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Abstract

Recently, there has been a growing interest in newly developed econometric tools to conduct counterfactual analysis when a treated unit experiences a policy intervention, and an artificial control group has to be constructed. Adopting novel penalized synthetic control methods, we quantify the causal impact of the multi-modal Øresund fixed link on the adjacent cross-border regions in Skåne and Zealand. The treatment impacts on the intertwining metropolitan regions of Copenhagen and Malmö are positive. However, the impact on the Copenhagen metropolitan area is overlaid by the Great Belt strait fixed link, which was opened shortly before. An array of robustness tests supports our interpretations, and several appendices supplement the presentation in the paper.

JEL-Codes: C210, C540, H540, O110, R120, R420.

Keywords: Øresund fixed link, cities, infrastructure, economic development, synthetic control method.

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1. Introduction

In the European Union (EU), the development of trans-European networks plays a pivotal role. The importance of EU infrastructure development budgets reflects the view that transport infrastructure is one of the key mechanisms in order to achieve economic development, increase regional GDP per capita, promote employment, facilitate mobility, and enhance accessibility.[1]

As part of this overall strategy, it is sometimes claimed that that the Øresund region is a flagship model of cross-border European integration.[2] The coast-to-coast Øresund link opened in July 2000 is a combined bridge and tunnel link across the Øresund Sound which connects the Kattegat strait with the Baltic Sea. It comprises the Øresund tunnel between Amager and the artificial island Peberholm, and the Øresund Bridge between Peberholm and Lernacken. The multi-modal link is composed of a two-lane motorway and a dual rail track. The total length is 16 kilometers. The fixed link marks an upturn in mobility at the international, national and regional level along one of the busiest traffic routes between the Scandinavian peninsula and the European continent. It acts as the catalyst for the formation of a common labor and housing market and the strengthening of commercial ties which lies at the heart of the vision of the intertwining metropolitan areas of Copenhagen and Malmö. By this means it aims at increasing the attractiveness and competitiveness of the Øresund region.

Impacts of the fixed coast-to-coast link have been studied by papers at the juncture of economic geography, transportation economics, and regional economics. A descriptive-empirical overview of the regions involved, the policy mix, the bi-national political integration visions, and the achievements is provided by Nauwelaers et al. (2013). Selected economic geography analyses of cross-border innovation and knowledge creation across the strait are provided by Hansen (2013) and Lundquist and Winther (2006). A more specific analysis of the changed transport flows, economic trade and travel patterns is provided by Knudsen and Rich (2013) and Persson et al. (2022). An economic analysis of the effects of the coast-to-coast infrastructure project in the early years up to 2007, which is methodologically somewhat more like the present work, is provided by Achten et al. (2019). In addition to limiting the effects to the short time span up to 2007, the study does not consider numerous recent methodological advances in the policy evaluation literature. Chen and Lin (2020) have shown that the proliferation of cross-country transport networks and associated travel time reductions have facilitated the growth of horizontal and vertical cross-border investment and thus the creation of an evolutionary cross-border functional unit. This paper deals with the longer-term growth effects ascertainable in Skåne and Zealand. Was the fixed Øresund link a win-win, a win-lose or a lose-lose story for the adjacent Copenhagen and Malmö metropolitan areas?

[1] There is a multifaceted literature on the productivity of infrastructure investment. According to the IMF (2020, 33-54), increasing infrastructure investment by 1% of GDP would lift GDP by 0.25-0.5% in the first year, and up to four times that after the second.

[2] For the political rhetoric about the region, see https://www.webuildvalue.com/en/reportage/with-the-bridge-comes-a-region.html. Comparable statistics measuring the integration in the Øresund region in several areas is available in the Orestat Database (http://www.orestat.se/sv/oresundsdatabasen-engelsk).

The obvious challenge in estimating the causal effects of the large-scale Øresund infrastructure project is the endogeneity of the cross-border investment decision. Thus, the objective of the paper is to go beyond the official discourse of the EU that tends to take the benefits of cross-border integration for granted. We address this challenge by employing several recent developments of the synthetic control methodology in order to address the hard problem of causality. A further focus is on the identification of heterogenous regional effects of geographic connectivity. Thus, the contribution of this paper is twofold, one applied methodological and the other substantive.

The roadmap of the paper is as follows. Section 2 provides a taxonomy of penalized synthetic control estimators. The Øresund's relative peripherality globally will also be a topic of consideration. Section 3 discusses the data. Section 4 applies penalized synthetic control estimators to the Øresund fixed link, including robustness tests. The research thus bridges causal inference methods with a regional economic policy question. The final section 5 concludes. Supplementary analyses, estimates and robustness checks are relegated to appendices.

2. A Taxonomy of Recent Synthetic Control Estimators

The fixed coast-to-coast connection can be considered as a natural experiment. Fuchs-Schündeln and Hassan (2016, p. 925) define "natural experiments as historical episodes that provide observable, quasi-random variation in treatment". Valid inference about counterfactuals is essential for the key goals of scientific policy advice, answering "what if" questions, and estimating causal effects. The key problem in this context is the choice of the comparison regions that serve as counterfactual or control group to determine the effects of a treatment. Ideally, the control group has identical characteristics to the regions affected by the intervention so that the only difference is that one group received treatment, the other did not. Put differently, both units are comparable along all dimensions except for the treatment. In practice, this ideal comparison unit doesn't exist and thus identification of causal effects is challenging. One option is to pursue comparative case studies. In such case studies, the researcher compares the path of the aggregate outcome variable for a unit affected by the intervention with the evolution of the same outcome variable for another unit.[3] The problem here is that aggregate geographical entities differ widely in their characteristics, making the selection of the control unit highly problematic. Against the background of this methodological problem, this paper maps out the economic gains or losses after the opening of the fixed cross-country Øresund link employing synthetic control toolbox methods.

In response to the challenges of identification of causal treatment effects, the quasi-experimental synthetic control method (SCM) was developed in a series of seminal papers by Abadie and Gardeazabal (2003), Abadie et al. (2010) and Abadie et al. (2015). Since its inception, the quasi-experimental SCM for

[3] Examples of this direct comparison approach are Fuchs-Schündeln et al. (2010) and Young (1992).

estimating causal treatment effects for idiosyncratic historical events has become an important part of the toolbox used in the policy evaluation literature.[4]

The design of the SCM is like that of the traditional difference-in-difference setting where the goal is to find an appropriate control unit that is comparable to the treatment unit (the city or region that is exposed to an intervention). In this paper, as we are interested in testing the effect of the Øresund fixed link, the ideal solution would be to find a region that did not experience a large infrastructure project but is very similar to the Øresund region. However, in reality no one region is likely to match the Øresund region that closely. Therefore, the SCM approach employs a data-driven procedure that uses a weighted average of untreated control regions to construct an artificial or synthetic Øresund region. The goal of the synthetic Øresund region is to reproduce the trajectory of economic growth in the real Øresund region prior to the opening of the fixed link. Then, after the opening of the fixed link, the difference in the trajectories between the synthetic and real Øresund region can be summarized as the causal impact of the infrastructure project. In a nutshell, the synthetic Øresund region is the counterfactual growth trajectory that the Øresund regions in Skåne and Zealand would have experienced had the fixed link not been built (Abadie et al. 2015). The SCM allows for time-varying treatment, but the impacts may also be aggregated over the entire post-treatment period for retrieval of an average treatment effect of the treated (ATT) like difference-in-difference designs. Furthermore, the SCM approach can be applied to multiple treated regions.

Abadie et al. (2015) elaborate in detail on the limitation for inference in comparative case studies, given the absence of randomization and that probabilistic sampling is not employed to select sample units. However, inference can be undertaken by means of permutation methods or so-called placebo experiments. Verifying the baseline model results through alternation of the treatment time (in-time placebos) and attributing the treatment to regions in the donor pool (in-space placebos) offers two out of many ways to study whether the effects found are robust. For example, for the latter type of tests, each metropolitan region of the donor pool would individually serve as a treated region. This creates a fan-chart type of distribution of placebo effects. In turn, the baseline results would be deemed robust in case the impact of the treated region falls outside or is squarely at the upper range of the placebo tests. Despite the placebo test, the determination of the donor pool weights and thus the doppelganger remains the Achilles heel of the SCM procedure. This concern has encouraged recent methodological advances and extensions of the SCM approach.[5]

To achieve rigor, two state-of-the-art SCM estimation approaches formalizing the selection of controls and predictors using alternative data driven procedures shall be highlighted and employed below. A concomitant effect is that we illustrate the application and differences of recent methodological work.

[4] Athey and Imbens (2017, p. 9) consider the development of the SCM to be, "… arguably the most important innovation in the policy evaluation literature in the last 15 years". For a recent review of the popular technique, see Abadie (2021). Imbens and Wooldridge (2009) discuss the SCM among other recent developments in the econometrics of program evaluation, including differences-in-differences. For a sweeping and authoritative survey of the SCM literature, see Gilchrist et al. (2022).

[5] Abadie et al. (2015) caution that the SCM may not provide meaningful estimations if the synthetic unit does not closely match the treatment unit prior to the intervention.

As a first step, we provide a formal definition of the penalized and biased-corrected SCM of Abadie and L'Hour (2021). Suppose data for $J+1$ EU regions $j = 1,2,\cdots,J+1$ are given, where the first region ($j = 1$) is the treated region. The remaining $j = 2,\cdots,J+1$ regions are untreated donor pool regions. Let $Y_{j,t}^{I}$ be the outcome under intervention/treatment, while $Y_{j,t}^{N}$ represents the result under non-intervention/treatment. The total observation period comprises T periods, where T_0 periods precede the intervention. This results in $T - T_0 > 0$ post-intervention periods. The observable outcome is

$$Y_{j,t} = Y_{j,t}^{N} + \tau_{j,t} D_{j,t} \tag{1}$$

where $D_{j,t}$ is a (0,1) dummy variable indicating if region j has been treated in period t. The single-period intervention effect for the treated region j in period t is

$$\tau_{j,t} = Y_{j,t}^{I} - Y_{j,t}^{N} \ , \tag{2}$$

while the total intervention impacts over all post-intervention periods $T - T_0 > 0$ are given by

$$\left(\tau_{j,T_0+1}, \cdots, \tau_{j,T}\right) . \tag{3}$$

It is this sequence that is to be estimated. The region affected by the intervention and thus $Y_{j,t}^{I}$ is observable. On the contrary, the counterfactual outcome $Y_{j,t}^{N}$ is unobservable and must therefore be estimated for $T - T_0 > 0$. Classic SCM reproduces $Y_{j,t}^{N}$ as a linear combination of regions in the donor pool which have characteristics comparable to those of the treated region:

$$Y_{1,t}^{N} = \sum_{j=2}^{J+1} w_j Y_{j,t} \quad \forall\, t \tag{4}$$

It is prudent to highlight some assumptions implicit in the classical SCM approach. This includes in particular that the weights of the sparse similar controls are restricted to be nonnegative and to sum to one, i.e. the treated unit must lie within the convex hull of the control units to safeguard against extrapolation bias (Ho et al., 2007 and King and Zeng, 2006 and 2007). One can also put it this way: The convex hull provides a natural check of the distance from the data, thereby alerting one to the dangers of counterfactuals which require extrapolation.

The requirement for estimating the weights w_j of the individual donor regions is that they should reflect as closely as possible the pre-intervention development of the treated region. Let X_1 and X_0 be the $K \times 1$

vector of predictors of $Y_{j,t}$ in the treated unit $j = 1$ and the $(K \times J)$ matrix of predictors of $Y_{j,t}$ in the donor pool units $j > 1$, respectively. Classic SCM then chooses the weights w_j to minimize

$$\|X_1 - X_0 W\| = \sqrt{\sum_{k=1}^{K} v_k \left(X_{k,1} - w_2 X_{k,2} - \cdots - w_{J+1} X_{k,J+1}\right)^2} \tag{5}$$

subject to $\sum_{j=2}^{J+1} w_j = 1$ and $w_j > 0 \quad \forall\, j \in \{2, \cdots, J+1\}$. $\|X\| = \sqrt{X'IX}$ is the Euclidian norm and I is the identity matrix. Abadie and Gardeazabal (2003) have introduced the non-negative constants, $v_1, \cdots, v_k$, to standardize the predictors by making v_k equal to the inverse of the variance of $\left(X_{k,1}, \cdots, X_{k,J+1}\right)$. This modus operandi rescales all rows of $[X_1 : X_0]$ to have unit variance.

The main contribution of Abadie and L'Hour (2021) is to propose a penalized version of the classic SCM estimator to ensure uniqueness of the SCM solutions. In other words, the penalization term serves as a tiebreaker to ensure a preferable and sparse solution. For a penalization constant $\lambda > 0$, the penalized vector of weights $W^* = \left(w_2^*, \cdots, w_{j+1}^*\right)'$ on donor pool regions minimizes

$$\|X_1 - X_0 W^*\|^2 + \lambda \sum_{k=1}^{K} w_k^* \, \|X_1 - X_0\|^2 \,, \tag{6}$$

The positive penalty term λ in (6) aims at reducing interpolation bias and delivering a unique solution. As λ increases, each weight will attenuate and the set of donor regions with non-zero weight will become sparse.[6] Two borderline cases are readily apparent. As $\lambda \rightarrow \infty$, the penalized SCM estimator converges to nearest neighbor matching. On the contrary, if λ is close to zero, i.e. $\lambda \rightarrow 0$, then (6) mirrors the SCM that minimizes the sum of pairwise matching discrepancies among all solutions for the unpenalized SCM estimator. Cross-validation techniques and elastic net regression can be employed to choose λ. Elastic Net penalization adds the penalty term $\gamma\left(W_{i,n_1+1}^2 + \cdots + W_{i,n}^2\right)$ to the objective function given in equation (6). Abadie and L'Hour (2021) have shown that the penalized SCM estimator yields the aspired unique and sparse solution.[7]

Another issue discussed in the recent SCM literature is the potential biases of the classic SCM estimator. For example, Abadie et al. (2010) have analyzed the SCM bias properties in case that $Y_{i,t}^N$ is

[6] Viewed from the lens of non-uniqueness, there is less compelling reason to include a penalization term in the setting with a single treated unit. However also in the case, including a penalization term in the objective function is useful since it shifts the resultant W towards a less biased synthetic control.

[7] In order to evaluate the pre-treatment SCM fit, we also estimate Cohen's unit-free D statistic, which is the standardized mean difference in a baseline covariate between the treatment and the control group. Thereby, the D statistic should be below the assumed critical value of 0.25. $D = 0.25$ means that the imbalance between the groups is more than a quarter of a standard deviation for a particular variable (see, e.g., King and Zeng, 2006).

generated by a linear factor model by assumption.[8] As a consequence of the resulting bias, Abadie and L'Hour (2021) have suggested a bias-correction analogous to Abadie and Imbens (2011). Let $\mu_{0,t}$ be a sample regression function estimated by regressing the outcomes of the untreated $j > 1$ regions $Y_{j,t}$ on the predictor values X_0 of the untreated outcomes. Then the bias-corrected treatment effect from inexact matching at time t then is

$$\tau_{1,t} = \left(Y_{j,t} - \sum_{j=2}^{J+1} w_j^* Y_{j,t} \right) - \left(\sum_{j=2}^{J+1} w_j^* \mu_{0,t}(X_j) - \mu_{0,t}(X_1) \right) \tag{7}$$

The first term on the right-hand side in (7) is the classic SCM estimator. The second term uses a regression adjustment to correct for imperfect matching between the predictor values for the treated unit and the predictor values for the regions in the donor pool. Moreover, it is worth mentioning that the estimation procedure allows the calculation of significance levels using permutation-based inference methods.[9]

Another solution to concerns about SCM outcome trajectories has independently been suggested by Ben-Michael et al. (2021). More specifically, Ben-Michael et al. (2021) have proposed an augmented synthetic control method with a Ridge regression (Ridge ASCM), motivated by the inverse-propensity weighted estimator of Robins et al. (1994). The penalized Ridge ASCM generalizes the original SCM proposal to situations where a good pre-treatment match between treatment and synthetic regions is not achievable. A corollary of this is the abandonment of the non-negative weights' restriction in the ASCM approach. Ben-Michael et al. (2021) show that the preferred method to choose the weights is the solution to a constrained optimization problem, which can be implemented using a Ridge-regularized linear regression model to minimize the pre-treatment discrepancy between the observed outcome and the predicted outcome. In a nutshell, Ridge ASCM uses an outcome model to estimate the bias due to the poor pre-intervention match and then corrects for the bias in the original SCM estimate. The optimal penalty term in the Ridge estimator mitigating parameter proliferation is computed using a cross-validation approach minimizing the mean square error. The objective of the penalty term is to entail a dimension reduction that improves the performance of the subsequent estimator.[10] Finally, in the wake of the inference approach of Chernozhukov (2021), Ben-Michel et al. (2021) offer approximate confidence bands for the treatment effect.[11]

[8] As part of the analysis, Abadie and L'Hour (2021) have shown that if the SCM perfectly matches the pre-treatment characteristics of the treated region, i.e., $X_1 = X_0 W$, then the bias of the estimated treatment effect τ is bounded by the ratio of the magnitude of factor model noise term ε_{it} to the number of pre-treatment periods T_0.

[9] See Abadie and L'Hour (2021), p. 1823. For the uncertainty calculation in SCM also see Cattaneo et al. (2021).

[10] The improved pre-treatment fit has resemblances with the bias correction for inexact matching (Abadie and Imbens, 2011).

[11] Lastly, it is worth noting that further augmented SCM estimators have been suggested. Carvalho et al. (2018) and Masini and Medeiros (2021) have suggested sparsity-inducing LASSO estimators of counterfactual outcomes to prevent overfitting. In addition to establishing probability concentration results for the predicted counterfactuals, they have proposed asymptotically valid resampling-based inference methods for treatment effects. An obvious disadvantage of this approach is that the interpretability of the estimated counterfactuals and their weights is lost.

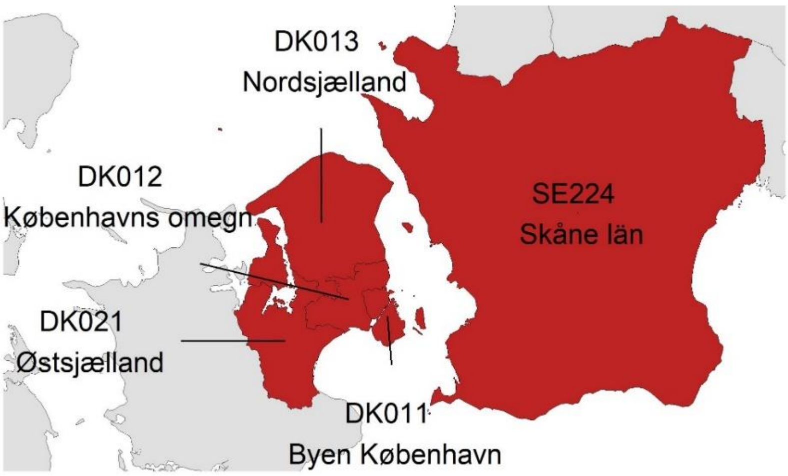
In the next sections we shall demonstrate the SCM estimators in action. The application in question is the cross-country Øresund fixed, which was opened in 2000. In light of the alternative state-of-the-art SCM estimators, we shall use the bias-correction proposed by Abadie and L'Hour (2021) for inexact matching on predictor values between a treated region and its synthetic control donor regions as our baseline method. The Ben-Michael et al. (2021) estimator will be used for robustness tests. Regardless of how the weights are constructed, SCM studies need to make further decisions regarding the donor pool. In the following, we will not only document these decisions, but also demonstrate the sensitivity regarding these decisions. This offers design recommendations applicable for other research issues.

3. Data

In this section we describe the data, economic characteristics of the Øresund region, and some stylized patterns emerging from the data. In order to develop a useful delineation of the treated region, it is crucial to identify the policy goals to which transport infrastructure investment is meant to contribute. Two objectives are given in this context: (i) to improve the global competitiveness of cross-border regions by cross-border transport links, enhancing efficiency; and (ii) to stimulate Pan-European international trade by improving strategic links in the transport networks. The following analysis is limited exclusively to the more proximate regional effects of the fixed Øresund link. Wider economic impacts beyond the vicinity of the link are not considered.

To investigate the regional economic growth effects of the Øresund fixed link, we use data from the Annual Regional Database of the European Commission's Directorate General for Regional and Urban Policy (ARDECO). The observation period covers years from 1981 until 2018. The ARDECO database contains consistent time-series data for European regions at detailed NUTS levels.[12]

Figure 1: The NUTS3 Regions Constituting the Copenhagen and Malmö Metropolitan Areas



[12] We use the NUTS 2016 classification. More information about NUTS classification can be found on https://ec.europa.eu/eurostat/web/nuts/background.

Note: Copenhagen is traversed by estuaries and canals. Running northeast–southwest, they bisect The NUTS3 region DK011.

The chosen regional delineation of the Øresund region is shown in Figure 1. It is constituted by the metropolitan areas of Copenhagen and Malmö. The formal definition includes the NUTS3 (Nomenclature Unités Territoriales Statistiques) regions Byen København (DK011), Københavns omegn (DK012), Nordsjælland (DK013) and Østsjælland (DK021) on the Danish side and the NUTS3 region Skåne län (SE224) on the Swedish side which constitute the Copenhagen and Malmö metropolitan areas respectively. The regional breakdown of the data into two metropolitan areas allows us to identify not only the overall effect, but also potential regional disparities across different places and economic agents. Such heterogeneities would be hidden by estimates of the total Øresund treatment effect. Even if the overall infrastructure project is a win-win situation, i.e., both adjacent regions in Skåne and Zealand gain, one may gain more than the other. In principle, however, a win-lose result is also entirely conceivable.

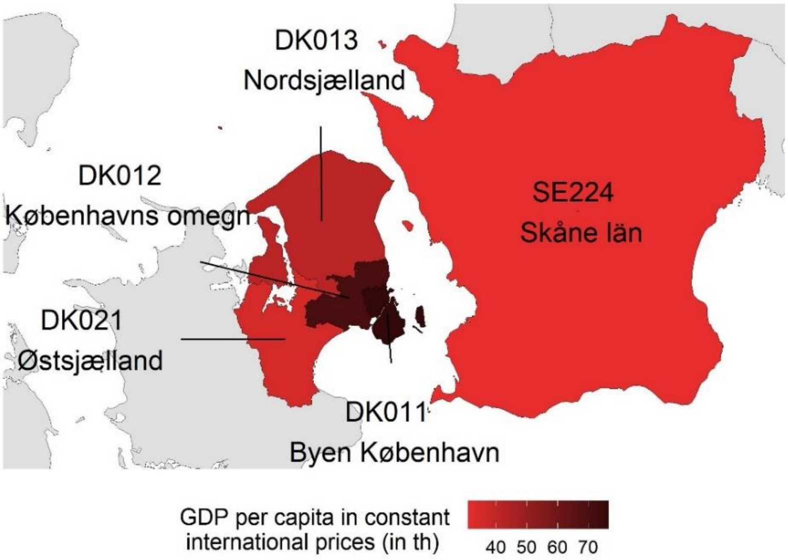
In 1999, the two parts of the Øresund region experienced a concurrent asymmetric macroeconomic shock. On 1 January 1999, the Eurozone came into existence and the common currency, euro was adopted by 11 member states in the Europe. Denmark and Sweden opted for retaining their national currencies. Since then, Denmark operates a target zone exchange rate regime with a narrow fluctuation margin of ±2.25% around its central rate vis-à-vis the euro, while the Swedish krona is a floating exchange rate which has experienced substantial exchange rate fluctuations in the last two decades. In order to eliminate exchange rate-related short-run fluctuations in per capita incomes, purchasing power parities are used.

The ARDECO database comprises regional GDP in constant prices national currency. Regional GDP adjusted for purchasing power parity (PPP) is not available. The corresponding adjustments have therefore been made with the PPP conversion factors available in the IMF WEO database. This results in the time series for the regional GDP in constant 2017 international US dollars. Finally, the regional GDP per capita data are derived by dividing PPP-adjusted GDP by total population. To illustrate the heterogeneous initial situation prior to the opening of the fixed left, Figure 2 presents regional PPP- adjusted per capita incomes across the Øresund metropolitan area prior to the opening of the fixed coast-to-coast link using coloured income intervals. Particularly noticeable is the Danish-Swedish regional income gap in the Øresund Metropolitan Area.

A crucial part in the SCM implementation is the choice of variables and covariates for the estimation of the synthetic control weights. Botosaru and Ferman (2019) and Ferman et al. (2020) have recently revisited the role of covariates in the SCM. They have provided Monte Carlo evidence that matching only on many pre-treatment outcomes is sufficient and renders further theory-inspired covariates redundant. Given this evidence, we also employ an all-lag "outcome only" SCM modelling approach.[13]

[13] This course of action has also been chosen by, for example, Billmeier and Nannicini (2013), Born et al. (2020), Dustmann et al. (2017), and Gobillon and Magnac (2016).

Figure 2: PPP Adjusted GDP per Capita in the NUTS3 Regions Constituting the Øresund Metropolitan Area in the Year 1999



Including unsuitable regions with substantial divergent behavior into the donor pool is a recipe of bias and overfitting (Abadie 2021, p. 401). To limit this risk, we restrict the donor pool in a fivefold manner. First, we include only metropolitan regions which have joined the European Union during the sample period considered.[14] Second, we exclude the Eastern European transition countries. Third, we remove all the EU outermost regions from the donor pool. The outermost regions are geographically remote regions defined in the "Treaty on the Functioning of the European Union". Fourth, we also remove the remaining Swedish and Danish regions from the donor pool.[15] Finally, the lowest 1% and highest 1% percentiles of the data are removed. Trimming the dataset helps to eliminate the influence of outliers or data points in the tails.[16] The result is that we are left with 169 potential metropolitan regions in the donor pool to create the synthetic (artificial) comparison unit, i.e., the doppelganger. Next, we discuss the empirical findings, gauge the sensitivity of our results along several dimensions, and uncover several nuanced empirical facts.

4. Quantifying the Øresund Fixed Link Impact

We now turn our attention to Øresund fixed link impact using the synthetic control estimator of Abadie and L'Hour (2021) using the software package developed by Wiltshire (2021). Following Zhou and Hastie (2005) and Friedman et al. (2008), the bias-correction was carried out by means of the Elastic Net regression. Elastic Net is a middle ground between Ridge Regression and Lasso Regression seeking to improve on both L1 and L2 regularization by combining them. In addition to the penalty strength term λ,

[14] For the list of EU metropolitan regions, see https://ec.europa.eu/eurostat/web/metropolitan-regions/background .
[15] Baum et al. (2020) and Goldmann and Wessel (2020) have shown that large-scale infrastructure projects impact the growth of adjacent regions in a variety of ways.
[16] An alternative to trimming would be the use of the novel machine learning SCM algorithm of Amjad et al. (2018). The machine learning approach first de-noises the data and then estimates ridge-regularized coefficients.

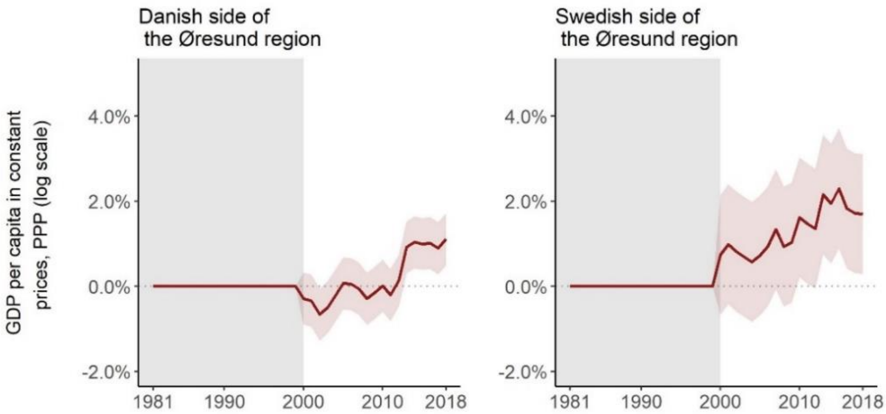
this requires the estimation of the Elastic Net mixing hyperparameter determining the mixing ratio of both L1 and L2 regularization. Both parameters were jointly determined relying on K-fold cross-validation minimizing the cross-validation mean squared error (CV-MSE). Thereby, data is randomly split into k subsets of approximately equal size. At a time, one-fold is treated as a validation set and rest of the folds $(k-1)$ as a training set. The process is repeated until each fold is used as a validation set. The k-fold CV estimate is calculated by taking the average of the k estimates of test error.

We now turn our attention to the bias-corrected SCM estimation results. From a theoretical perspective, the effects of transport costs have been central to the New Economic Geography, and detailed treatments can be found in Fujita et al. (1999), Combes et al. (2008), Puga (1999, 2002) and Allen and Arkolakis (2019), among others. Kay et al. (1989) have analyzed the welfare implications of building the channel tunnel between the UK and France, concluding that the fixed link is welfare enhancing. When trade costs are high, economic activity is dispersed. If trade costs fall, this promotes the agglomeration of activities with increasing returns. This is compounded by migration, eventually generating new core-periphery patterns which may imply the relative (and perhaps absolute) decline of some regions. Large cities can act centripetally, drawing in human and financial capital from elsewhere. But they can also act as centrifugal forces, diffusing growth outwards to surrounding regions and cities (through supply chains, commuter income effects, and demand-induced income gains). The ambiguity of the models and the evidence makes empirical work especially important.

The baseline bias-corrected SCM estimation results according to Abadie and L'Hour (2021) are given in Figure 3. The percentage GDP per capita differences between the treated regions and their synthetic counterparts are displayed. The comparison between estimated GDP gaps in percent for regular SCM versus bias-corrected SCM is given in Figure A1 in the Appendix. Complementary to this Table A1 provides detailed table with GDP gaps, permutation-based two-sided *p*-values, and the associated data-driven weights. The Appendix also makes explicit the contribution of each donor pool region to the counterfactual. The comparison of the estimation results for the Danish and Swedish sides of the Øresund region shows - especially in the very beginning - somewhat stronger impacts on the Swedish side. In contrast, positive impacts for the Copenhagen metropolitan region only occurred with a longer time lag. As a qualification, however, it must be noted that the initial effects are not significant on the Swedish side either. This result highlights the need to calculate SCM confidence intervals. Overall, the estimation results thus provide some evidence of a center-periphery integration type which has been categorized as "integration by specialization" by Decoville et al. (2013). In this functional division of space, cross-border commuting takes place primarily from the periphery towards the metropolitan center leading to a process of cross-border suburbanization.[17]

[17] After the bridge opening, labor market integration, measured by commuting, rose considerably – mainly from Sweden to Denmark (in 2018, 90% of the Øresund commuters lived in Sweden and worked in Denmark). Differences in salaries and housing prices – both higher in Denmark, and unemployment rates – higher in the Skåne region – drove this pattern.

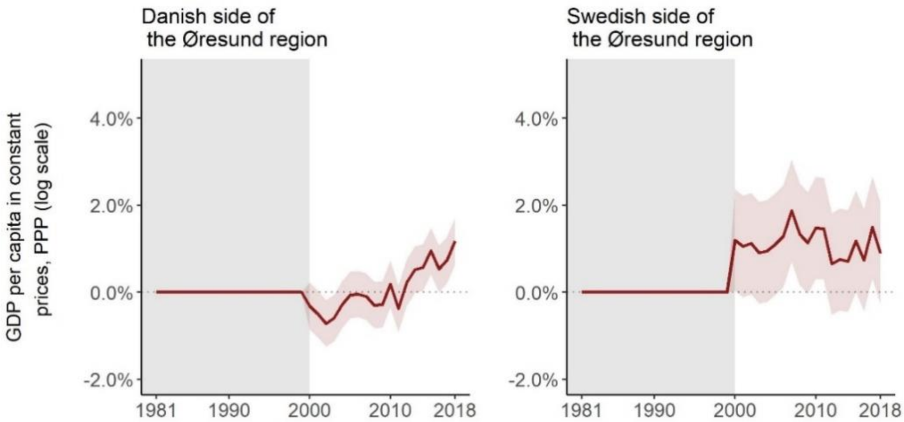
Figure 3: Benchmark Treatment Effects on GDP per Capita in %



Notes: The graphs show the bias-corrected percent difference of PPP adjusted GDP per capita in constant international US dollars for the Øresund regions and their synthetic counterparts. The pink coloured areas indicate the one standard deviation confidence intervals. The grey shaded area denotes the pre-treatment period. Source: Author's calculations.

Ferman et al. (2020) have recently warned that the flexibility to choose the counterfactual for the treated regions undermines the advantages of the SCM and potentially allows to cherry pick some statistically significant specifications even when there is no effect. For this reason, in a next step we present a robustness test with a markedly restricted donor pool. To this end we have trimmed the top 15% of the donor pool regions using the maximum pre-treatment period Euclidian distance threshold.[18] The underlying question is whether a noticeable change in the donor pool leads to substantially different SCM estimates. The re-estimated bias-corrected treatment effects are given in Figure 4.

Figure 4: Treatment Effects on GDP per Capita for the Trimmed Donor Pool Choice in %



Notes: The graphs show the bias-corrected percent difference of PPP adjusted GDP per capita in constant international US dollars for the Øresund regions and their synthetic counterparts for the more limited donor pool. The pink coloured areas indicate the one standard deviation confidence intervals. The grey shaded area denotes the pre-treatment period.

[18] Abadie and Vives-i-Bastida (2022) demonstrate persuasively that a large donor pool whose pre-treatment trajectories do not resemble the treated unit pre-treatment trajectory may lead to overfitting and large post-treatment errors. In order to avoid that, they propose trimming the donor pool based on the Euclidean distance.

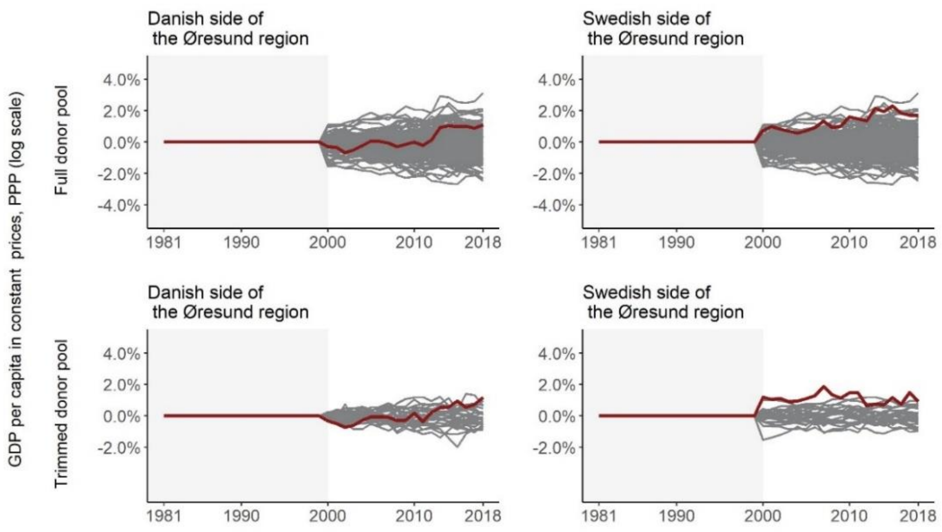
The findings in Figure 4 and Figure A2 and Table A2 in the Appendix reveal that the positive treatment impacts are robust with respect to this change in model specification. The outcome of large-scale cross-country infrastructure projects for the adjacent regions can basically be categorized as win-win (the adjoining regions in both countries come out ahead after the treatment), or win-lose (the adjoining regions in one country benefit to the detriment of the other). The nature and the distribution of the estimated SCM treatment effects in Figures 3 and 4 support the win-win scenario of better inter-regional connectivity. However, this is subject to the caveat of weak significance.

The estimation results are consistent with the hypothesis of Baum-Snow (2007) and Baum-Snow et al. (2017, 2020) that an enhanced transport infrastructure causes suburbanization. On the contrary, the estimation results contradict the "straw effect". Behrens et al. (2007) have extended the basic theoretical geography model to many regions and rather complex interaction patterns allowing not only for differences in the size of demand in different regions but also for differences in the quality of infrastructure links connecting them. The conceivable consequence is that despite better accessibility a smaller sub-region may be cast under a hub shadow if it is located very close to another sub-region with even greater demand. Behrens et al. (2007, pp. 639-640) have called this the "straw effect" because economic activity migrates to the larger sub-region as a result of reductions in transport costs as juice in a glass in sucked up by a straw. The corollary is an increasing income divergence triggered by better accessibility and increased intra-regional competition.

Can the impacts in Figure 3 and Figure 4 be interpreted in a causal sense? To verify the logic behind our identification strategy, we have also carried out placebo experiments (Abadie et al. 2010, 2015). These auxiliary analyses help the reader judge whether the estimated treatment impacts reliably measure the fixed link effect or instead reflect random error, misspecification, confounding variables, or something else. We expose each donor pool region to a placebo shock in the year 2000. When treatment is randomized, this becomes classical randomization inference.[19] The test is informative about bias to the extent that we believe that the fixed link treatment effect does not operate in the placebo regions. Figure 5 shows the actual GDP gaps (thick lines) as well as the various placebo GDP gaps in the hypothetical donor regions (gray lines) for the three subregions. The graphs confirm the above assessment that the impact in Skåne län is particularly significant.

[19] Abadie et al. (2010) acknowledge that the randomization inference assumption is restrictive, as treatment is not randomly assigned. In the absence of random assignment, they interpret the p-value as the probability of obtaining an estimate value for the test statistics at least as large as the value obtained using the treated case as if the intervention were randomly assigned among the data.

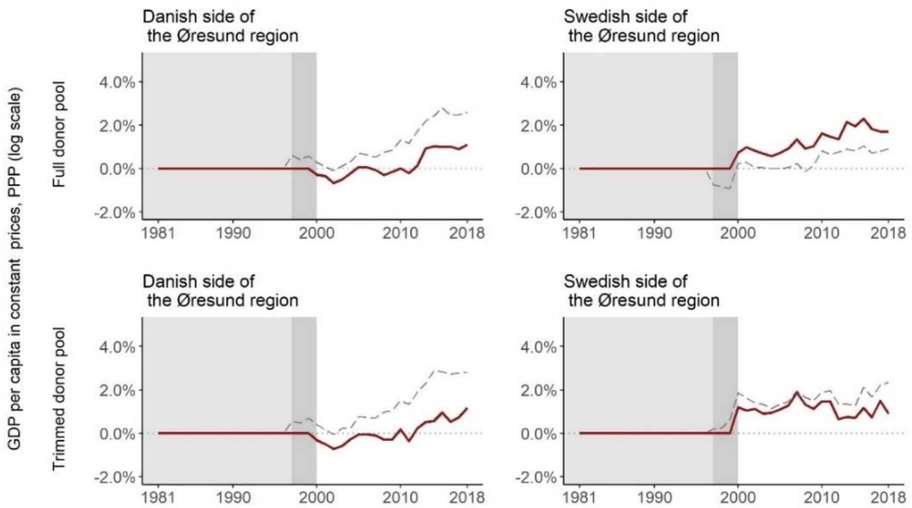
Figure 5: Placebo Tests in Space for GDP per Capita Gaps in %



Note: The chart shows the estimated model on each untreated region in the donor pool, assuming it was treated at the same time, to get a distribution of in-space placebo effects.

As an alternative to our placebo-in-space test, we reassign the treatment not in space but in time. In particular, we estimate the SCM with the fixed link opening falsely assumed to have occurred in the year 1997 when the partial opening of the fixed link occurred between Copenhagen and the small island Sprogø in the middle of the Great Belt. Since the fixed link connecting Sweden and Denmark was not opened until July 2000, the years 1997-1999 are essentially "false" years in which the intervention did not actually occur. Running these alternative models allows us to ascertain whether there is evidence that the fixed link – impact was present in the data prior to its opening. We plot the SCM with this hypothetical alternative treatment years in Figure 6 along with the average estimated treatment effect and two-sided p-values in the Table 1. The hypothetical brought-forward period has been highlighted in the graphs by a darker gray bar.

Figure 6: In-Time Placebo Test of GDP per Capita Gaps in %



Note: The charts show the calculated benchmark Øresund fixed link income per capita impacts for the 2000 completion date (solid red lines) and the impacts for the partial 1997 opening date (dashed black lines).

Table 1: Average Treatment Effects for in-time Placebo Tests of GDP per Capita Gaps in %

Treatment Timing	Danish side of the Øresund Area	Swedish side of the Øresund Area
Entire donor pool		
1997	1.13% (0.06)	0.27% (0.25)
2000	0.19% (0.29)	1.31% (0.02)
Trimmed donor pool		
1997	1.28% (0.00)	1.48% (0.00)
2000	0.07% (0.31)	1.12% (0.00)

Note: Table presents estimated average differences between the treated region and its' synthetic donor in % along with the two-sided p-values in the brackets.

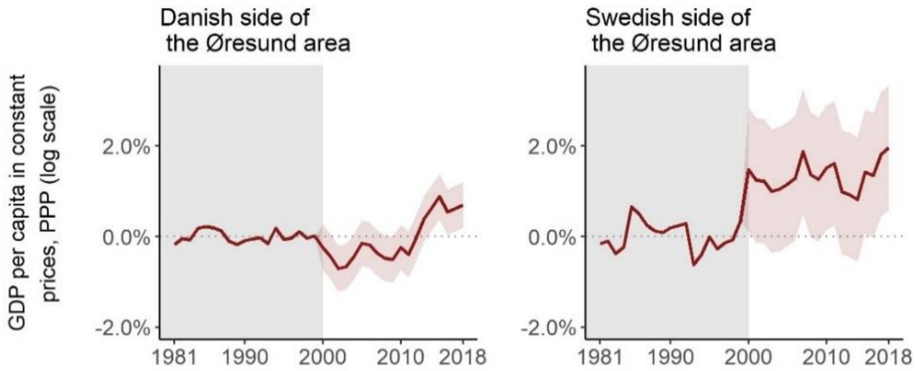
Figure 6 shows the baseline GDP gaps (solid lines) as well as the placebo GDP gaps (dashed lines) under the artificial assumption that the opening of the Øresund coast-to-coast connection and thus the treatment would hypothetically have already taken place in 1997. The graphical diagnostics provide insightful and nuanced results. Looking at the Swedish side of the Øresund metropolitan region, no increase in PPP-adjusted GDP per capita incomes is discernible within the frontloaded period 1997-2000. In contrast, a positive increase on the Danish side of the Øresund metropolitan region can be seen even before the year 2000 which is supported by the statistically significant average treatment effect reported in the Table 1. In addition to the inobservance of anticipation effects, the Great Belt strait fixed link (Storebælt) may have contributed to this as a confounding variable.[20] The Great Belt strait link crosses the Great Belt strait between the Danish islands of Zealand and Funen. It consists of a road bridge and a railway tunnel between Zealand and the small island Sprogø in the middle of the Great Belt, and a bridge for both road and rail traffic between Sprogø and Funen.[21]

The Great Belt strait fixed link crosses the main channel for exchange of waters between the Baltic Sea and the North Sea and has become the most important connection between East and West Denmark. Given the geographical setting, the new infrastructure implied a substantial reduction in travel costs, not only for connections between the two islands, but also between the Copenhagen area and the rest of Denmark and Europe. Previously taking one hour by ferry, the Great Belt can now be crossed in ten minutes. The fixed link opened to rail traffic in 1997 and to road traffic in 1998. Hence, one can say that the Great Belt Strait fixed link is a key element in connecting Denmark's different regions within the Danish archipelago with profound consequences for all transport modes and geographical areas.[22]

[20] In the case economic agents expected the opening of the fixed link in 2000 and behavioral changes occurred in advance, the SCM does not fully capture the true effect of the fixed link by looking at GDP per capita before and after the opening. This would generate a downward bias in the results and would therefore make it more difficult to identify treatment effects.
[21] For an analysis of Denmark's transition from interisland sea transport to fixed links, see Knowles, 2000).
[22] De Borger et al. (2019) have analyzed the impact of the Great Belt fixed link on total factor productivity of Danish firms using stochastic frontier techniques. The micro econometric evidence reveals large productivity effects in the adjacent regions of the fixed link.

The preceding Great Belt growth stimulus in Figure 6 may also explain the strikingly delayed impact of the completion of the Oresund link on the GDP growth of the Copenhagen metropolitan region in Figure 3 and 4. In other words, the in-time placebo tests illustrate that it is difficult to "partial out" the causal impact of infrastructure projects in geographical proximity which were completed in close succession. It is noteworthy that this interplay of both fixed links hasn't been considered in previous analyses. The in-time placebo tests thus add nuance to the existing literature.

Figure 7: Robustness Checks for the Treatment Effects on GDP per Capita Using the Augmented Synthetic Control Method Proposed by Ben-Michael et al (2021)

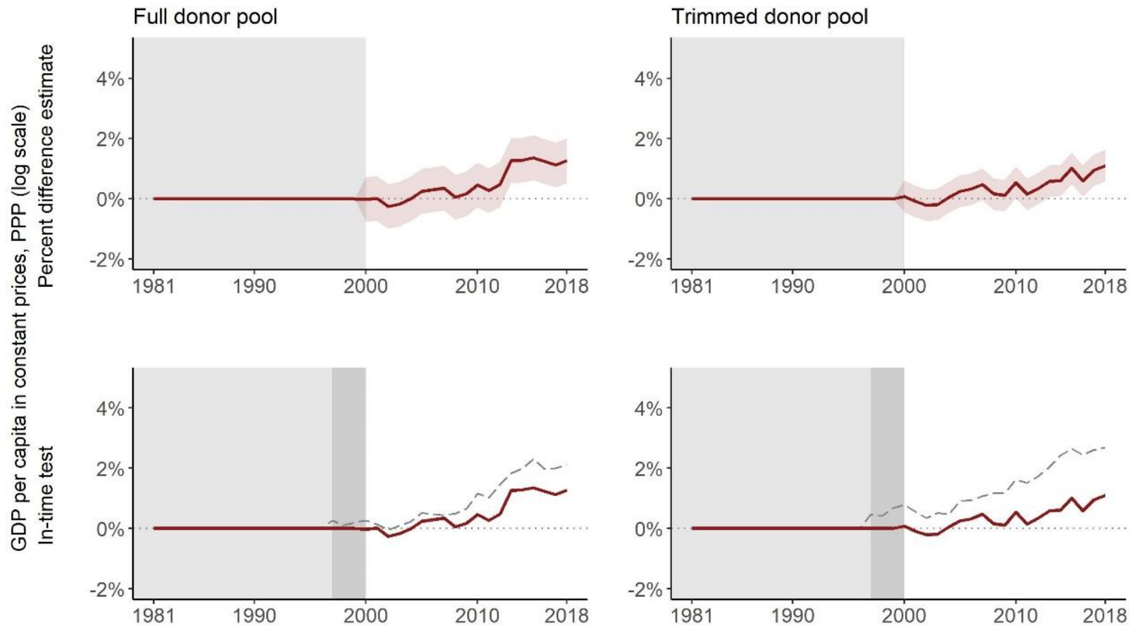


Note: The graphs show the percent difference of PPP adjusted GDP per capita in constant international US dollars for the Øresund regions and their synthetic counterparts derived using the augmented SCM of Ben-Michael et al (2021) using the trimmed donor pool. The grey shaded area denotes the pre-treatment period.

Finally, as a final robustness test, SCM estimation method sensitivity will be checked. To this end, we employ the augmented SCM proposed Ben-Michael et al (2021). As the method is best suited for a relatively small donor pool relative to the time dimensions, we carry out robustness tests using the trimmed donor pool with the most similar metropolitan regions only. The estimation results in Figure 7 indicate that the effect size and dynamics are robust to the method of choice.

The qualitatively and quantitatively different impacts for the metropolitan regions of Copenhagen and Malmö finally lead to the question of the overall impact for the Øresund region. For that purpose, we aggregate sub-divisional effects, weighting them with both regions' share of GDP to entire regions' GDP as proposed by Abadie and L'Hour (2021) and Cattaneo et al (2021). The aggregate effects are given in Figure 8 below. Overall, there is a consistently positive momentum that becomes increasingly significant over time.

Figure 8: Weighted Aggregate GDP per Capita Effects for the Whole Øresund Region



Note: Upper panel represents percent difference estimate and lower panel in-time placebo test estimates for whole Øresund region. The weights are calculated from the GDP shares of both regions with respect to the whole region.

As a final empirical analysis and robustness check, we present the estimation results for the panel data approach to program evaluation according to Hsiao et al. (2012) and Hsiao and Zhou (2019). For statistical inference, in-time placebo tests are again employed.[23]

Similarly to the SCM, RCM employs N cross-sectional units for $i = 1, \dots, N$, observed over t periods. The pre-treatment periods can be defined as $t = 1, \dots, T_0$, and the post-treatment periods as $t = T_0 + 1, \dots, T$. Unit $i = 1$ is defined as the treated unit, and all remaining units, $i = 2, \dots, N$, are potential counterfactual units. In case of $P > T_0 - 1$, Lasso is the method of choice for the model selection. Lasso enables estimating a sparse model by including all the predictors P and imposing a L_1 penalty, which minimizes the mean squared error

$$\min_{\delta_1, \delta} \left\{ \sum_{t=1}^{T_0} \left(y_{i,t} - \delta_1 - \delta' z_t \right)^2 + \lambda \left|\left|\delta\right|\right|_1 \right\}, \tag{8}$$

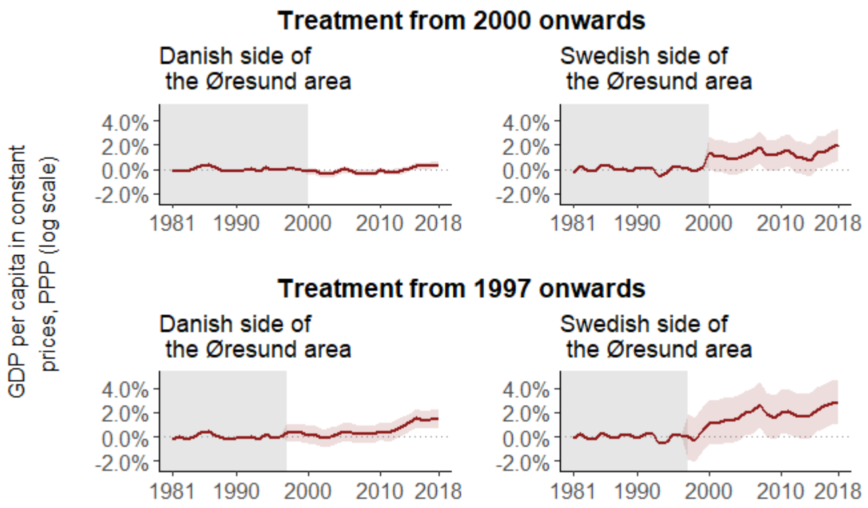
where $\lambda \geq 0$ represents the penalty parameter, and $\lambda\left|\left|\delta\right|\right|_1$ is the penalized L_1 norm of the vector δ. For choosing the optimal tuning parameter λ, we use K-fold cross-validation with $K = 10$. The model with optimal penalty parameter λ minimizing the cross-validation mean squared error is given by

[23] For other applications of the panel data approach for causal treatment evaluation, inter alia see Ouyang and Peng (2015) and Du and Zhang (2015). For an empirical comparison of SCM vs. RCM, see Gardeazabal and Vega-Bayo (1997).

$$\min_{\lambda} CVMSE(\lambda) = \frac{1}{K}\sum_{k=1}^{K}\left\{\frac{1}{n_k}\sum_{t\in F_k}(y_{1,t} - \widehat{\delta_{1,k}} - \widehat{\delta_k'} z_t)^2\right\}. \quad (9)$$

To evaluate if the baseline SCM estimations are robust, we estimate the RCM counterfactual for the trimmed sample, consisting of the 15% closest donors according to the Euclidean distance. The resulting GDP gaps of the fixed links are summarized in Figure 9.

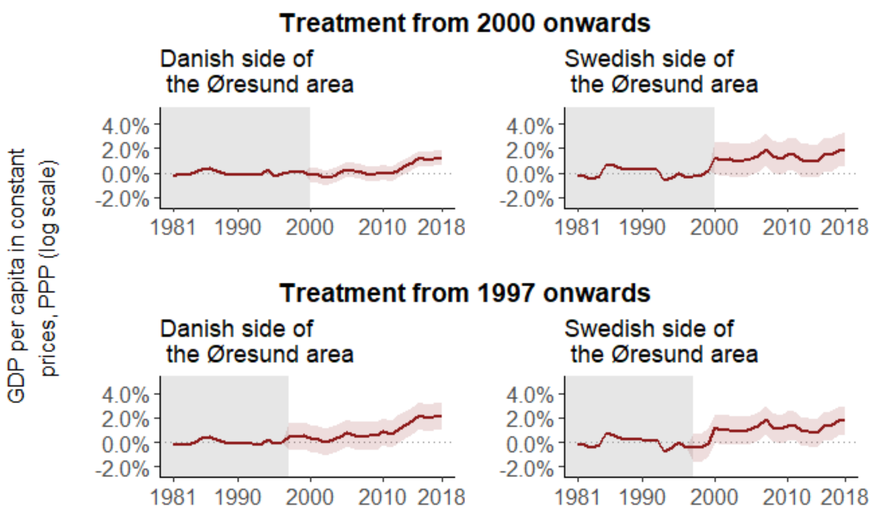
Figure 9: Estimated RCM GDP Gaps in % Using the Trimmed Donor Pool



Note: The Figure shows the GDP gap in % per capita of the treated unit (solid red line) and the RCM donor pool (black dashed line). The detailed regression results are available upon request to maintain the article's brevity.

In a further robustness check we solely employ the appropriate donor pool found using the bias-corrected SCM estimates. The associated RCM estimation results are given in the Figure 10.

Figure 10: Estimated RCM GDP Gaps in % Using the Bias-Corrected SCM Donor Pool



Note: The Figure shows the GDP gap in % of the treated unit (solid red line) and the RCM donor pool (dashed black line). The detailed regression results are available upon request to maintain the article's brevity. Source: Author's calculations.

The estimation results by and large confirm the previous results. By comparison, the fixed link treatment effect on the Swedish side is again more accentuated. Moreover, the in-time placebo tests do not lead to any change in the impulse-response functions and thus no indication of effects of the Great Belt fixed link crossing the Great Belt strait between the Danish islands of Zealand and Funen. On the contrary, the results for the Danish side are ambiguous. On the one hand, the effects are significantly smaller - as also shown before. On the other hand, they are again more significant when the year 1997 is chosen as the treatment date. This confirms the positive influence of the Great Belt fixed link on the economic development of Sjælland, the largest and most populous island of Denmark, lying between the Kattegat and the Baltic Sea.

5. Conclusions

Economic policy analysis is causal analysis. It quantifies policy impacts. It elucidates the mechanisms producing outcomes in order to understand how they operate. Causal analysis is grounded in a thought experiment - what would happen if determinants of outcomes are changing. To this end, credible hypothetical worlds are developed, analyzed, and tested using real world data.

There is a striking difference between the positive narratives of a win-win cooperation among policymakers and the more mistrustful reactions among experts. As a rule, the outcome of large-scale cross-country infrastructure projects for the adjacent regions can be categorized as win-win (the adjoining regions in both countries come out ahead after the treatment), win-lose (the adjoining regions in one country benefit to the detriment of the other) or lose-lose (the adjacent regions in both countries are worse off after the treatment). In order to evaluate treatment effects, it is necessary to construct a counterfactual outcome for the treated region for a scenario in which there were no treatment. The data-driven bias-corrected SCM allows to construct a counterfactual outcome for the treated unit from a set of potential donor regions. A reasonable conclusion of the SCM toolbox estimation results is that the overall impact of the fixed coast-to-coast link is positive. Furthermore, the causal positive impact is particularly pronounced on the Swedish side of the metropolitan area, while the positive outcome on the Danish side is masked by the completion of the Great Belt fixed link which took place shortly before.

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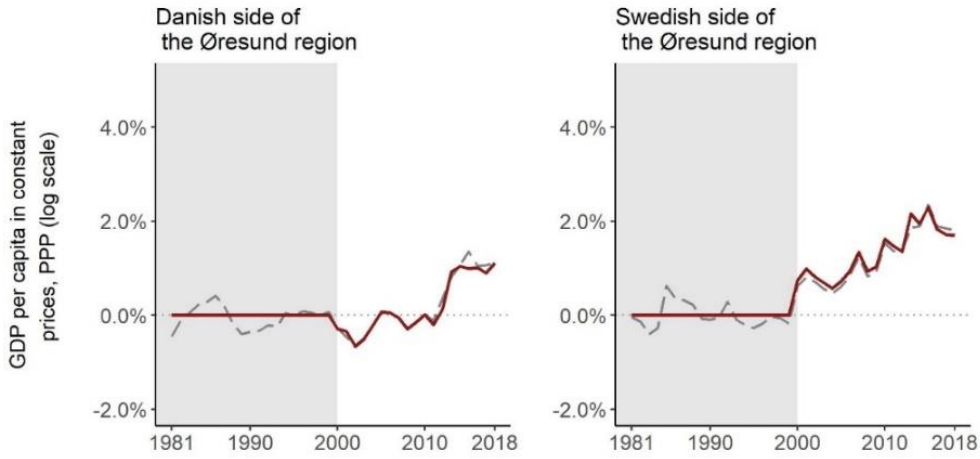
Appendix to the Paper

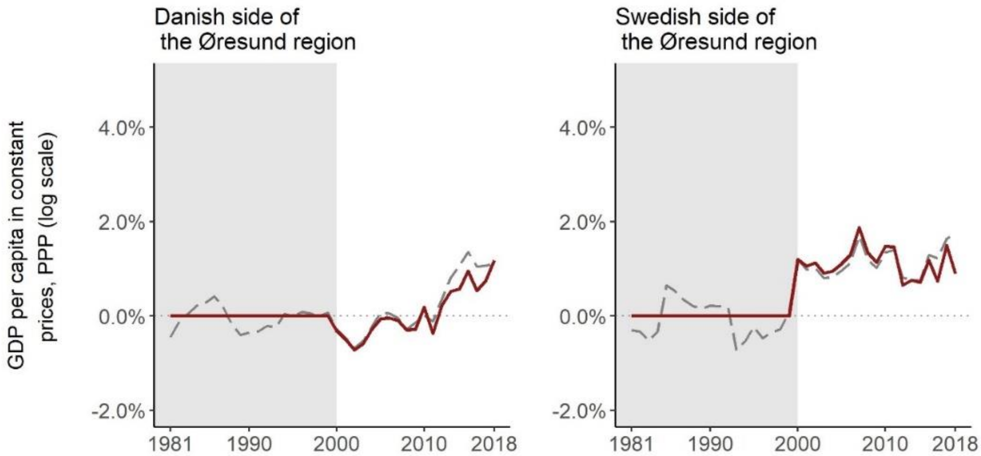
"Regional Economic Impacts of the Øresund Cross-Border Fixed Link: Cui Bono?"

Figure A1: Estimated GDP per Capita Gaps in % for Regular SCM versus Bias-Corrected SCM Using the Entire Donor Pool



Notes: The solid red lines represent the GDP per capita gaps between the Øresund region and the SCM donor regions estimated using bias-corrected SCM. The dashed grey lines represent the corresponding gaps using the regular SCM methodology. The detailed regression results are available upon request.

Figure A2: Estimated GDP per Capita Gaps in % for Regular SCM Versus Bias-Corrected SCM Using the Trimmed Donor Pool



Notes: See Figure A1.

Table A1: Estimation Results Using the Entire Donor Pool

	Danish Side of the Øresund Area		Swedish Side of the Øresund Area	
Year	Synthetic Control	Synthetic Control with Bias-Correction	Synthetic Control	Synthetic Control with Bias-Correction
2000	-0.030 (0.165)	-0.031 (0.129)	0.064 (0.053)	0.008 (0.482)
2001	-0.050 (0.106)	-0.037 (0.165)	0.084 (0.035)	-0.003 (0.447)
2002	-0.075 (0.088)	-0.073 (0.094)	0.075 (0.065)	-0.029 (0.259)
2003	-0.058 (0.159)	-0.055 (0.153)	0.059 (0.088)	-0.013 (0.418)
2004	-0.024 (0.353)	-0.024 (0.347)	0.049 (0.135)	0.002 (0.394)
2005	0.008 (0.324)	0.008 (0.324)	0.064 (0.118)	0.032 (0.224)
2006	0.006 (0.353)	0.006 (0.382)	0.087 (0.053)	0.019 (0.276)
2007	-0.004 (0.565)	-0.006 (0.529)	0.129 (0.029)	0.006 (0.365)
2008	-0.031 (0.382)	-0.032 (0.371)	0.087 (0.082)	0.007 (0.400)
2009	-0.016 (0.429)	-0.017 (0.429)	0.097 (0.059)	0.025 (0.324)
2010	0.001 (0.453)	0.001 (0.459)	0.159 (0.012)	0.083 (0.112)
2011	-0.012 (0.453)	-0.022 (0.382)	0.142 (0.029)	0.059 (0.200)
2012	0.04 (0.282)	0.015 (0.388)	0.146 (0.041)	0.089 (0.118)
2013	0.09 (0.141)	0.101 (0.106)	0.192 (0.012)	0.116 (0.094)
2014	0.117 (0.1)	0.114 (0.076)	0.196 (0.012)	0.148 (0.047)
2015	0.149 (0.053)	0.110 (0.100)	0.244 (0.006)	0.183 (0.024)
2016	0.116 (0.088)	0.112 (0.076)	0.197 (0.018)	0.154 (0.029)
2017	0.118 (0.082)	0.099 (0.100)	0.193 (0.024)	0.147 (0.047)
2018	0.124 (0.082)	0.122 (0.047)	0.188 (0.024)	0.145 (0.053)
ATT	0.025 (0.306)	0.021 (0.288)	0.129 (0.018)	0.136 (0.018)
RMSE	0.026	0.000	0.026	0.000
Cohens' D	0.175	0.000	0.189	0.000
Donor weights	Region	Weight	Region	Weight
	Milano Roma Prato Wien Bruxelles	0.579 0.265 0.117 0.027 0.013	Taranto Turku Blackburn - Blackpool - Preston Kirklees Málaga - Marbella Cádiz	0.398 0.377 0.143 0.066 0.014 0.002
# of potential donor regions		169		169

Notes: The Table presents the estimated differences between the treated region and the synthetic donor. The p-values in the brackets are calculated using the permutation method.

Table A2: Estimation Results for the Trimmed Donor Pool

	Danish Side of the Øresund Area		Swedish Side of the Øresund Area	
Year	Synthetic Control	Synthetic Control with Bias-Correction	Synthetic Control	Synthetic Control with Bias-Correction
2000	-0.030 (0.192)	-0.035 (0.038)	0.123 (0.000)	0.124 (0.000)
2001	-0.05 (0.154)	-0.054 (0.038)	0.102 (0.000)	0.109 (0.000)
2002	-0.075 (0.077)	-0.079 (0.077)	0.105 (0.000)	0.116 (0.000)
2003	-0.058 (0.231)	-0.065 (0.077)	0.083 (0.038)	0.094 (0.000)
2004	-0.024 (0.462)	-0.031 (0.192)	0.086 (0.038)	0.099 (0.000)
2005	0.008 (0.346)	-0.007 (0.538)	0.100 (0.038)	0.115 (0.000)
2006	0.006 (0.423)	-0.004 (0.577)	0.117 (0.038)	0.134 (0.000)
2007	-0.004 (0.577)	-0.011 (0.577)	0.177 (0.000)	0.196 (0.000)
2008	-0.031 (0.462)	-0.033 (0.346)	0.124 (0.000)	0.140 (0.000)
2009	-0.016 (0.423)	-0.031 (0.346)	0.106 (0.077)	0.118 (0.00)
2010	0.001 (0.423)	0.021 (0.346)	0.140 (0.000)	0.154 (0.000)
2011	-0.012 (0.577)	-0.041 (0.385)	0.146 (0.000)	0.152 (0.000)
2012	0.040 (0.192)	0.025 (0.308)	0.085 (0.154)	0.068 (0.077)
2013	0.090 (0.192)	0.057 (0.231)	0.081 (0.154)	0.079 (0.077)
2014	0.117 (0.154)	0.063 (0.115)	0.079 (0.154)	0.075 (0.115)
2015	0.149 (0.115)	0.105 (0.000)	0.135 (0.038)	0.123 (0.000)
2016	0.116 (0.154)	0.060 (0.154)	0.128 (0.038)	0.078 (0.038)
2017	0.118 (0.154)	0.082 (0.077)	0.172 (0.038)	0.157 (0.000)
2018	0.124 (0.154)	0.130 (0.000)	0.184 (0.000)	0.095 (0.038)
ATT	0.025 (0.231)	0.008 (0.308)	0.120 (0.000)	0.117 (0.000)
RMSE	0.026	0.000	0.040	0.000
Cohens' D	0.175	0.000	0.325	0.000
Donor weights	Region	Weight	Region	Weight
	Milano Roma Prato Wien Bruxelles	0.579 0.265 0.117 0.027 0.013	Dundee Ipswich Portsmouth	0.398 0.154 0.449
# of potential donor regions		25		25

Note: See Table A1. The weights in the Danish donor pool are unaltered, as trimming does not lead to any change in the potential Danish donor pool.