Journal of Economic Growth (2023) 28:571–600
https://doi.org/10.1007/s10887-022-09221-5

Democratization, leader education and growth: firm-level evidence from Indonesia

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Accepted: 15 December 2022 / Published online: 15 February 2023

Abstract
Does the economic success of democratization depend on newly elected leaders' characteristics? We exploit the unique Indonesian democratization process, where districts exogenously democratized in different years. In a census of manufacturing plants, employment drops by 5% in districts that elect a non-college educated mayor, while employment stays constant under college graduates. Non-college educated mayors substantially raise taxation but provide less infrastructure, do not spend more on social programs, and are more often involved in corruption cases. Other leader attributes and district characteristics, as well as tests for pre-treatment trends, for selection on unobservables, and for close elections do not explain away the important role of leaders' education in shaping local policies and growth.

Keywords Democratization · Political leader education · Manufacturing · Indonesia

JEL Classification D72 · D78 · H11 · H70 · I25 · L60 · O10

1 Introduction

A large literature has studied the economic consequences of democratization, but we know much less about the conditions that determine whether and to what extent a transition to democracy is successful. The existing body of research has focused on a country's level of economic development as potential driver and finds mixed results (Rodrik & Wacziarg, 2005; Aghion et al., 2008; Acemoglu et al., 2019). Fortunato and Panizza (2015) and Acemoglu et al. (2019) further show that a better educated population increases the success of democratization. An entirely different literature which does not study political system changes finds that national leaders matter for economic growth (Jones & Olken, 2005), and that educated leaders generate higher growth than others (Besley et al., 2011). Given

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this evidence and a widespread belief that leaders matter particularly during challenging economic or political times, it is surprising that no study has asked how the characteristics of *newly elected democratic leaders* shape the economic success of nascent democracies. We identify leader *education* as a crucial determinant of the economic impact of democratization and also pin down underlying channels, thereby addressing a long-standing gap in the political leader literature highlighted by Besley et al. (2011): "The exact mechanism at work in explaining how leadership matters remains opaque. And one unresolved issue is to understand which growth-related policies are affected by leaders." (p. 219).

We focus on Indonesia, which became the world's third largest democracy after the fall of President Suharto in 1998 following more than 30 years of autocratic rule. A unique feature of Indonesia's transition to democracy is that at the sub-national district level, the last mayor that had been appointed by the Suharto regime ("Suharto mayor", henceforth) was allowed to finish his or her five-year term before a new mayor was democratically elected. At the moment of Suharto's resignation the remaining time until the end of term of the Suharto mayor varied by district and is unrelated to district characteristics and trends, as Martinez-Bravo et al. (2017) and additional evidence in this paper show. This implies staggered and exogenous timing of democratization over the period 1999–2003, which we exploit via a difference-in-difference specification at the sub-national level. Thereby we improve identification relative to the existing democracy and growth literature which typically studies data on multiple countries where democratization is a result of country-specific and potentially unobserved characteristics. Leader education might still reflect other leader attributes or local factors, but we show that a wide range of such characteristics, as well as tests for pre-treatment trends, for selection on unobservables, and for close elections do not explain away the important role of leader education in shaping local policies and growth.

We find robust evidence that manufacturing-sector economic outcomes after democratization are worse in districts where the democratic leader does not have a college degree, irrespective of the last autocratic leader's education level. Manufacturing represents 25% of Indonesian GDP and has been targeted as the principal growth engine by the national government[1], similar to many other developing countries. As mechanisms we identify increased taxation, less provision of physical infrastructure and more corruption under non-college educated mayors. While such mayors might simply be elected for having other priorities than supporting local manufacturing, we do not find that they spend more on items such as family welfare, health, housing, environment, religion, or education.

Data on manufacturing come from the annual census of manufacturing plants with 20 or more employees. These panel data allow us to study the impact of democratization on a given plant relative to 'counterfactual plants' in the same four-digit industry, province and year in districts that did not yet democratize, thereby refining identification compared to the existing literature which has focused on aggregate data such as national GDP. We find that in districts where the democratic mayor has no college degree, employment of incumbent manufacturing plants drops by 5% in the first few years after the mayor election. We also show that this effect is not only relative but also absolute, thus reflecting actual lay-offs. When the democratic mayor does have a college degree this negative impact is entirely offset, such that democratization has no effect over our sample period. We find no impact of democratization when we do not condition on mayor education, and observe similar patterns for plant revenue and total factor productivity.

[1] See for example https://www.thejakartapost.com/news/2019/02/11/manufacturing-sector-to-drive-indonesias-economy-bappenas.html.

Once democracy is established, leader education appears to lose relevance: the education level of the *second* democratic mayor does not have a statistically significant impact on local manufacturing, no matter the education level of the first democratic mayor.[2] We also find that the negative employment effects under first democratic mayors without college education equally hold when the last Suharto mayor has no college degree, such that the local mayor education level is unchanged. These additional findings show that leader education matters particularly as a country democratizes, and perhaps more generally during times of political or institutional change, which is a novel result in the literature.

Although the timing of democratization is exogenous, a potential concern is that democratic leader education is endogenous to district characteristics and developments or other leader attributes. In this regard we show that among a comprehensive set of mayor- and district characteristics that might determine the impact of democratization, the only robust driver of democratic mayor education is the education level of the Suharto mayor.[3] Our results are robust to including this and all of our other controls, which rules out for example that low leader education is simply a result of democratization paired with limited education of the local population.[4] We also show that (1) prior to democratization, manufacturing employment exhibits common trends across districts that later elect a college-educated mayor and those that do not; and (2) our results are robust to evaluating the impact of democratization and mayor education relative to a restricted set of control districts that elect a mayor with the same education level later, where unobserved factors are likely more similar. These two findings clearly speak against the presence of confounding developments at the local level. Finally, we apply the method of Oster (2019) and analyze close elections of the second democratic mayors (vote share data for the first democratic mayors are unavailable) to corroborate that the threat of endogenous leader education is very limited in our setting, if at all present.

We identify several channels through which local manufacturing performs worse under non-college educated leaders. First, using plant-level data on annual payments of indirect taxes, fees and levies, we find that the local tax incidence on manufacturing generally increases after democratization, but it increases twice as much under mayors without college education. However, we do not find evidence that total or social welfare expenditure increases, suggesting that not all extra revenue benefits the district. We also show that large plants, exporters and capital-intensive plants experience both a larger rise in the tax incidence and a larger drop in employment under mayors without a college degree. The taxation channel thus provides one reason why on average we do not find positive effects of democratization, contrary to recent cross-country level evidence (Acemoglu et al., 2019). In Sect. 2 we discuss the roots of this detrimental channel, which may be partly attributed to a specific design of democratization that has been adopted in other countries as well.

Second, we use longitudinal survey data to highlight another mechanism: after democratization the local business community perceives a significant deterioration of both availability and quality of local physical infrastructure, and the effect is driven by districts with non-college educated mayors. Combined with our taxation results, these findings suggest

[2] This is consistent with Carnes and Lupu (2016) who present evidence that more educated leaders do not perform better than others, using different samples containing mostly established democracies.

[3] In 10% of districts in our sample the last Suharto mayor is elected as first democratic mayor, but the described correlation as well as our findings are robust to excluding these districts.

[4] The inclusion of leader controls also reveals that democratic mayor age, gender, and previous occupation do not affect the success of democratization, but mayors that were born in the district that they lead do produce better outcomes.

that after democratization less educated leaders enact worse policies at a higher cost to local manufacturing.

Third, we identify corruption as a mechanism: a novel mayor-level dataset that we hand-collect reveals a negative and statistically significant correlation between a democratic mayor's education level and involvement in official corruption cases. All documented mechanisms are highly relevant because taxation, infrastructure and corruption are often cited among the most serious business constraints in developing and emerging economies, including Indonesia. The mechanisms are also internally consistent: more corruption can explain how a larger increase in taxation is accompanied by less infrastructure provision and no additional expenditure on other items. In the context of democratization, our results on mechanisms are not only in line with the finding of Keefer (2007) that young democracies engage in excessive rent seeking, under-provide public goods and are relatively corrupt, but they also highlight (insufficient) education as a driver of these issues.

Finally, a question that follows from both our results and those of related studies is through which underlying channels education affects leader behaviour. Based on evidence from different strands of the broader education literature, we discuss that a heavier tax burden, the neglect of infrastructure, and corruption by less educated mayors likely reflect a weaker understanding of the underlying costs and thus less competence. Moreover, these issues may be explained by different beliefs and values or other factors such as a more myopic attitude of less educated leaders towards their career.

1.1 Related Literature

We build on a large body of work that analyzes the impact of democracy on growth and finds mixed results overall (Helliwell, 1994; Barro, 1996; Tavares & Wacziarg, 2001; Rodrik & Wacziarg, 2005; Persson & Tabellini, 2006; Papaioannou & Siourounis, 2008; Doucouliagos & Ulubaşoğlu, 2008; Bates et al., 2012; Murtin & Wacziarg, 2014; Madsen et al., 2015; Acemoglu et al., 2019). We contribute to this cross-country literature by improving identification via our subnational approach that exploits random timing of democratization and by showing that the economic success of a nascent democracy depends on the education level of the newly elected leader.[5] By studying the transition from the last Suharto mayor to the *first* democratic mayor, we also contribute to a scarce literature on the immediate and short-run effects of democratization (Rodrik & Wacziarg, 2005; Acemoglu et al., 2019).

Our paper also adds to a small quantitative literature analyzing the Indonesian democratization process. Martinez-Bravo et al. (2017) show that the longer the Suharto mayor stays in power, the worse are governance outcomes after democratization, which is attributed to elite capture. Hallward-Driemeier et al. (2021) find that the disruption of political connections to Suharto due to democratization leads to more competition in manufacturing sectors disproportionately exposed to cronyism.[6] While these papers also exploit the staggered

[5] In terms of identification our study relates to Fujiwara (2015) and Burgess et al. (2015), who also exploit within-country variation to analyze the impact of various aspects of democracy on different outcomes.

[6] Moreover, Martinez-Bravo (2014) finds that the body of local officials that a district inherits from the Suharto regime determines the extent of fraud and clientelistic spending in the first democratic election. Without conditioning on mayor education, Abeberese et al. (2021) find that total factor productivity (TFP) increases after democratization. They measure TFP using the Levinsohn and Petrin (2003) method, while we estimate it using the more recent method by De Loecker and Warzynski (2012).

nature of the democratic transition, they do not shed light on the fundamental role of leader characteristics in shaping the outcomes of democratization.

Beyond democratization, our paper relates to a literature studying the effect of political leader education on diverse (socio-)economic outcomes (Dreher et al., 2009; Besley et al., 2011; Carnes & Lupu, 2016; Martinez-Bravo, 2017; He & Wang, 2017; Pertuze et al., 2019; Brown, 2020; Lahoti & Sahoo, 2020; François et al., 2020). Our contribution is to both study growth effects of leader education *and* identify underlying mechanisms, and to highlight the importance of leader education during a political transition. More broadly, our paper relates to a literature on leaders and growth which does not focus on leader education (Jones & Olken, 2005; Yao & Zhang, 2015; Easterly & Pennings, 2020) and to studies analyzing the effect of CEO education (Chevalier & Ellison, 1999; Bertrand & Schoar, 2003; Beber & Fabbri, 2012; Miller et al., 2015; King et al., 2016). Finally, we add to a body of work highlighting the importance of political leader characteristics other than education, such as gender (Chattopadhyay & Duflo, 2004; Clots-Figueras, 2011, 2012; Brollo & Troiano, 2016), nativeness (Hodler & Raschky, 2014), age (Yao & Zhang, 2015; Alesina et al., 2019), previous occupation (Dreher et al., 2009; Beach & Jones, 2016; Neumeier, 2018), prior experience in office (Freier & Thomasius, 2016), and heroic credentials (Cagé et al., 2021).

The remainder of the paper is structured as follows. Section 2 discusses the context of democratization in Indonesia, Sect. 3 data and key variables and Sect. 4 our empirical strategy. Section 5 presents our results and Sect. 6 concludes.

2 Background

President Suharto's regime lasted from 1965 to 1998 and was characterized by tight control of Indonesian citizens and opposition parties. Following the Asian financial crisis and the disclosure of several corruption cases, Suharto was forced to step down on 21 May 1998 amid nationwide protests. A transitional government led by Suharto's vice president Habibie assumed power and set the scene for the first free democratic elections since 1955, held on 7 June 1999. The main opposition party PDI-P clearly won these elections, followed by Suharto's party Golkar which continued to represent the autocratic style of his regime and served as a pool for former members of the military and the bureaucracy (Hadiz, 2010). Besides the national parliament and president also provincial and district parliaments were elected, and the new district parliament (DPRD) was responsible for electing a new district mayor.[7] However, this (indirect) democratic mayor election only took place once the last mayor that had been appointed by the Suharto regime finished his or her five-year term.[8] This creates variation in the timing of first democratic mayor elections: in districts where the last Suharto mayor was appointed in the second half of 1994, the local parliament could elect the mayor within months after the legislative election of 1999, while in other districts the Suharto mayor could stay in office until as late as the beginning of 2003. Starting from 2005, mayors were directly elected by the district

[7] Indonesia counted 297 districts at the end of 1997: 57 cities ("kota"), 235 rural districts ("kabupaten") and five districts comprising the capital city of Jakarta. While data on the Jakarta districts are missing, we exclude these districts from our analysis anyway since they form one city and are thus less distinct than the other districts.

[8] The new mayor needed the support of at least 50% of DPRD members to be appointed (jointly with "his" or "her" vice-mayor). The appointment occurred directly after the election.

population once the five-year term of the incumbent had expired. Both before and after the fall of Suharto, mayors have been entitled to serve a maximum of two terms. Some Suharto mayors could therefore be elected as first democratic mayor, which happened in nine districts in our sample.

The mayor position entails a considerable amount of authority, in particular over local policies, regulations and the district budget (Martinez-Bravo et al., 2017). Although Law 22/1999 grants the local parliament the right to disapprove the district budget and regulations proposed by the mayor, and to reject the mayor's annual accountability speech, this has not occurred frequently in practice (Hofman & Kaiser, 2006). In line, Von Luebke (2009) finds that mayors rather than citizen groups or local parliaments tend to initiate policy.[9] Mayors have thus been the main driver of local governance outcomes after the fall of Suharto. For these reasons, we adopt the notion of Martinez-Bravo et al. (2017) that democratization at the local level was triggered by the mayor election rather than the 1999 legislative elections.

The process of democratization was accompanied by decentralization, which Indonesia implemented nationwide on 1 January 2001. The country thereby pursued a similar strategy as several other developing nations across Latin America, South Asia and sub-Saharan Africa, particularly during the "third wave" of democratization after the 1980s. The motivation is that decentralization has the potential to promote democracy, participation, and empowerment at the local level (Kulipossa, 2004). The Indonesian decentralization laws transferred a substantial amount of power from the central government to the districts, largely bypassing the provincial level (see e.g. Jones, 2004).[10] This empowered local parliaments but also strengthened the mayor position, for example in the field of public goods provision (Hofman & Kaiser, 2006).

In post-decentralization Indonesia, the principal source of revenue for districts are non-earmarked transfers from the central government. The largest transfer ("DAU" = General Allocation Fund) is allocated based on local population, area, poverty rate, and other factors, and is set at 25% of central government domestic revenue in total (Brodjonegoro, 2004). The larger scope of action for mayors and the discretion over the use of transfers implies that decentralization is a key ingredient in creating a link between democratic mayor characteristics and the local success of democratization. We therefore design our empirical strategy so that our coefficients capture the impact of local democratization conditional on decentralization being in place. Our approach also isolates the impact of democratization from direct effects of the implementation of decentralization in 2001 (see Sect. 4).

While allowing discretionary use, the predominance of central government transfers as source of revenue also reflects that the fiscal decentralization law 25/1999 "continues the reluctance to give local governments any meaningful ability to raise local revenue" (Brodjonegoro, 2004, p. 129). Indeed, the official locally derived revenue ("PAD") made up less than 10% of the local budget for 87% of districts in 2002 (Brodjonegoro, 2004). Many district governments have expressed their dissatisfaction about too low funding to promote regional development, especially in relation to new infrastructure provision (Brodjonegoro, 2009). Led by the powerful democratic mayor, local governments have frequently used this

[9] Since the business survey underlying Von Luebke (2009)'s results was conducted between April 2005 and March 2006, it is reasonable to assume that the responses mostly refer to the first democratic mayor.

[10] The central government retained control over defence and security, justice, international relations, monetary and fiscal policy and religion. Decentralization was implemented on the basis of Law 22/1999 which focused on administrative aspects and Law 25/1999 which focused on fiscal aspects.

perceived lack of funding to justify the introduction of new local taxes and levies, which have been described as "illegal and disruptive" (Brodjonegoro, 2009, p. 207) and "distorting" (Ray, 2009, p. 151).[11] This matters particularly for manufacturing since "the easiest targets for these new additional revenues are unfortunately the local businesses that seem to be powerless against this challenge" (Brodjonegoro, 2009, p. 207). Similarly, Hofman et al. (2009) highlight the "high relative importance of political factors" for the local business climate and point out that "manufacturing in particular is prone to illegal levies, either by government officials or the surrounding community" (p. 110). "Illegal exactions" are in turn the most commonly cited factor that negatively affects the local business climate in a 2002 survey of companies (see Ray, 2009, p. 164).[12] The business community has further listed policy uncertainty, "demands by inexperienced local governments empowered by decentralization", and corruption as serious constraints (Dhume, 2004, p. 66).[13]

Our findings confirm the view of the above-discussed literature that the new and illegal exactions had a detrimental impact, rather than help stimulate local development: while manufacturing plant-level payments on indirect taxes, fees and levies rise with democratization, we observe no increase in total development expenditure or relevant sub-categories at the district level. This suggests that at least parts of the extra tax revenue served to increase mayors' personal rent. This interpretation is consistent with the analysis of Lewis (2003), which "offers no support for the contention that regional governments create new taxes and charges because they lack fiscal capacity." (p. 187; see also Hadiz, 2010). Most importantly, we contribute to this discussion by highlighting that a key local driver of excessive taxation, corruption, and insufficient focus on infrastructure is the education level of the first democratic mayor. This link echoes in a 2003 statement of Indonesia's minister for Administrative and Bureaucratic Reform, where he argues that the majority of civil servants are "under-educated" and less than half "know what they are doing and do their jobs properly" (Webber, 2006, p. 408).

3 Data

3.1 Main variables and data sources

Our key data ingredients are information on the district-specific timing of the first democratic mayor election, mayor education level data, and plant-level manufacturing data. Table 1 reports descriptive statistics.

We obtain information on election timing and mayor education from the data repository of Monica Martinez-Bravo and Andreas Stegmann (Martinez-Bravo & Stegmann, 2018).

[11] Law 25/1999 explicitly allows the introduction of new local taxes and levies to contribute to districts' own revenue. However, districts only submitted less than half of them for review as required by law (making the rest illegal), and the central government has been accused of being too lax in passing those taxes and levies that were submitted (Lewis, 2003).

[12] 30% of respondents indicated illegal exactions as a factor, followed by non-tariff barriers/constraints (24%), infrastructure constraints (21%), formal taxes and charges (13%), and lack of security (12%).

[13] With the distribution of power to the district level, corruption has become more decentralized (Basri & (2004). Economic update,, 2003), which typically leads to a larger group of people who have to be bribed and thus a higher total bribe payment per transaction compared to a centralised system (Bardhan, 1997). The National Survey of Corruption 2001 found that 87% of firms regarded corruption in the public sector as 'common'. 41% of respondents stated that they frequently or always pay bribes in the course of business (Khouw, 2004).

The source distinguishes the education categories 'Less than Bachelor degree', 'Bachelor degree', 'Master degree' and 'PhD degree'. We compute a dummy variable *College Degree* which equals one if the democratic mayor holds at least a bachelor degree and zero otherwise. Law 22/1999 requires mayors to have completed junior high school (*Sekolah Menengah Pertama*), which implies that all democratic mayors in our sample have at least nine years of schooling. The first democratic mayor has a college degree in 79% of districts in our final sample, while the last Suharto mayor has a college degree in 63% of districts.[14] These numbers are consistent with the result of Besley and Reynal-Querol (2011) that at the aggregate (country) level, democratization leads to an increase in leader education levels. We also use the data from Martinez-Bravo and Stegmann (2018) to control for democratic mayors' age, gender, birth district, a dummy indicating prior work in the private sector, political party affiliation, and the education level of the last Suharto mayor, and exploit data on the field of study of college-educated democratic mayors. Selected data points on some variables are missing, but we are mostly able to fill the gaps through other sources.[15] We do not have information on vote shares in the first democratic mayor elections, but we use such data for the second democratic mayor elections in a robustness check (see Table OA11 in the Online Appendix (OA)).

The annual census of manufacturing plants (IBS) is collected and compiled by the Statistical Agency of Indonesia (*Badan Pusat Statistik* (BPS)) and has produced a panel of manufacturing plants that employ at least 20 employees in the particular year. We use mainly employment but also revenue, total factor productivity, and investment to measure performance. For our analysis of mechanisms, we employ data on plants' reported payments of indirect taxes, fees and levies, and a proxy for bribe payments. We further use plants' district location and sector information, which we translate into the ISIC Rev. 3.1 classification.[16]

To analyze additional mechanisms we use data from the Regional Autonomy Watch *KPPOD*, which has conducted annual surveys in slightly varying sub-samples of districts across Indonesia from 2001 onwards. This effort has generated district-level data on the availability and quality of local physical infrastructure such as streets or telephone service, and on local institutional quality such as the consistency of regulations or law enforcement, as perceived by the local business community.[17] Data on institutions are collected through surveying local business actors and consulting a panel of experts, while for infrastructure *KPPOD* complements these sources with actual availability and quality data collected

[14] The fraction of districts that elect a college graduate as first democratic mayor does not systematically rise or fall over time during 1999–2003: among districts that democratize in 1999 (and are included in our baseline sample, see below), 100% elect a college-educated mayor, while the ratios for 2000, 2001, 2002 and 2003 equal 78%, 82%, 86% and 72%, respectively.

[15] See Section OA4 in the Online Appendix for details on data sources and the construction of variables.

[16] For around 4% of plants that operate during 1998–2004, the census records two or more districts as location over this time period. We cannot be sure if this reflects real events or measurement error. The reason is that districts split and proliferated over time and district codes were sometimes reused and reassigned, and while we track these changes, some errors may remain. We drop these multi-district plants from our sample to address the mentioned measurement concerns, the potential worry that certain plants self-select into districts that democratize early, and to ensure that plant fixed effects absorb district fixed effects in our empirical specification.

[17] Several studies have highlighted that *perceptions* on the state of a variable may not fully reflect the *actual state* of the variable, for example Olken (2009) in the context of corruption. This is arguably less of an issue in our setting since we account for time-invariant factors at the district (or a more general) level and time-varying factors at the provincial (or a more general) level that draw a wedge between the perceived and actual state of infrastructure or institutions.

Table 1 Summary statistics

	Mean	Median	Min	Max	Sdev	N
Panel I: Plant-year-level variables						
# Employees	156.677	38	20	15,836	463.379	29,994
ln(# Employees)	4.061	3.638	2.996	9.670	1.137	29,994
ln(Revenue)	14.210	13.850	7.601	22.844	2.039	29,994
ln(1+Investment)	6.161	8.007	0	23.660	6.229	26,046
ln(TFP)	2.246	2.252	1.281	2.974	0.120	22,865
ln(Wage bill / # Employees)	8.170	8.254	0.573	15.749	0.935	29,993
Indirect tax payments / Value added	0.030	0.005	0.000	0.565	0.081	23,873
Gifts, donations etc. / Value added	0.009	0.004	0.000	0.097	0.015	21,702
Panel II: Mayor- and district-level variables						
Democratic mayor has college degree	0.792	1	0	1	0.408	96
Democratic mayor is female	0.052	0	0	1	0.223	96
Democratic mayor is born in district	0.554	1	0	1	0.500	83
Democratic mayor age at time of election	48.901	50	26	61	6.717	81
Democratic mayor works in private sector pre-election	0.247	0	0	1	0.434	81
Democratic mayor is member of Golkar	0.444	0	0	1	0.500	72
Suharto mayor has college degree	0.625	1	0	1	0.487	96
ln(GDP per capita 2000)	1.378	1.322	0.481	3.177	0.547	96
Education of working age population 2000	1.058	0.974	0.469	1.638	0.271	96
Population 2000	715,835	661,510	47,970	2,780,820	533,884	96
ln(Population 2000)	13.185	13.402	10.778	14.838	0.824	96
Population density (=population per square mile) 2000	3,925	1,767	14.387	32,400	6,259	96
ln(Population density 2000)	7.482	7.476	2.666	10.386	1.305	96
Religious fractionalization 2000 (HHI)	0.891	0.961	0.439	0.998	0.138	96
City	0.260	0	0	1	0.441	96
Golkar wins 1999-elections	0.250	0	0	1	0.435	96
1999-election vote share HHI	0.305	0.268	0.162	0.764	0.130	94
Panel III: District-year-level variables						
Post Election Year	0.633	1	0	1	0.482	480
Election Year	0.190	0	0	1	0.392	480
ln(Infrastructure)	5.651	5.677	4.745	6.201	0.322	129
ln(Institutional quality)	6.271	6.280	5.333	7.069	0.352	129
Panel IV: Democratic mayor corruption case data						
At least research, no matter if acquitted later (full sample)	0.511	1	0	1	0.503	92
At least research, no matter if acquitted later (data on all controls available)	0.493	0	0	1	0.504	71
At least research and not acquitted later	0.451	0	0	1	0.501	71
At least investigation and not acquitted later	0.408	0	0	1	0.495	71
At least declared defendant and not acquitted later	0.408	0	0	1	0.495	71
Convicted	0.282	0	0	1	0.453	71

Table 1 (continued)

This table provides summary statistics on the variables used in our analysis. Values larger than 1000 are rounded to the nearest integer. *Education of working age population* is the district average across the entire population with age 15–65 and takes one of the following values at the individual level: 0 = less than primary education completed, 1 = primary education completed, 2 = secondary education completed, 3 = college degree obtained. The variables *Indirect tax payments/Value added* and *Gifts, donations, etc. / Value added* are winsorized from above at the 1% level. For illustrative purposes, the raw scores of *Infrastructure* and *Institutional quality* are multiplied by 10,000 before taking the log such that all numbers are larger one and the log is thus non-negative. See Section OA4 in the Online Appendix for a detailed description of variables and data sources. The sample underlying the variables in Panel II and the first two variables in Panel III is the set of 96 districts that are included in our baseline sample (see Table 2); for some variables, the sample is smaller due to data availability. For the remainder of Panel III, the sample corresponds to the one in columns 3–6 of Table 4; for Panel IV, the samples correspond to the ones in Table 5

by the BPS. Data for the period 2002–2004 constitute a panel, which we exploit in our analysis.

We also hand-collect a novel dataset on mayor-level corruption cases using data from the *Corruption Eradication Commission* (KPK), the watchdog *Indonesia Corruption Watch* (ICW), and additional sources. For each democratic mayor in our sample, this dataset informs us whether the mayor was involved in an official corruption case (which is true for 50% of mayors) and which stage the case has reached (research, investigations, taken to court, convicted, or acquitted). We detail this dataset and the institutional background in Section OA4.3.

Finally, we collect data on additional district-level variables from different sources, specifically GDP per capita, population, population density, education of the working age population, 1999 election outcomes, religious fractionalization, city versus rural district status, and public expenditure items (see Section OA4).

3.2 Sample of districts, plants and years

We choose the time interval from 2000 to 2004 as our sample period. Thereby we analyze the transition from the last Suharto mayor to the first democratic mayor conditional on Suharto and the transitional government being out of power, and thus against the background of a constant national political setting. Starting in 2000 also ensures that 1999 election outcomes are predetermined controls rather than outcomes or endogenous variables. Since we focus on the *first* democratic mayor, we drop the year 2004 for districts where the second democratic mayor is elected in 2004.

The starting point of our district selection process is the set of 297 districts that existed at the end of 1997, and thus shortly before the fall of Suharto. First, we drop the five districts comprising the capital city of Jakarta, due to missing data and their different nature (see footnote 7). Following Martinez-Bravo et al. (2017) we then drop remaining districts that may endanger our identification strategy or conceptually do not allow to estimate our effect of interest, which is the impact of the direct transition from the Suharto mayor to the democratic mayor. Both issues apply to districts that split between the fall of Suharto

and 2004; we therefore exclude the 87 districts that were involved in a district split (either as "parent" or "child") over 1998–2004.[18] In 65 other districts, the Suharto mayor's term expired between the fall of Suharto in May 1998 and the local legislative elections in June 1999, which implies that the Suharto mayor's successor was selected by the transitional government. Since we can only speculate about the nature of these appointments, we exclude these districts from our sample. We further drop eight districts for which we do not know whether the mayor is selected by the transitional government or the 1999-elected local parliament. In 19 of the remaining districts, an interim mayor was installed to serve for a period of up to around one year between the last Suharto mayor and the first democratic mayor. Since the underlying reasons are unclear but appear district-specific and may represent confounding factors, we drop these districts as well. Based on the same reasoning, we exclude five districts in which the last Suharto mayor stepped down before the end of his or her five-year term and another four districts where the first democratic mayor stepped down prematurely within our sample period. Missing data on one district brings us to a set of 103 districts, of which 26 are cities and 77 are rural districts. Since two of these 103 districts do not have medium- or large-scale manufacturing over 2000–2004, and due to data availability and the chosen fixed effect structure in our specifications, our regressions include at most 96 districts.[19]

4 Empirical strategy

We set up a difference-in-difference (DiD) specification with staggered treatment across space, exploiting that local mayor elections occurred in different years across Indonesian districts. Specifically, our empirical model is the following:

$$\begin{aligned} ln(Y_{ijkpt}) = {} & \beta_1 PostElec_{kt} + \beta_2 [PostElec_{kt} \times CollegeDegree_k] \\ & + \beta_3 ElecYear_{kt} + \beta_4 [PostElec_{kt} \times X_k] \\ & + \mu_i + \omega_{jt} + \delta_{pt} + \epsilon_{ijkpt} \end{aligned} \tag{1}$$

where Y_{ijkpt} is outcome variable Y of manufacturing plant i in four-digit ISIC Rev. 3.1 industry j in district k in province p at time t. $PostElec_{kt}$ equals one in the years after the democratic mayor election and zero otherwise; and $ElecYear_{kt}$ equals one in the mayor election year and zero otherwise, and is mainly included to clearly separate the pre- and post-election period, given that manufacturing plant data are annual while elections happen throughout the year. $CollegeDegree_k$ is a dummy that takes one if the democratic mayor in district k has a college degree and zero otherwise. X_k is a vector of mayor- and district-level control variables that are measured at the beginning of our sample period if they vary over

[18] Once a "child" district splits off, an interim executive is selected who oversees the transition process until the election of a new mayor by a newly established local parliament (see Fitrani et al. (2005) for further details on the sequence of political events in a newly established district). More importantly, the election of a new mayor by the local parliament usually occurs soon after the split rather than only at the end of the five-year term of the Suharto mayor in the "parent" district. This implies that any impact we would attribute to democratization for these districts may actually reflect the effect of the district split itself, or the factors that caused the split (see Pierskalla (2016) and Bazzi and Gudgeon (2021) for an analysis of factors that determine the likelihood of a district to split).

[19] In Table OA8 we show that our results are largely representative for the entire population of 1997-districts. Note that in regressions that do not use manufacturing plant-level data, for consistency we drop districts that do not feature in any of our baseline manufacturing regressions (see Tables 2, 3 and 4).

time and are described further below. μ_i are plant fixed effects, which also nest district fixed effects since we drop plants for which the census records two or more districts as location over our sample period.[20] These fixed effects control for (1) unobserved and time-invariant factors that influence the education level of the first democratic mayor and local manufacturing characteristics; and (2) any difference in manufacturing characteristics across the groups of districts that differ in terms of the democratization year.[21] ω_{jt} are four-digit industry-times-year fixed effects and δ_{pt} are province-times-year fixed effects. These fixed effects control for example for the fact that Indonesia decentralizes in 2001 and the possibility that decentralization has a different impact across industries or provinces in Indonesia. We cluster standard errors at the district level.

β_1 captures the effect of the democratic election of a mayor without college education, while β_2 captures the differential impact of democratization when the newly elected mayor does have a college degree. Given our fixed effects structure, the effects captured by β_1 and β_2 are relative to plants in the same four-digit industry, province and year. In the case of β_1 these 'counterfactual plants' are located in districts that did not yet democratize, while for β_2 they are located in democratized districts in which the democratic mayor has no college degree. Such that β_1 and β_2 indicate effects conditional on decentralization being in place rather than (weighted) average effects across the pre- and post-decentralization period, we drop the year 2000 for the five districts where the mayor election occurred in 1999.

There are three identifying assumptions that must hold such that β_1 and β_2 are unbiased estimators of the described effects. The first is that the timing of the democratic mayor election is as good as randomly assigned across the districts in our sample. Athey and Imbens (2022) show that given random treatment timing in a staggered DiD setting, the standard DiD estimator is an unbiased estimator of a weighted average causal effect. Under the additional assumption of no anticipation effects—which we show to be valid in Table OA10—this average effect is conceptually meaningful, as all individual effects involve switching from not being treated to being treated. The random timing assumption is plausible for several reasons. In all districts in our sample, the timing of the first democratic mayor election is determined by the term end of the last Suharto mayor. This term end is a function of the timing of previous mayor terms, which in turn is determined by different accumulations of early term ends since the latter part of the Dutch colonial period, be it for health or other reasons. Based on this setting, Martinez-Bravo et al. (2017) conclude that the appointment timing of the last Suharto mayors—which determines the election timing of the first democratic mayors in our sample – is plausibly as good as randomly assigned. As supporting evidence, the authors show that the appointment timing of the last Suharto mayor is uncorrelated with the level of a wide range of district-level variables (see their Appendix-B Table 3). We complement these findings by showing that there is no correlation between the election year of the first democratic mayor and the level and growth rate of manufacturing outcomes at the district level prior to Suharto's fall (see Table OA8). Furthermore, we corroborate the validity of the first identification assumption by showing that prior to democratization, manufacturing employment exhibited parallel trends across districts with different

[20] Controlling for district fixed effects implies that we do not need to include $CollegeDegree_k$ and X_k as separate, non-interacted terms.

[21] The plant and nested district fixed effects do not control for any differential impact of democratization depending on the local presence of such time-invariant or other, time-varying factors. This motivates the inclusion of $PostElec_{kt} \times X_k$ into our specification.

democratic mayor election years (see Table OA9). Finally, our results are robust to applying the estimator of De Chaisemartin and d'Haultfœuille (2020), which is preferred if there are both heterogeneous treatment effects and the timing of the democratic mayor election is not as good as randomly assigned (see Table OA10).

The second identification assumption is that conditional on our controls, democratic mayor education is exogenous to time-varying factors that impact local manufacturing. If democratic mayor education is solely determined by the composition of the local parliament elected in 1999, then this assumption is valid because the election results are a time-invariant factor captured by district fixed effects. If there are unobserved variables that affect mayor education even conditional on the 1999 election results, then these may be at least partly captured by the included province-times-year fixed effects and/or industry-times-year fixed effects. More importantly, we show that prior to democratization, manufacturing employment exhibits parallel trends across districts that later elect a college graduate as first democratic mayor and those that do not (see Table OA9)—which provides direct empirical support for the assumption's validity. Event study graphs (see Figure OA1) complement these results by illustrating that both in districts that democratize under a college-educated mayor and those that do not, there are no significant trends in employment prior to democratization. We also show that the estimated impact of the *second* democratic mayors' education level is no different when we focus on close elections (as discussed, vote share data for the first mayor elections are unavailable), which provides indirect support for the unbiasedness of our main results (see Table OA11). Our results are further robust to evaluating the impact of democratization and mayor education relative to a restricted set of control districts that elect a mayor with the same education level later, where unobserved factors are likely more similar (see Table OA10). Finally, we apply the recent method of Oster (2019), which evaluates robustness to omitted variable bias by analyzing the relative movement of the treatment coefficient and R-squared upon the inclusion of controls, and obtain reassuring results (see Section OA2). All these findings underpin the validity of the second identification assumption.

The third identification assumption is that conditional on the controls in vector X_k, democratic mayor education is exogenous to (time-varying or fixed) variables that determine the impact of democratization on local manufacturing. We therefore include an extensive set of variables into X_k, which are motivated by the existing literature and the Indonesian context. Democratic mayor-level controls are gender, age, and dummies indicating whether the mayor (1) works in the private sector pre-election; (2) is born in the district; and (3) is member of the Golkar party, respectively. We also control for whether the last Suharto mayor has a college degree. District-level controls are GDP per capita, average education level of the local working age population, population, population density, religious fractionalization, a city dummy, political competition in the local 1999-parliament (measured via a Herfindahl-Hirschman index using 1999 election vote shares), and a dummy indicating whether Golkar wins the 1999 elections. To avoid simultaneity and to make sure that these controls are predetermined (see "bad control problem", Angrist and Pischke, 2008), we measure time-varying variables at the beginning of our sample period. Table OA12 shows that among the mentioned controls, only Suharto mayor education significantly and consistently correlates with democratic mayor education across different specifications. The vector X_k therefore includes only this variable in our baseline specification, while in robustness checks we add a separate interaction with all controls (see Tables OA13–OA15). Given our rich set of controls, the result that most of them do not correlate with democratic mayor education and do

not affect the local success of democratization, and the discussed evidence based on vote share data and the method of Oster (2019), we are confident that the third identification assumption holds as well.

5 Results

5.1 Democratization, mayor education and manufacturing outcomes

To analyze real effects of democratization and democratic mayor education in the manufacturing sector, we estimate Eq. (1) for the number of employees, revenue, total factor productivity, investment, and the wage bill divided by the number of employees as dependent variables. Our main focus is on employment, which we analyze in Table 2. In column 1 we estimate Eq. (1) without the interaction terms, and find that the average impact of democratization on manufacturing employment is not significantly different from zero. However, column 2 shows very different results depending on the education level of the newly elected mayor. The marginal effects at the bottom of column 2 show that employment is unaffected in districts with college-educated mayors, while employment significantly *drops* by around 5% after the election of mayors that do not have a college degree.[22] The results are highly robust to controlling for any potential effects of Suharto mayor education after democratization (column 3).[23] In column 4 we test whether the effect of democratic mayor education *depends* on the Suharto mayor's education level.[24] The results show that this is not the case: the election of a college-educated mayor has no employment effects both when the Suharto mayor has a college degree and when he or she does not (see the bottom two marginal effects in column 4), and the negative effect of electing a non-college graduate is large (see first marginal effect) and not significantly different (see third coefficient) when the last Suharto mayor is also not college-educated. This shows that our main results do not merely reflect the effect of a change in leader education irrespective of democratization. We explore this finding further by analyzing the effect of the *second* democratic mayor's education level on manufacturing employment. We do so over the period 2004-2009, thus after all districts elected their first democratic mayor and before districts elected their third democratic mayor. The results are reported in Table OA6 and show no statistically significant change in manufacturing employment as a non-college graduate (or a college graduate) is elected as second democratic mayor, irrespective of the first democratic mayor's education level. Since in 39 out of 76 districts in this sample the first democratic

[22] In Table OA16 we show that the effect does not depend on whether the democratic mayor has only an undergraduate college degree or also a graduate degree.

[23] To enable a comparison of the coefficient on *Post Election Year* across columns 2 and 3, we demean the dummy variable *Suharto mayor has college degree* in column 3 based on the column-specific sample. We do the same whenever we include *Post* × *Suharto mayor has college degree* in Tables 3 and 4 to enable an unconditional interpretation of the coefficient on *Post Election Year*. Without demeaning *Suharto mayor has college degree*, for example in column 3 of Table 2 the coefficient on *Post Election Year* would indicate the effect of democratization under a democratic mayor without college education when the last Suharto mayor does not have a college degree.

[24] In this column we include *Suharto mayor has college degree* without first demeaning the variable, thus the top row coefficient indicates the effect of democratization when neither the democratic nor the last Suharto mayor have a college degree. The marginal effects on all other mayor education combinations are listed in the bottom of column 4.

Table 2 Democratization, mayor education and manufacturing employment

Dependent variable →	ln(# Employees)			
	(1)	(2)	(3)	(4)
Post Election Year	− 0.013	− 0.052***	− 0.052***	− 0.042***
	(0.010)	(0.013)	(0.013)	(0.015)
Post × Democratic mayor has college degree		0.048***	0.048***	0.038**
		(0.013)	(0.013)	(0.015)
Post × Suharto mayor has college degree			− 0.002	− 0.029
			(0.013)	(0.019)
Post × Suharto mayor has c-degr. × Dem. mayor has c-degr.				0.032
				(0.023)
Election Year	− 0.009	− 0.011	− 0.011	− 0.010
	(0.012)	(0.012)	(0.011)	(0.012)
Plant FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes
Sample Period	00-04	00-04	00-04	00-04
Observations	29,994	29,994	29,994	29,994
#Districts	96	96	96	96
#Plants	6914	6914	6914	6914
Marginal effects				
Democratic mayor has no college degree		− 0.052***	− 0.052***	
		(0.013)	(0.013)	
Democratic mayor has college degree		− 0.005	− 0.004	
		(0.011)	(0.012)	
Dem. mayor has no c-degree and Suharto mayor has none				− 0.042***
				(0.015)
Dem. mayor has no c-degree and Suharto mayor has one				− 0.071***
				(0.017)
Dem. mayor has c-degree and Suharto mayor has none				− 0.004
				(0.015)
Dem. mayor has c-degree and Suharto mayor has one				− 0.002
				(0.012)

In this table we study the impact of local democratization and democratic mayor education on manufacturing plants with at least 20 employees. See Sect. 3 for a description of our sample selection. The dependent variable is the log number of employees at the plant level. *Post Election Year* takes one in the years after the democratic mayor election and zero otherwise; the remaining variables are self-explanatory dummy variables. At the bottom of the table we display marginal effects. The first marginal effect in column 2 is equal to the coefficient in the top row, the second equals the sum of the coefficients in the first two rows. In column 3 we demean *Suharto mayor has college degree* based on the column-specific sample to enable a comparison of the coefficient on *Post Election Year* across columns 2 and 3. The marginal effects in column 4 (in which we do not demean any variable) equal the sum of the relevant coefficients. Standard errors in parentheses are clustered at the district level. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level

Table 3 Democratization, mayor education and other manufacturing outcomes

Dependent Variable →	ln(Revenue)		ln(TFP)		ln(1+Investment)		ln(Wage bill/# Empl.)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Election Year	− 0.040	− 0.151**	− 0.002	− 0.008**	− 0.377	− 0.194	− 0.040	− 0.099*
	(0.049)	(0.072)	(0.003)	(0.004)	(0.231)	(0.367)	(0.037)	(0.053)
Post × Democratic mayor has college degree		0.165**		0.008***		− 0.180		0.076
		(0.073)		(0.003)		(0.368)		(0.062)
Plant FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Period	00-04	00-04	00-04	00-04	00-04	00-04	00-04	00-04
Observations	29,994	29,994	22,865	22,865	26,046	26,046	29,993	29,993
#Districts	96	96	95	95	92	92	96	96
Marginal effects								
Democratic mayor has no college degree		− 0.151**		− 0.008**		− 0.194		− 0.099*
		(0.072)		(0.004)		(0.367)		(0.053)
Democratic mayor has college degree		0.014		0.001		− 0.374		− 0.023
		(0.049)		(0.003)		(0.289)		(0.045)

In this table we study the impact of local democratization and democratic mayor education on manufacturing outcomes other than employment. See Sect. 3 for a description of our sample selection and Table 2 for a description of the explanatory variables. *Total Factor Productivity* (*TFP*) is obtained from Javorcik and Poelhekke (2017). To prevent losing a fair share of plant-years, we measure investment as *ln(1+Investment)*. See Table 2 for a description of the explanatory variables. The variable *Post* × *Suharto mayor has college degree* is included in the even columns but not shown. We demean *Suharto mayor has college degree* based on the column-specific sample before computing the interaction with *Post Election Year* so that the coefficient on *Post Election Year* has an unconditional interpretation (rather than indicating the effect of democratization under a non-college educated mayor when the last Suharto mayor does *not* have a college degree). *Election Year* is always included but not shown. Standard errors in parentheses are clustered at the district level. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level

mayor is re-elected for a second term, we cannot rule out that the absence of significant results is due to limited variation in mayor education during this second period; however, taken together with the results in column 4 of Table 2, these findings suggest that leader education matters particularly as a country democratizes (and decentralizes), and perhaps more generally during times of political or institutional change. This is a novel result in the literature.

In Table 3 we study other manufacturing outcomes. Manufacturing revenue (columns 1–2) and total factor productivity (columns 3–4) significantly fall after democratization if the democratic mayor has no college degree, while these variables are unaffected if the mayor does have a college degree. The magnitude of the revenue reduction is strikingly high at around 15%. Plant investment (columns 5–6) does not significantly change with democratization, and there is no heterogeneity with respect to democratic mayor education. While speculative, the absence of a significant investment reduction under non-college educated mayors might be explained by survey evidence that "uncertainty in doing business locally has been increasing since 1999" (Brodjonegoro, 2004, p. 130), thus already before democratization in most districts. The election of the new mayor might have decreased this uncertainty and thereby stimulated investment, while the negative effects underlying our results on employment, revenue, or TFP might have offset such a positive impact. The results of columns 7–8 of Table 3 suggest that if anything, the wage bill divided by the number of employees falls rather than rises after democratization.

5.1.1 Relative versus absolute effects

Since β_1 captures *relative* effects (see Sect. 4), our results are not informative on whether employment actually declines after the election of a non-college educated mayor or if employment growth remains positive but is reduced. To investigate this, we take the sample of column 3 in Table 2, keep districts with democratic mayors without college education, compute the average log employment at the plant level before and after the mayor's election, take the difference of the two numbers and generate the mean across all 1,318 plants. This mean equals -0.051, which clearly indicates that employment falls also in an absolute sense.

5.1.2 Time dimension of effects

In Fig. 1 we employ event study regressions to analyze the time dimension of the effects on employment, revenue and TFP. We extend Eq. (1) with one lead and two lagged dummies relative to the year of democratization: two years before, one year after, and two or more years after, such that the estimated effects are relative to two excluded periods (one and three years before). This is necessary because all of our districts are treated eventually, see Borusyak and Jaravel (2017).[25] The graphs show that democratization under non-college educated mayors has an immediate impact, and that the effects increase over time and are thus persistent over our sample period.

[25] By dropping the earliest possible indicator (which is the dummy indicating three years before treatment, given that our sample period is 2000–2004 and the last districts democratize in 2003), as well as the indicator of one period prior to treatment, we follow Baker et al. (2022).

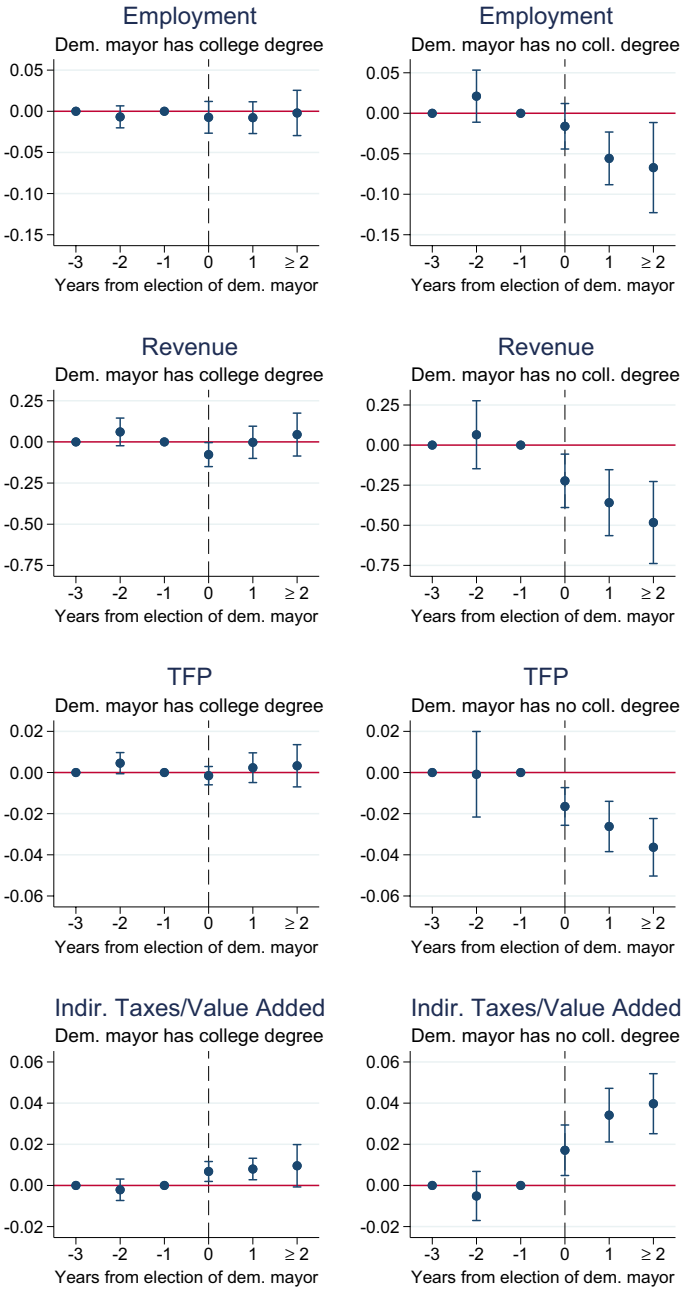

Fig. 1 Timing of effects. *Notes* The graphs are based on event study regressions. We estimate the following specification for the sample of districts with a college-educated democratic mayor (left panel) and the sample of districts without (right): $ln(Y_{ijkpt}) = \beta_0 + \beta_1 ElecYear_{k,-2} + \beta_2 ElecYear_{k,0} + \beta_3 ElecYear_{k,1} + \beta_4 ElecYear_{k,\geq 2} + \mu_i + \omega_{jt} + \delta_{pt} + \epsilon_{ijkpt}$, where e.g. $ElecYear_{k,-2}$ is a (lead) dummy that equals one if in district k the democratic mayor election occurs two years later. Dots indicate point estimates and lines indicate 90% confidence intervals based on standard errors clustered at the district level. The sample period is 2000–2004.

5.2 Mechanisms

What causes the drop in manufacturing performance under democratic mayors without college education, and why does it not occur under college graduates?

5.2.1 An increasing tax incidence

Given the high relevance of local taxes, fees and levies for doing business after democratization and decentralization (see Sect. 2), we start by analyzing the plant census variable "expenditure on indirect taxes". It includes sales taxes, fees for business permits, the building and land tax (*PBB*), road use tax (*SWP3D*), import duties, custom fees, and other levies, except income and personal taxes. Given this broad definition the variable likely provides an accurate representation of the overall incidence of local taxes, fees and levies on manufacturing, and we simply refer to the variable as "indirect taxes", "taxes" or "taxation" in the following.[26] The results displayed in the fourth panel of Fig. 1 and in column 1 of Table 4 show that after democratization, manufacturing plants pay significantly more indirect taxes per rupiah of value added. Column 2 of Table 4 reveals that the increase is significantly larger under non-college educated mayors. The magnitude of the effect under such mayors (a two percentage-point rise) is very large, considering that the average ratio of indirect tax payments to value added equals 3 percent (see Table 1). The results therefore provide a plausible explanation for the decline in employment and other real outcomes.[27]

In order to test the robustness of this conclusion we also analyze different sub-samples of manufacturing plants. Table OA1 focuses on employment and companion Table OA2 on indirect taxes. The results are very reassuring: larger, exporting, and capital-intensive plants face greater employment cuts that are accompanied by higher tax increases, while other plants that face small employment changes also experience small deviations in taxation.[28]

[26] Survey evidence from 2002 reveals that informal levies are typically imposed during transport and/or distribution and that most firms prefer to absorb the resulting cost rather than pass it on to buyers or suppliers (see Ray, 2009). This corroborates the detrimental nature of such levies for businesses. The fact that broadly formulated categories such as "other levies" are also included in the census variable is valuable for our sub-national approach because most included items (such as sales taxes and the building and land tax) are determined by the central government, just like income and personal taxes.

[27] From a theoretical perspective and assuming that a rise in indirect taxes represents an increase in the marginal cost of production, higher indirect taxes imply that a manufacturing plant sooner reaches the profit-maximizing level of production and therefore has a lower level of employment.

[28] In Sect. OA1 we discuss survey evidence that helps to understand the reasons for heterogeneity in taxation across different types of plants.

5.2.2 Decreasing quality of infrastructure

Mayors can also influence the provision and maintenance of local physical infrastructure, which in turn is important for manufacturing. We therefore regress the log of a district-level score of general infrastructure provided by *KPPOD* for the years 2002–2004 on democratization (see columns 3–4 of Table 4). We adjust Eq. (1) to the more aggregate nature of the data: we drop industry-times-year fixed effects and replace plant fixed effects by district fixed effects, but continue to include province-times-year fixed effects. Column 3 shows that the combination of availability and quality of local physical infrastructure significantly decreases after the election of the democratic mayor. This matches the numerous Indonesian news reports on deteriorating infrastructure and a lack of attention by local governments to improve the quality of public service delivery during the democratization process (Brodjonegoro, 2009). Column 4 shows that the negative impact is driven by mayors without a college degree. Since a depreciation of public infrastructure increases the cost of producing and/or transporting goods, this result likely provides an additional explanation for the poor performance of manufacturing plants under non-college educated democratic mayors.

In Panel I of Table OA3 we deepen our analysis by studying the individual components of infrastructure. Our results continue to hold for infrastructure availability and quality separately, and elements that may deteriorate or improve relatively fast such as "quality of telephone service" are affected more by democratization and mayor education. This is intuitive given the relatively short period of analysis.

5.2.3 Total expenditure and spending on other public goods

Are college-educated mayors better able to generate funding from higher levels of government, which enables them to spend more on infrastructure and implies a smaller need for local taxes? We do not find empirical support for this hypothesis: public expenditure by college graduates is not significantly higher (see Table OA4).

It is also possible that mayors without a college degree are simply elected for having promised policies that focus on other areas than supporting the local manufacturing sector. However, Table OA4 also shows that non-college educated mayors do not spend relatively more on local development.[29] Furthermore, in Table OA5 we analyze subcategories of district-level development expenditure and do not obtain evidence that mayors without college education spend more on non-business items such as family welfare, health, housing, environment, religion, or education. The result that large increases in indirect taxes, fees and levies under non-college educated mayors are not accompanied by more government spending is consistent with the hypothesis that these mayors are more corrupt. We investigate this potential link in the next subsection.

5.2.4 Local institutions and corruption

Having a democratic mayor with lower educational attainment could be related to worsening institutions and corruption, which may also affect the business environment.

[29] Development expenditure + Routine expenditure = Total expenditure. Routine expenses are mostly "Expenditure on Employees", such as the salaries of local public servants.

Table 4 Mechanisms

Dependent Variable →	Indirect taxes/value added		ln(Infrastructure)		ln(Institutions)		Gifts, donations etc./ value added	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Election Year	0.009***	0.020***	− 0.220*	− 0.401***	− 0.263	− 0.440	0.001	0.001
	(0.003)	(0.004)	(0.119)	(0.132)	(0.247)	(0.301)	(0.001)	(0.001)
Post × Democratic mayor has college degree		− 0.010**		0.232**		0.161		0.001
		(0.004)		(0.087)		(0.199)		(0.001)
Plant FE	Yes	Yes	–	–	–	–	Yes	Yes
Industry-Year FE	Yes	Yes	–	–	–	–	Yes	Yes
Province-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	–	–	Yes	Yes	Yes	Yes	–	–
Sample Period	00-04	00-04	02-04	02-04	02-04	02-04	00-04	00-04
Observations	23,873	23,873	129	129	129	129	21,702	21,702
#Districts	94	94	50	50	50	50	96	96
Marginal effects								
Democratic mayor has no college degree		0.020***		− 0.401***		− 0.440		0.001
		(0.004)		(0.132)		(0.301)		(0.001)
Democratic mayor has college degree		0.010***		− 0.169		− 0.279		0.001
		(0.003)		(0.110)		(0.242)		(0.001)

In this table we analyze potential mechanisms through which the education level of the first democratic mayor affects the performance of manufacturing plants. See Sect. 3 for a description of our sample selection and Table 2 for a description of the explanatory variables. In columns 1–2 we use data on the manufacturing plant census variable "expenditure on *indirect taxes*". The dependent variable in columns 3–4 is the log of an annual district-level score of physical *infrastructure*, while in columns 5–6 we focus on the log score of the quality of *institutions*. Data on these variables are from the Regional Autonomy Watch *KPPOD*. The sample period in columns 3-6 is 2002–2004 due to data availability. In columns 3-6 we drop districts that do not feature in any of our manufacturing plant-level regressions (see Tables 2, 3 and 4) to ensure sample consistency. In columns 7–8 we use data on the plant-level variable "expenditure on *gifts, donations* and the like". The dependent variables in columns 1–2 and 7–8 are winsorized from above at the 1% level. The variable *Post* × *Suharto mayor has college degree* is included in the even columns but not shown. We demean *Suharto mayor has college degree* based on the column-specific sample before computing the interaction with *Post Election Year* (see also the Notes of Table 3). *Election Year* is always included but not shown. Regarding fixed effects, "–" means that the specific fixed effects are not applicable, or, in the case of district fixed effects in columns 1–2 and 7–8, that they are nested by plant fixed effects and thus need not be included separately. Standard errors in parentheses are clustered at the district level. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level

In columns 5–6 of Table 4 we report the district-level effect of democratization and democratic mayor education on institutional quality over the period 2002–2004, as measured by *KPPOD*. The coefficients' signs point in the same direction as our real outcome and infrastructure results, but they are not statistically significant. In Panel II of Table OA3 we study the individual components of institutional quality and find similar results: variables such as "consistency of regulations" and overall "law certainty" appear to score higher under college-educated mayors—possibly indicating that bureaucracies under such mayors write clearer rules—but the coefficients are largely insignificant.

We can also use the more granular plant-level data that are available for more years to study "gifts, donations and the like" (*hadiah, sumbangan dan sejenisnya*), which has been interpreted as a proxy for bribe payments.[30] The results in columns 7–8 indicate that democratization and democratic mayor education do not affect plant-level expenditure on gifts and donations per rupiah of value added.

Gifts and donations are at best an indirect indicator of corrupt activities by the local democratic mayor because such expenses are also a choice variable of the plant (Fisman & Svensson, 2007; Vial & Hanoteau, 2010), and plants might for example require some time to understand the susceptibility of a new mayor to bribes. Moreover, a newly elected mayor may be not corrupt, but in the short run be unable to detect and limit bribes that have long been extorted by Suharto officials inherited from the old regime. For these reasons, we hand-collect a novel dataset on *mayor*-level corruption involvement (see Section OA4.3 for details). In Table 5 we regress indicators for whether individual democratic mayors are cited in an official corruption case and the outcome of the case on mayor-level characteristics, including education. The results show that mayors without a college degree are significantly more often researched, investigated, declared defendant, and convicted of corruption (see columns 1–6). These results might partly reflect that mayors with a college degree are more able to hide corrupt activities, prevent a corruption case, or block a case from moving forward; however, columns 7 and 8 show that among mayors for which at least research on potential corruption is conducted, college-educated mayor cases are not more likely to be closed early in the process. The mayor-level corruption evidence is therefore overall consistent with the negative effects of democratization on local manufacturing under non-college educated mayors, and can explain the co-existence of higher taxation and worse infrastructure under such mayors.

5.2.5 College-degree field of study

In Table OA7 we test whether our findings on the different manufacturing outcomes are driven by a particular type of college degree (i.e. field of study). The results provide some indication that democratic mayors with a degree in the area of political science, administration, and government are better able to promote employment and revenue, but the evidence is less clear for TFP and indirect taxes. This suggests that the effects we find generally hold across all college degrees.

[30] For example, Brodjonegoro (2004) refers to the variable as "information on bribery at the local level that is implicitly recorded (but underestimated) in the annual industrial survey conducted by the Central Statistics Agency (BPS)" (p. 130). A drawback is that the variable does not include certain types of bribery such as commissions, contract shares, and option prices that are below or above market prices (Vial & Hanoteau, 2010).

Table 5 Mechanisms (continued): Mayor-level corruption involvement

Dependent variable (Corruption Indicator) →	=1 if at least research on corruption, no matter if acquitted later		=1 if at least research and not acquitted later	=1 if at least investigation and not acquitted later	=1 if at least declared defendant and not acquitted later	=1 if convicted of corruption	0 = only research, 1 = only investigation, 2 = only defendant, 3 = convicted	
Sample →	All mayors (districts)						Only mayors with corruption case	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democratic mayor has college degree	-0.297***	− 0.343**	− 0.291*	− 0.318**	− 0.318**	− 0.270*	− 0.153	− 0.524
	(0.111)	(0.131)	(0.147)	(0.157)	(0.157)	(0.149)	(0.329)	(0.434)
Female democratic mayor		− 0.189	− 0.175	− 0.107	− 0.107	− 0.031		1.070
		(0.239)	(0.233)	(0.265)	(0.265)	(0.238)		(0.658)
Democratic mayor age in electionyear		0.011	0.010	0.004	0.004	0.007		− 0.037
		(0.012)	(0.011)	(0.012)	(0.012)	(0.010)		(0.032)
Democratic mayor born in district		− 0.021	0.003	− 0.016	− 0.016	0.170		0.406
		(0.135)	(0.133)	(0.136)	(0.136)	(0.116)		(0.340)
Dem. m. worked in private sector pre-elec.		0.025	0.061	0.053	0.053	− 0.086		− 0.261
		(0.142)	(0.144)	(0.158)	(0.158)	(0.143)		(0.613)
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations (mayors, districts)	92	71	71	71	71	71	44	32

In this table we study the correlation between a mayor's corruption status and his or her education level as well as other mayor characteristics, on the basis of the districts in the sample of Table 2. See Section OA4.3 in the Online Appendix for details on this novel dataset and relevant background information. In columns 1 and 2 the dependent variable equals one if at least one of the relevant bodies (prosecution service, KPK, official courts, police) conducted research on possible corruption by the mayor, no matter if the mayor was later acquitted of all charges or not. In columns 3–6 we define acquitted mayors as not corrupt, thus the dependent variables take the value zero for such mayors. Therefore, unless the mayor was later acquitted, in column 3 the dependent variable equals one if at least research was conducted; in column 4 if at least official investigations were initiated; in column 5 if the mayor was declared an official suspect and thus became a defendant at court; and in column 6 if the mayor was convicted of corruption. In columns 7 and 8 we exclude districts where there is no known corruption case regarding the democratic mayor, thus we restrict the sample to districts in which the dependent variable in columns 1 and 2 equals one. The dependent variable in columns 7–8 equals zero for research only, one for investigations only, two for defendant status only, and three for a conviction. Acquitted mayors were defendants prior to the acquittal, thus for these mayors we define the dependent variable to take the value two in columns 7–8. In all columns we drop districts that do not feature in any of our manufacturing plant-level regressions (see Tables 2, 3 and 4) to ensure sample consistency. Robust standard errors are in parentheses. ***Significant at 1% level; **Significant at 5% level; *Significant at 10% level

5.2.6 Deeper mechanisms: education and mayor behavior

An important question is *why* non-college educated mayors raise taxation by more, neglect infrastructure, and are more corrupt, thereby harming local manufacturing. Krcmaric et al. (2020) lay out four mechanisms through which biographical characteristics might affect political leader behavior: (1) competence and skill, (2) material interests, (3) beliefs and values, and (4) other's perceptions. The authors point out that "education, particularly university-level education, is commonly considered a key formative experience that affects outcomes through all four mechanisms" (p. 137). Better education can thus lead to better governance through each of these channels. However, empirical evidence on these or other mechanisms is scarce, and often focuses on specific degrees rather than a college degree more generally (Flores et al., 2013; Nelson, 2017). We therefore discuss a broader education literature that goes beyond leaders, which sheds light on why leaders' characteristics may affect taxation policy, infrastructure provision, and corruption combined.

Along the competence and skill channel, more educated mayors may better understand the detrimental effect of excessive taxation, worsening infrastructure and corruption on the manufacturing sector. While it is difficult to test these hypotheses directly, they receive support from a literature showing that education leads to higher cognitive ability—not only in the short term (Brinch & Galloway, 2012; Carlsson et al., 2015) but also decades later at an older age (Banks & Mazzonna, 2012). The abundant evidence that educated individuals earn higher wages and produce better economic outcomes further corroborates that education and competence are positively related.

Higher skills might also be reflected in the ability to assemble a better team around oneself. We test this hypothesis in our setting by studying the correlation between the education level of the first democratic mayor and the education level of the average civil servant in the same district at the time.[31] While mayors do not have full control over the local appointment of civil servants, the decentralization laws did grant them extensive rights to make decisions on the careers of village heads and other civil servants (Martinez-Bravo, 2014). However, the (unreported) results show no statistically significant correlation between mayor and civil servant education and the coefficient sign is not constant across different specifications. This is consistent with the hypothesis that "team assembly effects" are not particularly relevant.

Skill-based explanations may also interact with mayors' material interests: based on a better understanding of the involved costs, more educated mayors might refrain from corruption, excessive taxation, or neglecting infrastructure with the goal of raising aggregate output and thereby also personal income. In a similar vein, college-educated mayors may implement growth-enhancing policies in order to increase their chance of re-election, especially if they have longer time horizons in mind than less educated mayors. The latter

[31] Data on civil servant education come from the 2005 intercensal population survey via IPUMS International (Minnesota Population Center, 2018). For each district in our sample, we average the micro data (which takes either 0 = less than primary completed, 1 = primary completed, 2 = secondary completed, 3 = university completed) across all individuals working in the sector "government administration" (the average of the district means equals 2.1). All first democratic mayors have been elected by 2005 and we drop districts from this analysis where the first democratic mayor has been replaced by the second at the time of the survey (June 2005), thereby maximizing the likelihood that civil servants in the sample were appointed by the mayors we study in this paper. Note that we do not test for team assembly effects by studying the correlation between mayor and vice mayor education levels because we do not have data on vice mayors' education level.

appears plausible since Warner and Pleeter (2001) and Falk et al. (2018) show that more educated individuals in the US and around the world are more patient, and Jung et al. (2021) even demonstrate a causal effect of education on patience using Indonesian data. Another material interest channel could be that college-educated mayors are less corrupt because money plays a smaller role in motivating them to run for office in the first place, not least because education is typically linked to wealthier backgrounds (Björklund & Salvanes, 2011).

Regarding beliefs and values, more educated mayors may for example be more altruistic, and therefore aim at larger aggregate output in order to increase the district population's income. This hypothesis receives support from studies showing that conditional on personal income and other factors, education is positively correlated with charitable giving (Forbes & Zampelli, 2013), unconditional helping behavior (Westlake et al., 2019), and social engagement such as community service (Helliwell & Putnam, 2007). College-educated mayors might also refrain from corruption because they have more integrity, but studies analysing the correlation between education and honesty (Abeler et al., 2014; Hübler et al., 2018) do not support this hypothesis. Finally, more educated mayors might be less corrupt because college graduates typically belong to higher social classes in the Indonesian society (Booth, 2021), in which the detection of corruption involvement likely carries higher non-monetary costs—not least because social status itself is a function of what Tirole (1996) calls "collective reputations" (Galiani & Weinschelbaum, 2013).

5.3 Robustness checks

We perform and discuss a large range of robustness checks in the Online Appendix (see Section OA2), some of which we already mention in the discussion of our key identification assumptions in Sect. 4. For reasons of space and relevance, we mainly focus on manufacturing employment as outcome variable in these exercises. We start by discussing robustness checks that test the validity of our first identification assumption (Tables OA8, OA9, OA10) and then move on to checks that address the second (Tables OA9, OA10, OA11, and Figure OA1) and the third identification assumption (Tables OA11–OA16). We conclude by presenting robustness checks that address other potential concerns such as district splits and sample selection bias (Tables OA8, OA16, OA17). Our results are robust to this battery of tests.

6 Conclusion

We provide novel evidence that the education level of newly elected democratic leaders crucially affects the economic success of democratization at the local level. In terms of economic outcomes we focus on the manufacturing sector, a key growth engine particularly for developing and emerging economies for which we have highly granular plant-level panel data. Our results show that in Indonesian districts where the democratic mayor has a college degree, democratization has no effect on manufacturing performance, while the impact is significantly *negative* under mayors without a college degree. For identification, we exploit the unique feature that in Indonesia democratization exogenously occurred at different times at the sub-national district level over the period 1999–2003. Thereby we also improve identification relative to a large literature on the effects of democracy that uses cross-country data. We also pin down mechanisms: non-college educated mayors increase

local taxes, fees and levies by more than mayors with a college degree, and also invest less in infrastructure. While it could be that mayors without a college degree have different priorities than supporting manufacturing, we find no evidence that they rather support items such as family welfare, health, housing, environment, religion, or education. Instead, we find that non-college educated mayors are more likely to be involved in corruption cases. The education level of the local leader is thus closely related to good governance.

Additional findings indicate that more leader education is most beneficial during a democratic transition, and perhaps more generally during times of political or institutional change. Overall, our study thereby makes an important contribution to both the literature on democracy and growth and the literature on the effect of political leaders on economic outcomes. In terms of policy, our results suggest that a college degree requirement for political leaders—which exists in Turkey, Azerbaijan, or Kenya, and is hotly debated in India[32]—are most useful during a democratization period. Via achieving better economic outcomes such as employment, leader education may also make democratic transitions more durable. Our results therefore contain important lessons for other countries that have or will transition to democracy, particularly for developing countries where weak governance and infrastructure constraints are more prevalent.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s10887-022-09221-5.

Acknowledgements This paper was previously circulated under the title "Electing Educated Leaders during Democratization: Evidence from Indonesia". We thank Monica Martinez-Bravo and Andreas Stegmann for providing data for this project, and Shuka Ishikawa for excellent research assistance. We thank Vimal Balasubramaniam, Maurice Bun, Jonathan Dingel, Aysil Emirmahmutoglu, Gunnar Eskeland, Kevin Evans, Andreas Ferrara, Hengky Kurniawan, Peter Lanjouw, Mohammed Mardan, Sultan Mehmood, Yingjie Qi, Felix Schilling, Maximilian Todtenhaupt, Shaoda Wang, Floris Tobias Zoutman, conference participants at the 2021 International Conference in Development Economics, the 2021 German Development Economics Conference, the 2021 Nordic Conference in Development Economics, the International Workshop on The Political Economy of Democracy and Dictatorship (PEDD) 2021, the KVS New Paper Sessions 2020, the 2020 UEA PhD Student Workshop in Urban Economics as well as seminar participants at Vrije Universiteit Amsterdam, Erasmus University Rotterdam, NHH Norwegian School of Economics, and the Monash and Warwick University Applied Young Economist Seminar for valuable comments.

Funding Open Access funding provided by Norwegian School Of Economics.

[32] See Article 101 of Turkey's constitution and Article 100 of Azerbaijan's constitution, both of which can be accessed in English language at https://www.constituteproject.org/countries?lang=en. For Kenya, see the country's 2011 Election Act, at https://www.iebc.or.ke/uploads/resources/kqI5cmgeyB.pdf. Finally, regarding India, see for example https://www.indiatoday.in/education-today/news/story/well-educated-persons-are-needed-in-politics-to-transform-it-union-minister-1950451-2022-05-17, or https://indianexpress.com/article/opinion/columns/narendra-modi-pm-degree-arvind-kejriwal-delhi-university-ba-degrees-of-exclusion-2792374/.

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