The Economics of State Fragmentation - Assessing the Economic Impact of Secession

Jo Reynaerts and Jakob Vanschoonbeek

KU Leuven, Faculty of Economics and Business, VIVES

22 February 2016

THE ECONOMICS OF STATE FRAGMENTATION: ASSESSING THE ECONOMIC IMPACT OF SECESSION

Jo Reynaerts* and Jakob Vanschoonbeek*, †

VIVES Discussion Paper 52. This version:
December 11, 2018

Abstract

This paper provides empirical evidence that declaring independence significantly lowers per capita GDP based on a large panel of countries covering the period 1950-2016. To do so, we rely on a semi-parametric identification strategy that controls for the confounding effects of past GDP dynamics, anticipation effects, unobserved heterogeneity, model uncertainty and effect heterogeneity. In a difference-in-difference setting, we demonstrate that 30 years after newly formed states declared independence, their inhabitants typically experience per capita GDP levels which lie 23% below those of countries which in all relevant aspects most closely resembled their own country's economic situation just prior to independence. We subsequently propose a novel quadruple-difference bias correction procedure to demonstrate the robustness of these findings. Finally, we develop a two-step estimator to shed some light on the primary channels driving our results. We find tentative evidence that the adverse effects of independence decrease in population size, pointing to the presence of economies of scale, and that they are also mitigated when newly independent states avoid violent secession, liberalize their trade regime or use their new-found political autonomy to democratize. We fail to find clear-cut evidence of the relevance of macroeconomic uncertainty or the economic desirability of declaring independence by referendum.

Keywords: Independence dividend; panel data; dynamic model; synthetic control method; difference-in-difference; triple-difference; quadruple-difference; control function approach

JEL Classification: C14, C32, H77, O47

*KU Leuven - University of Leuven, Faculty of Economics and Business, Research Centre for Regional Economics
†Ph.D. Fellow of the Research Foundation - Flanders (FWO)

The authors thank Jože Damijan, Jan De Loecker, Klaus Desmet, Geert Dhaene, Joep Konings, Daniel Millimet, Sašo Polanec, Santanu Roy, Tim Salmon, Klaas Staal, David Stadelmann, Marvin Suesse, Angelos Theodorakopoulos, Jeroen Van den bosch, Stijn Vanormelingen and participants to the 32^{nd} annual congress of the European Economic Association in Lisbon, the Economics Seminar Series at Southern Methodist University, the VIVES Research Seminar, the Center of Excellence: Governments & Markets Seminar at the University of Leuven and the VIVES Workshop on the "Stability of Regions, Culture and Institutions" for valuable comments and suggestions on earlier versions of this paper.

1 Introduction

Historically, state formation tended to be a tale of the aggregation of resources, power and territory.[1] Over the course of the last century, however, the world has witnessed a persistent trend towards state fragmentation, raising the importance of understanding its economic consequences. This is especially so since independence movements increasingly embed their case in the economic realm (Rodríguez-Pose & Gill, 2005). In the wake of the Scottish independence referendum, for example, the Financial Times (2014) reports that

> Alex Salmond, Scotland's first minister who is leading the campaign for independence, said [...] that each household would receive an annual "independence bonus" of £2,000 - or each individual £1,000 - within the next 15 years if the country votes to leave the UK. The UK government, in contrast, claimed that if Scots rejected independence each person would receive a "UK dividend of £4,000 ... for the next 20 years".

In spite of its current poignancy, there is still surprisingly little empirical research on the economic impact of secession and our knowledge on how independence processes have affected economic trajectories of actual newly independent countries (NICs) remains highly imperfect. In this light, this paper presents estimates of monetary per capita independence gains/losses for a large panel of countries for the period covering 1950-2013.

There are at least three motivations for this exercise. First of all, the theoretical literature on the relation between state fragmentation, state size and economic growth delivers contradictory results. Hence, it remains theoretically ambiguous whether and to what extent a declaration of independence can be expected to meaningfully affect the economic outlook of a NIC. Second, the empirical literature on this subject is disappointingly small (Rodríguez-Pose & Stermšek, 2015). This implies that it is also unclear what can be learned from past instances of state fragmentation. Finally, the *expected* economic impact of secession does shape people's views on the merits of independence today and thus also shapes electoral behavior.[2] Getting a clearer view on the actual economic consequences of secession should serve to yield a more efficient democratic decision-making process.

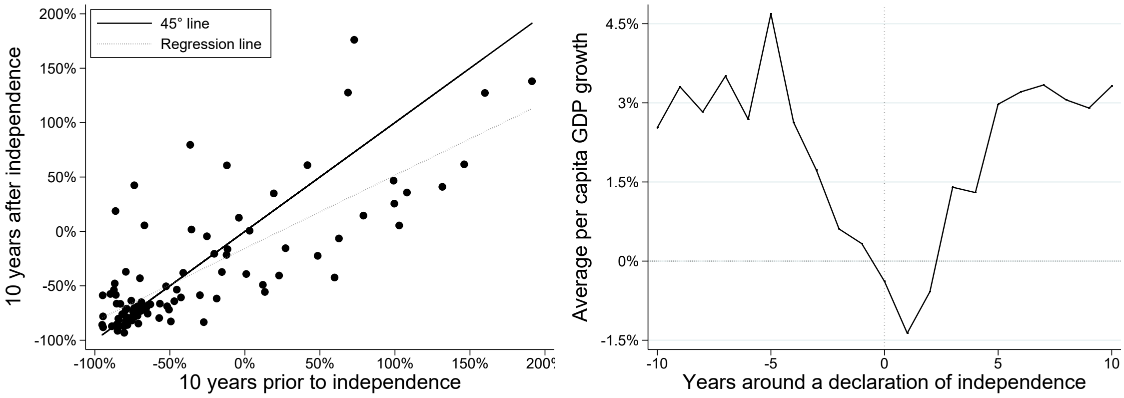
In order to provide a preliminary view on the existence as well as the magnitude of the independence dividend, Figure 1a presents difference-in-difference estimates of the impact of declaring independence on the relative economic performance of NICs, where the 'relative economic performance' of a country is here defined as the percentage discrepancy between its own and worldwide per capita GDP. More specifically, the figure plots the relative economic performance of NICs ten years *after* their independence declaration against their relative economic performance ten years *prior* to independence. The vertical

[1] See, for instance, Tilly (1990) and Lake and O'Mahony (2004).

[2] Curtice (2013), for instance, reports opinion research results indicating that 52% of Scots would support independence if it were clear beforehand that this would make them £500 a year *better off*, but that support for independence drops to 15% if this decision is anticipated to come at a yearly *cost* of £500.

distance of each point on the graph to the ray of equality reflects a difference-in-difference estimate for the net gain of independence for a specific NIC. As can be seen, the figure provides tentative evidence that the decision to declare independence did affect the relative economic performance of most NICs, and sometimes substantially so. Also apparent is the heterogeneity of this effect across countries, where some NICs outperformed the rest of the world in terms of per capita GDP growth during the period under consideration, whereas others seemingly incurred an independence cost. Nevertheless, the population-weighted difference-in-difference estimate suggests that the net gain of independence tended to be negative and decreased per capita income by roughly 38%, 10 years after independence.

Figure 1: **Trends in per capita GDP around a declaration of independence**



(a) Relative economic performance

(b) Growth

Note: Figure 1a plots the relative economic performance of each NIC in the 10^{th} post-independence year against its relative economic performance in the 10^{th} pre-independence year. Figure 1b plots average per capita GDP growth in the group of NICs, in a period stretching from 10 years before up until 10 years after their declaration of independence. The number of years before (-) or after (+) secession is indicated on the horizontal axis.

The crude correlation in figure 1a, however, could also be driven by other omitted factors. Indeed, several challenges complicate the estimation of the causal impact of declaring independence on economic outcomes emanating from omitted variable bias, simultaneity, anticipation effects, effect heterogeneity and model uncertainty. First, as shown in figure 1a, NICs and established countries differ quite extensively in terms of their underlying socio-economic structure. More specifically, the figure suggests that the group of NICs is predominantly composed of economically less developed regions.[3] Therefore, a simple comparison of the economic performance of NICs *vis-á-vis* established states may not only reflect the effect of declaring independence, but may also reflect the effects of differing growth determinants which were already apparent in the pre-independence period. Second, as illustrated in figure 1b, NICs, in the run-up to their declaration of independence, typically experience sharp declines in per capita GDP growth rates. As per capita GDP trajectories tend to be highly persistent, this raises an obvious endogeneity concern. In

[3]Table 1 provides a more detailed account.

other words, it is important to distinguish the causal impact of declaring independence on future growth potential ruling out any feedback-effects of past growth dynamics that might have affected the incentives to secede. Third, this pre-secession growth-dip is also consistent with the presence of anticipation effects, indicating that state fragmentation may already have an economic impact in the years prior to the actual decision to secede. Failure to account for these *ex ante* effects will generally result in an underestimation of the full economic impact of secession. Fourth, the economic impact of declaring independence might differ both across countries and across time, such that an aggregate independence dividend estimate may be sensitive to the chosen time horizon and country sample. Finally, the lack of convergence on the functional form capturing the economic impact of declaring independence in the theoretical literature raises concerns with respect to the sensitivity of the estimated parameters to specific functional form assumptions.

To mitigate these concerns, this paper develops a semi-parametric estimation strategy rooted in the synthetic control method pioneered by Abadie and Gardeazabal (2003). This methodology allows to simulate, for each NIC, the counterfactual post-independence per capita GDP trajectory that would be observed, in the hypothetical case that it would have decided *not* to declare independence. By comparing these simulated trajectories with their observed counterparts, we are able to track both country-specific and aggregate independence dividends over time. Our central results show robust and statistically significant evidence that the decision to secede lowered per capita GDP trajectories in NICs, and persistently so. The baseline estimates of the aggregate long-run welfare cost of independence, in terms of per capita GDP foregone, range from 20% to 40%. Yet, there is considerable cross-country heterogeneity in the economic impact of secession.

To address a well-known drawback of this methodology, namely the difficulty of assessing the statistical significance of the estimates, we extend the placebo test approach put forward by Abadie, Diamond, and Hainmueller (2007, 2010, 2014) to propose a novel quadruple-difference bias correction procedure. Most reassuringly, we find comparatively little effect on per capita GDP when applying the simulation procedure on countries *unaffected* by state fragmentation, while the negative per capita GDP discrepancy between NICs and their counterfactuals in the post-independence period also clearly exceeds the discrepancy between both typically observed in the pre-independence period. In addition, our main conclusions also remain qualitatively unchanged when we parametrically control for potential contamination effects stemming from the economic effects of independence in other recently formed states. Although these findings underscore that our estimated independence dividends are unlikely to be driven by simulation inaccuracy, matching inaccuracy or spillover effects, we show that not correcting for these three potential sources of bias tends to inflate both the estimated net cost of independence as well as its persistence.

One advantage of estimating country-specific independence gains, is that we can also rely on these estimates to characterize their implications for a number of historical independence waves. Doing so, we find that our results largely corroborate the existing literature. With respect to the earliest independence waves in our sample, we find that former British colonies tend to economically outperform their former Belgian, French and Portuguese counterparts in the post-independence period, in line with the often mentioned arguments that the British left behind better institutions and a better educated working population. Concerning the more recent independence waves, we find that the Soviet breakup had the most adverse economic impact. This finding resonates with the view that rent-seeking was more pervasive in former Soviet countries than in the other newly formed Eastern and Central European countries, whose geographical locations moreover generated stronger incentives to quickly implement sound economic reforms to maximize the prospects of European Union membership. A broader comparison furthermore indicates that the earlier decolonization waves had less adverse economic effects than the more recent break-ups of the Soviet Union, Yugoslavia and Czechoslovakia, suggesting that there may have been economic gains associated with the elimination of colonial drain.

Interestingly, our empirical findings also offer a dissenting appraisal of the transition literature, suggesting that the estimated transition costs for newly formed transition countries are actually mainly driven by the economic effects of their independence declaration. This is related to the complication that the independence declarations of some NICs in our sample coincided with their transition from a planned to a market economy. This, in turn, implies that the estimated independence dividends for these newly formed *transition* countries may at least partially reflect what are actually transition costs, which would have been born *irrespective* of the decision to declare independence. Nevertheless, it turns out that parametrically correcting these independence dividend estimates by removing synthetic control estimates of these transition costs in a subsample of transition countries that did *not* declare independence in the 1990's does not qualitatively alter our findings. In sharp contrast, purging the estimated independence dividends from the adverse effects that can plausibly attributed to the transition process suggests that more than half of the estimated independence costs for newly formed transition countries can effectively be attributed to their declaration of independence. Thus, our findings reverberate with Linn's (2004) concern that the existing literature on transition in Central and Eastern Europe and the former Soviet Union may *overestimate* transition costs by neglecting that most of these countries simultaneously decided to break away from their mother countries.[4]

Since the primary purpose of this paper is to conduct a first, careful attempt to quantify the overall economic effects of independence for a broad number of historical instances of state fragmentation, it is important to recognize that our results only reflect the consequences of each particular country's independence experience. In a second and more

[4] More specifically, Linn (2004, p. 2) mentions that *"when one reviews the economic and econometric literature on transition in Central and Eastern Europe and the FSU, one finds a large number of regression analyses relating economic growth over the transition years as the independent variable to a number of explanatory variables, usually consisting of a mix of parameters reflecting so-called 'initial conditions' and 'market-oriented reforms' "* but goes on to worry that *"in none of the econometric studies is there an explicit recognition of the fact that the Soviet Union broke apart into independent nations"*.

speculative part, we move beyond mere description and try to extract more general lessons from the available historical episodes of state breakup. We do so by looking for patterns in these estimated independence dividends that can identify the primary channels through which secession affected growth potential in these newly formed states.

For this purpose, we develop a two-step procedure that regresses the estimated independence payoffs on a number of potential channels: trade openness, country size, macroeconomic uncertainty, the intensity of conflict and the level of democracy. In addition to its importance in terms of policy implications, this provides a meaningful way to empirically evaluate the various claims laid out in the existing literature. We find tentative evidence that the adverse economic effects of independence dissipate when trade barriers fall, democratic institutions improve, the population size of the newly formed state grows and post-independence military violence is avoided. Gauging the relative importance of these channels over a wide range of historical instances of state fragmentation, trade often turns out as the strongest predictor of post-independence economic performance, underscoring its prevalence in many contemporary independence discussions. We fail to find clear-cut evidence of the relevance of macroeconomic uncertainty and the economic desirability of declaring independence by referendum. Finally, a robustness check parametrically controls for the potential endogeneity of the estimated independence gains and their potential determinants to demonstrate the robustness of the results.

Our argument is closely related to existing economic thinking on the consequences of state fragmentation, which can at least be traced back to the conference on the *Economic Consequences of the Size of Nations* held by the International Economic Association in 1957, the proceedings of which were published in a compendium in 1960 (Robinson, 1960). A persistent narrow focus on this related issue of country size, however, seemingly prevented the ensuing literature to develop a more comprehensive approach to study the economic impact of state-breakup. In addition, the relation between state size and economic growth remains theoretically ambiguous. Thus, although country size is considered growth-neutral in early neo-classical, closed-market growth models such as Solow (1956), more recent work in growth theory includes either some form of agglomeration effect (Krugman, 1991) or a scale effect (Romer, 1986; Barro & Sala-i Martin, 2004; Aghion & Howitt, 2009), benefiting growth potential in larger states.[5] Larger countries are also thought to benefit from scale economies in the public sector, due to their ability to spread the costs of public policy over a larger population (Alesina & Wacziarg, 1998; Alesina & Spolaore, 2003). Nevertheless, Alesina, Spolaore, and Wacziarg (2000) and Ramondo and Rodríguez-Clare (2010) contend that smaller countries can compensate the costs imposed by the limited size of their domestic market by increased trade openness. Furthermore, it has been frequently asserted that the free-rider problem is less disruptive of collective action in smaller states, facilitating a more flexible and effective economic policy (Kuznets,

[5] Jones (1999, p. 143), for instance, argues that in reviewing three classes of endogenous growth models *"the size of the economy affects either the long-run growth rate or the long-run level of per capita income."*.

1960; Streeten, 1993; Armstrong & Read, 1995; Yarbrough & Yarbrough, 1998). Finally, smaller countries may benefit from a more homogenous population, easing the accumulation of social capital and generalized trust (Armstrong & Read, 1998).

Another related line of research emphasizes the negative effects implied by the policy uncertainty and the fear of potential conflict arising from the decision to secede. Onour (2013) develops a macroeconomic model to analyze the adverse effects on asset market stability and government debt sustainability of a small open economy splitting up in two independent parts. Other studies maintain that a high propensity of policy change may reduce both investment and the speed of economic development by triggering domestic and foreign investors to delay economic activity or exit the domestic economy by investing abroad (Gupta & Venieris, 1986; Alesina, Ozler, Roubini, & Swagel, 1996) and inducing purchasers of government bonds to require higher risk premiums, increasing the cost of providing government (Somers & Vaillancourt, 2014).[6]

The political science literature, on the other hand, has emphasized that secession generally involves some degree of (military) conflict (Fearon, 1998; Spolaore, 2008), resulting in human capital losses, reductions in investment and trade diversion, all of which are generally associated with lower levels of growth. Additionally, these costs may be persistent as Fearon and Laitin (2003b) find that NICs face drastically increased odds of civil war onset, possibly due to the loss of coercive backing from the mother country. Following Murdoch and Sandler (2004), the impact of secession is thus expected to be codetermined by the existence, intensity, duration and timing of conflict.

In examining the influence of colonial heritage on post-independence economic performance, a different strand of the literature stresses the relevance of the initial conditions left behind by the mother country (Acemoglu, Simon, & Robinson, 2001; Acemoglu, Johnson, & Robinson, 2002). Nunn (2007, 2008), for instance, uncovers a negative relation between the number of slaves exported in former African colonies and their current economic performances, suggesting that Africa's underdevelopment since independence can be partially explained by the weakening effect of these slave trades on domestic property right institutions. In a similar spirit, Bertocchi and Canova (2002) conclude that colonial origin lies at the root of contemporary growth differentials in Latin America and Africa due to institutional persistence.

In addition, the more recent transition economy literature points out that the identity of neighboring countries may matter too in shaping incentives to implement political and economic reform (Roland, 2002; Fidrmuc, 2003).[7] In the context of the Soviet breakup, moreover, Suesse (2017, p. 32) finds that prospective secessions economies may severely disrupt trade flows such that *"the possibility of secession may be enough to deter trade or*

[6] Walker (1998) mentions that when the intensity to secede is large, a declaration of independence may actually *reduce* policy uncertainty since this decision clarifies that the current government will collapse.

[7] A more comprehensive discussion of the economic impact of the demise of colonial rule in Africa and Latin America is offered by Bates, Coatsworth, and Williamson (2007) and Prados De La Escosura (2009).

investment, even without secession actually having taken place".

One hitherto overlooked issue is the temporal coincidence of surges of secession and surges of democracy (Spencer, 1998; Alesina & Spolaore, 1997; Alesina et al., 2000). Dahl, Gates, Hegre, and Strand (2013), for instance, provide empirical evidence that the wavelike shape of the global democratization process is (at least partially) explained by the wavelike shape of state entry, finding that NICs are initially considerably more democratic compared to the rest of the world but are also more susceptible to subsequent reversal. Although it is unclear whether secession operates as a democratization tool or whether democracies are more liable to demands for autonomy, this suggests that the effect of declaring independence is at least partially contingent on ensuing democratization processes in NICs.[8] The link between democracy and economic development, however, is itself subject to an inconclusive academic literature.[9]

This study is also directly related to a relatively small empirical literature that has attempted to uncover the link between state fragmentation and economic performance. Sujan and Sujanova (1994) develop a macroeconomic simulation model to estimate the short-term economic impact of the Czechoslovakian dissolution into the Czech Republic and Slovakia, concluding that the decision to separate reduced GDP by 2.2% in the Czech Republic and by 5.7% in Slovakia. Bertocchi and Canova (2002) use a difference-in-difference approach to establish, for a restricted number of former colonies, that there may be substantial growth gains from the elimination of extractive institutions. Somé (2013) relies on a synthetic control approach to demonstrate that former African colonies that declared independence through wars suffer larger income losses than African colonies that declared independence without conflict, at least in the short to medium run. Most recently, Rodríguez-Pose and Stermšek (2015) use panel data on the constituent parts of former Yugoslavia to estimate an independence dividend concluding that, once relevant factors such as war are taken into account, there is no statistically significant relation between achieving independence and economic performance while independence achieved by conflict seriously dents growth prospects. Small sample size and conflicting results, however, limit the extent to which these results can be extrapolated to other instances of state fragmentation. Moreover, these models generally do not account for omitted variable bias, simultaneity, anticipation effects and model uncertainty.

Other empirical studies have focused on estimating the economic effects of unification. In a cross-country set-up, Spolaore and Wacziarg (2005) propose a three-stage least squares approach to analyze the market size effect and the trade reduction effect of 123 hypothetical pairwise mergers between neighboring countries concluding that full integration, on average, would reduce annual growth by 0.11% while market integration would boost it by an estimated 0.12%. Abadie et al. (2007, 2014) use the synthetic control method to tease

[8] Conversely, these findings also suggest that the link between democracy and economic development may be confounded by the economic impact of state fragmentation, an issue overlooked in the existing literature.
[9] Gerring, Bond, Barndt, and Moreno (2005) provide a recent summary of this literature.

out the per capita economic payoff of the 1990 German reunification for West Germany, concluding that actual 2003 West German per capita GDP levels are about 12% below their potential level due to unification.

Finally, the link between country size and economic performance is scrutinized in a number of empirical studies which *"typically find that smaller country size is likely to be associated with higher concentration of the production structure, higher trade openness, higher commodity and geographic concentration of trade flows* [and] *larger government"* (Damijan, Damijan, & Parcero, 2013, p. 6). Whether country size affects growth remains disputed, as some studies fail to find any significant relationship (Backus, Kehoe, & Kehoe, 1992; Milner & Westaway, 1993) while others report a significant negative relation with either per capita GDP (Easterly & Kraay, 2000; Rose, 2006; Damijan et al., 2013) or economic growth (Alouini & Hubert, 2012).

The remainder of this paper is organized as follows. Section 2 describes the construction of the dataset, provides data sources and reports some descriptive statistics. Section 3 presents the independence dividend estimates emanating from the semi-parametric route. This section also contains a variety of robustness checks. Section 4 presents empirical evidence on the channels through which secession affects economic growth potential and also performs a number of robustness checks. Section 5 concludes.

2 Data and descriptive statistics

To shed light on the relation between declarations of independence and the ensuing per capita GDP dynamics in newly formed states, we construct an annual panel comprising 196 countries and covering the period 1950-2016. In what follows, 80 of those countries will be referred to as 'established countries', in the sense that these are countries that already gained independence *before* 1950. The remaining 122 countries will be called 'newly independent countries' (NICs), reflecting that these countries declared independence anywhere between 1950 and 2013. To identify the year of independence of each country in the sample, we primarily rely on and extend data on state entry as reported in Griffiths and Butcher (2013). Table A2 provides a full list of all NICs and their year of independence.

Our dependent variable is the log of per capita GDP, which will proxy the economic performance of these countries, while our choice of control variables is primarily rooted in the growth literature. Depending on the specification, it includes the average years of education, life expectancy and population density to capture differences in terms of human capital and differential population effects. As it is argued to be a determinant of both economic performance and state fragmentation, we include a measure of trade openness.[10] Similarly, given that democratization processes appear to be both related to the decision to secede and (possibly) to economic outcomes, we also utilize a composite index of democracy. Furthermore, as independence is rarely achieved without some form

[10] See, for instance, Alesina and Spolaore (1997), Alesina et al. (2000) and Alesina and Spolaore (2003).

of conflict, we include the per capita number of war deaths as reported by Bethany and Gleditsch (2005) to capture the adverse economic effects associated with the existence, intensity and duration of military conflict.[11] In addition, mimicking Gibler and Miller (2014a), we define a 'political instability'-dummy indicating whether a country experienced a two-standard-deviation change in its democracy score during the previous observation year. To control for the adverse effects of macroeconomic instability, we include a dummy variable indicating known banking and debt crises from Reinhart and Rogoff (2011).[12]

We draw on a wide variety of data sources to obtain a dataset that is as extensive as possible. Capitalizing on prior work by Fearon and Laitin (2003a), to address the potential issues of measurement error and misreporting of per capita GDP[13], we depart from the real per capita GDP information contained in The Madison Project (2017), we subsequently maximally extend these estimates forward and backwards relying on the growth rate of real per capita income provided by the World Bank (2016) and finally approximate remaining missing observations by use of a third-order polynomial in (i) a country's level of CO2 emissions (World Resources Institute, 2015; World Bank, 2016), (ii) a year dummy and (iii) a region dummy. To make sure that our results are not driven by the data construction procedure, we also construct an alternative index of real per capita GDP by aggregating per capita GDP information from multiple data sources, though this did not affect any of our conclusions.[14] With regard to the alternative growth determinants, we generally rely on a similar third-order polynomial approximation strategy to synthetize relevant information contained in various data sources. Appendix A reports all relevant data sources for these constructed variables, provides a more detailed description of the variable-specific data manipulation procedure utilized and reports some diagnostics.

Table 1, then, reports the most important descriptive statistics separately for established countries and (future) NICs while also assessing to what extent both groups significantly differ from each other in terms of these underlying growth determinants. The results confirm our prior findings: (future) NICs, on average, are significantly poorer in per capita terms and they also tend to have a less educated population, a lower life expectancy and less democratic institutions. Nevertheless, they tend to be somewhat less sensitive to military conflict, experience less (known) instances of financial crises and, as suggested in the existing literature, they also tend to be more stable politically and favor a more liberal trade regime. All in all, these summary statistics thus suggest that NICs manifest less favorable growth determinants when compared to more established states.

[11] We primarily rely on the 'best estimates' of each specific country-year number of battle deaths. In case these are unavailable, we take the simple average of the lowest and highest estimates instead.

[12] To preserve a maximal amount of observations in the analysis, missing values of the index are set to 0.

[13] For a discussion of data variability and consistentization issues across successive versions of the Penn World Table, see Johnson, Larson, Papageorgiou, and Subramanian (2013); for a discussion on the reliability of pre-independence per capita income estimates of former Soviet states, see Fischer (1994).

[14] As noted in Appendix A, baseline per capita GDP correlates strongly with the alternative estimates, at 0.99 for their 11214 common observations. Results based on these alternative per capita GDP estimates are available from the authors on request.

Table 1: **Summary statistics**

	Established countries			Newly independent countries				
Variable	*Obs.*	*Mean*	*Std. Dev.*	*Obs.*	*Mean*	*Std. Dev.*	*Mean diff.*	*P-value*
GDP per capita	4809	8385.96	13553.55	7189	3830.14	5473.454	-4555.81	0.00
Population (millions)	4868	47.33	146.793	7539	9.71	25.265	-37.63	0.00
Years of schooling	4627	6.68	3.24	6533	5.08	3.256	-1.60	0.00
Life expectancy	4527	66.06	10.834	7077	58.39	12.097	-7.68	0.00
Openness	4702	0.54	.418	5687	0.84	.539	0.31	0.00
Battle deaths per head	4868	0.00	0	7539	0.00	0	0.00	0.02
Population density	4868	270.11	1539.554	7495	134.50	444.31	-135.61	0.00
Democracy	4806	23.61	13.731	5410	17.45	10.421	-6.16	0.00
Political instability	4806	0.00	.058	5410	0.00	.03	-0.00	0.01
Macroeconomic instability	5122	0.46	.91	8079	0.09	.447	-0.38	0.00

Note: Data construction and sources provided in section 2 and appendix A. Statistics for NICs include information pertaining to the pre-independence period. The last column reports the p-value for the two-sided t-test that the two means are equal.

3 Semi-parametric estimation of the independence dividend

This section follows a semi-parametric route to identify the causal relation between declarations of independence and ensuing per capita GDP dynamics in NICs. After outlining the general estimation strategy, we first provide a motivating example. Subsequently, we derive baseline estimates of both country-specific and aggregate independence payoffs. Before discussing the implications of our estimates pertaining to the various independence waves in our sample, a subsequent subsection first formulates a finite-sample bias-correction procedure to control for the potential biases stemming from simulation inaccuracy, matching inaccuracy and spillover effects. A final subsection performs a variety of robustness checks and most notably deals with the complication that the independence declarations of some NICs in our sample coincided with their transitions from planned to market economies: a process which may well have engendered economic consequences in itself, but whose potential economic effects would be subsumed in our independence dividend estimates.

3.1 Estimation strategy

To mitigate both omitted variable bias, endogeneity and heterogeneity concerns and to deal with the potential problem of model uncertainty, we rely on the synthetic control method pioneered by Abadie and Gardeazabal (2003) and further developed in Abadie et al. (2007, 2010, 2014). Although the details of this approach are deferred to appendix B, which provides a more formal description, in a nutshell, this method estimates the effect of a given policy shock (in this case, declaring independence) by comparing the evolution of an outcome variable of interest (in this case, log per capita GDP) for the affected country with the evolution of the same variable for a so-called 'synthetic control' country. This synthetic control country, then, is constructed as a weighted average of unaffected control countries (in this case, all other independent countries which did not recently gain independence themselves) that matches as closely as possible the country affected by the policy shock, before the shock occurs, for a number of unaffected predictors of the outcome variable. Intuitively, the trajectory of the outcome variable in the synthetic control country can be understood to mimic what would have been the path of this variable in the affected country, if the policy shock had never occurred.

Appendix B highlights how the primary strength of the synthetic control method lies in the lack of conditions it imposes on unobserved characteristics, making it robust for the confounding effects of time-varying unobserved characteristics at the country level as long as the number of pretreatment periods is large and the pre-independence match is good. Moreover, as long as the aforementioned conditions are satisfied, this estimator is robust to endogeneity as well. For example, if secession partly happens as a reaction to falling per capita GDP levels, by definition, the per capita GDP levels of the synthetic control country match with those of the seceding country in the pre-independence period such that these unfavorable past GDP dynamics should manifest their potential economic effects in the synthetic control unit as well. In contrast to a panel regression framework, this method also safeguards against flattening out useful variation in the economic impact of secession across countries and time, by allowing the estimation of both country-specific and aggregate net independence dividends over time. Finally, this method does not require formal modeling nor estimation of any of the population parameters associated with the observed growth determinants, making it more robust against model uncertainty.

3.2 A motivating example

To illustrate this methodology, consider the example of Ukraine, which declared itself independent from the Soviet Union in 1991. To estimate what would have been the post-independence per capita GDP trajectory of Ukraine in absence of secession, we rely on the remaining 153 countries in our sample which were independent in 1991, but were not confronted with state state fragmentation between 1981 and 1991, to construct a weighted average country that best resembles Ukraine in the pre-independence period for a number of growth predictors. As it turns out, the optimal set of weights constructs this synthetic version of Ukraine as a weighted average of - in decreasing order of their corresponding weights - Malaysia, Romania, South Korea and Australia, see table 2.

Table 2: **Optimal weights for synthetic Ukraine**

Country	w^*
Malaysia	.562
Romania	.276
South Korea	.082
Australia	.08

Table 3 below suggests that the synthetic version of Ukraine, in effect, provides a much better comparison for pre-independence Ukraine than the global average of our sample. As is apparent from the table, average pre-independence per capita GDP levels in Ukraine are practically indistinguishable from their synthetic counterpart, in contrast to the somewhat lower levels witnessed in the rest of the world during this period. Moreover, the synthetic version of Ukraine is also much more similar to the actual pre-independence Ukraine in terms of population, population density, trade openness, educational attainment, life expectancy and the per capita number of battle deaths suffered.

Table 3: **Predictor balance before secession (1981-1990)**

Predictor	*Ukraine*	*Synthetic Ukraine*	*World*
Per capita GDP	5214.203	5248.127	4574.852
log Population	17.744	16.756	18.801
Population density	84.17	87.31	142.414
Educational attainment	9.065	7.782	5.411
Life expectancy	69.98	69.858	64.065
Trade openness	1.019	.833	.315
Battle deaths (per 1000 heads)	0	0	.027

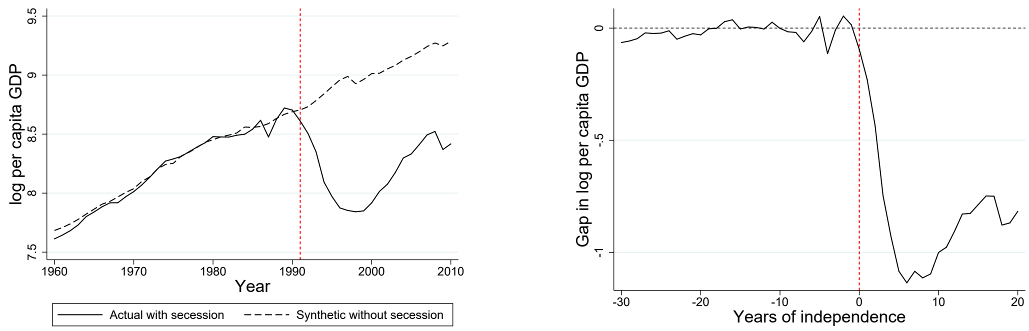
Note: Growth predictors are averaged over the 1981-1990 period. The last column reports population-weighted averages computed over all independent countries.

The central intuition behind the synthetic control method, then, is that the only potentially economically meaningful difference between Ukraine and its synthetic version post-1991 is that Ukraine declared independence whereas its synthetic version did not. Therefore, to derive the economic significance of the Ukrainian declaration of independence, we can compare the post-independence per capita GDP trajectories of now-independent Ukraine and its synthetic version. To do so, the left panel of figure 2 below plots the evolution of log per capita GDP in Ukraine (full line) as well as synthetic Ukraine (dashed line) between 1960 and 2011. Note, first, that both series are practically indistinguishable during the entire pre-independence period. Thus, even though this synthetic version of Ukraine was constructed by only taking into account the last 10 years prior to independence, it turns out to be well capable of assessing Ukranian per capita GDP dynamics over the entire 1960-1990 period.[15] Combined with the close fit obtained for the pre-independence growth predictors in both groups, as reported in table 3, this suggests that the proposed combination of other independent countries adequately reproduces the economic situation in Ukraine in absence of state fragmentation.

[15]The slight diversion between both series in the pre-independence period might suggest the presence of anticipation effects in the years preceding the Ukrainian declaration of independence. To take these into account, as suggested by Abadie et al. (2010), we redid the exercise redefining the timing of independence to have occurred three years prior to the actual decision to secede. None of the results are qualitatively affected by this.

The estimated economic effect of the Ukrainian declaration of independence is given by the difference between the actual and synthetic trajectories in the post-independence period. For this reason, the right panel of figure 2 plots the yearly gaps in per capita GDP between Ukraine and its synthetic counterpart for a period stretching from 30 years prior up until 20 after Ukraine's secession from the Soviet Union. Note that, since both series are expressed in logarithmic form, the discrepancy between both reflects the percentage payoff of having declared independence in terms of per capita GDP foregone.

Figure 2: **Trends in per capita GDP: Ukraine versus synthetic Ukraine**



(a) Per capita GDP: Ukraine vs. synthetic Ukraine (b) The economic impact of secession (Ukraine)

Note: The left figure plots the log per capita GDP trajectories in Ukraine (full line) and synthetic Ukraine (dashed line) between 1961 and 2011; the right figure plots the discrepancy between both trajectories during the same period. The Ukranian independence declaration is marked by the vertical red dashed line.

The figure suggests that the Ukranian declaration of independence had an immediate and increasingly adverse impact on per capita GDP levels in the first five years after secession. After this initial negative payoff, however, our results indicate that Ukraine never fully recovered in the ensuing 15 years but, on the contrary, consistently underperformed *vis-á-vis* its synthetic counterpart. This suggests that, at least in the Ukranian case, the negative independence dividend is persistent. Moreover, the estimated long-run cost implies that, 20 years after its declaration of independence, Ukrainian per capita GDP still lies around 82% below its potential level due to state fragmentation.

3.3 Baseline results

As explained in the previous section, a closer inspection of the Ukrainian case through the lens of the synthetic control method suggests that the net payoff of independence is large and negative. Nevertheless, Ukraine might be an outlier in terms of both the immediate and persistent effects of declaring independence, limiting extrapolation potential. Therefore, subject to data availability, this subsection applies the synthetic control method to each NIC in the sample and characterizes both country-specific and aggregate independence dividends as well as their evolution over time.

Figure 3 displays several versions of the results of this exercise. First, consider the top-left panel which plots the results seperately for each available NIC in our sample. The gray lines represent the per capita GDP gaps between each NIC and its respective synthetic version (corresponding to the results displayed in figure 2b) in the period stretching from 10 years before up until 30 years after their declaration of independence. The superimposed black line depicts the yearly population-weighted gap in the sample while the superimposed red line captures the average gap computed over the entire pre- and the post-independence period respectively. Apparent from this figure is the large cross-country heterogeneity in the economic impact of secession, which clearly shows several examples of NICs appearing to have benefited in economic terms from having declared independence.[16]

As the figure also indicates, the synthetic control method provides a reasonably good fit for the per capita GDP trajectories between NICs and their synthetic counterparts in the pre-independence period. The average pre-independence RMSPE in the full sample is about 0.102, which is quite small but does reflect that NICs already underperformed somewhat relative to their synthetic counterparts in the pre-independence period. More specifically, per capita GDP levels in NICs on average lie 0.7% below those of their synthetic versions even in the last 10 years *prior* to their respective declarations of independence. In the post-independence period, however, their underperformance clearly worsens and the average percentage discrepancy increases to -21%. Interestingly, NICs do not appear to recover in the longer run as the population-weighted average independence dividend equals -23.2% in the 30^{th} post-independence year. In other words, when their country celebrates its 30^{th} birthday, inhabitants of NICs typically experience per capita GDP levels which lie 23% below those of countries which, in all relevant aspects, most closely resembled their own country's economic situation just prior to its decision to secede.

Nevertheless, figure 3a also indicates that the synthetic control method fails to adequately reproduce per capita GDP trajectories for some NICs in the pre-independence period. East Timor, for instance, is the country with the worst pre-independence fit (RMSPE=0.472). Given its extraordinary low pre-independence per capita GDP trajectory, it should come as no surprise that its growth path cannot be adequately approximated by any linear combination of the available control countries. By extension, this complication applies to all NICs with extreme values in their pre-independence characteristics. As the post-independence gaps of these poorly fitted cases may merely reflect differences in their underlying economic characteristics, rather than actual independence dividends[17], figures 3b to 3d plot the results when the sample is progressively restricted to include only the 80%, 60%, 40% and 20% best matched cases in terms of their pre-independence RMSPE. In each of these trimmed samples, the synthetic control method provides an excellent fit (the associated average RMSPE's equal 0.063, 0.048, 0.036 and 0.024 respectively). Sacrificing quantity for quality, however, does not qualitatively affect our primary conclusions:

[16]Country-specific results are reported in table A3, while figure 11 connects the implications of our results to the existing literature on a number of historical instances of state fragmentation.

[17]Since they are unlikely to even approximately satisfy conditions (1A) through (3A).

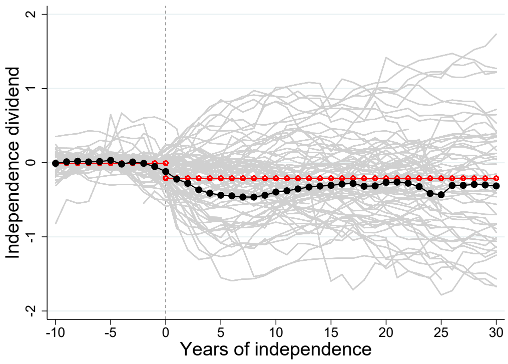
each of these figures suggests that NICs face immediate and increasing costs of secession in the first 5 years after they gain independence, while these costs also appear quite persistent and reduce per capita GDP levels by anywhere between 15%-30% in the long run.

Since there does not appear to be a consensus on the optimal cut-off of pre-independence RMSPE to avoid biases stemming from poor-fit, the bottom figure utilizes a more data-driven procedure to impose a threshold value (or caliper) defining the maximal allowed RMSPE. More specifically, in the tradition of propensity-score matching, Rosenbaum and Rubin (1985) suggest using an optimal caliper of 0.25 times the standard deviation of the linear propensity score. Adapting this to the present context, figure 3f imposes a caliper amounting to 0.5 times the samplewide standard deviation in pre-independence RMSPE. Once again, this results in an excellent pre-independence fit as suggested by the average RMSPE, which now equals 0.029, while our primary conclusions remain robust.

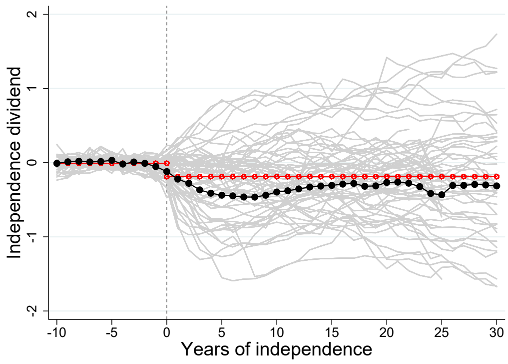
Finally, to gauge the statistical significance of these results, figure A2 verifies whether a causal interpretation is warranted by their distribution. Plotting the same sequence of population-weighted independence dividend estimates along with 95% confidence intervals, we confirm that the per capita GDP discrepancy between NICs and their synthetic counterparts are statistically indistinguishable from 0 in the pre-independence period.[18] More importantly, these graphs confirm that - irrespective of the selected sample - NICs tend to underperform versus their synthetic versions in the entire post-independence period.

[18] To take the potential presence of anticipation effects into account, we redid the analysis shifting the timing of independence to have occurred 3 years earlier, obtaining qualitatively similar results.

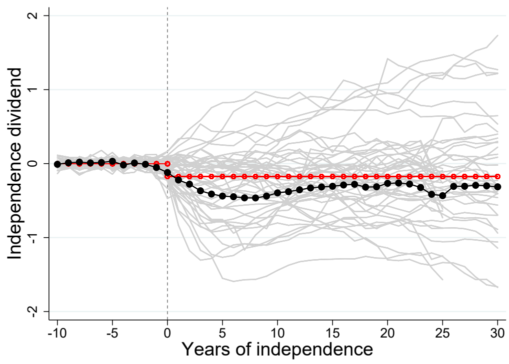
Figure 3: **Impact of secession in selected countries**



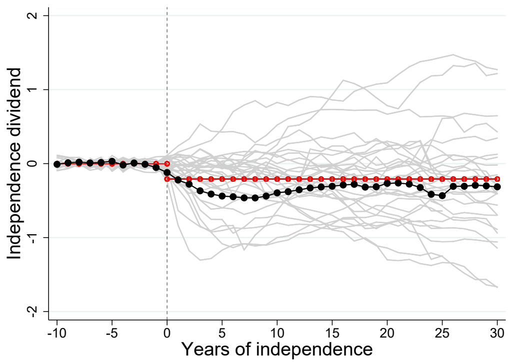
(a) All cases



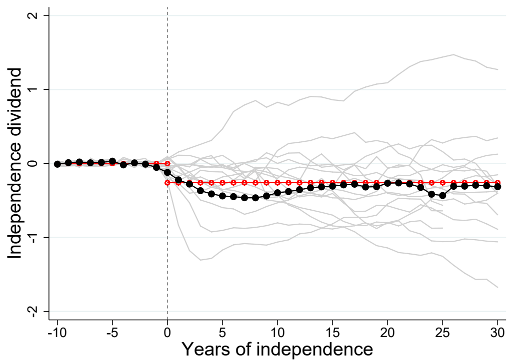
(b) 80% best matched cases



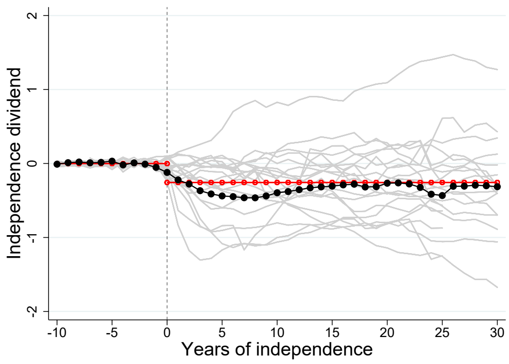
(c) 60% best matched cases



(d) 40% best matched cases



(e) 20% best matched cases



(f) All cases within caliper

Note: Each gray line plots the yearly log per capita GDP discrepancy between the per capita GDP trajectory of a specific NIC and its synthetic counterpart around their declaration of independence. The black line depicts the yearly population-weighted average gaps; the red line displays the pre- and post-independence average gaps. The number of years before (-) or after (+) independence are indicated on the horizontal axis. The top-left panel contains all available cases, subsequent panels include only results of the 80, 60, 40 and 10% best matched cases in terms of their pre-independence RMSPE. The bottom figure includes only those cases for which the pre-independence RMSPE falls within the data-driven caliper cut-off amounting to 0.5 times the samplewide standard deviation in pre-independence RMSPE.

3.4 Finite-sample bias correction

As noted in the introduction, one drawback of this estimation procedure lies in the absence of a systematic way to assess the degree of uncertainty surrounding synthetic control estimates of treatment effects. In this section, we propose a finite-sample bias correction procedure to sequentially correct for three potential sources of bias while at the same time quantifying the ensuing degree of estimation uncertainty: *(i)* matching quality, which relates to the economic comparability of NIC and synthetic NIC in absence of state fragmentation; *(ii)* simulation quality, which depends upon the extent to which synthetic NICs adequately reproduce the counterfactual trajectories NICs would have experienced in absence of state fragmentation; and *(iii)* contamination effects, arising from the economic effects of independence in other recently formed states.

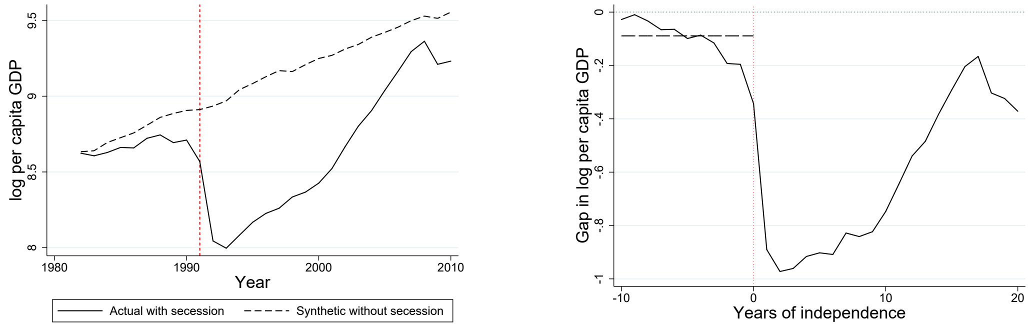
3.4.1 Matching quality

First, recall that the synthetic control method critically hinges upon the close similarity between countries in the pre-independence period to eliminate the potential bias of unobserved heterogeneity.[19] This motivates a closer inspection of the results in trimmed samples. As an alternative way to control for unobserved heterogeneity, one which avoids imposing arbitrary cut-offs to exclude poor-fitting cases, we develop a difference-in-difference estimator along the lines of Campos, Coricelli, and Moretti (2014) to assess whether the per capita GDP discrepancy between NICs and synthetic NICs in any given post-independence year statistically significantly exceeds its 10-year pre-independence average value. Indeed, as NICs are unaffected by state fragmentation in the pre-independence period by construction, the distribution of pre-independence per capita GDP discrepancies between NICs and synthetic NICs is taken to approximate the sampling distribution of the per capita GDP discrepancy between both emanating from their unobserved heterogeneity.

Further illustrating the rationale for this bias-correcting exercise, figure 4 plots the year-on-year per capita GDP discrepancy between Armenia and synthetic Armenia in the period surrounding its 1991 secession from the Soviet Union. In analogy to the Ukranian example discussed in section 3.2, the figure suggests that the Armenian declaration of independence served to lower growth potential in the short to medium run but also remained quite persistent over time. As can be seen in figure 4b, however, Armenia slightly underperforms compared to synthetic Armenia even in the pre-independence period. This suggests that the size and compositional limitations associated with the Armenian donor pool of potential control countries produce a synthetic counterfactual which only imperfectly approximates the economic situation of actual Armenia in absence of state fragmentation. More specifically, the dashed line signifies that the typical *pre-independence* per capita GDP discrepancy between both countries amounted to -9%.

[19] See equations (6A) and (7A).

Figure 4: **Unobserved heterogeneity: Armenia versus synthetic Armenia**



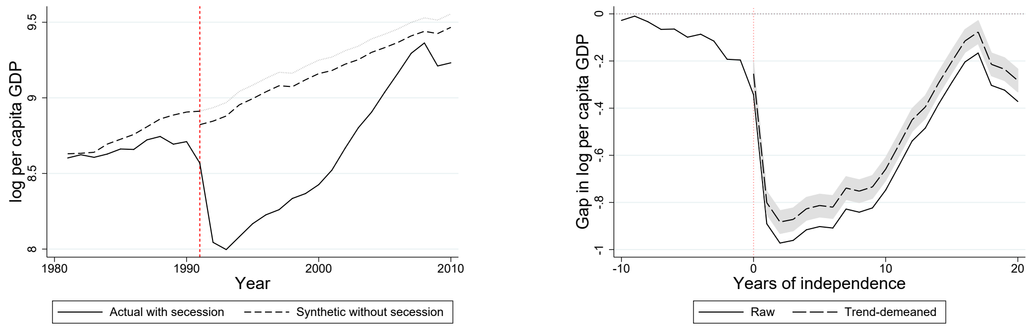
(a) Actual vs. synthetic per capita GDP

(b) The economic impact of secession

Note: Figure 4a plots the log per capita GDP trajectories of Armenia (full line) and synthetic Armenia (dashed line) between 1981 and 2012; figure 4b plots the discrepancy between both trajectories during the same period. The dashed line in the right figure visualizes the average pre-independence discrepancy between both countries.

In this light, one can reasonably expect synthetic Armenia to continue to outperform Armenia in the post-independence period, at a rate determined by their unobserved heterogeneity, regardless of Armenia's decision to secede. To correct for matching quality, we proceed by assuming that the distribution of pre-independence outcome differences between both countries can be taken to reflect the outcome discrepancy emanating from their unobserved heterogeneity. Figure 5a purges the per capita GDP trajectory of synthetic Armenia from matching inaccuracies by removing this average pre-independence discrepancy in the post-independence period, while figure 5b plots the resulting trend-demeaned Armenian independence dividend trajectory. Reassuringly, the figure indicates that the post-independence per capita GDP discrepancy remains unusually large compared to the distribution of discrepancies typically observed in absence of state fragmentation.[20] Thus, the corrected Armenian independence dividend trajectory is unlikely to reflect unobserved heterogeneity but measures the economic impact of secession as intended.

Figure 5: **Accounting for matching quality: Armenia**



(a) Actual vs. synthetic per capita GDP

(b) The economic impact of secession

Note: Figure 5a plots the log per capita GDP trajectory of Armenia (full line) and both the uncorrected (dotted line) and trend-demeaned (dashed line) versions of synthetic Armenia; figure 5b plots the raw (full line) and trend-demeaned (dashed line) independence dividend trajectory, defined in equations (10A) and (1) respectively.

[20] The 95% confidence interval quantifies the uncertainty stemming from matching inaccuracy, where larger variations in the observed pre-independence discrepancies increase measured uncertainty.

To formalize this approach, denoting the weighting vector defining the synthetic counterpart of NIC j by $w_{ij}^* = [w_{1j}^*, \ldots, w_{Ij}^*]$, we define the *trend-demeaned* independence dividend for NIC j, s years after it declared independence as:

$$\hat{\beta_{j,s}}^{tDD} = \underbrace{\left(y_{j,T_0+s} - \sum_{i \neq j} w_{i,j}^* y_{i,T_0+s} \right)}_{\text{raw treatment effect}} - \underbrace{\left(\sum_{t=T_0-10}^{T_0-1} \left(y_{j,t} - \sum_{i \neq j} w_{i,j}^* y_{i,t} \right) \right)}_{\text{matching inaccuracy}} \tag{1}$$

Table A3 reports trend-demeaned independence dividend estimates for each available NIC in our sample. Compared to the raw estimates, trend-demeaned estimates tend to be slightly lower in absolute value. Hence, not correcting for matching quality slightly inflates the estimated independence dividend. Nevertheless, trend-demeaned estimates are quantitatively and qualitatively very similar to their uncorrected counterparts. A closer inspection of the results plotted in figure A3 reveals that, irrespective of the time-horizon, roughly 60 to 80% of NICs suffered economic costs of secession even after correcting for matching quality, with the remaining 20 to 40% experiencing a net independence gain.

3.4.2 Simulation quality

Second, note that the confidence intervals plotted in figures A2 and 5b only express the uncertainty associated with the *magnitude* of the estimated gaps, either across NICs or relative to the pre-independence period. One additional source of uncertainty concerns their *reliability*, which critically hinges on the extent to which synthetic control countries adequately reproduce the per capita GDP trajectories NICs would have experienced in absence of state fragmentation. To the extent that they do not, estimated independence dividends may not only be attributed to the decision to secede but also to poor simulation quality.[21] To study the robustness of the results in this regard, we extend the placebo test approach developed by Abadie et al. (2010) to quantify the probability of obtaining estimates of this magnitude by pure chance. To do so, we reapply the synthetic control method to each potential control country in a particular NIC's donor pool.[22] As the countries involved are unaffected by state breakup by construction, the resulting distribution of 'placebo' dividends is taken to approximate the sampling distribution of the independence dividend estimate under the null hypothesis of a zero effect.

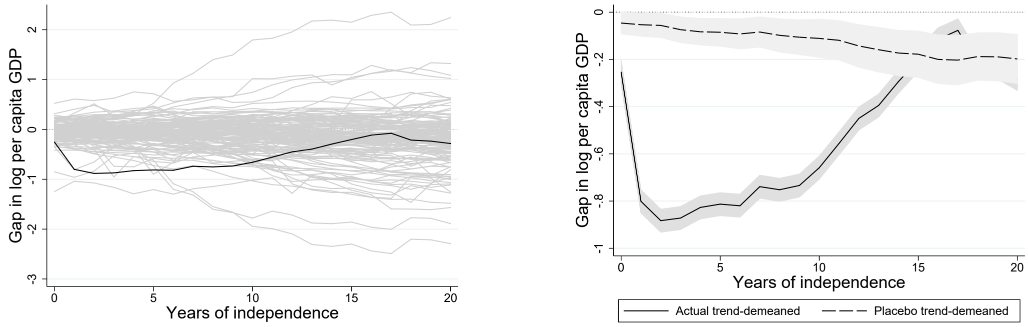
Reconsidering the Armenian example, figure 6 plots the actual trend-demeaned Armenian independence dividends against the distribution of trend-demeand placebo gaps, resulting from an application of the synthetic control algorithm to each of its 153 potential control countries. Although placebo countries tend to under-perform somewhat *vis-à-vis* their synthetic counterparts as well, their per capita GDP trajectories track each other much more closely, especially in the short- to medium run. Moreover, in stark con-

[21] In terms of our model, poor simulation quality primarily originates from differing transitory shocks or, equivalently, cross-country residual variability, see equation (9A).

[22] Eliminating observations pertaining to the NIC itself in the process, to avoid contamination effects.

trast to actual Armenia, per capita GDP discrepancies in its placebo group typically do not react strongly, if at all, when their corresponding comparison country is assumed to have declared independence. This underlines the capacity of the simulation procedure to approximate the economic behavior of countries in absence of state fragmentation.

Figure 6: **Trends in per capita GDP: Armenia versus placebo NICs**

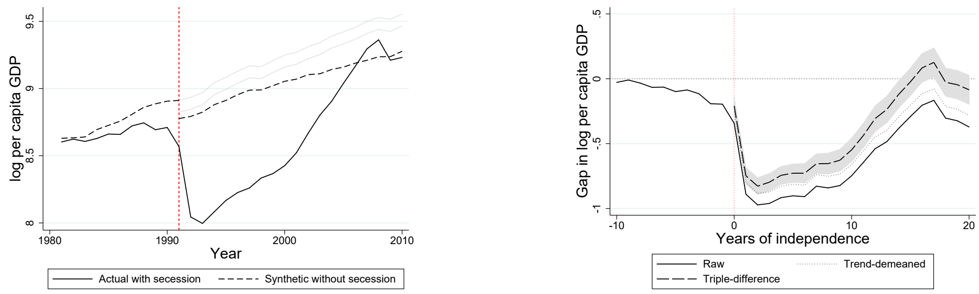


(a) Armenian vs. placebo trajectories (b) Armenian vs. placebo distributions

Note: Figure 6a plots trend-demeaned Armenian independence dividend estimates (black line) against the trend-demeaned placebo independence dividends pertaining to its 154 potential control countries (grey lines); figure 6b plots the corresponding distribution of the actual (full line) and placebo (dashed line) estimates.

Nevertheless, placebo countries also have a tendency to under-perform vis-á-vis their synthetic counterparts. To account for simulation inaccuracies, we assume that the distribution of placebo estimates approximates the sampling distribution of independence dividend estimates under the null hypothesis of a zero effect. Figure 7a corrects the trend-demeaned trajectory of synthetic Armenia by also removing the typical trend-demeaned discrepancy observed in its placebo group. Figure 7b now only find evidence of the Armenian trend-demeaned independence dividend trajectory lying outside the distribution of trend-demeaned placebo gaps in the short to medium run. The long term negative Armenian independence dividend estimates, on the other hand, are consistent with the null hypothesis of a zero effect and thus may merely reflect simulation inaccuracies.[23]

Figure 7: **Accounting for matching & simulation quality: Armenia**



(a) Actual vs. synthetic per capita GDP (b) The economic impact of secession

Note: Figure 7a plots the log per capita GDP trajectory in Armenia (full line), the uncorrected and trend-demeaned (dotted lines) as well as the triple-difference (dashed line) versions of synthetic Armenia; figure 7b plots the raw (full line), trend-demeaned (dotted line) and triple-difference (dashed line) independence dividend trajectory, defined in equations (10A), (1) and (2) respectively.

[23]The 95% confidence interval quantifies uncertainty the stemming from matching & simulation inaccuracy, where both larger pre-independence discrepancies and greater post-independence outcome deviations in placebo countries increase measured uncertainty.

Formally, indexing the control countries in NIC j's donor pool by $k \in [1, \ldots, K_j]$, the *triple-difference* estimate of the independence dividend s years after secession is given by

$$\hat{\beta}_{j,s}^{DDD} = \Bigg[\underbrace{\Big(y_{j,T_0+s} - \sum_{i \neq j} w_{i,j}^{*} y_{i,T_0+s} \Big)}_{\text{raw treatment effect}} - \underbrace{\Big(\sum_{t=T_0-10}^{T_0-1} \Big(y_{j,t} - \sum_{i \neq j} w_{i,j}^{*} y_{i,t} \Big) \Big)}_{\text{matching inaccuracy}} \Bigg] - \underbrace{\frac{1}{K_j} \sum_{k \neq j}^{K_j} \Bigg[\Big(y_{k,T_0+s} - \sum_{i \neq k, i \neq j} w_{i,k}^{*} y_{i,T_0+s} \Big) - \Big(\sum_{t=T_0-10}^{T_0-1} \Big(y_{k,t} - \sum_{i \neq k} w_{i,k}^{*} y_{i,t} \Big) \Big) \Bigg]}_{\text{simulation inaccuracy}} \qquad (2)$$

Country-specific triple-difference estimates of the independence dividend are reported in table A3. Compared to their uncorrected counterparts, triple-difference estimates of the independence dividend also tend to be lower in absolute value. That being said, a look at figure A3 reveals that correcting for matching as well as simulation quality does not qualitatively affect our previous conclusions. Thus, our estimates indicate that declaring independence tended to be costly in the long run for about 50% of the NICs in our sample whereas only 35% of them experienced a long run independence gain.

3.4.3 Contamination effects

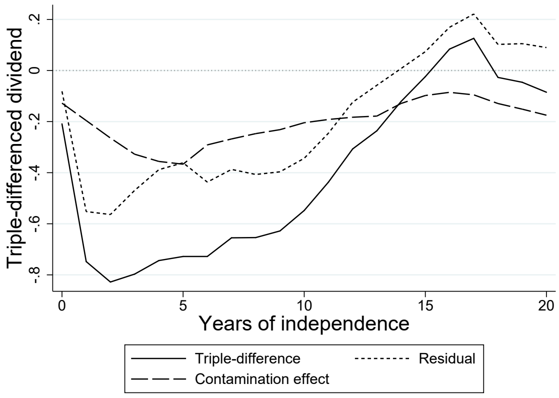
Third, note that the spatio-temporal clustering of state entry may give rise to spillover effects.[24] Indeed, although their respective governments may have had little influence over them, contamination effects may explain the severe independence costs estimated for former members of the Soviet and Yugoslav multi-state systems (see figure 11). To study their potential relevance, we disentangle the 'pure' independence effect from potential contamination effects by parametrically computing the 'pure' economic impact of independence as the residual from a regression of a specific NIC's triple-difference independence dividend trajectory on the triple-difference independence dividend trajectories of all other recently formed states. Indeed, as this residual vector is orthogonal to the included independence dividend trajectories by construction, it serves as a conservative estimate of the 'pure' economic impact of the isolated independence declaration.

Turning once again to the Armenian example, figure 8 plots the parametric decomposition of its triple-difference independence dividend trajectory into a contamination effect and the 'pure', residual economic impact of independence. The figure suggests that contamination effects built up in the short to medium run yet remained persistently negative in the longer run and thus partially explain the Armenian independence cost. Nevertheless, the decomposition also confirms that the pure, residual economic effect of the Armenian independence declaration was negative in the short to medium run, suggesting

[24] Among other factors, contemporary state entry may affect growth potential in former country members through trade disruptions (Head, Mayer, & Ries, 2010), collapse of international payments systems (Åslund, 2012) or border wars (Bates et al., 2007).

that Armenia would have experienced a decennial per capita GDP dip irrespective of the adverse contamination effects that can plausibly be attributed to the Soviet break-up.

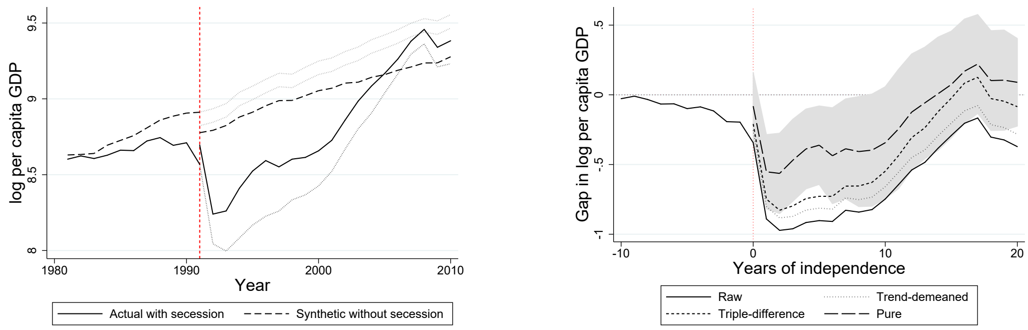
Figure 8: **Decomposing the net independence gain: Armenia**



Note: The figure plots the parametric decomposition of the triple-difference Armenian independence dividend trajectory (full line) into a contamination effect (long-dashed line) and a residual effect (short-dashed line).

Conceptually, one can think of this approach as purging the observed triple-difference Armenian per capita GDP trajectory from contamination effects by removing parametrically estimated contamination effects in the post-independence period, as shown in figure 9a. Figure 9b once again indicates that the poor Armenian growth performance in the post-independence period may be partially driven by economic effects of independence in other recently formed states. Nevertheless, converging evidence suggests that the Armenian independence declaration would have resulted in a decennial per capita GDP dip even in the absence of further Soviet disintegration.

Figure 9: **The pure economic effect of independence in Armenia**



(a) Actual vs. synthetic per capita GDP

(b) The economic impact of secession

Note: Figure 9a plots actual (dotted line) and contamination-corrected (full line) Armenian per capita GDP against uncorrected, trend-demeaned (dotted lines) and triple-difference (dashed line) GDP per capita in synthetic Armenia; figure 9b plots raw (full line), trend-demeaned (dotted line), triple-difference (short-dashed line) and pure (dashed line) independence dividend trajectories defined in equations (10A), (1), (2) and (4) respectively.

Formally, to identify the contamination effects experienced by NIC j, we limit attention to NICs that became independent in a time window of 10 years around its own independence declaration. First, we regress NIC j's triple-difference independence dividend trajectory on those of the L_j other NICs. In order not to exhaust degrees of freedom[25], we estimate a parsimonious model that selects the included contamination effects through Efron, Hastie, Johnstone, and Tibshirani's (2004) least angle regression algorithm:[26]

$$\hat{\beta}^{DDD}_{j,s} = \lambda_0 + \sum_{l \neq j}^{L_j} \lambda_l \hat{\beta}^{DDD}_{l,s} + \epsilon_{j,s} \tag{3}$$

where the previous discussion clarifies that $\forall l \in L_j : T_l \in (T_j - 10, \ldots, T_j + 10)$.

Subsequently, we rely on the parametric approximation of the aggregated contamination effect, $\sum_{i \neq j}^{I} \hat{\lambda}_i \hat{\beta}^{DDD}_{i,s}$, to estimate the pure economic impact associated with the independence declaration of NIC j, s years after independence as

$$\begin{aligned}\hat{\beta}^{pure}_{j,s} = & \underbrace{\Bigg[\Big(y_{j,T_0+s} - \sum_{i \neq j} w^*_{i,j} y_{i,T_0+s}\Big)}_{\text{raw treatment effect}} - \underbrace{\Big(\sum_{t=T_0-10}^{T_0-1}\Big(y_{j,t} - \sum_{i \neq j} w^*_{i,j} y_{i,t}\Big)\Big)\Bigg]}_{\text{matching inaccuracy}} - \underbrace{\sum_{l \neq j}^{L_j} \hat{\lambda}_l \hat{\beta}^{DDD}_{l,s}}_{\text{contamination effect}} - \\ & \underbrace{\frac{1}{K_j}\sum_{k \neq j}^{K_j}\Bigg[\Big(y_{k,T_0+s} - \sum_{i \neq k, i \neq j} w^*_{i,k} y_{i,T_0+s}\Big) - \Big(\sum_{t=T_0-10}^{T_0-1}\Big(y_{k,t} - \sum_{i \neq k} w^*_{i,k} y_{i,t}\Big)\Big)\Bigg]}_{\text{simulation inaccuracy}}\end{aligned} \tag{4}$$

To estimate the degree of uncertainty, we bootstrap $\hat{\beta}^{pure}_{j,s}$ by bootstrapping (i) the time window utilized to remove matching inaccuracies where, in each bootstrap sequence, $\min t \in (T_0 - 10, \ldots, T_0 - 1)$; (ii) the subsample of potential control countries, $K \subseteq K_j$, considered to remove simulation inaccuracies; and (iii) the subsample of other NICs, $L \subseteq L_j$, included to remove contamination effects. Thus, measured uncertainty increases in the variability of pre-independence discrepancies, post-independence outcome deviations in placebo countries and estimates of aggregated contamination effects.

Country-specific estimates of the pure economic impact of secession, reported in table A3, tend to have the same sign as their triple-difference counterparts while also being slightly lower in absolute value in the short to medium run. Thus, spillover effects mainly appear to affect the economic outlook in NICs in the first 10 post-independence years. Figure A3 illustrates that 20% of NICs appear to have suffered a pure long run economic independence cost while a similar fraction experienced a pure independence gain.

3.4.4 Comparison of the aggregate results

To conclude, figure 10 provides a bird's-eye view of the implications of these bias-correction inferential exercises by plotting the various population-weighted independence dividend estimates discussed in this section. To adequately represent estimation uncertainty, in each case, standard errors of the aggregate independence dividend estimates are computed by block bootstrapping over countries while randomly drawing the yearly indepen-

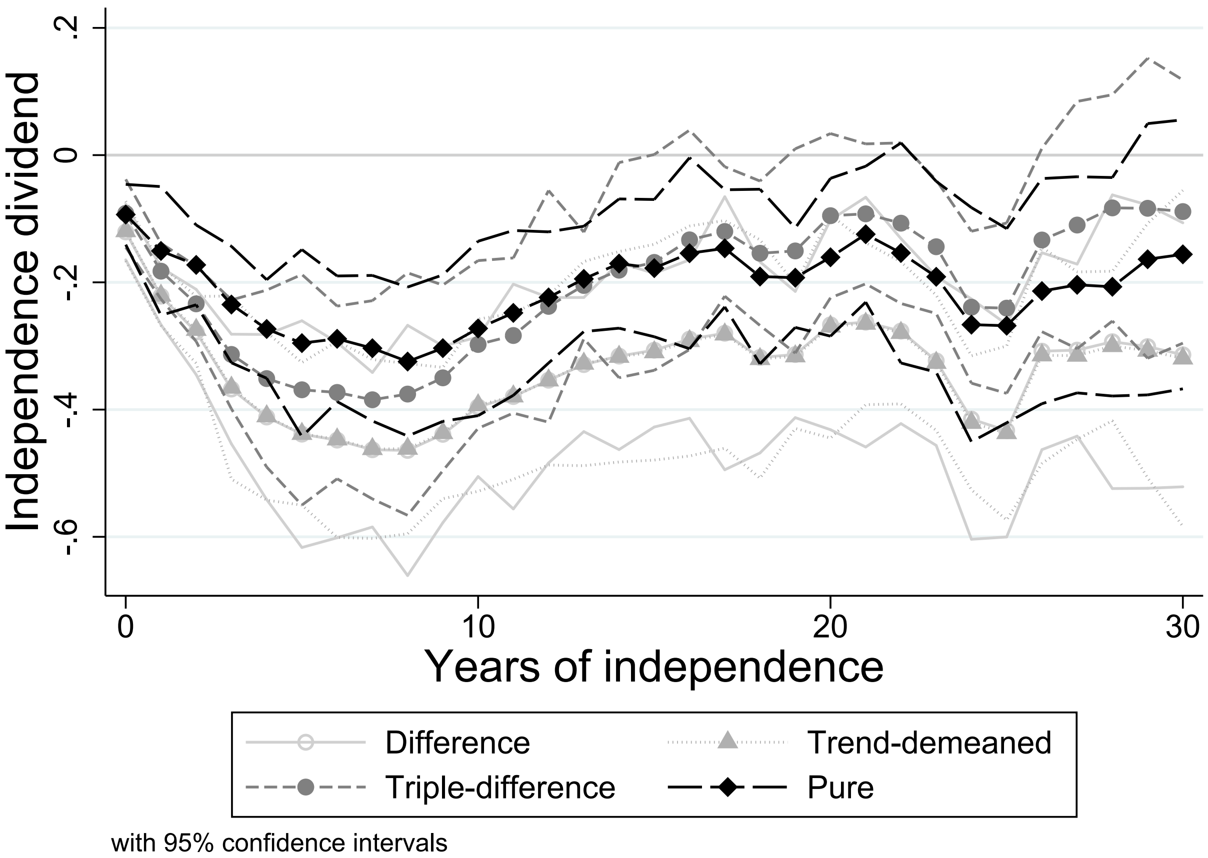
[25]As the L_j concurrent trajectories may outnumber NIC j's available independence dividend estimates.
[26]The least angle regression estimator is implemented by Efron et al.'s (2004) *lars*-command in Stata 13.1.

dence dividend estimates from a normal distribution with mean and standard deviation as determined by the corresponding country-level independence dividend estimates in each iteration of the bootstrap.[27] Thus, measured uncertainty increases in the variability of independence dividend estimates across (populous) NICs *and* estimation uncertainty in the country-specific independence dividend estimates as described in the previous subsections.

Irrespective of the estimator, there is a clear pattern of negative independence dividends in the short to medium run while cross-country heterogeneity obscures a clear assessment of the long run independence payoff. The raw estimates seem more sensitive to simulation than to matching inaccuracy, as correcting for simulation quality yields the most pronounced upward correction. There is no strong evidence that contamination effects meaningfully affected growth potential in the full sample, as the triple-difference and pure independence dividend estimates largely coincide. Interestingly, estimates of the pure economic impact of secession are fairly stable across bootstrap iterations and do not depend strongly on the time window considered to remove matching inaccuracy, the available potential control countries or the potential contamination effects considered.

[27] More specifically, note that equation (2) allows us to estimate both the triple-difference estimate of the independence dividend of NIC i in its s^{th} post-independence year, $\hat{\beta}_{i,s}^{DDD}$, as well as the associated standard error, which we denote by $\hat{\sigma}_{i,s}^{DDD}$. Therefore, when bootstrapping the population-weighted triple-difference independence dividend, in any bootstrap iteration including NIC i, we draw $\beta_{i,s}^{DDD} \sim \mathcal{N}\left(\hat{\beta}_{i,s}^{DDD}, \left(\hat{\sigma}_{i,s}^{DDD}\right)^2\right)$ to accurately capture estimation uncertainty of the estimate for $\beta_{i,s}^{DDD}$.

Figure 10: **Semi-parametric estimates of the economic impact of secession**



Note: The figure plots the yearly average *uncorrected* synthetic control estimates of the independence dividend (hollow dots), as defined in equation (10A), against the corresponding trend difference-in-difference (triangles), triple-difference (squares) and pure (diamonds) estimates related to equations (10A), (1), (2) and (4). Block-bootstrapped 95% confidence intervals are based on 500 iterations and are robust against heteroskedasticity, serial correlation at the country level and estimation uncertainty in individual independence dividend estimates. The number of years after secession is indicated on the horizontal axis.

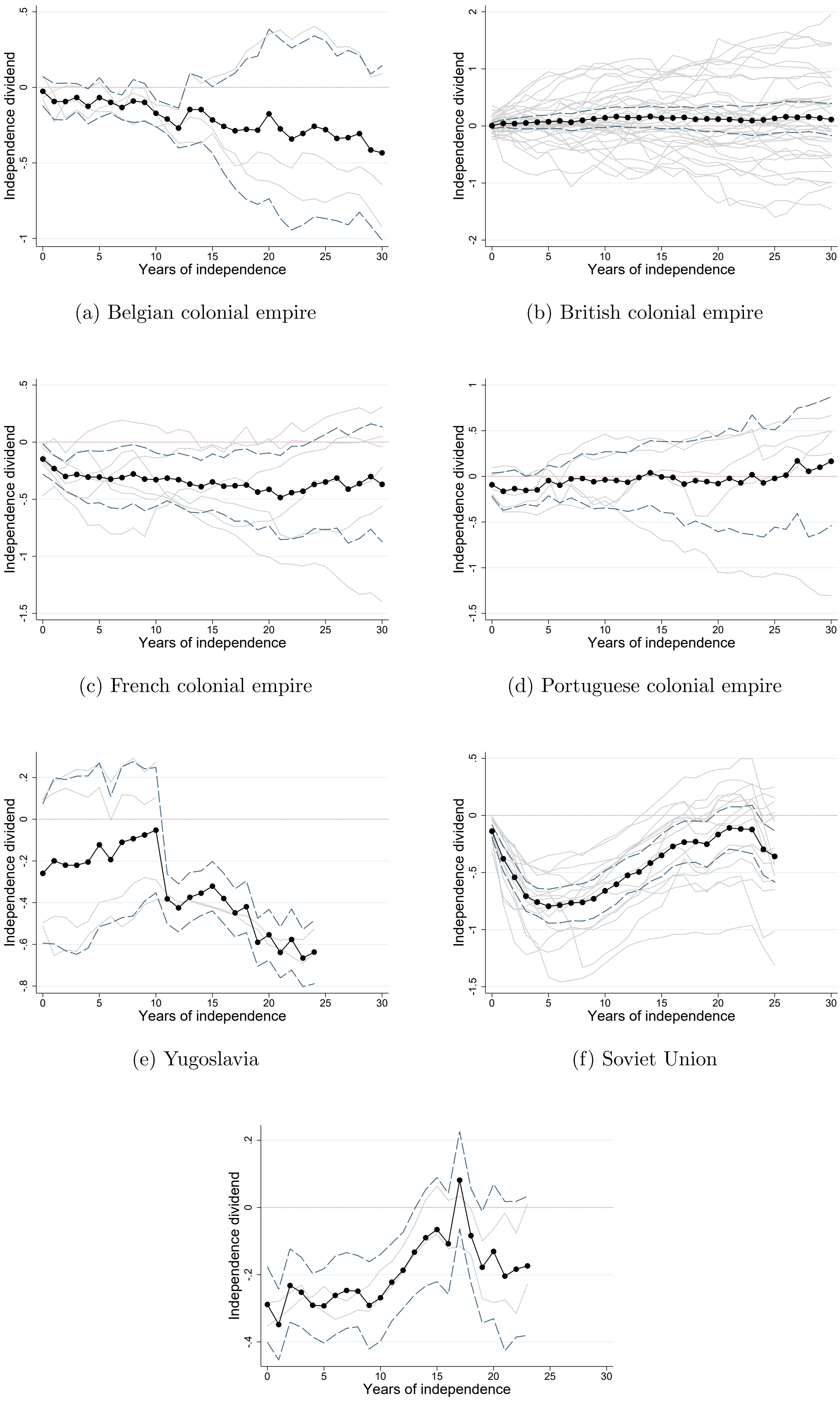
3.5 Selected results

To put more empirical flesh on the bones, figure 11 supplements the large-scale econometric analysis of the previous sections by highlighting the results pertaining to a number of historical instances of state fragmentation and connecting them to the existing literature on this topic. More specifically, the figure characterizes the economic consequences associated with the disintegration of the Belgian, British, French and Portuguese colonial empires, comparing these with the implied economic effects stemming from the dissolution of the Soviet Union, Yugoslavia and - most recently - Czechoslovakia.

Recall that the identity of the mother country is thought to play an important role in explaining cross-country heterogeneity in the economic impact of secession, see section 1. In this regard, it is often argued that former British colonies prospered relative to their French, Spanish, Portuguese and Belgian counterparts because the British left behind better institutions (Acemoglu et al., 2001, 2002) and were more successful in educating their dependents (Grier, 1999). Interestingly, our results are largely consistent with this story and suggest that, in sharp contrast to NICs with other colonial heritages, former British colonies did not tend to suffer adverse economic consequences as a result of becoming independent and even enjoyed an independence gain of around 10% in the medium run. More surprisingly, although Belgian and Portuguese dominations are often considered the most detrimental and exploitative (Bertocchi & Canova, 2002), only former Belgian colonies appear to have suffered adverse economic consequences of colonial demise in the form of an increasing reduction in per capita GDP that amounted to 50% of potential per capita GDP in the 30^{th} post-independence year. Similarly, former French colonies appear to have suffered a persistent independence cost of around 20% in per capita GDP terms.

In the same vein, Roland (2002), Svejnar (2002) and Fidrmuc (2003) maintain that the extent of state capture and rent-seeking was more pervasive in the Soviet Union than in other Eastern and Central European countries and that these differential initial conditions, often proxied by the distance from Western Europe, go a long way in explaining the under-performance of former Soviet states vis-á-vis other NICs in the region. Furthermore, they argue that this mechanism may have been amplified by differential prospects of EU membership, which enhanced incentives for law enforcement and protection of property rights in potential member states. Our results are testimony to this, indicating that the group of former Soviet members initially suffered the most adverse effects of state breakup. In comparison, the Yugoslavian successor states seem initially less affected by state fragmentation while the economic costs associated with the Czechoslovakian 'Velvet Divorce' were both more modest and much less persistent. However, the available estimates also suggest that the violent Yugoslavian disintegration entailed persistent adverse economic consequences for the earliest breakaway regions.

Figure 11: **Triple-difference estimates: historical instances of state fragmentation**



(a) Belgian colonial empire

(b) British colonial empire

(c) French colonial empire

(d) Portuguese colonial empire

(e) Yugoslavia

(f) Soviet Union

(g) Czechoslovakia

Note: The figures plot yearly, triple-difference estimates of the independence dividend trajectories associated with selected historical instances of state fragmentation. Each gray line plots the trajectory of a specific former member state; the black lines depict the aggregate independence dividend trajectory for each independence wave; the dashed lines depict the 95% confidence interval. Block-bootstrapped confidence intervals are based on 500 iterations and are robust against heteroskedasticity, serial correlation at the country level and estimation uncertainty in individual independence dividend estimates. The number of years after independence are indicated on the horizontal axis.

3.6 Robustness results

3.6.1 Accounting for transition costs

One remaining worry with the independence dividend estimates in figures 11e through 11g is that they may be partially driven by the costs these NICs experienced from their transition from planned to market economies, since these transition costs could have materialized irrespective of their choices to secede. Indeed, as the transition process temporally coincided with the independence declarations of the countries involved, transition costs may at least partially explain the severe independence costs estimated for the breakup of the former Soviet, Yugoslav and Czechoslovakian states. To study their potential relevance, we aim to disentangle the independence effect from these transition costs by semi-parametrically computing transition costs in a group of 'established' transition countries, namely those transition countries that did *not* recently declare independence, and subsequently subtracting these from the independence dividend estimates pertaining to newly formed transition countries. To the extent that the distribution of transition costs in these established transition countries can be taken to reflect the transition costs that would have been experienced by the new transition countries in our sample, this approach allows us to purge the relevant independence dividend estimates from transition costs.

Figure 12a demonstrates this reasoning by reconsidering the Ukrainian example from section 3.2 and compares the synthetic control estimates for the per capita GDP discrepancy between Ukraine and its synthetic counterpart in the 20-year period around its declaration of independence with the contemporary per capita GDP discrepancies observed in the five established transition countries mentioned in Roland (2000), i.e. Albania, Bulgaria, Hungary, Poland and Romania. As can be seen, all these established transition countries also started to underperform with respect to their synthetic counterparts despite *not* having declared independence in 1991. More specifically, these results suggests that they effectively incurred a persistent transition cost of around 20% to 40% in per capita GDP terms. To account for the transition costs that would also be experienced by Ukraine absent secession, we assume that the distribution of estimated transition costs in these five established transition countries can be taken to approximate the portion of the per capita GDP discrepancy between Ukraine and synthetic Ukraine that stems from Ukraine's transition process towards a market economy.

More specifically, figure 12b corrects the triple-differenced trajectory of synthetic Ukraine by also removing the typical triple-differenced discrepancy observed in the group of established transition countries. Doing so, figure 12c nevertheless indicates that the post-independence per capita GDP discrepancy between Ukraine and synthetic Ukraine remains unusually negative compared to the contemporary distribution of discrepancies observed in established transition countries. Moreover, figure 12c also shows that correcting the Ukrainian triple-differenced independence dividends for the discrepancy that can reasonably be attributed to transition costs only results in a modest upward revision of its independence dividend trajectory, suggesting that the bulk of Ukraine's underperformance in the post-independence period stems from its independence declaration or, in other words, that only a small part of it appears to be driven by transition costs.

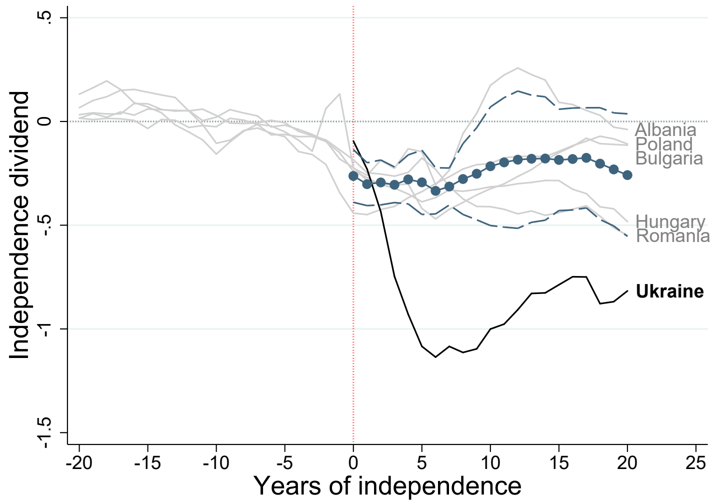
Generalizing this approach, denote the contemporary transition countries in newly formed transition country j's donor pool by $m \in [1, \ldots, M_j]$ and define the *quadruple-difference* estimate of its independence dividend in the s^{th} post-independence year as

$$\begin{aligned}\hat{\beta}^{pure}_{j,s} = & \Bigg[\underbrace{\Big(y_{j,T_0+s} - \sum_{i \neq j} w^*_{i,j} y_{i,T_0+s}\Big)}_{\text{raw treatment effect}} - \underbrace{\Big(\sum_{t=T_0-10}^{T_0-1} \Big(y_{j,t} - \sum_{i \neq j} w^*_{i,j} y_{i,t}\Big)\Big)}_{\text{matching inaccuracy}}\Bigg] - \\ & \underbrace{\frac{1}{M_j}\sum_{m \neq j}^{M_j}\Bigg[\Big(y_{m,T_0+s} - \sum_{i \neq m, i \neq j} w^*_{i,m} y_{i,T_0+s}\Big) - \Big(\sum_{t=T_0-10}^{T_0-1} \Big(y_{m,t} - \sum_{i \neq m} w^*_{i,m} y_{i,t}\Big)\Big)\Bigg]}_{\text{transition cost}} - \\ & \underbrace{\frac{1}{K_j}\sum_{k \neq j}^{K_j}\Bigg[\Big(y_{k,T_0+s} - \sum_{i \neq k, i \neq j} w^*_{i,k} y_{i,T_0+s}\Big) - \Big(\sum_{t=T_0-10}^{T_0-1} \Big(y_{k,t} - \sum_{i \neq k} w^*_{i,k} y_{i,t}\Big)\Big)\Bigg]}_{\text{simulation inaccuracy}}\end{aligned} \quad (5)$$

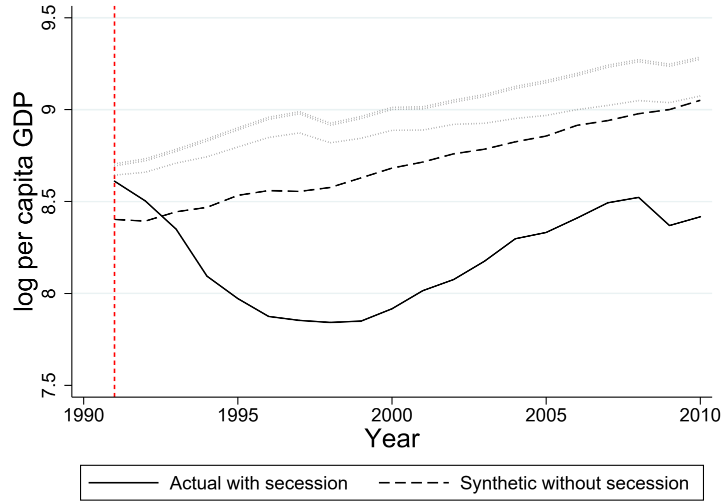
Figure 12d plots the aggregate triple-differenced yearly independence dividend estimates for all newly formed transition countries in our sample, namely the successor states to the Soviet Union, Yugoslavia and Czechoslovakia, and compares these with their quadruple-differenced counterparts. As can be seen, purging the triple-differenced independence dividends from discrepancies that can plausibly be attributed to the transition process results in a modest upward revision of the independence dividend. Nevertheless, as the figure also shows, most of the under-performance of these newly formed transition countries can be reasonably attributed to their decision to declare independence, such that our prior conclusions remain qualitatively unaffected. These findings are consistent with the idea that secessions are more disruptive in economic terms within planned economies, due to the collapse of the integrated economic space and the severing of supply chains as well as the weakness of the institutions in their constituent parts.

For completeness, figure A4 plots the quadruple-differenced versions of the estimated independence dividend trajectories in figure 11 while table A4 compares the country-specific quadruple-differenced independence dividends of the newly formed transition countries in our sample with their triple-differenced counterparts.

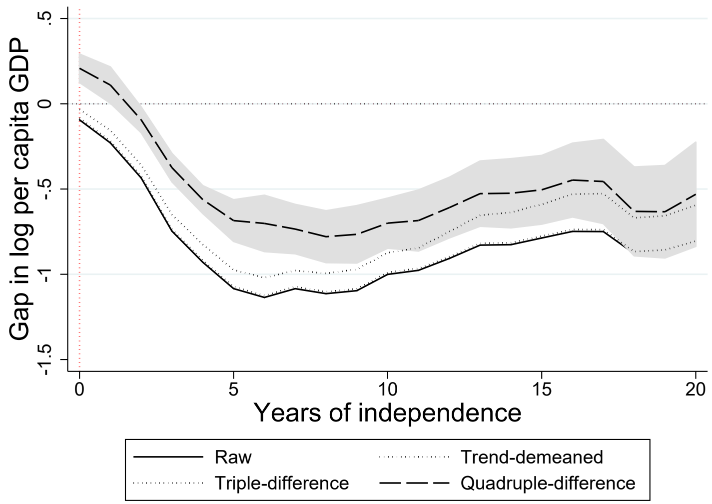
Figure 12: **Accounting for transition costs**



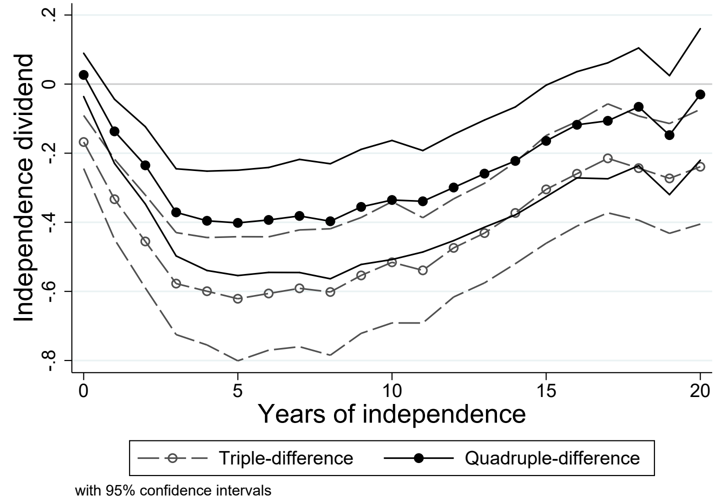
(a) Actual vs. synthetic per capita GDP: Ukraine & established transition countries



(b) Actual vs. synthetic per capita GDP (Ukraine)



(c) The economic impact of secession (Ukraine)



(d) Semi-parametric estimates of the independence dividend: transition countries

Note: Figure 12a plots triple-difference Ukrainian independence dividend estimates (black line) against the triple-difference independence dividends pertaining to five established transition countries (grey lines) along with their 95% confidence intervals (blue dashed lines). Figure 12b plots the log per capita GDP trajectory in Ukraine (full line), the uncorrected , trend-demeaned and triple-difference (dotted lines) as well as the quadruple-difference (dashed line) versions of synthetic Ukraine; figure 12c plots the raw (full line), trend-demeaned and triple-difference (dotted line) as well as the quadruple-difference (full line) independence dividend trajectories that are respectively defined in equations (10A), (1), (2) and (5). Figure 12d plots the yearly population-weighted triple-difference (hollow circles) and quadruple-difference (full circles) estimates of the independence dividend in newly formed *transition* countries, as outlined in equations (2) and (5). Block-bootstrapped 95% confidence intervals are based on 500 iterations and are robust against heteroskedasticity, serial correlation at the country level and estimation uncertainty in individual independence dividend estimates. Years of independence are indicated on the horizontal axis.

3.6.2 Additional robustness checks

Finally, figure A5 briefly reports the results of three additional robustness checks. First, one additional worry may be that the lack of spatial constraints on the composition of the sample of potential control countries makes the results susceptible to differences in regional development. Although this risk should be mitigated by constructing counterfactual countries based on the similarity of (the drivers of) their economic performance in the ten-year period preceding their independence declaration, we also redid the analysis imposing a spatial constraint limiting the sample of potential control countries to those countries with an intercapital distance of maximally 2000 km to the NIC under consideration. We find that the estimated net independence gain becomes even more negative. More specifically, comparing the economic performance of newly formed states with comparable economies that are located in direct geographical proximity suggests that declaring independence lowers per capita GDP by an estimated 50%, and quite persistently so.

An opposite concern might be that allowing nearby countries to serve as potential controls reduces the credibility of the stable unit treatment value assumption, since it is more likely that any economic effects of independence will spill over to nearby countries, making it tenuous to maintain that the group of control countries is itself unaffected by the independence declaration. To verify the relevance of this concern, we also redid the analysis requiring an intercapital distance of minimally 4000 km to be included in the pool of potential control countries. As doing so once again yields highly similar results, we find no empirical evidence of severe violations of the stable unit treatment value assumption.

Similarly, a third concern may be that the only restriction imposed on the composition of the sample of potential control countries, namely that NICs cannot serve as potential control countries in the first 10 years after independence, conflicts with our finding of a persistent independence effect. As a result, there might be a perceived risk of counterfactual trajectories being contaminated with the effects of independence stemming from their constituent parts. Although this contamination risk should also be mitigated by conditioning inclusion in the group of control countries on the comparability of economic growth performances in the immediate pre-independence period, we nevertheless re-estimated our baseline specification imposing a more stringent temporal constraint requiring potential control countries to be independent for at least 30 years at the time that the NIC under consideration became independent. Doing so, however, yields highly comparable independence dividend estimates which once again suggest slightly *more* adverse independence effects in the long run. At face value, these robustness results thus suggest that our baseline results offer a lower bound for the true average independence cost in our sample.

Finally, to demonstrate that these findings appear to hold irrespective of the estimation procedure employed, Reynaerts and Vanschoonbeek (2016) also formulate a parametric approach to estimate the independence payoff, obtaining similar results.

4 Two-step estimates of the determinants of the independence dividend

So far, our findings suggest that the independence dividend tends to be substantial, negative and fairly persistent. Yet, there also is considerable heterogeneity in the economic impact of secession across countries and time. From a policy perspective, one lingering issue concerns understanding the economic channels through which secessionist processes affect growth potential in NICs. Building on prior results, this final extension proposes a two-step approach to shed some light on the primary economic channels determining both the sign as well as the magnitude of country-specific independence payoffs. After outlining the estimation strategy, we present the baseline results along with some robustness checks.

4.1 Estimation strategy

To evaluate the various channels through which the decision to secede might affect growth potential, we refine the methodology put forward by Campos et al. (2014) and regress the semi-parametric independence dividend estimates on several potential determinants. Doing so, we limit our attention to the first 30 years following secession and consider the potential channels most commonly cited in the theoretical literature, see section 1: the presence of (dis)economies of scale, as proxied by population size and trade openness; the impact of military conflict, as captured by the per capita number of battle deaths; the relevance of ongoing processes of democratization, incarnated in an index of democracy; and the effect of macroeconomic uncertainty, as reflected in a financial crisis dummy indicating known episodes of debt and/or banking crises.

In determining the relative importance of these potential determinants, a first complication is that the values of these growth determinants may not be perfectly comparable over time. The gradual liberalization of trade, for instance, makes it likely that more recent NICs also benefit from more trade opportunities, obscuring a clear definition of trade openness. To improve intertemporal comparability, we normalize each continuous predictor by dividing it by its population-weighted sample-average value in each year. A second obvious difficulty is that the interpretation of the regression coefficients is sensitive to the scale of the inputs. Therefore, in a subsequent step, all normalized continuous predictor variables are standardized to convert them to a common scale.[28,29] Additionally, to take into account that global patterns in trade liberalization may have gradually reduced the economic cost of secession, in addition to region, mother country and years-of-independence dummies, all specifications also include (calendar) year dummies. Finally, to control for the potential biases stemming from poor matching and simulation quality,

[28]Dummy variables remain unchanged since their coefficients can already be interpreted directly.

[29]As noted by Schielzeth (2010), there has been some controversy about this approach to measure the relative importance of predictor variables since there is no unique way to partition the variation in the dependent variable when predictor variables are correlated. Firth (1998) provides a more comprehensive overview of the relevant literature.

we also include as control variables both the average per capita GDP discrepancy observed in the last 10 years *prior* to independence as well as the average contemporary placebo independence dividend estimated in the group of potential control countries.

More specifically, denoting the estimated net gain of independence of NIC i located in region r pertaining to the s^{th} post-independence year, which coincides with calendar year t, by $\hat{\beta}_{i,r,t,s}$, we estimate the following second-step regression model:

$$\hat{\beta}_{i,r,t,s} = \alpha_1 + \alpha_2 \bar{\beta}_i + \alpha_3 \bar{\beta}^{placebo}_{i,r,t,s} + \lambda \mathbf{X}_{i,r,t,s} + \eta_s + \delta_r + \mu_t + \phi_m + \epsilon_{i,r,t,s} \tag{6}$$

where $\bar{\beta}_i = \frac{1}{10} \sum_{t=T_0-10}^{T_0-1} \hat{\beta}_{i,t}$ denotes the average per capita GDP discrepancy between NIC i and its synthetic version in the pre-independence period; $\bar{\beta}^{placebo}_{i,r,t,s} = \frac{1}{K_i} \sum_{j \neq i} \hat{\beta}_{j,r,t,s}$ captures the average contemporary placebo dividend estimate in NIC i's group of K_i potential control countries; $\mathbf{X}_{i,r,t,s}$ denotes the $(1 \times X)$ vector of normalized and standardized predictors of the independence dividend; η_s captures the S years-of-independence fixed effects; δ_r denotes the R region fixed effects; μ_t contains the T year fixed effects; ϕ_m is a dummy identifying the mother country of each NIC i; and the error term, $\epsilon_{i,r,t,s}$, collects all random, transitory shocks to the independence dividend. Note that the coefficients collected in λ reflect the standard deviation elasticity of the independence dividend with respect to each of its normalized predictors, such that larger coefficients identify more influential predictors. In this light, it makes sense to define the *relative importance* of each normalized predictor $x \in X$, λ_x, as the expected percentage change in the independence dividend associated with its standard deviation increase.

One final concern is related to the fact that the dependent variable is itself an estimate, implying that OLS-estimation may produce underestimated standard errors (Lewis & Linzer, 2005). To adequately represent first-stage estimation uncertainty, standard errors are computed by a bootstrapping procedure that comprises re-sampling over the set of independence dividends with replacement while randomly drawing the values of the dependent variable from a normal distribution with mean and standard deviation equal to those of the corresponding first-step independence dividend estimates in each subsequent iteration of the bootstrap.[30] As an alternative, since the standard errors from the estimated dependent variable are known, we also estimate an 'estimated dependent variable'-model where consistent standard errors are computed using the feasible generalized least squares (FGLS) weighting method proposed by Lewis and Linzer (2005, p. 351-352). The advantage of the latter method is that it gives more weight to more precisely estimated independence dividends, thereby correcting for the differential precision in the first-step estimation, at the cost of rendering the interpretation of the coefficients more ambiguous.

[30]See also footnote 27.

4.2 Baseline results

Table 4 summarizes the results of this exercise relying on the most conservative triple-difference estimates of the independence dividend, reporting the results separately for both standard error estimation procedures outlined in the previous subsection. As expected, one common pattern is the significant and positive persistence in per capita GDP discrepancies. Thus, the finding that NICs experiencing large per capita GDP discrepancies *vis-á-vis* their synthetic versions during the pre-independence period tend to continue doing so after becoming independent confirms the importance of controlling for matching quality in our estimation model. Similarly, the existence of a negative relation between estimated independence dividends in NICs and the average placebo independence dividend estimate points to the importance of explicitly controlling for simulation quality, though the FGLS standard errors suggest that the issue of simulation inaccuracy is not very significant.

More importantly, columns (1a) and (1b) provide a first, rudimentary glance into the economic mechanisms behind our empirical findings. Several explanations to account for the observed variation in the estimated net gains of secession are confirmed. First, we obtain positive estimates for the effect of trade openness, suggesting that unusually trade open NICs have a clear *ceteris paribus* tendency to economically outperform their more protectionist counterparts during the first 30 years of independence in our sample. This corroborates previous theoretical findings which suggest that trade openness counteracts the adverse effects of decreased domestic market size. Secondly, all else equal, the adverse effects of declaring independence appear to be decreasing in the population size of the newly formed state. In line with the endogenous growth literature, we thus find evidence that more populous NICs tend to economically outperform their less-populated counterparts, pointing to the relevance of economies of scale. Third, democratization appears as another channel through which newly formed states can reduce the adverse economic effects associated with their decision to secede, resonating with prior empirical evidence that democracy does cause growth, to paraphrase Acemoglu, Suresh, Restrepo, and Robinson (2014). Fourth, as suggested by the political science literature, we also find evidence that post-independence growth prospects worsen if independence declarations are followed by military violence or financial crises, although the latter effect cannot be precisely estimated in the FGLS-regression. Finally, a quick comparison of the coefficient values suggests that trade openness is the strongest predictor of post-independence economic performance in our sample, identifying the ability to maintain trade relations as the most imperative channel to improve economic growth prospects in newly formed states.

Columns (2a) and (2b) dig a little deeper by also including initial per capita GDP as a crude way to control for omitted variable bias, by accounting for the level of economic development. Perhaps surprisingly, although the results indicate that more developed NICs experience slightly lower independence costs, the effect of the initial economic situation is so small that it can not be precisely estimated through FGLS. More importantly, our previous findings are unaffected by explicitly controlling for economic development.

Subsequently, columns (3a) and (3b) verify Qvortrup's (2014) claim that declaring independence by referendum might be particularly conducive to peaceful political settlements, such that successful independence referendums potentially mitigate at least some of the economic costs of secession. To do so, we add a dummy variable indicating the occurrence of a successful independence referendum prior to the official declaration of independence. We find some evidence that these procedural aspects may matter, as the coefficient of the referendum-dummy is positive, statistically significant and comparatively large in the bootstrapped model. Our previous findings once again remain unaltered.

Columns (4a) and (4b), finally, demonstrate how these findings also appear robust to the inclusion of a battery of fixed effects, by adding dummy variables indicating membership to the EU, the OPEC, the NATO, the African Union and ASEAN to the previous model. Interestingly, we find evidence that oil-producing countries faced lower independence costs in both models, suggesting that the immovability of natural resources can give a slight economic edge to the economic outlooks of aspirant states endowed with them.

It may be useful to compare these results with the various fixed effects in equation (6). To do so, figure A6 summarizes the estimated year fixed effects, years-of-independence fixed effects, region fixed effects and the estimated relevance of the identity of the mother country. First of all, in line with the existing literature, figure A6a finds evidence of global trade liberalization gradually lowering the economic costs of independence as there is a clear and persistent upward trend in the estimated year fixed effects, which might co-explain the recent surge in separatism throughout the world. Additionally, figure A6b shows that independence dividends, all else equal, do not appear to slowly erode but on the contrary have a tendency to grow more negative over time. Figure A6c shows that newly formed states in the region of Europe & Central Asia have a clear tendency to outperform NICs in other parts of the world, potentially as a result of the growth-enhacing effects of (the requirements to gain access to) the European internal market. Finally, figure A6d suggest that the decolonization had less adverse economic effects than the more recent break-ups of the Soviet Union, Yugoslavia and Czechoslovakia, suggesting that there may have been economic gains associated with the elimination of colonial drain.

In conclusion, we find consistent evidence across a variety of estimation models that the adverse effects of independence are decreasing in the population size of newly formed states and that they are also mitigated when opening up to trade or building more democratic institutions. Nevertheless, independence costs also unambiguously appear to increase in the incidence of military violence in the post-independence period. Trade openness emerges as the strongest predictor of post-independence economic performance in all these specifiactions, underscoring the prevalence of this topic in many contemporary independence discussions. We fail to find clear-cut cross-model evidence on the relevance of macroeconomic uncertainty and the economic desirability of declaring independence by referendum.

Table 4: **Determinants of the independence dividend**

	Bootstrap				Feasible Generalized Least Squares			
Channel	(1a)	(2a)	(3a)	(4a)	(1b)	(2b)	(3b)	(4b)
$\bar{\beta}$	0.01**	0.01**	0.00**	-0.03**	0.05***	0.05***	0.06***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
$\bar{\beta}^{placebo}$	-0.13***	-0.13***	-0.14***	-0.11**	-0.08	-0.08	-0.06	-0.04
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)
Trade openness	0.14***	0.13***	0.13***	0.11***	0.26***	0.26***	0.27***	0.28***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Population size	0.03***	0.03***	0.03***	0.03***	0.06***	0.06***	0.05***	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Democracy	0.03**	0.03**	0.02**	0.03**	0.03**	0.03**	0.04***	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Financial crisis	-0.03**	-0.04**	-0.03**	-0.08**	0.03	0.03	0.00	0.05
	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
Battle deaths	-0.02**	-0.02**	-0.02**	-0.03***	-0.04***	-0.04***	-0.04***	-0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Initial per capita GDP		0.00**	0.01**	-0.04***		0.01	0.01	-0.03*
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.02)
Referendum dummy			0.08**	0.07**			-0.07*	-0.06
			(0.04)	(0.03)			(0.04)	(0.04)
EU dummy				0.15**				-0.03
				(0.09)				(0.10)
OPEC dummy				0.24***				0.24***
				(0.05)				(0.06)
NATO dummy				-0.14**				-0.09
				(0.09)				(0.10)
African Union dummy				-0.45***				-0.05
				(0.07)				(0.12)
ASEAN dummy				0.34***				0.01
				(0.09)				(0.10)
Observations [# countries]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]
Adjusted R^2	0.28	0.28	0.28	0.30	0.28	0.28	0.28	0.30
Region dummies	yes	yes	yes	yes	yes	yes	yes	yes
Mother country dummies	yes	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Years-of-independence dummies	yes	yes	yes	yes	yes	yes	yes	yes
Bootstrap iterations	500	500	500	500	.	.	.	.

Note: This table reports estimates of the relative importance, as defined in equation (6), of several determinants of the triple-difference independence dividend, as defined in equation (2). The first 4 columns report bootstrapped standard errors, based on 500 iterations and robust against estimation uncertainty in the first-step independence dividend estimates. The last 4 columns report standard errors computed by the feasible generalized least squares method proposed by Lewis and Linzer (2005, p. 351-352).

4.3 Robustness results

One important limitation with the relative importance estimates in the previous section is that they ignore the potential endogeneity between estimated treatment effects (in this case, the estimated independence dividends) and their potential determinants. More specifically, if economic agents in NICs possess prior knowledge on any efficiency gains that are associated with the independence declaration at the time that economically relevant decisions are made, this might lead to endogeneity bias if these decisions are partially determined by prior beliefs about the (unobserved) efficiency gain of independence. To address the endogeneity issue, we borrow and adapt an estimator from the total factor productivity literature to parametrically proxy and control for the unobserved efficiency gain of independence, based on the assumption that fixed capital investment decisions of NICs contain useful information on the (perceived) efficiency gain of independence.

Although the technical details are relegated to appendix C, the main idea is to follow Olley and Pakes (1996) in assuming that fixed capital investments take time before they augment the existing capital stock, implying that both national fixed capital stocks ($K_{i,r,t,s}$) and fixed capital investments ($I_{i,r,t,s}$) can function as a signal for the perceived efficiency gain of independence in year t, as higher expected efficiency gains should make it more profitable to increase the (future) fixed capital stock. This, in turn, suggests that we can control for the endogeneity concern by adding a so-called control function to regression equation (6), that proxies for the perceived efficiency gain of independence in NIC i in year t through a polynomial of order O in $K_{i,r,t,s}$ and $I_{i,r,t,s}$.[31] The underlying identification assumption is that, conditional on contemporary values of the fixed capital stock, national fixed capital investment demand can serve as a good proxy for the perceived efficiency gain of independence such that its inclusion should alleviate simultaneity concerns.

Table 5 reports the corresponding endogeneity-robust estimates for the relative importance of the potential determinants of the triple-difference independence dividends, in analogy to the estimation results reported in table 4.[32] Surprisingly, we find little evidence for the presence of endogeneity bias as most endogeneity-robust coefficients do not statistically significantly differ from their baseline counterparts. Nevertheless, we find moderate evidence that one potential channel might be endogenous. More specifically, the estimated favorable impact of trade openness shrinks considerably once we parametrically control for the unobserved efficiency gain of independence. This finding is consistent with the conjecture that trade openness is mostly a byproduct of growth-enhancing independence declarations, rather than the other way around.

Reassuringly, the endogeneity-robust results nevertheless confirm that our baseline findings seem qualitatively robust to endogeneity. Interesingly, the four channels of trade openness, population size, democracy and military conflict that emerge as the most consistent predictors of the independence dividend all attain comparable estimated relative importance across the (unweighted) bootstrapped endogeneity-robust estimation models.

Finally, tables A5 through A7 also verify the sensitivity of the results with respect to the specific first-step estimation procedure utilized to estimate the independence dividend, by sequentially replacing the triple-difference independence dividend estimates with their raw, trend-demeaned and placebo-demeaned counterparts in our baseline regression. Once again, we obtain broadly similar results, such that our findings seem to hold irrespective of the first-step estimation procedure utilized to estimate the independence dividend.

[31] The empirical application relies on a second order polynomial, such that $O = 2$. Data on gross fixed capital, gross fixed capital formation and the yearly depreciation rate of fixed capital are derived from Feenstra, Inklaar, and Timmer (2015) and World Bank (2016) - see appendix A.

[32] Note that the sample size differs between the baseline and robustness models, due to the large number of missing observations for fixed capital (formation) necessary to implement the control function approach. Therefore, strictly speaking, there is no direct comparison between the baseline and endogeneity-robust results as any discrepancy could be either due to the inclusion of the control function, the composition of the estimation sample, or both. We ignore this slight complication in the text.

Table 5: **Endogeneity-robust determinants of the independence dividend**

	Bootstrap				Feasible Generalized Least Squares			
Channel	(1a)	(2a)	(3a)	(4a)	(1b)	(2b)	(3b)	(4b)
$\bar{\beta}$	-0.00**	-0.02**	-0.02**	-0.05***	0.01	0.01	0.02	0.03*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
$\bar{\beta}^{placebo}$	-0.06**	-0.08**	-0.06**	-0.00**	0.07	0.06	0.05	0.04
	(0.06)	(0.05)	(0.06)	(0.06)	(0.09)	(0.09)	(0.09)	(0.11)
Trade openness	0.03**	0.04***	0.04***	0.03**	0.13***	0.14***	0.14***	0.14***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)
Population size	0.04***	0.05***	0.04***	0.02***	0.14***	0.14***	0.14***	0.18***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)	(0.04)
Democracy	0.04***	0.04***	0.03***	0.02**	0.06***	0.06***	0.06***	0.07***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Financial crisis	-0.01**	-0.01**	-0.02**	-0.04**	-0.09	-0.08	-0.07	-0.15*
	(0.04)	(0.04)	(0.04)	(0.04)	(0.07)	(0.07)	(0.07)	(0.08)
Battle deaths	-0.02**	-0.02**	-0.02**	-0.03***	-0.02***	-0.02***	-0.02***	-0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Initial per capita GDP		0.04***	0.03***	-0.02**		0.01	0.01	0.02
		(0.01)	(0.01)	(0.01)		(0.03)	(0.03)	(0.04)
Referendum dummy			0.04**	0.04**			-0.05	-0.06
			(0.03)	(0.03)			(0.05)	(0.05)
EU dummy				0.11**				0.16
				(0.08)				(0.14)
OPEC dummy				0.33***				-0.09
				(0.08)				(0.19)
NATO dummy				-0.09**				-0.21
				(0.09)				(0.14)
African Union dummy				-0.54***				-0.36**
				(0.08)				(0.18)
ASEAN dummy				-0.04**				-0.03
				(0.07)				(0.12)
Observations [# countries]	1568 [57]	1568 [57]	1568 [57]	1568 [57]	1568 [57]	1568 [57]	1568 [57]	1568 [57]
Adjusted R^2	0.44	0.45	0.45	0.48	0.44	0.45	0.45	0.48
Region dummies	yes	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Years-of-independence dummies	yes	yes	yes	yes	yes	yes	yes	yes
Control function [$O = 2$]	yes	yes	yes	yes	yes	yes	yes	yes
Bootstrap iterations	500	500	500	500	.	.	.	.

Note: This table reports estimates of the relative importance of several determinants of the triple-difference independence dividend using the control function approach summarized in equation (17A). As explained in appendix C.1, all estimation models contain a second-order polynomial in fixed capital stocks and gross fixed capital formation, both expressed as shares of GDP. The first 4 columns report bootstrapped standard errors, based on 500 iterations and robust against estimation uncertainty in the first-step independence dividend estimates. The last 4 columns report standard errors computed by the feasible generalized least squares estimated-dependent-variable method proposed by Lewis and Linzer (2005, p. 351-352).

5 Conclusion

In tandem with the worldwide surge in separatism, independence movements increasingly tend to defend their cause based on economic considerations. However, whether or not there are economic benefits from declaring independence remains largely unexplored. This study is the first to examine the economic impact of secession for a broad sample of newly independent countries, focusing on a large time period covering the years 1950 to 2016.

Relying on a semi-parametric estimation strategy to control for the confounding effects of past GDP dynamics, anticipation effects, unobserved heterogeneity between newly formed and more established states, model uncertainty as well as effect heterogeneity, we present robust evidence that secession historically hampered economic growth potential.

Our central results suggest that the decision to secede reduced per capita GDP anywhere between 20% and 40% in the NICs in our sample. From a methodological perspective, we develop a novel quadruple-difference procedure that sequentially accounts for matching quality, simulation quality and contamination effects, providing informative statistical inference on the reliability of synthetic control estimates of treatment effects. Applying this procedure, we confirm the existence of a statistically significant negative independence dividend in the short to medium run, with cross-country heterogeneity slighlty obscuring the average long-run impact of independence. Moreover, a robustness check finds that the economic underperformance of newly formed *transition* countries appears to be in large part driven by their independence declaration and *not* by the transition process, suggesting that the existing literature may have overestimated transition costs.

A more speculative second part then tries to move beyond mere description and looks for empirical evidence of more general lessons that can be learned. We develop a two-step estimator to verify to what extent the estimated independence dividends of the NICs in our sample meaningfully correlate with a number of their underlying characteristics. In line with much of the existing literature, we find tentative evidence that the adverse effects of independence decrease in population size, suggesting that the independence in our sample are partially driven by the losses in economies of scale which newly formed states inevitably incur when declaring independence. Nevertheless, we also find evidence that NICs can mitigate at least some of the adverse effects of declaring independence by opening up to trade, improving democratic institutions and avoiding violent secession. Interestingly, trade openness often emerges as the strongest predictor of post-independence economic performance, in line with prevalence of this topic in many contemporary independence debates. We fail to find clear-cut evidence on the relevance of macroeconomic uncertainty and the economic desirability of declaring independence by referendum. A robustness check combines both parametric and semi-parametric techniques to control for endogeneity and demonstrates the stability of these results.

In light of these findings, two additional future research questions naturally arise. First, do these results generalize to other regions contemplating independence today? Indeed, since we estimate the average treatment effect on the treated, extrapolation to contemporary and future aspirant countries may be problematic to the extent that they differ non-trivially from the historical cases of state fragmentation considered in this analysis.[33] Second, do these findings generalize to the non-economic spectrum? This also remains an open question, as independence may come with compensating political (Alesina & Spolaore, 1997, 2003), re-distributional (Bolton & Roland, 1997) or other effects.

[33]Note, for instance, that there are no historical examples of highly economically developed regions declaring independence from their mother countries, such that it remains unclear what the historical experience can teach us about contemporary cases such as Scotland or Catalonia. Nevertheless, the two-step approach developed in section 4 is a first attempt to offer some guidance, by at least verifying to what extent historical patterns in the estimated independence dividends conform to theoretical expectations.

References

Abadie, A., Diamond, A., & Hainmueller, J. (2007). Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *NBER Technical Working Paper No. 335*, 1–49.

Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association*, *105*(490), 493–505.

Abadie, A., Diamond, A., & Hainmueller, J. (2014). Comparative Politics and the Synthetic Control Method. *American Journal of Political Science*, *59*(2), 495–510.

Abadie, A., & Gardeazabal, J. (2003). The Economic Costs of Conflict : A Case Study of the Basque Country. *American Economic Review*, *93*(1), 113–132.

Acemoglu, D., Johnson, S., & Robinson, J. A. (2002). Reversal of Fortune : Geography and Institutions in the Making of the Modern World Income Distribution. *The Quarterly Journal of Economics*, *117*(4), 1231–1294.

Acemoglu, D., Simon, J., & Robinson, J. A. (2001). The Colonial Origins of Comparative Development: An Empirical Investigation. *The American Economic Review*, *91*(5), 1369–1401.

Acemoglu, D., Suresh, N., Restrepo, P., & Robinson, J. A. (2014). Democracy Does Cause Growth. *NBER Working Paper No. 20004*, 1–66.

Ackerberg, D. A., Caves, K., & Frazer, G. (2015). Identification Properties of Recent Production Function Estimators. *Econometrica*, *83*(6), 2411–2451.

Aghion, P., & Howitt, P. (2009). *The Economics of Growth*. Cambridge: MIT Press.

Alesina, A., Ozler, S., Roubini, N., & Swagel, P. (1996). Political Instability and Economic Growth. *Journal of Economic Growth*, *1*(2), 189–211.

Alesina, A., & Spolaore, E. (1997). On the Number and Size of Nations. *Quarterly Journal of Economics*, *112*(4), 1027–1056.

Alesina, A., & Spolaore, E. (2003). *The Size of Nations*. Cambridge: MIT Press.

Alesina, A., Spolaore, E., & Wacziarg, R. (2000). Economic Integration and Political Disintegration. *American Economic Review*, *90*(5), 1277–1296.

Alesina, A., & Wacziarg, R. (1998). Openness, Country Size and Government. *Journal of Public Economics*, *69*(3), 305–321.

Alouini, O., & Hubert, P. (2012). Country Size , Economic Performance and Volatility. *Documents de Travail de l'OFCE*, 1–27.

Armstrong, H., & Read, R. (1995). Western European Micro-States Regions : The Advantages and EU Autonomous of Size and Sovereignty. *World Development*, *23*(I), 1229–1245.

Armstrong, H., & Read, R. (1998). Trade and Growth in Small States: The Impact of Global Trade Liberalisation. *The World Economy*, *21*(4), 563–585.

Åslund, A. (2012). Why a Breakup of the Euro Area Must Be Avoided: Lessons from

Previous Breakups. *Peterson Institute for International Economics Policy Brief 12-20*, 1–15.

Backus, D. K., Kehoe, P. J., & Kehoe, T. J. (1992). In Search of Scale Effects in Trade and Growth. *Journal of Economic Theory*, *58*(2), 377–409.

Barro, R. J., & Lee, J.-W. (1994). *Data Set for a Panel of 138 Countries.* Retrieved from http://admin.nber.org/pub/barro.lee/

Barro, R. J., & Lee, J.-W. (2012). *A New Data Set of Educational Attainment in the World, 1950-2010.* Retrieved from http://www.barrolee.com/data/full1.htm

Barro, R. J., & Sala-i Martin, X. (2004). *Economic Growth* (2nd ed.). Cambridge: MIT Press.

Bates, R., Coatsworth, J., & Williamson, J. (2007). Lost Decades: Postindependence Performance in Latin America and Africa. *The Journal of Economic History*, *67*(04), 917–943.

Bertocchi, G., & Canova, F. (2002). Did Colonization Matter for Growth? An Empirical Exploration into the Historical Causes of Africa's Underdevelopment. *European Economic Review*, *46*(10), 1851–1871.

Bethany, L., & Gleditsch, N. P. (2005). Monitoring Trends in Global Combat: A New Dataset of Battle Deaths. *European Journal of Population*, *21*(2-3), 145–166.

Bolton, P., & Roland, G. (1997). The Breakup of Nations: a Political Economy Analysis. *Quarterly Journal of Economics*, *112*(4), 1057–1090.

Campos, N., Coricelli, F., & Moretti, L. (2014). Economic Growth and Political Integration : Estimating the Benefits from Membership in the European Union Using the Synthetic Counterfactuals Method. *IZA Discussion Paper No. 8162*, 1–40.

Center for Systemic Peace. (2015). *Polity IV Annual Time-Series, 1800-2015.* Retrieved from http://www.systemicpeace.org/inscr/p4v2014.xls

CLIO Infra. (2015). *Clio Infra Data Project.* Retrieved from https://www.clio-infra.eu/

Correlates of War Project. (2012). *National Material Capabilities, Version 5.0.* Retrieved 2018-04-05, from http://www.correlatesofwar.org/data-sets/national-material-capabilities/national-material-capabilities-v4-0

Correlates of War Project. (2015). *Trade, Version 4.0.* Retrieved 2018-04-05, from http://www.correlatesofwar.org/data-sets/bilateral-trade/international-trade-1870-2009-v3-0

Curtice, J. (2013). *The Score At Half Time: Trends in Support for Independence.* London.

Dahl, M., Gates, S., Hegre, H., & Strand, H. (2013). Why Waves? Global Patterns of Democratization, 1820-2008. *PRIO Working Paper*, 1–41.

Damijan, J., Damijan, S., & Parcero, O. (2013). Is There a Premium in the Size of Nations. *VIVES Discussion Paper 40*, 1–42.

Das, S., Roberts, M. J., & Tybout, J. R. (2007). Market Entry Costs, Producer Heterogeneity, and Export Dynamics. *Econometrica*, *75*(3), 837–873.

Easterly, W., & Kraay, A. (2000). Small States , Small Problems? Income, Growth and Volatility in Small States. *World Development*, *28*(11), 2013–2027.

Efron, B., Hastie, T., Johnstone, I., & Tibshirani, R. (2004). Least Angle Regression. *The Annals of Statistics*, *32*(2), 407–499.

Fearon, J. D. (1998). Commitment Problems and the Spread of Ethnic Conflict. In D. A. Lake & D. Rothschild (Eds.), *The international spread of ethnic conflict: Fear, diffusion and escalation* (pp. 107–206). Princeton: Princeton University Press.

Fearon, J. D., & Laitin, D. D. (2003a). *Additional Tables for "Ethnicity, Insurgency, and Civil War".*

Fearon, J. D., & Laitin, D. D. (2003b). Ethnicity, Insurgency, and Civil War. *American Political Science Review*, *97*(1), 75.

Feenstra, R. C., Inklaar, R., & Timmer, M. P. (2015). The Next Generation of the Penn World Table. *American Economic Review*, *105*(10), 3150–3182.

Fidrmuc, J. (2003). Economic Reform, Democracy and Growth during Post-Communist Transition. *European Journal of Political Economy*, *19*(3), 583–604.

Financial Times. (2014). *Dispute over Economic Impact of Scottish Independence.* Retrieved from http://www.ft.com/intl/cms/s/2/f14c4912-e640-11e3-9a20-00144feabdc0.html{#}axzz3alvrBE2F

Firth, D. (1998). Relative Importance of Explanatory Variables. In *Conference on statistical issues in the social sciences* (pp. 1774–1775). Stockholm.

Fischer, S. (1994). Russia and the Soviet Union Then and Now. In *The transition in eastern europe, volume 1* (Vol. 1, pp. 221–258).

Freedom House. (2015). *Freedom in the World.* Retrieved from https://freedomhouse.org/sites/default/files/IndividualCountryRatingsandStatus{%}2C1973-2015{%}28FINAL{%}29.xls

Gerring, B. J., Bond, P., Barndt, W. T., & Moreno, C. (2005). Economic Growth: A Historical Perspective. *World Politics*, *57*(3), 323–364.

Gibler, D. M., & Miller, S. V. (2014a). External Territorial Threat, State Capacity, and Civil War. *Journal of Peace Research*, *51*(5), 634–646.

Gibler, D. M., & Miller, S. V. (2014b). *"External Threat, State Capacity and Civil War", Conflict Onset Dataset.* Retrieved from http://file.prio.no/journals/JPR/2014/51/5/ReplicationGibler{&}Miller2014.zip

Giuliano, P., Mishra, P., & Spilimbergo, A. (2013). Democracy and Reforms: Evidence from a New Dataset. *American Economic Journal: Macroeconomics*, *5*(4), 179–204.

Grier, R. M. (1999). Colonial Legacies and Economic Growth. *Public Choice*, *98*(3), 317–335.

Griffiths, R. D., & Butcher, C. R. (2013). Introducing the International System(s) Dataset (ISD), 1816–2011. *International Interactions*, *39*(5), 748–768.

Gupta, D. K., & Venieris, Y. P. (1986). Income Distribution and Sociopolitical Instability as Determinants of Savings : A Cross-sectional Model. *Journal of Political Economy*, *94*(4), 873–883.

Head, K., Mayer, T., & Ries, J. (2010). The erosion of colonial trade linkages after independence. *Journal of International Economics*, *81*(1), 1–14.

Heston, A., Summers, R., & Aten, B. (1994). *Penn World Table Version 5.6.* Retrieved from http://www.rug.nl/research/ggdc/data/pwt/v56/pwt56{_}forweb.xls

Johnson, S., Larson, W., Papageorgiou, C., & Subramanian, A. (2013). Is Newer Better? Penn World Table Revisions and Their Impact on Growth Estimates. *Journal of Monetary Economics*, *60*(2), 255–274.

Jones, C. I. (1999). Growth: With or Without Scale Effects? *American Economic Review*, *89*(2), 139–144.

Jovanovic, B. (1982). Selection and the Evolution of Industry. *Econometrica*, *50*(3), 649–670.

Krugman, P. (1991). Increasing Returns and Economic Geography. *Journal of Political Economy*, *99*(3), 483.

Kuznets, S. (1960). Population Change and Aggregate Output. In G. B. Roberts (Ed.), *Demographic and economic change in developed countries* (Vol. I, pp. 324–351). Cambridge: Columbia University Press.

Lake, D., & O'Mahony, A. (2004). The Incredible Shrinking State: Explaining Change in the Territorial Size of Countries. *Journal of Conflict Resolution*, *48*(5), 699–722.

Lewis, J. B., & Linzer, D. A. (2005). Estimating Regression Models in which the Dependent Variable is Based on Estimates. *Political Analysis*, *13*(4), 345–364.

Linn, J. F. (2004). Economic (Dis)Integration Matters : The Soviet Collapse Revisited. *Paper prepared for the conference on "Transition in the CIS: Achievements and Challenges", Moscow (2004)*, 1–25.

Marschak, J., & Andrews, W. H. (1944). Random Simultaneous Equations and the Theory of Production. *Econometrica*, *12*(3/4), 143–205.

Melitz, M. J. (2003). The Impact of Trade on Intra-Industry Reallocations and Aggregate Industry Productivity. *Econometrica*, *71*(6), 1695–1725.

Melton, J., Meserve, S. A., & Pemstein, D. (2010). Democratic Compromise: A Latent Variable Analysis of Ten Measures of Regime Type. *Political Analysis*, *18*(4), 426–449.

Milner, C., & Westaway, T. (1993). Country Size and the Medium-term Growth Process: Some Cross-country Evidence. *World Development*, *21*(2), 203–211.

Munck, G. L., & Verkuilen, J. (2002). Conceptualizing and Measuring Democracy. Evaluating Alternative Indices. *Comparative Political Studies*, *35*(1), 5–34.

Murdoch, J., & Sandler, T. (2004). Civil Wars and Economic growth: Spatial Dispersion. *American Journal of Political Science*, *48*(1), 138–151.

Nunn, N. (2007). Historical Legacies: A Model Linking Africa's Past to its Current Underdevelopment. *Journal of Development Economics*, *83*, 157–175. doi: 10.1016/j.jdeveco.2005.12.003

Nunn, N. (2008). The Long-Term Effects of Africa's Slave Trades. *Quarterly Journal of Economics*, *123*(1), 139–176.

Olley, S. G., & Pakes, A. (1996). The Dynamics of Productivity in the Telecommunications Equipment Industry. *Econometrica*, *64*(6), 1263–1297.

Onour, I. A. (2013). South Sudan Secession: Modelling the Fiscal Shock and its Spillover Effects. *UNISCI Discussion Paper 33*, 139–152.

Ornaghi, C., & Van Beveren, I. (2011). Using Proxy Variables to Control for Unobservables When Estimating Productivity: A Sensitivity Analysis. *LICOS Discussion Paper No. 28*, 1–45.

Prados De La Escosura, L. (2009). Lost Decades? Economic Performance in Post-Independence Latin America. *Journal of Latin American Studies*, *41*(02), 279.

Qvortrup, M. (2014). Referendums on Independence , 1860 - 2011. *The Political Quarterly*, *85*(1), 57–64.

Ramondo, N., & Rodríguez-Clare, A. (2010). Growth, Size, and Openness: A Quantitative Approach. *American Economic Review*, *100*(2), 62–67.

Reinhart, C. M., & Rogoff, K. S. (2011). From Financial Crisis to Debt Crisis. *American Economic Review*, *101*(5), 1676–1706.

Reynaerts, J., & Vanschoonbeek, J. (2016). The Economics of State Fragmentation: Assessing the Economic Impact of Secession - Addendum. *VIVES Discussion Paper 55*, 1–18.

Robinson, E. A. G. (Ed.). (1960). *Economic Consequences of the Size of Nations: Proceedings of a Conference Held by the International Economic Association.* London: Macmillan.

Rodríguez-Pose, A., & Gill, N. (2005). On the 'Economic Dividend' of Devolution. *Regional Studies*, *39*(4), 405–420.

Rodríguez-Pose, A., & Stermšek, M. (2015). The Economics of Secession: Analysing the Economic Impact of the Collapse of the Former Yugoslavia. *Territory, Politics, Governance*, *3*(1), 73–96.

Roland, G. (2000). *Transition and Economics : Politics, Markets, and Firms* (4th ed.). Cambridge, Massachussetss: MIT Press.

Roland, G. (2002). The Political Economy of Transition. *Journal of Economic Perspectives*, *16*(1), 29–50.

Romer, P. M. (1986). Increasing Returns and Long-run growth. *Journal of Political Economy*, *94*(5), 1002–1037.

Rose, A. (2006). Size Really Doesn't Matter: In Search of a National Scale Effect. *Journal of the Japanese and International Economies*, *20*(4), 482–507.

Rosenbaum, P. R., & Rubin, D. B. (1985). Constructing Control Group Using Multivariate Matched Sampling Methods That Incorporate Propensity Score. *The American Statistician*, *39*(1), 33–38.

Schielzeth, H. (2010). Simple Means to Improve the Interpretability of Regression Coefficients. *Methods in Ecology and Evolution*, *1*, 103–113.

Solow, R. M. (1956). A Contribution to the Theory of Economic Growth. *The Quarterly Journal of Economics*, *70*(1), 65–94.

Somé, M. Z. (2013). *Africa's Wars of Liberation : Impact and Economic Recovery* (Dissertation). University of California.

Somers, K., & Vaillancourt, F. (2014). Some Economic Dimensions of the Sovereignty Debate in Quebec: Debt, GDP and Migration. *Oxford Review of Economic Policy*, *30*(2), 237–256.

Spencer, M. (1998). *Separatism: Democracy and Disintegration.* Maryland: Rowman & Littlefield Publishers.

Spolaore, E. (2008). Civil Conflict and Secessions. *Economics of Governance*, *9*(1), 45–63.

Spolaore, E., & Wacziarg, R. (2005). Borders and Growth. *Journal of Economic Growth*, *10*(4), 331–386.

Streeten, P. (1993). The Special Problems of Small Countries. *World Development*, *21*(2), 197–202.

Suesse, M. (2017). Breaking the Unbreakable Union: Nationalism, Disintegration and the Soviet Economic Collapse. *The Economic Journal*, 1–35.

Sujan, I., & Sujanova, M. (1994). The Macroeconomic Situation in the Czech Republic. *CERGE-EI Working Paper Series 46*, 1–31.

Svejnar, J. (2002). Transition Economies: Performance and Challenges. *Journal of Economic Perspectives*, *16*(1), 3–28.

The Conference Board. (2015). *Total Economy Database.* Retrieved from https://www.conference-board.org/data/economydatabase/index.cfm?id=27762

The Madison Project. (2017). *The Madison Project, version 2017.* Retrieved from http://www.ggdc.net/maddison/maddison-project/data.htm.

Tilly, C. (1990). *Coercion, Capital and European States.* Cambridge: MA: Blackwell.

United Nations Development Program. (2015). *Mean Years of Schooling (of Adults) (Years).* Retrieved from http://hdr.undp.org/sites/default/files/103006.xls

United Nations Population Division. (2015). *World Population Prospects: The 2015 Revision.* Retrieved from http://esa.un.org/unpd/wpp/DVD/Files/1{_}Indicators

Vanhanen, T. (2014). *Measures of Democracy 1810-2012 [computer file]. FSD1289, version 6.0.* Retrieved from http://www.fsd.uta.fi/en/data/catalogue/FSD1289/meF1289e.html

Walker, E. (1998). Chechnya and the Economic Consequences of Secession. *Contemporary Caucasus Newsletter*, *3*(7), 7–10.

World Bank. (2016). *World Development Indicators.* Retrieved 2017-01-10, from http://data.worldbank.org/data-catalog/world-development-indicators

World Resources Institute. (2015). *CAIT Climate Data Explorer.* Retrieved from http://cait.wri.org

Yarbrough, B., & Yarbrough, R. (1998). Unification and Secession: Group Size and 'Escape from Lock-In'. *Kyklos*, *51*, 171–195.

A Data construction and sources

In order to ensure a dataset that is as complete as possible, we draw on a wide variety of data sources to construct several variables used in the empirical analysis. This section describes in more detail the variable-specific data manipulation procedure utilized to construct these variables. Table A1 summarizes the data sources and construction for the main variables of interest (indicated by $^{\diamond}$) while also reporting some diagnostics.

GDP per capita (baseline)$^{\diamond}$: To construct our baseline estimates of the country-specific per capita GDP trajectories, we rely on a third-order polynomial approximation procedure that builds on Fearon and Laitin (2003a). We depart from the estimates for per capita GDP measured in 1990 Geary-Khamis dollars and reported by The Madison Project (2017). This series starts in 1950 and ends in 2010 and provides 8477 (63.2%) of our 13405 country years. Subsequently, we maximally extend these estimates forward to 2016 and backwards to 1960 using the growth rate of real per capita GDP provided by the World Bank (2016), thereby adding another 2651 (19.8%) country-year observations. Afterward, we remove 16 isolated country-year observations pertaining to the pre-independence situation in the group of former Soviet states. In a next step, we regress these baseline log per capita GDP estimates on log per capita CO2 emissions, as reported by the World Resources Institute (2015), a vector of year dummies, a region dummy for each of the seven regions distinguished by the World Bank (2016), their squared and cubic values as well as all possible interactions up to the third order. We then use the growth rate of the predicted per capita GDP trajectories to maximally extend the baseline series forward and backwards, adding another 869 (6.5%) observations.[34] Data on the country-specific emission levels of CO2 are available between 1950 and 2012 and, in itself, these correlate fairly strongly with the baseline per capita income estimates, at 0.83 for their 10050 common observations. That being said, with a correlation coefficient of 0.89, predicted per capita GDP levels correlate even more strongly with the baseline estimates. Finally, evaluating this least squares third-order polynomial model's predictive accuracy on an observation-by-prediction basis, we find that 55% of the baseline log per capita GDP observations fall within the 99% confidence intervals of their predicted counterparts. Although this indicates a fairly good match between the model's data-generating process and our reference series, this further motivates extending the reference data by relying on the growth rates implied in these alternative predictions, rather than the predicted values themselves.

In order to further extend the existing data series, we repeat this exercise by sequentially using information on log per capita CO2 emissions contained in World Bank (2016) and primary energy consumption as reported by Correlates of War Project (2012). The World Bank (2016) data on CO2 emissions runs from 1960-2016 and also shows a strong

[34]There remain several countries lacking any income estimates in the baseline series, but for which data on the level of CO2 emissions are available. For these countries, we use the predicted per capita GDP trajectories instead.

correlation with baseline log per capita GDP (0.83 for their 8840 common observations). The Correlates of War Project (2012) data on primary energy consumption runs from 1816-2012 and shows a moderately positive correlation with baseline log per capita GDP (0.66 for their 8802 common observations). Nevertheless, the third-order polynomial predicted per capita GDP trajectories once again correlate even more strongly with their baseline counterparts, yielding a correlation coefficient of respectively 0.89 and 0.85, while the predictive accuracy of these models respectively attains 54% and 58%. Once again using the growth rates of predicted real per capita GDP to further extend the existing series forward and backwards adds another 361 (2.69%) observations for each of both sources. The remaining 687 (5.1%) country-year observations remain missing.[35]

GDP per capita (alternative)$^{\Diamond}$*:* In order to make sure that our findings are not driven by the data construction process, we also construct alternative per capita GDP estimates. To do so, we synthetize a wide variety of data sources containing information on country-specific levels of real per capita GDP. More specifically, we consider the information in Barro and Lee (1994); Heston, Summers, and Aten (1994); The Madison Project (2017); Feenstra et al. (2015); The Conference Board (2015); World Bank (2016).

To derive our alternative per capita GDP trajectory, we apply the following so-called regular data construction procedure: *(i)* linearly interpolate missing observations in all available data sources, *(ii)* selecting the most complete source (i.e. the source with the most country-year observations) as the baseline series. Subsequently, *(iii)* from the alternative data sources, select the dataset for which the overlapping path is most strongly correlated with that of the base series and *(iv)* use the variation in the alternative source to approximate as much missing values in the base series as possible. First, if the non-overlapping observations in the alternative source pertain to a country already appearing in the base series, use the growth rates in the alternative source to maximally extend the base series forward and backwards. Second, if the non-overlapping observations in the alternative data source pertain to a country not covered in the base series, express its per capita GDP relative to that of the United States to approximate missing observations in the base series. Finally, *(v)* repeat steps *(iii)*-*(v)* for each remaining data source.

Table A1, then, summarizes the percentage contribution of each data source to the total number of observations as well as the correlation with the base series. Interestingly, the correlation between the common 11892 baseline and alternative per capita GDP estimates equals 0.96, giving further credence to our polynomial approximation approach to construct our baseline estimates. Unsurprisingly, our empirical results are not sensitive to which measure of economic performance we use. Therefore, to economize on space, further results pertaining to the alternative per capita GDP estimates are not reported.

Population$^{\Diamond}$*:* Data on the evolution of country-specific population size between 1950

[35]In each data source, we only rely on non-zero observations and treat zero observations as missing.

and 2015 are obtained from Barro and Lee (1994); Heston et al. (1994); The Madison Project (2017); CLIO Infra (2015); Feenstra et al. (2015); United Nations Population Division (2015); World Bank (2016). Aggregation across datasets is obtained by applying the regular data construction procedure outlined earlier. Doing so, our consolidated indicator of population size is constructed by: *(i)* linearly interpolating missing observations in all data sources; *(ii)* selecting the most complete as the baseline series; *(iii)* selecting the alternative dataset for which the overlapping path is most strongly correlated with that of the base series; *(iv)* using the variation in the alternative source to approximate as much missing values in the base series as possible; and *(v)* repeating steps *(iii)*-*(v)* for each remaining data source. As the correlation between all these different sources is nearly perfect (cf. Table A1), our population variable is not sensitive to the selection of the base series or the specific sequence of extensions.

Educational attainment$^{\diamondsuit}$*:* In order to construct a consolidated index representing the average years of education attained in each country-year, we first gather data on the average years of education as reported by Barro and Lee (1994, 2012); CLIO Infra (2015); United Nations Development Program (2015) and secondary education enrollment rates from Barro and Lee (1994); World Bank (2016). In a second step, since most of these data are only reported in five-yearly intervals, we linearly interpolate missing observations in each dataset. This seems reasonable, as far as educational attainment evolves gradually over time. Subsequently, as it is the most extensive data series, the CLIO Infra (2015) data on average years of education is selected as baseline series. Covering the period 1870-2010, it provides 7964 (69.5%) country-year estimates for the average years of education. In a next step, we maximally extend these estimates forward to 2016 and backwards to 1950 using the growth rates implied in the average years of education data reported by United Nations Development Program (2015), adding another 1454 (10.85%) estimates. Subsequently, we rely on the least squares third-order polynomial approximation strategy outlined earlier to further extend this baseline series where possible. Afterward, we linearly interpolate interrupted time series to add 103 (0.77%) more country-years. 2091 (15.6%) country-years remain missing.

As detailed in Table A1, the correlation with the baseline values is fairly strong for both the overlapping raw alternative estimates as well as the third-order polynomial predictions, with correlation coefficients ranging from 0.90 to 0.97. In addition, the predictive accuracy of our various third-order polynomial models generally is fairly high, where the number of baseline estimates falling within the 99% confidence intervals of their predicted counterparts range from 56.7% to 70%.

Life expectancy$^{\diamondsuit}$*:* Data on life expectancy is obtained from Barro and Lee (1994); CLIO Infra (2015); World Bank (2016), where linear interpolation is first employed to add a small number of missing observations. Since the correlation between the overlapping observations in these datasets is near perfect, as detailed in Table A1, our consolidated variable of interest is constructed by averaging across all available data sources, leaving 1260 (9.4%) country-year observations missing.

Trade openness$^{\diamondsuit}$*:* Data on trade openness, defined as the value of imports and exports relative to GDP, are obtained from Heston et al. (1994); Correlates of War Project (2015); Feenstra et al. (2015); World Bank (2016). After linearly interpolating missing observations in each dataset, we select the Feenstra et al. (2015) data as our baseline. This dataset covers the period 1950-2011 and provides us with 9041 (67.44%) country-year observations. Subsequently, we maximally extend the existing data forward and backwards using the growth rates implied in the World Bank (2016) data for an additional 1145 (4%) country-year observations. Finally, relying on the least squares third-order polynomial approximation procedure outlined above, we fill another 322 (2.4%) country-year observations based on the Heston et al. (1994) data and another 489 (3.65%) country-year observations based on the Correlates of War Project (2015) data.[36] 2425 (18.09%) country-year observations remain missing.

Democracy$^{\diamondsuit}$*:* In order to construct a composite index of democracy, we incorporate information on 8 measures of democracy: Melton, Meserve, and Pemstein (2010); Giuliano, Mishra, and Spilimbergo (2013); Center for Systemic Peace (2015); Gibler and Miller (2014b); Vanhanen (2014); CLIO Infra (2015); Freedom House (2015).[37] After linearly interpolating missing observations in each data set, as it is the most extensive data source, we consider Freedom House (2015) as our baseline series. Freedom House's (2015) continuous measure of democracy, which is based on a country's degree of political competition and political participation, provides us with 6553 (71.27%) democracy estimates. Subsequently, sequentially relying on the alternative democracy measures, we apply the third-order polynomial approximation approach described earlier to maximally extend this baseline series forward and backwards. After this procedure, 2513 (18.75%) country-year observations remain missing.

The fairly high correlation between both raw alternative as well as third-order polynomial predicted democracy values and baseline values reported in Table A1, where correlation coefficients range from 0.8 to 0.97, serves to motivate this approach. In addition, the predictive accuracy which is in excess of 65% in all third-order polynomial models except one provides further evidence that these alternative democracy indexes provide useful information to assess missing values in the baseline series.

Fixed capital stock (% GDP): Data on national fixed capital stocks are derived from Feenstra et al. (2015). This dataset covers the period 1950-2014 and provides us with 7494 (55.9%) country-year observations for national fixed capital stocks expressed in constant 2005 US dollars. Subsequently, we maximally extend this baseline series forward

[36] Furthermore, we remove 17 negative data points resulting from the polynomial approximation procedure.

[37] For a comparison of various democracy indices, see among others Munck and Verkuilen (2002) and Melton et al. (2010)

and backward by applying the perpetual inventory method, relying on the depreciation rates for national fixed capital stocks also reported by Feenstra et al. (2015) and the available information on gross fixed capital formation (see below), adding another 122 (0.1%) country-year observations. After this procedure, 5789 (43.19%) country-year observations remain missing.

Gross Fixed Capital Formation (% GDP): Data on gross fixed capital formation come from World Bank (2016) and Feenstra et al. (2015). First, we rely on the perpetual inventory method to derive gross fixed capital formation from the available information on the values (in constant 2005 US dollars) of the fixed capital stock and the yearly depreciation rate of fixed capital stocks reported by Feenstra et al. (2015). This procedure provides us with 7352 (54.85%) country-year observations. Subsequently, we maximally extend this baseline series forward and backwards by using the growth rates of gross fixed capital formation as reported in constant 2010 dollars by the World Bank (2016), adding another 1057 (7.88%) observations. World Bank (2016) data on gross fixed capital formation are available between 1960 and 2016 and, reassuringly, correlate fairly strongly with the gross fixed capital formation estimates we derived from the information reported by Feenstra et al. (2015), at 0.89 for their 4871 common observations. After this procedure, 4996 (37.27%) country-year observations remain missing.

Table A1: **Constructed variables: data sources and components**

Variable	*Data source*	*Description*	*% Obs. [% Int.]*	*r / r̂*	*Accuracy*
GDP per capita*** (baseline)	**The Madison Project (2017)**	GDP per capita (1990 int. GK $)	63.24 [0]	1 / .	.
	World Bank (2016)	GDP per capita (constant 2005 $)	19.78 [0.5]	0.83 / .	.
	World Resources Institute (2015)	Total CO2 emissions (Metric Tons)	6.48 [0]	0.84 / 0.89	55.19
	World Bank (2016)	Per capita CO2 emissions (Metric Tons)	2.69 [0]	0.83 / 0.89	54.03
	Correlates of War Project (2012)	Primary Energy Consumption (Metric Ton Coal Equivalent)	2.69 [0]	0.66 / 0.85	57.57
	n.a.	missing	5.12 [0]	. / .	.
GDP per capita** (alternative)	**The Madison Project (2017)**	GDP per capita (1990 int. GK $)	63.24 [0]	1 / .	.
	The Conference Board (2015)	GDP per capita (1990 int. GK $)	11.23 [0.73]	1 / .	.
	Barro and Lee (1994)	GDP per capita (1985 int. prices)	1.28 [0.95]	0.98 / .	.
	Heston et al. (1994)	Real GDP per capita	0.4 [0]	0.97 / .	.
	World Bank (2016)	GDP per capita (constant 2005 $)	12.51 [0]	0.83 / .	.
	Feenstra et al. (2015)	GDP per capita (chained PPPs, 2005$)	0.3 [0]	0.90 / .	.
	n.a.	missing	11.03 [.]	. /.	.
Population**	**CLIO Infra (2015)**	Total population	75.08 [55.39]	1 / .	.
	Heston et al. (1994)	Total population	4.93 [0]	1 / .	.
	Feenstra et al. (2015)	Total population	5.69 [0]	1 / .	.
	Barro and Lee (1994)	Total population	0.07 [0.01]	1 / .	.
	World Bank (2016)	Total population	10.26 [0]	1 / .	.
	The Madison Project (2017)	Total population	0.56 [0]	1 / .	.
	Correlates of War Project (2012)	Total population	0.16 [0]	1 / .	.
	n.a.	missing	1.76 [.]	. / .	.
Education***	**CLIO Infra (2015)**	Average years of education	59.41 [52.69]	1 / .	.
	United Nations Development Program (2015)	Average years of education	10.85 [0.4]	0.94 / .	.
	Barro and Lee (2012)	Average years of education	6.35 [0.51]	0.95 / 0.97	65.41
	Barro and Lee (1994)	Average years of education	1.06 [0.87]	0.93 / 0.95	70.09
	World Bank (2016)	Secondary enrollment rate	5.71 [2.43]	0.90 / 0.94	56.72
	Barro and Lee (1994)	Secondary enrollment rate	0.25 [0.18]	0.90 / 0.94	61.40
	Linearly interpolated	.	0.77 [0.77]	. / .	.
	n.a.	missing	15.6 [.]	. / .	.
Health*	**CLIO Infra (2015)**	Life expectancy	77.99 [0]	1 / .	.
	World Bank (2016)	Life expectancy	76.71 [0.6]	0.99 / .	.
	Barro and Lee (1994)	Life expectancy	22.16 [17.05]	0.97 / .	.
	n.a.	missing	7.35 [.]	. / .	.
Trade Openness***	**Feenstra et al. (2015)**	(imports + exports)/GDP	67.44 [0.00]	1 / .	.
	World Bank (2016)	(imports + exports)/GDP	8.54 [0.00]	0.80 / .	.
	Heston et al. (1994)	(imports + exports)/GDP	2.4 [0.00]	0.70 / 0.84	70.42
	Correlates of War Project (2015)	(imports + exports)/GDP	3.65 [0.00]	0.40 / 0.77	59.25
	n.a.	missing	18.09 [.]	. / .	.
Democracy***	**CLIO Infra (2015)**	Vanhanen Index of Democracy	49.10 [1.34]	1 / .	.
	Vanhanen (2014)	Vanhanen Index of Democracy	22.17 [0.14]	0.97 / .	.
	Gibler and Miller (2014b)	Combined Polity2 Index	1.89 [0]	0.90 / 0.94	68.03
	Melton et al. (2010)	Unified Democracy Scores	0.76 [0.15]	0.89 / 0.93	65.81
	Giuliano et al. (2013)	Freedom House Index	3.74 [0.46]	0.81 / 0.89	27.63
	Freedom House (2015)	Freedom House Index	0.14 [0.1]	0.80 / 0.92	66.43
	Center for Systemic Peace (2015)	Revised Combined Polity Score	0.47 [0]	0.82 / 0.91	66.36
	Linearly interpolated	.	3.45 [3.45]	. / .	.
	n.a.	missing	18.75 [.]	. / .	.

Note: Baseline sources in bold. * indicates that the consolidated variable is obtained by averaging across all available data sources, ** indicates that the consolidated variable is obtained by applying the regular data construction procedure outlined in appendix A, *** indicates that the consolidated variable is obtained by applying the third-order polynomial approximation procedure outlined in appendix A. The percentage of linearly interpolated country-years contributions by each data source in square brackets. r reports the correlation between baseline and alternative values, $\hat{r}$ reports the correlation between baseline and third-order polynomial predicted values. Where relevant, the last column reports the percentage of baseline observations falling withing the 99% confidence intervals of their third-order polynomial predicted counterparts.

B Estimation strategy

Section 3 proposes a semi-parametric estimation procedure to quantify the net per capita GDP gain of independence that is rooted in the synthetic control framework pioneered by Abadie and Gardeazabal (2003). This section provides a more formal description of this estimation procedure and sheds more light on its underlying identifying assumptions.

To do so, suppose that in a sample containing $J+1$ countries, indexed by $i = \{1, \ldots, J+1\}$, observed over T time periods, indexed by $t = \{1950, \ldots, T_0, \ldots, T\}$, country j decides to declare independence at time $t = T_0$ and that we are interested in determining the causal effect of this decision, if any, on its per capita GDP trajectory. To do so, denote by y_{jt}^N the level of log per capita GDP that would be observed in country j if it did not (yet) declare independence, and let y_{jt}^T denote the outcome that would be observed if country j declared itself independent prior to time $t+1$. Abstracting from anticipation effects, the causal economic effect of declaring independence at time $t \geq T_0$ is defined as $\beta_{jt} = y_{jt}^T - y_{jt}^N$.[38] The observed outcome for each country i can be written as

$$y_{i,t} = y_{i,t}^N + \beta_{i,t} NIC_{i,t} \tag{1A}$$

where $NIC_{i,t}$ is an independence dummy equal to 1 for each NIC in each year after it gained independence and 0 otherwise while $\beta_{i,t}$ captures the economic impact of secession of country i at time t.

It follows that estimating the causal impact of country j's declaration of independence at time t, $\hat{\beta}_{jt}$, boils down to estimating the counterfactual, post-independence per capita GDP trajectory that would be observed in that country if it had never declared independence, $\hat{y}_{j,t}^N$:

$$\hat{\beta}_{j,t} = y_{j,t} - \hat{y}_{j,t}^N \ , \quad t \geq T_0 \tag{2A}$$

Although $y_{j,t}^N$ remains unobserved for $t \geq T_0$, suppose we do know $y_{i,t}^N$ to linearly depend on a number of observed growth determinants in each country i. More specifically, suppose we summarize the country-specific information on x observed growth determinants in a $(n \times 1)$ vector of unaffected observed covariates denoted by $\mathbf{X}_i = [x_{i,1}, \ldots, x_{i,n}]$, where $n \leq Tx$. Note that $\mathbf{X}_i$ may contain past or future values of the observed characteristics as long as these are unaffected by country j's decision to secede. In addition, assume that we do not observe all the relevant characteristics determining $y_{j,t}^N$ and denote by $\mathbf{Z}_i$ the $(m \times 1)$ vector collecting all of these, potentially time-varying, unobserved growth determinants, where $m \leq (T_0 - 1950)$. Note that $\mathbf{Z}_i$ may also subsume a country fixed effect. Finally, assume $y_{i,t}^N$ is subject to year fixed effects, η_t, and a mean-zero transitory

[38] If anticipation effects are at play, T_0 should be redefined to coincide with the first period these play a role. We will come back to this.

shock, $\epsilon_{i,t}$. Summarizing, we assume $y^N_{j,t}$ to be given by

$$y^N_{j,t} = \theta_t \mathbf{X}_j + \lambda_t \mathbf{Z}_j + \eta_t + \epsilon_{j,t} \tag{3A}$$

where θ_t and λ_t denote the $(1 \times n)$ and $(1 \times m)$ vectors of unknown, potentially time-varying, population parameters associated with $\mathbf{X}_j$ and $\mathbf{Z}_j$ respectively.

To simulate the counterfactual post-independence $y^N_{j,t}$-trajectory that would be observed in NIC j in absence of state fragmentation, consider a linear combination of the remaining J control countries defined by the weighting vector $\mathbf{W}^* = [w^*_1, \ldots, w^*_{j-1}, w^*_{j+1}, \ldots, w^*_{J+1}]$, in such a way that the following four conditions hold: (i) the resulting weighted vector of unaffected observed characteristics, $\sum_{i \neq j}^{J+1} w_i \mathbf{X}_i$, exactly mirrors that of country j, $\mathbf{X}_j$, (ii) the pre-independence outcome path is identical in the seceding country an its synthetic counterpart, (iii) control countries receiving positive weight were independent themselves at the time of country j's declaration of independence but (iv) none of them declared independence themselves in the 10 years preceding country j's declaration of independence. Note that this last condition is imposed to ensure that the control group itself is not contaminated by economic effects of secession and/or its anticipation stemming from one of its component parts. Formally, assume there exists a $\mathbf{W}^*$ such that:

Condition 1A

$$\sum_{i \neq j}^{J+1} w^*_i \mathbf{X}_i = \mathbf{X}_j \quad ,$$
$$\mathbb{E}\left[\mathbf{X}_i | NIC_{j,t}\right] = \mathbb{E}\left[\mathbf{X}_i\right] \quad \forall i \in \{1, \ldots, J+1\} \;\; \& \;\; \forall t \in T$$

Condition 2A

$$\sum_{i \neq j}^{J+1} w^*_i y^N_{i,1950} = y^N_{j,1950} \quad , \quad \ldots \quad , \quad \sum_{i \neq j}^{J+1} w^*_i y^N_{i,T_0-1} = y^N_{j,T_0-1}$$

Condition 3A

$$\exists t \in \{T_0 - 10, \;\ldots\;, T\} : NIC_{i,t} - NIC_{i,t-1} = 1 \quad \Leftrightarrow \quad w^*_i = 0$$

Observe that, by use of equation (3A), the value of the outcome variable of this synthetic control country can be written as

$$\sum_{i \neq j}^{J+1} w^*_i y^N_{i,t} = \theta_t \sum_{i \neq j}^{J+1} w^*_i \mathbf{X}_i + \lambda_t \sum_{i \neq j}^{J+1} w^*_i \mathbf{Z}_i + \eta_t + \sum_{i \neq j}^{J+1} w^*_i \epsilon_{i,t} \tag{4A}$$

such that the discrepancy between the outcome path that would be observed in (future) NIC j in absence of state fragmentation (equation (3A)) and that of its synthetic counterpart (equation (4A)) satisfying conditions (1A) through (3A) is given by:

$$y_{j,t}^N - \sum_{i \neq j}^{J+1} w_i^* y_{i,t}^N = \lambda_t \big(\mathbf{Z}_j - \sum_{i \neq j}^{J+1} w_i^* \mathbf{Z}_i \big) + \big(\epsilon_{j,t} - \sum_{i \neq j}^{J+1} w_i^* \epsilon_{i,t} \big) \tag{5A}$$

Note that this also holds in the pre-independence period and denote by $\mathbf{Y}_i^P$, λ^P and ϵ_i^P the $\Big((T_0 - 1950) \times 1 \Big)$ vector, the $\Big((T_0 - 1950) \times m \Big)$ matrix and the $\Big((T_0 - 1950) \times 1 \Big)$ vector with the tth row equal to $y_{i,t}^N$, λ_t and $\epsilon_{i,t}$ respectively. This implies that the pre-independence discrepancy between NIC j's (fully observed) $y_{j,t}^N$-trajectory and that of its synthetic version can be written as:

$$Y_j^P - \sum_{i \neq j}^{J+1} w_i^* Y_i^P = \lambda^P \Big(\mathbf{Z}_j - \sum_{i \neq j}^{J+1} w_i^* \mathbf{Z}_i \Big) + \Big(\epsilon_j^P - \sum_{i \neq j}^{J+1} w_i^* \epsilon_i^P \Big) \tag{6A}$$

or, equivalently:

$$\lambda^P \Big(\mathbf{Z}_j - \sum_{i \neq j}^{J+1} w_i^* \mathbf{Z}_i \Big) = \sum_{i \neq j}^{J+1} w_i^* \epsilon_i^P - \epsilon_j^P \tag{7A}$$

Pre-multiplying both sides of equation (7A) by the inverse of λ^P , $(\lambda^{P'} \lambda^P)^{-1} \lambda^{P'}$, yields[39]

$$\mathbf{Z}_j - \sum_{i \neq j}^{J+1} w_i^* \mathbf{Z}_i = (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \Big(\sum_{i \neq j}^{J+1} w_i^* \epsilon_i^P - \epsilon_j^P \Big) \tag{8A}$$

Finally, inserting this expression for $\mathbf{Z}_j - \sum_{i \neq j}^{J+1} w_i^* \mathbf{Z}_i$ in equation (5A) yields an expression for the discrepancy between the (partly unobserved) full outcome path that would be observed in the seceding country, j, in absence of state fragmentation and the same (fully observed) outcome path for its synthetic version, $\mathbf{W}^*$:

$$y_{j,t}^N - \sum_{i \neq j}^{J+1} w_i^* y_{i,t}^N = \lambda_t (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \sum_{i \neq j}^{J+1} w_i^* \epsilon_i^P - \lambda_t (\lambda^{P'} \lambda^P)^{-1} \lambda^{P'} \epsilon_j^P - \sum_{i \neq j}^{J+1} w_i^* \left(\epsilon_{j,t} - \epsilon_{i,t} \right) \tag{9A}$$

Abadie et al. (2010) prove that under standard conditions, if the number of preintervention periods (T_0-1950) is large relative to the scale of the transitory shocks ($\epsilon_{i,t}$), the right-hand side of equation (9A) will tend towards zero. This suggests using

$$\hat{\beta}_{j,T_0+s} = y_{j,T_0+s} - \sum_{i \neq j}^{J+1} w_i^* y_{i,T_0+s} \tag{10A}$$

as an estimator for the independence dividend of country j, s years after independence.

In practice, since there often does not exist a set of weights that exactly satisfies conditions (1A) through (3A), standard practice is to construct the synthetic control such

[39] Note that assuming $m \leq T_0 - 1950$ ensures that λ^P is nonsingular and thus has a well-defined inverse.

that these conditions hold approximately. In the empirical exercise of subsection (3.3), we do so by relying on the nested optimalization algorithm developed by Abadie et al. (2014, Appendix B), which defines the optimal weight vector $\mathbf{W}^*$ such that each synthetic control country minimizes the Root Mean Squared Prediction Error (RMSPE) of pre-independence outcomes (see equation (6A)).[40] We restrict the pretreatment period to maximally 10 years prior to the declaration of independence for each NIC in the sample, discarding those NICs lacking sufficient pretreatment information.[41] Our choice of pretreatment characteristics stems from the growth literature and includes population size, population density, educational attainment, life expectancy, trade openness and per capita battle deaths.

[40] The synthetic control algorithm is implemented by Abadie et al.'s (2010) *synth*-command in Stata 13.1.
[41] Table A2 lists the NICs included in the synthetic control algorithm.

C A control function approach to account for endogeneity

One relevant concern with the second-step results presented in subsection 4.2 is that they ignore the potential endogeneity of the first-stage independence dividend estimates and their potential determinants. This appendix details the control function we employ to circumvent the endogeneity issue by parametrically controlling for it. Subsection C.1 formalizes this endogeneity issue and the control function approach we propose to deal with it. Subsection C.2 reports the results of a monotonicity check that aims to determine whether a critical identifying assumption of this estimation approach is met in our data.

C.1 The control function approach

To see how endogeneity might emerge in our baseline model, assume that the residual in equation (6) can be divided into an exogenous and an endogenous component, such that relative importance of each potential channel can be estimated by

$$\hat{\beta}_{i,r,t,s} = \alpha_1 + \alpha_2 \bar{\beta}_i + \alpha_3 \bar{\beta}^{placebo}_{i,r,t,s} + \lambda \mathbf{X}_{i,r,t,s} + \eta_s + \delta_r + \mu_t + \phi_m + \underbrace{\omega_{i,r,t,s} + \psi_{i,r,t,s}}_{\epsilon_{i,r,t,s}} \quad (11A)$$

$\omega_{i,r,t,s}$ and $\psi_{i,r,t,s}$ thus measure deviations of independence dividends from their expected values. Paraphrasing Ornaghi and Van Beveren (2011, p. 6), the difference between both unobservables is that $\omega_{i,r,t,s}$ refers to unobserved factors that *are* observed by inhabitants of NICs and are likely to affect their economic decisions (eg. political (in)stability) while $\epsilon_{i,r,t,s}$ collects all random, transitory shocks to the independence dividend unobserved by the NIC (as well as the econometrician) and thus affecting economic performance but *not* economic decisions (eg. unexpected natural disasters). We refer to $\omega_{i,r,t,s}$ as the 'efficiency gain of independence' noting that it can be either positive (eg. independence reduces political instability and, thus, improves growth prospects) or negative (eg. independence increases political instability and, thus, worsens growth prospects).[42]

The most important takeaway from equation (11A) is that the identification of the unknown parameters requires the potential determinants of the independence dividends to be exogenous or, analogously, to be unaffected by the (perceived) efficiency gain of independence, $\omega_{i,r,t,s}$. Indeed, in this case $\mathbf{E}\left(x_{i,r,t,s}\omega_{i,r,t,s}\right) = 0 \ \forall x \in \mathbf{X}$ by assumption and the regression coefficients $\hat{\lambda}$ are unbiased estimates for the true parameters.

Nevertheless, it may seem unlikely that the potential determinants of the independence dividend are exogenous and that economic agents in NICs disregard any information on the efficiency gain of independence when taking economically relevant decisions. If economic agents have a good knowledge of the efficiency gain of independence, $\omega_{i,r,t,s}$, and especially

[42]Thus, the efficiency gain of independence is defined as the residual from the relation between the true independence dividend, its the underlying growth determinants and all random, transitory shocks.

if these efficiency gains are persistent, endogeneity arises because economic decisions will partially reflect beliefs about $\omega_{i,r,t,s}$.[43] More specifically, assume that the efficiency gain of independence follows a first order Markov process such that

$$\omega_{i,r,t,s} = \mathbf{E}\left(\omega_{i,r,t,s} \mid \omega_{i,t-1,s-1}\right) + \xi_{i,r,t,s} = g\left(\omega_{i,t-1,s-1}\right) + \xi_{i,r,t,s} \tag{12A}$$

with $g(\cdot)$ an unknown function and $\xi_{i,r,t,s}$ a surprise news component unforeseen at time $t-1$. This allows us to rewrite equation (11A) as

$$\hat{\beta}_{i,r,t,s} = \alpha_1 + \alpha_2\bar{\beta}_i + \alpha_3\bar{\beta}^{placebo}_{i,r,t,s} + \lambda\mathbf{X}_{i,r,t,s} + \eta_s + \delta_r + \mu_t + \phi_m + g\left(\omega_{i,t-1,s-1}\right) + \xi_{i,r,t,s} + \psi_{i,r,t,s} \tag{13A}$$

Formally, endogeneity arises whenever economic agents in a specific NIC know the (expected) efficiency gain of independence at the time economically relevant decisions are made such that the efficiency gain simultaneously affects economic performance, $\hat{\beta}_{i,r,t,s}$, and the decisions contained in $\mathbf{X}_{i,r,t,s}$. For instance, NICs may reap the benefits of increasing efficiency gains of independence by opening up to trade, introducing an upward bias in the value of the coefficient estimate for the relative importance of trade openness.[44]

A similar simultaneity issue has long been the central focus of the vast methodological literature surrounding total factor productivity estimation, which at least dates back to the seminal work by Marschak and Andrews (1944).[45] Olley and Pakes (1996) were the first to solve this issue by explicitly controlling for the unobserved confounder using proxy variables. Modifying their approach to fit our purposes, our identification strategy relies on the assumption that the fixed capital stock of a NIC, $K_{i,r,t,s}$, is fully determined by choices made in period $t-1$ through the following law of motion:[46]

$$K_{i,r,t,s} = \left(1-\delta_t\right) K_{i,t-1,s-1} + I_{i,t-1,s-1} \tag{14A}$$

where δ_t captures the yearly depreciation rate of the fixed capital stock and $I_{i,r,t,s}$ measures gross fixed capital formation. Note that this law of motion assumes that it takes a full year for fixed capital investments to translate into fixed capital. Crucially, this implies that both fixed capital ($K_{i,r,t,s}$) as well as fixed capital investments ($I_{i,r,t,s}$) depend on the expected efficiency gain of independence in year t, $g(\omega_{i,t-1,s-1})$, as higher expected efficiency gains should make it more profitable to increase the fixed capital stock. In addition, assuming

[43] If efficiency gains of independence are not persistent, $g\left(\omega_{i,t-1,s-1}\right)$ would drop in equation (13A) such that endogeneity would only arise if the potential determinants of the independence dividend would depend on the 'surprise' news component.

[44] On the one hand, income growth may translate into import growth; on the other hand, wide evidence shows that productive firms self-select into export markets due to the large fixed costs associated with foreign market entry (Jovanovic, 1982; Melitz, 2003; Das, Roberts, & Tybout, 2007).

[45] For a recent overview of this literature, see for instance Ackerberg, Caves, and Frazer (2015).

[46] The empirical application relies on national fixed capital and gross fixed capital formation shares of GDP. Data on gross fixed capital, gross fixed capital formation and the yearly depreciation rate of fixed capital are derived from Feenstra et al. (2015) and World Bank (2016). For more information on data construction and sources, see appendix A.

that capital investments are chosen after the realization of the news component, investment will also depend on the efficiency shock and we have that $I_{i,r,t,s} = f\left(K_{i,r,t,s}, \omega_{i,r,t,s}\right)$.

Under the additional assumption that investment in fixed capital is strictly increasing in the unobserved efficiency gain of independence, this suggests proxying the efficiency gain of independence by inverting the investment demand function.[47] The unobserved efficiency gain of independence is then defined as

$$\omega_{i,r,t,s} = f^{-1}\left(K_{i,r,t,s}, I_{i,r,t,s}\right) \tag{15A}$$

Intuitively, the fixed capital investment decisions of NICs in addition to their existing fixed capital stock are thus taken to contain useful information on the (perceived) efficiency gain of their independence declaration at a certain point in time. In this sense, the control function can be considered to proxy for (unobserved) 'business sentiment' or 'confidence in the economic future' in the immediate post-independence period. This, in turn, suggests adding the control function, $f^{-1}\left(K_{i,r,t,s}, I_{i,r,t,s}\right)$, to regression equation (11A) to control for simultaneity bias. More specifically, equation (15A) allows us to express both the expected efficiency gain ($g\left(\omega_{i,t-1,s-1}\right)$) as well as the the innovation in the efficiency gain of independence ($\xi_{i,r,t,s}$) as a function of observables and hence to control for $\omega_{i,r,t,s}$ by simply adding this control function to equation (13A) as follows:

$$\hat{\beta}_{i,r,t,s} = \alpha_1 + \alpha_2\bar{\beta}_i + \alpha_3\bar{\beta}^{placebo}_{i,r,t,s} + \lambda\mathbf{X}_{i,r,t,s} + \eta_s + \delta_r + \mu_t + \phi_m + f^{-1}\left(K_{i,r,t,s}, I_{i,r,t,s}\right) + \psi_{i,r,t,s} \tag{16A}$$

Estimation of (16A)is further complicated by the fact that $f^{-1}\left(K_{i,r,t,s}, I_{i,r,t,s}\right)$ has an unknown functional form. To proceed, in line with Olley and Pakes (1996), we assume that it can be approximated by polynomial expansion of order O such that $f^{-1}\left(K_{i,r,t,s}, I_{i,r,t,s}\right) \approx \sum_{o=0}^{O}\sum_{m=0}^{O-o}\alpha_{o,m}K^{o}_{i,r,t,s}I^{m}_{i,r,t,s}$.[48] More specifically, this implies that the single-stage least squares model summarized in equation (16A) can be implemented by estimating

$$\hat{\beta}_{i,r,t,s} = \alpha_1 + \alpha_2\bar{\beta}_i + \alpha_3\bar{\beta}^{placebo}_{i,r,t,s} + \lambda\mathbf{X}_{i,r,t,s} + \eta_s + \delta_r + \mu_t + \phi_m + \sum_{o=0}^{O}\sum_{m=0}^{O-o}\alpha_{o,m}K^{o}_{i,r,t,s}I^{m}_{i,r,t,s} + \psi_{i,r,t,s} \tag{17A}$$

Equation (17A) summarizes the estimation procedure we use in robustness section 4.3 to eliminate endogeneity bias by explicitly controlling for it, under the assumption that efficiency shocks occur *before* fixed capital investments are made.

[47] If this monotonicity assumption would be violated, it would be impossible to map every potential value of $I_{i,r,t,s}$ to a unique value for the unobserved efficiency gain of independence, $\omega_{i,r,t,s}$, which would essentially invalidate this estimation procedure. Given its central role in the estimation procedure, Ornaghi and Van Beveren (2011) propose a monotonicity test to verify to what extent the monotonicity assumption holds in the actual data. The results for our data are discussed in appendix C.2. For similar reasons, inversion of f also requires that the efficiency gain of independence is the *only* unobservable entering the inversion function.

[48] Note that the inclusion of the polynomial expansion implies that we can no longer identify the capital coefficient, as it is collinear with the polynomial in $K_{i,r,t,s}$ and $I_{i,r,t,s}$. In contrast to the total factor productivity literature, we are not interested in the capital coefficient however, such that this property does not complicate our estimation procedure.

C.2 Monotonicity check

The estimator discussed in the previous subsection relies on the crucial assumption that gross fixed capital formation is strictly increasing in the (unobserved) efficiency gain of independence, as a necessary condition for the control function to accurately proxy for the latter. This subsection implements a specification test to verify whether this identification assumption is likely to be met in our data.

Ornaghi and Van Beveren (2011) propose a simple monotonicity test to check whether this identification assumption is likely to hold in particular datasets. Building on this procedure, note that the monotonicity assumption in our setting boils down to assuming that for any given value of the fixed capital stock, NICs make larger gross investments in the fixed capital stock the higher the (unobserved) efficiency gain of independence.

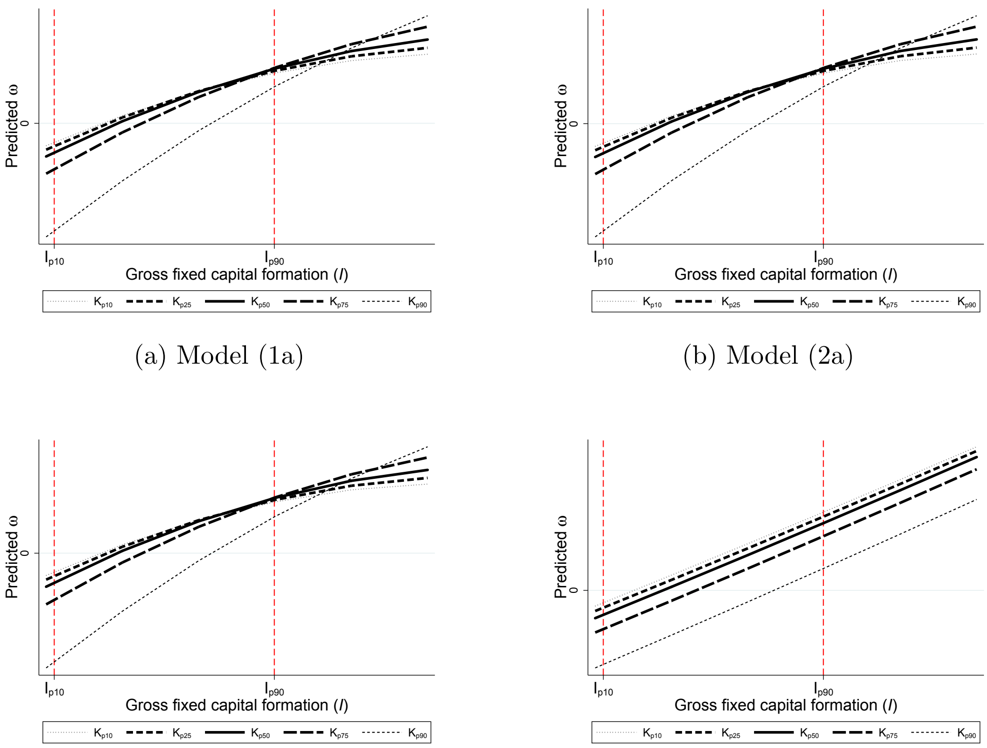
One crude way of assessing this is to approximate the unobserved efficiency gain as the residual of the regression formalized in equation (11A), abstracting from the random shock $\psi_{i,r,t,s}$ and hence implicitly assume that $\omega_{i,r,t,s} \approx \omega_{i,r,t,s} + \psi_{i,r,t,s}$. Subsequently, this residual can regressed on a polynomial in fixed capital and gross fixed capital investment from the appropriate order to compute the expected efficiency gain of independence for any value of both predictors. Formally, denoting the residual in equation (11A) by $\varpi_{i,r,t,s} = \omega_{i,r,t,s} + \psi_{i,r,t,s}$, the monotonicity test boils down to comparing various predictions for the estimated efficiency gain of independence, $\hat{\varpi}_{i,r,t,s}$, from the following model

$$\hat{\varpi}_{i,r,t,s} = \alpha_0 + \sum_{o=0}^{O} \sum_{m=0}^{O-o} \alpha_{o,m} K^{o}_{i,r,t,s} I^{m}_{i,r,t,s} + \zeta_{i,r,t,s} \tag{18A}$$

More specifically, we rely on the estimation results for this model to compute the predicted the efficiency gain of independence for all gross fixed capital investment values contained within the support of $I_{i,r,t,s}$ while sequentially fixing the value of $K_{i,r,t,s}$ at its 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} percentile. The idea is thus to fix the value of fixed capital at one of these five percentile values and to subsequently check whether the predicted efficiency gain of independence effectively monotonically increases over the support of gross fixed capital formation in our sample. If this would be the case, such that $I_{i,r,t,s} > I_{j,t,s} \Rightarrow \hat{\varpi}_{i,r,t,s} > \hat{\varpi}_{j,t,s}$, this would constitute empirical evidence that the monotonicity assumption is not violated in the data. Needless to say, this validity check can detect cases where the monotonicity assumption is violated in the data, yet can only provide necessary but not sufficient evidence that the monotonicity assumption actually holds.

Figure A1 reports the result of the monotonicity tests pertaining to the various the estimation models discussed in subsection 4.2. Reassuringly, the figures suggest that the monotonicity assumption seems not to be violated in any part of the support of $I_{i,r,t,s}$ for any the selected percentile values of $K_{i,r,t,s}$. Thus, at first glance, all observations in our sample appear to satisfy the monotonicity assumption - well above the 80%-threshold for valid inference proposed by Ornaghi and Van Beveren (2011). We conclude that

Figure A1: **Monotonicity test**



(a) Model (1a)

(b) Model (2a)

(c) Model (3a)

(d) Model (4a)

Note: This figure plots the efficiency gain of independence as predicted by the estimation model of equation (18A), $\hat{\varpi}_{i,r,t,s}$, when fixing $K_{i,ts}$ at its 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} percentile values, respectively indicated by K_{p10}, K_{p25}, K_{p50}, K_{p75}, K_{p90} in the figures, and gradually increasing the value of $I_{i,r,t,s}$ over its support, where I_{p10} and I_{p90} respectively show the 10^{th} and 90^{th} percentile values for $I_{i,r,t,s}$ in our sample. The raw efficiency gain of independence, or $\hat{\varpi}_{i,r,t,s}$ in equation (18A), is estimated as the residual of the model summarized in equation (11A). The results are reported for the different estimation models reported in table 4, as identified in the subtitles.

the monotonicity tests fail to find evidence of the critical identification assumption of monotonicity being violated in our data, thus allowing for valid estimation.

Table A2: **Newly Independent Countries: 1950-2016**

Country	*Year*	*Country*	*Year*	*Country*	*Year*
Libya	1951	Uganda◇	1962	Tuvalu	1978
Cambodia*	1953	Kenya◇	1963	Kiribati◇	1979
Laos	1953	Zanzibar	1963	St. Lucia◇	1979
German Democratic Republic	1954	Malawi◇	1964	St. Vincent and the Grenadines◇	1979
Republic of Vietnam	1954	Malta*◇	1964	Vanuatu◇	1980
Vietnam	1954	Zambia◇	1964	Antigua & Barbuda◇	1981
German Federal Republic	1955	Gambia◇	1965	Belize◇	1981
Morocco◇	1956	Maldives	1965	St. Kitts and Nevis	1983
Sudan	1956	Singapore*◇	1965	Brunei◇	1984
Tunisia	1956	Zimbabwe◇	1965	Federated States of Micronesia	1986
Ghana◇	1957	Barbados◇	1966	Marshall Islands	1986
Malaysia◇	1957	Basutoland (Lesotho)◇	1966	Namibia◇	1990
Guinea*	1958	Botswana◇	1966	Armenia*◇	1991
Benin◇	1960	Guyana◇	1966	Azerbaijan◇	1991
Burkina Faso	1960	Yemen People's Republic	1967	Belarus◇	1991
Cameroon	1960	Equatorial Guinea	1968	Estonia*◇	1991
Central African Republic	1960	Mauritius◇	1968	Georgia*◇	1991
Chad	1960	Nauru	1968	Kazakhstan◇	1991
Congo	1960	Swaziland◇	1968	Kyrgyzstan◇	1991
Cyprus◇	1960	Fiji◇	1970	Latvia*◇	1991
Democratic Republic of the Congo◇	1960	Tonga	1970	Lithuania*◇	1991
Gabon	1960	Bahrain◇	1971	Moldova◇	1991
Ivory Coast	1960	Bhutan◇	1971	Russia◇	1991
Madagascar	1960	Oman	1971	Tajikistan◇	1991
Mali	1960	Qatar◇	1971	Turkmenistan*◇	1991
Mauritania	1960	United Arab Emirates◇	1971	Ukraine*◇	1991
Niger	1960	Bangladesh◇	1972	Uzbekistan*◇	1991
Nigeria◇	1960	Bahamas◇	1973	Bosnia and Herzegovina*	1992
Senegal	1960	Grenada◇	1974	Croatia*◇	1992
Somalia	1960	Guinea-Bissau◇	1974	Slovenia◇	1992
Togo	1960	Angola◇	1975	Czech Republic◇	1993
Kuwait	1961	Cape Verde◇	1975	Eritrea	1993
Sierra Leone◇	1961	Comoros◇	1975	Macedonia*	1993
Syria	1961	Mozambique◇	1975	Slovakia◇	1993
Tanzania	1961	Papua New Guinea◇	1975	Palau*◇	1994
Algeria*◇	1962	Sao Tome and Principe◇	1975	East Timor*◇	2002
Burundi◇	1962	Suriname◇	1975	Montenegro*◇	2006
Jamaica*◇	1962	Seychelles◇	1976	Serbia	2006
Ruanda◇	1962	Djibouti◇	1977	Kosovo	2008
Samoa*	1962	Dominica	1978	South Sudan*	2011
Trinidad and Tobago◇	1962	Solomon Islands◇	1978		

Note: * indicates countries that gained independence following a successful independence referendum. Data on historical independence referendums and their outcomes are taken from Qvortrup (2014).
◇ indicates countries included in the synthetic control algorithm (see section 3).

Table A3: **Semi-parametric estimates of the economic impact of secession**

	t = 0 + 1				**t = 0 + 5**				**t = 0 + 20**			
Country	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$
Algeria	-0.40	-0.39***	-0.39***	-0.08	-0.77	-0.77***	-0.72***	-0.15	-0.37	-0.37***	-0.24***	0.20*
Angola	-0.40	-0.41***	-0.39***	-0.53***	-0.42	-0.43***	-0.36***	-0.44***	-0.46	-0.47***	-0.30***	-0.46***
Antigua & Barbuda	0.31	0.33***	0.34***	0.22***	0.82	0.84***	0.87***	0.61***	0.95	0.97***	1.13***	0.72***
Armenia	-0.89	-0.80***	-0.75***	-0.55***	-0.90	-0.81***	-0.73***	-0.36***	-0.37	-0.28***	-0.09	0.09***
Azerbaijan	-0.45	-0.47***	-0.42***	-0.24***	-1.27	-1.28***	-1.20***	-0.84***	0.10	0.08**	0.28***	0.24***
Bahamas	-0.64	-0.63***	-0.62***	-0.10	-0.84	-0.83***	-0.79***	-0.10	-0.00	0.01	0.18***	0.39***
Bahrain	0.24	0.21***	0.23***	0.07*	0.43	0.40***	0.45***	0.07	0.67	0.64***	0.85***	0.01
Bangladesh	-0.24	-0.24***	-0.22***	-0.35***	-0.36	-0.36***	-0.29***	-0.44***	-0.39	-0.39***	-0.18***	-0.54***
Barbados	0.08	0.13***	0.16***	0.06*	-0.09	-0.03	0.01	0.03	-0.63	-0.58***	-0.48***	-0.02
Basutoland (Lesotho)	0.07	0.08***	0.11***	0.17***	-0.21	-0.20***	-0.16***	-0.16***	-0.22	-0.21***	-0.11**	-0.15**
Belarus	-0.25	-0.24***	-0.18***	-0.01	-0.60	-0.59***	-0.51***	-0.17**	0.20	0.21***	0.40***	0.13*
Belize	0.03	0.03	0.03	-0.17***	-0.01	-0.01	0.01	-0.21***	0.30	0.31***	0.43***	0.02
Benin	0.03	0.03	0.03	-0.21***	0.12	0.12***	0.14***	-0.50***	-0.10	-0.10***	0.01	-0.56***
Bhutan	-0.04	-0.03	-0.02	-0.17***	-0.12	-0.11	-0.06*	-0.11**	-0.06	-0.06	0.15**	-0.00
Botswana	0.16	0.16***	0.19***	0.03	0.47	0.47***	0.52***	0.19***	1.09	1.10***	1.20***	0.65***
Brunei	0.46	0.25*	0.26**	0.48***	-0.28	-0.49***	-0.43***	-0.01	-0.34	-0.54***	-0.36***	0.09
Burundi	-0.03	-0.03*	-0.02	-0.06***	-0.01	-0.01	0.04	0.04	-0.57	-0.57***	-0.44***	-0.17***
Cape Verde	-0.25	-0.21***	-0.18***	-0.08*	0.04	0.09**	0.16***	0.28***	0.19	0.23***	0.40***	0.43***
Comoros	-0.33	-0.32***	-0.30***	-0.68***	-0.46	-0.46***	-0.39***	-0.90***	-0.15	-0.14***	0.03	-0.80***
Croatia	-0.43	-0.72***	-0.65***	-0.58***	-0.32	-0.61***	-0.51***	-0.48***	-0.55	-0.83***	-0.60***	-0.51***
Cyprus	0.34	0.35***	0.34***	0.29***	0.25	0.26***	0.27***	0.21***	1.42	1.43***	1.53***	0.77***
Czech Republic	-0.39	-0.35***	-0.28***	-0.15***	-0.42	-0.39***	-0.31***	-0.17***	-0.53	-0.50***	-0.28***	-0.17***
Democratic Republic of the Congo	-0.22	-0.22***	-0.23***	-0.11***	-0.18	-0.17***	-0.16***	-0.06**	-0.72	-0.72***	-0.62***	-0.34***
Djibouti	-0.27	-0.27***	-0.24***	-0.12***	-0.29	-0.30***	-0.26***	-0.11**	-1.14	-1.14***	-1.01***	-0.31***
East Timor	-0.64	-0.18	-0.13**	0.25**	-1.01	-0.54	-0.44***	-0.01				

continued on next page

continued

	t = 0 + 1				**t = 0 + 5**				**t = 0 + 20**			
Country	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$
Estonia	-0.41	-0.43***	-0.37***	-0.23***	-0.42	-0.44***	-0.35***	-0.20***	-0.01	-0.02	0.18***	0.13**
Fiji	0.22	0.30***	0.30***	0.04	0.85	0.92***	0.95***	0.48***	0.63	0.71***	0.88***	0.96***
Gambia	0.11	0.12***	0.15***	0.09**	-0.06	-0.05***	-0.00	-0.09**	-0.40	-0.40***	-0.31***	-0.24***
Georgia	-0.83	-0.83***	-0.78***	-0.83***	-1.17	-1.17***	-1.08***	-1.11***	-0.55	-0.55***	-0.34***	-0.64***
Ghana	-0.11	-0.10***	-0.11***	-0.03**	0.07	0.07***	0.07***	0.08***	-0.41	-0.41***	-0.34***	-0.39***
Grenada	-0.19	-0.15***	-0.15***	-0.30***	-0.35	-0.30***	-0.26***	-0.42***	-0.01	0.03	0.20***	-0.09
Guinea-Bissau	0.13	0.11***	0.12***	0.01	0.02	0.00	0.08**	-0.05	0.24	0.23***	0.43***	-0.09
Guyana	-0.19	-0.12**	-0.10	-0.06*	-0.22	-0.15**	-0.11*	-0.08*	-1.02	-0.94***	-0.89***	-0.52**
Jamaica	0.03	0.00	0.02	0.05	-0.00	-0.03	0.03	0.07*	-0.90	-0.93***	-0.83***	-0.59***
Kazakhstan	-0.43	-0.43***	-0.38***	-0.13***	-0.79	-0.79***	-0.70***	-0.24***	-0.06	-0.06*	0.13**	0.10***
Kenya	-0.12	-0.10***	-0.08*	-0.00	-0.04	-0.02	0.02	0.09***	-0.43	-0.41***	-0.32***	0.04
Kiribati	-0.54	-0.29*	-0.25**	-0.04	-0.74	-0.50***	-0.47***	-0.24***	-1.04	-0.80***	-0.63***	-0.30***
Kyrgyzstan	-0.34	-0.32***	-0.27***	-0.01	-0.91	-0.88***	-0.80***	-0.10	-0.80	-0.78***	-0.58***	-0.11
Latvia	-0.75	-0.77***	-0.71***	-0.42***	-0.89	-0.91***	-0.82***	-0.50***	-0.52	-0.54***	-0.34***	-0.18***
Lithuania	-0.49	-0.47***	-0.42***	-0.31***	-0.78	-0.76***	-0.68***	-0.49***	-0.29	-0.27***	-0.07	-0.20***
Malawi	-0.02	-0.02**	-0.01	0.01	-0.03	-0.03***	-0.01	0.06***	-0.26	-0.26***	-0.18***	0.01
Malaysia	-0.17	-0.17***	-0.18***	-0.11***	-0.06	-0.06**	-0.06**	-0.03	-0.19	-0.19***	-0.09	-0.03
Malta	0.05	0.05**	0.06	0.07**	-0.07	-0.08***	-0.05	-0.02	0.78	0.78***	0.87***	0.58***
Mauritius	-0.22	-0.23***	-0.21***	-0.25***	-0.20	-0.20***	-0.17***	-0.24***	0.12	0.11***	0.22***	0.20
Moldova	-0.36	-0.25***	-0.20***	-0.46***	-1.08	-0.98***	-0.89***	-1.00***	-0.77	-0.66***	-0.47***	-0.54***
Montenegro	-0.13	0.12***	0.18***	0.10**	-0.05	0.20***	0.27***	0.12*				
Morocco	-0.12	-0.12***	-0.12***	-0.13***	-0.35	-0.35***	-0.34***	-0.32***	-0.80	-0.80***	-0.69***	-0.63***
Mozambique	-0.30	-0.36***	-0.34***	-0.14***	-0.12	-0.18***	-0.11***	-0.09	-0.09	-0.15***	0.02	-0.06
Namibia	-0.20	-0.12***	-0.07**	-0.07	-0.21	-0.13***	-0.03	0.02	-0.16	-0.08***	0.14**	0.34***
Nigeria	-0.02	-0.02	-0.03	-0.12***	0.11	0.11***	0.12***	-0.05**	0.28	0.28***	0.38***	0.07
Palau	-0.05	-0.02	0.05	0.03	-0.18	-0.15***	-0.06	-0.35***	0.29	0.31***	0.52***	-0.08

continued on next page

continued

	t = 0 + 1				t = 0 + 5				t = 0 + 20			
Country	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$
Papua New Guinea	-0.06	-0.11	-0.09	0.12**	0.06	0.01	0.07	0.31***	-0.31	-0.36***	-0.19**	0.13
Qatar	0.26	0.03	0.05	0.07***	0.65	0.42***	0.46***	0.31***	-0.17	-0.40***	-0.20**	-0.56*
Ruanda	-0.05	-0.05***	-0.05***	-0.06**	-0.18	-0.18***	-0.14***	-0.13***	0.22	0.22***	0.35***	0.13**
Russia	-0.21	-0.23***	-0.17***	-0.18***	-0.60	-0.61***	-0.51***	-0.51***	-0.07	-0.08***	0.14**	0.01
Sao Tome and Principe	-0.01	0.02	0.05*	-0.20	-0.15	-0.11***	-0.03	-0.29	-1.30	-1.27***	-1.05***	-0.77***
Serbia	-0.07	0.06***	0.13***		-0.05	0.08***	0.15***					
Seychelles	0.09	0.11***	0.15***	-0.02	-0.01	0.01	0.07**	-0.13	-0.04	-0.02	0.14**	-0.04
Sierra Leone	-0.04	-0.04***	-0.05***	-0.03	-0.37	-0.38***	-0.34***	-0.15	-0.22	-0.22***	-0.09*	0.06
Singapore	0.01	0.04*	0.06	0.08**	0.36	0.39***	0.42***	0.25***	0.95	0.98***	1.07***	0.47***
Slovakia	-0.50	-0.40***	-0.33***	-0.06***	-0.42	-0.31***	-0.24***	0.01	-0.38	-0.27***	-0.06	0.03
Slovenia	-0.56	-0.53***	-0.47***	-0.20**	-0.53	-0.50***	-0.40***	-0.23***	-0.83	-0.79***	-0.56***	-0.27***
Solomon Islands	0.20	0.20***	0.24***	0.28***	0.05	0.05	0.08*	0.18***	0.17	0.17***	0.33***	0.22**
St. Lucia	-0.12	-0.09	-0.06	-0.15*	0.15	0.18**	0.20***	-0.04	0.43	0.46***	0.60***	0.19**
St. Vincent and the Grenadines	0.22	0.23***	0.26***	0.35***	0.67	0.68***	0.71***	0.58***	1.01	1.02***	1.16***	0.63***
Suriname	0.22	0.19***	0.21***	0.27**	-0.17	-0.19***	-0.12**	0.20*	-1.39	-1.42***	-1.25***	-0.56***
Swaziland	0.26	0.23***	0.24***	0.10*	-0.14	-0.18***	-0.14***	0.23**	-1.43	-1.47***	-1.36***	0.08
Tajikistan	-0.65	-0.62***	-0.55***	-0.40**	-1.55	-1.51***	-1.42***	-1.09***	-1.28	-1.24***	-1.04***	-0.79***
Trinidad and Tobago	0.04	0.04	0.06	0.06***	0.03	0.03	0.09*	0.06*	0.12	0.12***	0.23***	0.22***
Turkmenistan	-0.27	-0.27***	-0.22***	0.25***	-0.76	-0.75***	-0.67***	0.35***	-0.32	-0.31***	-0.12**	0.52***
Uganda	-0.02	-0.01	0.00	0.01	-0.03	-0.02**	0.04	0.07	-0.81	-0.80***	-0.69***	-0.52***
Ukraine	-0.23	-0.22***	-0.16***	0.16***	-1.08	-1.07***	-0.97***	-0.24***	-0.82	-0.81***	-0.59***	0.01
United Arab Emirates	-0.01	-0.23***	-0.21***	-0.22***	0.49	0.26***	0.32***	-0.08	0.57	0.34***	0.56***	0.52**
Uzbekistan	-0.35	-0.33***	-0.28***	-0.29***	-0.60	-0.58***	-0.49***	-0.27**	-0.16	-0.14***	0.06	-0.27***
Vanuatu	-0.29	-0.29***	-0.25***	-0.38***	-0.26	-0.26***	-0.22***	-0.18*	-0.80	-0.81***	-0.61***	-0.28**
Zambia	0.12	0.13***	0.15***	0.15**	-0.06	-0.05**	-0.02	-0.06	-0.68	-0.66***	-0.57***	-0.49***
Zimbabwe	-0.10	-0.12***	-0.09**	-0.15***	0.03	0.01	0.06	0.12***	-0.35	-0.37***	-0.31***	-0.39***

continued on next page

continued

	t = 0 + 1				t = 0 + 5				t = 0 + 20			
Country	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$

Note: This table reports country-specific, semi-parametric estimates of the independence dividend. Results are reported for all available NICs and pertain to the 1^{st}, 5^{th} and 20^{th} year after independence respectively. Columns headed by $\hat{\beta}_{jt}$ report the estimated percentage difference between per capita GDP for the NIC listed in the first column and its synthetic control version, corresponding to equation 10A; columns headed by $\hat{\beta}_{jt}^{tDD}$ report the trend-demeaned independence dividend estimate, net of its 10-yearly pre-independence average, as outlined in equation **??** ; columns headed by $\hat{\beta}_{jt}^{DDD}$ report the trend- and placebo-demeaned independence dividend estimate, as defined in equation 2 ; columns headed by $\hat{\beta}_{jt}^{pure}$ report the quadruple independence dividend estimate, as defined in equation 4. Standard errors are robust against heteroskedasticity and serial correlation at the country level. Bootstrapped standard errors of the pure independence dividend based on 250 replications. The number of years after secession is indicated on the horizontal axis.

*** p<0.01, ** p<0.05, * p<0.1.

Table A4: **Semi-parametric estimates of the economic impact of secession in transition countries**

	t = 0 + 1		**t = 0 + 5**		**t = 0 + 20**	
Country	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{DDDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{DDDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{DDDD}$
Armenia	-0.75***	-0.51***	-0.73***	-0.51***	-0.09	0.13
Azerbaijan	-0.42***	-0.18***	-1.20***	-0.98***	0.28***	0.50***
Belarus	-0.18***	0.06***	-0.51***	-0.28***	0.40***	0.62***
Croatia	-0.65***	-0.45***	-0.51***	-0.31***	-0.60***	-0.45***
Czech Republic	-0.28***	-0.07***	-0.31***	-0.11	-0.28***	-0.01
Estonia	-0.37***	-0.13***	-0.35***	-0.13**	0.18***	0.39***
Georgia	-0.78***	-0.53***	-1.08***	-0.85***	-0.34***	-0.16
Kazakhstan	-0.38***	-0.14***	-0.70***	-0.48***	0.13**	0.35**
Kyrgyzstan	-0.27***	-0.02	-0.80***	-0.57***	-0.58***	-0.36***
Latvia	-0.71***	-0.47***	-0.82***	-0.60***	-0.34***	-0.12
Lithuania	-0.42***	-0.18***	-0.68***	-0.45***	-0.07	0.14
Moldova	-0.20***	0.04**	-0.89***	-0.67***	-0.47***	-0.25*
Montenegro	0.18***	0.16***	0.27***	0.31***		
Russia	-0.17***	0.12*	-0.51***	-0.23***	0.14**	0.34***
Slovakia	-0.33***	-0.12***	-0.24***	-0.04	-0.06	0.21
Slovenia	-0.47***	-0.26***	-0.40***	-0.20**	-0.56***	-0.40***
Tajikistan	-0.55***	-0.31***	-1.42***	-1.20***	-1.04***	-0.86***
Turkmenistan	-0.22***	0.02	-0.67***	-0.44***	-0.12**	0.10
Ukraine	-0.16***	0.09***	-0.97***	-0.74***	-0.59***	-0.39***
Uzbekistan	-0.28***	-0.04	-0.49***	-0.27***	0.06	0.27**
ZzSerbia	0.13***	0.11***	0.15***	0.19**		

Note: This table reports country-specific, semi-parametric estimates of the independence dividend. Results are reported for all available newly formed transition countries and pertain to the 1^{st}, 5^{th} and 20^{th} year after independence respectively. Columns headed by $\hat{\beta}_{jt}^{DDD}$ report the trend- and placebo-demeaned independence dividend estimate, as defined in equation 2 ; columns headed by $\hat{\beta}_{jt}^{DDDD}$ report the quadruple-difference independence dividend estimate, as defined in equation 5. Standard errors are robust against heteroskedasticity and serial correlation at the country level. Bootstrapped standard errors of the pure independence dividend based on 250 replications. The number of years after secession is indicated on the horizontal axis.
*** p<0.01, ** p<0.05, * p<0.1.

Table A5: **Determinants of the raw independence dividend**

Channel	**Bootstrap** (1a)	(2a)	(3a)	(4a)	**Feasible Generalized Least Squares** (1b)	(2b)	(3b)	(4b)
$\bar{\beta}$	0.09***	0.09***	0.09***	0.05***	0.09***	0.10***	0.09***	0.09***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
$\bar{\beta}^{placebo}$	-0.06**	-0.06**	-0.06**	-0.05**	0.03	0.03	0.04	0.03
	(0.05)	(0.05)	(0.05)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
Trade openness	0.13***	0.13***	0.13***	0.11***	0.20***	0.19***	0.18***	0.19***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Population size	0.03***	0.03***	0.03***	0.02***	0.02**	0.02***	0.03***	0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Democracy	0.03**	0.03**	0.02**	0.03**	0.06***	0.06***	0.04***	0.04**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Financial crisis	-0.02**	-0.02**	-0.02**	-0.08**	0.11**	0.11**	0.11**	0.09*
	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)	(0.05)	(0.06)	(0.06)
Battle deaths	-0.02**	-0.02**	-0.02**	-0.03***	-0.02***	-0.02***	-0.02***	-0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Initial per capita GDP		0.00**	0.01**	-0.03***		-0.03*	-0.02	-0.03*
		(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	(0.02)
Referendum dummy			0.10***	0.07**			0.11***	0.07*
			(0.04)	(0.03)			(0.04)	(0.04)
EU dummy				0.11**				0.13*
				(0.10)				(0.07)
OPEC dummy				0.23***				0.05
				(0.05)				(0.10)
NATO dummy				-0.13**				-0.11*
				(0.08)				(0.07)
African Union dummy				-0.46***				-0.14
				(0.06)				(0.13)
ASEAN dummy				0.36***				0.13
				(0.09)				(0.15)
Observations [# countries]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]
Adjusted R^2	0.31	0.31	0.31	0.33	0.31	0.31	0.31	0.33
Region dummies	yes	yes	yes	yes	yes	yes	yes	yes
Mother country dummies	yes	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Years-of-independence dummies	yes	yes	yes	yes	yes	yes	yes	yes
Bootstrap iterations	500	500	500	500	.	.	.	.

Note: This table reports estimates of the relative importance, as defined in equation (6), of several determinants of the raw independence dividend, as defined in equation (10A). The first 4 columns report bootstrapped standard errors, based on 500 iterations and robust against estimation uncertainty in the country-year specific first-step independence dividend estimates. The last 4 columns report standard errors computed by the feasible generalized least squares estimated-dependent-variable method proposed by Lewis and Linzer (2005, p. 351-352).

Table A6: **Determinants of the trend-demeaned independence dividend**

	Bootstrap				**Feasible Generalized Least Squares**			
Channel	(1a)	(2a)	(3a)	(4a)	(1b)	(2b)	(3b)	(4b)
$\bar{\beta}$	0.01**	0.01**	0.00**	-0.03**	0.01	0.02	0.01	0.00
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
$\bar{\beta}^{placebo}$	-0.06**	-0.06**	-0.06**	-0.04**	0.07	0.07	0.04	0.05
	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)
Trade openness	0.13***	0.13***	0.13***	0.11***	0.23***	0.22***	0.21***	0.21***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Population size	0.05***	0.05***	0.06***	0.05***	0.03**	0.03**	0.03**	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Democracy	0.03***	0.04***	0.02**	0.03***	0.07***	0.07***	0.05***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Financial crisis	-0.05**	-0.05**	-0.05**	-0.11***	0.06	0.06	0.05	0.06
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Battle deaths	-0.02**	-0.02**	-0.02**	-0.03***	-0.02***	-0.02***	-0.02***	-0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Initial per capita GDP		0.00**	0.01**	-0.03***		-0.02	-0.02	-0.06**
		(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	(0.03)
Referendum dummy			0.11***	0.09***			0.05	0.07**
			(0.04)	(0.03)			(0.04)	(0.04)
EU dummy				0.13**				0.00
				(0.09)				(0.07)
OPEC dummy				0.19***				0.31***
				(0.05)				(0.10)
NATO dummy				-0.14**				-0.01
				(0.09)				(0.06)
African Union dummy				-0.47***				-0.27**
				(0.07)				(0.14)
ASEAN dummy				0.37***				0.04
				(0.09)				(0.13)
Observations [# countries]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]
Adjusted R^2	0.29	0.29	0.29	0.31	0.29	0.29	0.29	0.31
Region dummies	yes	yes	yes	yes	yes	yes	yes	yes
Mother country dummies	yes	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Years-of-independence dummies	yes	yes	yes	yes	yes	yes	yes	yes
Bootstrap iterations	500	500	500	500	.	.	.	.

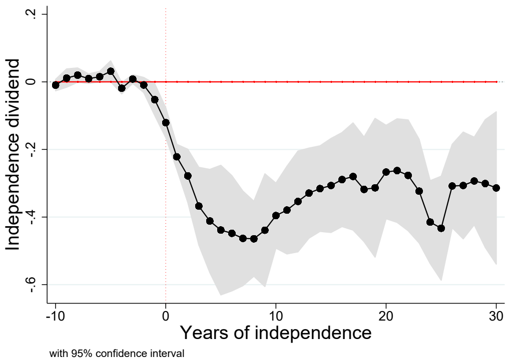
Note: This table reports estimates of the relative importance, as defined in equation (6), of several determinants of the trend-demeaned independence dividend, as defined in equation (1). The first 4 columns report bootstrapped standard errors, based on 500 iterations and robust against estimation uncertainty in the country-year specific first-step independence dividend estimates. The last 4 columns report standard errors computed by the feasible generalized least squares estimated-dependent-variable method proposed by Lewis and Linzer (2005, p. 351-352).

Table A7: **Determinants of the placebo-demeaned independence dividend**

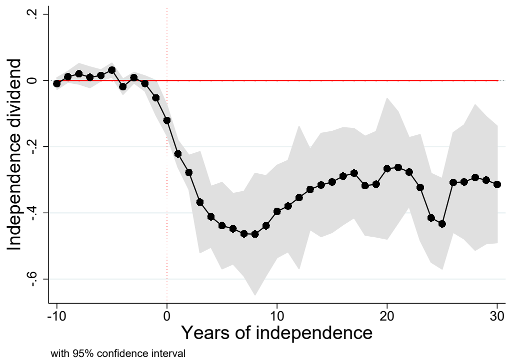
	Bootstrap				**Feasible Generalized Least Squares**			
Channel	(1a)	(2a)	(3a)	(4a)	(1b)	(2b)	(3b)	(4b)
$\bar{\beta}$	0.09***	0.09***	0.09***	0.05***	0.12***	0.13***	0.13***	0.11***
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
$\bar{\beta}^{placebo}$	-0.14***	-0.13***	-0.13***	-0.12**	-0.04	-0.04	-0.05	-0.07
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)
Trade openness	0.13***	0.14***	0.13***	0.11***	0.23***	0.23***	0.25***	0.24***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Population size	0.02***	0.03***	0.03***	0.03***	0.08***	0.08***	0.06***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Democracy	0.03**	0.03**	0.02**	0.03**	0.02	0.02	0.03*	0.03
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Financial crisis	-0.03**	-0.03**	-0.02**	-0.08**	-0.07	-0.07	-0.07	-0.09
	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)	(0.07)
Battle deaths	-0.03**	-0.03**	-0.02**	-0.03***	-0.04***	-0.04***	-0.04***	-0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Initial per capita GDP		0.00**	0.01**	-0.03***		-0.00	-0.00	-0.04***
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Referendum dummy			0.10***	0.07**			-0.06	-0.14***
			(0.03)	(0.04)			(0.04)	(0.05)
EU dummy				0.12**				-0.06
				(0.08)				(0.13)
OPEC dummy				0.22***				0.26***
				(0.05)				(0.07)
NATO dummy				-0.14**				-0.02
				(0.10)				(0.07)
African Union dummy				-0.47***				-0.26*
				(0.07)				(0.14)
ASEAN dummy				0.36***				0.33**
				(0.09)				(0.15)
Observations [# countries]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]	2157 [77]
Adjusted R^2	0.29	0.29	0.29	0.31	0.29	0.29	0.29	0.31
Region dummies	yes	yes	yes	yes	yes	yes	yes	yes
Mother country dummies	yes	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Years-of-independence dummies	yes	yes	yes	yes	yes	yes	yes	yes
Bootstrap iterations	500	500	500	500	.	.	.	.

Note: This table reports estimates of the relative importance, as defined in equation (6), of several determinants of the placebo-demeaned independence dividend. The first 4 columns report bootstrapped standard errors, based on 500 iterations and robust against estimation uncertainty in the country-year specific first-step independence dividend estimates. The last 4 columns report standard errors computed by the feasible generalized least squares estimated-dependent-variable method proposed by Lewis and Linzer (2005, p. 351-352).

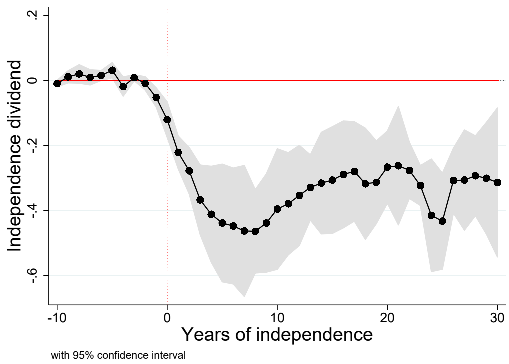
Figure A2: **Average impact of secession in selected countries**



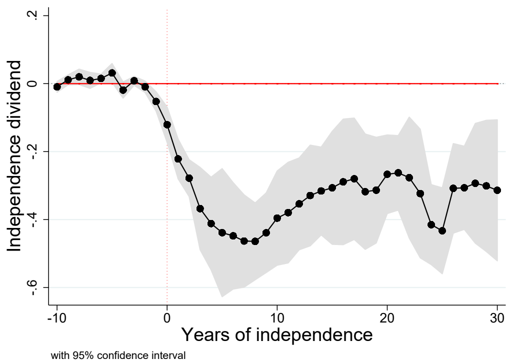
(a) All cases



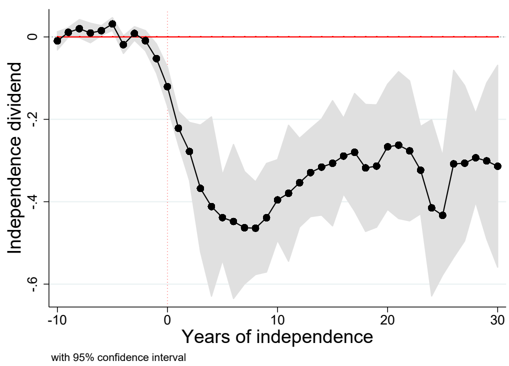
(b) 80% best matched cases



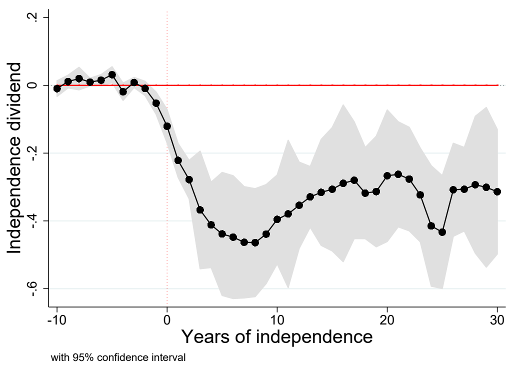
(c) 60% best matched cases



(d) 40% best matched cases



(e) 20% best matched cases

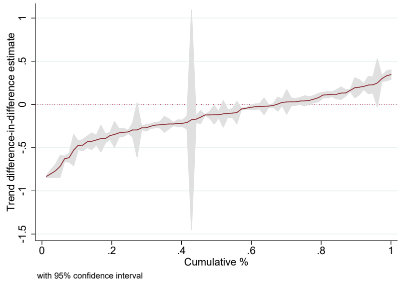


(f) All cases within caliper

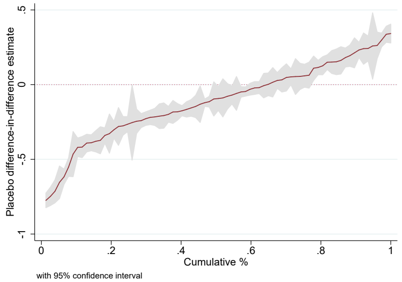
Note: This figure plots the yearly population-weighted average percentage per capita GDP gap between NICs and their synthetic counterparts, along with bootstrapped 95% confidence intervals based on 500 iterations. The number of years before (-) or after (+) independence are indicated on the horizontal axis. The top-left panel contains all available cases, subsequent panels include only results of the 80, 60, 40 and 20% best matched cases in terms of their pre-independence RMSPE. The bottom figure includes only those cases for which the pre-independence RMSPE falls within the data-driven caliper cut-off amounting to 0.5 times the samplewide standard deviation in pre-independence RMSPE.

Figure A3: **Cumulative estimates of independence dividends at selected time-points**

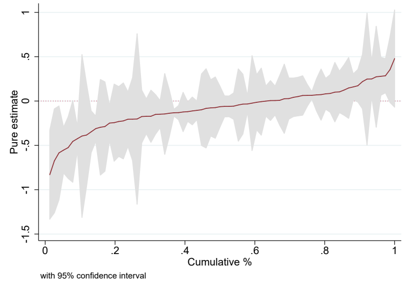
1^{st} year after independence



(a) Trend-demeaned

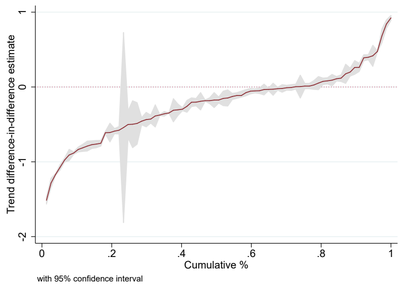


(b) Triple-difference

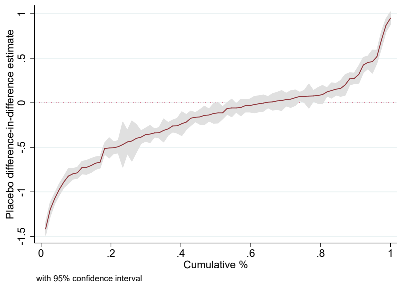


(c) Pure

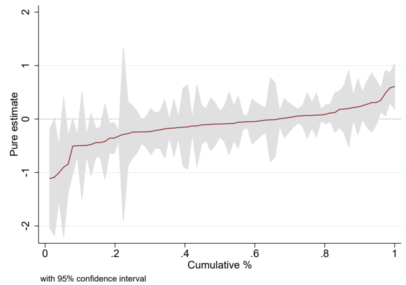
5^{th} year after independence



(a) Trend-demeaned

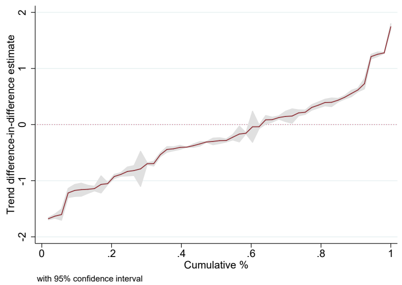


(b) Triple-difference

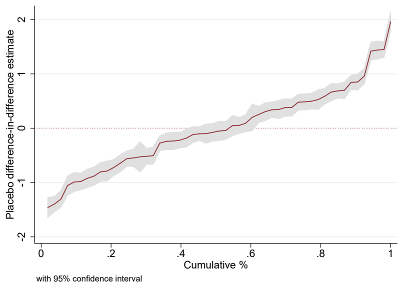


(c) Pure

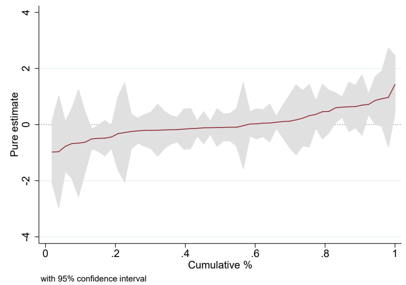
30^{th} year after independence



(a) Trend-demeaned



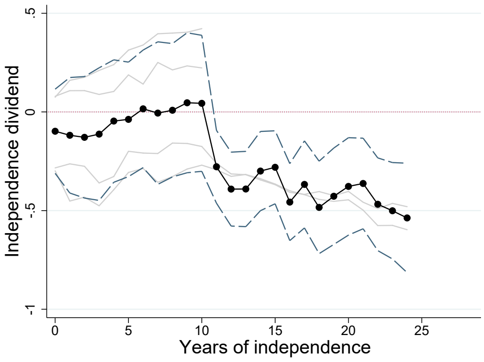
(b) Triple-difference



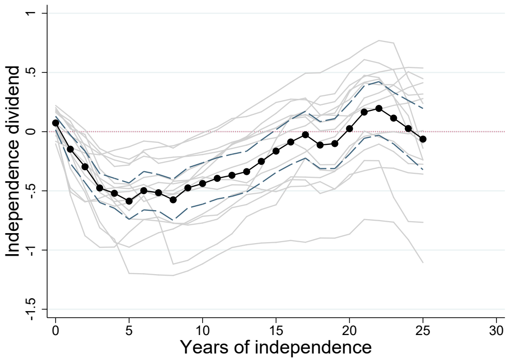
(c) Pure

Note: This figure plots the cumulative distribution of the country-specific trend-demeaned, triple-difference and pure independence dividend estimates reported in table A2, along with 95% confidence intervals. The horizontal axis indicates the proportion of NICs with an independence dividend estimate below the cut-off value indicated on the vertical axis. Estimated independence dividends pertain to the 1^{st}, 5^{th} and 30^{th} post-independence year respectively.

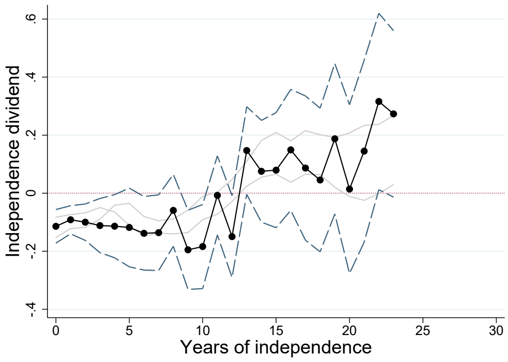
Figure A4: **Quadruple-difference estimates of the independence dividend**



(a) Yugoslavia



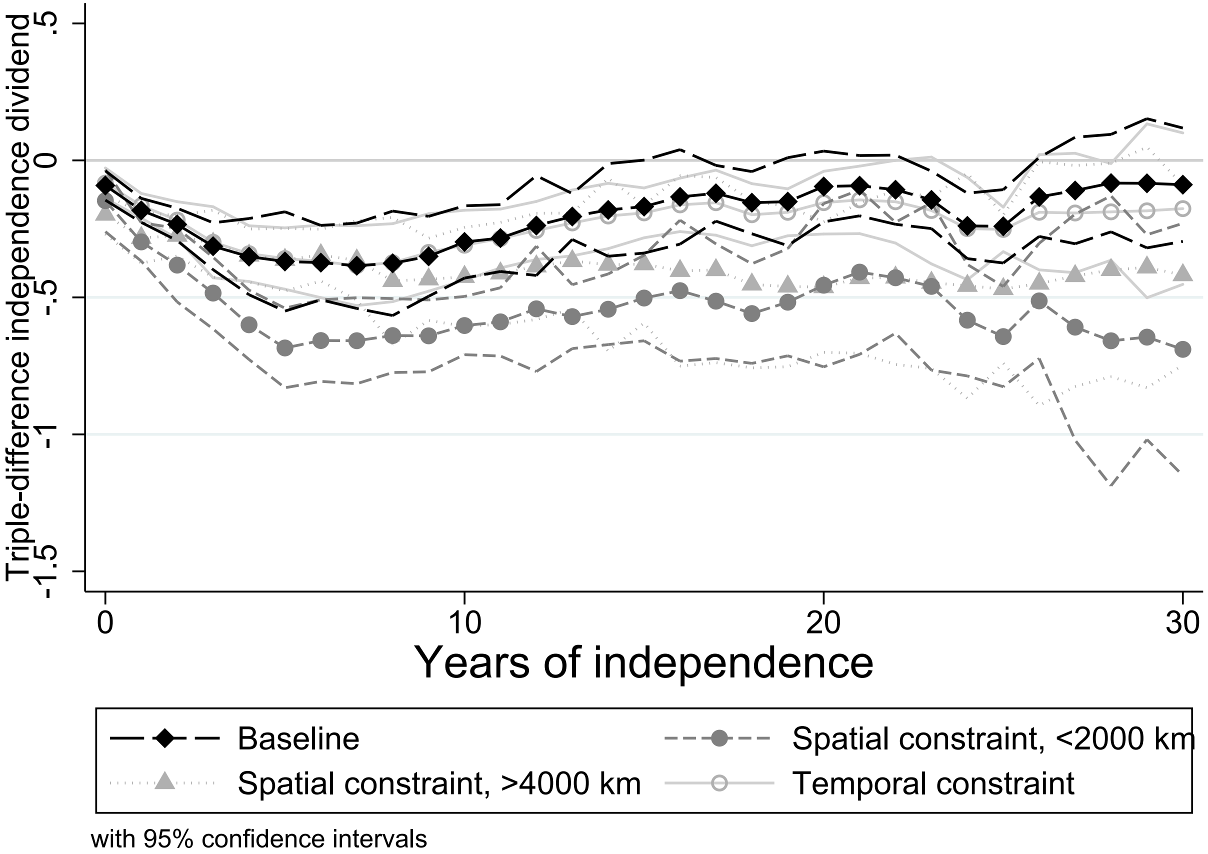
(b) Soviet Union



(c) Czechoslovakia

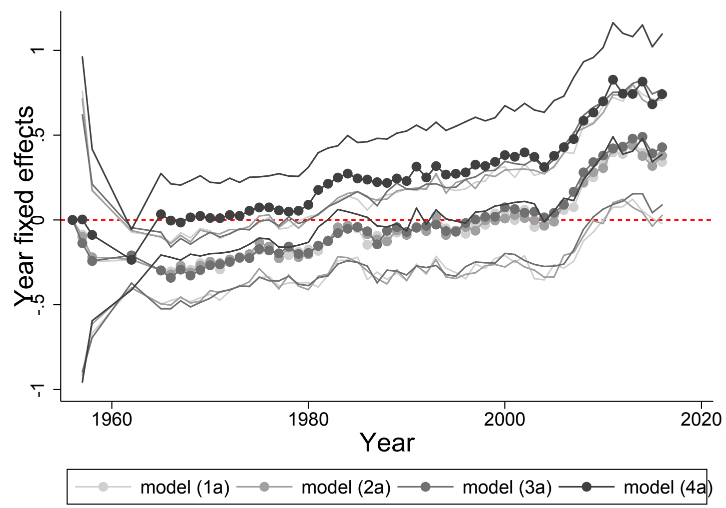
Note: The figures plot yearly, quadruple-difference estimates of the independence dividend trajectories associated with selected historical instances of state fragmentation. Each gray line plots the trajectory of a specific former member state; the black lines depict the aggregate independence dividend trajectory; the dashed lines depict the 95% bootstrapped confidence interval, clustered at the country level and based on 250 replications. The number of years after independence is indicated on the horizontal axis.

Figure A5: **Semi-parametric estimates of the economic impact of secession: robustness results**

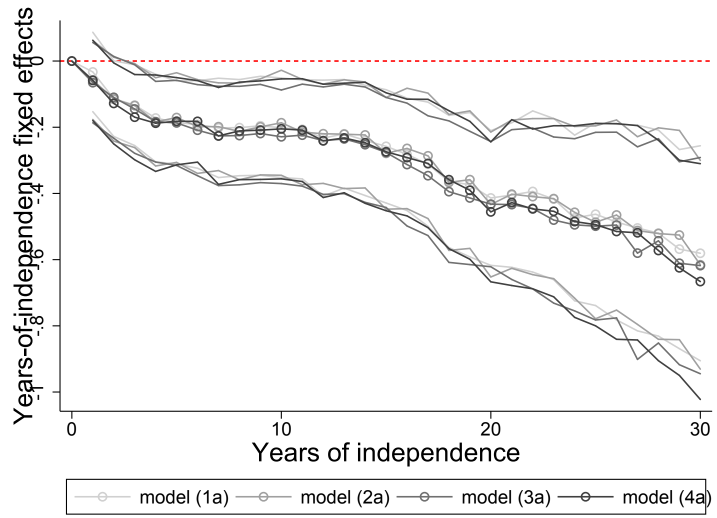


Note: The figure compares the baseline population-weighted triple-difference independence dividends (squares) with alternative triple-difference estimators that respectively impose a spatial constraint to limit the pool of potential control countries to countries that are located in direct geographical proximity of the NIC under consideration (full circles); or to countries that are on the contrary located far away from the NIC under consideration (traingles); or to countries that have been independent for at least 30 years when the NIC under consideration declared independence (hollow circles). Block-bootstrapped 95% confidence intervals are based on 500 iterations and are robust against heteroskedasticity, serial correlation at the country level and estimation uncertainty in individual independence dividend estimates. The number of years after secession is indicated on the horizontal axis.

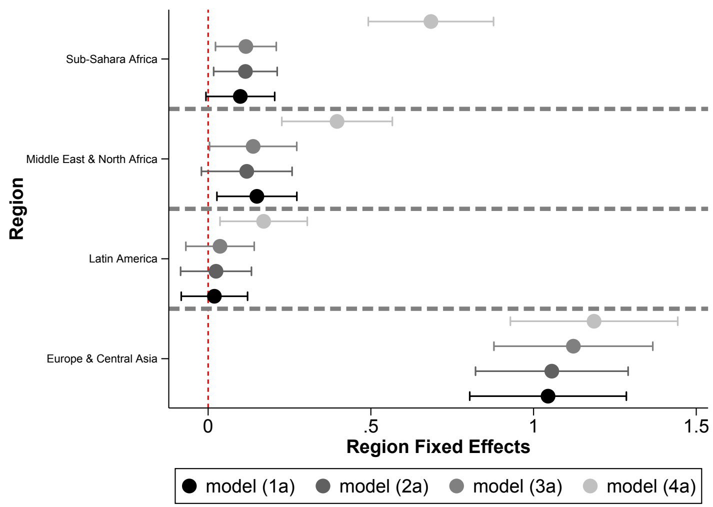
Figure A6: **Determinants of the triple-difference independence dividend: fixed effects**



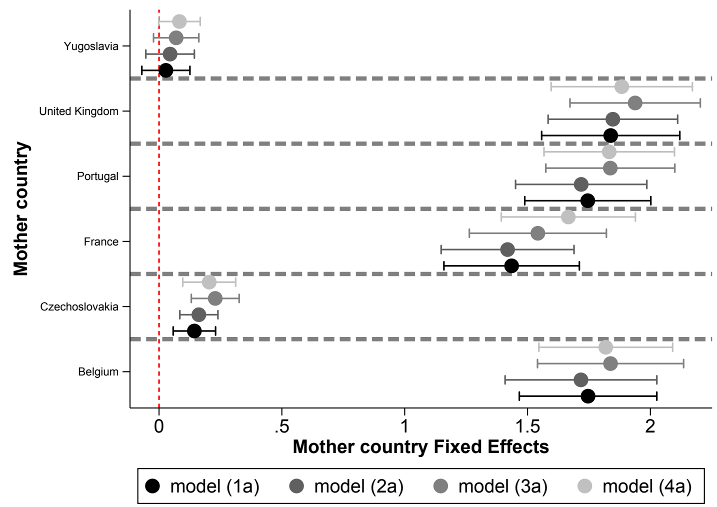
(a) Year fixed effects



(b) Years-of-independence fixed effects



(c) Region fixed effects



(d) Mother country fixed effects

Note: This figure respectively plots estimates for the year-fixed effects, the years-of-independence fixed effects, the region fixed effects and the most relevant mother-country fixed effects corresponding to the bootstrap estimation models reported in columns (1a) through (4a) of table 4. Year fixed effects are relative to 1980; years-of-independence fixed effects are relative to the year of independence ; region fixed effects are relative to East Asia & Pacific; mother-country fixed effects are relative to the Soviet Union.