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Abstract

The Swiss debt brake is widely appreciated as one of the most rationally designed fiscal rules in the world and was thus also discussed as blueprint in the debates about fiscal rules in Germany, the European Union member states and Israel. However, evidence that this rule really contributes to sound federal finances does not exist yet. We investigate the effectiveness of the Swiss debt brake by employing the Synthetic Control Method. We find that the introduction of this fiscal rule improved the budget balance by about 3.6 percentage points on average in a post-intervention period covering five years.

JEL-Codes: H110, H600.

Keywords: Swiss debt brake, cyclically adjusted budget balance, government debt, synthetic control method.

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1 Prelude

On December 2, 2001 Swiss citizens voted in favour of the introduction of a fiscal rule at the national level. After the debt to GDP ratio of the Swiss federal government increased from 13 percent in 1991 to 25 percent in 1997, government, parliament and the general public wanted to break this trend and start with a consolidation of the federal budget. This is in line with the deficit bias of political decision-makers in developed economies already emphasized in the survey by Alesina and Perotti (1995). In addition, the institutional logic of the budgetary process at the Swiss federal level induces a deficit bias. Tax rate increases require a change of the Swiss constitution whereas an increase in government expenditure only needs a simple majority of representatives (Geier, 2011). As a result, spending increases were rather financed by budget deficits than tax increases because of political reasons and institutional restraints. Switzerland introduced the federal debt rule to abolish deficit biases and finally arrive at sound federal finances.

For the new debt rule in Germany, introduced for similar reasons, the Federal Ministry of Finance recommended a budget that is 'close-to-balance'. Due to the difficulty of precisely defining public investment spending the German debt brake does not include any investment orientation or golden rule. Borrowing for non-cyclical reasons is however allowed for in the size of 0.35 percent of GDP.[1] Borrowing for cyclical reasons is symmetric under the Ministry's proposal since the proposal includes a compensation account which is debited if revenues fall below expenditures. Similar to the Swiss rule the compensation account is armed with an upper limit. So a record of the compensation account larger than the limit must be cut back under the limit (Baumann et al., 2008; BMF, 2008; Kastrop and Snelting, 2008).[2]

In 2011 the European Union member states agreed on the European Fiscal Compact in response to the European debt crisis. Under this treaty the respective states consented to implement a balanced budget rule at the general government level. Although it is the member states' responsibility to decide on the details of their fiscal rule the compact requires some features following the Swiss balanced budget rule and the German debt brake. So it calls for a balanced budget and limits the structural deficit to 0.5 percent of GDP. The national rules must be accompanied by an automatic correction mechanism that becomes effective in the case of non-compliance with the rule. In line with the compact, Austria, Cyprus, Finland, France, Ireland, Italy, Luxembourg, Latvia, Portugal and Spain introduced a balanced budget rule that allows for a small structural deficit in general. In Austria the rule is accompanied by a compensation account of German

[1] The German debt brake applies to the federal level and the states. Borrowing for structural reasons is allowed for the federal level only.

[2] However, the German fiscal rule differs from the Swiss one e.g. in terms of the estimation of the output gap and the exceptions from the rule. For a lengthy description of the enacted version of the German fiscal rule, see Feld (2010).

style. Spain and Portugal implemented an automatic correction mechanism in terms of a financial plan that corrects the deviations. In Latvia the structural deficit of the subsequent years automatically gets reduced in the case of non-compliance (Burret and Schnellenbach, 2013).

In Israel the experience with fiscal rules indicates that they were hardly obeyed in the past. For Israel it was important to obtain a fiscal rule that directly impinges on spending trends and that directly punishes systematic deviations from the rule. Debrun et al. (2008) value the compensation account to be helpful in a way that systematic deviations from the fiscal target do not fall into oblivion. Finally, they recommend an expenditure rule for Israel including a feedback mechanism that adjusts the expenditure growth ceiling according to the long-run debt target.

Despite this political popularity, it is contested whether fiscal rules actually contribute to fiscal sustainability. On the one hand, a debt reducing effect of fiscal rules is supported by some empirical studies. Bohn and Inman (1996) consider the budget surplus of the general fund of 47 U.S. states covering the 1970-91 time span and find a clear positive and statistically significant effect for strong rules, especially the no-carry-over deficit rule. This result for the U.S. states is confirmed by Alesina and Bayoumi (1996) and Hou and Smith (2010). The former study reports supportive evidence also for a broader measure of the budget surplus. Hou and Smith (2010), in constrast, find that narrow budget measures are better constrained by fiscal rules than broad ones. Additionally they find that fiscal rules are more effective if they contain a technical requirement instead of charging political actors. In focusing on constitutional limits on guaranteed bond indebtedness in the U.S. states for the 1961-90 period, Kiewiet and Szakaly (1996) find that guaranteed debt is lower when the issuance of bonds is prohibited at all. A qualified majority in parliament or revenue-based limitations are, in contrast, not helpful in reducing the debt level.

On the other hand, there is also some evidence that calls the effectiveness of formal fiscal rules into question. In their 1980 cross-section analysis covering the U.S. states Abrams and Dougan (1986) do not find a robust significant effect on government spending if borrowing is allowed for. Although using a panel structure of the 1961-89 period and focusing on debt growth to deal with the nonstationarity of debt data, Clingermayer and Wood (1995) cannot find a significant effect of formal debt constraints in the U.S. states. Instead they confirm that limitations of revenue or spending are not helpful in reducing debt growth. In also considering the U.S. states, von Hagen (1991) does not find a significant effect of a debt rule or a strong balanced budget rule on debt per capita or the debt ratio based on a test for equal means. However, he finds that the median-debt-per-capita is lower in the country group with a debt rule or a strong balanced budget rule. Eventually, the study of Hou and Smith (2009) can also be interpreted as questioning the effectiveness of formal fiscal rules. They find that the no-carry-over deficit rule is obeyed as informal rule even if it is not fixed by law. This basically means that politicians and

eventually voters behave in a fiscally conservative way.

For Switzerland there is evidence for the 1980-98 time period, the 1986-97 time span and the 1980 to 2011 period that strong statutory fiscal constraints significantly reduce deficits and debt at the cantonal level (Burret and Feld, 2014, 2016; Feld and Kirchgaessner, 2001, 2008; Schaltegger, 2002). Regarding the local level fiscal rules reduce deficits. Beyond also providing evidence for the effectiveness of fiscal rules Luechinger and Schaltegger (2013) show that fiscal rules improve the accuracy of budget projections. The authors interpret this result as a decrease in strategic behaviour during the budget process. Using financial market data from 1981 to 2007, Feld et al. (2013) report that cantonal fiscal rules reduce spreads, although the re-establishment of a no-bailout regime regarding local jurisdictions has a quantitatively larger negative effect on cantonal spreads. However, there is no evidence yet on the effects of the Swiss federal debt brake on federal finances.

The literature could be summarised such that fiscal rules are the more effective the more restrictive the design of the rule is. That is, a strong balanced budget rule works better than a revenue rule or a debt rule, and a technical rule works better than a political one. Since the Swiss federal fiscal rule meets these requirements it can be assumed that it might work and the premature praise seems justifiable. Admittedly, this does not provide evidence that the rule is indeed effective.

Thus, this is the first paper that analyses the effects of the Swiss federal debt rule. As the Swiss debt brake is legally effective only since a dozen years, standard time series methods cannot be used. While the 2003 budget was the first under the debt brake, the government had to apply a transitional period that lasted until 2005. Therefore, we employ the Synthetic Control Method to analyse the effect of the Swiss debt rule's introduction on the cyclically-adjusted budget balance, the central government debt ratio as well as the general government debt ratio. Aside other results, we find an improvement of the budget balance by about 3.6 percentage points in a post-intervention period covering five years.

The paper proceeds as follows: section 2 explains the mechanics of the Swiss fiscal rule, surveys the literature that discusses the features of this rule and deals with the start-up difficulties of the years 2003 to 2005. The next section then outlines the working of the empirical method we use. While section 4 reports the data, section 5 provides the results. Section 6 concludes.

2 The Design of the Swiss Balanced Budget Rule

The Swiss fiscal rule basically consists of a ceiling of total central government expenditures, i.e., expenditures in the next fiscal year must follow the predicted revenues for that fiscal year. Additionally, predicted revenues are multiplied by a factor that corrects for the position within the budget cycle. The basic mode of operation can be illustrated by

equation (1)

$$E_t = k \cdot R_t \tag{1}$$

where the expenditure ceiling E for financial year t equals the revenue forecast R for the same year t multiplied by the adjustment factor k. This factor is given by

$$k = \frac{Y_t^T}{Y_t} \tag{2}$$

with Y_t^T being the potential real GDP (trend) and Y_t being the predicted real GDP of the year t. In case of an under-utilisation of capacities, the ratio of potential real GDP over predicted real GDP is larger than one and expenditures can exceed predicted revenues. In times of booms, in contrast, the factor k forces the budget to generate a (yearly) surplus which results in a balanced budget over the cycle. Eventually, the calculation of the factor k relies on an adjusted Hodrick-Prescott filter instead of using a production function approach.

If total expenditures exceed the ceiling, the (additional) deficit is booked in a compensation account. Deficits in this account must be redeemed in the subsequent fiscal years. However, the terms of the amortisation are not specified. If the deficit of the compensation account gets to large, i.e., it exceeds 6 percent of effective expenditures of the last fiscal year, the government must reduce it below 6 percent of expenditures within three years.

Beyond that, the calculation of the expenditure ceiling comprises investment spending but disregards windfall revenues (which therefore do not endanger the rules' stringency) and the social security system. The law also considers the need for extraordinary expenditures. Yet, this is possible only for a finite number of cases which are conclusively stated in the law. Every single case needs the confirmation by the majority of parliament. Since 2010 the rule also covers the extraordinary budget (BV, 2002; FHG, 2006; FHG, 2010; Geier, 2011).

As mentioned the Swiss debt rule was given credit from scientists and practitioners alike. So it is esteemed that the rule targets the deficit instead of debt as the former is under direct control of policy-makers. Furthermore, a balanced budget target is welcomed because a surplus target would certainly raise the desire to spend these surplus amounts in line with short-run political demands instead of using them to unburden future generations. In addition, a balanced budget target can be better understood by the public than a somewhat arbitrary threshold for the debt ratio (Danninger, 2002; Debrun et al., 2008).

Secondly, the rule's way of considering the business cycle is also acknowledged. Instead of demanding the budget to balance 'over the cycle' the rule is based on revenue and GDP forecasts of the next financial year only. Prediction errors which often go along

with medium term forecasts of macroeconomic variables are thus minimised (Danninger, 2002).

The centrepiece of the rule, the compensation account, is explicitly appreciated by Debrun et al. (2008). Thanks to this error correction mechanism the violation of the fiscal rule does not jeopardise the sustainability of public finances which is the ultimate aim of a fiscal rule. Finally, the rule is commended for being enshrined in the constitution and for the fact that the escape clauses are listed conclusively in the law (Danninger, 2002).

However, there is also some criticism. Danninger (2002) points out that the Hodrick-Prescott filter comes up with an end-point bias that is not convincingly solved in the Swiss debt brake. When calculating the adjustment factor k for the fiscal year t the trend-GDP value of that year is always the last value of the trend-GDP time series. The HP-filter formula (not shown) reveals that its first term minimises a deviation of the trend from the real values. Its second term, in contrast, penalises changes in the slope and thus smoothes the trend time series. If a GDP forecast of the year $t+1$ shall not be used in order to avoid prediction errors, this second term of the HP-filter formula cannot be calculated for the year t because a trend-GDP value of the year $t+1$ does not exist. Thus the trend-GDP value is biased towards the real GDP value in year t. To correct for this the Swiss Federal Finance Administration (henceforth: SFFA) uses a modified HP-filter since 2004. Under this modification the smoothing factor λ is multiplied by the factor 1.5 for the penultimate trend-GDP value and is multiplied by factor 3 for the last trend-GDP value. Although the modified HP-filter cannot eliminate the end-point bias it clearly shrinks it (Bruchez, 2003a). An alternative would be to use a production function approach which additionally might be better able to capture the country's economic situation than the HP-filter. However, this approach is based on many parameter assumptions which might be subject to political leverage (Colombier, 2004, 2006). A minor point of criticism related to the filtering procedure is the somewhat arbitrary choice of the smoothing factor $\lambda = 100$. Danninger (2002) argues that there is no theoretical reasoning behind it. The SFFA, however, points to the fact that the choice of the smoothing factor is in line with European Union standards (Colombier, 2006).

The calculation of the expenditure ceiling at least implicitly assumes the GDP-elasticity of revenues to be one and fixed over time. Danninger (2002) notes that this is not necessarily the case. Colombier (2004) admits that some studies report a GDP-elasticity of revenues larger than one but counters that these studies are not relevant as they include revenue categories legally irrelevant to the Swiss debt brake. He concludes that there is no clear evidence of a GDP-elasticity of revenues different from one. An alternative would be to employ a revenue trend instead of revenue forecasts combined with a GDP trend. However, a reliable revenue trend estimation method would be necessary then. If the revenue trend is based on a HP-filtering procedure the end-point bias would still be in

place.

Another point of criticism refers to long-lasting recessions. Redeeming deficits from the compensation account might become problematic under such circumstances because it would take place in a situation of under-utilisation of capacities. Fiscal policy acts in a procyclical way then. Similarly, fiscal policy becomes procyclical in a long-lasting recession because the HP-filter adjusts the GDP trend downwards if the effective GDP decreases over a longer time period. This reduces the output gap and forces the fiscal policy to be more restrictive although the recession is still running (Danninger, 2002).

Finally, it is argued that investment spending will become too low under the rule. Since it is well known that investment spending positively impacts on economic growth it might be helpful or even necessary to foster investment spending. Moreover, politicians tend to cut investment spending when forced to cut overall spending. Yet, the problem with investment spending is its precise definition. There is a danger that consumption spending is declared as investment spending which would undermine the functioning of the rule. This is precisely the reason why the authorities in Germany departed from the golden rule of public finances. The SFFA argues that a special fund might be put in place in the case of a need of investment spending. However, this again leads to a decrease in transparency (Colombier, 2004, 2006).

Before starting the analysis some words are in place regarding the introduction phase of the rule (2003-05). The budget of the year 2003 and the medium-term financial plan 2004-06 were both the first of its kind under the new rule. To calculate the expenditure ceiling, however, the SFFA used the classical HP-filter instead of the modified one at that time. Because of the end-point bias the expenditure ceiling was inappropriately low and the budget ran into a deficit. Moreover, the adjustment factor k does not react very precisely to corrections of the GDP forecast. So at the time of the budget preparation in year $t-1$ only a GDP forecast is available. That is the calculation of the expenditure ceiling is based on this GDP forecast and the respective trend GDP value. If then later that year the GDP forecast is being revised the GDP trend changes due to the relatively large impact of the last trend GDP value on the overall trend. However, the ratio of Y_t^T over Y_t is affected only weakly because both values Y_t^T and Y_t have changed. This was precisely the case in 2003 when the adjustment factor k did not appropriately react to the revision of the GDP forecast (Colombier, 2004). Consequently, the federal government introduced a transition period and debt ratio increased in the years 2003 to 2005 in spite of the introduction of the debt brake (see figure 8 and the discussion below).

3 The SCM Estimator

The Synthetic Control Method (SCM) combines the analysis of case studies with quantitative procedures regarding the approximation of the treated unit's trajectory in the

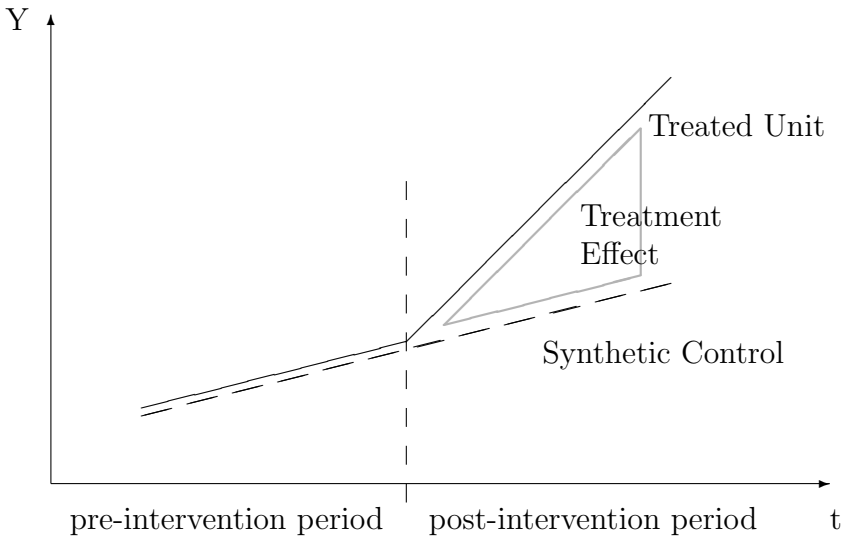

Figure 1: Stylised Representation of the Synthetic Control Method

pre-intervention period as well as the statistical significance of the intervention. Since it allows for the analysis of only one or a few interventions, it is very well-suited to discover the effects of political institutions on policy or economic outcomes. The following description of the method follows Abadie et al. (2010, 2015).

In principle the method depicts the trajectory of an outcome variable for one single unit of interest (treated unit) in the pre-intervention period as well as the post-intervention period. The method then aims at approximating the path of the outcome variable of the treated unit in the pre-intervention period as best as possible with that path of a control group. This control group is called 'synthetic' since it does not consist of only one comparison unit (e.g. one country) but of (the weighted average of) a positive number of units. Eventually the trajectory of the outcome variable of the synthetic control is depicted for both periods. So the path of the control group in the post-period shows how the outcome variable of the treated unit would have evolved without the intervention. The difference between both trajectories in the post-period thus provides the Synthetic Control Estimator. A stylised representation of the method is given in figure 1 where the solid line represents the outcome variable (e.g. the budget balance) of the treated unit and the dashed line represents the outcome variable of the synthetic control group, respectively. The effect of the intervention on the outcome variable is then represented by the grey triangle.

More formally the method is based on a sample of $i + j$ units. While i depicts the treated unit, $j = 1, ..., J$ is a positive number of potential comparison units. All these units together constitute the donor pool. Units that are not in the pool obviously cannot be part of the control group and ultimately the researcher decides on which units are in the donor pool. Time runs from $t = 1, ..., T$ whereas T_{pre} is a positive number of pre-

intervention time periods. T_{post}, in contrast, entails a positive number of post-intervention time periods.[3] Thus it holds that $T = T_{pre} + T_{post}$.

The approximation of the outcome variable of the treated unit is not obtained by simply using the weighted average of the outcome variables of the synthetic control group but it is based on some $k = 1, ..., K$ (economic) characteristics that are predictor variables of the outcome variable.[4] In case of the budget balance this might for instance be the unemployment rate. X_i then is a $(K \times 1)$ vector of these predictor variables for the treated unit in the pre-intervention period. The predictors are averaged over this period. Likewise, the $(K \times \text{J})$ matrix X_J covers the same K (averaged) predictor variables for the same time period but for all J potential comparison units in the donor pool. Since the predictor variables differ in their ability to predict the outcome variable a weight v_k is assigned to every predictor variable k based on a regression.

In a next step the synthetic control is obtained by compiling the $(J \times 1)$ vector of weights $W = (w_1, ..., w_J)'$. The synthetic control thus is the weighted average of the comparison units. These weights are required to be non-negative and sum up to one so that $0 \leq w_j \leq 1$ and $w_1 + ... + w_J = 1$ holds. Usually some units in the donor pool are not chosen to be part of the synthetic control which means that the respective comparison units get a zero w-weight. The W-vector is then chosen to as best as possible approximate the matrix X_J with the vector X_i. That is, it minimises the difference $v\|X_i - X_J W\|$. The ultimate aim of the optimisation is to minimise the root mean square prediction error (RMSPE)[5] of the outcome variable in the pre-intervention period.[6]

The vector W^* is then used to depict the outcome variable of the synthetic control. Let $Y_i = (Y_{i,T_{pre}+1}, ..., Y_{i,T})'$ be a $(T_{post} \times 1)$ vector that contains the values of the outcome variable for the treated unit in the post-intervention period. And for the units in the donor pool the $(T_{post} \times J)$ matrix Y_J contains the values of the outcome variable for all countries in the pool. The estimation of the intervention effect is now given by the difference between the T_{post}-values of the outcome variable of the synthetic control and that of the treated unit, respectively. So for any point in time $t > T_{pre}$ the synthetic control estimator (SCE) can be written as

$$SCE_t = Y_{i,t} - \sum_{j=1}^{J} w_j^* Y_{j,t}. \tag{3}$$

In the subsequent analysis we calculate the treatment effect of the overall post-intervention

[3] The year of the intervention itself neither belongs to the pre-period nor to the post-period.

[4] Lags of the outcome variable are often used as predictors, too.

[5] RMSPE=$\left(\frac{1}{T_{pre}} \sum_{t=1}^{T_{pre}} \left(Y_{i,t} - \sum_{j=1}^{J} w_j^* Y_{j,t} \right)^2 \right)^{\frac{1}{2}}$

[6] The sequence is: 1) find initial v-weights via regression, 2) find optimal w-weights and calculate pre-RMSPE, 3) find alternative v-weights via iteration method, calculate w-weights and pre-RMSPE, 4) repeat step three until pre-RMSPE is minimised.

period T_{post} as the average treatment effect, i.e., $\frac{1}{T_{post}} \sum_{T_{pre}+1}^{T} (SCE_t)$.

We then want to know whether the observed treatment effect occured by chance. However, the selection of cases is not completely at random because the scholar has some leeway in selecting the units into the pool. Furthermore the sample size is rather small and the method does not provide a coefficient of a variable for which a Gaussian distribution in the population can be assumed. Therefore results stemming from the SCM are usually proved by placebo studies. In this analysis we rely on cross-unit placebo tests. The intervention then is assigned to and the analysis is conducted for every single country in the donor pool.[7] The year of intervention is that of the treated unit. 'Placebo' then means that an intervention is imposed to a country although it obviously did not face one.[8] After receiving the results one can then calculate the post-intervention RMSPE as well as the ratio of the post-intervention RMSPE over the pre-intervention RMSPE for every country. The intervention effect can be labeled significant if the treated unit comes up with a post-pre-ratio that is large relative to the ratios of the donor pool countries.

To quantify the statistical significance we refer to the concept of the p-value and calculate the probability to find a country in the donor pool with a post-pre-ratio that is of the same size or even larger than that for the treated unit. This probability is then compared to a certain significance level (e.g. $\alpha = 5$ percent) to evaluate the statistical significance of the result obtained in the analysis.[9]

After all we can seriously rely on the results only if all distorting influences are excluded. So first of all we shall care for observable as well as unobservable characteristics. The former are basically taken into consideration via the inclusion of predictor variables. With respect to the latter (but also the observable characteristics) Abadie et al. (2010) argue that their influence is smaller the longer the pre-intervention time period is. If it is possible to trace the outcome variable of the treated unit for a long time it can be assumed that this is because the units are similar to each other concerning the aspects that impact on the outcome variable. This holds for both observable and unobservable characteristics. In fact, the length of the pre-period is restricted by data availability.

Furthermore, we restrict our sample to OECD member countries and even a selection out of it (see section 4). So countries with substantial economic or political structural breaks different from Switzerland and the OECD countries are excluded. Similarly, we restrict the post-intervention period to five years to prevent the trajectory of the synthetic control from influences unrelated to the intervention.

One particular characteristic that hardly can be measured but is particularly discussed

[7] An alternative would be in-time placebos. The intervention then is assigned to the treated unit but at a point in time where there was no intervention.

[8] When we run the placebo test for a country in the pool the treated unit itself is excluded. So if we e.g. run the analysis for Australia, Switzerland is not part of the donor pool.

[9] This significance test is rather strong. If there are twenty countries in the pool and the treated unit comes up with the largest post-pre-ratio the utmost significance level is 0.05. If, however, the treated unit comes up with the second largest ratio this level jumps up to 0.1.

when it comes to Switzerland is fiscal conservatism. The reasoning here is that the introduction of the fiscal rule does not cause an improvement of the budget balance by itself because the Swiss citizens and with them the Swiss politicians inherently prefer low spending and slight borrowing.

Bohn and Inman (1996) deal with fiscal conservatism in the U.S. states such that they declare the southern states to be fiscally conservative. Furthermore, they include the average percentage of voters who consider themselves to be conservative and declare the Republicans to be more conservative than the Democrats. But the effect of the states', voters' or legislators' (political) conservatism does not turn the effect of the deficit rule into insignificance nor does it reduce the size of the effect considerably. To test for fiscal conservatism Dafflon and Pujol (2001) create an index based on referenda dealing with fiscal affairs regarding the central level submitted to voters in the Swiss cantons. So voters are classified to be conservative if they show a high acceptance rate in favour of tax rate increases, spending cuts or measures that aim at reducing the deficit. They find that the demand for public borrowing is lower in a canton the more conservative the residents are. Consequently, Krogstrup and Wälti (2008) are exclusively testing for voter preferences as omitted variable. In their analysis covering the Swiss cantons the impact of a fiscal rule on the budget balance remains significant even when it is controlled for the fiscal preferences of the voters. Similarly, Funk and Gathmann (2013) augment their estimation of the effect of direct democracy on government spending by the inclusion of some measures that capture voter preferences. They find that this relationship gets considerably weaker in terms of size and statistical significance once voter preferences are incorporated.

Hence it can be questioned what the separate impact of fiscal rules is. Or put differently, we risk to get a treatment effect that is too large if we do not capture fiscal conservatism. More importantly, we would not be able to quantify the real size of the treatment effect as we cannot separate the treatment effect from fiscal conservatism in the post-intervention period. Again, we follow Abadie et al. (2010) in these arguments. If citizens and politicians in Switzerland behave fiscally conservative this should affect the outcome variable in the pre-intervention period. Since this method is based on matching it would assign a positive w-weight to countries that are also fiscally conservative or are at least able to approximate the conservative spending behaviour in Switzerland. This means that the fiscal conservatism of Switzerland is captured by the approximation of the pre-period and the treatment effect can be attributed to the introduction of the fiscal rule.[10] Beyond that, Funk and Gathmann (2013) clearly show that fiscal conservatism is rather stable over time. In this respect the inclusion of a respective measure would not help to considerably improve the pre-RMSPE as the SC Estimator emulates the varia-

[10] If, in contrast, Swiss citizens are fiscally conservative but the politicians do not act correspondingly we have a classical principal agent problem with too generous spending in the pre-period. This period does not capture fiscal conservatism because it does not exist on the politicians' side. The fiscal rule comes up with a separate effect because it forces the politicians to stick to citizens' preferences.

tion of the outcome variable over time. Thirdly, Angrist and Krueger (1999) argue that omitting crucial variables would lead to the misspecification of the empirical model. This request is less strong, however, under the Matching Estimator as the covariates mainly contribute to the fulfilment of the homogeneity assumption (Gangl and DiPrete, 2004). Thus we are convinced that our results are unbiased even if fiscal conservatism is not explicitly controlled for.

Next, we shall consider reverse causality. Applied to our analysis this would mean that a certain shape of the treatment effect has led to the intervention. As the treatment effect occurs undoubtedly after the policy intervention we feel confident that reverse causality is not a serious obstacle in our context.

Eventually it is necessary that there are no spillover effects. This means that there must not be an effect of the intervention on the outcome variable of the treated unit or the donor pool units during the pre-intervention period. The same holds for the outcome variable of any donor pool unit in the post-intervention period. An effect of the intervention on Switzerland's outcome in the pre-period could come up as an anticipation. However, anticipation effects are captured by the approximation similar to fiscal conservatism. And we do not know a case in which the Swiss balanced budget rule influenced the spending behaviour of another country in the pool. Eventually, Switzerland may be too small to have such an impact.

4 Data

First and foremost we are interested in the effect of the Swiss debt brake on the budget balance. Thus we look at the total federal government budget balance which is total federal government revenues net of total federal government expenditure. These variables are given in national currency and in current prices. While total revenue entails total sales (market output and output for own final use), payments for non-market output, subsidies, property income, total taxes, total social contributions, other transfers and capital transfers, total expenditure mainly covers intermediate consumption, compensation of employees, subsidies, interest payments, taxes, social benefits, social transfers in kind, current transfers, capital transfers, gross capital formation and net acquisition of non-financial non-produced assets (OECD, 2007). The budget balance is expressed in percent of GDP and is adjusted by the output gap to obtain the cyclically adjusted budget balance (see section 5.1).

Additionally we are interested in the effect on government debt measured by the federal government debt ratio as percentage of GDP. This covers debt issuance of the federal government level (marketable and non-marketable) excluding state and local government debt as well as social security funds. Moreover, we look at general government debt as percentage of GDP. This covers all government units (federal, state, local) as well as

nonmarket-nonprofit institutions that are financed by government units. Public corporations are excluded (Abbas et al., 2010).

In order to find out whether compliance with the rule is achieved by an increase in revenues or a decline in expenditures we also intend to test the effect of the debt brake on the revenue ratio as well as the expenditure ratio. However, these variables are rather volatile and an approximation in the pre-intervention period is not possible to a satisfying extent. We also subtract social expenditures from government expenditures without any particularly helpful result.

In choosing variables that are good predictors of debt development we follow the literature (e.g. Bohn and Inman, 1996). First we consider the natural logarithm of real GDP (per capita) as Wagner's law claims that government spending is higher the more developed the economy is. Second, we consider the annual growth rate of GDP, the growth of unemployment as well as the rate of unemployment as debt development clearly depends on the business cycle. Moreover, the growth of GDP reflects a country's capability to repay debt. Thirdly, the natural logarithm of total population is included as it captures the idea that some government spending categories are affected by economies of scale. Finally, we include the percentage of people aged 65 and older and the sum of imports and exports as an indicator for the openness of a country. These two variables describe important determinants of social spending and thus government spending. Whereas the share of the elderly is assumed to be positively correlated to health spending the theory behind the trade indicator is that governments of open economies are incentivised to compensate the losers of globalisation (Rodrik, 1998). These predictor variables are always supported by the lagged outcome variable of certain years which often is the first and the last year of the pre-intervention period. Among these predictors we search for a combination that best supports the approximation of the real Swiss data.

As mentioned already we restrict our sample to OECD member countries. However, data is not available for all of these nations and all variables of interest. Thus we basically restrict our sample to countries for which data is available for the budget balance, the central government debt ratio and the general goverment debt ratio. So our sample contains Australia, Austria, Belgium, Canada, the Czech Republic, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, the Slovak Republic, Spain, Sweden, the United Kingdom and the USA. Hence our donor pool basically consists of 22 countries.

With respect to Denmark (1997), New Zealand (2002) and Sweden (2003) there was one single observations missing in the general government debt ratio time series. However, since this variable works as dependent variable the whole time series cannot be used if one observation is missing. In order to be able to use this data we generated the missing observations by employing a linear interpolation. As government debt generally moves rather smoothly we think that a linear interpolation for one single observation is

appropriate. For the United Kingdom central government debt ratio data was missing for the 1993-1997 time span in our data source which is OECD.Stat. In order to be able to include the United Kingdom we use central government debt data from the OECD Fiscal Decentralisation Database instead. This data covers the total liabilities excluding insurance technical reserves and is given as percentage of GDP. For the years 1998 to 2010 for which both sources provide data the average deviation amounts to 2.69 percentage points. This shows that the grafted time series behaves very similar to the original data. For the Czech Republic and the Slovak Republic both central government debt data and general government debt data start in 1992 or 1993. We thus choose the latter year to be the starting point of the respective pre-intervention periods. So the pre-intervention period starts a little later than without this restriction but this is for the benefit of including these countries in the analysis. Likewise, we choose the post-intervention period of the central government debt analysis to end in 2009 (instead of 2010) in order to be able to include Japan in this analysis.

For the three countries Israel, South Korea and Poland we have data for two of the dependent variables. We choose Israel to be part of the analysis of the budget balance and the general government debt ratio as data is available for these two series only. South Korea and Poland are included as robustness check for the analysis of the central and general government debt ratio.

5 Empirical Analysis

5.1 The Effect on the Budget Balance

At first glance the variable 'government expenditure' is the rule's target figure. However, expenditures are related to predicted revenues of the same fiscal year. This renders the Swiss fiscal rule a balanced budget rule. Thus we first check its impact on the budget balance, and by applying the HP-filter to available GDP data, we calculate the cyclically adjusted budget balance. Unlike the SFFA we use the classical Hodrick-Prescott filter (with a smoothing factor $\lambda = 100$). On the one hand this is because the classical filter is easier to implement. On the other hand the SFFA needs this modified filter because the classical one has an end-point bias (see section 2). However, we do not face this problem. Whereas trend-GDP data is available for the 1980-2013 or 1990-2013 time span our pre-intervention period starts in 1995 and ends in 2007 in order to estimate the effect of the debt brake as precisely as possible. The Synthetic Control Method is able to capture the short run effect of a policy intervention. A large extension of the period around the intervention thus runs the risk of capturing structural shocks that are not related to the Swiss debt rule's introduction.

Our calculation of the cyclically adjusted budget balance is in line with the standard methodology used, e.g., in the European Union fiscal framework (Mourre et al., 2013)

and can be expressed by

$$CAB = \frac{B}{Y} - \frac{Y - Y^T}{Y^T} \tag{4}$$

with $\frac{B}{Y}$ as the nominal budget balance to GDP ratio and $\frac{Y-Y^T}{Y^T}$ as the percentage of the output gap on the trend-GDP. Regarding the minuend of the equation we use GDP data in current prices since revenues and expenditures are also expressed in current prices. Consequently, we also use (trend) GDP data in current prices in the subtrahend of the equation. Concerning the minuend, price changes should not affect the public finance variables very much since they are expressed as percentage of GDP. Similarly, price movements should largely be cancelled out in the subtrahend as the nominal business cycle element $Y - Y^T$ is also expressed as percentage of nominal GDP. Of course, this assumes that revenues and expenditures are affected by price changes in the same way as the output variable. The output gap usually is multiplied by the factor ϵ that captures the cyclical sensitivity of the budget balance. However, since the SFFA in their calculation of the budget balance implicitly assumes the revenue elasiticity to be one (Colombier, 2004) we refrain from using an overall elasticity different from one.

To test the effect of the rule's introduction on the budget balance we start the pre-intervention period in 1995. This is because revenue and expenditure data starts in 1995 for most of the countries in the sample. We rely on the consistent sample as explained in section 4 which is enriched by Israel since revenue and expenditure data is available for it. This leaves us with 23 countries in the donor pool. Among the predictor variables named in section 4 we use the natural logarithm of total population and the growth rate of unemployment as these variables proved to be helpful in minimising the pre-RMSPE. Additionally, we use the lagged cyclically adjusted budget balance of the years 1995, 1999 and 2000. Since the budget of 2003 was the first under the rule we declare the year 2002 to be the year of intervention. Although the referendum took place in 2001 this is unproblematic because the referendum took place at the end (December) of that year. The post-intervention period consequently starts in 2003 and ends in 2007 as it shall cover no more than five years. Referring to what is recommended in section 3 one could argue that the pre-intervention period is short. However, the budget balance is rather volatile because it is composed of revenue and expenditure data which are rather volatile, too. As this affects the approximation we value a 7-year-long pre-period to be reasonable.

The w-weights are given in table 1. All countries used for this analysis are also part of the synthetic control. In fact, this makes the Synthetic Switzerland a real 'synthetic' control since Switzerland is not replicated by a small number of countries with large w-weights each. Instead, many countries contribute to the Synthetic Switzerland with a w-weight smaller than 1 percent. And although four countries (Ireland, Israel, the Netherlands and the Slovak Republic) stand out with a relatively large w-weight, none

of these weights is larger than 30 percent.[11] The predictor balance is given in table 2. This balance shows the quality of the approximation in the pre-period indicating it for every single predictor variable. The column 'Treated' provides for the value of the respective predictor variable for Switzerland that is averaged over the pre-intervention period. The entire column thus provides the vector X_i explained in section 3. Likewise, the column 'Synthetic' provides the mean value of the same predictor variable for Synthetic Switzerland. So this column is related to matrix X_J. As can be seen the average fit between Switzerland and the synthetic control is very precise for all explaining variables. This is also reflected by the pre-RMSPE which amounts to 0.43.

Figure 2 now exhibits the graphical result. Except for small deviations between the treated unit and the synthetic control in 1996, 1997, 1998 and 2001, the fit is absolutely congruent. For the years after the rule's introduction we see a clear positive effect. That is, the budget balance would have performed worse without the fiscal rule. The average treatment effect is then calculated as explained in section 3. It amounts to 3.678 percentage points. The result of the subsequently conducted cross-country placebo test can be seen in figure 3. Switzerland clearly stands out with the largest post-pre-ratio among the countries. The probability to find a country in the pool with a post-pre-ratio of the size of Switzerland or even larger is 1/24=0.042.

To test the robustness of this result we eliminate the countries with the smallest w-weights from the pool. So we discard the Czech Republic and Germany in the first step. These countries contribute to the Synthetic Switzerland with a w-weight of 0.003 and 0.004, respectively.[12] The w-weights as well as the graph (not shown) are very similar to the baseline estimation with the pre-RMSPE amounting to 0.44. Conducting placebo tests again leads to figure 4 showing that Switzerland again comes up with the highest post-pre-ratio. The p-value equivalent is 1/22=0.045.

We then drop New Zealand as it shows up with a w-weight of 0.006.[13] Again, there are no considerable changes compared to the baseline estimation concerning the graph (not shown). The pre-RMSPE accounts for 0.441. Figure 5 shows that Switzerland can assert its position in the ranking of the post-pre-ratios. The p-value equivalent increases only

[11] Fiscal consolidation took different paths across these countries. Ireland showed an expenditure restraint and increased indirect taxes (OECD, 2003). The Slovak Republic, besides the revenue-neutral tax-rate-cut-cum-base-broadening reform, mainly reduced social security benefits (Moore, 2005). The Netherlands adjusted on the expenditure side via the freeze of wages, job cuts, the reduction in healthcare benefits and cuts in subsidies. To roughly 20 percent this country consolidated its public finances via the abolition of tax exemptions (OECD, 2004).

[12] This leads to the following w-weights: Australia 0.01, Austria 0.007, Belgium 0.01, Canada 0.013, Denmark 0.013, Finland 0.007, France 0.016, Ireland 0.28, Israel 0.143, Italy 0.008, Japan 0.007, Luxembourg 0.01, the Netherlands 0.174, New Zealand 0.006, Norway 0.007, Portugal 0.21, Slovak Republic 0.236, Spain 0.008, Sweden 0.007, the United Kingdom 0.009 and the USA 0.009.

[13] This leads to the following w-weights: Australia 0.01, Austria 0.008, Belgium 0.011, Canada 0.012, Denmark 0.014, Finland 0.009, France 0.014, Ireland 0.281, Israel 0.144, Italy 0.008, Japan 0.008, Luxembourg 0.011, the Netherlands 0.173, Norway 0.009, Portugal 0.02, Slovak Republic 0.235, Spain 0.009, Sweden 0.008, the United Kingdom 0.009 and the USA 0.009.

slightly to 1/21=0.048.

Thirdly, we drop Austria, Italy, Japan and Sweden which show up with a w-weight of 0.008.[14] The pre-RMSPE amounts to 0.462. Once more the graph is very similar to the one of the baseline estimation and thus not shown. And once more Switzerland comes up with the largest post-pre-ratio (see figure 6). The p-value equivalent accounts for 1/17=0.059.

Finally, we discard Norway and Finland which contribute to the Synthetic Switzerland with a w-weight of 0.01 and 0.011, respectively.[15] Still the parameters are very similar. This holds for the pre-RMSPE (0.48), the graph of the cyclically adjusted budget balance (not shown) and the p-value equivalent (1/15=0.067). The latter indicates that our result is robust and significant at the 7 percent level. Eventually, figure 7 reveals that Switzerland keeps the first position among the post-pre-ratios. Moreover, the distance to the country that comes second is distinct in all the figures of post-pre-ratios. From the baseline estimation as well as from the robustness checks we conclude that the Swiss balanced budget rule indeed contributes to an improvement of the (cyclically adjusted) budget balance.

5.2 The Effect on the Government Debt Ratio

Beyond analysing the effect on the budget balance we also check the effect of the debt rule on the debt ratio. Admittedly, the rule does not directly aim at reducing the debt ratio. However, a fiscal rule should help to achieve sustainable public finances more or less irrespective of what the target variable precisely is. And the status of financial sustainability can best be captured by the debt ratio.

So at first we look at the federal government debt ratio as dependent variable and subsequently we look at the general government debt ratio. The general government level is more appropriate when it comes to the overall fiscal sustainability. However, the Swiss fiscal rule applies to the federal government level with the cantons having their own fiscal rules. Moreover, the comparison between the federal and the general government level can indicate whether there is some dislocation from the former to the latter.

Starting with the central government debt ratio we again rely on the sample mentioned in section 4. Israel is not added because central government debt data is missing for the years before 1997. So there are 22 countries in the pool. In this analysis we use the percentage of people aged 65+ and the rate of unemployment as well as the lagged

[14] This leads to the following w-weights: Australia 0.014, Belgium 0.014, Canada 0.018, Denmark 0.019, Finland 0.011, France 0.02, Ireland 0.269, Israel 0.142, Luxembourg 0.016, the Netherlands 0.166, Norway 0.01, Portugal 0.029, Slovak Republic 0.232, Spain 0.012, the United Kingdom 0.013 and the USA 0.012.

[15] This leads to the following w-weights: Australia 0.017, Belgium 0.02, Canada 0.019, Denmark 0.027, France 0.02, Ireland 0.268, Israel 0.137, Luxembourg 0.026, the Netherlands 0.16, Portugal 0.034, Slovak Republic 0.226, Spain 0.016, the United Kingdom 0.016 and the USA 0.013.

outcome variable of the years 1993, 1997, 2001 as predictors. Although data is available for the earlier years we start the pre-period in 1993 in order to be able to include the Czech Republic and the Slovak Republic into the analysis. Due to an intersection of both trajectories in the post-intervention period (2003-05) we extend this period to more than five years. In order to be able to include Japan, however, the post-period ends in 2009 instead of 2010. The post-period thus covers seven years.

The w-weights are given in table 3. In contrast to the budget balance analysis a considerably smaller number of countries is used to approximate the debt development in Switzerland. Two of these four countries show up with relatively large weight of about 30 percent and almost 60 percent, respectively. So almost 90 percent of the debt trend of Switzerland is represented by France and Luxembourg. The predictor balance (table 4) points to a very precise fit in the pre-period. Only the approximation in terms of the unemployment rate is a little less precise. The pre-RMSPE accounts for 0.836.

Figure 8 now provides the result. As can be seen the approximation is relatively good except for the 1997-99 period. The hump in Switzerland's data scarcely can be imitated by any synthetic control. In the post-period one can see that debt in Switzerland exceeds that of Synthetic Switzerland for three years after the introduction (2003-05). We ascribe this effect to the rule's introduction phase explained in section 2. In the years to follow, however, the debt ratio clearly decreased below the level of 2001. The debt of the synthetic control, in contrast, increases remarkably from 2008 onwards. To some extent this might be driven by the situation in France which weightily contributes to Synthetic Switzerland and strongly suffered from the debt crisis. Sadly, we cannot run the placebo tests due to the intersection of the two trends in the post-period. And we cannot say what the trend of Synthetic Switzerland would have been without the debt crisis. Thus we hardly can tell anything about the debt progress of Switzerland without the introduction of the fiscal rule.

As mentioned earlier, South Korea and Poland are not part of our basic sample. So we now include these two countries. The post-period again ends in 2009. Searching for the smallest pre-RMSPE uncovers that the natural logarithm of real GDP and the share of elderly people are the predictors that best help to minimise the pre-RMSPE. Instead of Japan, the Czech Republic (0.059) and Finland (0.035) now contribute to the synthetic sibling. France (0.401), Luxembourg (0.473), and Sweden (0.033) are still part of the synthetic control whereas France and Luxembourg again explain about 90 percent of the synthetic control's trajectory. The pre-RMSPE worsens only slightly (0.92). The graph of the post-period looks very much the same (and is thus not shown) which is reasonable since neither South Korea nor Poland contribute to the synthetic control. If we instead simply include South Korea and Poland in the previous analysis and do not search for the best fit the pre-RMSPE amounts to 0.942. The new w-weights are: Austria 0.129, Finland 0.156, Japan 0.066 and Luxembourg 0.65. The post-period trajectories of Switzerland and

its synthetic sibling still move very similar to the baseline analysis (not shown).

Next we turn to the general government debt ratio as dependent variable. Again we rely on the sample described in section 4. Since general government debt data is available we include Israel in the analysis. This leaves us with 23 countries in the pool. As explaining variables we employ the natural logarithm of population, the rate of unemployment and the lagged debt ratio of the years 1993, 1997 and 2001. Whereas the pre-intervention period again runs from 1993 to 2001 we once more extend the post-period up to 2010 due to an intersection of the post-period trajectories. The post-period thus covers eight years.

The w-weights are now given in table 5. Similar to the federal government debt analysis a limited number of countries contribute to Synthetic Switzerland. France is not part of the synthetic control group anymore and the w-weight of Luxembourg is considerably smaller compared to the central government debt analysis. In contrast, Denmark and the United Kingdom explain almost two-thirds of Switzerland's debt development. The predictor balance shows that the approximation works very precisely regarding the lagged outcome variable and rather well with respect to the remaining predictors. The pre-RMSPE amounts to 0.712.

The graphical result can be seen in figure 9. In the first five years of the pre-period the approximation is very accurate. Then Synthetic Switzerland, to some extent, tries to copy the hump of the 1997-99 period. In the post-period general government debt of the Synthetic Switzerland evolves differently compared to the federal government debt analysis. After a slight increase in 2003 and 2004 it declines in 2005, 2006 and 2007. Similar to the central government debt analysis it rises from 2008 onwards. Once again, we cannot conduct the placebo tests due to the intersection of the trajectories. And we hardly can state what the path of Synthetic Switzerland would have been without the financial crisis. Since the debt of the synthetic control starts to decrease in 2005 we cannot deduce what a potential treatment effect from 2008 onwards would look like. Nevertheless we can assume the nonexistence of debt dislocation effects. This is because the trajectories of the federal government debt ratio as well as the general government debt ratio of (real) Switzerland look very similar in the post-intervention period. That is the pressure to put federal public finances in order is not realised by stressing subnational government levels. This makes sence as the cantons have fiscal rules themselves.

In a next step, we again include South Korea and Poland as robustness check. Our search for the best fit of the pre-intervention period, however, leads us to the same predictor variables and the same Synthetic Switzerland with a very similar pre-RMSPE (0.711) and a graphical result (not shown) compared to the previous analysis.

5.3 On the Introduction of Fiscal Rules among the Comparison Units

As mentioned in section 3 the outcome variable of any control unit must not be affected by shocks in the time period under consideration for the result to be unbiased. This

also means that the introduction of a fiscal rule in any country of the synthetic control group would bias the values of their cyclically adjusted budget balance. This is especially relevant for the post-intervention period. The best way to get rid of this problem is to drop all countries from the donor pool that introduced a fiscal rule between 1995 and 2007.[16]

This applies to 16 out of 23 countries in the donor pool. That is, Australia, Austria, Canada, Czech Republic, Denmark, Finland, France, Israel, Japan, Luxembourg, Norway, Portugal, Slovak Republic, Spain, Sweden and the United Kingdom either introduced (changed) a national fiscal rule (central or general government level) or joined the Maastricht Treaty (IMF, 2009; Budina et al., 2012). A serious analysis would thus not be possible anymore. Hence we remove every single affected country from the pool and check what the trajectory of Synthetic Switzerland in the post-period looks like without this one country.

When we drop the countries with a w-weight smaller than 0.02 (see table 1) the treatment effect remains unchanged in every single case. The pre-RMSPE is always very close to that reported in section 5.1. Thus we argue that there is no bias stemming from these countries in the post-period. When we look at the four countries with a relatively large w-weight, we find that Ireland and the Netherlands did not introduce a fiscal rule in the respective time period. Dropping Israel leads to a pre-RMSPE of 0.593 which is only slightly poorer than the RMSPE of the baseline estimation. Again, a remarkable treatment effect can be observed (not shown) and, in fact, this one is even larger than the baseline effect. It amounts to 5.817 percentage points. Yet, this makes sense because Israel expanded its Deficit Reduction Law by a provision to limit the growth of central government expenditures in 2004 (IMF, 2009; Budina et al., 2012).

The only country that is problematic in this sense is the Slovak Republic. The approximation of the years 1996, 1997, 1998, 2000 and 2001 is rather imprecise if we remove it from the pool. The pre-RMSPE now amounts to 1.247. However, this country did not introduce a fiscal rule at the national level in the respective time period but joined the European Union in 2004. Similar to the case of Israel we argue that joining the Maastricht Treaty might have improved the budget balance rather than worsened it. The gap between the trajectory of Switzerland and Synthetic Switzerland would be larger if the Slovak Republic did not join the European Union. So this case but even more the Israeli case suggests that the treatment effect we see in figure 2 is a conservative estimation. Overall, the budget balance analysis benefits from the fact that many control units contribute with a small w-weight which creates a 'really' Synthetic Switzerland.

[16] As we do not obtain a treatment effect in either of the debt ratio analyses this section deals with the (cyclically adjusted) budget balance analysis only.

5.4 The Debt Brake in the Medium Run

The Swiss fiscal rule aims at balancing the budget in the medium run. In the light of a positive GDP growth this would lead to a decrease of the debt ratio. However, Schips et al. (2003) show that exactly balancing the budget is not possible over the business cycle. This is because revenues follow a positive trend over time whereas the factor k does not follow a trend. The cause of the positive revenue trend can be seen in the stable relationship between revenues and GDP. In conducting a simulation based on GDP and revenue data of the 1988-2001 period they show that the rule might bring debt back to its initial level in the medium run but that this is not necessarily the case. Thus a decreasing debt ratio cannot be expected in the medium run. Bruchez (2003b) also finds that a structurally balanced budget is not possible in the medium run. But beyond that he theoretically as well as numerically shows that the expected average deficit in the long run is rather small. Thus he concludes that the Swiss fiscal rule is able to stabilise the debt level in the long run. And this, in turn, means that it promotes the decrease of the debt ratio in the long run. This is supported by Geier (2004) who applies the rule to three artificial series of GDP-data and revenue data. That is a sinusoidal business cycle, a random business cycle and a random walk with drift. He finds that the budget is almost always balanced over the long term.

The study of Bodmer (2006) does not value the effectiveness of the fiscal rule against a certain numerical target. Neverthelesss he finds that the deficits of the 1989-2002 period would have been considerably smaller if the rule had been in place at that time already. He concludes this from a simulation where he uses provisional GDP data for year $t-1$ as well as the revenue estimate and the GDP estimate for year t for every single year of the 1989-2002 time period. Under the assumption that the rule is complied with, he calculates the notional expenditure limit and the notional deficit which is then compared to the true deficit in year t.

To contribute to the discussion about the effectiveness of the fiscal rule in the medium run we extend the post-intervention period up to 2013 and rerun the budget balance analysis. Here we need to discard Australia, Japan and New Zealand as revenue and eypenditure data is not available for 2013. However, their individual contribution to the Synthetic Switzerland is very small (see table 1). This leaves us with twenty countries in the donor pool. The pre-RMSPE increases only slightly (0.446) and the w-weights of the remaining countries change only marginally, too. The impact of dropping these countries on the change in the development of Synthetic Switzerland can thus be neglected. Figure 10 shows that the clear distance between the cyclically adjusted budget balance of both Switzerland and Synthetic Switzerland persists up to 2013. The average treatment effect accounts for 4.706 percentage points. This suggests that deficits would have been larger without the rule and the rule is effective also in the medium run. However, one should be

cautious in relying on this medium-term result as the Synthetic Control Method is able to capture the short run effect of a policy intervention. A large extension of the post-period, in contrast, runs the risk of capturing structual shocks of both Switzerland and Synthetic Switzerland that are not related to the rule's introduction. However, the treatment effect lends support to the idea that the Swiss fiscal rule is effective in improving the budget balance also in the medium run.

6 Conclusions

The Swiss federal debt rule has become a role model for other countries. First, Germany adapted it to its constitutional framework, before the Eurozone countries in the fiscal compact accepted to introduce their own national debt rules. Second, other countries, e.g., Israel followed the ideas of the Swiss debt rule in reforming their own legislation. In all these countries, the effectiveness of fiscal rules is politically challenged in order to revise them before they can show any effect. Even in the Swiss case, it is difficult to provide evidence regarding the effectiveness of the debt rule using traditional econometric methods.

This paper is the first that provides evidence for the effectiveness of the Swiss balanced budget rule. In contrast to previous studies that try to uncover the impact of this fiscal rule using simple simulation exercises, we rely on the Synthetic Control Method. We find that the rule improved the cyclically adjusted budget balance by about 3.6 percentage points of GDP on average in the five years after its introduction. This treatment effect is robust and persists also in the medium run. Concerning the effect on the debt ratio of both the federal government level and the general government level we cannot provide clear-cut results. For methodological reasons, this is mainly due to the difficulties arising from the 2003-05 transition period that causes an intersection of the debt trajectories in the post-intervention period. Additionally, the debt trend of the synthetic control group may be biased by the European debt crisis from 2008 onwards.

Overall, the evidence provided in this paper strongly suggests a causal impact of the Swiss federal debt brake on federal finances. With lower structural deficits, the federal government could return to sound public finances. There is no reasons to believe that such intelligent fiscal rules have a different effect in Germany or in other countries. The opposition against fiscal rules may simply originate from the expectation that they actually restrain governments from being profligate.

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Tables and Figures

State	w-Weight	State	w-Weight	State	w-Weight
Australia	0.009	Germany	0.004	Norway	0.006
Austria	0.006	Ireland	0.286	Portugal	0.014
Belgium	0.008	Israel	0.145	Slovak Republic	0.237
Canada	0.013	Italy	0.007	Spain	0.007
Czech Republic	0.003	Japan	0.007	Sweden	0.006
Denmark	0.011	Luxembourg	0.009	United Kingdom	0.007
Finland	0.005	Netherlands	0.181	USA	0.009
France	0.015	New Zealand	0.005		

Table 1: Cyclically Adjusted Budget Balance, w-Weights

Variable	Treated	Synthetic
lnPOP	15.779	15.783
Unemp growth	-4.33	-4.328
CAB 1995	-1.292	-1.297
CAB 1999	0.408	0.414
CAB 2000	-2.426	-2.422

Table 2: Cyclically Adjusted Budget Balance, Predictor Balance

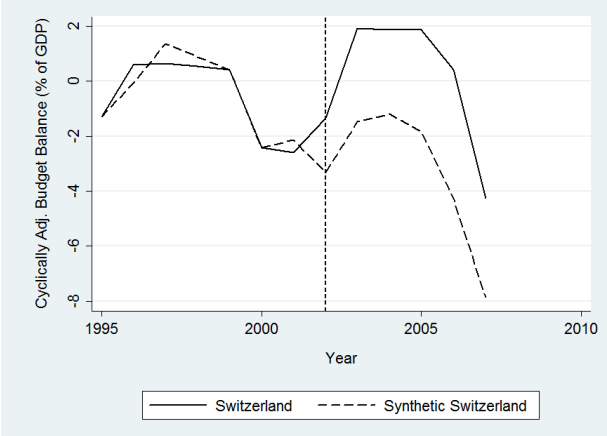

Figure 2: Cyclically Adjusted Budget Balance

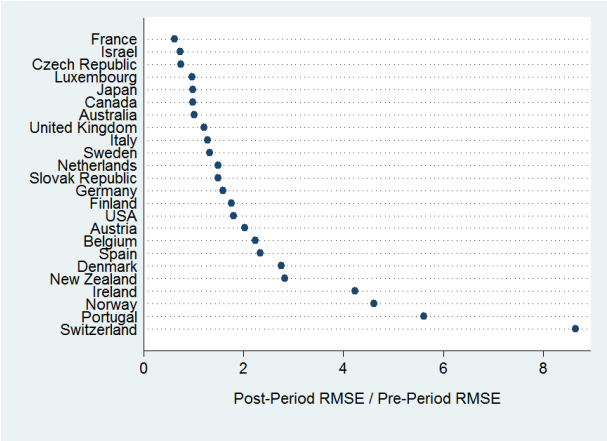

Figure 3: Cyclically Adjusted Budget Balance, Post-Pre-Ratios

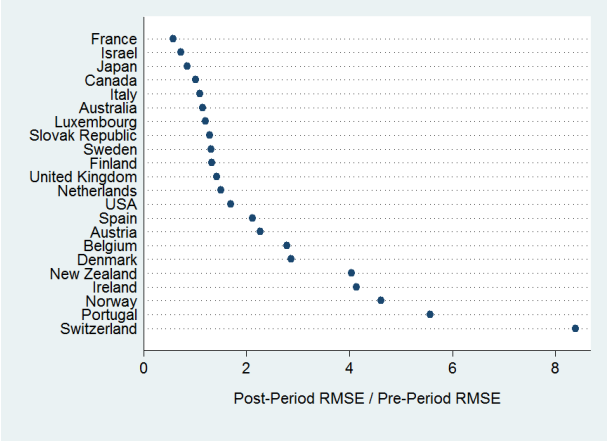

Figure 4: Cyclically Adjusted Budget Balance (without CZE, DEU), Post-Pre-Ratios

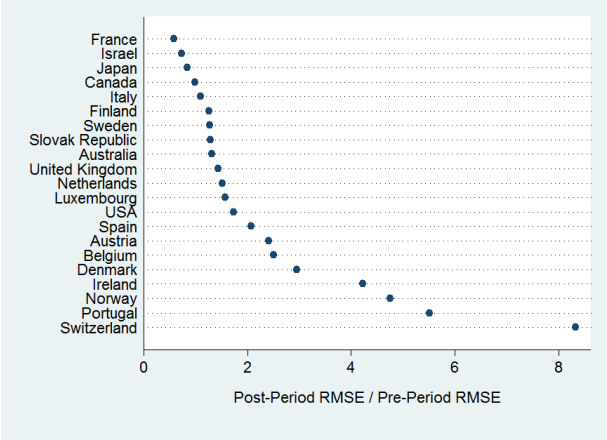

Figure 5: Cyclically Adjusted Budget Balance (without CZE, DEU, NZL), Post-Pre-Ratios

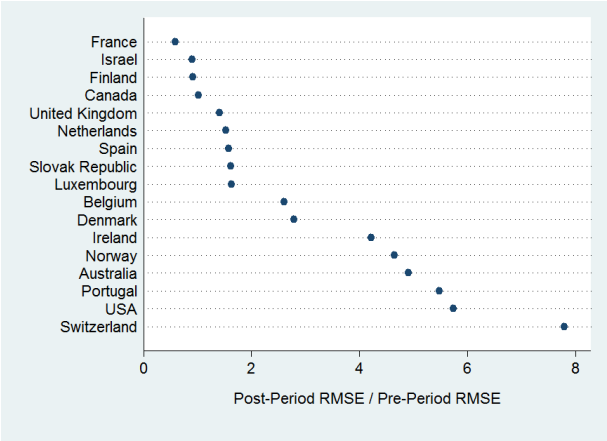

Figure 6: Cyclically Adjusted Budget Balance (without AUT, CZE, DEU, ITA, JPN, NZL, SWE), Post-Pre-Ratios

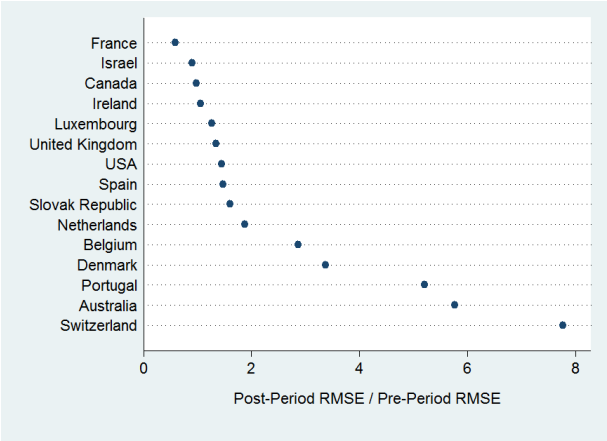

Figure 7: Cyclically Adjusted Budget Balance (without AUT, CZE, DEU, FIN, ITA, JPN, NOR, NZL, SWE), Post-Pre-Ratios

State	w-Weight	State	w-Weight
France	0.308	Luxembourg	0.571
Japan	0.031	Sweden	0.091

Table 3: Federal Government Debt Ratio, w-Weights

Variable	Treated	Synthetic
POP65	14.946	14.816
Unemp rate	3.451	5.678
CGDR 1993	19.25	19.362
CGDR 1997	25.274	25.293
CGDR 2001	24.822	24.91

Table 4: Federal Government Debt Ratio, Predictor Balance

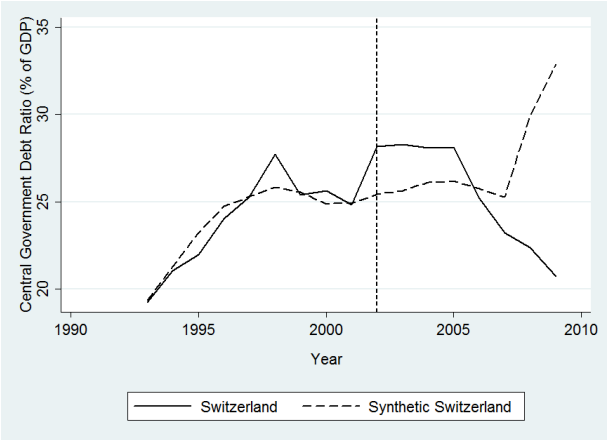

Figure 8: Federal Government Debt Ratio

State	w-Weight	State	w-Weight
Denmark	0.328	Luxembourg	0.189
Japan	0.167	United Kingdom	0.316

Table 5: General Government Debt Ratio, w-Weights

Variable	Treated	Synthetic
lnPOP	15.774	16.29
Unemp rate	3.451	5.461
GGDR 1993	49.6	49.67
GGDR 1996	60.5	60.433
GGDR 2001	61	61

Table 6: General Government Debt Ratio, Predictor Balance

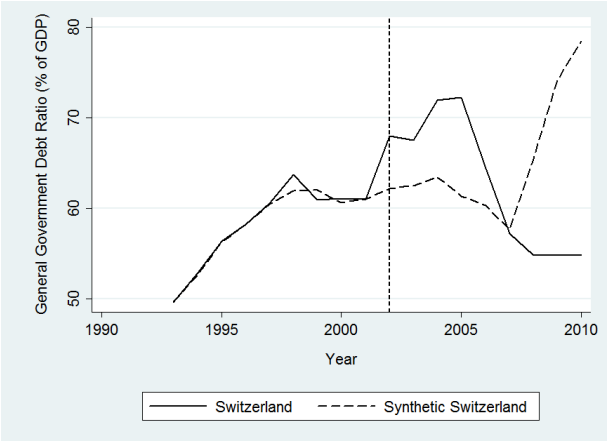

Figure 9: General Government Debt Ratio

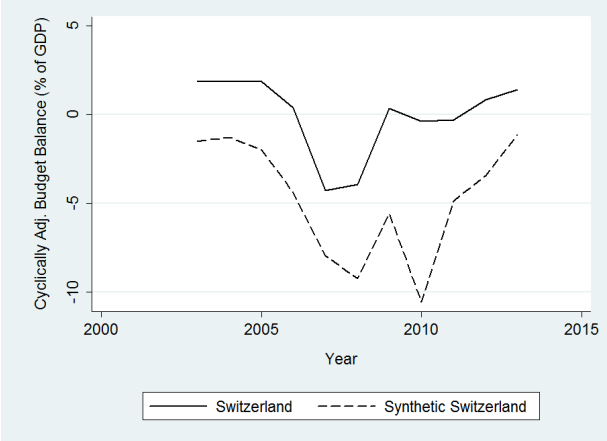

Figure 10: Cyclically Adjusted Budget Balance, extended post-period

Appendix: Variables

CAB cyclically adjusted budget balance as percentage of GDP, source: OECD.Stat and own calculations.

CGDR total central government debt ratio (as percentage of GDP), stocks: outstanding amounts, source: OECD.Stat., for the United Kingdom we use total liabilities excluding insurance technical reserves as percentage of GDP, source: OECD Fiscal Decentralisation Database.

GDPgrowth annual growth rate of GDP, source: OECD.Stat.

GGDR general government debt ratio (as percentage of GDP), source: Historical Public Debt Database.

lnGDP natural logarithm of GDP per head, US$, constant prices, constant PPP's, reference year 2005, source: OECD.Stat and own calculations.

lnPOP natural logarithm of total population, source: World Development Indicators and own calculations.

Pop65 population aged 65 and above as percentage of total population, source: World Development Indicators.

Trade imports of goods and services plus exports of goods and services as percentage of GDP, source: World Development Indicators and own calculations.

Unemp growth annual growth rate of unemployment, source: OECD.Stat - Economic Outlook No. 88 database and own calculations.

Unemp rate total unemployment, in percent of total labour force, source: World Development Indicators.

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