AUTOCRATIC TRANSITIONS AND GROWTH

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ABSTRACT

In this paper we use a transparent statistical methodology – synthetic control methods – to implement data-driven comparative studies about the impact of autocratic transition on real per capita GDP. The applied methodology compares the growth of countries that experienced a transition to autocracy with the growth of a convex combination of similar countries that remained democratic, and it accommodates for the time-varying impact of unobservable heterogeneity. We implement this statistical framework on a panel of 160 countries, and focus on 14 episodes of transition from democracy to autocracy. We find that the effects of autocratic transitions come in all shapes and sizes, since our data are split in almost equal parts between insignificant, negative, and positive effects. We also find that negative effects tend to get worse over time, and that African countries are badly affected by the autocratic transition possibly because of a resource curse.

Keywords: autocracy, democracy, growth, synthetic control methods.

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1. Introduction

The economic growth literature has witnessed three main strands of research. The first, in the 50s and 60s, was primarily concerned with the accumulation of capital, along Solovian lines. The second, in the 70s and 80s, was concerned with the policies that governments may implement in order to achieve sustained growth. The third claims that institutions are the main determinants of growth. Probably the most basic institutional feature concerns the democratic vs. autocratic nature of the state. With this respect, the results of the literature have been disappointing, uncovering a very weak relationship between democracy and growth.

Recently, Persson and Tabellini (2006, 2008) have explored several issues on the relationship between democracy and growth emphasizing the role of heterogeneity by using a semi-parametric methodology (i.e., diff-in-diff propensity score matching) that relaxes linearity and is therefore well suited for the study of this feature. They find much larger effects than those commonly found in the literature. In particular, transitions from democracy to autocracy have an average negative effect on growth of about 2 percentage points, which implies a 45% income drop at the end of their sample.

In this paper, we apply the synthetic control method proposed by Abadie and Gardeazabal (2003) and Abadie *et al*. (2010) to perform data-driven comparative case studies. We evaluate the effect of a binary treatment – autocratic transition – on real per capita income in a panel framework. The proposed methodology accounts for the presence of a time-varying impact of country unobservable characteristics, and therefore overcomes a major drawback of more standard estimators. Put in a different way, we ask whether the establishment of an autocratic regime in year T_0 will lead to higher growth in the years $T_0 + i$ compared to similar countries that remained democratic. We therefore estimate the dynamic treatment effects of autocratic transition on growth over time (within a 10-year horizon). The advantage of this approach rests on the transparent construction of the counterfactual outcome of the treated country, that is, a linear combination of untreated countries: the synthetic control. The comparison countries that form the synthetic control (and their relative weights) are selected based on their similarity to the treated country before the treatment takes place, both with respect to past realizations of the outcome and the standard covariates used in the growth literature. The transparency of this (matching) algorithm safeguards against drawing inference from (disputable) parametric extrapolation.

To anchor our results to the existing literature, we use the data set by Persson and Tabellini (2006, 2008). Because the synthetic control method is best suited for sharp regime changes, we define as autocratic transition – the treatment of interest – a drop of at least 10

points in the Polity IV Index (Marshall and Jaggers, 2009) making a country cross the democratic threshold at zero. We are, therefore, able to identify 14 autocratic transitions in our data, which range from 1963 to 2000. Note that the diff-in-diff matching results by Persson and Tabellini (2008) are based on 18 autocratic transitions satisfying their common support condition, 10 of which overlap with our set of feasible country experiments.

We find that the effects of autocracy transitions come in all shapes and sizes since our dataset is split in almost equal parts between insignificant, negative, and positive effects. We also find that negative effects tend to get worse over time, and that African countries are badly affected by these transitions, possibly because of a specific resource curse.

The paper is organized as follows. Section 2 reviews the literature on the effects of both democracy and autocracy on growth. Section 3 outlines the statistical methodology. Section 4 presents the data and discusses the empirical results. Section 5 concludes.

2. Literature review

2.1 Democracy and growth

The literature on democracy and growth has uncovered an extremely weak (and more often negative) relationship between these variables. We can distinguish between two strands of empirical literature: the first employing cross-sectional data, and the second using panel data.[1] There is also a small literature on the effects of autocracy on growth that we take into account.

Barro (1996) analyses a cross-section of about 100 countries from 1960 to 1990. The dependent variables are the growth rate of real per capita GDP over 1965-1975, 1975-1985, 1985-1990 using a system of three equations. Estimations are done by instrumental variables: the instruments are the five-year earlier value of log GDP, the actual values of schooling, life-expectancy, rule of law and terms of trade, and the earlier values of the other variables (which include the fertility rate, government consumption ratio, public educational spending ratio, black market premium, investment ratio) involved in the estimations. Democracy is measured by Bollen (1960) and the Gastil Index. The estimated coefficient of democracy is negative but not significantly different from zero, which Barro claims to be weakly negative. However, there is a positive nonlinear effect: the middle level of democracy is the most favorable to

[1] For earlier surveys of economic theories on the link between democracy and growth, see Przeworski and Limongi (1993) and Przeworski *et al.* (2000); de Haan and Siermann (1995) provide a useful summary table of several studies reporting the conclusion, the employed measure of democracy, the critiques to that measure, and the econometric specification.

growth, the lowest comes second, and the highest third. According to Barro, maintenance of the rule of law, free markets, small government consumption and high human capital are the most important determinants of growth.

In a similar setting, Tavares and Wacziarg (2001) aim at distinguishing between the possible channels that link democracy and growth. They identify eight variables that can possibly be endogenous to democracy, therefore weakening Barro results: political instability, governance distortions, government size, human capital, income inequality, trade openness, and physical capital accumulation. To achieve identification a number of exogenous variables are used, belonging to the following groups: cultural, demographic, gravity, historical, log of income per capita and its squared value. A system of eight simultaneous equations is estimated via three-stage least squares. Data are five-year averages of the variables involved in the analysis for 65 countries over 1970-1989. They find that the overall effect of democracy on growth is negative. This result is the outcome of a positive effect growth via human capital accumulation and reduction of income inequality, and a negative effect of reduced physical capital accumulation and increased government consumption.

Starting from the inconclusive results of this literature, Aghion *et al*. (2007) decompose the effect of democracy on several sectors, characterized by different productivity levels and patterns of technological development. Using UNIDO data for 28 manufacturing sectors for 180 countries for the period 1963 to 2003, they find that democratic institutions and political rights enhance growth of sectors close to the technological frontier. Second, they notice that political rights are associated with freedom of entry and the latter is especially important for sectors close to the technological frontier. Third, more advanced economies benefit more from democratic institutions and therefore the demand for democracy should increase with the level of per capita income in a country.

A more recent wave of research applies panel data with a diff-in-diff methodology.[2] In particular, Giavazzi and Tabellini (2005) and Persson and Tabellini (2006, 2008) apply this methodology on about 160 countries over the period 1960-2000, exploring issues ranging from the relationship between political and economic liberalizations to the effect of democracy on growth, taking into account (time-invariant) unobservable heterogeneity. In

[2] Note that this methodology considers as *treated* those countries that underwent a single transition from autocracy to democracy, and as *control* those countries that remained autocracies. The methodology rests on two assumptions. First, without any regime change, growth in treated countries should counterfactually have been the same as in control countries, other things being equal. Second, heterogeneity in the effects of democracy should be unrelated with the occurrence of democracy itself.

general, these results seem more favorable to a positive link between democracy and growth. Their treatment definition is similar to ours, as they focus on regime change rather than the simple (cross-sectional) comparison of different regimes.

Giavazzi and Tabellini (2005) study the timing of political and economic liberalizations. They find that political reforms cause economic liberalizations, but they cannot rule out feedback effects in both directions. Moreover, countries that first liberalize and then become democracies do much better than countries that pursue the opposite sequence. Persson and Tabellini (2006) report that a new parliamentary democracy is more prone to pursue economic liberalizations than a new presidential democracy but, given the results of Giavazzi and Tabellini (2005), liberalizations following democratizations have weaker effects on growth. Parliamentary democracies raise government consumption much more than presidential democracies, reducing growth. Spending effects of the electoral system are less pronounced and do not affect growth.

Grosjean and Senik (2010) aim at disentangling the direction of causality from democracy to support for a market economy, and from market development to support for democracy. In doing so they use a spatial regression discontinuity approach based on frontier-zones in Central and Eastern Europe and in the former Soviet Union that are under different political regimes but are integrated. They find that a positive and significant effect of democracy on support for a market economy, but no effect of the impact of market liberalization on support for democracy.

Persson and Tabellini (2009) note that, if democracy has a positive effect on growth and long-run income, it also raises the returns to investment. Because investment reacts to expected returns, expected, and not just actual, regime change affects growth. Growth will accelerate before an anticipated democratization, and decelerate well before an anticipated coup. In their model the probability of regime change depends on a country's "democratic capital". This capital is assumed to accumulate in years of democracy and in countries with democratic neighbors, but to depreciate under autocracy. The results are consistent with the model in the usual sample, and also in another one dating from 1850 to 2000.

Persson and Tabellini (2008) explore issues on the relationship between democracy and growth emphasizing the role of heterogeneity. They combine the above mentioned method with the propensity score matching estimator. This semi-parametric methodology relaxes linearity and it is therefore well suited for the study of heterogeneity, an issue already

in the background in the early studies on democracy and growth.[3] The cost is a loss in the efficiency of the estimates. They also find that the discrepancies relative to the parametric results are driven by large differences in the composition of the treatment and control groups, making linearity a doubtful assumption. Moving to the results, they uncover a positive but insignificant effect of transitions from autocracy to democracy.

Papaioannou and Siourounis (2008) consider democratization processes for about 65 countries over 1960-2000. They employ an event study approach and analyze growth before and after democratizations. The dynamic panel estimates imply that democratizations are associated with a one percent increase in real per capita growth. Furthermore, during the transition, growth is slow and even negative; after the third post-democratization year, growth peaks and stabilizes at a higher level.

Acemoglu *et al*. (2008) are interested in the causal relationship between income and democracy, that is, the "modernization hypothesis": the increase in industrialization, urbanization, wealth and education lead to more democracy. They employ two strategies: first use country fixed effects to get rid of country specific characteristics that may affect both variables. Second, they apply an instrumental variables strategy, by using past savings rates and changes in the incomes of trading partners. The estimated coefficients are not significantly different from zero both in the post-WWII era and over 100 years. The authors claim that the positive "correlation between changes in income and democracy is caused some societies have embarked on divergent development paths at some *critical junctures* during the past 500 years" (Acemoglu *et al*., 2008: 813). They identify these critical junctures with factors dating back 500 years ago (constraints on the executive, year of independence, religious affiliation). In a companion work, Acemoglu *et al*. (2010) look at the effect of income on transition to and from democracy. Again, they show that in a linear model with country fixed effects the correlation disappears, and this is also true in double hazard model for the simultaneous estimation of these two possible transitions.

2.2 Autocracy and growth

[3] Diff-in-diff propensity score matching, similarly to the synthetic control method we apply in this paper (see Section 3), relaxes the linearity assumption of the standard diff-in-diff and, by transparently checking for the existence of a common support between treated and control units, avoid drawing inference from parametric extrapolation. However, this methodology, unlike ours, still relies on the assumption that the impact of unobservable confounders must be time-invariant.

The economic analysis of the link between autocracy and growth is much less developed. The 'stationary bandits' theory of dictatorship (McGuire and Olson, 1996) explains different economic performances among autocracies in a model in which an autocrat maximizes his private consumption subject to the probability of staying in power. If a dictator expects to stay in power for a long period of time, he has an incentive to promote economic development because he will then increase his private consumption through increased tax revenues resulting from economic growth.[4]

Besley and Kudamatsu (2008) develop a theory on the accountability of an autocrat with respect to the 'selectorate', the group of individuals on whom the leader depends to hold into power. Good policy is implemented when the selectorate removes poorly performing leaders. This happens if the selectorate's hold on power is not too dependent on a specific leader being in office. The paper empirically establishes cases where autocracy has been successful according to various criteria. They use these case studies to identify the selectorate in specific instances of successful autocracy, and also show that leadership turnover in successful autocracies is higher than in unsuccessful autocracies. Finally, they demonstrate – by exploiting leadership deaths from natural causes – that successful autocracies appear to have found ways for selectorates to nominate successors without losing power.

In our framework, the treatment of interest is not autocracy *per se* but autocratic transition. The estimation of the effects of autocratic transition has been highlighted in a few papers. As mentioned above, Persson and Tabellini (2008) find an average negative effect on growth of leaving democracy of about 2 percentage points, and this accounts for a 45 percent loss of income over the sample. Furthermore, the transparency of their matching estimator in small samples allows them to identify the countries excluded from the estimation because outside of the common support (see Table 13.3b), as well as the estimated treatment effect for each episode of autocratic transition (see Figure 13.5). Among their 18 autocratic transitions, 4 experienced a mildly positive growth effect (below 2 percentage points), 7 a mildly negative effect, and 7 a strong negative effect (above 2 percentage points). Below we will come back in more detail to the comparison of our country results with those of their study.

[4] Grossman and Noh (1994) and Overland *et al.* (2005) assume that a dictator's survival is more likely if he adopts welfare-enhancing policies. Acemoglu and Robinson (2006) assume that welfare-enhancing policies directly reduce the dictator's survival prospects while increasing the survival chance through competition for power with a challenger. Consequently, successful autocrats are either those who are secure enough or those who face tough competition from a challenger.

Finally, note that Persson and Tabellini's (2008) results complement those of Minier (1998), who finds that countries that become less democratic grow more slowly than comparable countries. In contrast, Durham (1999) discovers that policy discretion associated with less democratic regimes inhibits investment in poorer countries. This stands out against the theory. Also, single-party dictatorships have higher investment ratios, but do not grow faster than party-less regimes.

3. Methodology

In this paper, we apply the synthetic control method (SCM) – developed by Abadie and Gardeazabal (2003) and extended in Abadie *et al.* (2010) – to the investigation of the effects of autocratic transitions on growth. Under this approach, a weighted combination of potential comparison countries – the synthetic control – is constructed to approximate the most relevant characteristics of the country affected by the intervention. After a regime change (i.e., a transition to autocracy) takes place in a given country, the SCM can be used to estimate the counterfactual situation of this country in the absence of the regime change by looking at the outcome trend of the synthetic control. In our empirical application, we consider countries that become autocracies in a particular year and compare them with countries that remain democratic for at least 10 more years (or until the end of the sample).

To summarize the SCM approach, it is useful to reason in terms of potential outcomes in a panel set-up. Assume that we observe a panel of I_C +1 countries over T periods. Only country i receives the treatment (i.e., becomes an autocracy) at time $T_0 < T$, while the remaining I_C potential control countries remain democratic. The treatment effect for country i at time t can be defined as:

$$\tau_{it} = Y_{it}(1) - Y_{it}(0) = Y_{it} - Y_{it}(0) \qquad (1)$$

where Y_{it}(T) stands for the potential outcome associated with $T \in \{0,1\}$, that is, real GDP per capita according to whether the economy is democratic or autocratic. The estimand of interest is the vector of dynamic treatment effects ($\tau_{i,T0+1}$, ..., $\tau_{i,T}$). For any period $t > T_0$, the estimation of the treatment effect is complicated by the missing counterfactual $Y_{it}(0)$. Abadie *et al.* (2010) show how to identify the above dynamic treatment effects under the following general model for potential outcomes:

$$Y_{jt}(0) = \delta_t + v_{jt} \qquad (2)$$

$$Y_{jt}(1) = \delta_t + \tau_{jt} + v_{jt} \tag{3}$$

$$v_{jt} = Z_j \theta_t + \lambda_t \mu_j + \varepsilon_{it} \tag{4}$$

where Z_j is a vector of relevant observed covariates that are not affected by the intervention and can be either time-invariant or time-varying; θ_t is a vector of parameters; μ_j is a country-specific unobservable; λ_t is an unknown common factor; and ε_{jt} are transitory shocks with zero mean. In the present context, as all of the variables in Z_j (initial GDP, population growth, secondary school enrollment, and investment share) refer to the pre-treatment period, the assumption that they are not affected by the treatment means that we have to rule out "anticipation" effects, i.e., that those variables immediately change in response to the anticipation of the future reform. Interestingly, the above model allows for the impact of unobservable country heterogeneity to vary with time, while, on the contrary, the usual diff-in-diff (fixed-effects) specification imposes λ_t to be constant across time.

Define $W = (\omega_1, \ldots, \omega_{Ic})'$ as a generic ($I_C \times 1$) vector of weights such that $\omega_j \geq 0$ and $\sum \omega_j = 1$. Each value of W represents a potential synthetic control for country i. Further define $\overline{Y}_j^k = \sum_{s=1}^{To} k_s Y_{js}$ as a generic linear combination of pre-treatment outcomes. Abadie *et al*. (2010) show that, as long as we can choose W^* such that:

$$\sum_{j=1}^{Ic} \omega_j^* \overline{Y}_j^k = \overline{Y}_i^k \text{ and } \sum_{j=1}^{Ic} \omega_j^* Z_j = Z_i \tag{5}$$

then

$$\hat{\tau}_{it} = Y_{it} - \sum_{j=1}^{Ic} \omega_j^* Y_{it} \tag{6}$$

is an unbiased estimator of τ_{it}. Condition (5) can hold exactly only if $(\overline{Y}_i^k, Z_i)$ belongs to the convex hull of $[(\overline{Y}_1^k, Z_1), \ldots, (\overline{Y}_{Ic}^k, Z_{Ic})]$. Hence, the synthetic control W^* is selected so that condition (5) holds approximately: the distance (or pseudo-distance) between the vector of pre-treatment characteristics of the treated country and the vector of the pre-treatment characteristics of the potential synthetic control is minimized with respect to W^* and according to a specified metric.[5] The deviation from condition (5) imposed by this

[5] In the empirical analysis, we use the (data-driven) distance metric calculated by the Stata routine *synth*, available at: www.people.fas.harvard.edu/_jhainm/software.htm. See Abadie *et al*. (2010) for technical details.

implementation process, however, can be assessed in the data, and it should be shown as a complementary output of the analysis.

In words, the synthetic control algorithm estimates the missing counterfactual as a weighted average of the outcomes of potential comparison countries (i.e., the synthetic control of the treated country). The weights are chosen so that the pre-treatment outcome and the covariates of the synthetic control are, on average, very similar to those of the treated country. This approach comes with the evident advantages of transparency (as the weights W^* identify the countries that are used to estimate the counterfactual outcome of the country that became autocratic) and flexibility (as the set of I_C potential controls can be appropriately restricted to make the underlying country comparisons more sensible). Furthermore, the SCM rests on identification assumptions that are weaker than those required by estimators commonly applied in the growth literature. As discussed above, while standard panel models can only control for confounding factors that are time invariant (fixed effects) or share a common trend (diff-in-diff), the model specified in equation (4) allows the effect of unobservable confounding factors to vary with time.

The only limitation of the SCM is that it does not allow to assess the significance of the results using standard (large-sample) inferential techniques, as the number of units in the control pool and the number of periods covered by the sample are usually quite small in comparative case studies like ours. As suggested by Abadie *et al.* (2010), however, placebo experiments can be implemented to draw inference. Below we implement cross-sectional placebo tests, which consist in applying the SCM to every country in the pool of potential controls; this is meant to assess whether the estimated effect for the treated country is large relative to the effect for a country chosen at random. In particular, placebo testing compares the estimated treatment effect for the country under investigation with all the (fake) treatment effects of the control countries, obtained from experiments where each control country is assumed to shift to autocracy in the same year of the treated country. If the estimated effect in the treated country is larger than those in most of the (fake) experiments, we can safely conclude that the baseline results are not just driven by random chance.

4. Data and empirical results

Moving to the data, we define as autocratic transition a drop of at least 10 points in the Polity IV Index (Marshall and Jaggers, 2009) by crossing the zero value.[6] The value of zero is the

[6] The "Polity Score" captures this regime authority spectrum on a 21-point scale ranging from -10 (hereditary monarchy) to +10 (consolidated democracy). Appendix I briefly describes the transitions in our sample.

standard threshold used in the literature to distinguish autocracies (negative values) from democracies (positive values). Starting with a data set of about 160 advanced and developing countries, we identify 14 episodes of transition to autocracy. The time-span runs from 1963 to 2000, but for each country we end up using different time-spans based on the year in which the transition took place. In fact, we use 1963 to T_0 as the pre-treatment period, and T_0 to T_0+5 (or T_0+10) as the post-treatment periods, where T_0 is the year of the autocratic transition. The set of control variables used in this paper includes those usually employed in the growth literature (initial GDP, investment as a share of GDP, population growth, and secondary school enrollment). The outcome – real per capita GDP – and the control variables are drawn from the data set by Persson and Tabellini (2006, 2008).

We now discuss each SCM country experiment and the associated placebo test, in order. In every experiment, the synthetic control is constructed from a worldwide pool of potential comparisons countries: that is, all eligible countries that are a democracy and remain so within ten years after the treatment at time T_0.[7] Tables 1 through 3 compare the treated countries and their synthetic control before and after the transitions by both per capita GDP and explanatory variables; the root mean squared prediction error (referred to the pre-treatment fit) is also reported. Appendix II records the countries that are included in the donor pool of each SCM experiment, and Appendix III lists those that are indeed selected as components of the estimated synthetic control (in particular, those having a weight higher than 0.02). Figures 1 through 14 allow a graphical evaluation of the pre- and post-treatment fit by looking at the proximity of the outcome trends of each treated country (solid line) and its synthetic control (dashed line) before and after T_0 (i.e., on the left and on the right of the vertical dashed line, respectively). These figures – in the right panel – also report the placebo tests for each SCM experiment. There, the solid line represents the pre- and post-treatment outcome difference between the treated country and its synthetic control (i.e., the counterpart of the baseline estimation results reported in the left panel of each figure), while the dashed lines refer to the (fake) placebo experiments where each of the potential comparison countries is assumed to be the treated country at T_0.

[7] As a robustness check, in the spirit of Billmeier and Nannicini (2009b), for each treated country we also implemented an alternative experiment with a restricted donor pool of potential controls, including only countries in the same macro area of the treated country (OECD, Latin America, Asia, or Africa). For most experiments, the pre-treatment fit was quite poor; in the remaining feasible experiments, however, the findings were qualitatively similar to those presented in this section (results available upon request).

Table 1 considers OECD and Asian economies. The economic consequence of the transition looks negligible for Greece, as its real per capita GDP five years later is very similar, only slightly lower, to the outcome of the synthetic control. For Greece, the SCM experiment ends in 1974 (the year of a new transition from autocracy to democracy) and we therefore cannot report the outcome difference at T_0+10. Figure 1 shows that the patterns of real per capita income in the treated country and synthetic control are very close to each other after T_0 (left panel), and the placebo test confirms the insignificance of the dynamic treatment effects (right panel). The autocratic transition in Greece had no significant impact on growth. This is in contrast with the result by Persson and Tabellini (2008, Figure 13.5), who estimate a strong negative treatment effect for Greece.

For the Philippines, we have almost no effect of the autocratic transition at T_0+5 (-0.01 percentage points per year), and a small positive effect (+0.59) ten years after the transition. In Figure 2, the synthetic control, in fact, shows an excellent fit of GDP both before and after the treatment. In the case of South Korea, the economic outcome of the transition is strongly positive in the first five years (+7.92 percentage point per year), and still sizable – albeit smaller – ten years after the coup (+3.98). Figure 3 illustrates this baseline result in the left panel, and the placebo test confirms its robustness, because the post-treatment difference between South Korea and its synthetic control is the upper bound of all the differences in the (fake) placebo experiments. Pakistan presents a zero effect at T_0+5, and an extremely small effect at T_0+10. The insignificance of the effect is confirmed by the placebo test in the right panel of Figure 4. While the results for Philippines and Pakistan are not inconsistent with those by Persson and Tabellini (2008, Figure 13.5), we find a strong positive effect for South Korea, as opposed to a mildly negative effect in their study.

[Table 1 about here]

[Figures 1-4 about here]

Table 2 considers Latin American economies. Again, the effects of autocratic transitions are mixed. Panama displays a mildly positive effect five years after the coup (+1.59), but this effect is strongly reduced at T_0+10 (+0.35). Figure 5 (left panel) shows that there is a growth acceleration immediately before the coup, a positive gap is then created with respect to similar countries that remained democratic, and from 1973 there is a growth reduction that brings Panama back on the same path of its synthetic control. The placebo test (right panel) confirms the robustness of the positive effect after five years (because it is

greater than those of most fake experiments), but not after ten years. Similarly, a small positive effect is found for Uruguay (+0.65), but in this case the effect is strongly reinforced in the longer run (+3.10 at T_0+10). The placebo test in the right panel of Figure 7 confirms the positive effect for Uruguay, especially at the end of the post-treatment period.

[Table 2 about here]

[Figures 5-8 about here]

Peru and Chile, instead, exhibit negative effects. The former, five years after the coup, had a decline of -0.65 percentage points per year in GDP with respect to similar democratic countries, and this effect reached -3.10 after ten years. The latter experienced a decrease of 4.48 percentage points per year within five years, and -1.56 after ten years. The case of Chile is interesting because it is widely recognized as a success story in Latin America. Indeed, in Figure 8, we observe a serious drop in GDP in the years after the Pinochet takeover, due to the harsh fight between the *junta* and opponents, but this is followed by a strong recovery that brings the country back to the growth pattern of the other countries of the control, when free-market policies are implemented. Yet, the recovery is far from complete at the end of the post-treatment period. The placebo tests for Peru (Figure 6) and Chile (Figure 8) confirm the baseline negative results, as the post-treatment outcome difference between the treated country and its synthetic control is the lower bound of all the differences in the (fake) placebo experiments. For Latin America, Persson and Tabellini (2008, figure 13.5) find the same qualitative result for Uruguay and Peru, but the opposite result for Chile and Panama.

Table 3 considers African economies. In the analysis of African countries, a more consistent and negative effect of autocratic transitions emerges, and this outcome tends to exacerbate over time. This is the case of Uganda (-0.51 percentage points per year in the first five years after the transition, and -4.03 after ten years), Nigeria in 1984 (-3.12 and -3.81), Sierra Leone (-0.67 and -0.90), and Gambia (-3.33 in five years).[8] Mildly positive effects can be found in the transitions of Lesotho (+0.52 after five years and +2.93 after ten years) and Nigeria in 1966 (+1.10 and +0.91). The placebo tests, however, do no always confirm the robustness of the above results. Only the negative results of the experiment for Gambia are clearly confirmed (see the right panel of Figure 14), because the, negative, post-treatment solid line is the lower bound of all the others. At a closer look, however, also Uganda and Nigeria in 1984 display a negative impact of the autocratic transition that is higher than those

[8] For Gambia we can only estimate GDP at T_0 + 5 due to data limitations.

of most placebo experiments: this is only overshadowed – in Figures 9 and 11, respectively – by a scale effect (that is, by the fact that some experiments involving richer countries display very large differences in absolute terms). The results for Nigeria in 1966, Sierra Leone, and Lesotho are not robust to placebo testing. At the end of the day, all of the three African experiments that pass the placebo check show a negative impact of autocratic transition on growth.

The results for Africa are not easily comparable to those by Persson and Tabellini (2008) because Uganda, Lesotho, and Nigeria fall outside their common support and are therefore excluded by the estimates. Yet, their results for Sierra Leone are consistent with ours, while those for Gambia are not.

[Table 3 about here]

[Figures 9-14 about here]

The negative results in Africa are probably related with the presence of a "resource curse" (see, among others, Sachs and Warner, 2001): when an oligarchy puts its hands over natural resources, it tends to extract rents form them and not to invest in the development of the country. In this respect, note that Nigeria is an oil-exporting country; Sierra Leone is a producer of diamonds, iron and bauxite; and Uganda sells abroad copper and cobalt. Gambia does not fall into this group, but the downturn was mainly due to economic sanctions set by the United Kingdom – its major trading partner – after the coup. Indirectly, this conclusion is reinforced by the circumstance that the first autocratic transition in Nigeria did not produce a negative effect on growth in a time in which oil was not yet a source of income for the government: it accounted for about 9% of GDP in 1966 and for about 18% in 1984 (Sala-i-Martin and Subramanian, 2003); in terms of total revenues it raised from 25% to 74% (Forrest, 1992).[9] This is also consistent with Acemoglu *et al.* (2010) who claim that military coups are more likely in resource-rich countries.

5. Conclusions

In this paper we have analyzed the economic effect of leaving democracy in 14 episodes through a novel econometric approach for case studies, the synthetic control method. This approach allows taking into account a time-varying impact of country heterogeneity, and therefore overcomes a major drawback of more standard estimation techniques.

[9] Lesotho became an important diamonds producer in the second half of the '70s.

We get a number of interesting results. First, we find an almost equal split of our episodes between positive (South Korea, Panama, and Uruguay), negative (Chile, Peru, Uganda, Nigeria in 1984, and Gambia), and insignificant (Greece, Philippines, Pakistan, Nigeria in 1966, Sierra Leone, and Lesotho) consequences of autocratic transitions on economic growth. The negative effect shown by Chile is more the outcome of a very large medium-term fall in GDP that takes a long time to get recovered. With this respect, we find evidence contrasting with the (more pronounced) negative results found by Persson and Tabellini (2008). Second, when the effect of the transition is negative, it tends to worsen over time (with the significant exception of Chile), whereas the opposite is not usually true. Third, the negative effects on GDP in Africa are possibly due to the interaction of the autocratic transition with a resource curse.

Acknowledgements

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Table 1: Predictor and outcome means for the autocracy treatment (OECD and Asia)

	Greece 1967	Synthetic Control
Secondary school	43	36.85
Population growth	0.610	1.713
Investment share	0.338	0.204
Average Pre-treatment GDP	4185.7	4186.9
GDP at $T_o + 5$	7670	7862.75
GDP at $T_o + 10$		
RMSPE		71.436
	Philippines 1972	Synthetic Control
Secondary school	37.6	24.69
Population growth	3.055	2.554
Investment share	0.150	0.137
Average Pre-treatment GDP	792.5	789.5
GDP at $T_o + 5$	1070	1106.75
GDP at $T_o + 10$	1180	1113.25
RMSPE		4.403
	South Korea 1972	Synthetic Control
Secondary school	34.53	8.27
Population growth	2.467	1.792
Investment share	0.200	0.143
Average Pre-treatment GDP	1640.83	1649.24
GDP at $T_o + 5$	3460	2478.08
GDP at $T_o + 10$	4170	2982.56
RMSPE		117.534
	Pakistan 1977	Synthetic Control
Secondary school	12.62	22.76
Population growth	2.903	2.216
Investment share	0.138	0.126
Average Pre-treatment GDP	238.76	239.42
GDP at $T_o + 5$	346	348.35
GDP at $T_o + 10$	410	422.69
RMSPE		14.587

Notes: The table shows the mean values of predictors and outcomes for the treated country and the synthetic control, respectively. Predictors: pre-treatment real GDP per capita, secondary school enrollment, population growth, investment share. Outcome: real GDP per capita. The value of each predictor is averaged over the pre-treatment period. The values of the outcome refer to five years ($T_0 + 5$) and ten years ($T_0 + 10$) after the treatment year T_0. RMSPE stands for Root Mean Squared Prediction Error.

Table 2: Predictor and outcome means for the autocracy treatment (Latin America)

	Panama 1968	Synthetic Control
Secondary school	31.5	11.58
Population growth	2.913	2.192
Investment share	0.205	0.124
Average Pre-treatment GDP	1763.75	1766.45
GDP at $T_o + 5$	2600	2407.95
GDP at $T_o + 10$	2690	2596.94
RMSPE		41.151
	Peru 1968	Synthetic Control
Secondary school	20	26.16
Population growth	2.842	2.661
Investment share	0.352	0.128
Average Pre-treatment GDP	2116.25	2116.131
GDP at $T_o + 5$	2450	2849.94
GDP at $T_o + 10$	2480	3633.14
RMSPE		23.549
	Uruguay 1972	Synthetic Control
Secondary school	46.6	27.62
Population growth	0.985	3.425
Investment share	0.123	0.326
Average Pre-treatment GDP	3859.16	3888.76
GDP at $T_o + 5$	4470	4328.88
GDP at $T_o + 10$	4710	3593.12
RMSPE		199.020
	Chile 1973	Synthetic Control
Secondary school	31.8	23.8
Population growth	2.160	2.98
Investment share	0.188	0.164
Average Pre-treatment GDP	2224.61	2218.96
GDP at $T_o + 5$	2340	3015.79
GDP at $T_o + 10$	2300	2726.61
RMSPE		46.154

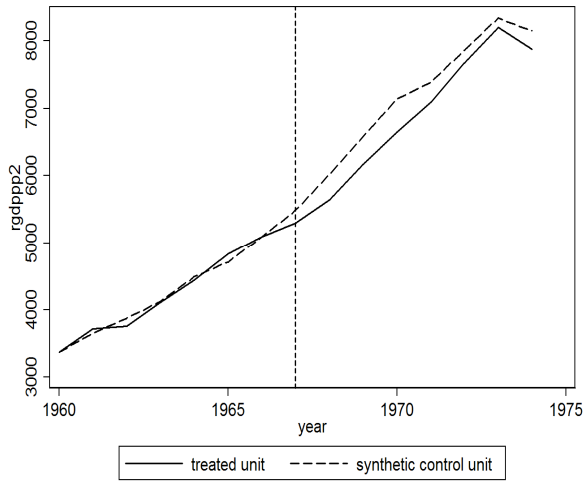
Notes: See above.

Table 3: Predictor and outcome means for the autocracy treatment (Africa)

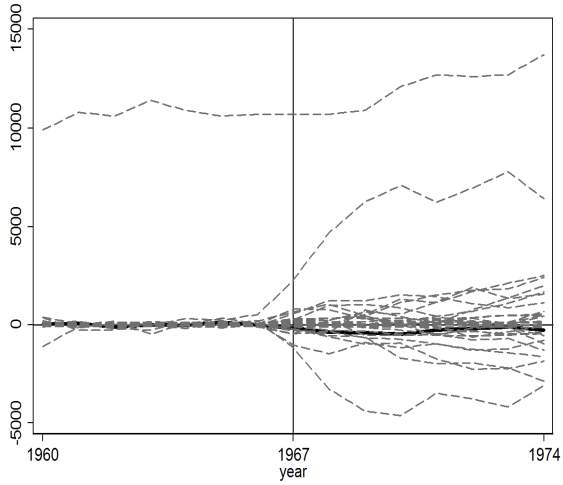
	Uganda 1966	Synthetic Control
Secondary school	3.5	5.78
Population growth	3.954	2.529
Investment share	0.016	0.031
Average Pre-treatment GDP	205.66	208.23
GDP at $T_o + 5$	233.71	239.87
GDP at $T_o + 10$	200.63	335.35
RMSPE		0.000028
	Nigeria 1966	Synthetic Control
Secondary school	4.5	19.027
Population growth	2.568	2.423
Investment share	0.042	0.139
Average Pre-treatment GDP	230.5	230.74
GDP at $T_o + 5$	294	278.58
GDP at $T_o + 10$	319	295.29
RMSPE		2.781
	Nigeria 1984	Synthetic Control
Secondary school	17.21	28.48
Population growth	2.809	2.277
Investment share	0.078	0.125
Average Pre-treatment GDP	276.2	279.54
GDP at $T_o + 5$	246	357.99
GDP at $T_o + 10$	254	410.86
RMSPE		38.980
	Sierra Leone 1971	Synthetic Control
Secondary school	5.33	18.24
Population growth	1.679	2.5140
Investment share	0.021	0.145
Average Pre-treatment GDP	242.27	242.70
GDP at $T_o + 5$	278	287.68
GDP at $T_o + 10$	289	317.61
RMSPE		5.164
	Lesotho 1970	Synthetic Control
Secondary school	3.5	16.26
Population growth	1.992	2.541
Investment share	0.033	0.141
Average Pre-treatment GDP	206.8	207.70
GDP at $T_o + 5$	258	251.43
GDP at $T_o + 10$	364	281.46
RMSPE		9.642
	Gambia 1994	Synthetic Control
Secondary school	14.3	36.16
Population growth	3.254	2.203
Investment share	0.043	0.125
Average Pre-treatment GDP	354	362.73
GDP at $T_o + 5$	365	547.65
GDP at $T_o + 10$		
RMSPE		30.792

Notes: See above.

Figure 1 – Trends in real GDP per capita, Greece 1967

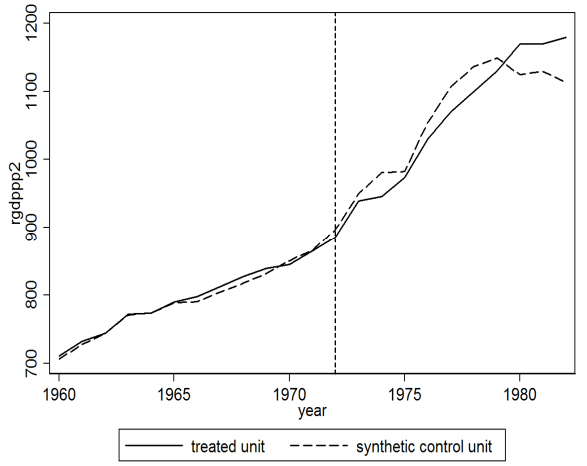


Greece vs. Synthetic Control

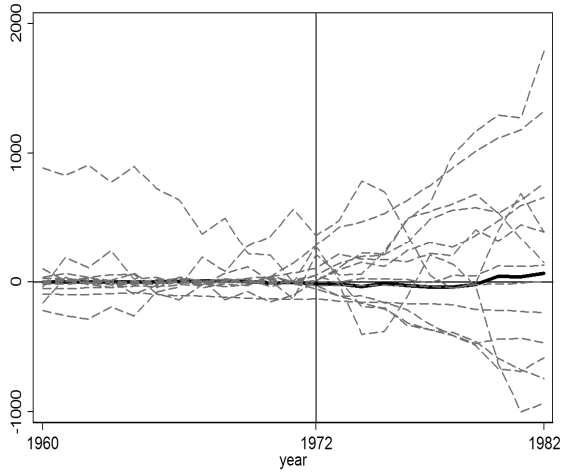


Placebo test

Figure 2 – Trends in real GDP per capita, Philippines 1972

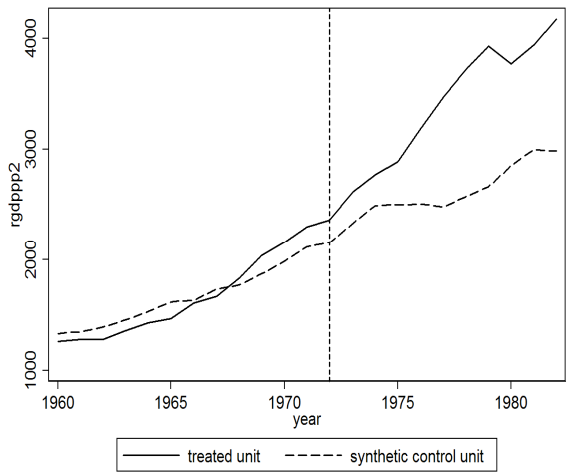


Philippines vs. Synthetic Control

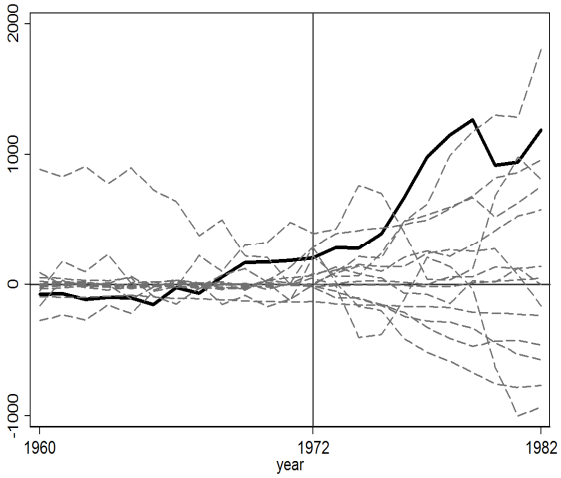


Placebo test

Figure 3 – Trends in real GDP per capita, South Korea 1972

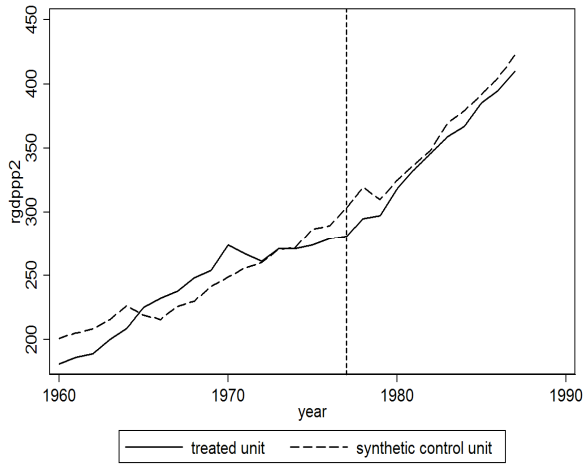


South Korea vs. Synthetic Control

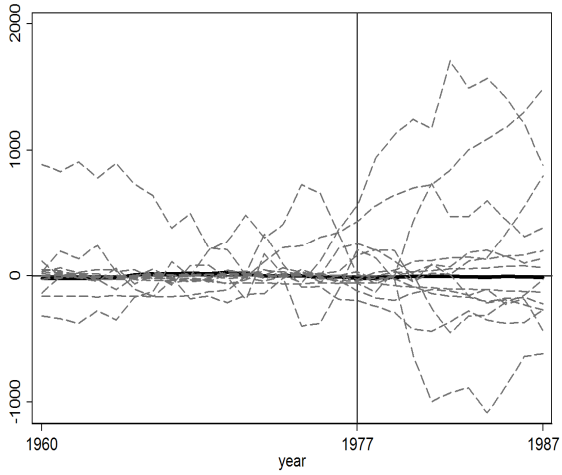


Placebo test

Figure 4 – Trends in real GDP per capita, Pakistan 1977

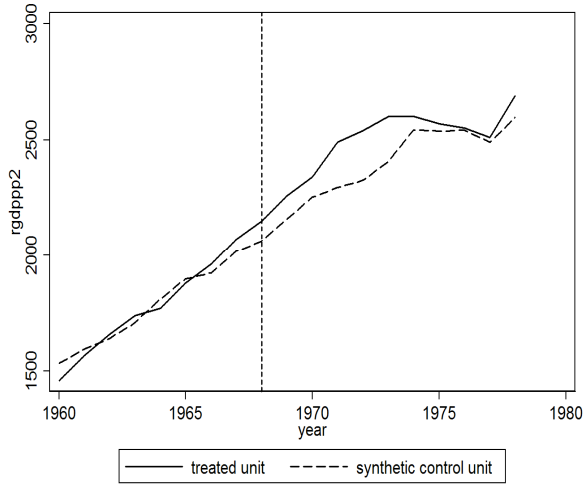


Pakistan vs. Synthetic Control

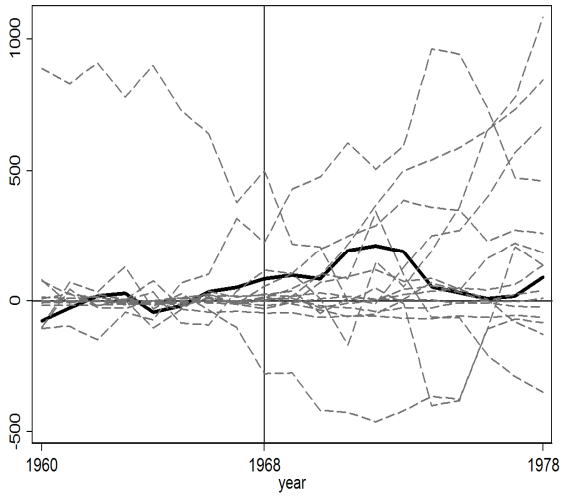


Placebo test

Figure 5 – Trends in real GDP per capita, Panama 1968

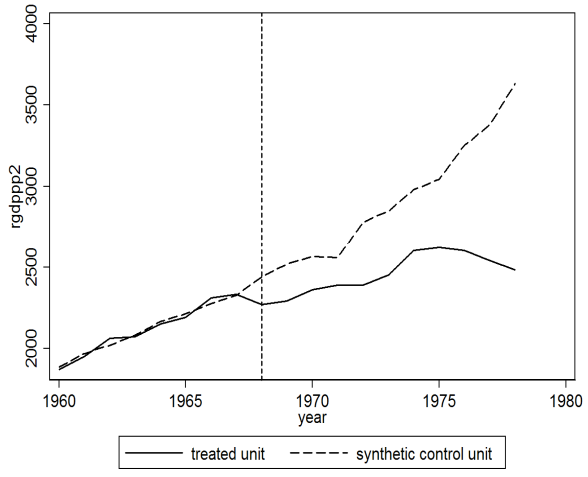


Panama vs. Synthetic Control

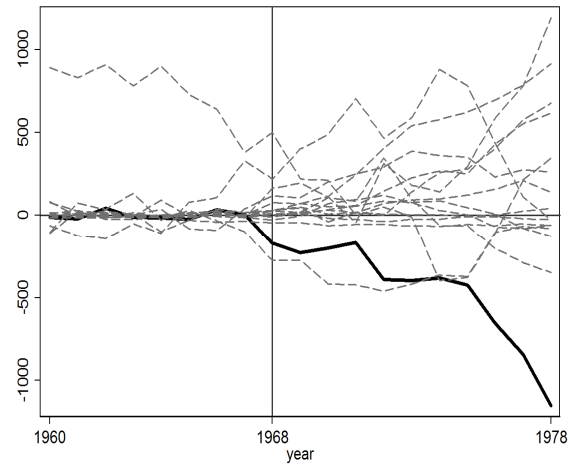


Placebo test

Figure 6 – Trends in real GDP per capita, Peru 1968

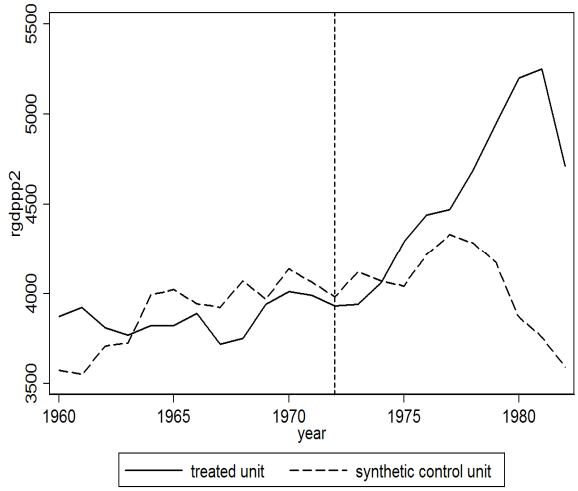


Peru vs. Synthetic Control

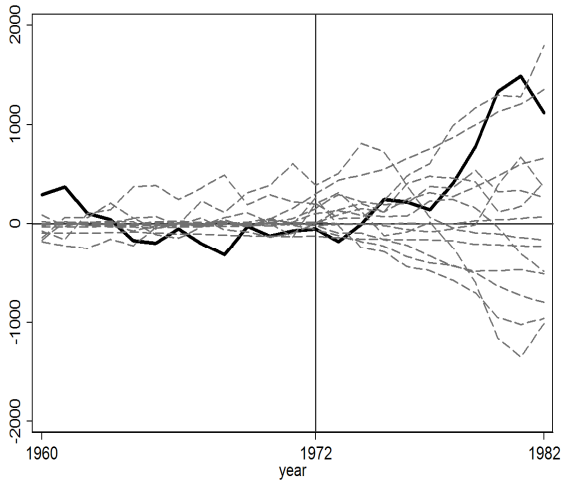


Placebo test

Figure 7 – Trends in real GDP per capita, Uruguay 1972

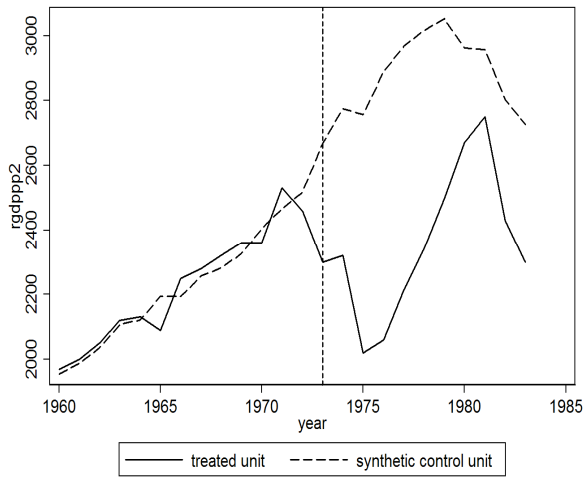


Uruguay vs. Synthetic Control

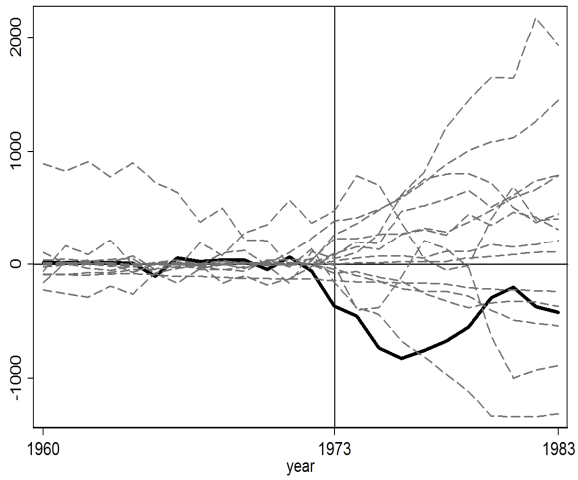


Placebo test

Figure 8 – Trends in real GDP per capita, Chile 1973

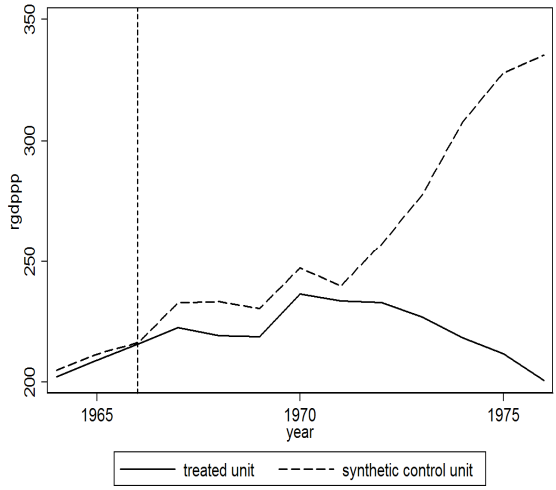


Chile vs. Synthetic Control

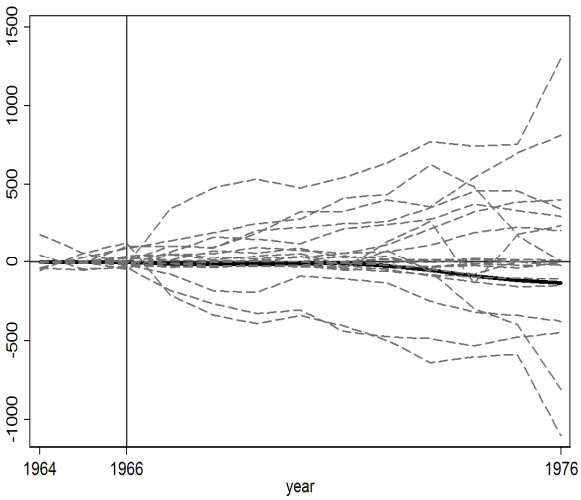


Placebo test

Figure 9 – Trends in real GDP per capita, Uganda 1966

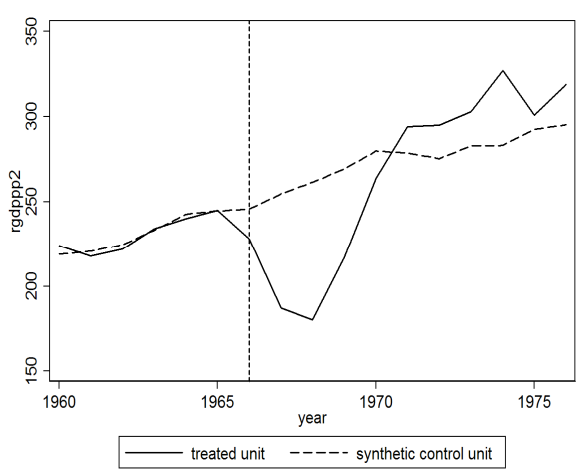


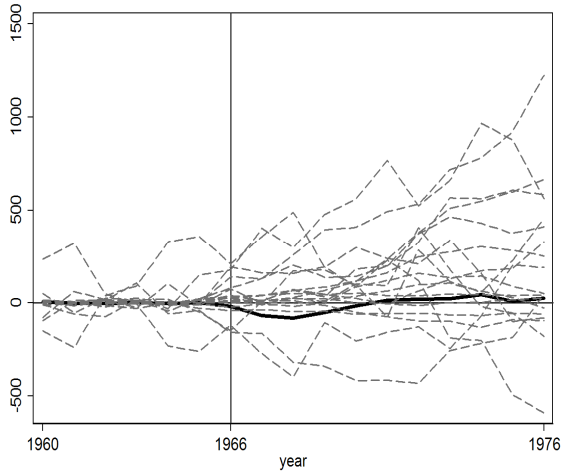
Uganda vs. Synthetic Control



Placebo test

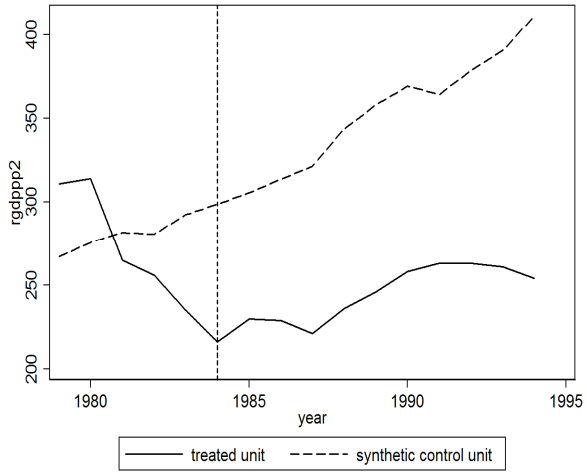
Figure 10 – Trends in real GDP per capita, Nigeria 1966

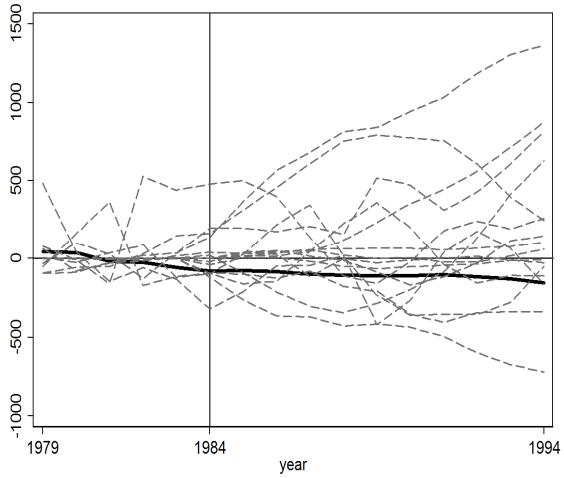




Nigeria 1966 vs. Synthetic Control Placebo test

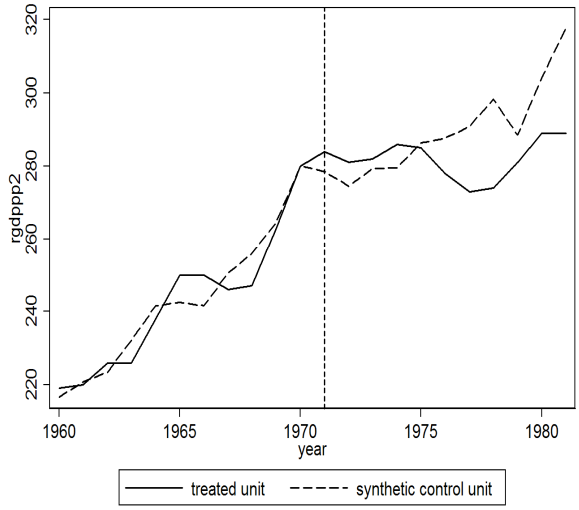
Figure 11 – Trends in real GDP per capita, Nigeria 1984

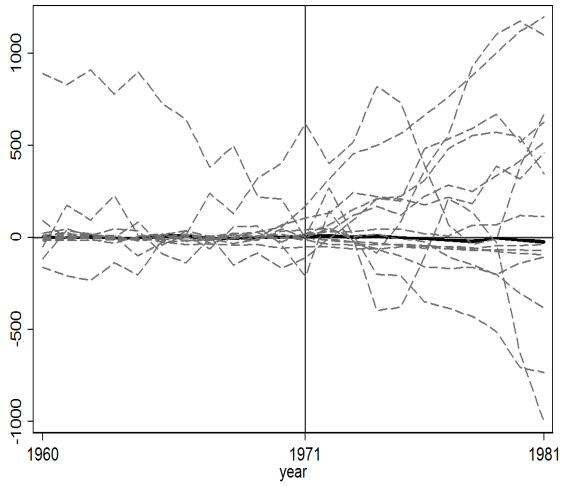




Nigeria 1984 vs. Synthetic Control Placebo test

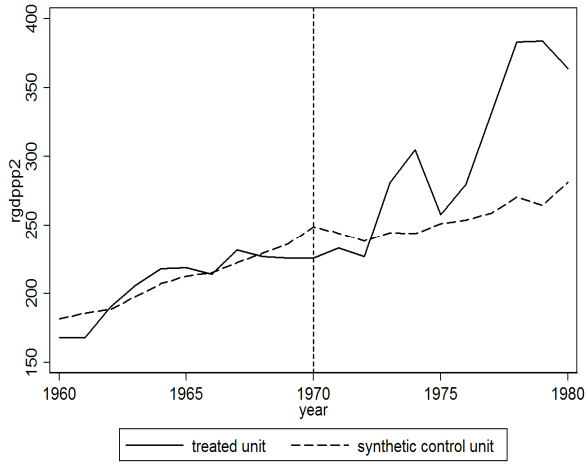
Figure 12 – Trends in real GDP per capita, Sierra Leone 1971



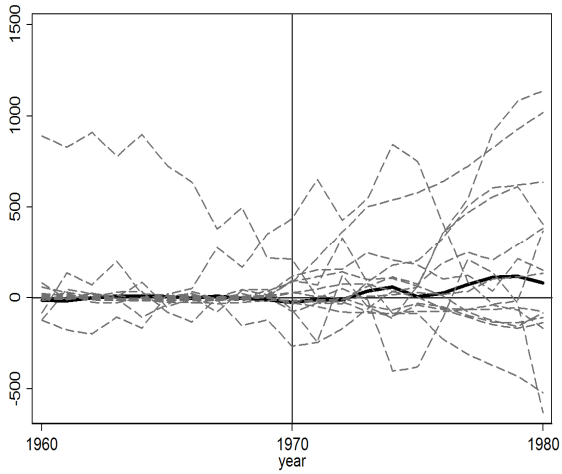


Sierra Leone vs. Synthetic Control Placebo test

Figure 13 – Trends in real GDP per capita, Lesotho 1970

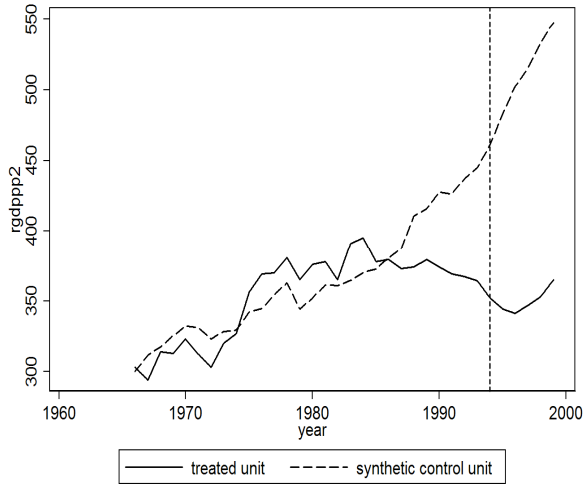


Lesotho vs. Synthetic Control

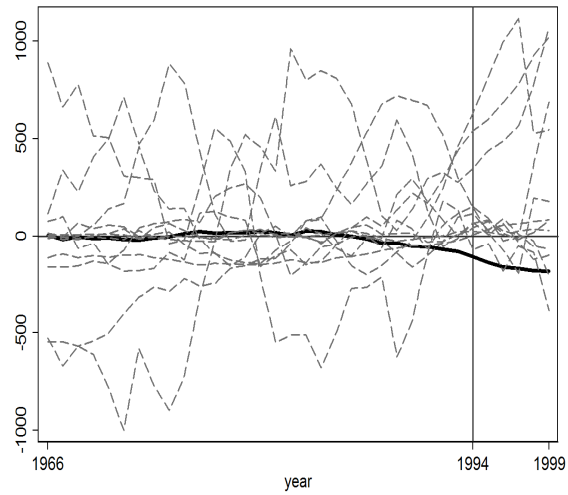


Placebo test

Figure 14 – Trends in real GDP per capita, Gambia 1994



Gambia vs. Synthetic Control



Placebo test

Appendix I - Transitions to autocracy[10]

Chile 1973: On September 11, 1973 the government of President Salvador Allende was overthrown by the military in a coup d'état. General Augusto Pinochet assumed power after deposing President Salvador Allende, establishing an anti-communist military dictatorship that ruled until 1990.

Gambia 1994: Until the military coup in 1994, Gambia was ruled by President Dawda Kairaba Jawara, who had been elected five times in a raw. In 1994, the Armed Forces Provisional Ruling Council (AFPRC) deposed the Jawara government and banned opposition political activity. Lieutenant Yahya A.J.J. Jammeh, chairman of the AFPRC, became head of state.

Greece 1967: Rule by the military started in the morning of 21 April 1967 with a coup d'état led by a group of colonels of the Greek military, and ended in July 1974. "The Regime of the Colonels" refers to a series of right-wing military governments that ruled Greece from 1967 to 1974.

Lesotho 1970: In January 1970 the ruling Basotho National Party (BNP) lost the first post-independence general elections 36. Prime Minister Leabua Jonathan refused to cede power to the Basotho Congress Party (BCP), declared himself Tona Kholo, and imprisoned the BCP leadership.

Nigeria 1966: The first military coup was in January and led by a collection of young leftists under Major Emmanuel Ifeajuna and Chukwuma Kaduna Nzeogwu. This coup was counter-acted by another successful plot, which allowed Lt Colonel Yakubu Gowon to become head of state.

Nigeria 1984: A new constitution was approved in 1977, and in 1979 Shehu Shagari won democratic elections. A military coup in 1983 established the High Military Council as new ruling body.

Panama 1968: A military coup overthrew the elected government headed by Arnulfo Arias Madrid. Generale Omar Torrijos exerted power over the junta until his death in 1981 in an alleged plane accident.

Pakistan 1977: In 1977 Zulfikar Ali Bhutto enjoyed an unprecedented victory and became Prime Minister, and then a staged military coup headed by Zia-ul-Haq apprehended him on issues of vote-rigging and banned all political activities leading the nation into a martial law.

Peru 1968: The elected government of President Fernando Belaunde Terry was deposed by coup; he was succeeded by General Juan Velasco Alvardo.

Philippines 1972: Barred from seeking a third term, President Ferdinand Marcos declared martial law on September 21, 1972, under the guise of increased political instability and resurgent Communist and Muslim insurgencies, and ruled the country by decree.

[10] The source is Wikipedia, accessed on November 4th, 2009.

Sierra Leone 1971: After a closely contested general elections in March 1967, Siaka Stevens, candidate of the All People's Congress (APC) was appointed new prime minister. He was subject to a number of coups. On April 19, 1971, parliament declared Sierra Leone a Republic. Siaka Stevens' title was changed from Prime Minister to President. Guinean troops requested by Stevens to support his government were in the country from 1971 to 1973. The opposition boycotted the 1973 general election.

South Korea 1972: On December 6, 1971, Park declared a state of national emergency. On July 4 of the following year, he announced plans for reunification with North Korea. Park declared martial law in October 1972, dissolving the National Assembly. The Fourth Republic began with the adoption of the Yusin Constitution in November 1972. This new constitution gave Park effective control over the parliament.

Uganda 1966: In 1966, Milton Obote overthrew the king Edward Muteesa II, the President and Commander in Chief of the armed forces. A Parliament dominated by the Uganda People's Congress changed the constitution, and Obote became president. The elections were suspended, ushering in an era of coups and counter-coups, which would last until the mid-1980s. Obote was deposed twice from office, both times by military coup.

Uruguay 1972: President Jorge Pacheco declared a state of emergency in 1968, followed by a further suspension of civil liberties in 1972 by his successor, President Juan María Bordaberry. After defeating the Tupamaros, the military seized power in 1973. In 1980, the army forces proposed a change in the constitution that would be passed with a referendum. The "No" to the reform won the vote with 57.2% of the votes. In 1984, massive protests against military rule broke out, and the country returned to civilian rule with national elections held later in the same year.

Appendix II - Treated countries and donor pools for the synthetic control experiments

Greece 1967

Potential controls	Australia, Austria, Belgium, Denmark, Finland, France, Iceland, Ireland, Israel, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Sweden, Switzerland, United Kingdom, United States, Fiji, India, Malaysia, Papua New Guinea, Sri Lanka, Botswana, Colombia, Costa Rica, Jamaica, Mauritius, South Africa, Zimbabwe Trinidad & Tobago, Venezuela.

Philippines 1972

Potential controls	Fiji, India, Malaysia, Papua New Guinea, Sri Lanka, Botswana, Colombia, Costa Rica, Jamaica, Mauritius, South Africa, Zimbabwe Trinidad & Tobago, Venezuela.

South Korea 1972

Potential controls	Fiji, India, Malaysia, Papua New Guinea, Sri Lanka, Botswana, Colombia, Costa Rica, Jamaica,Trinidad & Tobago, Mauritius, South Africa, Zimbabwe, Venezuela.

Pakistan 1977

Potential controls	Fiji, India, Malaysia, Papua New Guinea, Sri Lanka, Botswana, Colombia, Costa Rica, Jamaica, Mauritius, Trinidad & Tobago, Venezuela, South Africa, Zimbabwe.

Panama 1968

Potential controls	Colombia, Costa Rica, Jamaica, Trinidad & Tobago, Venezuela, Mauritius, South Africa, India, Malaysia, Papua New Guinea, Sri Lanka, Botswana, Pakistan, Zimbabwe, Fiji.

Peru 1968

Potential controls	Colombia, Costa Rica, Jamaica, Trinidad & Tobago, Venezuela, India, Malaysia, Fiji, Papua New Guinea, Sri Lanka, Botswana, Mauritius, South Africa, Pakistan, Zimbabwe.

Uruguay 1972

Potential controls	Colombia, Costa Rica, Jamaica, Trinidad & Tobago, Venezuela, India, Malaysia, Zimbabwe, Papua New Guinea, Sri Lanka, Botswana, Mauritius, South Africa, Fiji.

Chile 1973

Potential controls	Colombia, Costa Rica, Jamaica, Trinidad & Tobago, Venezuela, India, Malaysia, Papua New Guinea, Sri Lanka, Botswana, Mauritius,South Africa, Zimbabwe,Fiji.

Uganda 1966

Potential controls	Botswana, Mauritius, South Africa, Zimbabwe, India, Malaysia, Sri Lanka, Colombia, Costa Rica, Jamaica, Venezuela, Uruguay, Philippines, South Korea, Chile, Pakistan, Fiji,Trinidad & Tobago,Papua New Guinea,Gambia.

Nigeria 1966

Potential controls	Botswana, Mauritius, South Africa, Zimbabwe, India, Malaysia, Papua New Guinea, Sri Lanka, Colombia, Costa Rica, Jamaica, Trinidad & Tobago, Venezuela, Uruguay, Philippines, South Korea, Chile, Pakistan, Fiji.

Sierra Leone 1971

Potential controls	Botswana, Mauritius, South Africa, Zimbabwe, India, Malaysia, Papua New Guinea, Sri Lanka, Colombia, Costa Rica, Jamaica, Trinidad & Tobago, Venezuela, Pakistan, Fiji.

Lesotho 1970

Potential controls	Botswana, Mauritius, South Africa, Zimbabwe, India, Malaysia, Papua New Guinea, Sri Lanka, Colombia, Costa Rica, Jamaica, Trinidad & Tobago, Venezuela, Pakistan, Fiji.

Nigeria 1984

Potential controls	Botswana, Mauritius, South Africa, India, Malaysia, Papua New Guinea, Sri Lanka, Colombia, Costa Rica, Jamaica, Trinidad & Tobago, Venezuela, Thailand, Dominican Republic, Ecuador, Peru.

Gambia 1994

Potential controls	Botswana, Mauritius, South Africa, India, Malaysia, Papua New Guinea, Sri Lanka, Colombia, Costa Rica, Jamaica, Trinidad & Tobago,Venezuela.

Appendix III – Control countries with weight higher than 0.02 in the synthetic control

ASIA

PHILIPPINES 1972

Synth. Control	
Costa Rica	0.115
India	0.608
Malaysia	0.124
Mauritius	0.116
Venezuela	0.032

SOUTH KOREA 1972

Synth. Control	
Botswana	0.602
South Africa	0.398

PAKISTAN 1977

Synth. Control	
Botswana	0.057
India	0.920
Papua N. Guinea	0.023

OECD

GREECE 1967

Synth. Control	
Papua N. Guinea	0.415
Japan	0.288
Jamaica	0.214
South Africa	0.052
New Zealand	0.031

LAAM

PANAMA 1968

Synth. Control	
Papua N. Guinea	0.495
South Africa	0.320
Trinidad & Tob.	0.185

PERU 1968

Synth. Control	
Costa Rica	0.392
Malaysia	0.059
South Africa	0.034
Trinidad & Tob.	0.515

URUGUAY 1972

Synth. Control	
Mauritius	0.056
Venezuela	0.944

CHILE 1973

Synth. Control	
Costa Rica	0.391
Fiji	0.052
Mauritius	0.312
South Africa	0.150
Venezuela	0.095

AFRICA

UGANDA 1966

Synth. Control	
Botswana	0.127
Gambia	0.784
India	0.030

NIGERIA 1966

Synth. Control	
Costa Rica	0.021
India	0.608
Pakistan	0.371

SIERRA LEONE 1971

Synth. Control	
India	0.556
Pakistan	0.378
Zimbabwe	0.061

LESOTHO 1970

Synth. Control	
India	0.398
Pakistan	0.601

NIGERIA 1984

Synth. Control	
India	0.983

GAMBIA 1994

Synth. Control	
India	0.969