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

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

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Abstract

This paper provides empirical evidence that declaring independence significantly lowers per capita GDP based on a large panel of countries covering the period 1950-2013. 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. Our baseline results indicate that declaring independence reduces per capita GDP by around 20% in the long run. We subsequently propose a novel triple-difference procedure to demonstrate the stability of these results. Another methodological novelty consists of the development of a two-step estimator to shed some light on the primary channels driving our results. We find robust evidence that the adverse effects of independence increase in the extent of surface area loss, pointing to the presence of economies of scale, but that they are mitigated when newly independent states liberalize their trade regime or use their new-found political autonomy to democratize.

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

JEL Classification: C14, C32, H77, O47

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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, Scotlands 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 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.

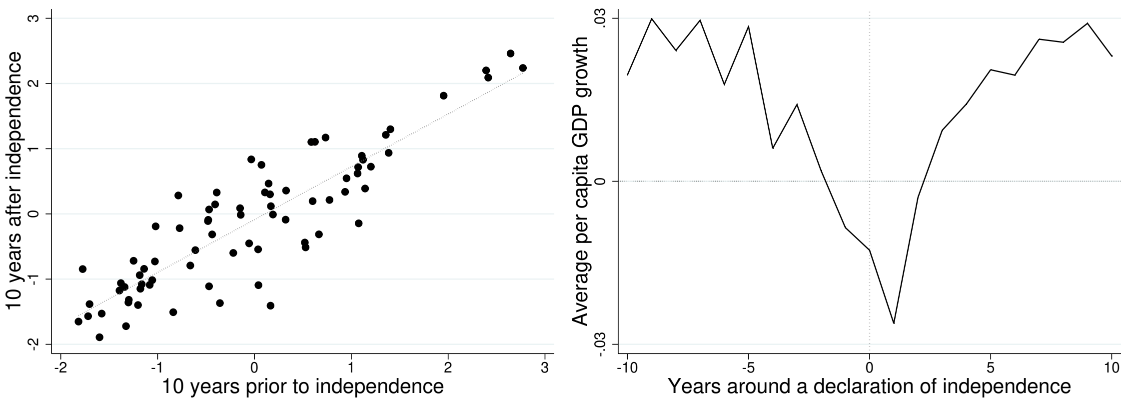
In order to provide a preliminary view on the existence as well as the magnitude of the independence dividend, Figure 1a presents difference-in-difference estimates of the impact of declaring independence on the relative economic performance of NICs, where the 'relative economic performance' of a country 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 declaration of independence 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 pertaining to 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 this period whereas others seemingly incurred an independence cost. Nevertheless, the aggregate difference-in-difference estimate of -.08 suggests that the net gain of independence tended to be negative and lowered per capita income by 8%, 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 also the effect of pre-independence differences in terms of economic growth determinants. Second, as illustrated in figure 1b, NICs, in the run-up to their declaration of independence, are typically confronted with a sharp decline in per capita GDP growth rates. As per capita GDP trajectories tend to be highly persistent, this raises an obvious endogeneity concern. In other words, it is important to

[3]Table 1 provides a more detailed account.

distinguish the causal impact of declaring independence on future growth potential, ruling out any feedback-effects past growth dynamics might have on the contemporary incentive 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 20.5% to 46.4%. Yet, there is considerable cross-country heterogeneity in the economic impact of secession.[4]

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 triple-difference procedure to assess the reliability of the results. Most reassuringly, we find 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. This underscores that our results are unlikely to be driven by simulation inaccuracy or unobserved heterogeneity. Nevertheless, we show that not correcting for potential biases stemming from simulation and matching quality tends to inflate both the estimated net cost of independence as well as its persistence.

One additional methodological contribution concerns the development of a two-step procedure to shed some light on the channels primarily driving the sign and the magnitude of these independence payoffs. To do so, we regress the estimated independence payoffs

[4]To demonstrate that these findings appear to hold irrespective of the estimation procedure employed, Appendix B follows a parametric approach to estimate the independence payoff, obtaining similar results.

on a number of underlying characteristics and evaluate various 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 set-up provides a meaningful way to empirically evaluate the various claims laid out in the existing literature. Doing so, we find tentative evidence that the cost of independence increases in the degree of surface area loss, pointing to the presence of economies of scale. These negative effects dissipate, however, when trade barriers fall or democratic institutions improve. We fail to find clear-cut results with respect to the effect of state size, macroeconomic uncertainty and the intensity of military conflict.

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, 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

[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."*.

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 & Robinson, 2009). Due to institutional persistence, moreover, colonial origin is argued to be at the root of contemporary growth differentials in Latin America and Africa (Bertocchi & Canova, 2002). 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]

One hitherto overlooked issue is the temporal coincidence of surges of secession and surges of democracy (Spencer, 1998). 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) use 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) rely on a difference-in-

[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).

[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 summary of this literature.

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 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 results 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. 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 1950-2013. 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 116 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 Gleditsch and Ward (1999). Table A5 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 of conflict, we include the 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 dummy variables indicating 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 (2013), we subsequently maximally extend these estimates forward and backwards relying on the growth rate of real per capita income provided by the World Bank (2015) 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, 2015), (ii) a year dummy and

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

[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 are set to 0 in both indexes.

[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).

(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. In addition they tend to be somewhat more sensitive to military conflict and experience more instances of debt crises. As suggested in the existing literature, however, they also tend to be less politically unstable 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.

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	4999	9086.64	14225.64	6901	4011.41	5341.306	-5075.23***	0.00
Population (millions)	5118	46.22	143.853	7222	8.34	19.888	-37.88***	0.00
Population density	5118	265.84	1508.283	7222	128.29	432.939	-137.55***	0.00
Years of schooling	4874	6.22	3.123	6318	4.98	3.217	-1.23***	0.00
Life expectancy	4786	65.60	11.185	6836	57.92	12.053	-7.69***	0.00
Openness	4667	0.58	.51	5098	0.84	.482	0.27***	0.00
Battle deaths per capita	5118	0.00	.001	7222	0.00	.001	0.00*	0.09
Democracy	4937	15.65	14.194	4800	8.44	9.854	-7.21***	0.00
Political instability	4937	0.00	.049	4800	0.00	.032	-0.00*	0.10
Banking crisis	3520	0.10	.302	960	0.10	.295	-0.01	0.59
Debt crisis	3520	0.11	.313	960	0.20	.399	0.09*	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.

[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.

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. A last subsection formulates an inferential framework to perform some robustness checks.

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). 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.

To see why this works, 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^N_{jt} the level of log per capita GDP that would be observed in country j if it did not (yet) declare independence, and let y^T_{jt} 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 $\beta_{jt} = y^T_{jt} - y^N_{jt}$.[15] The observed outcome for each country i can be written as

$$y_{i,t} = y^N_{i,t} + \beta_{i,t} NIC_{i,t} \tag{1}$$

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.

[15]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.

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, $\hat{y}^N_{j,t}$:

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

Although $y^N_{j,t}$ remains unobserved for $t \geq T_0$, suppose we do know $y^N_{i,t}$ 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 $y^N_{j,t}$ 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 $y^N_{i,t}$ is subject to year fixed effects, η_t, and a mean-zero transitory shock, $\epsilon_{i,t}$. Summarizing, we assume $y^N_{j,t}$ to be given by

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

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 1

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

Condition 2

$$\sum_{i\neq j}^{J+1} w_i^* y_{i,1950}^N = y_{j,1950}^N \ , \ \ldots \ , \ \sum_{i\neq j}^{J+1} w_i^* y_{i,T_0-1}^N = y_{j,T_0-1}^N$$

Condition 3

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

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

$$\sum_{i\neq j}^{J+1} w_i^* y_{i,t}^N = \theta_t \sum_{i\neq j}^{J+1} w_i^* \mathbf{X}_i + \lambda_t \sum_{i\neq j}^{J+1} w_i^* \mathbf{Z}_i + \eta_t + \sum_{i\neq j}^{J+1} w_i^* \epsilon_{i,t} \tag{4}$$

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

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

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:

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

or, equivalently:

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

Pre-multiplying both sides of equation (7) by the inverse of λ^P , $(\lambda^{P'} \lambda^P)^{-1} \lambda^{P'}$, yields[16]

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

Finally, inserting this expression for $\mathbf{Z}_j - \sum_{i\neq j}^{J+1} w_i^* \mathbf{Z}_i$ in equation (5) yields an expression for the discrepancy between the (partly unobserved) full outcome path that would be

[16]Note that the assumption that $m \leq T_0 - 1950$ is made to ensure that λ^P is nonsingular and thus has a well-defined inverse.

observed in the seceding country, j, in absence of state fragmentation and the same (fully observed) outcome path for its synthetic version, $\mathbf{W}^*$:

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

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 (9) will tend towards zero. This suggests using

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

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

Note that the primary strength of the synthetic control method is the lack of conditions imposed on the m 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, note that 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, θ_t, making it more robust against model uncertainty.

In practice, since there often does not exist a set of weights that exactly satisfies conditions 1 through 3, standard practice is to construct the synthetic control such that these conditions hold approximately. In the empirical exercise below, we do so by relying on the nested optimalization algorithm developed by Abadie and Gardeazabal (2003, 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 (6)).[17] 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.[18] 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.

[17] The synthetic control algorithm is implemented by Abadie et al.'s (2010) *synth*-command in Stata 13.1.
[18] Table A5 lists the NICs included in the synthetic control algorithm.

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 - Panama, Romania, the United States and South Korea, see table 2.

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

Country	w^*
Panama	.405
Romania	.397
United States	.103
Korea Republic	.095

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 considerably higher 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 number of battle deaths suffered.

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

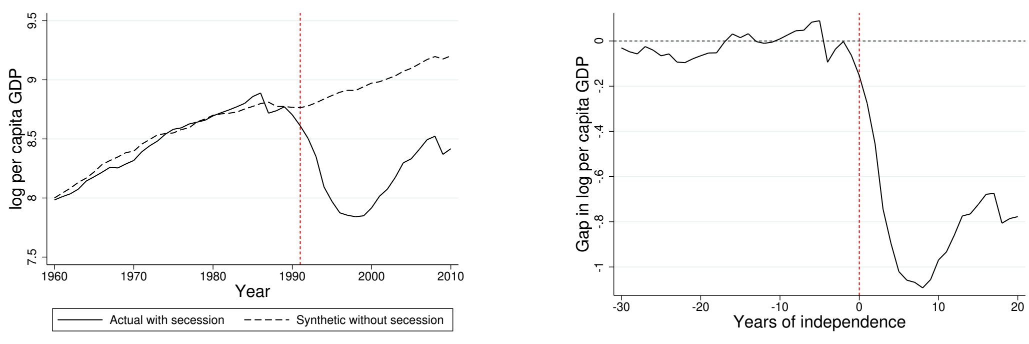
Predictor	*Ukraine*	*Synthetic Ukraine*	*World*
Per capita GDP	4357.760	4495.777	6608.467
log Population	17.745	16.293	15.175
Population density	84.317	84.253	216.558
Educational attainment	8.831	8.570	5.367
Health	70.048	70.994	62.988
Trade openness	1.019	.958	.721
Battle deaths (per 1000 heads)	0	.009	.107

Note: Growth predictors are average over the 1981-1990 period. The last column reports 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.[19] 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.

The estimated economic effect of the Ukraine 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 log form, the discrepancy between both reflects the percentage per capita payoff of having declared independence.

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.

[19]Note also, however, that we see a slight diversion between both series even in the (immediate) pre-independence period, suggesting the presence of anticipation effects in the two 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 Ukrainian independence to have occurred three years prior to the actual decision to secede. None of the results are qualitatively affected by this.

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 lies around 78% 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 different versions of the results of this exercise. First, consider the top-left panel which plots the results for each separate NIC in our sample for which sufficient data are available. The gray lines represent the gaps in per capita GDP 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 average 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.[20]

As the figure also indicates, the synthetic control method provides a reasonably good fit for the (log) per capita GDP trajectories between NICs and their respective synthetic counterparts in the pre-independence period. The average pre-independence RMSPE in the full sample is about 0.105, 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 0.5% 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 under-performance clearly worsens and the average percentage discrepancy increases to -19.6%. Interestingly, NICs generally do not appear to recover in the longer run as the negative aggregate independence

[20] Country-specific results are reported in table A6.

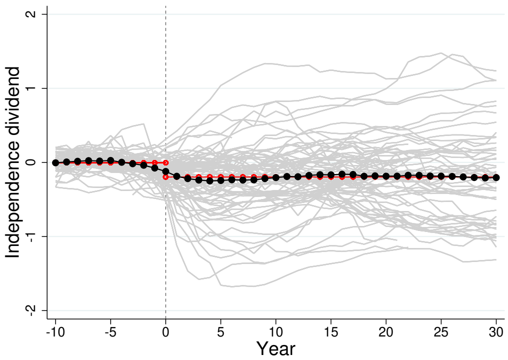
dividend equals -21.3% in the 30^{th} post-independence year. In other words, when their country celebrates its 30^{th} birthday, inhabitants of NICs typically experience per capita GDP levels which lie 21% below those of countries which, in all relevant aspects, most closely resembled their own country's 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. Malawi, for instance, is the country with the worst pre-independence fit (RMSPE=0.33). 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 more 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[21], figures 3b to 3d plot the results when the sample is progressively restricted to include only the 50%, 20% and 10% 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 0.05, 0.034 and 0.02 respectively). Sacrificing quantity for quality, however, does not qualitatively affect our primary conclusions: 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 32%-46% in the long run.

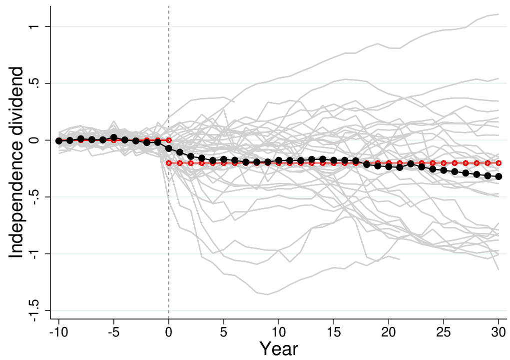
As there does not appear to be a consensus on the optimal cut-off of pre-independence RMSPE to avoid biases stemming from poor-fit, the bottom figure utilizes a more data-driven procedure to impose a threshold value (or caliper) defining the maximal allowed RMSPE. More specifically, in the tradition 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 3e 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 0.024, while our primary conclusions remain robust.

[21]Since they are unlikely to even approximately satisfy conditions 1 through 3.

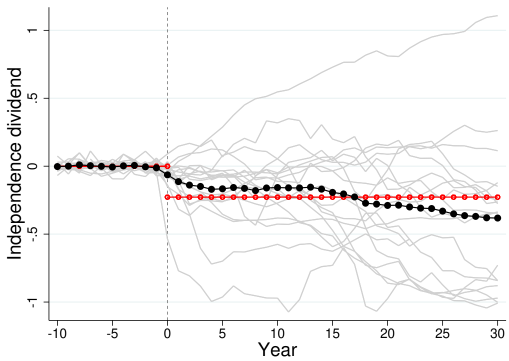
Figure 3: **Impact of secession in selected countries**



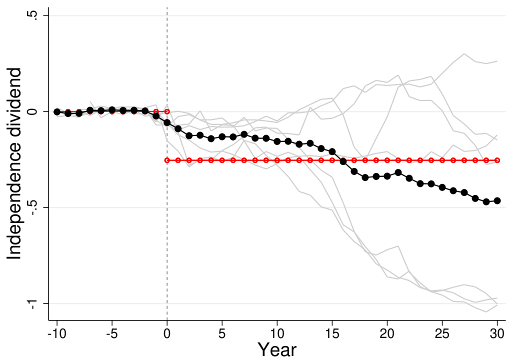
(a) All cases



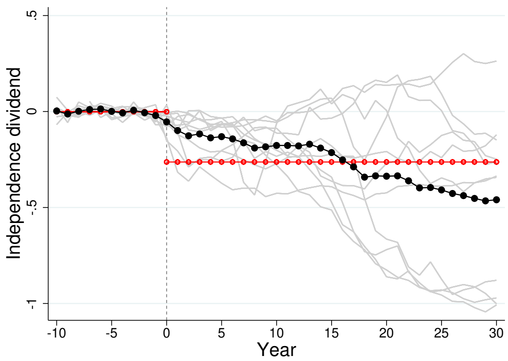
(b) 50% best matched cases



(c) 25% best matched cases



(d) 10% best matched cases



(e) All cases within caliper

Note: Each gray line plots the yearly percentage gap 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 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 50, 25 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 Statistical inference

To gauge the significance of these estimates, we first check whether a causal interpretation is warranted by their distribution. Therefore, figure A3 plots the same sequence of aggregate independence dividend estimates but now includes a 95% confidence interval. Notice that the pre-independence per capita GDP discrepancy between NICs and their synthetic counterparts in the full sample gradually erodes to become statistically indistinguishable when trimming the sample according to goodness-of-fit. More importantly, these graphs confirm the tendency of NICs to increasingly underperform *vis-á-vis* their synthetic counterparts in the entire post-independence period, irrespective of the selected sample. The larger independence costs estimated in the smaller samples, where meaningful pre-independence discrepancies between NICs and synthetic NICs are absent, suggest that the full-sample results effectively underestimate the true economic impact of secession by ignoring anticipation effects.

Additionally, 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.[22]This motivated 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-yearly 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. In this sense, the significance of the actual independence dividend estimates can be evaluated against their distribution in the pre-independence period.

Further illustrating the rationale for this inferential exercise, figure A4 plots the discrepancy between Ukraine and synthetic Ukraine in the first post-independence year against the distribution of per capita GDP discrepancies between both in the pre-independence period. Clearly, the per capita GDP discrepancy between Ukraine and synthetic Ukraine in the first post-independence year is unusually large compared to the distribution of per capita GDP discrepancies observed between both in absence of state fragmentation. The first-year independence dividend estimate is therefore unlikely to reflect unobserved heterogeneity, but measures the economic impact of secession as intended.

To formalize this approach, we specify a trend difference-in-difference estimator in order to obtain *trend-demeaned* estimates of the independence dividend. Intuitively, a causal interpretation of our results would be problematic if these estimated trend-demeaned post-

[22]See equations (6) and (7).

independence gaps, net of their average pre-independence discrepancy, did not significantly differ from zero. Formally, denoting the weighting vector defining the synthetic counterpart of NIC j by $w^*_{ij} = [w^*_{1j}, \ldots, w^*_{Ij}]$, we define the trend difference-in-difference estimate of the economic independence dividend for NIC j, s years after it declared independence, as:

$$\hat{\beta_{j,s}}^{tDD} = \left(y_{j,T_0+s} - \sum_{i \neq j} w^*_{i,j} y_{i,T_0+s} \right) - \left(\sum_{t=T_0-10}^{T_0-1} \Big(y_{j,t} - \sum_{i \neq j} w^*_{i,j} y_{i,t} \Big) \right) \tag{11}$$

The resulting country-specific trend difference-in-difference independence dividend estimates are reported in table A6. Compared to the raw independence dividend estimates, trend-demeaned estimates tend to be slightly lower in absolute value. Hence, not correcting for the extent of observed pre-independence discrepancies tends to slightly inflate the estimated net cost of independence. Nevertheless, trend-demeaned estimates are quantitatively and qualitatively very similar to their raw counterparts and thus largely back the story that emerged earlier. More specifically, a closer inspection of the results plotted in figure A5 reveals that, irrespective of the time-horizon, roughly 60 to 70% of NICs suffered economic costs of secession even after correcting for matching quality, while the remaining 20 to 30% appears to have benefited from becoming independent. This also underscores the high persistence in the estimated independence dividend trajectories, implying that adversely affected NICs generally show little sign of recovery in the longer run.

Note, however, that the confidence intervals plotted in figures A3 and A5 only express the degree of 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 counter-factual per capita GDP trajectories NICs would have experienced in absence of state fragmentation. Indeed, 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.[23] To study the robustness of the results in this regard, we depart from 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 NICs' donor pool.[24] As the countries involved in this exercise 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 hypothesis of a zero effect. In this light, the significance of actual independence dividend estimates can be evaluated against their corresponding distribution of placebo gaps.

Once again reconsidering the Ukrainian example, figure A6 plots the actual Ukrainian independence dividends against the distribution of placebo gaps, resulting from an ap-

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

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

plication of the synthetic control algorithm to each of its 153 potential control countries. Clearly, the estimated independence dividend trajectory for Ukraine lies well outside the distribution of placebo gaps, suggesting that it is unusually negative under the null hypothesis of a zero effect. This is especially so if we restrict attention to the best-matched potential control countries, by imposing a caliper equal to the RMSPE attained by synthetic Ukraine. All of this indicates that the Ukranian independence dividend trajectory is not likely to be driven by simulation inaccuracy, but reflects the economic impact of secession as intended.

Figure A7 suggests that this conclusion also holds for the full sample, by plotting the full-sample distribution of actual independence dividend estimates against those of their placebo counterparts in the 40-year period surrounding a declaration of independence. Although placebo countries tend to under-perform somewhat *vis-á-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 contrast to NICs, per capita GDP discrepancies in the placebo group typically do not react strongly, if at all, when their corresponding NIC declares its independence. This underlines the capacity of the synthetic control algorithm to approximate the economic behavior of countries in absence of state fragmentation, especially apparent in the best-matched cases, thereby bolstering the reliability of our prior findings.

To formalize this approach, we specify a placebo difference-in-difference estimator in order to obtain *placebo-demeaned* estimates of the independence dividend. Intuitively, a causal interpretation of our results would seem unwarranted if artificially reassigning the decision to secede to countries unaffected by processes of state fragmentation would yield placebo dividend estimates similar or larger in absolute value. Formally, indexing the control countries in NIC j's donor pool by $k \in [1, \ldots, K_J]$, we define the placebo difference-in-difference estimate of the independence dividend of NIC j, s years after it declared independence, as:

$$\hat{\beta_{j,s}}^{pDD} = \left(y_{j,T_0+s} - \sum_{i \neq j} w^*_{i,j} y_{i,T_0+s} \right) - \frac{1}{K_j} \sum_{k \neq j}^{K_j} \left(\left(y_{k,T_0+s} - \sum_{i \neq k, i \neq j} w^*_{i,k} y_{i,T_0+s} \right) \right) \quad (12)$$

Country-specific placebo difference-in-difference independence dividend estimates are reported in table A6 and plotted in figure A8. Correcting the magnitude of the synthetic control estimates for discrepancies that can reasonably attributed to poor simulation quality results in estimates lower in absolute value suggesting, once again, that our baseline results slightly overestimate the true independence cost. Consequently, figure A8 illustrates that only 50% of the NICs in the sample appears to have suffered adverse long-run consequences from becoming independent, while close to 40% of them appear to have benefited in economic terms from having done so.

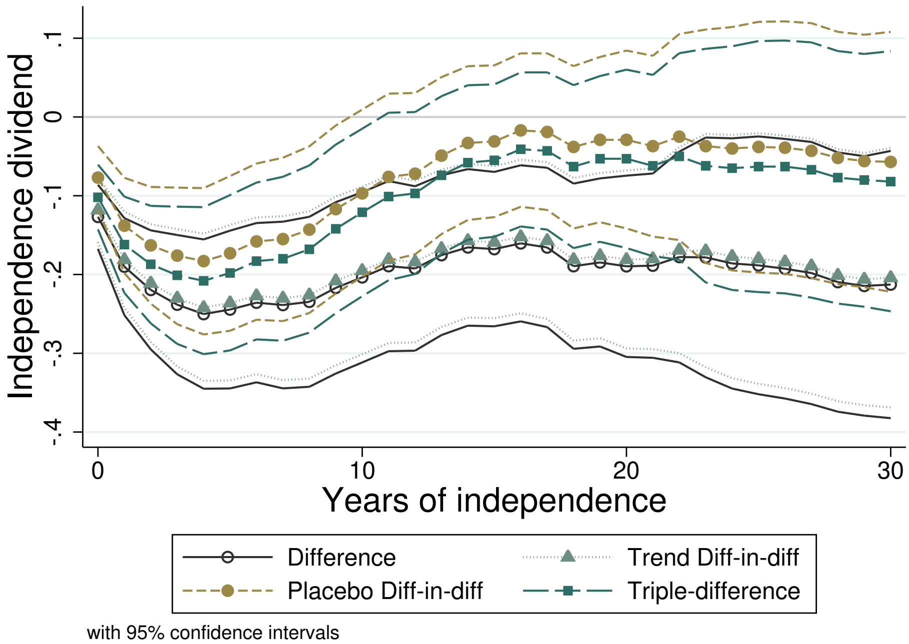
Fourthly, to mitigate potential biases emanating from either unobserved heterogeneity between NICs and synthetic NICs or simulation inaccuracies in the counterfactual per capita GDP trajectories, or a combination of both, we consider removing both the 10-yearly average pre-independence discrepancy as well as the contemporary average discrepancy observed in the placebo group from the estimated independence dividend. Intuitively, a causal interpretation of our results would seem premature if the change in the relative performance of NICs versus synthetic NICs, after they have declared independence, would have the same direction and magnitude as the relative performance deviation typically observed in placebo countries. Formally, we propose the following triple-difference estimator of the economic impact of secession in NIC j, s years after secession:

$$\begin{aligned}\hat{\beta}_{j,s}^{DDD} =& \left[\left(y_{j,T_0+s} - \sum_{i\neq j} w_{i,j}^{*} y_{i,T_0+s}\right) - \left(\sum_{t=T_0-10}^{T_0-1}\left(y_{j,t} - \sum_{i\neq j} w_{i,j}^{*} y_{i,t}\right)\right)\right] - \\ & \frac{1}{K_j}\sum_{k\neq j}^{K_j}\left[\left(y_{k,T_0+s} - \sum_{i\neq k, i\neq j} w_{i,k}^{*} y_{i,T_0+s}\right) - \left(\sum_{t=T_0-10}^{T_0-1}\left(y_{k,t} - \sum_{i\neq k} w_{i,k}^{*} y_{i,t}\right)\right)\right]\end{aligned} \tag{13}$$

The resulting country-specific triple-difference estimates of the post-independence independence dividends are reported in table A6 and plotted in figure A9. Unsurprisingly, 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 A9 reveals that correcting for matching as well as simulation quality does not qualitatively affect our previous inferential conclusions. In tandem with our previous results, our estimates thus indicate that declaring independence tends to be costly since over 50% of the countries in the sample experienced a negative payoff of independence, 30 years after they became independent, whereas only 35% experienced a net independence gain.

Finally, to get a sense of the general implications of these various inferential exercises, figure 4 provides an overview of the various aggregate independence dividend estimates by plotting the synthetic control estimates (hollow dots), trend difference-in-difference (triangles), placebo difference-in-difference (diamonds) and triple-difference (squares) independence dividend estimates for each year in the 30-year period following state fragmentation, along with their 95% confidence intervals. As noted, correcting for potential biases stemming from matching and simulation quality generally reduces these aggregate negative independence dividend estimates. Nevertheless, the figure suggests that the raw independence dividend estimates are more sensitive to simulation inaccuracies than to unobserved heterogeneity, as correcting for the magnitude of the placebo independence dividend typically leads to the most pronounced upward correction. In conclusion, our most conservative triple-difference estimates, which eliminate any discrepancy between NICs and synthetic NICs potentially driven by unobserved heterogeneity or poor simulation quality, confirm that there is a clear pattern of negative independence dividends in the full sample, at least in the short- to medium run. In the long run, however, this clear pattern largely dissipates as a result of increasing cross-country heterogeneity in the estimated triple-differenced independence dividends.[25]

Figure 4: **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 (10), against the corresponding trend difference-in-difference (triangles), placebo difference-in-difference (diamonds) and triple-difference (squares) estimates related to equations (11), (12) and (13), along with their 95% confidence intervals. The number of years after secession is indicated on the horizontal axis.

3.5 Selected results

To put more empirical flesh on the bones, this section supplements the large-scale econometric analysis by highlighting the results pertaining to a number of historical instances of state fragmentation. More specifically, figure 5 characterizes the dynamics of 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 independence dividend trajectories, 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

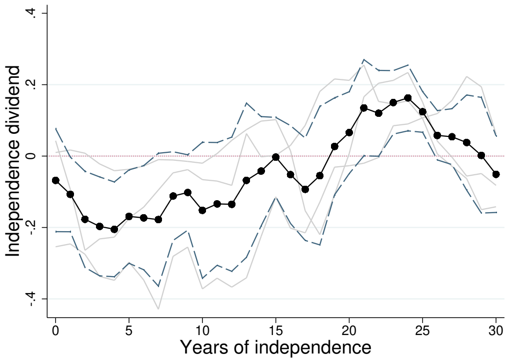
[25]Since these estimators ignore the potential presence of anticipation effects, they are likely to yield a lower bound for the true economic cost of state fragmentation.

better institutions (Acemoglu et al., 2001; Acemoglu & Robinson, 2009) and were more successful in educating their dependents (Grier, 1999). Interestingly, our results largely back this story and suggest that, in sharp contrast to NICs with other colonial heritages, former British colonies tended to experience a small independence gain in the medium run, amounting to around 10% in per capita GDP terms in the 10^{th} post-independence year. More surprisingly, although Belgian and Portuguese dominations are often considered the most detrimental and exploitative (Bertocchi & Canova, 2002), former French colonies appear to have suffered the most adverse economic consequences of colonial demise in the form of a persistent 20% reduction in per capita GDP. While - in the aggregate - Portuguese colonies seem largely unaffected by their decision to go it alone, former Belgian colonies and protectorates do appear to have suffered a short-run cost of independence but were able to revert this to a positive long-run independence dividend. The latter suggests that there may be economic gains associated with the elimination of colonial drain.

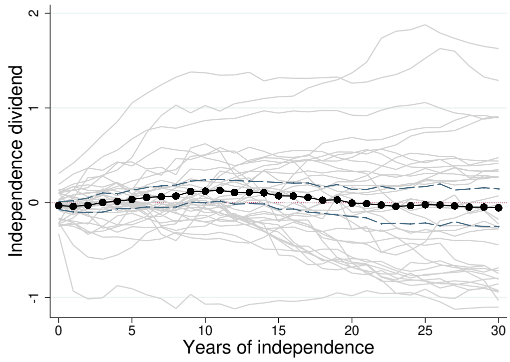
In the same vein, Roland (2002), Svejnar (2002) and Fidrmuc (2003) maintain that the extent of state capture and rent-seeking was more pervasive in the Soviet Union than in other Eastern and Central European countries and that these differential initial conditions, often proxied by the distance from 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 European membership, which enhanced incentives for patterns of law enforcement and protection of property rights in potential member states. Our results are testimony to this, as former Soviet members clearly suffered more adverse economic consequences of secession in comparison to Yugoslavian and Czechoslovakian successor states. More specifically, our results indicate that the group of former Soviet members suffered the most adverse and persistent effects of state breakup which, at their peak, amounted to a staggering 80% cut in per capita GDP and which only evaporated 20 years after independence. In comparison, the economic costs associated with the Czechoslovakian 'Velvet Divorce' were both more modest and much less persistent while the post-independence performance of the successor states that emerged from the demise of Yugoslavia, if anything, looks even more rosy.

All in all, we consider the fact that our estimation results do not considerably diverge from the prior qualitative findings in the existing literature as an additional indication of their reliability and validity.

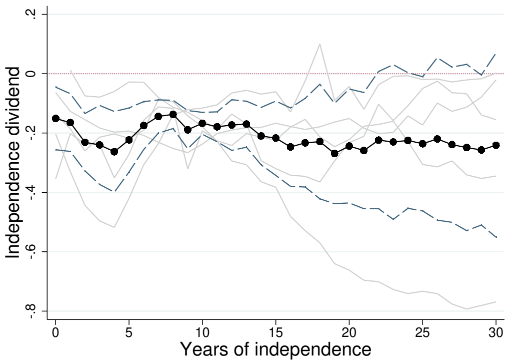
Figure 5: **Triple-difference estimates: historical instances of state fragmentation**



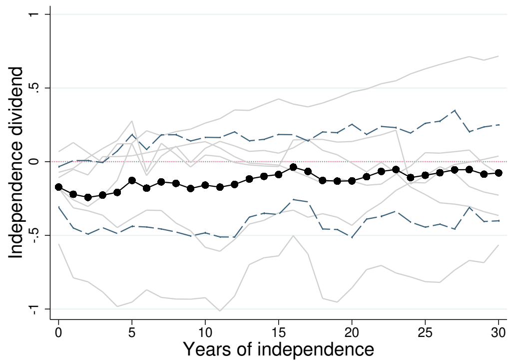
(a) Belgian colonial empire



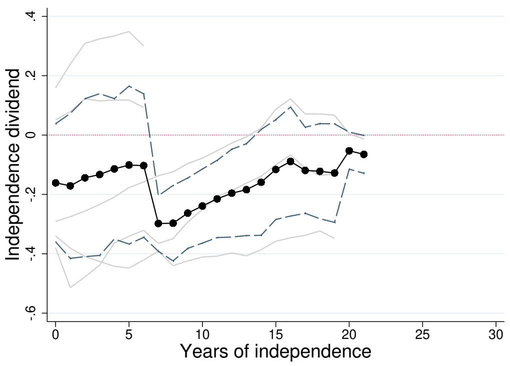
(b) British colonial empire



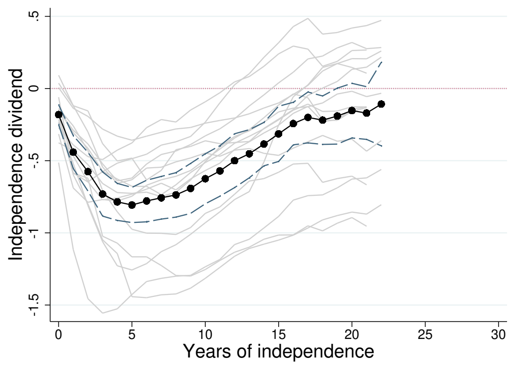
(c) French colonial empire



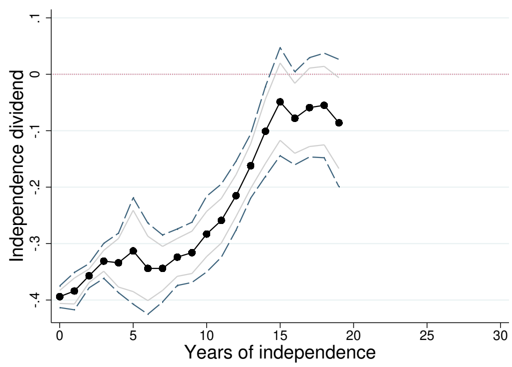
(d) Portuguese colonial empire



(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; the dashed lines depict the 90% bootstrapped confidence interval, clustered at the country level and based on 250 replications. The number of years before (-) or after (+) independence are indicated on the horizontal axis.

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. This extension builds on prior results to propose 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 and discuss some robustness checks.

4.1 Estimation strategy

To evaluate the various channels through which the decision to secede might affect per capita GDP trajectories in newly formed states, 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 a declaration of independence 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 state size (in square kilometers), the extent of surface area loss and trade openness; the impact of persistent 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 dummy variable indicating episodes of debt and/or banking crises.

In determining the relative importance of these potential determinants, one obvious difficulty is that the interpretation of the regression coefficients is sensitive to the scale of the inputs. Therefore, all continuous predictor variables are standardized to convert them to a common scale.[26,27] Another complication stems from the possibility that the independence dividend trajectories themselves, as well as their relation to their underlying determinants, may change over time.[28] To capture these dynamics, we include dummies for each post-independence year as well as their interaction with all other predictor variables. Finally, to take into account that global patterns in trade liberalization may have gradually reduced the economic cost of secession[10], in addition to region dummies, all specifications also include (calendar) year dummies to capture region as well as year fixed effects.

More specifically, denoting the estimated net gain of independence of NIC i located in

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

[27]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.

[28]Our prior results, for instance, suggest that the adverse effects of independence tend to erode over time.

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 model:

$$\hat{\beta}_{i,r,t,s} = \lambda X_{i,r,t,s} + \lambda_s \Big(X_{i,r,t,s} \times s \Big) + \eta_s + \delta_r + \mu_t + \epsilon_{i,r,t,s} \tag{14}$$

where $X_{i,r,t,s}$ denotes the $(1 \times X)$ vector of standardized predictors of the independence dividend; $\Big(X_{i,r,t,s} \times s \Big)$ denotes their interaction with the S years-of-independence dummy, which allows for a differential relation in each post-independence year; 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; and the error term, $\epsilon_{i,r,t,s}$, collects all random, transitory shocks to the independence dividend.

Note that, in the current set-up, the coefficients collected in λ and λ_s reflect the standard deviation elasticity of the independence dividend with respect to its predictors in the s^{th} post-independence year, such that larger coefficients are taken to identify more influential predictors. In this light, it makes sense to define the *relative importance* of each predictor $x \in X$ in a given post-independence year s, $\Delta_{x,s}$, as the expected percentage change in the independence dividend associated with its standard deviation increase:

$$\Delta_{x,s} = \frac{\partial \hat{\beta}_{i,r,t,s}}{\partial x} = \hat{\lambda}_x + \hat{\lambda}_{s,x} \tag{15}$$

where $\hat{\lambda}_x$ and $\hat{\lambda}_{s,x}$ refer to the parameter estimates associated with the x^{th} predictor in the vector of standardized predictors in equation 14. As noted by Gelman and Pardoe (2007), if one is willing to consider the X included predictors causally, $\Delta_{x,s}$ corresponds to an expected causal effect under a counterfactual assumption.

4.2 Baseline results

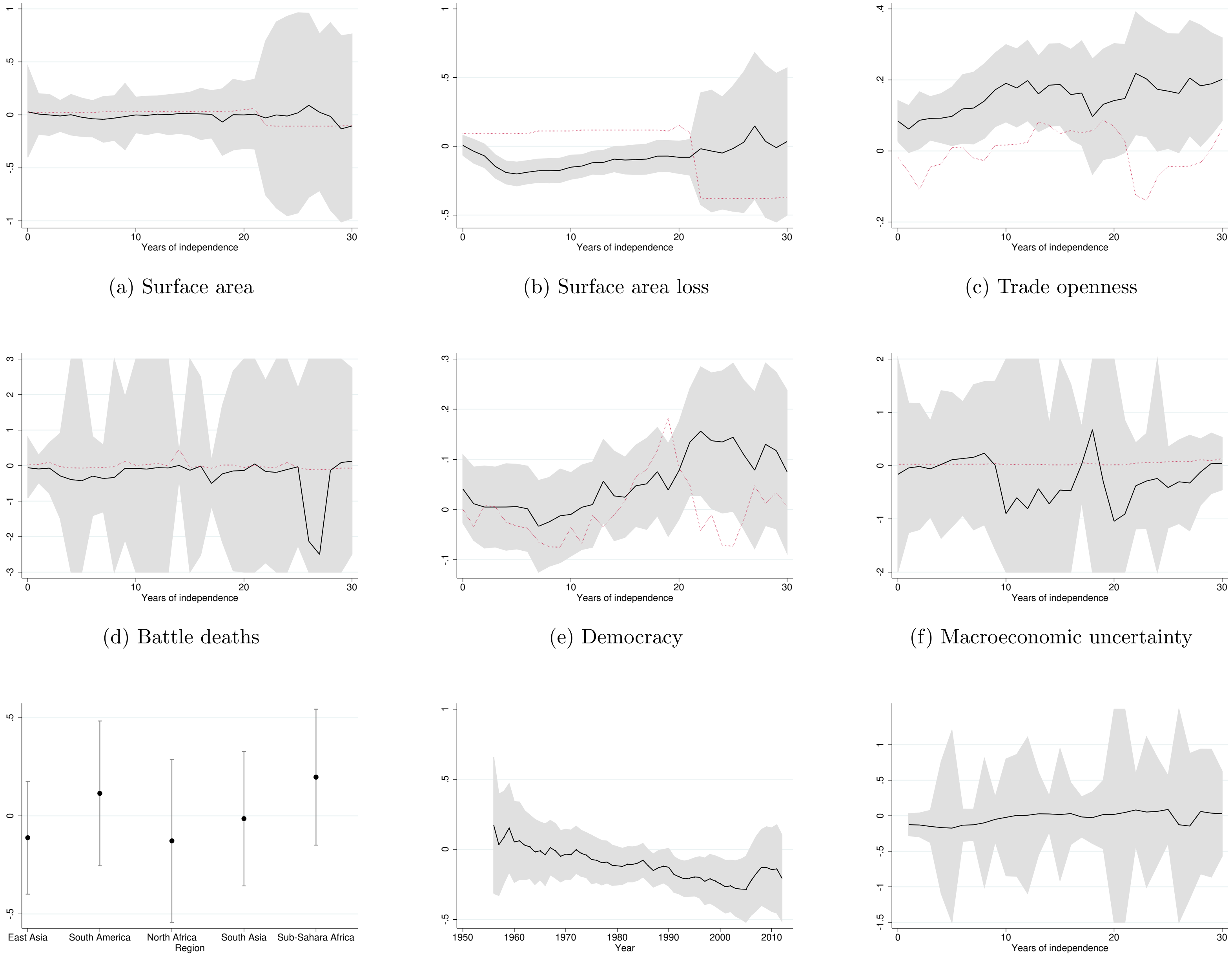
Figure 6, then, provides the results of an investigation of the economic channels influencing the most conservative triple-difference estimates of the independence dividend. More specifically, the black lines plot the evolution of the relative importance of these potential determinants along with their 90% confidence intervals, while the red dotted lines indicate the yearly average values of these standardized predictors actually observed in our sample.

Several explanations to account for the observed variation in the estimated net gains of secession are confirmed. First, our results indicate that the adverse effect of declaring independence is increasing in the extent of surface area loss, at least in the short to medium run. In line with the endogenous growth literature, these results are thus consistent with the hypothesis that separation from larger-sized entities entails more pronounced adverse effects due to a larger reduction in scale economies. We obtain positive estimates for the effect of trade openness throughout the period under consideration, corroborating previous theoretical findings which suggest that trade openness counteracts the adverse effects of decreased domestic market size. Note that, by this reasoning, the negative trade shock typically observed in the immediate post-independence period (indicated by the red line in figure 6c) is expected to aggravate the short-run effect of independence on economic performance and may co-explain the typical post-independence dip in per capita GDP also visible in figure 4. Finally, we fail to discern a meaningful effect of state size since smaller-sized NICs do not appear to be outperformed by their larger-sized counterparts.

Second, although the point estimates indicate that it is negatively related to the independence dividend, the effect of military conflict cannot be precisely estimated and, hence, the intensity of military violence does not seem meaningfully related to the magnitude of the independence dividend. More interesting is that our results are consistent with democratization being a second channel through which newly formed states can reduce the adverse effects of secession. Compared to the beneficial effect of opening up to trade, however, our results indicate that the impact of the democratization process is only visible in the longer run while its relative importance tends to be more modest. Note that the fact that NICs tend to regress to more authoritarian rule in the immediate post-independence period potentially contributes to more pronounced short-run independence costs. Finally, we also find tentative evidence that the adverse effects of declaring independence may be severely aggravated when these decisions are followed by instances of sovereign debt default or banking crises, although the coefficients tend to be imprecisely estimated.

It may be useful to compare these standard deviation elasticities with the region, year and years-of-independence fixed effects. Doing so, figure 6g is indicative of a small amount of regional heterogeneity in the economic impact of secession: Sub Saharan NICs appear to have benefited the most from their declaration of independence while North African NICs suffered the worst effects. In contrast to the existing literature, we find no evidence of global trade liberalization gradually lowering the economic costs of independence. Quite the contrary, our results indicate that declaring independence became *more* costly over time, reducing per capita independence dividends by approximately 20% in 2013 in comparison to 1980. Finally, the last figure shows that independence dividends, all else equal, tend to worsen in the short to medium run but do not erode in the longer run.

Figure 6: **Determinants of the triple-differenced independence dividend**



(a) Surface area
(b) Surface area loss
(c) Trade openness
(d) Battle deaths
(e) Democracy
(f) Macroeconomic uncertainty
(g) Region fixed effects
(h) Year fixed effects
(i) Years-of-independence fixed effects

Note: The top and the middle row plot yearly estimates of the relative importance, as defined in equation 15, of several determinants of the triple-differenced independence dividend (black line) against their sample-average values (red lines). 90% bootstrapped confidence intervals, clustered at the country level and based on 250 replications, are plotted in gray. For reference, the bottom row plots the region, year and years-of-independence fixed effects: region fixed effects are relative to Europe & Central Asia; year fixed effects are relative to 1980; years-of-independence fixed effects are relative to the year of independence.The number of years after secession is indicated on the horizontal axis.

4.3 Robustness checks

As a first robustness check, we consider the sensitivity of our estimates with respect to the specific first-step estimation procedure, by sequentially replacing the triple-differenced independence dividend with its raw, trend- and placebo-demeaned counterpart, see figures A10 through A12. Doing so, however, we obtain broadly similar results. Therefore, our prior conclusions hold irrespective of the first-step estimation procedure utilized to estimate the independence dividend.

In figure A13, we display the results of adding educational attainment, life expectancy and the level of per capita GDP as control variables. While the former serve to control for the potential effects of human capital differences between NICs, the latter addition allows us to verify whether the impact of secession differs in the degree of economic development. As can be seen, the effect of human capital remains somewhat elusive while we find tentative evidence that richer regions experience less pronounced independence costs. Nevertheless, adding these controls does little to affect our prior conclusions.

In a next step, we extend the original model and also include pre-independence RMSPE and the absolute value of the average contemporary placebo independence dividend to control for matching and simulation quality, see figure A14. Although we find some evidence that simulation inaccuracies cloud the short-run triple-difference estimates, when large inaccuracies in the placebo group coincide with more negative independence dividend estimates, it also turns out that our prior conclusions are not sensitive to explicitly controlling for simulation and matching quality in our estimation model.

Finally, notice that we can also estimate a more restrictive model that eliminates all the - potentially confounding - variation in time-invariant covariates. Figure A15, therefore, re-estimates the original model but now includes country instead of region fixed effects. Unsurprisingly, this manipulation causes the relative importance of our time-constant predictors to be less precisely estimated. However, the beneficial effect of increased trade openness and democratization remains visible in this model.

All in all, while the second-step results are less clear-cut than our first-step findings, they suggest that the post-independence per capita GDP dip observed in our sample is initially mainly driven by decreasing scale economies and trade flows and a short-term regression towards more authoritarian institutions, while the longer-run cost may be brought about by increased costs of macroeconomic uncertainty. Nevertheless, increased trade openness and ongoing processes of democratization appear to have bolstered growth potential in these newly formed states, mitigating - at least partially - independence costs. Although this strategy does not allow for a conclusive identification and ranking of all the channels influencing the independence payoff, due to the unresolved issues of omitted variable bias and endogeneity, the consistent patterns of variation in the independence payoffs certainly make these channels prime candidates for further research.

5 Conclusion

In tandem with the recent surge in secessionist tendencies, independence movements from all over the world 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 of its kind 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 2013.

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 heterogeneity, we present robust evidence that secession statistically significantly hampers growth potential in newly formed states. More specifically, our central results suggest that the decision to secede slashed per capita GDP in NICs anywhere between 20% and 46% in the long run. From a methodological perspective, to demonstrate the stability of the results, we develop a novel triple-difference procedure to provide informative statistical inference on the reliability of synthetic control estimates of treatment effects. Applying this procedure, we confirm the existence of a negative independence dividend in the short to medium run, with cross-country heterogeneity obscuring the average long-run impact of independence.

Another novelty is the construction of a two-step estimator to identify the primary channels through which secessionist processes have influenced per capita GDP trajectories in NICs, combining both parametric and semi-parametric techniques. In line with much of the existing literature, we find robust evidence the the adverse effects of independence increase in the extent of surface area loss, pointing to the presence of economies of scale, but that NICs can mitigate at least some of the adverse effects of declaring independence by opening up to trade and improving democratic institutions. Mirroring the lack of consensus in the existing literature, we fail to find clear-cut results with respect to the effect of state size while the effect of military conflict an macroeconomic uncertainty also remain elusive, leaving ample room for future research.

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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 while also, where relevant, reporting some diagnostics.

GDP per capita (baseline): 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 (2013). This series starts in 1950 and ends in 2010 and provides 8794 (70.1%) of our 12544 country years. Subsequently, we maximally extend these estimates forward to 2013 and backwards to 1960 using the growth rate of real per capita GDP provided by the World Bank (2015), thereby adding another 1956 (15.6%) country-year observations. Afterward, we remove 14 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 (2015), their squared and cubic values as well as all possible third-order interactions. We then use the growth rate of the predicted per capita GDP trajectories to maximally extend the baseline series forward and backwards, adding another 1107 (8.8%) country-year observations.[29] 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 0.86 for their 9840 common observations. That being said, with a correlation coefficient of 0.9, 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 58% 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 using information on log per capita CO2 emissions contained in World Bank (2015). The World Bank (2015) data on CO2 emissions runs from 1960-2013 and also shows a strong correlation with baseline log per capita GDP (0.86 for their 8227 common observations). Neverthe-

[29] 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.

less, the third-order polynomial predicted per capita GDP trajectories once again correlate even more strongly with their baseline counterparts, yielding a correlation coefficient of 0.9, while the predictive accuracy of this model attains 57%. Once again using the growth rate of predicted real per capita GDP to further extend the existing series forward and backwards adds another 56 (0.05%) observations. The remaining 644 (5.1%) country-year observations remain missing.[30]

GDP per capita (alternative): 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 (2013); Gibler and Miller (2014b); CLIO Infra (2015); Feenstra, Inklaar, and Timmer (2015); The Conference Board (2015); World Bank (2015).

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 11214 baseline and alternative per capita GDP estimates equals 0.988, 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: Data on the evolution of country-specific population size between 1950 and 2013 are obtained from Barro and Lee (1994); Heston et al. (1994); The Madison Project (2013); CLIO Infra (2015); Feenstra et al. (2015); United Nations Population Division (2015); World Bank (2015). Aggregation across datasets is obtained by applying the

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

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: 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); secondary education enrollment rates from Barro and Lee (1994); World Bank (2015); and enrollment in tertiary education from the World Bank (2015). In a second step, since most of these data are only reported five-yearly, 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 Barro and Lee (2012) data on average years of education is selected as baseline series. Covering the period 1950-2010, it provides 8723 (69.5%) country-year estimates for the average years of education. In a next step, we maximally extend these estimates forward to 2013 and backwards to 1980 using the growth rates implied in the average years of education data reported by United Nations Development Program (2015), adding another 849 (6.8%) 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 (0.8%) more country-years. 1352 (10.8%) 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 0.74 to 0.98. 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 56.6% to 71.4%.

Life expectancy: Data on life expectancy is obtained from Barro and Lee (1994); CLIO Infra (2015); World Bank (2015), 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 965 (7.7%) country-year observations missing.

Trade openness: Data on trade openness, defined as the value of imports and exports relative to GDP, are obtained from Heston et al. (1994); Feenstra et al. (2015); World Bank (2015). 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 9143 (72.9%) country-year observations. Subsequently, we maximally extend the existing data forward and backwards using the growth rates implied in the World Bank (2015) data for an additional 503 (4%) country-year observations. Finally, relying on the least squares third-order polynomial approximation procedure outlined above, we fill another 119 (0.9%) country-year observations based on the Heston et al. (1994) data. 2779 (22.2%) country-year observations remain missing.

Democracy: 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 (2014); Gibler and Miller (2014b); Vanhanen (2014); CLIO Infra (2015); Freedom House (2015); Skaaning, Gerring, and Bartusevičius (2015).[31] After linearly interpolating missing observations in each data set, as it is the most extensive data source, we consider Vanhanen (2014) as our baseline series. Vanhanen's (2014) continuous measure of democracy, which is based on a country's degree of political competition and political participation, provides us with 9240 (73.7%) 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, 2807 (22.4%) 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 0.78 to 0.98, serves to motivate this approach. In addition, the high predictive accuracy of our various third-order polynomial models, ranging from 59.9% to 87.7%, provides further evidence that these alternative democracy indexes provide useful information to assess missing values in the baseline series.

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

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

Variable	*Data source*	*Description*	*% Obs. [% Int.]*	*r / $\hat{r}$*	*Accuracy*
GDP per capita*** (baseline)	**The Madison Project (2013)**	GDP per capita (1990 int. GK $)	70.11 [.]	1 / .	.
	World Bank (2015)	GDP per capita (constant 2005 $)	15.59 [.]	0.91 / .	.
	World Resources Institute (2015)	Total CO2 emissions (Metric Tons)	8.82 [.]	0.86 / 0.90	58.00
	World Bank (2015)	Per capita CO2 emissions (Metric Tons)	0.45 [.]	0.86 / 0.90	57.15
	n.a.	missing	5.13 [.]	. / .	.
GDP per capita** (alternative)	**The Madison Project (2013)**	GDP per capita (1990 int. GK $)	72.05 [1.95]	1 / .	.
	CLIO Infra (2015)	GDP per capita (1990 int. GK $)	0.02 [0.00]	1 / .	.
	The Conference Board (2015)	GDP per capita (1990 int. GK $)	6.42 [0.00]	0.99 / .	.
	Barro and Lee (1994)	GDP per capita (1985 int. prices)	1.28 [0.96]	0.98 / .	.
	Heston et al. (1994)	Real GDP per capita	0.31 [0.01]	0.96 / .	.
	World Bank (2015)	GDP per capita (constant 2005 $)	8.46 [0.00]	0.91 / .	.
	Feenstra et al. (2015)	GDP per capita (chained PPPs, 2005$)	0.58 [0.00]	0.90 / .	.
	Gibler and Miller (2014b)	Real GDP per capita (1985 $)	0.43 [0.00]	0.77 / .	.
	n.a.	missing	10.47 [.]	. /.	.
Population**	**CLIO Infra (2015)**	Total population	90.94 [67.08]	1 / .	.
	Heston et al. (1994)	Total population	0.47 [0.00]	1 / .	.
	Feenstra et al. (2015)	Total population	2.12 [0.00]	1 / .	.
	Barro and Lee (1994)	Total population	0.08 [0.06]	1 / .	.
	World Bank (2015)	Total population	4.21 [0.00]	1 / .	.
	The Madison Project (2013)	Total population	0.40 [0.00]	1 / .	.
	United Nations Population Division (2015)	Total population	0.16 [0.00]	1 / .	.
	n.a.	missing	1.63 [.]	. / .	.
Education***	**Barro and Lee (2012)**	Average years of education	69.54 [54.72]	1 / .	.
	United Nations Development Program (2015)	Average years of education	6.77 [0.61]	0.98 / .	.
	Barro and Lee (1994)	Average years of education	0.89 [0.74]	0.96 / 0.98	71.39
	CLIO Infra (2015)	Average years of education	4.75 [4.24]	0.95 / 0.97	65.19
	World Bank (2015)	Secondary enrollment rate	6.18 [2.54]	0.88 / 0.93	57.59
	Barro and Lee (1994)	Secondary enrollment rate	0.27 [0.19]	0.87 / 0.92	65.14
	World Bank (2015)	Tertiary enrollment rate	0.53 [0.26]	0.74 / 0.87	56.65
	Linearly interpolated	.	0.82 [0.82]	. / .	.
	n.a.	missing	10.78 [.]	. / .	.
Health*	**CLIO Infra (2015)**	Life expectancy	84.50 [0.65]	1 / .	.
	World Bank (2015)	Life expectancy	77.57 [0.03]	0.99 / .	.
	Barro and Lee (1994)	Life expectancy	23.48 [18.06]	0.97 / .	.
	n.a.	missing	7.35 [.]	. / .	.
Trade Openness***	**Feenstra et al. (2015)**	(imports + exports)/GDP	72.89 [0.00]	1 / .	.
	World Bank (2015)	(imports + exports)/GDP	4.01 [0.00]	0.84 / .	.
	Heston et al. (1994)	(imports + exports)/GDP	0.95 [0.00]	0.70 / 0.83	67.98
	n.a.	missing	22.15 [.]	. / .	.
Democracy***	**Vanhanen (2014)**	Vanhanen Index of Democracy	73.66 [0.15]	1 / .	.
	CLIO Infra (2015)	Vanhanen Index of Democracy	1.27 [0.00]	0.97 / 0.98	87.73
	Gibler and Miller (2014b)	Combined Polity2 Index	0.81 [0.00]	0.90 / 0.94	68.38
	Melton et al. (2010)	Unified Democracy Scores	0.29 [0.16]	0.89 / 0.93	66.43
	Giuliano et al. (2013)	Freedom House Index	0.33 [0.18]	0.81 / 0.92	65.62
	Freedom House (2015)	Freedom House Index	1.07 [0.49]	0.82 / 0.91	59.91
	Skaaning et al. (2015)	Lexical Index of Electoral Democracy	0.10 [0.00]	0.79 / 0.90	66.08
	Center for Systemic Peace (2014)	Revised Combined Polity Score	0.02 [0.00]	0.78 / 0.89	97.66
	n.a.	missing	22.38 [.]	. / .	.

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 Parametric estimation of the independence dividend

In this appendix, we follow a parametric approach to identify the independence dividend. After outlining the general economic model, a next subsection explains the model selection procedure. Subsequently, we present our baseline results and perform some additional robustness checks. Finally, a last subsection provides instrumental variable estimates of the independence dividend.

B.1 Estimation strategy

Based on the literature review, see section 1, we are interested in estimating the following underlying economic model:

$$y_{i,t} = \beta_0 + \beta_1 NIC_{i,t} + \sum_{p=1}^{P} \beta_{1+p} E_t[NIC_{i,t+p}] + \sum_{q=1}^{Q} \alpha_q y_{i,t-q} + \theta X_{i,t} + \lambda Z_{i,t} + \delta_i + \eta_t + \epsilon_{i,t} \quad (16)$$

where $y_{i,t}$ is the log of per capita GDP of country i at time t, $NIC_{i,t}$ is an independence dummy equal to 1 for each NIC in the first 30 years after it gained independence and 0 otherwise, $X_{i,t}$ is a $(1 \times X)$ vector of observed controls, Z_{it} is a $(1 \times Z)$ vector of time-varying unobserved growth determinants, δ_i captures the I country fixed effects, η_t denotes T year fixed effects and the error term, $\epsilon_{i,t}$, collects all random transitory shocks to per capita GDP. The model also includes up to Q lags of the dependent variable to allow for persistency in GDP dynamics. E_t denotes the expectation taken with respect to the information set at time t, reflecting the potential presence of anticipation effects up to P years prior to a declaration of independence.[32] The coefficient of interest is β_1, as this is the coefficient that will capture the economic impact of secession: a statistically significant and positive estimate would indicate the presence of independence gains while a statistically significant negative estimate would signify an independence cost.

As is standard, we deal with the potential problem of time-constant omitted variable bias by removing time-constant unobserved heterogeneity, which may include hard-to-quantify variables such as cultural norms or political institutions, using the standard within estimator. To eliminate common per capita GDP trends that may be correlated with declaring independence, year dummies are included in each specification as well.

To deal with the potential presence of finite anticipation effects, we estimate a quasi-myopic model replacing the expectation of independence p years prior to the actual independence declaration, $E_t[NIC_{i,t+p}]$, with leads of the independence-dummy, $NIC^p_{i,t}$. More specifically, $NIC^p_{i,t}$ is a dummy variable equal to 1 if country i will declare independence at time $t+p$, such that the *ex ante* effect of secession at time $t+p$ is estimated by β_{1+p}.

[32] As discussed in the introduction, the pre-secession dip in per capita GDP may reflect anticipation effects. If so, Malani and Reif (2015) show that their omission generates omitted variables bias.

Denoting the within transformation of y_{it} by $\tilde{y_{i,t}} = y_{i,t} - \frac{1}{T_i}\sum_{t=1}^{T_i} y_{i,t}$, our baseline linear regression specification related to equation (16) can be summarized as follows:

$$\tilde{y}_{i,t} = \beta_1 N\tilde{I}C_{i,t} + \sum_{p=1}^{P} \beta_{1+p} N\tilde{I}C^{p}_{i,t} + \sum_{q=1}^{Q} \alpha_q \tilde{y}_{i,t-q} + \theta \tilde{X}_{i,t} + \eta_t + \tilde{\epsilon}^{Z}_{i,t} \tag{17}$$

where the error term, $\tilde{\epsilon}^{Z}_{i,t} = \lambda \tilde{Z}_{i,t} + \tilde{\epsilon}_{i,t}$, also subsumes variation over time in the z unobserved growth determinants and the contents of $\mathbf{X}$ are detailed below.

In order for this relation between per capita GDP and the independence dummy to be dynamically stable, the sum of the coefficients of the lagged dependent variables ($\sum_{q=1}^{Q} \alpha_q$) measuring the persistence in per capita GDP should be smaller than one. If this would not be the case, a one-time decision to secede would cause explosive changes in per capita GDP trajectories. To check whether explosive behavior can be ruled out, an appropriate F-test can be employed (Acemoglu, Suresh, Restrepo, & Robinson, 2014). This is a relevant issue, as the β_1-coefficient in the regression model summarized in equation (17) only estimates the immediate economic impact of secession or, equivalently, the effect of declaring independence if independence is expected to last for one single period. Computing the effect of a permanent secession, β, involves accounting for the serially correlated nature of per capita GDP which, under dynamic stability, boils down to:

$$\hat{\beta} = \frac{\hat{\beta}_1}{1 - \sum_{q=1}^{Q} \hat{\alpha_q}} \tag{18}$$

Finally, a well-known problem with the model specified in equation (17) lies in the so-called Nickell (1981) bias, which is inherent to the estimation of auto-regressive fixed effects models and arises due to the mechanical correlation of the lagged dependent variables and the error term. The fact that this bias declines in the length of the time series (T), however, suggests that dynamic panel bias should be negligible given the long time-dimension of our dataset.[33] Nevertheless, to account for dynamic panel bias, we also consider two alternative estimators which are unbiased for finite T. First, we use a one-step generalized methods of moment (GMM) estimator for dynamic panel models that was introduced by Arellano and Bond (1991) and further developed by Arellano and Bover (1995). Exploiting the absence of residual auto-correlation, country fixed effects are first removed by applying the forward orthogonal deviations transformation to all regressors while dynamic panel bias is mitigated by subsequently instrumenting the transformed lagged dependent variables using suitable lagged levels of y_{it}.[34] In order to avoid an over-fitting bias (Roodman, 2006) the number of lagged instruments is restricted to a maximum of 5. In addition, we consider the iterative bootstrap-based bias correction procedure for the fixed effects estimator in dynamic panels proposed by Everaert and Pozzi (2007).[35]

[33] In the model lacking additional controls, each country is observed 57 times on average.
[34] The GMM estimator is implemented by Roodman's (2006) *xtabond2*-command in Stata13.1.
[35] The bias correction is implemented by De Vos and Ruyssen's (2015) *xtbcfe*-command in Stata13.1.

B.2 Model selection

The inclusion of lagged dependent variables and anticipation dummies in equation (17) serves to mitigate endogeneity and omitted variable bias concerns, but requires us to determine of the 'optimal' number of lagged dependent variables, Q^*, as well as the 'optimal' number of anticipation dummies, P^*. To do so, we build on Stock and Watson (2012) who suggest two approaches to choosing the optimal order of an auto-regressive model: through a statistical significance criterion or using an information criterion. This first approach starts with a model including many lags and subsequently sequentially checks whether the coefficient on the last lag (or trailing lag) significantly differs from 0. If not, the last lag is dropped and the procedure is repeated until the 'optimal' number of lags is determined. By a similar procedure, in the second approach, the number of lags to be included is selected according to the model minimizing the AIC criterion. As this second approach penalizes model complexity, in contrast to the first approach, it is expected to yield a more parsimonious model, reducing estimation uncertainty but potentially omitting valuable information contained in more distant lags.

Since we need to determine both Q^* and P^*, we consider the statistical significance and the information criterion approach in two different sequential scenarios. In the first sequential scenario, $Q^* \rightarrow P^*$, we start from the bivariate fixed-effects model to determine the optimal number of lagged dependent variables (Q^*) first and only after this decide on the number of anticipation terms (P^*) to be included. In the second sequential scenario, $P^* \rightarrow Q^*$, the order is reversed and we first decide on P^* and subsequently determine Q^*.

To choose the optimal number of terms in either sequential scenario, we will consider two trails: in the first, which we will refer to as the *long* trail, we start with a 'long' model including 10 sequential terms and subsequently sequentially shorten the model at the last term - the trailing lag or the trailing anticipation term - whenever our model selection criterion dictates so. In the second, so-called *short*, trail we start with our fixed effects bivariate model and subsequently rely on either model selection criterion to determine whether a more extensive model specification is to be preferred.

Finally, to select the optimal specification resulting from this exercise, based on the principle of parsimony, we adopt the most concise candidate model (i.e. the model which minimizes $P^* + Q^*$) lacking first-order auto-correlation in the residuals.[36] Although deciding on the number of lags and anticipation terms to include is always and by necessity somewhat arbitrary, this approach at least has the added benefit of providing some empirical evidence suggesting a well specified estimation model.

The results of this exercise are reported in table A2. As it turns out, including 4 lags of the dependent variable sufficiently accounts for past GDP dynamics while relevant anticipation effects are limited to the first 2 pre-secession years.

[36] The intuition behind this approach is that residual auto-correlation in a dynamic model *"often indicates that the functional form has not been completely specified"* (Wooldridge, 2009, p. 412).

Table A2: **Quasi-myopic, autoregressive models of per capita GDP**

Model selection criterion	*Sequence*	*Trail*	*Result*	*AR(1)*
Statistical significance (t-test)	$Q^* \to P^*$	*short*	$Q^* = 4; P^* = 2$	.21
Statistical significance (t-test)	$Q^* \to P^*$	*long*	$Q^* = 8; P^* = 2$	.01
Statistical significance (t-test)	$P^* \to Q^*$	*short*	$Q^* = 4; P^* = 10$	.21
Statistical significance (t-test)	$P^* \to Q^*$	*long*	$Q^* = 4; P^* = 10$	.21
Information criterion (AIC)	$Q^* \to P^*$	*short*	$Q^* = 1; P^* = 2$	.00
Information criterion (AIC)	$Q^* \to P^*$	*long*	$Q^* = 1; P^* = 9$	.00
Information criterion (AIC)	$P^* \to Q^*$	*short*	$Q^* = 1; P^* = 10$	.00
Information criterion (AIC)	$P^* \to Q^*$	*long*	$Q^* = 1; P^* = 10$	.00

Note: All estimations include the independence dummy as well as both country and year dummies. The first three columns specify the model selection procedure. Column 4 indicates the resulting candidate model. AR(1) reports the p-value of the Wooldridge (2009) test for the absence of first-order auto-correlation in the residuals of the candidate model. Standard errors are robust for heteroskedasticity and clustered at the country-level.

B.3 Baseline results

The most straightforward situation to derive a reliable parametric estimate of the economic impact of secession would occur when, conditional on past GDP dynamics, anticipation effects as well as country and year fixed effects, the decision to secede and its expectation are entirely unrelated to any other past or present growth determinant while, at the same time, dynamic panel bias can be safely ignored due to the time-span of our dataset. To formalize these ideas, assume that the following conditions are satisfied

Condition A1

$$\mathbb{E}\left[\tilde{NIC}_{i,t}\tilde{\epsilon}^{ZX}_{i,s}\right] = 0 \quad \forall\ t \geq s$$
$$\mathbb{E}\left[\tilde{NIC}^1_{i,t}\tilde{\epsilon}^{ZX}_{i,s}\right] = 0 \quad \forall\ t \geq s$$
$$\mathbb{E}\left[\tilde{NIC}^2_{i,t}\tilde{\epsilon}^{ZX}_{i,s}\right] = 0 \quad \forall\ t \geq s$$

Condition A2

$$\mathbb{E}\left[\tilde{y}_{i,t-q}\tilde{\epsilon}^{ZX}_{i,s}\right] = 0 \quad \forall\ t, s\ \&\ \forall\ q \in \{1, \ldots, Q^*\}$$

where it is understood that $\tilde{\epsilon_{i,t}}^{ZX} = \theta\tilde{X}_{i,t} + \lambda\tilde{Z}_{i,t} + \tilde{\epsilon}_{i,t}$.

Although, at first sight, these might seem rather strong assumptions, Wooldridge (2009) emphasizes that including lagged dependent variables provides a useful, though crude, strategy to control for more general omitted variable bias.

In this case, the standard within regression of the model outlined in equation (17) lacking any additional controls, in effect leaving the **X**-matrix empty, would provide us with a reliable estimate of the β_1-coefficient. Column (1) in the top panel of table A3 reports the primary results of such an exercise while the full results are relegated to table A7. Similar to all the other results we present in this section, per capita GDP trajectories turn out to be highly persistent (in this case, $\sum_{q=1}^{4} \alpha_q = 0.974$). Nevertheless, an F-test clearly rejects the null hypothesis of dynamic instability. More importantly, the estimated

β_1-coefficient is significantly negative and implies that per capita GDP is reduced by approximately 0.4% in the year of independence. The serially correlated nature in per capita GDP trajectories, however, implies that this adverse effect will accumulate in the ensuing years and converge to a long-run payoff of -0.16. In other words, on average, a declaration of independence significantly reduces per capita GDP levels by an estimated 16% in the long run. In addition, the results also indicate the presence of significantly negative *ex ante* effects associated with a future state break-up, lowering per capita GDP by an estimated 1.6% and 2.4% in the last two years prior to independence. Anticipation effects thus appear to partially explain the pre-secession growth dip typically observed in future NICs.

Nevertheless, the literature review contains several indications that condition A1 may not hold in practice. One complication might arise since NICs, upon gaining independence, tend to liberalize their trade regime to compensate for reduced market size. Secessions also appear to be associated with increased pressures to democratize, accelerating the transition towards more democratic political institutions. Our analysis has also abstracted from political and economic relations among countries. This might be problematic, as Alesina et al. (2000) argue that economic integration induces political disintegration. To control for the economic significance of evolutions in these, and other, alternative growth determinants we also consider a specification including several control variables. More specifically, we consider a compact control vector, $\mathbf{X}^{\mathbf{compact}}$, capturing contemporary evolutions in democracy, trade openness, military conflict, human capital, surface area, population differentials and political as well as macroeconomic instability. In addition, we include dummy variables reflecting (historical) membership of the EU, the OECD, the NATO, ASEAN, MERCOSUR, the AU and the group of oil exporting countries.

By taking these covariates out of the error term, it now becomes more likely that condition A1 holds with respect to the decision to secede and its prior anticipation. This, however, comes at the cost of having to impose similar exogeneity assumptions on each additional control (Wooldridge, 2009). We can express this idea formally by rewriting condition A1 such that we now require:

Condition A1a

$$
\begin{aligned}
&\mathbb{E}\left[N\tilde{I}C_{i,t}\tilde{\epsilon}^{Z}_{i,s}\right] = 0 && \forall\ t \geq s \\
&\mathbb{E}\left[N\tilde{I}C^{1}_{i,t}\tilde{\epsilon}^{Z}_{i,s}\right] = 0 && \forall\ t \geq s \\
&\mathbb{E}\left[N\tilde{I}C^{2}_{i,t}\tilde{\epsilon}^{Z}_{i,s}\right] = 0 && \forall\ t \geq s \\
&\mathbb{E}\left[\tilde{x}_{i,t}\tilde{\epsilon}^{Z}_{i,s}\right] = 0 && \forall\ t \geq s\ \&\ \forall\ x\ \in\ \mathbf{X}^{\mathbf{compact}}
\end{aligned}
$$

where the error term, $\tilde{\epsilon}^{Z}_{i,t} = \lambda\tilde{Z}_{i,t} + \tilde{\epsilon}_{i,t}$, now contains less information.

The results of this compact control specification are reported in column (2) of the top panel of table A3. Table A7 demonstrates that control variables tend to have the expected sign and that most of them are statistically significant. With regard to the coefficients of interest, although a lot of observations are lost due to the inclusion of these alternative growth determinants, the overall pattern is fairly similar. The results do suggest a slightly larger short-run cost of secession, which is now estimated to cut per capita GDP by 0.5% in the first post-independence year, and a more adverse anticipation effect, which is limited to the first pre-independence year and which reduces GDP per capita by 3.9% in the run-up to state break-up. A slightly smaller persistence in per capita GDP implies that the long-run, per capita cost of secession now roughly amounts to 12.4%, somewhat smaller but still broadly similar to our previous estimate.

Finally, to check whether the implosion of multi-ethnic states is more costly in economic terms we also include interactions of the independence dummy with a dummy indicating ex-Soviet states as well as a dummy identifying former members of Yugoslavia. Finally, Qvortrup (2014) suggests that declaring independence by referendum is particularly conducive to peaceful political settlements such that successful independence referendums potentially mitigate at least some of the economic costs of secession. To take these procedural aspects into account as well, we add the interaction between the independence dummy and a dummy indicating the occurrence of a successful independence referendum prior to the official declaration of independence.

The primary results stemming from the inclusion of this more extensive vector of control variables are reported in the last upper column of table A3. As can be seen, controlling for these additional growth determinants does not qualitatively affect the results although, compared to the baseline results, the long-run negative payoff of state fragmentation is now estimated to be somewhat larger, reducing per capita GDP by roughly 16% in the long run. We find some evidence that the implosion of multi-ethnic states is more adverse in economic terms, as the estimated economic cost associated with of the break-up of the Soviet union severely exceeds its sample-average counterpart, although former Yugoslavian countries do not appear to have suffered disproportionally from the implosion of Yugoslavia. Finally, declaring independence by referendum does seem to mitigate these reported secession costs as the coefficient associated with the interaction of the referendum- and the independence-dummy is statistically significant and positive.

Recall that the standard within estimator ignores dynamic panel bias by imposing condition A2. As mentioned, this might be reasonable given the long time-span of our data set. To verify whether this effectively is the case, we repeat the previous exercise using the orthogonal deviations GMM estimator instead, which is employed here to purge dynamic panel bias. The corresponding estimates are reported in columns (1) to (3) in the middle panel of table A3. Reassuringly, overall, the GMM estimates for all the coefficients involved correspond well with the associated within estimates, confirming the intuition that our prior results were not driven by dynamic panel bias. Correspondingly, the estimated payoffs of state fragmentation in both the pre- and the post-independence period turn out to be fairly similar and suggest a significant long-term effect of a 9.8% to 15.3% decrease in per capita GDP due the decision to secede. Interestingly, we find confirmation that ex-Soviet members suffered disproportionally from the breaking up of their mother country and that independence-by-referendum seems preferable from an economic point of view. Importantly, we test the absence of second order auto-correlation in the differenced error terms, which underpins the GMM estimator, and fail to find evidence that this exclusion restriction is not met.

Alternatively, Everaert and Pozzi (2007) develop a simulation-based approach to estimate and remove dynamic panel bias through an iterative bootstrap-procedure. By analogy, the results of applying this estimator instead are reported in columns (1) to (3) in the bottom panel. Once again, bias-corrected estimates closely track their corresponding within estimates. That being said, a slightly larger estimated persistence in per capita GDP trajectories leads to a somewhat more pronounced estimated long-run secession cost, which now hovers around 19% to 32% in per capita terms. Finally, the bootstrap-based bias correction model does indicate that declarations of independence that are preceded by a succesful independence referendum tend to do better while no confirmation is found that former Soviet or Yugoslav states faced above-average costs of state break-up.

Table A3: **Parametric estimates of the economic impact of secession**

		Within estimates	
Independence dummy	-0.004**	-0.005**	-0.007**
	(0.002)	(0.003)	(0.003)
Ex ante effect (t - 1)	-0.024***	-0.039***	-0.038***
	(0.008)	(0.013)	(0.014)
Ex ante effect (t - 2)	-0.016***	-0.007	-0.006
	(0.005)	(0.010)	(0.011)
Independence dummy × Soviet dummy			-0.024**
			(0.011)
Independence dummy × Yugoslav dummy			0.009
			(0.019)
Independence dummy × referendum dummy			0.014***
			(0.003)
Observations [# countries]	11,128 [192]	8,318 [187]	8,318 [187]
Adjusted R-squared	0.980	0.978	0.978
Country + Year FE	yes	yes	yes
Control vector	none	$\mathbf{X}^{\mathbf{compact}}$	$\mathbf{X}^{\mathbf{extensive}}$
$\sum_{q=1}^{4} \alpha_q$ [F-test <1]	.974 [0]	.956 [0]	.956 [0]
Long-run effect [p-value]	-.161 [.037]	-.124 [.038]	-.16 [.013]
		Deviations GMM estimates	
Independence dummy	-0.006**	-0.006**	-0.007**
	(0.003)	(0.003)	(0.003)
Ex ante effect (t - 1)	-0.020**	-0.039***	-0.038***
	(0.009)	(0.013)	(0.013)
Ex ante effect (t - 2)	-0.011**	-0.006	-0.005
	(0.006)	(0.010)	(0.011)
Independence dummy × Soviet dummy			-0.022*
			(0.011)
Independence dummy × Yugoslav dummy			0.009
			(0.018)
Independence dummy × referendum dummy			0.014***
			(0.004)
Observations [# countries]	10,935 [192]	8,131 [187]	8,131 [187]
Country + Year FE	yes	yes	yes
Control vector	none	$\mathbf{X}^{\mathbf{compact}}$	$\mathbf{X}^{\mathbf{extensive}}$
$\sum_{q=1}^{4} \alpha_q$ [F-test <1]	.934 [0]	.951 [0]	.952 [0]
p-value AR2	.213	.265	.298
Long-run effect [p-value]	-.098 [.044]	-.118 [.038]	-.153 [.017]
		Bootstrap-based bias-corrected estimates	
Independence dummy	-0.004*	-0.006**	-0.007***
	(0.002)	(0.002)	(0.002)
Ex ante effect (t - 1)	-0.025**	-0.040***	-0.039***
	(0.010)	(0.010)	(0.011)
Ex ante effect (t - 2)	-0.017***	-0.006	-0.005
	(0.005)	(0.011)	(0.011)
Independence dummy × Soviet dummy			-0.020
			(0.016)
Independence dummy × Yugoslav dummy			0.008
			(0.032)
Independence dummy × Referendum dummy			0.015***
			(0.004)
Observations	11,111 [192]	8,318 [187]	8,318 [187]
Country + Year FE	yes	yes	yes
Control vector	none	$\mathbf{X}^{\mathbf{compact}}$	$\mathbf{X}^{\mathbf{extensive}}$
$\sum_{q=1}^{4} \alpha_q$ [F-test <1]	.988 [.001]	.971 [0]	.972 [0]
Long-run effect [p-value]	-.32 [.117]	-.192 [.019]	-.256 [.004]

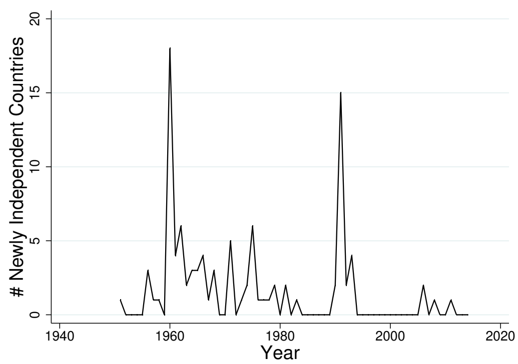
Note: The table presents parametric estimates of the effect of declaring independence on log per capita GDP. The top panel uses the within estimator; the middle panel uses the orthogonal deviations GMM estimator, which treats all lagged dependent variables as endogenous and all other variables as predetermined. The bottom panel uses the bootstrap-based bias-correction estimator implemented with i.i.d resampling, analytical heterogeneous initiation and bootstrapped standard errors based on 50 iterations. F-test reports the p-value of the persistence in GDP being smaller than 1; AR(2) reports the p-value of the Arellano and Bond (1991) test for second-order auto-correlation in first differences. The estimated long-run economic impact of secession, as well as the p-value for this being different from 0, is reported in the bottom row. Standard errors of the long-run effect are computed through the delta-method, see Papke and Wooldridge (2005). The full results are reported in table A7.
*** p<0.01, ** p<0.05, * p<0.1.

B.4 Robustness check: instrumental variable estimates

One major drawback of the previous estimators lies in the fact that they critically hinge on the strict exogeneity assumption imposed on the independence dummy and its anticipation effects, see conditions A1 and A1a. Although we relaxed this assumption by including several alternative growth predictors as controls, it is still possible for the decision to secede to be related to any number of past or present unobserved growth determinants, z. For example, increasing inter-regional income differentials in the mother country may both depress future growth and stimulate demands for autonomy.[37] To more extensively deal with the concern of unobserved local economic conditions co-determining both incentives to secede as well as growth potential, we conclude this parametric route by developing an instrumental variable (IV) approach.

To do so, recall that secessions historically have tended to occur in waves (Spencer, 1998; Fazal & Griffiths, 2008; Dahl et al., 2013; Qvortrup, 2014). This observation is visualized in figure A1, which plots world wide state entry for each year between 1950 and 2013: the 1960-peak coincides with the African decolonization process; the elevated level of state entry between 1970 and 1980 reflects several South American countries gaining independence; the peak around 1990 captures the dissolution of both the Soviet Union and Yugoslavia. Following similar arguments as those related to the spatio-temporal clustering of global democratization waves (Acemoglu et al., 2014), we argue that, while it is still unclear why state fragmentation has a tendency to occur in waves, these waves appear unrelated to contemporary economic trends. Rather, they seem to reflect political demands for self-determination and democracy. To the extent that this is the case, global waves of secession provide an attractive source of exogenous variation in local incentives to secede.

Figure A1: **Global waves of secession**



Note: The figure plots net state entry in the world in the period 1950-2013.

[37] See Alesina and Rodrik (1994) and Persson and Tabellini (1994) for evidence on income inequality being a drag on economic growth. For a theoretical explanation as to why inter-regional income inequalities may trigger secessionist tendencies, see Bolton and Roland (1997).

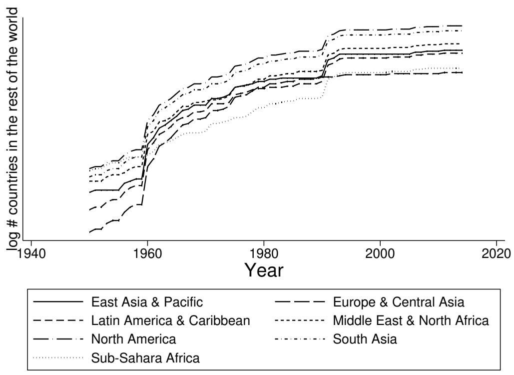
Therefore, to instrument the independence dummy and its anticipation effects, we construct an index of global, non-regional secession waves for each of the seven regions distinguished by the World Bank (2015). More specifically, this index collects the natural log of the total number of countries in the rest of the world, excluding countries in the own region. Formally, denoting the relevant regions by $r \in \{1, \ldots, 7\}$, we define our index of non-regional secession waves, $I_{i,t}$, for country i in region r at time t as

$$I_{i,t} = ln\left(\sum_{j \notin r} 1_{j,t}\right) \tag{19}$$

where $1_{j,t}$ is an indicator function equal to 1 if country j is independent at time t.

Figure A2 plots the values of this index for the period 1950-2013. Irrespective of geographical location, there are clear temporary upward trends in the values of the index reflecting that global waves of secession were tightly clustered in time during this period. For example, the total number of countries in the rest of the world drastically increased for each region around 1960, except for Sub-Sahara Africa where a lot of these NICs were located. Similarly, the break-up of the Soviet Union and Yugoslavia caused an increase in the value of the index for each region except Europe & Central Asia.

Figure A2: **Non-regional waves of secession**



Note: The figure plots non-regional state entry for the seven regions distinguished by the World Bank (2015).

We contend that this index provides us with both strong and valid instruments for the independence dummy and its anticipation terms in the first-stage regressions. First, given the time-clustered nature of state entry at the global level, this index should capture global political opportunities of independence and reflect, among other factors, the global political willingness to accept the right to self-determination. This, in turn, is expected to co-determine local aspirations of independence, implying that the index contains useful information to gauge local incentives to secede. Our first-stage results, reported below, provide further empirical evidence in this regard. On the other hand, we maintain that there is no *a priori* theoretical reason why the (evolution in the) total number of countries in the rest of the world should influence local per capita GDP trajectories, except through their influence on local incentives to secede. To maximally ensure that the latter condition holds, we exclude regional trends in the computation of the index since these could proxy for the presence of local trade disruptions, military conflict and/or macroeconomic instability. In addition, we show below that explicitly including these covariates as controls does not qualitatively affect the results. Finally we only rely on contemporary and future values of the index because it seems unlikely that unknown future evolutions in non-regional state formation would alter current economic behavior, as opposed to known past trends. In order to enable a Hansen-type test for the overall validity of the instrumentsinstruments, we include the contemporary values of this index along with its nearest three leads.. This yields the following fixed effects two-stage least squares model:

$$
\begin{aligned}
\tilde{y_{i,t}} &= \beta_1 N\tilde{I}C_{i,t} + \beta_2 N\tilde{I}C^1_{i,t} + \beta_3 N\tilde{I}C^2_{i,t} + \sum_{q=1}^{4} \alpha_q \tilde{y}_{i,t-q} + \theta \tilde{X}_{i,t} + \eta_t + \tilde{\epsilon}^Z_{i,t} \\
N\tilde{I}C_{i,t} &= \sum_{s=0}^{3} \sigma_s \tilde{I}_{i,t+s} + \sum_{q=1}^{4} \phi_q \tilde{y}_{i,t-q} + \xi_t + \tilde{\upsilon}_{i,t} \\
N\tilde{I}C^1_{i,t} &= \sum_{s=0}^{3} \varphi_s \tilde{I}_{i,t+s} + \sum_{q=1}^{4} \rho_q \tilde{y}_{i,t-q} + \zeta_t + \tilde{u}_{i,t} \\
N\tilde{I}C^2_{i,t} &= \sum_{s=0}^{3} \nu_s \tilde{I}_{i,t+s} + \sum_{q=1}^{4} \varpi_q \tilde{y}_{i,t-q} + \chi_t + \tilde{\gamma}_{i,t}
\end{aligned}
\tag{20}
$$

where the relevant exclusion restrictions now boil down to

Condition 3

$$
\begin{aligned}
&\mathbb{E}\left[N\tilde{I}C_{i,t}\left(\tilde{I}_{i,t}, \tilde{I}_{i,t+1}, \tilde{I}_{i,t+2}, \tilde{I}_{i,t+3}\right)'\right] && \neq 0 \\
&\mathbb{E}\left[N\tilde{I}C^1_{i,t}\left(\tilde{I}_{i,t}, \tilde{I}_{i,t+1}, \tilde{I}_{i,t+2}, \tilde{I}_{i,t+3}\right)'\right] && \neq 0 \\
&\mathbb{E}\left[N\tilde{I}C^2_{i,t}\left(\tilde{I}_{i,t}, \tilde{I}_{i,t+1}, \tilde{I}_{i,t+2}, \tilde{I}_{i,t+3}\right)'\right] && \neq 0 \\
&\mathbb{E}\left[\tilde{\epsilon}^Z_{i,s}\left(\tilde{I}_{i,t}, \tilde{I}_{i,t+1}, \tilde{I}_{i,t+2}, \tilde{I}_{i,t+3}\right)'\right] && = 0 \ \forall \ t \geq s
\end{aligned}
$$

The baseline results of our IV-approach are reported in column (1) of table A4. In line with subsequent estimations, the strong first-stage results (F-statistic = 227) confirm the expected correlation between contemporary incentives to secede in the own country and contemporary as well as future realizations of independence in the rest of the world. Reassuringly, a Hansen J-test fails to reject the null hypothesis that our over-identification restrictions hold (p-value = 0.78), finding no evidence of global waves of secessions co-determining local per capita GDP trajectories outside of their impact on local incentives to secede. Similar to our previous results, there is a fairly high amount of persistence in per capita GDP (in this case, $\sum_{q=1}^{4} \alpha_q = 0.973$) but an F-test rejects dynamic instability.

With regard to the coefficients of interest, first note that our results now suggest the absence of any relevant anticipation effects, as the coefficient associated with the lead of the independence dummy is now statistically insignificant. The absence of meaningful pre-independence trends in per capita GDP trajectories of NICs nearing their declaration of independence is reassuring, in the sense that this points towards the instrument exogeneity requirement being met: *prior* to their actual declaration of independence, global waves of secession seem not systematically negatively related to economic conditions in future NICs. Only to the extent that they coincide with the period following a NIC's *actual* declaration of independence do they statistically significantly and negatively affect per capita GDP trajectories in NICs. Remarkably, the estimated adverse long-run impact of state fragmentation is now roughly twice as high in comparison to the simple pooled OLS estimates reported earlier. More specifically, our benchmark IV-estimates imply a short-run per capita secession cost of around 1.4% and an implied long-run cost of 54%, which are in the range of our semi-parametric estimates.

Column (2), then, controls for the possibility that global, non-regional waves of secession do violate the exclusion restriction of instrument validity by proxying for local military conflict, macroeconomic instability and/or trade disruptions. Reassuringly, as can be seen, our baseline results are fairly stable with respect to the inclusion of these control variables and the estimated long-run per capita cost of independence now increases to 75%. The third column, finally, confirms once again our prior finding that independence-by-referendum seems preferable from an economic point of view.

One possible explanation for these rather gloomy estimates of the economic impact of secession is that they stem from the bluntness of the instruments at our disposal, which approximate *country-level* incentives to secede with *regional-level* information on non-regional secession waves. Doing so, our first-stage results might lead to an over-accentuation of the economic trends of highly secessionist countries located in historically secession-rich regions, such as Sub Sahara Africa. Compared to highly secessionist countries located in secession-poorer regions, these countries may face more severe independence costs precisely because of the accumulation of adverse effects of state fragmentation in their associated region.[38] Keeping this in mind, exploiting variation in contemporary and future global, non-regional waves of secessions qualitatively confirms our prior conclusions suggesting that past instances of state fragmentation persistently adversely impacted per capita GDP trajectories in NICs, and potentially severely so.

[38] Venables (2010), for instance, claims that excessive state fragmentation in Sub Sahara Africa hampers growth by, among other factors, diminishing agglomeration economies.

Table A4: **Economic impact of secession (IV estimates)**

	IV regression results		
	(1)	(2)	(3)
Independence dummy	-0.014***	-0.035**	-0.041**
	(0.004)	(0.015)	(0.018)
Ex ante effect (t - 1)	-0.033	-0.057	-0.075
	(0.023)	(0.125)	(0.149)
Ex ante effect (t - 2)	0.009	-0.018	-0.038
	(0.017)	(0.077)	(0.092)
Independence dummy × Soviet dummy			-0.019
			(0.102)
Independence dummy × Yugoslav dummy			0.003
			(0.087)
Independence dummy × referendum dummy			0.038***
			(0.014)
Observations	10,768 [192]	8,184 [187]	8,184 [187]
Adjusted R-squared	0.979	0.976	0.958
Country + Year FE	yes	yes	yes
Control vector	none	$\mathbf{X}^{\mathbf{compact}}$	$\mathbf{X}^{\mathbf{extensive}}$
$\sum_{q=1}^{4} \alpha_q$ [F-test <1]	.973 []	.954 []	.954 []
Hansen-J test [p-value]	.078 [.78]	1.444 [.23]	1.063 [.303]
F-test [first stage]	227.58	23.13	13.92
Long-run effect [p-value]	-.537 [0]	-.753 [.023]	-.895 [.026]

Note: This table presents fixed effects two-stage least squares estimates of the effect of gaining independence on per capita GDP, where both the decision to secede and its anticipation are instrumented by the four nearest (future) non-regional secession waves. Standard errors, robust against heteroskedasticity and serial correlation at the country level, in parantheses. F-test reports the p-value of the persistence in GDP being smaller than 1; F-test [first stage] reports first stage F-statistic of the excluded instruments; Hansen-J reports the p-value of a Hansen over-identification test. The estimated long-run economic impact of secession, as well as the p-value for this being different from 0, are reported in the bottom row. Standard errors of the long-run effect are computed through the delta-method, see Papke and Wooldridge (2005).

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

Table A5: **Newly Independent Countries: 1950-2015**

Country	*Year*	*Country*	*Year*	*Country*	*Year*
Libya	1951	Uganda$^{\diamond}$	1962	Saint Lucia$^{\diamond}$	1979
Cambodia*	1953	Kenya$^{\diamond}$	1963	Saint Vincent & the Grenadines$^{\diamond}$	1979
Lao PDR	1953	Malawi$^{\diamond}$	1964	Antigua & Barbuda$^{\diamond}$	1981
Vietnam	1954	Malta*$^{\diamond}$	1964	Belize$^{\diamond}$	1981
Morocco$^{\diamond}$	1956	Zambia$^{\diamond}$	1964	Vanuatu$^{\diamond}$	1981
Sudan	1956	Gambia$^{\diamond}$	1965	Saint Kitts & Nevis$^{\diamond}$	1983
Tunisia	1956	Maldives	1965	Brunei Darussalam$^{\diamond}$	1984
Ghana$^{\diamond}$	1957	Singapore*$^{\diamond}$	1965	Marshall Islands	1986
Malaysia$^{\diamond}$	1957	Zimbabwe$^{\diamond}$	1965	Micronesia*	1986
Guinea*	1958	Barbados$^{\diamond}$	1966	Namibia$^{\diamond}$	1990
Benin$^{\diamond}$	1960	Botswana$^{\diamond}$	1966	Yemen	1990
Burkina Faso	1960	Guyana$^{\diamond}$	1966	Armenia*$^{\diamond}$	1991
Cameroon	1960	Lesotho$^{\diamond}$	1966	Azerbaijan$^{\diamond}$	1991
Central African Republic	1960	South Yemen	1967	Belarus$^{\diamond}$	1991
Chad	1960	Equatorial Guinea	1968	Estonia*$^{\diamond}$	1991
Congo$^{\diamond}$	1960	Mauritius$^{\diamond}$	1968	Georgia*$^{\diamond}$	1991
Congo Republic	1960	Swaziland$^{\diamond}$	1968	Kazakhstan$^{\diamond}$	1991
Cote d'Ivoire	1960	Fiji$^{\diamond}$	1970	Kyrgyz Republic$^{\diamond}$	1991
Cyprus$^{\diamond}$	1960	Bahrain$^{\diamond}$	1971	Latvia*$^{\diamond}$	1991
Gabon	1960	Bangladesh$^{\diamond}$	1971	Lithuania*$^{\diamond}$	1991
Madagascar	1960	Oman	1971	Moldova$^{\diamond}$	1991
Mali	1960	Qatar$^{\diamond}$	1971	Russian Federation$^{\diamond}$	1991
Mauritania	1960	United Arab Emirates$^{\diamond}$	1971	Tajikistan$^{\diamond}$	1991
Niger	1960	Bahamas$^{\diamond}$	1973	Turkmenistan*$^{\diamond}$	1991
Nigeria$^{\diamond}$	1960	Grenada$^{\diamond}$	1974	Ukraine*$^{\diamond}$	1991
Senegal	1960	Guinea-Bissau$^{\diamond}$	1974	Uzbekistan*$^{\diamond}$	1991
Somalia	1960	Angola$^{\diamond}$	1975	Croatia*$^{\diamond}$	1992
Togo	1960	Cabo Verde$^{\diamond}$	1975	Slovenia$^{\diamond}$	1992
Kuwait	1961	Comoros$^{\diamond}$	1975	Czech Republic$^{\diamond}$	1993
Sierra Leone	1961	Mozambique$^{\diamond}$	1975	Eritrea*	1993
Syrian Arab Republic$^{\diamond}$	1961	Papua New Guinea$^{\diamond}$	1975	Macedonia*$^{\diamond}$	1993
Tanzania$^{\diamond}$	1961	Sao Tome & Principe$^{\diamond}$	1975	Slovak Republic$^{\diamond}$	1993
Algeria*$^{\diamond}$	1962	Suriname$^{\diamond}$	1975	Palau*$^{\diamond}$	1994
Burundi$^{\diamond}$	1962	Seychelles$^{\diamond}$	1976	Timor-Leste*$^{\diamond}$	2002
Jamaica*$^{\diamond}$	1962	Djibouti$^{\diamond}$	1977	Montenegro*$^{\diamond}$	2006
North Yemen	1962	Dominica	1978	Serbia$^{\diamond}$	2006
Rwanda$^{\diamond}$	1962	Solomon Islands$^{\diamond}$	1978	Kosovo	2008
Samoa*	1962	Tuvalu	1978	South Sudan*	2011
Trinidad and Tobago$^{\diamond}$	1962	Kiribati$^{\diamond}$	1979		

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

Table A6: **Semi-parametric estimates of the economic impact of secession**

	t = T_0 + 1				**t = T_0 + 5**				**t = T_0 + 20**			
Country	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$
Algeria	-.219	-.216***	-.205***	-.202***	-.304	-.301***	-.276***	-.272***	-.285	-.282***	-.226***	-.222***
Angola	-.77	-.792***	-.76***	-.788***	-.963	-.985***	-.924***	-.953***	-.975	-.997***	-.827***	-.856***
Antigua & Barbuda	.424	.409***	.475***	.418***	1.04	1.024***	1.109***	1.053***	1.127	1.111***	1.318***	1.262***
Armenia	-0.682	-.613***	-.599***	-.586***	-.604	-.535***	-.499***	-.486***	.184	.253***	.405***	.419***
Azerbaijan	-.528	-.547***	-.446***	-.52***	-1.288	-1.306***	-1.183***	-1.257***	-.002	-.021	.219***	.146**
Bahamas	-.347	-.447***	-.331***	-.448***	-.385	-.485***	-.351***	-.469***	-.44	-.54***	-.308***	-.425***
Bahrain	-.071	-.068***	-.043***	-.05***	-.069	-.066***	-.045**	-.052**	-.861	-.859***	-.736***	-.743***
Bangladesh	-.316	-.309***	-.302***	-.298***	-.373	-.366***	-.366***	-.361***	-.424	-.417***	-.307***	-.303***
Barbados	.068	.071***	.08***	.083***	.12	.123***	.141***	.144***	-.085	-.082***	-.028	-.024
Belarus	-.33	-.317***	-.247***	-.289***	-0.683	-.67***	-.578***	-.62***	.14	.153***	.361***	.319***
Belize	-.09	-.092***	-.039*	-.081***	-.262	-.264***	-.191***	-.233***	.088	.087***	.262***	.22***
Benin	-.008	-.006	.013	.012	-.063	-.062***	-.027	-.028	-.271	-.269***	-.231***	-.232***
Botswana	-.132	.186***	-.12***	.197***	.14	.457***	.16***	.478***	.995	1.313***	1.053***	1.37***
Brunei Darussalam	-.1	-.246***	-.024	-.21***	-.305	-.451***	-.196***	-.381***	-.615	-.761***	-.39***	-.575***
Burundi	-.402	-.261***	-.388***	-.246***	-.468	-.327***	-.439***	-.297***	-.23	-.088**	-.169***	-.027
Cabo Verde	-.256	-.267***	-.241***	-.259***	.099	.087***	.144***	.126***	.336	.324***	.491***	.473***
Comoros	-.336	-.334***	-.325***	-.33***	-.455	-.452***	-.416***	-.42***	-.187	-.184***	-.039	-.043
Congo	.002	.004	.021	.017	-.066	-.065***	-.033	-.037*	-.028	-.026***	.011	.007
Croatia	-.429	-.549***	-.338***	-.514***	-.286	-.406***	-.166***	-.341***				
Cyprus	.025	.046	.044**	.06*	-.008	.013	.026	.042	.481	.502***	.52***	.535***
Czech Republic	-.523	-.435***	-.452***	-.407***	-.518	-.43***	-.429***	-.385***				
Djibouti	-.206	-.211***	-.162***	-.185***	-.259	-.264***	-.195***	-.218***	-0.825	-.831***	-.639***	-.662***
Estonia	-.313	-.419***	-.231***	-.391***	-.321	-.427***	-.217***	-.377***	.202	.097**	.423***	.263***
Fiji	-.178	-.007	-.164***	0	-.163	.008	-.146***	.017	-.12	.051	-.005	.158***
Gambia	.147	.153***	.162***	.17***	.068	.074***	.084***	.092***	-.171	-.166***	-.107***	-.1**

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	$\mathbf{t} = T_0 + \mathbf{1}$				$\mathbf{t} = T_0 + \mathbf{5}$				$\mathbf{t} = T_0 + \mathbf{20}$			
Country	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$
Georgia	-1.121	-1.146***	-1.029***	-1.116***	-1.459	-1.484***	-1.34***	-1.427***	-.772	-.797***	-.54***	-.627***
Ghana	-.096	-.097***	-.104***	-.108***	-.035	-.035**	-.013	-.017	-.665	-.665***	-.622***	-.626***
Grenada	-.209	-.211***	-.192***	-.211***	-.142	-.144***	-.092***	-.111***	-.024	-.026	.128***	.109**
Guinea-Bissau	.16	.121***	.175***	.13***	.118	.08***	.166***	.122***	.023	-.016	.182***	.137***
Guyana	-.041	-.021	-.03**	-.01	-.069	-.049*	-.045**	-.026	-.746	-.726***	-.662***	-.643***
Jamaica	-.043	-.043	-.03**	-.031	-.065	-.064	-.039**	-.04	-.639	-.638***	-.59***	-.59***
Kazakhstan	-.457	-.457***	-.375***	-.43***	-.776	-.776***	-.671***	-.726***	-.186	-.186***	.036	-.019
Kenya	-.081	-.06**	-.073***	-.052**	.005	.026	.019	.04	-.199	-.178***	-.154***	-.132***
Kiribati	-.988	-.959***	-.939***	-.933***	-.986	-.957***	-.913***	-.907***	-1.174	-1.145***	-.988***	-.982***
Kyrgyz Republic	-.164	-.146***	-.082***	-.119***	-0.699	-.681***	-.595***	-.632***	-.535	-.517***	-.314***	-.351***
Latvia	-.548	-.716***	-.466***	-.689***	-.627	-.795***	-.523***	-.746***	-.155	-.323***	.066	-.157***
Lesotho	-.133	.157***	-.121***	.169***	-.175	.115***	-.154***	.136***	-.118	.173***	-.06*	.23***
Lithuania	-.467	-.493***	-.384***	-.466***	-.755	-.781***	-.651***	-.732***	-.283	-.309***	-.062	-.143***
Macedonia	-.353	-.409***	-.281***	-.381***	-.437	-.493***	-.348***	-.448***				
Malawi	-.44	-.121***	-.429***	-.11***	-.324	-.005	-.31***	.009	-.478	-.16***	-.456***	-.138***
Malaysia	-.097	-.094***	-.105***	-.105***	-.081	-.078***	-.06***	-.06**	.136	.14***	.179***	.179***
Malta	.043	.034	.053***	.045*	.063	.054**	.078***	.07***	.406	.397***	.45***	.442***
Mauritius	-.263	-.258***	-.25***	-.248***	-.218	-.212***	-.202***	-.2***	.024	.029	.094**	.097**
Moldova	-0.576	-.613***	-.494***	-.586***	-1.178	-1.216***	-1.074***	-1.166***	-1.022	-1.06***	-.802***	-.894***
Montenegro	.276	.226***	.378***	.239***	.378	.329***	.487***	.349***				
Morocco	-.12	-.122***	-.126***	-.127***	-.205	-.207***	-.191***	-.193***	-.208	-.21***	-.15***	-.151***
Mozambique	-.236	-.317***	-.225***	-.313***	-.337	-.418***	-.298***	-.385***	-.492	-.573***	-.344***	-.432***
Namibia	-.247	-.176***	-.166***	-.153***	-.141	-.07*	-.032	-.02	-.121	-.05	.117**	.13**
Nigeria	-.043	-.039***	-.024	-.025	-.052	-.048***	-.018	-.019	.006	.01	.045	.043
Palau	-.084	-.019	.007	.01	-.182	-.117**	-.072***	-.069				
Papua New Guinea	-.171	-.183***	-.16***	-.179***	-.354	-.366***	-.315***	-.335***	-.451	-.464***	-.304***	-.324***

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Country	t = T_0 + 1				t = T_0 + 5				t = T_0 + 20			
	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$
Qatar	.257	.029	.284***	.047	.645	.417***	.667***	.431***	-.168	-.396***	-.044	-.28***
Russian Federation	-.31	-.334***	-.225***	-.306***	-.709	-.732***	-.595***	-.677***	-.283	-.307***	-.045	-.126**
Rwanda	-.107	-.108***	-.092***	-.093***	-.203	-.204***	-.174***	-.174***	.152	.151***	.212***	.212***
Saint Kitts & Nevis	.335	.283***	.395***	.303***	.659	.607***	.764***	.673***	.787	.735***	1.016***	.924***
Saint Lucia	-.097	-.133***	-.046**	-.103***	-.057	-.094***	.022	-.034	.182	.146***	.386***	.33***
Saint Vincent & the Grenadines	.032	.054*	.082***	.084***	.148	.17***	.228***	.23***	.484	.506***	.688***	.689***
Sao Tome & Principe	-.027	-.047*	-.003	-.045	.271	.251***	.32***	.277***	-.11	-.13***	.033	-.01
Serbia	.009	.065***	.112***	.079***	.047	.103***	.152***	.118***				
Seychelles	.095	.108***	.131***	.132***	.032	.045**	.088***	.089***	.302	.315***	.478***	.479***
Singapore	.036	.058***	.051***	.075***	.287	.31***	.303***	.327***	0.812	.834***	.875***	.899***
Slovak Republic	-.467	-.388***	-.397***	-.361***	-.366	-.287***	-.277***	-.241***				
Slovenia	-.317	-.31***	-.226***	-.275***	-.249	-.242***	-.127***	-.177***	-.148	-.141***	.053	.004
Solomon Islands	-.004	.023	.047***	.058**	.08	.106***	.141***	.152***	.148	.174***	.325***	.336***
Suriname	-.118	-.151***	-.107***	-.147***	-.28	-.313***	-.241***	-.281***	-.716	-.749***	-.568***	-.608***
Swaziland	.129	.126*	.136***	.129**	.386	.383***	.395***	.388***	.457	.454***	.531***	.524***
Syrian Arab Republic	.076	.077*	.081***	.083**	-.208	-.207***	-.19***	-.188***	.257	.258***	.293***	.295***
Tajikistan	-.791	-.624***	-.714***	-.598***	-1.658	-1.491***	-1.559***	-1.442***	-1.196	-1.028***	-.972***	-.856***
Tanzania	-.244	-.233***	-.239***	-.228***	-.283	-.273***	-.265***	-.253***	-.326	-.315***	-.289***	-.278***
Timor-Leste	-.429	-.116	-.326***	-.092	-.587	-.274*	-.477***	-.244***				
Trinidad & Tobago	-.053	-.054**	-.039***	-.042*	-.111	-.112***	-.081***	-.085***	-.244	-.245***	-.2***	-.203***
Turkmenistan	-.157	-.146***	-.075***	-.119***	-.543	-.531***	-.439***	-.482***	-.045	-.033	.176***	.133**
Uganda	-.026	-.026***	-.011	-.014	-.033	-.033***	-.003	-.005	-.72	-.719***	-.675***	-.678***
Ukraine	-.276	-.287***	-.191***	-.259***	-1.02	-1.031***	-.907***	-.975***	-.777	-.788***	-.539***	-.607***
United Arab Emirates	-.147	-.211***	-.12***	-.193***	-.047	-.111***	-.025	-.097***	-.735	-.799***	-.611***	-.683***
Uzbekistan	-.217	-.164***	-.134***	-.137***	-.459	-.407***	-.355***	-.358***	-.012	.04**	.209***	.207***
Vanuatu	-.132	-.171**	-.08***	-.16***	.016	-.023	.088***	.008	-.725	-.764***	-.551***	-.631***

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Country	$\mathbf{t} = T_0 + \mathbf{1}$ $\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$	$\mathbf{t} = T_0 + \mathbf{5}$ $\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$	$\mathbf{t} = T_0 + \mathbf{20}$ $\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{pDD}$	$\hat{\beta}_{jt}^{DDD}$
Zambia	.133	.13***	.146***	.143***	-.039	-.042**	-.023	-.025	-.737	-.739***	-.679***	-.682***
Zimbabwe	-.124	-.139***	-.114***	-.127***	.079	.064***	.097***	.083***	-.463	-.479***	-.397***	-.411***

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 10 ; 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 11 ; columns headed by $\hat{\beta}_{jt}^{pDD}$ report the placebo-demeaned independence dividend estimate, net of its average contemporary placebo gap, as outlined in equation 12 ; columns headed by $\hat{\beta}_{jt}^{DDD}$ report the trend- and placebo-demeaned independence dividend estimate, as defined in equation 13. Standard errors are robust for heteroskedasticity.

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

Table A7: **Baseline parametric estimates (full results)**

	Within estimates		
	(1)	(2)	(3)
Independence dummy	-0.004**	-0.005**	-0.007**
	(0.002)	(0.003)	(0.003)
Independence dummy × Soviet dummy			-0.024**
			(0.011)
Independence dummy × Yugoslav dummy			0.009
			(0.019)
Independence dummy × referendum dummy			0.014***
			(0.003)
Ex ante effect (t - 1)	-0.024***	-0.039***	-0.038***
	(0.008)	(0.013)	(0.014)
Ex ante effect (t - 2)	-0.016***	-0.007	-0.006
	(0.005)	(0.010)	(0.011)
log Per capita GDP (t-1)	1.091***	1.136***	1.135***
	(0.034)	(0.032)	(0.032)
log Per capita GDP (t-2)	-0.030	-0.095**	-0.095**
	(0.048)	(0.039)	(0.039)
log Per capita GDP (t-3)	-0.010	-0.012	-0.013
	(0.046)	(0.025)	(0.025)
log Per capita GDP (t-4)	-0.077***	-0.072***	-0.071***
	(0.023)	(0.018)	(0.018)
log Trade openness		0.005	0.005
		(0.004)	(0.004)
log Surface area		-0.070	-0.064
		(0.062)	(0.062)
log Educational attainment		-0.007	-0.007*
		(0.004)	(0.004)
Life Expectancy		0.001***	0.001***
		(0.000)	(0.000)
log Battle deaths		-3.440	-3.475
		(2.660)	(2.681)
log Population density		-0.029***	-0.030***
		(0.009)	(0.009)
Democracy		0.000	0.000
		(0.000)	(0.000)
Macroeconomic uncertainty		-0.008***	-0.008***
		(0.002)	(0.002)
Political instability		-0.038***	-0.038***
		(0.014)	(0.014)
Oil exporting countries		0.001	0.001
		(0.005)	(0.004)
WTO-membership		0.004	0.005
		(0.003)	(0.003)
EU-membership		-0.005	-0.005
		(0.004)	(0.004)
AU-membership		-0.002	-0.001
		(0.005)	(0.005)
ASEAN-membership		0.029***	0.029***
		(0.008)	(0.008)
OECD-membership		-0.008	-0.008
		(0.005)	(0.005)
MERCOSUR-membership		-0.003	-0.003
		(0.004)	(0.004)
NATO-membership		-0.003	-0.003
		(0.004)	(0.004)
Observations [# countries]	11,128 [192]	8,318 [187]	8,318 [187]
Adjusted R-squared	0.980	0.978	0.978
Country + Year FE	yes	yes	yes
Control vector	none	$\mathbf{X}^{\mathbf{compact}}$	$\mathbf{X}^{\mathbf{extensive}}$
$\sum_{q=1}^{4} \alpha_q$ [F-test <1]	.974 [0]	.956 [0]	.956 [0]
Long-run effect [p-value]	-.161 [.037]	-.124 [.038]	-.16 [.013]

Note: See relevant notes table A3.
*** p<0.01, ** p<0.05, * p<0.1.

Baseline parametric estimates (full results) *continued*

	Deviations GMM estimates		
	(1)	(2)	(3)
Independence dummy	-0.006**	-0.006**	-0.003
	(0.003)	(0.003)	(0.002)
Independence dummy × Soviet dummy			-0.009
			(0.009)
Independence dummy × Yugoslav dummy			0.011
			(0.021)
Independence dummy × referendum dummy			0.009
			(0.006)
Ex ante effect (t - 1)	-0.020**	-0.039***	-0.034**
	(0.009)	(0.013)	(0.014)
Ex ante effect (t - 2)	-0.011**	-0.006	-0.002
	(0.006)	(0.010)	(0.010)
log Per capita GDP (t-1)	1.095***	1.163***	1.259***
	(0.045)	(0.067)	(0.069)
log Per capita GDP (t-2)	-0.116**	-0.151*	-0.193**
	(0.055)	(0.084)	(0.085)
log Per capita GDP (t-3)	0.005	-0.009	-0.012
	(0.048)	(0.026)	(0.027)
log Per capita GDP (t-4)	-0.050**	-0.053***	-0.064***
	(0.021)	(0.020)	(0.019)
log Trade openness		0.006	0.006
		(0.004)	(0.004)
log Surface area		-0.080	-0.070
		(0.066)	(0.066)
log Educational attainment		-0.007	-0.008*
		(0.005)	(0.005)
Life Expectancy		0.001***	0.001***
		(0.000)	(0.000)
log Battle deaths		-3.326	-3.359
		(2.625)	(2.648)
log Population density		-0.033***	-0.033***
		(0.012)	(0.012)
Democracy		0.000	0.000
		(0.000)	(0.000)
Macroeconomic uncertainty		-0.009***	-0.009***
		(0.003)	(0.003)
Political instability		-0.037***	-0.037***
		(0.014)	(0.014)
Oil exporting countries		0.001	0.001
		(0.005)	(0.005)
WTO-membership		0.005*	0.005
		(0.003)	(0.003)
EU-membership		-0.004	-0.004
		(0.004)	(0.004)
AU-membership		-0.003	-0.002
		(0.005)	(0.005)
ASEAN-membership		0.031***	0.031***
		(0.010)	(0.010)
OECD-membership		-0.009	-0.009
		(0.006)	(0.006)
MERCOSUR-membership		-0.004	-0.004
		(0.004)	(0.004)
NATO-membership		-0.003	-0.003
		(0.004)	(0.004)
Observations	10,935 [192]	8,131 [187]	8,131 [187]
Country FE	yes	yes	yes
Control vector	none	$\mathbf{X}^{\mathbf{compact}}$	$\mathbf{X}^{\mathbf{extensive}}$
$\sum_{q=1}^{4} \alpha_q$ [F-test <1]	.934 [0]	.951 [0]	.952 [0]
p-value AR2	.213	.265	.298
Long-run effect [p-value]	-.098 [.044]	-.118 [.038]	-.153 [.017]

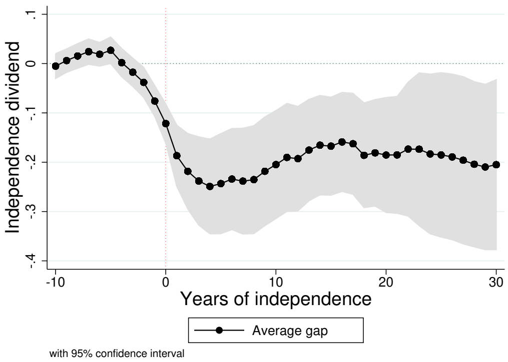
Note: See relevant notes table A3.
*** p<0.01, ** p<0.05, * p<0.1.

Baseline parametric estimates (full results) *continued*

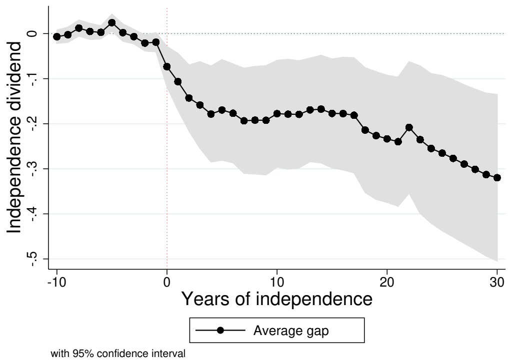
	Bootstrap-based bias corrected estimates		
	(1)	(2)	(3)
Independence dummy	-0.004*	-0.006**	-0.007***
	(0.002)	(0.002)	(0.002)
Independence dummy × Soviet dummy			-0.020
			(0.016)
Independence dummy × Yugoslav dummy			0.008
			(0.032)
Independence dummy × Referendum dummy			0.015***
			(0.004)
Ex ante effect (t - 1)	-0.025**	-0.040***	-0.039***
	(0.010)	(0.010)	(0.011)
Ex ante effect (t - 2)	-0.017***	-0.006	-0.005
	(0.005)	(0.011)	(0.011)
log Per capita GDP (t-1)	1.106***	1.149***	1.149***
	(0.031)	(0.035)	(0.035)
log Per capita GDP (t-2)	-0.031	-0.096**	-0.096**
	(0.044)	(0.042)	(0.041)
log Per capita GDP (t-3)	-0.009	-0.012	-0.012
	(0.049)	(0.028)	(0.028)
log Per capita GDP (t-4)	-0.077***	-0.070***	-0.069***
	(0.025)	(0.017)	(0.016)
log Trade Openness		0.005	0.005
		(0.004)	(0.004)
log Surface area		-0.040	-0.033
		(0.064)	(0.065)
log Educational attainment		-0.007	-0.007
		(0.005)	(0.005)
Life Expectancy		0.001***	0.001***
		(0.000)	(0.000)
log Battle deaths		-3.297	-3.339
		(4.782)	(4.764)
log Population density		-0.025***	-0.026***
		(0.007)	(0.007)
Democracy		0.000	0.000
		(0.000)	(0.000)
Macroeconomic uncertainty		-0.008***	-0.008***
		(0.002)	(0.002)
Political instability		-0.040**	-0.040***
		(0.017)	(0.014)
Oil exporting states		0.001	0.002
		(0.005)	(0.005)
WTO-membership		0.004	0.004
		(0.003)	(0.003)
EU-membership		-0.005	-0.005
		(0.004)	(0.004)
AU-membership		-0.003	-0.002
		(0.005)	(0.004)
ASEAN-membership		0.029***	0.029***
		(0.010)	(0.010)
OECD-membership		-0.007	-0.006
		(0.006)	(0.006)
MERCOSUR-membership		-0.001	-0.001
		(0.004)	(0.004)
NATO-membership		-0.005	-0.005
		(0.004)	(0.004)
Observations	11,111 [192]	8,318 [187]	8,318 [187]
Country + Year FE	yes	yes	yes
Control vector	none	$\mathbf{X}^{\textbf{compact}}$	$\mathbf{X}^{\textbf{extensive}}$
$\sum_{q=1}^{4} \alpha_q$ [F-test <1]	.988 [.001]	.971 [0]	.972 [0]
Long-run effect [p-value]	-.32 [.117]	-.192 [.019]	-.256 [.004]

Note: See relevant notes table A3.
*** p<0.01, ** p<0.05, * p<0.1.

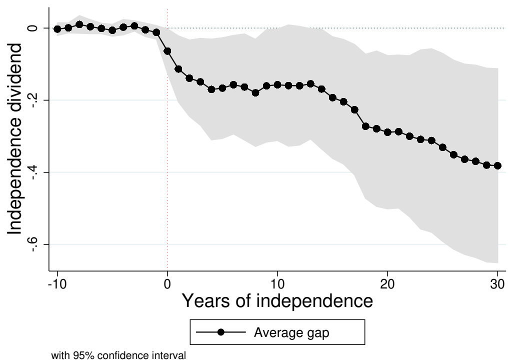
Figure A3: **Average impact of secession in selected countries**



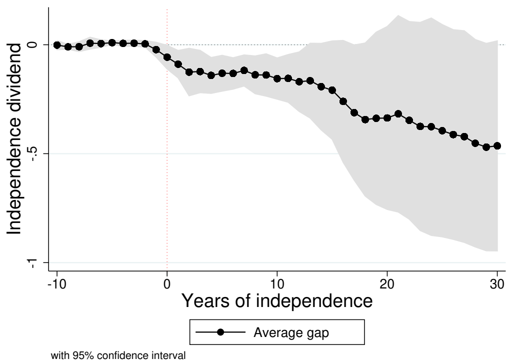
(a) All cases



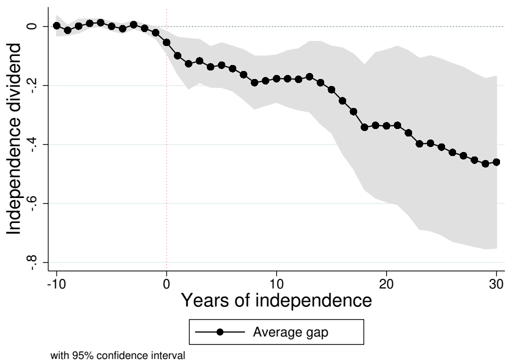
(b) 50% best matched cases



(c) 25% best matched cases



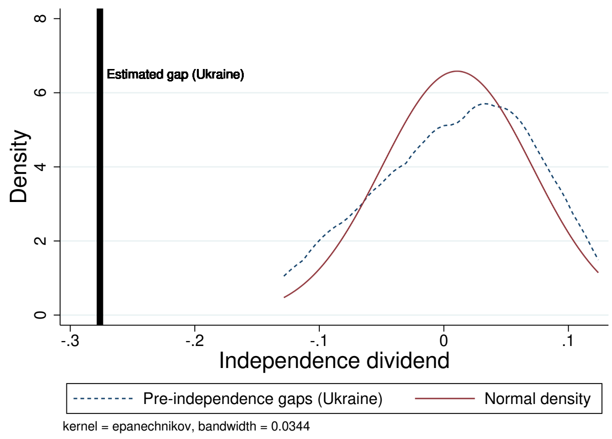
(d) 10% best matched cases



(e) All cases within caliper

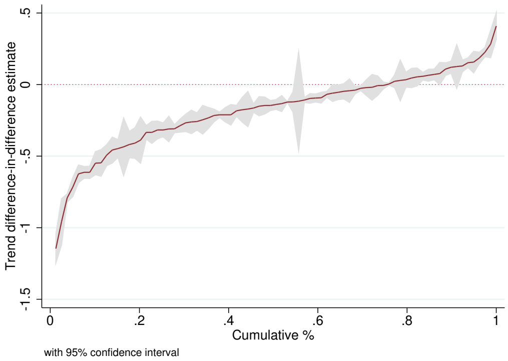
Note: This figure plots the yearly average percentage gap between NICs and their synthetic counterparts, along with the 95% confidence interval. The number of years before (-) or after (+) independence are indicated on the horizontal axis. The top-left panel contains all available cases, subsequent panels include only results of the 50, 25 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.

Figure A4: **Matching quality: post- versus pre-independence Ukraine**

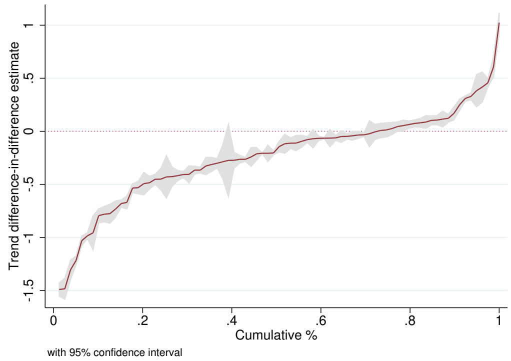


Note: The figure plots the distribution of the yearly discrepancy in per capita GDP between Ukraine and synthetic Ukraine in the 1981-1990 period. The Ukranian independence dividend estimate pertaining to the first post-independence year is indicated by the vertical black line.

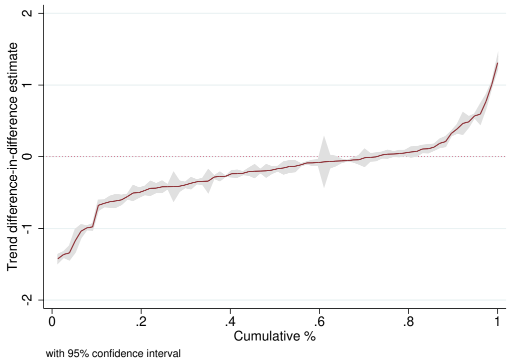
Figure A5: **Cumulative trend difference-in-difference estimates of independence dividends at selected time-points**



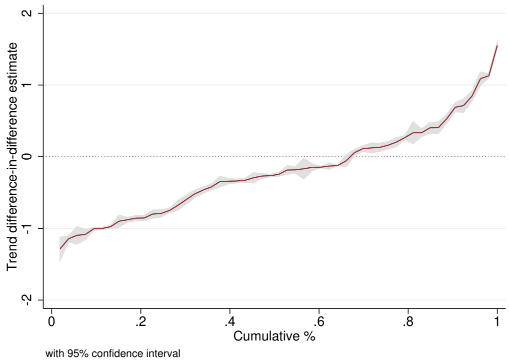
(a) 1^{st} year after independence.



(b) 5^{th} year after independence.



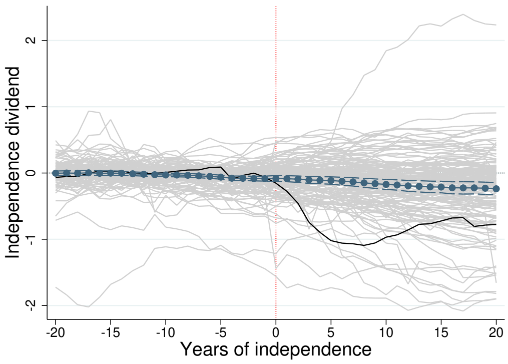
(c) 10^{th} year after independence.



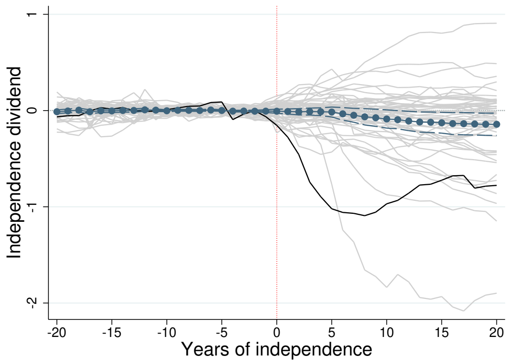
(d) 30^{th} year after independence.

Note: This figure plots the cumulative distribution of the country-specific trend difference-in-difference estimates also reported in table A6, along with their 95% confidence intervals. More specifically, the horizontal axis indicates the proportion of NICs with a trend-demeaned independence dividend estimate below the cut-off value indicated on the vertical axis. Estimated independence dividends pertain to the 1^{st}, 5^{th}, 10^{th} and 30^{th} post-independence year respectively.

Figure A6: **Simulation quality: Ukraine versus 153 control countries**



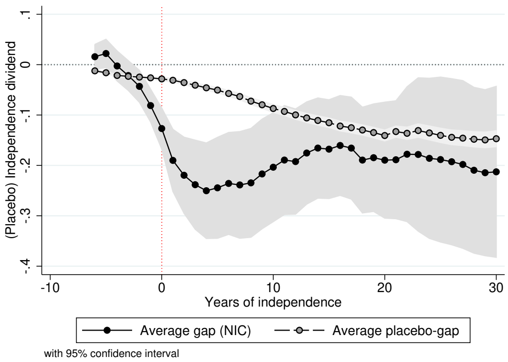
(a) All placebo gaps.



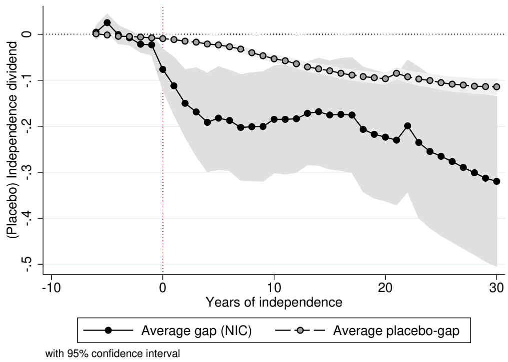
(b) Placebo gaps within caliper.

Note: The left figure plots the yearly average percentage gap between Ukraine and synthetic Ukraine (black line) against the yearly average placebo gaps between each of its 153 potential control countries and their synthetic versions (gray lines). The right panel includes only those potential control countries for which the pre-independence RMSPE is smaller or equal than the pre-independence RMSPE attained by synthetic Ukraine. The green line depicts the average placebo gap, along with the 95% confidence interval. The number of years before (-) or after (+) independence are indicated on the horizontal axis.

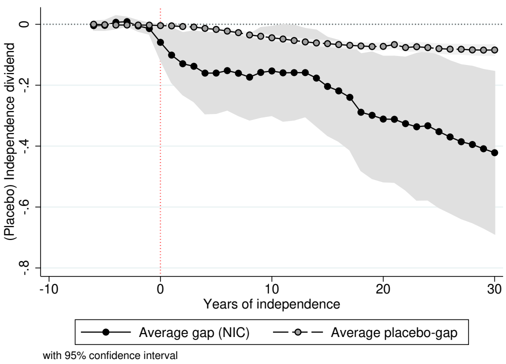
Figure A7: **Simulation quality: NICs versus placebo countries**



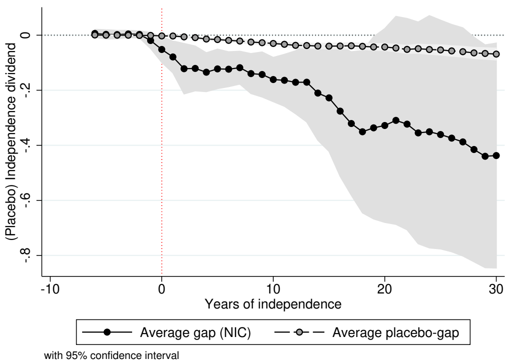
(a) All cases



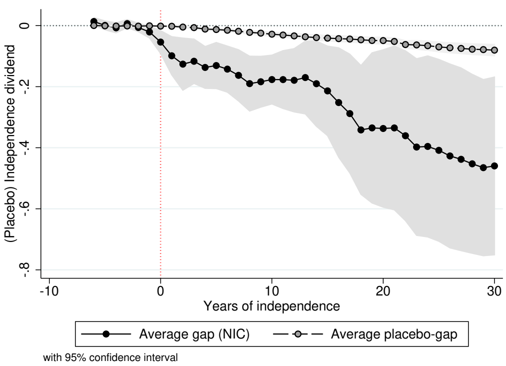
(b) 50% best matched cases



(c) 25% best matched cases



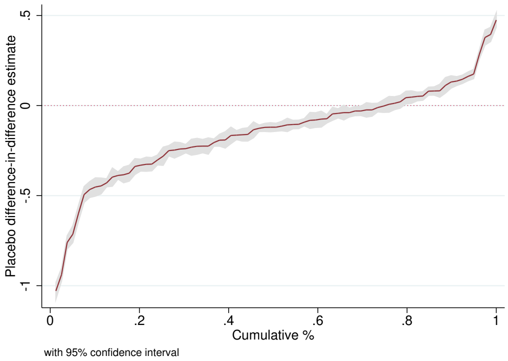
(d) 10% best matched cases



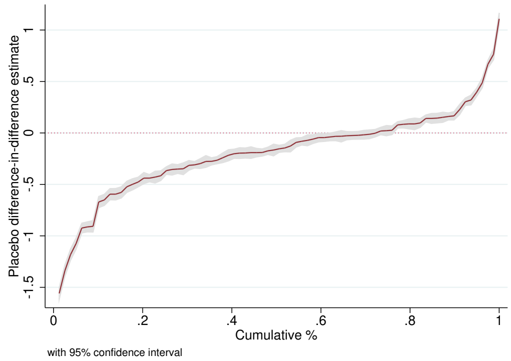
(e) All cases within caliper

Note: This figure plots the yearly average percentage gap between NICs and synthetic NICs (black dots) against the yearly average percentage placebo gap between their potential control countries and their synthetic counterparts (grey dots) along with a 95% confidence interval. 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 50, 25 and 10% best matched NICs, in terms of their pre-independence RMSPE, and their associated placebo countries. The bottom figure includes only those NICs (and their associated placebo countries) 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.

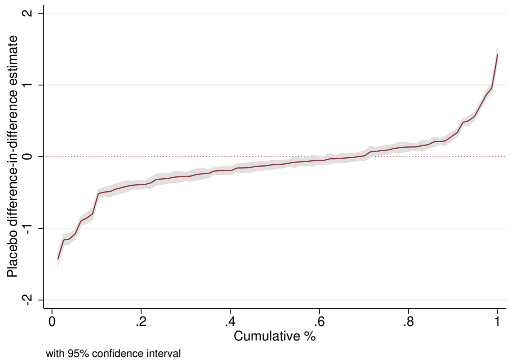
Figure A8: **Cumulative placebo difference-in-difference estimates of independence dividends at selected time-points**



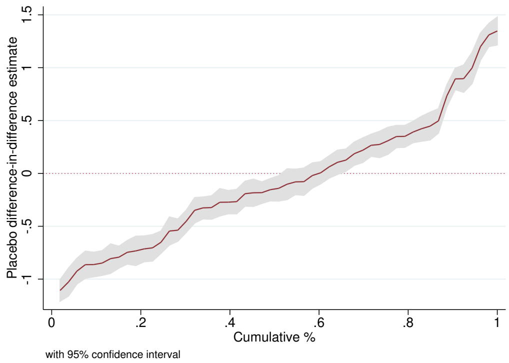
(a) 1^{st} year after independence.



(b) 5^{th} year after independence.



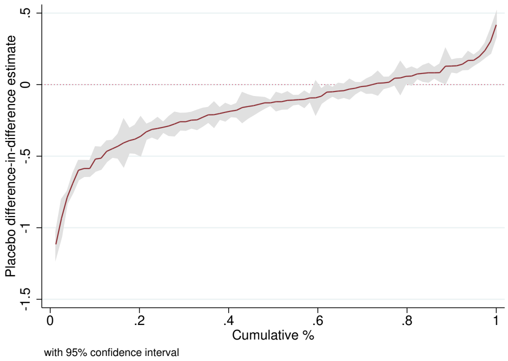
(c) 10^{th} year after independence.



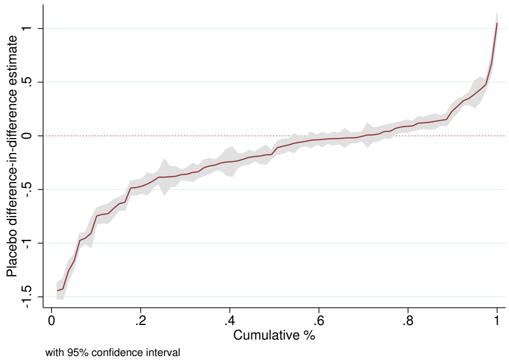
(d) 30^{th} year after independence.

Note: This figure plots the cumulative distribution of the country-specific placebo difference-in-difference estimates also reported in table A6, along with their 95% confidence intervals. More specifically, the horizontal axis indicates the proportion of NICs with a placebo-demeaned independence dividend estimate below the cut-off value indicated on the vertical axis. Estimated independence dividends pertain to the 1^{st}, 5^{th}, 10^{th} and 30^{th} post-independence year respectively.

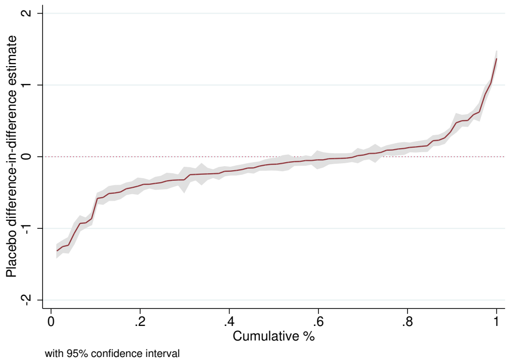
Figure A9: **Cumulative triple-difference estimates of independence dividends at selected time-points**



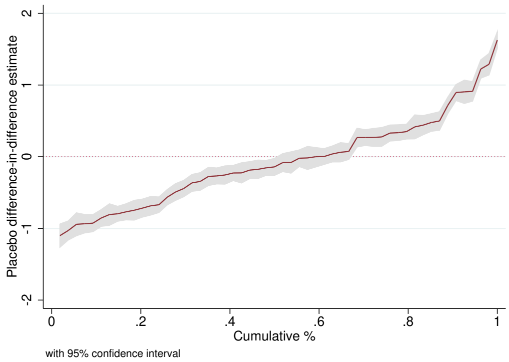
(a) 1^{st} year after independence.



(b) 5^{th} year after independence.



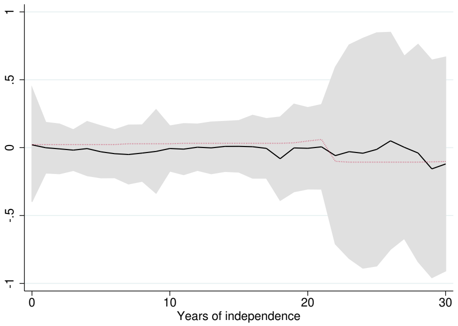
(c) 10^{th} year after independence.



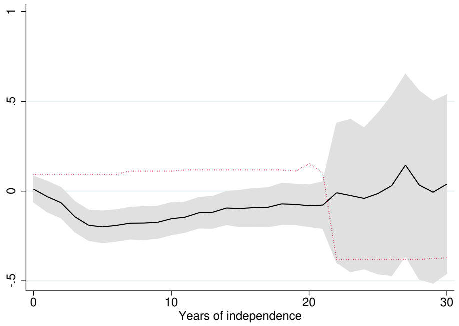
(d) 30^{th} year after independence.

Note: This figure plots the cumulative distribution of the country-specific triple-difference estimates also reported in table A6, along with the 95% confidence interval. More specifically, the horizontal axis indicates the proportion of NICs with a trend- and placebo-demeaned independence dividend estimate below the cut-off displayed on the vertical axis. Estimated independence dividends pertain to the 1^{st}, 5^{th}, 10^{th} and 30^{th} post-independence year respectively.

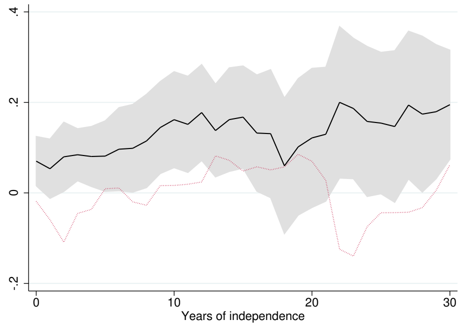
Figure A10: **Determinants of the raw independence dividend**



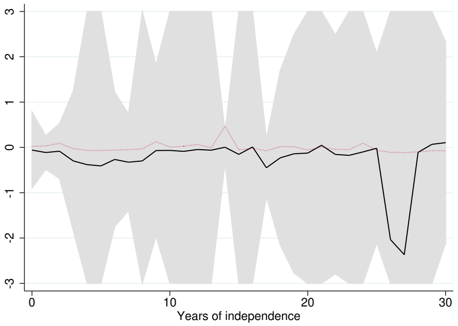
(a) Surface area



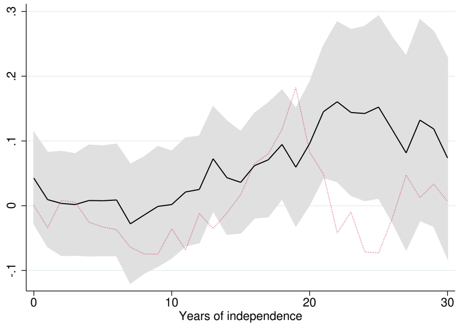
(b) Surface area loss



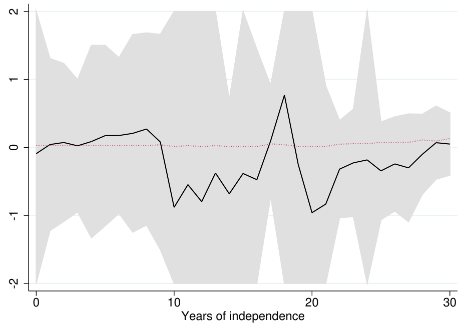
(c) Trade openness



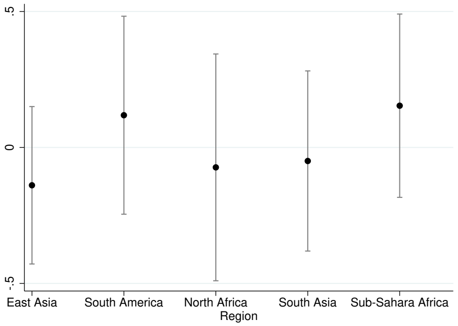
(d) Battle deaths



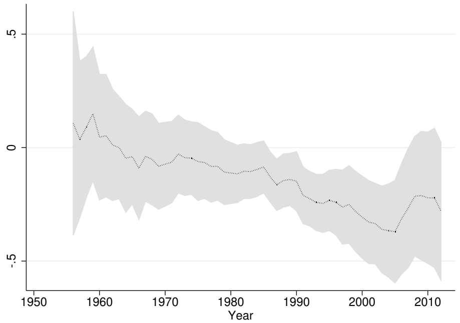
(e) Democracy



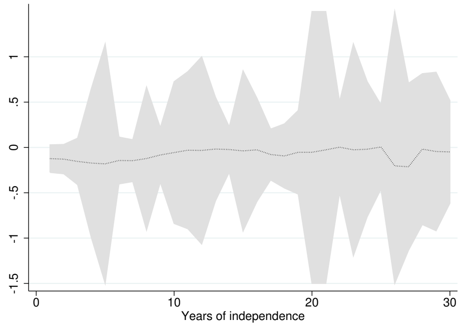
(f) Macroeconomic uncertainty



(g) Region fixed effects



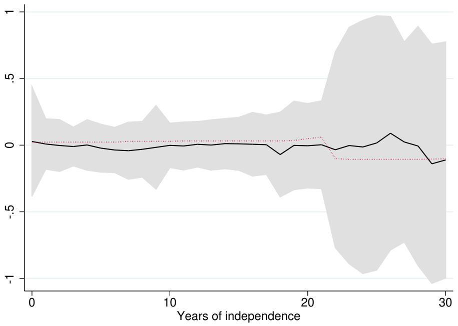
(h) Year fixed effects



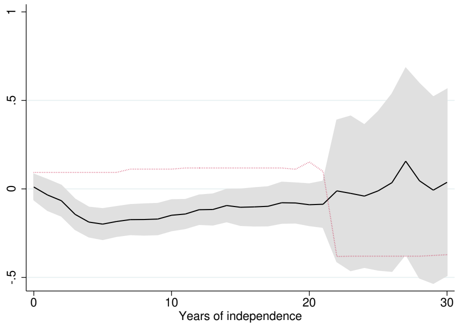
(i) Years-of-independence fixed effects

Note: The top and the middle row plot yearly estimates of the relative importance, as defined in equation 15, of several determinants of the raw independence dividend (black line) against their sample-average values (red lines). 90% bootstrapped confidence intervals, clustered at the country level and based on 250 replications, are plotted in gray. For reference, the bottom row plots the region, year and years-of-independence fixed effects: region fixed effects are relative to Europe & Central Asia; year fixed effects are relative to 1980; years-of-independence fixed effects are relative to the year of independence.The number of years after secession is indicated on the horizontal axis.

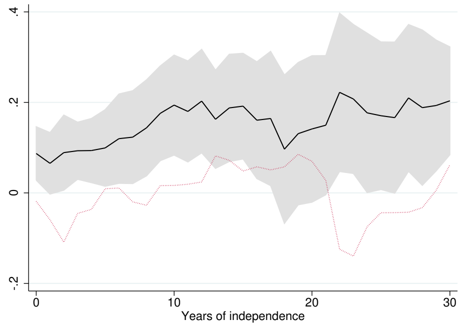
Figure A11: **Determinants of the trend-demeaned independence dividend**



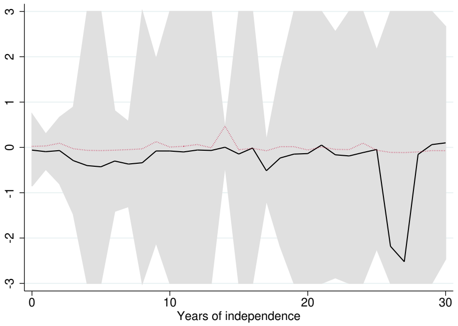
(a) Surface area



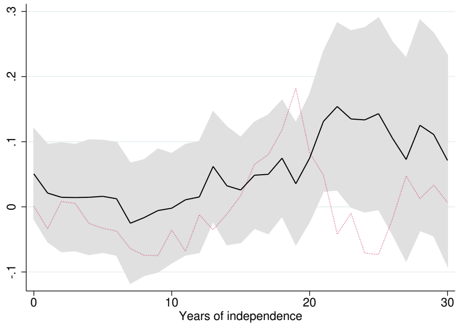
(b) Surface area loss



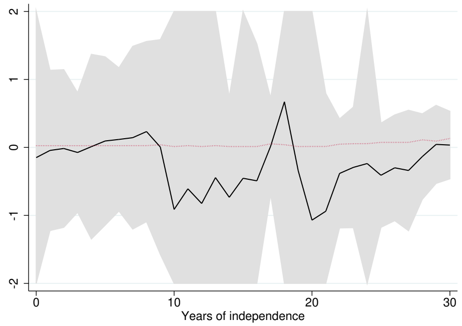
(c) Trade openness



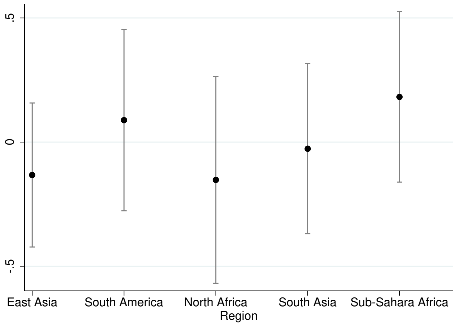
(d) Battle deaths



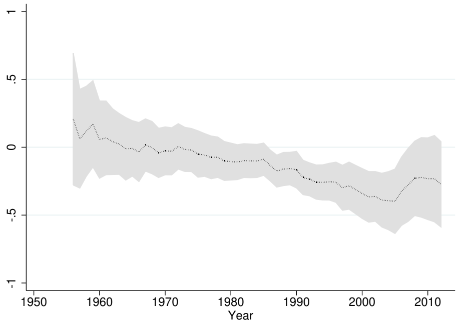
(e) Democracy



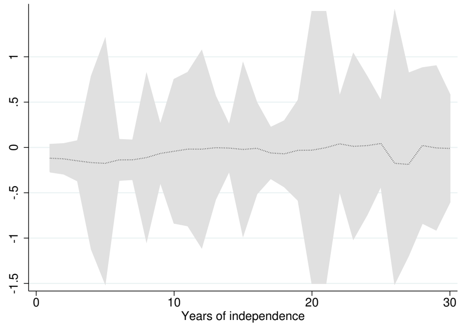
(f) Macroeconomic uncertainty



(g) Region fixed effects



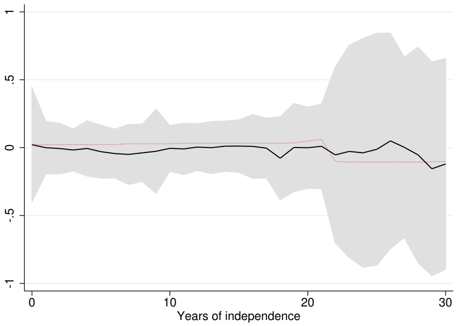
(h) Year fixed effects



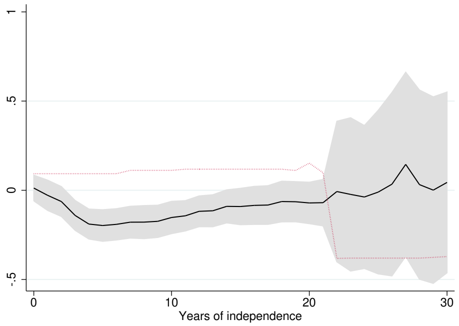
(i) Years-of-independence fixed effects

Note: The top and the middle row plot yearly estimates of the relative importance, as defined in equation 15, of several determinants of the trend-demeaned independence dividend (black line) against their sample-average values (red lines). 90% bootstrapped confidence intervals, clustered at the country level and based on 250 replications, are plotted in gray. For reference, the bottom row plots the region, year and years-of-independence fixed effects: region fixed effects are relative to Europe & Central Asia; year fixed effects are relative to 1980; years-of-independence fixed effects are relative to the year of independence.The number of years after secession is indicated on the horizontal axis.

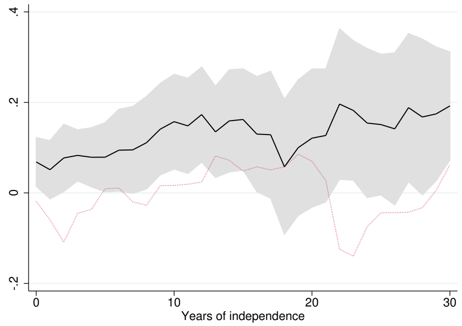
Figure A12: **Determinants of the placebo-demeaned independence dividend**



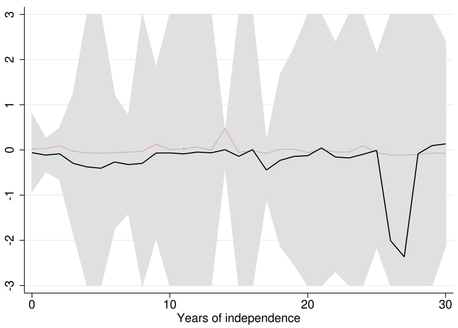
(a) Surface area



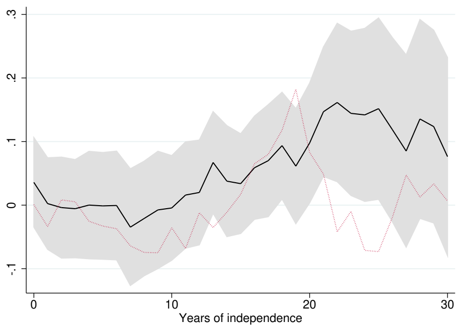
(b) Surface area loss



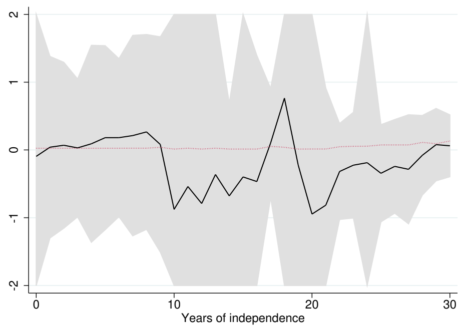
(c) Trade openness



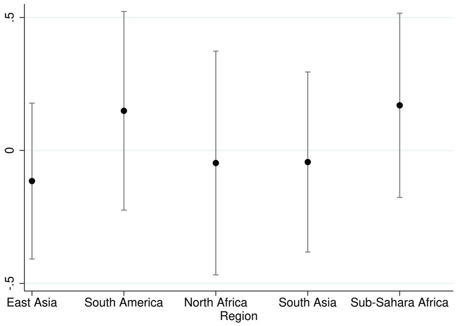
(d) Battle deaths



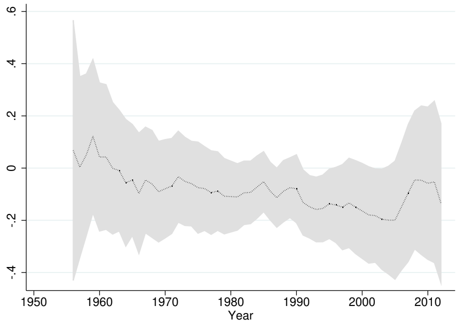
(e) Democracy



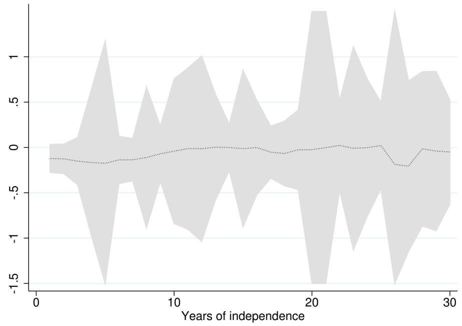
(f) Macroeconomic uncertainty



(g) Region fixed effects



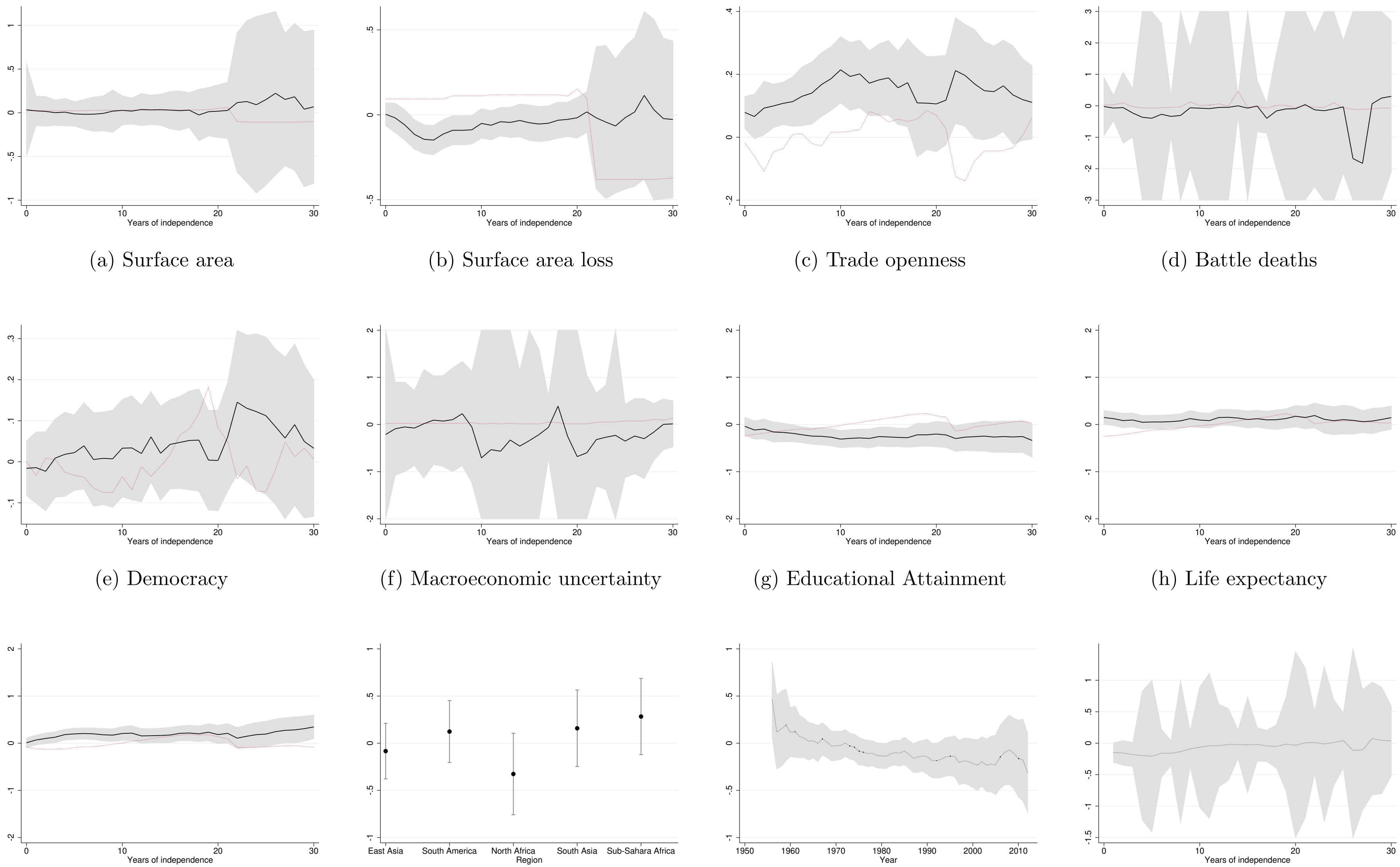
(h) Year fixed effects



(i) Years-of-independence fixed effects

Note: The top and the middle row plot yearly estimates of the relative importance, as defined in equation 15, of several determinants of the placebo-demeaned independence dividend (black line) against their sample-average values (red lines). 90% bootstrapped confidence intervals, clustered at the country level and based on 250 replications, are plotted in gray. For reference, the bottom row plots the region, year and years-of-independence fixed effects: region fixed effects are relative to Europe & Central Asia; year fixed effects are relative to 1980; years-of-independence fixed effects are relative to the year of independence.The number of years after secession is indicated on the horizontal axis.

Figure A13: **Determinants of the triple-differenced independence dividend: robustness *(1)***



(a) Surface area

(b) Surface area loss

(c) Trade openness

(d) Battle deaths

(e) Democracy

(f) Macroeconomic uncertainty

(g) Educational Attainment

(h) Life expectancy

(i) log Per capita GDP

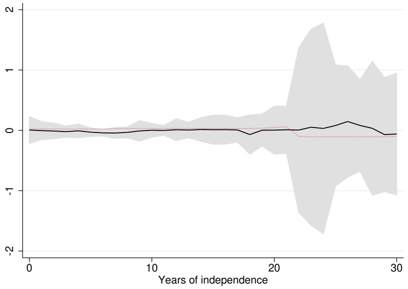
(j) Region FE

(k) Year FE

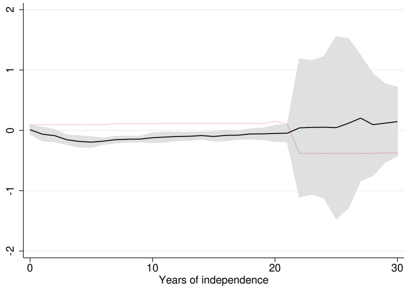
(l) Years-of-independence FE

Note: The top and the middle row plot yearly estimates of the relative importance, as defined in equation 15, of several determinants of the triple-differenced independence dividend (black line) against their sample-average values (red lines). 90% bootstrapped confidence intervals, clustered at the country level and based on 250 replications, are plotted in gray. For reference, the bottom row plots the region, year and years-of-independence fixed effects: region fixed effects are relative to Europe & Central Asia; year fixed effects are relative to 1980; years-of-independence fixed effects are relative to the year of independence.The number of years after secession is indicated on the horizontal axis.

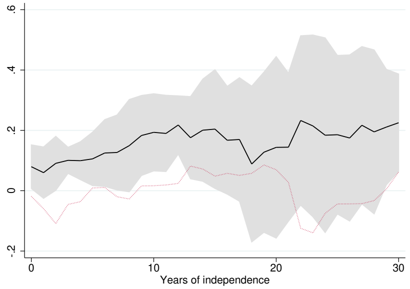
Figure A14: **Determinants of the triple-differenced independence dividend: robustness *(2)***



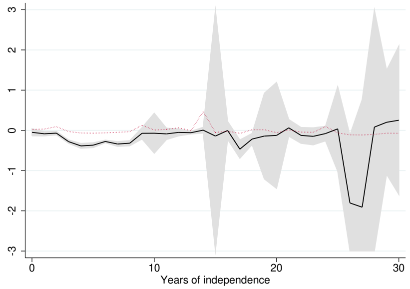
(a) Surface area



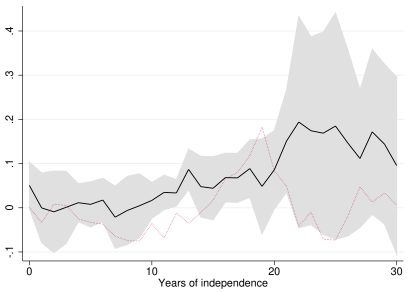
(b) Surface area loss



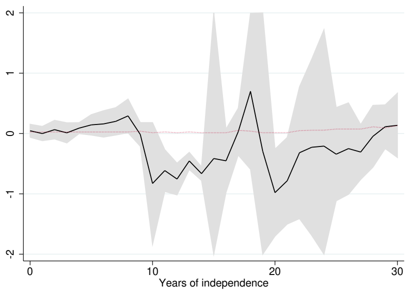
(c) Trade openness



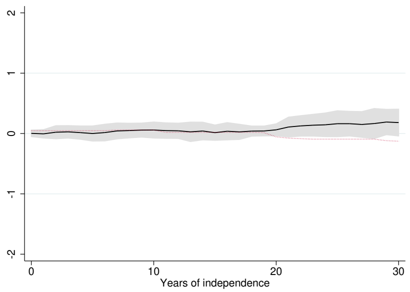
(d) Battle deaths



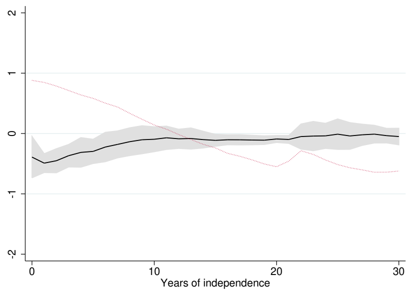
(e) Democracy



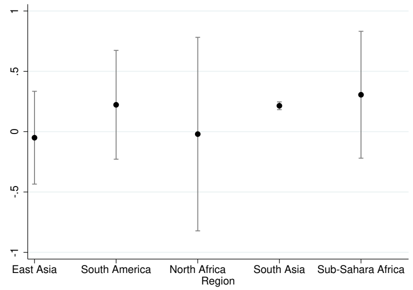
(f) Macroeconomic uncertainty



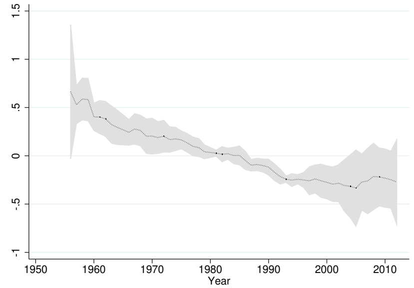
(g) Matching quality



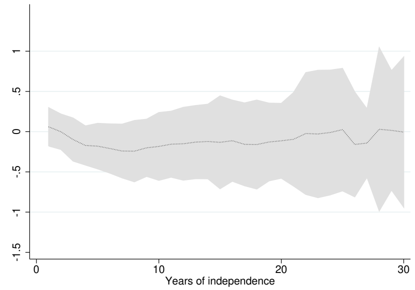
(h) Simulation quality



(i) Region FE



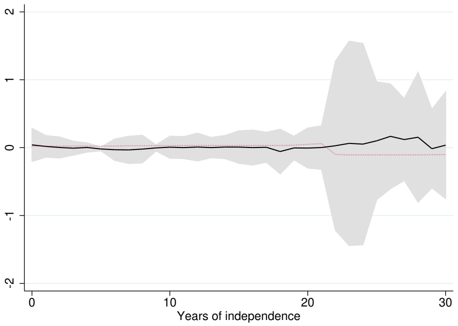
(j) Year FE



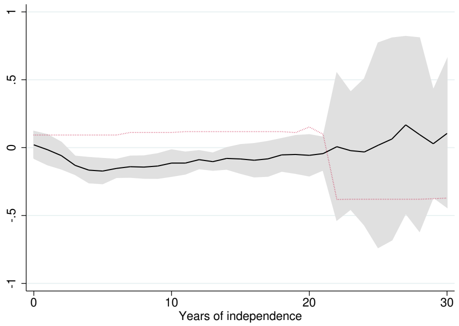
(k) Years-of-independence FE

Note: The top and the middle row plot yearly estimates of the relative importance, as defined in equation 15, of several determinants of the triple-differenced independence dividend (black line) against their sample-average values (red lines). 90% bootstrapped confidence intervals, clustered at the country level and based on 250 replications, are plotted in gray. For reference, the bottom row plots the region, year and years-of-independence fixed effects: region fixed effects are relative to Europe & Central Asia; year fixed effects are relative to 1980; years-of-independence fixed effects are relative to the year of independence.The number of years after secession is indicated on the horizontal axis.

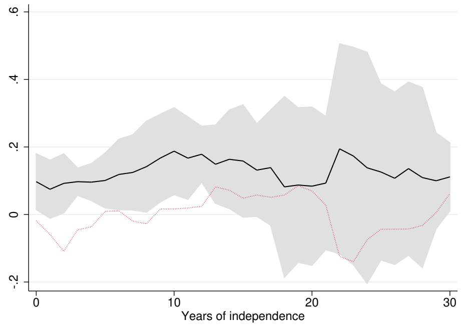
Figure A15: **Determinants of the triple-differenced independence dividend: robustness** ***(3)***



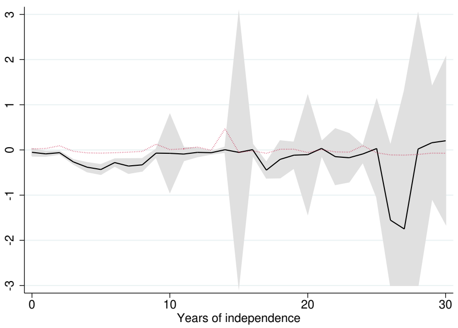
(a) Surface area



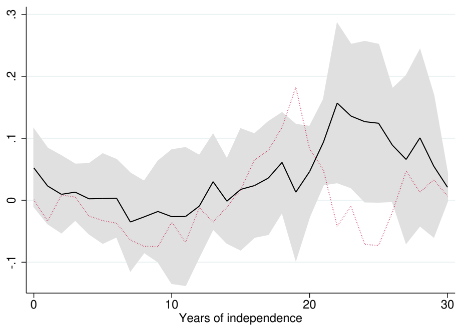
(b) Surface area loss



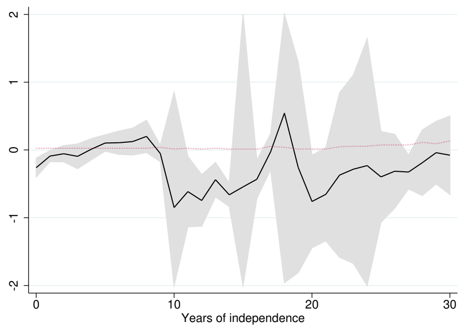
(c) Trade openness



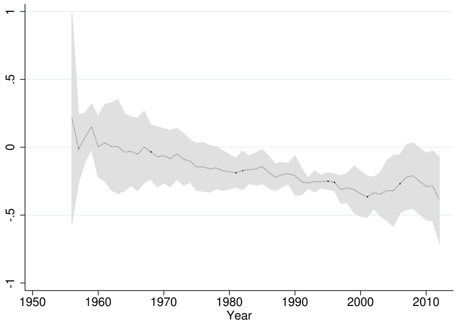
(d) Battle deaths



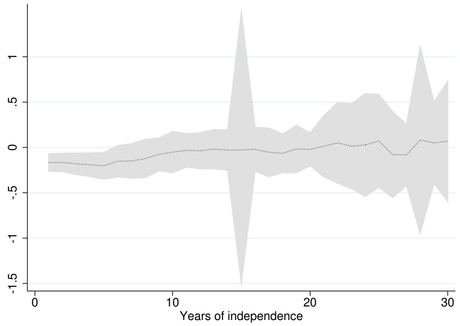
(e) Democracy



(f) Macroeconomic uncertainty



(g) Year fixed effects



(h) Years-of-independence fixed effects

Note: The top and the middle row plot yearly estimates of the relative importance, as defined in equation 15, of several determinants of the triple-differenced independence dividend (black line) against their sample-average values (red lines). 90% bootstrapped confidence intervals, clustered at the country level and based on 250 replications, are plotted in gray. For reference, the bottom row plots year and years-of-independence fixed effects: year fixed effects are relative to 1980; years-of-independence fixed effects are relative to the year of independence. Country fixed effects are included but not shown. The number of years after secession is indicated on the horizontal axis.