Proportional Representation and Right-Wing Populism: Evidence from Electoral System Change in Europe

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Abstract

How much do electoral institutions matter for the rise of populist parties? Evidence on this question is mixed with some scholars arguing that the role of electoral rules is small. We provide new evidence for the impact of electoral system change. The United Kingdom's adoption of a proportional electoral system for European elections in 1999 provides a unique opportunity to study the link between electoral rules and the ascendancy of right-wing populist parties. Employing both synthetic control and difference-in-difference methods, we estimate that the electoral reform increased the vote share of right-wing populists by about 12 to 13.5 percentage points on average. During a time when populism was rising across Europe, the reform abruptly shifted populist votes in the UK above the European trend and above more plausible comparison cases. Our results also imply that caution is needed when empirical results based on partial reforms are extrapolated to electoral system change.

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How much do electoral institutions matter for the rise of right-wing populism? Political scientists continue to debate this question. In this research note, we use a fundamental electoral system change to study the contribution of changes in political institutions to the rise of right-wing populist (RWP) parties in Europe. In 1999 the United Kingdom adopted a proportional electoral system for European elections. It replaced the traditional first-past-the-post system in single member districts. Theories of electoral institutions and strategic coordination by voters and politicians imply that such an institutional change can be conducive to the entry and growth of new parties. With its combination of multi-member districts and a proportional electoral formula, a proportional representation (PR) system reduces the prospect that a vote for a new entrant is "wasted" (Duverger 1954). Introducing PR for country-wide elections—for the first time in British history—may have contributed to the spectacular growth of new RWP parties above the European trend. But has it? In this note, we provide a controlled test.

We analyze a unique institutional change—the introduction of a PR system for European elections in 1999 in the United Kingdom—and find that it is an important institutional factor behind the rise of right-wing populists. This electoral system change takes place within a Europe-wide assembly. It allows us to model the causal impact of adopting PR *at scale* in a comparable institutional setting and thus to better account for alternative explanations. Specifically, we use a synthetic control approach (Abadie, Diamond and Hainmueller 2010) to estimate changes in electoral support for RWP parties compared to a 'synthetic UK' without such a reform. Compared to the synthetic control case (which shows the same pre-reform levels of electoral RWP support), the introduction of PR increased the average vote share for RWP parties in the UK by about 13 percentage points in the subsequent four elections. Further analyses based on flexible difference-in-difference models confirm these results. A major beneficiary of this change was the UK Independence Party (UKIP). In 1994, UKIP won only 1% of the vote and failed to obtain a single seat, while by 2014 it had become the UK's largest party in the EP. Taken together, our best estimates suggest that the electoral reform accounts for roughly one-half of the observed growth in right-wing populism in European election in the UK.

While our analyses leverage the unique institutional change in the UK, it is important to stress that they are still based on observational data and estimates depend on particular modeling assumptions. To enhance the plausibility of our results, we (i) employ two different model setups (with different identifying assumptions); we (ii) enhance model robustness using a large number of time-varying variables whose timing might be confounded with the reform effect (including mainstream party positions, public Euroscepticism, immigration inflows, and economic globalization); (iii) we provide extensive specification and placebo tests in the appendix.

The estimated impact of the reform is substantively important and theoretically plausible. Theories of electoral institutions imply a switch from first-past-the-post elections in single member districts to multi-member district PR makes the electoral system more permissive and increases the limit on the viable number of parties (Cox 1997).

At the same time, in the empirical populism literature the relevance of electoral rules "continues to be questioned" (Norris and Inglehart 2019: 317). Many studies focus on demand-side factors, such as globalization and cultural anxieties (for reviews, see Golder 2016; Norris and Inglehart 2019), while a complementary body of research examines the impact of electoral institutions and political opportunity structures. However, assessing the causal impact of electoral system change is fraught with well-known difficulties, and the existing literature finds only mixed and contradictory support for the idea that more proportional electoral systems are a causal driver of RWP *votes* (Golder 2016: 486; Muis and Immerzeel 2017: 913). Moving beyond studies of populism, the literature on the success of new (and niche) parties debates the impact of electoral rules and faces the same empirical challenges (Lago 2021; Meguid 2005). Because electoral system change is rare, early research on the vote effects of electoral systems is largely cross-sectional. To mitigate endogeneity problems, some comparative studies use election-year panel data and leverage within-country changes in median district magnitude (e.g., Golder 2003). While an important contribution, these studies effectively consider a different research question, namely the effect of adjustments *within* existing systems on a single dimension.

Our results have notable implications for research on institutions and the success of populist and new parties more broadly. They indicate that extrapolating evidence from marginal reforms to the effect of electoral system change might underestimate its magnitude. While several studies report null results, the *largest* point estimate of the impact of median district magnitude on RWP vote shares from Golder (2003: p. 451, Table 2) implies that an increase in district magnitude, such as the one included in the reform studied here, increases RWP votes by about 6.9 percentage points. We find that the overall impact of the reform might be almost twice as large. This makes sense as electoral system change is a bundle and provides a focal point to strategic actors.

Altogether, our contribution stressed the importance of electoral system change to understand the ascendancy of RWP parties. To be clear, our analysis should not be interpreted as a horse race between institutional and other explanations. Our period of study is marked by a Europe-wide trend in rising RWP votes, which has been explained by several structural factors, including a backlash against globalization and rising economic insecurity (Colantone and Stanig 2018; Muis and Immerzeel 2017). Our aim it not to explain this general trend, but to assess whether electoral system change can help to explain the sudden change in electoral fortunes of populist parties in the UK, given underlying demand-side factors.

Institutional setting

Implementing a Labour manifesto pledge, the government of Tony Blair adopted a reform of the electoral system used to elect the UK's 87 members of the EP. The European Parliamentary Elections Act of 1999 replaced plurality voting in single member districts with closed-list PR in 11 multi-member districts.[1] Following the reform, the median British member of the European Parliament is elected in a district with 8 seats, compared to 1 seat before. The new system was first used in 1999 and it marks the first time a proportional electoral system was employed nationally in the UK. This reform was introduced to accommodate a potential coalition partner in Westminster (the Liberal Democrats); it did not result from a groundswell in support for populist alternatives (Fielding 2003: pp.50-55; Farrell and Scully 2007: ch. 4).

Other EU countries have used proportional electoral systems since 1979 and experience no reform between the 1994 and 1999 EP elections. While some minor institutional adjustments occurred in subsequent elections, the basic rules of the game remained in place until 2014 in the eight other countries that held European elections since 1979 (Belgium, Denmark, France, Germany, Greece, Ireland, Italy, and the Netherlands; we refer to them as EU8). They make up the pool of cases from which draw to construct a counterfactual synthetic UK.[2] The fact that European elections across countries are held for the same assembly in the same supranational system of government controls for the structure of legislative and executive institutions. This setting provides a fruitful environment for a comparative case study. We classify RWP parties competing in European elections between 1979 and 2014 following two recent and comprehensive comparative data sets drawing on a large secondary literature and expert surveys. In the UK, this includes both UKIP and the British National Party (BNP). Appendix A provides further details.[3]

Empirical results

The synthetic control approach (see Abadie, Diamond and Hainmueller 2010 and Appendix B.1), enables us to compare the post-reform electoral support for RWP parties in the UK with a synthetic control case. This synthetic UK is constructed to closely resemble the UK, prior to the 1999 electoral reform, both in terms of votes for RWP parties and in terms of predictors of RWP votes, from a pool of donor countries. In addition to pre-reform RWP vote shares, our

[1] Excluding Northern Ireland, which retained its single transferable vote system. The electoral rules for the House of Commons remained unchanged.

[2] Luxembourg is excluded a priori due to its size and unusual economic structure.

[3] The term *right-wing populist* refers to political parties that are populist (i.e., anti-elitist and anti-pluralist) and culturally conservative or exclusionary (Müller 2017; Golder 2016). It resembles what Norris and Inglehart (2019) call authoritarian populism. Other definitions used in the literature identify the same set of parties (see appendix A).

set of pre-reform characteristics includes public Euroscepticism, mainstream party positions on European integration from expert surveys, satisfaction with national democracy, unemployment rate, generosity of unemployment insurance, capital openness, Chinese import competition, immigrant inflows, and government partisanship (see Appendix A for data details).[4]

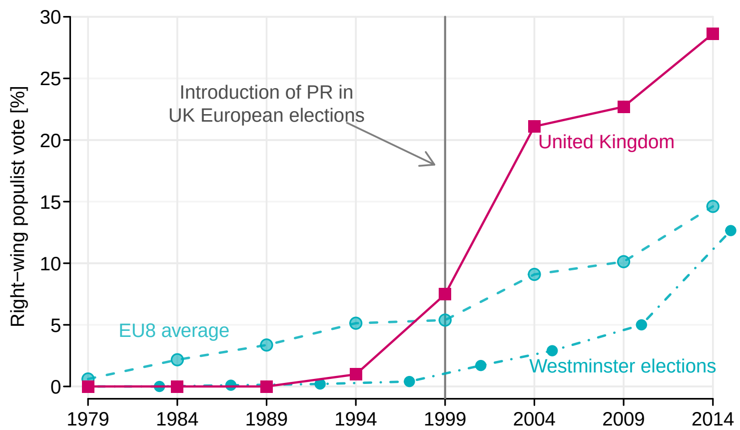

Figure I
Development of right-wing populist vote shares
This figure plots right-wing populist vote shares in elections to the European Parliament in the United Kingdom and the EU8. Vote shares in UK national (Westminster) elections are displayed for comparison.

Before turning to the results of the statistical analyses, it is instructive to consider two simpler comparisons. Figure I plots the evolution of the combined vote share of RWP parties in the UK in EP elections between 1979 and 2014 compared to an unweighted average of EU8 countries and UK national (i.e,. Westminster) elections. It shows that electoral support for RWP parties in the UK is virtually zero until 1994, when UKIP first entered the European electoral arena but received only one percent of the vote. With the introduction of PR in the 1999 EP election, the RWP vote in Britain increases more than seven-fold, and grows monotonically until the 2014 election. Comparing the UK's four pre-reform and post-reform EP elections reveals an average gap in RWP votes of 19.7 percentage points. This trajectory encompasses the spectacular growth of UKIP and the more modest growth of the BNP, which first entered European elections in 1999. However, assessing how much, if any, of this gap can be attributed to the reform requires

[4]Identifying a synthetic control to provide a plausible counterfactual for the post-reform UK entails simultaneously optimizing two sets of weights, one for countries and one for predictor variables. We apply a recently improved algorithm to solve this nested optimization problem (Becker and Kloessner 2018), which allows data on election results and pre-reform country characteristics to vary over time (for technical details, see Appendix B.1).

a plausible comparison case. The steep rise of RWP in UK European elections after the reform stands in contrast to the evolution in the EU8 as well as in Westminster elections.

The EU8 already experienced a noticeable rise in right-wing populism in EP elections before 1999. In the last election before the reform, the average RWP vote is already five times larger than in the UK. Thus, Figure I suggests that a simple EU8 average makes for an inadequate counterfactual comparison case. Westminster elections provide an intuitively appealing comparison case for EP elections in the UK that holds constant country and time-varying factors shaping populist demand. Indeed, pre-reform levels of support for RWP are practically identical in EP and Westminster elections, while diverge sharply after the electoral reform (increasing with a relatively steeper slope in the PR elections). Nonetheless, this within-country comparison has limitations. In particular, it does not account for the strategic interdependence of the two electoral arenas. The success of RWP in Europe may spill-over into national elections (Dinas and Riera 2018). Voters may also strategically balance higher support for RWP parties in EP elections with lower support in national ones (Carrubba and Timpone 2005). In the latter case, using Westminster as the comparison group *overstates* the impact of the electoral reform.

Figure II illustrates our main results using a synthetic control case. Panel (a) plots the evolution of the combined vote share of RWP parties in the UK in EP elections between 1979 and 2014 compared to the synthetic UK. It shows that the growth of RWP votes in the UK sharply diverges from the synthetic UK with the introduction of PR in 1999. In contrast to the EU8, the synthetic UK closely approximates pre-reform RWP votes in the UK. It is comprised of a weighted combination of the Netherlands, Ireland, and Belgium (see Table B.1). While countries (country weights) are chosen to optimally match synthetic and observed pre-reform trends, the resulting set of countries makes substantive sense as well.[5] They are highly open economies with strong trade and historical links to the UK. Similar to pre-reform UK, Ireland and Belgium have high effective electoral thresholds making the entry of new parties difficult (Farrell and Scully 2007: 75); in the Netherlands, the threshold is lower but not as low as in the national parliament, as the number of available seats is much smaller.[6]

With the introduction of PR, a gap of 6.9 percentage points sharply emerges between the UK and its synthetic counterpart, with an even larger gap in the following 2004 election. This

[5]Synthetic UK is a close match to the UK in all four pre-reform elections with a root mean squared prediction error of only 0.0072. France is excluded from the donor pool in Figure II due to missing immigration data. When France is included (and immigration inflows excluded), it gets zero weight and the effect estimate is slightly larger (see Figure B.3).

[6]In contrast to regression-based approaches, it is a feature of the SCM that country weights are zero for some potential control units. See Appendix B.2 for more information. Figure B.2 shows that our results are robust to changing the composition of country donors.

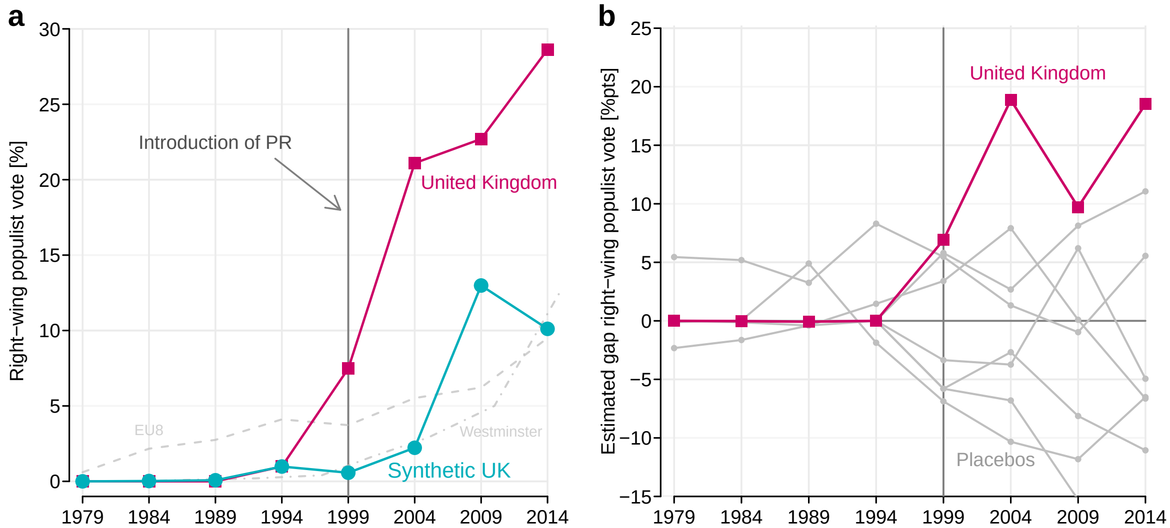

Figure II
The effect of electoral system change on the right-wing populist vote in Europe
Panel (a) plots right-wing populist vote shares in European Parliament elections in the UK (magenta line), which in 1999 replaced its first-past-the-post with a PR electoral system, compared to three control cases: 'synthetic' UK constructed via generalized synthetic control estimation (green line), the EU8 average and Westminster elections (dashed lines). Panel (b) plots the difference in populist right vote shares [%pts] between treated and synthetic control units. Grey lines plot gaps for placebo-treated units.

gap—shown by the bold line in panel (b) of Figure II— estimates the impact of the electoral reform on the electoral performance of the populist right in the UK. Averaging over all post-reform EP elections, the difference in RWP vote shares between the UK and its synthetic counterpart amounts to 13.5 percentage points. The magnitude of this change is substantively important and theoretically plausible. Our estimate is unlikely to be the result of chance alone. We create in-space placebo estimates by applying the SCM to each of the eight potential donor countries, assuming that 1999 is the placebo election. Placebo estimates are plotted as gray lines in Panel (b) and reveal that no country shows an estimated increase in RWP votes as large or larger than the UK in any of the post-reform elections.[7]

It is noteworthy that the synthetic UK sees a larger average growth in right-wing populism than Westminster elections, leading to a *smaller* estimate of the reform's impact. This comparatively conservative result illustrates the appeal of the method. Using information on pre-reform outcomes as well as predictors' of populist votes, SCM appears to better capture underlying structural changes.

[7] Thus, calculating a one-sided exact p-value for the average gap of 13.5 points would obviously yield $p < 0.000$. See Appendix Table B.2 for election-specific values.

Table I
Difference-in-difference estimates of electoral reform impact on RWP vote.

M1	Two-group, two-period DiD	14.34	(2.10)
M2	Multiple-period panel DiD	16.11	(2.09)
M3	Panel DiD, parallel trends conditional on covariates		
	Devolution (regional authority index)	15.26	(2.55)
	+ Economic Integration (capital openess, Chinese imports)	15.08	(2.24)
	+ Welfare generosity (unemployment insurance)	15.06	(1.77)
	+ Euroskepticism (citizen attitudes, party positions)	13.39	(1.87)
	+ Immigration inflows[a]	12.82	(1.72)

Notes: Estimates with standard errors in parentheses. *M1* is a two-period analysis using pre- and post-reform averages. N=18. Wild bootstrap SEs (5000 replicates, Rademacher weights). *M2:* Time-average ATT from multi-period DiD panel estimator (Callaway and Sant'Anna 2020). N=72. *M3* relaxes the parallel trends assumption conditional on covariates. Bootstrapped standard errors using 5000 replicates.

a Covers 1984 to 2014 and exludes France due to missing immigration inflow data.

Employing difference-in-difference (DiD) estimators, which make different identification assumptions, confirms the SCM results. Table I summarizes the DiD estimates.[8] Throughout, point estimates are similar to those from the SCM and statistically significant. Model *M1* contrasts differences between average RWP vote shares in the UK and EU8 before and after the reform. It accounts for time-invariant unobserved differences between the UK and other countries as well as common shocks, including treaty changes and factors that increase the demand or supply of populism. Results from a panel DiD estimator with multiple periods are very similar (Model *M2*). Next, we estimate several model specifications (*M3*) that relax the parallel trends assumption by conditioning on observed covariates (Callaway and Sant'Anna 2020). We sequentially add potentially fast-moving covariates: political decentralization, economic integration, welfare state generosity, Euroscepticism (attitudes and mainstream party positions) and immigration inflows. The most conservative point estimate from this flexible specification suggests that the reform increases the RWP vote by 12.8 percentage points, which is quite close to the average estimate from the SCM.

Our results have to be interpreted against a backdrop of rising support for RWP across Europe in general and more plausible comparison cases in particular. In the UK, reduced concerns about wasted votes and lower barriers to entry in EP elections since 1999 allowed this support to manifest itself at the ballot box in a way that was not possible in first-past-the-post elections. Our preferred

[8] See Appendix C for a more detailed discussion of model specifications, a comparison of identifying assumptions in SCM and DiD models, tests for pre-reform non-parallel trends (Table C.2), and a placebo analysis (Figure C.1).

estimates from SCM and DiD methods imply that the introduction of PR accounts for between one-half and two-thirds of the observed growth in RWP votes in the UK. The substantive impact of the reform is consistent with existing theory (Cox 1997) and experimental evidence (Hix, Hortala-Vallve and Riambau-Armet 2017) showing that introducing a PR system substantively relaxes incentives of voters and elites to coordinate on one of the two previously dominant parties. It is also well-aligned with evidence from an electoral reform in Norway showing that the introduction of PR reduced voter coordination against the Labour Party (Fiva and Hix 2020).

In additional analyses we find that the reform increased the effective number of electoral parties by about one (Table E.1). This increase in the number of effective parties is the flip side of reallocating votes from mainstream parties to the RWP challengers. We believe that the vote impact of the reform is unlikely to stem from the difference between closed-list and open-list PR. In the EU8, only Ireland and Italy have an open-list PR system and our results are robust to excluding them (see Figure B.2). The other countries give voters no or limited scope to change candidates' list placement (Farrell and Scully 2007: 77). We also find no evidence that the reform shaped EU attitudes directly (Appendix Table D.1).

Discussion

This article highlights a relatively neglected institutional factor behind the recent rise of right-wing populism in the European electoral arena: the adoption of a proportional electoral system for European elections in the UK in 1999. We find that this electoral system change entailed a sizable increase in the vote share of RWP parties. Our findings contrast with the common view in the populism literature that electoral rules play at best a modest role. Caution is needed when existing empirical results based on partial reforms are extrapolated to electoral system change.

The reform we studied concerns European elections, which are considered second-order to national parliamentary elections. However, this does not mean that they are irrelevant for national politics, which they might effect via spillovers (e.g., Dinas and Riera 2018). Relatedly, the decision of Conservative party leader David Cameron to hold a referendum on a British withdrawal from the European Union has been explained by several scholars as "an attempt to stem rising support for the Eurosceptic populist UKIP"' (Norris and Inglehart 2019: 371).

Our findings do not condemn the use of PR in European elections. Proportional electoral systems can serve an important voice function and electoral system design usually requires an evaluation of trade-offs. Indeed, there is evidence that the introduction of PR in the UK has increased ideological congruence between voters and legislators, though at the cost of lower legislative effort (Becher and Menéndez González 2019). Nor do our results imply that first-past-the-post systems are free of populism: it being more difficult for new RWP parties to become successful, political entrepreneurs might try to capture a mainstream party (as some observers have argued happened in the U.S.).

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Online Appendix for "Proportional Representation and Right-Wing Populism: Evidence from Electoral System Change in Europe"

Contents

A. Data details

Parties and votes Table A.1 lists the right-wing populist parties included in the study. Recall that our study covers the nine EU member states, including the UK, that participated in all European elections between 1979 and 2014. As noted in main text, Luxembourg is excluded due to its size. The Synthetic control analyses furthermore exclude France, since no pre-reform data on immigration inflows is available. Subsection B.4 below provides synthetic control results when including France (and excluding immigration).

The terminology in the literature varies and, perhaps unavoidably, concepts remain somewhat contested (Golder 2016; Kitschelt 2007; Mudde 2007; Müller 2017; Muis and Immerzeel 2017; Zulianello 2020). But there is considerable conceptual overlap between different approaches, and empirically the literature broadly agrees in terms of identifying the universe of cases that fall under the umbrella of right-wing populism in the countries under study. Following Müller (2017) and similar to several other scholars, *right-wing populist* parties are defined as political parties that are populist (i.e., anti-elitist and anti-pluralist) and culturally conservative or exclusionary (Golder 2016). The core claim of populist parties is that "only some of the people are really the people. Think of Nigel Farage celebrating the Brexit vote by claiming it had been a 'victory for real people' " (Müller 2017: 22). This definition of right-wing populism closely resembles what Norris and Inglehart (2019) call authoritarian populism and Mudde (2007) calls radical populist right. It is intentionally broad rather than narrow (Kitschelt 2007) and identifies core elements present in all expressions of right-wing populism.

Empirically, right-wing populist parties are identified based on the agreement of two sources: (1) the Comparative Political Data Set (Armingeon et al. 2018: A3-4), which identifies right-wing populist parties from 1960 to 2016; (2) the list of authoritarian-populist parties based on the analysis of expert surveys in a two-dimensional space by Norris and Inglehart (Norris and Inglehart 2019: Table 7.2). The resulting list is also identical to the recent compilation of Zulianello (2020), except that the latter does not cover the earlier years of our study. Data on vote shares in European elections is from the Parliaments and Governments database (Döring and Manow 2019). Their dataset is also used to calculate the effective number of electoral parties used as an additional outcome variable in OA Table C.4.

Table A.1 shows that in Ireland and in the UK before 1994 the sources identify no viable RWP party competing in elections. This is substantively meaningful information that we use in the analysis. In the pre-reform UK, possible entrants trying to compete in the single-member district system faced a high electoral threshold. Similarly, Ireland is a case where the effective electoral threshold under its STV system—17.4 on average in the 2004 election (Farrell and Scully 2007: 75)—is higher than in most other countries represented in the European Parliament because it uses four electoral districts to elect a total of 15 MEPs. This imposes high hurdles for the entry of any new party. Theoretically, the effect of electoral system reform on vote shares may be driven by the extensive margin (i.e., party entry) and/or the intensive margin (i.e., the

Table A.1
Right-Wing Populist Parties and EU countries identified by various sources, 1979-2014.

	Party Name (English)	Abbreviation
Belgium	Flemish Bock	VB
Belgium	National Front	FN-NF
Denmark	Danish People's Party	DF
France	National Front	FN
Germany	Alternative for Germany	AfD
Germany	National Democratic Party	NPD
Germany	Republicans	REP
Germany	German People's Union	DVU
Greece	Independent Greeks	AE
Greece	People's Association – Golden Dawn	XO
Greece	Popular Orthodox Rally	LAOS
Ireland	none	
Italy	Northern League	LN
Italy	National Alliance (formerly Social Movement, MSI-DN)	AN
Italy	Brothers of Italy - National Centre-right	FdI-CN
Netherlands	Centre Democrats	CD
Netherlands	List Pim Fortuyn	LPF
Netherlands	Party for Freedom	PVV
UK	UK Independence Party	UKIP
UK	British National Party	BNP
UK	Anti-Federalist League (only Westminster 1992)	
UK	Referendum Party (only Westminster 1997)	

Note: See Online Appendix section A for the definition of right-wing populist parties and related literature. Empirically, parties are identified based on the agreement of two sources: Armingeon et al. (2018) and Norris and Inglehart (2019). While there are different conceptual treatments, there is broad agreement on the set of core RWP parties for the elections under study (1979 to 2014). Data on vote shares in European elections is from the Parliaments and Governments database (Döring and Manow 2019).

growth of an initially small or niche party). In game theoretic models, a party's decision to enter the electoral arena is endogenous to beliefs about its vote-winning potential given the existing party system, electoral rules and voting behavior (Cox 1997: ch. 8-9). That is, having no RWP party competing in a given election is theoretically and substantively meaningful. In election-years without a RWP party competing, their vote share is zero.

Other variables Information on electoral institutions for the European Parliament are from European Parliament Directorate General for Research (1997, 1999). In addition, Farrell and Scully (2007: ch.4) provide an excellent overview. Sources for covariates are as follows:

- Bi-annual survey data on Euroscepticism and Satisfaction with national democracy are from the European Commission's online database Eurobarometer Interactive (https://ec.europa.eu/commfrontoffice/publicopinion).
- The measure of Chinese import competition is from the Atlas of Economic Complexity (The Growth Lab at Harvard University 2019).
- The regional authority index (Marks et al. 2008) used in our DiD models is a summary measure of the authority of regional governments (for subnational units with a population of at least 150,000). It is obtained from the updated database of Hooghe et al. (2016).
- Mainstream party position on European integration are calculated from the Chapel Hill Expert Survey trend file (Bakker et al. 2020), for 1999-2014, and the earlier Ray and Marks/Steenbergen Party Dataset, for 1979-1994 (both retrieved from https://www.chesdata.eu/). Each dataset contains a variable measuring the overall orientation of the party leadership towards European integration, with responses ranging from 1 (strongly opposed) to 7 (strongly in favor). We calculate the mean for mainstream parties in a given country and year, using years close to the next EP election.
- Data on immigration inflows in each EP election period are from the United Nations Department of Economic and Social Affairs (UNDESA) immigration flow database (United Nations Populations Division 2015). Inflows are defined as the number of individuals (irrespective of their citizenship status) seeking to establish residence in the destination country in a given year. We aggregate yearly flow data to EP election periods. While this database provides us with the most comprehensive coverage, it lacks information for France in the pre-reform years. The consequence of this is that our synthetic control analysis reported in the main text excludes France. We provide an alternative analysis with France included in appendix B.4 and find no substantive difference in results. The panel difference-in-difference models reported in the main text indicate instances where France is excluded.
- Our remaining economic and political controls are obtained from the Comparative Political Data Set (Armingeon et al. 2018).

If a covariate time series in a specific country has missing observations in a given year, we interpolate (predict) the time series using a Kalman filter from a flexible, country-specific local linear trend time series model (Harvey 1990).

B. Synthetic control analysis

Abadie et al. (2010) provides an excellent introduction to the synthetic control method (SCM). This exposition follows Becker and Kloessner (2018) and highlights that the SCM can be implemented using predictor variables at a more disaggregated time scale (i.e., we have covariates that vary within election periods) and employing an improved algorithm for calculating the required weights.

B.1. Method

Denote by Y_{tj} the right-wing populist vote share for country j out of $J+1$ countries at election t ($t = 1, \ldots, T$). The treated country (United Kingdom) is denoted by $j = 1$, the remaining $J = 8$ countries are possible donors for a synthetic UK. Denote the number of pre-reform elections by T^{pre} so that $Y_{1j} \ldots Y_{T^{pre}j}$ are observed prior to the reform, and $Y_{T^{pre}+1,j} \ldots Y_{Tj}$ are observed subsequently.

A SCM estimator models the effect of the reform using a weighted linear combination of optimally chosen donor countries representing counterfactual outcomes that would have obtained absent the change of electoral rules. More precisely, it generates $\hat{Y}_{T^{pre}+1,j} \ldots \hat{Y}_{Tj}$ which approximate the unobserved counterfactual post-reform outcomes $\tilde{Y}_{T^{pre}+1,j} \ldots \tilde{Y}_{Tj}$. In the post-reform period the effect of the reform on the tth observation (with $t > T^{pre}$) is given by $Y_{t1} - \tilde{Y}_{t1}$ approximated empirically by $Y_{t1} - \hat{Y}_{t1}$.

This empirical approximation is achieved by weighting donor countries by a vector of weights $W = (w_2, \ldots, w_{J+1})'$ (with weights constrained to be non-negative and sum to unity). The approximated outcome for $\tilde{Y}_{t1}$ is obtained as

$$\hat{Y}_{t1}(W) = \sum_{j=2}^{J+1} w_j Y_{tj} \tag{1}$$

The effect of the reform δ_t can then be estimated by the difference between actual and synthetic post-reform outcomes

$$\hat{\delta}_t(W) := Y_{t1} - \hat{Y}_{t1}(W). \tag{2}$$

Optimal weights would minimize the approximation error $\tilde{Y}_{t1} - \hat{Y}_{t1}(W)$. As the first term is an (unobservable) counterfactual, Abadie et al. propose to pursue two objectives. First, pursue the best possible pre-reform approximation $Y_{t1} - \hat{Y}_{t1}(W)$ by minimizing the root mean squared error

$$e_Y(W) := \sqrt{\frac{1}{T^{pre}} \sum_{t=1}^{T^{pre}} \left(Y_{t1} - \sum_{j=2}^{J+1} Y_{tj} w_j \right)^2}. \tag{3}$$

Second, in order for the post-reform counterfactual values to be approximated well, ensure that the synthetic controls also approximate a set of K variables that are predictive of right-wing vote shares . Denote by X_{ksj} the observed value of variable $k (k = 1, \ldots, K)$ for country $j (j =$

$1, \dots, J+1$) at time $s(s = 1, \dots, S_k)$. We use a different time index to signify that these variables may be available at a more disaggregated time scale. The difference between observed values for the United Kingdom and its synthetic control approximation are given by $X_{ks1} - \sum_{j=2}^{J+1} X_{ksj} w_j$ for each covariate. Then the second quantity to be minimized is given by

$$e_X(V, W) := \sqrt{\sum_{k=1}^{K} v_k \frac{1}{S_k} \sum_{s=1}^{S_k} \left(X_{ks1} - \sum_{j=2}^{J+1} X_{ksj} w_j \right)^2}. \tag{4}$$

Here v_k are non-negative weights (collected in $V = (v_1, \dots, v_K)'$ which capture the relative importance of each variable in predicting vote outcomes. Note that this criterion is purely *predictive*, not a reflection of the causal role of these variables. In our application, X includes changing political and economic conditions (such as, among others, support for the EU, unemployment rates, and Chinese import exposure) as well as lagged outcomes. Below, we present sensitivity analyses showing that results are not sensitive to the choice of pre-reform characteristics or the sequential exclusion of countries in the donor pool. As the lagged outcome variable, we include RWP vote shares in 1994 in order to capture the general upward trend in populist votes.

In terms of identifying the effect of interest, the smaller $e_X(V, W)$ and $e_Y(W)$ the smaller is the potential bias of the estimated $\hat{\delta}_t(W)$. See Abadie et al. (2010) for a detailed discussion of how $e_X(V, W) = e_Y(W) = 0$ enables the estimation of the effect of interest. To estimate this model (see Becker and Kloessner (2018) for full details), define the function W^* which maps covariate weights v_k onto weights for donor units minimizing the approximation error of covariates: $W^*(V) := \arg\min_W e_X(V, W)$. Then use W^* to define the corresponding approximation error for right-wing votes:

$$e_Y^*(V) := e_Y(W^*(V)) \tag{5}$$

To optimally determine V minimize (5) above w.r.t. $v_1, \dots, v_K$ which produces optimal covariate weights $v_1^*, \dots, v_K^*$ and country weights $W^*(v_1^*, \dots, v_K^*)$.

B.2. Synthetic control details and results

Table B.1 shows the weights used in the SCM. Panel (a) displays weights attached to potential donor countries, panel (b) displays the weights assigned to covariates. Synthetic UK is predominantly composed of the Netherlands and Ireland. This deserves a couple of comments. First, we note that it is not unusual for the SCM to choose only a limited set of units from the pool of possible donors. For example, the study of Fowler (2013) uses SCM to examine the impact of the adoption of compulsory voting in Australia on turnout and social policy: In the analysis of pension spending, 2 out of 20 OECD countries receive most weight; in the analysis of turnout, 4 out of 20 countries receive most weight.[1] This is a feature, not a bug, of the approach that differs from traditional regression-based methods.

Secondly, the selection of Ireland and the Netherlands as key components of a synthetic UK is plausible. They are highly open economies with historical trade and political links to the UK. Ireland was the second Anglo-Saxon country in the EU at the time of the reform. As already noted, its electoral system—STV in small multi-member electoral districts—entails a high effective electoral threshold for new parties, similar to Britain's pre-reform system. While the effective threshold was lower in the Netherlands than in Ireland and the UK, it is politically relevant and high compared to national elections because of the the lower number of seats available to be allocated in the European electoral arena. Thus, the electoral weakness of RWP challengers in these countries in the early 1990s is consistent with institutional explanations. Similar to the UK, both countries were highly exposed to demand factors such as Chinese import competition and, subsequently, labor market competition from the EU's Eastern enlargement. In the Netherlands, a popular vote in 2005 soundly rejected the treaty to establish a Constitution for the EU. The country also saw a considerable rise of RWP parties, in particular the Party for Freedom (PVV). Moreover, the Netherlands does not use an open-list PR system but an ordered system in which there is only "limited scope for candidates to improve their list placement through personal votes" (Farrell and Scully 2007: 77). Recall that among the countries under study, only two (Ireland and Italy) have an open-list PR system (Farrell and Scully 2007: ch. 4), while the remaining ones employ a closed-list or ordered list system with limited scope for voters to change candidates' ranking on the party list.

That said, thirdly, the fact that some countries receive a zero weight in our baseline SCM does *not* mean that they are not a useful comparison case. It simply means that the method has found another combination of country weights that approximates pre-reform UK outcomes better. In line with this, we show below that our results are robust to sequentially excluding each possible donor country in turn. Our results are not driven by Netherlands or Ireland, though they get most of the weight because they provide the best fit for the pre-reform UK.

Panel (b) shows that the key predictor in X is the RWP vote share in 1994. This makes sense given the divergent trend in RWP that had emerged at the time. In previous applications of SCM, the lagged outcome variable often receives most weight (Becker and Kloessner 2018). It is important to note that this stage of the analysis is not a horse race between competing supply side explanations. Weights on covariates are determined solely by their potential to decrease a predictive criterion (cf. equation (4)). See Botosaru and Ferman (2019) for a detailed discussion of the role of covariates in synthetic control estimation. Substantively, these results simply indicate that beyond their impact on the strength of RWP parties in 1994, additional variables are not needed to construct a counterfactual UK. Below we show that alternative specifications that force different weight combinations on covariates produce comparable substantive results.

[1] Similarly, on country weights in other application, see for instance analyses of German reunification (Abadie et al. 2015) or terrorism in the Basque country (Abadie and Gardeazabal 2003; Becker and Kloessner 2018).

Table B.1
Entries of $W^*(V)$ and V matrices: estimated final weights of countries and additional predictor variables. Root mean squared error of approximation

(a) $W^*(V)$

Country	w_j
Belgium	0.018
Denmark	0.000
Germany	0.000
Greece	0.000
Ireland	0.229
Italy	0.000
Netherlands	0.753
RMSPE	0.0072

(b) V

Variable	v_k
Dissatisfaction with EU	0.000
Satisfaction with national democracy	0.000
Unemployment rate	0.000
Unemployment insurance generosity	0.000
Capital openess	0.000
Chinese imports	0.001
Right government share	0.000
Avg. party position EU policy	0.000
Migration inflow	0.000
RWP vote share in 1994	0.999
RMSPE	0.0383

Table B.2 provides estimates for the reform effect for each post-reform election with corresponding one-sided p-values based on placebo reform assignments to EU8 countries as shown in Figure 2 in the main text. Inference from synthetic controls has to be based on placebo tests, and when interpreting our p values one should keep in mind the small size of the sample. Nonetheless, Table B.2 signifies that the magnitude of the RWP vote share in the United Kingdom exceeds that of placebo-reform EU8 countries in every post-reform election.

Table B.2
Difference between United Kingdom and synthetic control for each post-reform election. One-sided placebo p-values

	European Election				
	1999	2004	2009	2014	1999-2014
$\Delta(Y_{1t}, Y_{0t})$	6.9	18.9	9.7	18.5	13.5
p-value	0.000	0.000	0.000	0.000	0.000

B.3. Specification tests

In the following two figures we present two sets of sensitivity analyses. In Figure B.1 we examine the influence of the choice of specific covariates. As our previous discussion indicated, the aim of SCM estimation is to minimize an objective criterion (RMSPE) not to produce a subjectively "meaningful" set of covariate weights. But a critical reader might nonetheless worry about the inclusion or exclusion of specific covariates, which changes the calculation of optimal weights, affects our conclusion (see Ferman et al. 2020 for a discussion of "cherry picking" of SCM covariates). We address this issue by forming *all* possible combinations of covariates in X and re-estimating the model. Figure B.1 plots estimates from 116 models representing various possible covariate combinations. Note that we excluded estimates from models that were decidedly worse in terms of their match between the synthetic control group and the pre-reform data (where the ratio of $e_X(V, W)$ relative to the main model is > 20).

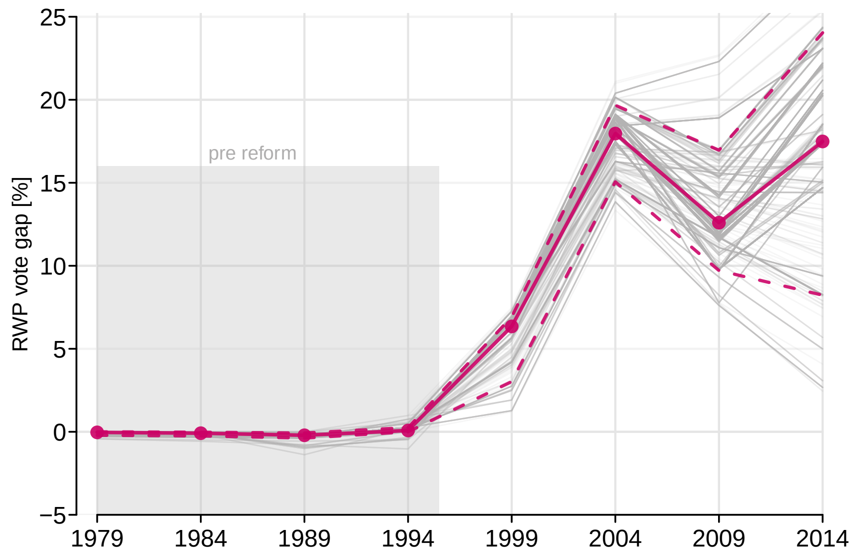

Figure B.1

Sensitivity analysis of synthetic control predictors. Average estimate and 95% bounds.

This figure plots RWP vote share gaps estimated from a series of models using all possible combination of covariates in the SCM estimator. The bold magenta line represents the average of all estimated models; dashed lines represent 95% bounds based on 116 estimates of possible covariate combinations. The bounds and average estimate signify that the key SCM result does not depend on the specific combinations of predictors added to the model. They show a pattern similar to the result in the main text: a clear increase in the RWP vote with the introduction of PR with an even greater increase in 2004.

Figure B.2 studies the impact of removing a country from the synthetic control donor pool. Removing a country induces different country weights as well as different weights on covariates. As a result, the approximation of the synthetic UK case to its observed counterpart in the pre-reform period can vary considerably (for example, it is worse when excluding the

Netherlands from the donor pool). Despite variation in the closeness of the approximation, we find the core pattern of our results generally confirmed. All panels of Figure B.2 show a close match between the United Kingdom and its synthetic counterpart in the pre-reform period.

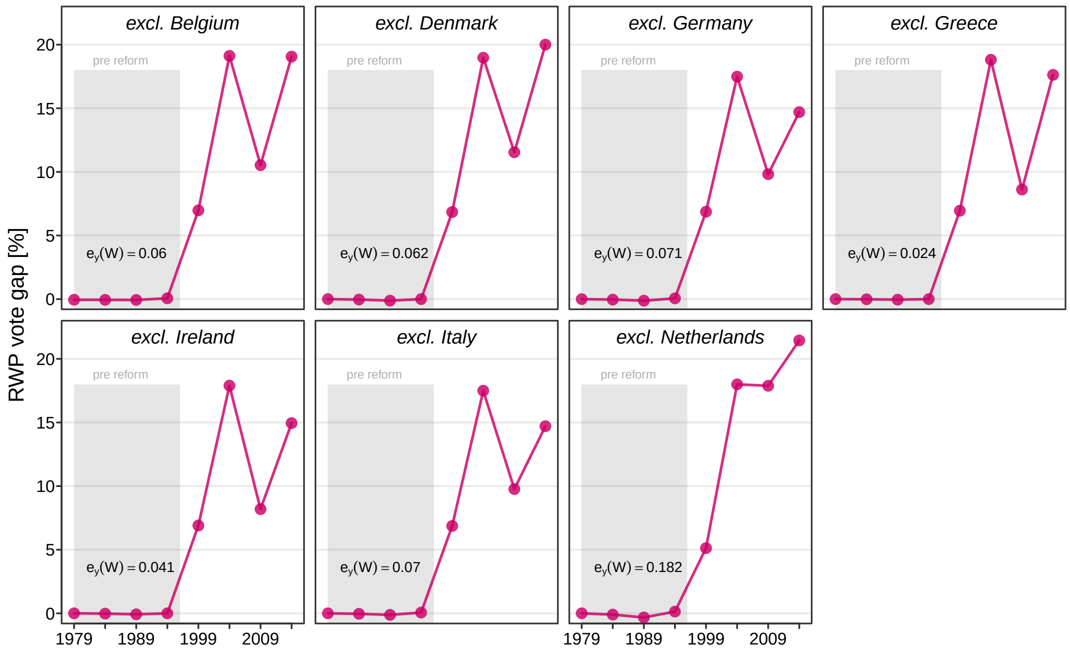

Figure B.2
Resulting SCM estimates when excluding a country from donor pool

This figure plots estimates from models excluding countries from the set of possible synthetic control donors. The magenta line shows resulting estimates. $e_Y(W)$ shows the root mean squared error of approximation in the pre-reform period.

B.4. Extended sample including France

Our SCM analyses reported in the main text excludes France due to missing pre-reform observations of immigration inflows. In this subsection, we present an alternative analysis that includes France (and consequently excluded immigration as a covariate). The right panel of Figure B.3 shows synthetic control estimates when including France while the left panel simply replicated the synthetic control used in the main text. A quick visual comparison of both panels reveals that the broad pattern of results remains unchanged. More specifically, results for the first two post-reform elections in 1999 and 2004 are virtually identical. In later elections (2009 and 2014) we obtain lower synthetic control values when excluding immigration inflows. Thus, the implied gap between the UK and its synthetic counterpart is somewhat larger. In this sense, the specification including immigrations is slightly more conservative and we thus chose it as the one presented in the main text.

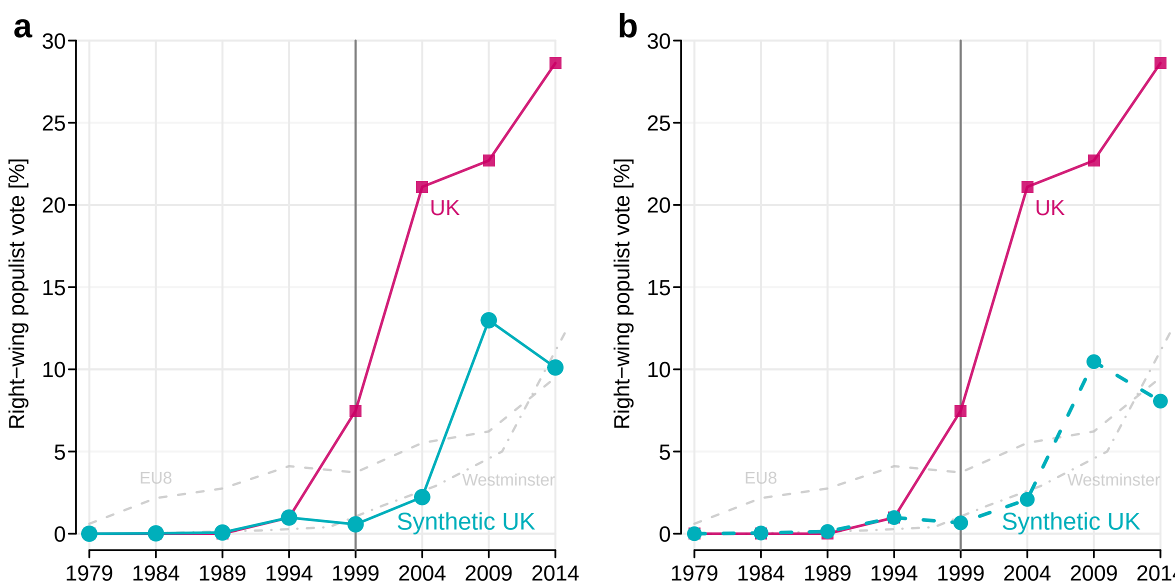

Figure B.3

Comparison of synthetic control UK estimates when including France.

Panel (a) shows the synthetic UK used in the main text, which includes immigration inflows as a covariate (which necessitates the exclusion of France). Panel (b) shows the synthetic UK estimate obtained when including France (and excluding immigration inflows as a covariate).

C. Difference-in-difference analyses

We conduct a set of additional analyses using difference-in-difference (DiD) specifications to complement our SCM results. These provide estimates of the electoral reform impact using different identifying assumptions. The following section discusses the difference in assumptions made between the synthetic control approach and DiD estimates.

Before doing so, we present a simple difference in means comparison between the UK and the EU8. Table C.1 shows mean support for populist parties [in percent] in the United Kingdom and the EU8 average before and after the reform in the UK, as well as the pre-post difference and the difference-in-differences. This calculation on the raw data shows the relatively larger magnitude of the change in the UK compared to the EU where the electoral rules stay constant. The difference of the differences (shown in the bottom right of Table C.1) provides an estimate close to the the baseline two-period, two-group DiD regression reported in the main text.

Table C.1
Support for right-wing populist parties (in percent) in the United Kingdom and EU8 before and after UK reform. Means and differences with standard errors in parentheses.

	Before reform	After reform	Difference
EU8	3.1	8.5	5.4
	(1.1)	(1.1)	(1.6)
United Kingdom	0.2	20.0	19.7
	(3.1)	(3.1)	(4.4)
Difference	−2.9	11.4	*14.3*
	(3.3)	(3.3)	*(4.7)*

C.1. Identifying assumptions in SCM and DiD approaches

This subsection discussed differences in identifying assumptions between synthetic control models and difference-in-difference analyses. We use a simple (parametric) model to focus on the core ideas. For each country i ($i = 1, \dots, n$) we observe T time periods, of which $t_1, \dots T_0$ are pre-reform periods and $T_0 + 1, \dots, T$ are post-reform. Denote by Y_{it}^1 and Y_{it}^0 the respective potential outcomes (vote shares) of country i at time t under either electoral reform or no-reform conditions. Reform status is denoted by an indicator variable, D_{it}, which is equal to 1 if country i at time t is exposed to the electoral reform and 0 otherwise. The potential outcome absent an electoral reform can be written as (Angrist and Pischke 2008):

$$Y_{it}^0 = X_{it}\beta + \lambda_t \mu_i + \delta_t + \epsilon_{it}. \tag{6}$$

Denoting by τ_{it} the (additive) effect of the reform, the potential outcome in the reform state is:

$$Y_{it}^1 = X_{it}\beta + \lambda_t\mu_i + \delta_t + \tau_{it} + \epsilon_{it}. \tag{7}$$

Here, X_{it} is a vector of observed covariates, μ_i are unobserved country characteristics which are time-constant but can have time-specific impacts captured by their associated coefficients λ_t; δ_t captures common time shocks; and ϵ_{it} are unobserved idiosyncratic shocks.

If we assume that the reform only affects subsequent elections in the country implementing it, we can write observed vote shares by the well-known switching equation:

$$Y_{it} = D_{it}Y_{it}^1 + (1 - D_{it})Y_{it}^0. \tag{8}$$

We are interested in the average treatment effect on the UK in each post-reform period:

$$\tau_t = E(Y_{it}^1 - Y_{it}^0 | D_{it} = 1). \tag{9}$$

If assignment to 'reform status' and vote shares are both influenced by μ_i then unobserved country characteristics are a confounder that may bias the estimated effect of the reform. More precisely, bias occurs when μ_i is imbalanced between the UK and other countries and $\lambda \neq 0$. Estimating τ_t necessitates making an assumption about Y_{it}^0—the outcome that would have occurred absent the reform. Since this is an *unobservable* quantity, the validity of this identifying assumption cannot be verified. Thus, our aim here is to employ two different assumptions and examine to what extent the resulting effect estimates agree. The two assumptions discussed next differ in what has to be conditioned upon in order to ensure that potential outcomes in the control condition Y_{it}^0 are independent of reform assignment.

Parallel trends assumption Assume that the change in Y^0 from time period t to t' is independent of the assignment to the reform country (UK) or unchanged countries (EU8), after conditioning on observable variables Abadie (2005): $E(Y_{it}^0 - Y_{it'}^0 | D_{it} = 1, X_{it}) = E(Y_{it}^0 - Y_{it'}^0 | D_{it} = 0, X_{it})$. In terms of our model above (see eq.6), unobserved country characteristics μ_i maybe be imbalanced between reform and non-reform groups, but their effect has to be constant over time ($\lambda_t = \lambda$). More compactly, we write

$$Y_{it}^0 \perp D_{it} | (X_{it}, t, \lambda\mu_i) \tag{10}$$

which encodes the assumption that the outcome in the control condition is independent from reform assignment after conditioning on observed covariates and both time and country fixed effects. The difference-in-difference analysis presented in the main text relies on this assumption (although it can be somewhat weakened with more sophisticated specifications).

Conditional independence (or ignorability) assumption A different assumption is that the expected values for the potential control outcome Y_{it}^0 for both reform and non-reform countries is the same in expectation after conditioning on observed covariates and *past outcomes* (Angrist and Pischke 2008). More formally,

$$Y_{it}^0 \perp D_{it} | (X_{it}, Y_{ih}^0). \tag{11}$$

Here, Y^0_{ih} is a vector of potential outcomes in h time periods before T_0. This assumption implies that countries with similar outcomes before the reform (Y^0_{it}, $t = 1, \ldots, T_0$) are expected to have similar potential control outcomes after it (Y^0_{it}, $t = T_0 + 1, \ldots, T$) after conditioning on covariates. The synthetic control approach used in our main text relies on this assumption.

C.2. Test for pre-reform non-parallel trends

Table C.2 tests for non-parallel trends in the pre-reform period. The test is based on two-way fixed effects models (with country and time fixed effects) including reform-time interactions in linear and quadratic form. We calculate cluster-robust p-values for these interactions specified without and with covariates. Overall, we find no evidence to reject the null hypothesis of parallel trends, especially when conditioning on the set of covariates we use in Table I in the main text.

Table C.2
Tests for pre-reform period non-parallel trends

	Linear trend		Quadratic trend	
	p_{rob}	p_{wild}	p_{rob}	p_{wild}
Test without covariates	0.102	0.034	0.429	0.324
Test with covariates	0.483	0.171	0.911	0.672

Note: Test based on generalized DiD model with country and time fixed effects. Entries are p values from tests of reform × linear time-trend and reform × quadratic time-trend interactions calculated using robust (HC3; p_{rob}) and cluster-wild-bootstrapped (p_{wild}) standard errors. Covariates include a regional authority index, capital openness index, Chinese import penetration, the generosity of unemployment insurance, Euroskepticism among citizens, and average party position on EU (of non-RWP parties).

C.3. Placebo DiD analysis

Figure C.1 plots the distribution of estimates from a placebo difference-in-difference analysis. We use specification M3 of Table I including devolution, economic integration, welfare generosity, and Euroscepticism and successively assign reform status to EU8 countries that did not actually experience a switch to PR. The median of placebo estimate is −4.3 percentage points, which is of the opposite sign of the UK estimate and more than three times smaller in terms of absolute size. Furthermore, the estimate of for the UK is clearly far in the tails of the distribution of placebo estimates. Thus, our placebo analysis suggests that the estimated size of the impact of the UK reform is unlikely to be found by chance.

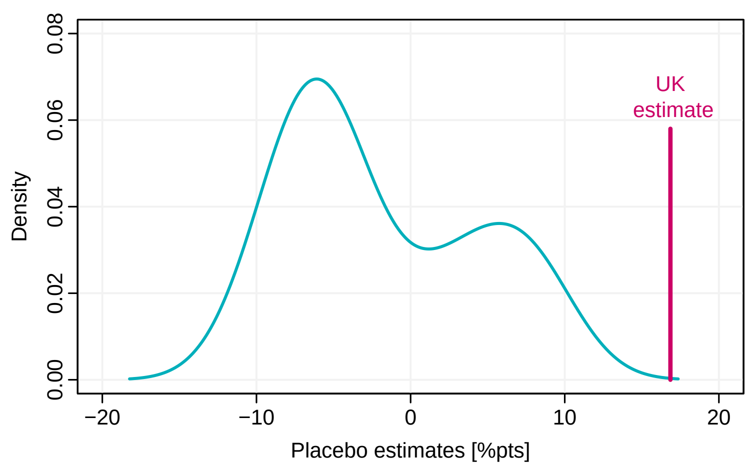

Figure C.1

Placebo difference-in-difference analysis.

Placebo estimates are generated by successively assigning reform status to non-reform countries. The median of placebo estimates is −4.3 percentage points. The figure visually illustrates the distribution of placebo estimates (using a Gaussian kernel density smoother) compared to the UK estimate. It suggests that the estimated size of the effect of the UK reform is unlikely to be found by chance.

D. Electoral reform and Euroscepticism

Table D.1 tests whether the reform had an effect on public attitudes toward the EU, measured as the share of respondents in a Eurobarometer survey who say that EU is a bad thing for their country, using the same difference-in-difference estimators employed for the analysis of RWP votes in the main text. Canonical theories of strategic electoral coordination under alternative electoral institutions imply that the reform made it much easier for voters to switch their support to new political parties critical of the political establishment without wasting their vote. These theories assume that political preferences are relatively stable and highlight that changing the rules of the game affects the strategic behavior of parties and voters. In reality, preferences may be affected as well. While a full analysis of the mechanisms requires individual-level panel data that are not available, the additional analyses using aggregate data summarized in Table D.1 do not reveal evidence that the reform changed public preferences toward the EU. We find no strong or statistically significant effect of the reform on aggregate public opinion toward the EU. This suggests that the reform did not work mainly through altering mass preferences on the European Union and is consistent with the strategic coordination channel of institutional theories.

Table D.1
Difference-in-difference estimates of electoral reform and Euroscepticism.

M1	Panel DiD (assuming parallel trends)	0.322	(0.196)
M2	Panel DiD, parallel trends conditional on covariates		
	Devolution (regional authority index)	0.335	(0.244)
	+ Economic Integration + welfare generosity	0.115	(0.189)
	+ Immigration inflows	0.137	(0.362)

Notes: Estimates with standard errors in parentheses. *M1:* Time-average ATT from multi-period DiD panel estimator. N=72. *M2* uses an estimator relaxing the parallel trends assumption conditional on covariates (Callaway and Sant'Anna 2020). Covariates include a regional authority index, capital openness index, Chinese import penetration, generosity of unemployment insurance, and immigration inflows (the latter limits the analysis period to 1984–2014 and excludes France due to missing immigration inflow data). See appendix for more details. Bootstrapped standard errors using 5000 replicates.

E. Electoral reform and the effective number of parties

In additional difference-in-difference analyses displayed in Table E.1, we use the effective number of electoral parties (ENEP) in EP elections as the dependent variable. It is calculated based on vote shares from the Parliaments and Governments database (Döring and Manow 2019). ENEP has been widely studied in the literature and does not require analysts to classify parties as populist or not. As noted in the main text, theories of electoral coordination imply that the electoral reform under study makes the electoral system more permissive to the entry and growth of new parties. Under the uncontroversial assumption that party competition under the original first-past-the-post system involves meaningful national issues and party labels, for instance, Gary Cox's work implies that the reform increases the upper limit on the viable number of parties (Cox 1997). Indeed, this is what we find using the same flexible model specification, which relaxes the parallel trends assumption, used to study RWP votes. The estimates in Table E.1 suggest that the switch to PR increased the effective number of electoral parties by approximately one. This holds whether covariates are included or not. Consistent with the theory, this finding indicates that impact of the reform on RWP votes reflects an increase in the size of the party system, not the substitution of one party by another.

Table E.1
Difference-in-difference estimates of effective number of electoral parties in post-reform election.

M1	Panel DiD (assuming parallel trends)	0.946	(0.336)
M2	Panel DiD, parallel trends conditional on covariates		
	Devolution (regional authority index)	1.036	(0.320)
	+ Economic Integration + welfare generosity	0.991	(0.327)
	+ Immigration inflows	1.083	(0.219)

Notes: Estimates with standard errors in parentheses. *M1:* First period ATT from multi-period DiD panel estimator. N=72. *M2* uses an estimator relaxing the parallel trends assumption conditional on covariates (Callaway and Sant'Anna 2020). Covariates include a regional authority index, capital openness index, Chinese import penetration, generosity of unemployment insurance, and immigration inflows (the latter limits the analysis period to 1984–2014 and excludes France due to missing immigration inflow data). See appendix for more details. Bootstrapped standard errors using 5000 replicates.

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