

ORIGINAL ARTICLE OPEN ACCESS

Have Autocrats Governed for the Long Term?

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Received: 21 June 2024 | **Revised:** 27 August 2024 | **Accepted:** 6 October 2024

Funding: The authors received no specific funding for this work.

Keywords: autocracy | democracy | long-term benevolence

ABSTRACT

The short answer is: probably not. We infer the priorities of national governments from observed outcomes, constructing a statistical proxy for long-term benevolence. Using data between 1960 and 2019 for more than 100 countries, we show that, on average, democracies score more highly on our measure. We then investigate whether variation in long-term benevolence can explain the ‘autocratic gamble’—the well-known tendency for growth rates to vary more widely across autocracies than across democracies. We show that the distribution of long-term benevolence under democracy first-order stochastically dominates its distribution under autocracy. Put differently, although there is an autocratic gamble in growth rates, there is no autocratic gamble in wider development outcomes. The rapid growth seen in some autocracies probably originates in regime self-interest rather than unusually benevolent leadership.

1 | Introduction

Many autocratic leaders have a bad name. Some have looted their countries or presided over many deaths. Others have remained in office for decades, even while their country’s economy fails to grow or experiences major turmoil. Yet some researchers still ask whether autocrats can make good long-term decisions and even whether they are better placed than democratic leaders to address the environmental challenges that will do much to define this century.

Moreover, there have been autocratic success stories. In the final third of the 20th century, countries such as China, Indonesia, Singapore and South Korea grew rapidly during extended periods of autocracy. One of the most firmly established facts in the empirical literature on democracy and autocracy is the existence of an ‘autocratic gamble’. The cross-section variance of growth is higher across autocracies than across democracies: Autocracies have seen both very good outcomes for growth and very bad. As Rodrik (2000, p. 18) put it, ‘The bottom line is that living under an authoritarian regime is a riskier gamble than living under a

democracy.’ This has been confirmed by a range of studies and methods¹. One of the earliest papers was Weede (1996); see also Sah (1991).

Perhaps the most natural and immediate explanation for the autocratic gamble is that autocrats vary in how much weight they place on the welfare of the general population. We call this the ‘heterogeneous altruists’ explanation. Some autocratic leaders have made long-term welfare a priority, while others—the kleptocrats—have achieved little beyond self-enrichment. Benevolent autocrats will typically pursue growth. They may even outperform many democracies, because they are less at the mercy of the electorate than similarly benevolent democratic leaders.

But in economics and political science, the analytical literature has instead tended to assume that autocrats always pursue their own self-interest. In some institutional settings, this may coincide with policies that promote investment and growth. In this case, some autocracies could outperform many democracies, when autocratic leaders have a greater personal stake than

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democratic leaders in long-run economic performance, or when fast growth helps to stabilize their legitimacy and support. An autocratic gamble could arise even without appealing to differences in the weight that leaders place on general welfare.

The central idea of this paper is that we can distinguish between these cases, by extending the analysis to development outcomes other than growth. If some autocrats place an unusually high weight on the welfare of the general population, this should be evident in wider development outcomes. An autocratic gamble should be visible for those outcomes, and not just growth. In contrast, a purely self-interested leader or regime may make growth a priority, but not wider development. In that case, few autocracies will score highly on non-growth outcomes, and the evidence for an autocratic gamble will be restricted to growth.

This paper will investigate this proposition in the data, which has rarely been done.² We construct a new measure of long-term benevolence by aggregating several indicators that an enlightened, farsighted autocrat would care about, and a purely self-interested leader might not. We look not only at the association between long-term benevolence and democracy but also, and more importantly, at how its distribution compares for autocracies and democracies. At least in this dataset, there is no autocratic gamble for wider development outcomes. We conclude that the rapid growth seen in some autocracies is likely to originate primarily in regime self-interest, rather than differences in benevolence.

To construct a statistical proxy for long-term benevolence, we use an indirect approach broadly similar to that of McQuilkin (2018). By combining information from several indicators, we seek to infer the underlying priorities of governments from observed outcomes rather than stated commitments. Given the aims of the paper, we deliberately orient our study away from traditional measures of institutions and liberal policies. Instead, our indicators include the priority given to health, education and other public goods, seen as valuable in themselves; in addition, we use several indicators of a systematic commitment to good decision-making. Our chosen indicators are influenced by government, can change in response to changes in the political regime and will have longer-term returns than some other dimensions of the quality of government. Hence, our work complements previous research by shifting the focus towards a regime's commitment to high-quality decisions and policies for the long-term common good.

When the indicators are combined, examples of countries with high long-term benevolence in the most recent period (2010–2019) include Denmark, Germany, Japan, Norway and Switzerland. When excluding OECD member countries, those with high long-term benevolence in the remaining sample include Barbados, Bhutan, Mauritius, Singapore, Taiwan and Uruguay. Those judged to have low long-term benevolence in the same time period include Azerbaijan, Sudan, Syria, Venezuela and Yemen.

We show that, on average, democracies score more highly on long-term benevolence. We ask whether this partial correlation might be influenced by selection and use several different estimators to examine the sensitivity of the results to alternative

identifying assumptions. The finding that democracies are more oriented to the long-term interests of their citizens is robust to adjusting for a wide range of exogenous controls. The use of controls does lower the partial correlation, but it remains sizeable: The presence of democracy is associated with higher long-term benevolence, by more than 0.85 of a standard deviation, in every decade, and by more than one standard deviation in four out of the six decades we consider.

Since we cannot rule out selection on unobservables, we also present IV estimates, instrumenting democracy with an irrigation-based instrument from Bentzen, Kaarsen, and Wingender (2017) and Eastern and Western Church exposure from Schulz (2022). These results come with caveats but help to shift the burden of proof towards those who want to argue that autocrats often govern in the long-term interests of their citizens.

After looking at mean outcomes, we turn to the central question of the autocratic gamble, which is also about variances. The high growth rates seen in 'economic miracles' under autocracy are sometimes attributed to enlightened leadership and a readiness to work for the long-term common good. The concept of the 'benevolent autocrat' has some fierce critics, notably Easterly (2013), but its validity has rarely been explored in the empirical literature. Can the autocratic gamble in growth rates be attributed to differential variation in the extent of long-term benevolence?

We find no evidence that it can. Indeed, we show that the distribution of long-term benevolence under democracy first-order stochastically dominates its distribution under autocracy. These results call into question the role of differences in long-term benevolence in explaining the autocratic gamble for growth rates. If the gamble does not originate in farsighted, enlightened autocrats, it seems more likely that regime self-interest will be central to the explanation.

The rest of the paper is organized as follows. The next section discusses the background to our study and relevant theory. Section 3 provides an overview of our approach and relates it to the literature. Section 4 describes the data and methods and presents our measure of long-term benevolence. To check the validity of our measure, Section 5 examines how it correlates with various other indicators, including public investment over time. We then turn to our main results. Section 6 considers the relationship between long-term benevolence and democracy, using several alternative treatment effect estimators. Section 7 asks whether differences in long-term benevolence can explain the autocratic gamble. Section 8 discusses one aspect of robustness, while Section 9 concludes.

2 | Theoretical Considerations

Interest in long-term social thinking has grown, as seen in books for a general audience such as Krznaric (2020), MacAskill (2022), Ord (2020) and White (2024) and more specialist treatments such as Cowen (2018) and Hale (2024).³ Such books are motivated partly by the failure of many governments to make farsighted decisions. The stakes are increasing, with the

social costs of short-term thinking likely to escalate rapidly over the course of this century. This reinforces the case for investigating long-term benevolence and whether it varies by political regime. This is interesting in itself and will also cast indirect light on why the autocratic gamble is such a prominent feature of observed growth rates.

2.1 | Background

Democracy can promote growth and development through electoral accountability, stronger incentives to provide public goods and protection for civil liberties, so that campaigners and the general public can safely exert pressure. Tang and Tang (2018) argue that democracy also has a unique advantage that autocracies cannot replicate: Democracies can accommodate bottom-up institutional and technological innovation that autocracies will typically seek to suppress. And looking at episodes where pro-market reforms failed, Callais and Lawson (2024) find an association between failure and autocratic political institutions.

The intrinsic merits of democracy are so obvious that its critics often have to fall back on a small set of arguments, of varying plausibility. Perhaps chief among these is the argument that autocrats, insulated from electoral pressures and media opposition, are well placed to make decisions that promote the long-term common good, even when these have short-term costs. Some early analysts of the Covid-19 pandemic argued that autocrats managed it more effectively than democracies; more recently, though, it has been argued that open societies have often had better outcomes, at least where trust in government is high.⁴

We will investigate whether autocrats ever seek the long-term common good and whether this can explain the autocratic gamble. These questions have become more pressing given the spread of ‘illiberal democracy’ (Zakaria, 1997) and the recent ‘democratic recession’ (Diamond 2008, 2015). The 2020 V-Dem Democracy Report noted that, for the first time since 2001, autocracies were in the majority.⁵ In a number of countries, even where formal elections remain, political rights and democratic social norms have been eroded. This makes it especially important to understand how outcomes differ under democracy compared to autocratic rule.

It has sometimes been argued that a ‘benevolent autocrat’ or ‘enlightened despot’ can take good, long-term decisions more effectively than democratic leaders. Sen (1999) refers to this as the ‘Lee thesis’, associated with Lee Kuan Yew’s long rule of Singapore (see Knutsen 2010). The transitional benefits of autocracy might be especially relevant at relatively low levels of development, if the preconditions for effective democracy are not yet in place. Huntington and Nelson (1976) suggested that, in the later stages of ‘modernization’, there will be a conflict between economic growth and political participation.

Along similar lines, some have argued for ‘growth first’, with democracy deferred until an economy has further developed. But these arguments do not have much evidence to draw on, beyond case studies of success under autocracy. The problem with

an informal assessment is that many autocratic regimes have failed, so case studies can be marshalled at will to support either side of the argument. After all, even newly-established democracies have sometimes seen farsighted decisions; for the case of Botswana, see Acemoglu et al. (2003a, pp. 99–100).

The balance of recent evidence suggests that democratizations are followed by faster growth (Acemoglu et al. 2019, S. Chen, Chernozhukov, and Fernández-Val 2019, C. Chen and Stengos 2024, Colagrossi, Rossignoli, and Maggioni 2020, Dube et al. 2023, Eberhardt 2022). In addition, Berg, Ostry, and Zettelmeyer (2012) show that growth spells are more likely to be sustained under democracy; Bluhm, de Crombrughe, and Szirmai (2020) find that weak political institutions are associated with longer and more severe crises; Imam and Temple (2024), using a very different method, similarly find that autocracies are more likely than democracies to experience severe and protracted collapses in output; and Millemaci, Monteforte, and Temple (2024) find that volatility is lower under democracy. On political institutions and volatility more broadly see, among many others, Acemoglu et al. (2003b), Klomp and de Haan (2009) and Malik and Temple (2009). But it is important to clarify that our paper is not seeking to revisit the relationships between democracy, volatility and growth, but instead to investigate a natural explanation for the autocratic gamble in growth rates.

2.2 | Relevant Theory

Some autocrats have succeeded in transforming their economies. Cases of fast growth under extended periods of autocracy include China, Indonesia, Singapore, South Korea and Taiwan. We are not seeking to question such examples, which help to explain why the autocratic gamble is so clear in the growth data. Rather, we want to know whether contrasting experiences under autocracy can be attributed to differences in long-term benevolence. ‘I do not know whether a genuinely benevolent regime has ever existed’, writes Wintrobe (1998, p. 14) ‘but the idea has a never-ending appeal’.

A pithy statement of the problem is given by Barro (1996, p. 3), when he writes that dictators:

come in two types: one whose personal objectives often conflict with growth promotion and another whose interests dictate a preoccupation with economic development... The theory that determines which kind of dictatorship will prevail is missing. (quoted in Overland, Simons, and Spagat 2005, p. 445).

There is some ambiguity here over whether leaders differ in their objective function or in their interests, where the latter would typically depend on circumstances. The explanation that we call ‘heterogeneous altruism’ appeals to differences between leaders in the weight they assign to the long-term welfare of the population. This may seem the natural place to start, and yet it has played only a minor role in the literature, perhaps because economists are often wary of ‘explanations’ that are based on unexplained differences in preferences. One way to take such

an account further may be to consider how particular leaders gain power, but first we need to assess whether differences in objective functions might be relevant.

An alternative is to stress the direct stake that a regime and its supporters can have in long-term economic success; see, among others, Olson (1993), McGuire and Olson (1996) and Salter (2015). That form of approach has been criticized by Robinson (1998), partly because some autocrats with an ‘encompassing’ interest in an economy have nevertheless presided over its collapse or destruction. Robinson suggests that the institutional prerequisites for development—such as protection for property rights and the rule of law—are ‘essentially antagonistic, indeed practically orthogonal, to the maintenance of the power of a dictator’ (Robinson 1998, p. 23). But in turn, this latter perspective does not explain the cases where autocrats have protected property rights and seen their countries achieve fast growth.

One reason democracies may not grow as fast as some autocracies is that electoral pressures deter governments from choosing policies that would have high short-run costs, or that would bring gains only in the long run; this is a large part of the Lee thesis. In theoretical models, democratic governments may show political myopia, as in Aidt and Dutta (2007) and Bonfiglioli and Gancia (2013). Whether this is enough to offset the benefits of democracy, as the Lee thesis assumes, is an open question.

In explaining why outcomes vary so widely, much attention has been given to the role of discounting. Outcomes may differ partly because of time horizons, which again could be either innate (some leaders are more patient) or a product of circumstances (some leaders have a more secure grip on power). Olson (1993) suggested that dynastic rule would be associated with longer horizons. Besley and Reynal-Querol (2017) find some supportive evidence that growth is higher under hereditary leaders, but only when the constraints on the executive are weak. In addition, hereditary leaders are relatively rare: under their core definition, they account for only 6.4% of the leaders in their sample (Besley and Reynal-Querol 2017, p. 126).

Clague et al. (1996) stress the likely heterogeneity of time horizons across autocratic regimes, varying with a leader’s hold on power, and across democracies depending on how long democracy has been established. In their empirical work they relate several indicators of secure property rights, including contract-intensive money (Clague et al. 1999), to measures of an autocrat’s tenure and (in a separate sample of democracies) the length of time that democracy has been continuously in place. One reading of their discussion is that leader horizons will vary more across autocracies than across all but the newest democracies, which could then explain the autocratic gamble discussed earlier, and which we examine in Section 7. But their view is that autocrats with long tenure (used as a proxy for long-term orientation) are likely to achieve better outcomes, while Jong-A-Pin and Mierau (2022) find that growth deteriorates as autocratic leaders age, perhaps reflecting a shortening time horizon.

The selection of leaders may work better under democracy: Besley and Reynal-Querol (2011) find that, on their definitions,

democracies are 20% more likely than autocracies to select highly educated leaders. Besley, Montalvo, and Reynal Querol (2011) find that, on average, more educated leaders preside over faster growth, while Li, Xi, and Yao (2020) find that more educated leaders choose a faster pace of liberal reforms. More generally, in autocracies, ineffective or damaging leaders can persist in office, and this may be one of the most significant costs of autocracy. Further, autocrats are also at risk of becoming cut off from reliable sources of information (Wintrobe 2009).

3 | Our Approach

3.1 | Key Concepts

The concept of long-term benevolence is central to this paper. It naturally overlaps with other concepts, including the quality of government, state capacity and empirical measures of governance. In an extended discussion, Agnafors (2013) argues that many past conceptions of the quality of government have been too narrow, and neglect dimensions such as a systematic commitment to good decision-making, and whether efficiency or state capacity are directed to good and desirable ends. Our preferred measure attempts to take these into account, so that we can investigate, at least indirectly, whether ‘benevolent dictators’ are common or rare. Discussions of benevolent autocrats have a long and often controversial history; for an extended critique of the concept, see Easterly (2013).

We are primarily interested in whether governments seek to promote the long-term welfare of their citizens. It is useful to consider two components of long-term benevolence, both of which are important. Some autocrats may have a long time horizon, without making the welfare of their citizens a central priority. In contrast, a democratic government may promote welfare, but only on a short horizon linked to the electoral cycle. This makes it especially interesting to ask how long-term benevolence is related to political institutions. It also means that we are addressing one of the main defences commonly made of autocracy.⁶

Since the underlying priorities of governments and national leaders are hard to observe directly, we must look for indirect approaches. In this paper, we treat long-term benevolence as a latent factor. By extracting the first principal component from a disparate set of indicators, we hope to use the variation they have in common to proxy for the long-term benevolence of national governments. Almost inevitably, each of the individual indicators could be criticized for this purpose, but the variation they have in common may isolate something important about underlying priorities.⁷

To select the indicators, we first consider some policy counterparts of long-term benevolence, starting with human capital and the priority given to public goods in general. Investment in human capital is likely to be a pro-growth policy in the medium to long term, and even modest increases in growth rates can bring huge gains in present value terms (Pritchett et al. 2016). But human capital also has intrinsic value: It should bring other long-term gains, such as better health, enhanced capabilities and more fulfilling lives. All these are

likely to be valued highly by a government genuinely making decisions for the long-term common good. In this sense, our perspective is related to the one advocated by Sen (1999) and his emphasis on GDP as a means to an end rather than an end in itself.

As well as three indicators based on public goods, we include three indicators of an embedded commitment to good decision-making, which is likely to pay off over the medium to long term.⁸ We then apply a principal components analysis to the set of six indicators, assuming that their common variation originates in the extent of long-term benevolence.

We favour indicators that can respond relatively quickly to changes in policy and national priorities. For example, as an education measure, we use a measure of the government's commitment to educational equality, rather than (say) the literacy rate of the adult population. The latter would not be a good measure for our purposes, because it reflects a long history of government decisions and cannot be altered quickly. The use of educational equality is attractive for another reason: We assume that a farsighted, enlightened leader should be especially keen to adopt measures for inclusive growth, such as egalitarian education and health policies. These will not only raise living standards and well-being but may also help to create the conditions for political stability and further growth in the future, and hence should be especially attractive to a benevolent government taking a long-term view.

In most cases, our chosen indicators overlap with distinct concepts, not least state capacity and the quality of government. The overlap is inevitable, not least because a leader governing for long-term welfare might want to invest in state capacity. What our chosen indicators have in common is that they are influenced by government, can change in response to changes in the political regime and have longer-term returns than some other dimensions of the quality of government. Although the final collection of indicators is rather heterogeneous, we use only the variation they have in common.

It will be natural to ask whether we can genuinely infer the priorities of governments from the indicators we have chosen. To check that our measure is not simply noise, we explore its correlations with several policy outcomes and use cross-country panel data to examine whether the measure Granger-causes public investment. We find that it does: On average, increases in measured long-term benevolence are followed by an increased share of public investment in GDP. More precisely, our measure of long-term benevolence has predictive power for public investment, even conditional on the recent history of public investment. These are not the main results in the paper but suggest that our proxy captures something meaningful.

The consequences of political institutions have been studied intensively, typically by comparing various outcomes under alternative political regimes in cross-section and panel datasets. We complement this work by broadening the criteria for success, using indicators of commitment to the common good; taking advantage of longer spans of data and advances in both measurement and methods; and asking whether the autocratic gamble

in growth rates is mirrored in a similar pattern for long-term benevolence.

3.2 | Related Work

Our work is most closely related to a section of Besley and Kudamatsu (2008), who found that human development, as proxied by measures of health and education, is more heterogeneous under autocracy than under democracy. The measures used are life expectancy at birth and primary school enrollment. A potential drawback of this approach is that such variables, especially life expectancy, are often slow moving. This will complicate the empirical relations between them and the political regime in place at a given point in time.⁹

In spirit, our analysis is also close to that carried out by Krznic and McQuilkin, and summarized in Krznic (2020, pp. 170–172). They take the measure of ‘intergenerational solidarity’ of McQuilkin (2018) and find a positive correlation with the V-Dem Liberal Democracy Index.¹⁰ The mean of the McQuilkin index is higher for democracies than autocracies. Our starting point is similar, but we take advantage of the V-Dem dataset to construct a time-varying measure of long-term benevolence for a much longer time span. To check its validity, we investigate whether our new measure Granger-causes public investment. We then compare the distributions (not just the mean) of long-term benevolence across political regimes.¹¹

The analysis assumes that government priorities can be traced in observable outcomes. Jones and Olken (2005) found evidence that national leaders matter for growth, using exogenous transfers of power arising from deaths in office. They showed that the deaths of leaders in autocratic regimes led to changes in growth, but the deaths of leaders in democratic regimes did not.¹² The effects of autocratic leaders are found to be larger when there are relatively few constraints on their power. At least in principle, we could attempt to investigate whether exogenous transfers of power lead to changes in our measure of long-term benevolence. In practice, the short-run measurement error in our proxy may be too great for this to yield reliable results, but it would be an interesting area for further work.

Some other previous work considers outcomes other than growth, such as public goods or the quality of government. Deacon (2009) finds that democracies are better at providing roads, safe water, sanitation and education, consistent with his theoretical model. Raabe, Sander, and Schneider (2024) argue that there is a consensus that autocracies provide fewer public goods; they show that autocracy also weakens the empirical link between ethnic diversity and less public good provision. Charron and Lapuente (2010) argue that outcomes at low levels of development may be better under autocracy, while Palma and Reis (2021) studied Portugal and found that young men educated under autocracy were more likely to be literate. Autocrats do sometimes invest in public goods; an example would be the rural school-building programme in Indonesia under Suharto (Duflo 2001). But the literature on political institutions and public goods typically examines straightforward associations, positive or negative. Apart from Besley and Kudamatsu (2008), the existence of an autocratic gamble for wider outcomes has rarely been studied and remains an open question.

4 | Data and Methods

4.1 | Data Sources

Our central data source is version 14 of the V-Dem dataset (Coppedge et al. 2024; Pemstein et al. 2024). The data on growth, which we use to revisit the autocratic gamble, are from version 10.01 of the Penn World Table (Feenstra, Inklaar, and Timmer 2015). The growth data raise the issue of whether the underlying national accounts data have sometimes been distorted under autocratic rule; see Magee and Doces (2015) and Martínez (2022). We have no obvious way to adjust for that here, but our main focus is the distribution of long-term benevolence rather than growth.

The time span considered is 1960–2019, and we computed averages over six non-overlapping 10-year periods, so that country-decades are the unit of observation. We use country-decades because 10 years should be long enough for the stabilization of

a political regime and for the effects of regime priorities to be apparent in the data. The group of countries considered is first restricted to those available in the Varieties of Democracy (V-Dem) dataset (v.14), leading to a sample of 160 countries, which is then reduced further by data availability.

The classification of country-decades follows the same approach as in Monteforte and Temple (2020). We start from the classification provided by the Regimes of the World variable (RoW) in the V-Dem dataset. We then consider a country-decade one of stable democracy if, for the entire decade, it was categorized as either an electoral or a liberal democracy. Conversely, stable autocracies are countries that were categorized as closed or electoral autocracies for an entire decade. The remaining country-decades are classified as undetermined and arise where countries are partial or hybrid democracies or are undergoing a political transition. The frequencies of the three regime types are summarized in Table 1.

TABLE 1 | Dataset summary for polities, country-decades.

Polity	Country-decades	Percent
Democracies	291	33.80
Autocracies	450	52.26
Closed autocracies	151	17.54
Electoral autocracies	152	17.65
Undetermined autocracies	147	17.07
Undetermined	120	13.94

Note: This table reports frequencies of political regimes for country-decades, for a sample based on the *LTB* dataset.

4.2 | Methods

To construct a statistical proxy for long-term benevolence, we select indicators among those available in the V-Dem dataset that could reflect the relevant priorities of government. Those indicators are listed in Table 2, together with their related description. We limited our selection to indicators that were not used by the V-Dem researchers to construct the RoW index of the political regime. The six indicators are chosen to reflect commitment to effective decision-making and the long-term common good, and to choices that will benefit citizen welfare in the long term.

Note that, individually, each of these variables could be questioned as an unpersuasive measure of long-term benevolence. As we noted earlier, our argument is that, if we analyse several

TABLE 2 | V-Dem set of indicators used for PCA.

Indicator	V-Dem name	Description
Reasoned justification	v2dlreason	When important policy changes are being considered, that is, before a decision has been made, to what extent do political elites give public and reasoned justifications for their positions?
Common good	v2dlcommon	When important policy changes are being considered, to what extent do political elites justify their positions in terms of the common good?
Respect counterarguments	v2dlcountr	When important policy changes are being considered, to what extent do political elites acknowledge and respect counterarguments?
Particularistic or public good	v2dlencmps	Considering the profile of social and infrastructural spending in the national budget, how ‘particularistic’ or ‘public goods’ are most expenditures?
Education equality	v2peedueq	To what extent is high quality basic education guaranteed to all, sufficient to enable them to exercise their basic rights as adult citizens?
Health equality	v2pehealth	To what extent is high quality basic healthcare guaranteed to all, sufficient to enable them to exercise their basic political rights as adult citizens?

Note: This table lists indicators used in the principal component analysis to extract *LTB*, our measure of long-term benevolence.

Source: V-Dem dataset version 14.

together, their common variation may be informative. Once we have constructed our measure of long-term benevolence (*LTB*), we will seek to show that it carries genuine information on the extent to which governments act in the long-term interests of their citizens. Later in the paper, we will report on results which use a principal components analysis based on a smaller set of indicators; most of our results still go through in that case.

In Table 3, we present descriptive statistics for growth and the constituent indicators for our *LTB* measure, for the sample of country-decades that we will be using in much of our analysis. The table shows means, medians, standard deviations and the minimum and maximum. The standard deviations are of particular interest. Looking at the more detailed summary of the descriptive statistics provided in Table 4, we can see that our six indicators tend to vary more across autocracies than across democracies, but in some cases only modestly.

Note that the first three indicators—on the commitment to good decision-making—do not appear to be simply proxies for the extent of democracy. Our indicators have higher means and medians under democracy than under autocracy, as one might expect, but looking at the standard deviations in Table 4, these indicators vary more across autocracies than across democracies; given our interest in explaining differences among autocracies, we will return to this later in the paper. For now, note that Chandra and Rudra (2015) argue that it is the quality of public deliberation which determines economic performance, beyond the type of regime (democratic/autocratic). Differences in the

quality of deliberation help to explain why performance varies across autocracies in particular.

But looking at Table 4, the autocratic-democratic ratio of standard deviations is much greater for the growth rate than the six indicators underlying *LTB*. It is this disconnect between the ‘autocratic gamble’ for growth and the lack of a similarly sharp contrast for the *LTB* indicators that we will investigate further, which leads to the most novel of our findings.

Results from the principal component analysis are summarized in Table 5 and Figure 1.¹³ The variables are first normalized so that higher values correspond to greater long-term benevolence; we therefore expect all the loadings for the first principal component to have the same sign. The first principal component accounts for around two-thirds of the total variance, with positive loadings of roughly equal magnitude on all the indicators. The other components individually account for no more than 15% of the total variance. The pattern of results suggests that the first principal component captures an important common component and hence may carry genuine information about long-term benevolence.

In the top panel of Table 6, we list the 20 country-decades with the highest long-term benevolence in the most recent decade, while in the lower panel we list the lowest scorers. We can see that the recent good performers include Denmark, Germany, Norway, Sweden and Switzerland. Countries where the index suggests low recent long-term benevolence include Azerbaijan, Sudan, Syria, Venezuela and Yemen. (In the appendix [Table A1], we present a version of this table which

TABLE 3 | Summary statistics for growth and *LTB* constituent indicators, 1960–2019.

	<i>n</i>	Mean	Median	St. dev.	Min	Max
Stable country-decades						
Growth rate	861	1.83	1.95	2.97	−13.25	16.37
Reasoned justification	861	0.49	0.53	1.29	−2.59	3.76
Common good	861	0.43	0.63	1.12	−3.00	2.83
Respect counterarguments	861	0.20	0.35	1.33	−3.22	3.01
Particularistic or public good	861	0.52	0.66	1.18	−2.79	3.16
Education equality	861	0.44	0.45	1.54	−3.16	3.54
Health equality	861	0.39	0.29	1.57	−3.05	3.66
Excluding high-income countries						
Growth rate	726	1.74	1.97	3.16	−13.25	16.37
Reasoned justification	726	0.20	0.26	1.13	−2.59	2.95
Common good	726	0.25	0.40	1.10	−3.00	2.83
Respect counterarguments	726	−0.04	0.14	1.26	−3.22	2.55
Particularistic or public good	726	0.28	0.45	1.09	−2.79	2.90
Education equality	726	0.08	−0.09	1.36	−3.16	3.50
Health equality	726	0.01	−0.24	1.36	−3.05	3.29

Note: This table reports summary statistics for growth rates and *LTB* constituent indicators, 1960–2019, using pooled country-decades and excluding those where the political institutions in place are classed as undetermined.

TABLE 4 | Regimes compared on growth and *LTB* constituent indicators, 1960–2019.

	<i>n</i>	Mean	Median	St. dev.	Min	Max
Growth rate						
Stable country-decades						
Democracies	291	2.37	2.21	1.83	−4.46	10.45
Autocracies	450	1.40	1.54	3.44	−13.25	13.33
Excluding high-income countries						
Democracies	162	2.56	2.67	2.10	−4.46	10.45
Autocracies	447	1.36	1.49	3.42	−13.25	13.33
Reasoned justification						
Stable country-decades						
Democracies	291	1.69	1.70	0.85	−0.66	3.76
Autocracies	450	−0.32	−0.35	1.00	−2.59	2.26
Excluding high-income countries						
Democracies	162	1.31	1.28	0.72	−0.66	2.95
Autocracies	447	−0.31	−0.35	1.00	−2.59	2.26
Common good						
Stable country-decades						
Democracies	291	1.25	1.29	0.66	−0.97	2.61
Autocracies	450	−0.13	0.09	1.09	−3.00	2.83
Excluding high-income countries						
Democracies	162	1.08	1.15	0.71	−0.97	2.42
Autocracies	447	−0.12	0.08	1.09	−3.00	2.83
Respect counterarguments						
Stable country-decades						
Democracies	291	1.40	1.43	0.64	−0.24	3.01
Autocracies	450	−0.66	−0.63	1.11	−3.22	1.93
Excluding high-income countries						
Democracies	162	1.21	1.23	0.61	−0.24	2.55
Autocracies	447	−0.65	−0.62	1.11	−3.22	1.93
Particularistic or public good						
Stable country-decades						
Democracies	291	1.38	1.33	0.86	−1.02	3.16
Autocracies	450	−0.05	−0.04	1.10	−2.80	2.90
Excluding high-income countries						
Democracies	162	0.99	0.99	0.79	−1.02	2.70
Autocracies	447	−0.05	−0.04	1.10	−2.80	2.90
Education equality						
Stable country-decades						
Democracies	291	1.57	1.87	1.36	−2.54	3.54

(Continues)

TABLE 4 | (Continued)

	<i>n</i>	Mean	Median	St. dev.	Min	Max
Autocracies	450	-0.22	-0.46	1.28	-3.16	2.69
Excluding high-income countries						
Democracies	162	0.86	0.77	1.34	-2.54	3.50
Autocracies	447	-0.22	-0.47	1.28	-3.16	2.69
Health equality						
Stable country-decades						
Democracies	291	1.71	2.01	1.27	-1.47	3.66
Autocracies	450	-0.39	-0.66	1.24	-3.05	3.09
Excluding high-income countries						
Democracies	162	1.07	1.19	1.23	-1.47	3.29
Autocracies	447	-0.39	-0.66	1.25	-3.05	3.09

Note: This table reports summary statistics for growth rates and *LTB* constituent indicators, 1960–2019, using pooled country-decades and excluding those where the political institutions in place are classed as undetermined.

TABLE 5 | Results of principal component analysis.

Variable	1st principal component		2nd principal component	
	Loading	Correlation	Loading	Correlation
Reasoned justification	0.431	0.863	-0.360	-0.345
Common good	0.408	0.816	-0.243	-0.234
Respect counterarguments	0.387	0.774	-0.468	-0.449
Particularistic or public good	0.409	0.819	-0.012	-0.011
Education equality	0.394	0.790	0.584	0.560
Health equality	0.418	0.838	0.502	0.481
Variance explained (percent)	66.76		15.34	

Note: This table reports the values of the loadings and the correlations between the first two principal components and the corresponding variables. See Table 2 for variable definitions.

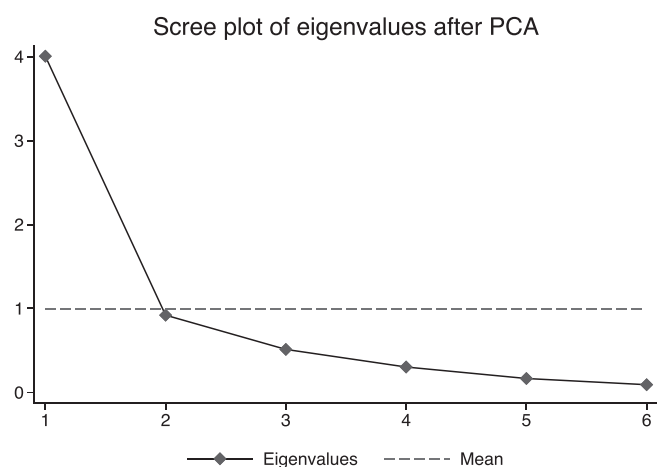


FIGURE 1 | Scree plot displaying the proportion of variance explained by each component.

draws on all the country-decades, rather than just the most recent period.)

We can see that almost all the best performers on *LTB* are democracies; the only autocracy to figure is Singapore.¹⁴ There are almost no democracies among the worst *LTB* performers; indeed, they are all autocracies apart from Paraguay. This is strikingly different from the patterns seen when looking at growth rates (e.g., Monteforte and Temple 2020) and already suggests that variation in *LTB* may be unable to explain the autocratic gamble.

5 | Checking the Measure

Any attempt to proxy a latent variable will be open to question, and so in this section, we ask whether our measure of long-term benevolence seems to be capturing the concept of interest.

TABLE 6 | Highest and lowest *LTB* in most recent country-decade.

Country	Years	<i>LTB</i>	Growth rate (%)	Democracy score
<i>Highest LTB</i>				
Norway	2010–2019	4.46	0.42	1
Denmark	2010–2019	4.19	1.49	1
Luxembourg	2010–2019	4.06	0.90	1
Germany	2010–2019	3.70	1.59	1
Switzerland	2010–2019	3.48	0.91	1
Sweden	2010–2019	3.43	1.68	1
Japan	2010–2019	3.39	1.40	1
Costa Rica	2010–2019	3.38	2.46	1
Netherlands	2010–2019	3.30	1.13	1
Portugal	2010–2019	3.24	1.16	1
Iceland	2010–2019	3.20	1.99	1
Uruguay	2010–2019	3.14	2.69	1
Estonia	2010–2019	2.91	3.74	1
France	2010–2019	2.89	0.95	1
Bhutan	2010–2019	2.85	4.34	1
Italy	2010–2019	2.83	0.01	1
South Korea	2010–2019	2.80	2.88	1
Australia	2010–2019	2.74	0.85	1
Canada	2010–2019	2.71	1.16	1
Singapore	2010–2019	2.64	3.16	0
<i>Lowest LTB</i>				
Madagascar	2010–2019	−1.72	0.19	0
Tajikistan	2010–2019	−1.77	4.47	0
Haiti	2010–2019	−1.82	−0.01	0
Pakistan	2010–2019	−1.87	2.00	0
Guinea	2010–2019	−1.91	3.39	0
Central African Republic	2010–2019	−1.91	−2.06	0
Honduras	2010–2019	−1.98	1.79	0
Paraguay	2010–2019	−2.10	2.93	1
Democratic Republic of the Congo	2010–2019	−2.13	2.73	0
Cambodia	2010–2019	−2.23	5.22	0
Burundi	2010–2019	−2.31	0.07	0
Bangladesh	2010–2019	−2.36	5.43	0
Chad	2010–2019	−2.40	0.04	0
Cameroon	2010–2019	−2.51	1.73	0
Turkmenistan	2010–2019	−2.67	6.29	0
Azerbaijan	2010–2019	−2.79	0.36	0

(Continues)

TABLE 6 | (Continued)

Country	Years	LTB	Growth rate (%)	Democracy score
Sudan	2010–2019	−2.81	1.82	0
Venezuela	2010–2019	−2.91	−10.34	0
Yemen	2010–2019	−3.02	−10.64	0
Syria	2010–2019	−3.92	−5.20	0

Note: 0 denotes country-decades classified as stable autocracies and 1 denotes stable democracies.

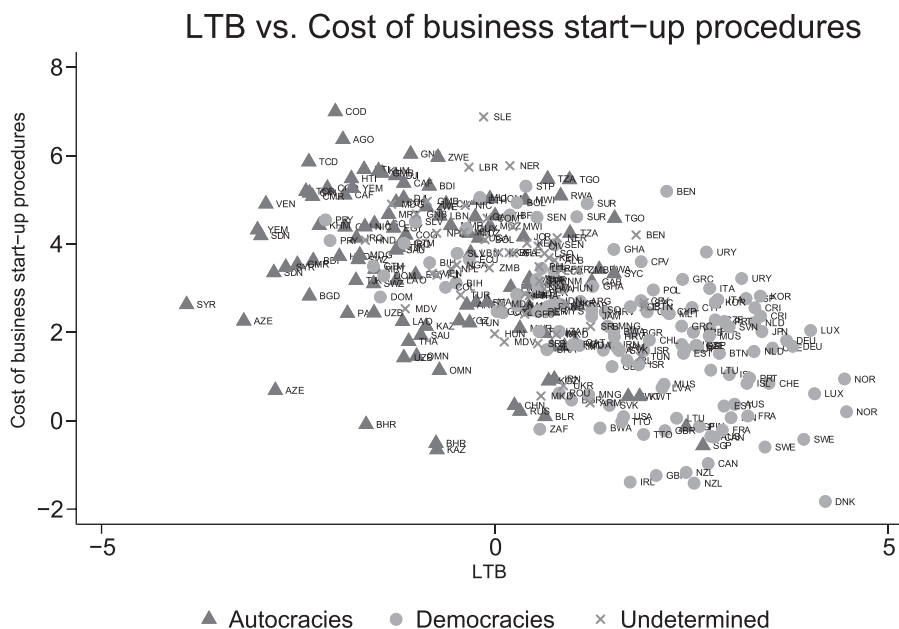


FIGURE 2 | Scatter plot of cost of business start-up procedures against LTB.

5.1 | Scatter Plots

We first plot the measure against other variables that might be associated with long-term benevolence. These variables are taken from the World Development Indicators, version 27 (World Bank 2024).¹⁵ They include (i) the (log of the) cost of business start-up procedures, measured as percentage of gross national income per head, to get a proxy for the ease of doing business within a country; (ii) a measure of a methodological assessment and overall statistical capacity, spanning the range 0–100, as a proxy for past investment in the quality of information on which policies can be based; (iii) a rating of policies for environmental sustainability, measured on a scale of 1 to 6, where higher values reflect better policies; and (iv) government expenditure on education, measured as a share of GDP. These can all be seen as reflecting the long-term quality of government but lack of long-term data precludes them from inclusion in the earlier principal components analysis.

We can see from Figures 2–6 that, in line with expectations, the (log of the) cost of business start-ups is negatively correlated with *LTB*, while the other variables are positively correlated

with *LTB*. The associations are strong enough to suggest that our measure of *LTB* reflects genuine differences across governments, despite the measurement error intrinsic to an exercise of this kind. Moreover, these associations are in areas such as environmental sustainability or the quality of statistics, where good policies have benefits that accrue over time.

5.2 | Predicting Public Investment

Ideally, we would have stronger grounds for seeing our *LTB* measure as capturing the concept of interest. In this subsection, we ask whether our measure helps to predict the evolution of public investment. To forestall misinterpretation, these are not the main results of the paper, but a way to assess the usefulness of the approach ahead of the main results in the next two sections.

Our measure of the share of public investment in GDP is computed as the difference between total and private gross fixed capital formation expressed as shares of GDP, using data from the World Development Indicators.¹⁶ If y_{it} is the share of public

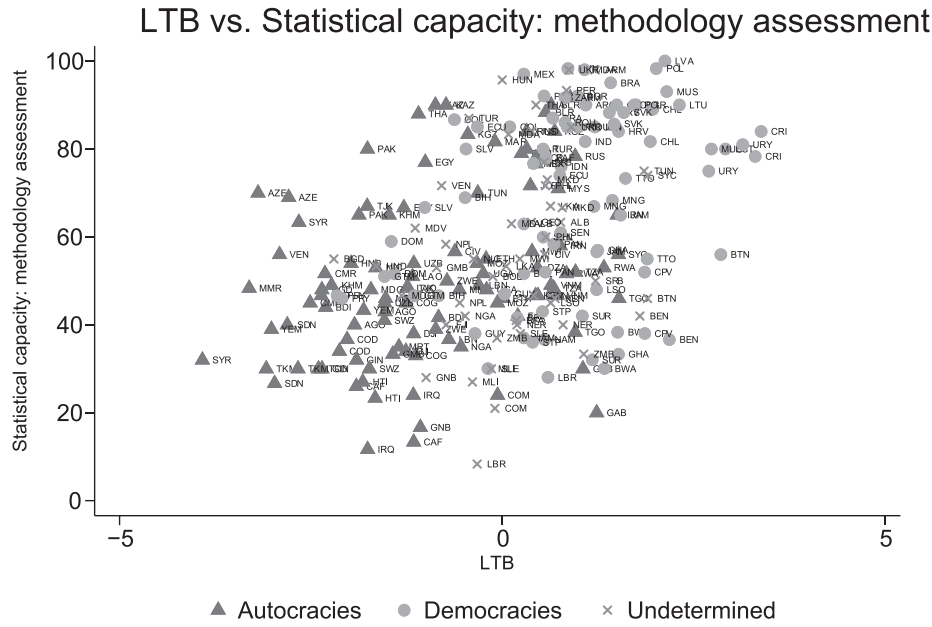


FIGURE 3 | Scatter plot of statistical capacity: methodology assessment against LTB.

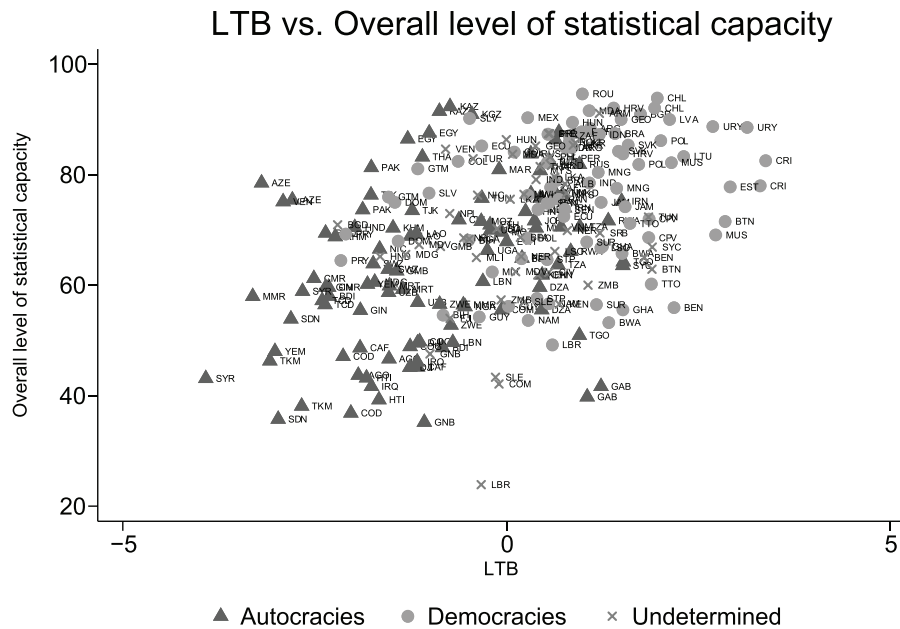


FIGURE 4 | Scatter plot of overall level of statistical capacity against LTB.

investment in GDP, we are interested in estimating a panel data model with country fixed effects and time effects:

$$y_{it} = \delta_t + \beta_1 y_{it-1} + \beta_2 y_{it-2} + \alpha_1 LTB_{it-1} + \alpha_2 LTB_{it-2} + \eta_i + v_{it} \quad (1)$$

First of all, we estimate the model by Within Groups, using 5-year averages to smooth out short-run effects. The results are shown in columns I and II of Table 7. It can be seen that the

first lag of *LTB* is significant at the 5% level, so (our proxy for) the extent of long-term benevolence Granger-causes public investment: Increases in long-term benevolence are followed by increases of the public investment share.

A potential objection to the Within Groups estimates is that they are subject to the Nickell bias (Nickell 1981). The severity of this bias is typically diminishing in the time span of the data; even when using 5-year averages, we have up to 10 time series

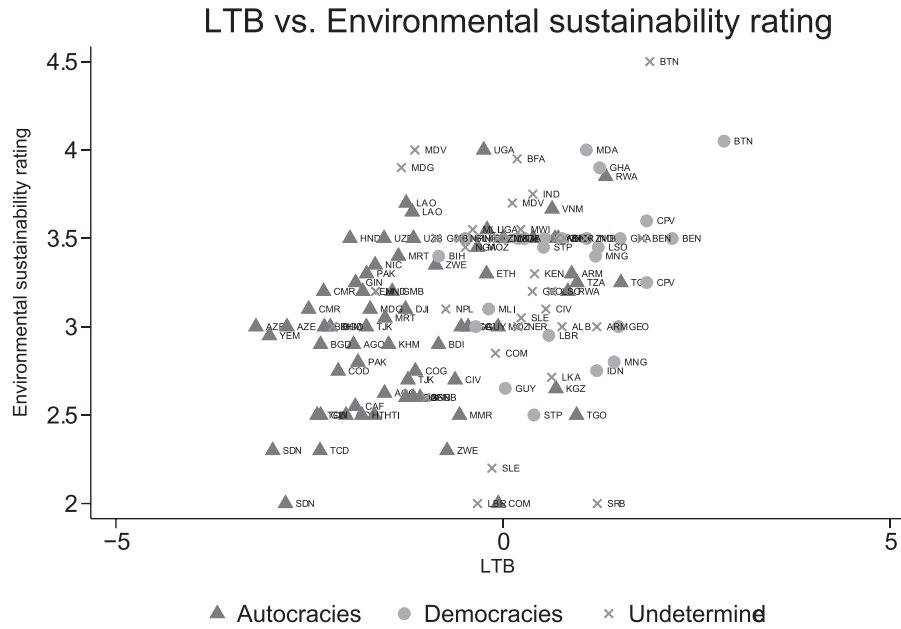


FIGURE 5 | Scatter plot of environmental sustainability rating against LTB.

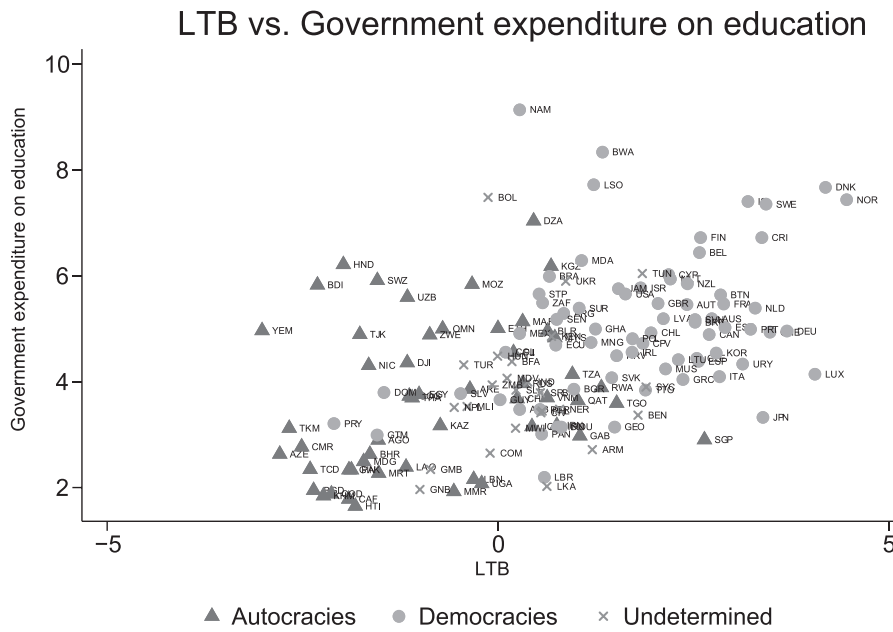


FIGURE 6 | Scatter plot of government expenditure on education against LTB—decade averages.

observations over 50 years, and then the Nickell bias should be fairly modest.¹⁷ But for those concerned about this dimension of robustness, we also report GMM estimates, using the estimator of Arellano and Bond (1991).

In our application, we use two lags of both public investment and *LTB*. Following Arellano (2003, p. 120) and Huang and

Temple (2005), we estimate (1) using a version of the Arellano-Bond estimator, here using moment conditions based on lagged levels for $t - 3$ and (where available) $t - 4$. In doing this, we lose one cross-section, since we need at least the lag at $t - 3$ to be available for the moment conditions for the first period.¹⁸ We do not use more distant lags to avoid the problem of ‘too many instruments’ (Roodman 2009).

TABLE 7 | Dynamic panel estimations and Granger-causality.

	Within groups		Arellano-Bond	
	I	II	III	IV
	b/se	b/se	b/se	b/se
Pub inv ($t - 1$)	0.4551*** (0.093)	0.4494*** (0.092)	0.4244 (0.407)	0.5175*** (0.145)
Pub inv ($t - 2$)	-0.1565** (0.073)	-0.1494** (0.073)	-0.04966 (0.272)	-0.08992 (0.107)
LTB ($t - 1$)		0.5507** (0.266)		1.542* (0.802)
LTB ($t - 2$)		-0.2529 (0.189)		-0.4954 (0.438)
Sample size	365	365	280	280
N	85	85	68	68
T	10	10	9	9
Hansen (p -value)			0.485	0.686
A-B test-AR1 (p -value)			0.539	0.062
A-B test-AR2 (p -value)			0.600	0.301

Note: This table reports estimates of short T panel data models allowing for country fixed effects, where the share of public investment in GDP is the dependent variable. In each case, we include a full set of time effects (not reported) and report standard errors clustered by country, based on the robust two-step estimator due to Windmeijer (2005).

* $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

We use the robust two-step estimator due to Windmeijer (2005), cluster the standard errors by country, and include a full set of time effects in every regression. We use the DIF-GMM estimator of Arellano and Bond (1991) rather than the SYS-GMM estimator of Arellano and Bover (1995) and Blundell and Bond (1998). Public investment is less persistent than some macroeconomic series, which means lagged levels are less likely to be weak instruments, and we can proceed without the extra mean stationarity assumptions needed for SYS-GMM.

The precise moment conditions we use are

$$E[\Delta v_{it} y_{it-j}] = 0 \quad j = 3, 4 \quad (2)$$

$$E[\Delta v_{it} LTB_{it-k}] = 0 \quad k = 3, 4 \quad (3)$$

while the time effects are naturally treated as strictly exogenous.

The results are shown in columns III and IV of Table 7. In column IV, the serial correlation test of Arellano and Bond (1991) indicates the existence of first-order serial correlation but not second-order, corresponding to the desired pattern.¹⁹ Again, the first lag of LTB is significant, although this time only at the 10% level.

The GMM results should be seen as tentative. As is common with these estimators, the results are sensitive to alternative instrument sets and decisions over collapsing sections of the

instrument matrix; LTB is sometimes significant, but not always. In the models we report, the Hansen test does not reject the over-identifying restrictions, and difference-in-Hansen tests do not reject the validity of subsets of the instruments, but the Sargan test (which is not robust to heteroskedasticity) does reject the over-identifying restrictions. Given this, and the sensitivity of the results to alternative instrument sets, the results from Within Groups should perhaps be given more weight. In those results, there is clear evidence that LTB Granger-causes public investment.

The use of panel data, eliminating country fixed effects through Within Groups or differencing, makes it hard to explain this finding in terms of time-invariant differences in fiscal capacity. To the extent that fiscal capacity is time-varying, we could see new investments in that capacity as related to shifts in long-term benevolence; as we have already noted, empirical concepts of the quality of government will inevitably overlap. Further, we find a positive effect even though public investment can sometimes be used for repression; see Gonzalez et al. (2024).

At this point, it might seem natural to investigate the relationship between LTB and growth. We deliberately eschew this, for two reasons. First, studying their relation in depth would require a paper in itself; and second, we do not want to digress from the central aim of this paper, namely, to see if there is evidence for the ‘heterogeneous altruists’ explanation of the

autocratic gamble. The empirical approach we adopt for that question does not stand or fall on whether long-term benevolence is associated with faster growth; we can be agnostic on that question.

6 | The Association With Democracy

Thus far, we have presented evidence that *LTB* may capture aspects of the long-term benevolence of governments. In this section, we study how our measure varies between autocracies and democracies. An obvious concern is selection: perhaps countries select into democracy depending on observable and unobservable variables that may confound estimates of the effect of democracy. We use two different approaches: first, methods that take into account selection on observables under an ignorability assumption; and second, we use instruments for democracy, based on the connection between potential for irrigation and modern-day autocracy (Bentzen, Kaarsen, and Wingender 2017) and the connection between historical exposure to the Church and modern-day democracy (Schulz 2022). Both approaches have individual weaknesses, but taken together, the results make it harder to argue that autocracies are better than democracies at advancing the long-term interests of their citizens.

6.1 | Selection on Observables

In this subsection, we consider treatment effect estimation under selection on observables, before considering IV estimation in the next subsection. We examine how the average treatment effect (ATE) of democracy on *LTB* varies with the inclusion of exogenous covariates, while assuming that the ATE is constant across countries but may vary over time.²⁰ As well as presenting simple models that adjust for these variables, we also use a LASSO-based approach to estimate the average treatment effect in the presence of potentially many controls and a logit selection equation.²¹

We will define our treatment, democracy, to be a binary variable denoted d . The potential outcomes for *LTB* without and with democracy will be denoted by y_0 and y_1 , respectively, while x represents our covariates. To estimate the average treatment effect consistently, we need overlap to hold: For each combination of covariates in the population, there should be a strictly positive chance of observing both democracy and autocracy. We also need ignorability in mean:

$$E(y_0|x, d) = E(y_0|x)$$

$$E(y_1|x, d) = E(y_1|x)$$

as in Wooldridge (2010, p. 909), for example. Note that this assumption does not rule out conditional variances $Var(y_0|x, d)$ and $Var(y_1|x, d)$ that vary with d .

Given the size of the sample, the large number of candidate exogenous controls, and the potential for lack of overlap, we use regression adjustment rather than more general methods. The array of controls is loosely based on the classic paper

on institutions and comparative development by Acemoglu, Johnson, and Robinson (2001). These include a set of religion and legal origin dummies, following standard arguments on the determinants of institutions (see, for example, La Porta et al. 1999) as well as geographic and climate proxies, such as continental dummies, distance from coast, a dummy for being landlocked, and absolute latitude, following McArthur and Sachs (2001) on the relationship between geography and institutions. We also include the log of population density on the basis that this may influence the transition to democracy (Epstein et al. 2006). In the appendix (Table B1), we report detailed variable descriptions and data sources, while in the Supporting Information, we provide an alternative set of estimates which includes ethnolinguistic fragmentation as an additional control (but with smaller sample sizes).

We work with a binary indicator of democracy, which takes the value of one for stable democracies and zero for stable autocracies and ‘undetermined’ regimes. We estimate cross-sections decade by decade and report the results in the upper panel of Table 8. Since the dependent variable is a proxy and measured with error, heteroskedasticity is likely; in the case of treatment effects with and without controls, but not with LASSO, we report standard errors that are heteroskedasticity-consistent using the ‘hc3’ estimates of MacKinnon and White (1985).

The table shows that adding exogenous covariates lowers the coefficient on democracy, especially in the final three cross-sections, but it remains significant at the 1% level. Since the controls do lead to lower ATEs, there is evidence of selection on observables; we address this and selection on unobservables shortly. Compared to simple regression adjustment, the use of LASSO leads to treatment effects that are lower in three decades out of six. We have confirmed that the ATEs are different between models with and without controls using Hausman tests, which reject equality for most of the decades; for discussion of testing approaches, see Pei, Pischke, and Schwandt (2019).

There are other grounds for caution: We have assumed the effects of controls are linear, and we have allowed for time series heterogeneity in the treatment effect, but not cross-section heterogeneity. What we can say is that selection on this set of observables is not enough to explain away the partial correlation between *LTB* and the extent of democracy. Long-term benevolence and democracy remain positively associated.

Indeed, the partial correlation is sizeable. In the LASSO results, the presence of democracy raises *LTB* by 1.50 of a standard deviation in the 1960s and by 1.57, 1.32, 0.74, 0.81, and 0.98 in the five subsequent decades, respectively. These are large effects. Given the strength of the association, there is not much support here for the contention of Mulligan, Gil, and Sala-i-Martin (2004) that democracies and non-democracies look alike in their public policies.²²

6.2 | IV Estimates

Given the indications of some selection on observables, selection on unobservables will be more of a concern. It is not clear that this would always work against us. For example, it has

TABLE 8 | Average treatment effect estimates.

	I	II	III	IV	V	VI
	1960s	1970s	1980s	1990s	2000s	2010s
	b/se	b/se	b/se	b/se	b/se	b/se
ATE	3.407*** (0.283)	3.629*** (0.271)	3.382*** (0.253)	2.627*** (0.219)	2.227*** (0.226)	2.250*** (0.225)
ATE with controls	3.228*** (0.446)	2.854*** (0.394)	2.396*** (0.358)	1.803*** (0.245)	1.536*** (0.211)	1.681*** (0.256)
ATE using LASSO	2.997*** (0.251)	3.194*** (0.264)	2.583*** (0.298)	1.322*** (0.472)	1.422*** (0.273)	1.717*** (0.182)
σ_{LTB}	1.999	2.038	1.955	1.792	1.750	1.759
N	98	125	125	148	149	149
ATE (2SLS)	3.956*** (0.614)	3.316*** (0.500)	3.941*** (0.977)	3.184*** (0.466)	3.911*** (0.699)	3.924*** (0.798)
ATE (control function)	4.316*** (0.609)	6.026*** (1.590)	3.928*** (1.101)	3.043*** (0.520)	2.187*** (0.843)	2.231*** (0.734)
2SLS first-stage F -stat	64.085	61.408	4.743	16.947	9.657	7.351
Hansen J (p -value)	0.429	0.757	0.519	0.396	0.418	0.828
σ_{LTB} (2SLS)	2.013	2.051	1.969	1.792	1.760	1.797
N (2SLS)	91	111	111	131	132	132
σ_{LTB} (control function)	2.321	2.092	1.969	1.784	1.629	1.792
N (control function)	54	100	111	130	117	130

Note: The dependent variable is average LTB over that decade. We report estimates of the average treatment effect (ATE) of a binary democracy indicator on LTB , decade by decade. In the upper panel, the first row presents the ATE from a bivariate regression, while the second row adjusts for exogenous covariates. The estimates with LASSO selection of covariates are presented in the third row. We report heteroskedasticity-robust ('hc3') standard errors based on MacKinnon and White (1985) for the first two cases and robust standard errors when using LASSO. The lower panel presents IV estimates where democracy is instrumented with the inherent potential for irrigation from Bentzen, Kaarsen, and Wingender (2017) and Church exposure from Schulz (2022), but excluding religion dummies (see text). The first row presents estimates from 2SLS with heteroskedasticity-robust standard errors and a corresponding first-stage F statistic and Hansen J p -values as reported. In the second row, we report ATE estimates using the same instruments but a control function approach, with robust standard errors.

* $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

sometimes been argued that Confucian societies are more likely to be autocratic, and may also be ones where leaders take a long-term view; in that case, taking into account selection would strengthen, not weaken, a positive association between democracy and long-term development outcomes.²³

In principle, we could examine potential sensitivity to the role of unobservables.²⁴ Our preferred approach is to combine two sources of instruments. The first is an irrigation-based instrument, taken from Bentzen, Kaarsen, and Wingender (2017). They show that an exogenous geographic characteristic, the share of agricultural land inherently suitable for irrigation, is correlated with modern-day political institutions: To be more specific, countries with land with irrigation potential are about six points less democratic on the 21-point Polity2 scale than countries with rain-fed agriculture.

The second instrument source is medieval Church exposure, taken from Schulz (2022). He argues that the Catholic Church's

medieval marriage regulations worked to dissolve strong kin networks, thereby contributing to the emergence of participatory institutions at both the country and the municipal level. In his cross-country results, ancestor-adjusted Eastern and Western Church exposure are strongly positively associated with the Polity IV democracy index.

This suggests using irrigation potential and Eastern and Western Church exposure as IVs for our binary democracy indicator, where the exclusion restriction requires these instruments to be uncorrelated with the error term in a model for LTB . These exclusion restrictions are open to criticism, and there are other caveats to our results, so the results of this subsection should be interpreted cautiously.

The results are shown in the lower panel of Table 8. The Schulz (2022) instruments turn out to be noticeably stronger in the first stage than the irrigation-based instrument. We first present results from a simple 2SLS estimator, with a small set

of exogenous covariates. In these results, democracy typically has a stronger effect on *LTB* than in the upper panel, which could be attributed to some combination of measurement error in the democracy indicator, selection on unobservables in the upper panel, or endogeneity of the instruments. The latter is the main concern. For all six decades, the overidentifying restrictions are not rejected, as the Hansen test *p*-values indicate. But the strength of the instruments in the first stage declines when a full set of religion dummies is added to the models (results not shown) and more generally the small sample sizes rule out using a larger set of exogenous covariates. This means that we should not push the interpretation of the 2SLS results too far.

Given that we have a binary treatment variable, we also present IV results based on a control function: In these estimates, the first stage is a probit, and we use GMM to take into account endogeneity and the sampling error in the second-stage regressors.²⁵ We are again using a reduced set of covariates. Once more, long-term benevolence and democracy are positively associated, with effects rather larger than in the earlier LASSO estimates.

In summary, the results of this subsection come with major caveats. The irrigation-based instrument lacks strength in the first stage of 2SLS, and the exclusion restrictions for all the instruments could be questioned. In addition, the strength of the instruments is weakened when adding a wider range of exogenous controls. But it is also worth pointing out that, although the association between *LTB* and democracy is of interest, it is really the existence of an autocratic gamble in development outcomes that we are most interested in, and that will be the subject of the next section.

There may still be some value in the analysis of this section. We agree with Deaton (2010, p. 425) that a systematic enquiry will often be preferable to making selective use of historical examples, which has been a common way to think about our questions of interest. We can at least narrow the space of admissible claims; in this case, to explain our results away, a firm believer in long-term benevolence under autocracy would now need to make strong assumptions about the extent and nature of selection, which may or may not be plausible. Note that such an account would have not only to posit selection but also to explain why none of our approaches resolve the problem. Our results at least work to shift the burden of proof, and this is sometimes the most that can be achieved in complex debates over historical experiences in a world with a relatively small number of countries to learn from.

7 | The Autocratic Gamble

It is interesting that, on average, long-term benevolence is higher under democracy, but we are mainly interested in the autocratic gamble. Can differences in long-term benevolence explain the tendency for growth to vary more across autocracies than across democracies? This is a question about distributions rather than conditional means. In this section, we compare the distributions of long-term benevolence between democracies and autocracies. These comparisons will show

that autocracies rarely score especially highly on long-term benevolence, so its variation will not readily explain the autocratic gamble.

We first present the distributions of growth rates for autocracies and democracies, to indicate the support for an autocratic gamble in our dataset. The Tukey box-plots are shown in the upper panel of Figure 7, while kernel density estimates can be found in the lower panel of the same figure. These confirm the pattern established by previous researchers: The variance of growth rates across autocracies clearly exceeds the variance of growth rates across democracies.²⁶

Next, we present the Tukey box-plots and kernel density estimates for our measure of long-term benevolence, in Figure 8. It can be seen that the distribution of *LTB* for autocracies is clearly displaced well to the left of that for democracies. There is no evidence that some autocracies especially excel in commitment to the long-term common good; on average, democracies seem to do better, as we found in the earlier treatment effect regressions. Note, also, that the autocratic gamble is typically framed as an unconditional relation—a difference between democracies and autocracies and a relatively stark one—and hence a comparison of the two distributions of *LTB* can be informative about the explanatory power of differences in long-term benevolence, without the need for controls. Given Figure 8, someone defending the idea that autocrats govern well for the long term would probably

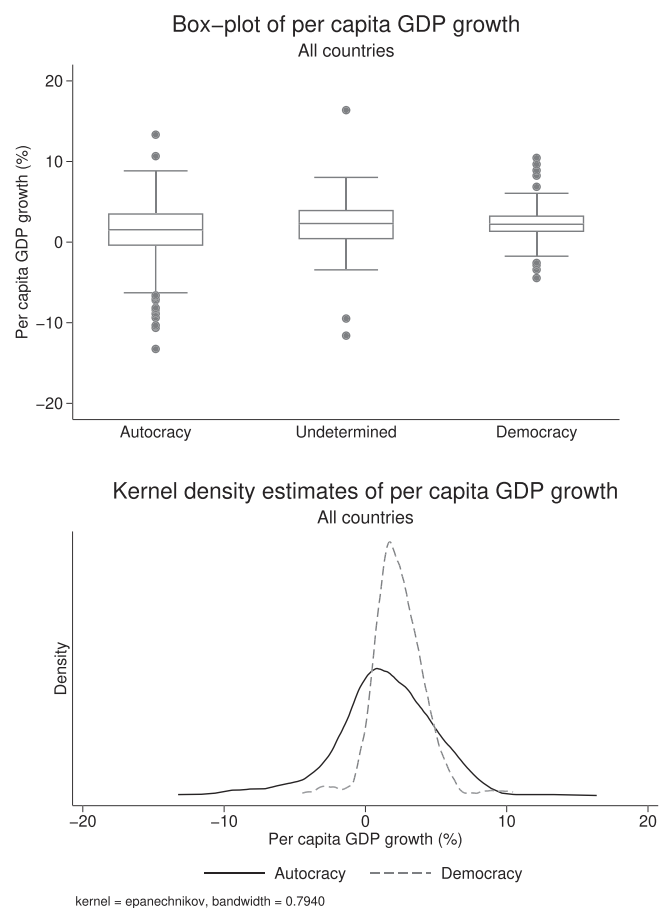


FIGURE 7 | Comparing distributions of growth rates across political regimes.

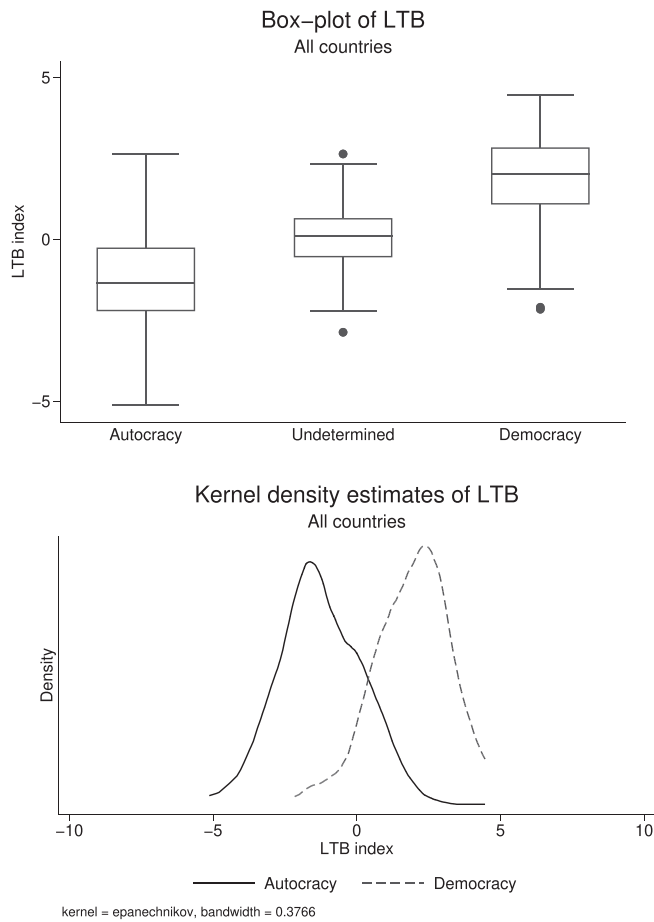


FIGURE 8 | Comparing distributions of *LTB* across political regimes.

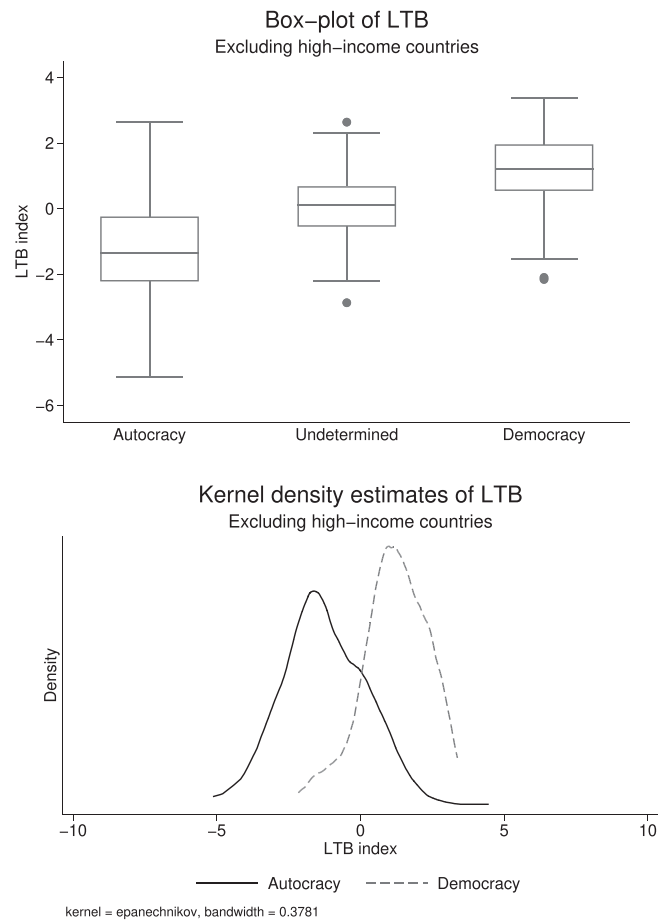


FIGURE 9 | Distributions of *LTB* excluding high-income countries.

need to make a relatively contorted argument about the role of other factors.

Since many democracies are high-income countries, we also consider the variation of *LTB* when we exclude high-income countries. (The countries excluded are the twenty countries with the highest GDP per head in 1960, plus Germany, Greece, Japan, Portugal and Spain, minus Argentina and Iran.) These patterns are shown in Figure 9. We then distinguish between closed autocracies and electoral autocracies, as in Figures 10 and 11. In all these cases, the patterns are similar to those previously shown. Contrary to popular belief, we find no evidence that some autocracies score especially highly for long-term benevolence relative to democracies.

To establish this with greater rigour, we next plot empirical cumulative distribution functions in Figure 12: upper panel for growth and lower panel for *LTB*. Denote the cumulative distribution function of *LTB* under democracy by $F_d(LTB)$ and that of *LTB* under autocracy by $F_a(LTB)$. It should be clear from the figure that the distribution of *LTB* for democracies first-order stochastically dominates the distribution of *LTB* for autocracies. That is, we are interested in whether

$$F_d(LTB) \leq F_a(LTB)$$

with strict inequality for at least part of the support of *LTB*. In words, where this condition holds with strict inequality, for any

chosen level of *LTB*, a higher share of democracies than autocracies will exceed that level. Much more loosely, this corresponds to a probability density under autocracy that is displaced firmly to the left of that under democracy.

The thick horizontal line in the figures indicates those sections of the support where the cumulative distribution functions are significantly different from each other; this uses the method of Goldman and Kaplan (2018) and the associated software of Kaplan (2019). This can be used to assess first-order stochastic dominance; we have also confirmed this dominance for the case of *LTB* more formally, using the approach of Davidson and Duclos (2000).²⁷ Not surprisingly, given the kernel density plots and empirical cumulative distribution functions already shown for the growth rate, there is no first-order stochastic dominance in the case of that variable.

8 | Robustness

We have sought to measure long-term benevolence by using six indicators to infer commitment to effective decision-making and the long-term common good, including choices that will benefit human capital and other public goods in the long term. But one possible objection to our results is that some of the chosen indicators are too close for comfort to aspects or constituents of democratic decision-making, so that the association between them is built into the analysis from the start. Perhaps, in Section 6

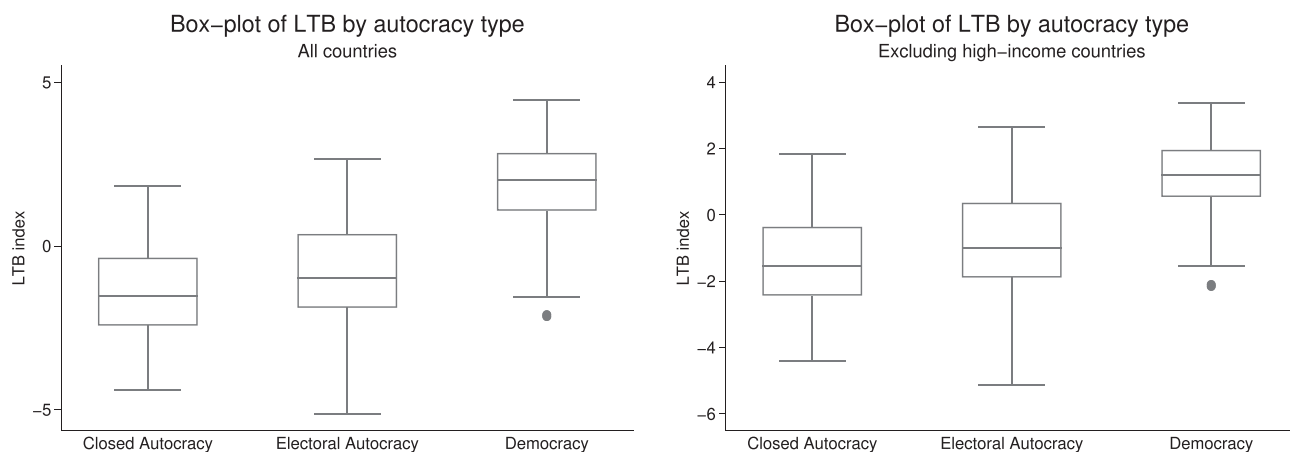


FIGURE 10 | Boxplots of *LTB* by autocracy type, total sample and excluding high-income countries.

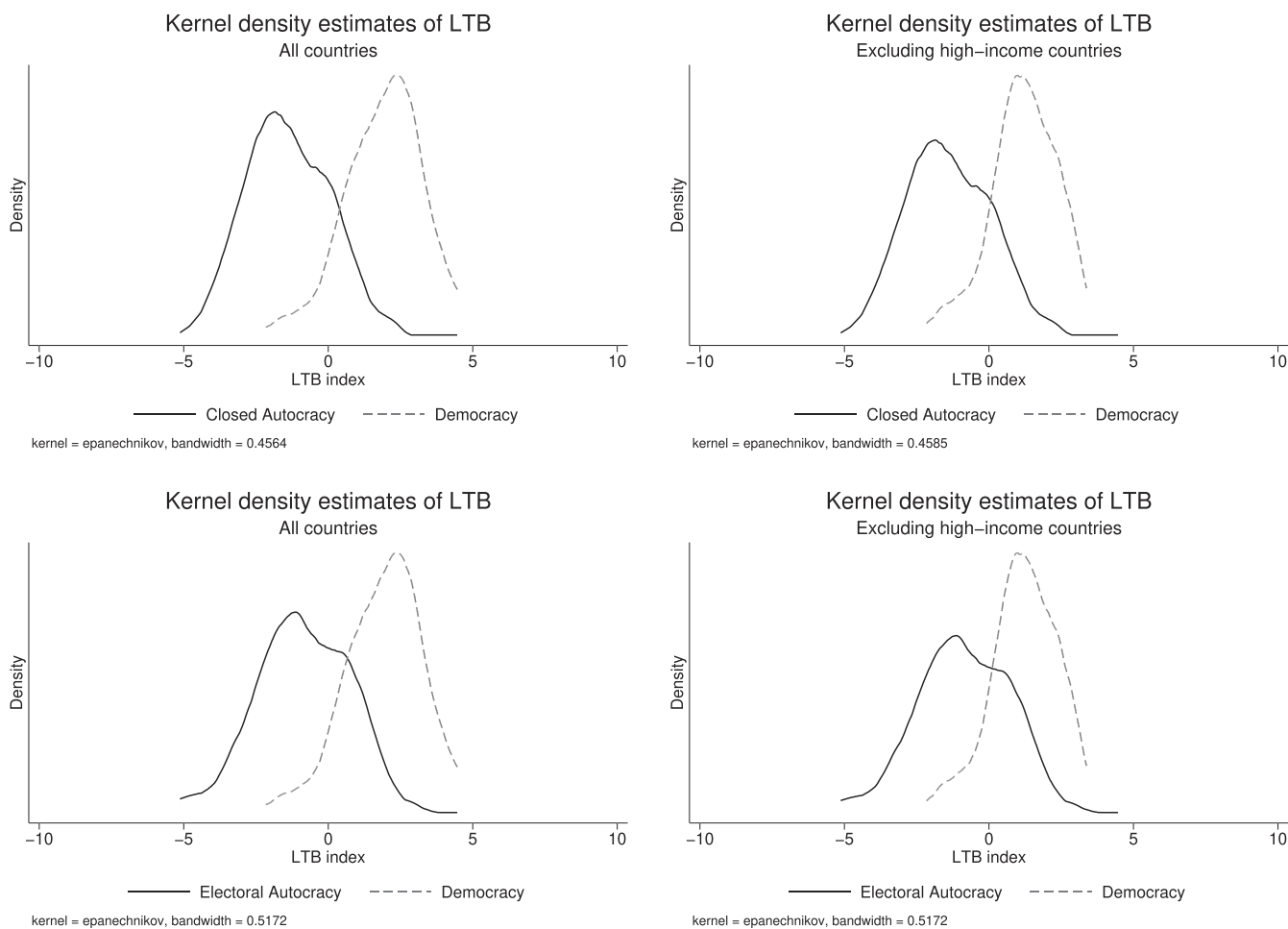


FIGURE 11 | Kernel density estimates of *LTB* by autocracy type, total sample and excluding high-income countries.

above, we are just a little too close to finding that some outcomes intrinsic to democratic procedures are more likely to be found under democracy.

As noted earlier, in Table 4, we can see that the three indicators we use for good decision-making indeed have higher average scores under democracy, as one might expect; but they also vary among both democracies and autocracies, and the variation is

greater under autocracy (see the standard deviations in the second to fourth panels of the table). This suggests that our *LTB* measure is likely to contain useful information about commitment to good policies, rather than simply proxying for the extent of democracy.

For readers who are not satisfied on this point, in this section we briefly consider what happens when we consider a narrower set of

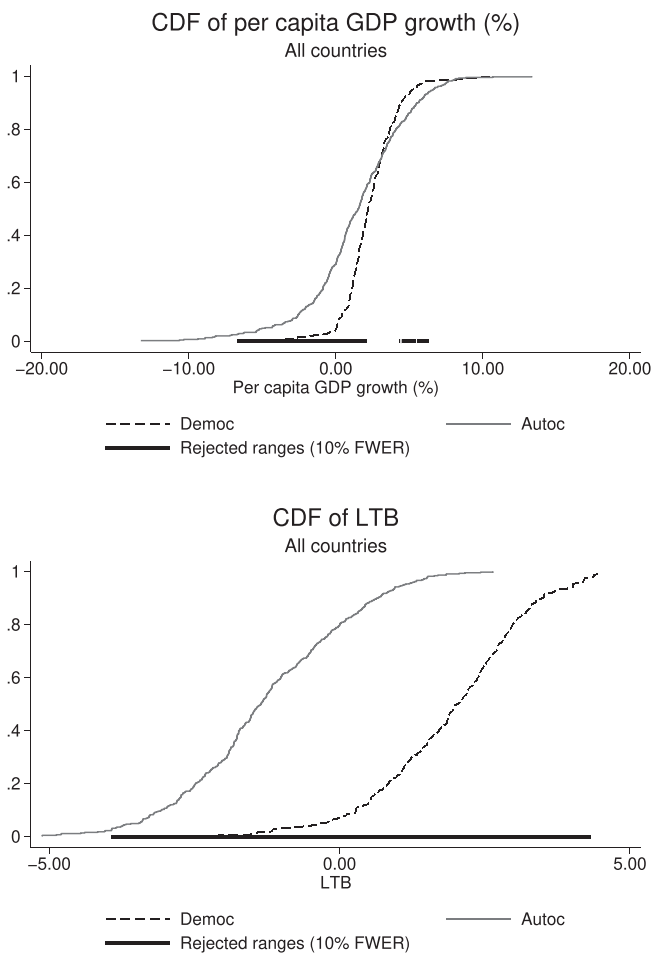


FIGURE 12 | Cumulative distribution functions of growth rates and *LTB*.

indicators. If we refer back to the list of indicators in Table 2, we now construct a measure of long-term benevolence based on only three: the priority given to public versus particularistic goods and the commitments to equality in education and health provision. Note that all three of these indicators could be favourable or unfavourable under autocracy; that is part of what we want to test.

We call this new measure *LTB* – 3 and present detailed results in the [Supporting Information](#). Repeating our analysis using this new measure, most of our earlier results go through. Kernel density plots show that the distribution of *LTB* – 3 for autocracies lies mostly to the left of the distribution for democracies. This is further confirmed by stochastic dominance analysis. The plot of the empirical cumulative distribution functions in the [Supporting Information](#) makes this clear, and this is confirmed by more formal testing using the approach of Davidson and Duclos (2000).

Perhaps the main limitation of this new measure is that it no longer has predictive power for public investment in Granger-causality regressions. This would be consistent with the idea that the new, narrower measure is less effective than our preferred wider measure at picking up commitment to the long-term common good, which partly involves a systematic commitment to making good decisions.

On wider questions of robustness, aspects of our analysis could be questioned: For example, the instrumental variable estimates use exclusion restrictions that may not be valid. And in the later investigation of the autocratic gamble in development outcomes, since we are comparing distributions, there is no obvious way to allow for long-run selection into democracy. In principle, the effects of selection could go either way, but as we will discuss below, this is where a supporter of enlightened autocrats would probably have to start.

9 | Conclusions

One of the best-established facts in the study of political institutions is that long-term growth rates vary more across autocracies than across democracies: this is the ‘autocratic gamble’. In this paper, we investigate whether differences in long-term benevolence can explain this contrast. The key argument is that, if some autocrats are unusually benevolent, this should be visible in an autocratic gamble for wider aspects of development, and not just growth. But that is not what we find in the data.

To make this argument, we construct a simple statistical proxy for long-term benevolence. As a check on its validity, we show that its time series variation helps to predict the evolution of public investment, even conditional on the past history of public investment. Using the within variation in a cross-country panel, we show that governments that increase their commitment to the long-term common good start to invest more. This helps to make the case that our empirical proxy is related to the concept of interest.

In our main results, we then show that long-term benevolence and democracy are positively associated. This association is weakened but not eliminated by the inclusion of exogenous covariates; IV estimates are also supportive, albeit with caveats. We then show that the distribution of long-term benevolence under democracy first-order stochastically dominates the distribution under autocracy. Hence, we find no evidence that some autocrats are especially likely to govern for the long-term common good. The results suggest that variation in long-term benevolence cannot readily explain the autocratic gamble. That gamble appears to be restricted to growth rates, and not wider aspects of development.

What does the analysis imply? First, accounts of successful dictatorial regimes in terms of unusual benevolence or altruism on the part of leaders should be treated even more cautiously; at least as a general rule, that explanation fails a simple empirical test. Second, and complementary to the evidence that democratization leads to growth, our work supports the existing consensus that democracy and wider development outcomes are also positively associated. This result applies under several different approaches to identification, and strengthens the case for seeing democracy as a route to better outcomes. The empirical proxy for long-term benevolence that we introduce could be used in further empirical work, perhaps to investigate how it varies with sub-types of the political institutions in place.

Inevitably, the analysis has some limitations. We have been clear that, in line with much of the literature, we should be

circumspect before claiming to have identified a causal effect of democracy. Long-run selection into democracy is likely to play a role, but our analysis at least narrows the space of admissible claims and suggests directions for future research. If many autocrats genuinely have governed for the common good, this must have been more than offset by a particular form of selection, namely a strong tendency for societies with the conditions necessary for long-term benevolent leadership to select into democracy. The historical process through which democracy emerges is likely to be complex and remains less than fully understood. Until more research is forthcoming, the extent to which the sceptical account is plausible we can leave to each reader to decide.

Author Contributions

All authors contributed equally to each section of the manuscript.

Acknowledgements

For comments and suggestions that improved the paper, we are grateful to the editor and referees and participants at the 8th WINIR Conference on Institutional Innovation and Evolution in Catania, the 19th SIDE-ISLE Annual Conference in Brescia, the 2024 RCEA International Conference in Economics, Econometrics, and Finance in London, the 23rd AISSEC Conference in Vicenza and the 11th IAAE Annual Conference in Thessaloniki, but the usual disclaimer applies. Open access publishing facilitated by Università degli Studi di Messina, as part of the Wiley - CRUI-CARE agreement.

Conflicts of Interest

The authors declare no conflicts of interest.

Data Availability Statement

The data that support the findings of this study are available from the corresponding author upon reasonable request.

Endnotes

- ¹ For recent analyses, see Knutsen (2021a, 2021b) and Monteforte and Temple (2020).
- ² The main exception seems to be one section of Besley and Kudamatsu (2008); we discuss their paper in Section 3.
- ³ See also the book chapter by Nair and Howlett (2020).
- ⁴ In a brief note, Bollyky et al. (2022) argue that democracies with high trust in government had the best outcomes. In contrast, Lago-Peñas, Martínez-Vázquez, and Sacchi (2022) find that political regimes were not central. But small sample sizes make reliable conclusions hard to draw.
- ⁵ See Lührmann et al. (2020). The data for 2022 show a worsening situation, with 72% of the world population living in autocracies, often ones that are becoming even more autocratic. Forty-three percent of the world population lives in countries which are moving further in an autocratic direction (Papada et al. 2023).
- ⁶ An earlier version of this paper framed the problem solely in terms of long-term orientation, but since it is empirically difficult to separate long-term orientation from a wider conception of benevolence, we now frame our analysis differently.
- ⁷ Previous applications of principal components analysis to cross-country data include Sirimaneetham and Temple (2009) on macroeconomic stability and O'Reilly and Murphy (2022) on state capacity.

- ⁸ As Dasgupta (2021), Heal (2016) and Ritchie (2024) make clear, sustainability is another aspect of farsighted policy. That will be easier to study once longer spans of data become available.
- ⁹ On the need for care in relating variables with different properties, see Paldam and Gundlach (2018).
- ¹⁰ For related indices of intergenerational justice, see the references in Krznicaric (2020, p. 169, footnote 16).
- ¹¹ A further difference between the two analyses is that we sometimes exclude high-income countries, to avoid the results being driven by broad-brush differences between those countries and the rest of the world.
- ¹² A later paper by Easterly and Pennings (2020) investigates the effects of individual leaders and whether they can be estimated precisely. But note that this is a very different question from whether leaders matter on average, which is the question of Jones and Olken (2005).
- ¹³ As in Sirimaneetham and Temple (2009), we carry out a preliminary check on the use of principal component analysis. In a limiting case where all the variables were orthogonal to each other, a principal component analysis would be of little value. To examine this, we carry out a Bartlett test for sphericity (Bartlett 1950), which rejects the null at the 1% level. We used the STATA `factortest` command developed by João Pedro Azevedo.
- ¹⁴ It may seem surprising that Singapore does not rank higher, but this is likely to reflect our focus on long-term benevolence, rather than simply long-term orientation.
- ¹⁵ Data as downloaded on 29 May 2024.
- ¹⁶ Data from version 27 of the WDI, downloaded 29 May 2024.
- ¹⁷ On the role of the time span, see, for example, Barro (2015). The key point is that using shorter time intervals, such as annual data, would typically increase the autoregressive coefficient, so the time span matters as well as the number of time series observations.
- ¹⁸ That is, the moment conditions for the first period use the lag at $t - 3$ but not that for $t - 4$.
- ¹⁹ Since this estimator is based on differencing, first-order serial correlation is to be expected.
- ²⁰ On selection on observables, see, for example, Lee (2005) and Wooldridge (2010).
- ²¹ We have also briefly considered doubly-robust estimation, using a smaller set of controls. The associated findings, not reported but available on request, are in line with those we discuss below.
- ²² Like us, Mulligan, Gil, and Sala-i-Martin (2004) report regressions with controls, but a few of their explanatory variables, such as GDP per capita, could be seen as post-treatment outcomes.
- ²³ Zhai (2017) examines the relationship between Confucian values and the extent of support for liberal democracy.
- ²⁴ Building on Altonji, Elder, and Taber (2005), Oster (2019) introduces an approach for examining sensitivity to selection on unobservables, but De Luca et al. (2019) draw attention to some unresolved issues in this approach. Masten, Poirier, and Zhang (2024) provide a nonparametric method that proceeds under very general assumptions, but the bounds it generates are likely to be wide for the sample sizes we consider.
- ²⁵ For more on this approach, see Wooldridge (2010, pp. 945–950). Some of our control function estimates use smaller samples to ensure overlap.
- ²⁶ This replicates the pattern in, for example, Cox et al. (2019, figure 2). For references to the wider literature, discussion, and robust variance tests, see Monteforte and Temple (2020).
- ²⁷ We implement this approach using the STATA `dom` command developed by David Stifel.

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Supporting Information

Additional supporting information can be found online in the Supporting Information section.

Appendix A

Country-Decade Ranking of *LTB* Over All DecadesTABLE A1 | Highest and lowest *LTB* in country-decades.

Country	Years	<i>LTB</i>	Growth rate (%)	Democracy score
Highest <i>LTB</i>				
Norway	2010–2019	4.46	0.42	1
Norway	2000–2009	4.44	1.05	1
Norway	1990–1999	4.44	2.92	1
Sweden	1980–1989	4.42	2.01	1
Norway	1980–1989	4.37	2.42	1
Norway	1970–1979	4.37	3.80	1
Sweden	1990–1999	4.32	1.29	1
Denmark	2000–2009	4.22	0.57	1
Denmark	1980–1989	4.21	1.85	1
Denmark	1990–1999	4.21	2.05	1
Denmark	2010–2019	4.19	1.49	1
Sweden	1970–1979	4.19	1.97	1
Denmark	1970–1979	4.18	1.89	1
Luxembourg	2010–2019	4.06	0.90	1
Luxembourg	1980–1989	4.01	3.99	1
Luxembourg	2000–2009	4.01	1.57	1
Luxembourg	1990–1999	4.01	3.22	1
Norway	1960–1969	4.01	3.52	1
Sweden	2000–2009	3.92	1.58	1
Germany	2000–2009	3.78	0.79	1
Lowest <i>LTB</i>				
Zimbabwe	1970–1979	−3.71	0.42	0
Djibouti	1970–1979	−3.73	−6.70	0
Democratic Republic of the Congo	1980–1989	−3.75	−0.88	0
Haiti	1970–1979	−3.77	2.12	0
El Salvador	1960–1969	−3.84	2.03	0
Haiti	1960–1969	−3.85	−1.24	0
Syria	2010–2019	−3.92	−5.20	0
Guinea-Bissau	1960–1969	−3.92	1.08	0
El Salvador	1970–1979	−3.93	2.10	0
Honduras	1970–1979	−3.93	2.37	0
Cambodia	1980–1989	−4.03	2.09	0
Cape Verde	1960–1969	−4.05	2.30	0
Namibia	1960–1969	−4.17	3.62	0
Uganda	1970–1979	−4.39	−4.18	0
Cambodia	1970–1979	−4.46	−7.20	0
Paraguay	1980–1989	−4.79	0.97	0

(Continues)

TABLE A1 | (Continued)

Country	Years	LTB	Growth rate (%)	Democracy score
Nicaragua	1960–1969	−4.80	4.08	0
Paraguay	1970–1979	−4.87	5.47	0
Paraguay	1960–1969	−5.12	0.90	0
Nicaragua	1970–1979	−5.13	−2.99	0

Note: 0 denotes country-decades classified as stable autocracies, 1 denotes stable democracies and 99 denotes undetermined country-decades.

Appendix B

Selection on Observables

TABLE B1 | Data description and sources.

Variable	Description	Source
Near coast	Percentage of the land surface area of each country that is within 100 km of the nearest ice-free coast	Nunn and Puga (2012) via Quality of Government Dataset
Pop density (log)	Midyear population divided by land area in square kilometres	WDI via Quality of Government Dataset
Latitude	Absolute value of the distance from the equator of the country, scaled to take values between 0 and 1, where 0 is the equator	Acemoglu, Johnson, and Robinson (2001)
Landlock	Dummy which equals to 1 if the country does not adjoin the sea.	Acemoglu, Johnson, and Robinson (2001)
Religion dummies	Percent of population that belonged to the three most widely spread religions of the world in 1980 (or for 1990–1995 for countries formed more recently). The four classifications are Roman Catholic, Protestant, Muslim and ‘other’.	Acemoglu, Johnson, and Robinson (2001)
Colonial dummies	Fivefold classification of the former colonial ruler of the country (based on original tenfold classification)	Authoritarian Regimes Dataset via Quality of Government Dataset
Ethnolinguistic fragmentation	Average of five different indices of ethnolinguistic fragmentation	Acemoglu, Johnson, and Robinson (2001)