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Political entrenchment and GDP misreporting

Abstract

By examining discrepancies between officially reported GDP growth figures and the actual economic growth implied by satellite-based night time light (NTL) density, we investigate whether democracies manipulate officially reported GDP figures, and if so, whether such manipulation pays political dividends. We find that the over-reporting of growth rates does indeed precede increases in popular support, with around a 1% over-statement associated with a 0.5% increase in voter intentions for the incumbent. These results are robust to allowing the elasticity of official GDP statistics to NTL to be country specific, as well as accounting for the quality of governance, and checks and balances on executive power.

JEL-Codes: D720, D730, O430.

Keywords: manipulation, political entrenchment, electoral cycles, trust, popular support, GDP, night lights.

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I only believe in statistics that I doctored myself.

Winston Churchill

1 Introduction

The evidence that high growth rates in the two years preceding US presidential elections increase the incumbent's odds of re-election (Fair 1978, 1982, 1988) supports the view that high economic growth serves to entrench incumbent governments. In fact, both democratic and autocratic leaders around the world are more secure in their power when growth rates are high (Burke and Leigh 2010; Treisman 2015). This voter tendency to base election decisions on official GDP reports thus gives incumbent governments an incentive to manipulate GDP statistics. As Campbell (1976) points out, "[t]he more any quantitative social indicator is used for social decision-making, the more subject it will be to corruption pressures and the more apt it will be to distort and corrupt the social processes it is intended to monitor" (p. 49).

Motivated by the incentives incumbents face to misreport GDP in order to secure re-election, we examine the link between popular support and GDP manipulation between 1992 and 2008 in 132 countries that account for over 90% of the world population. To do so, we combine official GDP statistics with satellite images of night time light density (NTL)[1] as an unbiased proxy for economic activity (cf. Henderson *et al.* 2012; Chen and Nordhaus 2011). After first documenting a systematic correlation between popular support and the size of the discrepancy between night time light (NTL) based growth predictions and government-reported official growth rates, we show that this correlation is driven entirely by the manipulation that tends to precede increases in popular support. We then use voting intention data to test whether manipulation of GDP reporting does indeed precede increased intention to vote for the incumbent. We find that a 1% over-statement of economic growth is followed by an approximately 0.5% increase in the population share intending to vote for the incumbent were an election to be held immediately, which suggests that manipulation pays political dividends. On the other hand, we find no evidence that incumbents manipulate reported growth as a result of poor popular support. It is especially worth noting that this pattern is (1) observable only in democratic regimes (i.e., those where elections are meaningful) and not in autocracies, and (2)

[1] The use of night light density has gained considerable traction in economics research over recent years (see, e.g., Hodler and Raschky 2014, De Luca *et al.* 2018, Michalopoulos and Papaioannou 2013, 2014, Amarasinghe *et al.* 2018, Dreher *et al.* 2018, Campante and Yanagizawa-Drott 2017, Castells-Quintana 2017, Weidmann and Schutte 2017).

not attributable to any systematic differences in governance quality across regime types, which are explicitly controlled for in our empirical estimates. We do observe, however, that countries with higher levels of interpersonal trust are more prone to fall prey to manipulation.

Our work contributes to a nascent literature that uses NTL imagery to assess the manipulation of GDP figures. The research to date, however, focuses almost exclusively on evaluating the degree of GDP manipulation in autocracies relative to democracies, with the pioneering work of Magee and Doces (2015) suggesting an approximately 1% over-statement of official growth figures in the former relative to the latter. Remarkably robust effects on the same order of magnitude are also reported by Martinez (2018), who additionally allows the response of GDP to night lights to be regime specific. We depart from the focus of these two studies both conceptually and methodologically: First, in examining the link between manipulation and popular support across all regime types, we focus not on estimating the extent to which autocracies overstate GDP figures relative to democracies but on how popular support relates to GDP manipulation. Second, given Roger's (2018) empirical evidence that the relation between official GDP statistics and NTL density varies widely across countries, we allow this relationship to be country specific, resulting in markedly more conservative manipulation estimates than in either Magee and Doces (2015) or Martinez (2018). We also limit our definition of manipulation to the discrepancies between reported growth and NTL-based growth predictions that are not captured by country-specific elasticities of NTL to GDP, including electrification rates, urbanization, absolute latitude, and any other country-specific unobservables.

2 Background

The newly emerging field of forensic economics (Zitzewitz 2012) engages in the general task of uncovering hidden behaviour in multiple domains, ranging from teachers cheating on exams, violations of international sanctions, and unnecessary surgeries to unfair judging of sports events. Some of this research, for example, documents manipulation of weather reports to attract more visitors (Zinman and Zitzewitz 2016), a special form of the deceptive advertising and practices to cheat consumers and citizens that have long been a concern in economics (e.g., Galbraith 1958, Akerlof and Shiller 2015). The reasons for resorting to such behaviour are widely covered in the extensive management science literature on earnings management (e.g., Pfaff and Ising 2010, Efendi *et al.* 2007, Bergstresser and Philippon 2006, Roychowdhury 2006). For instance, Chen *et al.* (2015), in a study of US public firms from 2004 to 2008, shows that interim CEOs are more likely than non-interim CEOs to manipulate bookkeeping and accounting figures to boost a firm's earnings performance and thus improve their chances of being promoted to a permanent position. One particularly interesting aspect is government manipulation of data, a topic rarely addressed in economics with the exception of a few important studies, including Dafflon and Rossi (1999), Forte (2001), Milesi-Ferretti (2005), Koen and van den Noord (2005), von Hagen und Wolf (2006), Irwin (2012a,b), and Barnett (2012), who investigate public accounting "fudges" related to the EU's Economic and Monetary Union (EMU), the size of public debt, fiscal gimmickry in Europe, monetary statistics undermining the U.S. Fed, and accounting devices to cover fiscal deficits, respectively.

Despite such examples, however, few in-depth empirical analyses exist of specific cases of data manipulation, with the rare example of Von der Lippe's (1996, 2002) investigation of the extent of manipulation and basic differences in the statistics-politics relation in Germany's eastern Communist Democratic Republic versus its western Federal Republic. This study even documents an important member of the East German Communist Party's Central Committee directly ordering officials to falsify data, particularly with respect to foreign trade, building construction, and crime.

Historically, substantial anecdotal evidence is available that autocracies manipulate data. One of the most devastating instances was during the Republic of China's Great Leap Forward (see Harari 2015, pp. 165–66, for a discussion) when local officials, afraid to voice concerns over Mao's impossible demands for increased agricultural production, fabricated the numbers to show dramatic growth in agricultural output. As the figures moved up the bureaucratic hierarchy, they were further exaggerated until the reported annual grain production for 1958 was 50% higher than actual production. On the basis of these production figures, the government sold millions of tons of rice to foreign countries, which led to the worst famine in history, causing the deaths of around 30 million people (Ashton *et al.* 1984, p. 614). In the Soviet Union, Stalin applied such exaggeration to population size, claiming 168 million (see Acemoglu and Robinson 2012, p. 129) for what the 1937 census reported as 162 million. He responded to the census figures by arresting census officials and either sending them to Siberia or having them shot. A second census ordered by Stalin in 1939 reported a population of 171 million. Jasny (1950) details numerous other Soviet examples of statistical manipulation, including contributions of trade to national income or concealment of unfavourable data. Manipulation in Italy was uncovered by Albert Hirschman, one of few researchers able to read between the lines of official fascist data as it became increasingly difficult to understand how the economy was actually performing (Adelman 2013, p. 142). Whereas Minister of Foreign Trade Felice Guarneri claimed that the Italian economy's commercial balances were fine, Hirschman showed that tourism was in crisis and hotel bed occupancy declining (p. 160).

Nor is such deception confined to autocracies; even democracies like the UK and US (e.g. Frankel 2011) fall victim to statistical manipulation, including the fudging of crime statistics by the Derbyshire police (BBC 2013), systematic underreporting of terrorist incidents (Drakos and Gofas 2006), and demonstration participants (e.g., Opp 2011). In Europe, manipulations by countries trying to enter the European Union (EU) have also attracted considerable attention, including Greek governmental claims of a public debt to GDP ratio of 115% at the end of 2009, then 127%, and finally 145% by the close of 2010 (Baralexis 2004). In a rare move for the official statistical office of a multinational organization reluctant to antagonize any member country (Kyriakidou 2013), the EU's statistical office (EUROSTAT 2010) was forced to officially state that Greece's figures, particularly those relating to the Maastricht indicators on public finances, were wrongly produced and submitted. During an interview, EUROSTAT's General Director made it clear that these figures were purposely falsified (see Krämer 2015). Likewise, Argentina under the two Kirchner presidents was guilty of gross statistical manipulation, including an official 2011 declaration by its National Institute of Statistics and Census of an inflation rate around 10% that independent sources determined to be no less than 25% (*Economist* April 23, 2011, pp. 75, 48).

3 Data

3.1 NTL Data

The recent explosion of data availability enabled by digital technology allows monitoring of countries' economic conditions in ways that the respective governments cannot control. One such method is using raw satellite data on NTL density across the globe (e.g., Hodler and Raschky 2014) to estimate national income. These high-resolution images, available since 1992, are captured between 20:30 and 22:00 local time each day by the Defense Meteorological Satellite Program's Operational Linescan System (DMSP-OLS). Once cleaned of cloudy days and ephemeral lights, the data are averaged annually in 30 arc-second by 30 arc-second sized cells (or approximately 1 square kilometre at the equator), each of which is then assigned a digital luminosity number from 0 (least luminous) to 63 (most luminous) in increments of 1. The NTL variable used here, borrowed from Henderson *et al.* (2012), is the natural logarithm of mean luminosity at the country level, which averages the digital luminosity calculations across all cells that fall within a country's national borders. The correlation between NTL density and actual

GDP is well-established both across and within nations (Henderson *et al.* 2012; Chen and Nordhaus 2011; Jean *et al.* 2016; Weidmann and Schutte 2017).

3.2 Institutional Features

We take our institutional characteristics from the International Country Risk Guide (ICRG 2013), whose 1984–2013 data covers 146 countries. In addition to assessing democratic accountability, bureaucratic quality, corruption, law and order, and government stability in each country-year, the ICRG provides sub-indices of government stability (popular support, government cohesion, and legislative strength) for the 2001–2009 period. Larger ICRG ratings denote desirable features, such as increased bureaucratic quality ratings indicating more efficient bureaucracies, and higher absence of corruption ratings signalling less corruption. Because the guide scores raw variables on different scales – for instance, 0–6 for corruption but 0–4 for bureaucratic quality – we rescale all ICRG variables between 0 and 1 for comparability and ease of interpretation (see Appendix Table A.1 for summary statistics, definitions, and sources for all variables used).

4 Econometric Approach

Borrowing from Magee and Doces (2015), we use the following empirical specification to estimate the within-country relation between official GDP figures and NTL density:[2]

$$\ln(GDP_{it}) = \gamma_i + \delta_t + \alpha_1 \ln(lights_{it}) + \epsilon_{it} \quad (1)$$

Conditional on country and year fixed effects, a 1% increase in NTL is associated with an α_1% change in reported GDP. Hence, taking first differences yields

$$\Delta \ln(GDP_{it}) = \eta_t + \beta_1 \Delta \ln(lights_{it}) + \varepsilon_{it} \quad (2)$$

We focus on popular support as our institutional feature of interest below. For the purposes of this exposition, we can use Equation (3), again borrowed from Magee and Doces (2015) to

[2] Neither Equation (1) nor any subsequent equations need a constant term because all specifications include a full set of year fixed effects.

investigate whether any given institutional feature X is associated with manipulation of official GDP figures:

$$\Delta \ln(GDP_{it}) = \eta_t + \beta_1 \Delta \ln(lights_{it}) + \beta_2 X_{it} + \varepsilon_{it} \qquad (3)$$

In the absence of manipulation, the reported GDP growth rates and NTL-based predictions should not vary systematically with changes in institutional feature X. In other words, given no manipulation, the discrepancy between the officially reported and NTL-predicted growth rates should be orthogonal to X and the coefficient of X in Equation (3), and statistically indistinguishable from zero.[3] Magee and Doces (2015) thus interpret a statistically significant β_2 as evidence of GDP manipulation. We illustrate this relation by rewriting Equation (3) as

$$\omega_{it} = \eta_t + \beta_2 X_{it} + \varepsilon_{it} \qquad (3')$$

where $\omega_{it} = \Delta \ln(GDP_{it}) - \Delta \ln(lights_{it})$. Equation (3') then estimates the effect of X on □, which measures the gap between GDP and NTL growth. Hence, Equation (3), rather than merely estimating the effect of X on reported growth rates conditional on NTL-predicted growth, instead measures the effect of X on the reported GDP to NTL-predicted growth discrepancy.

One potential shortcoming of this design is that the reported GDP growth rate may respond to the night light growth rate heterogeneously across countries. Differencing Equation (1), therefore removing the country fixed effects, allows us to rule out unobserved determinants of the reported GDP to NTL elasticities *only* when these unobservables have level effects *and* no growth effects. In our view, however, this latter assumption is overly strong because we see no compelling theoretical reason why the growth rate discrepancy cannot be correlated with country-specific unobservables. We also believe it overly restrictive to assume that the coefficient of $\Delta \ln(lights_{it})$, β_1 in Equation (3), is identical between countries. In reality, the extent to which NTL density changes affect reported GDP growth rates are likely to be country specific because multiple factors – including absolute latitude, electrification, or cultural norms of electricity usage at night – are likely to produce a heterogeneous cross-country response of $\Delta \ln(GDP_{it})$ to $\Delta \ln(lights_{it})$ (see Roger 2018). Thus, empirical estimates based on Equation

[3] As Magee and Doces (2015) point out, examining whether X affects the discrepancy between reported growth and night light growth via Equation (3) is equivalent to regressing the residuals from Equation (2) on X (p. 227).

(3) will tend to over-reject the null hypothesis that $\beta_2 = 0$, leading potentially to a mistaken conclusion of evidence of manipulation.

With these considerations in mind, we allow the response of reported GDP to NTL-predicted growth to be country specific by estimating variants of the following specification:

$$\Delta \ln(GDP_{it}) = \eta_t + \gamma_i * \Delta \ln(lights_{it}) + \lambda_2 X_{it} + \epsilon_{it} \tag{4}$$

where the coefficient of X is considerably more conservative than its analogue in Equation (3), which interprets any reported GDP to NTL-predicted growth discrepancies that are correlated with X as manipulation. The Equation (4) coefficient, in contrast, interprets as manipulation only those discrepancies between $\Delta \ln(GDP_{it})$ and $\Delta \ln(lights_{it})$ that are not due to country-specific elasticities of NTL to GDP, such as access to electricity, urbanization, cloud cover, tree cover, and any other country-specific and, to a first-order approximation, time-invariant factors.[4] We also estimate versions of Equation (4) in which sector sizes substitute for country dummies, thereby accounting for the ability of the reported to NTL-predicted growth discrepancies to reflect differences in the composition of national economies[5]. By controlling for the year fixed effects η_t, we also ensure that any patterns observed in the data are not driven by global economic fluctuations. In particular, including year fixed effects allows us to rule out the possibility that our results are due to differences in satellite settings and their ability to detect night time lights, which can vary from year to year. Thus, within a given year, λ_2 reflects correlations between institutional variables X and the size of the discrepancy between $\Delta \ln(GDP_{it})$ and $\Delta \ln(lights_{it})$. In other words, λ_2 reflects the correlation between the aforementioned variables that is not attributable to the unobserved heterogeneity that affects all countries equally, to a first-order approximation, in a given year.

The normative expectation for λ_2, conditional on governments truthfully reporting GDP figures is a mean of zero since X should be uncorrelated with the size of the $\Delta \ln(GDP_{it})$ to $\Delta \ln(lights_{it})$ discrepancy. Hence, rather than interpreting any correlation between this discrepancy and X for any single country as evidence of manipulation, we only interpret as such a coefficient λ_2 that is positive and significant when averaged across all countries and years.

[4] Because we empirically estimate the country-specific elasticities of reported to NTL-predicted growth, we place no restrictions on the amount of manipulation a country may engage in relative to its elasticity.

[5] When we use piecewise regressions to test whether the effect of X is more apparent in the positive or negative realm of NTL growth (i.e., whether $\lambda_{2|-\Delta \ln L}$ is different from $\lambda_{2|-\Delta \ln L}$), we find no systematic differences (with p<0.05 in all three estimations) in the two coefficient estimates for any institutional feature X.

To account for serial correlation, we cluster standard errors at the country level in all regressions, relaxing the assumption that residuals have identical error structures across all countries and allowing them instead to be country specific. At the same time, we recognize that because both our dependent and independent variables of interest are likely to exhibit serial correlation, the resulting standard errors of λ_2 may be severely downwardly biased in the absence of a suitable clustering adjustment (Bertrand *et al.*, 2004).

5 Results

5.1 Baseline results

For the sake of completeness, we first estimate the correlations between each of our institutional features and the reported to NTL-predicted growth discrepancy (Table 1) with year fixed effects included in all specifications. The first row of each panel follows Equation (3), which imposes the restriction that the elasticity of reported to NTL-predicted growth does not vary across countries. The second row of each panel interacts $\Delta \ln(lights_{it})$ with sectoral sizes, expressed as a percentage of GDP (see Appendix Table A.1); while the third row corresponds to Equation (4), which allows the response to be country specific.

The pattern outlined in Table 1 indicates that government stability and its three component variables are positively correlated with the size of the reported to NTL-predicted growth discrepancy. Based on the ICRG codebook definitions, we interpret government stability and its sub-components – popular support, government cohesion, and legislative strength – as *entrenchment* factors, denoting a more politically secure incumbent. ICRG defines government stability as the government's ability to (1) stay in power and (2) carry out its declared policy plans, with (2) captured by the legislative strength component variable. Government cohesion refers to the extent to which the executive is coalesced around the government's policy goals, while popular support is the level of support the government or its chief executive enjoys in opinion polls deemed credible by the ICRG (PRS 2018).[6] It is important to note that because the availability of such polls can reasonably be thought of as endogenous to regime type, then if the ICRG only reports popular support data for "softer" autocracies, our estimates may suffer from sample selection issues. Such is not the case, however, because popular support data are available

[6] Appendix Table A.2 displays pairwise correlations between government stability and the four variables related to checks and balances. Although these latter are highly correlated with one another ($0.45 < \rho < 0.65$), each is at best only modestly correlated with government stability ($-0.07 < \rho < 0.20$). The government stability variable thus appears to be measuring a different underlying phenomenon than the other variables.

for all 132 countries sampled. In fact, according to our estimates, increased GDP manipulation is associated with more entrenched governments both in terms of government stability and its three component variables. For example, a government enjoying full popular support tends to report growth rates 2.8% higher than a government with minimal popular support, a correlation driven entirely by manipulation preceding increases in popular support (see Section 5.3). We find no evidence, however, that incumbents manipulate reported growth as a result of poor popular support.

Table 1 also reveals that democratic accountability, bureaucratic quality, and absence of corruption are robustly associated with smaller reported to NTL-predicted growth discrepancies. In addition to being statistically meaningful, these effects are also economically significant: the most corruption-free countries (C = 1) report growth rates between 2% and 2.4% lower than the most corrupt countries (C = 0). Thus, these three factors, which broadly measure quality of governance and checks and balances on executive power, are systematically associated with less manipulation, which reassuringly implies that checks and balances on the power of the executive are working to reduce moral hazard.

Table 1. Correlations between ICRG institutional factors and the reported to NTL-predicted growth discrepancy.

			Δ ln (light)		Sector* lights	Country* lights	Year FE	Obs.	No. Countries	R^2	Prob. > F
DA	-.021**	(-2.56)	.056***	(4.7)	-	-	Yes	1617	132	0.091	
	-.022***	(-2.88)			Yes	-	Yes	1606	131	0.104	
	-.019**	(-2.21)			-	Yes	Yes	1617	132	0.221	0.000
BQ	-.017**	(-2.36)	.056***	(4.56)	-	-	Yes	1617	132	0.087	0.000
	-.017**	(-2.30)			Yes	-	Yes	1606	131	0.099	
	-.017**	(-2.39)			-	Yes	Yes	1617	132	0.22	
C	-.02**	(-2.07)	.058***	(4.81)	-	-	Yes	1617	132	0.084	0.000
	-.024***	(-2.69)			Yes	-	Yes	1606	131	0.099	0.000
	-.02**	(-2.10)			-	Yes	Yes	1617	132	0.218	
LO	0.0037	(0.49)	.06***	(4.94)	-	-	Yes	1617	132	0.077	
	0.0021	(0.29)			Yes	-	Yes	1606	131	0.089	0.000
	0.0043	(0.58)			-	Yes	Yes	1617	132	0.211	0.000
LS	.036***	(3.5)	.033**	(2.47)	-	-	Yes	979	132	0.113	
	.038***	(3.92)			Yes	-	Yes	974	131	0.146	
	.035***	(3.06)			-	Yes	Yes	979	132	0.3	0.000
GC	.026**	(2.11)	.035***	(2.62)	-	-	Yes	979	132	0.095	0.000
	.023*	(1.83)			Yes	-	Yes	974	131	0.123	
	.027*	(1.95)			-	Yes	Yes	979	132	0.286	
GS	.065***	(5.16)	.056***	(4.77)	-	-	Yes	1617	132	0.102	0.019
	.061***	(4.89)			Yes	-	Yes	1606	131	0.111	0.000
	.064***	(4.82)			-	Yes	Yes	1617	132	0.233	
PS	.028*	(1.95)	.037***	(2.79)	-	-	Yes	979	132	0.095	
	0.02	(1.49)			Yes	-	Yes	974	131	0.12	0.023
	.028*	(1.83)			-	Yes	Yes	979	132	0.285	0.000

Note: Robust standard errors clustered over countries; *t*-statistics in parentheses. DA: Democratic Accountability; BQ: Bureaucratic Quality; C: Corruption; LO: Law and Order; LS: Legislative Strength; GC: Government Cohesion; GS: Government Stability; PS: Popular Support. The last column (Prob. > F) shows the statistical significance of Wald tests for country*light and sector*light interactions. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

5.2 Entrenchment across regime types

To unpack the correlations observed in Table 1, we allow the effects of our institutional variables to be regime specific in Table 2. In column (1), we confirm the presence of both government entrenchment and institutional quality effects when both sets of variables are included in a given specification. Here, the point estimate for government stability is positive and strongly statistically significant ($p < 0.01$) even when the specification additionally includes ICRG variables that capture checks and balances and good governance. We also confirm that these good governance factors have a mitigating effect; particularly in countries with higher bureaucratic quality and a relative absence of corruption, which have smaller official GDP to night light growth discrepancies.

In line with Boix *et al.* (2013), who provide a binary classification of political regimes based on a definition of democracy as (i) holding free and fair elections and (ii) having a minimum threshold of suffrage, we separate each country-year into autocracies and democracies (Table 2, columns (2)–(5) and (6)–(10), respectively). The mitigating effect of checks and balances on GDP manipulation is present only in democracies, which, in contrast to autocracies, generally show negative and usually statistically significant values for bureaucratic quality and absence of corruption. Interestingly, bureaucratic quality cannot explain the reported to night light predicted growth discrepancy in autocracies, many of which are also developing countries with limited statistical capacity. Nevertheless, we find no evidence that higher capacity autocracies manipulate GDP figures any less.

Our estimates further reveal that once other institutional features are controlled for, government support is associated with greater misreporting in both regime types (Table 2, columns (2) and (6)). We unbundle this association through stepwise analysis of the effect of each component sub-index of the ICRG government support variable, whose limited availability (2001 onward) somewhat reduces our sample size. Specifically, we replace government stability with each of its sub-indices (legislative strength, government cohesion, and popular support) separately for autocracies and democracies (columns (3)–(5) and (7)–(9), respectively). Whereas we find no substantive evidence of any sub-index being correlated with misreporting in autocracies, we find robust evidence that in democracies, the effect of government stability is driven primarily by popular support. For example, a 0 to 1 switch in popular support is associated with a 3% increase in reported growth (column (9)), a correlation not explainable by bureaucratic quality or corruption. Thus, the effect is not driven by polities that in Boix *et al.*'s (2013) dataset are democratic but have weak bureaucracies (like Nicaragua or the Dominican Republic) or endemic corruption (like Zimbabwe or the Democratic Republic of the Congo).

This latter observation leads us to wonder whether the lack of correlation between popular support and manipulation in autocracies relative to democracies could be driven by differences in sample size. For instance, whereas popular support does not appear to be associated with manipulation in autocracies (column (6)), this correlation does occur in our larger sample of democracies (column (9)). We therefore assess whether the outcome would hold for similarly sized samples by randomly selecting 351 observations from the 628 in the democratic sample and re-running the column (9) specification. We perform this randomization 1,000 times to obtain 1,000 sets of coefficients and standard errors for popular support, while holding sample size constant at N = 351. We then plot the distribution of the standardized popular support coefficients in Figure 1. Almost the entire distribution of popular support coefficients lies to the right of zero (with all but two being positive). The value for the average effect is 0.083, while that for the effect at the fifth percentile (approximated as the mean effect minus two standard deviations) is 0.029 and positive. The fact that the effect is detectable even in smaller samples makes it highly unlikely that differences in the popular support coefficient across regime types results from differences in sample size.

Similarly, the lack of correlation between manipulation and either of government cohesion and legislative strength in autocracies (columns (3) and (4) of Table 2) could simply be due to under-powered tests. We therefore artificially increase the number of degrees of freedom by duplicating each observation; the results are shown in Appendix Table A.3. We are still unable to detect any significant relationships, suggesting that the differences across regime types are not due to differences in sample size.

As an additional precaution, because high growth in the two pre-election years tends to increase an incumbent's likelihood of retaining power (Fair 1978) — which further increases incentives to manipulate reported GDP — we account for elections' potential confounding effect by including election year and pre-election year dummies in our specification. Although these relations are not precisely estimated, we find both to be positively correlated with increases in reported GDP growth rates (Table 2, column (10)). In addition, the inclusion of election year dummies does not affect the size or significance of the popular support variable: the manipulation–popular support correlation does not appear to be driven by proximity to elections.

To check for strategic behaviour around election times, we interact popular support with the election year dummies in column (11). We find that manipulation tends to be lower in election years when the incumbent has popular support (PS), as shown by the negative coefficient of the PS * Election Year interaction. However, when the incumbent has a lack of popular support in election years, manipulation tends to be higher, as shown by the positive coefficient of Election Year. This pattern is consistent with incumbent governments deciding to manipulate based on potential costs and benefits. A government that already enjoys popular support in an election year may plausibly choose not to manipulate, as manipulation will be of little electoral benefit. Conversely, a government with lower levels of popular support may be more likely to try and skew votes in its favour by manipulating GDP numbers.

Table 2. Correlation between political entrenchment and the reported to NTL-predicted growth discrepancy across regime types.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	All Countries	Autocracies				Democracies					
GS	0.0587***	0.0828***				0.0383***					
	(0.0131)	(0.0270)				(0.0140)					
LS			0.0372				0.0105				
			(0.0403)				(0.0090)				
GC				0.0314				0.0106			
				(0.0347)				(0.0094)			
PS					0.0097				0.0302**	0.0333**	0.0361**
					(0.0356)				(0.0149)	(0.0154)	(0.0160)
DA	-0.0013	0.0065	-0.0020	-0.0071	-0.0125	0.0009	0.0023	0.0015	0.0019	0.0042	0.0038
	(0.0081)	(0.0145)	(0.0223)	(0.0169)	(0.0170)	(0.0119)	(0.0138)	(0.0138)	(0.0135)	(0.0134)	(0.0134)
BQ	-0.0162*	-0.0207	-0.0076	-0.0006	-0.0024	-0.0120	-0.0222**	-0.0225**	-0.0222**	-0.0228**	-0.0227**
	(0.0089)	(0.0200)	(0.0302)	(0.0332)	(0.0338)	(0.0083)	(0.0096)	(0.0095)	(0.0091)	(0.0091)	(0.0091)
C	-0.0261**	0.0020	0.0065	0.0029	0.0100	-0.0295***	-0.0435***	-0.0439***	-0.0450***	-0.0454***	-0.0454***
	(0.0121)	(0.0321)	(0.0558)	(0.0536)	(0.0507)	(0.0092)	(0.0102)	(0.0099)	(0.0099)	(0.0098)	(0.0097)
LO	0.0253***	0.0370**	0.0428*	0.0416**	0.0465**	0.0168*	0.0188*	0.0197**	0.0185*	0.0195**	0.0198**
	(0.0082)	(0.0159)	(0.0229)	(0.0190)	(0.0231)	(0.0098)	(0.0097)	(0.0096)	(0.0095)	(0.0094)	(0.0095)
Pre-election Year										0.0035	-0.0056
										(0.0036)	(0.0185)
Election year										0.0032	0.0234*
										(0.0029)	(0.0119)
PS * Pre-election Year											0.0162
											(0.0298)
PS * Election Year											-0.0347*
											(0.0187)
Average Marginal Effects											
PS											0.0325**
											(0.0150)
Pre-election Year											0.0040
											(0.0039)
Election Year											0.0031
											(0.0028)
Observations	1,617	589	351	351	351	1,028	628	628	628	601	601
R-squared	0.2525	0.3424	0.3552	0.3526	0.3474	0.2226	0.3754	0.3751	0.3888	0.3948	0.3998

Note: Dependent variable = Δ ln(GDP). All specifications include country dummies * Δ ln(lights) interactions and year fixed effects; robust standard errors in parentheses are clustered over countries. DA: Democratic Accountability; BQ: Bureaucratic Quality; C: Corruption; LO: Law and Order; LS: Legislative Strength; GC: Government Cohesion; GS: Government Stability; PS: Popular Support. . ***, ** and * denote significance at the 1%, 5%, and 10% level, respectively.

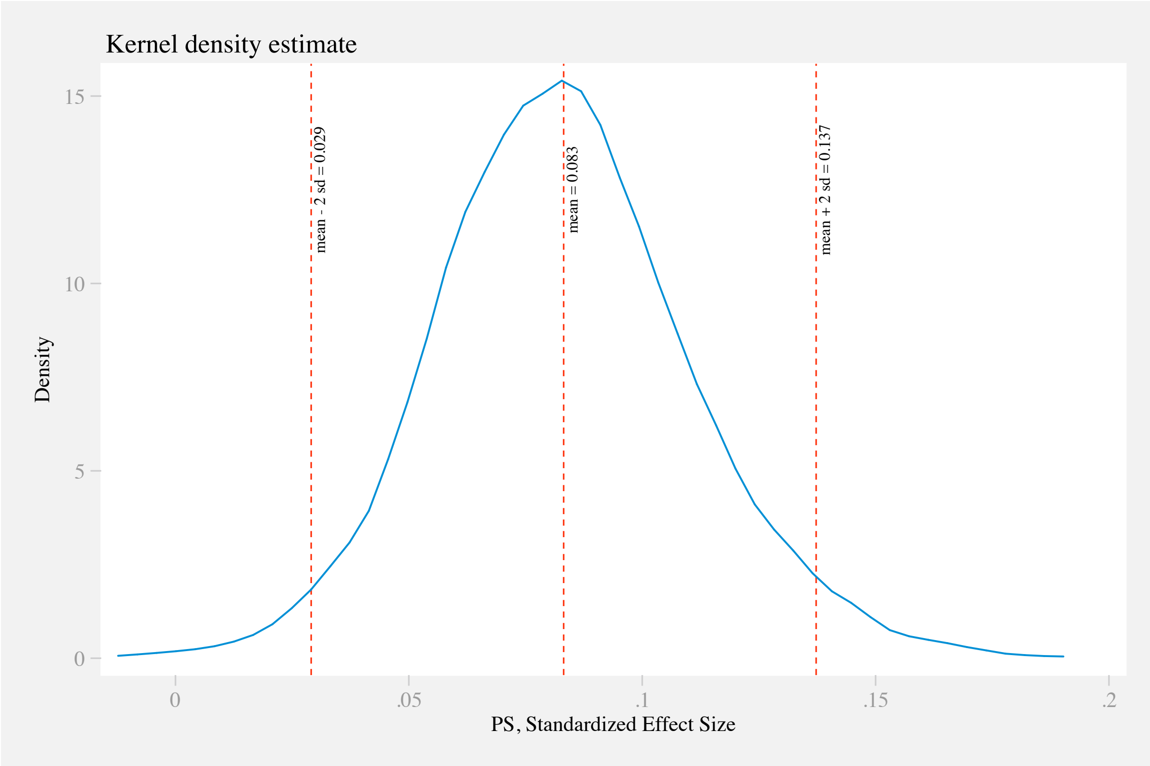

Figure 1. Kernel Density Estimates: Popular Support.

Notes: This figure plots the standardized effect sizes for popular support from 1,000 repetitions of the empirical specification from Table 2, column (9). Each repetition randomly selects 351 observations from the available 628 for comparison with the results obtained with N = 351 (Table 2 column (5)). We standardize the coefficients by dividing the mean by the standard deviation, calculated as the standard error times the square root of N. Kernel function: Epanechnikov; bandwidth: 0.0058.

5.3 Which comes first: manipulation or popular support?

Having shown that popular support is the entrenchment characteristic most closely related to GDP misreporting, we then ask whether it is the result or the cause of manipulation. Although we lack random assignment and thus cannot causally identify which of the two factors may trigger the other, we explore this question by exploiting time variation. We find no evidence, however, that the first lag of popular support is correlated with reported GDP growth (Table 3, column (1)), indicating that reported GDP does not tend to rise after popular support increases We do observe, however, that reported GDP is significantly correlated with future popular support (column (2)), suggesting that over-statements of GDP tend to precede rises in popular support for the incumbent. When we test for whether this effect is heterogeneous across regime types (columns (3) and (4)), we again find an effect only for democracies, with a significant and positive point estimate on the same order of magnitude as in previous estimates. In stark contrast, the coefficient of popular support in autocracies is small, insignificant, and negative, offering *prima facie* evidence that democratic regimes successfully manipulate reported growth for political gain.

Next, in Column (5), we interact popular support with the election year dummies. Our rationale for doing so is that a given regime may tend to have more popular support in the immediate aftermath of an election won by that regime. Thus, we would expect less manipulation by governments just elected. We find this to be the case empirically: the interaction of the election year dummy with the first lead in popular support is negative and statistically significant. Hence, the first lead in popular support remains a highly significant predictor of increased manipulation: its average marginal effect, shown at the bottom of column (5) in Table 3, is 0.0321, which is in the same order of magnitude as the non-interacted effect of the first lead of popular support shown in column (4).

Table 3. Popular support: result or cause of manipulation?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Δ ln(GDP)			% Intending to Vote for Incumbent	
	All Countries	All Countries	Autocracies	Democracies	Democracies	Democracies	Democracies
PS $_{t-1}$	0.0145						
	(0.0131)						
PS $_{t+1}$		0.0255**	-0.0087	0.0338***	0.0459***		
		(0.0123)	(0.0327)	(0.0128)	(0.0144)		
PS $_{t+1}$ * Election Year					-0.0461**		
					(0.0180)		
PS $_{t+1}$ * Pre-election Year					-0.0294		
					(0.0181)		
$\Delta\ \ln(\mathrm{GDP})_{t-1}^{Unexp}$						0.5242***	0.7091***
						(0.1184)	(0.1396)
$\Delta\ \ln(\mathrm{GDP})_{t-2}^{Unexp}$							1.3433**
							(0.5493)
DA	-0.0114	-0.0088	-0.0192	0.0070	0.0066	0.1877	-0.1624
	(0.0117)	(0.0100)	(0.0166)	(0.0122)	(0.0122)	(0.1496)	(0.2004)
BQ	-0.0115	-0.0149	0.0072	-0.0212***	-0.0217***	-0.0296	0.2025*
	(0.0116)	(0.0102)	(0.0314)	(0.0080)	(0.0079)	(0.0909)	(0.1047)
C	-0.0491***	-0.0367***	0.0145	-0.0415***	-0.0411***	0.0398	-0.0554
	(0.0157)	(0.0132)	(0.0409)	(0.0096)	(0.0094)	(0.0783)	(0.1300)
LO	0.0300***	0.0278***	0.0428**	0.0184*	0.0185*	-0.0218	0.0601
	(0.0103)	(0.0090)	(0.0199)	(0.0094)	(0.0094)	(0.0847)	(0.1128)
Pre-election Year	0.0014	0.0006	-0.0047	0.0016	0.0187*	0.0003	-0.0566
	(0.0033)	(0.0026)	(0.0051)	(0.0032)	(0.0112)	(0.0423)	(0.0779)
Election Year	0.0049*	0.0042*	0.0101**	0.0014	0.0290**	-0.0039	-0.0772
	(0.0026)	(0.0022)	(0.0043)	(0.0024)	(0.0113)	(0.0357)	(0.0463)
Average Marginal Effect							
PS $_{t+1}$					0.0321**		
					(0.0124)		
Pre-election Year					0.0015		
					(0.0032)		
Election Year					0.0020		
					(0.0025)		
Observations	819	1,052	379	673	673	89	54
R-squared	0.3002	0.2972	0.2918	0.3534	0.3608	0.3182	0.5023

Note: Dependent variable = Δ ln(GDP) in columns (1)–(5), which includes country dummy * Δ ln(lights) interactions. Dependent variable = intentions to vote for incumbent party if election were held tomorrow (WVS) in columns (6)–(7), where the key right-hand side variable Δ ln(GDP) Unexp is the residuals from a regression of Δ ln(GDP) on the interactions of country dummies and Δ ln(lights) as well as the set of year fixed effects. All specifications include year fixed effects; robust standard errors in parentheses are clustered over countries. DA: Democratic Accountability; BQ: Bureaucratic Quality; C: Corruption; LO: Law and Order; LS: Legislative Strength; GS: Government Stability; GC: Government Cohesion; PS: Popular Support. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

5.4 The magnitude of political gains

Although our documentation of increases in reported growth rates preceding increases in popular support is informative, ideally, we would like to know how many votes an incumbent government could gain by manipulating GDP figures. To do so, we construct a new dataset of voting intentions by combining incumbent party data from the Inter-American Development Bank's (2015) Database of Political Institutions with World Values Survey (WVS) responses, for 180 country-years, to the question, "Which party would you vote for if there was a national election tomorrow?". This data combination enables us to derive voting intentions for each incumbent party in each survey year.

To quantify the voting intention response to reported growth rates, we must first remove the growth rate variation stemming from country-specific responses of GDP to NTL. To do so, we first derive $\Delta\ln(GDP)^{Unexp}$ as the residuals from regressing Δ ln(GDP) on $\gamma_i * \Delta\ln(lights_{it})$, where γ_i is the vector of country dummies, and then estimate the following equation:

$$Vote\ Intentions_{it} = \tau_0 + \eta_t + \Delta\ln\left(GDP_{i,t-1}^{Unexp}\right) + \mathbf{X}_{it}\tau + \xi_{it} \qquad (5)$$

Once we take into account the country-specific responses of GDP to NTL, year fixed effects, and the full set of controls from Table 3, columns (1)–(4), a 1% increase in the lagged reported growth rate is associated with a 0.52% increase in voting intentions for the incumbent (see column (5)), an effect that is both economically and statistically significant.

Next, recalling that voters make their voting decisions based on the previous two years of data (Fair 1978), we test the robustness of this result to the inclusion of a second lag in the unexplained reported growth. We find that a 1% increase in the lagged reported growth rate in each of the two pre-survey years is associated with a 1.93%[7] (or a nearly 1% per year average) increase in intentions to vote for the incumbent (see Table 3, column (6)). Any further lags, however, are insignificant, indicating that, in line with Fair (1978), when making election decisions, voters only appear to remember the two most recently reported growth rates.

[7] This number is the sum of the coefficients of the first two lags of $\Delta\ln(GDP)^{Unexp}$.

5.5 Universality of the manipulation–entrenchment link

Even though the association between popular support and GDP manipulation in democracies appears robust, we recognize that positive point estimates may hide substantial degrees of heterogeneity. Hence, in our next set of estimates, we allow the reported growth rate–popular support correlation to be heterogeneous across countries. In the democratic sample, we interact the country dummies with the first lead of the popular support variable and estimate a separate slope for each country[8] while controlling for election years, the country dummy–light growth interaction, year fixed effects, and the ICRG quality of governance variables. We express this estimation as

$$\Delta \ln(GDP_{it}) = \lambda_0 + \eta_t + \gamma_i * \Delta \ln(lights_{it}) + \gamma_i * PS_{i,t+1} + \mathbf{X}_{it} + \epsilon_{it} \tag{6}$$

where $\mathbf{X}$ is the vector of the controls described above. We thus obtain individual coefficients for popular support for each country in the sample from the $\gamma_i * PS$ interactions. Because these coefficients speak to the correlation between higher reported GDP growth and future levels of popular support, they can be interpreted as the country-specific premium in popular support earned by a given incumbent by manipulating reported GDP growth. We plot these coefficients against their *p*-values in Figure 2.

[8] Interacting the country dummies with contemporary popular support yields similar results (see Appendix Figure A.1).

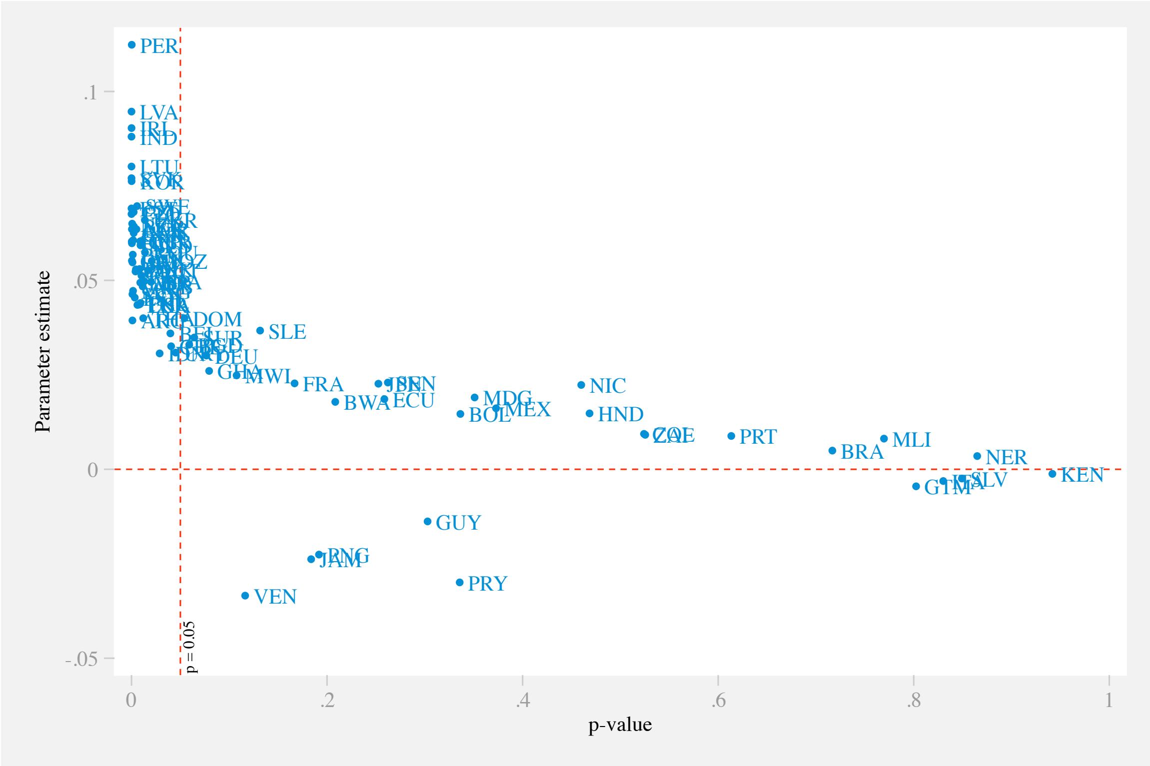

Figure 2. Coefficients of the Country Fixed Effects * Popular Support interactions, with the horizontal dashed line denoting a coefficient of 0 (no association between higher popular support and manipulation) and the vertical dashed line representing $p = 0.05$.

Plotting these estimates reveals a strikingly asymmetric pattern, with very few countries below the horizontal dashed line (denoting a zero coefficient) and no country with a negative and significant coefficient. Rather, many countries fall into the top-left region of the graph, with positive coefficients and p-values smaller than 0.05; while many more have positive coefficients, albeit imprecisely estimated. This visual overview of our results offers some reassurance that outliers or other idiosyncrasies do not drive the pattern documented so far: the popular support premium of manipulation is positive for nearly all countries and precisely estimated for most.

5.5 Easier to fool a trusting person

Given the inherent role of trust in the governance process, we conjecture that, all else being equal, it is easier to fool a trusting person. To test this conjecture, we construct a country-specific measure of interpersonal trust from the World Values Survey; specifically, the item that asks respondents to choose (a) “most people can be trusted” or (b) “when dealing with people, one can’t be too careful”. After calculating interpersonal trust as the fraction of respondents who chose (a) in the WVS, we correlate this trust with the popular support premium earned by manipulation (Figure 3) and plot it against the vector of country-specific coefficients from Figure 2. Doing so yields significant evidence of an upward sloping pattern, indicating that more trusting countries are more prone to rewarding incumbent government manipulation of GDP figures with popular support.

Figure 3. Popular support premium and trust in government, with test statistics for heteroskedastic errors.

6 Conclusions

Even though a lack of transparency on the true state of any economy hampers determination of the exact extent to which official statistics are manipulated, the recent marked increase in the availability of data provides new tools for monitoring economic conditions and identifying manipulative practices. In particular, it enables the use of quantitative analyses rather than case studies, which are often sketchy and speculative. One such tool – the use of satellite derived NTL images to proxy for economic factors – provides data and variables that, being issued by an independent scientific agency, are beyond governmental control. We use such imagery-based analysis to uncover a robust stylized fact: the manipulation of GDP statistics is systematically associated with increased popular support in democracies, but not in autocracies. In addition, even though our results cannot be interpreted causally, we find that increases in reported growth rates in democracies tend to occur *before* increases in popular support and *not after,* which suggests that in these regimes, manipulating GDP figures pays political dividends.

Admittedly, our results could be confounded by some third unobserved factor; however, for this latter to explain away our findings, the timing of its variation would have to closely match that of the reported growth rates. In fact, the effects we document are statistically and economically significant; in particular, our evidence that a 1% over-statement of the reported growth rate for two consecutive years increases voting intentions for the incumbent by approximately 2%. Considering that many elections are decided by small margins,[9] our results suggest that government manipulation of data can have serious effects on electoral outcomes.

[9] For example, the popular vote margin between Al Gore and George W. Bush in the 2000 United States presidential election was just 0.5%. The (since-annulled) second round run-off of the 2016 Austrian presidential election saw Alexander Van der Bellen win by a mere 0.6%.

APPENDIX

Table A.1 Summary statistics, variable definitions and sources.

Variables	Description	Mean	SD	Min.	Max.	N
□ *ln*(GDP)	Reported Gross Domestic Product growth are calculated using the first reported version of GDP estimates (local constant currency). GDP estimates were obtained from WDI Database Archives. We consider only the first GDP estimates made available to the World Bank (WDI) within the first two years in calculating the log difference.	0.030	0.062	-0.74	0.70	2791
□ *ln*(Light)	Source: Defense Meteorological Satellite Program's Operational Linescan System (DMSP-OLS), obtained from Henderson, Storeygard and Weil (2012).	0.035	0.18	-0.86	1.98	2265
Year	Calendar year.	2000.8	5.65	1991	2009	2791
Economic Structure	Source: National Accounts Main Aggregates Database – United Nations (see detail classifications of economic activities in International Standard Industrial Classification of All Economic Activities (ISIC Rev 3.1)). Economic structure are calculated as the relative contribution of economic sectors to GDP.					
ISIC A-B	Aggregation of economic activities of Agriculture, Hunting, Forestry, Fishing (% of GDP).	0.16	0.14	0.00053	0.78	2735
ISIC C-E	Aggregation of economic activities of Mining, Manufacturing, Utilities (% of GDP).	0.23	0.12	0.0091	0.85	2737
ISIC F	Aggregation of economic activities of Construction (% of GDP).	0.059	0.030	0.0065	0.28	2737
ISIC G-H	Aggregation of economic activities of Wholesale, Retail Trade, Restaurants and Hotels (% of GDP).	0.15	0.054	0.021	0.52	2737
ISIC I	Aggregation of economic activities of Transport, Storage and Communication (% of GDP).	0.089	0.037	0.012	0.28	2737
ISIC J-P	Aggregation of economic activities of Other Activities (% of GDP).	0.31	0.11	0.026	0.65	2737
Institutional Features	Source: International Country Risk Guide (ICRG) - PRS Group					
DA	Democratic Accountability (K) (original scale: 0-6)	0.67	0.26	0	1	1988
BQ	Bureaucratic Quality (L) (original scale: 0-4)	0.55	0.28	0	1	1988
C	Corruption (F) (original scale: 0-6)	0.49	0.22	0	1	1988
LO	Law and Order (I) (original scale: 0-6)	0.63	0.23	0	1	1988
LS	Legislative Strength (original scale: 0-4)	0.75	0.18	0.25	1	1137
GC	Government Cohesion (original scale: 0-4)	0.82	0.15	0.38	1	1137
GS	Government Stability (A) (original scale: 0-12)	0.67	0.16	0.083	1	1988
PS	Popular Support (original scale: 0-4)	0.60	0.14	0	0.98	1137
Trust	Source: World Value Survey (WVS 1981-2014) and European Value Study (EVS 1981-2008)					
Interpersonal Trust	Most people can be trusted (A165). Question wording: Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people? Responses: {Most people can be trusted; Can't be too careful; No answer; Don't know; Not applicable}. Original scale: 1 (Most people can be trusted) and 2 (Can't be too careful). Answers with 'No answer', 'Don't know' or 'Not applicable' are coded as missing values.	0.28	0.15	0.038	0.76	620
Intend to Vote for Incumbent	Ruling party vote intent (share of total electorate). Source: WVS and DPI. We identify the ruling party in each-country year from DPI as the incumbent party. Question wording: Which party would you vote for as a first choice if there was a national election tomorrow. The question number varies across WVS waves. Wave 6: V228. Wave 5: V231. Wave 4: V220. Wave 3: V210.	0.25	0.12	0.011	0.69	180
Democracy	Source: Boix et al. (2013) - dichotomous measure of democracy (Boix-Miller-Rosato).	0.59	0.49	0	1	2751

Table A.2. Pairwise correlations between ICRG variables.

	Democratic Accountability	Bureaucratic Quality	Corruption	Law and Order	Government Stability
Democratic Accountability	1.00				
Bureaucratic Quality	0.62	1.00			
Corruption	0.50	0.65	1.00		
Law and Order	0.45	0.64	0.62	1.00	
Government Stability	-0.02	0.05	-0.07	0.20	1.00

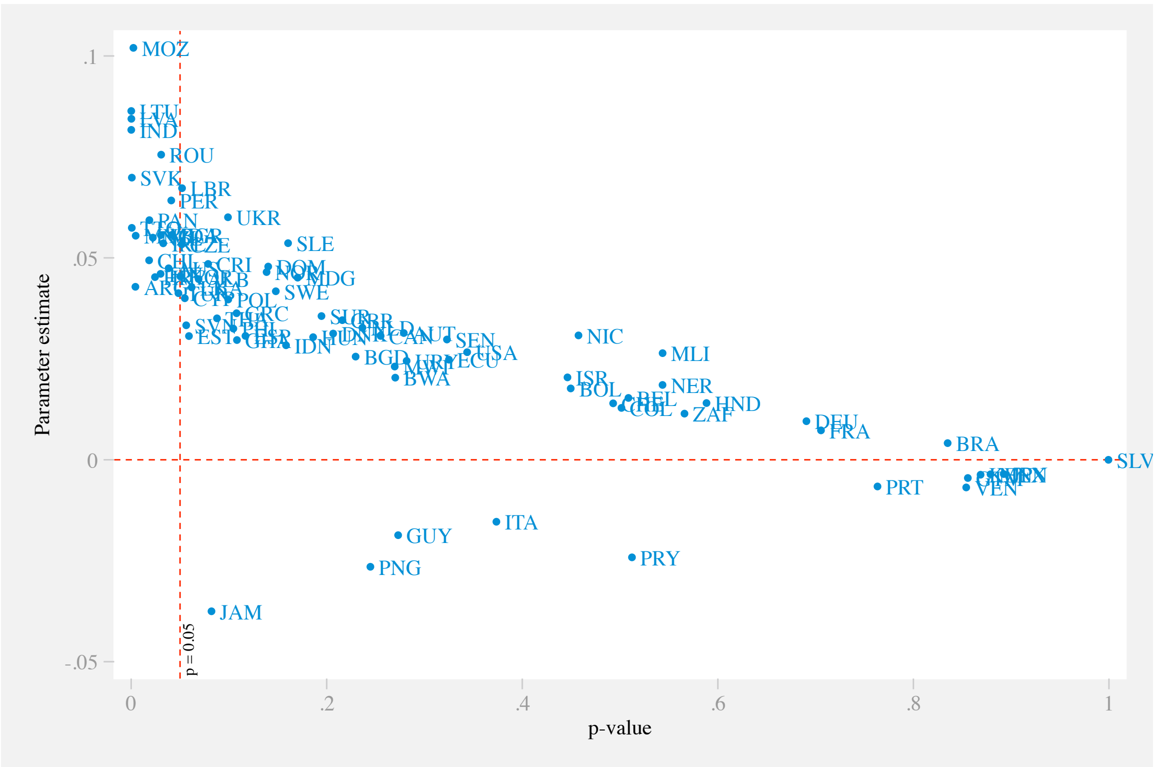

Figure A.1. Coefficients of [country fixed effects * popular support] interactions.

Notes. This figure is the counterpart of Figure 1 in the main text, which shows the coefficients of the interactions of country fixed effects with the first lead of popular support.

Table A.3. Examining whether the insignificant results in autocracies from Table 2 in the main text are due to under-powered tests.

	(1) Autocracies	(2) Autocracies	(3) Autocracies
LS	0.0372		
	(0.0381)		
GC		0.0314	
		(0.0328)	
PS			0.0097
			(0.0336)
DA	-0.0020	-0.0071	-0.0125
	(0.0211)	(0.0159)	(0.0161)
BQ	-0.0076	-0.0006	-0.0024
	(0.0285)	(0.0313)	(0.0319)
C	0.0065	0.0029	0.0100
	(0.0527)	(0.0506)	(0.0479)
LO	0.0428*	0.0416**	0.0465**
	(0.0216)	(0.0179)	(0.0218)
Observations	702	702	702
R-squared	0.3552	0.3526	0.3474

Note: Columns (1), (2), and (3) respectively replicate Columns (3), (4), and (5) from Table 2 in the main text. The sample size is increased by duplicating each observation. Dependent variable = Δ ln(GDP). All specifications include country dummies * Δ ln(lights) interactions and year fixed effects; robust standard errors in parentheses are clustered over countries. DA: Democratic Accountability; BQ: Bureaucratic Quality; C: Corruption; LO: Law and Order; LS: Legislative Strength; GC: Government Cohesion; GS: Government Stability; PS: Popular Support. . ***, ** and * denote significance at the 1%, 5%, and 10% level, respectively.

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