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 1950-2016. To do so, we rely on a semi-parametric identification strategy that controls for the confounding effects of past GDP dynamics, anticipation effects, unobserved heterogeneity, model uncertainty and effect heterogeneity. Our baseline results indicate that declaring independence reduces per capita GDP by around 20% in the long run. We subsequently propose a quadruple-difference procedure to demonstrate that the results are not driven by simulation and matching inaccuracies or spillover effects. A second methodological novelty consists of the development of a two-step estimator that relies on the control function approach to control for the potential endogeneity of the estimated independence payoffs and their potential determinants, to shed some light on the primary channels driving our results. We find tentative evidence that the adverse effects of independence decrease in territorial size, 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; quadruple-difference; control function 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 1950-2016.

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.

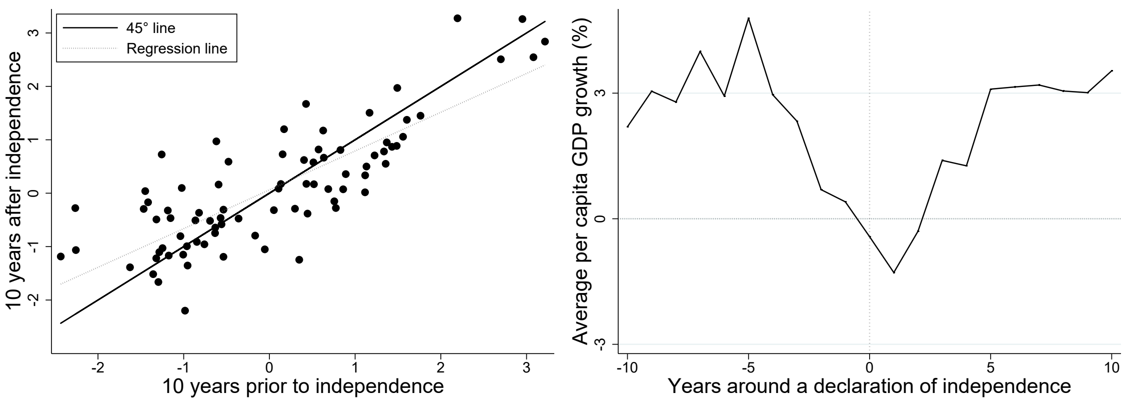
In order to provide a preliminary view on the existence as well as the magnitude of the independence dividend, Figure 1a presents difference-in-difference estimates of the impact of declaring independence on the relative economic performance of NICs, where the 'relative economic performance' of a country 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 .05 suggests that the net gain of independence tended to be positive and increased per capita income by 5%, 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% to 30%. 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 quadruple-difference inferential procedure. Most reassuringly, we find comparatively little effect on per capita GDP when applying the simulation procedure on countries *unaffected* by state fragmentation, while the negative per capita GDP discrepancy between NICs and their counterfactuals in the post-independence period also clearly exceeds the discrepancy between both typically observed in the pre-independence period. In addition, our main conclusions remain qualitatively unchanged when we parametrically control for potential contamination effects stemming from the economic effects of independence in other recently formed states. Although this underscores that they are unlikely to be driven by simulation inaccuracy, matching inaccuracy or spillover effects, we show that not correcting for these three potential sources of bias tends to artificially inflate both the estimated net cost of independence as well as its persistence. Finally, appendix C.2 accounts for the

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

complication that the independence declarations of some NICs in our sample coincided with their transition from a planned to a market economy by parametrically controlling for the estimated transition costs in transition countries that did *not* declare independence, finding that the economic underperformance of newly formed transition countries can in large part be attributed to their independence declaration and *not* to transition costs.

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 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. Obtaining reliable estimates for the relative importance of these channels is complicated, however, by the potential endogeneity of these estimated independence gains and their potential determinants. More specifically, if economic agents in NICs possess prior knowledge on any efficiency gains that are associated with the independence declaration at the time economic decisions are made, this might lead to endogeneity bias if these decisions are partially determined by prior beliefs about the (unobserved) efficiency gain of independence. To address the endogeneity issue, we borrow and adapt an estimator from the total factor productivity literature to parametrically proxy and control for the unobserved efficiency gain of independence, based on the assumption that fixed capital investment decisions of NICs contain useful information on the (perceived) efficiency gain of independence. 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. Implementing this procedure, we find tentative evidence that the cost of independence decreases in the territorial size of the new state, pointing to the presence of economies of scale. In addition, independence costs dissipate when trade barriers fall or democratic institutions improve. We fail to find clear-cut results with respect to the degree of surface area loss, 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 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] In the context of the Soviet breakup, moreover, Suesse (2017, p. 32) finds that prospective secessions may severely disrupt trade flows such that *"the possibility of secession may be enough to deter trade or investment, even without secession actually having taken place"*.

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

In examining the influence of colonial heritage on post-independence economic performance, a different strand of the literature stresses the relevance of the initial conditions left behind by the mother country (Acemoglu, Simon, & Robinson, 2001; Acemoglu, Johnson, & Robinson, 2002). Nunn (2007, 2008), for instance, uncovers a negative relation between

[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."*.

[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.

the number of slaves exported in former African colonies and their current economic performances, suggesting that Africa's underdevelopment since independence can be partially explained by the weakening effect of these slave trades on domestic property right institutions. In a similar spirit, Bertocchi and Canova (2002) conclude that colonial origin lies at the root of contemporary growth differentials in Latin America and Africa due to institutional persistence.[7] 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).

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

This study is also directly related to a relatively small empirical literature that has attempted to uncover the link between state fragmentation and economic performance. Sujan and Sujanova (1994) develop a macroeconomic simulation model to estimate the short-term economic impact of the Czechoslovakian dissolution into the Czech Republic and Slovakia, concluding that the decision to separate reduced GDP by 2.2% in the Czech Republic and by 5.7% in Slovakia. Bertocchi and Canova (2002) rely on a difference-in-difference approach to establish, for a restricted number of former colonies, that there may be substantial growth gains from the elimination of extractive institutions. Somé (2013) relies on a synthetic control approach to demonstrate that former African colonies that declared independence through wars suffer larger income losses than African colonies that declared independence without conflict, at least in the short to medium run. Most recently, Rodríguez-Pose and Stermšek (2015) use panel data on the constituent parts of former Yugoslavia to estimate an independence dividend concluding that, once relevant factors such as war are taken into account, there is no statistically significant relation between achieving independence and economic performance while independence achieved by conflict seriously dents growth prospects. Small sample size and conflicting results, however, limit the extent to which these results can be extrapolated to other instances of

[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 recent summary of this literature.

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 1950-2016. In what follows, 80 of those countries will be referred to as 'established countries', in the sense that these are countries that already gained independence *before* 1950. The remaining 116 countries will be called 'newly independent countries' (NICs), reflecting that these countries declared independence anywhere between 1950 and 2016. To identify the year of independence of each country in the sample, we primarily rely on and extend data on state entry as reported in Griffiths and Butcher (2013). Table 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 per capita number of war deaths as reported by Bethany and Gleditsch (2005) to capture the adverse economic effects associated with the existence, intensity and duration of military conflict.[11] In addition, mimicking Gibler and Miller (2014a), we define a 'political instability'-dummy indicating whether a country experienced a two-standard-deviation change in its democracy score during the previous observation year. To control for the adverse effects of macroeconomic instability, we include 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 (2016) and finally approximate remaining missing observations by use of a third-order polynomial in (i) a country's level of CO2 emissions (World Resources Institute, 2015; World Bank, 2016), (ii) a year dummy and

[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. Nevertheless, they tend to be somewhat less sensitive to military conflict, experience less (known) instances of debt crises and, as suggested in the existing literature, they also tend to more stable politically and favor a more liberal trade regime. All in all, these summary statistics thus suggest that NICs manifest less favorable growth determinants when compared to more established states.

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	4678	7132.38	8447.937	7320	4712.80	10506.23	-2419.59	0.00
Population (millions)	4679	49.24	149.414	7728	9.47	24.999	-39.78	0.00
Years of schooling	4573	6.67	3.245	6587	5.10	3.259	-1.57	0.00
Life expectancy	4507	66.00	10.817	7097	58.45	12.134	-7.55	0.00
Openness	4618	0.54	.421	5771	0.84	.537	0.30	0.00
Battle deaths per 100000 heads	4679	2.34	28.548	7728	1.20	13.804	-1.14	0.01
Population density	4679	95.64	106.019	7684	244.07	1298.429	148.43	0.00
Democracy	4680	23.49	13.786	5536	17.69	10.542	-5.80	0.00
Political instability	4680	0.00	.058	5536	0.00	.03	-0.00	0.01
Macroeconomic instability	4924	0.48	.924	8277	0.09	.442	-0.40	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 Empirical approach

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

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

3.2 A motivating example

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

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

Country	w^*
Malaysia	.585
China	.219
Panama	.097
United States of America	.092
Singapore	.006

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 lower levels witnessed in the rest of the world during this period. Moreover, the synthetic version of Ukraine is also much more similar to the actual pre-independence Ukraine in terms of population, population density, trade openness, educational attainment, life expectancy and the number of battle deaths suffered.

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

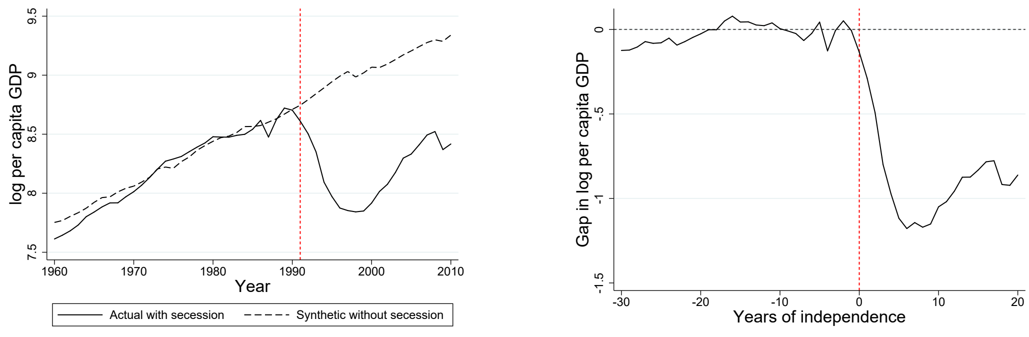
Predictor	*Ukraine*	*Synthetic Ukraine*	*World*
Per capita GDP	5254.475	5331.897	4574.367
log Population	17.745	17.538	18.817
Population density	84.297	83.385	141.961
Educational attainment	9.129	6.855	5.404
Life expectancy	70.054	69.954	64.032
Trade openness	1.019	.957	.297
Battle deaths (per 1000 heads)	0	0.000	0.003

Note: Growth predictors are averaged 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.[15] Combined with the close fit obtained for the pre-independence growth predictors in both groups, as reported in table 3, this suggests that the proposed combination of other independent countries adequately reproduces the economic situation in Ukraine in absence of state fragmentation.

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 logarithmic form, the discrepancy between both reflects the percentage per capita payoff of having declared independence in terms of per capita GDP foregone.

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



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

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

[15]The slight diversion between both series in the pre-independence period suggests 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 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 85% 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 seperately for each available NIC in our sample. The gray lines represent the per capita GDP gaps between each NIC and its respective synthetic version (corresponding to the results displayed in figure 2b) in the period stretching from 10 years before up until 30 years after their declaration of independence. The superimposed black line depicts the yearly 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.[16]

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 0.196, 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 3.4% 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 -29.4%. Interestingly, NICs generally do not appear to recover in the longer run as the negative aggregate independence

[16]Country-specific results are reported in table A6, while appendix C connects the implications of our results to the existing literature on a number of historical instances of state fragmentation.

dividend equals -29.5% 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 30% below those of countries which, in all relevant aspects, most closely resembled their own country's economic situation just prior to its decision to secede.

Nevertheless, figure 3a also indicates that the synthetic control method fails to adequately reproduce per capita GDP trajectories for some NICs in the pre-independence period. East Timor, for instance, is the country with the worst pre-independence fit (RMSPE=1.14). Given its extraordinary low pre-independence per capita GDP trajectory, it should come as no surprise that its growth path cannot be adequately approximated by any linear combination of the available control countries. By extension, this complication applies to all NICs with extreme values in their pre-independence characteristics. As the post-independence gaps of these poorly fitted cases may merely reflect differences in their underlying economic characteristics, rather than actual independence dividends[17], figures 3b to 3d plot the results when the sample is progressively restricted to include only the 80%, 60%, 40% and 20% best matched cases in terms of their pre-independence RMSPE. In each of these trimmed samples, the synthetic control method provides an excellent fit (the associated average RMSPE's equal 0.13, 0.10, 0.07 and 0.06 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 23%-30% in the long run.

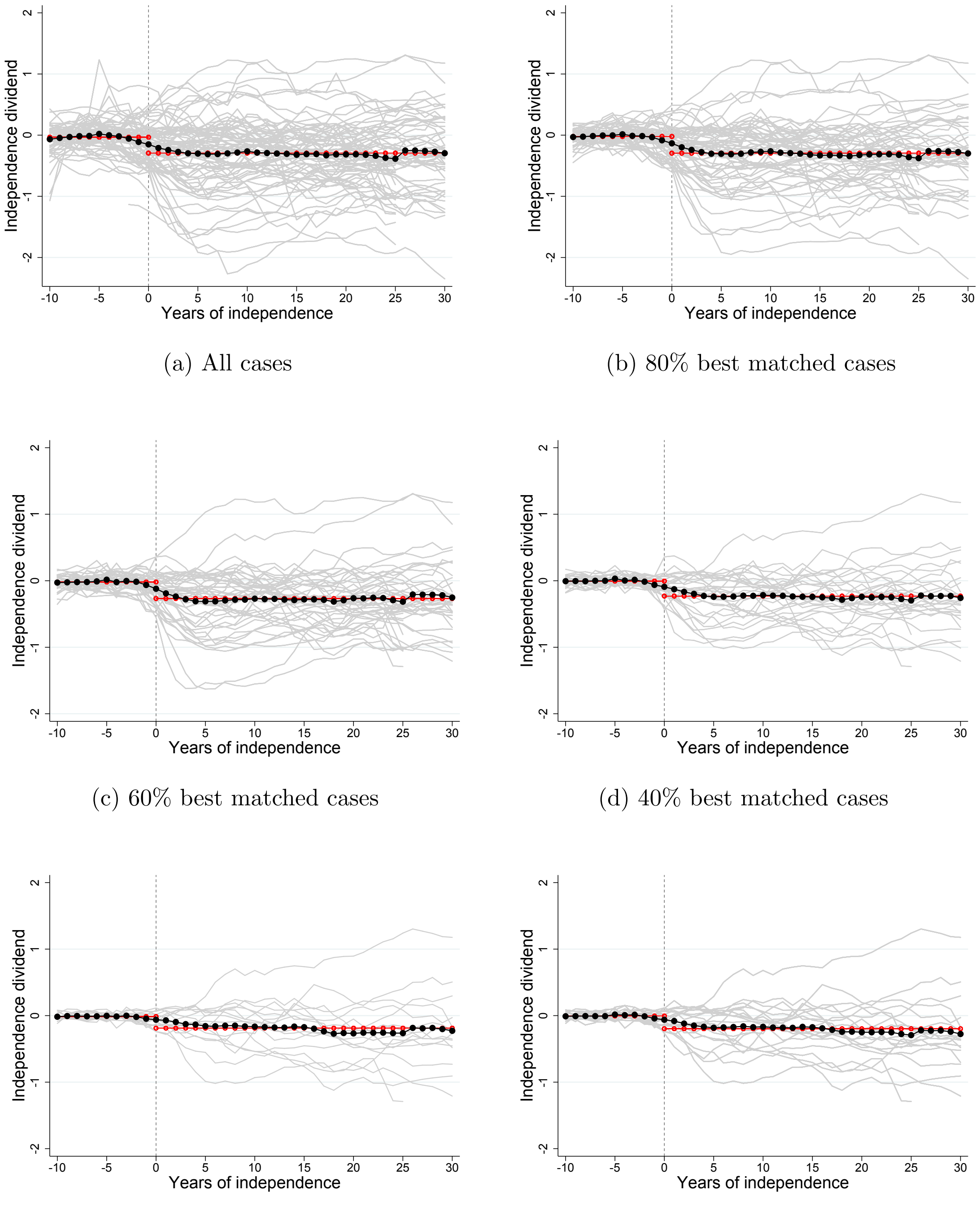
Since there does not appear to be a consensus on the optimal cut-off of pre-independence RMSPE to avoid biases stemming from poor-fit, the bottom figure utilizes a more data-driven procedure to impose a threshold value (or caliper) defining the maximal allowed RMSPE. More specifically, in the tradition of propensity-score matching, Rosenbaum and Rubin (1985) suggest using an optimal caliper of 0.25 times the standard deviation of the linear propensity score. Adapting this to the present context, figure 3f imposes a caliper amounting to 0.5 times the samplewide standard deviation in pre-independence RMSPE. Once again, this results in an excellent pre-independence fit as suggested by the average RMSPE, which now equals 0.06, while our primary conclusions remain robust.

Finally, as a first check on the significance of these results, figure A7 verifies whether a causal interpretation is warranted by their distribution. Plotting the same sequence of aggregate independence dividend estimates along with 95% confidence intervals, we find that the pre-independence per capita GDP discrepancy gradually erodes to become statistically indistinguishable when trimming the sample according to goodness-of-fit.[18] More importantly, these graphs confirm that - irrespective of the selected sample - NICs tend to underperform versus their synthetic versions in the entire post-independence period.

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

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

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



(a) All cases

(b) 80% best matched cases

(c) 60% best matched cases

(d) 40% best matched cases

(e) 20% best matched cases

(f) All cases within caliper

Note: Each gray line plots the yearly 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 80, 60, 40 and 10% best matched cases in terms of their pre-independence RMSPE. The bottom figure includes only those cases for which the pre-independence RMSPE falls within the data-driven caliper cut-off amounting to 0.5 times the samplewide standard deviation in pre-independence RMSPE.

3.4 Statistical inference

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

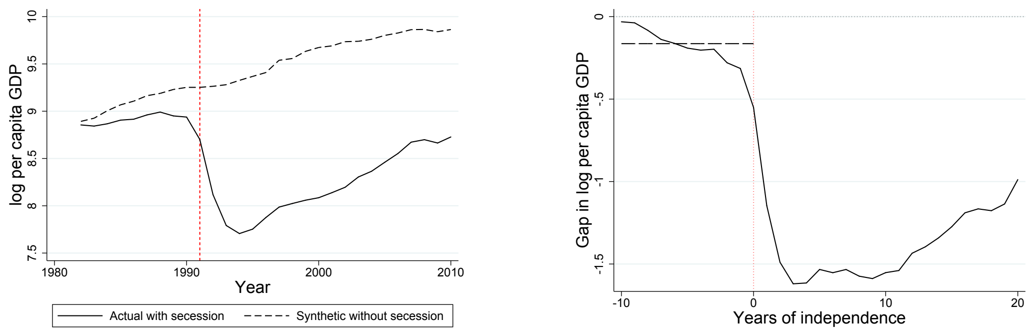
3.4.1 Accounting for matching quality

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

Further illustrating the rationale for this inferential exercise, figure 4 plots the year-on-year per capita GDP discrepancy between Georgia and synthetic Georgia in the period surrounding its 1991 secession from the Soviet Union. In analogy to the Ukranian example discussed in section 3.2, the figure suggests that the Georgian declaration of independence served to lower growth potential in the short but also remained quite persistent over time. As can be seen in figure 4b, however, Georgia slightly underperforms compared to synthetic Georgia even in the pre-independence period. This suggests that the size and compositional limitations associated with the Georgian donor pool of potential control countries produce a synthetic counterfactual which only imperfectly approximates the economic situation of actual Georgia in absence of state fragmentation. More specifically, the dashed line signifies that the typical *pre-independence* per capita GDP discrepancy between both countries amounted to -16%.

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

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

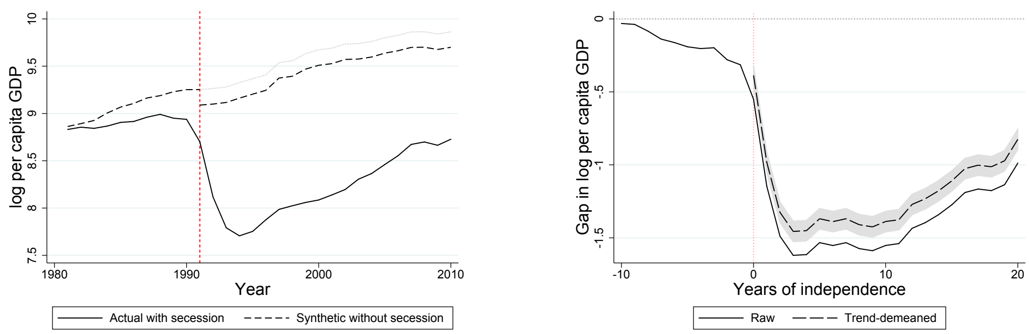


(a) Actual vs. synthetic per capita GDP (b) The economic impact of secession

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

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

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



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

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

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

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

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

Table A6 reports trend-demeaned independence dividend estimates for each available NIC in our sample. Compared to the raw estimates, trend-demeaned estimates tend to be slightly lower in absolute value. Hence, not correcting for matching quality slightly inflates the estimated independence dividend. Nevertheless, trend-demeaned estimates are quantitatively and qualitatively very similar to their uncorrected counterparts. A closer inspection of the results plotted in figure A8 reveals that, irrespective of the time-horizon, roughly 60 to 70% of NICs suffered economic costs of secession even after correcting for matching quality, with the remaining 20 to 30% experiencing a net independence gain.

3.4.2 Accounting for matching & simulation quality

Second, note that the confidence intervals plotted in figures A7 and 5b only express the uncertainty associated with the *magnitude* of the estimated gaps, either across NICs or relative to the pre-independence period. One additional source of uncertainty concerns their *reliability*, which critically hinges on the extent to which synthetic control countries adequately reproduce the per capita GDP trajectories NICs would have experienced in absence of state fragmentation. To the extent that they do not, estimated independence dividends may not only be attributed to the decision to secede but also to poor simulation quality.[21] To study the robustness of the results in this regard, we extend the placebo test approach developed by Abadie et al. (2010) to quantify the probability of obtaining estimates of this magnitude by pure chance. To do so, we reapply the synthetic control method to each potential control country in a particular NIC's donor pool.[22] As the countries involved are unaffected by state breakup by construction, the resulting distribution of 'placebo' dividends is taken to approximate the sampling distribution of the independence dividend estimate under the null hypothesis of a zero effect.

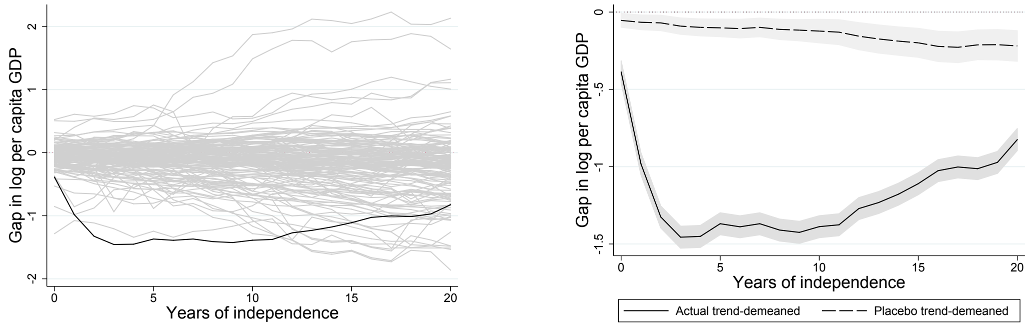
Reconsidering the Georgian example, figure 6 plots the actual trend-demeaned Georgian independence dividends against the distribution of trend-demeand placebo gaps, resulting from an application of the synthetic control algorithm to each of its 154 potential control countries. Although placebo countries tend to under-perform somewhat *vis-à-vis* their synthetic counterparts as well, their per capita GDP trajectories track each other much more closely, especially in the short- to medium run. Moreover, in stark contrast

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

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

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

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



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

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

Nevertheless, placebo countries have a tendency to under-perform vis-á-vis their synthetic counterparts as well. To account for simulation inaccuracies, we assume that the distribution of placebo estimates approximates the sampling distribution of independence dividend estimates under the null hypothesis of a zero effect. Figure 7a corrects the trend-demeaned trajectory of synthetic Georgia by also removing the typical trend-demeaned discrepancy observed in its placebo group. Once again, we find that the Georgian trend- and placebo-demeaned independence dividend trajectory plotted in figure 7b tends to lie well outside the distribution of its trend-demeaned placebo gaps and is unusually negative under the null hypothesis of a zero effect in the short to medium run.[23]

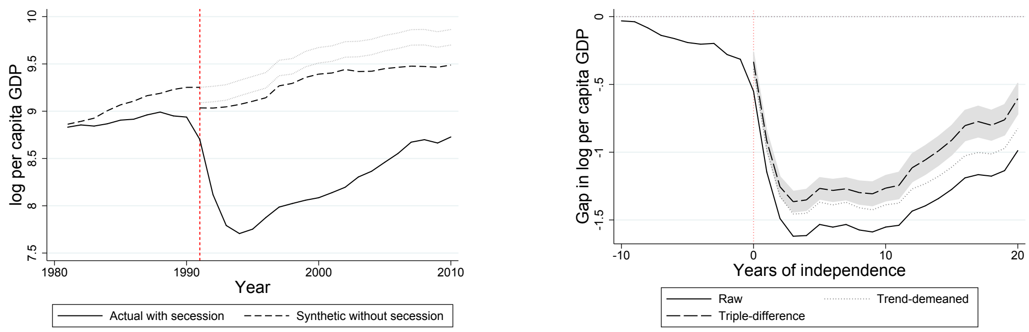
Formally, indexing the control countries in NIC j's donor pool by $k \in [1, \dots, K_j]$, the *triple-difference* estimate of the independence dividend s years after secession is given by

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

Country-specific triple-difference estimates of the independence dividend are reported

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

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



(a) Actual vs. synthetic per capita GDP

(b) The economic impact of secession

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

in table A6. 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 A8 reveals that correcting for matching as well as simulation quality does not qualitatively affect our previous conclusions. Thus, our estimates indicate that declaring independence tended to be costly in the long run for about 45% of the NICs in our sample whereas only 35% of them experienced a long run independence gain.

3.4.3 Accounting for matching & simulation quality & spillover effects

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

Turning once again to the Georgian example, figure 8 plots the parametric decomposition of its triple-difference independence dividend trajectory into a contamination effect and the 'pure', residual economic impact of independence. The figure suggests that contamination effects were persistently negative, and thus partially explain the large Georgian independence cost, especially in the medium run. In addition, the decomposition

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

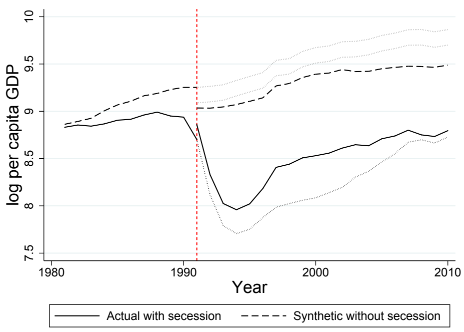
Figure 8: **Decomposing the net independence gain: Georgia**

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

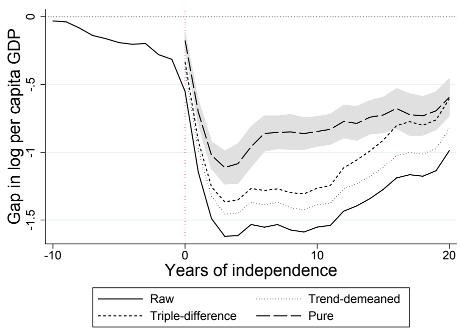
confirms that the pure economic effect of the Georgian independence declaration served to persistently lower growth potential, explaining why Georgia - despite suffering adverse contamination effects - suffered a long-run independence cost.

Conceptually, one can think of this approach as purging the observed Georgian per capita GDP trajectory from contamination effects by removing parametrically estimated contamination effects in the post-independence period, as shown in figure 9a. Figure 9b indicates that the Georgian growth dip is partially driven by economic effects of independence in other recently formed states. In the long run, however, spillover effects peter out and converging evidence points towards a 55% (pure) independence cost.

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



(a) Actual vs. synthetic per capita GDP



(b) The economic impact of secession

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

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

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

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

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

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

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

Country-specific estimates of the pure economic impact of secession, reported in table A6, tend to have the same sign as their triple-difference counterparts while also being slightly lower in absolute value in the short to medium run. Thus, spillover effects mainly appear to affect the economic outlook in NICs in the first 10 post-independence years. Figure A8 illustrates that 30% of NICs appear to have suffered a pure long run economic independence cost while a similar fraction experienced a pure independence gain.

Figure 10 summarizes the implications of our inferential exercise by plotting the various aggregate independence dividend estimates discussed in this section. Irrespective of the estimator, there is a clear pattern of negative independence dividends in the short to medium run while cross-country heterogeneity obscures a clear assessment of the long run independence payoff. The raw estimates seem more sensitive to simulation than to matching inaccuracy, as correcting for simulation quality yields the most pronounced upward correction. Contamination effects primarily seem to negatively affect growth potential in

[25] As the L_j concurrent trajectories may outnumber NIC j's available independence dividend estimates.

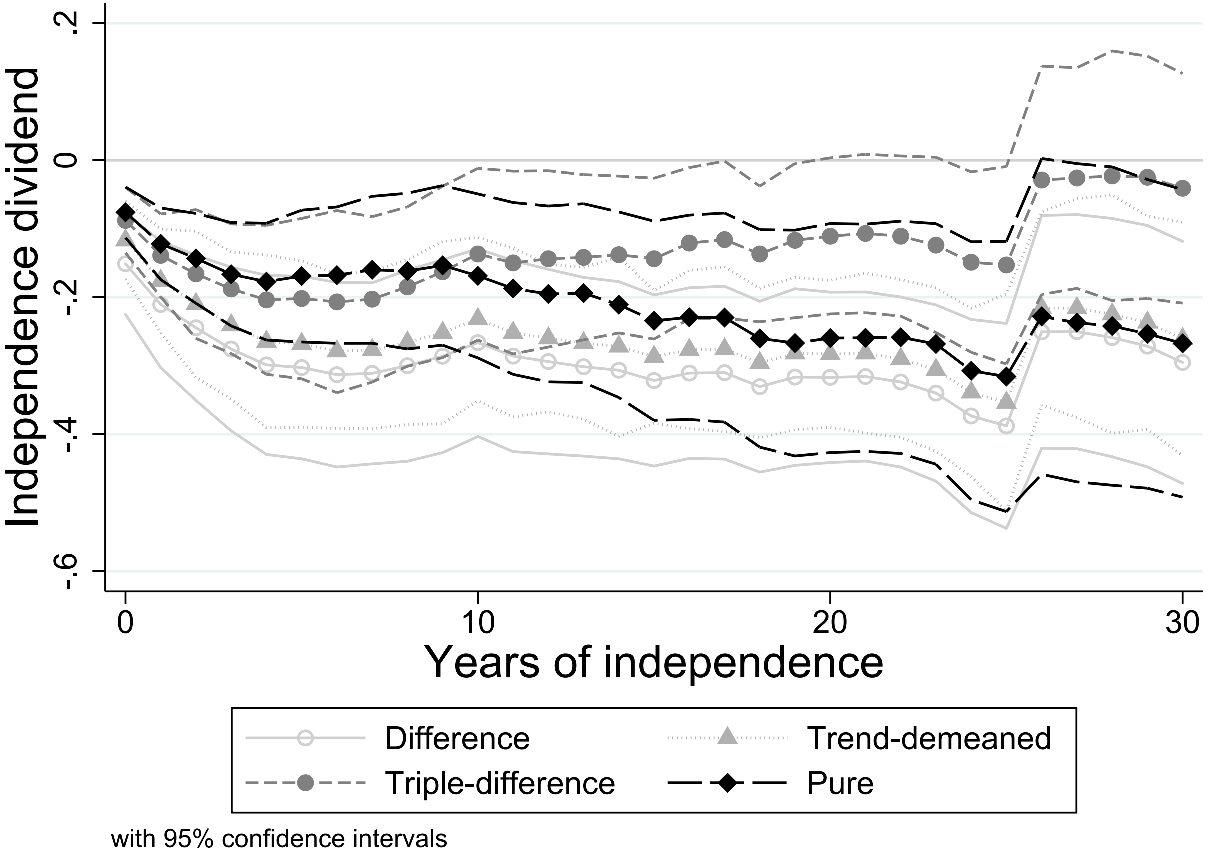
[26] The least angle regression estimator is implemented by Efron et al.'s (2004) *lars*-command in Stata 13.1.

the longer run, when the triple-difference and pure independence dividend estimates start to diverge. Interestingly, estimates of the pure economic impact of secession are fairly stable across bootstrap iterations and do not depend strongly on the time window considered to remove matching inaccuracy, the available potential control countries or the potential contamination effects considered.

Finally, appendix C.2 deals with the complication that the independence declarations of some NICs in our sample coincided with their transition from a planned to a market economy. Indeed, the estimated independence dividends of these transition countries may therefore partially reflect transition costs that would have been born irrespective of their decision to declare independence. Nevertheless, it turns out that parametrically correcting the relevant independence dividend estimates, by removing synthetic control estimates of these transition costs in transition countries that did *not* declare independence, does not qualitatively alter our findings. In sharp contrast, the results indicate that the per capita GDP discrepancies between newly formed transition countries and their synthetic counterfactuals are primarily driven by their decisions to secede and *not* by their transition to a market economy: purging the triple-difference independence dividends from the adverse effects that can plausibly attributed to the transition process leads to a median reduction in the estimated independence cost of around 37%, suggesting that at least 63% of the estimated independence costs for a representative newly independent *transition* country is effectively attributable to its declaration of independence. Thus, our findings reverberate with Linn's (2004) warning that the existing literature on transition in Central and Eastern Europe and the former Soviet Union may overestimate transition costs by neglecting that most countries simultaneously decided to break away from their mother countries.[27]

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

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



Note: The figure plots the yearly average *uncorrected* synthetic control estimates of the independence dividend (hollow circles), as defined in equation (10A), against the corresponding trend difference-in-difference (triangles), triple-difference (circles) and pure (diamonds) estimates related to equations (1), (2) and (4). 95% confidence intervals of the trend-demeaned and triple-difference independence dividend are robust against heteroskedasticity and serial correlation at the country level. Bootstrapped confidence interval of the pure independence dividend based on 250 replications. The number of years after secession is 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. Obtaining reliable estimates for the relative importance of these channels is complicated, however, by the fact that the net efficiency gain of independence remains unobserved. Intuitively, if NICs possess prior knowledge on any efficiency gains that might be associated with the independence declaration at the time economic decisions are made, this might lead to endogeneity bias if these decisions are partially determined by prior beliefs about the (unobserved) efficiency gain of independence.

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. To address the endogeneity issue, it borrows and adapts an estimator that explicitly models the unobserved efficiency gain of independence from the total factor productivity literature.[28] After outlining an estimation strategy to circumvent endogeneity bias by parametrically controlling for it, subsequent sections present the baseline results, robustness checks and a summary of the main findings.

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 40 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 variable indicating episodes of debt and/or banking crises.[29] In addition, although the corresponding estimates are not reported for the sake of brevity, all estimation models control for human capital differences in terms of educational attainment and life expectancy as well as for the transition costs captured by a dummy variable for all successor states to the Soviet Union, Yugoslavia and Czechoslovakia.

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

[29]The robustness checks presented in section 4.3 broaden the scope of the analysis to also consider additional potential channels, at the cost of limiting the available sample size.

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.[30,31] 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. 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, all specifications also include (calendar) year dummies to capture year fixed effects.

One important limitation is that Campos et al. (2014) ignore the potential endogeneity between estimated treatment effects (in this case, the estimated independence dividends) and their potential determinants. To see how this might matter, denote the estimated net gain of independence of NIC i pertaining to the s^{th} post-independence year, which coincides with calendar year t, by $\hat{\beta}_{i,t,s}$ and note that - under the current assumptions - the independence dividends are taken to linearly depend on each potential channel such that their relative importance is determined by the following model:

$$\hat{\beta}_{i,t,s} = \lambda \mathbf{X}_{i,t,s} + \lambda_s \Big(\mathbf{X}_{i,t,s} \times s \Big) + \eta_s + \mu_t + \omega_{i,t,s} + \epsilon_{i,t,s} \tag{5}$$

where $\mathbf{X}_{i,t,s}$ denotes the $(1 \times X)$ vector of standardized predictors of the independence dividend; $\mathbf{X}_{i,t,s} \times s$ denotes their interaction with the S years-of-independence dummies, which allows for a differential relation in each post-independence year; η_s captures the S years-of-independence fixed effects; and μ_t contains the T year fixed effects. The remaining two terms measure deviations of country-specific independence dividends from their expected value. Paraphrasing Ornaghi and Van Beveren (2011, p. 6), the difference between both unobservables is that $\omega_{i,t,s}$ refers to unobserved factors that are observed by NICs and are likely to affect economic decisions (eg. political (in)stability in the NIC) while $\epsilon_{i,t,s}$ collects all random, transitory shocks to the independence dividend unobserved by the NIC (as well as the econometrician) and thus affecting economic performance but *not* economic decisions (eg. unexpected natural disasters). In what follows, we will refer to $\omega_{i,t,s}$ as the 'efficiency gain of independence' noting that it can be either positive (eg. independence reduces political instability and, thus, increases growth potential) or negative (eg. independence increases political instability and, thus, decreases growth potential).[32]

The most important takeaway from equation (5) is that the identification of the un-

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

[31]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.

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

known parameters requires the potential determinants of the independence dividends to be exogenous or, analogously, to be unaffected by the (perceived) efficiency gain of independence, $\omega_{i,t,s}$. Indeed, in this case $\mathbf{E}\left(x_{i,t,s}\omega_{i,t,s}\right) = 0 \; \forall x \in \mathbf{X}$ by assumption and the regression coefficients $\hat{\lambda}$ and $\hat{\lambda}_s$ are unbiased estimates for the true parameters.

Nevertheless, it seems highly unlikely that the potential determinants of the independence dividend are exogenous and that economic agents in NICs disregard any information on the efficiency gain of independence when taking economically relevant decisions. If economic agents have a good knowledge of the efficiency gain of independence, $\omega_{i,t,s}$, and especially if these efficiency gains are persistent, endogeneity arises because economic decisions will partially reflect beliefs about $\omega_{i,t,s}$.[33] More specifically, assume that the efficiency gain of independence follows a first order Markov process such that

$$\omega_{i,t,s} = \mathbf{E}\left(\omega_{i,t,s} \mid \omega_{i,t-1,s-1}\right) + \xi_{i,t,s} = g\left(\omega_{i,t-1,s-1}\right) + \xi_{i,t,s} \tag{6}$$

with $g(\cdot)$ an unknown function and $\xi_{i,t,s}$ a surprise news component unforeseen at time $t-1$. This allows us to rewrite equation (5) as

$$\hat{\beta}_{i,t,s} = \lambda \mathbf{X}_{i,t,s} + \lambda_s \Big(\mathbf{X}_{i,t,s} \times s\Big) + \eta_s + \mu_t + g\left(\omega_{i,t-1,s-1}\right) + \xi_{i,t,s} + \epsilon_{i,t,s} \tag{7}$$

Formally, endogeneity arises whenever economic agents in a specific NIC know the (expected) efficiency gain of independence at the time economically relevant decisions are made such that the efficiency gain simultaneously affects economic performance, $\beta_{i,t,s}$, and the decisions contained in $\mathbf{X}_{i,t,s}$. For instance, NICs may reap the benefits of increasing efficiency gains of independence by opening up to trade, introducing an upward bias in the value of the coefficient estimate for the relative importance of trade openness.[34]

A similar simultaneity issue has long been the central focus of the vast methodological literature surrounding total factor productivity estimation, which at least dates back to the seminal work by Marschak and Andrews (1944). Olley and Pakes (1996) were the first to solve this issue by explicitly controlling for the unobserved confounder using proxy variables. Modifying their approach to fit our purposes, our identification strategy relies on the assumption that the fixed capital stock of a NIC, $K_{i,t,s}$, is fully determined by choices made in period $t-1$ through the following law of motion:[35]

$$K_{i,t,s} = \left(1 - \delta_t\right) K_{i,t-1,s-1} + I_{i,t-1,s-1} \tag{8}$$

[33] If efficiency gains of independence are not persistent, $g\left(\omega_{i,t-1,s-1}\right)$ would drop in equation (7) such that endogeneity would only arise if the potential determinants of the independence dividend depend on the 'surprise' news component.

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

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

where δ_t captures the yearly depreciation rate of the fixed capital stock and $I_{i,t,s}$ measures gross fixed capital formation. Note that this law of motion assumes that it takes a full year for fixed capital investments to translate into fixed capital. Crucially, this implies that both fixed capital ($K_{i,t,s}$) as well as fixed capital investments ($I_{i,t,s}$) depend on the expected efficiency gain of independence in year t, $g(\omega_{i,t-1,s-1})$, as higher expected efficiency gains should make it more profitable to increase the fixed capital stock. In addition, fixed capital investments may or may not be correlated with the efficiency shock ($\xi_{i,t,s}$) depending on the specific timing assumptions: if fixed capital investments are chosen before the realization of the surprise news component in year t, they are uncorrelated by assumption and we have that $I_{i,t,s} = f\Big(K_{i,t,s}, g\left(\omega_{i,t-1,s-1}\right)\Big)$; if, on the other hand, capital investments are chosen after the realization of the news component, investment will also depend on the efficiency shock and we have that $I_{i,t,s} = f\left(K_{i,t,s}, \omega_{i,t,s}\right)$.

Under the assumption that investment in fixed capital is strictly increasing in the unobserved efficiency gain of independence, this suggests proxying the efficiency gain of independence by inverting the investment demand function.[36,37] Depending on whether fixed capital investment decisions are assumed to be made before or after the realization of the news component, the unobserved efficiency gain of independence is defined as

$$\begin{aligned} \omega_{i,t,s} &= f^{-1}\left(K_{i,t,s}, I_{i,t,s}\right) \quad \text{if} \quad \mathbf{E}\left(I_{i,t,s}\xi_{i,t,s}\right) \neq 0 \\ g\left(\omega_{i,t-1,s-1}\right) &= f^{-1}\left(K_{i,t,s}, I_{i,t,s}\right) \quad \text{if} \quad \mathbf{E}\left(I_{i,t,s}\xi_{i,t,s}\right) = 0 \end{aligned} \tag{9}$$

Intuitively, the fixed capital investment decisions of NICs in addition to their existing fixed capital stock are thus taken to contain useful information on the (perceived) efficiency gain of their independence declaration at a certain point in time. In this sense, the control function can be considered to proxy for (unobserved) 'business sentiment' or 'confidence in the economic future' in the immediate post-independence period. This, in turn, suggests adding the control function, $f^{-1}\left(K_{i,t,s}, I_{i,t,s}\right)$, to regression equation (7) to control for simultaneity bias. Again, the specific implementation depends on the specific timing assumptions. If the realization of the news component is assumed to occur *before* fixed capital investment decisions are made, such that $\mathbf{E}\left(I_{i,t,s}\xi_{i,t,s}\right) \neq 0$, equation (9) allows us to express both the expected efficiency gain ($g\left(\omega_{i,t-1,s-1}\right)$) as well as the the innovation in the efficiency gain of independence ($\xi_{i,t,s}$) as a function of observables and hence to control

[36]If this monotonicity assumption would be violated, it would be impossible to map every potential value of $I_{i,t,s}$ to a unique value for the unobserved efficiency gain of independence, $\omega_{i,t,s}$, which would essentially invalidate this estimation procedure. For similar reasons, inversion of f also requires that the efficiency gain of independence is the *only* unobservable entering the inversion function (Ornaghi & Van Beveren, 2011). As a robustness check, section 4.3 relaxes the assumption of scalar unobservability by allowing investment demand to depend on a number of growth determinants.

[37]Given its central role in the estimation procedure, Ornaghi and Van Beveren (2011) propose a monotonicity test to verify to what extent the monotonicity assumption holds in the actual data. The results for our data are discussed in appendix D.2.

for $\omega_{i,t,s}$ by simply adding this control function to equation (7) as follows:

$$\hat{\beta}_{i,t,s} = \lambda \mathbf{X}_{i,t,s} + \lambda_s \Big(\mathbf{X}_{i,t,s} \times s \Big) + \eta_s + \mu_t + f^{-1} \left(K_{i,t,s}, I_{i,t,s} \right) + \epsilon_{i,t,s} \tag{10}$$

If the realization of the news component is assumed to occur *after* fixed capital investment decisions are made, such that $\mathbf{E} \left(I_{i,t,s} \xi_{i,t,s} \right) = 0$, equation (9) only allows us to express the *expected* efficiency gain $\Big(g \left(\omega_{i,t-1,s-1} \right) \Big)$, but *not* the news component $(\xi_{i,t,s})$, as a function of observables. This complicates the estimation process, because simply adding the control function to equation (5) only removes part of the endogeneity problem:

$$\hat{\beta}_{i,t,s} = \lambda \mathbf{X}_{i,t,s} + \lambda_s \Big(\mathbf{X}_{i,t,s} \times s \Big) + \eta_s + \mu_t + f^{-1} \left(K_{i,t,s}, I_{i,t,s} \right) + \xi_{i,t,s} + \epsilon_{i,t,s} \tag{11}$$

Note that the potential determinants of the independence dividend, $\mathbf{X}_{i,t,s}$, could still be correlated with the news component, $\xi_{i,t,s}$, in this scenario. More specifically, this would be the case if the realization of the news component occurs *before* the economic decisions contained in the $\mathbf{X}_{i,t,s}$-vector are taken but *after* fixed capital investment decisions are made. Given that the potential determinants of the independence dividend, $\mathbf{X}_{i,t,s}$, may still depend on the news component, $\xi_{i,t,s}$, which only arrives between time $t-1$ and time t, the potential determinants of the independence dividend contained in the $\mathbf{X}_{i,t,s}$-matrix need to be instrumented with their lags to avoid the endogeneity issue. The corresponding two-stage least squares estimation model in this scenario can be written as:

$$\begin{aligned} \hat{\beta}_{i,t,s} &= \lambda \tilde{\mathbf{X}}_{i,t,s} + \lambda_s \Big(\tilde{\mathbf{X}}_{i,t,s} \times s \Big) + \eta_s + \mu_t + f^{-1} \left(K_{i,t,s}, I_{i,t,s} \right) + \xi_{i,t,s} + \epsilon_{i,t,s} \\ \tilde{\mathbf{X}}_{i,t,s} &= \alpha \bar{\mathbf{X}}_{i,t-1,s-1} + \nu_{i,t,s} \end{aligned} \tag{12}$$

where $\bar{\mathbf{X}}_{i,t-1,s-1}$ denotes the $(X \times X)$ matrix containing the lagged values of the $(1 \times X)$ vector of standardized predictors of the independence dividend in each row and $\nu_{i,t,s}$ is the iid component capturing unobserved factors. Note that $\tilde{\mathbf{X}}_{i,t,s}$ is orthogonal to $\xi_{i,t,s}$ due to the timing assumptions, since $\xi_{i,t,s}$ is realized between $t-1$ and t while $\tilde{\mathbf{X}}_{i,t,s}$ only contains information pertaining to time $t-1$.

Estimation of (10) or (12) is further complicated by the fact that $f^{-1} \left(K_{i,t,s}, I_{i,t,s} \right)$ has an unknown functional form. To proceed, in line with Olley and Pakes (1996), we assume that it can be approximated by polynomial expansion of order J such that $f^{-1} \left(K_{i,t,s}, I_{i,t,s} \right) \approx \sum_{j=0}^{J} \sum_{m=0}^{J-j} \alpha_{j,m} K_{i,t,s}^{j} I_{i,t,s}^{m}$.[38] More specifically, this implies that the single-stage least squares model summarized in equation (10) can be implemented by es-

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

timating

$$\hat{\beta}_{i,t,s} = \lambda \mathbf{X}_{i,t,s} + \lambda_s \Big(\mathbf{X}_{i,t,s} \times s \Big) + \eta_s + \mu_t + \sum_{j=0}^{J} \sum_{m=0}^{J-j} \alpha_{j,m} K_{i,t,s}^{j} I_{i,t,s}^{m} + \epsilon_{i,t,s} \tag{13}$$

while the two-stage least squares model formally described in equation (12) can similarly be implemented by estimating

$$\begin{aligned} \hat{\beta}_{i,t,s} &= \lambda \tilde{\mathbf{X}}_{i,t,s} + \lambda_s \Big(\tilde{\mathbf{X}}_{i,t,s} \times s \Big) + \eta_s + \mu_t + \sum_{j=0}^{J} \sum_{m=0}^{J-j} \alpha_{j,m} K_{i,t,s}^{j} I_{i,t,s}^{m} + \xi_{i,t,s} + \epsilon_{i,t,s} \\ \tilde{\mathbf{X}}_{i,t,s} &= \alpha \bar{\mathbf{X}}_{i,t-1,s-1} + \nu_{i,r,s,t} \end{aligned} \tag{14}$$

Equations (13) and (14) summarize the estimation procedures used to eliminate simultaneity bias by explicitly controlling for the efficiency gain of independence under the assumption that efficiency shocks occur, respectively, *before* and *after* fixed capital investments are made. Since there is no way to verify which timing assumptions for the dynamic relation between fixed capital investments and the realization of the news component hold in the data at hand, namely whether $\mathbf{E}\left(I_{i,t,s}\xi_{i,t,s}\right) = 0$ or $\mathbf{E}\left(I_{i,t,s}\xi_{i,t,s}\right) \neq 0$, our approach is to estimate both models and compare the results.[39] Nevertheless, note that the two-stage least squares model of equation (14) is most robust against endogeneity since it yields unbiased estimates under *both* timing assumptions, while the single-stage least squares model of equation (13) is only unbiased if $\mathbf{E}\left(I_{i,t,s}\xi_{i,t,s}\right) \neq 0$.

In the robustness checks, we also experiment with polynomials for the efficiency gain of independence of different orders to verify the stability of the results. As the results remain qualitatively unchanged irrespective of the order of the polynomial, we adopt the standard practice in the total factor productivity literature and use a third order polynomial in our baseline model. In addition, to account for the fact that our dependent variable is itself an estimated parameter, we compute standard errors by a block bootstrapping procedure that comprises re-sampling over NICs to preserve as much as possible the time-dependency structure of the estimated independence gains and to gain some insight in the variability of the results over the NICs in our sample.

Finally, 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:

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

[39] Appendix D conducts a number of specification tests finding no evidence of critical identification assumptions being violated in our sample but detecting moderate evidence for the presence of endogeneity, suggesting that a bias-corrected estimator may be necessary to obtain unbiased estimates.

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 equations (13) and (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.

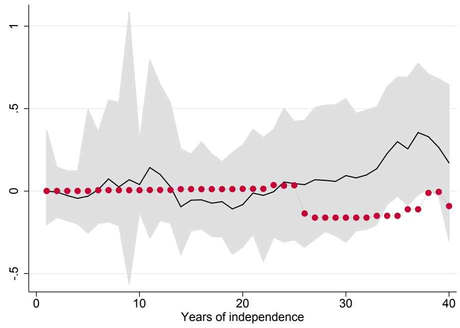
4.2 Baseline results

Figure 11, 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 estimates for the relative importance of these potential determinants along with their 90% confidence intervals, as estimated by the two-stage least squares model summarized in equation (12), while the red lines indicate the yearly average values of these standardized predictors actually observed in our sample.[40] As a first important observation, note that the relative importance of these channels can generally not be precisely estimated due to the small number of observations and the additional uncertainty emanating from the fact that the dependent variable is itself an estimated variable. As a result, this section will have to settle for more speculative findings that mainly serve to identify interesting avenues for further research.

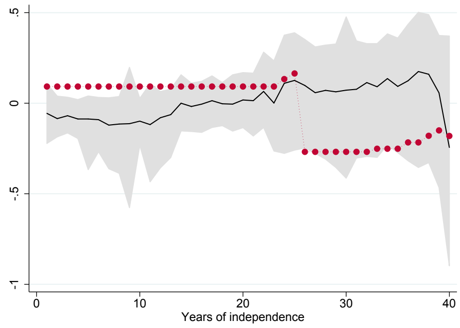
Keeping this in mind, however, several explanations to account for the observed variation in the estimated net gains of secession are confirmed. First, figure 11a indicates that state size positively affects the long-run growth potential of newly formed states as larger-sized NICs have a tendency to outperform their smaller-sized counterparts over time. At the same time, we also obtain positive estimates for the effect of trade openness, 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 11c) 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 10. In line with the endogenous growth literature, these results are consistent with the hypothesis that smaller NICs suffer more adverse economic consequences of secession due to the presence of economies of scale. Nevertheless, figure 11b fails to find evidence that indpendence costs also increase in the degree of surface area loss, suggesting that NICs sacrificing larger territories are generally not outperformed by NICs that retained more territory after independence.

[40] The model also controls for human capital differences between NICs, in terms of life expectancy and educational attainment, and the variability in the independence dividend that might reasonably be attributed to transition costs, see section 4.1. For brevity, full results are not reported in figure 11.

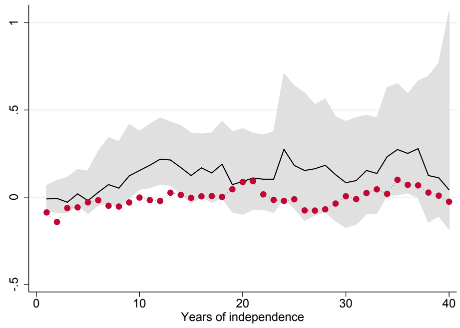
Figure 11: **Determinants of the independence dividend (baseline)**



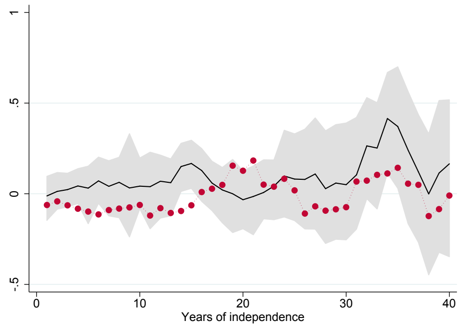
(a) Surface area



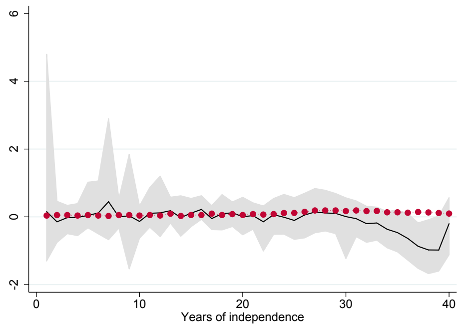
(b) Surface area loss



(c) Trade openness

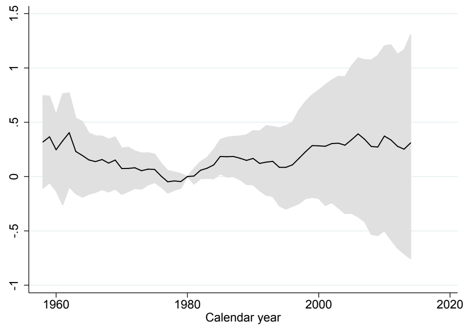


(d) Democracy

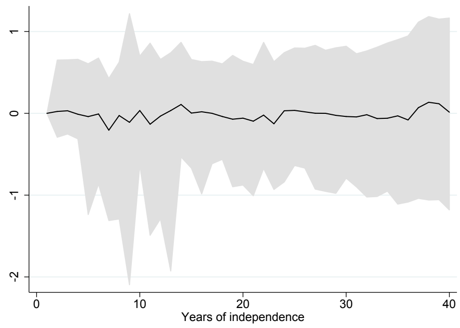


(e) Macroeconomic uncertainty

(f) Battle deaths



(g) Year fixed effects



(h) Years-of-independence fixed effects

Note: This figure plots estimates of the relative importance, as defined in equation (15), of several determinants of the triple-difference independence dividend (black line) against their sample-average values (red lines) in a 40-year period following an independence declaration. The relative importance is estimated by the two-step estimator described in equation (12) and contains a third-order polynomial in fixed capital and gross fixed capital formation to control for endogeneity. 90% block-bootstrapped confidence intervals, clustered at the country level and based on 500 replications, are plotted in gray. For reference, the bottom row plots the 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. Controls for human capital differences and transition costs are included but not reported.

More interestingly, our results are consistent with democratization being a second channel through which new states can reduce the adverse effects of secession. This corroborates prior empirical evidence that democracy does cause growth, to paraphrase Acemoglu, Suresh, Restrepo, and Robinson (2014). Figure 11e also finds tentative evidence the adverse long-run effects of declaring independence might be aggravated when these decisions are followed by instances of sovereign debt default or banking crises. 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.

Finally, it may be useful to compare these standard deviation elasticities with the year and years-of-independence fixed effects. In contrast to the existing literature, figure 11g finds no evidence of global trade liberalization gradually lowering the economic costs of independence as there is no clear persistent upward trend in the estimated year fixed effects. Quite the contrary, the point estimates seem to indicate that declaring independence was *most* costly in 1980. Finally, the last figure shows that independence dividends, all else equal, do not appear to slowly erode in the longer run.

In conclusion, we find tentative evidence that the adverse effects of independence are decreasing in the size of newly formed states and that they are also mitigated when opening up to trade or building more democratic institutions. Nevertheless, independence costs also appear to increase in the occurrence of banking and debt crises. In comparison, the relative importance of the beneficial effects of trade openness and democratization process seems to be outweighed by the adverse effects associated with macroeconomic uncertainty. At first glance, these results thus confirm the central importance of scale economies in determining the independence dividend but also point towards the importance of containing the risks of persistent democratic decay and - especially - of macroeconomic uncertainty in the post-independence period. Finally, our results indicate that these four channels appear to mainly play a role in the longer run, suggesting that it might take a while before good policy decision bear economic fruit after an independence declaration.

4.3 Robustness checks

The most important limitation of the control function approach is that it critically hinges on the assumption of scalar unobservability or, to be more specific, on the assumption that national investment in fixed assets *only* depends on the fixed capital stock and the unobserved efficiency gain of independence. Indeed, only in this case do the contemporary values for gross fixed capital formation and the fixed capital stock contain sufficient information to accurately gauge the perceived efficiency gain of independence.

Although a violation of this scalar unobservability assumption should have been picked up in the monotonicity checks presented in figure A5, which offer no evidence in this regard, a first robustness check nevertheless aims to more explicitly verify the justifiability of this identifying assumption through a stability test. The idea of this test is to construct a more reliable proxy for the perceived efficiency gain of independence, namely by also taking into account that the growth determinants under consideration *themselves* might have influenced gross fixed capital formation in NICs, and to subsequently verify the stability of the estimation results when utilizing this more reliable proxy to control for the perceived efficiency gain of independence. Intuitively, if the assumption of scalar unobservability would be justified in the baseline model, improving the reliability of the proxy for the perceived efficiency gain of independence by also accounting for additional observables that may affect gross fixed capital formation in NICs should not meaningfully affect the relative importance estimates for the potential determinants of the independence dividend. In this sense, stability of the relative importance estimates irrespective of the choice of the control function would be indicative of the justifiablity of the scalar unobservability assumption and reinforce the credibility of the baseline results.

The technical details are relegated to appendix E, but the main idea is to allow fixed capital investment demand of NIC i in year t and independence year s to also depend on the $(1 \times X)$ vector of standardized predictors of the independence dividend, in addition to the value of the existing capital stock. We thus proxy the unobserved efficiency gain of independence by inverting the extended investment demand function as follows

$$\omega_{i,t,s} \quad = f^{-1}\left(K_{i,t,s}, I_{i,t,s}, \mathbf{X}_{i,t,s}\right) \tag{16}$$

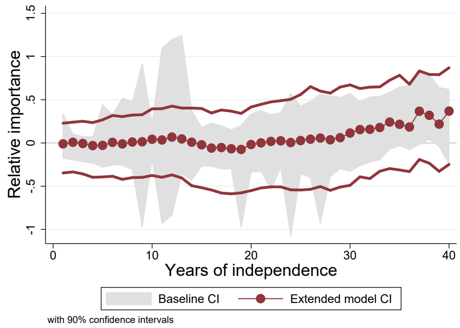
Crucially, this implies that the scalar unobservability assumption now relaxes to assuming that, *conditional* on these standardized predictors of the independence dividend, fixed capital investment demand *only* depends on the current value of the capital stock and the perceived efficiency gain of independence. Note that this is a less stringent identification assumption than the one underpinning the baseline model. Assuming that the $f^{-1}(.)$ function can be approximated parametrically by the linear, squared and cubic values of all X growth determinants in addition to the polynomial expansion of order 3 in $K_{i,t,s}$ and $I_{i,t,s}$, estimation of the relative importance of the standardized predictors of the independence dividend proceeds by estimating

$$\begin{aligned}
&\hat{\beta}_{i,t,s} = \lambda\tilde{\mathbf{X}}_{i,t,s} + \lambda_s\left(\tilde{\mathbf{X}}_{i,t,s} \times s\right) + \eta_s + \mu_t + \sum_{j=0}^{3}\sum_{m=0}^{3-j}\alpha_{j,m}K^{j}_{i,t,s}I^{m}_{i,t,s} + \sum_{x=1}^{X}\sum_{j=0}^{3}\theta_{x,j}x^{j}_{i,t,s} + \xi_{i,t,s} + \epsilon_{i,t,s} \\
&\tilde{\mathbf{X}}_{i,t,s} = \alpha\bar{\mathbf{X}}_{i,t-1,s-1} + \nu_{i,r,s,t}
\end{aligned} \tag{17}$$

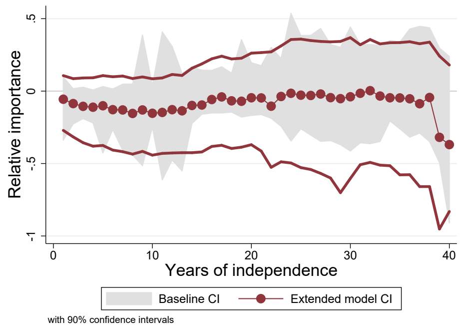
One drawback of this approach is that, as explained in appendix E, identification of the relative importance of each potential determinant of the independence dividend now requires an additional step in the estimation process, increasing measured uncertainty.[41]

[41] Intuitively, the collinearity of the standardized predictors of the independence dividend with the inverted investment demand function summarized in equation (17) now requires an additional step in the estimation procedure to separate out the relative importance of these standardized predictors, λ, from their effect on fixed capital investment demand, θ.

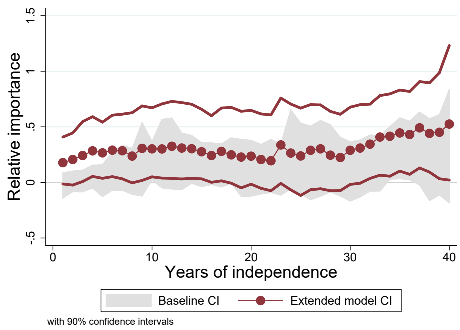
Figure 12: **Determinants of the independence dividend (robustness)**



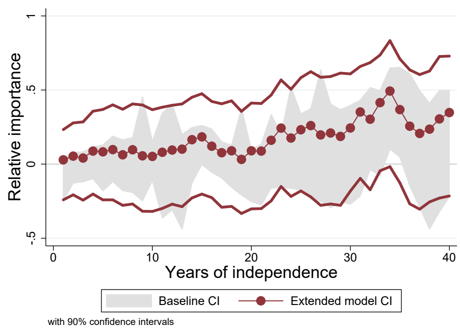
(a) Surface area



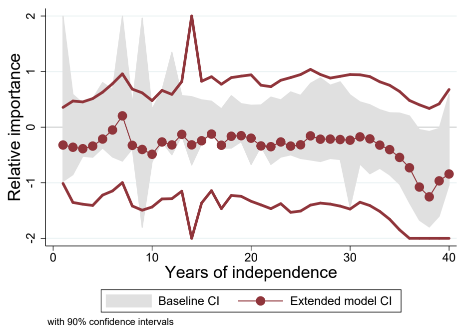
(b) Surface area loss



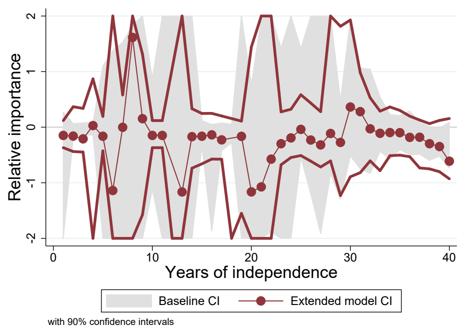
(c) Trade openness



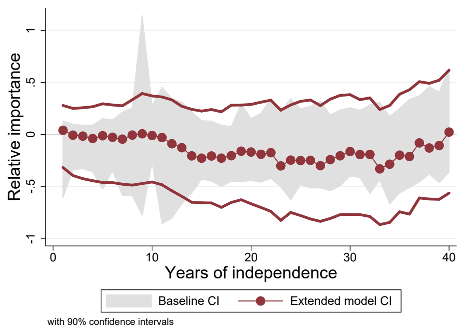
(d) Democracy



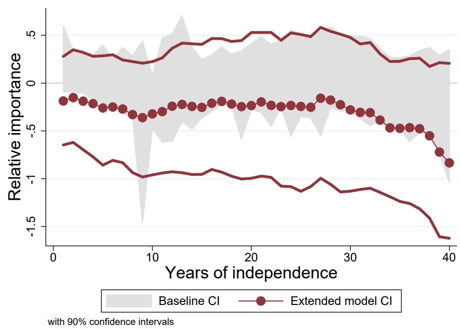
(e) Macroeconomic uncertainty



(f) Battle deaths



(g) Year fixed effects



(h) Years-of-independence fixed effects

Note: This figure plots estimates of the relative importance, as defined in equation (15), of several determinants of the triple-difference independence dividend (full lines) against the baseline results derived in section 4.2 (grey areas) in a 40-year period following secession. Relative importance is estimated by the non-linear least squares estimator described in equation (22A). 90% block-bootstrapped confidence intervals are clustered at the country level and based on 500 replications. For reference, the bottom row plots the 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. Controls for human capital differences and transition costs are included but not reported.

Figure 12 reports the relative importance estimates emanating from an application of this extension of the control function approach summarized in equation (17). The full red lines plot the temporal evolution of the estimated relative importance of the potential determinants of the independence dividend along with their 90% block-bootstrapped confidence intervals. For reference, the grey area plots the 90% confidence intervals of our baseline estimates, which were obtained by estimating the model defined in equation (12) including a third order polynomial in $I_{i,t,s}$ and $K_{i,t,s}$.

As can be seen, the confidence intervals of the extended model now reflect the extra uncertainty stemming from the additional step in the estimation procedure. Nevertheless, the extended model also finds clear evidence for the growth-enhancing effects of post-independence trade openess (figure 12c). The increase in measured uncertainty does, however, obscure the growth-enhancing effects of state size (figure 12a) and democracy (figure 12d) as well as the growth-inhibiting effects of macroeconomic uncertainty (figure 12e). No evidence is found for the relevance of the degree of surface area loss and the number of battle deaths. More importantly, though, both estimates for the relative importance of these potential determinants of the independence dividend are statistically indistinguishable from each other. The stability test thus does not offer empirical evidence suggesting the violation of scalar unobservability in the baseline regressions.

Finally, the appendix also report several additional robustness checks. First, we consider the sensitivity of our estimates with respect to the specific estimator that is used to control for endogeneity bias. Figure A9 therefore plots the estimated coefficients when using the other estimators discussed in section 4.1 and obtains similar results. Our previous conclusions thus seem to hold irrespective of which bias-correcting procedure we employ to control for endogeneity bias. Furthermore, as can be derived from the red lines in the figure, there is not much visual evidence of endogeneity bias in this dynamic model, as the uncorrected least squares estimates tend to coincide with their bias-corrected counterparts.

Subsequently, we verify the sensitivity of the results with respect to the specific first-step estimation procedure utilized to estimate the independence dividend, by sequentially replacing the triple-difference estimates with their raw, trend-demeaned and placebo-demeaned counterparts in our baseline regression, see figure A10. Once again, we obtain broadly similar results, allowing us to conclude that our prior findings hold irrespective of the first-step estimation procedure utilized to estimate the independence dividend.

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 A11. 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.

An important limitation is that the current set-up does not account for residual differences between NICs. Note, however, that the control function does allow us to parametrically control for *all* unobserved differences between NICs that are reflected in gross capital investment decisions, which should severely limit the risk of omitted variable bias. Nevertheless, to verify the sensitivity of our results to omitted variable bias, we extend the model by including various vectors of additional control variables. These extended models also allow us to shed some light on the relevance of some additional potential channels at the cost of reducing the sample size and increasing the variance of the estimates.[42]

Figure A12 displays the results of adding per capita GDP to verify whether the impact of secession differs in the degree of economic development, finding tentative evidence that richer regions experience less pronounced independence costs. Figure A13 shows the result of further adding two proxies for the quality of institutions, as proxied by an index that quantifies constitutional similarity to the US between 0 (no similarity) and 1 (identical) derived from Eicher and Kuenzel (2017) and state capacity as quantified by the national material capabilities (NMC) index developed by Correlates of War Project (2010). In addition, a dummy variable indicating whether a NIC gained independence by referendum is added to verify the claim that independence-by-referendum is preferable from an economic point of view (Qvortrup, 2014). We do not find evidence that constitutional features or declaring independence by referendum mitigates adverse economic consequences, nor do we find state capacity to be meaningfully related to the independence dividend. The latter finding is in line with the hypothesis that increased international military cooperation after 1945 allowed military weaker NICs to declare independence without having to bear the full costs for defending their integrity (Fazal & Griffiths, 2014). Our prior findings remain qualitatively unchanged in both exercises despite the reduction in sample size.

Subsequently, figure A14 demonstrates how these findings also remain qualitatively unaltered when we add a battery of fixed effects to the baseline model, where in addition to a set of region dummies we add dummy variables indicating membership to the EU, the OPEC, the NATO, the African Union, ASEAN as well as succesor states to the Soviet Union and Yugoslavia. Finally, notice that we can also estimate a more restrictive model that eliminates all the - potentially confounding - variation in time-invariant covariates. Figure A15 re-estimates the baseline model but now includes country fixed effects instead. Unsurprisingly, this manipulation causes the relative importance of our time-varying predictors to be less precisely estimated. Nevertheless, the beneficial long-run effects of increased trade openness and democratization remain visible in this model.

Finally, appendix F provides a bird's eye view of all second-step estimation results by reporting 'agnostic' estimates for the determinants of the independence dividend, that are calculated as the average estimates across all available estimation models, finding that

[42] Note that the sample size differs over the various models discussed in the following sections, due to the larger number of missing variables for a number of the additional control variables. Therefore, strictly speaking, there is no direct comparison between the results below and our baseline results as any discrepancy could be either due to the inclusion of the additional control variables, the composition of the estimation sample, or both. We ignore this slight complication in the text.

the relative importance estimates pertaining to the beneficial effects of post-independence territorial size, democracy and trade are remarkably stable across estimation procedures.

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 the loss in 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 military conflict and macroeconomic uncertainty. We find clear evidence that increased trade openness and ongoing processes of democratization bolstered growth potential in these newly formed states mitigating - at least partially - independence costs. The growth-enhancing effects of trade openness and democracy moreover appear to increase over the lifespan of NICs, signaling that it might take time before reforms bear economic fruit. The consistent patterns of variation in the independence payoffs across a number of estimation procedures that aim to control for endogeneity and omitted variable bias make these channels prime candidates for further research.

5 Conclusion

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

Relying on a semi-parametric estimation strategy to control for the confounding effects of past GDP dynamics, anticipation effects, unobserved heterogeneity between newly formed and more established states, model uncertainty as well as effect heterogeneity, we present robust evidence that secession statistically significantly hampers growth potential in newly formed states. Our central results suggest that the decision to secede reduced per capita GDP in NICs anywhere between 20% and 30% in the long run. From a methodological perspective, we develop a novel quadruple-difference procedure that sequentially accounts for matching quality, simulation quality and contamination effects, providing informative statistical inference on the reliability of synthetic control estimates of treatment effects. Applying this procedure, we confirm the existence of a statistically significant negative independence dividend in the short to medium run, with cross-country heterogeneity obscuring the average long-run impact of independence. Moreover, an empirical extension indicates that the severe economic underperformance of newly formed *transition* countries cannot be plausibly attributed to their transitions from planned to market economies, suggesting instead that the bulk of their output losses stem from their decisions to secede.

As a first taste of how these estimates can be exploited to gain more insight in the drivers of the independence dividend, we develop a two-step estimator to identify the primary channels through which secessionist processes influenced per capita GDP trajectories, combining both parametric and semi-parametric techniques to control for endogeneity. In line with much of the existing literature, we find tentative evidence that the adverse effects of independence decrease in the territorial size of the new state, 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.

In light of these findings, two additional future research questions naturally arise. First, do these results generalize to other regions contemplating independence today? Indeed, since we estimate the average treatment effect on the treated, extrapolation to contemporary and future NICs may be problematic to the extent that these differ non-trivially from the historical cases of state fragmentation considered in this analysis.[43] Second, do these findings generalize to the non-economic spectrum? This also remains an open question, as independence may come with compensating political (Alesina & Spolaore, 1997, 2003), re-distributional (Bolton & Roland, 1997) or other effects.

[43] The two-step findings discussed in the previous paragraph, however, are intended to offer a partial answer to this question.

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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 for the main variables of interest (indicated by $^\diamondsuit$) while also reporting some diagnostics.

GDP per capita (baseline)$^\diamondsuit$*:* 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 8477 (63.2%) of our 13405 country years. Subsequently, we maximally extend these estimates forward to 2016 and backwards to 1960 using the growth rate of real per capita GDP provided by the World Bank (2016), thereby adding another 2651 (19.8%) country-year observations. Afterward, we remove 16 isolated country-year observations pertaining to the pre-independence situation in the group of former Soviet states. In a next step, we regress these baseline log per capita GDP estimates on log per capita CO2 emissions, as reported by the World Resources Institute (2015), a vector of year dummies, a region dummy for each of the seven regions distinguished by the World Bank (2016), their squared and cubic values as well as all possible interactions up to the third order. We then use the growth rate of the predicted per capita GDP trajectories to maximally extend the baseline series forward and backwards, adding another 869 (6.5%) observations.[44] Data on the country-specific emission levels of CO2 are available between 1950 and 2012 and, in itself, these correlate fairly strongly with the baseline per capita income estimates, at 0.83 for their 10050 common observations. That being said, with a correlation coefficient of 0.89, predicted per capita GDP levels correlate even more strongly with the baseline estimates. Finally, evaluating this least squares third-order polynomial model's predictive accuracy on an observation-by-prediction basis, we find that 55% of the baseline log per capita GDP observations fall within the 99% confidence intervals of their predicted counterparts. Although this indicates a fairly good match between the model's data-generating process and our reference series, this further motivates extending the reference data by relying on the growth rates implied in these alternative predictions, rather than the predicted values themselves.

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

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

correlation with baseline log per capita GDP (0.83 for their 8840 common observations). The Correlates of War Project (2012) data on primary energy consumption runs from 1816-2012 and shows a moderately positive correlation with baseline log per capita GDP (0.66 for their 8802 common observations). Nevertheless, the third-order polynomial predicted per capita GDP trajectories once again correlate even more strongly with their baseline counterparts, yielding a correlation coefficient of respectively 0.89 and 0.85, while the predictive accuracy of these models respectively attains 54% and 58%. Once again using the growth rates of predicted real per capita GDP to further extend the existing series forward and backwards adds another 361 (2.69%) observations for each of both sources. The remaining 687 (5.1%) country-year observations remain missing.[45]

GDP per capita (alternative)$^\Diamond$*:* In order to make sure that our findings are not driven by the data construction process, we also construct alternative per capita GDP estimates. To do so, we synthetize a wide variety of data sources containing information on country-specific levels of real per capita GDP. More specifically, we consider the information in Barro and Lee (1994); Heston, Summers, and Aten (1994); The Madison Project (2013); Feenstra et al. (2015); The Conference Board (2015); World Bank (2016).

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

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

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

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

and 2015 are obtained from Barro and Lee (1994); Heston et al. (1994); The Madison Project (2013); CLIO Infra (2015); Feenstra et al. (2015); United Nations Population Division (2015); World Bank (2016). Aggregation across datasets is obtained by applying the regular data construction procedure outlined earlier. Doing so, our consolidated indicator of population size is constructed by: *(i)* linearly interpolating missing observations in all data sources; *(ii)* selecting the most complete as the baseline series; *(iii)* selecting the alternative dataset for which the overlapping path is most strongly correlated with that of the base series; *(iv)* using the variation in the alternative source to approximate as much missing values in the base series as possible; and *(v)* repeating steps *(iii)*-*(v)* for each remaining data source. As the correlation between all these different sources is nearly perfect (cf. Table A1), our population variable is not sensitive to the selection of the base series or the specific sequence of extensions.

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

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

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

Trade openness$^{\diamondsuit}$*:* Data on trade openness, defined as the value of imports and exports relative to GDP, are obtained from Heston et al. (1994); Correlates of War Project (2015); Feenstra et al. (2015); World Bank (2016). After linearly interpolating missing observations in each dataset, we select the Feenstra et al. (2015) data as our baseline. This dataset covers the period 1950-2011 and provides us with 9041 (67.44%) country-year observations. Subsequently, we maximally extend the existing data forward and backwards using the growth rates implied in the World Bank (2016) data for an additional 1145 (4%) country-year observations. Finally, relying on the least squares third-order polynomial approximation procedure outlined above, we fill another 322 (2.4%) country-year observations based on the Heston et al. (1994) data and another 489 (3.65%) country-year observations based on the Correlates of War Project (2015) data.[46] 2425 (18.09%) country-year observations remain missing.

Democracy$^{\diamondsuit}$*:* In order to construct a composite index of democracy, we incorporate information on 8 measures of democracy: Melton, Meserve, and Pemstein (2010); Giuliano, Mishra, and Spilimbergo (2013); Center for Systemic Peace (2015); Gibler and Miller (2014b); Vanhanen (2014); CLIO Infra (2015); Freedom House (2015).[47] After linearly interpolating missing observations in each data set, as it is the most extensive data source, we consider Freedom House (2015) as our baseline series. Freedom House's (2015) continuous measure of democracy, which is based on a country's degree of political competition and political participation, provides us with 6553 (71.27%) democracy estimates. Subsequently, sequentially relying on the alternative democracy measures, we apply the third-order polynomial approximation approach described earlier to maximally extend this baseline series forward and backwards. After this procedure, 2513 (18.75%) country-year observations remain missing.

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

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

[46]Furthermore, we remove 17 negative data points resulting from the polynomial approximation procedure.
[47]For a comparison of various democracy indices, see among others Munck and Verkuilen (2002) and Melton et al. (2010)

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

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

Constitutional Quality: To gain panel data on the quality of the constitutions of NICs, we follow the procedure developed by Eicher and Kuenzel (2017) and construct a Hamann similarity index of each country's constitutional features with the constitutional features of the US. Quantitative information on constitutional features comes from the Comparative Constitutions Project (2015), allowing us to quantify constitutional quality for 9990 (74.52%) country years. The remaining 3415 country years (25.48%) remain missing.

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

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

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

B Estimation strategy

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

To do so, suppose that in a sample containing $J+1$ countries, indexed by $i = \{1, \ldots, J+1\}$, observed over T time periods, indexed by $t = \{1950, \ldots, T_0, \ldots, T\}$, country j decides to declare independence at time $t = T_0$ and that we are interested in determining the causal effect of this decision, if any, on its per capita GDP trajectory. To do so, denote by y_{jt}^N the level of log per capita GDP that would be observed in country j if it did not (yet) declare independence, and let y_{jt}^T denote the outcome that would be observed if country j declared itself independent prior to time $t+1$. Abstracting from anticipation effects, the causal economic effect of declaring independence at time $t \geq T_0$ is defined as $\beta_{jt} = y_{jt}^T - y_{jt}^N$.[48] The observed outcome for each country i can be written as

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

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

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

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

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

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

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

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

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

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

Condition 1A

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

Condition 2A

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

Condition 3A

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

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

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

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

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

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

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

or, equivalently:

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

Pre-multiplying both sides of equation (7A) by the inverse of λ^P , $(\lambda^{P'} \lambda^P)^{-1} \lambda^{P'}$, yields[49]

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

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

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

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

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

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

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

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

that these conditions hold approximately. In the empirical exercise of subsection (3.3), we do so by relying on the nested optimalization algorithm developed by Abadie et al. (2014, Appendix B), which defines the optimal weight vector $\mathbf{W}^*$ such that each synthetic control country minimizes the Root Mean Squared Prediction Error (RMSPE) of pre-independence outcomes (see equation (6A)).[50] 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.[51] 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.

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

C Selected results

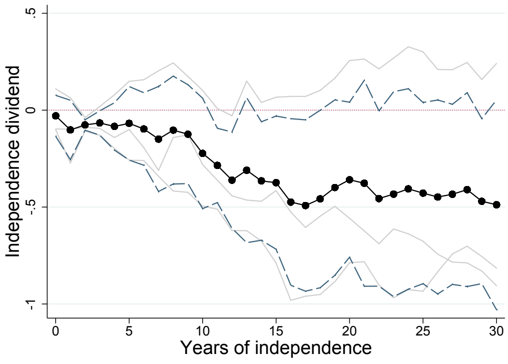
To put more empirical flesh on the bones, this appendix supplements the large-scale econometric analysis of section 3 by highlighting the results pertaining to a number of historical instances of state fragmentation and connecting them to the existing literature on this topic. More specifically, section C.1 reports the estimated economic consequences for the successor states grouped by their respective parent countries. Subsequently, section C.2 proposes an inferential procedure to account for the possibility that the independence dividend estimates pertaining to newly formed transition economies are partially driven by the costs associated with their transitions from planned to market economies.

C.1 Economic impact of historical instances of state fragmentation

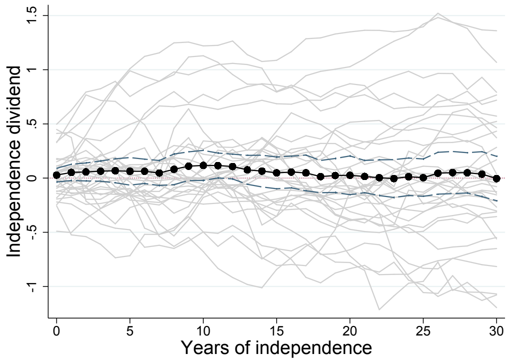
Figure A1 characterizes the economic consequences associated with the disintegration of the Belgian, British, French and Portuguese colonial empires, comparing these with the implied economic effects stemming from the dissolution of the Soviet Union, Yugoslavia and - most recently - Czechoslovakia. Recall that the identity of the mother country is thought to play an important role in explaining cross-country heterogeneity in the economic impact of secession, see section 1. In this regard, it is often argued that former British colonies prospered relative to their French, Spanish, Portuguese and Belgian counterparts because the British left behind better institutions (Acemoglu et al., 2001, 2002) and were more successful in educating their dependents (Grier, 1999). Interestingly, our results are largely consistent with this story and suggest that, in sharp contrast to NICs with other colonial heritages, former British colonies did not tend to suffer adverse economic consequences as a result of becoming independent and even enjoyed an independence gain of around 10% in the medium run. More surprisingly, although Belgian and Portuguese dominations are often considered the most detrimental and exploitative (Bertocchi & Canova, 2002), only the Belgian colonies appear to have suffered the adverse economic consequences of colonial demise in the form of an increasing reduction in per capita GDP that amounted to 50% in the 30^{th} post-independence year. Similarly, former French colonies appear to have suffered a persistent independence cost of around 20%.

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 underperformance of former Soviet states vis-á-vis other NICs in the region. Furthermore, they argue that this mechanism may have been amplified by differential prospects of EU membership, which enhanced incentives for law enforcement and protection of property rights in potential member states. Our results are testimony to this, indicating that the group of former Soviet members suffered the most adverse and persistent effects of state breakup. In comparison, the Yugoslavian successor states seems least affected by state fragmentation while the economic costs associated with the Czechoslovakian 'Velvet Divorce' were both more modest and much less persistent.

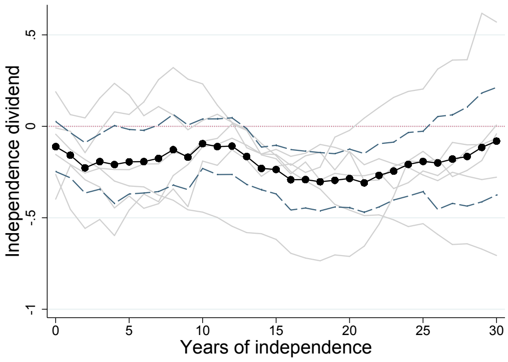
Figure A1: **Triple-difference estimates: historical instances of state fragmentation**



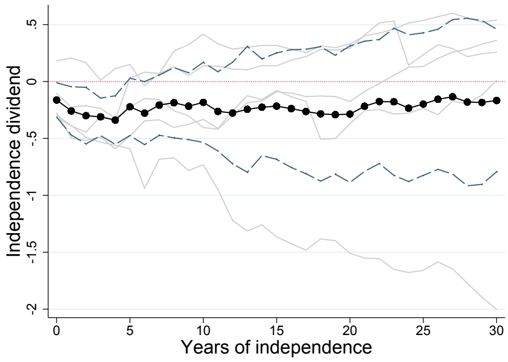
(a) Belgian colonial empire



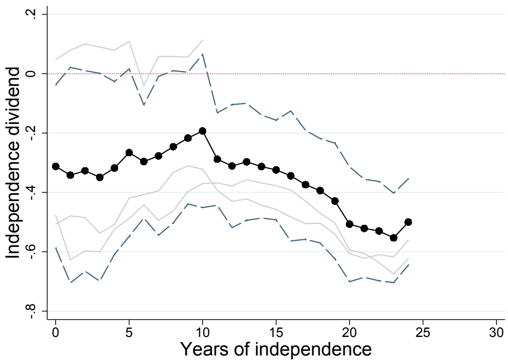
(b) British colonial empire



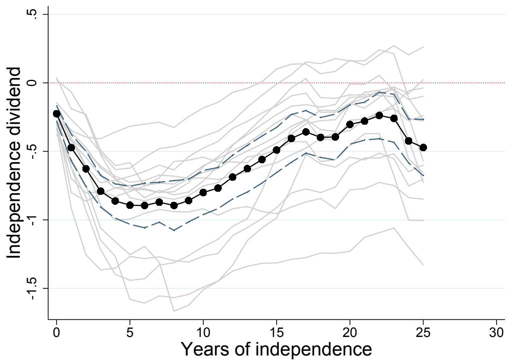
(c) French colonial empire



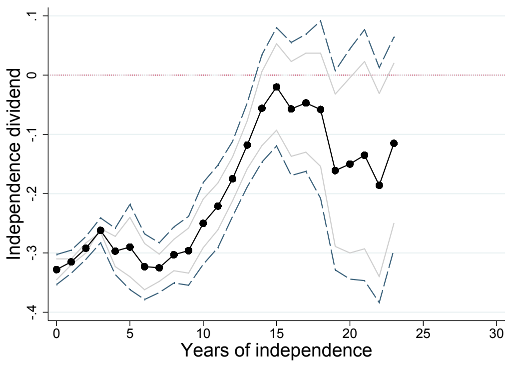
(d) Portuguese colonial empire



(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 95% bootstrapped confidence interval, clustered at the country level and based on 250 replications. The number of years after independence are indicated on the horizontal axis.

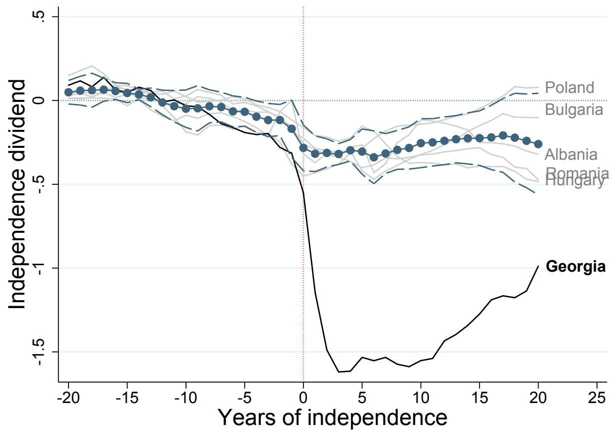
C.2 Accounting for transition costs

One remaining worry with the independence dividend estimates in figures A1e through A1g is that they may be partially driven by the costs these NICs experienced from their transition from planned to market economies, since these transition costs could have materialized irrespective of their choices to declare independence. Indeed, as the transition process temporally coincided with the independence declarations of the countries involved, transition costs may at least partially explain the severe independence costs estimated for the breakup of the former Soviet and Yugoslav states. To study their potential relevance, we aim to disentangle the independence effect from these transition costs by semi-parametrically computing transition costs in a group of 'established' transition countries, namely those transition countries that did *not* recently declare independence, and subsequently subtracting these from the independence dividend estimates pertaining to newly formed transition countries. To the extent that the distribution of transition costs in these established transition countries can be taken to reflect the transition costs that would have been experienced by the new transition countries in our sample, this approach allows us to purge the relevant independence dividend estimates from transition costs.

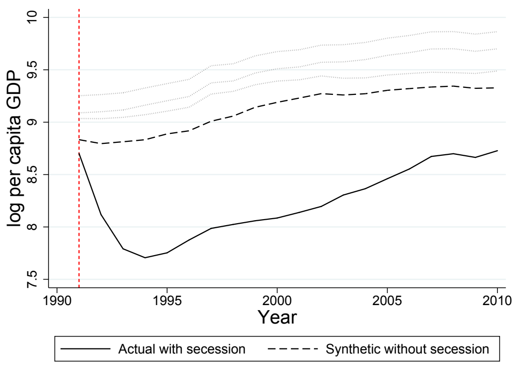
Figure A2a demonstrates this reasoning by reconsidering the Georgian example in section 3.4 and compares the synthetic control estimates for the per capita GDP discrepancy between Georgia and its synthetic counterpart in the 20-year period around its declaration of independence with the contemporary per capita GDP discrepancies observed in the five established transition countries mentioned in Roland (2000), i.e. Albania, Bulgaria, Hungary, Poland and Romania. As can be seen, all these established transition countries also started to underperform with respect to their synthetic counterparts despite *not* having declared independence in 1991. More specifically, these results suggests that they effectively incurred a persistent transition cost of around 20% expressed in per capita GDP terms. To account for the transition costs that would also be experienced by Georgia absent secession, we assume that the distribution of estimated transition costs in these five established transition countries can be taken to approximate the portion of the per capita GDP discrepancy between Georgia and synthetic Georgia that stems from Georgia's transition process towards a market economy.

More specifically, figure A2b corrects the triple-differenced trajectory of synthetic Georgia by also removing the typical triple-differenced discrepancy observed in the group of established transition countries. Nevertheless, figure A2c indicates that the post-independence per capita GDP discrepancy between Georgia and synthetic Georgia remains unusually negative compared to the contemporary distribution of discrepancies observed in established transition countries. Thus, the quadruple-corrected Georgian independence dividend trajectory is unlikely to reflect transition costs. Moreover, figure A2c also shows that correcting the Georgian triple-differenced independence dividends for the discrepancy that can reasonably be attributed to transition costs only results in a modest upward revi-

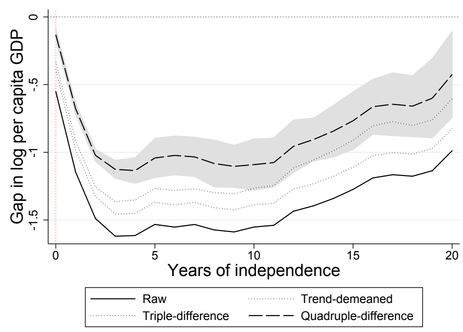
Figure A2: **Accounting for transition costs: Georgia**



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



(b) Actual vs. synthetic per capita GDP



(c) The economic impact of secession

Note: Figure A2a plots triple-difference Georgian independence dividend estimates (black line) against the triple-difference independence dividends pertaining to five established transition countries (grey lines) along with their 95% confidence intervals (blue dashed lines). Figure A2b plots the log per capita GDP trajectory in Georgia (full line), the uncorrected , trend-demeaned and triple-difference (dotted lines) as well as the quadruple-difference (dashed line) versions of synthetic Georgia; figure A2c plots the raw (full line), trend-demeaned and triple-difference (dotted line) as well as the quadruple-difference (full line) independence dividend trajectories that are respectively defined in equations (10A), (1), (2) and (11A).

sion of its independence dividend, suggesting that the bulk of Georgia's underperformance in the post-independence period stems from its independence declaration or, in other words, that only a small part of it is driven by transition costs.

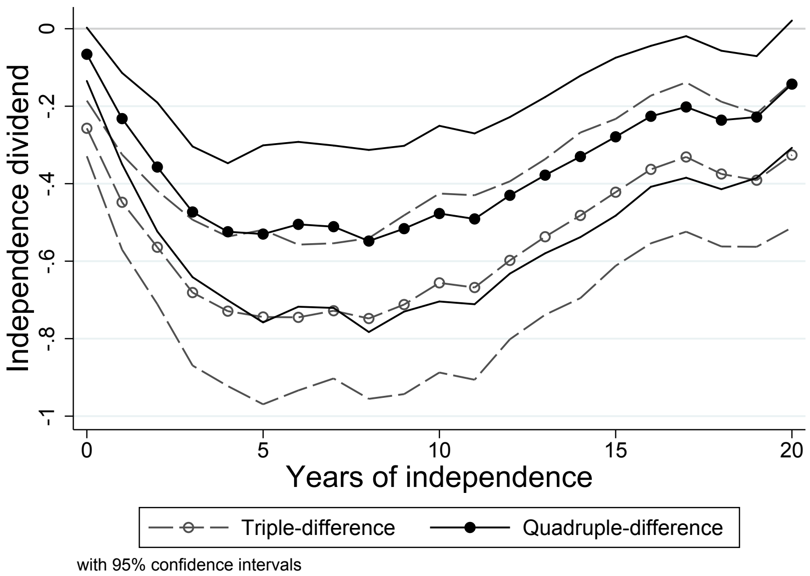
Generalizing this approach, denote the contemporary transition countries in newly formed transition country j's donor pool by $m \in [1, \ldots, M_j]$ and define the *quadruple-difference* estimate of its independence dividend in the s^{th} post-independence year as

$$\begin{aligned}
\hat{\beta}^{pure}_{j,s} = & \Bigg[\underbrace{\Big(y_{j,T_0+s} - \sum_{i \neq j} w^*_{i,j} y_{i,T_0+s} \Big)}_{\text{raw treatment effect}} - \underbrace{\Big(\sum_{t=T_0-10}^{T_0-1} \Big(y_{j,t} - \sum_{i \neq j} w^*_{i,j} y_{i,t} \Big) \Big)}_{\text{matching inaccuracy}} \Bigg] - \\
& \underbrace{\frac{1}{M_j} \sum_{m \neq j}^{M_j} \Bigg[\Big(y_{m,T_0+s} - \sum_{i \neq m, i \neq j} w^*_{i,m} y_{i,T_0+s} \Big) - \Big(\sum_{t=T_0-10}^{T_0-1} \Big(y_{m,t} - \sum_{i \neq m} w^*_{i,m} y_{i,t} \Big) \Big) \Bigg]}_{\text{transition cost}} - \\
& \underbrace{\frac{1}{K_j} \sum_{k \neq j}^{K_j} \Bigg[\Big(y_{k,T_0+s} - \sum_{i \neq k, i \neq j} w^*_{i,k} y_{i,T_0+s} \Big) - \Big(\sum_{t=T_0-10}^{T_0-1} \Big(y_{k,t} - \sum_{i \neq k} w^*_{i,k} y_{i,t} \Big) \Big) \Bigg]}_{\text{simulation inaccuracy}}
\end{aligned} \tag{11A}$$

Figure A3 plots the aggregate triple-differenced yearly independence dividend estimates for all newly formed transition countries in our sample, namely the successor states to the Soviet Union, Yugoslavia and Czechoslovakia, and compares these with their quadruple-differenced counterparts. As can be seen, purging the triple-differenced independence dividends from discrepancies that can plausibly be attributed to the transition process results in a modest upward revision of the independence dividend. Nevertheless, as the figure also shows, most of the underperformance of these newly formed transition countries can be reasonably attributed to their decision to declare independence, such that our conclusions remain qualitatively unaffected. These findings are consistent with the idea that secessions are more disruptive in economic terms within planned economies, due to the collapse of the integrated economic space and the severing of supply chains as well as the weakness of the institutions in their constituent parts.

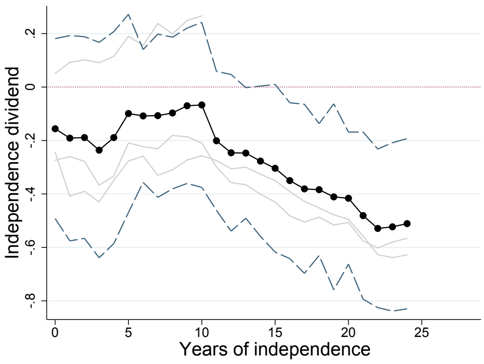
For completeness, figure A4 plots the quadruple-differenced versions of the estimated independence dividend trajectories in figure A1 while table A2 compares the country-specific quadruple-differenced independence dividends of the newly formed transition countries in our sample with their triple-differenced counterparts.

Figure A3: **Semi-parametric estimates of the independence dividend: transition countries**

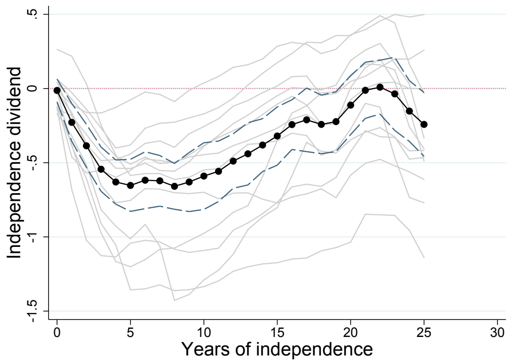


Note: The figure plots the yearly average triple-difference (hollow circles) and quadruple-difference (full circles) estimates of the independence dividend, as outlined in equations (2) and (11A). Bootstrapped confidence intervals based on 500 replications. Years of independence are indicated on the horizontal axis.

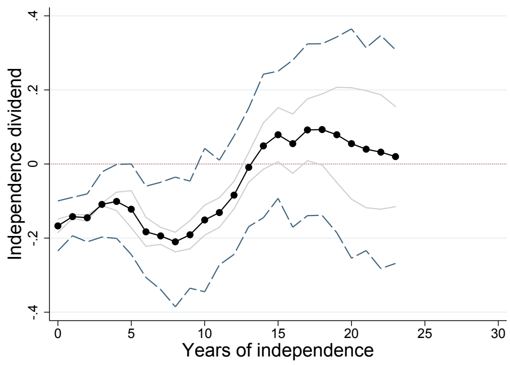
Figure A4: **Quadruple-difference estimates of the independence dividend**



(a) Yugoslavia



(b) Soviet Union



(c) Czechoslovakia

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

Table A2: **Semi-parametric estimates of the economic impact of secession in transition countries**

	t = 0 + 1		**t = 0 + 5**		**t = 0 + 20**	
Country	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{DDDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{DDDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{DDDD}$
Azerbaijan	-.617***	-.378***	-1.441***	-1.201***	-.106	.092
Belarus	-.267***	-.027	-.622***	-.381***	.16***	.357***
Croatia	-.627***	-.408***	-.488***	-.277***	-.594***	-.495**
Czech Republic	-.31***	-.137***	-.34***	-.172**	-.3***	-.095
Estonia	-.348***	-.108***	-.317***	-.076	.165**	.362***
Georgia	-.916***	-.678***	-1.267***	-1.042***	-.605***	-.425**
Kazakhstan	-.424***	-.184***	-.78***	-.539***	-.009	.189*
Kyrgyzstan	-.403***	-.163***	-.995***	-.754***	-.781***	-.583***
Latvia	-.699***	-.46***	-.793***	-.552***	-.272***	-.075
Lithuania	-.418***	-.178***	-.683***	-.442***	-.149***	.048
Moldova	-.558***	-.319***	-1.254***	-1.013***	-.542***	-.345***
Russia	-.361***	-.072	-.766***	-.51***	-.152**	.007
Slovakia	-.32***	-.147***	-.24***	-.072	0	.206
Slovenia	-.479***	-.26***	-.419***	-.209***	-.606***	-.507***
Tajikistan	-.618***	-.386***	-1.58***	-1.357***	-1.229***	-1.035***
Ukraine	-.067**	.218***	-.838***	-.586***	-.456***	-.273
Serbia	.079**	.092	.109***	.19		

Note: This table reports country-specific, semi-parametric estimates of the independence dividend. Results are reported for all available newly formed transition countries and pertain to the 1^{st}, 5^{th} and 20^{th} year after independence respectively. Columns headed by $\hat{\beta}_{jt}^{DDD}$ report the trend- and placebo-demeaned independence dividend estimate, as defined in equation (2) ; columns headed by $\hat{\beta}_{jt}^{DDDD}$ report the quadruple-difference independence dividend estimate, as defined in equation (11A). Standard errors are robust against heteroskedasticity and serial correlation at the country level. The number of years after secession is indicated on the horizontal axis.

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

D Specification tests

The estimators discussed in section 4.1 all rely on the crucial assumption that gross fixed capital formation is strictly increasing in the (unobserved) efficiency gain of independence, as a necessary condition for the control function to accurately proxy for the latter. This appendix implements a specification test to verify whether this identification assumption is likely to be met in our data. Before doing so, a first section checks whether the inclusion of the control function is even necessary to obtain unbiased estimates for the relative importance of various potential determinants of the independence dividend, by implementing a test of coefficient equality across the uncorrected model and the various bias-corrected models to test for the presence of endogeneity.

D.1 Endogeneity check

Before applying the endogeneity-correction procedures summarized in equations (13) and (14), it might be useful to verify whether there is an endogeneity issue in the first place. Indeed, if there would be no endogeneity issue to begin with, adding the control function would serve no practical purpose and the simple least-squares regression in equation (5) would suffice to estimate the relative importance of the potential determinants of the independence dividend. In the spirit of Hausman (1978), this suggests that computing the (uncorrected) least squares estimates and comparing them with their bias-corrected counterparts might be informative to determine the presence and the nature of endogeneity bias. More specifically, if there would be statistically significant differences in the uncorrected and the corrected estimates, this would be indicative that the (unobserved) efficiency gain of independence is correlated with the predictors and would point towards the necessity of including the control function to obtain unbiased estimates.

For simplicity and brevity, we restrict our attention to a so-called compact estimation model that abstracts from the dynamic relation between independence dividend trajectories and their underlying determinants. Formally, this implies that we compute the uncorrected least-squares estimates for the relative importance of the potential determinants of the independence dividends as:

$$\hat{\beta}_{i,t,s} = \lambda \mathbf{X}_{i,t,s} + \eta_s + \mu_t + \epsilon_{i,t,s} \tag{12A}$$

Subsequently, we compare these uncorrected estimates with their bias-corrected counterparts and formally test for statistically significant differences between both. The different bias-correction procedures discussed in section 4.1 imply that we can evaluate $(2 \times 3) = 6$ sets of bias-corrected coefficient estimates, since we consider two specific estimators - namely the single-stage least square and two-stage least squares models derived from Olley and Pakes (1996) - and 3 potential choices for the order of the polynomial in fixed capital and gross fixed capital formation - $J \in (2, 3, 4)$. Formally, the compact versions of the single-stage and two-stage bias-correction procedures summarized in equations (10) and (12) respectively boil down to estimating

$$\hat{\beta}_{i,t,s} = \lambda \mathbf{X}_{i,t,s} + \eta_s + \mu_t + \sum_{j=0}^{J} \sum_{m=0}^{J-j} \alpha_{j,m} K_{i,t,s}^{j} I_{i,t,s}^{m} + \epsilon_{i,t,s} \tag{13A}$$

$$\begin{aligned} \hat{\beta}_{i,t,s} &= \lambda \tilde{\mathbf{X}}_{i,t,s} + \eta_s + \mu_t + \sum_{j=0}^{J} \sum_{m=0}^{J-j} \alpha_{j,m} K_{i,t,s}^{j} I_{i,t,s}^{m} + \epsilon_{i,t,s} \\ \tilde{\mathbf{X}}_{i,t,s} &= \alpha \bar{\mathbf{X}}_{i,t-1,s-1} + \nu_{i,r,s,t} \end{aligned} \tag{14A}$$

The second column of table A3 reports the uncorrected estimates for the relative importance of the potential determinants of the triple-difference independence dividends while the third to last columns compare these with their various bias-corrected counterparts. Furthermore, the table identifies which corrected coefficients statistically significantly differ from their uncorrected counterparts, relying on a Chow test to formally test for coefficient equality across the uncorrected baseline model and the various bias-corrected models.[52] Surprisingly, we find little evidence for the presence of endogeneity bias as most corrected coefficients do not statistically significantly differ from their uncorrected counterparts. One potential explanation for this, however, is that the relative importance of the various potential determinants of the independence dividend can often only very imprecisely be estimated. In other words, the wide confidence intervals around both corrected and uncorrected point estimates mechanically increase the probability that the test statistic will accept the null hypothesis of coefficient equality.

Nevertheless, we find moderate evidence that two potential channels might be endogenous. First, the estimated adverse impact of battle deaths on the independence dividend shrinks once we parametrically control for the unobserved efficiency gain of independence. This finding is consistent with the conjecture that NICs facing economically costly independence trajectories are also more susceptible to violent conflict crises in the post-independence period, such that violence is primarily a byproduct of growth-inhibiting independence declarations rather than the other way around. Second, the beneficial effect of trade openness also decreases in the bias-corrected models, indicating that economically beneficial independence declarations may have a tendency to increase trade flows which potentially causes the uncorrected least squares model to slightly overestimate the growth-enhancing effects of increasing trade openness in the post-independence period.

Although section 4.3 demonstrates how the corrected and uncorrected estimates for the relative importance of the potential determinants of the independence dividend generally

[52]Note that the standard errors used to compute the test statistic were not obtained by bootstrapping and thus potentially underestimate the true standard errors, because they ignore the uncertainty in the dependent variable, $\hat{\beta}_{i,t,s}$, which is itself an estimate for the true independence dividend, $\beta_{i,t,s}$. Therefore, the table can only provide weak evidence for the presence of endogeneity bias, because the potentially underestimated confidence intervals for each coefficient bias the test statistic towards finding statistically significant differences between corrected and uncorrected coefficients.

Table A3: **Second-step estimates: coefficient comparison by estimator**

Channel	$\hat{\beta}^{OLS}$	2^{nd} order polynomial $\hat{\beta}^{OP}$	$\hat{\beta}^{OP2}$	3^{th} order polynomial $\hat{\beta}^{OP}$	$\hat{\beta}^{OP2}$	4^{th} order polynomial $\hat{\beta}^{OP}$	$\hat{\beta}^{OP2}$
Surface area	**.019**	**.034**	**.041**	**.02**	**.027**	**.013**	**.02**
	-.132 / .272	*-.122 / .217*	*-.108 / .223*	*-.149 / .195*	*-.132 / .219*	*-.155 / .195*	*-.147 / .204*
Surface area loss	**-.031**	**-.013**	**-.013**	**-.013**	**-.013**	**-.02**	**-.019**
	-.185 / .097	*-.153 / .117*	*-.144 / .098*	*-.143 / .092*	*-.148 / .097*	*-.155 / .101*	*-.15 / .1*
Trade openness	**.127**	**.082**	**.094**	**.071**	**.084**	**.067***	**.081**
	.03 / .255	*-.02 / .204*	*-.003 / .238*	*-.035 / .21*	*-.012 / .215*	*-.035 / .224*	*-.022 / .236*
Democracy	**.076**	**.099**	**.1**	**.089**	**.091**	**.073**	**.074**
	.008 / .199	*.006 / .162*	*.014 / .174*	*-.002 / .162*	*.008 / .157*	*-.022 / .147*	*-.025 / .147*
Macroeconomic uncertainty	**-.096**	**-.044**	**-.041**	**-.027**	**-.028**	**-.043**	**-.044**
	-.422 / .138	*-.276 / .195*	*-.256 / .213*	*-.253 / .222*	*-.254 / .207*	*-.295 / .185*	*-.284 / .173*
Battle deaths	**-.047**	**-.027**	**-.029**	**-.016****	**-.018****	**-.019****	**-.021****
	-.114 / -.009	*-.073 / .017*	*-.083 / .022*	*-.067 / .027*	*-.074 / .02*	*-.077 / .022*	*-.074 / .019*
Obs [# countries]	2162 [64]	2046 [61]	2078 [61]	2046 [61]	2078 [61]	2046 [61]	2078 [61]
R^2	.189	.291	.291	.313	.312	.327	.326
# reps	500	500	500	500	500	500	500

Note: This table reports the different bias-corrected estimates for the relative importance of several potential determinants of the triple-difference independence dividends that stem from the bias-correction procedures discussed in section 4.1 and compares them with their uncorrected counterparts as defined in equation (5). For simplicity, the estimations ignore the interactions of the predictor variables with the years-of-independence dummies. The results show the relevant coefficient estimates when endogeneity bias is eliminated by the single-stage least squares model summarized in equation (10) (β^{OP}) or the two-stage least squares model described in equation (12) (β^{OP2}). Both the point estimates and the 90% bootstrapped confidence intervals are reported for different choices of the order of the polynomial in gross fixed capital and gross fixed capital formation, as identified in the row headings. ***, ** and * specifies whether a basic Chow test indicates that the corrected coefficient estimate statistically significantly differs from its uncorrected counterparts at the 1, 5 and 10% levels, respectively.

tend to coincide in the dynamic versions of this model, the knowledge that imprecisely estimated point estimates make it more difficult to reliably test for coefficient equality and that - despite this - we still find moderate empirical evidence for the presence of endogeneity leads us to conclude that a bias-corrected estimator may be necessary to obtain unbiased estimates.

D.2 Monotonicity check

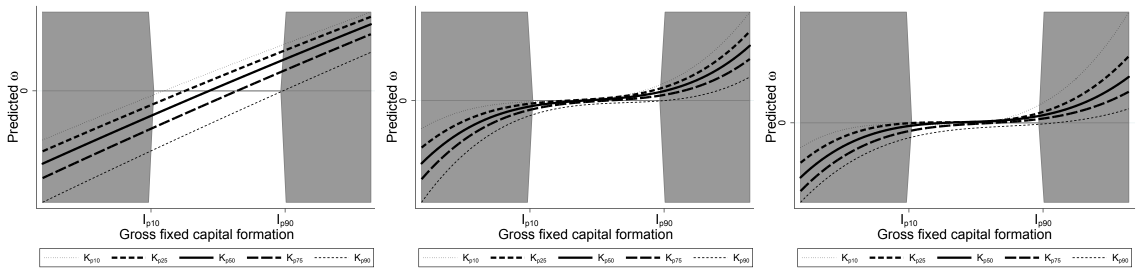
Ornaghi and Van Beveren (2011) propose a simple monotonicity test to check whether this identification assumption is likely to hold in particular datasets. Building on this procedure, note that the monotonicity assumption in our setting boils down to assuming that for any given value of the fixed capital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

One crude way of assessing this is to approximate the unobserved efficiency gain as the residual of the regression formalized in equation (5), abstracting from the random shock $\epsilon_{i,t,s}$ hence implicitly assuming that that $\omega_{i,t,s} \approx \omega_{i,t,s} + \epsilon_{i,t,s}$. Subsequently, this residual can regressed on a polynomial in fixed capital and gross fixed capital investment from the appropriate order to compute the expected efficiency gain of independence for any value of both predictors. Formally, denoting the residual in equation (5) by $\varpi_{i,t,s} = \omega_{i,t,s} + \epsilon_{i,t,s}$, the monotonicity test boils down to comparing various predictions for the estimated efficiency gain of independence,$\hat{\varpi}_{i,t,s}$, from the following model

$$\hat{\varpi}_{i,t,s} = \alpha_0 + \sum_{j=0}^{J} \sum_{m=0}^{J-j} \alpha_{j,m} K_{i,t,s}^{j} I_{i,t,s}^{m} + \zeta_{i,t,s} \tag{15A}$$

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

Figure A5: **Monotonicity test**



(a) 2^{nd} order polynomial (b) 3^{rd} order polynomial (c) 4^{th} order polynomial

Note: This figure plots the efficiency gain of independence as predicted by the estimation model summarized in equation (15A), when fixing $K_{i,ts}$ at its 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} percentile values, respectively indicated by K_{p10}, K_{p25}, K_{p50}, K_{p75}, K_{p90} in the figures, and gradually increasing the value of $I_{i,t,s}$ over its support, where I_{p10} and I_{p90} respectively show the 10^{th} and 90^{th} percentile values for $I_{i,t,s}$ in our sample. The raw efficiency gain of independence, or $\hat{\varpi}_{i,t,s}$ in equation (15A), is estimated as the residual of the model summarized in equation (5). The results are reported for different choices for the order of the polynomial in fixed capital and gross fixed capital formation, as identified in the subtitles. Our baseline specification, detailed in section 4, corresponds to figure A5b.

Figure A5 reports the result of the monotonicity tests pertaining to the various the estimation procedures outlined in section 4.1. Reassuringly, the figures suggest that the monotonicity assumption seems not to be violated in any part of the support of $I_{i,t,s}$ for any the selected percentile values of $K_{i,t,s}$. Thus, at first glance, all observations in our sample appear to satisfy the monotonicity assumption - well above the 80%-threshold for valid inference proposed by Ornaghi and Van Beveren (2011). We conclude that the monotonicity tests fail to find evidence of the critical identification assumption of monotonicity being violated in our data, thus allowing for valid estimation.

E An extension of the control function approach

One potential concern with respect to the control function approach proposed in section 4.3 is that the signal on the perceived efficiency gains of independence that can be extracted from fixed capital stocks and gross fixed capital formation in NICs may become too noisy if fixed capital investment demand also depends on other factors, besides the contemporary value of the fixed capital stock. If this would be the case, actual processes of gross fixed capital formation would only be imperfectly described by equation (8) and it would become necessary to account for other factors that may have influenced fixed capital investment demand to derive a reliable signal for the perceived efficiency gains of independence from contemporary fixed capital investment decisions in NICs. Indeed, once these factors are accounted for, fixed capital investment demand would once again solely depend on the current value of the capital stock and the perceived efficiency gain of independence such that a reliable signal for the perceived efficiency gain of independence could still be extracted from the combined information on fixed capital stocks, gross fixed capital formation *and* these additional factors. More formally, this approach boils down to adding a $(1 \times C)$ vector, $\mathbf{C}_t$, of control variables that affect gross fixed capital formation to the investment demand equation such that the following assumption holds

$$\mathbf{E}\left(I_{i,t,s} \mid \mathbf{C}_{i,t,s}\right) = f\left(\omega_{i,t,s}, K_{i,t,s}\right) \tag{16A}$$

If there exists a set of control variables that satisfies equation (16A) and the standard monotonicity assumption holds, the contemporary values of these additional control variables in addition to values of the existing fixed capital stocks and gross fixed capital formation in NICs contain sufficient information to determine the perceived efficiency gain of their independence declaration at a each point in time.

This section relies on this intuition to outline a robustness check verifying the credibility of the scalar unobservability assumption in the baseline model of section section 4.2. The central intuition of this robustness check is that *if* investment demand also depended on the $(1 \times X)$ standardized predictors of the independence dividend, equation (16A) requires the addition of these to the control function to obtain an accurate proxy for the perceived efficiency gain of independence. The finding that the relative importance estimates for the standardized predictors of the independence dividend do *not* significantly alter after adding them to the control function would therefore constitute further evidence of the nonviolation of the scalar unobservability assumption in the baseline model.

More formally, this approach boils down to relaxing the implicit assumption in equation (9) and to allow fixed capital investment demand of NIC i in year t and independence year s, $I_{i,t,s}$, to also depend on the $(1 \times X)$ vector of standardized predictors of the independence dividend. In this scenario, we can thus proxy the unobserved efficiency gain of independence by inverting the extended investment demand function, see equation (16).

Note that we now need to assume that, *conditional* on the value the observed growth determinants of the NIC, fixed capital investment demand *only* depends on the current value of the capital stock and the perceived efficiency gain of independence, which is a less stringent identification assumption then the one made in subsection 4.2. Retaining the monotonicity assumption, which presupposes that investment in fixed capital is strictly increasing in the unobserved efficiency gain of independence, simply adding this extended control function to regression equation (7) suffices to control for endogeneity bias and allows us to proceed by estimating the following model[53]

$$\hat{\beta}_{i,t,s} = \overbrace{\beta_0 + \lambda \mathbf{X}_{i,t,s} + \underbrace{f^{-1}\left(K_{i,t,s}, I_{i,t,s}, \mathbf{X}_{i,t,s}\right)}_{\omega_{i,t,s} \approx g(\omega_{i,t-1,s-1}) + \xi_{i,t,s}}}^{\bar{\bar{\beta}}_{i,t+1,s+1} = \phi\left(K_{i,t,s}, I_{i,t,s}, \mathbf{X}_{i,t,s}\right)} + \lambda_s \Big(\mathbf{X}_{i,t,s} \times s\Big) + \eta_s + \mu_t + \epsilon_{i,t,s} \tag{17A}$$

One additional complication is that, since the growth determinants $\mathbf{X}_{i,t,s}$ are now included in the inverted fixed capital investment function, $\omega_{i,t,s} = f^{-1}\left(K_{i,t,s}, I_{i,t,s}, \mathbf{X}_{i,t,s}\right)$, identification of the relative importance of the potential determinants of the independence dividend in each post-independence year s, $\lambda + \lambda_s$, is prevented by the fact that $\mathbf{X}_{i,t,s}$ is collinear with the inverted investment demand function in $I_{i,t,s}$, $K_{i,t,s}$ and $\mathbf{X}_{i,t,s}$. Indeed, the collinearity of the growth determinants requires us to rewrite equation (17A) as

$$\hat{\beta}_{i,t,s} = \lambda_s \Big(\mathbf{X}_{i,t,s} \times s\Big) + \eta_s + \mu_t + \phi\left(K_{i,t,s}, I_{i,t,s}, \mathbf{X}_{i,t,s}\right) + \epsilon_{i,t,s} \tag{18A}$$

where $\phi\left(K_{i,t,s}, I_{i,t,s}, \mathbf{X}_{i,t,s}\right) = \beta_0 + \lambda \mathbf{X}_{i,t,s} + f^{-1}\left(K_{i,t,s}, I_{i,t,s}, \mathbf{X}_{i,t,s}\right)$.

Identifying λ proceeds in two steps. First, assuming that the $f^{-1}(.)$ function can be approximated parametrically by polynomial expansion of order 3 in all possible interactions of $K_{i,t,s}$ and $I_{i,t,s}$ in addition to the linear, squared and cubic values of all X growth determinants, such that $f^{-1}\left(K_{i,t,s}, I_{i,t,s}, \mathbf{X}_{i,t,s}\right) \approx \sum_{j=0}^{3} \sum_{m=0}^{3-j} \alpha_{j,m} K_{i,t,s}^{j} I_{i,t,s}^{m} + \sum_{x=1}^{X} \sum_{j=0}^{3} \alpha_{x,j} x_{i,t,s}^{j}$, estimation of (18A) yields consistent estimates for $\hat{\lambda}_s$, $\hat{\eta}_s$, $\hat{\mu}_t$, $\hat{\phi}\left(K_{i,t,s}, I_{i,t,s}, \mathbf{X}_{i,t,s}\right)$ and $\hat{\epsilon}_{i,t,s}$.

To identify λ in a second step, consider the expected value of $\bar{\bar{\beta}}_{i,t+1,s+1} = \hat{\beta}_{i,t+1,s+1} - \hat{\lambda}_{s+1}\Big(\mathbf{X}_{i,t+1,s+1} \times s+1\Big) - \hat{\eta}_{s+1} - \hat{\mu}_{t+1} - \hat{\epsilon}_{i,t+1,s+1}$ conditional on the information at time t

$$\mathbf{E}\Big[\bar{\bar{\beta}}_{i,t+1,s+1} \mid K_{i,t+1,s+1}, \mathbf{X}_{i,t+1,s+1}\Big] = \beta_0 + \lambda \mathbf{X}_{i,t+1,s+1} + \mathbf{E}\left[\omega_{i,t+1,s+1} \mid \omega_{i,t,s}\right] \tag{19A}$$

Substituting the law of motion for $\omega_{i,t,s}$ of equation (6) for $\mathbf{E}\left[\omega_{i,t+1,s+1} \mid \omega_{i,t,s}\right]$ yields

$$\mathbf{E}\Big[\bar{\bar{\beta}}_{i,t+1,s+1} \mid K_{i,t+1,s+1}, \mathbf{X}_{i,t+1,s+1}\Big] = \beta_0 + \lambda \mathbf{X}_{i,t+1,s+1} + g\left(\omega_{i,t,s}\right) + \xi_{i,t+1,s+1} \tag{20A}$$

[53] A modified version of the monotonicity test developed in appendix D.2 finds no evidence of the monotonicity assumption being violated over any significant portion of the support of $I_{i,t,s}$.

and from equation (16)

$$\begin{aligned}&\mathbf{E}\big[\bar{\bar{\beta}}_{i,t+1,s+1} \mid K_{i,t+1,s+1}, \mathbf{X}_{i,t+1,s+1}\big] = \\ &\beta_0 + \lambda \mathbf{X}_{i,t+1,s+1} + g\big(f^{-1}\left(K_{i,t,s}, I_{i,t,s}, \mathbf{X}_{i,t,s}\right)\big) + \xi_{i,t+1,s+1}\end{aligned} \tag{21A}$$

which, finally, is equivalent to

$$\begin{aligned}&\mathbf{E}\big[\bar{\bar{\beta}}_{i,t+1,s+1} \mid K_{i,t+1,s+1}, \mathbf{X}_{i,t+1,s+1}\big] = \\ &\beta_0 + \lambda \mathbf{X}_{i,t+1,s+1} + g\Big[\phi\left(K_{i,t,s}, I_{i,t,s}, \mathbf{X}_{i,t,s}\right) - \quad \beta_0 - \lambda \mathbf{X}_{i,t,s}\Big] + \xi_{i,t+1,s+1}\end{aligned} \tag{22A}$$

As explained in section 4.1, instrumenting the potential determinants of the independence dividends contained in the $\mathbf{X}_{i,t,s}$-matrix with their lags to obtain $\tilde{\mathbf{X}}$ ensures the orthogonality of the news component, $\xi_{i,t,s}$, to both contemporary capital stocks as well as contemporary growth determinants such that identification of λ (and β_0) can be obtained by exploiting the following moment conditions

$$\mathbf{E}\left[\xi_{i,t,s} \;\middle|\; \begin{matrix} K_{i,t,s} \\ \tilde{\mathbf{X}}_{i,t,s} \end{matrix}\right] = 0 \tag{23A}$$

This implies that λ can be identified by finding the vector of values for λ for which $\xi_{i,t+1,s+1}$ in equation (22A) is as close to 0 as possible. More specifically, relying on the consistent estimates for $\hat{\eta}_s$, $\hat{\mu}_t$, $\hat{\epsilon}_{i,t,s}$ and $\hat{\phi}\left(K_{i,t,s}, I_{i,t,s}, \tilde{\mathbf{X}}_{i,t,s}\right)$ obtained from the first-step regression of the model in equation (18A) and once again relying on a third order polynomial to parametrically approximate the g function in equation (22A), identification of λ (and β_0) proceeds by estimating the following specification through non-linear least squares:

$$\bar{\bar{\beta}}_{i,t+1,s+1} = \sum_{j=0}^{3} \left[\hat{\phi}\left(K_{i,t,s}, I_{i,t,s}, \tilde{\mathbf{X}}_{i,t,s}\right) - \beta_0 - \lambda \tilde{\mathbf{X}}_{i,t,s}\right]^j + \beta_0 + \lambda \mathbf{X}_{i,t+1,s+1} + \xi_{i,t+1,s+1} \tag{24A}$$

As noted by Ornaghi and Van Beveren (2011), estimating equation (24A) is the least squares equivalent of minimizing $\xi_{i,t+1,s+1}$ in equation (22A). Finally, as was the case in subsection 4.2, standard errors can be obtained by applying block-bootstrapping techniques that comprise re-sampling over NICs. To mitigate the effects of extreme outliers due to small sample size, worst-fit bootstrap estimation models with sums of squared residuals in excess of ten times the sum of squared residuals of the reference estimation model are ignored in the bootstrap procedure.

F Agnostic estimates of the determinants of the independence dividend

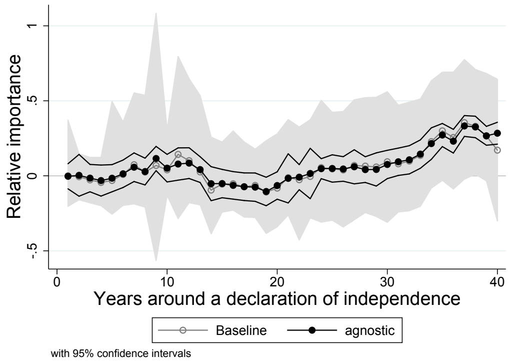
Sections 4.1 through 4.3 present a battery of estimates for several potential determinants of the independence dividend. Although our main estimation results remain quite stable across these models, we remain agnostic on which model produces the most reliable estimates for the relative importance of these various channels. This section follows this agnosticism to its logical conclusion by trying to extract the common message from all these estimation models. More specifically, figure A6 compares the findings of our baseline model against 'agnostic' estimates that are calculated as the weighted average estimate across all 18 available models and giving each estimation model equal weight.[54]

As can be seen, figure A6 confirms that the estimates of the baseline model seem quite representative for these so-called agnostic estimates for the relative importance of the various channels. Once again, post-independence surface area, democracy and trade openness are identified as the most important channels through which the independence declarations affected growth potential of the newly formed states in our sample while their impact is mainly visible in the medium to long run. Moreover, the comparatively small confidence intervals around these agnostic estimates demonstrate that the relative importance estimates of these three channels remain very comparable across estimation models when they are applied to the full sample of available NICs, whereas the comparatively large confidence intervals of the baseline models highlight that the relative importance estimates are more sensitive to fluctuations in the sample of available NICs. Note, however, that relative importance estimates of macroeconomic uncertainty tend to fluctuate wildly across estimation models such that model agnosticism implies that its relation to the independence dividend cannot be precisely estimated.

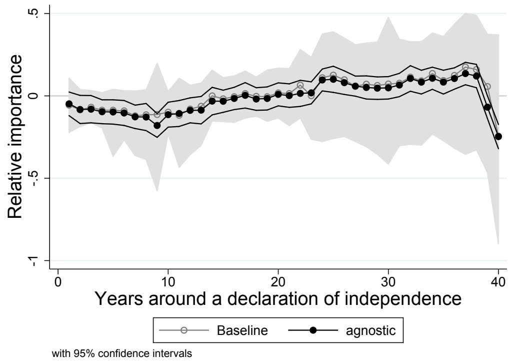
Finally, table A4 reports agnostic estimates for the relative importance of several channels of the independence dividend for the short, medium and long run. Interestingly, the estimated relative importance of most channels increases over time such that their economic impact gets progressively more pronounced over time. Nevertheless, even in the first ten post-independence years more trade-open, democratic and peaceful NICs have a clear tendency to outperform their more autarkic, autocratic and bellicose counterparts. Focusing on the statistically significant point estimates, military conflict turns out to have had the most detrimental impact, suggesting that it is also important from an economic perspective to contain the risk of prolonged independence wars. In addition, both trade

[54]Note that in the notation of equation (15), the variance of the pooled estimates for N estimates for the relative importance of channel x in the post-independence period $s \in (1, \ldots, 40)$ is defined as $Var\left(\frac{1}{N}\sum_{i\in(1,\ldots,N)} \Delta_{x,s,i}\right) = \left(\frac{1}{N}\right)^2 \sum_{i\in(1,\ldots,N)} Var(\Delta_{x,s,i}) + 2\sum_{1<i<j<N} Cov\left(\Delta_{x,s,i}, \Delta_{x,s,j}\right)$. To compute the covariance between the various estimates of the relative importance of the potential determinants of the independence dividends, we performed a bootstrap procedure that estimates the necessary pair of estimates by bootstrapping over the countries in our sample and subsequently computing the covariance in the pairwise estimates that correspond to each iteration of the bootstrap.

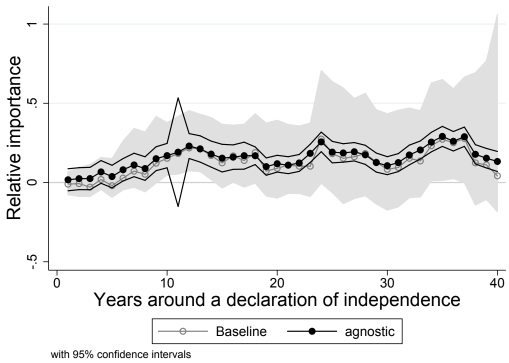
Figure A6: **Determinants of the independence dividend: agnostic estimates**



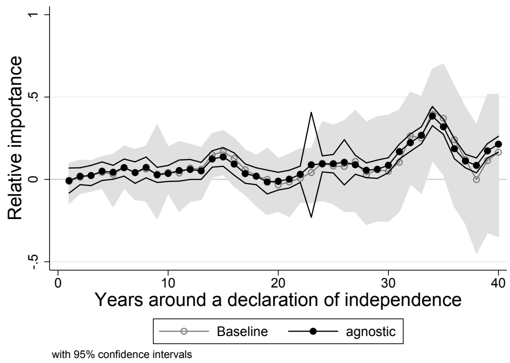
(a) Surface area



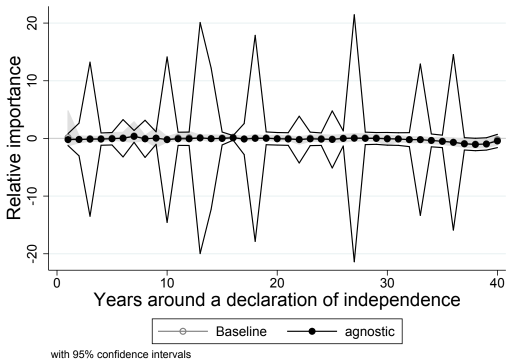
(b) Surface area loss



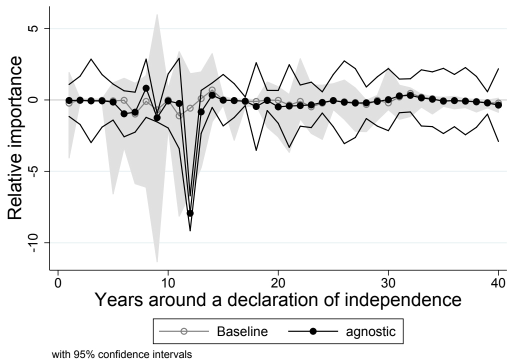
(c) Trade openness



(d) Democracy



(e) Macroeconomic uncertainty



(f) Battle deaths

Note: This figure compares baseline and agnostic estimates of the relative importance, as defined in equation (15), of several determinants of the independence dividend in a 40-year period following an independence declaration. The baseline estimates for relative importance is estimated by the two-step estimator described in equation (12) and contains a third-order polynomial in fixed capital and gross fixed capital formation to control for endogeneity; the agnostic estimates are computed as the average estimate accross the 18 estimation models discussed in sections 4.1 through 4.3 and giving each model equal weight. 95% bootstrapped confidence intervals are based on 500 replications. Controls for educational attainment, life expectancy and transition costs included but not shown.

openness and democratic institutions appear to significantly improve the long run growth prospects of newly formed states. Compared to the beneficial effect of opening up to trade, however, our results indicate that the impact of the democratization process tends to be more modest. We find no clear evidence that the effects of independence are unambiguously affected by territorial size or financial crises at any of these longer time horizons.

Table A4: **Second-step results: agnostic estimates**

Channel	$\hat{\beta}^{agnostic}_{t\leq 5}$	$\hat{\beta}^{agnostic}_{5<t<10}$	$\hat{\beta}^{agnostic}_{t\geq 10}$
Surface area	**-.056** *-.752 / .641*	**.01** *-.686 / .706*	**.03** *-.085 / .144*
Surface area loss	**-.011** *-45.2 / 45.178*	**-.059** *-45.248 / 45.13*	**.091** *-7.355 / 7.537*
Trade openness	**.034** *.02 / .048*	**.12** *.106 / .134*	**.18** *.177 / .183*
Democracy	**.026** *.014 / .037*	**.05** *.039 / .06*	**.113** *.11 / .115*
Macroeconomic uncertainty	**-.139** *-1.293 / 1.014*	**.03** *-1.245 / 1.306*	**-.211** *-.45 / .029*
Battle deaths	**-.067** *-.501 / .366*	**-.463** *-.76 / -.167*	**-.398** *-.456 / -.341*

Note: This table reports agnostic estimates for the relative importance of potential determinants of the independence dividend, computed as the weighted average estimate across all 18 available estimation models and giving each model equal weight. The agnostic estimates are pooled over the short run ($s \leq 5$), the medium run ($5 < s < 10$) and the long run ($s \geq 10$), respectively, where s refers to the number of post-independence years. Point estimates are reported along with their 90% bootstrapped confidence intervals, based on 500 iterations.

Table A5: **Newly Independent Countries: 1950-2016**

Country	*Year*	*Country*	*Year*	*Country*	*Year*
Libya	1951	Zanzibar	1963	Vanuatu◇	1980
Cambodia*	1953	Malawi◇	1964	Antigua & Barbuda◇	1981
Laos	1953	Malta*◇	1964	Belize◇	1981
German Democratic Republic	1954	Zambia◇	1964	St. Kitts and Nevis	1983
Republic of Vietnam	1954	Gambia◇	1965	Brunei◇	1984
Vietnam	1954	Maldives	1965	Federated States of Micronesia	1986
German Federal Republic	1955	Singapore*◇	1965	Marshall Islands	1986
Morocco◇	1956	Zimbabwe◇	1965	Liechtenstein	1990
Sudan	1956	Barbados◇	1966	Namibia◇	1990
Tunisia	1956	Basutoland (Lesotho)◇	1966	Armenia*◇	1991
Ghana◇	1957	Botswana◇	1966	Azerbaijan◇	1991
Malaysia◇	1957	Guyana◇	1966	Belarus◇	1991
Guinea*	1958	Yemen People's Republic	1967	Estonia*◇	1991
Benin◇	1960	Equatorial Guinea	1968	Georgia*◇	1991
Burkina Faso	1960	Mauritius◇	1968	Kazakhstan◇	1991
Cameroon	1960	Nauru	1968	Kyrgyzstan◇	1991
Central African Republic	1960	Swaziland◇	1968	Latvia*◇	1991
Chad	1960	Fiji◇	1970	Lithuania*◇	1991
Congo	1960	Tonga	1970	Moldova◇	1991
Cyprus◇	1960	Bahrain◇	1971	Russia◇	1991
Democratic Republic of the Congo◇	1960	Bhutan◇	1971	Tajikistan◇	1991
Gabon	1960	Oman	1971	Turkmenistan*◇	1991
Ivory Coast	1960	Qatar◇	1971	Ukraine*◇	1991
Madagascar	1960	United Arab Emirates◇	1971	Uzbekistan*◇	1991
Mali	1960	Bangladesh◇	1972	Bosnia and Herzegovina*	1992
Mauritania	1960	Bahamas◇	1973	Croatia*◇	1992
Niger	1960	Grenada◇	1974	San Marino	1992
Nigeria◇	1960	Guinea-Bissau◇	1974	Slovenia◇	1992
Senegal	1960	Angola◇	1975	Andorra	1993
Somalia	1960	Cape Verde◇	1975	Czech Republic◇	1993
Togo	1960	Comoros◇	1975	Eritrea	1993
Kuwait	1961	Mozambique◇	1975	Macedonia*	1993
Sierra Leone◇	1961	Papua New Guinea◇	1975	Monaco	1993
Syria	1961	Sao Tome and Principe◇	1975	Slovakia◇	1993
Tanzania	1961	Suriname◇	1975	Palau*◇	1994
Algeria*◇	1962	Seychelles◇	1976	East Timor*◇	2002
Burundi◇	1962	Djibouti◇	1977	Montenegro*◇	2006
Jamaica*◇	1962	Dominica	1978	Serbia◇	2006
Ruanda◇	1962	Solomon Islands◇	1978	Kosovo	2008
Samoa*	1962	Tuvalu	1978	South Sudan*	2011
Trinidad and Tobago◇	1962	Kiribati◇	1979		
Uganda◇	1962	St. Lucia◇	1979		
Kenya◇	1963	St. Vincent and the Grenadines◇	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).
◇ indicates countries included in the synthetic control algorithm (see section 3).

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

	t = 0 + 1				**t = 0 + 5**				**t = 0 + 20**			
Country	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$
Algeria	-.25	-.225***	-.213***	-.125***	-.465	-.44***	-.381***	-.188***	-.204	-.18***	-.022	-.024
Angola	-.567	-.517***	-.495***	-.675**	-.584	-.534***	-.478***	-.651*	-.583	-.533***	-.371***	-.568**
Antigua & Barbuda	.485	.442***	.446***	.323**	1.031	.987***	1.01***	.922***	1.246	1.202***	1.349***	1.663***
Armenia	-1.138	-.866***	-.8***	-.735**	-1.125	-.853***	-.748***	-.609*	-.588	-.315***	-.088	-.266
Azerbaijan	-.767	-.683***	-.617***	-.32*	-1.63	-1.547***	-1.441***	-1.055**	-.416	-.333***	-.106	.29
Bahamas	-.611	-.551***	-.524***	-.348	-.841	-.781***	-.716***	-.432	-.638	-.578***	-.385***	.1
Bahrain	-.335	-.112	-.091	-.08	-.385	-.163	-.112	-.287	-.820	-.597***	-.378***	-.979
Bangladesh	-.282	-.268***	-.251***	-.28**	-.369	-.356***	-.299***	-.318*	-.436	-.422***	-.246***	-.316
Barbados	-.193	-.086	-.058	-.079	-.293	-.187***	-.151**	-.172*	-.901	-.795***	-.717***	-.428
Basutoland (Lesotho)	.077	.086***	.114***	.002	-.194	-.186***	-.149***	-.26**	-.203	-.194***	-.117**	-.479
Belarus	-.29	-.333***	-.267***	-.374	-.684	-.727***	-.622***	-.764*	-.024	-.067**	.16***	-.264
Belize	.001	-.147**	-.145***	-.041	-.026	-.174***	-.16***	-.02	.065	-.082	.033	1.096**
Benin	-.199	-.2***	-.209***	-.121*	-.248	-.25***	-.237***	-.168**	-.36	-.361***	-.268***	-.205**
Bhutan	-.582	-.213	-.202***	-.146*	-.47	-.101	-.059*	-.035	-.219	.151	.351***	-.132
Botswana	.024	.079***	.107***	.139**	.288	.343***	.379***	.42***	.895	.950***	1.028***	1.296***
Brunei	.704	.374**	.375***	-.188	.002	-.327**	-.272**	-.735**	-.051	-.38**	-.183	-.614
Burundi	-.133	-.11***	-.098***	-.098*	-.182	-.16***	-.1**	.002	-.738	-.716***	-.558***	.024
Cape Verde	-.548	-.409***	-.39***	-.099	-.17	-.031	.03	.409**	.032	.171***	.33***	.752***
Comoros	-.468	-.476***	-.454***	-.397***	-.507	-.515***	-.458***	-.48***	-.293	-.301***	-.138**	-.289
Croatia	-.35	-.685***	-.627***	-.34	-.243	-.578***	-.488***	-.017	-.455	-.79***	-.594***	-.404
Cyprus	.374	.418***	.422***	.112	-.106	-.062	-.036	.039	-.258	-.214***	-.126*	.785**
Czech Republic	-.424	-.366***	-.31***	-.111	-.476	-.418***	-.34***	-.065	-.572	-.514***	-.3***	-.108
Democratic Republic of the Congo	-.279	-.277***	-.273***	-.162**	-.286	-.284***	-.258***	-.128**	-.877	-.874***	-.787***	-.65***
Djibouti	.022	-.064**	-.027	-.326*	.113	.027	.065	-.244	-.537	-.623***	-.458***	-.233
East Timor	-1.35	-.207**	-.157***		-1.649	-.506**	-.387***					

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continued

Country	t = 0 + 1				t = 0 + 5				t = 0 + 20			
	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$
Estonia	-.313	-.414***	-.348***	-.237	-.322	-.422***	-.317***	-.252	.038	-.062	.165**	-.115
Fiji	-.126	.206***	.216***	.299**	.617	.949***	.99***	1.207***	.239	.571***	.752***	1.499***
Gambia	.12	.131***	.161***	.215**	-.01	0	.049	.11	-.214	-.204***	-.097*	-.164
Georgia	-1.146	-.982***	-.916***	-.729***	-1.533	-1.369***	-1.267***	-1.018***	-.988	-.825***	-.605***	-.627***
Ghana	-.031	-.03	-.04	-.098**	.086	.087**	.081***	-.001	-.481	-.48***	-.392***	-.542***
Grenada	-.119	-.038	-.023	-.113	-.303	-.223***	-.156***	-.132	.082	.162***	.368***	.606
Guinea-Bissau	.423	.211***	.207***	.05	.312	.1**	.149***	.07**	.336	.124***	.292***	.207
Guyana	-.163	.022	.042	.013	.019	.204***	.232***	-.038	-.156	.029	.098	-.364
Jamaica	.106	.079	.095*	-.016	.086	.059	.123**	.009	-.775	-.802***	-.661***	-.611**
Kazakhstan	-.488	-.49***	-.424***	-.118	-.883	-.885***	-.78***	-.419**	-.234	-.236***	-.009	.089
Kenya	-.13	-.113***	-.084*	-.062**	-.083	-.066**	-.01	-.088*	-.437	-.42***	-.309***	-.178
Kiribati	.32	.156	.192	-.269***	-.019	-.183	-.161	-.375***	-.75	-.914***	-.779***	-.008
Kyrgyzstan	-.65	-.469***	-.403***	-.118	-1.282	-1.1***	-.995***	-.570**	-1.189	-1.007***	-.781***	-.32*
Latvia	-.777	-.765***	-.699***	-.55**	-.91	-.898***	-.793***	-.666**	-.511	-.499***	-.272***	-.375
Lithuania	-.406	-.484***	-.418***	-.439**	-.711	-.788***	-.683***	-.706***	-.299	-.376***	-.149***	-.477**
Malawi	.001	.011	.027	-.031	.072	.081***	.106**	-.005	-.222	-.212***	-.125***	-.377
Malaysia	-.128	-.132**	-.136***	-.062	-.157	-.161***	-.161***	-.007	-.245	-.249***	-.148**	-.031
Malta	-.112	-.077***	-.063	.071	-.259	-.223***	-.203***	-.004	.197	.233***	.303***	1.101***
Mauritius	-.132	-.189***	-.176***	-.233***	-.096	-.152***	-.125***	-.153**	.481	.424***	.516***	.273
Moldova	-1.111	-.624***	-.558***	-.24**	-1.846	-1.359***	-1.254***	-.868*	-1.256	-.769***	-.542***	-.343
Montenegro	-.292	.06*	.117***	-.039	-.187	.165***	.225***	.304***				
Morocco	-.126	-.125***	-.126***	-.099	-.346	-.345***	-.326***	-.288*	-.823	-.822***	-.711***	-.622
Mozambique	-.077	-.245***	-.224***	-.339	-.112	-.281***	-.224***	-.22**	-.168	-.337***	-.175***	-.008
Namibia	-.284	-.143***	-.093***	-.273	-.196	-.054**	.038	-.264*	.185	.327***	.548***	-.091
Nigeria	-.164	-.166***	-.162***	-.083**	-.119	-.121***	-.095*	-.013	-.051	-.053*	.035	.068
Palau	-.535	0	.062	-.014	-.933	-.398***	-.321***	-.112	-.389	.146***	.344***	.136

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Country	**t = 0 + 1**				**t = 0 + 5**				**t = 0 + 20**			
	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$	$\hat{\beta}_{jt}$	$\hat{\beta}_{jt}^{tDD}$	$\hat{\beta}_{jt}^{DDD}$	$\hat{\beta}_{jt}^{pure}$
Papua New Guinea	-.057	.025	.046	.27	.093	.174*	.23***	.479	-.221	-.139	.023	.607
Qatar	.257	.029	.052	.053	.645	.417***	.465***	-.083	-.168	-.396***	-.195**	-1.858**
Ruanda	.063	.052**	.064**	-.06	.1	.089***	.149***	-.081	.109	.098***	.256***	.084
Russia	-.589	-.423***	-.361***	-.199**	-1.028	-.861***	-.766***	-.541	-.533	-.366***	-.152**	-.342**
Sao Tome and Principe	-.452	-.413***	-.386***	-.111	-.704	-.665***	-.589***	-.128	-1.748	-1.71***	-1.508***	-.782*
Serbia	-.169	.007	.079**		-.154	.021	.109***					
Seychelles	-.04	.038**	.083***	-.086	-.103	-.025	.039	-.231	-.141	-.063***	.104*	-.4**
Sierra Leone	.105	.103***	.108***	.112	.044	.042***	.088***	.096*	.167	.165***	.289***	.416
Singapore	-.016	.035	.065	.039***	.109	.161***	.206***	.232***	.133	.184***	.255***	.579***
Slovakia	-.535	-.376***	-.32***	-.159	-.476	-.317***	-.24***	-.055	-.372	-.213**	0	.074
Slovenia	-.578	-.536***	-.479***	-.407	-.553	-.511***	-.419***	-.402	-.847	-.805***	-.606***	-.663
Solomon Islands	.596	.507**	.544***	.483*	.672	.583***	.604***	.292	-.183	-.272	-.119	1.143
St. Lucia	-.464	-.545***	-.502***	.238	-.088	-.169	-.135	.549	-.163	-.244**	-.07	1.671*
St. Vincent and the Grenadines	.575	.582***	.625***	.451*	.711	.719***	.753***	.811**	.689	.697***	.871***	1.89*
Suriname	.49	.371***	.393***	.312**	.078	-.041	.015	.575	-1.251	-1.37***	-1.208***	.168
Swaziland	.235	.144*	.159**	.109	.542	.45***	.496***	.067	.684	.593***	.744***	-.589
Tajikistan	-.916	-.672***	-.618***	-.403	-1.916	-1.672***	-1.58***	-1.242**	-1.685	-1.441***	-1.229***	-.797
Trinidad and Tobago	.055	.052	.066	.006	.04	.037	.1*	.008	.123	.12***	.242***	.102
Turkmenistan	-.338	-.255***	-.189***	-.072	-.771	-.687***	-.582***	-.352	-.461	-.377***	-.151**	-.225
Uganda	-.266	-.253***	-.239***	.015	-.553	-.539***	-.477***	.036	-.97	-.957***	-.835***	-.605*
Ukraine	-.289	-.273***	-.211***	-.171	-1.117	-1.101***	-1.004***	-.898*	-.862	-.845***	-.621***	-.602**
United Arab Emirates	.024	-.23***	-.209***	-.142	.560	.306***	.356***	-.119	.694	.44***	.659***	-.751*
Uzbekistan	-.626	-.482***	-.416***	-.292	-.976	-.831***	-.726***	-.482	-.719	-.574***	-.348***	-.291
Vanuatu	.011	.035	.064	-.23	.121	.145**	.171***	-.079	-.318	-.294***	-.142*	.463**
Zambia	.226	.184***	.203***	.153	.031	-.01	.023	-.004	-.516	-.557***	-.473***	-.583
Zimbabwe	-.104	-.128***	-.103**	-.241*	0	-.024	.018	.021	-.337	-.361***	-.268***	-.831

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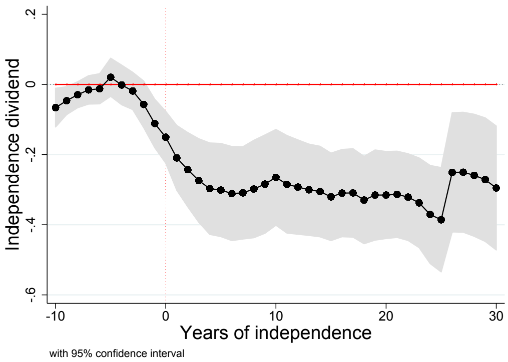
continued

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

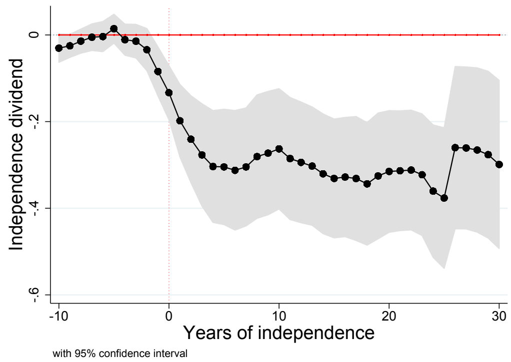
Note: This table reports country-specific, semi-parametric estimates of the independence dividend. Results are reported for all available NICs and pertain to the 1^{st}, 5^{th} and 20^{th} year after independence respectively. Columns headed by $\hat{\beta}_{jt}$ report the estimated percentage difference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

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

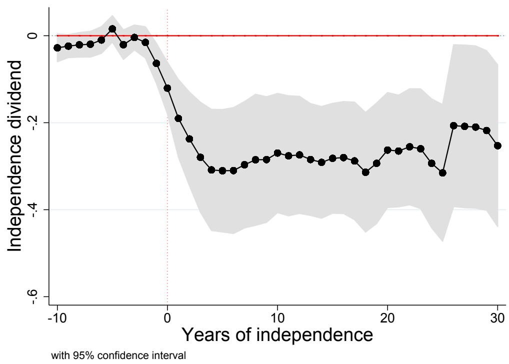
Figure A7: **Average impact of secession in selected countries**



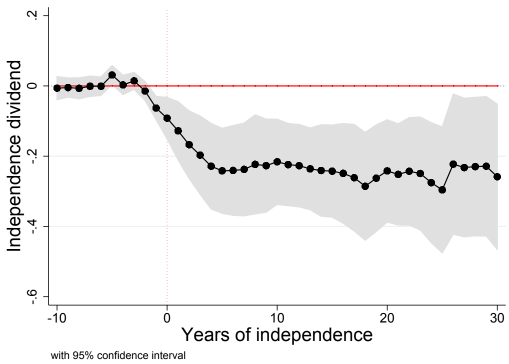
(a) All cases



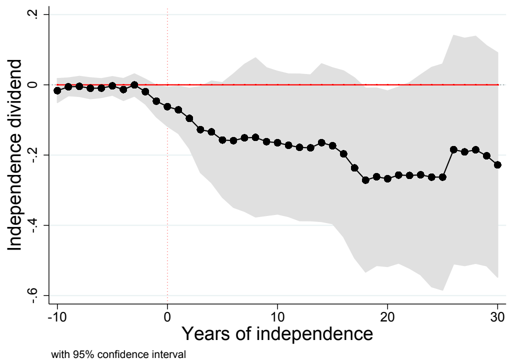
(b) 80% best matched cases



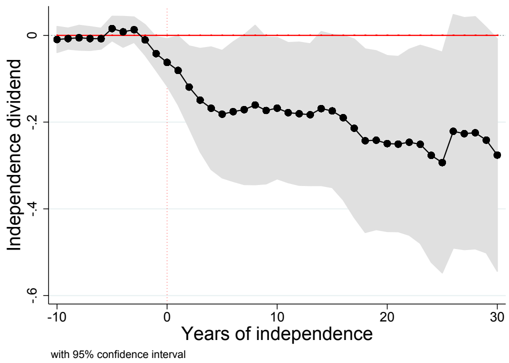
(c) 60% best matched cases



(d) 40% best matched cases



(e) 20% best matched cases

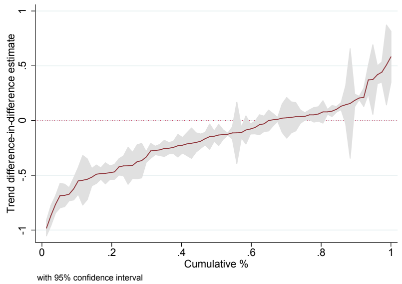


(f) All cases within caliper

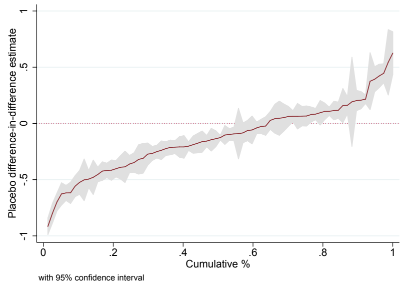
Note: This figure plots the yearly average percentage gap between NICs and their synthetic counterparts, along with the 95% confidence interval. The number of years before (-) or after (+) independence are indicated on the horizontal axis. The top-left panel contains all available cases, subsequent panels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

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

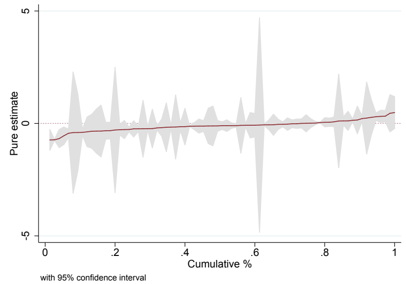
1^{st} year after independence



(a) Trend-demeaned

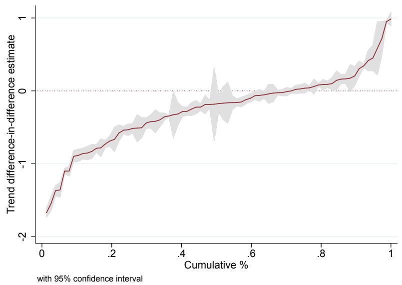


(b) Triple-difference

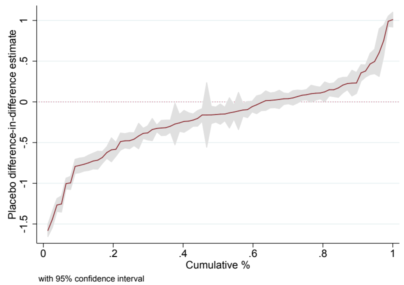


(c) Pure

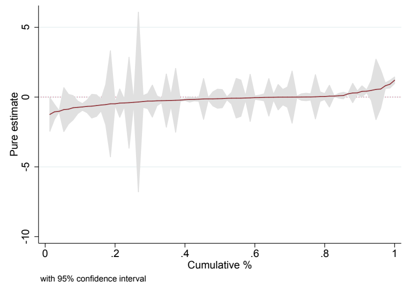
5^{th} year after independence



(a) Trend-demeaned

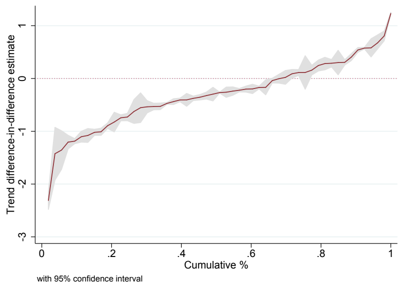


(b) Triple-difference

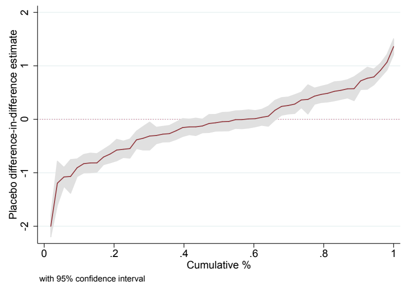


(c) Pure

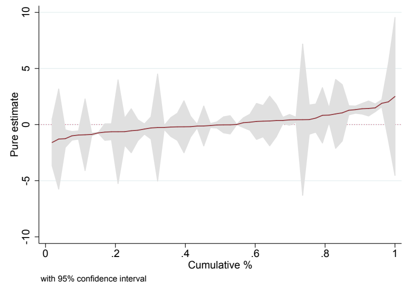
30^{th} year after independence



(a) Trend-demeaned



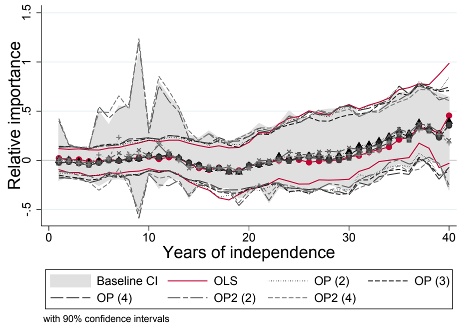
(b) Triple-difference



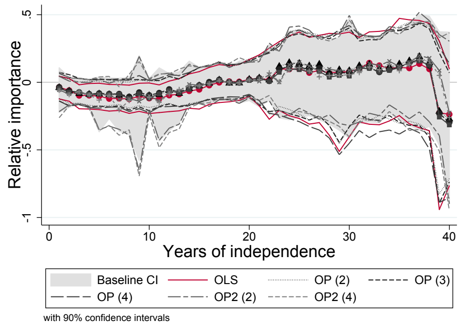
(c) Pure

Note: This figure plots the cumulative distribution of the country-specific trend-demeaned, triple-difference and pure independence dividend estimates reported in table A5, along with 95% confidence intervals. The horizontal axis indicates the proportion of NICs with an independence dividend estimate below the cut-off value indicated on the vertical axis. Estimated independence dividends pertain to the 1^{st}, 5^{th} and 30^{th} post-independence year respectively.

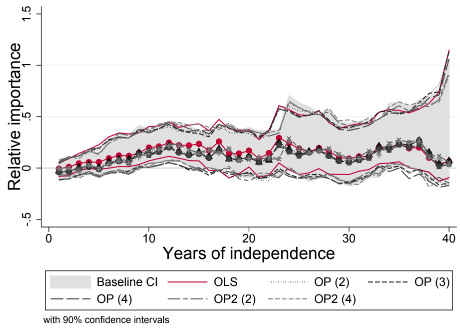
Figure A9: **Determinants of the independence dividend: comparing estimators**



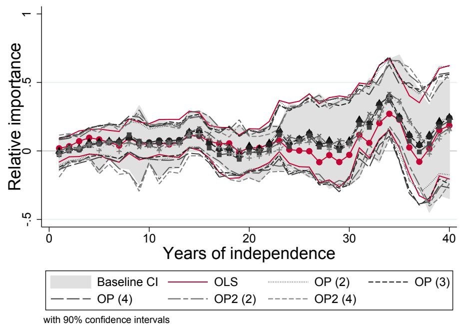
(a) Surface area



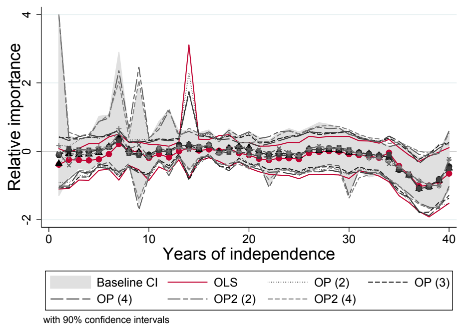
(b) Surface area loss



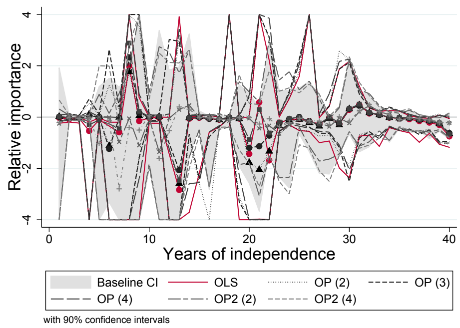
(c) Trade openness



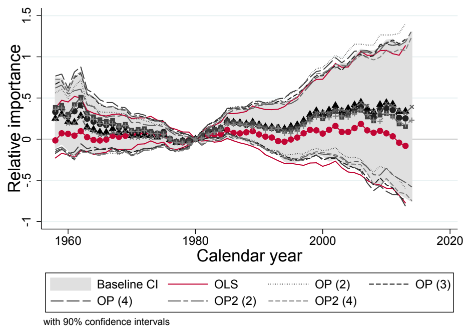
(d) Democracy



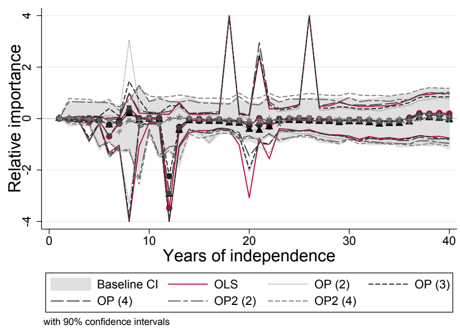
(e) Macroeconomic uncertainty



(f) Battle deaths



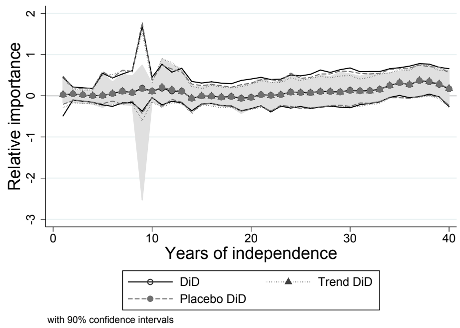
(g) Year fixed effects



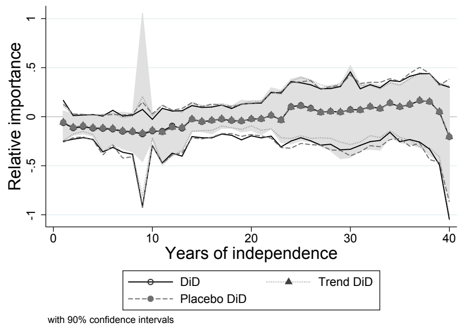
(h) Years-of-independence fixed effects

Note: This figure compares estimates of the relative importance, as defined in equation (15), of several determinants of the triple-difference independence dividend as estimated by the uncorrected OLS-estimator described in equation (5); the single-stage OP-estimator described in equation (10); and the two-stage OP2-estimator summarized in equation (12) for various choices of the order of the polynomial in fixed capital and gross fixed capital formation that are indicated between brackets. 90% bootstrapped confidence intervals are clustered at the country level and based on 500 replications. For reference, the relevant confidence intervals of the baseline model are plotted in gray.

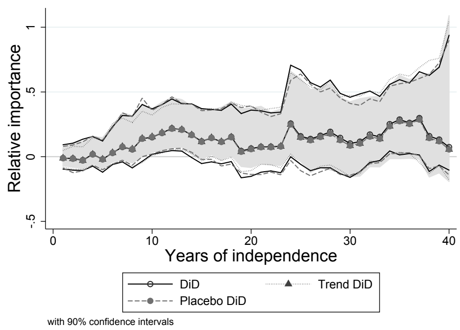
Figure A10: **Determinants of the independence dividend: comparing independence dividend estimates**



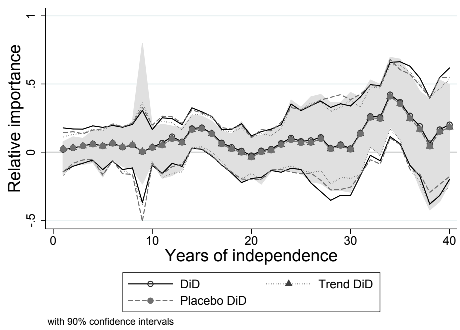
(a) Surface area



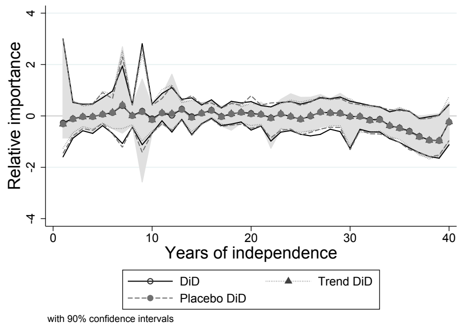
(b) Surface area loss



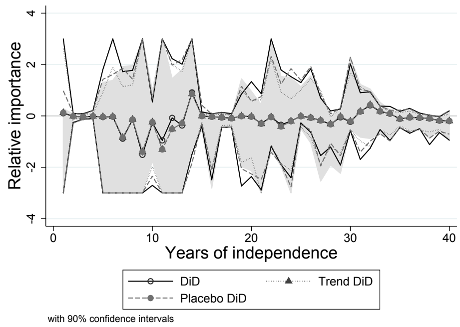
(c) Trade openness



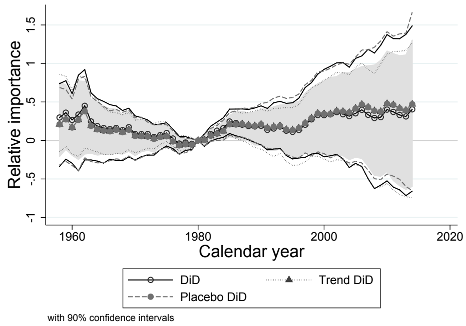
(d) Democracy



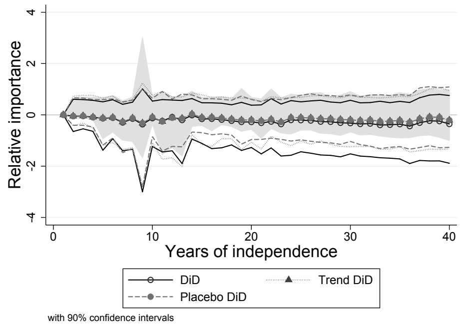
(e) Macroeconomic uncertainty



(f) Battle deaths



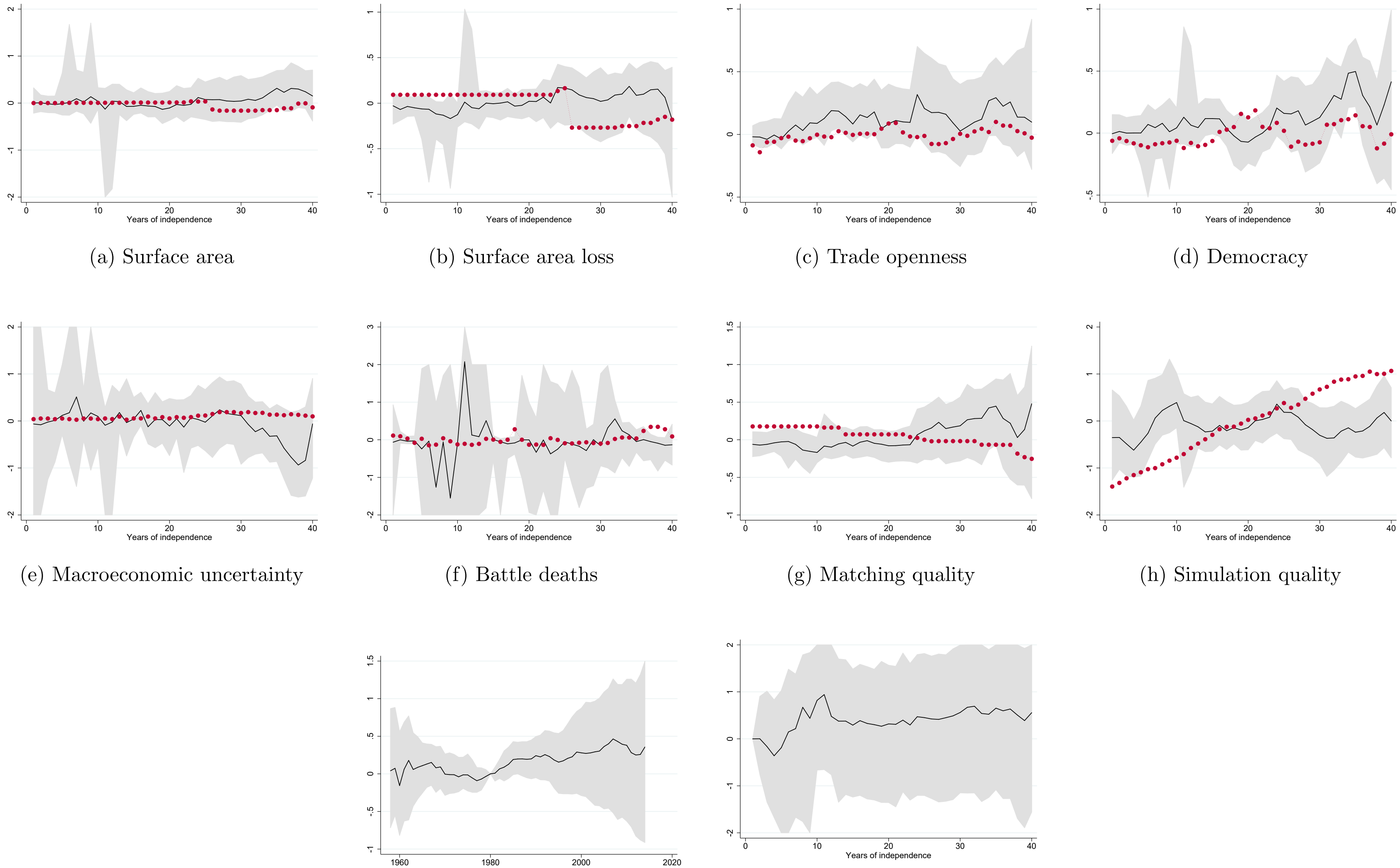
(g) Year fixed effects



(h) Years-of-independence fixed effects

Note: This figure compares estimates of the relative importance, as defined in equation (15), of several determinants of the independence dividend as estimated by the two-stage OP2-estimator with a third-order polynomial in fixed capital and gross fixed capital formation and summarized in equation (17A). 90% bootstrapped confidence intervals are clustered at the country level and based on 500 replications. For reference, the relevant confidence intervals of the baseline model are plotted in gray.

Figure A11: **Determinants of the independence dividend: controlling for matching & simulation quality**



(a) Surface area

(b) Surface area loss

(c) Trade openness

(d) Democracy

(e) Macroeconomic uncertainty

(f) Battle deaths

(g) Matching quality

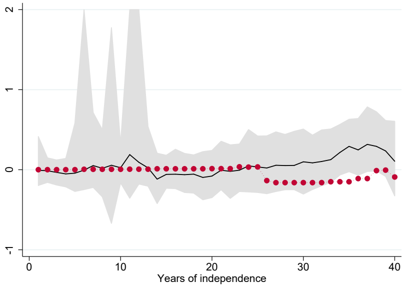
(h) Simulation quality

(i) Year fixed effects

(j) 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 500 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. Controls for human capital differences and transition costs are included but not reported.

Figure A12: **Determinants of the independence dividend: controlling for per capita GDP**

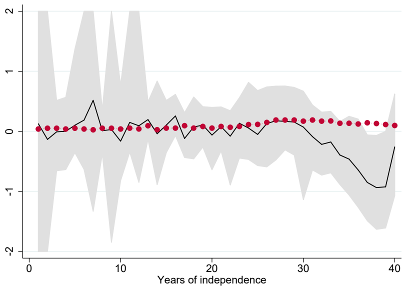


(a) Surface area

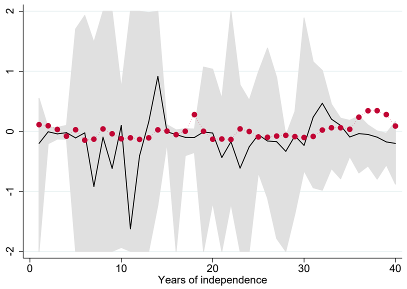
(b) Surface area loss

(c) Trade openness

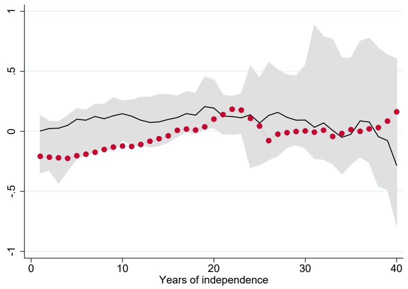
(d) Democracy



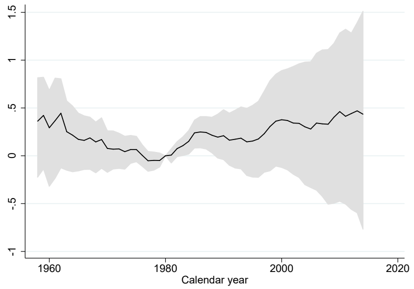
(e) Macroeconomic uncertainty



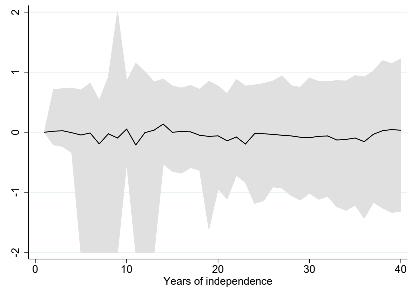
(f) Battle deaths



(g) Per capita GDP



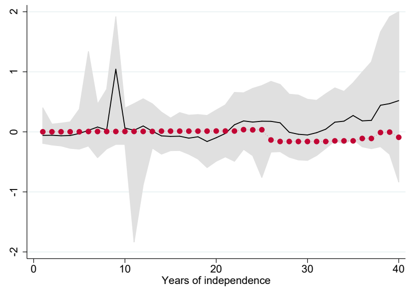
(h) Year fixed effects



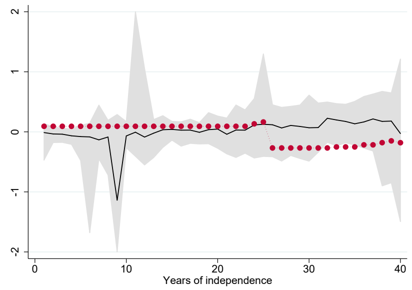
(i) Years-of-independence fixed effects

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 500 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. Controls for human capital differences and transition costs are included but not reported.

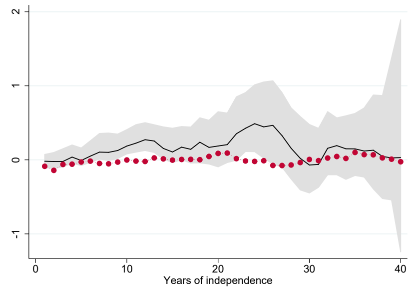
Figure A13: **Determinants of the independence dividend: controlling for per capita GDP, consitutional features, state capacity & independence by referendum**



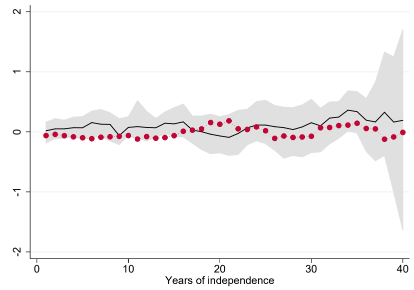
(a) Surface area



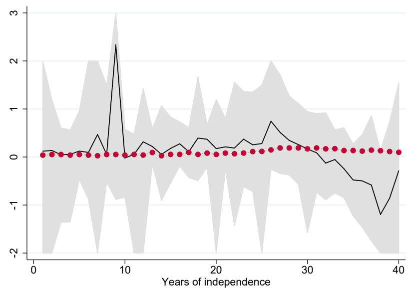
(b) Surface area loss



(c) Trade openness

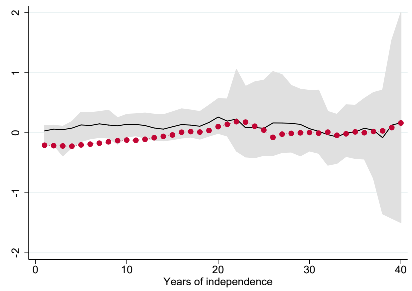


(d) Democracy

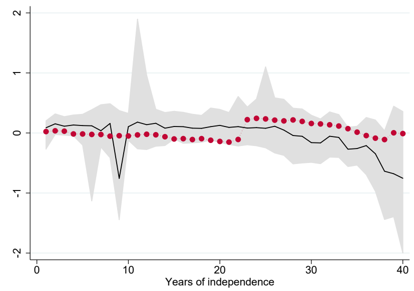


(e) Macroeconomic uncertainty

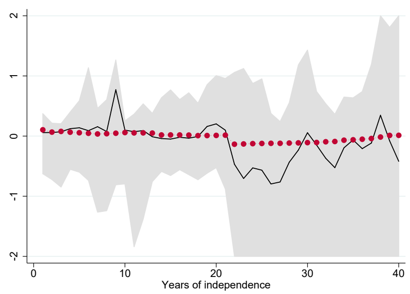
(f) Battle deaths



(g) Per capita GDP

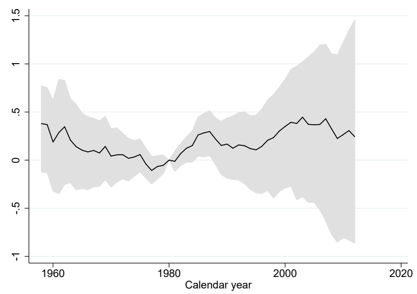


(h) Constitutional quality

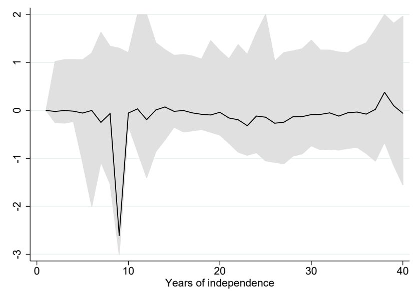


(i) State capacity

(j) Independence-by-referendum



(k) Year fixed effects



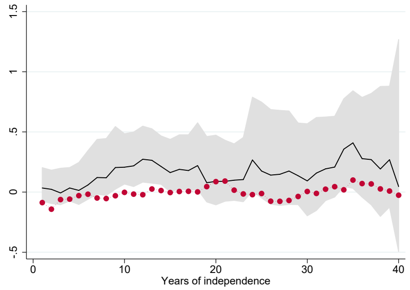
(l) Years-of-independence fixed effects

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 500 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. Controls for human capital differences and transition costs are included but not reported.

Figure A14: **Determinants of the independence dividend: controlling for location and international organization membership**

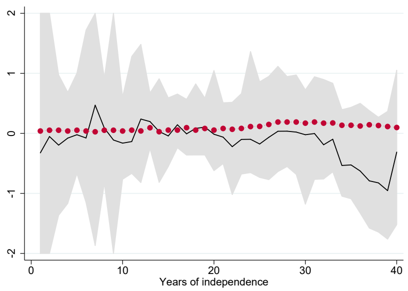
(a) Surface area

(b) Surface area loss

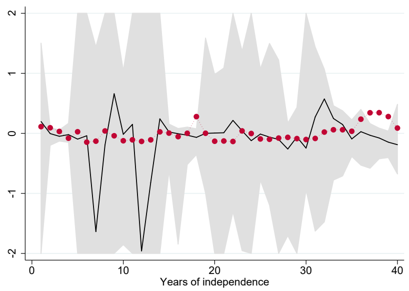


(c) Trade openness

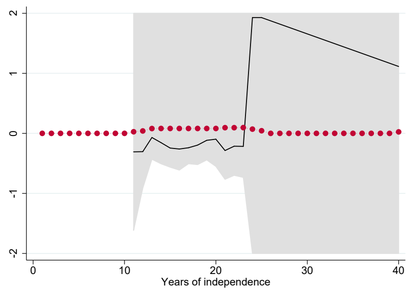
(d) Democracy



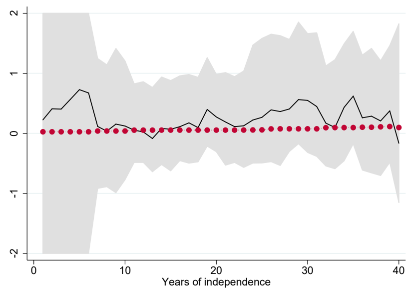
(e) Macroeconomic uncertainty



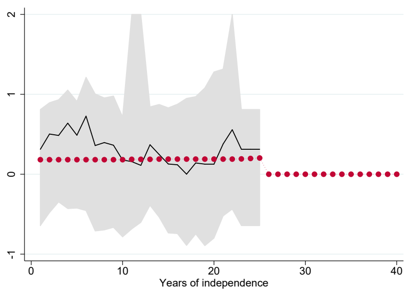
(f) Battle deaths



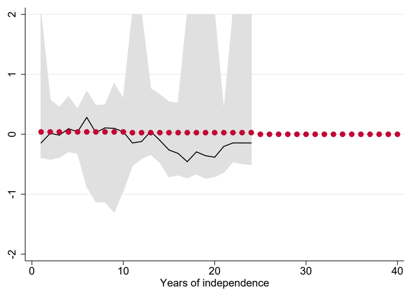
(g) European union fixed effect



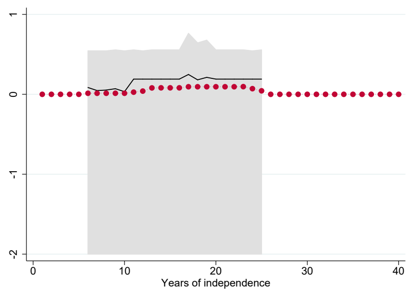
(h) OPEC fixed effect



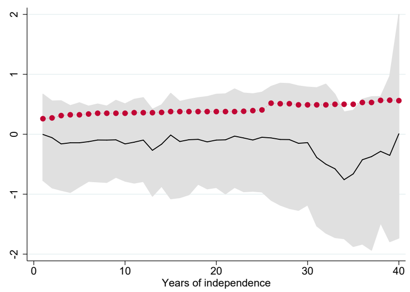
(i) Former Soviet members



(j) Former Yugoslav members

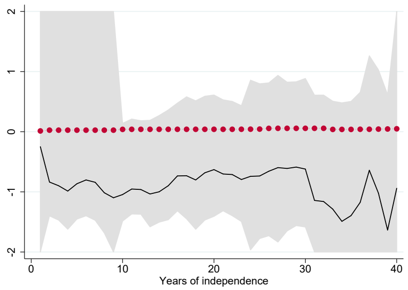


(k) NATO fixed effect

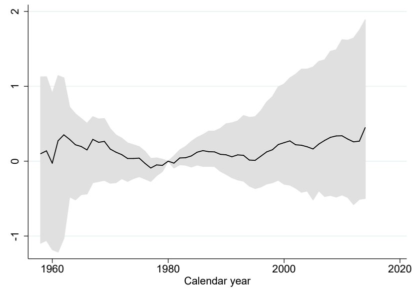


(l) African Union fixed effect

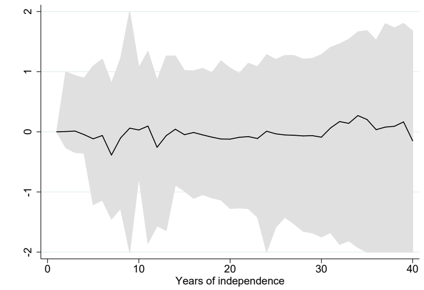
Note: The top and the middle row plot yearly estimates of the relative importance, as defined in equation (15), of several determinants of the raw independence dividend (black line) against their sample-average values (red lines). 90% bootstrapped confidence intervals, clustered at the country level and based on 500 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. Controls for human capital differences and transition costs are included but not reported.



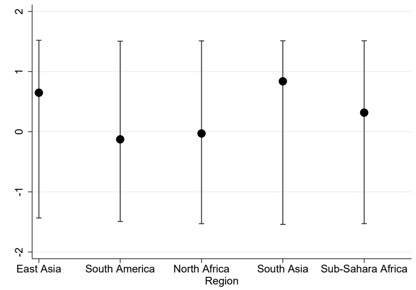
(m) ASEAN fixed effect



(n) Year fixed effects



(o) Years-of-independence fixed effects



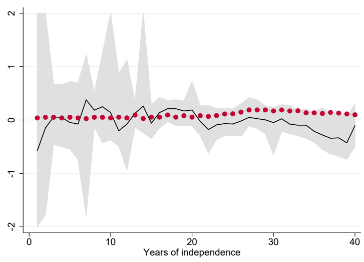
(p) Region fixed effects

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 500 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. Controls for human capital differences and transition costs are included but not reported.

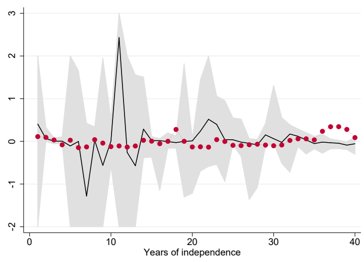
Figure A15: **Determinants of the independence dividend: controlling for country fixed effects**

(a) Trade openness

(b) Democracy



(c) Macroeconomic uncertainty



(d) Battle deaths

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 500 replications, are plotted in gray. The figure only includes estimates for the relative importance of the main time-varying potential determinants of the independence dividend. As in all other estimation models, this model also controls for human capital differences, transition costs, year fixed effects and years-of-independence fixed effects.