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# The Political U: New Evidence on the Economic Costs of Hybrid Regimes

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## ABSTRACT

Recent research establishes a positive causal effect from democracy to income, although this evidence relies mostly on binary regime classifications. We extend the identification framework of Acemoglu et al. (2019) to a classification that distinguishes democracies, autocracies, and hybrid regimes for about 170 countries over 1960–2024. We find hybrid regimes generate approximately 20 percent lower GDP per capita than either alternative, robust to a wide range of estimators, measures, and specifications. This effect is not a transition penalty, supporting the view that hybrid regimes are a durable institutional equilibrium with sustained economic costs rather than a simple transitional phase.

**JEL Classification:** C33, D72, F15, O43, P16

## 1 | Introduction

Inclusive institutions are essential for long-run economic development. Economic institutions are inclusive when they support the protection of property rights, guarantee broad access to economic opportunities, and shore up a level playing field for competition (Acemoglu et al. 2005 and Durlauf 2020). These conditions are more likely to emerge under democratic regimes, which provide the political underpinnings— such as accountability, electoral competition, and constraints on executive authority— that support inclusiveness. On the other hand, non-democratic regimes are more likely to require extractive institutions that suppress competition in order to concentrate the distribution of power and wealth among the few. Despite the obvious explanatory power of such inclusive-extractive dichotomy, it would be a mistake to imagine that there is nothing important to learn from the large number of intermediate, hybrid and cases in-between.

The dichotomy between democratic and non-democratic regimes cannot capture the complexity underlying the relationship between regime type, the degree of institutional inclusiveness, and economic development. Recent theoretical literature broadly acknowledges the nuanced nature of this relationship (see, among others, Bernhardt et al. 2022; Guriev and Papaioannou 2022; Acemoglu and Robinson 2023; Gratton and Lee 2024). In political science, there is recognition of the existence and significance of hybrid regimes—that is, political regimes that are neither perfect democracies nor perfect autocracies (e.g., Gates et al. 2006). Diamond (2002) refers to these as "hybrid" regimes, Epstein et al. (2006) and Fearon (2011) call them as "partial democracies," Bogaards (2009) and Croissant and Merkel (2019) name them "defective democracies," and Zakaria (1997) labels them "illiberal democracies." According to Epstein et al. (2006), "We also learn that the frontier of this line of inquiry has shifted away from the study of autocracies and democracies and toward the study of partial democracies. As we show here, the behavior of these systems largely determines the level, rate, and properties of democratization. While thus influential, partial democracies, being highly heterogeneous, are poorly understood. The study of democratization, we therefore conclude, should place them at its focus" (Epstein et al. 2006, p. 552). Nevertheless, by reducing political regimes to a stark dichotomy, most of the empirical research so

far has refrained from characterizing the causes and costs of hybrid regimes. Hybrid regimes raise important questions for understanding the institutional foundations of economic development.

The main hypothesis in this paper is that the economic performance of hybrid regimes is significantly inferior to that of both democracies and autocracies (i.e., the political U). Preliminary descriptive evidence already points in this direction. Figure 1 reports fitted values from simple specifications in which GDP per capita (in logarithms, multiplied by 100) is regressed on continuous measures of democracy derived from Varieties of Democracy (V-Dem) and Polity, together with their squared terms. In both cases, the estimated relationship appears distinctly non-monotonic, with intermediate levels of democratization associated with lower predicted income levels than both fully autocratic and fully democratic regimes. The shaded areas report 95% confidence intervals.[1]

Hybrid regimes often combine weak democratic institutions with limited accountability and persistent elite influence, resulting in institutional instability, policy uncertainty, and a fragile rule of law—all of which may hinder long-run economic performance (Acemoglu and Robinson 2008; Gates et al. 2006; Knutsen and Nygård 2015). The econometric results we report here using yearly data for about 170 countries over the 1960–2024 period robustly support this hypothesis. Moreover, our estimates suggest that this effect is quantitatively important: our estimates indicate that hybrid regimes lead to a 20% decline in economic performance vis-à-vis both democracies and autocracies. We also find that this effect does not reflect a transition penalty; rather, it reinforces the view that hybrid regimes constitute a durable and self-sustaining institutional equilibrium, one associated with persistent and non-trivial economic costs instead of representing merely a temporary or transitional phase along a broader development path.

To empirically assess our main hypothesis, while at the same time ensuring continuity and comparability with the existing empirical literature based on dichotomous regime specifications (among others Acemoglu et al. 2019 and Dube et al. 2025), the paper develops a multi-step empirical strategy.

First, we construct a novel three-way or trichotomous classification of political regimes that explicitly accounts for the existence of hybrid regimes. This approach refines standard binary classifications and better captures the institutional diversity observed in practice. We find hybrids to be a distinct regime type with an average duration of almost 15 years. Although the duration of hybrid regimes is significantly shorter than that of democracies (average duration of democracies is 23 years), it is not far away from the average duration of autocracies, which is about 17 years.

Second, we implement this classification within the dynamic panel framework proposed by Acemoglu et al. (2019), using a two-way fixed effects (TWFE) model to estimate the long-run impact of different regime types on GDP per capita.

Third, we apply the local projection difference-in-differences (LP-DiD) estimator proposed by Dube et al. (2025), which

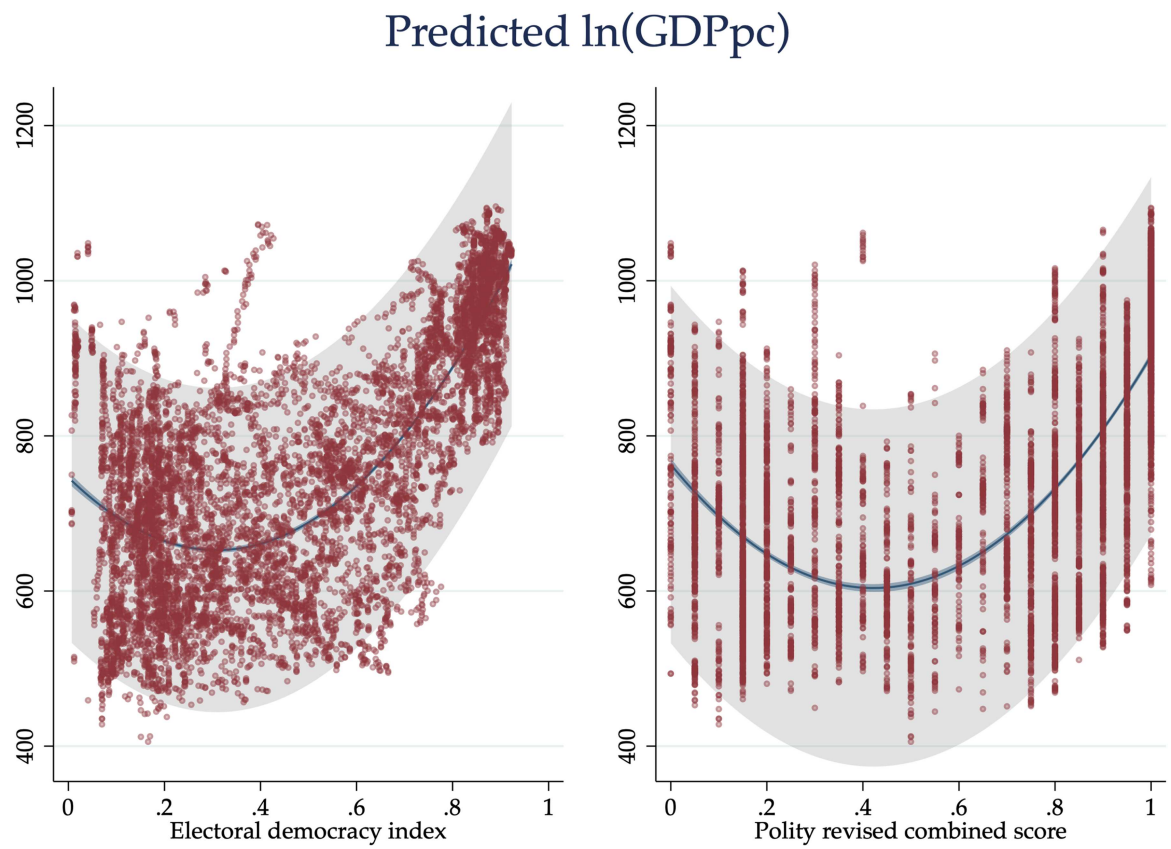


**FIGURE 1** | Predicted values of GDP per capita for different continuous democracy indexes.

allows for a flexible analysis of dynamic treatment effects. This method is particularly useful in capturing the distinct trajectories of democratization. Countries transitioning away from autocracy may eventually consolidate as democracies after an interim period, while others may struggle to complete the transition, with potentially very different effects on their economic growth trajectory.

Fourth, we incorporate recent methodological advancements in difference-in-differences (DiD) econometrics[2] by implementing the heterogeneity-robust DiD estimator proposed by de Chaisemartin and D'Haultfœuille (2020). This approach accounts for heterogeneous treatment effects across countries and time periods, adjusting for differences in treatment intensity and ensuring that average treatment effects remain unbiased. Given the likely variation in the effects of regime changes across different cohorts defined by country and time, this estimator is particularly well-suited. These are important additions, especially in light of Doucette's (2025) remarks that "One should be careful when interpreting the magnitude of effects found in cross-national analyses of the effects of democracy and recognize the uncertainty inherent in such estimates" (2025, p. 1557).

The rest of the paper is organized as follows. Section 2 provides a short review of the related literature. Section 3 focuses on data and issues surrounding the measurement of political regimes. Section 4 reports our estimation results on the relationship between political regimes and economic development. Section 5 presents a set of robustness checks. Section 6 discusses the broader implications of our findings. Section 7 concludes.

## 2 | Related Literature

The relationship between regime type and economic growth is one of the most extensively studied questions in political economy, and several major surveys and meta-analyses now cover this ground (Colagrossi et al. 2020; Gerring et al. 2022). In this section, we focus on three interrelated strands that bear most directly on our analysis: the evidence on whether the democracy-growth relationship is linear or non-linear; the treatment of hybrid regimes as a distinct economic category; and the debate over whether hybrid arrangements constitute genuine equilibria or merely transitional states between consolidated regime types.

The current methodological benchmark in this literature is Acemoglu et al. (2019), who find that democracy has a positive causal effect on long-run per capita income of approximately 20 percent, using dynamic panel data and instruments based on regional democratization waves. Dube et al. (2025) confirm this result using a local projections difference-in-differences approach. Both studies, however, rely on a binary democracy classification by construction, leaving open the question of non-linearity. The theoretical case for expecting a non-linear relationship goes back to Barro (1996), who proposed an inverted U-shape driven by two opposing forces: the removal of autocratic predation in the early phases of democratization boosts growth, while the redistributive pressures and distortionary taxation accompanying democratic consolidation eventually reduce it, implying that intermediate regimes might perform best. Subsequent empirical work has been largely skeptical of this specific prediction (Colagrossi et al. 2020 for a review).[3] Libman (2012) argues that our results diverge from Barro's inverted U, and the divergence traces to identification strategy: unlike prior work, we apply heterogeneity-robust difference-in-differences methods to a trichotomous classification, which changes what is being estimated and how regime differences are compared.

The theoretical case for treating intermediate regimes as a distinct economic category rests on a well-established set of arguments. McGuire and Olson (1996) show that the worst economic outcomes arise when competing elites lack both the autocrat's encompassing interest in aggregate output and democracy's majoritarian constraint on predation. Hybrid arrangements create precisely this configuration: multiple actors extract resources within short time horizons while no single actor internalizes the aggregate costs. Tornell and Lane (1999) formalize a related result, showing that moving from one to multiple competing interest groups reduces growth. Acemoglu and Robinson (2008) add that hybrid regimes are characterized by an especially wide gap between formal institutional rules and actual practice, which reduces the credibility of policy commitments and raises uncertainty for private investors. Cox and Weingast (2017) develop a related argument focused on property rights specifically. Gurr (1974) had earlier captured this configuration in the concept of anocracy, which combines autocratic and democratic elements in ways that are internally inconsistent and generate chronic instability. Taken together, these contributions predict that hybrid regimes will produce worse economic outcomes than either pure type, not simply because they occupy an intermediate position on a democracy scale, but because of the specific institutional pathologies they share.

The comparative politics literature has given empirical content to the hybrid category through a rich body of typological work. Levitsky and Way (2002) identify competitive authoritarianism, in which formally democratic institutions coexist with systematic manipulation and selective repression that prevent genuine accountability. Diamond (2002) maps the related category of electoral authoritarianism, and Bogaards (2009) and Croissant and Merkel (2019) develop further sub-types. What this literature makes clear is that the hybrid category is internally heterogeneous: intermediate democracy scores can capture regimes with very different institutional configurations and, as Miller (2015) shows, divergent economic implications depending on how regime type is operationalised, the time period examined, and the regional context. We take this heterogeneity seriously in our empirical work by examining robustness across alternative thresholds and by allowing for heterogeneous treatment effects across countries and time periods.

An important question for our argument is whether hybrid regimes are genuine political equilibria or merely transitional states. Much of the democratization literature, building on Huntington (1991) and subsequent work, treats intermediate regime scores as reflecting a country in transit between consolidated types rather than a stable configuration. On this view, the economic costs we observe would reflect the disruption of transition itself rather than the sustained operation of a hybrid

institutional equilibrium. The evidence, however, supports the equilibrium view. Epstein et al. (2006) show that transitions out of partial democracy toward full democracy or autocracy are roughly equally likely even over long time horizons, and a substantial share of countries remain in this state for extended periods. Gates et al. (2006) and Knutsen and Nygård (2015) similarly find that institutional incoherence and elevated instability are structural features of the hybrid regime type, not residues of recent transitions. We take this evidence seriously in our empirical design: we show below that the estimated effect of hybrid status on income persists when we restrict attention to countries with extended consolidated spells in the hybrid category, ruling out a purely transition-driven interpretation.

Measuring hybrid regimes is not straightforward. The Polity index is the most widely used source, but Vreeland (2008) identifies a circularity problem: Polity incorporates indicators of political violence directly into its construction, creating bias when Polity-based regime categories are used to explain political instability outcomes. V-Dem offers a more institutionally grounded alternative, decomposing democracy into distinct electoral, liberal, participatory, deliberative, and egalitarian components. Our strategy, described in Section 3 to combine Polity, V-Dem, and Freedom House data and to verify that our main results hold across all three sources and across a wide range of threshold specifications used to define the hybrid category.

## 3 | Data and Measurement

On a theoretical level, economists and political scientists are now starting to converge on their appreciation and understanding of democratic political institutions. Conceptually, we believe there is little difference between Dahl and Rodrik, some of the most prominent thinkers on these issues in political science and economics, respectively. Dahl (1998) asks what political institutions does "large-scale democracy" require? His answer focuses on six factors: (1) elected officials, (2), free, fair, and frequent elections, (3) freedom of expression, (4) alternative sources of information, (5) associational autonomy, and (6) inclusive citizenship. In his conceptualization, Rodrik (2000) argues that democracy is a multi-dimensional "meta-institution," which encompasses among other aspects: elected representatives, civil liberties and political rights, checks and balances, organized opposition, independent judiciary, apolitical bureaucracy, and rule of law. The convergence between these views is clear.

On an empirical level, however, political scientists prefer to separate out democracies from autocracies and to use continuous measures of both, while economists tend to conflate the two (i.e., favor democracies and autocracies measured on a single scale) but tend to use a simpler dichotomous, dummy variable approach. Our contribution is to apply state-of-the-art causal identification methods to a trichotomous classification established in comparative politics since Epstein et al. (2006). The main objective of our empirical analysis is to accommodate and test the possibility that the relationship between political and economic development is non-linear and to understand the economic implications of intermediate regimes.

There is a long tradition in measuring political institutions since at least the early 1970s (e.g., the Gastil indexes of Freedom House). Today, the two most prominent data sources are Polity and V-DEM. They both draw heavily on the Dahl and Rodrik conceptualizations above. The Polity dataset is still the most widely used measure (Marshall and Gurr 2020), while the Varieties of Democracy (V-DEM) dataset is more recent and enriches the measurement of political regimes by providing a more nuanced and multidimensional perspective (Maerz et al. 2021; Lührmann et al. 2018). In order to maximize the comparability with previous studies and to ensure that our results are not dependent on the specific source of measurement of the political regime, we use both Polity and V-DEM databases, as well as Freedom House, another older database that is still widely used in the empirical literature.[4] Irrespective of which data source one uses as a measure of democracy, we show below that they relate to per capita income in similar ways, namely, they support a U-shaped relationship.

We start out with a three-way or trichotomic classification of political regimes of autocracies, democracies, and hybrid regimes. The main reason we offer for this choice is comparability with previous studies and ease of exposition (with our results and conclusions remaining largely unchanged when using continuous measures of democracy, as we later demonstrate).

Our starting point is that Polity classifies political regimes along a continuous scale, within which intermediate regimes—labeled as "anocracies"—can be identified. The Polity index ranges from −10 to +10. Based on this, we construct a first trichotomous classification as follows:

i. Autocracies: $-10 \leq \text{Polity} < -5$ (34.15% of country-year observations);

ii. Intermediate regimes (anocracies): $-5 \leq \text{Polity} \leq +5$ (24.77%);

iii. Democracies: $+5 < \text{Polity} \leq +10$ (41.08%).

A similar classification exercise could in principle rely on the Varieties of Democracy (V-Dem) data, which provide an established categorical measure—the Regimes of the World (RoW) index—distinguishing between *closed autocracies*, *electoral autocracies*, *electoral democracies*, and *liberal democracies*. However, this classification is based on multiple indicators simultaneously, making it less directly comparable with a single continuous measure such as the Polity score.

For the purposes of our analysis, we therefore adopt a more parsimonious approach and rely on a single continuous indicator from V-Dem, namely the Electoral Democracy Index (*polyarchy*), which ranges between 0 and 1. This measure is widely used in the literature and constitutes a core component of the RoW classification. Since no standard thresholds exist for deriving regime categories from the polyarchy index, we proceed in two steps. First, we examine how the RoW categories overlap with the three Polity-based groups. This comparison reveals that: (a) Polity autocracies largely correspond to V-Dem closed autocracies, including the lower bound of electoral autocracies; (b) Polity democracies overlap predominantly with

electoral and liberal democracies, and they also capture most upper-bound electoral autocracies; and (c) Polity anocracies map primarily into electoral autocracies.

Second, we use this empirical overlap to define thresholds for a trichotomous classification based on V-Dem polyarchy. Specifically, we set:

i. a lower threshold at the 75th percentile of the polyarchy distribution among closed autocracies and lower-bound electoral autocracies (polyarchy = 0.174);

ii. an upper threshold at the 75th percentile of the distribution among electoral autocracies (polyarchy = 0.368).

These thresholds allow us to construct categories broadly comparable to those obtained using Polity. We also verify that our results are robust to alternative threshold choices and, more importantly, to specifications that rely directly on the underlying continuous measures.

Our final step is to combine information from Polity, V-Dem, and Freedom House into a unified classification. Formally, we define regime type for country $c$ in year $t$ ($D_{ct}$) as follows:

- *Democracy* ($D_{ct} = 2$) if *none* of the following conditions hold:
  i. V-DEM $\leq 0.368$;
  ii. Polity IV $\leq +5$;
  iii. Freedom House is worse than "Free."
- *Autocracy* (i.e., $D_{ct} = 1$) if *at least one* of the following conditions holds:
  i. V-DEM $\leq 0.174$ and Polity IV $< -5$, while Freedom House is missing or different from "Free."
  ii. Freedom House is "Not Free" and neither V-DEM $> 0.368$ nor Polity $> +5$.
- *Hybrid Regime* ($D_{ct} = 1$) otherwise.

We generate this three-way or trichotomic classification of political regimes following the criteria described above for about 170 countries from 1960 to 2024 for a total of 9920 country-year observations. Note that it is possible to extend this classification to earlier years, but at the cost of fewer countries and, more importantly, of comparability with other studies.

How does our classification compare with that in other studies? Acemoglu et al. (2019) focus on the 1960 to 2010 period, so we constrain our comparison accordingly. Their dichotomous classification has also been adopted in subsequent applications of the LP-DiD approach, such as Dube et al. (2025), which rely on the same dataset and democracy coding. In their sample, they classify 4825 country-year observations as non-democracies and 3430 as democracies. According to our classification, out of their 4825 non-democracies we have 2503 as autocracies (i.e., 51.9% of their total), 2303 (47.7%) as intermediate regimes and 19 as democracies.[5] Out of the 3430 country-year observations they classify as democracies, our approach classifies 2373 as democracies (i.e., about 69.2% of their total) and 1057 as intermediate regimes (and none as autocracies). The two classifications compare well in the sense that intermediate regimes take up about one-third of Acemoglu et al.'s democracies and autocracies, and as such our three-way approach does not contradict but actually refines the dichotomous approach prevalent in the literature.

Another important feature that compares well with the regime classification in Acemoglu et al. (2019) is that in our three-way classification, the magnitude of changes in the Polity index occurring in regime transitions is sizeable and thus, as in the dichotomic approach, it does not seem to reflect measurement errors (Appendix A has a detailed analysis of transition dynamics). Within our sample, 433 observations (4 percent of the total) are identified as transition years (i.e., years in which we observe a change of regime using our classification). More precisely, we identify 256 episodes of democratization and 177 reversals. The vast majority of these are transitions from or to hybrid regimes, thus supporting the view that hybrid is the most unstable regime among the three. Indeed, we only identify 7 transitions (of a total of 433) by countries moving directly from autocracy to democracy, and none directly from democracy to autocracy. Finally, we also note that 14.8 percent of the democratization episodes take place in the year of independence[6] or in one of the 3 years after independence, while only 6.2 percent of reversals take place in the same time window.

We calculate an average of 2.5 transitions per country in the 1960–2024 period. Of which, 33 countries in our sample (19.4 percent of the total) experience no transitions at all, and approximately 28 percent of the countries have experienced more than 3 transitions. The countries with the highest number of transitions are Argentina, Brazil, Sudan (10), Bolivia, Ecuador, Honduras, Lesotho, and Peru (8). From a total of 170 countries in our sample, a substantial 99 experienced both democratizations and reversals within the 1960–2024 period. On the other hand, 35 countries experienced only democratic transitions without reversals, and only 3 countries experienced only reversals (Laos, Singapore, and Syria). We believe that overall, these figures compare well to those from previous studies that use a dichotomous classification of democracy.

## 4 | Empirical Results

In this section, we present our main results on the relationship between political and economic development using the data discussed in the previous section. We employ mainly two distinct econometric approaches—one stressing GDP lags and year-country fixed-effects, and the other incorporating the latest advances in difference-in-differences econometrics—to assess the nature and extent of empirical support for our central hypothesis.[7]

### 4.1 | Baseline Results

Using yearly data for the variables described in the previous section for a sample of about 170 developing and developed countries covering the period from 1960 to 2024,[8] Table 1 presents results using the econometric framework developed by Acemoglu et al. (2019). They propose a dynamic panel model for GDP, which includes autoregressive dynamics and both country and year fixed effects, the former to absorb the impact of time-invariant country characteristics and the second to

**TABLE 1** | Effect of democracy, autocracy, and intermediate regimes (omitted) on (Log) GDP per Capita – lags, country, and year fixed effects.

| Dep. Var.: (Log) GDP per Capita | WITHIN ESTIMATES | | | | ARELLANO AND BOND ESTIMATES | | | |
|---|---|---|---|---|---|---|---|---|
| | (1-lags) | (2-lags) | (4-lags) | (8-lags) | (1-lags) | (2-lags) | (4-lags) | (8-lags) |
| Autocracy | 0.331 | 0.420* | 0.495** | 0.789*** | 0.077 | 0.102 | 0.320 | 0.826** |
| | (0.339) | (0.249) | (0.239) | (0.274) | (0.458) | (0.372) | (0.342) | (0.357) |
| Democracy | 0.553 | 0.597** | 0.657** | 0.709** | 0.526 | 0.567 | 0.739** | 0.817** |
| | (0.364) | (0.284) | (0.271) | (0.298) | (0.510) | (0.381) | (0.369) | (0.371) |
| log GDP first lag | 0.981*** | 1.298*** | 1.263*** | 1.264*** | 0.961*** | 1.272*** | 1.242*** | 1.235*** |
| | (0.004) | (0.035) | (0.034) | (0.035) | (0.006) | (0.037) | (0.034) | (0.034) |
| log GDP second lag | | −0.322*** | −0.211*** | −0.218*** | | −0.310*** | −0.202*** | −0.209*** |
| | | (0.035) | (0.042) | (0.040) | | (0.036) | (0.042) | (0.039) |
| log GDP third lag | | | −0.025 | −0.021 | | | −0.022 | −0.017 |
| | | | (0.028) | (0.028) | | | (0.027) | (0.027) |
| log GDP fourth lag | | | −0.054*** | −0.056** | | | −0.057*** | −0.054** |
| | | | (0.017) | (0.023) | | | (0.018) | (0.022) |
| p-value, lags 5–8 | | | | 0.794 | | | | 0.817 |
| Long-run effect of autocracy | 17.063 | 17.776 | 18.148* | 25.108*** | 1.964 | 2.674 | 8.091 | 18.770** |
| | (17.910) | (11.005) | (9.355) | (9.142) | (11.624) | (9.684) | (8.669) | (8.093) |
| Long-run effect of democracy | 28.537 | 25.294** | 24.093** | 22.576** | 13.404 | 14.824 | 18.709* | 18.557** |
| | (19.670) | (12.336) | (10.191) | (9.825) | (12.886) | (10.238) | (9.828) | (8.694) |
| Effect of autocracy after 25 years | 6.604 | 10.427* | 12.795** | 19.437*** | 1.242 | 2.038 | 6.781 | 16.373** |
| | (6.780) | (6.290) | (6.326) | (6.862) | (7.371) | (7.394) | (7.268) | (7.115) |
| Effect of democracy after 25 years | 11.045 | 14.836** | 16.987** | 17.477** | 8.478 | 11.300 | 15.679* | 16.187** |
| | (7.283) | (7.133) | (7.126) | (7.504) | (8.160) | (7.695) | (8.067) | (7.516) |
| Persistence of GDP process | 0.981*** | 0.976*** | 0.973*** | 0.969*** | 0.961*** | 0.962*** | 0.961*** | 0.956*** |
| | (0.004) | (0.003) | (0.004) | (0.005) | (0.006) | (0.006) | (0.006) | (0.007) |
| AR2 test p-value | | | | | 0.00 | 0.06 | 0.68 | 0.89 |
| Observations | 8584 | 8426 | 8106 | 7466 | 8422 | 8264 | 7944 | 7304 |
| Countries in sample | 162 | 162 | 162 | 162 | 162 | 162 | 162 | 162 |

*Note:* This table reports the estimation results of the model specification represented by Equation (1). It uses the trichotomous classification of political regimes described in Section 3. The reported coefficients of Autocracy and Democracy are multiplied by 100 to ease their interpretation, and all standard errors are robust to heteroscedasticity. The omitted political regime is Intermediate. Columns 1–4 report the *within estimator* results. Columns 5–8 report *Arellano-Bond GMM* results. Columns 4 and 8 include eight lags of the dependent variable as controls, but for lags 5–8, only the *p*-value of a test for joint significance is reported. The *AR2 test p-value* is the *p*-value for a test of serial correlation in the residuals of the GDP series. The long-run effects and the effects over the next 25 years are also reported in the table (see footnote 15 for a description of their computation). Standard errors are clustered at the country level and are reported in brackets. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

absorb the impact of time-variant characteristics other than per capita GDP. Table 1 reports the estimated coefficients from the following specification:

$$y_{ct} = \beta_1 Autocracy_{ct} + \beta_2 Democracy_{ct} + \sum_{j=1}^{l} \theta_j y_{ct-j} + \alpha_c + \delta_t + \varepsilon_{ct}, \quad (1)$$

where $y_{ct}$ is the log of GDP per capita in country $c$ at time $t$, $Autocracy_{ct}$ is a dummy variable equal to 1 if country $c$ has been coded as an autocracy at time $t$ (and 0 otherwise); $Democracy_{ct}$ is a dummy variable equal to 1 if country $c$ has been coded as a democracy at time $t$ (and 0 otherwise); while the 'intermediate or hybrid regime' is the omitted category. In addition, time-invariant country characteristics are absorbed by a set of country fixed effects $\alpha_c$, and $l$ lags of $y_{ct}$ control for the dynamic pattern of GDP per capita as in Acemoglu et al. (2019). A full set of year fixed effects $\delta_t$ is also included. Table 1 reports Within and Arellano-Bond estimates.

There are various results worth highlighting. Column 1 shows the results for a single lag of per capita GDP. The value of this coefficient is significantly less than 1, which indicates support for the assumption that the dynamic panel estimators are consistent and with well-behaved limit distributions, that is, that GDP and democracy follow stationary processes, conditional on year and country fixed effects. More lags of per capita GDP are added in the columns 2 to 4 which yield richer dynamics. As reported at the bottom panel of the table, the extent of the "persistence of the GDP process" is almost identical to that found in column 1.[9] Moreover, this also holds with respect to the second half of the table (columns 5 to 8) in which we report Arellano-Bond GMM results that address the well-known concern about the "Nickell bias" that follows the use of the within estimator in dynamic model specifications. Note that in columns 4 and 8 we include

four more lags of GDP for a total of eight lags, but we do not report these coefficients, just the *p*-value for their test of joint significance (which indicates they do not jointly affect GDP.)

In column 1 of Table 1 only the coefficients on autocracy and democracy are positive but not statistically significant. Moving forward from column 1 by adding more GDP lags does substantially change this result in that the coefficients of both autocracy and democracy become positive and statistically significant (with column 3 being the preferred specification). This suggests that the two "extreme" regimes perform significantly better than the hybrid political regime (omitted category) constituting an important piece of evidence in support of our central hypothesis.

From these estimates in Table 1, we can also derive the long-run effect of both autocracy and democracy.[10] Specifically, the results in column 3 show that a permanent transition to democracy increases GDP per capita by 18.15 percent in the long run, while to autocracy raises it by 24.09 percent.[11] When using the corresponding GMM estimates, the effects are still sizeable (columns 7–8) and broadly comparables in economic magnitude. However, the statistical significance of the effect for autocracy weakens, with *p*-values of 0.14 and 0.04 in columns 7 and 8, respectively. Similar conclusions emerge when focusing on the estimates for "the next 25 years."[12]

Even though our findings suggest that permanent transitions from hybrid regimes to either democracy or autocracy bring long-run benefits, the actual trajectories of these transitions are highly heterogeneous. In our sample, the average duration of autocracies (about 17 years) is shorter than that of democracies (about 23.4 years). As a result, many transitions from intermediate regimes to autocracy are eventually reversed within a 25-year horizon—i.e., before the positive long-run differential relative to hybrid regimes can fully materialize.[13] Once this probability of reversal is taken into account, the expected benefits of transitions from intermediate regimes to autocracy are smaller than those associated with transitions to democracy. At the same time, the average duration of hybrid regimes (14.9 years) is sufficiently long to rule out the possibility that their weaker performance is driven solely by the negative effects of transition periods—a point that is further supported by our subsequent analysis below and robustness checks.

Thus, the extent to which these shifts translate into sustained economic gains depends on the nature of the transition process itself—whether it represents a permanent move toward a more consolidated regime or a more uncertain, potentially unstable evolution. To move beyond static comparisons of regime types, it is therefore crucial to analyze how democratization (or reversals) unfold over time, considering the dynamics of the different paths countries take, which we do next.

### 4.2 | The Dynamics of Democratic Transitions

To shed light on the transition dynamics, and following Acemoglu et al. (2019), we introduce semiparametric estimates to address two key limitations of the dynamic linear panel model: (i) its heavy reliance on the linearity assumption, which imposes restrictions on how the cumulative effects of democracy on GDP evolve over time, and (ii) its implicit constraint that the effects of transitions to and from democracy are symmetric in absolute value. In order to do this, we leverage the Local Projections Approach to Difference-in-Differences (LP-DiD) proposed by Dube et al. (2025). Rather than estimating the impact of a permanent regime transition as above, this approach allows for the examination of the cumulative impact of a regime shift at a given point in time without assuming that it cannot be reversed in subsequent years. Note that Dube et al. (2025) apply this new approach to Acemoglu et al. (2019), providing us with a unique opportunity to incorporate our non-binary classification of political regimes and highlight the non-monotonic effects of regime transitions. The LP-DiD specification of Acemoglu et al. (2019) for estimating the effect of democracy on economic growth is as follows:

$$y_{c,t+h} - y_{c,t-1} = \beta_h^{\text{LP DiD}} \Delta NR_{ct} + \delta_t^h + \sum_{k=1}^{p} \gamma_k^h y_{c,t-k} + \varepsilon_{ct}^h$$

restricting the estimation sample to:

$$\begin{cases} \textit{newly treated} \;\; NR_{ct} = 1; NR_{c,t-j} = 0 \text{ for } 1 \le j \le L \\ \textit{clean controls} \;\; NR_{c,t-j} = 0 \text{ for } 0 \le j \le L \end{cases},$$

where: in each year $t$, the newly treated units are countries $c$ that move to a *new regime* at $t$ and have experienced no other changes in treatment status in the previous $L$ years. Clean controls are countries that have remained in the *old regime* continuously for at least $L$ years; $\delta_t^h$ are time effects; $y_{c,t-k}$ represent different lags of the dependent variable so that the parallel trends assumption holds conditional on lagged GDP.

We adopt the Dube et al. (2025) approach but redefine the categories of *old* and *new* political regimes to account for different types of transitions along the democratic spectrum. In Dube et al. (2025), $NR = D$, where $D$ follows the Acemoglu et al. (2019) binary classification of *non-democracies* and *democracies*. Here, we move beyond this dichotomy. We begin with the most basic specification, where $L = 1$, implying a 1-year time window for defining clean controls. While assuming that treatment effects stabilize after just 1 year is a strong assumption, it serves as a useful starting point, as Dube et al. (2025) demonstrate that this specification is equivalent to the Acemoglu et al. (2019)'s semiparametric model (Acemoglu et al. 2019, Section IV).

Figure 2 illustrates how the results vary depending on the type of regime transition considered. The solid line represents the estimated effect of a regime change on GDP (in log) over time, while the shaded area depicts the 95% confidence interval.

Figure 2a follows the threshold definitions set by Acemoglu et al. (2019). Because our data set is longer, being updated through 2024, and because of the exclusion of a few small island nations, this graph does not exactly replicate Figure 3 of Acemoglu et al. (2019). This said, it confirms the positive and increasing impact of transitioning from a non-democracy to a democracy over time. Figure 2b considers a higher threshold,

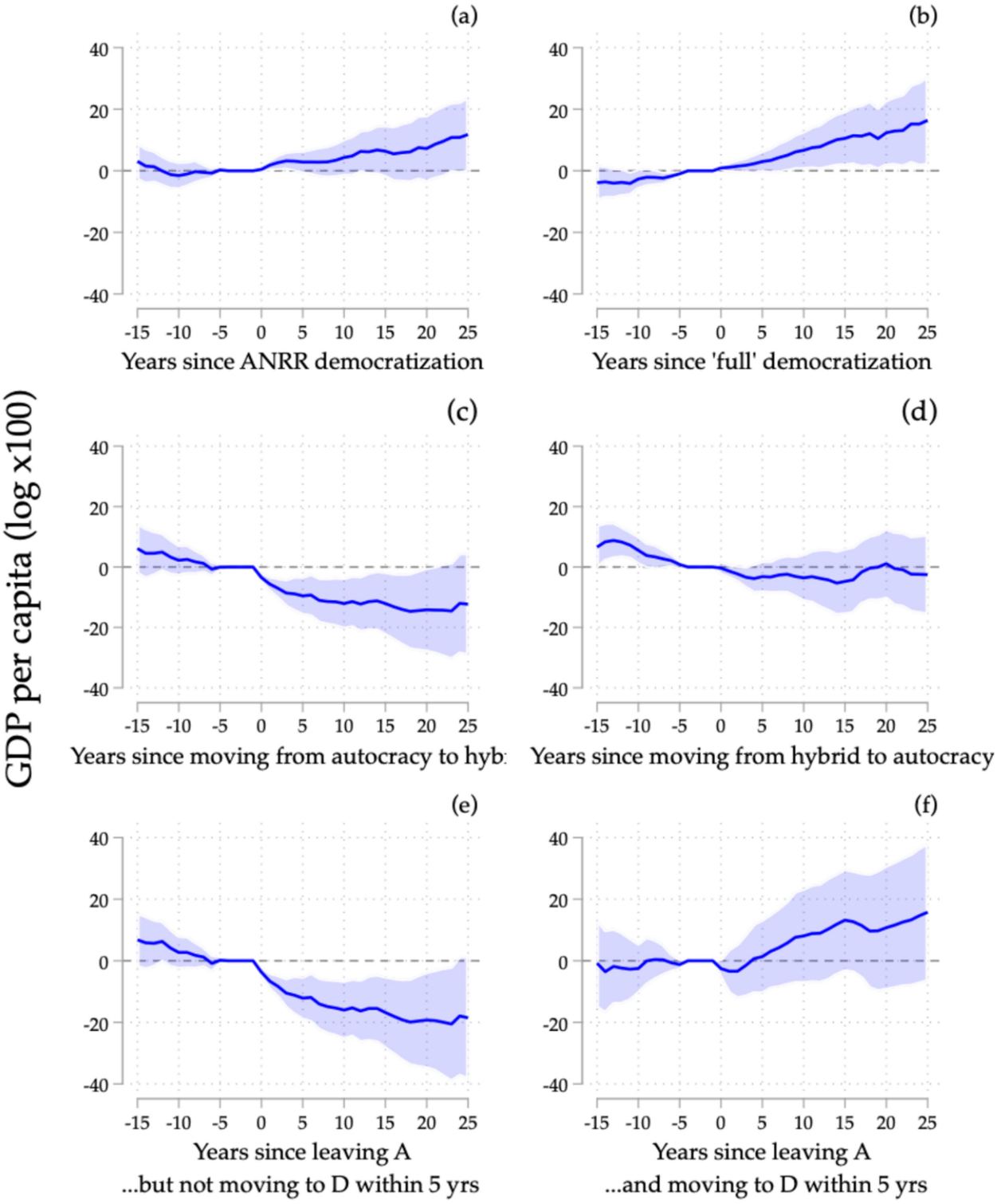


**FIGURE 2** | Estimated effects on GDP per capita of transitions between regime types using both Acemoglu et al. (ANRR) and our classification. The solid line shows the estimated effect of a regime change on log GDP per capita over time, while the shaded area represents the 95% confidence interval. Panel (a) reports transitions from non-democracy to democracy using the Acemoglu et al. (2019) binary classification. Panel (b) reports transitions to full democracy using our trichotomous classification. Panel (c) reports transitions from full autocracy to hybrid regimes. Panel (d) reports transitions from hybrid regimes to autocracy. Panel (e) reports transitions from full autocracy to hybrid regimes among countries that failed to consolidate into full democracy within five years. Panel (f) reports transitions from full autocracy to full democracy within five years. The specification assumes a one-year window ($L = 1$) for defining clean controls.

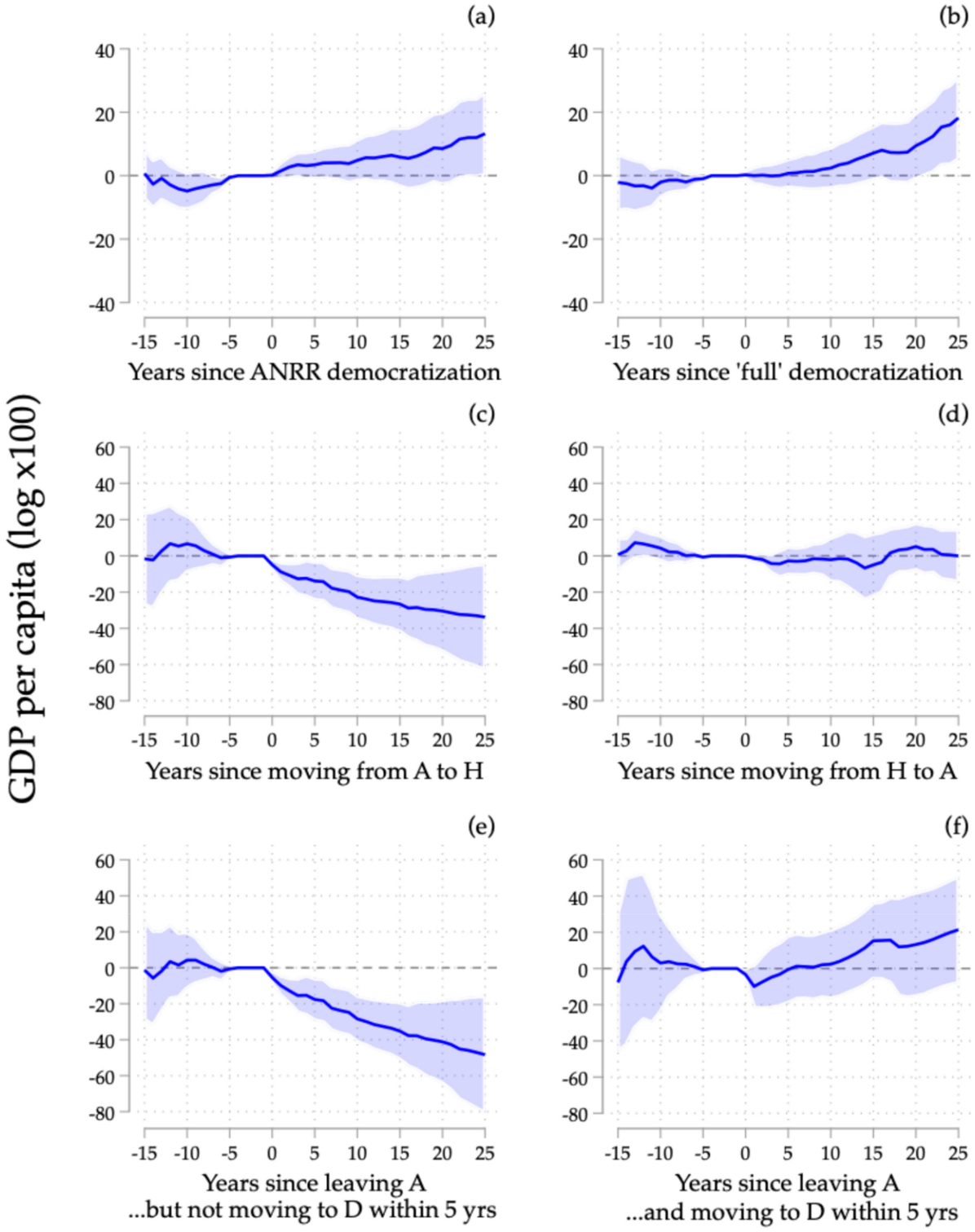


**FIGURE 3** | Estimated effects on GDP per capita of transitions between regime types using both Acemoglu et al. (ANRR) and our classification. The solid line shows the estimated effect of a regime change on log GDP per capita over time, while the shaded area represents the 95% confidence interval. Panel (a) reports transitions from non-democracy to democracy using the Acemoglu et al. (2019) binary classification. Panel (b) reports transitions to full democracy using our trichotomous classification. Panel (c) reports transitions from full autocracy to hybrid regimes. Panel (d) reports transitions from hybrid regimes to autocracy. Panel (e) reports transitions from full autocracy to hybrid regimes among countries that failed to consolidate into full democracy within five years. Panel (f) reports transitions from full autocracy to full democracy within five years. The specification assumes a fifteen-year window ($L = 15$) for defining clean controls.

defining the transition to *full* democracies according to our classification as detailed in the previous section. Here, the control group consists of countries that are *not* full democracies. The positive effect is not only confirmed but appears even stronger over time, suggesting that the beneficial impact of democratization on growth may be predominantly driven by countries that achieve full democratic consolidation. Figure 2(c), in contrast, shows that the impact on economic growth trends negatively when considering transitions *from full autocracies to hybrid regimes*, in line with our earlier findings. By its turn, Figure 2d illustrates that the reverse transition—*from hybrid regimes to autocracies*—does not yield a perfectly symmetric effect, but remains positive, reinforcing the notion that hybrid regimes tend to generate worse economic outcomes than autocracies and, of course, democracies. We further decompose Figure 2c by splitting the newly treated units—countries that exited autocracy at time $t$— into two subgroups: those that failed to transition to full democracy within 5 years (Figure 2e) and those that successfully completed the transition within 5 years (Figure 2f). While we acknowledge that conditioning the estimation sample on treatment status between $t+1$ and $t+5$ may introduce bias, this distinction provides a unique perspective on how exiting autocracy can lead to starkly divergent outcomes depending on whether the transition stalls in a hybrid regime or progresses toward full democracy.

Next, and in line with Dube et al. (2025), we extend the analysis by assuming a longer stabilization period of $L = 15$ years before treatment effects fully materialize, thereby restricting the selection of clean controls accordingly. Figure 3 shows that, encouragingly, this specification strengthens the key findings of our analysis. Specifically, the negative effect of transitioning from autocracy to a hybrid regime becomes even more statistically significant (Figure 3c), particularly for countries that fail to move toward full democracy within 5 years (Figure 3e). Also, the positive effect of transitioning *from hybrid regimes to autocracies* also becomes more pronounced (Figure 3f). These results further reinforce our finding that a hybrid regime impact on economic growth may be even more detrimental than that of autocratic regimes.

If the impact of democratization on economic growth were a monotonic process, the specific threshold used to distinguish between non-democracies and democracies should not significantly alter the conclusions of the analysis. In other words, if, at every level of a continuous measure of democracy, the more democratic countries consistently outperform the less democratic ones, a positive effect on growth would be observable in the data regardless of whether a higher or lower threshold is set.

However, our findings suggest otherwise. At the lower end of the democracy spectrum, the expected monotonic relationship does not hold. On the contrary, for countries emerging from full autocracy, the impact on economic growth is, on average, negative. Growth turns positive only if these nations manage to consolidate into full democracies within 5 years.

It is important to note that our analysis does not contradict the idea that democratization fosters higher economic growth over the medium to long term. Rather, it shows that when democratization begins from extremely low levels—where a country is not merely a non-democracy but a full autocracy—the transition away from autocratic rule does not guarantee future economic growth. In such cases, the process can lead to worse economic performance, which, in turn, may threaten the very success of the same democratic transition. Indeed, if the population does not experience tangible improvements in living conditions and economic opportunities within a reasonable timeframe, the incentives to revert to autocratic rule may increase. The longer a country remains in an intermediate phase – having exited autocracy but failing to consolidate democracy—the greater the risk of reversal. In policy terms, this underscores the importance of not only initiating democratization but ensuring that the process is completed effectively and within a relatively short period to secure both political and economic stability.

Finally, we should also recognize that any analysis that transforms continuous variables into categorical ones—whether dichotomous, trichotomous, or otherwise—is inevitably subject to the critique that its results may be sensitive to the specific thresholds used to define different categories. However, the existence of a threshold at which the empirical evidence shifts in the opposite direction provides strong evidence that the mere initiation of a democratization process is not a sufficient condition for sustained economic growth. Rather, the effects of democratization depend critically on both the initial conditions and the country's ability to consolidate democratic institutions within a relatively short timeframe.

### 4.3 | Heterogeneity in Treatment

LP-DiD and semiparametric estimates are not substitutes for the dynamic linear panel model. In fact, these approaches rely on different assumptions. On the one hand, unlike the dynamic linear panel model, LP-DiD does not rely on a linearity assumption; on the other hand, it restricts unobserved country heterogeneity to be common across all countries in the control sample, conditional on the recent trajectory of GDP per capita (see also Acemoglu et al. 2019 for further discussion). The consistency of results across these distinct methodological approaches significantly strengthens the implications of our analysis.

While the LP-DiD specification allows us to flexibly track the dynamic evolution of democratization (or reversal) effects over time, it does not account for potential biases arising from heterogeneous treatment effects across countries and periods. To address this limitation, we complement our analysis by applying the heterogeneity-robust DiD estimator proposed by de Chaisemartin and D'Haultfœuille (2020). This method refines our estimates by adjusting for differences in treatment intensity and ensuring that the average treatment effects remain unbiased despite cross-country and temporal variations in political transitions. Following the reasoning of de Chaisemartin and D'Haultfœille (2020), the coefficient on Democracy (Autocracy) estimated from a standard TWFE approach is a weighted average of the individual treatment effects ($TE_{c,t}$), with weights ($w_{c,t}$) depending on: (i) the average treatment in countries across years ($D_{c,.}$), (ii) the average treatment in years across countries ($D_{.,t}$), and (iii) the average treatment across countries and years ($D_{.,.}$).

Therefore, weights are not the same for each treated (*c*,*t*) cell, and standard TWFE models may not be robust to heterogeneous treatment effects, that is, when the treatment effect is not constant between groups or over time (de Chaisemartin and D'Haultfœille 2020). Indeed, if not homogenous, the treatment effects, up- and down-weighted by the TWFE approach, could systematically differ and, consequently, bias the estimated coefficient for the average treatment effect. This is the case, for example, if the treatment effects (i.e., the effects of democracy or autocracy) are the largest (or smallest) in countries or years where the average treatment (i.e., the frequency of autocracies or democracies) is the highest (or lowest). Furthermore, the TWFE approach can generate negative weights and, in the worst case, induce a sign reversal, with the coefficient on the treatment being positive even if the individual treatment effects are strictly negative (and vice versa).[14]

There is now a growing body of literature addressing these concerns, in terms of both diagnostic tools and alternative approaches using heterogeneity-robust DiD estimators. Concerning diagnostics, we find that the probability of sign reversal is low within our sample (as shown in Appendix C). However, the estimated coefficient is more likely to upweight early years and poor countries (in terms of GDP per capita in 1960). This may bias the average treatment effect if individual treatment effects systematically differ in these periods and countries. Since the diagnostic tests are not sufficient to rule out the potential bias induced by heterogenous treatment effects, de Chaisemartin and D'Haultfœuille (2020) propose an alternative heterogeneity-robust DiD estimator that still rely on parallel trend assumptions but is valid even if the treatment effect is heterogeneous over time or across groups (unlike TWFE regressions).

The new estimand DIDM is a weighted average of: (i) DiDs comparing the outcome evolution in two consecutive years of countries going from untreated (hybrid) to treated (alternatively autocracy or democracy)—that is, the "switchers in"—and of countries untreated (hybrid) at both dates; (ii) DiDs comparing the outcome evolution in two consecutive years of countries going from treated (alternatively autocracy or democracy) to untreated (hybrid)—that is, the "switchers out"—and of countries treated (alternatively autocracy or democracy) at both dates. de Chaisemartin and D'Haultfœuille (2020) also propose placebo estimators to test the parallel trends assumptions underlying DIDM. The placebo essentially compares the outcome's evolution from $t-2$ to $t-1$, in countries that switch and do not switch treatment between $t-1$ and $t$. A placebo showing a significant deviation from 0 would suggest that treated countries exhibit distinct trends prior to the switch, compared to untreated countries used for reconstructing counterfactual trends, thus raising questions about the assumption of parallel trends and the reliability of the estimates.

Table 2 shows the results obtained applying the DIDM both to transitions between autocracies and hybrid regimes and to transitions between democracies and hybrid regimes. In the autocracy specification (Panel A), we find that switching from hybrid regimes to full autocracies increases GDP per capita by about 1.7 percentage points (DIDM = 1.716), while in the democracy specification (Panel B), we find that switching from hybrid regimes to full democracies increases GDP per capita by about 1.4 percentage points (DIDM = 1.415). These results confirm the positive coefficients obtained with the TWFE estimation, and the magnitude of the coefficients is now somewhat larger. In both specifications, the placebo estimates are not significantly different from zero. Therefore, the heterogeneity-robust DiD estimator proposed by de Chaisemartin and D'Haultfœuille (2020) confirms our main results above regarding this being a causal non-linear relationship.

Furthermore, as explained in de Chaisemartin and D'Haultfoeuille (2022a), the DIDM may be biased in the presence of dynamic effects, meaning that past treatments might influence the current outcome. Moreover, this is particularly relevant when the design is not staggered, as is the case in our study, where groups can switch in and out of the treatment. To address this concern, de Chaisemartin and d'Haultfoeuille (2022b) propose treatment effect estimators robust to heterogenous and dynamic treatment effects when the design is not staggered. One limitation of these estimators lies in their exclusive focus on the first regime change (switch) for each country, subsequently comparing the ensuing economic performance (outcome) with a *status quo* scenario. As our design is not staggered, meaning several countries undergo multiple

**TABLE 2** | Alternative heterogeneity-robust $DiD_M$ estimator.

| **Panel A – Autocracy specification (transitions between autocracies and hybrid regimes)** | | | | | | |
|---|---|---|---|---|---|---|
| | **Estimate** | **Standard Error** | **Lower-Bound** | **Upper-bound** | ***N*** | **Switchers** |
| Treatment effect (autocracy) | 1.716 | 0.715 | 0.314 | 3.118 | 2945 | 182 |
| Placebo | −0.045 | 0.404 | −0.838 | 0.748 | 2611 | 160 |
| **Panel B – Democracy specification (transitions between democracies and hybrid regimes)** | | | | | | |
| | **Estimate** | **Standard Error** | **Lower-Bound** | **Upper-bound** | ***N*** | **Switchers** |
| Treatment effect (democracy) | 1.415 | 0.532 | 0.373 | 2.458 | 4435 | 147 |
| Placebo | 0.049 | 0.528 | −0.985 | 1.083 | 3917 | 127 |

*Note:* This table reports the estimation results for heterogeneity-robust $DID_M$ estimators of the instantaneous treatment effect and of placebo tests of the parallel trend assumption. The estimates reported in this table are obtained controlling for 2 lags of the dependent variable. Standard errors are clustered at the country level. Lower- and Upper-bound are those corresponding to a 95 percent confidence interval. *N* is the number of observations used in the estimation, and *Switchers* is the number of switchers the estimated treatment effects apply to. *Switchers* are also reported for the placebo test and reveal the number of switchers for which the placebo estimator can be calculated. The higher the number of switchers the placebo estimator apply to, the more the placebo test is reliable.

switches in and out over time, the interpretation of these estimators could be relatively complex.

To address these limitations, we restrict the analysis to a staggered setup where the treatment varies only once and in one direction. The estimators applied in this context are easily interpretable. The downside is that a staggered design requires keeping only observations consistent with such a design, leading to the loss of a significant amount of information. To overcome this loss of information, exploiting the relatively long number of years covered by the panel data, we consider different sub-periods for each country as if they were pertaining to distinct countries. This approach allows us to avoid completely dropping the relevant information. This may introduce a bias if the outcome of each country depends not only on the current treatment but also on past treatments. To mitigate this bias, we only consider cases where the regime remains stable for a sufficient number of years before the treatment (same regime for at least 5 years prior to the regime change) and for a sufficient number of years after the treatment (same regime for at least 5 years following the regime change). In this way, we substantially reduce the possibility that the observed treatment effect is influenced by previous treatment changes.

In this staggered treatment adoption setup, we can now utilize the estimand proposed by Callaway and Sant'Anna (2021). Specifically, we adopt the augmented inverse-probability weighting estimand (although we have verified that similar results obtain with the regression adjustment estimand). In this setup, we implicitly group all the units into cohorts, where each cohort *g* contains all the units that are treated for the first time at time *t*. For instance, "cohort 1972" consists of all the units for which the year 1972 marks the first year of the regime change. As we are in a staggered setup, we know that once a unit is treated, it will remain treated in all the following years. To account for heterogeneous treatment effects, the estimand proposed by Callaway and Sant'Anna (2021) estimates the average treatment effects separately for each combination of cohort *g* and time *t*. To visualize the evolution of the treatment effect after the treatment, it is possible to average within the times exposed to treatment (e.g., 1 year after the treatment started, 2 years after the treatment started, and so on). This also allows us to explore whether some anticipation of treatment exists, that is, whether there is a significant effect at any time of exposure before the treatment. In our main specification, we also control for the first and second lags of our dependent variable. Moreover, in order to increase the number of observations in the control group, we consider as control group not simply the never-treated group (i.e., units that are never treated) but rather the not-yet-treated group (i.e., when estimating the treatment effect for the cohort g at time t, we control for the units that are not in group g and not yet treated at time t).

Figure 4 shows these results for (a) the shift from autocracy to hybrid and (b) the shift from democracy to hybrid. Significant negative effects are confirmed in both from autocracy to hybrid and

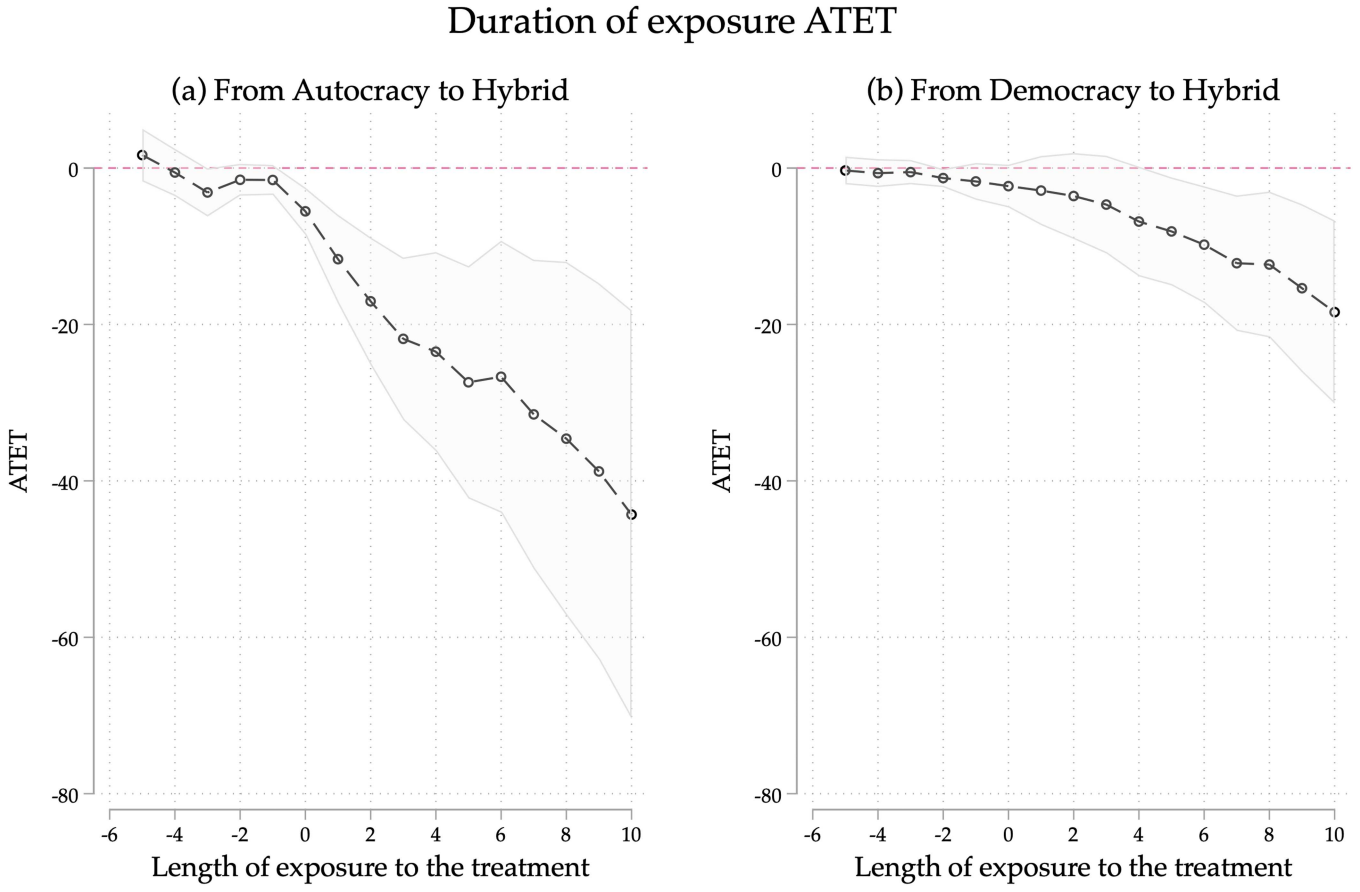


**FIGURE 4** | Pairwise comparisons in a difference-in-differences framework. Panel (a) reports the autocracy specification, focusing on transitions from autocracy to hybrid regimes. Panel (b) reports the democracy specification, focusing on transitions from democracies to hybrid regimes.

from democracy to hybrid transitions. Moreover, the negative effect increases with the exposure to treatment. The more countries remain within a hybrid regime after the transition, the more the economic performance deteriorates (compared to the status quo).

To underscore the significance of our DiD approach, we also applied the same analysis using the framework and data sample provided by Acemoglu et al. (2019), which as noted employs a binary representation of political regimes (democracy vs. non-democracy). Interestingly, within this framework, only transitions from democracy to non-democracy exhibit statistically significant adverse impacts on GDP per capita. In contrast, transitions from non-democracy to democracy do not yield significant effects.[15] This result would leave a gap in the evaluation of the effects of democratization, as a large fraction of political transitions in the post-WWII period were transitions out of autocratic regimes. The results from our trichotomic framework help fill this gap, offering a possible explanation for the lack of significant effects of transitions from nondemocracy to democracy. On one hand, many democratization events in the Acemoglu et al. (2019) framework correspond to partial improvements in democracy indices that are insufficient to transition countries out of the hybrid regime status. On the other hand, we also uncover a novel and statistically significant negative effect associated to transitions from full autocracies to hybrid regimes, which are in part classified as democracies in Acemoglu et al. (2019). This highlights some of the main limitations of a dichotomous classification of political regimes and underscores the importance of providing a fuller characterization of the economic costs of hybrid regimes.

## 5 | Additional Evidence and Robustness Checks

As an additional exercise, we report pooled OLS estimates that exploit cross-country variation while controlling for a rich set of time-invariant country characteristics (Table 3). While this specification is more susceptible to omitted variable bias and therefore not our preferred identification strategy, it provides a useful benchmark to assess whether the patterns identified in the fixed-effects models also emerge in the cross-section. Specifically, Table 3 reports the estimated coefficients for the following specification:

$$y_{ct} = \alpha + \theta\beta_1 Autocracy_{ct} + \beta_2 Democracy_{ct} + \sum_{i=1}^{p} \gamma_i x_c^{(i)} + \delta_t + \varepsilon_{ct}, \quad (2)$$

where $y_{ct}$ is the log of GDP per capita in country $c$ at time $t$, $Autocracy_{ct}$ is a dummy variable equal to 1 if country $c$ has been coded as an autocracy at time $t$ (and 0 otherwise); $Democracy_{ct}$ is a dummy variable equal to 1 if country $c$ has been coded as a democracy at time $t$ (and 0 otherwise); while the "intermediate or hybrid regime" is the omitted reference category. In addition, $x_c^{(i)}$ for $i = 1, ..., p$ is a set of time-invariant country characteristics, including legal origins, ethnic and religious fractionalization, colonial history, geographical regions, and initial per capita GDP. A full set of year fixed effects $\delta_t$ is also included. Reassuringly, the results are broadly consistent with our main findings.

The first column of Table 3 contains a bare-bones specification that only includes dummies for democracies and autocracies (with "hybrid regimes" the omitted variable), and no country, region, or year fixed effects. In this simple setting, there is clear evidence that democracies exhibit substantially higher economic performance than both hybrid regimes and autocracies, while autocracies, in turn, display significantly higher GDP levels than hybrid regimes. The inclusion of year fixed effects in column 2 further strengthens the relative performance of autocracies. This suggests that unobserved time-specific factors may simultaneously reduce the probability of being an autocracy while increasing GDP per capita. Year fixed effects help mitigate spurious correlations arising from contemporaneous trends, such as global increases in GDP per capita alongside a declining prevalence of autocratic regimes.

Next, we enlarge these basic results with legal origins and religious affiliation dummy variables in the expectation of capturing an additional set of important country-specific characteristics that may have bearings on the democracy-income relationship. One might expect that the Socialist legal origin will be associated with worse economic development outcomes than the others (with French legal origin as the omitted category). One may also hypothesize along similar line for Muslim religious affiliation (with Catholic as the omitted category). In column 3 we add to the specification in column 2 only legal origins, in column 4 we add only religious affiliation, and in column 5 we add both plus measures of ethnic and religious fractionalization. Our baseline results do not change, as there is still strong evidence of a nonmonotonic relationship. It is important to notice that the effect of democracy on economic performance is still more than 10 times larger than that of autocracy (both with respect to hybrid regimes), which makes it perhaps not too difficult to reconcile it with the prevailing view (cf. Acemoglu et al. 2019) of a linear relationship between political and economic development.

This size discrepancy changes once we account for countries' initial conditions, particularly GDP per capita at the beginning of the observation period, and whether a country is a former colony, as reported in column 6. In column 6, we add the "colony" dummy variable from Cervellati et al. (2014)[16] and find that it clearly does not change our main findings. However, the new specification reduces the coefficient on democracy more than that on autocracy, making the two coefficients more comparable in size. This provides further support for the nonlinear, U-shaped relationship that we argue fundamentally characterizes the income–democracy nexus. Column 7 adds region fixed-effects, coded according to the World Bank classification, and our basic results remain strong.

In column 8, we follow Acemoglu et al. (2019, Appendix A9.3) and introduce an interaction between region and "initial regimes". Acemoglu et al. (2019) operationalize initial regimes using country characteristics in 1960 (including: British colonies, French colonies, civil dictatorships, military dictatorships, mixed and presidential democracies, parliamentary democracies, royal dictatorships, and socialist regimes). This yields 34 region*regime cells. Our main results do not change.

A potential concern with our analysis is that GDP data may be subject to systematic measurement error across political

**TABLE 3** | Effect of democracy and autocracy on (Log) GDP per Capita – controlling for slow-moving country characteristics.

| Dep. Var.: (Log) GDP per Capita | Pooled OLS | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Autocracy | 10.898*** | 17.487*** | 17.970*** | 23.975*** | 18.381*** | 15.184*** | 14.769*** | 17.505*** |
| | (3.632) | (3.775) | (3.679) | (4.048) | (3.786) | (2.284) | (2.243) | (2.143) |
| Democracy | 209.594*** | 208.970*** | 183.720*** | 203.518*** | 150.348*** | 34.339*** | 34.293*** | 41.464*** |
| | (2.869) | (2.856) | (2.899) | (3.264) | (3.296) | (2.265) | (2.227) | (2.113) |
| English Legal Origin | | | −19.024*** | | 37.802*** | 29.943*** | 33.865*** | 10.782*** |
| | | | (2.880) | | (3.546) | (2.143) | (2.162) | (2.933) |
| Socialist Legal Origin | | | −1.137 | | −22.900*** | −32.404*** | −59.834*** | −118.397*** |
| | | | (3.719) | | (6.294) | (3.778) | (4.538) | (10.760) |
| Scandinavian Legal Origin | | | 141.268*** | | 172.973*** | 72.912*** | 73.081*** | 52.707*** |
| | | | (6.898) | | (12.901) | (8.102) | (7.801) | (8.159) |
| German Legal Origin | | | 131.317*** | | 117.855*** | 52.969*** | 52.527*** | 43.425*** |
| | | | (7.113) | | (7.289) | (4.501) | (4.294) | (4.547) |
| Religion: Protestant, %pop | | | | 0.400*** | −1.028*** | −0.815*** | −0.643*** | −0.516*** |
| | | | | (0.070) | (0.128) | (0.082) | (0.081) | (0.090) |
| Religion: Muslim, %pop | | | | −0.346*** | −0.413*** | 0.054* | −0.040 | −0.406*** |
| | | | | (0.048) | (0.048) | (0.029) | (0.041) | (0.052) |
| Religion: Other, %pop | | | | −0.977*** | −1.127*** | −0.042 | −0.057 | 0.030 |
| | | | | (0.048) | (0.058) | (0.037) | (0.039) | (0.046) |
| Religion Fractionalization | | | | | 32.132*** | 18.868*** | 27.958*** | 1.001 |
| | | | | | (7.706) | (4.790) | (4.696) | (5.027) |
| Ethnic Fractionalization | | | | | −182.722*** | −116.371*** | −67.450*** | −79.026*** |
| | | | | | (5.932) | (3.653) | (3.903) | (4.156) |
| Colony | | | | | | −26.855*** | −7.768*** | 3.790 |
| | | | | | | (2.255) | (2.499) | (3.017) |
| GDP per capita in 1960, ln | | | | | | 1.279*** | 0.982*** | 0.975*** |
| | | | | | | (0.012) | (0.017) | (0.017) |
| Year F.E. | NO | YES | YES | YES | YES | YES | YES | YES |
| Region F.E. | NO | NO | NO | NO | NO | NO | YES | (absorbed) |
| Region × initial regime F.E. | NO | NO | NO | NO | NO | NO | NO | YES |
| Observations | 8739 | 8739 | 8739 | 7728 | 7234 | 6961 | 6961 | 6961 |
| $r^2$ | 0.40 | 0.41 | 0.47 | 0.46 | 0.57 | 0.85 | 0.87 | 0.90 |

*Note:* This table reports the estimation results of the model specification represented by Equation (2). It uses the trichotomous classification of political regimes described in Section 3. The reported coefficients are multiplied by 100 to ease their interpretation. *Legal Origin: French* and *Omitted Religion: Catholic* are the omitted categories chosen as the benchmark for the *Legal Origin* and *Religion* categories, respectively. In column (7), 7 regions are considered (World Bank classification): Africa; East Asia and the Pacific; Eastern Europe and Central Asia; Western Europe and other developed countries; Latin America and the Caribbean; the Middle East, and the North of Africa; South Asia. In column (8), following Acemoglu et al. (2019, Appendix A9.3), we introduce *region* × *initial regime* classification (in this case, we have 34 region × regime cells). Initial regimes are based on country characteristics in 1960 (including British colonies, French colonies, civil dictatorships, military dictatorships, mixed and presidential democracies, parliamentary democracies, royal dictatorships, and socialist regimes). Standard errors in brackets. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

regimes. Martínez (2022) shows that autocracies may overstate economic growth by substantial margins using night-time light data as an alternative proxy for economic activity. If this were the case, the relative economic performance of autocracies could be overstated in our baseline results, potentially weakening the evidence in favor of a non-monotonic relationship between political regimes and economic development.

Several studies use night-lights data with a seminal paper being Henderson et al. (2012) and recent surveys provided by

Donaldson and Storeygard (2016) and Gibson et al. (2020). The night-lights data is collected by satellite and as such has important limitations in terms of time coverage (i.e., data is unavailable before the 1990s). For our objectives, we use the same night-lights data used in Martínez (2022) for the period 1992–2018 and compare previous results for our three political regimes obtained with GDP per capita with those obtained with night-lights. Figure 5 shows that GDP per capita and light intensity are highly correlated (correlation = 0.74) and, more importantly for our purposes, without any clear biases due to differences in political regimes.

Table 4 revisits our econometric specification using slow-moving country characteristics (legal origins, religious fractionalization, initial per capita GDP, etc.), but instead of measuring economic performance in GDP terms, we focus on night-light intensity as an alternative proxy. Given the more limited time coverage of night-lights data, this specification has the additional advantage of being less data-intensive, as it does not rely on lagged GDP or other variables requiring longer time series.

The results strongly confirm the non-monotonic relationship between economic performance (now proxied by night-lights) and political regimes. In particular, the sequence of results remains consistent with our baseline findings: the effect of democracy is substantially larger than that of autocracy across specifications, and this difference narrows once we take into account initial GDP per capita, making the relative effects of democracy and autocracy more comparable in size. Moreover, the relative overperformance of autocracies compared to hybrid regimes appears even more robust (compared to Table 3) when using night-lights. This pattern is not necessarily in contrast with the view that measurement error in GDP may partly reflect strategic misreporting by non-democratic regimes. If anything, it may suggest that such incentives are even stronger in some hybrid regimes, such as electoral autocracies, where governments face both domestic and international pressures to signal economic success.

As a first step, we re-estimate our baseline model after symmetrically trimming 5 percent of the observations from the tails of the GDP per capita distribution (2.5 percent from each tail). Reassuringly, the results (Table 5) remain qualitatively unchanged and, if anything, become even more statistically significant than in the baseline specification (Table 1). This suggests that our main findings are not driven by extreme observations; rather, the presence of such outliers appears to attenuate the precision of the estimated effects.

We also verify that our results are not driven by recent dynamics associated with the COVID-19 pandemic. To this end, we re-estimate our baseline specification restricting the sample to the pre-pandemic period, ending in 2019. Reassuringly, our main findings are confirmed and, if anything, become even more statistically significant (see Table B.3 in the Appendix).

In addition, we examine whether the relatively weaker performance of hybrid regimes may be driven by short-lived economic downturns associated with periods of regime transition. As noted in previous sections, hybrid regimes are not merely transitional: their average duration exceeds 10 years, and the observed performance gap tends to accumulate over time rather than reflecting temporary disruptions around regime change. Nonetheless, we explicitly test this possibility in Table 6. First, we augment our baseline specification by controlling for transition years through a dummy variable equal to one in year $t$ and year $t+1$ if t is a transition year, and

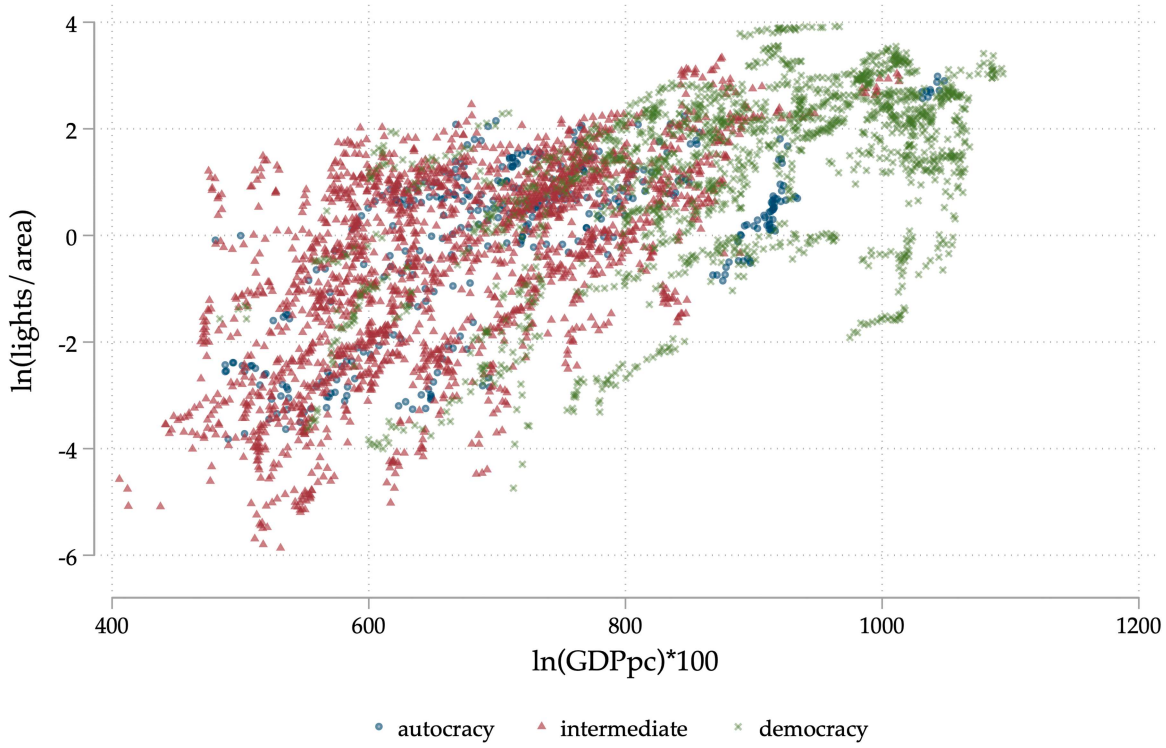


**FIGURE 5** | Night-lights and GDP per capita in the different regime types (trichotomous classification).

**TABLE 4** | Effect of Democracy and Autocracy on Night Lights (1992–2018) – controlling for slow-moving country characteristics.

| | Pooled OLS | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Dep. Var.: lnNTL** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** |
| Autocracy | 60.279*** | 71.877*** | 58.897*** | 115.622*** | 50.422*** | 21.716** | 49.679*** | 63.598*** |
| | (8.892) | (8.470) | (8.485) | (9.643) | (9.577) | (8.916) | (9.155) | (9.459) |
| Democracy | 170.145*** | 169.811*** | 157.117*** | 210.255*** | 125.880*** | 33.345*** | 52.733*** | 39.200*** |
| | (5.740) | (5.458) | (5.670) | (6.531) | (6.682) | (6.528) | (6.147) | (5.814) |
| English Legal Origin | | | −25.378*** | | 63.071*** | 79.192*** | 50.810*** | −46.461*** |
| | | | (6.078) | | (7.216) | (6.430) | (6.469) | (8.617) |
| Socialist Legal Origin | | | 47.138*** | | −28.611** | −19.437* | −144.602*** | −357.372*** |
| | | | (6.738) | | (12.091) | (10.673) | (13.748) | (29.821) |
| Scandinavian Legal Origin | | | 19.166 | | 289.818*** | 211.108*** | 214.062*** | 85.037*** |
| | | | (16.011) | | (27.704) | (24.798) | (23.230) | (23.677) |
| German Legal Origin | | | 147.589*** | | 137.158*** | 69.035*** | 77.733*** | 23.274* |
| | | | (15.295) | | (15.100) | (13.278) | (12.473) | (13.198) |
| Religion: Protestant, %pop | | | | −1.370*** | −3.624*** | −3.829*** | −3.089*** | −2.222*** |
| | | | | (0.152) | (0.268) | (0.245) | (0.234) | (0.257) |
| Religion: Muslim, %pop | | | | −0.152 | −0.092 | 0.032 | −0.781*** | −1.088*** |
| | | | | (0.098) | (0.099) | (0.086) | (0.118) | (0.149) |
| Religion: Other, %pop | | | | −0.879*** | −1.141*** | −0.293*** | −0.363*** | −0.135 |
| | | | | (0.099) | (0.120) | (0.112) | (0.114) | (0.132) |
| Religion Fractionalization | | | | | 141.000*** | 110.848*** | 162.862*** | 57.775*** |
| | | | | | (15.394) | (13.908) | (13.324) | (13.894) |
| Ethnic Fractionalization | | | | | −276.484*** | −209.072*** | −132.557*** | −87.817*** |
| | | | | | (12.290) | (11.087) | (11.444) | (11.863) |
| Colony | | | | | | −57.357*** | −22.139*** | −22.508*** |
| | | | | | | (6.399) | (6.861) | (8.116) |
| GDP per capita in 1960, ln | | | | | | 0.970*** | 0.638*** | 0.900*** |
| | | | | | | (0.032) | (0.045) | (0.047) |
| Year F.E. | NO | YES | YES | YES | YES | YES | YES | YES |
| Region F.E. | NO | NO | NO | NO | NO | NO | YES | (absorbed) |
| Region × initial regime F.E. | NO | NO | NO | NO | NO | NO | NO | YES |
| Observations | 4460 | 4460 | 4433 | 3735 | 3442 | 3172 | 3172 | 3172 |
| $r^2$ | 0.17 | 0.25 | 0.28 | 0.32 | 0.45 | 0.60 | 0.67 | 0.74 |

*Note:* This table reports the estimation results of the model specification represented by Equation (1). It uses the trichotomous classification of political regimes described in Section 3. The reported coefficients of Autocracy and Democracy are multiplied by 100 to ease their interpretation. The dependent variable (lnNTL) is the natural logarithm of the area-weighted average of a country's cell-level night-time-light digital number (Martínez 2022). Lights data covering the period 1992–2024 are aggregated by Martínez (2022) starting from data on night-time light provided by the National Oceanic and Atmospheric Administration (NOAA). These data are then extended to 2024 using harmonized NTL data from DMSP-OLS and VIIRS instruments provided by Li et al. (2020). For additional information about other explanatory variables, see Table B.1. Standard errors are clustered at the country level and are reported in brackets. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. After these additional analyses, we further assess the robustness of our main specification by examining whether our results are sensitive to the exclusion of potentially influential observations, years, and countries.

zero otherwise (columns 1–4). Second, we re-estimate the model excluding all transition years from the sample (columns 5–8). These results are consistent with our main findings. Although excluding transition years leads to some loss of statistical significance in the difference between autocracies and hybrid regimes, the estimated coefficients retain the same sign. Our findings also remain robust to excluding each country's year of independence and the three

**TABLE 5** | Effect of democracy and autocracy on (Log) GDP per Capita after trimming 5 percent of the observations – lags, country, and year fixed effects.

| | Within Estimates | | | | Arellano and Bond Estimates | | | |
|---|---|---|---|---|---|---|---|---|
| **Dep. Var.: (Log) GDP per Capita** | **(1-lags)** | **(2-lags)** | **(4-lags)** | **(8-lags)** | **(1-lags)** | **(2-lags)** | **(4-lags)** | **(8-lags)** |
| Autocracy | 0.546 | 0.528** | 0.573** | 0.913*** | 0.903* | 0.797** | 0.627* | 0.932*** |
| | (0.338) | (0.246) | (0.228) | (0.269) | (0.466) | (0.394) | (0.331) | (0.333) |
| Democracy | 0.604 | 0.616** | 0.675** | 0.753** | 0.605 | 0.642* | 0.713** | 0.696** |
| | (0.368) | (0.286) | (0.273) | (0.300) | (0.458) | (0.362) | (0.350) | (0.345) |
| log GDP first lag | 0.978*** | 1.305*** | 1.278*** | 1.280*** | 0.963*** | 1.263*** | 1.241*** | 1.245*** |
| | (0.003) | (0.034) | (0.034) | (0.032) | (0.007) | (0.037) | (0.036) | (0.033) |
| log GDP second lag | | −0.330*** | −0.236*** | −0.250*** | | −0.302*** | −0.217*** | −0.234*** |
| | | (0.033) | (0.048) | (0.045) | | (0.035) | (0.047) | (0.044) |
| log GDP third lag | | | −0.022 | −0.020 | | | −0.020 | −0.016 |
| | | | (0.029) | (0.031) | | | (0.027) | (0.030) |
| log GDP fourth lag | | | −0.048*** | −0.040* | | | −0.042** | −0.036 |
| | | | (0.018) | (0.023) | | | (0.018) | (0.022) |
| *p*-value, lags 5–8 | | | | 0.080 | | | | 0.254 |
| Long−run effect of autocracy | 24.831 | 21.118** | 20.312** | 27.234*** | 24.351* | 20.717** | 16.541* | 21.740*** |
| | (15.830) | (10.192) | (8.561) | (8.319) | (13.029) | (10.304) | (8.972) | (7.835) |
| Long-run effect of democracy | 27.490 | 24.657** | 23.948** | 22.472** | 16.316 | 16.668* | 18.816** | 16.223** |
| | (17.868) | (11.940) | (10.171) | (9.491) | (12.350) | (9.439) | (9.231) | (8.036) |
| Effect of autocracy after 25 years | 10.587 | 12.944** | 14.606** | 22.413*** | 14.878* | 15.749** | 13.259* | 18.969*** |
| | (6.569) | (6.115) | (5.926) | (6.615) | (7.685) | (7.760) | (7.099) | (6.816) |
| Effect of democracy after 25 years | 11.721 | 15.113** | 17.221** | 18.494** | 9.969 | 12.671* | 15.083** | 14.156** |
| | (7.218) | (7.160) | (7.185) | (7.678) | (7.500) | (7.172) | (7.430) | (6.970) |
| Persistence of GDP process | 0.978*** | 0.975*** | 0.972*** | 0.966*** | 0.963*** | 0.962*** | 0.962*** | 0.957*** |
| | (0.003) | (0.003) | (0.003) | (0.005) | (0.007) | (0.006) | (0.005) | (0.006) |
| AR2 test *p*-value | | | | | 0.00 | 0.19 | 0.99 | 0.78 |
| Observations | 8130 | 7958 | 7618 | 6953 | 7955 | 7786 | 7449 | 6787 |
| Countries in sample | 162 | 162 | 161 | 160 | 162 | 162 | 161 | 160 |

*Note:* This table reports the estimation results of the model specification represented by Equation (1) in the main text. It uses the trichotomous classification of political regimes described in Section 3. The regressions are run after symmetrically trimming 5 percent of the observations from the tails of the GDP per capita distributions (2.5 percent on the left and 2.5 percent on the right). The reported coefficients of Autocracy and Democracy are multiplied by 100 to ease their interpretation, and all standard errors are robust to heteroscedasticity. Columns 1–4 report the *within estimator* results. Columns 5–8 report *Arellano-Bond GMM* results. Columns 4 and 8 include eight lags of the dependent variable as controls, but for lags 5–8 only the *p*-value of a test for joint significance is reported. The *AR2 test p-value* is the *p*-value for a test of serial correlation in the residuals of the GDP series. Following Acemoglu et al. (2019), long-run effects and the effects over the next 25 years are also reported in the table (see footnote 15 for a description of their computation). Standard errors are clustered at the country level and are reported in brackets. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

subsequent years, a period often characterized by substantial institutional and economic instability.[17]

To rule out the possibility that small economies exert a disproportionate influence on our results, we replicate the analysis excluding the 16 countries with fewer than one million inhabitants, based on 2010 population data. Table 7 (columns 1–4) shows that our main findings remain unchanged. We further verify that the results are fully robust when extending this exclusion to a broader set of 26 countries with fewer than one million inhabitants[18]. Taken together, these findings indicate that our results are not driven by the behavior of relatively small economies in terms of population size.

In addition, it has been argued that large fluctuations in oil prices may not be fully captured by standard country fixed effects when considering OPEC countries (e.g., Paldam 2022). To address this concern, we augment our baseline specification by introducing OPEC × year fixed effects, thereby allowing for different time dynamics between OPEC and non-OPEC countries.[19] Once again, Table 7 (columns 5–8) confirms our main results, ruling out the possibility that the observed U-shaped relationship is driven by the specific economic dynamics of OPEC countries.

To assess whether our results are sensitive to time-varying economic and political factors that may jointly influence both regime type and GDP, we re-estimate our specification including the same set of covariates used by Acemoglu et al. (2019, Table 4). The results are broadly confirmed (see Table B.4 in the Appendix).

We also adopt an alternative dynamic specification in which the dependent variable is expressed in growth rates rather than

**TABLE 6** | Controlling for transition years $[t; t+1]$.

| Dep. Var.: (Log) GDP per Capita | Controlling for transition in years $[t; t+1]$ | | | | Excluding year $t$ and $t+1$ | | | |
|---|---|---|---|---|---|---|---|---|
| | (1-lags) | (2-lags) | (4-lags) | (8-lags) | (1-lags) | (2-lags) | (4-lags) | (8-lags) |
| Autocracy | 0.361 | 0.448* | 0.528** | 0.810*** | 0.050 | 0.090 | 0.319 | 0.839** |
| | 0.335) | (0.247) | (0.236) | (0.270) | (0.448) | (0.365) | (0.335) | (0.353) |
| Democracy | 0.611* | 0.639** | 0.698** | 0.739** | 0.579 | 0.598 | 0.766** | 0.846** |
| | (0.363) | (0.281) | (0.270) | (0.298) | (0.497) | (0.373) | (0.366) | (0.370) |
| Transition years $[t; t+1]$ | −1.802*** | −1.310*** | −1.164*** | −1.165*** | −2.352*** | −1.657*** | −1.429*** | −1.403*** |
| | (0.381) | (0.306) | (0.282) | (0.310) | (0.588) | (0.470) | (0.410) | (0.394) |
| log GDP first lag | 0.980*** | 1.294*** | 1.260*** | 1.260*** | 0.961*** | 1.267*** | 1.238*** | 1.231*** |
| | (0.004) | (0.036) | (0.034) | (0.035) | (0.006) | (0.038) | (0.035) | (0.035) |
| log GDP second lag | | −0.318*** | −0.210*** | −0.216*** | | −0.305*** | −0.200*** | −0.207*** |
| | | (0.035) | (0.042) | (0.040) | | (0.037) | (0.042) | (0.040) |
| log GDP third lag | | | −0.024 | −0.020 | | | −0.021 | −0.016 |
| | | | (0.028) | (0.028) | | | (0.027) | (0.027) |
| log GDP fourth lag | | | −0.054*** | −0.055** | | | −0.056*** | −0.053** |
| | | | (0.017) | (0.022) | | | (0.018) | (0.022) |
| p-value, lags 5–8 | | | | 0.814 | | | | 0.838 |
| Long-run effect of autocracy | 18.374 | 18.834* | 19.360** | 25.843*** | 1.280 | 2.355 | 8.116 | 19.092** |
| | (17.578) | (10.963) | (9.364) | (9.078) | (11.498) | (9.544) | (8.540) | (7.930) |
| Long-run effect of democracy | 31.063 | 26.908** | 25.619** | 23.597** | 14.889 | 15.692 | 19.490** | 19.257** |
| | (19.432) | (12.196) | (10.172) | (9.844) | (12.730) | (10.124) | (9.853) | (8.689) |
| Effect of autocracy after 25 years | 7.193 | 11.052* | 13.576** | 19.841*** | 0.805 | 1.785 | 6.749 | 16.536** |
| | (6.680) | (6.228) | (6.261) | (6.745) | (7.241) | (7.246) | (7.109) | (6.957) |
| Effect of democracy after 25 years | 12.161* | 15.789** | 17.964** | 18.117** | 9.361 | 11.894 | 16.207** | 16.679** |
| | (7.224) | (7.035) | (7.058) | (7.439) | (7.989) | (7.543) | (7.998) | (7.449) |
| Persistence of GDP process | 0.980*** | 0.976*** | 0.973*** | 0.969*** | 0.961*** | 0.962*** | 0.961*** | 0.956*** |
| | (0.004) | (0.003) | (0.004) | (0.005) | (0.006) | (0.006) | (0.006) | (0.007) |
| Observations | 8584 | 8426 | 8106 | 7466 | 8422 | 8264 | 7944 | 7304 |
| Countries in sample | 162 | 162 | 162 | 162 | 162 | 162 | 162 | 162 |

*Note:* This table reports the estimation results of the model specification represented by Equation (1) in the main text. The only difference with Table 1 is the additional control variable *Transition years*, which is a dummy variable equal to 1 in year t and year $t+1$ if t is a transition year; 0 otherwise. The reported coefficients of Autocracy and Democracy are multiplied by 100 to ease their interpretation. Standard errors are clustered at the country level and are reported in brackets. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

levels. While our baseline model is specified in levels and includes lags from 1 to 8 of GDP per capita, we now model GDP growth (measured as the change in log GDP per capita, Δlog GDP per capita) as a function of its own lags and lagged GDP per capita. In particular, we include lags of GDP growth from 1 to 8 in order to flexibly capture short- and medium-run dynamics, together with one lag of GDP per capita to account for conditional convergence. This specification allows us to disentangle the role of past income levels from recent growth dynamics and to verify that our results are not driven by the specific functional form adopted in the baseline model. Reassuringly, the main results are confirmed (see Table B.5 in the Appendix).

As a further robustness check, we assess the sensitivity of our results to the choice of thresholds in the classification based on V-Dem. As discussed in Section 3, the construction of the trichotomous classification of political regimes requires defining thresholds based on the polyarchy index, chosen to align with the empirical overlap with the classification based on Polity. Specifically, we set a lower threshold at the 75th percentile of the polyarchy distribution among closed autocracies and lower-bound electoral autocracies and an upper threshold at the 75th percentile of the distribution among electoral autocracies. While Tables B.1 and B.2 in the Appendix show that our findings are robust to alternative classifications based solely on the Polity index, as well as to specifications using continuous measures of democracy (including quadratic terms), a remaining concern is the sensitivity to the specific threshold choice. Reassuringly, Table B.6 in the Appendix shows that our findings remain unchanged when both thresholds are set at the 50th percentile rather than the 75th.

One additional important robustness check is to examine whether regime transitions are systematically related to prior GDP dynamics (reverse causality). Using a probit specification,

**TABLE 7** | Excluding small economies and controlling for OPEC-specific behavior.

| Dep. Var.: (Log) GDP per Capita | Excluding small economies | | | | Controlling for OPEC-specific behavior | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (1-lags) | (2-lags) | (4-lags) | (8-lags) |
| Autocracy | 0.420 | 0.490** | 0.549** | 0.742*** | 0.306 | 0.407 | 0.476* | 0.759*** |
| | (0.332) | (0.246) | (0.240) | (0.275) | (0.354) | (0.261) | (0.251) | (0.281) |
| Democracy | 0.348 | 0.443* | 0.537** | 0.544* | 0.485 | 0.540* | 0.591** | 0.611** |
| | (0.326) | (0.266) | (0.266) | (0.286) | (0.354) | (0.274) | (0.260) | (0.285) |
| log GDP first lag | 0.981*** | 1.295*** | 1.269*** | 1.274*** | 0.980*** | 1.293*** | 1.259*** | 1.257*** |
| | (0.004) | (0.037) | (0.037) | (0.037) | (0.004) | (0.035) | (0.033) | (0.035) |
| log GDP second lag | | −0.318*** | −0.235*** | −0.248*** | | −0.317*** | −0.207*** | −0.211*** |
| | | (0.037) | (0.046) | (0.043) | | (0.034) | (0.041) | (0.040) |
| log GDP third lag | | | −0.012 | −0.001 | | | −0.027 | −0.022 |
| | | | (0.028) | (0.026) | | | (0.028) | (0.029) |
| log GDP fourth lag | | | −0.048*** | −0.066*** | | | −0.052*** | −0.056** |
| | | | (0.017) | (0.024) | | | (0.016) | (0.023) |
| *p*-value, lags 5–8 | | | | 0.576 | | | | 0.819 |
| Long-run effect of autocracy | 22.275 | 21.345* | 21.134** | 25.451*** | 15.215 | 17.022 | 17.359* | 23.871*** |
| | (17.622) | (10.972) | (9.656) | (9.632) | (18.013) | (11.349) | (9.729) | (9.246) |
| Long-run effect of democracy | 18.458 | 19.284* | 20.652** | 18.678* | 24.130 | 22.578* | 21.564** | 19.240** |
| | (16.896) | (11.365) | (10.090) | (9.650) | (17.853) | (11.546) | (9.582) | (9.099) |
| Effect of autocracy after 25 years | 8.440 | 12.244** | 14.201** | 18.270*** | 6.053 | 10.025 | 12.194* | 18.446*** |
| | (6.622) | (6.198) | (6.273) | (6.752) | (7.034) | (6.524) | (6.592) | (6.957) |
| Effect of democracy after 25 years | 6.994 | 11.062* | 13.877** | 13.408* | 9.600 | 13.297** | 15.148** | 14.868** |
| | (6.477) | (6.599) | (6.822) | (6.937) | (6.978) | (6.780) | (6.732) | (7.000) |
| Persistence of GDP process | 0.981*** | 0.977*** | 0.974*** | 0.971*** | 0.980*** | 0.976*** | 0.973*** | 0.968*** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.003) | (0.004) | (0.005) |
| Observations | 7832 | 7690 | 7402 | 6826 | 8584 | 8426 | 8106 | 7466 |
| Countries in sample | 146 | 146 | 146 | 146 | 162 | 162 | 162 | 162 |

*Note:* This table reports the estimation results of the model specification represented by Equation (1) in the main text. In columns 1–4, the difference relative to Table 1 lies in the exclusion of countries with fewer than one million inhabitants. In columns 5–8, the difference with Table 1 is that we control for OPEC*year fixed effects, i.e. distinguishing year fixed effects for OPEC and non-OPEC countries. The reported coefficients of Autocracy and Democracy are multiplied by 100 to ease their interpretation. Standard errors are clustered at the country level and are reported in brackets. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

we estimate the probability of transitions from and out of hybrid regimes as a function of lagged GDP per capita and year effects (see Table B.7 in the Appendix). These results broadly confirm that periods of economic decline are associated with a higher likelihood of transitions away from hybrid regimes, towards either autocracy or democracy, while higher levels of GDP per capita are associated with a lower probability of transitions to autocracy and a higher probability of transitions to democracy. These findings are consistent with Acemoglu et al. (2019), although our specification distinguishes explicitly between transitions toward autocracy and democracy. These patterns are also informative about the potential role of reverse causality. If lower GDP were to increase the likelihood of transitions into autocracy, one would expect autocratic regimes to be associated with lower levels of GDP. However, this is not borne out in the data. If anything, this suggests that reverse causality, if present, would bias our estimates downward, thereby reinforcing rather than undermining our main result that autocracies display higher average economic performance than hybrid regimes.

These results above help confirm that hybrid regimes are not merely, and not always, a transitory stage. In particular, higher levels of GDP are associated with a lower probability of transitions both toward autocracy and toward full democracy, implying that countries may persist in intermediate regimes. This suggests a higher degree of persistence than commonly assumed.

## 6 | Discussion

The findings we present above do not dispute or contradict the view that democracy is an economically superior choice to autocracy. Yet they prompt us to recognize that the relationship between economic and political development is not monotonic, or in other words, that the best way to visually describe the democracy-income relationship is perhaps not a straight line. According to our results, this relationship seems best described in the data as non-linear. Our point is that we agree with the

economic superiority of democracy, but we think the relationship in the data needs to be better understood.

Our main finding is that the causal relationship from political to economic development is U-shaped. The results above show that "hybrid regimes" lead to significantly inferior economic performance compared to that led by both "democracies" and "autocracies." We estimate the long-run magnitude of this effect to be about 20 percent. We start with specifications favored by the frontier papers in this literature (e.g., Acemoglu et al. 2019) that are heavy on fixed effects and lags and then move to recently developed methods from the Difference-in-Differences (DiD) literature (e.g., de Chaisemartin and D'Haultfœille 2020 and Dube et al. 2025), noting that the results from these different strategies corroborate each other.

All our results taken together suggest that intermediate regimes may perform worse because they are more unstable, for instance, in terms of political and policy instability. But what about autocracies? Why is it that autocracies may perform as well as democracies? The answer we offer may not seem hugely sophisticated but has two well-grounded, and possibly related aspects: (a) democracies tend to last significantly longer than autocracies, and (b) autocracies are more likely to generate growth spurts (i.e., short-duration high-growth episodes), while democracies tend to generate lower but longer-lasting growth.[20]

Regarding the first aspect, there is a long strand of political science dedicated to understanding "authoritarian durability." It is not just that democracies last much longer (in our sample we calculate that autocracies last on average around 18 years, while democracies last almost 25 years on average), but the reasons for autocratic longevity are still imperfectly understood. Lachapelle et al. (2020) examine all authoritarian regimes since 1900 and argue that regimes born out of "violent social revolutions" are those that turn out to be more durable. Systematically eliminating political opposition (which is what violent social revolutions entail) may increase the expected durability of the incoming political regime but it is also likely to generate substantial costs in terms of the quality of future economic policy, among other things.

Regarding autocracies generating more growth spurts than democracies, there is a broad range of evidence in this regard from both economics and political science. Quinn and Woolley (2001) show that democracies produce significantly more stable and less variable growth than autocracies. Hausmann et al. (2005) estimate that the effect of transition to autocracy on growth accelerations is three times larger than the same effect of a transition to democracy (p. 323), a result also supported by Jones and Olken (2008) using a quite different set-up.

One may expect the range of econometric approaches we use to be able to distinguish between these two extremes, but that does not seem to be the case. Recall we use country and year fixed effects which one would normally expect to pick up these spurts. Moreover, we carry out a detailed analysis of outliers that did not yield major reasons for concern. And yet, we find a series of notable examples of autocracies delivering extremely high but relatively short episodes of growth in the experience, for instance, of the Brazilian, Greek and Portuguese "miracles" in the 1960s. At the same time, more recent episodes of democratic erosion and authoritarian consolidation within intermediate regime categories, such as Hungary and Turkey, do not appear clearly inconsistent with the broader empirical pattern documented in this paper. While growth in these countries has remained positive, they do not appear to have systematically outperformed, and may even have underperformed, comparable countries starting from similar levels of GDP per capita and following more democratic institutional trajectories (Appendix D).

In short, we do find that hybrid regimes perform significantly worse than either democracies or autocracies. Yet there remain difficulties separating the latter two regimes econometrically. It may not be possible to do so, and one may need to consider other approaches, perhaps based on event studies or synthetic control methods for comparative case studies, yet at the cost of loss of generality. Although the income-democracy relation is best described as non-linear, we believe there is sufficient detail in our results to suggest that democracies are economically superior because the income growth they generate tends to be less volatile and longer-lasting than the economic performance we observe from autocracies.

## 7 | Conclusions

The objective of this paper is to further our understanding of the relationship between economic and political development, that is, between income and democracy. Economists have so far provided a large number of important insights into this matter. A key and well-established stylized fact in this literature is that the relationship between democracy and income per capita is robust yet unbalanced: democracy does cause economic growth, but the relationship the other way around does not enjoy the same level and depth of empirical support. In this paper, we assess the strength and shape of this relationship and investigate a set of mechanisms or channels that we believe is more in line with those highlighted in the theoretical literature.

Our main hypothesis is that the causal relationship between political and economic development is U-shaped, that is, that the economic performance of hybrid regimes is significantly inferior to that of both "democracies" and "autocracies." To test this hypothesis, we use a new and improved identification strategy and data. Our results do not depend on the choice of measure of political regime. The non-linear relation is found using the Polity measure or using V-DEM measures of political regime.

The main finding is that of broad empirical support for our central conjecture about the economic costs of hybrid regimes. The effect is also quantitatively important: an autocracy achieves about 20 percent higher GDP per capita than an "hybrid regime" in the long run (25 years) while a democracy achieves a similar estimated figure of about 20 percent higher long run GDP per capita. This finding is comparable to the figure the seminal paper by Acemoglu et al. (2019) presents based on a binary indicator of democracy.

Our results are robust to changes in estimator choice, measurement of democracy, outliers and time heterogeneity as shown above. We believe the differences-in-differences estimates, both for the local projections and for the heterogeneous treatment methods, that we present are sufficiently compelling to mitigate concerns related to endogeneity and reverse causality.

In discussing these results, we agree with the main gist of the current economics literature (that democracies are a superior choice in purely economic terms to autocracies) but that the best characterization of this relationship in the data is one of non-linearity instead of a linear relationship as driven by the importance of hybrid regimes.

We offer three main suggestions for future research. First, although we believe our paper provides novel, nuanced and valuable insights into the causal relationship from political to economic development, we leave the task of more detailed identification and quantification of the mechanisms or channels for future research. For example, our results suggest the possibility of policy and political instability as main channels. One advantage of this suggestion is its ample theoretical backing as, for instance, the well-known emphasis in the literature on the threat of revolution (Acemoglu and Robinson 2008). Here we also argue that further differentiating between formal and less formal manifestations of political instability (Campos and Nugent 2003) is a promising avenue for future research. Once finer measures are constructed, one can test the conjecture of whether transitions from autocracy to hybrid are driven by changes in the executive (political instability proper) while transitions from democracy to hybrid are driven by slightly different mechanisms (policy instability), such as either the strategic weakening of the judiciary and of the media. Relatedly, it would also be interesting to investigate whether and how the uncertainty generated by political instability differ from uncertainty from other sources (Ahir et al. 2022). In short, our reading of our main findings points to mechanisms and the heterogeneity of instability types as main topics for future research.

A second suggestion we put forward is in terms of further investigating the underlying components as well as the different dimensions of democracy. On the latter, we show that the results we present in this paper for electoral democracy also obtain for the other dimensions of democracy (in Appendix E we show they obtain for electoral, liberal, participatory, deliberative and egalitarian democracy). Relatedly, it would also be interesting to go beyond aggregate income and study whether and how political regimes differently affect its main components' trends and volatility, that is, consumption, investment, exports, and government expenditures.

A third and final promising avenue for future research is to examine the non-monotonic relationship in light of the current wave of populism and democratic backsliding. According to our classification, such cases fall into the category of intermediate or hybrid regimes, which our results associate with significant adverse political and economic outcomes. While the literature on the economics of populism suggests that political costs are clearer than economic ones, our findings point to the possibility that current populist regimes, all classified as hybrid in our framework, may perform economically worse over the long run than full democracies.


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### Data Availability Statement

The data that support the findings of this study are available on request from the corresponding author. The data are not publicly available due to privacy or ethical restrictions.

### Endnotes

[1] Similar patterns also emerge when relaxing the standard quadratic specification and adopting more flexible functional forms, such as third-order fractional polynomials. The main objective of the paper is to assess whether these descriptive regularities are confirmed by more rigorous econometric analysis.

[2] See Chaisemartin and D'Haultfœuille 2022a, and Roth et al. 2023, for a survey and synthesis of recent advances in the econometrics of difference-in-differences.

[3] Libman (2012) reviews the literature and argues that nonlinearity is more likely to be of a U-shape rather than an inverted-U like in Barro.

[4] Interestingly, when comparing these indices empirically, Paldam (2022) finds that they are dominated by the same latent variable: democratic transition.

[5] These are Argentina (1964-1965); Cyprus (1961–1962; 1968–1973), Estonia (1991), Latvia (1991), Lithuania (1991–1992), Mongolia (1992), and Slovenia (1991); Uganda (1963–1965).

[6] The source for years of independence by country is Besley et al. (2021).

[7] In Section 5 we report results from pooled panel regressions stressing the potential role of slow-moving characteristics. Such approach, which confirms the results reported here, sheds some light on the determinants of the country fixed effects.

[8] More specifically, out of potentially 170 countries we use 162 of them in the regressions because the following eight countries have severe data availability issues especially in terms of per capita GDP: North Korea, Taiwan, Afghanistan, Somalia, Myanmar, Serbia and Montenegro, United Arab Emirates, and São Tomé e Príncipe.

[9] The degree of persistence of the GDP process is obtained as the sum of the coefficients on the lagged dependent variables.

[10] The cumulative long-run effect can be obtained from dividing $\beta$ by $(1 - \sum_{j=1}^{p} \gamma_j)$, where p is the number of lags included on the right-hand side of the model specification. The same applies to transitions to autocracy.

[11] For ease of interpretation, throughout the paper, the reported coefficient on democracy is multiplied by 100.

[12] In Appendix B, we confirm the validity of these findings using the three-way classification of political regimes based solely on the Polity index. In this case, the estimated effects are, if anything, even more statistically significant. The non-monotonic relationship between the level of democracy and GDP per capita is also supported when directly using the Polity or (V-Dem) polyarchy measures, including their quadratic terms. However, when relying on the V-Dem measure, the coefficients lose statistical significance. One possible explanation is that the V-Dem polyarchy index, by construction, captures a broader set of *de facto* aspects of democratic functioning, whereas the "political U-shape" may be more strongly associated with *de jure* institutional features, which are more directly reflected in the Polity IV index.

[13] In line with Acemoglu et al. (2019), long-run effects and the effects over the next 25 years are computed under the assumption that the transition to democracy (or to autocracy) is permanent, compared to countries that permanently remain in an intermediate or hybrid regime. The effects over the next 25 years can be computed iteratively as follows: if $\beta$ is the coefficient on Democracy (i.e., its immediate impact on log GDP per capita) and $\gamma_j$ is the coefficient on the $j$th lag of log GDP per capita, then the effect of permanent democratization on log GDP per capita is: $\beta + \gamma_1 \bullet \beta$ after 1 year; $\beta + \gamma_1 \bullet (\beta + \gamma_1 \bullet \beta) + \gamma_2 \bullet \beta$ after 2 years; and so on recursively until year $t + 25$.

[14] It can be demonstrated that negative weights are likely to occur when there are some countries that are treated most of the time or when there are some years where most of the countries are treated (cf. de Chaisemartin and D'Haultfœille 2020).

[15] These results are available upon request.

[16] Cervellati et al. (2014) argue one should expect important heterogeneous effects related to colonial history and early institutions. Indeed, they find significant but heterogeneous effects of income on democracy, more specifically, they find negative effects from economic to political development (the opposite of what we are studying here) for former colonies and positive for non-colonies.

[17] Data on independence years are drawn from Besley et al. (2021). Results are available upon request.

[18] Results available upon request.

[19] Past or present OPEC members include Algeria, Angola, Congo, Ecuador, Equatorial Guinea, Gabon, Iran, Indonesia, Iraq, Kuwait, Libya, Nigeria, Qatar, Saudi Arabia, United Arab Emirates, Venezuela.

[20] See Alsaadi (2026) for an overview of autocracies' survival explanations and a new hypothesis.

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