The Leadership Quarterly 31 (2020) 101302

Contents lists available at ScienceDirect

The Leadership Quarterly

journal homepage: www.elsevier.com/locate/leaqua

How often do dictators have positive economic effects? Global evidence, 1858–2010☆

Stephanie M. Rizio[a,b], Ahmed Skali[c,d,*]

[a] Victoria Institute of Strategic Economic Studies, Victoria University, Australia
[b] School of Psychological Sciences, University of Melbourne, Australia
[c] College of Business, Royal Melbourne Institute of Technology, Australia
[d] Centre for Behavioural Economics, Society and Technology, Queensland University of Technology, Australia

ARTICLE INFO

JEL classification:
D72
H1
P16
P48

Keywords:
Benevolent autocrats
Democracy
Autocracy
Economic growth
Leaders

ABSTRACT

Supposedly well-intentioned dictators are often cited as drivers of economic growth. We examine this claim in a panel of 133 countries from 1858 to 2010. Using annual data on economic growth, political regimes, and political leaders, we document a robust asymmetric pattern: growth-positive autocrats (autocrats whose countries experience larger-than-average growth) are found only as frequently as would be predicted by chance. In contrast, growth-negative autocrats are found significantly more frequently. Implementing regression discontinuity designs (RDD), we also examine local trends in the neighbourhood of the entry into power of growth-positive autocrats. We find that growth under supposedly growth-positive autocrats does not significantly differ from previous realizations of growth, suggesting that even the infrequent growth-positive autocrats largely "ride the wave" of previous success. On the other hand, our estimates reject the null hypothesis that growth-negative rulers have no effects. Taken together, our results cast serious doubt on the benevolent autocrat hypothesis.

Introduction

Humans are hard-wired to perceive agency. By agency, we refer to the tendency to ascribe conscious intentions to phenomena that are not guided by any such intents. In the wild, this is a successful evolutionary strategy, even if it leads to false positives. It is better to interpret rustling in a nearby bush as caused by a predator or an ill-intended rival tribesperson, and be incorrect, than to ascribe it to the wind and be incorrect.

This tendency has remained with us into the present day. As social primates, we may also be inclined to accept the authority of a single individual, the alpha primate. Perhaps this is why we routinely attribute group-level outcomes to the actions of leaders, even when leaders have no control over outcomes (Weber, Camerer, Rottenstreich, & Knez, 2001), which may lead us to be accepting of autocratic leadership styles. Indeed, the leadership literature has recently shown that, in times of uncertainty, the order and predictability provided by a strongly hierarchical system can make the idea of autocratic leadership attractive in organizations (Harms, Wood, Landay, Lester & Lester, 2018; De Hoogh, Greer & Den Hartog, 2015).

At the country level, autocratic leaders are often credited with purposefully delivering good outcomes. A prime example is the late Lee Kuan-Yew, who is widely credited with Singapore's prosperity. Easterly and Pennings (2017) discuss several examples from academic and media experts praising the work of Yew, Rwandan President Paul Kagame, and other supposedly benevolent autocrats. A number of theoretical arguments have been made regarding the benevolent autocrat hypothesis; these arguments are discussed below (for a review, see Easterly, 2011).

If autocrats are to be interpreted as benevolent, they should frequently be associated with good economic outcomes. In this paper, we ask whether that is the case. We view this as an important question for economic development and research in political leadership, but also a timely question, as the rise of "strongman" figures is becoming more and more prevalent. In an era where voters are willingly trading their political freedoms in exchange for promises of strong economic performance to strongman figures like Donald Trump, Vladimir Putin or Recep Tayyip Erdogan, it is important to understand whether autocratic

☆ Thoughts and comments from the editors (Harry Garretsen, Janka Stoker and Roberto Weber), two anonymous reviewers, Mohammed Ait Lahcen, and Ananta Neelim are gratefully acknowledged.

* Corresponding author.
E-mail addresses: Stephanie.Rizio@vu.edu.au (S.M. Rizio), ahmed.skali@rmit.edu.au (A. Skali).

https://doi.org/10.1016/j.leaqua.2019.06.003
Received 2 December 2017; Received in revised form 24 June 2019; Accepted 30 June 2019
Available online 16 July 2019

leaders do deliver economic growth. Our empirical results indicate that they do not.

Using data on economic growth, political regimes and political leaders over the 1858–2010 period, we document a robust empirical pattern in the data. Growth-positive autocrats, which are those leaders whose countries experience larger than average economic growth, are found only as frequently as one would predict based on chance alone. This finding is robust to several alternate definitions of political regimes, and also to alternate measures of economic success. Thus, we find no evidence to suggest that autocratic leaders are successful at delivering economic growth in any systematic way. In contrast, we show that growth-negative autocrats are found significantly more frequently than chance would predict. Taken together, these two results cast serious doubt on the view that autocratic leaders are successful at promoting economic growth.

Motivated by these two results, we then ask whether growth changes systematically when growth-positive and growth-negative autocrats take political office. Since political transitions do not happen independently of economic conditions, we are interested in determining whether growth-positive autocrats, although infrequent, really do deserve the credit for turning around their country's economic fate. We therefore implement regression discontinuity designs centered around the date of entry into office for those autocrats we estimate to be growth-positive. We find no evidence to suggest that the growth trajectory is different in the five or ten years post-entry than it was in the pre-entry period. Therefore, it appears that even the rare growth-positive autocrats largely find themselves at the right place at the right time and "ride the wave" of pre-existing growth. In the interest of completeness, we apply the same level of scrutiny to growth-negative autocrats; after all, one could hypothesize that these autocrats found themselves at the wrong place at the wrong time. Our empirical tests strongly reject this hypothesis: economic growth is found to be significantly lower after the entry into power of an economically "bad" autocrat.

This paper contributes to the literature on political leaders and economic growth, pioneered by Jones and Olken (2005). Exploiting the timing of leader deaths while in power, of natural or accidental causes, as a source of exogenous variation in political leadership, they find that political leaders have a significant impact on growth. They also find that this effect is driven by autocratic leaders, and not by democratic leaders. Their estimation method only attempts to elicit whether an effect is present, but does not deal with the direction of the effect. As pointed out by Jones, (2008, p. 7), "whether leaders can be good, bad, or both is an open empirical question." Our results indicate that autocrats do affect economic growth; we show that this effect only happens for the worse, and not for the better, as growth-positive autocrats are only as frequent as chance would predict, but growth-negative autocrats are far more frequent.[1] Coupled with recent results from the micro-level leadership literature, our results offer a cautionary tale as to the potential consequences of autocratic leadership. In particular, Hiller, Sin, Ponnapalli and Ozgen (2018) meta-analyze the findings from 152 studies (N = 68,395) on the consequences of authoritarian leadership, with particular reference topaternalistic leadership.[2] They find that task performance, pro-social behaviour, and creativity, among other variables, are all negatively affected by authoritarian leaders. In addition, Schyns and Schilling (2013) document large-scale evidence from 57 studies that the consequences of bad leaders in organizations are dire, especially as regards counterproductive employee behaviour in the workplace.

Ours is not the first paper to study whether benevolent autocrats can arise. Easterly (2011) compares the variance of leader effects across regime types. He finds that leadership quality is less variable in autocracies than in democracies and thus concludes that autocratic leaders cannot explain the high variance of growth in autocracies, and are responsible for neither high- nor low-growth episodes. We depart from Easterly (2011) methodologically, as we estimate frequencies for good and bad autocrats, as detailed below, and compare them to a normative expectation rooted in statistical theory. Our results also stand in contrast: we show that good autocrats are rare and bad autocrats are frequent. In addition, we show that even the rare "good" autocrats appear to ride the wave of pre-existing success. A related paper by Easterly and Pennings (2017) develops a shrinkage-based method to produce least squares estimates of leader contributions to growth. They conclude that political leaders at large are unimportant for economic growth. Our work differs from Easterly and Pennings (2017), as we do not argue that leaders are unimportant: our argument is that, consistent with the rational choice view, *autocratic* leaders are rarely good, and frequently bad, for economic growth.

The remainder of this paper is organized as follows. Background provides an overview of recent empirical work on the aggregate effects of democracy on economic outcomes, as well as theoretical arguments both in favour and against the benevolent autocrat hypothesis. In Data, we present our data and some descriptive statistics. Empirical Approach and Main Results introduces our empirical approach and main results. In Sensitivity Analysis, we assess the robustness of our results to several important considerations. In Accounting for Local Trends in Growth Rates, we present the results from regression discontinuity design estimates, comparing growth across the date of entry of particular leaders of interest. Concluding Remarks offers some concluding remarks.

Background

Aggregate effects of democracy on economic development

A large literature explores the democracy and development nexus. The two main hypotheses that have emerged from this field of inquiry are: (i) income drives democracy (Lipset, 1959): upon becoming wealthier, citizens demand more political rights[3]; and (ii) democracy drives income: good political institutions enforce property rights, provide the right incentives for investment in physical and human capital, and foster long-term economic growth, as proposed by von Hayek (1960) (for a review, see Doucouliagos & Ulubasoglu, 2006, 2008; Méon & Sekkat, 2016; Tavares & Wacziarg, 2001; De Haan, 2007). The earlier evidence in this literature is mixed and varies conditional on the composition and size of the sample (see Madsen, Raschky, & Skali, 2015). However, in recent years, owing to improvements in approaches to identification and data availability, the emerging consensus from this literature is that democracy does drive economic development. We highlight some important and recent contributions below.

In one of the most compelling studies of democratization and growth, Papaioannou and Siourounis (2008) exploit democratic transitions in the 1960–2003 period as natural experiments in a difference-in-differences framework. Papaioannou and Siourounis (2008) estimate that switching from autocracy to democracy increases the growth rate of income per capita by 1%, above and beyond the growth rate experienced by similar countries which did not experience a regime shift (see also Rodrik & Wacziarg, 2005). Using regional waves of

[1] See also Dreher, Lamla, Lein, and Somogyi (2009), who find that the professional and educational background of political leaders predict their propensity to engage in economic reforms; Hodler and Raschky (2014), who show that leaders channel funds towards their birth regions at the expense of other sub-national regions; Sidorkin and Vorobyev (2018), who find that regional governors in Russia display more corrupt behaviour near the expected end of their tenures.

[2] For global evidence on cross-cultural variation in the endorsement of different modes of paternalistic leadership, see Mansur, Sobral and Goldszmidt (2017).

[3] See also Freund and Jaud (2013) and Murtin and Wacziarg (2014) for recent empirical studies examining the determinants of democratic transitions.

democratization as time-varying instruments for democracy, Acemoglu, Naidu, Restrepo and Robinson (forthcoming) also find a positive and highly significant effect of democracy on economic development. Aligned with Papaioannou and Siourounis (2008), they also find an increase in per capita growth rates of 1 to 1.7% approximately (Acemoglu, Naidu, Restrepo and Robinson, forthcoming, Table 3). Gründler and Krieger (2016) address a central issue in the measurement of institutional quality: the coarseness of democracy scales, which are typically discrete integer scales. For example, the *Polity2* score (Marshall & Gurr, 2014) ranges from −10 (least democratic) to +10 (most democratic), in steps of 1. Gründler and Krieger's (2016) argument is that these discrete scales are not sufficiently responsive to changes in democracy; they therefore use support vector machines, a class of pattern recognition algorithms from the machine learning literature, to construct a continuous democracy indicator. They find that their indicator, which is more sensitive than traditional measures of democracy, robustly predicts economic growth. Using linguistic distance-weighted democracy as an instrument for domestic democracy, Madsen et al. (2015) also document a positive effect of institutions on economic prosperity, even after controlling for the potential endogeneity of human capital.

Growth in autocracies

Motivation

Thus, the emerging consensus in the recent empirical literature points to positive aggregate effects of democratic institutions on economic development. However, in order to understand the rationale behind the benevolent autocrat hypothesis, we now turn to a discussion of some salient mechanisms that have been hypothesized in the earlier literature, through which an autocratic leader may deliver pro-growth policies. Then, guided by both theory and history, we discuss pitfalls of the benevolent autocrat hypothesis.

But first, who is the benevolent autocrat? In Coolidge and Rose-Ackerman's (2000, p. 60) words, "the benevolent autocrat wants to maximize economic output by allowing the market to work, using government intervention only to correct market failures. In other words, the benevolent autocrat (espouses) the goal of GDP maximization." Thus, the benevolent autocrat takes decisions that are welfare-maximizing, not self-serving, in the same manner as the hypothetical social planner does in modern macroeconomics. Yet, while economists are well aware that the social planner is merely a modeling tool, the benevolent autocrat holds sway in real life. Several political leaders have been described as benevolent autocrats by journalists and political commentators, including Rodrigo Duterte in the Philippines,[4] Qaboos bin Said in Oman,[5] and even Francisco Franco in Spain.[6] With Coolidge and Rose-Ackerman's (2000) definition in mind, we proceed to discussing how autocrats may deliver favourable economic outcomes.

Democratic pitfalls

A first class of explanations as to why autocratic leaders may promote economic growth focuses on potential pitfalls of the democratic process. In Huntington's (1968) exposition, economic growth can be promoted by prioritizing future-oriented investments, such as investments in infrastructure, for example. Given a standard budget constraint, a corollary of increasing spending on investment is that spending on current consumption must decline to make way for spending on investment. This, in Huntington's words, can engender "popular discontent" (p. 50). If investment decisions are made via the electoral system, it is therefore theoretically possible that a democratic society may choose lower investment and therefore lower long-run growth. Sirowy and Inkeles (1990, p. 129) also note that democratically elected government officials may be prone to frequently shifting policies in order to cater to the electorate's current mood, rather than "focusing exclusively on policies oriented toward national development in the long-run."

More generally, popular demands may lead to counter-productive decisions. Hewlett (1979) argues that successive, democratically elected Brazilian governments in the 1950s and 1960s were unable to reign in the rampant inflation. In Hewlett's view, inflation was due to the pressure to print more currency in order to finance Brazil's import substitution program. The main beneficiaries of import substitution were the working class, upon which elected governments relied for re-election; this popular demand, Hewlett argues, effectively tied the hands of authorities into espousing sustained inflation.

The political economy of lobbying can also deliver forms of rent-seeking activities that are unique to democracies. While rent-seeking by individuals in positions of power can happen in all regime types, democracies may arguably be more prone to lobbying by sufficiently large and/or well-organized groups. In the context of international trade restrictions, Bhagwati (1982, p. 990) points out that "lobbying to install a distortionary tariff is undoubtedly directly unproductive from an economic viewpoint, though it may possess a political legitimacy and value as constituting an element of a vigorous, pluralistic democracy". Under the relatively heroic assumption that the autocrat is insensitive to demands from lobby groups, this type of lobbying should be less commonly found in autocratic regimes than in democratic regimes.

Good economic outcomes: by choice or by constraint?

A second class of explanation as to why high growth is observed under autocratic leaders considers the political economy constraints the autocrat faces. Acemoglu (2008) develops a theoretical model studying the interplay of taxation, competition, and political regimes. Acemoglu focuses on a comparison between democratic regimes and one particular type of autocracy: oligarchies. In his model, democracies and oligarchies differ along two key dimensions. First, political power under oligarchy is more concentrated: major owners of physical capital hold more political clout than under democracy. As a consequence, barriers to entry are higher in oligarchies, which reduces economic efficiency. Second, because democracies aggregate preferences in a one-person-one-vote fashion, taxation under democracy redistributes resources from entrepreneurs to workers. Acemoglu shows that, in the short-run, if taxes under democracy are sufficiently large and barriers to entry under oligarchy have sufficiently small distortive effects, then oligarchies can out-perform democracies. Crucially, however, this effect is short-lived. The equilibrium path Acemoglu finds is that oligarchies will fall behind democracies even if the two initial conditions are met (high tax in democracy, low distortion from entry barriers in oligarchy). At the oligarchic onset, entrepreneurs successfully lobby for high barriers to entry. These entry barriers cause only minor distortions in the economy, as entrepreneurs are among the most productive agents, and barriers allow them to earn high pay-offs from their productivity. The distribution of productivity across agents, however, changes over time. This time variation is partly stochastic (entrepreneurial skills follow a Markov process in Acemoglu's model), and partly reflective of compositional changes in the economy. This can happen, for example, if "new investment opportunities may be in industry whereas existing elites specialize in agriculture" (Acemoglu, 2008, p. 3), such that the initially high-productivity individuals are no longer necessarily employed in high-productivity fields.

Besley and Kudamatsu (2008) propose a selectorate theory of autocracy. By selectorate, they mean a group of people, akin to a political and economic elite, who hold substantial power in the appointment of the country's chief executive and can choose to remove the autocrat from office at any point. Their model seeks to explain why good growth

[4] Reyes (2018), "Rappler rises", The Manila Times.

[5] White (2011), "Oman's benevolent autocrat may avoid a similar fate to Libya's Gaddafi", The Guardian.

[6] Lonergan (2018), "'He is unfit to broadcast' – Irish radio host George Hook slammed after calling fascist Franco his 'favourite dictator", The Irish Post.

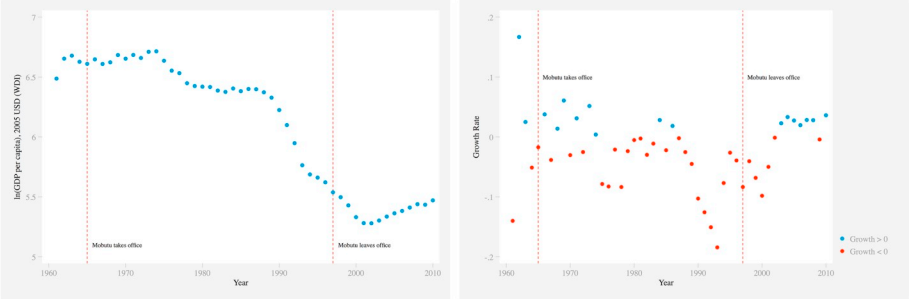

Fig. 1. The Democratic Republic of the Congo's Economy under Mobutu.
Note: The vertical lines denote the years of entry and exit of Mobutu into political office.
Source: World Development Indicators.

can occur even despite weak checks and balances on executive power. In their model, the autocrat can come from one of two competing groups *A* or *B*. If, for example, an autocrat who is a member of group *A* under-performs, group *A*'s selectorate can choose to remove the autocrat and open the leadership position to a contest. This can lead to the original leader being replaced by another leader from group *A*, or by a leader from group *B*. While in power, an autocrat may choose to try and "entrench" her power, which diminishes the selectorate's capacity to remove her from office. Besley and Kudamatsu show that good growth can be achieved when attempts at entrenchment from the dictator are unsuccessful. If the selectorate is secure, then the autocrat's optimal strategy autocrat's best interest. Any other strategy would lead to the autocrat being removed by the strong selectorate, which decreases the autocrat's payoff.

In a similar vein, De Luca, Litina, and Sekeris (2015) document the crucial role of inequality in the political economy of dictatorships. Their theoretical model shows that pro-growth dictators can be observed as a consequence of the strategic choices made by autocrats and elites. When the autocrat owns a large stake in the economy, she has an incentive to encourage economic growth, since she personally benefits from doing so. Elites respond, rationally, by supporting the autocrat in order to benefit from the increased economic activity. Their paper clearly highlights that self-serving behaviour, not benevolence, can account for pro-growth autocracies, providing another important theoretical backbone as to why autocrats may strategically be pro-growth.

Implications for benevolence

The picture that emerges from the literature discussed in Good Economic Outcomes: By Choice or By Constraint? is that autocrats may have strategic, self-serving reasons to be pro-growth; however, pursuing growth strictly out of benevolence appears unlikely. This, in our view, is an important distinction, with practical implications for how researchers in this field should approach policy recommendations. On the one hand, when constrained by a particular institutional structure, like in Besley and Kudamatsu's (2008) framework, an autocrat can be strategically pro-growth. The benevolent autocrat view, on the other hand, contends that an autocrat can be pro-growth even in the absence of a suitable set of incentives, simply out of a deeply held desire to do the "right" thing. If the benevolent autocrat hypothesis is to be believed, then we should advocate for the removal of checks and balances on the executive and put the fate of nations in the hands of unconstrained rulers.

Theory and history both tell us that this would be irresponsible advice. Olson's (1993) seminal work emphasizes that leaders who are not held accountable destroy incentives to invest in physical capital. This is true for both the autocrat with a short time horizon (the roving bandit) and the autocrat with a longer time horizon (the stationary bandit), since political succession in autocracies is uncertain, as it does not happen in a regularized leadership contest. These concerns are not simply theoretical. Rulers without checks and balances frequently lead to arbitrary rule-making, predatory behaviour, and economic disaster. There are countless examples of autocratic regimes turning into full-fledged kleptocracies with dire economic consequences. A salient example is the Democratic Republic of the Congo (then Zaire) under Mobutu Sese Seko. Fig. 1 summarizes the economic trajectory of the country during Mobutu's reign. The left-hand side panel plots log GDP per capita over time since independence from Belgium in 1960. During Mobutu's tenure (1965–1997), the average Zairean citizen saw her income drop from 6.5 to 5.5 approximately, on a logarithmic scale. This corresponds to income per capita being halved. The right-hand side of Fig. 1 plots annual growth rates over time; red dots correspond to negative growth, while blue dots correspond to positive growth. Growth was strongly negative for most of Mobutu's tenure, and began increasing after his departure.

Meanwhile, Mobutu himself amassed a personal fortune in the neighbourhood of $5 billion (Winsome Lesie 1987, p. 72), which he acquired largely through the "diversion of Zairean government funds, embezzlement of export earnings, and the massive diversion of foreign loans and aid" (Edgerton, 2002, p. 211). His rule was characterized by complete impunity for his actions. For example, his relatives took $71 million from the national bank in 1977, for personal use (Acemoglu, Verdier, & Robinson, 2004, p. 169). Mobutu replaced public officials at will, and even used the justice system to imprison whoever he saw fit, often, ironically, under charges of corruption (Young & Turner, 1985, p. 165). Foreign-owned firms were frequently nationalized (Acemoglu et al., 2004, p. 171), and inflation reached an annual rate of 7000%.[7]

In taking advantage of a lack of checks and balances on executive power to act as a bandit in Olson's sense, Mobutu is far from alone. Jean-Bédel Bokassa famously crowned himself Emperor of Central Africa (now the Central African Republic) in 1977, in a grandiose ceremony financed by state coffers. The ceremony was estimated to cost $30 million ($124 million in 2018 USD).[8] Bokassa also maintained a stranglehold on economic activity, and took no prisoners in protecting his business interests. Most infamously, he had schoolchildren arrested and killed for protesting against being forced to wear school uniforms produced by a company owned by Bokassa's wife.[9] Other famous bandits include Jean-Claude Duvalier and his father and predecessor François in Haiti, who were both renowned for their luxurious lifestyles while the country remained mired in poverty, Rafael Trujillo in the Dominican Republic, the Kim dynasty in North Korea, and countless others.

[7] Zagorin (2001), "Leaving fire in his wake", Time Magazine.
[8] French (1996), "Jean-Bedel Bokassa, Self-Crowned Emperor Of the Central African Republic, Dies at 75", New York Times.
[9] Lazareva (2017), "In the Central African Republic, nostalgia for a leader who is said to have fed his critics to the crocodiles", Washington Post, https://www.washingtonpost.com/news/democracy-post/wp/2017/06/22/in-the-central-african-republic-nostalgia-for-a-leader-who-is-said-to-have-fed-his-critics-to-the-crocodiles/?utm_term=.7c4a6b6759a1

Unconstrained power can have disastrous consequences even if the leader does not behave like Olson's bandit. Without safeguards against unilateral decisions, autocratic leaders are free to implement whatever policies they see fit, sometimes with tragic outcomes for ordinary citizens. Mao Zedong is widely credited as the mastermind of China's Great Leap Forward, which induced a collapse in grain prices and subsequently caused famine and malnutrition for millions of Chinese (Li & Yang, 2005). Exploiting spatial variation in the intensity of Great Leap Forward policies, Gooch (2017) and Liu and Zhou (2017) show that the negative impacts on human capital accumulation and economic growth are still found decades onwards.

Still, although there are reasons to suspect "benevolent" autocrats are few and far between, whether that is the case in the data is an open empirical question, with which this paper is concerned. We view this as an important question in light of the waning support for democracy (Foa & Mounk, 2016), with citizens trading in their democratic rights in exchange for promises of growth. It is important to note that we will only be able to observe whether economic growth is high or low under a certain leader and a certain set of political institutions, meaning that one cannot distinguish observationally between benevolent autocrats and strategically pro-growth autocrats. As a consequence, our results will likely over-state the frequency of benevolent autocrats. Our results can therefore be interpreted as an upper bound on the frequency of benevolence, for which we find no evidence, despite using this conservative route.

Data

Main variables

Political leaders

To identify political leaders, we rely on the latest iteration of Archigos (Version 4.1; Goemans, Gleditsch, & Chiozza, 2009, 2016). Goemans, Gleditsch and Chiozza document the effective head of state, that is, the person holding most of the executive power in 188 countries from 1840 to 2015. In many cases, the chief executive of the nation is clearly defined, like Robert Mugabe in Zimbabwe until November 2017; other instances are not so clear. That is, there are instances in which more than one person could be interpreted as the executive leader of the country. This is generally the case when the leader of a country is a figurehead for another person who pulls the strings of power, so to speak, such that the official leader does not hold most of the de facto power. In such cases, Archigos relies on the historical record to ascertain which individual was the effective leader in a given country and year. For example, Michel Domingue was officially the president of Haiti from June 1874 to April 1876. The seminal texts on Haitian politics, however, describes his nephew Septimus Rameau as the "true ruler" of Haiti, who manoeuvred to become Vice-President with "plenary executive powers" (Heinl, Heinl, Heinl, & Lanham, 1996, p. 245). Similarly, while Abu Sadat Mohammad Sayem was the official chief martial law administrator from November 1975 to April 1977 in Bangladesh, the consensus view is that army general Ziaur Rahman was the effective ruler for that time period.[10] Archigos closely scrutinizes such cases and identifies as a leader the person who holds most of the executive power as reflected in the historical record.

In the Archigos codebook, Goemans et al. (2009, 2016) provide comprehensive information on each of the leaders they identify. While a small number of cases may be debatable, Archigos is a highly acclaimed, award-winning dataset and is widely used across economics, political science and leadership studies. We therefore rely on Archigos as our source of information for political leaders. A total of 2330 leaders are covered in Archigos.

[10] See p. 655 of the codebook for version 2.9 of Archigos.

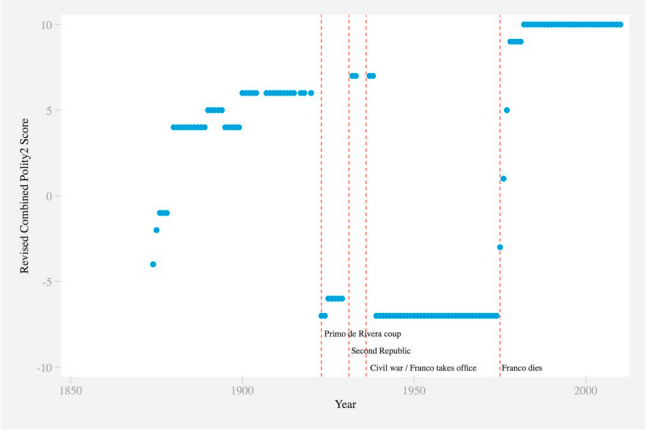

Fig. 2. Spain's *Polity2* score over time.
Source: Authors' calculations using data from Polity IV.

Political institutions

The Polity IV dataset (Marshall & Gurr, 2014) is our source of information for the political regime in place in each country-year. Polity IV provides several political variables for 185 countries over the 1800–2016 period. We use the revised combined democracy score, Polity2, to classify a country as either democratic or autocratic. The raw Polity2 measure ranges from most autocratic (-10) to most democratic ($+10$). The Polity2 variable captures the competitiveness and transparency of the recruitment process of politicians, executive constraints, and political participation. While it is difficult to accurately quantify a concept as broad as political institutions, the consensus in the literature is that the Polity IV dataset provides the best available measure of democracy (Glaeser, La Porta, Lopez-de Silanes, & Shleifer, 2004).

As an illustration of how the Polity IV project tracks a country's institutions over time, Fig. 2 plots the Polity2 variable for Spain from 1874 to 2010. Starting from a Polity2 score of -4 in 1874, Spain made steady democratic progress to a Polity2 score of 6, until a 1923 military coup installed Primo de Rivera at the helm. The Polity2 score dropped to -7 and -6, before returning to 7 during the Second Spanish Republic, starting in 1931. The Spanish Civil War and Franco's rule, from 1936, brought the Polity2 score down to -7, where it would stay until Franco's death in 1975. Spain then rapidly became a full democracy (*Polity*2 $= 10$) by 1982, and has kept this coding since.

Following Brückner and Ciccone (2011), we depart from the original Polity IV coding for interregnum periods. Polity IV defines interregnum periods as those where no single authority has effective control over political power, as is the case, for example, during full-fledged civil wars. The original Polity IV coding assigns a neutral score of 0 to these periods. However, this renders them indistinguishable from "true" zeroes, where a country has institutions that are halfway between full democracy and full autocracy. Following Brückner and Ciccone (2011), we therefore set interregnum periods to missing.

GDP per capita

We measure GDP per capita from the latest update of the well-known Maddison dataset (Bolt & Zanden Jan, 2014). While there are many widely used sources of GDP data, our choice of the Maddison dataset is motivated by the fact that it is the only one which extends far back in time. The Penn World Tables and World Development Indicators would have been other valid choices, but only Maddison allows us to go back all the way to 1858 for some countries, when data are first available for each of Archigos and Polity IV. Since our analysis focuses on panel data, which produces estimates that grow more consistent as the number of time periods increases, we opt to use the Maddison dataset, rather than being confined to the post-World War II time period. Overall, our sample includes 8431 observations from 133 countries

Table 1
Leader-level descriptive statistics.

	Democratic	Autocratic
Growth	0.020***	0.011
95% CI	[0.018–0.023]	[0.008–0.015]
*Polity*2	8.793***	−2.559
95% CI	[8.678–8.908]	[−2.898 to −2.220]
Tenure length	5.734	9.096***
95% CI	[5.416–6.053]	[8.401–9.790]
N	651	617

Notes. ***, **, and * denote significance at the 1, 5, and 10% levels respectively. Significance levels are from *t*-tests for equality of means across democratic and autocratic leaders. A leader is an autocrat if the mean *Polity*2 score during her tenure is strictly smaller than 6.

during the 1858–2010 time period. It should be noted that our sample is restricted to independent polities only, as those are the ones for which Polity2 and leader data from Archigos are available.

Non-parametric evidence

Table 1 displays leader-level descriptive statistics, for key variables of interest, for the 1268 leaders in our sample. For the purposes of this section, we treat an individual leader as autocratic if the mean Polity2 score during her tenure is strictly smaller than 6, as detailed in Econometric Specification below. This cut-off is chosen following the Polity IV codebook definitions,[11] and also has the effect of yielding approximately equal numbers of autocrats and democrats. By construction, Polity2 is larger under democratic leaders. We also note that democratic leaders, as expected, tend to have shorter tenures (5.7 years vs. 9.1 years in autocracies), as many democracies have term limits. The most interesting fact emerging from Table 1 is that growth rates under democratic leaders are almost twice as large as under autocratic leaders. This difference is statistically significant at the 1% level.

However, the inequality in means alone may be uninformative as to the growth effects of the "best" autocrats. Specifically, it may be that autocrats are over-represented in both the upper quintiles of the growth distribution. If autocrats are also over-represented in the lower quantiles, then a net negative effect may obscure an interesting heterogeneity. We therefore examine the quintiles of the empirical distribution of growth rates in Table 2. In order to compare quintiles across leader types, we implement the procedure proposed by Johnson et al. (2015). Their test splits the sample in bins of equal size, then compares the frequency of each sub-group within each bin and summarizes the results with a Pearson χ^2 test. In Table 2, we define five quintiles of approximately equal size and check whether autocrats are over-represented in the upper ranges of the growth distribution. The opposite is true: autocrats are over-represented only in the first and second quintiles, and are under-represented in the upper three quintiles. Pearson's χ^2 rejects the null hypothesis that the distribution of growth is identical across leader types, as does a Kolmogorov-Smirnov test ($p < 0.001$ in both cases). This provides prima facie evidence that growth is higher under democratic leaders in other parts of the empirical distribution, besides the mean.

In Table 3, we examine whether regime type changes frequently within leader tenures. We find that it does not: adopting 6 as our cut-off point on the Polity2 scale, approximately 97% of leader tenures are characterized by either no change at all, or by changes that do not affect the binary coding of regime types we employ below. With the cut-off of 6, we only find 21 democratizations (1.5%) and 9 autocratizations (0.06%) out of 1411 tenures in our sample. We therefore employ a binary coding of political institutions throughout, which we do eventually relax in Column (3) of Table 6.

[11] The Polity IV project defines democracies as those countries for which *Polity*2 ≥ 6. See also our discussion in Delayed Growth.

Table 2
Quintiles of growth by leader type.

Quintile	Democratic		Autocratic		Total	
	Number of leaders	Frequency	Number of leaders	Frequency		
1	90	13.82	**164**	**26.58**	254	20.03
2	123	18.89	**130**	**21.07**	253	19.95
3	**147**	**22.58**	107	17.34	254	20.03
4	**152**	**23.35**	102	16.53	254	20.03
5	**139**	**21.35**	114	18.48	253	19.95
Total	651	100	617	100	1268	100
Distributional Tests	Pearson		$\chi^2 = 39.482$	p = 0.000		
	Kolmogorov-Smirnov		$D = 0.148$	p = 0.000		

Notes. A leader is an autocrat if the mean *Polity*2 score during her tenure is strictly smaller than 6. Over-represented leader types in each quintile are shown in bold.

Table A.2 in the Appendix presents variable definitions and sources for all variables used in this paper. Summary statistics are available in Table A.3, while within- and between-panel correlation matrices are available in Tables A.4 and A.5. Country-level summary statistics for the number of leaders, leader tenures, and democracy scores are given in Tables A.7–A.10 in the Appendix.

Empirical approach and main results

Econometric specification

We begin with a straightforward, albeit naive, estimation of the effects of leaders on economic growth. This approach is naive in the sense that it attributes all economic growth during the tenure of a particular leader to the leader herself. We purposefully choose an approach which is prone to over-stating the importance of leaders; as we will see, even this charitable approach yields no support for the benevolent autocrat hypothesis. The baseline model takes the form:

$$\Delta\ ln(GDP\ p.\ c.)_{it} \approx g_{it} = \alpha_0 + \sum_{z=1}^{n} \beta_z L_{zit} + \epsilon_{it} \tag{1}$$

where the difference operator Δ subtracts the natural logarithm of per capita GDP from its first lag, which approximates the annual rate of economic growth. Eq. (1) estimates the growth rate g_{it} as a function of a set of leader dummies L_{zit}. For example, the dummy variable $L_{Blair,\ i,\ t}$ is set equal to one for the United Kingdom in the years when Tony Blair was Prime Minister, and zero for all other country-years. We define one such leader dummy for each leader in our sample. β_z is the average growth rate during the leader's tenure relative to the constant α_0, which is the average growth rate across all countries and years. This empirical approach follows the identification of CEO fixed effects by Bertrand and Schoar (2003) and of sub-national leaders in China by Yao and Zhang (2015).

Since we are interested in estimating the effects of autocratic leaders on economic growth, we modify Eq. (1) as follows:

$$g_{it} = \alpha_0 + \sum_{z=1}^{N} \beta_z L_{zit} * Autocracy_{it} + \epsilon_{it} \tag{2}$$

That is, we interact our leader dummies with a binary indicator of democracy, $Autocracy_{it}$. We employ several alternate definitions of autocracy, as detailed below, in recognition of the fact that political regimes come in many shades along the autocracy-democracy spectrum.

The Polity IV project distinguishes three main types of regimes: (i) autocracies (countries with a revised combined score *Polity*2 ≤ − 6);

Table 3
Changes in institutions and leader tenures.

Transition	Conditions	Number	Fraction
No change	$\Delta\ Polity2 = 0$	1201	0.85
Remains democratic	$\Delta\ Polity2 \neq 0\ \&\ Min\ (Polity2) \geq 6$	118	0.08
Remains autocratic	$\Delta\ Polity2 \neq 0\ \&\ Max\ (Polity2) < 6$	62	0.04
Becomes democratic	$\Delta\ Polity2 > 0\ \&\ Min\ (Polity2) < 6\ \&\ Max\ (Polity2) \geq 6$	21	0.01
Becomes autocratic	$\Delta\ Polity2 < 0\ \&\ Min\ (Polity2) < 6\ \&\ Max\ (Polity2) \geq 6$	9	0.01
	Total	1411	1.00

Notes. A set of consecutive years in a given country constitutes a single tenure if the same leader serves in all years. The number of unique tenures (1411) is therefore higher than the number of leaders (1268) in our dataset, since some leaders have multiple tenures.

(ii) hybrid regimes ($-6 < Polity2 < 6$); and (iii) democracies ($Polity2 \geq 6$). In our initial estimates, we adopt a broad definition of autocracy and code all non-democracies (categories (i) and (ii) above) as autocracies. This allows us to accomplish two objectives. First, we are able to capture a larger number of leaders than we would with a smaller cut-off. Second, we avoid frequent switches in the value of $L_z * Autocracy$, since $L_z * Autocracy$ is equal to 1 only when leader z is in power and the autocracy dummy is switched on. Thus we are able to capture the effects of autocratic leaders even if they lead towards more democratization. For example, if a leader starts with $Polity2 = -10$ but leads her country through a large democratization process, up to $Polity2 = 6$, our Autocracy dummy would be switched on for all of that leader's tenure, allowing us to capture the leader's growth effect in an uninterrupted fashion.[12]

We estimate independent and identically distributed (i.i.d.) errors for two reasons. First, cluster-robust standard errors are computationally difficult to obtain, since our specification includes many binary independent variables, and the most obvious choice of clustering variable (countries) is the same as the panel variable. Second, we believe this is a conservative route, as heteroskedastic errors will tend to be larger than their homoskedastic counterparts. The only exception to this rule is where intra-cluster correlations are negative; we see no reason to suspect that the error structure should be negatively correlated within countries. Note that we do not attempt to estimate β_z for leaders who spend only one year in power, as such tenures are too short to be informative.

Statistical test

After estimating Eq. (2), we retrieve the individual $\widehat{\beta_z}$ coefficients. Adopting a significance level $\alpha = 0.05$, a test of whether leader z's contribution to growth is positive, is a one-sided test of the alternative hypothesis $H_a : \beta_z > 0$. We therefore define a leader as growth-positive, in our regression results, if $\widehat{\beta_z} > 0$ and $p < \alpha/2$. The ratio of growth-positive autocrats to all autocrats, r, which is the frequency of occurrence of growth-positive autocrats, is the rejection rate of the null hypothesis $H_0 : \beta_z = 0$ in favour of the specific alternative hypothesis $H_a : \beta_z > 0$.

If variation in leader contributions to growth arose due to chance alone, then what would be the expected value of the rejection rate r? If leader effects are a random variable arising due to chance, then we would expect leader contributions to economic growth to be normally distributed with mean 0. Therefore, 95% of the leader effects should fall within two standard deviations of the mean. The remaining 5% of the leader effects should form the left and right tails of the distribution, with 2.5% at each tail.

This means that 2.5% of autocrats, if the data are generated purely by chance, are expected to be growth-positive; the expected value of the rejection rate is therefore 0.025. After estimating the $\widehat{\beta_z}$ coefficients and calculating r, we then test the following null hypothesis that positive autocrat contributions to growth are as frequent as one would predict due to chance alone:

$$H_0 : r - 0.025 = 0 \tag{3}$$

Implementing this test, as we will see, we systematically fail to reject the null hypothesis that the rejection rate for growth-positive autocrats differs from its expected value, suggesting that growth-positive autocrats occur only as frequently as we would predict due to chance alone. We perform the same test for growth-negative autocrats and find that they, on the contrary, occur far more frequently than the normative expectation.

Main results

We report our results in Table 4. Column (1) presents unconditional estimates. In Panel (a), we first confirm that autocrats, at large, do matter for economic growth. This is the case if the $L * Autocracy$ are jointly significant. The corresponding F-test strongly rejects the null hypothesis that autocrats do not matter, in all specifications. In Panel (b), we report the mean effect of autocrats on economic growth, along with its 95% confidence interval. Economic growth is found to be smaller under autocracy, corroborating Madsen et al. (2015) and Acemoglu, Naidu, Restrepo and Robinson (forthcoming). The effect is statistically and economically significant, and precisely estimated, with tight confidence intervals around -0.8%. To preserve space, we report t-statistics instead of confidence intervals in the remainder of this paper.

In Panel (c), we present right-tailed rejection rates and the corresponding p-values from Eq. (3). In the baseline specification (Column (1)), growth-positive autocrats are found to occur 3.3% of the time, which is only marginally larger than the 2.5% we expect due to chance alone. Most importantly, the p-value for the difference between those two numbers is large ($p = 0.24$). The evidence therefore suggests that growth-positive autocrats occur only as frequently as predicted by chance.

In Panel (d), we study growth-negative autocrats in the same fashion we studied growth-positive autocrats in Panel (c). The frequency of growth-negative autocrats is 6.8%, which is significantly larger than the expected rejection rate of 2.5% ($p < 0.000$). Our results for the two categories of autocrats stand in stark contrast and offer a sobering tale for the benevolent autocrat hypothesis. Finally, the lower panel of Table 4 displays additional information on the composition of the sample, as well as the R^2.

An immediate concern is that our results may be driven by low-quality autocrats which spend small amounts of time in power. If this were the case, we would risk under-counting growth-positive autocrats and over-counting growth-negative autocrats, which would attenuate our previous results. We therefore restrict the sample to leaders who

[12] Of course, if a leader were to lead a country into democracy proper ($Polity2 \geq 6$), the autocracy dummy would switch off. This is a rather infrequent occurrence, which happens in approximately 1% of the tenures we examine (see Table 3). We investigate this issue in more detail in Column (3) of Table 6.

Table 4
Leader effects. Autocracy = 1 if ***Polity2*** $<$ **6**; 0 otherwise.

	Dependent variable: g_{it}				
	(1)	(2)	(3)	(4)	(5)
		Tenure > ...			
	Baseline	2 years	5 years	10 years	Year FE
	(a) H_0: autocrat dummies jointly insignificant: $\beta_1 = \beta_2 = \ldots = \beta_n = 0$				
Prob. $> \chi^2$	0.00	0.00	0.00	0.00	0.00
	(b) Mean autocrat effect β_z				
Point estimate	−0.008	−0.007	−0.007	−0.009	−0.009
95% CI	[−0.012 to −0.005]	[−0.011 to −0.003]	[−0.012 to −0.002]	[−0.013 to −0.004]	[−0.013 to −0.006]
	(c) Growth-positive autocrats: $\beta > 0$				
Frequency	0.033	0.030	0.040	0.021	0.035
p-Value	0.236	0.495	0.174	0.715	0.170
	(d) Growth-negative autocrats: $\beta < 0$				
Frequency	0.068	0.062	0.076	0.058	0.068
p-Value	0.000	0.000	0.001	0.053	0.000
Num. autocrats	661	569	327	189	661
Countries	133	133	129	120	133
Observations	8431	7982	6126	4006	8431
Time period	1858–2010	1858–2010	1858–2010	1858–2010	1858–2010
R^2	0.18	0.16	0.15	0.14	0.26

Notes. Panel least squares estimates. g_{it} is the growth rate of real per capita GDP in country i and year t. An autocrat is growth-positive (negative) if growth during her tenure is significantly larger (smaller) than 0. Column (5) includes a set of year fixed effects. All specifications include a constant term.

had tenures longer than 2, 5, and 10 years, respectively, in Columns (2), (3) and (4). Our results are unaffected: we still find the frequency of growth-positive autocrats to be no greater than the normative expectation, while the frequency of growth-negative autocrats is still significantly larger than 2.5%.

In Column (5), we introduce year dummies into the specification, which allows us to control for macroeconomic events that affect all countries equally, such as global recessions. Our results are unaffected; in fact, Column (5) records one more growth-positive autocrat than Column (1). Since year fixed effects do not decrease the number of occurrences of growth-positive autocrats, while reducing the residual variance and potentially improving the signal-to-noise ratio of our estimates, we include them in our specifications from this point forward.

As a robustness check on our choice of cut-off, we repeat this analysis using alternate cut-offs for democracy on the Polity2 scale. We enumerate every point between the upper limit of hybrid regimes (*Polity*2 = 6) and the most commonly used definition of autocracy in the literature (*Polity*2 $<$ 0, see Jones & Olken, 2005; Persson & Tabellini, 2006; Besley & Kudamatsu, 2008; Besley, Montalvo Jose, & Reynal-Querol, 2011), giving us 6 alternate cut-off points to assess our results against. The rejection rates we estimate in this fashion are similar to those presented in this section and are available in Tables A.13–A.18 in the Appendix.

Sensitivity analysis

Graphical summary of the results

In his presidential address to the European Public Choice Society, De Haan (2007) highlights the sensitivity to model specification as a key limitation of the democracy and growth literature. In this paper (and the appendix), we therefore pay close attention to this issue and estimate 136 rejection rates (68 for each of growth-positive and growth-negative autocrats), as discussed in detail below. We plot these 136 rejection rates along with their p-values in Fig. 3. The green and red dots correspond to rejection rates for growth-positive and growth-negative autocrats respectively. The picture offers a sobering tale for the benevolent autocrat hypothesis.

Rejection rates (in green) for positive effects of autocrats are tightly distributed around the expected value of 0.025, and are rarely significant. In fact, the few that are significant (in the top-left region of the graph) are smaller than the expected rejection rate. Autocrats with negative effects (in red) are overwhelmingly found in the top-right region of the graph, where rejection rates are larger than 0.025 and p-values are smaller than 0.05. Thus, when we take a bird's eye view of our results, we find that autocrats with positive effects are found at best as frequently as predicted by chance, while autocrats with negative effects are found in abundance.

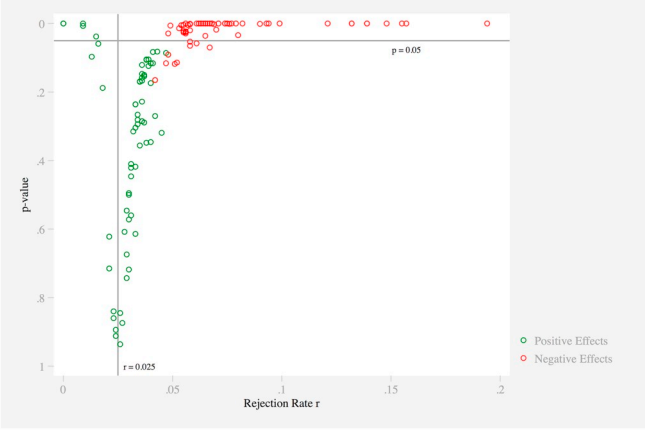

Fig. 3. Rejection Rates and p-values: A Graphical Summary.
Notes. The vertical line at $r = 0.025$ denotes the expected rejection rate. The horizontal line at $p = 0.05$ denotes the significance threshold.
Source: Authors' calculations.

Endogeneity of leadership transitions

In Table 4, we took leaders as given. However, national leadership can change when economic conditions change. For example, coups are less likely when the economy is performing well (Londregan & Poole, 1990). It is also well-known that good growth in the previous one or two years provides the incumbent an electoral advantage in democracies (Fair, 1978, 1982, 1988). Moreover, Freund and Jaud (2013) show that regime changes in general, rather than democratization, deliver growth dividends. It is therefore important to examine whether our results are driven by political change. Thus, in this section, we estimate the probability of leadership transitions and include it as a covariate in our empirical specification.

If poorly performing dictators are more likely to be replaced, then one would expect them to be under-represented in our sample, as they are quickly pushed out by poor growth. Conversely, autocrats who experience good growth are likely to become entrenched, such that they should be over-represented in the sample. This type of selection bias tends to favour the benevolent autocrat hypothesis, so our previous results are likely to be conservative. Still, whether the likelihood of leadership transition affects our results is an open empirical question. If the probability of leadership change does affect our results, it should be included as a control variable in Eq. (2).

In order to obtain this control variable, we estimate the likelihood of leadership transition as a function of economic conditions, and across regime types by regressing:

$$Pr\,(Leadership\ Change)_{it} = \alpha_0 + \gamma_i + \phi_t + \delta_1 g_{i,t-1} + \delta_2 Autocracy_{i,t-1} + \delta_3 (g * Autocracy)_{i,t-1} + \mu_{it} \quad (4)$$

The dependent variable, *Pr*(*Leadership Change*), is a dummy variable set equal to 1 if the leader in country i and year t is different from the leader in year t - 1, and 0 otherwise. δ_1, the coefficient of xtitg, captures the responsiveness of leadership change to country-specific growth rates across all regime types. δ_2, the coefficient of Autocracy, captures the differential propensity to change leaders in autocracies relative to democracies. Finally, the coefficient of the interaction term, δ_3, allows growth to have heterogeneous effects on leadership transitions conditional on regime type. This set-up is similar to a differences-in-differences framework with one discrete difference (Autocracy) and one continuous difference (g). We obtain predicted probabilities of leadership change under different variants of Eq. (4) and summarize these in Table 5 (Table A.19s in the Appendix provides full regression results). Specifically, we allow for up to five lags of the independent variables.

Across the board, the probability of leadership change is larger in democracies than in autocracies, with differences ranging from 2.5% to 4.4%. These differences are significant at the 1% level in all cases. Since including further lags does not alter the results, we employ the predicted probabilities of leadership change from the one-lag specification (Column (1) of Table A.19) as a control variable in Eq. (2).

Table 5
Predicted probability of leadership transition by regime type.

Lags	Democracy	Autocracy	Difference
1	0.169	0.026	0.044***
	[0.001]	[0.001]	[0.001]
2	0.168	0.126	0.043***
	[0.001]	[0.001]	[0.001]
3	0.165	0.126	0.039***
	[0.001]	[0.001]	[0.001]
4	0.156	0.127	0.029***
	[0.001]	[0.001]	[0.001]
5	0.151	0.127	0.025***
	[0.001]	[0.001]	[0.001]

Notes. See Table A.19 in the Appendix for full regression results. ***, ** and * indicate significance at the 1, 5, and 10% levels respectively.

The results are shown in Column (1) of Table 6. Once we account for the probability of leadership change, we find that the frequency of growth-positive autocrats is 0.016, which is even smaller than the expected value of 0.025. This difference is weakly significant ($p = 0.059$). Growth-negative autocrats, on the other hand, are much more likely than previously to be found than due to chance alone, with a frequency of 0.155.

A potential shortcoming of Eq. (4) is that lagged values of the growth rate may themselves be endogenous to the probability of leadership transitions. This would be the case if poorly performing leaders were more likely to be replace. Since we would ideally like to circumvent the possibility of reverse causality running from growth to political transitions, we need to find a suitable instrument for g. To this end, we instrument g with growth rates in trading partners, weighted by trade shares, which we call g^F:

$$g_{it}^F = \sum_{j=1}^{N} \theta_{ij} * g_{jt} \quad (5)$$

where the trade share θ is the sum of exports plus imports between country i and country j divided by the sum of exports and imports for country i, and g_{jt} is growth in country j. Trade data comes from the Correlates of War project (see Table A.2 in the Appendix for more details).

The rationale for this instrument is that economic shocks propagate through trade networks. Thus, we expect a country to grow faster if its trade partners are experiencing faster growth, and vice-versa. For example, the United States' trading partners are likely to experience a downturn if the United States is going through a recession, as the United States would be less likely to import goods and services from its trading partners. For g^F to act a suitable instrument for g, two conditions must be met. First, the two variables should be sufficiently correlated, such that g^F if not a weak instrument. We find empirical evidence in support of this proposition: the Sanderson-Windmeijer F-test of excluded instruments is 56.08, which comfortably clears the rule-of-thumb critical value of 10 (see Tables A.19 and A.20 in the Appendix for full regression results). Second, the exclusion restriction must hold: g^F is a valid instrument for g if g^F has no direct or indirect effect on *Pr*(*Leadership Change*) other than through its effect on g. In other words, the growth rate of trading partners must affect the likelihood of political transition at home only through its effect on the domestic growth rate. We expect this condition to hold in this case, as it is unlikely that economic developments in trading partners affect domestic politics through some channel other than the domestic economy. While we cannot test for overidentification, as we only have as many instruments as endogenous regressors, we check whether the exclusion restriction is likely to hold by adding g^F as a regressor in the structural equation (Column (3) of Table A.20). We find that, when the excluded instrument is included in the specification alongside the endogenous regressor, no meaningful correlation remains between the instrument and the dependent variable, suggesting that all of the effect of g^F on the probability of transition happens through g. This is consistent with the exclusion restriction being satisfied.

The first step in our procedure is to estimate the following first stage equation:

$$g_{it} = \alpha_0 + \gamma_i + \phi_t + \tau g_{i,t-1}^F + \upsilon_{it} \quad (6)$$

We obtain the predicted probability of transition in each country-year as above and use it as a control variable in Column (2) of Table 6. The frequency of growth-positive autocrats is now 0.022 and is not

Table 6
Political institutions: alternate codings.

	Dependent variable: g_{it}					
	(1)	(2)	(3)	(4)	(5)	(6)
	Control Pr(leader change)		Leader Ever	Country Full	Country Closed	Country Open
	OLS	IV	Autocratic	Autocracy	Anocracy	Anocracy
	(a) H_0: Autocrat dummies jointly insignificant					
Prob. $> \chi^2$	0.00	0.00	0.00	0.00	0.00	0.00
	(b) Mean autocrat effect β_z					
Point estimate	−0.024	−0.013	−0.010	−0.005	−0.013	−0.014
t-Statistic	−14.304	−8.495	−6.099	−1.611	−4.990	−5.466
	(c) Growth-positive autocrats: $\beta > 0$					
Frequency	0.016	0.022	0.033	0.047	0.021	0.040
p-Value	0.059	0.600	0.236	0.086	0.622	0.116
	(d) Growth-negative autocrats: $\beta < 0$					
Frequency	0.155	0.097	0.082	0.058	0.056	0.099
p-Value	0.000	0.000	0.000	0.020	0.025	0.000
Num. autocrats	638	595	661	277	288	425
Countries	133	132	133	133	130	112
Observations	8035	7201	8431	8431	5990	3638
Time period	1859–2010	1873–2010	1858–2010	1858–2010	1863–2010	1864–2010
R^2	0.26	0.25	0.26	0.18	0.26	0.15
S-W F-stat.		56.08				

Notes. S-W: Sanderson-Windmeijer. Panel least squares estimates with year fixed effects. g_{it} is the growth rate of real per capita GDP in country *i* and year *t*. An autocrat is growth-positive (negative) if growth during her tenure is significantly larger (smaller) than 0. The *Autocracy* dummy in Column (3) is set equal to one if *Polity*2 < 6 in at least one year of the leader's tenure, and zero otherwise. Regime types in Columns (4)–(6) follow the Polity IV classification. Autocracy: *Polity*2 < -5; closed anocracy: $-5 \leq$ *Polity*2 ≤ 0; open anocracy: $0 <$ *Polity*2 < 6. Column (4) compares autocratic leaders to leaders in closed anocracies, open anocracies, and democracies. Column (5) compares leaders in closed anocracies to leaders in open anocracies and democracies. Column (6) compares leaders in open anocracies to leaders in democracies. All specifications include a constant term.

significantly different from 0.025. Growth-negative autocrats remain more frequent than expected at 0.097 ($p < 0.001$).

Alternate regime codings

Was the leader ever an autocrat?

A potential shortcoming of our Autocracy dummy is that it is only switched on when the country is autocratic, such that it may not capture the entire tenure of a given leader. To illustrate, suppose leader z led her country through a democratization process. The $Leader_z * Autocracy$ variable is initially equal to 1, but once the country ceases to be autocratic, $Leader_z * Autocracy$ switches to 0 even if leader z is still in power. This coding has the potential to exclude high growth (under democracy) that could reasonably have been attributed to leader z, under the assumption that leader z purposefully led the democratization process.

In Column (3) of Table 6, we therefore replace the Autocracy dummy with a *Leader Ever Autocratic* dummy. The latter is equal to 1 if at any point during the leader's tenure, the Polity2 score was smaller than 6, and 0 otherwise. This alternate coding ensures that we track the entire tenure of the leader, without excluding potentially more successful years. Empirically, our results are unaffected: the growth-positive rejection rate is 0.033 ($p = 0.236$), while the growth-negative rejection rate remains very large and significant ($r = 0.082$; $p = 0.000$).

Autocracies and anocracies

So far, we have shown that growth-positive autocrats are found only as frequently as one would expect due to chance alone. It could be, however, that along the autocracy - democracy spectrum, there is a bliss point at which individual leaders have more freedom to implement pro-growth policies than they would have been able to, had institutions been more democratic. Thus, one could expect that, say, leaders in mildly autocratic societies have good growth effects, which our previous specifications are unable to detect because "mild" autocrats are lumped together with "strong" autocrats under the Autocracy dummy. We therefore need a hierarchy of autocracies to investigate this issue.

The Polity IV project offers such a classification. Other than democracies (*Polity*2 ≥ 6), Polity IV defines, by decreasing degree of autocracy, the following three types of regimes. First, autocracies proper (*Polity*2 < -5) are fully institutionalized autocratic regimes, which score consistently low on the democratic features discussed in Political Institutions. As of 2010, examples include Saudi Arabia, North Korea, Azerbaijan and Qatar. Second, closed anocracies ($-5 \leq$ *Polity*2 ≤ 0), like Singapore, Angola or Jordan, have some democratic features but are still largely autocratic. Third, open anocracies ($0 <$ *Polity*2 < 6), including Algeria, Thailand and Bangladesh, have a substantial degree of democratic arrangements, but nonetheless retain autocratic features. For example, Algeria (*Polity*2 $= 2$ in 2010) holds legislative elections that have been described as free by international observers, but elected officials' work is widely thought to be secondary to the wishes of the Algerian military.[13]

In Columns (4)–(6) of Table 6, we use this classification to study how leaders in a given regime type compare to leaders in more democratic regimes. We define the following three dummy variables:

$$Full\ Autocracy = \begin{cases} 1\ if\ Polity2 < -5 \\ 0\ otherwise \end{cases}$$

[13] Mahjar-Barducci (2012), "A Prolonged State of Agony in Algeria", Haaretz.

$$Closed\ Anocracy = \begin{cases} 1\ if\ -5 \leq Polity2 \leq 0 \\ 0\ if\ Polity2 > 0 \\ \varnothing\ otherwise \end{cases}$$

$$Open\ Anocracy = \begin{cases} 1\ if\ 0 < Polity2 < 6 \\ 0\ if\ Polity2 \geq 6 \\ \varnothing\ otherwise \end{cases}$$

For example, the *Open Anocracy* dummy is switched on (equal to 1) for open anocracies, off (equal to 0) for democracies, and undefined for closed anocracies and autocracies. The reference category only includes regimes that are more democratic than open anocracies, allowing us to estimate growth effects for leaders in open anocracies relative to leaders in democracies.

In Columns (4)–(6), we find that the previously documented pattern of rare growth-positive autocrats, and frequent growth-negative autocrats, is also found when we allow the growth effects of leaders to be heterogeneous by regime type. There is weak evidence in Column (4) that growth-positive autocrats occur more frequently in full autocracies, but the estimate is only significant at the 10% level. Of the 68 rejection rates for positive effects we estimate in this paper, another three are significant at the 10% level, and none are significant at the 5% level.

Time considerations

Delayed growth

In this section, we consider the possibility that a leader's effect on growth may be delayed. It is certainly plausible that, say, if a leader started a program of structural reforms aimed at growing the economy, measurable effects on the growth rate should not be expected in the short-run. If this is the case, then our previous results would likely underestimate the frequency of growth-positive autocrats. We therefore allow leaders to affect growth with a time lag in Columns (1)–(3) of Table 7.

There is no clear guideline as to how long an autocrat's effect should take to appear, if it exists. Thus, we use the mean autocratic tenure length as a lag, which is 9.096 years (Table 1), in Column (1). We therefore lag *Leader* ∗ *Autocracy* by 9 years in Column (1) of Table 7; the results show that growth-positive autocrats are still as frequent as randomly assigned, while growth-negative autocrats are more frequent. Thus, the empirical evidence in the medium-to-long run is consistent with our previous results.

Perhaps this result is due to short-run noise; arguably, the first few years of a leader's tenure can be spent laying down the foundations for good growth. In Columns (2) and (3), we therefore set our *Leader* ∗ *Autocracy* variables equal to 0 if the leader is in her first or second year in power. This allows us to disregard potentially troublesome early years, from the leader's perspective, and focus on the later part of the tenure. Our results are unchanged, suggesting that the patterns we observed above are not merely transitory.

Perverse incentives near end of tenure

As emphasized by Olson (1993), the bandit with a short time horizon has greater incentives to enrich herself at the expense of other agents in the economy. Assuming autocrats have information about the likelihood of their tenure ending in the near future, it is therefore plausible that autocrats nearing the end of their tenures "cash in," in anticipation of their departure, thereby lowering growth rates. Sidorkin and Vorobyev (2018) show that regional governors in Russia display more corrupt behaviour when their tenures are drawing to a close. In addition, low growth may itself trigger public discontent and autocratic exits. Thus it may be that autocracy in itself is not the entire reason why growth-negative autocrats are more frequent than expected. In Columns (4) and (5) of Table 7, we therefore amend our *Leader* ∗ *Autocracy* dummy to be equal to 0 in the last year or last two years of the leader's tenure, in order to determine whether the frequency of growth-negative autocrats is inflated by poor growth at the end of tenures. We find that

Table 7
Time considerations.

	Dependent variable: g_{it}				
	(1)	(2)	(3)	(4)	(5)
	Lag: mean	Excluded years			
	autocratic tenure	First	First 2	Last	Last 2
	(a) H_0: autocrat dummies jointly insignificant				
Prob. $> \chi^2$	0.00	0.00	0.00	0.00	0.00
	(b) Mean autocrat effect β_z				
Point estimate	−0.005	−0.007	−0.006	−0.005	−0.004
t-Statistic	−3.260	−3.853	−2.594	−3.206	−2.072
	(c) Growth-positive autocrats: $\beta > 0$				
Frequency	0.036	0.032	0.036	0.028	0.034
p-Value	0.147	0.315	0.167	0.608	0.266
	(d) Growth-negative autocrats: $\beta < 0$				
Frequency	0.065	0.056	0.063	0.049	0.064
p-Value	0.000	0.000	0.000	0.006	0.000
N. autocrats	612	623	523	634	534
Countries	132	82	133	133	133
Observations	6878	8431	8431	8431	8431
Time period	1867–2010	1858–2010	1858–2010	1858–2010	1863–2010
R^2	0.26	0.24	0.23	0.22	0.21

Notes. Panel least squares estimates with year fixed effects. g_{it} is the growth rate of real per capita GDP in country *i* and year *t*. An autocrat is growth-positive (negative) if growth during her tenure is significantly larger (smaller) than 0. The mean autocratic tenure is 9.1 years; *Leader* ∗ *Autocracy* is therefore lagged 9 years in Column (1). *Leader* ∗ *Autocracy* is equal to 0 in the appropriate years indicated in the headers of Columns (2)–(5), and otherwise defined as explained in the main text. All specifications include a constant term.

Table 8
Alternate economic outcomes.

	Dependent variable				
	(1) Employment	(2) g^{TFP}	(3) No recession Pr($g > 0$)	(4) No large recession Pr($g > -0.02$)	(5) Large expansion Pr($g > 0.02$)
	(a) H_0: autocrat dummies jointly insignificant				
Prob. $> \chi^2$	0.00	0.03	0.00	0.00	0.00
	(b) Mean autocrat effect β_z				
Point estimate	0.041	−0.004	−0.098	−0.083	−0.088
t-Statistic	0.090	−1.549	−8.160	−8.189	−7.195
	(c) $\beta > 0$				
Frequency	0.040	0.013	0.015	0.000	0.024
p-Value	0.346	0.097	0.038	0.000	0.894
	(d) $\beta < 0$				
Frequency	0.121	0.047	0.139	0.148	0.094
p-Value	0.000	0.116	0.000	0.000	0.000
N. autocrats	149	235	661	661	661
Countries	130	97	133	133	133
Observations	2393	4030	8431	8431	8431
Time period	1991–2010	1951–2010	1858–2010	1858–2010	1858–2010
R^2	0.46	0.13	0.26	0.26	0.26

Notes. Panel least squares estimates with year fixed effects. Linear probability models in Columns (3)–(5). g^{TFP} is the growth rate of total factor productivity. All specifications include a constant term. See Appendix for variable definitions and sources.

such is not the case: growth-negative autocrats are still found between 4.9% and 6.4% of the time ($p = 0.006$; $p = 0.000$ respectively).

Alternate economic outcomes

In this section, we examine whether autocrats deliver on other economic outcomes. In Column (1) of Table 8, we use employment rates as our dependent variable. We use employment rates, rather than unemployment rates, as our outcome variable, in order to preserve the interpretation of what we previously termed "positive" and "negative" leaders given in panels (c) and (d) of our tables. It is conceivable that, if autocracies can work for the people, autocratic leaders would ensure that unemployment is low. Column (1) of Table 8 suggests otherwise: unemployment-positive leaders are again as frequent as randomly assigned, while unemployment-negative leaders are found a sizeable 12.1% of the time.

In Column (2), we use the growth rate of Total Factor Productivity (TFP) as our dependent variable, which we label g_{it}^{TFP}. TFP is calculated as a residual from Cobb-Douglas production functions which take labour and capital as their inputs. As such, TFP tells us how much of economic growth cannot be explained by labour and capital. The main source of TFP is technology, which in endogenous growth models is considered to be the key to improvements in prosperity. We do not find evidence that TFP growth-negative autocrats are frequent: the estimated frequency (0.047) is larger than the expected value of 0.025, but not significantly so ($p = 0.116$). On the other hand, there is weak evidence that TFP growth-positive autocrats are less frequent than expected ($r = 0.013$; $p = 0.097$).

Motivated by Broadberry and Wallis (2017),[14] we examine economic expansions and recessions in Columns (3)–(5). Using historical data extending all the way back to the thirteenth century, Broadberry and Wallis argue that the key to long-term economic success is crisis avoidance, rather than increases in the growth rate. Therefore, we investigate whether autocratic leaders are systematically better at avoiding recessions. In Column (3), we employ a *No Recession* dummy as our dependent variable. *No Recession* is set equal to 1 if the growth rate is strictly positive, and 0 otherwise. We find that only 1.5% of autocratic leaders are positively correlated with the absence of recessions. This frequency is significantly smaller than the normative expectation ($p = 0.038$). On the other hand, a large 13.9% of autocrats are associated with more frequent recessions.

Given that an episode of small but negative growth may be an accident of history, we define a stricter recession avoidance dummy in Column (4). The *No Large Recession* dummy is only switched on when the growth rate is larger than −0.02, thus allowing some scope for smaller recessions to occur without counting them against a leader's record. The result is even starker: we are not able to find a single autocrat with a positive β, suggesting that autocrats are much worse than democratic leaders at avoiding recessions.

Despite the above, perhaps a benevolent autocrat can lead her country through a large economic expansion. We check for this possibility in Column (5), where we define a *Large Expansion* dummy with a cut-off of $g = 0.02$, mirroring our definition of large recessions. We find no evidence that large expansions are more frequent than chance. Expansion-negative autocrats, however, are found to occur approximately 9.4% of the time ($p = 0.000$).

Potential causes of recession avoidance

After documenting that rich countries have become successful by reducing the frequency and severity of recessions, Broadberry and Wallis (2017) study five potential causes which may explain recession avoidance. These are (1) demography; (2) armed conflict; (3) structural change; (4) technology; and (5) institutions. If leaders correlate with these factors, then it is possible that our results from Table 8 deliver a biased picture of leader effects. For example, an autocratic leader may find herself in power when a war breaks out due to circumstances outside the leader's control, which would affect the likelihood of a recession and therefore confound our estimates of the leader's effect.

In Table 9, we therefore control for Broadberry and Wallis's five

[14] We are grateful to an anonymous reviewer for suggesting this extension.

Table 9
Broadberry-Wallis potential causes of recession avoidance.

	Dependent variable: *Pr(No Recession)*				
	(1) Birth rate & death rate	(2) UCDP conflicts	(3) % Agricultural & % industrial	(4) g^{TFP}	(5) Rule of law
	(a) H_0: autocrat dummies jointly insignificant				
Prob. $> \chi^2$	0.00	0.00	0.00	0.00	0.00
	(b) Mean autocrat effect β_z				
Point estimate	−0.060	−0.128	−0.053	−0.089	−0.040
t-Statistic	−3.875	−8.859	−2.830	−5.036	−1.370
	(c) $\beta > 0$				
Frequency	0.024	0.009	0.029	0.009	0.000
p-Value	0.912	0.000	0.674	0.007	0.000
	(d) $\beta < 0$				
Frequency	0.139	0.194	0.132	0.157	0.132
p-Value	0.000	0.000	0.000	0.000	0.000
N. autocrats	373	448	273	235	114
Countries	132	133	126	97	131
Observations	5543	6468	3904	4030	1439
Time period	1960–2010	1946–2010	1960–2010	1951–2010	1996–2010
R^2	0.27	0.25	0.26	0.31	0.30

Notes. Linear probability models estimated via panel least squares with year fixed effects. An autocrat is growth-positive (negative) if growth during her tenure is significantly larger (smaller) than 0. All specifications include a constant term. See Appendix for variable definitions and sources.

factors. We use the natural logarithms of crude birth rates and crude death rates as our demographic variables in Column (1), the number and type of conflict events in Column (2), the shares of agriculture and industry as a percentage of total GDP in Column (3) as our measures of structural composition, g^{TFP} as a measure of technology in Column (4), and rule of law in Column (5). Note that, since our independent variables $\beta * Autocracy$ already capture democratic features, we employ rule of law as an additional measure of institutions. The full regression results with point estimates for each control variable are given in Table A.21 in the Appendix.

An interesting pattern emerges from Table 9. Once we control for Broadberry and Wallis's potential recession-avoiding factors, positive autocrats are found significantly less frequently than chance would predict in 3 out of 5 specifications, and almost exactly ($r = 0.024$; $r = 0.029$) as frequently as per chance in 2 out of 5. Negative autocrats, on the other hand, are abundant, and are found between 13% and 19% of the time approximately ($p = 0.000$ in all cases).

Orthogonal realizations of growth

In Table 10, we investigate whether leader tenures coincide with orthogonal realizations of growth. By orthogonal, we mean events that are outside the leader's control. In Column (1), we include a country-specific linear trend term. Growth-positive autocrats now occur 1.8% of the time approximately, although this result is not statistically significant ($p = 0.188$); while growth-negative autocrats remain more frequent than expected. In Column (2), we account for business cycles by removing the cyclicality of the growth rate. We do so by including the first four lags of the growth rate as control variables, as recently advocated by Hamilton (2017). We use Hamilton's parsimonious method rather than a more complex filter, like the Hodrick-Prescott filter, since Hamilton documents the presence of spurious cycles in the latter. Neither trends nor cycles explain away the rarity of growth-positive autocrats and abundance of growth-negative autocrats.

In Column (3), we account for trade shocks by including the average growth rate in trading partners, weighted by trade shares, in our regression, to account for network effects in the global economy, following Acemoglu, Johnson, Robinson, and Yared (2008). Our rationale is that a nation's growth tends to decline when its trading partners are doing worse. Thus, we effectively assess whether the possibility that autocracies may be more prone to "importing" recessions explains our results. We find no evidence to suggest that this is the case.

In Columns (4)–(6), we consider commodity price shocks as a source of exogenous realizations of growth. For this, we rely on a database of commodity prices collected by Bazzi and Blattman (2014). Since the primary goal of their study was to examine the link between income shocks and armed conflict, they collect data for all countries in Africa, the Middle East, Asia and Latin America over the 1957–2007 period. For each country-year, Bazzi and Blattman (2014) report export prices and outputs for 65 commodities (see their paper for a list of commodities). The price shock indicator is constructed as the average of all commodity prices weighted by their respective lagged export shares. This indicator allows us to precisely capture the effect of world price shocks on exporting countries; for example, holding all else equal, a large increase in the price of oil will act as an exogenous positive income shock to oil-rich Algeria, but not to oil-less Morocco. Column (4) presents the baseline estimates, including all commodities. Many well-performing autocracies, especially in the Middle East, are oil-rich; in Column (5), we therefore focus on oil and gas price shocks, ignoring all other commodities. The results are virtually identical to those in Column (4).

In Column (6), we explicitly address the issue of market power in world commodity markets. Our commodity price shock regressions have so far ignored that individual leaders can have a hand in manipulating world prices if their countries produce a substantial amount of the world supply. Bazzi and Blattman (2014) construct an alternate commodity price shock indicator which is designed to tackle this issue. This alternate indicator "omit(s) from a nation's price shock any products where they produce more than a 10 percent share of global exports" (p. 8). Our results stand as above.

Table 10
Orthogonal realizations of growth.

	Dependent variable: g_{it}					
	(1) Control: trend	(2) Control: cycle	(3) Control: g_{it}^F	(4)	(5) Commodity price Shocks	(6)
				All commodities	Oil and gas	No price-makers
	(a) H_0: autocrat dummies jointly insignificant					
Prob. $> \chi^2$	0.00	0.00	0.00	0.00	0.00	0.00
	(b) Mean autocrat effect β_z					
Point estimate	−0.699	−0.007	−0.009	−0.009	−0.009	−0.009
t-Statistic	−4.364	−4.864	−3.778	−4.149	−4.151	−4.102
	(c) $\beta > 0$					
Frequency	0.018	0.031	0.041	0.036	0.036	0.033
p-Value	0.188	0.446	0.116	0.285	0.285	0.418
	(d) $\beta < 0$					
Frequency	0.077	0.074	0.074	0.075	0.075	0.069
p-Value	0.000	0.000	0.000	0.000	0.000	0.002
N. autocrats	661	556	393	334	334	334
Countries	133	133	82	91	91	91
Observations	8431	7016	5479	3960	3960	3960
Time period	1858–2010	1862–2010	1858–2010	1958–2007	1958–2007	1958–2007
R^2	0.29	0.25	0.29	0.23	0.23	0.23

Notes. Panel least squares estimates with year fixed effects. g_{it} is the growth rate of real per capita GDP in country *i* and year *t*. Column (1) includes a country-specific time trend. Column (2) accounts for cyclicality by controlling for the first four lags of g_{it}, following Hamilton (2017). Column (3) includes g_{it}^F, the trade share-weighted growth rate of trading partners (see Eq. (5)). Columns (4)–(5) include the relevant commodity price shock indicators from Bazzi and Blattman (2014). Column (6) excludes the commodities for which a country produces > 10% of the world's export from that country's commodity price shock indicator. An autocrat is growth-positive (negative) if growth during her tenure is significantly larger (smaller) than 0. All specifications include a constant term. See Appendix for variable definitions and sources.

Further robustness checks

In Table 11, we perform additional analyses on the sensitivity of our results to a number of other important considerations. First, in Column (1), we ask which of autocrats and democrats are better placed to secure property rights. This variable (see Appendix Table A.2 for definitions and sources) measures the degree to which government enforces citizens' rights to private property. As is well-known, secure property rights provide incentives to invest in physical capital, which is important for economic growth. Some autocratic regimes have secure property rights, like Singapore for example, so it is important to understand whether pro-property rights autocrats are found frequently. The results indicate that they are not; however, autocrats with negative effects on property rights are found more commonly than chance would predict ($p = 0.034$).

In Column (2), we check whether our results are confounded by the durability of the regime, which is measured as the number of years a given regime is in place, following the Polity IV project. If political instability, rather than autocracy itself, is responsible for the rarity of growth-positive autocrats, then including regime durability in our specification should remove this source of bias. On the other hand, as shown by Jong-A-Pin and De Haan (2011), growth accelerations are less likely to occur in longer-established regimes, which also could explain why growth-positive autocrats are so rare in our sample. Our results remain unchanged when we control for regime durability.

We control for colonial origin in Column (3), as different colonizers likely left very different institutional arrangements which can subsequently affect growth. We include a dummy variable for each of the following colonial powers: the United Kingdom, France, Spain, the Netherlands, Italy, the United States, Portugal, and Belgium. There is no evidence to suggest that our results were previously confounded by the omission of these variables.

In Column (4), we check whether the changing composition of the autocratic and democratic group of countries over time affects the results. We include the share of autocracies in the world in the current year as an additional regressor. This variable is defined as $s_t = N_{autocracies}/N_{countries}$, where a country is an autocracy if $Polity2 < 6$. Since this variable only exhibits time variation but no cross-sectional variation, we omit the year fixed effects from Column (4). Empirically, the changing composition of the autocratic sample does not appear to matter, as our results remain unchanged.

Finally, in Columns (5) and (6), we consider government spending, as a percentage of GDP, on two non-economic outcomes: health and education. Our rationale for doing so is that these are outcomes that matter a great deal, especially in developing countries, and also may not translate immediately into economic growth. An autocrat may be spending large sums on health and education in order to lay out strong foundations for economic growth, which our previous estimates would not capture. We find no evidence to suggest "benevolence" with respect to these two outcomes, either: high spenders are found as frequently as chance would predict, and low spenders are found far more frequently.

Accounting for local trends in growth rates

Motivation and empirical framework

So far, we shown that growth-positive autocrats are, at best, only as frequent as one would expect by chance. We have also shown that this pattern is robust to the use of alternate economic outcomes, definitions of political regimes, and many other considerations. In this section, we take a closer look at those leaders we found to be growth-positive and growth-negative in our baseline results of Table 4. Since Column (5) has one more growth-positive autocrat than Column (1), we use leaders from the former specification as our sample of interest. These leaders,

Table 11
Further robustness checks.

	(1) DV: property rights	(2) Control: regime durability	(3) Control: colonial origin	(4) Control: % autocratic in world	(5) DV: gov. spending	(6)
					Health	Education
			(a) H_0: autocrat dummies jointly insignificant			
Prob. $> \chi^2$	0.00	0.00	0.00	0.00	0.00	0.00
			(b) Mean autocrat effect β_z			
Point estimate	−0.118	−0.010	−0.005	−0.008	−0.010	−0.005
t-Statistic	−7.106	−5.272	−2.918	−3.992	−8.052	−4.418
			(c) $\beta > 0$			
Frequency	0.045	0.035	0.036	0.033	0.033	0.038
p-Value	0.319	0.169	0.121	0.236	0.614	0.348
			(d) $\beta < 0$			
Frequency	0.080	0.068	0.061	0.067	0.067	0.093
p-Value	0.034	0.000	0.000	0.000	0.070	0.000
N. autocrats	112	660	661	661	120	182
Countries	128	133	133	133	129	131
Observations	1808	8412	8431	8431	1909	2189
Time period	1995–2010	1858–2010	1858–2010	1858–2010	1995–2010	1970–2010
R2	0.35	0.26	0.26	0.18	0.29	0.29

Notes. Panel least squares estimates with year fixed effects. The dependent variable in Columns (2)–(4) is g_{it}, the growth rate of real per capita GDP in country *i* and year *t*. An autocrat is growth-positive (negative) if growth during her tenure is significantly larger (smaller) than 0. All specifications include a constant term. See Appendix for variable definitions and sources.

along with their year of entry into power, are displayed in Tables A.11 and A.12 in the Appendix.

Our objective in this section is to determine whether growth-positive autocrats, infrequent as they are, can be attributed the good growth of their countries during their tenures. In particular, it could be that nations select particularly good leaders in difficult economic times, in which case growth-positive autocrats, although they are infrequent, really do deserve credit for turning around their country's fate. More generally, it is plausible that pre-existing trends in the data lead us to either under-state or over-state the importance of those autocrats we found to be growth-positive or growth-negative. This is the empirical question with which this section is concerned.

The entry of a growth-positive autocrat into power can be described as a discrete treatment the country receives at date d. Starting from this premise, we can therefore investigate whether growth is systematically different following treatment (a positive treatment effect). We estimate this treatment effect using a regression discontinuity design (RDD) centred around d, the year of arrival into power of growth-positive autocrats. The RDD approach compares outcomes (rates of economic growth) just after the cut-off date to outcomes just before the cut-off date. The intuition is that units just below the cut-off are a good counterfactual for units just above the cut-off, as illustrated by Thistlethwaite and Campbell (1960). In their seminal paper, they estimate the treatment effect of scholarships on scholarship recipients by comparing the career outcomes of (i) individuals whose grades were immediately above the minimum required for a scholarship, to (ii) individuals whose grades were just short of the minimum required for a scholarship.[15]

The RDD approach stipulates that observational units are assigned to treatment based on a cut-off, while all other factors vary smoothly. This set-up mirrors the entry of autocrats into power: a nation's political leadership changes sharply at $d = 0$, while other factors are, to a first-order approximation, varying smoothly across the cut-off. If growth-positive autocrats have a positive treatment effect, then growth within their countries should vary systematically in the post-treatment period relative to the pre-treatment period. Therefore, we are interested in estimating a within-country treatment effect, which is applied at different times in different countries. In order to account for differences between countries and differences in treatment dates, we first regress GDP growth rates on a set of country and year fixed effects. The residuals from this regression, which we call G_{it}, capture rates of economic growth that are not explained by country and year effects. Our approach estimates the treatment effect of growth-positive autocrats on G_{it} in the neighbourhood of the treatment date d. Econometrically, our model takes the following form:

$$G_{it} = \alpha_0 + \gamma\ Treatment_{it} + f(T_{it}) + \mu_{it} \tag{7}$$

where the parameter of interest, γ, is the treatment effect, and $f(T_{it})$ is a smooth function of time, which is indexed by T. Time serves as the assignment variable in this set-up: Treatment is a dummy variable equal to one if $T \geq d$ and zero otherwise. The treatment effect γ is therefore the difference in outcomes above and beyond differences that are due to the time variation, which are captured by $f(T_{it})$. Following Hahn, Todd, and Van der Klaauw (2001), we estimate separate local polynomials for $f(T_{it})$ on each side of $d = 0$. This is because the spirit of RDD estimation is to compare conditional expectations for the outcome variable when approaching from below the cut-off ($d < 0$), to conditional expectations when approaching from above. Lee and Lemieux (2010, p. 319) therefore recommend against imposing the restriction that the slope of $f(T_{it})$ is the same across the cut-off, as this would mean data from above the cut-off is used to estimate the limit, approaching from below, of the conditional expectation of G_{it}.

A central assumption behind the validity of the regression discontinuity design is that observational units should not be able to precisely manipulate their assignment status (Lee & Lemieux, 2010, p. 283). This assumption appears to be justified in this instance: while regression discontinuity designs often deal with policy interventions to which individuals or firms can choose to sign up, thereby introducing a classical selection problem, in this paper we are comparing the time

[15] For a comprehensive review of RDD applications in economics, see Lee and Lemieux (2010).

path of economic growth prior to and after the entry of particular political leaders only in those countries which were treated. Therefore, we see no obvious reason why sample selection should be a grave concern in this particular application of the RDD approach.

Bandwidth selection

Since our approach exploits a discontinuity in time, we are wary of using observations that are too far from the cut-off date (Hausman & Rapson, 2018). Because RDD estimation relies on comparing outcomes just above the cut-off date to outcomes just below the cut-off date, we limit our observation windows to five or ten years on either side of d. The results obtained under either scenario are consistent. In addition to limiting the number of observations available for estimation, we also pay careful consideration to bandwidth selection. The RDD literature refers to the choice of observations on each side of the cut-off as "bandwidth," and offers practical advice for bandwidth selection.

The central idea to optimally select a bandwidth is that there is a trade-off between bias and precision (Lee & Lemieux, 2010, p. 320). On one hand, larger bandwidths are more likely to yield precise estimates, as they make use of more observations. On the other hand, larger bandwidths may also suffer from larger biases, as they use observations that are further away from the cut-off, which is inconsistent with what the RDD estimator sets out to accomplish (comparing limits of the conditional expectation for the outcome variable from above and below the cut-off). An optimal bandwidth selection is therefore one that achieves balance between precision and bias.

For this purpose, the econometrics literature has suggested two main alternative bandwidth selectors; we implement both below. Imbens and Kalyanaraman (2012) propose a data-driven rule for a choice of bandwidth in order to minimize the mean squared error of the treatment effect. Their bandwidth selector is regularized to account for small samples sizes. Building from Imbens and Kalyanaraman (2012), Calonico, Cattaneo, and Titiunik (2014a) propose a different bandwidth selector with more attractive statistical properties, including consistent preliminary bandwidths, which are used in the computation of the confidence intervals. Calonico et al. (2014a); Calonico, Cattaneo, and Titiunik (2014b) also propose a bias correction for the conventional RD estimator, as well as a procedure for calculating robust confidence intervals for the bias-corrected estimator. The objective of the bias correction is to account for the fact that local polynomial estimators tend to perform poorly in Monte Carlo simulations in finite samples (see Calonico et al., 2014a for more details). We implement all of these guidelines in our estimates below.

Results

Enumerating all combinations of bandwidths (Calonico-Cattaneo-Titiunik vs. Imbens-Kalyanaraman), RD estimators (conventional/bias-corrected/bias-corrected with robust CIs), and windows of observation (five vs. ten years), we obtain twelve treatment effects for growth-positive autocrats (Panel (a) of Table 12). In all specifications, we fail to reject the null hypothesis that growth is not different across the entry date of these autocrats. Thus, we find no evidence to suggest that these leaders had any kind of positive growth effects. Rather, our RDD estimates suggest that these leaders may have simply found themselves at the right place and at the right time, and ended up "riding the wave" of previous economic success.

In Panel (b), we apply the same level of scrutiny to growth-negative autocrats from Column (5) of Table 4. After all, if growth-positive autocrats tend to ride positive waves, then growth-negative autocrats may conversely find themselves at the wrong place at the wrong time. This view is not borne out by the data: we strongly reject the null hypothesis that growth-negative autocrats have no effect in all but one specification. Our RDD estimates therefore show that the entry of this particular group of leaders into power is accompanied by significantly lower growth rates in the post period relative to the pre period. This is in stark contrast to our lack of significance from Panel (a). Our results suggest that poor economic performance is systematically associated with the person in power in autocracies; good economic performance is not.

Table 12
Regression discontinuity design estimates.

	(1)	(2)	(3)	(4)
	$d \pm 5$ *years*		$d \pm 10$ *years*	
Bandwidth	CCT	IK	CCT	IK
(a) Growth-positive autocrats				
Treatment effect				
Conventional	0.061	0.061	0.033	0.034
	[0.048]	[0.048]	[0.030]	[0.029]
Bias-corrected	0.047	0.047	0.028	0.041
	[0.048]	[0.048]	[0.030]	[0.029]
Robust	0.047	0.047	0.028	0.041
	[0.033]	[0.033]	[0.039]	[0.047]
N selected	74	74	133	133
N available	161	161	287	287
Power	0.969	0.969	0.999	0.999
(b) Growth-negative autocrats				
Treatment effect				
Conventional	−0.066**	−0.066**	−0.085***	−0.083***
	[0.030]	[0.030]	[0.023]	[0.020]
Bias-corrected	−0.065**	−0.065**	−0.095***	−0.096***
	[0.030]	[0.030]	[0.023]	[0.020]
Robust	−0.065***	−0.065	−0.095***	−0.096***
	[0.021]	[0.043]	[0.030]	[0.026]
N selected	112	112	202	202
N available	244	244	460	460
Power	0.986	0.986	0.999	0.999

Notes. Regression discontinuity design estimates. *d* refers to the treatment date. Under the null hypothesis, the treatment effect is equal to zero, such that the local trends on either side of *d* are identical. CCT: Calonico et al. (2014a) optimal bandwidth. IK: Imbens and Kalyanaraman (2012) optimal bandwidth. ***, ** and * denote statistical significance at the 1, 5 and 10% levels respectively.

Statistical power

A potential concern with our RDD estimates is that, since they rely on a significantly smaller number of observations than our panel estimates, they may be affected by a non-trivial amount of noise in the data. If this is the case, then the likelihood of type II error increases, meaning the RDD estimates may, mistakenly, fail to reject the null hypothesis. This is highly relevant in this context, since in Panel (a) of Table 12, we systematically fail to reject the null hypothesis that the growth-positive leaders of Table 4 Column (5) have zero effect.

We therefore perform power calculations for our RDD estimates and report them in the lower section of each panel. Examining statistical power is a "critical parameter in assessing the scientific value of an empirical study" (Ioannidis, Stanley, & Doucouliagos, 2017, p. 236). Power ranges between 0 and 1 and is formally defined as the probability of finding an effect where one does exist. Power is a function of the sample size, desired significance level (we adopt $\alpha = 0.05$ here) and standardizedeffect size. We calculate standardized effect sizes according to Cohen's (1992) d.[16]

For a statistical test to be considered sufficiently powered, the convention in the social sciences (Cohen, 1965) is that the likelihood of

[16] Letting ρ denote treatment status, Cohen's d is equal to $\frac{|\overline{G}_{\rho=1} - \overline{G}_{\rho=0}|}{\sqrt{(\sigma^2_{\rho=1} + \sigma^2_{\rho=0})/2}}$, where $\overline{G}$ is the mean value of G_{it} in the group of interest.

type II error should be no more than four times as large as the likelihood of type I error. The latter, rejecting the null when it is false, is given by $\alpha = 0.05$. Accommodating a probability of type II error less than or equal to 0.20, a test is therefore considered sufficiently powered if the statistical power exceeds 0.8. The powers we report in Table 12 are all > 0.96, indicating that all of our tests are sufficiently powered. The difference in significance levels across panel (a) and panel (b) is therefore highly unlikely to be spurious.

Concluding remarks

We have provided a systematic assessment of the benevolent autocrat hypothesis and documented three main empirical regularities. First, autocratic leaders with positive effects are found, at best, only as frequently as one would expect due to chance alone. This pattern holds at the extensive margin, when we use rates of economic growth as our dependent variable, but also at the intensive margin, when the likelihood of economic expansions and recessions are used as outcome variables of interest. This pattern is further confirmed for alternate economic outcomes, including unemployment rates and TFP growth. We also find no evidence of an autocratic "bliss point": when compared to leaders in more democratic regimes, autocratic leaders were found to have positive effects only as frequently as chance predicted. We also provided evidence showing that our results are unlikely to be driven by the endogeneity of leadership transitions, adverse economic events which happen to coincide with leader tenures, leaders who serve only few years in office, or time lags.

Second, in stark contrast, autocratic leaders with negative effects are found far more frequently that chance would predict. This result is also remarkably robust: under the different scenarios we considered, as described above, the largest p-value we found for the frequency of negative effects was 0.118, out of 68 p-values estimated. Thus, we document a robust asymmetric pattern: growth-positive autocrats are at best as rare as chance would predict, and sometimes even more rare, while growth-negative autocrats abound.

Third, we examined whether growth-positive and growth-negative autocrats are simply individuals who fortuitously found themselves on the right or wrong side of history. We implemented regression discontinuity designs to study whether growth follows a significantly different path prior to and after the assumption of political power by leaders who would then go on to record positive or negative growth. There, we again found a strong asymmetric pattern. We systematically failed to reject the null hypothesis that growth trends were similar across the entry dates of growth-positive leaders, but we did systematically reject the analogous hypothesis for growth-negative leaders. The data therefore suggest that autocrats with positive growth are largely "riding the wave" of previous economic success. On the other hand, the assumption of power of growth-negative autocrats appears to be largely associated with marked declines in growth.

In the era of (democratically elected) revered leaders with authoritarian tendencies like Donald Trump, Vladimir Putin or Recep Tayyip Erdogan, we hope our results illustrate that beliefs in benevolent autocrats are fundamentally misguided. As compelling a narrative as it may be, the benevolent autocrat hypothesis simply does not stack up against the evidence. We are hopeful that our results help provide pause for thought, before endorsing any narrative at face value.

One limitation of our research is that we do not directly identify which leaders may have had truly benevolent motives. While our empirical approach cannot observationally distinguish between benevolent and strategically pro-growth autocrats, we can reasonably interpret the lack of evidence for the two types of leaders combined as lack of evidence for the benevolent sub-type, if it exists at all. How to reliably identify leader motives is a difficult but promising empirical task, and a potentially fruitful one for future research to explore. Since beliefs in benevolent autocrats appear to be widespread, this raises the question of how to remedy those beliefs. In the current era of misinformation and post-truth politics (Suiter 2016), finding an accurate answer to this question may help consolidate the health of democratic societies.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.leaqua.2019.06.003.

References

Acemoglu, D. (2008). Oligarchic versus democratic societies. *Journal of the European Economic Association, 6*(1), 1–44.

Acemoglu, D., Johnson, S., Robinson, J. A., & Yared, P. (2008). Income and democracy. *American Economic Review, 98*(3), 808–842.

Acemoglu, D., Naidu, S., Restrepo, P., and Robinson, J. A. Democracy does cause growth. Journal of Political Economy.

Acemoglu, D., Verdier, T., & Robinson, J. A. (2004). Kleptocracy and divide-and-rule: A model of personal rule. *Journal of the European Economic Association, 2*(2–3), 162–192.

Bazzi, S., & Blattman, C. (2014). Economic shocks and conflict: Evidence from commodity prices. *American Economic Journal: Macroeconomics, 6*(4), 1–38.

Bertrand, M., & Schoar, A. (2003). Managing with style: The effect of managers on firm policies. *The Quarterly Journal of Economics, 118*(4), 1169–1208.

Besley, T., & Kudamatsu, M. (2008). Making autocracy work. In E. Helpman (Ed.). *Institutions and economic performance*. Cambridge, MA: Harvard University Press.

Besley, T., Montalvo Jose, G., & Reynal-Querol, M. (2011). Do educated leaders matter? *The Economic Journal, 121*(554), F205–F227.

Bhagwati, J. N. (1982). Directly unproductive, profit-seeking (DUP) activities. *Journal of Political Economy, 90*(5), 988–1002.

Bolt, J., & Zanden Jan, L. (2014). The Maddison project: Collaborative research on historical national accounts. *The Economic History Review, 67*(3), 627–651.

Broadberry, S., & Wallis, J. J. (2017). *Growing, shrinking, and long run economic performance: Historical perspectives on economic development. NBER working paper no. 23343*.

Brückner, M., & Ciccone, A. (2011). Rain and the democratic window of opportunity. *Econometrica, 79*(3), 923–947.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014a). Robust data-driven inference in the regression-discontinuity design. *Stata Journal, 14*(4), 909–946.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014b). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica, 82*(6), 2295–2326.

Cohen, J. (1965). *Statistical power analysis for the behavioral sciences.* Mahwah, NJ: Lawrence Erlbaum Associates.

Cohen, J. (1992). Statistical power analysis. *Current Directions in Psychological Science, 1*(3), 98–101.

Coolidge, J., & Rose-Ackerman, S. (2000). Kleptocracy and reform in African regimes: Theory and examples. *Corruption and Development in Africa* (pp. 57–86). London: Palgrave Macmillan.

De Haan, J. (2007). Political institutions and economic growth reconsidered. *Public Choice, 131*(3-4), 281–292.

De Hoogh, A. H., Greer, L. L., & Den Hartog, D. N. (2015). Diabolical dictators or capable commanders? An investigation of the differential effects of autocratic leadership on team performance. *The Leadership Quarterly, 26*(5), 687–701.

De Luca, G., Litina, A., & Sekeris, P. G. (2015). Growth-friendly dictatorships. *Journal of Comparative Economics, 43*(1), 98–111.

Doucouliagos, C., & Ulubasoglu, M. A. (2006). Economic freedom and economic growth: Does specification make a difference? *European Journal of Political Economy, 22*(1), 60–81.

Doucouliagos, H., & Ulubasoglu, M. A. (2008). Democracy and economic growth: A meta-analysis. *American Journal of Political Science, 52*(1), 61–83.

Dreher, A., Lamla, M. J., Lein, S. M., & Somogyi, F. (2009). The impact of political leaders' profession and education on reforms. *Journal of Comparative Economics, 37*(1), 169–193.

Easterly, W. (2011). *Benevolent Autocrats, New York University Development Research Institute Working Paper No. 76*.

Easterly, W., & Pennings, S. (2017). *Shrinking dictators: How much economic growth can we attribute to national leaders? DRI working paper no. 94*.

Edgerton, R. B. (2002). *The troubled heart of Africa: A history of the Congo.* New York: St. Martin's Press.

Fair, R. C. (1978). The effect of economic events on votes for president. *The Review of Economics and Statistics, 60*(2), 159–173.

Fair, R. C. (1982). The effect of economic events on votes for president: 1980 results. *The Review of Economics and Statistics, 64*(2), 322–325.

Fair, R. C. (1988). The effect of economic events on votes for president: 1984 update. *Political Behavior, 10*(2), 168–179.

Foa, R. S., & Mounk, Y. (2016). The danger of deconsolidation: The democratic disconnect. *Journal of Democracy, 27*(3), 5–17.

Freund, C., & Jaud, M. (2013). On the determinants of democratic transitions. *Middle East Development Journal, 5*(1), 1350005-1.

Glaeser, E. L., La Porta, R., Lopez-de Silanes, F., & Shleifer, A. (2004). Do institutions cause growth? *Journal of Economic Growth, 9*(3), 271–303.

Goemans, H. E., Gleditsch, K. S., & Chiozza, G. (2009). Introducing archigos: A dataset of political leaders. *Journal of Peace Research, 46*(2), 269–283.

Goemans, H. E., Gleditsch, K. S., & Chiozza, G. (2016). *Archigos 4.1: A dataset of political leaders.* 1875–2015.
Gooch, E. (2017). Estimating the long-term impact of the great Chinese famine (1959–61) on modern China. *World Development, 89*, 140–151.
Gründler, K., & Krieger, T. (2016). Democracy and growth: Evidence from a machine learning Indicator. *European Journal of Political Economy, 45*, 85–107.
Hahn, J., Todd, P., & Van der Klaauw, W. (2001). Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica, 69*(1), 201–209.
Hamilton, J. (2017). *Why you should never use the Hodrick-Prescott filter. NBER working paper no. 23429*.
Harms, P. D., Wood, D., Landay, K., Lester, P. B., & Lester, G. V. (2018). Autocratic leaders and authoritarian followers revisited: A review and agenda for the future. *The Leadership Quarterly, 29*(1), 105–122.
Hausman, C., & Rapson, D. S. (2018). Regression discontinuity in time: Considerations for empirical applications. *Annual Review of Resource Economics, 10*, 533–552 Jun.
Heinl, R. D., Heinl, N. G., Heinl, M., & Lanham, M. D. (1996). *Written in blood : the story of the Haitian people, 1492-1995*. University Press of America.
Hewlett, S. A. (1979). Human rights and economic realities: Tradeoffs in historical perspective. *Political Science Quarterly, 94*(3), 453–473.
Hiller, N. J., Sin, H. P., Ponnapalli, A. R., & Ozgen, S. (2018). Benevolence and authority as WEIRDly unfamiliar: A multi-language meta-analysis of paternalistic leadership behaviors from 152 studies. *The Leadership Quarterly.*.
Hodler, R., & Raschky, P. A. (2014). Regional Favoritism. *The Quarterly Journal of Economics, 129*(2), 995–1033.
Huntington, S. P. (1968). *Political order in changing societies.* New Haven, CT: Yale University Press.
Imbens, G., & Kalyanaraman, K. (2012). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of Economic Studies, 79*(3), 933–959.
Ioannidis, J. P. A., Stanley, T. D., & Doucouliagos, H. (2017). The power of bias in economics research. *The Economic Journal, 127*(605), F236–F265.
Johnson, W. D., Beyl, R. A., Burton, J. H., Johnson, C. M., Romer, J. E., & Zhang, L. (2015). Use of Pearson's chi-square for testing equality of percentile profiles across multiple populations. *Open Journal of Statistics, 5*(5), 412–420.
Jones, B. (2008). National leadership and economic growth. In S. N. Durlauf, & L. E. Blume (Eds.). *New Palgrave dictionary of economics*. London: Palgrave Macmillan.
Jones, B., & Olken, B. (2005). Do leaders matter? National leadership and growth since world war II. *Quarterly Journal of Economics, 120*(3), 835–864.
Jong-A-Pin, R., & De Haan, J. (2011). Political regime change, economic liberalization and growth accelerations. *Public Choice, 146*(1-2), 93–115.
Lee, D. S., & Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature, 48*(2), 281–355.
Lesiie, W. (1987). *The World Bank and structural transformation in developing countries: The case of Zaire.* Boulder, CO: Lynne Rienner Publishers.
Li, W., & Yang, D. T. (2005). The great leap forward: Anatomy of a central planning disaster. *Journal of Political Economy, 113*(4), 840–877.
Lipset, S. M. (1959). Some social requisites of democracy: Economic development and political legitimacy. *The American Political Science Review, 53*(1), 69–105.
Liu, C., & Zhou, L.-A. (2017). *Estimating the short- and long-term effects of Mao Zedong's economic radicalism*.
Londregan, J., & Poole, K. (1990). Poverty, the coup trap, and the seizure of executive power. *World Politics, 42*(2), 151–183.
Madsen, J. B., Raschky, P. A., & Skali, A. (2015). Does democracy drive income in the world, 1500–2000? *European Economic Review, 78*, 175–195.
Mansur, J., Sobral, F., & Goldszmidt, R. (2017). Shades of paternalistic leadership across cultures. *Journal of World Business, 52*(5), 702–713.
Marshall, M. G., & Gurr, T. R. (2014). *Polity IV project: Political regime characteristics and transitions, 1800–2013.* Center for Systemic Peace.
Méon, P.-G., & Sekkat, K. (2016). *A time to throw stones, a time to reap: How long does it take for democratic transitions to improve institutional outcomes? Working papers CEB 16-016.* ULB - Université Libre de Bruxelles.
Murtin, F., & Wacziarg, R. (2014). The democratic transition. *Journal of Economic Growth, 19*(2), 141–181.
Olson, M. (1993). Dictatorship, democracy, and development. *The American Political Science Review, 87*(3), 567–576.
Papaioannou, E., & Siourounis, G. (2008). Democratisation and growth. *The Economic Journal, 118*(532), 1520–1551.
Persson, T., & Tabellini, G. (2006). Democracy and development: The devil in the details. *American Economic Review, 96*(2), 319–324.
Rodrik, D., & Wacziarg, R. (2005). Do democratic transitions produce bad economic outcomes? *American Economic Review, 95*(2), 50–55.
Schyns, B., & Schilling, J. (2013). How bad are the effects of bad leaders? A meta-analysis of destructive leadership and its outcomes. *The Leadership Quarterly, 24*(1), 138–158.
Sidorkin, O., & Vorobyev, D. (2018). Political cycles and corruption in Russian regions. *European Journal of Political Economy, 52*, 55–74.
Sirowy, L., & Inkeles, A. (1990). The effects of democracy on economic growth and inequality: A review. *Studies in Comparative International Development, 25*(1), 126–157.
Tavares, J., & Wacziarg, R. (2001). How democracy affects growth. *European economic review, 45*(8), 1341–1378.
Thistlethwaite, D. L., & Campbell, D. T. (1960). Regression-discontinuity analysis: An alternative to the ex post facto experiment. *Journal of Educational Psychology, 51*(6), 309–317.
von Hayek, F. (1960). *The constitution of liberty.* Chicago: University of Chicago Press.
Weber, R., Camerer, C., Rottenstreich, Y., & Knez, M. (2001). The illusion of leadership: Misattribution of cause in coordination games. *Organization Science, 12*(5), 582–598.
Yao, Y., & Zhang, M. (2015). Subnational leaders and economic growth: Evidence from Chinese cities. *Journal of Economic Growth, 20*(4), 405–436.
Young, C., & Turner, T. (1985). *The rise and decline of the Zairian State.* Madison: University of Wisconsin Press.