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Better out than in? Regional disparity and heterogeneous income effects of the euro

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Keywords

currency union, euro, synthetic control method, regional income disparity

JEL Classification

C21, E65, F45, O52, R1

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Better out than in? Regional disparity and heterogeneous income effects of the euro

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1. Introduction

On 1 January 1999, 11 countries fixed their exchange rates against the new virtual euro currency, trading their monetary autonomy for monetary integration. In the two decades since then, the euro has circulated its physical currency in 2002, survived through the Great Recession and European sovereign debt crisis and added eight new members to its number. On the other hand Denmark and Sweden, two conspicuous absentees, held referenda and opted out of the euro in 2000 and 2003 respectively. Danish voters preferred to keep the krone and maintain its peg with the euro, while Sweden chose to preserve its floating exchange rate. These two countries rejected the euro despite the fact that they were estimated to gain substantially from increased trade if they adopted the common currency.[1] While the aggregate referendum results were close in both countries, there was, however, considerable variation in voting patterns across different regions. This raises the question of whether the euro would have affected regional incomes heterogeneously: would the euro help poorer regions catch up or help the rich get richer? Who would have benefited most from the adoption of the euro?

Our research question draws on various theoretical areas such as the optimal currency area (OCA) theory, trade theories and growth or convergence theories. The euro has prompted a large amount of research into whether this currency union has improved the economic welfare of its members. The OCA theory pioneered by Mundell (1961) suggests several criteria for and against joining a currency union. A country which exhibits a high volume of trade, synchronous business cycles, a high degree of market integration and labour mobility with other potential union members would be expected to reap greater economic benefits from adopting a common currency. However, this comes at the cost of monetary policy autonomy and the ability to stabilise the national economy against asymmetric shocks. Moreover, the eurozone does not have a formal fiscal transfer mechanism to reallocate resources in the event that one country experiences an asymmetric shock, and additionally, the euro convergence criteria restrict national fiscal policy. The theoretically ambiguous welfare effect of currency unions thus motivates this empirical analysis.

Given that a country may experience net gains or losses from euro membership, this paper focuses on whether these gains or losses are distributed uniformly across regions within a country. Trade theories suggest that gains may not be spread evenly across the country, depending on comparative advantages or resource endowments. Theories of regional inequality predict conflicting effects: on one hand, disparities across regions should shrink with increased integration since diminishing returns to factors of production would cause

[1] For examples, see Frankel and Rose (2002) and Rose (2000).

poorer regions with higher returns to grow more quickly; on the other hand, regional disparities can be exacerbated by new opportunities for economies of scale and specialisation (see Bouvet (2010) and Bertola (2010)). Since a single country is made up of numerous regions with varying economic characteristics, it may not be safe to assume that the entire country experiences benefits or costs uniformly. While the literature has predominantly focused on the impact of the euro on the aggregate welfare of euro-adopting countries, we conduct a disaggregated analysis and provide a richer study of heterogeneous regional effects.

Specifically, we measure the effect of staying out of the euro on Danish and Swedish regional income and disparity. This analysis builds primarily upon the work of Puzzello and Gomis-Porqueras (2018), who assess the income effect of the euro on national GDP per capita, and Bouvet (2010), who examines the effect of the euro on regional income disparities. We use the synthetic control method to conduct a counterfactual analysis and estimate what the path of real GDP per capita would have been in each Danish and Swedish region at the NUTS 2 level had they adopted the euro in 1999. Using these estimates, we are able to calculate whether each region experienced net benefits or losses across 1999-2017 as a result of staying out of the euro. Within each country, we then compare the income disparity across regions in terms of their actual GDP per capita values against the disparity in the counterfactual GDP per capita estimates to determine whether regional income disparity would have been higher or lower had they joined the euro with the initial euro-adopters.

Using the synthetic control method, we find evidence that the euro has heterogeneous regional effects on income not only in terms of the direction of the effect, but also in terms of its magnitude. In particular, we find that the Capital Region in Denmark is the only Danish region better off without the euro in terms of GDP per capita, while all other Danish regions are worse off. In contrast, all regions in Sweden are better off without the euro, though to varying magnitudes; the income gains from staying out of the euro range as high as 23 percent in West Sweden. While many Swedish regions experience statistically significant benefits from staying out of the euro in the long run, the effect in Danish regions remain insignificant. The contrasting results between Denmark and Sweden highlight the difference in exchange rate regimes between the two countries. The effect in Denmark is small and largely insignificant due to the fact that the Danish krone was already fixed to the euro and additional income effects from adopting the common currency were unlikely to be substantial. On the other hand, the finding that all Swedish regions are significantly better off without the euro in the long run emphasises the importance of an independent monetary policy under floating exchange rate. For Sweden, potential efficiency gains and increased trade from monetary integration with the eurozone are outweighed by the benefits of macroeconomic stability offered by monetary autonomy. These findings are consistent across a wide range of robustness tests.

In terms of distributional effects, we find that regional income disparity in Denmark as measured by both the Gini coefficient and Theil index is higher as a result of staying out of the euro. In Sweden, disparity increases among the non-capital regions from staying out of the euro, but there is some convergence with Stockholm as a whole. Hence, the effect on regional disparity in Sweden is ambiguous and sensitive to the measure of disparity used. Regional disparity among Danish regions and non-capital Swedish regions is lower in the counterfactual scenario with the euro due to higher-income regions having a tendency to suffer larger losses when joining the currency union relative to lower-income regions. This could be due to further integration causing specialisation, which increases regions' vulnerability to asymmetric shocks and thus the cost of foregoing monetary policy autonomy. Furthermore, export-intensive regions in Sweden are particularly vulnerable to fluctuations in the international economy and benefit most from retaining their national currency and floating exchange rate.

This paper contributes to the existing literature both in terms of methodology as well as content. Firstly, while the primary focus of euro-related synthetic control literature has been on the aggregate effects of the common currency on income,[2] we take a distinctly novel approach and perform a disaggregated analysis. Since aggregate studies may be vulnerable to endogeneity and reverse causality issues, we exploit regional level exogeneity to assess the effect of euro adoption on income. Our analysis shows that joining a currency union can have heterogeneous effects upon regions within the same country not only with respect to the direction of the treatment effect, but also in relation to its magnitude. Secondly, the literature on the distributional effects of the euro has been relatively scarce,[3] and the direction of the effect is theoretically ambiguous. We fill this gap in the literature and show that the adoption of the euro can have consequences on regional income disparity. Finally, we extend upon the synthetic control literature which has primarily relied on falsification or placebo tests for drawing inference. One drawback of the suggested placebo tests in the literature is that they are only able to test significance over the entire post-treatment period. We draw upon the event study methodology and apply the synthetic control weights to construct unique confidence intervals and assess statistical significance in each post-treatment period. A key feature of the synthetic control method is that the treatment effect can vary over time. Hence, a dynamic inference test which can capture the significance of the treatment effect in

[2] For examples, see Puzzello and Gomis-Porqueras (2018), Verstegen, van Groezen, and Meijdam (2017), and Fernández and Garcia-Perea (2015). Also see Campos, Coricelli, and Moretti (2019) for the effect of EU membership and Born, Müller, Schularick, and Sedláček (2019) for the effect of Brexit.

[3] For examples, see Bouvet (2010) and Bertola (2010).

each period is more suited to this methodology, particularly in the case when the treatment effect may take time to accumulate or dissipate after an initial impact.

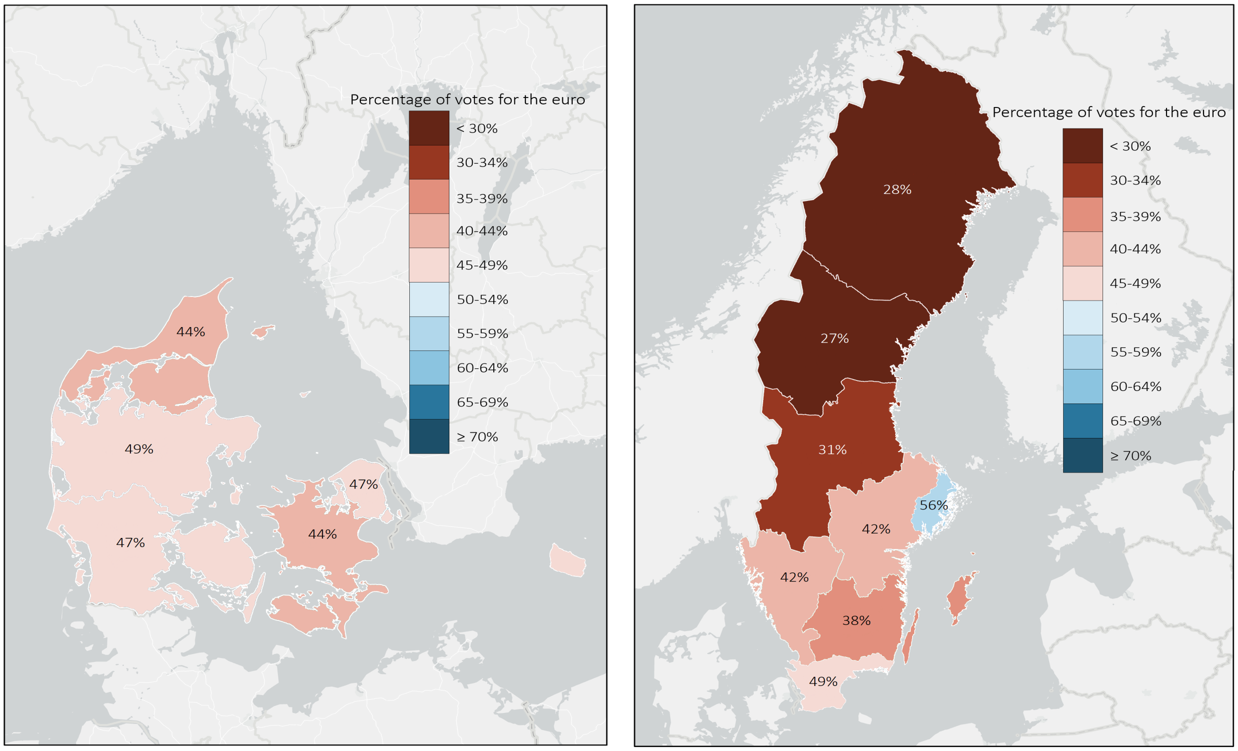
The rest of the paper is organized as follows. Section 2 describes the background behind the euro referendums in Denmark and Sweden. In Section 3 we describe the synthetic control methodology and data. Section 4 presents the estimation results, and Section 5 assesses their significance and robustness. We discuss the implications of our findings in Section 6 and conclude in Section 7.

2. Background

While European integration has developed over several decades, the formal foundations of the common currency—via participation in the Exchange Rate Mechanism II (ERM II)—were laid out in 1992 through the signing of the Maastricht Treaty. While all EU members are obliged to adopt the common currency, Denmark was one of two countries, along with the United Kingdom, which negotiated rights to opt out of the monetary union. Nonetheless, Denmark has voluntarily participated in the ERM II since 1999, pegging its krone to the euro. On the other hand, having joined the EU later in 1995, Sweden is obliged to adopt the euro as a member of the EU. However, Sweden has maintained a floating exchange rate regime by deliberately not meeting one of the euro convergence criteria.

Both Denmark and Sweden held referenda on eurozone membership in 2000 and 2003, respectively. However, the alternatives the voters were choosing between differed in the two countries due to their contrasting exchange rate regimes. With the krone's peg to the euro, Denmark had minimal monetary autonomy to forgo from adopting the euro in comparison to Sweden. In the Danish referendum, 47 percent of all voters were in favour of the common currency while 53 percent voted against, whereas in Sweden, the margin was slightly larger with 42 percent in favour and 56 percent against. Jupille and Leblang (2007) find that the Danish referendum was primarily determined by sovereignty and national identity considerations such as the replacing the name of the currency and transferring policymaking from the national to the supranational level. On the other hand, the Swedish referendum was determined by both political and economic factors, with the adoption of the euro implying a transfer of monetary policy autonomy as well as abandoning the floating exchange rate.

While the aggregate referendum results were close in both countries, the two countries do differ in terms of variation in regional voting patterns as shown in Figure 1. Here, the regions correspond to the second level of sub-national disaggregation of the Nomenclature of Territorial Units for Statistics (NUTS 2) by Eurostat, which is the level of disaggregation



(a) Denmark, 2000

(b) Sweden, 2003

Figure 1: Euro referendum results by region

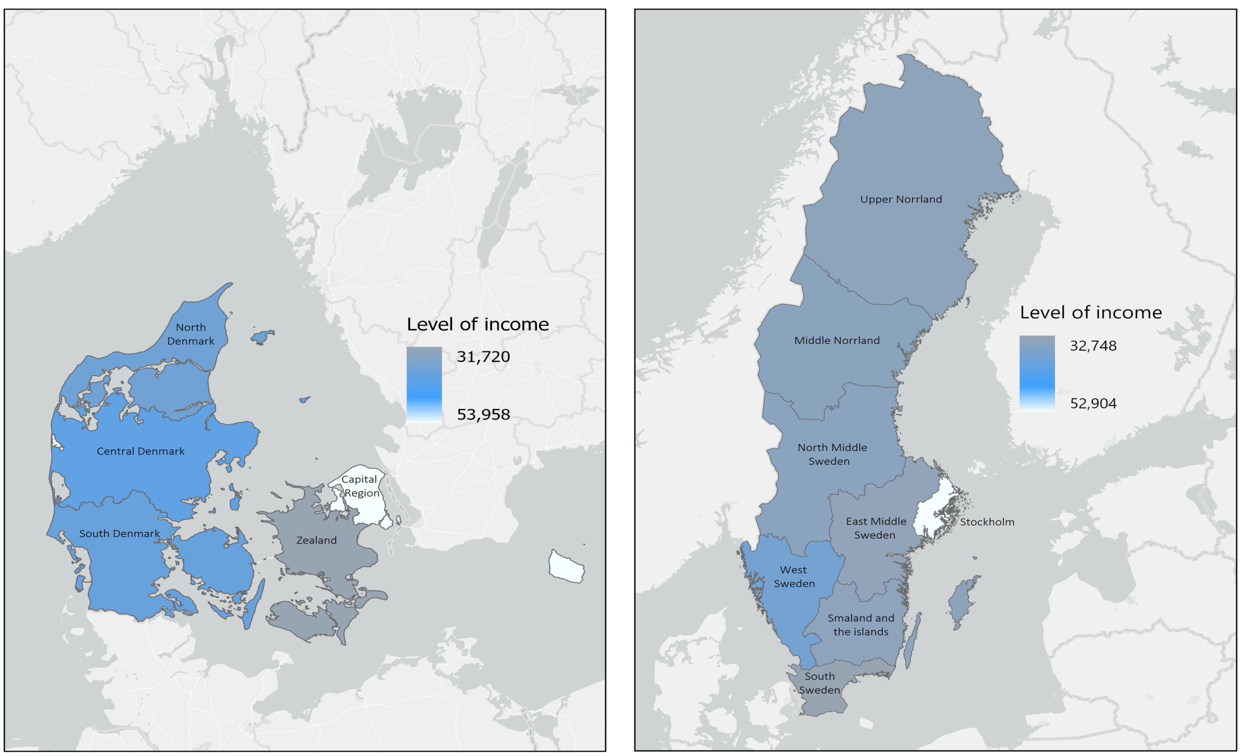
where regional policies of the EU are targeted.[4] As shown in the left panel, voting patterns were relatively homogeneous among Danish regions, with all regions narrowly rejecting the euro.[5] In contrast, some Swedish regions revealed a stronger preference for one side over the other. As shown in the right panel, most of the 'yes' votes were concentrated around Stockholm and southern Sweden, while the regions further north voted overwhelmingly against the euro. The proportion of votes for the euro varied greatly, being twice as large in Stockholm than in the northernmost Upper Norrland region. Jonung and Vlachos (2007) found Swedish regions with larger differences compared to the eurozone in terms of employment or industrial structures tended to vote against the euro, primarily driven by the fact that

[4]In Denmark, these five administrative regions are primarily responsible for healthcare, social services and regional development but do not levy taxes. In Sweden, no governance occurs at the NUTS 2 level, but each region is formed out of a small group of counties with the exception of Stockholm which is itself a single county. Coordination with the central government, healthcare, transport and culture are overseen at the county level in Sweden. The name of each region is shown in Table in the Appendix.

[5]Denmark re-defined its regions in 2007, abolishing 13 counties and some municipalities, forming the five regions depicted in Figure 1(a). The referendum results for these five regions have been estimated based on the counties absorbed by each region. At the county level, the proportion of votes in favour of the euro ranged from 41-51 percent.

monetary policy could act as a stabilisation tool for asymmetric shocks. In addition, areas with stronger export exposure tended to vote against the euro. In Denmark, less regional variation in voting behaviour was found, and those who voted against the euro were found at both ends of the political spectrum. Thus, a regional analysis of the effect of the euro would also reveal whether regions' voting preferences were in line with their economic interests.

The real GDP per capita for each region in both countries, measured in constant 2015 euros, are illustrated in Figure 2. For Denmark, the Capital Region (containing Copenhagen) had the highest level of per capita income with a a large high-tech entrepreneurial base, whereas its neighboring Zealand was the furthest behind in living standards with significantly lower productivity and education levels. South Denmark is particularly export-oriented, due to its shared border with Germany. For Sweden, Stockholm has the highest per capita income followed by West Sweden that contains the largest port in Scandinavia, Gothenburg. Traditionally, northern regions had been reliant on mining and natural resources, while southern regions were agriculture-based.



(a) Denmark, 2000

(b) Sweden, 2003

Figure 2: Regional real GDP per capita, constant 2015 euros

3. Methodology and Data

In this section, we describe our synthetic controls approach to analyse the effect of Denmark and Sweden's decision to opt out of the euro on per capita income of each region and the regional disparity and discuss some key assumptions, followed by the description of data we use.

3.1. Synthetic control method

The main methodology of choice is the synthetic control (SC) method proposed by Abadie and Gardeazabal (2003) and further developed by Abadie, Diamond, and Hainmueller (2010, 2015) The SC method has recently become a popular approach for comparative case studies and has also been used to quantify the treatment effects of shocks or policy interventions. Such exercises require the selection of an appropriate control or counterfactual to estimate the treatment effect, and the SC method provides a data-driven approach to construct a counterfactual. The rationale behind the SC method is that a weighted average of control units gives a better approximation of the counterfactual than a single control unit or simple average. This is particularly the case in macroeconomic analysis where it is often difficult to find a suitable comparative unit for a country or large region. The synthetic control is constructed using a weighted average of all potential control units such that the synthetic control unit best resembles the treated unit during the pre-treatment period in terms of the outcome variable and chosen predictor variables. The more closely the synthetic control unit matches the treated unit prior to the treatment date, the more confidence we have that deviations between the two after the treatment date can be attributed to the effect of the treatment. A long pre-treatment period is desirable, so that the synthetic control is able to pick up unobserved heterogeneity across regions over time. In our analysis, the treatment group refers to each region in Denmark and Sweden *not* adopting the euro, while the control regions are selected from euro-adopting countries.[6] Since the SC method exploits the pre-intervention data to form better counterfactual values, it is often preferred over other program evaluation methods, such as difference-in-differences, in comparative case studies, making it a widely used method in the short period of time since its inception. See Athey and Imbens (2017) and Abadie (2021) for a broader overview of the methodology and its applicability.

[6]This inverts the traditional notion of what is considered to be a treatment or control, but is not a novel approach. For example, Saia (2017) studies UK's decision not to adopt euro on its external trade using the SC method. For either notions, the treatment effect is simply the difference in per capital income level between the treated and synthetic control unit.

3.2. Key assumptions

Exogeneity of the treatment

To evaluate the causal effect of any policy intervention or treatment, a key assumption is that the treatment is exogenous and that there is no reverse causality or self-selection bias. At an aggregate level, the treatment would be endogenous if Denmark or Sweden made the decision to stay out of the euro based on potential economic outcomes. However, the treatment is exogenous to each region within Denmark and Sweden. This is because the decision was made at an aggregate level at a referendum in both countries and not determined by the economic considerations of any particular region. In fact, no single region was able to decisively influence the aggregate outcome nor a region can individually self-select into joining the eurozone on its own. Some regions may exert more influence in determining the referendum outcome, such as the capital regions due to their population size and political power. While this is possible, we argue that this was not the case in the Danish or Swedish euro referendum. In Denmark, the voting margins were very close in the Capital Region, the highest income region in the country (47 percent in favour). In Sweden, Stockholm was the only region with a majority 'yes' vote (56 percent); evident from the referendum result, however, they were not able to sway the final outcome. Obviously, the more disaggregated the regions, the more confident we can be in the exogeneity of the treatment. However, there are limitations to the degree of disaggregation that is possible with regional data availability. Hence, we use the same level of disaggregation as in Bouvet (2010) and Campos, Coricelli, and Moretti (2015).

Another argument in favour of exogeneity is found in Puzzello and Gomis-Porqueras (2018) and Gabriel and Pessoa (2020), who use the SC method on national level data for countries which joined the eurozone. They argue that these countries joined the currency union for political rather than economic reasons, since many economists believed that the eurozone was not an optimal currency area. Hence, they argue that reverse causality is not a concern for their analysis. Since the Danish referendum was primarily based on political rather than economic considerations, the decision to opt out of the euro could also be exogenous. However, we are cautious to claim that a country's decision to join the currency union was completely independent from economic considerations, particularly given expected gains from intra-eurozone trade of the magnitude estimated by Frankel and Rose (2002).[7] Hence, disaggregation adds another layer to the argument in favour of exogeneity of the treatment in addition to the reasoning that the euro referendum was purely for political reasons.

[7] Frankel and Rose (1998) stress that historical examination of a country's suitability for a currency union is inherently endogenous as economic and political considerations are often overlapping in practice.

No spillover effects

Another key assumption of the SC method is that the control group is unaffected by the treatment. That is, regions in the eurozone countries should not be affected by Denmark or Sweden's decision to not adopt the euro in 1999. This assumption would be violated if regions within the eurozone would have traded a lot more with Denmark and Sweden had they adopted the euro, improving the economic welfare of the control regions. This would lead the counterfactual scenario estimated by the synthetic control to look worse than it actually would have been. Given that Denmark and Sweden are small countries compared to the size of the rest of the eurozone, any possible spillover effects are likely to be small. The size of the spillover effects may also be attenuated by the fact that the euro boosts trade with even non-members (see Micco, Stein, and Ordoñez (2003), Saia (2017), Larch, Wanner, and Yotov (2018)), meaning that increased trade between euro and non-euro regions would be accounted for in the control regions to a certain extent.[8]

An important distinction to highlight is that any effects from the creation of the eurozone on Danish or Swedish regions are not a threat to this assumption nor a source of bias. This is because the treatment scenario is the existence of the eurozone without Denmark or Sweden's participation, while the counterfactual scenario is the existence of the eurozone with Denmark and Sweden's participation. Note that in both scenarios, the eurozone exists. Hence, any effect that the creation of the eurozone had on Denmark and Sweden in the treatment scenario is a valid and necessary part of the treatment scenario. The research question is not asking what the treatment effect would have been if the eurozone had never existed, but rather both the factual and counterfactual scenario is predicated on the existence of the eurozone. The only difference between the two situations is whether Denmark or Sweden also participate in the currency union.

No anticipatory effects

Our final key assumption is the absence of anticipatory effects in the predictor variables. This requires the effects of the treatment to only manifest after the treatment date. If regions anticipate the treatment before the treatment date and act accordingly, then the synthetic control is likely to underestimate the true effect. Given that none of the initial eurozone countries were expecting Denmark or Sweden to participate in the currency union prior to 1999, the treatment was anticipated, and this may pose a problem if the eurozone

[8]If any regions were to be affected by Denmark and Sweden's non-entry, it would most likely be the regions in the neighbouring countries such as Germany or Finland. We exclude regions from these countries from the control group as a robustness check in section 5.5.

countries diverted trade or investment from Denmark or Sweden to other future eurozone members prior to 1999. However, Frankel and Rose (2002) finds no evidence of trade diversion connected with the euro. On the other hand, if eurozone countries began to trade more with each other in the lead up to the introduction of the euro, this may increase the economic performance of the control countries. However, Saia (2017) finds that strong negative trade effects for the UK's decision to stay out of the euro would have begun only from the introduction of the physical currency in 2002. Nevertheless, since the synthetic control algorithm chooses weights to match the treated unit as closely as possible prior to 1999, any anticipatory effects are likely to cause an underestimation rather than overestimation of the treatment effect.[9]

3.3. Data

We use annual data from Cambridge Econometrics' European Regional Database and the Annual Regional Database of the European Commission's Directorate General for Regional and Urban Policy (ARDECO) which provide socio-economic data at sub-national regions. Sub-national regions are classified according to Eurostat's Nomenclature of Territorial Units for Statistics (NUTS) where NUTS 2 refers to the second level of disaggregation below the national territory. This is also the level of disaggregation at which regional policies of the EU are directed. Denmark is divided into five NUTS 2 regions while Sweden is divided into eight. The database also includes regions from all 12 initial euro-adopting countries[10] providing further disaggregated 166 regions.

While using disaggregated data greatly increases the number of potential control units available to construct the synthetic control, care must be taken in selecting control groups for each region. Control regions which suffer large structural shocks which would not have been felt in the treatment region should be excluded from the control group. We exclude regions from East Germany due to data unavailability prior to 1991 as well as all regions of Greece due to the sovereign debt crisis since 2010. Following Abadie, Diamond, and Hainmueller (2015), we also exclude Ireland due to its expansion from the 1990s to late 2000s fueled by foreign direct investment.[11] In the end, we work with 130 regions as potential donor pool for control units.

[9] As a robustness check, the treatment can be assigned to an earlier year to account for anticipatory effects. This is a further advantage of the synthetic control method over traditional panel models as it allows for the effect of the treatment to vary over time.

[10] Greece joined the virtual euro in 2001 but circulated the physical currency in 2002 along with all the other 11 initial euro-adopting countries.

[11] Other regions excluded are Groningen in the Netherlands due to accounting changes, overseas French territories and small islands in Spain and Portugal.

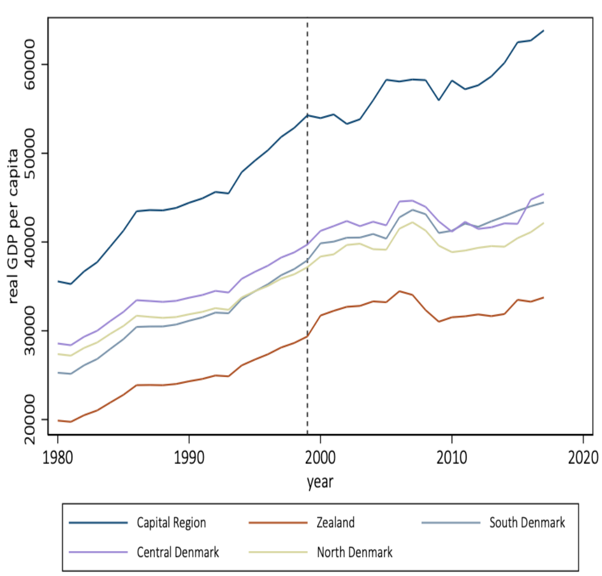
As it is crucial to select control regions with similar characteristics to the treated region, we select regions which do not significantly deviate in real GDP per capita from the treatment region. This implies that each treatment region has a unique donor pool and weights. This is to avoid interpolation biases, where large differences between the control regions and the treatment region are averaged out. For non-capital regions in both countries, the control group comprises of regions that do not deviate more than 30 percent in GDP per capita from the treated region. Due to the capital regions in both countries being one of the richest in Europe, we raise our threshold to 35 percent to find similar regions in the control group.[12] For Swedish regions, we make an exception to the aforementioned criteria to include Finnish regions in the control groups. The inclusion of Finland is important as it joined the EU in 1995 along with Sweden (and Austria). Thus, their inclusion should enable us to disentangle the effects of euro adoption from EU membership. Campos, Coricelli, and Moretti (2019) find that EU membership had only small positive effects for Austria, Finland and Sweden, with Sweden having the smallest gains. This indicates that EU membership is not a structural shock which would warrant the exclusion of Austria and Finland from the control group. Both Finland and Sweden experienced financial crises in the early 1990s; thus, Finnish regions tend to be good control units for Swedish regions in terms of improving the pre-treatment fit as was shown in Gyoerk (2017). This may raise the concern that the respective crises affected these two countries in different ways structurally; however, to address this concern we exclude Finnish regions from the control group to check the sensitivity of the Swedish synthetic controls in Section 5.5.

Our sample period begins in 1980 due to data availability at the regional level, allowing a pre-treatment period of 19 years. We select 1999 as our intervention year similar to Saia (2017).[13] Having a longer pre-treatment period also improves the ability of the synthetic control to capture the unobserved heterogeneity between regions across time. Further, the assumptions of no anticipatory or spillover effects are not overly onerous in this treatment scenario and are likely to be small as discussed in Section 3.2. Even if the effects of the euro only start to emerge with the circulation of the physical currency in 2002, the advantage of the SC method is that setting an earlier intervention year does not bias the estimate of the treatment effect as discussed previously. The post-intervention period therefore starts in 1999 and ends in 2017, totalling 19 years.

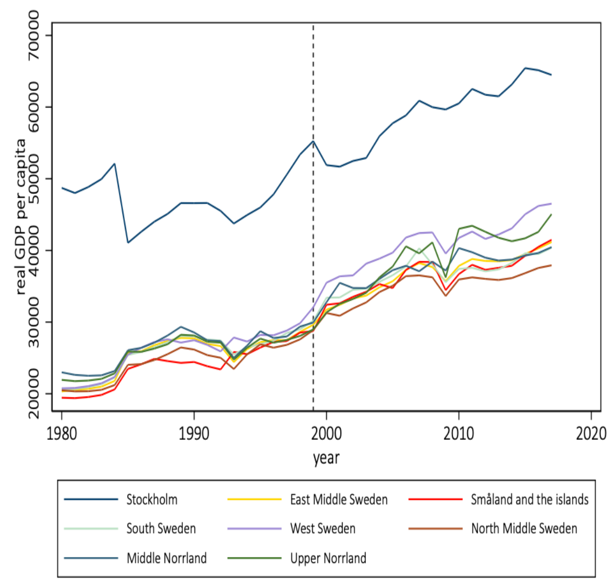
[12] Adjustments to these thresholds up or down by 10 percent have minimal effects on the main results.

[13] On the other hand, Puzzello and Gomis-Porqueras (2018) choose 1995 as the intervention year to account for anticipatory effects without biasing the treatment effect. We use the actual year of euro introduction as 1995 is also the year in which Sweden joined the EU.

The outcome variable is the regional real GDP per capita measured in constant 2015 euros. The predictor variables we use for the synthetic control estimation are all measured at the regional level: real GDP per capita averaged over three pre-intervention sub-periods of 1980–1986, 1987–1992 and 1993–1998, total population, total employment, investment to GDP ratio, hours worked, and share of gross value added by sector. These variables have been chosen based on Puzzello and Gomis-Porqueras (2018) and prior literature, however some price and trade variables are not measured at the regional level.



(a) Danish regions



(b) Swedish regions

Figure 3: Regional real GDP per capita, constant 2015 euros

As we are also interested in the profile of regional income disparity and its dynamics, we plot the profile of real GDP per capita in constant 2015 euros for all sample periods in Figure 3 with the vertical dashed line representing the intervention year. As shown on the left, the Capital Region in Denmark, containing Copenhagen, unsurprisingly is the region with the highest income per capita, while Zealand is the lowest. From a visual inspection, it appears that the regions have been diverging post 1999 in terms of income. For Sweden, the Stockholm region has the highest income, while the other Swedish regions are closer together. Since 1980, it appears that the other regions are closer to Stockholm than they were previously,[14] however the spread between the non-capital regions has increased too. As a simple before-and-after comparison is not indicative of a causal effect of non-adoption of

[14]There is a large drop in GDP per capita in Stockholm just prior to 1985, mainly coming from a fall in GDP through gross fixed capital formation. Since we cannot conclusively identify the cause of this event, we begin our analysis for Stockholm in 1985 to exclude this drop.

the euro, we compare the post 1999 disparity in regional incomes in this actual scenario with the counterfactual scenario constructed using the synthetic controls to examine whether this disparity would have been larger or smaller, had each countries decided to adopt the euro.

4. Results

In this section, we present the synthetic control estimates for each region to examine whether they each experienced net gains or losses from staying out of the euro both graphically and quantitatively. We then measure the disparity across regions in the counterfactual scenario and compare this against the actual disparity observed. The statistical significance of these results are shown in Section 5.

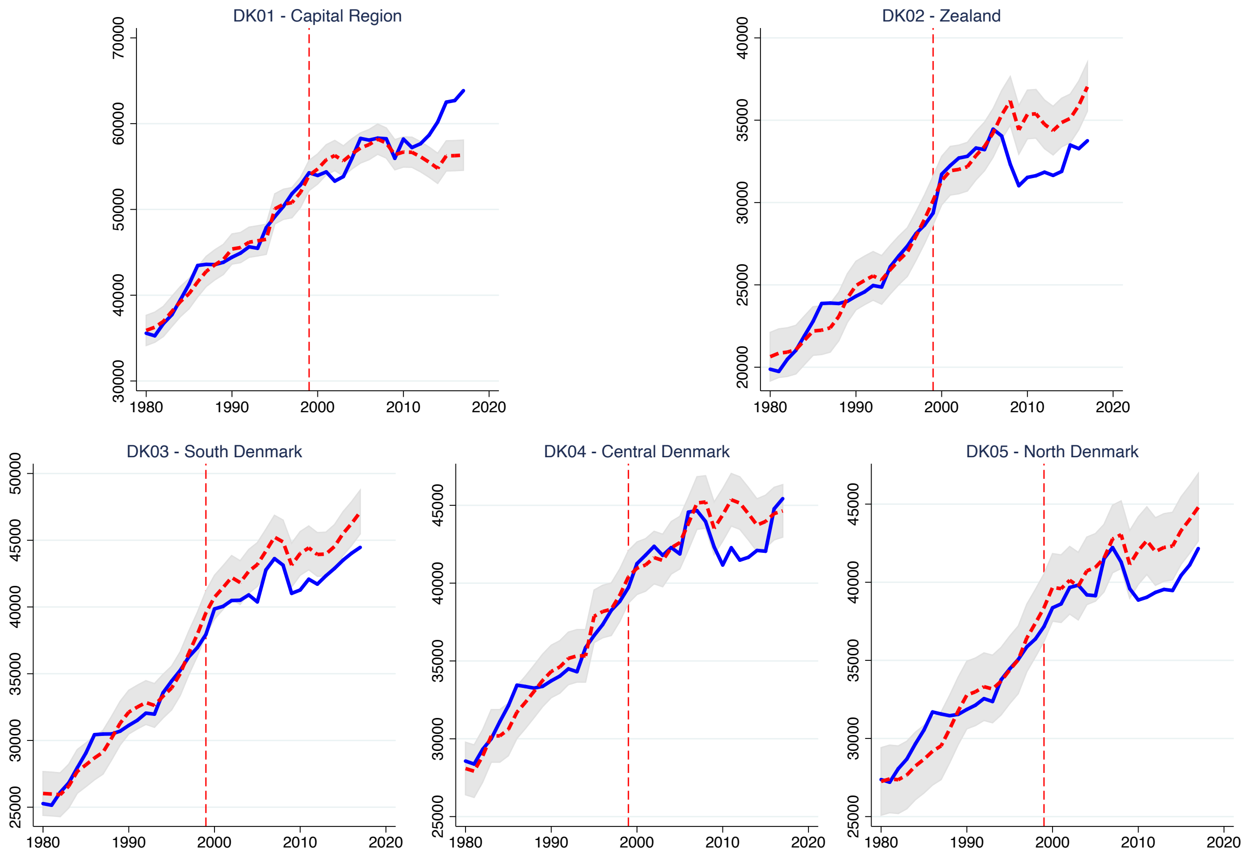
4.1. Denmark

The results of the synthetic control method applied to each Danish region are shown in Figure 4. The profile for treated region is shown in solid blue line while that of synthetic unit is in red dashed line. The vertical dashed line represents the intervention year. If the treated region is above (below) the synthetic region, the Danish region is better (worse) off in terms of GDP per capita as a result of Denmark staying out of the euro. The shaded area represents values that are within two standard deviations of the pre-treatment difference between the treated and synthetic unit. While this is not a formal test of statistical significance, it gives an indication as to whether the treatment effect is large in each post-treatment year relative to deviations observed in the pre-treatment period. In the Appendix, Table A2 shows the list of potential donors with positive weights in the synthetic control unit as well as the root mean square prediction error (RMSPE) which measures the gap between the variable of interest for the treated unit and its synthetic counterpart for all pre-treatment periods.[15]

Although the treatment effect of staying out of the euro appears small initially for most regions, larger divergences between the synthetic and treated unit emerge from the late 2000s, coinciding with the GFC and European sovereign debt crisis. Looking across regions, we find heterogeneous effects from euro adoption in the long run. On one hand, the Capital Region is the only region better off without the euro in the long run. In 2017, the Capital Region's per capita GDP gained about 13.3 percent (or around 7,500 euros) by staying out of the euro. In contrast, the other four regions would benefit under a euro adoption scenario, particularly from the late 2000s onwards. Zealand, the poorest region, experienced a loss of 10.6 percent (or around 3,800 euros) in per capita GDP from staying out of the euro in

[15]Table A5 in the Appendix summarizes the predictor variables for each Danish region and its synthetic counterpart.

Figure 4: Synthetic control vs treated Danish regions



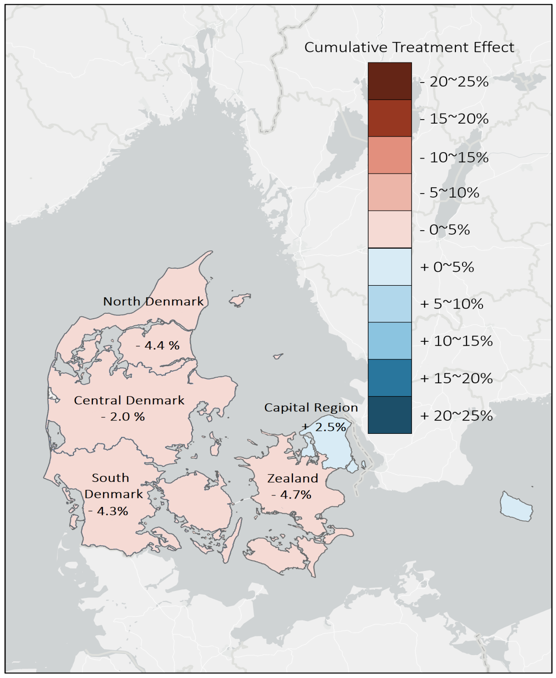
Note: Vertical axis measures real GDP per capita in constant 2015 euros. The profile for treated region is shown in blue line while that of synthetic unit is in red dashed line. Vertical dashed line represents the intervention year. The shaded area represents values that are within two standard deviations of the pre-treatment difference between the treated and synthetic unit.

2008, the year with the largest treatment effect. The middle income regions (South Denmark, Central Denmark and North Denmark) would also have been better off with the euro overall. Denmark's recovery from the GFC appears to be slow in most regions. However, Central Denmark appears to catch up with its synthetic counterpart by the end of the sample period.

As the treatment effects appear to accumulate over time, we calculate the cumulative treatment effect up until 2017 for each region, calculated as the total difference between the treated and synthetic unit's GDP per capita in the post-treatment period divided by the total income of the synthetic unit in the post-treatment period. As depicted in Figure 5, the cumulative treatment effect ranges from positive in the Capital Region (around 2.5 percent) to negative in all other regions ranging from 2.0 to 4.7 percent.

As all the non-capital regions in Denmark report gains—in particular, the most for the poorest Zealand region—under the euro adoption, we would expect the income disparity to narrow over time in Denmark. To formally quantify the income disparity across regions, we

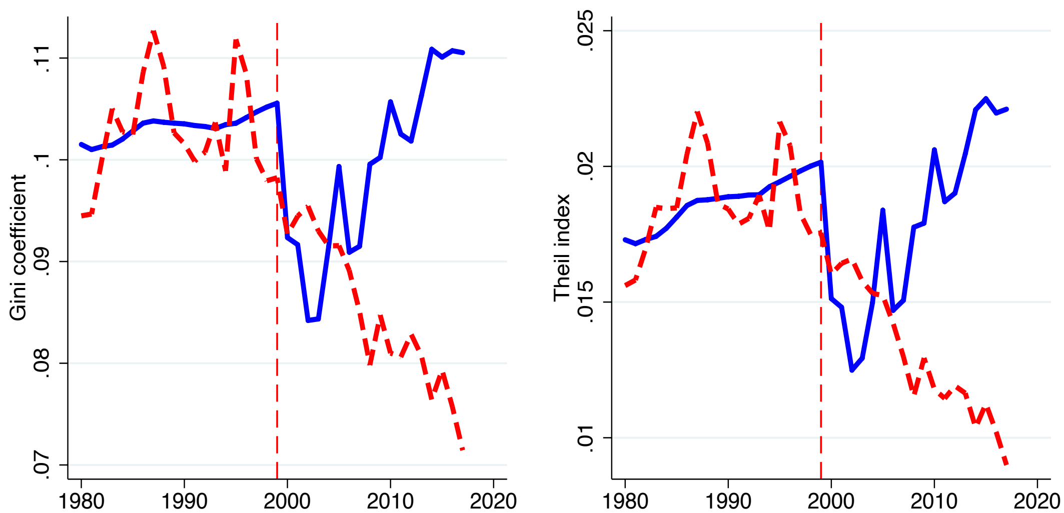
Figure 5: Cumulative effect of staying out of the euro on Danish regions



use both the Gini coefficient and Theil index to compare the disparity across the GDP per capita values actually observed with the counterfactual GDP per capita estimates obtained from the SC method. In calculating the Gini coefficient or Theil index, the regions are not given different weightings according to regional population as the focus of this research is on regional disparities, not inequalities across individuals. As shown in Figure 6, there is a fall in actual regional disparity in Denmark immediately after 1999, due to the Capital Region experiencing a downturn, whilst the other regions do not. This is likely attributed to the early 2000s recession which affected the Capital Region more so than any other region in Denmark due to its prominent financial centre. However, regional disparity rises with the recovery of the Capital Region, and exhibits an upward trend for the rest of the sample period. In the counterfactual scenario, regional income disparity declines gradually over time in the post-treatment period. This is due to slow recovery in the Capital Region after the GFC (and potentially the sovereign debt crisis), while the lower-income regions start to converge. Thus, regional income disparity in Denmark is higher as a result of staying out of the euro. This finding is not unique to the Gini coefficient or Theil index but is also consistent across other measures of disparity.[16]

[16]While there are various choices of measures that can be used to measure disparity or inequality, each comes with its own benefits, drawbacks and sensitivities. Since there are only 5 regions in Denmark, there is no optimal choice of disparity measure. Other disparity measures such as the coefficient of variation, Atkin-

Figure 6: Regional income disparity in Denmark



Note: The disparity index for actual units is shown in solid blue line while that of synthetic units is in red dashed line. Vertical dashed line represents the intervention year.

This result is further confirmed by taking the average values of the Gini coefficient and Theil index in the actual and counterfactual scenarios across the pre-treatment and post-treatment periods, shown in Table 1. In the pre-treatment period, the average values of both Gini coefficient and Theil index are quite close across the actual and counterfactual scenarios while the post-treatment period reveals divergence in the degree of income disparity. A simple back-of-the-envelope calculation of the difference in differences shown in the last column of the table suggests an increment of 0.0138 units as measured by the Gini coefficient (or 16.1 percent as a fraction of post-treatment counterfactual disparity) on average as a result of Denmark's decision to stay out of the euro. Similarly for the Theil index, an increase of 0.0048 units translates into a 36 percent jump in disparity as a result of staying out of the euro. Keeping in mind that the two measures take values over a different range, percentage changes are not directly comparable. Qualitatively, however, both measures imply that the overall effect of staying out of the euro was to increase regional income disparity in Denmark.

Table 1: Average regional income disparity in Denmark

	Pre-treatment (1980-1998)			Post-treatment (1999-2017)			
	Actual	Counterfactual	(1)	Actual	Counterfactual	(2)	(2)–(1)
Gini coefficient	0.1031	0.1029	0.0002	0.0995	0.0855	0.0140	0.0138
Theil index	0.0186	0.0187	-0.0001	0.0180	0.0133	0.0047	0.0048

Note: Columns labelled (1) and (2) measure the difference between actual and counterfactual disparity in each sub-period. Column labelled "(2)–(1)" measures the difference in differences.

4.2. Sweden

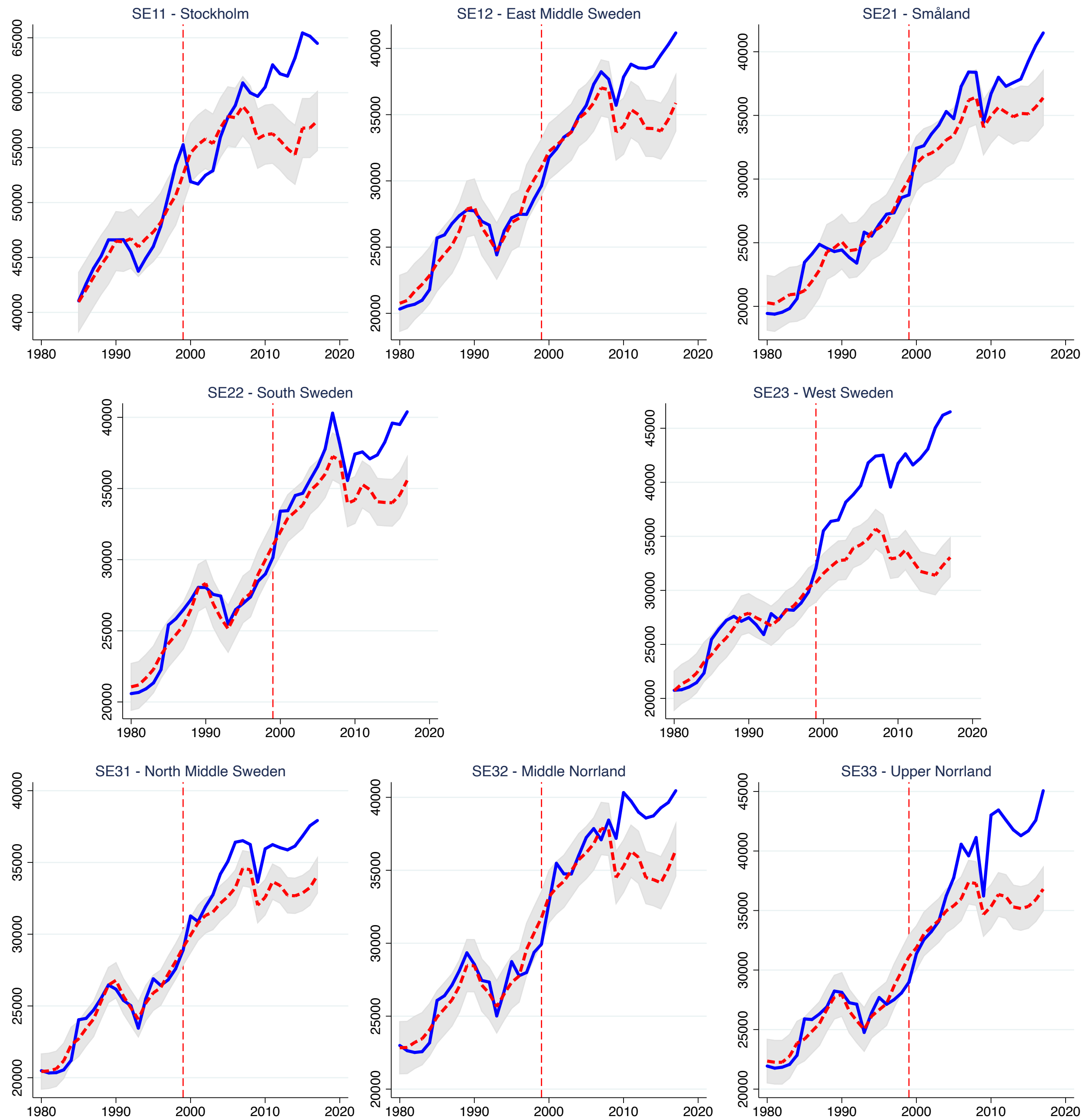
For Sweden, Figure 7 displays the results of the synthetic control method applied to each region.[17] Note that the analysis for Stockholm begins in 1985 due to a large drop just prior to this year which otherwise would make constructing a comparable counterfactual very difficult. All Swedish regions are consistently better off without the euro by the end of the sample period. More importantly, the treatment effect seems to accumulate over time. While the direction of the effect is uniform, the magnitudes are varied. Most regions begin to have a large treatment effect after recovering from the economic downturn in the late 2000s caused by the GFC. On the other hand, the synthetic regions do not appear to recover as quickly, potentially due to the sovereign debt crisis and the absence of independent monetary policy as a stabilising instrument during this period of crisis. At the end of our sample period, West Sweden and Upper Norrland gained the most from staying out of the euro. In 2017, they are better off by around 13,500 and 8,300 euros, or 40.7 and 22.5 percent gain in real GDP per capita, respectively, as a result of opting out of euro. These two regions also have the highest per capita income after Stockholm. On the other hand, Stockholm is initially slightly worse off due to an economic downturn resulting from the bursting of the dot com bubble but recovers and grows faster than its synthetic counterpart in the long run. By 2017, Stockholm is better off without the euro by around 7,100 euros or 12.3 percent of its income. All other remaining regions report an income gain of around 11 to 15 percent in 2017.

The cumulative treatment effect of staying out of the euro is illustrated in Figure 8. Consistent with the distribution of gains at the end of the sample period, West Sweden once again records the largest combined gains from opting out of the euro, followed by Upper Norrland. As both regions are highly exposed to trade, one might expect them to gain significantly from increased trade due to adoption of the common currency. However, their

son's indices and generalised entropy indices all depict a very similar story and reveal the same conclusion.

[17] Table A6 in the Appendix summarizes the predictor variables for each Swedish region and its synthetic counterpart.

Figure 7: Synthetic control vs treated Swedish regions

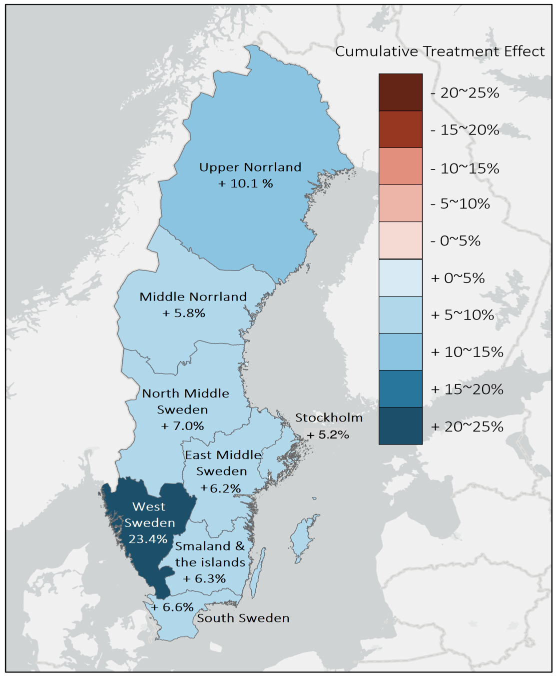


Note: Vertical axis measures real GDP per capita in constant 2015 euros. The profile for treated region is shown in blue line while that of synthetic unit is in red dashed line. Vertical dashed line represents the intervention year. The shaded area represents values that are within two standard deviations of the pre-treatment difference between the treated and synthetic unit.

reliance on exports would also render them more vulnerable to international business cycle fluctuations, which is discussed later in Section 6. The cumulative gain for all other regions range from 5 to 7 percent, with Stockholm showing the smallest gains.

Next, we discuss the distributional effect of the euro. Whilst all regions experience long

Figure 8: Cumulative effect of staying out of the euro on Swedish regions

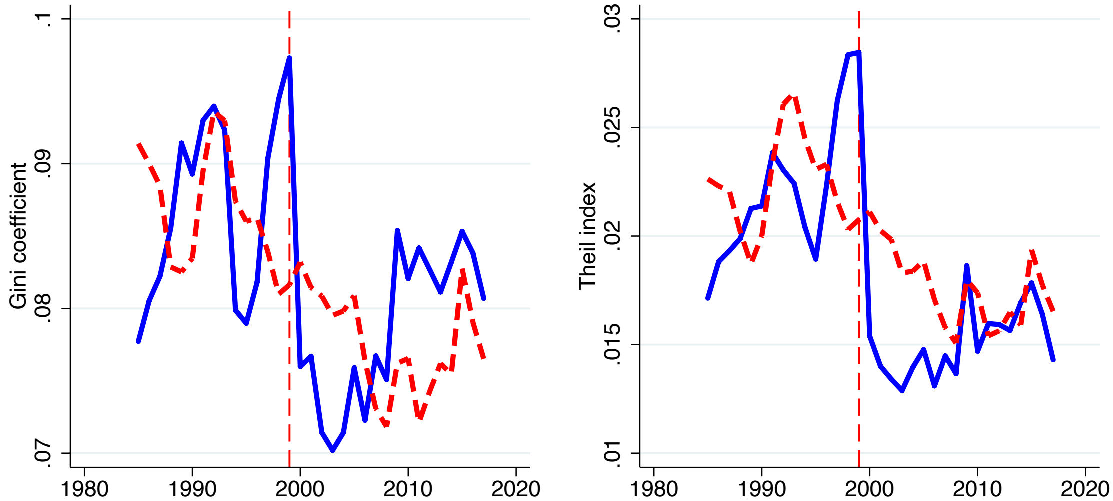


run gains from staying out of the euro, the regions which gain the most are the upper-income regions with the exception of Stockholm. In that respect, one might expect the upper-middle-income regions catch up and converging with the richest Stockholm while on the other hand, their gap with lower-income areas widens more leading to a greater divergence in income. Once again, we quantify the regional income disparity in Sweden by using the Gini coefficient and Theil index.

Since the counterfactual for Stockholm was only constructed from 1985 onwards, we compare the regional disparity post 1985 as shown in Figure 9. The dot com bubble and bust in the early 2000s hit Stockholm harder than any other region, causing regional disparity to fall. This is expected as the capital region has a larger financial centre and high tech industry presence. Upon recovery in mid 2000s, regional income disparity in Sweden rises as Stockholm grows faster than other regions in the long run. The income disparity among the synthetic control units is generally lower post 1999.

Finally, Table 2 presents the average values of the Gini coefficient and Theil index in the pre-treatment and post-treatment periods under both scenarios. The average pre-treatment values of both the Gini coefficient and Theil index are almost identical in both settings, while there is a small divergence in the post-treatment period, with the disparity in the actual scenario being on average higher than the disparity in the counterfactual scenario. A simple back-of-the-envelope calculation of the difference in differences shown in the last

Figure 9: Regional income disparity in Sweden



Note: The disparity index for actual units is shown in solid blue line while that of synthetic units is in red dashed line. Vertical dashed line represents the intervention year.

column of the table suggests an increment of 0.002 units as measured by the Gini coefficient (or 3.0 percent as a fraction of post-treatment counterfactual disparity) on average as a result of Sweden's non-adoption of the euro. The Theil index, on the other hand, shows an opposite direction decrease of 0.001 units (or 7 percent). Once again, the percentage changes in these indices are not comparable across these measures due to different units of measurement used. However, the direction of the effect is the main interest. The effect of staying out of the euro on regional disparity is evidently sensitive to the choice of disparity measure used. This is due to the fact that in the actual scenario, there is greater divergence among the non-capital regions but greater convergence with Stockholm at the same time. Since Stockholm is the biggest outlier in the income distribution, different disparity measures which have varying degrees of sensitivity towards income changes at the higher end of the distribution may produce different results.[18]

5. Inference and Robustness

A drawback of the synthetic control method is the inability to conduct traditional hypothesis testing. As such, the synthetic control literature has relied primarily on falsification

[18]Due to Stockholm appearing to be an outlier, we consider regional income disparities of all regions excluding Stockholm, using results for the entire sample period from 1980 onward. This is shown in Figure A1 and Table A4 in the Appendix.

Table 2: Average regional income disparity in Sweden

	Pre-treatment (1985-1998)			Post-treatment (1999-2017)			
	Actual	Counterfactual	(1)	Actual	Counterfactual	(2)	(2)–(1)
Gini coefficient	0.0865	0.0871	-0.0006	0.0796	0.0778	0.0018	0.0023
Theil index	0.0217	0.0225	-0.0008	0.0158	0.0178	-0.0020	-0.0012

Note: Columns labelled (1) and (2) measure the difference between actual and counterfactual disparity for each sub-period. Column labelled "(2)–(1)" measures the difference in differences.

tests to assess statistical significance of the treatment effect. In this section, we perform two types of placebo tests. In Section 5.1, we perform placebo 'in space' tests which consist of falsification tests on control regions which did not receive the treatment of staying out of the euro, and comparing the actual treated regions' treatment effects to the placebo distribution of treatment effects. In Section 5.2, we backdate the treatment year to 1995 to assess whether the effect observed in the actual intervention year (1999) is large relative to years where there was no treatment. Next, in Section 5.3, we conduct an event study, applying the weights obtained from the synthetic control results in the previous chapter to construct confidence intervals for each post-treatment period.

We check whether the benchmark results in Section 4 are sensitive to various robustness tests. In Section 5.4, we check the sensitivity of the results to the inclusion or exclusion of a particular region in the control group. This is to ensure that the results are not being driven by the idiosyncratic behaviour of any particular control region. Finally, in Section 5.5, we exclude regions from neighbouring countries in the control group to check against the possibility that regions from neighbouring countries might have experienced spillover effects as discussed in Section 3.2.[19]

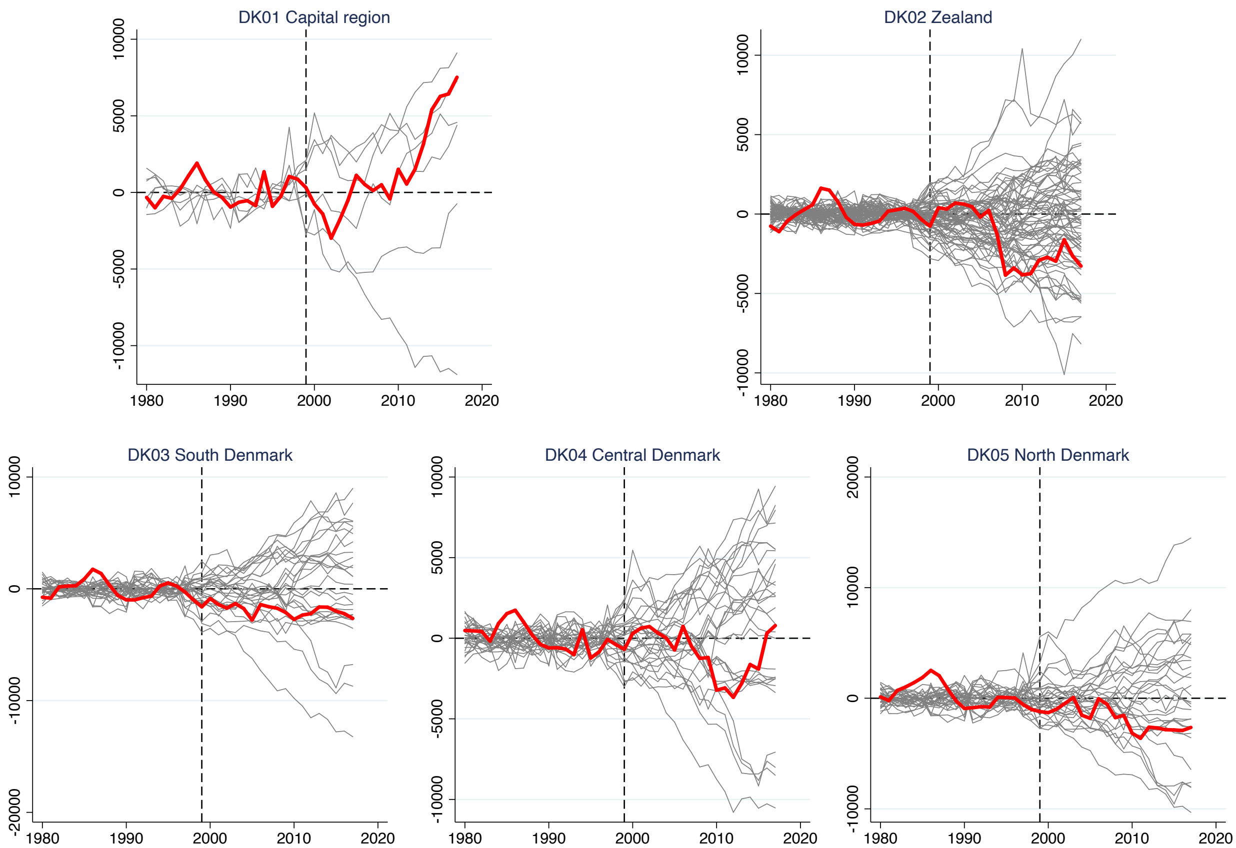
5.1. Placebo 'in space' tests

To assess whether the magnitude of the treatment effect is statistically significant, Abadie, Diamond, and Hainmueller (2010) suggest performing falsification tests on the control units. If the magnitude of the treatment effect for the treated region is extreme compared to the placebo distribution, then the effect is taken to be significant. We repeat the synthetic control method on each region in the control group for Danish and Swedish regions and plot their distributions in Figures 10 and 11 against the treated regions. The red line represents the treatment effect: the difference in GDP per capita between the treated unit and the

[19]Additional robustness checks are also reported in the Online Appendix. These include: alternative specifications in log and de-meaned transformation; constrained regression using lagged outcomes only as per Doudchenko and Imbens (2016); aggregate and further disaggregated analyses.

synthetic control. This should be close to zero prior to 1999 and diverge afterwards. The grey lines represent the difference in GDP per capita between the placebo treated unit and the placebo synthetic control. As in Abadie, Diamond, and Hainmueller (2010), we exclude from these graphs placebo regions with a poor pre-treatment fit—more than twice the root mean squared prediction error (RMSPE) of the treatment region—as these placebo tests are not informative with regards to the relative rarity of observing a post-intervention gap of its size.

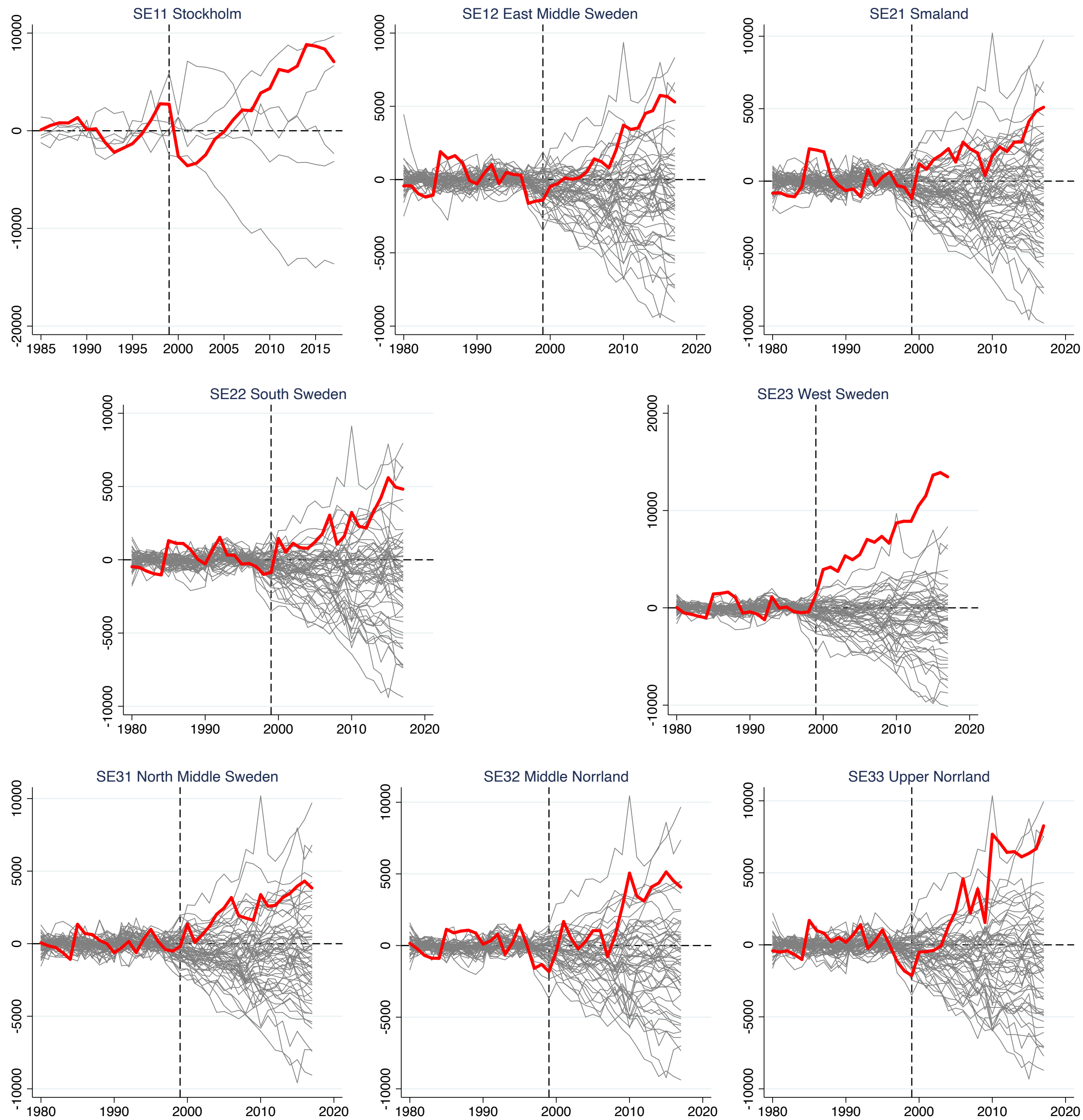
Figure 10: Placebo 'in space' tests, Danish regions



Note: Vertical axis measures the treatment effect in constant 2015 euros. The profile for treated region is shown in red line while that of the each of the donors is in grey line. Vertical dashed line represents the intervention year.

When comparing the treatment effect (difference between the treated and synthetic GDP per capita values) in Danish regions to the placebo distribution, they fit well within the placebo distribution, and thus cannot be interpreted as statistically significant. However, the treatment effect does appear to be accumulating in the Capital Region, with the treatment effect at the upper end of the distribution in 2017. For the Swedish regions, West Sweden is clearly an outlier with respect to the placebo regions. Some other regions appear to be on

Figure 11: Placebo ‘in space’ tests, Swedish regions



Note: Vertical axis measures the treatment effect in constant 2015 euros. The profile for treated region is shown in red line while that of the each of the donors is in grey line. Vertical dashed line represents the intervention year.

the positive edge of the distribution such as Stockholm, Upper Norrland, South Sweden and Middle Norrland, particularly towards the end of the sample period.

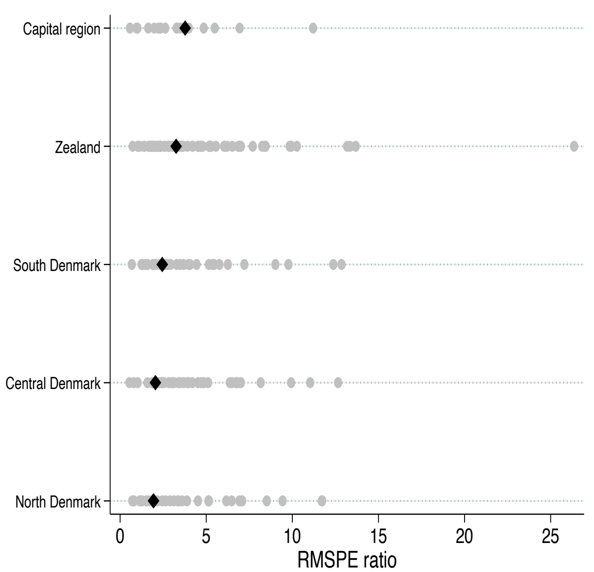
Another approach to determine significance suggested by Abadie, Diamond, and Hainmueller (2010) is to construct a p-value using a test statistic of the ratio of the post-treatment to pre-treatment RMSPE. The pre-treatment RMSPE is calculated using the squared difference between the treated and synthetic unit in each year before 1999, then finding the square root of the mean. The post-treatment RMSPE is calculated similarly over the years 1999-2017. The RMSPE ratio test statistic for a region j is given by:

$$\chi_j \equiv \frac{RMSPE^j_{post}}{RMSPE^j_{pre}} = \frac{\sqrt{\frac{1}{T-T_0}\sum_{t=T_0+1}^{T}(Y^j_{1,t} - Y^j_{0,t}W^j)^2}}{\sqrt{\frac{1}{T_0}\sum_{t=1}^{T_0}(Y^j_{1,t} - Y^j_{0,t}W^j)^2}} \tag{1}$$

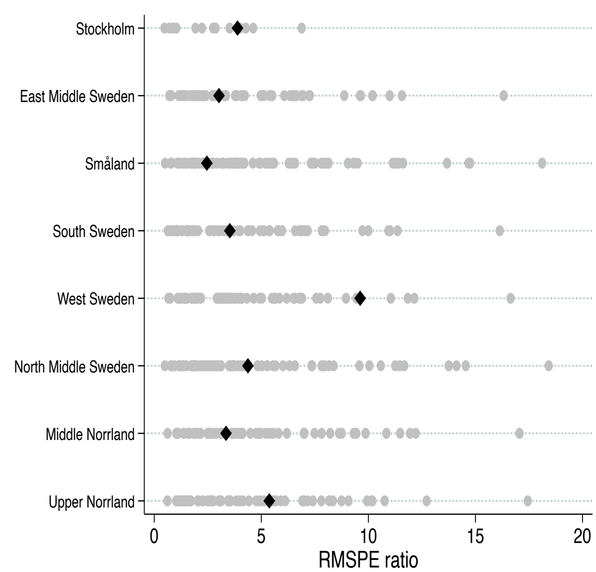
where T_0 is the year of policy intervention and $Y^j_{1,t}$ and $Y^j_{0,t}W^j$ represent the outcomes for treated and synthetic control unit, respectively.

Figure 12: Placebo distribution of RMSPE ratios

(a) Danish regions



(b) Swedish regions



Note: RMSPE ratio of the treated region is shown in black diamond dot while those of the placebo units are shown in grey round dots.

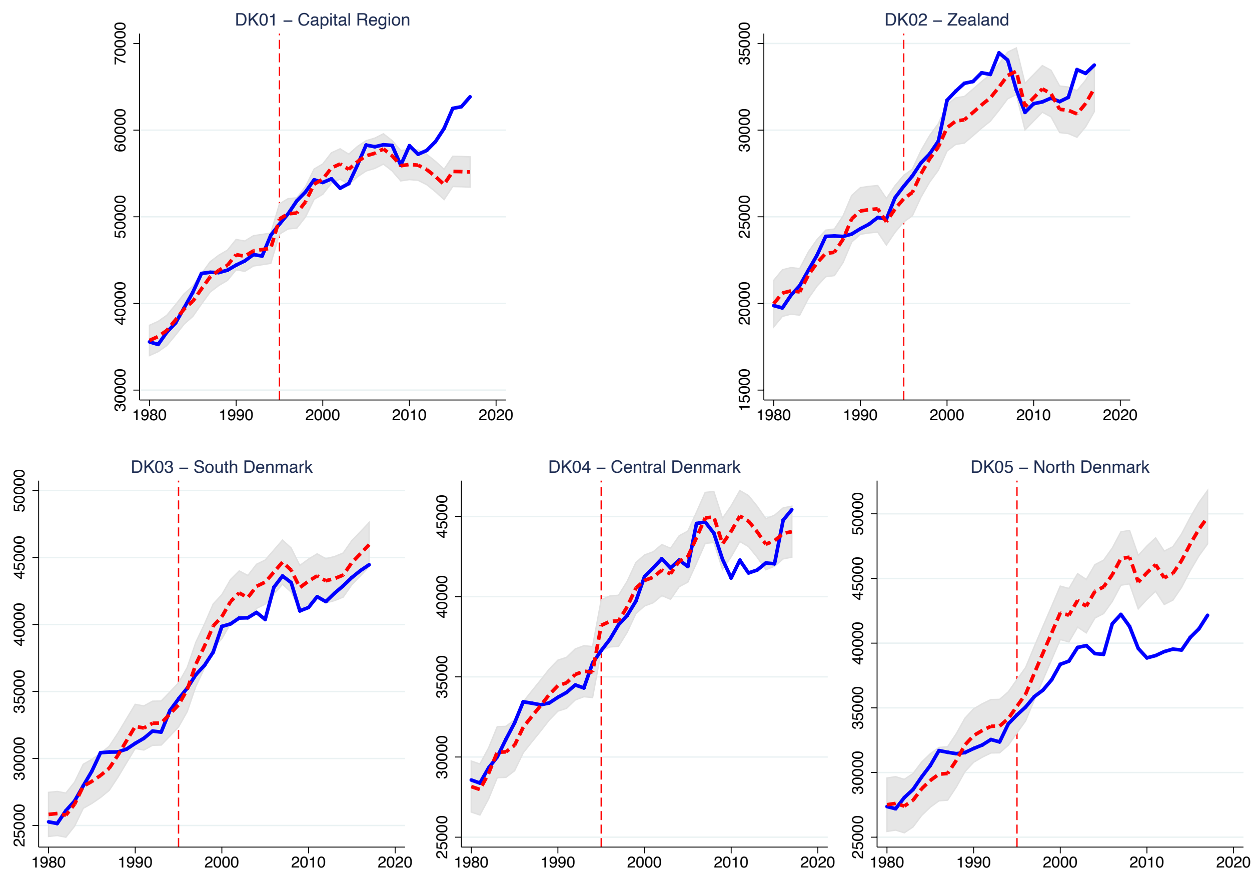
If the treated region has a large RMSPE ratio compared to the placebo units, then the treatment effect is taken to be significant. By using a ratio, it means that a large post-treatment effect is not necessarily indicative of a significant treatment effect if the pre-treatment RMSPE is also large. Following this approach, we calculate the RMSPE ratios for all five Danish and eight Swedish regions in Figure 12 along with the placebo regions' RMSPE ratios. We then use the rank of the treated region's RMSPE ratio against the placebo RMSPE ratios to derive a p-value. The p-value represents the probability of obtaining an estimate at least as large as the one observed for the treated unit when the intervention is assigned at random in the data set. Except for the case of West Sweden where the p-value of 0.08 falls within the conventional range of statistical significance used in the synthetic controls literature, no other region is statistically significant overall based on this test statistic. One drawback of the p-value using the RMSPE ratio statistics is that it does not test significance at each point in time, but is an overall test of significance over the entire post-treatment period.

5.2. Placebo 'in time' tests

Another common falsification test is to reassign the treatment to an earlier year. If the treatment effect in the placebo intervention year is comparable in size to the treatment effect in the actual intervention year, then the treatment effect observed cannot be taken to be significant. Due to regional data only being available from 1980 onwards, we reassign the intervention year to 1995 to ensure the pre-treatment period is still sufficiently long to capture unobserved heterogeneity in the synthetic matching process. The period of interest is 1995-1998, where there should be minimal treatment effect in comparison to the post-1999 period observed in Section 4. As shown in Figure 13, this mostly holds despite some divergence between the treated and synthetic unit in Central and North Denmark. On the other hand, for Sweden, it is difficult to choose an appropriate placebo intervention year due to financial crisis in the early 1990s followed by the EU accession in 1995. In fact, by 1995, Sweden had only just recovered from a recession and GDP per capita may not have returned to a steady trend. As such, choosing an intervention year prior to 1991 or 1995 would attribute a large treatment effect to this period of time which would inflate the placebo estimates artificially. Therefore, some caution must be given in relation to the placebo-in-time estimates for Sweden when we visually inspect the outcome in Figure 14.

Next, similar to Puzzello and Gomis-Porqueras (2018), we compare the post-treatment to pre-treatment RMSPE ratio between the placebo intervention year (1995) and the actual intervention year (1999) in Table 3. For all Danish regions and most Swedish counterparts, the placebo RMSPE ratio is visibly smaller than that observed in the actual intervention year. This indicates that the synthetic control method is picking up the causal effect of staying out of the euro on the GDP per capita of these regions, though some of these effects may be small. In particular, the two capital regions have the largest differences between the placebo and actual intervention effect. However, the RMSPE ratio is limited for the purpose of inference as it only provides an indication of overall significance of the treatment effect and cannot test the significance of the treatment effect in each post-treatment year.

Figure 13: Placebo 'in time' tests, Danish regions



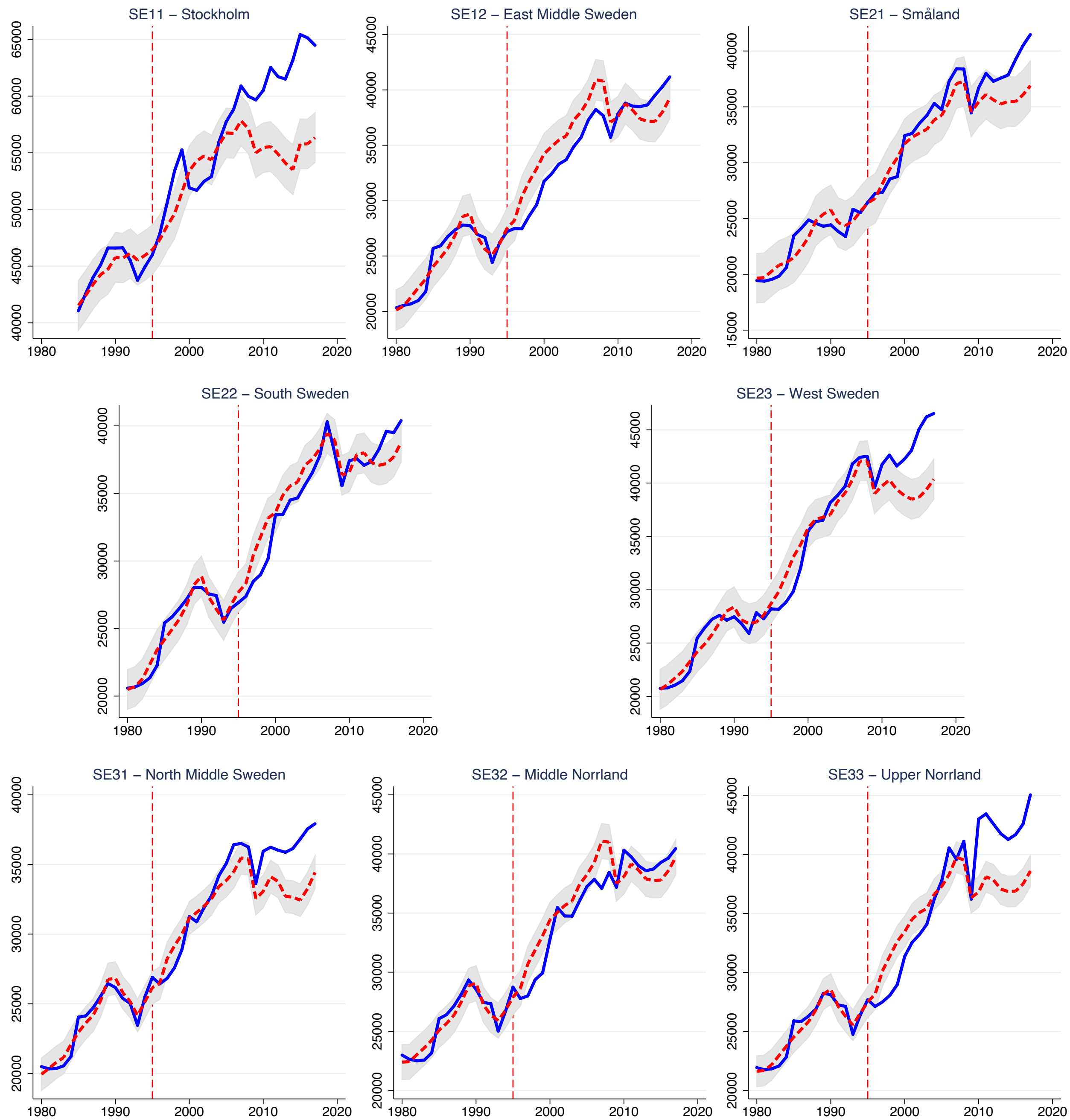
Note: Vertical axis measures the treatment effect in constant 2015 euros. The profile for treated region is shown in blue line while that of synthetic unit is in red dashed line. Vertical dashed line represents the backdated placebo intervention year.

Backdating the treatment year to 1995 can also test for the presence of anticipatory effects without biasing the treatment effect as discussed in Section 3.2. While it is reassuring that the RMSPE ratios in 1995-1998 are almost always smaller than those after 1999 and that the directions of the treatment effects afterwards are unchanged, this is not conclusive evidence that there were no anticipatory effects. Nonetheless, the presence of anticipatory effects prior to 1999 would lead to a lower bound estimate of the true treatment effect as noted in Campos, Coricelli, and Moretti (2019).

5.3. Event study and confidence intervals

The synthetic control literature has primarily relied on falsification tests to assess statistical significance. However, the RMSPE ratio and related p-value are unable to test the significance of the treatment effect in each post-treatment period. To do this, we extend on Peri and Yasenov (2015) who re-examine the Mariel Boatlift study by Card (1990) us-

Figure 14: Placebo 'in time' tests, Swedish regions



Note: Vertical axis measures the treatment effect in constant 2015 euros. The profile for treated region is shown in blue line while that of synthetic unit is in red dashed line. Vertical dashed line represents the backdated placebo intervention year.

ing the synthetic control method in combination with classical regression. We apply the synthetic control weights obtained from the benchmark results and conduct an event study to construct confidence intervals for each year. An event study is similar to a generalized

Table 3: Post to pre-treatment RMSPE ratios

Region	1995-1998	1999-2017
Capital Region	1.125	4.462
Zealand	1.059	2.155
South Denmark	1.067	1.962
Central Denmark	1.283	1.917
North Denmark	1.788	5.503
Stockholm	2.080	5.339
East Middle Sweden	2.457	2.384
Småland and the islands	0.601	2.021
South Sweden	2.542	1.884
West Sweden	2.472	3.450
North Middle Sweden	1.981	4.076
Middle Norrland	2.704	2.347
Upper Norrland	3.490	5.849

Note: Columns labelled "1995-1998" and "1999-2017" refer to the post-treatment period in the calculation of RMSPE ratios. For both cases, pre-treatment period covers all years prior to 1995.

difference-in-differences estimation with lead and lag terms.[20] Using the last pre-treatment period (T_0) as the reference or base year, the idea is that the coefficients in the pre-treatment period (or the leads) should not be statistically different from zero, and one can expect that future policy changes are unlikely to be associated with current outcomes. On the other hand, the coefficients after the intervention allow us to assess whether the treatment effect is immediate or lagged over time with different intensities, if anything significant. The event study specification is:

$$Y_{i,t} = \alpha + \gamma_j + \lambda_t + \sum_{s=1}^{T_0-1} \beta_s Z_{i,s} + \sum_{m=1}^{T-T_0} \beta_m Z_{i,T_0+m} + \epsilon_{j,t} \tag{2}$$

where $Y_{i,t}$ refers to the outcome of interest at time t in region i, α is a constant, γ_i and λ_t are region and year fixed effects, $\epsilon_{i,t}$ is the error term and $Z_{i,t}$ represents an interaction term between an indicator for the year and an indicator equalling one for the treated region and zero otherwise. The sample consists of the treated region, given a weight of one in the regression, and the regions in the control group weighted by their respective weights[21],

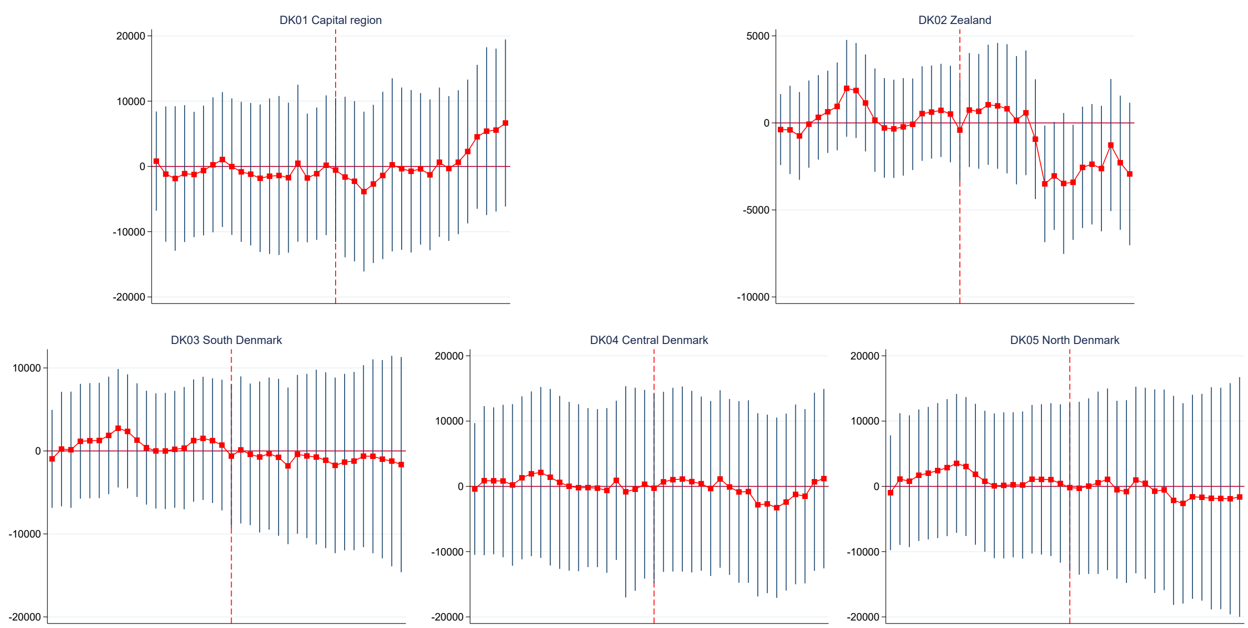
[20] A similar approach is taken in Cho (2020) that selects all donors with positive weights for the creation of synthetic control unit.

[21] The regions with positive weights are listed in Table A2 and A3.

summing to one.

For the $t < T_0$, β_t is expected to be statistically insignificant in support of the parallel trends assumption. Given that the SC method is designed to replicate the pre-treatment GDP per capita of the treated region, this should be satisfied by definition. Additionally, statistically significant interaction terms during the pre-treatment period could indicate the presence of anticipatory effects. Conversely, for $t > T_0$, β_t coefficients can be statistically significant. This allows us to capture the dynamics of the treatment effect: the treatment effect can be significant at particular years, take time to emerge, or have an instant or short-run impact and then dissipate. The event study methodology enables us to trace the statistical significance of the treatment effect in each post-treatment year, in contrast to the RMSPE ratio test statistic which only captures overall significance. Figures 15 and 16 depict the point estimates of treatment effect in each region for each period and the associated 90 percent confidence intervals. Given the small sample size, the standard errors are large. Point estimates and standard errors are provided in the Online Appendix.

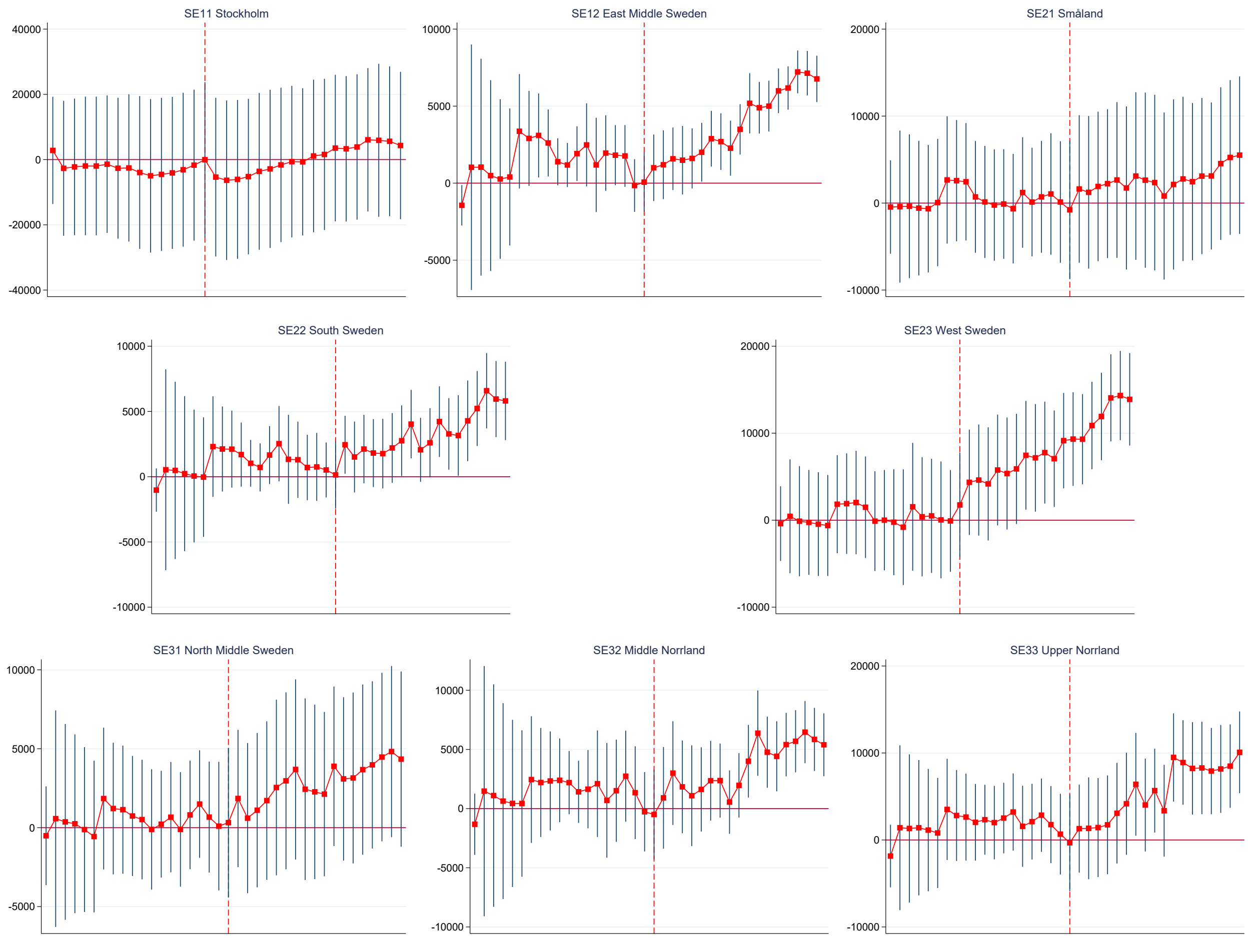
Figure 15: Event study, Danish regions



Note: Horizontal axis measures years since 1980 with the intervention year denoted with vertical dashed line. Each point represents coefficient estimates of lead and lag terms in equation (2) relative to the base year of 1998 with vertical line around it representing the 90 percent confidence interval.

For Denmark, the results of the event study confirm the previous inference tests. There are no significant effects prior to 1999, and hardly any afterwards with some isolated exceptions found in Zealand. Consistent with our earlier findings, the effect of staying out of the euro appears to be largely insignificant in Denmark, due to the fact that Denmark already

Figure 16: Event study, Swedish regions



Note: Horizontal axis measures years since 1980 (1985 for Stockholm) with the intervention year denoted with vertical dashed line. Each point represents coefficient estimates of lead and lag terms in equation (2) relative to the base year of 1998 with vertical line around it representing the 90 percent confidence interval.

had a fixed exchange rate with the euro, meaning that joining the euro would be unlikely to have a large economic effect.

For Sweden, interaction coefficients are virtually all statistically insignificant prior to the intervention year. This supports the parallel trends assumption and the absence of anticipatory effects. Post-intervention, our event study is able to capture significant positive effects for most regions, with exception of Stockholm, Småland and North Middle Sweden that remain statistically insignificant for all post-treatment periods. As seen previously, the treatment effect appears to be accumulating in all regions. East Middle Sweden and West Sweden start to become significantly better off without the euro from 2006 onwards and the other regions follow similar path from late 2000s.

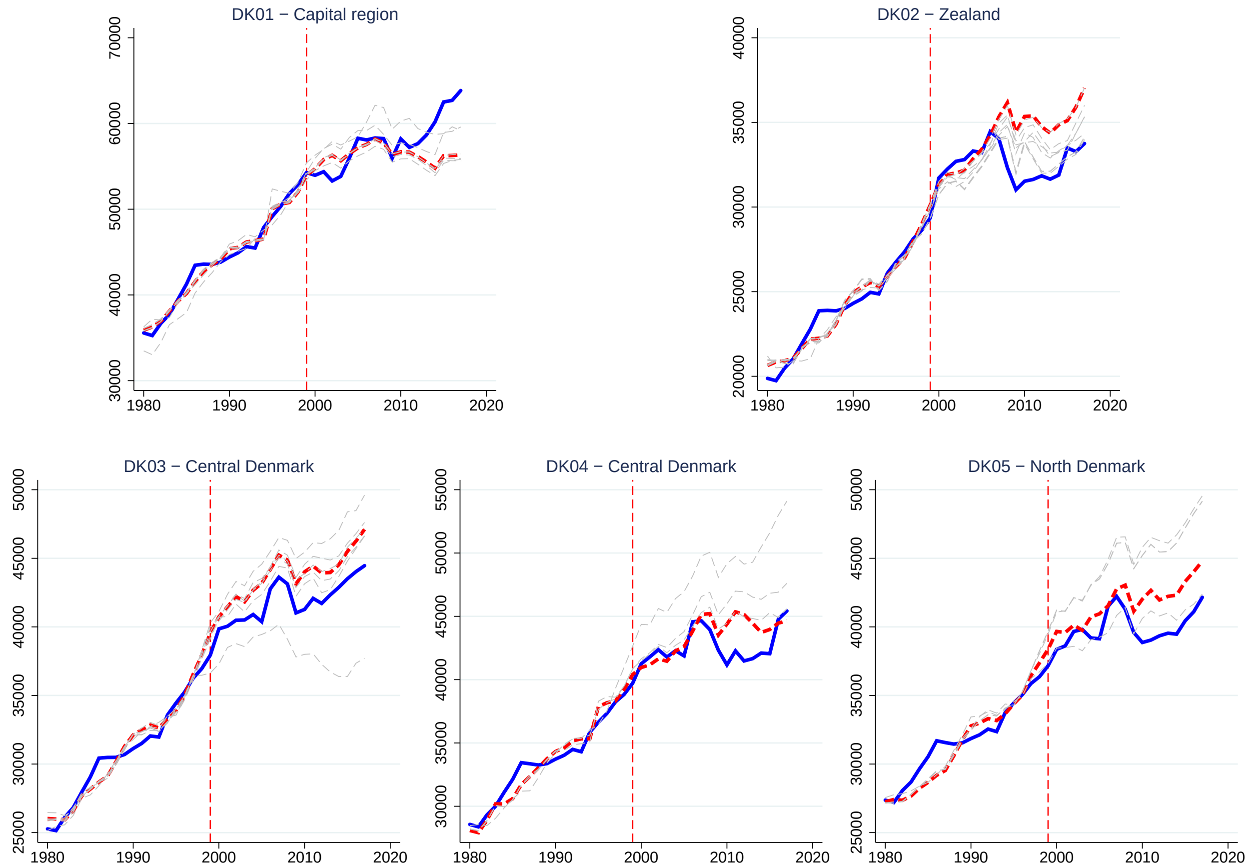
Using the event study methodology with the weights obtained from the synthetic control method allows for a dynamic test of significance. This is an advantageous feature over the RMSPE ratio test statistic which only captures overall significance. If a treatment effect dissipates over time, takes time to emerge or accumulate, the RMSPE ratio could fail to capture a significant effect. The RMSPE ratio is also sensitive to the number of post-treatment periods in the sample. If the post-treatment period includes time periods where the intervention effect has dissipated, then the RMSPE ratio may not capture a significant effect. However, if the post-treatment period is cut off earlier, then the same test statistic can capture a significant effect. A key feature of the synthetic control method is the fact that the treatment effect can vary over time. Thus, an inference test which can capture significance at each point in time is more suitable to complement this methodology.

5.4. Leave-one-out robustness tests

One way the design of the study may influence the result comes from the choice of regions in the donor pool with positive weights assigned. If dropping one region from this donor pool creates a large effect on the outcomes without a discernible change in pre-intervention fit, a re-examination might be necessary to assess whether the change in the magnitude of the estimate is caused by an idiosyncratic behaviour of the removed region. As such, we iteratively exclude each region with a positive weight in the construction of the benchmark synthetic control unit and rerun the synthetic control algorithm to check the sensitivity of the results. Figures 17 and 18 depict the treated unit in a solid blue line, the original synthetic counterfactual in a red dashed line while the new synthetic counterfactuals with one region left out are shown in grey dashed lines.

Reassuringly, the direction of the results remains largely unchanged for Denmark, although the magnitudes at times increase. All non-capital regions are worse off without the euro as in the baseline results. The original synthetic control is never extreme compared to the synthetic controls with one region dropped out. However, the pre-treatment fit worsens at times, leading to a larger treatment effect as observed in some regions. Nonetheless, if the results in Denmark were driven by the idiosyncratic behaviour of particular control regions for Central or North Denmark, this robustness test suggests that the results in Section 4 provide a lower bound estimate of the treatment effect. The largest change in Denmark occurs in the Capital Region. When either Vienna or Brussels is dropped, the synthetic control performs slightly better than the actual Capital Region up until the early 2010s, however the difference is not large. The reason for this is because the Capital Region is one of the highest income regions in the entire sample and is thus more sensitive to the exclusion of either of the two capital cities from its control group. Nonetheless, the Capital Region eventually outperforms the synthetic control and thus the conclusions regarding Danish re-

Figure 17: Danish synthetic controls with one region left out

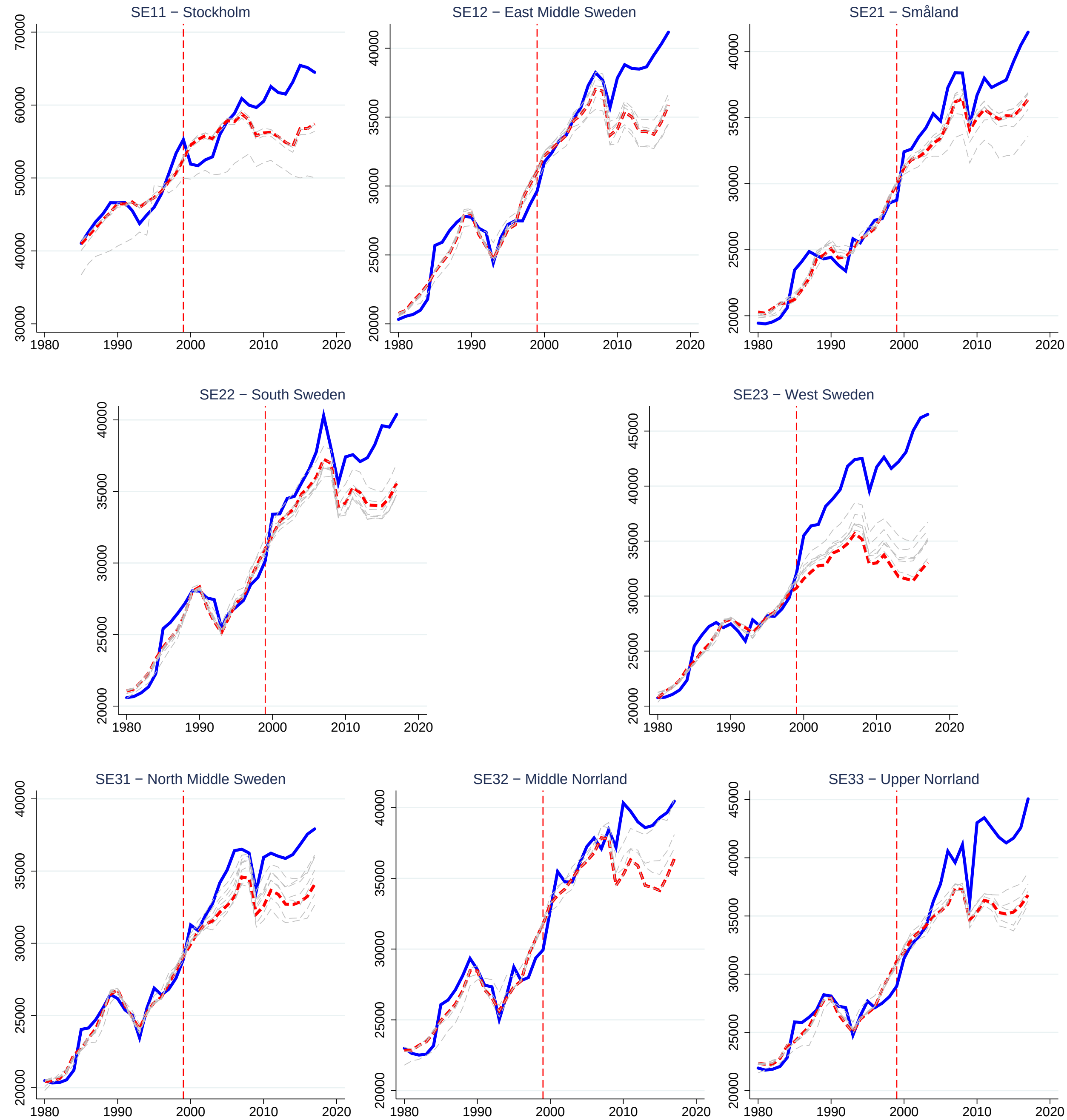


Note: The profile for treated region is shown in blue line while that of synthetic unit in the benchmark case is in red dashed line. New synthetic counterfactuals with one region left out are shown in grey dashed lines.

gional disparity in the long run remain unchanged. The other region that is sensitive to the exclusion of a particular region is Central Denmark when Noord-Holland is excluded from the donor pool. As Noord-Holland contributes the most in the creation of the benchmark synthetic control unit, without this region, the treatment effect switches from negative to positive.

The results for Swedish regions, shown in Figure 18, are also quite robust. The grey lines are mostly concentrated around the red dashed line, indicating that the results are not being driven by the idiosyncratic behaviour of any particular control unit. Importantly, the directions of all the treatment effects remain positive. Similar to the case of Copenhagen, our result for Stockholm is sensitive to the exclusion of Brussels, without which the treatment effect is still positive but the pre-treatment RMSPE more than triples. This highlights the importance of some regions that are crucial in the construction of synthetic control unit for capital cities.

Figure 18: Swedish synthetic controls with one region left out

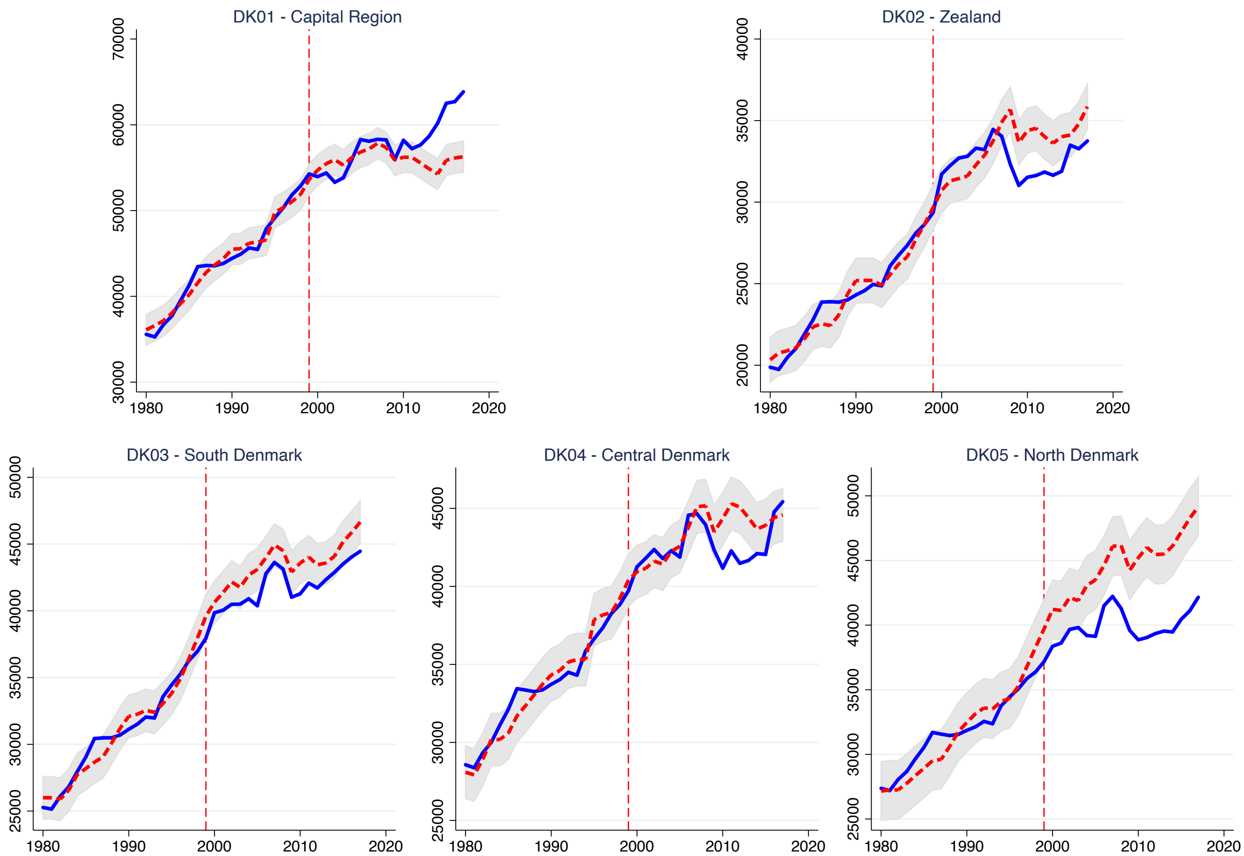


Note: The profile for treated region is shown in blue line while that of synthetic unit in the benchmark case is in red dashed line. New synthetic counterfactuals with one region left out are shown in grey dashed lines.

5.5. Leave-neighbours-out robustness tests

One of the key assumptions regarding our SC design is that the control group should not be affected by the treatment. As our treatment is at a regional level, one might expect some regions to be more linked to border-sharing regions of other countries. To address this concern for possibile spillover effect, we repeat the SC method on all treated regions without including any regions from neighbouring countries in the control group. While this test cannot definitively exclude the possibility of spillover effects, if any control regions were to be affected by Denmark or Sweden's decision to opt out of the euro, it would most likely be regions from neighbouring countries. As such, we exclude all regions from Germany for our analysis for Danish regions, and similarly exclude all Finnish regions for the case of Sweden. These results are shown in Figures 19 and 20.

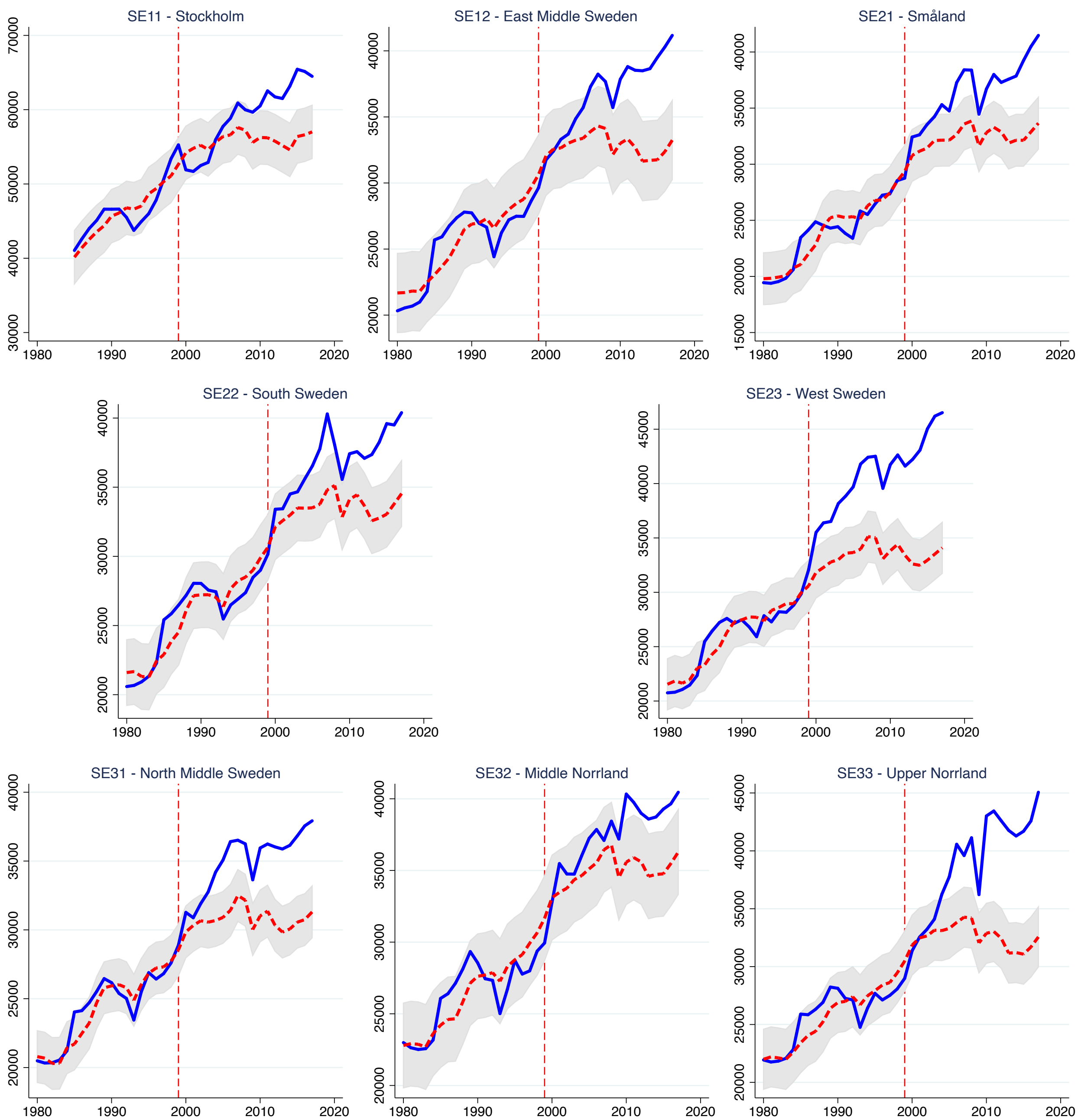
Figure 19: Danish synthetic controls without German regions



Note: The profile for treated region is shown in blue line while that of new synthetic unit in the benchmark case is in red dashed line.

The exclusion of regions from Finland in particular is an important robustness check, as Finnish regions often take on a large positive weight in the synthetic controls of Swedish regions. Finnish regions are often included as a controls for Swedish regions as both countries experienced a financial crisis in the early 1990s. Thus, there may be a concern that Sweden and Finland's separate financial crises affected them in structurally different ways which in turn affected the trajectory of GDP per capita in the synthetic control units for Sweden. Dropping all Finnish regions from the control groups of Swedish regions checks that the Swedish results are not being driven by the idiosyncratic behaviour of Finnish regions. Reassuringly, the results with the exclusion of Germany (for Denmark) and Finland (for Sweden) are almost identical to the baseline results, although the pre-treatment fit worsens slightly in some Swedish regions. Nonetheless, the overarching conclusions remain unchanged.

Figure 20: Swedish synthetic controls without Finnish regions



Note: The profile for treated region is shown in blue line while that of new synthetic unit in the benchmark case is in red dashed line.

The robustness of the benchmark outcome is also documented through the cumulative treatment effect as shown in Table 4. For Denmark, all non-capital regions are still worse off as a result of opting out of euro while the magnitude of positive treatment effect in the capital region remains relatively unchanged. For Sweden, all regions are once again better off in the long run with their decision to stay out of euro and the regional variations are quite comparable to the benchmark case with Stockholm and West Sweden recording the smallest and largest gain, respectively. While the qualitative outcomes remain unchanged, all Swedish regions experience higher treatment effects compared to the benchmark scenario and this may result in a different interpretation of the regional disparity profile. In Table 5, we report the difference-in-differences post-treatment average inequality indices for both countries. For Denmark, both inequality measures indicate that staying out of euro was associated with an increase in regional income disparity. On the other hand, in Sweden, the benchmark scenario reported mixed results with a rise in the Gini coefficient in the long run and a slight fall in the Theil index. However, under the robustness check, both measures report fall in their values hinting at an overall decrease in regional income disparity.

Table 4: Cumulative treatment effect under leave-neighbors-out

Region	Benchmark	Leave-neighbors-out
DK01	2.5%	3.2%
DK02	-4.7%	-2.7%
DK03	-4.3%	-3.8%
DK04	-2.0%	-2.0%
DK05	-4.4%	-10.7%
SE11	5.2%	6.0%
SE12	6.2%	11.5%
SE21	6.2%	12.4%
SE22	6.6%	9.9%
SE23	23.3%	22.2%
SE31	7.0%	13.3%
SE32	5.7%	6.8%
SE33	10.1%	18.7%

Note: Column labelled "Benchmark" enumerates the cumulative treatment effect depicted in Figure 5 and 8. Column labelled "Leave-neighbors-out" reports the same cumulative treatment effect for each regions when the neighboring nations are removed from the control group.

6. Discussion

The two countries provide an interesting contrast when examining the economic effect of the decision to opt out of the euro. Both countries had similar voting patterns on an aggregate level at their respective referenda, but differed in regional preferences. In terms of GDP per capita, we find that all but the Capital Region would have been slightly better

Table 5: Average regional income disparity under leave-neighbors-out

	Denmark		Sweden	
	Benchmark	No-Germany	Benchmark	No-Finland
Gini coefficient	0.0138	0.0151	0.0023	-0.0057
Theil index	0.0048	0.0049	-0.0012	-0.0041

Note: Columns labelled "Benchmark" enumerate reports the last column figures in Table 1 and 2. Columns labelled "No-Germany" and "No-Finland" reports the same difference-in-differences estimate under leave-neighbors-out test.

off with the euro. In contrast, all Swedish regions are better off without the euro, though to varying degrees.

The magnitude and significance of these results are likely to be driven by the differences in the exchange rate regimes of the two countries. Since the Danish krone was already fixed to the euro, the additional step of adopting the common currency was unlikely to have a substantial income effect. The region with the largest effect is Zealand, which loses close to 5 percent of its total income post 1999 as a result of staying out of the euro. These unrealised gains could be potentially due to the fact that sharing a common currency has an effect beyond simply eliminating exchange rate volatility (see Rose, 2000). In essence, Denmark has already paid the cost and sacrificed their monetary policy autonomy by fixing their exchange rate, but does not reap the full efficiency benefits of increased trade and reduced transaction costs. Nonetheless, studies such as Saia (2017) found evidence that the members of the euro currency union also trade more with non-members. Hence, it is not surprising that the treatment effects for Danish regions are small and statistically insignificant. In the Swedish referendum, the two choices at stake were more extreme. Joining the eurozone would mean the loss of independent monetary policy as well as the floating exchange rate.[22] Swedish voters had to weigh the benefits of increased efficiency from the common currency against the stability offered by monetary policy. Hence, the effect of opting out of the euro on Swedish regions is larger than in Danish regions as expected. Notably, all Swedish regions are better off without the euro, and the effect appears to be accumulating as time passes. In particular, the Swedish regions recover from the GFC relatively quickly in comparison to their synthetic counterparts. This could be attributed to the fact that Sweden retained their monetary policy autonomy during this period of crisis and also largely avoided the European sovereign debt crisis. In fact, the Swedish krona was able to depreciate against the euro during this time, thus insulating the national economy from the economic downturn to

[22] There are also differences in the fiscal regimes where Denmark voluntarily follows the Stability and Growth Pact and Fiscal Compact while Sweden is not bound by fiscal restrictions.

some extent. Whilst Denmark and Sweden initially experienced similar output contractions during the GFC, recovery was much faster in Sweden. This result corroborates other studies such as Gyoerk (2017) and Lin and Chen (2017) that find Sweden to be better off in the long run on aggregate in terms of labour productivity and real income. On the other hand, our finding contrasts against Reade and Volz (2009) who argue that Sweden had minimal monetary autonomy to lose and would benefit from joining the euro. One possible reason for this difference could be due to different sample period, as we find evidence that the positive benefits from staying out of the euro start to become noticeably visible post GFC.

Within Sweden, there is regional variation with respect to the magnitude of the treatment effects. West Sweden experiences the largest gains from staying out of the euro. West Sweden includes the second largest city in Sweden, Gothenburg, and features the largest port in Scandinavia handling almost 30 percent of Sweden's foreign trade. Given this region's heavy reliance on trade, it is somewhat surprising to see that it experiences such a large positive effect from staying out of the euro since the common currency is expected to boost trade and facilitate access to a larger market for Swedish exporters. However, Swedish exports became more diversified towards non-eurozone countries since 2000. Jonung and Vlachos (2007) also found that voters in export-oriented regions were significantly less likely to vote for the euro with higher weight placed on stabilising the economy against asymmetric shocks over the reduced transaction costs from the euro. With declining share of exports towards the eurozone, exporters appear to value the flexible exchange rate and independent monetary policy over potential efficiency gains from the euro.

An interesting observation is that the relatively higher-income regions appear to be the ones suffering most from the adoption of the euro, while the lower-income regions either slightly gain from euro adoption or experience smaller losses. This reveals that reducing regional disparity may not necessarily be desirable as a sole policy objective if reduced disparity is driven by stagnation in higher-income regions rather than growth in lower-income regions. In Denmark, the Capital Region would be worse off with the euro while the other regions would be better off. In Sweden, West Sweden and Upper Norrland would have suffered significant losses under the common currency while North Middle Sweden, the poorest region, would have suffered the smallest loss. This may seem counterintuitive, as high performing regions should be able to reap gains from specialisation from increased economic integration. However, specialisation also comes at the risk of increased vulnerability to asymmetric or idiosyncratic shocks. Thus, the direction of the treatment effect on regional income distribution remains theoretically ambiguous. These empirical results suggest that foregoing monetary policy autonomy and a flexible exchange rate may lead to large and substantial losses which outweigh potential gains from specialisation.

Our findings suggest that adopting a common currency can have heterogeneous effects on regions within a country not only in terms of the direction of the effect as evident in the case of Denmark, but also in terms of its magnitude as seen in Sweden. The decision to stay out of the euro increased regional disparity in Denmark and non-capital regions in Sweden. Although these results will not apply exactly to other prospective eurozone members, these findings reveal that inter alia, distributional effects should be taken into account when considering adopting a common currency.

7. Conclusion

In the two decades since the creation of the eurozone, the literature has primarily focused on the aggregate effects of the currency union while little attention has been paid on distributional effects. Using the synthetic control method, we conduct a counterfactual analysis on Denmark and Sweden who both chose to opt out of the euro via referendum. We investigate whether each region in Denmark and Sweden gained or lost from staying out of the euro in terms of GDP per capita and whether the income disparity between regions is higher or lower within each country as a result.

Our findings have important policy implications, revealing that currency union membership can have heterogeneous income effects across regions and affect regional income disparity. Given that committing to a currency union is an extremely long and costly process, and the fact that the Treaties of the European Union (EU) are silent on the possibility of leaving the eurozone, a counterfactual exercise to evaluate the potential implications of adopting the currency can be helpful to prospective euro countries who are currently in the EU. While it is not possible to generalise these results to other EU members, a similar analysis can be undertaken for regions within the countries of interest. Issues of regional disparity are important to policymakers and voters, particularly those in lower-income regions who may be concerned as to whether the euro would enable them to catch up to higher-income regions or leave them further behind. Abandoning a national currency can have implications for regional welfare and disparity not only through the loss of monetary policy, but also through restriction of fiscal policies. The loss of these stabilising policy tools is also a concern to regions which are specialised in certain sectors or vulnerable to asymmetric shocks. This is highlighted by the finding that relatively higher-income and export-oriented regions would have suffered larger losses from the adoption of the euro. While there are also many political concerns to weigh up in relation to monetary integration, careful consideration must nonetheless be given to the economic benefits and costs of currency union membership prior to commitment. This paper quantifies the net effect of the euro on income at a regional level and reveals that distributional concerns regarding currency unions should be given a more prominent focus in future policymaking and research.

Last but not the least, while we quantify the overall economic impact of staying out of the euro on regions in Denmark and Sweden in this paper, there are nonetheless many extraneous political considerations that are relevant to the decision of whether or not to adopt the euro. These political issues are beyond the scope of this paper, but should be considered in conjunction with the economic consequences of monetary integration.

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Appendix

Table A1: NUTS 2 regions of Denmark and Sweden

NUTS 2 code	English region name	Original region name
DK01	Capital Region	Hovedstaden
DK02	Zealand	Sjælland
DK03	South Denmark	Syddanmark
DK04	Central Denmark	Midtjylland
DK05	North Denmark	Nordjylland
SE11	Stockholm	Stockholm
SE12	East Middle Sweden	Östra Mellansverige
SE21	Småland and the islands	Småland med öarna
SE22	South Sweden	Sydsverige
SE23	West Sweden	Västsverige
SE31	North Middle Sweden	Norra Mellansverige
SE32	Middle Norrland	Mellersta Norrland
SE33	Upper Norrland	Övre Norrland

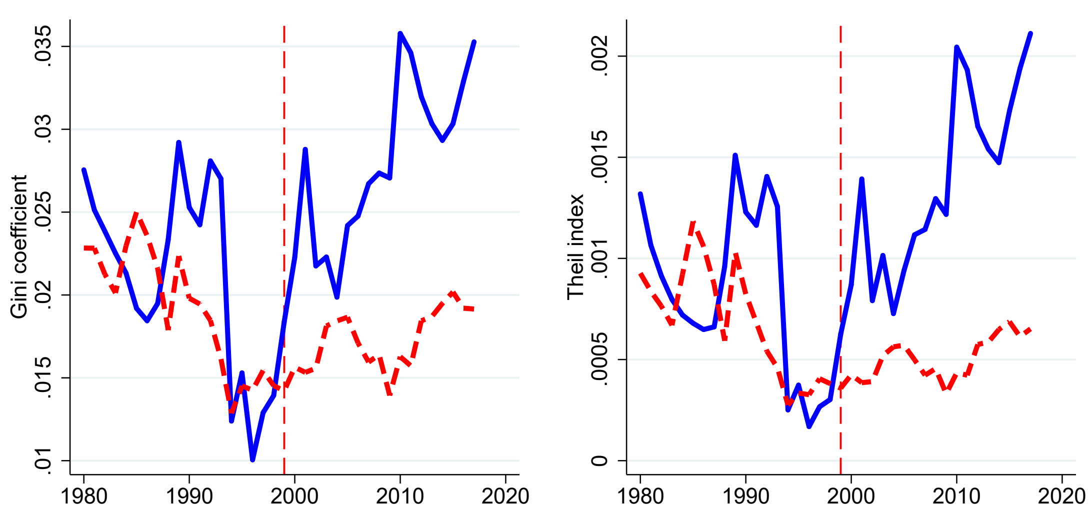
Table A2: Synthetic control weights for Danish regions

Region	Controls units with positive weight (in brackets)	RMSPE
DK01	Wien, AT (0.412), Bruxelles, BE (0.399), Hamburg, DE (0.037), Åland, FI (0.078), Île de France, FR (0.019), Noord-Holland, NL (0.054)	854.19
DK02	Vorarlberg, AT (0.068), Brabant Wallon, BE (0.162), Karlsruhe, DE (0.017), South Finland, FI (0.079), Valle d'Aosta, IT (0.147), Drenthe, NL (0.146), Overijssel, NL (0.355), Zuid-Holland, NL (0.025)	717.75
DK03	Salzburg, AT (0.134), Düsseldorf, DE (0.011), Åland, FI (0.101), Bolzano, IT (0.02), Friuli-Venezia Giulia, IT (0.251), Noord-Holland, NL (0.484)	795.65
DK04	Wien, AT (0.401), Salzburg, AT (0.41), Champagne-Ardenne, FR (0.189)	823.43
DK05	Île de France, FR (0.222), Champagne-Ardenne, FR (0.34), Trento, IT (0.093), Noord-Holland, NL (0.346)	1095.14

Table A3: Synthetic control weights for Swedish regions

Region	Controls units with positive weight (in brackets)	RMSPE
SE11	Bruxelles, BE (0.654), West Finland, FI (0.098), South Finland, FI (0.176), Île de France, FR (0.073)	1297.36
SE12	West Finland, FI (0.131), South Finland, FI (0.554), North & East Finland, FI (0.002), Alsace, FR (0.032), Rhône-Alpes, FR (0.121), Valle d'Aosta, IT (0.161)	1031.89
SE21	Steiermark, AT (0.112), Limburg, BE (0.458), West Finland, FI (0.141), Helsinki-Uusimaa, FI (0.165), Valle d'Aosta, IT (0.034), Zeeland, NL (0.089)	1046.02
SE22	West Finland, FI (0.142), South Finland, FI (0.472), North & East Finland, FI (0.058), Åland, FI (0.068), Valle d'Aosta, IT (0.025), Liguria, IT (0.216), Lombardia, IT (0.018)	799.47
SE23	Liège, BE (0.206), West Finland, FI (0.069), Helsinki-Uusimaa, FI (0.007), South Finland, FI (0.315), Lazio, IT (0.386), Overijssel, NL (0.018)	880.03
SE31	Vorarlberg, AT (0.019), West Finland, FI (0.127), South Finland, FI (0.349), Åland, FI (0.081), Abruzzo, IT (0.208), Friuli-Venezia Giulia, IT (0.002), Zeeland, NL (0.214)	596.63
SE32	West Finland, FI (0.185), South Finland, FI (0.458), Valle d'Aosta, IT (0.35), Zeeland, NL (0.007)	874.83
SE33	West Finland, FI (0.187), South Finland, FI (0.348), Åland, FI (0.12), Valle d'Aosta, IT (0.116), Zeeland, NL (0.228)	892.92

Figure A1: Regional income disparity in Sweden excluding Stockholm



Note: The disparity index for actual units is shown in solid blue line while that of synthetic units is in red dashed line. Vertical dashed line represents the intervention year.

Table A4: Average regional income disparity in Sweden excluding Stockholm

	Pre-treatment (1980-1998)			Post-treatment (1999-2017)			
	Actual	Counterfactual	(1)	Actual	Counterfactual	(2)	(2)–(1)
Gini coefficient	0.0210	0.0193	0.0018	0.0276	0.0172	0.0104	0.0086
Theil index	0.0008	0.0007	0.0001	0.0013	0.0005	0.0008	0.0007

Note: Columns labelled (1) and (2) measure the difference between actual and counterfactual disparity for each sub-period. Column labelled "(2)–(1)" measures the difference in differences.

Table A5: Summary statistics of predictor variables: treated and synthetic control regions in Denmark

	Average real GDP per capita			Labour demographics			$\frac{Inv}{GDP}$	Share of gross value added					
	(1980–86)	(1987–92)	(1993–98)	Population	Employment	Hours worked		Agriculture	Industry	Construction	Retail	Financial	Service
DK1	38496.9	44329.2	49589.0	1418.4	832.3	1241.8	17.2	0.1	12.3	3.7	30.1	30.2	23.6
Synth-DK1	38317.5	44598.6	49384.2	1413.3	788.2	1320.8	20.4	0.4	11.3	5.4	30.1	29.5	23.2
DK2	21385.7	24268.1	26970.4	830.8	317.9	483.6	17.2	1.4	20.3	8.2	20.3	20.1	29.7
Synth-DK2	21364.6	24240.8	26940.4	733.7	317.5	485.6	22.3	1.8	20.3	8.2	21.1	20.1	28.3
DK3	27255.8	31058.9	34741.7	1189.7	576.5	840.4	17.0	1.4	27.4	7.2	22.9	17.6	23.4
Synth-DK3	27009.9	31342.5	34893.5	1602.9	676.4	1098.9	18.4	1.5	17.6	6.7	30.3	22.1	21.9
DK4	30419.6	33705.2	36869.8	1145.3	584.7	858.0	16.1	1.6	26.7	7.3	23.4	17.1	23.8
Synth-DK4	29655.5	33876.9	37376.3	1056.7	949.4	523.8	23.0	1.5	17.0	8.6	28.0	22.1	22.8
DK5	29029.8	31851.2	34644.4	581.5	277.1	403.8	15.1	3.1	25.0	8.0	21.2	18.1	24.6
Synth-DK5	27968.0	31831.5	35011.9	3676.4	2618.4	1723.8	19.7	2.5	15.9	7.2	25.0	25.7	23.8

Note: The table summarizes predictor variables for each region in Denmark and its synthetic counterpart. Real GDP per capita is in constant 2015 euros. Population and employment is in thousands of persons. Hours worked is in millions. Investment to GDP measures gross fixed capital formation divided by total GDP for each region in percentage. Sectoral gross value added divided by total gross value added for the region for agriculture, forestry, fishing; industry; construction; wholesale, retail, transport, accommodation and food services, information and communications; financial and business services; non-market services.

Table A6: Summary statistics of predictor variables: treated and synthetic control regions in Sweden

	Average real GDP per capita			Labour demographics			$\frac{Inv}{GDP}$	Share of gross value added					
	(1980–86)	(1987–92)	(1993–98)	Population	Employment	Hours worked		Agriculture	Industry	Construction	Retail	Financial	Service
SE11		45294.4	47745.9	1634.9	984.4	1522.8	20.6	0.3	11.3	7.5	24.2	33.1	23.6
Synth-SE11		44923.7	48054.1	1734.0	958.0	1490.9	18.8	1.0	12.1	4.7	26.0	32.3	24.1
SE12	22278.6	27215.0	26905.4	1456.5	695.6	1086.9	24.7	2.2	22.6	10.3	16.5	21.6	26.7
Synth-SE12	22384.7	26570.1	27279.7	1503.9	655.2	1112.8	23.8	2.5	20.2	9.8	24.5	19.5	23.6
SE21	20913.6	24233.9	26820.9	794.8	379.5	603.0	27.7	3.9	21.9	8.9	20.1	19.4	25.9
Synth-SE21	20874.0	24276.0	26709.4	894.6	378.1	639.9	25.6	2.2	21.8	8.4	22.8	20.1	24.6
SE22	22437.1	27460.3	27289.1	1213.4	585.8	936.9	22.4	2.9	16.8	8.3	22.8	21.8	27.4
Synth-SE22	22626.1	26835.3	27519.6	1318.9	568.7	1019.6	23.0	2.9	19.3	8.3	26.8	19.0	23.6
SE23	22617.1	27027.6	28359.6	1682.0	827.8	1308.2	22.4	1.4	20.3	8.5	21.4	22.9	25.4
Synth-SE23	22636.1	27048.9	28381.8	2599.5	1089.3	1958.1	19.9	2.1	18.2	7.1	25.2	22.9	24.6
SE31	21586.2	25546.6	26114.2	855.7	384.4	601.8	21.3	2.0	23.1	10.5	19.2	18.3	27.0
Synth-SE31	21589.4	25529.1	26131.1	900.0	387.5	689.9	22.0	4.1	21.4	8.9	24.6	18.4	22.5
SE32	23764.6	27989.0	27600.5	394.7	182.6	284.8	22.8	2.6	21.2	9.0	20.1	17.9	29.2
Synth-SE32	23838.3	27297.0	27918.6	794.0	359.6	637.6	25.1	2.6	19.1	10.3	26.9	19.2	21.9
SE33	23170.0	27327.4	26927.9	514.1	241.4	375.1	22.8	3.6	18.3	8.9	19.2	18.3	31.8
Synth-SE33	23219.8	26691.6	27252.6	729.5	327.0	568.8	23.8	4.0	19.9	9.2	25.1	18.3	23.5

Note: The table summarizes predictor variables for each region in Sweden and its synthetic counterpart. For SE-11 (Stockholm), pre-Euro periods are 1986–1992 and 1993–1998. Real GDP per capita is in constant 2015 euros. Population and employment is in thousands of persons. Hours worked is in millions. Investment to GDP measures gross fixed capital formation divided by total GDP for each region in percentage. Sectoral gross value added divided by total gross value added for the region for agriculture, forestry, fishing; industry; construction; wholesale, retail, transport, accommodation and food services, information and communications; financial and business services; non-market services.