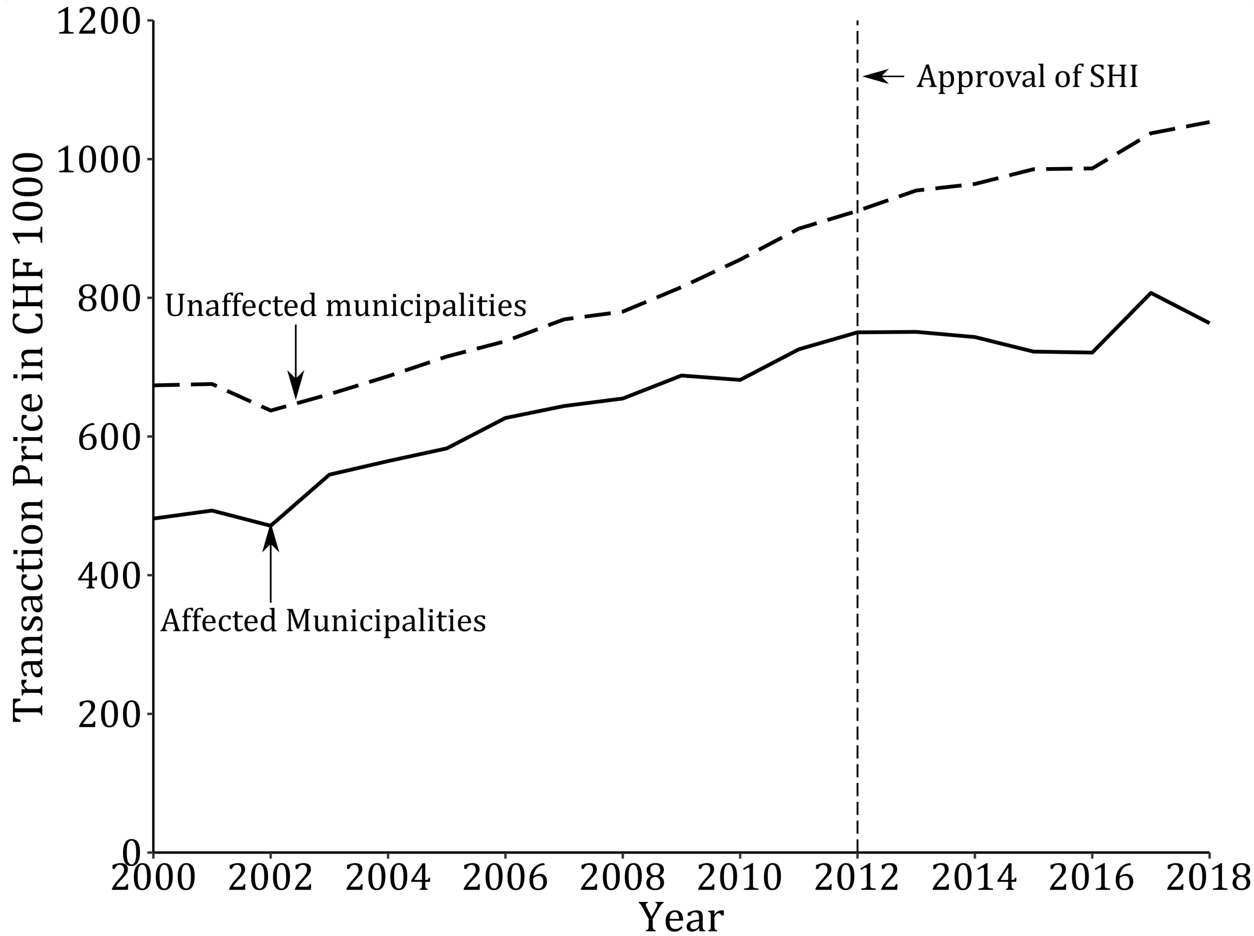


Fig. 10 Trends in transaction prices: Transaction prices in affected vs. transaction prices in unaffected municipalities

B Appendix: Placebo Difference-in-Differences

In Tables 2 and 3, the results of placebo interventions are presented. Table 2 is based on transaction-level data, i.e. every single real estate transaction is an observation. The results of Table 2 are estimated with the following regression equation:

$$Y_{imcgt} = \alpha + \gamma Treat_g + \lambda_t + \delta(Treat_g * Post_t) + X'_{imcgt}\beta + \mu_c + \epsilon_{imcgt}, \tag{7}$$

where Y is the transaction price of the property sold in year t and transaction i, taking place in municipality m and canton c. The municipality either belongs to the control ($g = 0$) or the treatment group ($g = 1$). $Treat_g$ is a dummy taking the value 1 if transaction takes place in an affected municipality and zero otherwise, λ_t is a year dummy, $Post_t$ is a dummy which switches on for post-treatment years, X_{imcgt} is a set of time-variant control variables (see Table 2) and μ_c are canton fixed effects. The coefficient of interest is δ indicating the effect of the (placebo) treatment. In all estimations in Tables 2 and 3 only years 2000 to 2011 are considered, i.e. all years before the actual SHI took place. E.g. for the placebo intervention in 2006 (i.e. column 1 in Table 2), $Post_t$ switches on for years 2006 to 2011. Because there was no actual intervention until 2012, there should be no effect caused by this placebo intervention. However, Table 2 reports a significant effect for every placebo intervention.

In Table 3, data is aggregated by year and municipality. Thus, instead of observations on transaction-level the observations are on municipality level. As in the SCM applied in the main analysis of the paper, only municipalities with at least one transaction in every year is considered. Hence, the results in Table 3 are based on the same data as in the SCM approach. The estimation equation remains really similar:

$$Y_{mcgt} = \alpha + \gamma Treat_g + \lambda_t + \delta(Treat_g * Post_t) + X'_{mcgt}\beta + \mu_c + \epsilon_{mcgt}, \tag{8}$$

The only changes are that the price level Y_{mcgt} and the time-variant controls X_{mcgt} are aggregated on municipality level. Again, effects of placebo interventions in Table 3 are significant.

Table 2 Difference-in-differences placebo tests: transaction-level data

	Dependent variable: Log of price				
	Placebo 06[a]	Placebo 07[a]	Placebo 08[a]	Placebo 09[a]	Placebo 10[a]
	(1)	(2)	(3)	(4)	(5)
Treatment	−0.161***	−0.154***	−0.151***	−0.150***	−0.148***
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Treatment x Post	0.032***	0.022***	0.019***	0.016**	0.014*
	(0.006)	(0.006)	(0.007)	(0.007)	(0.008)
Constant	11.098***	11.097***	11.096***	11.096***	11.096***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Year and Canton fixed effects	Yes	Yes	Yes	Yes	Yes
Time-variant contr.[b]	Yes	Yes	Yes	Yes	Yes
Observations	173,280	173,280	173,280	173,280	173,280
Adjusted R^2	0.64	0.64	0.64	0.64	0.64

Notes: Only pre-intervention years 2000 to 2011 are included.

a Each model indicates the year of the placebo intervention. I.e. the dummy *Post* takes value 1 after the placebo intervention and zero otherwise.

b Yearly transactions, number of rooms, plumbing units and garages, quality, state and micro-location of the property as well as second home rate.

*p<0.1; **p<0.05; ***p<0.01; standard errors clustered at municipality-year level in parentheses.

Table 3 Difference-in-differences placebo tests: municipality-level data

	Dependent variable: Log of price				
	Placebo 06[a]	Placebo 07[a]	Placebo 08[a]	Placebo 09[a]	Placebo 10[a]
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.188^{***}	-0.177^{***}	-0.170^{***}	-0.160^{***}	-0.153^{***}
	(0.022)	(0.021)	(0.021)	(0.021)	(0.020)
Treatment x Post	0.096^{***}	0.087^{***}	0.090^{***}	0.080^{***}	0.076^{***}
	(0.019)	(0.020)	(0.021)	(0.022)	(0.026)
Constant	10.807^{***}	10.806^{***}	10.800^{***}	10.796^{***}	10.793^{***}
	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)
Year and Canton fixed effects	Yes	Yes	Yes	Yes	Yes
Time-variant contr.[b]	Yes	Yes	Yes	Yes	Yes
Observations	7,356	7,356	7,356	7,356	7,356
Adjusted R^2	0.70	0.70	0.70	0.70	0.70

Notes: Only pre-intervention years 2000 to 2011 are included. Transactions are aggregated by year and municipality and only municipalities with at least one transaction in every year are considered.
a Each model indicates the year of the placebo intervention. I.e. the dummy *Post* takes value 1 after the placebo intervention and zero otherwise.
b Yearly transactions, number of rooms, plumbing units and garages, quality, state and micro-location of the property as well as second home rate.
*p<0.1; **p<0.05; ***p<0.01; standard errors clustered at canton-year level in parentheses.

C Appendix: Group-Specific Time Trends

Angrist and Pischke (2008) propose to allow for group-specific time trends in order to check the validity of the common trend assumption. It is discouraging, if these group-specific time trends are significantly different from each other or if the estimated effects of interests are changed by the inclusion of group-specific time trends. Therefore, a group-specific time trend is included in the regression Equations (7) and (8), while all other variables remain unchanged:

$$Y_{imcgt} = \alpha + \gamma Treat_g + \lambda_t + \rho(Treat_g * t * Pre_t) + \delta(Treat_g * Post_t) + X'_{imcgt}\beta + \mu_c + \epsilon_{imcgt}. \quad (9)$$

Note that t is the time trend and Pre_t is an indicator variable for the pre-intervention period, i.e. Pre_t takes the value 1 for per-intervention years and the value 0 for post-intervention years. The idea of the interaction $Treat_g * t * Pre_t$ is to include only pre-intervention group-specific time trends,[26] which should be the same according to the parallel trends assumption. Again, the estimations are conducted with transaction-level (Table 4) and municipality-level data (Table 5). Data of all years (i.e. 2000 to 2018) is included in these estimations. Results show that the group-specific trends are significant and that allowing for group-specific time trends changes the estimated effect of the SHI tremendously for the estimation on transaction-level as well as on municipality-level.[27]

[26]Note that the time trend t starts from 1 for the year 2000 to 19 for year 2018.

[27]Note, that time-variant covariates included in the regression might be *bad controls*, if they respond as well to the SHI. Since this is likely, transaction-level regressions are run with and without these time-variant covariates, while municipality-level regressions are run with time-variant covariates or pre-determined covariates. Pre-determined covariates are the pre-intervention averages of time-variant covariates.

Table 4 Difference-in-differences with group-specific time trends: transaction-level data

	Dependent variable: Log of price			
	(1)	(2)	(3)	(4)
Treatment	-0.140^{***}	-0.178^{***}	-0.245^{***}	-0.290^{***}
	(0.005)	(0.008)	(0.008)	(0.012)
Treatment x Year x Pre		0.006^{***}		0.007^{***}
		(0.001)		(0.001)
Treatment x Post	-0.019^{***}	0.019^{**}	-0.038^{***}	0.007
	(0.006)	(0.008)	(0.009)	(0.012)
Constant	11.126^{***}	11.130^{***}	13.385^{***}	13.388^{***}
	(0.006)	(0.006)	(0.006)	(0.006)
Year and Canton fixed effects	Yes	Yes	Yes	Yes
Time-variant contr.[c]	Yes	Yes	No	No
Observations	243,824	243,824	243,824	243,824
Adjusted R^2	0.66	0.66	0.21	0.21

Notes: Data of all years (2000 to 2018) is included.
a Yearly transactions, number of rooms, plumbing units and garages, quality, state and micro-location of the property as well as second home rate.
* p<0.10, ** p<0.05, *** p<0.01; standard errors clustered at municipality-year level in parentheses.

Table 5 Difference-in-differences with group-specific time trends: municipality-level data

	Dependent variable: Log of price			
	(1)	(2)	(3)	(4)
Treatment	-0.163^{***}	-0.272^{***}	-0.059^{***}	-0.148^{***}
	(0.018)	(0.025)	(0.020)	(0.027)
Treatment x Year x Pre		0.017^{***}		0.014^{***}
		(0.003)		(0.003)
Treatment x Post	0.034^{**}	0.145^{***}	-0.003	0.086^{***}
	(0.016)	(0.024)	(0.017)	(0.025)
Constant	10.910^{***}	10.931^{***}	9.876^{***}	9.886^{***}
	(0.035)	(0.035)	(0.076)	(0.076)
Year and Canton fixed effects	Yes	Yes	Yes	Yes
Time-variant contr.[a]	Yes	Yes	No	No
Pre-determ. contr.[b]	No	No	Yes	Yes
Observations	11,647	11,647	11,647	11,647
Adjusted R^2	0.72	0.72	0.68	0.68

Notes: Data of all years (2000 to 2018) is included.
a Yearly transactions, number of rooms, plumbing units and garages, quality, state and micro-location of the property as well as second home rate.
b Pre-determined controls are pre-intervention averages of the time-variant controls.
* p<0.10, ** p<0.05, *** p<0.01; standard errors clustered at canton-year level in parentheses.

D Appendix: Granger Causality Testing

A closely related possibility to check the identification assumption of the DD approach is to test for causality in the spirit of Granger (Angrist and Pischke, 2008). Granger causality tests check whether a past intervention predicts an outcome, while a future intervention does not. I.e. lags and leads of the intervention are included in order to estimate post-treatment effects resp. anticipatory effects. Effects of future policy changes should not matter for the contemporaneous outcome. Leads and lags are included in the regression equation in order to check for these anticipatory and post-treatment effects (Angrist and Pischke, 2008):

$$\begin{aligned} Y_{imcgt} &= \alpha + \gamma Treat_g + \lambda_t + \\ &\sum_{\tau=0}^{m} \delta_{-\tau}(Treat_g * Post_{t-\tau}) + \sum_{\tau=1}^{q} \delta_{+\tau}(Treat_g * Post_{t+\tau}) + X'_{imcgt}\beta + \mu_c + \epsilon_{imcgt}. \end{aligned} \tag{10}$$

The sums allow for m lags and q leads and again, the equation is estimated with transaction-level and municipality level data.[28] The estimates in Table 6 show significant effects in several of the five years before the SHI vote took place. This pattern does not appear to be consistent with a causal interpretation of the SHI effect on prices.

[28] Note, that time-variant covariates included in the regression might be *bad controls*, if they respond as well to the SHI. Since this is likely, transaction-level regressions are run with and without these time-variant covariates, while municipality-level regressions are run with time-variant covariates or pre-determined covariates. Pre-determined covariates are the pre-intervention averages of time-variant covariates.

Table 6 Difference-in-differences with Granger causality testing

	Dependent variable: Log of price			
	Transaction-level[a]		Municipality-level[b]	
	(1)	(2)	(3)	(4)
Treatment	−0.116***	−0.150***	−0.096***	−0.204***
	(0.006)	(0.006)	(0.021)	(0.019)
Treat. x Dummy 2007	0.056***	0.028**	0.081**	0.062*
	(0.017)	(0.012)	(0.036)	(0.037)
Treat. x Dummy 2008	0.031	0.028**	0.107***	0.106**
	(0.019)	(0.013)	(0.038)	(0.042)
Treat. x Dummy 2009	0.032*	0.026**	0.086**	0.101***
	(0.018)	(0.011)	(0.036)	(0.033)
Treat. x Dummy 2010	0.006	0.019*	0.083**	0.116***
	(0.018)	(0.011)	(0.037)	(0.038)
Treat. x Dummy 2011	0.027	0.031***	0.087**	0.109***
	(0.018)	(0.012)	(0.035)	(0.035)
Treat. x Dummy 2012	0.026	0.017	0.098**	0.113***
	(0.018)	(0.012)	(0.041)	(0.036)
Treat. x Dummy 2013	0.002	0.016	0.110***	0.114***
	(0.019)	(0.012)	(0.033)	(0.033)
Treat. x Dummy 2014	−0.010	0.019	0.063	0.121***
	(0.019)	(0.012)	(0.040)	(0.037)
Treat. x Dummy 2015	−0.070***	−0.026*	0.003	0.092**
	(0.021)	(0.014)	(0.039)	(0.038)
Treat. x Dummy 2016	−0.069***	−0.015	−0.009	0.082**
	(0.021)	(0.015)	(0.040)	(0.037)
Treat. x Dummy 2017	−0.042**	−0.028**	0.023	0.041
	(0.021)	(0.014)	(0.038)	(0.037)
Treat. x Dummy 2018	−0.080***	−0.078***	−0.051	−0.029
	(0.022)	(0.015)	(0.040)	(0.040)
Year and Canton FE	Yes	Yes	Yes	Yes
Time-variant contr.[c]	No	Yes	No	Yes
Pre-determ. contr.[d]	No	No	Yes	No
Observations	243,824	243,824	11,647	11,647
Adjusted R^2	0.21	0.66	0.68	0.72

Notes: These estimations include leads and lags of the intervention. Data of all years (2000 to 2018) is included.
a Observations are on transaction level.
b Observations are on municipality level. I.e. transactions are aggregated by year and municipality
c Yearly transactions, number of rooms, plumbing units and garages, quality, state and micro-location of the property as well as second home rate.
d Pre-determined controls are pre-intervention averages of the time-variant controls.
*p<0.1; **p<0.05; ***p<0.01; standard errors clustered at municipality-year resp. canton-year level in parentheses.

E Appendix: Impact Channels

E.1 Impact Channel: Increase in Housing Supply

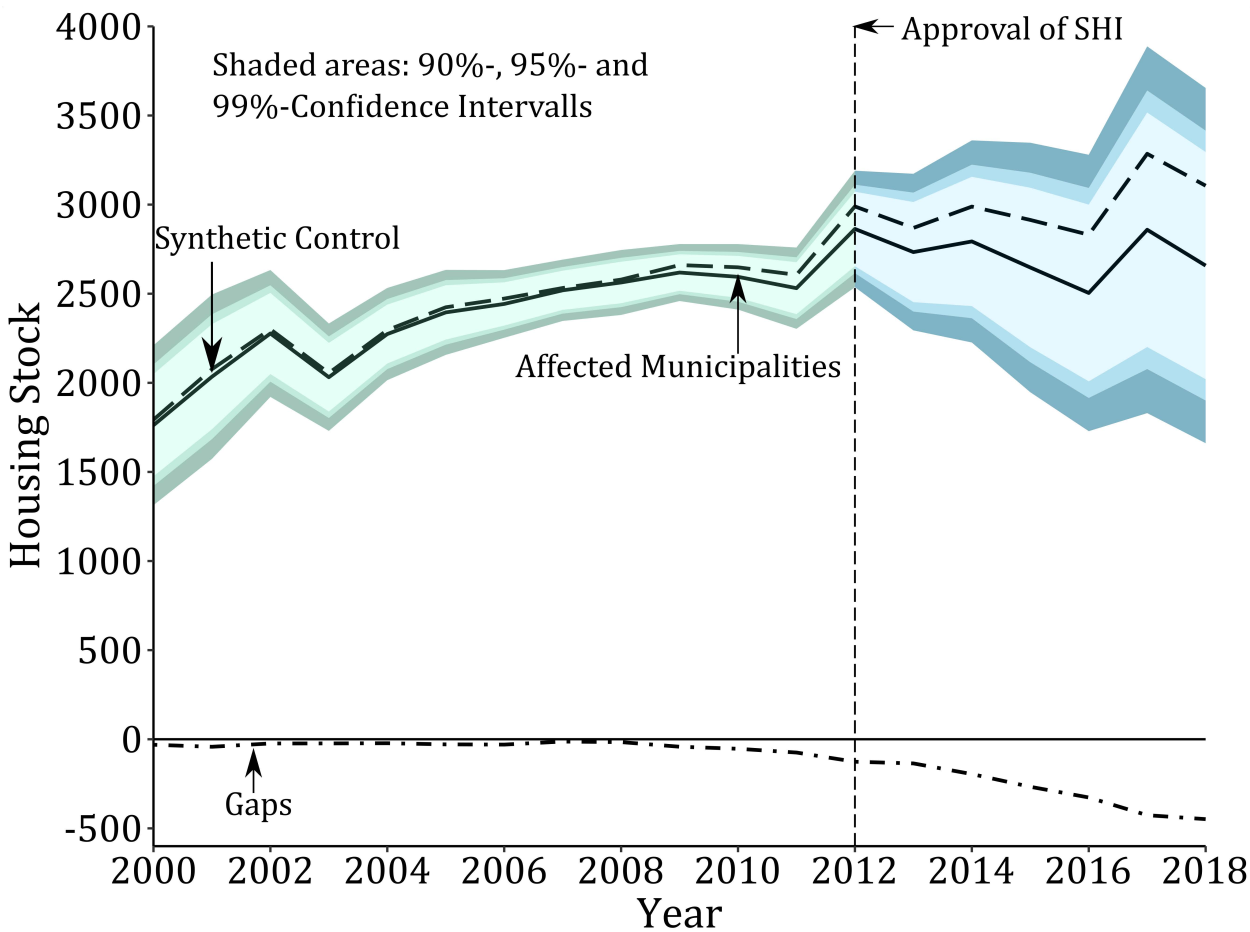


Fig. 11 Effect on housing supply (in dwelling units).
Notes: CI are based on 10,000 placebo runs and the outcome of 2007 is included.

E.2 Impact Channel: Adverse Effects on Local Economies

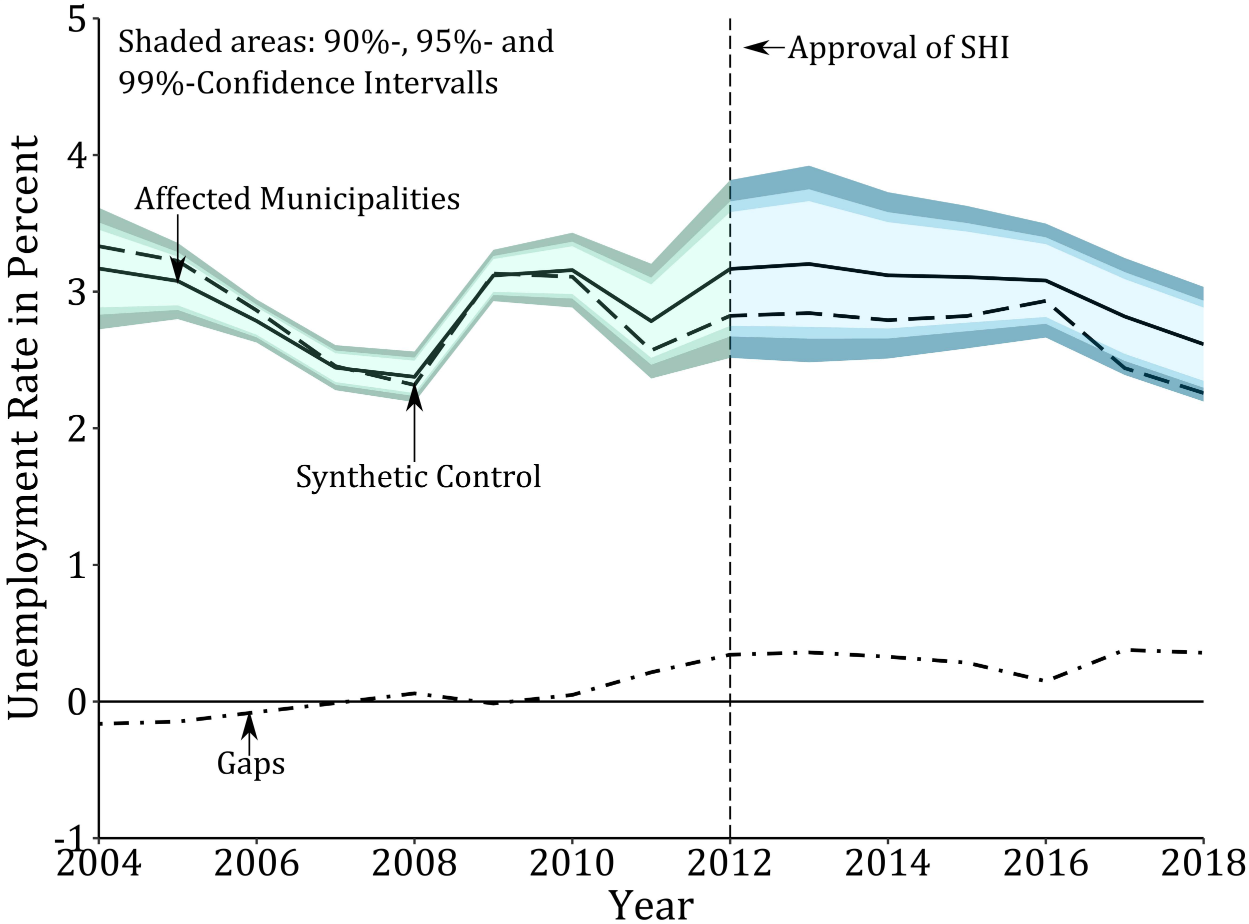
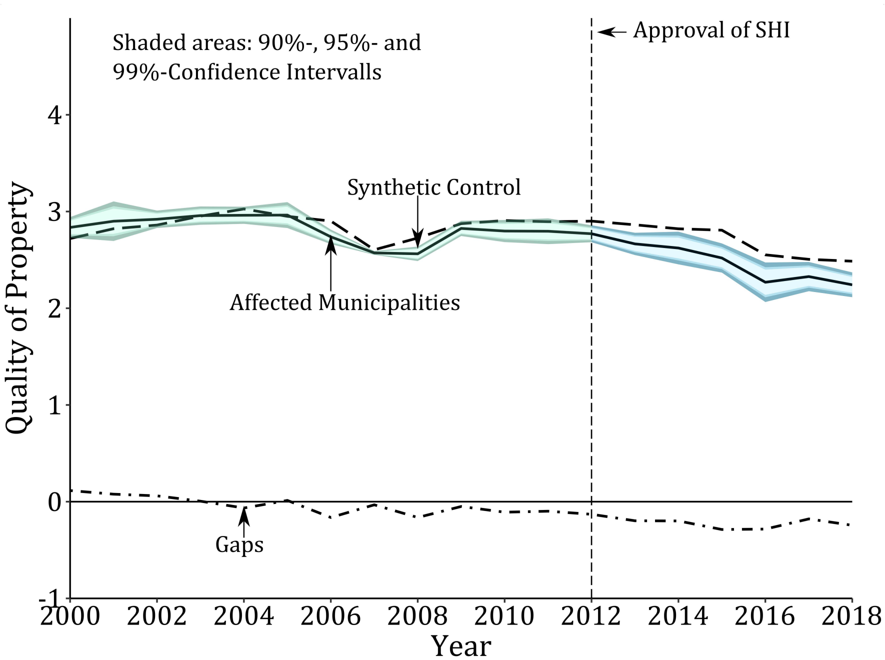
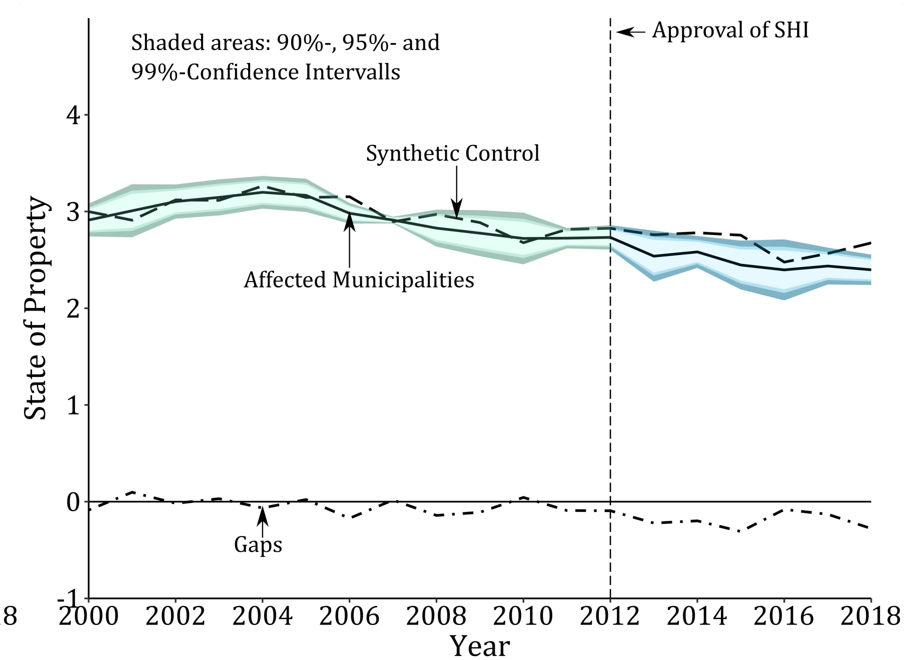


Fig. 12 Effect on unemployment.
Notes: CI are based on 10,000 placebo runs and only the pre-intervention outcome variable is used as predictor (constrained regressions method). Only municipalities with a standard deviation of unemployed smaller than a quarter of the average of unemployed (24 treated municipalities and 445 control municipalities) included.

E.3 Impact Channel: Legal Uncertainty and Lock-In Effect



(a) Effect on quality of properties.



(b) Effect on state of properties.

Fig. 13 Effect on property characteristics.

Notes: CI are based on 10,000 placebo runs and outcome of 2007 is included. Quality resp. state of the property are used as outcome variables, while predictors are the same as in benchmark estimations including transaction prices.