The Economic Costs of a Secessionist Conflict: The Case of Catalonia*

Alejandro Esteller-Moré (Universitat de Barcelona & IEB)[a]
Leonzio Rizzo (Università di Ferrara & IEB)[b]

Abstract. Due to the pro-independence demands of part of its electorate, the political fit of Catalonia within Spain has given rise to notable political tensions over the last few years. This conflict has progressively affected several dimensions of Catalan society, including, potentially, the economy. The illegal referendum on independence, held in October 2017, marked the climax of political and social tensions, leading to a Constitutional crisis and further stoking the conflict as opposed to offering any hope of an early resolution. We analyze a complete set of margins potentially affected by the referendum, including real (aggregate demand and supply) and financial responses. Using a synthetic control method, we find strong evidence of the outflow of short-term bank deposits after the referendum; while, on the real side, we find evidence of responses in aggregate supply (number of capital increases and number of new firms registered).

Keywords: Referendum, Independence, Synthetic Control Method, Capital Flows, Economic Uncertainty

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[a] @aestellermore // linkedin.com/in/alejandro-esteller-moré-12990b13 // ORCID: 0000-0002-7785-6734

[b] @leonziorizzo // linkedin.com/in/leonzio-rizzo-16889b26 // ORCID: 0000-0001-8099-8699

1. Introduction

Over the last 15 years, Catalan politics have been framed within what has been almost constant conflict with the central government. The origins of this conflict are diverse. The restoration of democracy in Spain in 1977 saw the creation of the so-called "Autonomous Communities" (henceforth ACs) and although this meant a relatively major decentralization of expenditure and tax responsibilities (Lago-Peñas, Fernández-Leiceaga, and Vaquero-García 2017), successive governments in Catalonia have called for a special status – similar to that of the Basque Country and Navarre, as explicitly recognized in the Constitution – and better financial treatment in the regional financing system. However, these demands have yet to be met resulting in today's severe social and political conflict. While several authors have analyzed the social and political nature of this conflict, here we aim to quantify its economic impact.

More specifically, by employing a synthetic control method (SCM), we analyze several economic margins that may have been affected by the conflict. The conflict can be considered to have peaked on October 1, 2017, when the Catalan executive organized a unilateral referendum on independence, a poll that was considered illegal by the central executive. Up to then, the channels of political communication between both executives were open, albeit unsuccessfully. The illegality committed by the Catalan executive, though, completely broke those channels making unlikely a quiet solution to the political conflict, leading to a Constitutional crisis. And this is still the case; due to the illegal referendum, no solution is foreseen in the short or even in the medium run. Although the (regional and national) police intervened at some polling stations to prevent it from going ahead, 43.03% of the electorate voted, with 90.18% of these votes being cast in favor of independence.[1] Thus, our post-treatment period is the fourth quarter of 2017 onwards.

Although the origins of the conflict can be traced back, as we said, to at least 15 years before that date, the impact on *economic uncertainty* was especially great in 2017 and concentrated particularly in the AC of Catalonia. This impact is illustrated in Figure 1 below, which shows how often the expression "economic uncertainty" (i.e. number of

[1] Given the illegal nature of the referendum, these data are unofficial. For more details, see: https://en.wikipedia.org/wiki/2017_Catalan_independence_referendum

references to this search term) appeared in Spanish newspapers in relation to a specific AC during the 2014-19 period (unfortunately, data is not available per quarter). The search was performed using *Factiva*. Given the large number of zeros at the beginning of the period (and none in 2019), we reparametrized the search results for each region so as to have a value of 100 in 2019. In the case of Catalonia (thick dashed line), a peak occurs in 2017. Specifically, there were 22, 30, 36, **322**, 104 and 58 news items linking Catalonia and "economic uncertainty" in 2014, 2015, 2016, **2017**, 2018, and 2019 (to the end of June), respectively.[2] For the rest of the regions, 2017 was not a notable year in this respect, with the exceptions of the Balearic Islands (8 news items) and Madrid (218 news items, a figure that is some distance from the number recorded for Catalonia). This exercise provides initial evidence of the adequacy of the treatment year – note, though, we will work with quarterly data – chosen for Catalonia and of the singular nature of Catalonia (treated region). In the empirical analysis, however, we run placebo tests to corroborate these initial impressions.

[FIGURE 1 AROUND HERE]

Unlike, for example, the Brexit vote (Born et al. 2019), the referendum held in Catalonia was, according to Spanish legislation, illegal. This explains why our focus is on the economic costs of the conflict in Catalonia, rather than on the economic costs of (a potential) secession. In fact, after the referendum, if anything, the likelihood of (a negotiated) secession diminished and the expected duration of the conflict increased. This interpretation is confirmed by the sequence of events that occurred in the three weeks immediately following the referendum: the King of Spain's televised address two days after the referendum,[3] in which he called on citizens to respect the rule of law and the unity of Spain; the imprisonment of the civil leaders of the referendum on October 15 accused of insurrection; and the decision taken by the cabinet of the central government to implement "Article 155" of the Constitution, which allows the national government to take over the running of an AC, including its finances and police, on October 27, the same day as the Catalan parliament passed a unilateral declaration of

[2] The full dataset is available upon request.

[3] https://edition.cnn.com/2017/10/03/europe/catalonia-general-strike-protests-barcelona/index.html

independence.[4,5] These decisions, caused as a consequence of the referendum, together with the police intervention in an attempt at thwarting the referendum, are clear indications that, regardless of the result, the illegal referendum did not end the conflict but, on the contrary, that it would now be more intense and prolonged after October 1, 2017. Thus, the illegal referendum is the proxy we use to account for the intensity of the conflict, whose solution since then on is far from being foreseen and the channels of communication between the agents involved have collapsed[6] as a result of the Constitutional crisis.

To provide an idea of the support for secession and of the evolution of that support, Figure 2 shows the percentage support for Catalan independence since March 2015. According to official data collected by the Government's survey institute, support has never risen above 50% (Fig. 2), and while a causal relationship cannot be derived from this time series, pro-independence sentiment peaked precisely in October 2017.

[FIGURE 2 AROUND HERE]

In our empirical setting, to identify the costs of the conflict, we distinguish between real margins of response (differentiating at the same time between the impact on aggregate demand and that on aggregate supply) and financial margins of response. The evidence of a causal impact is particularly incisive for this latter margin. Following the events of October 2017, a significant amount of short- and long-term bank deposits took flight from Catalonia – 36 million euros during the first quarter immediately after the referendum, of which 33 million were short-term deposits (but, note, we are only able to

[4] See the official explanation published by the Spanish Foreign Office on the application of direct rule: http://www.exteriores.gob.es/Embajadas/HELSINKI/EN/Noticias/Documents/The%20application%20of%20Article%20155%20of%20the%20Spanish%20Constitution.pdf

[5] On October 10, the president of the Catalan executive formally recognized the results of the referendum and declared independence, but in the same speech, he went on to say that this declaration would be put on hold in the belief that this would favor negotiations with central government, a meeting which would never take place.

[6] The regional financing system is negotiated between the central executive and the ACs' executives (excluding the Basque Country and Navarre) by means of a multilateral institutional forum so-called "Consejo de Política Fiscal y Financiera". The Catalonian Minister of Finance attended the meetings on 7/30/2015, on 12/1/2016, and even a few months before the referendum, on 6/29/2017. However, he did not attend the meeting held on 8/22/2018; on 2/7/2020 the Catalonian executive sent a top official. Thus, we think this exemplifies well that the channels of communication have collapsed as a consequence of the referendum.

provide robust causal evidence for this latter type of deposit). In the case of real margins of response, on the demand side, GDP per capita and unemployment were unaffected by the conflict; however, we do detect an impact on the supply side. Specifically, a year and a half after the referendum, we estimate a 2,318 reduction in the number of new firms registered in Catalonia with respect to those that would have been created in a synthetic Catalonia without the referendum. Likewise, a study of the number of capital increases in Catalonia shows there to have been 1,200 fewer corporation capital increases with respect to those in synthetic Catalonia a year and a half after the referendum. As such, supply, in general, seems to be particularly reactive to the intensification of the secessionist conflict as a consequence of the illegal referendum. This means that, despite the negligible short-term impact on GDP per capita, the impact on aggregate supply could limit the potential output of the Catalan economy in the medium/long-term, particularly, if the conflict persists. We think this result is particularly important.

According to Skaperdas et al.'s (2009) classification of conflicts, the one we analyze is under the category of low-level conflicts (strikes, protests or road blockades, basically); however, it could still have disruptive impacts on the economy. The economic impact of disruptions caused by low-level conflicts, though, have hardly been estimated. In this vein, Sun and Yu (2020) have recently estimated – also using the SCM – the economic costs of Tibetan demand for secession from China taking advantage of the 1987-1989 Tibetan Unrests. They estimate GDP p.c. would have been 27% larger in the 1988-2007 period in absence of those unrests[7]. The paper by Abadie and Gardeazábal (2003) is also closely related to ours, as deals with a conflict in Spain, it stems from a demand for independence and also uses SCM – a technique that they pioneered –, but the nature of the conflict is quite distinct, as it involves terrorism. The authors estimated that GDP p.c. in the Basque Country was 10% lower than it would have been in the absence of terrorism in the period 1975 to 1995. Although in both cases, the start of the conflict dates back, they both identify particular moments of time – when a series of riots and uprisings hold due to the actions of Tibetan separatists or when the terrorist activity becomes a large-scale phenomenon in the Basque Country, respectively – to estimate

[7] The papers by Riascos and Vargas (2011) or Evia, Laserna, and Skaperdas (2008) estimate the impact of low-level conflicts using different methodologies from SCM, but in contrast with our analysis the origin of those conflicts is not separatism.

the costs of a secessionist conflict. In our case, we take advantage of an illegal referendum, whose occurrence was, as in those papers, independent of the state of the economy.

Our paper is also related to Galasso (2019), in which the author links several shocks that have occurred since 2006 in Catalonia to the stock exchange valuation of Catalan and Spanish firms. He argues that, depending on estimates of the positive or negative impact of the corresponding shock on that valuation, one can infer whether the market positively or negatively assesses the prospects of more decentralization or even of the increased chances of Catalonia becoming independent. According to this empirical framework, the referendum was found to have a strong negative effect on all Catalan firms and on Spanish ones in the tradable sector.

Finally, our paper is also related to the theoretical literature linking decentralization and the aspirations for secession. Cederman, Hug, and Wucherpfennig (2015) and Madiès et al. (2018) discuss the impact of a higher level of decentralization on secession: is it a peaceful solution for autonomy? Or, on the contrary, as it seems to be the case of Catalonia, thanks to the greater possibilities afforded by autonomy, does it fuel separatist campaigns? Gibilisco (2019) has developed a dynamic model of center-periphery relations, from which a trade-off emerges for the central government: repression prevents today's mobilization *but* increases grievances and so greater expectations of peripheral mobilization in the future. Bordignon and Brusco (2001) analyze the optimality of secession rules. While ex-post they are optimal (to avoid large costs of a conflict), they might not be so ex-ante as long as this could precisely incentivize secession demands. In any case, in absence of the "right to secede" and given a level of decentralization, the equilibrium outcome is Pareto inefficient: a political and social conflict. That is, both the regions and the central government would be better off if both actors sought to scale down the conflict (see, for example, Spolaore, 2008). For a detailed discussion, see the recent review on this topic by Kimbrough, Laughren, and Sheremeta (2020).

The rest of the paper is organized as follows. In Section 2, we explain the roots of the conflict between Catalonia and Spain. In Section 3, we describe our empirical methodology for estimating these costs. Section 4 presents the main results and Section

5 present the results of placebo experiments designed to check the robustness of our basic results. Section 6 concludes.

2. Social Conflict due to Secessionist Aspirations: The Catalan Case

As discussed in the Introduction, since the restoration of democracy, the fit of Catalonia within democratic Spain has been far from smooth. Almost all Catalan executives have sought to achieve a special status for Catalonia within Spain, the most common demand being that it be recognized as a nation within the State.[8] This demand, together with others for greater self-government (including tax administration, language policy and judiciary system), generated a political consensus in Catalonia to draft a new "Statute of Autonomy" to replace the one passed in 1979. As we show below, some commentators consider that the failure to introduce this new Statute marked the beginning of the end of the peaceful coexistence of Catalonia within Spain (Castells 2014), a conflict that was to climax in 2017 giving rise to a Constitutional crisis.

With the explicit backing of the then president of the Spanish executive, José Luís Rodríguez Zapatero, the Catalan parliament passed a new draft of the Statute in 2005 with the support of 120 of its 135 members. However, two more steps had to be completed before its formal adoption: the new Statute had to be agreed to by the central legislature and, then, put to a referendum in Catalonia. The bill was approved by Spain's lower house in March 2006 and by the upper house a month and a half later. However, this new draft was not exactly the same as the one the Catalan parliament had voted on. Following negotiations led by Zapatero with Catalan leaders, key matters, including the recognition of Catalonia as a nation, were excluded before it was put to the vote in the lower house. In the referendum held in Catalonia in June 2006, turnout was low (49.41%) but the Statute received the support of 73.9% of these voters. Thus, the outcome was probably not as expected on two counts: first, the content of the Statute of Autonomy differed from that in the draft passed by the Catalan legislature and, second, voter turnout was disappointingly low. Even worse from the point of view

[8] Note that the Spanish Constitution does not define Spain as a federal State; although, since its inception the way it operates has, on occasion, been referred to as a form of "non-institutional competitive federalism" (see Colomer 2018). This informal (non-institutional) and competitive (absence of horizontal, but also vertical, collaboration) setup appears to have infused the system with instability resulting in the mismatch between Catalonia and its place within the State.

of the Catalan institutions was that in June 2010 the Constitutional Court, in response to an appeal lodged by the right-wing "People's Party", declared 14 articles of the Statute unconstitutional.

Within this political framework, there was a progressive rise in secessionist demands.[9] According to Colomer (2018), Catalan politics shifted from seeking to construct a nation to seeking to construct a State, though he argues that this new stage was led by the political elite and did not reflect the real will of the Catalan people. Notably, he argues the content of the political platforms tended to be more extremist than the demands of the citizens. Yet, according to both this author and to Barrio, Barberà, and Rodríguez-Teruel (2018), the shift in ambition has led to a polarization in Catalonia and to a surge of populism. These populist policies have typically centered around arguments of *optimal* economic outcomes for Catalans if secession is achieved. Muñoz and Tormos (2015) have proposed an interesting empirical framework to attenuate potential biases attributable to this populism as they conduct a robust estimate of the causality running from economic expectations and citizens' preferences for secession. They conclude that while these expectations certainly play a role, the main drivers are national identity and partisanship.

All things considered, what cannot be denied is that there is a conflict between Catalonia and the central government of Spain, a conflict that was originally caused by the unsuccessful attempts to introduce the new Statute as agreed to by the Catalan Parliament. The climax of this conflict occurred in October 2017 when the regional government promoted a referendum for independence, which was declared illegal by the central executive. Both layers of government hold massive stakes in the conflict: on the one hand, the unity of Spain; on the other, the creation of an independent Catalonia. Given what is the potentially enormous value of both of these aims, the transaction costs (in the same way as the costs attributable to the conflict) may well be deemed

[9] To complement the data shown in Figure 2 and to illustrate the magnitude of this "rise", in October 2006, 14% of those surveyed by the CEO were in favor of independence. This figure subsequently rose to 17.4% in Oct. 2008, and then to 25.2% in Oct. 2010, before jumping to 44.3% in Oct. 2012 and 45.3% in Oct. 2014. Statistical source: http://evoluceo.ceo.gencat.cat/ceo/inici/evoluceo.html#/main/evolucio. Thus, from a floor at 14%, support for independence has risen notably since 2010.

irrelevant by both sides to the conflict (Bookman 1993), at least to some degree[10]. We aim at providing a full description of those economic costs, if any. Those costs might be driven through different channels. The conflict might create political deadlocks –within the Catalan legislative, but also due to the absence of negotiations between the Spanish and the Catalan executive –, which might have an impact on the economy (see in a similar context, Albalate and Bel 2019). There might be an impact on the aggregate demand (including on tourism) due to strikes, protests or road blockades (see Skaperdas et al. 2009, section 1.2, for a review). Finally, these conflicts might generate uncertainty as to how and when they will be solved. As we have argued, since the illegal referendum, uncertainty has increased and there are no prospects that the conflict will vanish. Haddow et al. (2013) (see Table A of their paper and the references cited therein), identify several margins that might be affected by a higher level of uncertainty: it might increase precautionary savings by households affecting, thus, the aggregate demand through a lower level of private consumption, or impact the production and investment by firms including the set-up of new ones (Holm, Opper, and Nee 2013), or even create distortions on the financial system. We aim at quantifying the economic impact driven through all these potential margins.

3. Empirical Analysis

3.1. Identification: Synthetic control method

In this section, we use the SCM to test how the 2017 referendum impacted on the Catalan economy. On the one hand, we distinguish between real margins – GDP and unemployment (aggregate demand), the number of new firms registered, and the number of capital increases made by existing corporations (aggregate supply) – and financial margins of response – amount of long- and short-term bank deposits domiciled in Catalonia. All data are in per capita terms.

It is well known that this model is well suited to explain GDP and unemployment. However, for the other margins analyzed it can also be important to take account of

[10] This, for example, is also the stance taken by Galí (2013: 6) when discussing the consequences of independence for the financial system in Catalonia.

time-varying unobservables, as the SCM does[11]. Regarding long-term and short-term deposits, one important time-varying unobservable is uncertainty (see Dreze and Modigliani 1972; Guiso, Jappelli, and Terlizzese 1992; or Hahm and Steigerwald 1999). In the case of Spain, it has been shown that uncertainty variation across regions and over time is an important determinant of regional saving (Bande and Riveiro 2013). For example, the increase in uncertainty after the 2008 recession has forced households to increase precautionary savings, and therefore to lower consumption expenditures. Nevertheless, uncertainty is difficult to identify by using sharp control variables, and so a factor model – taking into account time-varying unobservables, like uncertainty – is particularly appropriate. As regards the determinants of capital increase, they are linked to asymmetric information problems between managers and investors (Myers and Mailuf 1984), which drive the choice between internal and external sources of finance. Profitability, asset tangibility, firm size and growth opportunity are other firm-specific capital structure determinants generally accepted (see Alves and Ferreira 2011; Antoniou, Guney, and Paudyal 2008; De Jong, Kabir, and Nguyen 2008; Harris and Raviv 1991; or Rajan and Zingales 1995). The degree of asymmetric information, asset tangibility and growth opportunity are all unobservable variables that can vary along time and across regions justifying again the adoption of a factor model for the number of capital increases. Finally, location of new firms is determined by many factors; in particular, spill-over knowledge of the location area is very important in the Spanish case (Jofre-Monseny, Marín-Lopez and Viladecans-Marsal 2014). These spillovers depend on various technologies adopted that are specific by geographical area and can vary over time. They are very difficult to control with specific variables; hence, also in this case a factor model controlling for time-varying unobservables can be useful to pick up the spill-over determinants of the location of new firms.

The SCM, first employed by Abadie and Gardeazabal (2003), involves building an "artificial counterfactual" (the synthetic control), which is then compared to the actual treated unit (Abadie, Diamond, and Hainmueller 2015; and for a recent review, see

[11] In particular, note since the pre-treatment outcome is influenced by both unobserved and observed confounders, units with similar values of the outcomes in the pretreatment period are also likely to have similar values of time-varying unobserved confounding factors (Abadie, Diamond, and Hainmueller 2010). This suggests that if the synthetic control balances the treated unit outcome with a weighted combination of control units pretreatment, the time-varying confounding factor component will also be balanced. In particular, with the SCM, the underlying factor model allows to control for unobservables common to all units that have differential impacts on the outcome over time.

Abadie 2019). Specifically, a set of units with the same features as those of the treated unit constitutes a "donor pool" for the construction of this artificial counterfactual. This unit is built as a weighted average of a number of pre-intervention characteristics chosen to resemble those of the treated unit before treatment. The calculated weights are then applied to the outcome variable under analysis during the post-treatment period. Thus, the post-treatment performance of the control group represents the performance of the treated unit as if the real-life intervention had not occurred.

Our analysis fulfills the Stable Unit Treatment Value Assumption (SUTVA) regarding the *consistency of the treatm*ent that is well defined, and it is the date of the referendum coinciding with the start of the Constitutional crisis. The *no interference property* might not always hold, which would bias the conclusions of the empirical analysis if the regions of the donor pool were also affected by the referendum indirectly. To check whether this hypothesis holds in our setting, we will re-run the model by excluding from the donor pool those regions that a priori we suspect they could have been impacted by spill-over effects.

In line with Abadie, Diamond, and Hainmueller (2015), we take a sample of K+1 units, indexed by k, where k =1 is the "case of interest" or the "treated unit", and k = 2... K+1 are the "potential comparisons" making up the donor pool. The units are observed at the same time t periods, t = 1, ... T with given pre- and post-intervention periods. The synthetic control is the weighted average of the units in the donor pool; thus, it is a $(K \times 1)$ vector of weights $W = (w_2, \ldots, W_{K+1})$, with $0 \leq w_k \leq 1$ for $k = 2, \ldots K$ s.t. $w_2 + \ldots + w_{J+1} = 1$. $X_1 = (s \times 1)$ is the vector of the values for the pre-intervention characteristics of the treated unit, and $X_0 = (s \times K)$ is the matrix containing the values of the same variable for all the other units in the donor pool. These variables are chosen on the understanding they are good predictors of the outcome variable within vector X_1 and the pre-intervention values of the outcome variable may themselves be included. A vector of weights, W, is selected so that the size of the difference between the pre-intervention characteristics of the treated unit and those of the units making up the donor pool is minimized. Specifically, the vector W is chosen to minimize the weighted mean square error $(X_1 - X_0W)' V(X_1 - X_0W)$, where V is a diagonal of predictor weights, which reflects the relative importance assigned to the predictor variables when the discrepancy between X_1 and X_0W is measured. We choose the predictor weights V, in line with

Abadie, Diamond, and Hainmueller (2010), by minimizing $(Z_1 - Z_0W(V))' (Z_1 - Z_0W(V))$, where Z_1 is a vector and Z_0 is a matrix of the dependent variable before the treatment for the treated and for the donor groups, respectively.

We then let $Y_1 = (T_1 \times 1)$ be the vector of the post-intervention values of the outcome for the treated unit and let $Y_0 = (T_1 \times K)$ be the matrix containing the values of the same variables for all the units in the donor pool. The synthetic control estimator of the effect of the treatment is given by $(Y_1 - Y_0W)$. Since we construct a synthetic control unit with similar behavior to that of the treated unit in the pre-intervention period, a discrepancy in the outcome variable after the intervention is interpreted as the true effect of the intervention itself.

Compared to a regression-based approach, the SCM has the fundamental advantage of explicitly showing the extent to which each unit contributes to the counterfactual, whereby the weights are data-driven (Abadie, Diamond, and Hainmueller 2010, 2015). Otherwise, comparative case studies, that is, a direct comparison of Catalonia's performance with that of another single region – if this is available, and in any case its selection would not be based on statistical criteria – may not be sufficiently accurate, since it might capture the effects not only of the referendum but also those originating from other shocks that may also have impacted the outcome variable. In fact, in contrast to traditional regression analyses that require large samples and many treatment units, the SCM works well even when there is only one treated unit, as it is in our case (Abadie 2019). Finally, as previously highlighted and justified, in contrast with a difference-in-difference (fixed-effects) model, the underlying factor model allows to control for unobservables common to all units that have differential impacts on the outcome over time (Abadie, Diamond, and Haimueller 2020), thus, potentially dealing with endogeneity from omitted variable bias.

3.2. Data sample and empirical strategy

In applying the SCM, we use all 16 of Spain's Autonomous Communities as the "donor pool". Given the brevity of time elapsed since October 2017, we use high-frequency data. Specifically, we use a panel dataset spanning the quarters from the first

quarter of 2000 to the first quarter of 2019. Thus, we have 77 quarters in total, seven occurring after the referendum. We run six iterations of the SCM, using as outcome variables a combination of real (i.e. to account for demand responses, GDP per capita and unemployment rate, and to account for supply responses, flow of new firms registered in Catalonia and number of capital increases by firms) and financial (monetary amount of bank short and long term deposits in banks domiciled in Catalonia) margins of response.

We use 13 socio-economic variables as predictors of Catalonia's pre-intervention characteristics: labor activity rate, private investment as a share of GDP, the relative importance of the different economic sectors (i.e. GDP share of agriculture, industry, construction and services), population density, and level of education (Table 1). All these variables might influence our demand and supply variables of interest (Table 2). We use the same set of variables for each margin of response. Monetary values are expressed at 2010 prices[12].

[INSERT TABLE 1 AROUND HERE]

[INSERT TABLE 2 AROUND HERE]

In addition, and in line with Abadie, Diamond, and Hainmueller (2010), so as to obtain a more accurate replicate of the "artificial counterfactual" of the treated region before treatment, we also include four lags of the dependent variable before the intervention. Once the synthetic control weights are obtained, they are then applied to the outcome variables for the whole period of analysis to obtain the counterfactual post-treatment behavior of Catalonia. Finally, the synthetic control dependent variable is compared with the corresponding variable for the real Catalonia to test the relevance of the treatment.

4. Basic Results

As stated above, we distinguish between real and financial margins of response, and within the former between the impact on aggregate demand and the impact on proxies of aggregate supply. We begin by analyzing the existence of real responses.

[12] See the Appendix for statistical sources.

4.1 Real margins of response

4.1.1. Aggregate demand

If we focus on GDP per capita, synthetic Catalonia emerges as a combination of the Balearic Islands (weight=11.5%), the AC of Madrid (0.87%), the Basque Country (52.5%) and the AC of Valencia (27.3%). Thus, in terms of GDP per capita, the Basque Country is the region that most closely resembles Catalonia during the pre-treatment period. From Figure 3a, we can conclude that the GDP per capita of synthetic Catalonia does not differ from that of real Catalonia after the referendum, the shaded area representing plus/minus one standard deviation of the difference between synthetic and real Catalonia before the referendum. As such, this area provides an upper and lower limit of an interval within which real Catalonia would fall if it had been included in the pre-treatment period.

After the referendum, just as before, the trajectory of real Catalonia lies inside the shaded area, suggesting there has been no impact on aggregate demand. This finding is confirmed if aggregate demand variables such as the number of transacted dwellings, including the number of mortgages, the number of cars purchased, and the number of tourists visiting Catalonia are analyzed.[13]

[INSERT FIGURE 3a AROUND HERE]

When we analyze the unemployment rate (see Figure 3b), we see that in the pre-treatment period the synthetic Catalonia and real Catalonia are close to each other except the final period prior to the treatment when unemployment in real Catalonia starts decreasing with respect to synthetic Catalonia. The difference in unemployment rate continues to hold even after the referendum, but this cannot be caused by the referendum.

[INSERT FIGURE 3b AROUND HERE]

[13] We found only a very weak, temporary impact of the number of tourists coming from the rest of Spain. This and all the other results cited above are available upon request.

4.1.2. Aggregate supply

4.1.2.1 Number of capital increases

In the case of the number of capital increases per capita, synthetic Catalonia emerges from a different combination of Spanish regions: the AC of Murcia (0.76%), the AC of Madrid (66.2%), Navarre (10.7%) and La Rioja (15.5%). This margin could have a long-term impact on potential GDP provided a decrease in capital increases might reflect a fall in corporate investment.

As Figure 4a shows, both series follow a very similar path prior to the referendum. Note that this variable is cyclical within a year. However, after the referendum, the number of capital increases in real Catalonia drops below the number of increases in synthetic Catalonia, above all in the third and fourth quarters after the referendum. We filtered seasonality by using the quarterly moving average, as it can be seen by Figure 4b the result with Catalonia with a lower number of capital increase than real Catalonia after the referendum continues to hold.

[INSERT FIGURE 4a AROUND HERE]
[INSERT FIGURE 4b AROUND HERE]

The quantitative results are shown in Table 3: seven quarters before the referendum, the number of capital increases per capita lies within the upper/lower bound interval for synthetic Catalonia, indicating that there is no significant difference between real and synthetic Catalonia in this regard. In contrast, in the six quarters after the referendum, the number of capital increases per capita lies outside this interval (highlighted in italics in the table); specifically, it lies below the lower bound, indicating a significant fall in real Catalonia with respect to the benchmark. This negative impact remains evident even at the beginning of 2019.

[INSERT TABLE 3 AROUND HERE]
[INSERT TABLE 4 AROUND HERE]

We computed the percentage point (p.p.) difference between the variation in the path

taken by synthetic Catalonia with respect to the referendum (in percentage terms) and the variation in the path taken by Catalonia with respect to the referendum (in percentage terms) per quarter (Table 4, column 2). The difference was between 8.9 p.p. and 7.4 p.p. in the first two quarters after the referendum, rising to 31.7 p.p. in 2018Q3, 21.9 p.p. in 2018Q4 and 7.7 p.p. in 2019Q1. This indicates that between the referendum and 2019Q1, real Catalonia registered almost 1,200 fewer capital increases than synthetic Catalonia (Table 4, column 3).

4.1.2.2. Registration of new firms

We also analyzed whether the number of new registered firms varied as a consequence of the referendum. To do so, synthetic Catalonia emerged from the following combination of Spanish regions: Andalusia (0.34%), the AC of Madrid (35.4%), Navarre (10.7%), La Rioja (12.2%), the Balearic Islands (32.1%), and Aragón (16.9%).

[INSERT FIGURE 5a AROUND HERE]

As Figure 5a shows, before the referendum, real and synthetic Catalonia followed a very similar trajectory. Here again, this variable is cyclical within each year. The impact of the referendum on this margin, in this instance, is not so clear-cut. After the referendum, the number of new firms registered in per capita terms lies outside the shaded area (representing plus/minus one standard deviation of the difference between synthetic and real Catalonia before the referendum) in the very short-run. In section 5.2, we provide empirical evidence using placebo tests to corroborate causality. Again, we filtered for seasonality by using the quarterly moving average and the result is even more clear-cut (Figure 5b) than when the variable is not filtered.

[INSERT FIGURE 5b AROUND HERE]

Table 5 confirms that real Catalonia lies outside the shaded area immediately after the referendum until 2018Q3. Note, however, that the value of this margin of response is very close to the lower interval bound.

[INSERT TABLE 5 AROUND HERE]

As with the previous supply margin, we computed the percentage point (p.p.) difference between the variation in the path taken by synthetic Catalonia with respect to the referendum (in percentage terms) and the variation in the path taken by Catalonia with respect to the referendum (in percentage terms) per quarter (Table 6, column 2), as well as the absolute loss (Table 6 column 3). The difference ranged between 14.4 p.p., and 5.8 p.p. in 2019Q1. This means that between the referendum and 2019Q1, Catalonia has registered 2,318 fewer new firms than synthetic Catalonia.

[INSERT TABLE 6 AROUND HERE]

4.2. Financial margins of response

We now analyze the financial margins of response by examining the evolution of short- and long-term deposits domiciled in Catalonia before and after the treatment.

4.2.1 Short-term bank deposits

To explain the evolution of short-term bank deposits, whose mobility costs are relatively smaller than those of long-term deposits, synthetic Catalonia is formed from the combination of the following ACs: Canary Islands (37.9%), the AC of Madrid (23.9%), Navarre (23.9%) and the AC of Valencia (19.2%).

As Figure 6 shows, while both series followed a largely similar evolution before the referendum, immediately after there was a sudden and marked fall in short-term deposits p.c. in Catalonia with respect to the synthetic control. This trajectory is also evident in Table 7, where we see that while the p.c. value for real Catalonia is within the upper and lower bounds of synthetic Catalonia before the referendum, it lies well below the lower bound for all quarters after the referendum.

[INSERT FIGURE 6 AROUND HERE]
[INSERT TABLE 7 AROUND HERE]

When we compute the discrepancy in percentage points with respect to the referendum between the variation shown by synthetic Catalonia and that shown by real Catalonia, we find a difference of 27 p.p. in each of the first two quarters after the referendum, falling to 20 p.p. in the first quarter of 2019. This represents a loss in deposits of almost 33 million euros in the first two quarters after the referendum in real Catalonia. Despite a slight recovery, in 2019Q1 the loss due to the referendum was still more than 24 million euros (see Table 8).

[INSERT TABLE 8 AROUND HERE]

4.2.2 Long-term bank deposits

In the case of long-term bank deposits p.c., synthetic Catalonia emerges from a combination of Aragón (30.1%), the AC of Madrid (0.63%), Navarre (12.8%), the AC of Murcia (16.2%), Canary Islands (31.1%), and the Balearic Islands (0.34%).

As Figure 7 shows, the two Catalonias, real and synthetic, followed a very similar evolution before the referendum. After the referendum, however, we detect a small decrease in the p. c. deposits of real Catalonia with respect to those of our synthetic control, causing the evolution of real Catalonia after the referendum to fall outside the constructed margins of confidence. Specifically, for the six quarters after the referendum, the value of deposits in real Catalonia lies outside the interval for synthetic Catalonia (Table 9).

[INSERT FIGURE 7 AROUND HERE]
[INSERT TABLE 9 AROUND HERE]

When we compute the percentage difference in the variation with respect to the referendum between synthetic and real Catalonia, we find a maximum variation in the first two quarters immediately after the referendum between 7 and 6 p.p. (Table 10). The trend is decreasing thereafter reaching 1.2 p.p. in 2019. This means that the loss suffered by real Catalonia with respect to the synthetic control was more than 2,500 million euros in the first two quarters after the referendum, decreasing thereafter to 470 million euros in 2019Q1. These impacts are much less marked than those estimated for

the short-run bank deposits. As we argue in Section 5.4, we cannot in fact conclude that the referendum had an impact on long-run bank deposits.

[INSERT TABLE 10 AROUND HERE]

5. Placebo Experiments[14]

In this section, we run a set of robustness tests to validate our main results. For the margins for which we found some evidence of causality (i.e. all but GDP per capita and unemployment rate), we run AC and time placebo tests, changing, respectively, the treated region and the time of the shock. If we are definitely estimating a causal effect due to the referendum, we do not expect to find any effect in the placebo tests. We also checked if our results are driven by any particular region, excluding regions with a weight greater than zero from the donor pool; we finally replicate the main results by excluding from the predictors the outcome variables measured before the referendum.

5.1 Capital increases

In the case of the number of capital increases, we first estimate the synthetic control for each of the ACs in the donor pool while exposing them to the treatment in 2017Q3. If our benchmark estimate is in fact detecting the causal effect of the referendum, the divergence of the region-specific synthetic controls from the respective data after the treatment date should be considerably smaller than that recorded in the case of Catalonia.

Table 11 shows the results of the placebo experiments by region for the number of capital increases. In what follows, we use the ratio between the post- and pre-treatment of the root mean squared prediction error (RMSPE), where the higher the ratio, the greater the difference between the treated and synthetic units in the post-treatment case with respect to that of the pre-treatment (column 1, Table 11). The RMSPE is equal to the square root of the mean of the square of the difference between the treated and the synthetic control. The second and third columns contain the RMSPE for the pre- and

[14] In the on-line Appendix, we perform a Sparse Synthetic Control as a further robustness test.

post-treatment periods, respectively.

[INSERT TABLE 11 AROUND HERE]

Thus, column 1 of Table 11 quantifies just how closely the region-specific synthetic controls follow the data post-treatment relative to the pre-treatment fit. Catalonia has a ratio greater than one; however, we find that a number of other regions also have a coefficient higher than one: the Balearic Islands, Cantabria, Castile and León, Extremadura, La Rioja and Galicia. A priori, this could be evidence of spill-over effects (Born et al. 2019); yet, in this instance, this does not appear to be an especially credible hypothesis. Technically, however, our assumption that the donor pool countries are unaffected by the treatment is potentially violated. To test the reliability of our results, we therefore restrict the donor pool to just those ACs with a ratio below one. Qualitative results remain unchanged (see Figure 8).

[INSERT FIGURE 8 AROUND HERE]

In Figure 9, we plot the results of different permutations, that is, we consider the possibility that each AC is a treated region. This figure highlights the sign of the difference between the synthetic and the real AC. A ratio between the post- and pre-referendum RMSPEs greater than one may also be due to a higher value for the synthetic than for the real AC, which is exactly the opposite result to the one we obtained when Catalonia was the treated region. Specifically, we plot the difference between the real AC and its corresponding synthetic counter-factual. The bold line corresponds to the difference in the case of Catalonia. The estimated trend for Catalonia is clearly negative post-treatment, and notably lower than the estimated trend for the rest of the ACs. During the pre-treatment period, the series for Catalonia oscillates around zero and, in all circumstances, it does not show a particularly different trend from that of the rest of the ACs. The series estimated for the Balearic Islands and for Cantabria, however, at various post-referendum points, fall below that of Catalonia. Nevertheless, these two ACs present a much higher RMSPE before the treatment than that presented by Catalonia. In other words, in these two cases, the reason why they fall below Catalonia seems to be due to a lack of fit, being the two ACs with the largest pre-treatment RMSPE (see Table 11).

[INSERT FIGURE 9 AROUND HERE]

To test for the adequacy of the treatment quarter, we run another placebo experiment. To do so, we change the treatment quarter to a period before the referendum was held. Specifically, we use 2012Q3, 2010Q1 and 2007Q3. We find no significant impact around the date of these three "fake-referendums" (see Figure 10).

[INSERT FIGURE 10 AROUND HERE]

Following Abadie, Diamond, and Hainmueller (2015) and Bilgel and Karahasan (2019), we check if our results are driven by any particular region, excluding those with a weight greater than zero from the donor pool. Furthermore, given the historical similarities and the pro-independence pressures present also in the Basque country, we excluded this AC from the donor pool as well.

[INSERT FIGURE 11 AROUND HERE]

All results, but excluding the AC of Madrid, confirm the main specification leaving the synthetic Catalonia above the real Catalonia after the referendum, even if the values get closer than when we use all the donors (see Figure 11).

We replicate the main analyses by excluding from the predictors the outcome variables measured before the referendum. As it is shown in Figure 12, results do not change.

[INSERT FIGURE 12 AROUND HERE]

5.2. Registration of new firms

In the case of the registration of new firms, the placebo test running permutations of the treated region confirms that there are other ACs with a ratio between the post- and pre- referendum RMSPEs that is greater than the one we estimate for Catalonia of 1.41 (see Table 12).

We thus re-compute the value for synthetic Catalonia by excluding all regions with a

ratio greater than one, but including Extremadura and Madrid for which a placebo could not be run[15]. In this case, the Balearic Islands (36.3%), La Rioja (28.7%) and the AC of Madrid (35%) make up the synthetic Catalonia. Our qualitative results remain unchanged (see Figure 13).

[INSERT FIGURE 13 AROUND HERE]

[INSERT TABLE 12 AROUND HERE]

[INSERT FIGURE 14 AROUND HERE]

As with the previous margin, Figure 14 shows that after the referendum the difference between the real and synthetic Catalonia is negative, and almost always below the value of the placebos.

[INSERT FIGURE 15 AROUND HERE]

The results of the time placebo tests are shown in Figure 15. Again, around the dates of these alternative treatment quarters (2012Q3, 2010Q1 and 2007Q3), we do not observe any change in the trends between real and synthetic Catalonia.

We also check for this margin if our results are driven by any particular region, excluding those with a weight greater than zero from the donor pool and also the Basque Country. The results relative to the post treatment still hold even if in the pre-treatment there are some quarters where the synthetic Catalonia seems to have a larger value than the real Catalonia (Figure 16).

[INSERT FIGURE 16 AROUND HERE]

We replicate the analyses by excluding from the predictors the outcome variables measured before the referendum. As it is shown in Figure 17, results do not change.

[INSERT FIGURE 17 AROUND HERE]

[15] Given the technical restrictions of the SCM, it makes difficult to obtain good predictions, if any, for extreme units (Doudchenko and Imbens 2016, 9). This is certainly the case of the AC of Madrid (Spain's richest region) and of Extremadura (poorest region).

5.3. Short term bank deposits

Table 13 shows the results of the AC-placebo experiments for short-term bank deposits. Again, it was not possible to run a placebo test for Madrid and for Extremadura.

[INSERT TABLE 13 AROUND HERE]

While Catalonia presents the highest ratio, Aragón and the AC of Valencia also have high values (greater than one). An inspection of Figure 18 shows that the real Aragón and the real AC of Valencia have levels of short-run deposits that are higher than their corresponding synthetic controls after the referendum in Catalonia. Here we cannot reasonably disregard possible spill-over effects from Catalonia to its neighboring regions. In both regions, short-run bank deposits continued to increase in the post-treatment period, although immediately after the referendum the decrease in short-term bank deposits in Catalonia with respect of synthetic Catalonia was much greater than the corresponding increase in Aragón and in the AC of Valencia (Table 14).

[INSERT TABLE 14 AROUND HERE]
[INSERT FIGURE 18 AROUND HERE]

There are other regions that present a ratio greater than one and which might, therefore, violate the assumption requiring the donor pool to be exclusively composed of ACs that are unaffected by the treatment. Thus, here too, we consider restricting the donor pool to only those ACs whose ratio is below one, plus the AC of Madrid and Extremadura. The results are very similar to those obtained with our baseline specification (see Figure 6). Synthetic Catalonia now emerges from a combination of the Canary Islands (55.4%), the AC of Madrid (27.8%) and La Rioja (16.8%).

[INSERT FIGURE 19 AROUND HERE]

The results in Figure 19 are confirmed by Figure 20, where we can see that after the referendum the difference between the synthetic and the real Catalonia is much larger than for any of the other placebos.

[INSERT FIGURE 20 AROUND HERE]

As in the other margins, we also run placebo tests to check whether the results are contingent on the treatment quarter. Hence, we run the baseline specification considering 2012Q3, 2010Q1 and 2007Q3 as the alternative treatment quarters. Figure 21 clearly shows that following these alternative treatment quarters there is no difference between the real and the synthetic Catalonia.

[INSERT FIGURE 21 AROUND HERE]

Finally, we check if our results are driven by any particular region. The results do not change evidencing the sharp decrease in short term deposits after the referendum (see Figure 22).

[INSERT FIGURE 22 AROUND HERE]

We replicate estimates by excluding from the predictors the outcome variables measured before the referendum. As it is shown in Figure 23, results do not change either.

[INSERT FIGURE 23 AROUND HERE]

5.4. Long-term bank deposits

In the case of this margin, the ratio presented by Catalonia is not the largest; in fact, it is below one (0.52) (Table 15). Hence, we are unable to conclude that the referendum had a causal effect on the path of long-term deposits after the treatment quarter, and so we do not perform a robustness test for this margin.

[INSERT TABLE 15 AROUND HERE]

6. Conclusions

The intensification of the conflict between Catalonia and the central government after the referendum offers an opportunity to test the economic impact of a secessionist conflict. Importantly, while these tensions have been present for a period of years, they came to an obvious head in October 2017 following the holding of an illegal referendum in Catalonia, promoted by the Catalan executive. Given this referendum was not negotiated with the Central executive, levels of political and social conflict have, as predicted and as subsequent events have shown, increased. In short, the referendum did not mark the end of the process, but a sharp increase in the conflict leading to a Constitutional crisis.

Using a SCM, we have shown that the referendum has not only affected real variables on the supply side, including the number of corporate capital increases and the number of new firms being registered, but also the key financial variable of the amount of short-term bank deposits. Specifically, we have shown that in Catalonia the number of capital increases one year after the referendum decreased with respect to those in synthetic Catalonia by 1,200 and that the number of registered firms fell by 2,318. However, the most striking and, at the same time, the most robust result concerns the performance of short-term deposits following the referendum: there being a massive outflow of money from Catalonia. In the first quarter after the referendum, these deposits fell in value with respect to those for synthetic Catalonia by 33 million euros, and one year after the referendum they were still 24 million euros below those calculated for synthetic Catalonia. This is a clear warning of how sensitive capitalists are to uncertainty cause by a conflict. Such behavior could at least temporarily cause liquidity problems to the Catalan financial entities[16].

While in the short run we do not observe real economic impacts on the demand side as a consequence of the peak in the conflict, the real impacts detected on the supply side point to negative effects in the potential output of Catalonia in the medium/long run. Our framework of analysis – having distinguished between the impact on the aggregate demand and on the aggregate supply – allows to identify these potential economic

[16] In September 2020 a private foundation (https://www.11onze.cat) was launched to set up a Catalonian bank to be registered in an EU country different from Spain. The reason for this is that "one of the main tools for an independent state to be operative is the banking system, in particular, a financial entity that can control the flows of revenue and guarantee the correct functioning of the economy" (https://www.elmon.cat/economia/neix-onze-banc-catala-fora-abast-madrid-espanya_2135307102.html).

negative effects in the medium/long run even if we do not dispose of a longer time span after the treatment year. All in all, under a low-level conflict as the one we have analyzed, the impact seems more subtle with respect to more intense conflicts. While we do not see any impact on aggregate demand (captured by GDP p.c. or the unemployment rate), the estimated reaction of firms (supply side) still points to potential negative economic effects.

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ON-LINE APPENDIX: Further Robustness Test: Sparse Synthetic Control

By means of the Sparse Synthetic Control, it is possible to evaluate the trade-off between sparsity and goodness of fit in the choice of the number of regions that contribute to the synthetic control. Following Abadie, Diamond, and Hainmueller (2015) and Bilgel and Karahasan (2019), given *k* regions in the donor pool with weight greater than zero, for each specification we construct synthetic controls for Catalonia with a sequential combinations of *k, k-1, k-2, ...* and a single-control region. For each l = *k, k-1, k-2, ...1* combination, we choose the one that minimizes the RMSPE before the treatment.

Regarding the number of capital increases p.c., we start dropping one region, the AC of Murcia, from the donor pool with a weight greater than zero, driving us to a combination of three donors which compared to all possible other combinations is the one releasing the lowest RMSPE before the treatment. By using the same technique, we then run the estimate by dropping the two regions Murcia and Navarre, and finally we use only one counterfactual region, the AC of Madrid (see Table A.1).

[INSERT TABLE A.1 AROUND HERE]

In all three cases, when we reduce the number of regions contributing to the synthetic control, the main result holds even if when we build up the synthetic control by using only one region in the pretreatment period the synthetic Catalonia seems to diverge a bit from the real one (Figure A.1).

[INSERT FIGURE A.1 AROUND HERE]

When we look at registration of new firms and use four regions, we drop Aragón; in the case of three regions, we drop Andalusia and La Rioja; in the case of two regions, we use the AC of Madrid and Andalusia; with only one region, we use Balearic Islands (see Table A.2).

[INSERT TABLE A.2 AROUND HERE]

The main result still holds with four and with three regions. It seems to be weaker with

two regions and it holds with one region even if during the pretreatment period the synthetic Catalonia remains sometimes a bit distant from the real one (see Figure A.2).

[INSERT FIGURE A.2 AROUND HERE]

Finally, we look at short-run deposits for which we built up synthetic control with three regions dropping the AC of Valencia, we then have only two regions dropping the AC of Madrid and the AC of Valencia and finally only one region, Navarre (see Table A.3).

[INSERT TABLE A.3 AROUND HERE]

With three regions, the main result holds, with two and one region the post-treatment results holds; however, for some time spans during the pre-treatment period the synthetic Catalonia diverges from the real Catalonia (see Figure A.3).

[INSERT FIGURE A.3 AROUND HERE]

All in all, we conclude the sparse treatment synthetic control tests confirm our main results.

APPENDIX: Statistical Sources of Data

Variable	Source	Period	WEB Source
MARGINS OF RESPONSE			
GDP per capita	AIReF	2000-2019(1)	https://www.airef.es/es/la-airef-publica-la-estimacion-del-segundo-trimestre-de-la-composicion-por-ccaa-del-pib-nacional/
Short- and long-run bank deposits	BdE (BIEST)	2000-2019(1)	http://app.bde.es/bie_www/faces/bie_wwwias/jsp/op/Home/pHome.jsp
New registered corporations	INE	2008-2019(1)	https://www.ine.es/jaxiT3/Tabla.htm?t=13913
Number of capital increases	INE	2008-2019(1)	https://www.ine.es/jaxiT3/Tabla.htm?t=13914
COVARIATES			
Level of education	FEDEA	2000-2016	https://www.fedea.net/datos-economia-regional-y-urbana/
Private investment	INE	2000-2016	https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736167628&menu=resultados&idp=1254735576581
Labor activity rate	INE	2000-2019	https://www.ine.es/jaxiT3/Tabla.htm?t=4218&L=0
Relative importance of sector of economic activity	INE	2000-2019	https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736167628&menu=resultados&idp=1254735576581

The Economic Costs of a Secessionist Conflict: The Case of Catalonia*

Abstract. Due to the pro-independence demands of part of its electorate, the political fit of Catalonia within Spain has given rise to notable political tensions over the last few years. This conflict has progressively affected several dimensions of Catalan society, including, potentially, the economy. The illegal referendum on independence, held in October 2017, marked the climax of political and social tensions, leading to a Constitutional crisis and further stoking the conflict as opposed to offering any hope of an early resolution. We analyze a complete set of margins potentially affected by the referendum, including real (aggregate demand and supply) and financial responses. Using a synthetic control method, we find strong evidence of the outflow of short-term bank deposits after the referendum; while, on the real side, we find evidence of responses in aggregate supply (number of capital increases and number of new firms registered).

Keywords: Referendum, Independence, Synthetic Control Method, Capital Flows, Economic Uncertainty

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1. Introduction

Over the last 15 years, Catalan politics have been framed within what has been almost constant conflict with the central government. The origins of this conflict are diverse. The restoration of democracy in Spain in 1977 saw the creation of the so-called "Autonomous Communities" (henceforth ACs) and although this meant a relatively major decentralization of expenditure and tax responsibilities (Lago-Peñas, Fernández-Leiceaga, and Vaquero-García 2017), successive governments in Catalonia have called for a special status – similar to that of the Basque Country and Navarre, as explicitly recognized in the Constitution – and better financial treatment in the regional financing system. However, these demands have yet to be met resulting in today's severe social and political conflict. While several authors have analyzed the social and political nature of this conflict, here we aim to quantify its economic impact.

More specifically, by employing a synthetic control method (SCM), we analyze several economic margins that may have been affected by the conflict. The conflict can be considered to have peaked on October 1, 2017, when the Catalan executive organized a unilateral referendum on independence, a poll that was considered illegal by the central executive. Up to then, the channels of political communication between both executives were open, albeit unsuccessfully. The illegality committed by the Catalan executive, though, completely broke those channels making unlikely a quiet solution to the political conflict, leading to a Constitutional crisis. And this is still the case; due to the illegal referendum, no solution is foreseen in the short or even in the medium run. Although the (regional and national) police intervened at some polling stations to prevent it from going ahead, 43.03% of the electorate voted, with 90.18% of these votes being cast in favor of independence.[1] Thus, our post-treatment period is the fourth quarter of 2017 onwards.

Although the origins of the conflict can be traced back, as we said, to at least 15 years before that date, the impact on *economic uncertainty* was especially great in 2017 and concentrated particularly in the AC of Catalonia. This impact is illustrated in Figure 1 below, which shows how often the expression "economic uncertainty" (i.e. number of

[1] Given the illegal nature of the referendum, these data are unofficial. For more details, see: https://en.wikipedia.org/wiki/2017_Catalan_independence_referendum

references to this search term) appeared in Spanish newspapers in relation to a specific AC during the 2014-19 period (unfortunately, data is not available per quarter). The search was performed using *Factiva*. Given the large number of zeros at the beginning of the period (and none in 2019), we reparametrized the search results for each region so as to have a value of 100 in 2019. In the case of Catalonia (thick dashed line), a peak occurs in 2017. Specifically, there were 22, 30, 36, **322**, 104 and 58 news items linking Catalonia and "economic uncertainty" in 2014, 2015, 2016, **2017**, 2018, and 2019 (to the end of June), respectively.[2] For the rest of the regions, 2017 was not a notable year in this respect, with the exceptions of the Balearic Islands (8 news items) and Madrid (218 news items, a figure that is some distance from the number recorded for Catalonia). This exercise provides initial evidence of the adequacy of the treatment year – note, though, we will work with quarterly data – chosen for Catalonia and of the singular nature of Catalonia (treated region). In the empirical analysis, however, we run placebo tests to corroborate these initial impressions.

[FIGURE 1 AROUND HERE]

Unlike, for example, the Brexit vote (Born et al. 2019), the referendum held in Catalonia was, according to Spanish legislation, illegal. This explains why our focus is on the economic costs of the conflict in Catalonia, rather than on the economic costs of (a potential) secession. In fact, after the referendum, if anything, the likelihood of (a negotiated) secession diminished and the expected duration of the conflict increased. This interpretation is confirmed by the sequence of events that occurred in the three weeks immediately following the referendum: the King of Spain's televised address two days after the referendum,[3] in which he called on citizens to respect the rule of law and the unity of Spain; the imprisonment of the civil leaders of the referendum on October 15 accused of insurrection; and the decision taken by the cabinet of the central government to implement "Article 155" of the Constitution, which allows the national government to take over the running of an AC, including its finances and police, on October 27, the same day as the Catalan parliament passed a unilateral declaration of

[2] The full dataset is available upon request.

[3] https://edition.cnn.com/2017/10/03/europe/catalonia-general-strike-protests-barcelona/index.html

independence.[4,5] These decisions, caused as a consequence of the referendum, together with the police intervention in an attempt at thwarting the referendum, are clear indications that, regardless of the result, the illegal referendum did not end the conflict but, on the contrary, that it would now be more intense and prolonged after October 1, 2017. Thus, the illegal referendum is the proxy we use to account for the intensity of the conflict, whose solution since then on is far from being foreseen and the channels of communication between the agents involved have collapsed[6] as a result of the Constitutional crisis.

To provide an idea of the support for secession and of the evolution of that support, Figure 2 shows the percentage support for Catalan independence since March 2015. According to official data collected by the Government's survey institute, support has never risen above 50% (Fig. 2), and while a causal relationship cannot be derived from this time series, pro-independence sentiment peaked precisely in October 2017.

[FIGURE 2 AROUND HERE]

In our empirical setting, to identify the costs of the conflict, we distinguish between real margins of response (differentiating at the same time between the impact on aggregate demand and that on aggregate supply) and financial margins of response. The evidence of a causal impact is particularly incisive for this latter margin. Following the events of October 2017, a significant amount of short- and long-term bank deposits took flight from Catalonia – 36 million euros during the first quarter immediately after the referendum, of which 33 million were short-term deposits (but, note, we are only able to

[4] See the official explanation published by the Spanish Foreign Office on the application of direct rule: http://www.exteriores.gob.es/Embajadas/HELSINKI/EN/Noticias/Documents/The%20application%20of%20Article%20155%20of%20the%20Spanish%20Constitution.pdf

[5] On October 10, the president of the Catalan executive formally recognized the results of the referendum and declared independence, but in the same speech, he went on to say that this declaration would be put on hold in the belief that this would favor negotiations with central government, a meeting which would never take place.

[6] The regional financing system is negotiated between the central executive and the ACs' executives (excluding the Basque Country and Navarre) by means of a multilateral institutional forum so-called "Consejo de Política Fiscal y Financiera". The Catalonian Minister of Finance attended the meetings on 7/30/2015, on 12/1/2016, and even a few months before the referendum, on 6/29/2017. However, he did not attend the meeting held on 8/22/2018; on 2/7/2020 the Catalonian executive sent a top official. Thus, we think this exemplifies well that the channels of communication have collapsed as a consequence of the referendum.

provide robust causal evidence for this latter type of deposit). In the case of real margins of response, on the demand side, GDP per capita and unemployment were unaffected by the conflict; however, we do detect an impact on the supply side. Specifically, a year and a half after the referendum, we estimate a 2,318 reduction in the number of new firms registered in Catalonia with respect to those that would have been created in a synthetic Catalonia without the referendum. Likewise, a study of the number of capital increases in Catalonia shows there to have been 1,200 fewer corporation capital increases with respect to those in synthetic Catalonia a year and a half after the referendum. As such, supply, in general, seems to be particularly reactive to the intensification of the secessionist conflict as a consequence of the illegal referendum. This means that, despite the negligible short-term impact on GDP per capita, the impact on aggregate supply could limit the potential output of the Catalan economy in the medium/long-term, particularly, if the conflict persists. We think this result is particularly important.

According to Skaperdas et al.'s (2009) classification of conflicts, the one we analyze is under the category of low-level conflicts (strikes, protests or road blockades, basically); however, it could still have disruptive impacts on the economy. The economic impact of disruptions caused by low-level conflicts, though, have hardly been estimated. In this vein, Sun and Yu (2020) have recently estimated – also using the SCM – the economic costs of Tibetan demand for secession from China taking advantage of the 1987-1989 Tibetan Unrests. They estimate GDP p.c. would have been 27% larger in the 1988-2007 period in absence of those unrests[7]. The paper by Abadie and Gardeazábal (2003) is also closely related to ours, as deals with a conflict in Spain, it stems from a demand for independence and also uses SCM – a technique that they pioneered –, but the nature of the conflict is quite distinct, as it involves terrorism. The authors estimated that GDP p.c. in the Basque Country was 10% lower than it would have been in the absence of terrorism in the period 1975 to 1995. Although in both cases, the start of the conflict dates back, they both identify particular moments of time – when a series of riots and uprisings hold due to the actions of Tibetan separatists or when the terrorist activity becomes a large-scale phenomenon in the Basque Country, respectively – to estimate

[7] The papers by Riascos and Vargas (2011) or Evia, Laserna, and Skaperdas (2008) estimate the impact of low-level conflicts using different methodologies from SCM, but in contrast with our analysis the origin of those conflicts is not separatism.

the costs of a secessionist conflict. In our case, we take advantage of an illegal referendum, whose occurrence was, as in those papers, independent of the state of the economy.

Our paper is also related to Galasso (2019), in which the author links several shocks that have occurred since 2006 in Catalonia to the stock exchange valuation of Catalan and Spanish firms. He argues that, depending on estimates of the positive or negative impact of the corresponding shock on that valuation, one can infer whether the market positively or negatively assesses the prospects of more decentralization or even of the increased chances of Catalonia becoming independent. According to this empirical framework, the referendum was found to have a strong negative effect on all Catalan firms and on Spanish ones in the tradable sector.

Finally, our paper is also related to the theoretical literature linking decentralization and the aspirations for secession. Cederman, Hug, and Wucherpfennig (2015) and Madiès et al. (2018) discuss the impact of a higher level of decentralization on secession: is it a peaceful solution for autonomy? Or, on the contrary, as it seems to be the case of Catalonia, thanks to the greater possibilities afforded by autonomy, does it fuel separatist campaigns? Gibilisco (2019) has developed a dynamic model of center-periphery relations, from which a trade-off emerges for the central government: repression prevents today's mobilization *but* increases grievances and so greater expectations of peripheral mobilization in the future. Bordignon and Brusco (2001) analyze the optimality of secession rules. While ex-post they are optimal (to avoid large costs of a conflict), they might not be so ex-ante as long as this could precisely incentivize secession demands. In any case, in absence of the "right to secede" and given a level of decentralization, the equilibrium outcome is Pareto inefficient: a political and social conflict. That is, both the regions and the central government would be better off if both actors sought to scale down the conflict (see, for example, Spolaore, 2008). For a detailed discussion, see the recent review on this topic by Kimbrough, Laughren, and Sheremeta (2020).

The rest of the paper is organized as follows. In Section 2, we explain the roots of the conflict between Catalonia and Spain. In Section 3, we describe our empirical methodology for estimating these costs. Section 4 presents the main results and Section

5 present the results of placebo experiments designed to check the robustness of our basic results. Section 6 concludes.

2. Social Conflict due to Secessionist Aspirations: The Catalan Case

As discussed in the Introduction, since the restoration of democracy, the fit of Catalonia within democratic Spain has been far from smooth. Almost all Catalan executives have sought to achieve a special status for Catalonia within Spain, the most common demand being that it be recognized as a nation within the State.[8] This demand, together with others for greater self-government (including tax administration, language policy and judiciary system), generated a political consensus in Catalonia to draft a new "Statute of Autonomy" to replace the one passed in 1979. As we show below, some commentators consider that the failure to introduce this new Statute marked the beginning of the end of the peaceful coexistence of Catalonia within Spain (Castells 2014), a conflict that was to climax in 2017 giving rise to a Constitutional crisis.

With the explicit backing of the then president of the Spanish executive, José Luís Rodríguez Zapatero, the Catalan parliament passed a new draft of the Statute in 2005 with the support of 120 of its 135 members. However, two more steps had to be completed before its formal adoption: the new Statute had to be agreed to by the central legislature and, then, put to a referendum in Catalonia. The bill was approved by Spain's lower house in March 2006 and by the upper house a month and a half later. However, this new draft was not exactly the same as the one the Catalan parliament had voted on. Following negotiations led by Zapatero with Catalan leaders, key matters, including the recognition of Catalonia as a nation, were excluded before it was put to the vote in the lower house. In the referendum held in Catalonia in June 2006, turnout was low (49.41%) but the Statute received the support of 73.9% of these voters. Thus, the outcome was probably not as expected on two counts: first, the content of the Statute of Autonomy differed from that in the draft passed by the Catalan legislature and, second, voter turnout was disappointingly low. Even worse from the point of view

[8] Note that the Spanish Constitution does not define Spain as a federal State; although, since its inception the way it operates has, on occasion, been referred to as a form of "non-institutional competitive federalism" (see Colomer 2018). This informal (non-institutional) and competitive (absence of horizontal, but also vertical, collaboration) setup appears to have infused the system with instability resulting in the mismatch between Catalonia and its place within the State.

of the Catalan institutions was that in June 2010 the Constitutional Court, in response to an appeal lodged by the right-wing "People's Party", declared 14 articles of the Statute unconstitutional.

Within this political framework, there was a progressive rise in secessionist demands.[9] According to Colomer (2018), Catalan politics shifted from seeking to construct a nation to seeking to construct a State, though he argues that this new stage was led by the political elite and did not reflect the real will of the Catalan people. Notably, he argues the content of the political platforms tended to be more extremist than the demands of the citizens. Yet, according to both this author and to Barrio, Barberà, and Rodríguez-Teruel (2018), the shift in ambition has led to a polarization in Catalonia and to a surge of populism. These populist policies have typically centered around arguments of *optimal* economic outcomes for Catalans if secession is achieved. Muñoz and Tormos (2015) have proposed an interesting empirical framework to attenuate potential biases attributable to this populism as they conduct a robust estimate of the causality running from economic expectations and citizens' preferences for secession. They conclude that while these expectations certainly play a role, the main drivers are national identity and partisanship.

All things considered, what cannot be denied is that there is a conflict between Catalonia and the central government of Spain, a conflict that was originally caused by the unsuccessful attempts to introduce the new Statute as agreed to by the Catalan Parliament. The climax of this conflict occurred in October 2017 when the regional government promoted a referendum for independence, which was declared illegal by the central executive. Both layers of government hold massive stakes in the conflict: on the one hand, the unity of Spain; on the other, the creation of an independent Catalonia. Given what is the potentially enormous value of both of these aims, the transaction costs (in the same way as the costs attributable to the conflict) may well be deemed

[9] To complement the data shown in Figure 2 and to illustrate the magnitude of this "rise", in October 2006, 14% of those surveyed by the CEO were in favor of independence. This figure subsequently rose to 17.4% in Oct. 2008, and then to 25.2% in Oct. 2010, before jumping to 44.3% in Oct. 2012 and 45.3% in Oct. 2014. Statistical source: http://evoluceo.ceo.gencat.cat/ceo/inici/evoluceo.html#/main/evolucio. Thus, from a floor at 14%, support for independence has risen notably since 2010.

irrelevant by both sides to the conflict (Bookman 1993), at least to some degree[10]. We aim at providing a full description of those economic costs, if any. Those costs might be driven through different channels. The conflict might create political deadlocks –within the Catalan legislative, but also due to the absence of negotiations between the Spanish and the Catalan executive –, which might have an impact on the economy (see in a similar context, Albalate and Bel 2019). There might be an impact on the aggregate demand (including on tourism) due to strikes, protests or road blockades (see Skaperdas et al. 2009, section 1.2, for a review). Finally, these conflicts might generate uncertainty as to how and when they will be solved. As we have argued, since the illegal referendum, uncertainty has increased and there are no prospects that the conflict will vanish. Haddow et al. (2013) (see Table A of their paper and the references cited therein), identify several margins that might be affected by a higher level of uncertainty: it might increase precautionary savings by households affecting, thus, the aggregate demand through a lower level of private consumption, or impact the production and investment by firms including the set-up of new ones (Holm, Opper, and Nee 2013), or even create distortions on the financial system. We aim at quantifying the economic impact driven through all these potential margins.

3. Empirical Analysis

3.1. Identification: Synthetic control method

In this section, we use the SCM to test how the 2017 referendum impacted on the Catalan economy. On the one hand, we distinguish between real margins – GDP and unemployment (aggregate demand), the number of new firms registered, and the number of capital increases made by existing corporations (aggregate supply) – and financial margins of response – amount of long- and short-term bank deposits domiciled in Catalonia. All data are in per capita terms.

It is well known that this model is well suited to explain GDP and unemployment. However, for the other margins analyzed it can also be important to take account of

[10] This, for example, is also the stance taken by Galí (2013: 6) when discussing the consequences of independence for the financial system in Catalonia.

time-varying unobservables, as the SCM does[11]. Regarding long-term and short-term deposits, one important time-varying unobservable is uncertainty (see Dreze and Modigliani 1972; Guiso, Jappelli, and Terlizzese 1992; or Hahm and Steigerwald 1999). In the case of Spain, it has been shown that uncertainty variation across regions and over time is an important determinant of regional saving (Bande and Riveiro 2013). For example, the increase in uncertainty after the 2008 recession has forced households to increase precautionary savings, and therefore to lower consumption expenditures. Nevertheless, uncertainty is difficult to identify by using sharp control variables, and so a factor model – taking into account time-varying unobservables, like uncertainty – is particularly appropriate. As regards the determinants of capital increase, they are linked to asymmetric information problems between managers and investors (Myers and Mailuf 1984), which drive the choice between internal and external sources of finance. Profitability, asset tangibility, firm size and growth opportunity are other firm-specific capital structure determinants generally accepted (see Alves and Ferreira 2011; Antoniou, Guney, and Paudyal 2008; De Jong, Kabir, and Nguyen 2008; Harris and Raviv 1991; or Rajan and Zingales 1995). The degree of asymmetric information, asset tangibility and growth opportunity are all unobservable variables that can vary along time and across regions justifying again the adoption of a factor model for the number of capital increases. Finally, location of new firms is determined by many factors; in particular, spill-over knowledge of the location area is very important in the Spanish case (Jofre-Monseny, Marín-Lopez and Viladecans-Marsal 2014). These spillovers depend on various technologies adopted that are specific by geographical area and can vary over time. They are very difficult to control with specific variables; hence, also in this case a factor model controlling for time-varying unobservables can be useful to pick up the spill-over determinants of the location of new firms.

The SCM, first employed by Abadie and Gardeazabal (2003), involves building an "artificial counterfactual" (the synthetic control), which is then compared to the actual treated unit (Abadie, Diamond, and Hainmueller 2015; and for a recent review, see

[11] In particular, note since the pre-treatment outcome is influenced by both unobserved and observed confounders, units with similar values of the outcomes in the pretreatment period are also likely to have similar values of time-varying unobserved confounding factors (Abadie, Diamond, and Hainmueller 2010). This suggests that if the synthetic control balances the treated unit outcome with a weighted combination of control units pretreatment, the time-varying confounding factor component will also be balanced. In particular, with the SCM, the underlying factor model allows to control for unobservables common to all units that have differential impacts on the outcome over time.

Abadie 2019). Specifically, a set of units with the same features as those of the treated unit constitutes a "donor pool" for the construction of this artificial counterfactual. This unit is built as a weighted average of a number of pre-intervention characteristics chosen to resemble those of the treated unit before treatment. The calculated weights are then applied to the outcome variable under analysis during the post-treatment period. Thus, the post-treatment performance of the control group represents the performance of the treated unit as if the real-life intervention had not occurred.

Our analysis fulfills the Stable Unit Treatment Value Assumption (SUTVA) regarding the *consistency of the treatm*ent that is well defined, and it is the date of the referendum coinciding with the start of the Constitutional crisis. The *no interference property* might not always hold, which would bias the conclusions of the empirical analysis if the regions of the donor pool were also affected by the referendum indirectly. To check whether this hypothesis holds in our setting, we will re-run the model by excluding from the donor pool those regions that a priori we suspect they could have been impacted by spill-over effects.

In line with Abadie, Diamond, and Hainmueller (2015), we take a sample of K+1 units, indexed by k, where k =1 is the "case of interest" or the "treated unit", and k = 2... K+1 are the "potential comparisons" making up the donor pool. The units are observed at the same time t periods, t = 1, ... T with given pre- and post-intervention periods. The synthetic control is the weighted average of the units in the donor pool; thus, it is a $(K \times 1)$ vector of weights $W = (w_2, \ldots, W_{K+1})$, with $0 \leq w_k \leq 1$ for $k = 2, \ldots K$ s.t. $w_2 + \ldots + w_{J+1} = 1$. $X_1 = (s \times 1)$ is the vector of the values for the pre-intervention characteristics of the treated unit, and $X_0 = (s \times K)$ is the matrix containing the values of the same variable for all the other units in the donor pool. These variables are chosen on the understanding they are good predictors of the outcome variable within vector X_1 and the pre-intervention values of the outcome variable may themselves be included. A vector of weights, W, is selected so that the size of the difference between the pre-intervention characteristics of the treated unit and those of the units making up the donor pool is minimized. Specifically, the vector W is chosen to minimize the weighted mean square error $(X_1 - X_0W)' V(X_1 - X_0W)$, where V is a diagonal of predictor weights, which reflects the relative importance assigned to the predictor variables when the discrepancy between X_1 and X_0W is measured. We choose the predictor weights V, in line with

Abadie, Diamond, and Hainmueller (2010), by minimizing $(Z_1 - Z_0W(V))' (Z_1 - Z_0W(V))$, where Z_1 is a vector and Z_0 is a matrix of the dependent variable before the treatment for the treated and for the donor groups, respectively.

We then let $Y_1 = (T_1 \times 1)$ be the vector of the post-intervention values of the outcome for the treated unit and let $Y_0 = (T_1 \times K)$ be the matrix containing the values of the same variables for all the units in the donor pool. The synthetic control estimator of the effect of the treatment is given by $(Y_1 - Y_0W)$. Since we construct a synthetic control unit with similar behavior to that of the treated unit in the pre-intervention period, a discrepancy in the outcome variable after the intervention is interpreted as the true effect of the intervention itself.

Compared to a regression-based approach, the SCM has the fundamental advantage of explicitly showing the extent to which each unit contributes to the counterfactual, whereby the weights are data-driven (Abadie, Diamond, and Hainmueller 2010, 2015). Otherwise, comparative case studies, that is, a direct comparison of Catalonia's performance with that of another single region – if this is available, and in any case its selection would not be based on statistical criteria – may not be sufficiently accurate, since it might capture the effects not only of the referendum but also those originating from other shocks that may also have impacted the outcome variable. In fact, in contrast to traditional regression analyses that require large samples and many treatment units, the SCM works well even when there is only one treated unit, as it is in our case (Abadie 2019). Finally, as previously highlighted and justified, in contrast with a difference-in-difference (fixed-effects) model, the underlying factor model allows to control for unobservables common to all units that have differential impacts on the outcome over time (Abadie, Diamond, and Haimueller 2020), thus, potentially dealing with endogeneity from omitted variable bias.

3.2. Data sample and empirical strategy

In applying the SCM, we use all 16 of Spain's Autonomous Communities as the "donor pool". Given the brevity of time elapsed since October 2017, we use high-frequency data. Specifically, we use a panel dataset spanning the quarters from the first

quarter of 2000 to the first quarter of 2019. Thus, we have 77 quarters in total, seven occurring after the referendum. We run six iterations of the SCM, using as outcome variables a combination of real (i.e. to account for demand responses, GDP per capita and unemployment rate, and to account for supply responses, flow of new firms registered in Catalonia and number of capital increases by firms) and financial (monetary amount of bank short and long term deposits in banks domiciled in Catalonia) margins of response.

We use 13 socio-economic variables as predictors of Catalonia's pre-intervention characteristics: labor activity rate, private investment as a share of GDP, the relative importance of the different economic sectors (i.e. GDP share of agriculture, industry, construction and services), population density, and level of education (Table 1). All these variables might influence our demand and supply variables of interest (Table 2). We use the same set of variables for each margin of response. Monetary values are expressed at 2010 prices[12].

[INSERT TABLE 1 AROUND HERE]

[INSERT TABLE 2 AROUND HERE]

In addition, and in line with Abadie, Diamond, and Hainmueller (2010), so as to obtain a more accurate replicate of the "artificial counterfactual" of the treated region before treatment, we also include four lags of the dependent variable before the intervention. Once the synthetic control weights are obtained, they are then applied to the outcome variables for the whole period of analysis to obtain the counterfactual post-treatment behavior of Catalonia. Finally, the synthetic control dependent variable is compared with the corresponding variable for the real Catalonia to test the relevance of the treatment.

4. Basic Results

As stated above, we distinguish between real and financial margins of response, and within the former between the impact on aggregate demand and the impact on proxies of aggregate supply. We begin by analyzing the existence of real responses.

[12] See the Appendix for statistical sources.

4.1 Real margins of response

4.1.1. Aggregate demand

If we focus on GDP per capita, synthetic Catalonia emerges as a combination of the Balearic Islands (weight=11.5%), the AC of Madrid (0.87%), the Basque Country (52.5%) and the AC of Valencia (27.3%). Thus, in terms of GDP per capita, the Basque Country is the region that most closely resembles Catalonia during the pre-treatment period. From Figure 3a, we can conclude that the GDP per capita of synthetic Catalonia does not differ from that of real Catalonia after the referendum, the shaded area representing plus/minus one standard deviation of the difference between synthetic and real Catalonia before the referendum. As such, this area provides an upper and lower limit of an interval within which real Catalonia would fall if it had been included in the pre-treatment period.

After the referendum, just as before, the trajectory of real Catalonia lies inside the shaded area, suggesting there has been no impact on aggregate demand. This finding is confirmed if aggregate demand variables such as the number of transacted dwellings, including the number of mortgages, the number of cars purchased, and the number of tourists visiting Catalonia are analyzed.[13]

[INSERT FIGURE 3a AROUND HERE]

When we analyze the unemployment rate (see Figure 3b), we see that in the pre-treatment period the synthetic Catalonia and real Catalonia are close to each other except the final period prior to the treatment when unemployment in real Catalonia starts decreasing with respect to synthetic Catalonia. The difference in unemployment rate continues to hold even after the referendum, but this cannot be caused by the referendum.

[INSERT FIGURE 3b AROUND HERE]

[13] We found only a very weak, temporary impact of the number of tourists coming from the rest of Spain. This and all the other results cited above are available upon request.

4.1.2. Aggregate supply

4.1.2.1 Number of capital increases

In the case of the number of capital increases per capita, synthetic Catalonia emerges from a different combination of Spanish regions: the AC of Murcia (0.76%), the AC of Madrid (66.2%), Navarre (10.7%) and La Rioja (15.5%). This margin could have a long-term impact on potential GDP provided a decrease in capital increases might reflect a fall in corporate investment.

As Figure 4a shows, both series follow a very similar path prior to the referendum. Note that this variable is cyclical within a year. However, after the referendum, the number of capital increases in real Catalonia drops below the number of increases in synthetic Catalonia, above all in the third and fourth quarters after the referendum. We filtered seasonality by using the quarterly moving average, as it can be seen by Figure 4b the result with Catalonia with a lower number of capital increase than real Catalonia after the referendum continues to hold.

[INSERT FIGURE 4a AROUND HERE]
[INSERT FIGURE 4b AROUND HERE]

The quantitative results are shown in Table 3: seven quarters before the referendum, the number of capital increases per capita lies within the upper/lower bound interval for synthetic Catalonia, indicating that there is no significant difference between real and synthetic Catalonia in this regard. In contrast, in the six quarters after the referendum, the number of capital increases per capita lies outside this interval (highlighted in italics in the table); specifically, it lies below the lower bound, indicating a significant fall in real Catalonia with respect to the benchmark. This negative impact remains evident even at the beginning of 2019.

[INSERT TABLE 3 AROUND HERE]
[INSERT TABLE 4 AROUND HERE]

We computed the percentage point (p.p.) difference between the variation in the path

taken by synthetic Catalonia with respect to the referendum (in percentage terms) and the variation in the path taken by Catalonia with respect to the referendum (in percentage terms) per quarter (Table 4, column 2). The difference was between 8.9 p.p. and 7.4 p.p. in the first two quarters after the referendum, rising to 31.7 p.p. in 2018Q3, 21.9 p.p. in 2018Q4 and 7.7 p.p. in 2019Q1. This indicates that between the referendum and 2019Q1, real Catalonia registered almost 1,200 fewer capital increases than synthetic Catalonia (Table 4, column 3).

4.1.2.2. Registration of new firms

We also analyzed whether the number of new registered firms varied as a consequence of the referendum. To do so, synthetic Catalonia emerged from the following combination of Spanish regions: Andalusia (0.34%), the AC of Madrid (35.4%), Navarre (10.7%), La Rioja (12.2%), the Balearic Islands (32.1%), and Aragón (16.9%).

[INSERT FIGURE 5a AROUND HERE]

As Figure 5a shows, before the referendum, real and synthetic Catalonia followed a very similar trajectory. Here again, this variable is cyclical within each year. The impact of the referendum on this margin, in this instance, is not so clear-cut. After the referendum, the number of new firms registered in per capita terms lies outside the shaded area (representing plus/minus one standard deviation of the difference between synthetic and real Catalonia before the referendum) in the very short-run. In section 5.2, we provide empirical evidence using placebo tests to corroborate causality. Again, we filtered for seasonality by using the quarterly moving average and the result is even more clear-cut (Figure 5b) than when the variable is not filtered.

[INSERT FIGURE 5b AROUND HERE]

Table 5 confirms that real Catalonia lies outside the shaded area immediately after the referendum until 2018Q3. Note, however, that the value of this margin of response is very close to the lower interval bound.

[INSERT TABLE 5 AROUND HERE]

As with the previous supply margin, we computed the percentage point (p.p.) difference between the variation in the path taken by synthetic Catalonia with respect to the referendum (in percentage terms) and the variation in the path taken by Catalonia with respect to the referendum (in percentage terms) per quarter (Table 6, column 2), as well as the absolute loss (Table 6 column 3). The difference ranged between 14.4 p.p., and 5.8 p.p. in 2019Q1. This means that between the referendum and 2019Q1, Catalonia has registered 2,318 fewer new firms than synthetic Catalonia.

[INSERT TABLE 6 AROUND HERE]

4.2. Financial margins of response

We now analyze the financial margins of response by examining the evolution of short- and long-term deposits domiciled in Catalonia before and after the treatment.

4.2.1 Short-term bank deposits

To explain the evolution of short-term bank deposits, whose mobility costs are relatively smaller than those of long-term deposits, synthetic Catalonia is formed from the combination of the following ACs: Canary Islands (37.9%), the AC of Madrid (23.9%), Navarre (23.9%) and the AC of Valencia (19.2%).

As Figure 6 shows, while both series followed a largely similar evolution before the referendum, immediately after there was a sudden and marked fall in short-term deposits p.c. in Catalonia with respect to the synthetic control. This trajectory is also evident in Table 7, where we see that while the p.c. value for real Catalonia is within the upper and lower bounds of synthetic Catalonia before the referendum, it lies well below the lower bound for all quarters after the referendum.

[INSERT FIGURE 6 AROUND HERE]
[INSERT TABLE 7 AROUND HERE]

When we compute the discrepancy in percentage points with respect to the referendum between the variation shown by synthetic Catalonia and that shown by real Catalonia, we find a difference of 27 p.p. in each of the first two quarters after the referendum, falling to 20 p.p. in the first quarter of 2019. This represents a loss in deposits of almost 33 million euros in the first two quarters after the referendum in real Catalonia. Despite a slight recovery, in 2019Q1 the loss due to the referendum was still more than 24 million euros (see Table 8).

[INSERT TABLE 8 AROUND HERE]

4.2.2 Long-term bank deposits

In the case of long-term bank deposits p.c., synthetic Catalonia emerges from a combination of Aragón (30.1%), the AC of Madrid (0.63%), Navarre (12.8%), the AC of Murcia (16.2%), Canary Islands (31.1%), and the Balearic Islands (0.34%).

As Figure 7 shows, the two Catalonias, real and synthetic, followed a very similar evolution before the referendum. After the referendum, however, we detect a small decrease in the p. c. deposits of real Catalonia with respect to those of our synthetic control, causing the evolution of real Catalonia after the referendum to fall outside the constructed margins of confidence. Specifically, for the six quarters after the referendum, the value of deposits in real Catalonia lies outside the interval for synthetic Catalonia (Table 9).

[INSERT FIGURE 7 AROUND HERE]
[INSERT TABLE 9 AROUND HERE]

When we compute the percentage difference in the variation with respect to the referendum between synthetic and real Catalonia, we find a maximum variation in the first two quarters immediately after the referendum between 7 and 6 p.p. (Table 10). The trend is decreasing thereafter reaching 1.2 p.p. in 2019. This means that the loss suffered by real Catalonia with respect to the synthetic control was more than 2,500 million euros in the first two quarters after the referendum, decreasing thereafter to 470 million euros in 2019Q1. These impacts are much less marked than those estimated for

the short-run bank deposits. As we argue in Section 5.4, we cannot in fact conclude that the referendum had an impact on long-run bank deposits.

[INSERT TABLE 10 AROUND HERE]

5. Placebo Experiments[14]

In this section, we run a set of robustness tests to validate our main results. For the margins for which we found some evidence of causality (i.e. all but GDP per capita and unemployment rate), we run AC and time placebo tests, changing, respectively, the treated region and the time of the shock. If we are definitely estimating a causal effect due to the referendum, we do not expect to find any effect in the placebo tests. We also checked if our results are driven by any particular region, excluding regions with a weight greater than zero from the donor pool; we finally replicate the main results by excluding from the predictors the outcome variables measured before the referendum.

5.1 Capital increases

In the case of the number of capital increases, we first estimate the synthetic control for each of the ACs in the donor pool while exposing them to the treatment in 2017Q3. If our benchmark estimate is in fact detecting the causal effect of the referendum, the divergence of the region-specific synthetic controls from the respective data after the treatment date should be considerably smaller than that recorded in the case of Catalonia.

Table 11 shows the results of the placebo experiments by region for the number of capital increases. In what follows, we use the ratio between the post- and pre-treatment of the root mean squared prediction error (RMSPE), where the higher the ratio, the greater the difference between the treated and synthetic units in the post-treatment case with respect to that of the pre-treatment (column 1, Table 11). The RMSPE is equal to the square root of the mean of the square of the difference between the treated and the synthetic control. The second and third columns contain the RMSPE for the pre- and

[14] In the on-line Appendix, we perform a Sparse Synthetic Control as a further robustness test.

post-treatment periods, respectively.

[INSERT TABLE 11 AROUND HERE]

Thus, column 1 of Table 11 quantifies just how closely the region-specific synthetic controls follow the data post-treatment relative to the pre-treatment fit. Catalonia has a ratio greater than one; however, we find that a number of other regions also have a coefficient higher than one: the Balearic Islands, Cantabria, Castile and León, Extremadura, La Rioja and Galicia. A priori, this could be evidence of spill-over effects (Born et al. 2019); yet, in this instance, this does not appear to be an especially credible hypothesis. Technically, however, our assumption that the donor pool countries are unaffected by the treatment is potentially violated. To test the reliability of our results, we therefore restrict the donor pool to just those ACs with a ratio below one. Qualitative results remain unchanged (see Figure 8).

[INSERT FIGURE 8 AROUND HERE]

In Figure 9, we plot the results of different permutations, that is, we consider the possibility that each AC is a treated region. This figure highlights the sign of the difference between the synthetic and the real AC. A ratio between the post- and pre-referendum RMSPEs greater than one may also be due to a higher value for the synthetic than for the real AC, which is exactly the opposite result to the one we obtained when Catalonia was the treated region. Specifically, we plot the difference between the real AC and its corresponding synthetic counter-factual. The bold line corresponds to the difference in the case of Catalonia. The estimated trend for Catalonia is clearly negative post-treatment, and notably lower than the estimated trend for the rest of the ACs. During the pre-treatment period, the series for Catalonia oscillates around zero and, in all circumstances, it does not show a particularly different trend from that of the rest of the ACs. The series estimated for the Balearic Islands and for Cantabria, however, at various post-referendum points, fall below that of Catalonia. Nevertheless, these two ACs present a much higher RMSPE before the treatment than that presented by Catalonia. In other words, in these two cases, the reason why they fall below Catalonia seems to be due to a lack of fit, being the two ACs with the largest pre-treatment RMSPE (see Table 11).

[INSERT FIGURE 9 AROUND HERE]

To test for the adequacy of the treatment quarter, we run another placebo experiment. To do so, we change the treatment quarter to a period before the referendum was held. Specifically, we use 2012Q3, 2010Q1 and 2007Q3. We find no significant impact around the date of these three "fake-referendums" (see Figure 10).

[INSERT FIGURE 10 AROUND HERE]

Following Abadie, Diamond, and Hainmueller (2015) and Bilgel and Karahasan (2019), we check if our results are driven by any particular region, excluding those with a weight greater than zero from the donor pool. Furthermore, given the historical similarities and the pro-independence pressures present also in the Basque country, we excluded this AC from the donor pool as well.

[INSERT FIGURE 11 AROUND HERE]

All results, but excluding the AC of Madrid, confirm the main specification leaving the synthetic Catalonia above the real Catalonia after the referendum, even if the values get closer than when we use all the donors (see Figure 11).

We replicate the main analyses by excluding from the predictors the outcome variables measured before the referendum. As it is shown in Figure 12, results do not change.

[INSERT FIGURE 12 AROUND HERE]

5.2. Registration of new firms

In the case of the registration of new firms, the placebo test running permutations of the treated region confirms that there are other ACs with a ratio between the post- and pre- referendum RMSPEs that is greater than the one we estimate for Catalonia of 1.41 (see Table 12).

We thus re-compute the value for synthetic Catalonia by excluding all regions with a

ratio greater than one, but including Extremadura and Madrid for which a placebo could not be run[15]. In this case, the Balearic Islands (36.3%), La Rioja (28.7%) and the AC of Madrid (35%) make up the synthetic Catalonia. Our qualitative results remain unchanged (see Figure 13).

[INSERT FIGURE 13 AROUND HERE]
[INSERT TABLE 12 AROUND HERE]
[INSERT FIGURE 14 AROUND HERE]

As with the previous margin, Figure 14 shows that after the referendum the difference between the real and synthetic Catalonia is negative, and almost always below the value of the placebos.

[INSERT FIGURE 15 AROUND HERE]

The results of the time placebo tests are shown in Figure 15. Again, around the dates of these alternative treatment quarters (2012Q3, 2010Q1 and 2007Q3), we do not observe any change in the trends between real and synthetic Catalonia.

We also check for this margin if our results are driven by any particular region, excluding those with a weight greater than zero from the donor pool and also the Basque Country. The results relative to the post treatment still hold even if in the pre-treatment there are some quarters where the synthetic Catalonia seems to have a larger value than the real Catalonia (Figure 16).

[INSERT FIGURE 16 AROUND HERE]

We replicate the analyses by excluding from the predictors the outcome variables measured before the referendum. As it is shown in Figure 17, results do not change.

[INSERT FIGURE 17 AROUND HERE]

[15] Given the technical restrictions of the SCM, it makes difficult to obtain good predictions, if any, for extreme units (Doudchenko and Imbens 2016, 9). This is certainly the case of the AC of Madrid (Spain's richest region) and of Extremadura (poorest region).

5.3. Short term bank deposits

Table 13 shows the results of the AC-placebo experiments for short-term bank deposits. Again, it was not possible to run a placebo test for Madrid and for Extremadura.

[INSERT TABLE 13 AROUND HERE]

While Catalonia presents the highest ratio, Aragón and the AC of Valencia also have high values (greater than one). An inspection of Figure 18 shows that the real Aragón and the real AC of Valencia have levels of short-run deposits that are higher than their corresponding synthetic controls after the referendum in Catalonia. Here we cannot reasonably disregard possible spill-over effects from Catalonia to its neighboring regions. In both regions, short-run bank deposits continued to increase in the post-treatment period, although immediately after the referendum the decrease in short-term bank deposits in Catalonia with respect of synthetic Catalonia was much greater than the corresponding increase in Aragón and in the AC of Valencia (Table 14).

[INSERT TABLE 14 AROUND HERE]
[INSERT FIGURE 18 AROUND HERE]

There are other regions that present a ratio greater than one and which might, therefore, violate the assumption requiring the donor pool to be exclusively composed of ACs that are unaffected by the treatment. Thus, here too, we consider restricting the donor pool to only those ACs whose ratio is below one, plus the AC of Madrid and Extremadura. The results are very similar to those obtained with our baseline specification (see Figure 6). Synthetic Catalonia now emerges from a combination of the Canary Islands (55.4%), the AC of Madrid (27.8%) and La Rioja (16.8%).

[INSERT FIGURE 19 AROUND HERE]

The results in Figure 19 are confirmed by Figure 20, where we can see that after the referendum the difference between the synthetic and the real Catalonia is much larger than for any of the other placebos.

[INSERT FIGURE 20 AROUND HERE]

As in the other margins, we also run placebo tests to check whether the results are contingent on the treatment quarter. Hence, we run the baseline specification considering 2012Q3, 2010Q1 and 2007Q3 as the alternative treatment quarters. Figure 21 clearly shows that following these alternative treatment quarters there is no difference between the real and the synthetic Catalonia.

[INSERT FIGURE 21 AROUND HERE]

Finally, we check if our results are driven by any particular region. The results do not change evidencing the sharp decrease in short term deposits after the referendum (see Figure 22).

[INSERT FIGURE 22 AROUND HERE]

We replicate estimates by excluding from the predictors the outcome variables measured before the referendum. As it is shown in Figure 23, results do not change either.

[INSERT FIGURE 23 AROUND HERE]

5.4. Long-term bank deposits

In the case of this margin, the ratio presented by Catalonia is not the largest; in fact, it is below one (0.52) (Table 15). Hence, we are unable to conclude that the referendum had a causal effect on the path of long-term deposits after the treatment quarter, and so we do not perform a robustness test for this margin.

[INSERT TABLE 15 AROUND HERE]

6. Conclusions

The intensification of the conflict between Catalonia and the central government after the referendum offers an opportunity to test the economic impact of a secessionist conflict. Importantly, while these tensions have been present for a period of years, they came to an obvious head in October 2017 following the holding of an illegal referendum in Catalonia, promoted by the Catalan executive. Given this referendum was not negotiated with the Central executive, levels of political and social conflict have, as predicted and as subsequent events have shown, increased. In short, the referendum did not mark the end of the process, but a sharp increase in the conflict leading to a Constitutional crisis.

Using a SCM, we have shown that the referendum has not only affected real variables on the supply side, including the number of corporate capital increases and the number of new firms being registered, but also the key financial variable of the amount of short-term bank deposits. Specifically, we have shown that in Catalonia the number of capital increases one year after the referendum decreased with respect to those in synthetic Catalonia by 1,200 and that the number of registered firms fell by 2,318. However, the most striking and, at the same time, the most robust result concerns the performance of short-term deposits following the referendum: there being a massive outflow of money from Catalonia. In the first quarter after the referendum, these deposits fell in value with respect to those for synthetic Catalonia by 33 million euros, and one year after the referendum they were still 24 million euros below those calculated for synthetic Catalonia. This is a clear warning of how sensitive capitalists are to uncertainty cause by a conflict. Such behavior could at least temporarily cause liquidity problems to the Catalan financial entities[16].

While in the short run we do not observe real economic impacts on the demand side as a consequence of the peak in the conflict, the real impacts detected on the supply side point to negative effects in the potential output of Catalonia in the medium/long run. Our framework of analysis – having distinguished between the impact on the aggregate demand and on the aggregate supply – allows to identify these potential economic

[16] In September 2020 a private foundation (https://www.11onze.cat) was launched to set up a Catalonian bank to be registered in an EU country different from Spain. The reason for this is that "one of the main tools for an independent state to be operative is the banking system, in particular, a financial entity that can control the flows of revenue and guarantee the correct functioning of the economy" (https://www.elmon.cat/economia/neix-onze-banc-catala-fora-abast-madrid-espanya_2135307102.html).

negative effects in the medium/long run even if we do not dispose of a longer time span after the treatment year. All in all, under a low-level conflict as the one we have analyzed, the impact seems more subtle with respect to more intense conflicts. While we do not see any impact on aggregate demand (captured by GDP p.c. or the unemployment rate), the estimated reaction of firms (supply side) still points to potential negative economic effects.

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ON-LINE APPENDIX: Further Robustness Test: Sparse Synthetic Control

By means of the Sparse Synthetic Control, it is possible to evaluate the trade-off between sparsity and goodness of fit in the choice of the number of regions that contribute to the synthetic control. Following Abadie, Diamond, and Hainmueller (2015) and Bilgel and Karahasan (2019), given *k* regions in the donor pool with weight greater than zero, for each specification we construct synthetic controls for Catalonia with a sequential combinations of *k, k-1, k-2, ...* and a single-control region. For each l = *k, k-1, k-2, ...1* combination, we choose the one that minimizes the RMSPE before the treatment.

Regarding the number of capital increases p.c., we start dropping one region, the AC of Murcia, from the donor pool with a weight greater than zero, driving us to a combination of three donors which compared to all possible other combinations is the one releasing the lowest RMSPE before the treatment. By using the same technique, we then run the estimate by dropping the two regions Murcia and Navarre, and finally we use only one counterfactual region, the AC of Madrid (see Table A.1).

[INSERT TABLE A.1 AROUND HERE]

In all three cases, when we reduce the number of regions contributing to the synthetic control, the main result holds even if when we build up the synthetic control by using only one region in the pretreatment period the synthetic Catalonia seems to diverge a bit from the real one (Figure A.1).

[INSERT FIGURE A.1 AROUND HERE]

When we look at registration of new firms and use four regions, we drop Aragón; in the case of three regions, we drop Andalusia and La Rioja; in the case of two regions, we use the AC of Madrid and Andalusia; with only one region, we use Balearic Islands (see Table A.2).

[INSERT TABLE A.2 AROUND HERE]

The main result still holds with four and with three regions. It seems to be weaker with

two regions and it holds with one region even if during the pretreatment period the synthetic Catalonia remains sometimes a bit distant from the real one (see Figure A.2).

[INSERT FIGURE A.2 AROUND HERE]

Finally, we look at short-run deposits for which we built up synthetic control with three regions dropping the AC of Valencia, we then have only two regions dropping the AC of Madrid and the AC of Valencia and finally only one region, Navarre (see Table A.3).

[INSERT TABLE A.3 AROUND HERE]

With three regions, the main result holds, with two and one region the post-treatment results holds; however, for some time spans during the pre-treatment period the synthetic Catalonia diverges from the real Catalonia (see Figure A.3).

[INSERT FIGURE A.3 AROUND HERE]

All in all, we conclude the sparse treatment synthetic control tests confirm our main results.

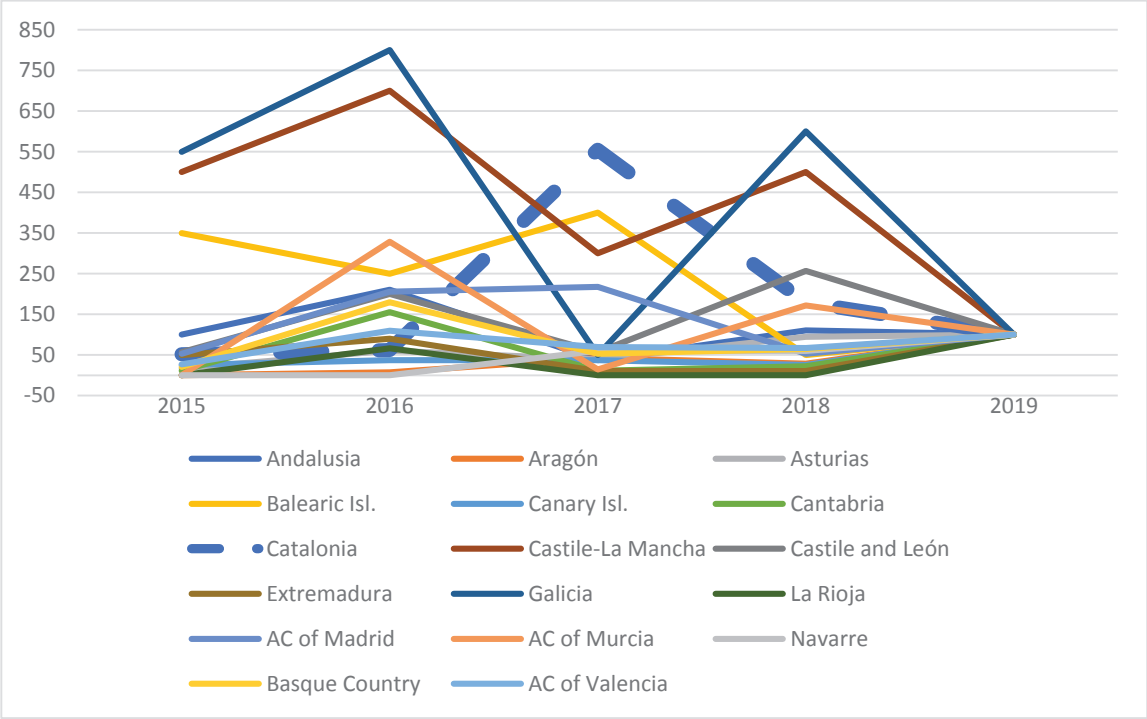
APPENDIX: Statistical Sources of Data

Variable	Source	Period	WEB Source
MARGINS OF RESPONSE			
GDP per capita	AIReF	2000-2019(1)	https://www.airef.es/es/la-airef-publica-la-estimacion-del-segundo-trimestre-de-la-composicion-por-ccaa-del-pib-nacional/
Short- and long-run bank deposits	BdE (BIEST)	2000-2019(1)	http://app.bde.es/bie_www/faces/bie_wwwias/jsp/op/Home/pHome.jsp
New registered corporations	INE	2008-2019(1)	https://www.ine.es/jaxiT3/Tabla.htm?t=13913
Number of capital increases	INE	2008-2019(1)	https://www.ine.es/jaxiT3/Tabla.htm?t=13914
COVARIATES			
Level of education	FEDEA	2000-2016	https://www.fedea.net/datos-economia-regional-y-urbana/
Private investment	INE	2000-2016	https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736167628&menu=resultados&idp=1254735576581
Labor activity rate	INE	2000-2019	https://www.ine.es/jaxiT3/Tabla.htm?t=4218&L=0
Relative importance of sector of economic activity	INE	2000-2019	https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=1254736167628&menu=resultados&idp=1254735576581

Table

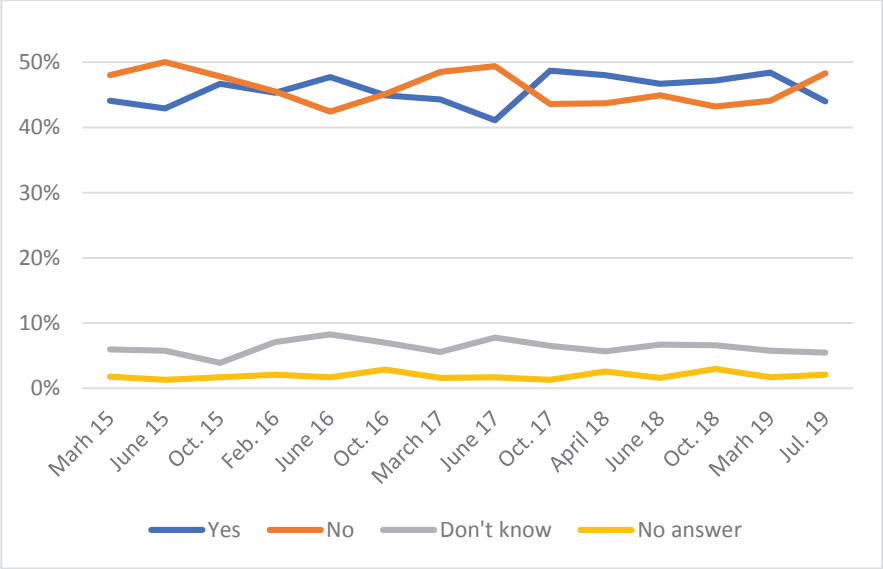
FIGURES

Figure 1: Frequency of appearance of the term "economic uncertainty" in Spanish newspapers (2014-19).



Source: FACTIVA, several years.

Figure 2: *Do you want Catalonia to become an independent State?*



Source: Centre d'Estudis d'Opinió (CEO), "Valors politics";
http://evoluceo.ceo.gencat.cat/ceo/inici/evoluceo.html#/main

Figure 3a: Catalonia *vs.* Synthetic Control – Real GDP per capita

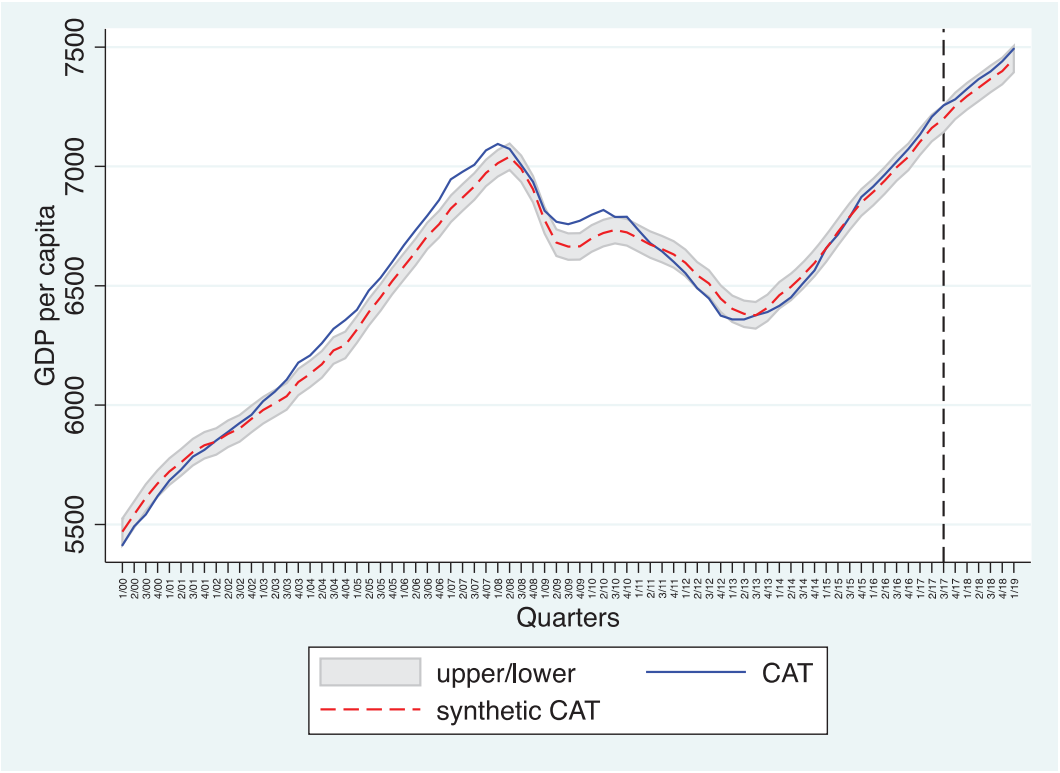


Figure 3b: Catalonia *vs.* Synthetic Control – unemployment rate

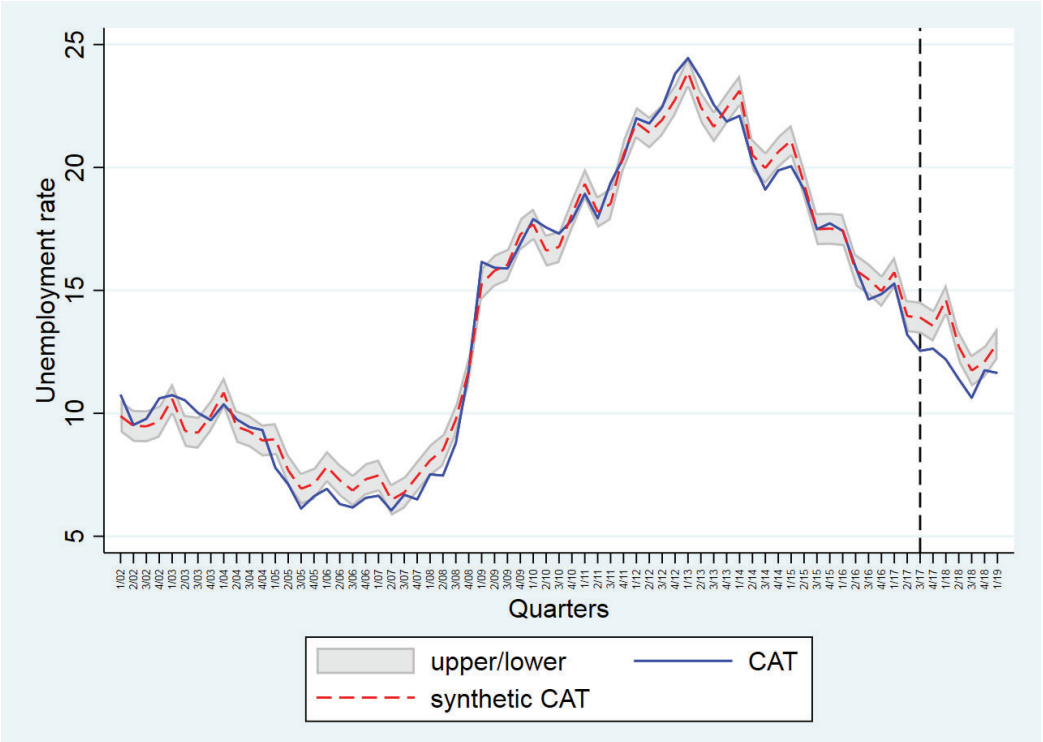


Figure 4a: Catalonia *vs.* Synthetic Control – Capital increases in per capita terms

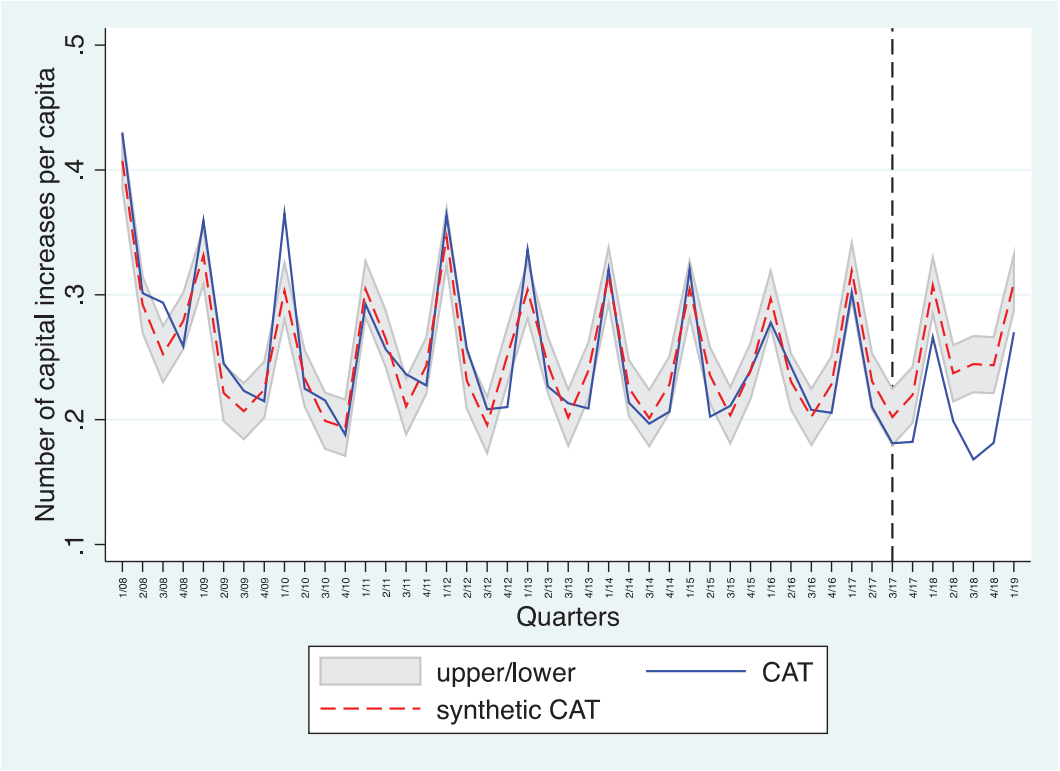


Figure 4b: Catalonia *vs.* Synthetic Control – Capital increases in per capita terms, quarterly moving average.

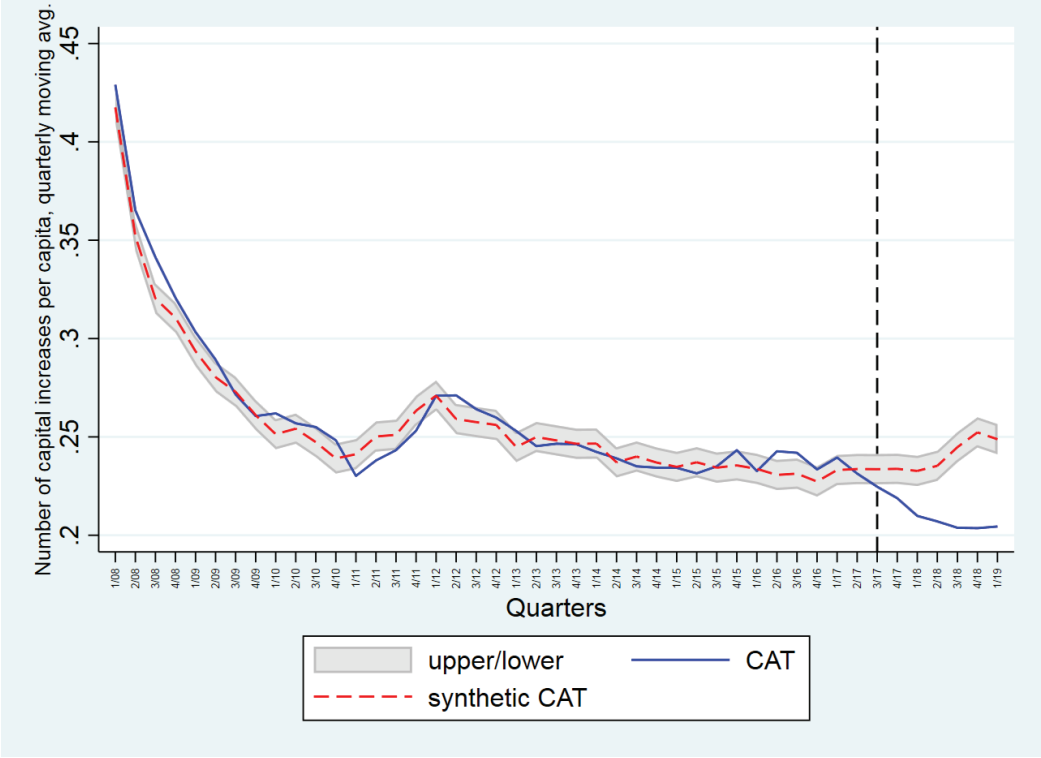


Figure 5a: Catalonia *vs*. Synthetic Control - Registration of new firms in p.c. terms

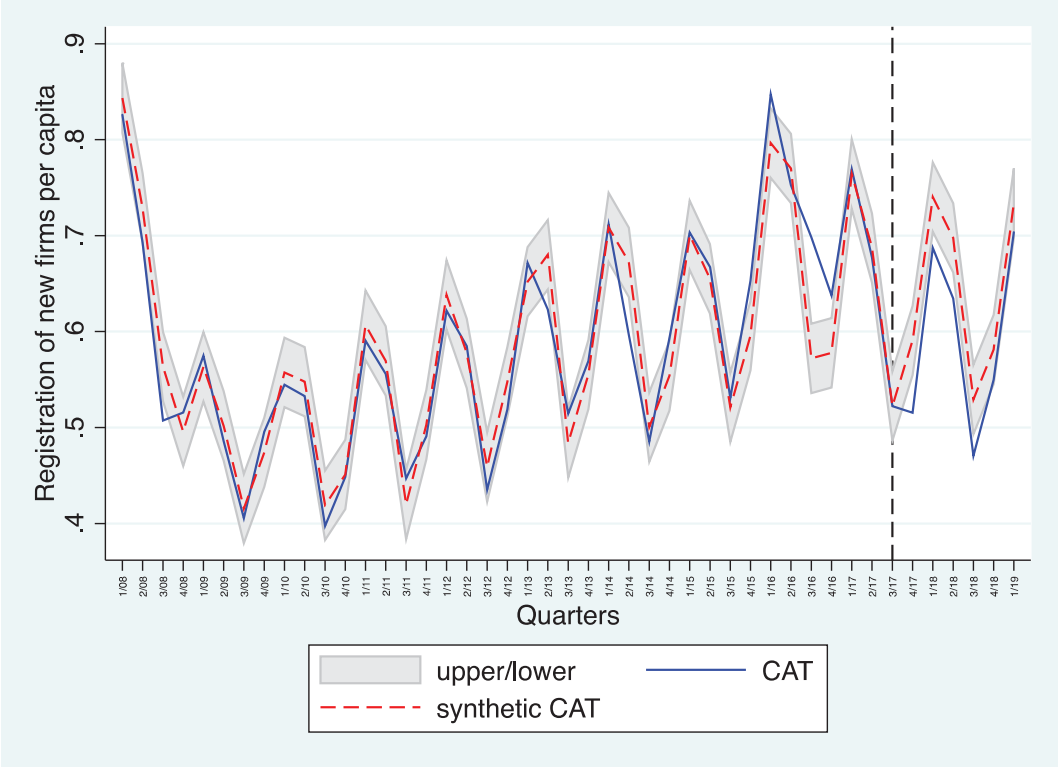


Figure 5b: Catalonia *vs*. Synthetic Control - Registration of new firms in p.c. terms, quarterly moving average.

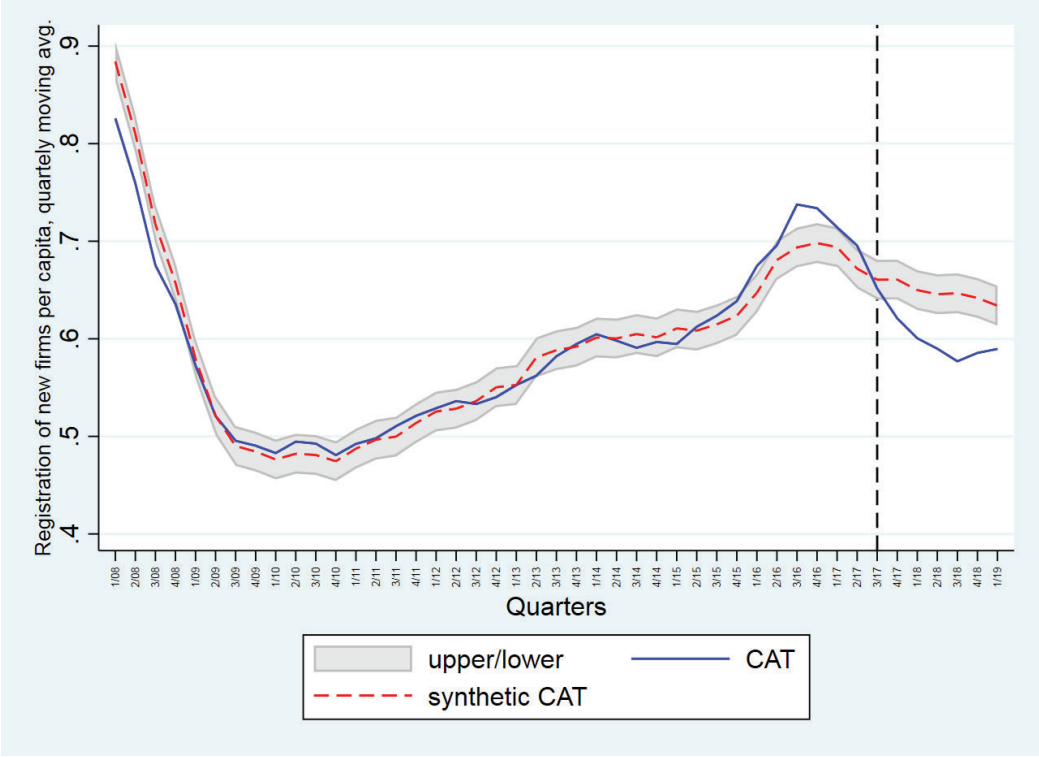


Figure 6: Catalonia *vs*. Synthetic Control – Short-term bank deposits per capita

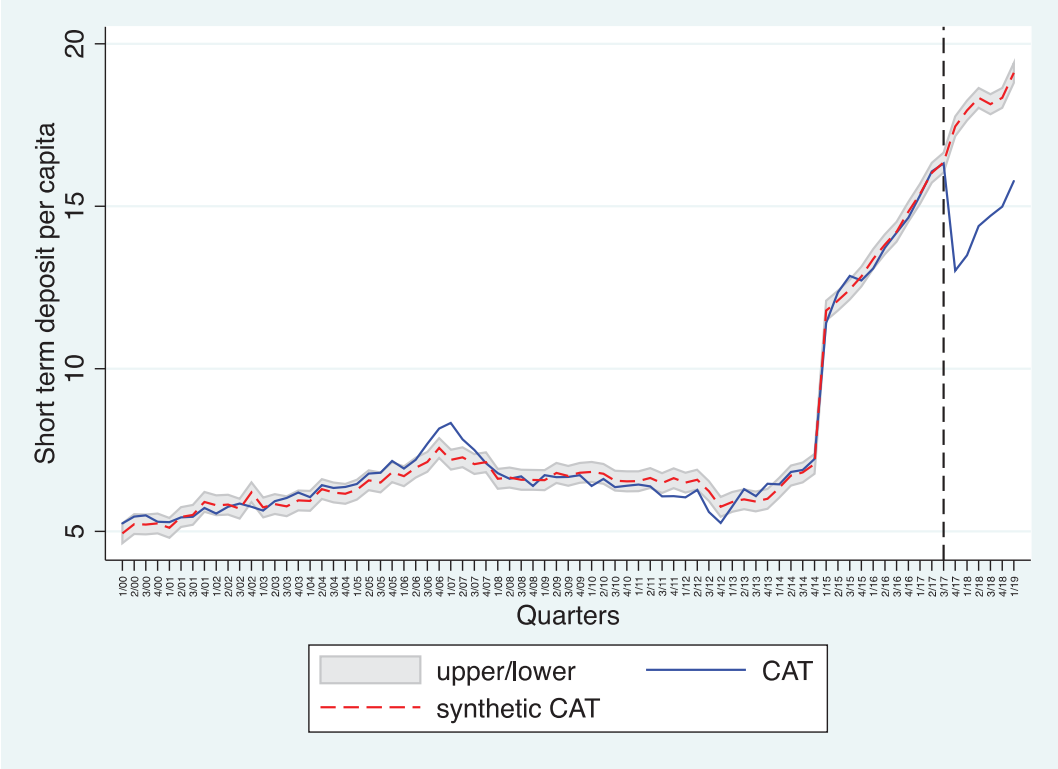


Figure 7: Catalonia *vs*. Synthetic Control – Long-term bank deposits per capita.

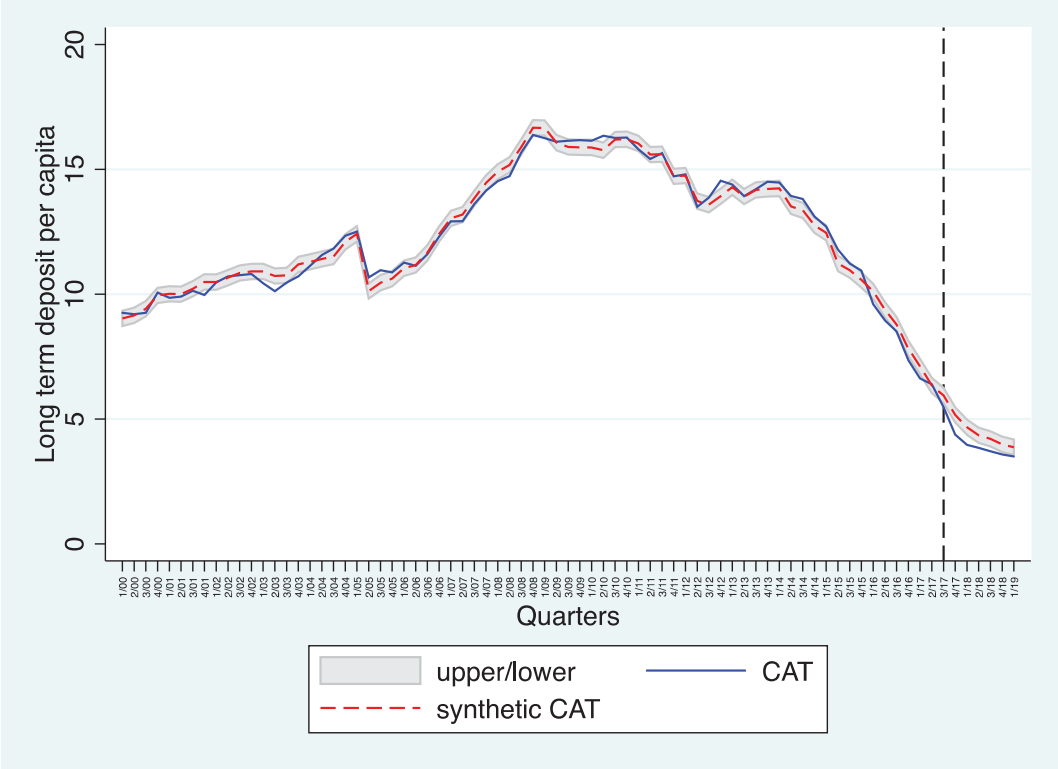


Figure 8: Catalonia *vs.* Synthetic Control – number of capital increases, excluding ACs with a ratio>1

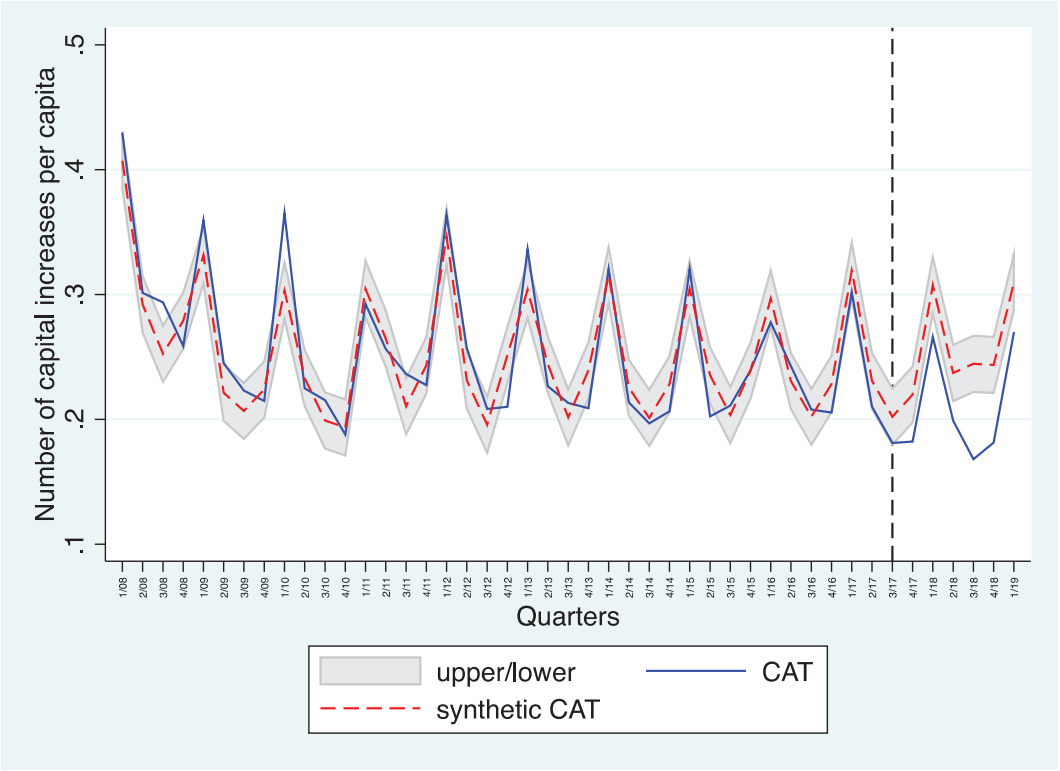


Figure 9: Difference between real and synthetic Catalonia *vs.* spatial placebos. Number of per capita capital increases

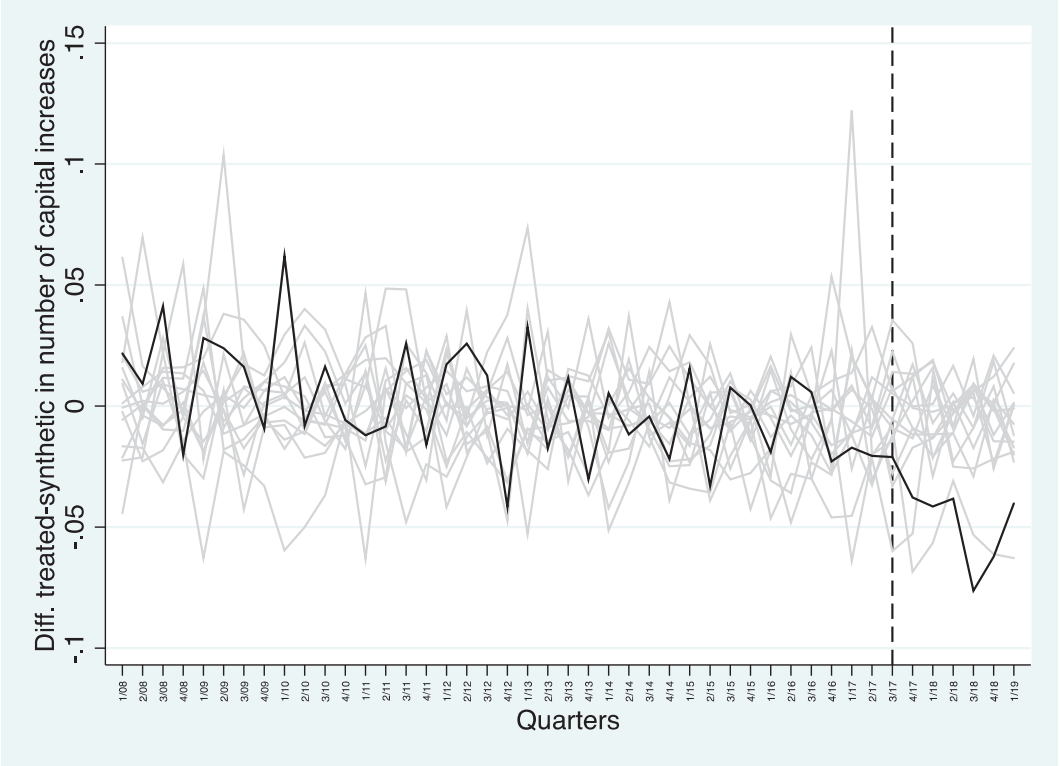


Figure 10: Catalonia *vs.* Synthetic Control – number of per capita capital increases, time placebo tests.

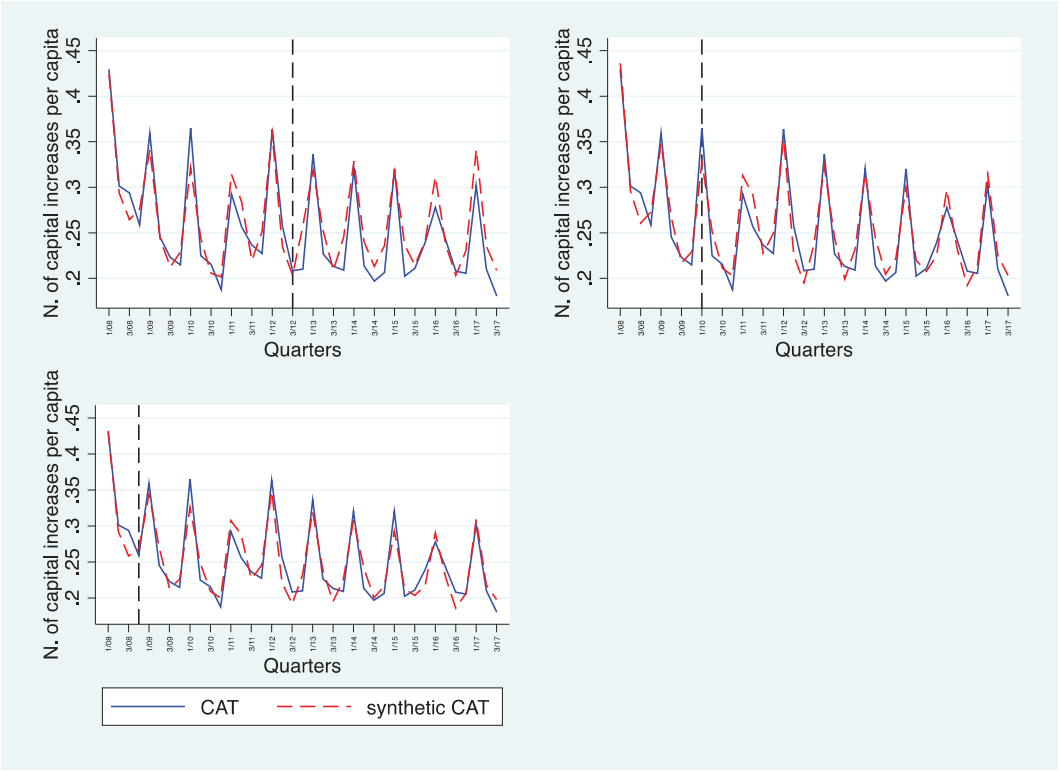


Figure 11: Catalonia *vs.* Synthetic Control – number of per capita capital increases, leave-one-out distribution.

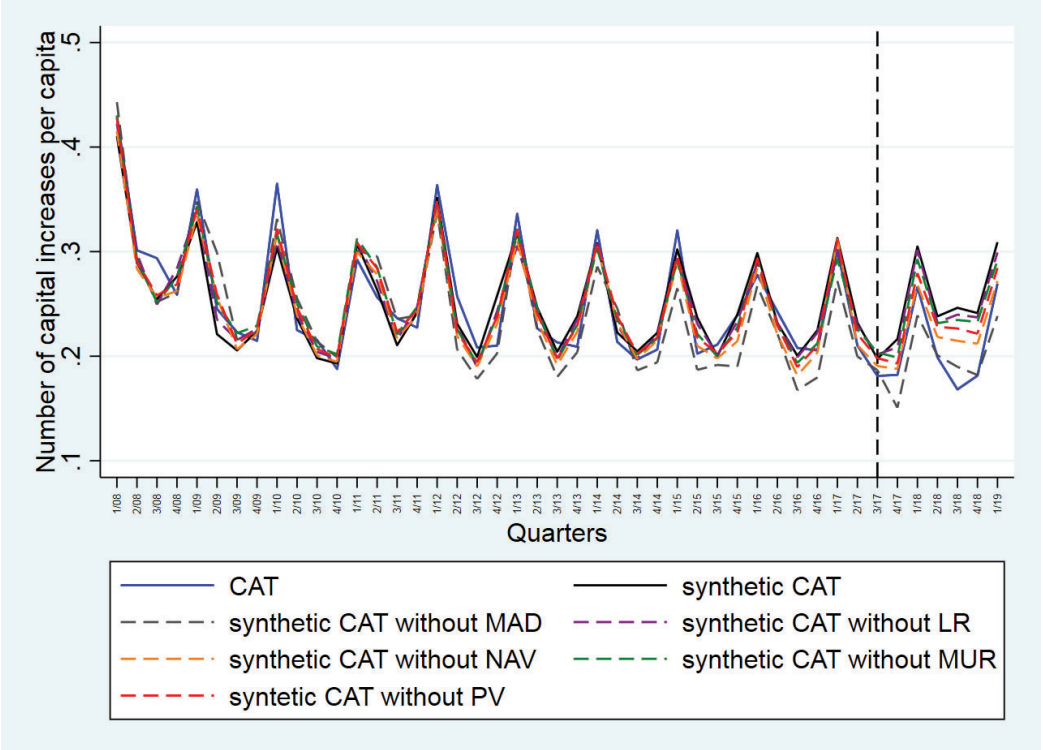


Figure 12: Catalonia *vs.* Synthetic Control – number of per capita capital increases, using only non-outcome pre-treatment characteristics as predictors.

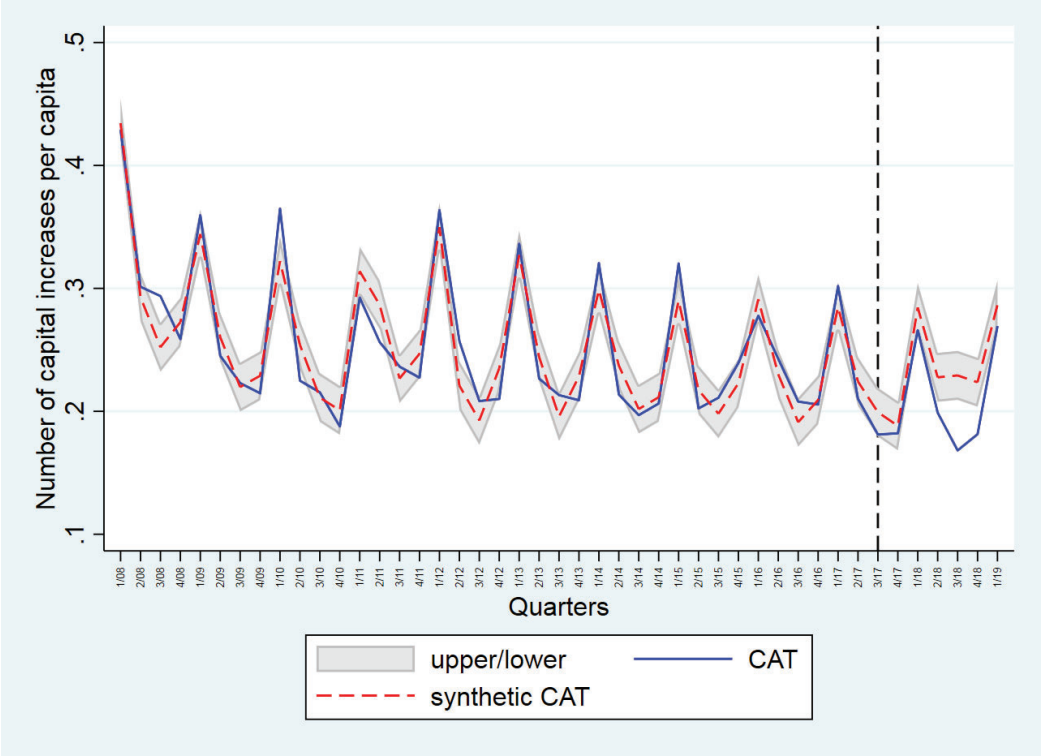


Figure 13: Catalonia *vs.* Synthetic Control – per capita registration of new firms, excluding ACs with a ratio>1

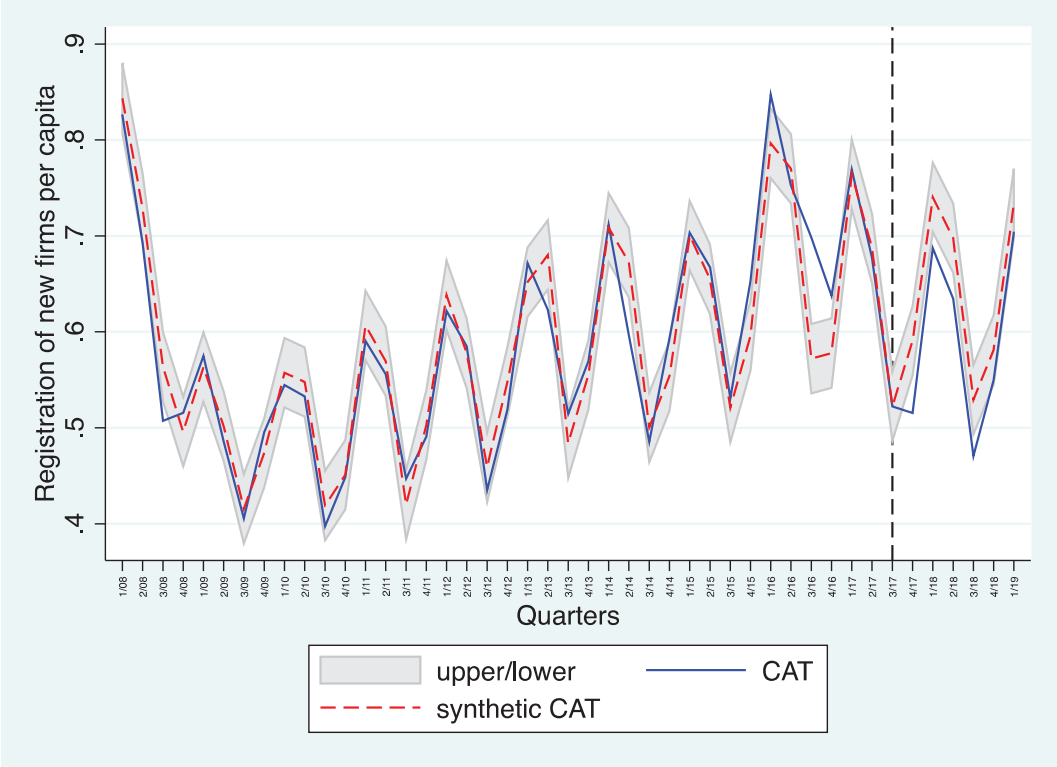


Figure 14: Difference between real and synthetic Catalonia *vs.* spatial placebos. Number of per capita registrations of new firms.

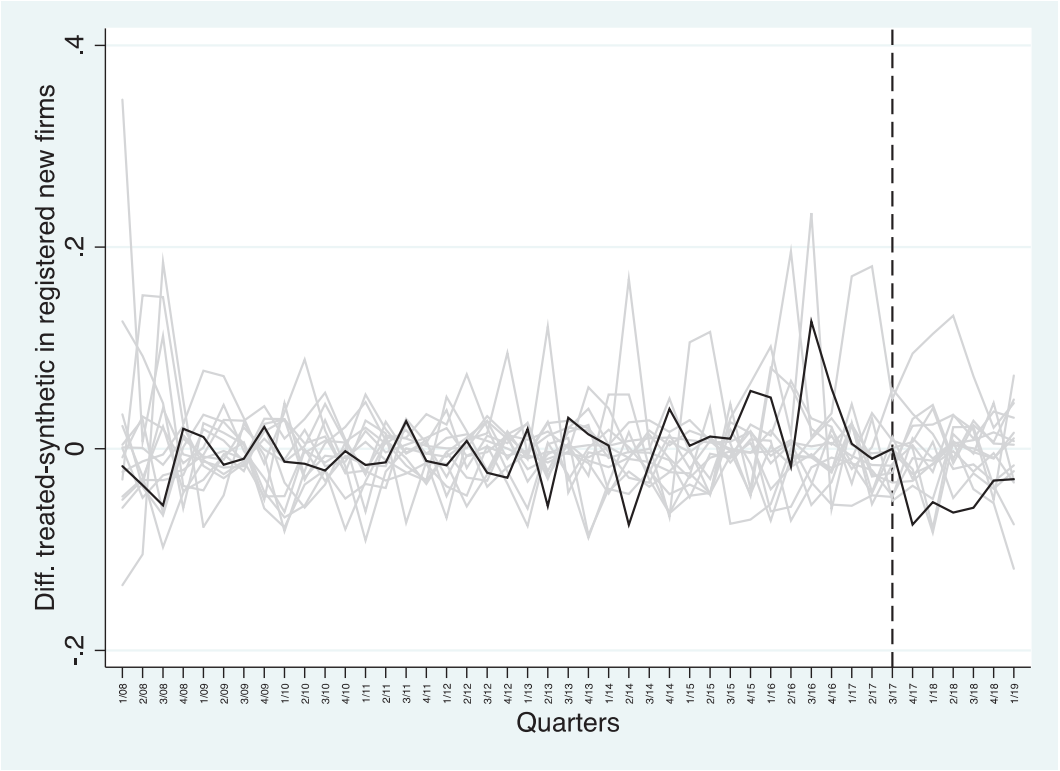


Figure 15: Catalonia *vs.* Synthetic Control – number of new firms per capita, placebo tests.

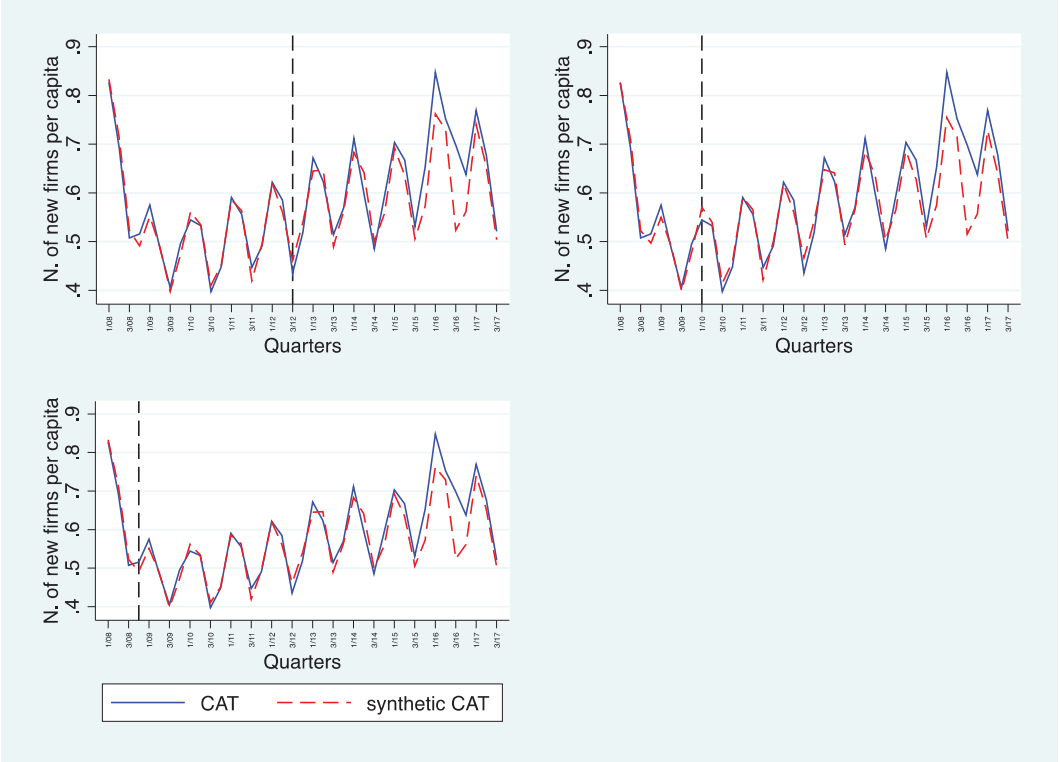


Figure 16: Catalonia *vs.* Synthetic Control – number of new firms, leave-one-out distribution.

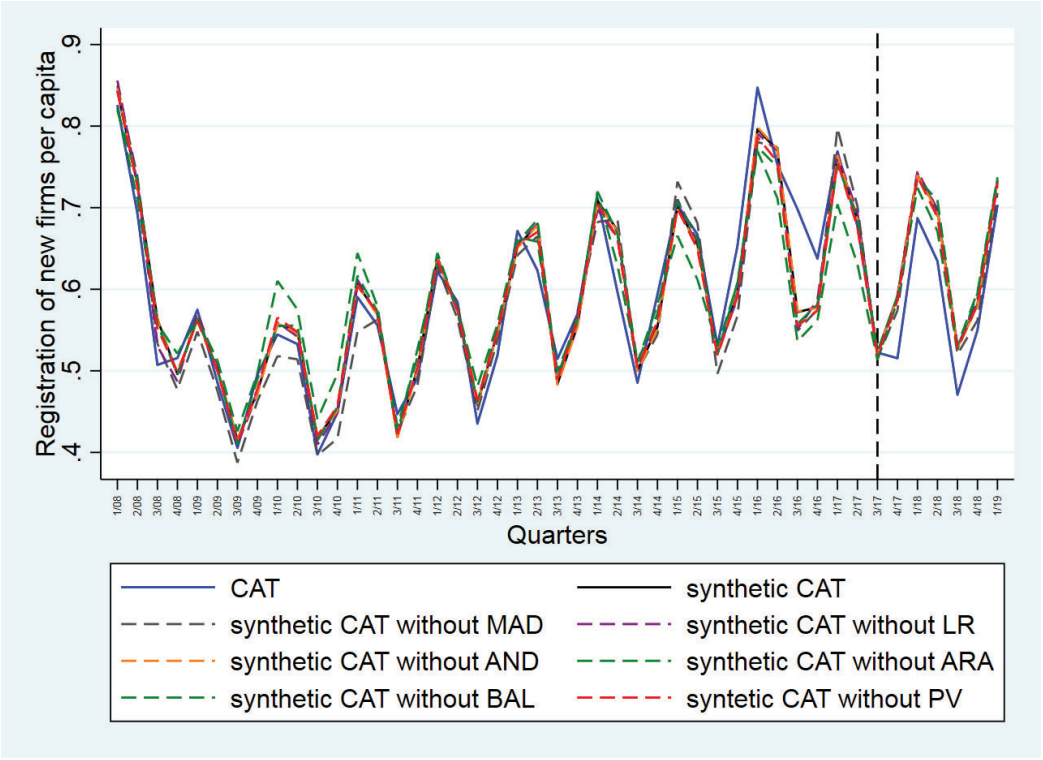


Figure 17: Catalonia *vs.* Synthetic Control – number of new firms, using only non-outcome pre-treatment characteristics as predictors.

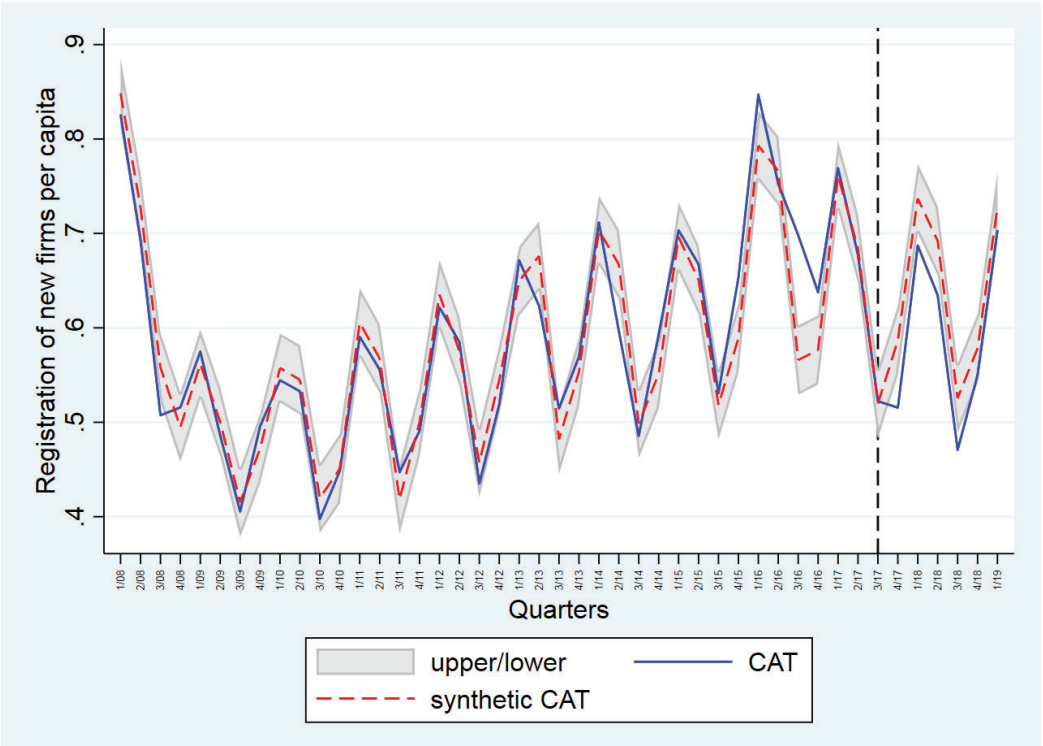


Figure 18: Valencia (VAL) and Aragon (ARA) vs. synthetic – Short-term deposits.

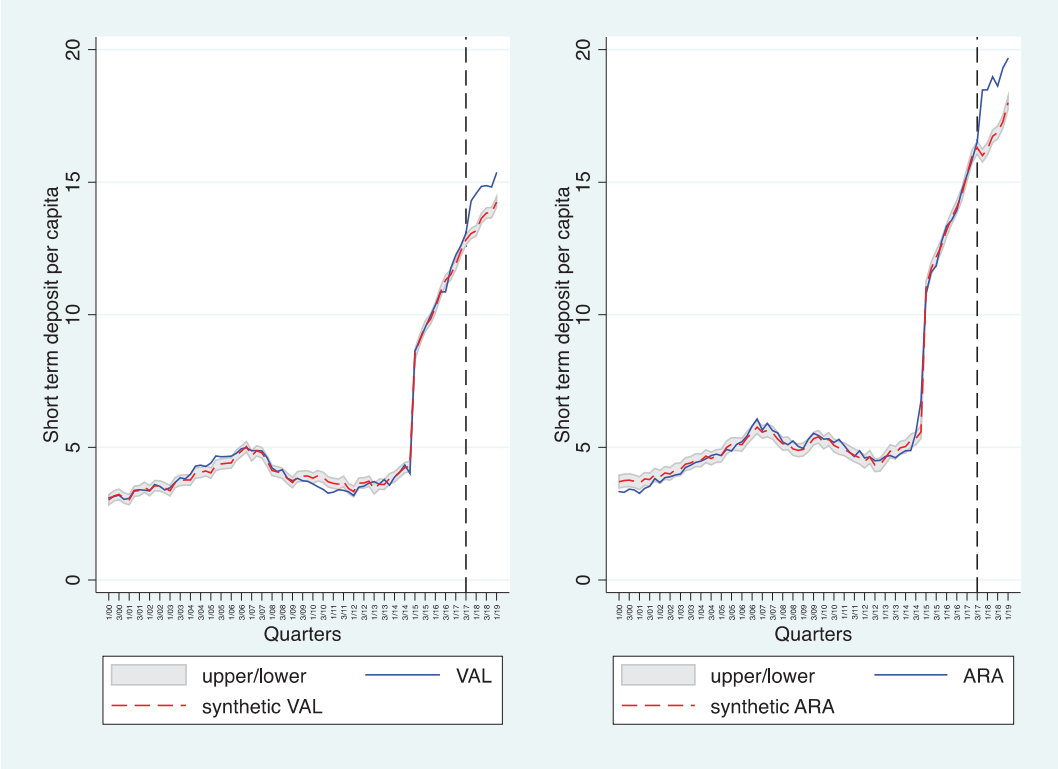


Figure 19: Catalonia *vs*. Synthetic Control – Short-term deposits. Synthetic without communities with a ratio>1.

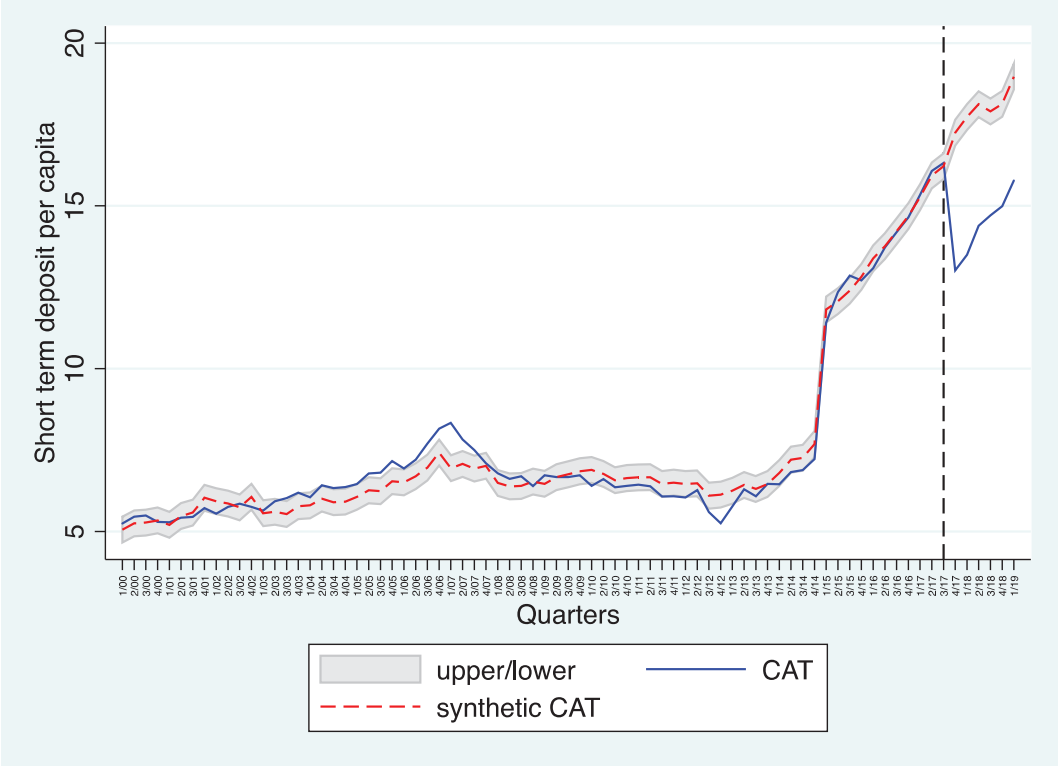


Figure 20: Difference between real and synthetic Catalonia vs. spatial placebos. Short-run deposits.

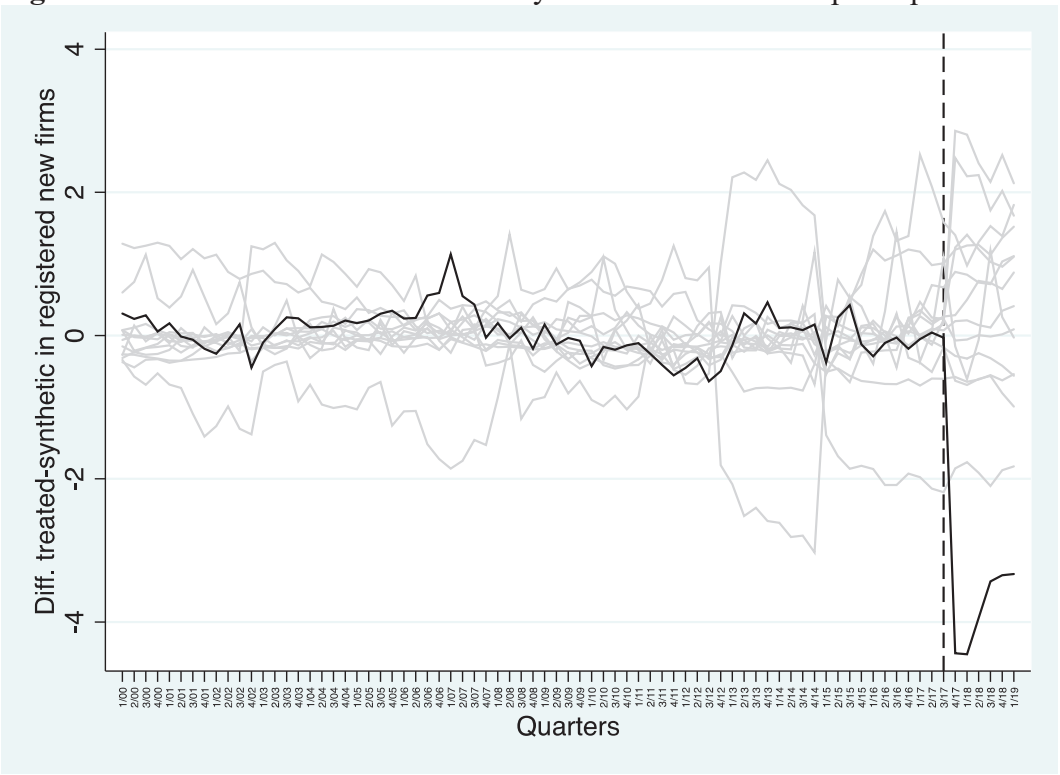


Figure 21: Catalonia *vs*. synthetic Control – Short-term deposits, placebo tests.

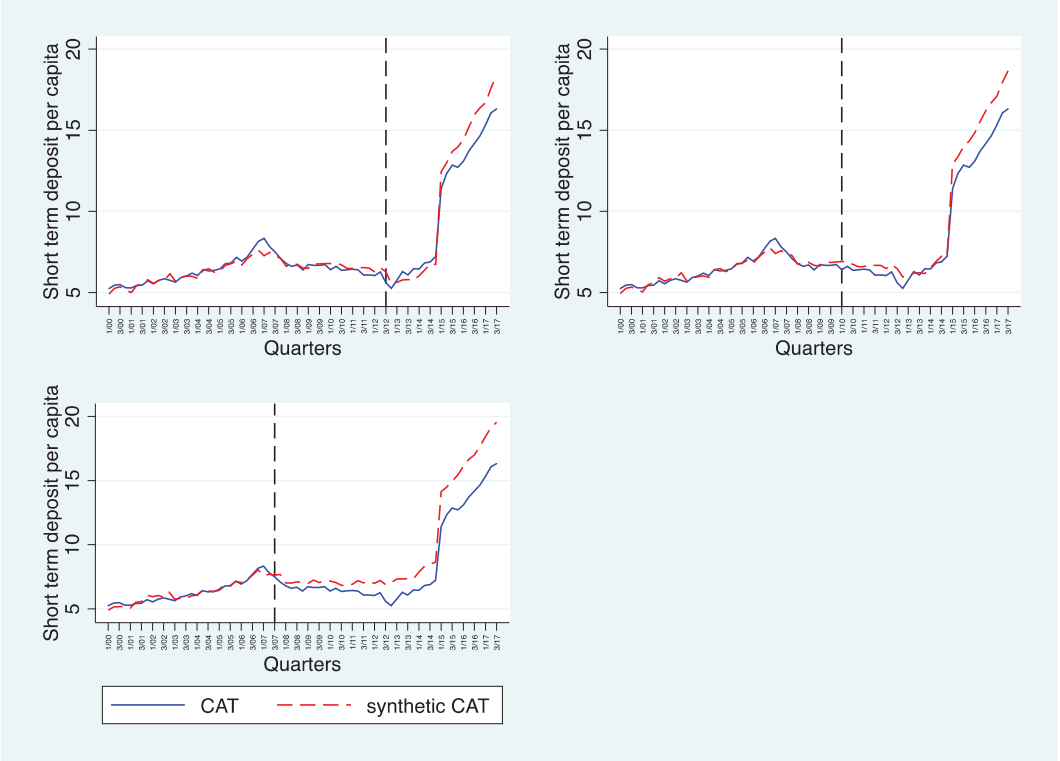


Figure 22: Catalonia *vs*. synthetic Control – Short-term deposits, leave-one-out distribution.

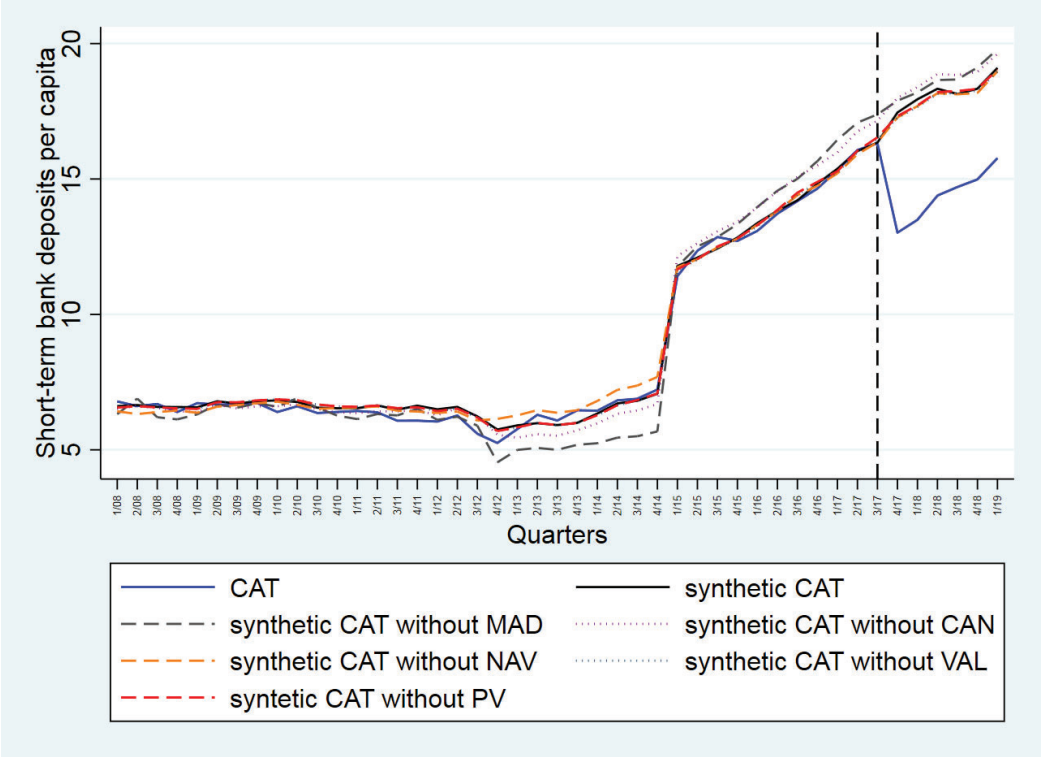


Figure 23: Catalonia *vs.* Synthetic Control – short term deposits, using only non-outcome pre-treatment characteristics as predictors.

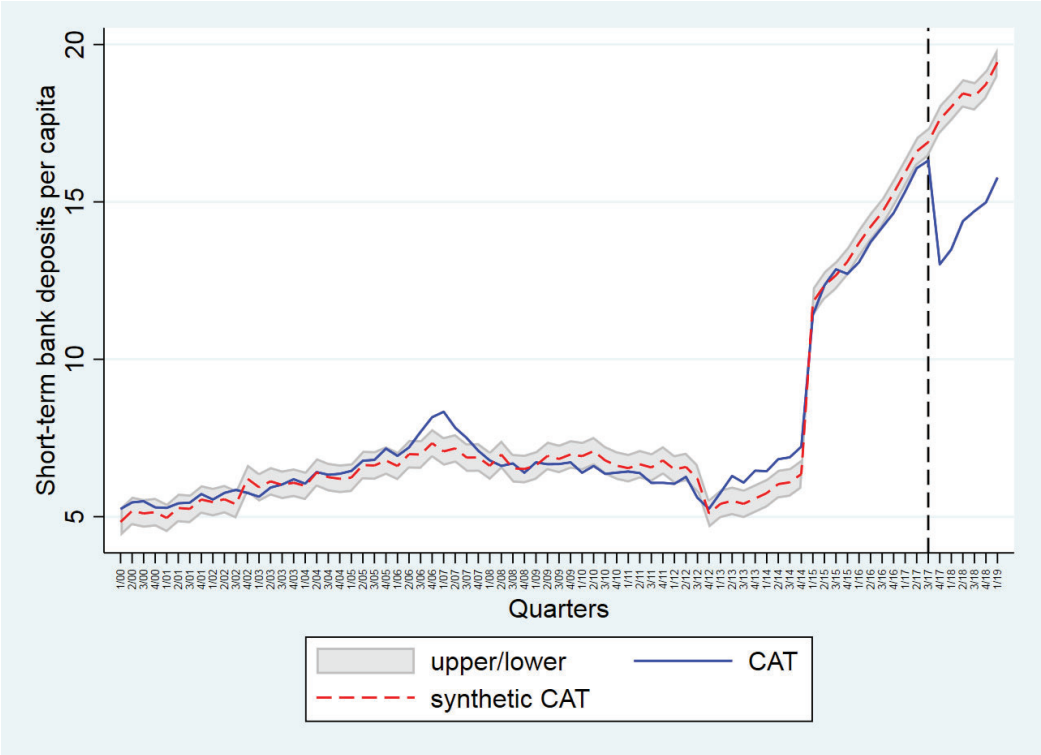


Figure 24: Catalonia *vs.* Synthetic Control – number of per capita capital increases. Sparse synthetic control.

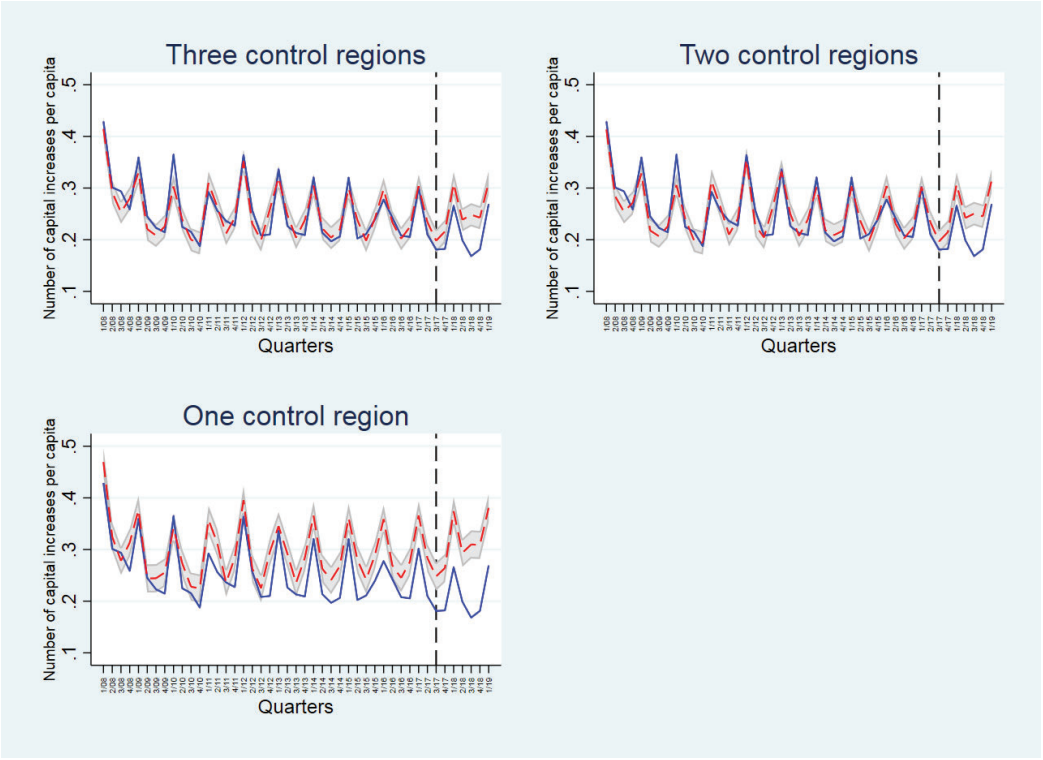


Figure 25: Catalonia *vs.* Synthetic Control – registration of new firms. Sparse synthetic control.

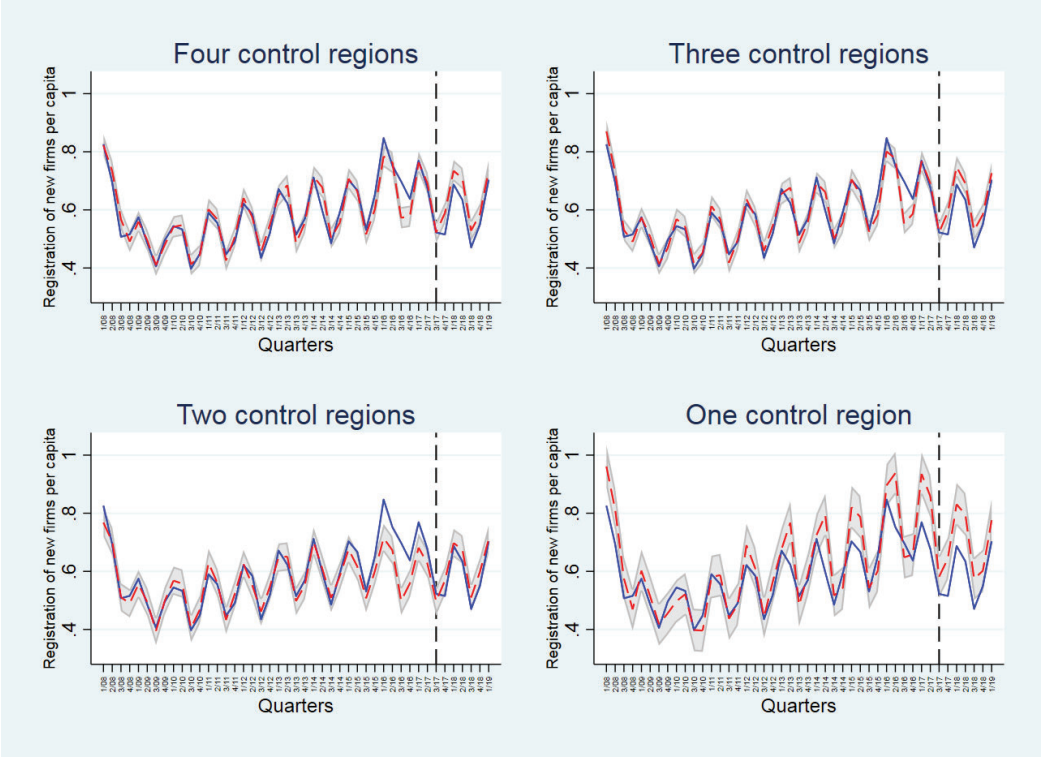
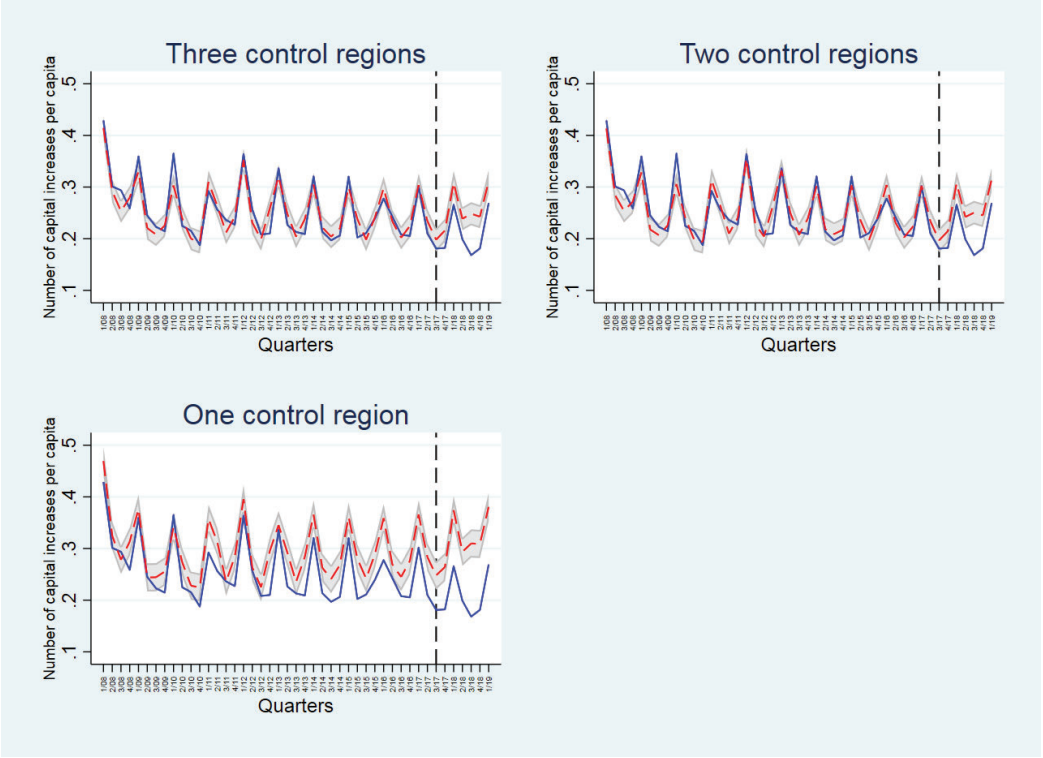


Figure 26: Catalonia *vs.* Synthetic Control – short term deposits. Sparse synthetic control.



Table

Table 1: Summary statistics of predictors in the pre-treatment period, per quarter.

VARIABLES	(1) N	(2) Mean	(3) Std. Dev.	(4) Min	(5) Max
Labor activity rate	1,190	57.27	4.629	41.76	69.58
Private investment/GDP	1,156	0.260	0.0611	0.138	0.475
Agriculture/GDP	1,190	0.0372	0.0294	0.000607	0.142
Industry/GDP	1,190	0.194	0.0674	0.0706	0.332
Construction/GDP	1,190	0.109	0.0325	0.0442	0.183
Service/GDP	1,190	0.660	0.0791	0.534	0.862
Population density	1,190	158.72	174.24	21.98	830.00
Population quota over 25 years old (y.o.) with primary education	1,156	0.337	0.0657	0.177	0.519
Population quota over 25 y.o. with first cycle of secondary education	1,156	0.245	0.0287	0.184	0.330
Population quota over 25 y.o. with second cycle of secondary education	1,156	0.214	0.0397	0.106	0.293
Population quota over 25 y.o. with first cycle of higher education	1,156	0.0842	0.0159	0.0497	0.135
Population quota over 25 y.o. with second cycle of higher education	1,156	0.0992	0.0346	0.0416	0.237
GDP per capita	1,190	5,459	1,108	3,179	8,443

Table 2: Summary statistics of the dependent variables, per quarter.

VARIABLES	(1) N	(2) Mean	(3) Std. Dev.	(4) Min	(5) Max
Unemployment rate	1,173	15.381	7.204	4.28	36.77
Per capita new firms	765	0.429	0.153	0.189	0.990
Per capita number of firms increasing capital	765	0.168	0.0670	0.0448	0.472
Per capita short-term deposits	1,309	6,551	5,359	1,674	37.198
Per capita long-term deposits	1,309	14,011	18,559	1,650	114,254

Table 3: Number of per capita capital increases: Real *vs.* Artificial Catalonia

Quarter/Year	Lower bound	Upper bound	Treated
Pre-treatment period			
1/2016	0.276	0.321	0.278
2/2016	0.210	0.254	0.243
3/2016	0.178	0.223	0.208
4/2016	0.203	0.247	0.206
1/2017	0.291	0.335	0.302
2/2017	0.210	0.254	0.210
3/2017	0.175	0.220	0.181
Post-treatment period			
4/2017	*0.194*	*0.239*	*0.182*
1/2018	*0.283*	*0.327*	*0.266*
2/2018	*0.216*	*0.260*	*0.199*
3/2018	*0.224*	*0.268*	*0.168*
4/2018	*0.219*	*0.263*	*0.181*
1/2019	*0.287*	*0.331*	*0.269*

Table 4: Number of capital increases p.c. in Catalonia. Percentage point difference with respect to the referendum between synthetic and real Catalonia.

Quarter/year	Number of capital increase losses in p.p.	Accumulated number of capital increase losses
4/2017	8.90	120
1/2018	7.42	220
2/2018	10.67	364
3/2018	31.74	793
4/2018	21.89	1,088
1/2019	7.66	1,191

Notes: Second column reports percentage point loss in number of per capita capital increases computed as the difference in post-referendum paths. Third column reports the loss in the number of capital increases computed as column 2 multiplied by the level of capital increases in 2017Q3 and accumulated thereafter quarter by quarter.

Table 5: New firms registered in p.c. terms: Real *vs.* Synthetic Catalonia

Quarter/Year	Lower bound	Upper bound	Treated
Pre-treatment period			
1/2016	0.760	0.833	0.847
2/2016	0.734	0.806	0.752
3/2016	0.536	0.608	0.698
4/2016	0.542	0.614	0.638
1/2017	0.728	0.800	0.769
2/2017	0.651	0.723	0.677
3/2017	0.486	0.559	0.522
Post-treatment period			
4/2017	*0.555*	*0.627*	*0.516*
1/2018	*0.704*	*0.776*	*0.687*
2/2018	*0.662*	*0.734*	*0.634*
3/2018	*0.493*	*0.565*	*0.471*
4/2018	0.545	0.618	0.550
1/2019	0.698	0.770	0.704

Table 6: Number of new firms registered in Catalonia. Percentage point difference with respect to the referendum between synthetic and real Catalonia.

Quarter/year	Nº of new firm losses in p.p.	Absolute nº of new firm losses
4/2017	14.38	560
1/2018	10.11	953
2/2018	12.09	1,424
3/2018	11.19	1,860
4/2018	6.01	2,094
1/2019	5.77	2,318

Notes: Second column reports percentage point loss in number of new firms per capita computed as the difference in post-referendum paths. Third column reports the absolute number of losses in new firms computed as column 2 multiplied by the number of new firm levels in 2017Q3 and accumulated thereafter quarter by quarter.

Table 7: Short-term deposits per capita: Real *vs.* Synthetic Catalonia

Quarter/Year	Lower bound	Upper bound	Treated
Pre-treatment period			
1/2016	13.071	13.681	13.086
2/2016	13.520	14.130	13.723
3/2016	13.910	14.520	14.188
4/2016	14.526	15.136	14.647
1/2017	15.076	15.686	15.332
2/2017	15.725	16.335	16.071
3/2017	16.042	16.652	16.316
Post-treatment period			
4/2017	*17.152*	*17.762*	*13.019*
1/2018	*17.640*	*18.250*	*13.495*
2/2018	*18.028*	*18.638*	*14.388*
3/2018	*17.834*	*18.444*	*14.705*
4/2018	*18.030*	*18.640*	*14.987*
1/2019	*18.799*	*19.408*	*15.774*

Table 8: Short-term deposits per capita. Percentage point difference with respect to the referendum between synthetic and real Catalonia.

Quarter/year	Deposit losses in p.p.	Absolute losses of deposits (millions euros)
4/2017	26.99	32,810
1/2018	27.07	32,890
2/2018	23.96	29, 130
3/2018	20.83	25,320
4/2018	20.30	24,680
1/2019	20.18	24,540

Notes: Second column reports percentage point loss in real short-term deposits per capita computed as the difference in post-referendum paths. Third column reports losses in millions of euros computed as column 2 multiplied by the deposit level in 2017Q3.

Table 9: Long-term bank deposits per capita: Real *vs.* Synthetic Catalonia.

Quarter/Year	Lower bound	Upper bound	Treated
Pre-treatment period			
1/2016	9.808	10.420	9.598
2/2016	9.083	9.695	8.953
3/2016	8.475	9.088	8.512
4/2016	7.519	8.131	7.360
1/2017	6.798	7.411	6.631
2/2017	6.038	6.650	6.396
3/2017	5.640	6.253	5.477
Post-treatment period			
4/2017	*4.864*	*5.476*	*4.378*
1/2018	*4.365*	*4.977*	*3.964*
2/2018	*4.047*	*4.659*	*3.847*
3/2018	*3.904*	*4.516*	*3.715*
4/2018	*3.681*	*4.293*	*3.585*
1/2019	*3.571*	*4.183*	*3.507*

Notes: The second column reports the value for synthetic Catalonia plus a standard deviation of the difference between the value for real Catalonia and that for the synthetic control before the referendum. The third column reports the value for synthetic Catalonia minus the previous standard deviation. The fourth column shows the value for real Catalonia.

Table 10: Long-term bank deposits per capita. Percentage point difference with respect to the referendum between synthetic and real Catalonia.

Quarter/Year	Deposit loss in p.p.	Deposit absolute loss (million)
4/2017	7.02	2,864
1/2018	6.16	2,516
2/2018	2.97	1,210
3/2018	2.97	1,211
4/2018	1.58	646
1/2019	1.16	472

Notes: Second column reports percentage point loss in real long-term deposits per capita computed as the difference in post-referendum paths. Third column reports losses in millions of euros computed as column 2 multiplied by the long-term deposit level in 2017Q3.

Table 11: Region placebo experiments for number of capital increases.

Region	Ratio	RMSPE ante	RMSPE post
Extremadura	1.980	0.018	0.036
Castile and León	1.789	0.012	0.021
Catalonia	1.495	0.022	0.033
Balearic Islands	1.321	0.034	0.045
Cantabria	1.280	0.031	0.039
La Rioja	1.156	0.021	0.025
Galicia	1.081	0.013	0.014
Castile-La Mancha	0.990	0.019	0.019
Canary Islands	0.984	0.018	0.018
Andalusia	0.910	0.009	0.009
AC of Valencia	0.898	0.009	0.008
Aragon	0.695	0.022	0.015
Asturias	0.700	0.019	0.013
Navarre	0.662	0.023	0.015
Basque Country	0.657	0.016	0.010
AC of Murcia	0.565	0.026	0.015

Table 12: Region placebo experiments for the registration of new firms per capita.

Region	Ratio	RMSPE ante	RMSPE post
Asturias	2.79	0.02	0.06
Andalusia	2.58	0.02	0.04
AC of Murcia	2.00	0.03	0.06
Basque Country	1.60	0.03	0.05
Cantabria	1.48	0.03	0.05
Catalonia	1.41	0.04	0.05
Aragon	1.29	0.03	0.04
AC of Valencia	1.27	0.02	0.03
Canary Islands	1.26	0.04	0.05
Castile and León	1.12	0.02	0.03
Galicia	1.10	0.02	0.02
Castile-La Mancha	0.70	0.07	0.05
Navarre	0.69	0.06	0.04
La Rioja	0.61	0.07	0.04
Balearic Islands	0.57	0.08	0.05

Table 13: Region placebo experiments for short-term deposits.

Autonomous Community	Ratio	RMSPE ante	RMSPE post
Catalonia	11.60	0.31	3.54
Aragon	7.23	0.24	1.73
AC of Valencia	4.38	0.19	0.84
Galicia	2.67	0.14	0.39
Balearic Islands	2.57	0.41	1.04
Cantabria	2.34	0.23	0.53
Navarre	1.94	1.11	2.16
AC of Murcia	1.20	0.17	0.20
La Rioja	1.03	0.23	0.24
Asturias	0.99	0.24	0.24
Castile-La Mancha	0.81	0.23	0.18
Castile and León	0.62	0.46	0.28
Canary Islands	0.31	0.93	0.29
Basque Country	0.23	1.23	0.29
Andalusia	0.13	0.29	0.04

Table 14: Estimated flows of capital from Catalonia to neighboring ACs

Quarter	Catalonia outflow	Aragón inflow	AC of Valencia inflow	Share of inflows in neighboring ACs out of total outflows from Catalonia
4/2017	32,810	2,976	4,777	23.63%
1/2018	32,890	2,634	5,648	25.18%
2/2018	29,130	2,648	4,592	24.85%
3/2018	25,320	1,999	3,716	22.57%
4/2018	24,680	2,350	3,378	23.21%
1/2019	24,540	1,878	4,031	24.08%

Notes: The first three columns report losses in millions of euros computed as in Table 8.

Table 15: Region placebo experiments for long-term deposits.

Autonomous Community	Ratio	RMSPE ante	RMSPE post
AC of Murcia	2.45	0.28	0.68
AC of Valencia	1.29	0.57	0.73
Andalusia	0.84	0.11	0.10
Basque Country	0.68	0.50	0.34
Aragon	0.65	0.46	0.30
Catalonia	0.52	0.30	0.16
Asturias	0.47	0.35	0.16
Extremadura	0.46	0.13	0.06
Galicia	0.34	0.46	0.16
Cantabria	0.27	0.31	0.08
Castile-La Mancha	0.19	0.25	0.05
Castile and León	0.17	0.60	0.10
La Rioja	0.16	0.64	0.10

Navarre	0.15	1.04	0.16

Table 16: Number of capital increases per capita – sparse synthetic control

Synthetic combination	*Countries and W-Weights*			
	MAD	LR	NAV	MUR
Four control countries	0.662	0.155	0.107	0.076
Three control countries	0.679	0.19	0.131	
Two control countries	0.701	0.299		
One control country	1			

Table 17: registration of new firms per capita – sparse synthetic control

Synthetic combination	*Countries and W-Weights*				
	MAD	BAL	AND	LR	ARA
Five control countries	0.354	0.321	0.034	0.122	0.169
Four control countries	0.311	0.339	0.211	0.139	
Three control countries	0.346	0.322			0.332
Two control countries	0.415		0.585		
One control country		1			

Table 18: Short term deposits capita – sparse synthetic control

Synthetic combination	*Countries and W-Weights*			
	NAV	CAN	MAD	VAL
Four control countries	0.189	0.379	0.239	0.192
Three control countries	0.225	0.528	0.247	
Two control countries	0.99	0.01		
One control country	1			