



Should the UK have joined the Euro while in the EU? New evidence using the synthetic control and synthetic difference-in-differences methods

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Received: 14 January 2025 / Accepted: 18 October 2025
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Abstract

The question of whether the euro promotes development in adopting countries remains open, with the answer likely varying across cases. Taking the UK as an example, this paper investigates whether the UK would have had a higher per capita GDP had it adopted the European currency in 1999. The study utilizes two conceptual frameworks derived from the literature, which offer potentially opposing predictions regarding the euro's impact on economic development. To estimate the causal treatment effect of the UK's policy of not adopting the euro, two contemporary methods for counterfactual analysis are applied: the Synthetic Control and the Synthetic Difference-in-Differences methods. While the results from both methods indicate that the UK would not have benefited from higher per capita income had it joined the euro, neither would it have suffered from a lower per capita income. The findings are cross-validated and demonstrated to be robust to changes in methodology, predictor selection, the donor pool, and the introduction of a placebo treatment year.

Keywords Euro · United Kingdom · Synthetic control method · Synthetic difference-in-differences

JEL codes E58 · F15 · F45 · O10

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1 Introduction

Does introducing the euro enhance economic growth in the adopting countries, and if so, why? Twenty-five years after the introduction of the common currency, there remains no clear answer to this question. Even top economists are divided. For example, the Kent A. Clark Center for Global Markets (2024) surveyed 49 elite economists on the statement: “Europe’s economic growth performance over the last 25 years has been measurably better than it would have been in the absence of the single currency.” The economists did not overwhelmingly support this argument. Although 51% agreed or strongly agreed, some disagreed or even strongly disagreed, and quite a few (20%) were uncertain. The same experts are even more divided on whether the euro makes it more difficult for member states “to respond effectively to country-specific macroeconomic disturbances.”¹

This lack of consensus among the most prestigious experts might arise because “the single currency” can be approached in multiple ways. Some see it primarily as a monetary policy mechanism. For example, Karl Whelan (*ibid.*) argued that “[m]onetary and exchange rate policy are generally neutral over the long-run”; the growth effect, therefore, must be “minor.” A different approach considers the common currency as a deeper level of “economic integration;” for example, in the same survey, Agnès Bénassy-Quéré stated that “there is serious empirical evidence of the positive impact of the single market.” A deeper level of economic integration might have a growth impact, even theoretically (Rivera-Batiz & Romer, 1993). Yet another way to view the euro is as an international financial system. For example, Charles Wyplosz supported this position, stating that it “prevented currency crises.” Additionally, John Vickers stated that an “independent monetary policy has been valuable for the UK.”

Debates on the euro are often seen as a matter of economics versus politics. As a result, criticisms of the Eurozone are often interpreted as politicians not doing what economists tell them to. The economic perspective is said to be overridden by political considerations (De Grauwe, 2013; Sinn, 2018). In contrast, economists’ criticisms of the Eurozone as “flawed” (Wyplosz, 2016) typically lack a clear conceptual framework for defining what is flawed.

Determining whether the euro would have promoted a member state’s economic development had it joined the Eurozone requires a conceptual framework. Therefore, this paper identifies two alternative conceptual frameworks: market-preserving federalism (MPF) and optimum currency areas (OCA). The literature reflects arguments from both perspectives. Both identify some conditions under which a common currency is beneficial, but these conditions do not overlap.

The conditions, however, can vary across countries; therefore, an answer to the question is country-specific. The UK merits a closer look for two reasons. First, what would have happened to economic development in the UK if it had joined the euro remains an open question; the economics literature has not provided a convincing answer. Second, the predictions of the two frameworks we have identified might be different or even in conflict.

¹ Paul de Grauwe, for example, a well-known expert on the EMU, an author of a widely used textbook on it (De Grauwe, 2022), disagreed with the first and agreed with the second proposition.

By contrasting these two theoretical frameworks with their pessimistic views on the long-run growth impact of the euro for the UK, this paper aims to provide new estimates of the effect of the UK's policy of not adopting the euro. In this endeavour, we apply two recent powerful methods in counterfactual analysis: The first is the Synthetic Control Method (SCM) developed by Abadie and Gardeazabal (2003) and then further developed by Abadie et al. (2010, 2015), which endogenously constructs the counterfactual from data based on an optimisation method.

The synthetic control estimator is well-suited for contexts where the common trends assumption of the additive fixed effects model does not hold, offering a data-driven approach. While we present the SCM as an improvement over the methods summarized in Sect. 3, it has inherent limitations, which we discuss in Sect. 4. To address these limitations, we also employ the more recent Synthetic Difference-in-Differences (SDID) method proposed by Arkhangelsky et al. (2021). The SDID combines the advantages of the SCM and the traditional Difference-in-Differences (DID) method, and it also serves as a crucial robustness check. The final results, which are cross-validated and demonstrated to be robust to changes in methodology, predictor selection, the donor pool, and the introduction of a placebo treatment year, indicate that the UK would neither have gained nor lost in terms of per capita income had it joined the euro.

The findings of the paper are significant for two reasons. First, we broaden the theoretical framework for assessing the euro's impact by distinguishing between MPF and OCA approaches whereas existing literature (see Sect. 3) almost exclusively relies on the OCA when it comes to empirical research on the UK case. Second, due to more appropriate new econometric methods, we provide an accurate estimation of the causal treatment effect of the UK's policy of not adopting the euro.

What makes our theoretical argument and empirical estimation particularly interesting is the existence of findings that appear controversial from the UK's perspective. The adoption of the euro has not been shown to have a positive effect on economic growth in countries that adopted it, especially the larger ones.² By contrast, the UK, which did not join the common currency, has also been shown to have suffered from this decision in several respects. For instance, in terms of trade and FDI inflows, the effect of non-adoption appears to be negative. Saia (2017), for example, found a negative impact on trade – even with countries outside the Eurozone – while Sanso-Navarro's (2011) results show a similar effect on FDI inflows from the US. The puzzle, then, is that while other countries did not benefit from joining the euro in terms of GDP per capita, the UK did not benefit from staying out in terms of trade and FDI inflows. Does this imply that the UK would have benefited from joining despite the experience of other countries? This is the question our paper seeks to explore.

² Gabriel and Pessoa (2024) conclude that, on average, adoption of the euro did not alter the trajectory of real GDP in the countries that joined the Eurozone, but it had a negative effect on larger economies such as Germany and France. Similarly, the findings of Puzzello and Gomis-Porqueras (2018) show heterogeneous effects among early adopters: Belgium, France, Germany, and Italy would have had higher GDP per capita had they not adopted the Euro, while Ireland would have experienced lower per capita income and the Netherlands would have been roughly unaffected. Moreover, Cerqua et al. (2022) found no consistent positive impact of the euro across nine regions in five Eastern European late-adopter countries.

The rest of the paper is structured as follows. Section 2 summarizes the two approaches to the euro and their predictions for the UK. In Sect. 3, we present the SCM and the SDID method. Section 4 presents the model, data and the results, together with the statistical inferences and robustness checks for the SCM including the SDID analysis. Section 6 concludes.

2 Approaches to the Euro

Which theories suggest that a common currency would affect economic growth in countries that adopt it, and what would these theories predict for a euro-adopting UK? As indicated in the introduction, two major conceptual frameworks are commonly used to analyse the effect of a common currency on economic growth. One is the theory of Optimum Currency Areas (OCA), which economists rely on almost exclusively when addressing this question.³ The alternative perspective – explaining why the euro might promote growth by encouraging compliance with fiscal and monetary rules set out in the EU’s official documents – is the Market-Preserving Federalism (MPF) approach. This framework views the Eurozone as a potential example of market-preserving federalism. These two approaches – OCA and MPF – are not mutually exclusive, but they may lead to different conclusions on whether a common currency benefits a member state such as the UK.

2.1 The common currency in the Market-preserving federalism framework

When considering theoretical frameworks that align with the formal fiscal and monetary institutions of the Eurozone, the MPF model (Weingast, 1995) emerges as a natural choice. First, several authors (McKinnon, 1997; Riet, 2015) explicitly analyse the Eurozone from this point of view. Second, the MPF framework helps organize ideas about the common currency that contrast with those of the OCA theory – ideas that are often inconsistently labeled as “ordoliberal” (Brunnermeier et al., 2016, pp. 62–69), “Hayekian” (Selgin, 2013), or even “monetarist” (De Grauwe, 2013).

To view the common currency as an element of the MPF framework is to emphasize the importance of preserving a free common market. Federalism has long been regarded as a safeguard of economic freedom, based on the idea that “ambition counteracts ambition” (Publius, 2008/1788), thereby enhancing the autonomy of member states. As Hayek (1948/1939, p. 256) noted, in such an ideal “interstate” federation, “the latitude given to the national central banks will be restricted.”

Although a common currency imposes restrictions on national central banks, it is not sufficient to meet the requirements of the MPF framework. Weingast (1995) identified the institutional conditions necessary for federal systems to sustain economic freedom in a self-enforcing manner. In addition to the two obvious features – a hierarchy of governments and the autonomy of each level – he outlined three further

³ As Jonung and Drea (2010) show, just before and during the early years of monetary integration in Europe, the OCA theory was almost exclusively the only lens that US economists used to examine the Euro.

conditions. One of these is that “lower governments face a hard budget constraint, that is, they have neither the ability to print money nor access to unlimited credit” (Weingast, 1995, p. 4; italics omitted). A common currency alone cannot guarantee this constraint; the condition is “not met if the central government bails out the lower one whenever the latter faces fiscal problems” (ibid.).

Competition among lower levels of government would be less effective without a hard budget constraint. If neither other governments nor the central government could credibly commit to bailing out local governments, sustaining a system of rent-seeking would become prohibitively costly for them (Weingast, 1995, pp. 5–6). McKinnon (1997, pp. 1575–1576), for example, argued that in “Europe before the monetary unification,” national governments, supported by their central banks, were able to finance inefficient industries to a greater extent than U.S. states. In light of Hayek’s (1990/1976) argument, this phenomenon also stems from the lack of currency competition within Europe. However, to sustain an MPF system, the federal (central) government’s budget constraint must also be hard; otherwise, it “can easily assume expenditure obligations that would normally fall to the states or localities” (McKinnon, 1997, p. 1578).

According to the MPF approach, therefore, a common currency does not necessarily strengthen institutional competition between governments, nor does it automatically lead to a higher level of economic freedom and faster economic growth. Under certain conditions, however, a common currency may serve as a more credible commitment to a rule-based monetary policy than a simple currency peg – particularly if exiting the Eurozone is more difficult than abandoning a peg.

Are such conditions endogenous? Does joining the Euro incentivise governments to comply with the rules of fiscal constraint such as those in the Maastricht Treaty (TFEU, 2020, Arts. 123–127, 282), the Stability and Growth Pact (Council Regulation (EC) No. 1466/97, 1997), or the Fiscal Compact (Treaty on Stability, 2012)? At the outset of the Euro project, it appeared that even the countries that did not meet the Maastricht criteria were converging towards them, and “even the least disciplined among the Union’s members had implemented rigorous anti-inflationary and fiscal reform policies” (Padoan, 2004, p. 96). Critiques by several economists of the Eurozone’s architecture suggest otherwise, however. Even prior to the launch of the euro, many economists had expressed concerns about the credibility of the fiscal rules (Jonung & Drea, 2010, pp. 14–20; McKinnon, 1995, p. 471), and, as Spolaore (2013, p. 137) concluded in retrospect, they have never been credible.⁴

That is why, as many argue, there has been a drift from the original architecture towards one in which member states and the central bank face constraints that are softer than necessary. Those who view institutional competition as the cornerstone of the “economic constitution of the European Community” (Streit & Mussler, 1994) have been sceptical about the integration process at least since the Maastricht Treaty. Their concern is that the process has been dominated by “integration by intervention”

⁴ The idea that budget constraints became softer for governments after the introduction of the euro is also supported by Bouvet (2021). She finds that inequality of net incomes would have been higher without the euro in several countries, and attributes it to the more generous welfare programmes that lower interest rates made possible for governments in “peripheral countries”.

rather than “integration by framework activities,” shifting the vision of a “common market as a self-organising system” towards a rent-seeking community marked by “redistributive struggles” (ibid., pp. 329–330, 338, 346). In this process, the common currency has had the “effect of ‘entrenching the source and root of all monetary evil, the government monopoly of the issue and control of money’” (Hayek, 1990/1976, p. 29), rather than serving as a mechanism of intergovernmental competition.

Vaubel (2004) argued that the EMU’s monetary policy is slowly converging to that preferred by the median voter in the Governing Council of the ECB, while Blankart (2013) constructed a model of why countries with different fiscal regimes cannot benefit (in the long run) from the common currency and why the common monetary policy will be more inflationary than stable. Different fiscal regimes combined with a lack of credible fiscal rules may have contributed to the lowering of interest rates in the “periphery” of the Eurozone. Alongside weak financial institutions, this led to a loosening of budget constraints for governments in Spain, Portugal, Greece, and Ireland (Fernández-Villaverde et al., 2013), and delayed institutional reform. The German government, by contrast, facing capital outflows and no reduction in interest rates, found it necessary to liberalise its labour market institutions.

Government budget constraints have continued to weaken since the financial crisis. Sinn (2018, pp. 1406–1409) and Magnin and Nenovsky (2024) demonstrated this trend by showing how the ECB has become increasingly involved in fiscal policy since 2008. Simultaneously, as Riet (2015, pp. 116–125) documented, member states have sought to “round the corners” of the trilemmas created by the EMU through various policies that are either explicitly or implicitly inconsistent with the principles of an MPF. These include “fiscal devaluation,” protectionism through domestic subsidies, and pressuring the financial sector to purchase government bonds. During the same period, ECB officials have also ceased emphasizing the importance of “structural reforms” (Braun et al., 2024), possibly as a consequence of weakened institutional competition.

According to the MPF framework, the conditions required for a common currency to benefit member states are institutional – if not cultural – and exogenous. That is, the common currency does not itself incentivise member states to fulfil MPF requirements.

2.2 The common currency in the optimal currency areas framework

Contrary to the views of the MPF framework, the theory of Optimum Currency Areas (OCA) does not view the integrated market as being threatened by discretionary monetary policy. Instead, it regards a common currency primarily as a policy tool for economic stabilisation. More effective stabilisation policies can lead to faster growth and improved policy outcomes – particularly when growth is driven by internal demand or exports (Faresse et al., 2023, p. 216). The OCA theory emphasises the benefits of a common currency, such as reduced transaction and information costs, lower exchange rate risks, and increasing returns to scale on money (Wyplosz, 2016, pp. 571, 580–581). These benefits are typically taken as given. The core question, however, concerns the conditions under which a common currency (or fixed exchange rate) makes stabilisation policy more costly than it would be for a country that issues

its own currency or maintains a soft peg or flexible exchange rate. As Horváth's (2003) review of the theory illustrates, OCA theory focuses on whether monetary union membership increases or decreases a country's ability to adjust to shocks that generate balance of payments imbalances.

Horváth (2003) added the degree of openness and product diversification to the classic criterion of labour mobility originally identified by Mundell (1961). However, it remains unclear whether higher or lower levels of these factors make participation in a monetary union more advantageous. In addition, policy-related criteria – reflecting the diversity of member states' preferences regarding stabilisation policies (Padoan, 2004) – may also need to be considered. These include “similarity of rates of inflation, degree of price and wage flexibility, and the real exchange rate variability” (Horváth, 2003, p. 17). Another important criterion is the extent to which a country's government is committed to maintaining a fixed exchange rate.

It is not evident, however, that such policy criteria should be regarded as part of the OCA theory. De Grauwe (2013) emphasized the absence of “divergent trends” (asymmetric shocks), along with flexibility in goods and labour markets, as two interacting conditions. He also highlighted the existence of a budgetary union as a factor that can make a monetary union optimal, even if the other two conditions are not met. De Grauwe further argued that economic policy criteria – such as the convergence criteria – were not derived from OCA theory, but rather emerged from the political compromises necessary to gain public support in Germany for replacing the Deutschmark with the euro. In contrast to the MPF approach, interpreting OCA theory in a narrower sense – as focused on the conditions under which monetary policy becomes more effective – may be more analytically useful.

The condition requiring the absence of asymmetric shocks may be endogenous. Frankel and Rose's (1998) influential study showed that the more intensive the trade between two countries, the more strongly their business cycles tend to correlate. That is, if a common currency increases trade (Mayer et al., 2019), and if the nature of the business cycles is sufficiently similar, then the most important criterion may indeed be endogenous.

2.3 Contrasting the two approaches and their predictions for the UK

Although both theories support the idea that a monetary union – under certain conditions – can promote economic growth in member states, specific features of the Eurozone give rise to scepticism from both perspectives. From the MPF viewpoint, one may be sceptical due to the lack of credibility surrounding the fiscal rules; from the OCA perspective, scepticism arises from the implicit neglect of OCA criteria in the Eurozone's institutional design. A general scepticism about the Eurozone as a whole does not necessarily imply the same for the UK's potential accession – however – or does it? We argue that it does.

To illustrate how predictions might differ for the UK, Table 1 contrasts the two views of the common currency across five aspects, each represented by a row in the table. First, the two approaches identify different reasons why a common currency might promote growth. The MPF approach regards the common currency as part of an institutional framework that prevents governments from encroaching on economic

Table 1 The common currency in MPF and OCA

	MPF	OCA
source of economic growth	economic freedom through competition among governments	scale effects through economic integration
conditions to be met	similarity of the fiscal regimes limited government rule-following policies	lack of asymmetric shocks (divergent trends) small, open economies transfer union
conditions endogenous?	no	partially yes
costs	incredibility of the fiscal constraints	constraints on national stabilisation policies
key idea	institutional and policy competition between member states	central banks conducting stabilisation policies and crisis management

freedom. In contrast, the OCA approach views it as the lubricant facilitating international trade and the associated economies of scale. Second, the conditions deemed necessary for growth differ between the two frameworks. The OCA model focuses on economic rather than political-economic conditions, thereby excluding government incentives from its analysis. Third, it is easier to conceive of the OCA criteria as endogenous, whereas, as argued above, the policy criteria of the MPF approach are not endogenous. Fourth, constraints on governments are seen as a benefit in MPF theory but as a cost according to OCA⁵. The inverse is also true: a common currency constrains national governments and central banks from responding as they see fit – this is considered a cost in the OCA approach but a benefit in the MPF framework. This fourth difference leads to a fifth, which we term the key ideas underlying each theory. The MPF theory lacks the concept of stabilisation policy, while the OCA theory omits the idea of institutional competition, including competition between currencies. These two concepts, therefore, constitute the core distinctions between the two approaches.

These conditions might not be contradictory: open economies with perfectly synchronized business cycles might simultaneously maintain hard budget constraints and free markets. However, the two frameworks can easily yield divergent conclusions. When local or central governments exceed the constraints prescribed by MPF theory, they may be acting in exactly the way the OCA framework recommends – to stabilize output and prevent unemployment or deflation.

From the perspective of the MPF, the question is whether potential Eurozone countries need the euro to establish an MPF, and whether the UK would have been closer to such federalism had it adopted the euro in the past. The answer appears to be negative. The first reason is the institutional drift described in Sect. 2.1. With the hard budget constraints of member states and the ECB slowly deteriorating, institutional

⁵ The conflict between this aspect of the two approaches, which in a crisis situation intensifies, is one of the reasons why the debate about the euro is often seen as a “battle of economic ideas” (Brunnermeier et al., 2016) or even as a “German-French difference” (ibid., pp. 58–84).

competition cannot be expected to incentivise governments to reform their institutions and policies to increase productivity – even with a common currency.

Second, the fiscal institutions of the member states must be compatible (Blankart, 2013, 2014) and maintain a balanced budget that supports low inflation. Britain established a centralised fiscal regime much earlier than France (Dincecco, 2009); thus, it is questionable whether British fiscal institutions are more compatible with those of Germany than with those of France.⁶ Brunnermeier et al. (2016, p. 252) stated that the “Anglo–American vision” is “aligned with the French view” because both endorse using monetary and fiscal policies to manage crises and stabilise the economy. However, such a belief is not part of the MPF approach, as indicated in Table 1. Therefore, prevailing economic beliefs in Britain might not support a European MPF. Indeed, it has always been difficult for the British government to accept the fiscal and monetary constraints enforced by the “federal” authorities in continental Europe, as shown by its shaky relationship with the different European fixed exchange rate regimes in the past (Farese et al., 2023). Yet, the lack of an enforcement mechanism to punish violations of the Maastricht criteria is a cost for member states (Gyoerk, 2017), not only because of the risk that the UK might be required to contribute to bailing out fellow EMU members if they partially default on debts.

Economic freedom – the primary growth-enhancing mechanism in the MPF framework – has traditionally been at least as high in the UK as in the Eurozone. This is even more evident when the UK is compared with Eurozone countries of similar size, such as France, Germany, or Italy. Among these three, the UK’s level of economic freedom is closest to that of Germany (Gwartney et al., 2024). Accordingly, the UK’s experience within the Eurozone might have most closely resembled that of Germany.

Public anti-EU sentiment is more intense in Britain than in any other country in the EU. Gancia et al. (2020) explained this by the fact that the UK is relatively large; therefore, an increase in the depth of the EU will hurt its import industry more than benefit its consumers. However, as predicted by Streit and Mussler (1994), such distributional concerns lie at the heart of debates within the EMU and are likely to remain so (Streeck & Elsässer, 2016). That is, the common currency may intensify both cross- and within-country pressures for redistribution, making it compliance with the fiscal constraints that the MPF framework requires even less likely.

From the perspective of the OCA, the conclusions are less obviously negative. A detailed economic cost–benefit analysis has been reported in the framework of five tests conducted by Treasury (2003), which reached a negative conclusion.⁷ The verdict of the H.M. Treasury relied on the view that the OCA conditions do not hold for the UK, which, is a very much researched topic with ambiguous empirical results.⁸

⁶ Some raw numbers of fiscal policy confirm this: the average primary balance of the central budget over the period 1999–2022 was –1.99% of GDP for Britain, –1.66 for France, and +0.788 for Germany. General government gross debt in Britain was 100.4% of GDP in Britain, 111.8% in France, and 66.1% in Germany for 2022. Their average yearly inflation rates from 1980 to 1999 were 4.66, 5.14, and 2.66% (IMF, 2024a, b, c).

⁷ Note that Artis (2006) argued that despite the economic analysis in the five tests, “joining or not joining” has been a highly political issue in nature.

⁸ For an affirmative result, see Lee and Saucier (2011) who evidenced that, on the one hand, the UK did not display macroeconomic asymmetries, and on the other, the exchange rate correlations between the

The most critical cost of a common currency in the OCA approach is associated with the loss of the ability to use monetary policy to accommodate external shocks, smooth business cycles, and increase competitiveness by devaluing the currency (De Grauwe, 2022, pp. 3–10), especially at times of (financial) crisis (Minford, 2008).⁹ Furthermore, macroeconomic cyclical instability would have increased due to the loss of independent monetary policy, as Minford et al. (2004) argued. According to their estimation, the “boom-and-boost” factor (i.e., the variability of the economy) would have been about twice as great for the UK.

In the OCA approach, the benefits arise from integration reducing various types of international transaction costs associated with trading goods and assets. Minford (2004) argued that, first, for a country with an advanced banking system such as the UK, the benefits derived from lower transaction costs due to using the same currency appear to be much smaller than for other countries (only 0.1% of GDP as opposed to 0.4%). Moreover, the one-off currency conversion cost would likely offset this benefit.

Second, reducing exchange rate risks against the euro could increase trade volume and FDI flows with Eurozone members (Micco et al., 2003; Minford, 2004; Layard et al., 2002; Sanso-Navarro, 2011). However, scholars do not agree on whether this potential gain would materialise in practice for the UK. For instance, Micco et al. (2003) estimated significant benefits from EMU membership regarding increased trade and a positive spillover effect that boosts trade among both members and non-members. Layard et al. (2002) argued that participants must adopt a single currency to reap these benefits, as do the states in the US. In contrast, Minford (2008) offered the opposing view that since the UK is unique in that its trade with the US (in dollars) is much higher than other EU countries, the benefit from reduced exchange rate risk would be less crucial for the UK, meaning joining the euro would not eliminate exchange rate risk. Saia (2017), however, found that the UK experienced a significant loss as a result of its decision not to join the euro, whereas Mayer et al. (2019) attributed trade effects to EU membership generally, but not specifically to the euro.

The third potential gain could arise from the transparency of price comparisons in border areas, which, according to Minford (2004), would not be relevant for the UK, as it is an island and thus lacks “border people” who could be “constantly involved in price comparison.”¹⁰ Additionally, harmonising taxes, social support, and regulations – expected to emerge within the EMU – would also impose costs: these measures work against flexible labour markets, reducing labour competitiveness and ultimately output.¹¹

Pound Sterling and the German and French currencies were high enough. Both findings point to the argument that the OCA conditions hold.

⁹ Of course, if the shocks are similar in each country of the monetary union, the common monetary policy would be sufficient, but if shocks treat the members differently, the lack of monetary policy at the national level would be crucial (Gyoerk, 2017).

¹⁰ We thank one of our anonymous reviewers for rightly pointing out that price comparisons must matter for long-distance trade. That said, the effect of a common currency on whether individuals act on price comparisons appears to be more relevant for local non-tradeable goods than for tradeable ones.

¹¹ Note that Layard et al. (2002) did not see this risk because, in their opinion, joining the euro would not imply tax harmonization.

The 2008 financial crisis provided another compelling and sufficient argument for the UK to join the EMU and adopt the euro as soon as technically feasible (Buiter, 2008). At that time, immediate UK membership in the Eurozone was dictated by both the conventional OCA criteria (convergence, flexibility, labour mobility, fiscal flexibility) and a new financial stability criterion. Without Eurozone membership, the UK is more vulnerable to a triple financial crisis – a banking, currency and sovereign debt crisis – because it belongs to a group of countries characterized by the inconsistent “quartet”: (1) a small country with (2) a large internationally exposed banking sector, (3) a currency that is not a global reserve currency, and (4) limited fiscal capacity relative to the possible size of the banking sector solvency gap.

Based on the net balance between costs and benefits, scholars have argued either in favor of or against Britain’s joining the euro, as illustrated by two prominent papers: Buiter (2008) in favor and Minford (2004) against.¹² Scholars are also divided over whether they think that OCA conditions hold (Buiter, 2008) or not (e.g., Hallett, 2002), especially regarding the criterion of symmetric business cycles (De Haan et al., 2008; Koopman & Azevedo, 2008) and financial stabilisation (Giavazzi & Wyplosz, 2015; Wyplosz, 2016). The correlation between UK and Eurozone business cycles (in industrial production) has been somewhat unstable, but it increased after 1999: it rose from 0.37 in the period 1991–1997 to 0.77 for 1999–2007 (Campos & Macchiarelli, 2020, pp. 1795–1796). This finding is supported by Campos and Macchiarelli’s (2020) more sophisticated core/periphery measure based on a VAR estimation of an AD–AS model. According to this model, the probability of the UK being in the periphery decreased substantially after 1999 but remained unstable. Consistent with this result, Quah and Ho (2025, p. 7) estimate that UK business cycles were much more correlated with those of the Eurozone than with those of the US (0.65 vs. 0.46) during the period 1999–2008 – but not thereafter, when both correlations dropped substantially (0.14 vs. 0.15).

Since the MPF and OCA frameworks are not mutually exclusive, both may predict a positive effect. However, when these frameworks are used to evaluate the EMU as it currently functions, the predictions are less optimistic. The MPF framework anticipates economic growth through the preservation of economic freedom, provided that the government budget constraint is sufficiently strict. The rules of the Eurozone, however, do not effectively enforce such constraints and therefore do not lead to a freer economy – particularly in the case of a country that is already relatively free.

The lack of budget discipline might even be seen as good news from an OCA perspective. Even if the OCA criteria are met, the actual constraints on active fiscal and monetary policies may hinder growth. As De Grauwe (2022, pp. 242–247) dismissed the structural explanation for the slow recovery of Eurozone countries and attributes it to unnecessary austerity – an approach the UK, as a non-member, was able to avoid.

It seems, then, that the positive predictions of the ideal versions of both frameworks are tempered by the actual functioning of the EMU and the specific circumstances of the UK.

¹² Mulhearn and Vane (2005) gave an account of the opposing views of Minford (2004) and Buiter (2008).

3 Methodology

Surprisingly, only few scholars have conducted causal empirical studies estimating what might have happened if the UK had joined the euro, using various econometric methods.

For instance, Frankel and Rose (2002) applied a gravity model – including a key dummy variable for currency union membership – and used a two-stage approach to estimate the potential effects on trade. According to their results, the UK would have experienced a 20% increase in per capita income had it joined the euro. Pesaran et al. (2007) conducted a counterfactual analysis using a more recent method, Global VAR. Their findings align with those of Frankel and Rose (2002), also indicating that the UK would have enjoyed higher per capita income (and lower interest rates) within the Eurozone. However, Mazumder and Pahl (2013) contradicted these conclusions. Based on estimates from three versions of the Phillips curve for the UK, their counterfactual analysis suggests that the UK made the right decision by not joining the euro, as it would have faced higher unemployment and lower output. In a more recent study, Ioannatos (2021) employed a counterfactual analysis using the Difference-In-Differences method and found that the UK's per capita income would have been, on average, 15.48% higher after adopting the euro.

The above studies can be criticized on two grounds. First, their methods rely on statistical forecasts to construct the counterfactual, whereas data-driven approaches are generally considered superior. Second, the DID method is best suited for situations where many units are exposed to the treatment (Arkhangelsky et al., 2021); however, this condition does not hold in the present case. Moreover, the method assumes parallel trends between treated and control units – a key assumption that is not valid in our context.

In what follows, we apply two recent methods for counterfactual empirical analysis – the Synthetic Control Method and the Synthetic Difference-in-Differences method – which are regarded as alternative approaches for estimating the causal treatment effect. A notable advantage of these methods is that they are data-driven and offer improved performance relative to the techniques used in the aforementioned literature.

3.1 The SCM

The SCM is a method designed to evaluate how a “treatment” (policy change) causally influences a particular unit, referred to as the treated unit. The analysis uses several untreated units that did not undergo the treatment as units for the synthetic control. The core principle of SCM involves estimating the counterfactual outcome for the treated unit, that is, the outcome that would have been observed in the absence of the treatment. This estimation, known as the synthetic control, is derived as a weighted average of the outcome variables from the untreated units. Then the method minimizes the distance between the outcome value of the synthetic control and the treated unit, measured by the root mean square prediction error (RMSPE) (Abadie et al., 2015, p. 505). Once the synthetic control values are calculated, we compare them

with the actual outcome values of the treated unit in the posttreatment period. Any observed divergence between these values is subsequently attributed to the treatment.

The above can be formalized as follows (Abadie, 2021). Let us assume that we observe $J + 1$ units; the first unit is exposed to the treatment at the time $1 < T_0 < T$, and the remaining J units are not directly or indirectly affected by the treatment. Let Y_{it} indicate the observed value of the outcome for the i -th unit at time $t = 1, 2, \dots, T$, and Y_{it}^N be the value of the outcome variable that would be observed for unit i in the absence of the treatment. For units $i \neq 1$, $Y_{it}^N = Y_{it}$. The treatment effect can be denoted by

$$\alpha_{1t} = Y_{1t} - Y_{1t}^N$$

At time $t = T_0 + 1, \dots, T$, where Y_{1t} is the observed (actual) outcome of the treatment unit and Y_{1t}^N is the counterfactual (estimation of the synthetic control).

Let X_i be a $(k \times 1)$ vector of pretreatment characteristics of the treatment unit, and similarly, X_0 be a $(k \times J)$ matrix containing the same variables for the untreated units. We then find a vector of nonnegative weights that sum to one, $W^* = (w_2^*, w_3^*, \dots, w_{J+1}^*)$ bringing the weighted value of X_0 as close as possible to X_1 . Then Y_{1t}^N can be approximated by $\sum_{j=2}^{J+1} w_j^* Y_{jt}$.

Let V be the diagonal weight matrix of each variable predicting the synthetic control. Now we minimize the distance between the pretreatment characteristics of the treated unit and that of the J control units given by

$$\|X_1 - X_0 W\| = \sqrt{(X_1 - X_0 W)' V (X_1 - X_0 W)}$$

Accordingly, an unbiased estimate of the treatment effect is given

$$\text{by } \hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}.$$

While Abadie (2021, p. 408) argued only that the applicability of SCM is constrained by the context of the problem and data requirements, more recently, Hollingsworth and Wing (2022) advanced toward explicitly identifying the assumptions underlying the SCM design. Their five assumptions are as follows:

- (1) There is no interference between the units. Each unit's potential outcome in a period depends only on that unit's treatment exposure.¹³
- (2) An interactive fixed-effects model models the time series for each unit's untreated potential outcomes. The untreated potential outcomes can be decomposed into

¹³ This assumption rules out the possibility of spillover effects from the treated unit to one or more of the untreated units. This also implies that there are no anticipation effects associated with future treatment exposures in any period.

two unobserved components: unit-specific responses to time-varying common and noncommon factors that differ across units and periods.¹⁴

- (3) There are three requirements vis-à-vis the noncommon factors that pose limitations on the sources of differential trends across units: the variance of the noncommon factors is finite, there is no serial or spatial correlation in the noncommon factors, and the noncommon factors have a mean of zero for each unit and period.
- (4) The common factors generating the unit-specific trends are linearly independent during the pretreatment period, meaning they are not perfectly multicollinear during the pretreatment period.
- (5) Some weights create a synthetic control that perfectly matches the pretreatment time series of the treated units. The weights are nonnegative and sum to 1.

In the case of the UK not adopting the euro, the underlying assumptions can be summarized in the following three points:

- (1) Neither the UK's decision to stay out of the euro nor expectations about that policy influence the macroeconomic outcomes of the countries included in the donor pool.
- (2) The structural determinants of the outcomes (i.e., common factors) remain stable before and after the launch of the euro, although their effects on outcomes may differ across countries.
- (3) Transitory shocks that affect outcomes in the UK or the donor pool countries are independent of similar shocks in other countries, as well as of the structural determinants. This assumption applies to changes in outcomes that are not explained by the common factors, rather than to any deviation from a trend – since the common factors are time-varying.

It would be difficult to find another country for which the above criteria are better met than the UK. First, in principle, a “spillover effect” appears plausible due to the euro's impact on trade. However, as discussed in Sect. 2, this trade effect was relatively weak in the UK. Second, the UK's decision to stay out of the euro may not have been particularly surprising, given its historically reluctant participation in European exchange rate regimes since World War II. The proposal of a “hard ECU” in 1990 – well before the introduction of the euro – made it clear that the British government might have supported a “common currency” but was unwilling to adopt a “single currency” (Phelan, 2015, pp. 400–407). In terms of shock correlation, the UK is also somewhat distinct: its supply shocks are positively correlated with those in Europe, while its demand shocks show a negative correlation. Overall, the correlation is low enough that “the position of the UK is noteworthy” (De Grauwe, 2022, p. 79).

These considerations justify applying the SCM to our analysis. The design of our model reflects concerns about the “threats to validity” identified by Hollingsworth

¹⁴ Under this assumption, units may follow complicated differential trends over time because each unit responds differently to a common set of underlying time-varying factors. In this sense, the parallel trend assumption of the Difference-In-Differences and the two-way fixed effects is relaxed.

and Wing (2022), who argue that it is not necessary to explicitly rely on their five abstract assumptions, but rather to focus on the potential threats to them.

3.2 SDID method

Arkhangelsky et al. (2021) recently developed a new estimator for causal effects with panel data. This estimator builds on the insights behind the widely used DID and the SCM. The new method, the SDID, combines the attractive features of the two.

To illustrate SDID, let us have a balanced panel with N units and T periods, where the outcome for unit i in period t is denoted by Y_{it} . The exposure to the treatment is denoted by $W_{it} \in \{0, 1\}$. Let us assume that the first N_{co} control units are not treated, while the last N_{tr} treated units are exposed after time T_{pre} . As in the case of SCM, we must find weights $\hat{\omega}^{sdid}$ that align the pretreatment trends in the outcome of the untreated and treated units. In this method, in addition to the unit weights, time weights λ_{it} are also determined, balancing the pre and posttreatment periods. Unit weights are designed so that the average outcome for the treated units is approximately parallel to the weighted average for the control units. Time weights are designed such that the average posttreatment outcome for each control unit differs by a constant from the average of the pretreatment outcomes for the same control unit. In summary, SDID weights, as opposed to SCM weights, allow an intercept term, meaning that parallel trends are sufficient instead of the perfect match between the pretreatment trends of the treated and untreated units because the fixed effects term will absorb any constant difference. However, note that if we omitted the intercept, we would have the same weights as in SCM with one treated unit.

Using unit and time weights in a basic two-way fixed effects regression to estimate the average treatment effect on treated (ATT), we obtain the following¹⁵:

$$\left(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta} \right) = \arg \min \left[\sum_{i=1}^N \sum_{t=1}^T (Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right]$$

Using the time and unit weights offers two advantages (Arkhangelsky et al., 2021). First, using only similar units and similar periods makes the estimator more robust; second, the estimator is more precise by removing systematic parts of the outcome. Arkhangelsky et al. (2021) argued that the latter is more credible if the SCM and SDID estimators are different.

¹⁵ For the sake of comparison, let us consider the traditional DID estimates in which ATT is given by solving the following two-way fixed effects regression problem without either time or unit weights:

$$\left(\hat{\tau}^{did}, \hat{\mu}, \hat{\alpha}, \hat{\beta} \right) = \arg \min \left[\sum_{i=1}^N \sum_{t=1}^T (Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau)^2 \right]$$

It is also worth looking at the estimations in the SCM given by the following problem:

$$\left(\tau^{sc}, \hat{\mu}, \hat{\beta} \right) = \arg \min \left[\sum_{i=1}^N \sum_{t=1}^T (Y_{it} - \mu - \beta_t - W_{it}\tau)^2 \hat{\omega}_i^{sc} \right]$$

4 Empirical analysis

4.1 The model and data

The treatment year is 1999, when the euro was introduced, and the “treatment” itself is the UK’s policy of not introducing the euro.¹⁶ Based on the suggestion by Abadie (2021) – namely, that the pretreatment period should be long enough to allow accurate econometric estimation, but short enough to avoid issues associated with structural breaks in the time-series data – we opt for a period of approximately 20 years; accordingly, the pretreatment period is defined as 1980–1998. The choice of this relatively long pretreatment period is aligned with Hollingsworth and Wing (2022, pp. 10–14), who suggest that using a more extended period might help identify “dormant factors,” i.e., low-frequency influences that might have an independent effect only in the posttreatment period. The reason why we are more concerned about “dormant factors” than about structural breaks is not that we believe that “dormant factors” are more probable among the European countries in our sample. Rather, the reason is that the SDID method – discussed earlier and used as a robustness check in Sect. 5.1.6 – is more helpful in identifying structural breaks than in identifying “dormant factors” when used together with SCM. The posttreatment period is defined as 1999–2015, ending in the year before the Brexit referendum. This year appears to be an appropriate endpoint for the posttreatment period since the referendum may have constituted an asymmetric shock, potentially influencing the UK’s economic performance. As a result, the two periods are of roughly equal length.

Abadie et al. (2015) and Abadie (2021) provide clear guidelines regarding the selection of explanatory variables in the SCM. They advise, first, including the pretreatment values of the outcome variable or averages of those values over part of the pretreatment period, and second, incorporating values of other predictors from specific years during the pretreatment period and/or their averages over a portion of that period. Our choice of predictors is informed partly by findings from the growth regression literature and partly by the methodological considerations highlighted by Campos et al. (2019, p. 91), who emphasized that “the choice of predictors of the outcome should include variables that can approximate the path of the country affected by the intervention.” In this sense, the selection of predictors involves a degree of arbitrariness and is subject to experimentation, thus allowing for robustness checks using an alternative predictor set. Our outcome variable is per capita income measured by per capita GDP at constant 2015 USD taken from the World Development Indicators (World Bank, 2024). As explanatory variables we use the share of investments within GDP measured by gross capital formation as a percentage of GDP (World Bank, 2024), life expectancy at birth¹⁷ (World Bank, 2024), the share of

¹⁶ Note that several other EU member states did not join the euro in 1999 besides the UK, a fact which does not affect our analysis provided they will not be included in the donor pool.

¹⁷ We thank one of our referees for pointing out that life expectancy at birth is not an obvious choice. It is, however, a reasonable one. Data show (Prados de la Escosura, 2023) that the relationship between GDP per capita and life expectancy is strongly correlated, and this correlation does not cease to exist among high-income countries such as the Western European ones we are considering. What might be even more

exports within GDP (World Bank, 2024), and the Area2 and Area4 subindexes of the Economic Freedom of the World index (Gwartney et al., 2021).

The donor pool should consist of countries that did not experience the treatment – in this case, those that did join the euro. Abadie et al. (2015) noted, selecting the donor pool is not always straightforward and involves a degree of subjectivity; however, following their methodological recommendations can help minimize the risk of arbitrariness. The primary criterion in donor pool selection is that the countries included should be as similar as possible to the treated unit. Yet, the researcher must decide which attributes define this similarity. Units (i.e., countries) deemed too dissimilar from the treated unit should be excluded, as should those that experienced significant idiosyncratic shocks during the study period – provided such shocks would likely not have affected the treated unit (Abadie et al., 2015; Abadie, 2021).¹⁸ Moreover, it is important to consider that including a larger number of countries in the donor pool increases the risk of overfitting (Abadie et al., 2015; Abadie, 2021; Hollingsworth & Wing, 2022).

To ensure the similarity between the donor pool countries and the treated unit (the UK), we carefully selected countries for inclusion in the donor pool. First, we limited our selection to EU member states as of the treatment year, resulting in 14 countries (excluding the UK). Next, we excluded those that, like the UK, did not adopt the euro in 1999 – specifically Greece, Denmark, and Sweden. In the third stage, we removed countries that adopted the euro but joined the EU too close to the treatment year, such as Austria and Finland. Including these would significantly shorten the pretreatment period, introducing bias into the synthetic control estimation.

This process left us with nine countries: Belgium (BEL), Luxembourg (LUX), the Netherlands (NLD), Italy (ITA), France (FRA), Germany (DEU), Ireland (IRL), Spain (ESP), and Portugal (PRT). However, including Spain and Portugal is potentially problematic, as both countries significantly differ from the UK in several key respects – most notably their significantly lower levels of per capita GDP. For this reason, we also excluded them from the donor pool. Conversely, when considering Luxemburg, its significantly higher per capita GDP and much lower market size could serve as a basis for its inclusion. As a result, the final donor pool consists of Belgium (BEL), the Netherlands (NLD), Italy (ITA), France (FRA), Germany (DEU) and Ireland (IRL).

important is that, for these high-income countries, the correlation differs from the pattern observed in other groups of countries.

¹⁸ In this latter respect, at first glance, the fact that the posttreatment period includes the 2008 financial crisis and its aftermath might seem problematic. However, its effect is usually understood as having influenced “northern” countries differently from the “southern” ones (De Grauwe, 2022, pp. 14, 116–119; Sinn, 2014), with the latter group usually including Greece, Italy, Portugal, Spain, Ireland, and Cyprus. Ireland, however, stands out, as its crisis management was quite different to that of the other five countries (Sinn, 2014, pp. 5–6) – so much so that, afterwards, it is seen as northern (Rusek, 2025). The southern countries in our donor pools might then be a source of a “dormant factor”. Our synthetic UK, however, eventually gives zero positive weight to any southern countries, as will be shown in Sect. 4.2. All this means that we have no reason to assume any asymmetric shocks between the UK, on the one hand, and the countries “making” the synthetic UK, on the other.

4.2 Baseline SCM results

Table 2 presents the actual values of the five covariates alongside those for the synthetic UK. As shown, the differences between the synthetic control and the actual UK values are minimal, indicating that the synthetic control provides a strong approximation of the UK

However, some countries in the donor pool were assigned zero weight. The countries with positive weights are the Netherlands (0.813), Germany (0.11), and Belgium (0.077).

The SCM analysis is best illustrated in Fig. 1, which shows the predicted synthetic UK alongside the actual data. The curves for the actual and synthetic UK are very close during the pretreatment period. In the posttreatment period, a slightly wider gap emerges before 2010 but nearly disappears afterward; overall, the average gap remains very small.

The treatment effects (i.e. the gaps) are shown in Fig. 2. The visualisation of the yearly treatment effects allows us to assess potential non-common factors with non-zero mean, which would constitute a violation of Assumption 3 in Sect. 3.1. Specifically, the 2008 financial crisis might be thought to have had different effects on the UK and the Eurozone because of the quicker recovery in the UK (Restrepo-Echavarría & Ariashttps, 2017). However, as shown in the figure, the peak around 2008 is not exceptional; a similar one occurred around 1988. In addition, we believe that the construction of the donor pool makes a non-common shock after 2008 highly unlikely, as it only includes the so-called northern countries and excludes the so-called southern ones.

The composition of our donor pool, together with the evidence from Fig. 2, supports the conclusion that although the 2008/2009 crisis might have represented a

Table 2 Predictors balance in the pretreatment period

Covariate	Treated	Synthetic control	Synthetic control bias	Predictor weight
per capita GDP, averaged for 1980–1998	29880.9958	29853.7656	−0.09%	0.7197
life expectancy, averaged for 1980–1998	75.6228	76.5083	1.17%	0.0099
investment share within GDP, averaged for 1980–1990	22.1941	22.3521	0.71%	0.0793
Area2 subindex of the EFW index, averaged for 1980–1998	8.0425	7.9597	−1.03%	0.1204
Area4 subindex of the EFW index, averaged for 1980–1998	9.0725	8.8251	−2.73%	0.0707

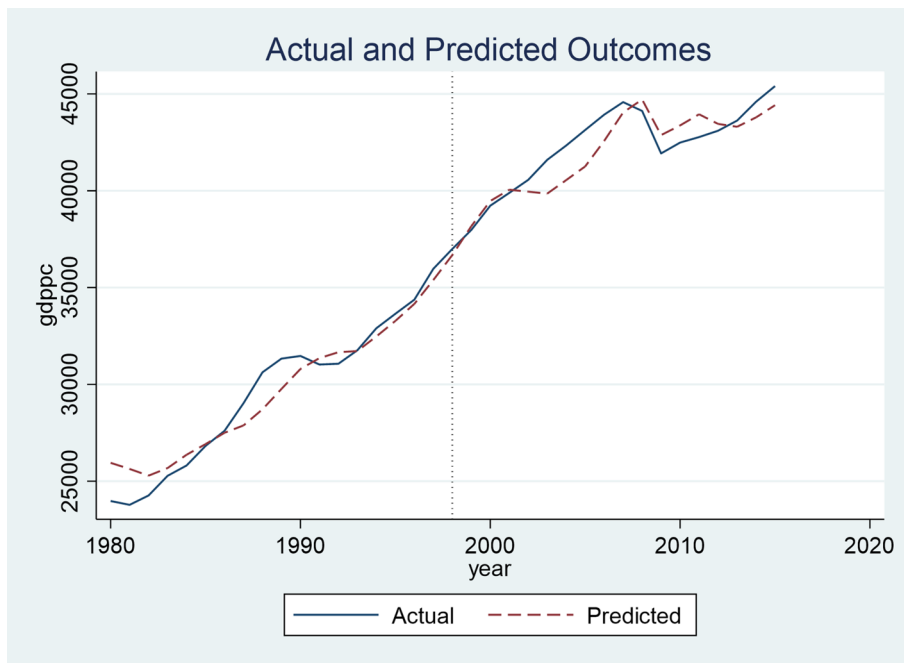


Fig. 1 Prediction of the synthetic UK

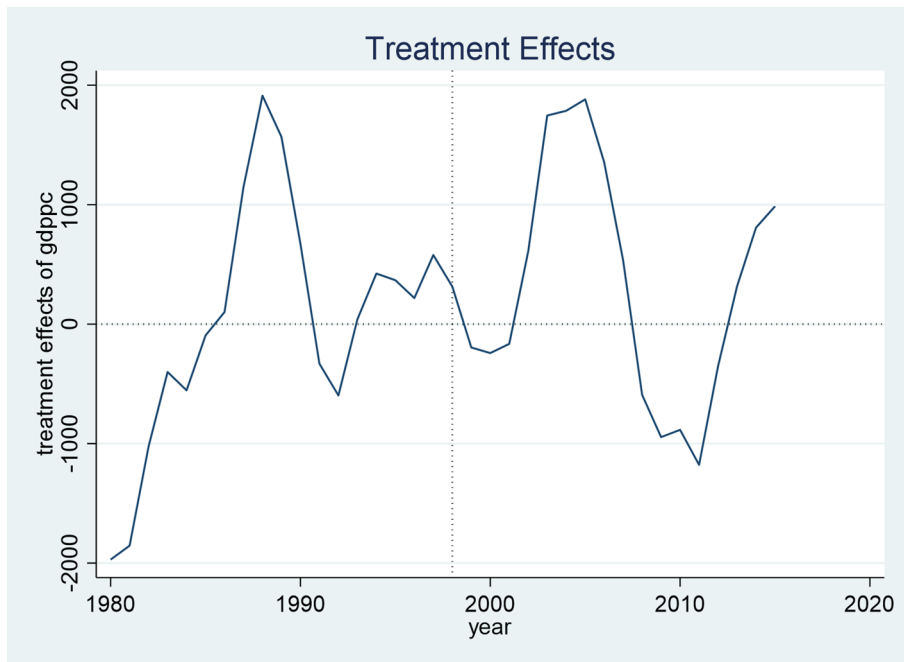


Fig. 2 Treatment effects

non-common factor, it did not exhibit a non-zero mean. This conclusion aligns with the common interpretation of the crisis and the policy responses to it. According to this view, the shock triggered different policy responses (Fischer, 2021; Sinn, 2014) – to the extent that one can argue the “more aggressive” monetary policy (Fischer, 2021, p. 8) contributed to the smoother recovery path of the UK compared to the Eurozone as a whole. Nevertheless, in our analysis, the crisis does not qualify as a non-zero-mean factor because the experience of the northern countries (Germany, Austria, Finland, Netherlands, Luxembourg, Belgium, France, and later Ireland) differed markedly from that of the southern ones (Greece, Italy, Spain, Portugal, and Cyprus). The northern countries suffered significantly less, as the less aggressive (more conservative) monetary and fiscal policies of the ECB aligned more closely with their needs (see also footnote 18). Thus, the most plausible interpretation when comparing the UK and the northern Eurozone countries in our donor pool is that there was a common shock, with heterogeneous policy responses, but no resulting structural changes.

The mean actual outcome over the entire period is €42,427, while the mean predicted outcome is €42,105. The difference between these values gives us a mean treatment effect of €322, implying that the UK would have experienced a lower per capita GDP had it joined the Eurozone.

Although the average treatment effect is small, statistical inference tests are needed to determine whether it is significantly different from zero. However, conventional statistical inference methods are not available for SCM (Abadie et al., 2010). Therefore, Abadie et al. (2010, 2015) and Abadie (2021) proposed an inferential technique to “replace” traditional significance testing, namely, the placebo test.

4.3 Statistical inferences

Two kinds of placebo tests can be performed. The first is the “in-space” placebo test, used to assess statistical significance.¹⁹ This test examines whether a treatment effect would be detected if untreated units were hypothetically treated. In practice, we estimate the treatment effect for each untreated unit in the donor pool by calculating synthetic control estimates for countries that did not experience the treatment.²⁰ However, pretreatment fit is critical for this statistical inference. Abadie et al. (2010) and Abadie (2021) suggest that any donor country with an RMSPE 5 to 20 times higher than that of the treated unit in the pretreatment period should be excluded from the placebo analysis. Since this ratio is about 5 for Ireland, we excluded the country from the placebo tests shown in Fig. 3.

The figure allows us to compare the estimated effect of the UK not adopting the euro (blue curve) with the placebo effects obtained for five countries in the donor pool (grey curves). If the UK’s estimated effect is substantially larger than the placebo effects (see Abadie et al., 2015), the impact can be considered statistically sig-

¹⁹ An “in-time” placebo test serves as a robustness check (see below).

²⁰ The idea underlying this placebo test is the usual permutation test, in which the critical value of a test statistic is computed under all possible permutations of the “treatment” assignments in the control units (Hahn & Shi, 2017).

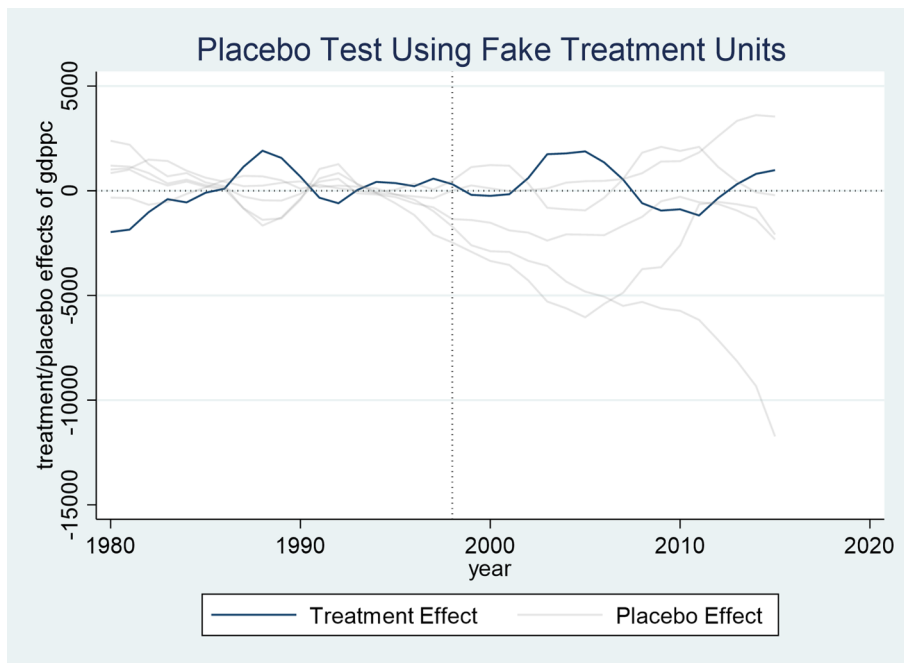


Fig. 3 “In-space” placebo effects

nificant. However, the evidence does not support this conclusion. As shown in Fig. 3, the UK’s treatment effect is the closest to zero, indicating that the “fake treatment units” (grey curves) actually exhibit larger treatment effects in absolute value. This suggests that the treatment would have had a more significant impact on the untreated units than on the treated one, implying that the treatment effect for the UK cannot be considered statistically significant.

However, besides the above simple visualization of the curves, we can calculate a test statistic (r_j), namely the ratio of the posttreatment and pretreatment Mean Squared Prediction Error (MSPE) – which is the square of RMSPE – as proposed by Abadie et al. (2010), measuring the quality of the fit of a synthetic control for unit j in the posttreatment period relative to the quality of the fit in the pretreatment period. Figure 4 presents this ratio for five units in the donor pool.

For the treatment effect to be considered significant, the ratio should be unusually large for the treated unit. However, Fig. 4 shows the opposite for the UK: its ratio is actually the lowest among all countries. In fact, to indicate that the UK’s GDP was affected by the treatment, the ratio would need to be much higher. If a country were chosen at random, the probability of observing a post-/pretreatment MSPE ratio as large as the UK’s is 1. Therefore, the “in-space” placebo test does not support the statistical significance of the treatment effect for the UK.

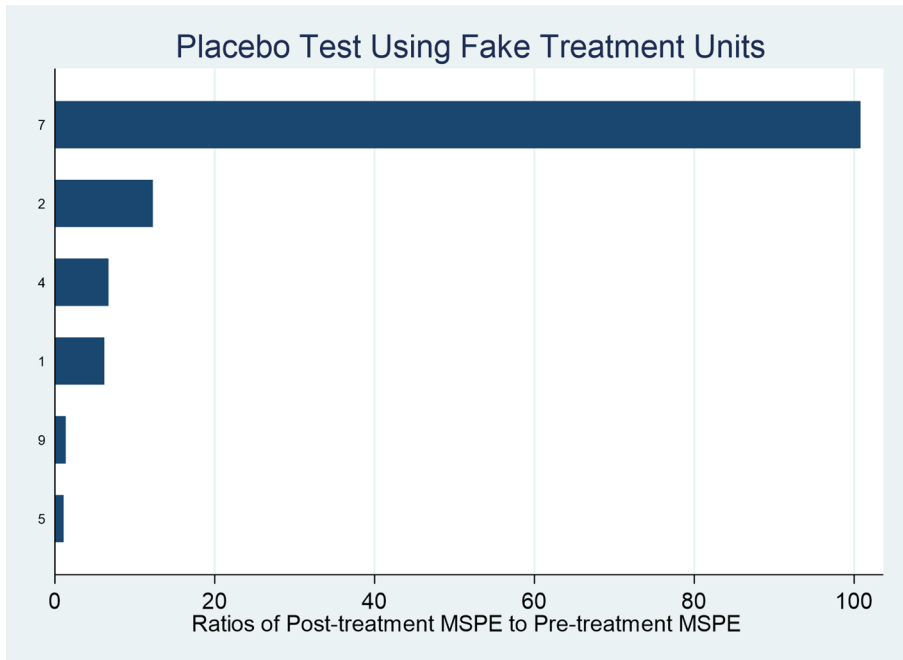


Fig. 4 Post-/Pretreatment period MSPE. 7:ITA, 2:DEU, 4:FRA, 1:BEL, 9:NLD, 5:GBR

4.4 Robustness checks

4.4.1 “In-time” placebo test

Based on Abadie (2021), an “in-time” placebo test involves backdating the treatment year to a point earlier in the pretreatment period. This approach allows the synthetic control to diverge from the actual values before the real treatment year; however, if no divergence occurs prior to the true treatment, the validity of the results is supported. Since we have a relatively long pretreatment period, we arbitrarily set 1992 as the placebo treatment year – more than five years before the establishment of the Eurozone. Figure 5 presents the SCM analysis results using this backdated treatment year.

Figure 5 shows that the actual UK and the synthetic UK move almost identically, just as in Fig. 1 with the true treatment year. This indicates that using a placebo (backdated) treatment year did not affect the synthetic UK’s trajectory. Therefore, the backdating test further supports and validates our baseline estimations presented in Fig. 1.

4.4.2 Leave-one-out analysis

In this analysis, we examine how the estimated treatment effect changes if one untreated unit is discarded from the donor pool. We drop all units with positive weight from the pool, one-by-one. The results are shown in Fig. 6.

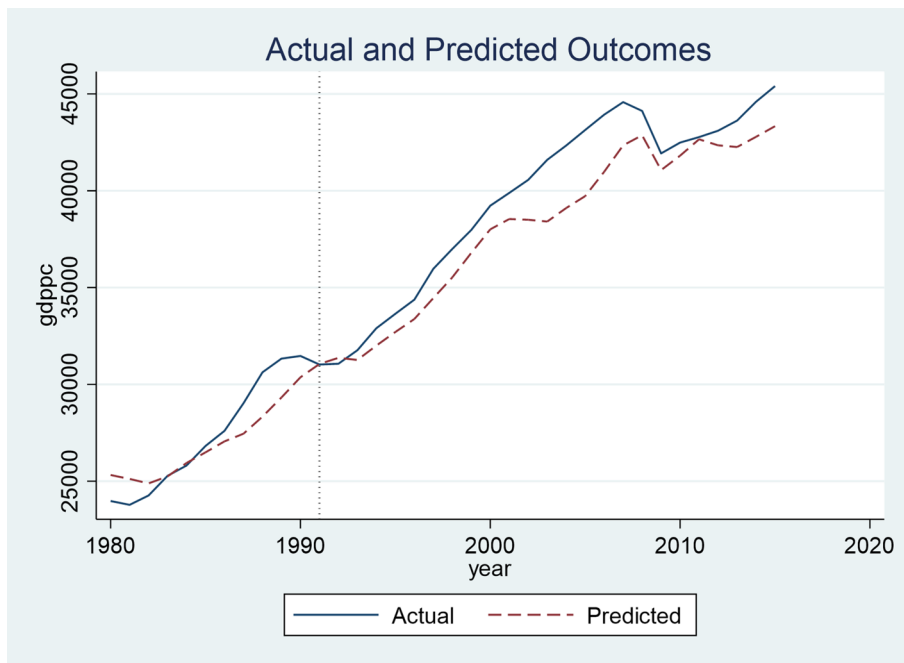


Fig. 5 Prediction for the synthetic UK if treatment occurred in 1992

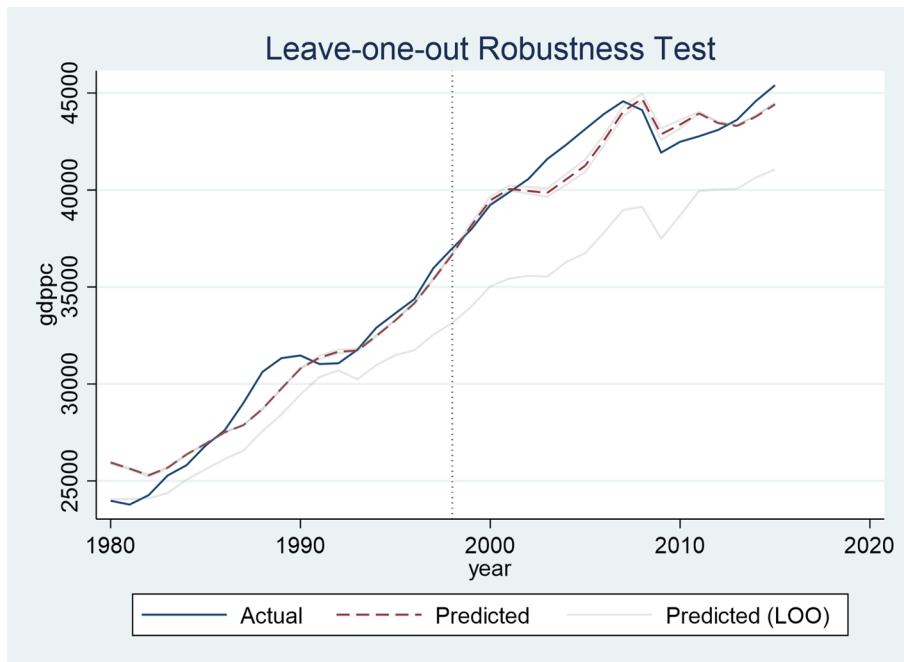


Fig. 6 Leave-One-Out robustness check

In an ideal model, the predicted leave-one-out curves – generated by omitting one donor country at a time – should closely follow the original predicted curve. This would indicate that the results are robust and not overly dependent on any single country. However, we observe that one leave-one-out curve deviates noticeably from the predicted curve, which somewhat weakens the robustness of our findings.

4.4.3 Different predictors

In this analysis, we use a slightly different set of predictors by replacing life expectancy at birth with the share of exports within GDP (World Bank, 2024). Despite this change, the synthetic control bias remains small, and the synthetic UK continues to closely approximate the actual UK, as illustrated in Fig. 7. Comparing Fig. 7 with Fig. 1 reveals virtually no difference, indicating that altering the set of covariates does not affect the posttreatment predictions. The donor countries assigned positive weights are NLD (0.808), DEU (0.121), BEL (0.049) and FRA (0.022), which is nearly identical to the composition found using the original set of covariates.

Figure 8 presents the treatment effects derived from this alternative set of covariates. Comparing it with Fig. 2 indicates that changing the predictors does not materially alter the results discussed earlier.

Since the probability of obtaining a post-/pretreatment MSPE ratio as large as the UK's is 1, the treatment effects should not be considered statistically significant, similar to the baseline results.

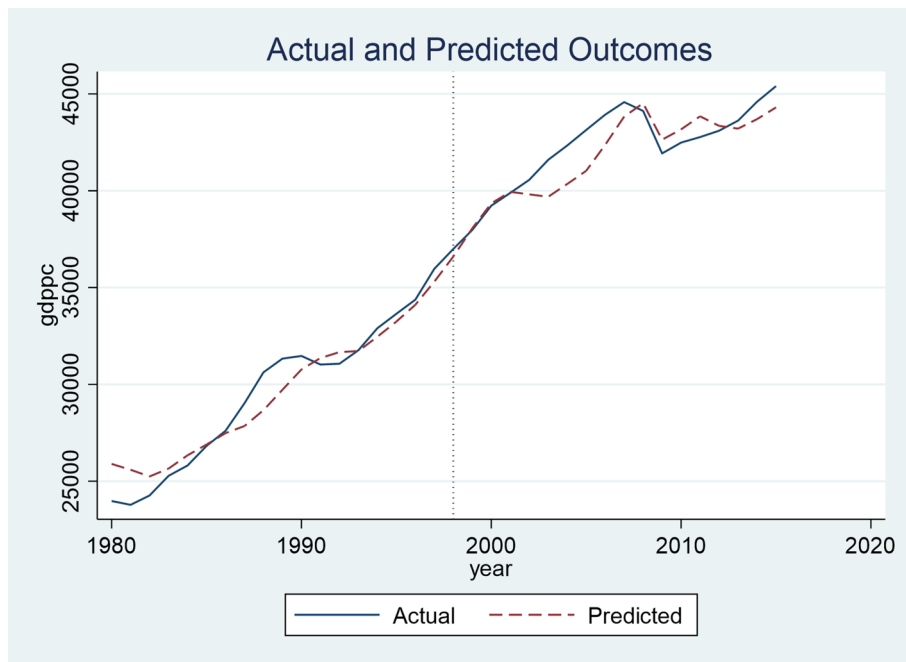


Fig. 7 Prediction of the synthetic UK with different covariates

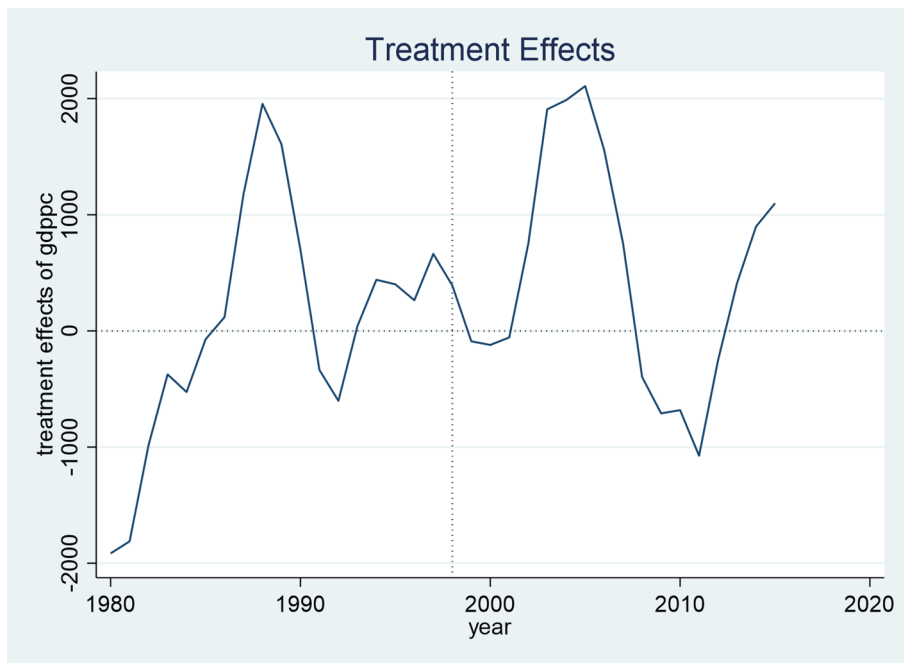


Fig. 8 Treatment effects with different covariates

4.4.4 Cross-validation

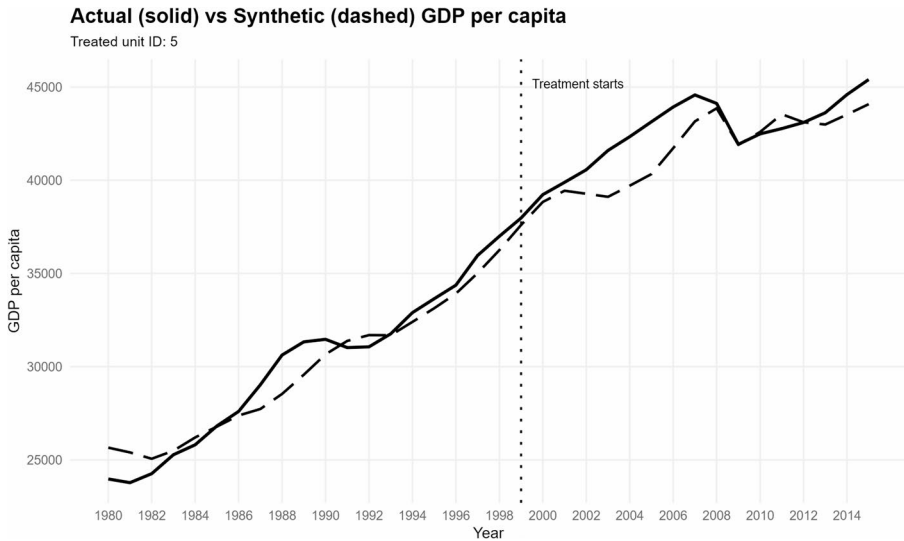
Abadie et al. (2015) argue that matching predictors over the entire pretreatment period may overweight variables that help fit past data but are less useful for predicting future (posttreatment) outcomes, leading to overfitting. To address this issue, they supplement the SCM analysis with a cross-validation strategy that selects predictor weights to improve out-of-sample (pretreatment) prediction of the treated unit's outcome. This cross-validation approach provides a more robust weighting scheme and tests the stability of the synthetic control; therefore, it can serve as a robustness check for the baseline SCM results.

In the cross-validation procedure, we use the same set of covariates and the same donor pool as in the baseline SCM and take the averages of the predictors.²¹ The optimal predictor weights are reported in Table 3. The cross-validation assigns weights of 0.271 to Germany and 0.729 to the Netherlands, which is only slightly different from the baseline result.

²¹ When dividing the pretreatment period into training and validation sets for cross-validation, we adhere to the common practice of favoring a longer training period (typically 60–70% of the total length). Consequently, we designated the first 11 years as the training period and the last 7 years as the validation period of the pretreatment data. To obtain annual data for Area2 and Area4 of the EFW index (1970–2000), where observations are only available every fifth year, we linearly interpolated the missing observations. Specifically, we used the average of the beginning and end years of each six-year period to fill in the four intervening years. As these indices measure institutions and policies that rarely undergo large, sudden changes over such a short timeframe, this interpolation method is unlikely to introduce significant bias.

Table 3 Optimal predictors weights in the cross-validation

Covariate	Optimal predictor weight	Predictor weight in the baseline SCM from Table 2
per capita GDP	0.6605	0.7197
life expectancy	0.0000	0.0099
investment share within GDP	0.0040	0.0793
Area2 subindex of the EFW index	0.0000	0.1204
Area4 subindex of the EFW index	0.3355	0.0707

**Fig. 9** Prediction of the synthetic UK with cross-validation

The predicted and actual per capita GDP values based on cross-validation (Fig. 9) closely mirror those from the baseline analysis.

However, visual inspection alone is insufficient; therefore, we calculate a p-value using post-/pretreatment period RMSPE ratios from the “in-space” placebo tests (see Fig. 4 for the baseline values). Figure 10 presents a histogram visualizing the results of these placebo tests²², which are used to assess the statistical significance of the estimated treatment effects. The grey bars represent the distribution of the post-/pretreatment period RMSPE ratios for all non-treated units, while the red vertical line marks the ratio for the treated unit. As in Fig. 4, the key idea of the placebo tests is to determine whether the treated unit’s ratio is extreme compared with the distribution of placebo ratios. In this case, the treated unit’s ratio of approximately 2 is not an extreme outlier. The p-value, calculated as the proportion of placebo ratios equal to or greater than the treated unit’s ratio, is 0.667. Given this high p-value, the observed

²² We applied the same exclusion criterion to the placebo tests as to the baseline case: removing any untreated unit with a pretreatment RMSPE that was at least five times greater than the treated unit’s RMSPE. This approach aligns with the methodology suggested by Abadie et al. (2010) and Abadie (2021).

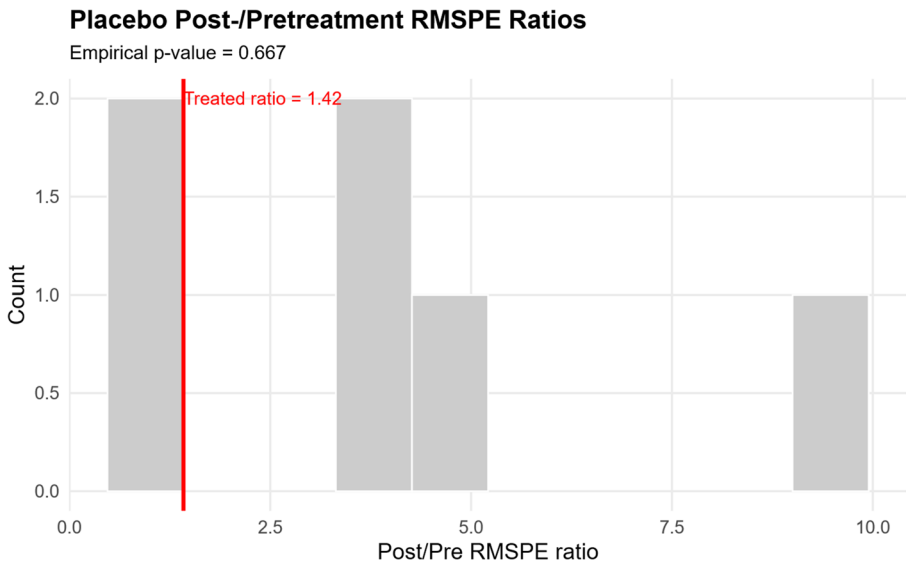


Fig. 10 Histogram of post-/pretreatment period RMSPE ratios

treatment effects are not statistically significant. This implies that the UK's per capita GDP would not have been higher had it joined the euro.

4.4.5 Different donor pool

We also tested whether the results are robust to a different set of donor countries. To form an alternative donor pool, we removed Ireland from the baseline set, since although it is considered an economy similar to other northern European ones, around the financial crisis of 2008 some authors lumped it together with other fragile southern economies, as we saw in footnote 18. Because of this uncertain position of Ireland, it seems reasonable – at least for a robustness test – to exclude it from the donor pool to avoid even such an unlikely source of an asymmetric shock.²³ As demonstrated below, the model performs even better than the baseline one. The synthetic UK is composed of the same countries as the baseline, maintaining similar weights (NLD: 0.848, DEU: 0.112, BEL: 0.040). The predictor weights range between 0.390 (Area2) and 0.025 (life expectancy), while the synthetic bias falls between -2.49% (Area4) and 1.24% (share of investments within GDP). The actual and predicted UK outcomes closely track each other (Fig. 11), indicating negligible treatment effects (the average treatment effect over the posttreatment period is €211).

The p-value arising from placebo tests is 1.00 (see Fig. 12), which reaffirms that the treatment effects are not significant, consistent with the baseline case.

²³ We believe the meaningful change to the donor pool is not enlarging it – since we already determined the largest possible set – but rather restricting it.

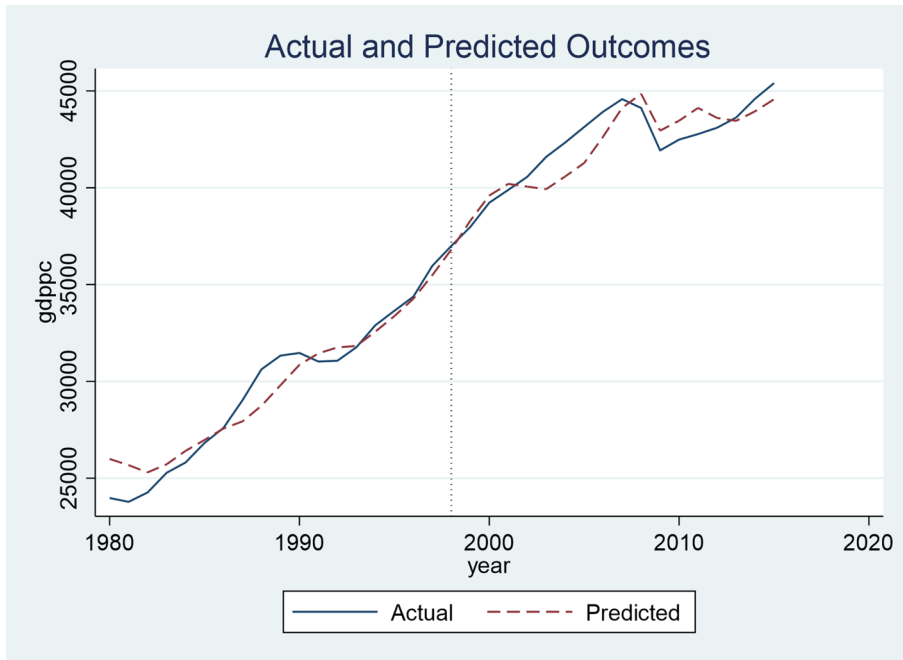


Fig. 11 Prediction of the synthetic UK with five donors

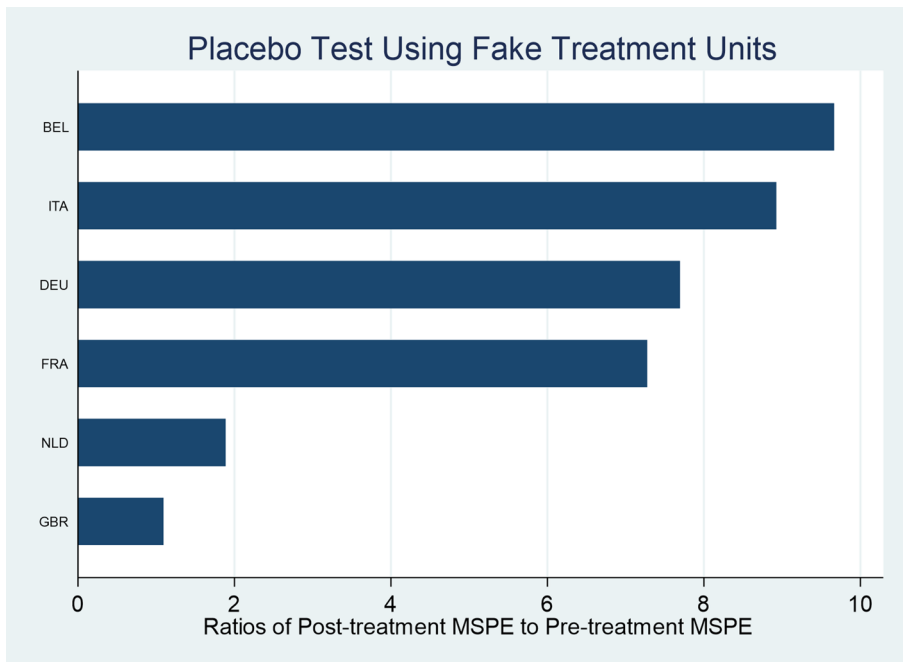


Fig. 12 Post-/pretreatment period MSPE ratios

Overall, results from this restricted donor pool fully support the baseline finding that the UK would not have benefited from higher GDP per capita had it adopted the euro.

4.4.6 SDID analysis

An alternative method for estimating the causal treatment effect is the recently developed SDID approach (Arkhangelsky et al., 2021), which we use as a robustness check for the SCM results. Its primary advantage over SCM is that it allows researchers to conduct traditional statistical inference²⁴. We apply the SDID analysis using the same donor pool and time period as in the SCM analysis, adhering to the guidance from Abadie et al. (2010) and Abadie (2021) regarding pretreatment RMSPE, as pretreatment fit remains equally important for SDID (see Sect. 3.2). We conduct the analysis both with and without covariates.²⁵

Figures 13 and 14 present the time series for the UK (Treated) and the control unit (Control), both without and with covariates. As shown, the two curves run nearly

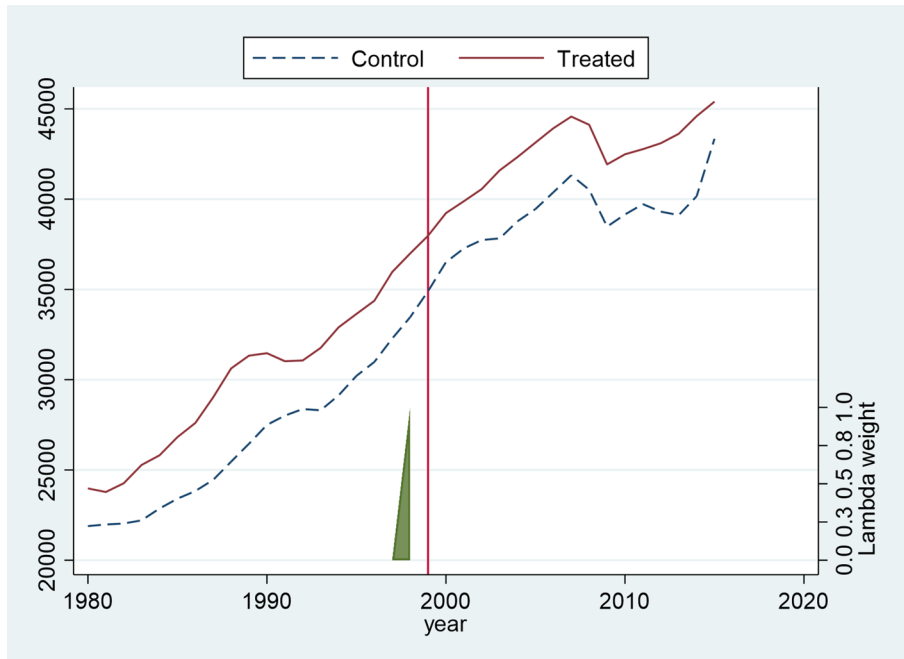


Fig. 13 SDID prediction of the UK and the control group without covariates (1980–2015)

²⁴ As mentioned above, statistical inferences for SCM are based on the permutation test whose validity usually requires a certain symmetry assumption, which is often violated in the context of synthetic control studies (Hanh & Shi, 2017).

²⁵ Note that the SDID method requires a strongly balanced panel dataset. Consequently, due to missing data for some variables used in the SCM analysis (specifically, the Area2 and Area4 indexes), we substituted these with the Consumer Price Index (CPI) and the share of investment within GDP, both sourced from the World Bank (2024).

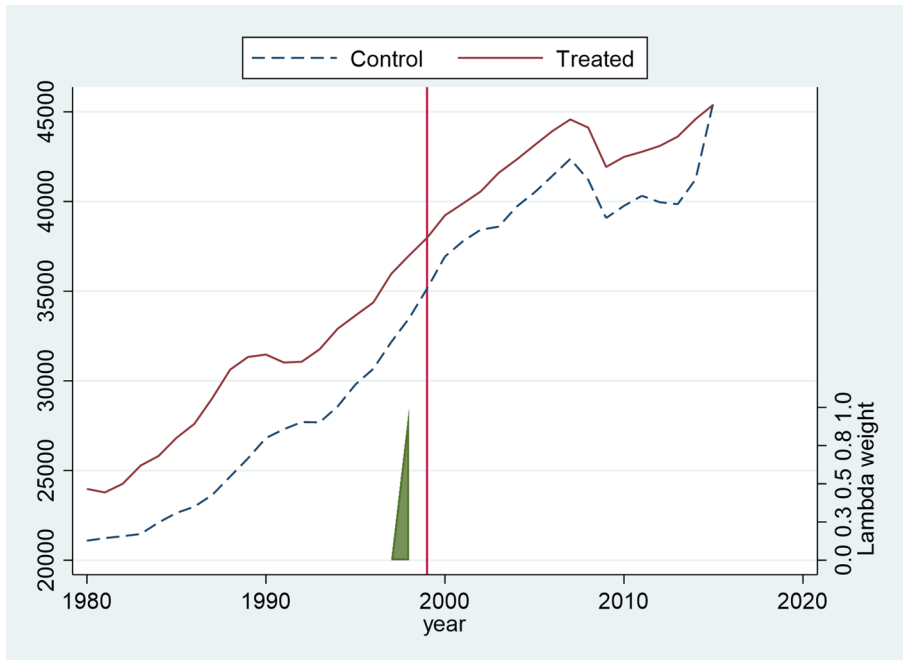


Fig. 14 SDID prediction of the UK and the control group with covariates (1980–2015)

Table 4 Average treatment effect estimations from the SDID

	without covariates	with covariates (CPI, I/GDP)
ATT	−163	−798
p-value	0.955	0.802

parallel during the pretreatment period. More importantly, this parallelism continues into the posttreatment period, indicating a negligible treatment effect. The close parallel trends demonstrate that the model performs very well (for further details, see Arkhangelsky et al., 2021).

Table 4 presents the precise values of the average treatment effect on the treated (ATT) for both estimations. As indicated by the p-values, none of the effects are statistically different from zero. The SDID results are consistent with those from the baseline SCM, both suggesting that had the UK joined the Euro in 1999, its per capita income would not have been higher between 1999 and 2015.

When comparing the treatment effects estimated by the SDID with those from the baseline SCM, we observe that the SDID estimates are different. Although we have conducted several robustness checks for the SCM and applied a specialized statistical inference test (Sect. 4.3), these results cannot be considered fully reliable due to the absence of traditional significance testing and the various “threats” to validity discussed in Sect. 3.1. In contrast, the SDID results are more robust, benefiting from

a conventional significance test and relying on fewer potentially invalidating assumptions. Therefore, when discrepancies arise between the two methods, we place greater confidence in the SDID findings and conclude that the UK did not experience a significant treatment effect from the policy of not adopting the euro.

5 Conclusions

This paper has focused on providing new evidence on whether the UK would have experienced higher per capita GDP had it adopted the EU's common currency in 1999. Although the UK left the EU in 2020, this question remains relevant for at least two reasons. First, the literature still lacks convincing counterfactual analyses on the topic. The two methods employed here – the SCM and the SDID approach – offer robust estimations of the causal treatment effect of the UK's decision not to adopt the euro. Second, obtaining new empirical evidence is crucial, as the issue remains politically charged; economic facts are essential for informed theorising about adopting a common currency.

We framed this empirical question by contrasting two main approaches to the euro: the Optimum Currency Area (OCA) theory and the Monetary Policy Framework (MPF). While both explain how a common currency might boost economic growth, their conclusions differ, and neither unequivocally supports the claim that adopting the euro would have increased British per capita income. Specifically, the OCA theory offers a mixed prediction, whereas the MPF suggests a negative effect.

The empirical methods used in this paper provide a significant advantage over the standard Difference-In-Differences approach by improving estimation precision. Both the SCM and SDID relax the strict parallel trends assumption required by traditional two-way fixed effects models, which implies unit fixed effects that vary across units but do not change over time and time fixed effects that change over time but do not vary across units (Arkhangelsky et al., 2021).

The aim of employing both methods was to verify whether the results were contingent on a particular methodological approach. Our findings suggest they are not: both methods consistently indicate that the UK would not have experienced higher per capita income had it joined the euro. These results remain robust across a range of checks, including a different set of explanatory variables, the use of a placebo treatment year, the exclusion of individual donor countries in the SCM analysis, and the use of an alternative donor pool. Furthermore, the findings are confirmed through cross-validation.

We did not find a significant treatment effect for the UK, suggesting that the benefits and costs associated with adopting the common currency likely balanced each other out. In other words, any positive effects predicted by the OCA theory may have been offset by the costs identified by the MPF approach. While these findings offer insights, lessons for countries outside the Eurozone should be drawn cautiously, as each country's circumstances are unique.

Acknowledgements The paper was supported by the University of Debrecen Program for Scientific Publication. We are grateful to two anonymous referees for the constructive suggestions that helped substantially to improve the paper. All remaining errors are ours.

Funding Open access funding provided by University of Debrecen.

Data Availability The data that support the findings of this study are available from the corresponding author upon request.

Declarations

Conflict of interests The authors have no relevant financial or non-financial interests to disclose.

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