THE GROWTH EFFECT OF DEMOCRACY: IS IT HETEROGENOUS AND HOW CAN IT BE ESTIMATED?

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Abstract

We estimate the effect of political regime transitions on growth with semi-parametric methods, combining difference in differences with matching, that have not been used in macroeconomic settings. Our semi-parametric estimates suggest that previous parametric estimates may have seriously underestimated the growth effects of democracy. In particular, we find an average negative effect on growth of leaving democracy on the order of −2 percentage points implying effects on income per capita as large as 45 percent over the 1960-2000 panel. Heterogenous characteristics of reforming and non-reforming countries appear to play an important role in driving these results.

JEL Code: E2, O4, P.

Keywords: growth, democracy, development, political institutions.

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1 Introduction

Political regimes can change suddenly, because of coups, popular revolts, or the death of leaders. Such changes provide an opportunity to assess whether economic policies or performance are influenced by political institutions. A number of recent papers have exploited this opportunity. Using more or less the same difference-in-difference methodology, they have all estimated the average effects of democratic transitions on economic growth, or some other measures of economic performance, using a post-war panel data set (see e.g., Giavazzi and Tabellini (2005), Papaioannou and Siourounis (2004), Persson (2005), Persson and Tabellini (2006), Rodrik and Wacziarg (2004)). While the difference-in-difference strategy yields interesting results, which are considerably more credible than those from a standard cross-sectional regression, it still rests on strong identifying assumptions.[1]

The goal of this paper is to reassess the relation between democracy and growth, while relaxing some of these strong identifying assumptions. To reach this goal, we re-estimate the average effect of political transitions on economic growth by means of semi-parametric methods. Broadly speaking , we combine aspects of difference-in-difference methods with aspects of propensity-score methods, by giving more weight to the comparisons of reforming and non-reforming countries that have similar probabilities of experiencing democratic reform. Specifically, we first estimate the probability of regime change conditional on a number of observable variables. We then use this estimated probability, the propensity score, to evaluate the difference in growth performance between the countries with and without a regime change. Under the standard assumptions in the propensity-score literature (the selection-on-observables and common-support assumptions), this empirical strategy yields consistent estimates of the average effect of political regime changes, in cases when a standard difference in difference strategy would not. A theoretical paper by Abadie (2005) further discusses this approach to estimation.[2]

[1] It is hard to find good instruments for regime changes. Jones and Olken (2005, 2006) imaginatively use unexpected deaths of leaders, and the contrast between successful and unsuccessful assassination attempts on leaders, respectively. The latter approach allows them to estimate the likelihood of a democratic transition, but it is likely to generate too weak an instrument (too few successful assassinations and too imprecise timing) for democracy.

[2] Athey and Imbens (2006) generalize the difference in difference methodology along related but different lines. Their non-parametric approach also allows for hetereogeneous treatment effects, but relies on estimating the entire distribution of counterfactual out-

Heckman et al. (1997) evaluate similar non-experimental estimators, using data from a large-scale US social experiment with job training. Blundell et al. (2004) apply a combination of matching and difference in differences when estimating the effect of UK job training programs. To our knowledge, the present paper is the first to apply matching *cum* difference-in-difference methods in a macroeconomic context.[3] The macro setting raises specific issues that are not present is standard microeconomic applications, such as a relatively small sample and different treatment (reform) dates for different observations.

Our empirical findings suggest that empirically relevant heterogeneities are indeed present across countries, meaning that the flexibility allowed by semi-paramatric methods is important. We show that transitions from autocracy to democracy are associated with an average growth acceleration of about 1 percentage point, producing a gain in per capita income of about 13 percent by the end of the sample period. This 1 percent growth effect is imprecisely estimated, but larger than most of the estimates in the literature using straight difference-in-difference methods (see the references mentioned above). The effect of transitions in the opposite direction is even larger: a relapse from democracy to autocracy slows down growth by almost 2 percentage points on average, which implies an income fall of about 45 percent at the end of the sample. These effects are much larger than those commonly found in the literature.

The paper proceeds to discuss the main econometric issues (Section 2), describe the data (Section 3), and provide a benchmark with the straight difference-in-difference approach (Section 4). We then discuss some preliminaries in the matching procedure (Section 5), present the paper's main results on how democracy affects growth (Section 6), and conclude (Section 7).

2 Econometric Methods

This section introduces a number of econometric issues and methods to deal with them. Most of it can probably be skimmed through by econometrically proficient readers who are familiar with the methods used in the treatment

comes for the treatment group in the absence of treatment.

[3]Persson and Tabellini (2003) apply propensity-score methods to evaluate the effect of alternative constitutional features, but they compare a cross section of countries and do not exploit temporal variation in the data

literature.

Our goal is to estimate the average causal effect of becoming a democracy on economic growth. To simplify the argument, we assume throughout the section that we have access to a sample consisting of data from only two types of countries: "treated" countries that experience a single transition from autocracy into democracy, and "control" countries that remain autocracies throughout the sample period.[4] For each country in this sample, we observe economic growth in country i and year t, $y_{i,t}$, a dummy variable equal to one under democracy, $D_{i,t}$, and a vector of covariates, $\mathbf{x}_{i,t}$.

2.1 Difference in difference estimates

Several recent papers (see the Introduction) have estimated the average effect of democracy on growth from a panel regression like:

$$y_{i,t} = \phi D_{i,t} + \boldsymbol{\rho}\mathbf{x}_{i,t} + \alpha_i + \theta_t + \varepsilon_{i,t} \ , \tag{1}$$

where α_i and θ_t are country and year fixed effects. This specification seeks to estimate the parameter ϕ by difference in differences, by comparing average economic growth after the democratic transition minus growth before the transition in the treated countries to the change in economic growth in the control countries over the same period.

This estimation method allows for any correlation between the democracy dummy $D_{i,t}$ and time-invariant country features – e.g., that fast-growing countries are more likely to become democratic than slow-growing ones – since the growth effects of these country features are all captured by the country fixed effect, α_i. Nevertheless, identification rests on an important assumption: the selection of countries into democracy have to be uncorrelated with the *country-specific and time-varying* shock to growth, $\varepsilon_{i,t}$.

This in turn corresponds to two restrictive assumptions. First, absent any regime change, average growth in treated countries should (counterfactually) have been the same as in control countries (conditional on $\mathbf{x}_{i,t}$). This would fail, e.g., if democratic transitions are enacted by far-sighted leaders, who have a lasting impact on growth irrespective of the regime change, or if

[4] For the time being, we thus neglect transitions from democracy to autocracy, and exclude from the sample countries that always remained democracies. We also neglect multiple transitions, and only consider countries that had a single transition from autocracy into democracy. These complications are all dealt with in later sections.

political transitions coincide with other events – such as the economic transitions towards free markets in former socialist countries – that may have a lasting impact on economic growth.

To make this assumption more credible, the existing literature typically attempts to increase the similarity between treated and controls by including in the vector $\mathbf{x}_{i,t}$ several covariates, such as initial per capita income, indicators for war years or socialist transitions, indicator variables for continental location (Africa, Asia and Latin America) interacted with year dummy variables, and so on.

The second restrictive assumption is that heterogeneity in the effects of democracy should not be systematically related to the occurrence of democracy itself. Circumstances of regime changes differ widely across time and space, as do the types of political institutions adopted or abandoned. Thus, the effects of a crude democracy indicator are likely to differ across observations. If we neglect this heterogeneity and estimate the average effect of democracy as in (1), the unexplained component of growth, $\varepsilon_{i,t}$, also includes the term $(\phi_{i,t} - \phi)D_{i,t}$, where $\phi_{i,t}$ is the country-specific effect of democracy in country i and year t. Identification of ϕ now requires heterogeneity in the effect of reforms to be uncorrelated with their occurrence. This assumption fails, e.g., if countries self-select into democracy based on the growth effect of regime changes (e.g., $D_{i,t} = 1$ more likely when $\phi_{i,t} > \phi$).

To cope with this assumption, the dummy variable for democratic transitions is sometimes interacted with other observable features of democratic transitions (such as the nature of democratic institutions that are acquired, or the sequence of economic and political reforms). But this strategy quickly runs into the curse of dimensionality problem. The possible interactions and covariates are simply too many, relative to the limited number of democratic transitions.

2.2 Matching estimates based on the propensity score

To circumvent the curse of dimensionality, the recent microeconometric literature has often come to rely on semi-parametric methods based on the propensity score. Typically these applications concern a cross section of individuals. But a few recent papers have combined difference-in-difference estimates with matching based on the propensity score, exploiting repeated observations for the same individuals. Abadie (2005) discusses an estimation strategy that uses the propensity score to carry out estimates in the spirit of difference in differences, while Heckman et al. (1997) and Blundell et al. (2004) provide theory as well as microeconometric applications.

The general idea is very intuitive. Performance – growth, in our case – before and after the treatment date is observed for the treated group and the control group. Conventional difference in differences compare the average change in performance for all the treated with the average change in performance for all the controls, on the two sides of a common treatment date. The matching approach instead compares each treated individual with a set of "similar" controls, and a difference-in-difference estimate is computed with reference only to the matched controls. This way, controls similar to the treated are given large weight, and controls very dissimilar to any treated observation may even be deemed entirely non-comparable, i.e., they are left unmatched and given zero weight. Similarity is measured by the one-dimensional metric of the propensity score, i.e., the probability of receiving treatment conditional on a set of covariates. Basically, the effect of treatment is estimated by comparing groups of individuals with similar distributions of those covariates that enter the estimation of the propensity score.

The microeconometric papers mentioned above discuss the econometric theory behind this methodology, and we refer the reader to these papers for more details. In this section, we confine ourselves to stating and explaining the main identifying assumptions. For this purpose, we need some notation adapted from Persson and Tabellini (2003) and Abadie (2005).

2.2.1 The parameter of interest

As above, let D be an indicator for democracy ($D = 1$) or autocracy ($D = 0$). Time is indexed by k, which corresponds to (an average over) years before ($k = 0$) and after ($k = 1$) the year of democratic transition. Let $Y_{i,k}^{D}$ denote *potential* growth of country i in period k and democratic state D (we use the symbol Y, in distinction from y in the previous subsection, since growth in period k is now an average of yearly growth rates during k). The individual treatment effect of democracy in country i and period k is then $Y_{i,k}^{1} - Y_{i,k}^{0}$, the effect on growth in period 1 if this country switched from autocracy to democracy.

Consider a subset of the treated countries (i.e., countries with $D_{i,1} = 1$) with similar (time-invariant) characteristics, $\mathbf{X}_i$. The expected effect of democracy on growth in each of these countries is:

$$\alpha(\mathbf{X}_i) = E(Y_{i,1}^{1} - Y_{i,1}^{0} |\ \mathbf{X}_{i,}\ D_{i,1} = 1)\ ,\ .$$

where the expectations operator E refers to unobserved determinants of growth in democracy. Our parameter of interest is the *average effect of treatment on the treated*, namely :

$$\alpha = \mathrm{E}\alpha(\mathbf{X}_i) = \mathrm{E}\left\{E(Y_{i,1}^1 - Y_{i,1}^0 | \ \mathbf{X}_{i,} \ D_{i,1} = 1)\right\} \ , \tag{2}$$

where the outer expectations operator E is taken over $\mathbf{X}$ in the part of the sample treated with democracy. This parameter measures the effect of democracy on growth in the countries that actually experienced the transition, relative to what would have happened had they remained autocracies. In other words, the relevant counterfactual is remaining under autocracy. Without additional assumptions, the parameter α does not say anything about what growth would have been if the countries that remained autocracies had instead become democracy (this would be a statement about the effect of treatment on the non-treated).

The fundamental problem of causal inference is that potential growth in the counterfactual regime is not observed. We only observe *actual* growth in one of the two possible political regimes. In particular, in period 1 we only observe $Y_{i,1}^1$ in the countries that actually became democratic (the treated) and $Y_{i,1}^0$ in the countries that actually had no transition (the controls). But the term $Y_{i,1}^0$ (counterfactual growth in a democracy if it had remained an autocracy) on the right-hand side of (2) is not observed.

2.2.2 Selection on observables

To come up with an observable counterpart to $Y_{i,1}^0$, we can make the key identifying assumption (cf. Abadie, 2005):

$$E(Y_{i,1}^0 - Y_{i,0}^0 | \ \mathbf{X}_i, \ D_{i,1} = 1) = E(Y_{i,1}^0 - Y_{i,0}^0 | \ \mathbf{X}_i, \ D_{i,1} = 0) \ . \tag{3}$$

The right- hand side of (3) is the (observed) average change in growth between periods 1 and 0 in countries that remained autocracies throughout (the control group). The left-hand side is the (unobserved) average change in growth that the countries which actually became democracies (the treated group) would have experienced had they remained autocratic. Thus, the critical assumption is that, *conditional on* $\mathbf{X}$, without their democratic transition the treated countries would have followed a growth path parallel to that of the control countries. This is the analog of the *selection on observables* assumption in a simple cross-sectional context[5].

Decomposing the expectations operators on both sides of (3), all the terms are observable except for one: $E(Y_{i,1}^{0}|\ \mathbf{X}_{i,},\ D_{i,1}=1)$. Thus, assumption (3) enables us to obtain an observable counterpart of this unobserved counterfactual, that can be used to estimate the parameter of interest in (2). Intuitively, by conditioning on a large enough set of covariates $\mathbf{X}$, we can replace unobserved period 1 growth under autocracy in the treated countries (the term $E(Y_{i,1}^{0}|\ \mathbf{X}_{i,},\ D_{i,1}=1)$) with observed growth under autocracy over the same period (the term $E(Y_{i,1}^{0}|\ \mathbf{X}_{i},\ D_{i,1}=0)$) in those control countries that have similar covariates $\mathbf{X}_i$.

Importantly, this argument does not impose any functional-form assumption on how democracy impacts on growth. Because the relevant conditional expectations in (3) can all be computed non-parametrically, we can estimate our the parameter of interest, α, non-parametrically just by comparing (weighted) mean outcomes. This is the central difference between matching and linear regression. Matching allows us to draw inferences from *local* comparisons only: as we compare countries with similar values of $\mathbf{X}$, we do not rely on counterfactuals very different from the observed factuals. However, this desirable property requires that any *unobserved* heterogeneity in the response of growth to democracy be non-systematic across the two groups of countries.

2.2.3 Propensity score and common support

In practice, however, the dimension of $\mathbf{X}$ is too large for direct matching to be viable. This is where the propensity score methodology is helpful. An important result due to Rosenbaum and Rubin (1983) implies that comparing countries with the same *probability of democratic transition* (*treatment*) given the controls $\mathbf{X}$, is equivalent to comparing countries with similar values of $\mathbf{X}$.

Specifically, let

$$p_i = p(\mathbf{X}_i) = \text{ Prob } [D_{i,1} = 1 | \mathbf{X}_i]$$

be the conditional probability that country i has a democratic transition during our sample period, given the vector of controls, $\mathbf{X}_i$. This conditional

[5] As Abadie (2005) notes, equation (3) coincides with the so called *selection on observables* assumption used in cross sectional studies if in addition we also have $E(Y_{i0}^{0} \mid \mathbf{X}_i, D_{i1}=1) = E(Y_{i1}^{0} \mid \mathbf{X}_i, D_{i1}=0)$.

probability is also called the *propensity score*. Assume that the propensity score is bounded away from 0 and 1 for all countries, an assumption known as the so-called *common-support* condition:

$$0 < p(\mathbf{X}_i) < 1, \text{ all } \mathbf{X}_i \; . \tag{4}$$

Rosenbaum and Rubin (1983) show that, in a cross sectional setting, conditioning on the vector $\mathbf{X}$ is equivalent to conditioning on the scalar p. If (4) is satisfied in our two-period context, (3) implies:

$$E(Y_{i,1}^0 - Y_{i,0}^0 | \; p(\mathbf{X}_i), \; D_{i,1} = 1) = E(Y_{i,1}^0 - Y_{i,0}^0 | \; p(\mathbf{X}_i), \; D_{i,1} = 0) \; , \tag{5}$$

For countries with similar propensity scores, realized transitions to democracy are random and uncorrelated with growth. We can thus replace the unobserved counterfactual on the left-hand side of (5) with the observed factual on the right-hand side of (5).

2.2.4 What do we gain?

The main advantage of this semi-parametric (semi-parametric because we have to estimate the propensity score) approach over the parametric difference-in-difference approach is that it relaxes linearity. We can thus allow for any heterogeneity in the effect of democracy, as long as it is related to the observable covariates $\mathbf{X}$. Suppose e.g., that richer countries are more likely to become democracies, and that democracy also works better in richer countries. Then the linear estimates corresponding to equation (1) would be biased unless we also included an interaction term between income and the democracy dummy. This bias is removed if income is included among the covariates $\mathbf{X}$ used to estimate the propensity score. Of course, unobserved heterogeneity remains a problem. Any omitted variable *uncorrelated with* $\mathbf{X}$ that influences both the adoption and the effects of democracy would violate selection on observables. But since – as a practical matter – economic, social and cultural characteristics tend to cluster a great deal across countries, unobserved differences among countries may well correlate with observed differences.

A second advantage of this approach is that it allows a simple diagnostic to check that the distribution of observed covariates is balanced between the countries in the treated group and the control group. If the distribution of a specific covariate is very unbalanced in the two samples of countries, it is important to check if the results are robust to including this variable when estimating the propensity score. Intuitively, if the treated and controls have similar covariates the linearity assumption entailed in conventional difference in difference is just a convenient local approximation. If they do not, the dissimilarity may bias the results.

Of course, there is no free lunch. The main cost of a semi-parametric approach is that the estimates are less efficient than parametric estimates (under the null of the assumed functional form). Given the small samples in macroeconomics relative to standard micro applications, the loss in precision is non-neglible.

2.2.5 Implementation in practice

Our actual sample – unlike the stylized example and typical microeconomic applications like training programs – has different transition dates T_i, for different observations $i = 1, ..., I$. Of course, our estimation procedure will have to cope with this additional complication. Also different from the example in this section, the actual sample includes transitions from democracy to autocracy. This presents no conceptual problems (see further below), however, so we can continue to think about treatment as a transition into democracy. In practice, we implement the estimation in five steps.

(i) We begin by defining a group of treated and a group of control countries and estimate the probability of treatment. This is done in a cross section by means of a logit regression, where the dependent variable equals one for all countries making a transition at some time within the sample and zero for those that don't, and where all the covariates are time invariant. The estimated probability of a transition to democracy is our measure of the propensity score.

(ii) Next, for each country treated with democracy, we compute average growth before and after the date of transition, T_i. The difference between these two averaged growth rates is denoted by g_i. Thus, we measure

$$g_i = \frac{1}{N_i^a} \sum_{t>T_i} y_{i,t} - \frac{1}{N_i^b} \sum_{t<T_i} y_{i,t} \ , \tag{6}$$

where $y_{i,t}$ is the yearly growth in period t and N_i^b and N_i^a are the number of years *b*efore and *a*fter and the transition date in country i. (The next section describes how we deal with multiple transitions, so for now think about the procedure as applying to a set up where each country has at most one transition in the sample period.)

(iii) Subsequently, we match each treated country with some of the controls. For each of these controls, we compute the difference in average growth over the periods before and after the transition date in the treated country they are matched with: the expression is thus identical to (6), except that $y_{i,t}$ is replaced with $y_{j,t}$. We denote the resulting variable as g_i^j where the j superscript refers to a certain country j among the controls and i refers to the treated country. In doing this, we make sure that the years over which g_i and g_i^j are computed exactly coincide.

(iv) For each treated country, we then compute the weighted average of the non-parametric difference-in-difference estimator $\hat{\alpha}_i$:

$$\hat{\alpha}_i = g_i - \sum_j w_{i,j} g_i^j \ , \tag{7}$$

where $w_{i,j} \geq 0, \sum_j w_{i,j} = 1$, are weights based on the propensity score. These weights differ depending on the detailed properties of the matching estimators and some controls may receive zero weight if they are very different from the treated country with which they are matched. The parameter $\hat{\alpha}_i$ is our estimate of the effect of democratic transition on growth in country i. Intuitively, it measures how growth in country i changed after the transition, relative to a weighted average of the (similar) controls it is matched with.

(v) Finally, we compute the average estimated effect of transitions to democracy in the group of treated countries, $\hat{\alpha}$, as a simple average of the individual $\hat{\alpha}_i$ estimates, namely:

$$\hat{\alpha} = \frac{1}{I} \sum_i \hat{\alpha}_i \tag{8}$$

where I denotes the number of treated countries in our sample. This is our estimator of the average effect of democracy on growth (the average effect of treatment on the treated).

Clearly, this procedure may use each control country several times, as the same controls may be matched with several treated countries and possibly at different dates. This matters for the computation of the standard error of our estimators, since it may introduce correlation between g_i^j and g_k^j – i.e., between growth in control country j when it is used as a control for treated countries i and k. Of course, the correlation will be positive and higher the closer are the transition dates of i and k, while the correlation between g_i^j and g_k^j might even be negative if the transition dates are far part. The appendix provides analytic expressions for the standard error of α under two alternative assumptions: (a) the variables g_i^j and g_k^j are independent, (b) the variables g_i^j and g_k^j are perfectly correlated. While (b) certainly yields an upper bound, the true standard errors might be lower than under (a) if negative correlation between g_i^j and g_k^j is prevalent. When computing the standard errors, we assume that all treated countries have the same variance, as do all control countries. We also neglect that the weights are estimated in a first step (i.e., we treat the propensity score as known). Both assumptions are standard in the applied literature (see, e.g., Lechner, 2000).[6]

3 Data and Sample Definitions

Our panel data set includes annual data on economic growth and political regimes for as many countries as possible over the years 1960-2000. Economic growth is measured as the yearly growth rate of per-capita income, and the source is the Penn World Tables. We classify a country as democratic if the *polity2* variable in the Polity IV data set is strictly positive. The threshold of 0 for *polity2* corresponds to a generous definition of democracy, but has the advantage that many large changes in the *polity2* are clustered around 0. This is important, since we want to identify the causal effect of regime transitions on growth exploiting the time variation in the data. A definition of democracy based on a higher threshold for *polity2* would classify as democratic transitions also very gradual changes in the underlying indicators of *polity2*, that are unlikely to be associated with significant changes in political regimes.[7]

We also include some other covariates, that will be introduced and defined in context. The resulting panel is unbalanced, partly because of data availability and partly because countries do not enter the data set until their year of independence.

[6] An alternative – to be pursued in future work – would be to compute the standard errors by bootstrapping. Doing so would take into account that the weights $w_{i,j}$ are uncertain, since they are based on (logit) estimates of the propensity score.

[7] An alternative would be to use a classification of political regimes, based on a finer subdivision of the 21-step scale for the *polity 2* score. This would turn the analysis into the domain off multiple tretments (see e.g., Lechner, 2001)

From this panel data set we construct two partly overlapping samples, which are used to study transitions to democracy and autocracy, respectively. When studying transitions into democracy, we include as control countries those that remain autocracies throughout the sample period, while the treated countries are those that experience at least one transition from autocracy to democracy. We call this sample the "democratic transitions" sample. When studying transitions into autocracy, the control countries remain democracies throughout, while the treated countries have at least one transition from democracy to autocracy. This is called the "autocratic transitions" sample.

In selecting these two samples, we had to deal with a number of complications. A few countries experience transitions close to the beginning or the end of the period for which growth data are available. Since we expect it to take some time for transitions to influence growth, we discard the transitions that take place in the last three years of the available sample. We also discard reforms in the first three years of the panel to avoid a poor estimate of growth before the transition. Specifically, we set to missing the observations of growth after (or before) a transition, if the transition is not followed (or preceded) by at least three years of growth data. The country is then considered a control, as if the transition did not occur.

In a few countries, especially in Africa and Latin America, we observe transitions that only last for a few years. We discard those lasting (strictly) less than four years, to avoid hinging the estimation on very short growth episodes. As in the beginning or end of sample transitions, we set growth to missing during the years of these short transitions, and classify the country as if the transition did not occur.

In another few countries, we observe more than one long spell of democracy or autocracy. Chile, for instance, starts out as a democracy in 1960, it becomes an autocracy (the Pinochet regime) in 1973, and it returns to democracy in 1989. This means that Chile is a treated country both when treatment is defined as transition to democracy, and when treatment is defined as transition to autocracy. Therefore, Chile is included as treated in the democratic transitions sample for the years from 1973 (when it first becomes an autocracy) until the end of the sample. It is also included as treated in autocratic transitions sample from 1960 until 1988 (the last year of autocracy). We apply similar sample selection rules to other countries that experience more than one spell in the same regime lasting more than three years.

When transitions are defined in this way, most countries have no more than a single transition in one or both directions. Guatemala, Uganda and Nigeria, however, have two transitions in the same direction. We deal with the transitions in these three countries in two different ways: they are either excluded because the propensity score is outside of the common support range (see below), or included with the transitions in the same direction assumed independent (as if each transition applied to a different treated country).

4 Difference-in-Difference Estimates

To provide a benchmark, Table 1 presents results from traditional difference-in-difference estimation with yearly data. These results correspond to estimates of equation (1) in various samples. Besides country and year fixed effects, the covariates $\mathbf{x}_{i,t}$, include per-capita income lagged once, year fixed effects interacted with indicators for Latin America and for Africa, indicators for war years and lagged war years, and an indicator for formerly socialist countries in Central and Eastern Europe and the Asian provinces of the former Soviet Union after 1989. This specification is similar to those in the existing literature (e.g., Giavazzi and Tabellini 2005, or Persson and Tabellini 2006).

Column 1 imposes the assumption that the effect on growth of a transition into democracy is the same as the negative of the effect on growth of as transition into autocracy. The effect of democracy is thus estimated in the full sample. As in the earlier papers above, we find that the effect of democratic transitions is positive, inducing a growth acceleration of about 0.5 percentage points. Although not statistically significant, the point estimate is not a trivial effect from an economic point of view. The long-run effect is dampened by the relatively high estimated convergence rate, however. With a convergence rate of 5.5 percent per year, a growth acceleration of about 0.5 percentage points implies a long-run positive effect of democracy on the level of per capita income of almost 10 percent.[8]

The remainder of Table 1 does not impose the symmetry constraint, but estimates the effect of democracy separately from transitions to democracy (columns 2 and 3) and transitions to autocracy (columns 4 and 5), allowing these two effects to differ. Note that when estimating the effect of autocratic transitions in columns 4 and 5, we still display the effect of being a democracy, computed as the negative growth effect of transitions away from democracy. In column 2, we let the sample include only the countries that became democracies plus the countries that remained autocracies throughout.[9] In column 3, we add to the sample those countries that remained democratic throughout. Analogously, the sample behind column 4 includes the countries that became autocracies and the more restricted set of countries that remained democratic throughout, while the sample behind column 5 includes both permanent democracies and autocracies. All the estimates in Table 1 convey a similar message: democracy induces a positive, but small and generally insignificant, growth acceleration. The positive effect of transitions to democracy appears larger in absolute value (and in one case statistically significant) than the negative effect of transitions to autocracy.

5 Matching preliminaries

We now turn to the main contribution of the paper, namely the matching approach to estimating the growth effects of democracy. Before getting to the actual estimates, however, we need to go through a number of preliminary

[8] The coefficient ϕ on the democracy indicator D measures the impact effect on growth $y_t - y_{t-1}$. Because lagged (log) income y_{t-1} enter on the RHS of the estimated equation with coefficient β, the long-run effect on income can be computed as

$$\frac{dy}{dD} = -\frac{\phi}{\beta}\ .$$

With estimates $\widehat{\phi} = 0.5$ and $\widehat{\beta} = -0.055$, we obtain a long-run income gain of 0.09. i.e., about 9 percent. Since the convergence rate β is likely overestimated in yearly data (due to cyclical fluctuations in income), this is almost surely an underestimate of the long-run income gain.

[9] This is, of course, the "democratic transition" sample defned in Section 3. In this section, we avoid the term control countries, however, since in a difference in difference estimation with different treatment dates, all countries that do not have a reform in period t effectively serve as controls for those countrie that do have a reform in t.

steps including some diagnostics. This section is devoted to these preliminaries.

5.1 Estimating the propensity score

As explained in Section 2, the first step to implement a matching *cum* difference-in-difference estimator is to estimate the propensity score, the probability of treatment, in a cross section of countries (i.e., ignoring the time dimension). We do this separately for the events of becoming a democracy and becoming an autocracy, because we want to allow the effect of the covariates on the probability of transition to be different for the two events. In the democratic transitions sample, the dependent variable is thus zero for the countries that remained autocracies, and one for the countries that experienced at least one transition towards democracy. In the autocratic transitions sample, the dependent variable is zero for the countries that remained democracies throughout, and one for the countries that experienced at least one transition towards autocracy. Thus, the samples are partly overlapping (because some countries like Chile appear in both samples).

We estimate the propensity score with a logit regression. The selection of the covariates to enter this regression is a crucial decision, that trades off two opposite concerns. On the one hand, the selection on observables assumption would suggest to include many covariates to ensure that the propensity score is indeed a balancing function. On the other hand, we don't want to predict treatment too well, so as not to violate the common-support assumption. Here is an instance, where the macroeconomic setting bites. Most microeconomic application concentrate on the first concern, because the sample is large enough that even rare events – like an actual transition for an observation with a low propensity score – would still occur in large enough numbers to allow meaningful comparisons (and small standard errors). But in our context we also have to worry about not excluding too many countries whose state is predicted too well. Thus, we include a limited number of variables that are likely to influence both the occurrence of regime transitions and its economic effects, and we check the robustness of the results to two alternative specifications. The set of covariates is the same in the democratic and autocratic transitions samples.

To capture differences in economic development, we include real per capita income at the beginning of the sample. As explained above, different countries enter our samples at different dates, depending on political history or data availability. To increase comparability, we measure each country's per capita income in the first year it enters a given sample relative to US per capita income in the same year. We call the resulting variable *income relative to the US*.

The countries in these samples have very different political histories. Some of them have a long history with entry into democracy in the distant past, or a prolonged autocratic spell. Others became independent some time during the sample period or few years before. To mitigate this important source of heterogeneity, we condition on what Persson and Tabellini (2006b) call *domestic democratic capital*, which measures the incidence of democracy in each country since 1800 (or since the year of independence, if later). This variable is assumed to accumulate in years of democracy, but to depreciate under autocracy. The depreciation rate is estimated by Persson and Tabellini (2006b) to fit the hazard rates in a time series regression where the dependent variable is exit from democracy and from autocracy. This variable is re-scaled to lie between 0 and 1, where a 1 corresponds to the steady state value of a country never exiting from democracy. In this paper, we measure *domestic democratic capital* in the first year when a country enters the sample.

Transitions to democracy or autocracy often occur in waves that include several neighboring countries. To capture this phenomenon, we include a variable measuring the geography of democracy around 1993 (the first year in our sample, when we have data for all formerly socialist countries in Central and Eastern Europe). This variable, called *foreign democratic capital*, is a slight variation on a similar measure used in Persson and Tabellini (2006b). For each country, it is defined as the incidence of democracy in 1993 among all other countries within a 1750 km radius (the radius refers to the distance between the capitals). By the definition of a share, this variable too lies between 0 and 1, where a 1 captures the case where all countries in the neighborhood are democratic.

Since the sample period varies in length across countries, and since the probability of a regime transition is higher the longer is the duration of the relevant time period, we also control for the length of the period during which we have available data for each country, a variable called *length of sample*. This variable is introduced to eliminate the possibility that sample length covaries systematically with growth performance.

Wars are often destabilizing for political regimes and, of course, they also hurt economic activity. Thus, we include as a covariate the fraction of war years (including both inter-state and civil wars) over the total period length for which growth data are available, a variable called *war years*.

Finally, regime transitions are more likely for countries that start out with a value of our democracy index, *polity2*, closer to the threshold of zero. At the same time, a high initial value of *polity2* might have an independent effect on the economic consequences of regime changes (for instance because a regime change might correspond to a more gradual transition). For this reason, we also consider including the value of *polity2* in the first year a country enters the sample. As we shall see, however, the inclusion of this variable increases a great deal the predictive power of the logit regressions in the sample of autocratic transitions. This, in turn, leads to a much smaller set of treated countries that safely meet the common support condition. Hence, we discuss results with and without the *initial value of polity* 2.

The results of the logit regressions are displayed in Table 2. Columns 1 and 2 refer to the democratic transitions sample, with and without the inclusion of the *initial value of polity2. Domestic democratic capital* considerably raises the probability of a transition towards democracy, as expected. *Foreign democratic capital* has a similar positive effect, but this effect is not statistically significant. The frequency of wars discourages democratic transitions, an effect that is statistically significant. *Income relative to the US* has no effect. Finally, the inclusion of the *initial value of polity2* makes no difference. Overall the pseudo R^2 (the improvement in the likelihood associated with the inclusion of the covariates in addition to a constant) is 0.17, suggesting that these covariates leave a lot of residual variation unexplained.

Columns 3 and 4 refer to the autocratic transitions sample, with and without the *initial value polity2.* Here *income relative to the US* has strong predictive power, with richer countries less likely to relapse into autocracy, as expected.[10] *Foreign democratic capital* also helps to predict transitions to autocracy, although here the sign is opposite of what one would expect. As anticipated, the inclusion of the *initial value of polity2* makes a big difference: the variable is highly significant and with the expected sign, and when it is included the Pseudo R^2 jumps from 0.43 to 0.61. Overall, these covariates help to predict transitions from democracy to autocracy much better than transitions in the opposite direction. As already discussed, this is a

[10]The results on income are consistent with the results in the annual hazard rates estimated by Persson and Tabellini (2006b), who find that income does not explain transitions out of autocracy, but does slow down transitions out of democracy.

mixed blessing, since it makes the selection on observables assumption more credible, but at the same time strains the credibility of the common support assumption.

Figure 1 depicts the density of the estimated propensity score from columns 1 and 3 respectively of Table 2 (i.e., the specification that does not include the *initial value of polity2*), for both treated and control countries. Observations outside of the common support we impose are dropped and not displayed in Figure 1 (see the discussion in the next subsection). As one would expect from the estimation results, the distribution of the propensity scores for the treated and the controls are more similar in the sample of democratic transitions, where treatment is predicted less well, than in the sample of autocratic transitions. Both samples display considerable overlap between treated and control countries, however. Overall, the figure suggests that matching should work well, at least if the local comparisons are made within relatively broad regions of the propensity score (a coarse balancing function), so as to guarantee overlap .

5.2 Countries inside the common support

The first column of Tables 3a and 3b report the full list of countries in each of the two samples. These are sorted in ascending order of the estimated propensity scores, which are displayed in the third column. To facilitate reading the table, the name of treated countries are indicated by boldface font, whereas the name of control countries are not. The same information is given in column 2: the variable *treated* in the second column equals 0 for the countries in the control group and 1 for the countries in the treated group. The last two columns of each table report the change in *polity2* in the year of the regime transition, and the year of that (those) transition(s).

It is important to verify that the common-support assumption is not obviously violated, and possibly to drop observations for which the estimated propensity score is too close to its bounds of 0 and 1. Consider the democratic transitions sample in Table 3a. At the lower bound (the top of the table), we are comfortably away from 0. The first observation, Yemen, is a control with an estimated propensity score of 0.17. The third observation, Iran is the first treated country (according to our generous definition, Iran became a democracy in 1997), with an estimated propensity score of 0.28. At the upper end (the bottom of the table), instead, several treated countries are predicted very well to switch into democracy. There is no firm rule for how to deal with this situation. We choose to drop all treated observations with a propensity score above 0.9. This has the advantage of not drawing inferences from Guatemala (the unique country to experience two long spells of democracy), and gives a fair margin away from unity. Adopting a higher upper bound and including more countries would not affect the estimates. But the results are sensitive to a more conservative, lower upper bound, essentially because Haiti (with an estimated propensity score of 0.887) is a large outlying observation which makes some difference. We comment more on this below.

Next, consider the autocratic transitions sample in Table 3b, where we face the opposite problem. The controls (that remained democracies throughout) are predicted very well around 0 and there is little overlap with treated countries, while at the upper end the lack of overlap is less serious. Here, we choose to drop all observations with an estimated propensity score below 0.075 and above 0.93. At the upper end the choice is made so that the Nigeria and Uganda (the only two treated countries with multiple spells of autocracy) are dropped from the sample. But adopting a higher or lower threshold would not change the results. At the lower end, one outlying observation matters quite a bit for the results: Belarus, which starts out as a (weak) democracy, and drops into dictatorship after a few years. Since the time period where we have data for Belarus is very short, and since the next treated country is Greece with a much higher propensity score (0.19 vs. 0.07 for Belarus), we choose to be conservative and exclude Belarus from the common support. At the low end, we thus start the sample with Austria, a control with a propensity score slightly above 0.075. Adopting an even more conservative, higher bound for the common support does not affect the final results.

5.3 The balancing property

To what extent is the propensity score a balancing function, i.e., how well does our matching on the propensity score balance the distribution of relevant covariates across treated and control countries? The answer to this question is important, because this is where the value added of this methodology lies. Tables 4a and 4b provide the answer for our two samples of democratic and autocratic transitions.

Each double row in the table refers to a specific covariate. We consider all covariates included in the logit regressions of Table 2 (including the *ini-*

tial value of polity2), plus three dummy variables for continental location (in Latin America, or Asia, or Africa). The upper single row (labeled unmatched) for each variable displays the simple average of that variable in the treated groups and control group, respectively, plus the $t$statistic and the p-value for the null hypothesis that these averages are the same in the treated and control group. This first set of statistics is calculated over the full set of countries listed in Tables 3a and 3b, respectively, before imposing the common support assumption. Clearly, the null of equal means is rejected for many variables in either or both of the tables. Thus, treated and control countries differ systematically with regard to economic development (*relative income*), political history (*domestic democratic capital*), and -political geography (*foreign democratic capital*). Initial democracy as measured by *polity2* is also very different in the treated and control groups in the "autocratic transitions" sample. Finally, the treated and control groups also seem to be drawn from different continents (in particular with regard to Latin America and Africa).

The lower single row for each variable (labeled matched) present a similar set of statistics calculated in a different way. First, we impose the common support assumption for both the treated and the control countries, as discussed above. We then calculated the means for the treated countries. Clearly, this changes their means for the treated group. Second, we display the *matched* means for the control countries, namely a weighted average where each control country receives a weight based on the propensity score, corresponding to the matching procedure described in the next subsection (see also equations (7) and (8) above).

Clearly, matching equalizes the means of all covariates used in the logit regression. Interestingly, it also reduces the difference in means of some of the other covariates, Africa and Latin America in Table 4b, Latin America in Table 4a. This gives some credence to our earlier expectation that observed (included among the covariates) and unobserved (not included among the covariates) country characteristics may be correlated. In the autocratic transitions sample, however, the variable *initial value of polity2* retains a very different distribution in the treated and control groups, which suggests the importance of also conditioning on the *initial value of polity2* in this sample.

Overall, and with the caveat just mentioned on *initial value of polity2*, matching seems indispensable to achieve a balanced distribution of covariates between treated and control countries – the so-called balancing property. Without matching based on the propensity scores, the two samples are quite different. This means that the assumption of linearity can not be treated as an innocuous linear approximation. Various interaction effects may thus bias the inference drawn from traditional difference-in-difference regressions.

6 Matching Estimates

With the preliminaries of the previous section in hand, we are ready to estimate the effect of political transitions on the treated countries. This section is devoted to the estimation results.

6.1 Democratic transitions

We start with transitions towards democracy. To get a benchmark, we start by reporting linear regression estimates obtained with a two-step procedure suggested in a recent paper by Bertrand et al. (2004). The purpose of that procedure is not to address heterogeneity giving bias in the coefficients, but serial correlation yielding (upward) bias in the standard errors. The procedure treats the data in a similar way, however, in its averaging the outcome of interest before and after the treatment. Because they impose the parametric assumptions of a linear regression, these estimates provide a useful perspective on the final results from the non-parametric matching procedure..

Specifically, the Bertrand et al estimates are obtained as follows. In a first step, growth is regressed against country and year fixed effects in a sample with yearly data from *all* countries, treated and controls. Then, the estimated residuals of the treated countries only are retained and averaged before and after each country's transition date. This yields a panel of two periods with only treated countries. Finally, the averaged residuals in this panel are regressed against a constant and a dummy variable, which is equal to 1 in the second period (after the transition) and 0 in the first (before the transition). The estimated coefficient and standard errors thus correspond to the difference in difference estimator of the average effect of transition in the treated countries. As explained by Bertrand et al. (2004), this procedure removes the serial correlation in the yearly residuals – a potential problem in the yearly regressions of Table 1.

Column 1 of Table 5 implements this procedure for all countries in the democratic transitions sample, where the control countries are those that remained autocracies throughout and the treated are those that made a transition to democracies. The estimated coefficient, although not statistically significant, implies an average growth acceleration of 0.6 percentage points after transitions to democracy. Despite the different procedure and specification, this estimate is remarkably similar to that reported in Table 1, column 2 (contrary to Table 1, the first step does not include initial income, indicators for wars, socialist transitions, and continents interacted with years). In the democratic transitions sample, the average date of reform is in the late 1980s, with about twelve years of post-transition growth. This implies an average effect on per capita income at the end of the sample of about 7-8 percent. This estimate is consistent with the long-run effects on income implied by Table 1. In column 2 of Table 5, we drop control and treated countries outside of the common support defined in the Section 5 (cf. Table 3a). The point estimate increases a bit, but remains statistically insignificant.

Columns 3 to 6 of Table 5 present the matching estimates. In columns 3 and 4, the underlying specification of the propensity score does not condition on the *initial value of polity2*, while in columns 5 and 6 it does. All estimators are based on Kernel matching, i.e., the weight on specific controls are declining in their distance in propensity score to the treated country they are matched with. Columns 3 and 5 weigh control countries with the Epanechnikov measure, which gives zero weight to all controls whose estimated propensity score differs by more than 0.25 to that of the treated country. Columns 4 and 6 use a Gaussian kernel, which gives all control countries weights that approach zero for the more distant controls – see Leuven and Sianesi (2003) for more detailed information. Note that each country in the control group is used several times in the matching, particularly when we use the Gaussian kernel. As explained in Section 2, we compute two sets of standard errors: the lowermost parenthesis below each point estimate corresponds to an upper bound.

All our estimates form a consistent picture despite the different covariates and matching procedures. The point estimate of the effect of democratic transitions ranges between 0.83 and 1.08, an economically relevant effect that is considerably higher than the linear estimates. Recalling that the effect refers to average growth during an average post-transition period, which lasts about twelve years, a growth acceleration of 1 percent implies that per capita income is 13 percent higher at the end of the sample. Despite the magnitude of the point estimate, the standard errors are large enough that the effect remains statistically insignificant. This is not unexpected, given that matching estimators are not likely to be very precise in such a small sample. To say it differently: we are trading off unbiasedness against efficiency.

An important property of the matching estimation procedure is that it directs our attention to heterogenous effects of democratic transitions in different countries, pointing to influential observations and to other relevant features of the data. Figures 2 and 3 explore these issues.

Figure 2 displays histograms of the distribution of the variables g_i (in the left panel) and g_i^j (in the right panel), defined in Section 2. Intuitively, Figure 2 shows the change in average growth after democratic transitions in the groups of treated countries (the left panel), and control countries (the right panel) at comparable dates. The treated countries have observations symmetrically distributed around 0, except for a large positive outlier, namely Haiti where democracy was associated with a growth acceleration of about 19 percent. There are some outliers in the group of control countries as well, but these are less influential because the control group is much larger than the treated group. More importantly, the distribution of the change in growth in the control countries is clearly tilted to the left and has its mass below zero. Thus, the positive point estimate in Table 5 is not due to an improvement in growth in the countries that became democratic (with the exception of Haiti), but rather due to a deterioration of growth in the control countries that remained autocracies. In other words, under a causal interpretation, by becoming democracies the treated countries avoided the growth slump that hit the permanent autocracies in the control group.

Figure 3 displays the contribution to the average growth effect of treated countries by their propensity scores. Specifically, the vertical axis plots the estimator $\hat{\alpha}_i$ defined above, namely the estimated effect in treated country i, while the horizontal axis reports the estimated propensity score in country i expressed as the average change in growth rate (in percentage points per year) .[11] This figure reveals that there is no systematic relationship between the individual treatment effect and the estimated probability of treatment. This is reassuring, because selection into treatment is not systematically correlated with performance, in accordance with the identifying assumption. The figure also shows that the growth effects of democratic transitions are very heterogeneous across countries, with impact effects ranging from -5 to $+5$ percentage points. Together with the unbalanced distribution of

[11] The growth estimates refer to column 3 in Table 5.

covariates across treated and control countries (cf. Table 4a and 4b), this suggests that the linear estimates are quite fragile. As already noted, Haiti remains an influential outlier even after matching (dropping Haiti from the sample would reduce the estimated growth effect almost by a half). Finally, note that much of the heterogeneity in the effect of treatment derives from less developed countries with rather fragile democratic institutions, such as Uganda, Guyana, Congo, Romania. This is not unexpected, because growth is likely to be more volatile in such countries, and autocracies are likely to be associated with highly corrupt and bad dictatorships. It is reassuring, however, that we find no systematic relationship between these heterogeneous effects and some of the observed covariates, such as per-capita income or the intensity of the treatment (as measured by the change in the polity2 score associated with democratic transitions). This can be guessed already by a cursory look at the symmetric distribution of countries in Figure 3, and is confirmed by a more careful analysis where we regress the individual treatment effect against the observed covariates.

6.2 Autocratic transitions

Finally, we turn to the autocratic transitions sample with countries treated with a transition to autocracy and a control group of democracies which are politically stable during the sample period. The estimates are displayed in Table 6, with columns exactly analogous to those of Table 5. Here, the estimates captures the effect of transition to autocracy, and thus we expect them to have a negative sign.

Consider the two-step linear estimates in columns 1 and 2. In this case, it makes a big difference whether or not we impose the common support. When all observations are included (column 1), the effect of a relapse into autocracy is essentially zero (a point estimate of 0.17, with a large standard error). Dropping all observations outside of the common support (column 2), however, turns the estimate negative and almost statistically significant: according to the point estimate, a transition to autocracy cuts average yearly growth by 0.84 percent. As shown in Table 3b, the observations outside the common support are made up by a large group of very solid democracies, unlikely treated Belarus, and a few African countries at the opposite extreme of the propensity score. Belarus in particular is a very influential observation, because its growth rate accelerates dramatically towards the end of the sample when it also turns to autocracy. These countries are indeed very different from most of the other countries in the sample. Thus the estimates in column 2, which restrict attention to countries on the common support, may be the most reliable.

The remaining columns of Table 6 report the matching estimates, which all deliver a similar and robust message. A transition into autocracy cuts average yearly growth by a statistically significant and large amount, which ranges from -1.6 to -2.4 percentage points. The average year of autocratic transition is about 1975. This makes the level effects at the end of the sample very large: a reduction in the post-transition growth rate of, say, -1.8 percentage points sustained for 25 years corresponds to a 45 percent loss of per capita income.

The estimated treatment effect is not particularly sensitive to including the *initial value of polity2* among the covariates in the underlying propensity score. This is reassuring, in light of the unbalanced distribution of this variable across the treated and control groups (cf. Table 4b). However, when the *initial value of polity2* enters the estimated propensity score, the number of countries on the common support shrinks further, because treatment is predicted quite well.[12] As a result, the estimates become more sensitive to the weighting procedure (cf. columns 5 and 6).

Figures 4 and 5 illustrate the contribution of individual countries to these estimates, in the same way as Figures 2 and 3. Figure 4 contrasts with the democratic transition case in Figure 2, in that the treated group has a distribution with mass below a zero change in growth, while the distribution for the group of control counties seems centered at, or slightly above, zero. Thus, the estimated negative growth effect of autocracy is mainly due to a growth deccelaration in countries that relapsed into autocracy. Once we impose the common support, there appears to be no influential outliers in the group of treated countries.

Figure 5 plots the estimates of the individual treatment effects against the estimated propensity scores.[13] As in Figure 3, there is considerable heterogeneity. But we detect no systematic relation to the estimated propensity score (nor against other covariates). Moreover, no single treated country

[12] When we condition also on the *initial value of polity*2 we change the range corresponding to the common support to those treated and control countries with an estimated common support in the range (0.11-0.98). In Table 5, the definition of the common support remains instead the same irrespective of whether we condition or not the *initial value of polity*2.

[13] The estimates refer to column 3 in Table 6.

appears particularly influential. Instead, most countries have a large and negative effect of treatment, suggesting that the large negative estimate of the average effect in Table 6 is quite robust.

7 Concluding Remarks

We have estimated the effect of political regime transitions on growth in a new way, paying close attention to heterogenous effects. Our non-parametric matching estimates suggest that previous parametric estimates may have seriously underestimated the growth effects of democracy. In particular, we find an average negative effect on per capita income of leaving democracy as large as 45 percent over the sample. We also find clear indications 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. While our matching estimates do allow for heterogeneity in a very general way, it is important to recall that they rest on the specific assumption of selection on observables.

As far as we know, our paper is the first to combine matching and difference in differences in a macroeconomic context. This seems a promising avenue for further work on the effects of reform. In the context of political reforms and growth, it would be natural to investigate the effects of different types of democracy (or different types of autocracy, as do Besley and Kudamatsu, 2007). But similar estimation techniques could be used to empirically analyze also other types of reform, where we might suspect the effects to be quite heterogenous. Reforms introducing central bank independence and/or inflation targeting may be a particular case in point.

8 Appendix

Here we compute the standard error of the estimator $\hat{\alpha}$ given in (8) – see also Lechner (2000) for a similar derivation. Combining (8) and (7), we have:

$$\hat{\alpha} = \frac{1}{I}\sum_i g_i - \frac{1}{I}\sum_i \sum_j w_{i,j} g_i^j \tag{9}$$

Suppose that all treated countries have the same variance $\sigma_T^2 = Var(g_i|$ i is treated), and that all control countries also have the same variance,

$\sigma_C^2 = Var(g_i^j |\ i$ is treated, j is a control). Assume further that $w_{i,j}$ are known scalars, and that all g_i observations are mutually uncorrelated. If g_i^j and g_k^j are also mutually uncorrelated for $i \neq k$ and all j, then

$$Var(\hat{\alpha}) = \frac{\sigma_T^2}{I} + \sigma_C^2 \frac{\sum_i \sum_j (w_{i,j})^2}{I^2} \tag{10}$$

This is our lower bound for the estimated variance of α.

Suppose instead that g_i^j and g_k^j are perfectly correlated for $i \neq k$, but that g_i^j and g_i^l are mutually uncorrelated for $j \neq l$ (i.e. observations corresponding to different control countries are mutually uncorrelated, while observations drawn from the same control are perfectly correlated when that control is used several times for different treated countries). Then:

$$Var(\hat{\alpha}) = \frac{\sigma_T^2}{I} + \sigma_C^2 \frac{\sum_j (\sum_i w_{i,j})^2}{I^2} \tag{11}$$

This is our upper bound for the estimated variance of $\hat{\alpha}$.

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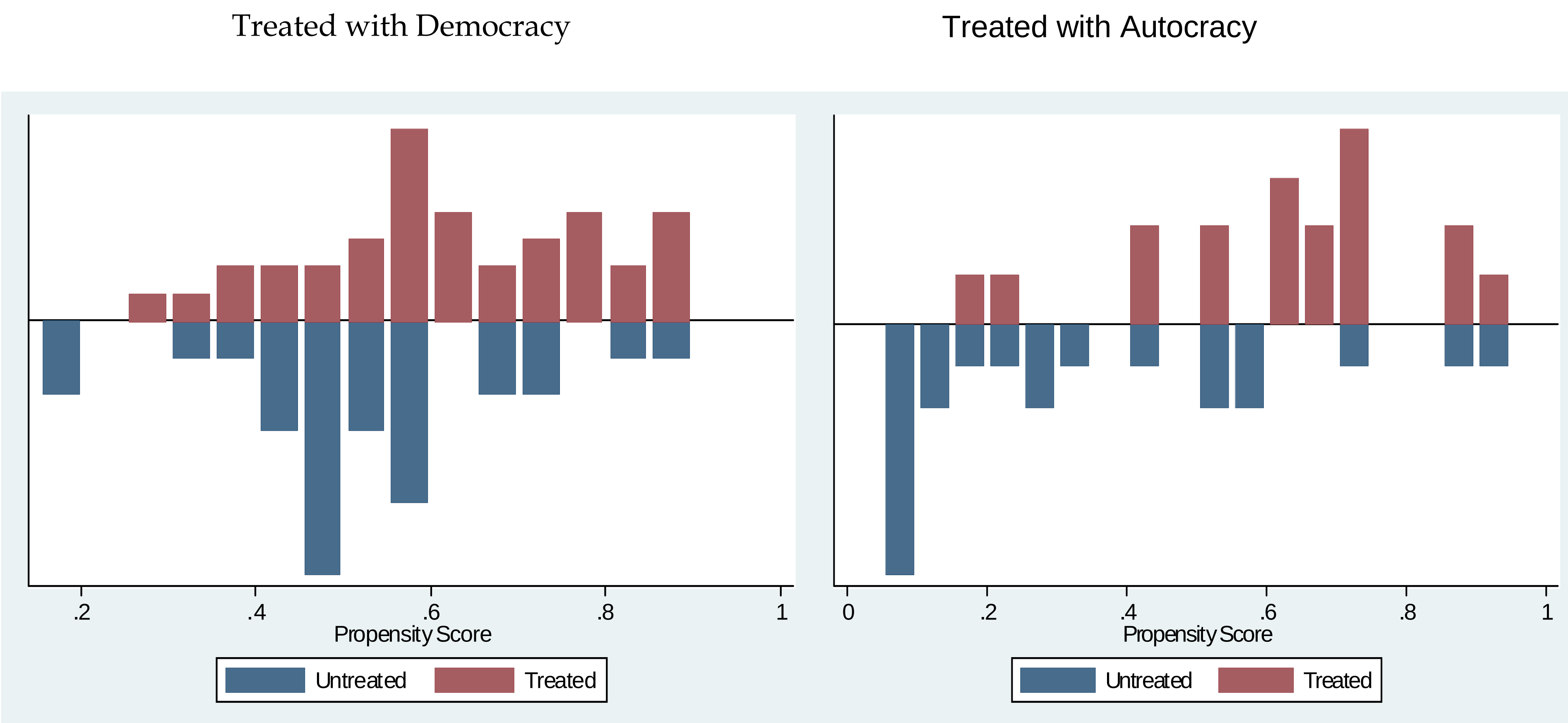
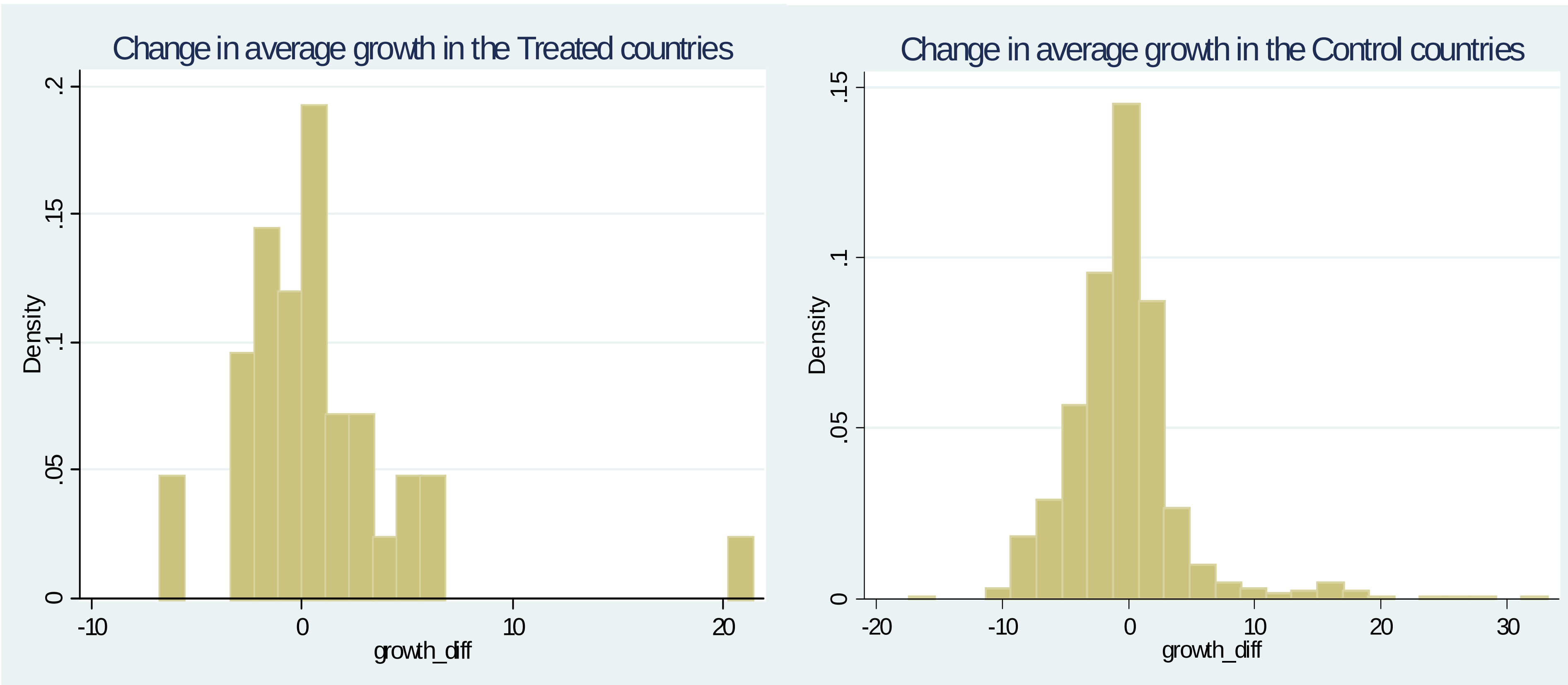
Figure 1 Estimated propensity scores



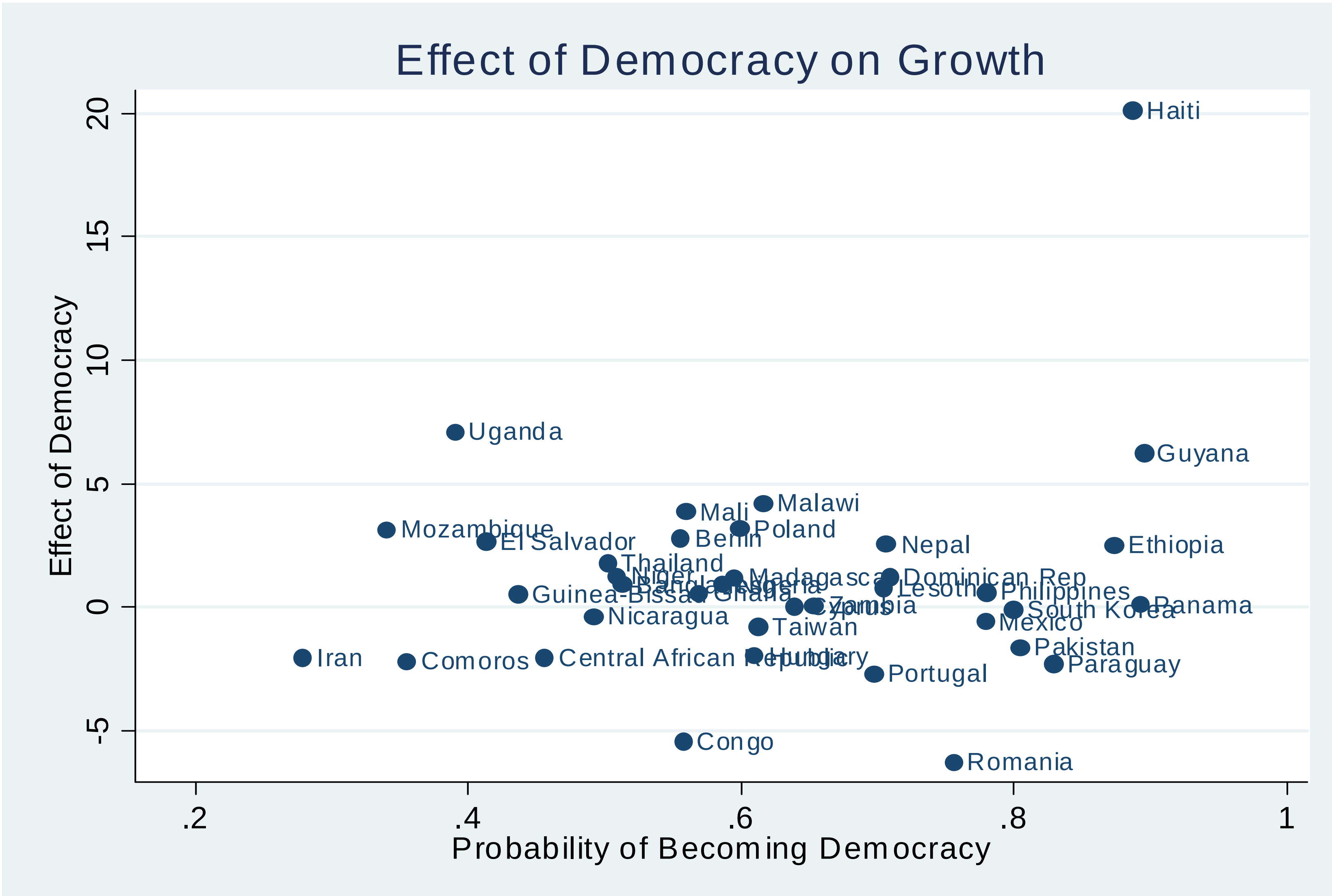
Note: The estimates correspond to columns 1 and 3 respectively of Table 2

Figure 2   Change in growth after transition to democracy



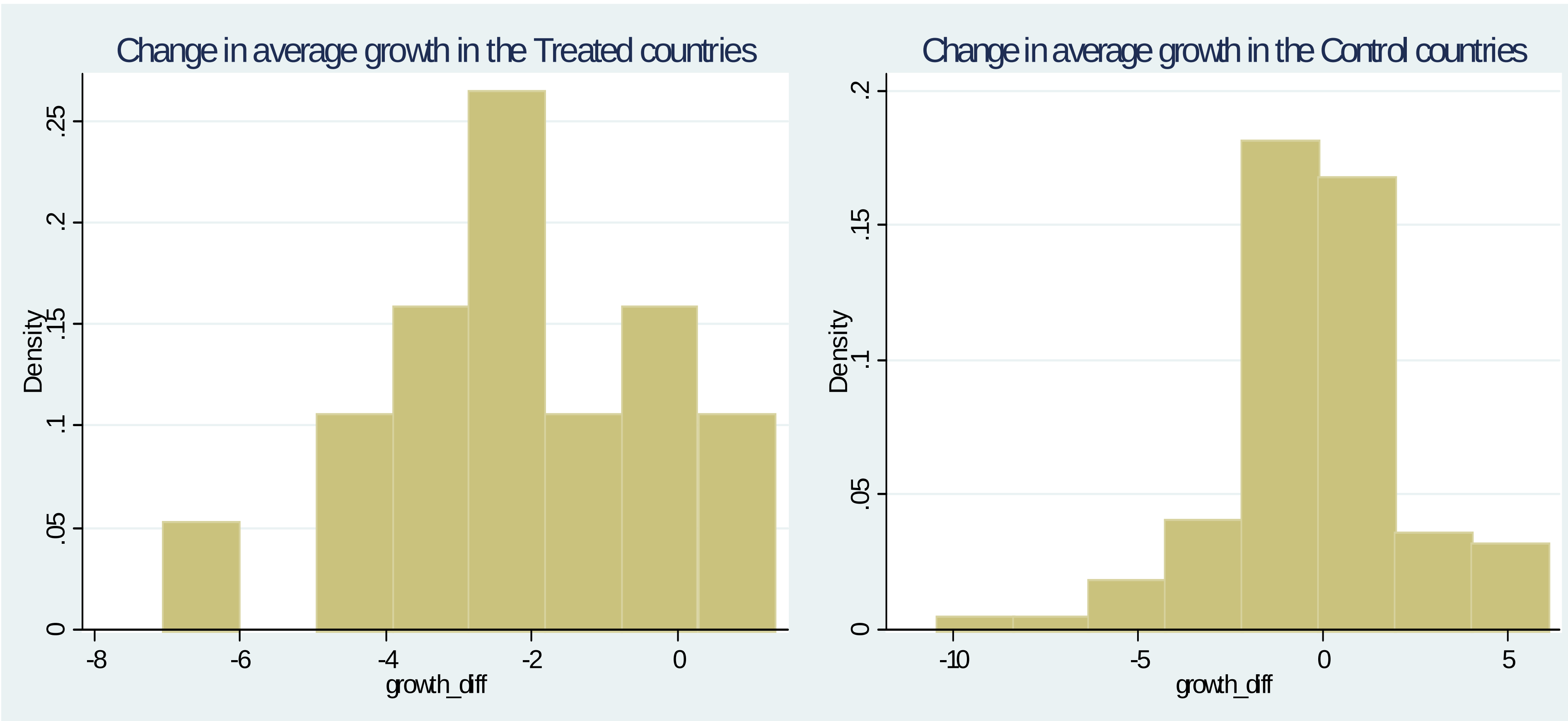
Note: The horizontal axis in each histogram plots the difference between growth after and before reform dates (expressed in percentage points per year)

Figure 3   Effect of democratic transitions in each treated country



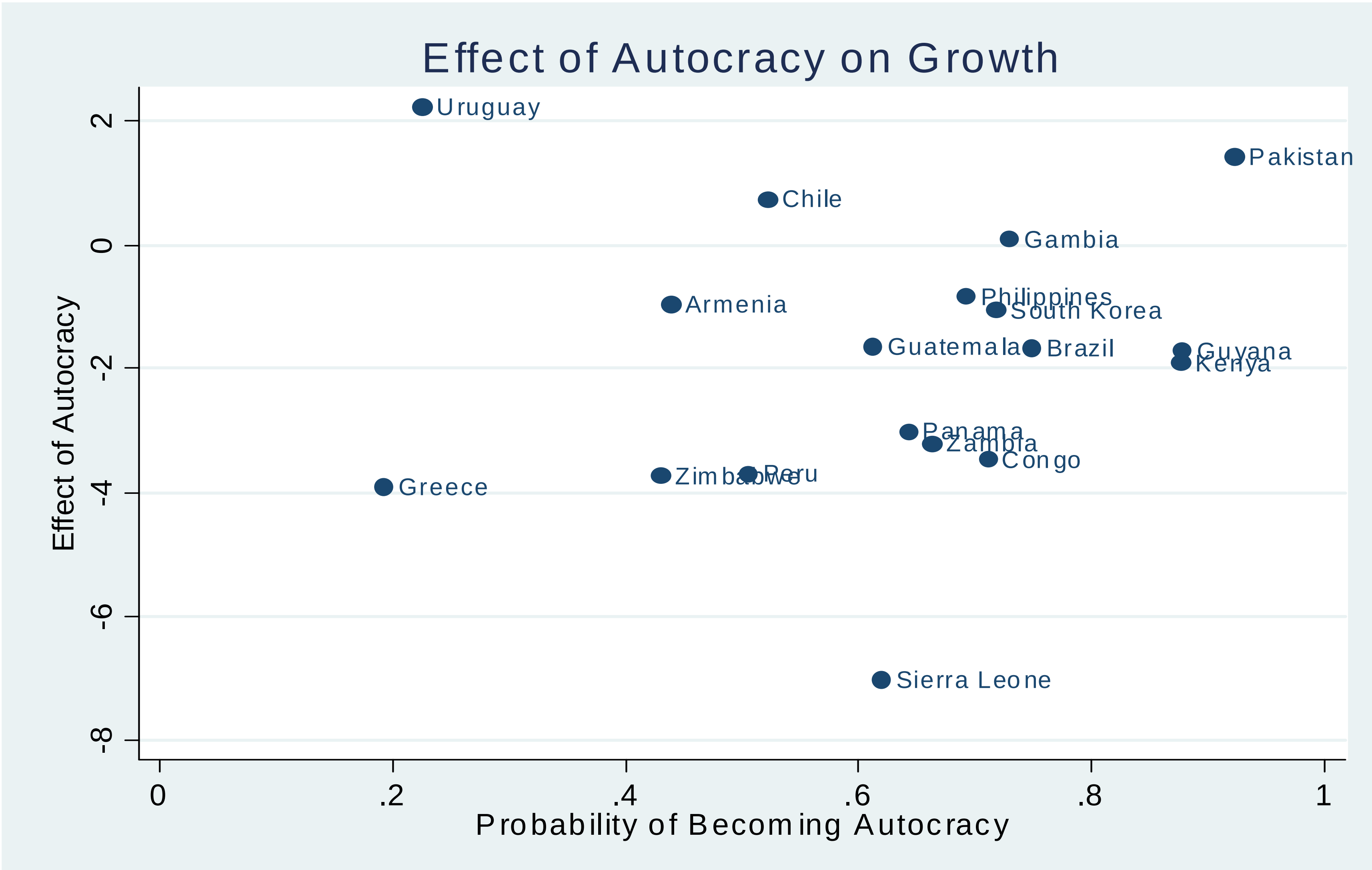
Note: The vertical axis measures the yearly growth effect of democracy in percentage points. Estimates refer to column 3 in Table 5.

Figure 4   Change in growth after becoming autocracy



Note: The horizontal axis in each histogram plots the difference between growth after and before reform dates (expressed in percentage points per year)

Figure 5   Effect of autocratic transitions in each treated country



Note: The vertical axis measures the yearly growth effect of democracy in percentage points. Estimates refer to column 3 in Table 6.

Table 1 Democracy and growth: difference in difference estimates on yearly data

	(1)	(2)	(3)	(4)	(5)
			Growth		
Democracy	0.48	0.58	0.73	0.26	0.35
	(0.34)	(0.54)	(0.42)*	(0.65)	(0.63)
Lagged income	- 5.45	- 6.20	- 5.38	- 5.04	- 6.06
	(0.62)***	(0.81)***	(0.65)***	(0.97)***	(0.93)***
Treatment	Transition to democracy and autocracy	Transition to democracy	Transition to democracy	Transition to autocracy	Transition to autocracy
Control group	Permanent autocracy or democracy	Permanent autocracy	Permanent autocracy or democracy	Permanent democracy	Permanent autocracy or democracy
Observations	4323	2554	4000	1985	2924
N. countries	138	76	123	70	97
Adj. R-sq.	0.08	0.08	0.08	0.13	0.08

Note: Robust standard errors in parentheses: * significant at 10%; ** significant at 5%; *** significant at 1%
Other Covariates: country and year fixed effects; year fixed effects interacted with indicators for Latin America and for Africa, indicators for war years and lagged war years, and an indicator for formerly socialist countries in Central and Eastern Europe and the Asian provinces of the former Soviet Union after 1989.

Table 2 Estimates of the propensity score

	(1)	(2)	(3)	(4)
	Democratic transition		*Autocratic transition*	
Length of sample	2.40	2.52	2.63	4.08
	(1.97)	(1.95)	(1.50)*	(2.20)*
Income relative to the US	- 0.002	- 0.003	- 0.03	- 0.02
	(0.005)	(0.005)	(0.01)***	(0.01)**
War years	- 8.35	-8.14	- 3.69	- 10.33
	(4.71)*	(4.84)*	(5.58)	(7.13)
Domestic democratic capital	8.73	8.82	0.65	-0.35
	(4.25)**	(4.20)**	(2.29)	(2.05)
Foreign democratic capital	1.73	1.90	3.26	2.42
	(1.21)	(1.24)	(1.26)***	(1.31)*
Initial value of polity2		0.04		-0.89
		(0.06)		(0.22)***
Observations	77	77	70	70
Pseudo R-sq	0.17	0.17	0.43	0.61

Note: Robust standard errors in parentheses: * significant at 10%; ** significant at 5%; *** significant at 1%
Relative income, domestic democratic capital, initial value of polity2 are measured in first year of sample, *foreign democratic capital* is measured in 1993.

Table 3a Transitions from autocracy to democracy

Country	*Treated*	*Propensity score*	*Change in polity2*	*Date of reform*
Yemen	0	.1712141	.	
Angola	0	.1947455	.	
Iran	1	.2785125	9	1997
Chad	0	.3203447	.	
Mozambique	1	.3398073	12	1994
Comoros	1	.354881	11	1990
Vietnam	0	.3581062	.	
Uganda	1	.3897252	10	1980
El Salvador	1	.4127302	2	1982
Sierra Leone	0	.4226772	.	
Equatorial Guin.	0	.424049	.	
Guinea-Bissau	1	.4358898	11	1994
Zaire	0	.4407421	.	
Tanzania	0	.4520402	.	2000
Morocco	0	.4527073	.	
Central African Republic	1	.4552693	12	1993
Rwanda	0	.4708738	.	
Mauritania	0	.4757592	.	
Algeria	0	.4805619	.	
Guinea	0	.4810042	.	
Nicaragua	1	.4910639	7	1990
Burundi	0	.4922749	.	
Thailand	1	.5017168	4	1978
Syria	0	.5023594	.	
Niger	1	.5082768	8	1991
Bangladesh	1	.5125053	11	1991
Senegal	0	.5249349	.	2000
Gabon	0	.537788	.	
Ivory Coast	0	.5521293	.	2000
Togo	0	.5554183	.	
Benin	1	.555422	6	1991
Congo	1	.5571044	6	1992
Mali	1	.5590481	7	1992
Cameroon	0	.5675696	.	
Ghana	1	.5689386	3	1996
Jordan	0	.5769697	.	
Nigeria	1	.5864162	7	1979
Madagascar	1	.594099	8	1991
Burkina Faso	0	.5977144	.	1977
Poland	1	.5982632	11	1989

Country	*Treated*	*Propensity score*	*Change in Polity2*	*Date of Reform*
Hungary	1	.6095265	6	1989
Taiwan	1	.611932	8	1992
Malawi	1	.6158609	15	1994
Cyprus	1	.638754	7	1968
Zambia	1	.653224	15	1991
Singapore	0	.6654041	.	
Indonesia	0	.6893978	.	1999
Portugal	1	.69704	6	1975
Lesotho	1	.7038091	15	1993
Nepal	1	.7060294	7	1990
Dominican Republic	1	.7089661	9	1978
China	0	.7145793	.	
Tunisia	0	.7278883	.	
Romania	1	.7553898	7	1990
Mexico	1	.7785828	4	1994
Philippines	1	.7795237	7	1986
South Korea	1	.799453	6	1987
Pakistan	1	.8041176	12	1988
Paraguay	1	.8284625	10	1989
Egypt	0	.8383721	.	
Cuba	0	.8655669	.	
Ethiopia	1	.8730649	1	1993
Haiti	1	.8866652	14	1994
Panama	1	.8921999	16	1989
Guyana	1	.8947882	13	1992
		Outside Common Support		
Guatemala	1	.9190304	8	1966
Guatemala	1	.9190304	4	1986
Ecuador	1	.9237149	14	1979
Honduras	1	.9413305	2	1980
Brazil	1	.9437772	10	1985
Spain	1	.9685184	4	1976
Argentina	1	.979982	16	1983
Uruguay	1	.9839289	16	1985
Bolivia	1	.9866512	15	1982
Peru	1	.9885088	5	1979
Greece	1	.9948298	8	1974
Chile	1	.9977797	9	1989

Note: The propensity score is estimated as in column 1 of Table 2

Table 3b Transitions from democracy to autocracy

Country	*Treated*	*Propensity score*	*Change in polity2*	*Date of reform*
		Outside Common Support		
New Zealand	0	.0014931	.	
Australia	0	.0016789	.	
Iceland	0	.0040472	.	
South Africa	0	.0105352	.	
Switzerland	0	.0115997	.	
Czech Republic	0	.0148975	.	
Slovenia	0	.0238694	.	
United States	0	.0261698	.	
Luxembourg	0	.0281385	.	
Israel	0	.0299115	.	
Denmark	0	.0345439	.	
Germany	0	.0352485	.	
Sweden	0	.0398666	.	
Papua New Guinea	0	.0476861	.	
France	0	.04837	.	
United Kingdom	0	.0497661	.	
Netherlands	0	.0540976	.	
Fiji	0	.0557607	.	1987
Canada	0	.0612058	.	
Venezuela	0	.0615961	.	
Slovak Republic	0	.063058	.	
Latvia	0	.063171	.	
Ukraine	0	.0654528	.	
Italy	0	.0667572	.	
Belarus	1	.0720809	-7	1995
Russia	0	.0729471	.	
		Inside Common Support		
Austria	0	.0757894	.	
Finland	0	.0819311	.	
Norway	0	.0822244	.	
Belgium	0	.0840312	.	
Japan	0	.0974352	.	
Bulgaria	0	.0998625	.	
Estonia	0	.1184082	.	
Namibia	0	.1368068	.	
Trinidad & Tobago	0	.180688	.	

Country	*Treated*	*Propensity score*	*Change in Polity2*	*Date of Reform*
Greece	1	.1918558	-11	1967
Macedonia	0	.2195661	.	
Uruguay	1	.2241872	-6	1972
Ireland	0	.2807057	.	
Sri Lanka	0	.2912095	.	
Malaysia	0	.3415968	.	
Zimbabwe	1	.4292819	-7	1987
Turkey	0	.4345146	.	1980
Armenia	1	.4382235	-9	1996
Peru	1	.5047568	-12	1968
Chile	1	.5215374	-13	1973
Costa Rica	0	.52407	.	
Mauritius	0	.541923	.	
Jamaica	0	.553453	.	
Colombia	0	.5750838	.	
Guatemala	1	.6118631	-4	1974
Sierra Leone	1	.6188506	-7	1971
Panama	1	.6420545	-11	1968
Zambia	1	.6628014	-2	1968
Philippines	1	.6917624	-11	1972
Congo	1	.7105513	-11	1997
South Korea	1	.717416	-12	1972
Albania	0	.7235891	.	1996
Gambia	1	.729219	-15	1994
Brazil	1	.7480876	-6	1964
India	0	.8504922	.	
Kenya	1	.8767781	-2	1966
Guyana	1	.878488	-1	1978
Botswana	0	.9226773	.	
Pakistan	1	.9228303	-15	1977
		Outside Common Support		
Nigeria	1	.9312006	-14	1966
Nigeria	1	.9312006	-14	1984
Lesotho	1	.9540992	-18	1970
Uganda	1	.9912787	-7	1966
Uganda	1	.9912787	-3	1985

Note: The propensity score is estimated as in column 3 of Table 2

Table 4a Treated vs Controls: countries that became democracies

Variable	*Sample*	*Mean*		*t-test*	
		Treated	*Control*	*t*	*p > |t|*
Relative income	Unmatched	-201.16	-228.1	1.59	0.116
	Matched	-222.22	-220.4	-0.12	0.91
Domestic democratic capital	Unmatched	0.12	0.02	3.01***	0.00
	Matched	0.05	0.03	0.64	0.53
Foreign democratic capital	Unmatched	0.60	0.43	2.55***	0.01
	Matched	0.51	0.48	0.44	0.66
Length of sample	Unmatched	0.92	0.87	1.25	0.22
	Matched	0.90	0.90	0.01	0.99
War years	Unmatched	0.04	0.05	-0.50	0.62
	Matched	0.04	0.04	-0.08	0.94
Initial value of polity2	Unmatched	-4.78	-5.07	0.29	0.77
	Matched	-5.03	-5.43	0.39	0.70
Latin America	Unmatched	0.37	0.04	3.45***	0.00
	Matched	0.22	0.06	1.99*	0.05
Asia	Unmatched	0.14	0.14	0.0	1.00
	Matched	0.19	0.20	-0.08	0.93
Africa	Unmatched	0.33	0.71	-3.49***	0.00
	Matched	0.43	0.67	-2.05**	0.04

Note: *polity2, relative income, democratic capital* are measured in first year of sample, f*oreign democratic capital* is measured in 1993. Matching is based on the estimates reported in column 1 of Table 2.
When computing the unmatched means we do not impose the common support restriction, while we do when computing the matched means.

Table 4b Treated vs. controls: countries that became autocracies

Variable	*Sample*	*Mean*		*t-test*	
		Treated	*Control*	*t*	*p > |t|*
Relative income	Unmatched	-217.89	-95.43	- 6.50***	0.00
	Matched	-194.20	-185.44	-0.41	0.69
Domestic democratic capital	Unmatched	.10	.25	-2.49**	0.01
	Matched	.137	.16	-0.33	0.74
Foreign democratic capital	Unmatched	.57	.69	-1.44	0.15
	Matched	.61	.71	-0.97	0.34
Length of sample	Unmatched	.84	.75	1.13	0.26
	Matched	.88	.80	0.99	0.33
War years	Unmatched	.05	.03	1.46	0.15
	Matched	.04	.05	-0.09	0.93
Initial value of polity2	Unmatched	4.12	8.68	- 6.67***	0.00
	Matched	3.39	8.13	- 4.41***	0.00
Latin America	Unmatched	.28	.11	1.90*	0.06
	Matched	.39	.33	0.37	0.71
Asia	Unmatched	.16	.09	0.96	0.34
	Matched	.17	.19	-0.17	0.87
Africa	Unmatched	.44	.09	3.83**	0.00
	Matched	.33	.20	0.88	0.39

Note: *Polity2, relative income, democratic capital* are measured in first year of sample, f*oreign democratic capital* is measured in 1993. When computing the unmatched means we do not impose the common support, when computing the matched means we do.

Table 5 Democracy and growth: OLS and Matching estimates of the growth effect of becoming a democracy

	(1)	(2)	(3)	(4)	(5)	(6)
			Growth			
Growth effect of democracy	0.60	0.74	1.08	1.19	0.83	1.01
in the group of treated countries	(0.54)	(0.68)	(0.78)	(0.77)	(0.79)	(0.77)
			(1.24)	(1.25)	(1.25)	(1.26)
Estimation	Diff in diff 2 steps	Diff in diff 2 steps	Matching	Matching	Matching	Matching
Kernel			Epanechnikov	Normal	Epanechnikov	Normal
Propensity score conditional on *initial value of polity2*			No	No	Yes	Yes
Inside common support	No	Yes	Yes	Yes	Yes	Yes
N. Treated countries	49	37	37	37	36	36
N. Control countries			28	28	28	28
N. Controls incl. repetitions			651	937	639	910

Note: Cols (1-2): Standard errors in parenthesis. Cols (3)-(6): First parenthesis: standard errors estimated assuming independent observations, second parenthesis: standard errors estimated assuming perfect correlations of repeated observations in control countries.
Cols (1-2): Outcome variable: Averaged residual of a regression of growth on country and year fixed effects. First step of Diff in diff 2 steps: OLS of yearly growth on country and year fixed effects, in a sample that also includes the control countries, second step: OLS of averaged residuals in the treated countries only (averaged before and after treatment respectively), on dummy variable equal to 1 after treatment
Cols (3-6): Outcome variable: change in average growth (after – before reform year).
Common support imposed (according to Table 3a) as indicated in all columns.

Table 6 Democracy and growth: OLS and Matching estimates of the growth effect of becoming an autocracy

	(1)	(2)	(3)	(4)	(5)	(6)
			Growth			
Growth effect of autocracy	0.17	-0.84	- 1.97	- 1.85	- 2.38	- 1.55
in the group of treated countries	(0.72)	(0.42)*	(0.58)***	(0.53)***	(1.31)**	(0.75)**
			(1.00)**	(0.92)**	(3.59)	(1.57)
Estimation	Diff in diff 2 steps	Diff in diff 2 steps	Matching	Matching	Matching	Matching
Kernel			Epanechnikov	Normal	Epanechnikov	Normal
Propensity score conditional on *initial value of polity2*			No	No	Yes	Yes
Inside common support	No	Yes	Yes	Yes		
N. Treated countries	20	18	18	18	14	14
N. Control countries			18	18	15	15
N. Controls incl. repetitions			107	289	34	176

Note: Cols (1-2): Standard errors in parenthesis. Cols (3)-(6): First parenthesis: standard errors estimated assuming independent observations, second parenthesis: standard errors estimated assuming perfect correlations of repeated observations in control countries.
Cols (1-2): Outcome variable: Averaged residual of a regression of growth on country and year fixed effects. First step of Diff in diff 2 steps: OLS of yearly growth on country and year fixed effects, in a sample that also includes the control countries, second step: OLS of averaged residuals in the treated countries only (averaged before and after treatment respectively), on dummy variable equal to 1 after treatment
Cols (3-6): Outcome variable: change in average growth (after – before reform year).
Common support imposed (according to Table 3a) as indicated in all columns, except in cols (5-6), where it is [0.11, 0.98]

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