The Democracy-Dictatorship measure: Alternation in power and biased estimates of democracy's effect on economic growth

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Abstract

Incumbents voluntarily leaving office after losing elections is a hallmark of democracy. Hence, the most prominent binary regime measure – the DD, or ACLP, measure – includes observed alternations in office as a necessary condition for being coded as democratic. We argue that this criterion may lead to biases when empirically investigating relationships between democracy and outcome variables of interest, since government alternation is not only a function of regime type. We demonstrate this by analyzing how the criterion leads to underestimating the effect of democracy on economic growth. First, we explain and theoretically model the bias. Since the probability of incumbents losing democratic elections is reduced by strong economic performances, young democracies with high growth may falsely be coded dictatorships by DD; their popular governments have yet to lose an election. We design different empirical tests to identify whether the expected bias exists or not. Notably, we employ only DD's own coding rules and differences in information about regimes from different points in time to estimate the bias. As expected, using DD leads to underestimating democracy's effect on growth. We also present resembling arguments and test how DD may bias results on democracy's relationship with civil war onsets and coup d'etats.

1 Introduction

There is a wide variety of democracy measures available, drawing on different conceptualizations of democracy and using quite different indicators and aggregation methods (see Munck and Verkuilen 2002; Coppedge et al. 2011). Although most extant democracy measures correlate highly, choosing one over another can affect the conclusions drawn in empirical work. When applying the Polity, Freedom House, and Vanhanen's Polyarchy indexes, Casper and Tufis (2003) report that only three of nine investigated determinants of democracy are robust. Cheibub, Gandhi and Vreeland (2010) show that choice of measure also matters for estimates on how democracy affects economic growth (see also, e.g. Krieckhaus 2004) and civil war onset (see also, e.g. Vreeland 2008). Choice of measure matters.

One of the most used and trusted measures of political regime types is the popular DD (Democracy and Dictatorship) measure (Cheibub, Gandhi and Vreeland 2010) – also known as the ACLP (Alvarez-Cheibub-Limongi-Przeworski) measure (Alvarez et al. 1996; Przeworski et al. 2000). It draws on the Schumpeterian notion that contested elections are the crux of democracy; identifying whether or not elections exist and, if so, whether they are contested are critical tasks when coding regimes. At the heart of the DD coding scheme lies the requirement that regimes should only be scored democratic if there has been at least one observed alternation in power under. In this paper, we point out that this requirement – the alternation rule, henceforth – is problematic and may induce biases when investigating (causes and) effects of democracy and *alternations in power are somehow endogenous to these effects*. We focus on how using DD leads to underestimating the effect of democracy on economic growth.

DD was initially introduced in Alvarez et al. (1996), and further elaborated on in Przeworski et al. (2000). It has later been used in numerous high-profile publications to investigate, for example, how income inequality (Houle 2009), oil resources (Haber and Menaldo 2011) and Islam (Potrafke 2012) impact on democracy, or how democracy affects, for instance, states' credit access and credit ratings (Beaulieu, Cox and Saiegh 2012), transparency (Hollyer, Rosendorff and Vreeland 2011) and gender inequality in ed-

ucation (Cooray and Potrafke 2011). Highlighting the importance of using objective and minimalist measures, Cheibub, Gandhi and Vreeland (2010) make a strong case for DD. The clear coding rules leave little room for subjective judgements, and thus unsystematic errors, giving DD a reliability-edge over measures such as Polity and Freedom House. Furthermore, Polity and Freedom House involve multiple components and indicators, and "including more dimensions along which to classify political regimes makes it harder to specify the causal mechanisms that link regime and the outcome of interest" (Cheibub, Gandhi and Vreeland 2010, 73). In contrast, DD draws on a minimalist conceptualization of democracy as a regime allowing for contested multi-party elections.

DD has played a central role in studies of links between income and democracy. In their seminal book Przeworski et al. (2000) use DD to, for instance, investigate how democracy affects economic growth, concluding that democracy has little or no effect. Although several later contributions using different measures report a positive relationship (e.g. Baum and Lake 2003; Gerring et al. 2005; Papaioannou and Siourounis 2008), the results in Prezeworski et al. remain widely acknowledged. One reason is skepticism towards employing maximalist measures (see Cheibub, Gandhi and Vreeland 2010); factors such as executive constraints (a dominant component of the Polity Index) or low corruption (an indicator in the Freedom House Index) may drive the correlation with growth, and there is no consensus on whether they are part of the democracy concept. Below, however, we provide strong indications that (even) "minimalist democracy" may have a far more positive impact on economic growth than previously held. We show that DD's alternation rule leads to biases when estimating the effect of democracy on growth. As the large literature on economic voting shows (see Lewis-Beck and Stegmaier 2000), democratic governments are less likely to be tossed by voters when growth is high. Hence, (fairly young) democracies may fail to pass the alternation rule not because they are autocratic, but because they have economically well-performing governments. In other words, (young) democracies presiding over very high growth are at particular risk of being falsely classified as dictatorships.

Below, we first describe DD, focusing on the alternation rule and its use of retrospec-

tive coding. Thereafter, we discuss why the alternation rule may lead to biased results on democracy and growth, first illustrating by considering the case of Botswana, before presenting the more general argument. Thereafter, we design different tests to evaluate whether the expected bias is at work. For example, we compare results produced by DD with results generated by another minimalist democracy measure (Boix, Miller and Rosato 2013) not relying (solely) on the alternation rule for coding contested elections. We also re-code DD – using identical coding rules, but varying the year of measurement – to exploit that updated information on the true nature of regimes corrects errors in DD. Hence, we can evaluate DD-induced biases by comparing estimates from regressions based on data from, say, 1946–1990 and using "real-time" coded DD (using government-alternation information available in 1990) with regressions on the same observations but applying the information we have at current. This allows us to investigate how estimates change as history unfolds and we learn about the true nature of a regime – with errors in the DD coding being "corrected". Our empirical analysis clearly shows that DD underestimates the effect of democracy on economic growth, and the bias is sizeable. We also briefly discuss whether resembling biases are present in estimates of democracy's impact on coup d'etats and civil war onsets, but find less clear evidence of biases for these relationships.

2 The DD measure

DD explicitly draws on a minimalist democracy concept with contestation as the only component (Munck and Verkuilen 2002). Contested multiparty elections is the (necessary and sufficient) institutional requirement for being considered democratic. Hence, Przeworski et al. (2000, 15) define democracy simply as a regime where "those who govern are selected through contested elections". Different researchers have criticized such minimalist and procedural definitions, contending that democracy is a multi-faceted concept (see, e.g. Dahl 1971; Munck and Verkuilen 2002). Another potential validity problem relates to judging what a contested election really is, and whether contestation is not a continuous

rather than dichotomous characteristic. Przeworski et al. (2000, 16) claim an election is contested when the opposition at least has "some chance of winning office", but there are substantial problems related to delineating what "some chance" is (see Boix, Miller and Rosato 2013). Yet, its proponents argue in favor of a minimalist, procedural definition, in part, because of its implications for reliable operationalization and the related benefits for subsequent empirical analysis of democracy's determinants and consequences. As highlighted by Cheibub, Gandhi and Vreeland (2010) democracy indices relying on more complex concepts involving multiple dimensions run into problems of "subjectiveness and arbitrariness" (p. 75). In other words, the benefits of a minimalist definition relates to precision and stringency, enabling "objective" measurement.[1] We will not engage in the debate on how to best conceptualize democracy here. We rather investigate the claim that DD – the most popular minimalist democracy measure – is well suited for empirical research on the correlates of democracy.

The logical structure of DD is quite simple. A regime is classified as democratic *if and only if* it passes four specific rules, and as dictatorial if failing at least one. The two first consider whether or not public offices are filled through elections, and the two other whether (eventual) elections were actually contested. The first rule is "[t]he chief executive must be elected", and the second is "[t]he legislature must be elected" (Przeworski et al. 2000, 15). The third is "[t]here must be more than one party", and this is extended to consider whether or not governments subsequently established no-party or one-party rule or permanent electoral domination (Przeworski et al. 2000, 20–22).

Contestation is, however, not exhausted by the presence of multiparty elections, and observing *de facto* contestation is more difficult than identifying whether multi-party elections take place. Among regimes holding multi-party elections, there are some that "hold elections only because the opposition cannot win and some in which the opposition would not be allowed to assume office had it won" (Alvarez et al. 1996, 13). These are regimes in which contestation is merely a fiction. Several autocracies hold multi-party

[1]Reduced reliability may, however, be an issue with *dichotomous* regime measures. Elkins (2000) shows how the lower quantity of misclassifications may be balanced out by more serious classification errors – e.g. falsely coding a democracy as dictatorship on DD versus falsely giving 6 rather than 5 on Polity.

elections that are neither free nor fair, where the opposition has little chance of obtaining power (e.g. Schedler 2006; Levitsky and Way 2010). Trying to combine coding rules that are "objective", and thus reliable, with the difficulties of observing contestation leads to the fourth DD criterion for coding regimes as democratic: "[A]n alternation in power under electoral rules identical to the ones that brought the incumbent to office must *have taken place*" (Cheibub, Gandhi and Vreeland 2010, 69 (our itallics)).[2] The underlying logic of this *alternation rule* is that regimes – or should one perhaps say governments – must *prove* they are democratic through accepting an election loss and subsequently stepping down. Although many governments prefer to publicly state that they are democratically minded, observers can not know whether this is true before they are tested and forced to reveal their *real preferences* through losing an election. If a regime observes alternation of government through elections, DD then provides the democracy score retrospectively for all years operating under the current set of "regime rules". This leads Cheibub, Gandhi and Vreeland (2010) to re-code the original Alvarez et al. (1996) score for Bangladesh for 1986–1990, not because they use different rules but because they have obtained new information (government alternation in 1996) with time passing.

Scoring whether elections are contested or not, and hence whether a regime is democratic or not, may lead to two types of errors – so-called "Type I errors", coding non-democracies as democracies, or "Type II errors", coding democracies as non-democracies. The alternation rule is explicitly constructed to minimize Type I errors, thereby also increasing the chance of making Type II errors. Alvarez et al. (1996) and Cheibub, Gandhi and Vreeland (2010) are clear that the alternation rule may lead to systematic errors by assigning the dictatorship-label to democracies that have popular governments yet to

[2]Yet, it is not always clear what the relevant regime rules are. Mexico observed alternation after elections in 2000. The preceding PRI regime had existed for several decades, holding multi-party elections since the late-1920s. However, these elections were not free and fair; Mexico was arguably an electoral autocracy (Magaloni 2006). Should Mexico be backcoded as democratic until the 1920s? Cheibub, Gandhi and Vreeland (2010) solve this by referring to changes in the organization arranging federal elections; "we date the transition to democracy to 2000, when Fox, the candidate of one of the opposition parties, was sworn into the presidency. The electoral rules were changed under the Zedillo presidency (1994-2000) when, in 1996, an accord between the ruling PRI and the two opposition parties (PAN and PRD) ended the PRIs control of the Federal Electoral Institute" (p.71). Hence, they avoid scoring Mexico in previous decades such that it have lacked face validity. Yet, they arguably do so by invoking the kind of subjective evaluations DD is supposed to mitigate; what constitutes the regime rules, and how substantial must changes in particular rules be to generate regime change?

lose an election. Still, given that the counter-factual (what would the government have done had it lost an election?) is unobservable, they argue it is better to err on the side of caution with assigning the democracy label to regimes. This leads, for instance, to Botswana and South Africa being coded as dictatorships by Cheibub, Gandhi and Vreeland (2010), as the Botswana Democratic Party (BDP) and African National Congress (ANC), respectively, have yet to lose an election. Furthermore, if DD had been coded in, say, the late 1980s, Japan would have been classified as dictatorial, due to the Liberal Democratic Party's (LDP) electoral dominance until 1993. In Western Germany, which few would dispute was democratic after WWII, it took until 1969 for the Christian Democratic Union (CDU) led government to lose power through an election. Hence, DD may sometimes classify genuine democracies as dictatorships simply because their governments have yet to lose elections. Although this has long been recognized, we argue that it is much more consequential than previously supposed.

3 Why DD may yield biased estimates of democracy's effect on economic growth

One key problem with the alternation rule is that the probability of making Type II errors may be endogenous to the potential correlates of democracy we are interested in studying. Here, we show how this applies to economic growth. Before presenting the general argument, we show how using DD may generate downward biases in the estimated effect of democracy on economic growth by considering a particularly illustrative case:

3.1 Botswana

The economic growth miracles of the East Asian Tiger states have received much scholarly and public attention, and the current Chinese growth miracle even more so. Another spectacular economic success story has, however, taken place in the small and landlocked Sub-Saharan African country of Botswana. Przeworski et al. (2000, 177) identify that among all regimes in their dataset, the current Botswanaian regime presided over the

fastest-growing economy; from 1966–1990 Botswana's average annual GDP growth was 9.6 percent. Using updated data from Maddison (2007), Botswana's (per capita) income increased more than tenfold from 1966–2008, from 473 to 4769 USD (1990 USD, PPP-adjusted). Although the extensive diamond reserves is often mentioned as an enabling condition, abundance of such resources has not contributed to high growth – rather to the contrary – in countries such as Sierra Leone and DR Congo (e.g. World Bank 2003, 127). In line with analysis of the institutional contingencies of the "resource curse" (e.g. Mehlum, Moene and Torvik 2006), Botswana's benevolent institutional framework was arguably crucial in transforming resource abundance into high growth (e.g. Acemoglu, Johnson and Robinson 2001). Indeed, case studies on Botswana describe how the country's institutional framework has incentivized politicians to pursue a range of growth-enhancing policies, from prudent macroeconomic- and exchange rate policies to provision of productivity-enhancing public investment in education, health and infrastructure (e.g. Leith 2005; Danevad 1995; Tsie 1996).

In addition to Botswana's economic successes after decolonization from Britain in 1966, the literature also highlights its political successes. Botswana has since independence been characterized by fairly transparent institutions and a liberal regime with multi-party elections and decent protection of various political and civil liberties. By many observers, Botswana was long considered one of few democracies in Sub-Saharan Africa (e.g. Bratton and van de Walle 1997; Lindberg 2006), and remains one of only eight countries in Africa rated "Free" by Freedom House. The latest Polity score (2010) also ranks Botswana as highly democratic (+8). Botswana's elections are single-constituency plurality elections, which should reduce the number of effective parties and increase the probability of having single-party governments (Powell 2000). The BDP has held a majority of parliamentary seats and votes in all elections since 1969, although its vote-share has declined quite substantially in the last decades. Therefore, Botswana fails to pass the alternation rule, and is scored a dictatorship by DD.

Yet, Botswana is explicitly highlighted as a potential Type II error – a genuine democracy misclassified as an autocracy – by Alvarez et al. (1996). Case studies of Botswanaian

politics then also indicate – as do the Polity and Freedom House coding mentioned above – that the multi-party elections and the wider political system have "induced the Government to be responsive to the interests of various segments of Botswana society" (Danevad 1995, 401). Furthermore, there is "no constraints on the opposition, little visible repression, [and] no apparent fraud" (Alvarez et al. 1996, 10). The proponents of DD are, of course, right in that the various leaderships of the BDP have never been forced to reveal their intentions on accepting election defeat, and one could speculate "whether elections are not held in Botswana only because the ruling party is certain to win them and whether the ruling party would yield office if it ever lost" (Alvarez et al. 1996, 10). Yet, few observers have questioned the freeness and fairness of Botswana's elections, and other institutional features and acts by political leaders have generally been interpreted as in concordance with democratic ideals.

Thus, the BDP may consistently win elections – with the consequence that Botswana continues to be scored a dictatorship by DD – simply because it is highly popular among voters. Indeed, the BDP's popularity may stem, in large part, from its apt handling of economic policies, and the resulting high growth rates. Hence, the high growth rates may paradoxically contribute to the regime being misclassified as a dictatorship. *If* there counterfactually would have been turnovers in Botswana, should the BDP have been less popular and actually lost a free and fair election, also Przeworski et al. (2000) and Cheibub, Gandhi and Vreeland (2010) would agree that Botswana is erroneously classified as a dictatorship from de-colonization onwards. Hence, Botswana's success in terms of high growth may actually have contributed to the country being falsely put in the DD's dictatorship category all the way back to 1966. Coding errors in regime type being endogenous to high growth is particularly problematic if one wants to draw inferences regarding how regime type influences growth. Although Botswana may be an extreme case, the suggested mechanism could work also in other countries failing on the alternation rule, such as Montenegro, Namibia, and The Seychelles. Hence, analysis using DD may underestimate the effect of democracy on growth since some high-growing, young democracies are scored as dictatorships because their successful governments are too pop-

ular to lose elections. Had DD been coded some decades ago, a number of fast-growing post-WWII democracies would have been coded as dictatorships as well. Western Germany and Japan are cases where post-war governments stayed in office for a very long time, arguably in part because of popular economic policies and high growth.

This resonates with one of the stronger findings in political economy: Voters tend to re-elect governments when the country performs well economically and throw governments out when the economy is not performing well (see, e.g. Powell and Whitten 1993; Lewis-Beck and Stegmaier 2000). This may stem from retrospective voting mechanisms – votes are reward-or-punishment tools used to discipline politicians (e.g. Ferejohn 1986) – or because voters use past performance as an information signal on the competencies of those in power (e.g. Besley 2006). As noted in a survey of the field:

> Economic conditions shape election outcomes in the worlds democracies. Good times keep parties in office, bad times cast them out. This proposition is robust, as the voluminous body of research reviewed here demonstrates. The strong findings at the macro level are founded on the economic voter, who holds the government responsible for economic performance, rewarding or punishing it at the ballot box. Although voters do not look exclusively at economic issues, they generally weigh those more heavily than any others, regardless of the democracy they vote in. (Lewis-Beck and Stegmaier 2000, 183)

3.2 Modeling the bias

To more precisely illustrate how the alternation rule may lead to biased inferences on democracy and growth, we let p be the probability of the opposition gaining office through elections. Although it may be very low in some regimes, $p > 0$; even manipulated elections inevitably carry some elements of uncertainty, as the autocrat may be unable to perfectly control the election result and wider outcome. Moreover, $p < 1$ as the opposition is never guaranteed a victory, even in democracies. Further, p is arguably endogenous to several factors, but we focus on two. We let ϕ denote the regime characteristics that are

relevant for affecting the probability of opposition takeover. ϕ could, for instance, reflect the intensity of preferences for holding on to office relative to other motivational aspects, thus indicating how large costs or risk the incumbent would accept to steal an election. Alternatively, one could model all actors as homogeneous and let the "institutional environment" determine the costs and benefits of electoral malpractice. In any case, ϕ relates to characteristics relevant for distinguishing whether a regime is democratic or dictatorial.

ϕ might be considered continuous; both the relevant motivational characteristics and the strength of institutional checks affecting benefits and costs of manipulating elections are graded phenomena. Nevertheless, to keep in line with DD we model ϕ as dichotomous. When $\phi = 0$, we have a dictatorship where the ruler is either intensely motivated by staying in power or where the institutional environment induces low costs of manipulating elections. When $\phi = 1$ the regime is democratic; elections are freely and fairly conducted and the ruler will not or cannot stay in power after an election loss. Independently of whether ϕ is continuous or dichotomous, $\frac{\partial p}{\partial \phi} > 0$; more democratic regimes have a higher probability of government alternation through elections. Still, p is a function also of other factors, including economic growth, η, which is one vital performance indicator, also for economic voting (Lewis-Beck and Stegmaier 2000). Opposition parties have a harder time winning elections over incumbent governments that are perceived to be economically competent, both in democracies and in dictatorships (see Magaloni 2006). Hence, $p = p(\phi, \eta)$ and $\frac{\partial p}{\partial \eta} < 0$.

Under certain assumptions, notably including that η is exogenous and uncorrelated with ϕ, it is unproblematic to use the alternation rule – which employs information from the realized outcomes from the p-distribution – to draw inferences on whether $\phi = 0$ (dictatorship) or $\phi = 1$ (democracy). Albeit judgements for particular regimes will be associated with measurement errors, the rule provides a proper signal, on average, when democracy and growth are unrelated. Yet, one question addressed in Przeworski et al. (2000), is whether regime type influences economic growth – i.e. whether $\eta = \eta(\phi)$. Przeworski et al. (2000) report no clear effect of democracy, as measured by DD,

on growth.[3] Later studies using different regime measures – not relying as heavily on an alternation rule – have reported a positive relationship (e.g. Baum and Lake 2003; Knutsen 2011*b*). Hence, it may be that $\frac{\partial \eta}{\partial \phi} > 0$. If so, estimates of democracy's effect on growth based on regime measures relying on government alternation may be downward biased. To illustrate this, we consider fairly simple functional specifications of p and η. We model growth (η) as a linear function of regime type (ϕ):

$$\eta = b_0 + b_1\phi \tag{1}$$

We model the probability of government alternation (p) as endogenous to ϕ and η using a logistic function:

$$p = \frac{1}{1 + e^{-(\beta_0 + \beta_1\phi - \beta_2\eta)}} \tag{2}$$

We rearrange and take logs of Equation 2, before inserting the expression for ϕ obtained from Equation 1, to obtain an expression for growth as a function of probability of government alternation:

$$\begin{aligned} e^{-(\beta_0+\beta_1\phi-\beta_2\eta)} &= \frac{1-p}{p} \Rightarrow \\ -(\beta_0 + \beta_1\phi - \beta_2\eta) &= ln(1-p) - ln(p) \Rightarrow \\ \beta_0 + \beta_1(\frac{\eta - b_0}{b_1}) - \beta_2\eta &= ln(p) - ln(1-p) \Rightarrow \\ \eta &= \frac{b_1 ln(p) - b_1 ln(1-p) + b_0\beta_1 - b_1\beta_0}{\beta_1 - b_1\beta_2} \end{aligned} \tag{3}$$

We can now determine how probability of opposition victory is related to growth:

[3]However, this is only when using growth in total GDP. Also Przeworski et al. (2000) find evidence of a positive effect of democracy on GDP per capita growth.

$$\frac{\partial \eta}{\partial p} = \frac{1}{\beta_1 - b_1\beta_2}(\frac{b_1}{p} - (-1) * \frac{b_1}{1-p}) = \frac{1}{\beta_1 - b_1\beta_2}\frac{b_1(1-p) + b_1 p}{p(1-p)} = \frac{b_1}{(\beta_1 - b_1\beta_2)(p-p^2)} \tag{4}$$

From Equation 1, we know the first-derivative of growth with respect to regime type is b_1. When comparing this to the first-derivative of growth with respect to probability of government alternation from Equation 4, we see that they are only equivalent when $b_1 = 0$. In this case, regressions using p as proxy for ϕ would estimate the true effect on growth, namely zero. If $b_1 \neq 0$, regressions using p – or measures based on p such as DD – would yield biased estimates. The size of the bias $(b_1 - \frac{b_1}{(\beta_1 - b_1\beta_2)(p-p^2)})$ depends on the true effect of democracy on growth, on the effects of regime type *and* of growth on probability of government alternation, and on the underlying probability of government change.

Indeed, we cannot even *a priori* determine the sign of $\frac{\partial \eta}{\partial p}$, even if the true effect of democracy on growth (b_1) is positive – $p - p^2 > 0$, but whether $\beta_1 - b_1\beta_2$ is positive or negative depends on the real-world effects of regime type on growth and of regime type and growth on probability of government alternation. If both the estimated effect of democracy on growth and of growth on probability of opposition victory are substantially large, regression models using regime measures based on alternation rules may *estimate* that democracy reduces economic growth even if democracy *actually* enhances growth. However, if the correlation between regime type and alternation is very strong and the effect of economic growth on alternation is modest, a measure using an alternation rule would likely provide a positive, but downward biased, estimate of democracy on growth.

4 Empirical analysis: DD and the democracy–growth relationship

We design different tests to check for the above-hypothesized bias. Below, we for instance use DD's own coding criteria to create a "real-time DD" measure that allows us to investigate what happens to growth estimates when Type II errors (democracies falsely coded as dictatorships) are corrected by new historical information. We also employ a measure, from Boix, Miller and Rosato (2013), that closely resembles DD but does not exclusively rely on the alternation rule to investigate the bias.

4.1 Preliminary tests: re-coding Type II regimes

The average GDP per capita growth is 1.81 for observations classified as autocracies by DD (n=5134, 1946–2008; see Appendix Table 1) and 2.52 (n=4003) for regimes coded democracies. Interestingly, average growth for countries passing the three first DD rules, but failing the alternation rule, is 2.99. Hence, these regimes have growth rates more similar to DD-coded democracies than the remaining DD-coded dictatorships (1.56). Indeed, these regimes' GDP per capita, on average, grow 0.5 percent faster than the regimes coded democratic by DD. If (many of) the Type II regimes are *actually democratic*, DD-based estimates on how democracy affects growth will be downward biased. Yet, Type II regimes may grow faster because they are associated with other characteristics conducive to high growth, and we therefore investigate the proposed bias more thoroughly by running regression models.

There is no consensus on how to specify regression models investigating the effect of democracy on growth (see Doucouliagos and Ulubasoglu 2008). We present a fairly parsimonious specification, but the findings relevant for our argument – on the direction and size of the bias induced by using DD – are fairly robust across control variable and lag-length specifications, estimation techniques, treating democracy as endogenous or exogenous, and data sources (see Appendix). Here, we employ cross section time series data and an OLS model adjusting for panel-level heteroskedasticity, panel-specific

AR(1) autocorrelation, and contemporaneous correlation. GDP per capita growth in percent is the dependent variable. We include *Ln GDP per capita* (level) – which is highly correlated with democracy – to control for probable convergence effects (Barro and Sala-i Martin 2004). Second, we include *Ln population*, since population size may affect market specialization and economies of scale, and thus economic growth (Romer 1990), and possibly regime type (Dahl and Tufte 1973). The income and population data are from Maddison (2007). Third, we control for *Ln(regime duration+1)*, using Polity IV data, as a proxy for political instability. Political instability likely affects growth particularly through affecting the investment climate (Alesina et al. 1996). Fourth, high ethnic fractionalization may be detrimental to growth (Easterly and Levine 1997), but also to democratization prospects and democratic durability (Alesina et al. 2003). Hence, we control for the *Ethnic Fractionalization Index* from Alesina et al. (2003), ranging from 0–1. Fifth, we control for *region dummies*, since geographic, cultural and political-historical factors related to specific regions may impact on both regime type and growth (see, e.g. Acemoglu et al. 2008). Finally, we add *decade dummies*; different time periods have been associated both with varying global growth rates (Maddison 2007) and varying democratization patterns (Huntington 1991). We lag the independent variables 5 years to account for likely delays in effects on growth (see Papaioannou and Siourounis 2008).

Model I in Table 1 runs this baseline specification, with DD as regime measure, on 6701 observations distributed over 153 countries and with maximum time series (on dependent variable) from 1950–2008. The DD coefficient is only 0.15 – suggesting that democracies' GDP per capita on net grows 0.15 percent faster than dictatorships', holding the different controls constant. According to this regression, democracy is not systematically related to economic growth (t=0.70). Model II is similar to Model I except for coding *all* regimes that passed the three first DD rules as democracies. As discussed, the Type II regimes may be democracies that have not yet observed government alternation due to having popular incumbent governments, in part stemming from high growth rates. Indeed, Model II using the re-coded DD measure strikingly reports an estimate more than *four times the size* of that in Model I (0.70 vs 0.15). Furthermore, the coefficient for this

re-coded DD turns highly significant (t=3.5). Above, we highlighted Botswana as *one* notable high-growing Type II regime likely miscoded as autocratic by DD. Indeed, when re-running our baseline Model I, and flipping *only* the current Botswanaian regime from the autocratic to the democratic column (Model III), DD increases by 75 percent (from 0.15 to 0.27), and the t-value increases from 0.7 to 1.3.

	Model I Baseline DD	Model II Re-coded DD: All Type II democratic	Model III Re-coded DD: Botswana democratic	Model IV Baseline DD on BMR sample	Model V BMR regime measure
	b/(t)	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy	0.151	0.695****	0.265	0.201	0.427**
	(0.70)	(3.47)	(1.26)	(0.93)	(2.10)
Ln GDP per capita	-0.315	-0.385	-0.328	-0.301	-0.325
	(-1.25)	(-1.57)	(-1.31)	(-1.20)	(-1.31)
Ln population	0.036	0.015	0.030	0.037	0.031
	(0.58)	(0.23)	(0.48)	(0.58)	(0.48)
Ln regime duration	-0.046	-0.047	-0.044	-0.036	-0.035
	(-0.53)	(-0.53)	(-0.51)	(-0.40)	(-0.39)
Ethnic fractionaliz.	-1.683****	-1.609****	-1.657****	-1.678****	-1.662****
	(-4.08)	(-3.91)	(-4.03)	(-4.01)	(-3.97)
E.Europe – ex-Soviet	0.950*	1.185**	1.008**	0.908*	1.039**
	(1.92)	(2.43)	(2.05)	(1.83)	(2.09)
Sub-Saharan Africa	-1.561**	-1.437**	-1.509**	-1.468**	-1.348**
	(-2.51)	(-2.38)	(-2.43)	(-2.33)	(-2.14)
Asia	0.073	0.241	0.120	0.165	0.270
	(0.12)	(0.40)	(0.20)	(0.27)	(0.44)
M.East–N.Africa	-0.514	-0.193	-0.435	-0.441	-0.268
	(-1.13)	(-0.44)	(-0.97)	(-0.97)	(-0.59)
Latin America	-1.165***	-1.154***	-1.149***	-1.118**	-1.062**
	(-2.68)	(-2.70)	(-2.65)	(-2.55)	(-2.43)
N	6701	6701	6701	6592	6592
Countries	153	153	153	151	151

Table 1: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Models are OLS with PCSE, adjusting for panel-level heteroskedasticity, panel-specific AR(1) autocorrelation, and contemporaneous correlation. Independent variables are lagged 5 years. Maximum time series is 1950–2008 on dependent variable, GDP per capita growth (in percentage). Decade dummies and constant omitted from table.

The point estimate in Model II resembles those of recent studies employing more comprehensive democracy measures (see, e.g. Gerring et al. 2005; Papaioannou and Siourounis 2008; Knutsen 2011*b*). While it has been noted that DD yields different estimates of democracy's effect on growth than, for example, Polity or Freedom House (Krieckhaus 2004; Cheibub, Gandhi and Vreeland 2010; Knutsen 2011*b*), the common interpretation is that this stems from the latter incorporating additional dimensions such as executive constraints or civil liberties protection. Hence, proponents of a minimalist democracy concept have had reason to consider the results in Przeworski et al. (2000) – suggesting little or no effect – to provide more reasonable estimates of democracy's impact on growth. Our results, however, suggest that the difference in estimates may rather stem from a bias generated by one particular coding rule of DD, and that even "minimalist

democracy" may increase growth.

4.2 Comparing DD with BMR

However, a simple recoding of DD as in Model II may count several competitive authoritarian regimes (Levitsky and Way 2010) as democratic; most researchers would likely not find it proper to count regimes with multi-party elections and, for example, large-scale election fraud as democratic. Thus it may be more proper to compare DD estimates with estimates based on the Boix, Miller and Rosato (BMR; 2013) measure. This measure is also minimalist and dichotomous, and although BMR includes a participation criterion – "[A] majority of adult men has the right to vote" (p.8) – this does not differentiate DD and BMR in the post-WWII era studied below. However, instead of coding contestation of elections solely through an alternation rule BMR requires that "[T]he legislature (or the executive if elected directly) is chosen in free and fair elections" (p.8). More specifically, Boix, Miller and Rosato (2013)

> rely primarily, but not exclusively, on the concept of electoral turnover emphasized in Przeworski et al. (2000). We take any instance of electoral executive turnover to an opposition party as a strong indicator of free and fair elections. However, the presence of electoral turnover is neither necessary nor sufficient ... Accordingly, we checked the history of those cases with no electoral turnover for a sufficiently long period of time (over two electoral terms) to examine whether internal coups, external interventions, abuses of state power, or reports of fraud could explain the prolonged control of the executive by the same party. If there were none and we observed contested elections, we coded the period as having free and fair elections. If a peaceful governmental turnover was observed, we applied the same check to determine how far back in time the condition of free and fair elections applied. (p. 1531)

Alternation of government is but *one* noisy signal of whether elections are contested or not, and by including different types of information we may arrive at more precise

judgements. Consequentially, BMR may be better at mitigating Type II errors (coding democracies as dictatorships) than DD.[4] We therefore compare BMR and DD with respect to their relationship with economic growth; if the above-hypothesized bias exists, BMR should produce a stronger positive correlation.

The DD coefficient in Model IV (6592 observations) is 0.2, and far from statistically significant (t=0.9). In contrast, Model V using BMR reports a democracy coefficient more than twice the size (0.4) on the same sample. Indeed, the latter coefficient is statistically significant at 5 percent (t=2.1). These results further indicate that models using DD might underestimate the impact of democracy on growth. We also re-ran Model IV using the original DD coding, but recoding (only) the 93 non-democratic regime-years considered democratic by BMR. This increases DD from 0.20 to 0.27, and the t-value from 0.93 to 1.33.

The differences reported above are not highly contingent on the specification choices made (see Appendix). Random- and fixed effects versions also report that the estimated effect of democracy increases substantially when going from Model I to II and from Model IV to V (although BMR is no longer statistically significant). Further, changing the lag-time on the independent variables to 1 or 3 years barely changes the results from Table 1. The overall picture is retained when adding controls such as population growth, log of oil and gas income or plurality religion dummies. The results are also kept in some, but not all, specifications (Heckman Treatment-Effects and System GMM models) entering democracy as an endogenous regressor.

[4]Interestingly, BMR may also avoid some Type I errors that DD does not; the turnover criterion may be *too lenient* for coding certain regimes as democratic. Some governments manipulating elections and trying to retain office by other illegitimate means still lose office after elections (e.g Levitsky and Way 2010). As Boix, Miller and Rosato (2013) notes, "Cheibub et al. (2010) code Kyrgyzstan as democratic for 2005-2006 following the electoral turnover in the aftermath of the Tulip Revolution. In contrast, we code this period as autocratic given the violence associated with the turnover, the oppressive rule of elected President Kurmanbek Bakiyev, and a 2009 election marred by fraud and state manipulation" (p. 1532). Sometimes, undemocratic intentions on the part of leaders are insufficient for avoiding turnover after elections.

4.3 Re-coding DD and comparing estimates using information available at different points in time

Although the above regressions provide some indications that DD's alternation rule generates a systematic bias in the estimated effect of democracy on growth, proponents of DD might suspect the results in, for example, Model V to be wrong because of the subjective judgements introduced in BMR. Hence, we employ a less controversial method for assessing the potential bias related to using DD: We *only* use DD's coding rules, the observable information we have today, and the historical information concerning government alternation we had at particular previous points in time. In addition to using the information on government alternations that Cheibub et al. had in 2010 – these are embedded in the original DD coding; remember that DD employs retrospective coding – we also construct "real-time DD scores" based on the historical information on transitions that was available in a given year (t). In other words, we use the coding rules of DD to code what the measure *would have* looked like had it been coded in, for instance, 1990, 1980 or 1970. Indeed, all democracies have at some point been Type II regimes; it takes time from competitive elections are planned and instituted until a country observes its first election-induced government alternation. The current Japanese regime was in place shortly after World War II, but the first alternation occurred only in 1993 when the LDP failed to retain its majority in the Diet Lower House elections. In post-war Western Germany it took until 1969 for the CDU-led government to cede power after an election loss to Willy Brandt and the SPD. Had DD been coded in 1968, both Japan and Western Germany would have been scored dictatorships.

At $t+1$ *some* of the regimes that were falsely coded as dictatorships at t are corrected because of observed government alternations. In 1994, for instance, we *knew* the post-war Japanese and German regimes were truly competitive, and these two Type II errors could then be corrected. Even better, the actual DD measure coded by Cheibub et al. is based on the historical information available in 2010; Type II errors identified by more recent alternations, such as in South Korea (1997) and Taiwan (2000), are thus accounted for. Therefore, we can assess the bias – or at least its direction – by running pairs of regression

on the exact same sample (e.g. 1946–1980) using "real time DD" *and* "original DD" as coded by Cheibub et al. This allows us to objectively and with certainty identify the impact of removing a number of Type II errors. If the above-hypothesized bias exists, we should observe a more positive/less negative effect on growth as we correct more Type II errors. In other words, regressions using real-time DD should provide far lower estimates of democracy's effect on growth than regressions using original DD. If the Type II error codings are uncorrelated with economic growth, however, the regressions will yield similar estimates of democracy's effect on growth.

	1946-70 Realtime DD *b/(t)*	1946-70 Original DD *b/(t)*	1946-80 Realtime DD *b/(t)*	1946-80 Original DD *b/(t)*	1946-90 Realtime DD *b/(t)*	1946-90 Original DD *b/(t)*
Democracy	-0.388	-0.216	-0.271	0.167	0.326	0.492*
	(-1.38)	(-0.65)	(-0.92)	(0.51)	(1.22)	(1.68)
Ln GDP p.c.	-0.194	-0.198	-0.785**	-0.836**	-0.468	-0.494*
	(-0.64)	(-0.64)	(-2.11)	(-2.20)	(-1.58)	(-1.68)
Ln population	-0.076	-0.071	-0.074	-0.081	-0.011	-0.018
	(-0.89)	(-0.85)	(-0.84)	(-0.95)	(-0.14)	(-0.24)
Ln regime dur.	-0.084	-0.079	-0.137	-0.143*	-0.150	-0.137
	(-0.81)	(-0.76)	(-1.57)	(-1.64)	(-1.55)	(-1.44)
Ethnic fraction.	-0.559	-0.495	-1.619****	-1.551***	-1.672****	-1.679****
	(-0.97)	(-0.84)	(-3.41)	(-3.25)	(-3.40)	(-3.42)
E.Eur. – ex-Sov.	0.375	0.484	-0.482	-0.187	-0.331	-0.143
	(0.59)	(0.77)	(-0.92)	(-0.35)	(-0.54)	(-0.23)
S.S. Africa	-2.849***	-2.726***	-3.613****	-3.435****	-2.354****	-2.214***
	(-2.97)	(-2.91)	(-4.14)	(-3.92)	(-3.31)	(-3.04)
Asia	-1.251*	-1.142	-1.887**	-1.726**	-0.876	-0.800
	(-1.73)	(-1.60)	(-2.42)	(-2.20)	(-1.24)	(-1.13)
M.East–N.Afr.	-0.464	-0.386	-1.394**	-1.182**	-1.090**	-0.927*
	(-0.82)	(-0.65)	(-2.40)	(-1.99)	(-2.02)	(-1.67)
Latin America	-1.818****	-1.664***	-2.486****	-2.345****	-1.616***	-1.609***
	(-3.37)	(-3.29)	(-4.61)	(-4.35)	(-3.27)	(-3.26)
N	2154	2154	3424	3424	4731	4731

Table 2: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Models are OLS with PCSE, adjusting for panel-level heteroskedasticity, panel-specific AR(1) autocorrelation, and contemporaneous correlation. Independent variables are lagged 5 years. Maximum time series is given in top row (for independent variables). Realtime DD is calculated using historical information on alternations available at last year of sample. Original DD coding is from Cheibub et al. (2010). Decade dummies and constant omitted from table.

We run growth regressions on time series going from 1946–t, using both original and real-time DD scores, letting t vary between 1965 (to ensure decent time-series) and 1995 (to ensure the recoding of a sufficient number of Type II regimes).[5] Table 2 presents these pairs of regression models for t equal to 1970, 1980 and 1990. As expected, the Cheibub

[5]We employ data from Adam Przeworski's PIPE dataset to identify starting dates of regimes that were democratic prior to 1946, and the first actual alternation observed in Type II regimes. For more recent regimes, we employ the coding criteria (passing three first DD rules) and data of Cheibub et al. to identify Type II regimes. More detailed information is provided in our online codebook.

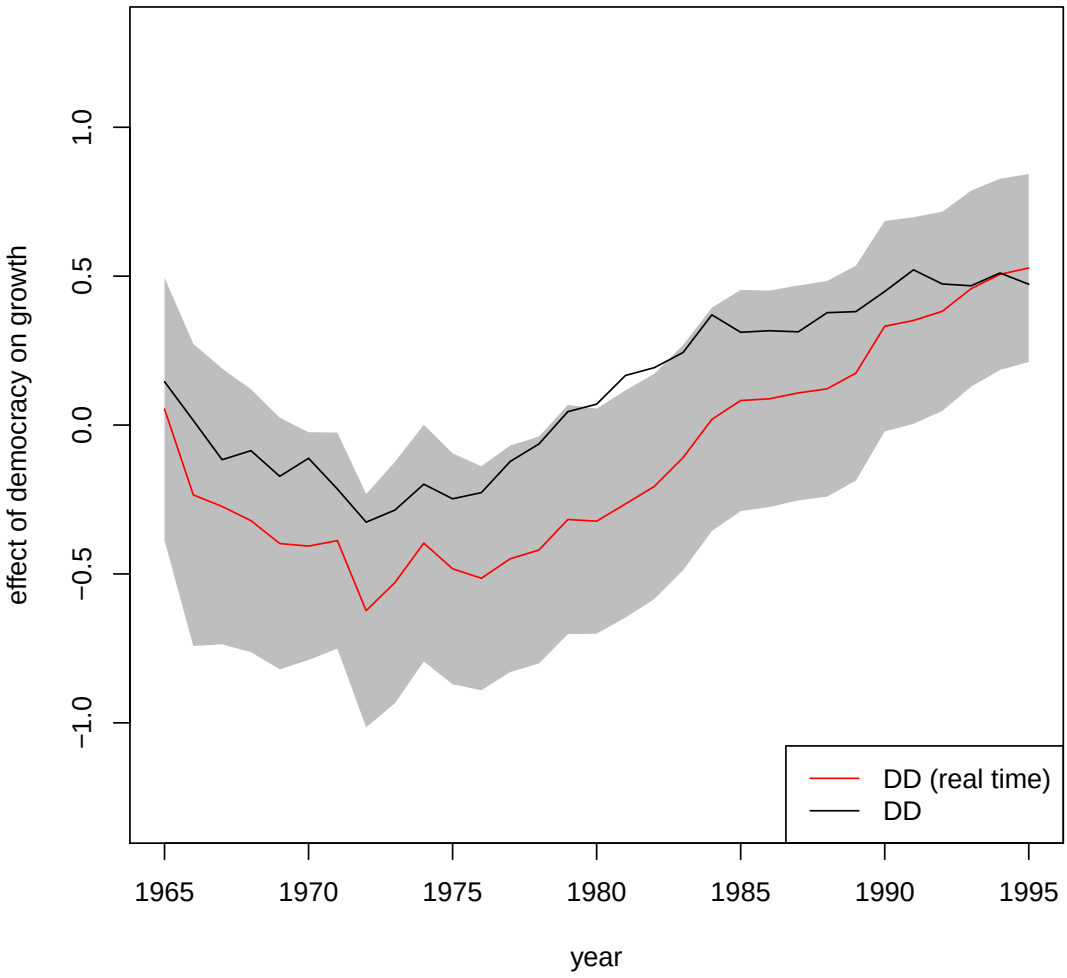

Figure 1: Estimated effect of democracy according to DD as coded by Cheibub et al. (2010) and to real-time DD coding (with 90 percent CIs) from OLS PCSE regressions, specified as in Table 2, for samples from 1946 to year given by x-axis.

et al. DD coding – which has removed some Type II errors because of the passage of time – provides higher point estimates of democracy's effect on growth than real-time DD for all three years. For 1980 the real-time DD coefficient is actually negative (-0.27) whereas the Cheibub et al. DD coefficient is positive (0.17); the difference in estimated effects – which can only be due to the removal of *some* Type II errors – almost totals half a percent GDP per capita growth. For 1990, both DD coefficients are positive, and the difference is smaller. Yet, the original DD yields a weakly significant effect on growth (t=1.68), whereas real-time coded DD does not (t=1.22), further illustrating that

the above-described bias may impact on our interpretations of democracy's economic consequences.

Figure 1 shows that the expected bias not only materializes for the years investigated in Table 2; the figure reports coefficient estimates for original and real-time DD for all years (i.e. for pairs of regressions run on samples from 1946 to that particular year). The Cheibub coded DD estimates – based on updated historical information on government alternation – systematically yields a higher estimated effect of democracy on growth than the estimates utilizing less historical information. Indeed, for a couple of years (mid-1980s) the original DD coefficient was outside the 90 percent confidence interval of the real-time DD coefficient. Thus, correcting (some of) the Type II errors the DD coding rules inevitably produce systematically leads to more "optimistic" results regarding how democracy affects growth. And, we should remember that other likely Type II errors, such as Botswana, have yet to be corrected in the Cheibub et al. coding.[6]

5 Does DD induce bias for other relationships?

Is the bias proposed here restricted to economic growth, or does it apply to other relationships? A resembling bias to that identified above may affect DD estimates concerning other causes and consequences of democracy, *if* the likelihood of Type II errors is correlated with these proposed causes and consequences. Empirically investigating whether such biases exist can be done by using similar tests to those above. To illustrate how this might be the case, we briefly discuss effects of democracy on civil war onset and coup d'etats, but these do likely not exhaust the list of relationships for which there *might* exist a bias induced by the alternation rule.

Cheibub, Gandhi and Vreeland (2010) argue that the estimated effect of regime type on the probability of civil war onset hinges on choice of democracy measure. They further

[6]Another interesting trend is also observable from the figure; democracy's estimated effect on growth varies over time, with the inclusion of more recent observations leading to a clearer positive effect. This is observed by Knutsen (2011*a*), who argues that increasing volumes of available foreign direct investment, changes in production structures – moving from physical capital- to human capital based economies – and changes in the international-political context – with more conditionalities placed on development loans and aid – may have contributed to democracy becoming increasingly beneficial for growth.

argue in favor of estimating such a relationship using DD. Yet, DD-based estimates of the relationship between democracy and civil war *might* be prone to the same kind of biases as estimates of the democracy–growth relationship. In particular, democratic systems where the prospects for government alternation are lower may be more conflict-prone, since groups expecting to be "permanent minorities" have stronger incentives to take up arms (see, e.g. Przeworski 1991; Cederman, Wimmer and Min 2010). Hence, DD may underestimate the propensity of civil war in democracies, since the possibly most conflict-prone democratic systems – those which have yet to observe government alternation after elections – will be falsely coded as dictatorships. A similar argument can be applied to coups. In the countries where – in spite of free and fair elections – being a permanent minority is more likely, opponents of the regime might have much stronger incentives to engage in the risky business of staging a coup. In sum, when there is a lack of prospects for turnovers through election, groups not occupying power may use extra-constitutional means to obtain the kind of government they like. This may lead to Type II regimes containing both autocracies *and* those democracies that are particularly coup- and civil-war prone. This, in turn, indicates that DD may exaggerate the extent to which democracies are peaceful and stable polities. In both these cases, we would expect that; we get a less pacifying effect of democrcy as we correct type II errors, that democracies where type II is considered democratic will be less peaceful, and that democracies when coded with the Boix et.al coding will be less peacefull.

We explored these arguments by conducting similar tests as those presented for growth in table 2 above; We first compare original DD results with results from using the real-time DD measure (see Appendix), to see whether the correction of type II errors has a systematic effect on the coefficient of democracy, before comparing the original DD measure to a measure where all type II cases are coded as democratic and the Boix et.al measure. For the regime type–civil war relationship, we employ and adjust (e.g. add ethnic fractionalization and economic growth as controls) the main model in Cederman, Hug and Krebs (2010). However – despite the intriguing theoretical argument – we do not find strong and systematic evidence of the expected bias when using DD to estimate

the impact of democracy on civil war onsets.

Regarding coup d'etats, we employ the *coup attempts* models in Powell (2012) as point of departure. Here, we find that real time DD is negatively and strongly significantly linked to coup risk (see appendix), while the restrospectively coded original DD has a weaker negative effect. This is in accordance with our expectation presented above; we seem to modify the estimated effect democracy has on reducing coup-risk as we correct Type II errors. One plausible interpretation is that where the incumbent's willingness to cede power after an election defeat is fairly certain and recently proven, regime opponents might find electoral competition to be preferable to the risky business of staging a coup. However, we do not find strong support for our other expectations. These two applications illustrate how the investigation of this bias can be done in other settings than economic growth.

6 Conclusion

Using subjective indicators to code democracy may generate various problems; democracy measures such as Polity and Freedom House are, for example, likely associated with substantial unsystematic measurement errors. Hence, Alvarez et al. (1996), Przeworski et al. (2000) and Cheibub, Gandhi and Vreeland (2010) put a high prize on objective coding rules, ensuring that their DD measure scores high in terms of inter-coder reliability. However, there are other types of measurement errors; all coders may be *systematically* wrong when in unison following objective rules that use limited information. Although the DD measure is rightly praised for its beneficial properties (e.g. Munck and Verkuilen 2002; Clark, Golder and Golder 2012), we show above that its alternation rule for coding contested elections leads to some serious problems. As has long been known (see Alvarez et al. 1996), a number of democracies are likely falsely coded as dictatorships by DD. Hence, we should expect some attenuation bias – meaning that coefficients are drawn towards zero – when using DD to, for instance, estimate the impact of democracy on economic growth or civil war onset; we are comparing a subset of the world's democracies

to a group of regimes consisting of both democracies and dictatorships.

Yet, when it comes to economic growth, we argue that the bias may actually be more severe than what we would expect from simply misclassifying random democracies as dictatorships. There is good reason to expect that the misclassified democracies are often (young) democracies with above-normal economic performance: If voters systematically reward competent governments, and punish governments that fail to perform, governments in high-growing democracies are less likely to face election losses. This, in turn, disallows the regime in question to pass the DD measure's alternation rule. As a consequence, we may actually be comparing old democracies *and* young democracies with low growth, on the one hand, with dictatorships *and* young democracies with high growth, on the other, when using DD to estimate the effect of democracy on growth. Above, we provided strong evidence showing that this is not only a theoretical possibility; using different empirical tests, we identify a bias in regressions using DD. A side-implication of this is that we should put less confidence in widely cited studies based on the DD (notably Przeworski et al. 2000) reporting that democracy does not matter for economic growth, and more confidence in results based on other measures, which often find that democracy, on net, increases growth (for a review, see Knutsen 2012).

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A Data Appendix

	Mean	Median	Std. dev.	N
Autocracy as coded by DD	1.81	1.91	6.53	5134
Indisputable autorcacies	1.54	1.71	6.68	3990
Type 2 regimes	2.99	2.77	5.72	1144
Democracy as coded by DD	2.52	2.58	3.64	4003
Democracy + Type 2 regimes	2.63	2.62	4.21	5147
Autocracy, Boix et al.	1.76	1.87	6.48	4891
Democracy, Boix et al.	2.55	2.60	3.71	3659

Table A.1: Summary statistics for GDP per capita growth (in percent) over regime type.

	Model I Baseline DD	Model II Re-coded DD: Type 2 democratic	Model III Re-coded DD: Botswana democratic	Model IV Baseline DD on Boix et al. sample	Model V Boix et al. regime measure
	b/(t)	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy	0.049	0.662**	0.125	0.085	0.217
	(0.19)	(2.54)	(0.46)	(0.33)	(0.83)
Ln GDP p.c.	-1.382****	-1.463****	-1.397****	-1.365****	-1.386****
	(-4.62)	(-4.96)	(-4.63)	(-4.53)	(-4.55)
Ln population	-0.188	-0.203	-0.189	-0.182	-0.185
	(-1.32)	(-1.42)	(-1.32)	(-1.28)	(-1.29)
Ln regime dur.	0.030	0.043	0.034	0.038	0.043
	(0.31)	(0.43)	(0.34)	(0.38)	(0.42)
Ethnic fract.	-1.986**	-1.882**	-1.973**	-1.969**	-1.960**
	(-2.28)	(-2.24)	(-2.26)	(-2.25)	(-2.25)
E.Eur–Soviet	0.373	0.599	0.408	0.381	0.453
	(0.60)	(0.96)	(0.66)	(0.61)	(0.73)
S.S. Africa	-4.154****	-3.994****	-4.131****	-4.050****	-3.995****
	(-4.30)	(-4.07)	(-4.28)	(-4.17)	(-4.12)
Asia	-1.590*	-1.391	-1.567*	-1.549*	-1.496*
	(-1.86)	(-1.61)	(-1.83)	(-1.79)	(-1.74)
M.East–N.Afr.	-1.848****	-1.484***	-1.801****	-1.792****	-1.698****
	(-3.59)	(-2.82)	(-3.53)	(-3.47)	(-3.40)
Latin America	-2.502****	-2.457****	-2.491****	-2.457****	-2.426****
	(-4.86)	(-4.68)	(-4.85)	(-4.76)	(-4.73)
1940s	-2.557****	-2.468****	-2.552****	-2.508****	-2.478****
	(-4.55)	(-4.35)	(-4.54)	(-4.42)	(-4.37)
1950s	-2.290****	-2.185****	-2.282****	-2.238****	-2.219****
	(-5.51)	(-5.25)	(-5.50)	(-5.25)	(-5.23)
1960s	-1.373****	-1.221***	-1.361****	-1.320***	-1.299***
	(-3.40)	(-3.00)	(-3.36)	(-3.22)	(-3.16)
1970s	-2.949****	-2.744****	-2.930****	-2.918****	-2.889****
	(-8.40)	(-7.75)	(-8.31)	(-8.18)	(-8.12)
1980s	-3.478****	-3.312****	-3.463****	-3.456****	-3.434****
	(-9.37)	(-8.94)	(-9.30)	(-9.24)	(-9.23)
1990s	-1.579****	-1.550****	-1.577****	-1.594****	-1.589****
	(-6.22)	(-6.05)	(-6.21)	(-6.17)	(-6.14)
Constant	21.043****	21.275****	21.103****	20.703****	20.780****
	(5.35)	(5.43)	(5.36)	(5.25)	(5.26)
N	6701	6701	6701	6592	6592

Table A.2: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All models are random effects models with errors clustered on country. Independent variables are lagged with 5 years. Maximum length of time series is 1950–2008 on dependent variable, GDP per capita growth (in percentage).

	Model I Baseline DD	Model II Re-coded DD: Type 2 democratic	Model III Re-coded DD: Botswana democratic	Model IV Baseline DD on Boix et al. sample	Model V Boix et al. regime measure
	b/(t)	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy	0.000	0.490*	0.000	0.025	0.113
	(0.00)	(1.86)	(0.00)	(0.10)	(0.47)
Ln GDP p.c.	-3.336****	-3.375****	-3.336****	-3.353****	-3.362****
	(-6.29)	(-6.41)	(-6.29)	(-6.21)	(-6.21)
Ln population	-1.342*	-1.388*	-1.342*	-1.439*	-1.440*
	(-1.70)	(-1.76)	(-1.70)	(-1.81)	(-1.81)
Ln regime dur.	0.134	0.146	0.134	0.146	0.149
	(1.29)	(1.40)	(1.29)	(1.39)	(1.41)
1940s	-5.588****	-5.525****	-5.588****	-5.661****	-5.637****
	(-4.71)	(-4.65)	(-4.71)	(-4.72)	(-4.72)
1950s	-4.869****	-4.798****	-4.869****	-4.932****	-4.914****
	(-5.47)	(-5.36)	(-5.47)	(-5.48)	(-5.47)
1960s	-3.053****	-2.950****	-3.053****	-3.075****	-3.058****
	(-3.99)	(-3.83)	(-3.99)	(-3.98)	(-3.97)
1970s	-3.940****	-3.797****	-3.940****	-3.961****	-3.940****
	(-7.23)	(-6.91)	(-7.23)	(-7.21)	(-7.21)
1980s	-4.078****	-3.962****	-4.078****	-4.078****	-4.063****
	(-8.66)	(-8.44)	(-8.66)	(-8.62)	(-8.66)
1990s	-1.895****	-1.873****	-1.895****	-1.913****	-1.909****
	(-6.70)	(-6.55)	(-6.70)	(-6.68)	(-6.67)
Constant	52.699****	53.406****	52.699****	54.387****	54.416****
	(3.41)	(3.46)	(3.41)	(3.48)	(3.48)
N	6701	6701	6701	6592	6592

Table A.3: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All models are fixed effects models with errors clustered on country. Independent variables are lagged with 5 years. Maximum length of time series is 1950–2008 on dependent variable, GDP per capita growth (in percentage).

	Model I Baseline DD	Model II Re-coded DD: Type 2 democratic	Model III Re-coded DD: Botswana democratic	Model IV Baseline DD on Boix et al. sample	Model V Boix et al. regime measure
	b/(t)	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy	0.115	0.614***	0.202	0.172	0.403**
	(0.54)	(2.95)	(0.97)	(0.83)	(2.08)
Ln GDP p.c.	-0.262	-0.320	-0.271	-0.253	-0.277
	(-1.02)	(-1.26)	(-1.06)	(-0.99)	(-1.10)
Ln population	0.059	0.041	0.055	0.066	0.060
	(0.93)	(0.63)	(0.87)	(1.00)	(0.90)
Ln regime dur.	0.164*	0.164*	0.166*	0.171*	0.171*
	(1.84)	(1.84)	(1.85)	(1.89)	(1.88)
Ethnic fract.	-1.844****	-1.794****	-1.830****	-1.891****	-1.882****
	(-4.84)	(-4.62)	(-4.81)	(-4.89)	(-4.86)
E.Eur–Soviet	0.360	0.590	0.407	0.350	0.485
	(0.56)	(0.91)	(0.64)	(0.54)	(0.77)
S.S. Africa	-1.012	-0.898	-0.966	-0.887	-0.766
	(-1.53)	(-1.38)	(-1.46)	(-1.34)	(-1.16)
Asia	0.456	0.609	0.493	0.514	0.611
	(0.75)	(1.02)	(0.81)	(0.86)	(1.02)
M.East–N.Afr.	-0.067	0.219	-0.006	0.032	0.201
	(-0.15)	(0.49)	(-0.01)	(0.07)	(0.43)
Latin America	-0.892**	-0.882*	-0.877*	-0.844*	-0.786*
	(-1.96)	(-1.95)	(-1.93)	(-1.86)	(-1.74)
1940s	0.824	0.910	0.841	0.918	0.981
	(1.04)	(1.17)	(1.06)	(1.16)	(1.24)
1950s	-0.351	-0.239	-0.331	-0.208	-0.161
	(-0.57)	(-0.40)	(-0.54)	(-0.34)	(-0.26)
1960s	-0.130	0.035	-0.109	-0.072	-0.024
	(-0.24)	(0.07)	(-0.20)	(-0.13)	(-0.04)
1970s	-0.823	-0.608	-0.796	-0.736	-0.682
	(-1.54)	(-1.16)	(-1.49)	(-1.38)	(-1.28)
1980s	-2.839****	-2.652****	-2.819****	-2.799****	-2.758****
	(-5.40)	(-5.13)	(-5.36)	(-5.31)	(-5.24)
1990s	-1.872****	-1.829***	-1.866****	-1.854****	-1.844***
	(-3.30)	(-3.27)	(-3.29)	(-3.30)	(-3.28)
Constant	4.833*	5.051*	4.866*	4.498*	4.567*
	(1.85)	(1.93)	(1.87)	(1.73)	(1.76)
N	7307	7307	7307	7182	7182

Table A.4: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All models are OLS with PCSE, adjusting for panel-level heteroskedasticity, panel-specific AR(1) autocorrelation, and contemporaneous correlation. Independent variables are lagged with 1 year. Maximum length of time series is 1950–2008 on dependent variable, GDP per capita growth (in percentage).

	Model I Baseline DD *b/(t)*	Model II Re-coded DD: Type 2 democratic *b/(t)*	Model III Re-coded DD: Botswana democratic *b/(t)*	Model IV Baseline DD on Boix et al. sample *b/(t)*	Model V Boix et al. regime measure *b/(t)*
Democracy	0.223	0.629***	0.324	0.270	0.407**
	(1.07)	(3.27)	(1.57)	(1.31)	(2.11)
Ln GDP p.c.	-0.320	-0.373	-0.331	-0.296	-0.310
	(-1.26)	(-1.49)	(-1.30)	(-1.17)	(-1.23)
Ln population	0.052	0.035	0.047	0.062	0.059
	(0.90)	(0.60)	(0.81)	(1.03)	(0.96)
Ln regime dur.	0.002	0.001	0.003	0.006	0.005
	(0.02)	(0.01)	(0.04)	(0.07)	(0.06)
Ethnic fract.	-1.750****	-1.704****	-1.731****	-1.760****	-1.764****
	(-4.72)	(-4.59)	(-4.68)	(-4.63)	(-4.63)
E.Eur–Soviet	0.520	0.705	0.578	0.477	0.560
	(0.92)	(1.26)	(1.04)	(0.85)	(1.00)
S.S. Africa	-1.443**	-1.384**	-1.393**	-1.312**	-1.237**
	(-2.31)	(-2.30)	(-2.23)	(-2.09)	(-1.98)
Asia	0.137	0.249	0.182	0.204	0.269
	(0.22)	(0.41)	(0.29)	(0.33)	(0.43)
M.East–N.Afr.	-0.385	-0.154	-0.313	-0.294	-0.184
	(-0.86)	(-0.36)	(-0.70)	(-0.65)	(-0.41)
Latin America	-1.172***	-1.184***	-1.156***	-1.115**	-1.075**
	(-2.63)	(-2.70)	(-2.60)	(-2.50)	(-2.42)
1940s	-1.284*	-1.198	-1.263	-1.217	-1.165
	(-1.66)	(-1.58)	(-1.64)	(-1.59)	(-1.52)
1950s	-0.962	-0.869	-0.940	-0.892	-0.860
	(-1.56)	(-1.44)	(-1.53)	(-1.45)	(-1.40)
1960s	-0.792	-0.650	-0.770	-0.757	-0.728
	(-1.43)	(-1.20)	(-1.39)	(-1.38)	(-1.33)
1970s	-1.759***	-1.581***	-1.731***	-1.717***	-1.691***
	(-3.21)	(-2.95)	(-3.16)	(-3.16)	(-3.12)
1980s	-3.240****	-3.087****	-3.219****	-3.206****	-3.190****
	(-5.97)	(-5.80)	(-5.93)	(-5.93)	(-5.92)
1990s	-1.893****	-1.859****	-1.885****	-1.919****	-1.915****
	(-3.43)	(-3.42)	(-3.42)	(-3.52)	(-3.52)
Constant	6.485***	6.746***	6.546***	6.016**	6.047**
	(2.58)	(2.72)	(2.60)	(2.42)	(2.44)
N	7002	7002	7002	6885	6885

Table A.5: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All models are OLS with PCSE, adjusting for panel-level heteroskedasticity, panel-specific AR(1) autocorrelation, and contemporaneous correlation. Independent variables are lagged with 3 years. Maximum length of time series is 1950–2008 on dependent variable, GDP per capita growth (in percentage).

	Model I Baseline DD	Model II Re-coded DD: Type 2 democratic	Model III Re-coded DD: Botswana democratic	Model IV Baseline DD on Boix et al. sample	Model V Boix et al. regime measure
	b/(t)	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy	0.154	0.687****	0.305	0.205	0.481**
	(0.70)	(3.44)	(1.41)	(0.93)	(2.34)
Population growth	-4.661	-4.811	-4.656	-4.097	-4.137
	(-0.57)	(-0.59)	(-0.56)	(-0.49)	(-0.50)
Ln GDP p.c.	-0.286	-0.356	-0.304	-0.271	-0.302
	(-1.12)	(-1.44)	(-1.19)	(-1.06)	(-1.20)
Ln population	0.047	0.023	0.040	0.050	0.041
	(0.72)	(0.35)	(0.61)	(0.77)	(0.63)
Ln regime dur.	-0.020	-0.021	-0.018	-0.007	-0.006
	(-0.22)	(-0.24)	(-0.20)	(-0.08)	(-0.06)
Ethnic fract.	-1.396****	-1.333***	-1.366***	-1.404***	-1.385***
	(-3.30)	(-3.15)	(-3.24)	(-3.28)	(-3.23)
E.Eur–Soviet	1.128**	1.354***	1.206**	1.084**	1.237**
	(2.23)	(2.69)	(2.41)	(2.13)	(2.42)
S.S. Africa	-1.439**	-1.321**	-1.366**	-1.338**	-1.194*
	(-2.28)	(-2.15)	(-2.17)	(-2.10)	(-1.87)
Asia	0.104	0.270	0.167	0.180	0.304
	(0.16)	(0.43)	(0.26)	(0.28)	(0.47)
M.East–N.Afr.	-0.365	-0.054	-0.261	-0.297	-0.089
	(-0.76)	(-0.11)	(-0.55)	(-0.61)	(-0.18)
Latin America	-1.027**	-1.016**	-1.001**	-0.978**	-0.911**
	(-2.26)	(-2.26)	(-2.21)	(-2.13)	(-1.99)
1940s	-1.001	-0.921	-0.974	-0.947	-0.869
	(-1.20)	(-1.11)	(-1.17)	(-1.12)	(-1.03)
1950s	-0.733	-0.627	-0.705	-0.687	-0.625
	(-1.14)	(-0.99)	(-1.10)	(-1.05)	(-0.95)
1960s	-0.592	-0.427	-0.562	-0.538	-0.479
	(-1.01)	(-0.73)	(-0.96)	(-0.89)	(-0.80)
1970s	-2.310****	-2.103****	-2.273****	-2.281****	-2.221****
	(-4.00)	(-3.66)	(-3.93)	(-3.86)	(-3.76)
1980s	-3.013****	-2.839****	-2.984****	-2.990****	-2.947****
	(-5.23)	(-4.95)	(-5.18)	(-5.09)	(-5.01)
1990s	-1.017*	-0.978*	-1.007*	-1.031*	-1.017*
	(-1.79)	(-1.73)	(-1.78)	(-1.78)	(-1.76)
Constant	6.118***	6.498***	6.213***	5.801**	5.900***
	(2.66)	(2.90)	(2.70)	(2.54)	(2.60)
N	6567	6567	6567	6461	6461

Table A.6: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All models are OLS with PCSE, adjusting for panel-level heteroskedasticity, panel-specific AR(1) autocorrelation, and contemporaneous correlation. Independent variables are lagged with 5 years. Maximum length of time series is 1950–2008 on dependent variable, GDP per capita growth (in percentage).

	Model I Baseline DD	Model II Re-coded DD: Type 2 democratic	Model III Re-coded DD: Botswana democratic	Model IV Baseline DD on Boix et al. sample	Model V Boix et al. regime measure
	b/(t)	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy	0.117	0.622***	0.246	0.163	0.347
	(0.53)	(3.07)	(1.14)	(0.74)	(1.63)
Ln oil/gas income	-0.025	-0.008	-0.021	-0.021	-0.015
	(-0.47)	(-0.16)	(-0.40)	(-0.38)	(-0.28)
Ln GDP p.c.	-0.279	-0.367	-0.302	-0.272	-0.301
	(-0.99)	(-1.35)	(-1.08)	(-0.98)	(-1.10)
Ln population	0.043	0.019	0.035	0.044	0.037
	(0.62)	(0.26)	(0.51)	(0.63)	(0.52)
Ln regime dur.	-0.064	-0.068	-0.062	-0.046	-0.046
	(-0.65)	(-0.70)	(-0.64)	(-0.46)	(-0.46)
Ethnic fract.	-2.138****	-2.130****	-2.118****	-2.182****	-2.177****
	(-4.70)	(-4.76)	(-4.67)	(-4.70)	(-4.69)
E.Eur–Soviet	0.849	1.012**	0.902*	0.793	0.882*
	(1.63)	(1.97)	(1.75)	(1.53)	(1.69)
S.S. Africa	-1.374**	-1.263**	-1.321**	-1.247*	-1.161*
	(-2.10)	(-1.99)	(-2.03)	(-1.89)	(-1.76)
Asia	0.224	0.376	0.267	0.293	0.363
	(0.35)	(0.60)	(0.42)	(0.46)	(0.56)
M.East–N.Afr.	-0.447	-0.195	-0.371	-0.372	-0.253
	(-0.95)	(-0.43)	(-0.79)	(-0.78)	(-0.53)
Latin America	-0.920**	-0.932**	-0.909**	-0.855*	-0.821*
	(-2.00)	(-2.06)	(-1.98)	(-1.84)	(-1.77)
1940s	-1.455	-1.359	-1.439	-1.401	-1.333
	(-1.62)	(-1.53)	(-1.61)	(-1.55)	(-1.47)
1950s	-1.096	-0.970	-1.070	-1.052	-1.012
	(-1.50)	(-1.34)	(-1.47)	(-1.42)	(-1.37)
1960s	-0.691	-0.523	-0.662	-0.633	-0.589
	(-1.16)	(-0.88)	(-1.11)	(-1.03)	(-0.96)
1970s	-2.366****	-2.169****	-2.334****	-2.339****	-2.300****
	(-4.04)	(-3.73)	(-3.99)	(-3.91)	(-3.84)
1980s	-3.080****	-2.913****	-3.055****	-3.062****	-3.034****
	(-5.27)	(-5.01)	(-5.23)	(-5.13)	(-5.08)
1990s	-1.074*	-1.031*	-1.064*	-1.086*	-1.074*
	(-1.87)	(-1.81)	(-1.85)	(-1.86)	(-1.83)
Constant	6.633**	7.197***	6.801**	6.406**	6.562**
	(2.48)	(2.76)	(2.54)	(2.43)	(2.51)
N	6190	6190	6190	6098	6098

Table A.7: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All models are OLS with PCSE, adjusting for panel-level heteroskedasticity, panel-specific AR(1) autocorrelation, and contemporaneous correlation. Independent variables are lagged with 5 years. Maximum length of time series is 1950–2008 on dependent variable, GDP per capita growth (in percentage).

	Model I Baseline DD	Model II Re-coded DD: Type 2 democratic	Model III Re-coded DD: Botswana democratic	Model IV Baseline DD on Boix et al. sample	Model V Boix et al. regime measure
	b/(t)	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy	0.253	0.756****	0.369*	0.307	0.501**
	(1.16)	(3.93)	(1.71)	(1.40)	(2.47)
Sunni	-1.244**	-1.005*	-1.146*	-1.161*	-0.998
	(-1.98)	(-1.72)	(-1.83)	(-1.83)	(-1.58)
Shia	-2.133**	-1.860**	-2.044**	-2.076**	-1.879**
	(-2.50)	(-2.21)	(-2.40)	(-2.43)	(-2.16)
Catholic	-0.355	-0.006	-0.255	-0.191	-0.000
	(-0.50)	(-0.01)	(-0.36)	(-0.26)	(-0.00)
Protestant	-0.918	-0.633	-0.823	-0.784	-0.603
	(-1.44)	(-1.05)	(-1.29)	(-1.19)	(-0.92)
Orthodox	0.054	0.409	0.160	0.333	0.509
	(0.07)	(0.55)	(0.21)	(0.41)	(0.64)
Hindu	-0.442	-0.333	-0.396	-0.358	-0.263
	(-0.51)	(-0.40)	(-0.46)	(-0.40)	(-0.30)
Buddhist+	0.994	1.245	1.115	1.074	1.279
	(1.20)	(1.58)	(1.35)	(1.26)	(1.48)
Indigenous	-2.256****	-1.993****	-2.171****	-2.136****	-1.961***
	(-3.70)	(-3.39)	(-3.58)	(-3.43)	(-3.14)
Ln GDP p.c.	-0.486*	-0.565**	-0.500*	-0.476*	-0.498*
	(-1.84)	(-2.19)	(-1.91)	(-1.81)	(-1.92)
Ln population	0.009	-0.010	0.004	0.008	0.004
	(0.14)	(-0.16)	(0.06)	(0.13)	(0.06)
Ln regime dur.	-0.032	-0.028	-0.029	-0.013	-0.012
	(-0.37)	(-0.32)	(-0.33)	(-0.15)	(-0.13)
Ethnic fract.	-0.874**	-0.807**	-0.846**	-0.916**	-0.898**
	(-2.27)	(-2.08)	(-2.21)	(-2.36)	(-2.31)
E.Eur–Soviet	0.534	0.723	0.592	0.469	0.592
	(1.12)	(1.54)	(1.25)	(0.98)	(1.22)
S.S. Africa	-1.628**	-1.527**	-1.572**	-1.462**	-1.354**
	(-2.52)	(-2.41)	(-2.43)	(-2.21)	(-2.05)
Asia	-0.891	-0.715	-0.848	-0.751	-0.653
	(-1.14)	(-0.90)	(-1.08)	(-0.90)	(-0.79)
M.East–N.Afr.	0.033	0.371	0.117	0.205	0.373
	(0.07)	(0.74)	(0.23)	(0.39)	(0.71)
Latin America	-1.707****	-1.757****	-1.691****	-1.648****	-1.605****
	(-3.59)	(-3.78)	(-3.57)	(-3.47)	(-3.38)
1940s	-1.067	-0.994	-1.047	-1.007	-0.938
	(-1.37)	(-1.29)	(-1.34)	(-1.28)	(-1.19)
1950s	-1.058*	-0.959	-1.037*	-1.003	-0.957
	(-1.68)	(-1.54)	(-1.65)	(-1.55)	(-1.48)
1960s	-0.758	-0.590	-0.734	-0.700	-0.657
	(-1.32)	(-1.03)	(-1.28)	(-1.19)	(-1.12)
1970s	-2.341****	-2.127****	-2.311****	-2.304****	-2.263****
	(-4.14)	(-3.79)	(-4.09)	(-4.00)	(-3.93)
1980s	-3.067****	-2.882****	-3.043****	-3.039****	-3.010****
	(-5.45)	(-5.16)	(-5.41)	(-5.30)	(-5.25)
1990s	-1.086*	-1.044*	-1.078*	-1.091*	-1.081*
	(-1.95)	(-1.89)	(-1.94)	(-1.93)	(-1.91)
Constant	9.121****	9.221****	9.092****	8.738****	8.598****
	(3.55)	(3.66)	(3.54)	(3.40)	(3.35)
N	6701	6701	6701	6592	6592

Table A.8: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All models are OLS with PCSE, adjusting for panel-level heteroskedasticity, panel-specific AR(1) autocorrelation, and contemporaneous correlation. Independent variables are lagged with 5 years. Maximum length of time series is 1950–2008 on dependent variable, GDP per capita growth (in percentage).

	Model I Baseline DD	Model II Re-coded DD: Type 2 democratic	Model III Re-coded DD: Botswana democratic	Model IV Baseline DD on Boix et al. sample	Model V Boix et al. regime measure
	b/(t)	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy	1.982	1.210*	-0.049	2.103	0.898
	(0.44)	(1.72)	(-0.03)	(0.53)	(0.15)
Ln GDP p.c.	-0.429	-0.360	-0.145	-0.438	-0.277
	(-0.62)	(-1.45)	(-0.42)	(-0.70)	(-0.30)
Ln population	-0.065	-0.065	-0.032	-0.063	-0.045
	(-0.44)	(-0.59)	(-0.31)	(-0.44)	(-0.32)
Ln regime dur.	-0.032	-0.072	-0.084	-0.020	-0.063
	(-0.21)	(-0.79)	(-0.84)	(-0.14)	(-0.49)
Ethnic fract.	-1.483*	-1.510**	-1.715**	-1.473*	-1.630*
	(-1.85)	(-2.36)	(-2.29)	(-1.92)	(-1.79)
E.Eur–Soviet	1.992	1.410**	0.915	2.020	1.393
	(0.83)	(2.55)	(0.96)	(0.96)	(0.44)
S.S. Africa	-0.419	-1.171	-1.382	-0.321	-0.943
	(-0.19)	(-1.63)	(-1.57)	(-0.16)	(-0.38)
Asia	1.154	0.647	0.367	1.258	0.775
	(0.62)	(0.95)	(0.45)	(0.73)	(0.34)
M.East–N.Afr.	0.650	-0.021	-0.687	0.750	-0.034
	(0.22)	(-0.04)	(-0.60)	(0.28)	(-0.01)
Latin America	-0.761	-1.150***	-1.189***	-0.710	-0.966
	(-0.80)	(-3.07)	(-2.78)	(-0.81)	(-0.78)
1940s	-1.214*	-1.324***	-1.443***	-1.176*	-1.271
	(-1.81)	(-2.68)	(-2.80)	(-1.79)	(-1.17)
1950s	-1.101	-1.209***	-1.385***	-1.043	-1.238
	(-1.45)	(-2.95)	(-3.03)	(-1.38)	(-1.32)
1960s	-0.588	-0.657*	-0.949**	-0.544	-0.773
	(-0.72)	(-1.69)	(-2.10)	(-0.72)	(-0.76)
1970s	-2.271*	-2.376****	-2.801****	-2.230**	-2.572*
	(-1.92)	(-5.94)	(-5.14)	(-2.08)	(-1.79)
1980s	-3.007***	-3.071****	-3.427****	-2.977****	-3.249***
	(-3.23)	(-7.73)	(-6.78)	(-3.52)	(-2.82)
1990s	-1.410****	-1.426****	-1.479****	-1.428****	-1.471****
	(-4.90)	(-5.57)	(-5.78)	(-4.90)	(-4.79)
Constant	7.853**	8.150***	7.138***	7.718**	7.437*
	(2.29)	(3.15)	(2.74)	(2.36)	(1.92)
N	6673	6673	6673	6592	6592

Table A.9: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All models are Heckman-type Treatment-Effects model. Independent variables are lagged with 5 years. Democracy is considered endogenous, and WAVE from Knutsen (2011*b*) – capturing whether the regime originated within one of Huntington's (1991) reverse waves of democratization or not – is used as instrument in the first stage. Maximum length of time series is 1950–2008 on dependent variable, GDP per capita growth (in percentage).

	Model I Baseline DD	Model II Re-coded DD: Type 2 democratic	Model III Re-coded DD: Botswana democratic	Model IV Baseline DD on Boix et al. sample	Model V Boix et al. regime measure
	b/(t)	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy	-0.064	0.547	-0.089	-0.064	1.469**
	(-0.11)	(1.31)	(-0.30)	(-0.11)	(2.11)
Ln GDP p.c.	-2.621****	-2.629****	-2.860****	-2.621****	-2.698****
	(-13.80)	(-17.15)	(-19.45)	(-13.80)	(-11.72)
Ln population	-0.371*	-0.362**	-0.091	-0.371*	-0.224
	(-1.69)	(-2.28)	(-0.55)	(-1.69)	(-1.33)
Ln regime dur.	0.217****	0.104	0.306****	0.217****	0.083
	(3.71)	(0.90)	(3.32)	(3.71)	(0.86)
Ethnic fract.	3.851	-1.891	2.216	3.851	-4.306*
	(1.46)	(-1.45)	(1.13)	(1.46)	(-1.94)
E.Eur–Soviet	-4.349****	-2.844**	-3.235****	-4.349****	-1.414
	(-4.03)	(-2.00)	(-4.45)	(-4.03)	(-0.84)
S.S. Africa	-9.231****	-5.522****	-7.346****	-9.231****	-2.212
	(-4.26)	(-5.56)	(-3.49)	(-4.26)	(-0.99)
Asia	-4.605****	-1.181	-4.020***	-4.605****	-1.425
	(-3.54)	(-1.02)	(-2.98)	(-3.54)	(-0.72)
M.East–N.Afr.	-5.739****	-3.511****	-4.566**	-5.739****	-0.828
	(-3.49)	(-3.66)	(-2.54)	(-3.49)	(-0.40)
Latin America	-3.358***	-1.567	-2.399*	-3.358***	-0.027
	(-3.12)	(-1.55)	(-1.73)	(-3.12)	(-0.02)
1940s	8.297	0.260	-4.961****	8.297	-3.824****
	(0.81)	(0.02)	(-4.43)	(0.81)	(-10.27)
1950s	-2.493****	-2.658****	-4.102****	-2.493****	-2.117****
	(-10.08)	(-9.66)	(-4.11)	(-10.08)	(-3.98)
1960s	-1.393****	-1.331****	-1.409****	-1.393****	-1.001**
	(-7.38)	(-6.27)	(-4.86)	(-7.38)	(-2.33)
1970s	-2.769****	-2.526****	-2.728****	-2.769****	-2.235****
	(-18.05)	(-17.13)	(-16.33)	(-18.05)	(-5.61)
1980s	-2.908****	-2.648****	-2.987****	-2.908****	-2.337****
	(-22.35)	(-18.74)	(-24.41)	(-22.35)	(-5.50)
1990s	-1.268****	-1.176****	-1.228****	-1.268****	-1.059****
	(-13.12)	(-20.36)	(-15.74)	(-13.12)	(-9.15)
Lag dep. var.	0.109****	0.113****	0.108****	0.109****	0.105****
	(43.75)	(41.36)	(36.11)	(43.75)	(29.29)
Constant	32.984****	33.170****	30.369****	32.984****	30.609****
	(8.60)	(12.61)	(8.46)	(8.60)	(7.96)
N	6583	6583	6583	6583	6476

Table A.10: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All models are System GMM models, where democracy is considered endogenous. Independent variables are lagged with 5 years. Maximum length of time series is 1950–2008 on dependent variable, GDP per capita growth (in percentage).

	Model I Baseline DD	Model II Re-coded DD: Type 2 democratic	Model III Re-coded DD: Botswana democratic	Model IV Baseline DD on Boix et al. sample	Model V Boix et al. regime measure
	b/(t)	*b/(t)*	*b/(t)*	*b/(t)*	*b/(t)*
Democracy	0.001	0.009*	0.001	0.001	-0.001
	(0.13)	(1.96)	(0.20)	(0.13)	(-0.14)
Ln population	-0.003	-0.009*	-0.003	-0.003	-0.002
	(-0.65)	(-1.84)	(-0.65)	(-0.65)	(-0.46)
Ln regime dur.	0.004****	0.005****	0.004****	0.004****	0.005****
	(7.72)	(9.47)	(7.65)	(7.72)	(7.30)
1940s	-0.022****	-0.021****	-0.022****	-0.022****	-0.024****
	(-7.97)	(-6.66)	(-7.91)	(-7.97)	(-7.35)
1950s	-0.026****	-0.028****	-0.026****	-0.026****	-0.027****
	(-12.08)	(-14.48)	(-11.94)	(-12.08)	(-10.37)
1960s	-0.010****	-0.012****	-0.010****	-0.010****	-0.012****
	(-5.43)	(-7.55)	(-5.37)	(-5.43)	(-5.52)
1970s	-0.019****	-0.019****	-0.019****	-0.019****	-0.020****
	(-10.44)	(-12.62)	(-10.34)	(-10.44)	(-9.50)
1980s	-0.038****	-0.038****	-0.038****	-0.038****	-0.038****
	(-27.28)	(-31.03)	(-27.07)	(-27.28)	(-22.68)
1990s	-0.026****	-0.026****	-0.026****	-0.026****	-0.027****
	(-50.00)	(-45.28)	(-49.61)	(-50.00)	(-33.31)
Lag dep. var.	0.991****	0.989****	0.991****	0.991****	0.987****
	(333.54)	(313.31)	(330.41)	(333.54)	(271.19)
Constant	0.144***	0.248****	0.144***	0.144***	0.165***
	(2.63)	(4.28)	(2.64)	(2.63)	(3.01)
N	7189	7189	7189	7189	7066

Table A.11: **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All models are System GMM models, where democracy is considered endogenous. Independent variables are lagged with 1 year. Maximum length of time series is 1950–2008 on dependent variable, log GDP per capita.

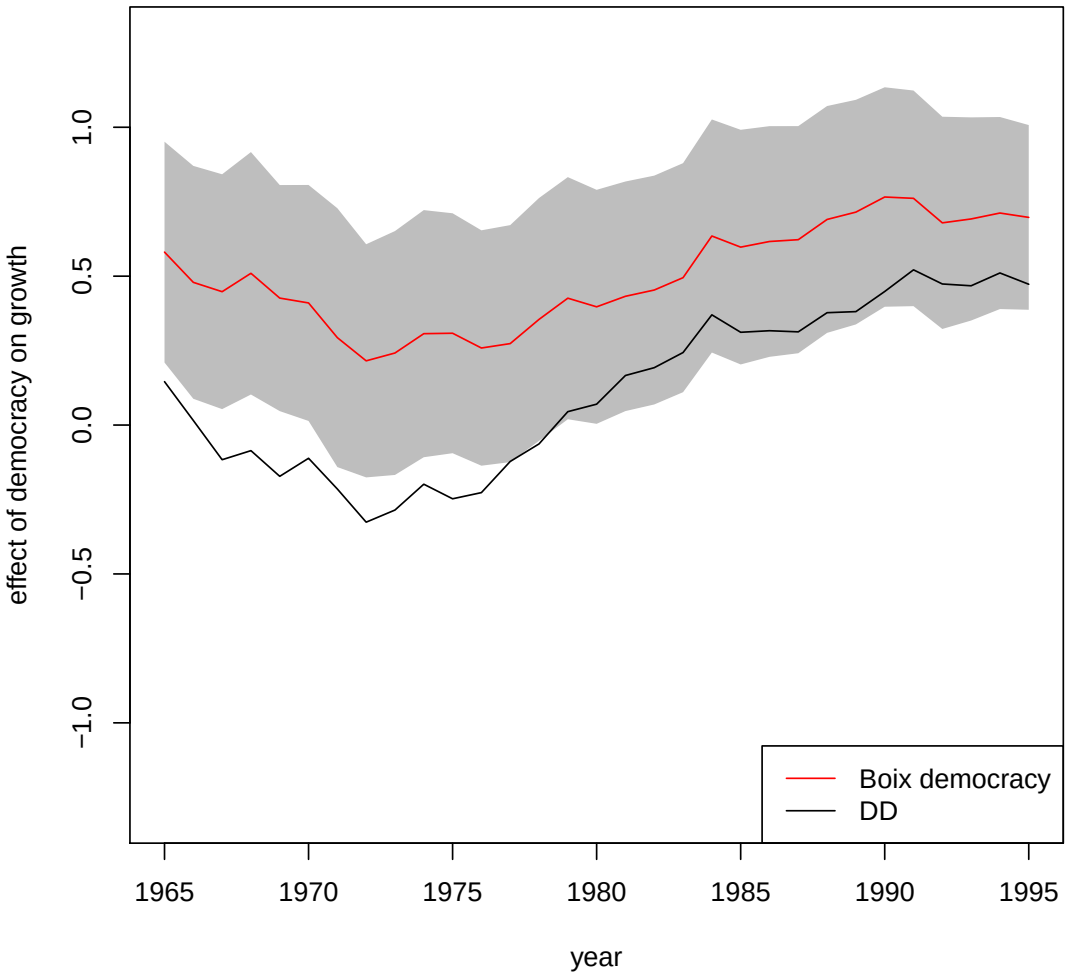

Figure A.1: The figure shows estimated effect of democracy according to Cheibub et al. (2010) DD coding and to Boix et al. (2013) (with 90 percent confidence intervals) from OLS PCSE regressions (specified as in Table 2) for samples from 1946 to year as given by x-axis.

Figure A.1 shows that running equivalent regressions comparing the Boix et al and DD measures for different time series also finds a quite stable and systematic difference – the former consistently yields a larger effect of democracy on growth (typically around 0.3 difference); the DD coefficient is, for samples ending before 1980, even outside of the 90 percent confidence interval of the Boix et al. coefficient.

DV:	1946-70 Original DD Civil war onset	1946-70 Realtime DD Civil war onset	1946-80 Original DD Civil war onset	1946-80 Realtime DD Civil war onset	1946-90 Original DD Civil war onset	1946-90 Realtime DD
Democracy	-0.520	-0.615	-0.577*	-0.347	-0.320	-0.186
	(-1.17)	(-0.94)	(-1.86)	(-0.79)	(-1.06)	(-0.48)
Ln GDPpc	-0.518***	-0.536***	-0.299*	-0.358**	-0.312***	-0.345**
	(-2.87)	(-2.86)	(-1.95)	(-2.13)	(-2.61)	(-2.53)
Ln Population	0.330***	0.312***	0.384****	0.364****	0.297****	0.289****
	(3.03)	(2.83)	(4.86)	(4.85)	(4.27)	(4.08)
Growth (last five years)	0.112**	0.112**	0.090**	0.097**	0.083***	0.089***
	(2.34)	(2.39)	(2.21)	(2.44)	(2.97)	(3.17)
ELF	1.230**	1.223*	1.178**	1.277**	1.047**	1.114***
	(2.01)	(1.92)	(2.43)	(2.48)	(2.55)	(2.60)
Peace years	-0.006	-0.029	0.053	0.031	-0.137	-0.155
	(-0.03)	(-0.15)	(0.32)	(0.19)	(-0.98)	(-1.09)
Spline 1	0.011	0.010	0.008	0.007	-0.002	-0.002
	(1.36)	(1.25)	(1.20)	(1.10)	(-0.32)	(-0.41)
Spline 2	-0.009***	-0.009**	-0.003	-0.003	0.000	0.000
	(-2.60)	(-2.49)	(-1.41)	(-1.36)	(0.24)	(0.30)
Spline 3	0.007***	0.007***	0.001	0.001	-0.000	-0.000
	(3.16)	(3.10)	(1.38)	(1.40)	(-0.17)	(-0.16)
Year	0.034	0.037	0.028*	0.029*	0.019*	0.020*
	(1.37)	(1.41)	(1.83)	(1.79)	(1.76)	(1.84)
N	1446.000	1452	2232	2237	3010	3015

Table A.12: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Logit models of the impact of regime type on civil war onset. T-statistics in parenthesis, standard errors clustered on countries.

	Baseline DD	Original DD: Type II democratic	Baseline DD: Boix et.al sample	Boix et.al regime measure
DV:	Civil war onset	Civil war onset	Civil war onset	Civil war onset
Democracy	-0.292	-0.422**	-0.217	-0.255
	(-1.35)	(-2.24)	(-0.93)	(-1.09)
Ln GDPpc	-0.309****	-0.295****	-0.326****	-0.316***
	(-3.30)	(-3.30)	(-3.40)	(-3.29)
Ln population	0.312****	0.314****	0.319****	0.318****
	(5.57)	(5.68)	(5.58)	(5.62)
Growth (last five years)	0.065***	0.066***	0.066***	0.065***
	(2.67)	(2.72)	(2.68)	(2.66)
ELF	1.018***	1.008***	1.027***	1.048***
	(2.90)	(2.89)	(2.86)	(2.90)
Peace years	-0.002	0.003	0.033	0.032
	(-0.02)	(0.03)	(0.30)	(0.29)
Spline1	0.005	0.005	0.006	0.006
	(1.22)	(1.25)	(1.44)	(1.44)
Spline2	-0.002	-0.002	-0.002*	-0.002*
	(-1.62)	(-1.64)	(-1.79)	(-1.79)
Spline3	0.000**	0.000**	0.000**	0.000**
	(2.39)	(2.42)	(2.39)	(2.40)
Year	0.012**	0.014**	0.012*	0.012**
	(2.01)	(2.23)	(1.91)	(1.97)
N	4242	4250	4130	4147

Table A.13: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Logit models of the impact of regime type on civil war onset. T-statistics in parenthesis, standard errors clustered on countries.

	1946-70 Original DD	1946-70 Realtime DD	1946-80 Original DD	1946-80 Realtime DD	1946-90 Original DD	1946-90 Realtime DD
DV:	Coup attempt	Coup attempt	Coup attempt	Coup attempt	Coup attempt	Coup attempt
Democracy	0.477	(.)	-0.010	-3.099**	0.240	-1.894**
	(1.26)	(.)	(-0.03)	(-2.26)	(0.82)	(-2.15)
Military regime	2.350****	2.045****	1.896****	1.739****	1.815****	1.606****
	(6.44)	(5.75)	(5.76)	(5.83)	(7.29)	(7.02)
Mil. exp. per soldier	-0.456**	-0.319	-0.406***	-0.343**	-0.290**	-0.241**
	(-2.44)	(-1.46)	(-2.69)	(-2.28)	(-2.42)	(-2.03)
Change in mil. exp.	-0.009	-0.013	-0.049	-0.102	-0.027	-0.077
	(-1.27)	(-1.38)	(-0.29)	(-0.57)	(-0.28)	(-0.51)
Ln military personell	-0.349****	-0.324****	-0.254****	-0.240****	-0.220****	-0.204****
	(-3.74)	(-3.29)	(-3.76)	(-3.46)	(-4.05)	(-3.66)
Gdpgrowth	-1.128	-1.314	-0.222	-0.293	-1.437	-1.262
	(-0.43)	(-0.55)	(-0.14)	(-0.19)	(-1.09)	(-1.03)
ln Gdppc	0.126	0.317	0.026	0.093	-0.109	-0.019
	(0.60)	(1.54)	(0.15)	(0.59)	(-0.82)	(-0.15)
instability	0.114****	0.164***	0.123****	0.134****	0.105****	0.115****
	(3.44)	(3.17)	(5.32)	(5.35)	(6.06)	(6.04)
Yrs. since a coup	-0.104	-0.085	-0.175**	-0.168**	-0.214****	-0.211****
	(-0.63)	(-0.51)	(-2.17)	(-2.11)	(-3.34)	(-3.35)
Spline 1	0.004	0.004	-0.001	-0.001	-0.003*	-0.003
	(0.09)	(0.09)	(-0.33)	(-0.31)	(-1.72)	(-1.58)
Spline2	-0.002	-0.002	-0.018	-0.020	0.002	0.002
	(-0.05)	(-0.05)	(-0.99)	(-1.04)	(1.36)	(1.25)
Spline3	(.)	(.)	0.031	0.034	-0.001	-0.001
	(.)	(.)	(1.11)	(1.17)	(-1.14)	(-1.11)
N	955	766	2146	2147	3436	3437

Table A.14: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Logit models of the impact of regime type on risk of coup attempt. Replication of Powell and Thyne 2012. T-statistics in parenthesis, standard errors clustered on countries.

	Baseline DD	Re-coded DD: Type II democratic	Baseline DD Boix et.al sample	Boix et. al regime measure
DV:	Coup attempt	Coup attempt	Coup attempt	Coup attempt
Democracy	-0.082	-0.306	-0.074	-0.317
	(-0.31)	(-1.30)	(-0.28)	(-1.10)
Military regime	1.666****	1.566****	1.674****	1.588****
	(7.73)	(7.35)	(7.70)	(7.63)
Mil. exp. per soldier	-0.259***	-0.254***	-0.255***	-0.248***
	(-2.78)	(-2.77)	(-2.74)	(-2.72)
Change in mil. exp.	-0.027	-0.038	-0.026	-0.039
	(-0.28)	(-0.30)	(-0.29)	(-0.31)
ln military personell	-0.216****	-0.210****	-0.206****	-0.194****
	(-4.08)	(-4.01)	(-3.89)	(-3.71)
Gdpgrowth	-1.802	-1.752	-1.823	-1.762
	(-1.59)	(-1.56)	(-1.60)	(-1.58)
ln Gdppc	-0.084	-0.072	-0.095	-0.091
	(-0.71)	(-0.63)	(-0.80)	(-0.78)
instability	0.106****	0.109****	0.105****	0.108****
	(6.38)	(6.44)	(6.25)	(6.33)
Yrs. since coup	-0.221****	-0.225****	-0.218****	-0.227****
	(-3.95)	(-4.04)	(-3.87)	(-4.04)
spline1	-0.002**	-0.002**	-0.002*	-0.002**
	(-2.01)	(-2.03)	(-1.87)	(-1.96)
spline2	0.001	0.001	0.001	0.001
	(0.94)	(0.94)	(0.79)	(0.85)
spline3	0.000	0.000	0.000	0.000
	(0.45)	(0.45)	(0.56)	(0.52)
N	4728	4732	4696	4703

Table A.15: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Logit models of the impact of regime type on risk of coup attempt. Replication of Powell and Thyne 2012. T-statistics in parenthesis, standard errors clustered on countries.