# The Implications of Monetary Union for Income Inequality: An Empirical Assessment

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Promises of economic convergence and material prosperity have been reliable companions of the European integration process, also accompanying the creation of economic and monetary union (EMU) and the introduction of the euro. Headline GDP figures support this narrative. During the first decade of the common currency, periphery countries grew faster on average than their peers from the core. This convergence between countries, however, masked differing convergence processes within countries. While periphery countries broadly observed reductions in income inequality levels during this period, core countries experienced increases. Table 1 summarises this trend, displaying average real GDP growth and after-tax Gini figures at the beginning of monetary union in 1999 and at the end of its first decade in 2008, coinciding with the Global Financial Crisis, for selected euro area countries.

While studies analysing the effects of monetary union on growth and inter-country convergence abound (Barrell et al. 2008; Drake and Mills 2010; Fernández and García Perea 2015), empirical research investigating the consequences of EMU for within-country convergence – that is inequality – are scarce.

This is in spite of a growing awareness of the negative economic and societal effects of inequality (Pickett and Wilkinson 2010; Piketty 2014; van der Weide and Milanovic 2018). Against this backdrop, the present article provides an empirical assessment of the within-country inequality implications of the first decade of monetary union.

From a theoretical perspective, there are several channels through which monetary union can be expected to impact income inequality. These do not necessarily pull in the same direction, highlighting the need for empirical evaluation. First, through intensifying trade and technology ties between countries, economic and monetary integration can be argued to affect inequality developments by changing relative factor prices and remunerations (Acemoglu 2002; Krugman 2008). Second, the financialization literature stipulates that deeper financial integration tends to increase inequality by increasing capital’s bargaining power over labour (Kohler et al.

2019). Third, the interplay of national institutional frameworks and the EMU's institutional architecture has enabled member countries to pursue distinct growth regimes (Johnston and Regan 2016), entailing variegated outcomes for inequality (Matthijs 2016).

**Table 1: Average real GDP growth and after-tax Gini, selected euro countries**

| | Avg. real GDP growth | After tax Gini | | |
|---|---|---|---|---|
| | 1999–2008 | 1999 | 2008 | %-change |
| Austria | 2.31 | 26.7 | 27.9 | 4.5 |
| Belgium | 2.16 | 26.3 | 25.3 | –3.8 |
| Finland | 3.17 | 24.5 | 25.3 | 3.3 |
| Germany | 1.54 | 25.7 | 28.5 | 10.9 |
| Netherlands | 2.21 | 24.5 | 26.5 | 8.2 |
| **Core** | **2.28** | 25.5 | 26.7 | **4.7** |
| Greece | 3.56 | 33.8 | 32.6 | –3.6 |
| Ireland | 4.92 | 31.5 | 29.5 | –6.3 |
| Italy | 1.20 | 33.1 | 32.6 | –1.5 |
| Portugal | 1.38 | 33.6 | 33.9 | 0.9 |
| Spain | 3.48 | 30.9 | 32.0 | 3.6 |
| **Periphery** | **2.91** | 32.6 | 32.1 | **–1.5** |
| France | 1.90 | 27.9 | 28.8 | 3.2 |

Source: GDP growth rates taken from Penn World Tables. After tax Gini data taken from SWIID, values scaled between 0 and 100.
Note: France listed separately as it is not part of either country group.

This paper contributes to the handful of empirical studies assessing the relationship between monetary union and within country household income inequality. While insightful, the existing panel econometrics-based literature has shortcomings in their respective identification strategies or is not able to fully take country heterogeneity into account. Some studies, for example, rely on a lagged explanatory variable GMM-IV methodology (Arestis and Phelps 2018; Cesaroni et al. 2019), an approach which has been criticised for the assumptions required to exogenise the endogenous explanatory variables (Reed 2015; Bellemare et al. 2017). By measuring EMU through a dummy variable (Bertola 2010; Bouvet 2010) or not including a comparison or control group in their sample (Bertola 2010; Bouvet 2010; Cesaroni et al. 2019), other studies do not account for the possibly variegated effects of monetary union on inequality and lack a comprehensive set of counterfactuals. By using the synthetic control method – a quasi-experimental technique for counterfactual analysis – Bouvet (2021) mitigates some of these concerns. A drawback of her study is that she relies on a short pre-intervention period to construct her counterfactuals. For a robust synthetic control estimation, however, having a long pre-treatment period is important (Abadie 2021).

Like Bouvet (2021), we employ the synthetic control method to test the effects of monetary union on within-country income inequality. We find the introduction of the euro to have had pronounced effects on inequality developments in Germany and Spain, increasing inequality in the former, while reducing it in the latter. We see our results in line with the growth regime channel (Matthijs 2016): Pursuing an export-led strategy, Germany suppressed wages to boost exports and dampen imports, entailing negative repercussions for its income distribution. Spain, on the other hand, engaged in a debt-fuelled consumption and real estate boom with beneficial consequences for wage growth, hence lowering inequality. We report insignificant results of euro adoption on inequality for four other euro area member states for which we were able to construct valid counterfactuals.[1]

The remainder of the paper is structured as follows: Section 1. discusses the different theoretical channels through which economic integration can impact on inequality. Section 2. reviews the relevant empirical literature. Section 3. introduces the synthetic control methodology and discusses its advantages and disadvantages over panel econometrics-based approaches. Section 4. outlines the data used in the empirical analysis and section 5. presents and discusses the estimation results. Section 6. concludes the paper.

## 1. Different channels explaining the effects of EMU on inequality

Before turning to our empirical analysis, this section outlines the theoretical motivation behind our research question. We identify four channels in the literature through which economic integration and thus EMU can be argued to affect within-country household inequality: a trade, technology, financialization and a growth regime channel. The trade and technology channels both affect inequality through changing relative factor prices. The financialization channel focuses on changes in the power relations between capital and labour to explain inequality trends. The growth regime channel, finally, points to the interplay between national and EMU level institutions as the driving forces behind inequality movements.

In our research, we are primarily concerned with first order market effects of economic integration on inequality and leave considerations about the interaction between different welfare state regimes and economic inte-

1 These are Finland, France, Ireland and Italy. We were unable to produce valid counterfactuals for Greece and Portugal. Due to data limitations, we do not include Austria, Belgium, Luxembourg and the Netherlands in our study.

gration for future research. We therefore focus on the Gini index for market income, as opposed to the Gini for post-tax and post-transfer income.

We discuss these four channels and their plausibility in the context of EMU below, starting with the trade channel.

## The trade channel

As the pinnacle of European economic integration, the EMU has further deepened trade ties between euro area member states. In a Heckscher-Ohlin framework, the Stolper-Samuelson theorem (Stolper and Samuelson 1941) holds that international trade will lead to specialisation according to relative factor abundance; entailing relative wage increases for skilled workers vis-á-vis unskilled workers in advanced, skilled labour abundant countries and relative wage increases for unskilled workers vis-á-vis skilled workers in unskilled labour abundant countries (see Krugman 2008 for a more contemporary context). It thus predicts trade to increase inequality in skilled labour abundant countries, while decreasing it in unskilled labour abundant countries. Its usefulness for analysing the EMU context might, however, be limited. Not only have core assumptions of the theorem – full employment and international factor immobility – been violated for some time. But EMU countries also all belong to the same country group of advanced economies, impeding a clear delineation between skilled and unskilled labour abundant countries. Subsequent contributions to trade theory have highlighted the possibility of offshoring causing an overall increase in inequality given that the offshored activities are considered high skill in the destination country (Feenstra and Hanson 1996). Mapped onto the EMU context, the aforementioned problem of country group homogeneity remains, however.

## The technology channel

A popular argument advanced by neoclassical economics to explain rising wage inequality since the 1980s relates to skill biased technological change. The basic tenant of this hypothesis is that recent technological advances – instead of being factor neutral – have favoured skilled labour over unskilled labour, leading to a wage premium for skilled workers, thereby driving wage inequality (see Acemoglu 2002 for an overview and Card and DiNardo 2002 for a critical assessment). As far as trade also leads to a diffusion of technologies across countries, trade integration can therefore be argued to increase inequality in technology-importing countries. Thus, by increasing the diffusion of technology through trade, the EMU could be argued to affect inequality. Empirical support for this hypothesis appears to be mixed, however (compare, for example, the results in Jaumotte et al. 2013 and Stockhammer 2017).

## The financialization channel

While both the trade and technology channels see inequality developments driven by changes in relative factor prices, the financialization channel points to changes in the power relations between labour and capital as driving inequality trends, for example by increasing exit options of capital or incentivising shareholder value maximization practices (Kohler et al. 2019). Several econometric studies investigating the effects of financialization on the wage share (the functional income distribution), find both statistically and economically significant negative effects on inequality (see the literature summarised in Table 1 in Kohler et al. 2019).

Since EMU has not only intensified trade but also financial integration amongst member countries, it is a priori not unreasonable to assume that this has also contributed to inequality developments in the common currency area. In this case we would expect a uniform negative effect across euro countries. This expectation is, however, at odds with the variegated inequality trends observed in the descriptive statistics outlined in the introduction, where we note a tendential decline of inequality in periphery countries and an increase in core countries.

## The growth regime channel

Finally, the growth regime channel identifies the interplay between national and EMU institutions as an explanation for inequality movements in euro countries (Matthijs 2016). Entering a monetary union enabled participating countries to pursue different growth models, each of them relying on different components of aggregate demand (Johnston and Regan 2016; Hall 2018). On the one hand, EMU core countries, with Germany as the archetype, engaged in an export-led growth strategy, relying on successful wage suppression throughout the economy and low inflation, keeping domestic demand in check. By restraining wage growth of large segments of the economy, such a strategy can lead to increases in inequality. EMU periphery countries on the other hand, engaged in a debt-fuelled and consumption-led growth strategy. The specific forms of this type varied, with countries like Greece more reliant on public debt, while Spain, for example, saw sharp increases in private debt accompanied by a real estate bubble (Nölke 2016).[2] Especially this latter variant, by relying on mass consumption and growth in low skill-intensive sectors like the construction industry, can be argued to reduce inequality. As the case of Italy exemplifies, there are also countries that failed to adopt a successful growth strategy under the euro (Baccaro and Pontusson 2016).[3] Taken together, we

[2] Portugal experienced increases in both public and private debt.

[3] Average GDP growth rate for the euro area was 2.1 per cent during the first ten years of EMU, compared with 1.2 per cent for Italy.

would thus expect the growth regime channel to lead to different inequality trajectories in different countries, with inequality increasing in core countries, decreasing in periphery countries and limited effects in countries that failed to successfully adopt a growth strategy. This pattern is reflected in the descriptive statistics provided in the introduction.

As this section has highlighted, the overall impact of European economic integration on within country income inequality is a priori unclear from a theoretical perspective. The different channels identified predict at times opposing tendencies. On the one hand, the financialization and the technology mechanism imply on average negative effects of closer economic integration for within-country inequality. The implications of the trade channel for the income distribution, on the other hand, depend on the country in question and its respective factor endowments. With all EMU countries belonging to the group of advanced economies its application is, however, impeded in this context. Finally, the growth regime channel predicts variegated impacts of EMU on inequality, depending on the country's growth strategy. Having discussed theoretical considerations in this section, we now turn to a survey of the existing empirical literature on the EMU-inequality nexus.

## 2. EMU and inequality: some empirics

Arestis and Phelps (2018), Bertola (2010), Bouvet (2010; 2021) and Cesaroni et al. (2019) are the available empirical studies that explicitly analyse the relationship between European economic integration and inequality. Before proceeding with our own analysis, we will briefly review these studies and their findings, as well as outline the gaps in the empirical literature where we seek to contribute.

The two earliest contributions on this topic are Bertola (2010) and Bouvet (2010). Bertola analyses a balanced panel dataset of the EU15 member states (without Luxembourg) over the period 1995–2005, using two-way fixed effects estimation. Euro adoption is modelled using a dummy variable. His results indicate that monetary union led to a slight increase in income inequality. While being the first study to analyse EMU's impact in inequality, Bertola's empirical analysis also has a couple of limitations which later contributions sought to address: his study analyses a limited counterfactual (only EU member states are included) and has a short time span of estimation. Furthermore, estimating a uniform effect does not allow for heterogeneity between country groups. Finally, identification rests on the assumption that using two-way fixed effects is sufficiently able to deal with endogeneity concerns and that inequality trends between euro countries follow a parallel trend. Bouvet (2010) looks at another angle of inequality by examining the implications of EMU using interregional inequality of GDP-per-capita instead of country level Ginis. Her analysis focuses on 197 NUTS2 regions (within EU15 sans Ireland and Luxembourg) over the period 1977–2003. She employs a country fixed effects specification and finds that monetary union has worsened regional disparities in poorer countries while reporting insignificant effects for richer countries within the euro area. Bouvet likewise uses a dummy variable to measure EMU effects on inequality. Her results are thus not directly comparable to our and other contributions to the literature as interregional and country-level household income inequality need not be perfectly correlated. Both Bertola's (2010) and Bouvet's (2010) estimation strategy do not include counterfactual comparators outside the EU.

The most methodologically sophisticated attempt to estimate the inequality dynamics of monetary union has been undertaken by Arestis and Phelps (2018). They analyse a sample of 34 countries (19 EMU, 6 EMU candidate, and 10 non-EMU countries, 7 of which are outside the EU) over the period 1970–2013. The estimation strategy consists of (i) two-way fixed effects, (ii) dynamic lagged dependent variable panel, (iii) iteratively reweighted least squares, and (iv) GMM-IV using lagged explanatory variables as instruments. They measure monetary union through different dummies that delineate the stages of euro introduction as well as including a rich set of covariates. Their analysis finds that the overall effect of euro adoption has been to decrease inequality. Moreover, they find the results to be strongest for periphery countries (in Eastern and Southern Europe) and non-significant for core countries. Their results are driven by the dampening effect of trade on inequality, partially supporting the predictions of the Stolper-Samuelson theorem. To alleviate endogeneity concerns the authors use lagged explanatory variables to instrument for their contemporaneous counterparts. While this approach is common in the literature it has also been criticised for the assumptions required to sufficiently exogenise the endogenous explanatory variables (Reed, 2015; Bellemare et al., 2017). For example, their lagged financial-openness indicator (the Chinn-Ito index) could influence inequality not only through its direct effect on contemporaneous financial openness (which implies exogeneity) but also through indirect intertemporal effects on inequality. One can reasonably assume that last year's financial-openness affects this year's inequality, even though this year's financial-openness might have changed. Thus, Arestis and Phelps's results (2018) require somewhat restrictive assumptions on the data-generating process to hold.

Cesaroni et al. (2019) likewise use a GMM-IV (lagged explanatory variable) estimation technique to investigate the determinants of income inequality in twelve euro countries spanning the period 1980–2015. While not directly estimating the effect of monetary union, the authors use trade and financial openness indicators to proxy the effect of European integration. Their results suggest that the integration process has had different effects for core and periphery countries with inequality declining in core and increasing in periphery countries. This result is mostly driven by southern periphery countries. The main drawback of their analysis is that it encompasses euro countries only, thereby not providing a counterfactual. The concerns over successful identification using GMM-IV outlined above apply here as well.

In a recent paper, Bouvet (2021) uses the synthetic control method to estimate the effect the euro introduction had on adopting countries. Broadly speaking, her results suggest that entering a currency union has exacerbated gross-income inequality while at the same time reducing net-income inequality. Bouvet argues that this decline in net-income inequality was achieved because lower interest rates following the start of monetary union aided countries in the financing of their welfare programmes. Using the synthetic control method, Bouvet's study mitigates some of the problems of the empirical strategies employed in the papers discussed above. The method is designed to deal with the lack of a counterfactual and provides a non-parametric framework for quasi-experimental analysis requiring only limited assumptions. Probably the main drawback of Bouvet (2021) is the short pre-intervention period of seven years, starting the training period only in 1992. As noted by Abadie (2021), a long pre-treatment period is important to limit the bias of the synthetic control estimator. In their seminal contributions, Abadie and Gardeazabal (2003), Abadie et al. (2010) and Abadie et al. (2015), use pre-intervention periods of 18 to 30 years, for example. Moreover, the Maastricht treaty, which set out the institutional framework of what would become the Eurozone, was signed in 1992. Thus, a training period starting in 1992 also suffers from potential anticipation effects in constructing the synthetic controls. If the effects of euro introduction are anticipated before its formal introduction in 1999, then the synthetic control constructed on pre-intervention data will already measure some of the treatment effect. Although Bouvet (2021) includes a discussion of in-time placebos to account for this, the lack of training data preceding the potential onset of anticipation effects call into question the validity of this robustness check. Finally, Bouvet (2021) also includes both the Global Financial Crisis and the euro crisis in her post-intervention period. This can be problematic if these two events represent a structural and or asymmetric shock to countries in her donor pool (Abadie et al. 2015). In particular, the Global Financial Crisis is arguably a separate treatment by itself with the potential to amplify and or reverse the inequality effects of euro adoption. Evaluating the euro's inequality effects up until the mid-2010s measures the treatment effects of both monetary union and the GFC, thus hampering clear identification.

The empirical literature surveyed above paints an inconclusive picture of the consequences of economic integration for inequality. This is due to the different samples (both time and countries) as well as methodological approaches employed. Furthermore, for the majority of the empirical contributions there are concerns regarding clear identification of the effect of euro adoption on inequality. They either do not provide a counterfactual (Bertola 2010[4]; Cesaroni et al. 2019) or insufficiently deal with endogeneity of the EMU effect and omitted variable bias (Bouvet 2010; Arestis and Phelps 2018). Using GMM-IV with lagged explanatory variables as instruments does not, in our view, automatically and sufficiently tackle the problem of simultaneity. The synthetic control method we employ in this paper can mitigate these endogeneity concerns. It is designed to deal with the lack of a counterfactual and provides a non-parametric framework for quasi-experimental analysis requiring only limited assumptions. While Bouvet (2021) also uses this method, her short training period raises concerns over the validity of her estimation.

## 3. Methodology

To perform our analysis, we use the synthetic control method for comparative case studies, pioneered by Abadie and Gardeazabal (2003) and extended by Abadie et al. (2010, 2015). We briefly summarise its key concepts in this section.

The synthetic control method compares the dependent variable, country-level income inequality in our case, of the country of interest to an artificially constructed counterfactual. This 'synthetic' control unit is constructed using a convex combination of control countries (the 'donor pool') that best resembles the treatment unit before the intervention takes place, using data on both the outcome variable as well as a set of covariates that correlate with the outcome variable. The trajectories of the outcome variable for the treatment country and the synthetic control can then be compared to assess the effect of the treatment[5].

The method has previously been used to analyse EMU's effect on GDP per capita (Férnandez and Perea 2015) and real exchange rates (El-Shagi et al. 2016), the impact of the Stability and Growth Pact on member states' government debt (Köhler and König 2015), the economic benefits of the EU (Campos et al. 2014), the effect of EMU on current account balances

4 Bertola (2010) only includes three non-euro countries (Denmark, Sweden, and the UK) in his sample.

5 We omit a detailed formal description of the synthetic control method which can be found in Abadie et al. (2010).

(Hope 2016), as well as the effect of the euro on income inequality (Bouvet 2021).

An appealing feature of the synthetic control method is that it combines the advantages of comparative case studies with a data-driven approach to selecting and constructing the counterfactual. This creates a quasi-experimental setting, which lends itself to the estimation of causal treatment effects. A drawback of the method is that it does not allow for the disaggregation of overall effects into different channels.

As the synthetic control method does not lend itself to classic methods of statistical inference, we employ so-called in-space placebos to assess the validity of our findings (Abadie 2021). This procedure randomly reassigns the treatment to units from the donor pool and compares them with their optimal synthetic control unit (constructed using the other donor pool units). Similarly large results of the synthetic control estimation for the placebo treatments would indicate that the observed effects for the real treatment unit are not due to the treatment effect. This allows for the construction of *p*-values, as exposing all donor units to the treatment will generate a distribution of intervention effects against which the true synthetic control estimate can be compared.

There have been a number of methodological advancements in the synthetic control literature in recent years. Ben-Michael et al. (2021), for example, present an augmented synthetic control estimator that allows for the creation of synthetic units outside the convex hull of control cases. We incorporate this extension as a robustness check to our baseline model. On the other hand, the generalized synthetic control method (Xu 2017) and the bias corrected synthetic control inference (Chernozhukov et al. 2021) are not relevant to our study as they either place strong assumptions on the data-generating process or require both larger T and N than our sample permits.

## 4. Data

To conduct the estimations, we use a balanced, yearly panel dataset running from 1975–2007[6]. The outcome variable is market household income inequality, as measured by the before-tax Gini coefficient. The outcome variable data stems from the Standardized World Income Inequality Database SWIID (Solt 2020). Comparable cross-country inequality data of

6 We restrict our estimation period to 2007 as this represents nine years after the introduction of the euro and should thus be able to capture most of the effects this has had on inequality. Extending the sample beyond that would increase the likelihood of other policy changes or economic events – like the Global Financial Crisis – affecting inequality and would thus dilute any potential euro effect.

this kind is not without its drawbacks (see the 2015 special issue of the Journal of Economic Inequality for a review of cross-country inequality data), but for the country groups we use in our analysis the SWIID appears to provide reasonably reliable data. The criticisms levelled against the use of SWIID (Jenkins 2015) mainly apply to inequality data for developing economies. As our baseline estimations use data from OECD economies these concerns are mitigated in our setting.

In addition to income inequality, we use a number of covariates of inequality to estimate the synthetic controls. Our choice of covariates is informed by some of the existing empirical literature investigating income inequality (Carey and Horiuchi 2013; Stockhammer 2017; Arestis and Phelps 2018, Hartwell et al. 2019). We use real GDP per capita, a country's overall population as well as population density, the employment rate (of the working age population), trade openness, the share of adults who have completed (i) secondary and (ii) tertiary education, the degree of financial market openness, the capital stock as a share of GDP, the government consumption share of GDP and the share of labour compensation over GDP. The data on covariates comes from the Penn World Tables (GDP, employment rate, trade openness, government share, labour share)[7], the Barro-Lee education database (secondary and tertiary education)[8], the United Nations World Population Prospects (population and population density),[9] and the Chinn-Ito database (Chinn and Ito 2008) (financial market openness)[10].[11]

To deal with missing observations for covariates we linearly interpolate missing values if we have observations for both the year preceding and the year following the missing value. Observations with missing values at either the beginning or the end of the dataset are dropped. As a result, we obtain a balanced panel dataset running from 1975 until 2007 with 34 countries, containing 8 potential treatment countries and 26 potential controls. Data limitations prohibited us from going back longer in time, as this would have further restricted our sample of treatment countries[12]. While the synthetic control literature often restricts the donor pool to countries that are geographically close to the treatment country, we opt for a less re-

7 https://cran.r-project.org/web/packages/pwt9/pwt9.pdf

8 http://www.barrolee.com/

9 https://population.un.org/wpp/Download/Standard/Population/

10 http://web.pdx.edu/~ito/Chinn-Ito_website.htm

11 As discussed in Section 1 above, the financialization literature highlights labour power as an important determinant of income inequality (Kohler et al. 2019). We therefore also include a measure of union density, taken from the OECD's / AIAS' ICTWSS database, as a covariate in one of our robustness checks conducted in Section 5 below.

12 Shortening the pre-treatment period by five years (and starting the panel dataset in 1980) does not increase the number of treatment countries for which data is available while also not significantly expanding the list of countries available for the donor pool.

stricted donor pool of OECD countries to increase the likelihood that the synthetic units match the covariates of the treatment units (El-Shagi 2016). As an additional robustness check we rerun the estimations using an expanded control sample of 34 upper-middle income countries, using the World Bank's upper-middle income country GDP threshold.

## 5. Results

Having outlined the synthetic control method and the data we use in the previous sections, this section presents and discusses the results of our analysis. Given data availability constraints, we conduct estimations of the effect of euro adoption on inequality for Finland, France, Germany, Greece, Italy, Ireland, Portugal, and Spain. Although this is only a subset of euro countries, it presents a wide range of different types of political economies in Europe with different levels of income and economic development, and different regimes governing capital and labour markets, taxation, and welfare states (Esping-Andersen 1990).

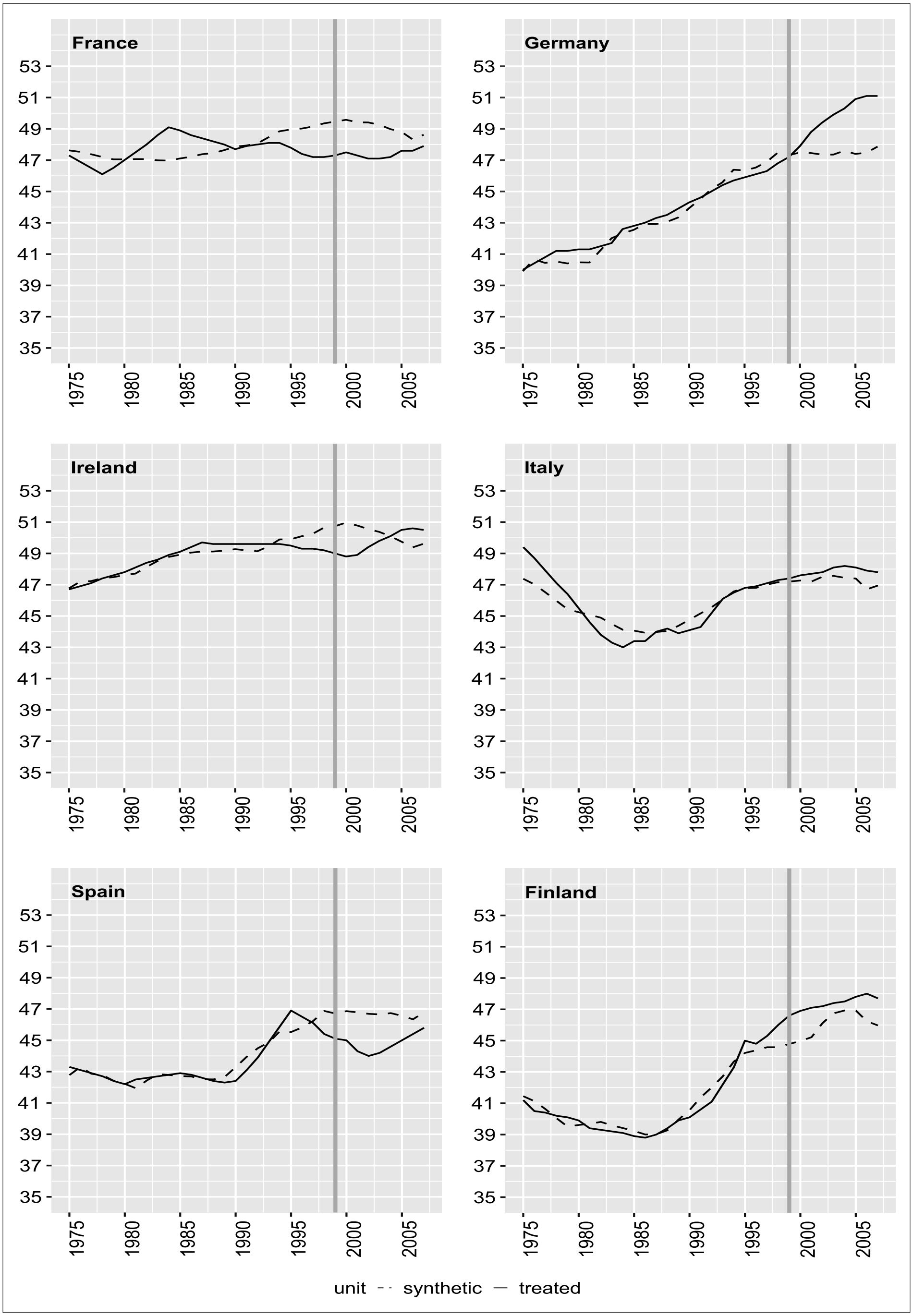
One of the crucial assumptions for the synthetic control method to work is that the pre-treatment trajectory of the synthetic unit closely resembles the trajectory of the actual unit (Abadie et al. 2015). Only then can a divergence of post-treatment trajectories be truly interpreted as representing the results of the treatment. The pre-treatment mean squared prediction error (MSPE) can be used as a metric to quantify the difference between the treatment unit and its synthetic control. Too large an MSPE suggests that the synthetic control unit does not track the pre-treatment unit closely and invalidates the results. We follow Abadie et al. (2010) in using 2 as a threshold for the MSPE. Table 3 summarizes the MSPE for each of the treatment countries. The synthetic control method is able to sufficiently track the pre-treatment trajectories of all treatment countries except for

**Table 2: Pre-treatment MSPE for treatment countries**

| Country | MSPE | MSPE below 2 |
|---|---|---|
| Finland | 0.31 | TRUE |
| France | 1.29 | TRUE |
| Germany | 0.23 | TRUE |
| Greece | 2.67 | FALSE |
| Ireland | 0.24 | TRUE |
| Italy | 0.73 | TRUE |
| Portugal | 3.66 | FALSE |
| Spain | 0.31 | TRUE |

Note: The MSPE was calculated over the pre-treatment period of 1975–1998.

## Figure 1: Path plots for treatment units




Note: Trajectory of Gini index of market income (0–100 scale) for treated (solid) and synthetic units (dashed).

Greece (MSPE of 3.28) and Portugal (MSPE of 3.66). To err on the side of caution we exclude both Greece and Portugal from the results discussion.

Table A1 in the Appendix gives the weight of the control countries making up each of the synthetic units for the treatment countries (including only countries with nonzero weights). In addition, Table A2 gives the relative importance of each of the covariates used to estimate the synthetic control units.

The results of the comparison between the treated units and their synthetic controls are presented in Figure 1 which shows the trajectories of the treated unit (solid line) and its synthetic control (dashed line) for the entirety of our dataset. The vertical line represents the onset of the treatment, the introduction of the euro in 1999. An effect on inequality exists if the trajectory of the treatment and synthetic control lines diverge considerably thereafter.

To be able to gauge the significance of the effect of EMU treatment on inequality in our sample, we rely on the use of placebo tests for each of our treatment units (Abadie 2021). As discussed in the methodology section, in-space placebos artificially reassign the treatment (introduction of the euro) to each of the control countries in the donor pool. Subsequently, we estimate an additional synthetic control unit for each of the control countries. Each control unit and its new synthetic control is then compared by computing the post/pre-MSPE ratio, i.e. the ratio of the effect size after treatment (the MSPE from 1999–2007) to the goodness of the fit of the synthetic control before treatment (the MSPE from 1975–1998). This procedure ensures that the post-treatment effect is weighted by the goodness of fit of the synthetic control and reduces the effect of random deviations. We therefore obtain a distribution of effect sizes for an artificial treatment. Only if the effect size for the actually treated unit (e.g. France) is sufficiently larger than the average of the effect sizes for each of the artificially treated control countries can we reject the null hypothesis of no effect of euro

**Table 3: *p*-values of placebo tests for treatment countries**

| Country | MSPE | MSPE below 2 |
|---|---|---|
| Finland | 0.18 | 2/11 |
| France | 0.54 | 6/11 |
| Germany | 0.09* | 1/11 |
| Ireland | 0.18 | 2/11 |
| Italy | 0.73 | 8/11 |
| Spain | 0.09* | 1/11 |

Note: * denotes the test being significant at the 10% level. Note that due to the discrete nature of the testing procedure (comparing ranks), the minimum *p*-value that can be achieved is 0.09 as the sample size of the donor pool is 11.

adoption on inequality. Figures A2–A9 in the appendix show the distribution of results of the placebo tests. In-space placebos also allow for the calculation of *p*-values (Abadie 2021) which simply represent the relative rank of the post/pre-MSPE of the treated country versus all other countries in the donor pool. Table 3 provides the results of these placebo tests.

## Discussion of cases

For Finland, France, Ireland, and Italy there is no clear significant effect of monetary union on inequality. (i) The synthetic control unit of France is below pre-euro inequality of real France in the 1980s and moves above it in the mid-1990s, about 5 years before the euro was introduced. The gap between the synthetic and real unit therefore already exists at the time of treatment and cannot be attributed to the euro. Concurrently, France's *p*-value from the placebo test is at 0.54, making rejection of the null hypothesis of no effect of monetary union on inequality unlikely. (ii) In the case of Ireland, the synthetic unit tracks real Ireland well but also overshoots inequality of the real unit in the years preceding treatment. The gap between the synthetic and real unit narrows after EMU introduction and disappears by about 2004. Although Ireland has the lowest *p*-value among the cases where we do not find significant effects (0.18), the magnitude of the effect is too small to be of economic significance. (iii) For Italy, the synthetic unit is able to track the U-shaped curve of inequality in real Italy in the 1980s and 1990s with only a slight divergence of the two units in the 1980s. Euro introduction appears to show a slight effect with inequality in real Italy exceeding its synthetic counterpart. However, a very high *p*-value (0.73) makes attribution of a significant effect impossible. (iv) The case of Finland looks promising. Not only does the synthetic unit track real Finland well before the euro was introduced but there is also a noticeable divergence between the two after the start of monetary union. However, the gap starts to emerge about two years before the treatment and the overall effect also does not seem particularly pronounced. This conclusion is bolstered by a *p*-value of 0.18.

The strongest results are visible for the case of Germany. The tracking of the synthetic unit is very good (MSPE of 0.23) and the introduction of the euro coincides with a very clear divergence of the real and synthetic units. From 1999 onwards inequality of real Germany soars by about 4 Gini percentage points compared to its synthetic counterpart. This is a clear indication of the effect monetary union had on inequality in Germany. While many idiosyncratic policy changes occurred in Germany during the 1990s and 2000s none of them coincide this clearly with the divergence in inequality between the synthetic and real unit as the beginning of the currency union: German reunification, which could arguably effect income inequality simply due to the fact that Eastern Germany was significantly poorer than Western Germany at the time of reunification, went into effect in 1990 already. The ability of synthetic Germany to track real German inequality up until 1999 negates any effect of German reunification on these results. In a similar vein, the far-reaching social policy reforms in Germany ("Agenda 2010") only started in late 2003 and did not come into full effect until 2005. The divergence between synthetic and real Germany clearly precedes this. The significance of these effects is corroborated by a p-value of 0.09[13].

We also observe significant results for the case of Spain. While the tracking of the synthetic unit is less accurate than in the case of Germany, synthetic Spain still tracks real Spain quite well (MSPE of 0.31) with slight divergences in inequality around 1990 and 1995. The two units converge just before the treatment period, however. Following the start of monetary union, we see a clear trend of decreasing inequality in Spain compared to its synthetic unit, culminating in a difference of about 3.5 percentage points between real and synthetic Spain by 2004. There is no policy event of a significance similar to the adoption of the euro happening in Spain at the end of the 1990s that could explain this effect. Just as with Germany, the *p*-value for Spain stands at 0.09[14].

Overall, from the four channels outlined in section 1, we see our results as lending most support to the growth model perspective. As argued by Matthijs (2016), the interplay between member states' and EMU's institutional infrastructure fostered divergent inequality trends in core and periphery countries. By fixing exchange rates, monetary union allowed the export-oriented political economies of the core to capitalise on their abilities to restrain wage growth in their economies. Labour's restraint benefitted capital, which saw its share in GDP increase during this time[15], widening inequality. For peripheral political economies, on the other hand, the introduction of the euro heralded a decade of on average high growth, driven by a debt-fuelled expansion of domestic demand. Employment growth and rising wages were the consequences, entailing falling levels of inequality.

The other channels discussed in section 2 are less convincing in explaining the trajectories of inequality in Spain and Germany. We would expect both the financialization and the technology channel to lead to overall increases in inequality, which is at odds with the developments in Spain. And while the Stolper-Samuelson theorem (Stolper and Samuelson 1941) can

13 Given the size of the control sample ($n$ = 11), a $p$-value 0.09 is the lowest $p$-value that can be achieved and corresponds to the pre/post-MSPE ratio in Germany being higher compared to all placebo counterparts.

14 Again, coinciding with the highest ranking in the pre/post-MSPE ratio.

15 According to AMECO data, the adjusted wage share of core countries fell by 3.7 percent during the first decade of EMU (1999–2008).

in principle account for differential effects on inequality, it can be questioned whether its application to this western European context is appropriate. Not only do both Germany and Spain belong to the group of advanced economies, but the validity of key assumptions of the theorem – full employment and factor immobility – also cannot be upheld.

The growth model perspective also helps to explain why we fail to find an effect of monetary union on inequality for Finland, France, Ireland, and Italy – that is, why we cannot reject our H0 for these countries.

In the case of Finland, we do observe a slight exacerbating effect of EMU introduction on income inequality. This result is in line with expectations as Finland is usually classified as an export-driven economy (Nölke 2016). However, the size of the effect is significantly smaller than in the case of Germany. Two factors explain this divergence: First, while maintaining a current account surplus throughout, Finland simultaneously had high wage growth throughout the 1990s and the 2000s, making its growth model less dependent on exports and more reliant on domestic consumption. This in turn weakens the effect the euro had on exacerbating inequality. Second, Finland suffered a severe financial crisis in the early 1990s. The strong rebound effect observed during the late 1990s coincides with the introduction of the euro, thus blurring its potential effect of increasing inequality.

We observe similarly ambiguous results for France. This comes as no surprise as the literature on comparative capitalism classifies France as fitting neither fully into the export-led nor the consumption-led growth model. In particular, France is characterized by both a large domestic non-traded sector as well moderate wage settlements engineered by the state (Johnston and Regan 2016).

Just as with Finland, the results for Ireland point in the right direction. The Post Keynesian literature normally classifies Ireland as a debt-led country (Stockhammer 2016; Kohler and Stockhammer 2021), with the comparative capitalism literature seeing it as more of a borderline case (Johnston and Regan 2016; Nölke 2016). We would thus expect inequality in synthetic Ireland to overshoot real Ireland, which is indeed what we observe. A major problem for our estimation, however, is that the Good Friday Agreement, which ended decades of violence and terror across the island of Ireland, was signed in 1998, just a year prior to the start of monetary union. Such major events coinciding with each other makes it very difficult to credibly disentangle their effects on our outcome variable. We still report our results for Ireland for the sake of completeness but would advise for caution in their interpretation.

Finally, it is also not surprising that we fail to find a substantial effect of the introduction of the euro on inequality developments in Italy. Italy's GDP growth performance has been poor since the start of monetary union, averaging a meagre 1.2 per cent during the first decade of the euro, compared with 2.1 per cent for the euro area. Italy failed to adopt a successful growth strategy under the euro and hence we also find no effect of its adoption on inequality developments there.

While we do not find a significant impact of monetary union on inequality for Finland, France, Ireland, and Italy we do observe fairly large (between 3 and 4 percentage points) effects on inequality in the two archetypical cases for each growth model, Germany, and Spain. As expected, the introduction of the euro leads to higher inequality in the export-oriented growth model of Germany whereas it lowered inequality in the debt-oriented growth model of Spain.

## Robustness checks

We conduct several robustness checks. First, we re-estimate our model using an extended donor pool of 34 upper-middle income and high-income countries[16] for all countries in our sample.[17] We use the same covariates and time periods to estimate these results. The results broadly correspond to that of the baseline model with a restricted donor pool. Figure A10 in the Appendix gives detailed results. For all countries we observe a similar sign and size of the effect as for the restricted control pool, increasing confidence in our results.[18]

We also rerun our model using the Augmented Synthetic Control Method (ASCM) developed by Ben-Michael et al. (2021). While the standard synthetic control unit is restricted to lie within the convex hull of the donor pool countries, ASCM relaxes that restriction and constructs synthetic control units by extrapolating outside of the convex combinations of the donor countries. The purpose of ASCM is to construct better synthetic counterfactuals especially in the absence of good pre-intervention fits. To avoid excessive levels of extrapolation ASCM relies on shrinkage estimator-based techniques. We follow Ben-Michael et al. (2021) and use a Ridge

16 We follow the World Bank definition of upper-middle income countries as those with a GNI per capita (PPP) higher than $3,956 in 2018 (seehttps://data.worldbank.org/country/XT).

17 The list of control countries is: Argentina, Australia, Brazil, Canada, Chile, Colombia, Costa Rica, Hungary, India, Indonesia, Japan, Malaysia, Mexico, Norway, Panama, Peru, Philippines, Singapore, South Africa, Sri Lanka, Sweden, Thailand, United Kingdom, United States;

18 As an additional robustness check, we re-estimate our baseline model this time including the OECD's/AIAS' ICTWSS measure of union density as a covariate. Due to missing data, we have to exclude Greece and Portugal from this estimation. Both qualitatively and quantitatively, results remain essentially the same as compared to our baseline specification. Results are not reported here but are provided by the authors upon request.

regression-based ASCM objective function to penalize extrapolation. We do not find that ASCM significantly improves pre-treatment fit for any of our cases. Thus, we still fail to produce a sufficient pre-treatment fit for Greece and Portugal for the synthetic control method to perform valid inference on these cases. Further, the sign and size of the remaining cases for which we have good pre-interventions fits does not change significantly when we use ASCM. Figure A11 in the Appendix compares the results of our baseline specification and ASCM.

For Germany and Spain, the two countries we find significant results for, we also perform covariate, in-time placebo, and donor pool sensitivity analyses. For the first, we re-estimate the model for the two countries consecutively with increasing numbers of covariates (Abadie 2021). The most parsimonious specification estimates the model using only the lagged dependent variable to construct the synthetic counterfactual. For Germany, the results remain broadly the same even when only lagged values of the dependent variable are used as predictors. This is not the case for Spain, where only using lagged values of the market Gini produce a poor pre-treatment fit. As soon as more covariates are added, however, the results of the fully specified model are replicated fairly well. Figure A12 in the Appendix presents these results.

For the donor pool sensitivity analysis (what Abadie et al. 2015, call 'leave-one-out'), we again re-estimate the models for Germany and Spain iteratively, but this time dropping one of the countries from the donor pool in each run. This is done to ensure that our findings are not purely driven by one country in the donor pool. Figure A13 in the Appendix displays the results of this exercise. For neither country, dropping a country from their donor pool leads to substantive changes in the observed effect.

The in-time placebo analysis artificially reassigns the treatment to an earlier point in time (Abadie et al. 2015) and constructs the synthetic control unit up until this earlier treatment. This analysis can be used as a falsification and anticipation test. The falsification test helps to gauge whether an effect occurs when treatment is randomly assigned. The anticipation test measures the extent to which the effects of introducing the euro are felt prior to its actual launch. In our analysis, we artificially reassign monetary union to start in each of the years between 1993 and 1998 (see Figure A14 in the appendix). Our results indicate that there are no anticipation effects in the case of Germany and moderate anticipation effects for Spain. This is to be expected as the convergence of real interest rates prior to EMU introduction, which was completed by 1997, undoubtedly increased the availability of cheap credit to boost consumption. For both countries the falsification test does not result in substantial treatment effects, mitigating potential concerns that our baseline results are driven by random variation.

# 6. Conclusion

This paper contributes to the literature on the effects of monetary union on within country income inequality. To mitigate concerns over identification, we employ the synthetic control method, a quasi-experimental technique for counterfactual analysis. Our results indicate that for Germany and Spain, the first decade of monetary union led to sizable but diverging effects on income inequality: Inequality increased in Germany whereas it decreased in Spain. These results are in line with the predictions of a growth model analysis of European political economies (Matthijs 2016). In this reading, the introduction of the euro allowed Germany to double-down on its export-led growth strategy, underpinned by suppressing wage growth. Spain, on the other hand, experienced a debt-fuelled growth spurt, leading to employment and wage growth, especially in low-skilled sectors.

The Global Financial Crisis (GFC) put an end to Spain's growth model as capital inflows dried up and market sentiment regarding Spain's fiscal sustainability deteriorated. Spain's subsequent economic trajectory from 2014 onwards was predicated on liberalisation of the economy and welfare state retrenchment (Hopkin 2016). Germany's growth model, on the other hand, continued unabated. It was indeed in the decade following the GFC that Germany recorded its largest current account surpluses to date. However, as it relies on the availability of a large export market sustained by a common currency, it is questionable whether the export-led growth strategy is ultimately sustainable and can be extended as a strategy to other economies within the Eurozone. In general, such a strategy is also limited by the fact that the world as a whole has to have a balanced current account. Similar to Kohler and Stockhammer (2021), future research could further investigate to what extent these growth strategies have evolved in the latter half of the 2010s.

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## Abstract

The promise of greater material prosperity and economic convergence has underpinned the process of European economic integration. Its consequences for income inequalities within individual countries, however, have so far been little discussed. This paper contributes to the literature by investigating the effects of European economic integration on intra-country income inequality using the synthetic control method, thereby mitigating common concerns of identification encountered by panel econometric-based approaches. We find that out of our sample of eight countries the introduction of the euro – the pinnacle of economic and monetary union – had significant effects on inequality in Germany and Spain. From the several theories outlined in the literature, our results lend most support to the growth regime channel.

## Zusammenfassung

Versprechen von wirtschaftlicher Konvergenz und größerem materiellen Wohlstand haben den Prozess der wirtschaftlichen Integration Europas stets begleitet. Dessen Auswirkungen auf die Einkommensverteilung innerhalb von einzelnen Ländern haben in der akademischen Forschung jedoch bisher vergleichsweise wenig Aufmerksamkeit erlangt. Dieser Artikel ergänzt die bestehende Literatur, indem er die Folgen der wirtschaftlichen Integration Europas auf nationale Einkommensverteilungen mittels der synthetischen Kontrollmethode (engl. „synthetic control method") schätzt. Diese Schätzmethode mildert gängige Bedenken in ökonometrischen Paneldatenanalysen bezüglich der Identifikation von Effekten. Signifikante Effekte der Euroeinführung – der bisherige Höhepunkt der wirtschaftlichen Integration – werden aus einem Sample von acht Ländern für Deutschland und Spanien gefunden. Von den verschiedenen Wirkungsmechanismen, die in der Literatur diskutiert werden, unterstützen diese Ergebnisse die Wachstumsregime-Perspektive.

**Schlüsselwörter:** Einkommensungleichheit, Europäische Währungsunion, Synthetische Kontrollmethode.

**Keywords:** Income Inequality, European Monetary Union, Synthetic Control Method.

**JEL-codes:** P16, D63, N14, N10.

# Appendix

## Table A1: Country weights for synthetic control

| Finland: Synthetic unit countries | Coefficients | Ireland: Synthetic unit countries | Coefficients |
|---|---|---|---|
| Canada | 0.05 | Australia | 0.06 |
| Hungary | 0.32 | Canada | 0.24 |
| Norway | 0.63 | Chile | 0.68 |
| **France: Synthetic unit countries** | **Coefficients** | United Kingdom | 0.01 |
| Chile | 0.43 | **Italy: Synthetic unit countries** | **Coefficients** |
| Japan | 0.08 | Chile | 0.11 |
| Mexico | 0.32 | Hungary | 0.02 |
| Norway | 0.13 | Mexico | 0.55 |
| United Kingdom | 0.03 | Norway | 0.32 |
| **Germany: Synthetic unit countries** | **Coefficients** | **Spain: Synthetic unit countries** | **Coefficients** |
| Canada | 0.62 | Canada | 0.58 |
| Sweden | 0.16 | Hungary | 0.09 |
| United Kingdom | 0.22 | Japan | 0.04 |
| **Greece: Synthetic unit countries** | **Coefficients** | Mexico | 0.29 |
| Australia | 0.11 | **Portugal: Synthetic unit countries** | **Coefficients** |
| Chile | 0.54 | Chile | 0.55 |
| Mexico | 0.26 | Mexico | 0.17 |
| Norway | 0.09 | United Kingdom | 0.28 |

## Table A2: Covariates for real and synthetic units

| Finland | Treated (average 1975–1998) | Synthetic (average 1975–1998) |
|---|---|---|
| Employment rate | 0.468 | 0.474 |
| Real GDP (million 2011 USD) | 108,212.100 | 148,324.400 |
| Population (millions) | 4.935 | 7.481 |
| Labour share of GDP | 0.642 | 0.592 |
| Capital stock (over GDP) | 4.905 | 3.608 |
| Share of government consumption | 0.173 | 0.197 |
| Trade openness | 0.713 | 0.706 |
| Population density (people per sq. km) | 16.172 | 44.991 |
| Share of adults completed secondary education | 18.577 | 20.317 |
| Share of adults completed tertiary education | 8.086 | 8.123 |
| Index of capital openness | 1.250 | –0.171 |
| Market Gini Index | 41.200 | 41.485 |

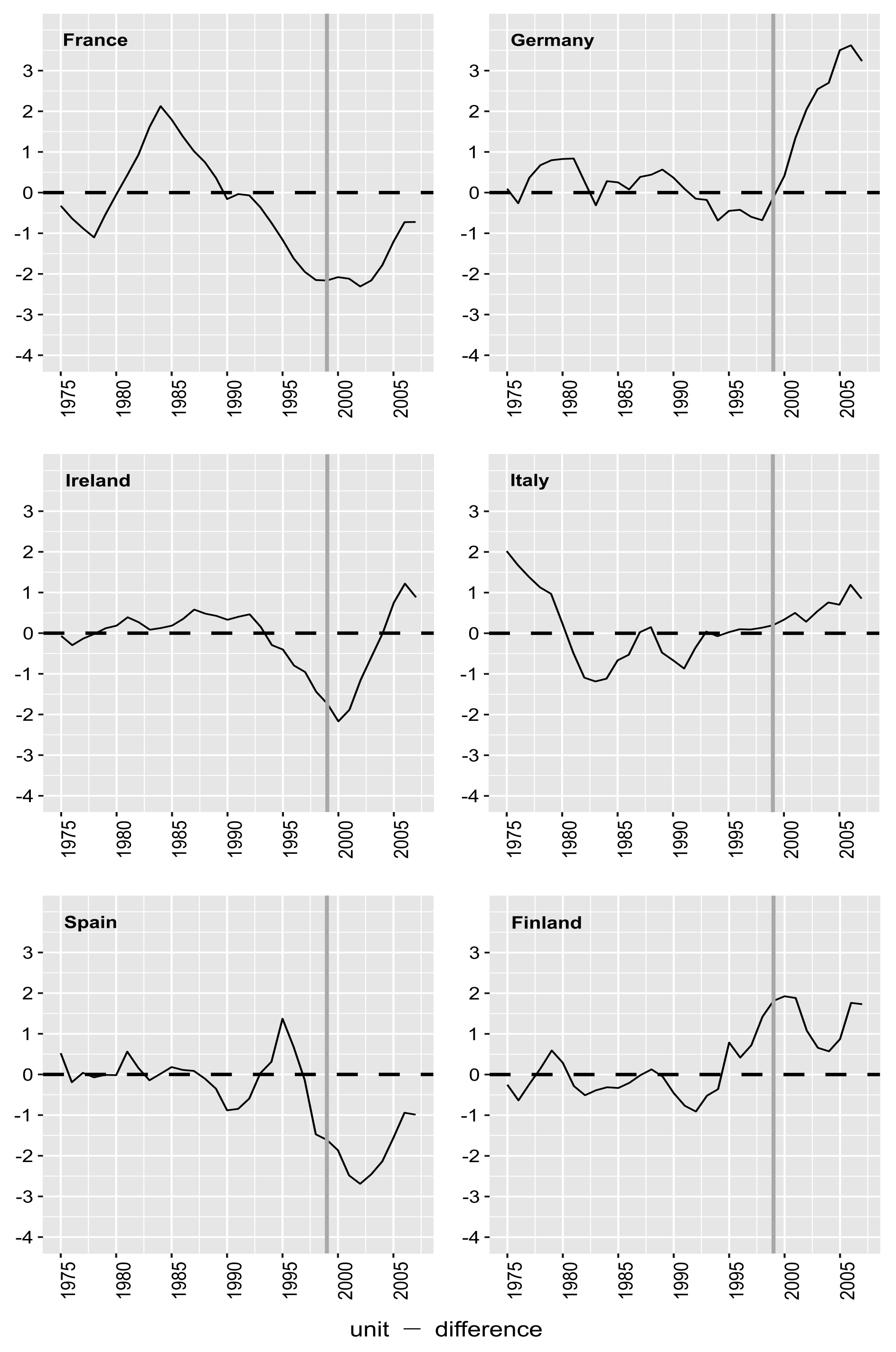
## Table A2: Continued

| France | Treated (average 1975–1998) | Synthetic (average 1975–1998) |
|---|---|---|
| Employment rate | 0.404 | 0.355 |
| Real GDP (million 2011 USD) | 134,835.200 | 629,550.800 |
| Population (millions) | 57.407 | 43.556 |
| Labour share of GDP | 0.658 | 0.483 |
| Capital stock (over GDP) | 4.628 | 3.068 |
| Share of government consumption | 0.175 | 0.165 |
| Trade openness | 0.447 | 0.317 |
| Population density (people per sq. km) | 104.320 | 56.227 |
| Share of adults completed secondary education | 17.090 | 17.003 |
| Share of adults completed tertiary education | 5.715 | 6.435 |
| Index of capital openness | 0.500 | –0.280 |
| Market Gini Index | 47.708 | 47.766 |
| **Germany** | **Treated (average 1975–1998)** | **Synthetic (average 1975–1998)** |
| Employment rate | 0.484 | 0.461 |
| Real GDP (million 2011 USD) | 186,511.300 | 768,551.400 |
| Population (millions) | 79.158 | 30.265 |
| Labour share of GDP | 0.670 | 0.646 |
| Capital stock (over GDP) | 4.448 | 4.258 |
| Share of government consumption | 0.152 | 0.172 |
| Trade openness | 0.574 | 0.575 |
| Population density (people per sq. km) | 227.107 | 56.566 |
| Share of adults completed secondary education | 18.646 | 22.907 |
| Share of adults completed tertiary education | 6.989 | 11.518 |
| Index of capital openness | 2.000 | 1.753 |
| Market Gini Index | 43.275 | 43.168 |
| **Greece** | **Treated (average 1975–1998)** | **Synthetic (average 1975–1998)** |
| Employment rate | 0.390 | 0.346 |
| Real GDP (million 2011 USD) | 170,406.500 | 366,135.000 |
| Population (millions) | 10.185 | 30.193 |
| Labour share of GDP | 0.490 | 0.489 |
| Capital stock (over GDP) | 5.654 | 2.878 |
| Share of government consumption | 0.160 | 0.169 |
| Trade openness | 0.290 | 0.322 |
| Population density (people per sq. km) | 78.248 | 21.472 |
| Share of adults completed secondary education | 21.580 | 21.631 |
| Share of adults completed tertiary education | 9.677 | 7.217 |
| Share of adults completed tertiary education | –0.444 | –0.426 |
| Market Gini Index | 49.007 | 48.832 |

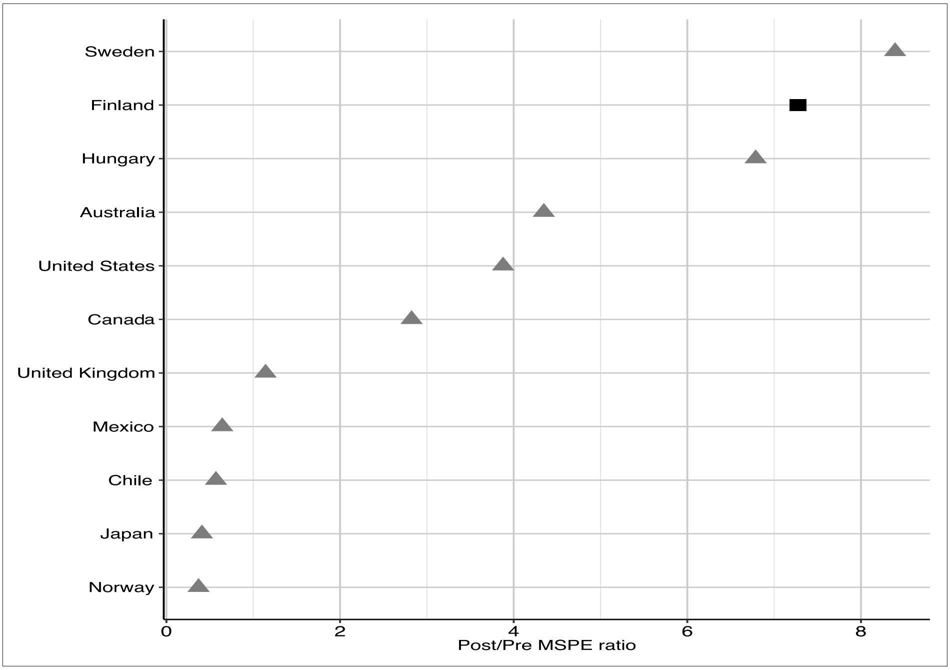
## Table A2: Continued

| **Ireland** | **Treated (average 1975–1998)** | **Synthetic (average 1975–1998)** |
|---|---|---|
| Employment rate | 0.351 | 0.352 |
| Real GDP (million 2011 USD) | 59,618.570 | 285,721.000 |
| Population (millions) | 3.523 | 16.597 |
| Labour share of GDP | 0.554 | 0.543 |
| Capital stock (over GDP) | 4.247 | 2.936 |
| Share of government consumption | 0.186 | 0.196 |
| Trade openness | 1.152 | 0.344 |
| Population density (people per sq. km) | 50.726 | 14.023 |
| Share of adults completed secondary education | 21.301 | 23.851 |
| Share of adults completed tertiary education | 8.684 | 7.990 |
| Share of adults completed tertiary education | 0.375 | –0.432 |
| Market Gini Index | 48.758 | 48.753 |
| **Italy** | **Treated (average 1975–1998)** | **Synthetic (average 1975–1998)** |
| Employment rate | 0.385 | 0.373 |
| Real GDP (million 2011 USD) | 128,140.800 | 554,263.000 |
| Population (millions) | 56.780 | 46.563 |
| Labour share of GDP | 0.573 | 0.475 |
| Capital stock (over GDP) | 4.888 | 3.190 |
| Share of government consumption | 0.142 | 0.137 |
| Trade openness | 0.372 | 0.408 |
| Population density (people per sq. km) | 192.211 | 30.322 |
| Share of adults completed secondary education | 15.901 | 13.783 |
| Share of adults completed tertiary education | 3.586 | 6.147 |
| Share of adults completed tertiary education | –0.042 | 0.130 |
| Market Gini Index | 45.538 | 45.517 |
| **Spain** | **Treated (average 1975–1998)** | **Synthetic (average 1975–1998)** |
| Employment rate | 0.345 | 0.418 |
| Real GDP (million 2011 USD) | 625,320.200 | 825,707.300 |
| Population (millions) | 38.678 | 44.436 |
| Labour share of GDP | 0.640 | 0.608 |
| Capital stock (over GDP) | 4.538 | 3.650 |
| Share of government consumption | 0.135 | 0.145 |
| Trade openness | 0.307 | 0.410 |
| Population density (people per sq. km) | 76.860 | 36.916 |
| Share of adults completed secondary education | 7.542 | 19.282 |
| Share of adults completed tertiary education | 5.792 | 10.005 |
| Share of adults completed tertiary education | 0.333 | 1.181 |
| Market Gini Index | 43.554 | 43.586 |

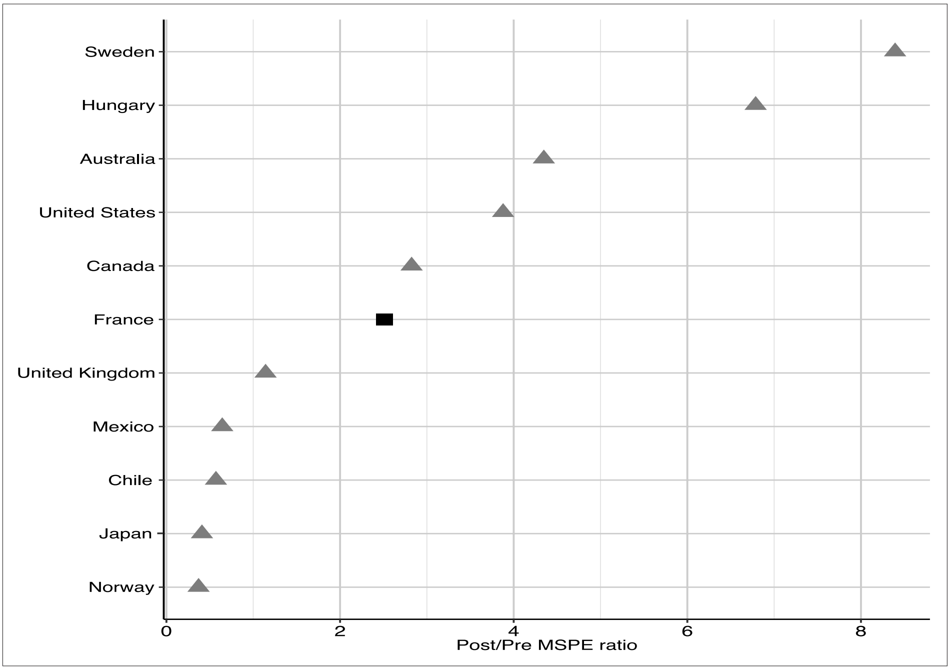
## Figure A1: Gaps plot for baseline estimation



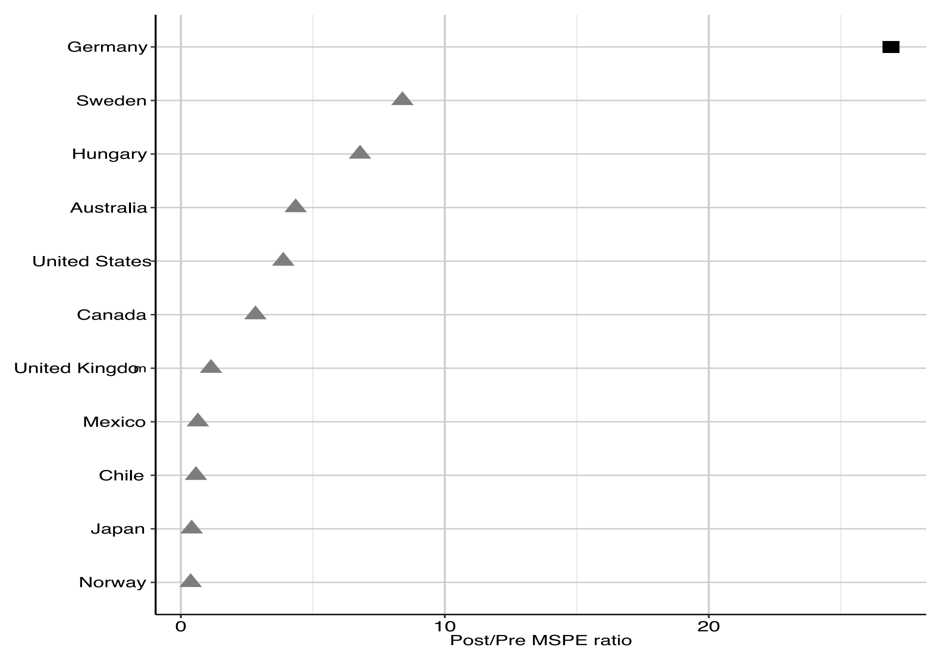
## Figure A2: Placebo plot for Finland



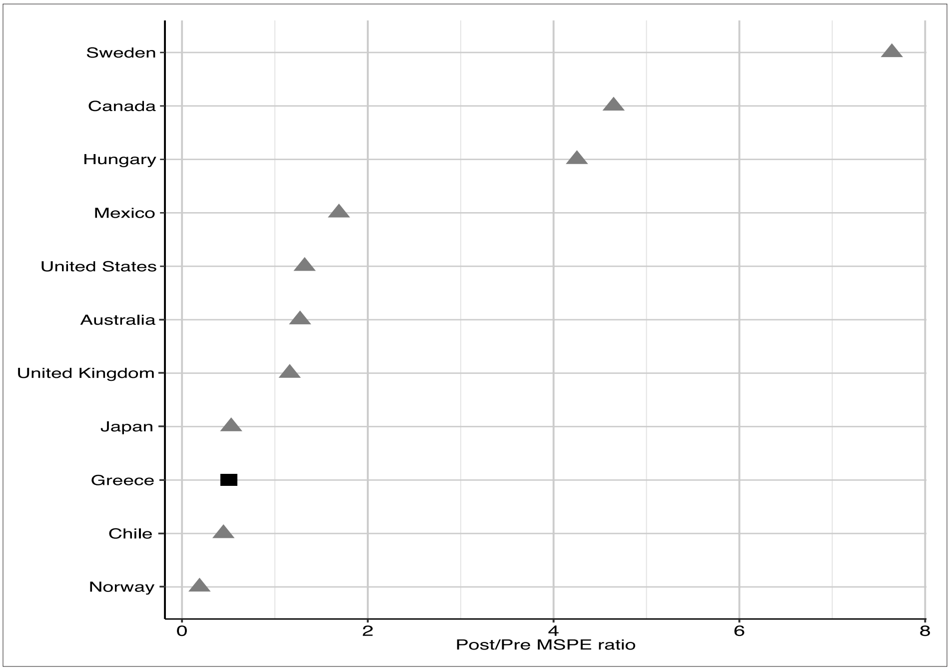

## Figure A3: Placebo plot for France



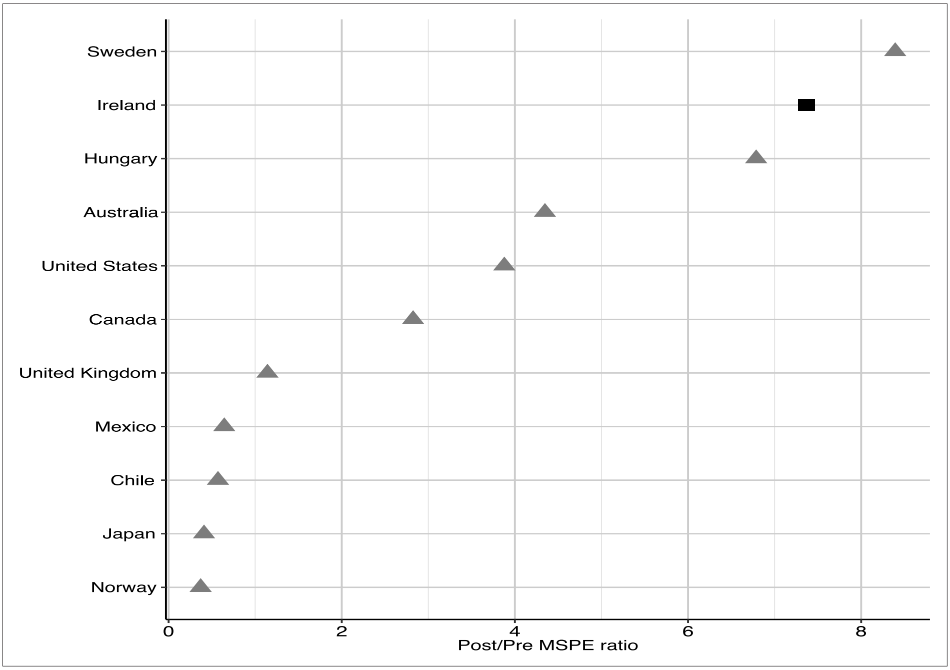
## Figure A4: Placebo plot for Germany



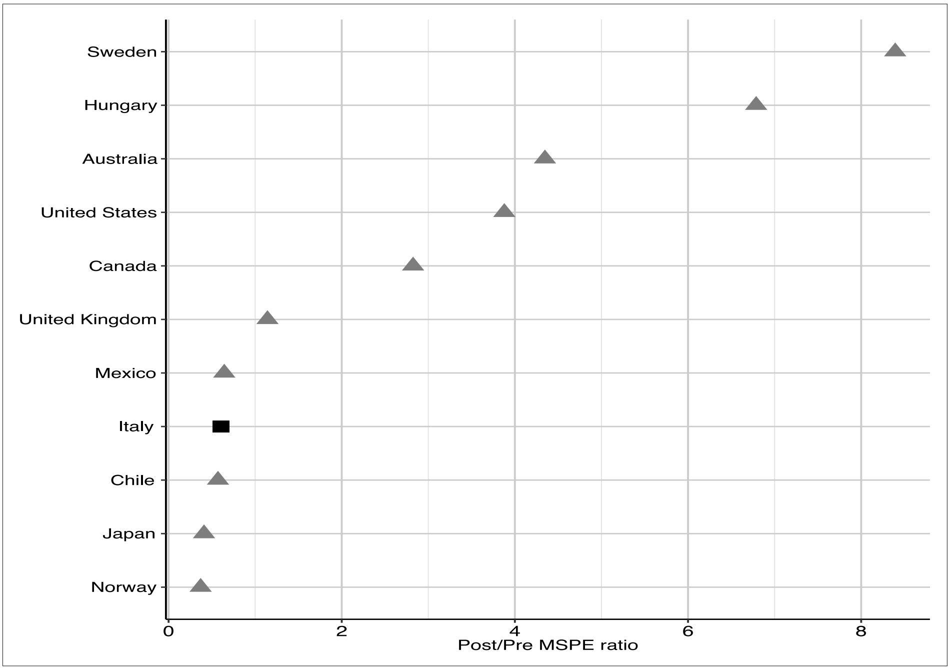

## Figure A5: Placebo plot for Greece



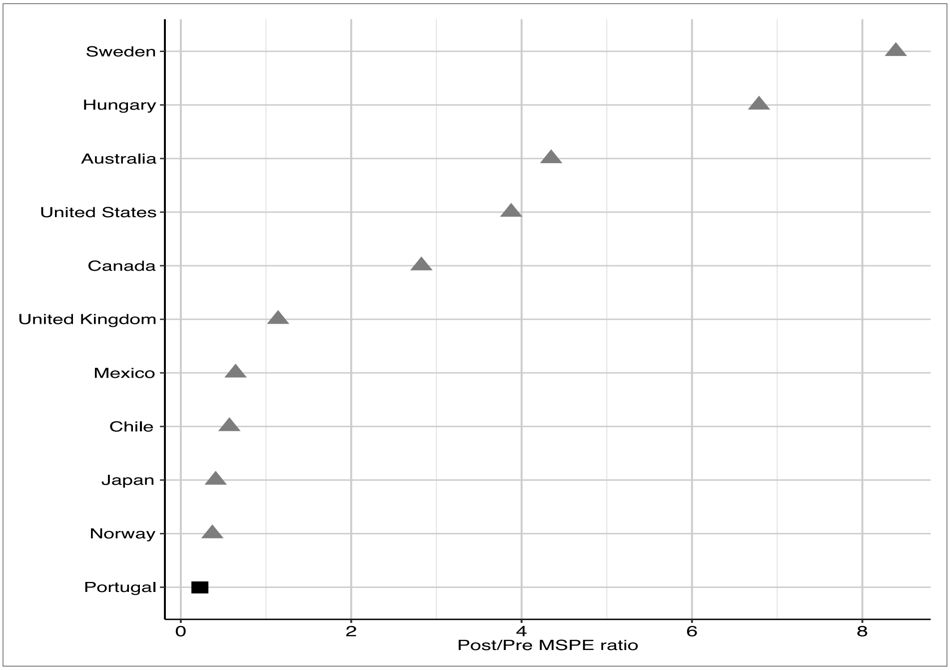
## Figure A6: Placebo plot for Ireland




## Figure A7: Placebo plot for Italy



## Figure A8: Placebo plot for Portugal




## Figure A9: Placebo plot for Spain

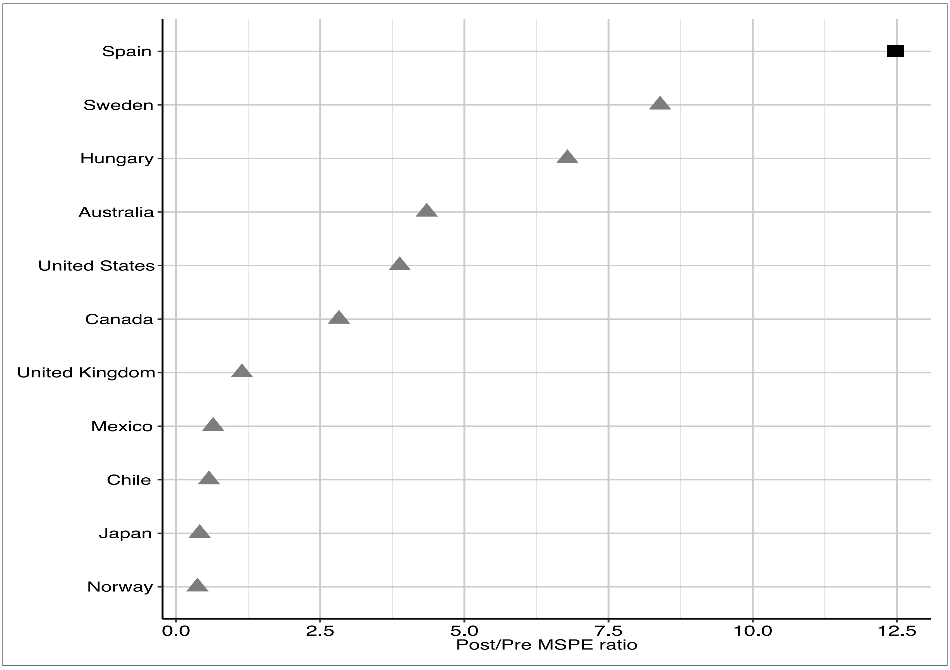
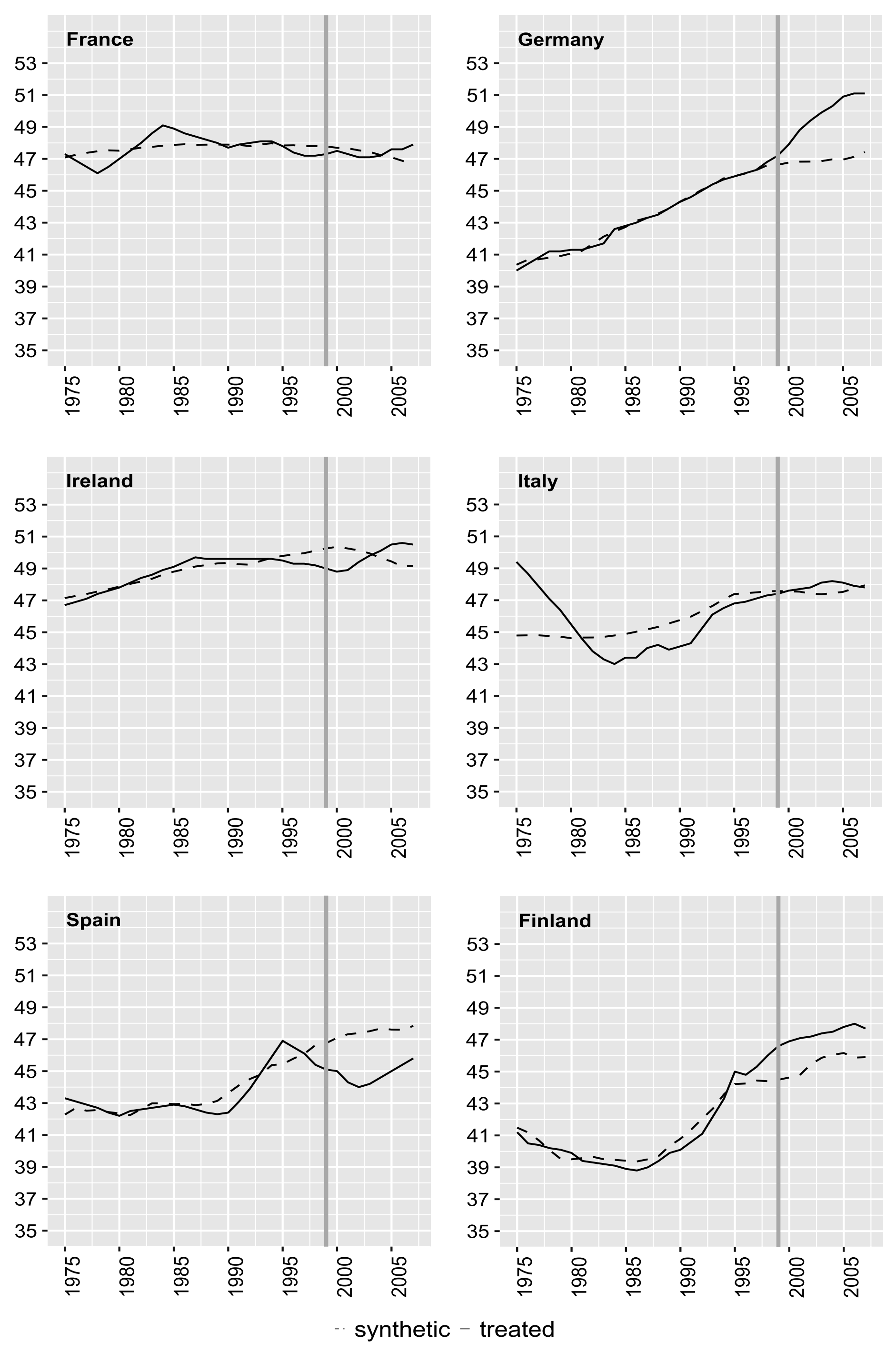


**Figure A10: Path and Gap plots for upper middle-income countries**



## Figure A10: Continued

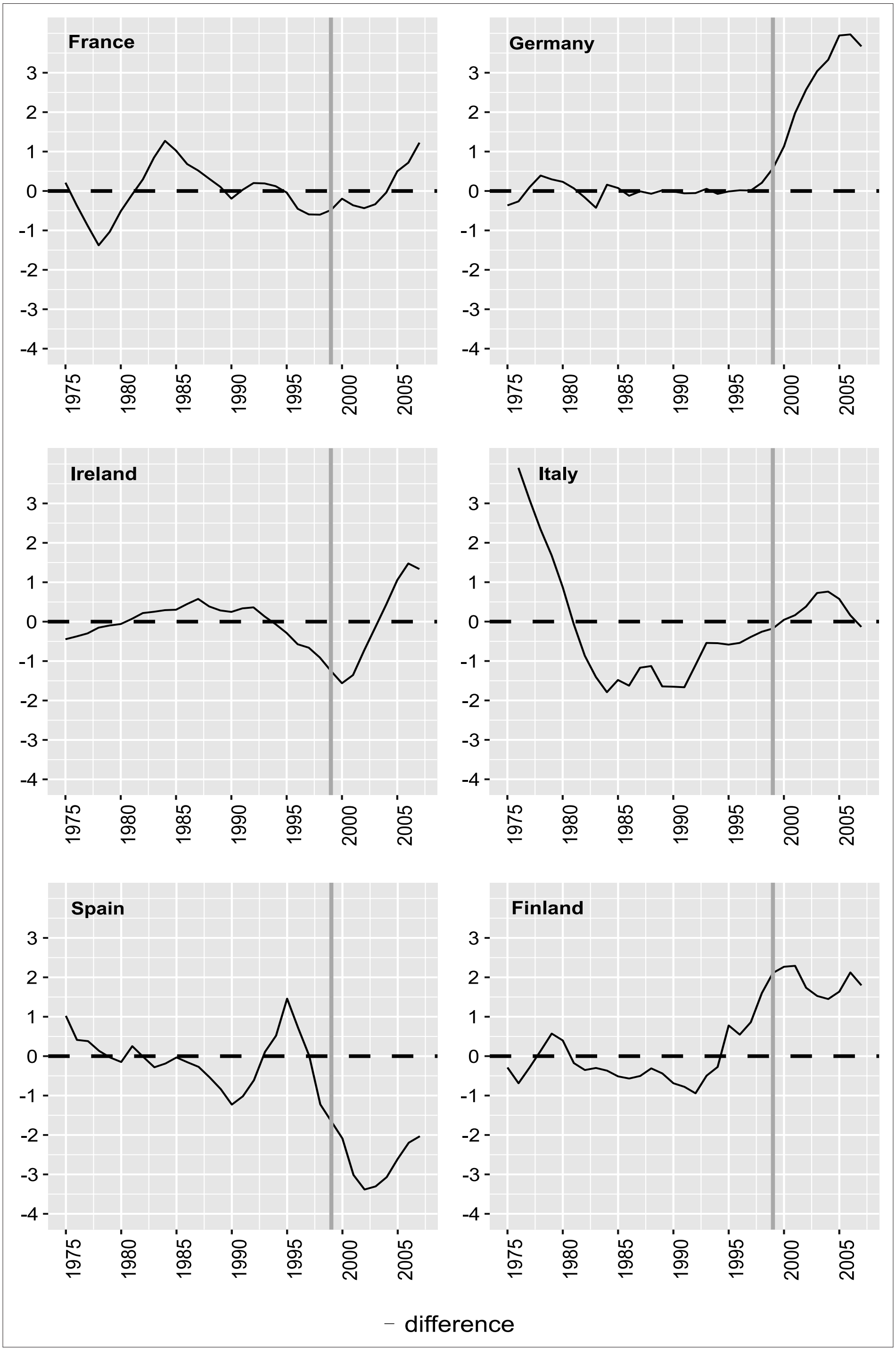


**Figure A11: Comparison of synthetic and augmented synthetic control method gap plots for Germany and Spain**

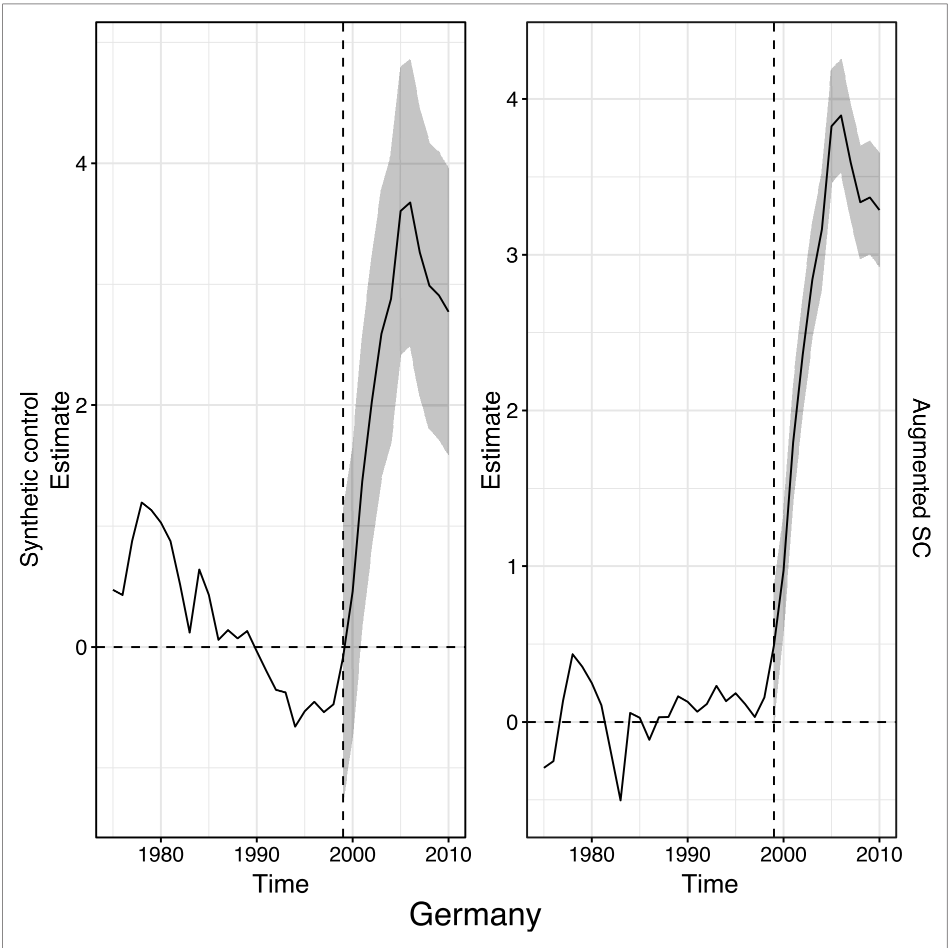


**Figure A11: Continued**

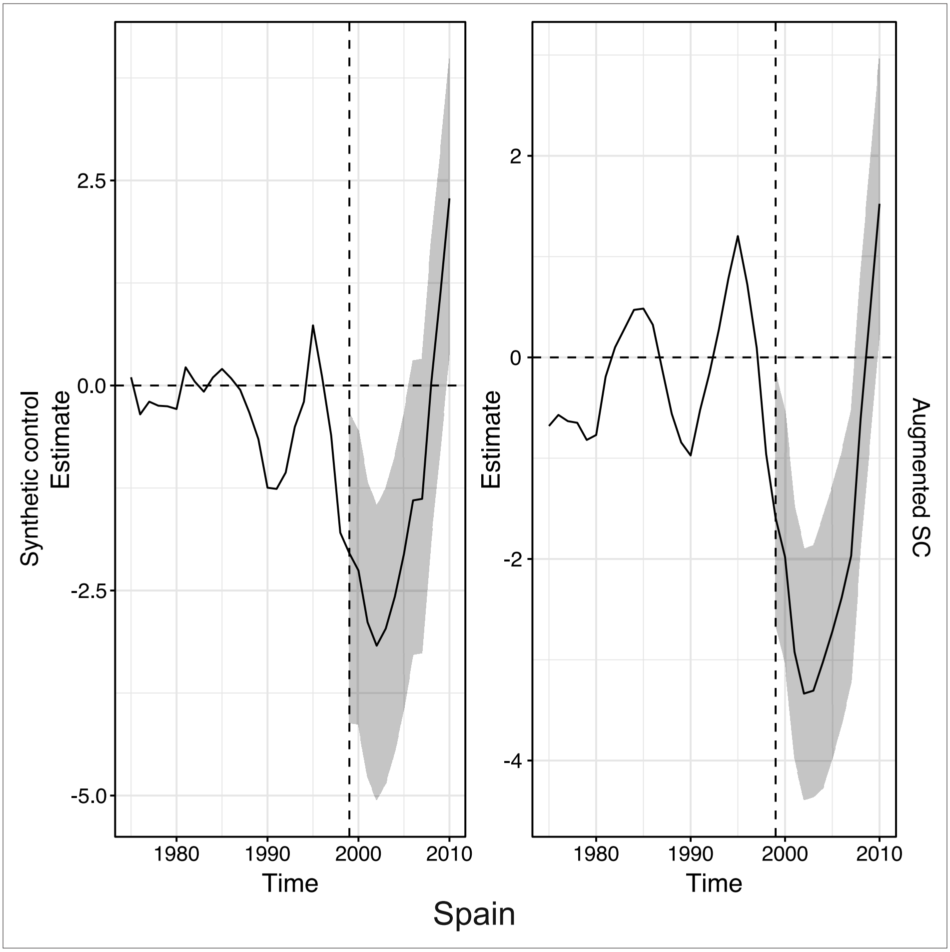


**Figure A12: Covariate sensitivity analysis for Germany and Spain**

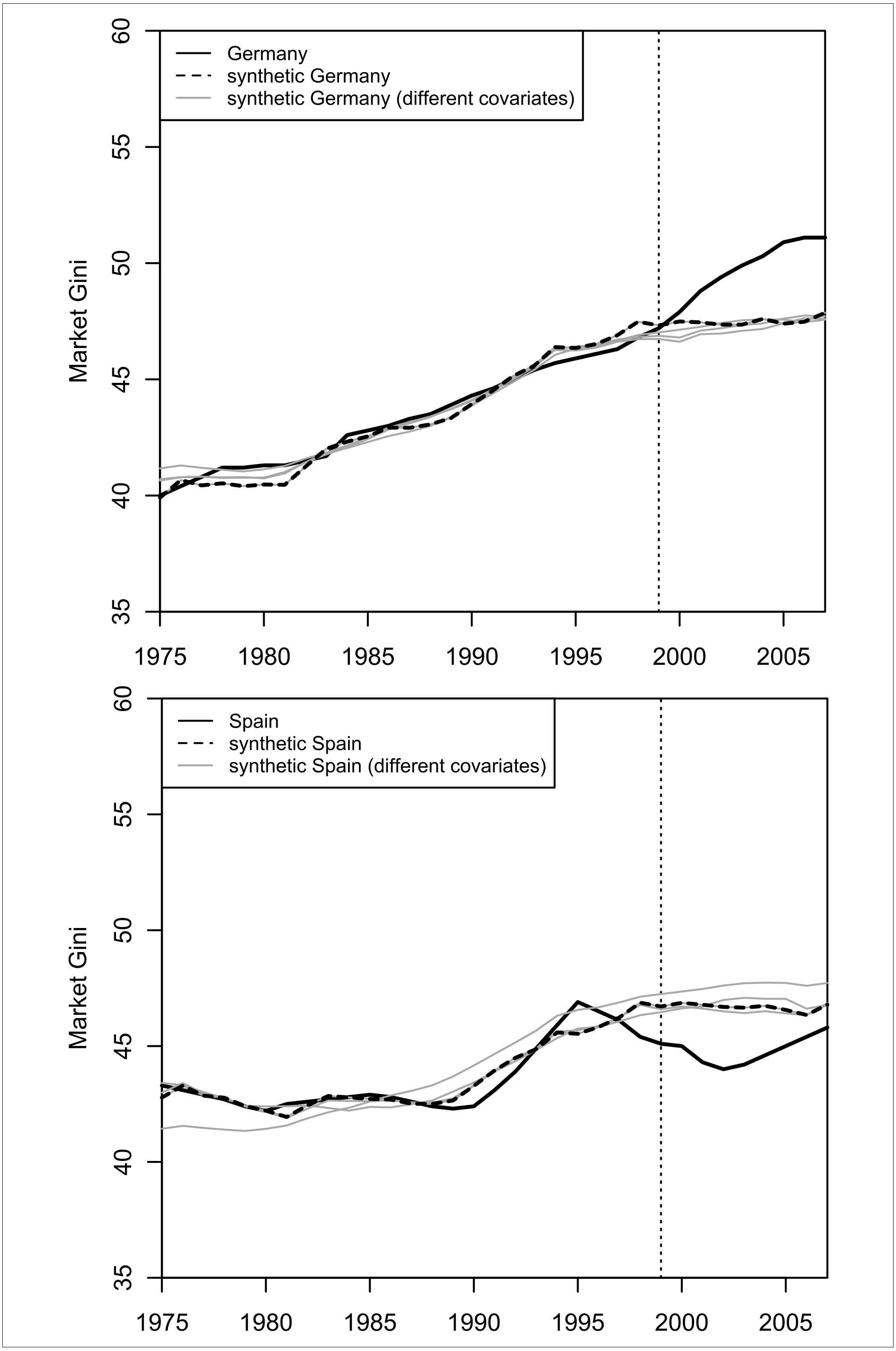
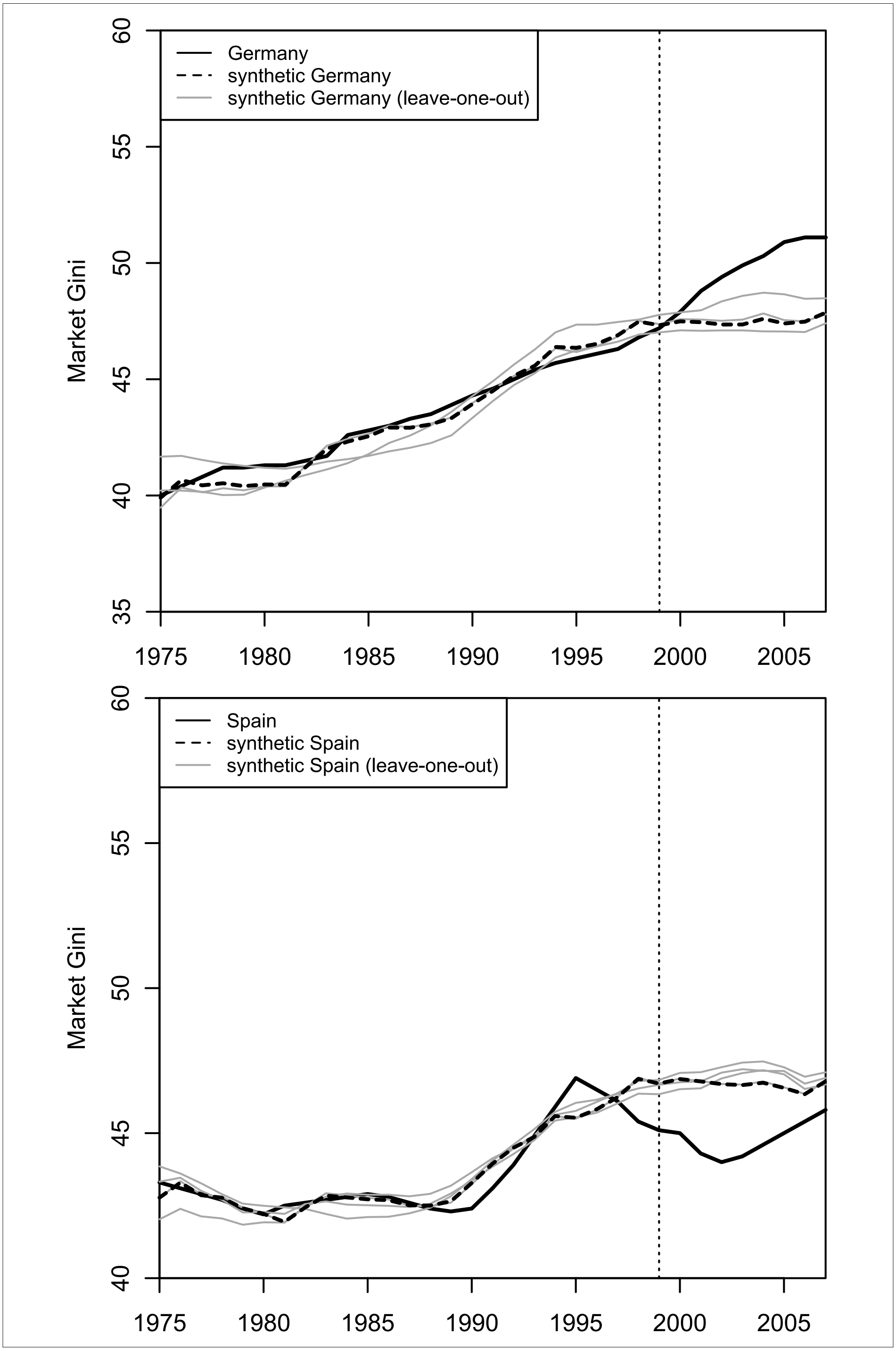


**Figure A13: Donor pool sensitivity analysis for Germany and Spain**



## Figure A14: In-time placebos for Germany and Spain